Highlights

Negative Externalities of America's Toughest Immigration Law: Not So Sweet Home Alabama

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- H.B.56 caused the percent of the population that is foreign born to fall in Alabama.
- The law reduced Alabama GDP by around $600 million over the period 2011-2014.
- The law is meant to affect undocumented immigrants but affects documented immigrants.

Negative Externalities of America's Toughest Immigration Law: Not So Sweet Home Alabama

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Abstract

Alabama's 2011 immigration law (H.B. 56) significantly limits the economic opportunities of undocumented immigrants and intensifies their prosecution. Using American Community Survey (ACS) data, we test whether the law has led to an unintended reduction of the foreign-born legal resident population of the state due to their connections to undocumented immigrants. Using synthetic control, we estimate a 16% drop in Alabama's immigrant population during the 2011-2014 period of study. This suggests that Alabama has forgone around 600 million USD in total expenditures, including up to $50 Million in forgone state tax revenue over 2011-2014.

Keywords: Alabama, H.B.56, Immigration Policy, Synthetic Control

⋆Declarations of interest: none

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1. Introduction

We examine the demographic and economic effects of the passage of a 2011 Alabama law, H.B.56. This legislation is designed to encourage self-deportation of undocumented immigrants from the state as well as to discourage them from moving to Alabama in the first place. This paper is the first of its kind to use synthetic control to measure the effect, of an immigration policy which targets undocumented immigrants, on the decisions of documented immigrants about where to live.

From 2010 to 2014, according to the United States American Community Survey (ACS), the total immigrant population of Alabama fell by 11,240 individuals, from 168,596 to 157,356 during a time when almost every single other US state's foreign born populations increased in absolute size (United States Census Bureau, 2015).

These numbers by themselves hint at an underlying story, but are not proof of any effect of H.B.56 on the settlement decisions of immigrant populations. We use synthetic control (SC) estimation to test the idea that H.B.56 has indirectly created an "immigrant deficit", the difference between Alabama's actual immigrant population and that of the counterfactual in which H.B.56 was never instated. We reject the null hypothesis that the law has had no effect, which is consistent with the theory that the immigration law has put downward pressure on Alabama's immigrant population.

Our estimates suggest that H.B.56 has reduced (whether through increased out-migration and/or decreased in-migration) the percentage of the Alabama population that is foreign born non-citizens (PPFBN) by .182 percentage points (pp) on average in treatment years, or around .72 pp cumu-

latively over the period of study. Alabama's PPFBN was 2.11% in 2014. This suggests that H.B.56 has reduced the non-citizen immigrant population in Alabama by around 25% over the 2011-2014 period. Based on the proportions of immigrants who are citizens as opposed to non-citizens, this translates to around a 16% drop in the overall immigrant population in Alabama over the treatment period. Our conservative estimates suggest that H.B.56 has lead to a reduction of the total income earned by Alabamans by half a billion USD over the 2011-2014 period, and decreased Alabama state tax revenues by up to $30 million USD during the same period.

These results contribute to the literature concerning immigration and immigration policy. Ours is the first paper to use synthetic control to identify a link between anti undocumented immigrant policies and a reduction in the population of documented immigrants who would not be directly affected by such a policy. The results are important for policy debate concerning immigration. The contentious climate surrounding immigration policy in the US, particularly concerning immigration from Mexico and other south and central american countries highlights the importance of immigration policy and its externalities for policy makers and voters. The results are pertinent for the debate over whether to increase enforcement against undocumented immigration at the southern border, as this could affect the population of documented immigrants and their choices of where to live.

Hereafter, "undocumented immigrants" refers to individuals living in the US without proper authorization from the government, and "documented immigrants" refers to foreign-born people with US citizenship, a US green card, or any other official acknowledgement of that individual's authorization to

live and/or work in the US. A state's population of documented immigrants added to its population of undocumented immigrants equals the state's total immigrant population. In the interest of brevity, we will use the placeholders PPFB, PPFBC, and PPFBN to refer to; the percentage of a state's population that is foreign born, the percentage of a state's population made up of foreign born US citizens, and the percentage of a state's population consisting of foreign born non-citizens, respectively (PPFB = PPFBC+PPFBN).

2. Background Information

Anecdotally, many Hispanic Alabamans have reported experiencing increased suspicion and race-based discrimination since H.B.56 was passed (Sarlin, 2013; Constable, 2012). Hispanic children have been victims of increased intimidation and bullying at school, leading to decreased attendance (Struckman, 2011; Weishar, 2011). This threatening atmosphere appears to have caused many documented immigrants to leave Alabama in order to work, live, and stay connected to undocumented friends and family members more directly affected by the law (National Immigration Law Center, 2012; Post, 2012).

US national immigration policy has shifted along the spectrum from open immigration, to completely closing borders for particular groups of people, both during peace and times of war (Rowen, 2016). There were 41.3 million immigrants living in the US in 2013, and around 80 million people in the US were first or second generation residents and/or citizens (Zong and Batalova, 2015). Immigration policy directly and indirectly affects millions of people within the US. Around a million people gained US legal permanent

resident status each year from 2000 - 2013. Over half a million undocumented immigrants were apprehended each year during the same period, and an estimated 11.4 million undocumented persons resided in the US in 2011 (US Department of Homeland Securty, 2012, 2014). Immigration policy can have significant positive or negative effects on immigrants who move to the US. Asylum seekers can theoretically benefit from preferential status in immigrating. Once immigrants attain legal status in the US they may even have an incentive to "pull the ladder up behind themselves" and to push for more strict immigration standards aimed at reducing further immigration, and hence competition for employment.

Even before the passage of H.B.56 or laws like it, life for both documented and undocumented immigrants could oftentimes be difficult and uncertain. The Southern Poverty Law Center writes that;

> *"They [Latinos in the South] are routinely cheated out of their earnings and denied basic health and safety precautions. They are regularly subjected to racial profiling and harassment by law enforcement. They are victimized by criminals who know they are reluctant to report attacks. And they are frequently forced to prove themselves innocent of immigration violations, regardless of their legal status." (Southern Poverty Law Center, 2009)*

Bohn et al. (2014) analyze the Legal Arizona Workers Act (LAWA), also known as SB 1070, using synthetic control to compare levels of likely undocumented workers between Arizona and synthetic Arizona. The state legislature passed LAWA in 2007, requiring all employers to verify new hires' authorization to work in the US using the E-Verify document verification

system. This legal change created harsh penalties for employers who don't use E-Verify in their hiring procedures and also for those who hire undocumented immigrants, thus erecting significant barriers to undocumented persons acquiring work in Arizona. The authors find that the percentage of the population which is statistically more likely to be undocumented immigrants (working age, non-college-educated Hispanic males) saw a significant decline in the wake of LAWA's passage, suggesting self deportation from and/or reduced in-migration to the state of that segment of the population (Bohn et al., 2014). Authors Hoekstra and Orozco-Aleman write in their 2017 paper about the effects of SB 1070 on Mexican immigration to Arizona. Through using a novel dataset containing survey responses of US-bound Mexican migrants, they find compelling evidence suggesting that the passage of LAWA reduced the rate of undocumented workers moving to Arizona by 30 to 70 percent. Their work, like ours, suggests a significant effect of state immigration policy on immigrants' decisions of where to live in the US. While these papers help to uncover evidence of an immigration policy's effects on *undocumented* immigrants, we are the first authors to measure the effects of tough immigration policy on the decisions of *documented* immigrants about where to live in the US.

Zhang et al. (2016) measure the effects that H.B.56's implementation has on crime in Alabama. They look particularly at the effect that the law has on violent and property crime rates in the state. They found that the law caused an increase in violent crime, while having a slightly negative or zero effect on property crime rates (Zhang et al., 2016).

