# Intended and unintended effects of state tuition benefits to undocumented students : Institution and individual-level evidence

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**Abstract**

I investigate how allowing lower tuition for undocumented students at public colleges improves education outcomes, changes institutional pricing patterns, and lowers long-run fertility rates. I use administrative data and a residual method to quantify the actual number of undocumented students at school level in the pre-reform period. Exploiting the reform's staggered adoption across states and time, as well as variation in the intensity of exposure to the reform across institutions –- I find a higher enrollment of undocumented students at the treated states' 'more exposed' community colleges. Transfer, technical and vocational colleges drive the enrollment outcomes. In contrast to enrollment, there is strong evidence of higher graduation of undocumented students at both 2-year and 4-year colleges in the treated states. I also observe that students at these 'more exposed' institutions experience modest tuition reductions. There is negligible displacement of Americans in treated public colleges. Undocumented females reduce their fertility in response to higher educational attainment –- driven by delayed marriage and household formation decisions. My findings indicate that the education and fertility benefits to undocumented students come with no significant unintended costs to other students. I estimate that the reform costs around $16.4 million per year on average.

**Keywords:** Subsidies, Education, Pricing, Incidence, Major Choice, Undocumented Students, Fertility, Family Structure, Documented students.

**JEL Classification:** I22,I23,I28,J15, K37.

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# 1 Introduction

The decision to withdraw DACA in September 2017, separation of families who crossed the border illegally in April 2018 and increased number of deportations of undocumented immigrants at the US-mexico border in the last 2 years have led to renewed interest in policies related to undocumented immigrants.[2] One prominent reform that some US states have implemented is allowing undocumented students to pay the lower in-state tuition rates in public colleges, rather than the much higher out-of-state tuition rates.[3] Existing literature suggests that these tuition subsidies have raised the college enrollment of Mexican non-citizens using household surveys. However, we do not know about how the intensity of exposure to the reform based on the historical presence of undocumented students in an institution –- affects the education outcomes of these students, and the pricing policies of the institutions affected by the reform. [4]Moreover, there is lack of causal evidence on how these lower college costs affect the long run fertility choices of eligible undocumented immigrants and their subsequent living arrangements.

In this paper, I focus on undocumented immigrant youth because they have historically had lower access to postsecondary education and labor market opportunities. I estimate effects of the intensity of exposure to the tuition subsidy reform on college enrollment, time-to-degree, and college retention of undocumented students. I also examine whether this reform causes 'more exposed' public institutions to charge higher tuition for all students, thereby reducing the net benefits for the undocumented ones. I further explore whether this reform affects the long run fertility and living arrangement choices of eligible undocumented individuals. Starting from 2001 to date, 22 out of 50 US states have

[2]For official DACA cancellation order, See https://www.whitehouse.gov/briefings-statements/president-donald-j-trump-restores-responsibility-rule-law-immigration/.For executive order related to alien families' separation, see, https://www.whitehouse.gov/presidential-actions/affording-congress-opportunity-address-family-separation/.

[3]For example, in AY 2016-17, out of state tuition and fees ($28,229) of Ohio State University was almost thrice of their in state tuition and fees ($10,037).

[4]Note that, I calculate the actual number of undocumented students in an institution, using data from IPEDS and administrative records from SEVIS.

allowed tuition subsidies for undocumented immigrants in public colleges. I exploit the staggered roll-out of the reform across states and time, coupled with variation provided by the intensity of exposure to the reform across institutions, in a difference-in-differences framework to answer the above research questions. The primary sources of data used in my analysis are annual institution level data from Integrated Post-secondary Education Data System (IPEDS), yearly institution level data on foreign student visa holders from SEVIS, and yearly individual data from American Community Survey.

Policies providing tuition subsidy to undocumented students can have both direct and spillover effects. First, they may influence college entry decisions of undocumented students. Given that students may positively select into various colleges, I derive the Average Treatment on the Treated (ATT) estimates of the reform by comparing the education outcomes of eligible students within institutions. I find that the reform increases the NRA share of first-year enrollment by 0.026 percentage points in public 2 year colleges, for every 1% of the undocumented share in the treated states. I then use the SEVIS records and a residual method to identify the exact number of undocumented students by institution for the pre-policy year 2003. The advantage of distinguishing the 'highly exposed' institutions by counting the precise number of undocumented students in a school is that, my reform effects are large –- less prone to attenuation due to mismeasurement of undocumented students. The estimates reveal a 24% (baseline mean: 42.9) increase in the number of NRAs in treated state 2-year institutions with above median number of pre-policy undocumented students. Among the 2 year colleges, there is increased enrollment of these students in high transfer and technical & vocational colleges. In case of public 4 year institutions, the reform causes the NRA share to rise by 0.003 percentage points, for every 1% of the undocumented share in the treated states. This estimate is statistically insignificant. However, the effect size is economically meaningful –- the number of NRAs in treated state 4-year institutions 'more exposed' to the reform increases by around 14% (baseline mean: 42.5). The modest rise in enrollment at the 4 year institutions can be attributed to the more competitive ones, as per Barron's ratings of institutions. There is no evidence of students sorting into different majors in 'more exposed' treated states –- an intuitive result given

that there are no major specific pricing policies in the design of this reform.

Second, I provide evidence on the college completion effects of the reform. This question is worth empirical analysis because the impact of the reform on college graduation of undocumented students is theoretically ambiguous. On one hand, the lower price of education makes college more affordable for them and hence incentivises them to continue their education and finish it. On the other hand, these financial benefits may induce more low ability undocumented students to apply to colleges than before. This selection effect can undo the above positive effect and be partially responsible for the lower retention rates among them. Understanding the college progression and completion effects of the undocumented students is different from that of other students, and hence I discuss its importance below. Some studies suggest that college completion rates have decreased, mainly among poor students, despite their higher college enrollment rates (Bailey and Dynarski, 2011). Moreover, undocumented students may not have the same incentives to complete their college education as compared to other legal students. This may be because their returns to education are lower as they face constant possibilities of deportation. They may thus respond less to changes in college prices and be less likely to graduate from college, relative to documented students and natives.[5] The above reasons underscore the importance of studying the effects of the reform on undergraduate college completion rates of these students. My analysis suggests that, the NRA share of total graduates increases by 0.013 percentage points in community colleges, for every 1% of the undocumented share in the treated states. In fact, there is a 10% (baseline mean: 44.6) rise in the number of NRAs graduating from these treated state institutions 'highly exposed' to the reform. Additionally, the number of NRAs graduating from treated state 4-year institutions 'heavily exposed' to the reform increases by 6.5% (baseline mean:41.7).

One possible unintended effect of this reform can be that such policies hurt natives and legal immigrants by reducing their enrollment slots, as often argued by the opponents of the reform. However, I find no negative effects on the enrollment or graduation rates of

[5]Note that, they can change their undocumented status through marriage to citizens (Duncan and Trejo, 2007).

the Americans in treated states 'more exposed' to the reform. In fact, I find mild positive effects on the enrollment of natives in community colleges, timed with the reform. This is consistent with the explanation that the net tuition from undocumented students helps to support the cost of enrolling more natives (Shih, 2017). This can also be explained by the fact that the community colleges were not operating at capacity prior to the reform, and hence possibilities of displacement of native students do not arise.

Another potential unintended effect can be institutions charging higher tuition as a result of providing these subsidies. But I observe that on average, students do not experience increases in their tuition in 'more exposed' institutions of the treated states. [6]This uniformly applies to instate and out-of-state tuition; public 2 year and 4 year colleges, as well as flagship schools. This seems intuitive, because in most states, public colleges do not have the ability to raise their tuition without approval from their governing bodies. I however find evidence of small decreases in institutional aid in these particular colleges, timed with the reform. Thus, the net pass-through of the subsidies on the prices students pay in these 'more exposed' institutions is small.

The above results are robust to several alternative specifications, including the Callaway and Sant'Anna(2021) estimates. I further conduct a battery of placebo tests to rule out alternative channels for the increased enrollment of undocumented students. These include –- confounding labor market conditions, political environment, and changes in the menu of courses offered by the public colleges.

In the second half of the paper, my results indicate that likely undocumented females respond to the increased educational attainment induced by the reform by delaying their fertility and household formation decisions. In fact, for every 100 likely undocumented females, there are 4 fewer births in the treated states. I posit the cause of this reduced fertility as the higher opportunity cost of a child associated with a mother's loss of time from the labor force as she becomes more educated because of the reform. We can also think of it as the higher opportunity cost of time devoted to child rearing activities due to

[6]In this context, it is important to mention that identifying the 'more exposed' institutions by counting the exact number of undocumented students in each school is crucial for obtaining this result.

women being enrolled in colleges. It is also possible that women learn about contraception methods in school and their time spent at school reduces their time available for romantic relationships. This is reflected in women delaying their marriage and forming their own households.

To sum up, the results of this paper support the effectiveness of the reform in improving the education outcomes of eligible undocumented students. However, I uncover significant heterogeneity in the impact of the reform by type of institution, which cannot be captured by the existing literature using household surveys. Additionally, the reform has positive indirect effects beyond education, on fertility and household formation decisions of undocumented students.These gains come with no unintended costs to other students in the same institution.

My results can help answer some policy relevant questions. First, they can inform the discourse on –- to what extent we can improve the low education levels of undocumented students by reducing the price of education. For this, it is essential to explore their education outcomes beyond the first order effects on college enrollment. Second, they can shed light on whether undocumented students respond to the tuition subsidies by switching majors. This has implications for gender and racial inequality in the labor market earnings. Third, my estimates can inform the policymakers on how different public school authorities respond to the reform by adjusting along the margins of tuition and institutional aid. This is useful to understand the incidence of the subsidies on the students and the schools. Fourth, the fertility effects of increased educational attainment of a disadvantaged group has important implications. As these undocumented females delay childbearing, they may be better able to invest more time and money in the child's upbringing thereby improving the child's health and education. Thus, this reform has far reaching inter-generational consequences and may improve the welfare of both parents and children.

The remainder of the paper is organised as follows. In section 2, I describe the basic features of the reform. Section 3 presents my contribution in the related literature. Section 4 describes the data and identification strategy. In section 5, I present my results. Section 6 shows a number of robustness checks and placebo analyses. Section 7 concludes with an

estimate of the cost of the reform.

# 2 Policy Background

There were 12 million undocumented immigrants in US as of January, 2015 and they represented 3.74% of the total US population(Department of Homeland Security, December 2018). Of these undocumented immigrants,39% were in the age group of 18-34 years and could potentially be affected by the tuition subsidy reform. Because US has a large number of undocumented immigrant youth having below average educational attainment, education related immigration policies are important.

The 1982 Plyler vs Doe case allowed undocumented students access to education until high school. However, this case did not provide them access to further higher educational institutions. Section 505 of the Illegal Immigration Reform and Responsibility Act (IIRRA) of 1996 prohibits states from giving any postsecondary education benefits to undocumented immigrants unless citizens were also given these benefits. This federal mandate became effective in July 1998 and prevents undocumented immigrants from paying the much lower in-state tuition rates. These undocumented students are not eligible for FAFSA (Free Application for Federal Student Aid) or any other federal sources of funding for higher education. This increases their difficulties in obtaining funds to cover their costs of college. (Perez et al, 2010; Suarez- Orozco et al, 2015). In many states, these undocumented immigrants are either charged out-of-state or international tuition rates in public higher educational institutions which sometimes make it difficult for many meritorious students to afford going in for higher education. Under these difficult funding circumstances, some states have made life easier for these youth by allowing them to study in public colleges and universities at subsidised tuition rates. These tuition subsidy policies can affect their costs of attending college and potentially their education decisions.

Since 2001, twenty-two US states have provided tuition subsidies to undocumented students in public colleges (See Table 1 and Figure 1). In addition, eight states offer state

financial aid along with tuition subsidies to undocumented students. [7] However, these students are banned from tuition subsidies in states like Alabama, Arizona, Georgia, Missouri, South Carolina and Indiana. [8]

Table 1 shows the list of all states that have allowed tuition subsidies for undocumented immigrants from 2001 to date. It also shows the date on which the reform became effective in each state. The states which have adopted these policies include those with relatively high proportion of undocumented immigrants( for example, New Jersey, New York, Texas, California) as well as those with low proportion of them (for example, Michigan, Minnesota)(see figure A1).[9]

The subsidy that is offered to undocumented immigrants can be thought of as a price discrimination strategy, since these students are probably short of funds. These students can have substantial cost savings due to the reform. In 2016-17, the average tuition subsidy for full-time undergraduate students in public 4 year institutions was $16,050 while in public 2 year institutions, it was $4,512 (Digest of Education Statistics, 2017). Among the states adopting the tuition subsidy reform, the subsidy ranged from as low as $8,204 in Minnesota to as high as $24,824 in Michigan in case of public 4 year institutions. For public 2 year colleges, the subsidy was lowest at $743 in Minnesota and highest at $8339 in Connecticut.[10]

[7]The states which offer financial aid to undocumented students as of December 2017, include California, Colorado, Texas, New Mexico, Oregon, Utah, Minnesota and Washington.

[8]The general eligibility criteria for receiving these tuition subsidies are: 1)students must attend an in-state high school for a specified time period (1-3 years) 2)they must obtain a high school or equivalent degree from the state 3) they must have been accepted to a public college 4) they must sign an affidavit that they wish to file for legal immigration status.

[9]The states with the black diamond marker are the treated states in figure A1.

[10]These state level figures of tuition subsidy are for the year 2016-17. See figure A2 for state level variation in tuition subsidies in 2016-17.

# 3 Related Literature and Contribution

My study is related to a much broader literature that has used quasi-experimental evidence to estimate the elasticity of demand for college. These studies include examination of state level programs (Dynarski(2000); Cornwell, Mustard and Sridhar (2006); Kane(2003); Kane (2007); Abraham and Clark (2006)) as well as federal programs (Seftor and Turner (2002); Turner (2017); Carruthers and Welch (2015); Denning, Marx and Turner (2018); Bednar and Gicheva (2013)).[11] Similar to these studies, my analysis uses a quasi experiment to identify the elasticity of demand for college. However, different from these studies, I focus on a reform targeted to the specific group of undocumented students –- who are hard to identify in any household survey. Broadly, I find higher college attendance of the undocumented students in 'more exposed' institutions of the treated states, consistent with these studies.

The spillover effects of tuition subsidy reform on Americans I consider in this paper contribute to the existing studies that broadly look at the effects of immigrants on natives in educational outcomes in different settings (Hoxby (1998); Borjas(2004); Gould et al (2009); Bound, Turner and Walsh(2009); Machin and Murphy (2017); Shih (2017)). This literature finds mixed evidence ranging from negative effects (Borjas, 2004) ; no effects/mild negative effects (Gould et al,2009) to positive effects(Shih, 2017). My estimates suggest statistically insignificant but positive effects of the reform on enrollment of domestic students in community colleges of treated states 'more exposed' to the reform. Nonetheless, I find insignificant but mild negative effects on native students in public 4-year colleges, timed with the reform.

There are six previous analyses that have examined the effect of tuition subsidies on college enrollment of likely undocumented immigrants.Using data from 2000-2005 American Community Survey (ACS), Chin and Juhn(2011) do not find any statistically significant effects on college enrollment. This is primarily because less time had passed after the

[11]See Dynarski (2002) for other papers that use the quasi-experimental approach to estimate the elasticity of demand for college education.

adoption of the laws for undocumented immigrants to avail of them. Using data from Current Population Survey outgoing rotation groups (ORG) over the period 1997-2005, Kaushal (2008) however finds that the tuition subsidy reform is associated with a 2.5 percentage point (31%) increase in college enrollment of likely undocumented students. Flores(2010)' estimates, using CPS-ORG data for a similar time period (1998-2005), suggest that foreign-born non-citizen Latinos were 1.54 times more likely to enroll in college in the treated states. Using administrative data from five universities in Texas, Dickson and Pender(2013) find that a tuition subsidy of $1000 increases enrollment of non-citizens at non-flagship universities by 2-3 percentage points. Koohi(2017)shows a 1.2 percentage point (12% of sample mean) increase in college enrollment of Mexican non-citizens in the treated states, using ACS data (2000-2015). Amuedo-Dorantes and Sparber(2014) use monthly CPS data over the time period 1999- 2012 to find that the reform increases the college enrollment probability of Mexican non-citizen students by 4 percentage points.

The conflicting results on college enrollment in the above papers may be due to differences in datasets used, in time periods considered, in regression methods used, and in the selection criteria of undocumented immigrants who are most likely affected by the reform.

There is little causal evidence to show how tuition policies affect college retention and completion of undocumented students without using household surveys. There are two exceptions. The first is Conger and Turner (2017) who examine the effects of a price shock caused by the temporary removal of tuition subsidies in CUNY on undocumented students' retention, credits, grades and degree receipt. Note that, their paper analyses a temporary price shock in a particular state of New York, whereas I consider permanent price shocks in all states of the US which have adopted the reforms. The second is Dickson and Pender(2013) who find no significant differences between Texas citizens and non-citizens in retention rates from the first year to the second, as a result of a subsidy. Their paper only considers the state of Texas and all non-citizen students, rather than undocumented students enrolled in public universities of Texas. Their estimates may not provide a comprehensive and externally valid picture of the college completion effects of the reform.

