

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's population characterized as noncitizen Hispanic. We use the synthetic control method to select a group of states against which Arizona's population trends can be compared. We document a notable and statistically significant reduction in the proportion of the Hispanic noncitizen population in Arizona. The decline observed matches the timing of LAWA's implementation, deviates from the time series for the synthetic control group, and stands out relative to the distribution of placebo estimates for other states in the nation.

I. Introduction

OVER the past 25 years, the unauthorized immigrant population residing in the United States has grown considerably. Since the 1986 passage of the Immigration Reform and Control Act (IRCA), legislation that adjusted the legal status of most unauthorized immigrants in the United States at the time, the undocumented immigrant population subsequently grew to approximately 3 million in 1990 and to roughly 11 million by 2009 (Passel & Cohn, 2010). Post-IRCA, there has been no comprehensive federal legislation intended to address unauthorized immigration aside from efforts to strengthen border enforcement.

Recent years have witnessed a sea 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 deter unauthorized immigration to specific states. Most of these state laws aim to increase the costs to employers and undocumented immigrants of unauthorized employment, and thus shift labor demand to legal workers who tend to compete in the labor market with unauthorized immigrants.

The 2007 Legal Arizona Workers Act (LAWA) is arguably one of the strictest of these state laws. LAWA 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 federal earnings and immigration databases. In May 2011, the U.S. Supreme Court upheld the constitutionality of LAWA, paving the way for further such state legislation and emboldening the efforts of states that already have such laws in effect. Interestingly, mandatory use of E-Verify for all new hires is a central proposal in national discussions of how to tackle unauthorized immigration and is likely to be part of any future comprehensive immigration reform.

In this paper, we assess whether the passage and implementation of LAWA has affected the internal composition

of Arizona. Specifically, we test for an effect of LAWA on the proportion of the state population most likely to be unauthorized: prime-working-age noncitizen Hispanic with relatively low levels of educational attainment. We use the synthetic control method developed by Abadie, Diamond, and Hainmueller (2010) to select a group of states against which the population trends of Arizona can be compared. We find notable and statistically significant pre-post LAWA declines in the proportion of the population likely to be unauthorized. Our estimates range from declines of 1½ to 2 percentage points.

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 who are Hispanic naturalized citizens, a population group not targeted by the legislation. Here, we find no evidence of a relative decline. We also demonstrate that our results are robust to alternative definitions of the post- and pretreatment periods and are not being driven by the spillover of population into neighboring states. Our 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. We find a large pre-post LAWA increase in rental vacancy rates but no corresponding changes in owner-occupied housing vacancy rates.

II. 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, 91 laws were enacted in 34 states.¹ Many of these laws mandate the use of the federal E-Verify system for certain subsets of employers (for example, over a certain size or firms with state contracts) and impose penalties on both undocumented immigrants working illegally and the employers that hire them.²

Passed in 2007, LAWA is arguably the most comprehensive legislation in this realm. It mandates the use of E-Verify by all Arizona employers to establish the identity and work eligibility of all newly hired employees made after

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

² See Lofstrom, Bohn, and Raphael (2011) for more information on state laws against the hiring of unauthorized immigrants and the E-Verify program.

January 1, 2008.³ The law imposes sanctions on employers who “knowingly” hire unauthorized immigrants, including a business license suspension for the first offense and revocation on a second.⁴ LAWA substantially increased the number of employers using E-Verify. Arizona employers registered with E-Verify increased from fewer than 300 in March 2007 to over 38,000 in January 2010 (roughly one-quarter of employers in the state).⁵ Arizona employers account for one-third of nationwide registrations in the system,⁶ and are more than twenty times as likely as employers in California to use E-Verify.⁷ Roughly 700,000 new hires made between October 2008 and September 2009 in Arizona (roughly half of all new hires over this period) were run through E-Verify.⁸

To the extent that LAWA has made it more difficult for unauthorized immigrants to find work in Arizona, this should be reflected in the internal composition of state residents. Specifically, those planning to migrate illegally to Arizona may decide to migrate elsewhere. Second, undocumented immigrants residing in Arizona pre-LAWA may choose to leave due to perceived or actual increases in difficulty finding employment.

Aside from reductions in the undocumented immigrant population, the legislation may also reduce the relative size of the legal immigrant or native-born population of the state. This could occur through several channels. First, some legal immigrants, naturalized citizens, and native born have household members (spouses, parents, siblings) who are unauthorized. Since migration often involves whole households, some authorized immigrants or citizens may leave Arizona as a result of LAWA’s impact on a household member.

Furthermore, the population of legal workers may decline if it becomes increasingly difficult to find employment in Arizona. This might occur due to an increase 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 Social Security Administration (SSA) and Department of Homeland Security (DHS) records. If a match between provided information and the administrative records cannot be made, the E-Verify system returns a report of nonconfirmation to

the employer. A formal evaluation of E-Verify by Westat (2007) found that less than 1% of natives but almost 10% of foreign-born U.S. citizens received an erroneous nonconfirmation of work authorization. To the extent that such nonconfirmations make it more difficult to find and hold employment, legal foreign-born residents of Arizona may have an incentive to work elsewhere.

