Closing the Immigrant-Native Education Gap: The Effect of Tuition Equity Reform in Texas

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Abstract

This paper estimates the impact of tuition equity reform on the educational outcomes of undocumented immigrant high school students. This type of reform, which grants in-state tuition to qualifying immigrant students, can be interpreted as a partial relaxation of the institutional constraints associated with lack of legal immigration status. Exploiting administrative data from education agencies in Texas, we employ a generalized differences-in-differences framework to produce within-school, across-cohort estimates of the impact of the 'Texas Dream Act' on a range of educational outcomes ranging from college demand to college-bound investments during high school. Estimates show a significant closing of the college demand gap between immigrant and control group high school graduates. However, estimates regarding college-bound investments contain mixed results. We attribute this to a complex policy environment in public high schools during the analysis period. Results suggest that affordable college access policies can have a significant impact on the attainment of the immigrant population at the college entrance stage, but that, given other policies in place, college tuition incentives down the educational ladder may not be sufficiently salient to generate spillover effects.

JEL codes: I22, I23, I28, I32, J15, J18, J24, K37, O15, F22

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1 Introduction

Immigration reform is one of the centerpieces of an increasingly polarized political debate in the United States. For the last two decades, part of the debate has focused on the adjustment of status of unauthorized immigrants that have resided in the country from a young age and for extended periods of time. Members of this group are typically from an impoverished background and perform poorly academically. Moreover, U.S. federal law mandates that public schools provide K-12 education for this population. Given that the state makes large investments in their public education, it is relevant to inquire whether observed academic gaps between undocumented immigrants and natives are due to differences in cognitive development or to the institutional constraints associated with lack of immigration status.[1] This paper attempts to answer this question by evaluating the effect of tuition equity reform on the educational outcomes of undocumented high school students in Texas.

As of 2015, eighteen states in the U.S. have enacted 'tuition equity laws' granting resident tuition rates (and in some cases, including that of Texas, state financial aid) to qualifying undocumented students.[2] By approving House Bill 1403, Texas became the first state to approve this type of law in July of 2001. The initiative came to be known as the 'Texas Dream Act' (henceforth, TDA). The reform generated a large reduction in the cost of college attendance for undocumented students, moving them from out-of-state to in-state status in terms of tuition and fees. However, theoretically, it is no clear whether such a reform would have positive effects on the educational attainment of undocumented immigrants. On the one hand, this group is forbidden from participating in the formal labor market. For this and other reasons, the expected return to higher education for this group is difficult to assess, but it is likely safe to assume that it is considerably lower than that of natives.[3] On the other hand, college education may have consumption value in and of itself, not just due to the intrinsic value of education but also because academic institutions have been identified as a partial safe haven from the duress of lack of immigration status once adulthood has been reached (Gonzales, 2010).

One of the main challenges in studying the outcomes of undocumented immigrants is measurement. Indeed, the very notion of undocumented status suggests that this population is not easy to identify in data sources, administrative or survey-based, as these individuals may not

[1] Undocumented immigrants are not eligible for most government services, cannot legally work in the country, and are subject for deportation.

[2] These include: Texas, California, Minnesota, New Mexico, Illinois, New York, Michigan, Washington, Oregon, New Jersey, Maryland, Rhode Island, Connecticut, Kansas, Colorado, Utah, Nebraska, and Oklahoma.

[3] The political climate is much more relevant for the formation of expectations for this segment of the population, given that adjustment of immigration status and/or deportation has become central to a newly radicalized political debate on immigration policy . Hence, policy volatility that is binding with regard to expected labor market returns.

appear in formal records or may have incentives to deny information requests during surveys. We keep this measurement problem in mind when constructing a proxy indicator of undocumented immigrant status based on administrative high school records. We also develop an approach to validate this proxy indicator. We do so by exploiting the fact that TDA beneficiaries were obliged to sign affidavits promising to attain legal immigrant status as soon as possible. By relating proxy indicator rates at high schools to affidavit rates at nearby colleges, We can assess whether the proxy is capturing the correct population.

After finding the treated, We estimate the effects of the reform using administrative data from Texas high schools in a generalized differences-in-differences framework. Specifically, We exploit within-school, between-cohort variation to draw comparisons between the educational outcomes of undocumented high school students across cohorts, using the outcomes of their native classmates as a fine-grained control. We present evidence that although control group students are substantially different to the treated in levels, their similarity in trends validates our research design. Preferred estimates of the impact of the reform on college demand are regression-adjusted differences in college planning rates between immigrant and non-immigrant Hispanic students attending the same high school, across the pre and post reform period.

The analysis points to the following conclusions. First, undocumented students are a severely disadvantaged group. They rank lower than non-immigrant Hispanics (a group already considered disadvantaged) in many educational outcomes such as high school graduation rates, standardized exam scores, and college demand. Second, the reform led to a 5 to 6 percentage point increase in college demand among undocumented high school graduates, closing the gap with the comparison group by 90%. We present further evidence suggesting that the law led to an increase in college enrollment and completion in the treated. However, constraints in the administrative data limit our ability to directly estimate these effects. Third, we find mixed evidence on whether the reform led to increases in college-bound investments during high school. While there are some relative improvements in standardized test scores, fundamental outcomes such as graduation and dropout suffered during this time period.

This paper is related to an emergent literature on the effect of tuition equity policy on the outcomes of immigrant students (Kaushal, 2008; Flores, 2010; Chin et al., 2010; Conger et al., 2015). Several of these studies employ U.S. census data to draw between-state comparisons between immigrant and native college enrollment rates. While these identification strategies are appealing based on their representative nature, the mixed conclusions drawn from these studies and the coarse nature of the immigrant indicator found in census data, have been a shortcoming of this class of methods. To our knowledge, we are providing the first analysis of the tuition equity reform movement based on high-quality administrative panel data from

state agencies. The ability to observe a range of demographic variables as well as longitudinal variation in immigrant indicators, allows us to construct a reliable categorization of this elusive population. Furthermore, the richness of the data allows to study effects on outcomes related to college-bound investment during high school, the first in the literature to our knowledge.

Most relevant for our study, Conger and Turner (2015) exploit administrative data from the City University of New York to estimate the effects of a temporary increase in tuition for undocumented students. Their identification strategy is straightforward and quite credible as they can observe undocumented status directly from college records, thereby avoiding altogether the measurement issues that have plagued the literature. Their estimates show large effects of tuition shocks on the re-enrollment and degree attainment of undocumented college students. We see the results of our analysis as complimentary to them. Their estimates are valid for a special sample of undocumented students, namely those that are already attending college. Our analysis can be interpreted as testing for effect at other stages of development. We test for effects both at the key juncture of high school and college, and the high school years, when investments are made toward post-secondary education. Thus we test whether tuition-equity creates positive externalities (i.e. effects beyond the direct price effects of tuition changes on college enrollment) by generating improvements in outcomes among undocumented youth during high school years.

The rest of the paper is organized as follows. Section two describes the historical and legislative background to the advent of TDA, as well as theoretical considerations regarding its potential effects. Section three presents our empirical strategy, including data sources, measurement, and causal inference. Section four presents our estimates and interprets the results. Finally, section five concludes with policy implications and motivation for future research on this topic.

2 Background and Theory

In the groundbreaking *Plyler v. Doe* decision in 1982, the U.S. Supreme Court struck down a Texas law which withheld state funds for educating children who had not been legally admitted to the United States, and authorized local school districts to deny enrollment to such students.[4] The decision effectively obliged all school districts in the nation to provide K-12 education to undocumented immigrants. In the case's majority opinion, Justice Brennan observed that denying the children in question a proper education would likely contribute to "the creation and perpetuation of a subclass of illiterates within our boundaries, surely adding to the problems

[4] 457 U.S. 202

and costs of unemployment, welfare, and crime." In 1996, however, the United States Congress enacted the *Illegal Immigration Reform and Immigrant Responsibility Act*, which allowed states to pass statutes denying undocumented students from in-state tuition, financial aid, or even bar their enrollment in public colleges and universities all together.[5] Hence, federal law guaranteed a free K-12 education for undocumented immigrants, but their accessibility to college was to be determined by state law.

Political rhetoric toward young unodocumented immigrants began to change in 2001, with the introduction of the *Development, Relief, and Education for Alien Minors Act* (DREAM Act). The bill intended to provide conditional residency to undocumented immigrants that met the following conditions: had arrived to the U.S. before the age of 16; had spent at least 5 years in the country; had no known criminal record; and had graduated from a U.S. high school. While never enacted into law, the DREAM Act influenced the immigration debate within lower levels of government. The same year, Texas was the first state in the nation to pass legislation which permitted immigrant students to access in-state tuition and state-provided financial aid, with the approval of House Bill 1403 (it would soon be followed by California and others). The provision targets any student that is a non-U.S.-citizen, but it was expected to have its greatest impact on the undocumented immigrant population (Brennan, 2001). The law became known as the *Texas Dream Act* (TDA), alluding to the similarity between this legislation and its federal counterpart; TDA beneficiaries must have clean criminal record, as well as graduated from a Texas public high school, and resided in the state for at least one year. Additionally, and of significant importance for this study, the law obliged beneficiaries to sign a notarized affidavit in which the individual made a legal promise to file an application for permanent residence "at the earliest opportunity that [the student is] eligible to do so".

TDA greatly reduced the prospective cost of attendance for undocumented high school students. Depending on the type of college, the difference between non-resident and resident tuition can range between 50% and 75% in Texas.[6] Before the reforms, this price difference, coupled with the lack of access to government financial aid and credit constraints in the private sector, effectively raised prohibitive barriers to higher education for undocumented students, who are known to be a high poverty rate group (Card and Raphael, 2013; Belanger, 2001; Passel and Cohn, 2009). Standard economic theory would suggest that such a reform; a subsidy to higher education coupled with relaxation of credit constraints, should lead to a rise in college demand and enrollment. However, the educational choice environment for young undocumented immigrants is more complex than that of native-born students. Undocumented students face a

[5]Division C of Pub.L. 104?208, 110 Stat. 3009-546, enacted September 30, 1996.