My main contributions in the above literature are twofold. The *first* is to look at

enrollment, graduation, major choice, and tuition and institutional aid outcomes of undocumented students within institutions –- thereby taking into consideration the selection of these students to certain institutions, as predicted by the above studies with household surveys. Accounting for the positive selection effects is crucial, as I find smaller treatment effects on the enrollment of these students in the treated states. Further explorations reveal that the enrollment effects are statistically significant only in public 2 year colleges, but not in public 4 year ones. This is an important new result suggesting that –- the reform does not raise the college enrollment of new undocumented students at all public colleges uniformly. In contrast, the graduation effects within 6 years of starting college, are significant for both public 2 year and 4 year colleges. Moreover, none of the public institutions 'more exposed' to the reform charge higher tuition to the students, timed with the reform. In other words, the reform leads to a rich heterogeneity in both consumer and firm behavior across the institutions –- which would not be captured in the household surveys. These are new findings compared to the existing literature, facilitated by the *second* and more substantial contribution outlined below. It is to identify the exact total number of undocumented students in an institution using administrative records. This helps me to refine the treatment group by finding those institutions 'more exposed' to the reform i.e. those institutions having above median share of actual undocumented students in a pre-policy period. By doing so, I am able to derive new and more accurate causal estimates of the reform. This is not feasible with the household surveys in the US –- which cannot pin down the undocumented immigrants –- mainly for educational outcomes. They rely on Mexican/Hispanic non-citizens –- whose share in the undocumented population has been declining since 2000 due to deportations (Figure 3).

My paper also contributes to the large literature studying the causal effects of mandatory schooling requirements on fertility both in developed and developing countries (Appleton, 1996; Leon, 2004; Breierova and Duflo, 2004; Black, Devereux and Salvanes, 2008; Monstad, Propper and Salvanes , 2008; Osili and Long, 2008; Kim, 2010; McCrary and Royer, 2011; Lavy and Zablotsky, 2011; Chicoine, 2012; Geruso and Royer, 2018). However, my study differs from these papers in the following important way. While these

papers study policy changes relating to primary and secondary schooling for women, I focus on an educational reform that has increased the access to post-secondary education for undocumented women. This is a different reform also because it made college more affordable for those at the lower tail of the education distribution. In contrast, most of the existing papers look at non-financial incentives that increase individuals' number of years of education. Moreover,these papers focus on teenage pregnancy, whereas I explore fertility choices during the ages of 17-35 years when the undocumented women are most likely to be pursuing their undergraduate degrees. I further provide suggestive evidence of mechanisms that can explain the lower fertility rates of undocumented females in this unique setting –- delays in marriage due to longer schooling years, higher opportunity cost of raising children and better knowledge of contraceptive methods due to more educational attainment.

The only other paper which looks at the effects of tuition subsidy reform on teenage childbearing is Koohi (2017). She finds negative effects of the reform on the probability of giving birth for Mexican non-citizen teenagers. She argues that these teenagers are sufficiently forward looking, who reduce their teenage fertility rates in response to increased future economic opportunities provided by the reform. While she only focuses on Mexican non-citizen teenagers, I examine the long-run fertility effects on potentially undocumented females between 17-35 years who are more likely to be exposed to the reform. Additionally, I try to understand the marriage and living arrangement patterns that can explain the delayed childbearing of these females.

# 4 Data and Identification Strategy

## 4.1 Data

I primarily use three datasets to explore the education and fertility effects of tuition subsidies on undocumented students. First, I use annual institution-level data from IPEDS for the years 2000-2017, to analyse the impact of the reform on enrollment, graduation and

choice of major of undocumented students by various categories of institutions. IPEDS data also enables me to examine the pass-through rates of this subsidy reform to other students in the same institution. Comparing outcomes of undocumented students within institutions addresses the issue of selection into institutions –- present in the existing studies with repeated cross-sectional household surveys like American Community Survey (ACS) and Current Population Survey (CPS). Additionally, it facilitates analysis of a rich heterogeneity in outcomes –- both within and across institutions. It is well known that the above household surveys lack information on the legal status of the individuals. This prevents most of the existing studies from accurately identifying undocumented students, which may in turn result in biased estimates of treatment effects of the reform. In view of this challenge, I supplement the IPEDS data with administrative records on number of F1 and M1 visa students by institution, state of residence in US, and country of origin for the year 2003. These records are obtained from Immigration and Customs Enforcement through a Freedom of Information Act (FOIA) request. Below, I describe how the linking of these two datasets enables me to track the number of undocumented students by institution in 2003. Third, for the fertility outcomes, I use US Census 2000 (5% sample) and ACS (2001-2017) as the main data sources. The ACS contains information on marriage, fertility and living arrangement outcomes of individuals residing in US along with their demographic information on gender, age, race and year of immigration (for immigrants). The larger sample size of the ACS makes it preferable over CPS for analysing fertility and household formation outcomes. Moreover, the yearly sampling frequency of ACS is sufficient for my research question.

For the institution level outcomes, I consider 'Non-Resident Aliens' as a proxy for undocumented students. However, I refine the treatment group in two ways to capture the actual number of undocumented students in an institution as closely as possible. The treatment groups are defined in section 4.2. My preferred treatment group is 'Non-resident aliens' in treated state public institutions with a higher exposure to the reform –- defined as institutions with above median number of undocumented students in the pre-treatment year 2003. The idea is that, timed with the reform, more undocumented students –- among

the larger group of 'Non-Resident Aliens' –- will attend those schools having higher pre-reform share of them.

As per the IPEDS, 'Non-Resident Alien' definition excludes resident aliens and eligible non-citizens who have entered US as legal immigrants for the purpose of obtaining permanent resident alien status and who have either of the following documents-Alien registration card (Form I-551 or I-151), Temporary resident card (Form I-688)or Arrival-departure record (Form I-94) indicating their legal immigration status. There are two salient observations on this definition which need to be discussed. First, this measure includes all nationalities of undocumented immigrants, thus leading to a better identification of undocumented status than Mexican/Hispanic non-citizens which is currently used in the literature. Note that, the proportion of Mexicans among undocumented immigrants has been declining since 2000 due to more deportations (Figure 3). Second, 'Non-resident aliens' in IPEDS data covers both undocumented students as well as students on F1 and M1 visas. I link the SEVIS records on the total number of F1 and M1 visa students by institution with the IPEDS data on total number of 'Non-resident aliens' for the year 2003.[12] The linkage is done based on a fuzzy matching method applied to the institution names. This method resulted in $> 93\%$ successful matches. I then compute the number of undocumented students by school –- by subtracting the number of F1 and M1 students from the total number of 'Non-Resident Aliens'.[13] I am constrained to limit the pre-reform year to 2003, rather than 2000 or earlier –- because SEVIS began mandatory use in 2003 as per the guidelines of Department of Homeland Security and the Department of State.

SEVIS data offers several advantages over other administrative data on international students. The information collected by SEVIS is compulsory during the visa application procedure of the student, and during the federally mandated certification process for schools

[12]Total number of 'NRAs' includes undergraduate, graduate, Phd and first professional students. Information on research Phd students is from Survey of Earned Doctorates.

[13]The fact that NRAs are always larger in number than F1 and M1 visa students from SEVIS, serves as a check that IPEDS data incorporates both documented and undocumented students. It is hard to procure data on only F1 and M1 undergraduate students from the SEVIS –- and so I am unable to compute exact number of undergraduate undocumented students by school, and use it as a dependent variable.

who want to admit international students. When an international student gets admitted to a US school, he fills out an I-20 visa form and takes it to a US consulate overseas where the information is entered into SEVIS, and a visa is issued. The institution confirms the student's enrollment in classes, along with demographics and other information –- upon their entry to the US. The institution regularly updates SEVIS about any changes in the student's enrollment status and program of study, until the student's departure from the US. Students who are found to violate the visa requirements are reported to Immigration and Naturalisation Services (INS) through SEVIS. To minimize errors in the information entered into the SEVIS –- due to either misreporting by individuals or application failures –- a number of validations are conducted. Both institutions and immigration officials carry out systematic reviews of a student's information. INS audits the institutions for compliance with the reporting requirements every two years. Institutions can lose the ability to admit international students if they do not comply with the federal regulations. These monitoring activities ensure significantly high data accuracy of the SEVIS records.

For fertility related outcomes, following Amuedo-Dorantes and Sparber (2014), I use Mexican/Hispanic non-citizens as a proxy for likely undocumented immigrants. I further consider only Mexican/Hispanic non-citizens who arrived to the US after 1981 and by age 14 since they are most likely to be affected by the tuition subsidy reform.[14]

I combine institution level data from the IPEDS and micro data from the ACS with state level data on the enactment dates of the tuition subsidy reform for undocumented students. This state level data has been obtained from National Conference of State Legislatures (NCSL) and ULead Network. I assemble information on state level immigration enforcement policies from a number of sources. Data on 287(g) agreements is from Amuedo-Dorantes and Bansak(2014), Kostadini et al(2013). Data on omnibus immigration laws and E-verify mandates is collected from National Conference of State Legislatures (NCSL). Unemployment data is obtained from Bureau of Labor Statistics while income

[14]Note that Dorantes and Sparber (2014) do not take into account these criteria to determine the group most likely affected by the reform. In fact, I find that Mexican non-citizens who arrived to US after 14 years can be used as a placebo to study the reform effects.

and poverty data come from Bureau of Economic Analysis. State level median house prices are from Zillow. I have taken the voting data from the Office of the Clerk, US House of Representatives. [15]

In Panel A of table 2, I show the weighted summary statistics of some education and reform variables for three disjoint groups- Likely undocumented immigrants (Mexican non-citizens), Foreign born citizens and US natives. Panel B gives a sense of how tuition subsidies to undocumented students affect the long run outcomes of various citizenship groups through summary statistics on educational attainment, employment rates and family income of 30-45 year age-groups in 2017. Panel A reveals that there is large difference in college enrollment rates between likely undocumented immigrants and the other two groups (19% as compared to 52% for foreign born citizens and 42% for natives). Thus it is worth exploring whether these inequalities in college education can be reduced if the state intervenes by providing tuition subsidies to undocumented students. Panel B indicates that the educational attainment of likely undocumented immigrants is lowest as compared to the other two groups. Their low educational attainment is also reflected in their low employment rates in the second row of panel B. These undocumented immigrants also belong to families with low average incomes. Thus, they are financially constrained to bear the costs of higher education. It remains an empirical question whether these tuition subsidies have actually benefited them by improving their access to higher education –- taking into account the role of sorting to favorable schools.

In table 3, I show the weighted summary statistics of some fertility and living arrangement variables for different citizenship groups. Likely undocumented students in my sample have the lowest probability of remaining single and the highest probability of staying with their parents relative to foreign citizens and natives. Moreover, they are twice likely to have at least one child under age 5 and to give birth to a child as compared to documented immigrants and natives. These high fertility rates among undocumented females are a cause of concern and motivates my analysis on the role of education reform in bridging the

[15] see http://history.house.gov/Institution/Election-Statistics/Election-Statistics/.

fertility gap between undocumented and documented immigrants.

## 4.2 Identification Strategy

The goal of the analysis is to estimate the causal effects of tuition subsidy reform for undocumented students on their education, fertility and household formation outcomes. Treatment and control states are defined according to when the reform was implemented in the state of current residence or education of an individual. My identification strategy relies on the state time variation in adoption of the reform. A second source of variation comes from the differential effects of the reform, induced by differences in the initial population share of undocumented immigrants. The share of undocumented immigrants I consider in the paper –- uses the actual number of undocumented immigrants identified from administrative records –- either by state, or a more granular institution level.

For college enrollment, graduation, major choice specifications –- non-resident aliens in public colleges of treated states more exposed to the reform (i.e. states with a higher share of 1990 undocumented immigrants), form the treatment group. In complementary specifications, I also consider non-resident aliens in treated state public institutions which are more exposed to the reform (i.e. institutions with above median share of 2003 undocumented students), as the treatment group. For fertility and household formation outcomes, Mexican non-citizen high school graduates who are 17-28 years old, have arrived to US within 14 years of age and reside in treated states constitute the treated group. [16]In case of education outcomes, the control group consists of similar individuals in treated states/treated state institutions less exposed to the reform. Alternatively, I use similar people residing in states without the reform as the control group –- for fertility related

[16]This is my preferred definition of the treatment group, in line with the existing literature. However, the results are qualitatively similar if I use the Hispanic non-citizen group, or expand the age range of the individuals to 17-35 years. While the upper bound of this age range has been chosen arbitrarily, it is a reasonable one –- motivated by the fact that undocumented students in community colleges are likely to be older than in other public colleges.

outcomes.

For the institution level outcomes, I estimate the following difference-in-differences specification:

$$Y_{ist} = \alpha + \beta D_{st} + \gamma D_{st} * U_{os} + \theta X_{st} + \psi_i + \delta_s + \phi_t + \epsilon_{ist} \quad (1)$$

Here, i denotes institution, s state and t year. $D_{st}$ is a binary variable that takes the value 1 if state s offers in-state tuition rates to undocumented students at time t and 0 otherwise.[17] $U_{os}$ denotes the share of undocumented immigrants in state s in the baseline period 1990. This share is calculated as $\frac{Undocumented_{s,1990}}{Pop_{s,1990}}$. Following Hines and Peri(2019), I include only the working age population (16-64 years) in the 'population' variable. [18] I obtain state-level records on the exact number of undocumented immigrants from the Department of Homeland Security. [19] $X_{st}$ consists of baseline demographic and economic covariates like proportion of female, polynomials of average age, proportion of asian, black and other races, unemployment rate, per capita personal income, proportion of votes for the republicans. Y is the outcome of interest. Equation 1 also includes institution fixed effects $\psi_i$, and year fixed effects $\phi_t$. I also control for whether a state offers financial aid and drivers' licenses to undocumented immigrants through two separate binary variables. [20] With treatment varying at the state level, I cluster standard errors by state, to allow for correlations in the error terms of institutions in each state. For the main outcomes, I also report standard errors based on a clustered (by state) wild bootstrap-t procedure with 1000

[17] I consider D to be 1 from the date the law became effective in each state. I also drop observations of New York from 1999 through 2001 because the State University of New York (SUNY) and City University of New York (CUNY) had different policies on tuition rates for undocumented immigrants during this period.

[18] I define population in hundreds, so that the variable can be interpreted as percent.

[19] It is possible that the share variable is correlated with covariates included in $X_{st}$. However, to reduce such concerns, I have included the share variable for the year 1990, a decade before the start of my analysis period.

[20] see https://www2.law.temple.edu/csj/files/fdl.pdf, which suggests that 85% of undocumented immigrant respondents in Pennsylvania had to give up educational opportunities, a better school or scholarship due to lack of driver's license.

replications.The coefficient of interest is $\gamma$ which measures the causal effect of the reform on outcomes Y in treated states with higher exposure to the reform. Thus, in my setting, the reform dummy interacted with the pre-treatment share of undocumented immigrants in a state captures the intensity of exposure of the treated state to the reform, and not just the presence or absence of the reform in the state. The institution level regressions are weighted by how much of the total student population the institution represents, at the baseline.

I also estimate another variant of specification 1 –- to precisely identify the institutions with higher exposure to the reform. In this specific case, I restrict the time period of my analysis to 2004-2017, dropping the states which have rolled-out the reform prior to 2004. This restriction ensures that I can accurately measure the exact number of undocumented students by institution in a pre-treatment period 2003 –- the first year SEVIS was introduced to track the F1,M1, and J1 visa students (the documented international students) in the US educational system. As mentioned in the data section, I obtain the number of undocumented students in each institution by deducting the number of documented students from the total number of non-resident aliens, reported in the IPEDS data. To this end, I interact the reform dummy with a dummy for institutions having above median number of undocumented students in 2003. [21] I report the estimates of $\gamma$, which compares Y among treated state institutions with a higher exposure to the reform, relative to institutions with a lower exposure, before and after the reform.

For the individual outcomes using ACS data, my specification is:

$$Y_{ist} = \alpha + \beta D_{st} + \gamma X_{ist} + \theta Z_{st} + \delta_s + \phi_t + \epsilon_{ist} \tag{2}$$

Observations refer to individual i living in state s at time t. Y is the outcome of interest. X includes individual level characteristics such as age, indicator variables for gender and race and a continuous variable measuring the number of years a person has stayed in United

[21] Results are similar if I instead interact the reform dummy with a dummy for institutions having above median *share* of undocumented students in 2003.

States in case of immigrants. Z includes state level covariates like unemployment rates, per capita personal income/median household income, proportion of people below the poverty line, median house price, proportion of votes for the republicans.[22] As mentioned in the data section, I also control for state-level immigration enforcement policies in equation 2. $\delta_s$ denotes state fixed effects, and $\phi_t$ indicates year fixed effects. The parameter of interest here is $\beta$, measuring the causal impact of the tuition subsidy reform to undocumented youth on their fertility, marriage and living arrangement outcomes.

An obvious concern with using difference-in-differences model presented in equations 1 and 2 is the potential for different trend in outcomes between treatment and control groups before the reform change.If this is the case, then we will have biased estimates of $\gamma$ in equation 1, and $\beta$ in equation 2. Below, I will elaborate and focus on the assumptions needed to pin down the average treatment effects (ATE) of the reform in equation 1 –- because it is not a straightforward difference-in-differences case with a binary treatment.

Before discussing the assumptions, I will introduce some notation. Suppose there are $\mathcal{T}$ time periods (t=1,2,..$\mathcal{T}$), with variation in the time when units first receive their treatment(G=g $\in \mathcal{G}$). Let $G = \mathcal{T}+1$ for the never-treated units –- i.e. the 28 states which have either banned the reform or never implemented the reform. There are no units treated in the first period 2000, so $\mathcal{G} \subseteq \{2, 3, ....\mathcal{T}+1\}$. Treated units receive the dose D=d $\in \mathcal{D}$, where the treatment space $\mathcal{D}$ =$\{0\} \cup \mathcal{D}_+$. Let potential outcomes be denoted as $Y_{it}(g, d)$, indexed by treatment timing and dose of treatment. In this setting, the dose is the pre-treatment share of undocumented immigrants in a state/institution. I further assume that once treated units receive this dose, they continue to receive the same dose in all subsequent periods –- which largely holds true in my case. This allows me to classify a treated unit by their timing of treatment adoption and the amount of dose received. Let $Y_{it}(0) = Y_{it}(\mathcal{T}+1, 0)$ represent unit i's untreated potential outcome with d=0.Additionally, let $W_{it} = D_i \mathbb{I}(t \geq G_i)$, be the amount of dose experienced by unit i in time period t –- with $W_{it} = 0$ for units that are not yet treated by t.

