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A License Is Not a Work Permit: Status-Blind Occupational Licensing and the Brain Waste of Undocumented Immigrants

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Abstract Since 2014, seven US states have used a provision of federal law to let residents obtain professional and occupational licenses regardless of immigration status. Supporters expected these laws to help undocumented college graduates move into jobs that match their education. We test this expectation with the American Community Survey for 2009–2024, the licensing questions of the Current Population Survey for 2015–2024, and a stacked triple-differences design that compares likely undocumented immigrants with natives in reform and other states. The laws did not reduce overeducation among likely undocumented college graduates: the estimate is 0.69 percentage points (standard error 0.58) against a pre-reform rate of 58.3 percent. After correcting for the misclassification of legal status, we can rule out reductions larger than 1.05 points with conventional standard errors and larger than 4.80 points with randomization inference, at most 21 percent of the overeducation gap with natives. The laws also did not raise license holding or employment in licensed occupations. Foreign credentials do not explain the result, because immigrants educated in the United States did not respond either. Immigrants eligible for Deferred Action for Childhood Arrivals, who hold work permits, show consistent but imprecise gains. The evidence indicates that the binding constraint is the lack of work authorization: a license can be obtained but not used in wage employment. We also show that conventional pre-trend tests reject far too often when few states are treated, and we calibrate them by randomization inference.

Keywords Occupational licensing · Undocumented immigrants · Overeducation · Brain waste · Work authorization · DACA

JEL Classification J44 · J61 · J24 · K37 · L51

1 Introduction

About 1.8 million unauthorized immigrants aged 25 and older in the United States hold a bachelor's degree or a higher degree (Gelatt et al., 2025). Many of them work in jobs that need much less schooling than they have. In the states we study, 59 percent of employed college graduates who are likely to be undocumented held a job for which they were overeducated before the reforms we examine, compared with 36 percent of college-educated natives. This waste of skills, often called "brain waste," lowers the earnings of the workers concerned and the output of the economy (Batalova et al., 2016; Mattoo et al., 2008; Ortega & Hsin, 2022).

Occupational licensing is often blamed for part of this waste. About one in five American workers holds a government-issued license (Cunningham, 2019), earlier surveys put the share of workers whose job requires a license close to three in ten (Kleiner & Krueger, 2013), and many licensed occupations, such as teaching, nursing, engineering, accounting and law, normally require a college degree. Since 1996, federal law has treated a professional license as a "public benefit" that immigrants who are not lawfully present may

not receive, unless a state legislature passes a law that makes them eligible.[1] Beginning with California in 2014, seven states have passed such laws. They allow licensing boards to issue licenses regardless of immigration status, usually by accepting an Individual Taxpayer Identification Number (ITIN) in place of a Social Security number (Table 1). Supporters presented these laws as a way to let undocumented graduates use the skills they already have.

A license, however, is not a work permit. The Immigration Reform and Control Act of 1986 (IRCA) still forbids employers to hire workers who lack federal work authorization, and it requires employers to verify the eligibility of every new hire.[2] An undocumented nurse or teacher who obtains a state license still cannot be hired legally by a hospital or a school district. Whether status-blind licensing reduces brain waste therefore depends on which barrier binds: the license or the right to work. If the license is the binding constraint, the laws should move undocumented graduates into jobs that match their education. If work authorization is the binding constraint, the laws should matter mainly for the few who can work on their own account and for immigrants who already hold a work permit, such as recipients of Deferred Action for Childhood Arrivals (DACA).

This paper tests these two views. We combine the American Community Survey (ACS) for 2009–2024 with the licensing questions of the monthly Current Population Survey (CPS) for 2015–2024 and with a database of state laws that we assembled from statutes, regulations and official notices. Our design is a stacked triple-differences comparison. We ask whether the gap in overeducation between likely undocumented immigrants and natives narrowed in states that adopted status-blind laws, relative to the same gap in the 40 states that adopted no law of this kind. Comparing the two groups within the same state and year removes any shock that hits all workers in a state, such as a local boom or another state policy. Comparing reform states with other states removes shocks that hit undocumented immigrants everywhere, such as changes in federal enforcement. Because only seven states adopted the laws, we pay close attention to statistical inference and rely on the wild cluster bootstrap and on randomization inference rather than on conventional cluster-robust standard errors alone (Conley & Taber, 2011; MacKinnon & Webb, 2018, 2020).

We reach four conclusions. First, the laws did not reduce overeducation. Our main estimate is 0.69 percentage points (standard error 0.58), against a pre-reform rate of 58.3 percent, and no estimate among 24 alternative measures, samples and timing choices shows a statistically significant reduction. Second, this null result is informative. Our proxy for undocumented status inevitably includes some legal non-citizens who could already obtain licenses, which pulls estimates toward zero. After correcting for this misclassification, we can rule out reductions in overeducation larger than 1.05 percentage points with conventional standard errors, and larger than 4.80 points with the wider spread of placebo estimates. Even the second figure is only 21 percent of the 22.4-point gap in overeducation between likely undocumented graduates and natives. Third, the laws did not change the outcome on which they act most directly. Likely undocumented immigrants in reform states were no more likely

[1] See 8 U.S.C. § 1621(a), (c)(1)(A) and (d), added by the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (Pub. L. 104-193). Section 2.1 describes these provisions.

[2] See 8 U.S.C. § 1324a(a)(1)(A), (a)(2) and (b), added by the Immigration Reform and Control Act of 1986 (Pub. L. 99-603). Section 2.3 gives details.

to hold a government license, or to work in a licensed occupation, after the laws than before. The null is not explained by foreign credentials that licensing boards may not recognize, because immigrants who completed their schooling in the United States did not respond either. Fourth, the one group that holds a work permit, DACA-eligible immigrants, shows the pattern that the laws were meant to produce. When all laws open to DACA recipients are pooled, the point estimates indicate a rise of 3.64 percentage points in employment in licensed occupations and a fall of 3.13 points in overeducation. These estimates are imprecise, because few DACA-eligible immigrants are college graduates, but they have the same sign in every comparison we make.

Taken together, the evidence indicates that the binding constraint on undocumented college graduates is the lack of work authorization rather than the license itself. A license can be obtained, but it cannot be used in wage employment, which is where most licensed graduates work.

The paper contributes to three literatures. The first studies how occupational licensing affects immigrants. Licensing requirements reduce the entry of immigrants into licensed occupations and shape where they work (Cassidy & Dacass, 2021; Federman et al., 2006; Peterson et al., 2014), and relicensing rules for foreign-trained physicians affect who re-enters the profession (Kugler & Sauer, 2005). The closest study is Chung (2024), who shows in this journal that California's reform raised the employment of likely undocumented immigrants, with the largest gains in lower-education and blue-collar licensed occupations. We study all seven status-blind laws together, focus on college graduates and on skill mismatch rather than employment, and measure license holding directly in the CPS. Our results complement those of Chung (2024): the laws may help where a license can be used in self-employment, but they do little for graduates whose licensed jobs require an employer. More broadly, our results add to the evidence on the costs and benefits of licensing for workers and consumers (Blair & Chung, 2019; Gittleman et al., 2018; Kleiner, 2000; Kleiner & Krueger, 2013; Kleiner & Soltas, 2023; Redbird, 2017), including the large differences in licensing across states documented in this journal by Kleiner & Vorotnikov (2017).

The second literature studies how legal status shapes the labor market outcomes of immigrants. Legal status raises the wages of immigrants who obtain it (Kaushal, 2006; Kossoudji & Cobb-Clark, 2002; Rivera-Batiz, 1999), and DACA raised employment and changed schooling decisions among eligible young people (Amuedo-Dorantes & Antman, 2017; Hsin & Ortega, 2018; Kuka et al., 2020; Pope, 2016). Undocumented men work at least as many hours as legal immigrants but earn less (Borjas, 2017; Borjas & Cassidy, 2019), and the lack of legal status keeps many undocumented workers out of occupations that would match their skills (Ortega & Hsin, 2022). Our results show that removing one occupational barrier while leaving the legal barrier to employment in place achieves little: access to licenses and the right to work are complements. The paper also relates to studies of other state policies aimed at undocumented immigrants, such as employment-verification mandates and driver's licenses (Amuedo-Dorantes et al., 2020; Amuedo-Dorantes & Bansak, 2012; Bohn et al., 2014; Orrenius & Zavodny, 2015); we control for both in all our estimates.

