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How States Can Reduce the Dropout Rate for Undocumented Immigrant Youth: The Effects of In-State Resident Tuition Policies

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Abstract

As of December 2011, 13 states have adopted an in-state resident tuition (IRT) policy that provides in-state tuition to undocumented immigrants and several other states are considering similar legislation. While previous research focuses on how IRT policies affect college entry and attainment, this study examines the effect these policies have on high school dropout behavior. Using the Current Population Survey (CPS) and difference-in-difference models, this paper examines whether IRT policies reduce the likelihood of dropping out of high school for Mexican foreign-born non-citizens (FBNC), a proxy for undocumented youth. The policy is estimated to cause an eight percentage point reduction in the proportion that drops out of high school. The paper develops an integrated framework that combines human capital theory with segmented assimilation theory to provide insight into how IRT policies influence student motivation and educational attainment at the high school level.

Keywords

Immigrant; Education Policy; Latino; Hispanic; Adolescent; Immigration Policy

Responding to the lack of comprehensive immigration policies at the federal level, states have increasingly sought to protect their own interests by adopting state and local level immigration related policies (Gonzales 2009; GoŸdziak and Martin 2005; Laglagaron et al. 2008; Olivas 2008). A policy area that has captured significant state attention is determining college access for undocumented immigrants. In 1996, the federal Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) prohibited states from providing in-state resident tuition benefits to undocumented immigrants unless all US citizens and nationals were eligible for the same benefits. Within the guidelines of the IIRIRA, however, several states have reduced access barriers to higher education for undocumented immigrants residing in their state (Flores and Chapa 2009).

As of December 2011, 13 states have adopted an in-state resident tuition (IRT) policy that provides in-state tuition to undocumented immigrants and at least 20 others have considered similar legislation (IHELG 2008; NCSL 2010; NILC 2012; Olivas 2010). Given that out of

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state tuition often exceeds 140% of resident tuition, the size of these tuition discounts is substantial (Gonzales 2009). For the undocumented population, which is ineligible for federal and most state financial aid (Frum 2008; Szelenyi and Chang 2002) and which experiences high rates of poverty (Borjas 2011; Gonzales 2009; Passel 2005a), this tuition discount can significantly reduce the financial burden a family faces when trying to send their children to college.

While previous research focuses on how IRT policies affect college entry and attainment (Chin and Juhn 2011; Flores 2007, 2010a; 2010b; Flores and Chapa 2009; Kaushal 2008), this study examines the effect these policies have on high school dropout behavior. One of the main pro-policy arguments is that by offering a more affordable college education, IRT policies provide a strong incentive for high school completion (Fuligni and Perreira 2009; Gonzales 2009; Murray, Batalova and Fix, 2007; NILC 2005; Reich and Barth 2010; Russell 2007). Policy advocates argue that financial barriers to higher education imposed by out of state tuition costs decrease student motivations and contribute to the high dropout rate for undocumented youth (Abrego 2006; Horwedel 2006; Marklein 2003; Mead 2004; Menjìvar 2008; Milliken 2010). State estimates in Nebraska, for instance, suggest that 50% of undocumented immigrant youth drop out of high school (Milliken 2010), and national estimates of adults (age 25–64) indicate that undocumented immigrants are less likely to have a high school diploma (50%) than legal immigrants (75%) and natives (91%; Passel 2005b). Particularly concerning is the educational attainment of undocumented Mexican-American immigrants. While the majority of Mexican-American youth are documented, Mexican-Americans make up the majority (59%) of the undocumented population (Gonzales 2009; Passel 2008) and have the highest dropout rate of any immigrant group (Fry 2003; NCES 2009; Perreira et al. 2006).

Prior research indicates that undocumented immigrant children—which make up almost two million of the nation's K-12 population (Passel 2005b)—face significant financial, legal, and cultural challenges that hinder high school attainment. Similar to their documented counterparts, undocumented immigrant Latino youth suffer several known risks for high school failure (Perreira, Harris, and Lee 2006), including living in low SES families, attending resource poor schools, and living in racially and economically isolated neighborhoods (Gonzales 2009; Greenman and Hall 2013; Suàrez-Orozco et al. 2011). Unlike their documented counterparts, however, undocumented Latino youth have fewer cultural resources, including school attachment, parental engagement, and college aspirations, to buffer the negative consequences of these risks (Abrego 2006; Perreira, Harris and Lee 2006; Suàrez-Orozco et al. 2011). Moreover, as these youth enter high school they begin their "transition to illegality" as they confront legal limitations in obtaining a driver's license, applying for a part-time job, and accessing higher education (Gonzales 2011). As a result, many of these youth are "pulled-out" of high school to help meet family economic needs and to adhere to the strong work-ethic values associated with Mexican labor migration (Bachmeier and Bean 2011; Bradley and Renzulli 2011).

In an economic era where the financial and employment consequences of high school dropout have never been higher (Lofstrom 2007), the decision to leave high school early will have lasting implications for the well-being of undocumented immigrant youth and the

states where they reside. To the extent that IRT policies encourage undocumented immigrant youth to stay in school by reducing institutional barriers, these policies will shape the future economic trajectories of these youth and the states where they reside. Consequently, states have a vested interest in determining whether providing in-state tuition to undocumented immigrants can reduce dropout behavior.

Using the Current Population Survey (CPS), this paper employs a difference-in-difference model (DD) to examine whether IRT policies targeting undocumented immigrants reduce the likelihood of dropping out of high school for Mexican foreign-born non-citizens (FBNCs), one of the strongest proxies available for undocumented youth. The paper develops an integrated framework that combines human capital theory (Becker 1964) with segmented assimilation theory (Portes and Rumbaut 2006) to provide insight into how IRT policies influence high school dropout decisions.

Background

In-State Tuition Policy History

In 2001, Texas adopted the first IRT policy that allowed undocumented students who meet specific residency criteria to qualify for in-state tuition.[1] As of December 2011, 12 other states—California in 2001; Utah and New York in 2002; Washington, Oklahoma[2] and Illinois in 2003; Kansas in 2004; New Mexico in 2005; Nebraska in 2006; Wisconsin[3] in 2009; Maryland and Connecticut in 2011—have adopted similar policies. Several other states have also considered similar legislation but had not yet enacted it as of date (Flores 2007; NCSL 2011; NILC 2012; Olivas 2010; Rhymer 2005).

The adoption of these IRT policies remains controversial. Several legal challenges[4] have been made against these policies and other states have adopted or considered counter legislation. Four states—Arizona, Colorado, Georgia, and Indiana—have barred undocumented immigrants from receiving in-state tuition benefits (NCSL 2011). South Carolina and Alabama have gone a step further and banned undocumented students from attending any of it public colleges, while for a short time North Carolina's state's attorney general banned undocumented students from attending community colleges (Gonzales 2009; NILC 2011). The map in Figure 1 provides a geographic description of the states that have adopted or considered IRT policy legislation (Flores 2007; Olivas 2008; NCSL 2010, 2011; NILC 2011, 2012; Rhymer 2005; Zaleski 2008). The states labeled in solid dark grey have adopted an IRT policy,[5] while the states labeled with cross-hatches have adopted an IRT ban

[1]To adhere with the IIRIRA regulations, states have adopted conditions for eligibility to ensure that US citizens and legal permanent residents (LPRs) who meet the policy requirements but no longer live in the state also qualify for the in-state tuition rate. While the specific conditions vary from state to state, each state policy includes three general requirements (NILC 2009): 1) attend a school in the state for a certain number of years; 2) graduate from high school in the state or receive a state issued GED; and 3) sign an affidavit stating that they have either applied to legalize their status or will do so as soon as eligible.

[2]In 2007 Oklahoma adopted a repeal that prohibited undocumented immigrants from receiving in-state tuition benefits. The State Board of Regents, however, can award in-state tuition waivers for undocumented immigrants if they meet same criteria specified in the original IRT policy.