[22]The controls included in Z are baseline characteristics interacted with the year fixed effects, to avoid being endogenous to the reform.

For $\gamma$ to identify the average treatment effect and average causal response of dose d, for group g, at time period t, I invoke the following two assumptions:

$$For\ \ all\ \ g \in \mathcal{G}, t = g(1)\mathcal{T}, d \in \mathcal{D}, E(\Delta Y_t(0)|G = g, D = d) = E(\Delta Y_t(0)|G = k)$$
$$for\ all\ groups\ k \in \mathcal{G}\ \ such\ that\ \ t < k\ (i.e.\ pre{-}treatment\ periods\ for\ group\ k)\ \ {-}{-}PT \tag{3}$$

$$For\ \ all\ \ g \in \mathcal{G}, t = g(1)\mathcal{T}, d \in \mathcal{D}, E(Y_t(g,d){-}Y_{t-1}(0)|G = g, D = d) = E(Y_t(g,d){-}Y_{t-1}(0)|G = g)$$
$$and\ \ (E(\Delta Y_t(0)|G = g, D = d) = E(\Delta Y_t(0)|G = k)\ for\ all\ groups\ k \in \mathcal{G}\ \ such\ that\ \ t < k$$
$$(i.e.\ pre - treatment\ periods\ for\ group\ k)\ \ {-}{-}SPT \tag{4}$$

Equation 3 is the standard parallel trends assumption in a difference-in-differences setup with continuous treatment. It says that the path of untreated potential outcomes for group g in post-treatment periods is the same as the path of outcomes among all groups that are not yet treated in that period –- this includes both the never treated groups and the not-yet-treated groups. Though this assumption is weaker than saying that the paths of untreated potential outcomes are same across all groups, doses and time periods –- it is a more plausible one in my context. This is because less exposed and more exposed institutions in states which have banned the reform or have never adopted the reform can differ in unobservables in some pre-treatment periods –- not accounted for by the fixed effects, controls, state-time trends and other policies included in the model.[23] Equation 4 is a stronger version of the parallel trends assumption. It is different from equation 3, because it involves potential outcomes under different dosage amounts 'd' rather than only untreated potential outcomes. The usefulness of this assumption arises from the fact that it allows for some selection into a particular dose, but mandates that on average across all doses, there is no selection into a particular dose. Thus, while undocumented students may select into a particular public college with a higher pre-determined share of them –-

[23]Note that, there are more of such pure control states, than the treated ones in my sample.

on average, there is no selection into more exposed public colleges. If Assumptions 3 and 4 hold true, I can identify the following treatment effects:

$$ATE(g,t,d) = E(Y_t - Y_{g-1}|G = g, D = d) - E(Y_t - Y_{g-1}|W_t = 0)$$

$$ACR(g,t,d) = \frac{\partial E[Y_t - Y_{g-1}|G = g, D = d]}{\partial d}$$

Equations 1 and 2 are estimated under the assumption that conditional on covariates X and Z, the reform was exogenous in each treatment state. To provide visual evidence for that, I test for the existence of confounding pre-trends through event study analyses as specified in the below two equations. The first specification is for institution (i) in state (s) at time (t):

$$Y_{ist} = \alpha + \sum_{n=-5,n\neq-1}^{5} \beta_n D_{st}^n + \sum_{n=-5,n\neq-1}^{5} \gamma_n D_{st}^n * U_{os} + \theta X_{st} + \psi_i + \delta_s + \phi_t + \epsilon_{ist} \quad (5)$$

The second specification is for resident (i) of state (s) at time (t):

$$Y_{ist} = \alpha + \sum_{n=-5,n\neq-1}^{5} \beta_n D_{st}^n + \gamma X_{ist} + \theta Z_{st} + \delta_s + \phi_t + \epsilon_{ist} \quad (6)$$

Here, n can be considered as the event time i.e. the number of years before or after the reform was implemented with n=0 representing the year the reform was implemented. I omit the indicator for $n = -1$. Thus $n = -1$ is the reference year, with each $\beta$ coefficient measured relative to the period before implementation. [24]The coefficients $\gamma_{-5}$ to $\gamma_{-2}$ / $\beta_{-5}$ to $\beta_{-2}$ in equations 5 and 6 respectively, can help to determine whether the trends in various outcomes are significantly different between the treatment and control groups. The coefficients ($\gamma_1$ to $\gamma_5$/ $\beta_1$ to $\beta_5$) are included to see whether the effects of the reform persist over time.

Note that, these pre-trend tests designed to detect violations of parallel trends cannot differentiate between equations 3 and 4. To rule out possible selection into the treatment

[24]I bin up the end points (-5 and 5) which are years outside my event window.

groups and shut down alternative explanations that favor improved enrollment of undocumented students, I carry out certain falsification exercises described in section X. I also test the sensitivity of my results by varying the choice of the comparison group.

Finally, I implement the doubly robust estimator of Callaway and Sant'Anna(2021) –- which is robust to treatment effect heterogeneity across states and to concerns about negative weights and inappropriate comparisons which may bias the estimates in a conventional two-way fixed effects framework (Callaway and Sant'Anna, 2021; Goodman-Bacon 2021; Sun and Abraham 2021; de Chaisemartin and D'Haultfoeuille, 2020). Any difference-in-differences research design must impute counterfactual trends in the outcome variable for the treated units. The doubly robust estimator of Callaway and Sant'Anna(2021) constructs these counterfactual trends in two steps. First, to identify the comparison group of states that resemble the treated group, it weights observations by the inverse of the cohort 'g' specific propensity score $p_g(X)$. Second, it imputes a counterfactual trend in the dependent variable $Y_t$ between time t and some base period $b = g - 1$, by regressing $\Delta Y_{t,b} = Y_t - Y_b$ on the same vector of covariates X in a sample comprising the chosen control group, and then predicting the changes for the treated cohort. The estimator of ATT(g,t) –- the group of states first treated at time period 'g' in year 't' is given by:

$$\widehat{\beta_{g,t}} = \frac{1}{n_{g,t}} \sum \left( \frac{G_g}{\frac{1}{n_g}\sum G_g} - \frac{\frac{\widehat{p_g(X)}C_g}{1-\widehat{p_g(X)}}}{\frac{1}{n_g}\sum \frac{\widehat{p_g(X)}C_g}{1-\widehat{p_g(X)}}} \right) (\Delta Y_{t,b} - \widehat{\Delta Y_{t,b}(X)}) \tag{7}$$

I aggregate $\widehat{\beta_{g,t}}$ across all cohorts/groups and time periods, and report overall difference-in-differences estimates of the effect of the reform on education and fertility outcomes of undocumented immigrants –- using never treated states as the comparison group.

# 5 Results

## 5.1 College enrollment

This section discusses the relationship between tuition subsidy reform and college enrollment rates of undocumented students as well as Americans.

First, I present results of the event study analysis as specified in equation 5. Figures 4 and 5 show the effects of the reform on enrollment of first year NRAs in public 2 year and 4 year colleges respectively, of treated states with higher exposure to the reform –- for 5 years before and after the reform. These figures plot the estimated coefficients of $\gamma$ from equation 5 along with their 95% confidence intervals. I observe all of the coefficients prior to the reform to be precise null and insignificant. There seems to be no confounding pre-trends, thereby supporting my identification strategy. Enrollment starts to rise for NRAs in 2 year colleges immediately after the reform was adopted. This effect seems to persist over time. For 4 year colleges, the post-treatment effects continue to remain flat. This lends support to my belief that the reform was successful in raising college enrollment rates among undocumented students –- primarily in community colleges.

I next show the results from estimation of equation 1 in Table 4. Each cell in Table 4 shows the estimates of $\gamma$ from a separate regression. In Column 1, I report the results from a baseline regression with only institution and year fixed effects and other policies. Columns 2 and 3 add controls and state time trends respectively, to the baseline model.

My preferred specification suggests that the reform increases the NRA share of first-year enrollment by 0.026 percentage points in public 2-year colleges, for every 1% of the population that is undocumented in the treated states (Column 2). This estimate is significant at the 1% level. The estimates in Table 5 help us to better understand the magnitude of the affected students. The reform increases the number of NRAs enrolling in treated 2-year institutions with above median number of pre-determined undocumented students by 24% (baseline mean: 42.9). This translates to an average growth of 10 undocumented students enrolling per 2-year institution, among institutions more exposed to the reform. In case of

public 4-year colleges, the reform causes the NRA share to rise by 0.009 percentage points, for every 1% of the population that is undocumented in the treated states. I observe an average increase of 6 undocumented students enrolling per 4-year college, among more exposed institutions (Table 5). Though these effects on 4-year institutions are statistically insignificant, they are non-trivial in terms of magnitude –- as I can rule out increases of more than 15 undocumented students per institution. These results are highly robust even when state time trends are controlled for.

I then probe further into the results of 4 year colleges –- exploring the heterogeneous enrollment effects of undocumented students by selectivity of the public 4-year institutions. The selectivity measure used is Barron's ratings for an institution. The ratings are updated every ten years. 'Barron's College Guide' classifies certain four-year public institutions into six categories based on acceptance rates, college entrance exam scores, and the minimum class rank and grade point average required for admission. I use the ratings of an institution in the most recent year before the reform was adopted. I classify more competitive institutions as those having a rating of 1,2, or 3 –- whereas the less competitive ones have a rating of 4,5, or 6.[25] Table 6 shows the corresponding estimates. While the enrollment effects are precisely small and positive in the less competitive colleges, we can't say so for the more competitive ones. [26] It seems that the increased enrollment of undocumented students in public 4-year colleges can be attributed to the more competitive institutions.

In addition to the enrollment effects by selectivity of institutions, I examine another dimension of heterogeneity in enrollment –- the Carnegie classification of institutions. In Table 7, I find increased enrollment of undocumented students in the following types of Associate's colleges- High Transfer, Mixed Transfer/Vocational & Technical, High Vocational & Technical in treated states which are more exposed to the reform. I observe no significant rise in enrollment in Baccalaureate Arts & Science colleges. However, for 4

[25]For a sample list of institutions in each of these categories, see Appendix X.

[26]In fact, these estimates are imprecise. We can rule out increases of more than 0.067 percentage points in the NRA share of first year enrollment, if the undocumented share in the population is 1% in the treated states.

year colleges specialising in health professions, business & management and arts, music & design in panels E, F, G –- no definite conclusions can be drawn because of the small sample sizes. Overall, it seems that undocumented students are more likely to attend those colleges which provide them hands-on training in specific skills and prepare them better for the labor market.

It is useful to compare my findings with those of previous studies of the reform using household surveys. While Dorantes and Sparber (2014) and Kaushal(2008) estimate enrollment effects in the range of 3.7-4.2 percentage points, and 2.5 percentage points respectively, the comparable estimates in my specifications are much smaller. They are in the range of 0.001 percentage points-0.003 percentage points, depending on the type of public college and the specification used (see Table A1). Even my main preferred specification estimates from colleges with a higher exposure to the reform, reveal smaller enrollment effects of undocumented students. This is consistent with positive selection into institutions present in the studies of existing literature. When I attempt to address those sorting issues and also refine the treatment group to minimise the measurement errors –- the enrollment effects decline, as expected.

The results in Table 4 indicate that the reform does not have any detrimental effect on natives which was a concern raised by the opponents of this reform. This is in line with Kaushal (2008)'s and Dorantes and Sparber (2014)'s finding. In fact, for public community colleges, the native share of first year enrollment rises by a statistically insignificant 0.04 percentage points, if the share of undocumented immigrants in treated states is 1%. This can be partially explained by the fact that existing capacity is idle in the short run, combined with a lower in-state and out-of-state tuition in those particular institutions with a higher share of pre-policy undocumented students in them –- which limits possibilities of undocumented students replacing Americans both in the short and long term.[27]

To address the possibility that the difference-in-difference estimates in equation 1 may be upward biased because of pre-existing trends correlated with the tuition subsidy reform,

[27]See Table 11 for the results on tuition rates.

I also carry out triple difference regressions for the enrollment outcomes using private colleges as additional control group. In other words, I compare outcomes Y between 'higher exposure' treated states and 'lower exposure' treated states, before and after the reform, in public colleges relative to private colleges. I include a full set of two way fixed effects- state by public fixed effects, public by year fixed effects, and state by year fixed effects. These fixed effects are likely to absorb most of the pre-existing trends and unobserved factors that may bias the difference-in-difference estimates.Though I do not find evidence of significant spillovers of the treatment on private colleges –- thereby validating it's use as a control group (see Panel A of Table 4), it is likely that undocumented students may reallocate from expensive private colleges to cheaper public colleges due to some other policies. In that scenario, the triple difference estimates would be overestimating the actual treatment effect of the reform on undocumented students. Panel A of Table A7 shows the results, with the above caveat in mind.

Together, these results imply that lower tuition rates given to undocumented students have achieved the goal of meaningfully increasing their enrollment rates within colleges, while imposing minimum costs on other groups of the society.

## 5.2 Choice of major

It is well known that an individual's choice of major during his postsecondary education affects his labor market prospects and creates gender differences in labor market outcomes. [28] A rational individual will choose a major based on his expectations of labor market returns, non-wage considerations like enjoyability of the course, role of family, peers, professors, and role models in influencing their decisions –- among other factors. Undocumented immigrants have more uncertain employment prospects even with DACA –- compared to documented immigrants and natives. Hence, their choices of major may be different from other demographic groups. In light of this, it becomes pertinent to study whether the

[28] See Patnaik, Wiswall and Zafar (2020) for an excellent review on the determinants of college major choice.

reform also affects the field choice of undergraduate undocumented students.

Table 8 shows the coefficients of $\gamma$ from equation 1. The broad fields I consider are –- Arts and Humanities, Business, Health and Medicine, Multi/Inter-disciplinary Studies, Public and Social Services, Science, Math, Engineering and Technology, Social Sciences, Trades and Personal Services.[29] The NRA share does not exhibit changes in enrollment by major in treated states with higher exposure to the reform. These estimates are small in magnitude and precise. [30] One possible reason for this null result is that the reform does not contain any major specific pricing policies to meaningfully alter the preferences and beliefs of undocumented students to sort into certain majors. [31]Alternatively, institutions can have capacity constraints operating through student-faculty ratios, or constraints on the infrastructure which can limit the type of courses they offer to the undocumented students. While it is hard to pin down the exact reasons, it is intuitive that the above two factors partially contribute to explaining the small effects.

## 5.3 College retention and graduation

In this section, I first discuss the relationship between tuition subsidy reforms and college graduation rates of undocumented students. These graduation rates include both Associate's and Bachelor's degrees, obtained within 6 years of enrollment.

The results of the event study analysis for 2-year and 4-year graduation rates of undocumented students are presented in figures 6 and 7 respectively. All of the coefficients before

[29]I have grouped the 2 digit program codes from IPEDS into broad categories of majors using College Board classification. See Appendix B. Note that, the field of degree from IPEDS as per the Classification of Instructional Programs (CIP) codes for 1990, 2000, and 2010 has been made consistent over the years using crosswalks obtained from the NCES website. I have considered only primary majors in Associate's and Bachelor's degrees in public institutions.

[30]These null effects on major choice rule out undocumented students' selection into majors. Hence it increases our confidence in disentangling the causal effects of the reform on their enrollment decisions –- without using individual records and comparing their enrollment within the same major and year.

[31]See, for example, Stange(2015), Evans(2017), Andrews and Stange(2019) for analysis of major specific costs on college major choice.

the reform are close to zero and precise insignificant. Thus my identification strategy is supported with little evidence of confounding pre-trends in their graduation rates. Moreover, the graduation rates begin to steadily increase after the reform's implementation. Thus, it appears that the reform not only raises the college enrollment rates of undocumented students as a first order effect but also increases their completion outcomes. The reform effectively reduces the probability of permanent dropout of these students from college. It is possible that these immigrant youth desire to obtain more education so that they are able to secure better paying jobs after their degree completion as DACA gave them legal work permits during the time period of my study. Note that for number of college years completed, the event study (figure 8) shows the absence of pre-trends , also supporting my identification strategy. The point estimates and confidence intervals do suggest small increases in the time-to-degree after the reform, even though the coefficients are statistically insignificant.

The effects of the reform on retention and completion outcomes of undocumented students is theoretically ambiguous. There may be self selection of higher ability students into colleges after the reform which may increase their retention rates. On the other hand, there may be dropouts within the first 2 years of college when these students learn about their academic performance or their own abilities (Stinebrickner and Stinebrickner, 2014) which may reduce their retention rates –- even when their financial hardships are taken care of.

In table 9, I report the results from estimating equation 1. The reform increases the NRA share of total graduates by 0.013 percentage points in public community colleges, for every 1% of the population that is undocumented in the treated states. These results hold up when I add controls or state-time trends to the model. In fact, there is a 10% (baseline mean: 44.6) increase in the number of NRAs graduating from treated state institutions with above median number of pre-determined undocumented students. That is, the reform leads to around 5 additional undocumented students graduating per 2-year institution, among institutions more exposed to the reform. For 4-year colleges, the NRA share of total graduates rises by 0.017 percentage points, for every 1% of the undocumented

share in the treated states. This effect is significant at the 5% level. To estimate the corresponding size of the affected group –- I observe around 2-3 additional undocumented students graduating per 4-year institution, among institutions more exposed to the reform (Table 10).

