The causal effects of measures against unauthorized workers on labor market outcomes

– The experience of Arizona

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Abstract

In this thesis I investigate the labor market outcomes following the imposition of restrictive measures against unauthorized labor. I do this by studying the causal effects of the Arizona's migration laws LAWA and SB 1070 on labor market outcomes in terms of employment, wages and the usual hours worked per week. In 2008, Arizona implemented The Legal Arizona Workers Act (LAWA), demanding all employers to use the verification system E-Verify to validate the authorization of employees, and forbid employers of knowingly hire unauthorized immigrant workers. In 2010, The Arizona Senate Bill 1070 (SB 1070) followed, illegalizing unauthorized workers to work or apply for a job in the state, and requiring immigrants to carry compulsory documents with them at all times. Using data from the American Community Survey between the years of 2001 and 2016 and a Synthetic Control Method (SCM), I show that LAWA and SB 1070 had a negative impact on employment in Arizona of 1.1 to 3.6 percentage points, and a decline in yearly income of between 2040 to 4750 dollars for the working age population of Arizona. The effect is larger for the Hispanic low-educated population, and I find evidence of negative effects on Hispanic low-educated authorized workers as well. I find no clear evidence of improved labor market outcomes among the competing group of non-Hispanic low-educated workers, indicating that they are not substitutes to the unauthorized population. I interpret my results as evidence of mismatches on the labor markets with a lack of substitutability between authorized and unauthorized workers. I conduct permutation tests to establish inference (Abadie et al, 2010) and perform additional robustness checks to validate my results.

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1. Introduction

During the last decade, the world has experienced an increased movement of people between countries all over the world, estimated to be around 244 million current international migrants (IOM, 2018). Large flows of immigration have resulted in more than 11 million people living in the US without permission (Migration Policy Institute, 2016). The escalated inflow in the US has led to an increase in the labor supply of low-educated workers. While these flows have limited influence on the employment of natives within local labor markets (Card, 1990), looking at a more aggregate level, immigrants have the potential of displacing similarly skilled native workers (Borjas, 2015). The question of whether to strengthen immigration policy to restrict unauthorized workers' access to labor markets has thus become a debated issue, especially in the US. The purpose of imposing migration policies, that limits access to labor markets for unauthorized workers, is to make it more expensive for employers to hire unauthorized immigrant workers and to make it less attractive for the group to settle in the state due to limited labor opportunities. By reducing the number of unauthorized workers, additional labor market opportunities are enabled. Thus, the policy also strives to make it a more favorable environment for the competing authorized workers

Arizona implemented The Legal Arizona Workers Act (LAWA) in 2008, first of its type and one of the strictest migration reforms in the US. The Act demand all employers to use the verification system E-Verify to validate the authorization of employees, and forbid employers of knowingly hire unauthorized immigrant workers. In 2010 a second reform, called The Arizona Senate Bill 1070 (SB 1070) followed, imposing stricter measures against unauthorized workers. The Act illegalizes unauthorized workers to work or apply for a job in the state, and requires immigrants to carry compulsory documents with them at all times.

Previous studies conclude that the implementation of LAWA and SB 1070 impacts the number of unauthorized workers residing in the country. The event of Arizona is thus an ideal case study to learn from in terms of the possible labor market outcomes following the imposition of restrictive measures against unauthorized labor.

This thesis investigates the causal effect of the implementation of the laws LAWA and SB 1070 on labor market outcomes for the population of Arizona. I study labor market outcomes by use of data from the American Community Survey on employment, wages and the usual hours worked per week between the years of 2001-2016. I estimate the effect using the

Synthetic Control Method (SCM), which is preferable when dealing with case studies where the reform happens in one state. The approach reduces the arbitrariness of the construction of the control group using a matching procedure to assign weights to different control states, creating the best match between the control and treated group prior to the intervention (Abadie et al, 2010). Using the SCM approach, I find very well-matched controls in terms of pre-period outcomes. While I investigate the aggregated effect for the working age population, I expect subpopulations to be affected differently. A clear majority (91 percent) of the unauthorized population in Arizona is born in Mexico and Central America (Migration Policy Institute, 2018). I investigate the effect on the Hispanic low-educated population and the non-Hispanic low-educated population separately to address the question of substitutability among competing workers. To establish causal inference of my results, I conduct permutation tests (Abadie et al, 2010) and perform additional robustness checks.

I find negative effects on wages as well as employment for the population of Arizona. While an effect is present both connected to the introduction of LAWA and to SB 1070, the magnitude of the estimates varies over time. The employment level diminishes between 1.1 and 3.6 percentage points for the entire population and the wages are negatively affected by between 2040 to 4750 dollars during the post-treatment period. Further, the effect on the average hours worked per week is insignificant. In terms of heterogeneity, I find evidence of subpopulations being affected differently. Low-educated Hispanics experience a strong negative effect on all outcomes. Wages diminishes by between 2370 and 7095 dollars, employment by between 1.8 to 11.8 percentage points, and the average hours worked per week by 2.4 and 5.8 hours. However, I find no significant evidence of an effect on employment among the non-Hispanic low-educated population. While there is some evidence of a negative impact on wages after the introduction of SB 1070, the results vary a lot over time. Therefore, I am hesitant to draw further conclusions. The results hold using multiple robustness checks and I thus consider them to be robust.

The previous findings on the effect of emigration and tightening of migration laws are limited. The direction and magnitude of the estimated outcomes are related to the elasticities of competing workers, the context they work in, and the strictness and enforcement level of the laws. Neither theory nor previous work is enough to predict the outcome, and it is thus an empirical question.

While some authors study the effect of migration law reforms in terms of impact on the number of residing unauthorized workers (Good, 2013; Bohn et al, 2013; Hoekstra et al, 2017), employment (Amuedo-Dorantes et al, 2012; Good, 2013; Orrenius et al, 2015) and wages (Orrenius et al, 2015; Bohn et al, 2015), I study the dynamic impact of the introduction of both LAWA and SB 1070. To my knowledge, the labor market outcomes of SB 1070 have previously never been addressed prior to this study. Additionally, I provide a dynamic analysis of the effect of both SB 1070 and LAWA on labor market outcomes, between 2008-2016, with data previously never used in this context, contributing to the literature on labor market outcomes of imposing restrictive measures against unauthorized labor.

The remainder of the thesis is organized as follows. Chapter 2 describes the changes in Arizona migration laws, previous studies on the topic, and the theoretical implications of tightening migration policies. Chapter 3 explains the data and my empirical strategy. Chapter 4 presents the results together with the permutation tests. Chapter 5 provides robustness checks validating my results. Chapter 6 discusses the findings of the thesis and concludes.

2. Background

The following section presents the two law changes addressed in my thesis. The second part introduces previous literature and theoretical framework relevant for my topic.

2.1 Arizona migration laws - LAWA and SB 1070

LAWA and E-Verify

The Legal Arizona Workers Act (LAWA) was passed in July 2007 and implemented on the first of January 2008 (State of Arizona, 2007). The purpose of introducing LAWA was to improve the labor outcomes of the native and naturalized citizens of the state. The act forbid employers to knowingly hire unauthorized immigrant workers in their business. From the first of January 2008, employers have to run their recruitments through a system called E-Verify. The tool is a searchable database containing all verified workers residing in the country, developed by the United States Department of Homeland Security. The employer can only use E-Verify in connection to a hiring decision, and not with the sole purpose of identifying unauthorized workers. If the verification process is not passed (called "tentative non-confirmation"), the employee can appeal the denial. If the employee does not correct the error, the employer is obligated to terminate the employment (E-Verify 2018; National immigration law center, 2011). LAWA prescribes very strict sanctions if the terms are not followed. The state has a right to suspend business licenses in the case of deliberately hiring unauthorized workers. At the first offense, the business license is suspended during a period of time. If a second offence occurs, the business license is revoked.

One issue with the system is the delay of the verification process, especially regarding the elongated procedure of the "tentative non-confirmation" cases (Bohn et al, 2013). The delays could result in additional costs connected to hiring a worker that may not pass the verification process, such as initiated employee training and postponed start state of new recruitments. If this is true, the workers with the highest probability of being unauthorized are more expensive than those with low probability of being in the group. In my case, the group would be low-educated Hispanics.

SB 1070

In 2010, The Arizona Senate Bill 1070 (SB 1070) came into effect and the immigration policy of the state was further restricted. The Act made it illegal for an unauthorized worker to apply for a job or work in the state. Thereto, the law required immigrants to carry the compulsory documents with them at all times. SB 1070 mandated state and local law enforcement officials to require demonstration of needed documentation in any case of suspicion of a person being an unauthorized worker (State of Arizona, 2010).

This was called the "show me your paper" clause and it generated heavy criticism since it was argued to encourage racial profiling. In June 2012, the case Arizona v. the United States ruled that the law violated the Supremacy Clause of the US Constitution. Parts of the law were withdrawn, including the toughest requirement of immigrants carrying registration documents, the possibility for the police to arrest anyone suspected to be an unauthorized immigrant and making it a crime for unauthorized immigrants to search for a job (Arizona v. United States (2012) No 11-182).

2.2 Theoretical implications and previous literature on migration

Most literature on migration study the impact of immigrants entering a market. Fewer studies have been done regarding the effects of emigration, or the effects of imposing measures against illegal workers and forcing illegal immigrants to leave the country. To understand the possible mechanisms of the imposition of such measures and a large outflow of immigrants, I present some earlier studies and theoretical predictions on the effects of immigration as well.

The purpose of a law such as LAWA or SB 1070 is to increase the cost of hiring unauthorized immigrant workers and to reduce the attraction as an immigrant to settle in the state. The implementation of LAWA (including E-Verify) and SB 1070, and the presumptive penalties in case of violation, should according to the purpose of the laws thus reduce both the labor demand and labor supply of unauthorized workers. The emigration of unauthorized workers unable, or unwilling to take the risk, of finding a job is likely to affect the labor supply negatively. A few studies investigate the effects of LAWA and SB 1070 on labor supply. Bohn et al (2014) find evidence of a reduction in the unauthorized population of 17 percent as a result of LAWA. Hoekstra and Orozco-Aleman (2017) show that the passage of SB 1070 reduce the flow of unauthorized immigrants into Arizona by 30 to 70 percent. In addition, Good (2013) concludes a reduction in unauthorized population of around 24 percent for the likely unauthorized population as a result of the implementation of E-Verify and similar state policies.

