Employment Eligibility Verification Requirements and Local Labor Market Outcomes

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Abstract

In the late 1990s, the U.S. federal government developed E-Verify, a web-based employment eligibility verification system intended to limit employment opportunities for work-ineligible immigrants. We estimate the impact of state-level private-sector E-Verify mandates on local labor market outcomes. We document declines in formal sector employment and employment turnover after mandate passage, with effects concentrated among those most likely to be work-ineligible. Using newly available data, we show that larger firms are far more likely to comply with mandates. We exploit within-state variation in adherence and in mandated usage to identify employment spillovers from larger to smaller firms, as well as a reduction in the number of large firms. We find no evidence that work-ineligible populations relocate or that native-born workers' labor market outcomes improve in response to mandates.

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1 Introduction

As global migration flows rose over recent decades, United States federal immigration policy focused resources on strengthening border security and raising the costs of entering into the U.S. illegally. U.S. Border Patrol spending correspondingly rose almost ten-fold over the past two decades, to $4.3 billion in Fiscal Year 2018. In spite of this unidimensional focus of federal immigration policy, states have selectively adopted policies designed to make undocumented immigration less attractive to potential migrants by reducing access to public benefits, by increasing cooperation between local/state law enforcement and federal immigration authorities, and by strengthening employment eligibility verification systems. The adoption of employment eligibility verification systems, in particular, has the potential to dramatically reshape the immigration landscape by eliminating undocumented immigrants' access to formal sector labor markets and the associated earnings gains that have motivated past waves of migration to the U.S.

The primary system used to verify immigrants' work eligibility is E-Verify, a largely voluntary electronic verification system developed by the U.S. Immigration and Naturalization Service (INS) in 1997.[1] The E-Verify system allows employers to rapidly identify work-ineligible immigrants by matching information submitted on the Employment Eligibility Verification Form I-9 to Social Security Administration and Department of Homeland Security records. Since 2006, partial or comprehensive mandates have been adopted by twenty-two states that require the E-Verify system be used to verify employment eligibility of new hires.

In this paper we study how the passage and enforcement of state-level, private sector E-Verify mandates have affected local labor market outcomes for subpopulations with varying rates of likely employment ineligibility and for native-born workers, the intended

[1] The INS was abolished in 2003 and replaced by the U.S. Citizenship and Immigration Services (USCIS), Immigration and Customs Enforcement (ICE), and Customs and Border Patrol (CBP) offices in the Department of Homeland Security.

beneficiaries of these policies. The welfare implications of these state-level policies are ambiguous. The substitutability of natives and work-eligible immigrants for undocumented workers will determine whether these subgroups benefit from falling undocumented labor supply in formal sector markets, while constraints placed on the hiring of undocumented workers will raise the costs that firms face. Understanding the complex impacts of expanded E-Verify usage is particularly relevant at present. Recent comprehensive immigration reform proposals, such as legislation passed by the U.S. Senate in 2013, and the White House's FY 2019 Budget Message (OMB, 2018) have called for a federal private-sector E-Verify mandate.

To analyze the impacts of E-Verify mandates, we first employ Quarterly Workforce Indicators (QWI) and American Community Survey (ACS) data in a county-level difference-in-differences research design. We show that the effects of E-Verify mandates on employment amplify over time. When we subset workers by likely employment eligibility, we find that employment declines among Hispanic and likely work-ineligible subpopulations are notably larger than those found in prior work (Orrenius and Zavodny, 2015, 2016; Orrenius et al., 2018; Amuedo-Dorantes and Bansak, 2014). This divergence is explained by the inclusion of geography-specific linear time trends in past work that attenuate estimated impacts in the presence of treatment effects that increase with time since mandate passage. To probe the robustness of our findings in the presence of dynamic treatment effects, we residualize outcomes on treatment group-specific linear trends estimated using only pre-period data (Goodman-Bacon, 2019).

We next document large declines in Hispanic worker turnover (hires and separations) that parallel employment losses. This type of "job lock" is driven by the fact that E-Verify mandates apply only to newly-hired workers and represents a notable labor market distortion induced by E-Verify mandates. Our work finds no evidence that native-born workers benefit from E-Verify mandates and some evidence they are harmed by them.

In particular, we estimate marginally significant employment declines among native-born workers who are the most substitutable for undocumented immigrants, such as young, male workers without college degrees. We also find suggestive evidence of corresponding declines in labor market earnings for these workers.

We complement this analysis with an investigation of novel administrative data from the Department of Homeland Security on usage of the E-Verify system. We use this data to estimate the effect of E-Verify mandates on usage and document a high degree of non-compliance. Specifically, we show that E-Verify usage is quite low among firms with fewer than 20 employees and their usage is largely unaffected by passage of a mandate. We correspondingly demonstrate that Hispanic employment losses are concentrated in large firms and, using County Business Patterns data, we show that the number of large firms declines significantly in response to the passage of E-Verify mandates. The disproportionate decline in large firm employment represents an unintended consequence of E-Verify mandates and suggests that the costs imposed on firms that do comply with these mandates may be substantial. To the best of our knowledge, ours is the first study to document the disproportionate costs that E-Verify mandates impose on larger firms.

The heterogeneous impacts between large and small firms motivate a second research design that is used to investigate within-state and within-county employment spillovers. Since some E-Verify mandates exclude smaller firms, and even when covered smaller firms have a lower compliance rate with mandates, counties that have a larger share of employment in small firms will be impacted less by statewide mandates. We thus use baseline variation across counties in the firm size distribution to construct a measure of effective E-Verify coverage in the presence of a mandate. Conditioning on state-by-quarter fixed effects, we identify important spillover effects that reflect the movement of workers from jobs in high-compliance to low-compliance counties and from jobs in larger to smaller firms. Importantly, these models rely on a distinct source of variation in E-Verify

coverage than the traditional variation across states and time exploited in our and others' earlier analyses and so also demonstrate robustness to a variety of potential identification concerns.

Finally, we use ACS data to show that the size of the potentially undocumented population does not change in response to passage of E-Verify mandates, with the exception of Arizona, which experienced a population decline following passage of an E-Verify mandate and other measures designed to deter illegal immigration. The divergence between our findings and the evidence from past work that E-Verify mandates lead to undocumented population declines more broadly (see, for instance, Orrenius and Zavodny, 2016) appears to be explained by our focus on the timing of mandate passage rather than subsequent enforcement and by the inclusion of geography-specific linear time trends in past work. We provide evidence that increases in supplementary household income sources may explain the lack of any significant estimated impact on the mobility of the work-ineligible subpopulation.

Immigration policy is currently among the most hotly debated political issues. A vast academic literature has sought to understand how immigration, both legal and undocumented, impacts American firms and the economic fortunes of the native-born.[2] While evaluating the efficacy of E-Verify is important for understanding the limits of policy, an improved understanding of the impact of E-Verify helps deepen our understanding of the ultimate gains or losses from immigration.[3] Moreover, our work contributes to a greater understanding of the role of state and local policies, including cooperation agreements with federal authorities, in influencing labor market and immigration outcomes.[4]

[2] This literature is recently reviewed and discussed in Lewis and Peri (2015) and Dustmann et al. (2016a). Other recent examples are Chassamboulli and Peri (2015), Dustmann and Glitz (2015), Dustmann et al. (2016b), and Clemens et al. (2018).

[3] Our work also contributes to understanding of the role of legal status in immigrant outcomes because the increased use of E-Verify may have the effect of creating much sharper distinctions in the labor market outcomes of immigrants with different legal statuses. See, for instance, Borjas and Cassidy (2019).

[4] For example, other recent work studies the impacts of the Secure Communities Act (East et al.,

2 E-Verify Background, Mandates, and Usage

The 1952 Immigration and Nationality Act officially made employers responsible for ensuring that their employees are legally eligible to work in the United States, but enforcement of this requirement remained limited over subsequent decades. Beginning in 1986 the eligibility verification process was streamlined and strengthened through a requirement that all newly hired employees fill out Employment Eligibility Verification Form I-9. This form requires new employees to submit documentation of their identify and their authorization to work in the United States, for example through a combination of a passport, Permanent Resident Card, or other approved documents. Federal law requires that employers maintain I-9 forms, but does not mandate that the employer verify the authenticity of the information or documents provided. Concerns arose in subsequent years regarding the accuracy and timeliness of verification of employee eligibility based on I-9 form submissions (Orrenius and Zavodny, 2015, Meissner and Rosenblum, 2009).

In 1997 an electronic verification system was developed by the U.S. Immigration and Naturalization Service (INS) to improve the efficiency of the employee verification process. The E-Verify program provides employers with access to an electronic database that allows for rapid verification of work eligibility. There is no federal mandate to use the E-Verify system to verify the accuracy of information on the I-9 form. Rather, federal legislation requires only that E-Verify be used for all employees in a given firm or else not be used at all by the firm.[5] While there are no monetary costs to firms to use the E-Verify system, there are non-trivial set-up, training, and compliance costs to using the system. These costs are particularly cumbersome for small firms, which a 2011 analysis suggested would spend $2.6 billion on compliance-related costs if forced to utilize E-Verify (Arvelo, 2011). Firms that use E-Verify turn over employment data to the Department

2019; East and Velasquez, 2019) and the 287(g) program (Bohn and Santillano, 2017).

[5] Beginning in 2009, the Federal Acquisitions Regulation requires federal contractors, with some exceptions, to use E-Verify for all new employees.

of Homeland Security for statistical analysis, which employers may worry could trigger audits or immigration enforcement raids.[6]

In 2006, Colorado, Georgia, and North Carolina became the first states to enact mandates that require E-Verify usage for particular types of new hires.[7] Currently 22 states have enacted some type of E-Verify mandate. E-Verify requirements vary significantly across states, ranging from requirements imposed in nine states that E-Verify be used by all or nearly all employers, to less comprehensive E-Verify requirements covering only state agencies and state contractors/subcontractors. Table 1 lists all state-level E-Verify laws. Note that many mandates were phased-in over several years, with larger firms covered initially and smaller firms covered in later years.[8] Penalties for non-compliance vary across states from modest fines to suspension of a business license. In our benchmark analyses, we consider states with mandates covering only state agencies and state contractors/subcontractors as untreated as we find no evidence that these mandates impact local labor market outcomes in the absence of more comprehensive private sector mandates. Nonetheless, we show that findings are robust to controlling separately for the presence of state agency and state contractor/subcontractor mandates.

A unique contribution of our work is in providing the first assessment of the effect of state E-Verify mandates on usage of the system. We obtained administrative records from the USCIS via a Freedom of Information Act request that include counts of enrollment by firms in the E-Verify system, counts of total E-Verify queries, and counts of queries deemed work ineligible, separately by county, detailed industry, firm size, and year-quarter from 2004 to 2016. These data are an important part of our research design because they allow

[6] For example, see https://www.shrm.org/resourcesandtools/hr-topics/talent-acquisition/pages/pros-and-cons-registering-for-everify.aspx.

[7] Data on state E-Verify laws comes from the National Conference of State Legislatures (2015) and individual state statutes.

[8] Several counties in California enacted E-Verify mandates. These were overturned by subsequent state law that prohibited lower levels of government from enacting such mandates. Illinois also prohibits lower levels of government from enacting E-Verify mandates. We are not aware of any other sub-state E-Verify mandates.

us to assess how common E-Verify usage was prior to a mandate's passage and to evaluate the change in usage associated with mandate passage as well as enforcement. In addition, these data are used to evaluate heterogeneity in adherence to state-level mandates as a function of firm size.

New hires (the population subject to E-Verify mandates) are measured in the Quarterly Workforce Indicators (QWI) data. The QWI contain aggregate data on employment, hires, separations, and other labor market measures by geographic area, industry, firm size, and a limited number of worker demographic characteristics from 2004 through the second quarter of 2015.[9] The QWI is created by the United States Census Bureau from matched employer-employee data that is itself created from state and federal administrative records and surveys. Much of the information on employment and hires comes from state Unemployment Insurance (UI) records, which cover 96 percent of civilian wage and salary jobs.[10] The measure of hires that we use includes all people who had earnings from an employer in a particular quarter but did not have earnings from that employer in the previous quarter. Our measure of separations is similarly defined to include all individuals with earnings from an employer in a particular quarter but not in the subsequent quarter. We identify employment changes using the available end-of-quarter employment measure that characterizes employment on the last day of the given quarter.

Figure 1 shows the ratio of E-Verify queries to new hires from 2004 to 2015. E-Verify usage was quite low prior to 2006 and began to rise after the relaunch of the web interface with enhanced features (including photo matching for individuals who have a Permanent Resident Card or Employment Authorization Document), and public outreach in 2007.[11]

[9] Although our findings are largely unchanged if we extend the sample to include earlier years, 2004 is the first year in which all states that subsequently pass mandates are included. QWI files with non-missing employment, hires, and separations records were available through the second quarter of 2015 when accessed.

[10] Detailed information about the QWI data is available at https://lehd.ces.census.gov/data.

[11] A summary of the history of the E-Verify program is given at https://www.uscis.gov/e-verify/about-program/history-and-milestones.

In 2006, 1.4 percent of hires were queried. The ratio rose to 22 percent in 2010 and 32 percent in 2015.[12] 2008 was the first year that any private sector hires, other than state contractors, were subject to an E-Verify mandate. Figure 1 also shows the fraction of private-sector hires that were subject to an E-Verify mandate. We estimated this coverage rate by applying applicable state laws based on firm size.[13] The coverage rate rises from zero in 2007 to 15 percent in 2015.

Figure 2 shows the ratio of E-Verify queries to hires separately by firm size. E-Verify usage is quite uncommon among firms with fewer than 20 employees, where about six percent of hires were queried in 2015. By contrast, over 40 percent of hires in firms with 20 or more employees were queried in 2015. This disparity is not because of state mandates that exclude small firms since the majority of states with private sector mandates eventually covered all firms.[14] Rather, the disparity is likely caused, in part, by the fact that some portion of the set-up and compliance costs are fixed and therefore higher on a per-hire basis for small firms. Some of the disparity is also likely due to larger firms being more likely to be federal or state contractors and therefore subject to a mandate. In Section 4, we demonstrate that mandate passage sharply increases E-Verify usage by larger firms while smaller firms experience a more marginal increase in usage.

A small existing literature has investigated labor market impacts of E-Verify mandates.[15] This past work has generally identified state-level employment declines among

[12] The E-Verify queries data in Figure 1 includes queries by both public and private-sector entities, while our extract of the QWI data covers only the private sector. Thus the ratio of queries to hires overstates the fraction of private sector hires that are queried.

[13] The data on hires in the QWI is grouped into firm size bins that do not always coincide with the E-Verify mandate thresholds, which induces some measurement error in our coverage rate. For QWI firm size bins that include both firms that are and are not subject to a mandate, we impute coverage for this descriptive exercise by assuming a uniform firm size distribution within each bin. Our measure of coverage does not take into account any others exclusions to a law.

[14] The exceptions are Tennessee, Georgia, Utah, and North Carolina which exclude firms with fewer than six, fewer than 11, fewer than 15, and fewer than 25 employees. In each of these states except for North Carolina, some fraction of firms with under 20 employees would still be subject to mandates.

[15] Other recent work has turned to investigating downstream outcomes, including foreign direct investment responses, educational enrollment, and health insurance (Amuedo-Dorantes et al., 2015; Gunadi, 2018; Churchill, 2019).

likely work-ineligible subpopulations in response to E-Verify enforcement but is otherwise inconclusive regarding the net labor market impacts of (and costs associated with) E-Verify mandates. The best-known, state-level E-Verify case studies examine the migration and labor market impacts of Arizona's 2007 Legal Arizona Workers Act (LAWA), which mandated statewide E-Verify usage. These studies identify a significant decline in the state population characterized as non-citizen Hispanic in response to LAWA's passage, but find no evidence of improvement in employment outcomes for non-Hispanic low-skilled workers (Bohn et al., 2014, 2015). Moreover, LAWA was passed during a period in which Arizona enacted multiple laws which were widely perceived as "anti-immigrant" (Matthews, 2005; Duara, 2016), suggesting that the undocumented population might have been particularly responsive to the passage of LAWA given the overall state climate. The most comprehensive empirical research on the aggregate labor market impacts of the scale-up of E-Verify usage includes Amuedo-Dorantes and Bansak (2014), Orrenius and Zavodny (2015), and Orrenius et al. (2018). These studies examine the employment and wage effects of E-Verify mandates passed in multiple states and find mixed evidence of whether any benefits accrue to likely work-eligible sub-populations, likely due to differences in the data sources used, among other factors. Orrenius and Zavodny (2016) employs a similar approach to examine changes in state-level likely undocumented populations and finds evidence that E-Verify mandates lead to reductions in this population, driven by declines in the number of recent migrants living in a given state.[16]

[16] Although we replicate this finding when examining undocumented population responses to E-Verify mandate enforcement in specifications with geography-specific linear time trends included, we find no such impact in benchmark specifications that study responses to mandate passage or in those that incorporate treatment group-specific linear trends estimated using only pre-period data.

3 Data sources

We use three complementary data sources on labor market outcomes. Our benchmark specifications employ outcomes constructed using QWI data from 2004 to 2015, which we described in Section 2. These data give accurate measures of aggregate employment, hires, and separations by quarter, county, firm size, industry, and Hispanic ethnicity.[17] These data cover formal sector, wage and salary workers. The data do not cover self-employed workers, independent contractors, or those who work in informal or uncovered jobs. QWI data does not include any information about a worker's eligibility to work in the United States. We analyze these data for Hispanics and non-Hispanics separately. While the population of Hispanic workers includes both natives and immigrants, and the subpopulation of Hispanic immigrants includes both work-eligible and work-ineligible immigrants, we anticipate that changes in employment patterns driven by E-Verify legislation will be most likely to manifest themselves as changes in Hispanic employment patterns given that the share of Hispanic workers who are likely undocumented is substantially higher than the share of non-Hispanic workers without work eligibility, a fact we document below.

We also analyze data from the ACS at both the individual and county level.[18] These data allow us to focus more directly on workers most likely to be undocumented and ineligible to work in the United States, and workers who are potentially most affected by changes in labor market outcomes among undocumented workers. ACS data have a number of advantages. First, they contain variables that allow us to study geographic movement, household-level earnings, self-reported employment status (which may include informal employment), and self-employment, which are not available in the QWI. Rich demographic data allow us to focus on treatment effects among more narrow classifications

[17] QWI data is available for both public and private sector employment. We only analyze data on private sector employment.

[18] We first present specifications at the county level to parallel QWI regressions. We turn to individual-level specifications when investigating mechanisms that may help to explain our findings.

of individuals, including low-skilled, native-born individuals. However, the ACS does not contain information on the legal status of foreign-born persons and so we follow an existing literature and impute that a respondent is undocumented if that person is a non-citizen, not currently serving in the Armed Forces or a veteran, has not completed high school, arrived to the US in or after 1980, was not born in Cuba and is not a public sector employee.[19] Averaged over our sample period, 48.6 percent of Hispanics are foreign-born and 35.2 percent of these are likely undocumented. In contrast, 10.3 percent of non-Hispanics are foreign-born and 5.8 percent of these are likely undocumented.

Two important drawbacks of the ACS are, first, that it is a sample and thus provides a noisier measure of employment; second, geographic coverage is more limited than in the QWI. Individuals in the ACS are classified by their Public Use Microdata Area (PUMA), which are areas created by the Census Bureau that contain at least 100,000 people. To construct county-level estimates, we thus employ a cross-walk that maps PUMAs into each of the 3,142 counties (or county-equivalents).[20] Finally, ACS data is annual, rather than quarterly.