[6]Author's own calculations employing the Texas Higher Education Coordinating Board's "Overview of Tution and Fees", available at http://www.thecb.state.tx.us/reports.

complicated cost-benefit analysis when deciding whether to attend college. An undocumented high school graduate not only needs to take into account the relative value of work experience versus human capital attainment, but also the heightened risk of deportation associated with undocumented work; the wage penalty associated with undocumented status; the limited access to high skill occupations; and the value of school as a safe haven from deportation authorities.[7] Indeed, in many cases it is not straightforward to compute whether there is a positive expected pecuniary return to higher education for the undocumented, assuming that they stay in the U.S. and their status remains constant. Hence, the question of whether the price incentives generated by the reform would have a significant effect on undocumented graduates' demand for college is an empirical one.

Another policy-relevant aspect of the effects of TDA is whether it had any 'spillover effects' on the achievement of undocumented students prior to the decision to attend college. The definition of externality here refers to the policymaker's perspective. The authors of the reform were mainly concerned with college access for undocumented immigrants, yet it is possible that this reform affected the decisions of undocumented students down the educational ladder, by potentially incentivizing students to improve performance during high school, given the prospect of affordable college access. Indeed, an influential literature in structural labor economics assumes that students engage in a dynamic optimization problem when making education choices during high school (Keane and Wolpin, 1997). Hence, an interesting question in this setting is whether the prospective price reduction generated by the reform incentivized students to engage to a greater deal in 'college-bound' investments during high school. These investments can materialize in a number of ways, including endogenous changes in the following observable high school outcomes: graduation, drop out, enrollment in advanced placement (AP) courses, enrollment in dual credit courses (i.e. courses that count toward a college degree), course passing rate, attendance rate, standardized exam scores, and discipline events.

The ideal empirical environment to evaluate a reform such as TDA would hold constant any other government initiatives that intend to change the public education system in any form. This condition is generally difficult to achieve in practice, and this case is no exception. Most prominently, TDA's enactment year partially coincided with the enactment of the federal No Child Left Behind Act (NCLB) and an introduction of a more rigorous (and notably more difficult) standardized exam in the state, the Texas Assessment of Knowledge and Skills (TAKS).[8]

[7]For instance, the Famliy Educational Rights and Provacy Act (FERPA) forbids the use of unauthorized immigrant's school records by immigration authorities as evidence for deportation proceedings. This regulation is specially relevant during post-secondary education, when immigrant students are receiving financial aid that can be used for subsistence.

[8]The TAKS replaced the TAAS in 2003. According to the TEA, the TAKS was meant to be more comprehensive and rigorous than its predecessor. See: http://tea.texas.gov/student.assessment/taks/

The NCLB Act required all schools in the nation to administer standardized testing to their students; track the progress of the different vulnerable student subgroups within the schools (immigrants was one of these categories). While Texas already has standardized testing when NCLB passed, the introduction of the TAKS was meant to 'raise the bar' in the high school exit exam, which students had to pass in order to graduate.

Given this policy environment and the simultaneity of other reforms, causal estimates of the effect of TDA must be interpreted with caution. On the one hand, one would not expect changes in the standardized testing environment to affect the college going decisions of individuals that already have a high school diploma, precisely the population that the authors of TDA were targeting. As they already possess a high school diploma, college pricing is a more salient topic for them. On the other hand, students that are still in high school face a number of policy changes that could themselves affect the college-bound investment behavior. It may difficult to fully attribute changes in this popluation's behavior to TDA, but the exercise is still informative.

3 Empirical Strategy

3.1 Measurement

The primary challenge to studying the socioeconomic outcomes of undocumented immigrants is measurement. This group is popularly referred to as 'living in the shadows' given that most of these individuals leave a minimal paper trail. A majority of studies regarding this population rely on U.S. census questions which ask how long an individual has resided in the country, and whether the individual is an American citizen (Kaushal, 2008; Flores, 2010; Chin et al., 2010). While this serves as a nationally representative benchmark, it is likely that census-based statistics on this population are flawed. Indeed, it is possible that the trust that undocumented immigrants give to federal surveys, and hence the reliability of their responses, fluctuates with the rhetoric of the national political debate on immigration reform.

My analysis attempts to ameliorate measurement concerns by exploiting TEA administrative school data. Arguably, these records provide an improvement on the accuracy of information about student's immigration status for several reasons. First, personal information in educational records is protected by FERPA. This is made apparent to families when requests for information are made, which may reduce response anxiety. Second, enrollment in certain school programs is contingent on immigrant status. Hence, it may often be the case that families have an incentive to provide accurate information. Third, and most importantly for this

analysis, school records allow one to observe the longitudinal dimension of a student's immigrant indicator variable. We exploit this feature of the data, along with TEA guidelines for recording the immigrant indicator, and the fact that legal immigrant status is something that seldom changes from year to year, to motivate our definition of the treatment proxy.

The TEA records an immigrant flag for all K-12 students attending Texas public schools. Formally, this flag is meant to capture all students that are between 3 to 21 years of age, were born outside the U.S., and have not attended a U.S. school for 3 full academic years. FERPA regulation forbids schools from requesting information on the legal immigration status of students. Nonetheless, we argue that the share of TEA-defined immigrant students with an immigration status that would make them ineligible for TDA benefits is likely small. An important caveat of this flag for the purposes of this study is that immigrant flags are meant to be turned on for at most 3 years. If one were to naively use these raw indicators as a proxy for treatment status, one would only capture students that arrived to U.S. during their the last few years since observation. This would miss any eligible students that have been in the US system for longer than a few years, precisely the reforms target population. We take this caveat into account when constructing the treatment proxy used in this study.

We define the treatment indicator to capture any hispanic student that was flagged as an immigrant for 3 consecutive years, at any point during their enrollment in the TEA system.[9] This definition implicitly assumes that a student that was an eligible immigrant during elementary school is still eligible through-out the rest of her K-12 education. It is difficult to assess the degree of misclassification generated by this assumption. On the one hand, ignoring the longitudinal aspect would lead us to throw out immigrants that have been in the country for extended periods of time and lack status, a population that has been the focus of the debate in recent times. On the other hand, this simple definition rules out the possibility that immigrant students can gain legal residence status during their K-12 education.

As mentioned in Section 2, Texas Dream Act beneficiaries were obliged to submit a notarized affidavit promising to attain legal residence status at the earliest opportunity. This information is available in the THECB administrative records and provides one the cleanest measures of undocumented status, akin to the one used by Konger and Turner (2015). Since this flag is only available in college records during the post-reform period, it cannot be exploited to directly estimate the impact of the reform on high school and earlier outcomes. Nonetheless it opens an opportunity to validate the proxy measure used as the treatment indicator in the rest of the analysis. To operationalize this test, We make use of the empirical observation that 2-year

[9]We choose to focus on Hispanic students since they are by far the largest immigrant group in Texas and because our control group is motivated similarities in educational attainment between hispanic natives and their immigrant counterparts, see section 3.2.

colleges typically obtain most of their enrollment from local high school graduates. We compute sending rates from high school graduation cohorts to 2-year colleges, calling the college with the highest sending rate, the college that is 'linked' to this high school. The idea behind the validation test is that, if the proxy measure is good at capturing the treated, then high school cohorts with high proxy-immigrant shares should be linked to community colleges with high affidavit shares of incoming enrollment. In other words, when looking at a cross-section of high schools, the coefficient of an OLS regression of the affidavit-enrollment share for the linked 2-year college on the proxy immigrant-cohort share of the high school, should be positive and statistically significant.

Figure 1 shows a binned scatter plot providing a visual representation of this test. The vertical axis shows the affidavit-enrollment share at linked community colleges, while the horizontal axis measures the immigrant-cohort share of the high school. The conditional expectation function of affidavit shares conditional on proxy shares is clearly positively sloped and concave. This function conflates the mean measurement error in our proxy with the mean college enrollment rate of undocumented students. Thus, it is not possible to directly assess the degree of measurement error in the proxy from this figure alone. Nonetheless, Figure 1 elucidates two empirical patterns. First, a positive first derivative of this function suggests that our undocumented proxy indicator is not only capturing noise, it is predictive of undocumented status in a real sense. Second, a negative second derivative of this conditional expectation function suggests that schools that have high shares of immigrants have lower college enrollment rates than those with low immigrant shares. This pattern makes sense in a context of inequality in education between high poverty schools (which are the school that undocumented immigrants typically attend, see Table 1) and affluent ones. Inequality in educational resources in this respect has been documented widely in the economics of education finance literature (Cascio and Reber, 2013; Lafortune et al. 2016; Card and Krueger, 1992).

3.2 Data and Sample Selection

The analysis draws on data from the Texas Education Agency (TEA) via the Texas Education Research Center. These longitudinal student records consist of school identifiers, demographic characteristics, attendance, graduation, disciplinary action, dropout, and standardized test scores for every K-12 student attending public school in Texas. We analyze the effect of the reform by comparing the outcomes between cohorts of students. Data is available for the 1994 – 2014 school years, but we focus our attention to the 1998 – 2006 cohorts for the following reasons. First, as mentioned above, our treatment definition relies on the longitudinal aspect of enrollment records. We need to be able to look back on a students' enrollment at least three

years to tag her as potentially eligible TDA beneficiary. Weighing the tradeoff between being able to correctly classify more students and having sufficient pre-periods to assess the validity of the identification assumptions, we choose the first cohort in the analysis to be those that either graduated to entered high school in 1998.

Second, our main of outcome of interest, whether a student is planning to attend college, is only recorded in high school graduation files up to 2006. It should be noted that our estimates of the impact of the reform are applicable only to the population of immigrant students conditional of having graduated high school. This is not an innocuous restriction given that this population has particularly low graduation rates, and should be kept in mind in assessing the external validity of these estimates. Additionally, the Texas Dream Act applied to all eligible students attending a Texas high school after 2001. The law was passed during the summer of 2001 and came into effect immediately. Therefore, the first school cohort to graduate or enter high school with the reform in place was 2002. This logic defines the treatment year for our differences-in-differences analysis. Hence, by letting 2006 cohort to be the last cohort in the analysis, which also restricts our attention to a relatively tight time period around the reform, we end up with four cohort pre and post of the reform to drive our analysis.

