Assessing the Effect of E-Verify Mandates on Employment

José D. Pacas

University of Minnesota

Abstract

The electronic employment verification system, known as E-Verify, is widely considered an important component of immigration reform. Lacking federal action, various states have passed laws requiring the use of E-Verify to certain employers. Using Basic Monthly Current Population Survey (CPS) data from January 1994 through December 2014, I use a state-level difference-in-difference approach to estimate the effect of E-Verify mandates on employment. I find that E-Verify mandates that require all employers in a state to use E-Verify have a negative effect on employment of likely unauthorized workers. In order to address the identification of unauthorized immigrants in the CPS, I adapt a logical imputation strategy used for estimating the unauthorized immigrant population (Warren, 2014) using the Annual Social and Economic Supplement (ASEC) of the CPS from 2002 through 2014. Results show that typical proxy groups for likely unauthorized immigrants (i.e. non-naturalized immigrants from Latin America with low educational attainment) result in inconsistent effects of E-Verify mandates as compared to those using the imputation strategy.

1 Introduction

Immigration reform is a timely and important policy issue in the United States. Currently, there are an estimated 11 million unauthorized immigrants living in the country which amounts to about 26 percent of the entire immigrant population (Warren, 2014) (Passel, Cohn and Gonzalez-Barrera, 2013). The electronic employment verification system, known as E-Verify, is widely considered an important component of immigration reform. As of 2013, E-Verify is used nationwide by more than 480,000 employers of all sizes and is joined by about 1,400 new participating companies every week. Indeed, since E-Verify was launched at a nationwide scale, the number of participating growers has grown by nearly two-thousand percent (from about 25,000 in 2007 to about 482,000 in 2013) (Citizenship and Services, 2014). More importantly, the costs of running program are high; studies estimate that it will cost 800 million dollars for 4 years while other studies estimate that an E-Verify mandate would cost businesses 2.7 billion dollars . As immigration reform continues to be debated at a national level, any law that requires mandatory employment verification would only increase the use of E-Verify. Given the costliness of the program and its potential expansion, this paper analyzes the effect of state mandates that require the use of E-Verify by employers.

The explicit goal of the E-verify program is to "*reduce* unauthorized employment without undue burden on employers or contributing to discrimination" (Westat, 2009). Therefore, the overarching goal of this paper is to analyze whether the E-Verify program achieving its own goal. This paper exploits variation in state mandates that make it obligatory for employers to use E-Verify to assess its effect on employment. The first order question is: What is the effect of mandating E-Verify on the employment of unauthorized immigrants? The second order question is: What is the effect of mandating E-Verify on the employment of authorized workers?

Two papers examine these particular questions and find conflicting results. Amuedo Dorantes and Bansak (2013) find that both universal and public-sector mandates reduce the likelihood of employment of likely unauthorized workers while having no effect on naturalized Hispanic workers but increasing the likelihood of native workers. Meanwhile, Orrenius and Zavodny (2014) find that universal E-Verify mandates had no effect on the likely of employment of likely unauthorized male workers and a positive effect for likely unauthorized female workers. Among other authorized groups, the authors find a positive effect on the likelihood of employment only for Mexican-born naturalized citizens.

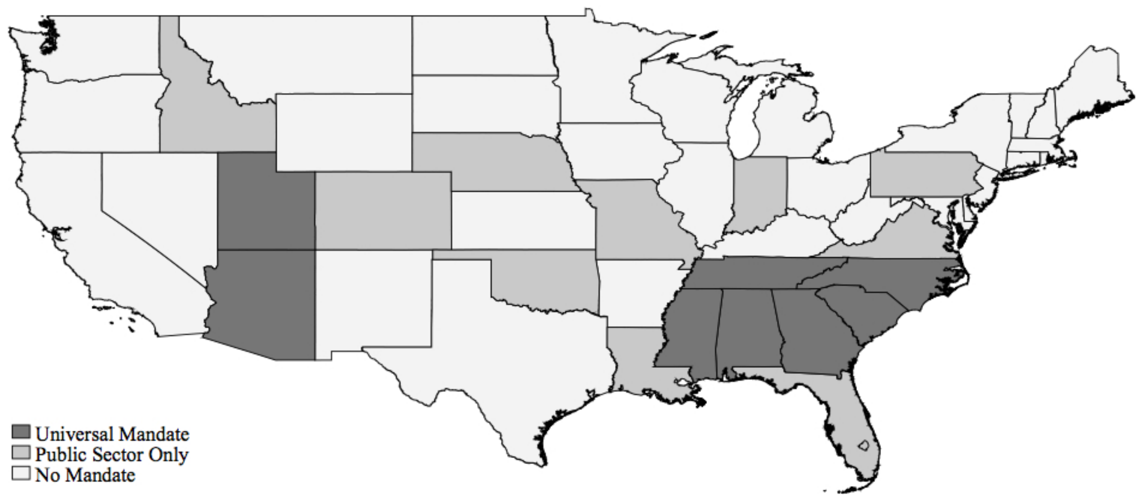
Of note, these researchers have used two main proxies: 1) Immigrants who have at most a high school diploma, are Hispanic, not naturalized U.S. citizens and under 45 years of age (Amuedo Dorantes and Bansak, 2013) and 2) immigrants who have at most a high school diploma, are from Mexico and are not naturalized U.S. citizens (Orrenius and Zavodny, 2014). Lacking the real authorization status of immigrants, the use of these proxies is justified on the grounds that these groups have been shown to be a good representation of the most likely unauthorized (as documented by (Passel, Cohn and Gonzalez-Barrera, 2013)).

In this paper, I add to the literature in three ways. First, I use a difference-in-difference estimation strategy (similar to Autor, Donohue III and Schwab (2006)) where I explicitly control for the pre- and post-treatment periods of the E-Verify policies. By doing so, I am able to more carefully analyze any potential anticipatory employment effects as well as lags in the effect of E-Verify on employment. Second, I adapt the Warren (2014) methodology for identifying likely unauthorized immigrants to the Current Population Survey in order to have a better benchmark by which to compare the two aforementioned proxies as measures of likely unauthorized immigrants. The Warren (2014) methodology uses the same logical edit approach as the Passel/Pew Hispanic Center numbers and thus is a good benchmark for comparing these likely unauthorized proxies in the context of E-Verify. And third, the enactment dates for universal E-Verify mandates are mostly in 2012. Given that Amuedo Dorantes and Bansak (2013) only includes data up to December 2011 and (Orrenius and Zavodny, 2014) look at data up to December 2012, I use data up to December 2014 to account for any potential lag in the effects of E-Verify on employment for all estimations.

2 Background on E-Verify

This section reviews basic information regarding E-Verify. As the Department of Homeland Security explains "E-Verify is an Internet-based system that compares information from an employee's Form I-9, Employment Eligibility Verification, to data from U.S. Department of Homeland Security and Social Security Administration records to confirm employment eligibility." Form I-9 is used for verifying the identity and employment authorization of individuals hired for employment in the United States. All U.S. employers must ensure proper completion of Form I-9 for each individual they hire for employment in the United States.

Historically, E-Verify is an offspring of ongoing immigration control reforms. The Immigration Reform and Control Act (IRCA) of 1986 required employers to examine documentation from each newly hired employee to prove his or her identity and eligibility to work in the United States - leads to I-9. The Illegal Immigration Reform and Immigrant Responsibility Act of 1996 (IIRIRA) was enacted and led to Basic Pilot Program of E-Verify. In 1999, the Designated Agent Basic Pilot Program was launched and, in 2007, the Basic Pilot was improved and renamed E-Verify. The growth of E-Verify has been exponential since then with almost 500 employers enrolled today and over 20 million cases being verified each year.



Source: LawLogix. Note: Map created by author

Figure 1: States with E-Verify Mandates

Lacking federal action, states have passed laws requiring the use of E-Verify to certain employers, some to all, and some states have made it illegal to use E-Verify at all until accuracy and timeliness issues are resolved. Table 1 (Appendix) and Figure 1 (below) display the states who have enacted E-Verify mandates by their date of enactment and the sectors the mandate affects. Eight states have enacted universal mandates that require all employers to use E-Verify on all *new* employees while ten have enacted mandates that affect employers in the public sector or that contract with the state. Two important facts should be noted. First, these mandates are relatively recent and thus, if there is a lag in the effect, the impact of the mandate will be difficult to measure. To address this, I use data up to December 2014. Second, as clearly evident in Figure 1, many of the states that have enacted universal E-Verify are in the south. Since many southern states have immigration policies that are not welcoming to immigrants, a potential confounding unobservable factor may be concurrent anti-immigrant legislation. While I do not explicitly control for these factors in this paper, I propose two ways to do so in the discussion part of the paper.

3 Basic Theory and Empirical Evidence

E-Verify mandates should, in theory, reduce the demand for labor of unauthorized immigrants. Standard economic theory predicts that, in the short run, when immigrants and natives are substitutes (perfect), this will lead to lower employment rates for unauthorized immigrants. Substitute labor (similarly skilled labor) should increase at the same time and result in an increase in employment rates for similarly skilled workers. The overall effect on employment levels of a state will depend on the elasticity of demand for each respective group. The direction of the effect on overall employment of a state is thus an empirical question.