By reducing the number of unauthorized workers holding employment, the expected outcome would be to make it a more favorable labor market for the authorized workers residing in the state. By imposing laws like LAWA and SB 1070, the employer now meets a higher cost and risk when employing people within the targeted population. Thus, the benefit of hiring unauthorized immigrant workers should diminish, and thus result in reduced employment and potentially lower wages among the targeted group. The latter to compensate the higher cost associated with the hiring of the unauthorized worker post implementation of the laws. A study by Bohn and Lofstrom (2013) find evidence of worsened labor possibilities for Hispanic low-educated non-citizen men, who they use as proxy for unauthorized workers. Good (2013) finds a reduction in employment of 10 to 20 percent for likely unauthorized workers as a result of implementing E-Verify.

As highlighted by Bohn and co-authors (2013), the actual outcome of authorized workers depends on whether they act as substitutes to the unauthorized workers, and if they compete in the labor market. Bohn, Lofstrom and Rafael (2015) study the labor market outcomes of the implementation of LAWA on low-skilled, native born and legal immigrant workers up until the year of 2009. They conclude that LAWA has not improved labor outcomes of native low-skilled workers, in contrary to its purpose. Another study by Amuedo-Dorantes and Bansak (2012) finds some support for positive impact on the employment for non-Hispanic, native-born men as a result of E-Verify. There is to my knowledge very scarce evidence of the labor market outcomes regarding the implementation of SB 1070.

There is literature available that study the substitutability of low-skilled natives and immigrants. While Good (2013) find evidence of native workers being imperfect substitutes to immigrant workers, Orrenius and Zavodny (2015) find evidence of substitutability between unauthorized and U.S.-born Hispanic workers. Ottaviano and Peri (2012) conclude that natives and immigrants are imperfect substitutes for workers in the US within the same group of education, experience and gender, since they choose different occupations and hold different skills. Card (2009) argues that if immigrants and natives within the same group are imperfect substitutes, the ones most affected would be those within the same competing category. Good (2013) not only find negative effects from emigration laws targeting unauthorized workers on employment among competing authorized Hispanic or white low-skilled workers, but also evidence of a substitution effect for the group of native, low-skilled blacks who experience an increase in employment.

Studies on immigration suggest that the relationship between immigrants (authorized and unauthorized) and native workers could rather be a case of complementarity (Foged et al, 2016; Ottaviano et al, 2012; Card, 2009). Foged and Peri (2016) ask whether immigrants can be viewed as complements or substitutes on the labor market and if natives gain or lose as a result of immigrant inflow. They find that native workers moved from manual to non-manual occupations. Thereto, native workers' labor outcomes were either positively affected or not at all and the authors argue that complementarity may be an explanation.

Studies find statistical evidence of negative impact on wages among the unauthorized population as a result of LAWA and E-Verify (Bohn et al, 2013, Orrenius et al, 2015). Orrenius and Zavodny (2015) states that the predicted outcome on wages for unauthorized workers depend on whether the employer will continue to hire the group. If the demand for the unauthorized labor goes down, the wages will likely also go down. If the supply goes down and there is still a demand for the labor, there could be a positive shift in wages.

The impacts on other workers once again depends on whether a group of workers is a substitute or a complement to the targeted unauthorized workers. The evidence on the effects on competing groups is yet again ambiguous. There is evidence of increased earnings among low-skilled non-Hispanic native workers as a result of LAWA (Bohn et al, 2015). Bansak and Amuedo-Dorantes (2012) find evidence of positive effects on hourly wages for non-Hispanic, native-born men as a result of introducing various E-Verify legislations, indicating substitutability of unauthorized immigrant workers and low-skilled natives. However, they find no effect for naturalized Hispanic workers. Orrenius and Zavodny (2015) though find evidence of increased earnings among US-born Hispanic men.

The well-studied event of the Marielitos (Card, 1990; Borjas, 2015), a large group of Cubans immigrating to Florida resulting in an exogenous increase in the labor supply, could give further contribution to the understanding of how wages of competing workers are affected by unauthorized workers in the labor market. The studies also highlight the importance of finding the right control group. Card studies the impact among unskilled workers and find no significant effect on unemployment or wages. Card attributes the results to the Miami labor market's absorptive capacity of labor supply. However, Borjas (2003) measures the impact of immigration on separate groups of natives defined by education, decade and 5-year potential experience. He concludes that immigration reduces wages of native-born workers. Borjas

(2015) revisits the case of the Marielitos and concludes that Card's results are dependent on the choice of control group. Unlike Card, he suggests that the correct control group consists of high-school dropouts since a large part of the Marielitos (60 percent) holds less than a high-school degree. Using a synthetic control, Borjas is able to show significant negative impact on wages. The importance of choosing a correct control group is thus essential to establish causal effects.

For employers to fully avoid the risk of hiring unauthorized workers, the employer must be able to distinguish authorized from unauthorized workers. Since E-Verify only is applicable after a hiring decision, the employer cannot discriminate between authorized and unauthorized workers prior to the verification process. This imposes the risk of employers to apply subjective frameworks in the hiring process, relying on their own extrapolative capability, predicting legal status through signals such as ethnicity, accent or surname (Bohn et al, 2013). Phelps (1972) and Arrow (1973) presented the theory of statistical discrimination. They argue that in the absence of information about a person's ability, the decision-maker will rely on group averages. In the context of the labor market, an employment decision could be based on visible features such as race.

Reforms such as LAWA and SB 1070 could have spillover effects to other states (Bohn et al, 2014). Unauthorized workers may leave for neighboring states due to the implementation of restrictive measures against unauthorized labor. Including these states in my analysis could impact my results due to general equilibrium effects. Assuming that unauthorized workers have a lower reservation wage, a downward pressure on wages could then occur when the group enters a neighboring state. Thus, depending on what group the researcher chooses to study, the result could have different directions of bias.

There are some issues that can affect the outcomes of the reform. First, if not all sectors use E-Verify, unauthorized workers could reallocate to certain industries. Since the law demands all employers, this is not dealt with in this thesis. Second, as highlighted by Bohn and co-authors (2014), the workers leaving the formal sector risk going "under the radar" for informal work instead, which would put a downward bias on the results. Lastly, identification theft or fraud in the verification process is also a threat to the analysis. This is not accounted for within the framework of this thesis.

In summary, the predicted effect on employment and wages for the unauthorized population should according to previous studies (Bohn et al, 2013; Good, 2013) be negative. The predicted effect on labor outcomes on authorized workers is however not easy to determine since it depends on whether a group of workers are substitutes or complements, and also the size of the effect on labor demand and labor supply. In case of statistical discrimination, individuals comparable to the targeted group could face negative outcomes. Given that theoretical framework is not enough to solely predict the effects of restrictive measures against unauthorized labor such as LAWA and SB 1070, together with the lack of literature on the topic gives reason for more empirical work.

3. Data description and empirical methodology

I use data from the American Community Survey (ACS) ranging from 2001-2016. It consists of repeated cross-section micro-level data collected yearly. The dataset contains information on different labor outcomes such as employment status and income, and also demographic information. I exploit individual labor market data on individuals in the labor force and in working age (between 18 and 65) from all US states, excluding those that implemented similar migration laws during the period of interest (Alabama, Georgia, Indiana, South Carolina and Utah) and those bordering Arizona to exclude the possibility of general equilibrium effects[1].

I study the effects on the labor market by measuring the impact on employment, usual hours worked per week and the yearly income[2]. The ACS also include information on the industrial affiliation of the respondent. In table 1, I present the industry categories that I use to construct different industry share variables.

Table 1. Industry variable composition

Industry variable composition

	Industry category
Industry group 1	Agriculture, forestry, fishery, mining and construction
Industry group 2	Manufacturing
Industry group 3	Transportation, communication, utilities, wholesale and retail trade
Industry group 4	Finance, insurance, real estate, business, repair and personal services
Industry group 5	Information, entertainment and recreation, professional and related services, public administration and active duty military

3.1 Synthetic control

In an optimal setting, the difference-in-differences (DD) method is the ideal approach to study the effects of policy reforms. The method compares the outcomes of two groups, treated and control, pre- and post-introduction of a treatment. It assumes common trends, implying that the treated unit (in my case Arizona) and the control group should have the same trends in the pre-

[1] The excluded states consist of New Mexico, California and Nevada (and Utah, previously excluded).

[2] The employment variable is a dummy containing information on whether an individual is employed or not. The yearly income variable represents the income generated by working salary. The last outcome variable is an approximation from the survey respondent of how many hours he or she usually works per week. The data collection of the survey is conducted throughout the whole year, implying that data points from one year could have been collected in January as well as in December.

period, and post-period at the absence of treatment. The treated group is exposed to the treatment in the post-period, and not in the pre-period. The average treatment effect of the treated is calculated by observing the differences of the change in the treated group and the change in the control group (Angrist et al, 2009).

However, when the pre-trend assumption does not hold, the DD approach becomes invalid. Also, when analyzing the impacts of a case study where the reform only occurs in one state, there is the issue of ambiguity when choosing a comparison state. There is a risk of subjectivity and a challenge in choosing the control group that creates the counterfactual outcome that the treated unit would have experienced at the absence of the treatment. The solution is the Synthetic Control Method (SCM) introduced by Abadie and Gardeazabal (2003). The SCM is a matching procedure that constructs a vector of weights using a number of donor entities, in my case states. The weighted combination of donor states creates a synthetic control that closely matches the outcome of the treated state during the pre-treatment period (Abadie et al, 2010). The underlying argument for the methodology is that a weighted combination of many states is a better comparison than any other state alone. Abadie et al (2010) states that the SCM reduces the arbitrariness of the control group construction, by use of a matching procedure to construct a control group as similar as possible to the treated prior to intervention.

