## How do Employment Verification Mandates Impact U.S. Agriculture?

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#### Abstract

Employment verification mandates require firms to prove each new hire's legal right to work in the U.S. and sanction employers who knowingly hire undocumented workers. The effects of such policies are of particular concern to the agricultural sector, which draws from a labor pool in which undocumented workers have been historically overrepresented. This paper empirically estimates the impact of state-level mandates on farm outcomes. By exploiting geographic variation in statewide E-Verify mandates implemented between 2007 and 2012, I use an identification strategy that combines both differences-in-differences and regression discontinuity techniques to isolate the effect of E-Verify on agricultural labor patterns. While addressing a number of estimation challenges, I find that worker density falls by roughly 35% at the policy-change border for states with stringent mandates. Furthermore, the average farm spends less per worker within counties adjacent to this same set of E-Verify states, suggesting some presence of spillover effects.

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## 1 Introduction

With the absence of contemporary federal immigration reform, U.S. state legislatures have been pressed to address growing public concern over illegal immigration and the employment of unauthorized workers. Following the passage of the *Legal Arizona Workers Act* (LAWA) and the U.S. Supreme Court's ruling in *Chamber of Commerce v. Whiting* (2011), a wave of universal employment verification mandates have been introduced at the state-level. The Court found that while the Immigration Reform and Control Act (1986) prevents states from bringing civil or criminal cases against employers who knowingly hire undocumented workers, states are at liberty to impose harsher penalties for non-compliant firms, such as increased fines or business license suspension (Mendoza and Ostrander, 2015). Since 2007, Arizona, Alabama, Georgia, Mississippi, North Carolina, South Carolina, Tennessee, and Utah have passed universal<sup>1</sup> employment verification mandates effective by the end of 2012 (Feere, 2012; Mendoza and Ostrander, 2015; LawLogix Group, Inc., 2012)<sup>2</sup>.

The focus of this study is to explore how E-Verify mandates affect U.S. agriculture, an industry that has historically drawn from a labor pool in which undocumented workers are overrepresented. Employment verification mandates are of particular concern for agricultural production, where there is evidence that despite the availability of visa programs<sup>3</sup>, as many as 50% of workers are undocumented<sup>4</sup>. Labor is particularly important for crops which require manual harvesting and processing, such as vegetables, fruit, tree nuts, and horticultural products (Kandel, 2008)<sup>5</sup>. Agricultural interest groups have consistently opposed employment verification legislation that does not directly address the unique labor need of U.S. farms, citing the inadequacy of the current H2-A agricultural visa program and the

<sup>&</sup>lt;sup>1</sup>For the purposes of this study, I define an employment verification mandate to be universal if it is binding for all or nearly all public and private employers in the state. I use the terms "private" and "universal" interchangeably.

 $<sup>^2 \</sup>mathrm{See}$  Figure 5. A full list of employment verification mandates relevant to this study are summarized in Table 17.

<sup>&</sup>lt;sup>3</sup>Such as the H2-A Temporary Agricultural Worker Program.

<sup>&</sup>lt;sup>4</sup>See Figure 3.

<sup>&</sup>lt;sup>5</sup>See Figure 4.

difficulties of securing domestic labor (NASDA, 2015; American Farm Bureau Federation, 2015). A common concern is that current visa programs are inflexible and slow, making it difficult for farmers to meet immediate labor demands for time-sensitive phases of production or harvest.

Given that in many states employers bound by E-Verify face non-trivial sanctions for knowingly hiring undocumented workers, its seems reasonable to expect the theoretical net effect of such policies would be to reduce demand for unauthorized labor, all else equal. Farms relying on a seasonal influx of unauthorized labor will have to substitute towards legally authorized workers or even modify production practices if feasible. Farmers argue that this process is costly and may result in a drastic fall in net farm exports and increased commodity prices.

While the existing body of research on employment verification mandates has been concerned with either migrant presence or individual-level labor market dynamics, the core analysis of this study seeks to empirically assess the impact of universal mandates applied at the U.S. state-level on production outcomes for farms. I pay particular attention agricultural labor patterns, such as the number of workers hired, labor expenditures, and worker density. The study is organized as follows: Section 2 provides a brief overview of the history of employment verification mandates in the U.S. I summarize a selection of relevant studies in Section 3. Section 4 introduces the data utilized in the empirical analysis. I explain the identification strategy and empirical methodology in Section 5. Empirical findings are discussed in Section 6. Finally, I discuss various interpretations and any implications for the findings of this analysis in Section 7. Tables containing regression results, checks for model robustness or validity, and summary statistics are contained in Appendices A, B, and C, respectively. E-Verify policy details are briefly summarized in Appendix D. Any referenced figures not appearing in text are located in Appendix E.

## 2 Background

Prior to recent interest in employment verification amongst state legislatures, the Immigration Reform and Control Act (1986) represents one of the earliest efforts by the federal government to address a rising population of undocumented workers in the U.S. labor force in the modern era. The policy agenda of IRCA is concentrated in two main provisions. The first provision grants amnesty to agricultural workers and migrants continuously living in the U.S. since 1982. The second provision makes hiring undocumented workers illegal and requires employers to verify each new hire's legal right to work in the U.S. (United States Congress, 1986). Violations are punishable by fine and employers who knowingly engage in the "pattern or practice" of hiring unauthorized workers now face increased penalties and even the possibility of incarceration.

One of the principle goals of IRCA was to reduce the flow of undocumented workers into the U.S. labor force. Several authors have investigated the effects of IRCA and its efficacy in accomplishing its stated objectives. Orrenius and Zavodny (2003) find that while amnesty did not increase migration in the long run, the employment verification mandate had no effect on the flow of undocumented workers. With respect to farm labor, the expected effect of IRCA was to be a stabilization in worker turnover trends. Farm operators would be able to substitute towards a steadier domestic source of legally authorized workers with the assistance of farm labor contractors, ultimately reducing labor market demand for unauthorized labor. Using evidence from agricultural production in California, Taylor and Thilmany (1993) find that farm labor turnover actually increased between the passage of IRCA and 1990 at an average annual rate of 5.8%. Given that the Seasonal Agricultural Worker (SAW) program of IRCA offered a pathway to legal status in the U.S. for unauthorized migrants working in agriculture for 90 days through 1996, the authors suggest that persisting migrant flows may be a result of durable operator preferences for "documented illegals" and the ability to shift IRCA liability onto farm labor contractors<sup>6</sup>. On the other hand, opponents to IRCA

 $<sup>^{6}</sup>$ As discussed in following sections, E-Verify presents a similar opportunity for employers to avoid liability

initially predicted that amnesty would lead to a mass exodus of undocumented workers out of agriculture into other sectors. Tran and Perloff (2002) demonstrate that IRCA did not induce workers to transition out of agriculture: the amnesty provision seems to have actually *increased* the likelihood of staying in agriculture for undocumented workers between 1986 and 1996 relative to workers who entered the U.S. more recently. Ultimately, IRCA proved ineffective in halting the flow of undocumented workers into the U.S. and did not prompt a dramatic reorganization of farm labor practices.

The next major development in immigration legislation was the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 (IIRIRA). Among other provisions, IIRIRA tasked U.S. Immigration and Naturalization Services with assessing the viability of several alternative employment verification systems (U.S. Citizenship and Immigration Services, 2015). One of these competing systems, the Basic Pilot Program, began in 1997 and accepted voluntary enrollment of employers in California, Florida, Illinois, Nebraska, New York and Texas. Known today as E-Verify, the system has since expanded from 1.064 to 602,621 employers enrolled nationally between 2001 and the end of 2015. Now operated by the Department of Homeland Security, employers electronically submit I-9 employment eligibility forms for prospective hires which are then checked for a match against a federal database of documents proving legal status, such as a Social Security number, U.S. passport, driver's license, or naturalization forms (U.S. Citizenship and Immigration Services, 2016). The advent of nearly instant electronic employment status confirmation significantly reduces the cost of compliance and works to encourage E-Verify enrollment for employers. It should also be noted that in the absence of an active state mandate, employers are free to enroll in E-Verify of their own volition.

A key difference between IRCA and recent developments in state E-Verify mandates is that while IRCA merely requires employers to provide I-9 documentation to the federal government, most E-Verify mandates shift the burden and liability of legal verification onto associated with hiring undocumented workers. individual employers. Furthermore, in addition to criticisms of lax enforcement, penalties for violations under IRCA are arguably milder than some of the more stringent state mandates (e.g. temporary or permanent revocation of business licensure). The loss of a business license may particularly catastrophic to farmers, who are lack much of the geographic mobility afforded to other industries. Comparing the two regimes on this characteristic alone, one should therefore expect the reduction in unauthorized labor demand to be stronger under a stringent<sup>7</sup> E-Verify mandate than under IRCA.

### 3 Literature Review

#### 3.1 Migrant Labor: Theoretical and Empirical Foundations

From a theoretical perspective, it may seem natural that the employer sanctions brought about by E-Verify mandates essentially represent some form of payroll tax that ought to reduce the demand for labor (Borjas, 2015). Employment verification mandates place the *de jure* burden of hiring undocumented workers onto employers, who must now commit to using the system for every new employee and ultimately hire from a smaller labor pool of either domestic or legally authorized migrant workers. However, neoclassical economic theory predicts that under the assumptions of a perfectly competitive labor market, the impact of an employment verification mandate would be shared equally between workers and firms regardless to which party the sanctions are applied. As the employer faces risk of fine or business license suspension, the net effect of this negative shock to labor demand should be to lower the wage and employment level for all workers in the short run, *ceteris paribus*.

In reality, there are several features of the labor market and E-Verify mandates that may result in more nuanced dynamics. First, many of the current mandates reduce or completely remove liability from firms who enroll in E-Verify, thus freeing employers from the risk

 $<sup>^{7}</sup>$ E-Verify mandates in the U.S. vary by state implementation. The differences for the purposed of this study are discussed in Section 4.2.

of hiring unauthorized workers with fraudulent documentation. Furthermore, agricultural employers may be able to further mitigate risk by hiring workers through third party labor contractors (Taylor and Thilmany, 1993). Therefore the net effect of E-Verify mandates on labor demand may be muted or absent. Second, E-Verify mandates will almost certainly have a heterogeneous regional impact on the labor pool (Borjas, 2015). Instead of a market wide reduction in employment and wages, unauthorized workers may relocate to more amenable labor markets. It is therefore reasonable to predict that outcomes for authorized workers in regions impacted by employment verification mandates ought to improve in the short run as a result of a thinning of the labor pool, while the relocation of unauthorized workers may flood the labor market of regions in which mandated employment verification is absent<sup>8</sup>. In addition to these factors, the extent to which employment verification policies influence undocumented labor availability and demand likely hinge on the intensity of compliance monitoring, the severity of punishments for non-compliant employers, and the potential for documentation fraud.

It is also worth noting several trends from the empirical literature on migrant labor. There is recent empirical evidence to suggest that compared with other demographic groups, male undocumented workers are characterized by particularly inelastic labor supply preferences. Borjas (2016) finds that the likelihood of employment for unauthorized male laborers in the U.S. is greater than both documented migrants and native born workers<sup>9</sup>. Furthermore, these labor preferences do not seem to be sensitive to fluctuations in wages and the differences between demographics are even accentuated by demographic controls. There is also evidence that migration flows of unauthorized migrants both into and within the United States has been changing over time due to a variety of factors which can be classified broadly into structural (e.g. policy changes, economic conditions) or demographic shifts. For example, the number of immigrants from Mexico living in the United States peaked around 2007 and immigration has even become net negative since the beginning of the subprime mortgage

<sup>&</sup>lt;sup>8</sup>Potential for relocation serves as the motivation for the estimation of spillover effects in Section 5.3.1.