Two important issues may confound these effects. First, employment may remain unchanged if there is high noncompliance rates and fraud. In this case, however, I would expect the E-Verify mandate to increase the marginal cost of hiring a new unauthorized worker. Whether this increase will be enough to incentivize employers to substitute for authorized workers is not clear. Indeed, Amuedo-Dorantes, Puttitanun and Martinez-Donate (2013) find that there is no statistically significant association between E-Verify mandates and the difficulties reported in accessing services by unauthorized immigrants, suggesting that noncompliance may be a real issue. Second, unauthorized workers may choose to migrate to different states that do not require E-Verify to find work. If this happens, the employment level of a state with an E-Verify mandate may remain unchanged. That is, the total in-labor-force population may change proportionately with the total employed population and thus result in no change in the employment level. Similarly, this may also happen in the state to which unauthorized migrate. To overcome this issue, I use monthly data that is weighted by the proportion of working age people in a given group (all workers, unauthorized workers, US born whites, etc.) for a given year in a given state relative to the national population of that given group. This weighting approach allows me to account for the growing population of a particular state in a given year relative to the country. Moreover, weighting by population share in a given year avoids placing greater importance on later months which may simply be reflecting the growth in the national population.

Two papers have estimated the effect of E-Verify mandates on employment to date. Catalina Amuedo-Dorantes and Cynthia Bansak, in a working paper and an AER Papers & Proceedings article, analyze Current Population Survey data from January 2004 to December 2011 (Amuedo Dorantes and Bansak, 2013) (Amuedo-Dorantes and Bansak, 2012). They employ a person-level linear probability model controlling for individual-level characteristics, industry fixed-effects, time fixed-effects, state-level time trends and monthly state unemployment rates. The authors find that both universal and public-sector mandates reduce the unemployment of likely unauthorized workers. One major shortcoming of this paper is the short time horizons that are analyzed. Considering that many of mandates have been enacted after 2011, it seems unlikely that the effects of these mandates will be fully observed. I overcome this by using the most recent data available (December 2014).

Similarly, Pia Orrenius and Madeline Zavodny use a simple difference-in-difference approach with year, month and state fixed effects (Orrenius and Zavodny, 2014). The authors use 2002-2012 Basic Monthly CPS data and find that E-Verify mandates reduce unauthorized Mexican immigrants' wages while increasing the labor force participation of likely unauthorized female Mexican immigrants. Like the paper mentioned above, the authors only analyze data from 2002-2012. A final issue is the identification of unauthorized populations which I discuss in more detail below.

Another relevant paper is a paper by Sarah Bohn, Magnus Lofstrom, and Steven Raphael titled "Did the 2007 Legal Arizona Workers Act Reduce the State's Unauthorized Immigrant Population?" (Bohn, Lofstrom and Raphael, 2013). Using CPS Basic Monthly data from January 1998 through December 2009, the authors use synthetic control method to select group of states against which the population trends of Arizona can be compared. They find a significant reduction in the proportion of the Arizona population that is foreign-born and in particular, that is Hispanic noncitizen. Importantly, this paper indirectly assess an effect of E-Verify in that LAWA requires employers to use E-Verify. However, the paper does not look at the effect on employment levels. Moreover, I look to analyze the effects in all states that mandate E-Verify while their paper only looks at Arizona.

4 Data and Model

The data used in this paper are the Basic Monthly Current Population Survey files from January 1994 through December 2014 as well as the 1994 through 2014 Annual Social and Economic Supplement (ASEC) (King et al., 2010) (note that IPUMS-CPS does not have the January 2014-December 2014 Basic Monthly CPS files available; these were added separately by the author). The CPS collects employment data on about 130,000 individuals a month and, because of its high response rate, it is typically assumed that unauthorized immigrants respond to the survey. Estimates on the unauthorized population in the U.S. were typically calculated using the Annual Social and Economic Supplement (commonly referred to as the March Supplement). I opt to use the Basic Monthly files in order to establish results that are comparable to the estimates from Orrenius and Zavodny (2014) and Amuedo Dorantes and Bansak (2013). By using the Basic Monthly files, I increase my sample size over 10-fold in comparison to using only the ASEC files. Finally, the Basic Monthly files allow me to identify the enactment of a mandate at the month-year level, rather than the year-level, and thus allows me to precisely identify the dates of the potential effects of E-Verify.

Table 2 (Appendix) breaks presents employment-to-population ratios for different subgroups across different mandate regimes. Column 1 presents the average employment-to-population levels across all states and all years. Column 2 presents these estimates only for states that eventually adopt universal E-Verify mandates. Column 3 presents estimates for states that eventually adopt E-Verify mandates that only apply to public sector employers. Column 4 presents the estimates for all states that eventually adopt any sort of E-Verify mandates (i.e. the combination of Column 2 and 3). The first pattern to notice is that universal adopters tend to have a slightly lower employment rate than the national average.

The employment-to-population ratio of unauthorized immigrants to working age (18-64) unauthorized immigrants show the opposite pattern. Across the two different definitions used in this paper, the employment ratios are higher for universal adopters than the national average.[1] This may reflect that eventual adopters respond to higher employment rates of unauthorized immigrants in order to curtail their employment. One last note on the summary statistics is that, because these

[1]Definition 1- self-identified Hispanic immigrants who have at most a high school diploma, are not naturalized U.S. citizens and are less than 39 years of age. Definition 2 - same as 1 except that Hispanics are identified by their reported country of birth.

are averages for the entire dataset, I am not teasing out employment effects of the recession. These statistics, therefore, are purely for expository purposes.

The variation in states that have mandatory E-Verify laws allows for the use of difference-in-difference estimation. The so-called natural experiment looks at exogenous sources of variation in policy that resemble experimental situations with a "treatment" group affected by the policy and a "comparison" group which is unaffected. The "causal" impact of the policy on labor supply, ignoring any structural considerations... *attempts to ignore the underlying theory and wishes to go straight to the effects of the particular policy.* I employ a strategy similar, if not almost identical, to Autor, Donohue III and Schwab (2006).

More explicitly, the estimation employed here compares the changes in employment in states that adopted E-Verify mandates in a given period to states that did not adopt any mandates during that same time period. I select a pre and post period of treatment that does not include all of 1994-2014. Doing so would mean that the pre-period for treatment would include over 15 years of data for some states. Rather, following Autor, Donohue III and Schwab (2006), I use up to 24 calendar months prior to implementation as the pre-period and months 13-36 for the post-period. Sensitivity analysis changes these periods to account for the fact that some states do not exhibit 13 months of post-period.

More formally, the econometric model used here is:

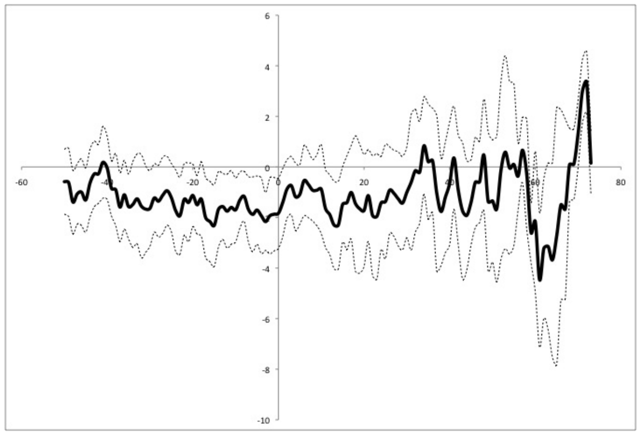
$$Y_{st} = \alpha + \beta_1 Treat_{st} + \beta_2 Post_{st} + \beta_3 Treat_{st} Post_{st} + \theta_s + \delta_t + \varepsilon_{st},$$

where $Treat_{st}$ indicates the 24 months before and the 36 months after adoption of an E-Verify Mandate in state s. $Post_{st}$ indicates the 13 to 36 month period after adoption. The coefficient on $Treat_{st}Post_{st}$, β_3, estimates the pre-to-post change in the employment levels in adopting states relative to the corresponding change in non-adopting states. θ_s is a vector of state dummies while δ_t is a vector of month-year dummies. I add a second specification that includes region x year dummies. Lastly, I weight the estimates in two different ways. First, by the share of the total population aged 18-64 in each state-year cell. Second, for each particular subgroup, by the share of the total population of that subgroup aged 18-64 in each state-year cell.

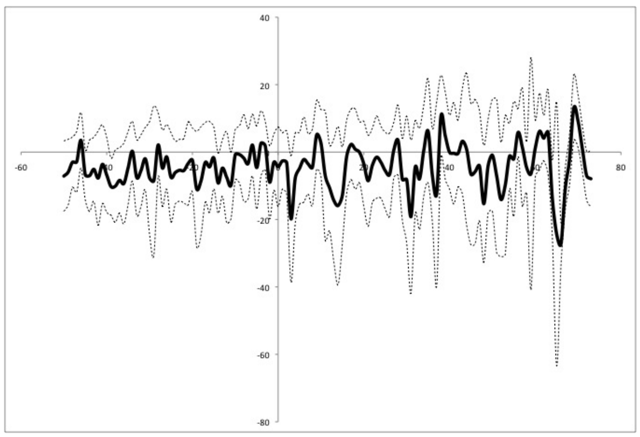
5 Results

I first present visual evidence of the effect of E-Verify mandates. I plot the log-employment-to-population ratios in adopting states relative to non-adopting states in the 4 years prior through as much as 8 years after adoption. Figures 2 and 3 presents the results for states that adopt any sort of E-Verify mandate. The estimates plot the difference between adopting states and non-adopting states. Notice that the points further to the right on the graphs represent the difference between states who have adopted E-Verify mandates for that many months compared to the non-adopters which means that only a handful of states will be part of the treatment group. This explains the larger confidence intervals in the right-most sections of the graphs. There is no visual evidence of an effect of all E-Verify mandates.

