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Technical Appendices

Lessons from the 2007 Legal Arizona Workers Act

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Appendix A. Data

Estimates of the Unauthorized Population

The prevailing methodology for estimating undocumented immigrants employs a "residual" method of estimation. The three primary sources employing this method are: Pew Hispanic Center reports by Jeffrey Passel and D'Vera Cohn (2008, 2009a, 2009b, 2010), independent estimates by Robert Warren, and DHS reports by Hoefer et al. (2010). Based on this method, national estimates of undocumented immigrants are the difference between estimates of the legal foreign-born population from the total foreign-born population. The legal foreign-born population is calculated from various official sources, primarily the Department of Homeland Security. These populations are broken into the following categories:

- Legal permanent residents
- Refugees, asylees, and parolees
- Legal temporary residents (students, professors, high-tech workers, and temporary visa holders)

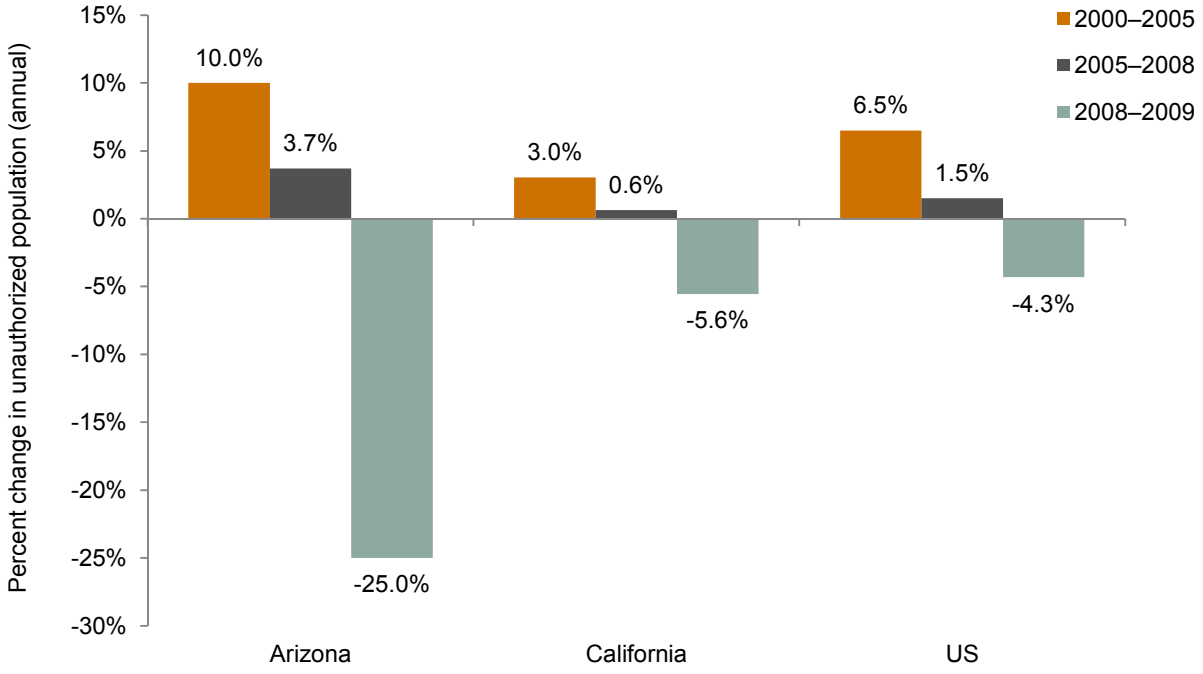
Estimates of the total foreign-born population are based on calculations from the Current Population Survey (CPS), with corrections for undercounting[1] of immigrants and misreporting of place of birth. The different components of the legal population are combined using various demographic methods, including mortality rates, and age-sex-country-specific emigration and interstate mobility rates.

Both Pew and DHS publish U.S. estimates, but they vary on the reporting of state-level estimates. DHS reports estimates of the unauthorized for the ten largest states, whereas Passel and Cohn publish a range of estimates for all states (at least in recent years). The Passel and Cohn methodology for smaller states varies somewhat from the residual method described above. The states we are primarily interested in for this report (Arizona, California, and southern border states) are available in both reports. However, there are few years (2006, 2007) for which the full set of Pew estimates are not available. Unpublished estimates from Robert Warren are another reliable source, using similar residual methodology but (1) basing estimates on the ACS rather than CPS and (2) applying information on flows rather than stocks of immigrants. We have obtained Warren estimates for the U.S. and California, and find that they track quite closely with estimates from Passel. The trend in population is the same but Warren estimates a slightly smaller growth in unauthorized immigration through 2007.

Mimicking Figure 2 in the report, Figure A1 presents estimates of the change in unauthorized population based on Passel and Cohn (2008, 2010).

[1] Since the CPS undercounts certain immigrant groups, the Pew Hispanic Center created undercount rates by age, sex, race and duration of residence based on the 2000 Census, which results in 2–3% undercount rate for legal immigrants. For undocumented immigrants, Pew created a set of estimates that results in a 12.5% undercount rate for undocumented immigrants, which is consistent with other research based on the 2000 Census.

FIGURE A1
Estimates of the percent change in unauthorized population from Pew Hispanic Center



SOURCE: Passel and Cohn (2008, 2010).

NOTE: These changes are calculated on the primary estimates in the Passel and Cohn; ranges for these estimates are also available.

As we noted, the Pew estimates suggest a larger decline in Arizona and smaller declines in both California and the U.S. Given the array of technical issues in calculating estimates of this population, the difference between this figure and Figure 2 of the report are not terribly surprising. Also, while certainly illuminating, these estimates of the unauthorized population do not allow us to probe much further beyond summary information about this population. For example, this methodology does not allow us to test whether LAWA was the cause in the large decline in Arizona relative to other areas. Hence, we move to a different methodology and data to answer the central question of this study.

ACS and CPS

We rely on individual level survey data from the U.S. Census Bureau, including the monthly Current Population Survey (CPS) for 1998–2009, American Community Survey (ACS) for 2005–2008, and Decennial Census for 2000. These data provide information on the employment and demographic characteristics of individuals with state-level geographic identifiers as well as information on race/ethnicity, education, age, other demographic characteristics, and most importantly legal immigration status (i.e. naturalized citizen or not). We primarily rely on the monthly CPS to analyze LAWA's impact. The CPS is available over the entire time period we need, 1998 through 2009. Though the sample size is considerably smaller than in the ACS, the CPS affords higher frequency data and the most recent wage data that is required for our empirical methodology. The smaller sample size means that we must rely upon relatively broader measures of population and employment, and also leads us to favor metrics like population ratios rather than population counts. In addition, the CPS does not provide information on English ability, which we would otherwise like

to use to identify individuals more likely to be unauthorized. We combine all monthly CPS files within years.[2]

For a better understanding of recent trends, we use data from the ACS and 2000 Census. The advantage of the ACS and Decennial Census of 2000 is its larger sample size as well as individual information that can more closely identify unauthorized immigrants. However, the drawback of the ACS is the time period it covers; it only dates back to 2005 and the most recent data available pertains to 2008. Importantly, earnings and wage information in the ACS refer to the twelve months prior to the survey period. However, the specific survey month is unknown to us, complicating an observation's assignment to a pre- or post-LAWA period. This is especially relevant for data from the 2008 ACS, which includes non-separable-identifiable earnings information for the periods before and after the passing of LAWA. For these reasons, the ACS and Decennial Census are used primarily to obtain reliable estimates of overall population and employment and for additional analyses provided in the Appendix.

The ACS and CPS data provide information on country of birth, U.S. citizenship status (citizen, naturalized, or non-citizen), and race/ethnicity. Using this information, the analysis behind the report's conclusions focuses on the outcomes of the following race and nativity groups:

- Foreign-born non-citizen Hispanics
- Foreign-born naturalized citizen Hispanics
- Native-born Hispanics
- Native-born non-Hispanic whites

Note that we use "foreign-born" and "immigrant" roughly interchangeably. Where allowed by the data, we also separate the effects on these groups according to education, focusing in particular on those with high school or less. These definitions help identify the effects on unauthorized immigrants and compare them to the effects on competing groups of workers. No representative datasets allow for identification of unauthorized immigrants directly. However, based on existing research (Passel and Cohn, 2009a and 2010), we can define which of the above groups are likely to include relatively higher proportions of unauthorized workers. This is described in greater detail in the text. The first three groups above are listed in descending order according to the probability of being unauthorized. The first group has the highest proportion of unauthorized immigrants, so we often refer to it as "likely unauthorized". Indeed, if reporting in the data were perfect, only the first group should contain any unauthorized at all. Naturalized and native-born are by definition authorized to work in the U.S. Lastly, our employment analysis focuses on groups most likely to be affected by LAWA and hence is restricted to working age men with relatively low education, those with a high school diploma or less.

Summary statistics of individual characteristics for the population groups defined above, before and after LAWA, are shown in Table A1. We also show summary statistics for African Americans, a group whose employment outcomes we only analyze using the larger ACS sample.

[2] Population weights in the CPS were revised between 2008 and 2009 and may affect estimated changes in population over this period (see Passel and Cohn, 2010). The bulk of our analyses rely on population proportions, however, and thus the estimates are not subject to the same bias from changes in sampling weights. We note where this exception is violated.

TABLE A1
Summary statistics by nativity-ethnicity group for men Age 16–60 with high school diploma or less in Arizona, 2006 and 2008

	Hispanic			Non-Hispanic native	
	Non-citizen	Naturalized	Native	White	Black
2006					
Employed	91.4%	79.6%	62.5%	72.3%	50.8%
Annual earnings	18,693	24,291	17,406	25,945	15,854
Age	30.7	40.2	30.5	35.3	33.5
Years since migration	4.5	22.6	N/A	N/A	N/A
Less than high school	70.9%	60.5%	48.3%	30.2%	37.5%
High school graduate	29.1%	39.5%	51.7%	69.8%	62.5%
Married	45.6%	70.5%	31.3%	37.8%	28.4%
No. children	0.65	1.36	0.63	0.49	0.62
No. children less than 5	0.23	0.20	0.17	0.11	0.12
School age child	19.6%	44.0%	20.4%	18.4%	20.9%
Speak only English	N/A	5.5%	41.0%	97.9%	97.1%
Speak English very well	N/A	37.7%	48.0%	1.6%	2.1%
Speak English well	N/A	26.6%	7.6%	0.3%	0.8%
Speak English but not well	46.5%	22.5%	2.3%	0.1%	0.0%
Do not speak English	53.5%	7.7%	1.0%	0.0%	0.0%
Number of observations	599	274	1,229	3,330	174
2008					
Employed	88.4%	81.7%	66.7%	69.4%	57.7%
Annual earnings	17,243	27,362	19,580	25,336	20,927
Age	31.5	41.5	30.4	34.7	32.8
Years since migration	5.7	24.5	N/A	N/A	N/A
Less than high school	81.3%	56.2%	47.3%	31.4%	42.3%
High school graduate	18.7%	43.8%	52.7%	68.6%	57.7%
Married	41.5%	67.1%	27.4%	32.7%	29.2%
No. children	0.63	1.42	0.56	0.43	0.53
No. children less than 5	0.18	0.23	0.14	0.09	0.13
School age child	19.3%	46.3%	17.3%	16.5%	19.4%
Speak only English	N/A	1.6%	46.4%	97.6%	98.7%
Speak English very well	N/A	41.9%	42.4%	2.1%	1.0%
Speak English well	N/A	35.4%	6.6%	0.2%	0.3%
Speak English but not well	46.9%	14.1%	3.5%	0.2%	0.0%
Do not speak English	53.1%	7.0%	1.1%	0.0%	0.0%
Number of observations	434	289	1,322	3,066	209

SOURCE: Authors' calculations from American Community Survey data

QCEW

We also employ summary-level statistics on employment from the Quarterly Census of Employment and Wages (QCEW), 1998–2009. The QCEW data series is also known as the ES-202 series, as it includes a count of establishments and employment and wages for all employers subject to state and federal Unemployment

Insurance (UI) laws. We rely on quarterly and annual data by state and MSA and by industry from the QCEW.

This data is summary- rather than individual-level, so the employment statistics we calculate are less subject to sampling error or reporting bias. However, because there is no individual information, we cannot compute employment for the specific population groups listed above. The only restrictions we can impose are on ownership categories of employers. Most of our analyses thus restrict the QCEW to private employers, since unauthorized immigrants are unlikely to work for public employers regardless of state policy.

Vacancy Data

We supplement our analysis using data on rental vacancy rates from the Housing Vacancy Survey, part of the Current Population Survey by the U.S. Census Bureau 2005–2009 (CPS/HVS). This survey provides quarterly information on vacancy rates by state in rental housing and owner-occupied housing. Vacancy rates provide another check on the observed declines in population based on survey data.