We also study changes in the number of establishments in operation using County Business Patterns (CBP) data, which are derived from the Business Register data collected by the U.S. Census Bureau. These data provide the number of establishments in operation at the county-by-firm size bin-by year level and represent the most comprehensive existing data source for establishment-level records (United States Census Bureau, 2019). Data are available for the first quarter of each year between 2004 and 2015.

[19] This definition combines criteria employed in Feigenberg (2020), Orrenius and Zavodny (2016) and Borjas and Cassidy (2019).

[20] PUMAs are only identified in the ACS beginning in 2005 so our county-level estimates cover the period 2005-2015. We extend the sample back to 2000 when estimating individual-level regressions in order to more credibly identify pre-period differences in outcomes as a function of future mandate passage.

4 Research Design and Empirical Findings

We now describe our empirical framework to identify changes in E-Verify usage in response to the enactment of legislation mandating its use and to examine resultant changes in labor market outcomes for exposed workers as a function of their likely employment eligibility. The ideal experiment to identify E-Verify program impacts would require the random assignment of E-Verify legislation passage and enforcement across place and time. Absent random variation in the passage and enforcement of E-Verify legislation, a key identification challenge is that, even in the absence of an E-Verify mandate, counties in states that pass and enforce E-Verify legislation may have subsequently experienced changes in labor market and immigration outcomes that differed from those in counties in states that did not pass such legislation. To identify the causal impacts of E-Verify legislation in the presence of potentially endogenous passage, we begin with event-study models that document that there are no pre-trends in E-Verify usage or in QWI-based Hispanic labor market outcomes prior to passage of E-Verify legislation. (A comprehensive set of event studies for all examined outcomes is included in the Appendix.) We then employ two complementary identification strategies to measure the effect of legislation on outcomes following passage and enforcement of employment verification mandates: The first approach uses variation across states and time in E-Verify mandates to identify the causal effect of mandates on average labor market outcomes in a difference-in-differences framework. The second approach uses data disaggregated to the firm size level to exploit within-state variation in the predicted coverage of and adherence to E-Verify mandates and to investigate within-state spillovers.

4.1 Event study models

We begin by presenting event study graphs that characterize differences in E-Verify query rates and in QWI-based measures of employment, hires, and separations among Hispanics in the years before and after passage of any private sector E-Verify legislation in a given state. Although the variation in mandate passage that we initially exploit is at the state level, we present county-level estimates throughout to facilitate comparisons with our subsequent within-state analyses and to improve precision. Our primary goal here is to assess whether there are differential trends in outcomes prior to passage of an E-Verify mandate. To do this, we estimate regression models with the following form:

$$Y_{cst} = \alpha + \sum_{y=-4}^{4} \beta_y Everify_{csty} + \gamma_t + \lambda_c + \epsilon_{cst} \quad (1)$$

where Y_{cst} is the ratio of queries in the E-Verify system to new hires, or the inverse hyperbolic sine (asinh) transformation of counts of employment, hires, or separations, in county c in state s in year-quarter t.[21] γ_t and λ_c represent year-quarter and county fixed effects. Finally, $Everify_{csty}$ is defined as an indicator variable that identifies whether E-Verify legislation covering any private sector workers (regardless of firm size) was passed in county c in state s in y years after year-quarter t (or $|y|$ years before for negative-valued y).

We focus here on the effects of passage, rather than of the effective date, of any private sector E-Verify mandate. Since there is a lag of one year, on average, between passage and enforcement, even if the conditional exogeneity assumption is satisfied with regards to the passage of E-Verify legislation, labor market responses to initial passage have the potential

[21] The asinh function closely parallels the natural logarithm function, but is well defined at zero (Card and Dellavigna, 2019). Only a small number of cells in county-level specifications have zero values for employment, hires, or separations and results are essentially identical using logs. When we disaggregate the data by firm size, however, zero values are more common. We show, in any case, that our county-level results are similar when we use ratios of outcomes to baseline employment

to bias estimates derived from models that focus on dates of enforcement. In practice, we find modest changes in outcomes after passage but prior to enforcement; overall, however, most estimates are insensitive to the definition of treatment timing employed.

Figure 3 plots regression coefficient estimates from Equation 1 and demonstrates that E-Verify mandate passage sharply increases E-Verify usage by firms. Panel A shows effects on the ratio of E-Verify queries to hires, by year relative to the date a mandate was passed. This ratio increases by 22 percentage points from four years prior to the mandate to four years after it, with a 15 percentage point jump during the first full year after the law was passed. Panels B through C show estimated effects on the ratio of queries to hires from models estimated separately for large and small firms. The ratio of queries to hires in firms with fewer than 20 employees rises by 10 percentage points, with a three percentage point increase in the first full year after the law was passed. We find a similarly small responsiveness to mandates that explicitly cover all private sector firms. By contrast, larger firms are far more likely to use E-Verify and their usage pattern shows a noticeable increase after E-Verify mandates are passed. Firms with 20 or more employees have a 23 percentage point increase in the first full year after the law was passed.

Figure 4 presents estimates from Equation 1 for outcomes characterizing Hispanic end-of-quarter employment, separations, and hires. We find no evidence of statistically significant pre-trends in any of the outcomes. All three labor market outcomes decline in the year after E-Verify passage and the effect sizes tend to grow over the subsequent years. Importantly, these figures provide support for the identifying assumption that the declines in Hispanic employment, hires and separations after E-Verify passage that we will document cannot be attributed to differential pre-trends that would have predicted diverging outcomes even in the absence of E-Verify legislation. The evidence that treatment effects amplify over time also motivates our careful assessment of the proper approach to controlling for group-specific linear trends in the subsequent analysis.

In Appendix Figures A1-A9, we present estimates from parallel event study models for all of the dependent variables that we consider below, in the QWI, ACS, and CBP samples. In specifications examining outcomes for Hispanics and likely work-ineligible individuals, we find little evidence of pre-trends in outcomes. As discussed in more detail below, we do, however, find evidence of differential pre-trends when examining outcomes for non-Hispanics in certain specifications. Across analyses, we assess the robustness of our results to specifications that residualize outcomes on treatment group-specific linear trends constructed using only pre-period data (Goodman-Bacon, 2019).

4.2 E-Verify mandates, query rates, and employment outcomes

We next estimate changes in E-Verify query rates and labor market outcomes associated with the passage and implementation of E-Verify legislation. Since E-Verify mandates apply only to newly-hired workers, we expect that there could be "job lock" based on immigration status among those who would be forced to verify employment eligibility if they switch employers. If true, this would lead to a decline in job separations among work-ineligible individuals after E-verify mandates are passed. A reduction in separations could contribute to a concurrent reduction in hires among work-ineligible individuals. By contrast, whether we observe an immediate decline in employment is theoretically uncertain; to the extent that work-ineligible workers forgo job transitions and/or job search, we may see limited aggregate changes in employment even in the presence of significant declines in hires and separations.

We begin our analysis with estimates of the effect of E-Verify legislation on the fraction of new hires that are queried though the E-Verify system. Our first research design builds on the existing literature and exploits state by year-quarter variation in E-Verify mandate passage and enforcement in a multi-state difference-in-differences estimation framework. Though we study mandates that are passed at the state level, we estimate

models at the county level to parallel our subsequent within-state analyses. By doing so, we also more flexibly account for within-state differences across local labor markets and consequently generate more accurate treatment effect estimates. The benchmark estimated specifications are of the following form:

$$Y_{cst} = \alpha + \beta_1 Everify_{cst} + \gamma_t + \lambda_c + \epsilon_{cst} \tag{2}$$

The included regressors are as defined in Equation 1, with the exception of $Everify_{cst}$, an indicator variable equal to one if E-Verify legislation that covers **any** private sector workers has been passed in county c state s by year-quarter t. Standard errors are clustered at the state level.

Results are presented in Table 2. Column 1 of Panel A indicates that passage of any private-sector E-Verify mandate is associated with a 14.8 percentage point increase in the fraction of hires queried in the system. In subsequent columns, we assess robustness along alternative dimensions. First, we verify that our estimates are not sensitive to the inclusion of county- and state-level covariates characterizing predicted labor market performance and the set of additional immigration enforcement measures already in place in county c in state s in year-quarter t. Specifically, we include indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants, as well as an indicator that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter. We also control for county-level predicted labor demand (i.e., a "Bartik instrument"), as well as interactions between year-quarter and the baseline (2004) state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio.[22]

[22] An indicator for whether a state has legislation in place to facilitate information-sharing with fed-

To account for the presence of potential non-parallel pre-trends for some outcomes, we also estimate models in which we residualize the dependent variable on treatment group-specific linear trends estimated using only pre-period data. In the presence of treatment effects that amplify over time, simply including linear time trends in the regression would conflate pre-trends with dynamic treatment effects and attenuate our estimates (Wolfers, 2006). Instead, we define treatment groups by the date of passage of the E-Verify mandate (there are six unique year-quarters of passage in our sample). We then regress the outcome variable Y_{cst} on county fixed effects, year-quarter fixed effects, group-specific linear time trends, and (in some models) covariates using only data from each group's pre-period. This provides estimates of group-specific time trends that are produced only from pre-period data. We then estimate Equation 2 with a dependent variable formed as the difference between the actual outcome and that predicted from the group-specific linear trend.[23] In practice, as can be seen in Columns 3-4 of Panel A of Table 2, estimated impacts are nearly identical when these alternative specifications are employed.

In Panel B of Table 2, we estimate specifications in which the treatment variable is

eral law enforcement is constructed by the Urban Institute from U.S. Immigration and Customs Enforcement records (Gelatt et al., 2017). Indicators for whether a state has legislation in place to strengthen protections for undocumented immigrants and for whether a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed are derived from the National Conference of State Legislatures (2015) and individual state statutes. We construct the "Bartik (1991) instrument" using the QWI data as the predicted change in employment in a county based on the county industry composition in 2004-2006 and national changes in employment across industries in each quarter. The state-level household debt-to-income ratio is constructed from the New York Fed Consumer Credit Panel (Federal Reserve Bank of New York, 2019). The remaining covariates are constructed based on data files from the U.S. Department of Commerce and from the Federal Reserve Bank of St. Louis online portal (Federal Reserve, 2020).

[23] In our context, Goodman-Bacon (2019) recommends including only year-quarters prior to the first mandate passage in any state when estimating these linear time trends. We include all untreated county by year-quarter cells to maximize the length of the pre-period and since the rollout of mandates is gradual (fewer than 0.5% of counties are exposed to mandates in 2007, the first year in which a mandate is passed). A decomposition indicates that 96% of our difference-in-differences estimates are derived from comparisons between counties in states that pass mandates and counties in states that never pass mandates. Since a substantial majority of counties are located in states that never pass mandates, the presence of spillover effects that could bias linear pre-trend estimates is likely to be limited. In practice, constructing linear time trends using only year-quarters prior to the first mandate passage in any state tends to increase coefficient magnitudes and reduce precision, but does not substantively change our findings.

an indicator that a mandate covering any private sector workers has gone into force in county c state s by year-quarter t. For comparison, we also present estimates in Panel C that jointly include indicators for both whether a mandate has been passed and whether it has gone into force. In sum, we find that estimates are similar (though slightly larger) when treatment is defined by enforcement rather than passage and we see that over 80% of the effect of E-Verify mandates on system usage loads onto enforcement of the E-Verify mandate. This latter finding suggests that any anticipatory labor market responses that occur after passage and prior to enforcement are not likely attributable to increased system usage.

We next turn to analyzing the labor market effects on Hispanic individuals measured in the QWI files. These are presented in Table 3. We employ two alternative transformations of relevant QWI outcome measures. First, Columns 1 through 4 present estimates of Equation 2 that use the inverse hyperbolic sine transformations of the dependent variables, which like the log transformation, approximates a pre-post percentage change in outcomes. Second, Columns 5 through 8 present estimates in which we construct employment, hires, and separations rates that divide contemporaneous outcomes by baseline (2003) county-level Hispanic employment.[24] We include this alternative transformation as a robustness check and to facilitate comparisons across employment, separations, and hires estimates. In our benchmark specifications presented in Column 1, we find a statistically significant 9.4 percent decline in Hispanic employment, a 13.5 percent decline in separations, and a 14.3 percent decline in hires. Column 2 indicates that results are similar when we include a rich battery of control variables. Columns 3 and 4 present estimates based on the previously-described residualization procedure (without and with controls included). Given the absence of any differential pre-trends for these outcomes, point estimates are expected to remain unchanged in the absence of sampling variation.

[24] We use 2004 as the baseline for Arizona as 2003 QWI data are unavailable.

In practice, estimates without controls (in Column 3) decline marginally and estimates with controls (in Column 4) increase marginally relative to the estimates presented in Columns 1 and 2. Nonetheless, the basic pattern of findings is unchanged.

We reach similar conclusions based on the estimates in Columns 5 through 8 that use rate-based dependent variables. In Panel A, our baseline specification indicates that passage of a mandate is associated with a decline in employment of 0.12 people per Hispanic person employed in 2003. The baseline coefficient estimates in Panels B and C indicate that hires and separations fall by about 0.06 per person employed in 2003, or about 13 percent of the mean number of transitions per worker.

In Appendix Table A1, we present estimates from specifications where the indicator for mandate passage is replaced with an indicator for whether a mandate has gone into effect and where both treatment indicators are included jointly. Though generally imprecise, estimates provide evidence of modest anticipatory labor market responses after mandate passage and prior to enforcement.

Overall, the employment and turnover declines we estimate among Hispanic workers are notably larger than those found in prior work (Orrenius et al., 2018). Most of this difference stems from the use in prior work of linear time trends to control for differential pre-trends, which attenuates estimates in the presence of effects sizes that grow over time. Our method of using only pre-period data to control for pre-trends avoids this. To give a sense of the quantitative importance of this distinction, we estimate a 9.4% Hispanic employment decline in our benchmark specification (Table 3, Panel A, Column 1). If we add county or treatment group linear time trends to this specification, the estimate falls to 3.7% and is no longer significant at conventional confidence levels.[25]

[25] Though our focus on date of mandate passage rather than enforcement could be expected to contribute to these divergent findings, in practice this is not the case. Effect sizes that grow over time mean that average post-event outcome values are higher in passage-based than enforcement-based models, while declining outcome values between passage and enforcement imply that pre-event outcomes are also higher in passage-based than in enforcement-based models. These pre- versus post-event differences across models appear similar in magnitude and so effectively cancel out.

In Table 4 and in Appendix Table A2, we present estimates analogous to those in Table 3 and in Appendix Table A1 but for non-Hispanic workers. Non-Hispanic workers could be affected in a number of ways. We estimate that 0.6 percent of non-Hispanics are likely undocumented and so their labor market outcomes could be negatively affected by the enactment of E-Verify mandates. The employment available to work-eligible individuals could increase or decrease, depending on whether they are substitutes or complements to individuals who are not eligible to work in the United States. Furthermore, if work-ineligible individuals experience "job lock," mobility for those who are work-eligible may also be depressed as a result, leading to declines in separations and hires above and beyond any measured employment effects. We focus on Columns 3-4 and 7-8 because labor market outcomes were declining for non-Hispanics prior to mandate passage (see Appendix Figure A1). The estimates in Table 4 are negative though much smaller than the effects on Hispanics and not significant at conventional levels. Importantly, given the low share of non-Hispanics likely to be work-ineligible, we can rule out employment gains greater than 2.3 percent among work-eligible, non-Hispanics in response to the passage of E-Verify mandates based on our most saturated model.

We next turn to our analysis of labor market effects measured in the ACS, which allows us to identify average treatment effects for individuals who are likely to be undocumented based on additional observable characteristics, as well as effects on subgroups of native-born individuals. We estimate models similar to Equation 2 using the inverse hyperbolic since transformation of the dependent variables, though the ACS data are annual and the only policy variable that we include is a dichotomous treatment variable indicating whether any private-sector E-Verify mandate has been passed by the end of the prior year. Panel A of Table 5 presents results from our baseline specification and Panel B presents results from models that add a pared-down set of control variables.[26] We first present

[26] Given the coarseness of the annual data, we exclude the control for whether a public sector or contractor/subcontractor E-Verify mandate has been passed since this control introduces substantial

employment effects of E-Verify mandates by Hispanic ethnicity and undocumented status. Here employment excludes self-employment since self-employed individuals are not subject to E-Verify mandates. We examine changes in self-employment patterns separately in the subsequent analysis. Columns 1 and 2 show mandates are associated with a 13.7 percent decline in employment among Hispanics and no effect among non-Hispanics, which mirror our results from the QWI. Columns three through five show that the policy impacts are largest for those we impute to be undocumented. In particular, E-Verify mandates reduce employment by 19.1 percent among likely undocumented Hispanics, by 8.9 percent among likely documented Hispanics, and by 19.8 percent among all likely undocumented individuals (regardless of ethnicity). Roughly 17% of Hispanic workers in the ACS sample are classified as likely undocumented while less than one percent of non-Hispanic workers are classified accordingly, which buttresses our interpretation of the estimates from the QWI that larger (negative) labor market impacts for Hispanic workers are driven by the relatively higher share of work-ineligible individuals within this subpopulation. Panel B of Table 5 shows that estimates are generally insensitive to the inclusion of additional controls.

A purported motivation for restricting employment opportunities among undocumented immigrants is to improve outcomes among the native-born. However, outcomes among the native-born could be helped or harmed, depending on whether they are substitutes or complements with undocumented migrant labor. Their outcomes could also be affected by false positives in the E-Verify system or by employers discriminating against native-born Hispanics. Our estimates in the remaining columns of Panel A of Table 5 indicate that E-Verify mandates, in fact, do not lead to employment gains among native-born workers. The estimate in column six shows a fairly precisely estimated zero effect

imprecision in the annual panel and is not a statistically significant predictor of QWI-based labor market outcomes. We also exclude the "Bartik instrument", which is not predictive of labor market outcomes in the ACS.

among the native-born population as a whole. Subsequent columns suggest that the passage of a private sector E-Verify mandate reduces employment among natives with a high school degree or less education and reduces employment among younger, low-skilled, male natives (aged 16 to 40) in particular. For these subgroups, however, we see some evidence of declining employment prior to mandate passage in Appendix Figure A2.

A central challenge with ACS-based analyses is the coarseness of the annual data and the limited number of pre-period observations. Although imperfect, we assess sensitivity in Appendix Table A3 to the residualization of outcomes based on pre-period treatment group-specific linear trends. We find somewhat larger treatment effects for Hispanics and the likely undocumented, while estimated impacts on subgroups of native workers remain negative but decrease in magnitude and are no longer significant at conventional levels in most instances.

