# Education, Fertility and Family Formation effects of State Tuition Benefits : Evidence from Tuition Subsidies to Undocumented Students

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**Abstract**

*Since 2001, twenty-two US states have allowed lower tuition rates for undocumented students at public colleges. I look at how the degree of exposure to the policy affects novel institution-level education outcomes of these students and the unintended negative spillover effects on other students. Exploiting state-time variation in these policies in a difference-in-differences framework and comparing within institutions, I find higher enrollment and graduation among likely undocumented students in less selective community colleges, in states with high pre-policy levels of undocumented immigrants. This is supported by their increased enrollment in high-transfer, technical and vocational colleges, as per the Carnegie classification. My results also indicate that an increasing number of these students graduate in health & medicine, trades & personal services in more policy-exposed states, consistent with their occupations. There seems to be negligible displacement of domestic students in community colleges in more policy- exposed states. Due to the higher in-state tuition charged by both public four-year and two-year colleges as a result of the policy, around 16% of the average annual subsidy provided to undocumented students is borne by other students in the form of higher tuition. The likely undocumented female students also respond to their increased educational attainment by reducing their fertility, driven by delayed marriage and household formation decisions. Using individual longitudinal data from the Survey of Income and Program Participation (SIPP), I find no substantial migration of likely undocumented students to the treated states to take advantage of the policy. Overall, my findings indicate that the education and fertility benefits to likely undocumented students dominate any unintended spillover effects on non-targeted students.*

[1‡] I am grateful to David Albouy, Alex Bartik, Ben Marx, George Deltas, Mark Borgschulte, Richard Akresh, Rebecca Thornton ,Russell Weinstein and other seminar participants for their helpful comments regarding this paper. All errors are my own.

# 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 policy 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 college enrollment of undocumented students. However, less is known about their direct effects on college retention or time to college graduation. The heterogenous effects on enrollment and graduation by detailed categories of institutions as well as the choice of major of these likely undocumented students also lack empirical support[4]. Moreover, we do not have any causal evidence as to how these lower college costs affect the fertility decisions of eligible undocumented students 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 the effect of the tuition subsidy reforms on college enrollment and college retention of likely undocumented students.[5] I also examine whether this policy leads states to charge higher

[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]By detailed categories of institution, I mean public vs. private colleges, 2 year vs 4 year colleges and the Carnegie classification system of colleges.

[5]In individual-level data, I consider Mexican non-citizens who enter US within 14 years of age as a proxy for undocumented students likely to be affected by the policy. In contrast, in case of institution-level data, I focus on non-resident alien undergraduates in states with higher pre-policy undocumented immigrants as a proxy for undocumented students more exposed to the policy.

tuition for all students, thereby reducing the net benefit for undocumented students. I further explore whether this policy affects the fertility and living arrangement choices of eligible undocumented individuals. Starting from 2001 to date, 22 out of 50 US states have allowed tuition subsidies for undocumented immigrants in public colleges and universities. I exploit the staggered roll-out of these policies across states and time and use a difference-in-differences design to answer the above research questions. The primary sources of data used in my analysis comprise of the monthly individual data from Current Population Survey, yearly individual data from American Community Survey and annual institution data from Integrated Post-secondary Education Data System (IPEDS).

Policies providing tuition subsidy to undocumented students can have both direct and spillover effects. First, they may influence college entry decisions of undocumented immigrants. While a number of papers have examined this issue, they have not been able to provide a definite answer to it. They have also ignored the detailed institution level enrollment effects and the choice of major of undocumented students, which can have important labor market implications. Kaushal (2008), Dickson and Pender(2013) Amuedo-Dorantes & Sparber(2014)and Koohi(2017) find evidence that these policies increase college enrollment of undocumented students in the states offering them, whereas Chin and Juhn(2010) find no significant impact on college attendance. In light of this, I start by analysing long run effects of the policy on college enrollment decisions of undocumented youth which can differ from short run effects as examined in the existing literature. [6] My results show that likely undocumented students are 3.6 percentage points more probable to attend college in the treated states. These estimates are consistent with the results of prior literature (Amuedo-Dorantes and Sparber (2014), Kaushal (2008)). More importantly, I explore the enrollment effects of this policy by type of institution, which the state level estimates could not capture. In particular, I find a 0.003 unit (base mean-0.018) increase in enrollment of first year NRA undergraduate students as a proportion of total number of first year undergraduate students in public 2 year colleges due to a 1 SD increase in the degree of

[6]Eight additional states have started offering these subsidies to illegal immigrants in the extended time period I consider.

policy exposure in treated states. These undergraduates appear to have higher enrollment in high transfer and technical & vocational schools. They are more likely to choose health & medicine, social sciences, trades & personal services as their majors, consistent with their higher shares in construction, transportation, health, education and food service related occupations.

Second, there is little causal evidence to show how these policies affect college retention and completion of undocumented immigrant youth.[7] This question is worth empirical analysis because effect of the policy on college graduation of undocumented students is theoretically ambiguous. On one hand, the lower price of education makes college more affordable for these students 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 be partially responsible for lower retention rates among undocumented students.

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 same incentive to complete their college education as compared to other legal students. This may be because their returns to education are lower in terms of weak labor market opportunities as they can be deported. They may thus respond less to changes in college prices and be less likely to graduate from college, relative to documented immigrants and natives.[8]However, in a descriptive study of un-

[7]There are two exceptions. The first is Conger and Turner (2017) who examine the effect 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 US which have adopted the reforms. The second is Dickson and Pender(2013) who find no significant difference between Texas citizens and non-citizens in retention rates from first year to second year as a result of the policy. Their paper, only considers the state of Texas and all non-citizen students, rather than undocumented students in particular, who are enrolled in public universities of Texas. Their estimates may not provide a comprehensive and externally valid picture of the college completion effects of the policy.

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

documented students enrolled in undergraduate colleges, Suarez-Orozco et al (2015) find that they demonstrated high levels of academic resilience. The above reasons underscore the importance of studying long run effects of the subsidy policy on undergraduate college completion rates of undocumented students. I find that likely undocumented youth are 3.2 percentage points more probable to attain their college degrees in treated states. At an institution level, the policy leads to a 0.0028 unit (base mean-0.035) increase in graduation of NRA undergraduate students as a proportion of total number of outgoing undergraduate students in public community colleges due to a 1 SD increase in the degree of policy exposure in treated states.

The results which are from Current Population Survey data, are also confirmed using the larger, nationally representative sample survey - American Community Survey.[9]

Third, households may respond to higher educational attainment of likely undocumented females due to the policy by changing their fertility and household formation decisions. A simple plot of the raw data shows that 17-28 year old Mexican non-citizen eligible females have much higher probability of giving birth to a child as compared to a similar group of Hispanic foreign born female citizens. We also observe a steady decline in the likelihood of giving birth of these Mexican non-citizen females over the period 2000-2017 (see figure I). Similarly, we also notice a steady increase in the likelihood of remaining childless for these Mexican non-citizen females during the period 2000-2017 (see figure II). This pattern in the raw data motivates me to explore whether the tuition subsidy policy plays a role in reducing fertility among likely undocumented females eligible for the policy. Additionally, I want to understand whether the decline in fertility of undocumented females is marital or non-marital. Finally, I want to explore what types of living arrangements these undocumented females choose, consequent to these reduced fertility rates.

To my knowledge, this is one of the first papers to look at the effects of education on long run fertility using the tuition subsidy policy. In addition, this is the first paper to find suggestive evidence of mechanisms driving the fertility decisions of likely undocumented

[9]I get similar results when I use only the observations for which an individual was sampled for the first time in CPS.

females, who have had significantly higher fertility rates over the decades as compared to foreign born citizens and natives. My findings indicate that undocumented females in my sample are 1.8-1.9 percentage points less likely to have a child under age 5 in their household in the treated states. They are 2.2-2.6 percentage points less likely to give birth to a child within the past year 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 policy. We can also think of it as the higher opportunity cost of time devoted to child rearing activities due to 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 marriage and forming their own households.

One possible unintended effect of this policy can be that such policies hurt natives and legal immigrants by reducing their enrollment slots, as often argued by the opponents of tuition subsidies. In fact, if the number of seats in colleges is rigid, an increase in enrollment of undocumented students will result in lower enrollment of legal immigrants and US natives. In view of the above claim, I explore whether the policy has any substitution effects on documented and native students. My results show there is almost no displacement of native students in community colleges of treated states more exposed to the policy. This can be explained by the fact that community colleges were not operating at capacity prior to the policy, and hence possibilities of displacement of native students do not arise. Another potential unintended effect can be states charging higher tuition and fees as a result of providing these subsidies. Using institution level panel data from IPEDS, I find significant increase in instate tuition in both public 4 year and public 2 year colleges of treated states. In particular, the policy leads to a 6% increase (average increase of $391) and 10% increase (average increase of $246) in annual instate tuition in public 4 year and 2 year colleges respectively. These increases are particularly pronounced amongst public flagship universities (average increase of $472). In essence, around 16% of the average annual subsidy provided to undocumented students is borne by all students in the form of higher tuition.

In sum, the results of this paper support the long run effectiveness of the policy in increasing college enrollment and college completion rates among eligible undocumented students. Additionally, the policy has positive indirect effects beyond education on fertility and household formation choices of likely undocumented students. However, these benefits come at the cost of unintended higher tuition for other groups of students.

My results can help answer some policy relevant questions. First, how did the tuition subsidy policy affect the educational attainment of undocumented youth beyond the first order effect of higher college enrollment? In fact, the low educational attainment of Mexican non-citizens as compared to other immigrants is a policy concern. It is important to understand whether we can improve their low education levels by reducing the price of education. Second, how did undocumented students respond to this policy through their choice of majors? This has implications for gender and racial inequality in labor market earnings. Third, do public schools respond to this tuition subsidy policy by increasing the price of education for all their students? If this is the case, then there is not complete pass through of tuition benefits to undocumented students. These students then face lower benefits than the actual amount under the policy. Fourth, the fertility effects of increased educational attainment of a disadvantaged group has important implications. As these likely 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, policies promoting education of those at the lower tail of the education distribution 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 discuss the policy background of state level tuition subsidy laws for undocumented immigrants. Section 3 is a review of related literature. Section 4 describes the data and estimation strategy used in the analysis. In section 5, I present my results. Section 6 shows a number of robustness checks and heterogeneity analyses. Section 7 concludes.

# 2 Policy Background

There were 12 million unauthorised 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 unauthorised immigrants,39% were in the age group of 18-34 years and can potentially be affected by the tuition subsidy policy. Because US has a large number of illegal 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 till high school. However, this case did not give 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 illegal 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 securing funds to cover their costs of college. (Perez et al, 2010; Suarez- Orozco et al, 2015). In many states, these illegal 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 unauthorised students. [10] However, these stu-

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

dents are banned from tuition subsidies in states like Alabama, Arizona, Georgia, Missouri, South Carolina and Indiana. [11]

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 policy 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).[12]

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 policy. 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).[13] Among states adopting the tuition subsidy policy, 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 institutions, the subsidy was lowest at $743 in Minnesota and highest at $8339 in Connecticut.[14]

# 3 Related Literature

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

[11]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.

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

[13]I have computed the tuition subsidy as difference between out-of-state tuition and required fees and in-state tuition and required fees. Figures are in current dollars.

[14]These state level figures of tuition subsidy are for 2016-17 AY. See figure A2 for state level variation in sticker price tuition subsidies in 2016-17.

level programs (Dynarski(2000); Cornwell, Mustard and Sridhar (2006); Kane(2003); Kane (2007); Abraham and Clark (2006)) as well as national level programs (Seftor and Turner (2002); Turner (2017); Carruthers and Welch (2015); Denning, Marx and Turner (2018); Bednar and Gicheva (2013)).[15] Similar to these studies, my analysis uses a quasi experiment in the form of lower tuition rates offered to undocumented students by different states in different years. As suggested by these studies, I also find higher college attendance among the recipients of resident tuition (i.e. undocumented students) in treated states. My results seem to suggest that these undocumented students do not get the entire benefit from the tuition subsidies as some colleges raise sticker price tuition rates in response to the policy. This is consistent with Turner(2017)'s result. Both Turner(2017)'s and mine paper indicate that recipients of financial aid programs do not receive complete benefit of the programs.

The spillover effects of tuition subsidy policy on documented immigrants and US natives that I consider in this paper contribute to the existing studies that look at the crowding out effects of domestic students by foreign students (Hoxby (1998); Borjas(2004); Gould et al (2009); Bound, Turner and Walsh(2009); Machin and Murphy (2017); Shih (2017)).

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 passage of the laws for undocumented immigrants to avail of it. Using data from Current Population Survey outgoing rotation groups over the period 1997-2005, Kaushal (2008) however finds that the tuition subsidy policy 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 treated states. Using administrative data from five universities in Texas, Dickson and Pender(2013) find that a tuition

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

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).

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 selection criteria of undocumented immigrants who are most likely affected by the policy.

It is also important to note that none of these papers look at college retention and tuition related outcomes of likely undocumented youth as a result of the tuition subsidy policy. Moreover, they do not look at the enrollment or completion effects of the policy at a detailed institutional level. Nor do they look at the field choices of these undocumented students. As these outcomes are important to correctly evaluate the benefits and costs of the policy, my paper fills in the gap in the literature.

The paper which is most closely related to my analysis is Amuedo-Dorantes and Sparber(2014). They use monthly CPS data and the time period spanning from 1999 to 2012 to find that the policy increases college enrollment probability of likely undocumented students by 4 percentage points. However, my study differs from the above paper in the following main respects. First, I extend their analysis on college enrollment for a longer time period (1999 to 2017) and take into account the effect of passage of full set of tuition subsidy laws in all the 22 states till date. Second, I examine the enrollment effects of the policy at a school level according to the following categories - public, private, public 2 year, public 4 year and Carnegie classification schools which reveals important heterogeneities, not captured in their state level estimates. Third, I study how policy affects field of major of likely undocumented students, using institution fixed effects which provides suggestive evidence of how changing price of education alters field of study. Fourth, I study the effects of the policy on institution level college retention and college completion rates of likely undocumented students which is not addressed in their paper. Fifth, using nationally representative data from IPEDS, I look at the effect of this policy on tuition rates in public 2 year and 4 year colleges to explore the extent of pass-through of the policy on

the demand side. Finally, I examine heterogeneous effects of the policy across states using synthetic control approach, which is not done in any of the papers mentioned above.

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 paper differs from these papers in the following important way. While these papers study policy changes that have increased access to primary and secondary schooling for women, I focus on an educational reform that has raised access to post-secondary education for undocumented women. This is a different reform from the ones considered in fertility literature because it made college more affordable for those at the lower tail of the education distribution. In contrast, most of the papers in existing fertility literature look at non-financial incentives that increase individuals' number of years of education. Moreover,these papers focus on teenage pregnancy whereas I explore fertility effects during the ages of 17-28 years when the undocumented women are most likely to be pursuing their undergraduate degree. I 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 policy on teenage childbearing is Koohi (2017). She finds negative effects of the policy on probability of giving birth before age 19 for Mexican non-citizens. She argues that Mexican non-citizen teenagers are sufficiently forward looking and reduce their teenage fertility rates in response to increased future economic opportunities provided by lower tuition rates. She only focuses on the probability of first birth of Mexican non-citizen females during their teenage years whereas I consider the fertility effects on potentially undocumented females between 17-28 years who are enrolled in college and eligible for the tuition benefits. Additionally, I try to understand the marriage and living arrangement patterns that can explain the

delayed childbearing of undocumented females.