3. H.B.56 as the Topic of this Analysis

H.B.56 is widely regarded as the "toughest" anti-illegal immigration law in the US. The legislation was introduced by Representative Micky Hammon on the first of March 2011, passed in June, and went into effect on September first of that year (National Conference of State Legislators, 2012). The rapidity with which the law was introduced and passed limits the possibility that individuals were able to anticipate its implementation and pre-adjust behaviors accordingly.

H.B.56, or The Beason-Hammon Alabama Taxpayer and Citizen Protection Act mandates that:

1. Law enforcement personnel must make an effort to determine the legal status of anyone reasonably suspected of living in the US without authorization.
2. Undocumented immigrants are barred from receiving any public benefits at the state or local level.
3. Public schools must find out which students and parents are undocumented immigrants, and provide the information to the state.
4. Landlords aren't allowed to rent to known undocumented immigrants.
5. Employers aren't allowed to knowingly hire undocumented immigrants, and are required to use the E-Verify document verification system to screen potential employees.
6. Any contract(s) entered into, between an undocumented immigrant and any party aware of their undocumented status, is null and void.

Subsequent legislation, H.B.658, which passed on May 18th of 2012, amended 13 of the 34 sections of H.B.56. Among the changes to H.B.56,

the newer legislation left in place provisions 1-6 above, as well as creating a requirement that courts record and publish information about undocumented immigrants who have appeared before them (National Conference of State Legislators, 2012). Throughout its legislative life, H.B.56 was widely opposed by civil and immigrant rights proponents, as well as being challenged by the US departments of Homeland Security, Justice, State, and Education. The constitutionality of many provisions of the law were challenged in the case of United States v. Alabama and Governor Robert J. Bentley. The controversial nature of the laws stems largely from the fear of significant negative effects on the lives of already-marginalized Alabama residents. The US Department of Justice explained in a letter to the state of Alabama that H.B.56 has created a hostile climate at schools for Hispanic children as well as having caused large increases in absenteeism among the same population (Post, 2012).

The Center for Business and Economic Research at the University of Alabama performed an H.B.56 cost-benefit analysis. They estimate that, instead of helping Alabama's fiscal situation and increasing statewide economic activity, H.B.56 has had a significant negative impact on taxes collected, increased immigration enforcement costs, and reduced economic output and business prospects for Alabama (Addy, 2012). Many businesses reported labor shortages due to much of their labor forces leaving the state or being afraid to show up for work for fear of apprehension and deportation (Constable, 2012; Rawls, 2011). Even documented US immigrants, particularly Hispanic individuals, have reported feelings of being unwelcome in Alabama after H.B.56 was passed (Rawls, 2011). Many of these documented immi-

grants have quit long-term careers and left Alabama to avoid being indirectly affected by H.B.56 or in order to stay connected to friends and family who may be in the US illegally and who have been forced to leave the state (Constable, 2012).

4. Methodology

4.1. Data Source(s)

The data used in our state-level analysis comes from the the American Community Survey (ACS), carried out by the United States Census Bureau (United States Census Bureau, 2015). This extensive data set is a representative random sample of the US population carried out in every state in the US, each year. The ACS doesn't differentiate between documented and undocumented individuals. We do not assume that all respondents are documented immigrants even though it seems reasonable to do so, given the significant apprehension that many undocumented immigrants feel about interacting with official government representatives. We assume that the proportion of immigrant respondents who are documented immigrants, and so should be included in our analysis, is equal to 72%. This comes from a 2010 estimate of the overall proportion of US immigrants classified as undocumented (Jeffrey Passel, 2017; Steven Camarota, 2017). The ACS data include overall population, employment levels, distribution of national origins, and immigration status, among other useful statistics. From these data, we construct our variables of interest such as the percentage of each state's population that is foreign-born, etc. All of these data are freely accessible on the internet.

4.2. Exploratory Difference In Differences Estimation

We use Difference in Differences (DID) estimations to explore the possible existence of a treatment effect, and to motivate the usage of synthetic control estimation. This technique can be used to find the treatment effect of a policy on a population, when at least one usable counterfactual control group exists for the comparison. The best counterfactual would be the observation of the treated unit in a theoretical time line in which the same unit, ceteris paribus, did not receive the treatment (passage of H.B.56). This is clearly an impossible exercise and so we must settle for the next best thing, a control unit as similar to the treatment unit as possible, but which does not receive the treatment. Consistency of DID estimation requires the assumption that the outcome variable of choice, PPFB, in both treatment and control groups, in the absence of any treatment, would have followed parallel time trends in the absence of the treatment. In our case, the treatment group is the state of Alabama, with the control states being Arkansas, Iowa, North Dakota, and Ohio. The control states chosen are picked through a process of first eliminating all states which have passed some sort of similar anti-undocumented-immigration law(s) since 2005. Then we eliminate any Alabama border states to avoid double-counting immigrants who might have "spilled-over" Alabama's borders into an adjacent state. After this we eliminate Atlantic and Pacific ocean coastal states as well as Alaska, Hawaii, and Puerto Rico, as they are all quite culturally and geographically different from Alabama and are not likely to be suitable control states. We also eliminate any states which have PPFB relatively far from Alabama's levels. For instance, California's PPFB hovers around 30% during the period of study,

as opposed to Alabama's, which is around 4%. The end result is a group of counterfactual states which are relatively geographically and economically similar to Alabama. A map of the geographical dispersion of the control states is shown in figure one.

The parallel trends assumption is inherently difficult to prove valid. However, a visual inspection of the data on the main outcome variable reveals that the trends in this variable are very similar between Alabama and the chosen control states, when compared with the rest of the US. Figure two shows this similarity. All of the control states show generally upward trends of PPFB before and after 2011. Alabama on the other hand shows a similar upward trend before 2011, followed by a downward trend instead of rebounding like the others.

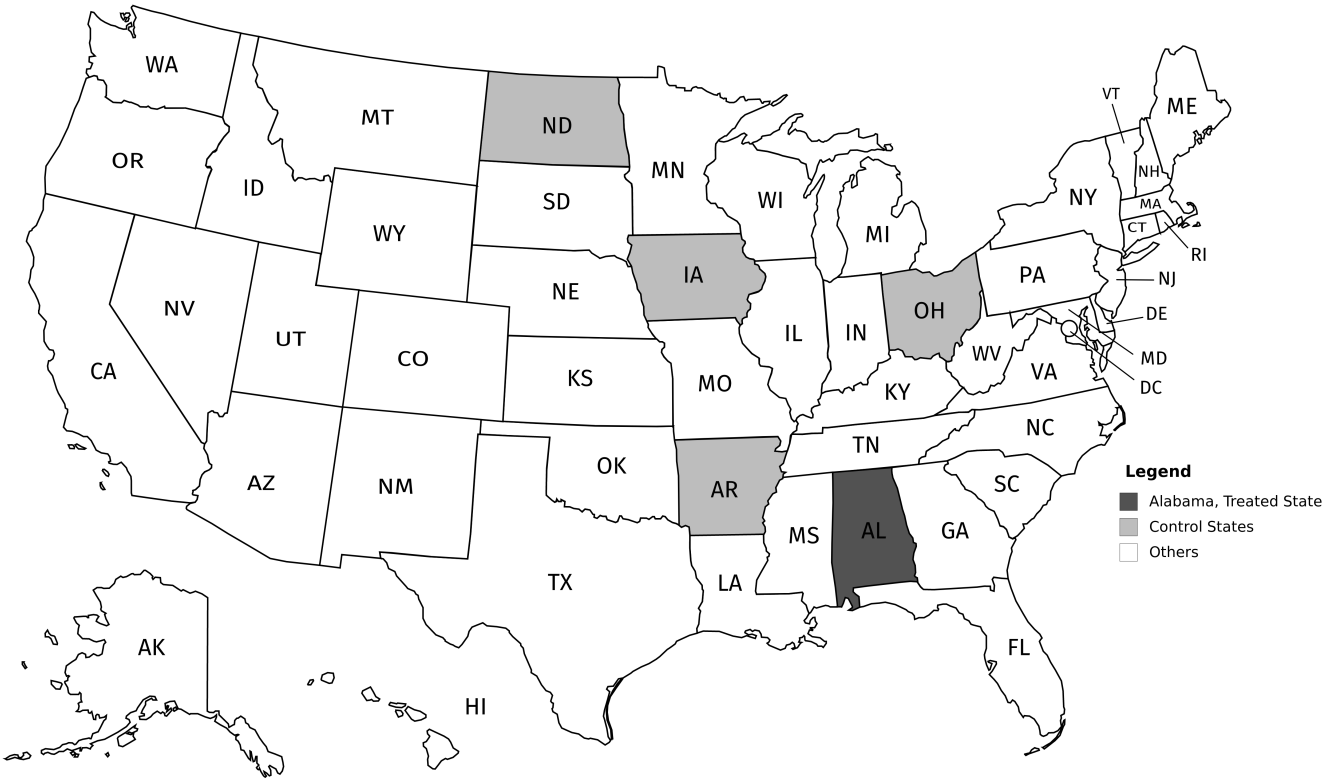

Figure 1: Map of US Treatment and Control States used for Difference In Differences Estimation