The third literature measures the overeducation and skill underuse of immigrants (Aleksynska & Tritah, 2013; Chiswick & Miller, 2008, 2009; Mattoo et al., 2008; Nielsen, 2011). Most of this work attributes the gap to the imperfect transfer of foreign education,

language and networks. We show that undocumented graduates educated in the United States are also far more often overeducated than natives (47 against 36 percent before the reforms), and that removing an occupational barrier specific to legal status did not reduce their overeducation.

Finally, we make a methodological point that applies to many evaluations of state policies. With few treated states, conventional cluster-robust tests reject the null hypothesis too often (Bertrand et al., 2004; Cameron et al., 2008; Conley & Taber, 2011; MacKinnon & Webb, 2018). We show that the problem is even more severe for the joint tests of pre-trends that applied researchers use to validate event studies. When we assign the laws to randomly chosen control states, a conventional joint test of pre-trends rejects at the 5 percent level in 48 percent of the placebo assignments. We therefore report pre-trend p-values calibrated by randomization inference, which is simple to implement and could be used more widely (Rambachan & Roth, 2023; Roth, 2022).

The rest of the paper is organized as follows. Section 2 describes the legal setting. Section 3 sets out the hypotheses. Section 4 describes the data and measures, and Section 5 the empirical strategy. Section 6 presents the main results and Section 7 examines why the laws had no effect. Section 8 discusses the implications and limitations, and Section 9 concludes.

2 Institutional background

2.1 Licenses as public benefits

The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 made immigrants who are neither "qualified aliens" nor lawful temporary visitors or parolees ineligible for "any State or local public benefit" (8 U.S.C. § 1621(a)). This group includes every immigrant who is not lawfully present. The act defines such benefits to include "any grant, contract, loan, professional license, or commercial license provided by an agency of a State or local government" (§ 1621(c)(1)(A)). It also gives states a way out. A state may make immigrants who are not lawfully present eligible for a benefit, but "only through the enactment of a State law after August 22, 1996, which affirmatively provides for such eligibility" (§ 1621(d)). Many licensing boards put the federal bar into practice by asking applicants for a Social Security number or for proof of lawful presence. Undocumented immigrants generally cannot obtain a Social Security number, so these requirements kept them out of licensed occupations even when they held the required degree and could pass the required examinations.

2.2 Status-blind licensing laws

Table 1 lists the laws we study. California was first. Senate Bill 1159, signed in September 2014 and in force from 1 January 2015, invoked § 1621(d) and provided that no licensing board within the state's Department of Consumer Affairs "shall deny licensure to an applicant based on his or her citizenship status or immigration status." It required boards to accept an ITIN in place of a Social Security number and gave them until 1 January 2016 to implement the change. We therefore treat 2016 as California's first full year of exposure and 2015 as a partial year. This choice differs from Chung (2024), who treats 2015 as the first

year after the reform; column (2) of Table 3 shows that the choice does not affect our results. Illinois followed with a law in force from January 2019, Nevada from July 2019, Oregon from January 2020, New Jersey from September 2020, New Mexico from June 2021 and Colorado from September 2021. The laws differ in detail. Oregon's law requires boards to accept an ITIN in place of a Social Security number, and Colorado's removes lawful-presence verification for all state and local licenses. All seven, however, remove the practical obstacle, the Social Security number or proof of lawful presence, that kept applicants without legal status out of licensed occupations. In each case we define the first treated year as the first calendar year in which the law was in force throughout.

Table 1 State laws on occupational licenses for immigrants without lawful status

State	Law	Enacted	In force	First full year	Who may obtain a license
A. Status-blind laws (used in all analyses)					
California	SB 1159 (Stats. 2014)	Sep 2014	Jan 2015	2016	All applicants; boards must accept an ITIN
Illinois	SB 3109 (P.A. 100-1078)	Aug 2018	Jan 2019	2019	All applicants; status may not be the sole reason for denial
Nevada	AB 275 (2019, ch. 627)	Jun 2019	Jul 2019	2020	All applicants; status may not be the sole reason for denial
Oregon	SB 854 (2019)	2019	Jan 2020	2020	Boards must accept an ITIN in place of an SSN
New Jersey	S2455 (P.L.2020, c.75)	Sep 2020	Sep 2020	2021	All applicants; lawful presence may not be required
Colorado	SB21-077 (2021, ch. 186)	May 2021	Sep 2021	2022	All applicants; lawful presence not required for any state or local license
New Mexico	SB 219 (Laws 2021, ch. 70)	Mar 2021	Jun 2021	2022	All applicants; legal-residency requirements removed
B. Laws limited to work-authorized applicants (used only for DACA-eligible immigrants)					
Nebraska	LB 947 (2016)	Apr 2016	Jul 2016	2017	DACA recipients
New York	Board of Regents regulations (2016)	May 2016	May 2016	2017	DACA recipients (licenses and teaching certificates)
Indiana	SB 419 (2018)	Mar 2018	Jul 2018	2019	Work-authorized applicants, including DACA recipients
Arkansas	HB 1735 (Act 746 of 2021)	Apr 2021	Jul 2021	2022	Work-authorized applicants, including DACA recipients

Enacted: month the bill was signed (Nebraska: veto override; New York: adoption of the regulation by the Board of Regents). First full year: first calendar year in which the law was in force throughout, used as the first treated year. California's law took effect in January 2015 but gave licensing boards until January 2016 to comply. ITIN: Individual Taxpayer Identification Number; SSN: Social Security number. Sources: statutes, legislative records and official notices listed in Online Resource 1, Table S1.

Four other states opened licenses only to immigrants with federal work authorization, a group that in practice consists mainly of DACA recipients: Nebraska and New York in 2016, Indiana in 2018 and Arkansas in 2021 (Table 1, Panel B). These laws do not change anything for undocumented immigrants without a work permit, so we use them only in the analysis of DACA recipients in Section 7.3. The remaining 40 states, which passed neither type of law, form our comparison group.[3]

[3] Three of these states adopted similar laws at the very end of our sample period: Tennessee in 2022 (for work-authorized applicants only), Delaware in 2023 and Washington in 2024. Dropping them from the comparison

2.3 A license is not a work permit

The state laws change who may hold a license. They do not change who may be employed. Under IRCA, it is unlawful "to hire, or to recruit or refer for a fee, for employment in the United States an alien knowing the alien is an unauthorized alien," or to continue to employ such a worker (8 U.S.C. § 1324a(a)(1)(A) and (a)(2)). Every employer must verify the identity and work eligibility of each new hire and attest to this verification under penalty of perjury on Form I-9 (§ 1324a(b)). Employers also may not obtain the labor of an unauthorized worker indirectly through a contract or subcontract if they know the worker's status (§ 1324a(a)(4)). Some states go further and require employers to check new hires through the federal E-Verify system (Amuedo-Dorantes & Bansak, 2012; Bohn et al., 2014; Orrenius & Zavodny, 2015).

These rules apply to employers. They do not reach a person who works on her own account, and the implementing regulations exclude independent contractors from the definition of an employee (8 C.F.R. § 274a.1(f) and (j)). An undocumented immigrant with a state license can therefore open her own practice, work as a contractor or run a small business, but she cannot be hired legally as a wage or salary worker. This distinction matters because most college graduates in licensed occupations work for an employer. Before the laws, only 1.1 percent of employed likely undocumented graduates in the reform states were self-employed in a licensed occupation, against 7.3 percent who were wage or salary workers in one. Teachers, nurses and engineers work mostly for schools, hospitals and firms, which must complete Form I-9 for every hire.