# 4 Data and Methodology

## 4.1 Data

I primarily use three datasets to explore the intended and unintended education and fertility effects of tuition subsidies to undocumented students. First, I use monthly data from Current Population Survey (CPS) covering the period July 1999-December 2017 to examine the college enrollment, college retention and inactivity outcomes.The CPS contains information on the outcomes of college enrollment, college graduation with an Associate's or Bachelor's degree, number of years of college credit and neither working nor enrolled in school i.e. inactivity along with demographic information such as gender, age and race. One advantage of CPS is that it provides monthly variation on schooling related variables.[16] I also qualitatively confirm the results from CPS data using annual data from American Community Survey (ACS) for the period 2000-2017. Additionally, I perform institution level analysis using IPEDS data to capture the heterogenous enrollment and graduation outcomes by various categories of institutions, which are not reflected in the state level analyses using CPS and ACS data. For the fertility outcomes, I use US Census 2000 (5% sample) and American Community Survey (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

[16]Utilising the rotating panel design of the CPS, monthly variation in the data can be useful to ascertain whether a student drops out in the middle of the semester. Another reason why monthly data may be important is when I conduct robustness checks in section 6.3 of my paper.I find that the policy has larger impact on college enrollment of likely undocumented students when I drop summer months when enrollment tends to be low. Similarly, when I keep only summer and winter months when most of the students graduate, I observe larger policy effects on college completion rates of these immigrants. In sum, the in and out dynamics of students from their schools during the year can be captured with monthly variation in school variables.

of immigration (for immigrants). The larger sample size of the ACS makes it preferable over CPS for analysing fertility and household formation outcomes where I do not require monthly variation in data for my research question. Similar to most other public use surveys, CPS and ACS do not include information on the legal status of the respondents. Hence, following Amuedo-Dorantes and Sparber (2014), I use Mexican non-citizens as a proxy for immigrants who are most likely to be unauthorised (see Figure 3). I further consider only Mexican 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 policy. [17]

For the institution level regressions, I consider 'Non-Resident Alien' in treated states with higher pre policy number of actual undocumented immigrants as a proxy for undocumented students. The idea is that, timed with the policy, more undocumented students among the larger group of 'Non-Resident Aliens' will settle in those treated states having higher pre-treatment actual number of undocumented immigrants. 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. This measure includes all nationalities of undocumented immigrants, thus leading to a better measure of undocumented status than Mexican/Hispanic non-citizens which is currently used in literature. Note that, according to figure 3 of my paper, the proportion of Mexicans among undocumented immigrants has been declining since 2000 due to more deportations. I believe this is the best proxy available in a public survey based dataset to capture undocumented status. [18]

I combine individual level micro data from CPS with state level data on the enactment

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

[18]See appendix figure A3 and A4 for a comparison between proxies used for undocumented status and the actual number of undocumented youth as reported by DHS

dates of the tuition subsidy policy for unauthorised students. This state level data has been obtained from National Conference of State Legislatures (NCSL) and ULead Network. I include binary controls for two state level policies - financial aid and driver's license policy for unauthorised immigrants that are considered important predictors of their education outcomes. [19] I also include dummies for state level immigration enforcement policies - 287(g) agreements, E-verify mandates, omnibus immigration laws in the fertility and family formation specifications. 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). In addition to the policy variables, I include the macroeconomic variables: state unemployment rates, per capita personal income (in 2017 dollars)/median household income (in 2017 dollars), state median house value and proportion of people below the poverty line. [20] Unemployment data was obtained from Bureau of Labor Statistics while income and poverty data were taken from Bureau of Economic Analysis. State level median house prices were collected from Zillow.

For college enrollment outcome, I focus on individuals aged 17-28 who have completed high school but have not yet obtained a Bachelor's degree. For college retention outcome, I limit the sample to individuals between 17 and 28 years old who have completed high school. For fertility and household formation outcomes, the sample consists of 17-28 year old high school graduates. In Panel A of table 2, I show the weighted summary statistics of the variables used in my regression analysis for three disjoint groups- likely unauthorised immigrants (Mexican non-citizens), Foreign born citizens and US natives. Panel B gives a sense of how tuition subsidies to Mexican non-citizens affect their long run outcomes through summary statistics on educational attainment, employment rates and family in-

[19] 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. Also see "Drivers' license reforms and job accessibility among undocumented immigrants" by Heepyung Cho(2018).

[20] The fertility and household formation regressions include all the above mentioned macroeconomic variables whereas the education regressions include only the state unemployment rates.

come of 30-45 year olds 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 unauthorised 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 income. Thus, it suggests that they are financially constrained to bear the costs of higher education. It remains to empirically examine whether these tuition subsidies have benefited them by improving their access to higher education.

In table 3, I show the weighted summary statistics of some fertility and living arrangement variables for the above three 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 as 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 of the role of education reform in bridging the fertility gap between undocumented and documented immigrants.

Second, to examine whether the policy affects the sticker price cost of college,I use annual data from Integrated Postsecondary Education Data System (IPEDS) spanning the academic years 2000/2001-2016/2017 to create a institution level panel dataset. IPEDS provides us with detailed information on both instate and out-of-state tuition and total cost of attendance for all degree granting public 2 year and 4 year institutions that have full time undergraduates enrolled in them. I have taken the voting data, which is an explanatory variable in these regressions, from the Office of the Clerk, US House of Representatives. [21]

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

## 4.2 Methodology

### 4.2.1 Difference-in-differences

I want to estimate the causal effects of tuition subsidy for undocumented students on education, fertility and household formation outcomes. I define treatment and control groups according to when the policy was implemented in the state of current residence of an individual. For college enrollment regressions, Mexican non-citizen high school graduates without a Bachelor's degree living in states which have adopted the policy form the treatment group. On the other hand, for college completion regressions, the treatment group is comprised of Mexican non-citizen high school graduates aged 17-28 years in states implementing the policy. Finally, for fertility 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 form the treatment group. For all the above regressions, similar individuals residing in states without the policy form the control group. I run separate regressions for likely undocumented immigrants, foreign born citizens and US natives. Foreign born citizens and US natives are not eligible for this policy and hence they can potentially serve as placebo groups. All regressions weight the observations by person/institution weights, include state and time fixed effects, control for financial aid and driver's license policies (for education outcomes) or immigration enforcement policies (for fertility and household formation outcomes) and cluster the standard errors by state.

My main identification strategy relies on the state time variation in adopting the law. The basic difference-in-differences regression model is given by:

$$Y_{ist} = \alpha + \beta D_{st} + \gamma X_{ist} + \theta Z_{st} + \delta_s + \phi_t + \epsilon_{ist} \tag{1}$$

Observations refer to individual i living in state s at time t. Y is the outcome of interest. $D_{st}$ is a binary variable that takes the value 1 if state s offers in-state tuition rates to unauthorised students at time t and 0 otherwise.[22] X includes individual level characteristics

[22]I consider D to be 1 from the date the law became effective in each state. I also drop observations of New York from July 1999 through October 2001 because the State University of New York (SUNY) and

such as age, indicator variables for gender and race and a continuous variable measuring the number of years a person has stayed in United 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.[23] As discussed in the data section, I also control for whether a state offers financial aid and drivers' licenses to undocumented immigrants through two separate binary variables. Equation 1 also includes state fixed effects $\delta_s$ and time fixed effects $\phi_t$. Finally, $\epsilon_{ist}$ represents the error term. The parameter of interest is $\beta$ which measures the causal impact of tuition subsidy policies to undocumented youth on education, fertility, marriage and living arrangement outcomes.

For the institution level regressions on undocumented students' enrollment, graduation rates and choice of major, I use the following difference-in-differences specification:

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

Here, i denotes institution, s state and t year. $U_{os}$ denotes the share of undocumented immigrants in state s in the baseline period 1990. This data on undocumented immigrants is obtained from Department of Homeland Security. [24] Y is number of first year 'Non Resident Alien' undergraduates enrolled in institution i as a fraction of total number of first year undergraduate enrollment in institution i. [25] The above equation 2 helps me to compare outcomes Y between institutions in treated states vs. control states, before vs. after the policy. It also enables me to compare the heterogenous effects of the policy

City University of New York (CUNY) had different policies on tuition rates for undocumented immigrants during this period.

[23]The controls included in Z are calculated as three year moving averages to avoid being endogenous to the policy.

[24]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.

[25]I only consider Title IV participating and degree granting institutions that have full time undergraduates enrolled in them.

in states with more high undocumented immigrants. All the other variables are same as equation 1. [26] Additionally, equation 2 also includes the institution fixed effects, $\psi_i$. The coefficient of interest is $\gamma$ which measures the causal effect of policy on outcomes Y in treated states with higher proportion of pre-treatment undocumented immigrants. Thus, in my case, the policy dummy interacted with pre-treatment share of actual undocumented immigrants in a state captures the degree of exposure of the treated state to the policy, and not just the presence or absence of the policy in the state. The institution level regressions are weighted by how much of the total student population the institution represents.

An obvious concern with using difference-in-differences model presented in equation 1 is the potential for different trend in outcomes between treatment and control states before the policy change.If this is the case, then we will have biased estimates of $\beta$. Equation 1 is estimated under the assumption that the policy was exogenous in each treatment state. To provide evidence for that, I test for the existence of confounding pre-trends through an event study analysis as specified in the below two equations. The first specification is for institution (i) in state (s) at time (t):

$$Y_{ist} = \alpha + \sum_{n=-5, n\neq -1}^{5} \beta_n D_{st}^n + \sum_{n=-5, n\neq -1}^{5} \gamma_n D_{st}^n * U_{os} + \theta X_{st} + \psi_i + \delta_s + \phi_t + \epsilon_{ist} \quad (3)$$

The second specification is for residents (i) of states (s) at time (t):

$$Y_{ist} = \alpha + \sum_{n=-5, n\neq -1}^{5} \beta_n D_{st}^n + \gamma X_{ist} + \theta Z_{st} + \delta_s + \phi_t + \epsilon_{ist} \quad (4)$$

Here, n can be considered as the event time i.e. the number of years before or after the policy was implemented with n=0 representing the year the policy 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. [27]The coefficients $\beta_{-5}$ to $\beta_{-2}$ can help to determine whether the trends in various outcomes is significantly different between

[26] $X_{st}$ 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.

[27] I bin up the end points (-6 and 6) which are years outside my event window.

the treatment and control states. The coefficients ($\beta_1$ to $\beta_5$) are included to see whether the effects of the policy persist over time.

It is possible that the difference-in-difference estimators may be upward biased because of pre-existing trends that are correlated with the tuition subsidy policy. Hence, I also carry out triple difference regressions for the fertility outcomes using Hispanic citizens as additional control group. In other words, I compare likely undocumented immigrants with Hispanic citizens, in treated vs. control states, before vs. after the policy.

$$Y_{icst} = \alpha + \beta D_{st} + \gamma X_{icst} + \theta Z_{st} + \delta_{cs} + \phi_{st} + \omega_{ct} + \epsilon_{icst} \tag{5}$$

Here, c refers to Hispanic citizens. I include a full set of two way fixed effects- state by citizenship fixed effects $\delta_{cs}$ , state by time fixed effects $\phi_{st}$, citizenship by time fixed effects $\omega_{ct}$. These fixed effects are likely to absorb most of the pre-existing trends and unobserved factors that may bias the difference-in-difference estimates.[28]

### 4.2.2 Synthetic control analysis

I analyse heterogenous effects across the treated states by using the synthetic control approach developed by Abadie and Gardeazabal (2003) and Abadie et al (2010). In this study, the idea of the approach is to evaluate the impact of tuition subsidies to undocumented students in one particular treated state by constructing a "synthetic" counterfactual state using a weighted convex combination of potential control states. My statistical inference is based on Abadie and Gardeazabal (2003) and Abadie et al (2010)'s idea of a placebo test. The idea is that each of the states in the control donor pool is assumed to be treated and each of these treatment effects is estimated by synthetic control approach. This inference method allows us to compare between the estimated treatment effect for the actual treated state and the distribution of the estimated treatment effects from the placebo test. Under the null hypothesis of no intervention, the estimated treatment effect of the actual

[28]I have checked that the policy has no effect on Y for Hispanic citizens, hence they may potentially be considered as valid additional counterfactual.

intervention should not be very different from the distribution of placebo treatment effects. The diagrams showing different treatment effects across the treated states using the above method are presented in the next section.

# 5 Results

## 5.1 College enrollment

This section discusses the relationship between tuition subsidy reforms and college enrollment rates.[29]

First, I present results of the event study analysis as specified in equation 3.[30] Figure 4 shows the effects of tuition subsidy reform on enrollment of NRAs in public 2 year colleges of treated states more exposed to the policy for 5 years before and after the policy. The figure shows the estimated coefficients of $\gamma$ from equation 3 along with their 95% confidence intervals. I observe all of the coefficients prior to the reform to be insignificant. There seems to be no confounding pre-trends, thereby supporting my identification strategy. [31] Enrollment starts to rise for NRAs in high undocumented immigrant treated states after the policy was implemented. This effect seems to be persistent over time. This lends support to my belief that this policy was successful in raising college enrollment rates among the target group.

I next show the results from estimation of equation 1 in panel A of Table 4. Each cell in Table 4 shows the estimated coefficients of $D_{st}$ from a separate regression. Column 1 shows the results from a baseline regression with only the state and date fixed effects and other

[29]Exploiting the rotating panel structure of CPS, there seems to be many transitions of individuals from 'not-in-college' to 'in-college' after the 8 month break, in the treated states. Similar transitions are not observed in the control states.In fact, dropouts were common in control states during the 8 month "out phase". Thus, it is possible that introduction of subsidies cause individuals in treated states who had forgone education when young, to continue their college when older. Details available on request.

[30]See Table A1 for the event study estimates.

[31]As I support my identification strategy with event studies that show the absence of pre-trends, I do not use propensity score matching which mainly addresses the problem of confounding pre-trends.

policies. Columns 2 and 3 add individual level control variables and state level covariates respectively to the baseline model. Column 4 further adds state specific time trends.

In Column 1, I find that likely undocumented students are 3.9 percentage points more likely to be enrolled in college in states offering tuition subsidies to undocumented immigrants. Given that the average enrollment rate of these unauthorised immigrants is 19% (Table 2), the policy can increase this group's enrollment rate by approximately 21%. This effect persists in the presence of individual and state level controls in Columns 2 and 3. Hence, providing these tuition subsidies seems to significantly increase the enrollment of likely unauthorised immigrant youth in undergraduate colleges. These enrollment effects appear to come from higher enrollment of these youth in public 2 year colleges (see table 5). In panel B, the standardised estimates suggest a 0.003 unit increase in enrollment of first year NRAs as a share of total number of first year undergraduate students (base mean: 0.018)in public community colleges due to a 1 SD increase in degree of exposure to policy in the treated states. These effects are quite robust and persist even when state time trends are controlled for. In contrast, I do not find much evidence of increase in enrollment of these undocumented students in public 4 year colleges. There is no effect on entry of undocumented students in private colleges which were not subject to the policy.