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

III. 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 who are Hispanic noncitizens, and the proportion of residents who fall within key subsets of this demographic group—in particular, Hispanic noncitizens with a high school diploma or less and of prime working age (15–45 years old). 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 other 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 working-age Hispanic noncitizens with relatively low levels of education.⁹

Table 1 describes trends in these population groups for the period from 1998 to 2009. Recall that LAWA was passed in mid-2007 and implemented in January 2008. Hence, the last two years constitute the posttreatment periods while population responses in 2007 are possible through anticipation of LAWA’s implementation. The proportion of

³ 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.

⁴ To date, legal action taken against employers for violating the provision of LAWA has been quite rare. As of April 2010, only three employers had been indicted for violations, all in a single county (Maricopa). (Santa Cruz, 2010).

⁵ Westat (2009) and Arizona Attorney General (2010), respectively.

⁶ Westat (2009), fraction nationwide as of June 2008, and Rosenblum (2009), fraction in Arizona as of February 2009.

⁷ Rosenblum (2009).

⁸ Berry (2010).

⁹ Estimates suggest that as of 2009, 80% of unauthorized immigrants nationwide were Hispanic, 58% were between the ages of 18 and 39, and the majority have fewer than twelve years of formal education (Passel & Cohn, 2010). In the subgroup of “likely unauthorized” defined as Hispanic noncitizen 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 noncitizen Hispanic immigrants resided in Arizona in 2008. For this same year, Passel and Cohn (2009a) estimate that there were 500,000 unauthorized immigrants in the state. Similarly, for the “likely unauthorized” subgroup, we estimate that 229,000 were in the labor market in Arizona in 2008 compared to the Passel and Cohn’s (2009a) estimate of 300,000.

TABLE 1.—TRENDS IN THE PROPORTION OF ARIZONA RESIDENTS WHO ARE FOREIGN BORN, NONCITIZENS, HISPANIC NONCITIZENS, AND ALL RESIDENTS AND BY EDUCATION FOR RESIDENTS 15 YEARS OF AGE AND OLDER, 1998–2009

	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009
Noncitizen	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 noncitizen	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 noncitizen 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 high school	0.067	0.069	0.061	0.057	0.057	0.062	0.076	0.064	0.064	0.069	0.059	0.047
High school graduate	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	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	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 noncitizens 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 high school	0.084	0.085	0.077	0.074	0.080	0.090	0.111	0.086	0.080	0.089	0.078	0.062
High school graduate	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	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	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.

Arizona residents who are noncitizens exhibits a modest upward trend between 1998 and 2006, increasing from 9.9% to 11.1% over this period. Beginning in 2007, the proportion of noncitizens begins to decline, reaching 8.3% by 2009 (a decline relative to 2006 of 2.8 percentage points). Population trends among Hispanic noncitizens are similar. There are slight increases in the proportion of the Arizona population described by this category between 1998 and 2006. After 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%). By 2009, this proportion declines to 10.4%, 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 presents trends in the proportion of the population 15 and over and 15 to 45 who are Hispanic noncitizens and 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 diploma or less. Because all four of these traits—noncitizen, Hispanic, working age, and lower levels of educational attainment—are predictive of undocumented status (Passel & Cohen 2009a, 2009b), the raw patterns in table 1 are consistent with a population response on the part of the undocumented to LAWAs passage.

To assess whether the observed relative population declines of the foreign born are being driven by a response to LAWAs, we need to identify a comparison state or states that chart the counterfactual path of population trends for Arizona. There are several strategies for constructing such a comparison group. One is 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. 2010, the donor pool). Define F_0 as a $k \times 1$ vector with elements equal to the proportion of the Arizona population that is noncitizen Hispanic in each year from 1998 through 2006 (the nine years we use throughout this paper as our preintervention period) plus additional covariates predictive of the presence of noncitizen 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 preintervention data vector 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 preintervention 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 preintervention Arizona. Specifically, the weighting vector is chosen by solving the constrained quadratic minimization problem

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

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

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 $F_0 - F_1W$ to the objective function being minimized.¹¹

Once an optimal weighting vector W^* is chosen, both the preintervention path and the postintervention 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 postintervention values for the synthetic control group serve as our counterfactual outcomes for Arizona. In addition to including all preintervention values of the dependent variable in F_0 and F_1 , we also include average values of the proportion of the state workforce 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: 1998–2000, 2001–2003, and 2004–2006.¹²

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 preintervention period 1998 through 2006 and $Outcome_{post}^{AZ}$ as the corresponding average for the two posttreatment 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 preintervention difference between the averages for Arizona and synthetic Arizona from the comparable postintervention difference, or

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

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

¹¹ The Stata procedure developed by Abadie et al. (2010) uses as the default a regression-based measure of V where matching variables that are strong predictors of the dependent variable are given more weight and the elements of V are normalized such that they sum to 1. Since we are matching on all preintervention 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 (set $V = I$) across preintervention values and have also estimated fully nested models that choose both optimal values of V and as W (as in Abadie & Gardeazabal, 2003). Because 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 preintervention values of the dependent variable are nearly identical to the results when covariates are included.

