

Immigration Policies and Human Capital: The Impact on Undocumented College Attendance

David Titus*

Job Market Paper[†]

October 29, 2025

(View the Most Updated Version Here)

Abstract

I estimate the impact of Universal E-Verify laws on the college attendance of undocumented Hispanics in the United States. To do so, I implement a series of event studies that account for staggered adoption over time, and I use a random forest algorithm as my primary approach to predict undocumented status. My results indicate that Universal E-Verify laws lower the college attendance of undocumented Hispanics ages 18-24 by about 3.7 percentage points. This is a substantial effect: only 15.7 percent of undocumented Hispanics ages 18-24 in treated states were enrolled in college following the passage of the laws. This effect is robust to using logical imputation on non-citizen Hispanics to proxy for undocumented immigrants, using a logit model instead of random forest, testing for migration spillover effects on bordering states, and considering potentially confounding impacts of other state-level policies. I develop a theoretical model that explains the mechanisms through which Universal E-Verify affects college education, and I test this model's implications. I find suggestive evidence that the effect is driven by a negative labor market shock on undocumented adults ages 25-54, which likely leads to worse schooling for their children and renders college less attainable. These findings indicate that employment restrictions targeting working-age undocumented adults hinder the human capital development of undocumented youth.

*PhD Candidate, Department of Economics, Cornell University, dwt45@cornell.edu

[†]A special thanks to Francine Blau, Michèle Belot, Lawrence Kahn, Evan Riehl and the Cornell LWIPS group, and all others who have provided helpful feedback and guidance. Any mistakes are my own.

I. Introduction

Undocumented immigrants became the focus of attention across many state legislatures in the 2000s and 2010s. This attention was in part due to the significant number of unauthorized immigrants in the country: the Pew Research Center found that there were approximately 12.2 million undocumented immigrants living in the United States in 2007. Although that number began to decline in the late 2010s, it has recently rebounded: Pew estimates indicate that the number of unauthorized immigrants climbed to 14.0 million in 2023.¹

In response to this large population of undocumented immigrants, policies designed to deter unauthorized immigration were enacted across many states. These policies were often police-based and intended to increase deportations.² However, also passed were laws designed to limit labor market opportunities for undocumented workers. Chief among them was Universal E-Verify, which mandates that private employers use the online E-Verify system to audit the legal status of prospective employees.

It is important to understand the effects that employment-based policies have on human capital accumulation, especially given the recent passage of new Universal E-Verify laws.³ In particular, it is vital to understand how restrictive employment policies on undocumented adults, which do not explicitly target education, can nevertheless lead to reduced academic attainment for undocumented youth.

The effects that restrictive policies have on college attendance have not been widely studied. While prior papers have demonstrated that policing and deportation policies lead to lower high school attendance, worse mental health outcomes, and poorer academic performance (Allen and McNeely 2017; Amuedo-Dorantes and Lopez 2017; Bucheli, Rubalcaba, and Vargas 2021; Lou and Escalante 2021), their potential impact on higher education has not been as extensively examined. There is some limited prior work analyzing police-based enforcement effects on college education: Bucheli, Rubalcaba, and Vargas (2021) demonstrate that schooling among Hispanics ages 19-24 drops as ICE arrest rates increase. However, to the best of my knowledge, no prior paper has directly examined whether employment-based policies like Universal E-Verify affect college attendance.

This effect could be substantial. If Universal E-Verify leads to worse labor market outcomes among undocumented parents, it may become more difficult for their children to perform well in school, let alone afford college. In addition, many states allow Universal E-Verify exemptions for small businesses such as restaurants or services within households, and those who are self-employed or in informal sectors that flout the law are not subjected to the E-Verify system. This means that Universal E-Verify may push undocumented workers into jobs that do not require a college degree (Amuedo-Dorantes and Bansak 2012, 2014; Bohn, Lofstrom, and Raphael 2014; Luo and Kostandini 2022). If highly skilled jobs are

¹See Passel and Krogstad (2025) for more details over years.

²For instance, Show-Me-Your-Papers laws compel police officers to audit the legal status of detainees upon reasonable suspicion of being undocumented. Statewide 287(g) laws, meanwhile, allow counties to enter into written agreements with the DHS so local law enforcement can collaborate with ICE in enforcing immigration law.

³Florida passed Universal E-Verify in 2023.

disproportionately affected, the marginal return to higher education is reduced. For these reasons, it is natural to suspect that Universal E-Verify leads to lower college attendance among undocumented immigrants.

In this paper, I use ACS data and design a series of event studies that account for staggered treatment adoption to estimate the impact of Universal E-Verify on the college attendance of undocumented Hispanics ages 18-24. Specifically, I implement both an extended two-way fixed effect strategy, following the insights from Wooldridge (2021), and a synthetic difference-in-differences (SDID) design as a robustness check (Arkhangelsky et al. 2021). These approaches are underutilized in the existing literature on the education of undocumented immigrants. Since undocumented status is not directly observable in large survey data, I predict undocumented status with a random forest algorithm, using the 2004 and 2008 Survey of Income and Program Participation (SIPP) as training data, and testing whether it yields similar results to more widely-used imputation methods. While not my primary focus, I also extend this strategy to evaluate the potentially confounding impacts of other statewide immigration legislation. To test for potential spillovers to citizens, I also examine whether Universal E-Verify affects the college attendance of citizen Hispanics.

I ultimately find that Universal E-Verify is associated with a 3.1 to 3.7 percentage point reduction in college attendance for undocumented Hispanics between ages 18 and 24, depending on the specification used. This effect is substantial relative to the average attendance of undocumented Hispanics, which was only 15.7 percent in post-treated states. This result is robust to specifications designed to address potential bias from selective out-migration and confounding state-level events. This result is also robust to using logit as my prediction method instead of random forest and to using logical imputation to proxy undocumented status. When I explore potential mechanisms, I show that Universal E-Verify lowers employment among adult undocumented Hispanics ages 25-54. While this finding is not new in the immigration literature, it suggests that Universal E-Verify negatively impacts the income of undocumented parents, which reduces household welfare, impairs secondary education outcomes, and renders college less attainable for children.

The rest of this paper is structured as follows:

I motivate the potential importance of Universal E-Verify to education in Section II. I explain the legislative background of Universal E-Verify and other policies of interest in Section III. I create a partial equilibrium model and describe the theoretical framework through which Universal E-Verify may affect college attendance in Section IV. I note my data sources in Section V. I outline various approaches for imputing and predicting undocumented status in Section VI. I present my primary empirical strategy in Section VII and explain specific approaches that can be taken. I report descriptive statistics for treatment and control groups in Section VIII. I provide my main results in Section IX. I discuss identification concerns and robustness checks in Section X, in which I also implement synthetic difference-in-differences to corroborate my primary method. I empirically explore the potential mechanisms from my theoretical framework in Section XI. I make my concluding remarks in Section XII.

II. Motivation: Universal E-Verify and Education

Previous research has found that greater immigration enforcement decreases schooling among undocumented children. Dee and Murphy (2019) find that ICE partnerships with local law enforcement reduce the number of Hispanics in public school by 10 percent. Bucheli, Rubalcaba, and Vargas (2021) demonstrate that higher ICE arrest rates negatively impact the school enrollment of Hispanic citizens and non-citizens alike, both among younger students and those who are college age. Kirksey et al. (2020) use panel data to evaluate performance gaps in school and discover that increased deportation rates are associated with both increased Latino absenteeism and larger White-Latino achievement gaps in mathematics. Amuedo-Dorantes and Lopez (2017) demonstrate that the schooling of the children of undocumented Hispanics is negatively affected by immigration enforcement regardless of the child’s citizenship status.

While most of these papers focus on police-based or deportation-based policies, the impact of employment-based policies is less explored. Amuedo-Dorantes and Lopez (2017) do find one interesting result along this dimension: while police-based policies particularly harm the schooling of younger children, employment-based policies (E-Verify legislation in their study) reduce the school attendance of children ages 14 to 17.

The potential for restrictions on undocumented employment to affect children comes through the labor market impact on parents. Previous research has found that E-Verify laws lead to worse outcomes for undocumented immigrants: Borjas and Cassidy (2019) demonstrate that the wage penalty for undocumented Hispanics compared to documented Hispanics increases by 4.5 percentage points due to Universal E-Verify. Orrenius and Zavodny (2015) find that male undocumented Mexican immigrants earn lower wages due to state-level E-Verify mandates. While they do not find an effect on employment in that paper, they do find that young male undocumented immigrants migrate away from states with E-Verify mandates to seek better labor market outcomes. Even with this out-migration, other papers do find employment effects. Orrenius, Zavodny, and Gutierrez (2018) estimate that Universal E-Verify lowers Hispanic employment by 3.1 percentage points. Hansen (2019) implements an event study and demonstrates similar effects, estimating that Universal E-Verify laws are associated with 3.4 percentage points of lower employment and 2.7 percentage points of lower wages for male non-citizen Hispanics.

The labor market shock caused by Universal E-Verify implies a negative effect on human capital accumulation for the children of undocumented immigrants. Pan and Ost (2014) demonstrate that sudden job loss among parents lowers the eventual college attendance for their children. This can occur because educational resources and college itself become less affordable due to the labor market shock. It is also possible that Universal E-Verify affects the marginal return to college education. For example, if industries that more strictly enforce Universal E-Verify hire highly educated workers relatively more than industries with lenient enforcement, young undocumented immigrants might be deterred from investing in college.

While these potential mechanisms affect undocumented immigrants, the impact on Hispanic citizens is unclear. On one hand, Hispanic citizens and non-citizens may compete for college enrollment slots, meaning a policy that lowers non-citizen Hispanic education

might raise citizen Hispanic education. On the other hand, the U.S.-born citizen children of undocumented immigrants may be as negatively affected by the labor market shocks as undocumented children. This latter mechanism could negate any positive aggregate effect for citizen Hispanics.

III. Legislative Background: Universal E-Verify and Other Laws

Universal E-Verify

In 1997, the federal government founded the Basic Pilot Program, later re-branded to E-Verify, as an improved way for employers to verify the work eligibility of prospective employees. The program uses an online database that attempts to match identifying information from an individual’s I-9 form to federal records. Failure to find a match signifies that the individual is ineligible to work in the United States. As of 2007, facial recognition data have been incorporated, allowing any accompanying pictures stored on a federal database to be sent to employers to ensure that the provided I-9 information matches the prospective employee.

Since the creation of this program, many states, along with the federal government, have required public employers and contractors to vet candidates through E-Verify. Nine states as of 2022 went further by requiring all private employers to use the system.⁴ These latter state-level laws are collectively referred to as Universal E-Verify. The first state to do so was Arizona, which implemented the Legal Arizona Workers Act (LAWA) in 2008. While the uptake of the E-Verify system in response to such laws is sometimes gradual, the number of employers who use Universal E-Verify dramatically increases following implementation (Ellis et al. 2014).

Although the goal of these laws is to strictly enforce regulations against employing unauthorized workers, there are some indications that the E-Verify system has gradually become less effective over time at preventing undocumented hires.⁵ Even in the Universal E-Verify states, the system is not always required for all firms, as exemptions often exist for businesses with fewer than a set number of employees.⁶ Furthermore, even with a sharp increase in use of the E-Verify system, many businesses do not comply: Orrenius, Zavodny, and Greer (2020) report that while E-Verify is much more widely used in Universal E-Verify states, take-up among all employers in these states remained under 50 percent in 2015. But even with non-universal uptake across employers, Universal E-Verify has been shown

⁴I count Louisiana, which in 2011 passed a law requiring all employers to either use E-Verify or retain the work-authorization documents of their employees. My results are similar if I consider Louisiana untreated. In addition, Florida became the 10th state to implement Universal E-Verify in 2023, which falls outside my sample range.

⁵Brier (2019) finds that E-Verify only prevented the hiring of 16.1 percent of unauthorized workers who encountered the system in 2018 – a great decline from when the system began to become widely used. This report nevertheless indicates that E-Verify had a substantial impact in its early years.

⁶The maximum number of employees that a business can employ without using E-Verify in these states varies by legislation. Some states also staggered the adoption time so that smaller business would not be required to use E-Verify for a couple years after the legislation was enacted.

to lower the employment and wages of likely-undocumented workers (Orrenius and Zavodny 2015; Orrenius, Zavodny, and Gutierrez 2018; Borjas and Cassidy 2019; Hansen 2019).

It should be noted that since many states implement some kind of significant immigration policy that potentially impacts education, it may be very challenging to isolate an effect of Universal E-Verify that is not confounded by other laws. I therefore account for and briefly analyze three other state-level policies that might affect college education apart from Universal E-Verify: Show-Me-Your-Papers (denoted SMYP), other omnibus immigration legislation that doesn't include Universal E-Verify or SMYP (denoted OIL), and in-state tuition (denoted TUIT).

Figure 1 maps these policies across the United States as of 2022. I also label TUIT states that passed supplemental laws granting state-level financial aid to undocumented immigrants. Note that all SMYP states are also Universal E-Verify states. Each of these law types should be fully understood to conceptualize the identification challenges presented by other policies.

SMYP

Some states that implemented Universal E-Verify also passed SMYP bills. The first of these was SB-1070 in Arizona, a 2010 law that required immigrants to constantly carry identification and for police to question anyone they reasonably suspect of being undocumented during a stop or arrest. Under the law, police were required to detain individuals lacking identification until legal status could be proven. South Carolina, Georgia, and Alabama passed similar provisions shortly after, but state courts pre-emptively blocked enforcement of parallel legislation in both Indiana and Utah, and these states never ultimately enforced the policy. The wider legal dispute reached the Supreme Court in the 2012 case *Arizona v. United States*. The Court decided that the sections of Arizona's law allowing warrantless detainment and requiring people to carry ID to be unconstitutional. However, the Court found that the section requiring police officers to determine the citizenship of detained individuals was constitutional.⁷ This portion of the law remains in effect today.

Stricter police-based enforcement policies like SMYP are associated with worse schooling outcomes (Amuedo-Dorantes and Lopez 2017). Since all SMYP states are also Universal E-Verify states and both sets of laws were passed in similar timeframes, it is important to investigate whether any estimated effect of Universal E-Verify is driven by SMYP.

OIL

In addition to Universal E-Verify and SMYP, states have passed packages of other restrictive immigration policies, commonly termed omnibus immigration legislation (OIL). These various policies may include restrictions on occupational licensing, E-Verify requirements for public employers, explicit bans on receiving in-state tuition and financial aid, or authorizations for various counties to pursue 287(g) agreements with ICE.

Since OILs may include both employment-based and police-based enforcement policies, they, like SMYP, could potentially impact education. To isolate the effect of any of these other restrictive policies, from this point forward, I define OIL as any omnibus package that

⁷*Arizona v. United States*, 567 U.S. 387 (2012).

did not include Universal E-Verify or SMYP.⁸

An overview of the timing of Universal E-Verify, OIL, and SMYP implementation by state is provided in Table 1. I choose to exclude Indiana and Utah from the list of SMYP states because those laws never went into effect.

TUIT

Not every new law has been restrictive. A growing number of states have allowed resident undocumented immigrants to pay in-state tuition at public universities. The first states to offer in-state tuition were California and Texas, which both passed legislation in 2001. By 2022, this policy had expanded to 21 states and the District of Columbia. 13 of those states also allowed undocumented immigrants to receive state-funded financial aid by 2022.

While it seems intuitive that in-state tuition policies would increase college attendance among undocumented immigrants, eligibility is subject to several key conditions. The laws typically require a prospective student to prove they have lived in the state for multiple years and have graduated from high school within the state. In addition, the application process to receive state-funded financial aid can be arduous, complicated, and underutilized.⁹ Perhaps most crucially, benefits are contingent on signing an affidavit promising to pursue legal residency. In other words, an undocumented immigrant must declare their legal status to the state government. Such a condition may deter those unwilling to admit their legal status. While many earlier papers estimate that in-state tuition leads to improved undocumented college attendance (Kaushal 2008; Flores 2010; Dickson and Pender 2013; Amuedo-Dorantes and Sparber 2014; Kuka, Shenhav, and Shih 2020; Averett et al. 2024), the results are sensitive to the dataset and methods used: Chin and Juhn (2011) use early ACS data and fail to find an impact of in-state tuition on non-citizen college attendance, while Averett et al. (2023), in an earlier version of their 2024 paper, use a staggered event study design and only find effects for non-citizen Mexicans rather than non-citizen Hispanics in general – a similar result to Bozick, Miller, and Kaneshiro (2016). Nevertheless, there is potential for these laws to confound an estimated effect of Universal E-Verify.

Since one might expect the passage of these tuition policies to primarily impact application to college, it is likely that any effect only becomes apparent once the next prospective students have the chance to apply. Therefore, I consider the year after which a law was first passed to be the event year, so long as the law was passed after the month of March. I provide a list of these years in Table 2.

⁸Specifically, I consider an OIL state to be one that did not pass Universal E-Verify or SMYP in the same year of its first omnibus package. Since states can pass different OILs in different years, I consider the first year of passage as the event year. By structuring the OIL treatment group in this way, I can isolate the potential overall effects of less prominent miscellaneous policies.

⁹For example, California began allowing access to state-funded financial aid in 2012, but the application process is complex to navigate, leading to only 14 percent of undocumented college students receiving aid in 2021 (see the California Student Aid Commission’s 2023 “Renewing the Dream” summary in the Undocumented Student Affordability Report for more information).