When I look at the disaggregated enrollment effects as per the Carnegie classification of institutions, 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 high undocumented immigrant treated states (see table 6). I observe no significant enrollment effects in Baccalaureate Arts & Science colleges. However, for 4 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. Thus overall, it seems likely undocumented students 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. While Dorantes and Sparber (2014) estimate effects in the range of 3.7-4.2 percentage points , my esti-

mates are very close and lie between 3.6-3.9 percentage points. However, my estimates are slightly larger than Kaushal (2008)'s estimates of 2.5 percentage point increase in college enrollment.

Row 3 of panel A table 4 shows that this policy 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, Column 3 of row 3 suggests that enrollment of natives rises by 0.7 percentage points due to the policy (2 % increase from the base enrollment of 42%) . Finally, even though a reduction in enrollment of foreign born citizens is observed from row 2, these effects are not statistically significant in any of the three preferred specifications.[32] This result stands in contrast to Dorantes and Sparber(2014)'s paper which indicates that Hispanic foreign born citizens face a strong adverse impact on their college enrollment as a result of this policy. The enrollment results are not driven by state time trends as shown in column 4. However, *in contrast* to the results obtained using surveys like CPS and ACS, my institution level regression from IPEDS indicates a strong displacement of domestic students in public 2 year colleges of treated states with a high degree of exposure to the policy. Thus, capturing within institution variation in entry may reveal important heterogeneities which are not observable in within state estimates done in literature.[33]

Figure A5 shows the college enrollment trends along with the corresponding estimated synthetic counterfactual for each of the treated states. In most of the states, we seem to find a rise in college enrollment rates after the reform.[34] This is particularly true for states that have introduced the policy in early 2000, like Texas, California, Utah, Washington, Illinois and Oklahoma.

[32]Using back of the envelope calculations, I find that for increase in enrollment of 1 LU student,0.23 foreign born citizens are displaced because of the policy. Thus, this reform causes a less than 1 for 1 crowd-out of foreign born citizens using within state variation.

[33]Table A4 shows the multiple outcomes corrected p values of table 6 outcomes using Benjamini Hochberg False Discovery Rate

[34]Note that when undocumented students faced a tuition fee hike in CUNY in 2002, their college enrollment decreased.

In table A16, I report the results from ACS data which are similar to the CPS results. In particular, likely undocumented students are 2.4 percentage points (13%) more probable to attend college in the treated states.

## 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. A rational individual will choose a major based on his expectations of labor market returns, among other factors. Undocumented immigrants have more uncertain employment opportunities even with DACA, as compared to documented immigrants and natives. Hence, their choice of major may be different from other citizens or legal non-citizens. It is important to consider how increased access to post-secondary education affects the field choice of undergraduate likely undocumented students.

Table 7 shows the coefficients of policy reform interacted with pre-treatment share of undocumented immigrants on the share of NRA undergraduates who majored in a program, as per equation 2. The data used for estimating this reduced form specification is from IPEDS. [35] The broad fields that 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, Trade and personal services.[36]. The NRAs in treated states which are more exposed to the policy prefer to major in Health & Medicine, Social Science, Trades and Personal services and Business fields. This seems consistent with their predominance in healthcare/social assistance,construction, transporta-

[35]Table A5 shows the multiple outcomes corrected p values of table 7 outcomes using Benjamini Hochberg False Discovery Rate

[36]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 CIP 1990, 2000, 2010 classification,is made consistent over the years using crosswalks obtained from NCES website. I have considered only primary majors in Associate's and Bachelor's degree in a public institution.

tion/warehousing, accomodation/food services occupations.

## 5.3 College retention and graduation

In this section, I first discuss the relationship between tuition subsidy reforms and college graduation rates. I consider an individual as college graduate if he holds an Associate or Bachelor's degree.

The results of the event study analysis for institution level college graduation rates are presented in figure 5.[37] All of the coefficients before the reform are insignificant. Thus my identification strategy is supported with little evidence of confounding pre-trends in college graduation rates. Moreover, the graduation rates begin to steadily increase after the policy was implemented.Thus, it appears that the policy not only raises likelihood of college enrollment as a first order effect but also increases likelihood of college completion of likely undocumented students. The policy 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. The increased retention of these likely undocumented students may be due to self selection of higher ability students into colleges after the reform. It may also be because of hiring better quality and experienced teachers. Note that for number of college years completed, the event study (figure 6) shows the absence of pre-trends , also supporting my identification strategy. The point estimates and confidence intervals do suggest a small increase in number of college years after the policy, even though the coefficients are insignificant.

In panel B of table 4, I report the results from estimating equation 1. Column 1 shows that likely undocumented students are 3.2 percentage points (27%) more probable to graduate in treated states. This effect is robust to the inclusion of individual and state level controls in Columns 2 and 3. However, the magnitude of the estimates declines to 2.6 percentage points and 2.8 percentage points respectively when individual and state level

[37]See Table A6 for the event study estimates.

covariates are added to the model. These results hold up when I add state time trends to the model in column 4. These estimates together seem to suggest that subsidies increase the educational attainment of these students.

The institution level regressions also seem to support the higher matriculation rates of these undocumented undergraduates from public colleges, mainly the 2 year ones (see Table 9). Specifically, as per the standardised estimates, the public 2 year colleges see a 0.0027 unit increase (base mean:0.035) in share of NRA graduates among total graduates due to a 1 SD increase in degree of exposure to policy among the treated states. There is no effect on graduation of NRAs in private colleges of treated states more exposed to the policy. This is because private colleges did not face lower tuition as a result of the policy.

Figure A6 illustrates the college graduation trends along with the corresponding estimated synthetic counterfactual for each of the treated states. Most of the treated states seem to show an increase in graduation rates after the reform. This includes states which have adopted the policy earlier as well as in the recent periods.

I next turn to the effect of tuition subsidies on number of years of college completed, the results of which are shown in panel A of table 8. CPS provides data on number of years of college credit earned by those with at least some college education but less than a Bachelor's degree. In Column 1, I find that the policy leads to a significant 6.5% increase in number of college years completed for likely undocumented students. This effect persists, though with a smaller magnitude, when I add individual and state controls in Columns 2 and 3 respectively. I find no evidence of increase in the number of years of education for foreign born citizens or natives as a result of the policy.

Table A16 shows the results with ACS data which support the conclusions made from CPS data. Likely undocumented students are 2.7 percentage points (27%) more probable to attain their undergraduate degrees in treated states. Moreover, there is a 7.8% increase in the number of college years they completed, as a result of the policy.

## 5.4 Tuition and fees of public colleges

A possible concern with the tuition subsidy policy is that all students may have to pay higher fees as a result of this subsidy provided by the government. [38]To understand the extent of pass through of subsidies to all students, I use nationally representative annual data from Integrated Postsecondary Education Data System (IPEDS) spanning the academic years 2000/2001-2016/2017. This dataset contains comprehensive information on in-state and out-of-state tuition and required fees for all degree granting public 2 year and 4 year institutions in US having full time undergraduate students. I create an institution level panel dataset to examine this question. Table 10 reports summary statistics of this dataset.

Table 11 shows the effects of the policy on sticker price tuition charged in public undergraduate institutions. The dependent variable in each of the regressions is ln(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. In Column 2, I add baseline controls for percentage of the state's youth population that is female, married, foreign-born, Asian, Black, other non-white race, average age and square of average age. I also control for the unemployment rate, the proportion of votes cast for Republican candidates for the US House of Representatives, the college educated share of white and Mexican people. In Column 3, I additionally add state time trends that will absorb any systematic time varying factors affecting treated states. Standard errors are clustered by state.

The tuition subsidy policy for undocumented students leads to 6% increase in in- state tuition and fees at public 4 year institutions. Given the average in-state annual tuition of $6522 for these 4 year colleges (Table 10), this is roughly equivalent to an average annual increase of $391 in these 4 year colleges. This increase in tuition in public 4 year colleges

[38]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/.

is mainly driven by flagship universities increasing both their in-state and out-state tuition to undergraduate students. My estimates suggest a $472 increase in annual in-state tuition and $426 increase in annual out-state tuition in these flagship universities. For public community colleges, the average increase in annual instate tuition due to the policy is $246, while the average decrease in out-of-state tuition after the policy is $221. When state time trends are included in the model, the coefficients retain their sign and statistical significance. Some back of the envelope calculations suggest that for an average annual tuition subsidy of $6979 in public colleges, there is an increase of $1144 (16.4%) annually in tuition of flagships and community colleges. This increased tuition is borne by all students and represents an unintended effect of the policy.

## 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 having 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 fertility more than otherwise. On the other hand, undocumented females may have higher fertility while being exposed to the tuition subsidy policy, if they think those children will secure citizenship rights for them. Because of this theoretical ambiguity, this section empirically examines the causal relationship between tuition subsidy reforms and fertility decisions 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 4. Figure 7 shows the effects of tuition subsidy reform on probability of having at least one child under age 5 in the household for 5 years before and 5 years after the policy change. I observe all

of the coefficients prior to the reform to be insignificant. There seems to be no confounding pre-trends, thereby supporting my identification strategy. The probability of having children starts to fall for likely undocumented females after the policy was implemented. The effect is most pronounced in the first two years after the policy. This lends support to my belief that the policy played a role in reducing fertility among the target group.

I next show the results from estimation of equation 1 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 undocumented females are 1.8-1.9 percentage points less probable to have a child under age 5 in their household in treated states. Given that the average probability of having a child is 39.1% for these unauthorised females (table 3), the policy 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 less probable to give birth to a child in last year in the treated states. With an average probability of giving birth to a child in the last 12 months being 14.3% for these undocumented females,this translates to a 15.4% - 18% reduction in fertility rates for this group. Hence, the tuition subsidy policy seems to significantly reduce fertility rates of potentially undocumented females in treated states. [39]Some calculations suggest that for every 100 likely undocumented females, there are 4 fewer children born in the treated states. This is 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. This result is also consistent with Monstad, Propper and Salvanes (2008) who use a compulsory schooling reform in Norway to provide evidence of increased education delaying first births

[39]I do not find any effects of the policy 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 policy on number of children in the household.

of women to their 20s and late 30s.

I try to explore whether this decline in fertility among likely undocumented females is marital or non-marital. Table 13 seems to suggest that this lower fertility is in part 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 (1.8-2.0 percentage points (base mean - 63.2%)) and not by these immigrants getting divorced/separated. In fact, the policy has no effect on divorce rates of likely unauthorised immigrants.

Table 14 shows the results for what are the consequences, in terms of living arrangements, of reduced fertility among likely undocumented females. These likely undocumented females have higher probability of living with unmarried partner in the treated states. This effect is statistically significant and quite large in magnitude(2.7 percentage points (base mean-3.02%)).[40] Hence, the decline in fertility of the undocumented females in my sample can be considered a result of lower marriage rates rather than non-marital reasons. I find that probability of living with parents falls by 3.2 percentage points (base mean-48%) for likely undocumented females in the treated states.[41] The above findings are robust when I limit my sample to likely undocumented females enrolled in school and exposed to the policy (Tables 15 and 16). However, these results should be cautiously interpreted as they represent selection into sample based on the policy. Note that, for those policy eligible likely undocumented females, their likelihood of living as head of the household rises in the treated states. This increase possibly captures undocumented females living in dormitories or in apartments with or without roommates.

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

[41]I define parental coresidence as living with one's own parent. 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 college dormitory are not considered coresident 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).

# 6 Sensitivity and Heterogeneity Analyses

## 6.1 Endogeneity

One possible concern that arises when studying about these policy reforms is endogeneity of the policy due to non-random location of immigrants across the states. It may be possible that undocumented immigrants may move to states which offer them tuition subsidies that will allow them to obtain more education. If this happens, then due to self-selection into treatment, my estimates may be biased. There would be concerns that the estimates do not accurately reflect treatment effect of the policy.

To address this concern, I look at whether the policy affects the likelihood of Mexican/Hispanic non-citizen to move to a treated state. For this purpose, I use individual level longitudinal data from 2001, 2004, 2008 and 2014 panels of Survey of Income and Program Participation. Table A8 shows the estimated coefficients of tuition subsidy policy for three age groups of Mexican/Hispanic non-citizens. Columns 2 and 3 are the age groups which are most likely to respond to tuition subsidy reform. Both these columns seem to indicate that these likely undocumented immigrants do not appear to substantially move to the treated states to take advantage of the lower costs of college.

## 6.2 Falsification tests

I show falsification checks through event studies for college enrollment and college graduation in Figures A7 and A8 respectively. I present these event studies for two separate groups-Mexican non-citizens who arrived to US within 14 years of age (likely undocumented students) and foreign born citizens. The former group is the one affected by the tuition subsidy policy while the latter group is not. So we do not expect foreign born citizens to change their education outcomes in response to the policy. That seems to be the case in figures A7 and A8 where these citizens' coefficients on college enrollment and college matriculation probabilities are statistically insignificant in the years after the policy is implemented. In case of institution level outcomes, I find no effect on enrollment and

graduation of documented non-resident aliens who are likely unaffected by the policy in various types of institutions in the treated states (see Table A2, Table A3, Table A7). This further lends support to the role of the policy in raising enrollment and retention of only undocumented students in two-year colleges of treated states.

In case of fertility outcomes, I do not find any effects of the policy 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 foreign born female citizens(see table 12).

## 6.3 Policy Interaction Effects

All of the regression specifications on education outcomes control for the state level policies of financial aid and drivers' license for undocumented immigrants. It is possible that these policies interacted with the tuition subsidy policy, can help to explain the heterogenous impact on education outcomes among the states with the subsidy reform. For example, one might expect that the positive effect on college enrollment and college completion due to the subsidy will be higher in states that also give drivers' licenses to undocumented immigrants.[42]

Table A9 presents the difference-in-differences estimates for likely undocumented students, where I have interacted tuition subsidy policy with financial aid policy and drivers' license policy.[43] The estimate of subsidy law interacted with drivers' license policy in Column 1 suggests that states which allow for drivers' license and tuition subsidies to undocumented immigrants experience a 3.5 percentage point increase in college enrollment outcome relative to states which offer only tuition subsidies and no drivers' licenses. Thus, states which make both postsecondary education more affordable and remove barriers to transportation for undocumented students witness more of such students attending schools.

[42]In a separate specification, I have controlled for tuition subsidy policy and DACA program (through indicator for DACA eligible student in post DACA period)and find no significant change in results. Hence, I believe my results are not driven by DACA program.

[43]I have also included the main effects of the policies in the regression specifications though their results are not shown in Table A9.

This is because both education and commuting costs are reduced, thereby allowing these minority groups easier access to postsecondary education schools. My results also show that additional effect of immigration enforcement policies in reducing fertility among likely undocumented females is 2.84 percentage points(base mean :20% ).

## 6.4 Alternative Specifications

As the policy is adopted in a staggered manner across states, I first start with decomposing the difference-in-differences estimator into three groups of 2X2 estimators as per Goodman Bacon (2018). Around 75-80% of the baseline estimate is explained by comparisons between treated and never treated states. Moreover, any bias in overall diff-in-diff estimate due to comparing earlier and later treated states is very small. This is supported by the fact that the coefficient on treated vs never treated is similar to the overall diff-in-diff estimate.[44] In table A10, I check the robustness of the coefficient of policy dummy $D_{st}$ to the exclusion of particular observations. Panel A indicates that the positive impact of the policy on college enrollment is robust to alternative specifications. Similarly, Panel C shows that positive effect of the policy on probability of getting a college degree persists under alternative specifications.