A natural question that may arise here is why we observe significant increases in graduation rates of undocumented students in 4 year colleges due to the reform, despite no significant increases in their first-year enrollment. One possible explanation is that we can't really say there are no enrollment effects in 4 year colleges in terms of magnitude –- the estimates are just noisy. Recall that, we find an average increase of 6 undocumented students enrolling, and increase of 2-3 undocumented students graduating per 4-year institution, among the institutions with 'higher exposure' to the reform. An alternate explanation is that, even if there are no substantial increases in new enrollment of these students in 4-year colleges, the increased graduation rates may reflect those of the existing cohorts i.e. those who were in their 2nd year and above, when the reform was rolled out in their institutions. Moreover, the increased graduation rates in 4-year colleges may also be driven by students who enroll in 2-year colleges, but later transfer to the 4-year ones.[32] The last explanation is a less plausible one in my setting, given that the transfer agreement policies are less likely to be timed with my reform, and coincide with the same treated set of institutions I consider.

I show the triple difference-in-difference estimates of the reform on graduation of undocumented students in Table A7. While I do not find significant effects on graduation of NRAs in private colleges of treated states more exposed to the reform –- the estimates in panel B of table A7 indicate positive and marginally significant effects on their graduation rates in all public colleges.

I next turn to the effect of tuition subsidies on number of years of college completed (Table A8). [33] In Column 1, I find that the reform leads to a significant 6.5% increase in

[32]My enrollment data captures only first time enrollment and not transfer-ins of students.

[33]This analysis uses data from CPS on number of years of college credit earned by those with at least some college education but less than a Bachelor's degree.

number of college years completed for likely undocumented students. This effect is robust to alternate specifications in columns 2-4. This evidence is consistent with improved retention outcomes of undocumented students and further suggests the role of the reform in lowering their drop-out rates.

## 5.4 Tuition and fees of public colleges

A possible concern with the tuition subsidy reform is that all students may have to pay higher fees as a result of this subsidy provided by the government. [34] However, an institution's response to the reform is expected to vary by institutional objectives across the sectors, and the market power of the institution. To understand the extent of pass through of subsidies to other students in those particular institutions with higher exposure to the reform, I use nationally representative annual data from Integrated Postsecondary Education Data System (IPEDS) spanning the years 2000-2017. This dataset contains comprehensive information on in-state and out-of-state tuition for all degree granting public 2 year and 4 year institutions in US having full time undergraduate students. Table A9 reports summary statistics of this dataset.

Table 11 shows the effects on sticker price tuition charged in public undergraduate institutions which are highly exposed to the reform. This analysis is restricted to the time period 2004-2017 because of data constraints on the actual number of undocumented students by institution, as mentioned in section 4.1. The dependent variable in each of the regressions is logarithm of tuition and required fees and it is measured in 2016 dollars. Separate regressions are run for public 2 year and 4 year institutions and for instate and out-of-state tuition. In Column 1, I include only institution and year fixed effects. Column 2 adds baseline controls as mentioned in the section on identification strategy while column 3 adds state time trends that will absorb any systematic time varying factors affecting the

[34] This concern arises from the famous Bennett hypothesis. This hypothesis argues that when government offers more student aid, it enables colleges and universities to increase tuition, negating the purpose of those government benefits. See https://www.brookings.edu/research/the-disinvestment-hypothesis-dont-blame-state-budget-cuts-for-rising-tuition-at-public-universities/.

treated states. Standard errors are clustered by state.

The reform leads to 6% decrease (baseline mean: $7494) in out-of-state tuition in community colleges with above median number of pre-policy undocumented students.[35]This is roughly equivalent to an average annual decrease of $420 in these 2-year colleges. Even for in-state tuition in 2-year colleges, I find an average decrease of $937 after the reform in 'more exposed' institutions –- though this effect is statistically insignificant. There are statistically insignificant but economically meaningful decreases in both instate and out-of-state tuition in 4-year colleges after the reform –- $158 and $611 respectively. The estimates of flagship universities do not provide evidence of increases in tuition rates due to the reform. So, students on average do not experience rises in their tuition in 'more exposed' institutions of the treated states.[36]This seems intuitive, because in most states, public colleges do not have the ability to raise their tuition without approval from the governing body. These results are in stark contrast to the ones obtained in table A10 –- which suggest significant increases in in-state tuition of all public colleges after the reform. These increases may be confounded by other political and institution-level factors uncorrelated with the reform. Thus, I believe that identifying the exact number of undocumented students by institution helps to accurately estimate the tuition effects of the reform –- by precisely classifying the institutions heavily exposed to the reform. Moreover, I can also infer that non-targeted groups seem to benefit from this reform in colleges where it has more 'bite'.

The results in table 11 are also somewhat different from what Dorantes and Sparber(2014) find, in a different setting and using different data. Their results reveal strong increases in instate and out-of-state tuition of flagship schools in the treated states, as well as increases in in-state tuition of community colleges in those states.[37] There seems to be

[35]The baseline mean in 2 year colleges is different from that in Table 10 given the different time period and coefficient of interest in this analysis.

[36]I find a small decrease in institutional aid in 'more exposed' institutions of the treated states after the reform. Results available on request.

[37]My confidence intervals rule out their estimates of in-state tuition in community colleges, when state time trends are included in the model.

some negative spillover effects of the reform in their setting, which is different from my conclusion.

## 5.5 Fertility

The economic payoff related to college going can lead to delayed fertility. The disadvantaged group of undocumented students who do not attend college have poor labor market prospects and lower opportunity costs of early motherhood. Hence, they generally have high fertility rates. The college attendance of such minority groups is expected to reduce their fertility rates more than the college attendance of those with higher likelihood of being in college. However, it is possible that uncertainty about future work prospects or arrest and deportation fears can cause these undocumented students to delay their childbirth decisions. On the other hand, undocumented females may have higher fertility while being exposed to the tuition subsidy reform, if they think those children will give them citizenship rights. Because of this theoretical ambiguity, this section examines the causal relationship between tuition subsidy reforms and fertility choices of likely undocumented females and the possible mechanisms explaining their lower fertility rates.

First, I present results of the event study analysis as specified in equation 6. Figure 9 shows the effects of tuition subsidy reform on the probability of having at least one child under age 5 in the household,for 5 years before and after the reform. I observe pretty flat trends prior to the reform, thereby supporting my identification strategy. The probability of having children starts declining for likely undocumented females after adoption of the reform. The effect is most pronounced in the first two years after the reform. Thus, the reform played a role in reducing fertility among the target group.

I next show the results from estimation of equation 2 in Table 12. As immigration enforcement measures during the time period of my study can affect the fertility of undocumented females, I control for them in each of the specifications on fertility, marriage and living arrangement outcomes. In particular, I control for state level 287 (g) agreements, omnibus immigration laws and E-verify mandates. In panel A, I find that likely undocu-

mented females are 1.8-1.9 percentage points less probable to have a child under age 5 in their household in the treated states. In other words, the reform can reduce this group's fertility rates by 4.6%-4.8%. Panel B, similarly, suggests that likely undocumented females are 2.2-2.6 percentage points (15.4%-18%) less probable to give birth to a child in the last year in treated states. [38]

Back-of-the envelope calculations suggest that for every 100 likely undocumented females, there are 4 fewer children born in the treated states. To put these estimates in the context of the current literature –- they are consistent with the estimates of Osili and Long(2008) who find 3-5 fewer births in Nigeria per 100 females for a 1 year increase in female schooling. My estimates are also in line with Monstad, Propper and Salvanes (2008) who use a compulsory schooling reform in Norway to provide evidence of increased education delaying first births of women to their 20s and late 30s.

After establishing the role of the reform in reducing fertility of the undocumented females, I explore whether this decline is marital or non-marital. Table 13 and Table A11 suggest that this lower fertility is partly explained by individuals delaying their marriage. Their decline in marriage rates (1.9-2.1 percentage points (base mean-33.7%)) is driven by undocumented immigrants remaining single (1.8-2 percentage points (base mean - 63.2%)) and not by these immigrants getting divorced/separated. In fact, the reform has no effect on divorce rates of likely undocumented immigrants.

Table 14 shows the results for the consequences –- in terms of living arrangements –- of reduced fertility among likely undocumented females. These females have higher probability of living with an unmarried partner in the treated states. This effect is statistically significant and quite large in magnitude(2.7 percentage points (base mean-3.02%)).[39] Hence, their decline in fertility can be considered a product of lower marriage rates rather

[38]I do not find any effects of the reform on the likelihood of having a child over age 5 in the household or on the number of children above 5 years in the household. It is possible that some older children of undocumented immigrants do not reside with their parents- they either stay with their grandparents or extended family members or independently in which case we will underestimate the effects of the reform on number of children in the household.

[39]The RELATE variable in ACS is used to construct measure of coresidence with adult partner.

than for non-marital reasons. I additionally find that their probability of living with parents falls by 3.2 percentage points (base mean-48%) in the treated states.[40] The above findings are robust when I limit my sample to likely undocumented females enrolled in school and hence directly affected by the reform (Tables A12 and A13). However, the latter set of results should be cautiously interpreted as they represent selection into sample based on the reform. Even without restricting the analysis to undocumented females enrolled in school, I can demonstrate the link between the reform and their fertility choices –- by showing the first stage of the reform increasing the enrollment of undocumented females in public colleges of 'more exposed' treated states (Table A2 and Table A6). Note that, for the group of reform eligible undocumented females, their likelihood of living as head of the household rises in the treated states –- possibly capturing life in dormitories or in apartments with or without roommates.

# 6 Sensitivity and Placebo Analyses

## 6.1 Endogeneity

One possible concern that arises is endogeneity of the reform due to non-random location of immigrants across the states. It is possible that undocumented immigrants move from the untreated states to the states offering tuition subsidies which will allow them to obtain more education. If this happens, then due to violation of Stable Unit Treatment Value Assumption (SUTVA), my estimates would be biased. I would be overstating the actual treatment effects of the reform.

[40] I define parental co-residence as living with one's own parents. I do not consider cases where individuals reside with their spouses' parents. Including those cases does not change my results as share of individuals staying with their spouses' parents is very small in my sample. Individuals staying in an institution, such as a college dormitory are not considered co-residence with a parent even if they mention their parents' home to be the permanent address. The ACS variables used to construct my measure of parental coresidence are: MOMLOC (mother's location in household), POPLOC (father's location in household) and RELATE(relationship to household head).

To address this concern, I look at whether the reform affects the likelihood of Mexican/Hispanic non-citizens' moving to a treated state. For this purpose, I use individual-level longitudinal data from the 2001, 2004, 2008 and 2014 panels of Survey of Income and Program Participation. Table A14 reports the estimated coefficients of tuition subsidy reform for three age groups of Mexican/Hispanic non-citizens. Columns 2 and 3 denote the age groups most likely to respond to the reform. Both columns indicate that these groups do not appear to substantially move to the treated states to take advantage of the lower costs of college. Thus, spillovers in the untreated states are not affecting my estimates.

## 6.2 Falsification tests

In this section, I attempt to rule out some alternate channels that can confound the treatment effects derived in this paper. First, I find no enrollment effects on domestic students who are unaffected by the reform, in more exposed public colleges of the treated states (Table 4). Moreover, there are no effects of the reform on the education outcomes of NRAs in all the treated states –- signalled by the binary treatment variable. In the 'lower exposure' treated states, international students on visas may comprise a larger portion of the NRAs. A lack of effects indicate that such students are not affected by the reform. Thus, the reform has no significant spillover effects on non-targeted groups of students.

Second, it is possible that the treated colleges changed their course offerings in conjunction with the reform to attract particular groups of students. To test this claim, I use a proxy measure of course offerings –- by counting the number of individual majors in which a degree was awarded at school i in year t. Figures A3 and A4 indicate little changes in the number of courses offered for Associate's and Bachelor's degrees in public colleges 'more exposed' to the reform.

Third, the reform could be correlated with some labor market conditions that might cause increased enrollment of undocumented students in community colleges. Even though I control for state level driver's license policies for undocumented immigrants in the specifications, this concern can be tested directly. I use the March CPS to construct three

measures of labour market conditions at the state-year level. I then examine whether these conditions change timed with the reform. Any significant changes in these conditions would make us skeptical of the validity of the parallel trends assumption. The reform does not affect the unemployment rate meaningfully, a common indicator of the overall health of the labor market (Figure A5). I also find no correlation between the reform and average employment of undocumented immigrants aged 35 and above (Figure A6).[41] The point estimates as well as 95% confidence intervals suggest no changes in the pattern of log earnings of the undocumented immigrants in the above age group (Figure A7).[42] These three sets of results signal that the relevant labor market conditions were orthogonal to the timing of the reform.

Finally, for the fertility outcomes, I do not observe any effects of the reform, either on the 'likelihood of giving birth to a child in the last year' or 'having at least 1 child below 5 years in the household' for both foreign born female citizens or natives(see table 12) –- groups ineligible for the reform. Thus, it is unlikely that other policies affecting the fertility rates of non-targeted groups significantly overlap with the reform I study.

## 6.3 Alternative Specifications

In table A15, I check the robustness of the coefficient $\gamma$ to alternative choices of the control group. I consider the enrollment and graduation outcomes of undocumented students in private, public 2 year and public 4 year colleges separately, in each of the panels. Column 1 reports the baseline estimates. Column 2 drops the states which explicitly ban the subsidy while column 3 excludes the states which have never offered the subsidy. Finally, column 4 retains only the treated states in the sample. For all colleges, the estimates are very similar across the specifications. The results for private colleges continue to remain small and precise across the different control groups. However, their enrollment estimates

[41] These undocumented immigrants are identified using the method outlined in Borjas(2017) and Borjas and Cassidy(2019).

[42] Additionally, proportion of votes for the Republicans does not show significant correlation with the reform (Figure A8).

turn negative –- albeit small in magnitude and within the confidence intervals of the other estimates –- when we drop the states that never offered the subsidy. The results for the public 4 year and 2 year colleges are also identical with my conclusions of the preferred specifications.

Because the two-way fixed effects (TWFE) design provides biased estimates of treatment effects when there is a staggered rollout of the treatment, and heterogeneous treatment effects, across the units–- as in our setting, I report the Callaway and Sant'Anna (2021) estimates in Table A16. These estimates, pertaining to the binary treatment variable associated with the reform, avoid the shortcomings of TWFE mentioned above. I use the never treated states as the counterfactual group. I find largely similar estimates for the education variables. However in case of fertility outcomes, the coefficient on 'given birth to a child in the last year' becomes marginally significant –- different from the conclusion of my preferred specification. Also, these fertility estimates are not highly robust in terms of sign, when I include the individual and state controls in the model.[43] But note that, I do not control for other policies in these specifications, and the instability in the relevant coefficients may be partially attributed to these confounding policies.

# 7 Conclusion

In this paper, I investigate the causal effects of providing tuition subsidies to undocumented students on both intended and unintended education and family formation outcomes. In particular, I explore detailed institution level education and pricing outcomes of the targeted group and spillovers to the ineligible groups –- using IPEDS data, supplemented with administrative data from the SEVIS. Additionally, I look at the long run fertility effects of likely undocumented students exposed to the reform and try to understand their subsequent living arrangements, which can explain the decline in their fertility. I use a difference-in-differences research design, exploiting variation in the timing and intensity of exposure to the reform across institutions and states, to examine my research questions.

[43]Results available on request.

There are four primary findings. I first find that the reform increases the NRA share of first-year enrollment by 0.026 percentage points in public 2 year colleges, for every 1% of the population that is undocumented in the treated states. I then use the SEVIS records to identify the exact number of undocumented students by institution for the pre-policy year 2003. Corresponding estimates reveal that the reform raises the number of NRAs in treated state institutions with above median number of pre-determined undocumented students by 24% (baseline mean: 42.9) in community colleges. Among the 2 year colleges, there is increased enrollment of these students in high transfer and technical & vocational colleges of treated states 'more exposed' to the reform. For public 4 year institutions, the reform causes the NRA share to rise by 0.003 percentage points, for every 1% of the undocumented share, in the treated states. This estimate is statistically insignificant. In specifications with SEVIS records, the number of NRAs in treated state institutions 'more exposed' to the reform rises by around 14% (baseline mean: 42.5) in 4 year colleges –- an economically meaningful, though insignificant growth. The modest rise in enrollment at the 4 year institutions can be attributed to the more competitive ones, as per Barron's ratings of institutions.

Second, I provide evidence on the college completion effects of the reform. In particular, the NRA share of total graduates increases by 0.013 percentage points in community colleges, for every 1% of the undocumented share, in the treated states. In fact, there is a 10% (baseline mean: 44.6) rise in the number of NRAs graduating from these treated state institutions 'highly exposed' to the reform. For 4-year colleges, the number of NRAs graduating from treated state institutions 'heavily exposed' to the reform increases by 6.5% (baseline mean:41.7).

Third, I observe that on average, students do not experience increases in their tuition in 'more exposed' institutions of the treated states. This uniformly applies to instate and out-of-state tuition; public 2 year and 4 year colleges, as well as flagship schools.

I find no effects on the enrollment or graduation rates of the Americans –- who are supposed to be unaffected by the reform. In fact, I find mild positive effects on the enrollment of natives in community colleges, timed with the reform. This is consistent with the

explanation that the net tuition from undocumented students helps to support the cost of enrolling more natives (Shih, 2017).

In the second half of the paper, my fourth set of results indicate that likely undocumented females respond to the increased educational attainment induced by the reform by delaying their childbearing and household formation. In particular, I find that the reform reduces female undocumented immigrants fertility rates by 1.8-1.9 percentage points (4.6%-4.8%). This is mainly driven by these females delaying their marriage, though they increasingly reside with their unmarried partners in the treated states.