4.3 Firms and heterogeneity in E-Verify coverage and adherence

In this section we extend our analysis to better understand the role of firms. To do so, we employ an alternative identification strategy that organizes the data by county, firm size, and year-quarter. We first examine heterogeneity in E-Verify system usage and in labor market impacts as a function of firm size. We leverage findings from these initial analyses to construct a county-level measure of predicted E-Verify exposure. Using this measure, we can control for unrestricted state-year-quarter fixed effects in our models to assuage any remaining concerns regarding internal validity and to assess the extent of within-state employment spillovers across areas with differing levels of predicted E-Verify coverage. To conduct the initial firm size-level analysis, we estimate models of the form

$$Y_{fcst} = \alpha + \beta Everify_{fcst} + \gamma_t + \gamma_{fc} + \epsilon_{fcst} \tag{3}$$

Here, Y_{fcst} reflects the outcome of interest for firm size bin f in county c in state s in year-quarter t and $Everify_{fcst}$ is an indicator for whether E-Verify legislation that covers *any* firms in firm size bin f has been passed by the end of year-quarter t. γ_t is a year-quarter fixed effect and γ_{fc} is a firm-size bin-by-county fixed effect. Although the raw QWI includes five firm size bins, data is frequently censored or missing for three intermediate bins, corresponding to firms with 20 to 499 employees. Consequently, we divide the sample into two bins: workers in firms with fewer than 20 employees and workers in firms with 20+ employees. Since data are least likely to be missing for the smallest (0-19 employee) bin, this approach allows us to maximize sample coverage by calculating employment in the 20+ employee bin as the difference between total employment and small firm employment.[27]

The estimates corresponding to Equation 3 are presented in the odd-numbered columns of Table 6 and characterize average treatment effects for the Hispanic subpopulation. In the even-numbered columns of Panel A, we present estimates that include year-quarter-by-firm size bin fixed effects to produce treatment effects separately by firm size bin. Column 2 indicates that increased E-Verify usage in response to mandate passage is driven primarily by larger firms. Column 4 correspondingly shows that these same firms drive employment declines. Interestingly, declines in hires and separations are similar in smaller and larger firms, suggesting that even workers in low-adherence small firms may experience "job lock" after the passage of E-Verify mandates, perhaps due to concerns regarding the likelihood that they will find alternative employment within the set of firms that exhibit similarly low adherence to existing E-Verify mandates.[28] Panel B of Table 6

[27] Firm size refers to the national employment size. For hires and separations, firm size refers to the number of employees in the firm that hired an individual or from which an individual separated.

[28] One concern with this approach that subsets workers by firm size is that firm size is itself an endogenous function of E-Verify mandates and employment outcomes. The fact that QWI defines firm size based on the prior year, however, partly alleviates this concern. Moreover, in Appendix Table A4, we show that the number of workers per establishment does not change significantly in response to E-Verify mandate passage in larger firms and is declining in smaller firms. These findings indicate that our estimates in Table 6 are not likely biased by endogenous changes in the firm size

and Appendix Table A5 show that results are robust to the inclusion of additional controls and to the residualization of outcomes on pre-period linear trends, respectively.

Consistent with the lack of an effect of E-Verify mandates on employment outcomes among non-Hispanics, we also find no statistically significant effects when stratified by firms size once we account for the differential pre-trends in non-Hispanic labor market outcomes. These estimates are reported in Appendix Table A6. This is consistent with the hypothesis that differences in adherence to E-Verify mandates between smaller versus larger firms explain the heterogeneous Hispanic employment responses that we identify.[29]

We next further examine the extent to which measured employment changes result from changes in the number of establishments in operation as compared to within-firm intensive margin changes in the number of employees. Increases in the cost of labor or in hiring costs could lead firms to close or relocate to other areas, or may deter firms from entering the market. We explore these effects using County Business Patterns (CBP) data.[30] Table 7 first presents coefficients from specifications that parallel those presented in Table 6, but are estimated at the annual level.[31] In columns one and two, the dependent variable is the total number of establishments in the given establishment size bin. The Column 1 estimate indicates that E-Verify mandate passage is associated with a significant 3.0 percent decline in the number of establishments, while Column 2 reveals that this decline is concentrated in establishments with 20 or more employees. This heterogeneity by establishment size is consistent with the finding from Table 6 that employment declines

bin to which given firms are assigned. One important caveat is that, due to data limitations, we construct Appendix Table A4 estimates based on establishment counts in combination with worker counts at the firm size (rather than establishment size) level. We provide a more extensive discussion of changes in the number of establishments in response to mandate passage below.

[29] Interestingly, we also find no evidence of heterogeneity in employment effects across industries as a function of likely undocumented employment shares.

[30] One limitation of this analysis is that CBP data can be used to analyze establishment rather than firm counts, while QWI and E-Verify queries data are disaggregated by firm size.

[31] CBP data are available for the first quarter of each year from 2004 to 2015 and so we estimate specifications at the annual level and employ an E-Verify passage measure that is an indicator for whether a mandate has been passed by the end of the first quarter in a given year.

are concentrated in larger firms. In column 3, we aggregate the data to the county-year level and identify a small and marginally significant 1.6 percent decline in the total number of establishments. This smaller aggregate effect is explained by the fact that most establishments have fewer than 20 employees and so changes in the number of smaller establishments will drive overall changes in the number of establishments. For comparison, in columns four through six, we replace the dependent variable with a measure of the number of employment-weighted establishments.[32] This specification is designed to better capture the share of jobs lost due to the reduction in the number of establishments in operation. We find a larger 3.6 percent decline in the county-year specification. Though this estimate should be interpreted cautiously given the actual (contemporaneous) underlying distribution of establishment sizes within each bin is not available in the CBP, the point estimate would imply that roughly three-quarters of total job losses are due to the reduced number of establishments in operation.[33]

4.4 E-Verify mandates and employment spillovers

In this subsection we assess the extent to which E-Verify mandates lead to shifts in employment from covered or compliant firms to others. In particular, some E-Verify mandates explicitly exclude small firms. Others phase-in coverage for small firms over time. We

[32] To do so, we first calculate the average establishment size by county and bin at baseline. As an example, a county with two establishments with 1-19 employees in a given year and with an average baseline size of 4 employees would have a weighted establishment value of 8. In contrast, a county that had one establishment with 1-19 employees and one establishment with 20+ employees in a given year and with average baseline sizes of 5 and 80, respectively, would have a weighted establishment value of 85. Since we are using employee counts by firm size to calculate average establishment sizes, this weighting undoubtedly introduces some degree of mismeasurement. We have verified that estimates appear similar if we instead construct weights based on the midpoint of the establishment size range within each bin.

[33] This estimate is based on the finding that passage of E-Verify legislation leads to a 4.7 percent reduction in total employment (combining the Hispanic and non-Hispanic samples) and a corresponding 3.6 percent decline in the number of employment-weighted establishments. We show corresponding residualization-based estimates in Appendix Table A7. Consistent with the visual evidence presented in Appendix Figure A4, estimates are largely unchanged though coefficient magnitudes in unweighted county-by-year specifications increase marginally and coefficient magnitudes in weighted county-by-year specifications decrease marginally.

have also shown that usage of E-Verify at small firms is low and largely unresponsive to mandates. Much of the employment effect of E-Verify mandates is concentrated in large firms. To what extent, therefore, does a state mandate shift employment from larger to smaller firms? This is important because spillovers arguably represent a clear welfare loss and do not advance any of the purported goals of E-Verify proponents.

We begin this analysis in Table 8, in which we leverage within-state variation in effective E-Verify coverage. Our prior analyses focused on changes in outcomes associated with passage of an E-Verify mandate. We now compare these to models that condition on a state by year-quarter fixed effect, which removes the common effect of passage of the mandate. The only remaining variation in E-Verify coverage in these models will be due to differences in the firm size distribution across counties. To the extent E-Verify coverage induces shifts in employment from high coverage to lower coverage areas, estimates in these models will be larger in magnitude than those in corresponding specifications that do not include state by year-quarter fixed effects.

To conduct this analysis, we exploit cross-county variation in the baseline share of employment in large firms in combination with variation in the timing of the passage of mandates covering each firm size bin and in adherence to these mandates. Specifically, we use data from the pre-period (2003) to construct county-specific measures of the share of employment in firms with 20+ employees. We then construct a time-varying county-level coverage measure that captures the share of private sector jobs that would be expected to adhere to E-Verify mandates in each year-quarter based on this baseline firm size distribution. Effective coverage is zero if a given firm size bin is not yet covered by an E-Verify mandate. To measure effective coverage conditional on the passage of a mandate, we exploit variation in adherence, as measured using DHS E-Verify query data in Table 6. Specifically, we scale the effective coverage of small firms by a factor of 0.255 to account for the relatively smaller "first stage" magnitude (characterizing the relationship between

mandate passage and E-Verify query rate) in small firms as compared to large firms. As an example, a county with 50 percent of employment in small firms at baseline has an effective coverage rate of 50 percent in each quarter in which only large firm mandates have been passed and has an effective coverage rate of 62.75 percent (50 percent+50 percent*0.255) in each quarter in which a mandate covers all firm sizes.

Odd-numbered columns in Panel A of Table 8 present estimates that correspond to Equation 2, but replace the prior E-Verify passage measure with this measure of predicted county-level coverage. Variation in coverage in these models is driven by passage of E-Verify mandates and the results closely mirror those presented in Table 3. In the even-numbered columns in Panel A of Table 8, we add state-by-year-quarter fixed effects to the specifications from the corresponding odd-numbered columns. These fixed effects control for the state-wide mandate in place and so variation in coverage is driven by differences in the baseline firm size distribution. Column 2 validates this alternative approach by demonstrating that higher predicted coverage significantly increases E-Verify usage.

Turning to labor market outcomes, in Column 4 we find a 36.5 percent decline in Hispanic employment in response to a 100 percentage point increase in predicted coverage. This point estimate is significantly larger than the benchmark employment decline estimated in Column 3. Without state-by-year-quarter fixed effects, the estimate in Column 3 captures both spillovers and the average pre-post difference in employment that results from the E-Verify mandate. In contrast, Column 4 exploits only variation that is conditional on the set of mandates in place, and so the notably larger estimated treatment effect in this specification is consistent with sizable employment spillovers from local labor markets with higher to lower levels of predicted coverage. This large estimated employment decline also suggests that unobservable, time-varying state-level factors correlated with E-Verify mandate passage cannot explain the measured Hispanic employment declines presented previously. Turning to job turnover measures, the specifications in columns 6

and 8 provide little evidence of spillovers on the separations or hires margins, consistent with the finding that declines in separations and hires appear fairly uniform across the firm size distribution. To confirm robustness, in Panel B we also present results based on a coverage measure that uses only variation across firm sizes in the timing of mandate enforcement and ignores variation in adherence. Across specifications, estimated patterns of labor market effects appear qualitatively similar.[34]

To provide additional evidence on the extent of sub-state employment spillovers, Table 9 estimates employment changes in small firms for Hispanic and non-Hispanic workers as a function of the same county-level predicted coverage measure included in Table 8 specifications. Column 1 of Panel A demonstrates limited (and insignificant) Hispanic employment declines in small firms in response to higher county-level coverage rates. Column 2 of Panel A restricts the sample to county-year-quarter cells in which small firms are not yet subject to E-Verify mandate enforcement and results appear similar. Column 3 of Panel A adds state-by-year-quarter fixed effects and shows that higher coverage is associated with a large and precisely-estimated 36.6 percent increase in Hispanic employment in small firms.[35] This relative increase in small firm employment in response to higher county-level coverage, in a specification which differences out any common deterrent effect associated with state-level mandate passage, is consistent with the presence of within-county spillovers as employment moves from larger (high-adherence) to smaller (uncovered or low-adherence) firms.

[34] For completeness, Appendix Table A8 presents parallel results for the non-Hispanic population; here, we find little evidence of comparable within-state employment spillovers for non-Hispanic workers.

[35] The corresponding estimate for non-Hispanics is 12.4% though Table 9 estimates for non-Hispanics should be interpreted cautiously given the earlier evidence of differential pre-trends for non-Hispanic labor market outcomes.

4.5 Understanding the response to E-Verify mandates

We have established that the passage of E-Verify mandates led to reductions in employment among Hispanic workers in general and among undocumented workers in particular. We next explore a range of alternative outcomes to better understand how labor markets and individuals adjusted to changing E-Verify coverage. In particular, we first ask whether employment verification requirements lead to declines in the likely work-ineligible population. We then examine changes in self-employment (which is not subject to employment verification) and self-employment income. We conclude this analysis by investigating impacts of E-Verify mandates on individual wage earnings and overall changes in household income.

We begin in Table 10 with an assessment of the impact of E-Verify mandates on the undocumented population in a county, based on our imputed measure of undocumented status described above. These regressions are estimated using (person-weighted) population counts in the American Community Survey. Paralleling the specifications estimated in Table 5, the only included policy variable is an indicator for whether any private-sector E-Verify mandate has been passed by the end of the prior year. The estimates in Columns 1-4 of Panel A show no significant effect of passage of an E-Verify mandate on either the total or Hispanic likely undocumented population, or on the total or Hispanic likely undocumented population that arrived to the U.S. within the prior year. These conclusions are unchanged when we add covariates to the model in Panel B. Though the regression estimates are quite noisy, it is clear from Appendix Figure A6 that there is little visual evidence of general population declines in response to mandate passage.[36] Consistent with Bohn et al. (2014), we do estimate a sizeable decline in the undocumented population in

[36] Indeed, when we residualize outcomes to account for potential pre-trends, the estimated impact on the recently arrived undocumented population becomes positive, consistent with the visual evidence of a slight decline in the recently-arrived, likely undocumented population that pre-dates mandate passage. These estimates are reported in Appendix Table A9.

Arizona, which passed an E-Verify mandate along with a number of other measures aimed at deterring illegal immigration during the sample period. We do not find evidence of population declines in other states that passed E-verify mandates.

In the remainder of our analysis, we investigate whether increases in alternative income sources can reconcile the evidence of large employment declines and statistically insignificant population changes for the likely undocumented population. For these analyses, included in Table 11 and in Table 12, we estimate models at the individual level. One drawback of doing so is that estimates become less directly comparable to our county-level QWI results. However, estimates of income changes at the county level are made particularly imprecise by small sample sizes for relevant subpopulations in combination with substantial within-cell variation in earnings as a function of demographic characteristics. By estimating specifications at the individual level, we can control flexibly for relevant demographic characteristics (age, gender and education), we can eliminate the mismeasurement associated with imputing county of residence (since individuals are now identified by state in these models), and we can extend the sample period back to 2000 in order to improve the precision of treatment group-specific linear pre-trend estimates.[37]

Turning to the estimation of supplementary income sources, one potential explanation for our contrasting findings regarding employment and population changes for the likely undocumented population is that the passage of mandates induces undocumented workers to move from regular, payroll employment (which is captured in the QWI data and may be subject to an E-Verify mandate) to self-employment (which is not measured in the QWI and would not be subject to an E-Verify mandate). In particular, to the extent that firms, in response to E-Verify mandates, are able to reclassify some of their labor force from employees to independent contractors, the QWI data would show de-

[37] In Appendix Table A10, we verify that our county-level employment results are qualitatively unchanged when we run individual-level logit models to capture percentage-wise employment changes. Evidence of employment declines for native workers and non-Hispanics in these specifications is explained by the same differential pre-trends for these subgroups discussed earlier.

clines in employment. In Columns 1 and 2 of Table 11, we present estimates of the impact of E-Verify mandates on self-employment earnings, measured through self-reports in the ACS. In practice, we see little evidence of changes in self-employment earnings in these specifications, and Columns 3 and 4 show a similar lack of significant changes in reported self-employment status. Though self-employment estimates are consistently positive, coefficient magnitudes are generally small. The event study plots presented in Appendix Figure A7 and the residualization-based estimates presented in Appendix Table A11 suggest that, to the extent there is any increase in self-employment activity, it occurs a number of years after mandate passage.

To provide a summary impact of passage of E-Verify mandates, we conclude with an analysis in Table 12 of effects on individual and household incomes. Our measures of annual earnings refer to income received in the year prior to the survey. We estimate the parameters of these models using an inverse hyperbolic sine transformation of the dependent variables and our Table 12 models include the set of control variables we have used in the ACS throughout.[38] Panels A and B present estimated effects of passage of E-Verify mandates on individual wage and salary income without and with residualizing outcomes to account for pre-trends.[39] Mirroring our corresponding effects on employment, we find that wage declines are largest for Hispanics and likely undocumented immigrants. In models that control for pre-trends, E-verify mandates are associated with about a 38 percent decline in wage income for likely undocumented respondents. We also find sizeable declines in wage income among low-skilled natives though non-linear pre-trends visible for some of these subgroups in Appendix Figure A8 suggest that these estimate should be interpreted cautiously.

In Panels C and D we assess effects on total household income from all sources for

[38] Estimates from models without these covariates are quite similar and presented in Appendix Table A12.

[39] Appendix Figure A8 presents event study plots for wage income and these indicate that pretrends in outcomes among non-Hispanics and the native-born are potentially important.

all household members. The effects of E-Verify mandates on household income are notably smaller than the corresponding effects on individual earnings. In Panel D, where we residualize outcomes on linear pre-trends, estimated impacts are negative but statistically indistinguishable from zero for all non-native subgroups. In particular, while wage income fell among likely undocumented immigrants by 38 percent, their implied decrease in household income is 6.5% (and statistically insignificant). These findings suggest that the household members of respondents with higher rates of work ineligibility seemingly increase their earnings in response to the passage of E-Verify mandates. This offsets the direct negative effects estimated for the work-ineligible population and helps to explain the lack of a significant migration response that we documented in Table 10.

5 Conclusions

This paper investigates the labor market impacts of employment eligibility authorization (E-Verify) mandates. A key contribution of our work is to document the impact of E-Verify mandates on usage of the system, relying on newly available administrative records from the Department of Homeland Security. Importantly, usage of E-Verify to verify employment eligibility of new hires is quite low in firms that employ fewer than 20 individuals. Mandates have a modest effect on usage, raising the ratio of queries to hires by about ten percentage points in the four years after a mandate is passed (from a baseline level of 4.5 percent). Usage in large firms is considerably higher, but still far from complete. In total, we estimate that four years after a mandate is passed, usage increases by 25 percentage points from a baseline level of 21 percent. Imperfect compliance in the face of a legal mandate is noteworthy because it implies there are important monetary and/or non-monetary barriers to using the system. Enactment of a nationwide mandate would exacerbate these costs.

We use two primary data sources – the Quarterly Workforce Indicators and the American Community Survey – and two complimentary research designs to estimate the labor market impacts of E-Verify mandates. We document that passage of a mandate leads to significant declines in Hispanic employment and in the employment of likely work-ineligible subpopulations. Our estimates are larger than those found in prior research. We find no evidence that non-Hispanics or natives correspondingly benefit from mandate passage. Consistent with our findings regarding usage of the E-Verify system, our analyses reveal that much of the employment decline is concentrated in large firms. Analysis of data from the County Business Patterns indicates that a substantial fraction of the employment decline is associated with a reduction in the number of large firms that locate in an area following passage of a mandate.

We find clear evidence that E-Verify mandates lead to a number of labor market distortions. First, mandates lead to reductions in both hires and job separations for Hispanic workers, even in small firms where E-Verify adherence is low. Second, we find evidence of important within-state spillovers in employment from large to small firms.

In sum, while E-Verify mandates may significantly reduce formal sector employment among work-ineligible individuals, these policies are not effective in significantly deterring undocumented migration. Moreover, the lack of gains experienced by native-born workers, the labor market distortions, and the disproportionate costs imposed on large firms suggest that the net aggregate costs associated with such mandates may be substantial.