<sup>&</sup>lt;sup>9</sup> This relationship is flipped for women: undocumented females are among the least likely to be employed.

crisis and subsequent Great Recession (Gonzalez-Barrera, 2015). Fan et al. (2015) find that the share of agricultural workers who migrate<sup>10</sup> within the U.S. has fallen by 60% over the last two and a half decades. The authors suggest that roughly a third of this decline can be explained by demographic changes of potential migrants while the remaining two thirds is a result of structural<sup>11</sup> or economic changes. These findings highlight the fact that the supply of undocumented workers in the U.S. undoubtedly responds to factors beyond the influence of employment verification mandates and seem to suggest that loosely coordinated state-level policies such as E-Verify may do little to stem the flow of undocumented workers into the U.S. labor force.

### 3.2 Policy Analysis

Finally, the present study draws heavily from a literature examining the effects of changes to either immigration or labor policy on industrial patterns and labor market outcomes. With respect to immigration policy, several authors have empirically estimated the effect of policy developments designed to discourage illegal immigration and unauthorized employment at the state or local level. Pham and Van (2010) investigate how regional implementations of anti-immigration policies<sup>12</sup> affect county employment and payroll by industry. They find that strict immigration enforcement reduces employment by 1-2% and aggregate payroll roughly by 0.8-1.9% for citizens *and* non-citizens. Although the authors are able to distinguish variation in labor market response by industry, one notable limitation of the County Business Patterns dataset is the omission of agricultural production (US Census Bureau, 2016). Similar evidence for complementarity between legally authorized workers and undocumented migrants is discussed by Hotchkiss et al. (2015), who find that an increase in the share of undocumented workers in a county leads to a slight increase in wages for domestic

<sup>&</sup>lt;sup>10</sup>A "migrant worker" is defined by the National Agricultural Workers Survey as a worker who travels more than 75 miles to work. Migrants involved in only a single crop "shuttlers" while those who work 2 or more crops are known as "follow the crop" (FTC) workers. They can be further decomposed into domestic or international migrants depending on how recently they entered the United States.

<sup>&</sup>lt;sup>11</sup>Fan et al. (2015) specifically point to stricter immigration policies and tougher border enforcement from the late 1990s through the early 2000s.

<sup>&</sup>lt;sup>12</sup>Such as immigrant exclusion from benefits or increased scrutiny by law enforcement.

workers. The authors favor an explanation in which the influx of foreign workers into menial positions enables native workers to leverage a relative advantage in communication skills for an overall boost in productivity. With respect to the impact of migrant policy change on agriculture, Kostandini et al. (2014) conduct a county level analysis targeting implementations of stringent immigration policies. Specifically, the authors find that counties that authorize local law enforcement agencies to exercise immigration control functions<sup>13</sup> experience a decrease in unauthorized labor presence as well as decreases in farm profitability and hired labor expenditures. They find little evidence that farms located in program counties substitute capital for labor, as measured by machinery assets and fuel expenditures. Another key finding of Kostandini et al. (2014) is that farms in counties adjacent to program regions are able to spend less per worker, suggesting the presence of spillover effects as workers may simply relocate to farms in nearby amenable counties, increasing the supply of farm labor.

A related branch of research in migration labor policy has explicitly focused on state-level implementations of employment verification mandates through E-Verify or similar systems to estimate their impact on undocumented worker presence and labor market outcomes. Using Current Population Survey data and a synthetic control unit approach to estimate the effect of universal employment verification mandates on the population of non-citizens, Bohn et al. (2013) find that Arizona's LAWA reduced the local population of "likely unauthorized" individuals by approximately 1.5-2%. Amuedo-Dorantes and Bansak (2012) use fixed effects differences-in-differences estimation to assess the impact of E-Verify mandates on wages and employment *within* the population of likely unauthorized workers. They find that unauthorized workers residing in states that implement universal mandates may be induced to relocate towards sectors in which either there exist exemptions or the risk of detection is less likely. The most significant shift is the movement of male workers from construction into agriculture. With respect to the present study, the findings by Amuedo-Dorantes and Bansak (2012) suggest that farms in E-Verify states may actually benefit from relative labor

<sup>&</sup>lt;sup>13</sup>Counties maintaining active agreements with U.S. Immigration and Customs Enforcement on IIRIRA's 287(g) provision U.S. ICE (2016).

supply abundance as workers shift into agricultural employment. Due to the heterogeneous nature of the recent wave of employment verification legislation at the state-level, the key insight for the purposes of this analysis is that the researcher must take care in identifying states in which the mandates apply to agricultural labor. Finally, the authors also find that wage and employment response varies by gender. Likely unauthorized women who remain in the labor pool see an increase in wages while men do not. The authors suggest that these phenomena may be driven by a number of different factors, such as the traditional labor roles migrants select into and differences in risk aversion or opportunity cost by gender. This differential impact of labor policy by gender has been consistently documented by other researchers (Orrenius and Zavodny, 2014; Borjas, 2016).

A number of theoretical models suggest that changes in immigration or labor policy leading to a dramatic reduction in the number of unauthorized workers would result in a net drain on the U.S. economy on a number of key outcomes. Zahniser et al. (2012) consider production outcomes and welfare by using a general equilibrium model to simulate shocks to the supply of unauthorized workers in the U.S. labor force. The authors find that a reduction of 5.8 million undocumented workers across all sectors of the economy would reduce agricultural output for labor-intensive crops by approximately 2-5.5%. Furthermore, GNP is expected to decline by 1% over the long run and the average real wage across all domestic workers would decrease by 0.3 to 0.6%. Devadoss and Luckstead (2011) calibrate a two-country model using the United States and Mexico to estimate the potential effect of increased resource allocation towards immigration enforcement on unauthorized labor and agricultural exports. They find that a 10% increase in either domestic or border enforcement over a 13 year period reduces unauthorized employment by approximately 8,000 to 9,000 workers, farm exports fall by roughly \$180 million USD ( $\approx \% 23$ ), and increased commodity prices for consumers in both countries. Although aggregate measures such as GDP and net exports are not directly considered in this study, the predicitons by these models contribute to a broader understanding of potential impacts due to policies aimed at reducing the presence of undocumented workers in the U.S. labor force.

## 4 Data

### 4.1 Agricultural Data

Non E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	9,619	0.06	0.02	0.00	0.30
Fuel Expenditures Share	9,090	0.11	0.09	0.01	0.56
Contract Labor Expenditures	$9,\!172$	9,289	22,481	116	$648,\!561$
Hired Labor Expenditures	$9,\!482$	26,287	$35,\!136$	173	573,026
Workers Hired	7,158	4.45	4.19	1.00	64.79
Workers Hired $\geq 150$ days	6,810	3.38	2.92	1.00	58.52
Workers Hired $< 150$ days	6,830	3.78	3.80	1.00	71.11
Expenditure per Hired Worker	7,058	5,851	$3,\!182$	168	$31,\!186$
Workers per Acre of Crops	6,620	0.26	2.40	0.00	94.00

Table 1: Agricultural Summary Statistics

E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	2,608	0.06	0.03	0.01	0.26
Fuel Expenditures Share	2,394	0.11	0.07	0.01	0.58
Contract Labor Expenditures	$2,\!449$	$9,\!625$	$20,\!190$	116	673,631
Hired Labor Expenditures	2,550	$23,\!698$	$28,\!444$	711	$341,\!587$
Workers Hired	$1,\!941$	4.33	3.07	1.00	44.92
Workers Hired $\geq 150$ days	1,769	3.56	2.62	1.00	39.38
Workers Hired $< 150$ days	1,770	3.68	2.57	1.00	41.00
Expenditure per Hired Worker	$1,\!887$	$5,\!237$	3,268	247	$26,\!582$
Workers per Acre of Crops	$1,\!826$	0.18	0.86	0.00	30.33

<sup>†</sup> County averages over all operations

To estimate the impact of E-Verify on U.S. farms, the primary data used in this study are county-level observations of agricultural aggregates drawn from the Census of Agriculture (National Agricultural Statistical Service, 2016). Taken every 5 years since 1997, the Census of Agriculture attempts to comprehensively record data for all U.S. farms in which at least \$1,000 of goods were produced or sold over the course of the census year. It should be noted that in adherence to operator privacy provisions required by U.S. law, measurements in counties characterized by a few dominant producers have been withheld so as to protect the anonymity of individual farms or operators (Title 7 U.S. Code § 2204g, 2008). Data used in this study cover all U.S. counties over four census years between 1997 and 2012<sup>14</sup>. Given changes to the Census questionnaire over time, consistent measures for the variables of interest in this study are observed at a minimum in years 2002, 2007, and 2012. For each agricultural outcome variable, I construct county averages using the aggregate measure divided by the number of operators in the county for which the variable is positive. This addresses the fact that for variables such as the total number of hired laborers, individuals who work at more than one farm may be counted multiple times (Martin, 2013). Any variable measured in U.S. dollars has been re-expressed in real terms using the Consumer Price Index for base year 1999. Summary statistics for agricultural measures considered in regression analysis are presented below in Table 1, which divides the sample by treatment status. A full set of descriptive statistics by subsample<sup>15</sup> are presented in the Appendix in Tables 11 and 12.

#### 4.2 Employment Verification Data

Data for state-level implementations of E-Verify programs are drawn from a set of documents published by the Center for Immigration Studies (Feere, 2012), the National Conference of State Legislatures (Mendoza and Ostrander, 2015), Troutman Sanders (Newman et al., 2015), and LawLogix, Inc. (LawLogix Group, Inc., 2012). Together, these documents give information regarding the timing, scope, penalties for violation, and implementation details for each employment verification mandate passed at the U.S. state-level. This information is summarized in Table 17 and Figure 1. For the purpose of this study, I define a public mandate as a state-level employment verification requirement that applies only to either state or local government employers or public contractors. Given agricultural production is typically a private market activity, I focus on universal mandates, in which E-Verify legislation is binding for all private employers. This data indicate that a wave of E-Verify

<sup>&</sup>lt;sup>14</sup>The data is accessible through the NASS QuickStats 2.0 Database: http://quickstats.nass.usda.gov/

<sup>&</sup>lt;sup>15</sup>e.g. the treatment, control, and "discontinuity" subsamples discussed in subsequent sections.



Figure 1: Status of E-Verify Mandates in the U.S. (Mendoza and Ostrander, 2015)

mandates for private employers implemented in nine states between agricultural census years 2007 and 2012. These states include Alabama, Arizona, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Utah.

There are several aspects regarding the timing and nature of the mandates that may play a critical role in structuring the analysis and interpreting any results. First, sanctions for non-compliance vary by implementation. For example, firms found in violation of North Carolina's HB36 may only face fines ranging from \$1,000 to \$10,000 whereas a single violation under the mandates in Arizona or Alabama results in the suspension of an employer's business license; a second violation carries the possibility of permanent revocation. Second, the mandates vary with respect to how each mandate is phased in and how it applies to firms of different size. Georgia, North Carolina, South Carolina, Tennessee, and Utah have phased in their universal mandates over time, typically beginning with the largest employers. Firms with 25 or fewer employees in North Carolina, 15 or fewer in Utah, and 6 or fewer in Tennessee are exempt from the mandates. Third, anecdotal evidence suggests enforcement of mandates is inconsistent across states and uniform data on enforcement is sparse (Feere, 2012). Fourth, Tennessee and Louisiana give private employers the option to either use E-Verify or retain other forms to prove legal authorization (i.e. state issued ID, birth certificate, etc.). The extent to which this choice between systems drives a meaningful difference in terms of deterring unauthorized workers may come down to the likelihood of detection for authorization fraud of one system over the other as well as an employer's willingness to take on any such additional risk. Fifth, the mandates for Alabama and North Carolina did not become effective until 4/1/2012 and  $10/1/2012^{16}$ , respectively. While workers may choose to relocate prior to the effective date and even a mid year introduction may disrupt the harvest of some crops, including Alabama and North Carolina in all specifications may be problematic. Finally, California and Illinois have actually gone as far as to block the use of E-Verify throughout the state. Although not explicitly addressed in this analysis, special attention ought to be paid to such regions that signal relative tolerance towards undocumented labor.