Appendix B. Theoretical Effects of State-Level Immigration Legislation on State Labor Markets

The intention of Arizona's LAWA was to deter the hiring of unauthorized immigrant workers by making it more expensive for employers to do so while simultaneously making Arizona less attractive to both existing and future unauthorized immigrants. A primary motivation behind such legislation is to improve labor market opportunities for those legally eligible to work in the U.S. Whether such legislation is effective in this regard depends in large part on the degree to which authorized and unauthorized workers compete with one another in the labor market. This in turn is likely to vary by skill, nativity, ethnicity, and the interaction of these dimensions.

To the extent that employers can distinguish unauthorized workers from authorized workers, state laws punishing employers who hire the unauthorized can be modeled as increasing the relative costs of hiring unauthorized workers. Standard labor demand theory predicts that this would induce two reinforcing effects on the demand for unauthorized immigrants and two offsetting effects on demand for other workers. Regarding the targeted workers, higher costs of hiring unauthorized workers should induce both employer substitution towards other workers as well as a general reduction in employment associated with the higher compliance costs (e.g., the costs of additional verification requirements and more risky hiring process, the expected value of the penalty if caught). These substitution and scale effects should unambiguously reduce demand for unauthorized workers, lowering employment levels and perhaps average wages.[3]

However, theory does not yield an unambiguous prediction for other groups of workers. To the extent that authorized immigrants and specific groups of the native-born are easily substitutable in production for unauthorized immigrants, employer substitution will boost labor demand for such workers and ultimately impact their average employment and earnings. However, the size of this substitution effect will be smallest for those who are least like the unauthorized. In fact, there are certain labor groups that are likely to be complementary in production with unauthorized labor, implying that, irrespective of the scale of production in the economy, an increase in the cost of hiring unauthorized workers would actually harm these natives.

For all workers, even those that are close substitutes for the unauthorized, higher compliance costs should result in a reduction in the overall scale of an organization's activities. Hence, offsetting substitution and scale effects do not permit prediction of the impact of these laws on documented immigrants and the native-born. Theory does suggest however that with perfect information on legal status those authorized workers most similar in skill to the unauthorized stand to gain the most.

When employers cannot easily distinguish the documented from the unauthorized, the potential impacts of such state legislation are complicated by the likelihood that employers form probabilistic assessments of the legal status of specific applicants and then act on those assessments. Specifically, if employers cannot tell

[3] Empirical research to date supports this theory. Orrenius and Zavodny (2009) find evidence that increases in employment enforcement after 9/11 reduced employment, hours and earnings of male Latin American immigrants relative to similar native-born workers, which is consistent with employers substituting legal workers for unauthorized. It is also possible that LAWA had a "chilling effects" and that it induced unauthorized immigrants to leave, or fewer to arrive, for other reasons than current employment outcomes (i.e. shifting the labor supply curve inwards, as opposed to a labor demand induced reduction). This would lead to an upward pressure on wages. Due to data limitations we are unable to draw any conclusions at the present time from our analysis of wages before and after LAWA.

with certainty who is and who is not authorized, employers may infer legal status through such visible signals as ethnicity, accent, or surname. Some employers wishing to reduce the probability of being fined or of having their business licenses revoked may avoid hiring applicants from specific groups altogether (for example, an outright ban against hiring Hispanic applicants). Given the imperfect signal of legal status, such statistical discrimination would likely impact both native and foreign-born applicants authorized to work in the U.S. Of course, such statistical discrimination is most likely to impact Hispanics with skills similar to those of unauthorized workers.

Concern that sanctions targeted towards the hiring of unauthorized immigrants would result in employment discrimination against Hispanics is not new. In fact, this was a key point of contention in the legislative debate surrounding the 1986 IRCA. IRCA created the legal obligation for employers to verify the identity and work eligibility of all applicants, and introduced a series of graduated sanctions for employers who failed to comply with the verification, record-keeping, and hiring provisions of the law. Opponents of the legislation expressed concern that these provisions would lead to discrimination against all Hispanics. Indeed, there is some research finding that in the immediate aftermath of IRCA's passage, the hourly earnings of Latinos did decline (Bansak and Raphael 2001).

A key difference between the mid-1980s and the present, however, is the past absence of an efficient system for verifying identity and work eligibility. During the 1980s, employees were required to present one of a number of pieces of identification to comply with IRCA, many of which were little known documents issued by the Immigration and Naturalization Service (INS). In the current period, the federal E-Verify system is available to employers, which ideally should provide unambiguous and accurate information regarding the work-eligibility of applicants. Several reports, however, suggest that the E-Verify system has its flaws. According to the American Immigration Lawyers Association (2008), the fundamental problem remains the fact that the Department of Homeland Security (DHS) must run the identity data it receives against the Social Security Administration (SSA) database. But the purpose and function of the SSA database was not meant for immigration enforcement. Moran (2007) argues that the SSA database has 17.8 million records that contain discrepancies that could result in a person (immigrant or U.S. citizen) being wrongly identified as not authorized for employment. According to a formal evaluation of the system by Westat (2007), less than 1 percent of natives but almost 10 percent of foreign-born U.S. citizens verified by the federal Web Basic Pilot between October 2006 and March 2007 received an erroneous non-confirmation of work authorization.

The potential for statistical discrimination adds a third partial effect of these state laws that again should vary with the observable characteristics of job applicants. In the event that workers receive a so-called "tentative non-confirmation" of work eligibility, they are given a certain period of time to contest and correct the finding. Note that employers can verify work eligibility only after they have decided to hire the applicant in question. Furthermore, if the new hire chooses to contest the "tentative non-confirmation," the employer is not allowed to change the employment status or work conditions of the employee based on that information while the case is being reviewed. If the new hire chooses not to contest the tentative non-confirmation or DHS and SSA determine that there are no errors in his or her record, the employer then has 8 days to fire the individual. The legal restrictions with respect to E-Verify add uncertainty and increase the expected hiring cost, and if relevant, the training cost. Employers wishing to avoid the risk of hiring workers eventually determined to be unauthorized by E-Verify may reduce their hiring of Latino applicants in particular and increase their hiring of applicants who are deemed lower risk, for whatever reason.

Our theoretical discussion suggests that LAWA should differentially impact the population and employment of different groups of Arizona workers. First, we expect to see the largest declines for sub-groups disproportionately comprised of unauthorized immigrants. For example, non-citizen Hispanic immigrants with low levels of educational attainment are the most likely to be unauthorized and thus are the most likely to be adversely affected by the law. Second, theory predicts that those who are most likely to be confused with the unauthorized, such as naturalized foreign-born Hispanics or native-born Hispanics with less education, may suffer statistical discrimination by employers wishing to minimize the risk of being sanctioned. If discrimination is perceived or actual, these workers may also be more likely to leave Arizona (or for workers who would have moved into Arizona for work, would choose to move elsewhere). Such workers, however, are also likely to benefit from employer substitution away from unauthorized immigrants. Low-skilled, non-Hispanic native-born workers (for example, non-Hispanic white workers) should not be impacted by statistical discrimination and should benefit from substitution away from unauthorized immigrant labor. However, each of these groups would be adversely impacted by any effect of the legislation on the scale of operations of businesses in the state. Since it is impossible to theoretically predict the net effects on employment of the channels discussed here, analyzing the impacts of these laws requires an empirical analysis.

Appendix C. Empirical Method

With the theoretical ordering of likely impacts presented in Appendix B, one approach to estimating the impact of LAWA on labor market outcomes would be to compute the pre-post implementation changes in population and employment rates for each group, and assess whether the relative changes conform to the predictions of theory. For example, our theoretical discussion predicts that the difference between the pre-post change in employment rates for foreign-born non-citizen Hispanics and native-born non-Hispanic whites should be negative (that is to say, the employment rate of the first group should have declined relative to the second).

However, a simple pre-post comparison of outcomes confounds the effects of LAWA with other population and employment trends happening concurrently, notably the Great Recession. To assess whether the pre-post changes are driven by a response to LAWA specifically, 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, states bordering Arizona. An alternative strategy would be to employ a data-driven search for a comparison group based on pre-LAWA population characteristics and trends. We pursue both approaches but begin describing the latter tack. We then discuss validation of the approach from the confounding factor of the Great Recession.

Evaluating LAWA Using the Synthetic Cohort Approach

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,…,J) denote states. The value j=0 corresponds to Arizona and j=(1,…,J) correspond to each of the other J states that are candidate contributors to the control group (or in the terminology of Abadie et. al, the "donor pool"). Define F_0 as a 9x1 vector with elements equal to the proportion of the Arizona population that is foreign-born in years 1998 through 2006 (the nine years we use throughout this paper as our pre-intervention period). Similarly, define the 9xJ matrix F_1 as the collection of comparable time series for each of the J states in the donor pool (with each column corresponding to a separate state-level time series for the period 1998 through 2006).

The synthetic control method identifies a convex combination of the J states in the donor pool that best approximates the pre-intervention time series for the treated state. Define the Jx1 weighting vector $W = (w_1, w_2, ..., w_J)'$ such that $\sum_{j=1}^{J} w_j = 1$, and $w_j \geq 0$ for j=(1,…,J). The product $F_1 W$ then gives a weighted average of the pre-intervention time series 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 the pre-intervention path for Arizona. Specifically, the weighting vector is chosen by solving the constrained quadratic minimization problem

$$W^* = \arg\min_{W} (F_0 - F_1 W)' V (F_0 - F_1 W)$$

$$s.t.$$

$$W' i = 1, w_j \geq 0, j = (1, ...J)$$

where V is a 9x9, 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.

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.

Our principal estimate of the impacts of LAWA on population and employment 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 $Outcome_{pre}^{AZ}$ and $Outcome_{post}^{AZ}$ 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

$$DD_{AZ} = (Outcome_{post}^{AZ} - Outcome_{post}^{synth}) - (Outcome_{pre}^{AZ} - Outcome_{pre}^{synth})$$

To the extent that LAWA induced net migration of the foreign-born out of Arizona, one would expect to find that $DD_{AZ} < 0$ for both population share effects as well as employment effects for the group containing a high proportion of unauthorized immigrants, i.e. non-citizen Hispanics. The employment effects on competing group, as our theoretical discussion makes clear, is ambiguous.

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 displaced in equation (2)[4] 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 $F(.)$, 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 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).

While the results from the synthetic control approach are described in the report text, detailed statistical results are given below.

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[4] Buchmueller, DiNardo and Valletta (2009) 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.

Analyzing Employment Outcomes Using Difference-in-Difference Approaches

To supplement the synthetic control approach, we also estimate traditional difference-in-difference estimates (DD) on individual-level data using the larger ACS sample. The CPS data used in the synthetic cohort approach imposes some sample size challenges, potentially resulting in imprecise estimates. This is particularly relevant for employment outcomes for relatively smaller groups defined by gender and other potentially relevant characteristics. The DD approach using the ACS data also allows us to control for additional sources of variation at the individual level, but requires that we choose the control states or area ourselves rather than letting the data decide. We subject our choice of control states to a number of robustness checks. The DD estimates allow for both comparisons within Arizona/across population groups and within population groups/across states. Our sample for all groups is restricted to those with only a high school diploma or less.

The use of ACS data also allows us to further refine our definition of a group containing a high proportion of unauthorized immigrants. This is due to both additional information available in the ACS (specifically, English proficiency) as well as the larger sample size. In our DD approach we therefore define the group of likely unauthorized immigrants to be working age recently arrived non-citizen immigrants of Hispanic origin with no more than a high school degree and limited English proficiency. We also include results for African-Americans, a relatively small group in Arizona who our theoretical discussion indicates may have experienced employment gains from LAWA.

We first contrast the pre-post change in employment outcomes for different groups within the state of Arizona, the idea being that the theory predicts the sign of the relative change in employment between groups. This provides insights not possible utilizing the synthetic cohort approach. The main strength of this strategy is that the alternative groups in the comparison will be subjected to the same economic forces impacting the state's economy, excepting the differential impact of the legislation of course. The main weakness of this strategy is that each comparison group should be impacted by the legislation through the labor-demand channels discussed in the previous section; that is to say, it is difficult to identify a within-state comparison group that serves to chart out the counterfactual employment path for the likely impacted group while not being impacted by the legislation itself. Hence, it is impossible to identify the absolute effect of the legislation on the employment outcomes of each group with within-state comparison groups (i.e., we can identify relative effects only).