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 equation (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 before and after LAWA, omitting it from the donor pool for estimating the placebo intervention effects 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 vary across the dependent variables. California received positive weight for all three dependent variables ranging from 0.487 for the proportion noncitizen Hispanic with high school or less among the prime age, to 0.747 for the proportion noncitizen 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 noncitizen Hispanic populations, growth in these “new destination” states during the early 2000s parallels that of Arizona. Table A1 in the online appendix for this

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.

¹⁴ As the proportion of noncitizen Hispanic drops sharply in Arizona, including Arizona in the donor pool for each placebo estimate should bias the placebo estimates toward 0 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.

TABLE 2.—STATES RECEIVING POSITIVE WEIGHTS FOR THE SYNTHETIC CONTROL GROUPS

Noncitizen Hispanic		Noncitizen Hispanic Age 15 and Over with High School Diploma or Less		Noncitizen Hispanic Age 15 to 45 with a High School Diploma 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).

paper presents average values for the matching covariates used to identify the synthetic comparison group for Arizona and for synthetic Arizona.

IV. Validating the Identification Strategy

Our empirical strategy requires that the enactment of LAWА represents an exogenous shock to the labor market. We are particularly concerned about two factors: potentially coincident economic conditions and endogenous policy changes. Regarding economic conditions, LAWА was debated and passed during a period of economic growth but was enacted at a time of declining labor market conditions in Arizona. Furthermore, LAWА was the end result of a lengthy legislative debate that crossed multiple legislative sessions and was targeted at a long-term problem rather than a yet-unseen economic decline.¹⁵

Nonetheless, because the Great Recession coincides with the implementation of LAWА, we must rule out that the recession is driving our results. There is evidence that the recession reduced the inflow of new immigrants to the United States 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 employer of unauthorized immigrants. Furthermore, construction is one of the biggest industries in Arizona (representing close to 11% of total private employment in 2006). Thus, it is important in our evaluation strategy to ensure that we do not attribute changes in population to LAWА if they were in fact driven by the decline in construction and real estate in Arizona specifically.

The recent recession caused a clear reduction in Arizona's workforce. Figure A1 in the online appendix shows strong employment growth from 2003 to 2006, with a noticeable slowdown in 2007. This was followed by 3% and 8% decreases in 2008 and 2009, respectively. Figure A1 also shows that the negative employment effects of the recession on employment were not any stronger in Arizona

¹⁵ Moreover, there was considerable uncertainty as to whether LAWА would be enacted on January 1, 2008. Federal lawsuits challenging its constitutionality 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 (Gonzales, 2007). These facts may also suggest that anticipatory impacts of LAWА are likely to be small.

than in neighboring areas, including inland California (an area that shares many of the characteristics and trends of Arizona and is hence used in our empirical analysis). Finally an application of the synthetic cohort method to employment growth fails to reveal a LAWА effect in Arizona.

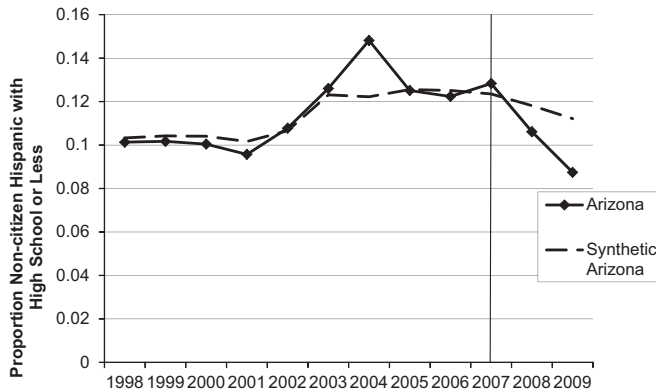
Importantly, the recession was precipitated by a housing crisis, which brought new housing construction to a near standstill. The fact that many unauthorized immigrants are, or maybe more accurately were, employed in the construction sector means that they may have been particularly affected by the recession. 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 (see figure A2 in the online appendix). Overall, the data indicate 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.

We are also concerned about the potential coincidence of federal immigration enforcement increases with the enactment of LAWА. While the U.S. Border Patrol (USBP) launched a number of enforcement initiatives over our analysis period, only those exactly coincident with LAWА and unique to Arizona threaten our identification strategy. The enforcement policies meeting these criterion potentially include Operation Streamline, implemented in the Tucson sector (covering the vast majority of the Arizona border) in January 2008,¹⁶ and border infrastructure enhancements in the Southwest Region from 2005 to 2009.¹⁷ We review official apprehension data, policy information, and research on the efficacy of these policies and find no compelling evidence that these disproportionately affected the unauthorized population in Arizona coincident with the implementa-

¹⁶ Kerwin and McCabe (2010).