IV. Theoretical Framework: The Effect on Schooling

To more formally identify the mechanisms through which Universal E-Verify may impact college attendance, consider the following partial equilibrium model for schooling decisions within an undocumented immigrant household, along with comparative statics for some parameters of interest:

Consider a household in which parent p lives for one period, and teenager/young adult k lives for two periods. In the first period, the parent decides all household allocations, while the teenager becomes the sole adult in the household in the second period. Both household members have log utility over consumption units c and are allocated T units of time per period. The parent earns wage w_p , which will be taken as exogenous. The teenager's units of time can be distributed between working and schooling. If the teenager works, they will earn a low-skill, entry-level wage w_0 per unit of time worked. If the teenager goes to school for s units of time, they will earn a wage of $w(s) = w_0 e^{rs}$ in the second period, where growth factor $r > 0$ represents the marginal return to an additional unit of schooling. Schooling is costly at a price of P_s . Finally, the parent is altruistic by a factor of $\alpha \in (0, 1)$, and the teenager is time discounting by a factor of $\beta \in (0, 1)$. For the sake of simplicity, this model assumes that intra-household allocations are independent of the relative earnings of household members.¹⁰

The maximization problem for the household takes the following form:

$$\begin{aligned} & \max_{s, c_p, c_k} \{ (1 - \alpha) \ln(c_k) + \alpha \beta \ln[(w_0 e^{rs})T] \} \\ & \text{subject to } c_p + c_k + P_s s \leq w_p T + w_0(T - s) \end{aligned}$$

This can be re-written to the more convenient equivalent form:

$$\begin{aligned} & \max_{s, c_p, c_k} \{ (1 - \alpha) \ln(c_k) + \alpha \beta r s \} \\ & \text{subject to } c_p + c_k + (P_s + w_0)s \leq T(w_p + w_0) \end{aligned}$$

Solving the maximization problem yields the following level of optimal schooling:

$$s^* = \begin{cases} \frac{T(w_0 + w_p)}{w_0 + P_s} - \frac{1}{\alpha \beta r}, & \text{if this term is between 0 and } T, \\ T, & \text{if the above term is } > T, \\ 0, & \text{if the above term is } < 0. \end{cases}$$

Consider the following partial derivatives, assuming an interior solution for s^* :

¹⁰This assumption is contested by household bargaining models and by empirical research demonstrating that intra-household allocations can change if one member's income changes, even if total household income is unchanged. Basu (1999) describes an alternative setup for a child labor setting in which α is a function of the income contributions of each household member.

$$\frac{\partial s^*}{\partial w_p} = \frac{T}{w_0 + P_s} > 0$$

$$\frac{\partial s^*}{\partial r} = \frac{1}{\alpha\beta r^2} > 0$$

$$\frac{\partial s^*}{\partial P_s} = -\frac{T(w_0 + w_p)}{(w_0 + P_s)^2} < 0$$

$$\frac{\partial s^*}{\partial w_0} = \frac{T(P_s - w_p)}{(w_0 + P_s)^2}$$

There are a few important implications. First, as the parent's wage decreases, schooling decreases. The underlying intuition is that the decline in parental income makes schooling less affordable. Notice also that a decline would occur even if schooling were free, i.e. $P_s = 0$. This is because lower parental income pushes the teenager/young adult into the labor force to stabilize household consumption. Second, we see the expected results for the price and marginal return to schooling: an increase in each one has a negative and positive effect on schooling, respectively. Third, the effect of a change in w_0 is ambiguous: $\frac{\partial s^*}{\partial w_0} > 0$ if and only if $P_s - w_p > 0$.

This result is explained by competing income and substitution effects: as w_0 decreases, the household can afford fewer units of schooling. At the same time, the household earns less income from the teenager/young adult working, which reduces the opportunity cost of schooling. The former effect dominates only if schooling is expensive relative to the parental wage. A potential implication of this result is that a decrease in w_0 might increase schooling when schooling is less expensive (e.g. if the teenager attends public high school) and decrease schooling when it is more expensive (e.g. when the teenager decides whether to enroll in college). Thus, investment in high school education may increase as entry-level undocumented wages decrease, all else being equal.

These implications are important to understanding how Universal E-Verify could potentially impact education. Recall that Universal E-Verify affects prospective employees. This implies not only a possible movement of job seekers into more informal, less enforced sectors, but also an increased cost to search for better paying work. Orrenius, Zavodny, and Gutierrez (2018) back this implication by showing that Universal E-Verify leads to less job turnover. Of course, there is also the direct negative employment effect on adult prospective employees. This effect implies that Universal E-Verify would not only lower a low-skilled entrant's wage w_0 , but also the parental wage w_p , and more broadly, the employment and overall earnings of the parent.¹¹ Previous research backs the model's prediction that worse parental labor market outcomes are detrimental to the future educational outcomes of children: Pan and Ost (2014) find that parental job loss is associated with ten percentage points of lower college enrollment among children who were between ages 15 and 17 at the time of

¹¹The model does not explicitly incorporate parental unemployment, but $w_p = 0$ may represent the situation in which the parent is not employed.

the job loss.

To investigate this possible mechanism, I can verify previous research on Universal E-Verify by examining whether labor market outcomes are impacted. This would involve focusing on non-citizen Hispanics, a group that is mostly made up of undocumented immigrants and is commonly used as a proxy given the lack of directly observable status, who are past college age (ages 25-54). If their outcomes are negatively impacted, a negative effect on the college attendance of undocumented children is to be expected.

Universal E-Verify's effect on the marginal return to schooling r , however, is unclear. On the one hand, firms with more highly educated occupations might comply more stringently with Universal E-Verify laws, while smaller firms might be exempted, and employers in more informal sectors might simply not comply. If the former group both uses E-Verify more often and disproportionately hires more highly educated workers, then r should decrease due to Universal E-Verify. On the other hand, low-educated undocumented Hispanics may be more seasonal in their employment and work in high-turnover jobs, meaning they may switch between employers more often. This means that this group could be exposed to more frequent E-Verify screenings than those in stable, high-skilled occupations. Low-skilled workers without college education might also lack the resources to find ways around the law that higher educated individuals may possess.¹² Thus, it is theoretically unclear how Universal E-Verify affects r .

I can test whether any decrease in college education is caused by a decline in the marginal return to schooling r . I can do this by examining whether labor market outcomes among highly educated undocumented workers are more impacted by Universal E-Verify than those of lower-educated workers. If higher-educated workers are more negatively affected, there would be evidence that the law lowers the marginal return to education.¹³

I can also partially test how Universal E-Verify affects schooling outcomes prior to college. I can do this by testing for effects on school attendance for those ages 14 to 17. An underlying idea from the model is that while college is expensive, secondary school is nominally free in most cases ($P_s = 0 < w_p$), meaning that at, say, the high school level, a decrease in w_0 should positively affect attendance, all else being equal. Thus, even with a decrease in w_p , it is theoretically unclear that schooling would be negatively impacted at this level, even though Amuedo-Dorantes and Lopez (2017) do estimate a negative effect.

A final important note is that this relatively simple model does not account for the fact that an immigrant can move between states in response to policy changes. The model can be extended to incorporate this possibility:

Suppose that the household in the first period is in location Q but can move to location R , which may offer different wages, schooling prices, and returns to education. There is, however, a cost M of moving. The maximization problem then becomes:

¹²For instance, one could provide an employer with an I-9 form containing the information of an authorized worker, which would prevent the E-Verify system from quickly flagging their status. It's possible that higher-educated undocumented immigrants might be more resourceful regarding this work-around.

¹³Suggestive evidence for this possibility can be inferred from Borjas and Cassidy (2019): according to their results, Universal E-Verify increases the undocumented wage penalty by 3.8 percentage points for low-educated undocumented males but by 4.8 percentage points for high-educated undocumented males.

$$\max_{L \in Q, R} \{V_Q, V_R\}$$

where V represents the total household utility of the two periods in each location, as derived from the original maximization problem. For location Q , this value is found by solving the same problem as before, assuming local exogenous variables w_{pQ} , w_{0Q} , r_Q , P_{sQ} . For alternate location R , the maximization problem becomes:

$$\begin{aligned} & \max_{s, c_p, c_k} \{ (1 - \alpha) \ln(c_p) + \alpha \ln(c_k) + \alpha \beta r_R s \} \\ & \text{subject to } c_p + c_k + (P_{sR} + w_{0R})s \leq T(w_{pR} + w_{0R}) - M \end{aligned}$$

It is relatively straightforward to solve for V_Q and V_R , assuming no corner solutions:

$$\begin{aligned} V_Q &= (1 - \alpha) \ln \left[\frac{(P_{sQ} + w_{0Q})}{\beta r_Q} \left(\frac{1 - \alpha}{\alpha} \right) \right] + \alpha \ln \left(\frac{P_{sQ} + w_{0Q}}{\beta r_Q} \right) + \frac{\alpha \beta r_Q T(w_{0Q} + w_{pQ})}{w_{0Q} + P_{sQ}} - 1 \\ V_R &= (1 - \alpha) \ln \left[\frac{(P_{sR} + w_{0R})}{\beta r_R} \left(\frac{1 - \alpha}{\alpha} \right) \right] + \alpha \ln \left(\frac{P_{sR} + w_{0R}}{\beta r_R} \right) + \frac{\alpha \beta r_R [T(w_{0R} + w_{pR}) - M]}{w_{0R} + P_{sR}} - 1 \end{aligned}$$

Suppose for a moment that locations Q and R initially are identical, but then Q passes Universal E-Verify. Assume also for simplicity that the policy only lowers w_{pQ} but affects nothing else. One can show that, under this setup, $V_R > V_Q$ if and only if $T(w_{pR} - w_{pQ}) > M$. In other words, the household will only move if the potential increase in parental earnings outweighs the cost of moving.

While this is only a simplified example, as no other parameter of interest is affected, it does demonstrate that undocumented immigrants will be more likely to move if they are more negatively affected by the policy and if their potential earnings are high enough to justify the moving cost. This presents a significant concern for any empirical estimation: highly educated, high-income workers may be more likely to move and send their children to college in a different state if they can afford the moving cost. Indeed, Ellis et al. (2014) find that those with a college education were more likely to move in response to Arizona's Universal E-Verify law. Such a pattern could bias the estimated effect to appear more negative. But at the same time, the cost of moving itself acts as a shock to household income, and the act of moving between schools may negatively affect child educational outcomes. Ellis et al. (2014) accordingly find the probability of moving to be negatively associated with the number of children in the household. Also, if these laws disproportionately affect low-income job seekers whose children were already unlikely to go to college absent treatment, it may be less likely that those who move would have attended college without the law. If anything, this would bias the estimated effect to be less negative. Either way, it is essential to consider migration between states.

V. Data: The ACS

My primary specifications use the 2005-2022 American Community Survey (ACS) 1 percent samples combined with the 2000-2004 ACS pilot years, although I also exclude the pilot years as an alternative specification.¹⁴ I will describe in more detail why I include these earlier years shortly, but generally, I use the ACS because I require extensive coverage to run event studies on state-level policies. This is because undocumented immigrants may be under-represented in surveys, treatment for Universal E-Verify up to 2022 occurs in only nine (and not necessarily large) states, and my sample of interest is constrained to college-age non-citizen Hispanics (18-24) out of secondary school and without at least a bachelor's degree. A large dataset is necessary to include enough observations for such a restricted sample universe.

Since the ACS from 2000-2004 was still in its pilot phase, those years include significantly fewer observations and do not cover intra-state regions nearly as comprehensively as the 1 percent samples. It could be possible that undocumented immigrants from these early years were selected into the survey differently than those in later years. Moreover, the limited sample size eliminates many state-year cells from 2000 to 2004.¹⁵ However, including these earlier years allows me to capture longer pre-trends for important treated states. Arizona, for instance, passed its Universal E-Verify law in 2008, meaning any estimated treatment effects more than three periods before the event time would not be included for that state if I only go as far back as 2005. Failing to go back further would render estimated pre-treatment effects more than three years before policy adoption less interpretable, as they would reflect different cohorts of treated states than my post-treatment effects. Given the pros and cons of each option, I present both the results with the full 2000-2022 time frame and the 2005-2022 time frame, although I consider 2000-2022 to be my primary specification. Ultimately, the choice does not usually make a noticeable difference to the estimates.

In addition to data on labor market outcomes, demographics, years since immigration, schooling, and college attendance, the ACS contains a discrete variable for English speaking ability. The survey question asks whether respondents speak exclusively English, speak English very well but not exclusively, or speak English well, not well, or not at all. I create a binary variable for fluency in which the former three categories are defined as fluent. This is a variable that I use for imputing undocumented status, as I will discuss in the next section. I also create a binary indicator for college education that is equal to one if an individual is attending college at the time of the survey.

The ACS does not include data on multiple state-level controls that may be important to this analysis. Thus, I use data on state-level unemployment rates, state-level GDP, and state-level college tuition prices from (1) the Iowa Community Indicators Program, (2) the United States Regional Economic Analysis Project, and (3) the 2000-2022 NCES Digest of Education Statistics. These variables act as controls for variation in economic conditions

¹⁴All ACS data is downloaded from IPUMS.

¹⁵While the 2005-2022 data contain sufficient observations for most state-year cells, less than 25 states typically contain at least 20 observations between 2001 and 2004, and only ten states are likewise populated in 2000.

and educational prices across states and time. I determine the years that various in-state tuition and financial aid policies were enacted using The Higher Ed Immigration Portal, and I use the National Conference of State Legislatures for enforcement-based and employment-based policies, along with existing legislative records from state databases to clarify any uncertainties. Finally, I use survey data from the 2004 and 2008 SIPP as training data to impute undocumented status, as I will next discuss.

VI. Imputing Undocumented Status

In the ACS (as in most survey data), true undocumented status is not observable. Thus, one of the most significant questions in using survey data to study undocumented immigrants is in choosing how to proxy for undocumented status. A commonly used solution is to simply restrict the sample to groups more likely to be undocumented, such as non-citizen Hispanics. The issue, of course, is that this sample inevitably includes a significant proportion of documented immigrants. A common alternative method is to logically impute whether a non-citizen is documented by checking if they meet some set of criteria. For example, Borjas and Cassidy (2019) use the following indicators to determine documented status:

1. The immigrant arrived before 1980
2. The immigrant receives Social Security, SSI, Medicare, Medicaid, or military benefits
3. The immigrant is in the military
4. The immigrant is in the public sector
5. The immigrant either receives or is a spouse of someone who receives rental subsidies, or resides in public housing
6. The immigrant’s spouse is confirmed to be a legal resident
7. The immigrant’s occupation requires licensing

The idea behind this approach is to whittle down the sample of non-citizens by eliminating individuals who are highly unlikely to be undocumented based on these observables. While logical imputation is a notable improvement in determining documented status, even this improved method will inevitably fail to remove all documented immigrants from the sample. Not only that, but Van Hook et al. (2016) and Ruhnke, Wilson, and Stimpson (2022) find that logical imputation can lead to more biased estimates relative to alternative statistical approaches. Logical imputation may also sometimes inadvertently eliminate true undocumented observations: Ruhnke, Wilson, and Stimpson (2022) demonstrate that a small number of undocumented immigrants claim to receive Medicaid in surveys.¹⁶ More significantly, since E-Verify mandates for public employees were only required at the federal level and in 20 states as of 2022, restricting the sample to those who don’t work in the public sector could selectively eliminate undocumented immigrants from states without any such

¹⁶Using Medicaid as an indicator would also be problematic for my sample of interest because the ACS does not record Medicaid coverage prior to 2008.

regulation.¹⁷ Furthermore, several states in recent years have made occupational licensing available for undocumented immigrants, further risking selective elimination from the sample based on state policies.¹⁸

A different approach is to use statistical methods, such as a logit model, based on training data in which undocumented status is verifiable. This method is not necessarily free from concerns of bias either: both Van Hook et al. (2016) and Ruhnke, Wilson, and Stimpson (2022) demonstrate that both logit and logical imputation can lead to bias by misclassifying observations. To explore potential improvements to these more widely used approaches, Ruhnke, Wilson, and Stimpson (2022) study whether machine learning algorithms make for better predictive tools. Specifically, they test the performance of both K-nearest neighbor matching and random forest in determining undocumented status compared to logit and logical imputation. A particular advantage of random forest is its lower likelihood of overfitting compared to other machine learning approaches (Buskirk and Kolenikov, 2015). They determine that random forest, when combined with logical imputation, outperforms logical imputation on its own, logit and logical imputation combined, and KNN and logical imputation combined - both in overall accuracy and in minimizing bias when estimating effects on health outcomes.

It is possible that such improvements may not apply to my setting for a few reasons. First, Ruhnke, Wilson, and Stimpson (2022) include non-Hispanics in their study, who are less likely to be undocumented. Second, since their paper does not focus on policies that impact employment or education, they can use these factors in their predictions, which I cannot do since these are potentially affected by Universal E-Verify. Third, Ruhnke, Wilson, and Stimpson (2022) include dummy variables for country of origin in their random forest algorithm, but I opt not to do the same: while they focus on a sample of interest in the mid to late 2000s, my sample extends all the way to 2022. Training the algorithm based on country of origin in 2004 or 2008 (and using that training to predict undocumented status for later periods) risks producing inaccurate predictions in later years if source country patterns have changed. This is especially pertinent, as undocumented immigrants from Hispanic countries to the United States have increasingly arrived from Central America rather than Mexico in more recent years.¹⁹ Including country of origin in 2004 or 2008 as a predictor would lead to an overestimated probability that a Mexican immigrant in later years is undocumented (and an underestimated probability that a non-Mexican Hispanic is undocumented).