To address this weakness, we also use a series of difference-in-difference estimates using comparison groups from neighboring states. The strength of this approach comes from the fact that the Arizona legislation is considerably less likely to have a direct impact on labor market outcomes for the comparison group. The key weakness of this alternative strategy is that the comparison groups, being residents in different states, may be subject to different temporal economic shocks when compared with workers in Arizona. In addition, if LAWA induces migration away from Arizona and into neighboring states, the across-state effects may be attenuated. Nonetheless, the results of the two strategies presented side-by-side should allow us to triangulate whether LAWA measurably impacted the Arizona labor market.

Within-state Comparisons

Our first DD empirical strategy is to estimate a series of difference-in-difference specifications making a host of within-state comparisons. These involve relative changes in employment and self-employment rates and

among the five lesser-skilled sub-groups defined in Technical Appendix A. The empirical specification for the within state difference-in-difference estimator comparing the group of likely unauthorized immigrants to another low-skilled group can be expressed as:

$$y_{it}^{j} = \alpha + \beta' \mathbf{X}_{it} + \gamma_0 LSG_i^j + \gamma_1 POST_{it} + \gamma_2 LSG_i^j POST_{it} + \varepsilon_{it} \qquad (1)$$

where, y_{it}^{j} is the labor market outcome of interest (i.e., variables indicating employment, self-employment or log hourly wages) for individual i in comparison group *j*, at time *t*, X_{it} is a vector of socioeconomic characteristics, such as age, schooling level, marital status, family composition a well as MSA fixed effects and possibly industry fixed effects, ε_{it} is an error term, and α, β, γ_0, γ_1 and γ_2 are parameters to be estimated. The key parameter in this equation, γ_2, measures the degree to which the pre-post change in the employment outcome observed in Arizona departs from that observed for otherwise similar individuals in the comparison group in Arizona. Theoretically, we expect this coefficient to be negative for those likely to be harmed by the law and positive for those likely to benefit. We also incorporate interaction effects to determine whether the estimated effects are different for females. We define the pre-LAWA period to be the years 2005 and 2006 and let POST dummy represent 2008. LAWA was implemented on January 1, 2008, but debate and passage of the law took place during much of 2007. Including 2007 in either the "pre" or "post" period would be inappropriate since it is unclear whether workers or firms adjusted their behavior in anticipation of LAWA's enactment.

Cross-state Comparisons

None of the possible within-state comparisons are likely to identify comparison groups that are not impacted by LAWA. If the law is achieving its intended impacts, all low-skilled workers regardless of race, ethnicity, or legal status will be impacted. Among the higher-educated workers, employment outcomes may still be impacted by LAWA through establishment scale effects or through skill complementarities with low-skilled unauthorized workers. Also, low-skilled and high-skilled workers are concentrated in different occupations and industries and may not experience business cycle swings equally. Hence, the strategy based on within-state comparisons alone is limited.

To address this weakness, we also compare the changes in employment outcomes for Arizona workers in each of the low-skilled groups identified above with the comparable groups of workers residing in Nevada, New Mexico, Utah as well as inland metropolitan statistical areas of California.[5] Each of these southwestern areas has substantial foreign-born Hispanic populations and is likely to be exposed to similar regional economic forces. While migration from Arizona in response to the legislation may also drive down employment in these neighboring regions, we believe that such a response is likely to be small relative to the total impact in Arizona. Moreover, the direction of the impact of such a migratory response should unambiguously be negative. Hence, the estimated effects on earnings and the probability of employment of the Arizona legislation using these cross-state comparisons will be lower-bound estimates.

Adopting equation (1) to the across-state comparisons is trivial. Calculating the regression-adjusted relative change in employment across states requires replacing the comparison group dummy variable with an Arizona dummy and estimating the equation with the sample restricted to the group of interest only. In the following model, the dummy variable $POST_{it}$ for person *i* in month *t* is equal to one for observations from months following the implementation of the Arizona legislation and the variable AZ_{it} is an indicator variable

[5] We include MSAs: Bakersfield , Fresno, Merced, Modesto, Riverside-San Bernardino , Stockton and Visalia-Tulare-Porterville.

for observations in Arizona. The model is estimated separately for each of the four population groups defined previously. This yields the cross-state within-population difference-in-difference estimate of the impact of LAWA:

$$y_{it} = \alpha + \beta' \mathbf{X}_{it} + \gamma_0 AZ_{it} + \gamma_1 POST_{it} + \gamma_2 AZ_{it} POST_{it} + \varepsilon_{it} \quad (2)$$

The key parameter in equation (2), γ_2, measures the degree to which the pre-post change in the employment probability observed in Arizona departs from that observed for observations in the same group in the comparison states. Theoretically, we expect this coefficient to be negative for those likely to be harmed by the law and positive for those likely to benefit.

Validating the Empirical Approach

In this section we discuss key assumptions our empirical approaches rests on in order to generate causal effects of state level legislation. We also seek to validate our empirical approach with a number of sensitivity and falsification tests which are discussed below. We also refer the reader to the report for additional discussions of the validation of our empirical strategies.

Endogenous timing of legislation

Our empirical strategy requires that the enactment of LAWA 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, LAWA passed while the economy was growing but was enacted at a time of declining labor market conditions in Arizona.

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 its neighboring states. To start, LAWA represents the ultimate manifestation of a fairly lengthy legislative debate that crossed multiple legislation sessions. Referenda addressing employment of unauthorized migrants were included on the 2004 and 2006 ballots, suggesting strong latent demand for legislation addressing immigration even during good economic times. Moreover, several bills were considered by the state legislature at least as early as 2004. In 2005 and 2006 then-Governor Janet Napolitano vetoed two immigration-related bills, arguably for being too weak on employer sanctions for hiring unauthorized workers. Thus, the final legislation passed in 2007 certainly evolved from a long multi-year debate within the state. There is little evidence suggesting that LAWA was a reaction to eroding labor market conditions.

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 (Arizona Republic 2007).

Suitable comparison area in the DD approach

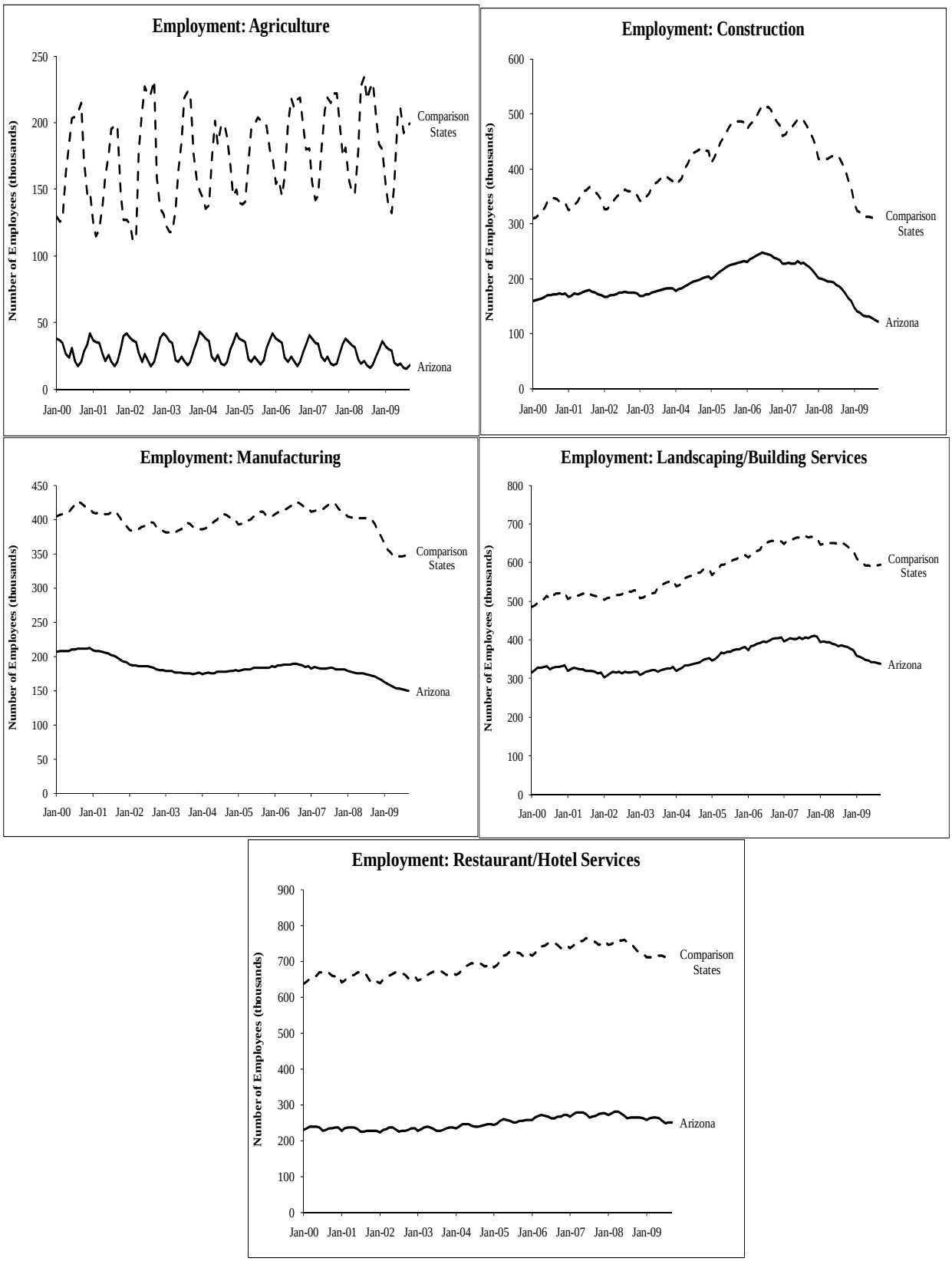
We utilize Nevada, New Mexico, Utah and inland areas of California to represent the counterfactual area of what would happen to the employment outcomes in Arizona had LAWA not been implemented. This requires that these states experienced similar labor markets and population trends as Arizona prior to LAWA was implemented. Table C1 indicates that the population trends in these two areas were quite similar in the pre-LAWA period. Firm survey data from the Quarterly Census of Employment and Wages (QCEW) also show similar overall employment trends prior to 2007, as well as by industries of particular importance to unauthorized workers (Figure C1).

TABLE C1
Population and growth in Arizona and comparison states, 2000–2008

Arizona								
Year	Total		Immigrants					
			All		Non-citizen Hispanics		Likely unauthorized	
2000	5,133,711		707,965		362,280		109,055	
2005	5,806,266	13.1%	911,967	28.8%	483,244	33.4%	224,656	106.0%
2006	6,166,318	6.2%	986,131	8.1%	530,692	9.8%	253,286	12.7%
2007	6,338,755	2.8%	1,050,860	6.6%	564,340	6.3%	273,097	7.8%
2008	6,500,180	2.5%	1,002,546	-4.6%	517,212	-8.4%	229,118	-16.1%
Inland California								
Year	Total		Immigrants					
			All		Non-citizen Hispanics		Likely unauthorized	
2000	6,419,281		1,288,618		654,073		123,742	
2005	7,343,683	14.4%	1,658,073	28.7%	840,197	28.5%	266,594	115.4%
2006	7,695,490	4.8%	1,752,132	5.7%	898,735	7.0%	292,518	9.7%
2007	7,767,479	0.9%	1,780,255	1.6%	905,043	0.7%	324,731	11.0%
2008	7,831,064	0.8%	1,756,193	-1.4%	874,970	-3.3%	310,531	-4.4%
Nevada, New Mexico, and Utah								
Year	Total		Immigrants					
			All		Non-citizen Hispanics		Likely Unauthorized	
2000	6,049,756		677,564		292,530		80,606	
2005	6,714,955	11.0%	822,435	21.4%	365,811	25.1%	155,633	93.1%
2006	7,000,191	4.2%	942,459	14.6%	446,936	22.2%	189,036	21.5%
2007	7,180,627	2.6%	965,041	2.4%	429,787	-3.8%	183,179	-3.1%
2008	7,320,947	2.0%	978,965	1.4%	442,002	2.8%	183,049	-0.1%

SOURCE: Authors' calculations from the 2000 Census and 2005–2008 ACS.

FIGURE C1
Employment in Arizona and comparison states by industry, 2000–2009



SOURCE: Authors' calculations from the Quarterly Census of Employment and Wages data.

NOTE: Figures include only employment in Private industry.

Lastly, in the empirical section, we perform a number of sensitivity checks to the definition of the states included in the comparison area and find no evidence that the conclusions in the report hinge on inclusion of any one of these states, or whether another important immigrant state, Texas, is included.[6]

Endogenous timing of enforcement

Aside from the possible simultaneous determination of employment outcomes and the passage of state laws, an additional threat to the internal validity of our research strategy concerns the possibility that the passage and implementation of LAWA may coincide with an increase in federal enforcement efforts pertaining to the employment of unauthorized migrants. Both worksite raids by Immigration and Customs Enforcement (ICE), as well as border enforcement activity controlled by Customs and Border Patrol (CBP)[7] have increased since 9/11. If these are centered disproportionately on Arizona and increased around the time LAWA was enacted, some of the change in population and employment we observe could be explained by federal enforcement. This could operate through a deterrent effect—unauthorized immigrants choosing not to come to Arizona—or directly through increased detention or deportation.