[3]Wisconsin revoked its law in June 2011 (NCSL 2011).

[4]In 2005, the same group of lawyers challenged both the Kansas and California statutes. The Kansas court ruled that the plaintiffs had no legal standing since only the Department of Homeland Security (DHS), not private citizens, has the right to enforce IIRIRA (IHELG 2008). The California Supreme Court upheld the IRT law by overturning a 2008 appellate court decision that had repealed the law (Lara 2011).

that excludes undocumented immigrants from receiving in-state tuition or have banned student from attending college all together. States in white with dots have considered both an IRT policy and an IRT ban, while states in grey with dots and states with diagonal lines (respectively) have only considered one of these policies. Lastly, the states in solid white have considered no legislative action. Overall, the figure reveals no strong regional trend or political state leanings (e.g., both heavily conservative states like Utah and liberal states like California have passed the law) driving state IRT policy legislation and indicates that the majority of US states have at least considered IRT policy legislation.

Factors influencing state adoption of either pro or anti undocumented student tuition policies remain largely unpredictable and no clear trends in state demographics have been detected (Flores 2007, 2010a; 2010b; Flores and Chapa 2009; Vargas 2011). Instead, case studies on the adoption of IRT policies suggests that the adoption of these policies is largely determined by idiosyncratic political processes related to policy framing (e.g., education vs. immigration), the social construction of the policy targets (e.g., children vs. criminals), and perceptions of jurisdictional authority (state vs. federal; Reich and Barth 2010; Reich and Mendoza 2008). Moreover, examining state legislative agenda setting, a crucial first step to policy adoption, McLendon, Mokher, and Flores (2011) found no evidence that state differences in political ideology, economic influences, or Latino legislative representation contributed to the likelihood that an IRT policy would achieve the legislative agenda. They did, however, find that female legislative representation and the size of the foreign-born population increased the likelihood that state legislatures would consider an IRT policy.

Lessons from Research on IRT Policies and College Behavior

Research indicates that IRT policies affect the post-secondary schooling decisions of young adults most likely to be undocumented. In her assessment of Mexican foreign-born non-citizen (FBNC; a proxy for undocumented) young adults (ages 17–28), Kaushal (2008) found that IRT policies increased college enrollment, proportion of students with some college education, and proportion of students with at least an associate's degree. Using FBNC Latinos as a proxy for undocumented, Flores and Chapa (Flores 2007, 2010a; 2010b; Flores and Chapa 2009) also found that IRT policies increased college enrollment rates but the effect was stronger for males and for states with long migration histories, which presumably had more resources to help students take advantage of the policy. Overall, the impact of IRT policies remains relatively small—college enrollment and attainment rates increased by only a few percentage points—in large part because a low percent of undocumented youth graduate from high school (Flores and Chapa 2009; Kaushal 2008).

Previous work has not thoroughly examined how IRT policies affect the high school dropout rate of undocumented youth. In their assessment of college behavior, Chin and Juhn (2011) and Kaushal (2008), found suggestive evidence that IRT policies influenced high school completion and dropout behavior, but the results were not significant. These results, however, should be interpreted cautiously. First, the narrow time span of their analyses (up to 2005 in both studies) may preclude their ability to detect a policy effect (Chin and Juhn

[5]Though not shown in this map, most of these policy adoption states have also considered counter legislation that would overturn the IRT policy.

2011). Second, Kaushal's analysis focused on high school completion of young adults ages 17–22 instead of dropout behavior of high school aged youth. Because immigrant youth, particularly Latino immigrants, are significantly more likely to repeat grades (Kao 1999; Tillman, Guo, and Harris 2006) they are less likely to graduate on time. Consequently, Kaushal's estimates are likely to be downwardly biased (i.e. find too little of an effect), since many of the young adults (especially those ages 17–19) examined have not had sufficient time to complete their high school education. Moreover, prior research indicates that measures of high school retention, attrition, and on-time completion provide a more accurate picture of youth performance in high school than the high school completion measure (Pharris-Ciurej, Hirschman, and Willhoft 2012). By focusing on the dropout behavior of 16–19 year olds, expanding the policy time frame, and assessing potential moderating effects this paper provides a stronger assessment of how IRT policies affect high school performance.

An Integrated Theoretical Framework: Human Capital and Segmented Assimilation

Within the human capital framework, the logic for how IRT policies affect educational attainment at the high school level is twofold. First, states that adopt IRT policies increase the post-secondary educational opportunities for undocumented immigrants by reducing the cost of tuition. Second, this future price reduction alters the cost-benefit calculation for human capital investment at the high school level. According to segmented assimilation theory (Portes and Rumbaut 2001; Portes and Zhou 1993), this cost-benefit calculation is also altered by the change in the social context of reception that the IRT policy creates. Policies of receiving governments define the economic and social opportunities afforded to immigrant populations, and in turn, shape the benefits and costs associated with the high school investment decision (Portes and Rumbaut 2006). In combination, these theories suggest that the human capital investment decision of undocumented immigrant youth is constrained by the low economic resources and high social barriers these youth face and that IRT policies can reduce these constraints.

Human Capital—Developed by Becker (1964), the human capital perspective assumes that individuals decide to invest in their education by weighing the expected benefits and costs—both monetary and non-monetary—of that decision. Through this calculation, individuals choose the optimal level of investment (i.e. maximize utility) that best aligns with their preferences. Similar to workers who must choose between labor and leisure, high school students must choose between hours of continued schooling and leisure by balancing the costs (e.g., time, income, and psychological stress) and benefits (e.g., future earnings and social prestige) of additional years of education. If higher grades in high school results in future higher earnings and/or educational opportunities, students may be willing to forego an hour of leisure, such as watching TV or playing video games, in order to invest that time in studying (Henry and Rubenstein 2002). Students make this decision, however, within economic and social environments that determine the availability of opportunities, and, thus, add to or detract from their costs and benefits (Becker 1993). For instance, low-income immigrant youth who work to support their families may have fewer hours of leisure to trade for studying.

Segmented Assimilation—The human capital investment decision of undocumented immigrant youth can be constrained by the social and economic challenges they encounter during the process of assimilation. According to the theory of segmented assimilation (Portes and Rumbaut 2001, 2006; Portes and Zhou 1993), the success of an immigrant's adaptation depends on a multitude of factors that comprise the social context of reception. These include congruence in the pace of acculturation within a family, economic barriers such as joblessness and concentrated poverty and social barriers such as racial discrimination (Portes and Rumbaut 2006) or the social isolation of minority groups (Massey 1990).

Thus, for undocumented immigrant children the human capital investment decision may be more complex than the traditional two good dichotomy (i.e. leisure and schoolwork). The severe financial hardships many undocumented immigrant families face can significantly constrain their educational investment decisions and force youth to choose not only between schoolwork and leisure but between work obligations at school and work obligations at home. With 40% of undocumented children living below the poverty line (compared to 17% of US-born children; Gonzales 2009), many undocumented immigrant youth must support their families by working part-time (and sometimes full-time) jobs and/or by helping parents run the household (e.g., cooking, cleaning, and caring for younger children; Fuligni 2001; Perreira et al. 2007). For them, allocating additional time to schoolwork may require a greater sacrifice than simply missing their favorite TV show. It may mean that their family has less money for basic necessities or that a younger sibling has no one to care for them.