Given the lack of policy uniformity, the simple estimation of the impact of employment verification mandates on agriculture will only result in an average effect of E-Verify. As this may mask potential heterogeneity in the response to E-Verify across states with distinct implementation characteristics, I will address this by considering separate impacts for different treatment sub-groups. In addition to pooling the nine previously mentioned states in the broadly categorized "universal E-Verify" treatment group, I will also use a restricted group for states that provide no employer exemptions and impose the toughest sanctions for noncompliance. This group of states who impose "robust" statewide mandates includes Alabama, Arizona, Mississippi, and South Carolina. The results summarized in Appendices A, B, and C refer to these groups as "9 state" and "4 state" treatment.

 $<sup>^{16}\</sup>mathrm{North}$  Carolina's E-Verify mandate beginning on 10/1/2012 only applies to firms with 100 to 500 employees

#### 4.3 Other Data

I use several other data sources to answer peripheral questions and construct covariates for the core analysis. A micro-level survey over a representative sample of crop workers in the U.S., the National Agricultural Workers Survey (NAWS), is used for Figures 3 and 4 to gain a better sense of the distribution of unauthorized labor in U.S. agriculture across crops and time (United States Department of Labor, 1989 - 2012). Data used to assess the political party composition of state legislative bodies come from the National Conference of State Legislatures (Mendoza and Ostrander, 2015). Agricultural and state GDP come from the Bureau of Economic Analysis (2016). Data used to construct measures of average annual statewide unemployment rates are compiled by the U.S. Bureau of Labor Statistics (2016) from the U.S. Census Bureau's Current Population Survey (CPS). Spatial data for U.S. administrative boundaries comes from the U.S. Census Bureau (2015).

## 5 Methodology

#### 5.1 Differences-in-Differences

To identify the effect of E-Verify mandates on farm outcomes, I employ a differences-indifferences (DD) strategy using the standard fixed effects model outlined by Bertrand et al. (2002) and Angrist and Pischke (2008),

$$Y_{ist} = \gamma_s + \lambda_t + \mathbf{X}'_{ist}\boldsymbol{\beta} + \delta E V_{st} + \epsilon_{ist}$$
(5.1)

where  $Y_{ist}$  is some agricultural outcome measure for county *i* in state *s* at time *t*. Summarized in Table 1, measures considered include workers hired, labor expenditures, worker density, and expenditure shares in fuel and labor<sup>17</sup>.  $\gamma_s$  and  $\lambda_t$  are state and year fixed effects.  $\mathbf{X}_{ist}$  is a vector of time-varying county and state-level covariates<sup>18</sup>. The policy regressor of interest,

<sup>&</sup>lt;sup>17</sup>Measures consistent with those used by Kostandini et al. (2014).

 $<sup>^{18}</sup>$ See Section 5.1.2 for a discussion of covariate choice.

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 $EV_{st}$ , is a dummy equal to 1 if all private and public employers in state s must comply with an E-Verify program at time t.  $\hat{\delta}$  will estimate the impact of a universal E-Verify mandate on  $Y_{ist}$ . To account for the possibility of error correlation within counties belonging to the same state, standard errors are clustered at the state-level (Bertrand et al., 2002).

This approach is confronted with several limitations given the nature of the data and experimental setting. First, it would be ideal to exploit variation in the timing in both the passage and implementation of employment verification mandates across states (Amuedo-Dorantes and Bansak, 2012; Orrenius and Zavodny, 2014). This is impossible given the fact that all E-Verify mandates were phased in between the 2007 and 2012 Census of Agriculture observations. Therefore the results of this study give an average E-Verify impact between 2007 and 2012, which may obscure dynamic effects for states with different implementation schedules. Second, the limited number Agricultural Census years<sup>19</sup> make the strategy of accounting for dependent variable trends in a parametric fashion unattractive<sup>20</sup>. Angrist and Pischke (2008) suggest that controlling for a strictly linear time trend using too few observation periods may lead to an unreliable estimate of the true underlying trend. Furthermore, although a natural solution would be to include state-year fixed effects to account for time-varying influences for each state (e.g. other relevant policies), such controls are omitted over concerns of collinearity with the E-Verify treatment. Finally, the Census of Agriculture questionnaire changes over time. While the measures included in this study are consistent between Census years, some of the most interesting farm characteristics such as the number of migrant workers have only been asked in the most recent Census.

The final aspect of employment verification that warrants attention is that should E-Verify requirements influence how farm operators report labor measures to the Census of Agriculture, there arises potential for biased estimation of the treatment effect through measurement  $\operatorname{error}^{21}$ . If farmers continue to illicitly employ undocumented workers despite

 $<sup>^{19}</sup>T = 3$  or 4 depending on the measure in question.

 $<sup>^{20}</sup>$ I do include nonparametric trends in future specifications. See Equation (5.3).

<sup>&</sup>lt;sup>21</sup>Formally, let  $\tilde{y} = y + v$  where  $\tilde{y}$  is the outcome reported to the Census of Agriculture, y is the true underlying measure, and v represents measurement error. Bias arises if there is reason to assume that v may

E-Verify, operators who may have included unauthorized workers in labor totals reported for earlier Censuses are now strongly incentivized to under-report. Furthermore, the issue of measurement error in the case of undocumented workers may extend well beyond the E-Verify context considered in this study, as it is difficult to accurately measure labor characteristics in industries that may feature significant informal employment using only data provided by employers<sup>22</sup>. Without a viable instrumental variable to address these issues presented by measurement error, I present the DD estimates while acknowledging the potential presence of bias.

#### 5.1.1 Inference

Prior to making any statements regarding causality, this identification strategy requires several additional justifications. The first concern is that of treatment assignment exogeneity. I address this requirement in two ways. First, five out of the nine private employer E-Verify mandates came into effect following the Supreme Court's 2011 ruling in *Chamber* of Commerce v. Whiting, where the Court found that the provisions of IRCA did not preempt states' rights to impose tougher sanctions against employers that knowingly hire undocumented workers. For this reason, I argue that the wave of E-Verify adoptions in the U.S. is largely an exogenous shock of states following Arizona's pioneering LAWA and ultimately gain judicial approval from the Whiting ruling, as states with inherent proclivities for employment verification mandates are suddenly legally permitted to implement them. This argument is less than ideal, however, as several of the mandates come into effect before the Whiting ruling and I therefore supplement the previous assertion with a second approach to give the E-Verify impact estimates a causal interpretation. By including covariates that may influence the likelihood that a particular state passes an E-Verify at a given point in time strengthens the argument that treatment is independent conditional on these factors (Angrist and Pischke, 2008). I argue that controlling for characteristics such as the share of a

be correlated with the passage of E-Verify (Pischke, 2007).

<sup>&</sup>lt;sup>22</sup>A common solution is to fortify the analysis with micro-level data to assess migrant presence (Amuedo-Dorantes and Bansak, 2012; Kostandini et al., 2014).

state's output represented by agricultural production, rate of unemployment, and prevailing political ideology within the state legislative body for better comparisons between states that are more likely to differ only with respect to the passage of a universal employment verification mandate. The selection of covariates such that treatment satisfies this so-called conditional independence assumption (CIA) are discussed in more detail in Section 5.1.2. Furthermore, state and time fixed effects control for unobserved differences that do not fluctuate over both time and states within the panel.

A second requisite for a causal interpretation of the DD approach is the validity of the so-called "parallel trends" assumption for each dependent variable. The DD estimate may be confounded should any pattern in the county-level farm outcomes of interest be diverging (or converging) between E-Verify and non E-Verify states *prior* to the passage of the mandates. To this end, I conduct simple t-tests on differences between the treatment and control groups in mean changes from 2002 to 2007 for each farm outcome variable of interest (i.e. a pre-treatment DD). There are several dependent variables that violate this requirement across all specifications and are therefore excluded from the regression analysis<sup>23</sup>. For example, changes in net farm cash are statistically different (p < 0.05) between E-Verify states were experiencing a decrease in farm profitability compared with an average increase in the control group. The results of these tests for the variables relevant to this analysis are summarized in Tables 9 and 10. Note that while some farm attributes summarized in these tables show non-parallel trends for a small number of subgroups, I avoid making causal statements for these groups when interpreting the results of the empirical analysis in Section 6.

As a third requirement, to properly interpret the estimated effect of E-Verify it is critical to establish a clear chain of causality between the change in employment verification policy and the farm level outcome. This study predicates on the fact that it is the introduction

 $<sup>^{23}</sup>$ Note that while it is possible to recover an unbiased (or at least less biased) treatment effect for farm outcomes with non-parallel trends, focusing on variables that satisfy this assumption simplifies the direct interpretation of the DD estimate.

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of E-Verify mandates driving any variation in farm labor and expenditure patterns, not the reverse. I test this assumption in the framework of Granger (1969) using an adaptation to the DD setting suggested by Angrist and Pischke (2008). If the assumed direction of causality is indeed correctly specified, one should expect no effect of simulating the counterfactual introduction of E-Verify prior to 2012 (i.e. a placebo treatment). I estimate the DD model with the addition of a single E-Verify policy "lead" for Census year 2007 in an effort to prevent any confounding influences to bias estimates of the treatment effect<sup>24</sup>. The coefficient estimates for the placebo treatment coefficient are summarized in Tables 6, 7, and 8. We can see from these results that there are indeed already a number of unobserved factors that are driving farm level outcomes within the states who go on the pass employment verification mandates. Together, these results indicate that we cannot simply take the estimates from Appendix A at face value as the true causal impact of E-Verify on farms. Although this precludes a simple interpretation of the DD results, one may still estimate a *net* effect of E-Verify policy given these pre-treatment differences. I discuss this process further in Section 6.

Finally, this DD setting requires that there exist no other unobserved factors varying across counties and time that are correlated with both the agricultural outcome of interest and the passage of E-Verify. For example, by omitting state-year fixed effects a causal interpretation of this model implicitly assumes that E-Verify is the *only* development that influences farm outcomes. Although the prudent selection of covariate controls seeks to address this problem, it may be infeasible to completely mitigate this source of omitted variable bias. Given the challenges of modelling every factor that might possibly influence either E-Verify or agricultural outcomes, it is this final assumption that requires the greatest leap of faith. While the true underlying E-Verify impact may differ from the results presented in Appendix A, I argue that the estimated effects presented in this study offer a reasonable approximation of average overall effect.

 $<sup>^{24}</sup>$ It is worth mentioning that although absent in both Equation (5.1) and the description of subsequent models, I include an E-Verify policy lead for *every* model estimated in this analysis.

#### 5.1.2 Covariate Selection

Non E-Verify	Ν	Mean	SD	Min	Max
Acres of $Cropland^a$	9,563	309	323	1	3,055
Republican Legislature	9,564	0.47	0.50	0	1
Split Legislature	9,564	0.27	0.44	0	1
State GDP $(Farm)^b$	9,936	$2,\!151$	2,058	8	$14,\!185$
State $GDP^b$	9,936	$327,\!483$	$333,\!451$	$19,\!671$	1,602,723
State Unemployment	$10,\!248$	0.06	0.02	0.03	0.17

Table 2: Covariate Balance

E-Verify	Ν	Mean	SD	Min	Max
Acres of Cropland <sup><math>a</math></sup>	2,617	171	206	2	2,070
Republican Legislature	$2,\!628$	0.43	0.50	0	1
Split Legislature	$2,\!628$	0.12	0.32	0	1
State GDP $(Farm)^b$	$2,\!628$	1,212	703	268	$3,\!175$
State $GDP^b$	$2,\!628$	$216,\!607$	$97,\!699$	69,074	$352,\!314$
State Unemployment	$2,\!628$	0.07	0.02	0.03	0.11

<sup>a</sup> Per operation average

<sup>b</sup> In millions USD (1999)

I select a set of observable county and state varying factors that may potentially influence farm outcomes to include as covariate controls. For each county, this set of controls include average acres of crop land and the geographic coordinates of a county's centroid. I also include time-varying state covariates including the political party composition of the state legislature, statewide GDP, agricultural GDP, and average annual unemployment measures in the agricultural Census year.