DHS enforcement activities include apprehension of unauthorized immigrants at the border and internally, investigations of worksites, and a variety of criminal investigations. Border control comprises by far the largest enforcement effort in terms of budgetary support, infrastructure, and manpower. For example, the number of arrests at the border in 2008 was over 100 times larger than the number of worksite arrests.[8] Although smaller-scale worksite enforcement efforts could have substantial chilling effects, large population effects are more likely to come from the significantly larger efforts at the border. So we start by examining border enforcement efforts in Arizona more closely.

There has been an increase in border enforcement since IRCA, but more recently, increases following 9/11. Arizona is one of the key southwest border states, so there has been consistent buildup of efforts in the state. For the purposes of the effects we estimate in this study, we are concerned only about differential enforcement in Arizona compared to other states. Our review of DHS border activity suggests there was not a significant buildup specifically in Arizona at precisely the time of LAWA. DHS began a strong initiative focused on the Arizona border, the "Arizona Border Control Initiative", in 2004, and this continued through 2006. However, this effort predates LAWA. Unless the impacts have a significant lag, the ABC initiative does not cause great concern here (and may even lead to an understatement of LAWA's impact).

Data on federal enforcement provides only some window into potential impacts on the unauthorized population in Arizona. Because DHS statistics record the number of arrests, deportations, or the like, they reflect not only changes in enforcement effort but also changes in the choices or activities of unauthorized immigrants. Regardless, we examine official enforcement statistics for any clues about differential trends in Arizona. First, after 2007, there were declines in border apprehensions across the board. Declines in the Tucson sector were in line with declines elsewhere. This is not only the most active portion of the border in Arizona, but also across the entire southwest. The Tucson sector apprehensions comprised between 35 to 43% of all border arrests since 2000. There appears to be no differential trend in line with the enactment of

[6] We are also in the process of implementing a synthetic cohort strategy for state level outcomes based on the Abadie, Diamond and Hainmueller (2009) approach.
[7] Both agencies operate within the Department of Homeland Security.
[8] This and the following statistics in this section are calculated from DHS official reports (Office of Immigration Statistics, 2010).

LAWA. Apprehensions in the Yuma sector fell precipitously, but continued on a downward trend started in 2006 and thus not likely due to spike in enforcement efforts coincident with LAWA.

In terms of investigation efforts through DHS, there is a clear decline in the number of arrests in Arizona. Whereas there were 10,526 arrests in 2006, there were only 904 and 636 in 2008 and 2009, respectively. If enforcement efforts were increasing we would not expect to see such a large drop.

In addition, the enforcement actions by counties within the state unrelated to LAWA may coincide with its implementation. In particular, the actions of Maricopa County Sheriff Arpaio may threaten the validity of our identification. While we cannot control for this within-Arizona issue in the synthetic control approach, we conduct a few tests in the DD method. In particular, we utilize geographic fixed effects to control for part of this issue, and conduct tests excluding Maricopa County from our analysis.

Appendix D. Empirical Results from Synthetic Control Method

Estimates of Population Effects

We begin with a graphical presentation of the Arizona population trends and the comparable population trends in synthetic Arizona for our three outcomes. Figure 5 in the report presents the proportion of each population that is foreign-born, that is non-citizen, and that is Hispanic non-citizen. Focusing first on the pre-intervention period 1998 through 2006, the figure reveals 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 (.00197 for the proportion foreign-born, .00367 for the proportion non-citizen, and .00438 for the proportion non-citizen Hispanic). Hence, the synthetic control groups match the pre-intervention values for Arizona quite well for each of the outcomes.

As background, Table D1 displays the states receiving positive weights in the construction of synthetic Arizona for our three 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. For the proportion foreign-born, seven states receive positive weights, with much weight going to traditional immigrant receiving states with relatively little weight on neighboring southwestern states. The solution weighting vector for the proportion non-citizen places positive weight on five states, with considerably weight placed on California (almost half). When we focus on Hispanic non-citizens the lion's share of weight is placed on California (0.747) with the remaining weight roughly split between North Carolina and Maryland.

TABLE D1
States receiving positive weights for the synthetic control groups

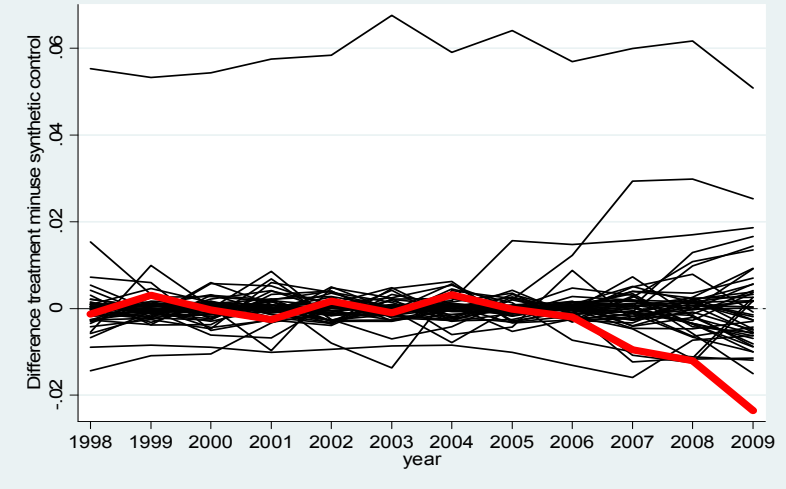
Proportion foreign-born		Proportion non-citizen		Proportion Hispanic non-citizen	
Alaska	0.091	California	0.441	California	0.747
California	0.161	DC	0.028	Maryland	0.122
Hawaii	0.197	New Jersey	0.118	North Carolina	0.131
Idaho	0.034	North Carolina	0.171	–	–
New York	0.225	Washington	0.242	–	–
Oregon	0.240	–	– – –		
Washington	0.145	–	– – –		

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

Regarding the post-intervention period, for each of the outcomes we observe sizable gaps (on the order of one to 2.5 percentage points) between Arizona and the synthetic control groups. For the foreign-born outcome, the gap begins to open up in 2007 and widens in each year thereafter. For the proportion non-citizen and the proportion non-citizen Hispanic, 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.

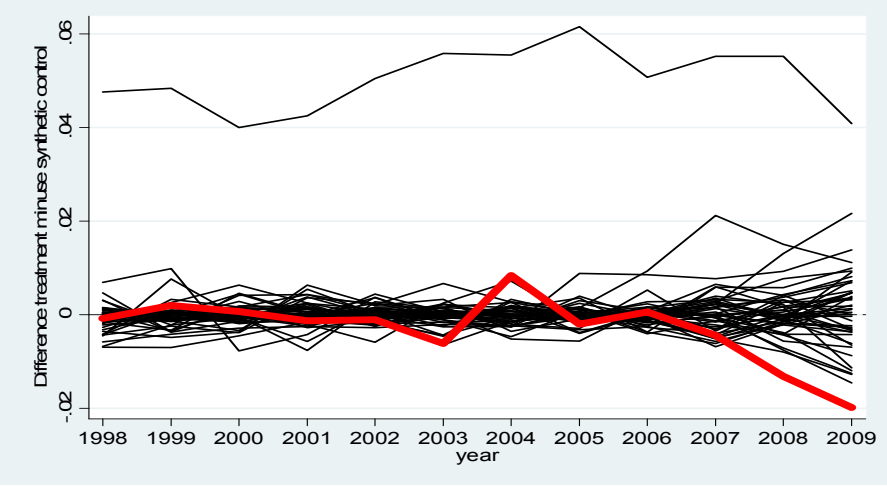
Figures D1 through D3 graphically display the raw data needed to conduct the permutation test of the significance of the relative declines in Arizona. Specifically, for each of the 46 donor states as well as for Arizona, the figures display the year-by-year difference between the outcome variable for 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.

FIGURE D1
Difference in the proportion foreign-born relative to the synthetic control group, all states (Arizona displayed with red line)



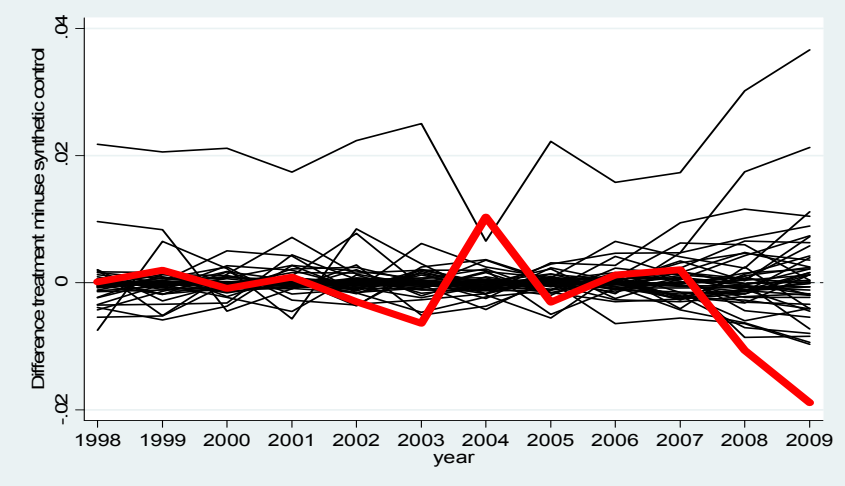
SOURCE: Authors' calculations from CPS, using synthetic control method.

FIGURE D2
Difference in the proportion non-citizen immigrant relative to the synthetic control group, all states (Arizona displayed with red line)



SOURCE: Authors' calculations from CPS, using synthetic control method.

FIGURE D3
Difference in the proportion non-citizen Hispanic immigrant relative to the synthetic control group, all states (Arizona displayed with red line)



SOURCE: Authors' calculations from CPS, using synthetic control method.

Second, for the post-intervention years when 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 values for non-citizens and Hispanic non-citizens.

Table D2 presents estimates of the difference-in-differences estimator laid out in equation (2) of Appendix C. For each outcome, the first column presents the mean difference between Arizona and the synthetic control for all years in the interval 1998 through 2006. The second column presents the comparable average difference for 2008 and 2009 while the third column presents the difference-in-difference. The fourth column presents the ranking of Arizona's difference-in-difference estimate (with states ranked from lowest to highest) in the distribution, created by combining the 46 placebo estimates for donor pool states with the estimate for Arizona. The final column presents the P-value from the one-tailed test of the null hypothesis that the relative change for Arizona is non-negative against the alternative that the difference-in-difference is negative. Note, this p-value is bounded from below by 0.021 (1/47).

The results in panel A show the estimates based on the entire resident population. For all three outcomes, the average difference relative to synthetic Arizona is basically zero in the pre-intervention period. For the proportion foreign-born, there is a relative decline for Arizona of 1.8 percentage points. Arizona's difference-in-difference estimate is the most negative, yielding the minimum P-value of 0.021. The outcome for the proportion non-citizen shows a difference-in-difference estimate of 1.7 percentage points. Hence, the overwhelming share of the decline in the foreign-born is driven by declines in the population of non-citizens. Again, the estimate for Arizona has the most negative value relative to the distribution of placebo estimates. Turning to the estimates for non-citizen Hispanics, the difference-in-differences estimate suggests a 1.5 percentage point decline in the proportion of Arizona residents that fall into this category. Again, the Arizona estimate is the most negative.

TABLE D2
Estimated impact of the passage and introduction of LAWA on various subsets of the foreign-born population of Arizona

	Average diff. relative to synthetic cohort, 9 pre-intervention years	Average diff. relative to synthetic cohort, 2008 and 2009	Change, post minus pre (difference-in-difference estimate)	Rank (lowest to highest)	P-value from one-tailed test, $P(\Delta<\Delta_{AZ})$
Panel A: As a proportion of all Arizona residents					
Foreign-born	0.000	-0.018	-0.018	1/47	0.021
Foreign-born Hispanic	0.000	-0.021	-0.021	1/47	0.021
Non-citizen Hispanic	0.000	-0.015	-0.015	1/47	0.021
Panel B: As a proportion of employed Arizona Residents					
Foreign-born	0.000	-0.026	-0.026	1/47	0.021
Foreign-born Hispanic	0.000	-0.025	-0.025	1/47	0.021
Non-citizen Hispanic	0.000	-0.018	-0.018	1/47	0.021

SOURCE: Authors' calculations from CPS data using synthetic control approach.