¹⁷ The Arizona Border Control Initiative built up infrastructure on Arizona's border with Mexico but predated LAWА by a few years. Regarding other policies, Operation Streamline greatly enhanced prosecution of unauthorized crossers in the Southwest border region between 2005 and 2009, the Secure Fence Act of 2006 mandated construction of 670 miles of reinforced fencing on the Southwest Border by 2008, and the Secure Border Initiative (SBInet) over 2005 to 2011 involved primarily technological enhancements to border security. SBInet is the easiest to address. While it was scheduled to be installed on the Arizona border in February 2008, delays plagued the program until its eventual cancellation in 2011 due to cost and inefficacy. While it is more difficult to ascertain exactly when various parts of the fence infrastructure were built specifically in Arizona, our review of apprehension data does not suggest that if it was built coincident with LAWА, or that it had any sizable impact on border crossing as measured by apprehensions.

FIGURE 1.—PROPORTION NONCITIZEN HISPANIC WITH A HIGH SCHOOL DIPLOMA OR LESS AMONG PRIME WORKING-AGE PERSONS IN ARIZONA AND THE SYNTHETIC COMPARISON GROUP, 1998–2009



tion of LAWA. While apprehensions declined 16% in Tucson in 2008, the share of all Southwest border apprehensions in Tucson remained remarkably stable (between 44% and 45% over 2007 to 2009, and never below 36% over a ten-year period).¹⁸ 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 higher penalties to unauthorized crossing.¹⁹ Furthermore, a number of studies find that labor market conditions and the costs of migration play a larger role in deterring unauthorized migration than border enhancements.²⁰

V. Basic Results

We begin with a graphical presentation of the Arizona population trends and the comparable population trends in synthetic Arizona. Here, we focus on the most refined subgroup containing the highest proportion of unauthorized immigrant workers: noncitizen Hispanics of prime working age with a high school diploma or less.²¹ Figure 1 presents the proportion of the prime working-age population that is noncitizen Hispanic with a high school education or less in Arizona and synthetic Arizona. Focusing first on the preintervention period 1998 through 2006, the figure reveals that the population trend for the synthetic control group closely matches the corresponding trend in Arizona. One exception occurs in 2004, where Arizona's proportion exceeds that of

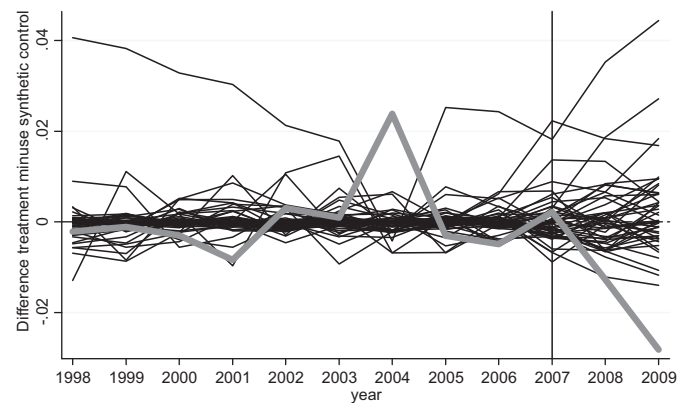
¹⁸ The Tucson sector covers the vast majority of the Arizona border and inland area; it also accounts for the largest share of border arrests in the Southwest region. In the Yuma sector (the extreme western border of Arizona), apprehensions declined 70% in 2007 and 79% in 2008, but composed only 1% to 4% of all apprehensions in the Southwest region over this period.

¹⁹ Lydgate (2010) in interviews with federal defenders.

²⁰ Kerwin and McCabe (2010), Cornelius et al. (2010), and Roberts et al. (2010). As Roberts et al. show, border security enhancements affect the cost of migration, as measured over two-year periods (thus likely with a lag).

²¹ Graphical presentations for all outcomes are available on request. Point estimates for all population groups are given in tabular form below.

FIGURE 2.—DIFFERENCE IN THE PROPORTION NONCITIZEN HISPANIC WITH A HIGH SCHOOL DIPLOMA OR LESS AMONG PRIME WORKING-AGE RELATIVE TO THE SYNTHETIC CONTROL GROUP, ALL STATES ARIZONA IS DISPLAYED WITH THE THICK GRAY LINE.



the synthetic control by about 3 percentage points. Our research suggests this outlier is related to an artifact of the CPS data and not to any underlying policy changes that would affect the location decisions of immigrants.²² The average preintervention difference between Arizona and synthetic Arizona is almost 0, with a root mean squared error of 0.0095. Hence, the synthetic control group matches the preintervention values for Arizona quite well.

In the postintervention period, figure 1 reveals a sizable gap (on the order of 2 percentage points) between Arizona and the synthetic control group. The gap relative to the synthetic control does not widen until 2008 and is wider still by 2009. Thus, the declines in the immigrant subpopulation observed in Arizona are not observed in states with comparable pre-LAWA population composition and dynamics.

Figure 2 displays 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 figure displays 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 difference for Arizona is displayed by the thick gray line. During the preintervention period, 1998 through 2006, the Arizona data points clearly lie within the distribution of placebo estimates, suggesting that Arizona is not an outlier during this period. In the postintervention years, the Arizona differences move to the bottom of the distribution in figure 2. By 2009 the state becomes a visible outlier.