The downside to not including as rich a set of covariates is that my predictions will likely be much less accurate, so it is unclear whether random forest will offer an advantage relative to logit or to logical imputation. I therefore run a similar set of simulations to determine whether either method offers any improvement over logical imputation alone.

As training data, I use the 2004 and 2008 SIPP samples of foreign-born Hispanics ages

¹⁷In addition to the nine Universal E-Verify states, Idaho, Indiana, Michigan, Minnesota, Missouri, Nebraska, Oklahoma, Pennsylvania, Texas, Virginia, and West Virginia have varying E-Verify requirements on public employees and contractors.

¹⁸Especially prevalent among these states is California, which has many undocumented immigrants and allows them to apply for occupational licenses across all professions.

¹⁹See Lopez, Passel, and Cohn (2021) for more details on changing migration patterns over time.

18-64. These two survey years contain questions about the legal status of immigrants upon entry into the United States. Specifically, it is asked whether (1) the respondent had been granted permanent legal resident status upon entering the country and (2) whether their status has changed since then.²⁰ From this set of responses, it is possible to directly observe self-reported permanent legal residency status. I use this binary indicator as a proxy for undocumented status. This is still not perfectly representative of true undocumented status due to long-term visa holders: the ACS surveys anyone with “usual” residency status (at least two months), so visa holders may be included even though they are not considered legal permanent residents. This means that long-term visa holders will remain in my SIPP proxied undocumented group if they answer that they do not have permanent residency status. Still, this proxy group does prevent lawful permanent residents like green card holders from being incorrectly identified as undocumented.

To test the performance of each approach, I implement the following procedure:

First, I begin with a logical imputation using only the most reliable indicators that someone is not undocumented. Namely, I limit the sample to Hispanics and drop anyone who meets the following criteria:²¹

1. receives any welfare payments such as food stamps
2. receives social security
3. supplemental support income
4. immigrated to the United States before 1981
5. is a veteran
6. is labeled as a citizen in the survey

In the case of the pooled 2004 and 2008 SIPP, this procedure whittles the sample down to 4,349 non-citizen Hispanic observations, 40.9 percent of whom are listed as undocumented.²²

Second, using this honed-down sample, I randomly assign 80 percent of remaining respondents into a simulated training group. I then run either random forest or logit on the training group using sex, poverty status, English fluency, years in the United States, number of children, age, whether one is married, whether they have a citizen spouse, and race as predictors. I use this training group to predict the undocumented status probability of the remaining 20 percent of observations that make up the simulated target group.

²⁰The 2014 SIPP also includes the first of these questions but excludes the second, so I elect to not use it.

²¹When exploring alternative approaches, I created binary variables for each of these criteria and include them in the prediction, rather than directly setting the probability of being undocumented to zero. However, this approach yields very slightly lower Brier and accuracy scores, so I elect not to use this method.

²²There is reason to believe that this proportion is not representative of the non-citizen Hispanic population. The Pew Research Center published data in 2023 indicating that there were 19.9 million foreign-born Hispanics in the US in 2021, 11.74 million of whom were not citizens (Moslimani and Bustamante 2023). Since a significant majority of the 10.5 million undocumented immigrants in the US as of 2021 were believed to be Hispanic, a representative proportion of non-citizen Hispanics who are undocumented should exceed 40.9 percent. This serves as evidence that there are either lower undocumented survey response rates or untruthful answers regarding legal residency status.

Third, I repeat the prior step in 100 simulations for both random forest and logit. For each simulation j , I calculate a Brier score:

$$\text{BrSc}_j = \frac{1}{K} \sum_{k=1}^K (\text{predict}_{kj} - \text{true}_k)^2$$

predict_{kj} is the predicted probability of being undocumented as calculated by random forest or logit, while true_k is the true undocumented status for observation k . I then average the 100 resulting scores together to calculate an average Brier score. A score equal to 0 means a perfect prediction fit, while a score equal to 1 implies a perfectly imprecise prediction.

The Brier score is a standard prediction test, although it does penalize outliers through the squared term more than a linear accuracy score. To ensure the robustness of my results to alternative measures of prediction performance, I create a linear accuracy score:

$$\text{ACC}_j = \frac{1}{K} \sum_{k=1}^K \left[(\text{predict}_{kj} \mid \text{true}_k = 1) + (1 - \text{predict}_{kj} \mid \text{true}_k = 0) \right]$$

I average the resulting accuracy scores together to create an average accuracy score. If the method being tested perfectly predicts undocumented status every time, the total sum would be equal to 1 since each accuracy score would equal 1. By the same logic, a perfectly inaccurate prediction each time would receive a score of 0.

The full results of these tests are copied in Appendix Table A1. The logit/logical imputation tandem yields a Brier score of 0.1862 and an average linear score of 0.4499, while the random forest/logical tandem yields 0.2419 and 0.5565. Clearly, both methods do not offer optimal predictions. However, even with the limited set of covariates used, they still offer improvement over logical imputation alone. To show this, I create analogous accuracy and Brier scores for logical imputation over 100 simulations simply by letting the predicted probability equal 1 for all observations in the target group. Using logical imputation alone only yields Brier/accuracy scores of 0.5874 and 0.4126. I ultimately choose random forest as my primary specification, although I also use logit as a robustness check given its better Brier score.²³ To extend these methods to my primary dataset, I use the combined 2004 and 2008 SIPP 18-64 Hispanic sample as training data for the 2000-2022 ACS and repeat my imputation procedure.

Appendix Figure A1 displays the histograms of imputed undocumented probability for all Hispanic non-citizens ages 18-24 out of secondary school with less than a bachelor's degree in the ACS. Included are both the random forest and logit predictions when combined with logical imputation in the first step. The average predicted probability within the sample is 0.446 for random forest and 0.505 for logit. The two approaches have a correlation coefficient

²³A possible advantage of random forest of logit is that it seems more robust to variations in the training data used. An earlier approach I took was to use only the 2008 SIPP. The accuracy and Brier scores hardly changed for random forest. However, the logit approach varied by a few percentage points from using both 2004 and 2008, with a better accuracy score when using the limited sample, at the cost of a worse Brier score.

of 0.499.

In addition to their sub-par predictive performance metrics, a concern with random forest and logit is that undocumented immigrants in the 2004 and 2008 SIPP might be too systematically different from observations in later survey years to produce accurate predictions across all years in my sample. Therefore, I also use logical imputation alone to hone my sample to those more likely to be undocumented. I report my primary results using this simple logical imputation group of non-citizen Hispanics (often simply denoted as “non-citizens” in my results figures) to see if these various imputation approaches make a noticeable difference.

VII. Empirical Strategy

General Setup

My primary strategy is to run an event study that accounts for the staggered adoption of Universal E-Verify across states over time. An event study is particularly advantageous in this setting. Not only does it account for unobserved, time-invariant factors that vary across treated and controlled units, but it provides dynamic estimated effects that are crucial for me to analyze: recall that the impact of Universal E-Verify may be concentrated among those who were still living with their parents and below college age, implying that it would take some time for the affected group to fully matriculate into the 18-24 college-age sample. This means that the effects of the policy could become more pronounced in later event years.

To account for staggered adoption, I use an approach that follows the extended two-way fixed effects (ETWFE) setup suggested by Wooldridge (2021).²⁴ Namely, I run the following estimation for each policy over time periods $1...T$, where 1 is the first calendar year and T is the final calendar year:

$$\text{College}_{ist} = \vartheta_0 + \sum_{g \in G} \sum_{\tau \neq -1} (\alpha_{g\tau} D_{g\tau}) + \mathbf{X}'_{ist} \boldsymbol{\beta} + \mathbf{Z}'_{st} \boldsymbol{\sigma} + \delta_s + \rho_t + \varepsilon_{ist}$$

Here, G is the set of all treated cohorts, where each cohort g shares initial treatment year $t_g \in \{1...T\}$ and may comprise multiple states. $D_{g\tau}$ is a binary indicator that equals 1 if an observation is in both cohort g and event year τ , where $\tau = t - t_g$.

\mathbf{X}'_{ist} is a vector of individual characteristics (number of children, a quadratic in age, binary variables for sex, marital status, and race, and a quadratic in years since migration to the US for non-citizen specifications), while \mathbf{Z}'_{st} is a vector of state-by-year controls (a one-year lagged unemployment rate, real GDP per capita in 2017 dollars, average public in-state tuition levels, average private college tuition levels, and binary policy indicators along with the average college enrollment of non-Hispanic natives and non-Hispanic Black natives ages 18-24).²⁵ I explain my binary policy indicators in more detail below. δ_s and ρ_t are state

²⁴A special thanks to Thomas A. Hegland for creating the wooldid package to implement this procedure.

²⁵A final potential control to use is DACA status. Any undocumented immigrant who entered the United States at age 16 or younger by 2007, and is either a veteran, is enrolled in education, or already has a high

and time fixed effects.²⁶

The coefficient of interest within a given cohort-event year cell is $\alpha_{g\tau}$, which represents the average treatment effect on the treated (ATT) for the cohort-event year pair (g, τ) . To estimate an overall ATT for the entire sample, each $\hat{\alpha}_{g\tau}$ must first be estimated. Once these estimated effects are obtained, the predicted outcomes for all treated observations from the underlying regression are found. These are subtracted from the counterfactual outcomes, which are found by setting $D_{g\tau}$ to zero. The resulting values are averaged across all treated observations in event years $\tau \geq 0$ to report an average treatment effect on the treated across all groups (as well as an average pre-treatment effect using the same steps for all $\tau < -1$).²⁷ By implementing the same procedure across cohorts within each event period, I also estimate dynamic pre-treatment and post-treatment effects across the event window.

For these estimated effects to be unbiased, the parallel trends assumption must hold conditional on covariates. There must also be no anticipation effect during pre-treatment years. Ensuring a lack of parallel pre-trends and anticipation is possible through inspection of both the overall pre-treatment ATT and the dynamics of the pre-treatment period effects.

The first states implemented Universal E-Verify in 2008, while the final state did so in 2013. Since my data goes from 2000 to 2022, I choose an event window with ten post-periods and eight pre-periods. I exclude any treated observations outside this window in my primary specification.²⁸ I also drop any state-year cells that contain fewer than 20 total observations to reduce noise, although keeping these observations does not meaningfully alter my primary results.²⁹ In all cases, standard errors are clustered at the state level.³⁰

Using state-level GDP per capita and lagged unemployment rates as controls may be problematic. While these variables are sometimes used as counter-cyclical controls for college education, they might be affected by Universal E-Verify.³¹ If so, including them would bias

school degree is eligible for DACA, which grants a renewable two-year deferral in deportation proceedings, as well as temporary work authorization. Whether one can legally work in formal sectors may influence college enrollment decisions, but DACA itself depends in part on educational attainment and thus depends indirectly on any policy that affects schooling. Introducing it to the estimation raises a significant bad control concern. I therefore have only tried using a binary variable indicating whether someone is potentially DACA-eligible given their immigration history, regardless of educational status. Including this variable yields no significant differences for my main results, so I exclude it.

²⁶Wooldridge (2021) also interacts covariates with event time and group effects, allowing treatment effects to vary by covariates in addition to cohorts and time. I do not implement this full specification due to overfitting and multicollinearity concerns stemming from many covariates and limited observations in many state-year cells. This tradeoff is addressed in Deb et al. (2024), who suggest adding covariates additively as an alternative. I accordingly take this approach for most specifications.

²⁷Both never-treated and not-yet-treated states serve as controls for groups that do not include those states.

²⁸As an alternative, I also tried a specification that displays the same event window but estimates average treatment effects without dropping any observations outside the window. The results were very similar.

²⁹The results without dropping these state-years are available upon request.

³⁰The standard errors for the overall ATTs are computed using the delta method with the initial estimated coefficients and clustered SEs.

³¹For instance, Hansen (2019) shows that E-Verify laws lead to lower state-level GDP, largely due to negative impacts on the agriculture and construction industries – the very industries that employ the most

the post-treatment effects, although controlling for state and year fixed effects may absorb much of the variation in these covariates and mitigate this concern. Just in case, I run alternate specifications that exclude these controls.

My ideal sample universe would only consist of undocumented immigrants. Therefore, I weight each observation by the predicted probability of being undocumented, as estimated by the random forest algorithm (or logit, as a robustness check). As an additional robustness check, I restrict the sample to observations with at least a 50 percent estimated probability of being undocumented. While this threshold approach hones the sample down to those more likely to be undocumented, it significantly reduces the sample size and relies heavily on the accuracy of my prediction method around the 50 percent cutoff, so I retain the weighting approach as my primary method.

Finally, to determine whether my results might differ from more common approaches, I also run event studies without using the predicted probabilities. This is analogous to the more commonly used logical imputation on non-citizen Hispanics. Any comparison of these latter results with my main specification is especially interesting: if the results differ, it would indicate the importance of using more advanced techniques. If the results are similar, it would help to vouch for the empirical findings of previous research that implement logical imputation alone.

To evaluate the effect on college attendance, I restrict my sample universe to anyone aged 18 to 24 who has not yet received at least a bachelor’s degree and is not attending primary or secondary school.³² Appendix Figure A2 reports frequencies for the resulting number of observations in Universal E-Verify states by cohort year. Overall, there are 20,459 such observations, representing 12.9 percent of the sample.

Other Policies and Alternative Specifications

A challenge to identification is that different states passed different laws at different times. States might enact a policy in the post-treatment window that alters their trends. A policy in a treated state might also be conflated with a different one that is passed at a similar time. For instance, each state that passed a SMYP provision also passed a Universal E-Verify law at most two years prior. Clearly, various policies are not randomly distributed between treated and control states. If these alternative policies affect college attendance, the parallel trends assumption would be threatened. Testing for potential effects of other state-level legislation is thus a worthwhile endeavor.

I explore two methods to account for alternative policy adoption:

My first method, which I incorporate into my main estimations for Universal E-Verify,

undocumented Hispanics.

³²Even though including those who entered the United States as adults might water down a key potential mechanism (namely, a negative effect on household income while in high school), these migrants may yet be negatively impacted if their college is funded by family members or friends who also moved to the US, if they still live with their parents, or if they had family members living in the US beforehand who sent remittances back to their home country. Furthermore, a large proportion of undocumented college students arrived as adults: in 2023, 43 percent of all undocumented college students arrived in the US at age 18 or later according to a June 2025 report from the Presidents’ Alliance on Higher Education and Immigration. This is a potentially important group to include in my primary specifications.

is to run separate event studies for Universal E-Verify, TUIT, and OIL while including a set of binary variables indicating whether a state passed any alternative law.³³ For instance, if I am running an event study on Universal E-Verify, I include binary indicators for whether a state has already passed SMYP, TUIT, or OIL.³⁴ A non-effect from the non-E-Verify policies would suggest that they do not confound my results. Even if these policies do have an effect, including them as binary indicators in my main estimations for Universal E-Verify mitigates this concern.

My second method concerns SMYP specifically. Because all SMYP states are Universal E-Verify states, it is especially important to test whether SMYP itself has an effect, and if it does, to ensure that any estimated effect of Universal E-Verify is not driven by SMYP. I run this test by implementing my primary event study approach, but I exclude the five Universal E-Verify states that did not adopt SMYP. Dropping these states allows me to test for a marginal effect on college attendance relative to other Universal E-Verify states by comparing the estimated treatment effects for the two policies. I also run an event study for Universal E-Verify that excludes the four SMYP states to see if any estimated effect remains. This approach provides an additional check that any Universal E-Verify effect is not confounded by SMYP.

VIII. Descriptive Statistics

Before evaluating my results, I briefly examine how pre-treatment and control groups differ from one another. Table 3 records descriptive statistics for non-citizen Hispanics ages 18 to 24 in treated and untreated states for Universal E-Verify, weighted by sample weights times the random forest predicted probability of being undocumented, in survey years 2000-2007 (pre-treatment) and 2013-2022 (post-treatment). These ranges are chosen because the first Universal E-Verify policy took effect in 2008, while the last one took effect in 2013. The selected ranges therefore yield a clean comparison between treated and control states before and after implementation.³⁵

The resulting (probabilistically weighted) undocumented Hispanics in pre-treated states attend less college than those in control states. Specifically, only 4.9 percent of 18-24 un-

³³A downside to using these binary policy indicators is that they capture only state-level policies but not potentially important county-level policies within states like 287(g). Even limiting the control group to states without any other state-level policies would not alleviate this concern. Such a restriction would also drastically reduce my sample size because most states have at some point passed at least one of my policies of interest, leading to multiple causal inference concerns. For instance, to isolate a “clean” control sample for Universal E-Verify, I must drop any state that passes TUIT, OIL, or SMYP. This restricts my treatment groups to Mississippi with event year 2008 and Louisiana/North Carolina/Tennessee with event year 2012, but most Mississippi years are dropped due to insufficient sample size within the state. Such a limited sample, along with the limited set of control states, runs into a small number of clusters problem combined with insufficient statistical power.

