UNIVERSITY OF CALIFORNIA

Los Angeles

State Law and Illegal Immigrants: A Study of Confounding of the Effects of Arizona Senate Bill 1070

A thesis submitted in partial satisfaction
of the requirements for the degree
M.S. in Statistics

by

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2021

Abstract

State Law and Illegal Immigrants: A Study of Confounding of the Effects of Arizona SB 1070 Policy

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University of California, Los Angeles, 2021

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Illegal immigration has long been a controversial issue in the U.S. In this paper, we will discuss the robustness of the findings in Illegal Immigration, State Law, and Deterrence (Hoekstra and Orozco-Aleman, 2017), in which the authors examine the effectiveness of Arizona SB 1070, allegedly one of the strictest immigration law ever passed to address the issue. Hoekstra and Orozco-Aleman (will be referred to as H & O-A going forward) show that the passage of the legislation reduced the flow of illegal border crossings into Arizona up to 70% which leads to the conclusion that potential undocumented immigrants are responsive to Arizona SB 1070. Using the approach for sensitivity analysis introduced in Cinelli and Hazlett (2020), we find that the robustness of the authors' findings are mixed among their study designs. However, all designs yield the same results, and an additional model using Baysesian approach also supports such results. This suggests that even though the findings may be indeed valid, they may be vulnerable to certain observable or unobservable confounders, and these concerns should be addressed in future studies.

The thesis of Andrew Jonathan Chang is approved.

Mark S. Hancock
Ying Nian Wu
Chad Hazlett, Committee Chair

Acknowledgements

I truly appreciate everything that has happened in my life, all the hardships I faced, the wonderful people I met, the help and opportunities I was given. From working 50 hours a week at a donut shop to attending community college, transferring to UCLA and later being admitted to the master program is a long and incredible journey. Thanks to everything and everyone that has helped me along the way to get here…

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1 Introduction

There are two main ideologies on immigration: those who claim that Americans should honor the history of the country as a nation of immigrants call for an immigration reform, while those on the opposing side insist that more restrictions on immigration will preserve the country's economy and the social services system. However, an important sub-topic of this debate is about undocumented immigrants, who are present in the country without proper authorizations. There are reportedly about 12 millions people living in the U.S. under this status, and the number has been increasing (Passel and Cohn, 2011). The federal and state government passed laws and regulations to address this issue, but their efficacy is often contentious.

H & O-A investigate one of these laws in their paper published in 2017: Illegal Immigration, State Law, and Deterrence. In this study, the authors employ several approaches to estimate the effect the SB 1070, an immigration legislation passed by the Arizona senate in April 2010. Since its passage, the law faced many challenges not only from the local communities and law makers, but also from U.S.' neighboring countries. Three months after it went into effect, the U.S. District Court in Arizona issued an injunction order, and two years later, it was determined by the Supreme Court that a large portion of the law was pre-empted by federal laws. In spite of its short life, H & O-A find that the law was effective in deterring illegal border crossings. The paper concluded that the law indeed significantly decreased the flow of illegal migrants through Arizona border during the time it was active, and that this effect was not merely by chance nor was overestimated by seasonal effects.

Nonetheless, as is common in observational studies, the estimates are vulnerable to unobserved confounding. In this paper, we will attempt to reproduce the results obtained in the paper and apply the Baysesian structural model approach to support the findings in H & O-A (2017). In addition, we will examine the sensitivity of such results with the tools introduced in Cinelli and Hazlett (2020). Finally, we will discuss other concerns about the data confounders that may impact the robustness of the estimated effects of SB 1070.

2 Background

2.1 Arizona Senate Bill 1070 Policy

The Arizona Senate Bill (SB) 1070, "Support Our Law Enforcement and Safe Neighborhoods Act," was passed by the state legislature in early 2010 and signed into law by Governor Jan Brewer on April 23, 2010. The bill made it a state crime to be unlawfully present in the United States and legalized police to stop and request the immigration status of any persons at the police officer's discretion.

The passage of the law drew significant national attention, especially among Latino communities, which the bill targeted and impacted the most. It sparked controversy across the country. The supporters deemed that the bill helped deter illegal immigrants, enhance public safety, and secure job opportunities for Americans while the opponents argued that the bill was unconstitutional and racist.

However, before the Arizona SB 1007 fully went into effect, three out of four provisions of the bill were blocked by a federal district court, whose decision was later upheld by the Supreme Court. Even though the full original version of Arizona SB 1070 was not successfully carried out, the passage and the announcement of the bill gained massive media attention in both the United States and Mexico, and consequentially, imposed a significant impact on the migrants' intention of crossing the border illegally (Hoekstra and Orozco-Aleman, 2017).

2.2 Data

H & O-A obtained their data from the Survey of Migration to the Northern Border (EMIF), which was conducted by Mexican authorities with the objective of studying the migrant flow along the US-Mexico border. At eight border cities and five Mexican airports, the survey was given at a broad list of locations, at which the migrants must pass: bus stations, train stations, international bridges, customs inspection points, access doors, boarding zones, gates, and baggage claim areas). Importantly, according to the Mexican National Population Council, the locations where the survey took place account for 94 percent of the total border crossings (Consejo Nacional de Población, 2013).

The survey consists of four questionnaires each of which is targeted at a different group of immigrants. H & O-A' research involved two of these groups. The first group consists of individuals who were at least 15 years old and reported an intention to cross the border in the next 30 days to work in the United States without authorization. The second group consists of those returning from the United States and had worked there illegally (Hoekstra and Orozco-Aleman, 2017).

H & O-A claimed that the data is a good candidate to be used for their study for several reasons. Firstly, the data is at monthly level and the questionnaires were conducted with the individuals who the announcement of the law has the highest impact on their migrating decisions. Secondly, the survey was carried out in Mexico, where respondents have more confidence to answer the questions truthfully. The EMIF data also provide the information on the ultimate destination of the respondents after they cross the border, in addition to where they plan to cross. The data showed that while 76 percent of illegal immigrants crossed the border in Arizona, only a few of them remained in the state (Hoekstra and Orozco-Aleman, 2017).

3 Methodology

3.1 Designs of Prior Study

The respondents' answers to whether they would less likely to choose Arizona as their ultimate destination after crossing the border were used to estimate the effects of SB 1070. There are three different designs that were used in H & O-A's paper. However, the sensitivity analysis will be focused on the reported results from the first two designs:

$$Destination\_AZ_{it} = \beta_0 + \beta_1 Post\_Passage_{it} + \beta_2 Post\_Injunction_{it} + \epsilon_{it} \tag{1}$$

where $Desitination\_AZ_{it}$ is an indicator equal to 1 for individual i at a time period t if the individual plans to reside in Arizona, $Post\_Passage$ and $Post\_Injunction$ are indicators for whether the law was passed and blocked by the judge, respectively.

The second design assumes that the log of the number of migrants destined for Arizona would have stayed at pre-SB 1070 levels, conditional on the month fixed effect as a control for seasonality:

$$\log(AZ_t) = \beta_0 + \beta_1 Post\_Passage_t + \beta_2 Post\_Injunction_t + \phi_t + \epsilon_t \tag{2}$$

where $\log(AZ_t)$ is the natural log of the number of migrants migrating to Arizona in month t, and ϕ_t is the fixed effect of month t.

3.2 Methods of Sensitivity Analysis

We will employ the sensitivity measures as well as other graphical tools that are introduced in Cinelli and Hazlett (2020).