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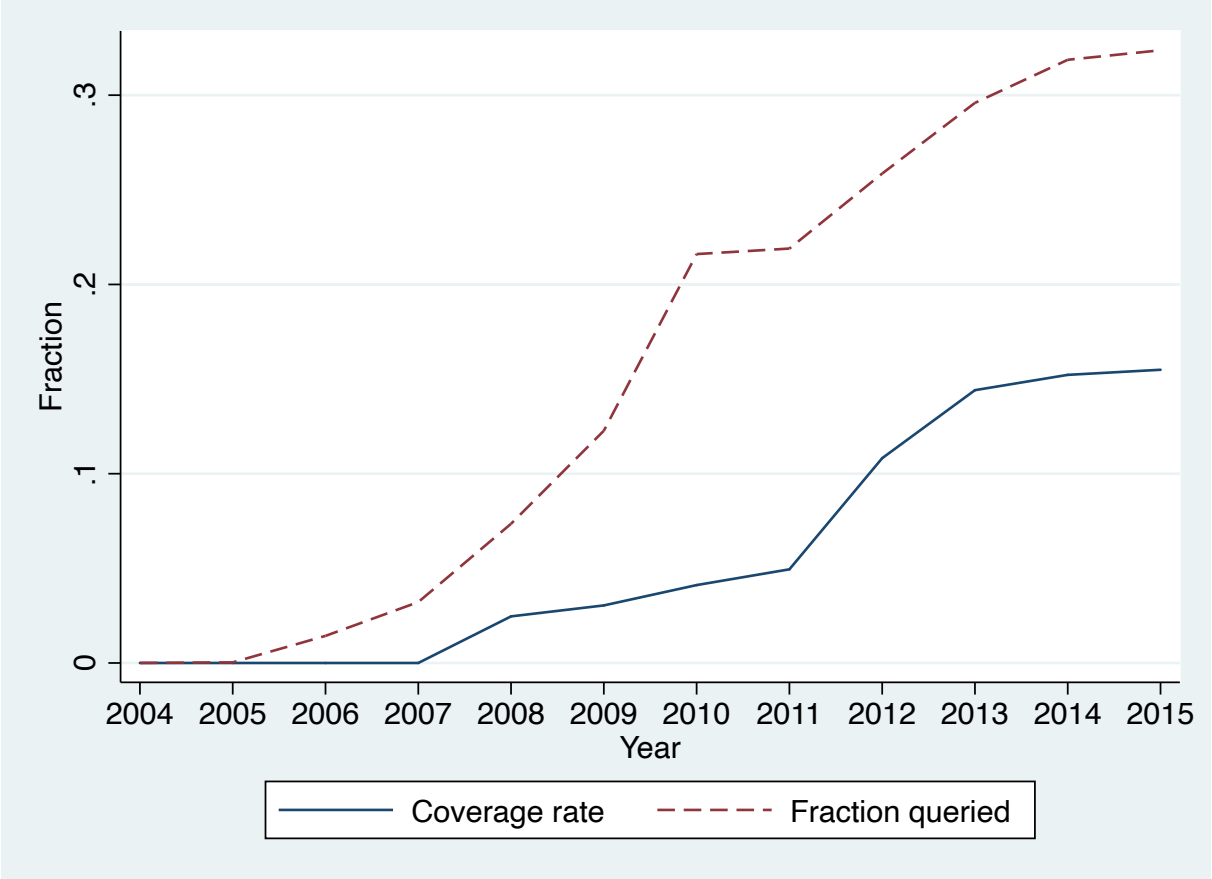
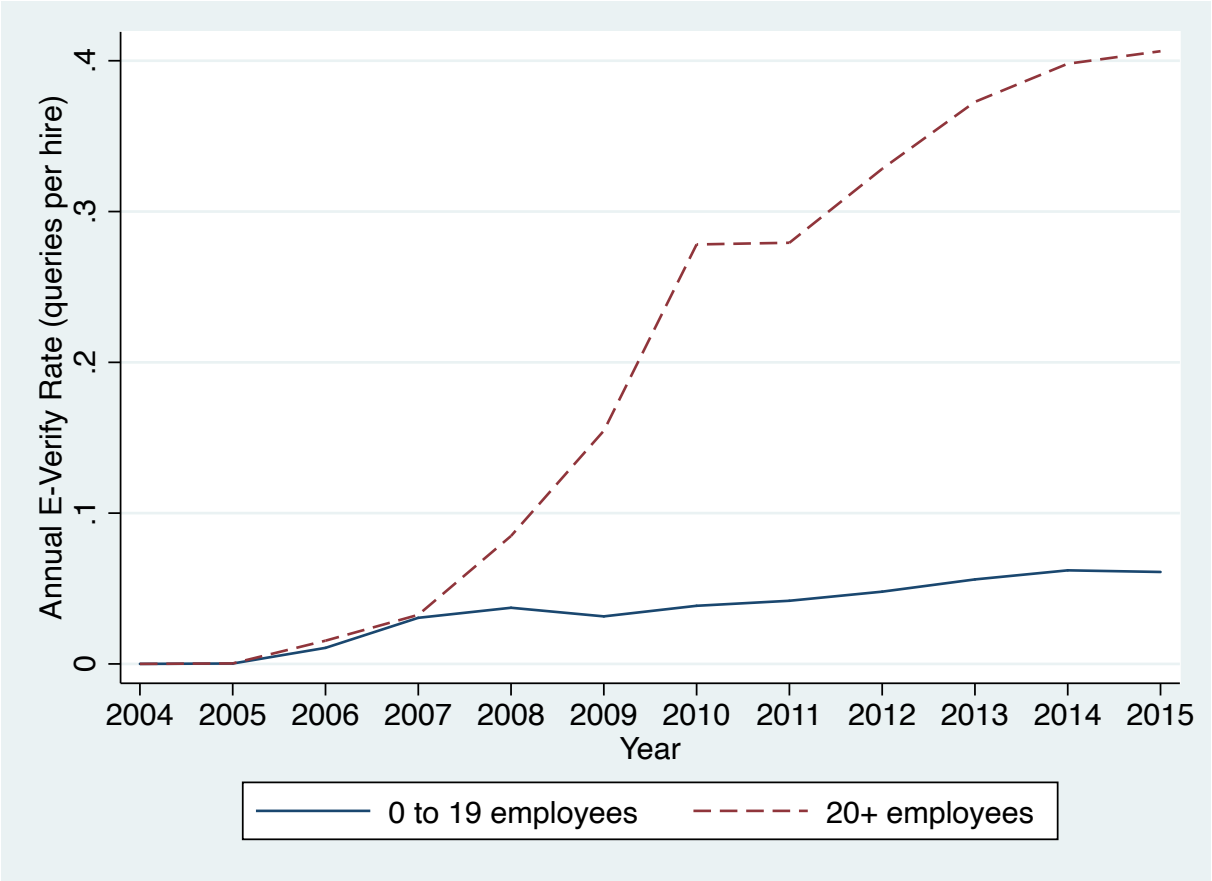
Figure 1: Annual Trends in E-Verify Usage



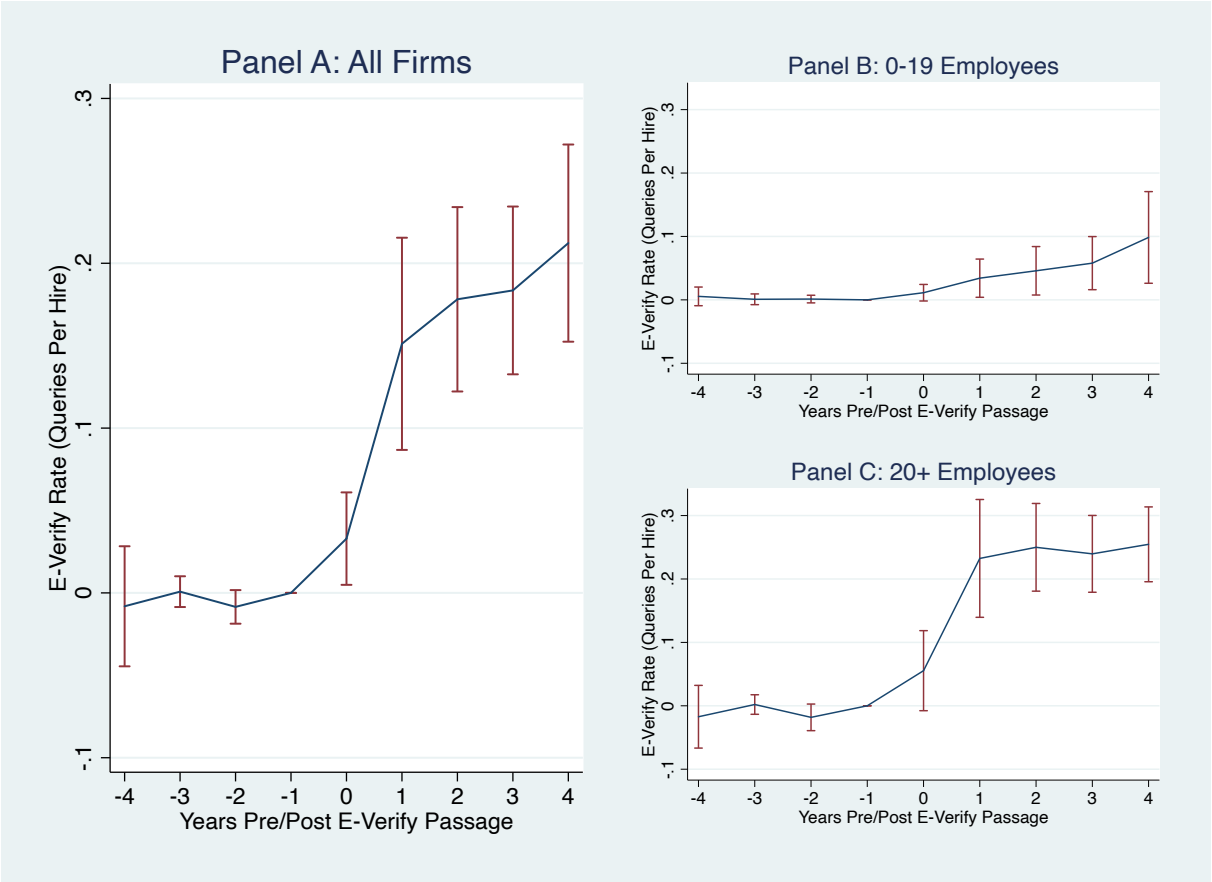
Data source: United States Department of Homeland Security data series.
Notes: This figure plots the annual E-Verify rate, defined as the number of E-Verify queries divided by the total number of new hires, and the annual fraction of all private sector hires subject to E-Verify mandates. New hires are measured using the QWI.

Figure 2: Annual Trends in E-Verify Usage by Firm Size



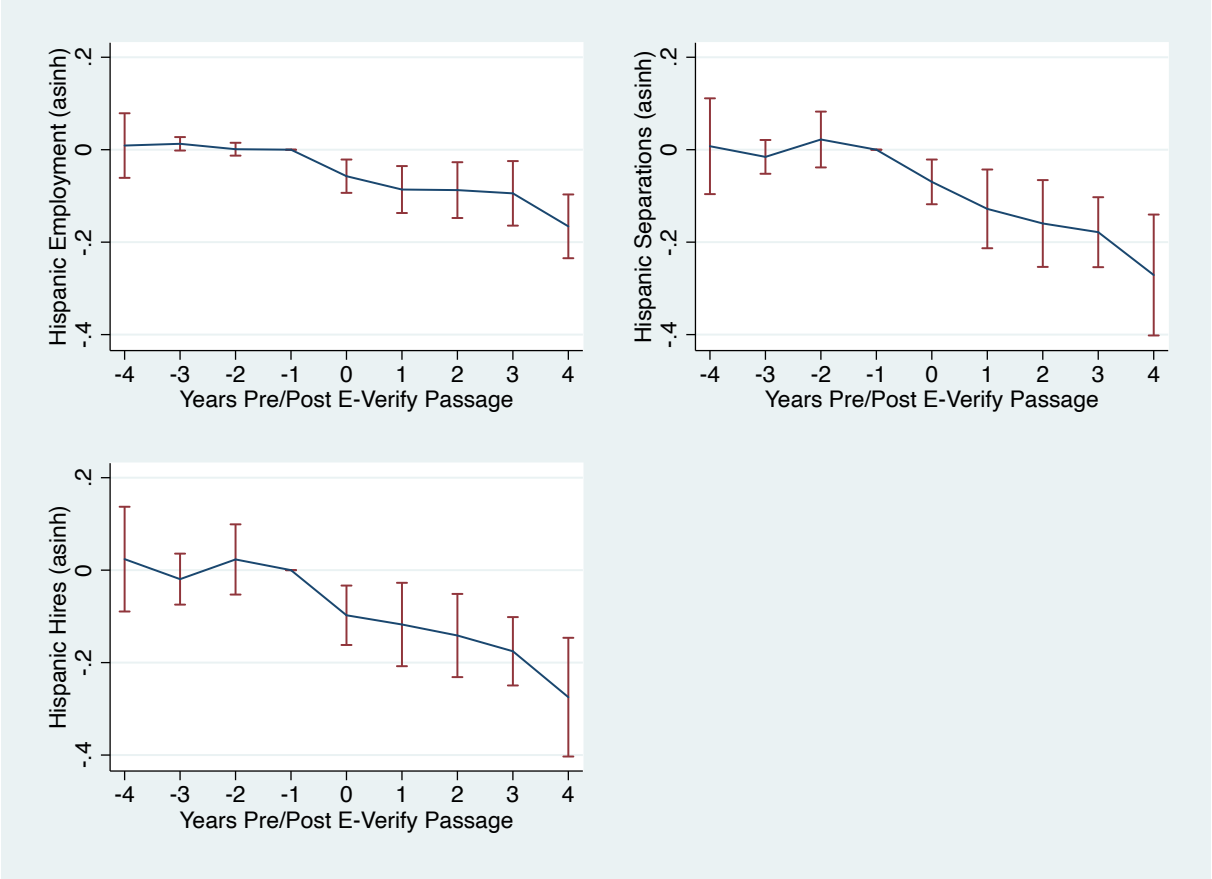
Data source: United States Department of Homeland Security data series.
Notes: This figure plots the annual E-Verify rate, defined as the number of E-Verify queries divided by the total number of new hires, separately by firm size bin. New hires are measured using the QWI.

Figure 3: Event Studies for E-Verify Usage by Firm Size



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the E-Verify rate, defined as the number of E-Verify queries divided by the total number of new hires in the referenced firm size bin(s), on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year-quarter fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0").

Figure 4: Event Studies for QWI-Based Hispanic Worker Outcomes (County-level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year-quarter fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers.

Table 1: State-level E-Verify Mandates

State	Citation	Year Enacted	Applies to:
Alabama	HB 56 HB 658	2011	All employers (phase in)
Arizona	HB 2779 HB 2745	2007 2008	All employers
Colorado	HB 1343 SB 139 SB 193	2006 2008 2008	State agencies, contractors
Florida	EO 11-02 EO 11-116	2011 2011	State agencies, contractors, subcontractors
Georgia	SB 529 HB 2 SB 447 HB 87 HB 742 HB 1027	2006 2009 2010 2011 2012 2012	Public employers, contractors, subcontractors (phase in) Private employers with 11+ employees (phase in)
Idaho	EO 2009-10	2009	State agencies, contractors
Indiana	SB 590	2011	State/local agencies, contractors
Louisiana	HB 342 HB 646 HB 996	2011 2011 2012	State/local contractors Option for private employers
Michigan	HB 5365	2012	Certain state agencies, contractors and subcontractors
Minnesota	EO 08-01	2008	Certain state contractors
Mississippi	SB 2988	2008	All employers (phase in)
Missouri	HB 1549	2008	Public employers, contractors, subcontractors
Nebraska	LB 403	2009	Public employers, contractors
North Carolina	SB 1523 HB 36 HB 786	2006 2011 2013	State agencies, universities Localities, all employers with 25+ employees (phase in) Excludes employees whose term of employment is less than nine months
Oklahoma	HB 1804	2007	Public employers, contractors, subcontractors
Pennsylvania	SB 637	2012	Public works contractors and subcontractors
South Carolina	HB 4400 SB 20 HB 4813	2008 2011 2012	Public employers, contractors, all private employers (phase in)
Tennessee	HB 1378	2011	Public employers, private employers with 6+ employees required to use E-Verify or retain specified employee documentation (phase in)
Texas	SB 374	2015	State agencies
Utah	SB 81 SB 39 SB 251 HB 116	2008 2009 2010 2011	Public employers, contractors, subcontractors Private employers with 15+ employees
Virginia	HB 737 HB 1859 SB 1049	2010 2011	State agencies Public contractors, subcontractors with 51+ employers
West Virginia	SB 659	2012	Certain public employers, contractors

Table 2: E-Verify Queries (County Level)

	(1)	(2)	(3)	(4)
	E-Verify Query Rate			
	Panel A: Passage-Based Treatment			
Mandate passage	0.148***	0.147***	0.131***	0.146***
	(0.023)	(0.025)	(0.022)	(0.022)
	Panel B: Enforcement-Based Treatment			
Mandate enforcement	0.176***	0.191***	0.154***	0.177***
	(0.025)	(0.028)	(0.028)	(0.029)
	Panel C: Passage- and Enforcement-Based Treatments			
Mandate passage	0.020	0.040***	0.004	0.030**
	(0.013)	(0.014)	(0.012)	(0.015)
Mandate enforcement	0.159***	0.160***	0.151***	0.154***
	(0.024)	(0.025)	(0.024)	(0.025)
Year-Quarter FE	X	X	X	X
County FE	X	X	X	X
Additional Controls		X		X
Residualized on Linear Trend			X	X
Mean of Dep. Var.	0.089	0.089	0.089	0.089
SD of Dep. Var.	[0.165]	[0.165]	[0.165]	[0.165]
Observations	136,629	136,629	136,629	136,629

Notes: The unit of observation is the county by year-quarter. Mandate passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year-quarter. Mandate enforcement is an indicator for whether any private sector E-Verify mandate has been enforced by the end of the given year-quarter. E-Verify rate is defined as the number of E-Verify queries divided by the contemporaneous total number of (Hispanic and non-Hispanic) hires.