Like the DD approach, the SCM compares the outcome of the treated and untreated group, where the latter serves as the counterfactual of the treated group. The SCM generates a vector of weights using a linear factor model (3) presented below to minimize the difference between the two groups, given the stated weight constraints. One advantage of the SCM is that it broadens the framework of the DD approach, allowing unobserved factors to vary over time. The synthetic control aims to generate a weighted combination such as:

$$\sum_{j=2}^{j+1} w_j * Z_j = Z_1 \qquad and \qquad \sum_{j=2}^{j+1} w_j * \mu_j = \mu_1 \qquad (1)$$

Where Z represents observable characteristics and μ represents unobserved variables. The latter cannot be measured, since they are unobserved. The solution is to match on the pre-outcomes and covariates. Abadie, Diamond and Hainmueller (2010; 2014) argue that when the number of observations during the pre-treatment period is large, matching on pre-treatment period outcomes will generate a control group that is such a good match, that it can control for unobserved factors. Thus, the synthetic control will provide an unbiased estimator. The effect is calculated according to the following:

$$\alpha_{it} = Y_{it} - \sum_{j=2}^{j+1} w_j * Y_{jt} \qquad (2)$$

where α_{1t} is the treatment effect, Y_i is the outcome for the treated, Y_{jt} the outcome of the control and w_j the weight generated by the linear factor model.

In order to establish the effect from LAWA and SB 1070 on labor outcomes, I construct the synthetic control using the pre-intervention trend for the outcome (employment, yearly income and usual hours worked per week), the share of workers in different industries (in line with Abadie et al, 2003; Jones et al, 2018) and lastly, the share of Hispanic population. The following linear factor model is formulated:

$$Y_{jt}^{0} = \delta_t + \theta_t Z_j + \lambda_t \mu_j + \varepsilon_{jt} \qquad (3)$$

Where δ_t represents common time effects, Z_j are the observed, pre-treatment covariates (not affected by the treatment), μ_j are unobserved variables, and ε_{jt} the unobserved shocks at the state level with zero mean. I present one example of the result from the predictors in Table 1[3]. All industry group predictors contain averages for the entire pre-treatment period. The synthetic control generates a better match for all outcome trends and most of the industry group variables than the unmatched average. By creating a better control group than the unmatched, I can identify a better estimate of the effect of the treatment. The matching variable outcome least similar to Arizona is the Hispanic population share. To validate my findings, I compare the outcomes of Arizona to states that are very similar in terms of location and share of Hispanic population in the robustness check section.

3 Examples of results from other matching procedures appear in the appendix.

Table 2.

Predictors	Arizona	Synthetic control	Unmatched
Hispanic population	0.2682	0.0273	0.0643
Employed(2007)	0.6955	0.6975	0.7274
Employed(2006)	0.7073	0.7065	0.7282
Employed(2005)	0.7106	0.7090	0.7328
Employed(2004)	0.7017	0.7019	0.7292
Employed(2003)	0.6973	0.6970	0.7293
Employed(2002)	0.6915	0.6926	0.7314
Employed(2001)	0.7039	0.7041	0.7396
Industry group 1	0.0988	0.1079	0.0928
Industry group 2	0.0766	0.0921	0.1035
Industry group 3	0.1765	0.1752	0.1771
Industry group 4	0.1088	0.0842	0.0993
Industry group 5	0.4073	0.4085	0.4248

Notes: Employed represents the employment rate. The industry groups are described in chapter 3. Hispanic population represents the share of Hispanics in the state.

In table 2, I present the weights of the donor states from the matching procedure described above. Each outcome variable has its own composition of weighted states.

Table 3. Synthetic control weights of donor states

Donor state	Weight
Rhode Island	.187
Michigan	.059
North Dakota	.302
West Virginia	.452

4. The effect on labor market outcomes

4.1 Results

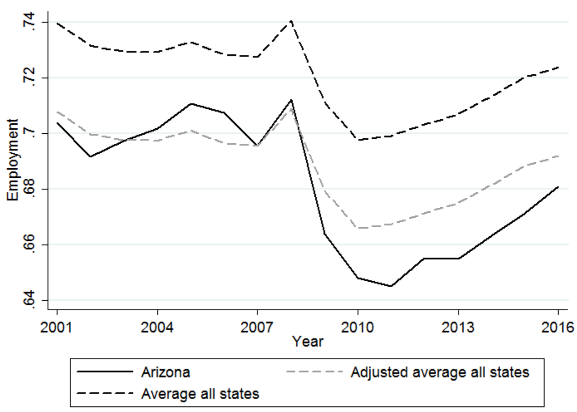
In this section, I present the main results from my analysis. First, I present graphic evidence of the effect of the reform. Second, I show the DD estimates over time in detail. Third, I present the heterogeneity of the results, including the impact of low-educated Hispanics and non-Hispanics. Forth, I show the causal inference of my results using permutation tests. Lastly, I present a robustness check.

As noted in section 3, the DD approach is best practice when estimating the effects of a policy change. The effect is then calculated using the following:

$$DD_{Az} = \left(Y_{Az}^{post} - Y_{All}^{post}\right) - \left(Y_{Az}^{pre} - Y_{All}^{pre}\right) \tag{4}$$

Where Y_{Az} represents the outcome of Arizona and Y_{All} the average outcome of the control states. But, with a difference in trends, the assumptions are no longer valid. Therefore, I apply the SCM. By matching pre-trends, I can calculate the DD estimates and interpret it as the average treatment effect of the treated.

First, Figure 1-6 plots a graphic representation of the main results. By comparing Arizona against the average of all states on the left side, Figure 1, 3 and 5, I illustrate the issue of difference in trends prior to the intervention. The grey dotted line shows the adjusted average of all states, validating that trends differ prior to treatment. Figure 2, 4 and 6 illustrate the effect of LAWA and SB 1070 on labor outcomes, comparing Arizona to its synthetic control. The dashed vertical lines illustrate the year of implementation of LAWA in 2008 and SB 1070 in 2010. At a first glance, there seems to be a drop in all outcome variables post the introduction of the reforms. The gap widens with the introduction of SB 1070. I present the calculated DD estimates in the following section.



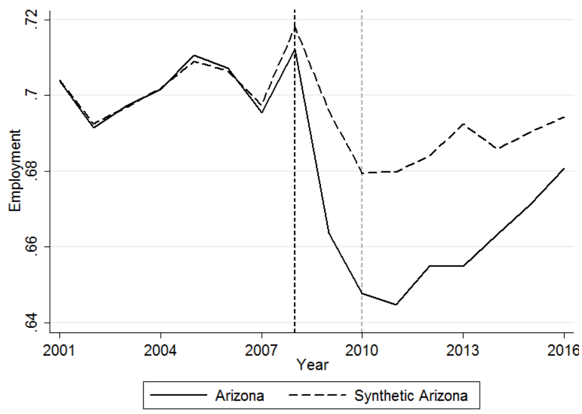


Figure 1 and 2. Plotted average employment in Arizona vs Average of all states and Arizona vs Synthetic Arizona

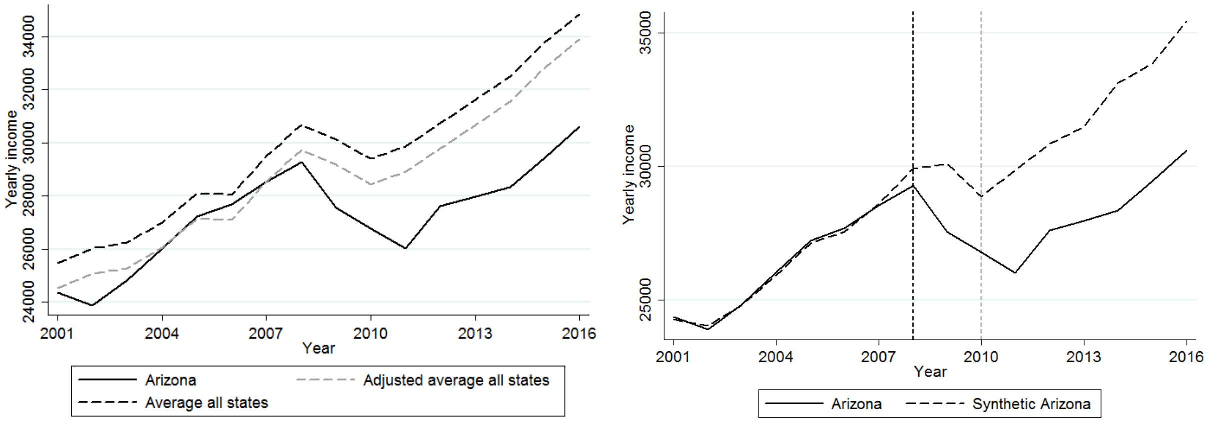


Figure 3 and 4. Plotted average yearly income in Arizona vs Average of all states and Arizona vs Synthetic Arizona

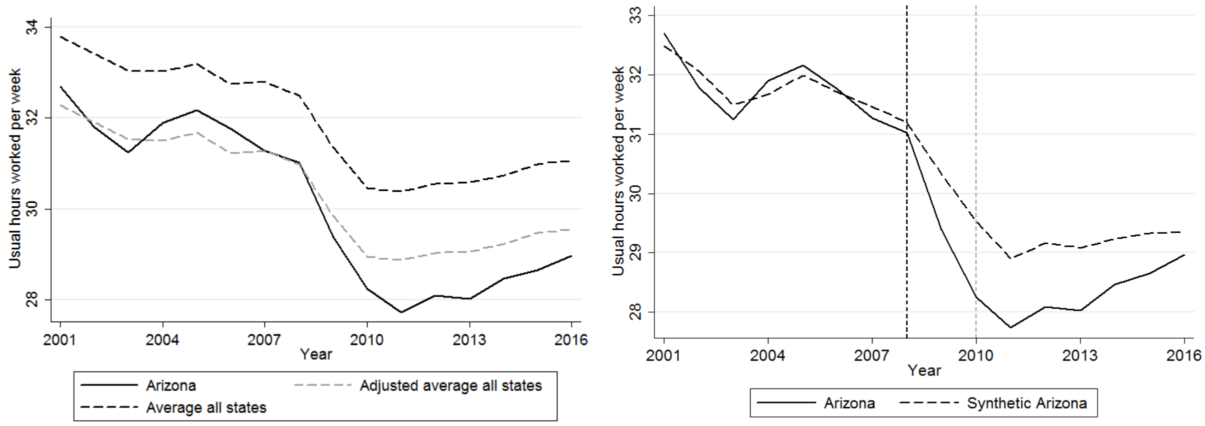


Figure 5 and 6. Plotted average hours worked per week in Arizona vs Average of all states and Arizona vs Synthetic Arizona