Suppose we wish to run the following model:

$$Y = \tau D + \boldsymbol{X\beta} + \gamma Z + \varepsilon_{full} \tag{3}$$

where D is the treatment variable, $\boldsymbol{X}$ is a set of observed covariates, and Z is an unobserved confounder. However, since Z is unobserved, we can only run:

$$Y = \tau_{res} D + \boldsymbol{X\beta_{res}} + \varepsilon_{res} \tag{4}$$

We call the difference between τ and τ_{res} 'bias' which is represented in the aforementioned paper by the following equation:

$$|\widehat{bias}| = \widehat{\text{se}}(\hat{\tau}_{res})\sqrt{\frac{R^2_{Y\sim Z|D,\boldsymbol{X}} R^2_{D\sim Z|\boldsymbol{X}}}{1 - R^2_{D\sim Z|\boldsymbol{X}}}(\text{df})} \tag{5}$$

Where $\widehat{\text{se}}$ is standard error, $R^2_{D\sim Z|\boldsymbol{X}}$ is the partial R^2 from regressing D on Z after controlling for $\boldsymbol{X}$ and is computed as:

$$R^2_{D\sim Z|\boldsymbol{X}} = 1 - \frac{\text{var}(D^{\perp \boldsymbol{X},Z})}{\text{var}(D^{\perp \boldsymbol{X}})} \tag{6}$$

where $D^{\perp \boldsymbol{X}}$ is the variable D after removing the components linearly explained by $\boldsymbol{X}$. Another important equation derived in the paper is the relative bias:

$$\text{relative bias} = \left|\frac{\widehat{\text{bias}}}{\hat{\tau}_{res}}\right| = \left|\frac{R_{Y\sim Z|D,\boldsymbol{X}} \times f_{D\sim Z|\boldsymbol{X}}}{f_{Y\sim D|\boldsymbol{X}}}\right| \tag{7}$$

where f is obtained from the partial Cohen's f [1].

In the extreme case that there exists a confounder Z that explains all the residual variance of the outcome, to eliminate the estimated effect, the amount that such confounder needs to be associated with the treatment is found to be exactly $R^2_{Y\sim D|\boldsymbol{X}}$. This is because if such confounder exists, then $R_{Y\sim Z|D,\boldsymbol{X}} = 1$ and the relative bias in equation (6) is also 1. This indicates that $|f_{D\sim Z|\boldsymbol{X}}| = |f_{Y\sim D|\boldsymbol{X}}|$ and hence, $R^2_{Y\sim D|\boldsymbol{X}} = R^2_{D\sim Z|\boldsymbol{X}}$.

The second robustness value is $\text{RV}_q = \frac{1}{2}\sqrt{f_q^4 + 4f_q^2 - f_q^2}$ which measures the amount of equal association a confounder needs to have with the treatment and the outcome to become problematic to the estimated effects. In this RV_q formula, $f_q := q|f_{Y\sim D|\boldsymbol{X}}|$ is the same partial Cohen's f mentioned as above multiplied by the proportion of reduction q on the treatment coefficient. Confounders that can explain at or more than RV_q of both of the treatment and of the outcome can reduce $q \times 100\%$ the estimated effect.

[1] Cohen's $f^2 = R^2/(1 - R^2)$

The last robustness value introduced by Cinelli and Hazlett in their paper is $\mathrm{RV}_{q,\alpha} = \frac{1}{2}\sqrt{f_{q,\alpha}^4 + 4f_{q,\alpha}^2 - f_{q,\alpha}^2}$ where $f_{q,\alpha} := q|f_{Y\sim D|\mathbf{X}}| - \frac{|t^*_{\alpha,\text{df}-1}|}{\text{df-1}}$ and df-1 is the t-value threshold for an α significance level and df-1 degrees of freedom. Effectively, this robustness value enables researchers to examine the sensitivity of the estimated effect regarding both its size and also at which significance level they are willing to accept.

3.3 Bayesian Structural Models for Causal Impact Inference

Kay H. Brodesen (2015) propose to infer causal impact on the basis of a diffusion-regression state-space model that predicts the counter- factual market response had no intervention taken place. According to the authors, unlike the classical difference-in-differences schemes, state-space models enable researchers to infer the temporal evolution of attributable impact, incorporate empirical priors on the parameters in a fully Bayesian treatment, and flexibly accommodate multiple sources of variation (Kay H. Brodesen, 2015).

The Bayesian structural time-series models can be defined as a pair of equations:

$$y_t = Z_t^T \alpha_t + \epsilon_t \tag{8}$$

$$\alpha_{t+1} = T_t \alpha_t + R_t \eta_t \tag{9}$$

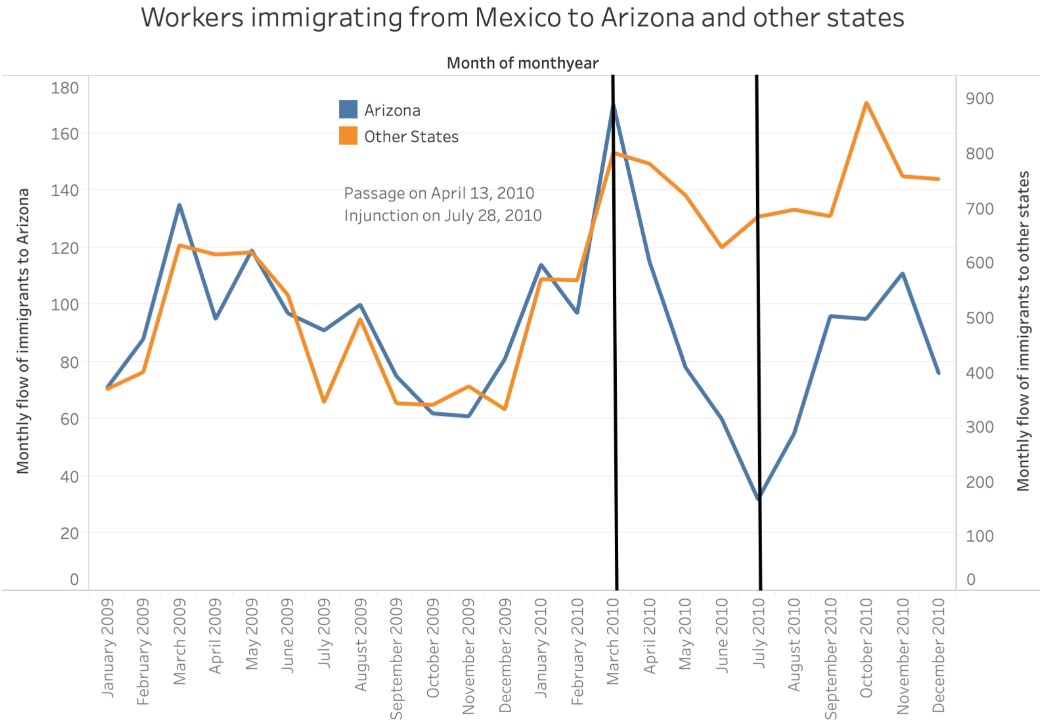
where $\epsilon \sim N(0, \sigma^2)$ and $\eta_t \sim N(0, Q_t)$, y_t is a scalar observation, Z_t is a d-dimensional output vector, T_t is a $d \times d$ transition matrix, R_t is a $d \times q$ control matrix, η_t is a q-dimensional system error with a $q \times q$ state-diffusion matrix Q_t.

Based on these models, the authors propose an approach that utilizes local linear trend model and Markov chain Monte Carlo algorithm for posterior inference to construct a state-space model that generalizes the difference-in- differences model.