2.4 DACA and laws limited to work-authorized immigrants

DACA, announced in June 2012, grants a renewable two-year deferral of deportation to immigrants who arrived in the United States before their 16th birthday, had lived in the country continuously since June 2007, were not above the age of 30 in June 2012, were in school or had finished high school or military service, and had no serious criminal record (Napolitano, 2012). Recipients may obtain an employment authorization document and a Social Security number (Pope, 2016). DACA recipients therefore hold the work permit that other undocumented immigrants lack. Before the state laws, however, many licensing boards still treated them as ineligible for a license under § 1621. The status-blind laws and the four laws limited to work-authorized applicants removed this barrier for them. DACA recipients are thus the one group for which the state laws relaxed the licensing barrier while the barrier to employment was already absent.

3 What should the laws do?

Consider an undocumented college graduate trained for a licensed occupation, say a nurse. Before the reform she has two options: an unlicensed job, which often requires much less schooling than she has, or no job. The reform adds a license. Whether she can use it depends on how she would work in the licensed occupation.

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group, together with Maryland, which considered a similar bill in 2022, does not change our results (Section 6.2 and Online Resource 1, Table S5).

If she works for an employer, she needs both a license and work authorization. The reform supplies the first but not the second. In this case the reform can move her into a licensed job only if employers ignore the verification rules or she uses documents that are not her own, both of which are risky and costly. If she can work on her own account, as a contractor, a private tutor or the owner of a small business, she needs only the license. The reform then opens a real option.

These considerations give three testable predictions.

- **Hypothesis 1 (the license binds).** If the license was the main barrier, the laws should raise license holding among likely undocumented immigrants, move them into licensed occupations and reduce their overeducation.
- **Hypothesis 2 (work authorization binds).** If the right to work is the main barrier, the laws should have little effect on likely undocumented graduates as a whole. Any effect should appear in self-employment and among immigrants who already have work authorization, of whom DACA recipients are the main group.
- **Hypothesis 3 (credentials bind).** Licensing boards may not recognize foreign degrees or experience (Chiswick & Miller, 2009; Peterson et al., 2014). If foreign credentials are the barrier, the laws should help undocumented immigrants educated in the United States more than those educated abroad, even without work authorization.

The tests in Sections 6 and 7 follow these predictions. Section 6 tests the central implication of Hypothesis 1 for overeducation. Section 7 examines license holding and job outcomes (Hypothesis 1), DACA recipients and self-employment (Hypothesis 2), and immigrants educated in the United States (Hypothesis 3).

4 Data and measurement

4.1 Samples

Our main data are the 1-year samples of the ACS for 2009–2024, obtained from IPUMS USA (Ruggles et al., 2025). We keep persons aged 18 to 64 who live in households or in non-institutional group quarters, which leaves 28.6 million person records. Most analyses use employed college graduates, defined as persons with a bachelor's degree or higher who are employed and report an occupation. This sample contains 357,727 likely undocumented immigrants and 6,614,549 natives. We use the ACS person weights throughout.

The ACS does not ask whether a worker holds a license. For this we use the monthly CPS for January 2015 to December 2024, obtained from IPUMS CPS (Flood et al., 2025). Since January 2015 the CPS has asked every adult whether he or she has a currently active professional certification or a state or industry license, whether any such credential was issued by a federal, state or local government, and whether it is required for the respondent's job (Allard, 2016). We use the CPS in two ways: to measure how licensed each occupation is, and to estimate directly whether the laws changed license holding.

4.2 Identifying likely undocumented immigrants

Neither survey records legal status. We therefore use the logical-imputation approach that is standard in this literature (Amuedo-Dorantes et al., 2020; Borjas, 2017; Chung, 2024). A non-

citizen is classified as likely undocumented unless he or she shows a marker of legal status. The markers are arriving in the United States before 1980, which made most long-term residents eligible for the 1986 legalization; being a veteran; being born in Cuba, since Cuban immigrants have a special path to legal status; working for a government; and receiving Medicare, Medicaid, food stamps (SNAP), Social Security or Supplemental Security Income, which are largely unavailable to immigrants without legal status. In the CPS, which lacks the benefit variables, we apply the other criteria.

This proxy captures most undocumented immigrants, but it also includes many legal non-citizens, such as permanent residents and temporary workers, whom no survey variable identifies (Van Hook et al., 2015). The problem is largest among college graduates, since many legal temporary workers are highly educated. To gauge it, we compare the size of our proxy group with an external estimate. In 2023 our ACS proxy contains 4.0 million college graduates aged 25 to 64, whereas the Migration Policy Institute estimates that 1.8 million unauthorized immigrants aged 25 or older held a bachelor's degree or higher in mid-2023 (Gelatt et al., 2025). Assuming that about 95 percent of them are younger than 65, only about 43 percent of our proxy group is truly undocumented. Legal non-citizens could obtain licenses before the reforms, so including them dilutes any effect of the laws. Section 5.3 shows how we correct for this dilution.

We also identify immigrants who were likely eligible for DACA. They are non-citizens who arrived before age 16 and no later than 2007, were born in the third quarter of 1981 or later, and have at least a high school diploma, are enrolled in school or are veterans. We classify immigrants as educated in the United States if they arrived before age 18 and as educated abroad if their age at arrival is at least their years of schooling plus six, which means they completed their schooling before arriving.

4.3 Measuring overeducation

We measure overeducation with the realized-matches method (Hartog, 2000; Kiker et al., 1997; Verdugo & Verdugo, 1989). For each occupation in the harmonized 2010 Census classification, we compute the mean and standard deviation of years of schooling among employed natives aged 25 to 64, pooling all years. A worker is overeducated if her years of schooling exceed the mean of her occupation by more than one standard deviation. A software developer with a bachelor's degree is not overeducated, because many developers have that degree. A taxi driver or a cashier with a bachelor's degree is, because few natives in those jobs have one. We drop occupations with fewer than 50 native observations.

Defining the thresholds on natives keeps them fixed when the composition of immigrant workers changes, and pooling the years keeps them fixed over time. The measure is not perfect: it ignores differences in the quality of degrees and in skills within an occupation (Leuven & Oosterbeek, 2011). We therefore check our results with four alternatives: thresholds of half a standard deviation and one and a half standard deviations; the modal years of schooling in the occupation; thresholds computed only from 2009–2013, before any law took effect; and thresholds computed separately for each year.

4.4 Measuring licensing

From the CPS we compute, for each occupation, the share of employed workers who hold a license issued by a government. We call an occupation licensed if at least half of its workers hold one, and assign this classification to ACS workers through their occupation. Our measure of licensed employment therefore reflects the occupation, not the individual. We also compute the share licensed in the worker's occupation as a continuous measure of licensing intensity. In the CPS itself, we measure license holding directly with three outcomes: holding a government-issued license, holding a license that is required for the current job, and holding any certification or license.

4.5 Descriptive statistics

Table 2 compares employed college graduates in the four groups we study. Likely undocumented graduates are much more often overeducated than natives (0.58 against 0.36) and naturalized citizens (0.47). They are less likely to work in a licensed occupation (0.09 against 0.24 for natives), and in the CPS far fewer of them hold a government-issued license (0.12 against 0.32). About 92 percent of them arrived as adults and 75 percent completed their schooling abroad, which is why many studies attribute their poor matches to imperfectly transferable foreign education (Chiswick & Miller, 2008, 2009). DACA-eligible graduates, who grew up and were educated in the United States, are younger and less often overeducated (0.45) than other likely undocumented graduates, but they remain far more overeducated than natives.