In such a policy context the most natural counterfactual group for causal inference is students that are ineligible to benefit from the reform but that are otherwise identical to eligible students. In Texas, undocumented immigrant students are predominantly from hispanic descent (Passel and Cohn, 2009, 2016). For this reason, we restrict the analysis sample to hispanic students only. This decision is also done in light of the academic gaps that exist prominently in Texas' public schools, in which hispanic and black students performing considerably worse across the board than their white and asian counterparts. These high-performing groups would not serve as a good counterfactual for our population of interest. Moreover, the comparability of the treated and control groups can be further improved by restricting comparisons within schools. This feature of the analysis is important if one assumes that are temporally-fixed relevant characteristics of school quality that are unobservable in administrative records. We provide a formal econometric statement motivated by this intuition in the next section.

3.3 Econometric Model and Identification Assumptions

In order to model the statistical relationship between the advent of the reform and the educational outcomes of K-12 students, we adopt a generalized differences-in-differences (DD) econometric framework. This strategy for causal identification is based on the idea that students that attend the same high school share similar unobserved characteristics, making their classmates a useful counterfactual for immigrant students, after accounting for fixed differences

between schools, common time effects, and a number of observable of characteristics. These assumptions are further strengthened by restricting the analysis sample to Hispanic students. We first estimate the following standard parametric DD model:

$$Y_{ist} = \varphi_s + \alpha_t + \delta D_i + \beta D_i \times Post_t + X'_{ist}\Gamma + \varepsilon_{ist} \tag{1}$$

where Y_{ist} is an educational outcome for student i, attending school s, in year t; D_i is the proxy immigrant indicator; $Post_t$ is an indicator for years after 2002, the post-reform period; X_{ist} is a vector of time-varying student characteristics[10]; φ_s and α_t are school and year fixed effects; and ε_{ist} is an idiosyncratic error component. The effect of the reform is thus captured by the β coefficient in this specification, if the identification assumptions hold.

We also estimate flexible DD specifications that allow for a statistical test of the common-trends identification assumption, as well as the temporal evolution of the effects of the reform:

$$Y_{ist} = \varphi_s + \alpha_t + \sum_{t'=1998}^{2006} \beta_t\big(D_i \times I(t = t')\big) + X'_{ist}\Gamma + \varepsilon_{ist} \tag{2}$$

In this case we are interested in the β_t coefficients. If the common trend assumption holds, we expect the coefficients that correspond to the pre-reform period should not be statistically different from zero. The coefficients corresponding to the post reform period estimate the immediate effect of the reform and its evolution over time. The parametric estimate, β, from specification (1) can be computed as a weighted average of the post-reform β_t's in specification (2).[11]

We test two separate theories using this econometric framework. First, the direct impact of the tuition reduction generated by the reform on the college demand of undocumented high school graduates. This tests if, and to what extent, undocumented immigrants' financial constraints are to blame for the gap in college demand between immigrants and natives. If the gap is not affected by the reform, we could conclude that other factors, as mentioned in the theory section above, are the ones that drive the existence of this gap. Second, we test the spillover impact of this price-reduction on a number of high school outcomes related to college-bound investment behavior during high school. We test for these effects using the sample of high school entering cohorts (commonly known as the 'freshman' class). Theoretically, students that observe the change in the college pricing schedule during their early high school

[10]They include the following: exit scores in math and reading, gender, age, English language learner status, free or reduced priced lunch status, TWC match status, Spanish-speaking household, gifted status, at risk of dropping out status, and special education.

[11]Abstracting from the role of covariates and fixed effects in the mode, the weights here would simply correspond to the relative size of each cohort in the sample.

years have time to endogenously adjust their behavior in order to be better prepared for college when graduation time comes. Ignoring the caveats mentioned in the theory section, observing positive results in the entering-cohort specifications would be consistent with forward-looking behavior by undocumented students during high school.

The central identification assumption of this econometric model can be summarized in the following phrase: conditional on observable characteristics, TDA-eligible and control group students in the same high school follow similar trends in educational outcomes. As is standard in the literature, one can partially test this assumption by observing pre-trends prior to the advent of the reform on the outcomes of the treatment and control groups. We devote considerable time to this test in Section 4. Besides the common-trend assumption, identification of causal effects is also threatened by the following scenarios. (i) Time-varying sorting of immigrants into high schools: If, for instance, better-prepared immigrant students sort into better high schools differentially across time in response to the reform. This would lead to omitted variable bias in the specification with respect to unobservable school-cohort effects, essentially biasing the first difference in our DD estimation, leading us to overestimate the impact of the reform. We believe that such active sorting of the immigrant population across the spectrum of school quality is unlikely, given the residential constrains generally faced by this group and the income inequality linked to the quality of schools. (ii) Misclassification error in the treated: It could be the case that out proxy for immigrant is not very good at capturing the eligible population hence invalidating the empirical exercise. This concern is ameliorated to the extent possible in section 3.1. Finally, (iii) simultaneous policy introductions: as mentioned above this is not a concern for the graduating-cohort regressions, as they are no longer affected by K-12 education policy, but it is a concern in the entering-cohort regressions. We come back to this issue in the next section.

4 Results

We begin by describing the analysis sample using summary statistics. Table 1 presents the mean characteristics of the graduating cohort analysis sample, separate by treatment status, for the period before and after the enactment of the reform. Column (1) reports the mean characteristics of the treated before the reform. On average, about 56% of proxy-eligible graduates report planning to attend college. Additionally, given that undocumented students are known to be economically disadvantaged and Spanish speakers, we find it reassuring al large share of those tagged as eligible by our proxy have participated in the English Language Learner (ELL), Free or Reduced Lunch programs (FRL) and report that Spanish is the main language spoken at home. Furthermore, Columns (1) and (2) show that the eligible group is significantly more

disadvantaged than the control group, with lower test scores, higher risk of dropout, and higher poverty rates than the control group. They also attended larger high schools that have higher proportions of minority and economically disadvantaged students. Columns (3) and (6) show that although the treated and control groups differ in a number of characteristics, the patterns of observable inequality between these groups hold to roughly the same extent in the pre and post reform periods[12]. Finally, taking the difference between columns (6) and (3) for the college plans row gives us an unadjusted differences-in-differences estimate of the effect of the reform, about a 6% increase in college plans for the treated.

Table 2 reports similar summary statistics for the entering cohort analysis sample. It shows mean end-of-high school outcomes for entering high school students. Outcomes are measured four years after entering high school for the first time. These statistics make it clear that both the treated and control groups are significantly disadvantaged, with high school graduation rates of 54% and 60%, respectively. Moreover, it is evident that this group is considerably in a worse position academically than the treated graduating cohort summarized in Table 1. This is evident for instance when looking at exit exam scores in column (1) of Table 2. Comparing these mean scores to column (1) in Table 1, it is easy to see that although TDA-eligible graduates are below median performers, they are positively selected from the pool of all eligible high schoolers. This is perhaps not surprising, but such selection should be kept in mind when interpreting the causal estimates in the next section. Moreover, Table 2 shows mixed changes in academic performance for eligible high schoolers after the introduction of TDA. While the test score gaps were reduced over the reform period, there were considerable losses for the treated in terms of high school graduation and drop out. We explore these puzzling patterns to a more detailed extent in the next section.

One of the main outcomes of interest in this analysis is whether a high school graduate is planning to attend college. This flag is recorded in the TEA's high school graduation files. Given that it is not directly linked to actual college enrollment, a relevant question to ask is to what extent, if any, the college plans variable predicts actual enrollment. Unfortunately, due to data constraints, we cannot directly test the effect of the reform on college enrollment. [13] However, we can observe college enrollment for the control group. For this group we can ask: do college plans predict actual college enrollment? One concern with such exercise is that, as we had mentioned before, the control group has significantly different observable characteristics

[12]See Appendix Figure A3 for a visual evaluation of the similarity of trends in covariates between the control and treated group.

[13]In Texas, there are no universal student ID numbers that are used both for K-12 records (TEA) and college records (Texas higher education coordinating board). The Texas ERC links data across these government agencies via scrambled social security numbers. However, since undocumented students don't typically have SSN's, this crosswalk does not function for this population and hence we cannot observe college enrollment for the TDA-eligible population.

from the treated (in levels, but not trends). Hence, the correlation between college plans and enrollment may not be comparable between these two groups. With this issue in mind, we balance out the control group in terms of observables using a propensity score re-weighting exercise using the individual and school characteristics reported in Table 1. We then regress a college enrollment dummy on a college plans dummy for the propensity score re-weighted control group. Table 3 presents the results. Columns (1) and (2) report the OLS coefficients when the outcome is enrollment in any higher education institution. Note that the OLS coefficient is positive and highly statistically significant whether we control for a variety of observable and unobservables or not. We get a similar result when focusing in on with 2-year colleges (columns (3) and(4)) and 4-year colleges (columns (5) and (6)). These results suggest that, after making the control group 'look' like the treated in terms of observables, planning to attend college is associated with a 16 to 22% increase in the probability of actually attending college.[14] These figures are suggestive that college plans is a relevant variable for assessing the effect of TDA. Henceforth, we interpret college plans as a measure of college demand and use the terms interchangeably.

4.1 College Demand among HS Grads

We now move on to our causal estimates of the effect of TDA on graduating cohorts' college demand. Table 4 presents the results from the parametric DD model in equation (1). Column (1) of table (4) shows estimates for a basic DD model with no controls or fixed effects of any kind; we call these the 'raw' DD estimates. First thing to note is that there is a statistically significant pre-period gap in college plans between eligible graduates and control ones, of about 5.92%. Second, note that the raw DD estimate of the effect of the reform, a 5.88% increase in college plans for the treated, amounts to a complete closing of the gap. Columns (1) - (4) gradually introduce a series control variables and fixed effects to this specification in order to assess the sensitivity of out estimates to omitted variable bias. Note that the coefficient of interest is quite stable across specifications, decreasing only slightly as we introduce controls. Our preferred estimate of the effect of the reform is in column (4). When we control for school and cohort fixed effects, as well as a number of observables, we estimate that TDA caused a 5.27% increase in college demand. This estimate is statistically significant at the 1% level. Column (5) controls for treatment-group-specific time trends. The results are essentially unchanged, but given the statistical balance in trends (see Figure 2) we find this specification to be unnecessary for the validity of our causal estimates.