To sum up, access to subsidised education seems to have increased the educational attainment of undocumented students in treated states, accounting for their selection into various institutions. However, there still remains a wide gap in enrollment and completion outcomes between these students and the legal immigrants and natives. Even with the DACA program, the uncertainties surrounding their legalisation and the strict eligibility requirements for this reform, may discourage them from demanding more education. The benefits of increasing educational achievement of undocumented youth can possibly be seen not only in the labor market but also in non-market areas. [44] For example, undocumented students with college degrees may make more informed decisions in the political and civic life of the society. Beyond the education benefits, there are indirect benefits of the reform in terms of reduced childbirths to undocumented females.

To get an idea of the cost of the reform, I do the following back-of-the-envelope calculation. My results suggest that the reform increases the number of NRAs enrolling in treated 2-year colleges with higher pre-reform presence of undocumented students by 24% (baseline mean: 42.9). In a sample of 5,969 observations(institution-year), this can be interpreted as 62,225 undocumented students being enrolled in 'more exposed' 2-year colleges due to the reform. [45] Multiplying the above number of enrolled undocumented students by the

[44] assuming these undocumented immigrants are not deported.

[45] Note that the enrollment number of undocumented students affected by the reform may appear large –- but it includes the total size of the affected group (plus a small number of the unaffected foreign visa-holder students) across all the public institutions of the US over the entire study period. Note that, this

average annual tuition subsidy in community colleges i.e. $3693, the per year cost of the reform turns out to be around $16.4 million.[46] These subsidies seem to be funded from taxpayers' money and its incidence would fall on some combination of college stakeholders: students, faculty, administrators, alumni, community partners.

To conclude, my study provides causal estimates of the direct and indirect benefits and spillover effects of positive permanent price shocks faced by undocumented students in public colleges. This analysis suggests the importance of considering both the behavior of consumers and firms in assessing the impact of targeted reforms. It can provide insights into determining the winners and losers, and calculating the overall welfare effect of such policies.

Even though I have identified the institutions with a higher historical presence of undocumented students, and therefore 'more exposed' to the reform from administrative records –- there is still a possibility that my treatment group consists of a small number of international students with valid visas who are ineligible for the reform. This can happen if foreign documented students enrol in the institutions 'more exposed' to the reform, timed with the reform. If so, then I would be over counting the total number of undocumented students. In that case, the difference-in-difference estimates of $\gamma$ would understate the true effects of the reform, and the coefficients should be regarded as the 'lower bound' of the treatment effects.

analysis uses SEVIS records and hence restricts the sample period from 2004-2017.

[46]This assumes that the overall cost of the reform is spread evenly across the years.

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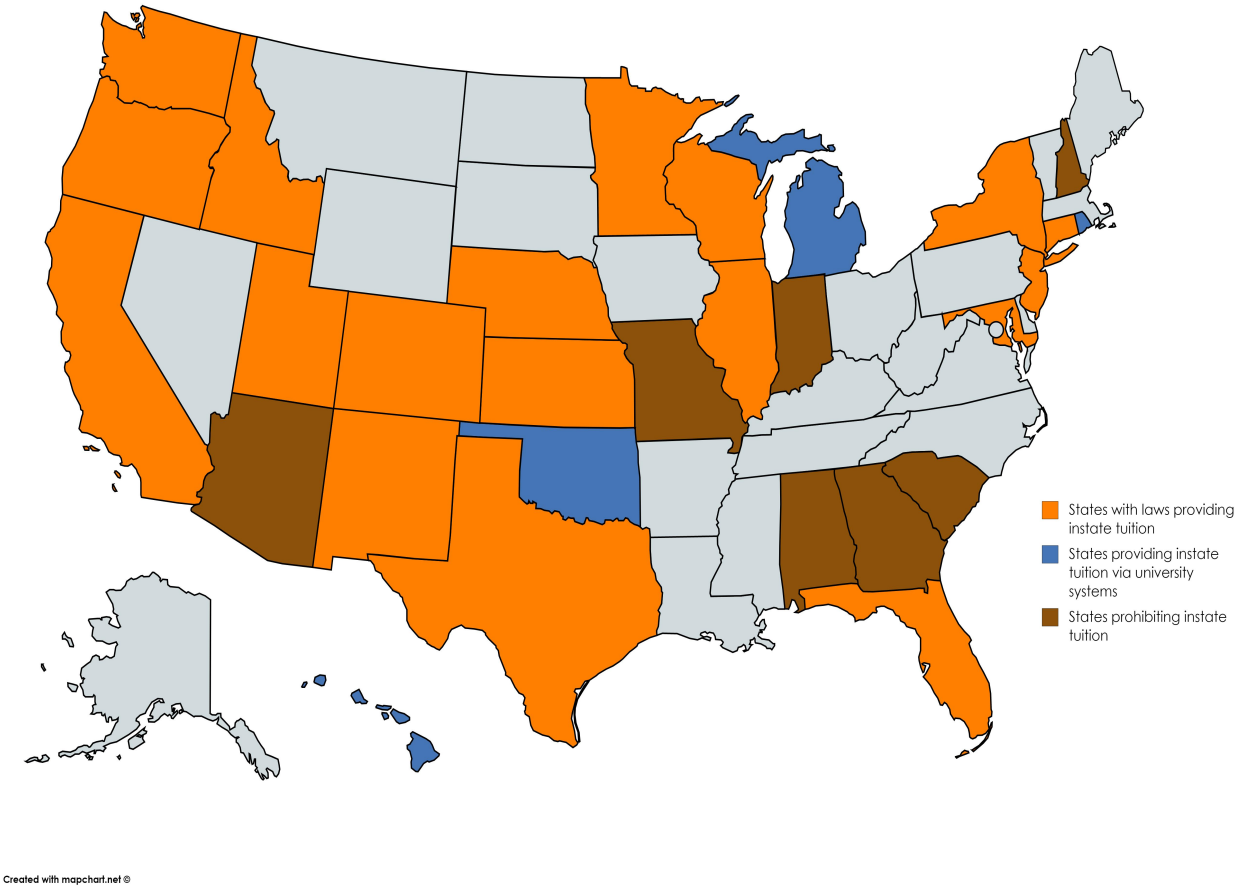
Figure 1: State legislation on resident tuition for undocumented immigrants




Source: NCSL

Figure 2: Overall undocumented immigrant population 1990-2016

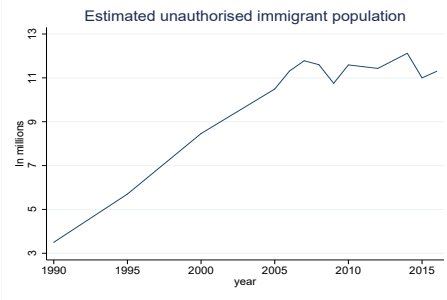



Figure 3: Mexican undocumented immigrant population 1990-2016

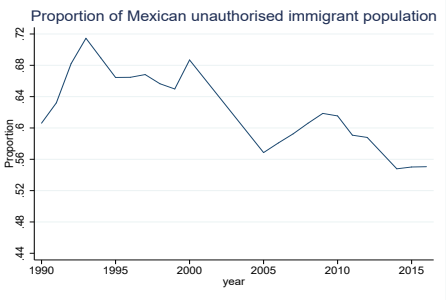



Figure 4: Event study : Two-year college enrollment (Institution level)

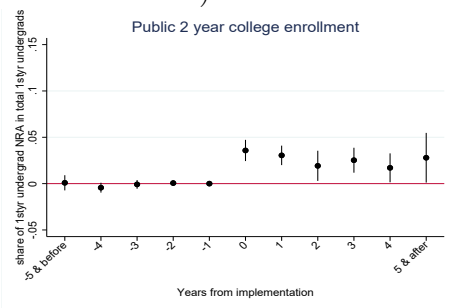



Figure 5: Event study: Four-year college enrollment(Institution level)

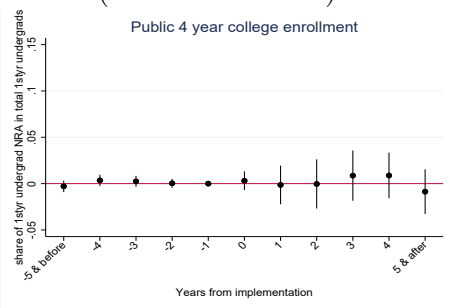



Figure 6: Event study : Two-year college graduation (Institution level)

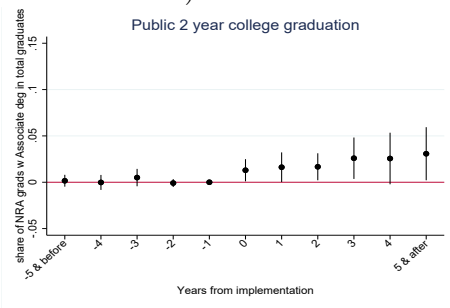



Figure 7: Event study: Four-year college graduation (Institution level)

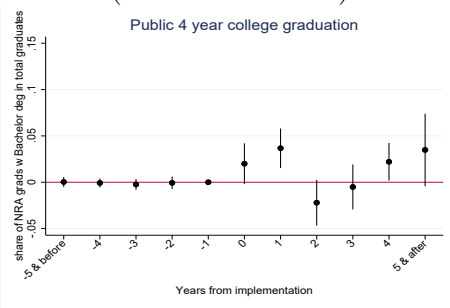


Figure 8: Event study : No. of college years

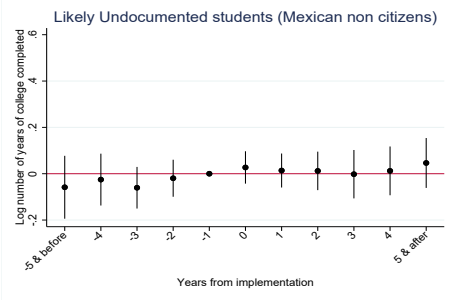



Figure 9: Event study: Fertility

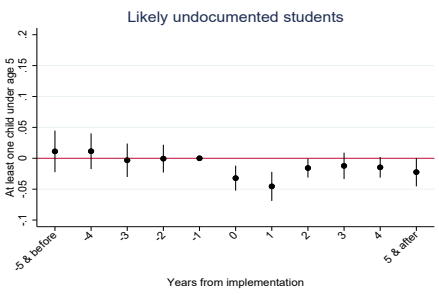


Table 1: State legislation on in-state tuition for undocumented immigrants

| State | Legislation | Effective Date | %LU | %Foreign-born citizens | %Natives |
|---|---|---|---|---|---|
| California | AB 540 | Jan,2002 | 8.2% | 11.4% | 72.4% |
| Texas | SB 1403 | Jul,2001 | 7.2% | 4.8% | 83.0% |
| Utah | HB 144 | Jul,2002 | 2.8% | 2.4% | 91.3% |
| New York | SB 7784 | Sep,2002 | 0.9% | 10.7% | 77.7% |
| Illinois | HB 0060 | Jun,2003 | 3.4% | 5.4% | 86.5% |
| Oklahoma | SB 596 | Jun,2003 | 1.7% | 1.6% | 94.5% |
| Washington | HB 1079 | Jul,2003 | 2.3% | 5.0% | 86.2% |
| Kansas | HB 2145 | Jul,2004 | 1.9% | 2.1% | 93.0% |
| New Mexico | SB 582 | Apr,2005 | 4.2% | 2.9% | 90.0% |
| Nebraska | LB 239 | Sep,2006 | 1.9% | 2.1% | 92.7% |
| Wisconsin | AB 75 | Jul,2009 | 1.3% | 1.8% | 94.9% |
| Connecticut | HB 6390 | Jul,2011 | 0.4% | 5.9% | 86.5% |
| Rhode Island | Residency reform | Sep,2012 | 0.3% | 5.8% | 86.3% |
| Maryland | SB 167 | Dec,2012 | 0.7% | 5.8% | 84.8% |
| Hawaii | Board of Regents | Mar,2013 | 0.2% | 9.6% | 80.6% |
| Colorado | SB 13-033 | May,2013 | 4.0% | 3.1% | 88.9% |
| Oregon | HB 2787 | Jul,2013 | 3.0% | 3.3% | 89.4% |
| Michigan | Board of Regents | Aug,2013 | 0.5% | 3.0% | 92.8% |
| Minnesota | SF 1236 | Jul,2013 | 0.9% | 2.8% | 92.4% |
| New Jersey | S 2479 | Jan,2014 | 1.1% | 9.7% | 78.8% |
| Florida | HB 851 | Jul,2014 | 1.2% | 9.0% | 79.6% |
| Idaho | SB 1280 | Mar,2016 | 2.7% | 1.8% | 93.0% |

Notes- In Oklahoma, the law was amended in November 2007 to allow the Board of Regents to decide on the reform and they continued with it. In Wisconsin, the law was revoked in June 2011. The percentages in columns 4,5 and 6 denote the average proportion of Mexican non-citizens, foreign born citizens, and natives respectively during the period July 1999-December 2017.
Source-*NCSL, ULEAD network*

Table 2: Summary Statistics

| | LU Immigrants | | Foreign-born citizens | | Natives | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| **Panel A: Summary statistics on school aged individuals** | | | | | | |
| College enrollment rate | 0.19 | 0.39 | 0.52 | 0.50 | 0.42 | 0.49 |
| Policy dummy | 0.55 | 0.50 | 0.50 | 0.50 | 0.33 | 0.47 |
| Neither working nor in school | 0.23 | 0.42 | 0.14 | 0.34 | 0.16 | 0.36 |
| Financial aid reform | 0.26 | 0.44 | 0.13 | 0.34 | 0.12 | 0.33 |
| Drivers' license reform | 0.10 | 0.30 | 0.09 | 0.29 | 0.08 | 0.27 |
| Graduation rate | 0.12 | 0.32 | 0.41 | 0.49 | 0.38 | 0.48 |
| **Panel B: Summary statistics on individuals aged 30-45 in 2017** | | | | | | |
| Years of schooling | 9.98 | 3.54 | 14.12 | 3.28 | 14.28 | 2.61 |
| Employment rate | 0.72 | 0.45 | 0.80 | 0.40 | 0.81 | 0.40 |
| At least some college | 0.14 | 0.35 | 0.65 | 0.48 | 0.69 | 0.46 |
| Household income | 41,962 | 30,306 | 77,273 | 46,570 | 78,230 | 45,672 |
| Observations | 13,413 | | 26,230 | | 245,645 | |

Notes: LU-Likely undocumented (Mexican non-citizens). The time period in panel A is July 1999-December 2017. The relevant sample for college enrollment rate is high school graduates between 17-24 years old who have not yet obtained a Bachelor's degree. The relevant sample for college graduation rate is individuals between 21-28 years old who have completed high school.Observations are weighted by person weights from IPUMS CPS.

Table 3: Summary Statistics-Fertility and household formation

| | LU Immigrants | | Foreign-born citizens | | Natives | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| Single/unmarried | 0.632 | 0.482 | 0.747 | 0.435 | 0.782 | 0.413 |
| Married | 0.337 | 0.473 | 0.224 | 0.417 | 0.187 | 0.390 |
| Divorced | 0.0285 | 0.1665 | 0.0282 | 0.1655 | 0.0299 | 0.1702 |
| Living w/ parents | 0.480 | 0.500 | 0.434 | 0.496 | 0.384 | 0.486 |
| Living as head of household | 0.266 | 0.442 | 0.330 | 0.470 | 0.353 | 0.478 |
| Living w/ unmarried partner | 0.0302 | 0.1712 | 0.0296 | 0.1695 | 0.0450 | 0.2072 |
| Mother at least some college degree | 0.035 | 0.185 | 0.230 | 0.421 | 0.220 | 0.414 |
| Father at least some college degree | 0.057 | 0.231 | 0.210 | 0.407 | 0.169 | 0.375 |
| Observations | 135,479 | | 278,690 | | 5,910,176 | |
| At least one child under age 5 | 0.391 | 0.488 | 0.191 | 0.393 | 0.209 | 0.407 |
| Observations | 61,757 | | 144,711 | | 3,021,304 | |
| Given birth to child in last year | 0.143 | 0.350 | 0.073 | 0.260 | 0.078 | 0.268 |
| Observations | 46,169 | | 113,530 | | 2,328,547 | |

Notes: LU-Likely undocumented (Mexican non-citizens). The time period is 2000-2017. For LU immigrants, sample consists of 17-28 year old who are high school graduates and have arrived to US within 14 years of age. They are the eligible students for receiving tuition subsidies. For foreign citizens and natives, the sample consists of 17-28 year old high school graduates. The summary statistics for "at least one child under age 5" and "given birth to child in last year" are pertaining to females only. Observations are weighted by person weights from IPUMS ACS.