Many immigrant families make the financial and familial sacrifices needed to invest in their youths' schooling because, in part, it means obtaining a better paying job that will enable youth to support their parents in the future (Fuligni 2001). However, if high school completion does not result in future higher earnings or advanced educational opportunities, undocumented youth may see little reason to forgo current income and the opportunity to economically support their families. Consequently, these youth may choose to enter the labor force at an earlier age and forgo additional years of education. These consequences are apparent in qualitative research, which finds that for many undocumented immigrant youth, knowing that even with a college degree their legal status will preclude them from obtaining a white collar, professional job (Greenman and Hall 2013), they become discouraged and lack the motivation to complete their high school coursework (Abrego 2006; Suàrez-Orozco et al. 2011).

In addition to economic hardships, undocumented immigrant youth must often overcome barriers due to social discrimination that can reduce the expected gains from their human capital investment. Policies of receiving governments as well as attitudes of natives can shape the non-monetary psychological cost and benefits of education (Portes and Rumbaut 2001, 2006). Within the school system, these social discrimination barriers serve as "pushout" factors that hinder the development of supportive school relationships and discourage students from staying in school (Bradley and Renzuilli 2011). By overriding the federal government's more exclusionary tuition policy, IRT policies are actively welcoming undocumented youth into their higher educational system and potentially reducing the psychological costs associated with social marginalization. Extant research has shown that

perceived discrimination detracts from immigrant youth's self-esteem (Rumbaut 1999), lowers their overall educational expectations and academic motivations (Rumbaut 1999; Schmader, Major and Gramzow 2001; Perreira et al. 2010), hinders their academic performance (Degarmo and Martinez 2006; Stone & Han, 2005), and increases their likelihood of dropping out of high school (Degarmo and Martinez 2006).

Preliminary evidence from California's IRT policy suggests that IRT policies have served as a welcoming symbol to undocumented Latino youth and increased their sense of social belonging. Conducting interviews with undocumented Latino youth before, shortly after, and 4-years after the policy passage, Abrego (2008) found that the policy reduced students' fear and stigma associated with being undocumented, provided students with a new positive identity, and increased their sense of legitimacy to claim their new right and to mobilize for new rights.

Research has not yet examined whether the psychological benefits associated with the passage of an IRT policy extends beyond the undocumented Latino population and benefits all Latino youth. Given that society frequently stereotypes Latinos, particularly Mexicans, as undocumented (Timberlake and Williams 2012), Latino youth in general may feel more accepted and less scrutinized with the passage of an IRT policy. Thus, I examine the effect IRT policies have on the Latino population in general.

Methods

Data

To be consistent with prior research on the effects IRT policies have on college behavior (Flores 2007, 2010a; 2010b; Flores and Chapa 2009; and Kaushal 2008), this paper employs a similar research design. The paper uses the Merged Outgoing Rotation Group (MORG) file from the Current Population Survey (CPS), a nationally representative sample sponsored by the U.S. Census Bureau and U.S. Bureau of Labor Statistics, for the years 1998 to 2011 (Feenberg and Rothl, 2007). Using a multistage stratified sample, the CPS collects monthly demographic and employment information from about 60,000 housing units across the United States for the civilian population age sixteen and older. Using a rotating interview system, each housing unit in the CPS is interviewed for four consecutive months, then ignored for the next eight months, and then interviewed again for four more months. The household unit and not the occupants are the sample, so if individuals or families move from a household unit they are not followed. Instead the new occupants are interviewed. The MORG file is a sub-set of the CPS, which combines survey information from months four and eight into one file for each housing unit surveyed, which means that individuals appear only once in any file year but may reappear in the next year. The MORG files have information on approximately 30,000 individuals for each monthly extract.

The CPS data have several strengths for assessing how high school dropout behavior has changed as states have adopted IRT policies. First, the data provide monthly, repeated cross-sections of a national sample of individuals that span the pre and post periods surrounding the adoption of IRT policies.[6] Second, while the CPS focuses on labor market outcomes, the data have information on educational attainment. Lastly, the CPS includes undocumented

immigrants in the survey[7] and has a strong proxy—Mexican foreign-born non-citizen (FBNC)—for their identification.[8] For ethical reasons, no governmental agency in the U.S. and few research surveys collect or indicate information on documentation status (Passel 2005b). Instead, researchers must rely on proxies for undocumented status. Treated separately or in combination Mexican ethnic identity, foreign-born status, and non-citizenship do not equate to undocumented status. However, given that 59% of undocumented immigrants are of Mexican origin and that more than half (56%) of foreign-born Mexicans are undocumented (compared to approximately 26% for non-Mexican foreign-born Latinos; Passel and Cohn 2008), FBNC Mexican is one of the strongest proxies available (Kaushal 2008). Among recent arrivals, the FBNC Mexican proxy is even stronger; approximately 80% to 85% of foreign-born Mexicans who have been in the US for less than ten years are undocumented (Passel and Cohn 2008).

Though Mexican FBNC is a strong proxy for undocumented status, reliance on this proxy does introduce measurement error. This is because my policy effect is averaged over the effect for undocumented immigrants and legal permanent residents (LPRs), the latter of which is unaffected by the IRT policy. The inclusion of LPRs downwardly biases the estimate and makes it more difficult to identify a policy effect. Providing a rough calculation of this measurement error problem, Kaushal (2005) estimates that a presence of 20% LPRs in the Mexican FBNC sample (a conservative estimate given that about 60% of foreign-born Mexicans are undocumented, meaning 40% should be LPRs or other lawfully present immigrants) along with the 10% undercount of the undocumented population in the CPS (Passel 2005a) leads to a 28% downward bias of the estimated policy effect. This would mean that the true policy effect should be 1.28 times the value of the estimated policy effect. Moreover, this measurement error increases the variance of the estimate and the probability of a Type II error—failing to reject the null hypothesis of no effect when the policy actually has an effect (Wooldridge 2010).

Sample

The primary sample (N=6,603) includes all self-identifying Mexican FBNCs aged 16–19 in the MORG files between the years 1998 and 2011. I focus on 16–19 year olds because research on Latinos suggests using a narrower age range (than the standard 16–24 year old range) to exclude labor migrants who come to the US to work and never enter the school system (Fry 2003). [9] The sample consists of legal permanent resident and foreign-born non-citizen Mexicans. For comparative purposes, I also include samples of non-Latino white, non-Latino black, and other Latino youth.

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[6]This is the main advantage over the American Community Survey, which was not fully implemented until 2005.
[7]Research by Passel (2005a) and the Census Bureau estimates that both the CPS and Census undercount the undocumented population by about 10% (Kaushal 2008).
[8]According to the Census Bureau, Census and CPS data are similarly effective at identifying the non-citizen population. Comparing Citizenship data from the American Community Survey (ACS) and Annual Social and Economic Supplement (ASEC) to the CPS, Menendez (2004) found that the identification of the proportion of non-citizens was higher (3 percentage points) in the ASEC than the ACS.
[9]Some researchers suggest using an even narrower age range (15–17; Fischer 2010; Oropesa and Landale 2009). The sample size was too small to run on this group, but I did run additional labor migrant checks. I excluded individuals not living with a parent or relative with the assumption that they are more likely to be labor migrants. Results were robust.

Outcome Measure

Dropout Status—I create a dropout status indicator (1=dropout; 0=else) for each individual using the National Center for Education Statistics' (NCES 2009) definition for status dropout, which is defined as not being enrolled in school and not having earned a high school diploma or GED.[10]

Analytical Strategy

This paper employs an extension of the difference-in-difference (DD) model that capitalizes on the exogenous variation created by each state's IRT policy adoption (Abadie 2005; Besley and Case 2000). The traditional DD estimate essentially compares an individual's likelihood of dropping out of high school after the adoption of an IRT policy to the likelihood of dropping out of high school for two groups: 1) a cross-section of Mexican FBNCs living in the same state but before the IRT policy was adopted (i.e. a pre and post comparison), and 2) a cross-section of Mexican FBNCs at the same time but residing in similar states that have not adopted the policy (i.e. a treatment and control comparison). In order to calculate an unbiased policy estimate, the DD estimate makes two assumptions. First, the treatment and control group are exposed to and respond similarly to other policy changes and general shocks (e.g., the adoption of No Child Left Behind or national immigration reforms). Second, the treatment and control group experience common trends (e.g., similar increases or decreases in high school dropout rates and/or growth in the immigrant population).