Specifications labelled "Residualized on Linear Trend" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include: county-level predicted labor demand (i.e., a "Bartik instrument"), indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants, as well as an indicator that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter. In addition, we include interactions between year-quarter and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 3: QWI-Based Hispanic Worker Outcomes (County Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Inverse Hyperbolic Sine Transform				Rate-Based Measure			
				Panel A: Employment				
Mandate passage	-0.094***	-0.081***	-0.069**	-0.112***	-0.124***	-0.102**	-0.113**	-0.169***
	(0.028)	(0.030)	(0.029)	(0.030)	(0.043)	(0.043)	(0.045)	(0.045)
Dep. Var. Mean	4955	4955	4955	4955	1.316	1.316	1.316	1.316
Dep. Var. SD	[35318]	[35318]	[35318]	[35318]	[0.519]	[0.519]	[0.519]	[0.519]
				Panel B: Separations				
Mandate passage	-0.135***	-0.125**	-0.114**	-0.162***	-0.058***	-0.052**	-0.048**	-0.070***
	(0.039)	(0.048)	(0.053)	(0.058)	(0.015)	(0.020)	(0.023)	(0.026)
Dep. Var. Mean	1173	1173	1173	1173	0.437	0.437	0.437	0.437
Dep. Var. SD	[7102]	[7102]	[7102]	[7102]	[0.325]	[0.325]	[0.325]	[0.325]
				Panel C: Hires				
Mandate passage	-0.143***	-0.136***	-0.095	-0.148**	-0.064***	-0.062***	-0.045*	-0.068**
	(0.040)	(0.051)	(0.058)	(0.062)	(0.017)	(0.022)	(0.026)	(0.028)
Dep. Var. Mean	1229	1229	1229	1229	0.461	0.461	0.461	0.461
Dep. Var. SD	[7420]	[7420]	[7420]	[7420]	[0.349]	[0.349]	[0.349]	[0.349]
Observations	138,004	138,004	138,004	138,004	137,488	137,488	137,488	137,488
Year-Quarter FE	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X
Additional Controls		X		X		X		X
Residualized on Trend			X	X			X	X

Notes: The unit of observation is the county by year-quarter. Mandate passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year-quarter. All rate-based measures divide relevant Hispanic worker outcomes by baseline (2003) Hispanic employment.
Specifications labelled "Residualized on Trend" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include: county-level predicted labor demand (i.e., a "Bartik instrument"), indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants, as well as an indicator that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter. In addition, we include interactions between year-quarter and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 4: QWI-Based Non-Hispanic Worker Outcomes (County Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Inverse Hyperbolic Sine Transform				Rate-Based Measure			
				Panel A: Employment				
Mandate passage	-0.044***	-0.050**	-0.006	-0.019	-0.043***	-0.051**	-0.009	-0.021
	(0.014)	(0.020)	(0.017)	(0.019)	(0.015)	(0.022)	(0.018)	(0.021)
Dep. Var. Mean	31355	31355	31355	31355	1.027	1.027	1.027	1.027
Dep. Var. SD	[103971]	[103971]	[103971]	[103971]	[0.165]	[0.165]	[0.165]	[0.165]
				Panel B: Separations				
Mandate passage	-0.101***	-0.104***	-0.022	-0.043	-0.023***	-0.024***	-0.007	-0.011
	(0.024)	(0.033)	(0.043)	(0.045)	(0.006)	(0.008)	(0.010)	(0.009)
Dep. Var. Mean	5908	5908	5908	5908	0.214	0.214	0.214	0.214
Dep. Var. SD	[19076]	[19076]	[19076]	[19076]	[0.088]	[0.088]	[0.088]	[0.088]
				Panel C: Hires				
Mandate passage	-0.093***	-0.096***	-0.013	-0.034	-0.022***	-0.024***	-0.005	-0.010
	(0.025)	(0.034)	(0.049)	(0.050)	(0.006)	(0.008)	(0.011)	(0.010)
Dep. Var. Mean	6137	6137	6137	6137	0.223	0.223	0.223	0.223
Dep. Var. SD	[19856]	[19856]	[19856]	[19856]	[0.097]	[0.097]	[0.097]	[0.097]
Observations	138,004	138,004	138,004	138,004	137,488	137,488	137,488	13,7488
Year-Quarter FE	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X
Additional Controls		X		X		X		X
Residualized on Trend			X	X			X	X

Notes: The unit of observation is the county by year-quarter. Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year-quarter. All rates divide relevant non-Hispanic worker outcomes by baseline (2003) non-Hispanic employment.

Specifications labelled "Residualized on Trend" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include: county-level predicted labor demand (i.e., a "Bartik instrument"), indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants, as well as an indicator that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter. In addition, we include interactions between year-quarter and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio.

Standard errors are clustered by state. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 5: ACS-Based Employment Outcomes (County Level)

	(1) Hispanics	(2) Non-Hispanics	(3) Likely Undocumented Hispanics	(4) Likely Documented Hispanics	(5) Likely Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Younger, Male Low-Skilled Natives	(9) Older, Male Low-Skilled Natives
Panel A: Benchmark Specification									
Mandate passage	-0.137***	-0.008	-0.191*	-0.089*	-0.198**	-0.014	-0.035***	-0.070***	-0.015
	(0.046)	(0.009)	(0.109)	(0.046)	(0.082)	(0.009)	(0.012)	(0.018)	(0.012)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Panel B: Additional Controls									
Mandate passage	-0.107**	-0.000	-0.203*	-0.060	-0.184**	-0.009	-0.033**	-0.061***	-0.018
	(0.048)	(0.010)	(0.116)	(0.047)	(0.090)	(0.012)	(0.015)	(0.023)	(0.011)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Addl Controls	X	X	X	X	X	X	X	X	X
Dep. Var. Mean	6137.6	33626.1	1056.8	5080.7	1196.4	33010.1	11187.7	3426.7	2803.1
Dep. Var. SD	[44974.8]	[91128.51]	[8340.5]	[37103.8]	[8926.4]	[88413.8]	[25844.1]	[8613.8]	[5787.5]
Observations	34,528	34,528	34,528	34,528	34,528	34,528	34,528	34,528	34,528

Notes: The unit of observation is the county by year. Mandate passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. Each outcome value is the inverse hyperbolic sine transform of the number of employed individuals with the referenced characteristic(s). Likely Undocumented is a probabilistic measure corresponding to non-citizen respondents who have not completed high school, were not born in Cuba, are not in the military and are non-veterans, are not employed by the government, and did not arrive to the US prior to 1980. Likely Documented workers are those not classified as Likely Undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Younger corresponds to respondents aged 16-40 and Older corresponds to respondents aged 41-64.
Additional controls include indicators for whether a state had any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants by the end of the prior year. In addition, we include interactions between year and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 6: E-Verify Queries and QWI-Based Hispanic Worker Outcomes (County-by-Firm Size Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	E-Verify (Query Rate)		Employment (asinh)		Separations (asinh)		Hires (asinh)	
Panel A: Benchmark Specification								
Covered	0.140***		-0.070***		-0.137***		-0.146***	
	(0.023)		(0.025)		(0.033)		(0.036)	
Covered x Small Firms		0.051***		-0.033		-0.110***		-0.121***
		(0.015)		(0.026)		(0.036)		(0.039)
Covered x Large Firms		0.200***		-0.101***		-0.166***		-0.173***
		(0.032)		(0.027)		(0.035)		(0.037)
County-by-Firm Size Bin FE	X	X	X	X	X	X	X	X
Year-Quarter FE	X		X		X		X	
YearQuarter-by-Firm Size Bin FE		X		X		X		X
Panel B: Additional Controls								
Covered	0.152***		-0.059**		-0.131***		-0.141***	
	(0.026)		(0.024)		(0.036)		(0.040)	
Covered x Small Firms		0.060***		-0.027		-0.111***		-0.122***
		(0.018)		(0.024)		(0.039)		(0.041)
Covered x Large Firms		0.210***		-0.086***		-0.157***		-0.164***
		(0.032)		(0.027)		(0.039)		(0.043)
County-by-Firm Size Bin FE	X	X	X	X	X	X	X	X
Year-Quarter FE	X		X		X		X	
YearQuarter-by-Firm Size Bin FE		X		X		X		X
Additional Controls	X	X	X	X	X	X	X	X
Dep. Var. Mean	0.086	0.086	2705.0	2705.0	640.1	640.1	670.6	670.6
Dep. Var. SD	[0.321]	[0.321]	[21826.4]	[21826.4]	[4318.9]	[4318.9]	[4473.9]	[4473.9]
Observations	226,276	226,276	252,572	252,572	252,572	252,572	252,572	252,572

Notes: The unit of observation is the firm size bin by county by year-quarter. Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). E-Verify rate is defined as the number of E-Verify queries divided by the contemporaneous total number of (Hispanic and non-Hispanic) hires; each other outcome value is the inverse hyperbolic sine transform of the given measure. Covered is an indicator for whether a given firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the given year-quarter. Additional controls include those described in Table 2. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 7: CBP Establishment Outcomes (County and County-by-Establishment Size Level)

	(1)	(2)	(3)	(4)	(5)	(6)
	Establishments (asinh)			Establishments, Weighted (asinh)		
	Panel A: Benchmark Specification					
Covered	-0.030**			-0.030***		
	(0.012)			(0.011)		
Covered x Small Estabs		-0.012			-0.012	
		(0.009)			(0.009)	
Covered x Big Estabs		-0.050***			-0.050***	
		(0.015)			(0.014)	
Mandate Passage			-0.016*			-0.036***
			(0.009)			(0.013)
County-by-Estab Size Bin FE	X	X		X	X	
Year FE	X		X	X		X
Year-by-Estab Size Bin FE		X			X	
County FE			X			X
	Panel B: Additional Controls					
Covered	-0.032**			-0.032***		
	(0.013)			(0.011)		
Covered x Small Estabs		-0.014			-0.015	
		(0.011)			(0.011)	
Covered x Big Estabs		-0.052***			-0.052***	
		(0.016)			(0.013)	
Mandate Passage			-0.017			-0.039***
			(0.010)			(0.014)
County-by-Estab Size Bin FE	X	X		X	X	
Year FE	X		X	X		X
Year-by-Estab Size Bin FE		X			X	
County FE			X			X
Addl Controls	X	X	X	X	X	X
Dep. Var. Mean	1181.0	1181.0	2362.0	17409.1	17409.1	34818.2
Dep. Var. SD	[5066.2]	[5066.2]	[8111.1]	[76989.5]	[76989.5]	[129447.1]
Observations	73,776	73,776	36,888	73,776	73,776	36,888

Notes: The unit of observation is the establishment size bin by county by year in Columns (1)-(2) and (4)-(5) and the county by year in Columns (3) and (6). Establishment size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Each outcome value is the inverse hyperbolic sine transform of the referenced measure. Covered is an indicator for whether the corresponding firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the first quarter of the given year (establishment count data is available annually for the first quarter). Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the first quarter of the given year. Establishments (Weighted) scales the number of establishments in each bin by the baseline county-specific average firm size in that bin.

Additional controls include those introduced in Table 5. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 8: E-Verify Query Rates and QWI-Based Hispanic Worker Outcomes as a Function of Predicted E-Verify Coverage (County Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	E-Verify Rate		Employment (asinh)		Separations (asinh)		Hires (asinh)	
				Panel A: Passage-Based Coverage				
Predicted Coverage	0.191***	0.518***	-0.122***	-0.365***	-0.169***	-0.098	-0.181***	-0.110
	(0.027)	(0.132)	(0.036)	(0.108)	(0.047)	(0.126)	(0.049)	(0.124)
				Panel B: Enforcement-Based Coverage				
Predicted Coverage	0.200***	0.382**	-0.102***	-0.189***	-0.168***	-0.163**	-0.167***	-0.173**
	(0.028)	(0.144)	(0.030)	(0.065)	(0.041)	(0.065)	(0.041)	(0.071)
County FE	X	X	X	X	X	X	X	X
Year-Quarter FE	X		X		X		X	
YearQuarter-by-State FE		X		X		X		X
Dep. Var. Mean	0.089	0.089	4955	4955	1173	1173	1229	1229
Dep. Var. SD	[0.164]	[0.164]	[35318]	[35318]	[7102]	[7102]	[7420]	[7420]
Observations	132,726	132,726	133,152	133,152	133,152	133,152	133,152	133,152

Notes: The unit of observation is the county by year-quarter. E-Verify rate is defined as the number of E-Verify queries divided by the contemporaneous total number of (Hispanic and non-Hispanic) hires. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers. To construct the Predicted Coverage measure, we first calculate the predicted share of workers covered by E-Verify legislation that has been either passed (in Panel A) or enforced (in Panel B) by the end of the given year-quarter, as determined by the baseline (2003) firm size distribution for all workers (in Columns 1-2) and for Hispanic workers (in Columns 3-8). In Panel A, this measure is scaled by 0.255 for workers in small firms (with fewer than 20 employees) to account for the relative intensity of E-Verify usage across smaller versus larger firms. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 9: QWI-Based Spillover Analyses (Small Firm Employment)

	(1)	(2)	(3)	(4)	(5)	(6)
	Hispanic Employment in Small Firms (asinh)			Non-Hispanic Employment in Small Firms (asinh)		
	Panel A: Passage-Based Coverage					
Predicted Coverage	-0.045	-0.033	0.366***	-0.056***	-0.050***	0.124***
	(0.029)	(0.022)	(0.049)	(0.016)	(0.014)	(0.039)
	Panel B: Enforcement-Based Coverage					
Predicted Coverage	-0.043	-0.025	0.232***	-0.046***	-0.048***	0.072***
	(0.027)	(0.024)	(0.042)	(0.014)	(0.015)	(0.012)
County FE	X	X	X	X	X	X
Year-Quarter FE	X	X		X	X	
YearQuarter-by-State FE			X			X
Dep. Var. Mean	1002.1	1026.2	1002.1	6823.5	6894.8	6823.5
Dep. Var. SD	[7127.1]	[7293.8]	[7127.1]	[19948.3]	[20245.2]	[19948.3]
Observations	124,214	117,251	124,214	124,214	117,251	124,214

Notes: The unit of observation is the county by year-quarter. Small firms are those with fewer than 20 employees. Each outcome value is the inverse hyperbolic sine transform of (Hispanic or non-Hispanic) employment in small firms. Columns 2 and 5 restrict the sample to county-year-quarter cells in which small firms are not yet subject to E-Verify mandate enforcement. To construct the Predicted Coverage measure, we first calculate the predicted share of workers covered by E-Verify legislation that has been either passed (in Panel A) or enforced (in Panel B) by the end of the given year-quarter, as determined by the baseline (2003) firm size distribution for Hispanic workers (in Columns 1-3) and for non-Hispanic workers (in Columns 4-6). In Panel A, this measure is scaled by 0.255 for workers in small firms (with fewer than 20 employees) to account for the relative intensity of E-Verify usage across smaller versus larger firms. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 10: ACS-Based Migration Outcomes (County Level)

	(1)	(2)	(3)	(4)
	Likely Undocumented Population		Likely Undocumented Abroad Last Year	
	All	Hispanic	All	Hispanic
	Panel A: Benchmark Specification			
Mandate Passage	-0.013	-0.020	-0.122	-0.100
	(0.092)	(0.111)	(0.111)	(0.133)
Year FE	X	X	X	X
County FE	X	X	X	X
	Panel B: Additional Controls			
Mandate Passage	0.045	0.002	-0.164	-0.149
	(0.073)	(0.098)	(0.108)	(0.130)
Year FE	X	X	X	X
County FE	X	X	X	X
Addl Controls	X	X	X	X
Mean of Dep. Var.	2098.6	1781.9	63.0	40.7
SD of Dep. Var.	[16070.2]	[14653.8]	[392.5]	[306.6]
Observations	34,528	34,528	34,528	34,528

Notes: The unit of observation is the county by year. In Columns (1)-(2), the outcome value is the inverse hyperbolic sine transform of the number of likely undocumented residents, defined as non-citizen respondents who have not completed high school, were not born in Cuba, are not in the military and are non-veterans, are not employed by the government, and did not arrive to the US prior to 1980. In Columns (3)-(4), the outcome value is the inverse hyperbolic sine transform of the number of likely undocumented residents abroad one year ago. Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. The sample is restricted to respondents aged 16-64.

Additional controls include indicators for whether a state had any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants by the end of the prior year. In addition, we include interactions between year and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 11: ACS-Based Self-Employment Outcomes (Individual Level)

	(1)	(2)	(3)	(4)
	Likely Undocumented Business Income (asinh)		Likely Undocumented Self-Employment Rate	
	All	Hispanic	All	Hispanic
	Panel A: Benchmark Specification			
Mandate Passage	0.016	0.013	0.002	0.003
	(0.059)	(0.068)	(0.007)	(0.007)
Year FE	X	X	X	X
State FE	X	X	X	X
	Panel B: Additional Controls			
Mandate Passage	0.037	0.037	0.003	0.005
	(0.054)	(0.062)	(0.006)	(0.006)
Year FE	X	X	X	X
State FE	X	X	X	X
Addl Controls	X	X	X	X
Mean of Dep. Var.	$1,050	$1,037	0.072	0.072
SD of Dep. Var.	[7,181]	[6,920]	[0.259]	[0.259]
Observations	851,908	700,938	851,908	700,938

Notes: The unit of observation is the individual. The outcome measure in Columns (1)-(2) is the inverse hyperbolic sine transform of business (self-employment) income and the outcome measure in Columns (3)-(4) is an indicator for self-employment. Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. The sample is restricted to respondents aged 16-64.

Additional controls include indicators for whether a state had any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants by the end of the prior year. In addition, we include interactions between year and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

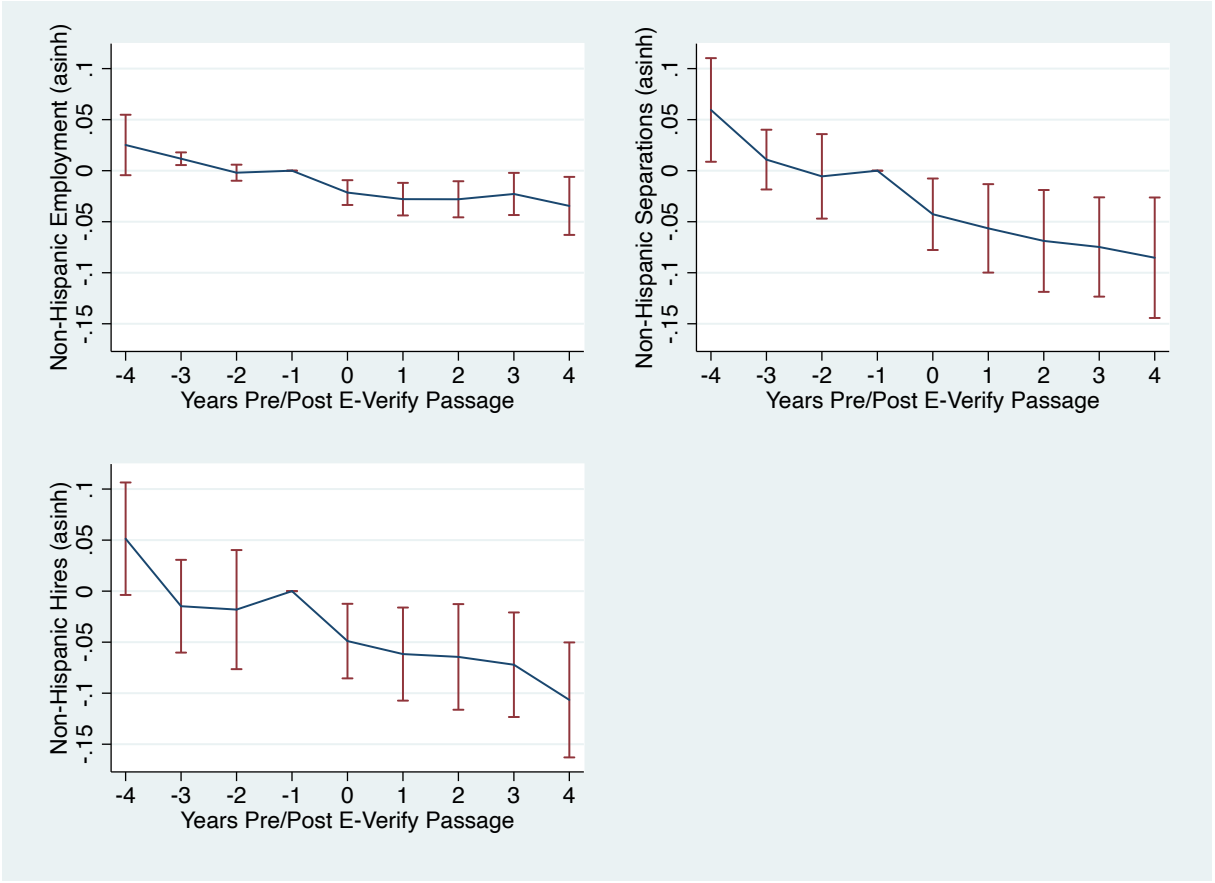
Table 12: ACS-Based Individual and Household Annual Earnings Measures (Individual Level)