Table 4: Dependent variable: Enrollment effects by sector of institution

Coefficients of Tuition Subsidy reform* state share of undocumented immigrants in 1990

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens | 0.005 | 0.004 | 0.011 | 10,461 |
| | (0.007) | (0.006) | (0.008) | |
| | [0.0072] | [0.0058] | [0.0080] | |
| Dep var mean | 0.044 | 0.044 | 0.044 | |
| Domestic students | -0.036 | -0.037 | -0.049 | 10,006 |
| | (0.047) | (0.047) | (0.048) | |
| Dep var mean | 0.786 | 0.786 | 0.786 | |
| **Panel B: Public 2 year colleges** | | | | |
| Non Resident Aliens | 0.020*** | 0.026*** | 0.038*** | 7,581 |
| | (0.006) | (0.005) | (0.007) | |
| | [0.0059] | [0.0050] | [0.0072] | |
| Dep var mean | 0.061 | 0.061 | 0.061 | |
| Domestic students | 0.074 | 0.072 | 0.040 | 7,228 |
| | (0.052) | (0.051) | (0.039) | |
| Dep var mean | 0.906 | 0.906 | 0.906 | |
| **Panel C: Public 4 year colleges** | | | | |
| Non Resident Aliens | 0.003 | 0.009* | 0.002 | 9,453 |
| | (0.005) | (0.005) | (0.008) | |
| | [0.0052] | [0.0065] | [0.0084] | |
| Dep var mean | 0.021 | 0.021 | 0.021 | |
| Domestic students | -0.106 | -0.103 | -0.115 | 9,453 |
| | (0.065) | (0.065) | (0.081) | |
| Dep var mean | 0.889 | 0.889 | 0.889 | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The first dependent variable in each panel is number of first year non resident alien undergraduates enrolled in institution i of a particular sector as a share of total number of first year undergraduates in institution i . The second dependent variable in each panel is number of first year domestic undergraduates enrolled in institution i of a particular sector as a share of total number of first year undergraduates in institution i.Each cell represents the coefficient of the interaction term between tuition subsidy reform and state undocumented immigrant share in 1990, from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors in parenthesis are clustered by state, and wild-cluster bootstrapped standard errors (by state) are in brackets. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 5: Dependent variable: Enrollment effects by sector of institution

Coefficients of Tuition Subsidy reform* 𝟙[Institution has above median number of undocumented students in 2003]

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens | 0.135 | 0.123 | 0.110 | 8,897 |
| | (0.111) | (0.107) | (0.107) | |
| Dep var mean | 26.3 | 26.3 | 26.3 | |
| **Panel B: Public 2 year colleges** | | | | |
| Non Resident Aliens | 0.271** | 0.256** | 0.243** | 6,392 |
| | (0.105) | (0.100) | (0.099) | |
| Dep var mean | 42.9 | 42.9 | 42.9 | |
| **Panel C: Public 4 year colleges** | | | | |
| Non Resident Aliens | 0.150 | 0.145 | 0.191* | 7,986 |
| | (0.113) | (0.108) | (0.100) | |
| Dep var mean | 42.5 | 42.5 | 42.5 | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The dependent variable in each panel is log number of first year non resident alien undergraduates enrolled in institution i of a particular sector.Each cell represents the coefficient of tuition subsidy reform interacted with a dummy variable indicating whether an institution has above median number of undocumented students in the pre-treatment period 2003, from a separate regression. For details on calculation of exact number of undocumented students by institution, see text. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 6: Dependent variable: Enrollment effects by selectivity of 4-year public institutions
Coefficients of Tuition Subsidy reform* state share of undocumented immigrants in 1990

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: More competitive institutions** | | | | |
| Non Resident Aliens | 0.034 | 0.026 | 0.033 | 632 |
| | (0.022) | (0.021) | (0.027) | |
| **Panel B: Less competitive institutions** | | | | |
| Non Resident Aliens | 0.005 | 0.004 | 0.012 | 8,821 |
| | (0.005) | (0.005) | (0.009) | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The dependent variable is number of first year non resident alien undergraduates enrolled in institution i as a share of total number of first year undergraduates in institution i, by selectivity of the four-year institution. The selectivity measure used is Barron's ratings for an institution in the most recent year prior to the reform's implementation. More competitive institutions are those with a rating of 1,2,or 3, while less competitive institutions have a rating of 4, 5, or 6. For details, see text. Each cell represents the coefficient of the interaction term between tuition subsidy reform and state undocumented immigrant share in 1990, from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 7: Dependent variable: Enrollment effects by Carnegie classification institutions

Coefficients of Tuition Subsidy reform* state share of undocumented immigrants in 1990

| | (1) | (2) | Observations |
|---|---|---|---|
| **Panel A: Associate's Colleges- High transfer** | | | |
| Non Resident Aliens | 0.027** | 0.034*** | 3,494 |
| | (0.010) | (0.008) | |
| Dep var mean | 0.0675 | 0.0675 | |
| **Panel B: Associate's Colleges- Mixed transfer/vocational & technical** | | | |
| Non Resident Aliens | 0.016*** | 0.017*** | 2,986 |
| | (0.005) | (0.005) | |
| Dep var mean | 0.056 | 0.056 | |
| **Panel C: Associate's Colleges- High vocational/technical** | | | |
| Non Resident Aliens | 0.027*** | 0.029*** | 1,190 |
| | (0.004) | (0.003) | |
| Dep var mean | 0.0514 | 0.0514 | |
| **Panel D: Baccalaureate Colleges: Arts & Sciences** | | | |
| Non Resident Aliens | -0.003 | -0.005 | 2,227 |
| | (0.010) | (0.010) | |
| Dep var mean | 0.037 | 0.037 | |
| **Panel E: Special Focus Four-Year: Health Professions** | | | |
| Non Resident Aliens | -0.029 | -0.022 | 90 |
| | (0.038) | (0.034) | |
| Dep var mean | 0.051 | 0.051 | |
| **Panel F: Special Focus Four-Year: Business & Management** | | | |
| Non Resident Aliens | -0.053*** | -0.061*** | 111 |
| | (0.006) | (0.005) | |
| Dep var mean | 0.122 | 0.122 | |
| **Panel G: Special Focus Four-Year: Arts, Music & Design** | | | |
| Non Resident Aliens | 0.010 | 0.011 | 372 |
| | (0.062) | (0.061) | |
| Dep var mean | 0.093 | 0.093 | |
| Controls | No | Yes | |
| License and financial aid policies | Yes | Yes | |
| Institution FE | Yes | Yes | |
| Year FE | Yes | Yes | |

Notes: The dependent variable is number of first year non resident alien undergraduates enrolled in institution i as a share of total number of first year undergraduates in institution i . Each cell represents the coefficient of the interaction term between tuition subsidy reform and state undocumented immigrant share in 1990, from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis.***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 8: Dependent variable: Final Choice of major in undergraduate degree

Coefficients of Tuition Subsidy reform* state share of undocumented immigrants in 1990

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Arts & humanities** | | | | |
| Non Resident Aliens | -0.004 | -0.002 | -0.006 | 27,593 |
| | (0.003) | (0.003) | (0.004) | |
| **Panel B: Business** | | | | |
| Non Resident Aliens | -0.004 | -0.009 | 0.007 | 27,593 |
| | (0.008) | (0.008) | (0.006) | |
| **Panel C: Health & medicine** | | | | |
| Non Resident Aliens | -0.006 | -0.006 | 0.001 | 23,082 |
| | (0.006) | (0.006) | (0.004) | |
| **Panel D: Multi/interdisciplinary studies** | | | | |
| Non Resident Aliens | 0.004 | 0.003 | 0.006 | 27,593 |
| | (0.006) | (0.006) | (0.006) | |
| **Panel E: Public & social services** | | | | |
| Non Resident Aliens | 0.004 | 0.004* | 0.008 | 27,593 |
| | (0.003) | (0.003) | (0.005) | |
| **Panel F: STEM** | | | | |
| Non Resident Aliens | -0.006 | -0.009 | 0.006 | 27,593 |
| | (0.009) | (0.009) | (0.005) | |
| **Panel G: Social sciences** | | | | |
| Non Resident Aliens | 0.006 | 0.005 | 0.010 | 27,593 |
| | (0.006) | (0.006) | (0.008) | |
| **Panel H: Trades & personal services** | | | | |
| Non Resident Aliens | 0.0003 | -0.003 | 0.004 | 27,593 |
| | (0.003) | (0.003) | (0.004) | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The sample comprises only public colleges. The dependent variable is number of non resident alien undergraduates (Associate's or Bachelor's degree) who majored in program j in institution i as a share of total number of undergraduates who majored in program j in institution i . Each cell represents the coefficient of the interaction term between tuition subsidy reform and state undocumented immigrant share in 1990, from a separate regression. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 9: Dependent variable: Graduation effects by sector of institution

Coefficients of Tuition Subsidy reform * state share of undocumented immigrants in 1990

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens | 0.012 | 0.012 | 0.006 | 11,285 |
| | (0.008) | (0.007) | (0.012) | |
| | [0.0078] | [0.0076] | [0.0120] | |
| Dep var mean | 0.040 | 0.040 | 0.040 | |
| **Panel B: Public 2 year colleges** | | | | |
| Non Resident Aliens | 0.013** | 0.014** | 0.019** | 8,172 |
| | (0.006) | (0.006) | (0.007) | |
| | [0.0059] | [0.0070] | [0.0077] | |
| Dep var mean | 0.035 | 0.035 | 0.035 | |
| **Panel C: Public 4 year colleges** | | | | |
| Non Resident Aliens | 0.014* | 0.017** | 0.016 | 10,019 |
| | (0.007) | (0.007) | (0.010) | |
| | [0.0074] | [0.0078] | [0.0106] | |
| Dep var mean | 0.038 | 0.038 | 0.038 | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The dependent variable is number of non resident aliens who graduated (with a Bachelor's or Associate's degree) as a share of total number of students who graduated from institution i. Each cell represents the coefficient of the interaction term between tuition subsidy reform and state undocumented immigrant share in 1990, from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors in parenthesis are clustered by state, and wild-cluster bootstrapped standard errors (by state) are in brackets. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 10: Dependent variable: Graduation effects by sector of institution

Coefficients of Tuition Subsidy reform* 𝟙[Institution has above median number of undocumented students in 2003]

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens | 0.005 | 0.003 | 0.018 | 8,821 |
| | (0.063) | (0.063) | (0.070) | |
| Dep var mean | 26.4 | 26.4 | 26.4 | |
| **Panel B: Public 2 year colleges** | | | | |
| Non Resident Aliens | 0.093** | 0.091** | 0.104** | 6,392 |
| | (0.043) | (0.042) | (0.043) | |
| Dep var mean | 44.6 | 44.6 | 44.6 | |
| **Panel C: Public 4 year colleges** | | | | |
| Non Resident Aliens | 0.065** | 0.059* | 0.137*** | 8,010 |
| | (0.030) | (0.030) | (0.032) | |
| Dep var mean | 41.7 | 41.7 | 41.7 | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The dependent variable is log number of non resident aliens graduating (with a Bachelor's or Associate's degree) from institution i of a particular sector. Each cell represents the coefficient of tuition subsidy reform interacted with a dummy variable indicating whether an institution has above median number of undocumented students in the pre-treatment period 2003, from a separate regression. For details on calculation of exact number of undocumented students by institution, see text. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 11: Dependent variable: Ln(tuition and required fees)

Coefficients of tuition subsidy reform*𝟙[Institution has above median number of undocumented students in 2003]

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| In state-Public 4 year | -0.040* | -0.036* | -0.022 | 9,884 |
| | (0.021) | (0.020) | (0.015) | |
| Out of state- Public 4 year | -0.049* | -0.044 | -0.037 | 9,884 |
| | (0.028) | (0.028) | (0.027) | |
| In state-Public 2 year | -0.025 | -0.032* | -0.018 | 12,250 |
| | (0.018) | (0.018) | (0.016) | |
| Out of state-Public 2 year | -0.061** | -0.059** | -0.056** | 12,250 |
| | (0.024) | (0.024) | (0.023) | |
| In state-Flagship | 0.007 | 0.0002 | 0.0008 | 6,216 |
| | (0.025) | (0.023) | (0.015) | |
| Out of state-Flagship | -0.020 | -0.020 | -0.004 | 6,216 |
| | (0.022) | (0.022) | (0.022) | |
| Controls | N | Y | Y | |
| Institution FE | Y | Y | Y | |
| Year FE | Y | Y | Y | |
| State time trends | N | N | Y | |

Notes: Data is taken from IPEDS. Time period considered is 2004-2017. The sample is restricted to degree granting, Title 4 participating public institutions that have full time first time undergraduates enrolled in them. Each cell represents the coefficient of tuition subsidy reform interacted with a dummy variable indicating whether an institution has above median number of undocumented students in the pre-treatment period 2003, from a separate regression. For details on calculation of exact number of undocumented students by institution, see text. Dependent variable is measured in log 2016 dollars. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 12: Effects of tuition subsidy reform on fertility decisions

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: At least one child under age 5 (females)** | | | | |
| LU immigrants | −0.010 | −0.018*** | −0.019*** | 61,372 |
| | (0.007) | (0.006) | (0.007) | |
| Foreign-born citizens | -0.006 | 0.001 | 0.002 | 140,693 |
| | (0.006) | (0.006) | (0.007) | |
| Natives | 0.0037 | 0.0031 | 0.0033 | 2,975,517 |
| | (0.0024) | (0.0022) | (0.0022) | |
| **Panel B: Whether given birth to a child in the last 12 months** | | | | |
| LU immigrants | -0.026** | -0.025** | -0.022** | 46,147 |
| | (0.0123) | (0.0122) | (0.011) | |
| Foreign-born citizens | 0.0003 | 0.002 | 0.002 | 113,166 |
| | (0.0064) | (0.0062) | (0.0057) | |
| Natives | 0.0008 | 0.0009 | 0.0009 | 2,324,806 |
| | (0.0013) | (0.0012) | (0.0012) | |
| Individual controls | No | Yes | Yes | |
| State controls | No | No | Yes | |
| Immigration enforcement policies | Yes | Yes | Yes | |
| State FE | Yes | Yes | Yes | |
| Time FE | Yes | Yes | Yes | |

Notes: In Panel A, the dependent variable is probability of having at least one child under age 5. In Panel B, the dependent variable is whether the female has given birth to a child in the last 12 months. The time period considered is 2000-2017. Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted using person weights from IPUMS ACS. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 13: Effects of tuition subsidy reform on marriage decisions

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Individual is never married/single** | | | | |
| LU immigrants | 0.011 | 0.018*** | 0.020*** | 134,494 |
| | (0.010) | (0.0068) | (0.0066) | |
| Foreign-born citizens | 0.005 | -0.003 | -0.004 | 271,288 |
| | (0.009) | (0.0064) | (0.0056) | |
| Natives | -0.003 | 0.0005 | 0.0004 | 5,822,825 |
| | (0.005) | (0.0041) | (0.0042) | |
| **Panel B: Individual is married** | | | | |
| LU immigrants | -0.012 | -0.019*** | -0.021*** | 134,494 |
| | (0.008) | (0.0055) | (0.0053) | |
| Foreign-born citizens | -0.004 | 0.004 | 0.005 | 271,288 |
| | (0.008) | (0.006) | (0.005) | |
| Natives | 0.003 | 0.0004 | 0.0005 | 5,822,825 |
| | (0.0042) | (0.0037) | (0.0038) | |
| **Panel C: Individual is divorced/separated** | | | | |
| LU immigrants | -0.003 | -0.0023 | -0.0025 | 134,494 |
| | (0.0049) | (0.0049) | (0.0048) | |
| Individual controls | No | Yes | Yes | |
| State controls | No | No | Yes | |
| Immigration enforcement policies | Yes | Yes | Yes | |
| State FE | Yes | Yes | Yes | |
| Time FE | Yes | Yes | Yes | |

Notes: The time period considered is 2000-2017. Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted using person weights from IPUMS USA. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table 14: Effects of tuition subsidy reform on living arrangements of undocumented females

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Individual is living with parents** | | | | |
| LU immigrants | $-0.034^{**}$ | $-0.029^{**}$ | $-0.032^{**}$ | 61,372 |
| | (0.015) | (0.014) | (0.013) | |
| **Panel B: Individual is living with unmarried partner** | | | | |
| LU immigrants | $0.028^{***}$ | $0.027^{***}$ | $0.027^{***}$ | 61,372 |
| | (0.010) | (0.009) | (0.009) | |
| **Panel C: Individual is living as head of the household** | | | | |
| LU immigrants | 0.0003 | 0.005 | 0.003 | 61,372 |
| | (0.0143) | (0.0141) | (0.0151) | |
| Individual controls | No | Yes | Yes | |
| State controls | No | No | Yes | |
| Immigration enforcement policies | Yes | Yes | Yes | |
| State FE | Yes | Yes | Yes | |
| Time FE | Yes | Yes | Yes | |

Notes: In Panel A, the dependent variable is probability that the female is living with parents. In Panel B, the dependent variable is probability that the female is living with unmarried partner. In Panel C, the dependent variable is probability that the female is living as head of the household. The time period considered is 2000-2017. Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted using person weights from IPUMS ACS. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