This paper makes several improvements to the traditional DD estimate. First, by using state fixed effects instead of a simple treatment dummy variable I allow for different intercepts for each state rather than just for the treatment and control group. These state fixed effects recognize that time invariant characteristics may be unique for each state within the treatment and control groups. Secondly, by using year fixed effects instead of a simple post dummy variable I allow for general shocks or time trends to differ across each year rather than just the pre-post period. Lastly, by interacting state fixed effects by year I control for state-specific linear trends that may be correlated with the educational outcomes of Mexican FBNCs. State-specific linear trends control for pre-policy trends in each state (e.g., growth in the undocumented population or decline in the dropout rate) that may confound the estimated policy effect. Policy effects are identified off of differences in these trends post-policy enactment (with a one-year lag) relative to the state's trends in the likelihood of dropout.

To answer whether state resident tuition policies decreased a student's likelihood of dropping out among FBNC Mexican students I estimate the following linear probability regression model:

[10]There is considerable debate as to whether GED recipients should be counted as high school graduates given that they have lower economic and post-secondary educational outcomes than regular high school graduates (Tyler and Lofstrom 2009). I follow the NCES definition because it is the most widely used indicator for high school dropout rates (Tyler and Lofstrom 2009). Most importantly, though, the NCES definition allows me to identify the full IRT policy effect given that both GED recipients and regular high school graduates are eligible for the policy.

$$\mathrm{DROPOUT}_{\mathrm{ijtm}}=\beta_0+\beta_1(\mathrm{POLICYSTATE}_{\mathrm{jt-1}})+\beta_2\,(\mathrm{INDIVIDUAL\ CHARACTERISTICS}_{\mathrm{ijtm}})+\beta_3\,(\mathrm{STATE\ CONDITIONS}_{\mathrm{jtm}})+\beta_4\,(\mathrm{STATEDUMMIES}_{\mathrm{j}})+\beta_5(\mathrm{YEARDUMMIES}_{\mathrm{t}})+\beta_6(\mathrm{MONTHDUMMIES}_{\mathrm{m}})+\beta_7(\mathrm{STATEDUMMIES}_{\mathrm{j}}*\mathrm{YEAR}_{\mathrm{t}})+\varepsilon_{\mathrm{i}}$$

i = 1, ... N (individuals)

j = 1, ..., 51 (states)

t = 1998, ...2011 (years)

m = 1, ...12 (months)

where $\mathrm{DROPOUT}_{\mathrm{ijtm}}$ is a binary indicator equal to 1 if the individual is a high school dropout. $\mathrm{POLICYSTATE}_{\mathrm{jt-1}}$ is a binary indicator equal to 1 if a state provided in-state tuition to undocumented immigrants in t−1.[11] This coefficient β_1 is the DD estimate, which indicates the effect of the policy for all Mexican FBNCs residing in a policy state a year after the policy was enacted.[12] The policy states include TX, CA, UT, NY, WA, OK, IL, KS, NM, NB and WI.[13] β_2 represents the coefficients from a vector of individual and household demographic controls that have been shown to affect an individual's likelihood of dropping out, including age, gender, living in an MSA, employment status, household structure[14] (household position of youth: non-family member, head of household, spouse, relative, and child), highest household education level[15] (less than high school, high school degree, some college, and college degree) and average years in the US (Carter 2005; Fry 2005; Perreira et al. 2006; Roscigno et al. 2005). β_3 represents the coefficients from a vector of time varying state-characteristics that may be correlated with policy adoption, including: the monthly unemployment rate (from the Bureau of Labor Statistics) to control for state specific economic shocks (McLendon, Mokher, and Flores 2011); the proportion of non-Latino white adults (ages 30–54) with a high school diploma and the proportion with some college to control for state-specific trends in education (Kaushal 2008); the proportion of Mexican adults (ages 30–54) with at least a high school diploma to control for state trends in Mexican educational aspirations (Foster and McLanahan 1996; Kaushal 2008); and the proportion of Mexican FBNCs in the population to control for state-specific migration trends. B_4 represents the coefficients from state fixed effects that control for both time invariant unobserved and observed state characteristics (e.g., state-specific educational policies or stagnant demographic composition). B_5 represents the coefficients from year fixed effects that control for general shocks or time trends presumed to affect both policy and non-policy states equally, such as national educational policies (e.g., NCLB) and trends

[11] I use the lagged policy variable (t−1) to identify the policy effect because evidence suggests it may take time for immigrant communities to become aware of the policy and change their behavior (Abrego 2008). Moreover, given the political tensions surrounding these policies (i.e. the legal challenges and counter legislation) many immigrant youth may have initially doubted the longevity of the adopted policy.

[12] The policy adoption date and enactment date varied for some states by a few months up to a year. I focus on the enactment date because this date marks when youth first became eligible for in-state tuition. I also ran the analysis using the adoption date as a sensitivity check and found similar results.

[13] Because I use CPS data up to 2011, Maryland and Connecticut are treated as control states since their policies were not enacted until 2011 and the lagged effect is not observed until 2012.

[14] Household position of youth serves as a proxy for family structure, which is not readily available in the MORG file.

[15] Highest household education level is the highest degree obtained by any household member age 15 and older. The measure serves as a proxy for parental education. I also ran results using an indicator of the highest education level of the head of household. Results were robust.

(e.g., nationwide decrease in dropout rate). B_6 indicates month fixed effects and controls for monthly variation in the likelihood of dropping out of high school (e.g., the lower likelihood of dropping out during the summer months). B_7 represents the coefficients from the remaining unobserved state-specific linear trends that influence the likelihood of dropping out. Lastly, ε_i represents individual random error. All data are weighted and robust standard errors are clustered by state-year[16] to correct for heteroskedasticity.

Results

Descriptive Analysis

To examine the educational effects of IRT policies, I first estimate T-tests to evaluate mean differences in the likelihood of dropping out, individual characteristics, and state conditions across the pre and post (t-l) policy period for both policy states and non-policy states. For this analysis, I calculate the average of each variable by state-year and then estimate mean differences in these variables for the pre and post years. For the non-policy states, I use the median policy enactment date, May 2003, to indicate the pre-post division. By May 2003, seven of the eleven policy states assessed had enacted their IRT policy.

The results indicate that the high school dropout rate for Mexican FBNCs in policy states decreased by 12 percentage points between the pre-post years, while the dropout rate in non-policy states decreased by 8 percentage points between the pre-post years, though the latter was only marginally significant (Table 1). These results fit with national trends that indicate dropout rates of the foreign-born population were declining during this time period (Fry 2007). The steeper decline in policy states, however, supports the hypothesis that the enactment of IRT policies reduces the likelihood of dropping out of high school.

Moreover, the similar demographic and economic changes observed between policy and non-policy states provide further support for their comparability. For both policy and non-policy states, Mexican FBNC youth in the post years, compared to those in the pre-years, had lived in the US for more years (policy states: 7.5 vs. 4.8; non-policy states: 7.1 vs. 4.1) and were less likely to be employed (policy states: 46% vs. 34%; non-policy states: 59% vs. 42%), to live with a relative (policy states: 20% vs. 17%; non-policy states: 20% vs. 16%), and to live in a household where no one had a high school degree (policy state: 48% vs. 40%; non-policy state: 52% vs. 43%). In terms of economic conditions, both policy and non-policy states experienced higher rates of unemployment in the post years compared to the pre-years (policy state: 6.50 vs. 5.07; non-policy state: 6.66 vs. 4.37).