Table 2 Employed college graduates by group, ACS 2009–2024

	Natives	Naturalized citizens	Likely undocumented	DACA-eligible
Overeducated	0.358	0.469	0.576	0.445
Works in a licensed occupation	0.235	0.206	0.093	0.144
Holds a government-issued license (CPS)	0.322	0.265	0.123	–
Years of schooling	16.83	17.05	17.18	16.52
Age	41.5	45.0	37.9	28.7
Female	0.523	0.505	0.403	0.522
Hispanic	0.070	0.183	0.179	0.445
Speaks English poorly or not at all	0.002	0.030	0.078	0.049
Arrived at age 18 or older	–	0.609	0.921	0.000
Completed schooling before arrival	–	0.451	0.746	0.000
Years in the United States	–	24.7	9.5	21.1
Self-employed	0.088	0.110	0.086	0.054
Log hourly wage	3.38	3.51	3.43	3.06
Observations (ACS)	6,614,549	746,870	357,727	18,050
Population per year (thousands)	41,985	4,980	2,655	143

Weighted means for employed persons aged 18–64 with a bachelor's degree or higher who report an occupation. Overeducated: years of schooling more than one standard deviation above the mean of employed natives aged 25–64 in the same occupation. Licensed occupation: at least half of the occupation's workers hold a government-issued license in the CPS. The CPS row refers to college graduates aged 25–64 in the CPS, 2015–2018. DACA-eligible immigrants are a subset of non-citizens defined in Section 4.2; – not applicable or not available.

5 Empirical strategy

5.1 Stacked triple differences

We compare two groups, likely undocumented immigrants and natives, in two kinds of states, those that adopted a status-blind law and those that did not, before and after the laws. The estimating sample consists of state × year × group cells. For each law cohort e, defined by the first full year of exposure (2016, 2019, 2020, 2021 and 2022), we build a separate dataset, or "stack," containing the states treated in that year and the 40 comparison states, for the years from $e-5$ to $e+4$. We then estimate on all stacks together

$$Y_{sgte} = \beta\, U_g T_{se} P_{te} + \alpha_{sge} + \lambda_{tge} + \mu_{ste} + X'_{sgte}\gamma + \delta_1 U_g D_{st} + \delta_2 U_g E_{st} + \varepsilon_{sgte}, \quad (1)$$

where Y_{sgte} is the share of overeducated workers in state s, group g and year t in stack e; U_g equals one for likely undocumented immigrants and zero for natives; T_{se} equals one for the states treated in stack e; and P_{te} equals one from year e onwards. The three sets of fixed effects are specific to each stack. State × group effects α_{sge} absorb permanent differences between groups in each state. Year × group effects λ_{tge} absorb national shocks to each group, such as DACA or changes in federal enforcement. State × year effects μ_{ste} absorb every shock common to all workers in a state and year, such as a local recession or a change in state taxes. The vector X_{sgte} contains the cell means of age, sex, marital status, Hispanic origin, years of schooling and poor English. D_{st} and E_{st} indicate state laws that give driver's licenses to undocumented residents and that require private employers to use E-Verify, interacted with the group indicator. Cells are weighted by their population, and standard errors are clustered by state.

The coefficient β in Eq. (1) measures how much the gap in overeducation between likely undocumented immigrants and natives changed in the reform states, relative to the change in the same gap in the comparison states. It is a triple difference (Gruber, 1994; Olden & Møen, 2022). Stacking compares each cohort of reform states only with states that never adopted a law, and therefore avoids the biased comparisons between early and late adopters that affect two-way fixed-effects estimators when treatment timing is staggered and effects differ across cohorts (Callaway & Sant'Anna, 2021; Cengiz et al., 2019; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Roth et al., 2023; Sun & Abraham, 2021). To trace the effects over time, we replace P_{te} with indicators for each year relative to the reform, from five years before to four years after, with the year before the reform as the reference.

The identifying assumption is that, without the laws, the gap between likely undocumented immigrants and natives would have evolved in the same way in reform and comparison states. The assumption allows reform states to differ from other states in their level of overeducation, in their economic conditions and in their trends, as long as these differences affect both groups alike. Before the laws, likely undocumented graduates in reform states and in comparison states were similar in age, schooling, sex and the share overeducated (Online Resource 1, Table S4). The event-study estimates in Section 6 provide a direct check on the assumption.

5.2 Inference with few treated states

Seven treated states form a small number of treated clusters, and conventional cluster-robust standard errors can then be much too small (Bertrand et al., 2004; Conley & Taber, 2011; Ferman & Pinto, 2019; MacKinnon & Webb, 2018). We therefore report two additional p-values for every estimate. The first comes from the restricted wild cluster bootstrap-t with Webb's six-point weights, which performs better than conventional methods with few treated clusters (Cameron et al., 2008; MacKinnon et al., 2023; MacKinnon & Webb, 2017, 2018; Webb, 2023). We use 9,999 replications for the main estimate and 1,999 for the others. The second comes from randomization inference (MacKinnon & Webb, 2020; Young, 2019). We assign each law cohort to the same number of randomly chosen comparison states, re-estimate the model, and repeat this 999 times for the main estimate and 299 times for the others. The p-value is the share of placebo estimates that are at least as large in absolute value as the actual estimate.

The placebo assignments also let us check how often conventional tests reject when the null hypothesis is true by construction. We record, for each placebo assignment, whether the cluster-robust t-test of β and the joint Wald test that all pre-reform event-study coefficients are zero reject at the 5 percent level. If these tests had the correct size, each would reject in about 5 percent of the placebo assignments. Section 6 shows that they reject much more often. We therefore also report a calibrated pre-trend p-value, defined as the share of placebo assignments whose pre-trend p-value is at least as small as the actual one.

5.3 Misclassification of legal status

Our proxy group mixes truly undocumented immigrants, who gained access to licenses, with legal non-citizens, who already had it. If a share s of the proxy group is truly undocumented and the laws do not affect legal non-citizens, the estimated effect on the proxy group equals s times the effect on undocumented immigrants (Aigner, 1973; Bound et al., 2001). The effect on undocumented immigrants is then β/s. We use this relationship to translate our estimates and their confidence intervals into statements about undocumented immigrants. For example, the largest reduction in overeducation that a 95 percent confidence interval cannot rule out is $-\left(\hat{\beta} - 1.96\,\hat{\sigma}\right)/s$, where $\hat{\sigma}$ is either the cluster-robust standard error or the standard deviation of the placebo estimates. Our benchmark for s is 0.43 (Section 4.2). Because this figure is uncertain, we also report results for a low value of 0.30 and a high value of 0.58.

5.4 Software and use of generative AI

All estimates were produced in Python with the pyfixest package and our own code for the bootstrap and randomization inference. The replication package reproduces every table and figure from the original IPUMS extracts. The authors used a generative AI tool (Claude, Anthropic) to help write and check the analysis code and to help draft and edit the text of the manuscript. The authors specified every analysis, reviewed all code and output, verified every reference and every number reported, and take full responsibility for the content of the paper.

6 Did the laws reduce overeducation?

6.1 Main estimates

Table 3 reports the main results. Column (1) gives our preferred estimate for employed likely undocumented college graduates. The laws changed the share overeducated by 0.69 percentage points, with a standard error of 0.58 and a 95 percent confidence interval from −0.45 to 1.83. The wild bootstrap p-value is 0.367 and the randomization-inference p-value is 0.637. Against a pre-reform rate of 58.3 percent in the reform states, the point estimate is close to zero and, if anything, positive. It gives no sign that the laws moved undocumented graduates into jobs better suited to their education.