³⁴I elect to not include financial aid policies as a binary indicator, as it is often passed in the same year as TUIT and might net out any treatment effect in the event year if used in specifications measuring the effect of TUIT.

³⁵For similar reasons, I use 2000-2001 as the pre-event window and 2022 as the post-window for TUIT, 2000-2005/2017-2022 for OIL, and 2000-2009/2011-2022 for SMYP (see Appendix Tables A2-A4).

documented Hispanics without a bachelor’s degree and out of primary/secondary school in pre-treated states were attending college between 2000 and 2007, compared to 9.5 percent in pre-treatment control states. Trends across most variables of interest seem roughly similar, although the gap in college attendance between treated and control states does increase by 2.5 percentage points.

These percentages alone do not reveal any clear changes after treatment. Examining the dynamics between treated and control states provides a more complete picture. Figure 3 plots average undocumented college attendance by year in ever-treated states and untreated states. Importantly, the raw trends between these two groups appear parallel up until 2010, around when more states began passing Universal E-Verify. College attendance trends for treated states flatten relative to untreated states between 2012 and 2016. However, given the occasional post-treatment spikes in college attendance, it is unclear from the raw trends alone if there is any effect. A rigorous estimation of dynamic post-treatment effects is crucial.

IX. Results

Main Results

Table 4 presents the overall ATTs for my primary event study estimations. I use random forest probabilities as weights in my first four specifications, but I also show two specifications in which I run an unweighted regression after restricting the sample to those with at least a 50 percent estimated undocumented probability. Across all specifications (including or excluding GDP per capita and the lagged unemployment rate, including or excluding the 2000-2004 pilot years, using or not using weights), Universal E-Verify is negatively associated with college attendance.

When I use random forest weights, the estimated post-treatment ATT is between -4.0 and -4.2 percentage points and is highly statistically significant. The average pre-treatment ATTs are negative but small and statistically insignificant in the weighted specifications. Figure 4 indicates that this slightly negative pre-trend was stable across years, implying that the difference between the pre-ATTs and post-ATTs likely reflects the true causal effect. This difference is between -3.1 and -3.7 percentage points depending on the specification. Note that according to the descriptive statistics in Table 3, the average college attendance for undocumented Hispanics was 15.7 percent on average from 2013 to 2022. Thus, in relative terms, this implies a 19 percent decrease from the counterfactual college attendance level in post-treatment state-years - a large effect. The specifications for the limited sample with at least a 50 percent undocumented probability yield even larger effects of -4.1 to -4.6 percentage points when the same post-event ATT and pre-event ATT difference is taken.

Table 5 displays the results using completely unweighted regressions for the non-citizen Hispanic sample with only logical imputation. Across all specifications, the estimated effects are similar to the ones that use random forest probabilities. Similar results also hold when I use logit undocumented probabilities instead of random forest, as displayed in Appendix Table A5. In that case, both the post-ATTs and the difference between the pre-ATTs and post-ATTs fall between -3.1 and -3.9 percentage points. The consistency of these estimations

provides greater supportive evidence. It also indicates that the three imputation strategies do not significantly differ in their estimates.

While examining the overall effects is essential, it is no less important to observe estimated effects across event time. These dynamics are illustrated using random forest and logit undocumented probabilities in Figure 4 and with logical imputation in Figure 5.³⁶

While the pre-treatment period effects show a slight downward pre-trend, most of these effects are statistically insignificant from zero using 95 percent confidence intervals. Furthermore, the post-period effects decline much more sharply than this trend. Four to eight years after treatment, undocumented Hispanics ages 18 to 24 are about 7 percentage points less likely to attend college. The effect is not as strong in the first couple treatment years, which matches the intuition that the effect operates through a labor market shock on parents: if parental outcomes are negatively affected, then the educational prospects of teenagers looking to attend college in future years would be affected. This would lead to lower college attendance only once former high school students matriculate into the college-age sample. It is also notable that there is no statistically significant treatment effect by event year 9 in my weighted specifications, although the standard error is large. While not definitive, this may be consistent with evidence that Universal E-Verify became a less detrimental hurdle for undocumented workers in the years following initial implementation, implying that they would produce a less detrimental effect on later college-age individuals.

Since the college attendance of non-citizen Hispanics seems to be negatively affected, it may be possible that there is a positive spillover on citizen Hispanics who compete for college admissions slots. However, the results indicate otherwise: as shown in Appendix Table A6 and Appendix Figure A4, there is no positive effect of Universal E-Verify on the college attendance of citizen Hispanics. If anything, there is a slightly negative effect. Such an effect may be driven by the citizen children of undocumented Hispanics. However, inspection of pre-event periods in Appendix Figure A4 reveals no parallel pre-trends and no clear pattern, so it is difficult to draw definitive conclusions.

Other Policies

Although I use binary policy indicators for other state-level laws as controls, it is worthwhile to investigate whether these policies produce effects on their own. I present the results for each of these three policies – TUIT, OIL, and SMYP – in Appendix Tables A7, A8 and A9, respectively. Event study dynamics for each of these policies are displayed in Appendix Figure A5. Recall that while specifications for TUIT and OIL include binary policy controls for Universal E-Verify, SMYP specifications exclude Universal E-Verify states without SMYP laws.

Interestingly, I find no evidence that TUIT increases undocumented college attendance. This may seem surprising given the importance of tuition prices to enrollment decisions. However, there are reasons to suspect that these policies might not be effective. Recall that

³⁶All event study figures in this paper include 95 percent confidence intervals. Also, since limiting the sample to 2005 and excluding GDP per capita and lagged unemployment rates does not noticeably alter the results, I use the full sample window with these covariates if I display only one specification. However, the 2005-2022 version of Figure 4 can be found in Appendix Figure A3 for comparison.

to be listed as an in-state resident under these policies, one must declare one’s undocumented status to the state government. This requirement may deter eligible beneficiaries from taking advantage of the policy. As previously discussed, not all papers estimate significant effects for non-citizen Hispanics. My results align with prior work that used staggered event studies and did not find robust effects for the overall non-citizen Hispanic population (Averett et al. 2023).

While my estimations indicate small positive post-treatment ATTs for OIL, inspection of Appendix Figure A5 reveals large and unstable pre-treatment effects. Due to this issue, I cannot determine whether these miscellaneous sets of laws affected college attendance.

Of the three alternative policies, only the event study for SMYP without non-SMYP Universal E-Verify states produces a stable pre-trend and a statistically significant effect on college attendance. The post-treatment ATTs vary between -3.5 and -3.7 percentage points. However, since all SMYP states are also Universal E-Verify states, and since the estimated effect for Universal E-Verify is similar in magnitude, it is unclear from this result alone whether SMYP or Universal E-Verify is the cause.

To ensure that my results for Universal E-Verify are not driven solely by the SMYP states, I drop SMYP states from my analysis and repeat my event study for Universal E-Verify. The results can be found in Appendix Table A10. The difference in the estimated post-ATTs and pre-ATTs declines, but the coefficient remains sizable at -2.1 to -2.4 percentage points for weighted estimations, and it is statistically significant at the 5-percent level in most of these specifications, even with the smaller sample of treated states. Universal E-Verify seems to have a noticeable impact that is robust to excluding SMYP states.

X. Robustness Checks: Migration, Legislatures, and SDID

Selective Migration

Selective migration poses a significant threat to identification in this framework. Ellis et al. (2014), Orrenius and Zavodny (2015), and Amuedo-Dorantes and Lozano (2019) all discuss how E-Verify mandates are associated with greater out-migration from treated to untreated states. The law in Arizona, for instance, doubled the out-migration rate among non-citizen Hispanics from about 1 to 2 percent in 2009, while the combined restrictive policies passed in Arizona between 2007 and 2010 prompted significant movement to nearby New Mexico and California (Ellis et al. 2014; Amuedo-Dorantes and Lozano 2019). While the migration effect of Universal E-Verify itself seems small in absolute terms, this and even greater out-migration caused by police-based policies in Universal E-Verify states can still cause bias in post-treatment effect estimations, not only through selective attrition out of the treated states, but also through contamination via spillovers to control states.

The direction of such bias depends on who moves in response to the law. If families facing worse labor market outcomes in treated states out-migrate when, absent migration, their children would have gone to college in the treated state, my results would be biased such that any negative effect would be too large in magnitude. On the other hand, if the children

of affected immigrants who leave were less likely to enroll in college absent treatment, then my results would be biased to be less negative. This latter possibility could be the case if low-income, low-skilled adults experience higher job turnover and are more often exposed to the E-Verify system. Nevertheless, Ellis et al. (2014) find evidence that the probability of moving in response to Arizona’s Universal E-Verify law was positively associated with college attainment. However, they also find that the probability of moving was noticeably smaller for couples with children. Since the educational effect of Universal E-Verify should be concentrated among children, this lower moving probability could mitigate the magnitude of any negative bias.

I evaluate whether selective migration poses a significant concern to my estimates. I do this in two ways. First, I re-run my primary specification, but I drop any border states from the control group, as well as any observations who moved between states in the past year. If selective out-migration creates spillovers in education to bordering states and significantly biases my estimates, this change should noticeably affect my results. Second, I re-run my primary event studies, except this time I drop the Universal E-Verify states from the sample. I instead assign treatment years to states that bordered the Universal E-Verify states. If the state in question borders more than one treated state, I define the treatment year as the first year any of their neighbors passed Universal E-Verify. For instance, California, Nevada, and New Mexico would be treated in 2008 since that is the year the law went into effect in Arizona. If immigrants with children who were more likely to go to college absent treatment selectively move to nearby states with less restrictive policies, one might expect the treatment effect in border states to be positive. If those who are less likely to go to college move, or if the disruption caused by moving leads to worse schooling outcomes, the effect could be negative.

The results of these two tests are found in Tables 6 and 7, along with the corresponding event study dynamics in Figure 6. I ultimately find no evidence that my results are biased to be more negative due to selective out-migration. As shown in Table 6, dropping border states from the analysis only makes my estimated effects larger in magnitude. The test on bordering states indicates that this increased magnitude may be due to negative spillovers: Table 7 reveals that there is a negative estimated impact on college attendance in bordering states of -2.5 to -3 percentage points. These combined results suggest that, if anything, selective out-migration biases my results to be less negative rather than more negative.

Controlling for State Legislatures

A separate potential concern is that my results are not picking up the effect of Universal E-Verify, but rather of the broader underlying political and social environment within treated states. If an anti-immigration environment is highly correlated with Universal E-Verify being passed, and if that environment would have driven undocumented immigrants out of college regardless of legislation, then the effect I estimate is not necessarily of the policy itself. I test this hypothesis by using the political party composition in each state legislature as a proxy for immigration attitudes. To create a variable for such a composition, I use data from the National Council of State Legislatures, along with historic data from Carl Klarner’s “State Partisan Balance Data, 1937 – 2011,” which have been combined for public use by Philip

Thomas.³⁷ From this data, I generate a variable indicating the proportion of Democrats within each state legislature by year (house and senate combined). This new control is not used in my primary results because it may be so highly correlated with policies that it may lead to over-controlling, even if the policies themselves have a true effect. On the other hand, the effect of any legislative makeup in a given state may be absorbed by state fixed effects if the political composition does not significantly shift over time, or by year fixed effects if the composition across states follows similar trends.

Appendix Table A11 reports the overall results while controlling for legislative composition. Re-running my primary specification while including this control hardly changes any of the results.

Synthetic Difference-in-Differences

One final concern is that not all untreated states in my various specifications are appropriate controls for Universal E-Verify states. This is especially true for places that pass any unanalyzed policies in earlier years that might affect undocumented education. For instance, while I use binary indicators to control for the passage of TUIT, OIL, and SMYP, I do not control for county-level policies or city-level policies. It is entirely feasible that some states are comprised of many localities with policies affecting undocumented education. Aside from policies, any other unaccounted-for factor correlated with college attendance may threaten the conditional parallel trends assumption. It is important to ensure that my results are robust to choosing appropriate control states that more closely match the pre-trends of treated states.

I implement a synthetic difference-in-differences design (Arkhangelsky et al. 2021) to alleviate this potential concern. While this approach does not solve the problem of shocks within control states in the post-treatment years, it does account for differences in pre-treatment years by creating a better-matched synthetic cohort of control states for each treated cohort. Unlike the standard synthetic control method, SDID also includes two-way fixed effects along with time weights to better match across pre-event periods. This means that SDID can estimate pre-treatment and post-treatment effects by event year, providing a comparable alternative to my primary ETWFE approach.

To use this method, all observations must be at the state-year level, so I cannot include individual characteristics as covariates. Instead, I keep the same state-level controls as in my event studies, but I also include the state-year averages of all individual-level covariates, weighting by sample weights times the random forest probability of being undocumented.³⁸ This procedure allows the SDID design to account for demographic changes in non-citizen Hispanics across states.

I briefly overview the SDID method developed by Arkhangelsky et al. (2021) and describe how this strategy estimates its average treatment effect on the treated (denoted $\hat{\alpha}_{sdid}$):

³⁷Data only goes up to 2021, so I add 2022 myself using data from the National Council of State Legislatures website.

³⁸The outcome of interest is now the average undocumented college attendance at the state level, calculated by averaging college attendance and weighting by the random forest undocumented probability.

$$\hat{\alpha}_{\text{sdid}} = \arg \min_{\alpha, \vartheta, \delta, \rho} \left\{ \sum_{s=1}^S \sum_{t=1}^T (\text{College}_{st}^{\text{res}} - \vartheta - \delta_s - \rho_t - \theta_{st}\alpha) \hat{\omega}_s^{\text{sdid}} \hat{\lambda}_t^{\text{sdid}} \right\}$$

Here, $\text{College}_{st}^{\text{res}}$ represents the residuals of a two-way-fixed effect regression of average college attendance on state-level covariates and state and year fixed effects (including the averaged versions of the individual-level covariates). Specifically, the twfe regression is run on not-yet-treated and untreated states. Fitted values are predicted for all treated and untreated states, and these are then subtracted from the actual values to yield $\text{College}_{st}^{\text{res}}$. ϑ , δ_s , and ρ_t represent a constant term, state fixed effects, and time fixed effects respectively. θ_{st} is a binary post-treatment indicator for state s . $\hat{\omega}_s^{\text{sdid}}$ and $\hat{\lambda}_t^{\text{sdid}}$ are state and time weights, as estimated by the algorithm described in Arkhangelsky et al (2021).³⁹ If there are no policy spillovers to control states and the parallel trends assumption holds conditional on the SDID weights, $\hat{\alpha}_{\text{sdid}}$ should provide an unbiased estimate for Universal E-Verify's effect on college attendance.

An advantage of SDID is that it can flexibly account for staggered adoption: to do so, the model is run separately for groups of states with different adoption years. SDID estimators for the average treatment effect are then calculated for each group. Next, these estimated treatment effects are combined using a weighted average based on the number of post-treatment state-years within each group. The overall SDID effect can also be disaggregated to event time effects, thus incorporating an event study design (Ciccia 2024).⁴⁰ To demonstrate this, suppose that each cohort g has event time t_g , and t can be any year from start year 1 to end year T ($t \in \{1 \dots t_g \dots T\}$). The effect for each relative event time τ in cohort g can be calculated from the following equation, where N_s^g is the number of treated states per cohort, and co is the set of never-treated states:

$$\begin{aligned} \hat{\alpha}_{g\tau}^{\text{sdid}} = & \frac{1}{N_s^g} \sum_{s \in g} \text{College}_{s,t_g-1+\tau}^{\text{res}} - \sum_{s \in co} \hat{\omega}_s^{\text{sdid}} \text{College}_{s,t_g-1+\tau}^{\text{res}} \\ & - \sum_{t=1}^{t_g-1} \left(\frac{1}{N_s^g} \sum_{s \in g} \hat{\lambda}_t^{\text{sdid}} \text{College}_{st}^{\text{res}} - \sum_{s \in co} \hat{\omega}_s^{\text{sdid}} \hat{\lambda}_t^{\text{sdid}} \text{College}_{st}^{\text{res}} \right) \end{aligned}$$

This equation simultaneously estimates the difference in outcomes between the treated and synthetic control groups for each event time while controlling for baseline differences. $\hat{\alpha}_{\tau}^{\text{sdid}}$ can then be estimated from the weighted average of all $\hat{\alpha}_{g\tau}^{\text{sdid}}$.

The downsides to this approach relative to ETWFE in this setting are threefold. First, restricting the sample to only state-year observations limits statistical power. Second, SDID needs a balanced set of states across all years, meaning that I cannot drop state-year cells

³⁹I also create block bootstrapped standard errors for the estimated effect. This involves re-sampling the states in the estimation 100 times with replacement. See Arkhangelsky et al. (2021) and Clarke et al. (2023) for more details on the weighting procedure, along with other possible methods for calculating standard errors when using SDID.

⁴⁰A special thanks to Diego Ciccia, Damian Clarke, Guido Imbens, and Daniel Paila  ir for creating the `sdid_event` package to easily implement this procedure.

based on sample size restrictions, leading to noisy estimates.⁴¹ I also cannot limit my event window to a certain number of periods, as doing so would also require dropping state-year cells outside the window. This means that any SDID result must include effects for every estimated event year, even if those event years are many years away from the policy implementation. Third, SDID requires many pre-treatment years to generate a useful synthetic control group. Since my first treatment year is 2008, only using 2005 and onwards from the ACS would fail to produce a precise synthetic control group for some of my treated states. This means that I must use the 2000-2004 pilot ACS years for SDID. Even with these disadvantages, such an approach is a worthwhile robustness check, as it ideally creates a more appropriate synthetic control for treated units while maintaining comparable two-way fixed effects.