Despite these similar demographic and economic changes, other potentially confounding state conditions also changed during this time period and could contribute to the observed policy effect. Between the pre-post years, only policy states experienced a decline in their overall educational attainment, as evidenced by the decrease in proportion of white adults with a high school diploma. At the same time, policy states experienced an increase in the proportion of Mexican adults with a high school diploma, a proxy for Mexican educational

[16]Because individuals can appear in the data twice, I ran two additional checks: 1) clustered the standard errors by individuals; and 2) dropped repeat observations. Results were robust. I cluster by state-year to be consistent with prior research on IRT policies and because that is the level of the policy.

aspirations, and an increase in the size of the Mexican FBNC population, a proxy for growth in the undocumented population. No similar change occurred for non-policy states. A simple mean DD calculation does not control for these uneven variations in educational attainment, educational aspirations, and migration trends.

Multivariate Analysis

I use a regression framework to control for these uneven variations and to identify an unbiased policy effect. A baseline model including a policy effect dummy variable and state and year fixed effects estimates the total unadjusted difference in the likelihood of dropping out of high school between Mexican FBNC youth in policy states, post policy and youth in non-policy states and policy states, pre-policy. I then subsequently add time-varying individual characteristics, time-varying state conditions, and state-specific linear trends to the regressions and evaluate how differences in each of these constructs contribute to the observed policy effect.

The baseline model indicates that the enactment of IRT policies is not associated with the likelihood of dropping out for Mexican FBNCs (Table 2, Model 1). While the coefficient is negative, it is not significant. This result does not change once I control for time-varying individual characteristics (Model 2) and state conditions (Model 3). The IRT policy coefficient remains negative but non-significant.

These models, however, do not control for unobserved state-specific time trends that may be biasing the results towards zero. Though the time varying state characteristics included in Model 3 control for some state specific time trends, other state trends remain unobserved. For example, while the variable on proportion of Mexican FBNCs serves as a proxy control for state growth in the undocumented population, this variable cannot distinguish between growth in the legal immigrant and undocumented populations. By interacting state fixed effects and year, I can control for linear growth in the undocumented population and other potential pre-policy state-specific linear trends.

When I include state-specific linear time trends (Model 4), I find that IRT policies are effective at reducing dropout behavior. The enactment of an IRT policy is associated with an eight percentage point reduction in the likelihood of dropping out of high school for Mexican FBNCs. Thus, the previous models that did not control for state-specific trends were downwardly biased (i.e. the reduction in the likelihood of dropout was too low).

Using the well-known omitted variable bias formula this result suggests that the policy effect was biased downward (net effect) due to the omission of state-specific trends, which were positively correlated with policy states but negatively correlated with a reduction in the likelihood of high school dropout.[17] For example, if the share of the Mexican FBNC population who were undocumented (compared to legal immigrants) grew more in policy states than non-policy states (i.e. the positive correlation), the omission of this growth would downwardly bias the policy effect since high school dropout rates are higher among the

[17]Another interpretation is that the policy effect was biased downward (net effect) due to the omission of state-specific linear trends, which were negatively correlated with policy states but positively correlated with a reduction in the high school dropout rate.

undocumented population than the legal immigrant population (i.e. the negative correlation; Passel 2005). Of course, there are a multitude of state trends that could account for this downward bias and the reason for the downward bias could differ for each state. The result only indicates that the net effect of excluding state-specific linear trends is a downward bias.

Falsification Tests and Moderating Effects

To further assess the robustness of this policy effect, I run the final model for other racial/ethnic groups. This comparison further reduces the threat that IRT policies are endogenous (Bertrand, Duflo and Mullainathan 2004; Shadish, Cook and Campbell 2002) by assessing whether the policy effect exists for non-targeted groups (i.e. the third difference), specifically: US-born non-Latino whites and US-born non-Latino blacks. If my hypothesis is correct and no other policy or contextual difference is driving my result, the estimate for other racial/ethnic groups should be small to non-existent. I also examine the effect the policy has on other Latino groups—non-Mexican Latino citizens and Mexican-American citizens—to assess whether they indirectly benefit from the policy.

As hypothesized, US-born non-Latino white and US-born non-Latino black youth are not affected by the policy as seen by the near zero and non-significant coefficients (Table 3). For the Latino populations, there is no indication that non-Mexican Latino citizens or Mexican-American citizens indirectly benefit from the policy. The coefficients are small and non-significant. In combination, these results provide strong evidence that the enactment of IRT policies reduce the likelihood of dropping out for youth most likely to be undocumented, Mexican FBNCs.

Because the time series dimension of the data introduces serial correlation that may bias the standard errors and lead to an over-rejection of the null hypothesis (Bertrand, Duflo, and Mullainathan 2004), I also run the final model using the Driscoll-Kraay standard errors correction. I use the user written XTSCC (Regression with Driscoll-Kraay standard errors) program in STATA (Hoechle 2007), which corrects for correlations of the residuals both between state-years and within state-years.[18] With this correction, my policy effect becomes marginally significant with a p-value of .103 (Table 4, Model 4, Part B). This result indicates that serial correlation had biased the standard errors and led to an over-estimate of statistical significance. The statistical significance tests in both the unadjusted and adjusted models, however, are likely to underestimate the true effect given the measurement error problem noted. The relatively large size of the policy effect and the marginal significance associated with the Driscoll-Kraay standard errors, despite this measurement error problem, provide strong evidence that IRT policies affect the likelihood of dropout for undocumented youth (Nickerson 2000).

As an additional robustness check, I address the concern that states may have adopted other state level education or immigration policies at the same time they adopted the IRT policy. Given the negative pre-existing trends in dropout behavior associated with policy states (as evidenced by the downward bias when state-linear trends were excluded in Table 2), these

[18]Because the Driscoll-Kraay correction is based on state-year level observations, these models lose the month variation available at the individual-level and lead to slightly different coefficient estimates.

states may have adopted a variety of other educational policies in addition to the IRT policy to reduce Mexican FBNCs dropout behavior. The previous results for Mexican American citizens and other Latinos partially address this issue. Given that these groups were not affected by the IRT policy, if other educational or immigration policies were confounding the results these policies would have to be narrowly targeting foreign-born, non-citizens—not just Latinos or immigrants in general.

Nevertheless, to further address the concern of a spurious policy effect, I run a falsification test arbitrarily setting the time of when a state became a policy state (i.e., t−2, t, t+1, and t +2). [19] I run the models without the Driscoll-Kraay standard errors, since I am more likely to find a policy effect without this correction. I also provide the same models with Driscoll-Kraay standard errors. If I find that the observed policy change occurred much earlier or later than when the policy was enacted (with a one-year lag), the results could suggest a spurious policy effect.

The only policy effect I detect (other than my original policy effect) is for t+1, one year before the policy was adopted (Table 4). As shown in Model 2, Part A, I find an 8 percentage point reduction in the dropout rate for Mexican FBNCs during this time period. While this policy effect becomes non-significant (with a p-value=.217) once I include Driscoll-Kraay standard errors (Model 2, Part B), the result still raises some concerns of a spurious policy effect.

While it is plausible that the t+1 result suggests an alternative policy effect, it is also plausible and highly likely that this result reflects the complexities of the politics surrounding the adoption of IRT policies and the mixed policy signals sent to undocumented youth. Research suggests that the policy window surrounding the adoption and enactment of IRT policies was not clear and concise in every state and may reflect a broader time period than the one-year lag I estimate. For some states the adoption of the IRT policy occurred with little fanfare; for others, however, the process was highly contentious and drawn out (Kobach 2007; Reich and Barth 2010; Reich and Mendoza 2008; Russell 2007). In California, for instance, an IRT policy was first passed by the legislature in 2000 but vetoed by the governor. The bill was subsequently adopted in 2001. A similar process occurred in Nebraska, Maryland and Connecticut. Adding to the mixed policy signal, some colleges and major university systems (particularly in New York, Texas and California) provided in-state resident tuition to undocumented students before their respective state adopted an IRT policy (Flores 2007; Kobach 2007). Moreover, in New York while the policy was first adopted in the summer of 2002 it was not enacted until the following summer.