	(1) Hispanics	(2) Non-Hispanics	(3) Likely Undocumented Hispanics	(4) Likely Documented Hispanics	(5) Likely Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Younger, Male Low-Skilled Natives	(9) Older, Male Low-Skilled Natives
Panel A: Individual Wage Income (Additional Controls)									
Mandate Passage	-0.165***	-0.153***	-0.286**	-0.107*	-0.262***	-0.146***	-0.141***	-0.150***	-0.127***
	(0.053)	(0.032)	(0.111)	(0.058)	(0.092)	(0.033)	(0.036)	(0.045)	(0.041)
Panel B: Individual Wage Income (Additional Controls and Residualized on Trend)									
Mandate Passage	-0.215***	-0.054*	-0.364**	-0.135***	-0.379**	-0.062*	-0.106***	-0.252***	-0.088**
	(0.064)	(0.028)	(0.175)	(0.047)	(0.182)	(0.030)	(0.034)	(0.054)	(0.041)
Dep. Var. Mean	$19,147	$29,987	$12,501	$20,613	$12,211	$28,735	$16,044	$15,311	$26,364
Dep. Var. SD	[27,923]	[44,839]	[15,159]	[29,818]	[15,883]	[43,013]	[23,865]	[21,839]	[32,742]
Panel C: Total Household Income (Additional Controls)									
Mandate Passage	-0.080***	-0.035**	-0.152***	-0.055***	-0.161***	-0.041***	-0.041**	-0.046***	-0.033*
	(0.019)	(0.013)	(0.055)	(0.018)	(0.048)	(0.014)	(0.017)	(0.017)	(0.017)
Panel D: Total Household Income (Additional Controls and Residualized on Trend)									
Mandate Passage	-0.024	-0.019	-0.080	-0.008	-0.065	-0.027*	-0.040*	-0.037*	-0.029
	(0.051)	(0.014)	(0.134)	(0.030)	(0.123)	(0.015)	(0.020)	(0.019)	(0.020)
Dep. Var. Mean	$60,893	$81,562	$45,313	$64,357	$46,808	$79,316	$61,009	$65,122	$60,691
Dep. Var. SD	[55,017]	[77,483]	[37,013]	[57,700]	[40,566]	[74,946]	[56,206]	[59,362]	[50,460]
Observations	4,002,365	28,171,568	684,157	3,318,208	831,908	27,546,260	11,750,109	3,220,871	2,802,430
Year FE	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X

Notes: The unit of observation is the individual. Each outcome value is the inverse hyperbolic sine transform of annual earnings and each column is restricted to individuals with the referenced characteristic(s). Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. Likely Undocumented and other groupings are as defined in Table 5. All specifications include the additional controls introduced in Table 5 and control for gender, age fixed effects, and educational attainment fixed effects. "Residualized" specifications are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels. Standard errors are clustered by state. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

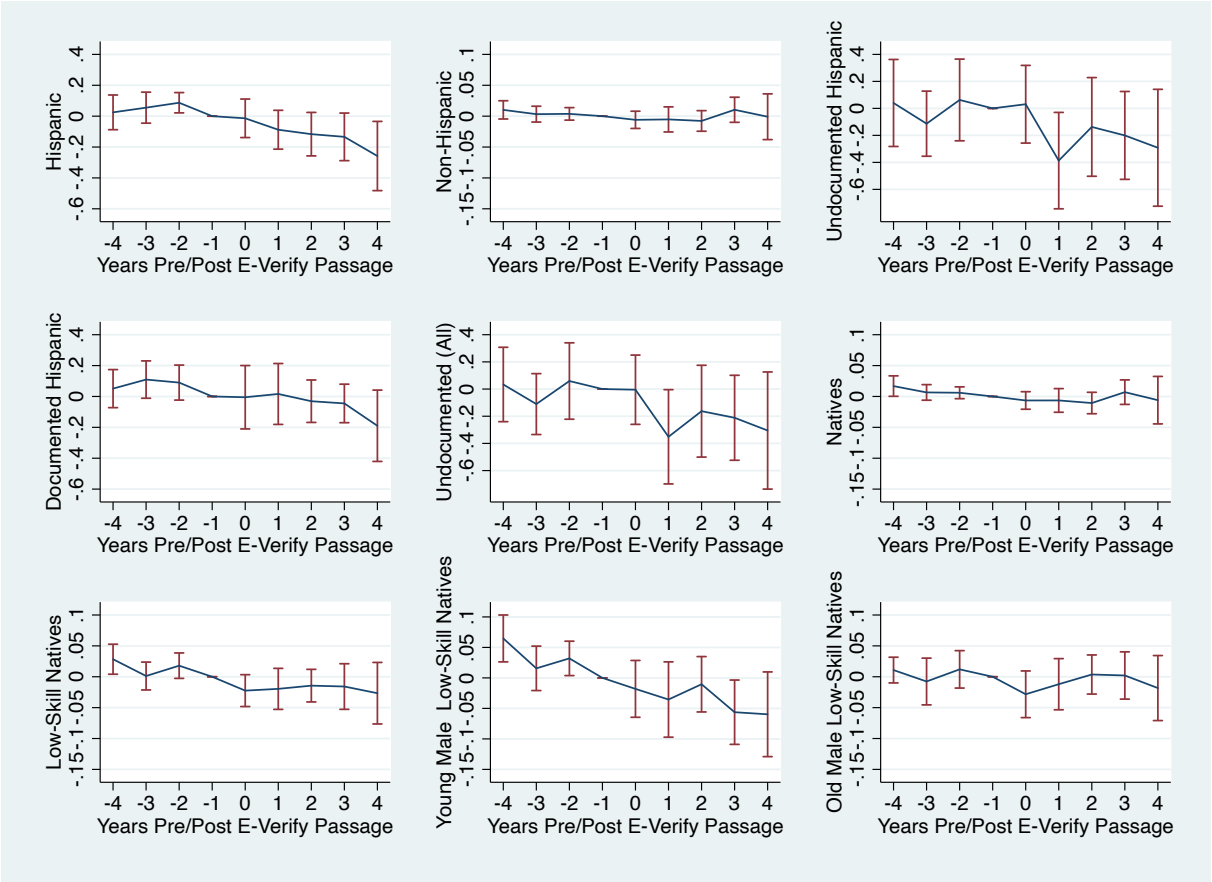
Appendix

Figure A1: Event Studies for QWI-Based Non-Hispanic Worker Outcomes (County-level)



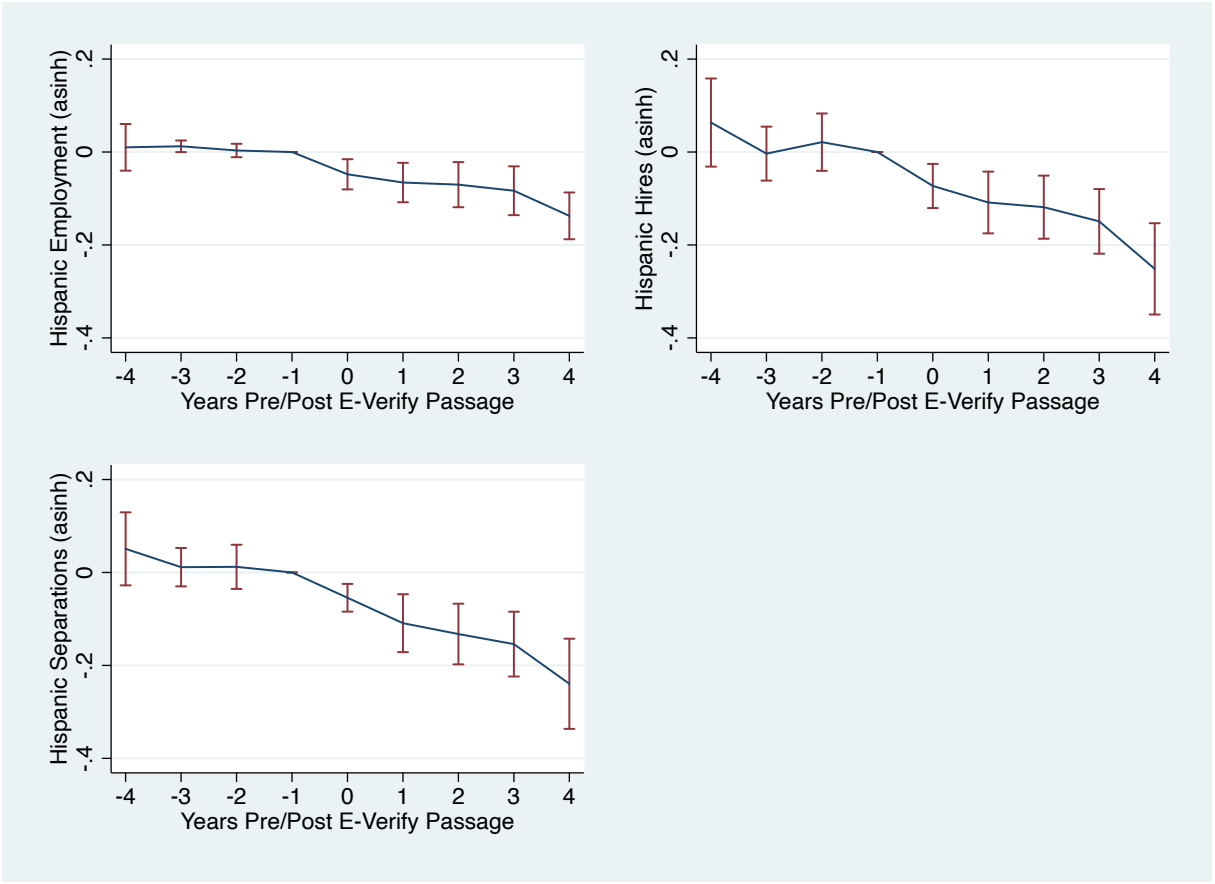
Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year-quarter fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for non-Hispanic workers.

Figure A2: Event Studies for ACS-Based Worker Outcomes (County-level)



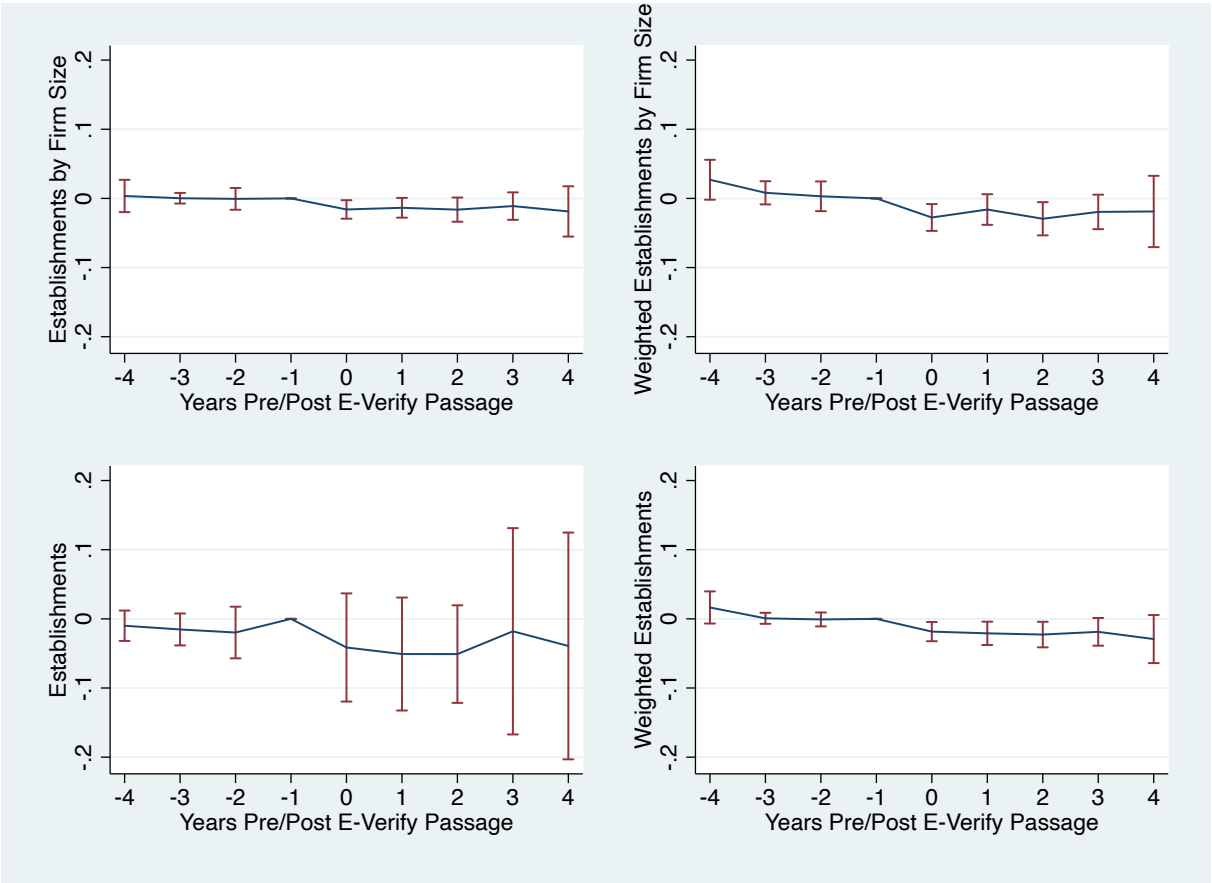
Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Each outcome value is the inverse hyperbolic sine transform of the number of employed individuals with the referenced characteristic(s). Likely Undocumented is a probabilistic measure corresponding to non-citizen respondents who have not completed high school, were not born in Cuba, are not in the military and are non-veterans, are not employed by the government, and did not arrive to the US prior to 1980. Likely Documented workers are those not classified as Likely Undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

Figure A3: Event Studies for QWI-Based Hispanic Worker Outcomes (County-by-Firm Size Level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-by-firm size bin level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate that covers the relevant firm size bin has been passed in the state in which a given county is located. Specifications include county-by-firm size bin and year-quarter fixed effects. $y = 0$ represents the year in which the first relevant private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers in a given firm size bin. Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees).

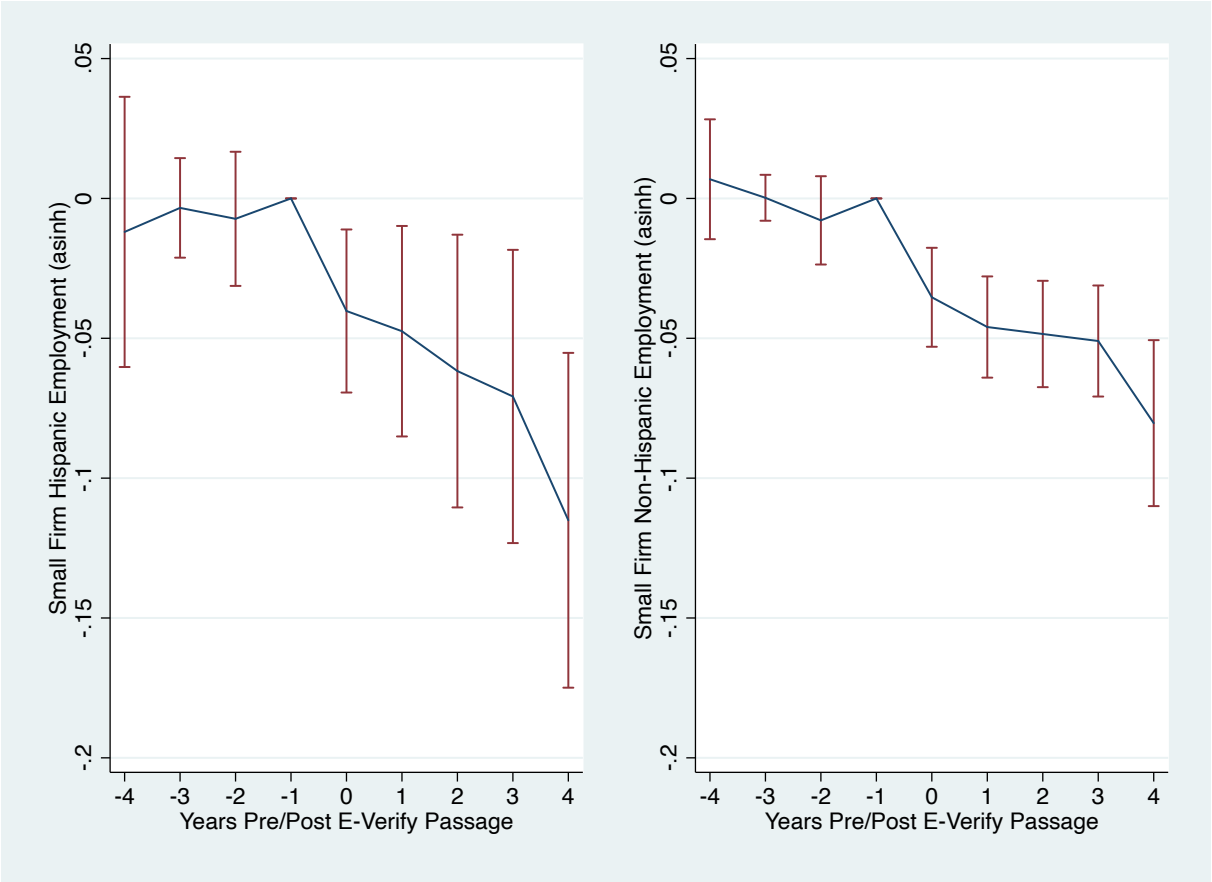
Figure A4: Event Studies for CBP-Based Establishment Outcomes (County and County-by-Establishment Size Level)



Notes: The upper two panels plot coefficients and 95% confidence intervals from a county-by-establishment size bin level regression of the referenced outcome measure on a set of dummies for years before and after the first year by which a private sector E-Verify mandate that covers the corresponding firm size bin has been passed by the end of Q1. Specifications include county-by-establishment size bin and year fixed effects. $y = 0$ represents the year in which the first relevant private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Establishment size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Establishments (Weighted) scales the number of establishments in each bin by the baseline county-specific average firm size in that bin.

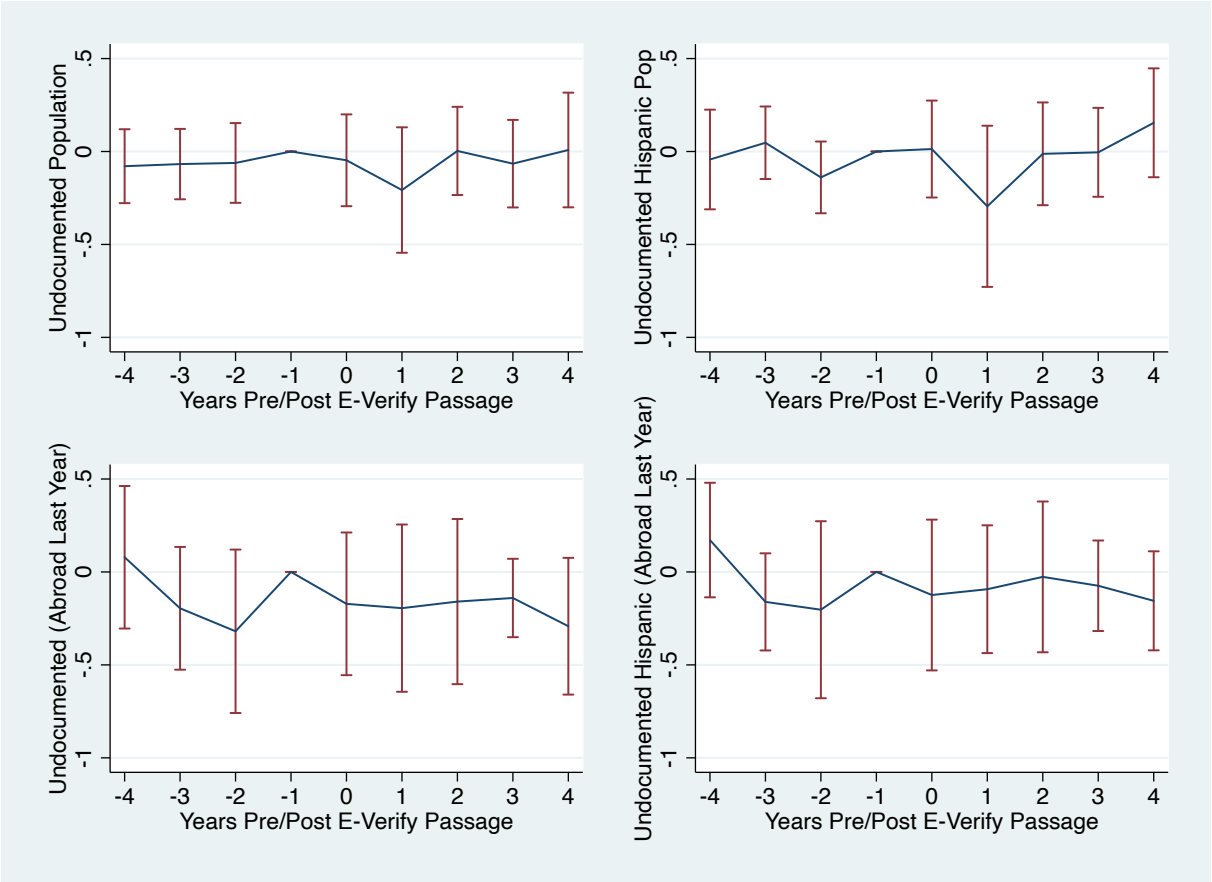
The lower two panels plot coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first year by which a private sector E-Verify mandate has been passed by the end of Q1 in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed by the end of Q1 and $y = -1$ is the omitted year (with the coefficient set equal to "0").

