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**Do immigrant outflows lead to native inflows?**

**An empirical analysis of the migratory responses**

**to US state immigration legislation<sup>\*</sup>**

Michael Good<sup>†</sup>

I estimate the impact on population and employment for 52 different demographic groups of the recent influx of state omnibus immigration laws targeting undocumented immigrants in the United States. I find evidence that while the demographic groups pinpointed as having higher percentages of undocumented individuals certainly experience population and employment 'outflows' from states implementing these immigration laws, there is a lack of associated 'inflows' for those demographic groups identified by economic theory as being probable substitutes for undocumented immigrants. Several segments designated as probable substitutes actually experience an adverse effect on population and employment. This finding provides rigorous empirical backing to existing anecdotal evidence of the same migratory phenomenon, resulting in clear policy implications in relation to the ongoing debate over immigration.

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## I. Introduction

The recent influx of state immigration legislation in the United States provides an ideal quasi-experimental setting appropriate for examining the connection between immigration and internal migration. Since 2006, eleven different states have enacted fourteen omnibus immigration laws, implementing broad restrictions in relation to issues affecting immigrants such as work authorization, public program benefits, education, human trafficking and the transport and harbor of unauthorized immigrants, identification and driver's license policies, and document-carrying policies. A simple analysis of the incentives created by these wide-reaching omnibus laws leads to an expectation of outflows of the undocumented immigrant population from those states implementing immigration laws, as well as possible inflows among other demographics not specifically targeted by the new laws. Anecdotal evidence certainly provides backing to this expectation of immigrant outflows, as there are numerous reports of large out-migrations, especially of the undocumented population, in states which have recently put omnibus immigration laws into practice.<sup>1</sup> However, much of this same anecdotal evidence also points to a lack of inflows of other demographic groups into the immigrant-vacated jobs, leading to a shortage of workers in certain areas where the outflows have been particularly strong. This anecdotal evidence begs the empirical analysis of a simple yet important question, which is the focus of this study: *Do immigrant outflows lead to native inflows?*

The approach to attack this question utilizes a straightforward, two-step process. First, I use microdata from the Current Population Survey (CPS) to analyze in great detail the pre- and post-implementation numbers relating to both population and employment of demographic groups targeted by the state immigration laws. By use of a difference-in-differences estimation that exploits the natural experiment setting and employs a treatment and control group (the treatment being the implementation of the state immigration law), I am able to verify if there truly are immigrant outflows in response to the change in state policy. Second, using the same microdata along with

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<sup>1</sup>See Robertson (2011) for one of many articles documenting this migratory response.

the treatment and control process, I analyze the pre- and post-implementation numbers of the demographic groups not targeted by the state immigration laws, permitting the identification of any native (or documented immigrant) population or worker inflows (outflows) in those states experiencing targeted immigrant outflows.

The importance of the study's findings is twofold. First, the documentation of the actual migratory response to the surge of state immigration laws is important in its own right. While much anecdotal evidence has been accumulated, detailed empirical analyses are lacking, possibly partly due to the relative newness of this trend in immigration legislation and the difficulty inherent in identifying the undocumented population. Second, there is an established literature examining the relationship between immigration and internal migration responses. However, despite numerous studies, this debate laden with key policy implications still has no clear victor. The present study contributes to the ongoing debate by providing additional rigorous empirical evidence as to the relationship between immigration and internal migration responses. My analysis indicates that while the demographic groups pinpointed as having higher percentages of undocumented individuals indeed experience population and employment outflows from states implementing these immigration laws, there is a lack of associated inflows for those demographic groups identified by economic theory as being probable substitutes for undocumented immigrants. Although minimal substitution is present, several segments of the population designated as probable substitutes actually experience an adverse effect on population and employment.

To my knowledge, this is the first paper that exploits the implementation of state omnibus immigration laws to explicitly examine the question posed above: Do immigrant outflows lead to native inflows? Card and Dinardo (2000) asks the question 'Do immigrant inflows lead to native outflows?' in the very title of the article - the experiment provided by the implementation of the current state immigration legislation now allows for a natural examination of the opposite side of their question, arguably just as important. This paper is most related to Raphael and Ronconi (2009), which finds a significant out-migration of Hispanic immigrants from states passing immi-

gration legislation. However, while similar in approach, their article examines only immigration laws specifically targeting employment and the sample is limited to pre-2009 implementation of laws, excluding a majority of this paper's richer sample.

After providing a brief background of recent U.S. state immigration law and a review of the relevant economic theory in section 2, I give specifics in section 3 as to the data and my empirical strategy. Section 4 discusses the results of the difference-in-differences estimations for the various demographic groups. I check for sensitivity and robustness in section 5, concluding in Section 6.

## **II. State Immigration Law**

### *A brief overview*

Evident from the statistics provided by the National Conference of State Legislatures, the recent growth in the number of state immigration laws enacted in the United States is astounding. Table 1 outlines the number of immigration laws by category passed by state legislatures during the period of 2005 to 2011, the number vetoed by governors, and the number ultimately adopted by states.<sup>2</sup> A majority of the laws target a specific issue in relation to undocumented immigrants, such as work authorization, public program benefits, or identification and licenses. While these focused laws certainly lower the incentive for an undocumented immigrant to reside in the respective state (some much more than others), a handful of states have implemented omnibus laws that address multiple issues at the same time. These broad-reaching laws theoretically have the largest effect on any outflows of immigrants, due to their nature of affecting not only employment opportunities, but also possibly limiting access to food, health, and education benefits, and in general creating an environment in which there is a constant threat of document verification and subsequent deportation. While the number of states enacting these multi-issue laws has been limited, 2011 brought a

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<sup>2</sup>All statistics in this section are obtained from the National Conference of State Legislatures website, [www.ncsl.org](http://www.ncsl.org). All categories listed at [www.ncsl.org](http://www.ncsl.org) are included in Table 1 with the exception of 'resolutions,' due to the fact that these laws tend to have no relation to a detrimental effect on immigrants. Budget laws are included in the 'Miscellaneous' category.

particular surge - five states enacted omnibus legislation, while some eight states had similar legislation pending by year's end. Appendix 1 lists the fourteen states having already passed omnibus laws, accompanied by a summary of the issues addressed in each law.

### *Theoretical impact*

Understanding the theoretical effect of the implementation of state omnibus immigration laws on the population and employment numbers of undocumented immigrants involves a simple cost and benefit analysis. Assuming that each individual has some benefit and some cost corresponding to living (working) in a particular geographic location, state immigration laws result in an increased cost burdening each undocumented individual.<sup>3</sup> This cost could take various forms: an evasion cost, a discrimination cost, or a psychological cost, just to name a few. If all individuals only accept positive payoffs, a first indicator of an individual's (re)location decision is if the associated benefit is still higher than the now greater cost. However, even if the total payoff remains positive, the relevant indicator is the comparison of that total payoff to all other available payoffs, those potentially realized by migration to another US state or migration to another country. Clearly, if other possible payoffs do not differ greatly from the payoff of the immigrant's state of residence before the implementation of omnibus immigration law, the extra cost imposed by implementation could change an individual's payoff-maximizing location, ultimately causing out-migration. However, if a particular location gives a pre-implementation payoff much higher than all other options, a payoff-maximizing undocumented individual could rationally choose to stay in a state even after implementation of immigration law.

An additional potential impact of immigration law is the effect implementation may have on employers, outlined in Raphael and Ronconi (2009). Most omnibus laws include some measure related to employment status verification, punishing employers caught hiring undocumented work-

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<sup>3</sup>Although a substantial literature examines individuals' decisions to migrate, many times detailing the specific associated costs and benefits at great length, I choose to lump these into one generalized cost and benefit for each individual for reasons of simplifying the demonstration of the theoretical impact of immigration law.

ers. If this is the case, employers now bear a higher cost related to hiring this segment of the population, translating into a lower payoff for undocumented individuals as the probability of obtaining the benefit associated with employment decreases.

Whether or not this hypothesized outflow of immigrants in turn incentivizes a response from the native or documented immigrant population is a more complicated question, due to the fact that the answer depends on the substitutability of these groups, a topic that numerous studies continue to examine. As much of the undocumented population is relatively low-skilled, these studies generally focus on the production substitutability of low-skilled natives (or low-skilled, foreign-born citizens) for immigrants. Ottoviano and Peri (2012) estimates that natives and immigrants of similar skill in the U.S. are imperfect substitutes, while Borjas *et al.* (2008) estimates an infinite substitution, pointing to the perfect substitutability of these demographic groups. After finding imperfect substitution among similarly-skilled natives and immigrants, Card (2009) points out that those most affected in terms of employment by the arrival of immigrants are those immigrants who had previously established residence in the area under examination.

As the issue of substitutability continues to be debated, various additional studies empirically analyse the related topic of whether immigrant inflows are associated with native outflows (the partner research question to the present study), often focusing on how these flows may in turn affect workers' wages. Studies such as Filer (1992), Frey (1995), Borjas *et al.* (1997), and Borjas (2006) contend that immigration inflows do lead to native outflows; on the other hand, others including Wright *et al.* (1997), Card (2001), Kritiz and Gurak (2001), and Peri (2008) argue that native outflows, if present, do not stem from immigration inflows.

One of the central economic arguments stated by immigration law supporters, that of 'immigrants take natives' jobs,' follows directly from the findings of the former group of studies. In the current context of state immigration legislation, this reasoning implies that if immigrants in fact emigrate to another state (or country), natives will fill those jobs that were previously 'taken.' In the words

of omnibus immigration law author Kris Kobach, this implication taken literally translates into 'if you want to create a job for a US citizen tomorrow, deport an illegal alien today.'<sup>4</sup> Logically, the possible inflow of natives could manifest itself in one of two fashions: 1) population *and* worker inflows of natives moving from other states looking to replace the immigrant-vacated positions, or 2) worker inflows from the native population already present in the respective state (previously either unemployed or not in the labour force). The latter possibility includes no evident change of the native *population*, as the only movement is in terms of employment. Following this line of thinking, this expected native inflow should be especially notable in times of high unemployment, such as those experienced during much of the period in focus.<sup>5</sup> Furthermore, since the geographical unit under examination is at the state level, the empirical setup is particularly conducive to observing this native inflow. Borjas (2006) points out that larger outflow (inflow) effects as a native response to immigration are found 'the easier that natives find it to "vote with their feet".' If inflows are indeed stemming mostly from within-state individuals previously not employed, this study obtains the largest possible inflow observable since there is absolutely zero cost to moving. In fact, a native inflow into employment does not even necessarily require 'voting with their feet.' Considering these conditions stacking the deck in favor of observing native inflows, if these inflows do not accompany immigrant outflows, the economic argument supporting stricter immigration policy quickly loses any traction it may have had.

### **III. Data and Empirical Strategy**

In detailing the different possible migratory manifestations, I examine the data from the CPS both in terms of population and employment for all individuals ages 20 to 60. Monthly microdata covers six years, allowing for analysis of the period August 2005 to September 2011. I classify the popu-

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<sup>4</sup>I first encountered this commentary from the Kansas Secretary of State, who doubles as immigration advisor to US presidential candidate Mitt Romney, as reported by Univision's *Noticiero Edición Nocturna* (Nightly News Edition). Video of Kobach's related comments, as part of the Conservative Political Action Conference (CPAC), is readily accessible on the Internet.