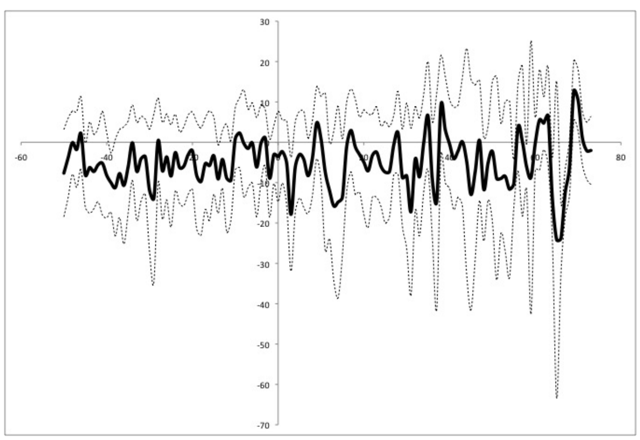
Figures 4 and 5 present the same outcomes but where the treatment group only include states that adopted universal E-Verify mandates. Here there is some visual evidence of an effect. Employment rates for all working ages seem to drop after then enactment of the policy. Unauthorized workers employment also seems to fall. Importantly, both definitions of unauthorized workers show a large spike after 40 months (about 3 years) of enactment.



(a) All Working-Age

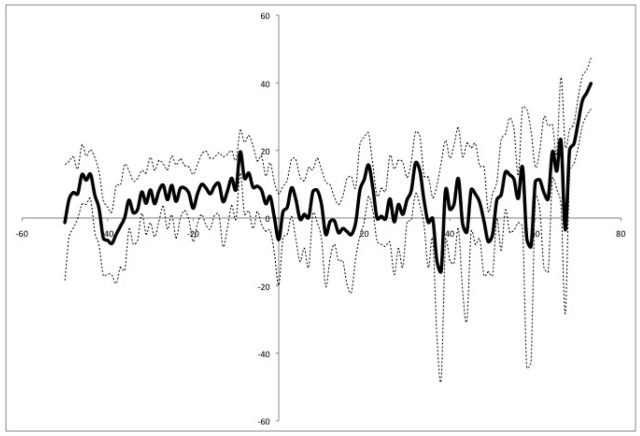


(b) Unauthorized Working-Age - Definition 1

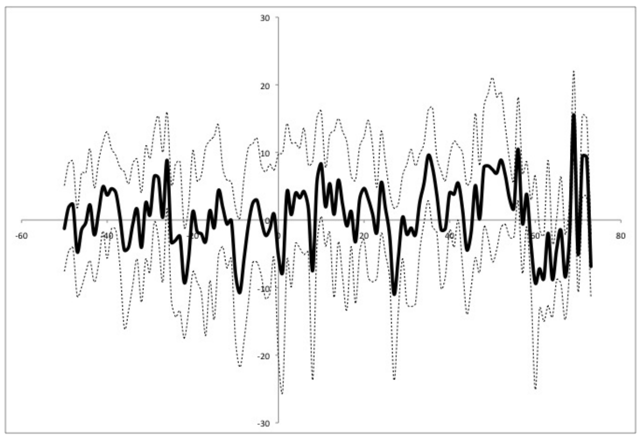


(c) Unauthorized Working-Age - Definition 2

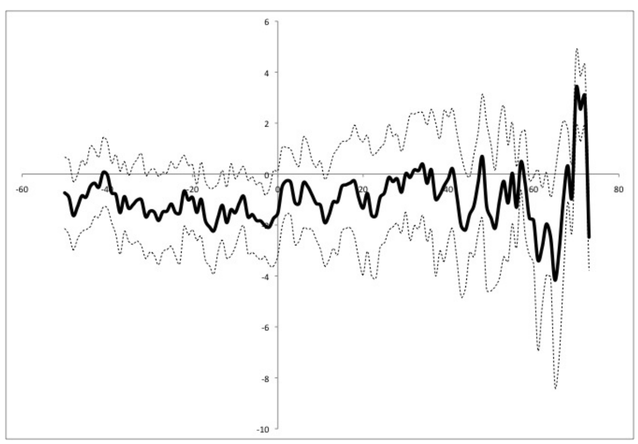
Figure 2: State Log Employment-to-Population Ratios before and After Adoption of Any Sector E-Verify Mandate: Monthly Leads and Lags



(a) Naturalized Hispanic Working-Age

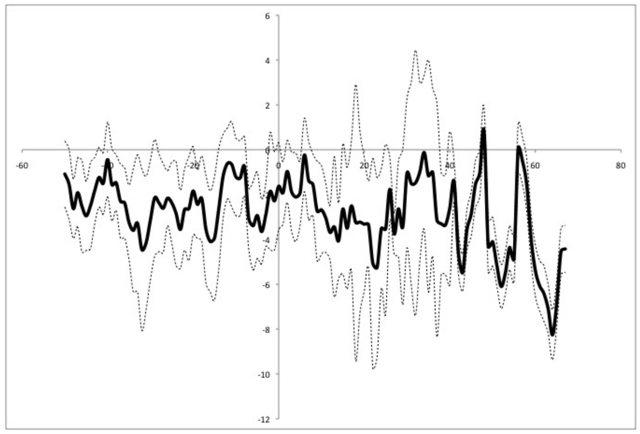


(b) Native-Born Hispanic Working-Age

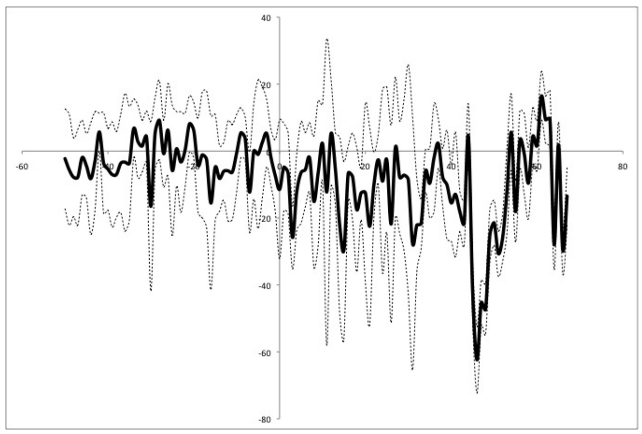


(c) U.S. Born White Working-Age

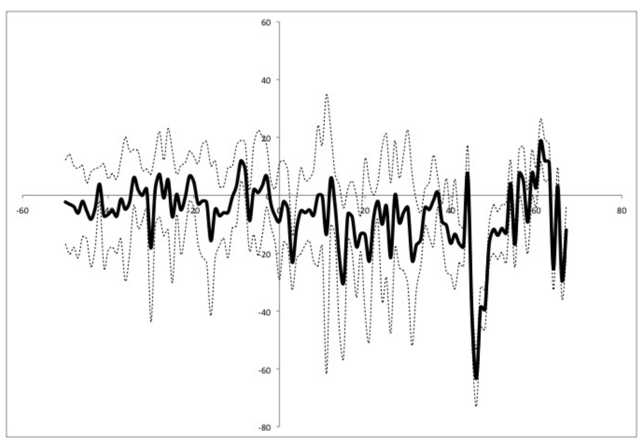
Figure 3: State Log Employment-to-Population Ratios before and After Adoption of Any E-Verify Mandate: Monthly Leads and Lags



(a) All Working-Age

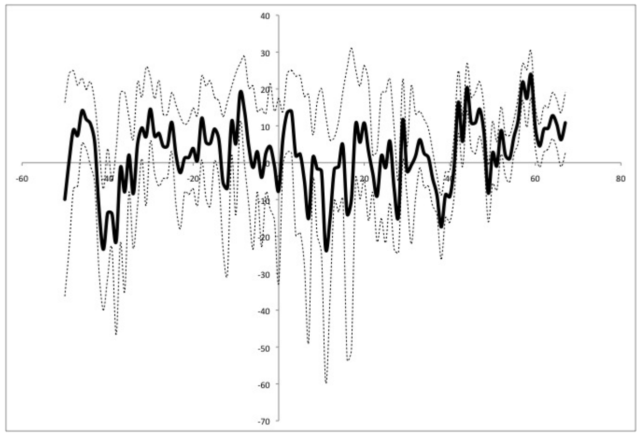


(b) Unauthorized Working-Age - Definition 1

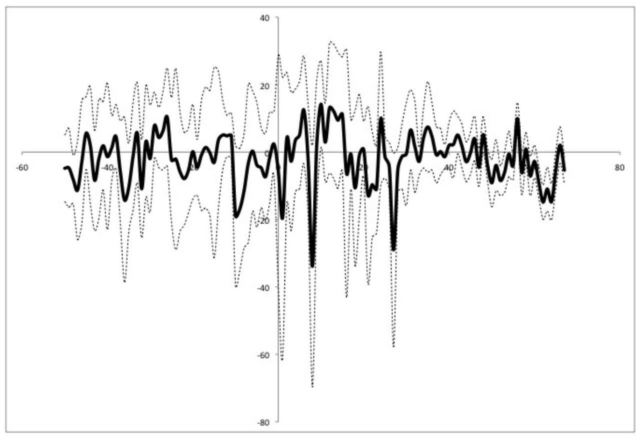


(c) Unauthorized Working-Age - Definition 2

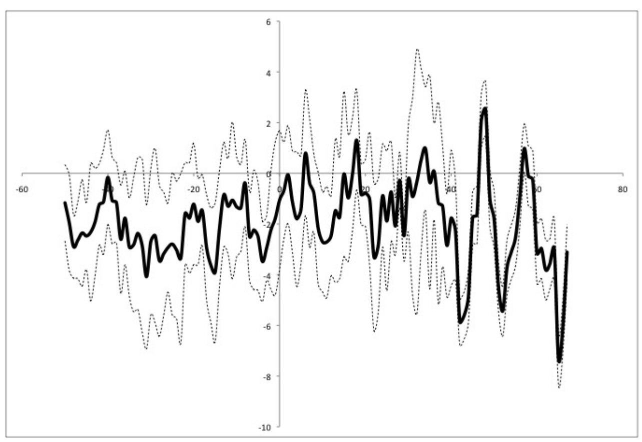
Figure 4: State Log Employment-to-Population Ratios before and After Adoption of Universal E-Verify Mandate: Monthly Leads and Lags