Figure 7 shows the SDID results for Universal E-Verify. Since I must have a balanced panel of states, I cannot implement my usual procedure of dropping treated states outside the event window. Even if that were an option, I should include as many pre-event periods as possible to better match synthetic cohorts for each treatment group. I thus display all 13 pre-event periods (the last Universal E-Verify state implemented the law in 2013) and 15 post-event periods (the first laws were implemented in 2008) for the 2000-2022 sample.⁴²

The estimated effect on college attendance using SDID is -3.6 percentage points. The standard error is 2.7 percentage points, meaning that this result is not statistically significant at the 10 percent level. However, the estimated coefficient closely mirrors the ETWFE results, and there are larger negative and statistically significant negative effects for post-treatment years 4 through 8 – the exact same years with the largest effects in my ETWFE design. Even with the much larger standard errors, this result strongly supports my main findings.

XI. Mechanisms: Schooling, Labor Market Effects, Child Migrants

Pre-College Schooling

As discussed in my theoretical framework, Universal E-Verify can affect college attendance through various mechanisms. Its impact on labor market outcomes within undocumented households could not only make college itself less affordable, but also the educational resources needed to attain better academic performance prior to college. At the same time, the various exemptions based on firm size, along with possible lower enforcement in in-

⁴¹If I were to make the same sample size restriction, I would be forced to drop every state that had even one year below the 20-observation threshold due to the requirement for a balanced panel. Even reducing the cutoff to 10 observations would still mean that many states are dropped, as smaller states contain at least one year in which there are fewer than 10 observations. I therefore make no state-year sample size restriction for 2000 through 2022. The result is that any state with at least one observation per year over that period remains in the sample, which is the case for most states (although Alaska, Idaho, Maine, Montana, New Hampshire, North Dakota, South Dakota, Vermont, West Virginia, and Wyoming are dropped since they have at least one year with no observations). Fortunately, all treated states remain.

⁴²This means that the estimated effects displayed before pre-treatment period -8 and after post-treatment period 9 do not include every treatment group. The final two post-treatment effects, for instance, are estimated only for Arizona and Mississippi.

formal occupations, suggest that Universal E-Verify’s labor market impact could be more pronounced for undocumented immigrants seeking high-skilled jobs. This would in turn imply a lower return to having a college degree and lower college attendance as a result.

Both potential mechanisms imply a possible effect on secondary schooling. Such an effect has been indicated before: Amuedo-Dorantes and Lopez (2017) find that E-Verify policies increase the dropout probability for Hispanic children ages 14-17 with likely-undocumented parents. While they exploit more granular timing in CPS data than exists in the ACS, I can attempt to examine this question, as the ACS records whether children have been in school in the past three months.

It should be noted that examining secondary schooling outcomes with my ETWFE approach is somewhat problematic for several reasons. First, I cannot calculate an undocumented probability or use a logical imputation for non-citizen Hispanics under age 18 since predictors such as spousal citizenship are not relevant for that age group. Second, more than 90 percent of Hispanic non-citizen children in this range are in school, meaning the linear ETWFE model may not be appropriate given a binary outcome with an average value near 1.⁴³ Third, my current controls such as number of children, marital status, or state-level college demographics are inappropriate for this group. Fourth, the total sample size of children ages 14 to 17 is significantly smaller. Fifth, I cannot directly observe factors such as test scores or academic performance in my data, which may decline even if there is no effect on dropping out of school. Sixth, unaccounted-for policies in control states that are known to disrupt education may lead to a parallel trends assumption violation. While this last point is also a concern for college education, it is particularly pertinent for secondary schooling: Amuedo-Dorantes and Lopez (2017) find that county-level police-based immigration enforcement policies like 287(G) produce an even larger effect on secondary schooling outcomes than E-Verify laws (an 18 percent increase in dropout probability for Hispanic children ages 14-17 with likely undocumented parents, vs a 9 percent increase for E-Verify laws).

I nevertheless apply my primary strategy for all non-citizen Hispanics ages 14-17. I further restrict the sample to those who are living with at least one logically-imputed, non-citizen parent so that I can determine and control for average parental college education. I also include controls for sex, race, age, squared age, years since migration, years since migration squared, my binary policy indicators, and the same state-level covariates as before, including GDP per capita and the lagged unemployment rate. My outcome variable is a binary indicator of whether the child has attended school in the past three months.

The results for the ETWFE estimation can be found in Table 8, with a corresponding event study graph in Figure 8. Unfortunately, Figure 8 reveals that pre-trends are far from stable. Also, there is, if anything, a positive effect of 1.5 to 1.9 percentage points on attendance. Recall that my theoretical model indicates that a positive effect on pre-college schooling is feasible through a substitution effect away from child labor. However, due to the lack of any parallel trends with this approach, it is impossible to draw any definitive conclusions using my ETWFE strategy. I therefore return to my SDID approach to analyze

⁴³Descriptive statistics on non-citizen Hispanics ages 14-17 are available upon request.

attendance below the college level, using the same procedure as before but with the new covariates for this sample. This approach is not necessarily advantageous either, both for reasons previously discussed and because the smaller sample size in this case will lead to more states being dropped to ensure a balanced panel. Still, by providing a balanced panel, this method may prove more interpretable.⁴⁴

I present the results for secondary schooling attendance using SDID in Figure 9. The set of states with at least one observation across all years that can be kept is much smaller in this specification, consisting of only 29 states in total. Of the Universal E-Verify states, only Arizona, Georgia, and North Carolina are included, as the other states had at least one year with no non-citizen Hispanics ages 14 to 17. The results, however, are much more stable with this approach, and a negative estimated effect emerges. This estimated treatment effect is a 4.5 percentage point decline in high school attendance. With a standard error of 2.5 percentage points, the effect is statistically significant at the 10 percent level.

This evidence for an effect on schooling prior to college is only suggestive given that it is not robust to my primary method, but it would be in line with the conclusions of Amuedo-Dorantes and Lopez (2017).

Labor Market Effects

Regardless of whether there is an effect on secondary schooling attendance itself, the crucial underlying mechanism in my model is the effect on adult labor market outcomes. I examine these paths.

I follow previous research by demonstrating that Universal E-Verify lowers employment among undocumented Hispanics. In particular, I focus on those ages 25 to 54 since they are outside of my college-age sample. I use the same ETWFE and SDID approaches as before.⁴⁵ Since I now focus on outcomes for those past college age, I also include a binary variable for if an observation had at least some college education. Also, to test whether Universal E-Verify disproportionately impacts low-skilled or high-skilled jobs, I present separate results for (1) the full sample, (2) observations with no post-secondary education, and (3) observations with at least some post-secondary education. If a disproportionate negative effect exists for the higher educated undocumented Hispanics, that would be evidence that Universal E-Verify lowers the marginal return to education. On the other hand, if the estimated effect on higher educated undocumented Hispanics is the same or less pronounced than the effect for those with low education, Universal E-Verify’s impact on college attendance must not operate through future returns to schooling.

The results for these estimations are in Table 9 and Figure 10 for ETWFE and in Appendix Figure A8 for SDID. The ETWFE estimates indicate that Universal E-Verify decreases the employment of undocumented Hispanics by 3.2 percentage points. Figure 10 also reveals that this effect is persistent across time and even grows to four percentage points

⁴⁴It should also be noted that the dearth of individual-level data led to more than half of all state-years being omitted when using ETWFE for the 14-17 age group according to my 20-observation limit – far more than in other estimations conducted in this paper.

⁴⁵For these specifications, I always exclude the state-level controls for GDP per capita and the lagged unemployment rate, although including them does not significantly alter the results.

in later years as more businesses begin to comply with the law. The overall effect is similar using SDID, which finds a -2.7-percentage point effect ($SE = 1.6$ percentage points).

The effect is primarily driven by those with lower education, as the estimated post-treatment effect for college-educated Hispanics is positive, although inspection of Figure 10 reveals that there are not stable pre-trends for this limited sample of the undocumented population. In either case, it does not appear that Universal E-Verify lowers the marginal return to education (if anything, it raises it by disproportionately affecting lower-educated immigrants). It is therefore likely that Universal E-Verify lowers college education primarily through a negative shock to the labor market outcomes of parents, as indicated by the negative overall employment effect. In the Appendix, I briefly examine the effect on annual wage and salary earnings, and the results seem to corroborate this shock.⁴⁶

Overall, analysis of potential mechanisms provides suggestive evidence that Universal E-Verify lowers undocumented attendance by negatively impacting undocumented teenagers while they are still in high school through labor market shocks to parents. By contrast, it does not appear that the policy negatively affects high-educated undocumented workers relative to low-educated workers, so the effect of Universal E-Verify does not operate through the return to education.

Child Migrants

If Universal E-Verify primarily affects college attendance through a shock on parents rather than on marginal returns to college, it should be the case that those who immigrated before age 18 are primarily affected since they are more likely to live with or be connected to parents in the United States. To verify that my results are robust to this smaller group, I split my 18-24 sample to those who immigrated into the country before and after age 18 and re-run my primary specifications for ETWFE and SDID.

Appendix Figure A11 displays the ETWFE and SDID approaches. The results confirm the underlying intuition that those under age 18 are affected: for ETWFE, there is a pre-trend with a pre-event ATT of -2.0 percentage points, but it is slightly upward sloping, and the post-event ATT is -5.2 percentage points. The difference between these values of -3.2 percentage points closely mirrors my primary result of -3.7 percentage points. The SDID specification estimates an even larger effect of -5.8 percentage points ($SE = 2.2$ percentage points). These results, especially the much larger and highly statistically significant SDID effect, provide further evidence that the underlying mechanism is an effect on parental outcomes while a young immigrant lives in their parents' household.

⁴⁶I briefly explore this shock further by running the same set of estimations on (1) annual wage/salary earnings in dollars, and (2) log hourly wages conditional on employment. While there is no statistically significant internal margin effect for wages (see Appendix Table A13 and Appendix Figure A7 for ETWFE and Appendix Figure A10 for SDID) there is for earnings, with a post-treatment effect of about -300 dollars. There is also to be a strong downward-sloping pre-trend up to a couple years before treatment, so this result is not definitive. Regardless, it is natural to assume that the loss in employment implies a loss in earnings. This assumption is somewhat corroborated by an apparent negative effect shown for SDID in Appendix Figure A10.

XII. Conclusion

I have documented a negative effect of Universal E-Verify on the college attendance of undocumented Hispanics. These findings are robust to analyzing potentially confounding restrictive policies; using various weighting schemes, sample time frames, and undocumented status prediction strategies; testing for potential bias from selective out-migration; and implementing synthetic difference-in-differences. I provide evidence that the key underlying mechanism is a negative labor market shock on undocumented parents, which can lead to worse educational outcomes in high school and greater difficulty in affording college. It also appears that while machine learning algorithms like random forest can be used to predict undocumented status, they do not result in significantly different results from using logit or logical imputation without a richer set of predictors – predictors that unfortunately may not be appropriate for many economic studies. The results of this paper imply that more stringent employment restrictions on undocumented immigrants lead to poorer higher education outcomes for their children. This novel finding carries important implications for the long-run human capital accumulation and labor market outlook of young undocumented immigrants.

Tables and Figures

TABLE 1
RESTRICTIVE LEGISLATION BY STATE

UNIVERSAL E-VERIFY	
State	Year Enforced
Arizona	2008
Mississippi	2008
Utah	2010
Alabama	2012
Georgia	2012
Louisiana*	2012
South Carolina	2012
Tennessee	2012
North Carolina**	2013
Florida***	2023
SHOW-ME-YOUR-PAPERS	
Arizona	2010
Alabama	2011
Georgia	2011
South Carolina	2011
EARLIEST OMNIBUS IMMIGRATION LEGISLATION ENFORCEMENT	
Colorado	2006
Georgia	2006
Oklahoma	2007
Arizona	2008
Missouri	2008
South Carolina	2008
Utah	2008
Nebraska	2009
Alabama	2011
Indiana	2011
North Carolina	2015
Texas	2017

Notes: Event year is generally considered the first year of enforcement.

*Businesses can retain identifying employee records in lieu of E-Verify.

**Enforcement began in October 2012, but only for firms with over 500 employees. Most firms were not bound until 2013.

***Passed outside of the final sample year.

TABLE 2
EDUCATION LEGISLATION BY STATE, EVENT YEAR

IN-STATE TUITION	
California, Texas	2002
Utah, New York	2003
Illinois, Oklahoma, Washington	2004
Kansas	2005
New Mexico	2006
Nebraska	2007
Wisconsin	2010
Connecticut	2012
Rhode Island, Maryland, Hawaii	2013
Colorado, Oregon, Minnesota, New Jersey	2014
Florida	2015
Kentucky	2016
District of Columbia	2017
Virginia	2021
Nevada	2022
Massachusetts*	2024
FINANCIAL AID	
Texas	2002
Washington	2004
New Mexico	2006
Illinois	2010
California	2012
Hawaii, Rhode Island	2013
Oregon	2014
District of Columbia	2017
Connecticut, Maryland, New Jersey	2019
Colorado, New York	2020
Virginia	2021
Nevada	2022
Arizona*	2023
Massachusetts, * Minnesota*	2024

*Passed outside max sample year.

TABLE 3
DESCRIPTIVE STATISTICS: UNIVERSAL E-VERIFY

	Pre-Control, 2000-2007			Pre-Treated, 2000-2007		
	Mean	SD	N	Mean	SD	N
College Attendance	0.095	0.294	40,946	0.049	0.215	7,662
English Fluency	0.208	0.406	40,946	0.209	0.407	7,662
Female	0.363	0.481	40,946	0.323	0.468	7,662
Age	21.379	1.861	40,946	21.360	1.884	7,662
Employed	0.682	0.466	40,946	0.719	0.450	7,662
YSM	5.586	4.808	40,946	4.443	4.020	7,662
Married	0.234	0.424	40,946	0.249	0.433	7,662
# Children	0.353	0.734	40,946	0.367	0.751	7,662
	Post-Control, 2013-2022			Post-Treated, 2013-2022		
	Mean	SD	N	Mean	SD	N
College Attendance	0.228	0.420	54,737	0.157	0.364	8,722
English Fluency	0.408	0.491	54,737	0.432	0.495	8,722
Female	0.402	0.490	54,737	0.391	0.488	8,722
Age	21.336	1.855	54,737	21.345	1.854	8,722
Employed	0.660	0.474	54,737	0.672	0.469	8,722
YSM	8.130	6.411	54,737	8.192	6.572	8,722
Married	0.127	0.333	54,737	0.147	0.354	8,722
# Children	0.261	0.662	54,737	0.316	0.722	8,722

Notes: All observations are ages 18-24 with less than a bachelor's degree. College attendance is binary and equal to 1 if the person is currently attending college. English Fluency, Female, Employed, and Married are also binary indicators. YSM is years since migration. All statistics apart from the number of observations are weighted by the random forest probability of being undocumented multiplied by sample weights.

TABLE 4
EFFECT OF UNIVERSAL E-VERIFY (RFORST UNDOCUMENTED)

Post-ATT	-0.042*** (0.010)	-0.041*** (0.008)	-0.041*** (0.011)	-0.040*** (0.008)	-0.067*** (0.011)	-0.065*** (0.013)
Pre-ATT	-0.005 (0.005)	-0.005 (0.005)	-0.008 (0.006)	-0.009 (0.006)	-0.021** (0.008)	-0.024** (0.009)
Post - Pre	-0.037*** (0.011)	-0.035*** (0.009)	-0.032** (0.012)	-0.031*** (0.010)	-0.046*** (0.014)	-0.041** (0.016)
2000–2022	Yes	Yes	No	No	Yes	No
State Econ. Controls	Yes	No	Yes	No	Yes	No
Undoc Weights	Yes	Yes	Yes	Yes	No	No
N	158,097	158,097	145,047	145,047	62,288	56,221

Notes: All above results use binary variables indicating if a state has passed TUIT, SMYP, or OIL. All specifications also control for the average state-year college education of all natives and Black natives ages 18-24, age, squared age, years since migration, years since migration squared, marital status, number of children, sex, race, average private and in-state public tuition prices for each year, and state and year FEs. No state-year cell with fewer than 20 observations is included. All observations are non-citizen Hispanics ages 18 to 24 and with less than a bachelor's degree. All specifications with undoc weights are weighted by the random forest probability of being undocumented. Specifications with no weighting only include observations with a random forest score ≥ 0.5 . "2000-2022" indicates whether the entire 2000-2022 sample or the 2005-2022 sample is used. "State Econ. Controls" indicates whether the one-year lagged unemployment rate and state-level real GDP per capita were included. Standard errors are clustered at the state level, and significance levels are denoted as ($P < 0.01^{***}$, 0.05^{**} , 0.10^{*}).