[14]Moreover, if TDA-eligible students have better unobservables than the control group (e.g. more intrinsic motivation), these figures should be interpreted as a lower bound of the association between college plans and college enrollment.

Figure 2 presents college demand results using the non-parametric DD specification in equation (2). Each point on the graph corresponds to the coefficient on a treatment dummy with cohort year interactions, where the 2001 graduating cohort is the omitted category. The coefficients corresponding to cohorts from earlier than 2002 afford us a statistical test for the parallel trend assumption central to the identification of these type of DD models. Note that the pre-treatment difference between the groups bounces around zero and that these coefficients are never statistically different from zero. We interpret this as the parallel trend assumption being satisfied. Furthermore, the post-treatment coefficients show an immediate effect of the reform which is stable across cohorts. The college demand gap between immigrants and natives seems to be closed immediately and permanently due to the reform. These results are reassuring that TDA had a significant effect on the human capital of the undocumented high school graduate population, providing the main result of the paper.

Another policy-relevant aspect of this reform is to identify precisely which type of eligible students became the beneficiaries of TDA. We approach this question in two ways. First, we employ the standard treatment effect heterogeneity analysis. Essentially, this method amounts to asking whether the average treatment effect of the reform was statistically different for different subgroups within the treated. Second, we employ a characteristics of the compliers analysis as developed by Card and Giuliano (2015). This method estimates the mean characteristics of those that were induced by the reform to plan to attend college, by differencing out the characteristics of eligible students that would have positive college demand regardless of the introduction of TDA.

Table 5 estimates treatment effect heterogeneity specifications for the parametric DD model (equation (1)). Each column corresponds to a different characteristic being tested for heterogeneity. Column (1) tests whether the effect was different across genders, which is not the case, given that the coefficient on the interaction corresponds to less than a 1% difference in mean effects. The same pattern holds for most of the characteristics tested, with some notable exceptions. First, column (3), whether a student's exit reading score is above the state-wide median. We see negative treatment effect heterogeneity for this group, that is, the law had a smaller effect for eligible individuals with high reading scores. This is most likely due to an 'always-taker' type of phenomenon; these students would have planned to attend college anyway. Second, whether a student is in a ELL program during high school, column (5). Student's with this characteristic have a much higher average treatment effect. We interpret this as evidence that the reform mainly empowered fringe immigrant student groups that would have not, without the advent of TDA, planned to attend college. Finally, schools that have below median test scores and those that have above median immigrant shares saw larger increases in college demand due to introduction of TDA. This is consistent with the previous results, namely that

the main beneficiaries of TDA were some of the most disadvantaged students in the state.

We now test estimate the mean characteristics of the complier group. The motivation for this exercise is similar to that in Table 5, but it is of a different nature. The estimates in Table 5 answer the question: did different subgroups respond more or less strongly to the reform? The current exercise asks a fundamentally different question. It tells us what type of student on average is planning to attend college due entirely to the introduction of TDA. In essence, it characterizes the average demographics of the students that are driving the average treatment effect that we estimated in Table 4 and Figure 2. Please turn to the appendix for a rigorous derivation of these propositions. The results of this exercise are shown in Table 6. We see that the complier group is about 80% female, 89% economically disadvantaged, and 27% English learner. Furthermore, on average the compliers are not high performing in standardized exams, with only 53% and 26% performing above median in the math and reading exit exams, respectively. Finally, and perhaps surprisingly, the average complier student has 75% probability of graduating from a high school with an above state-wide median graduation rate.

4.2 College-Bound Investments

The above section shows that TDA closed the gap in college demand between undocumented and native high school graduates. The strength of these results motivates another policy-relevant question. Do the tuition reductions generated by TDA create dynamic incentives for students still attending high school? If this is the case, the benefits of tuition equity reform would be noticed at education levels prior to the college-entry, providing a stronger reasoning for policy makers across the country to adopt such a policy. We test this theory by estimating DD models around the reform for 9th grade cohorts. The outcome variables in these regressions are high school outcomes related to college-bound investments, including: exit exam scores, high school graduation, dropout, course-passing rate, attendance rate, discipline event rate, number of credits attempted, advanced placement (AP) courses attempted, and dual credit courses attempted.[15]

Table 7 and Figure 3 present estimates from the parametric and non-parametric DD models, respectively. Notable immediately is that the raw parametric model estimates are negative and statistically significant almost across the board of our college investment outcomes, with the exception of exit exam scores. Taking these results at face value would lead us to conclude

[15]In the Texas education system, AP courses and dual credit courses can be redeemed as college credit toward a degree if certain conditions are met. Hence, students that enroll in these are typically in preparation for enrolling in college.

that TDA reduced college-bound investments for undocumented high schoolers, a puzzling conclusion given the strong positive results in the previous section. However, taking a look at Figure 3, we see that the story is more complicated than that. Each of the graphs in Figure 3 corresponds to covariate-adjusted flexible DD estimates for a different outcome. Panels (1) and (2) report results for exit scores. We can see that there are some pre-trends for these outcomes, and that the positive results for test scores in Table 7 are driven by a weighted combination of negative and positive differences in the post-reform cohorts.

More concerning perhaps are the results in panels (3) and (4) of Figure 3. These show considerable losses in high school graduation and dropout for the treated. It is difficult to attribute these results to pre-trends in the data, as there is hardly any evidence of these for high school graduation, but perhaps so for drop out. A similar pattern holds when we look at panels (5) through (8). Finally, panels (9) and (10) show that dual-credit and AP course enrollment are too rare of an event for our models to be able to pick out any effects that are statistically significant.

We argue that the negative results observed in this section are due to simultaneous policy introductions that coincided with the enactment of TDA. First, the No Child Left Behind Act was enacted in early 2002. NCLB introduced a number of standard that schools needed to maintain in order to keep the autonomy in their leadership, which gave the NCLB provisions bite. A relevant part of the NCLB provisions was that performance standards need to be achieved within subgroups of students considered vulnerable. Immigrant students were one of these categories, prompting school staff to shift their policy for addressing these students. This change in school behavior toward the eligible group coincided with the enactment of TDA. NCLB effects on high school outcomes of the undocumented might have been positive or negative. This has been a point of contention for academics that have studied the effects of this reform (Deming et al., 2013; Ahn and Vigdor, 2014; Dee and Jacob 2009). Second, Texas dramatically changed its standardized testing system in early 2003, when the TAKS replaced the TAAS. The new exam was meant to be considerably more difficult than its predecessor. In addition, unlike the TAAS, the TAKS could not be taken in Spanish. This fact could explain a differential effect of the introduction of this system on the TDA-eligible population, thus confounding our analysis.

More formally, the presence of these simultaneous policy introductions contaminate the treatment-post variable in the DD model, and compound the effect of all the reforms in our simple estimates. There is little that we can do to control for the effect of these policies. Due to this complication we conclude that the policy environment is too complex to elucidate whether tuition equity reform would have a trickle-down-type of effects to lower education levels, at least

with the current identification strategy. It should be noted that these econometric issues are not as much of a concern in the graduate cohort analysis in the previous section. Even though some of the later graduate cohorts were still in high school when this package of policies was introduced, by conditioning on high school graduation (an important outcome), we eliminate most of the concerns of contamination of our estimates.

5 Conclusion

A longstanding concern in the debate about undocumented immigration is the adjustment of status of minors who have attended U.S. public schools from a young age. Of special interest to education policymakers is whether the observed gaps in educational attainment between undocumented immigrant students and their native counterparts is due to differences in cognitive development or to the institutional constraints associated with lack of immigration status. The introduction of tuition equity reforms across different states in the nation provide a natural experiment to test these theories against each other. In light of this, an emerging literature has analyzed the effect of tuition equity reform on the educational outcomes of immigrant populations. These papers have generally employed survey-based data to estimate the impacts of the reform on college enrollment, with mixed results. More recently, Conger and Turner (2015), exploit administrative data from a university to assess the impact of tuition equity on undocumented students' re-enrollment and graduation.

We contribute to this literature by providing the first estimates of the impact of tuition equity employing administrative data from K-12 public education agencies in Texas, the first state to introduce tuition equity in 2001. These data allow us to accurately zoom in to the population that policymakers likely had in mind when drafting the reform: undocumented high school graduates. Moreover, motivated by the notion that college preparation starts well before high school graduation, we test for effects on college-bound investments for high school entering cohorts. As opposed to previous work, we provide a simple validation exercise which shows that our treated group is indeed composed of undocumented immigrants, the group that benefitted from the reform. Once we find the treated we estimate generalized differences-in-differences models comparing the outcomes of eligible students to those of non-immigrant hispanics attending the same high school, a natural control group in this setting.

Preferred estimates show that the reform led to a 5 percentage point increase in college demand for eligible graduates. This increase represents a shrinking of the gap in college demand between natives and immigrants of about 90%, in essence closing the gap entirely. Further analysis shows that those induced by the reform were predominantly from academically disad-

vantaged backgrounds attending schools with high poverty rates. However, we obtain mixed results when testing for effects on college-bound investments. We attribute these to the complex policy environment that prevailed in K-12 schools during this time period, with the introduction of several initiatives targeting the performance of student subgroups, including immigrants. We argue that these concerns are not an issue for our first set of estimates, as they condition on high school graduation.

Our analysis is subject to two important limitations, both related to the difficulty of studying undocumented immigration. First, we are unable to directly test the effect of the reform on college enrollment. Due to the manner in which identification systems work in education agencies we cannot link the treated population to college enrollment records.[16] Second, we are unable to perfectly identify the treated group. Even though our validation exercise shows that the treatment proxy is not off the mark, it still contains classification error, whose extent is difficult assess.