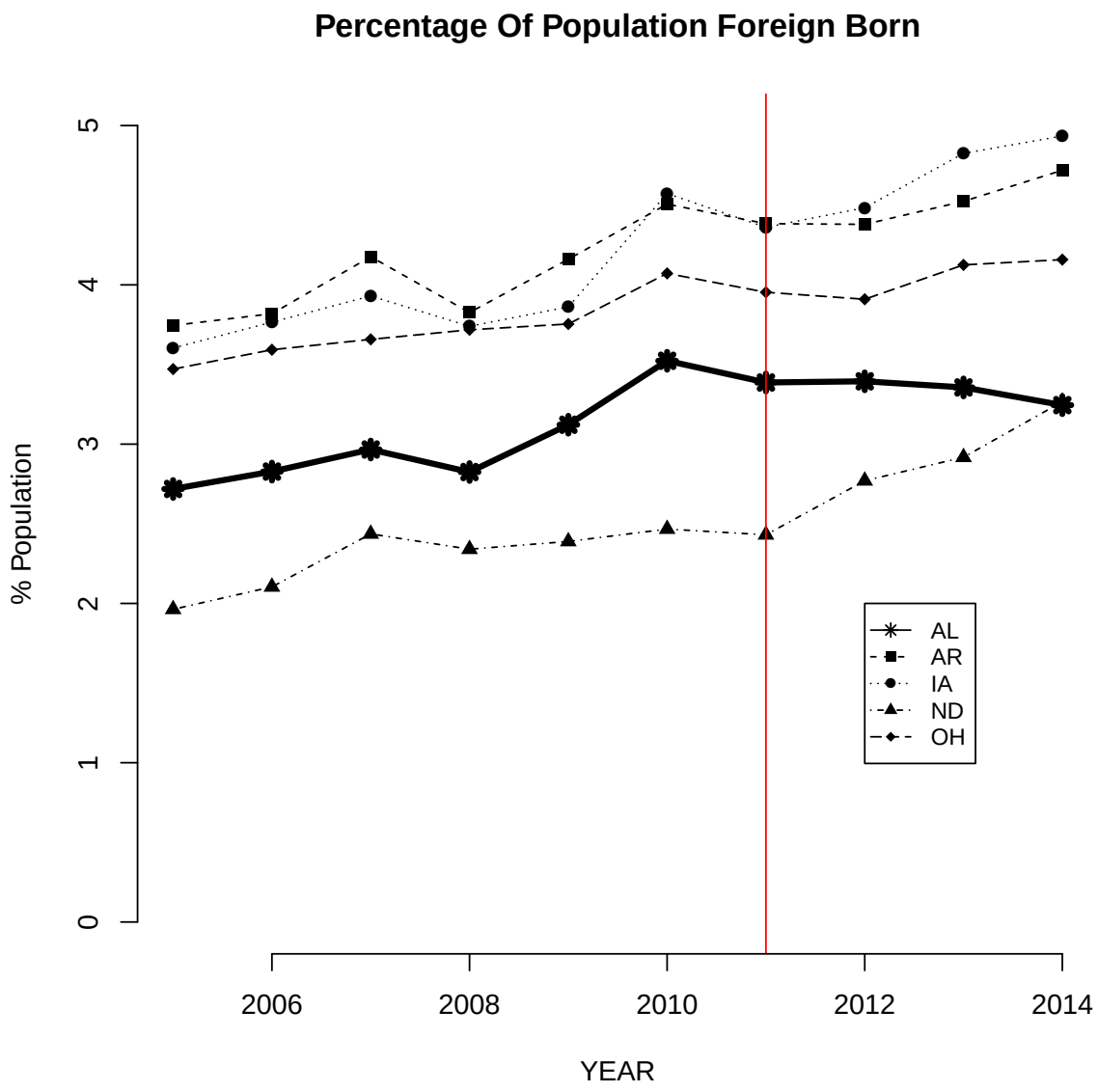

Figure 2: Parallel Trends Shown for the Percentage of the Population Foreign Born in Alabama and Control States

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Notes: Here we show the evolution of the Percent of the Population that is Foreign Born (PPFB) during the 2011-2014 study period for our treated state Alabama, and the Difference in Differences counterfactual states; Arkansas, Indiana, Ohio, and North Dakota.

The DID estimator $\hat{\theta}$ is used to estimate the average treatment effect of enacting H.B.56, and is defined as follows:

$$\hat{\theta} = \{\mathbb{E}[Y|D=1,t=1] - \mathbb{E}[Y|D=1,t=0]\} - \{\mathbb{E}[Y|D=0,t=1] - \mathbb{E}[Y|D=0,t=0]\}$$

- Y is the percent of the state population made up of immigrants
- D is a dummy variable for treatment which equals one for Alabama, and zero for each of the control states
- t indicates the time period and equals zero for pre-treatment (2005-2010), and one for post-treatment (2011-2014)

In order to consistently estimate the parameter of interest we must assume that the parallel trends assumption holds; that in the absence of treatment, the variable of interest would have followed the same path (at different levels) in both the treatment and control units. In order to implement the DID procedure, one can calculate the difference in the outcome variable of the treatment group (Alabama) before and after the treatment, and compare that with the same difference for the control group(s).

It is also possible to use a regression approach in order to estimate the parameter of interest, while controlling for time and/or state-invariant factors which might affect the PPFB. We use a number of different regression models, generally of the form:

$$y_{it} = \gamma + \lambda_t + \alpha_i + X_{it} + \delta D_{it} + \epsilon_{it}$$

Here the parameter of interest is δ, while i represents the state and, t represents the year. y_{it} is the PPFB. λ_t and α_i represent year and state fixed-effects respectively. D_{it} is an indicator for a treatment state and time

interaction. X_{it} represents control variables including each state's total population, population density, median household income, unemployment rate, GINI coefficient of income inequality, and the percent of workers who work in the agricultural, forestry, fishing, hunting, and mining sectors. ϵ_{it} is the exogenous error term.

4.3. Synthetic Control Methodology

Difference in differences is used as a way to examine the relevant data for prima facie evidence that there may be a relationship of interest. Based on this initial look, there does appear to be a strong correlation between the outcome variables in question and the passage of H.B.56. However, in this case there is only one treatment unit, and 4 control units. In order to credibly use DID as the main method of analysis, one would need many (in the $N > 30$ sense) treated and control units. Instead of having many units to analyze, one could find a truly appropriate counterfactual for Alabama (a state which is in every way like Alabama except which didn't pass H.B.56 or anything like it), then a simple DID with one treated and one control unit could be used to analyze the difference between Alabama and counterfactual Alabama. This counterfactual would need to be one for which the parallel trends assumption holds with Alabama. There doesn't exist another US state, or any other region in the world which plausibly fits this description. However, all is not lost because we have a data-driven way of creating this plausible counterfactual.