TABLE 5
EFFECT OF UNIVERSAL E-VERIFY (LOGICAL IMPUTATION)

Post-ATT	-0.041*** (0.009)	-0.042*** (0.007)	-0.040*** (0.009)	-0.041*** (0.007)
Pre-ATT	-0.008* (0.005)	-0.008* (0.005)	-0.011** (0.005)	-0.010** (0.005)
Post - Pre	-0.033*** (0.010)	-0.034*** (0.008)	-0.029*** (0.010)	-0.031*** (0.008)
2000–2022	Yes	Yes	No	No
State Econ. Controls	Yes	No	Yes	No
N	158,097	158,097	145,047	145,047

Notes: All above results use binary variables indicating if a state has passed TUIT, SMYP, or OIL. All specifications also control for the average state-year college education of all natives and Black natives ages 18-24, age, squared age, years since migration, years since migration squared, marital status, number of children, sex, race, average private and in-state public tuition prices for each year, and state and year FEs. No state-year cell with fewer than 20 observations is included. All observations are non-citizen Hispanics ages 18 to 24 and with less than a bachelor's degree. "2000-2022" indicates whether the entire 2000-2022 sample or the 2005-2022 sample is used. "State Econ. Controls" indicates whether the one-year lagged unemployment rate and state-level real GDP per capita were included. Standard errors are clustered at the state level, and significance levels are denoted as (P < 0.01***, 0.05**, 0.10*).

TABLE 6
EFFECT OF UNIVERSAL E-VERIFY – NO BORDER STATES

Post-ATT	-0.060*** (0.008)	-0.057*** (0.008)	-0.057*** (0.008)	-0.053*** (0.008)	-0.080*** (0.011)	-0.074*** (0.014)
Pre-ATT	0.008 (0.009)	0.007 (0.009)	0.006 (0.010)	0.004 (0.010)	-0.007 (0.014)	-0.010 (0.016)
Post – Pre	-0.068*** (0.012)	-0.064*** (0.012)	-0.063*** (0.013)	-0.057*** (0.013)	-0.073*** (0.018)	-0.064*** (0.021)
2000–2022	Yes	Yes	No	No	Yes	No
State Econ. Controls	Yes	No	Yes	No	Yes	No
Undoc Weights	Yes	Yes	Yes	Yes	No	No
N	61,817	61,817	57,047	57,047	26,810	24,358

Notes: All above results use binary variables indicating if a state has passed TUIT, SMYP, or OIL. All specifications also control for the average state-year college education of all natives and Black natives ages 18-24, age, squared age, years since migration, years since migration squared, marital status, number of children, sex, race, average private and in-state public tuition prices for each year, and state and year FEs. No state-year cell with fewer than 20 observations is included. All observations are non-citizen Hispanics ages 18 to 24 and with less than a bachelor's degree who don't live in a state bordering a Universal E-Verify state and didn't migrate from a different state in the past year. All specifications with undoc weights are weighted by the random forest probability of being undocumented. Specifications with no weighting only include observations with a random forest score ≥ 0.5 . "2000-2022" indicates whether the entire 2000-2022 sample or the 2005-2022 sample is used. "State Econ. Controls" indicates whether the one-year lagged unemployment rate and state-level real GDP per capita were included. Standard errors are clustered at the state level, and significance levels are denoted as ($P < 0.01^{***}$, 0.05^{**} , 0.10^{*}).

TABLE 7
EFFECT OF UNIVERSAL E-VERIFY ON BORDER STATES

Post-ATT	-0.028*** (0.009)	-0.030*** (0.008)	-0.026*** (0.009)	-0.027*** (0.008)	-0.028** (0.013)	-0.025* (0.014)
Pre-ATT	-0.004 (0.010)	-0.004 (0.010)	-0.002 (0.012)	-0.003 (0.012)	-0.003 (0.015)	-0.002 (0.017)
Post - Pre	-0.024* (0.014)	-0.026* (0.013)	-0.024 (0.015)	-0.024* (0.014)	-0.025 (0.020)	-0.023 (0.022)
2000–2022	Yes	Yes	No	No	Yes	No
State Econ. Controls	Yes	No	Yes	No	Yes	No
Undoc Weights	Yes	Yes	Yes	Yes	No	No
N	125,302	125,302	115,857	115,857	49,214	44,736

Notes: All above results use binary variables indicating if a state has passed TUIT, SMYP, or OIL. All specifications also control for the average state-year college education of all natives and Black natives ages 18-24, age, squared age, years since migration, years since migration squared, marital status, number of children, sex, race, average private and in-state public tuition prices for each year, and state and year FEs. No state-year cell with fewer than 20 observations is included. All observations are non-citizen Hispanics ages 18 to 24 and with less than a bachelor's degree who don't live in a Universal E-Verify state and didn't migrate from a different state in the past year. All specifications with undoc weights are weighted by the random forest probability of being undocumented. Specifications with no weighting only include observations with a random forest score ≥ 0.5 . "2000-2022" indicates whether the entire 2000-2022 sample or the 2005-2022 sample is used. "State Econ. Controls" indicates whether the one-year lagged unemployment rate and state-level real GDP per capita were included. Standard errors are clustered at the state level, and significance levels are denoted as ($P < 0.01^{***}$, 0.05^{**} , 0.10^{*}).

TABLE 8
EFFECT OF UNIVERAL E-VERIFY ON SECONDARY ATTENDANCE

Post-ATT	0.027*** (0.005)	0.026*** (0.004)	0.027*** (0.004)	0.026*** (0.004)
Pre-ATT	0.012*** (0.003)	0.012*** (0.003)	0.007** (0.003)	0.007** (0.003)
Post - Pre	0.015*** (0.005)	0.014*** (0.005)	0.019*** (0.005)	0.019*** (0.005)
2000–2022	Yes	Yes	No	No
State Econ. Controls	Yes	No	Yes	No
N	42,505	42,505	39,374	39,374

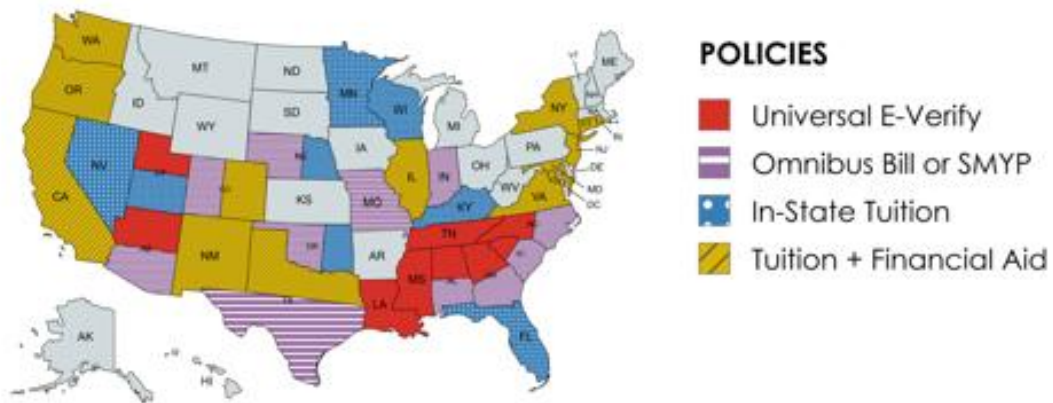
Notes: All above results use binary variables indicating if a state has passed TUIT, SMYP, or OIL. All specifications also control for the average state-year college education of all natives and Black natives ages 18-24, age, squared age, years since migration, years since migration squared, sex, race, average private and in-state public tuition prices for each year, and state and year FEs. No state-year cell with fewer than 20 observations is included. All observations are non-citizen Hispanics ages 14 to 17. "2000-2022" indicates whether the entire 2000-2022 sample or the 2005-2022 sample is used. "State Econ. Controls" indicates whether the one-year lagged unemployment rate and state-level real GDP per capita were included. Standard errors are clustered at the state level, and significance levels are denoted as ($P < 0.01^{***}$, 0.05^{**} , 0.10^{*}).

TABLE 9
EFFECT OF UNIVERSAL E-VERIFY ON EMPLOYMENT – BY EDUCATION GROUP

	All		No Post-Secondary		Post-Secondary	
Post-ATT	-0.031*** (0.001)	-0.032*** (0.001)	-0.038*** (0.002)	-0.038*** (0.002)	0.012*** (0.004)	0.011** (0.004)
Pre-ATT	0.001 (0.002)	0.001 (0.002)	-0.006*** (0.002)	-0.007*** (0.002)	0.029*** (0.003)	0.031*** (0.003)
Post - Pre	-0.032*** (0.002)	-0.032*** (0.002)	-0.032*** (0.003)	-0.032*** (0.003)	-0.017*** (0.005)	-0.020*** (0.005)
2000–2022	Yes	No	Yes	No	Yes	No
State Econ. Controls	No	No	No	No	No	No
Undoc Weights	Yes	Yes	Yes	Yes	Yes	Yes
N	1,013,555	958,887	820,170	776,297	193,385	182,590

Notes: All above results use binary variables indicating if a state has passed TUIT, SMYP, or OIL. All specifications also control for the average state-year college education of all natives and Black natives ages 18-24, age, squared age, years since migration, years since migration squared, marital status, number of children, sex, race, average private and in-state public tuition prices for each year, and state and year FEs. In addition, the specifications for "All" include a binary variable indicating whether an observation has at least some college education. No state-year cell with fewer than 20 observations is included. All observations are non-citizen Hispanics ages 25 to 54. All specifications with undoc weights are weighted by the random forest probability of being undocumented. "2000-2022" indicates whether the entire 2000-2022 sample or the 2005-2022 sample is used. "State Econ. Controls" indicates whether the one-year lagged unemployment rate and state-level real GDP per capita were included. Standard errors are clustered at the state level, and significance levels are denoted as ($P < 0.01^{***}$, 0.05^{**} , 0.10^{*}).

FIGURE 1
POLICIES BY STATE AS OF 2022



Notes: Utah also passed an omnibus bill, but I elect to display a maximum of two colors per state so that the picture is easier to visualize. I also include SMYP years as part of omnibus bills in this figure, as all SMYP laws were passed as part of a package. The SMYP states are Alabama, Arizona, Georgia, and South Carolina. Created with mapchart.net.

FIGURE 2
HISTOGRAMS OF UNDOCUMENTED WEIGHTS

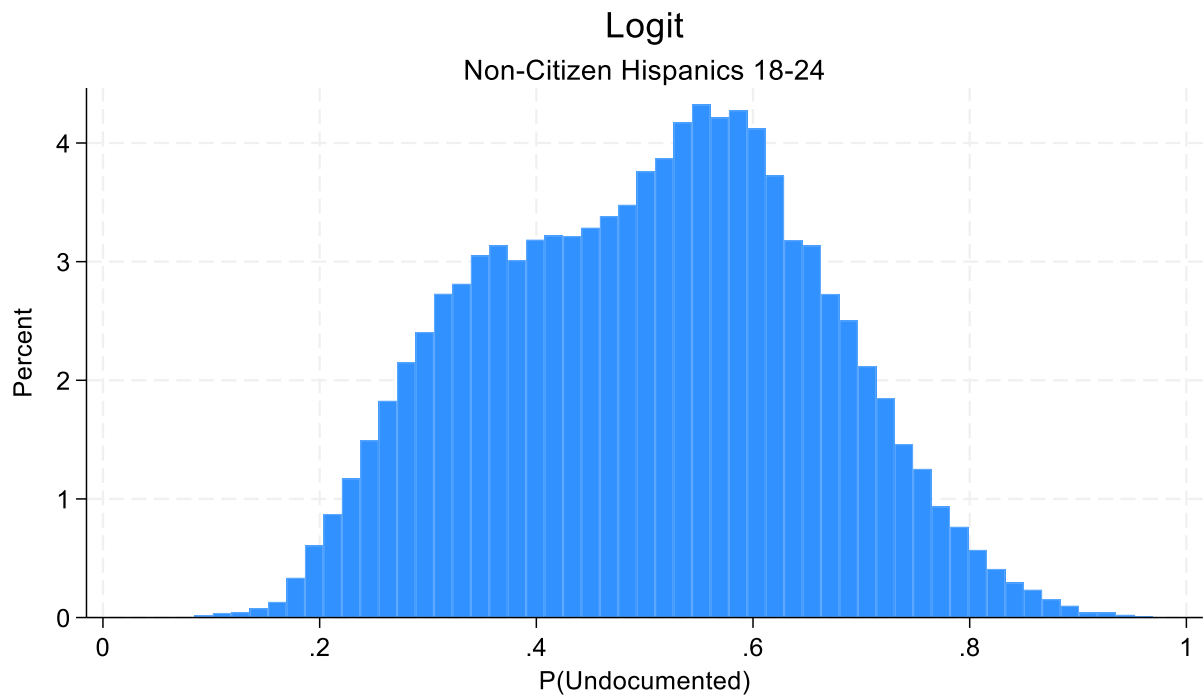
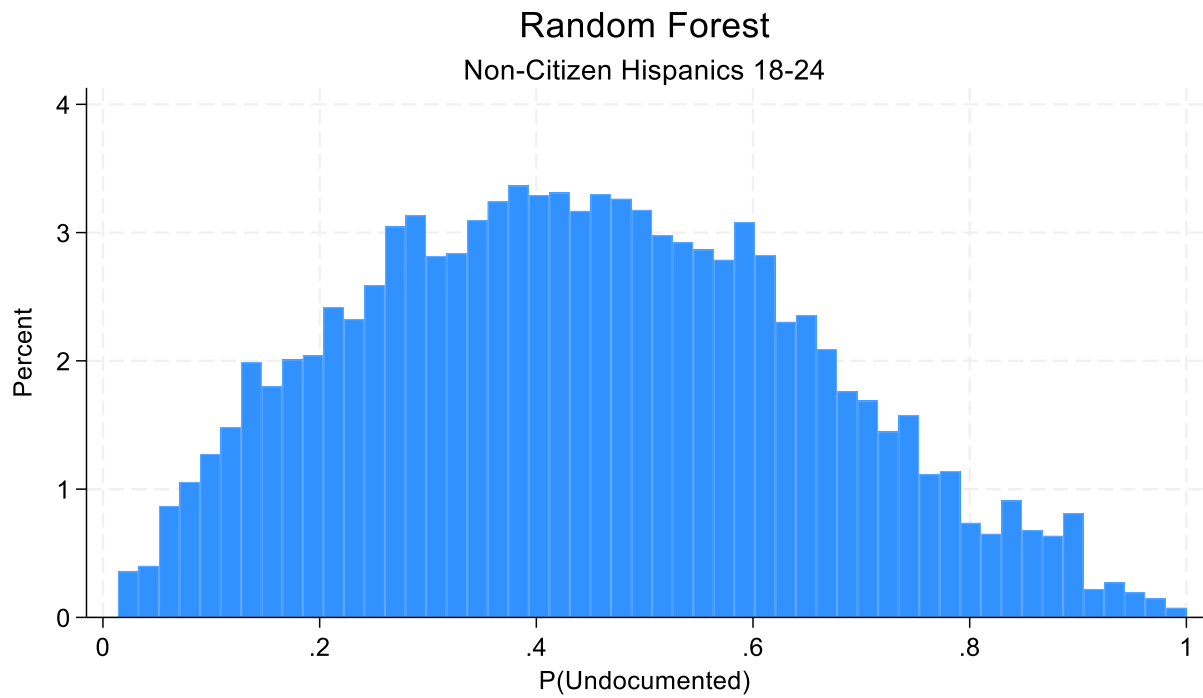
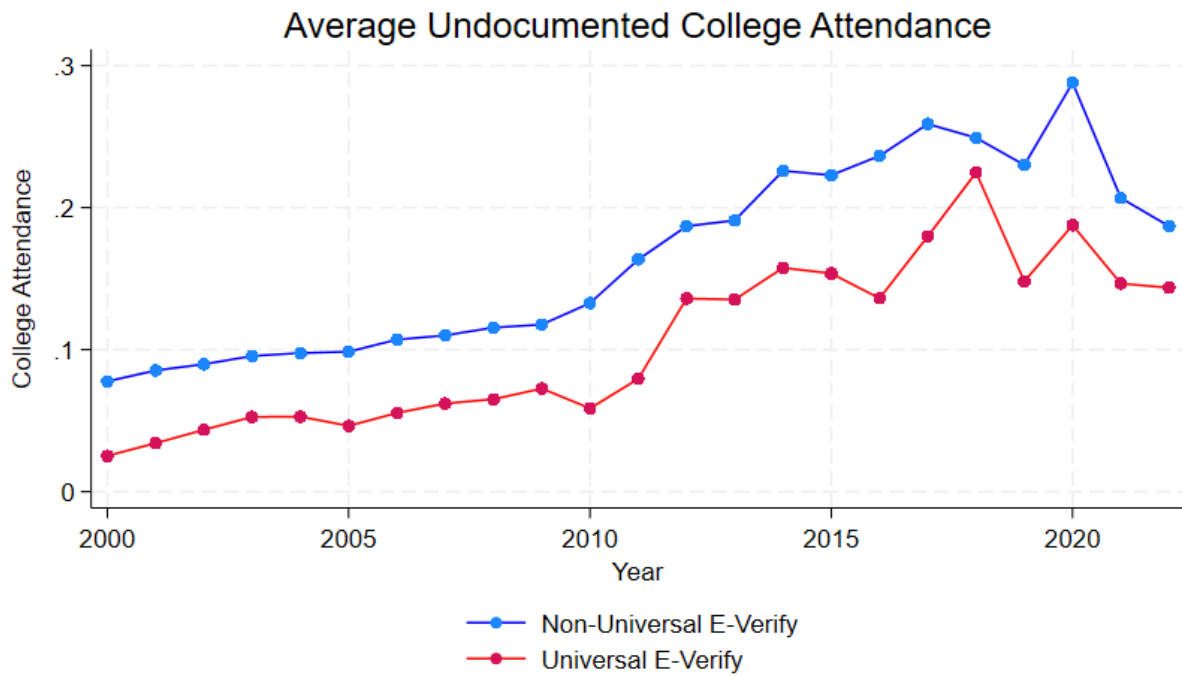
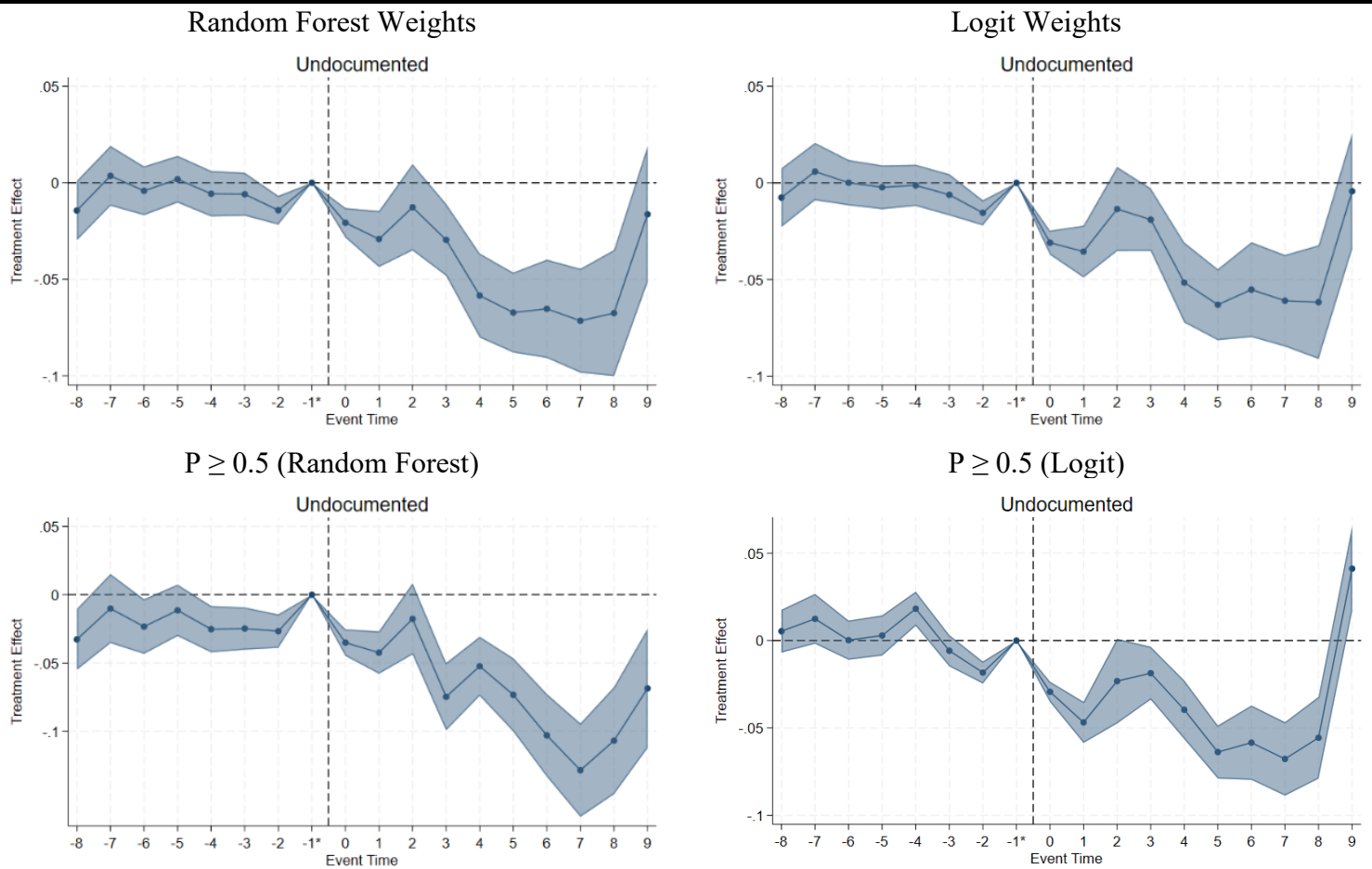