(a) Naturalized Hispanic Working-Age



(b) Native-Born Hispanic Working-Age



(c) U.S. Born White Working-Age

Figure 5: State Log Employment-to-Population Ratios before and After Adoption of Universal E-Verify Mandate: Monthly Leads and Lags

In regards to the spikes at around three years, one should remember that those estimates are really only capturing the differences in states that have had E-Verify for over three years. This would include the states of Arizona, Mississippi and Utah. The employment of naturalized Hispanics seems to rise after enactment while the U.S. born white population's employment seems to drop. An issue confounding these visual trends is that these dates mostly overlap with the recession and, if the recession affected adopting versus non-adopting states differently, then the patterns may be reflecting effects associated with the recession.

5.1 Regression Results

Table 3 (Appendix) presents the results from the various specifications of the model for each of the different subgroups for states with any E-Verify mandate. I find a negative effect on the likely unauthorized population (Definition 1 - self-identified Hispanic). Table 4 (Appendix) shows a similar pattern. The effect of universally mandated E-Verify on the overall employment-to-population ratio is negative (about -3 log points)[2]. That is, adopting universal E-Verify mandates decreases overall employment. The specification in Column 3 and 5 are identified as contrasting contemporaneous employment outcome in adopting states versus non-adopting states in the same geographic regions. This specification is preferred since economic conditions are likely to be similar within these regions which makes for a stronger causal argument (i.e. the changes identified are due to the change in policy and not idiosyncratic trends across states).

The results for both the unauthorized definitions show a large and negative effect of the universal E-Verify mandate. Indeed, the impact of the mandates reduces employment of unauthorized immigrants between 11 and 4 log points. The statistical significance is robust across three specifications. There is also evidence that the mandates decrease the employment of U.S. born Hispanics (columns 1 and 2). These results also make it clear that the estimates are sensitive to the weighting used in the regressions. My preferred weights are those that use the sub-population weights since it takes into account the change within in each state-year cell of that particular sub-population. This should account for changes in migration across these different sub-groups. Table 5 (Appendix) reports the results for the treatment effects of states that adopt only public-sector mandates.

[2]Log point to refer to a 0.01 change in the natural logarithm of the outcome measure. Log points are approximately equal to percentage points (equal to exp[log points] - 1.

5.2 Sensitivity Analysis

Table 6 (Appendix) reports the results of various sensitivity tests where the pre and post treatment time period are changed. Note that this table is only for the universal E-Verify mandates. The first column of each specification reports the results using weights from the entire working age population while the second columns use weights from the particular subgroups working age population. The results show that the effect on the unauthorized population are sensitive to the pre- and post-treatment periods. Once the post-period is increased to be greater than 25-36 months, the results become statistically significant. Most importantly, the results are rather robust for the entire working age population. Specifically, it seems that the employment of all working-age people is reduced as a result of universal E-Verify mandates. Lastly, the lack of significant results for Definition 2 of likely unauthorized workers suggests that the choice of proxy for likely unauthorized immigrants is extremely important.

6 Identifying Likely Unauthorized Immigrants

An important issue with all of these papers is the identification of unauthorized populations. Orrenius and Zavodny (2014) define "likely unauthorized" as immigrants who have at most a high school diploma, are from Mexico, and are not naturalized U.S. citizens. Amuedo Dorantes and Bansak (2013) extend this definition to include all Hispanics. Clearly, this definition is rather crude but is based on the most reliable data from Passel, Cohn and Gonzalez-Barrera (2013). In the analysis I performed above, I adopted the definition used in Amuedo Dorantes and Bansak (2013). Thus far, no paper has evaluated the validity of these proxies for likely unauthorized immigrants. While there is good reason to suspect that the measurement error in these proxies are random, there is also good reason to believe that the unauthorized immigrants that are not included in these proxies may be biasing these results. In this section I describe the Warren (2014) method for identifying likely unauthorized immigrants, how I adapted this methodology to the CPS, how the estimates roughly compare to published estimates and, most importantly, compare the Amuedo Dorantes and Bansak (2013) results using this new proxy for likely unauthorized immigrants.

There are various methods for identifying likely unauthorized immigrants in nationally-representative surveys such as the American Community Survey (ACS) and the CPS. For an overview of these methods, see Van Hook et al. (2014). In this paper, I adapt the Warren (2014) method which uses a logical edit method.[3] The general approach for identifying likely unauthorized immigrants using a logical edits approach is as follows. First, using the ACS, one picks out all the foreign-born population and picks out all the foreign-born people that are likely to be authorized based on logical observable characteristics. These include occupations that require legal status (i.e. government workers, police, etc.), legal temporary migrants, immediate relatives of U.S. citizens, people receiving certain public benefits and people from refugee countries. For the exact methodoloy, see Warren (2014). After these edits, there is a random selection step that ties the number of counted likely unauthorized immigrants to a set of population controls. These population controls are derived primarily from a different approach known as the residual method which in essence takes an "administrative" aggregate number of legal immigrants from DHS Annual Statistical Yearbooks and then uses the nationally representative surveys to calculate the total number of counted immigrants. The difference between the two, after adjusting for various other demographic issues (i.e. mortality, emigration, etc.), is the estimated total unauthorized immigrant population.

Both Warren (2014) and Passel, Cohn and Gonzalez-Barrera (2013) use the American Community Survey for their estimates. I adapt the methodology used in Warren (2014) to the CPS, which has precedence since earlier estimates of likely unauthorized were based on the Annual Social and Economic Supplement of the CPS (1994-2004, see Passel, Cohn and Gonzalez-Barrera (2013)). Figure 2 compares the estimated likely unauthorized populations of four different definitions: 1) "Logical edits" are the CPS-adapted logical edits estimates based off Warren (2014), 2) "Passel" are the published estimates from Passel, Cohn and Gonzalez-Barrera (2013), 3) "Proxy 1" or "Proxy - Mexican" are the estimates using immigrants who have at most a high school diploma, are from Mexico and are not naturalized U.S. citizens, and 4) "Proxy 2" or "Proxy - Hispanic" are the estimates using immigrants who have at most a high school diploma, are Hispanic, not naturalized U.S. citizens and under 45 years of age.

[3]Van Hook et al. (2014) present evidence that logical edit methods yield biased results. In the future, I look to use a statistical method for imputing likely unauthorized status as a robustness check on my findings.

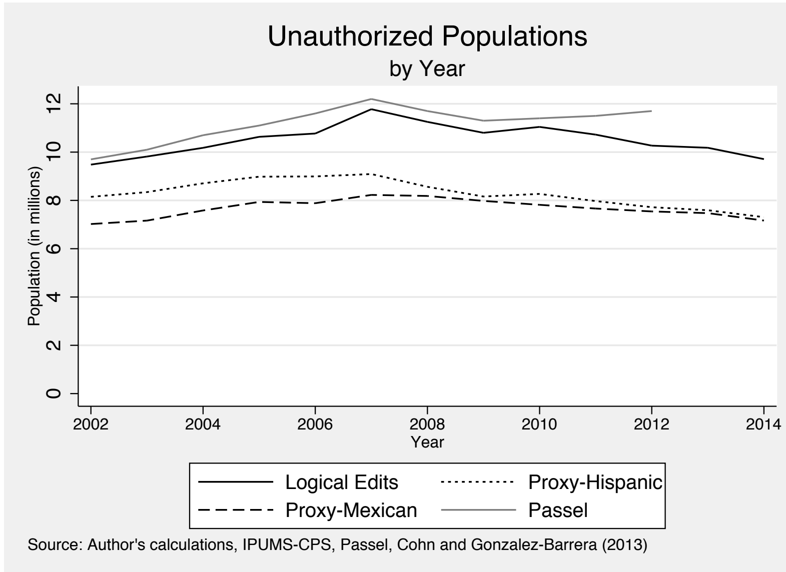


Figure 6

A couple patterns should be noted. First, and most obviously, the estimates for the two proxies are lower than the Warren and Passel numbers.This is obvious since these two proxies select subgroups of the likely unauthorized population. Importantly, the two sets of estimates follow rather parallel trends as compared to the Passel/Logical Edit methods. Second, the convergence between the two proxies after 2010 suggests that the use of the Mexican proxy may be warranted, as used by Orrenius and Zavodny (2014). Third, the logical edits estimates and the Passel estimates are rather similar. The differences between the two are most likely due to the undercount adjustment methods and to population control weights. As explained in Warren (2014), the undercount adjustment is the process in which more recently arrived immigrants are given a slightly higher weight in order to compensate for the likely higher nonresponse rates of recently arrived immigrants. The population control issue refers to the Census Bureau's population controls. For the ACS and CPS, survey weights are developed in order to make the surveys nationally representative. After each Decennial Census, these survey weights are adjusted to the most recent Census. Thus, in 2012, the survey weights are tied to the 2010 Census. Importantly, the survey weights are not tied to the 2010 Census for the pre-2010 years (i.e. 2008, 2009) though arguably the survey weights would be more accurate if tied to the 2010 Census instead of the 2000 Census. The 2010-2012 Passel estimates use the ACS which does not require reweighting. The CPS weights used in my analysis do not use this reweighting procedure and thus may explain the divergence between my estimates and the Passel estimates between 2010 and 2012. Despite these two issues, my estimates are rather close to the Passel estimates.