Thus, the t+1 policy effect I detect may be picking up some of the mixed policy signals undocumented youth were receiving prior to the actual state-level adoption and enactment of the IRT policy. These youth may have either believed they were eligible for in-state resident

[19] I also collected data from the US Department of Education to assess whether policy states compared to non-policy states allocated more resources to immigrant youth. While there is limited consistent data available across all the years, I calculated the per pupil expenditure of Title III funding for limited English proficient youth for the years 2004, 2005, and 2007 (OELA 2008OELA 2012). Per pupil expenditures were actually higher in non-policy states than policy states ($M_{Non\text{-}policy}$ M=163.32, $SD_{Non\text{-}policy}$=86.23; M_{Policy} M=129.836, SD_{Policy}=54.51; p<.05). These results suggest that an alternative state-level education policy targeting immigrant youth is not likely driving my results.

tuition (and actually were within some university systems) or were hopeful that by time they became of college age the policy would be adopted. The results in Table 4 provide support for this interpretation. Given that the process of policy adoption and enactment occurred over several years, it is not surprising that we begin to see a policy effect the year leading up to the actual enactment (Model 2), the year of enactment (Model 3; while not significant the coefficient suggests a negative effect), and the one-year post-enactment effect (Model 4). The policy effect that is strongest, however, is the one-year post-enactment period (as evidenced by the remaining marginal significance with Driscoll-Kraay standard errors). Thus, while the process of adopting an IRT policy and the availability of in-state tuition within some university systems may begin to encourage undocumented youth to stay in school, the actual enactment of the policy has the largest effect on these youth's school investment decision.

The threat of a spurious policy effect, however, remains a limitation of this study. It is still plausible that policy states adopted a variety of other educational policies in addition to the IRT policy to address the dropout problem among Mexican FBNCs. If this occurred, my IRT policy effect may be confounded by these other policies.

Additional Falsification Tests—I ran several other analyses to further assess the robustness of the results and to identify moderating effects by state migration history and gender (full results available upon request). First, I ran the final model on the Mexican FBNC sample using only the states that had considered an IRT policy (but failed to pass) as the control group, since these states may be more similar to IRT policy states than states that adopted an IRT ban or did not consider IRT legislation.[20] The results were robust ($b=-0.08$; s.e.=0.03; $p<.05$; n=5,237) providing further evidence of a policy effect. Second, I assessed the validity of the Mexican FBNC proxy by running the analysis using only individuals who had been in the US less than ten years, since they are more likely to be undocumented. The effect was slightly stronger ($b=-0.09$; s.e.=0.03; n=4,555; $p<0.05$). This increase in the policy effect demonstrates that the reliance on the Mexican FBNC proxy to identify undocumented youth is likely to underestimate the true policy effect.

Another concern is that instead of reducing the likelihood of high school dropout, IRT policies may simply encourage highly motivated immigrants seeking educational opportunities to settle in their state. Thus, I assessed the possibility of a policy feedback effect where IRT policies are attracting immigrants who are already more likely to graduate from high school. I ran the analysis using a sub-sample of Mexican FBNCs who migrated to the US before 2001 (the year the first policy was adopted), since these individuals would already have been settled in a state. The results were robust ($b=-0.08$; s.e.=0.03; n=5,728) and significant ($p<0.05$). With CPS data, I cannot assess the threat of interstate mobility, but recent research on welfare benefits indicates that immigrant populations are not responsive to state subsidies (Kaushal 2005).

[20]Because McLendon, Mokher, and Flores (2011) found that female legislative representation and the size of the foreign-born population were associated with the IRT legislative agenda setting process, I controlled for these factors as an additional robustness check. Data on female legislatures came from the Center for American Women and Politics (2012). Results were robust.

Moderating Effects—Given that not all policy states have the same infrastructure and immigrant support systems to help Mexican FBNCs take advantage of this policy, I followed Flores and Chapa's (2009) work and assessed whether the effect of IRT policies differed by state migration history. Using Flores and Chapa's definition, I defined states as traditional immigrant states and new immigrant states (including a sub-sample of new non-southern states) and ran the final model for a sub-sample of each state classification.[21] Similar to Flores and Chapa, I found that the policy effect was only significant in traditional settlement states, which presumably have stronger immigrant support systems to help immigrant youth succeed academically. The policy effect coefficient was still negative but non-significant and smaller in the new-settlement states. It is important to note, however, that the sample size for these states was significantly smaller, thus reducing the likelihood of finding an effect.

Overall, these results suggest that IRT policies are necessary but may not be sufficient to reduce the likelihood of high school dropout among undocumented immigrant youth. To be fully effective, IRT policies may need to be accompanied by the development of infrastructure and support systems that help undocumented immigrant youth succeed in high school, so they can benefit from the IRT policy in the future. Lastly, I did not find that the policy effect varied by gender. By interacting the policy variable with gender, I found that both males and females were equally affected by the policy as indicated by the non-significant interaction term.

Conclusion

In response to the federal government's ban on providing in-state resident tuition to undocumented immigrants, several states have adopted (13 as of December 2011) or have considered adopting their own in-state resident tuition (IRT) policy that extends in-state tuition benefits to their undocumented immigrant youth population. Though there is significant heated political discussion surrounding IRT policies for undocumented immigrants (Drachman 2006; Gonzales 2009; Rhymer 2004), little is actually known about the educational implications of such policies, particularly in K-12. This study examines a key argument proposed by policy advocates: IRT policies will motivate undocumented immigrant youth to complete high school because college is now more attainable (Fuligni and Perreira 2009; Gonzales 2009; Murray, Batalova and Fix, 2007; NILC 2005; Reich and Barth 2010; Russell 2007). This paper uses data from the Current Population Survey (CPS) and difference-in-difference models to assess the extent to which IRT policies reduce the likelihood of high school dropout for youth most likely to be undocumented, Mexican FBNCs.

I found that Mexican FBNCs are at high risk of dropping out of high school. The pre-policy enactment dropout rate for my sample of Mexican FBNCs was 42% in policy states and 50% in non-policy states. These estimates align with prior research that indicates approximately 40% of Mexican foreign-born youth (citizen and non-citizen) aged 16–19

[21]See Flores and Chapa's article for the state classifications. I classified Wisconsin, which had not adopted the policy during the time frame of their study, as a new non-southern state.

dropout of high school (Fry 2003). By focusing on only non-citizen foreign-born Mexican youth, my analysis highlights that the dropout problem is even higher for youth most likely to be undocumented.

The enactment of IRT policies may be a partial solution to the high dropout rate of Mexican FBNCs. My difference-in-difference calculations indicated that the enactment of IRT policies reduced the likelihood of dropping out of high school for Mexican FBNCs by eight percentage points. For states that have enacted the policy this reduces the average dropout rate from 42% to 34%--a near 19% reduction in the overall dropout rate for Mexican FBNCs. Further increasing the robustness of this finding, I found no policy effect for other ethnic/racial groups, including US-born non-Latino whites and US-born non-Latino blacks.