<sup>5</sup>Unemployment rates calculated from the CPS reflect an average of nearly 7% unemployment in states at respective times of omnibus law implementation. While high compared temporally to adjacent periods within states, the average is below the national unemployment rate average of 7.7% for the same month-year combinations of implementation.



lation and employment data according to 52 demographic groups, forming an essential part of my empirical strategy. Demographic indicators include citizenship/nativity, race/ethnicity, and education. Citizenship/nativity divides the population into four groups: US citizen/born in US, US naturalized citizen/foreign-born, noncitizen/foreign-born, and noncitizen/foreign-born/arrived to US after 1982. While the first three are clearly mutually exclusive, the latter two are not, intentionally allowing for what I call 'demographic narrowing.' Demographic narrowing is my main strategy for pinpointing and comparing the demographic groups most likely (not) affected by the implementation of immigration law. This necessity arises due to the fact that the CPS and other available data sources do not specify legal or illegal status of the noncitizen population. However, by use of undocumented population estimates and a well-constructed demographic narrowing process, I can confidently identify those groups that are theoretically most affected.<sup>6</sup> As an example, the specification of noncitizen/foreign-born/arrived to US after 1982 allows for pinpointing the narrowest group most likely targeted by the laws in terms of citizenship/nativity status. This group should have more members of illegal status than the noncitizen/foreign-born because of the Immigration Act of 1986, which legalized the residency of any immigrant present in the US prior to 1982. While the obvious presence of many legal residents in the 'noncitizen/foreign-born/arrived to US after 1982' category leaves this specification far from a perfect identification of the undocumented population, the narrowing achieves a second-best approximation of the targeted population.<sup>7</sup>

Race/ethnicity separates individuals into four groups as well: white/non-Hispanic, black/non-Hispanic, Asian/non-Hispanic, and Hispanic. This specific, perhaps unorthodox classification marks an intentional effort to avoid potential ambiguity, given the manner in which the CPS race and ethnicity questions are formulated. The survey classifies race by the options of 'white only,' 'black only,' 'American Indian only,' 'Asian only,' 'Hawaiian only,' or any combinations of the

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<sup>6</sup>Passel and Cohn (2009) outlines these estimates, signaling for example, that of the 11.9 million undocumented immigrants in the US, 76% are Hispanic.

<sup>7</sup>Achieving the 'best approximation,' i.e. exact identification of the undocumented population, through surveys such as the CPS may actually not be ideal for this paper's purposes. If survey participants were explicitly asked about legal residency status, many undocumented residents would likely not willfully respond to the survey, creating an even stronger tendency toward non-response from this demographic than that which already exists.

above; Hispanic or non-Hispanic status is determined in a separate question. The problem arises from the fact that these two classifications exhibit overlap; nearly all individuals who identify themselves as Hispanic also identify race as white or black, with a lower number identifying the other race categories. If this overlap were not accounted for and individuals were in turn classified simply as white, black, or Hispanic, a majority of Hispanics would be double-counted, causing an enormous problem of bias. Education simply classifies people as low-skilled or high-skilled, completion of high school marking the upper limit of the low-skilled category.<sup>8</sup>

For each of the 52 demographic groups, I specify an econometric model of the following form.

$$Y_{it} = c + \alpha_i + \gamma_t + \beta_1 Treat_{it} + \beta_2 Post_{it} + \beta_3 Treat_{it}Post_{it} + \epsilon_{it} \quad (1)$$

This fixed effects/difference-in-differences specification employs dummy variables for all states and month-year combinations, represented by  $\alpha_i$  and  $\gamma_t$ , respectively.  $Y_{it}$  is a placeholder for the variable of interest, either population or employment of state  $i$  in month-year  $t$  for each demographic group.  $Treat_{it}$  is a dummy variable taking a value of one for all  $t$  if state  $i$  receives treatment (omnibus immigration law implementation) at any point in the 2005 to 2011 period.  $Post_{it}$  is a dummy variable taking a value of one if for state  $i$ , period  $t$  is post-treatment. In turn,  $Treat_{it}Post_{it}$  takes a value of one only when state  $i$  actually receives treatment in month-year  $t$ .  $\alpha_i$  controls for any observable or unobservable systematic differences across states, while  $\gamma_t$  controls for any differences over time that may affect all states' outcomes, such as changes to federal immigration law.  $\beta_3$  is the coefficient of interest, the difference-in-differences estimate representing the change in the lefthand-side variable associated with omnibus immigration law implementation.<sup>9</sup> I

<sup>8</sup>This education dividing line is selected based on results from Card (2009) finding that 'high school equivalent' and 'college equivalent' workers are imperfect substitutes.

<sup>9</sup>While the difference-in-difference-in-differences (DDD) method is an attractive alternative to difference-in-differences (DD), sometimes allowing for a 'more convincing analysis of a policy change' (Imbens and Wooldridge, 2007), DDD does not improve on DD in this study's case. The inclusion in DDD of the within-state control group is problematic due to the fact that I expect some effect on the population and employment of those 'non-immigrant' demographic groups in states receiving treatment. If these groups were to form a within-state control group as part of the DDD estimator, the measured effect of the state immigration legislation would obtain an upward bias. By employing DD, I avoid this potential bias and therefore am able to separate out the effects on the various demographic groups and their respective responses to implementation of the immigration laws.

examine population and employment of the demographic groups in terms of both proportions and levels. First, by expressing population (employment) as logarithms, I use the specified equation to estimate the percentage change in the variable of interest associated with state immigration law implementation. Second, I use the logarithm of the demographic group to state total ratio for each variable of interest to estimate percentage changes in the composition of population (employment) associated with the implementation of state immigration law.<sup>10</sup>

For each set of regressions, I define two different control groups: 1) neighboring states of each respective state receiving treatment and 2) all U.S. states.<sup>11</sup> While use of the first control group permits a potential bias due to the possibility of outflows (inflows) affecting mainly neighboring states, this possibility seems not to have manifested itself. The data shows only slight changes in neighboring states' population and employment after immigration law legislation. Furthermore, this is to be expected - the immigrant outflows may be large especially in terms of percentages, however the associated levels result in only small changes for receiving states when spread out over many states of relocation. Nonetheless, the inclusion of the second control group, all US states, allows for a simple comparison, theoretically minimizing any bias that may exist through use of neighboring states in the regional control group.

The nature of the state omnibus immigration legislation process presents an additional problem in relation to defining the point of separation for pre- and post-treatment, due to the varying lapse of time between law enactment and implementation as well as various challenges to the legality of the omnibus laws (outlined in Appendix 1). I choose the established implementation date (labelled as "effective" in Appendix 1) as the separation between pre- and post-treatment. Legislation establishes this date at least by the time it is enacted, therefore giving a period on average of six

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<sup>10</sup>For each ratio, the population (employment) of the specific demographic group being examined serves as the numerator, while the state total serves as the denominator.

<sup>11</sup>I exclude states from control group consideration if treatment is received during or prior to the respective legislation implementation period under examination. For the baseline regressions, six months on either side of the legislation's implementation date form the legislation implementation period and enter as observations into the regressions.

months for the population to adjust their employment/residency plans according to their expectations. Therefore, even in states such as Oklahoma where legal challenges postponed complete implementation of the immigration legislation, these expectations and accordingly adjusted plans presumably still have an effect as far as population and employment movements resulting from the omnibus laws.

Two additional aspects of my empirical strategy address specific critiques as to the preciseness of difference-in-differences estimation expressed in the recent literature. First, by aggregating population and employment data at the state level rather than including individual-level observations from the CPS in the specified regressions, I avoid the problem of common group errors presented by multilevel data emphasized in Donald and Lang (2007). In addition, I follow the recommendations of Bertrand *et al.* (2004) in calculating Huber-White robust standard errors clustered by state for inference purposes, allowing for unrestricted error correlations across observations within states and thereby accounting for the serious concern of serial correlation in the data.

#### **IV. Results and Discussion**

Of the 52 demographic groups identified by the three indicators in this study, Passel and Cohn's figures help point to the group of 'noncitizen/foreign-born/arrived to US after 1982/Hispanic/low-skilled' as containing the highest percentage of undocumented population, and in turn theoretically most likely to be negatively affected in terms of population and/or employment by state immigration law implementation. In documenting the migratory outflows associated with the state omnibus laws, I include estimates in Tables 2 and 4 for this narrowest demographic, as well as the following broader demographic divisions, in descending order of expected percentage of undocumented population: 'noncitizen/foreign-born/arrived to US after 1982/Hispanic,' 'noncitizen/foreign-born/arrived to US after 1982,' 'noncitizen/foreign-born/Hispanic/low-skilled,' 'noncitizen/foreign-born/Hispanic,' and 'noncitizen/foreign-born.' State immigration law's impact on population is outlined in Table 2, while the impact on employment is below in Table 4.

In attempting to gauge the impact of the laws and any subsequent immigrant movements on the behaviour of other demographic groups, I focus on several segments of the population with no undocumented individuals that economic theory has presented as possible substitutes for the already-mentioned groups with the highest percentages of undocumented immigrants.<sup>12</sup> Borjas *et al.* (2011) argues that similarly-skilled immigrants and natives are perfect substitutes according to US labor evidence, while Card (2009) suggests that if immigrants and natives are imperfect substitutes, additional immigrants would be the group designated as nearest-to-perfect substitute for immigrants already present in the US. I take both findings to the data in the context of my study, documenting the following demographic groups' population changes in Table 3 and employment changes in Table 5: 'native/Hispanic/low-skilled,' 'native/white, non-Hispanic/low-skilled,' and 'native/black, non-Hispanic/low-skilled' in order to capture any native-immigrant substitutability, and 'naturalized citizen/foreign-born/Hispanic/low-skilled' in order to capture any immigrant-immigrant substitutability. To further shed light on the population and employment movement responses to immigration law, I also include the total change of state population (employment) associated with implementation of the legislation.

### *Impacts on Population*

Fig. 1 gives an idea as to the migratory outflows of immigrants in terms of population during the implementation period of omnibus immigration legislation. However, although an average decline in the examined demographic's proportion is observed in the six months after implementation, this decrease is evidence from raw data not yet taking advantage of the treatment and control difference-in-differences methodology necessary for obtaining the desired estimates of interest. Panel A of Table 2 shows the impact on population associated with the immigration law implementation for those demographic groups having the highest percentages of undocumented immigrants. I estimate that the implementation of omnibus laws is associated with a 24.41% decrease in the low-skilled,

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<sup>12</sup>By substitutes, I refer to the terminology used in labour economics, which identifies groups that tend to substitute or complement one another, classified according to characteristics such as citizenship status, race/ethnicity, and skill level.

Hispanic, noncitizen population arriving post-1982 to the US, when comparing states receiving treatment to those geographic neighbors not receiving treatment.<sup>13</sup> While this group is the group weighted most heavily with undocumented immigrants, even the group of foreign-born noncitizens, which certainly has a much lower percentage of undocumented population, experiences a decline of 9.93% associated with the law implementation. When the alternative control group of all US states is used, minimizing the possible bias created by expected inflows to neighboring states, the magnitude of these effects are only slightly attenuated, 19.02% and 8.22%, respectively. Interestingly, since the total population also experiences an associated decrease, albeit small in magnitude, the proportion changes detailed in Panel B of Table 2 are smaller in percentage terms than their Panel A level counterparts. All estimates in Table 2 are statistically significant at the 5% level, indeed pointing to a strong outflow of (undocumented) immigrant population in response to the state omnibus immigration laws.