The synthetic control estimation technique was introduced by Abadie et al. in 2003. The authors examined the effect of Basque country nationalist terrorism (terrorist attacks is the treatment) on GDP growth in the Basque

region of Spain. No plausible counterfactual exists for the Basque region of Spain. This theoretical counterfactual would essentially look just like the Basque region, except without having experienced the campaign of terrorist attacks that the real region did. In order to deal with this deficiency, the authors used the data to construct a counterfactual of this nature (Abadie and Gardeazabal, 2003).

They accomplished this through weighting the other provinces of Spain in a way that closely matched actual Basque region GDP/capita growth data and data on the predictors of GDP/capita during the pre-treatment period before the Basque nationalist terrorist attacks. In this way they created a synthetic Basque Country region. This data-driven method of creating a counterfactual leaves the authors with a plausibly useful comparison group where parallel trends closely holds by construction. Synthetic control is a useful technique for evaluating a change in one jurisdiction, when one has access to a group of control jurisdictions (here, regions within the same country) but none of the other regions is particularly well suited to being "the best" counterfactual or even a plausibly usable counterfactual.

What follows is a brief description of what the synthetic control software package is doing behind-the-scenes in our context.

The overall goal of synthetic control is to create one control unit from a convex combination of multiple control units. The created synthetic control unit is made in a way so that it, as closely as possible, replicates the outcome-relevant pre-treatment characteristics of the treated unit. The post-treatment-year outcomes for the treated unit and the synthetic control unit are then compared visually.

We define the weights which will eventually be applied to some set of the control states as w_j, $j \in [1, ..., J]$. Let $\vec{U_j}$ be a vector of pre-treatment averages over time of covariates that predict the outcome of interest for state j. $\overline{Z_j^r}$ is the r'th such average, $r \in [1, 2, ..., R]$, for the j'th control state. $j = 1$ for the treated state (Alabama). And so, $\vec{U_j} = (\overline{Z_j^1}, \overline{Z_j^2}, ..., \overline{Z_j^R})'$. For instance the current analysis includes in each $\vec{U_j}$ the over-time average gini coefficient, percentage of the population identifying as hispanic, population density, and percentage of workers in "rural" professions for a state.

$\bar{Y}_j^{K_m} \; \forall \; m \in M$ are M linear combinations over time of pre-treatment outcomes (PPFB, PPFBC, PPFBN). $\bar{Y}_j^{K_m}$ doesn't have to be an equally-weighted average, but it often is, as in the case of our analyses. We ideally would like for $\sum_{j=2}^{J+1} w_j * \bar{Y}_j^{K_1} = \bar{Y}_1^{K_1}$,..., $\sum_{j=2}^{J+1} w_j * \bar{Y}_j^{K_M} = \bar{Y}_1^{K_M}$, and $\sum_{j=2}^{J+1} w_j * \vec{U_j} = \vec{U_1}$ to all hold with equality in order to find a "perfect" counterfactual for which parallel trends would hold perfectly between it and the real treated unit. The estimator that we are trying to construct takes the form $\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$ which gives us an estimate of the difference between the outcomes for the treated unit and its synthetic control unit in each post-treatment time period. The characteristic data vector for each state is a vector which stacks the $\overline{Z_j^r}$ pre-treatment averages on top of the $\bar{Y}_j^{K_m}$linear combinations.

It is unlikely that these equations will ever all hold with equality. Instead, we choose a vector of control unit weights $\vec{W^*} = (w_{2,}^* w_3^*, ..., w_{J+1,}^*)'$ such that $w_j^* \geq 0$, for $j = 2, ..., J + 1$. $\vec{W^*}$ represents the linear combination of the control units making up the synthetic control unit. X_0 is a matrix containing the characteristic data of all non zero-weighted control units. X_1 is a vector

containing the characteristic data of the treated unit.

$$X_0 = \begin{bmatrix} \overline{Z_2^1} & \overline{Z_3^1} & ... & \overline{Z_{J+1}^1} \\ \vdots & \vdots & \ddots & \vdots \\ \overline{Z_2^R} & \overline{Z_3^R} & \ddots & \overline{Z_{J+1}^R} \\ & & & \\ \overline{Y}_2^{K_1} & \overline{Y}_3^{K_1} & \ddots & \overline{Y}_{J+1}^{K_1} \\ \vdots & \vdots & \ddots & \vdots \\ \overline{Y}_2^{K_M} & \overline{Y}_3^{K_M} & \cdots & \overline{Y}_{J+1}^{K_M} \end{bmatrix} \quad while \quad X_1 = \begin{bmatrix} \overline{Z_1^1} \\ \vdots \\ \overline{Z_1^R} \\ \\ \overline{Y}_1^{K_1} \\ \vdots \\ \overline{Y}_1^{K_M} \end{bmatrix}$$

The solution vector of weights $\vec{W}^*$ is chosen to minimize the distance

$$||X_1 - X_0\vec{W}||_v = \sqrt{(X_1 - X_0\vec{W})'\mathbf{V}(X_1 - X_0\vec{W})}$$

between the treatment unit's characteristic data vector X_1 and a weighted average of the characteristic data vectors of the control units $X_0\vec{W}$.

V is a positive definite diagonal matrix, and V^* is chosen from the set of all possible V to minimize the Mean Squared Prediction Error (MSPE) of the synthetic control unit's outcome variable, in relation to the treatment unit's actual outcomes for the pre-treatment years. As long as $J \leq (r + M) * T$ then we have more equations/observations than unknowns. J=34 states, r= 4 covariates, M= 1 linear combination (mean), T=11 years, $34 \leq ([4+1]*11)$. It is standard practice to then show the results of these types of estimations by plotting the path of the treated unit's outcomes against its synthetic control unit's counterfactual outcomes.

As per the filtering process described in Abadie et al. (2015), we remove

control states which are possibly affected by the policy change in question or which have implemented immigration policies similar to H.B.56 during or after 2005 and so are not acceptable control states in that regard (Abadie et al., 2015). Any states which border Alabama are removed from the control state pool, as they are more likely to be directly affected by this policy through migration from Alabama. States which have implemented the mandatory usage of E-verify in employment decisions are also removed. This leaves us 34 control states to choose from when assigning the weights for synthetic Alabama's creation. The weights assigned to control states which make up the synthetic control version of Alabama, for each of the three outcome variables of interest, are shown in table one through table three. The inclusion of Arkansas and Ohio in the synthetic control weighting increases confidence that they are appropriate controls in the DID setting.

Table 1: PPFB Synthetic Alabama Weights

WEIGHTS	STATE
0.147	Arkansas
0.762	Kentucky
0.003	Maine
0.002	Michigan
0.081	Ohio

4.4. Potential Pitfalls

If there are spillover effects of the law on control states, through immigrants moving to border states for instance, then the two estimation approaches won't be only capturing the treatment effect of the passage of H.B.56. Since none of the control states border Alabama, the magnitude

Table 2: PPFBC Synthetic Alabama Weights

WEIGHTS	STATE
0.087	Arkansas
0.001	Colorado
0.001	Connecticut
0.001	Delaware
0.002	Idaho
0.001	Illinois
0.002	Iowa
0.871	Kentucky
0.002	Maine
0.001	Massachusetts
0.002	Michigan
0.002	Minnesota
0.002	Montana
0.002	Nebraska
0.001	Nevada
0.001	New Hampshire
0.001	New Mexico
0.001	New York
0.002	North Dakota
0.002	Ohio
0.002	Oklahoma
0.002	Oregon
0.002	Pennsylvania
0.002	South Dakota
0.001	Texas
0.002	Vermont
0.001	Virginia
0.001	Washington
0.002	Wisconsin
0.002	Wyoming

Table 3: PPFBN Synthetic Alabama Weights

WEIGHTS	STATE
0.253	Arkansas
0.595	Kentucky
0.152	Ohio

of any spillovers which could exist is likely quite small, considering the magnitudes of our population change estimates.