Figure A5: Event Studies for QWI-Based Spillover Analyses (Small Firm Employment)



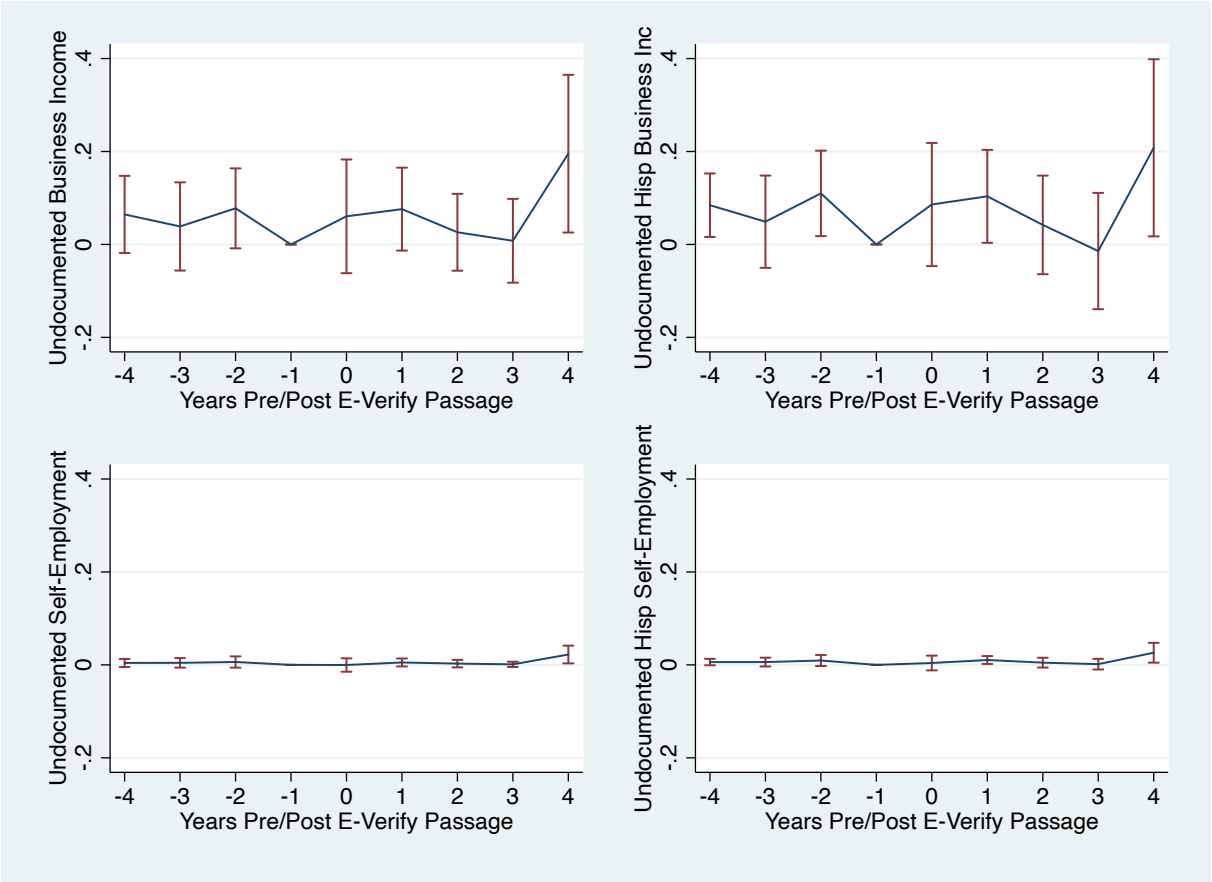
Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year-quarter fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Small Firm Employment measures total (Hispanic or non-Hispanic) county-level employment in firms with fewer than 20 employees and the associated outcome measures are inverse hyperbolic sine transformations of these values.

Figure A6: Event Studies for ACS-Based Migration Outcomes (County Level)



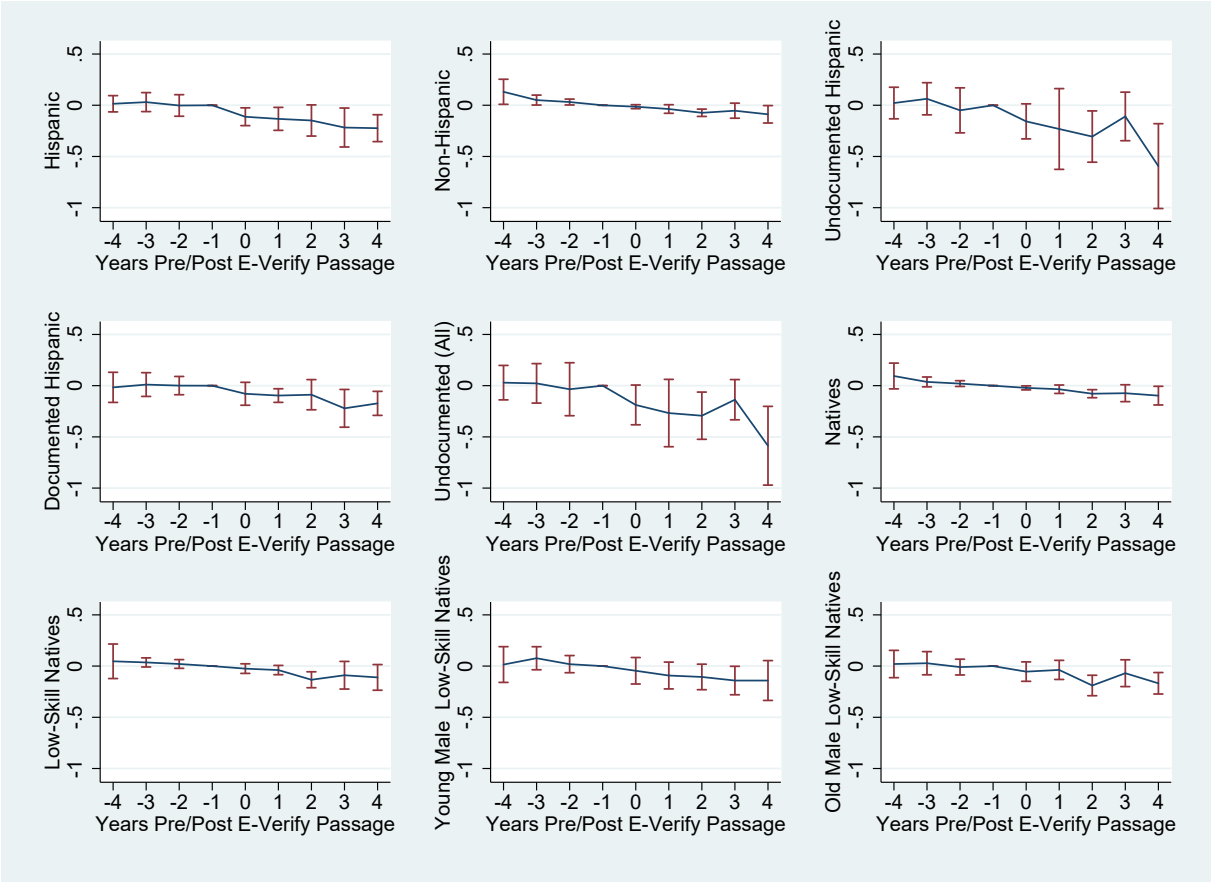
Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Undocumented Population is the inverse hyperbolic sine transform of the number of likely undocumented residents.

Figure A7: Event Studies for ACS-Based Self-Employment Outcomes (Individual Level)



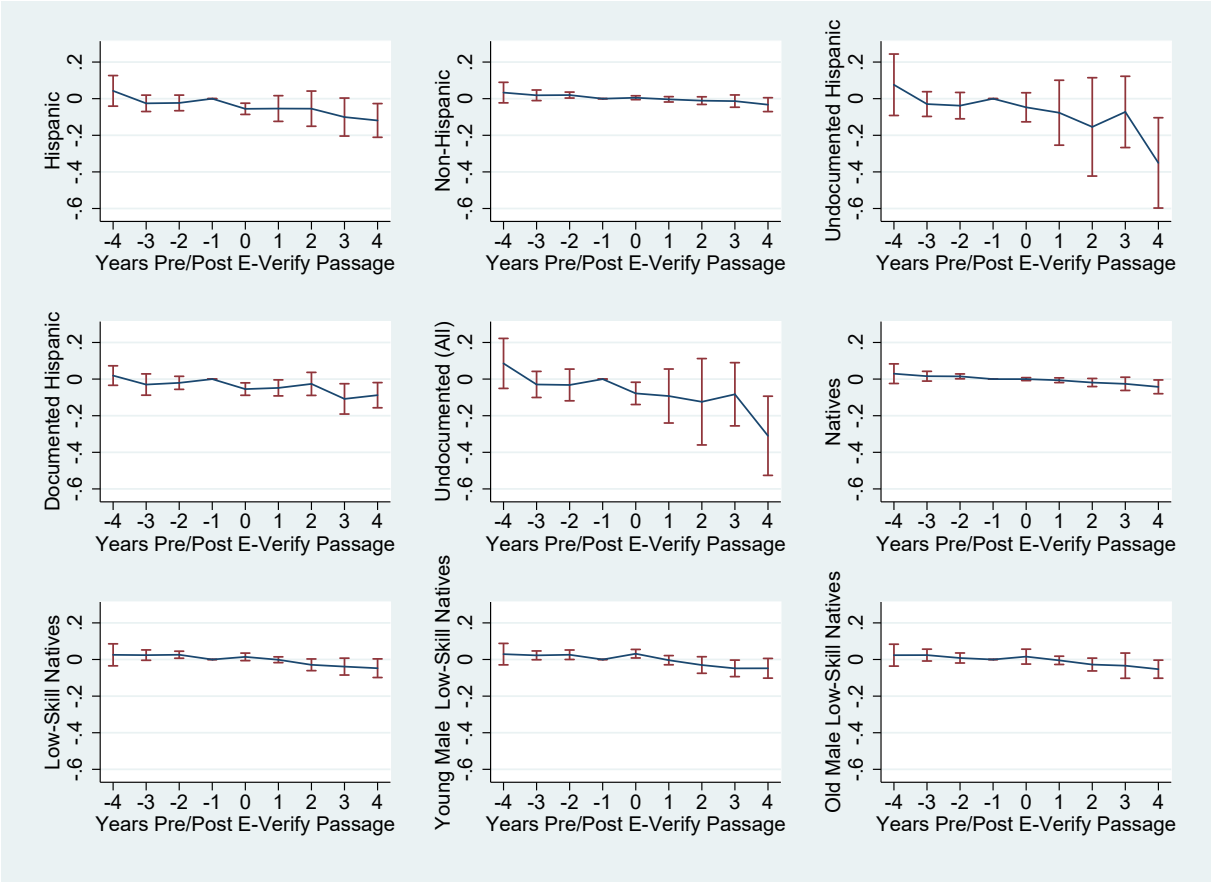
Notes: Each panel plots coefficients and 95% confidence intervals from an individual-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given individual is located. Specifications include state and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). The Business Income outcome is the inverse hyperbolic sine transform of business (self-employment) income and the self-employment outcome is an indicator variable.

Figure A8: Event Studies for ACS-Based Individual Wage Income (Individual Level)



Notes: Each panel plots coefficients and 95% confidence intervals from an individual-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given individual resides. Specifications include state and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Each outcome value is the inverse hyperbolic sine transform of annual individual wage income for individuals with the referenced characteristic(s). Undocumented is a probabilistic measure corresponding to non-citizen respondents who have not completed high school, were not born in Cuba, are not in the military and are non-veterans, are not employed by the government, and did not arrive to the US prior to 1980. Documented workers are those not classified as Undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

Figure A9: Event Studies for ACS-Based Household Total Income (Individual Level)



Notes: Each panel plots coefficients and 95% confidence intervals from an individual-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given individual resides. Specifications include state and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Each outcome value is the inverse hyperbolic sine transform of annual household total income for individuals with the referenced characteristic(s). Undocumented is a probabilistic measure corresponding to non-citizen respondents who have not completed high school, were not born in Cuba, are not in the military and are non-veterans, are not employed by the government, and did not arrive to the US prior to 1980. Documented workers are those not classified as Undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

Table A1: QWI-Based Hispanic Worker Outcomes (County Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Employment (asinh)				
Mandate passage					-0.062**	-0.049	-0.034	-0.060**
					(0.027)	(0.030)	(0.020)	(0.024)
Mandate enforcement	-0.093***	-0.078***	-0.060**	-0.091***	-0.040***	-0.039**	-0.044*	-0.065**
	(0.026)	(0.026)	(0.028)	(0.029)	(0.015)	(0.017)	(0.025)	(0.026)
Dep. Var. Mean	4955	4955	4955	4955	4955	4955	4955	4955
Dep. Var. SD	[35318]	[35318]	[35318]	[35318]	[35318]	[35318]	[35318]	[35318]
				Panel B: Separations (asinh)				
Mandate passage					-0.057*	-0.035	-0.027	-0.046
					(0.034)	(0.043)	(0.037)	(0.048)
Mandate enforcement	-0.146***	-0.140***	-0.105***	-0.144***	-0.097***	-0.112***	-0.109***	-0.144***
	(0.038)	(0.042)	(0.035)	(0.037)	(0.025)	(0.024)	(0.035)	(0.033)
Dep. Var. Mean	1173	1173	1173	1173	1173	1173	1173	1173
Dep. Var. SD	[7102]	[7102]	[7102]	[7102]	[7102]	[7102]	[7102]	[7102]
				Panel C: Hires (asinh)				
Mandate passage					-0.089**	-0.073	-0.038	-0.066
					(0.040)	(0.053)	(0.043)	(0.058)
Mandate enforcement	-0.144***	-0.136***	-0.069*	-0.101**	-0.068**	-0.079***	-0.071*	-0.102**
	(0.037)	(0.044)	(0.037)	(0.041)	(0.027)	(0.029)	(0.041)	(0.039)
Dep. Var. Mean	1229	1229	1229	1229	1229	1229	1229	1229
Dep. Var. SD	[7420]	[7420]	[7420]	[7420]	[7420]	[7420]	[7420]	[7420]
Observations	138,004	138,004	138,004	138,004	138,004	138,004	138,004	138,004
Year-Quarter FE	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X
Additional Controls		X		X		X		X
Residualized on Trend			X	X			X	X

Notes: The unit of observation is the county by year-quarter. Mandate passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year-quarter. Mandate enforcement is an indicator for whether any private sector E-Verify mandate has gone into effect by the end of the given year-quarter. Specifications labelled "Residualized on Trend" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include: county-level predicted labor demand (i.e., a "Bartik instrument"), indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants, as well as an indicator that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter. In addition, we include interactions between year-quarter and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A2: QWI-Based Non-Hispanic Worker Outcomes (County Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Panel A: Employment (asinh)			
Mandate passage					-0.041***	-0.042**	-0.006	-0.013
					(0.012)	(0.017)	(0.009)	(0.015)
Mandate enforcement	-0.039***	-0.043**	0.003	-0.004	-0.004	-0.010	0.001	-0.007
	(0.013)	(0.018)	(0.015)	(0.017)	(0.007)	(0.007)	(0.014)	(0.011)
Dep. Var. Mean	31355	31355	31355	31355	31355	31355	31355	31355
Dep. Var. SD	[103971]	[103971]	[103971]	[103971]	[103971]	[103971]	[103971]	[103971]
					Panel B: Separations (asinh)			
Mandate passage					-0.091***	-0.082**	-0.016	-0.022
					(0.024)	(0.031)	(0.026)	(0.036)
Mandate enforcement	-0.091***	-0.092***	0.002	-0.015	-0.012	-0.028	-0.007	-0.025
	(0.023)	(0.030)	(0.033)	(0.035)	(0.021)	(0.018)	(0.035)	(0.029)
Dep. Var. Mean	5908	5908	5908	5908	5908	5908	5908	5908
Dep. Var. SD	[19076]	[19076]	[19076]	[19076]	[19076]	[19076]	[19076]	[19076]
					Panel C: Hires (asinh)			
Mandate passage					-0.092***	-0.081**	-0.011	-0.019
					(0.023)	(0.031)	(0.028)	(0.039)
Mandate enforcement	-0.081***	-0.083**	0.016	0.002	-0.001	-0.019	-0.002	-0.019
	(0.025)	(0.032)	(0.033)	(0.035)	(0.021)	(0.019)	(0.040)	(0.032)
Dep. Var. Mean	6137	6137	6137	6137	6137	6137	6137	6137
Dep. Var. SD	[19856]	[19856]	[19856]	[19856]	[19856]	[19856]	[19856]	[19856]
Observations	138,004	138,004	138,004	138,004	138,004	138,004	138,004	138,004
Year-Quarter FE	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X
Additional Controls		X		X		X		X
Residualized on Trend			X	X			X	X

Notes: The unit of observation is the county by year-quarter. Mandate passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year-quarter. Mandate enforcement is an indicator for whether any private sector E-Verify mandate has gone into effect by the end of the given year-quarter.