4 Analysis of the Intention to Reside in Arizona

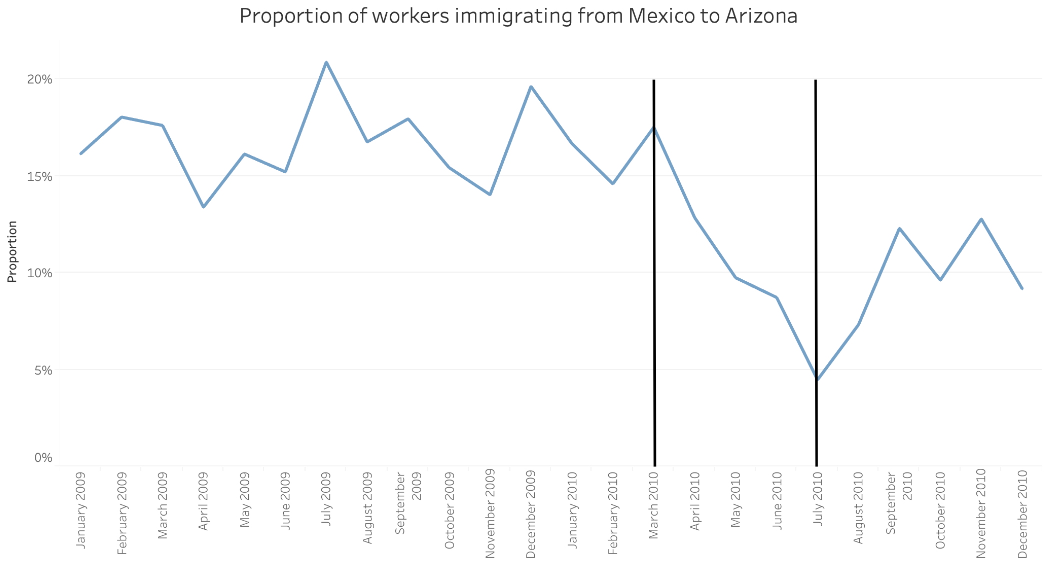
An important adversity of using EMIF data is that the information is self-reported. In spite of the advantages H & O-A claim for using such data, they are surveyed results. As a result, the quality of the data relies entirely on the respondents' honesty. In particular, it is uncertain whether the respondents actually crossed the border and ultimately resided in their intended destination state.

FIGURE 1



Figures 1 and 2 visualize the data subset which was used to estimate how much SB 1070 affected on the immigrant intention to reside in Arizona. Both of these figures provide primary evidence that the law may have had adversary impact on the immigrants' plan. The first figure reports the monthly flow of immigrants to Arizona and other states. A steep incline starting from April, when the passage of the law was announced, stands out from the graph. In the second figure, the proportion of individuals who are heading to Arizona also behaves in a similar pattern. One may reasonably argue that this could have been a seasonal effect. However,

FIGURE 2



according to the data of the period between 2005 and 2014, a similar trend of immigration flow to Arizona can only be observed in 2005 and 2006, when Arizona alien smuggling law was passed. In the latter year, the state passed a law that penalizes companies heavily if they are proved to have a pattern of hiring unauthorized workers. The effect of these strict laws were studied and appeared to be the accountable factor for the incline (Eagly, 2011).

4.1 Covariate Balance

TABLE 1

Immigration Flow Balance Table (2010)

Covariate	Treated Mean	Control Mean	t p-val	KS p-val
age	28.71	29.61	0	0.06
yschool	7.55	7.06	0	0.00

TABLE 2

Immigration Flow Balance Table (2009-2010)

Covariate	Treated Mean	Control Mean	t p-val	KS p-val
age	28.71	29.66	0	0.02
yschool	7.55	7.26	0	0.00

Another concern is that, the immigrants who were surveyed between March and July, 2010 may have backgrounds that are different enough to confound the treatment or the outcome. To examine the covariate balance, the t-test for means and Kolmogorov–Smirnov (KS) test for distributions are used. The results are reported in Table 1 and Table 2. The treated group is assumed to consists of those who were surveyed during April 2010 to July 2010, when the law was announced and later stroke down by a federal judge. For Table 1, we only check the data obtained in 2010. The difference in means for both age and education are significant, and the age distribution of the respondents do not appear to be similar as suggested by the KS p-value. In Table 2, we include all of the observations that are used to estimate the effect, and similar results can be observed. In spite of the significant p-values of these tests, considering the small difference in age and education background between treated and control groups, the covariate imbalance is not a big concern. However, since the H & O-A did not provide data for other observed covariates from the EMIF survey, the imbalance test can only be done with the age and education background.

4.2 Estimation Results and Robustness Values

4.2.1 Models Using Equation 1

The design mentioned in section 3.1 is replicated and presented in Table 3. There are seven specifications in total. The first model is as described in equation (1), section 3.1, and the other models include additional terms to account for month fixed effects and time-varying controls. The fifth model includes `pre6`, an indicator of six months prior to the passage of the law. No evidence of reduction in migrant flow to Arizona was found, indicated by insignificant estimate for the `pre6` term. The last model uses an adjusted version of the data to account for the possibility of migrant displacements to other states. All seven estimated effect of the law passage are negative and significant at $\alpha = 0.01$. Noticeably, the effect of the injunction

TABLE 3

Summary of Models for Intention to Reside in Arizona Outcome Using Equation 1

	Est.	S.E.	t-value	p-value	Sensemakr Results $R^2_{Y\sim D\|\mathbf{X}}$	$RV_{q=1}$	$RV_{q=1,\alpha=0.01}$
After Arizona Law *(April 2010 - December 2010)*							
Without covarites	-0.074	0.007	-10.356	0.000	0.7%	7.8%	5.9%
With month fixed effects	-0.067	0.009	-7.328	0.000	0.3%	5.6%	3.7%
With month fixed effects and time-varying controls	-0.092	0.032	-2.888	0.005	0.1%	2.3%	0.2%
With month fixed effects and linear time trend	-0.058	0.014	-4.092	0.000	0.1%	3.2%	1.2%
With month fixed effects and `pre6` indicator	-0.067	0.009	-7.328	0.000	0.3%	5.6%	3.7%
With month fixed effects and 2011 data included	-0.063	0.008	-8.266	0.000	0.3%	4.9%	3.4%
With month fixed effects and adjusted	-0.06	0.009	-6.383	0.000	0.3%	4.9%	3%
After temporary block by federal judge *(April 2010 - December 2010)*							
Without covarites	0.011	0.008	1.334	0.182	0%	1%	0%
With month fixed effects	0.001	0.013	0.095	0.924	0%	0.1%	0%
With month fixed effects and linear time trend	0.001	0.025	0.05	0.961	0%	0%	0%
With month fixed effects and time-varying controls	0.001	0.013	0.095	0.925	0%	0.1%	0%
With month fixed effects and `pre6` indicator	-0.006	0.014	-0.408	0.688	0%	0.3%	0%
With month fixed effects and 2011 data included	-0.004	0.007	-0.513	0.573	0%	0.3%	0%
With month fixed effects and adjusted	-0.006	0.013	-0.461	0.646	0%	0.4%	0%

Note: In the model with `pre6` indicator (six months prior to the passage of the law), the estimated coefficient of the `pre6` term has *insignificant* p-value with $\alpha = 0.01$. In the last model, the data are adjusted for the possibility that migrants were displaced to other states.

is not well supported by the data as the estimate ranges from positive to negative amongst the models, and none of the model yields a significant result.