NOTE: Average differences pre and post-intervention are estimates of the difference in the proportion of the Arizona population in the 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.

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 bodies, Arizona's population in 2006 stood at approximately 6.2 million. These estimates suggest a relative population loss of between 93,000 and 112,000.

Panel B of Table D2 presents comparable estimation results where the population is restricted to Arizona residents employed at a wage and salary job. Here relative declines in the foreign-born population can be driven either by residential mobility or a pre-post LAWA increase in the degree of difficulty experienced by foreign-born workers when looking for work. The patterns in Panel B are basically comparable to the results based on the entire resident population. The proportion foreign-born among the employed declines by 2.6 percentage points in Arizona relative to synthetic Arizona. The comparable estimate for non-citizens is 1.9 percentage points, while the estimate for non-citizen Hispanics is 1.8 percentage points. Again, the relative declines for Arizona are at the bottom of the distribution of placebo estimates for all of the states in the donor pool. Note, the estimated impacts on the relative representation of the foreign-born (and the sub-populations therein) among the employed are larger than the corresponding estimates for the entire population. This may be driven by either a dis-employment effect of LAWA that reduces the representation of the foreign-born beyond the impact of net migration, or perhaps a differential migration effect for foreign-born residents of working age. In the next section, we explore this in greater detail.

To summarize the results, we find pre-post LAWA declines in the representation of the foreign-born among the Arizona resident population. Corresponding declines do not occur in the synthetic control group. Most of the decline is concentrated among non-citizens and non-citizen Hispanics. Moreover, we observe a decline in immigrant representation among those with jobs that exceeds the comparable decline among the state's resident population more generally. 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.

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 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. Second, 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. Finally, we assess whether the impact of LAWA on population movements varies by sub-groups of the immigrant population defined by age and gender.

Some Specification Checks

We begin by exploring the sensitivity of the estimates to the definition of the post-treatment period. In Table D2, 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.

Panel A of Table D3 presents comparable estimates to those in Panel A of Table D2, but that include 2007 in the post-treatment period. Here we focus only on the results for all Arizona residents as we will explore age heterogeneity in greater detail below. The relative population declines for Arizona including the 2007 population are somewhat smaller (by 0.3 percentage points for the foreign-born outcome, by 0.9 percentage points for the non-citizen outcome, and by 0.6 percentage points for the non-citizen Hispanic outcome). However, it is still the case that the declines for Arizona are the largest when compared to the distribution of placebo estimates across the 46 potential donor states.

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 did 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.

TABLE D3
Alternative estimated impacts under different scenarios

	Average diff. relative to synthetic control, pre-intervention	Average diff. relative to synthetic cohort, post-intervention[a]	Change, post minus pre (difference-in-difference estimate)	Rank, lowest to highest	P-value from one-tailed test, $P(\Delta<\Delta_{AZ})$
Panel A: Including 2007 as a post-treatment Year					
Foreign-born	0.000	-0.015	-0.015	1/47	0.021
Non-citizen	0.000	-0.012	-0.013	1/47	0.021
Non-citizen Hispanic	0.000	-0.009	-0.009	1/47	0.021
Panel B: Dropping states that border Arizona from the donor pool					
Foreign-born	0.000	-0.019	-0.019	1/43	0.023
Non-citizen	0.002	-0.014	-0.016	1/43	0.023
Non-citizen Hispanic	0.008	-0.014	-0.022	1/43	0.023

SOURCE: Authors' calculations from CPS data using synthetic control approach.

NOTE: Average differences pre and post-intervention are estimates of the difference in the proportion of the Arizona population in the 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 the remaining states in the donor pool.

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 of the proportion of the population using the period definitions employed in Table D2 shows an absolute decline in the proportion of Arizona residents that are foreign-born of 1.4 percentage points (compared with our difference-in-difference estimate of 1.8 percentage points).[9] The comparable absolute declines for foreign-born Hispanic and non-citizen Hispanics are 1.6 and 1.3 percentage points, respectively. Hence, the relative declines that we estimate in Table D2 are driven primarily by compositional changes in Arizona rather than by 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 foreign-born declined by 1.8 percentage points. Relative to 2007, this corresponds to a LAWA-induced absolute population loss of roughly 112,000. Suppose that the entire 112,000 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 are foreign-born from the actual value in 2007 of 0.283 to the hypothetical value of 0.286. 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.

[9] Details available upon request.

Finally, when we restrict the donor pool to states that do not share a border with Arizona,[10] the difference-in-difference estimates as well as the statistical inferences are quite similar to our main estimates in Table D2. 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 TableD3. 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 D2). Moreover, the observed DD estimates for Arizona are still more negative than each of the remaining 43 placebo estimates for all three of our outcome variables.

Testing for Effects of LAWA on Alternative Population and Population-related Outcomes

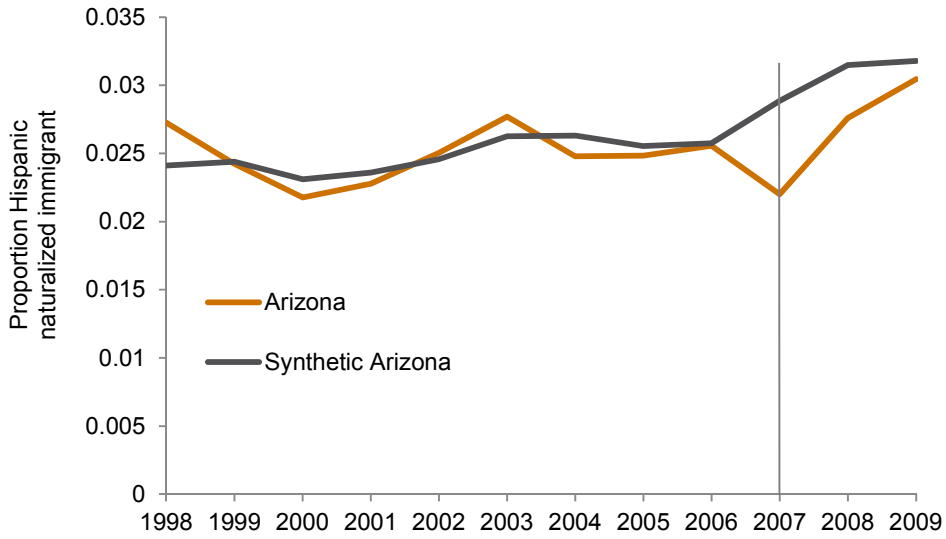
The enhanced employment verification requirements of LAWA are targeted specifically at foreign-born job seekers that are unauthorized to work in the United States. Thus, to the extent that there is a migratory response to the legislation, one would expect the largest population impact on groups with high proportions of 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. 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 document below, 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 D4 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 D5 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.

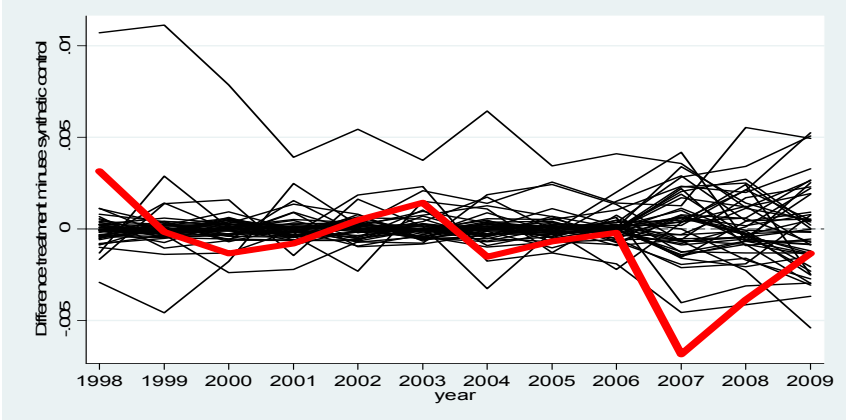
[10] Throughout the analysis we have been omitting Utah from the donor pool due to the presence of comparable (yet not identical) state legislation. In the tabulations in Table 4 Panel B we further drop California, Colorado, Nevada, and New Mexico from the potential donor pool.

FIGURE D4
Proportion Hispanic naturalized immigrant in Arizona and the synthetic control group, 1998–2009



SOURCE: Authors' calculations from CPS data using synthetic control method.

FIGURE D5
Difference in the proportion naturalized Hispanic immigrant relative to the synthetic control group, all states (Arizona displayed with red line)



SOURCE: Authors' calculations from CPS data using synthetic control method.

The first row of Table D4 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 D4 and D5, there appears to be little evidence that naturalized Hispanics responded to LAWA by migrating from the state.

TABLE D4
Estimated impact of the passage and introduction of LAWA on Hispanic naturalized citizens and on housing outcomes

	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

SOURCE: Authors' calculations from CPS data using synthetic control method.

NOTE: 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.

Turning to 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 non-citizen, 53 percent rent, while the comparable figure among households headed by a Hispanic non-citizen 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 non-citizen and non-citizen Hispanic households are 17 and 14 percent, respectively.

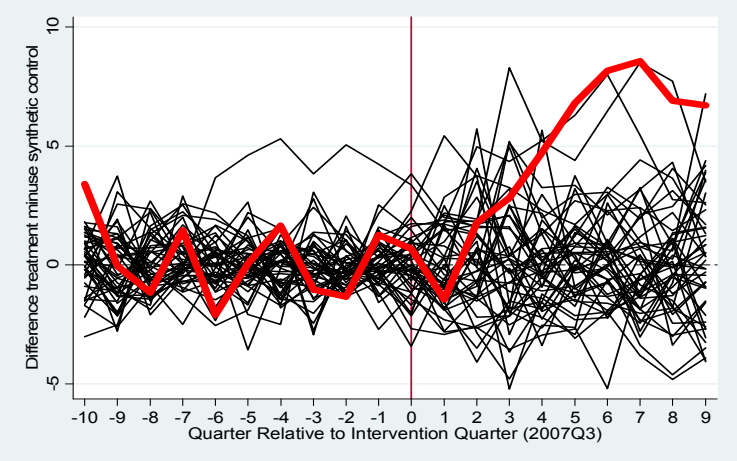
Given the relative concentration of immigrants in rental housing, population losses on the order implied by our difference-in-difference estimates in Table D2 should disproportionately impact the Arizona rental market. Here we asses this proposition by testing for pre-post LAWA changes in the rental 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 the third quarter 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 first quarter values, the three second quarter 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 LAWA reduced the proportion of the Arizona population that is foreign-born by 0.018. If we assume that this translates into a 1.8 percentage point decline in the number of Arizona households[11] and that the entirety of this decline occurs among rental households, then the rental vacancy rate should increase by 6.04 percentage points ([1.8/29.8]x100).

Figure 7 in the report 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 D6 and D7 here present comparable graphs for rental and owner-occupied vacancy rates. In Figure D7 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 is not indicative of an impact of LAWA on this variable.

FIGURE D6
Difference in rental vacancy rates relative to that of the synthetic control group, all states (Arizona displayed with red line)

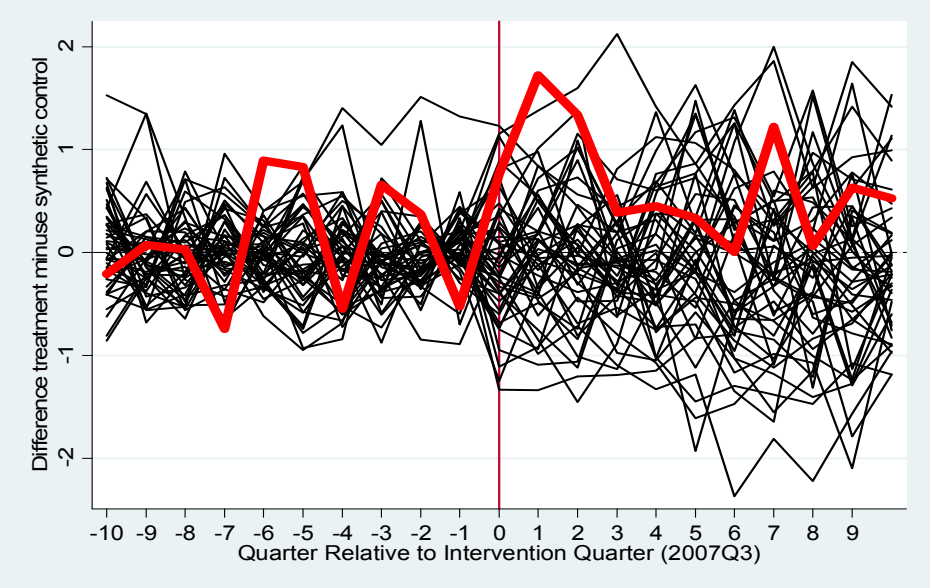


SOURCE: Authors calculations from CPS-HVS data using synthetic control method.