Table 4 shows the main results of the reform on employment, yearly income and usual hours worked per week. I calculate the estimated impact of LAWA and SB1070 on labor outcomes using the above stated DD estimation, comparing the treated to the untreated. More specifically, I calculate the following:

$$DD_{Az} = \left(Y_{Az}^{post} - Y_{Synth}^{post}\right) - \left(Y_{Az}^{pre} - Y_{Synth}^{pre}\right) \tag{5}$$

I show the estimates for separate years to distinguish the dynamic effects of the reforms. Panel A presents the unmatched calculated DD estimates of the effect of the reform, comparing Arizona against the average of all states. As addressed, when there is a difference in trends prior to intervention, the use of a conventional DD approach will not yield the actual treatment effect. The first column in Panel A, showing the effect two years prior to the intervention, verifies that there are differences in trends between the treated and untreated group, validating the choice of method to match the pre-trends using the SCM.

Panel B shows the main estimates using the SCM with matching on pre-trends for each individual outcome variable. First, the two reforms had a negative impact on overall employment of between 1.1 to 3.6 percentage points from 2009 and onwards. The effect in 2009 could be interpreted as a lagged impact of LAWA, while I cannot distinguish between the effects LAWA and SB 1070 from 2010 and onwards. The gap between Arizona and Synthetic Arizona narrows post 2013, but there is still a difference of about 1 percentage point in 2016. Second, the yearly reported wage diminishes with the introduction of the reforms. A smaller effect is visible in 2008. The gap then increases with time up until 2016. The greatest effect presents in 2016 when the wages of Arizona workers are on average 4,750 US dollars lower than Synthetic Arizona. LAWA and SB 1070 also has a negative impact on the last outcome variable usual hours worked per week. The effect shows already in 2009, which would indicate the impact of LAWA. The gap then widens at the introduction of SB 1070 and seems to narrow with time.

As mentioned in section 2, the effects are likely to differ for groups with different demographic characteristics. In the case of Arizona, a large share of the unauthorized population is Hispanic and holds less than a high-school degree (Migration Policy Institute, 2018). The group of unauthorized immigrant workers is not distinguishable in my data. However, the characteristics most likely to represent the group is that of low-educated (less than a high-school degree) Hispanics. I therefore conduct the SCM approach only on the low-educated Hispanic population, excluding all other individuals in the donor pool, in line with Borjas (2015). I exclude donor states with too few observations available in the dataset.

Panel C in Table 4 contains the estimated effect and Figure 7-12 contains graphic evidence. First, LAWA and SB 1070 have a negative impact on employment with the effect deepening with the introduction of SB 1070. The largest difference occurs in 2011, where the estimated negative effect on employment is 11.8 percentage points. Second, the estimations show that there is an increase in wages in 2008, followed by a sharp decrease the following years. The negative effect varies between 2555 to 7095 US dollars. The effect is similar for the sample of low-educated Hispanic citizens (naturalized and native), but the magnitude is not as large, as shown in Figure 8. This implies that the implementation of LAWA and SB 1070 not only affected the unauthorized workers, but also the residing Hispanic citizens in the state. Thus, this group does not seem to act as substitute for the unauthorized workers.

To further check for heterogeneity in my results I perform the SCM for low-educated non-Hispanics. Once again, I only include individuals that have the same characteristics as my treated group; non-Hispanics with less than a high school degree. Table D shows the DD estimates. The estimates indicate a positive impact on employment among the group of low-educated non-Hispanics as a result of the introduction of the reform. The effect on wages is not as straightforward. There seems to be an increase in wages the first couple of years, followed by a sharp reduction. The direction of the estimates on usual hours worked per week also vary with time. Figure 7-9 plots the effects for the different groups.

Table 4. DD estimates of the effect on labor outcomes

The effect of Arizona migration reform on labor outcomes

	-2	*2008*	*2010*	*2012*	*2014*	*2016*	*RMPSE*
Outcome			Panel A: Without matching				
Employment	0.0014	0.0035	-0.0178	-0.0161	-0.0182	-0.0110	0.0307
Yearly income	510.330	-463.23	-1659.95	-2174.51	-3218.94	-3283.54	1227.3448
Usual weekly hours worked	0.0484	0.0417	-0.6946	-0.9481	-0.7564	-0.5831	1.3403
			Panel B: Matched on pre-trends for individual outcomes				
Employment	-0.0007	-0.0039	-0.0297	-0.0270	-0.0207	-0.0115	0.001
Yearly income	29.891	-579.009	-2038.719	-3170.762	-4723.214	-4750.679	92.0484
Usual weekly hours worked	-0.1218	0.0142	-1.0964	-0.8869	-0.5791	-0.2052	0.205
			Panel C: Matched simultaneously on all pre-trends. low-educated Hispanics				
Employment	-0.0001	-0.0159	-0.0567	-0.0859	0.0183	-0.0677	0.000
Yearly income	-8.1340	868.51	-3492.128	-4810.215	-4647.284	-4045.02	0.000
Usual weekly hours worked	0.0092	-3.3322	-4.8660	-3.3526	-3.1402	-4.3714	0.000
			Panel D: Matched simultaneously on all pre-trends. low-educated non-Hispanics				
Employment	0.0049	0.0391	0.0549	0.0444	0.0613	0.0559	.0119
Yearly income	-164.80	1274.871	3293.9809	-3776.8801	-3342.6831	-4719.007	501.9111
Usual weekly hours worked	-0.3843	2.5394	3.0970	-0.8313	2.3676	-1.0831	.4176

Notes: All columns show the calculated effect for different years. The first column shows the effect prior to the reform. All matching is done using pre-trend outcomes, share of hispanic population and industry composition. Panel A shows the DiD estimate of Arizona and the average of all other states. Panel B-D show the comparison to Synthetic Arizona. The different outcomes have different number of synthetic cotrol states with individual combination of weights.
*RMPSE is the Root Mean Squared Prediction Error for the pre-intervention period.

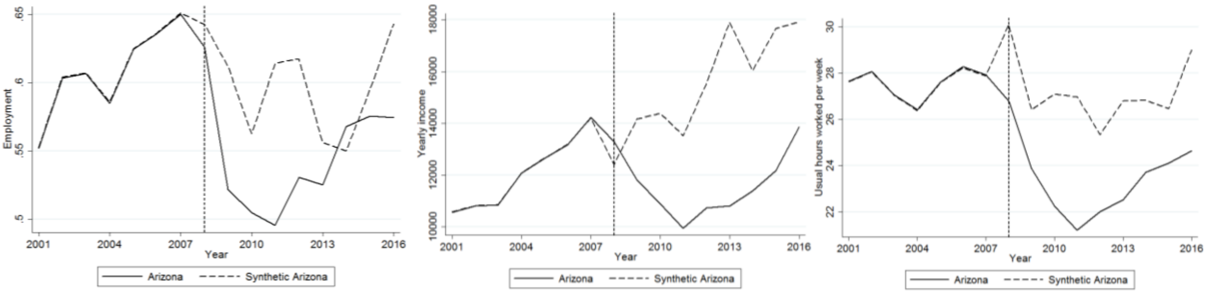


Figure 7. Hispanic low-educated workers

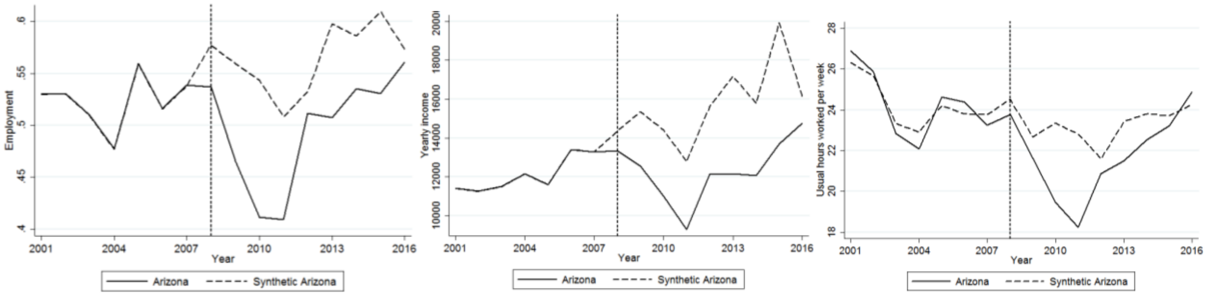


Figure 8. Hispanic low-educated citizens

As mentioned, the effect on low-educated non-Hispanics is not as clear and varies a lot over the years. The results are further validated in the following chapter.