We conclude that although undocumented immigrant students are a severely disadvantaged group, policies aimed at relaxing the constraints associated with lack of immigration status can have a significant impact on their assimilation and attainment. These results are difficult to reconcile with a negative selection theory of immigration. These results, however, apply only to those at the juncture of graduating high school and choosing whether to enter college, not at all an innocuous restriction in this setting. We hope further research will elucidate whether policies aimed at modifying undocumented immigration restrictions at other stages of the life cycle have an effect relevant outcomes not just related to educatiom but also income, remittances, tax contributions, and economic welfare.

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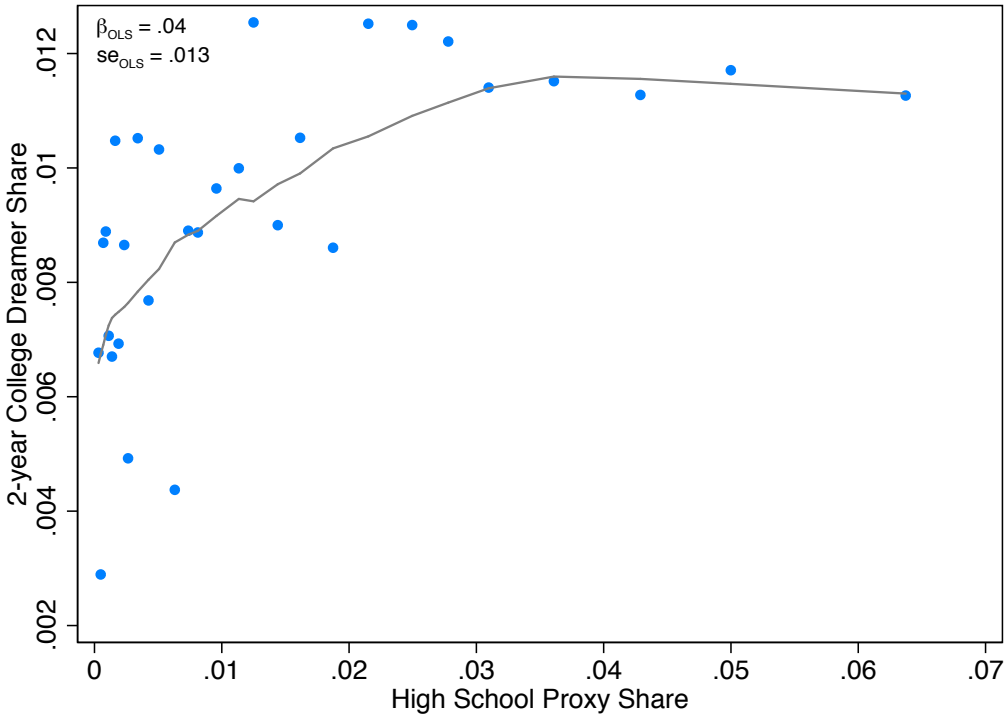
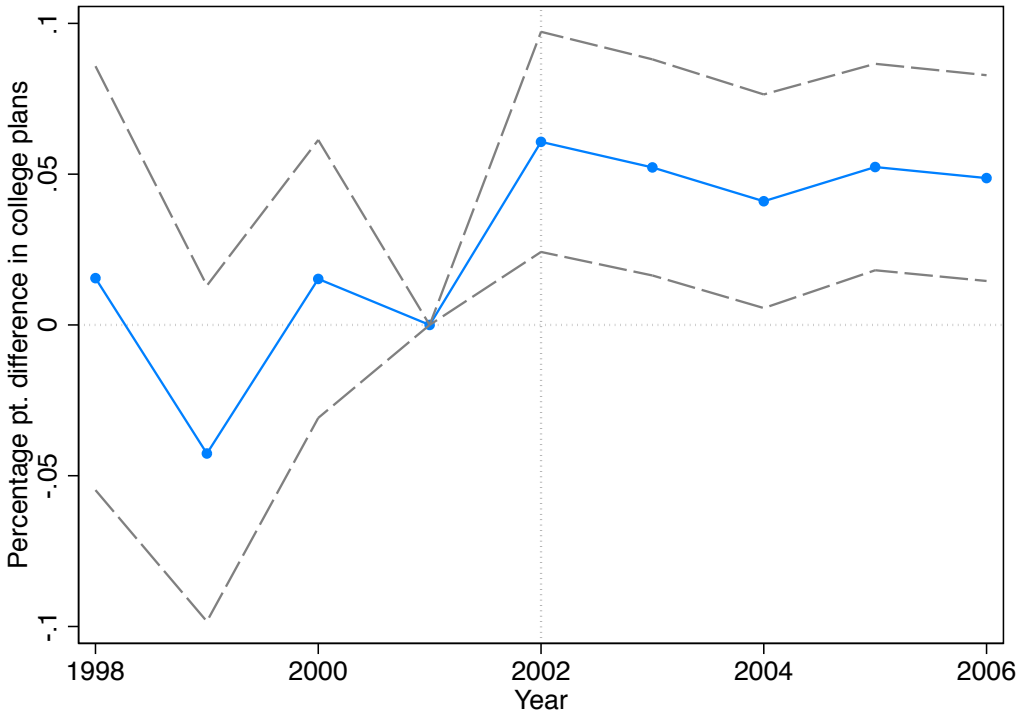
Figure 1: Validation of Undocumented Immigrant Proxy



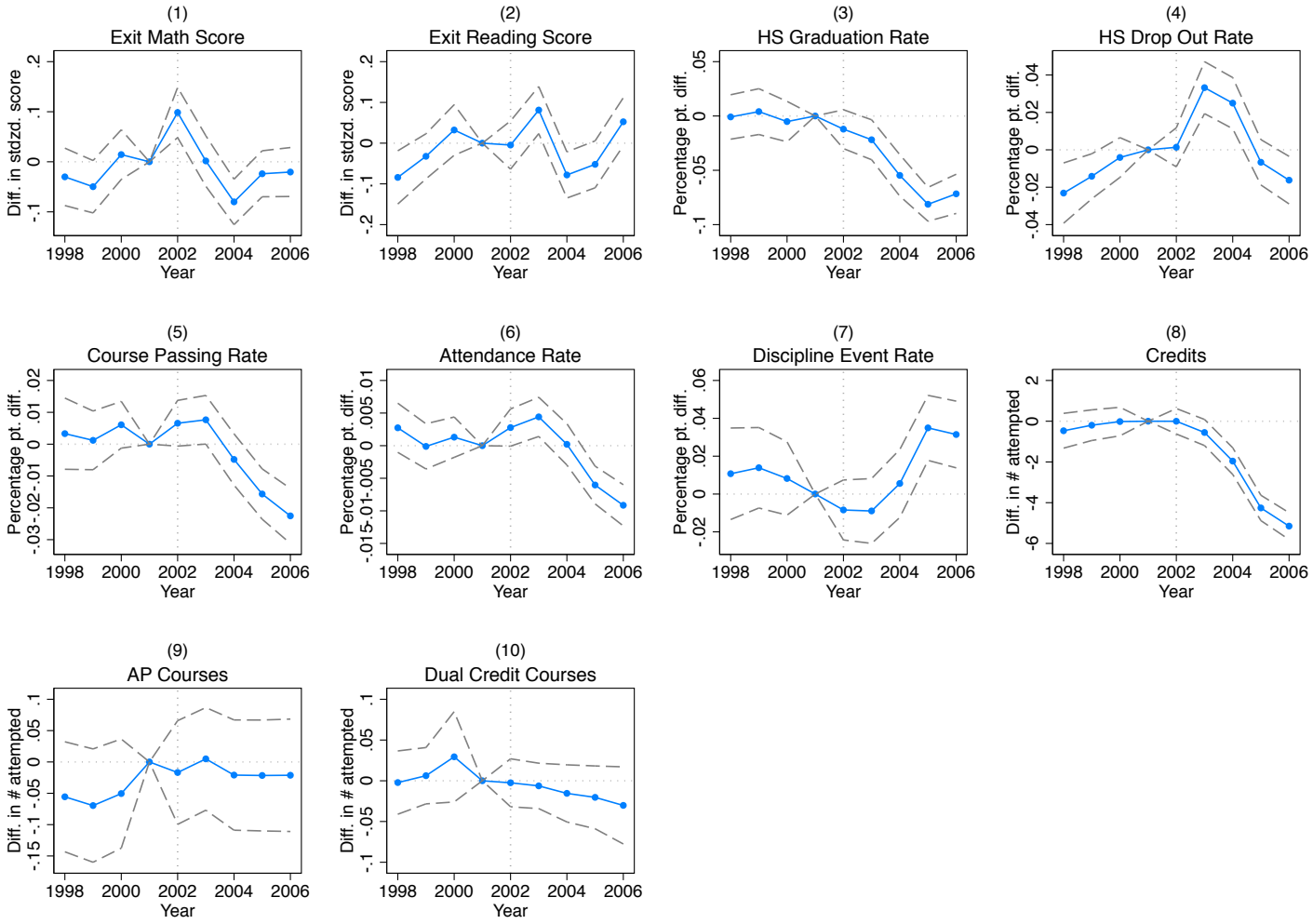
Note: Binned scatter plot with 30 quantiles. Sample is a cross section of all public high schools in Texas, linked to nearby 2-year colleges. The horizontal axis measures the share of a high school cohort that is tagged as an undocumented immigrant using our proxy, while the vertical axis measures the share of Texas Dream Act beneficiaries in the incoming freshman cohort at 2-year colleges located near the high school in question. The figure also shows a Lowess regression fit to the underlying micro data. It also reports the OLS coefficient and robust standard error of a simple univariate regression on these variables. The coefficient is positive and statistically significant.

Figure 2: Non-parametric DD estimates - Effect of TDA on Graduating Cohorts' College Plans



Note: Figure presents non-parametric DD estimates computed via OLS as presented in equation (2). The blue line denotes to estimated coefficients, while the dotted gray line denotes the corresponding 95% confidence intervals of these estimates. The model controls for school fixed effects, cohort fixed effects, and the following observable characteristics: exit scores in math and reading, gender, age, ELL status, FRL status, TWC match status, Spanish-speaking household, gifted status, at risk of dropping out status, and special education. The estimation sample is the universe of hispanic high school graduates from Texas for the years 1998-2006. Standard errors are clustered at the high school level.