NOTE: Intervention date is 2007 3rd quarter.

[11] A decline in the foreign-born population would impact both the numerator as well as the denominator of the ratio use to calculate the proportion foreign-born, and thus a decline in the proportion foreign-born of 0.018 implies a slightly smaller percentage population loss. However, to a first approximation assuming a 1.8 percentage point decline is reasonable. Moreover, we are applying the population change to changes in the number of households. 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.

FIGURE D7
Difference in owner-occupied housing vacancy rates relative to that of the synthetic control group, all states (Arizona displayed with red line)



SOURCE: Authors' calculations from CPS-HVS data using synthetic control method.

NOTE: Intervention dater is 2007 3rd quarter.

The last two rows of Table D4 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 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.

Testing for Heterogeneity in the Population Response by Age and Gender

Our final set of results assesses whether the migratory responses to LAWA vary within subsets of the foreign-born population defined by age and gender. There are several reasons that support an a priori expectation for heterogeneity in the impact of the law. First, the fact that LAWA does not apply retroactively

to all past hires suggests that those in relatively stable employment relationships may be less impacted by the law's enhanced verification requirements. Hence, one might expect greater stability and less of an impact on the relative representation of immigrants among relatively older Arizona residents. Second, children under 16 years of age generally do not work, and hence are not directly impacted by the law. Nonetheless, foreign-born children may be forced to migrate with parents who now face greater difficulty in finding employment as a result of LAWA. Finally, there is a fair degree of gender imbalance in the immigrant population, especially among more recent immigrants from Latin America (Raphael 2010). To the extent that male immigrants are more recent, more likely to be undocumented or perhaps more salient in that they are more likely to seek formal employment, one might expect differential impacts by gender.

Table D5 presents the results from difference-in-difference estimates of the effect of LAWA on the relative proportion in each immigrant category for three age groups: those under 16 years of age, those 16 to 45 years of age, and those 46 years of age and older. Beginning with children, for all three outcomes we observe declines in the proportion immigrant, with most of the decline being concentrated among children that are Hispanic non-citizens. The difference-in-difference estimates range from 1.4 to 2 percentage points and in each instance, the value for Arizona is less than all of the placebo estimates for the remaining states. The results for immigrants in the prime working age range (16 to 45) are similar yet somewhat larger than the results for children. Point estimates range from relative decline of 2.7 to 2.8 percentage points. Again, the difference-in-difference estimates for Arizona are in the far left tail of the distribution of placebo estimates. We find no evidence of an impact of LAWA on the proportion foreign-born, the proportion non-citizen, nor the proportion Hispanic non-citizen among Arizona residents 46 years and older.

TABLE D5
Estimated impact of the passage and introduction of LAWA on foreign-born population by age group

	Average diff relative to synthetic cohort, 9 pre-intervention years	Average diff relative to synthetic cohort, 2008 and 2009	Change, post minus pre (difference-in-difference estimate)	Rank, lowest to highest	P-value from one-tailed test, $P(\Delta<\Delta_{AZ})$
Panel A: Population under 16					
Foreign-born	0.001	-0.018	-0.019	1/47	0.021
Non-citizen	0.001	-0.019	-0.020	1/47	0.021
Non-citizen Hispanic	0.006	-0.008	-0.014	1/47	0.021
Panel B: Population 16 to 45					
Foreign-born	0.000	-0.027	-0.027	1/47	0.021
Non-citizen	0.000	-0.027	-0.027	1/47	0.021
Non-citizen Hispanic	0.000	-0.028	-0.028	1/47	0.021
Panel C: Population 46 and older					
Foreign-born	0.000	0.000	0.000	30/47	0.638
Non-citizen	0.000	-0.005	-0.005	10/47	0.212
Non-citizen Hispanic	0.000	-0.001	-0.001	11/47	0.234

SOURCE: Authors' calculations from CPS using synthetic control method.

NOTE: Average differences pre and post-intervention are estimates of the difference in the proportion of the Arizona population in the 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.

Finally, Table D6 focuses on the 16 to 45 year age range and presents separate estimates by gender. For the proportion foreign-born outcome, there is some evidence that the law had a larger impact on the representation of immigrants among men in Arizona relative to women, with the male difference-in-difference estimate exceeding the female estimate by one percentage point (in absolute value). However, the estimates for the proportion non-citizen and the proportion non-citizen Hispanic are essentially identical.

TABLE D6
Estimated impact of the passage and introduction of LAWA on foreign-born population, by gender

	Average diff. relative to synthetic cohort, 9 pre-intervention years	Average diff. relative to synthetic cohort, 2008 and 2009	Change, post minus pre (difference-in-difference estimate)	Rank, lowest to highest	P-value from one-tailed test, $P(\Delta<\Delta_{AZ})$
Panel A: Males 14 to 65 years of age					
Foreign-born	0.000	-0.035	-0.035	2/47	0.043
Non-citizen	0.001	-0.026	-0.026	1/47	0.021
Non-citizen Hispanic	0.000	-0.021	-0.022	2/47	0.043
Panel B: Female 14 to 65 years of age					
Foreign-born	0.000	-0.025	-0.025	1/47	0.021
Non-citizen	0.000	-0.026	-0.026	1/47	0.021
Non-citizen Hispanic	0.001	-0.021	-0.023	1/47	0.021

SOURCE: Authors' calculations from CPS using synthetic control method.

NOTE: Average differences pre- and post-intervention are estimates of the difference in the proportion of the Arizona population in the 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.

Estimates of Employment Effects

We next briefly present the full set of estimates for the employment analysis. This includes the estimated effects for the entire groups of interest, without education restrictions as well as separate results for low-skilled men and women. Given that the highest proportion of unauthorized immigrants is among low-skilled Hispanic non-citizen men (Passel and Cohn, 2009), we expect that if LAWA had any effects on employment, this is the group for whom it is most likely to be revealed. As Tables D7 and D8 show, this is indeed the only group for whom we find any evidence of a statistically significant LAWA effect.

TABLE D7
Estimated impact of the passage and introduction of LAWA: wage/salary employment

	Pre-average difference relative to synthetic cohort	Post-average difference relative to synthetic cohort	Change, post minus pre (difference-in-difference estimate)	Rank, difference-in-difference estimate	P-value from one-tailed test, P(|Δ|<|ΔAZ|)
Panel A: Hispanic non-citizens					
All	-0.0033	-0.1081	-0.1048*	43/45	0.067
High school or less	-0.0036	-0.0660	-0.0623	34/40	0.175
High school or less, men	-0.0009	-0.1151	-0.1142**	40/40	0.025
High school or less, women	-0.0138	-0.0294	-0.0156	24/40	0.425
Panel B: Hispanic citizens					
All	-0.0061	-0.0074	-0.0013	22/44	0.523
High school or less	-0.0104	0.0273	0.0377	21/35	0.429
High school or less, men	-0.0129	-0.0755	-0.0626	28/38	0.289
High school or less, women	-0.0042	0.0945	0.0987	22/35	0.400
Panel C: Hispanic natives					
All	0.0002	0.0229	0.0227	23/45	0.511
High school or less	0.0009	0.0513	0.0504	24/45	0.489
High school or less, men	-0.0001	0.0106	0.0107	23/45	0.511
High school or less, women	-0.0027	0.0054	0.0080	21/44	0.455
Panel D: Non-Hispanic white natives					
All	-0.0006	-0.0032	-0.0027	23/45	0.511
High school or less	0.0002	-0.0154	-0.0156	33/45	0.289
High school or less, men	0.0022	-0.0335	-0.0357	37/45	0.200
High school or less, women	0.0002	0.0142	0.0139	36/45	0.222

SOURCE: Authors' calculations from CPS using synthetic control method.

NOTE: Average differences pre- and post-intervention are estimates of the difference in the proportion of the Arizona population in the 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 the remaining states in the donor pool.

TABLE D8
Estimated impact of the passage and introduction of LAWA: self-employment

	Pre-average difference relative to synthetic cohort	Post-average difference relative to synthetic cohort	Change, post minus pre (difference-in-difference estimate)	Rank, difference-in-difference estimate	P-value from one-tailed test, P(Δ<ΔAZ| ΔAZ<0) or P(Δ>ΔAZ| ΔAZ>0)
Panel A: Hispanic non-citizens					
All	0.0002	0.0423	0.0421	41/45	0.111
High school or less	0.0000	0.0305	0.0305	32/40	0.225
High school or less, men	0.0002	0.0836	0.0834*	39/40	0.050
High school or less, women	0.0004	0.0012	0.0008	21/40	0.500
Panel B: Hispanic citizens					
All	0.0002	0.0200	0.0198	34/44	0.250
High school or less	0.0005	0.0235	0.0230	22/35	0.400
High school or less, men	0.0057	0.0849	0.0792	32/38	0.184
High school or less, women	-0.0001	-0.0327	-0.0326	23/35	0.371
Panel C: Hispanic natives					
All	-0.0001	-0.0072	-0.0072	32/45	0.311
High school or less	-0.0001	-0.0094	-0.0093	29/45	0.378
High school or less, men	0.0001	0.0002	0.0001	25/45	0.467
High school or less, women	-0.0006	-0.0088	-0.0082	25/44	0.477
Panel D: Non-Hispanic White Natives					
All	-0.0001	-0.0075	-0.0074	38/45	0.178
High school or less	-0.0003	-0.0117	-0.0115	41/45	0.111
High school or less, men	-0.0009	-0.0072	-0.0063	34/45	0.267
High school or less, women	-0.0004	-0.0067	-0.0063	31/45	0.333

SOURCE: Authors' calculations from CPS using synthetic control method.

NOTE: Average differences pre and post-intervention are estimates of the difference in the proportion of the Arizona population in the given category relative to the matched synthetic comparison group. The rank of the magnitude of difference-in-difference estimates is from smallest to largest for negative estimates and from largest to smallest for positive estimates. 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 the remaining states in the donor pool.

Appendix E. Empirical Results from Difference-in-Difference Method

In this appendix we present the difference-in-difference results of our employment analysis, relying on ACS data but restricting the analysis to the states bordering Arizona.

Employment Rates

We begin by presenting the across-states DD estimates of the impact of LAWA on employment rates (defined as employed in either wage and salary work or self-employment) for our five less-skilled population groups in Arizona and comparison states. Table E1 shows the full regression results using the DD method across Arizona and comparison states, with the key interaction terms in bold. The results suggest that low-skilled workers in Arizona experienced mostly similar changes between 2005–2006 and 2008 than those of low-skilled workers in the comparison states. The exception is native-born Hispanic men, who saw a slightly greater increase in the employment rate in Arizona compared to those in the neighboring area. The estimates point towards a 1.5 percentage point lower employment rate among our group of likely unauthorized immigrants, but the estimate is not statistically significant.