In-state and out-of-state tuitions differ by state. Hence the effects of the tuition subsidy may differ by the amount of tuition. To examine this, I re-estimate equation 1 with the dummy policy variable being replaced by the average tuition subsidy (out of state tuition-in state tuition) in state s at time t. Table A11 shows the coefficients of tuition subsidy in dollars, separately for public 4 year and public 2 year institutions. The results are qualitatively similar to using the policy dummy as the variable of interest.

The triple difference results on fertility and household formation outcomes as presented in Table A12 support my difference-in-difference estimates, suggesting that the tuition subsidy policy plays a role in delaying childbearing and marriage among likely undocumented females. There is also evidence of these females living more with unmarried partner and

[44]See appendix C

less with their parents in treated states after the policy.

### 6.5 Heterogeneity Analysis

In this section, I first explore whether the effects of tuition subsidy reform on college enrollment and college completion outcomes vary by gender. The results are presented in Table A13. The magnitude of the coefficients suggests that this policy has substantial positive impact on the education outcomes of likely undocumented females.

When I examine effects of the policy on marriage rates by gender, I find that the decision to delay their marriage is driven by female undocumented immigrants over their male counterparts (see Table A14). This seems consistent with the fact that the policy increases educational attainment of female unauthorised immigrants. Hence these females seem to postpone their marriage and family formation decisions and also reduce their fertility rates. This education policy may open up future economic opportunities for these women, including improving their labor market outcomes.

I next turn to assessing the differential effects of the policy by region. [45] Table A15 shows coefficients of the interaction between region dummy and policy dummy for likely illegal immigrant youth. From panel A, I find that the policy leads to significant rise in their probability of college enrollment in the Midwest, South and Western regions. Similarly, the increase in their college graduation rates as a result of the policy seems to be driven by Midwest and South regions.

## 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 tuition outcomes using nation-

[45] US states are classified into one of the following regions - Northeast region, Midwest region, South region, West region.

ally representative data from IPEDS, which has not been focused on in the literature. Additionally, I look at the novel long run effects of policy on fertility of likely undocumented students enrolled in school and try to understand their subsequent living arrangements which can explain this decline in fertility. I use a difference-in-differences research design, exploiting variation in policy across states and time, to examine my research questions.

I find that likely undocumented students are 3.6 percentage points (19%) more probable to attend college in treated states. These enrollment effects appear to be driven by higher enrollment of these youth in public 2 year colleges. Among the public community colleges, there is increased enrollment of these likely undocumented students in high transfer and technical & vocational colleges of treated states with more exposure to the policy. Additionally, a higher number of them choose to major in health & medicine, social sciences, trades & personal services which is consistent with the occupational choices of undocumented immigrants found in literature. More importantly, these youth are 2.8 percentage points (23%) more likely to obtain their college degrees in treated states. Interestingly, the effects on college enrollment and graduation seem to be driven mostly by females. These targeted subsidies also seem to have significant negative spillover effects on enrollment of domestic students in public community colleges. Another unintended effect of the policy is higher instate and outstate tuition charged by flagship universities along with higher instate tuition charged by other public colleges.

In the second half of the paper, my results indicate that likely undocumented females in my sample respond to the increased educational attainment due to the reform by delaying their childbearing and household formation. In particular, I find that the policy reduces female undocumented immigrants fertility rates by 1.8-1.9 percentage points (4.6%-4.8%). In fact, for every 100 likely undocumented females, there are 4 less births in treated states. This is mainly driven by these females delaying their marriage though they increasingly reside with adult unmarried partners in the policy exposed states.

To sum up, access to subsidised education seems to have increased the educational attainment of likely undocumented immigrant students in treated states. 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 stringent eligibility requirements for obtaining tuition subsidies may be some factors which 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. [46] For example, undocumented immigrants with college degrees may make more informed decisions in the political and civic life of the society. Beyond the education effects, there are indirect benefits of the policy in terms of reduced childbirths to likely undocumented females.

To get an idea of the government's expenditure on the policy from the survey data of ACS, I do the following back-of-the-envelope calculation. My results suggest that the policy leads to likely undocumented immigrants being 2.8 percentage points (base mean - 16%) more probable to be enrolled in college in the treated states. In a sample of 57,262 likely undocumented (Mexican non-citizen) immigrants exposed to the policy ,this translates to 19,183 Mexican non-citizens being enrolled in college due to the reform. Assuming all likely undocumented immigrants are enrolled in public community colleges due to the policy (as I show in my results using IPEDS) I multiply the above number of enrolled Mexican non-citizens by average amount of annual tuition subsidy in community colleges i.e. \$4004. So the cost of the reform turns out to be \$77 million in a year. 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 etc. In fact, there is $\approx$ 16% increase in annual tuition of flagships and public community colleges for each dollar of tuition subsidy provided to undocumented students which can more than recover the cost. At the same time, there are indirect benefits in terms of delayed childbearing of undocumented females. The reduction in number of births to undocumented youth in my sample can result in cost savings for the state by lowering Emergency Medicaid expenditures, which are primarily targeted to undocumented immigrants. On average, about \$2 billion a year is spent on this program, which is majorly

[46] assuming these undocumented immigrants are not deported.

used to treat pregnancy and childbirth complications (Galewitz 2013). Some back of the envelope calculations suggest that because of the tuition subsidy policies, there would be Medicaid cost savings of around $12.3 million associated with publicly funded births of likely undocumented women.[47] [48]

To conclude, my study provides causal estimates of direct and indirect benefits and unintended spillover effects of positive permanent price shocks faced by undocumented students in public colleges. From the above lower bound numbers, it seems that the cost savings generated by the policy is lower than the spending on the program.

[47]My results indicate that due to the policy, there are 4 less births per 100 undocumented women aged 17-28 in my sample. So among 26,342 17-28 year old likely undocumented women who are enrolled in school in my sample, there are 1054 less births. Since the average cost of a publicly funded birth is $11,647 (Sonfield et al, 2011), this policy leads to a $12.3 million Medicaid cost savings. Note that this calculation assumes that all undocumented females in my sample use Medicaid to pay for costs of delivery.

[48]Note that these are not exact but lower bound figures of costs and benefits as they are based on my selected sample of likely undocumented students.

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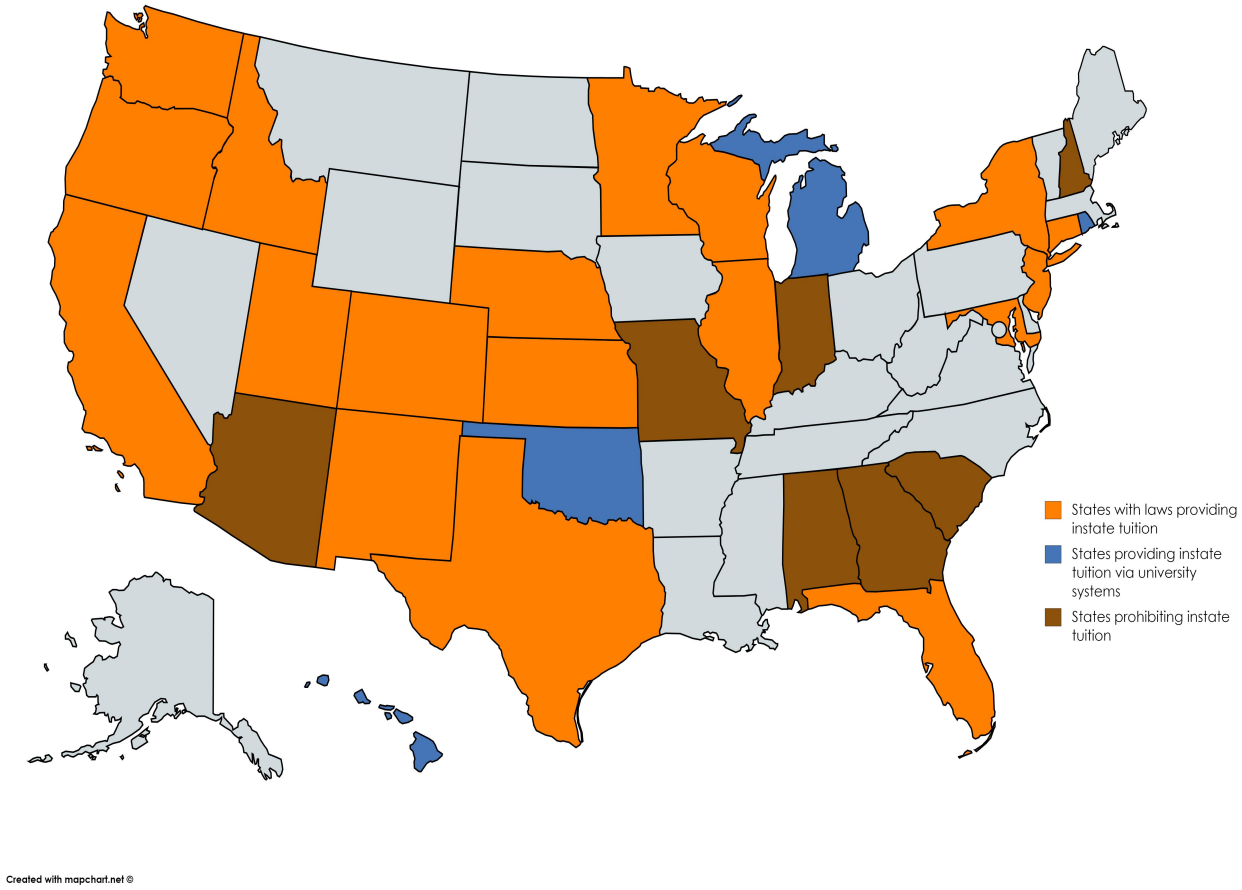
Figure 1: State legislation on resident tuition for undocumented immigrants




Source: NCSL

Figure 2: Overall unauthorised immigrant population 1990-2016

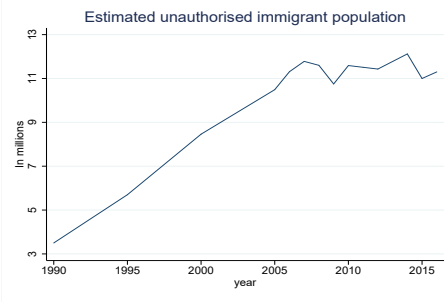



Figure 3: Mexican unauthorised immigrant population 1990-2016

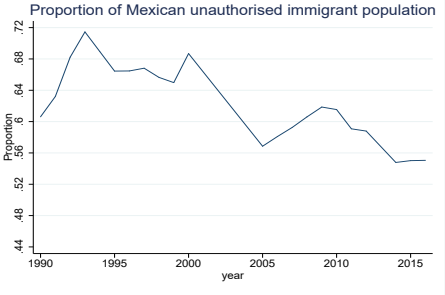



Figure 4: Event study : College enrollment (Institution level)

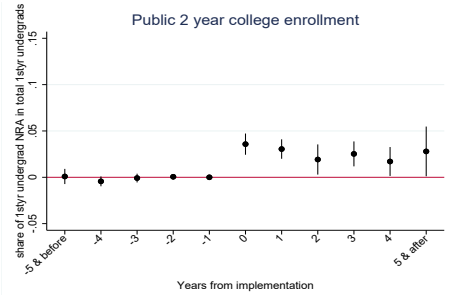



Figure 5: Event study: College graduation(Institution level)

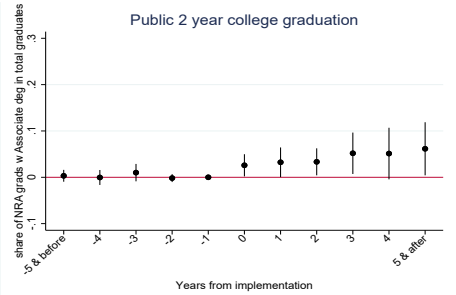



Figure 6: Event study : No. of college years

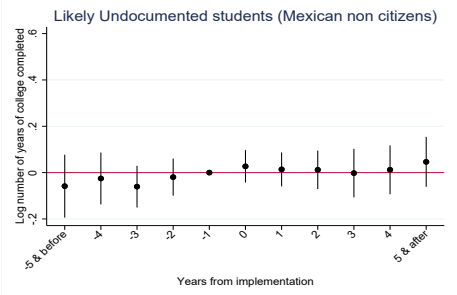



Figure 7: Event study: Fertility

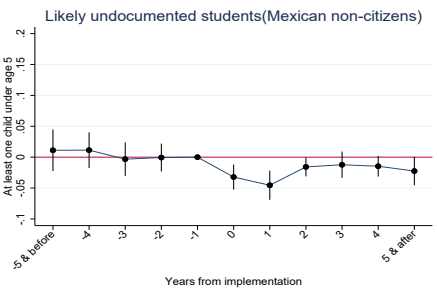


Table 1: State legislation on in-state tuition for undocumented immigrants

| 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 Policy | 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 policy and they continued with it. In Wisconsin, the law was revoked in June, 2011. The percentages in columns 4,5 and 6 denote average proportion of Mexican non-citizens, foreign born citizens and natives respectively during our sample period of 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 of observations used in regressions** | | | | | | |
| 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 policy | 0.26 | 0.44 | 0.13 | 0.34 | 0.12 | 0.33 |
| Drivers' license policy | 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: Individual is enrolled in college/Individual has an Associate's or Bachelor's degree

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | (4) | Observations |
|---|---|---|---|---|---|
| **Panel A: College enrollment** | | | | | |
| LU immigrants | 0.039** | 0.038** | 0.036** | 0.037** | 39,098 |
| | (0.016) | (0.015) | (0.014) | (0.015) | |
| Foreign-born citizens | -0.027 | -0.016 | -0.018 | -0.019 | 39,459 |
| | (0.018) | (0.016) | (0.016) | (0.017) | |
| Natives | 0.003 | 0.006 | 0.007 | 0.001 | 1,671,306 |
| | (0.008) | (0.008) | (0.007) | (0.009) | |
| **Panel B: College graduation** | | | | | |
| LU immigrants | 0.032** | 0.026** | 0.028** | 0.021** | 43,631 |
| | (0.012) | (0.012) | (0.012) | (0.010) | |
| Foreign-born citizens | 0.022 | 0.019 | 0.018 | 0.018 | 87,017 |
| | (0.021) | (0.020) | (0.021) | (0.022) | |
| Natives | 0.004 | 0.005 | 0.002 | 0.001 | 1,691,282 |
| | (0.004) | (0.004) | (0.004) | (0.006) | |
| 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 5: Dependent variable: Enrollment effects by sector of institution

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

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens | 0.005 | 0.004 | 0.011 | 10,053 |
| | (0.007) | (0.006) | (0.008) | |
| 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*** | 7,228 |
| | (0.006) | (0.005) | (0.007) | |
| Dep var mean | 0.018 | 0.018 | 0.018 | |
| 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 | 0.014** | 0.001 | 9,014 |
| | (0.005) | (0.006) | (0.008) | |
| Dep var mean | 0.021 | 0.021 | 0.021 | |
| Domestic students | -0.106 | -0.103 | -0.115 | 9,003 |
| | (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 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 interaction term between tuition subsidy reform and undocumented immigrant share in 1990 from a separate regression. Regressions are weighted according to how much of the total student population the institution represents. 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 Carnegie classification institutions

Coefficients of Tuition Subsidy reform* 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 | 0.022 | 0.022 | |
| **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 | 0.014 | 0.014 | |
| **Panel C: Associate's Colleges- High vocational/technical** | | | |
| Non Resident Aliens | 0.027*** | 0.029*** | 1,190 |
| | (0.004) | (0.003) | |
| Dep var mean | 0.008 | 0.008 | |
| **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) | (0.063) | |
| 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 interaction term between tuition subsidy reform and undocumented immigrant share in 1990 from a separate regression. Regressions are weighted according to how much of the total student population the institution represents. 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: Choice of major in undergraduate degree