²² In 2004 there was an unusually large adjustment to intercensal population controls due to revised estimates of immigration for the preceding years, which disproportionately affects the estimates of Hispanic immigrants (see Bureau of Labor Statistics, 2004). CPS weights were not adjusted accordingly. Population control revisions in subsequent years were much smaller. Note in figure 2 that we observe an outlier in the opposite direction for the proportion of immigrants in California. Furthermore, in a specification check (see table 3), we omit 2004 and earlier years from the preintervention period and find qualitatively similar results.

Table 3 presents estimates of several variants of the difference-in-differences estimator laid out in equation (2). For each outcome, the first two columns present the mean difference between Arizona and the synthetic control for two different groupings of the preintervention years: 1998 through 2006 and 2005 through 2006. The third column presents the average postintervention 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 that the *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 preintervention base period and then presents the results using the two-year preintervention base period. The results in panel A show the estimates for the narrowest subgroup focused on prime working-age people; panels B and C show results for all working age and all people, respectively.

Panel A reveals relatively small preintervention differentials between Arizona and synthetic Arizona in each comparison that widen considerably in the postintervention period (to -2.6 and -2 percentage points for prime working-age noncitizen Hispanics and those with high school or less, respectively). This yields difference-in-difference estimates between -1.6 and -2.7 percentage points depending on the length of the preintervention period. Within the distribution of placebo estimates, three of the four difference-in-difference estimates are the most negative, while one ranks second out of 47.

Panel B reveals slightly smaller point estimates when population changes are measured relative to all residents age 15 and above. Again, preintervention differences in these outcomes are very small (never greater than 0.001 in absolute value). Postintervention, the differentials widen to -1.2 to -1.3 percentage points. Difference-in-difference estimates for the two outcomes and the two alternative preintervention periods range from relative declines 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 results measured relative to the entire resident population. The average difference relative to synthetic Arizona is 0 in both of the defined preintervention periods. This difference, however, widens to -1.5 percentage points in 2008-2009, with difference-in-difference estimates of comparable magnitude. In both comparisons, Arizona's difference-in-difference estimate is the most negative, yielding the minimum *p*-value of 0.021.

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

TABLE 3.—ESTIMATED IMPACT OF THE PASSAGE AND INTRODUCTION OF LAWA ON THE REPRESENTATION OF NONCITIZEN HISPANICS AMONG ALL ARIZONA RESIDENTS, AMONG ARIZONA RESIDENTS 15 AND OVER, AND AMONG ARIZONA RESIDENTS 15-46 YEARS OF AGE

	Average Difference Relative to comparison, Nine Pre-intervention Years	Average Difference Relative to comparison, 2005 and 2006	Average Difference Relative to comparison, 2008 and 2009	Difference-in-Difference Analysis Relative to Nine Preintervention Years			Difference-in-Difference Analysis Relative to 2005/2006 Base Period		
				Change, Post-Pre	Rank, Lowest to Highest	<i>P</i> -Value from One-Tailed Test, $P(\Delta < \Delta_{AZ})$	Change, Post-Pre	Rank, Lowest to Highest	<i>P</i> -Value from One-Tailed Test, $P(\Delta < \Delta_{AZ})$
A. Relative to All Arizona Residents 15-45 Years of Age									
Noncitizen Hispanic	0.000	-0.004	-0.026	-0.027	1/47	0.021	-0.022	2/47	0.043
Non-Citizen Hispanic High School or less	0.000	-0.004	-0.020	-0.021	1/47	0.021	-0.016	1/47	0.021
B. Relative to Arizona Residents 15 and Over									
Noncitizen Hispanic	0.000	-0.001	-0.013	-0.014	1/47	0.021	-0.012	1/47	0.021
Noncitizen Hispanic, High school or less	0.000	-0.001	-0.012	-0.012	1/47	0.021	-0.011	1/47	0.021
C. Relative to Arizona Residents									
Noncitizen Hispanic	0.000	-0.001	-0.015	-0.015	1/47	0.021	-0.014	1/47	0.021

Average differences pre- and postintervention 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 employs 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 SUBPOPULATIONS OF THE FOREIGN BORN IN ARIZONA, 1998–2009

	Native Born	Foreign Born, Naturalized Citizens	Foreign Born, Noncitizens	Noncitizen Hispanics
1998	4,007,252	171,177	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 noncitizen, and noncitizen, Hispanics are 896, 2,263, and 1,832 respectively. The smallest samples size is 807 observations of foreign-born naturalized citizens in 1998.

million. The difference-in-difference estimates measured relative to the entire resident population in panel C imply population loss ranging from 86,800 to 93,000.

VI. 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 subgroups 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 a surge in domestic migration to Arizona. We then explore whether the estimation results are sensitive to the definition of the posttreatment period and perhaps biased by cross-state spillover. 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 our observed effects are an artifact of our focus on proportions. Table 4 presents estimates from the monthly CPS files of the Arizona nativeborn, naturalized foreignborn, foreign-born noncitizens, and noncitizen Hispanic populations.²³ Beginning with the patterns in the last two columns, we observe a steady increase in the noncitizen Hispanic population between 1998 and 2006, with an annual average growth rate of 5.3%. Between 2006 and 2009, the noncitizen Hispanic population declines absolutely by 125,549. The native population does indeed grow between 2006 and 2009, but 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

²³ Population estimates are tabulated by summing the population weights from the monthly files within a year and then dividing by 12.

of 3.1%. The comparable growth rates for 2007 and 2008 are 2.8% and 3.5%, respectively. Hence, these raw figures indicate that our difference-in-difference estimates are indeed being driven by absolute declines in the noncitizen Hispanic population.