In addition to assessing the robustness of the E-Verify treatment effects to external factors, these covariates are chosen for two primary purposes. First, as the conditional independence assumption for treatment assignment is invoked for the sake of robust estimation, I include factors that contribute to the likelihood that a state adopts an employment verification mandate. Since these mandates are passed in the state legislature, I include dummies to indicate the political party composition of each state during the Census year. Specifically, I include a pair of dummies equal to 1 if the state legislative body is either predominantly Republican or split between multiple political parties. Controlling for a prevailing political ideology within state legislative bodies is motivated by the fact that the passage of E-Verify mandates demonstrate moderately strong positive correlation with Republican-controlled state legislatures (r = 0.37). It also seems reasonable that states with large agricultural sectors would resist efforts to pass employment verification legislation. I therefore control for both agricultural and aggregate state real output. Finally, I also control for average annual statewide unemployment as demand for E-Verify may be stronger in states experiencing significant levels of unemployment and a desire to protect native workers.

A secondary rationale for including county level covariates would be to draw better comparisons between appropriate sets of control and treatment counties through matching process of least squares regression. This may ultimately lead to more precise estimates of the treatment effect. To this end, I control for average farm-level acreage in cropland as well as the latitude and longitude of each county's centroid. Although these attributes feature little or no variation over time, they improve the ability of regression mechanics to match counties differing only with respect to the prevailing state employment verification regime.

To avoid introducing bias through the inclusion of so-called "bad controls" (Angrist and Pischke, 2008), I consciously omit time-varying county attributes that although attractive for other reasons, may be influenced by E-Verify adoption itself. Such measures might include county unemployment rates or acreage in the production of labor-intesive crops<sup>25</sup>. Statewide measures are preferred given they are arguably less sensitive to any influences of E-Verify on average. For the nine state E-Verify specification, a summary of covariate balance between the program and non-program subsamples is shown in Table 2. A similar summary for the four-state specification can be seen in Table 16

 $<sup>^{25}</sup>$ Although not utilized in the present study, one might address this concern by including a measure of the covariate at some pre-treatment baseline.

#### 5.1.3 Labor Intensity

It is reasonable to assume that counties characterized by labor intensive agriculture may respond differently to E-Verify compared with counties in which labor is a less critical input. The geographic distribution of farm labor expenditure shares in Census year 2002 can be seen in Figure 6. Figure 7 further illustrates the level of variation in labor intensity in the southeastern U.S., a region characterized by a cluster of E-Verify states. Such variation in farm labor requirements may mask a differential response by farm labor requirements when only allowing for a single E-Verify effect estimate for all farms. For this reason I distinguish between counties that are deemed either "labor intensive" or "labor mild". I proxy for labor intensity using labor expenditures as a share of total operating expense in year 2002. I define a county to be labor intensive if the countywide labor expenditure share exceeds  $0.2^{26}$  in Census year 2002. The distribution of labor intense counties within the sample is depicted graphically in Figure 8. I estimate the preferred DD model<sup>27</sup> with an additional interaction between the E-Verify policy and labor intensity dummies. The results for this set of regressions are summarized in Table 3 and discussed in Section 6.1.

#### 5.2 Spatial Regression Discontinuity

In addition to the standard DD approach, I consider methods from a second identification strategy exploiting the fact that while E-Verify policies are enacted at the state-level, farms situated on either side of a border between an E-Verify state and a non E-Verify state are arguably quite similar along a number of unobserved characteristics relevant to agricultural production. This setting is well-suited for the application of techniques from regression discontinuity (RD) design<sup>28</sup>, in which the treatment status for county i,  $EV_{st}$ , is discontinuously determined by the state s in which i is located and year t while other unobserved factors contributing to the farm outcome measure  $Y_{ist}$  are thought to vary continuously across the

<sup>&</sup>lt;sup>26</sup>Approximately one standard deviation greater than the mean labor expenditure share for farms in 2002. <sup>27</sup>See Equation (5.3).

<sup>&</sup>lt;sup>28</sup> The variant of regression discontinuity considered in this study is alternatively termed either "border fixed effects" or "geographic regression discontinuity" in other literatures.

"policy-change border". Such factors may include crop suitability characteristics such as climate, soil quality, and elevation. While the identification of the E-Verify effect on farm labor in this study ultimately comes from differences-in-differences, I use this fact to foster control-treatment comparisons between farms that are less likely to differ on any number of unobserved characteristics. This approach is a refinement of the generic DD approach that aims to tease out potential heterogeneity in E-Verify response that may be masked by the estimation of a statewide average effect.

A number of authors in the traditional econometric literature exploit exogenous placement of borders to assess treatment effects for variation in regional policy. Holmes (1998) analyzes the location decisions of firms in states with right-to-work laws compared with prounion states, finding sharp increases in manufacturing activity around state borders when crossing into a right-to-work state. Black (1999) uses school attendance zones to estimate the school quality premium paid by homeowners within the same school district. She finds a positive increase in home values on homes just inside the attendance zones of higher quality schools. Pence (2006) finds that after comparing adjacent census tracts in neighboring states, lenders in states with foreclosure laws favoring the defaulter offer loans 3 to 7% smaller on average compared with all other states.

Prior to describing the preferred DD model ultimately throughout the regression analysis, I begin by considering a simple form of the RD model as described by Angrist and Pischke (2008):

$$Y_{ist} = \alpha + f_T^p(D_i) \times T_i + f_C^p(D_i) \times C_i + \delta E V_{st} + \epsilon_{ist}$$
(5.2)

Consistent with spatial applications of regression discontinuity designs (Keele and Titiunik, 2015), I define the variable determining a county's E-Verify treatment status (i.e. the forcing variable or score) as the distance from a county's centroid to the PCB,  $D_i$ . In this model,  $f_j^p(D_i) = \pi_{1j}D_i + \pi_{2j}D_i^2 + \ldots + \pi_{pj}D_i^p$  is a *p*-th order polynomial function of distance from county *i* to the policy-change border for treatment group  $j \in \{T, C\}$ . In other words, the model includes separate forcing variable functions for the treatment  $(T_i = 1\{i \text{ is in an E-Verify state}\})$  or control  $(C_i = 1\{i \text{ is in a non E-Verify state}\})$  side of the policy-change border. This allows distance from county i to the policy-change border to influence the effect of E-Verify on either side in a potentially nonlinear fashion.

#### 5.3 Reconciling Identification Strategies

While appropriate for cross sections, the simple regression discontinuity specification discussed thus far has ignored the fact that the E-Verify mandates within the sample are also a function of time. Because the employment verification regime is *jointly* determined by the year of observation and distance to the policy-change border, we cannot simply estimate the standard RD model described in Equation (5.2) using the entire county-year panel. I consider three alternative approaches to integrate the attractive properties of the RD design with DD identification. First, it would be possible to augment the RD model in Equation (5.2) with a complete set of time dummy interactions to reflect the fact that the wave of E-Verify mandates come into effect in 2012 for this sample. Second, the model Equation (5.2) can be estimated twice: first for Census year 2012 and then for years prior to 2012. The difference between these two regressions ought to isolate the treatment effect of E-Verify for farms along the policy-change border. All else equal, one should expect no discontinuity at the policy-change border for Census years 2007, 2002, and 1997. The final and preferred approach would be to estimate a DD model similar to Equation (5.1) on the subset of counties located within a neighborhood of the policy-change border, what Angrist and Lavy (1999) call the "discontinuity sample"<sup>29</sup>. In this way, the "DD along the policy-change border" specification retains the same interpretation as the original DD approach but estimates the impact of E-Verify on a set of counties that, conditional on observed covariates and fixed effects, are far more likely to differ only with respect to employment verification policy. I

<sup>&</sup>lt;sup>29</sup>In a spatial context, Keele and Titiunik (2015) refer to this group as the "naïve distance" sample.

use the following model for the remainder of the regression analysis:

$$Y_{ibst} = \gamma_s + \lambda_t + \phi_{bt} + g(D_i, \pi_j, T_i, C_i) + \mathbf{X}'_{ist}\boldsymbol{\beta} + \delta E V_{st} + \epsilon_{ibst}$$
(5.3)

In this model,  $\phi_{bt}$  is a vector of time-varying border segment dummies<sup>30</sup>. Border segment dummies are constructed by dividing the policy-change border into 100 mile segments (n =44 for the nine state specification), then associating each county with the nearest border segment, b. The inclusion of  $\phi_{bt}$  is designed to nonparametrically account for unobserved time-varying factors common to counties along shared border segments that may not be captured by simple distance alone. This set of fixed effects is preferable to the inclusion of state-year dummies, given such a specification would be collinear with the introduction of the E-Verify mandates. Motivated by the concerns associated with choosing the appropriate higher-order polynomial for  $f(D_{is})$  raised by Gelman and Imbens (2014), I opt for a model that is locally linear in the forcing variable  $D_{is}$ . I therefore define a function g, continuous in  $D_{is}$  for each treatment group, as follows:

$$g(D_i, \pi_j, T_i, C_i) = \pi_T D_i \times T_i + \pi_C D_i \times C_i$$

The remaining regression analysis proceeds as follows: after estimating Equation (5.3) with each dependent variable using the full sample to obtain a baseline E-Verify effect, I estimate the same model on a restricted set of counties counties within a certain distance bandwidth around the policy-change border. I follow the sample-driven bandwidth selection methods described by Imbens and Kalyanaraman (2012), which suggest an optimal spatial neighborhood about the policy-change border of approximately 150 miles for all dependent variables considered<sup>31</sup>. I also use 100 and 200 mile bandwidths as robustness checks. The policychange border and the counties located within the set of chosen bandwidths are represented

 $<sup>^{30}</sup>$ Adapted from Dell (2010).

<sup>&</sup>lt;sup>31</sup>Conditional upon the set of labor outcomes,  $Y_{ibst}$ , and county distances from the policy-change border,  $D_i$ .

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in Figure  $2^{32}$ . I estimate this model over the full nine state treatment specification as well as for the group of four "robust mandate" states.





#### 5.3.1 Spillover Effects

One might expect that the cost of relocation for undocumented workers impacted by E-Verify mandates along a policy-change border is low: workers may simply seek work in a neighboring state unbound by employment verification mandates. If farm laborers in counties along the policy-change border are indeed migrating from E-Verify counties to non E-Verify counties, the estimates from Equation (5.3) reflect net differences between the control and treatment groups in the discontinuity sample that may overstate the actual treatment effect for program counties. To refine these estimates, I estimate spillover effects from E-Verify using the set of control counties in non E-Verify states that are within 200 miles of the policy-change border. I

<sup>&</sup>lt;sup>32</sup>Nine-state treatment specification. Both specifications are used in regression analysis.

divide this group into "border" counties (within [0, 100) miles) and "interior" counties (within [100, 200) miles). This subsample is mapped for the nine state specification in Figure 9. The differences between border and interior farms in this control group subsample isolates any spillover effects driven by employment verification mandates as workers flow from E-Verify states into adjacent non E-Verify regions. I estimate E-Verify spillovers for this subsample with differences<sup>33</sup> using a modified version of Equation (5.3) in which border counties receive "treatment" in Census year 2012. These estimates are contained in Table 5 while the 2007 placebo coefficient can be found in Table 8.

### 6 Empirical Results

The results for the simple DD estimation<sup>34</sup> of the impact of E-Verify on farm outcomes are summarized in Table 3. This table also summarizes the coefficients that allow the E-Verify effect to vary by farm labor requirements. The results for estimating the DD model on the discontinuity sample<sup>35</sup> are summarized in Table 4. Estimates for spillover effects into non E-Verify states are presented in Table 5. The coefficient for the anticipatory 2007 E-Verify placebo are presented for each of these three sets of models in Tables 6, 7, and 8.