Table 3 Effect of status-blind licensing laws on overeducation

	(1)	(2)	(3)	(4)	(5)
	Main	California from 2015	Without California	All non-citizens	Born in Latin America
Estimate	0.69	0.42	0.31	0.44	−1.75
	(0.58)	(0.68)	(0.76)	(0.50)	(1.80)
95% confidence interval	[−0.45, 1.83]	[−0.92, 1.76]	[−1.17, 1.79]	[−0.55, 1.43]	[−5.28, 1.78]
p-value: wild cluster bootstrap	0.367	0.653	0.732	0.461	0.384
p-value: randomization inference	0.637	0.789	0.860	0.742	0.532
Pre-trend p-value (calibrated)	0.988	0.846	0.946	0.933	0.468
Pre-reform mean of treated group (%)	58.3	58.6	58.9	57.3	66.5
Observations (treated group)	79,781	79,353	31,625	96,894	10,832
Treated states	7	7	6	7	7

Estimates in percentage points. Outcome: overeducated (0/1) among employed likely undocumented college graduates relative to natives. Column (1): main specification. (2): California treated from 2015, when its law took effect. (3): California dropped. (4): all non-citizens as the treated group. (5): likely undocumented immigrants born in Latin America. Stacked triple-differences estimates on state × year × group cells, with stack-specific state × group, year × group and state × year fixed effects, cell means of demographic controls, and controls for state driver's-license and E-Verify laws interacted with the group indicator. Cells are weighted by population. Standard errors (in parentheses) are clustered by state. Wild bootstrap: restricted wild cluster bootstrap-t with Webb weights. RI: randomization inference, re-assigning each law cohort to randomly chosen comparison states. Calibrated pre-trend p-value: share of placebo assignments with a joint pre-trend test p-value at least as small as the actual one.

The other columns show that this conclusion does not depend on our main choices. Column (2) treats California as exposed from 2015, when its law took effect, rather than from 2016, when its boards had to comply. Column (3) drops California, the largest reform state, whose law came first and therefore dominates the later event-study years. Column (4) replaces our proxy with all non-citizens, an “intention-to-treat” group that requires no imputation of legal status. Column (5) restricts the proxy to immigrants born in Latin America, among whom a larger share of non-citizens is undocumented. None of these estimates shows a significant reduction in overeducation. The only negative point estimate, for immigrants born in Latin America, is imprecise (−1.75 points, standard error 1.80).

Fig. 1 shows the event-study estimates. Before the laws, the coefficients are small and show no trend; the calibrated pre-trend p-value is 0.988. After the laws, the coefficients stay close to zero for four years. Only the last coefficient, four years after the reform, is clearly positive (2.56 points, standard error 0.91). It is estimated only from the four earliest reform states (California, Illinois, Nevada and Oregon), and it points toward more overeducation, not less.

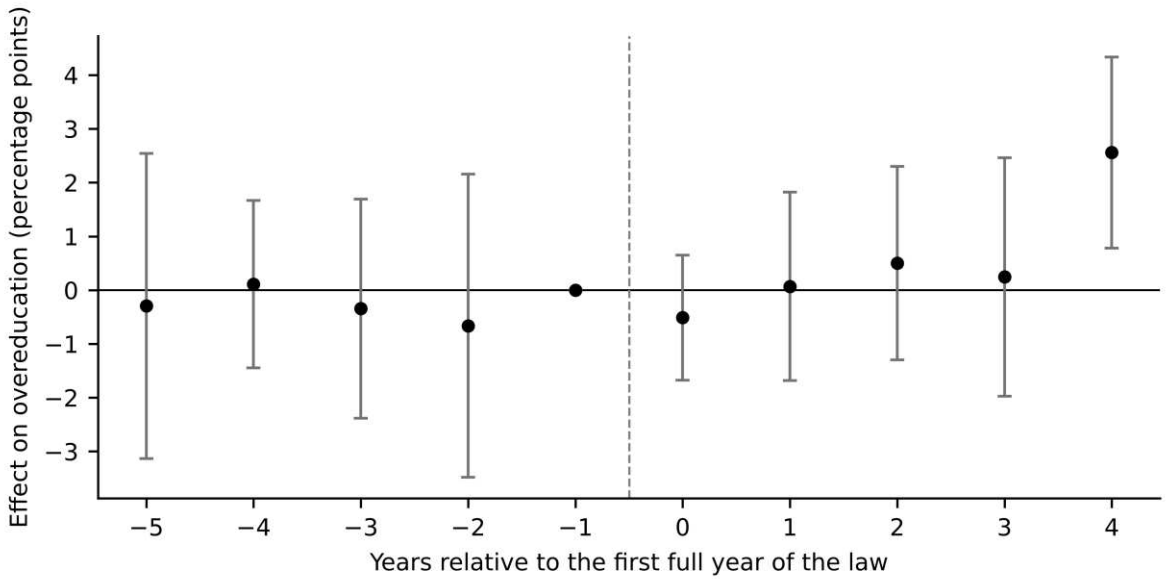


Fig. 1 Event-study estimates of the effect of status-blind licensing laws on overeducation. Employed likely undocumented college graduates relative to natives, ACS 2009–2024. Points are the coefficients on the interaction of the group indicator, the reform-state indicator and each year relative to the first full year of the law; the year before the law is the reference and is set to zero. Bars are 95 percent confidence intervals based on standard errors clustered by state. The specification is that of Table 3, column (1)

The placebo assignments confirm that conventional tests are unreliable in this setting. When the laws are assigned at random to comparison states, the cluster-robust t-test rejects the true null hypothesis of no effect in 17 percent of the assignments, and the cluster-robust joint test of pre-trends rejects in 48 percent, against a nominal rate of 5 percent. A researcher relying on the conventional pre-trend test would often reject valid designs. This is why we report calibrated pre-trend p-values throughout.

6.2 Robustness

Fig. 2 summarizes 24 variations of the main specification; Online Resource 1 (Table S5) reports the full results. They change the measure of overeducation (thresholds of 0.5 and 1.5 standard deviations, the modal schooling of the occupation, thresholds from the pre-reform years only, and thresholds by year); the treated group (excluding students, using all non-citizens, splitting by region of birth, and a placebo group of naturalized citizens); the timing (California from 2015, partial-year exposure for all states, dropping California's partial year, and a shorter event window); and the sample (dropping California, dropping 2020, when the ACS was collected under pandemic conditions, dropping comparison states that adopted related laws at the end of the period, and dropping the demographic controls). The last rows drop one reform state at a time. The estimates range from −1.75 to 0.92 percentage points, and none of them is significantly negative under any of our three tests. The placebo estimate for naturalized citizens, who could always obtain licenses, is 0.36 points.