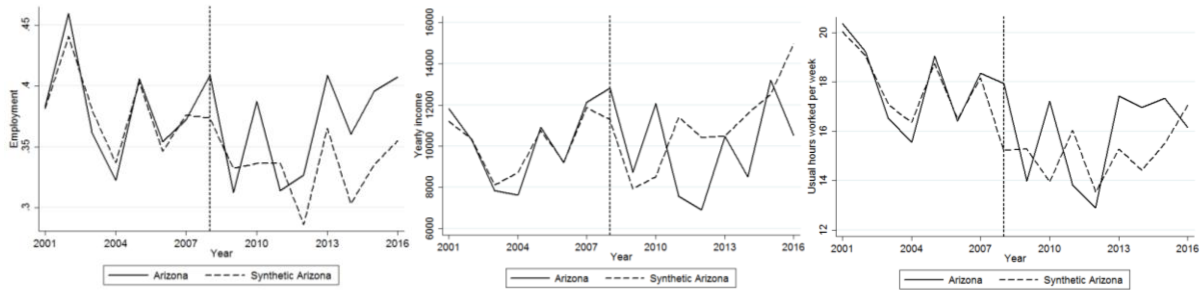


Figure 9. Non-Hispanic low-educated workers

4.2 How to perform inference

The presented results show the causal effect of the introduction of LAWA and SB 1070. However, I compare the macroaggregate of outcomes in Arizona and Synthetic Arizona which does not generate standard errors. I establish the significance in two steps by first performing permutation tests, and second by use of additional robustness checks, combining the SCM and DD approach. First, to show that my results are not an artifact of the synthetic control method or the sample selection, I simulate the introduction of LAWA and SB 1070 in states that in fact did not introduce it. If my identification strategy is correct, the effect estimated on these states should be on average zero, and the estimated effect should rarely if ever exceed the magnitude of my estimates of the effect of the real reform. More specifically, the proportion of times that the simulated placebo estimate exceeds in magnitude of the estimate of a real reform provides a measure of the p-value of my estimate (Abadie et al, 2010).

I assign placebo treatments for three different years for each state in the donor pool. Thus, all donor states are given synthetic controls using the same method as presented in section 3.1. I exclude Arizona from this sample in line with Bohn et al (2014). I then calculate DD estimates (Angrist et al, 2009) for one year and three years post-treatment for each placebo treatment. With the first estimate, I can validate the effect of LAWA and with the latter, the additional effect of SB 1070.

I plot the cumulative distribution of the estimates for each outcome, and create a sampling distribution for the DD_{Az} estimate. By sorting the DD estimates and plotting the cumulative distribution, the actual p-value is the position of Arizona on the y-axis plotted against Arizona's estimated effect of the reform (Chetty et al, 2009). This is marked by the line in each figure.

Figure 10 shows that the estimated effect on employment is significant at the 5%-level, with a p-value of 0.0236. Figure 11 presents the cumulative distribution of estimated placebo effects on income. The inference test shows a strongly significant effect on wages, with a p-value of 0.0157. Lastly, Figure 12 represents the inference exercise for the outcome usual hours worked per week. The effect on this outcome is insignificant.

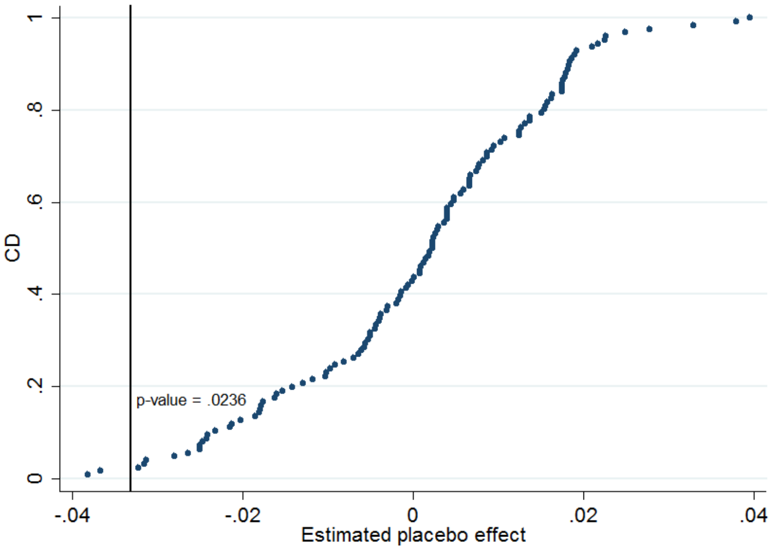


Figure 10. Distribution of placebo estimates: Employment

Notes: The figure plots the empirical distribution of placebo effects for employment three after treatment sets in (placebo 2009, 2008 and 2007; Arizona 2011). The CDF is constructed from 126 observations using the specification presented in chapter 3. The vertical line shows the treatment effect of Arizona.

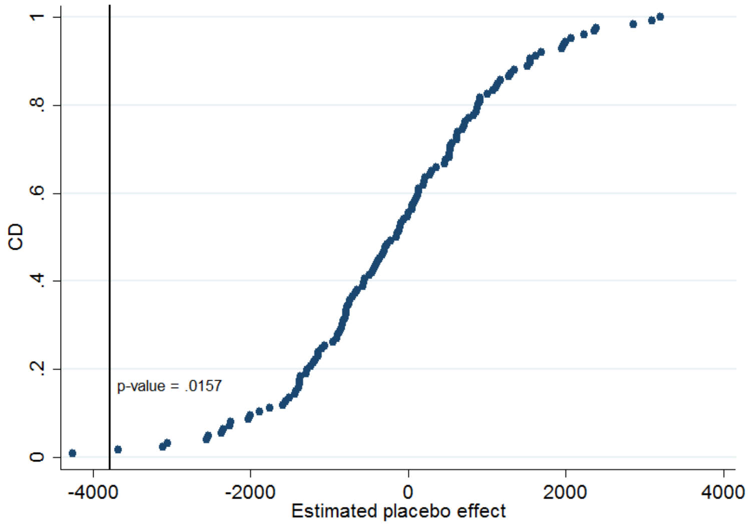


Figure 11. Distribution of placebo estimates: Yearly income

Notes: The figure plots the empirical distribution of placebo effects for yearly income three after treatment sets in (placebo 2009, 2008 and 2007; Arizona 2011). The CDF is constructed from 126 observations using the specification presented in chapter 3. The vertical line shows the treatment effect of Arizona.

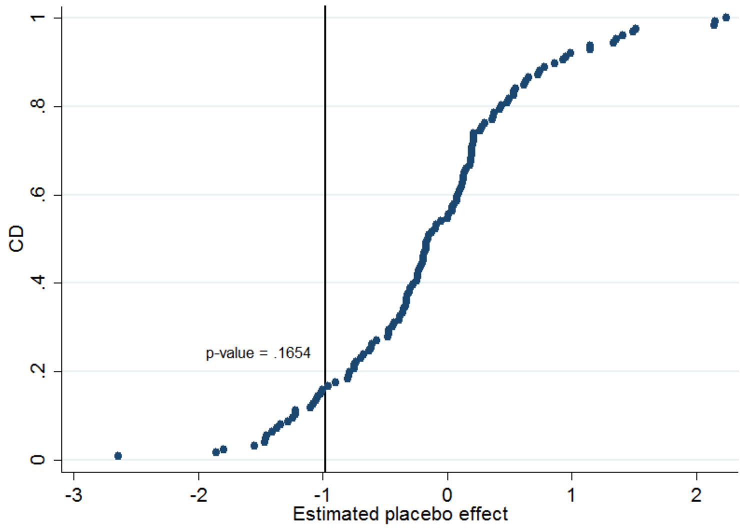


Figure 12. Distribution of placebo estimates: Usual hours worked per week

Notes: The figure plots the empirical distribution of placebo effects for usual hours worked per week three after treatment sets in (placebo 2009, 2008 and 2007; Arizona 2011). The CDF is constructed from 126 observations using the specification presented in chapter 3. The vertical line shows the treatment effect of Arizona.

Table 5 shows the p-values and estimated coefficients one year after LAWA and SB 1070 was introduced, together with the average effect of the reforms. The effect is significant for employment and yearly income both after the introduction of LAWA, SB 1070 and for the average of the entire treatment period. The effect on yearly income increases with the introduction of SB 1070. The effect on the usual hours worked per week is insignificant throughout the entire period.

Table 5. Estimated coefficients for all workers, p-values from permutation tests in parenthesis

	Entire population		
	2009	2011	Average
Employment	-0.0331 (0.0157)	-0.0304 (0.0236)	-0.0232 (0.0551)
Yearly reported income	-2500.715 (0.0315)	-3786.559 (0.0157)	-3259.5406 (0.0315)
Usual hours worked	-0.7503 (0.1181)	-0.9810 (0.1654)	-0.6489 (0.1969)
Average number of people in treatment group	33725	33725	33725
N of permutations	126	126	126

I next validate my findings of heterogeneous effects. As shown in Table 6, the impact of the reforms on low-educated Hispanics is significant for all outcome variables. The effect on employment is largest in 2011, with a negative impact of 11.8 percentage points significant at the 5-percent level. Not only wages, but also the usual hours worked per week are negatively affected by the reforms.

The case of the low-educated non-Hispanics is different. The effect is insignificant for all outcomes, except in 2011 where there seems to be a significant negative effect on wages at the 10-percent level. However, there is a positive effect in 2010 that is almost significant at the 10-percent level and the average effect show no significance.

Table 6.Estimated coefficients for low-educated Hispanics and non-Hispanics, p-values from permutation tests in parenthesis

	Low-educated Hispanics			Low-educated non-Hispanics			
	2009	*2011*	*Average*	*2009*	*2010*	*2011*	*Average*
Employment	-0.0894 (0.0727)	-0.1181 (0.0364)	-0.0515 (0.0727)	-0.0161 (0.3176)	0.0549 (0.1765)	-0.0183 (0.4235)	0.0370 (0.20)
Yearly reported income	-2369.604 (0.0909)	-3582.1791 (0.0727)	-3850.9886 (0.0364)	532.9611 (0.40)	3293.9809 (0.1059)	-4117.3583 (0.0588)	-1190.5046 (0.2118)
Usual hours worked	-2.566 (0.1273)	-5.7965 (0.0364)	-3.7896 (0.0909)	-1.473 (0.3059)	3.097 (0.1176)	-2.409 (0.1647)	0.6446 (0.3647)
Average number of people in treatment group	1236	1236	1236	403	403	403	403
N of permutations	54	54	54	84	84	84	84

In summary, the main results indicate that LAWA and SB 1070 had a significant impact on employment and yearly income. The effects vary among groups, being particularly large for the low-educated Hispanic population. The result is also similar looking only at low-educated

Hispanic citizens, although the results are not as significant for this group (see appendix). The subpopulation of low-educated non-Hispanics do not experience the same impact. I find no evidence of a substitution effect, but an indication of a negative effect on wages as a result of SB 1070. In the following chapter, I test the robustness of my results.