FIGURE 3
COLLEGE ATTENDANCE BY TREATMENT AND CONTROL STATES



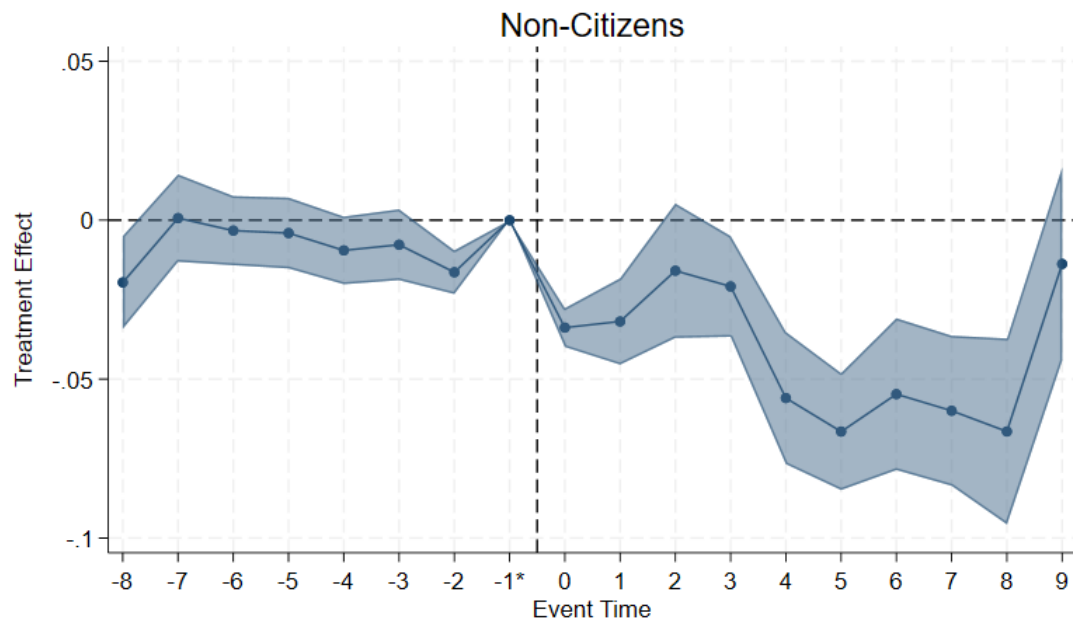
Notes: Values are plotted using sample weights times the random forest undocumented probability.

FIGURE 4
UNIVERSAL E-VERIFY EVENT STUDIES



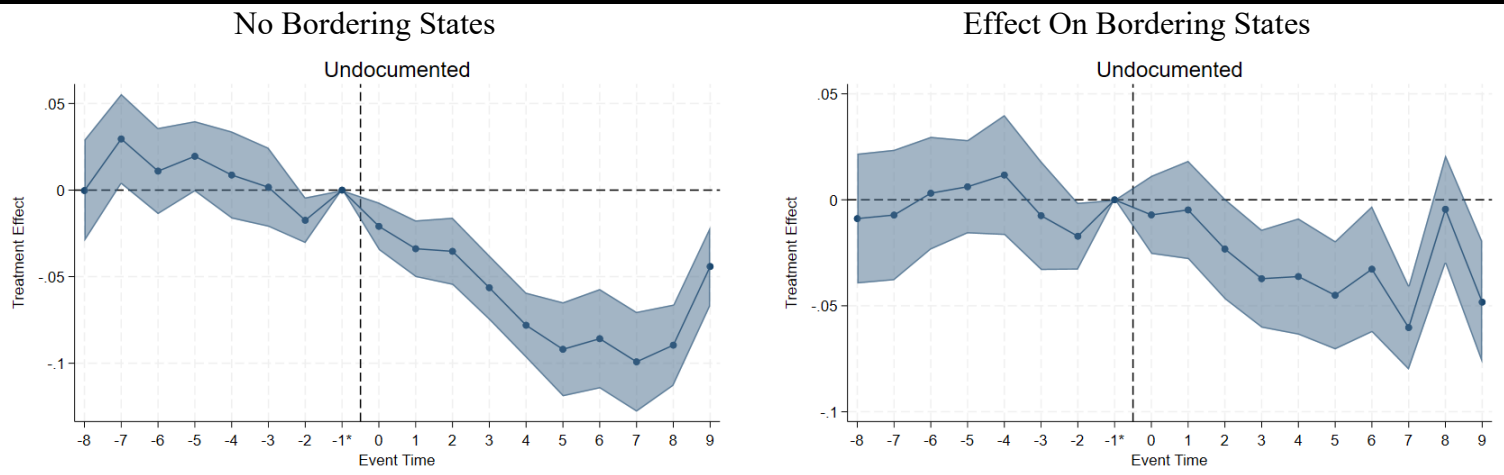
Notes: All figures depicted include 2000-2022, as well as controls for GDP per capita and the lagged unemployment rate.

FIGURE 5
EFFECT OF UNIVERSAL E-VERIFY (NON-CITIZENS – LOGICAL IMPUTATION)



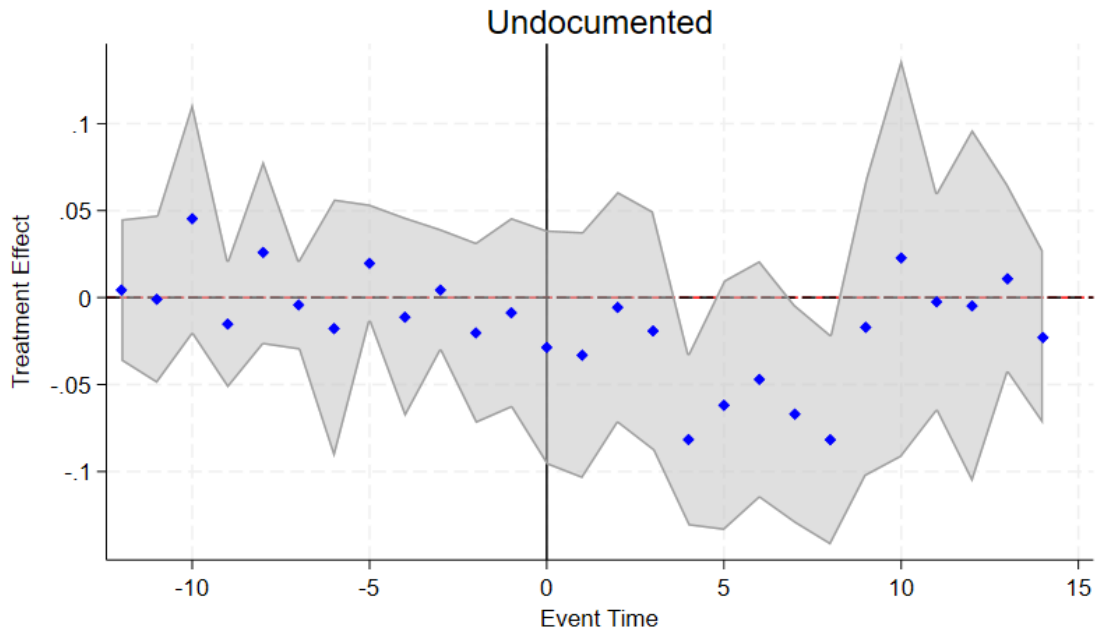
Notes: Includes 2000-2022, as well as controls for GDP per capita and the lagged unemployment rate.

FIGURE 6
UNIVERSAL E-VERIFY AND BORDERING STATES



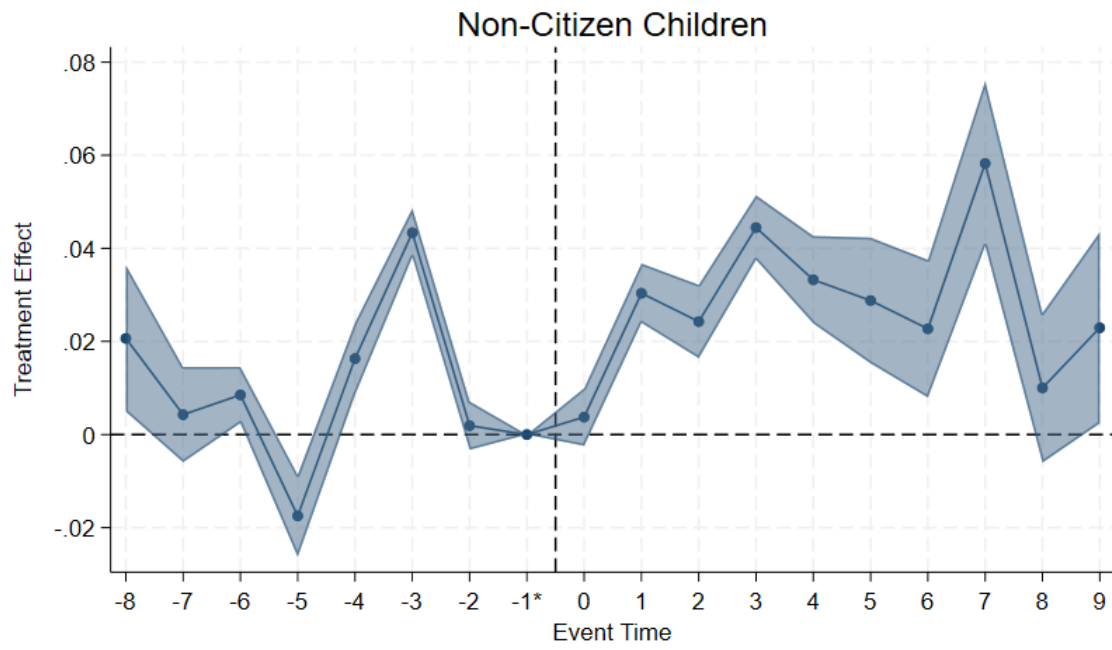
Notes: The figure on the left corresponds to the first column of Table 6. The figure on the right corresponds to the first column of Table 7.

FIGURE 7
SDID RESULTS FOR UNIVERSAL E-VERIFY ON COLLEGE EDUCATION



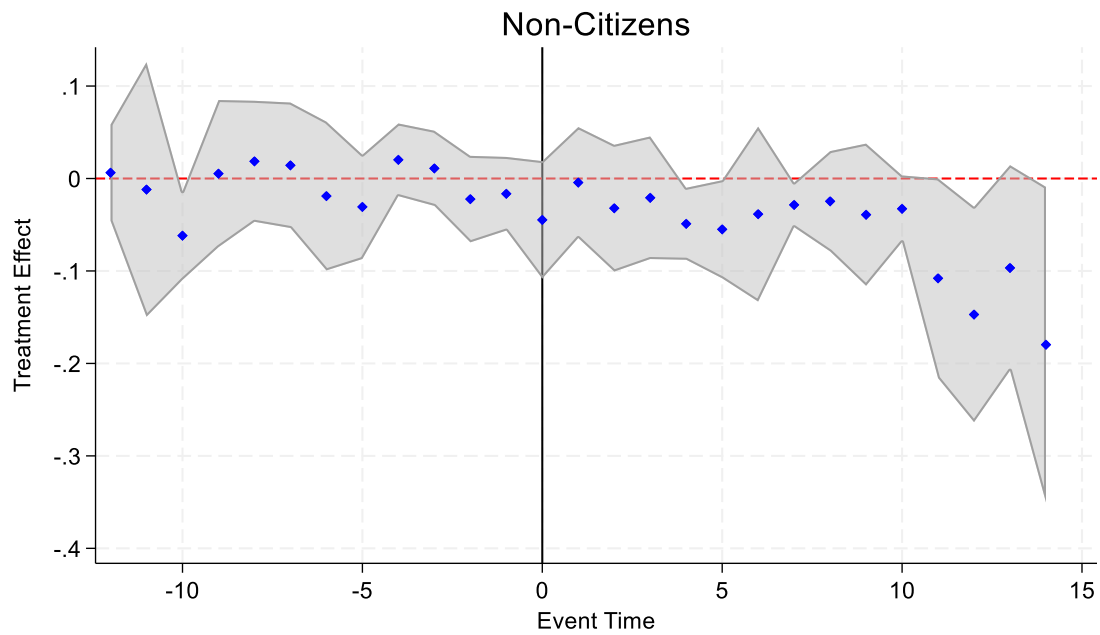
Notes: SDID results for the impact of Universal E-Verify on the college attendance of non-citizen Hispanics ages 18 to 24 with less than a bachelor's degree. Controls used include state-year cell averages of age, squared age, years since migration, years since migration squared, a marital status binary indicator, number of children, sex, and binary race indicators. These averages are all weighted by the random forest probability of being undocumented multiplied by the sample weights. Also included are the same state-year-level covariates as in the ETWFE specifications, including GDP per capita and the lagged unemployment rate.