Estimates in Table 3 outline the impact on those groups identified by economic theory as possible substitutes for immigrant groups, therefore leading to an expectation of inflows associated with immigrant outflows. However, of the four groups examined, only the native, black/non-Hispanic, low-skilled group has an associated population increase, magnitude of 6.51%. Both native and naturalized citizen groups of low-skilled Hispanics actually reflect a clear population decline, losing 11.57% and 4.70% respectively. While these estimates are not as significant as those in Table 2, the evidence does point to a lack of 'replacement' population for the outflow of immigrants. This fact is confirmed by the slightly negative estimates for total state population, -0.61% when employing the regional control group and -0.45% when including all US states. The negative impact on groups not targeted by omnibus legislation is a noteworthy finding, likely due to one or both of two possibilities. First, low-skilled, Hispanic legal immigrants (or natives) may be suffering a type of workplace discrimination. As some of the omnibus legislation affects employers directly, some employers may be more reluctant to hire any worker who fits whatever profile the employer

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<sup>13</sup>The estimates reported in tables 2 to 5 approximate percentage changes, as population (employment) is expressed in logarithmic form and estimates are then multiplied by a factor of 100.

may associate with undocumented workers. On the other hand, this result could reflect that individuals in this demographic anticipate discriminatory treatment as a result of the immigration laws and therefore relocate, or simply decide not to live and work in a place where laws could unfortunately lead to instances of racial profiling. An additional consideration mentioned in Raphael and Ronconi (2009) is the fact that some immigrant families are made up of both undocumented and documented immigrants, leading to the out-migration of all family members when the undocumented are targeted by immigration law. In either case, these figures point to a central complaint of opponents of the state omnibus immigration laws - that on top of the negative effect on undocumented immigrants, the legislation most likely has an unintended, adverse effect on certain segments of the legal, documented immigrant population.

### *Impacts on Employment*

Fig. 2 outlines the migratory outflows of immigrants in terms of employment during the implementation period of omnibus immigration legislation. Panel A of Table 4 displays estimates for the immigrant demographic groups' that reflect a decline in employment associated with implementation of the immigration laws. Estimates range from -20.73% to -10.62%, evidencing a notable fall in employment, however generally slightly smaller in magnitude than that of these same groups' population change using the regional control group. In line with the population estimates, use of all US states as the control group results in employment estimates of a lower magnitude for the examined immigrant demographic groups, with the largest effect being -16.32% for the noncitizen, Hispanic, low-skilled, arrived to U.S. post-1982 group. Once again, estimates for these groups are statistically significant, with only that of noncitizen/Hispanic not being significant at at least the 10% level, the majority significant at the 5% level.

In examining the possible substitute groups listed in Table 5, the estimates for employment highlight two important trends. First, the immigration legislation again appears to adversely affect segments of the population other than those groups with high percentages of undocumented im-

migrants, even groups that are hypothesized to be substitutes for the immigrant groups. Panel A of Table 5 shows a 10.89% and a 14.60% decrease in employment level, respectively, for native, low-skilled Hispanics and naturalized citizen, low-skilled Hispanics when using regional control groups. While these estimates do vary when changing to all US states as the control group of choice, they remain clearly negative. The same two possibilities hypothesized above in the discussion of the population estimates provide the clearest explanation as to why this result obtains. Second, for the group of native, low-skilled blacks (non-Hispanics), there does appear to be some substitute or replacement effect in terms of employment. Estimates of 7.79% and 10.68% for the regional and overall controls, respectively, signal a positive impact on employment for this demographic. However, this rise in employment is not shared by native, low-skilled whites (non-Hispanics), and the estimates throughout Table 5 are plagued by lower significance compared with those of Table 4. In fact, the overall gauge of the impact on employment, the estimate for the state total, is slightly negative using both control groups, just as in the case of population.

## **V. Checks on Initial Estimates**

While the above estimates paint a relatively clear picture of the migratory responses to state immigration law implementation, the skeptical reader may raise concerns as to several issues worth addressing, including the robustness, sensitivity, and reliability of the estimates.

### *Robustness and Sensitivity*

To this point, results obtain through the examination of a one-year implementation period, including all implementing states according to their effective date even if the actual implementation faces delays due to legal challenges. In order to first check the robustness of these results, I explore what changes may occur if Oklahoma's effective date of November 2007 is excluded from the treatment group. As detailed in Appendix 1, the courts initially prevented a major section of Oklahoma's omnibus law from taking effect. As previously explained, I include Oklahoma in the initial treat-



ment group based on those expectations that accompany the effective date, even if not all sections of the omnibus legislation ultimately take effect on this date. However, examining a second set of regressions excluding Oklahoma allows for a simple check of whether the initial results are unduly influenced by Oklahoma's inclusion.

Tables 6 and 7 clearly show that the baseline results are robust to the exclusion of Oklahoma's effective date from the treatment group. The coefficients displayed in Table 6 show all negative estimates larger in magnitude than those of Tables 2 and 4. This makes logical sense, since some immigrants waiting until the last moment (the expected effective date) to out-migrate may have opted to remain in Oklahoma when the employment section of this particular legislation did not take immediate effect, resulting in a smaller outflow than that which would have otherwise occurred. As in Table 2, all population estimates are significant at the 5% level, however unlike Table 4, all employment estimates are now significant at the 5% level, as well. Comparing Table 7 with the corresponding Tables 3 and 5, population estimates in Table 7 are larger in magnitude (both positive and negative), with no changes in sign from the previous tables. For example, using the regional control results in a 14.53% decrease for native, low-skilled Hispanics and a 11.44% increase for native, low-skilled blacks (non-Hispanics). On the other hand, Table 7 lists somewhat attenuated employment estimates for the regional control, all coefficients consistent in sign with the exception of native, low-skilled whites (non-Hispanics). Estimates for the change in total population (employment) are consistent with the baseline regressions, ranging from -0.36% to -0.57%, although just as before the estimates for 'natives' are not as significant as those for 'immigrants.'

Turning to the issue of timing, I examine how sensitive the results are to changing the length of the implementation period. Tables 8 and 9 display the estimates of interest given a total implementation period of 6 months in Panel A, while estimates from total implementation periods of 2 years are in Panel B.<sup>14</sup> Six-month population and employment coefficients from Table 8 are extremely

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<sup>14</sup>These are equivalent to a pre- and post-implementation period of 3 months each and 1 year each, respectively, compared with the baseline regressions using a pre- and post-implementation period of 6 months each.

similar in magnitude and significance to those of Tables 2 and 4, however two-year coefficients are less significant and attenuated in magnitude, employment estimates for foreign-born non-citizens turning slightly positive. While the magnitude and significance vary for six-month estimates in Table 9, all signs remain consistent with those from Tables 3 and 5 except for population estimates related to naturalized, low-skilled Hispanics. Two-year coefficients paint the same picture as the corresponding six-month estimates, however there is additional sign disagreement in the estimates for native, low-skilled blacks (non-Hispanics). In summary, while the baseline results do show some sensitivity to altering the length of the examined implementation period, both the six-month and two-year samples confirm general trends and resulting conclusions, the six-month results even matching those of the baseline regressions in magnitude and significance.

### *Endogeneity Bias?*

An important concern as to the reliability of the difference-in-differences estimates is the issue of endogeneity. The implementation of state omnibus immigration legislation must be exogenous in order for the estimates to capture the true effect on population and employment, free from any endogeneity bias. Raphael and Ronconi (2009) addresses this concern in detail, pointing to evidence from the Pew Hispanic Center's National Survey of Latinos (NSL) as discounting the possibility that the laws' implementation are actually driven by other changes, leading to a problem of endogeneity. They examine in detail both the possibility of higher levels of discriminatory attitudes as well as that of *changes* in levels of discriminatory attitudes leading to immigration law implementation; however, through the data provided by the NSL both of these possible arguments of endogeneity are discarded.

An additional cause of endogeneity could arise from the simple case of the (undocumented) immigrant population increasing in size to a certain percentage of a state's total population, arriving to a tipping point which in turn triggers the beginnings of the process of immigration law en-

actment and implementation.<sup>15</sup> It is common knowledge that a network effect indeed exists in the migration decision, resulting in regions with high percentages of immigrants tending to draw more immigrants.<sup>16</sup> However, while the network effect could point to a tipping point eventually obtaining, its existence is clearly not sufficient for the tipping point to obtain. In fact, evidence from Passel and Cohn (2009) refutes the tipping point theory, citing the fact that the 'undocumented immigrant population grew rapidly from 1991 to 2006 but has since stabilized.' Since this stabilization occurred prior to the influx of state omnibus immigration legislation, any hesitation as to endogeneity is somewhat pacified. However, to further examine this possibility, especially given the fact that a lag between stabilization and immigration legislation implementation could occur, I again examine the narrowest demographic - noncitizen/foreign-born/arrived to US after 1982/Hispanic/low-skilled - specifically, I examine the exact proportion of each state's population made up by this group, measured one year before implementation of omnibus legislation. This date serves as an approximate marker for when the legislative process of proposing and discussing new legislation actually takes place. Fig. 3 (Fig. 4) shows the corresponding proportions of population (employment) for each state implementing omnibus law, along with the proportions of the same demographic group for all other states not implementing immigration law during the same period. Table 10 lists the rank of each proportion compared to all other state counterparts for the same time period.

Confirming the evidence from Passel and Cohn (2009), there does not appear to be a tipping point that sets in motion the adoption of omnibus immigration legislation, as the proportions exhibit a wide variety in their rankings. Using regional states as a comparison group, Georgia, Utah and Arizona each rank third in the population measure among their respective regions, however Missouri, Oklahoma and South Carolina rank in the bottom half of their regions, South Carolina ranking only

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<sup>15</sup>One might imagine a different sort of tipping point, in which the (undocumented) immigrant population increases in size until it gains sufficient political power to in turn influence immigration policy away from omnibus legislation. However, I examine the tipping point in terms of triggering omnibus legislation due to the fact that many immigrants (clearly, undocumented immigrants) do not have voting rights in the US and therefore have minimal political clout.

<sup>16</sup>For a sample of the literature examining network effects and the migration decision, see McKenzie and Rapoport (2007).

ninth out of eleven. Regional employment rankings reflect much the same, although Nebraska joins the group of low-ranking states. Switching to all US states as a comparison group, Arizona and Utah both experience relatively high overall proportions of population (employment) of the targeted demographic group one year before implementation, however other implementing states can be classified as having relatively moderate or low proportions. Missouri, South Carolina and Oklahoma certainly qualify as having relatively low proportions of both population and employment, with overall population rankings of 27, 26 and 27, respectively. Additionally, Nebraska's overall rank of 30 for employment is the lowest statistic among the legislation-implementing states.

Table 11 outlines a further test providing evidence against the tipping point theory. Panel A displays the results of regressing the implementation of state immigration law on the one-year lag of the narrowest demographic proportion detailed above, while Panel B shows the results of regressing the implementation of state immigration law on the one-year lag of the *change* in the narrowest demographic proportion. Any positive relationship between this (change in) proportion and subsequent immigration law implementation should clearly appear as positive, significant coefficients in the regression results. However, coefficients are insignificant and imprecisely estimated, each respective 95% confidence interval including the possibility of both a negative and a positive relationship between the two respective variables. Furthermore, if these estimates were significant and precisely estimated, the association between the variables would remain small in scale given the interpretation of the estimates. The highest coefficient in magnitude in Panel A signals that even a one-unit increase in the narrowest demographic proportion (not possible since the independent variable is a proportion measured between zero and one) is only associated with a 51% increase in the probability that a state implements omnibus immigration legislation. Correspondingly, the coefficient of highest magnitude in Panel B signals that a one-unit increase in the change of the narrowest demographic proportion is associated with a 24% *decrease* in the probability that a state implement omnibus legislation.