We implement the sentivitiy analyses of Cinelli Hazlett (2020) using their software for R, `sensemakr`. The most important outputs of `sensemakr` are the robustness values and partial R^2's. The first one is $R^2_{Y\sim D|\mathbf{X}}$ (the proportion of variation in the outcome uniquely explained by the treatment), which is how strongly associated with the treatment an extreme confounder needs to be to eliminate the estimated effect if it explains all the residual variance of the outcome. The other two are $RV_{q=1}$ (how strong the equal association with treatment and outcome a confounder must have in order to reduce the estimated effect by $(100q)\%$), and $RV_{q=1,\alpha=0.01}$ (the partial R^2 value of both with the treatment and with the outcome to make the adjusted $1-\alpha$ confidence interval include $(1-q)|\hat{\tau}_{res}|$). For these seven models, a confounder must explain approximately 2% to 8% of the residual variance in treatment and outcome to entirely eliminate the treatment effect. To make the results insignificant at $\alpha = 0.01$, this amount ranges from 0.2% to roughly 6%. Finally, if there exists an extreme confounder that can explain all of the residual variance in the outcome, it has to be able to explain about 0.1% to 0.7% of the variance in the treatment. Given the research design, it seems unlikely that H & O-A would be able to rule out confounders of this scale, making it difficult to defend their estimates. Some confounders that may account for this small amount of variance in the outcome could be an improving job market or decreasing crime rate in the migrants' home country. The majority of migrants decided to cross the border illegally because they could not find a stable job or live in a high murder rate area. If their living conditions improve, it may be enough for them not to take the risks in crossing the border. Another factor could be an enhanced border patrol: only one month after the law was passed, president Barack Obama deployed 1,200 National Guard troops to the U.S.-Mexico border in response to Arizona senators' call for extra border security. Also during the summer of 2010, a string of violence and drug cartel activity was happening in Arizona, making the state a dangerous crossing point. With the month fixed effect terms added, all three values decrease. However, when additional terms are added into the model on top of the month fixed effects, the robustness values do not behave in the same way. In particular, adding the time-varying controls decreases the values heavily while adding the `pre6` term does not seem to affect these robustness measures.

TABLE 4

Summary of Models for Intention to Reside in Arizona Outcome Using Equation 1

					Sensemakr Results		
Month Since Arizona Law *(April 2010 - December 2010)*	Est.	S.E.	t-value	p-value	$R^2_{Y\sim D\mid\mathbf{X}}$	$RV_{q=1}$	$RV_{q=1,\alpha=0.01}$
Without covarites	-0.032	0.003	-11.148	0.000	0.8%	8.4%	6.5%
With month fixed effects	-0.039	0.004	-9.715	0.000	0.6%	7.4%	5.5%
With month fixed effects and time-varying controls	-0.048	0.014	-3.384	0.352	0.1%	2.6%	0.6%
With month fixed effects and linear time trend	-0.034	0.005	-6.512	0.013	0.3%	5%	3.1%
With month fixed effects and 2011 data included	-0.034	0.005	-6.512	0.000	0.3%	5%	3.1%
With month fixed effects and adjusted	-0.038	0.004	-8.633	0.000	0.5%	6.6%	4.7%

Note: In this table, the models use months since April 2010 as a predictor instead of whether or not a respondent intend to head for Arizona. In the last model, the data are adjusted for the possibility that migrants were displaced to other states.

In an alternative approach, H & O-A regress the outcomes on the time difference in months relative to when the law was passed and when the injunction took place. This approach allows a closer look into the effect of the law over time. The results of this approach together with its robustness values are reported in Table 4. Although negative effect of the law passage is also observed and statistically significant in this model, the estimates are reduced by half in size when compared with Table 3. The robustness values also slightly increase 0.3% to around 2%. Note that the results for the estimated effect of the injunction are not reported since as in the first approach, they change sign when different terms are added and are all insignificant at 0.01 level. Furthermore, their robustness values are extremely low in both approach. The survey data used in the H & O-A's study only covers up to two months after the injunction. In particular, there are 11,903 data points for pre-injunction period, but only 4,219 data points for post-injunction period. The estimated effect of the injunction is reported to be positive, but the paper does not address the highly insignificant p-values of such estimates.

4.2.2 Models Using Equation 2

TABLE 5

Summary of Models for Intention to Reside in Arizona Outcome Using Equation 2

					Sensemakr Results		
	Est.	S.E.	t-value	p-value	$R^2_{Y\sim D\|\mathbf{X}}$	$RV_{q=1}$	$RV_{q=1,\alpha=0.01}$
After Arizona Law *(April 2010 - December 2010)*							
With month fixed effects	-0.439	0.22	-1.994	0.0742	28.4%	46.2%	0%
With time-varying controls	-0.576	0.126	-4.566	0.0001	69.9%	75.4%	27.4%
With month fixed effects and linear time trend	-0.706	0.335	-2.111	0.0640	33.1%	49.8%	0%
With month fixed effects and `pre6` indicator	-0.439	0.232	-1.896	0.0559	28.5%	46.3%	0%
With month fixed effects and 2011 data included	-0.339	0.171	-1.977	0.0607	15.1%	34.2%	0%

For this section, the design (2) in section 3.1 is replicated, and the results are reported in Table 5. There are five different specifications. In contrast with the previous section, all specifications include a set of covariates. Using this approach, only three out of five models yield significant results even though all estimated values are consistently negative. Similarly, `sensemakr` is run on all models to investigate their robustness against unobserved confounders. For these five models, a confounder must explain approximately 34% to 75% of the residual variance in treatment and outcome to entirely eliminate the treatment effect. This is a tremendous improvement compared to the models using equation 1 where this number range is between 2% and 8%. To make the results insignificant at $\alpha = 0.01$, this amount is 27.4% for two models. The other models are not significant even at $\alpha = 0.05$, therefore, 0% are present in the $RV_{q=1,\alpha=0.01}$.

TABLE 6

Summary of Models for Intention to Reside in Arizona Outcome Using Equation 2

	Est.	S.E.	t-value	p-value	Sensemakr Results $R^2_{Y\sim D\mid\mathbf{X}}$	$RV_{q=1}$	$RV_{q=1,\alpha=0.01}$
Months Since Arizona Law *(April 2010 - December 2010)*							
With month fixed effects	-0.166	0.056	-2.985	0.0137	47.1%	59.8%	14.9%
With time-varying controls	-0.183	0.021	-8.591	0.0000	89.1%	90.1%	73.8%
With month fixed effects and linear time trend	-0.25	0.076	-3.294	0.009	54.7%	65%	9.2%
With month fixed effects and `pre6` indicator	-0.171	0.055	-3.096	0.0128	51.6%	62.9%	16.6%

Note: In this table, the models use months since April 2010 as a predictor instead of whether or not a respondent intend to head for Arizona.

Similar to the models in the previous section, as an alternative approach, H & O-A use the number of months since the Arizona SB 1070 was passed instead of the indicator of whether one intends to cross and stay in Arizona. The natural log of the number of migrants who are destined for Arizona is regressed on the time difference in months relative to when the law was passed and when the injunction took place. The results of these models together with its robustness values are reported in Table 6. Remarkably, with this approach, all estimated effects are consistently negative and statistically significant at $\alpha = 0.01$ level. Improvement in robustness values indicate that this approach yields the estimates that are more robust against potential unobserved confounders. Specifically, $R^2_{Y\sim D\mid\mathbf{X}}$ is now about 51% to 90%, $RV_{q=1}$ is between 51% to 89%, and $RV_{q=1,\alpha=0.01}$ ranges from 9% to as high as 73.8%. There are some hypothesized confounders that could make such differential impact on the outcome. A potential hypothesis includes a passage of a new policy in either the migrants' home country or the U.S., which would heavily punish the illegal border crossings in addition to the SB 1070. Although current immigration laws in the U.S. would bar anyone who has been in the U.S. without permission from reentering the country for up to 10 years as a punishment, little was done in the migrants' home country. Illegal immigrants that were deported back to their country often faced no punishments. If the involved countries strictly enforce new policies that would punish illegal border crossings, the number of migrants who choose to go to the U.S. through this path would decrease. Other hypotheses include enhanced border patrol in southern Mexico which prevents a vast number of migrants from central and south America from traveling further north, and the collapse of the coyote-cartel network. However, since border patrol enforcement can cost the government heavily, it is unlikely that these Latin American countries can fully and effectively guard their borders. A large part of the south side of the U.S.-Mexican border are controlled by the drug cartels. The coyotes, who help smuggling people into the U.S. illegally, have to pay the cartels a certain amount of money per migrant. If the cartel-coyote network around Arizona border collapses, the migrants will be sent to the U.S. through other parts of the border. However, this is unlikely to happen due to the huge profits human smuggling brings to the cartels. Other potential confounders mentioned in section 4.2.1 such as an improvement in the job market or decreasing crime rate in the migrants' home countries are very unlikely to have such large $RV_{q=1}$ values that are shown in this section. If the job market did improve, it probably did so at a low rate, and should not abruptly decrease the flow of migrants. A similar argument can be said for decreasing crime rate. Overall, the models presented in Table 6 are quite robust against strong potential confounders.