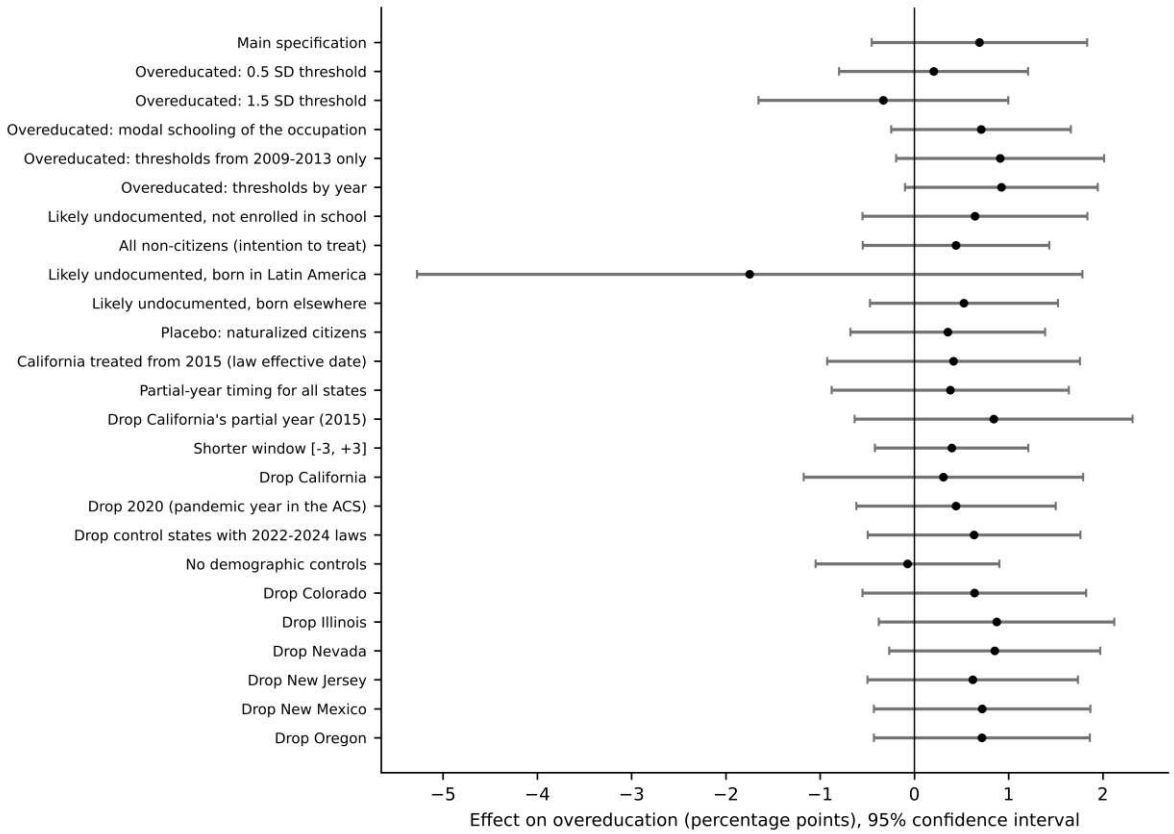


Fig. 2 Robustness of the main estimate. Each row is a separate estimate of the effect of the laws on overeducation, in percentage points, with its 95 percent confidence interval based on standard errors clustered by state. Online Resource 1 (Table S5) reports wild bootstrap and randomization-inference p-values for every row

6.3 How large an effect can we rule out?

A null result is useful only if the estimate is precise enough to exclude effects that would matter. Table 4 translates the main estimate into bounds on the effect for truly undocumented immigrants, using the misclassification correction of Section 5.3. With our benchmark share of truly undocumented immigrants in the proxy group (s = 0.43) and conventional standard errors, the largest reduction in overeducation that we cannot rule out is 1.05 percentage points. This is 5 percent of the 22.4-point gap between likely undocumented graduates and natives before the laws. If we use the spread of the placebo estimates instead, which is wider, the bound rises to 4.80 points, or 21 percent of the gap. Even under the pessimistic assumption that only 30 percent of the proxy group is undocumented, the bound is 6.89 points, or 31 percent of the gap. Whatever the true effect of the laws, they closed only a small part of the gap.

Table 4 How large a reduction in overeducation can be ruled out?

	Share truly undocumented (s)	Corrected effect (pp)	Largest reduction not ruled out (pp)	% of pre-reform rate	% of gap with natives
A. Uncertainty from cluster-robust standard errors					
Low s	0.30	2.30	1.51	2.6	6.7
Benchmark s	0.43	1.61	1.05	1.8	4.7
High s	0.58	1.19	0.78	1.3	3.5
B. Uncertainty from the spread of placebo estimates					
Low s	0.30	2.30	6.89	11.8	30.8
Benchmark s	0.43	1.61	4.80	8.2	21.5
High s	0.58	1.19	3.56	6.1	15.9

Based on the main estimate (Table 3, column 1). The estimate and the lower limit of its 95 percent confidence interval are divided by s, the share of the proxy group that is truly undocumented (benchmark from Section 4.2, with a low and a high alternative). The largest reduction not ruled out is minus the corrected lower limit. Cluster-robust: interval based on the cluster-robust standard error. Placebo spread: interval based on the standard deviation of the randomization-inference placebo estimates. The gap with natives is the difference in pre-reform overeducation rates in the reform states (Online Resource 1, Table S3).

7 Why did the laws not reduce brain waste?

7.1 Did more immigrants obtain licenses?

The most direct test of Hypothesis 1 asks whether the laws changed license holding. Before most laws took effect (2015–2018), 12 percent of likely undocumented college graduates in the CPS held a government-issued license, against 32 percent of college-educated natives. If the license had been the binding barrier, this gap should have narrowed in the reform states.

It did not. Panel A of Table 5 applies our design to the CPS for 2015–2024. Among college graduates, the laws changed the probability of holding a government license by −0.42 percentage points (standard error 1.12); among all likely undocumented adults, the change was 0.32 points (0.52). Fig. 3 shows no sustained change after the laws in either group: the yearly coefficients fluctuate around zero after the reforms as they did before. After correcting for misclassification, we can rule out increases in license holding larger than 5.5 percentage points among college graduates and 2.1 points among all adults with conventional standard errors (10.2 and 3.5 points with the placebo spread).[4] Other measures tell the same story: the laws did not raise the probability of holding a license required for the current job, of holding any certification or license, or of working in a licensed occupation (Online Resource 1, Table S6).

[4] The CPS licensing questions start in January 2015, so California's reform, whose first full year is 2016, can be studied with only one pre-reform year. Results that exclude California are similar (Online Resource 1, Table S6). Because the basic monthly CPS lacks the benefit variables, the CPS proxy differs from the ACS proxy; we compute its share of truly undocumented immigrants in the same way (Online Resource 1, Table S7).

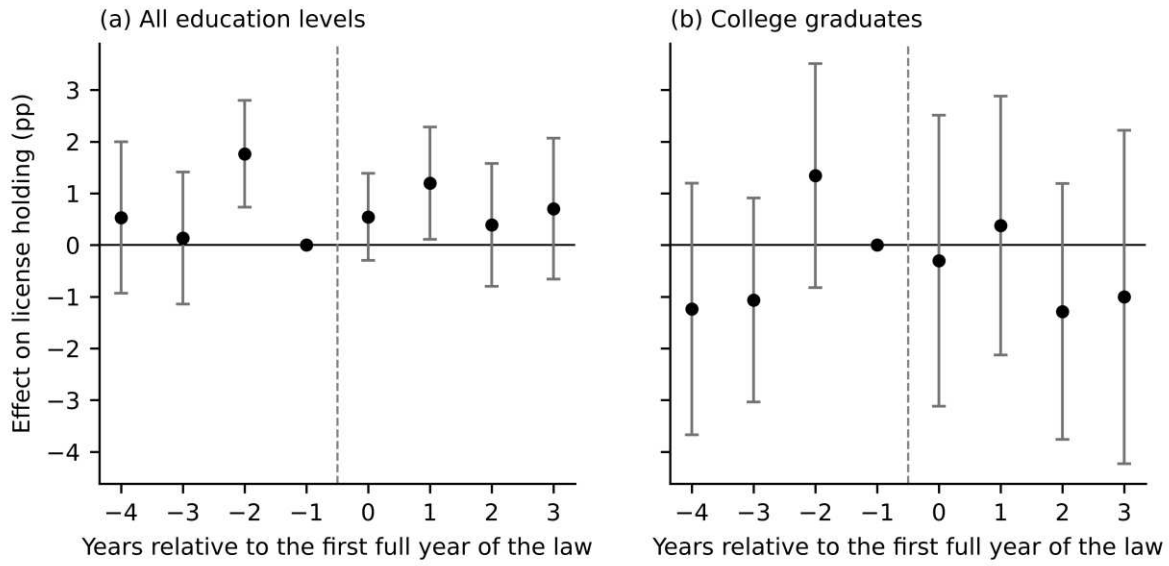


Fig. 3 Event-study estimates of the effect of the laws on holding a government-issued license, CPS 2015–2024. (a) Likely undocumented adults aged 25–64 of all education levels relative to natives. (b) College graduates. California is treated from 2016. Because the CPS licensing questions start in 2015, the earliest relative years are estimated only from the later reform states. Bars are 95 percent confidence intervals based on standard errors clustered by state