Together with the evidence provided by Raphael and Ronconi (2009) and Passel and Cohn (2009), these trends (or lack thereof) strongly signal that the tipping point theory is not in line with empirical fact. The identified empirical strategy and resulting estimates are indeed free from worries of endogeneity, thereby contributing a reliable estimation of the effect of immigration legislation on population and employment of the examined demographic groups.

## **VI. Conclusion**

The estimated impact of the implementation of state omnibus immigration legislation on total state population and employment is consistently slightly negative, ranging from -0.45% to -0.61% for population and -0.52% to -0.77% for employment. While somewhat imprecisely measured, these estimates' 95% confidence intervals indicate that they are almost certainly negative in sign, leaving only a relatively small possibility of any positive relationship. With the exception of native, low-skilled blacks (non-Hispanics), there appears to be no substitution in terms of inflows from those demographic groups predicted by theory as being probable substitutes for the undocumented immigrant population. Some of these groups actually experience an adverse impact on population and employment associated with implementation of the immigration legislation. This adverse impact is in addition to the highly statistically significant immigrant outflows of up to nearly a quarter of the corresponding population, as large as 24.41% in terms of population and 20.71% in terms of employment for the examined 'immigrant' demographic groups.

These findings point to the fact that omnibus legislation not only has a negative effect on the undocumented population, but it also unintentionally harms a much broader segment of the population. With substitution inflows on a smaller scale than the corresponding outflows, the empirical evidence clearly does not support the pro-immigration law partner statements of 'immigrants take natives' jobs' and 'if you want to create a job for a U.S. citizen tomorrow, deport an illegal alien today.' This combination of adverse effects on population and employment at three demographic levels - the undocumented population, the affected 'substitute' groups that actually do not sub-

stitute, and the total state populations - deserves careful consideration from not only economists, but from the wider policymaking community as well. As increasing the welfare related to states' population, especially in terms of employment, is the very reason posited by policymakers as justification for immigration law implementation, this trifecta of adverse effects leaves little doubt that state omnibus immigration legislation is not completing its stated mission.

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## Appendix 1

### 1. Georgia SB-529

- Enacted 17 April 2006; effective 1 July 2007
- work authorization, penalty for human trafficking, local immigration law enforcement, determination of legal status when arrested, eliminates public benefits

### 2. Oklahoma HB-1804

- Enacted 8 May 2007; effective 1 November 2007
- work authorization, identification/driver's licenses, local immigration law enforcement, felony to harbor/transport unauthorized immigrants, eliminates public benefits
- Legal Challenge
  - Employment section barred from taking effect, except for E-Verify system

### 3. Missouri HB-1549

- Enacted 7 July 2008; effective 28 August 2008 (1 January 2009)
- work authorization, identification/driver's licenses, local immigration law enforcement, eliminates public benefits

### 4. South Carolina HB-4400

- Enacted 4 June 2008; effective 4 June 2008 (except for work authorization - effective 1 January 2009)
- work authorization, identification/driver's licenses, local immigration law enforcement, eliminates public benefits, education restrictions

### 5. Utah SB-81

- Enacted 13 March 2008; effective 1 July 2009
- work authorization, identification/driver's licenses, local immigration law enforcement, eliminates public benefits

#### 6. Georgia HB-2

- Enacted 11 May 2009; effective 1 January 2010
- work authorization, determination of legal status when arrested, eliminates public benefits

#### 7. Missouri HB-390

- Enacted 7 July 2009; effective 7 July 2009
- work authorization, public benefits, education restrictions

#### 8. Nebraska LB-403

- Enacted 8 April 2009; effective 1 October 2009
- work authorization, eliminates public benefits

#### 9. Arizona SB-1070, HB-2162

- Enacted 23 April 2010; effective 29 July 10
- work authorization, local immigration law enforcement, warrantless arrest, citizens can sue agencies for noncompliance, document-carrying policy
- Legal challenge by U.S. Department of Justice
  - Three provisions preliminarily barred from taking effect
  - Pending appeal by Arizona governor Jan Brewer

#### 10. Utah H116, H466, H469, H497

- Enacted 15 March 2011; effective
- work authorization, immigrant integration, proposed temporary worker program by 2013
- Legal challenge by Utah Coalition of La Raza
  - H497 temporarily restrained

#### 11. Georgia HB-87

- Enacted 13 May 2011; effective 1 July 2011 (except public benefits - effective 1 July 2012)
- work authorization, local immigration law enforcement, eliminates public benefits
- Legal challenge by Georgia Latino Alliance for Human Rights et al.
  - Two provisions enjoined preliminarily

#### 12. Indiana SB-590

- Enacted 10 May 2011; effective 1 July 2011
- work authorization, eliminates public benefits, requests reimbursement of 'immigrant cost' from federal government
- Legal challenge by Ingrid Buquer, et al.
  - Two provisions enjoined preliminarily

#### 13. Alabama HB-56

- Enacted 9 June 2011; effective 1 September 2011
- work authorization, local immigration law enforcement, felony to harbor/rent to unauthorized immigrants, document-carrying policy, questioning of legal status in public schools

- Legal challenge by US Department of Justice
  - Two provisions barred from taking effect 14 October 2011

#### 14. South Carolina S-20

- Enacted 27 June 2011; effective 1 January 2012
- work authorization, identification/driver's licenses, eliminates public benefits, felony to harbor/rent to unauthorized immigrants
- Legal challenge by US Department of Justice
  - Pending challenge filed 31 October 2011

| Issue                       | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 | 2011 |
|-----------------------------|------|------|------|------|------|------|------|
| Education                   | 3    | 3    | 22   | 12   | 27   | 17   | 20   |
| Employment                  | 5    | 14   | 29   | 19   | 21   | 27   | 27   |
| Health                      | 0    | 0    | 14   | 11   | 28   | 17   | 23   |
| Human Trafficking           | 9    | 13   | 18   | 5    | 16   | 8    | 5    |
| ID/Driver's License         | 10   | 6    | 40   | 32   | 46   | 26   | 27   |
| Law Enforcement             | 5    | 8    | 16   | 12   | 16   | 37   | 39   |
| Miscellaneous               | 7    | 11   | 17   | 38   | 46   | 69   | 31   |
| Omnibus                     | 0    | 1    | 1    | 3    | 3    | 2    | 6    |
| Public Benefits             | 5    | 10   | 33   | 9    | 15   | 9    | 15   |
| Voting                      | 1    | 6    | 0    | 1    | 4    | 6    | 4    |
| Total Passed by Legislature | 45   | 72   | 190  | 142  | 222  | 218  | 197  |
| Vetoed by Governor          | 6    | 6    | 12   | 3    | 20   | 10   | 15   |
| Total Enacted               | 39   | 66   | 178  | 139  | 202  | 208  | 182  |

Table 2. Difference-in-differences estimates of the impact of state omnibus immigration laws on population: 'immigrants'

| Demographic   | A. $100 \times \ln(\text{population level})$ |        | B. $100 \times \ln(\text{population proportion})$ |        |
|---|--|--------|---|--------|
|   | (1)  | (2)    | (1)   | (2)    |
| Noncitizen/foreign-born/arrived to US after 1982/Hispanic/low-skilled | -24.41                                       | -19.02 | -23.75  | -18.51 |
| <i>s.e.</i>   | (6.59)                                       | (6.75) | (6.59)  | (6.75) |
| $R^2$   | 0.962  | 0.963  | 0.904   | 0.902  |
| <i>n</i>  | 901  | 3676   | 901   | 3676   |
| Noncitizen/foreign-born/arrived to US after 1982/Hispanic             | -22.92                                       | -19.27 | -22.26  | -18.79 |
| <i>s.e.</i>   | (6.59)                                       | (6.78) | (6.60)  | (6.79) |
| $R^2$   | 0.972  | 0.968  | 0.926   | 0.912  |
| <i>n</i>  | 910  | 3763   | 910   | 3763   |
| Noncitizen/foreign-born/arrived to US after 1982                      | -14.03                                       | -11.39 | -13.42  | -10.94 |
| <i>s.e.</i>   | (4.37)                                       | (4.02) | (4.38)  | (4.08) |
| $R^2$   | 0.978  | 0.982  | 0.920   | 0.925  |
| <i>n</i>  | 924  | 3874   | 924   | 3874   |
| Noncitizen/foreign-born/Hispanic/low-skilled                          | -19.57                                       | -14.42 | -18.92  | -13.92 |
| <i>s.e.</i>   | (5.96)                                       | (5.89) | (5.96)  | (5.89) |
| $R^2$   | 0.965  | 0.966  | 0.910   | 0.908  |
| <i>n</i>  | 909  | 3707   | 909   | 3707   |
| Noncitizen/foreign-born/Hispanic                                      | -18.36                                       | -14.75 | -17.71  | -14.28 |
| <i>s.e.</i>   | (5.92)                                       | (5.93) | (5.92)  | (5.93) |
| $R^2$   | 0.975  | 0.971  | 0.932   | 0.918  |
| <i>n</i>  | 913  | 3771   | 913   | 3771   |
| Noncitizen/foreign-born   | -9.93  | -8.22  | -9.33   | -7.77  |
| <i>s.e.</i>   | (3.92)                                       | (3.54) | (3.92)  | (3.58) |
| $R^2$   | 0.980  | 0.983  | 0.927   | 0.931  |
| <i>n</i>  | 924  | 3874   | 924   | 3874   |
| Control   | Region                                       | All    | Region  | All    |

Panel A: Each estimate is from a separate OLS regression with the log of population of the corresponding demographic group for state-month-year combinations as the dependent variable. Population includes all CPS data (using given sampling weights) for individuals ages 20 to 60 during each implementation period. All regressions include state and month-year effects, controlling for any existing systematic differences across states as well as any differences over time that may affect all states' outcomes. Column (1) reports estimates using the regional control groups, while column (2) employs all US states as a control for states implementing immigration legislation. Huber-White robust standard errors are reported in parentheses, allowing for unrestricted error correlations across observations within states.

Panel B: Each estimate is from a separate OLS regression with the proportion of the log of population of the corresponding demographic group in the total state population for state-month-year combinations as the dependent variable. Population includes all CPS data (using given sampling weights) for individuals ages 20 to 60 during each implementation period. All regressions include state and month-year effects, controlling for any existing systematic differences across states as well as any differences over time that may affect all states' outcomes. Column (1) reports estimates using the regional control groups, while column (2) employs all US states as a control for states implementing immigration legislation. Huber-White robust standard errors are reported in parentheses, allowing for unrestricted error correlations across observations within states.

The treatment sample in each panel includes all population observations six months before and after implementation for those states adopting omnibus immigration legislation. Control groups include observations for all states not adopting omnibus immigration legislation during the one-year implementation period. States having previously adopted omnibus legislation are additionally excluded from the control groups. The estimate reported is the interaction between the indicator for 1 to 6 months post-implementation and the treatment indicator.