Figure 3: Non-parametric DD estimates - Effect of Reform on College-Bound Investments.



Note: Figure presents estimated OLS coefficients on treatment-cohort interactions as presented in equation (2), for a range of high school outcomes related to college-bound investments. The blue line denotes to estimated coefficients, while the dotted gray line denotes the corresponding 95% confidence intervals of these estimates. The model controls for school fixed effects, cohort fixed effects, and the following observable characteristics: exit scores in math and reading, gender, age, ELL status, FRL status, TWC match status, Spanish-speaking household, gifted status, at risk of dropping out status, and special education. The estimation sample is the universe of hispanic entering cohorts in Texas, for the years 1998-2006. Standard errors are clustered at the high school level.

Table 1: Summary Statistics - Graduating High School Cohorts

	Pre 2002			Post 2002		
	Immigrant	Control	Difference	Immigrant	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Individual Characteristics						
Plans to Attend to College	0.56	0.62	-0.06	0.69	0.69	-0.00
Age	17.64	17.30	0.34	17.45	17.24	0.21
Female	0.51	0.52	-0.01	0.51	0.51	0.00
Gifted and Talented Prog.	0.09	0.09	0.01	0.05	0.08	-0.03
Special Education	0.04	0.11	-0.06	0.05	0.12	-0.07
At Risk of Dropping Out	0.64	0.50	0.14	0.72	0.57	0.15
Spanish Spoken at Home	0.82	0.30	0.52	0.85	0.36	0.49
English Language Learner (ELL)	0.87	0.24	0.63	0.97	0.42	0.54
Free or Reduced Price Lunch (FRL)	0.94	0.76	0.18	0.96	0.84	0.12
Exit Math Score	-0.12	0.10	-0.22	-0.15	0.03	-0.18
Exit Reading Score	-0.65	0.02	-0.68	-0.55	-0.01	-0.54
School Characteristics						
Cohort Size	346.14	325.59	20.56	375.17	348.20	26.98
Graduation Rate	0.41	0.47	-0.06	0.45	0.51	-0.05
Share FRL	0.53	0.45	0.08	0.59	0.50	0.09
Share Minority	0.80	0.68	0.11	0.79	0.69	0.10
Share Gifted and Talented	0.16	0.12	0.05	0.11	0.11	0.00
Share Immigrant	0.08	0.02	0.06	0.09	0.04	0.05
Observations	7434	241069	248503	22216	376372	398588

Note: Mean characteristics of graduating cohort sample reported. Sample consists of hispanic students that have just graduated from a public high school in Texas. The immigrant group corresponds to those flagged by our proxy as eligible to be TDA beneficiaries. The control group corresponds to all hispanic students that are never flagged as an immigrant in the TEA records. The pre-reform period corresponds to the 1998-2001 graduating cohorts, and the post period to the 2002-2006 cohorts. The ELL and FRL indicators are measured longitudinally, that is they indicate whether the student has ever been in these programs.

Table 2: Summary Statistics - Entering High School Cohorts

	Pre 2002			Post 2002		
	Immigrant	Control	Difference	Immigrant	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
High School Outcomes						
Graduation	0.54	0.60	-0.06	0.52	0.60	-0.08
Drop Out	0.09	0.08	0.01	0.16	0.14	0.03
Credits Attempted	38.11	35.95	2.16	38.23	37.49	0.74
AP Courses Attempted	0.69	0.84	-0.15	1.02	1.20	-0.18
Dual Credits Attempted	0.16	0.22	-0.06	0.21	0.31	-0.10
Course Passing Rate	0.81	0.80	0.01	0.82	0.82	-0.00
Attendance Rate	0.93	0.91	0.02	0.93	0.92	0.01
Discipline Event	0.45	0.52	-0.07	0.63	0.66	-0.03
Exit Math Score	-0.55	-0.27	-0.28	-0.47	-0.27	-0.19
Exit Reading Score	-1.07	-0.32	-0.75	-0.83	-0.26	-0.57
Individual Characteristics						
Age	14.58	14.39	0.18	14.42	14.30	0.11
Female	0.48	0.49	-0.00	0.49	0.49	-0.00
Gifted	0.03	0.07	-0.03	0.04	0.07	-0.03
Special Education	0.04	0.13	-0.09	0.05	0.13	-0.07
At Risk	0.55	0.52	0.03	0.77	0.57	0.20
Spanish Spoken at Home	0.89	0.38	0.52	0.90	0.44	0.46
ELL	0.96	0.41	0.55	0.99	0.52	0.46
FRL	0.96	0.86	0.10	0.97	0.90	0.08
G8 Math Score	-0.49	-0.27	-0.22	-0.44	-0.25	-0.18
G8 Reading Score	-0.75	-0.31	-0.44	-0.69	-0.26	-0.43
School Characteristics						
Cohort Size	585.84	514.49	71.36	584.23	525.39	58.84
Share FRL	0.64	0.55	0.08	0.67	0.60	0.07
Share Gifted	0.10	0.10	0.00	0.09	0.09	-0.00
Share Minority	0.83	0.72	0.11	0.82	0.74	0.09
Share Immigrant	0.09	0.03	0.05	0.11	0.05	0.05
Observations	20887	415996	436883	49509	613388	662897

Note: Mean characteristics of entering cohort sample reported. That is, those that were in 9th grade for the first time that year. Sample consists of hispanic students alone. The immigrant group corresponds to those flagged by our proxy as eligible to be TDA beneficiaries. The control group corresponds to all hispanic students that are never flagged as an immigrant in the TEA records. The pre-reform period corresponds to the 1998-2001 entering cohorts, and the post period to the 2002-2006 cohorts. The ELL and FRL indicators are measured longitudinally, that is they indicate whether the student has ever been in these programs.

Table 3: Correlation between College Plans and College Enrollment – P-score Re-weighted Control Group

	Any College		2-year College		4-yr College	
	(1)	(2)	(3)	(4)	(5)	(6)
College Plans	0.224***	0.165***	0.102***	0.0609***	0.142***	0.112***
	(0.0123)	(0.00903)	(0.00738)	(0.00569)	(0.00994)	(0.00575)
N	617427	617427	617427	617427	617427	617427
adj. R^2	0.056	0.204	0.030	0.166	0.027	0.117
Covariates		✓		✓		✓
Year FE		✓		✓		✓
Campus FE		✓		✓		✓

Note: Standard errors are clustered at the high school level. The estimation sample is the control group, restricting to hispanic graduates in the 1998-2006 graduating cohorts. Observations are re-weighted to match the observable characteristics of undocumented immigrant graduates, using the standard propensity score re-weighting methodology. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Parametric DD Estimates – Effect of Reform on Graduating Cohorts' College Plans

	(1)	(2)	(3)	(4)	(5)
Post	0.0695***	0.0630***			
	(0.00911)	(0.00941)			
Immigrant	-0.0592***	-0.0124	-0.0137	-0.0321**	-0.0290
	(0.0192)	(0.0170)	(0.0171)	(0.0128)	(0.0188)
Immigrant × Post	0.0588***	0.0441**	0.0449**	0.0527***	0.0578***
	(0.0194)	(0.0185)	(0.0185)	(0.0157)	(0.0177)
N	647091	647091	647091	647091	647091
adj. R^2	0.006	0.098	0.099	0.088	0.088
Covariates		✓	✓	✓	✓
Year FE			✓	✓	✓
Campus FE				✓	✓
Group Trends					✓

Note: Standard errors are clustered at the high school level. v. Table shows standard diffierences-in-differences estimates of the effect of the reform on the treated group's plans to attend college. Covariates include: Exit scores in math and reading, gender, age, ELL status (ever and currently), FRL status (ever and currently), TWC match status, Spanish-speaking household, Gifted status, at risk status, and special education. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Parametric DD Estimates – Heterogeneity of Treatment Effect on College Plans.

	Individual Characteristics							School Characteristics	
	(1) Female	(2) >p50 Math	(3) >p50 Reading	(4) FRL	(5) ELL	(6) At Risk	(7) Gifted	(8) >p50 Grad Rate	(9) >p50 Imm. Share
X	0.0809***	0.158***	0.155***	-0.0582***	-0.188***	-0.122***	0.186***	0.000792	-0.0393*
	(0.00433)	(0.00739)	(0.00688)	(0.0115)	(0.0141)	(0.0103)	(0.0131)	(0.0289)	(0.0235)
Immigrant	-0.0831***	-0.106***	-0.0771***	-0.133***	-0.0531***	-0.0821***	-0.0847***	-0.112***	-0.0726**
	(0.0212)	(0.0226)	(0.0211)	(0.0252)	(0.0192)	(0.0251)	(0.0180)	(0.0232)	(0.0341)
Immigrant × X	-0.0201*	0.000535	0.00999	0.0669***	0.00615	0.00802	-0.0625	0.0397*	-0.0399
	(0.0120)	(0.0129)	(0.0140)	(0.0173)	(0.0150)	(0.0164)	(0.0484)	(0.0238)	(0.0340)
Immigrant × Post	0.0603***	0.0689***	0.0685***	0.0670***	0.0411**	0.0657***	0.0623***	0.0887***	-0.0268
	(0.0209)	(0.0208)	(0.0206)	(0.0238)	(0.0193)	(0.0192)	(0.0194)	(0.0225)	(0.0378)
Imm. × Post × X	0.00498	-0.0180	-0.0341**	-0.00671	0.0542***	0.0121	0.0596	-0.0555*	0.0796**
	(0.0130)	(0.0136)	(0.0158)	(0.0194)	(0.0166)	(0.0183)	(0.0524)	(0.0299)	(0.0373)
N	647091	550897	538275	647091	647091	647091	647091	608440	647091
adj. R^2	0.016	0.031	0.031	0.011	0.016	0.030	0.019	0.009	0.009
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Campus FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Group Trends	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: Standard errors are clustered at the high school level. The estimation sample is the universe of Hispanic high school graduating cohorts 1998-2006. The title of each column denotes the trait which the specification is testing for treatment effect heterogeneity. The 'X's in the variable titles correspond to an indicator variable for said trait. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Characteristics of the Compliers - College Plans