The PPFB could have decreased in Alabama due to immigrants leaving the state, and the domestic-born population staying constant. Or if the number of immigrants in Alabama stayed constant, the PPFB could've decreased due to Alabama's non-immigrant population increasing. Or, if both populations were increasing (decreasing), the domestic-born population increasing (decreasing) at a faster (slower) rate than the foreign-born population would also cause the PPFB to fall. However, the PPFB for Alabama fell due to an absolute decrease in the immigrant population from 2010 to 2014, compared to an absolute increase in the domestic-born population over the same period of time. The number of foreign-born individuals in Alabama decreased during a time period when the number of immigrants was increasing in almost all of the control states, as in most states in the US. The immigrant populations in the DID control states each increased by up to about 25,000 people over the period from 2010 to 2014, whereas in the excluded states of California and New York the immigrant populations increased by more than 350,000 and more than 150,000 people respectively over the same time period.

The ACS does not differentiate between immigrants who are documented

or undocumented. This could lead to the estimates reflecting changes in the number of undocumented immigrants in Alabama, instead of documented immigrants as we intend. However, due to the nature of H.B.56's provisions and US immigration policy even before H.B.56, it has been observed that undocumented immigrants are significantly less likely than documented immigrants to respond to surveys or interact with government representatives (Nill, 2011; Toomey, 2014; Southern Poverty Law Center, 2012). It seems reasonable then to assume that the respondents to the surveys each year are predominantly documented immigrants who are not afraid of their status being revealed to government agencies through participation in the ACS. To address this concern, we conservatively assume that the proportion of respondents that are undocumented is equal to the overall estimated proportion of US immigrants who are undocumented which was about 28% in 2011. Scaling our estimates of impacts by .72 should take the documented/undocumented ambiguity of ACS data into account (Motel and Patten, 2013; Hoefer et al., 2012). This 28% which represents the proportion of immigrants who are undocumented in the US is likely an upper-bound for the proportion of ACS respondents who might be undocumented. We expect that the proportion of respondents who are documented immigrants to be much closer to 100% due to the propensity for undocumented immigrants not to interact with official government representatives, out of fear of apprehension and deportation (Toomey, 2014).

5. Estimation Results

5.0.1. Difference In Differences Estimation Results

As can be seen from the parallel trends graph in Figure 2, as well as looking at the time trends for PPFB for the large majority of all other US states there seems to have been a general increase in the PPFB until around 2010 when the PPFB started to fall for most states. These drops in PPFB were likely due to the reversal of net positive immigration to the US brought about by the 2008 financial crisis along with increased law enforcement presence at the southern border (Gonzalez-Barrera, 2015). However, the trend seems to have returned to an upward march by 2011-2012 in almost all states, with one glaring exception, Alabama.

The OLS difference in differences models whose outputs are displayed below in table four include state fixed effects to account for attributes of each state which stay constant over time and year fixed effects to account for US-wide effects of being within each year.

While the effects of H.B.56 on PPFB and PPFBC are not statistically significant, the coefficient on PPFB is negative, suggesting that passage of the law was, at minimum, negatively correlated with the percentage of Alabama's population made up of immigrants. This coefficient remains negative and becomes statistically significant when larger groups of control states are included in unreported DID estimations. It appears that the negative sign on PPFB is being driven by the statistically significant reduction in PPFBN. The effect of H.B.56 on PPFBN is estimated to be a drop of .227 percentage points, significant at the 5% level. The PPFBN coefficient remains at a similar magnitude, consistently negative and statistically significant at either the

Table 4: Difference In Difference Results

	PPFB	PPFBC	PPFBN
	(1)	(2)	(3)
Alabama, Post Treat.	−0.174	0.053	−0.227**
	(0.109)	(0.069)	(0.096)
N	50	50	50
R^2	0.971	0.955	0.961
Adjusted R^2	0.960	0.937	0.945
Residual Std. Error (df = 35)	0.151	0.096	0.133
F Statistic (df = 14; 35)	84.774***	53.419***	61.471***
Notes:	***Significant at the 1 percent level.		
	**Significant at the 5 percent level.		
	*Significant at the 10 percent level.		

1% or 5% level when more control states are added, also in unreported DID estimations.

5.0.2. Synthetic Control Estimation Results

Difference in differences results suggest that we would benefit from further examination of the data with a technique more suited to the particular situation at hand. We use synthetic control to examine the same question of whether or not H.B.56 had a negative effect on Alabama's PPFB, PPFBC and PPFBN. The results of these exercises are reported in panel (a) of figures three, four, and five.

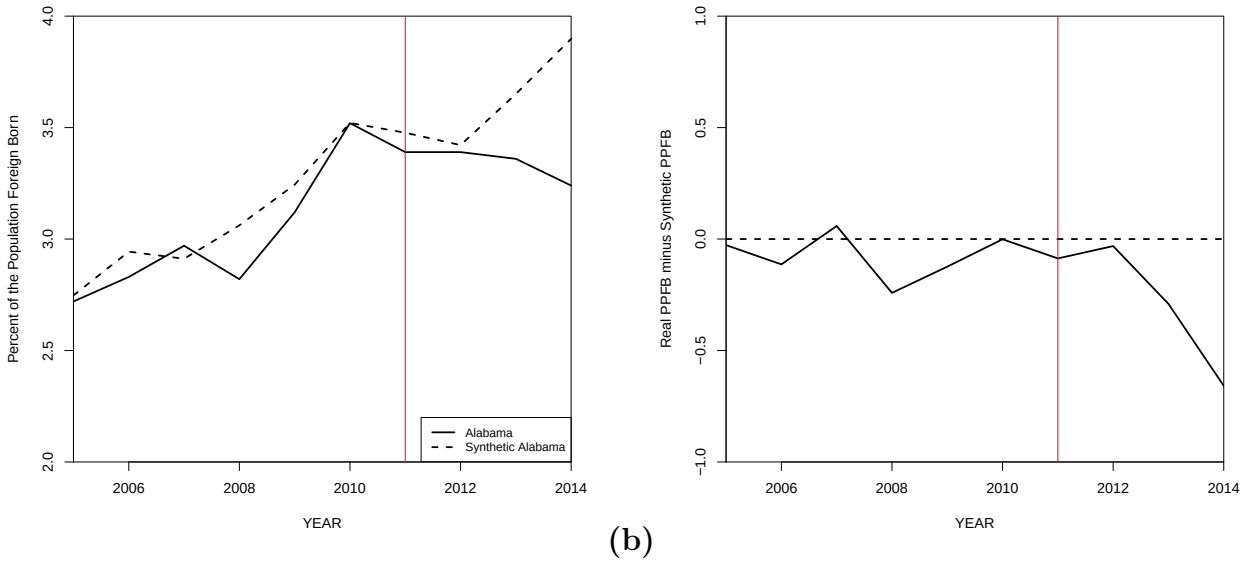
We see in panel (a) of figures three and five that there is a significant divergence between the outcome variables of concern for synthetic Alabama and real Alabama after 2011. Real Alabama's PPFB and PPFBN take a sharp downward turn away from their upward trends in synthetic Alabama.

Again we see in panel (a) of figure four that there appears to be a much smaller effect of the law on immigrant citizens. Most of the action in the movement of the immigrant population (PPFB) is coming from a strong effect on the immigrant non-citizens (PPFBN). The weights chosen for the US control states that make up synthetic Alabama in the estimations using the three different outcome variables of choice are reported in tables one through three.

The three "outcome gap graphs" in the panel (b) of figures three, four, and five show directly how much lower than synthetic Alabama that real Alabama's outcomes fall after 2011. They reveal that the gap between synthetic and real Alabama widens over time after 2011. The magnitude of the gaps in the case of immigrant citizens (PPFBC) are similar pre- and post-2011. Whereas, the gaps for immigrants overall (PPFB) and immigrant non-citizens (PPFBN) pre-2011 are quite small, and widen significantly after 2011. This leads us to believe that the effect on Alabama's immigrant population as a whole (PPFB) is coming mainly from H.B.56's impact on non-citizen immigrants.