Table 3. Difference-in-differences estimates of the impact of state omnibus immigration laws on population: 'natives'

| Demographic   | A. $100 \times \ln(\text{Population Level})$ |        | B. $100 \times \ln(\text{Population Proportion})$ |        |
|---|--|--------|---|--------|
|   | (1)  | (2)    | (1)   | (2)    |
| native/Hispanic/low-skilled                               | -11.57                                       | -7.16  | -10.97  | -6.71  |
| <i>s.e.</i>   | (7.28)                                       | (6.92) | (7.32)  | (6.97) |
| $R^2$   | 0.954  | 0.961  | 0.917   | 0.908  |
| <i>n</i>  | 886  | 3791   | 886   | 3791   |
| native/white,<br>non-Hispanic/low-skilled                 | -0.93  | -0.36  | -0.32   | 0.09   |
| <i>s.e.</i>   | (1.18)                                       | (1.21) | (1.14)  | (1.24) |
| $R^2$   | 0.996  | 0.997  | 0.978   | 0.982  |
| <i>n</i>  | 924  | 3876   | 924   | 3876   |
| native/black,<br>non-Hispanic/low-skilled                 | 6.51   | 5.80   | 7.11  | 6.23   |
| <i>s.e.</i>   | (5.40)                                       | (5.24) | (5.30)  | (5.11) |
| $R^2$   | 0.982  | 0.980  | 0.964   | 0.952  |
| <i>n</i>  | 894  | 3715   | 894   | 3715   |
| naturalized citizen/foreign-<br>born/Hispanic/low-skilled | -4.70  | -7.68  | -3.99   | -7.22  |
| <i>s.e.</i>   | (8.36)                                       | (8.08) | (8.27)  | (7.96) |
| $R^2$   | 0.925  | 0.931  | 0.831   | 0.806  |
| <i>n</i>  | 816  | 3483   | 816   | 3483   |
| total   | -0.61  | -0.45  | —   | —      |
| <i>s.e.</i>   | (0.38)                                       | (0.46) | —   | —      |
| $R^2$   | 0.999  | 0.999  | —   | —      |
| <i>n</i>  | 924  | 3876   | —   | —      |
| Control   | Region                                       | All    | Region  | All    |

Panel A: Each estimate is from a separate OLS regression with the log of population of the corresponding demographic group for state-month-year combinations as the dependent variable. Population includes all CPS data (using given sampling weights) for individuals ages 20 to 60 during each implementation period. All regressions include state and month-year effects, controlling for any existing systematic differences across states as well as any differences over time that may affect all states' outcomes. Column (1) reports estimates using the regional control groups, while column (2) employs all US states as a control for states implementing immigration legislation. Huber-White robust standard errors are reported in parentheses, allowing for unrestricted error correlations across observations within states.

Panel B: Each estimate is from a separate OLS regression with the proportion of the log of population of the corresponding demographic group in the total state population for state-month-year combinations as the dependent variable. Population includes all CPS data (using given sampling weights) for individuals ages 20 to 60 during each implementation period. All regressions include state and month-year effects, controlling for any existing systematic differences across states as well as any differences over time that may affect all states' outcomes. Column (1) reports estimates using the regional control groups, while column (2) employs all U.S. states as a control for states implementing immigration legislation. Huber-White robust standard errors are reported in parentheses, allowing for unrestricted error correlations across observations within states.

The treatment sample in each panel includes all population observations six months before and after implementation for those states adopting omnibus immigration legislation. Control groups include observations for all states not adopting omnibus immigration legislation during the one-year implementation period. States having previously adopted omnibus legislation are additionally excluded from the control groups. The estimate reported is the interaction between the indicator for 1-6 months post-implementation and the treatment indicator.



Table 4. Difference-in-differences estimates of the impact of state omnibus immigration laws on employment: 'immigrants'

| Demographic   | A. $100 \times \ln(\text{Employment Level})$ |        | B. $100 \times \ln(\text{Employment Proportion})$ |        |
|---|--|--------|---|--------|
|   | (1)  | (2)    | (1)   | (2)    |
| Noncitizen/foreign-born/arrived to US after 1982/Hispanic/low-skilled | -20.73                                       | -16.32 | -21.17  | -17.03 |
| <i>s.e.</i>   | (7.34)                                       | (7.08) | (7.22)  | (7.08) |
| $R^2$   | 0.953  | 0.956  | 0.885   | 0.882  |
| <i>n</i>  | 899  | 3627   | 899   | 3627   |
| Noncitizen/foreign-born/arrived to US after 1982/Hispanic             | -19.52                                       | -16.42 | -19.96  | -17.16 |
| <i>s.e.</i>   | (7.55)                                       | (7.25) | (7.44)  | (7.20) |
| $R^2$   | 0.964  | 0.961  | 0.903   | 0.890  |
| <i>n</i>  | 904  | 3715   | 904   | 3715   |
| Noncitizen/foreign-born/arrived to US after 1982                      | -12.89                                       | -9.12  | -13.41  | -9.89  |
| <i>s.e.</i>   | (4.98)                                       | (4.50) | (4.83)  | (4.45) |
| $R^2$   | 0.977  | 0.979  | 0.913   | 0.915  |
| <i>n</i>  | 923  | 3870   | 923   | 3870   |
| Noncitizen/foreign-born/Hispanic/low-skilled                          | -16.59                                       | -13.92 | -17.03  | -14.63 |
| <i>s.e.</i>   | (6.61)                                       | (6.18) | (6.46)  | (6.17) |
| $R^2$   | 0.958  | 0.960  | 0.896   | 0.892  |
| <i>n</i>  | 907  | 3670   | 907   | 3670   |
| Noncitizen/foreign-born/Hispanic                                      | -15.23                                       | -13.29 | -15.67  | -14.02 |
| <i>s.e.</i>   | (6.78)                                       | (6.37) | (6.65)  | (6.31) |
| $R^2$   | 0.968  | 0.965  | 0.913   | 0.901  |
| <i>n</i>  | 907  | 3731   | 907   | 3731   |
| Noncitizen/foreign-born   | -10.62                                       | -7.90  | -11.14  | -8.67  |
| <i>s.e.</i>   | (4.48)                                       | (4.07) | (4.33)  | (4.00) |
| $R^2$   | 0.979  | 0.982  | 0.920   | 0.925  |
| <i>n</i>  | 924  | 3871   | 924   | 3871   |
| Control   | Region                                       | All    | Region  | All    |

Panel A: Each estimate is from a separate OLS regression with the log of employment of the corresponding demographic group for state-month-year combinations as the dependent variable. Employment includes all CPS data (using given sampling weights) for individuals ages 20 to 60 during each implementation period. All regressions include state and month-year effects, controlling for any existing systematic differences across states as well as any differences over time that may affect all states' outcomes. Column (1) reports estimates using the regional control groups, while column (2) employs all US states as a control for states implementing immigration legislation. Huber-White robust standard errors are reported in parentheses, allowing for unrestricted error correlations across observations within states.

Panel B: Each estimate is from a separate OLS regression with the proportion of the log of employment of the corresponding demographic group among the total state employment for state-month-year combinations as the dependent variable. Employment includes all CPS data (using given sampling weights) for individuals ages 20 to 60 during each implementation period. All regressions include state and month-year effects, controlling for any existing systematic differences across states as well as any differences over time that may affect all states' outcomes. Column (1) reports estimates using the regional control groups, while column (2) employs all US states as a control for states implementing immigration legislation. Huber-White robust standard errors are reported in parentheses, allowing for unrestricted error correlations across observations within states.

The treatment sample in each panel includes all employment observations six months before and after implementation for those states adopting omnibus immigration legislation. Control groups include observations for all states not adopting omnibus immigration legislation during the one-year implementation period. States having previously adopted omnibus legislation are additionally excluded from the control groups. The estimate reported is the interaction between the indicator for 1 to 6 months post-implementation and the treatment indicator.

Table 5. Difference-in-differences estimates of the impact of state omnibus immigration laws on employment: 'natives'

| Demographic   | A. $100 \times \ln(\text{Employment Level})$ |         | B. $100 \times \ln(\text{Employment Proportion})$ |         |
|---|--|---------|---|---------|
|   | (1)  | (2)     | (1)   | (2)     |
| native/Hispanic/low-skilled                               | -10.89                                       | -4.05   | -11.46  | -4.82   |
| <i>s.e.</i>   | (10.28)                                      | (10.45) | (10.30)   | (10.54) |
| $R^2$   | 0.944  | 0.953   | 0.898   | 0.887   |
| <i>n</i>  | 872  | 3735    | 872   | 3735    |
| native/white,<br>non-Hispanic/low-skilled                 | 0.73   | 2.10    | 0.21  | 1.33    |
| <i>s.e.</i>   | (1.44)                                       | (1.36)  | (1.35)  | (1.30)  |
| $R^2$   | 0.993  | 0.995   | 0.967   | 0.974   |
| <i>n</i>  | 924  | 3876    | 924   | 3876    |
| native/black,<br>non-Hispanic/low-skilled                 | 7.79   | 10.68   | 7.24  | 9.93    |
| <i>s.e.</i>   | (6.54)                                       | (5.80)  | (6.56)  | (5.74)  |
| $R^2$   | 0.973  | 0.970   | 0.948   | 0.933   |
| <i>n</i>  | 872  | 3612    | 872   | 3612    |
| naturalized citizen/foreign-<br>born/Hispanic/low-skilled | -14.60                                       | -16.48  | -15.14  | -17.47  |
| <i>s.e.</i>   | (9.06)                                       | (7.84)  | (8.93)  | (7.79)  |
| $R^2$   | 0.921  | 0.922   | 0.828   | 0.788   |
| <i>n</i>  | 826  | 3397    | 826   | 3397    |
| total   | -0.52  | -0.77   | —   | —       |
| <i>s.e.</i>   | (0.58)                                       | (0.58)  | —   | —       |
| $R^2$   | 0.999  | 0.999   | —   | —       |
| <i>n</i>  | 924  | 3876    | —   | —       |
| Control   | Region                                       | All     | Region  | All     |

Panel A: Each estimate is from a separate OLS regression with the log of employment of the corresponding demographic group for state-month-year combinations as the dependent variable. Employment includes all CPS data (using given sampling weights) for individuals ages 20 to 60 during each implementation period. All regressions include state and month-year effects, controlling for any existing systematic differences across states as well as any differences over time that may affect all states' outcomes. Column (1) reports estimates using the regional control groups, while column (2) employs all U.S. states as a control for states implementing immigration legislation. Huber-White robust standard errors are reported in parentheses, allowing for unrestricted error correlations across observations within states.

Panel B: Each estimate is from a separate OLS regression with the proportion of the log of employment of the corresponding demographic group among the total state employment for state-month-year combinations as the dependent variable. Employment includes all CPS data (using given sampling weights) for individuals ages 20 to 60 during each implementation period. All regressions include state and month-year effects, controlling for any existing systematic differences across states as well as any differences over time that may affect all states' outcomes. Column (1) reports estimates using the regional control groups, while column (2) employs all U.S. states as a control for states implementing immigration legislation. Huber-White robust standard errors are reported in parentheses, allowing for unrestricted error correlations across observations within states.

The treatment sample in each panel includes all employment observations six months before and after implementation for those states adopting omnibus immigration legislation. Control groups include observations for all states not adopting omnibus immigration legislation during the one-year implementation period. States having previously adopted omnibus legislation are additionally excluded from the control groups. The estimate reported is the interaction between the indicator for 1 to 6 months post-implementation and the treatment indicator.