In the main results in table 3, we define the postperiod as calendar years 2008 and 2009 due to the fact that LAWA was implemented on January 1, 2008, and have excluded 2007 from the preperiod as well. One might contend that 2007 should be included as a posttreatment year as the legislation was passed mid-2007 and households may have migrated in anticipation of the law's passage and implementation. 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 noncitizen Hispanic with high school or less education among those prime working age and among those age 15 and over, as well as the proportion noncitizen Hispanic among all residents. The relative population proportion declines for Arizona, including the 2007 population, are somewhat smaller (by roughly one-half to seven-tenths of a percentage point compared to table 3). 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.

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 affected by the law should increase with time. Based on this reasoning, we prefer the estimates in table 3 that omit the 2007 values from any calculations.²⁴

An additional issue concerns potential bias caused by population spillover created by migration out of Arizona. In general, Arizona's population loss may be due to deterred future migration, foreign migrants leaving the country, or migrants leaving for other states. If the last is an important contributor to state population among those states contributing to the synthetic control group, then the suitability of the posttreatment path for the synthetic control group in chart-

²⁴ An additional concern is the effect of the passage of subsequent legislation in Arizona that authorized targeted police scrutiny of suspected unauthorized immigrants. The Support Our Law Enforcement and Safe Neighborhoods Act, commonly referred to as SB1070, was signed by the governor in April 2010 and scheduled to go into effect in July 2010. However, court injunctions prevented implementation of much of the law through May 2012, when SB1070 was tested and partially upheld in the U.S. Supreme Court. Our study period ends in 2009, and thus is unlikely to be affected by anticipatory effects of SB1070. However, we conduct a test of this by dropping 2009 from the posttreatment period. The results are qualitatively similar though smaller when 2009 is dropped (not presented but available on request). We prefer the results inclusive of 2009 given the likely cumulative effect of LAWA and the great uncertainty surrounding whether SB1070 would ever be implemented.

TABLE 5.—ALTERNATIVE DIFFERENCE-IN-DIFFERENCE ESTIMATES INCLUDING 2007 AS A POSTTREATMENT YEAR AND EXCLUDING STATE BORDERING ARIZONA FROM THE POTENTIAL POOL OF CONTRIBUTING STATES TO THE SYNTHETIC CONTROL

	Average Difference Relative to Nine Pre-intervention Years			Average Difference Relative to Comparison, 2005 and 2006			Average Difference Relative to Postintervention ^a			Difference-in-Difference Analysis Relative to Nine Preintervention Years			Difference-in-Difference Analysis Relative to 2005/2006 Base Period		
	Average Difference Relative to Nine Pre-intervention Years	Average Difference Relative to Comparison, 2005 and 2006	Average Difference Relative to Postintervention ^a	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, $P(\Delta < \Delta_{AZ})$	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, $P(\Delta < \Delta_{AZ})$
A. Including 2007 as a Posttreatment Year															
Noncitizen Hispanic, high school or less ages 15–45	0.000	-0.004	-0.013	-0.013	1/47	0.021	-0.009	2/47	0.043	-0.009	2/47	-0.009	2/47	0.043	
Noncitizen Hispanic, high school or less age 15 and over	0.000	-0.001	-0.006	-0.007	2/47	0.043	-0.006	3/47	0.064	-0.006	3/47	-0.006	3/47	0.064	
Noncitizen Hispanic	0.000	-0.001	-0.009	-0.009	1/47	0.021	-0.009	1/47	0.021	-0.008	1/47	-0.008	1/47	0.021	
B. Dropping States That Border Arizona from the Donor Pool															
Noncitizen Hispanic, high school or less, age 15 to 45	0.012	0.004	-0.016	-0.028	1/43	0.023	-0.020	1/43	0.023	-0.020	1/43	-0.020	1/43	0.023	
Noncitizen Hispanic, high school or less, age 15 and over	0.004	0.001	-0.016	-0.020	1/43	0.023	-0.020	1/43	0.023	-0.017	1/43	-0.017	1/43	0.023	
Noncitizen Hispanic	0.008	0.003	-0.014	-0.022	1/43	0.023	-0.022	1/43	0.023	-0.017	1/43	-0.017	1/43	0.023	

Average differences pre- and postintervention 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 employs the empirical distribution of the placebo effect estimates of LAWA for 46 additional states. ^aPostintervention period includes 2007 in panel A but excludes 2007 in panel B.

ing 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.