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

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Arts & humanities** | | | | |
| Non Resident Aliens | -0.007** | -0.008** | -0.006 | 27,593 |
| | (0.003) | (0.003) | (0.005) | |
| **Panel B: Business** | | | | |
| Non Resident Aliens | 0.017** | 0.010 | 0.017*** | 27,593 |
| | (0.007) | (0.006) | (0.006) | |
| **Panel C: Health & medicine** | | | | |
| Non Resident Aliens | 0.012*** | 0.014*** | 0.020*** | 23,082 |
| | (0.002) | (0.002) | (0.005) | |
| **Panel D: Multi/interdisciplinary studies** | | | | |
| Non Resident Aliens | -0.011** | -0.009** | -0.004 | 27,593 |
| | (0.004) | (0.004) | (0.009) | |
| **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 | -0.043*** | -0.039*** | -0.006 | 27,593 |
| | (0.006) | (0.005) | (0.006) | |
| **Panel G: Social sciences** | | | | |
| Non Resident Aliens | 0.021*** | 0.019*** | 0.020*** | 27,593 |
| | (0.003) | (0.003) | (0.004) | |
| **Panel H: Trades & personal services** | | | | |
| Non Resident Aliens | 0.015*** | 0.014** | 0.022*** | 27,593 |
| | (0.003) | (0.003) | (0.006) | |
| 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 interaction term between tuition subsidy reform and 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 8: 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 9: Dependent variable: Graduation effects by sector of institution

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

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: Private colleges** | | | | |
| Non Resident Aliens | 0.023 | 0.025 | 0.013 | 13,923 |
| | (0.015) | (0.013) | (0.017) | |
| Dep var mean | 0.040 | 0.040 | 0.040 | |
| **Panel B: Public 2 year colleges** | | | | |
| Non Resident Aliens | 0.027** | 0.029** | 0.037** | 10,845 |
| | (0.011) | (0.011) | (0.016) | |
| Dep var mean | 0.035 | 0.035 | 0.035 | |
| **Panel C: Public 4 year colleges** | | | | |
| Non Resident Aliens | 0.028* | 0.037** | 0.032 | 13,683 |
| | (0.014) | (0.017) | (0.020) | |
| 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 interaction term between tuition subsidy reform and undocumented immigrant share in 1990 from a separate regression. Regressions are weighted according to how much of the total student population the institution represents. 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 10: 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 11: 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 | -0.011 | 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** | -0.031** | -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 | 0.020* | 0.023** | 0.036** | 7,992 |
| | (0.011) | (0.010) | (0.014) | |
| 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 12: Effects of tuition subsidy policy 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 policy 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 policy 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.

Table 15: Effects of tuition subsidy policy 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 16: Effects of tuition subsidy policy 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 | 50,245 |
| | (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.

# Appendix A

Here, I briefly summarize the criteria I use to determine the category of 'likely undocumented' students in my analysis using CPS and ACS data. I also briefly describe the method used by Department of Homeland Security to get an estimate of undocumented population since I use this estimate to compute a measure of degree of exposure to the policy.

I consider only Mexican 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 policy. First, IRCA allowed immigrants who arrived to US by January 1, 1982 to become legal residents. Therefore, I remove Mexican non-citizens who arrived in 1981 or earlier to determine the group of likely undocumented immigrants. Second, since the laws require undocumented immigrants to attend high school for a certain number of years,I drop Mexican non-citizens who arrived at an age later than 14. This is because those who entered US after 14 years of age may not qualify for the tuition subsidy policy.

The undocumented immigrant population in US as of January 1st of a year is obtained by subtracting estimated legally resident population on January 1st of that year from the estimated foreign born population on January 1st of that year.

*Consider the first component computation.* The estimated foreign born population as of January 1, 2006 (say) is calculated by taking the foreign born population that entered US during 1980-2005 from ACS. This is then adjusted
1) for shifts in reference date from July 1, 2005 to January 1, 2006,
2) for exclusion of group quarters (if applicable i.e. for years 2000-2005 in ACS),
3)for undercount of natives, legally resident immigrants like refugees and undocumented immigrant population in ACS
to derive the final estimated foreign born population.

*Now, consider the second component computation.*Specifically, the estimated legally resident population as of January 1, 2006 is obtained by adding LPR, refugee and asylee resident population as of Jan 1, 2006 and native population as of Jan 1, 2006. Data on legally resident population was obtained from DHS administrative records in USCIS ap-

plication system. Data on arrival of refugees were obtained from Department of State. Data on asylum seekers were obtained from USCIS and Executive Office of Immigration Review of the Department of Justice. Data on native admissions were obtained from I-94 arrival-departure records of US Customs and Border Protection (CBP). Note that, the LPR, refugee and asylee resident population as of Jan 1, 2006 is obtained by adjusting the corresponding entry of these groups from 1980-2005 for death and emigration.

# Appendix B

The broad major categories shown in Table 7 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 C

In this section, I discuss the results of decomposing the difference-in-differences estimate using Goodman-Bacon methodology(2018). In a standard difference-in-differences model, the average treatment effect is the difference in change in outcomes between treated and control groups, before and after the treatment. However, when the treated units are exposed to the treatment at different points of time, as in the case of tuition subsidy policy, the difference-in-differences estimate is a weighted average of all possible 2 group-2 period (2X2). estimators. Some comparisons are made between units treated at a particular time (treatment) to untreated units (control). Other comparisons are made between units treated at two different times, using later treated group as control before its treatment starts and earlier treated group as controls after its treatment starts. To the extent comparisons are made between treatment groups treated at different points in time, this biases the single coefficient estimator away from the true treatment effect.

To identify how big the bias is in my DID estimates, I decompose the DID model into 3 groups of 2X2 estimators- Earlier treated vs later control, later treated vs earlier control, treated vs never treated. Note that, there is no always treated group in my sample because the entire implementation of policy occurs within my analysis period. In tables A17 and A18, I show the decomposition results for the main outcomes. Each column shows a 2X2 estimator with corresponding weights in brackets below the estimates. There are two key takeaways from these results: First, around 75-80% of the baseline estimate is explained by comparisons between treated and never treated states. This is consistent with large number of states in the sample who have never been exposed to the policy. Second, the remaining 20-25% of the baseline estimate is explained by timing variation among treatment groups i.e. those treated later serve as control group for earlier treatment group and those treated earlier serve as control group for later treatment group. However, in almost all cases, the diff-in-diff coefficient and the coefficient on treated vs never treated is very close. This indicates that any bias in overall diff-in-diff estimate due to comparing earlier and later treated states is very small. Thus,the diff-in-diff estimate does not represent a significant deviation from average post treatment effect.

# Appendix Figures

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

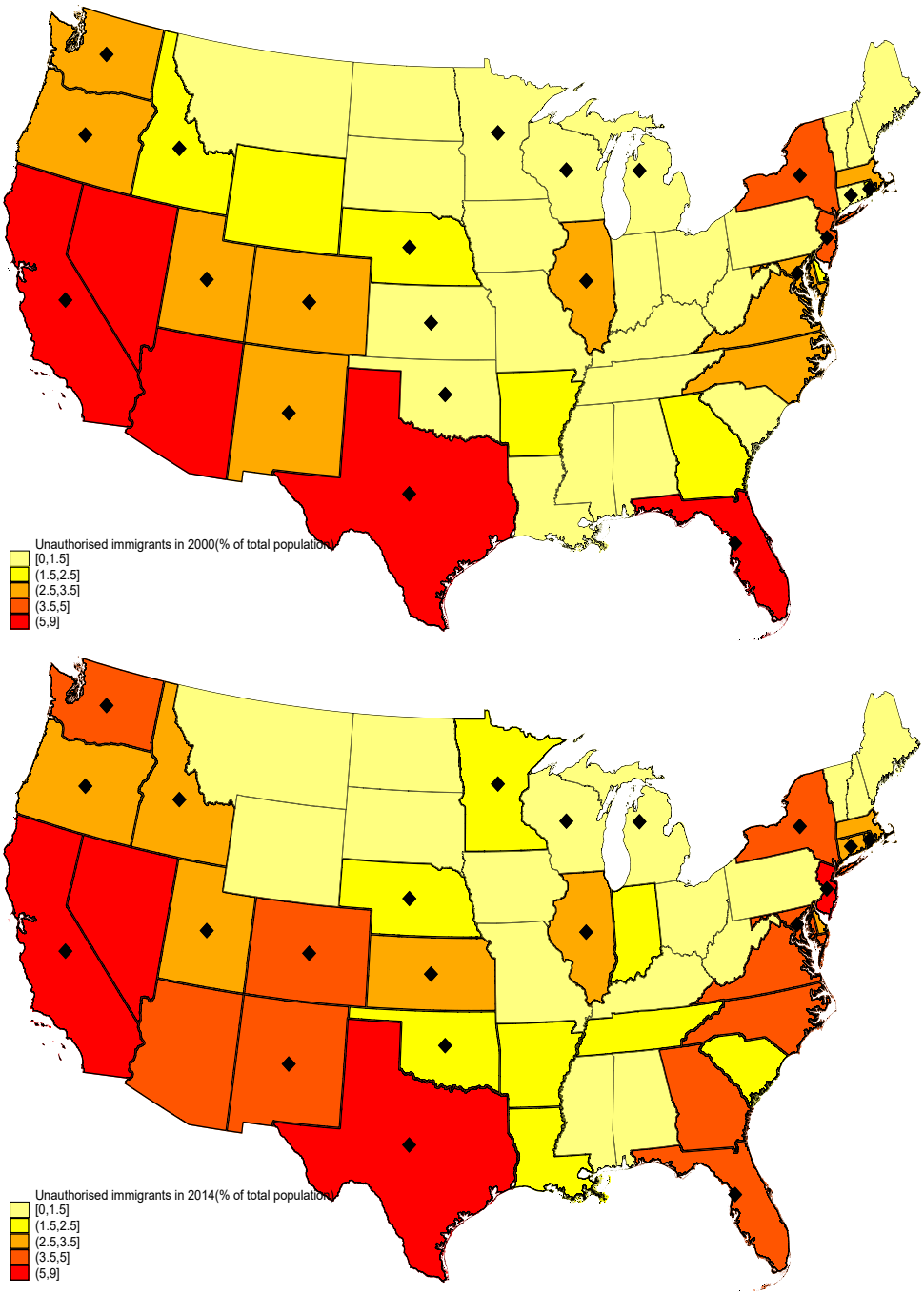
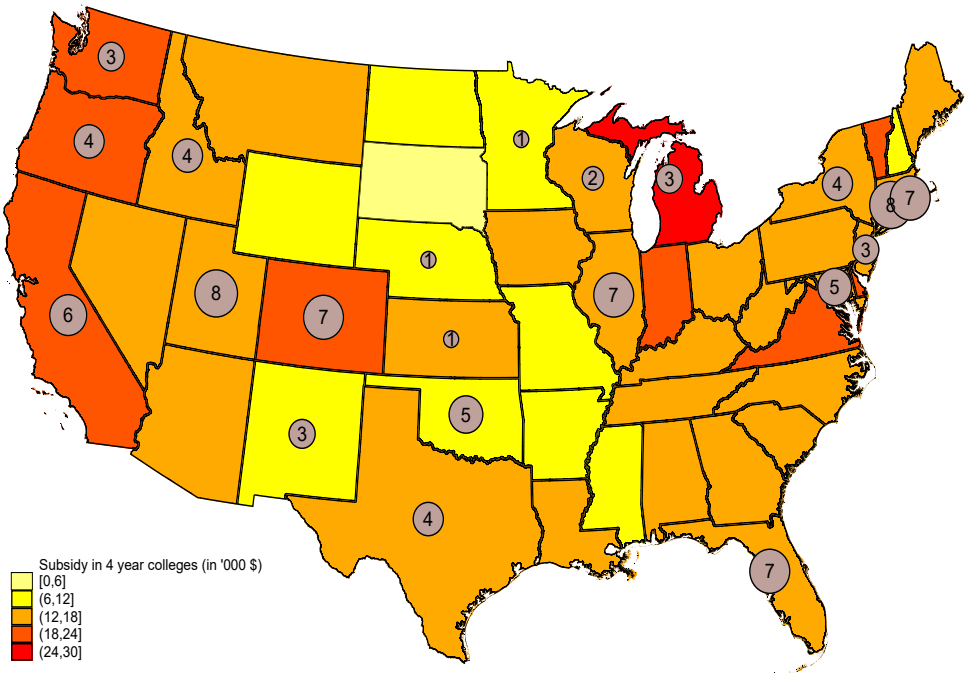
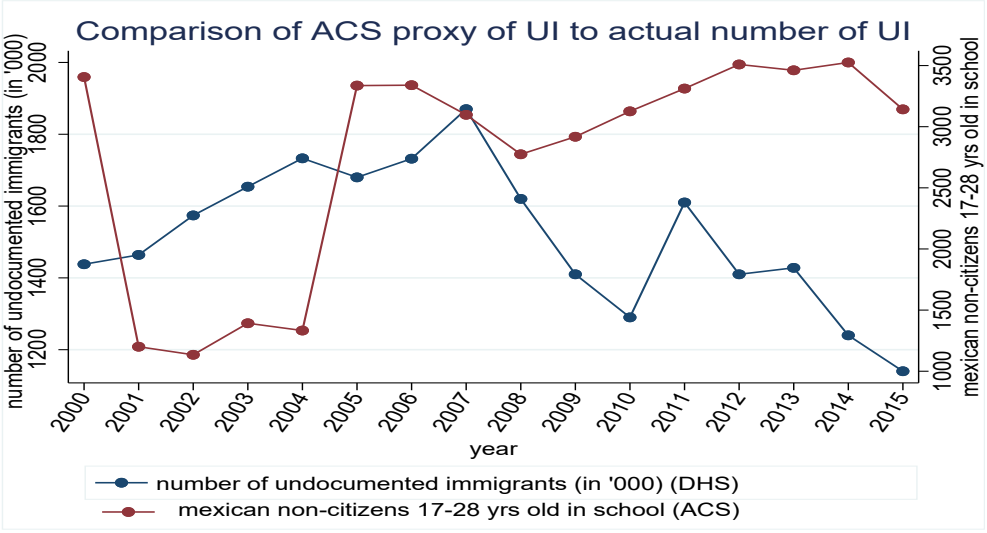


Figure A2: Sticker price tuition subsidies for public 4 year and 2 year colleges in 2016-17



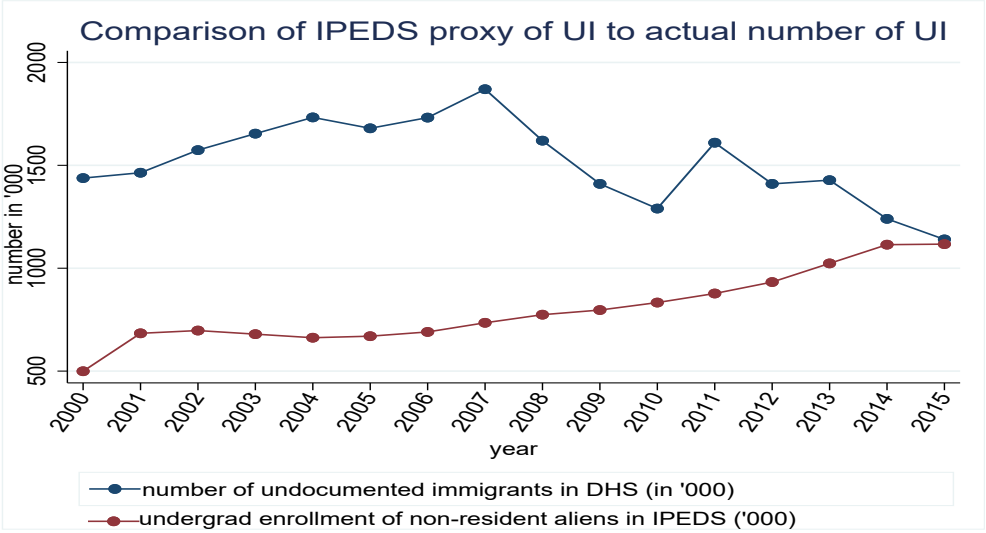

Note: The states with the circles are the treated states. The area of the circle is proportional to the amount of tuition subsidy (in '000 $) in public 2 year colleges.