4.3 Robustness checks

In this section, I present four different robustness checks. First, I test the results using a standard DD approach. Second, I use a combined DD and SCM approach. I conduct this robustness check for the heterogenous findings as well. Third, I perform the SCM including the neighboring states in the donor pool, and forth using states that are similar in terms of the share of Hispanic population.

In the first step I use the standard DD approach presented in section 3, commonly used when studying the effect on policy changes. A dummy variable represents the treatment period, taking the value of one with the treatment (from 2008) and the value zero otherwise. I construct the model according to the following:

$$Y_{it} = \beta_0 + \beta_1 * Arizona_i + \beta_2 * post_t + \beta_3 * (Arizona * postdummy)_{it} + \alpha_i + \varepsilon_{it} \quad (6)$$

$Arizona_i$ represents a country dummy variable and $post_t$ represent a set of dummy variables for each year of the treatment period. The variable $postdummy_{it}$ denotes a set of dummies for the years of the treatment period. The interaction variable $(Arizona*postdummy)_{it}$ represents the post-treatment period for Arizona. α_i denotes a vector of control variables. I cluster standard errors by state (Cameron et al, 2015; Bertrand et al, 2004), allowing for autocorrelation within the states, and assume independence between them.

Table 7 shows the results from the DD regressions. The neighboring states are excluded from the sample, as well as states with similar migration reforms. Panel A shows the result from running the DD analysis using all other states as control group. The first column shows a "placebo" treatment, two years prior to the intervention. The result is significant for all outcome variables, validating my choice of method since there is a difference in trends between Arizona and the control group before the treatment sets in. However, the results are similar to my main results, with negative significant effect on employment and yearly income. While the usual weekly hours worked variable show a significant negative effect, my main results indicate that the effect is insignificant but that the magnitude and direction of the results are similar.

Second, I use a combined DD and SCM approach. I use the donor weight calculated from the SCM and include them in the DD regressions. Panel B shows the estimated results. There is no significance pre-treatment, which is a sign of more similar trends before treatment. Panel B show similar estimations as my main results with week evidence of an effect from LAWA in 2008, but a significant effect in 2009 (see detailed appendix). The magnitude of the effect is similar to my main results. The same effect is visible for the yearly income variable, where the negative impact once again is significant from 2009 and onwards. The estimations of the effects on usual hours worked per week show a negative significant impact as a result of LAWA and SB 1070. The permutation tests do not show any significance of the outcome on usual hours worked per week, therefore I am careful to draw further conclusions.

I also check the robustness of heterogeneous results on low-educated Hispanics and low-educated non-Hispanics using the above described method with a combined DD and SCM approach. The results are in line with my main results, validating that the effect is larger for low-educated Hispanics. However, I find a significant positive effect on all outcome variables for the years of 2008 and 2010 for the low-educated non-Hispanics. This is not proven in the permutation tests, thus I am hesitant to draw further conclusions from this. But, the results could be an indication that the low-educated non-Hispanic population is not as negatively affected by the reform.

Since the neighboring states may have been affected by the reform, these are excluded as donor states. To ensure that the exclusion of the neighboring states does not affect my results, I perform a robustness check including the states stated in section 2. I get the same weighted control as when excluding the neighboring states, ensuring that my results are not affected by the elimination. Lastly, I perform the SCM approach to create a synthetic control with a similar share of Hispanic population than my main Synthetic Arizona. The direction and magnitude of my DD estimates are similar to my main result and validate my findings (see Appendix).

Table 7. Robustness checks

Robustness checks

	-2	2008	2010	2012	2014	2016
Outcome						
	Panel A: Without matching					
Employment	0.00793***	0.00179	-0.0125***	-0.0148***	-0.0133***	-0.00436
	(0.00189)	(0.00163)	(0.00307)	(0.00239)	(0.00229)	(0.00266)
Yearly income	448.4*	-297.4*	-399.0	-1,517***	-2,023***	-1,852***
	(226.2)	(174.2)	(423.6)	(451.2)	(406.9)	(477.8)
Usual weekly hours worked	0.210**	-0.173**	-0.474***	-0.949***	-0.564***	-0.306**
	(0.0996)	(0.0760)	(0.141)	(0.125)	(0.128)	(0.138)
	Panel B: DiD - Matched simultaneously on all pre-trends					
Employment	-0.00269	-0.00466	-0.0265	-0.0239**	-0.0156*	-0.00507
	(0.00778)	(0.0119)	(0.0161)	(0.00698)	(0.00580)	(0.0103)
Yearly income	32.58	6.177	-2,816**	-3,453***	-4,132***	-4,993***
	(228.9)	(378.3)	(925.6)	(550.0)	(339.7)	(934.1)
Usual weekly hours worked	-0.0280	-0.295	-0.758*	-0.783**	-0.257	0.198
	(0.194)	(0.202)	(0.343)	(0.235)	(0.169)	(0.373)
	Panel C: Matched simultaneously on all pre-trends, low-educated hispanics					
Employment	-0.000669	-0.0219	-0.0670**	-0.0873***	0.0190	-0.0838**
	(0.0367)	(0.0406)	(0.0305)	(0.0302)	(0.0790)	(0.0386)
Yearly income	-313.4	257.0	-3,924	-4,758**	-5,163***	-4,676**
	(547.2)	(1,165)	(2,570)	(2,286)	(1,477)	(1,976)
Usual weekly hours worked	0.429	-2.383	-3.818***	-2.346**	-2.750***	-3.412***
	(1.877)	(1.504)	(1.044)	(0.980)	(0.743)	(1.039)
	Panel D: Low-educated non-hispanics					
Employment	0.0098	0.0359*	0.0518***	0.0413	0.0582*	0.0528*
	(0.0317)	(0.0153)	(0.0103)	(0.0457)	(0.0249)	(0.0227)
Yearly income	41.2317	1,597**	3,617***	-3,454***	-3,020**	-4,396
	(761.8224)	(526.2)	(714.1)	(695.9)	(900.8)	(2,844)
Usual weekly hours worked	-0.018456	2.777*	3.333**	-0.595	2.604**	-0.844
	(1.7576)	(1.378)	(1.139)	(0.940)	(0.812)	(1.509)

Notes: All matching is done using pre-trend outcomes, share of hispanic population and industry composition. All regression are controlled for state, year, gender and education level. Panel A contains the result from a difference-in-difference regression with all states except for neghbouring states and states with similar migration law reforms. Panel B and C's control group consists of weights produced by the synthetic control method. Standard errors are clustered at state level.

6. Discussion and conclusion

This thesis estimates the effect of imposing restrictive measures against unauthorized labor on labor market outcomes. I analyze the causal effect of the implementation of the laws LAWA and SB 1070 on labor market outcomes for the population of Arizona. I use a SCM with the significance tested using permutation tests to assess the effect on employment, yearly income and the usual hours worked per week. My results indicate that the reforms have unintended negative consequences. My findings show that the reforms have a significant negative impact on employment of 1.1 to 3.6 percentage points, and on yearly income with 2040 to 4750 dollars for the population of Arizona. The latter effect is not driven by a reduction in the usual hours worked per week, moving from full-time to part-time work. My findings show that the impact on wages and employment increases with time, becoming more substantial after the introduction of SB 1070. Since SB 1070 imposes even harder measures than its precursor, this is to be expected.

The reforms have heterogeneous effects, with negative effects on Hispanic low-educated workers, in line with Bohn and co-authors (2015). The reforms have a negative impact on employment and wages, and also on the usual hours worked per week. LAWA and SB 1070 not only affects the unauthorized workers negatively, but seemingly also the authorized low-educated Hispanic citizens (not in line with Orrenius et al, 2015). Further, the intention of improved labor market outcomes of native and naturalized citizens is not achieved. While the estimates on employment are mostly positive for non-Hispanic low-educated workers, I find no significant effects using my main approach. There is evidence of a weakly significant negative impact on wages, but the results are mostly insignificant and vary a lot in magnitude and direction. The results hold when performing robustness checks using a DD approach and a combined DD and SCM approach and I consider them to be robust.

My results indicate that given the goal of the reform, to reduce the proportion of unauthorized workers in the labor market, the target has been achieved. The prevailing aim of the studied laws, that competing authorized workers would benefit from implementation, lacks anchoring in the results. There is not only a lack of positive results, but also evidence of unintended negative impact on competing workers. The introduction of the reforms imposes the risk of employers using observable characteristics to determine what person to hire. My findings suggest that statistical discrimination could be present, explaining the unintended negative impact on the group of low-educated Hispanic citizens. Also, according to my findings, the

Hispanic low-educated working age population was affected several years after the introduction of the reforms, indicating that the unauthorized workers that stay in the state experience persistent consequences from these reforms. Whether deteriorating work opportunities for unauthorized workers or improving opportunities for authorized workers is most prioritized, the results give different policy implications. However, given that the purpose is to generate better labor market outcomes for the population, the reforms are not effective.

To achieve improved labor market outcomes, the aspect of substitutability is essential to address. If the authorized workers that stay in Arizona do not possess the same skill composition, or if they are not willing to take the jobs that hypothetically would be made available given the law change, the reforms will not improve labor opportunities for the authorized workers. The weak evidence of improved labor outcomes of competing groups suggests a low level of substitutability. With a comprehensive and instant change to the labor market, some employers more dependent on unauthorized labor are more likely to be affected. The result of my analysis could indicate that there is a mismatch between labor supply and demand within certain branches. In fact, if the labor shortage becomes great enough, firms may even be forced to shut down, resulting in worsened labor opportunities for the authorized workers. If this is the case, the way to tackle unauthorized labor could rather be to find a way to absorb the enhanced labor supply that the unauthorized workers constitute.