As mentioned in Section 5.1.1, the results of the placebo tests indicate non-trivial effects amongst the set of counties within states adopting E-Verify mandates prior to the actual passage of the mandate. This suggests that the model fails to capture some unobservable influences varying between program and treatment counties, violating our assumption that E-Verify is the only regressor outside of the covariate specification contributing to changes in farm outcomes. Such unobserved influences certainly bias the coefficients estimated in Tables 3 and 4. However, it is still possible to recover a reasonable estimate of the mandate effects after netting out these influences. I therefore interpret any estimates from Tables

 $<sup>^{33}</sup>$ It is also worth noting that from the perspective of this set of control counties, any spillover effect attributed to the adoption of E-Verify by a neighboring state is truly an exogenous shock that requires little additional justification. This fact leads to easier causal interpretation of the DD estimate, assuming all other requirements hold.

 $<sup>^{34}</sup>$ Equation (5.3) using the entire sample.

 $<sup>^{35}</sup>$ Equation (5.3) using various sample bandwidths.

3 and 4 after subtracting the E-Verify 2007 placebo estimate from the actual 2012 policychange coefficient. I do not apply this correction for cases in which the placebo coefficient is not statistically significant. In the sections that follow, I discuss several of the more interesting estimates for the impact E-Verify on labor patterns in U.S. agriculture. I restrict my attention to coefficients with statistical significance beyond conventional levels (p < 0.05) and strive to place each effect into context.

#### 6.1 Labor Intensity

The results in Table 3 shed some light on how E-Verify affects farms with differing labor requirements. Labor intense farms in the four state specification (Alabama, Arizona, Mississippi, and South Carolina) are particularly impacted by employment verification. For this group, the average farm in labor intense counties spend roughly  $21\%^{36}$  less on hired workers compared with labor mild peers following the adoption of E-Verify. Similarly, labor intense farms hire significantly fewer workers following E-Verify adoption compared with labor mild farms in the four state specification. These operators hire 21% (1.3 fewer hired workers overall compared to the average farm in the four-state specification. This effect can be decomposed into 4% fewer workers hired for 150 days or more and 10% fewer hired for less than 150 days<sup>37</sup>. Unsurprisingly, the combination of decreases in employment and worker expenditures for the labor intense subsample indicates that the extent to which E-Verify causes farms to scale back labor-oriented production varies significantly by farm type. While these results are statistically significant and demonstrate that the average E-Verify effect estimated for all farms obscures a heterogeneous response by farm labor requirements, the magnitude of the effect in levels seems somewhat trivial for the average farm. The extent to which this effect for labor intense counties is valid for all agricultural operations in the U.S. may be limited. As we can see from Figure 8, labor intensive counties are far from randomly distributed across the nation and are often clustered near urban centers or coastal regions.

 $<sup>36</sup>e^{-0.233} - 1 = -21\%$ 

<sup>&</sup>lt;sup>37</sup>Note that the estimate for workers hired for 150 days or more has been adjusted using the statistically significant estimate from the placebo regressions.

Perhaps most importantly for the purposes of this study, the locations of these farms also seem to correlate strongly with areas in which foreign born individuals are overrepresented compared with the nation as a whole. At best, I contend that the additional response of labor intense counties in the four state specification to E-Verify reflects the fact that this subpopulation shares a common set of characteristics sensitive to employment verification policy.

#### 6.2 Border Effects

When considering the results of the border effects model (Table 4), some of the more interesting cases are instances in which the estimated E-Verify effect switches directions from the 2007 placebo to the 2012 actual post-treatment period. For example, the worker density coefficient for counties within 100 miles of the four state policy-change border switches from 0.114 to -0.317, suggesting that the net impact of E-Verify was to actually decrease worker density per acre of crops in program counties by approximately 35%<sup>38</sup>, on average *ceteris paribus.* With respect to the subsample mean for this group, this roughly translates to the average E-Verify farm employing 2 fewer workers per 100 acres of cropland compared to the an average of 5, all else equal. Within the same set of counties, the average farm hires 27%(or 1) fewer short term worker compared with to their peers across the policy-change border, relative to a mean of 3.1. There is also some mixed evidence across both treatment group specifications that E-Verify resulted in farms hiring fewer workers overall. While these effects demonstrate statistical significance, they are often relatively small in levels and may have little impact on farm operations<sup>39</sup>. For example, the RD models suggest that within 100 miles of the 9 state policy-change border, the number of workers hired for 150 days or more declines by 5%. However, this effect is trivial in levels and does not translate to even a single worker relative to the average. The 4 state specification is also characterized by statistically significant but economically trivial impacts. With the understanding that this model does

 $<sup>{}^{38}</sup>e^{(-.317-.114)} - 1 = -35\%$ 

<sup>&</sup>lt;sup>39</sup>Furthermore, as demonstrated with the results of Table 3, expenditure and worker declines for large-scale operations highly dependent on labor are considerably greater than the average.

not capture employment in any informal agricultural sector, these average effects most likely understate the true impact of E-Verify if there is reason to suspect that farms do not report the full number of unauthorized workers they employ.

#### 6.3 Border Spillovers

The results from the spillover models are designed to further disentangle the true E-Verify treatment impact from the border effect estimates discussed in the previous section. This is motivated by the concern that the simple estimation of E-Verify impact along the policychange border may overstate the impact of employment verification mandates for farms in program counties, as workers may simply move from the affected region into the control group. Table 5 suggests the presence of several statistically significant spillovers from E-Verify for the four state treatment group. First and perhaps most interesting is that for the four state treatment group, expenditures per hired worker falls by 11% for border counties in non E-Verify states when compared with interior counties. This is the only set of models within the present study to find a statistically significant change in expenditures per worker following the adoption of E-Verify and the result is consistent with a theory that farms in border counties enjoy an influx of cheap labor from policy-induced worker relocation. Furthermore, these farms spend 15% less on hired labor to complement this effect. A second interesting result is that worker density falls by 27% for border counties on average, compared to a subsample mean of 5 workers per 100 acres. Combining this from the decline in worker density estimated from the border effects models, worker density declines in *all* counties within 100 miles of the four state policy-change border relative to interior control counties but the decline is greatest amongst farms under E-Verify<sup>40</sup>. This would suggest that workers leaving field work in E-Verify counties are not simply moving to the nearest farm unaffected by employment verification.

 $<sup>^{40}\</sup>mathrm{Worker}$  density for E-Verify in this group is roughly 52% lower in 2012 compared with the interior county average.

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## 7 Discussion

In sum, this study has been an empirical exercise to assess the influence that employment verification mandates have over agricultural labor patterns. I estimate this effect within a quasi-experimental framework, exploiting a series of private-employer E-Verify programs adopted by U.S states between Agricultural Census years 2007 and 2012. This study differs from previous efforts given it is the first of its kind to empirically analyze this impact specifically for U.S. farms. In addition to using a standard differences-in-differences approach with county-level observations for all U.S. farms, I also examine the E-Verify effect through variation in labor dependence and treatment intensity. My preferred identification strategy compares counties located near borders between E-Verify and non E-Verify states, exploiting the fact that farms in these regions are more likely to differ *only* with respect to employment verification policy relative to comparisons made between arbitrary counties drawn from the national sample.

I find mixed evidence that E-Verify changes labor patterns for the average U.S. farm. While there seems to be no strong evidence to suggest that E-Verify results in a dramatic reorganization of agricultural labor for all program states, I contend that the results presented in this analysis suggest that the estimation of an overall average E-Verify effect masks a considerable amount of response heterogeneity. I am able to isolate localized, statistically significant E-Verify effects in two ways. First, I estimate a set of models that allow a separate E-Verify impact for farms highly dependent on labor. Second, I estimate the DD model using a set of comparable counties located along policy-change borders. I investigate such border effects further by estimate a set of spillover effects. Furthermore, these findings are sensitive to the set of E-Verify states considered.

There are several noteworthy conclusions that can be drawn from the empirical analysis of the E-Verify effect in this study. First, the treatment effect of E-Verify is most noticeable when considering a treatment group consisting of states featuring tough mandates: Alabama, Arizona, Mississippi, and South Carolina. The remaining conclusion discuss findings for this 'robust E-Verify" treatment sub-group. Second, E-Verify has a greater impact along many outcomes amongst farms that are classified as labor intense. Third, worker density falls by approximately 35% for farms impacted by E-Verify compared to the average county within 100 miles of the policy-change border. Moreover, worker density falls for all counties within 100 miles of the border, but the decline for E-Verify counties is the greatest ( $\approx -50\%$ compared with the average for non E-Verify interior counties). Finally, the analysis of E-Verify spillovers indicates that border counties in non-program states spend less per hired worker following E-Verify. The results of this analysis have been cautiously interpreted, given robustness checks suggest existence of unobserved influence that may shade the true influence of statewide E-Verify implementations on U.S. farms.

#### 7.1 Implications for future research

I conclude by discussing some limitations of this study, potential confounding factors, and implications for future research. Perhaps most notably, the identification strategy used in this paper strongly assumes that E-Verify is the only policy change common to the group of treatment states. Given the litany of state policy changes or other factors occurring between 2007 and 2012, this assumption may not hold. Any further analysis ought to account for a broader range of policy developments across U.S. states with the potential to affect the agricultural labor pool or other farm practices. Barring the consideration of different treatment subgroups, another limitation of this study is that it treats each employment verification implementations as equal. A more thorough reading of each policy might uncover some insight on how these various mandates differ across states. Third, the results of this analysis concede the possibility of bias through measurement error. Future researchers who revisit this empirical framework will certainly benefit by identifying some exogenous factor with which the passage of E-Verify may be instrumented. Finally, this analysis has not directly addressed the broader fiscal impact of E-Verify. Subsequent studies would do well

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to relate the E-Verify effect estimates discussed in this paper with more direct measures of industrial well-being, such as farm output and profitability.

This study provides guidance for future investigations of the E-Verify phenomenon. First, sensitivity to treatment group specification demonstrates that E-Verify policy is far from uniform. States vary considerably with respect to mandate design and enforcement, which ultimately determines the effect of each policy. Specifically, subsequent research would be enriched by acknowledging the wealth of variation in both state policy and agriculture that influences the overall effect of each mandate, as is apparent from the estimates in Section A. Second, future research would also benefit by exploring alternative data sources<sup>41</sup> or revisit this study at a point in time when the Census of Agriculture yields enough observations for more direct measures, such as the number of workers hired by migration status.

Until there is decisive federal action to regulate the employment of undocumented workers, employment verification will continue to evolve within the United States at the state and local levels. Each state mandate is a moving target, as legislatures strive to balance public demand for employment verification with the interests of industries such as agriculture, who are often highly dependent on the flexibility and availability of unauthorized labor. This study has shown that there are clearly meaningful impacts of E-Verify in localized contexts and it would be prudent for future innovations in employment verification policy to consider the unique labor needs of U.S. farms.

<sup>&</sup>lt;sup>41</sup>The National Agricultural Workers Survey, USDA's Cropland Data Layer, annual state-level agricultural statistics collected by NASS, etc.