Figure A3: ACS Undocumented immigrant student proxy vs actual number of undocumented youth



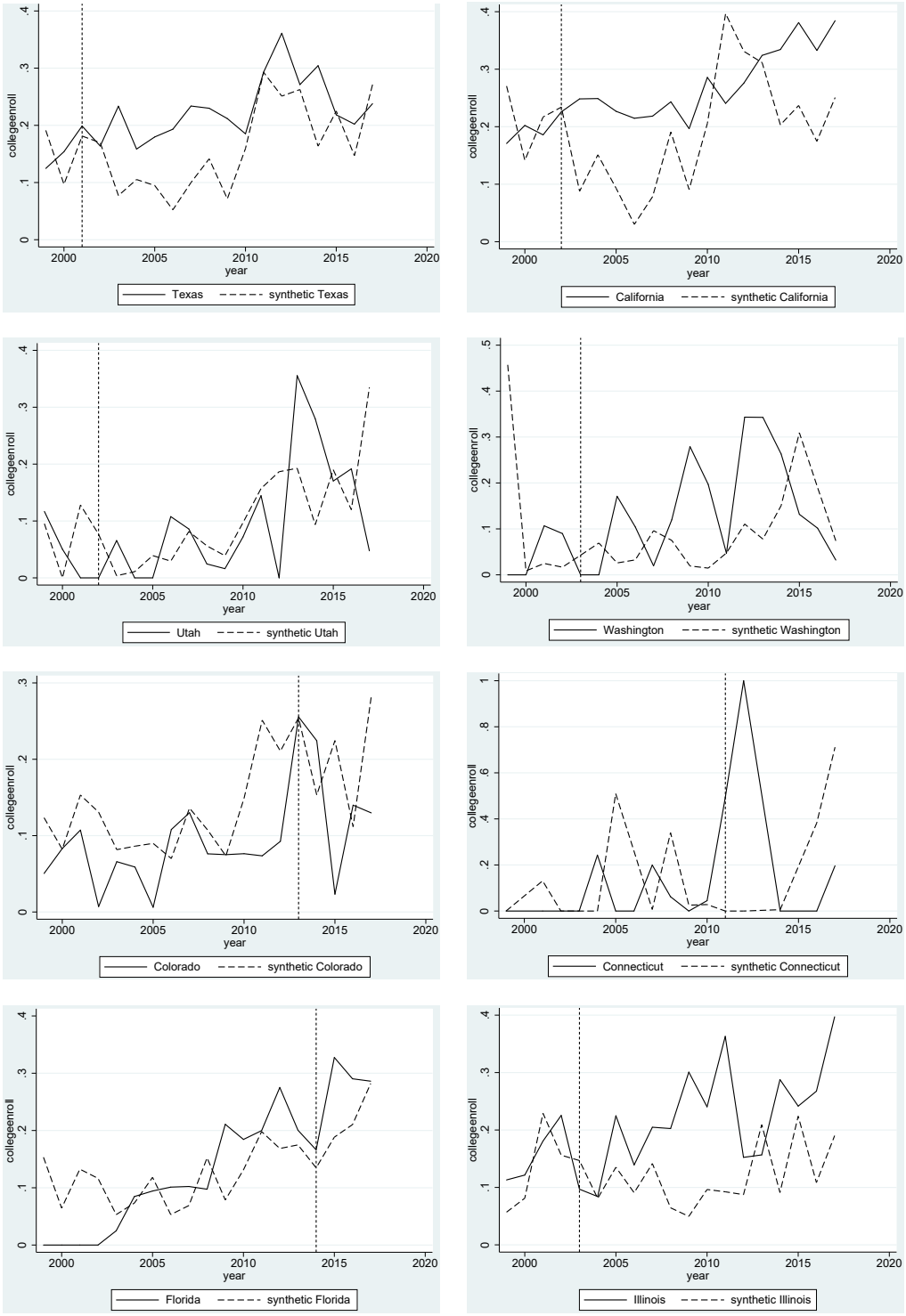

Notes: The undocumented immigrants data from DHS includes both school going and non-school going youth in the age group 18-24. In contrast,Mexican non-citizen data from ACS is limited to only school going youth in the age group 17-28. The ACS data does not include Mexican non-citizens living in group quarters in the years 2001-2004, hence their counts are especially low in those years.

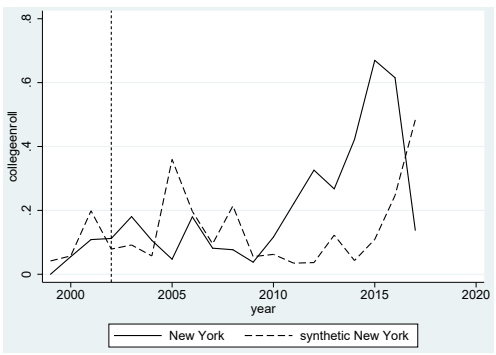
Figure A4: IPEDS Undocumented immigrant student proxy vs actual number of undocumented youth




Notes: The undocumented immigrants data from DHS includes both school going and non-school going youth in the age group 18-24. In contrast, non resident alien data from IPEDS is limited to only school going youth. This data minimises the undercount of school going undocumented immigrants in ACS as can be seen by comparing appendix figures 2 and 3.

Figure A5: College enrollment trends and synthetic control in treated states

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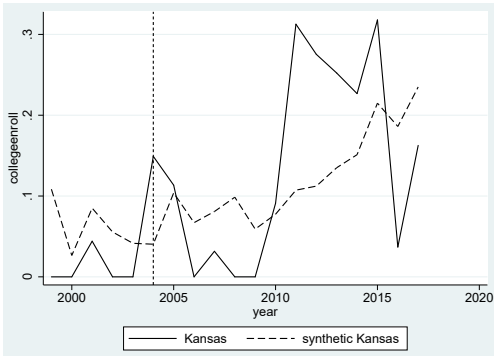
.2
.4
.6
.8
2000
2005
2010
2015
2020
year
New York
synthetic New York


collegeenroll
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2000
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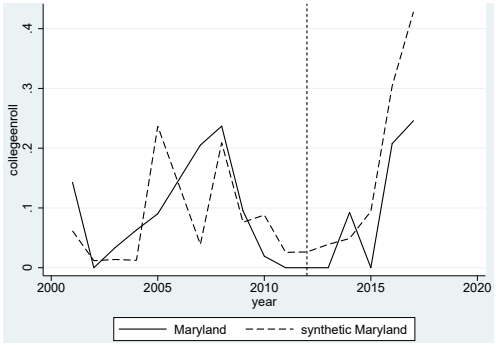
2020
year
Kansas
synthetic Kansas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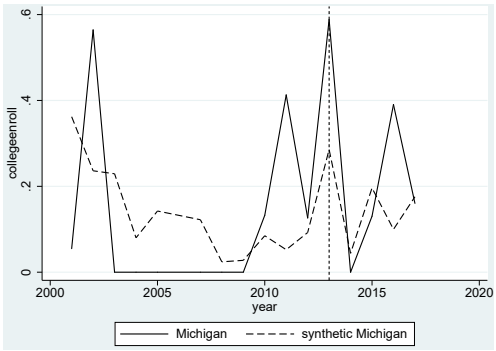
Maryland
synthetic Maryland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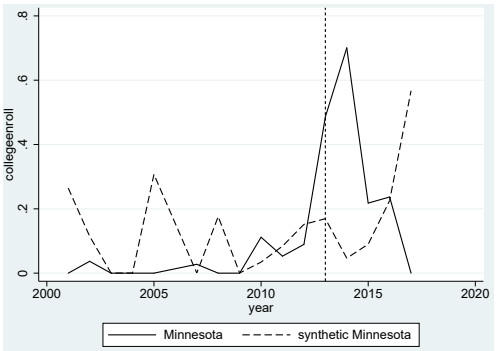
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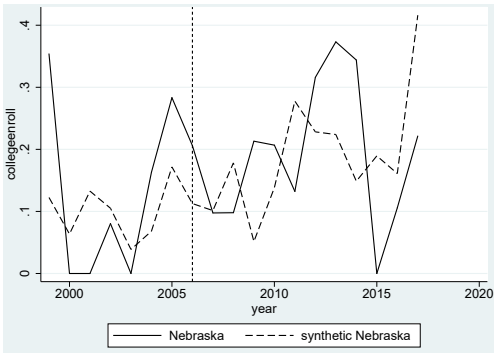
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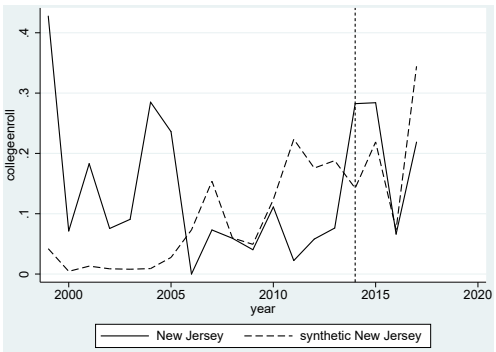
Nebraska
synthetic Nebraska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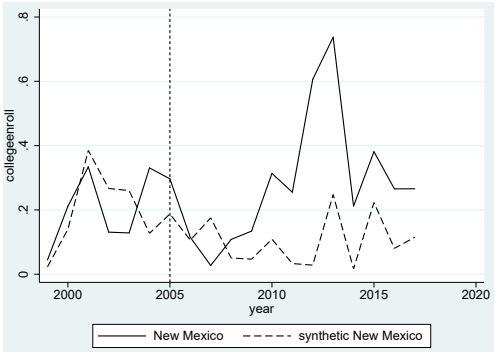
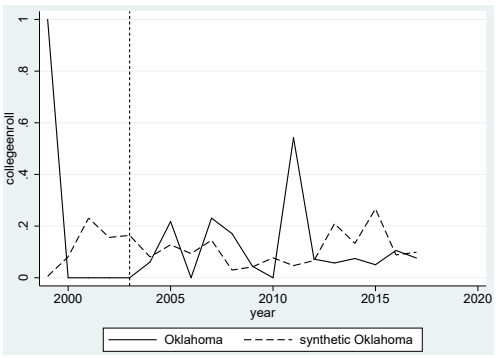
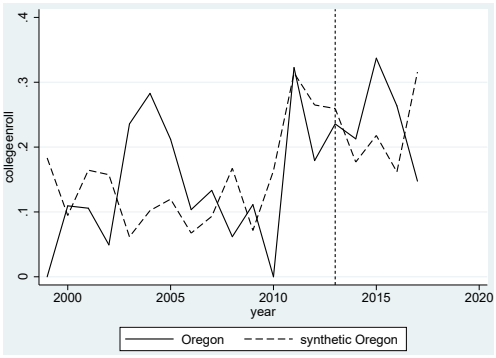
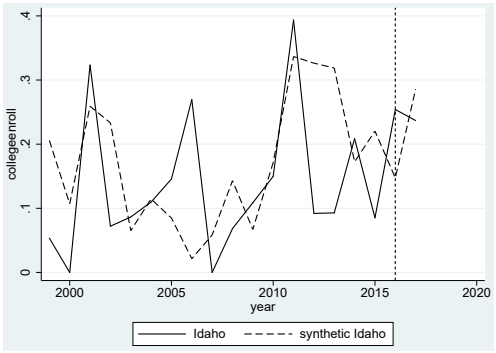
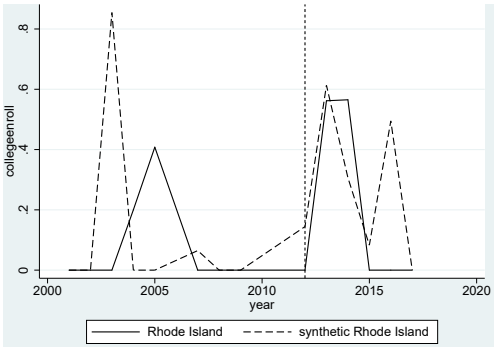
2015
2020
year
New Jersey
synthetic New Jersey


collegeenroll
0
.2
.4
.6
.8
2000
2005
2010
2015
2020
year
New Mexico
synthetic New Mexico


collegeenroll
0
.2
.4
.6
.8
1
2000
2005
2010
2015
2020
year
Oklahoma
synthetic Oklahoma


collegeenroll
0
.1
.2
.3
.4
2000
2005
2010
2015
2020
year
Oregon
synthetic Oregon


collegeenroll
0
.1
.2
.3
.4
2000
2005
2010
2015
2020
year
Idaho
synthetic Idaho


collegeenroll
0
.2
.4
.6
.8
2000
2005
2010
2015
2020
year
Rhode Island
synthetic Rhode Island