4.3 Benchmarking and Extreme Cases

In this section, we will discuss the impact of a hypothesized confounder or an extreme confounder, if exists, will have on the estimated effect. The R package `sensemakr` is employed again to render Figure 3, 4, 5, and 6. The first two figures depict the adjusted estimated effect of the law on the number of migrants at levels of hypothesized confounders parameterized by the strength of relationship to the treatment and the outcome. For the purpose of demonstration, the fixed effect of March and April are chosen to be the benchmarks since other observed covariates yields trivial results. Figure 3 shows the worst confounding that can exist if one was to assume that confounding is 2 times as bad as the fixed month effect of

FIGURE 3

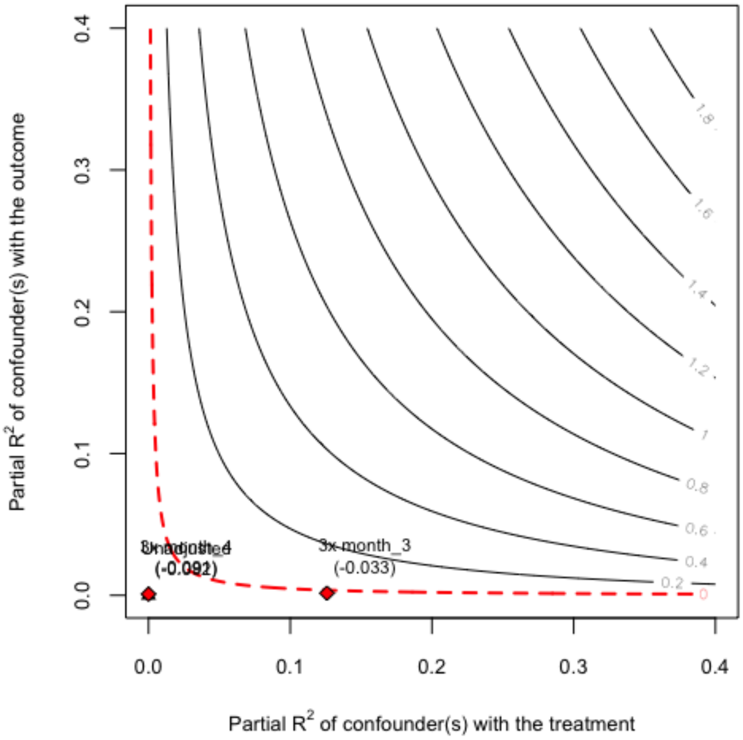


FIGURE 4

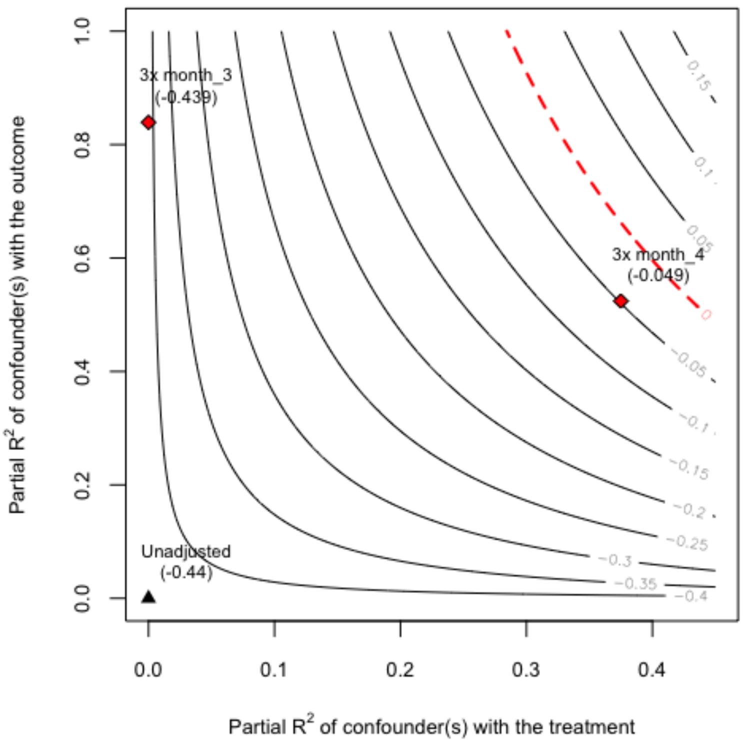


FIGURE 5

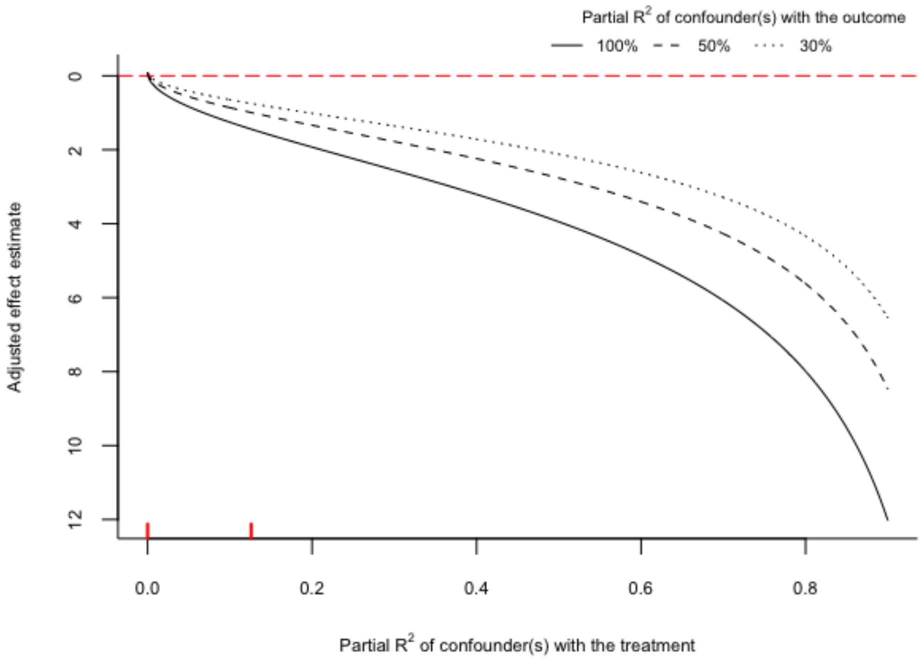
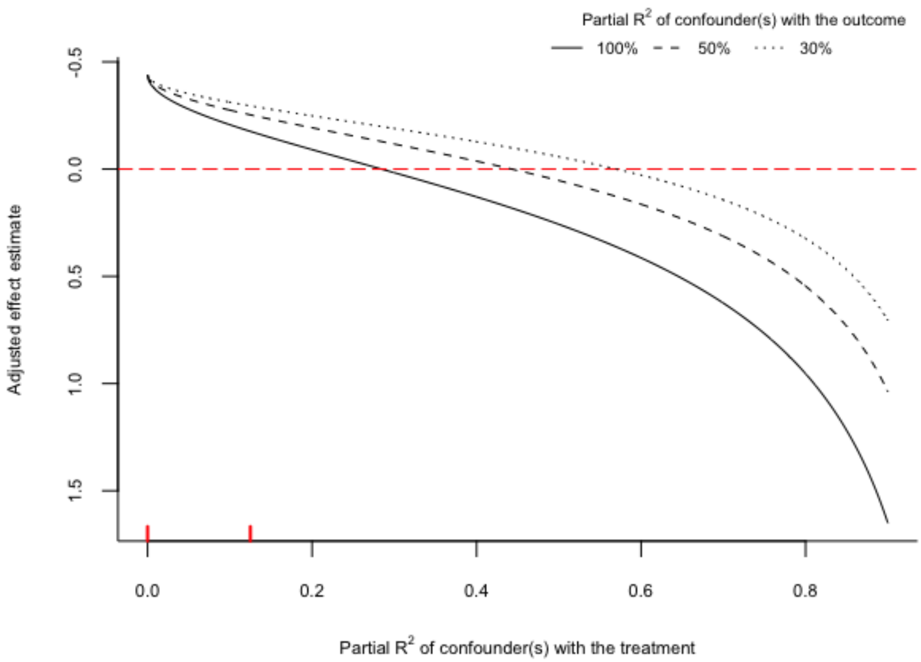