	(1)	(2)
Individual Characteristics		
Female	0.78	(0.17)
>p50 Exit Math Score	0.53	(0.20)
>p50 Exit Reading Score	0.26	(0.19)
FRL	0.89	(0.18)
ELL	0.27	(0.19)
Gifted	-0.185	(0.13)
School Characteristics		
>p50 Graduation Rate	0.74	(0.39)
N	617427	

Note: Standard errors are clustered at the high school level. The estimation sample is the universe of Hispanic high school graduating cohorts 1998-2006. Two-stage-least squares estimates of the characteristics of the complier group. Specifically, we estimate a 2SLS model in which the exogenous instrument in the post-treat indicator, and the outcome of interest is the interaction between treatment and the reported covariate. The endogenous variable is a treatment dummy. In addition, we control for group-specific trends in this specification. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Parametric DD Estimates – Effect of Reform on College-bound Investments – Raw Effects

	(1) Graduation	(2) Dropout	(3) Credits	(4) AP Courses	(5) Dual Credit	(6) Course-Passing Rate	(7) Exit Math Score	(8) Exit Reading Score	(9) Attendance Rate	(10) Discipline Event
Post	0.00289	0.0536***	1.539***	0.355***	0.0917***	0.0223***	-0.00220	0.0527***	0.00642***	0.148***
	(0.00284)	(0.00203)	(0.149)	(0.0198)	(0.0316)	(0.00192)	(0.00896)	(0.00767)	(0.000768)	(0.00450)
Immigrant	-0.0551***	0.00645	2.164***	-0.149***	-0.0624***	0.00864***	-0.282***	-0.751***	0.0156***	-0.0653***
	(0.00648)	(0.00403)	(0.294)	(0.0291)	(0.0155)	(0.00299)	(0.0208)	(0.0295)	(0.00128)	(0.00835)
Imm. × Post	-0.0269***	0.0210***	-1.419***	-0.0314	-0.0403**	-0.0130***	0.0882***	0.185***	-0.00449***	0.0335***
	(0.00581)	(0.00414)	(0.250)	(0.0308)	(0.0188)	(0.00299)	(0.0184)	(0.0254)	(0.00123)	(0.00685)
Observations	1099780	1099780	1086330	1086330	1086330	1086330	786675	774269	1099089	1099780
Adjusted R^2	0.001	0.007	0.002	0.004	0.002	0.002	0.003	0.020	0.002	0.022

Note: Standard errors are clustered at the high school level. The estimation sample is the universe of Hispanic 9th grade entering cohorts 1998-2006. The immigrant group corresponds to those flagged by our proxy as eligible to be TDA beneficiaries (see section 3.1). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

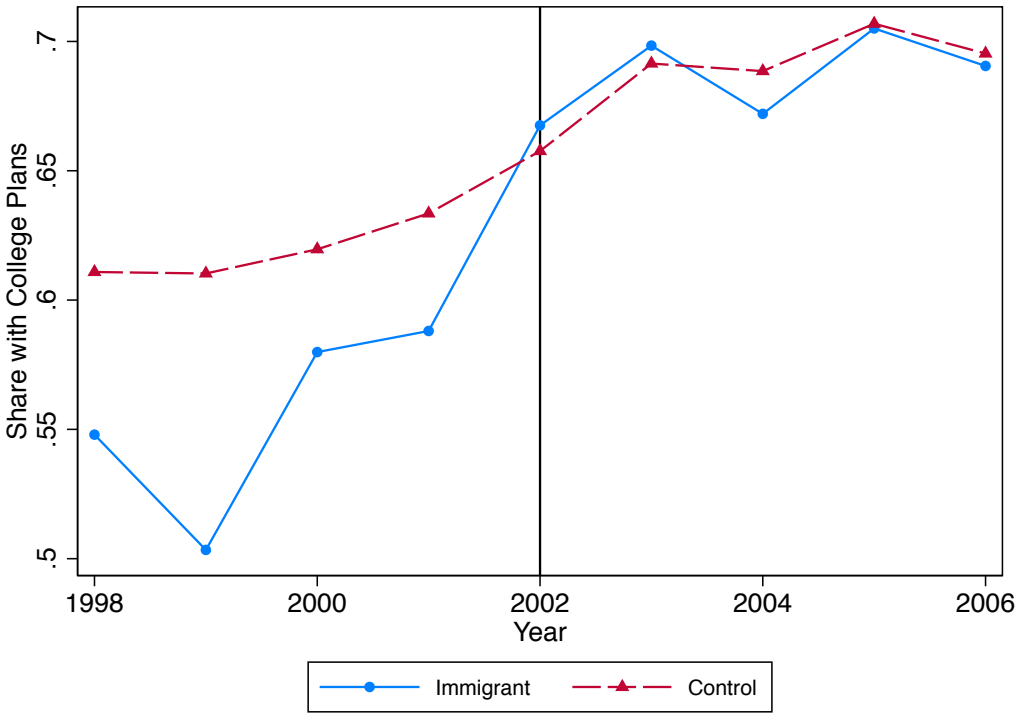
Table 8: Parametric DD Estimates – Effect of Reform on College-bound Investments – Adjusted Effects

	(1) Graduation	(2) Dropout	(3) Credits	(4) AP Courses	(5) Dual Credit	(6) Course-Passing Rate	(7) Exit Math Score	(8) Exit Reading Score	(9) Attendance Rate	(10) Discipline Event
Immigrant	0.123***	-0.00954**	8.419***	0.0770**	0.0237	0.0563***	0.0910***	-0.0623***	0.0253***	-0.0459***
	(0.00774)	(0.00417)	(0.364)	(0.0299)	(0.0189)	(0.00394)	(0.0153)	(0.0189)	(0.00136)	(0.00712)
Imm. × Post	0.0123	0.0373***	2.345***	0.0177	-0.00197	0.0202***	0.0742***	-0.00406	0.0101***	-0.0347***
	(0.00923)	(0.00568)	(0.349)	(0.0422)	(0.0199)	(0.00385)	(0.0228)	(0.0253)	(0.00150)	(0.00759)
Observations	1099780	1099780	1086330	1086330	1086330	1086330	786675	774269	1099089	1099780
Adjusted R^2	0.205	0.062	0.259	0.242	0.068	0.206	0.493	0.482	0.097	0.125
Covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Campus FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Group Trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: Standard errors are clustered at the high school level. The estimation sample is the universe of Hispanic 9th grade entering cohorts 1998-2006. The immigrant group corresponds to those flagged by our proxy as eligible to be TDA beneficiaries (see section 3.1). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