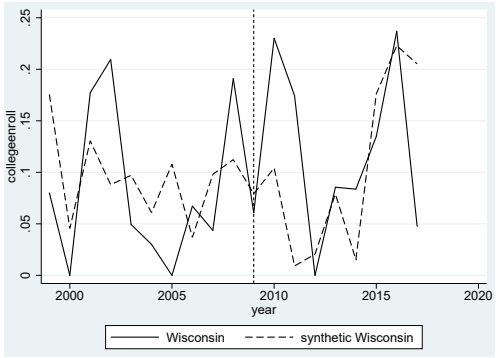
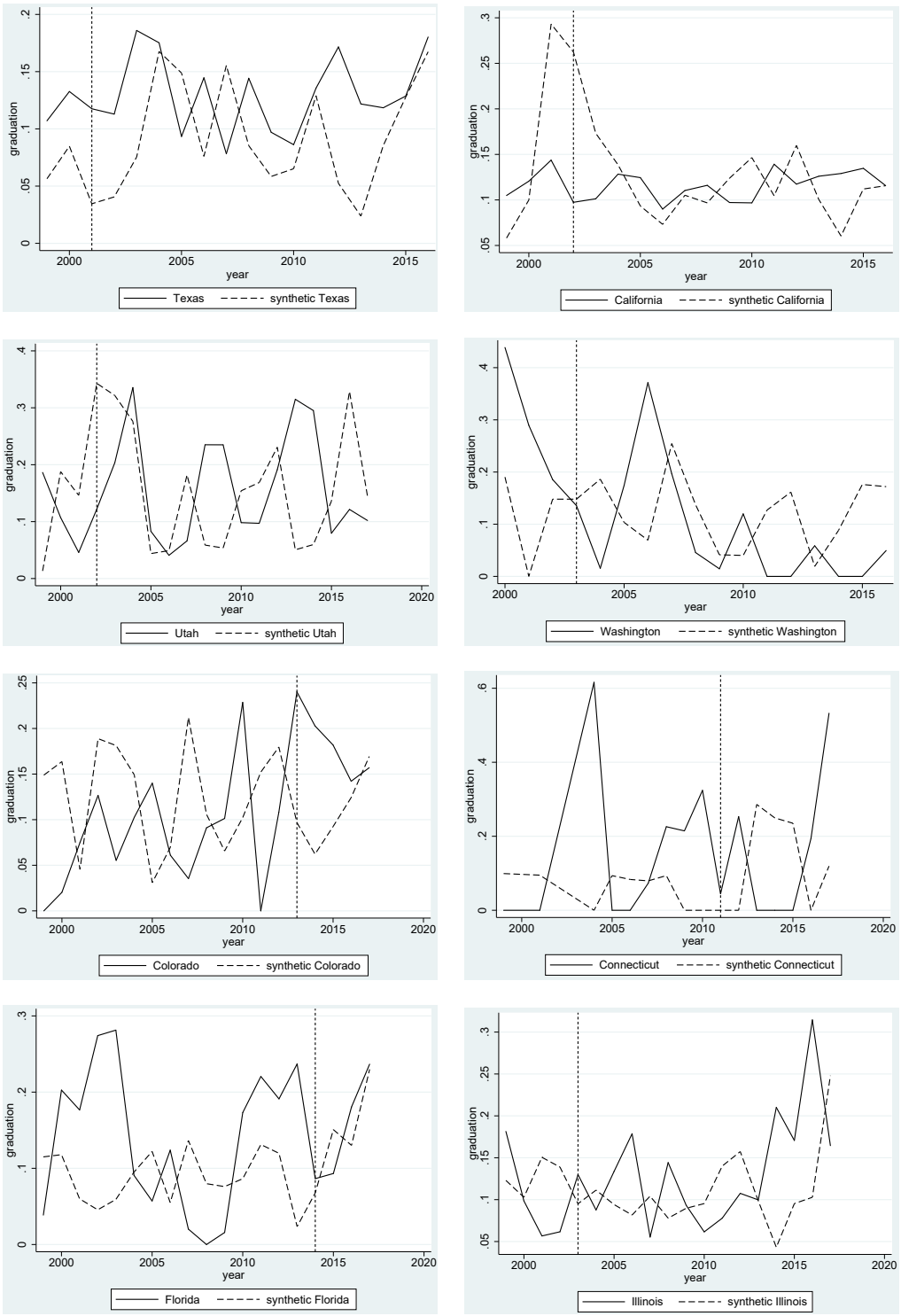
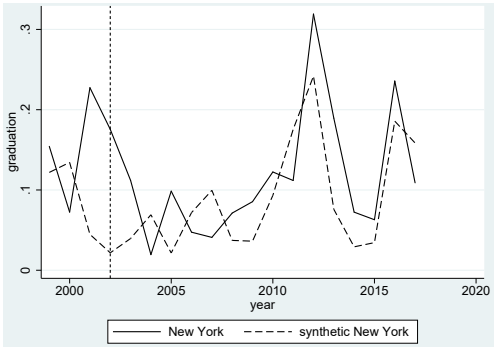
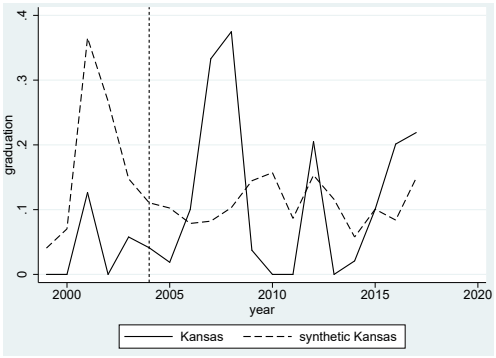
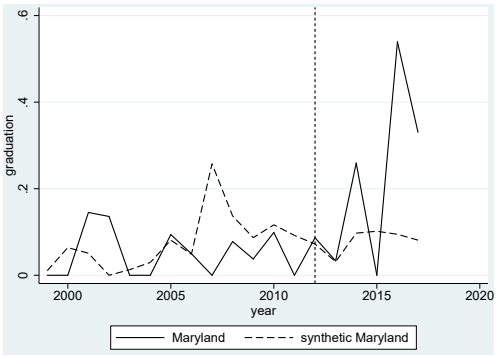

collegeenroll
0
.05
.1
.15
.2
.25
2000
2005
2010
2015
2020
year
Wisconsin
synthetic Wisconsin

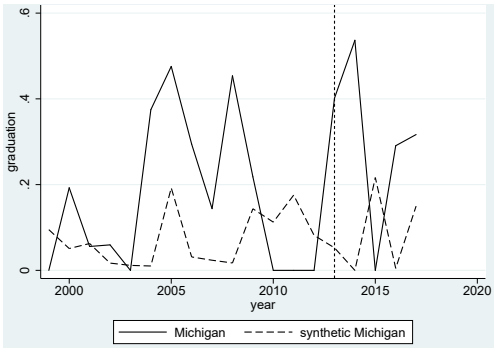
Figure A6: Graduation trends and synthetic control in treated states

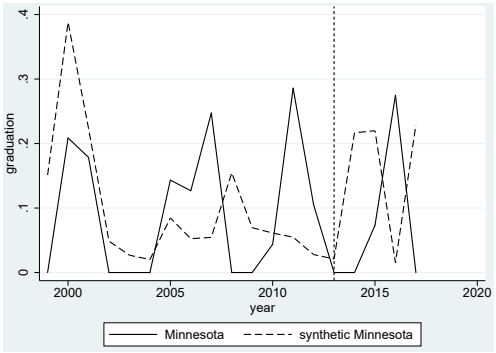


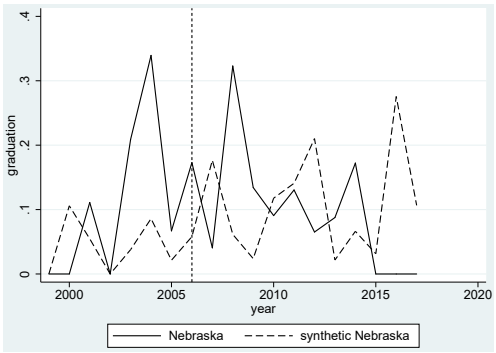

graduation
0
.1
.2
.3
2000
2005
2010
2015
2020
year
New York
synthetic New York

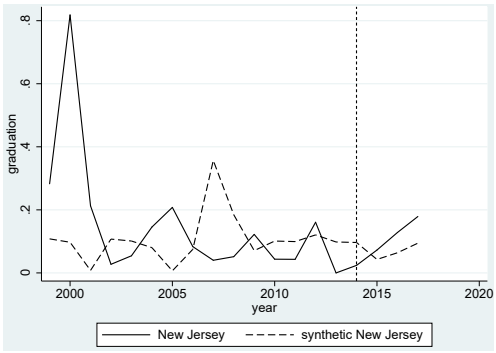

graduation
0
.1
.2
.3
.4
2000
2005
2010
2015
2020
year
Kansas
synthetic Kansas

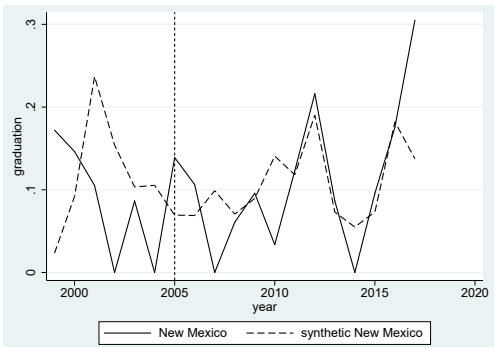

graduation
0
.2
.4
.6
2000
2005
2010
2015
2020
year
Maryland
synthetic Maryland


graduation
0
.2
.4
.6
2000
2005
2010
2015
2020
year
Michigan
synthetic Michigan


graduation
0
.1
.2
.3
.4
2000
2005
2010
2015
2020
year
Minnesota
synthetic Minnesota


graduation
0
.1
.2
.3
.4
2000
2005
2010
2015
2020
year
Nebraska
synthetic Nebraska


graduation
0
.2
.4
.6
.8
2000
2005
2010
2015
2020
year
New Jersey
synthetic New Jersey


graduation
0
.1
.2
.3
2000
2005
2010
2015
2020
year
New Mexico
synthetic New Mexico

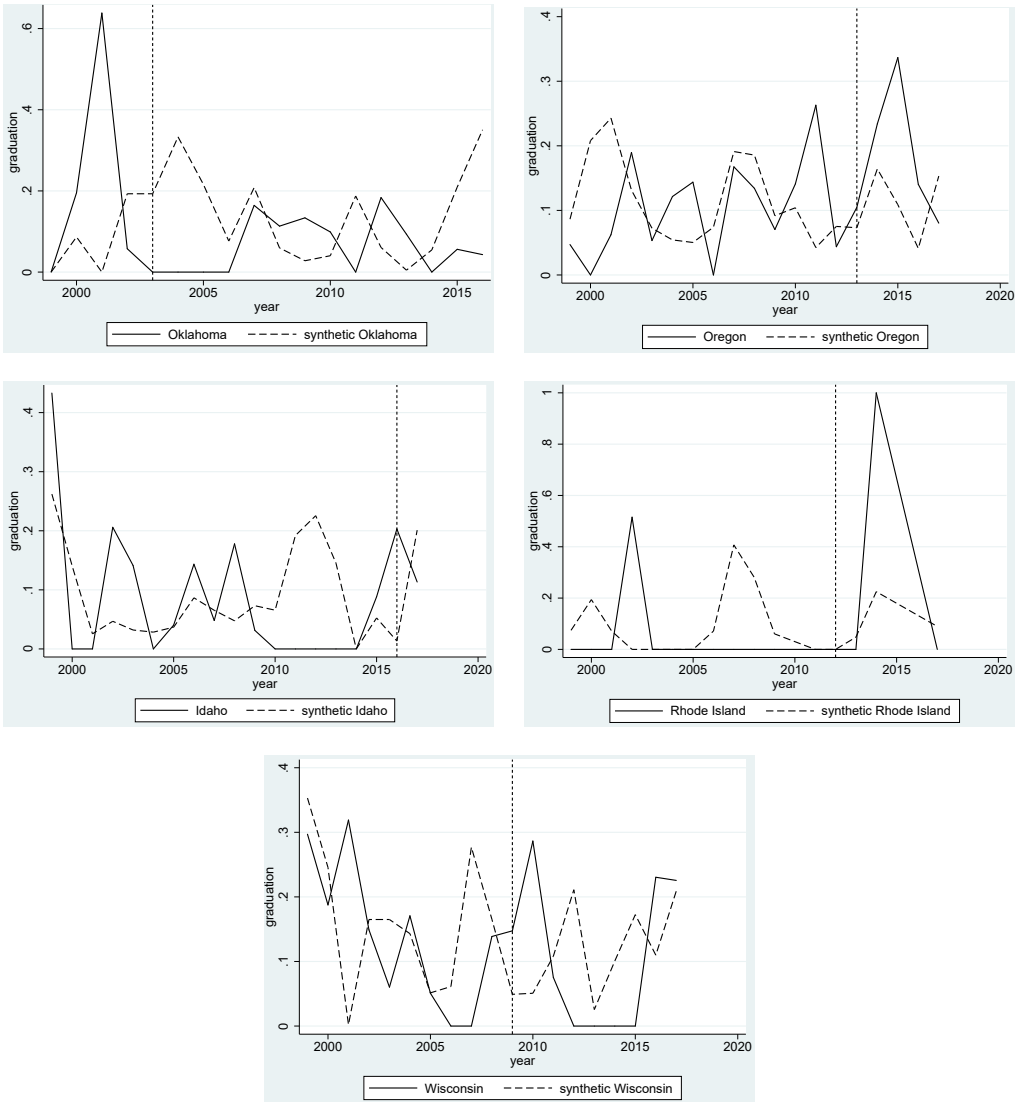



Figure A7: Placebo test: College enrollment

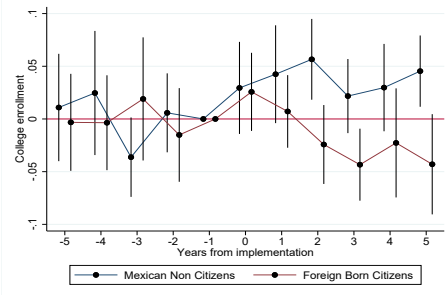
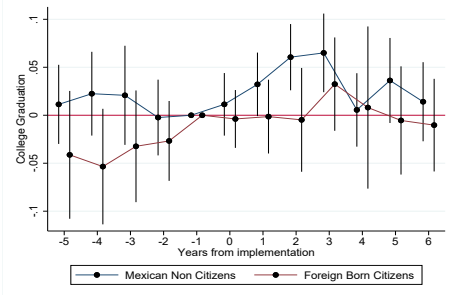



Figure A8: Placebo test: College graduation



# Appendix Tables

Table A1: Event study estimates of public 2 year college enrollment of Non Resident Alien undergraduates- Coefficients of Tuition Subsidy reform* share of undocumented immigrants in 1990

| | Leads+lags |
|---|---|
| 5 years before policy | 0.001 |
| | (0.004) |
| 4 years before policy | -0.004 |
| | (0.003) |
| 3 years before policy | -0.001 |
| | (0.002) |
| 2 years before policy | 0.0006 |
| | (0.001) |
| On year of policy implementation | 0.036*** |
| | (0.006) |
| 1 year after policy | 0.030*** |
| | (0.005) |
| 2 years after policy | 0.019** |
| | (0.008) |
| 3 years after policy | 0.025*** |
| | (0.007) |
| 4 years after policy | 0.017** |
| | (0.008) |
| 5 years after policy | 0.028** |
| | (0.013) |
| Covariates | Yes |
| Institution & Year FE | Yes |
| Observations | 7,657 |

Notes: The data is taken from IPEDS. Regressions are weighted according to how much of the total student population the institution represents. 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

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** | 24,750 |
| | (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 | 0.018 | 0.018 | 0.018 | |
| Domestic students | -0.001 | -0.004 | -0.018** | 16,393 |
| | (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 | 11,545 |
| | (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 according to how much of the total student population the institution represents. 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) | |
| Dep var mean | 0.022 | 0.022 | |
| **Panel B: Associate's Colleges- Mixed transfer/vocational & technical** | | | |
| Non Resident Aliens | 0.0025* | 0.0019 | 2,986 |
| | (0.0015) | (0.0015) | |
| Dep var mean | 0.014 | 0.014 | |
| **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) | |
| Dep var mean | 0.037 | 0.037 | |
| **Panel E: Special Focus Four-Year: Health Professions** | | | |
| Non Resident Aliens | -0.018 | -0.019 | 90 |
| | (0.028) | (0.027) | |
| Dep var mean | 0.051 | 0.051 | |
| **Panel F: Special Focus Four-Year: Business & Management** | | | |
| Non Resident Aliens | 0.005 | 0.002 | 111 |
| | (0.011) | (0.011) | |
| Dep var mean | 0.122 | 0.122 | |
| **Panel G: Special Focus Four-Year: Arts, Music & Design** | | | |
| Non Resident Aliens | 0.021 | 0.028 | 372 |
| | (0.025) | (0.022) | |
| 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 tuition subsidy reform from a separate regression. Regressions are weighted according to how much of the total student population the institution represents. 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 6 (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 | 0.01 | 0.062 | 0.17 | 0.868 | 0.046 | No |
| Arts, music & design controls | 0.011 | 0.063 | 0.175 | 0.868 | 0.05 | No |

Notes: The table shows the results of multiple hypothesis testing of the outcomes in Table 6 using Benjamini Hochberg Step-up False Discovery rate.