TABLE E1
Linear probability models of employment, Arizona and comparison states

Identification strategy	Across states, within population groups				
	Hispanic			Non-Hispanic native	
	Likely unauthorized	Naturalized immigrant	Native	White	Black
Arizona*ACS 2008	**-0.015**	**-0.026**	**0.051**	**0.003**	**0.031**
	(0.56)	**(0.68)**	**(2.18)**	**(0.24)**	**(0.65)**
Female*Arizona*ACS 2008	**0.0002**	**-0.012**	**-0.030**	**0.003**	**0.045**
	(0.001)	**(0.23)**	**(0.88)**	**(0.19)**	**(0.60)**
Arizona	0.022	-0.003	-0.010	-0.055	0.095
	(0.50)	(0.05)	(0.46)	(2.90)	(1.27)
ACS 2008	-0.028	0.017	-0.034	-0.026	-0.018
	(1.80)	(0.87)	(3.30)	(3.40)	(0.66)
Female	-0.499	-0.294	-0.132	-0.156	-0.032
	(31.99)	(14.37)	(14.72)	(18.77)	(1.15)
Female*Arizona	-0.017	-0.015	0.027	0.016	-0.032
	(0.61)	(0.43)	(1.55)	(1.13)	(0.67)
Female*ACS 2008	0.072	0.088	0.009	0.037	-0.004
	(3.72)	(3.34)	(0.60)	(3.18)	(0.10)
Age	0.041	0.053	0.053	0.039	0.052
	(11.94)	(10.75)	(23.29)	(26.52)	(13.84)
Age squared/100	-0.052	-0.064	-0.069	-0.052	-0.069
	(11.37)	(10.44)	(22.19)	(26.41)	(13.78)
High school graduate	0.035	0.085	0.205	0.205	0.229
	(3.42)	(6.46)	(22.66)	(33.26)	(11.86)
Married	-0.058	-0.033	0.062	0.056	0.097

continued

Identification strategy	Across states, within population groups				
	Hispanic			Non-Hispanic native	
	Likely unauthorized	Naturalized immigrant	Native	White	Black
	(5.40)	(2.52)	(7.51)	(9.43)	(4.36)
Number of children	-0.001	-0.003	-0.002	-0.012	0.006
	(0.09)	(0.53)	(0.30)	(2.78)	(0.47)
No. of children younger than 5	-0.036	-0.002	-0.005	-0.038	-0.066
	(3.65)	(0.13)	(0.59)	(7.33)	(3.44)
Children of school age	-0.009	0.004	0.001	0.025	0.016
	(0.53)	(0.25)	(0.07)	(2.76)	(0.50)
Years since migration	0.015	-0.002	-0.005	-0.007	0.011
	(2.88)	(0.75)	(0.89)	(0.80)	(0.85)
Years since migration^2/100	-0.109	0.001	0.011	0.018	-0.020
	(2.82)	(0.32)	(0.80)	(0.93)	(0.51)
Speaks only English		0.039	-0.012	0.274	0.223
		(0.99)	(0.26)	(1.36)	(1.68)
Speaks very well		0.128	-0.023	0.249	0.213
		(4.51)	(0.52)	(1.22)	(1.47)
Speaks English well	0.022	0.136	-0.051	0.239	0.101
	(1.44)	(4.90)	(1.12)	(1.17)	(0.54)
Speaks English but not well	0.036	0.071	-0.006	0.195	
	(3.06)	(2.98)	(0.11)	(0.96)	
R-Squared	0.298	0.156	0.179	0.135	0.191
Number of observations	11,799	8,089	30,934	64,370	4,795

NOTES: Comparison area consists of Nevada, New Mexico, Utah and Southern inland areas of California (Bakersfield, Fresno, Merced, Modesto, Riverside-San Bernardino, Stockton and Visalia-Tulare-Porterville). Estimates are based on 2005, 2006 and 2008 ACS. The t-statistics in parentheses are based on robust standard errors clustered around PUMAs. All models include MSA fixed effects.

The within-Arizona, across-groups differences in the post-pre LAWA changes in the employment rates indicate that LAWA reduced the employment rates of individuals in the likely unauthorized group by 4 percentage points when compared to low-skilled naturalized and native-born Hispanic men and native-born whites (Table E2). The estimated negative impact of LAWA is, however, not statistically significant relative to any of the comparison groups.

TABLE E2
Linear probability models of employment, in Arizona across population groups

Identification strategy	Within Arizona, across population groups			
	Hispanic		Non-Hispanic native	
	Naturalized immigrant	Native	White	Black
Likely unauthorized*ACS 2008	**-0.039**	**-0.042**	**-0.001**	**-0.040**
	(0.83)	**(1.32)**	**(0.03)**	**(0.80)**
Female*likely unauthorized*ACS 2008	**-0.008**	**0.097**	**0.025**	**0.040**
	(0.12)	**(1.70)**	**(0.59)**	**(0.61)**
Likely unauthorized	0.138	0.241	0.237	0.501
	(3.52)	(10.51)	(5.13)	(5.31)
ACS 2008	-0.005	0.014	-0.025	-0.001
	(0.14)	(0.66)	(2.39)	(0.02)
Female	-0.316	-0.100	-0.138	-0.077
	(10.01)	(6.13)	(10.89)	(2.05)
Female*likely unauthorized	-0.202	-0.428	-0.390	-0.441
	(5.94)	(12.91)	(15.51)	(9.53)
Female*ACS 2008	0.077	-0.024	0.041	0.031
	(1.72)	(0.78)	(3.04)	(0.49)
Age	0.045	0.055	0.043	0.050
	(8.10)	(13.80)	(14.76)	(8.53)
Age squared/100	-0.057	-0.070	-0.057	-0.065
	(7.67)	(12.73)	(14.44)	(8.40)
High school graduate	0.048	0.150	0.149	0.079
	(3.18)	(18.64)	(13.91)	(5.36)
Married	-0.060	0.000	0.008	-0.047
	(3.89)	(0.02)	(0.94)	(3.21)
Number of children	-0.002	-0.001	-0.004	0.007
	(0.22)	(0.11)	(0.52)	(0.53)
No. of children younger than 5	-0.028	-0.027	-0.036	-0.050
	(2.22)	(2.44)	(3.53)	(3.12)
Children of school age	-0.005	-0.005	0.023	-0.020
	(0.21)	(0.33)	(1.73)	(0.72)
Years since migration	0.001	-0.006	-0.002	0.003
	(0.23)	(1.89)	(0.49)	(0.30)
Years since migration^2/100	0.002	0.017	0.005	-0.016
	(0.34)	(1.66)	(0.36)	(0.23)
Speaks only English	-0.021	0.026	0.056	0.210
	(0.29)	(0.99)	(1.02)	(2.24)
Speaks very well	0.046	0.007	0.032	0.124
	(1.09)	(0.31)	(0.50)	(0.74)
Speaks English well	0.050	-0.019	-0.011	-0.002
	(1.80)	(0.79)	(0.39)	(0.09)
Speaks English but not well	0.062	0.053	0.050	0.057
	(3.75)	(3.11)	(2.73)	(3.11)
R-Squared	0.274	0.222	0.156	0.284
Number of observations	5,474	11,211	22,462	4,761

NOTES: Estimates are based on 2005, 2006 and 2008 ACS. The t-statistics in parentheses are based on robust standard errors clustered around PUMAs. All models include MSA fixed effects.

Overall, the set of difference-in-difference estimates of the employment rate suggest that LAWA did not have much of an impact on the employment probability of unauthorized Hispanic workers relative to other low-skilled workers in Arizona or to those of likely unauthorized workers in the comparison states. As we discuss in the report, the analysis of employment rate may hide shifts from employment in wage and salary work to self-employment. To shed light on this, we next turn to our estimates of the wage and salary employment rate, defining employment as wage and salary work only. We then present our results of the estimated effects of LAWA on the self-employment probability.

Wage and Salary Employment

Table E3 shows that our group of likely unauthorized immigrant men is the only group who experienced a statistically significant change in the wage and salary employment rate in the post-LAWA period. The estimate of close to 6 percentage point is large but is smaller than what we obtain using the synthetic cohort method. This may be due to the shorter post-LAWA period allowed for in the ACS data. Nonetheless, both approaches using different data sources reveal a strong decline in employment opportunities in formal employment. Similar to the synthetic cohort approach, the DD results reveal no significant negative LAWA impact for our group of likely unauthorized women.

TABLE E3
Linear probability models of wage/salary employment, Arizona and comparison states

Identification strategy	Across states, within population groups				
	Hispanic			Non-Hispanic native	
	Likely unauthorized	Naturalized immigrant	Native	White	Black
Arizona*ACS 2008	**-0.059**	**-0.058**	**0.038**	**0.009**	**0.050**
	(1.93)	**(1.34)**	**(1.57)**	**(0.64)**	**(1.02)**
Female*Arizona*ACS 2008	**0.029**	**0.032**	**-0.019**	**0.003**	**0.037**
	(0.63)	**(0.60)**	**(0.53)**	**(0.17)**	**(0.49)**
Arizona	0.070	0.036	-0.011	-0.073	0.097
	(1.32)	(0.50)	(0.42)	(3.71)	(1.31)
ACS 2008	-0.023	0.012	-0.030	-0.022	-0.026
	(1.37)	(0.53)	(2.71)	(2.49)	(1.00)
Female	-0.483	-0.259	-0.113	-0.116	-0.023
	(30.70)	(13.32)	(11.48)	(13.76)	(0.84)
Female*Arizona	-0.023	-0.040	0.026	0.026	-0.045
	(0.86)	(1.09)	(1.31)	(1.63)	(0.90)
Female*ACS 2008	0.070	0.102	0.002	0.032	0.001
	(3.43)	(3.43)	(0.11)	(2.54)	(0.02)
Age	0.036	0.049	0.050	0.034	0.047
	(9.73)	(10.22)	(21.13)	(24.23)	(11.26)
Age squared/100	-0.047	-0.061	-0.066	-0.047	-0.064
	(9.23)	(10.58)	(20.73)	(24.94)	(11.26)
High school graduate	0.025	0.070	0.205	0.202	0.223
	(2.39)	(5.85)	(22.64)	(35.86)	(11.10)
Married	-0.043	-0.043	0.052	0.027	0.085
	(3.78)	(3.11)	(6.14)	(4.57)	(3.84)
Number of children	-0.006	-0.003	0.001	-0.013	0.002
	(0.73)	(0.42)	(0.12)	(2.60)	(0.14)

continued

Identification strategy	Across states, within population groups				
	Hispanic			Non-Hispanic native	
	Likely unauthorized	Naturalized immigrant	Native	White	Black
No. of children younger than 5	-0.035	-0.003	-0.010	-0.035	-0.054
	(3.53)	(0.22)	(1.32)	(5.95)	(2.77)
Children of school age	-0.007	0.005	-0.014	0.014	0.024
	(0.40)	(0.29)	(0.98)	(1.51)	(0.72)
Years since migration	0.017	-0.004	-0.005	0.001	0.006
	(3.23)	(1.72)	(0.87)	(0.06)	(0.41)
Years since migration^2/100	-0.127	0.007	0.011	0.003	-0.005
	(3.28)	(1.46)	(0.84)	(0.14)	(0.11)
Speaks only English		0.021	-0.024	0.217	0.203
		(0.47)	(0.50)	(1.04)	(1.59)
Speaks very well		0.092	-0.035	0.176	0.148
		(3.20)	(0.72)	(0.83)	(1.04)
Speaks English well	0.013	0.090	-0.065	0.161	0.108
	(0.81)	(3.25)	(1.35)	(0.76)	(0.58)
Speaks English but not well	0.035	0.052	-0.008	0.106	
	(3.08)	(2.09)	(0.15)	(0.51)	
R-Squared	0.267	0.113	0.149	0.093	0.169
Number of observations	11,799	8,089	30,934	64,370	4,795

NOTES: Comparison area consists of Nevada, New Mexico, Utah and Southern inland areas of California (Bakersfield, Fresno, Merced, Modesto, Riverside-San Bernardino, Stockton and Visalia-Tulare-Porterville). Estimates are based on 2005, 2006 and 2008 ACS. The t-statistics in parentheses are based on robust standard errors clustered around PUMAs. All models include MSA fixed effects.

The within-Arizona results, Table E4, indicate that the decline among likely unauthorized men ranges between 3.9 percentage points (relative to naturalized Hispanic immigrants) and 9 percentage points (native-born African American men). It indicates that the post-LAWA decline is statistically significant at the 10 percent significance level relative to all groups except naturalized Hispanic immigrants.

TABLE E4
Linear probability models of wage/salary employment, in Arizona across population groups

Identification strategy	Within Arizona, across population groups			
	Hispanic		Non-Hispanic native	
	Naturalized immigrant	Native	White	Black
Likely unauthorized*ACS 2008	**-0.039**	**-0.075**	**-0.054**	**-0.094**
	(0.72)	**(2.15)**	**(1.74)**	**(2.03)**
Female*likely unauthorized*ACS 2008	**-0.043**	**0.118**	**0.054**	**0.072**
	(0.71)	**(1.98)**	**(1.24)**	**(1.08)**
Likely unauthorized	0.133	0.212	0.274	0.420
	(3.04)	(8.59)	(4.59)	(4.33)
ACS 2008	-0.044	0.006	-0.015	0.014
	(1.16)	(0.26)	(1.49)	(0.35)
Female	-0.305	-0.082	-0.089	-0.078
	(8.97)	(4.70)	(5.88)	(1.92)
Female*likely unauthorized	-0.199	-0.434	-0.426	-0.428

continued

Identification strategy	Within Arizona, across population groups			
	Hispanic		Non-Hispanic native	
	Naturalized immigrant	Native	White	Black
	(5.49)	(13.75)	(17.46)	(9.41)
Female*ACS 2008	0.136	-0.021	0.037	0.024
	(3.11)	(0.63)	(2.23)	(0.38)
Age	0.038	0.050	0.036	0.043
	(6.94)	(13.70)	(12.83)	(7.72)
Age squared/100	-0.048	-0.065	-0.050	-0.058
	(6.74)	(13.09)	(13.22)	(7.59)
High school graduate	0.037	0.151	0.152	0.076
	(2.36)	(15.86)	(14.44)	(4.56)
Married	-0.051	0.002	-0.011	-0.040
	(3.36)	(0.21)	(1.24)	(2.63)
Number of children	-0.012	-0.006	-0.005	-0.005
	(1.05)	(0.76)	(0.59)	(0.41)
No. of children younger than 5	-0.019	-0.022	-0.032	-0.037
	(1.71)	(1.96)	(3.49)	(3.06)
Children of school age	-0.004	-0.012	0.012	-0.015
	(0.17)	(0.66)	(0.89)	(0.56)
Years since migration	0.003	-0.004	0.002	0.007
	(1.00)	(1.10)	(0.44)	(0.74)
Years since migration^2/100	-0.002	0.015	0.004	-0.043
	(0.33)	(1.34)	(0.26)	(0.59)
Speaks only English	0.002	0.013	0.069	0.177
	(0.03)	(0.47)	(1.06)	(1.77)
Speaks very well	0.044	0.002	0.011	0.066
	(1.12)	(0.08)	(0.15)	(0.40)
Speaks English well	0.039	-0.023	-0.024	-0.002
	(1.44)	(0.90)	(0.84)	(0.07)
Speaks English but not well	0.064	0.049	0.047	0.058
	(4.07)	(3.42)	(3.02)	(3.69)
R-Squared	0.232	0.185	0.117	0.246
Number of observations	5,474	11,211	22,462	4,761

NOTES: Estimates are based on 2005, 2006 and 2008 ACS. The t-statistics in parentheses are based on robust standard errors clustered around PUMAs. All models include MSA fixed effects.