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Table

	9 S.	TRE TREATMENT	4 ST	ATE TREATMENT
Outcome	E-Verify	E-Verify $\times$ Labor Intense	E-Verify	$E-Verify \times Labor Intense$
Hired Labor Expenditures <sup><math>ab</math></sup>	-0.021	-0.131	-0.013	$-0.233^{***}$
	(0.048)	(0.085)	(0.073)	(0.081)
Contract Labor Expenditures <sup><math>ab</math></sup>	0.069	$-0.191^{***}$	$0.134^{*}$	-0.150
	(0.065)	(0.062)	(0.072)	(0.118)
Labor Expenditure Share <sup><math>a</math></sup>	0.003	-0.011	0.004	-0.024
	(0.004)	(0.013)	(0.005)	(0.027)
Fuel Expenditures Share <sup><math>a</math></sup>	-0.002	$0.014^{***}$	$-0.006^{*}$	0.009
	(0.002)	(0.005)	(0.003)	(0.014)
Expenditure per Hired Worker <sup><math>ab</math></sup>	0.008	-0.050	-0.036	-0.045
	(0.049)	(0.095)	(0.050)	(0.079)
Workers $Hired^{ab}$	0.008	-0.102	-0.076	$-0.235^{***}$
	(0.036)	(0.071)	(0.059)	(0.053)
Workers Hired $\geq 150 \text{ days}^{ab}$	0.005	-0.118	-0.070	$-0.229^{***}$
	(0.046)	(0.091)	(0.076)	(0.071)
Workers Hired $< 150 \text{ days}^{ab}$	-0.060	-0.043	$-0.094^{***}$	$-0.106^{***}$
	(0.038)	(0.060)	(0.030)	(0.036)
Workers per Acre of $Crops^{ab}$	0.004	0.031	-0.087	-0.232
	(0.085)	(0.163)	(0.191)	(0.390)

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 $^{\rm a}$  Per operation average.  $^{\rm b}$  Natural logarithm.  $^{\rm c}$  Statistical significance:  $^*p<0.1;\;^{**}p<0.05;\;^{***}p<0.01.$ 

Effects
Border
Table 4:

		9 STATE TRI	EATMENT			4 STATE TRI	EATMENT	
Outcome	All Counties	$[0, 100)^c$	$[0, 150)^c$	$[0, 200)^c$	All Counties	$[0, 100)^c$	$[0, 150)^c$	$[0, 200)^{c}$
Hired Labor Expenditures <sup><math>ab</math></sup>	-0.036	-0.101	-0.110	$-0.102^{*}$	-0.043	$0.115^{*}$	0.067	-0.048
	(0.044)	(0.083)	(0.074)	(0.061)	(0.070)	(0.069)	(0.072)	(0.076)
Contract Labor Expenditures <sup>ab</sup>	0.045	-0.123	-0.049	-0.016	0.116	0.058	$0.178^{***}$	0.090
	(0.063)	(0.096)	(0.089)	(0.079)	(0.071)	(0.111)	(0.064)	(0.057)
Labor Expenditure Share <sup><math>a</math></sup>	0.001	0.000	-0.003	0.000	0.000	$0.011^{**}$	0.005	0.000
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)
Fuel Expenditures Share <sup><math>a</math></sup>	0.000	-0.002	0.000	-0.001	-0.005	$-0.012^{***}$	$-0.006^{***}$	$-0.006^{**}$
	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
Expenditure per Hired Worker <sup>ab</sup>	0.000	-0.003	-0.016	-0.044	-0.045	0.086	0.012	-0.035
	(0.043)	(060.0)	(0.056)	(0.046)	(0.045)	(0.082)	(0.064)	(0.045)
Workers Hired $^{ab}$	-0.003	-0.021	-0.025	-0.019	-0.099	$-0.124^{**}$	-0.065	-0.092
	(0.038)	(0.058)	(0.047)	(0.048)	(0.063)	(0.055)	(0.054)	(0.058)
Workers Hired $\geq 150 \text{ days}^{ab}$	$-0.059^{*}$	$-0.156^{**}$	$-0.094^{**}$	-0.044	$-0.096^{***}$	-0.072	$-0.118^{**}$	$-0.079^{*}$
	(0.034)	(0.065)	(0.042)	(0.038)	(0.033)	(0.063)	(0.049)	(0.045)
Workers Hired $< 150 \text{ days}^{ab}$	-0.006	-0.075	-0.057	-0.039	-0.088	$-0.190^{**}$	-0.046	-0.085
	(0.047)	(0.088)	(0.052)	(0.055)	(0.078)	(0.079)	(0.059)	(0.070)
Workers per Acre of $Crops^{ab}$	0.010	-0.044	-0.076	0.022	-0.109	$-0.317^{***}$	-0.195	-0.143
	(0.084)	(0.145)	(0.123)	(0.119)	(0.172)	(0.086)	(0.126)	(0.158)
<sup>a</sup> Per operation average.								

<sup>b</sup> Natural logarithm. <sup>c</sup> [0, k) = counties within k miles of policy-change border. <sup>d</sup> Statistical significance: \*p < 0.1; \*\*p < 0.05; \*\*\*p < 0.01.

Outcome	9 State Treatment	4 State Treatment
Hired Labor Expenditures <sup><math>ab</math></sup>	-0.139	$-0.158^{**}$
-	(0.103)	(0.076)
Contract Labor Expenditures <sup><math>ab</math></sup>	-0.161	-0.072
	(0.110)	(0.078)
Labor Expenditure Share <sup><math>a</math></sup>	$-0.030^{***}$	$-0.021^{*}$
	(0.010)	(0.011)
Fuel Expenditures $Share^a$	0.008	$0.008^{*}$
	(0.005)	(0.004)
Expenditure per Hired Worker <sup><math>ab</math></sup>	-0.069	$-0.115^{**}$
	(0.068)	(0.052)
Workers $Hired^{ab}$	-0.091	-0.061
	(0.057)	(0.043)
Workers Hired $\geq 150 \text{ days}^{ab}$	-0.053	$-0.072^{**}$
	(0.048)	(0.035)
Workers Hired $< 150 \text{ days}^{ab}$	$-0.111^{*}$	-0.054
	(0.063)	(0.057)
Workers per Acre of $Crops^{ab}$	-0.106	$-0.312^{**}$
	(0.132)	(0.154)

Table 5: S	Spillover	Effects
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<sup>a</sup> Per operation average. <sup>b</sup> Natural logarithm. <sup>c</sup> Statistical significance: \*p < 0.1; \*\*p < 0.05; \*\*\*p < 0.01.

Model Validity & Robustness Checks Ю

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fy 2007 Placebo)	Table 6: Labor Intensity (E-Veri

	0 0 0	TATE TREATMENT	4 ST	ATE TREATMENT
Outcome	E-Verify	$E-Verify \times Labor Intense$	E-Verify	E-Verify $\times$ Labor Intense
Hired Labor Expenditures <sup><math>ab</math></sup>	-0.037	-0.020	0.007	-0.008
	(0.029)	(0.113)	(0.035)	(0.202)
Contract Labor Expenditures <sup><math>ab</math></sup>	0.069	-0.091	-0.028	0.013
	(0.055)	(0.077)	(0.065)	(0.098)
Labor Expenditure Share <sup><math>a</math></sup>	0.004	0.002	-0.004	-0.011
	(0.004)	(0.010)	(0.003)	(0.030)
Fuel Expenditures Share <sup><math>a</math></sup>	-0.002	$0.009^{***}$	$-0.007^{**}$	0.006
	(0.002)	(0.003)	(0.003)	(0.006)
Expenditure per Hired Worker <sup>ab</sup>	-0.030	0.024	-0.022	0.076
	(0.046)	(0.119)	(0.045)	(0.126)
Workers $Hired^{ab}$	0.042	-0.045	-0.028	-0.107
	(0.037)	(0.060)	(0.045)	(0.096)
Workers Hired $\geq 150 \text{ days}^{ab}$	0.052	$-0.127^{*}$	-0.037	$-0.187^{***}$
	(0.048)	(0.065)	(0.075)	(0.066)
Workers Hired $< 150 \text{ days}^{ab}$	-0.029	0.072	$-0.086^{***}$	0.020
	(0.032)	(0.070)	(0.019)	(0.131)
Workers per Acre of $Crops^{ab}$	0.036	0.287	-0.045	0.074
	(0.070)	(0.278)	(0.110)	(0.359)

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 $^{\rm a}$  Per operation average.  $^{\rm b}$  Natural logarithm.  $^{\rm c}$  Statistical significance:  $^*p<0.1;~^{**}p<0.05;~^{***}p<0.01.$ 

		9 STATE T	REATMENT			4 STATE TR	EATMENT	
Outcome	All Counties	$[0, 100)^c$	$[0, 150)^c$	$[0, 200)^{c}$	All Counties	$[0, 100)^{c}$	$[0, 150)^c$	$[0, 200)^{c}$
Hired Labor Expenditures <sup><math>ab</math></sup>	-0.032	$-0.094^{**}$	$-0.076^{***}$	$-0.082^{***}$	0.005	0.000	$0.083^{*}$	0.006
	(0.028)	(0.039)	(0.026)	(0.024)	(0.033)	(0.044)	(0.043)	(0.049)
Contract Labor Expenditures <sup><math>ab</math></sup>	0.060	0.072	0.054	0.045	-0.033	0.080	0.011	-0.054
	(0.058)	(0.057)	(0.058)	(0.068)	(0.065)	(0.082)	(0.077)	(0.086)
Labor Expenditure Share <sup><math>a</math></sup>	0.004	0.006	$0.009^{***}$	$0.009^{**}$	-0.005	0.006	0.005	-0.002
	(0.004)	(0.006)	(0.003)	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)
Fuel Expenditures Share <sup><math>a</math></sup>	-0.001	-0.001	0.001	-0.001	$-0.007^{**}$	$-0.013^{***}$	$-0.007^{**}$	$-0.008^{***}$
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)
Expenditure per Hired Worker <sup>ab</sup>	-0.026	-0.091	-0.066	$-0.069^{*}$	-0.015	$-0.083^{**}$	0.010	-0.018
	(0.039)	(0.067)	(0.046)	(0.042)	(0.039)	(0.039)	(0.041)	(0.039)
Workers Hired $^{ab}$	0.037	0.077	0.060	0.033	-0.038	-0.028	0.014	-0.018
	(0.037)	(0.047)	(0.047)	(0.043)	(0.045)	(0.053)	(0.040)	(0.038)
Workers Hired $\geq 150 \text{ days}^{ab}$	-0.011	$-0.107^{***}$	-0.039	0.003	$-0.077^{***}$	$-0.166^{***}$	$-0.105^{***}$	$-0.075^{**}$
	(0.030)	(0.041)	(0.049)	(0.035)	(0.022)	(0.035)	(0.040)	(0.036)
Workers Hired $< 150 \text{ days}^{ab}$	0.041	$0.117^{*}$	0.066	0.040	-0.052	0.076	-0.005	-0.028
	(0.048)	(0.061)	(0.061)	(0.057)	(0.073)	(0.073)	(0.082)	(0.070)
Workers per Acre of Crops <sup>ab</sup>	0.069	$0.222^{***}$	$0.160^{*}$	0.151	-0.039	$0.114^{**}$	0.032	-0.037
	(0.077)	(0.083)	(0.083)	(0.098)	(0.093)	(0.048)	(0.085)	(0.103)
<sup>a</sup> Per operation average.								
<sup>b</sup> Natural logarithm.								
$\begin{bmatrix} c \\ c \end{bmatrix} [0,k) = \text{counties within } k \text{ m}$	iiles of policy-cha	nge border.						
$^{\rm d}$ Statistical significance: ${}^*p<$	< 0.1; **p < 0.05;	$^{***}p < 0.01.$						

Table 7: Border Effects (E-Verify 2007 Placebo)

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Model Validity & Robustness Checks

Outcome	9 State Treatment	4 State Treatment
Hired Labor Expenditures <sup><math>ab</math></sup>	$-0.125^{*}$	$-0.124^{*}$
	(0.075)	(0.067)
Contract Labor Expenditures <sup><math>ab</math></sup>	-0.154	-0.064
	(0.109)	(0.073)
Labor Expenditure $\text{Share}^a$	$-0.026^{***}$	-0.014
	(0.010)	(0.012)
Fuel Expenditures $Share^a$	0.006	0.006
	(0.004)	(0.004)
Expenditure per Hired Worker <sup><math>ab</math></sup>	-0.032	$-0.076^{*}$
	(0.050)	(0.042)
Workers $\operatorname{Hired}^{ab}$	$-0.084^{**}$	-0.036
	(0.041)	(0.049)
Workers Hired $\geq 150 \text{ days}^{ab}$	-0.063	-0.046
	(0.044)	(0.043)
Workers Hired $< 150 \text{ days}^{ab}$	$-0.090^{**}$	-0.034
	(0.044)	(0.053)
Workers per Acre of $Crops^{ab}$	-0.103	$-0.276^{*}$
	(0.146)	(0.164)