FIGURE 6



March and April in terms of the residual variance of the treatment and outcome they explain. This figure involves the third model (with month fixed effect) of Table 3. A confounder that is three times as bad as March fixed effect does not affect the effect significantly (-0.092) while the effect is reduced by almost 70% to -0.033 if the confounder is thrice as bad as the April monthly fixed effect. Figure 4 involves the first model of Table 5, while similar trend can be observed if there exists a confounder that is three times as strong as the March fixed effect (the estimate remains unchanged), in this model, the damage to the estimated effect is more severe if it is proven that we have an unobserved confounder that is thrice as strong as April fixed effect. The estimate is reduced by approximately 90% to -0.049.

In section 4.2, we argue that using the second equation tremendously boosts up the robustness of estimates. However, the contour plots in this section reveal that, even with such highly robust estimates, unobserved confounders still play an important role in validating one's findings as they can potentially eliminate the treatment effect.

In firgure 5 and 6, we examine the extreme cases where a confounder becomes problematic to the results. In particular, we set $R^2_{Y|DX}$ to 100%, 50%, and 30% and investigate how strongly such a confounder needs to be associated with the treatment to problematically affect the estimate. The curves represent the cases where unobserved confounders explain all, half, or thirty percents of the left-out residual variance of the outcome. In figure 5, which corresponds to the model used in Figure 3, the problem is obvious since it indicates that if the unobserved confounders with such characteristic exist, the estimated effect would change sign with a very low $R^2_{D|X}$. In the case of Figure 6, which uses the same model as in Figure 4 for the most extreme case, the confounder(s) needs to be at least about 25% associated with the treatment to bring down the estimated effect to zero. This measure increases as we reduce the partial R^2 of confounder(s) with the outcome.

TABLE 7

Falsification Test for Intention to Reside in Arizona Outcome Using Equation 2

Bandwidth	Year	Est.	S.E.	t-value	p-value
3 months	2000	0.003	0.022	0.150	0.881
	2001	-0.047	0.024	-1.960	0.050*
	2002	-0.001	0.018	-0.032	0.975
	2003	-0.056	0.014	-3.935	0.000**
	2004	0.003	0.013	0.250	0.802
	2005	0.0002	0.013	-0.015	0.988
	2006	0.067	0.008	8.791	0.000**
	2012	-0.001	0.008	-0.097	0.922
	2013	-0.016	0.009	-1.907	0.057
	2014	0.020	0.010	1.939	0.053*
2 month	2000	0.028	0.026	1.073	0.284
	2001	-0.06	0.029	-2.085	0.037*
	2002	0.01	0.021	0.47	0.639
	2003	-0.051	0.017	-3.074	0.002*
	2004	0.019	0.015	1.242	0.214
	2005	0.011	0.015	0.717	0.473
	2006	0.059	0.009	6.701	0.000**
	2012	0.001	0.009	0.107	0.914
	2013	-0.013	0.01	-1.278	0.201
	2014	0.023	0.013	1.82	0.069
1 month	2000	0.029	0.034	0.843	0.399
	2001	0.005	0.035	0.147	0.883
	2002	-0.049	0.032	-1.554	0.121
	2003	-0.053	0.02	-2.589	0.010**
	2004	-0.001	0.021	-0.064	0.949
	2005	0.02	0.022	0.908	0.364
	2006	0.064	0.013	5.033	0.000**
	2012	0.003	0.011	-0.283	0.778
	2013	-0.027	0.014	-1.996	0.046*
	2014	0.046	0.018	2.593	0.010**

4.4 Falsification Test

Another concern regarding the credibility of the effect of the law on the intention to cross the border and ultimately reside in Arizona is whether the running variable, in this case, the number of months before or after the passage, has a significant effect on the outcomes independently of the treatment. In other words, the variation in the illegal immigrant flows may be solely a seasonal event that happens yearly. The author addressed this concern by adding variables that account for month fixed effect, linear time trend and time varying-controls. Although how these variables are calculated or obtained are not discussed in the paper, the data provides sufficient information to conduct a test for falsifications to examine the seasonal effect, if any. The regression discontinuity design (RDD) model is run on the dataset of the year from 2000 to 2006, and from 2012 to 2014, i.e. the dataset used to obtain Table 3 is excluded. We employ three different bandwidths and assume that in each year, those who answered the survey after April will be in the treated group. If the migrant flow changes seasonally, we expect to observe a decline in number of migrants who favor Arizona, and the paper results would be less reliable.

The estimated effect is calculated for each year with different bandwidths and is reported in Table 7. It is quite evident that the seasonal factors do not have significant effect on the outcome. Firstly, the estimates are not consistently negative across the years involved. The effect of the seasonal factors with significantly size can either overestimate the effect of the Arizona Senate Bill or large enough to change the sign of the actual effect if we do not control for those factors. It appears that most of the estimates are not significant at 0.05 level. However, in the year of 2006, the estimate is statistically significant and positive while for 2001 and 2003, it is found to be significant and negative. Nevertheless, there is no strong evidence to show that such significant effect are 'seasonal', i.e., how far off April itself can explain most of the variation in migrant flows. For this to be true, the estimate should be consistent in sign, if not in size, and should be statistically significant for most years. For example, in 2006, the Comprehensive Immigration Reform Act was passed by the Senate. This immigration reform bill allowed illegal immigrants who have been in the country for more than five years to apply for citizenship by paying fines and owed taxes. For the others, who have been in the country for at least two years, were allowed to stay in the country without fear of deportation, but after three years would have to leave the U.S. and could apply for citizenship abroad. Along with the maturing recession during that period, this could have been the momentum for an increase in migrant flow. Other non-seasonal factors may also account for the significant effect in 2001 and 2003. In conclusion, we do not have enough evidence to claim that whether the survey was conducted before or after April alone would have effect of the flow of migrants at the southern border.