6.1 Evaluating Proxies for Likely Unauthorized Immigrants

The figures presented above give only a descriptive picture of how good these proxies are for likely unauthorized immigrants. The more important exercise is to analyze the patterns each proxy demonstrates in regards to the outcome of interest (i.e. employment). Figure 7 plots the average employment rates by year for authorized and unauthorized populations for my estimates ("Unauthorized" and "Authorized") as well as the rates for the two proxies ("Proxy 1" - Mexican, "Proxy 2" - Hispanic). The main takeaway from this practice is that the trends are parallel. This trend supports the use of the two proxies since the parallel trends would suggest that any results using the proxies are likely to be a lower-bound on the effects of E-Verify on the employment of unauthorized/authorized immigrants.

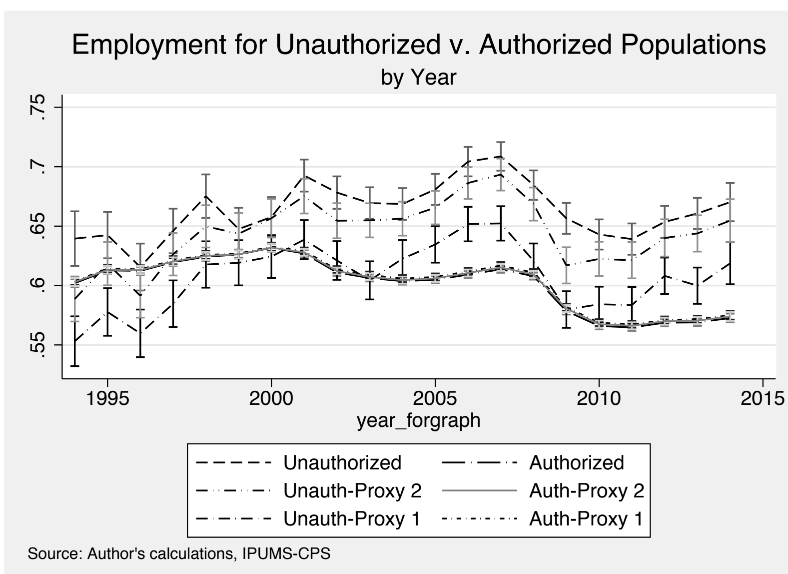


Figure 7

A final set of figures enlighten the discussion. Figure 8 through Figure 10 plot out the employment rates over time for the different likely unauthorized definitions in E-Verify and non-E-Verify states. Specifically, Figure 8 plots out the employment rates for likely unauthorized populations in states with any sort of E-Verify mandates. Though the rates seem to bounce around, the overall trends seems to be rather similar which suggests that the proxies may be a good reflection of the "true" employment rates of likely unauthorized immigrants. Figure 9 shows that this pattern is true for likely unauthorized populations in states with no E-Verify mandate. Figure 6 plots out the employment rates of authorized populations in both E-Verify and non-E-Verify states. The pattern clearly shows that the different proxies yield very similar patterns and that the employment levels for authorized populations in non-E-Verify states have been higher than those in E-Verify states since about 2004.

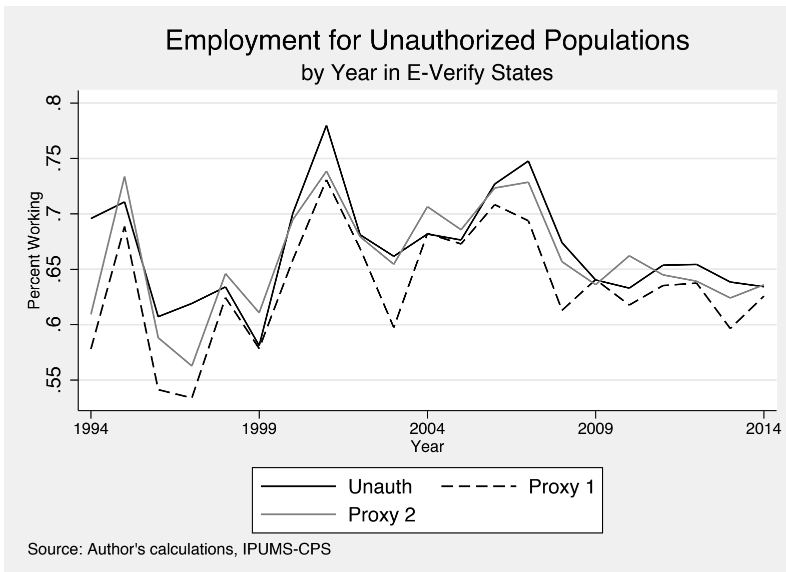


Figure 8

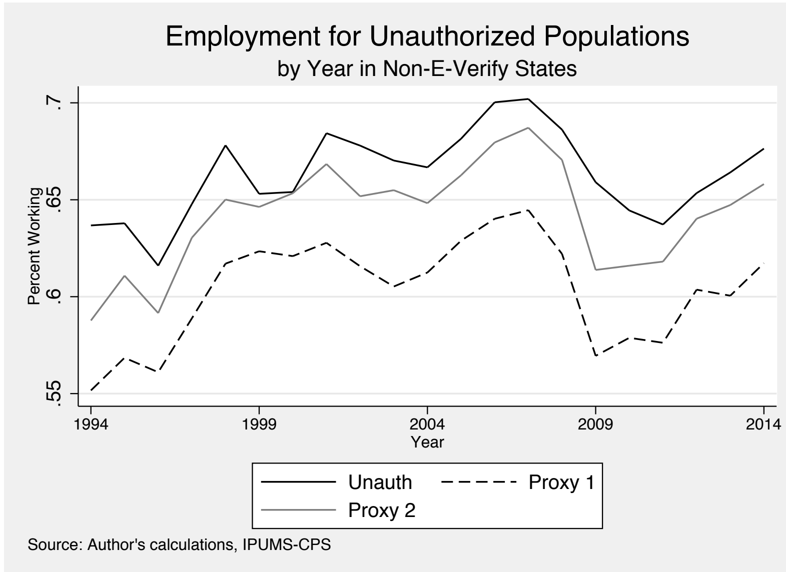


Figure 9

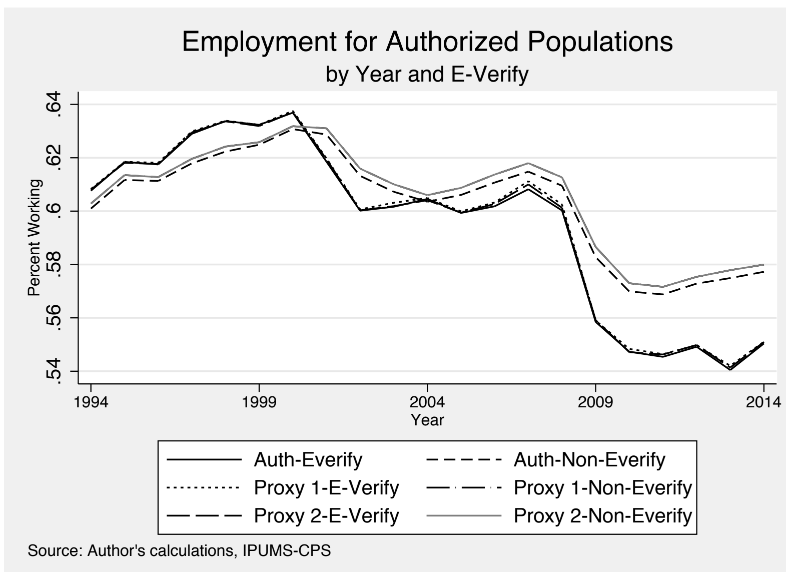


Figure 10

These descriptive results give evidence in favor of using these proxies for likely unauthorized immigrants. But in order to more fully analyze their effectiveness, I replicate the estimation from Amuedo Dorantes and Bansak (2013) and compare the results from the proxies with those using the logical edit approach. The model used by Amuedo Dorantes and Bansak (2013) is as follows:

$$L_{ist} = \alpha + \beta_1 E - Verify - all_{st} + \beta_2 E - Verify - public_{st} + X_{ist}\gamma + \beta_3 U_{st} + \delta_s + \theta_t + \delta_s t + \varepsilon_{ist},$$

where L is a dummy for employment, E-Verify-all is a dummy for a state with a universal E-Verify mandate, E-Verify-public is a dummy for a state with a public E-Verify mandate. X are individual-level controls including gender, race, age, marital status, number of children, educational attainment and industry fixed effects. U_{st} are monthly state unemployment rates, δ_s are state fixed-effects, θ_t are time fixed-effects and $\delta_s t$ are state-level time trends.

I run this model on three different datasets with two slightly different samples for employment. Since the logical edit approach requires using the ASEC, I use the ASEC CPS dataset from 2002-2014 (results presented in Table 7). Further, I use a sample where only people in the labor force are included and then a sample where all people are included regardless of their labor force status. This is done because there is ambiguity in Amuedo Dorantes and Bansak (2013) as to how they define their sample.