To compare the magnitude of estimated effects between our synthetic control estimates and our DID estimates we use the synthetic control-created synthetic Alabama as a counterfactual for Alabama in a standard DID estimation. The results of this analysis are shown in table five. We find that the magnitude of effect is of a similar size in both synthetic control and standard DID estimates. However, the synthetic control-based coefficient on PPFBN is -.137 instead of the -.227 from our preliminary DID exercise. We use the smaller-magnitude, more conservative estimate in our back of the envelope

Figure 3: Real vs Synthetic Alabama, PPFB



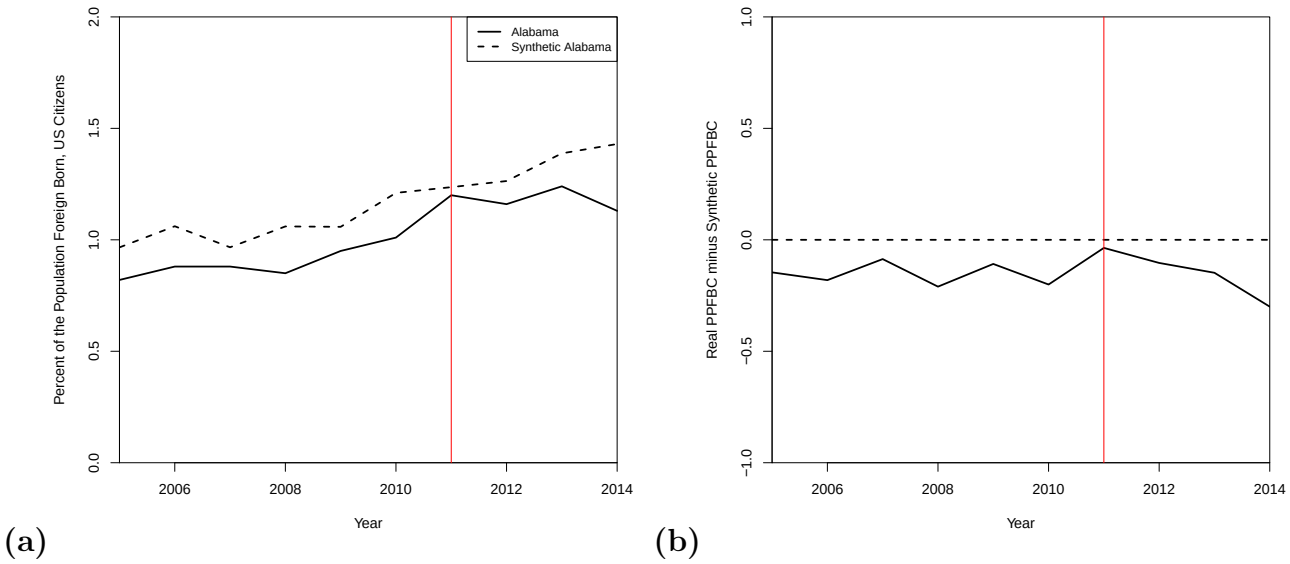
Notes: (a) Here we can see the divergence between the path of the percent of the population that is Foreign Born (PPFB) in Alabama and its synthetic Alabama counterfactual. (b) This graph shows the difference between real Alabama's PPFB and Synthetic Alabama's PPFB and how the magnitude of this difference increases over time.

calculations of economic impacts.

5.0.3. Synthetic Control Inference

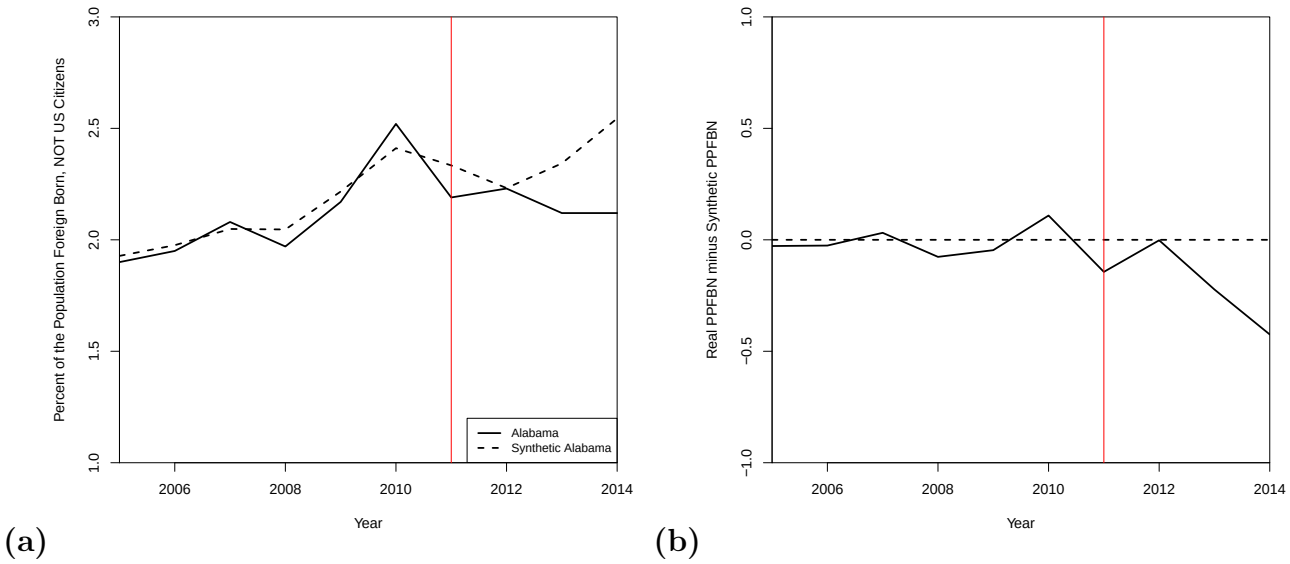
In order to reject the null hypothesis that there was no effect of H.B.56 on Alabama's documented immigrant population one needs a way to show that the relevant observed outcomes are not the result of random chance. The standard technique used in the synthetic control literature is one that uses the results of placebo tests and compares that to the results of our analysis of the actual treated state outcomes (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2011, 2015). The group of states used in these placebo

Figure 4: Real vs synthetic Alabama, PPFBC



Notes: (a) Here we do not see a large divergence between the path of the percent of the population that is Foreign Born Citizens (PPFBC) in Alabama and its synthetic Alabama counterfactual. (b) This graph shows the difference between real Alabama's PPFBC and Synthetic Alabama's PPFBC and how the magnitude of this difference doesn't change much over time.

Figure 5: Real vs synthetic Alabama, PPFBN



Notes: (a) Here we can see the divergence between the path of the percent of the population that is Foreign Born Non-Citizens (PPFBN) in Alabama and its synthetic Alabama counterfactual. (b) This graph shows the difference between real Alabama's PPFBN and Synthetic Alabama's PPFBN and how the magnitude of this difference increases over time.