Specifications labelled "Residualized on Trend" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include: county-level predicted labor demand (i.e., a "Bartik instrument"), indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants, as well as an indicator that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter. In addition, we include interactions between year-quarter and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A3: ACS-Based Employment Outcomes (County Level)

	(1) Hispanics	(2) Non-Hispanics	(3) Likely Undocumented Hispanics	(4) Likely Documented Hispanics	(5) Likely Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Younger, Male Low-Skilled Natives	(9) Older, Male Low-Skilled Natives
Panel A: Residualized on Trend									
Mandate passage	-0.234***	0.004	-0.409*	-0.055	-0.394*	0.004	-0.025*	-0.043*	-0.004
	(0.045)	(0.013)	(0.231)	(0.120)	(0.210)	(0.013)	(0.013)	(0.022)	(0.016)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Residualized	X	X	X	X	X	X	X	X	X
Panel B: Residualized on Trend + Additional Controls									
Mandate passage	-0.221***	0.013	-0.415**	-0.027	-0.457**	0.007	-0.012	-0.039	0.006
	(0.042)	(0.011)	(0.198)	(0.126)	(0.187)	(0.012)	(0.017)	(0.032)	(0.010)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Addl Controls	X	X	X	X	X	X	X	X	X
Residualized	X	X	X	X	X	X	X	X	X
Dep. Var. Mean	6137.6	33626.1	1056.8	5080.7	1196.4	33010.1	11187.7	3426.7	2803.1
Dep. Var. SD	[44974.8]	[91128.51]	[8340.5]	[37103.8]	[8926.4]	[88413.8]	[25844.1]	[8613.8]	[5787.5]
Observations	34,528	34,528	34,528	34,528	34,528	34,528	34,528	34,528	34,528

Notes: The unit of observation is the county by year. Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. Each outcome value is the inverse hyperbolic sine transform of the number of employed individuals with the referenced characteristic(s). Likely Undocumented is a probabilistic measure corresponding to non-citizen respondents who have not completed high school, were not born in Cuba, are not in the military and are non-veterans, are not employed by the government, and did not arrive to the US prior to 1980. Likely Documented workers are those not classified as Likely Undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Younger corresponds to respondents aged 16-40 and Older corresponds to respondents aged 41-64.
Specifications labelled "Residualized" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include indicators for whether a state had any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants by the end of the prior year. In addition, we include interactions between year and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A4: Workers per Establishment (County and County-by-Firm Size Level)

	(1)	(2)	(3)	(4)
	Log Workers per Establishment		Workers per Establishment	
	Panel A: Benchmark Specification			
Covered x Small Firms	-0.030***		-0.104***	
	(0.008)		(0.027)	
Covered x Big Firms	-0.006		0.004	
	(0.006)		(0.731)	
Mandate Passage		-0.020***		-0.435
		(0.004)		(0.416)
County-by-Firm Size Bin FE	X		X	
Year FE		X		X
Year-by-Firm Size Bin FE	X		X	
County FE		X		X
	Panel B: Additional Controls			
Covered x Small Firms	-0.029***		-0.001	
	(0.009)		(0.231)	
Covered x Big Firms	-0.005		0.126	
	(0.006)		(0.675)	
Mandate Passage		-0.021***		-0.354
		(0.005)		(0.379)
County-by-Firm Size Bin FE	X		X	
Year FE		X		X
Year-by-Firm Size Bin FE	X		X	
County FE		X		X
Additional Controls	X	X	X	X
Dep. Var. Mean	40.3	40.2	40.3	40.2
Dep. Var. SD	[44.0]	[44.2]	[44.0]	[44.2]
Observations	62,672	31,128	62,672	31,128

Notes: The unit of observation is the firm size bin by county by year in Columns (1) and (3) and the county by year in Columns (2) and (4). Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Covered is an indicator for whether a given firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the first quarter of the given year (establishment count data is available annually for the first quarter). Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the first quarter of the given year (when annual establishment counts are constructed).
Additional controls include those introduced in Table 5.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A5: QWI-Based Hispanic Worker Outcomes (County-by-Firm Size Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	E-Verify (Query Rate)		Employment (asinh)		Separations (asinh)		Hires (asinh)	
Panel A: Residualized on Linear Trends								
Covered	0.134***		-0.057**		-0.132***		-0.122**	
	(0.020)		(0.027)		(0.047)		(0.054)	
Covered x Small Firms		0.047***		-0.026		-0.113*		-0.107*
		(0.013)		(0.031)		(0.058)		(0.064)
Covered x Large Firms		0.192***		-0.083***		-0.154***		-0.140***
		(0.030)		(0.026)		(0.044)		(0.052)
County-by-Firm Size Bin FE	X	X	X	X	X	X	X	X
Year-Quarter FE	X		X		X		X	
YearQuarter-by-Firm Size Bin FE		X		X		X		X
Panel B: Residualized on Linear Trends + Additional Controls								
Covered	0.166***		-0.098***		-0.177***		-0.171***	
	(0.023)		(0.025)		(0.043)		(0.050)	
Covered x Small Firms		0.075***		-0.070**		-0.163***		-0.160***
		(0.014)		(0.028)		(0.051)		(0.056)
Covered x Large Firms		0.224***		-0.121***		-0.197***		-0.188***
		(0.031)		(0.027)		(0.044)		(0.052)
County-by-Firm Size Bin FE	X	X	X	X	X	X	X	X
Year-Quarter FE	X		X		X		X	
YearQuarter-by-Firm Size Bin FE		X		X		X		X
Additional Controls	X	X	X	X	X	X	X	X
Dep. Var. Mean	0.086	0.086	2705.0	2705.0	640.1	640.1	670.6	670.6
Dep. Var. SD	[0.321]	[0.321]	[21826.4]	[21826.4]	[4318.9]	[4318.9]	[4473.9]	[4473.9]
Observations	226,276	226,276	252,572	252,572	252,572	252,572	252,572	252,572

Notes: The unit of observation is the firm size bin by county by year-quarter. Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). E-Verify rate is defined as the number of E-Verify queries divided by the contemporaneous total number of (Hispanic and non-Hispanic) hires; each other outcome value is the inverse hyperbolic sine transform of the given measure. Covered is an indicator for whether a given firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the given year-quarter. Specifications labelled "Residualized on Linear Trends" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include those described in Table 2. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A6: QWI-Based Non-Hispanic Worker Outcomes (County-by-Firm Size Level)

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment (asinh)		Separations (asinh)		Hires (asinh)	
Panel A: Benchmark Specification						
Covered	-0.042***		-0.104***		-0.101***	
	(0.014)		(0.023)		(0.025)	
Covered x Small Firms		-0.042***		-0.102***		-0.106***
		(0.013)		(0.024)		(0.026)
Covered x Large Firms		-0.047***		-0.115***		-0.104***
		(0.016)		(0.026)		(0.028)
County-by-Firm Size Bin FE	X	X	X	X	X	X
Year-Quarter FE	X		X		X	
YQ-by-Firm Size Bin FE		X		X		X
Additional Controls	X	X	X	X	X	X
Panel B: Residualized on Linear Trends						
Covered	-0.011		-0.045		-0.042	
	(0.017)		(0.044)		(0.051)	
Covered x Small Firms		-0.016		-0.052		-0.060
		(0.020)		(0.053)		(0.059)
Covered x Large Firms		-0.012		-0.047		-0.035
		(0.016)		(0.042)		(0.048)
County-by-Firm Size Bin FE	X	X	X	X	X	X
Year-Quarter FE	X		X		X	
YQ-by-Firm Size Bin FE		X		X		X
Additional Controls	X	X	X	X	X	X
Dep. Var. Mean	17046.8	17046.8	3213.8	3213.8	3338.2	3338.2
Dep. Var. SD	[65372.1]	[65372.1]	[11731.0]	[11731.0]	[12094.6]	[12094.6]
Observations	252,572	252,572	252,572	252,572	252,572	252,572

Notes: The unit of observation is the firm size bin by county by year-quarter. Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Each outcome value is the inverse hyperbolic sine transform of the given measure. Covered is an indicator for whether a given firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the given year-quarter. Specifications "Residualized on Linear Trends" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include those described in Table 2. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A7: CBP-Based Establishment Outcomes (County and County-by-Establishment Size Level)

	(1)	(2)	(3)	(4)	(5)	(6)
	Establishments (asinh)			Establishments, Weighted (asinh)		
	Panel A: Residualized on Linear Trends					
Covered	-0.023			-0.031*		
	(0.014)			(0.016)		
Covered x Small Estabs		-0.008			-0.016	
		(0.013)			(0.014)	
Covered x Big Estabs		-0.040**			-0.047**	
		(0.016)			(0.018)	
Mandate Passage			-0.026**			-0.015
			(0.010)			(0.015)
County-by-Estab Size Bin FE	X	X		X	X	
Year FE	X		X	X		X
Year-by-Estab Size Bin FE		X			X	
County FE			X			X
Residualized on Trend	X	X	X	X	X	X
	Panel B: Residualized on Linear Trends + Additional Controls					
Covered	-0.032**			-0.034**		
	(0.013)			(0.013)		
Covered x Small Estabs		-0.018			-0.019*	
		(0.012)			(0.011)	
Covered x Big Estabs		-0.049***			-0.050***	
		(0.015)			(0.016)	
Mandate Passage			-0.034***			-0.026**
			(0.010)			(0.013)
County-by-Estab Size Bin FE	X	X		X	X	
Year FE	X		X	X		X
Year-by-Estab Size Bin FE		X			X	
County FE			X			X
Residualized on Trend	X	X	X	X	X	X
Addl Controls	X	X	X	X	X	X
Dep. Var. Mean	1181.0	1181.0	2362.0	17409.1	17409.1	34818.2
Dep. Var. SD	[5066.2]	[5066.2]	[8111.1]	[76989.5]	[76989.5]	[129447.1]
Observations	73,776	73,776	36,888	73,776	73,776	36,888

Notes: The unit of observation is the establishment size bin by county by year in Columns (1)-(2) and (4)-(5) and the county by year in Columns (3) and (6). Establishment size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Each outcome value is the inverse hyperbolic sine transform of the referenced measure. Covered is an indicator for whether the corresponding firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the first quarter of the given year (establishment count data is available annually for the first quarter). Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the first quarter of the given year. Establishments (Weighted) scales the number of establishments in each bin by the baseline county-specific average firm size in that bin. Specifications labelled "Residualized on Linear Trends" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data.

Additional controls include those introduced in Table 5. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A8: QWI-Based Non-Hispanic Worker Outcomes as a Function of Predicted E-Verify Coverage (County Level)

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment (asinh)		Separations (asinh)		Hires (asinh)	
	Panel A: Passage-Based Coverage					
Predicted Coverage	-0.056***	-0.014	-0.128***	0.014	-0.119***	0.038
	(0.019)	(0.036)	(0.030)	(0.066)	(0.031)	(0.061)
	Panel B: Enforcement-Based Coverage					
Predicted Coverage	-0.042**	0.016	-0.104***	-0.046	-0.094***	-0.023
	(0.016)	(0.032)	(0.026)	(0.035)	(0.027)	(0.068)
County FE	X	X	X	X	X	X
Year-Quarter FE	X		X		X	
YearQuarter-by-State FE		X		X		X
Dep. Var. Mean	31355.3	31355.3	5908.3	5908.3	6136.9	6136.9
Dep. Var. SD	[103971.4]	[103971.4]	[19075.9]	[19075.9]	[19855.7]	[19855.7]
Observations	133,152	133,152	133,152	133,152	133,152	133,152

Notes: The unit of observation is the county by year-quarter. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for non-Hispanic workers. To construct the Predicted Coverage measure, we first calculate the predicted share of workers covered by E-Verify legislation that has been either passed (in Panel A) or enforced (in Panel B) by the end of the given year-quarter, as determined by the baseline (2003) firm size distribution for non-Hispanic workers. In Panel A, this measure is scaled by 0.255 for workers in small firms (with fewer than 20 employees) to account for the relative intensity of E-Verify usage across smaller versus larger firms. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.
Standard errors are clustered by state.
* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A9: ACS-Based Migration Outcomes (County Level)

	(1)	(2)	(3)	(4)
	Likely Undocumented Population		Likely Undocumented Abroad Last Year	
	All	Hispanic	All	Hispanic
	Panel A: Residualized on Trend			
Mandate Passage	-0.112	-0.034	0.378**	0.502***
	(0.092)	(0.125)	(0.151)	(0.174)
Year FE	X	X	X	X
County FE	X	X	X	X
Residualized	X	X	X	X
	Panel B: Residualized + Addl Controls			
Mandate Passage	-0.097	0.064	0.375***	0.503***
	(0.081)	(0.101)	(0.139)	(0.147)
Year FE	X	X	X	X
County FE	X	X	X	X
Addl Controls	X	X	X	X
Residualized	X	X	X	X
Mean of Dep. Var.	2098.6	1781.9	63.0	40.7
SD of Dep. Var.	[16070.2]	[14653.8]	[392.5]	[306.6]
Observations	34,528	34,528	34,528	34,528

Notes: The unit of observation is the county by year. In Columns (1)-(2), the outcome value is the inverse hyperbolic sine transform of the number of likely undocumented residents, defined as non-citizen respondents who have not completed high school, were not born in Cuba, are not in the military and are non-veterans, are not employed by the government, and did not arrive to the US prior to 1980. In Columns (3)-(4), the outcome value is the inverse hyperbolic sine transform of the number of likely undocumented residents abroad one year ago. Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. The sample is restricted to respondents aged 16-64.

Specifications labelled "Residualized" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include indicators for whether a state had any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants by the end of the prior year. In addition, we include interactions between year and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A10: ACS-Based Employment Outcomes (Individual Level Logit)

	(1) Hispanics	(2) Non-Hispanics	(3) Likely Undocumented Hispanics	(4) Likely Documented Hispanics	(5) Likely Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Younger, Male Low-Skilled Natives	(9) Older, Male Low-Skilled Natives
Panel A: Benchmark Specification									
Mandate passage	-0.113***	-0.063***	-0.167***	-0.087**	-0.165***	-0.056***	-0.059***	-0.092***	-0.039**
	(0.036)	(0.014)	(0.049)	(0.043)	(0.044)	(0.015)	(0.020)	(0.027)	(0.015)
Year FE	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X
Panel B: Additional Controls									
Mandate passage	-0.095***	-0.062***	-0.146***	-0.072***	-0.125***	-0.60***	-0.058***	-0.093***	-0.063***
	(0.024)	(0.012)	(0.043)	(0.026)	(0.038)	(0.012)	(0.013)	(0.020)	(0.017)
Year FE	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X
Addl Controls	X	X	X	X	X	X	X	X	X
Dep. Var. Mean	0.654	0.694	0.649	0.655	0.625	0.687	0.571	0.577	0.657
Dep. Var. SD	[0.476]	[0.461]	[0.477]	[0.475]	[0.484]	[0.464]	[0.495]	[0.494]	[0.475]
Observations	4,153,319	29,174,900	701,058	3,452,261	852,041	28,600,938	12,317,199	3,520,499	2,936,364

Notes: The unit of observation is the individual. Each column presents results from a logit regression of an indicator for employment on mandate passage where mandate passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. Each column restricts the sample to individuals with the referenced characteristic(s). Likely Undocumented is a probabilistic measure corresponding to non-citizen respondents who have not completed high school, were not born in Cuba, are not in the military and are non-veterans, are not employed by the government, and did not arrive to the US prior to 1980. Likely Documented workers are those not classified as Likely Undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Younger corresponds to respondents aged 16-40 and Older corresponds to respondents aged 41-64.

Additional controls include indicators for whether a state had any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants by the end of the prior year. In addition, we include interactions between year and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio.

Standard errors are clustered by state. All specifications control for gender, age fixed effects, and educational attainment fixed effects.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A11: ACS-Based Self-Employment Outcomes (Individual Level)

	(1)	(2)	(3)	(4)
	Likely Undocumented Business Income (asinh)		Likely Undocumented Self-Employment Rate	
	All	Hispanic	All	Hispanic
	Panel A: Residualized on Trend			
Mandate Passage	0.096	0.101	0.008	0.009
	(0.063)	(0.077)	(0.007)	(0.007)
Year FE	X	X	X	X
County FE	X	X	X	X
Residualized	X	X	X	X
	Panel B: Residualized + Addl Controls			
Mandate Passage	0.090*	0.089	0.008	0.009
	(0.053)	(0.061)	(0.006)	(0.007)
Year FE	X	X	X	X
County FE	X	X	X	X
Addl Controls	X	X	X	X
Residualized	X	X	X	X
Mean of Dep. Var.	$1,050	$1,037	0.072	0.072
SD of Dep. Var.	[7,181]	[6,920]	[0.259]	[0.259]
Observations	851,908	700,938	851,908	700,938

Notes: The unit of observation is the individual. The outcome measure in Columns (1)-(2) is the inverse hyperbolic sine transform of business (self-employment) income and the outcome measure in Columns (3)-(4) is an indicator for self-employment. Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. The sample is restricted to respondents aged 16-64.

Specifications labelled "Residualized" are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. Additional controls include indicators for whether a state had any legislation in place to facilitate information-sharing with federal law enforcement or to strengthen protections for undocumented immigrants by the end of the prior year. In addition, we include interactions between year and baseline state-level unemployment rate, state-level log GDP per capita, state-level log housing starts, state-level log government expenditures, and state-level household debt-to-income ratio. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A12: ACS-Based Individual and Household Annual Earnings Measures (Individual Level)

	(1) Hispanics	(2) Non-Hispanics	(3) Likely Undocumented Hispanics	(4) Likely Documented Hispanics	(5) Likely Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Younger, Male Low-Skilled Natives	(9) Older, Male Low-Skilled Natives
Panel A: Individual Wage Income									
Mandate Passage	-0.183**	-0.147***	-0.287**	-0.128	-0.301***	-0.128***	-0.114**	-0.122*	-0.113**
	(0.080)	(0.035)	(0.122)	(0.093)	(0.102)	(0.039)	(0.054)	(0.066)	(0.050)
Panel B: Individual Wage Income (Residualized on Trend)									
Mandate Passage	-0.236***	-0.051	-0.369*	-0.157**	-0.382*	-0.062	-0.098	-0.231***	-0.093
	(0.082)	(0.040)	(0.204)	(0.065)	(0.204)	(0.048)	(0.068)	(0.064)	(0.060)
Dep. Var. Mean	$19,147	$29,987	$12,501	$20,613	$12,211	$28,735	$16,044	$15,311	$26,364
Dep. Var. SD	[27,923]	[44,839]	[15,159]	[29,818]	[15,883]	[43,013]	[23,865]	[21,839]	[32,742]
Panel C: Total Household Income									
Mandate Passage	-0.100***	-0.036**	-0.173**	-0.073**	-0.178***	-0.040**	-0.040**	-0.042**	-0.037*
	(0.034)	(0.015)	(0.071)	(0.032)	(0.061)	(0.016)	(0.020)	(0.021)	(0.020)
Panel D: Total Household Income (Residualized on Trend)									
Mandate Passage	-0.038	-0.015	-0.071	-0.027	-0.058	-0.023	-0.036	-0.031	-0.026
	(0.069)	(0.018)	(0.154)	(0.047)	(0.144)	(0.020)	(0.028)	(0.024)	(0.023)
Dep. Var. Mean	$60,893	$81,562	$45,313	$64,357	$46,808	$79,316	$61,009	$65,122	$60,691
Dep. Var. SD	[55,017]	[77,483]	[37,013]	[57,700]	[40,566]	[74,946]	[56,206]	[59,362]	[50,460]
Observations	4,002,365	28,171,568	684,157	3,318,208	831,908	27,546,260	11,750,109	3,220,871	2,802,430
Year FE	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X

Notes: The unit of observation is the individual. Each outcome value is the inverse hyperbolic sine transform of annual earnings and each column is restricted to individuals with the referenced characteristic(s). Mandate Passage is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. Likely Undocumented and other groupings are as defined in Table 5. All specifications control for gender, age fixed effects, and educational attainment fixed effects. "Residualized" specifications are estimated by residualizing outcomes on treatment group-specific linear trends estimated using only pre-period data. For outcomes transformed using the inverse hyperbolic sine function, we present mean values in levels. Standard errors are clustered by state. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.