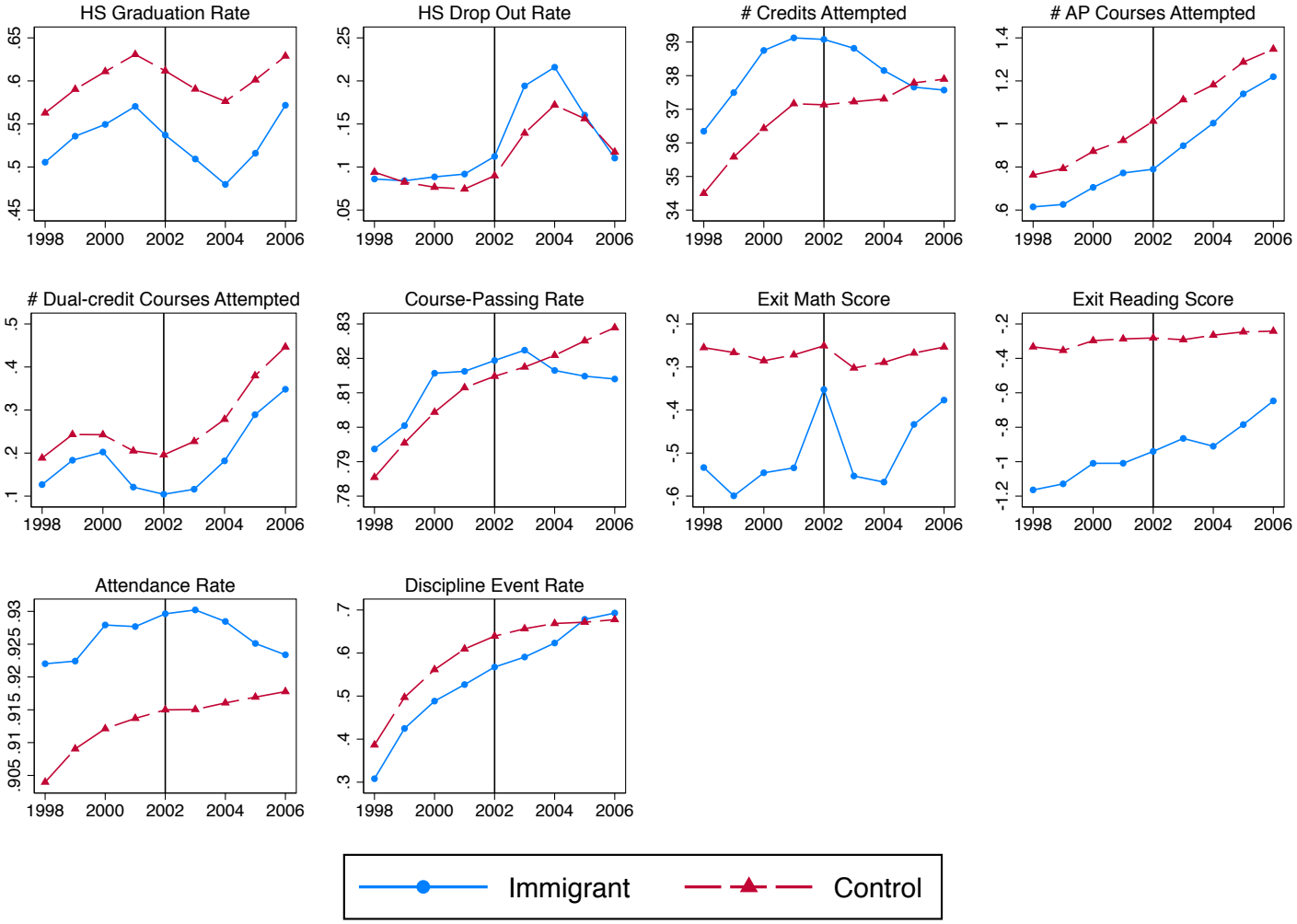
6 Appendix

Figure 4: Graduating Cohort – Raw College Plans Trends



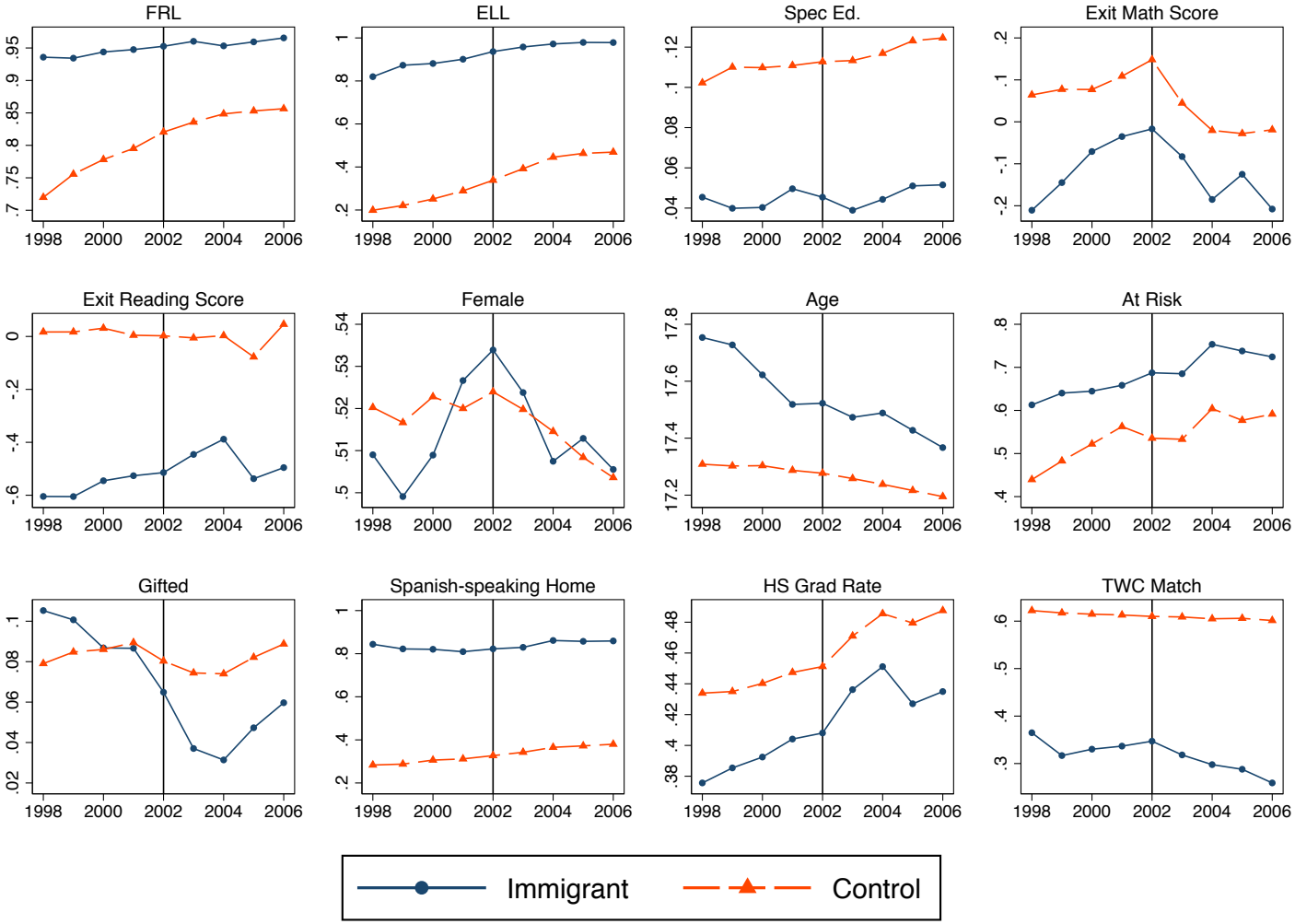
Note: Figure shows raw cohort shares for college demand, by treatment status. The estimation sample is the universe of Hispanic high school graduates. The vertical line denotes the year in which tuition equity reform was introduced.

Figure 5: Entering Cohort College-Bound Investments – Raw Outcome Trends



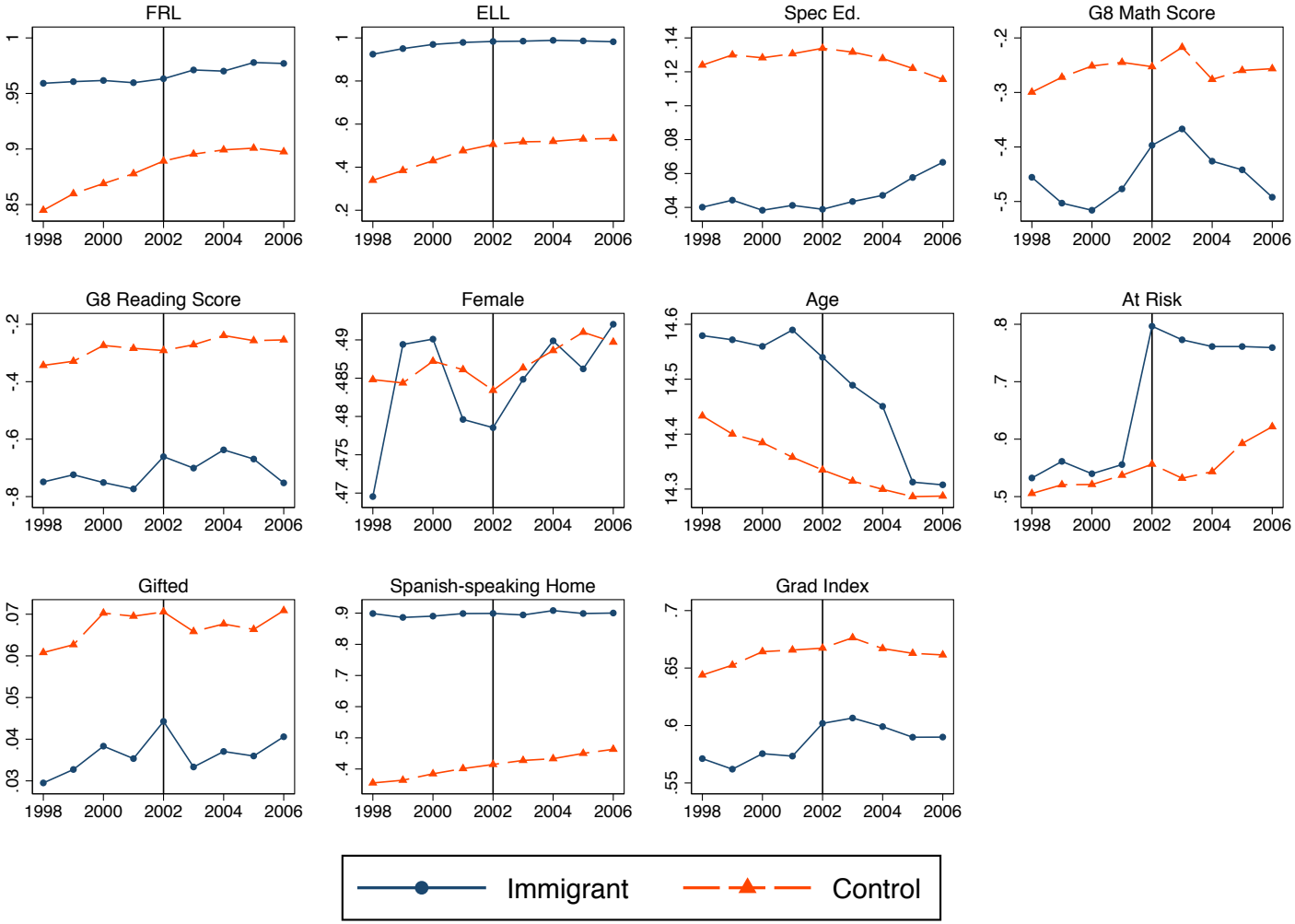
Note: Figure shows raw outcome shares for a range of high school outcomes, by treatment status. The outcomes include: high school graduation, dropout, number of school credits attempted, number of Advanced Placement courses attempted, number of dual-credit courses, course-passing rate, standardized exam scores in mathematics and reading, attendance rate, and discipline event rates. The estimation sample is the universe of Hispanic high school entering cohorts. The vertical line denotes the year in which tuition equity reform was introduced.

Figure 6: Graduating Cohort Covariate Trends



Note: Figure shows raw cohort shares for a range of covariates, by treatment status. The covariates include: free and reduced price lunch status (FRL), english language learner status (ELL), special education participant, exit standardized exam scores in mathematics and reading, gender, age at graduation, at risk of dropping out status, gifted and talented program participant, and indicator for whether spanish is the main language spoken at home, graduation rate at student's high school, and TWC match status. The estimation sample is the universe of Hispanic high school graduates. The vertical line denotes the year in which tuition equity reform was introduced.

Figure 7: Entering Cohort Covariate Trends



Note: Figure shows raw cohort shares for a range of covariates, by treatment status. The covariates include: free and reduced price lunch status (FRL), english language learner status (ELL), special education participant, 8th grade standardized exam scores in mathematics and reading, gender, age at graduation, at risk of dropping out status, gifted and talented program participant, and indicator for whether Spanish is the main language spoken at home, graduation rate at student's high school, and an index combining all of these demographics to predict the probability of high school graduation (using a simple probit model and computing predicted values). The estimation sample is the universe of Hispanic entering cohorts. The vertical line denotes the year in which tuition equity reform was introduced.