Focusing first on the difference between the two different definitions of likely unauthorized (Rows: Logical Edits and Proxy 2). The results show that the Logical Edits give different results for both Universal and Public Sector E-Verify using the In Labor Force population. Specifically, workers in universal E-Verify states using the logical edit see an increase of 5 percentage points while using the proxy shows no statistically significant effect. Meanwhile, workers in states with public sector E-Verify see a decrease of 4.4 percentage points in the likelihood of employment using the logical edit approach whereas the proxy results in a 1.7 percentage point increase.

Even when the sample is changed to include all people, the results are still inconsistent across the two likely unauthorized definitions. Though the effect of universal E-Verify mandates are both negative across the two definitions (4 percentage points v 10.3 percentage points). The effect of the public sector, however, go in opposite directions (3 percentage points v negative 8.1 percentage points). If we are to consider the logical edit method as a more accurate measure of likely unauthorized, then the difference in results should attenuate towards zero if the only difference between the two measures are due to random measurement error. These results suggest that the employment rates of immigrants using the proxy, even once all the controls are used, are unable to tease out unobservable differences related to employment that are captured once I use the logical edits method.

Since the ASEC must be used in order to use the logical edit method, these results are not completely comparable to Amuedo Dorantes and Bansak (2013). I thus replicate the model using all

Basic Monthly CPS data from January 2002 through December 2014 (note that this period include three more years than Amuedo Dorantes and Bansak (2013)). Here we note that the findings in Amuedo Dorantes and Bansak (2013) do not hold for either the full in labor force population or the full CPS sample. Specifically, in Table 2 of Amuedo Dorantes and Bansak (2013), the coefficients on Universal and Public Sector are -0.046 and -0.20, respectively. Using the same proxy for likely unauthorized, I find an effect of 0.028 for universal and -0.002 for Public Sector when using only the in labor force population. Even in using the full CPS sample, the results do not show the same pattern as Amuedo Dorantes and Bansak (2013). These results imply that the inclusion of the subsequent year after 2011 are important in explaining the effects of E-Verify mandates.

Another important implication of these findings is that the ASEC and the Basic Monthly CPS's will give different estimated effects of E-Verify mandates. The question thus becomes: Which dataset is better suited for analyzing the effects of E-Verify? The answer is obviously not clear cut. First, it warrants mentioning that the ASEC is less preferable in terms of sample size and representativeness of labor force status for the entire calendar year. Because the ASEC is fielded in March and augmented with more respondents from different months, the employment status question is not representative of the entire year but arguably just March. Table 9 reports the results from using only the March Basic Monthly CPS' from 2002-2014 which allows me to analyze the difference between using the Basic Monthly CPS versus the ASEC. The results suggest that the ASEC and the March Basic may be interchangeable. Specifically, the effects on universal mandates for the proxy in the ASEC are negative but not statistically significant while the public sector is positive and statistically significant. Meanwhile, just using the March Basic Monthly CPS, we see a negative and statistically significant effect for universal E-Verify and a positive and statistically significant effect for public sector mandates. This result is not entirely surprising but important in knowing that the ASEC does not differ significantly from the March Basic Monthly CPS results.

This being said, it still remains unclear whether the ASEC is preferable to the full set of Basic Monthlies. While the main advantage of using the ASEC is the ability to use the logical edit approach for likely unauthorized immigrants, the ASEC results (Table 7) do not yield similar results to those using the Basic Monthlies (Table 8). And while the increased sample size and variability in employment rates is gained in using the Basic Monthlies, Table 7 shows that the proxy for unauthorized immigrants is not representative of the better-identified unauthorized immigrants using the ASEC. The variation in monthly employment rates is of particular interest given that yearly estimates will not capture the exact enactment date effects and thus not give reliable estimates of the effect of E-Verify mandates.

In sum, this analysis shows that the use of proxies for likely unauthorized immigrants gives estimates of the effect of E-Verify that are arguably inconsistent to those using an arguably better method for identifying unauthorized immigrants. These results, however, are not directly comparable to those in Amuedo Dorantes and Bansak (2013) since I use the ASEC while they use the Basic Monthlies. Moreover, once I replicate Amuedo Dorantes and Bansak (2013) with 3 more years of data, the results found in Amuedo Dorantes and Bansak (2013) no longer hold. In the future, I look to use the ASEC to measure the difference in using the logical edit method as opposed to the proxy and somehow use this difference to construct a more refined measure of likely unauthorized than can then be used with the Basic Monthlies.

7 Future Research and Conclusion

Overall, I find evidence in favor of the effectiveness of universal E-Verify mandates in decreasing the overall employment levels of likely unauthorized immigrants. This effect ranges between negative 11 and negative 4 percent. These effects, however, are sensitive to the particular model specification and definition of likely unauthorized immigrant. While these results support the findings in Amuedo Dorantes and Bansak (2013), the evaluation of the proxy for likely unauthorized immigrants suggests that these proxies may be biased. Using the Warren (2014) method may allow for a potential statistical correction. Lastly, the use of 3 more years of data show that the results found in Amuedo Dorantes and Bansak (2013) no longer hold, suggesting that there may be important lags in the effect of E-Verify. This finding is corroborated by the main analysis in used in this paper (see Table 4 and Table 6).

A few threats to validity warrant discussion. First, it is likely that states that pass E-Verify mandates also pass other anti-immigration laws that may affect employment rates. Indeed, Arizona's Legal Arizona Workers Act included an E-Verify mandate as one of many measures to curtail unauthorized immigrants' employment. Without explicitly controlling for these other measures, the estimated effects of E-Verify mandates may very well be due to these other factors. Thus, it is necessary to find a way to control for this "policy climate". Various authors have developed a measure that would account for this immigration policy climate. For example, Leerkes, Leach and Bachmeier (2012) conduct factor analysis to code states into three different levels of immigration control: high, moderate and low. Using data on employers participation in E-Verify, restrictive state laws, county and city involvement in the 287(g) program, the authors are able to construct a single measure ("internal control index") for each state by year that is then used to classify each state into the different levels of control. By using these more refined measures by state and year, I could capture the immigration policy climate of each state.

This measure would also allow me to control for a state's previous exposure to E-Verify. Since E-Verify can also be voluntarily enrolled in by a firm, if a state has a lot of firms enrolled in E-Verify before the mandates, the marginal effect of mandating E-Verify may be very minimal. Having this measure control for this prior E-Verify enrollment will tease out this confounding factor.

One last threat to validity is the level of enforcement within each state of the E-Verify mandate. While the E-Verify program can track the number of cases employers process, there is no guarantee that employers are processing all potential hires. The need here is to find a measure that quantifies the enforcement of anti-immigration laws. Fortunately, various authors have conducted these studies and some would be suitable for my study. In particular, Watson (2010) codes information on 287(g) on a year-by-year basis between 1993 and 2002. Using a dataset that "consists of counts of Immigration and Naturalization Services 'deportable aliens located' as the result of internal investigations, by INS internal district, country of origin, and fiscal year" (Watson, 2010). The correlation between 287(g) enforcement and E-Verify mandates is arguably strong enough for this measure to be a good proxy of enforcement. Thus, using a measure like the one presented by Watson (2010) (but extended through 2014), it would be possible to control for enforcement levels of E-Verify mandates that may be confounding the analysis conducted thus far in this paper.

References

Amuedo-Dorantes, Catalina, and Cynthia Bansak. 2012. "The labor market impact of mandated employment verification systems." The American Economic Review, 102(3): 543–548.

Amuedo Dorantes, Catalina, and Cynthia Bansak. 2013. "Employment Verification Mandates and the Labor Market Outcomes of Likely Unauthorized and Native Workers." IZA Discussion Paper.

Amuedo-Dorantes, Catalina, Thitima Puttitanun, and Ana P Martinez-Donate. 2013. "How do tougher immigration measures affect unauthorized immigrants?" Demography, 50(3): 1067–1091.

Autor, David H, John J Donohue III, and Stewart J Schwab. 2006. "The costs of wrongful-discharge laws." The Review of Economics and Statistics, 88(2): 211–231.

Bohn, Sarah, Magnus Lofstrom, and Steven Raphael. 2013. "Did the 2007 Legal Arizona Workers Act reduce the state's unauthorized immigrant population?" Review of Economics and Statistics, , (0).

Citizenship, US, and Immigration Services. 2014. "History and Milestones."

King, Miriam, Steven Ruggles, J. Trent Alexander, Sarah Flood, Katie Genadek, Matthew B. Schroeder, and Brandon Trampe. 2010. "Integrated Public Use Microdata Series, Current Population Survey: Version 3.0." Minneapolis: University of Minnesota [Machine-readable database].

Leerkes, Arjen, Mark Leach, and James Bachmeier. 2012. "Borders behind the border: An exploration of state-level differences in migration control and their effects on US migration patterns." Journal of Ethnic and Migration Studies, 38(1): 111–129.

Orrenius, Pia M, and Madeline Zavodny. 2014. "How do e-verify mandates affect unauthorized immigrant workers?" IZA Discussion Paper.

Passel, Jeffrey, D Cohn, and A Gonzalez-Barrera. 2013. "Population Decline of Unauthorized Immigrants Stalls, May Have Reversed." Population.

Van Hook, Jennifer, James D Bachmeier, Donna L Coffman, and Ofer Harel. 2014. "Can We Spin Straw Into Gold? An Evaluation of Immigrant Legal Status Imputation Approaches." Demography, 1–26.