Overall, the set of difference-in-difference estimates of the employment rate strongly supports the synthetic cohort results that unauthorized immigrants who remained in Arizona faced worse employment opportunities in the wake of LAWA.

Self-employment

As alluded to above and discussed further in the report, it is possible that an analysis of the employment rates hide some important LAWA-induced workforce changes—recall that the DD employment rate estimates above revealed no convincing negative effect among likely unauthorized immigrants. For example, it is plausible that unauthorized immigrants who remained in Arizona will seek to avoid facing the work authorization verification mandate. This may be supported by employers of unauthorized immigrants.

One possible way to avoid the E-Verify verification requirement is to shift towards a contracting relationship with employers, as opposed to formal employment. In an effort to examine whether our data support this hypothesis we analyze whether LAWA induced individuals in the group of likely unauthorized immigrants to shift towards self-employment. We use the same difference-in-difference model specifications described above but using self-employment as the outcome variable of interest.

Table E5 shows that the estimated within-Arizona relative LAWA effect on self-employment ranges from a statistically insignificant -0.1 percentage point (compared to naturalized Hispanics) to a statistically significant approximate 5 percentage points (native whites and African-Americans). The across-state estimates in Table E6 indicate only a significant change among the likely unauthorized immigrant men (4.3 percentage points). Overall, these results provide further evidence of a strong LAWA-induced increase in the self-employment rate of unauthorized male workers in Arizona. The estimated smaller magnitude may be due to the fact that we are restricted to only one post-LAWA year.

TABLE E5
Linear probability models of self-employment in Arizona across population groups

Identification strategy	**Within Arizona, across population groups**			
	Hispanic		Non-Hispanic native	
	Naturalized immigrant	Native	White	Black
Likely unauthorized*ACS 2008	**-0.001**	**0.031**	**0.050**	**0.053**
	(0.03)	**(1.34)**	**(2.24)**	**(2.97)**
Female*likely unauthorized*ACS 2008	**0.035**	**-0.018**	**-0.025**	**-0.033**
	(1.03)	**(0.69)**	**(1.04)**	**(1.48)**
Likely unauthorized	0.004	0.028	-0.039	0.077
	(0.27)	(2.24)	(0.84)	(4.75)
ACS 2008	0.039	0.010	-0.009	-0.016
	(1.91)	(1.27)	(1.35)	(1.94)
Female	-0.012	-0.016	-0.048	-0.001
	(0.87)	(3.37)	(7.73)	(0.13)
Female*likely unauthorized	0.000	0.005	0.036	-0.010
	(0.01)	(0.67)	(3.29)	(0.76)
Female*ACS 2008	-0.058	-0.005	0.003	0.009
	(2.76)	(0.57)	(0.37)	(0.61)
Age	0.008	0.005	0.007	0.007
	(3.70)	(4.85)	(5.48)	(3.62)
Age squared/100	-0.008	-0.005	-0.007	-0.007
	(2.73)	(3.48)	(3.81)	(2.81)
High school graduate	0.009	-0.002	-0.003	0.002
	(1.24)	(0.36)	(0.59)	(0.27)
Married	-0.009	-0.002	0.019	-0.008
	(0.92)	(0.29)	(3.43)	(0.75)
Number of children	0.008	0.004	0.001	0.011
	(1.27)	(1.26)	(0.20)	(2.05)
No. of children younger than 5	-0.008	-0.005	-0.004	-0.011
	(0.77)	(0.78)	(0.69)	(1.17)
Children of school age	0.000	0.008	0.010	-0.003
	(0.02)	(1.08)	(1.20)	(0.36)

continued

Identification strategy	Within Arizona, across population groups			
	Hispanic		Non-Hispanic native	
	Naturalized immigrant	Native	White	Black
Years since migration	-0.002	-0.002	-0.003	-0.004
	(1.38)	(1.38)	(1.78)	(1.13)
Years since migration^2/100	0.004	0.001	0.000	0.023
	(1.12)	(0.41)	(0.09)	(0.73)
Speaks only English	-0.021	0.013	-0.014	0.033
	(0.96)	(1.17)	(0.28)	(2.01)
Speaks very well	0.003	0.005	0.021	0.059
	(0.16)	(0.53)	(0.42)	(1.83)
Speaks English well	0.013	0.004	0.013	0.000
	(1.04)	(0.33)	(0.92)	(0.03)
Speaks English but not well	-0.003	0.003	0.003	-0.002
	(0.32)	(0.33)	(0.27)	(0.24)
R-Squared	0.026	0.023	0.033	0.028
Number of observations	5,474	11,211	22,462	4,761

NOTES: Estimates are based on 2005, 2006 and 2008 ACS. The t-statistics in parentheses are based on robust standard errors clustered around PUMAs. All models include MSA fixed effects.

TABLE E6
Linear probability models of self-employment, Arizona and comparison states

Identification strategy	Across states, within population groups				
	Hispanic			Non-Hispanic native	
	Likely unauthorized	Naturalized immigrant	Native	White	Black
Arizona*ACS 2008	**0.043**	**0.032**	**0.013**	**-0.004**	**-0.017**
	(1.99)	**(1.41)**	**(1.68)**	**(0.58)**	**(1.34)**
Female*Arizona*ACS 2008	**-0.027**	**-0.042**	**-0.010**	**-0.001**	**0.007**
	(1.18)	**(1.66)**	**(0.93)**	**(0.12)**	**(0.44)**
Arizona	-0.048	-0.037	0.004	0.019	-0.008
	(1.77)	(2.61)	(0.37)	(2.25)	(0.27)
ACS 2008	-0.006	0.005	-0.003	-0.004	0.006
	(0.85)	(0.46)	(0.90)	(0.93)	(0.54)
Female	-0.016	-0.036	-0.018	-0.039	-0.007
	(2.17)	(3.86)	(5.83)	(11.93)	(1.17)
Female*Arizona	0.006	0.026	0.002	-0.010	0.010
	(0.57)	(1.52)	(0.42)	(1.48)	(0.87)
Female*ACS 2008	0.003	-0.015	0.004	0.004	-0.003
	(0.32)	(1.01)	(0.77)	(0.58)	(0.23)
Age	0.005	0.004	0.003	0.006	0.005
	(2.73)	(1.57)	(5.23)	(7.47)	(2.37)
Age squared/100	-0.005	-0.002	-0.003	-0.005	-0.005
	(1.82)	(0.75)	(3.45)	(5.24)	(2.03)
High school graduate	0.010	0.014	0.000	0.003	0.004
	(1.87)	(1.82)	(0.09)	(1.13)	(0.78)
Married	-0.015	0.011	0.009	0.029	0.013
	(2.22)	(1.37)	(3.04)	(9.53)	(2.36)

continued

Identification strategy	Across states, within population groups				
	Hispanic			Non-Hispanic native	
	Likely unauthorized	Naturalized immigrant	Native	White	Black
Number of children	0.004	-0.001	-0.003	0.001	0.004
	(1.54)	(0.25)	(0.79)	(0.45)	(0.74)
No. of children younger than 5	-0.001	0.002	0.005	-0.004	-0.011
	(0.22)	(0.31)	(1.54)	(0.88)	(1.51)
Children of school age	-0.001	0.001	0.016	0.011	-0.007
	(0.15)	(0.05)	(2.16)	(1.76)	(0.59)
Years since migration	-0.001	0.002	0.000	-0.007	0.005
	(0.35)	(1.78)	(0.14)	(2.28)	(0.81)
Years since migration^2/100	0.010	-0.005	-0.002	0.015	-0.015
	(0.56)	(1.92)	(0.35)	(1.51)	(0.97)
Speaks only English		0.023	0.011	0.057	0.019
		(1.32)	(0.85)	(3.85)	(2.14)
Speaks very well		0.039	0.010	0.072	0.065
		(3.52)	(0.77)	(4.20)	(1.52)
Speaks English well	0.009	0.050	0.013	0.079	-0.007
	(1.10)	(4.23)	(1.02)	(2.59)	(0.57)
Speaks English but not well	0.001	0.022	0.003	0.082	
	(0.19)	(2.23)	(0.18)	(3.27)	
R-Squared	0.024	0.025	0.022	0.032	0.022
Number of observations	11,799	8,089	30,934	64,370	4,795

NOTES: Comparison area consists of Nevada, New Mexico, Utah and Southern inland areas of California (Bakersfield, Fresno, Merced, Modesto, Riverside-San Bernardino, Stockton and Visalia-Tulare-Porterville). Estimates are based on 2005, 2006 and 2008 ACS. The t-statistics in parentheses are based on robust standard errors clustered around PUMAs. All models include MSA fixed effects.

Robustness Checks

We subject our findings to a number of robustness checks on specification, definition of population groups, and choice of comparison states (the results are not shown but available on request from the authors). The primary focus of our testing is on statistically significant findings in employment probability and self-employment. In all, we find little sensitivity in our estimates, leading to a high degree of confidence in our main results. Detailed results are not reported here, but are available upon request.

First, we test alternative definitions of the treatment group, the "likely unauthorized" immigrants. We use alternative definitions that either broaden or restrict the original definition: add older arrival cohorts, more levels of English ability, or restrict to high-school dropouts only. These changes have little impact on the point estimates and do not change the magnitude in a consistent pattern. Most likely due to greater within-group heterogeneity, increasing the size of the treatment group sample does not increase the precision of our results greatly. These tests are also important given that our synthetic cohort approach, using data from the CPS, does not allow for as fine a definition of likely unauthorized as we can use in the ACS. Last in terms of specifications, we also perform a falsification test and estimate the same model using only pre-LAWA data (2005-2006) to check whether one would pick up the LAWA effect in years before passage. We find no evidence of misidentification.

Next we conducted a test to determine whether the actions in a specific part of Arizona—Maricopa County—are driving our estimates of the LAWA effect. The sheriff of Maricopa County, Joe Arpaio, has become nationally known for pursuing aggressive measures against unauthorized immigrants for many years. This county is home to a large fraction of Arizona's population, and immigrant population in particular. If his efforts increased or became more widely known among unauthorized immigrants concurrent with LAWA, what we are estimating as the LAWA effect may be essentially mislabeled. For instance, if the sheriff's actions drive unauthorized immigrants out of Maricopa en masse at the same time as LAWA is enacted, the labor market opportunities for competing workers are likely to be significantly improved. This would be due to enforcement in that county rather than LAWA's enforcement. Our models already include MSA fixed effects, which purge the data of time-invariant MSA level unobserved heterogeneity. One strategy to eliminate our estimates of possible time-varying trend unique to Maricopa is to drop all individuals in Maricopa County from our models.

Due to the substantially smaller sample, not surprisingly, the precision of our estimates declines substantially when Maricopa County, where roughly half of Arizona's population resides, is excluded. Nonetheless, we do not notice substantial shifts in the magnitude of our employment probability estimates. Overall, we fail to find evidence that the actions specifically in Maricopa are driving the conclusions drawn with respect to LAWA's employment effects.

For the estimates of LAWA's impact on population groups in Arizona relative to other states, we conduct a check on the definition of comparison states. We test the comparison state definition by restricting it to exclude Inland California, since with a longer history of immigration, California may be less comparable. We do not find that our results are much sensitive to the exclusion of California. In fact, the estimated LAWA employment and self-employment effects on our group of likely unauthorized men are somewhat greater in magnitude when California is not included in the comparison group (statistically significant -6.9 and + 5.1 percentage points respectively).

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