# Appendix Figures

Figure A1: Undocumented immigrants as % of total population in 2000 and 2014

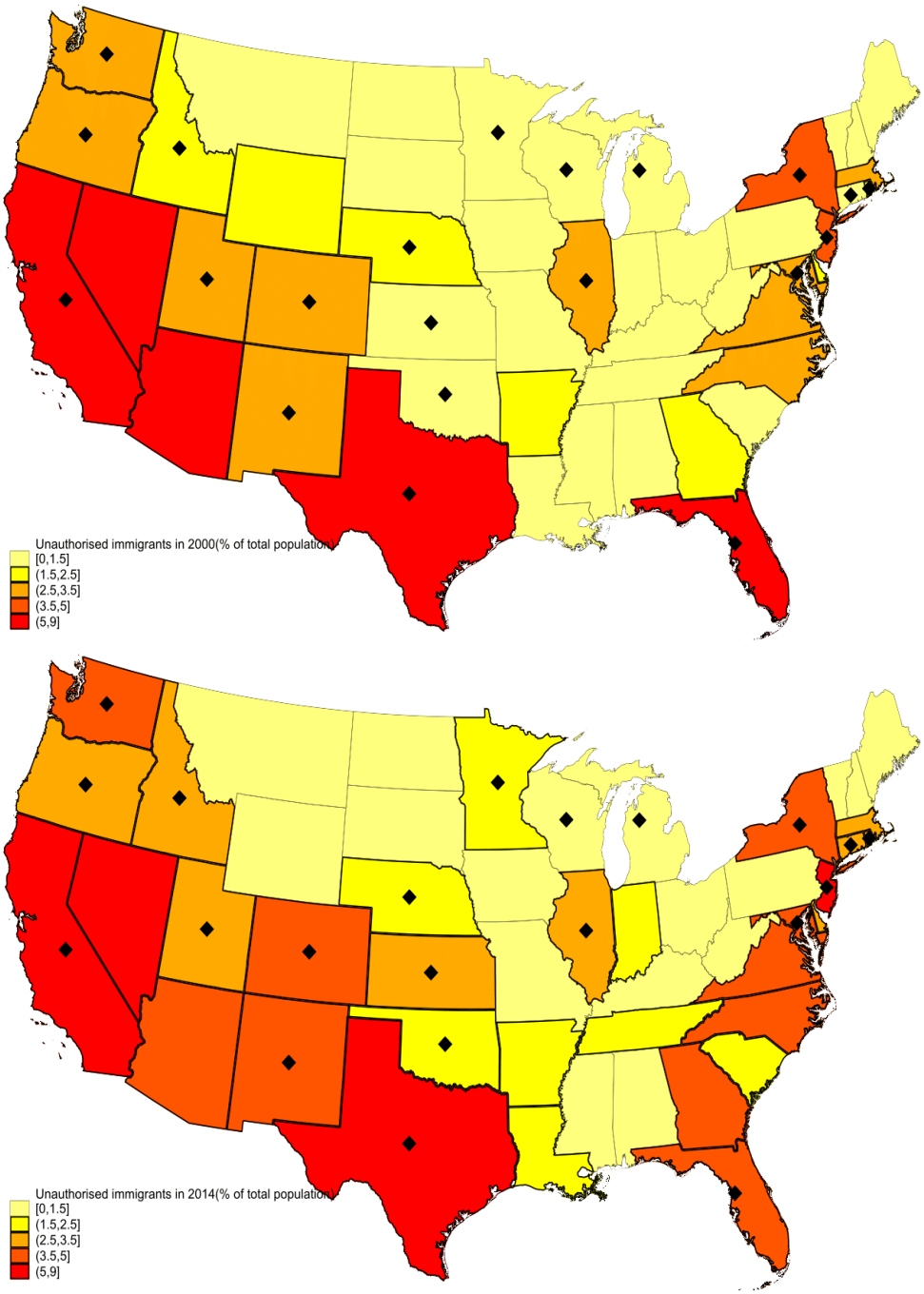
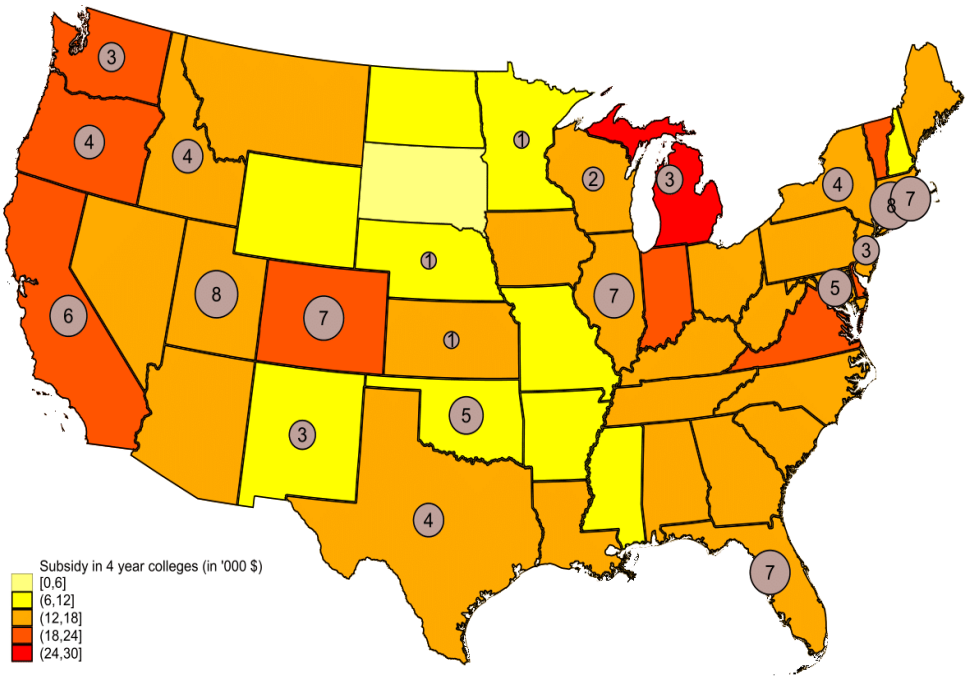


Figure A2: Sticker price tuition subsidies for public 4 year and 2 year colleges in 2016-17




Note: The states with the circles are the treated states. The area of the circle is proportional to the amount of tuition subsidy (in '000 $) in public 2 year colleges.

Figure A3: Placebo test: Courses offered for Associate's degree in public colleges

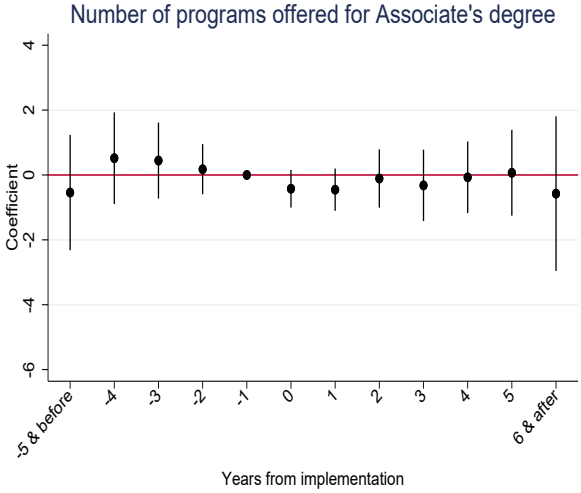



Figure A4: Placebo test: Courses offered for Bachelor's degree in public colleges

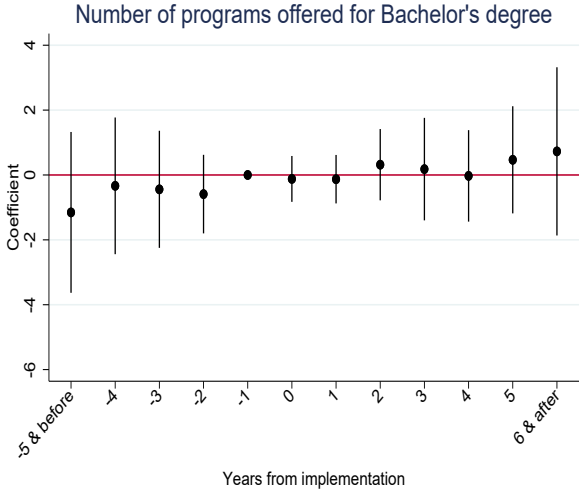


Figure A5: Placebo test: Confounding labor market shocks

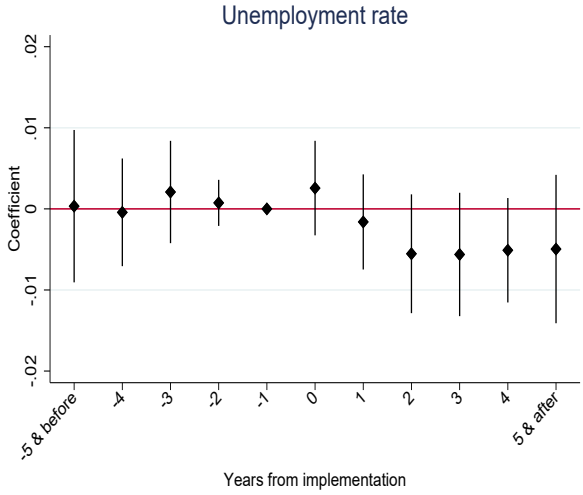



Figure A6: Placebo test: Confounding labor market shocks

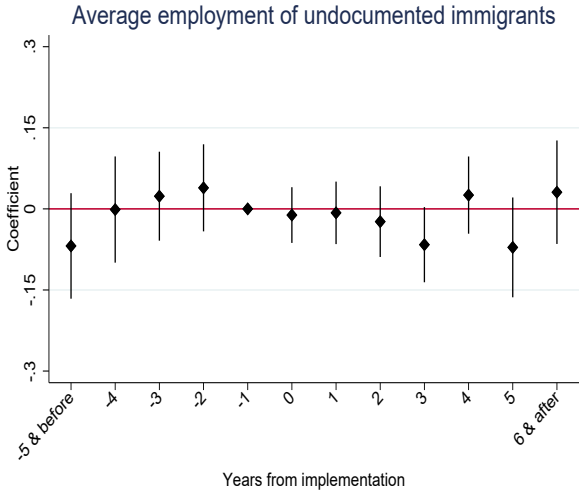



Figure A7: Placebo test: Confounding labor market shocks

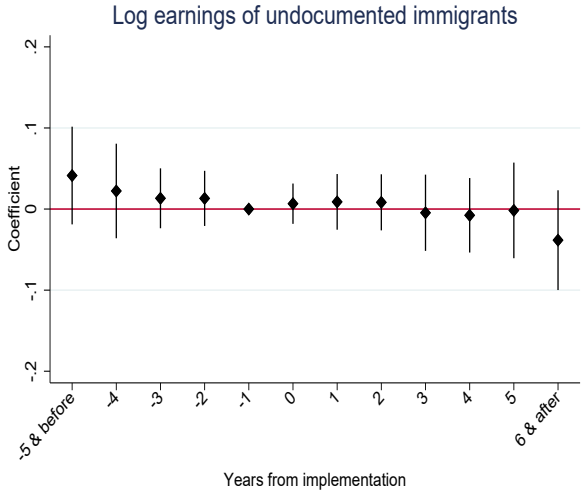



Figure A8: Placebo test: Confounding political variable shocks

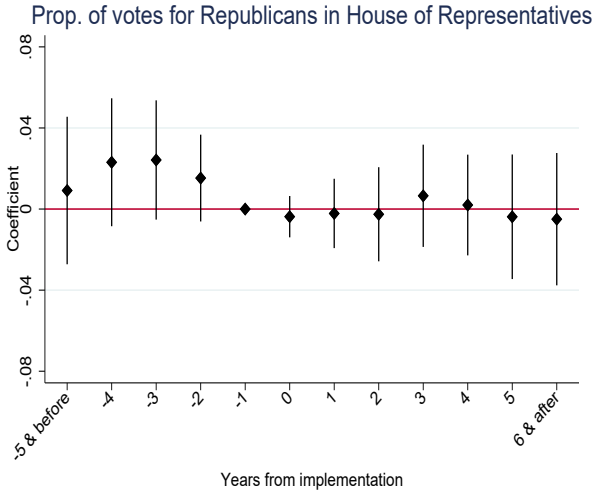


Table A1: Dependent variable: Enrollment effects by sector of institution

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens | 0.001 | 0.006 | 0.005 | 10,461 |
| | (0.007) | (0.007) | (0.008) | |
| Dep var mean | 0.044 | 0.044 | 0.044 | |
| Domestic students | 0.030 | 0.032** | 0.027** | 10,006 |
| | (0.018) | (0.015) | (0.013) | |
| Dep var mean | 0.786 | 0.786 | 0.786 | |
| **Panel B: Public 2 year colleges** | | | | |
| Non Resident Aliens | 0.001 | 0.002 | 0.003 | 7,581 |
| | (0.002) | (0.0018) | (0.004) | |
| Dep var mean | 0.061 | 0.061 | 0.061 | |
| Domestic students | -0.001 | -0.004 | -0.018** | 7,228 |
| | (0.014) | (0.012) | (0.009) | |
| Dep var mean | 0.906 | 0.906 | 0.906 | |
| **Panel C: Public 4 year colleges** | | | | |
| Non Resident Aliens | 0.001 | 0.00002 | 0.002 | 9,453 |
| | (0.002) | (0.002) | (0.004) | |
| Dep var mean | 0.021 | 0.021 | 0.021 | |
| Domestic students | 0.016 | 0.017 | 0.013 | 9,453 |
| | (0.016) | (0.015) | (0.025) | |
| Dep var mean | 0.889 | 0.889 | 0.889 | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The first dependent variable is number of first year non resident alien undergraduates enrolled in institution i as a share of total number of first year undergraduates in institution i . The second dependent variable is number of first year domestic undergraduates enrolled in institution i as a share of total number of first year undergraduates in institution i. Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A2: Dependent variable: Enrollment effects by sector of institution and gender

Coefficients of Tuition Subsidy reform* state share of undocumented immigrants in 1990

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens (Female) | 0.0008 | 0.002 | 0.015* | 10,461 |
| | (0.006) | (0.005) | (0.008) | |
| Non Resident Aliens (Male) | 0.005 | 0.005 | 0.007 | 10,425 |
| | (0.008) | (0.007) | (0.010) | |
| Male = Female? | Yes | | | |
| **Panel B: Public 2 year colleges** | | | | |
| Non Resident Aliens (Female) | 0.020*** | 0.025*** | 0.042*** | 7,581 |
| | (0.006) | (0.005) | (0.004) | |
| Non Resident Aliens (Male) | 0.022*** | 0.026*** | 0.037*** | 7,581 |
| | (0.006) | (0.005) | (0.003) | |
| Male = Female? | Yes | | | |
| **Panel C: Public 4 year colleges** | | | | |
| Non Resident Aliens (Female) | 0.0008 | 0.007 | 0.0007 | 9,453 |
| | (0.004) | (0.004) | (0.006) | |
| Non Resident Aliens (Male) | 0.006 | 0.010 | 0.006 | 9,453 |
| | (0.008) | (0.008) | (0.011) | |
| Male = Female? | Yes | | | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The dependent variable in each panel is number of first year non resident alien undergraduates enrolled in institution i of a particular sector as a share of total number of first year undergraduates in institution i (by gender). Each cell represents the coefficient of the interaction term between tuition subsidy reform and state undocumented immigrant share in 1990, from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A3: Dependent variable: Enrollment effects by Carnegie classification institutions

Coefficients of Tuition Subsidy reform

| | (1) | (2) | Observations |
|---|---|---|---|
| **Panel A: Associate's Colleges- High transfer** | | | |
| Non Resident Aliens | 0.00005 | 0.0001 | 3,494 |
| | (0.003) | (0.003) | |
| **Panel B: Associate's Colleges- Mixed transfer/vocational & technical** | | | |
| Non Resident Aliens | 0.0025* | 0.0019 | 2,986 |
| | (0.0015) | (0.0015) | |
| **Panel C: Associate's Colleges- High vocational/technical** | | | |
| Non Resident Aliens | 0.001 | 0.0001 | 1,190 |
| | (0.002) | (0.002) | |
| Dep var mean | 0.008 | 0.008 | |
| **Panel D: Baccalaureate Colleges: Arts & Sciences** | | | |
| Non Resident Aliens | 0.0004 | 0.001 | 2,227 |
| | (0.003) | (0.003) | |
| **Panel E: Special Focus Four-Year: Health Professions** | | | |
| Non Resident Aliens | -0.018 | -0.019 | 90 |
| | (0.028) | (0.027) | |
| **Panel F: Special Focus Four-Year: Business & Management** | | | |
| Non Resident Aliens | 0.005 | 0.002 | 111 |
| | (0.011) | (0.011) | |
| **Panel G: Special Focus Four-Year: Arts, Music & Design** | | | |
| Non Resident Aliens | 0.021 | 0.028 | 372 |
| | (0.025) | (0.022) | |
| Controls | No | Yes | |
| License and financial aid policies | Yes | Yes | |
| Institution FE | Yes | Yes | |
| Year FE | Yes | Yes | |

Notes: The dependent variable is number of first year non resident alien undergraduates enrolled in institution i as a share of total number of first year undergraduates in institution i . Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A4: Multiple hypothesis testing of outcomes in Table 7 (Benjamini Hochberg False Discovery Rate)

| (Outcome) | (Estimate) | (SE) | (t) | (pval) | (Critical pval) (under BH) | (Significant) (under BH) |
|---|---|---|---|---|---|---|
| High transfer no controls | 0.027 | 0.010 | 2.74 | 0.01 | 0.028 | Yes |
| High transfer controls | 0.034 | 0.008 | 4.33 | 0.000 | 0.004 | Yes |
| Mixed transfer no controls | 0.016 | 0.005 | 3.18 | 0.003 | 0.021 | Yes |
| Mixed transfer controls | 0.017 | 0.005 | 3.3 | 0.002 | 0.018 | Yes |
| High vocational no controls | 0.027 | 0.004 | 6.77 | 0.000 | 0.007 | Yes |
| High vocational controls | 0.029 | 0.003 | 8.63 | 0.000 | 0.011 | Yes |
| Arts & science no controls | -0.003 | 0.01 | -0.34 | 0.738 | 0.043 | No |
| Arts & science controls | -0.005 | 0.01 | -0.5 | 0.618 | 0.039 | No |
| Health no controls | -0.029 | 0.038 | -0.75 | 0.493 | 0.032 | No |
| Health controls | -0.022 | 0.034 | -0.64 | 0.555 | 0.036 | No |
| Business no controls | -0.053 | 0.006 | -8.54 | 0.003 | 0.025 | Yes |
| Business controls | -0.061 | 0.005 | -11.47 | 0.001 | 0.014 | Yes |
| Arts, music & design no controls | 0.010 | 0.062 | 0.17 | 0.868 | 0.046 | No |
| Arts, music & design controls | 0.011 | 0.061 | 0.175 | 0.868 | 0.05 | No |

Notes: The table shows the results of multiple hypothesis testing of the outcomes in Table 7 using Benjamini Hochberg Step-up False Discovery rate.