Warren, Robert. 2014. "Democratizing Data about Unauthorized Residents in the United States: Estimates and Public-Use Data, 2010 to 2013." Journal on Migration and Human Security, 2(4): 305–328.

Watson, Tara. 2010. "Inside the Refrigerator: Immigration Enforcement and Chilling Effects in Medicaid Participation." National Bureau of Economic Research Working Paper 16278.

Westat. 2009. "Findings of the E-Verify Program Evaluation."

Table 1: Summary of States with E-Verify Mandates

State	Date Enacted	Sectors
Alabama	May 2012	All Employers
Arizona	April 2008	All Employers
Georgia	January 2012	All Employers
Mississippi	July 2008	All Employers
North Carolina	October 2012	All Employers
South Carolina	January 2012	All Employers
Tennessee	January 2012	All Employers
Utah	July 2010	All Employers
Colorado	May 2008	Public Sector
Florida	January 2011	Public Sector
Idaho	July 2009	Public Sector
Indiana	May 2011	Public Sector
Louisiana	January 2012	Public Sector
Missouri	August 2009	Public Sector
Nebraska	October 2009	Public Sector
Oklahoma	November 2007	Public Sector
Pennsylvania	January 2013	Public Sector
Virginia	December 2013	Public Sector

Table 2: Summary Statistics - State-Year-Month Estimates

	(1) Entire Population	(2) Universal	(3) Public Sector	(4) Any Type
Total employed / Total ages 18-64 population	0.750 (0.0445)	0.717 (0.0449)	0.744 (0.0470)	0.734 (0.0479)
Total unauthorized / Total ages 18-64 unauth. - Def'n 1	0.655 (0.179)	0.680 (0.158)	0.660 (0.176)	0.668 (0.170)
Total unauthorized / Total ages 18-64 unauth. - Def'n 2	0.666 (0.197)	0.684 (0.170)	0.671 (0.186)	0.676 (0.180)
Total Male / Total ages 18-64 male	0.803 (0.0459)	0.788 (0.0590)	0.804 (0.0504)	0.798 (0.0545)
Total Female / Total ages 18-64 female	0.697 (0.0509)	0.650 (0.0370)	0.686 (0.0487)	0.672 (0.0477)
Total Naturalized Hisp. / Total ages 18-64 naturalized hisp.	0.775 (0.184)	0.761 (0.220)	0.750 (0.201)	0.755 (0.209)
Total U.S. Born Hisp. / Total ages 18-64 U.S. Born Hisp.	0.695 (0.119)	0.715 (0.161)	0.709 (0.111)	0.711 (0.133)
Total U.S. Born White / Total ages 18-64 U.S. Born White	0.769 (0.0431)	0.738 (0.0400)	0.762 (0.0451)	0.753 (0.0446)

Authors' estimates. CPS Basic monthly files from 1994-2014. Estimates weighted using CPS sampling weights. Standard deviations in parentheses.

Table 3: Difference-in-Difference Estimates of the Impact of Any E-Verify Mandate on State Employment-to-Population Ratio

	100 X ln(Employment/Population) Jan. 1994 - Dec. 2014				
	Unweighted	Weighted	Weighted	Weighted	Weighted
	(1)	(2)	(3)	(4)	(5)
All Working Age (18-64)	-0.926	-0.793	-0.544	-0.793	-0.544
	(0.783)	(0.703)	(0.579)	(0.703)	(0.579)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.887	0.876	0.892	0.876	0.892
Unauth. Working Age	-7.695*	-6.020*	-2.419*	-5.689	-2.024
Definition 1	(4.239)	(3.416)	(1.429)	(3.739)	(1.341)
n	10,278	10,278	10,278	10,278	10,278
Adj. R^2	0.109	0.125	0.337	0.142	0.354
Unauth. Working Age	-1.680	-3.146	-0.724	-2.484	0.131
Definition 2	(3.303)	(2.913)	(1.760)	(3.235)	(2.131)
n	11,053	11,053	11,053	11,053	11,053
Adj. R^2	0.104	0.136	0.300	0.158	0.325
Male Working Age	-0.572	-0.590	-0.577	-0.710	-0.690
	(0.933)	(0.796)	(0.797)	(0.739)	(0.739)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.837	0.844	0.844	0.859	0.859
Female Working Age	-1.234	-0.970	-0.985	-0.372	-0.386
	(0.739)	(0.740)	(0.744)	(0.559)	(0.563)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.852	0.834	0.833	0.850	0.849
Naturalized Hispanic	1.063	1.501	0.369	2.518	0.818
Working Age	(3.010)	(2.789)	(1.656)	(2.832)	(1.580)
n	11,024	11,024	11,024	11,024	11,024
Adj. R^2	0.081	0.083	0.153	0.100	0.180
U.S. Born Hispanic	-0.775	-1.293	-0.658	-0.503	0.401
Working Age	(2.579)	(2.267)	(1.673)	(2.202)	(1.283)
n	12,509	12,509	12,509	12,509	12,509
Adj. R^2	0.180	0.277	0.530	0.293	0.569
U.S. Born White	-0.441	-0.103	-0.262	0.234	0.211
Working Age	(0.678)	(0.712)	(0.653)	(0.586)	(0.599)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.854	0.840	0.850	0.858	0.865
Region x Year	No	No	Yes	No	Yes
Weight	None	All	All	Sub-pop	Sub-pop

Outcome for each panel is the particular sub-population employment to the total sub-population in that state.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 4: Difference-in-Difference Estimates of the Impact of Universal E-Verify Mandate on State Employment-to-Population Ratio

	100 X ln(Employment/Population) Jan. 1994 - Dec. 2014				
	Unweighted	Weighted	Weighted	Weighted	Weighted
	(1)	(2)	(3)	(4)	(5)
All Working Age (18-64)	-3.076***	-2.885***	-2.311***	-2.885***	-2.311***
	(0.808)	(0.560)	(0.683)	(0.560)	(0.683)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.890	0.881	0.896	0.881	0.896
Unauth. Working Age	-10.697**	-7.429***	-4.030**	-5.110	-3.289
Definition 1	(4.060)	(2.502)	(1.523)	(3.086)	(2.031)
n	10,278	10,278	10,278	10,278	10,278
Adj. R^2	0.109	0.125	0.339	0.141	0.356
Unauth. Working Age	-5.928	-5.653*	-2.826	-3.208	-1.810
Definition 2	(3.757)	(3.194)	(2.980)	(3.297)	(3.261)
n	11,053	11,053	11,053	11,053	11,053
Adj. R^2	0.104	0.134	0.300	0.157	0.325
Male Working Age	-2.931***	-2.632***	-2.616***	-2.378***	-2.354***
	(1.026)	(0.679)	(0.678)	(0.755)	(0.754)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.841	0.848	0.848	0.862	0.863
Female Working Age	-3.117***	-3.074***	-3.102***	-2.220***	-2.253***
	(0.787)	(0.723)	(0.722)	(0.815)	(0.814)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.854	0.838	0.837	0.853	0.852
Naturalized Hispanic	3.346	1.957	1.224	3.302	3.230
Working Age	(5.839)	(5.144)	(2.631)	(4.755)	(2.013)
n	11,024	11,024	11,024	11,024	11,024
Adj. R^2	0.081	0.083	0.153	0.100	0.180
U.S. Born Hispanic	-5.822*	-6.756*	-0.990	-5.644	1.088
Working Age	(3.447)	(3.430)	(2.060)	(3.747)	(2.133)
n	12,509	12,509	12,509	12,509	12,509
Adj. R^2	0.182	0.280	0.532	0.295	0.569
U.S. Born White	-2.173***	-2.035***	-2.218***	-1.359*	-1.452*
Working Age	(0.729)	(0.654)	(0.617)	(0.806)	(0.735)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.856	0.844	0.854	0.860	0.867
Region x Year	No	No	Yes	No	Yes
Weight	None	All	All	Sub-pop	Sub-pop

Outcome for each panel is the particular sub-population employment to the total sub-population in that state.

* p<0.1, ** p<0.05, *** p<0.01

Table 5: Difference-in-Difference Estimates of the Impact of Public Sector E-Verify Mandate on State Employment-to-Population Ratio

	100 X ln(Employment/Population) Jan. 1994 - Dec. 2014				
	Unweighted	Weighted	Weighted	Weighted	Weighted
	(1)	(2)	(3)	(4)	(5)
All Working Age (18-64)	0.055	0.428	0.280	0.428	0.280
	(0.707)	(0.807)	(0.470)	(0.807)	(0.470)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.890	0.881	0.896	0.881	0.896
Unauth. Working Age Definition 1	-7.040*	-5.719**	0.030	-6.151*	-0.058
	(3.578)	(2.710)	(1.700)	(3.176)	(1.901)
n	10,278	10,278	10,278	10,278	10,278
Adj. R^2	0.109	0.126	0.336	0.142	0.354
Unauth. Working Age Definition 2	-1.832	-3.229	0.081	-2.622	0.748
	(4.432)	(3.943)	(2.429)	(3.628)	(2.623)
n	11,053	11,053	11,053	11,053	11,053
Adj. R^2	0.104	0.135	0.301	0.157	0.325
Male Working Age	-0.222	-0.352	-0.359	-0.608	-0.607
	(0.816)	(0.976)	(0.979)	(0.884)	(0.888)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.841	0.848	0.848	0.863	0.863
Female Working Age	0.432	1.275	1.273	1.223**	1.220**
	(0.797)	(0.887)	(0.888)	(0.551)	(0.557)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.854	0.838	0.837	0.852	0.851
Naturalized Hispanic Working Age	-1.771	-1.974	-3.059	-0.823	-3.270
	(2.600)	(2.967)	(2.537)	(2.910)	(2.007)
n	11,024	11,024	11,024	11,024	11,024
Adj. R^2	0.081	0.084	0.153	0.100	0.180
U.S. Born Hispanic Working Age	-1.509	-0.870	-2.543	-2.289	-3.890**
	(2.692)	(2.388)	(2.210)	(2.221)	(1.537)
n	12,509	12,509	12,509	12,509	12,509
Adj. R^2	0.181	0.280	0.531	0.295	0.569
U.S. Born White Working Age	-0.031	0.280	0.257	0.123	0.293
	(0.688)	(0.954)	(0.874)	(0.710)	(0.644)
n	12,600	12,600	12,600	12,600	12,600
Adj. R^2	0.856	0.844	0.854	0.860	0.867
Region x Year	No	No	Yes	No	Yes
Weight	None	All	All	Sub-pop	Sub-pop

Outcome for each panel is the particular sub-population employment to the total sub-population in that state.

