Jason Cohn (Can be used as example)

A Replication Study of "Pricing Out the Disadvantaged? The Effect of Tuition Deregulation in Texas Public Four-Year Institutions"

I. Introduction and Background

In the paper, "Pricing Out the Disadvantaged? The Effect of Tuition Deregulation in Texas Public Four-Year Institutions," Stella M. Flores and Justin C. Shepherd examine the effect of a Texas tuition deregulation policy on the enrollment of Hispanic, Black, and low-income students in public research universities. In this paper, I replicate and extend their analysis.

In 2003, the state of Texas passed a law that allowed institutional governing boards to have authority over tuition-setting beginning in 2004, which led to increases in tuition costs over the next several years (Flores & Shepherd 2014). Meanwhile, Flores and Shepherd remind us that the number of Hispanic high school graduates in Texas had also been increasing during the same time period, specifically by 72 percent between 2002 and 2011. The number of Black high school graduates had also been increasing, though at a more modest rate (Flores & Shepherd 2014). With this new deregulation law, the state also required universities to reserve a small percentage of in-state tuition to use to offset the increased cost of attendance. Furthermore, they had to improve their performance on key metrics, one of which was student diversity (Texas Higher Education Coordinating Board 2010).

While those who support deregulation argue that it helps universities gain revenue that is much needed for improving student services, those who oppose the policy believe deregulation will cause tuition costs to rise so much that it will limit access for underserved populations (Flores & Shepherd 2014). Focusing the analysis on four-year research institutions is practically important, argue Flores and Shepherd. First, tuition deregulation tends to lead to larger tuition

cost increases at public research institutions than at other colleges and universities (Heller 2013). This focus is also important because underserved students are much more likely to graduate if they attend a more selective institution (Melguizo 2010). Therefore, a deregulation policy that limits access at these selective institutions will force many students to attend less selective colleges where their odds of completion are lower (Flores & Shepherd 2014). Flores and Shepherd's (2014) analysis seeks to understand "the effect of state-legislated tuition deregulation on the college enrollment of populations that are historically underrepresented in higher education" (101). They hypothesize that the deregulation policy will reduce first-time enrollment of underrepresented students.

In this paper, I attempt to replicate Flores and Shepherd's difference-in-differences (DD) analyses of the effect of tuition deregulation on Hispanic and Black student first-time enrollment. The DD analyses compare enrollment at institutions in Texas, the treatment group, with other regions of the country, which act as comparison groups. Though Flores and Shepherd also examine the policy's impact on low-income student enrollment, I choose not to replicate these findings, mainly because Flores and Shepherd's analysis of low-income student enrollment uses total enrollment rather than first-time enrollment. I believe this distinction dilutes findings due to the nature of low-income students already enrolled being more likely to continue at the same institution, even with higher tuition costs. I also extend the original paper with a regression discontinuity (RD) analysis. I conduct this RD analysis because I do not believe pre-policy trends in Hispanic and Black enrollment are parallel between the treatment and comparison groups.

My replication does not match the exact results of the DD analyses. Specifically, Flores and Shepherd find a negative effect of the policy on Hispanic student enrollment and a

marginally significant positive effect on Black student enrollment. I find no effect on Hispanic student enrollment and a marginally significant positive effect on Black student enrollment, though of a different magnitude than Flores and Shepherd's finding. In my RD extension, I find a significant positive effect of the policy on tuition and fees and no significant effect of the policy on Hispanic or Black enrollment.

II. Data

a. Data sources

Flores and Shepherd use panel data from three sources from 2001 to 2006. They use the Integrated Postsecondary Education Data System (IPEDS) to gather data on dependent variables and institutional controls. IPEDS data is self-reported by institutions through a number of surveys administered by the National Center for Education Statistics. They use the Federal Student Aid (FSA) data center, provided by the U.S. Department of Education, to gather data on an additional dependent variable. They use the American Community Survey (ACS), a yearly survey administered by the U.S. Census Bureau, to gather data on state controls.

I collect data from IPEDS, though I fail to collect two control variables: state appropriations per full-time equivalent (FTE) student and average tuition discount per FTE student. The state appropriations variable is only available starting in 2006, so I cannot access this variable for each year of the study. The tuition discount variable is no longer available within the IPEDS institutional expenses and revenues survey. I am unable to find any other data source with this information.