Table 5: Difference in Difference Between Alabama and Synthetic Alabama

	Dependent variable:		
	PPFB	PPFBC	PPFBN
	(1)	(2)	(3)
Alabama, Post Treatment	−0.161	0.029	−0.182**
	(0.124)	(0.055)	(0.073)
Observations	21	21	21
R^2	0.919	0.944	0.913
Adjusted R^2	0.820	0.876	0.806
Residual Std. Error (df = 9)	0.139	0.062	0.082
F Statistic (df = 11; 9)	9.264***	13.787***	8.547***

*p<0.1; **p<0.05; ***p<0.01

Notes: Here, we have performed the same type of standard Difference in Differences estimation as in Table 4 except our Synthetic Control-created synthetic Alabama is now Alabama's counterfactual. This exercise helps us to compare the same coefficients between our Difference in Difference and Synthetic Control estimations. We see the magnitude of the main coefficient of interest (PPFBN) is slightly reduced but remains economically and statistically significant at the 5% level.

tests doesn't include the control states which were removed from the main synthetic control estimations. First, we perform a synthetic control analysis on each of the control states as if each were the treatment state instead of Alabama. Then the outcome variable(s) of each state's synthetic version is compared to its actual outcomes and the difference is plotted over time as in panel (b) of figures three, four, and five. One can then overlay all of the graphs to see whether or not the differences between synthetic and real Alabama are uncommonly large when compared to the differences between the real and synthetic versions of the control states. If the post-treatment gaps are significantly larger for Alabama than what is "normal" for the group, this would suggest that our outcomes of interest are the result of the legal change in question and not spurious correlations in the data. The results of this exercise for the three outcome variables of interest are presented in figures six, seven, and eight.

When creating our inferential "gaps graphs", we limit our comparison to between Alabama and the states for which the synthetic control procedure produces a pre-treatment Mean-Squared Prediction Error (MSPE) of up to 5 times the MSPE resulting from synthetic Alabama's creation, following the literature (Abadie and Gardeazabal, 2003). These are shown in panel (a) of figures six, seven, and eight. We extend this to also include states within 10 times the Alabama MSPE with little change in interpretation as shown below in panel (b) of figures six, seven, and eight. The MSPE for the creation of each synthetic control state gives us an easily comparable statistic which quantifies the extent to which the synthetic version of each state exactly copies the pre-treatment outcome trajectory of its real version. An MSPE of

exactly zero would imply that the synthetic control procedure has created a counterfactual that exactly matches the real state's pre-treatment outcome data.

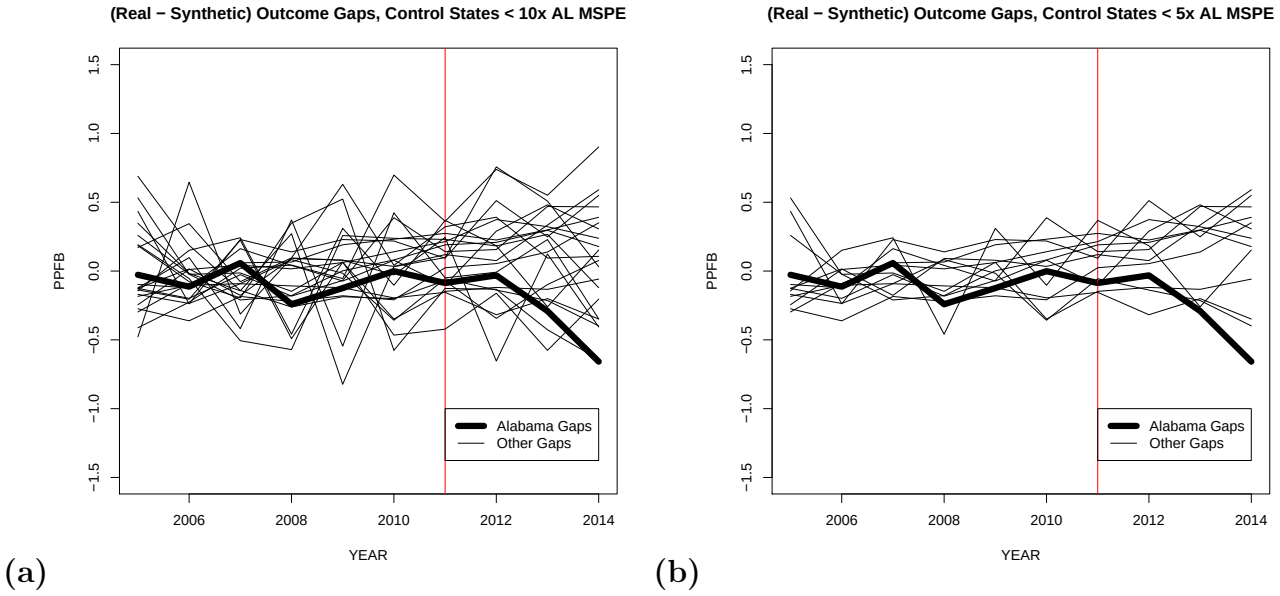
During the process of generating synthetic controls for all control states used as "faux-treated" placebos, we encountered some computational problems for small subsets of states (3 states in the PPFB estimations, 4 states for PPFBN, 1 state for PPFBC). Keeping or dropping the computationally problematic states doesn't affect inference in any significant way, and so we show the graphs without those states below.

We can see that Alabama's real-minus-synthetic-outcome gaps are bigger in absolute value than the mass of other states' real-minus-synthetic-outcome graphs. As time goes on, the Alabama gap gets larger and larger, suggesting an increased impact of the legislation over time.

5.1. Economic Impacts On Alabama

The gaps in the percentage of the population that is foreign born non-citizens between real and synthetic Alabama, multiplied by the population of Alabama in each post-treatment year gives us a rough estimate of the immigrant deficit caused by H.B.56. We multiply the immigrant deficit by the overall average income, and then the average Hispanic income in Alabama each year which gives us estimates for the income that the missing immigrants did not earn or spend in Alabama. Back-of-the-envelope calculations of the economic effect of this suggests that Alabama, assuming that the now-lost immigrants would have spent all of their income within the state, has forgone an average of $137,365,374 each year from 2011-2014 in economic expenditures by legal immigrants. This would suggest the state has forgone an

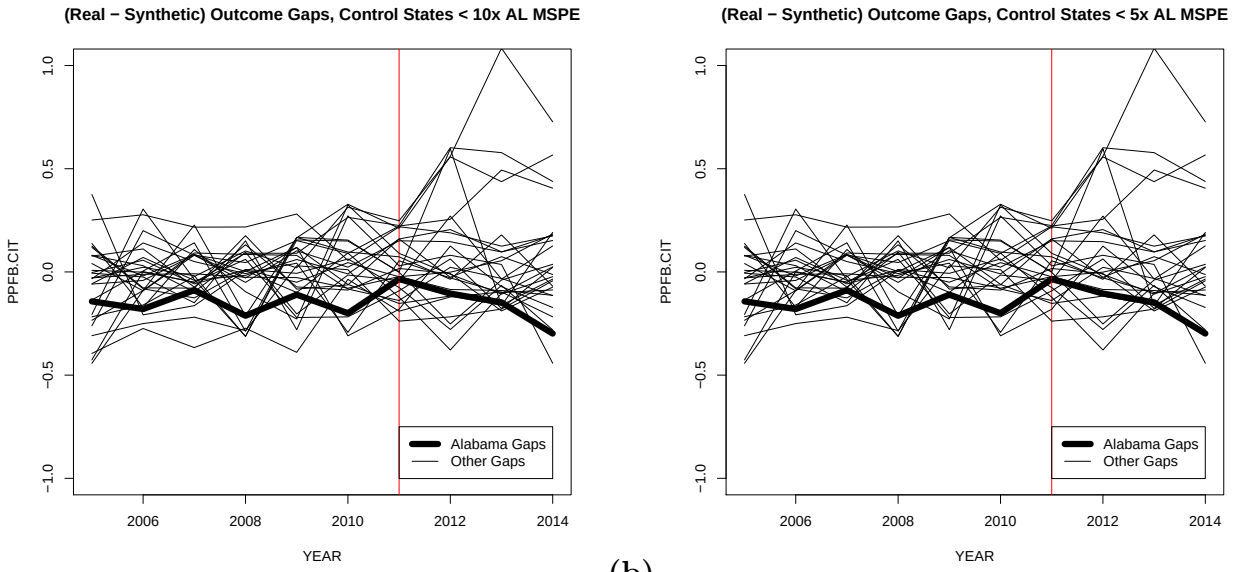
Figure 6: PPFB Gaps Between Synthetic and Real Versions of US States



(a) **(b)**

Notes: The MSPE of each state's synthetic version gives us an easily comparable statistic which quantifies the extent to which the synthetic version of each state exactly copies the pre-treatment outcome trajectory of its real version. An MSPE of exactly zero would imply that the synthetic control procedure has created a counterfactual that exactly matches the real state's pre-treatment outcome data. (a) Here we see the difference between synthetic and real Alabama (bolded) as compared to the same difference for a group of control states. The control states included in this group are those for which the MSPE of its SC-created synthetic version is within 10 times the MSPE of Alabama. (b) The control states included in this group are those for which the MSPE of its SC-created synthetic version is within 5 times the MSPE of Alabama.