Table 6. Difference-in-differences estimates of the impact of state omnibus immigration laws on population and employment (no Oklahoma): 'immigrants'

| Demographic   | A. Population |        | B. Employment |        |
|---|---------------|--------|---------------|--------|
|   | (1)           | (2)    | (1)           | (2)    |
| Noncitizen/foreign-born/arrived to US after 1982/Hispanic/low-skilled | -31.05        | -26.25 | -19.37        | -18.01 |
| <i>s.e.</i>   | (6.47)        | (6.84) | (6.72)        | (7.85) |
| $R^2$   | 0.966         | 0.963  | 0.961         | 0.957  |
| <i>n</i>  | 781           | 3117   | 779           | 3073   |
| Noncitizen/foreign-born/arrived to US after 1982/Hispanic             | -28.88        | -25.41 | -18.55        | -18.58 |
| <i>s.e.</i>   | (6.71)        | (7.12) | (7.06)        | (8.06) |
| $R^2$   | 0.975         | 0.968  | 0.968         | 0.961  |
| <i>n</i>  | 790           | 3189   | 7784          | 3149   |
| Noncitizen/foreign-born/arrived to US after 1982                      | -16.49        | -14.53 | -12.60        | -11.17 |
| <i>s.e.</i>   | (4.59)        | (4.13) | (4.79)        | (4.66) |
| $R^2$   | 0.979         | 0.981  | 0.978         | 0.979  |
| <i>n</i>  | 804           | 3286   | 803           | 3282   |
| Noncitizen/foreign-born/Hispanic/low-skilled                          | -27.38        | -23.01 | -15.54        | -14.97 |
| <i>s.e.</i>   | (5.80)        | (5.85) | (6.17)        | (6.85) |
| $R^2$   | 0.968         | 0.966  | 0.965         | 0.961  |
| <i>n</i>  | 789           | 3147   | 787           | 3113   |
| Noncitizen/foreign-born/Hispanic                                      | -25.27        | -22.19 | -14.45        | -14.82 |
| <i>s.e.</i>   | (5.95)        | (6.14) | (6.41)        | (7.08) |
| $R^2$   | 0.978         | 0.971  | 0.972         | 0.965  |
| <i>n</i>  | 793           | 3196   | 787           | 3162   |
| Noncitizen/foreign-born   | -13.24        | -12.30 | -10.42        | -9.97  |
| <i>s.e.</i>   | (4.09)        | (3.58) | (4.33)        | (4.19) |
| $R^2$   | 0.981         | 0.983  | 0.979         | 0.982  |
| <i>n</i>  | 804           | 3286   | 804           | 3283   |
| Control   | Region        | All    | Region        | All    |

Estimates correspond to those found in Tables 2 and 4, excluding Oklahoma's effective date of November 2007 from the treatment group. In addition, I report only population and employment levels, not proportions. Panel A displays estimates associated with population, while Panel B displays estimates associated with employment.

Table 7. Difference-in-differences estimates of the impact of state omnibus immigration laws on population and employment (no Oklahoma): 'natives'

| Demographic   | A. Population |        | B. Employment |        |
|---|---------------|--------|---------------|--------|
|   | (1)           | (2)    | (1)           | (2)    |
| native/Hispanic/low-skilled                               | -14.53        | -11.49 | -11.27        | -10.71 |
| <i>s.e.</i>   | (7.88)        | (7.47) | (10.17)       | (9.89) |
| $R^2$   | .952          | 0.962  | 0.940         | 0.954  |
| <i>n</i>  | 774           | 3223   | 761           | 3173   |
| native/white,<br>non-Hispanic/low-skilled                 | -0.67         | -0.24  | 0.85          | 1.28   |
| <i>s.e.</i>   | (1.23)        | (1.39) | (1.41)        | (1.45) |
| $R^2$   | 0.996         | 0.997  | 0.994         | 0.995  |
| <i>n</i>  | 804           | 3288   | 804           | 3288   |
| native/black,<br>non-Hispanic/low-skilled                 | 11.44         | 8.58   | 8.18          | 12.66  |
| <i>s.e.</i>   | (6.08)        | (5.96) | (6.64)        | (6.64) |
| $R^2$   | 0.982         | 0.980  | 0.974         | 0.970  |
| <i>n</i>  | 774           | 3159   | 753           | 3069   |
| naturalized citizen/foreign-<br>born/Hispanic/low-skilled | -6.25         | -10.27 | -11.65        | -17.00 |
| <i>s.e.</i>   | (8.79)        | (8.05) | (8.55)        | (7.94) |
| $R^2$   | 0.929         | 0.934  | 0.916         | 0.923  |
| <i>n</i>  | 707           | 2961   | 694           | 2882   |
| total   | -0.45         | -0.36  | -0.57         | -0.47  |
| <i>s.e.</i>   | (0.42)        | (0.49) | (0.57)        | (0.59) |
| $R^2$   | 0.999         | 0.999  | 0.999         | 0.999  |
| <i>n</i>  | 804           | 3288   | 804           | 3288   |
| Control   | Region        | All    | Region        | All    |

Estimates correspond to those found in Tables 3 and 5, excluding Oklahoma's effective date of November 2007 from the treatment group. In addition, I report only population and employment levels, not proportions. Panel A displays estimates associated with population, while Panel B displays estimates associated with employment.

Table 8. Difference-in-differences estimates of the impact of state omnibus immigration laws on population and employment: 'immigrants'

| Demographic   | A. Six-month implementation period |         |            |         | B. Two-year implementation period |        |            |        |
|---|------------------------------------|---------|------------|---------|-----------------------------------|--------|------------|--------|
|   | Population                         |         | Employment |         | Population                        |        | Employment |        |
|   | (1)                                | (2)     | (3)        | (4)     | (1)                               | (2)    | (3)        | (4)    |
| Noncitizen/foreign-born/arrived to US after 1982/Hispanic/low-skilled | -24.75                             | -19.29  | -10.09     | -14.60  | -7.11                             | -6.38  | -4.63      | -2.38  |
| <i>s.e.</i>   | (9.54)                             | (10.57) | (8.81)     | (10.38) | (5.08)                            | (5.23) | (5.95)     | (6.19) |
| $R^2$   | 0.968                              | 0.966   | 0.964      | 0.958   | 0.957                             | 0.962  | 0.945      | 0.955  |
| <i>n</i>  | 454                                | 1846    | 457        | 1817    | 1751                              | 7216   | 1737       | 7117   |
| Noncitizen/foreign-born/arrived to US after 1982/Hispanic             | -21.97                             | -16.54  | -12.10     | -16.89  | -8.26                             | -8.96  | -6.51      | -5.75  |
| <i>s.e.</i>   | (9.38)                             | (10.70) | (8.93)     | (10.12) | (5.11)                            | (5.08) | (6.05)     | (6.13) |
| $R^2$   | 0.976                              | 0.969   | 0.967      | 0.962   | 0.965                             | 0.968  | 0.952      | 0.960  |
| <i>n</i>  | 459                                | 1883    | 459        | 1854    | 1765                              | 7392   | 1747       | 7285   |
| Noncitizen/foreign-born/arrived to US after 1982                      | -10.90                             | -8.71   | -9.55      | -11.24  | -5.07                             | -4.17  | -2.50      | -2.41  |
| <i>s.e.</i>   | (6.25)                             | (5.86)  | (6.46)     | (5.89)  | (3.15)                            | (3.04) | (3.59)     | (3.55) |
| $R^2$   | 0.980                              | 0.982   | 0.981      | 0.981   | 0.976                             | 0.981  | 0.975      | 0.979  |
| <i>n</i>  | 462                                | 1936    | 462        | 1934    | 1801                              | 7629   | 1795       | 7617   |
| Noncitizen/foreign-born/Hispanic/low-skilled                          | -22.28                             | -17.71  | -10.85     | -14.74  | -3.60                             | -2.84  | -1.10      | 0.86   |
| <i>s.e.</i>   | (8.54)                             | (9.05)  | (8.40)     | (9.21)  | (4.56)                            | (4.65) | (5.24)     | (5.41) |
| $R^2$   | 0.970                              | 0.968   | 0.968      | 0.961   | 0.961                             | 0.965  | 0.952      | 0.958  |
| <i>n</i>  | 455                                | 1860    | 460        | 1839    | 1760                              | 7263   | 1747       | 7178   |
| Noncitizen/foreign-born/Hispanic                                      | -19.32                             | -14.56  | -11.59     | -15.53  | -5.27                             | -5.77  | -3.43      | -2.62  |
| <i>s.e.</i>   | (8.24)                             | (9.23)  | (8.20)     | (9.15)  | (4.52)                            | (4.47) | (5.35)     | (5.40) |
| $R^2$   | 0.978                              | 0.972   | 0.973      | 0.967   | 0.968                             | 0.971  | 0.959      | 0.964  |
| <i>n</i>  | 459                                | 1886    | 460        | 1863    | 1768                              | 7405   | 1751       | 7314   |
| Noncitizen/foreign-born   | -8.60                              | -7.51   | -8.10      | -10.36  | -1.98                             | -1.34  | 0.42       | 0.31   |
| <i>s.e.</i>   | (5.57)                             | (5.19)  | (5.76)     | (5.49)  | (2.84)                            | (2.71) | (3.27)     | (3.21) |
| $R^2$   | 0.981                              | 0.983   | 0.982      | 0.983   | 0.978                             | 0.983  | 0.977      | 0.982  |
| <i>n</i>  | 462                                | 1936    | 462        | 1934    | 1801                              | 7629   | 1798       | 7623   |
| Control   | Region                             | All     | Region     | All     | Region                            | All    | Region     | All    |

Estimates correspond to those found in Tables 2 and 4, however resulting from either six-month or two-year total implementation periods. Panel A displays estimates associated the six-month window, while Panel B displays estimates associated with the two-year window. I report only population and employment levels, not proportions.

Table 9. Difference-in-differences estimates of the impact of state omnibus immigration laws on population and employment: 'natives'

| Demographic   | A. Six-month implementation period |         |            |         | B. Two-year implementation period |        |            |        |
|---|------------------------------------|---------|------------|---------|-----------------------------------|--------|------------|--------|
|   | Population                         |         | Employment |         | Population                        |        | Employment |        |
|   | (1)                                | (2)     | (3)        | (4)     | (1)                               | (2)    | (3)        | (4)    |
| native/Hispanic/low-skilled                           | -24.60                             | -21.30  | -2.45      | -12.28  | -4.75                             | -2.56  | -7.98      | -5.70  |
| <i>s.e.</i>   | (10.57)                            | (9.87)  | (13.58)    | (13.32) | (6.20)                            | (5.91) | (7.12)     | (6.73) |
| $R^2$   | 0.958                              | 0.963   | 0.959      | 0.956   | 0.947                             | 0.960  | 0.937      | 0.950  |
| <i>n</i>  | 446                                | 1900    | 449        | 1875    | 1729                              | 7463   | 1701       | 7354   |
| native/white, non-Hispanic/low-skilled                | -3.23                              | -2.89   | -2.72      | -0.30   | -0.59                             | -0.43  | -1.55      | -1.40  |
| <i>s.e.</i>   | (1.37)                             | (1.61)  | (2.27)     | (2.02)  | (0.90)                            | (0.89) | (1.07)     | (1.04) |
| $R^2$   | 0.997                              | 0.997   | 0.996      | 0.994   | 0.996                             | 0.997  | 0.993      | 0.994  |
| <i>n</i>  | 462                                | 1938    | 462        | 1938    | 1801                              | 7632   | 1799       | 7632   |
| native/black, non-Hispanic/low-skilled                | 5.10                               | 4.16    | 13.17      | 15.59   | 0.47                              | -1.75  | 0.33       | -0.05  |
| <i>s.e.</i>   | (7.81)                             | (7.27)  | (10.62)    | (8.54)  | (4.20)                            | (4.08) | (4.73)     | (4.68) |
| $R^2$   | 0.984                              | 0.981   | 0.974      | 0.971   | 0.980                             | 0.979  | 0.972      | 0.971  |
| <i>n</i>  | 448                                | 1858    | 433        | 1818    | 1743                              | 7329   | 1696       | 7104   |
| naturalized citizen/foreign-born/Hispanic/low-skilled | 6.51                               | 6.64    | -1.05      | -3.15   | 8.55                              | 4.03   | -1.81      | 5.97   |
| <i>s.e.</i>   | (11.49)                            | (11.63) | (12.22)    | (12.40) | (6.20)                            | (5.83) | (3.05)     | (6.38) |
| $R^2$   | 0.933                              | 0.931   | 0.939      | 0.920   | 0.916                             | 0.928  | 0.908      | 0.921  |
| <i>n</i>  | 411                                | 1735    | 417        | 1691    | 1589                              | 6818   | 1565       | 6618   |
| total   | -0.65                              | -0.54   | -0.45      | -1.04   | 0.06                              | 0.09   | 0.26       | 0.12   |
| <i>s.e.</i>   | (0.43)                             | (0.58)  | (0.62)     | (0.50)  | (0.28)                            | (0.34) | (0.43)     | (0.44) |
| $R^2$   | 0.999                              | 0.999   | 0.999      | 0.999   | 0.999                             | 0.999  | 0.999      | 0.999  |
| <i>n</i>  | 462                                | 1938    | 462        | 1938    | 1801                              | 7632   | 1799       | 7632   |
| Control   | Region                             | All     | Region     | All     | Region                            | All    | Region     | All    |

Estimates correspond to those found in Tables 3 and 5, however resulting from either six-month or two-year total implementation periods. Panel A displays estimates associated with the six-month window, while Panel B displays estimates associated with the two-year window. I report only population and employment levels, not proportions.