4.5 Placebo and Difference in Difference Test

TABLE 8

TABLE 5—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE EFFECT OF SB 1070 ON THE NUMBER OF UNDOCUMENTED IMMIGRANTS GOING TO ARIZONA

Dependent variable: log of number of undocumented immigrants going to the US, by destination (Arizona or elsewhere)	1	2	3	4	5	6	7	8
After Arizona law × destined for Arizona	−0.771	−0.648	−0.746	−0.681	−0.739	−0.638	−0.662	−0.586
(April 2010–July 2010)	(0.228)	(0.149)	(0.335)	(0.207)	(0.230)	(0.143)	(0.324)	(0.204)
	[0.0023]	[0.0001]	[0.0608]		[0.0032]	[0.0000]	[0.0822]	
After temporary injunction × destined for Arizona	0.196	0.228	0.164	0.106	0.075	0.198	0.012	−0.078
(August 2010–December 2010)	(0.249)	(0.165)	(0.367)	(0.231)	(0.249)	(0.161)	(0.353)	(0.225)
Observations	48	48	48	48	48	48	48	48
Sample	All immigrants				Immigrants with known destinations			
Includes destination state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes year-by-month fixed effects	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Includes state-by-month fixed effects	No	No	Yes	No	No	No	Yes	No
Includes time-varying controls	No	Yes	No	No	No	Yes	No	No
Adjusts for potential displacement of Arizona immigrants to other states?	No	No	No	Yes	No	No	No	Yes

Notes: Each column in each panel represents a separate ordinary least squares regression. Robust standard errors are in parentheses, while empirical p-values measuring the proportion of permutation t-statistics lying to the left of the t-statistics are shown in square brackets. Time varying controls include the unemployment rates of Hispanics working in construction, services (business, repair, and personal services), and trade (wholesale and retail) in the states of Arizona, California, Florida, and Texas. In columns 4 and 8 we use estimated reductions in the number of people going to Arizona during each of the post-passage months and subtract them from those going to other states.

To show that the variance in the number of migrants did not appear by chance, H & O-A perform placebo test and investigate how often there have been a significant decline over a consecutive three month period in the past. The involved data are from January 2002 to December 2013, splits the data into 24-month periods, and assume the first 15 months to be pre-treatment period, the next 4 months as the treat period, and the following 5 month as the second treatment period (the law injunction). This resembles the actual data set used to obtain the estimated effect. H & O-A obtain 119 placebo estimates, and conclude that the placebo test in fact offers further evidence that the reduction in migration to Arizona in response to the SB 1070 law was unlikely to occur by chance.

In addition to the two models mentioned in Section 3, H & O-A also employ a difference-in-difference design (Table 8), which assumes that the relative change in migrant flow destined for Arizona before and after April 2010 would have been the same had the legislation never been passed. The results are reported in Table 8. However, the potential caveats of this approach are revealed in column 4 and 8, where the estimates are highly insignificant. In these columns, the author explicitly adjusted the migrant flow for the possibility that those who did not aim for Arizona actually chose another state instead of staying in Mexico. H & O-A argue that even though the bias could be present and hence, the insignificant results, it should be relatively small since the portion of migrants who are headed to other states are larger than to Arizona.

TABLE 9

Migrants to the United States - EMIF 2009-2010		**Recently arrived non-citizen Mexican migrants - ACS 2009-2010**	
Variable	Mean (Std. Dev.)	Variable	Mean (Std. Dev.)
Age	29.5	Age	35.0
	(10.7)		(15.3)
Years of schooling	7.3	Years of schooling	9.3
	(3.2)		(4.4)
Women	0.10	Women	0.35
Married	0.58	Married	0.43
Speaks English	0.09	Speaks English	0.50
State of destination in the U.S.		State of residence in the U.S.	
California	0.26	California	0.24
Arizona	0.13	Arizona	0.06
Florida	0.06	Florida	0.02
Texas	0.05	Texas	0.23
New York	0.03	New York	0.02
Illinois	0.02	Illinois	0.04
Colorado	0.02	Colorado	0.04
Georgia	0.02	Georgia	0.02
North Carolina	0.02	North Carolina	0.03
Observations	16,122	Observations	1,285

The EMIF sample includes all individuals surveyed who were migrating from Mexico to the US with the intent of crossing the border and entering the US within 30 days. The ACS sample includes surveyed individuals who migrated to the US during the year of the survey.

4.6 Bayesian Structural Model Approach

FIGURE 7

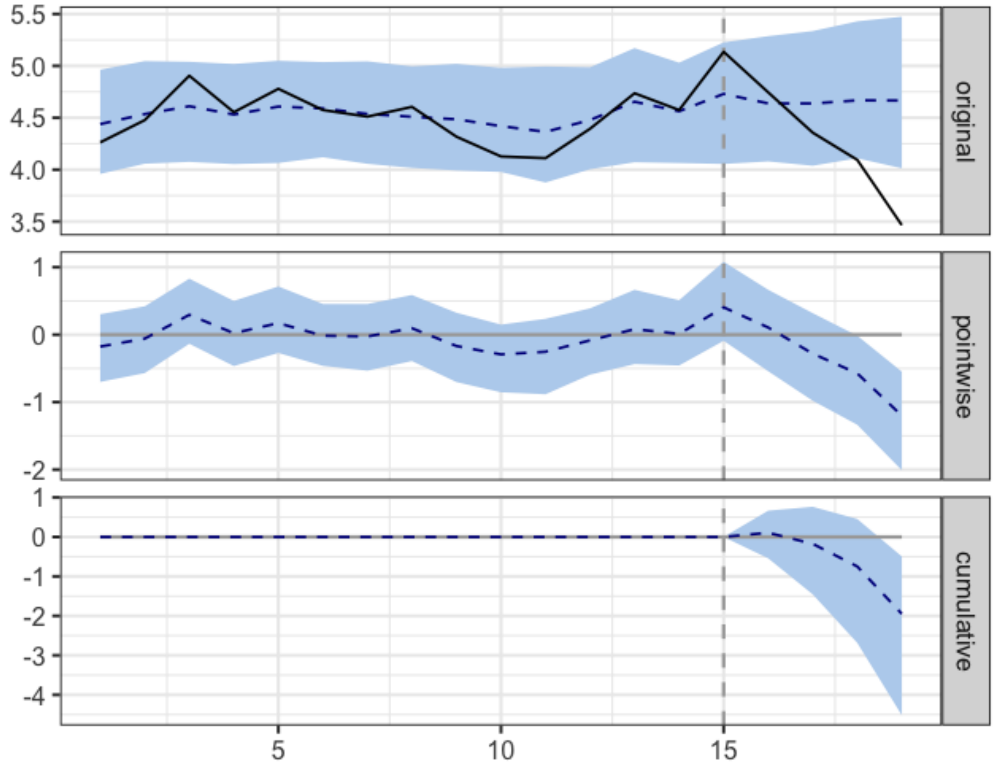


TABLE 10

Posterior Inference

	Average	Cumulative
Actual	4.2	16.7
Prediction (s.d.)	4.7 (0.26)	18.6 (1.03)
95% CI	[4.3, 5.3]	[17.2, 21.2]
Absolute effect (s.d.)	-0.49 (0.26)	-1.95 (1.03)
95% CI	[-1.1, -0.12]	[-4.5, -0.49]
Relative effect (s.d.)	-10% (5.5%)	-10% (5.5%)
95% CI	[-24%, -2.7%]	[-24%, -2.7%]

In this section, we will attempt to apply the method Brodersen et al (2015) proposed as mentioned in Section 3.3. The R package `CausualImpact` is employed to produce the plots shown in Figure 7. In order to apply this method, we need at least one time-series covariate and one time-series outcome. The data used for this part is the same as what was used to produce Table 5 and Table 6. The outcome of this model is set to be the natural log of migrant flow to Arizona from 2009 to 2010. The covariates that have significant p-value at 0.01 level in the third model of Table 5 will be included. In particular, we will include the time varying controls of California, Florida and Arizona. Similar to the other approaches, we have to assume that the survey results reflect the true intention of the respondents and that they will keep their intention. This method is however, a non-experimental approaches to causal inference, hence, it requires strong assumptions. Brodersen et al (2015) assume that there is a set control time series that are themselves not affected by the intervention. The model also assumes that the relationship between covariates and treated time series are similar between before and after treatment.