Table 5 License holding, jobs and pay

	Estimate (SE)	95% CI	p (wild)	p (RI)	Pre-trend p (RI)	Pre-reform mean
A. License holding, CPS 2015–2024 (percentage points)						
Holds a government license: college graduates	−0.42 (1.12)	[−2.62, 1.78]	0.785	0.823	0.756	11.4
Holds a government license: all education levels	0.32 (0.52)	[−0.70, 1.35]	0.639	0.779	0.231	6.7
License required for current job: employed graduates	−0.86 (1.30)	[−3.40, 1.68]	0.732	0.716	0.742	9.0
B. Jobs and pay, ACS 2009–2024, employed college graduates						
Works in a licensed occupation (pp)	−0.20 (0.42)	[−1.03, 0.63]	0.690	0.809	0.957	8.4
Share licensed in the occupation (pp)	−0.10 (0.40)	[−0.88, 0.68]	0.851	0.903	0.990	18.6
Natives' mean schooling in the occupation (years)	−0.01 (0.02)	[−0.05, 0.04]	0.793	0.856	0.880	15.00
Wage or salary worker in a licensed occupation (pp)	−0.35 (0.43)	[−1.19, 0.48]	0.524	0.676	0.746	7.3
Self-employed in a licensed occupation (pp)	0.15 (0.10)	[−0.03, 0.34]	0.231	0.485	0.452	1.1
Log hourly wage (× 100)	3.72 (1.68)	[0.43, 7.02]	0.158	0.137	0.916	3.38
Employed, all college graduates (pp)	1.08 (1.01)	[−0.90, 3.05]	0.528	0.227	0.649	73.1

Each row is a separate regression comparing likely undocumented immigrants with natives. Panel A uses the CPS for persons aged 25–64, with California treated from 2016; 2015 is its only pre-reform year. Panel B uses the ACS sample of Table 3 except for the last row, which includes all college graduates aged 18–64. Pre-reform mean in percent, except for occupational schooling (years) and the log hourly wage (log points). Stacked triple-differences estimates on state × year × group cells, with stack-specific state × group, year × group and state × year fixed effects, cell means of demographic controls, and controls for state driver's-license and E-Verify laws interacted with the group indicator. Cells are weighted by population. Standard errors (in parentheses) are clustered by state. Wild bootstrap: restricted wild cluster bootstrap-t with Webb weights. RI: randomization inference, re-assigning each law

cohort to randomly chosen comparison states. Calibrated pre-trend p-value: share of placebo assignments with a joint pre-trend test p-value at least as small as the actual one.

7.2 Jobs, job quality and earnings

Panel B of Table 5 turns to the ACS and to outcomes that would change if more undocumented graduates used licenses. The share working in a licensed occupation changed by −0.20 percentage points (0.42). The average schooling of natives in the occupations where likely undocumented graduates work, a measure of the skill level of their jobs, changed by −0.01 years (0.02). Neither wage or salary work in licensed occupations (−0.35 points) nor self-employment in licensed occupations (0.15 points) responded. Employment among all likely undocumented graduates did not change either (1.08 points). The only estimate in Panel B that is significant by conventional standards is for hourly wages, which rose by 3.7 log points (standard error 1.7). Neither the wild bootstrap ($p = 0.158$) nor randomization inference ($p = 0.137$) rejects the null hypothesis, however, and without any change in jobs or occupations we do not interpret it as an effect of the laws.

These results reject Hypothesis 1 for the average undocumented graduate. The laws did not change license holding, the type of job or the skill level of the job. They also suggest why. Self-employment, the one route through which a license can be used without a work permit, is rare among licensed graduates: before the laws only 1.1 percent of likely undocumented graduates in the reform states were self-employed in a licensed occupation. A law that makes licenses available to people who cannot be hired affects few college graduates, because the jobs for which their licenses would qualify them are mostly wage jobs.

7.3 Work authorization: evidence from DACA

Hypothesis 2 predicts that the laws should help immigrants who can be hired legally. DACA-eligible immigrants are such a group. Table 6 reports separate estimates for them, using three comparison groups that address different concerns. Natives of the same birth cohorts share the age profile of DACA-eligible immigrants. Undocumented immigrants of the same cohorts who are not DACA-eligible share their legal disadvantages except the work permit. Child arrivals who are too old for DACA share their age at arrival and their American schooling.

Table 6 Work authorization and foreign credentials

	Licensed occupation			Overeducated				
	Estimate (SE)	p (wild)	p (RI)	Estimate (SE)	p (wild)	p (RI)	Obs. (treated)	Treated states
A. Work authorization: DACA-eligible immigrants								
vs natives of the same birth cohorts	2.67 (1.61)	0.175	0.361	−2.95 (2.26)	0.272	0.625	4,525	7
vs likely undocumented, not DACA-eligible, same cohorts	2.81 (1.76)	0.220	0.341	−4.05 (2.67)	0.209	0.458	4,525	7
vs child arrivals too old for DACA	1.20 (3.69)	0.761	0.779	−4.39 (4.50)	0.366	0.652	4,525	7
vs natives, including laws limited to work-authorized applicants	3.64 (1.59)	0.086	0.151	−3.13 (1.99)	0.152	0.452	5,968	11
B. Foreign credentials: likely undocumented immigrants without DACA								
Educated in the US vs natives	−4.75 (2.07)	0.077	0.187	3.03 (3.19)	0.438	0.468	2,902	7
Educated abroad vs natives	−0.12 (0.47)	0.823	0.819	0.77 (0.71)	0.403	0.569	58,866	7
Educated in the US vs educated abroad	−4.85 (2.08)	0.079	0.171	2.68 (3.20)	0.538	0.475	2,902	7

Employed college graduates, ACS 2009–2024. Estimates in percentage points. Each cell is a separate regression of the outcome on the triple interaction for the treated group named in the row relative to the comparison group. Comparisons with natives of the same birth cohorts and with likely undocumented immigrants who are not DACA-eligible are restricted to persons born in 1981 or later. The last row of Panel A adds the four states whose laws covered only work-authorized applicants (Table 1, Panel B). Educated in the US: arrived before age 18; educated abroad: completed schooling before arrival. Stacked triple-differences estimates on state × year × group cells, with stack-specific state × group, year × group and state × year fixed effects, cell means of demographic controls, and controls for state driver's-license and E-Verify laws interacted with the group indicator. Cells are weighted by population. Standard errors (in parentheses) are clustered by state. Wild bootstrap: restricted wild cluster bootstrap-t with Webb weights. RI: randomization inference, re-assigning each law cohort to randomly chosen comparison states. Calibrated pre-trend p-value: share of placebo assignments with a joint pre-trend test p-value at least as small as the actual one.

All three comparisons point in the same direction. Relative to natives of the same cohorts, the point estimates indicate that the laws raised the share of DACA-eligible graduates working in licensed occupations by 2.67 percentage points and lowered their overeducation by 2.95 points. Relative to undocumented immigrants without DACA, the corresponding estimates are 2.81 and −4.05 points, and relative to child arrivals too old for DACA they are 1.20 and −4.39 points. When we add the four states whose laws covered only work-authorized applicants, which increases the number of reform states to 11, the estimates are 3.64 points for licensed employment (wild bootstrap p = 0.086, randomization-inference p = 0.151) and −3.13 points for overeducation (p = 0.152 and 0.452). Fig. 4 displays all these estimates.

These estimates are imprecise. The estimation windows contain only 4,525 person records of employed DACA-eligible college graduates in the reform states, and none of the DACA estimates is significant under randomization inference. We therefore do not claim to have shown that the laws helped DACA recipients. The pattern, however, is the one Hypothesis 2 predicts: estimates that are consistently positive for licensed employment and negative for overeducation among the immigrants who can be hired, and no gains among those who cannot. Measured against a pre-reform rate of 11.4 percent working in licensed

occupations, the pooled point estimate for DACA-eligible graduates is a relative increase of about 32 percent.