 $^{\rm a}$  Per operation average.  $^{\rm b}$  Natural logarithm.  $^{\rm c}$  Statistical significance:  $^*p<0.1;\;^{**}p<0.05;\;^{***}p<0.01.$ 

Full Sample	$\Delta \bar{Y}^C$	$\Delta \bar{Y}^T$	$\Delta \bar{Y}^T - \Delta \bar{Y}^C$	$\hat{t}$	$P(z >  \hat{t} )$
Labor Expenditure $Share^a$	0.02	0.02	0.00	0.73	0.47
Fuel Expenditures $Share^a$	-0.01	-0.01	0.00	1.24	0.22
Contract Labor Expenditures <sup><math>ab</math></sup>	0.28	0.33	-0.05	-1.22	0.22
Hired Labor Expenditures <sup><math>ab</math></sup>	0.14	0.14	-0.00	-0.06	0.95
Workers $Hired^{ab}$	-0.00	0.01	-0.01	-0.91	0.36
Workers Hired $\geq 150 \text{ days}^{ab}$	0.02	-0.02	0.04	2.22	0.03
Workers Hired $< 150 \text{ days}^{ab}$	-0.02	-0.02	0.00	0.01	0.99
Expenditure per Hired Worker <sup><math>ab</math></sup>	0.15	0.13	0.02	0.84	0.40
Workers per Acre of $Crops^{ab}$	-0.15	-0.12	-0.03	-0.94	0.35

Table 9: Pre-treatment Outcome Differences - 9 State Treatment

$\hline {\bf Discontinuity} \ {\bf Sample}^c$	$\Delta \bar{Y}^C$	$\Delta \bar{Y}^T$	$\Delta \bar{Y}^T - \Delta \bar{Y}^C$	$\hat{t}$	$P(z >  \hat{t} )$
Labor Expenditure Share <sup><math>a</math></sup>	0.02	0.02	0.00	0.57	0.57
Fuel Expenditures $Share^a$	-0.02	-0.01	-0.00	-1.94	0.05
Contract Labor Expenditures <sup><math>ab</math></sup>	0.26	0.35	-0.10	-1.87	0.06
Hired Labor Expenditures <sup><math>ab</math></sup>	0.13	0.16	-0.03	-0.97	0.33
Workers $Hired^{ab}$	-0.01	0.03	-0.05	-2.15	0.03
Workers Hired $\geq 150 \text{ days}^{ab}$	0.03	-0.01	0.04	1.65	0.10
Workers Hired $< 150 \text{ days}^{ab}$	-0.02	0.01	-0.03	-1.01	0.31
Expenditure per Hired Worker <sup><math>ab</math></sup>	0.15	0.14	0.02	0.59	0.56
Workers per Acre of $Crops^{ab}$	-0.07	-0.10	0.03	0.49	0.62

<sup>a</sup> Per operation average.<sup>b</sup> Natural logarithm.

<sup>c</sup> Within 150 miles of policy-change border.

<sup>d</sup> Y denotes the variable, the group  $G \in \{C, T\}$  where T includes universal E-Verify states and C denotes all other states. <sup>e</sup>  $\Delta \bar{Y}^G = \bar{Y}^G_{2007} - \bar{Y}^G_{2007}.$ 

Full Sample	$\Delta \bar{Y}^C$	$\Delta \bar{Y}^T$	$\Delta \bar{Y}^T - \Delta \bar{Y}^C$	$\hat{t}$	$P(z >  \hat{t} )$
Labor Expenditure $Share^{a}$	0.02	0.02	0.01	3.62	0.00
Fuel Expenditures $Share^a$	-0.01	-0.02	0.01	2.12	0.04
Contract Labor Expenditures <sup><math>ab</math></sup>	0.29	0.23	0.07	0.96	0.34
Hired Labor Expenditures <sup><math>ab</math></sup>	0.14	0.12	0.02	0.67	0.51
Workers $Hired^{ab}$	-0.00	0.00	-0.01	-0.21	0.83
Workers Hired $\geq 150 \text{ days}^{ab}$	0.02	-0.02	0.03	1.19	0.23
Workers Hired $< 150 \text{ days}^{ab}$	-0.01	-0.04	0.03	0.87	0.39
Expenditure per Hired Worker <sup><math>ab</math></sup>	0.15	0.12	0.03	0.86	0.39
Workers per Acre of $Crops^{ab}$	-0.14	-0.13	-0.01	-0.23	0.82
Discontinuity $Sample^{c}$	$\Delta \bar{Y}^C$	$\Delta \bar{Y}^T$	$\Delta \bar{Y}^T - \Delta \bar{Y}^C$	î	$P(z >  \hat{t} )$
Discontinuity Sample <sup><math>c</math></sup> Labor Expenditure Share <sup><math>a</math></sup>	$\frac{\Delta \bar{Y}^C}{0.02}$	$\frac{\Delta \bar{Y}^T}{0.02}$	$\frac{\Delta \bar{Y}^T - \Delta \bar{Y}^C}{0.01}$	$\hat{t}$ 4.13	$P(z >  \hat{t} )$ $0.00$
<b>Discontinuity Sample</b> <sup><math>c</math></sup> Labor Expenditure Share <sup><math>a</math></sup> Fuel Expenditures Share <sup><math>a</math></sup>	$\Delta \bar{Y}^C$ 0.02 -0.01	$\begin{array}{c} \Delta \bar{Y}^T \\ 0.02 \\ -0.02 \end{array}$	$\frac{\Delta \bar{Y}^T - \Delta \bar{Y}^C}{0.01}$ $0.00$		$P(z >  \hat{t} )$ $0.00$ $0.40$
$\begin{array}{c} \textbf{Discontinuity Sample}^c\\ \textbf{Labor Expenditure Share}^a\\ \textbf{Fuel Expenditures Share}^a\\ \textbf{Contract Labor Expenditures}^{ab} \end{array}$	$\Delta \bar{Y}^C$ 0.02 -0.01 0.31	$\Delta \bar{Y}^T$ 0.02 -0.02 0.23	$\begin{array}{c} \Delta \bar{Y}^T - \Delta \bar{Y}^C \\ 0.01 \\ 0.00 \\ 0.08 \end{array}$	$\hat{t}$ 4.13 0.85 1.07	$ \begin{array}{c} P(z >  \hat{t} ) \\ 0.00 \\ 0.40 \\ 0.29 \end{array} $
Discontinuity SampleLabor Expenditure ShareFuel Expenditures ShareContract Labor Expenditures $a^b$ Hired Labor Expenditures	$\begin{array}{c} \Delta \bar{Y}^{C} \\ 0.02 \\ -0.01 \\ 0.31 \\ 0.13 \end{array}$	$     \begin{array}{r} \Delta \bar{Y}^T \\             0.02 \\             -0.02 \\             0.23 \\             0.12 \end{array}     $	$\begin{array}{c} \Delta \bar{Y}^{T} - \Delta \bar{Y}^{C} \\ 0.01 \\ 0.00 \\ 0.08 \\ 0.01 \end{array}$		$P(z >  \hat{t} ) \\ 0.00 \\ 0.40 \\ 0.29 \\ 0.78$
$\begin{array}{c} \textbf{Discontinuity Sample}^{c} \\ \textbf{Labor Expenditure Share}^{a} \\ \textbf{Fuel Expenditures Share}^{a} \\ \textbf{Contract Labor Expenditures}^{ab} \\ \textbf{Hired Labor Expenditures}^{ab} \\ \textbf{Workers Hired}^{ab} \end{array}$	$\begin{array}{c} \Delta \bar{Y}^{C} \\ 0.02 \\ -0.01 \\ 0.31 \\ 0.13 \\ -0.01 \end{array}$	$\begin{array}{c} \Delta \bar{Y}^{T} \\ 0.02 \\ -0.02 \\ 0.23 \\ 0.12 \\ 0.01 \end{array}$	$\begin{array}{c} \Delta \bar{Y}^{T} - \Delta \bar{Y}^{C} \\ 0.01 \\ 0.00 \\ 0.08 \\ 0.01 \\ -0.02 \end{array}$	$\hat{t}$ 4.13 0.85 1.07 0.27 -0.53	$P(z >  \hat{t} ) \\ 0.00 \\ 0.40 \\ 0.29 \\ 0.78 \\ 0.60$
Discontinuity SamplecLabor Expenditure ShareaFuel Expenditures ShareaContract Labor ExpendituresabHired Labor ExpendituresabWorkers HiredabWorkers Hired $\geq 150 \text{ days}^{ab}$	$\begin{array}{c} \Delta \bar{Y}^C \\ 0.02 \\ -0.01 \\ 0.31 \\ 0.13 \\ -0.01 \\ -0.01 \end{array}$	$\begin{array}{c} \Delta \bar{Y}^T \\ 0.02 \\ -0.02 \\ 0.23 \\ 0.12 \\ 0.01 \\ -0.02 \end{array}$	$\begin{array}{c} \Delta \bar{Y}^T - \Delta \bar{Y}^C \\ 0.01 \\ 0.00 \\ 0.08 \\ 0.01 \\ -0.02 \\ 0.01 \end{array}$		$P(z >  \hat{t} )$ 0.00 0.40 0.29 0.78 0.60 0.79
Discontinuity SamplecLabor Expenditure ShareaFuel Expenditures ShareaContract Labor ExpendituresabHired Labor ExpendituresabWorkers HiredabWorkers Hired $\geq 150 \text{ days}^{ab}$ Workers Hired < 150 daysab	$\begin{array}{c} \Delta \bar{Y}^C \\ 0.02 \\ -0.01 \\ 0.31 \\ 0.13 \\ -0.01 \\ -0.01 \\ -0.03 \end{array}$	$\begin{array}{c} \Delta \bar{Y}^T \\ 0.02 \\ -0.02 \\ 0.23 \\ 0.12 \\ 0.01 \\ -0.02 \\ -0.04 \end{array}$	$\begin{array}{c} \Delta \bar{Y}^T - \Delta \bar{Y}^C \\ 0.01 \\ 0.00 \\ 0.08 \\ 0.01 \\ -0.02 \\ 0.01 \\ 0.01 \\ 0.01 \end{array}$	$\begin{array}{r} \hat{t} \\ 4.13 \\ 0.85 \\ 1.07 \\ 0.27 \\ -0.53 \\ 0.27 \\ 0.39 \end{array}$	$P(z >  \hat{t} )$ 0.00 0.40 0.29 0.78 0.60 0.79 0.70
<b>Discontinuity Sample</b> <sup>c</sup> Labor Expenditure Share <sup>a</sup> Fuel Expenditures Share <sup>a</sup> Contract Labor Expenditures <sup>ab</sup> Hired Labor Expenditures <sup>ab</sup> Workers Hired <sup>ab</sup> Workers Hired $\geq 150 \text{ days}^{ab}$ Workers Hired < 150 days <sup>ab</sup> Expenditure per Hired Worker <sup>ab</sup>	$\begin{array}{c} \Delta \bar{Y}^C \\ 0.02 \\ -0.01 \\ 0.31 \\ 0.13 \\ -0.01 \\ -0.01 \\ -0.03 \\ 0.14 \end{array}$	$\begin{array}{c} \Delta \bar{Y}^T \\ 0.02 \\ -0.02 \\ 0.23 \\ 0.12 \\ 0.01 \\ -0.02 \\ -0.04 \\ 0.12 \end{array}$	$\begin{array}{c} \Delta \bar{Y}^T - \Delta \bar{Y}^C \\ 0.01 \\ 0.00 \\ 0.08 \\ 0.01 \\ -0.02 \\ 0.01 \\ 0.01 \\ 0.01 \\ 0.02 \end{array}$	$\begin{array}{r} \hat{t} \\ \hline 4.13 \\ 0.85 \\ 1.07 \\ 0.27 \\ -0.53 \\ 0.27 \\ 0.39 \\ 0.47 \end{array}$	$\begin{array}{c} P(z >  \hat{t} ) \\ 0.00 \\ 0.40 \\ 0.29 \\ 0.78 \\ 0.60 \\ 0.79 \\ 0.70 \\ 0.64 \end{array}$

Table 10: Pre-treatment Outcome Differences - 4 State Treatment

<sup>a</sup> Per operation average.<sup>b</sup> Natural logarithm.