I contribute to the literature by adding knowledge regarding the impact of introducing measures against unauthorized workers on labor market outcomes. The external validity of the results holds with the assumption of similar inflow of illegal immigrants, with a comparable share of unauthorized labor. Also, the level of strictness of law enforcement will determine the extent of the effect. I leave to future studies to further analyze the heterogenous impacts of reforms such as LAWA and SB 1070. Recent studies on heterogeneous treatment effects (Athey et al, 2015) suggest that supervised machine learning predictions, together with causal inference tools can be used to better identify heterogeneous treatment effects. With this, policy-makers' decision-making could be helped to achieve better outcomes by the use of more tailormade reforms, based on findings with the use of machine learning (Athey et al, 2015), together with results from previous studies. The mismatches and unintended consequences of reforms as LAWA and SB 1070 could then possibly be minimized.

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DATA CITATION:

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Appendix

A1. Industry variable compositions

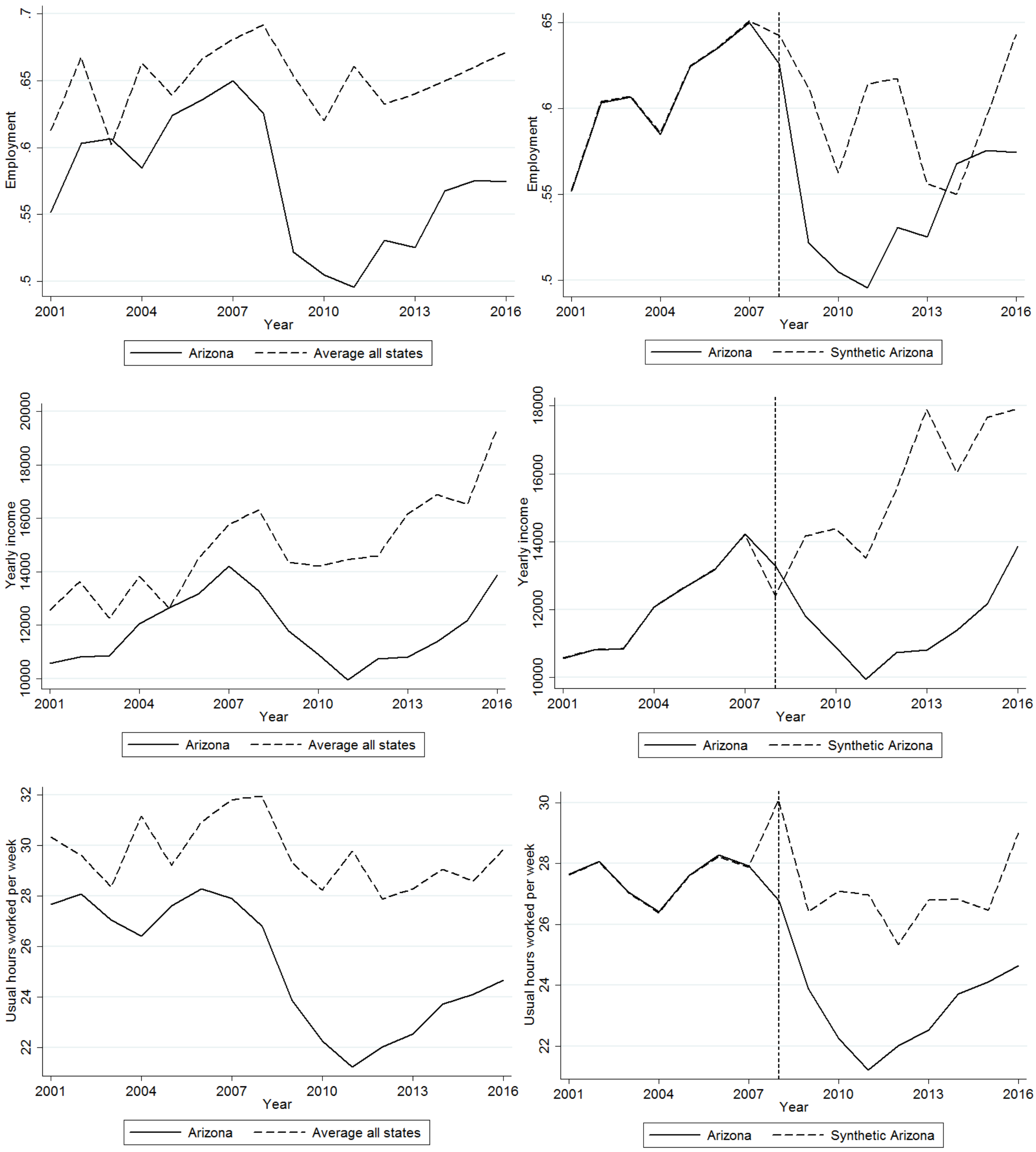
Predictors	Arizona	Synthetic control	Unmatched
Hispanic population	0.2682	0.0400	0.0643
Yearly income (2007)	28546.64	28601.29	29499.45
Yearly income (2006)	27694.24	27564.12	28053.11
Yearly income (2005)	27226.63	27126.39	28095.83
Yearly income (2004)	26040.65	25938.23	27016.57
Yearly income (2003)	24840.35	24828.16	26243.24
Yearly income (2002)	23899.52	24039.46	26036.04
Yearly income (2001)	24368.58	24282.19	25494.3
Industry group 1	0.0988	0.0921	0.0928
Industry group 2	0.0766	0.1323	0.1035
Industry group 3	0.1765	0.1788	0.1771
Industry group 4	0.1088	0.1067	0.0993
Industry group 5	0.4073	0.4155	0.4248

Predictors	Arizona	Synthetic control	Unmatched
Hispanic population	0.2682	0.0502	0.0643
Usual hours worked per week (2007)	31.2768	31.4659	32.7897
Usual hours worked per week (2006)	31.7595	31.7103	32.7382
Usual hours worked per week (2005)	32.1633	31.9923	33.1904
Usual hours worked per week (2004)	31.8953	31.6732	33.0172
Usual hours worked per week (2003)	31.2461	31.4935	33.0356
Usual hours worked per week (2002)	31.7921	32.0648	33.4123
Usual hours worked per week (2001)	32.6999	32.4956	33.7894
Industry group 1	0.0988	0.1008	0.0928
Industry group 2	0.0766	0.0902	0.1035
Industry group 3	0.1765	0.1764	0.1771
Industry group 4	0.1088	0.0878	0.0993
Industry group 5	0.4073	0.4107	0.4248

A2. Weights of main synthetic controls

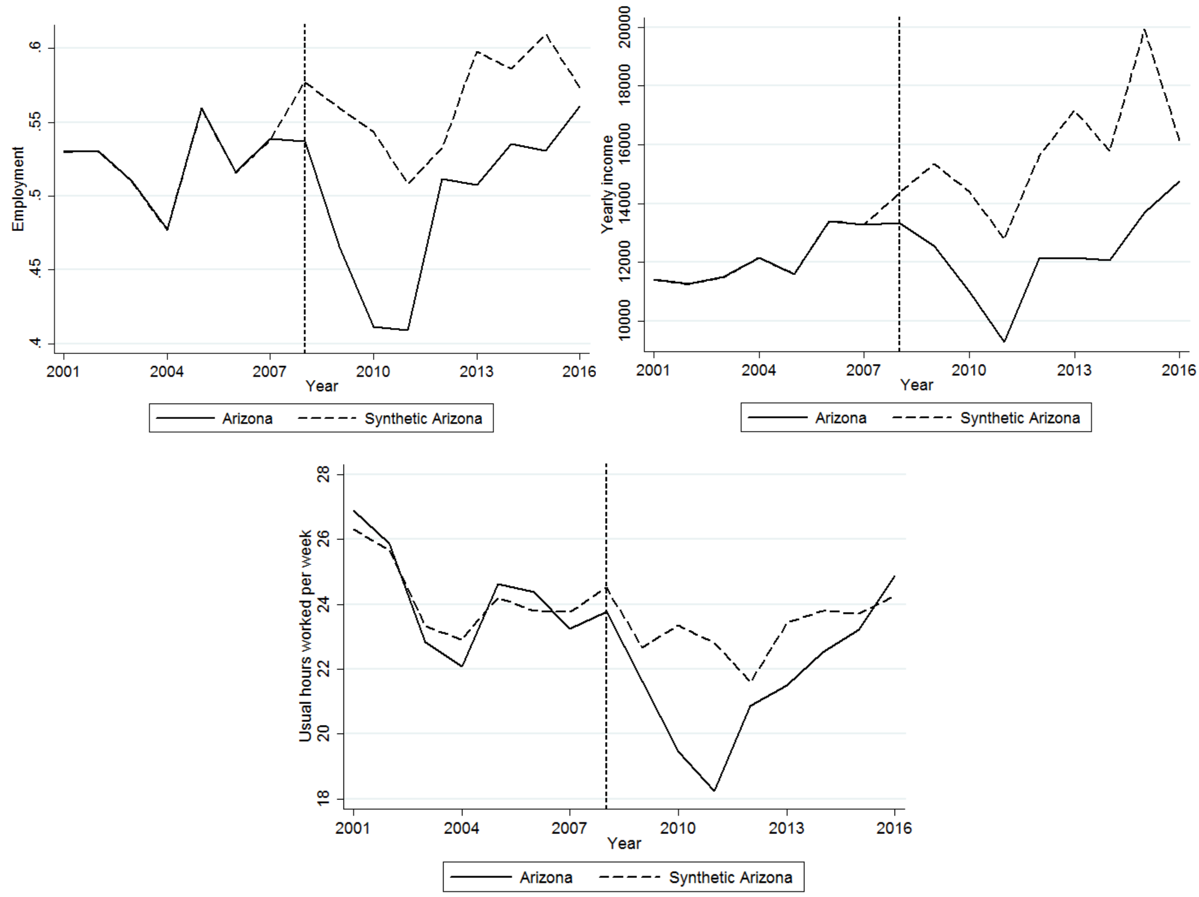
Wages		*Usual hours worked per week*	
Donor state	**Weight**	**Donor state**	**Weight**
Conneticut	0.013	Maine	0.168
Vermont	0.011	Florida	0.311
Iowa	0.824	Louisiana	0.364
Hawaii	0.093	Kentucky	0.021
District of Columbia	0.058	Maryland	0.69
		West Virginia	0.6
		Idaho	0.007