Table A5: 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 |
|---|---|---|---|---|---|---|
| Arts & humanities no controls | -0.007 | 0.003 | -2.43 | 0.019 | 0.031 | Yes |
| Arts & humanities controls | -0.008 | 0.003 | -2.67 | 0.01 | 0.027 | Yes |
| Arts & humanities controls,trends | -0.006 | 0.005 | -1.42 | 0.163 | 0.046 | No |
| Business no controls | 0.017 | 0.007 | -2.36 | 0.022 | 0.033 | Yes |
| Business controls | 0.010 | 0.006 | 1.64 | 0.108 | 0.038 | No |
| Business controls,trends | 0.017 | 0.006 | 2.81 | 0.007 | 0.025 | Yes |
| Health & medicine no controls | 0.012 | 0.002 | 4.86 | 0.000 | 0.002 | Yes |
| Health & medicine controls | 0.014 | 0.002 | 5.34 | 0.000 | 0.004 | Yes |
| Health & medicine controls,trends | 0.020 | 0.005 | 3.78 | 0.000 | 0.006 | Yes |
| Multidisciplinary study no controls | -0.011 | 0.004 | -2.59 | 0.012 | 0.029 | Yes |
| Multidisciplinary study controls | -0.009 | 0.004 | -2.33 | 0.024 | 0.035 | Yes |
| Multidisciplinary controls,trends | -0.004 | 0.009 | -0.49 | 0.625 | 0.05 | No |
| Public services no controls | 0.004 | 0.003 | 1.51 | 0.138 | 0.042 | No |
| Public services controls | 0.004 | 0.003 | 1.45 | 0.154 | 0.044 | No |
| Public services controls,trends | 0.008 | 0.005 | 1.61 | 0.114 | 0.039 | No |
| STEM no controls | -0.043 | 0.006 | -7.7 | 0.000 | 0.008 | Yes |
| STEM controls | -0.039 | 0.005 | -7.59 | 0.000 | 0.0104 | Yes |
| STEM controls, trends | -0.006 | 0.006 | -1.05 | 0.301 | 0.048 | No |
| Social science no controls | 0.021 | 0.003 | 7.38 | 0.000 | 0.012 | Yes |
| Social science controls | 0.019 | 0.003 | 5.99 | 0.000 | 0.014 | Yes |
| Social science controls, trends | 0.020 | 0.004 | 4.52 | 0.000 | 0.017 | Yes |
| Trade & pers serv no controls | 0.015 | 0.002 | 5.89 | 0.000 | 0.01875 | Yes |
| Trade & pers serv controls | 0.014 | 0.003 | 5.11 | 0.000 | 0.021 | Yes |
| Trade & pers serv controls, trends | 0.022 | 0.006 | 3.47 | 0.001 | 0.023 | Yes |

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 A6: Event study estimates of public 2 year college graduation of Non Resident Alien undergraduates- Coefficients of Tuition Subsidy reform* share of undocumented immigrants in 1990

| | Leads+lags |
|---|---|
| 5 years before policy | 0.003 |
| | (0.006) |
| 4 years before policy | -0.0004 |
| | (0.008) |
| 3 years before policy | 0.010 |
| | (0.009) |
| 2 years before policy | -0.002 |
| | (0.004) |
| On year of policy implementation | 0.026** |
| | (0.012) |
| 1 year after policy | 0.033** |
| | (0.014) |
| 2 years after policy | 0.032** |
| | (0.016) |
| 3 years after policy | 0.052** |
| | (0.022) |
| 4 years after policy | 0.051* |
| | (0.027) |
| 5 years after policy | 0.061** |
| | (0.028) |
| Covariates | No |
| Institution & Year FE | Yes |
| Observations | 10,845 |

Notes: The data is taken from IPEDS. Regressions are weighted according to how much of the total student population the institution represents. 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: 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 | 13,923 |
| | (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 | 10,845 |
| | (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 | 13,683 |
| | (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 according to how much of the total student population the institution represents. 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: 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) |
| Dep var mean | 0.00083 | 0.0013 | 0.00043 |
| 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 A9: Tuition subsidy reforms and policy interactions

| | College enrollment | College Graduation | Fertility |
|---|---|---|---|
| | (1) | (2) | (3) |
| Tuition subsidy | 0.036** | 0.028** | |
| | (0.014) | (0.012) | |
| Tuition subsidy * Financial aid | 0.010 | -0.013 | |
| | (0.016) | (0.016) | |
| Tuition subsidy * Drivers'license | 0.035** | -0.022 | |
| | (0.016) | (0.052) | |
| Tuition subsidy * Immigration enforcement | | | −0.0284** |
| | | | (0.0125) |
| Individual controls | Yes | Yes | Yes |
| State controls | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes |
| Observations | 39,098 | 43,631 | 26,342 |

Notes: Col 1 and col 2 estimates pertain to likely undocumented students. Col 3 estimates are for likely undocumented female students. Main effects are included in the regression but their estimates are not shown. Regressions are weighted using person weights from IPUMS CPS in case of college enrollment and college graduation / IPUMS USA in case of fertility. 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 A10: Robustness checks for likely undocumented students

| **Panel A: College enrollment** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Drop summer months | Drop southern states | Drop states explicitly banning subsidy | Drop states that never offered subsidy |
| Tuition subsidy | 0.039** | 0.042** | 0.031* | 0.032 |
| | (0.017) | (0.018) | (0.016) | (0.020) |
| Observations | 28,867 | 32,922 | 35,408 | 29,624 |
| **Panel B: College Graduation** | | | | |
| | (1) | (2) | (3) | (4) |
| | Keep fall,spring months | Drop southern states | Drop states explicitly banning subsidy | Drop states that never offered subsidy |
| Tuition subsidy | 0.051** | 0.025* | 0.011 | 0.032** |
| | (0.021) | (0.014) | (0.013) | (0.012) |
| Observations | 18,261 | 36,237 | 39,344 | 32,400 |

Notes: Regressions are weighted using person weights from IPUMS CPS. All regressions include individual controls, state controls, other policies , state and date fixed effects. 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: Coefficients of tuition subsidy in dollars

| | College enrollment | College graduation |
|---|---|---|
| Tuition subsidy (dollars in 1000s) | 0.0012** | 0.0011* |
| (Public 4 year colleges) | (0.0006) | (0.0006) |
| Tuition subsidy (dollars in 1000s) | 0.0054* | 0.0066** |
| (Public 2 year colleges) | (0.0029) | (0.0028) |
| Observations | 36,508 | 40,951 |
| Individual controls | Yes | Yes |
| State controls | Yes | Yes |
| State FE | Yes | Yes |
| Date FE | Yes | Yes |

Notes: All coefficients pertain to likely undocumented students. The time period used in these regressions is 2000-2016. 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 A12: Triple difference for family formation outcomes with Hispanic citizens as additional control group

Coefficients of Tuition Subsidy reform

| | (1) | (2) | (3) | Observations |
|---|---|---|---|---|
| **Panel A: At least one child under age 5 (females)** | | | | |
| LU immigrants | −0.039** | −0.030** | −0.030* | 61,372 |
| | (0.016) | (0.015) | (0.017) | |
| **Panel B: Given birth to child in last 12 months** | | | | |
| LU immigrants | -0.041*** | -0.040*** | -0.038** | 46,147 |
| | (0.011) | (0.010) | (0.011) | |
| **Panel C: Individual is never married/single** | | | | |
| LU immigrants | 0.018* | 0.024*** | 0.031*** | 134,494 |
| | (0.009) | (0.008) | (0.010) | |
| **Panel D: Individual is married** | | | | |
| LU immigrants | −0.018** | −0.023*** | −0.032*** | 134,494 |
| | (0.008) | (0.007) | (0.009) | |
| **Panel E: Living with parents(females)** | | | | |
| LU immigrants | -0.036* | -0.050** | -0.048** | 61,372 |
| | (0.021) | (0.020) | (0.021) | |
| **Panel F: Living with unmarried partner(females)** | | | | |
| LU immigrants | 0.032*** | 0.031*** | 0.031*** | 61,372 |
| | (0.010) | (0.009) | (0.009) | |
| 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 | |
| StateXTime FE | Yes | Yes | Yes | |
| StateXCitizenship FE | Yes | Yes | Yes | |
| CitizenshipXTime 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. 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: Heterogenous effects by gender

| | LU Immigrants | Foreign-born citizens | Natives |
|---|---|---|---|
| **Panel A: College enrollment - Male** | | | |
| Tuition subsidy reform | 0.014 | -0.002 | 0.007 |
| | (0.017) | (0.021) | (0.008) |
| Observations | 21,280 | 19,656 | 820,402 |
| **Panel B: College enrollment - Female** | | | |
| Tuition subsidy reform | 0.037** | -0.034 | 0.006 |
| | (0.016) | (0.021) | (0.007) |
| Observations | 17,818 | 19,803 | 850,904 |
| **Panel C: College graduation - Male** | | | |
| Tuition subsidy reform | 0.012 | 0.035 | -0.004 |
| | (0.021) | (0.037) | (0.005) |
| Observations | 24,000 | 40,050 | 805,940 |
| **Panel D: College graduation - Female** | | | |
| Tuition subsidy reform | 0.042** | 0.009 | 0.008 |
| | (0.017) | (0.022) | (0.006) |
| Observations | 19,631 | 46,967 | 885,342 |

Notes:Regressions are weighted using person weights from IPUMS CPS. All regressions include individual controls, state controls, other policies , state and date fixed effects. 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: Effects of tuition subsidy policy 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 A15: Heterogenous effects by region

| | Northeast | Midwest | South | West |
|---|---|---|---|---|
| **Panel A: College enrollment** | | | | |
| Tuition subsidy reform * Region | 0.009 | 0.041** | 0.041*** | 0.033* |
| | (0.027) | (0.020) | (0.015) | (0.017) |
| Observations | 39,098 | 39,098 | 39,098 | 39,098 |
| **Panel B: College Graduation** | | | | |
| Tuition subsidy reform * Region | 0.011 | 0.043*** | 0.030* | 0.006 |
| | (0.038) | (0.013) | (0.015) | (0.015) |
| Observations | 43,631 | 43,631 | 43,631 | 43,631 |

Notes: All coefficients pertain to likely undocumented students. Regressions are weighted using person weights from IPUMS CPS. All regressions include individual controls, state controls, state and date fixed effects. 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: Effect of tuition subsidy reform on education outcomes using ACS data

| | (1) | (2) | (3) | (4) | Observations |
|---|---|---|---|---|---|
| College enrollment | 0.024** | 0.026** | 0.028** | 0.038*** | 57,262 |
| | (0.010) | (0.011) | (0.011) | (0.012) | |
| College graduation | 0.027** | 0.024** | 0.025** | 0.037** | 78,455 |
| | (0.010) | (0.011) | (0.010) | (0.018) | |
| Log no. of college yrs | 0.078** | 0.063** | 0.066** | 0.081** | 27,168 |
| | (0.034) | (0.030) | (0.031) | (0.039) | |
| 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 | |
| Year FE | Yes | Yes | Yes | Yes | |
| State time trends | No | No | No | Yes | |

Notes: All coefficients pertain to likely undocumented students. 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 A17: Difference-in-differences Estimator Decomposition (State year panel)

| | (Earlier T vs Later C) | (Later T vs earlier C) | (T vs never T) | (Obs) |
|---|---|---|---|---|
| College enrollment | -0.002 | 0.026 | 0.027 | 57,262 |
| | [0.117] | [0.135] | [0.749] | |
| Diff-in-diff | 0.024 | 0.024 | 0.024 | |
| College graduation | 0.014 | 0.026 | 0.028 | 78,455 |
| | [0.108] | [0.129] | [0.764] | |
| Diff-in-diff | 0.027 | 0.027 | 0.027 | |
| Log no. college years | 0.176 | 0.041 | 0.062 | 27,168 |
| | [0.169] | [0.116] | [0.715] | |
| Diff-in-diff | 0.078 | 0.078 | 0.078 | |
| Atleast 1 child below 5yrs | 0.033 | -0.033 | -0.019 | 26,342 |
| | [0.116] | [0.145] | [0.739] | |
| Diff-in-diff | -0.016 | -0.016 | -0.016 | |
| Birth last year | -0.077 | -0.020 | -0.018 | 8,503 |
| | [0.043] | [0.154] | [0.803] | |
| Diff-in-diff | -0.019 | -0.019 | -0.019 | |
| Controls | No | No | No | |
| State FE | Yes | Yes | Yes | |
| Year FE | Yes | Yes | Yes | |

Notes: All coefficients pertain to likely undocumented students. T stands for treatment and C stands for control. Each column corresponds to a 2X2 estimate from Goodman Bacon decomposition with corresponding weights in brackets below the estimates. All regressions include no controls, state and year FE. Regressions are weighted using person weights from IPUMS ACS and aggregated to form a state-year panel.

Table A18: Difference-in-differences Estimator Decomposition (Institution year panel)

| | (Earlier T vs Later C) | (Later T vs earlier C) | (T vs never treated) |
|---|---|---|---|
| Instate tuition-Public 4yr | 0.102 | -0.09 | 0.047 |
| | [0.173] | [0.052] | [0.775] |
| Diff-in-diff | 0.049 | 0.049 | 0.049 |
| Observations | 12,708 | 12,708 | 12,708 |
| Outstate tuition-Public 4yr | -0.011 | -0.045 | -0.029 |
| | [0.173] | [0.052] | [0.775] |
| Diff-in-diff | -0.027 | -0.027 | -0.027 |
| Observations | 12,708 | 12,708 | 12,708 |
| Instate tuition-Public 2yr | 0.103 | -0.081 | 0.032 |
| | [0.155] | [0.071] | [0.773] |
| Diff-in-diff | 0.035 | 0.035 | 0.035 |
| Observations | 15,750 | 15,750 | 15,750 |
| Outstate tuition-Public 2yr | 0.047 | -0.173 | -0.123 |
| | [0.155] | [0.071] | [0.774] |
| Diff-in-diff | -0.100 | -0.100 | -0.100 |
| Observations | 15,750 | 15,750 | 15,750 |
| Instate tuition- Flagship | 0.093 | -0.033 | 0.065 |
| | [0.113] | [0.114] | [0.772] |
| Diff-in-diff | 0.057 | 0.057 | 0.057 |
| Observations | 7,992 | 7,992 | 7,992 |
| Outstate tuition- Flagship | 0.062 | -0.025 | 0.004 |
| | [0.113] | [0.114] | [0.772] |
| Diff-in-diff | 0.007 | 0.007 | 0.007 |
| Observations | 7,992 | 7,992 | 7,992 |
| Controls | No | No | No |
| Institution FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

Notes: T stands for treatment and C stands for control. Each column corresponds to a 2X2 estimate from Goodman Bacon decomposition with corresponding weights in brackets below the estimates. All regressions include no controls, institution and year FE. Regressions are unweighted and hence coefficients differ from column 1 of Table 11.