I did not find evidence that IRT policies have a positive spillover effect on the educational attainment of other Latino groups, particularly Mexican-Americans. Given the heated rhetoric that surrounds the issue of undocumented immigration and the conflation of Latino (particularly Mexican) with undocumented immigrant (Timberlake and Williams 2012), I had hypothesized that Latino youth in general may view the adoption of an IRT policy as a welcoming signal for all Latinos. However, when I ran the analysis on US citizens of Mexican or other Latino descent (e.g. Cubans, South Americans, and Central Americans), I found no policy effect. Because states that have adopted IRT policies have also adopted (or considered) policies that severely constrict immigrant rights (e.g. English language only laws; require K-12 schools to verify students' citizenship status; Laglagaron et al. 2008; NILC 2011), the adoption of an IRT policy may not serve as a sufficient welcoming signal to counteract the negative effects of more restrictive policies.

While this study employs of the strongest quasi-experimental research designs available, the difference-in-difference (i.e. comparison to non-policy states) along with a comparison to other racial/ethnic groups, and while results remained robust to several falsification checks (i.e. assessments of the treatment-control group match, the Mexican FBNC proxy, the policy feedback threat, and timing of policy adoption) study limitations remain. The main limitation is that I am not able to completely rule out a spurious policy effect. The state linear trends indicate that policy states were experiencing a relative rise in their dropout rate among Mexican FBNCs prior to the adoption of the policy. If these states adopted other policies in addition to the IRT policy to address this problem, my policy effect would be biased and would confound the IRT policy effect with the effect of these other policies. For this to occur, however, each of the different policy states would have to have enacted similar policy measures and these measures would have to narrowly target foreign-born, non-citizens and not immigrants or Latinos in general. Moreover, research on policy adoption as well as the falsification checks included in this study limit the plausibility of this threat (McLendon, Mokher, and Flores 2011; Reich and Barth 2010; Reich and Mendoza 2008).

The second main limitation of this study is measurement error driven by the reliance on Mexican FBNCs as a proxy for undocumented youth and within-state variation treatment effects. Because the sample includes students who are not actually in the treatment group the policy effect estimates are likely to be attenuated. Similarly, because I cannot control for within-state variation that stems from individual institutions of higher education adopting

their own IRT policy this measurement error further attenuates the policy effect. While the majority of higher education institutions follow their state's policy, some colleges in non-policy states allow undocumented immigrants to pay in-state tuition and some colleges in policy states prohibit undocumented immigrants from paying in-state tuition. This attenuation bias makes it difficult to interpret the marginal significance associated with the policy effect that I find once I adjust for Driscoll-Kraay standard errors. One interpretation is that the IRT law has no significant effect on the dropout behavior of Mexican FBNCs. However, the large size of the coefficient coupled with the marginal significance (despite the attenuation bias) suggests that IRT policies do have an effect. Thus, another interpretation is that the statistical tests I employ lack sufficient power to detect a non-marginally significant effect due to measurement error bias.

Despite these limitations, this study provides an essential evaluation of how IRT policies influence the schooling decisions of high school aged immigrant youth likely to be undocumented. Informing current state- and federal-level policy debates on facilitating college access for undocumented immigrants, this study provides strong evidence that state educational policies shape the academic adaptation of undocumented immigrant youth. Moreover, the results strongly support the segmented assimilation model by demonstrating that the social contexts of reception influence the adaptation of children of immigrants (Portes and Rumbaut 2006). States that exclude undocumented immigrants from receiving in-state tuition add to both the financial and discrimination constraints undocumented immigrant youth face and increase their risk for dropping out of high school. In contrast, states with in-state tuition options for undocumented immigrants increase school investment by reducing future educational costs and potentially reducing the psychological costs associated with social marginalization.

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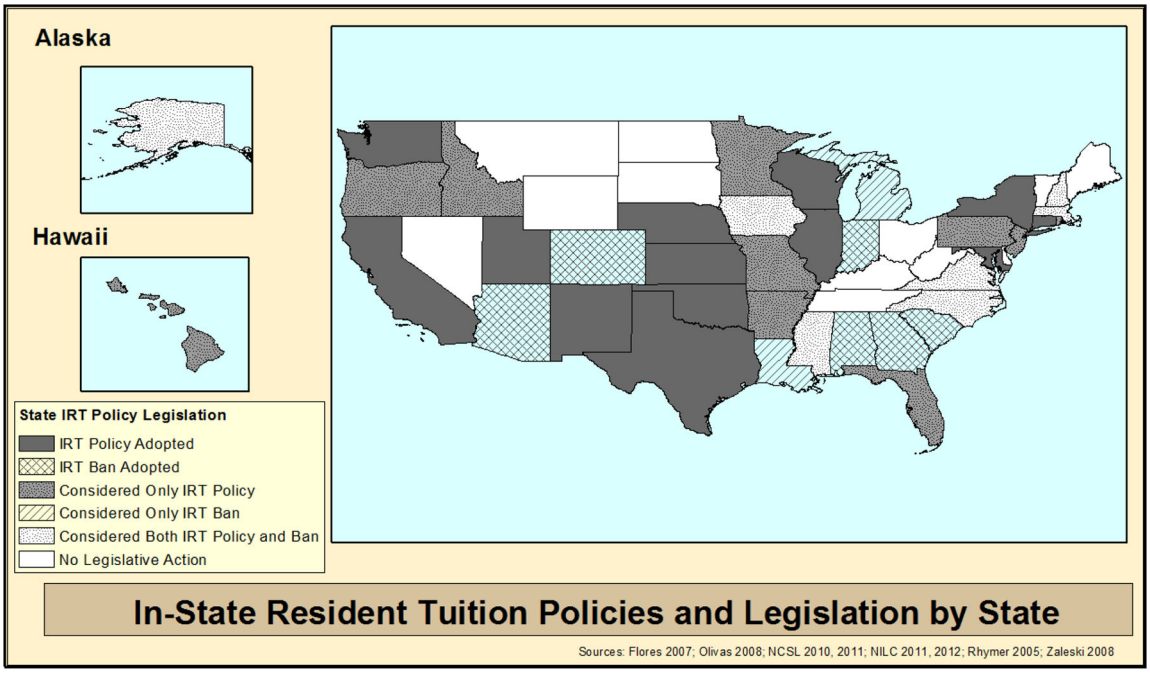

Figure 1.

Table 1

Summary Statistics for Mexican FBNC Sample Aggregated at State Level for Ages 16–19, Years 1998–2011 (Data Weighted)

	Policy States		Non-Policy States[2]	
	Pre-Policy[1]	Post-Policy	Pre-Policy	Post-Policy
	Mean SD	Mean SD	Mean SD	Mean SD
Individual Characteristics				
HS dropout	0.42 (.25)	0.30 (.17) **	0.50 (.34)	0.42 (.29) †
Age	17.75 (.57)	17.60 (.35)	17.86 (.72)	17.69 (.64) *
Female	0.40 (.23)	0.48 (.19) †	0.39 (.33)	0.42 (.29)
Avg. yrs in US	5.02 (2.08)	8.34 (2.11) ***	4.09 (2.81)	7.08 (3.07) ***
Employed	0.46 (.23)	0.34 (.17) **	0.59 (.35)	0.42 (.30) ***
Highest household education				
Less than H.S.	0.48 (.27)	0.40 (.17) †	0.52 (.36)	0.43 (.31) *
H.S. degree	0.35 (.27)	0.37 (.16)	0.28 (.35)	0.40 (.28) **
Some college	0.12 (.15)	0.17 (.13) †	0.13 (.23)	0.11 (.17)
College degree (reference)	0.05 (.08)	0.07 (.08)	0.06 (.18)	0.06 (.15)
Household position of youth				
Non-family member	0.11 (.13)	0.07 (.08) *	0.20 (.29)	0.16 (.23)
Head of household	0.18 (.27)	0.12 (.10)	0.16 (.25)	0.15 (.20)
Spouse	0.06 (.10)	0.04 (.08)	0.05 (.15)	0.05 (.15)
Relative	0.20 (.18)	0.17 (.16) **	0.20 (.27)	0.16 (.20) *
Child (reference)	0.46 (.27)	0.60 (.19)	0.38 (.37)	0.48 (.32)
MSA	0.80 (.31)	0.90 (.18) †	0.79 (.34)	0.79 (.26)
State Characteristics				
State unemp. rate	5.07 (1.08)	6.50 (2.17) ***	4.37 (1.13)	6.66 (2.46) ***
Prop. white adults w/some	0.60 (.04)	0.59 (.05)	0.58 (.06)	0.58 (.07)
Prop. white adults w/hs	0.89 (.05)	0.87 (.06) *	0.90 (.04)	0.90 (.04)
Prop. Mexican adults w/hs	0.47 (.14)	0.54 (.07) ***	0.46 (.29)	0.50 (.22)
Prop. Mexican FBNC	0.04 (.03)	0.06 (.03) *	0.03 (.02)	0.03 (.02)
State level N=	75	75	116	260

[1] Post policy is lagged by 12 months meaning the policy was enacted 12 months previous.