* p<0.1, ** p<0.05, *** p<0.01

Table 6: Sensitivity Analysis - Universal-Everify

	Pre=24 mon Post=24 mon		Pre=24 mon Post=12 mon		Pre=24 mon Post=25-36 mon		Pre=24 mon Post=48 mon		Pre=36 mon Post=48 mon		Pre=48 mon Post=48 mon	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
All Working Age	-2.906***	-2.479***	-2.506***	-2.329***	-2.057***	-1.462**	-2.832***	-2.207***	-2.385***	-1.834***	-2.068***	-1.715***
(18-64)	(0.599)	(0.679)	(0.475)	(0.571)	(0.562)	(0.592)	(0.531)	(0.651)	(0.491)	(0.516)	(0.553)	(0.529)
n	12600	12600	12600	12600	12600	12600	12600	12600	12600	12600	12600	12600
Adj. R^2	0.881	0.896	0.882	0.896	0.879	0.895	0.881	0.896	0.881	0.896	0.881	0.897
Unauth. Working	-3.472	-1.122	-6.765	-3.236	-3.744	-4.485**	-6.249*	-4.294**	-5.140*	-3.700***	-2.043	-2.274**
Definition 1	(3.366)	(1.745)	(5.452)	(3.465)	(2.268)	(1.917)	(3.110)	(1.741)	(2.568)	(1.044)	(2.781)	(1.088)
n	10278	10278	10278	10278	10278	10278	10278	10278	10278	10278	10278	10278
Adj. R^2	0.141	0.356	0.141	0.355	0.141	0.356	0.141	0.356	0.141	0.356	0.141	0.355
Unauth. Working	-2.622	-0.520	-8.760**	-5.251**	-3.010	-2.780	-3.794	-2.301	-3.113	-1.648	0.088	0.107
Definition 2	(3.252)	(3.299)	(3.945)	(2.419)	(2.745)	(2.601)	(3.378)	(2.934)	(2.751)	(2.004)	(2.871)	(1.893)
n	11053	11053	11053	11053	11053	11053	11053	11053	11053	11053	11053	11053
Adj. R^2	0.157	0.325	0.157	0.326	0.157	0.325	0.157	0.325	0.157	0.325	0.158	0.325
Weight	All	Sub-Pop	All	Sub-Pop	All	Sub-Pop	All	Sub-Pop	All	Sub-Pop	All	Sub-Pop

* p<0.1, ** p<0.05, *** p<0.01

Outcome for each panel is the particular sub-population employment to the total sub-population in that state.

All specifications include region x year dummies and are weighted.

Table 7: Estimates of the Impact of E-Verify Mandates on Probability of Employment - 2002 - 2014 - Annual Social and Economic Supplement

	In Labor Force Population			Full CPS		
	All	Male	Female	All	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Logical Edits						
Universal	0.050***	0.042***	0.024***	-0.040***	-0.050***	0.103***
	(0.004)	(0.005)	(0.006)	(0.007)	(0.006)	(0.010)
Public Sector	-0.044***	-0.062***	-0.046***	0.030***	-0.210***	0.335***
	(0.003)	(0.003)	(0.006)	(0.004)	(0.007)	(0.007)
N	60,266	39,338	20,928	83,335	44,552	38,783
Proxy 2						
Universal	-0.005	-0.029***	0.041***	-0.103***	-0.155***	0.072***
	(0.005)	(0.005)	(0.012)	(0.004)	(0.010)	(0.009)
Public Sector	0.017***	0.032***	-0.037**	-0.081***	-0.297***	0.196***
	(0.003)	(0.003)	(0.017)	(0.004)	(0.006)	(0.010)
N	47,993	32,596	15,397	67,650	36,817	30,833
Naturalized Hispanic						
Universal	0.047***	0.048***	0.107***	-0.161***	-0.407***	-0.012
	(0.004)	(0.008)	(0.017)	(0.011)	(0.013)	(0.020)
Public Sector	-0.093***	-0.001	-0.200***	0.047***	-0.052***	0.129***
	(0.005)	(0.007)	(0.008)	(0.015)	(0.013)	(0.017)
N	31,193	16,712	14,481	45,748	21,184	24,564
US-Born non-Hispanic						
Universal	-0.029***	-0.035***	-0.021***	-0.012***	-0.026***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Public Sector	-0.021***	-0.051***	0.014***	0.018***	-0.020***	0.052***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
N	1,007,447	515,796	491,651	1,536,977	728,680	808,297

Controls include gender (when applicable), race, age, marital status, number of children in household, educational attainment, industry fixed effects, state fixed effects, time (year, month) fixed effects, state specific time trends, unemployment rates. Standard errors clustered at the state level. All regressions use survey weights (wtsupp).
* p<0.1, ** p<0.05, *** p<0.01

Table 8: Estimates of the Impact of E-Verify Mandates on Probability of Employment - January 2002 - December 2014 - Basic Monthly CPS

	In Labor Force Population			Full CPS		
	All	Male	Female	All	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Likely Unauthorized						
Universal	0.028**	0.033***	0.046*	0.035	0.007	0.062**
	(0.013)	(0.007)	(0.025)	(0.021)	(0.049)	(0.029)
Public Sector	-0.002	-0.008	0.008	-0.023	-0.021	-0.050
	(0.013)	(0.019)	(0.040)	(0.021)	(0.026)	(0.044)
N	277,369	189,596	87,773	386,406	212,088	174,318
Naturalized Hispanic						
Universal	0.039	0.119*	-0.041	0.009	0.006	-0.024
	(0.046)	(0.063)	(0.037)	(0.046)	(0.084)	(0.020)
Public Sector	-0.022	0.001	-0.059	-0.076	0.008	-0.175**
	(0.030)	(0.020)	(0.066)	(0.045)	(0.050)	(0.086)
N	184,575	98,638	85,937	269,637	124,911	144,726
US-Born non-Hispanic						
Universal	-0.010***	-0.017***	-0.003	-0.003	-0.004	-0.005
	(0.001)	(0.001)	(0.003)	(0.006)	(0.006)	(0.005)
Public Sector	0.000	0.008***	-0.009	0.004	0.008	0.002
	(0.003)	(0.003)	(0.005)	(0.003)	(0.006)	(0.002)
N	8,708,946	4,492,400	4,216,546	13,562,270	6,461,583	7,100,687

Controls include gender (when applicable), race, age, marital status, number of children in household, educational attainment, industry fixed effects, state fixed effects, time (year, month) fixed effects, state specific time trends, unemployment rates. Standard errors clustered at the state level. All regressions use survey weights (wtfinl).

* p<0.1, ** p<0.05, *** p<0.01

Table 9: Estimates of the Impact of E-Verify Mandates on Probability of Employment - March 2002 - March 2014 - Basic Monthly CPS

	In Labor Force Population			Full CPS		
	All	Male	Female	All	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Likely Unauthorized Proxy						
Universal	-0.038***	0.081***	-0.293***	0.034***	0.014	0.135***
	(0.013)	(0.010)	(0.015)	(0.010)	(0.011)	(0.008)
Public Sector	0.042***	0.036***	0.003	-0.022***	-0.357***	0.305***
	(0.011)	(0.011)	(0.024)	(0.005)	(0.010)	(0.009)
N	22,735	15,545	7,190	31,927	17,549	14,378
Naturalized Hispanic						
Universal	0.035**	0.007	0.048	-0.017	-0.247***	0.098**
	(0.014)	(0.016)	(0.032)	(0.018)	(0.016)	(0.039)
Public Sector	-0.129***	-0.136***	-0.141***	0.164***	-0.386***	0.712***
	(0.012)	(0.011)	(0.024)	(0.013)	(0.026)	(0.014)
N	14,890	8,002	6,888	21,975	10,138	11,837
US-Born non-Hispanic						
Universal	-0.009***	-0.024***	0.008***	0.045***	0.013***	0.071***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.002)	(0.001)
Public Sector	-0.018***	-0.045***	0.013***	0.035***	0.012***	0.053***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
N	718,732	369,907	348,825	1,121,501	533,760	587,741

Controls include gender (when applicable), race, age, marital status, number of children in household, educational attainment, industry fixed effects, state fixed effects, time (year, month) fixed effects, state specific time trends, unemployment rates. Standard errors clustered at the state level. All regressions use survey weights (wtfinl).
* p<0.1, ** p<0.05, *** p<0.01