Table 10. Rankings one year before implementation

| State               | Population |       | Employment |       |
|---------------------|------------|-------|------------|-------|
|                     | Region     | All   | Region     | All   |
| Georgia             | 3/12       | 9/49  | 3/12       | 8/49  |
| Oklahoma            | 7/10       | 27/49 | 8/10       | 28/49 |
| Missouri            | 6/11       | 27/44 | 6/11       | 27/44 |
| South Carolina      | 9/11       | 26/44 | 8/11       | 25/44 |
| Utah                | 3/10       | 4/44  | 3/10       | 4/44  |
| Nebraska            | 4/11       | 21/44 | 7/11       | 30/44 |
| Arizona             | 3/10       | 4/44  | 3/10       | 5/44  |
| Group of Comparison | Region     | All   | Region     | All   |

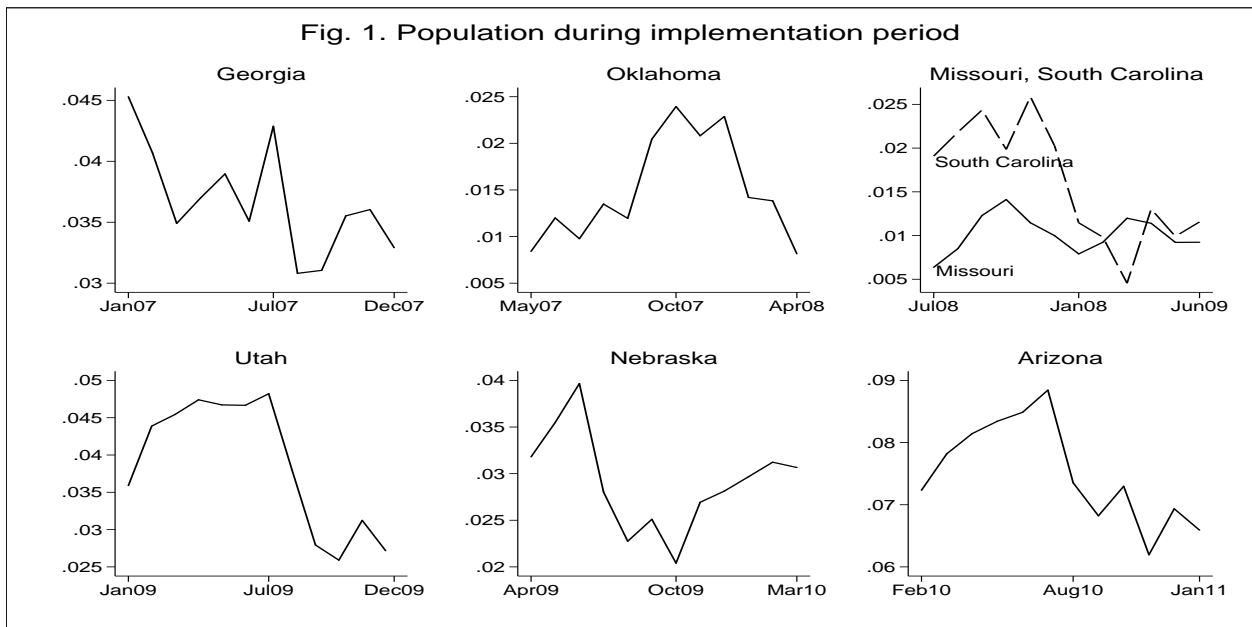
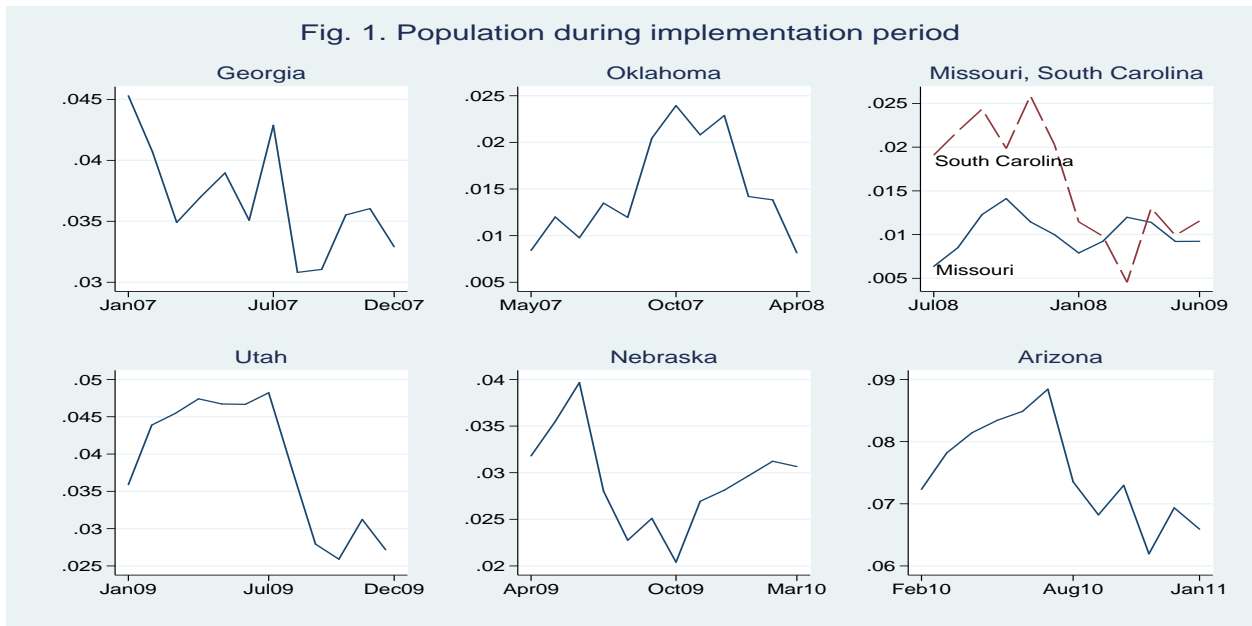
Population (employment) is measured as the proportion of low-skilled, Hispanic immigrants arriving to the US after 1982 among the total population (employed) in each state (ages 20 to 60), with a ranking of one designating the highest proportion.

Table 11. Estimates of the impact of the 'narrowest demographic' on state immigration legislation implementation

|                 | A. Percentage of 'narrowest demographic' |        |            |        | B. Change in percentage of 'narrowest demographic' |        |            |        |
|-----------------|--|--------|------------|--------|--|--------|------------|--------|
|                 | Population                               |        | Employment |        | Population   |        | Employment |        |
|                 | (1)                                      | (2)    | (3)        | (4)    | (1)  | (2)    | (3)        | (4)    |
| Coefficient     | 0.51                                     | 0.15   | 0.34       | 0.09   | -0.24  | -0.05  | -0.22      | -0.04  |
| <i>s.e.</i>     | (0.40)                                   | (0.10) | (0.44)     | (0.11) | (0.31)   | (0.08) | (0.34)     | (0.08) |
| 95% lower bound | -0.27                                    | -0.04  | -0.53      | -0.12  | -0.85  | -0.19  | -0.89      | -0.20  |
| 95% upper bound | 1.28                                     | 0.33   | 1.21       | 0.29   | 0.37   | 0.10   | 0.45       | 0.12   |
| Control         | Region                                   | All    | Region     | All    | Region   | All    | Region     | All    |

Each estimate is from an OLS regression with the dummy variable for omnibus immigration legislation implementation as the dependent variable. Reported estimates have the following interpretation for Panel A: a one unit increase in the percentage of the narrowest demographic in total state population (employment) is associated with a 100%\*coefficient increase in the probability of state immigration legislation implementation. Reported estimates have the following interpretation for Panel B: a one unit increase in the change in the percentage of the narrowest demographic in total state population (employment) is associated with a 100%\*coefficient increase in the probability of state immigration legislation implementation.

Note: Black and white figures are provided after each corresponding color figure.



Population is measured as the proportion of low-skilled, Hispanic immigrants arriving to the US after 1982 in the total state population (ages 20 to 60). Proportions displayed are measured during the one-year implementation period, consisting of six months before and after the implementation date of state omnibus immigration legislation. Implementation month-year combinations are indicated in the middle of the x-axis of each panel.