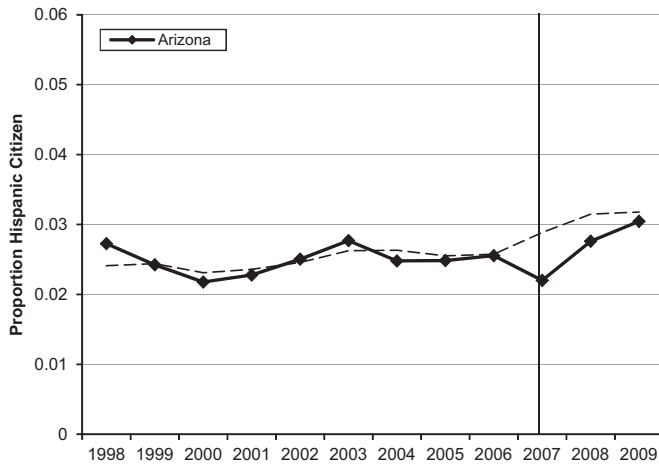
In the current application, there are several reasons to believe that such spillover is quantitatively unimportant. To start, our negative difference-in-difference estimates are driven largely by absolute declines in Arizona rather than increases in synthetic Arizona. For example, averaging the pre- and postintervention values in table 1 using the period definitions employed in table 3 shows an absolute decline in the proportion of Arizona residents who are prime working-age noncitizen Hispanic with less education of 1.8 percentage points (compared to the difference-in-difference estimate of 1.6 to 2.1 percentage points).

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. Our difference-in-difference estimates suggest that the proportion of Hispanic noncitizens 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 population moved to neighboring California. Such a population move would increase the proportion of California residents who are noncitizen Hispanic from the actual value in 2007 of 0.110 to the hypothetical value of 0.113.

Finally, when we restrict the donor pool to states that do not share a border with Arizona 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 border Arizona yields difference-in-difference estimates that are essentially the same as those that include these states in the donor pool. Moreover, the observed DD estimates for Arizona are still more negative than each of the remaining 42 placebo estimates for all outcomes.

Our final specification check involves testing for an effect of LAWA using a more traditional difference-in-difference estimator and inference techniques based on the actual CPS microdata. We explored three alternatives. First, we employ the weights generated by the synthetic cohort estimator (table 2) to select comparison states and reweight the contribution of each individual observation 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 1% level of confidence) difference-in-difference estimates that are similar in magni-

FIGURE 3.—COMPARISON OF THE PROPORTION HISPANIC NATURALIZED CITIZEN IN ARIZONA AND THE SYNTHETIC COMPARISON GROUP



tude to the estimates presented in table 3. These results are available on request.

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

LAWA is targeted specifically at unauthorized foreign-born job seekers. Thus, one would expect the largest population impact on groups with high proportions of unauthorized job seekers. Conversely, while legal immigrants may also leave the state due to, say, social connections with unauthorized immigrants, one would expect smaller population changes among the authorized. 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, for example, the Arizona housing market. Immigrants account for a relatively large share of Arizona households residing in rental housing. Moreover, the majority of the Arizona population resides in owner-occupied housing. These two facts suggest that a LAWA-induced population loss should disproportionately affect the rental market.

In this section we present evidence pertaining to these falsification tests. We begin by testing for an impact of LAWA on the proportion of Arizona residents who are Hispanic naturalized citizens. Figure 3 displays trends in the proportion who are Hispanic naturalized citizens for Arizona and for the synthetic control for Arizona for the period 1998 through 2009. Despite a dip in this series in 2007, this proportion appears roughly stable through the implementation of LAWA. Compared to the placebo distribution, the 2009 difference for Arizona lies well within the distribution of placebo estimates for other states (not shown here). This is reflected in the difference-in-difference estimator given in the first row of table 6. For the period 1998 through

2006, the average difference between Arizona and its synthetic control group is 0. For the two postintervention years (2008 and 2009), the difference is also 0 yielding difference-in-difference estimates of 0. The estimate ranks 29th of the 47 estimates yielding a p -value of the one-tailed test for a decline in this population variable of 0.617. Hence, there is little evidence that naturalized Hispanics responded to LAWA by migrating from the state.

Regarding the Arizona housing market, our tabulations of data from the 2006 American Community Survey (ACS) show that roughly 41% of Arizona households headed by the foreign born resided in rental housing compared with 28% of native-born households. Among households headed by a noncitizen, 53% rent; the comparable figure among households headed by a Hispanic noncitizen is 56%. 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%, respectively.

Given the relative concentration of immigrants in rental housing, LAWA-induced population losses should disproportionately affect the Arizona rental market. Here we assess this proposition by applying our synthetic comparison difference-in-difference estimator to rental and owner-occupied housing vacancy rates. 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). Since we have quarterly data, we define the preintervention period as all quarters prior to the third quarter of 2007. To identify the states contributing to the synthetic control, we match on annual average vacancy rates for the preintervention period as well as the seasonal averages of these values (the average of the three quarter 1 values, the three quarter 2 values, and so on) 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.²⁵

Before discussing the estimates, we calculate 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 accounted for 29.8% of Arizona households. Our main estimate suggests that LAWA 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 households²⁶ and that the entirety of this decline occurs among rental households, then the rental vacancy rate

²⁵ We match on preintervention values of the proportion in metropolitan areas, under 18, 18 to 29, 30 to 39, 40 to 49, 50 to 64, and 65 and over; the proportion nonwhite, Hispanic, foreign born, poor; and the proportion that rent.