<sup>c</sup> Within 150 miles of policy-change border.

<sup>d</sup> Y denotes the variable, the group  $G \in \{C, T\}$  where T includes universal E-Verify states and C denotes all other states.

$${}^{\rm e} \ \Delta \bar{Y}^G = \bar{Y}^G_{2007} - \bar{Y}^G_{2007}.$$

## C Summary Statistics

Non E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	9,619	0.06	0.02	0.00	0.30
Fuel Expenditures Share	9,090	0.11	0.09	0.01	0.56
Contract Labor Expenditures	9,172	9,289	$22,\!481$	116	$648,\!561$
Hired Labor Expenditures	$9,\!482$	26,287	$35,\!136$	173	$573,\!026$
Workers Hired	$7,\!158$	4.45	4.19	1.00	64.79
Workers Hired $\geq 150$ days	6,810	3.38	2.92	1.00	58.52
Workers Hired $< 150$ days	$6,\!830$	3.78	3.80	1.00	71.11
Expenditure per Hired Worker	7,058	$5,\!851$	$3,\!182$	168	$31,\!186$
Workers per Acre of Crops	$6,\!620$	0.26	2.40	0.00	94.00
E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	2,608	0.06	0.03	0.01	0.26
Fuel Expenditures Share	2,394	0.11	0.07	0.01	0.58
Contract Labor Expenditures	2,449	$9,\!625$	20,190	116	673,631
Hired Labor Expenditures	2,550	$23,\!698$	$28,\!444$	711	$341,\!587$
Workers Hired	1,941	4.33	3.07	1.00	44.92
Workers Hired $\geq 150$ days	1,769	3.56	2.62	1.00	39.38
Workers Hired $< 150$ days	1,770	3.68	2.57	1.00	41.00
	1 00 7	F 007	0.000	0.47	00 500
Expenditure per Hired Worker	1,887	5,237	3,268	247	26,582
Workers per Acre of Crops <b>E-Verify</b> Labor Expenditure Share Fuel Expenditures Share Contract Labor Expenditures Hired Labor Expenditures Workers Hired Workers Hired $\geq 150$ days Workers Hired < 150 days	6,620 N 2,608 2,394 2,449 2,550 1,941 1,769 1,770	0.26 Mean 0.06 0.11 9,625 23,698 4.33 3.56 3.68	2.40 SD 0.03 0.07 20,190 28,444 3.07 2.62 2.57	0.00 Min 0.01 0.01 116 711 1.00 1.00 1.00	94.00 Max 0.26 0.58 673,631 341,587 44.92 39.38 41.00

Table 11: Summary Statistics - Full Sample 9 State Treatment

 $^\dagger\,$  Per operation average.

Non E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	2,238	0.06	0.03	0.01	0.22
Fuel Expenditures Share	2,076	0.11	0.07	0.01	0.48
Contract Labor Expenditures	2,111	$7,\!617$	$15,\!848$	116	400,740
Hired Labor Expenditures	$2,\!176$	21,044	30,328	173	$435,\!673$
Workers Hired	$1,\!651$	3.95	2.71	1.00	39.41
Workers Hired $\geq 150$ days	1,511	3.27	3.04	1.00	58.52
Workers Hired $< 150$ days	1,522	3.37	2.12	1.00	33.13
Expenditure per Hired Worker	$1,\!607$	4,993	$3,\!401$	185	$22,\!629$
Workers per Acre of Crops	1,523	0.16	0.36	0.00	5.29

Table 12: Summary Statistics - Discontinuity Sample<sup>a</sup> 9 State Treatment

E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	$2,\!147$	0.06	0.03	0.01	0.26
Fuel Expenditures Share	1,982	0.11	0.07	0.01	0.58
Contract Labor Expenditures	2,021	9,986	21,815	116	673,631
Hired Labor Expenditures	$2,\!106$	23,357	28,957	711	341,587
Workers Hired	$1,\!602$	4.33	3.18	1.00	44.92
Workers Hired $\geq 150$ days	$1,\!459$	3.49	2.58	1.00	39.38
Workers Hired $< 150$ days	1,462	3.71	2.67	1.00	41.00
Expenditure per Hired Worker	1,563	$5,\!090$	$3,\!187$	247	21,196
Workers per Acre of Crops	1,514	0.16	0.52	0.00	10.57

 $^\dagger\,$  Per operation average.

<sup>a</sup> Within 150 miles of policy-change border.

Non E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	11,390	0.06	0.02	0.00	0.30
Fuel Expenditures Share	10,716	0.11	0.08	0.01	0.58
Contract Labor Expenditures	10,836	9,304	21,186	116	$648,\!561$
Hired Labor Expenditures	11,221	25,913	33,788	173	573,026
Workers Hired	8,474	4.45	4.03	1.00	64.79
Workers Hired $\geq 150$ days	8,003	3.42	2.89	1.00	58.52
Workers Hired $< 150$ days	8,026	3.80	3.66	1.00	71.11
Expenditure per Hired Worker	8,345	5,758	$3,\!199$	168	31,186
Workers per Acre of Crops	7,853	0.25	2.24	0.00	94.00

Table 13: Summary Statistics - Full Sample 4 State Treatment

E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	837	0.06	0.03	0.01	0.26
Fuel Expenditures Share	768	0.09	0.07	0.01	0.43
Contract Labor Expenditures	785	10,132	31,326	116	673,631
Hired Labor Expenditures	811	23,321	$34,\!544$	711	$341,\!587$
Workers Hired	625	4.05	3.16	1.24	36.80
Workers Hired $\geq 150$ days	576	3.41	2.51	1.00	21.35
Workers Hired $< 150$ days	574	3.28	2.21	1.00	24.51
Expenditure per Hired Worker	600	5,207	$3,\!317$	878	$26,\!582$
Workers per Acre of Crops	593	0.14	0.32	0.00	5.76

<sup>†</sup> Per operation average.

Non E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	2,369	0.06	0.03	0.01	0.25
Fuel Expenditures Share	$2,\!157$	0.11	0.07	0.01	0.58
Contract Labor Expenditures	2,206	9,363	16,267	174	400,740
Hired Labor Expenditures	2,311	23,385	29,197	415	435,673
Workers Hired	1,758	4.31	3.19	1.00	44.92
Workers Hired $\geq 150$ days	1,590	3.59	3.34	1.00	58.52
Workers Hired $< 150$ days	1,596	3.72	2.72	1.00	41.00
Expenditure per Hired Worker	1,712	5,209	3,262	247	$20,\!696$
Workers per Acre of Crops	$1,\!639$	0.19	0.88	0.00	30.33

Table 14: Summary Statistics - Discontinuity Sample<sup>a</sup> 4 State Treatment

E-Verify	Ν	Mean	SD	Min	Max
Labor Expenditure Share	833	0.06	0.03	0.01	0.26
Fuel Expenditures Share	765	0.09	0.07	0.01	0.43
Contract Labor Expenditures	782	10,133	$31,\!385$	116	$673,\!631$
Hired Labor Expenditures	808	23,161	$34,\!474$	711	$341,\!587$
Workers Hired	622	4.03	3.13	1.24	36.80
Workers Hired $\geq 150$ days	573	3.39	2.50	1.00	21.35
Workers Hired $< 150$ days	571	3.26	2.18	1.00	24.51
Expenditure per Hired Worker	598	$5,\!196$	$3,\!315$	878	$26,\!582$
Workers per Acre of Crops	590	0.14	0.32	0.00	5.76

 $^\dagger\,$  Per operation average.

<sup>a</sup> Within 150 miles of policy-change border.

Non E-Verify	N	Mean	SD	Min	Max
Acres of Cropland <sup><math>a</math></sup>	9,563	309	323	1	3,055
Republican Legislature	9,564	0.47	0.50	0	1
Split Legislature	9,564	0.27	0.44	0	1
State GDP $(Farm)^b$	9,936	2,151	2,058	8	$14,\!185$
State $GDP^{b}$	9,936	327,483	333,451	$19,\!671$	1,602,723
State Unemployment	10,248	0.06	0.02	0.03	0.17
E-Verify	N	Mean	SD	Min	Max
Acres of $Cropland^a$	$2,\!617$	171	206	2	2,070
Republican Legislature	2,628	0.43	0.50	0	1
Split Legislature	2,628	0.12	0.32	0	1
State GDP $(Farm)^b$	2,628	1,212	703	268	$3,\!175$
State $GDP^{b}$	$2,\!628$	216,607	$97,\!699$	69,074	352,314
State Unemployment	$2,\!628$	0.07	0.02	0.03	0.11

Table 15: Covariate Balance - 9 State Treatment

<sup>a</sup> Per operation average<sup>b</sup> In millions USD (1999)

Non E-Verify	Ν	Mean	SD	Min	Max
Acres of $Cropland^a$	$11,\!347$	286	309	1	$3,\!055$
Republican Legislature	$11,\!352$	0.47	0.50	0	1
Split Legislature	11,352	0.25	0.43	0	1
State GDP $(Farm)^b$	11,724	2,034	1,938	8	14,185
State $GDP^{b}$	11,724	317,709	$309,\!452$	$19,\!671$	1,602,723
State Unemployment	12,036	0.06	0.02	0.03	0.17
	,				

Table 16: Covariate Balance - 4 State Treatment

E-Verify	Ν	Mean	SD	Min	Max
Acres of $Cropland^a$	833	185	260	12	2,070
Republican Legislature	840	0.41	0.49	0	1
Split Legislature	840	0.05	0.23	0	1
State GDP $(Farm)^b$	840	838	266	363	1,216
State $GDP^b$	840	$117,\!015$	$36,\!535$	69,074	219,074
State Unemployment	840	0.08	0.02	0.05	0.11

<sup>a</sup> Per operation average
<sup>b</sup> In millions USD (1999)

#### **E-Verify Policy Details** D

State	Scope of Mandate	Effective Date	Notes
Alabama	All employers	4/1/2012	
Arizona	All employers	1/1/2008	
Colorado	State agencies, contractors	8/07/2006	
$Georgia^{\dagger}$	All employers	1/1/2012	
Idaho	State contractors	7/1/2009	
Indiana	Public agencies, contractors	7/1/2011	
Louisiana	All Employers	1/1/2012	
Minnesota	Public contractors	7/22/2011	
Mississippi	All employers	7/1/2008	
Missouri	Public employers, contractors	1/1/2009	
Nebraska	Public employers, contractors	10/1/2009	
North Carolina <sup>†</sup>	All employers	10/1/2012	$\geq 25 \text{ employees}$
Oklahoma	Public employers, contractors	11/1/2007	
Pennsylvania	Public contractors	1/1/2013	
South Carolina <sup>†</sup>	All employers	7/1/2009	
$\mathrm{Tennesee}^{\dagger}$	All employers	1/1/2012	$\geq 6 \text{ employees}$
Texas	Public agencies, contractors	9/1/2015	
$\mathrm{Utah}^\dagger$	All employers	7/1/2010	$\geq 15 \text{ employees}$
Virginia	State Agencies	12/1/2012	- °

Table 17: E-Verify Mandates for U.S. States

<sup>†</sup> Staggered implementation by employer size <sup>a</sup> Sources: Newman et al. (2015); Feere (2012); Mendoza and Ostrander (2015); LawLogix Group, Inc. (2012)

## **E** Figures



Figure 3: NAWS Respondents Legal Application Status (United States Department of Labor, 1989 - 2012)

# Figure 4: NAWS Respondents Legal Application Status by Crop (1989 - 2013 pooled) (United States Department of Labor, 1989 - 2012)









## Figure 6: Agricultural Labor Intensity - U.S. 2002



### Figure 7: Agricultural Labor Intensity - Southern U.S. 2002





Figure 9: Spillover Subsample



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