[2] Median policy enactment date (May 2003) indicates pre-post division for non-policy states

† p<.10,

* p<.05,

** p<.01,

*** p<.001 indicate significance level for mean comparisons between pre and post using T-tests.

Table 2

Impact of In-State Resident Tuition Policies on Mexican FBNC Youth's Likelihood of Dropping out of High School for Ages 16–19, Years 1998–2011 (Data Weighted)

	Model 1 Baseline Model	Model 2 Individual Characteristics	Model 3 State Conditions	Model 4 State-Specific Linear Trends
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Policy effect	−0.01 (.03)	−0.01 (.02)	−0.02 (.02)	−0.08 (.03) **
Individual Characteristics				
Age		0.04 (.01) ***	0.04 (.01) ***	0.04 (.01) ***
Female		−0.03 (.01) **	−0.04 (.01) **	−0.04 (.01) **
Avg. yrs in US		−0.01 (.00) ***	−0.01 (.00) ***	−0.01 (.00) ***
Employed		0.17 (.01) ***	0.16 (.01) ***	0.16 (.01) ***
Highest household education				
Less than H.S. vs. college degree		0.27 (.02) ***	0.26 (.02) ***	0.26 (.02) ***
H.S. degree vs. college degree		0.00 (.02)	−0.01 (.02)	−0.01 (.02)
Some college vs. college degree		−0.02 (.03)	−0.03 (.03)	−0.03 (.03)
Household position of youth				
Non-family vs. child		0.22 (.02) ***	0.22 (.02) ***	0.21 (.02) ***
Head of household vs. child		0.25 (.02) ***	0.26 (.02) ***	0.25 (.02) ***
Spouse vs. child		0.37 (.03) ***	0.37 (.03) ***	0.37 (.03) ***
Relative vs. child		0.22 (.02) ***	0.21 (.02) ***	0.21 (.02) ***
MSA		0.02 (.02)	0.01 (.02)	0.01 (.02)
State Characteristics				
State unemp. rate			−0.01 (.01) †	−0.01 (.01)
% white adults w/some college			0.05 (.20)	0.10 (.21)
% white adults w/hs diploma			0.13 (.32)	0.22 (.32)
% Mexican adults w/hs diploma			−0.05 (.04)	−0.06 (.04) †
% Mexican FBNC			0.23 (.43)	0.13 (.42)
Month FE	No	No	Yes	Yes
State & Year FE	Yes	Yes	Yes	Yes

N=6603

† p<.10,

* p<.05,

** p<.01,

*** p<.001

Notes: Standard errors are adjusted for clustering by state-year.

Table 3

Impact of In-State Resident Tuition Policies on Youth's Likelihood of Dropping out of High School Ages 16–19 by Racial/Ethnic Group, Years 1998–2011 (Data Weighted)

	U.S. Born Non-Latino White	U.S. Born Non-Latino Black	Non-Mexican Latino Citizen	Mexican American Citizen
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Policy effect	−0.01 (.01)	0.01 (.01)	0.01 (.02)	−0.02 (.01)
Individual Characteristics				
Age	−0.02 (.00) ***	0.01 (.00) ***	0.01 (.00) *	0.02 (.00) ***
Female	−0.01 (.00) ***	−0.03 (.00) ***	−0.03 (.01) ***	−0.03 (.01) ***
Employed	0.02 (.00) ***	0.02 (.01) ***	0.02 (.01) *	0.04 (.01) ***
Highest household education				
Less than H.S. vs. college degree	0.35 (.01) ***	0.24 (.01) ***	0.23 (.01) ***	0.19 (.01) ***
H.S. degree vs. college degree	0.06 (.00) ***	0.06 (.01) ***	0.05 (.01) ***	0.06 (.01) ***
Some college vs. college degree	0.02 (.00) ***	0.02 (.00) **	0.01 (.01) †	0.01 (.01)
Household position of youth				
Non-family vs. child	0.03 (.00) ***	0.01 (.01)	0.05 (.02) **	0.09 (.01) ***
Head of household vs. child	0.10 (.01) ***	0.07 (.01) ***	0.13 (.02) ***	0.15 (.01) ***
Spouse vs. child	0.12 (.01) ***	0.06 (.04)	0.25 (.06) ***	0.31 (.04) ***
Relative vs. child	0.03 (.00) ***	0.01 (.01)	0.02 (.02)	0.06 (.01) ***
MSA	0.00 (.00)	0.01 (.01)	−0.01 (.02)	0.01 (.01)
State Characteristics				
State unemp. rate	0.00 (.00)	0.00 (.00) †	0.00 (.01)	0.00 (.00)
% white adults w/some college	0.02 (.03)	−0.07 (.06)	−0.02 (.11)	−0.11 (.12)
% white adults w/hs diploma	−0.01 (.05)	0.09 (.09)	−0.05 (.19)	0.08 (.18)
% Mexican adults w/hs diploma	0.00 (.00)	0.00 (.01)	−0.01 (.01)	−0.03 (.03)
% Mexican FBNC	−0.12 (.10)	−0.29 (.22)	−0.05 (.33)	−0.02 (.29)
Month FE	Yes	Yes	Yes	Yes
State & Year FE	Yes	Yes	Yes	Yes
State linear trends	Yes	Yes	Yes	Yes
N=	202,435	35,167	12,557	22,205

† p<.10,

* p<.05,

** p<.01,

*** p<.001

Notes: Standard errors are adjusted for clustering by state-year.

Table 4

Falsification Check on Timing of Policy Enactment for the Full Model, Mexican FBNC Sample (Model 4 from Table 2)

	Model 1		Model 2		Model 3		Model 4		Model 5	
	2 Years Pre-Enactment (t+2)		1 Year Pre-Enactment (t+1)		Policy Enactment Year (t)		1 Year Post-Enactment (t−1)		2 Years Post-Enactment (t−2)	
	b (s.e.)	P-value	b (s.e.)	P-value	b (s.e.)	P-value	b (s.e.)	P-value	b (s.e.)	P-value
A. Full Model										
Policy effect	−0.01 (.04)	0.381	−0.08 (.03)	0.015	−0.04 (.03)	0.138	−0.08 (.03)	0.003	0.02 (.03)	0.496
B. Full Model with Driscoll-Kraay Standard Errors										
Policy effect	0.00 (.06)	0.976	−0.09 (.07)	0.217	−0.05 (.09)	0.561	−0.08 (.05)	0.103	0.05 (.06)	0.356

Notes: (1) The models in part A are based on individual-level observations (N=6603) and standard errors are adjusted for clustering by state-year. (2) The models in part B are based on state-year level observations (N=526) and include Driscoll-Kraay standard errors, which correct for serial correlation in addition to state-year clustering. (3) Because the Driscoll-Kraay correction is based on state-year level observations, the models in part B lose the month variation available at the individual-level and lead to slightly different coefficient estimates than that in part A.