²⁶ A decline in the foreign-born population would affect both the numerator and the denominator of the ratio used to calculate the proportion foreign born; 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.

TABLE 6.—ESTIMATED IMPACT OF THE PASSAGE AND INTRODUCTION OF LAWA ON HISPANIC NATURALIZED CITIZENS, RENTAL VACANCY RATES, AND VACANCY RATES FOR OWNER-OCCUPIED HOUSING

	Average Preintervention Difference Relative to the Synthetic Control ^a	Average Postintervention 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	0.000	0.000	29/47	0.617
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 postintervention 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 employs the empirical distribution of the placebo effect estimates of LAWA for 46 additional states.

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

^b For all outcomes, the postintervention period pertains to 2008 and 2009. For the rental vacancy rates, the postintervention 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 nonnegative 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 nonpositive against the alternative of an increase in vacancy rates.

FIGURE 4.—COMPARISON OF RENTAL VACANCY RATES IN ARIZONA AND THE SYNTHETIC COMPARISON GROUP

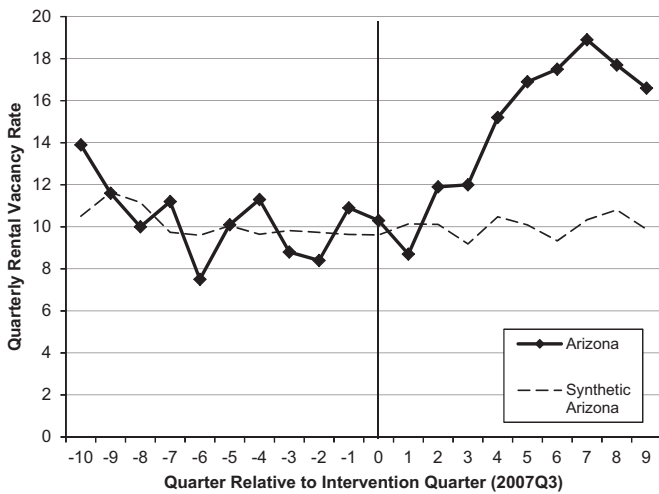
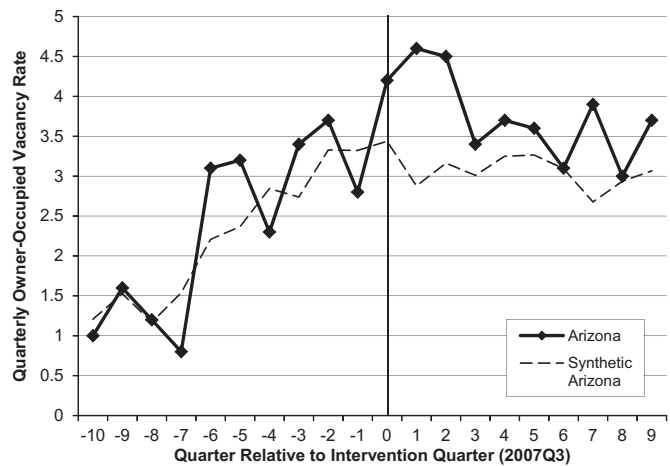


FIGURE 5.—COMPARISON OF VACANCY RATES FOR OWNER-OCCUPIED HOUSING UNITED IN ARIZONA AND THE SYNTHETIC COMPARISON GROUP



should increase by 5.03 percentage points ($[1.5/29.8] \times 100$).

Figure 4 displays the quarterly rental vacancy rates for Arizona and the synthetic control for 2005 through 2009 (quarters are labeled relative to quarter 3 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. In contrast, there is no observable effect on owner-occupied vacancy rates (figure 5).

The last two rows of table 6 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 preintervention Arizona values for both outcomes. During the postintervention quarters, the difference in rental vacancy rates between Arizona and synthetic Arizona increases to 5.8 percentage points. The difference-in-difference estimate of the impact of LAWA on rental vacancy rates is quite close to the post-treatment difference in means (5.6 percentage points) and quite close to our back-of-the-envelope calculation. Regard-

ing statistical inference, the pre-post LAWA increase in relative rental vacancy rates for 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.

VII. 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, driven in particular by the decline in low-skilled workers of prime working age. The decline observed for Arizona matches the timing of LAWA's implementation, deviates from the time series for the synthetic control group, and stands out relative to the distribution of placebo estimates for the remainder of the 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. Moreover, we do not observe similar increases in the vacancy rate for owner-occupied housing. This is sensible as those most likely to be affected by the law are disproportionately concentrated in rental housing.

While the focus of this paper has been on net changes in the internal composition of Arizona's population, a number of additional questions 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 (undocumented and documented) who 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 United States (Bansak & 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 (Card, 2001, 2005; Borjas, 2003; Ottaviano & Peri, 2008). LAWA intended to divert labor demand from the unauthorized foreign born to legal workers in the state, the majority of whom will be the native born. Further work should focus on theoretically modeling the exact channels through which such demand diversion would affect the employment outcomes of the native born and then empirically estimate the magnitude of any such impacts.

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