Fig. 2. Employment during implementation period

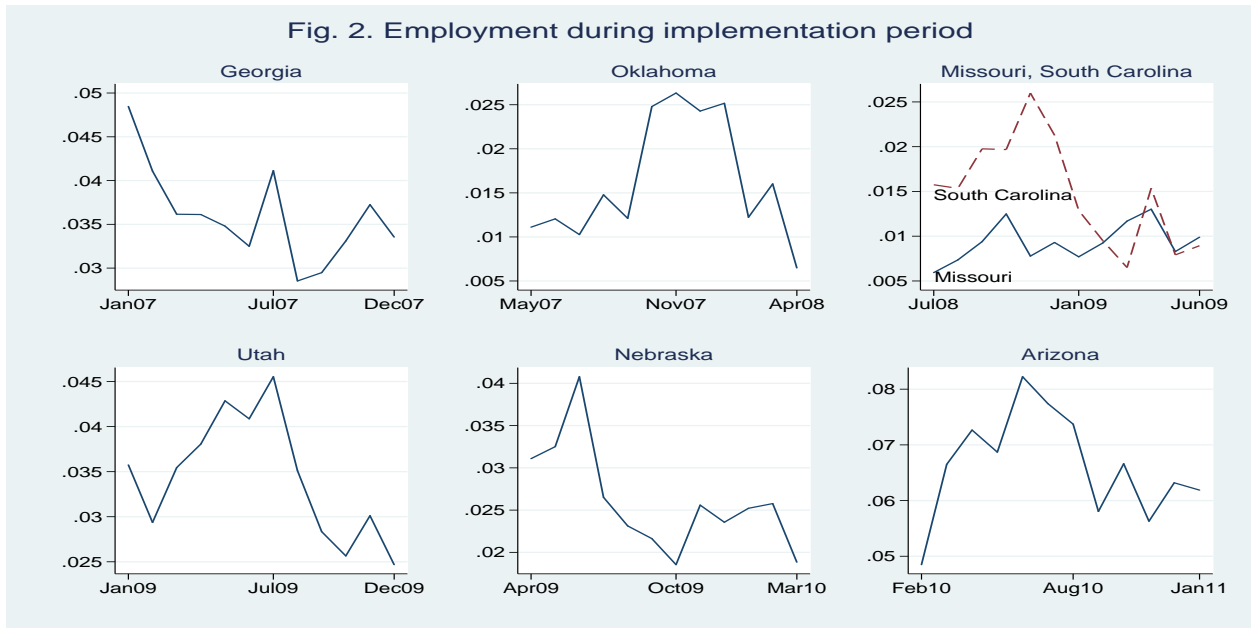
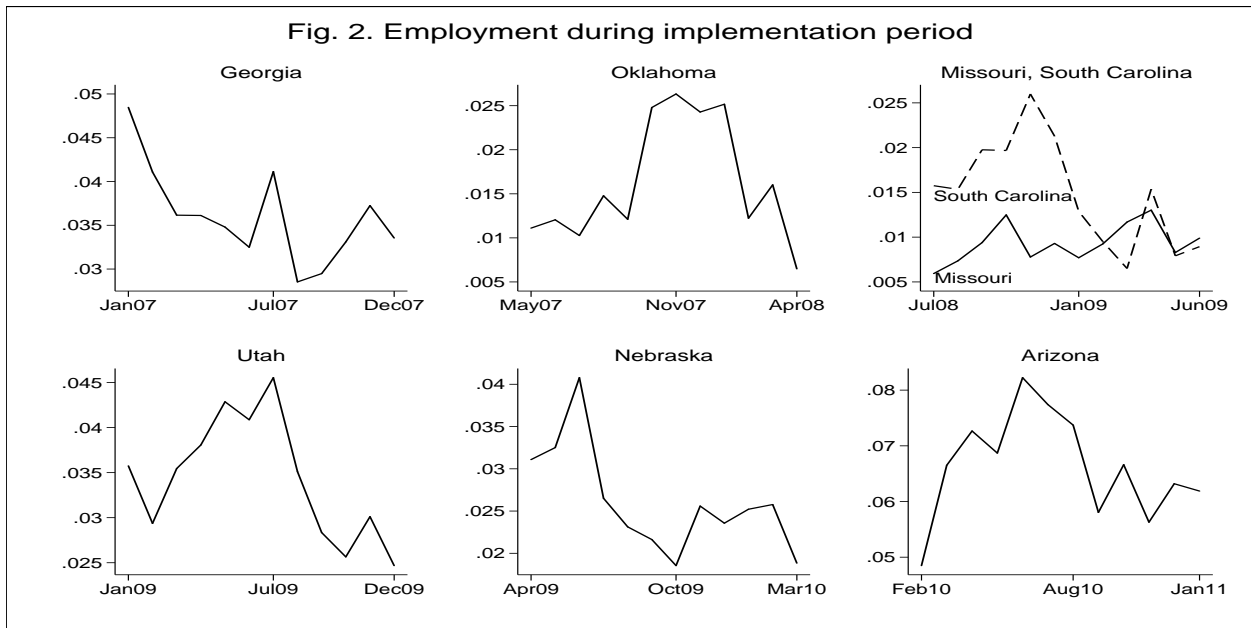


Fig. 2. Employment during implementation period



Employment is measured as the proportion of low-skilled, Hispanic immigrants arriving to the US after 1982 among the total employed in each state (ages 20 to 60). Proportions displayed are measured during the one-year implementation period, consisting of six months before and after the implementation date of state omnibus immigration legislation. Implementation month-year combinations are indicated in the middle of the x-axis of each panel.

Fig. 3. Population

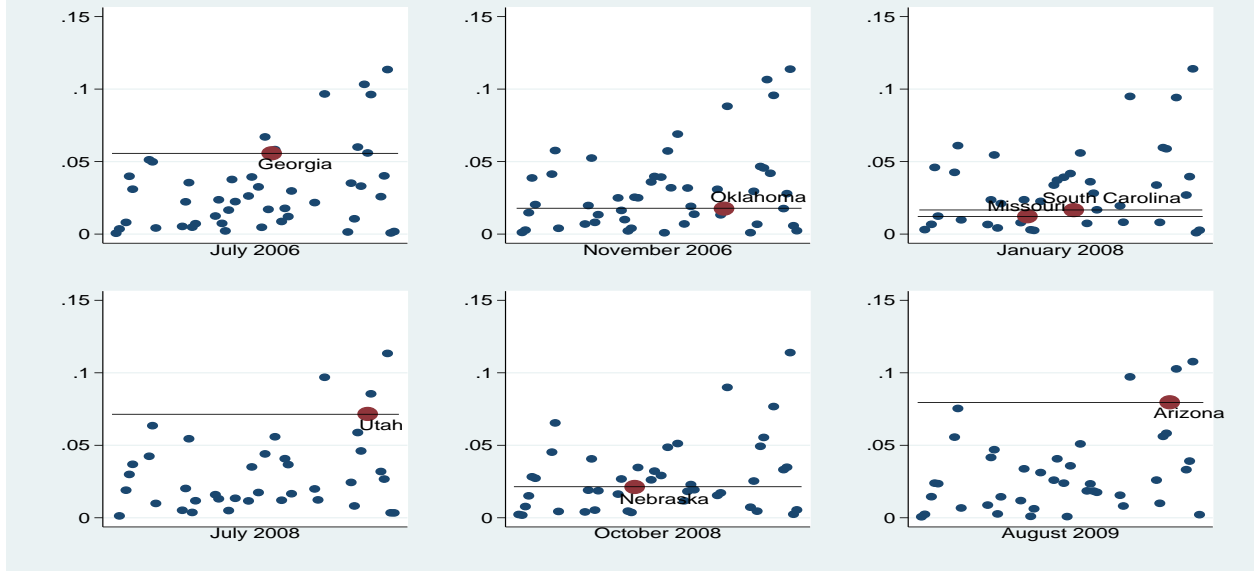
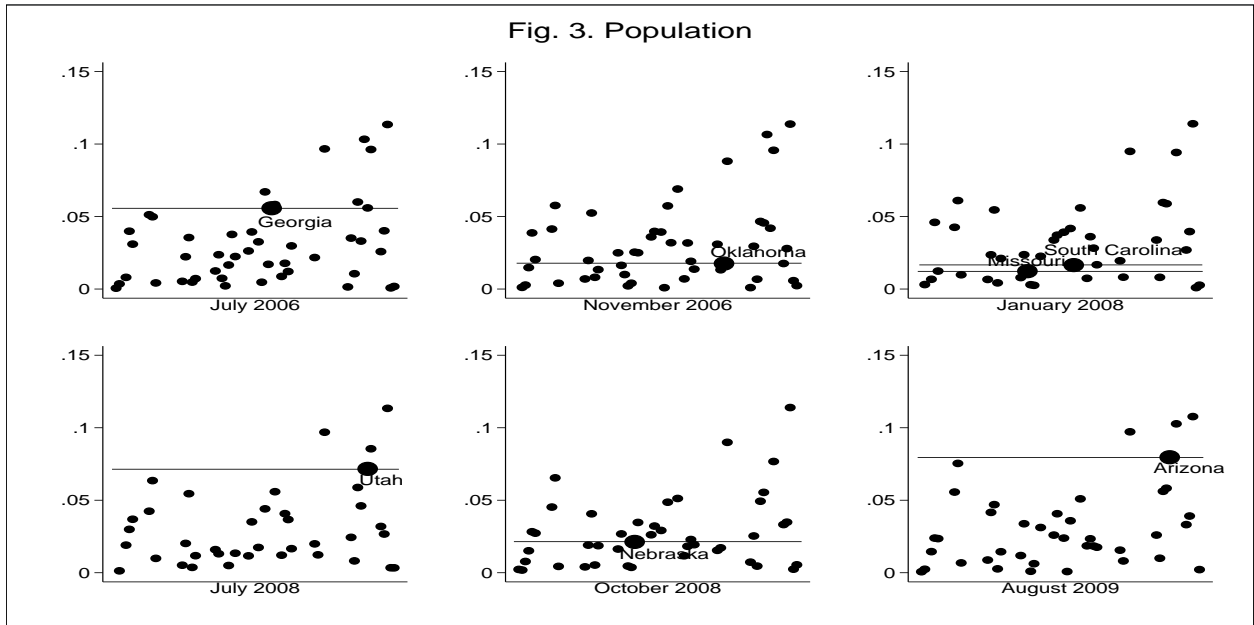
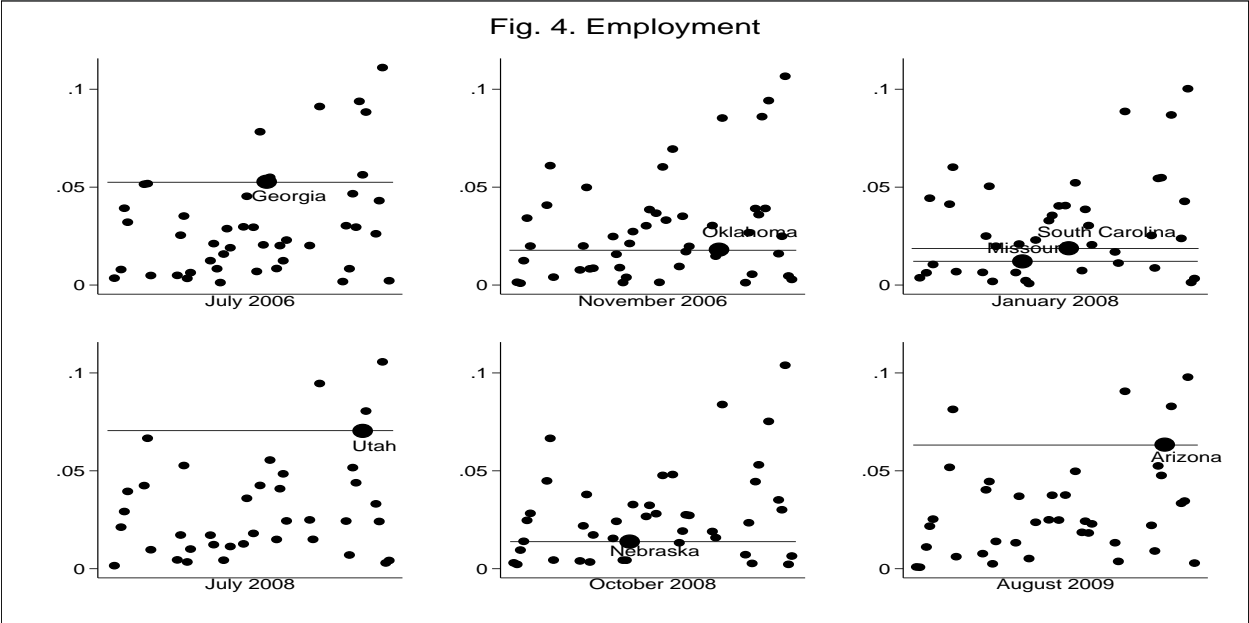


Fig. 3. Population



Population is measured as the proportion of low-skilled, Hispanic immigrants arriving to the US after 1982 in the total state population (ages 20 to 60). Proportions displayed are measured one year before implementation of omnibus immigration law for each state implementing legislation, as well as the corresponding nonimplementing states for each given implementation period.



Employment is measured as the proportion of low-skilled, Hispanic immigrants arriving to the US after 1982 among the total employed in each state (ages 20 to 60). Proportions displayed are measured one year before implementation of omnibus immigration law for each state implementing legislation, as well as the corresponding nonimplementing states for each given implementation period.