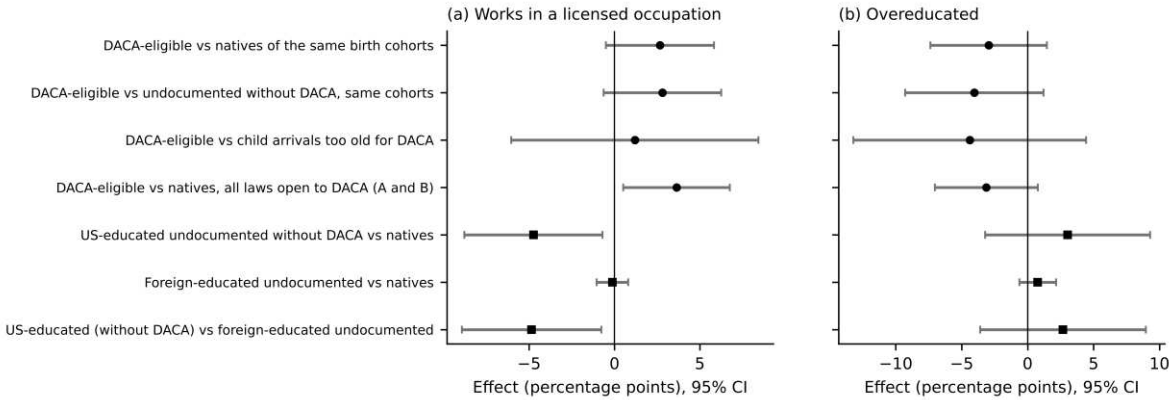


Fig. 4 Work authorization and foreign credentials. Effects of the laws on (a) working in a licensed occupation and (b) overeducation, in percentage points, for DACA-eligible immigrants (circles) and for likely undocumented immigrants educated in the United States or abroad (squares), each relative to the comparison group named on the left. Bars are 95 percent confidence intervals based on standard errors clustered by state. Table 6 reports the p-values

7.4 Foreign credentials

Hypothesis 3 attributes the null result to foreign credentials. Licensing boards may not recognize degrees or professional experience obtained abroad, so a license could be out of reach even after the laws. Panel B of Table 6 tests this by comparing undocumented immigrants educated in the United States, excluding those eligible for DACA, with natives and with undocumented immigrants educated abroad. If foreign credentials were the barrier, immigrants educated in the United States should have gained from the laws. They did not: their share in licensed occupations changed by −4.75 points and their overeducation by 3.03 points relative to natives, and their estimates are no larger than those of immigrants educated abroad. If anything, they moved away from licensed occupations, although this estimate rests on only 2,902 person records and is not significant under the wild bootstrap or randomization inference. Without a work permit, an American degree did not help either. This is consistent with evidence that undocumented young people raised and educated in the United States meet the legal barrier to employment when they leave school (Gonzales, 2011).

7.5 Less-educated workers and California

Chung (2024) finds that California's reform raised the employment of likely undocumented immigrants, with the largest gains in lower-education and blue-collar licensed occupations. Our design can be applied to the same population (Online Resource 1, Table S10). For all likely undocumented workers, the laws changed employment in licensed occupations by 0.07 points from a base of 3.8 percent. Self-employment in licensed occupations changed by 0.08 points, which is 15 percent of its low base. In California alone, the estimate for licensed employment is 0.38 points with Chung's timing and 0.24 points with ours. These estimates are positive but small and imprecise, and with a single treated state randomization inference cannot distinguish them from zero. They do not contradict the findings of Chung (2024), which concern employment and rest on a different comparison and design, but they suggest

that any gains among less-educated workers were modest. The laws may help workers in trades where independent work is possible; they did not help college graduates, whose licensed occupations are organized around employers.

8 Discussion

8.1 Interpretation and policy

The evidence fits Hypothesis 2 better than the alternatives. The laws did not change license holding, licensed employment or overeducation among likely undocumented graduates as a whole. The null result is precise enough to rule out more than a small narrowing of their large overeducation gap, and it holds for immigrants educated in the United States. The one group that holds a work permit, DACA-eligible immigrants, shows the expected pattern, even though our data are too thin to establish it with confidence. Taken together, these results suggest that the binding constraint on undocumented college graduates is not the license but the right to be hired.

This conclusion matters for the design of policy. Status-blind licensing laws are cheap to adopt, and we find no evidence that they worsened the outcomes of the immigrants they targeted. Our results do not imply that states should repeal them. The laws give undocumented immigrants a credential they can use if their legal situation changes, and they may help less-educated workers in trades where independent work is possible, as the evidence of Chung (2024) suggests. However, states should not expect these laws, on their own, to reduce brain waste among undocumented graduates. Most licensed jobs for college graduates are wage jobs, and the federal requirement of work authorization keeps undocumented graduates out of them whatever the state licensing rules are. The benefits of licensing access depend on federal policy toward work authorization, of which DACA is the most important current example. If the DACA program is narrowed or ended, the main group that could benefit from state licensing reforms will shrink. Conversely, policies that grant work authorization would raise the return to holding a license. For state policy makers who want to use the skills of undocumented graduates, measures that make independent practice feasible are likely to complement licensing reforms better than further changes in licensing rules alone.

More generally, our results show that the effect of removing one barrier to employment depends on the other barriers that remain. The barriers created by licensing and by immigration law are complements: removing the first while the second binds achieves little. Evaluations of licensing reforms for other groups, such as people with criminal records or military spouses, may face a similar problem when other legal barriers remain.

8.2 Limitations

Our analysis has several limitations. First, legal status is not observed. Our proxy misclassifies many legal non-citizens as undocumented, and our correction depends on an external estimate of the share truly undocumented. We report bounds for a wide range of values, and the conclusion that the laws closed at most a small part of the gap holds across this range. The correction also assumes that legal non-citizens were not affected by the laws. If some of them were affected, for example because licensing boards became more

welcoming to immigrants in general, our estimates would overstate the effect on undocumented immigrants, and the bounds would be conservative.

Second, the ACS does not record licenses, so our ACS measures of licensed employment refer to the occupation. A worker who obtains a license and stays in the same occupation, for example by moving from an unlicensed to a licensed position as a teacher, would not be counted. Our CPS estimates measure license holding directly, however, and they show no change either.

Third, the later laws are observed for only a few years. Colorado's and New Mexico's laws were in force for three full years by the end of our data, and obtaining a license can take time. Our event-study estimates show no sign of a reduction that grows over time in the early reform states, but longer series will be needed to judge the long-run effect of the recent laws.

Fourth, the laws could have changed where undocumented immigrants live. Licensing rules affect interstate migration (Johnson & Kleiner, 2020), and the reform states might have attracted undocumented graduates hoping to use a license. We cannot rule out such migration. However, to produce our null result it would have to offset a genuine reduction in overeducation almost exactly, which seems unlikely given that license holding did not change either.

Fifth, the DACA samples are small. The DACA results are consistent with our interpretation, but they are not precise enough to stand on their own. Finally, the overeducation measure compares schooling with the typical schooling of natives in the same occupation. It does not capture differences in the quality of degrees or mismatch within occupations (Leuven & Oosterbeek, 2011).

9 Conclusion

Since 2014, seven states have used a provision of the 1996 welfare reform to give undocumented immigrants access to professional and occupational licenses. We find that these laws did not reduce the overeducation of likely undocumented college graduates, did not raise their license holding, and did not move them into licensed occupations. The null result is precise: after correcting for the misclassification of legal status, the laws closed at most a small part of the overeducation gap between likely undocumented graduates and natives. Foreign credentials do not explain the result, because immigrants educated in the United States did not respond either. DACA-eligible immigrants, who hold work permits, show consistent though imprecise gains.

The lesson is simple. A license is not a work permit. For college graduates without legal status, the binding constraint on using their skills is the federal prohibition on hiring them, not the state license. State licensing reforms can complement, but not replace, policies that address work authorization.

Statements and Declarations

Competing interests. The authors have no relevant financial or non-financial interests to disclose.

Data availability. The ACS and CPS microdata are available from IPUMS USA and IPUMS CPS (Flood et al., 2025; Ruggles et al., 2025) under the IPUMS terms of use, which

do not permit redistribution. The replication package contains the code, exact extract specifications, the database of state laws with sources, and all intermediate results needed to reproduce every table and figure. It will be deposited in a public repository upon acceptance.

Code availability. All code is included in the replication package.

Ethics approval. Not applicable. The study uses publicly available, de-identified survey data.

Use of generative AI. See Section 5.4.

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