In contrast to the second model mentioned in section 3.1, this method does not assume that the natural log of the number of migrants destined for Arizona would remain the same at pre-SB 1070 levels, with monthly fixed effects and other seasonal factors controlled. Instead, this model assumes that we have a time series or a set of time series that are not affected by the presence of the legislation. Although the authors of the main paper do not specify what seasonal factors are tracked, for the purpose of demonstration in this section, we assume that they are not impacted by SB 1070. The package `CausalImpact` constructs a Bayesian structural time-series model, then uses this model to predict the counterfactual outcome of the natural log of the migrant flow. In other words, it predicts how the outcome would have been after April 2010 if the legislation had never been passed.

The results are presented in Table 10 and Figure 7. As shown in Table 10, during the post-intervention period, the outcome has an average value of approximately 3.98. If the legislation had never been passed, we would have expected an average outcome of 4.7. The 95% interval of this counterfactual prediction is [4.3, 5.3]. Subtracting this prediction from the observed outcome gives an estimate of the causal effect the legislation had on the natural log of migrant flow. This effect is -0.49 with a 95% interval of [-1.1, -0.12]. This negative effect observed during the intervention period is statistically significant since the probability of obtaining this effect by chance is very small (Bayesian one-sided tail-area probability p = 0.002). The result can also be inferred from the three plots in Figure 7. In the first plot, the black line represents the observed outcome, and the dotted line represents the predicted outcome. The vertical dotted line divides each plot into pre-treatment and post-treatment periods. It can be seen that the model has relatively good performance on fitting the observed outcome for the pre-treatment period, the resulted model is then used to predict the outcome in the post-treatment period, assuming that the intervention had never occurred. Based on this approach, if the SB 1070 had never been passed by Arizona legislators, the flow of the immigrants into Arizona would have been slightly increasing. The second plot shows the difference between observed data and counterfactual predictions. The difference fluctuates around zero up to March 2010 then significantly increases its magnitude towards the negative side for April, May and June of that year. Finally, the third box adds up the pointwise contributions from the second plot, yielding a plot of the cumulative effect of the SB 1070.

4.7 Potential issues with the data

In addition to the unobserved confounding issue analyzed above, we will discuss another factor that may have adverse impact on the internal validity of the study. As mentioned in Introduction section, the data come from the Survey of Migration to the Northern Border (EMIF) which is conducted by Mexican authorities. The National Population Council estimates that 94 percent of the total border crossings occur through locations convered by the survey. However, these locations consist of bus stations, train stations, international bridges, and custom inspection points. The ability of these sites to provide a representative sample of those who are planning to cross the border illegally is questionable. Even though the survey was conducted in the southern side of the border, which makes the respondents more comfortable to give truthful answers, choosing sensitive public places like inspection points and international bridges to give out the survey will less likely produce a representative sample. It was claimed that more than 90 percent of total crossings happened at these sites, but it was unclear how much of that were illegal crossings. Nonetheless, it is obviously unreasonable to assume that a significant amount of illegal crossings happened at locations mentioned above, due to the heavy presence of law enforcement officers.

Another concern about the data is that it provides surveyed information. The core question of the survey is whether one is planning to cross the border and reside in Arizona, but we do not know if one will carry out that plan successfully. Unfortunately, the study relies entirely on the truthfulness of the respondents' answers. The author acknowledge this concern and when comparing with the American Community Survey (ACS), which is a similar survey that collects information from Hispanic immigrants in U.S. southern states, they find low correlation scores between the two data. In particular, the respondents in the EMIF are relatively younger by roughly 5.5 years, have 2 years less of education, and are 90% percent male versus 65% in the ACS. Details about the main differences are reported in Table 9.

There are some explanations for this concern. The possibility that both surveys are not representative as intended is ruled out by H & O-A. Instead, it is claimed in the paper that since the two surveys target different groups, settled immigrants in ACS versus out-of-country potential migrants in EMIF, the measures should differ between the two set of data. They also believe that the difference is caused by the fact that 40% of ACS respondents are legal immigrants versus less than 10% in EMIF, and is consistent with the known differences between the two groups (Fry, 2006; Passel and Cohn, 2009; and Passel and Cohn, 2011). In other words, it is concluded that the other 40% of the ACS respondents, who claimed to have legal status, is source of the difference. However, this explanation contradicts with another claim from H & O-A. They argue earlier that since the EMIF survey is conducted in the southern side of the border, the respondents are more willing to reveal their undocumented status, and hence, has advantages over the ACS survey data. Based on this argument, the proportion of the ACS respondents that are undocumented could be much higher than the reported 60%, and the two samples could then be considered to be from the same population. Regardless of whether the respondents in ACS survey lied about their status, there is no reason for them to lie about their background information. This ultimately weakens the attempt to defend the credibility of the EMIF data set.

4.8 External Validity

Finally, we will discuss the external validity of the prior study results. While the analysis on the internal validity focuses on whether the result of a particular research is built upon a strong foundation and can stand against other possible explanations, external validity analysis examines if such result can be applied in other scenarios. High internal validity comes at the expense of poor external validity and vice versa. In addition to some legitimate concerns about their internal validity explained above, the results in this study may not come with strong external validity either. The ultimate goal of the H & O-A research is to answer the question of how efficient federal and state immigration policies are in deterring undocumented workers from entering the United States. Obviously, we cannot generalize the estimated effect of the Arizona SB 1070 legislation, which was created by the State of Arizona and only applicable within Arizona borders, to other parts of the country. Furthermore, this research only involves the migrants that intent to cross at the southern border, by land, have not been issued any non-immigrant visa, and most likely are citizens of South American countries. Other local factors such as labor market and other immigration-unrelated laws can also have large enough effect that can, in the worst scenario, eliminate the deterring effect of immigration laws. As a result, the conclusion made by H & O-A can only be applied in these specific settings.

5 Conclusion

To summarize, the conclusions that Arizona SB 1070 law effectively defer migrants from crossing the border illegally to reside in Arizona faces challenges for both internal and external validity. There are reasonable concerns about the choice of data, the covariate balance of the respondents, and the mixed robustness values among different study designs. However, some of these concerns can be alleviated if we can obtain a more sufficient set of data which includes more background information of the respondents as well as a clear explanation of how other time-variant covariates were calculated or obtained. Even though the robustness values are inconsistent between the models, the conclusions drawn from these models are ultimately similar. The extra Baysesian Structural Approach in section 4.6 also agrees with H & O-A's conclusion. Nonetheless, as explained in section 4.8, the diverse background of the migrants, time and local-varying factors such as economical health and social services available to undocumented workers can tremendously reduce the effect size. Hence, even if all of the internal validity concerns are resolved, we still may not be able to directly apply the authors' conclusion at broader levels.

6 Future Analysis

While enhancing the external validity of studies about immigration policies may be challenging because of the locality nature of immigration problems, the internal validity can potentially be improved through more thorough data collecting, accounting for covariate balance in the groups, and controlling for additional observable confounders. In addition to strengthening the robustness of the results of the initial study, one may also attempt to take a more comprehensive approach to answer the question of interest by studying other aspects and the root causes of the problem. The potential avenues for future analyses include discussing about the social connections of illegal immigrants in the US, the social backgrounds of their country of origin, the trade-off between the short term adverse consequences certain policies utilize to hinder these immigrants versus the possible long term fear they may face at home, how foreign governments respond to the passage of new immigration policies in the U.S., etc. Ultimately, an analysis that aims to refute or defend the initial findings should probably focus more on the local validity of the estimated effects of the Arizona law.

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