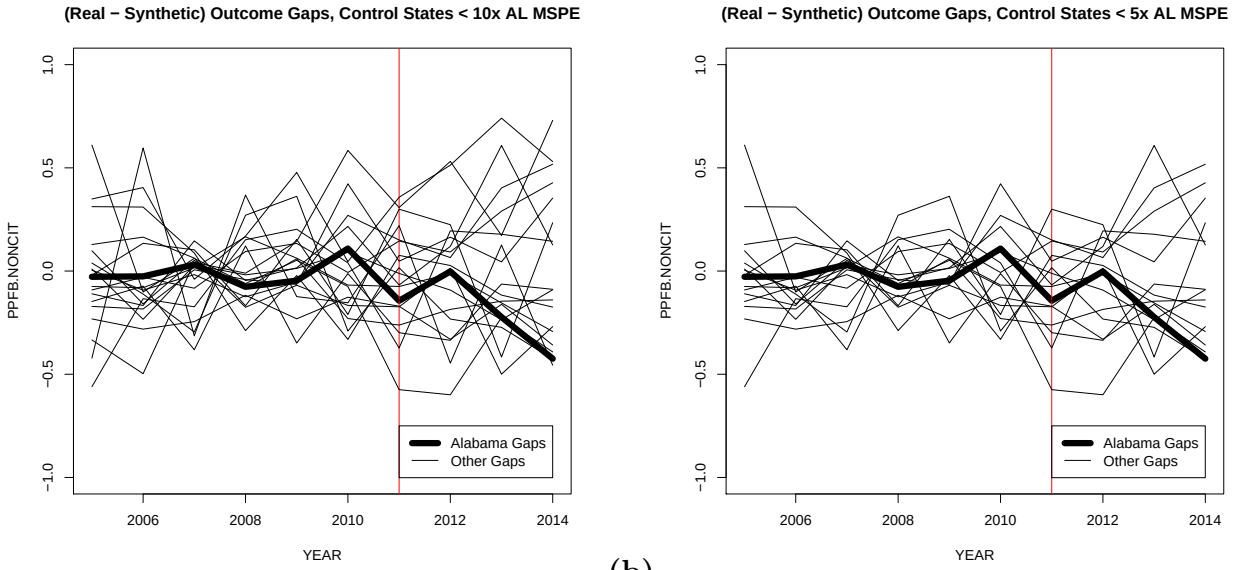
Figure 7: PPFBC Gaps Between Synthetic and Real Versions of US States



(a) **(b)**

Notes: The MSPE of each state's synthetic version gives us us an easily comparable statistic which quantifies the extent to which the synthetic version of each state exactly copies the pre-treatment outcome trajectory of its real version. An MSPE of exactly zero would imply that the synthetic control procedure has created a counterfactual that exactly matches the real state's pre-treatment outcome data. (a) Here we see the difference between synthetic and real Alabama (bolded) as compared to the same difference for a group of control states. The control states included in this group are those for which the MSPE of its SC-created synthetic version is within 10 times the MSPE of Alabama. (b) The control states included in this group are those for which the MSPE of its SC-created synthetic version is within 5 times the MSPE of Alabama.

Figure 8: PPFBN Gaps Between Synthetic and Real Versions of US States



(a) **(b)**

Notes: The MSPE of each state's synthetic version gives us an easily comparable statistic which quantifies the extent to which the synthetic version of each state exactly copies the pre-treatment outcome trajectory of its real version. An MSPE of exactly zero would imply that the synthetic control procedure has created a counterfactual that exactly matches the real state's pre-treatment outcome data. (a) Here we see the difference between synthetic and real Alabama (bolded) as compared to the same difference for a group of control states. The control states included in this group are those for which the MSPE of its SC-created synthetic version is within 10 times the MSPE of Alabama. (b) The control states included in this group are those for which the MSPE of its SC-created synthetic version is within 5 times the MSPE of Alabama.

SYNTH CONTROL	ECONOMIC ACTIVITY LOST	TAX REVENUE LOST
AL Average Income	$651,004,199	$32,550,210
AL Hispanic Average Income	$395,612,276	$19,780,614
DIFF IN DIFF		
AL Average Income	$729,915,895	$36,495,795
AL Hispanic Average Income	$425,651,317	$21,282,566

Notes: Here, we estimate the reduction in economic activity and state income tax revenues in Alabama. We use the range of coefficient estimates from our DID and SC estimations to create counterfactual non-citizen immigrant population sizes (PPFBN). We use the average income level, and the average income level for Hispanics, in Alabama during the post-treatment years in order to estimate the level of income no longer being made or spent in the state. The forgone state income tax numbers are calculated as 5% of the gross income level, per Alabama state tax data.

average of $6,868,269 in tax revenues each year as a result of passing H.B.56.

If we scale these estimates by .72 (the previously-mentioned estimate of the proportion of immigrants who are documented in the US), that leaves us with an estimate of GDP decline of $98,903,069, and $4,945,153 in average foregone tax revenues for each year from 2011-2014. This amounts to $395,612,276 in total forgone expenditures, which translates to $19,780,614 in total forgone state tax revenue during the 2011-2014 period.

We calculate the estimated impact using the difference in outcome between Alabama and its synthetic counterpart, as well as using our Diff in Diff coefficient, interacted with either overall Alabama average income or the average Hispanic income in Alabama for each post-treatment year. Our range of these estimates is displayed in Table 6.

6. Conclusions and Policy Recommendations

The results of both synthetic control and difference in differences estimations suggest that the passage of H.B.56 has had a significantly negative impact on the size of immigrant populations in Alabama. The post-treatment yearly drop it has caused in Alabama's PPFBN is estimated to be around .227 percentage points based on DID estimation and .182 percentage points by synthetic control estimation. This translates to a non-citizen immigrant deficit in the range of 6,500 to 11,000 people per post-treatment year for Alabama. Cumulatively, this represents an estimated %16 drop in the total immigrant population of Alabama. These displaced people would have been working at firms in Alabama, purchasing goods, paying taxes, and helping shape society. The total loss of GDP under the assumption of full employment in Alabama is estimated to be between $400 and $730 million including between $20 and $36 million in forgone gross state tax revenues.

The stories of discrimination and harassment of immigrant and visible minority populations, along with this reduction in legal residents of Alabama suggest that the passage of H.B.56 has caused significant impacts on the lives of individuals as well as the state as a whole. States and countries considering legislation of this type should learn from the experience of Alabama and carefully consider the potential costs of such draconian measures to limit undocumented immigration.

While this analysis strongly points to an out-migration of documented immigrants from Alabama as a result of passing H.B.56, there is still much research to be done in this domain. We have hypothesized that the channels through which this effect is manifested are; increased discrimination of

undocumented Hispanic people in Alabama, and pressure on documented individuals to leave the state to stay connected to their undocumented immigrant family and friend networks. However, the current research doesn't allow us to differentiate between the two causes. Having access to data which includes a higher level of detail, or panel data which tracks immigrants in their moves throughout the country would help us further isolate the causal links of interest. Performing this type of exercise for each state that has enacted similar omnibus anti-undocumented-immigrant legislation is a logical extension of this research and could lead to a consensus about the negative externalities that these types of regulations create for states that implement them.

Declarations

The authors declare that they have no competing interests.

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