FIGURE 8
EFFECT OF UNIVERAL E-VERIFY ON SECONDARY ATTENDANCE



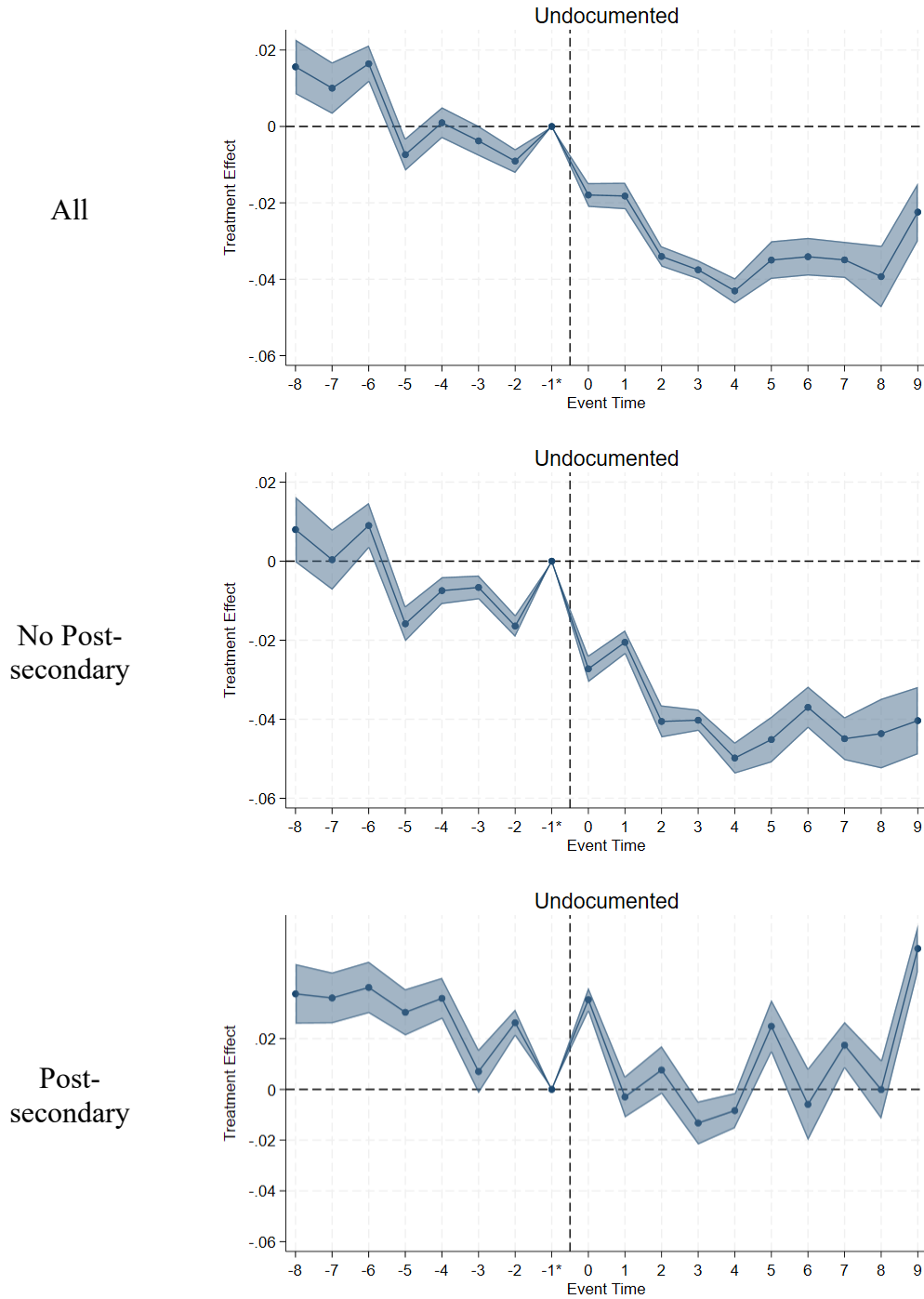
Notes: The above figure corresponds to the first column in Table 8.

FIGURE 9
SDID RESULTS FOR UNIVERSAL E-VERIFY ON SECONDARY ATTENDANCE



Notes: SDID results for the impact of Universal E-Verify on the secondary school attendance of non-citizen Hispanic children ages 14 to 17. Controls used include state-year cell averages of age, squared age, years since migration, years since migration squared, sex, and binary race indicators. These averages are all weighted by sample weights. Also included are the same state-year-level covariates as in the ETWFE specifications, including GDP per capita and the lagged unemployment rate.

FIGURE 10
EFFECT OF UNIVERSAL E-VERIFY ON EMPLOYMENT



Notes: Each figure corresponds to the matching employment results in Table 9 for the years 2000-2022 using random forest weights.

Appendix

width=!,height=!,pages=50-73, pagecommand=

References

- Allen, Chenoa D., and Clea A. McNeely.** “Do Restrictive Omnibus Immigration Laws Reduce Enrollment in Public Health Insurance by Latino Citizen Children? A Comparative Interrupted Time Series Study.” *Social Science & Medicine* 191 (October 2017): 19–29. <https://doi.org/10.1016/j.socscimed.2017.08.039>.
- Amuedo-Dorantes, Catalina, and Cynthia Bansak.** “The Labor Market Impact of Mandated Employment Verification Systems.” *American Economic Review* 102, no. 3 (May 1, 2012): 543–48. <https://doi.org/10.1257/aer.102.3.543>.
- Amuedo-Dorantes, Catalina, and Cynthia Bansak.** “Employment Verification Mandates and the Labor Market Outcomes of Likely Unauthorized and Native Workers.” *Contemporary Economic Policy* 32, no. 3 (July 2014): 671–80. <https://doi.org/10.1111/coep.12043>.
- Amuedo-Dorantes, Catalina, and Mary J. Lopez.** “The Hidden Educational Costs of Intensified Immigration Enforcement.” *Southern Economic Journal* 84, no. 1 (July 2017): 120–54. <https://doi.org/10.1002/soej.12207>.
- Amuedo-Dorantes, Catalina, and Fernando A. Lozano.** “Interstate Mobility Patterns of Likely Unauthorized Immigrants: Evidence from Arizona.” *Journal of Economics, Race, and Policy* 2, no. 1–2 (June 2019): 109–20. <https://doi.org/10.1007/s41996-018-0023-7>.
- Amuedo-Dorantes, Catalina, and Chad Sparber.** “In-State Tuition for Undocumented Immigrants and Its Impact on College Enrollment, Tuition Costs, Student Financial Aid, and Indebtedness.” *Regional Science and Urban Economics* 49 (November 2014): 11–24. <https://doi.org/10.1016/j.regsciurbeco.2014.07.010>.
- Arkhangelsky, Dmitry, Susan Athey, David A. Hirshberg, Guido W. Imbens, and Stefan Wager.** “Synthetic Difference-in-Differences.” *American Economic Review* 111, no. 12 (2021): 4088–4118. <https://doi.org/10.1257/aer.20190159>.
- Averett, Susan L., Cynthia Bansak, Grace Condon, and Eva Dziadula.** “The Gendered Impact of In-State Tuition Policies on Undocumented Immigrants’ College Enrollment, Graduation, and Employment.” *IZA Discussion Paper* 16698, IZA Institute of Labor Economics, 2023.
- Averett, Susan, Cynthia Bansak, Grace Condon, and Eva Dziadula.** “The Gendered Impact of In-State Tuition Policies on Undocumented Immigrants’ College Enrollment, Graduation, and Employment.” *Journal on Migration and Human Security* 13, no. 1 (2025): 64–86. <https://doi.org/10.1177/23315024241287505>.

Basu, Kaushik. “Child Labor: Cause, Consequence, and Cure, with Remarks on International Labor Standards.” *Journal of Economic Literature* 37, no. 3 (1999): 1083–1119.

Bohn, Sarah, Magnus Lofstrom, and Steven Raphael. “Do E-Verify Mandates Improve Labor Market Outcomes of Low-Skilled Native and Legal Immigrant Workers?: E-Verify and Legal Worker Outcomes.” *Southern Economic Journal* 81, no. 4 (April 2015): 960–79. <https://doi.org/10.1002/soej.12019>.

Borjas, George J., and Hugh Cassidy. “The Wage Penalty to Undocumented Immigration.” *Labour Economics* 61 (December 2019): 101757. <https://doi.org/10.1016/j.labeco.2019.101757>.

Bozick, Robert, Trey Miller, and Matheu Kaneshiro. “Non-Citizen Mexican Youth in U.S. Higher Education: A Closer Look at the Relationship between State Tuition Policies and College Enrollment.” *International Migration Review* 50, no. 4 (2016): 864–89. <https://doi.org/10.1111/imre.12167>.

Brier, David J. “E-Verify Let 12 Million Illegal Hires Happen Since 2006—80% of Attempts.” *Cato at Liberty Blog*, May 29, 2019. <https://www.cato.org/blog/e-verify-let-12-million-illegal-hires-happen-2006-80-beat-system>.

Bucheli, Jose R., Joaquín Alfredo-Angel Rubalcaba, and Edward D. Vargas. “Out of the Class and Into the Shadows: Immigration Enforcement and Education Among U.S.-Citizen and Foreign-Born Hispanics.” *AERA Open* 7 (January 2021): 233285842110563. <https://doi.org/10.1177/23328584211056349>.

California Student Aid Commission. “Renewing the Dream.” In *Undocumented Student Affordability Report*, 2023. <https://www.csac.ca.gov/undocumented-student-affordability-report>.

Chin, Aimee, and Chinhui Juhn. “Does Reducing College Costs Improve Educational Outcomes for Undocumented Immigrants? Evidence from State Laws Permitting Undocumented Immigrants to Pay In-State Tuition at State Colleges and Universities.” In *Latinos and the Economy: Integration and Impact in Schools, Labor Markets, and Beyond*, edited by David L. Leal and Stephen J. Trejo, Part II, pp. 63–94, 2011.

Ciccia, Diego. “A Short Note on Event-Study Synthetic Difference-in-Differences Estimators.” *arXiv:2407.09565*. Preprint, arXiv, November 1, 2024. <https://doi.org/10.48550/arXiv.2407.09565>.

Clarke, Damian, Daniel Pailanñir, Susan Athey, and Guido Imbens. “Synthetic Difference-in-Differences Estimation.” *arXiv:2301.11859*. Preprint, arXiv, February 13, 2023. <https://doi.org/10.48550/arXiv.2301.11859>.

Deb, Partha, Edward Norton, Jeffrey Wooldridge, and Jeffrey Zabel. *A Flexible, Heterogeneous Treatment Effects Difference-in-Differences Estimator for Repeated*

Cross-Sections. No. W33026. National Bureau of Economic Research, 2024.
<https://doi.org/10.3386/w33026>.

Dee, Thomas S., and Mark Murphy. “Vanished Classmates: The Effects of Local Immigration Enforcement on School Enrollment.” *American Educational Research Journal* 57, no. 2 (April 2020): 694–727. <https://doi.org/10.3102/0002831219860816>.

Dickson, Lisa, and Matea Pender. “Do In-State Tuition Benefits Affect the Enrollment of Non-Citizens? Evidence from Universities in Texas.” *Economics of Education Review* 37 (December 2013): 126–37. <https://doi.org/10.1016/j.econedurev.2013.08.006>.

East, Chloe N., Annie Laurie Hines, Philip Luck, Hani Mansour, and Andrea Velasquez. “The Labor Market Effects of Immigration Enforcement.” *Journal of Labor Economics*, June 6, 2022, 721152. <https://doi.org/10.1086/721152>.

Ellis, Mark, Richard Wright, Matthew Townley, and Kristy Copeland. “The Migration Response to the Legal Arizona Workers Act.” *Political Geography* 42 (September 2014): 46–56. <https://doi.org/10.1016/j.polgeo.2014.06.001>.

Hansen, Collin. “Sweet Casa Alabama (and Arizona, and...): Examining the Economic Outcomes of State Immigration Reform.” *SSRN Electronic Journal*, 2019.
<https://doi.org/10.2139/ssrn.3377360>.

Hegland, Thomas A. “wooldid: Estimation of Difference-in-Differences Treatment Effects with Staggered Treatment Onset Using Heterogeneity-Robust Two-Way Fixed Effects Regressions.” *Statistical Software Components* s459238, 2023.

Kaushal, Neeraj. “In-State Tuition for the Undocumented: Education Effects on Mexican Young Adults.” *Journal of Policy Analysis and Management* 27, no. 4 (September 2008): 771–92. <https://doi.org/10.1002/pam.20366>.

Kirksey, J. Jacob, Carolyn Sattin-Bajaj, Michael A. Gottfried, Jennifer Freeman, and Christopher S. Ozuna. “Deportations Near the Schoolyard: Examining Immigration Enforcement and Racial/Ethnic Gaps in Educational Outcomes.” *AERA Open* 6, no. 1 (January 2020): 233285841989907.
<https://doi.org/10.1177/2332858419899074>.

Kolenikov, Stanislav, and Trent D. Buskirk. “Finding Respondents in the Forest: A Comparison of Logistic Regression and Random Forest Models for Response Propensity Weighting and Stratification.” *Survey Methods: Insights from the Field (SMIF)*, 2015.
<https://doi.org/10.13094/SMIF-2015-00003>.

Kuka, Elira, Na’aama Shenhav, and Kevin Shih. “Do Human Capital Decisions Respond to the Returns to Education? Evidence from DACA.” *American Economic Journal: Economic Policy* 12, no. 1 (February 1, 2020): 293–324.
<https://doi.org/10.1257/pol.20180352>.

Lopez, Mark Hugo, Jeffrey S. Passel, and D’Vera Cohn. “Key Facts About the Changing U.S. Unauthorized Immigrant Population.” *Short Reads* (Pew Research Center), April 13, 2021. <https://www.pewresearch.org/short-reads/2021/04/13/key-facts-about-the-changing-u-s-unauthorized-immigrant-population/>.

Luo, Tianyuan, and Cesar L. Escalante. “Stringent Immigration Enforcement and the Mental Health and Health-Risk Behaviors of Hispanic Adolescent Students in Arizona.” *Health Economics* 30, no. 1 (January 2021): 86–103. <https://doi.org/10.1002/hec.4178>.

Luo, Tianyuan, and Genti Kostandini. “The Wage Impacts of Intensified Immigration Enforcement on Native and Immigrant Workers.” *Applied Economics* 54, no. 58 (December 14, 2022): 6656–68. <https://doi.org/10.1080/00036846.2022.2075539>.

Moslimani, Mohammad, and Luis Noe-Bustamante. “Facts on Latinos in the U.S. in 2021.” *Fact Sheet* (Pew Research Center), August 16, 2023. <https://www.pewresearch.org/race-and-ethnicity/fact-sheet/latinos-in-the-us-fact-sheet/>.

Orrenius, Pia M., and Madeline Zavodny. “The Impact of E-Verify Mandates on Labor Market Outcomes: E-Verify and Labor Market Outcomes.” *Southern Economic Journal* 81, no. 4 (April 2015): 947–59. <https://doi.org/10.1002/soej.12023>.

Orrenius, Pia M., Madeline Zavodny, and Emily Gutierrez. “Do State Employment Eligibility Verification Laws Affect Job Turnover?” *Contemporary Economic Policy* 36, no. 2 (2018): 394–409. <https://doi.org/10.1111/coep.12251>.

Orrenius, Pia M., Madeline Zavodny, and Sarah Greer. “Who Signs Up for E-Verify? Insights from DHS Enrollment Records.” *International Migration Review* 54, no. 4 (2020): 1184–211. <https://doi.org/10.1177/0197918320901461>.

Pan, Weixiang, and Ben Ost. “The Impact of Parental Layoff on Higher Education Investment.” *Economics of Education Review* 42 (October 2014): 53–63. <https://doi.org/10.1016/j.econedurev.2014.06.006>.

Passel, Jeffrey S., and Jens Manuel Krogstad. “U.S. Unauthorized Immigrant Population Reached a Record 14 Million in 2023.” *Pew Research Center*, August 21, 2025. <https://www.pewresearch.org/race-and-ethnicity/2025/08/21/u-s-unauthorized-immigrant-population-reached-a-record-14-million-in-2023/>.

Presidents’ Alliance on Higher Education and Immigration. “Undocumented Students in Higher Education: Breakdown of the Undocumented Student Population in U.S. Colleges and Universities.” June 2025. <https://www.presidentsalliance.org/wp-content/uploads/2025/07/Undocumented-Students-in-Higher-Education-June-2025.pdf>.

Ruhnke, Simon A., Fernando A. Wilson, and Jim P. Stimpson. “Using Machine

Learning to Impute Legal Status of Immigrants in the National Health Interview Survey.” *MethodsX* 9 (2022): 101848. <https://doi.org/10.1016/j.mex.2022.101848>.

Flores, Stella M. “State Dream Acts: The Effect of In-State Resident Tuition Policies and Undocumented Latino Students.” *The Review of Higher Education* 33, no. 2 (2009): 239–83. <https://doi.org/10.1353/rhe.0.0134>.

Thomas, Philip. “state-partisan-composition.” *GitHub Repository*.
<https://github.com/pstthomas/state-partisan-composition/tree/master/data>.

Van Hook, Jennifer, James D. Bachmeier, Donna L. Coffman, and Ofer Harel. “Can We Spin Straw Into Gold? An Evaluation of Immigrant Legal Status Imputation Approaches.” *Demography* 52, no. 1 (February 1, 2015): 329–54.
<https://doi.org/10.1007/s13524-014-0358-x>.

Wooldridge, Jeffrey M. “Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators.” *SSRN Electronic Journal*, 2021.
<https://doi.org/10.2139/ssrn.3906345>.