Table A5: Dependent variable: Graduation effects by sector of institution

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens | 0.003 | 0.004 | 0.008 | 11,285 |
| | (0.005) | (0.005) | (0.008) | |
| Dep var mean | 0.040 | 0.040 | 0.040 | |
| **Panel B: Public 2 year colleges** | | | | |
| Non Resident Aliens | 0.002 | 0.001 | 0.004 | 8,172 |
| | (0.004) | (0.003) | (0.005) | |
| Dep var mean | 0.035 | 0.035 | 0.035 | |
| **Panel C: Public 4 year colleges** | | | | |
| Non Resident Aliens | 0.003 | 0.006 | 0.010 | 10,019 |
| | (0.004) | (0.004) | (0.007) | |
| Dep var mean | 0.038 | 0.038 | 0.038 | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The dependent variable is number of non resident aliens who graduated (with a Bachelor's or Associate's degree) as a share of total number of students who graduated from institution i . Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A6: Dependent variable: Graduation effects by sector of institution and gender

Coefficients of Tuition Subsidy reform* state share of undocumented immigrants in 1990

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens (Female) | 0.007 | 0.006 | 0.007 | 11,285 |
| | (0.009) | (0.008) | (0.006) | |
| Non Resident Aliens (Male) | 0.015 | 0.016* | 0.006 | 11,285 |
| | (0.009) | (0.009) | (0.008) | |
| **Panel B: Public 2 year colleges** | | | | |
| Non Resident Aliens (Female) | 0.018*** | 0.018*** | 0.022*** | 8,172 |
| | (0.005) | (0.005) | (0.007) | |
| Non Resident Aliens (Male) | 0.010* | 0.009* | 0.014** | 8,172 |
| | (0.006) | (0.005) | (0.006) | |
| **Panel C: Public 4 year colleges** | | | | |
| Non Resident Aliens (Female) | 0.013* | 0.018** | 0.016** | 10,019 |
| | (0.007) | (0.007) | (0.006) | |
| Non Resident Aliens (Male) | 0.014 | 0.016* | 0.016* | 10,019 |
| | (0.009) | (0.009) | (0.009) | |
| Controls | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State time trends | No | No | Yes | |

Notes: The dependent variable in each panel is number of non resident aliens who graduated (with a Bachelor's or Associate's degree) from institution i of a particular sector as a share of total number of students who graduated from institution i (by gender). Each cell represents the coefficient of the interaction term between tuition subsidy reform and state undocumented immigrant share in 1990, from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A7: Triple difference-in-differences for enrollment and graduation of undocumented students using private colleges as additional control group

Coefficients of Tuition Subsidy reform * state share of undocumented immigrants in 1990 * Public college dummy

| | (1) (All) | (2) (Male) | (3) (Female) | Observations |
|---|---|---|---|---|
| **Panel A: Enrollment** | | | | |
| Non Resident Aliens | 0.036*** | 0.038*** | 0.034*** | 27,546 |
| | (0.012) | (0.013) | (0.012) | |
| **Panel B: Graduation** | | | | |
| Non Resident Aliens | 0.026* | 0.019* | 0.031*** | 27,386 |
| | (0.014) | (0.010) | (0.005) | |
| Institution FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |
| State X Public FE | Yes | Yes | Yes | |
| Public X Year FE | Yes | Yes | Yes | |
| State X Year FE | Yes | Yes | Yes | |

Notes: The dependent variable in Panel A is number of first year non resident alien undergraduates enrolled in institution i as a share of total number of first year undergraduates in institution i (by gender).The dependent variable in panel B is number of non resident aliens who graduated (with a Bachelor's or Associate's degree) from institution i as a share of total number of students who graduated from institution i (by gender). Each cell represents the coefficient of the interaction term between tuition subsidy reform , state undocumented immigrant share in 1990, and public college dummy, from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A8: Dependent variable: Ln(Number of years of college completed)

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | (4) | Observations |
|---|---|---|---|---|---|
| **Panel A: No. of college years** | | | | | |
| LU immigrants | 0.065** | 0.056** | 0.055** | 0.067** | 13,830 |
| | (0.028) | (0.027) | (0.026) | (0.031) | |
| Foreign-born citizens | 0.008 | 0.012 | 0.011 | 0.008 | 34,868 |
| | (0.014) | (0.013) | (0.013) | (0.014) | |
| Natives | 0.003 | 0.002 | 0.002 | 0.001 | 1,116,170 |
| | (0.005) | (0.005) | (0.005) | (0.005) | |
| Individual controls | No | Yes | Yes | Yes | |
| State controls | No | No | Yes | Yes | |
| License and financial aid policies | Yes | Yes | Yes | Yes | |
| State FE | Yes | Yes | Yes | Yes | |
| Date(Year*Month)FE | Yes | Yes | Yes | Yes | |
| State time trends | No | No | No | Yes | |

Notes: Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted using person weights from IPUMS CPS. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A9: Summary Statistics of variables used in tuition and fees regression (in 2016 dollars)

| Variable | Observations | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| Public 4 year colleges | | | | | |
| In state tuition and fees | 12,708 | 6,522 | 3,156 | 1,006 | 27,205 |
| Out of state tuition and fees | 12,708 | 16,333 | 7,242 | 1,063 | 47,004 |
| Public 2 year colleges | | | | | |
| In state tuition and fees | 15,750 | 3,375 | 1,944 | 1,002 | 15,904 |
| Out of state tuition and fees | 15,750 | 7,379 | 2,979 | 1,095 | 25,395 |
| Flagship universities | | | | | |
| In state tuition and fees | 7,992 | 7,154 | 2,937 | 1,131 | 18,618 |
| Out of state tuition and fees | 7,992 | 18,532 | 6,849 | 1,559 | 47,004 |

Notes: Data is taken from IPEDS. Time period considered is 2000-2017. All values are in 2016 dollars. The sample is restricted to degree granting, Title 4 participating public institutions that have full time first time undergraduates enrolled in them. In flagship institutions,I consider those institutions with names "University of X" or "X State University"

Table A10: Dependent variable: Ln(tuition and required fees)

Coefficients of tuition subsidy reform

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| In state-Public 4 year | 0.063*** | 0.059*** | 0.055*** | 12,708 |
| | (0.012) | (0.010) | (0.010) | |
| Out of state- Public 4 year | -0.011 | -0.006 | 0.010 | 12,708 |
| | (0.012) | (0.011) | (0.014) | |
| In state-Public 2 year | 0.103*** | 0.100*** | 0.073*** | 15,750 |
| | (0.021) | (0.020) | (0.014) | |
| Out of state-Public 2 year | -0.030** | -0.029** | -0.029* | 15,750 |
| | (0.013) | (0.013) | (0.016) | |
| In state-Flagship | 0.074*** | 0.066*** | 0.055*** | 7,992 |
| | (0.014) | (0.012) | (0.013) | |
| Out of state-Flagship | 0.007 | 0.009 | 0.014 | 7,992 |
| | (0.019) | (0.019) | (0.022) | |
| Controls | N | Y | Y | |
| Institution FE | Y | Y | Y | |
| Year FE | Y | Y | Y | |
| State time trends | N | N | Y | |

Notes: Data is taken from IPEDS. Time period considered is AY 2000-01 to 2016-17. The sample is restricted to degree granting, Title 4 participating public institutions that have full time first time undergraduates enrolled in them. Each cell represents the coefficient of tuition subsidy reform from a separate regression. Dependent variable is measured in log 2016 dollars. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A11: Effects of tuition subsidy reform on marriage decisions by gender

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Single - Male** | | | | |
| Tuition subsidy reform | -0.002 | 0.0096 | 0.0093 | 73,122 |
| | (0.010) | (0.0075) | (0.0077) | |
| **Panel B: Single - Female** | | | | |
| Tuition subsidy reform | 0.014 | 0.022** | 0.024** | 61,372 |
| | (0.013) | (0.0103) | (0.0096) | |
| **Panel C: Married - Male** | | | | |
| Tuition subsidy reform | 0.004 | -0.0078 | -0.0081 | 73,122 |
| | (0.010) | (0.0070) | (0.0071) | |
| **Panel D: Married - Female** | | | | |
| Tuition subsidy reform | -0.018 | −0.026*** | −0.028*** | 61,372 |
| | (0.012) | (0.009) | (0.008) | |
| **Panel E: Divorced/Separated - Male** | | | | |
| Tuition subsidy reform | -0.001 | -0.0008 | -0.001 | 73,122 |
| | (0.0038) | (0.0037) | (0.0038) | |
| **Panel F: Divorced/Separated - Female** | | | | |
| Tuition subsidy reform | -0.006 | -0.004 | -0.005 | 61,372 |
| | (0.0073) | (0.0073) | (0.0071) | |
| Individual controls | No | Yes | Yes | |
| State controls | No | No | Yes | |
| Immigration enforcement policies | Yes | Yes | Yes | |
| State FE | Yes | Yes | Yes | |
| Time FE | Yes | Yes | Yes | |

Notes: All coefficients pertain to likely undocumented immigrants. The time period considered is 2000-2017. Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted using person weights from IPUMS ACS. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A12: Effects of tuition subsidy reform on fertility decisions among those enrolled in school

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: At least one child under age 5** | | | | |
| LU immigrants | -0.0165* | -0.0186** | -0.0189** | 26,342 |
| | (0.0085) | (0.0081) | (0.0076) | |
| **Panel B: Whether given birth to child in last 12 months** | | | | |
| LU immigrants | -0.0192** | -0.0199** | -0.0230** | 8,503 |
| | (0.0093) | (0.0095) | (0.0113) | |
| Individual controls | No | Yes | Yes | |
| State controls | No | No | Yes | |
| Immigration enforcement policies | Yes | Yes | Yes | |
| State FE | Yes | Yes | Yes | |
| Time FE | Yes | Yes | Yes | |

Notes: All coefficients pertain to likely undocumented females. The time period considered is 2000-2017. Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted using person weights from IPUMS ACS. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A13: Effects of tuition subsidy reform on household formation decisions among those enrolled in school

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Individual is never married/single** | | | | |
| LU immigrants | 0.0445*** | 0.0361*** | 0.0341** | 26,342 |
| | (0.0128) | (0.0134) | (0.0133) | |
| **Panel B: Individual is divorced/separated** | | | | |
| LU immigrants | 0.0018 | 0.0020 | 0.0028 | 26,342 |
| | (0.0038) | (0.0037) | (0.0040) | |
| **Panel C: Individual lives as head of household** | | | | |
| LU immigrants | 0.0293 | 0.0529** | 0.0594** | 26,342 |
| | (0.0264) | (0.0256) | (0.0282) | |
| **Panel D: Individual lives with unmarried partner** | | | | |
| LU immigrants | 0.0120** | 0.013** | 0.017** | 26,342 |
| | (0.006) | (0.006) | (0.007) | |
| Individual controls | No | Yes | Yes | |
| State controls | No | No | Yes | |
| Immigration enforcement policies | Yes | Yes | Yes | |
| State FE | Yes | Yes | Yes | |
| Time FE | Yes | Yes | Yes | |

Notes: All coefficients pertain to likely undocumented females. The time period considered is 2000-2017. Each cell represents the coefficient of tuition subsidy reform from a separate regression. Regressions are weighted using person weights from IPUMS ACS. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A14: Tuition subsidy reforms and migration of LU immigrants

Dependent variable: Likelihood of migrating to treated state

| | All ages | 17-28 | 14-17 |
|---|---|---|---|
| | (1) | (2) | (3) |
| Tuition subsidy | 0.00098 | 0.0012 | 0.005 |
| | (0.00060) | (0.00086) | (0.007) |
| Observations | 125,001 | 37,797 | 4,541 |
| Individual FE | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes |

Notes: Each cell represents the coefficient of tuition subsidy reform from a separate regression. Dependent variable is likelihood of moving to a treated state s at time t. Regressions are weighted using person weights from SIPP. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A15: Sensitivity of baseline estimates to alternative choices of control group

Coefficients of Tuition Subsidy reform * state share of undocumented immigrants in 1990

| | (1) Baseline estimates | (2) Drop states explicitly banning subsidy | (3) Drop states never offered subsidy | (4) Keep only treated states |
|---|---|---|---|---|
| **Panel A: College enrollment-Private** | | | | |
| Non Resident Aliens | 0.004 | 0.005 | -0.001 | 0.003 |
| | (0.006) | (0.006) | (0.004) | (0.004) |
| Observations | 10,461 | 9,322 | 6,519 | 5,380 |
| **Panel B: College enrollment-Public 4 year** | | | | |
| Non Resident Aliens | 0.009* | 0.010* | 0.004 | 0.005 |
| | (0.005) | (0.006) | (0.005) | (0.005) |
| Observations | 9,453 | 8,127 | 6,591 | 5,265 |
| **Panel C: College Enrollment-Public 2 year** | | | | |
| Non Resident Aliens | 0.026*** | 0.028*** | 0.023*** | 0.025*** |
| | (0.005) | (0.004) | (0.005) | (0.005) |
| Observations | 7,581 | 6,785 | 5,605 | 4,809 |
| **Panel D: College Graduation-Private** | | | | |
| Non Resident Aliens | 0.012 | 0.010 | 0.010 | 0.008 |
| | (0.007) | (0.008) | (0.007) | (0.007) |
| Observations | 11,285 | 9,729 | 5,292 | 3,736 |
| **Panel E: College Graduation-Public 4 year** | | | | |
| Non Resident Aliens | 0.017** | 0.018** | 0.016** | 0.015* |
| | (0.007) | (0.008) | (0.0078) | (0.008) |
| Observations | 10,019 | 8,773 | 7,020 | 5,774 |
| **Panel F: College Graduation-Public 2 year** | | | | |
| Non Resident Aliens | 0.014** | 0.015** | 0.017** | 0.017* |
| | (0.006) | (0.007) | (0.008) | (0.009) |
| Observations | 8,172 | 7,577 | 6,937 | 6,342 |

Notes: The dependent variable is indicated in the panel header. Each cell represents the coefficient of the interaction term between tuition subsidy reform and state undocumented immigrant share in 1990, from a separate regression. Regressions are weighted by how much of the total student population the institution represents, at the baseline. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

Table A16: Enrollment and graduation and fertility effects of likely undocumented students (Callaway and Sant' Anna(2021) estimates) –- Coefficients of Tuition Subsidy reform

| | (Private) | (Public 2 year) | (Public 4 year) |
|---|---|---|---|
| | (1) | (2) | (3) |
| | **Panel A: Enrollment** | | |
| Non Resident Aliens | -0.001 | 0.004 | 0.001 |
| | (0.003) | (0.005) | (0.002) |
| | **Panel B: Graduation** | | |
| Non Resident Aliens | -0.010 | 0.008 | 0.003 |
| | (0.007) | (0.005) | (0.006) |
| | (One child under age 5) | (Given birth to a child) | |
| | (1) | (2) | |
| | **Panel C: Fertility** | | |
| LU immigrants | -0.010 | -0.023* | |
| | (0.061) | (0.012) | |
| Controls | No | No | No |
| Institution/State FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

Notes: The dependent variable in Panel A is number of first year non resident alien undergraduates enrolled in institution i as a share of total number of first year undergraduates in institution i. The dependent variable in Panel B is number of non resident aliens who graduated (with a Bachelor's or Associate's degree) as a share of total number of students who graduated from institution i . The dependent variable in Panel C is probability of LU immigrant having at least one child under age 5/ whether the LU female has given birth to a child in the last year. Effects are derived using the doubly-robust estimator of Callaway and Sant'Anna (2021) and display the ATT for all groups across all periods. The comparison group is the set of never treated institutions/states. Each cell represents the coefficient of tuition subsidy reform from a separate regression. In panels A and B, regressions are weighted by how much of the total student population the institution represents, at the baseline. In panel C, they are weighted using person weights from IPUMS USA. Robust standard errors are clustered at the state level and are shown in parenthesis. ***, **, * represent significance at 0.01,0.05 and 0.10 level respectively.

# Appendix A

The broad major categories shown in Table 8 and their constituent fields from IPEDS is given below.

**Arts & humanities**- Foreign languages and literatures, English Language and Literature/letters, Philosophy and Religion,Visual and Performing Arts.

**Business**- Business management and administrative services.

**Health & Medicine**-Health professions and related sciences.

**Multi/Interdisciplinary studies**- Area, ethnic and cultural studies, Marketing operations/Marketing and distribution, Home economics general, Vocational home economics, Liberal arts and studies, General sciences and humanities,Multi/interdisciplinary studies, Parks, recreation, leisure and fitness studies.

**Public and social services**- Law and legal studies,Military technologies, Theological studies and religious vocations,Protective services, Public administration and services.

**Science, Math & Technology**- Agricultural business and production, agricultural sciences, Conservation and renewable natural resources, Architecture and related programs, Communications technologies, Computer and information sciences, Engineering, Engineering related technologies,Biological sciences/life sciences, Mathematics, Physical sciences, Science technologies.

**Social Sciences**- Communications, Education, Library science, Psychology, Social sciences and history.

**Trades & personal services**- Personal and miscellaneous services, Construction trades, Mechanics and repairers, Precision production trades, Transportation and material moving workers.

# Appendix B

The below list provides an illustrative sample of 'more competitive' and 'less competitive' institutions, as per my IPEDS sample and Barron's 2008 rankings

**More competitive**

University of California-Los Angeles

University of Connecticut

University of Florida

Ohio State University

University of Georgia

Clemson University

University of Michigan-Ann Arbor

University of Kansas...

**Less Competitive**

University of Delaware

Northern Arizona University

Northwestern State University of Louisiana

University of Minnesota-Morris

University of Northern Iowa

University of Houston-Downtown

Sam Houston State University

Washington State University

California State University- Bakersfield.....