A3. Unmatched and matched averages of the Hispanic low-educated population



Notes: Total donor pool consisted of 26 states, dropped if the observations are less than 1000 per year

A4. Matched plotted averages of the Hispanic low-educated citizens in Arizona and Synthetic Arizona

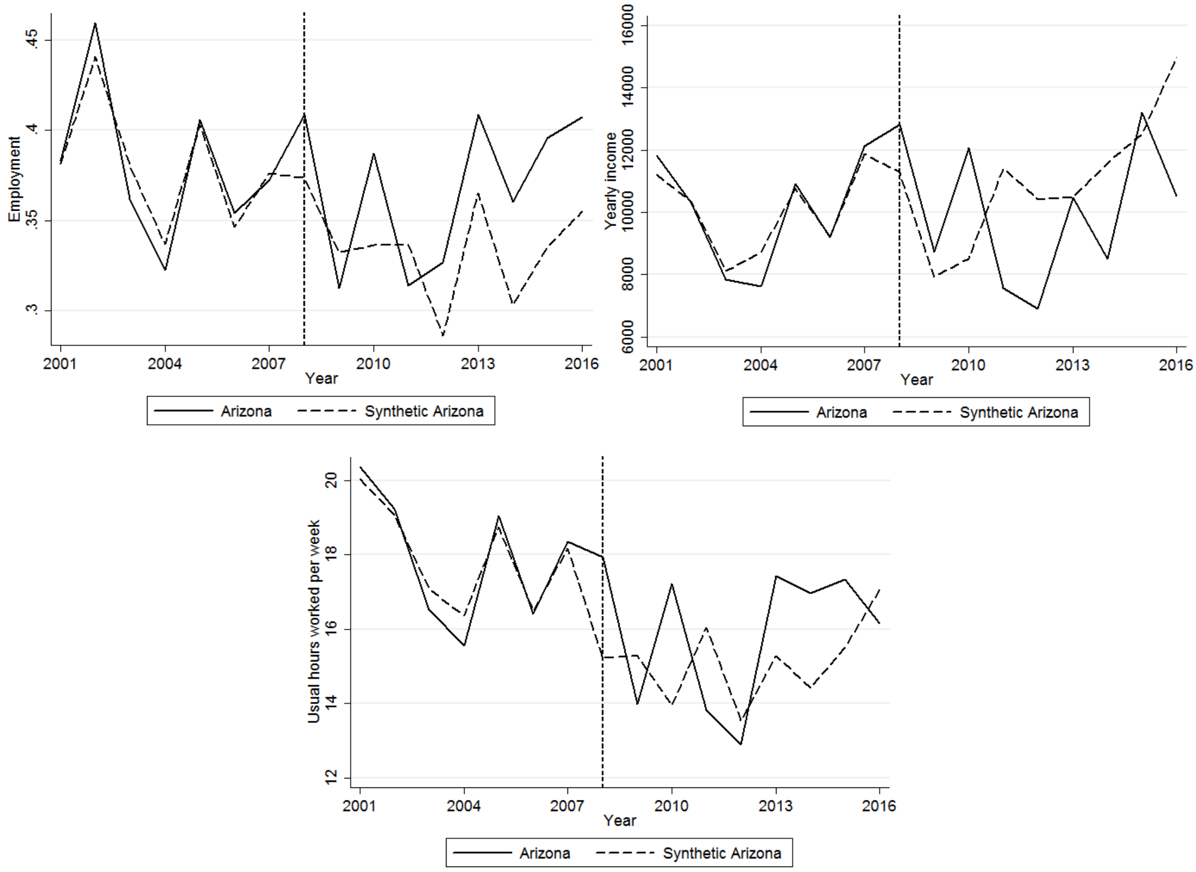


Notes: Total donor pool consisted of 19 states, dropped if the observations are less than 400 per year

A6. Difference-in-difference estimates from SCM (p-values in parenthesis)

	Hispanic low-educated citizens			
	2009	*2010*	*2011*	*Average*
	-0.0941	-0.1323	-0.0995	-0.0693
Employment	(0.1724)	(0.0862)	(0.1379)	(0.1552)
	-2802.335	-3396.892	-3491.150	-3403.886
Yearly reported income	(0.2154)	(0.3585)	(0.2308)	(0.2154)
	-0.5700	-3.3964	-4.0658	-1.0655
Usual hours worked	(0.3276)	(0.1552)	(0.1207)	(0.4138)
Average number of people in treatment group	399	399	399	399
N of permutations	57	57	57	57

A5. Non-Hispanic low-skilled population



Notes: Total donor pool consisted of 28 states, dropped if the observations are less than 400 per year

A7. Detailed robustness check - Difference-in-difference regression, all population

	Employment	Yearly income	Usual hours worked per week
yr06#arizona	-0.00269	32.58	-0.0280
	(0.00778)	(228.9)	(0.194)
yr08#arizona	-0.00503	10.39	-0.299
	(0.0126)	(373.5)	(0.199)
yr09#arizona	-0.0324**	-1,517***	-0.861*
	(0.0106)	(350.9)	(0.319)
yr10#arizona	-0.0265	-2,809**	-0.761*
	(0.0163)	(952.3)	(0.327)
yr11#arizona	-0.0312*	-4,484***	-0.696*
	(0.0144)	(838.8)	(0.302)
yr12#arizona	-0.0240**	-3,447***	-0.787**
	(0.00718)	(566.6)	(0.215)
yr13#arizona	-0.0309*	-3,472***	-0.510**
	(0.0122)	(425.7)	(0.121)
yr14#arizona	-0.0157*	-4,129***	-0.260
	(0.00600)	(337.9)	(0.163)
yr15#arizona	-0.00862	-4,372***	0.106
	(0.00761)	(807.8)	(0.260)
yr16#arizona	-0.00501	-4,984***	0.195
	(0.0101)	(962.5)	(0.380)
R-squared	0.986	0.993	0.977

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A8. Detailed robustness check - Difference-in-difference regression, low-educated Hispanic naturalized, citizens and non-citizens

	Employment	Yearly income	Usual hours worked per week
yr06#arizona	-0.000669	-313.4	0.429
	(0.0367)	(547.2)	(1.877)
yr08#arizona	-0.0219	257.0	-2.383
	(0.0406)	(1,165)	(1.504)
yr09#arizona	-0.117***	-2,965	-3.627***
	(0.0401)	(2,726)	(1.047)
yr10#arizona	-0.0670**	-3,924	-3.818***
	(0.0305)	(2,570)	(1.044)
yr11#arizona	-0.108***	-2,821**	-3.982***
	(0.0211)	(1,045)	(1.198)
yr12#arizona	-0.0873***	-4,758**	-2.346**
	(0.0302)	(2,286)	(0.980)
yr13#arizona	-0.0373	-7,714***	-2.745**
	(0.0625)	(2,340)	(1.217)
yr14#arizona	0.0190	-5,163***	-2.750***
	(0.0790)	(1,477)	(0.743)
yr15#arizona	-0.0120	-4,994**	-1.956
	(0.0392)	(2,141)	(1.366)
yr16#arizona	-0.0838**	-4,676**	-3.412***
	(0.0386)	(1,976)	(1.039)
R-squared	0.474	0.454	0.746

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A9. Additional robustness check - Difference-in-difference regression, Hispanic low-educated citizens

	Employment	Yearly income	Usual hours worked per week
yr06#arizona	0.0000	-2.416	0.676
	(0.0472)	(488.1)	(0.910)
yr08#arizona	-0.0406	-1,010	-0.647
	(0.0393)	(1,024)	(0.696)
yr09#arizona	-0.0941**	-2,803	-0.965
	(0.0379)	(1,775)	(1.586)
yr10#arizona	-0.132***	-3,405*	-3.791*
	(0.0297)	(1,841)	(1.826)
yr11#arizona	-0.0994	-3,492***	-4.460*
	(0.0689)	(780.6)	(2.004)
yr12#arizona	-0.0214	-3,512	-0.626
	(0.0344)	(2,667)	(0.948)
yr13#arizona	-0.0912**	-5,021	-1.808
	(0.0430)	(3,072)	(1.371)
yr14#arizona	-0.0514	-3,705***	-1.184
	(0.0628)	(1,060)	(2.032)
yr15#arizona	-0.0794**	-6,244*	-0.376
	(0.0365)	(3,333)	(1.756)
yr16#arizona	-0.0135	-1,391*	0.715
R-squared	0.764	0.592	0.855

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A9. Additional robustness check – Difference-in-difference regression, low-educated non-Hispanics

	Employment	Yearly income	Usual hours worked per week
yr06#arizona	0.00977	41.23	-0.0185
	(0.0317)	(761.8)	(1.758)
yr08#arizona	0.0373**	1,603**	2.774
	(0.0117)	(621.5)	(1.580)
yr09#arizona	-0.0178	861.4	-1.238
	(0.0130)	(1,963)	(1.011)
yr10#arizona	0.0532***	3,622***	3.331**
	(0.00601)	(789.4)	(1.341)
yr11#arizona	-0.0200	-3,789***	-2.173
	(0.0199)	(667.4)	(1.645)
yr12#arizona	0.0427	-3,448***	-0.598
	(0.0502)	(712.7)	(0.959)
yr13#arizona	0.0459**	53.65	2.203
	(0.0179)	(1,387)	(1.644)
yr14#arizona	0.0596*	-3,014**	2.602**
	(0.0288)	(831.9)	(0.935)
yr15#arizona	0.0625	742.9	1.861*
	(0.0447)	(634.4)	(0.764)
yr16#arizona	0.0542*	-4,391	-0.846
	(0.0253)	(2,904)	(1.597)
R-squared	0.901	0.692	0.826

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A10. Additional robustness check - Close states