For state controls, ACS data is only available beginning in 2005. Thus, I must use other data sources for the state controls. For unemployment rates, I use Bureau of Labor Statistics (BLS) data on monthly unemployment by state, using the December rate in each year. For per

capita income, I use Bureau of Economic Analysis (BEA) data on total income in combination with U.S. Census Bureau data on state population estimates. For state populations disaggregated by age and race/ethnicity, I use U.S. Census Bureau intercensal estimates.

b. Sample restrictions

To restrict my sample, I follow the considerations outlined by Flores and Shepherd. That is, I first limit my sample to public institutions with high or very high research activity. I also drop institutions in Virginia from the sample because Virginia restructured its tuition-setting system in 2005, one of the years in the post-treatment analysis, so its universities are not viable comparisons (Flores & Shepherd 2014). I exclude eight additional universities from the sample whose admissions rates are over 90 percent for all six years in the sample. My sample now consists of seven treatment institutions (institutions in Texas), matching the original treatment sample, and 117 comparison institutions outside of Texas, four more than in the original comparison sample, for a total of 124 institutions. Flores and Shepherd have only 120, but I am not able to figure out which four additional institutions they exclude. Since my sample size is slightly different, I do not expect to exactly match Flores and Shepherd's results.

c. Descriptive statistics

Table 1 presents means and standard deviations of each variable in the analysis. Through the descriptive statistics replication, I am able to find that the disparity in sample size comes partly from the Southeast region, and not from Texas, California, Florida, or the Southwest, as I am able to match many of the descriptive statistics for these regions. I was most successful in matching means and standard deviations for the IPEDS institutional data, apart from the tuition and fees variable, in which my replication has consistently slightly lower means than in the original paper. For the state controls, I am not able to exactly replicate the original means since I

use different data sources. However, my replication shows very similar time trends of these means, so I am confident the state controls have similar variation in my replication to that of the original data, even though they are not the same means. Therefore, they should not significantly impact the results of my replication analysis.

Based on only the summary statistics of the dependent variables, I am skeptical that there is a negative effect of the deregulation policy on Black and Hispanic student enrollment, as Flores and Shepherd hypothesize. Specifically, both means increase in the period after the policy was implemented, which is not surprising given the rise in population of these two groups in Texas. However, when looking also at the Southwest and Southeast regions, which Flores and Shepherd use as part of their main comparison groups, the time trends are similar.

III. Research Design

a. Difference-in-differences replication

Flores and Shepherd attempt to isolate the causal effect of Texas' tuition deregulation policy on first-time Hispanic and Black student enrollment at public research institutions. They employ a DD research design. Using a quasi-experimental design is necessary to isolate causality because changes in unobserved variables in Texas during the same year as the policy implementation would cause an endogeneity problem in a simple pre/post analysis. A DD design will control for these changes, as long as they are also present in the comparison regions.

Flores and Shepherd's treatment group is the set of public research institutions in Texas, with the treatment beginning in 2004. The analysis begins in 2001, giving them three years of data prior to the treatment and three years after. They use public research institutions in other parts of the country for comparison groups. For different specifications, they use different

regions of the country in order to obtain a viable comparison group based on population similarities.

A credible DD design relies on the assumption that the treatment and comparison groups have parallel trends in the dependent variable before the policy was implemented. Flores and Shepherd do not provide a visual showing the pre-policy trends in enrollment, which makes me skeptical that these trends are actually parallel. Therefore, I visualize the pre-policy trends in Figures 1 and 2. Figure 1 shows that the trends in Hispanic enrollment between Texas and their main comparison group are not parallel. In Figure 2, Texas and the main comparison group have trends that are also not parallel. Therefore, I do not expect these analyses to provide credible estimates.

Flores and Shepherd's dependent variables for the analyses I replicate are first-time Hispanic student enrollment and first-time Black student enrollment. They estimate the following model, which I use for my replication:

$$ENROLL = ß_0 + ß_1 TEXAS + ß_2 AFTER + ß_3 (TEXAS \times AFTER) + ß_4 X + \delta + \varepsilon$$

where *ENROLL* is the dependent variable, *TEXAS* is an indicator equal to one if an institution is in Texas, *AFTER* is an indicator equal to one for observations after the deregulation policy was implemented, *X* is a variety of institutional and state controls, and δ is year fixed effects. $ß_3$ is the difference-in-differences estimator, or the estimated policy effect, as it is the estimate for the interaction term that captures institutions both in Texas and after the policy's implementation. Institutional controls include first-time undergraduate enrollment, admissions rate, and tuition and fees. State controls include unemployment rate, per capita income, and subsets of the young population disaggregated by race. As they did, I also cluster standard errors at the institution level in order to account for the fact that each institution has multiple observations across years.

Flores and Shepherd (2014) state that they "include a categorical variable" for states rather than using fixed effects (107). Unsure of how they do this, after including state random effects into my replication to test whether this is their method, I contact Shepherd for clarification. According to Shepherd, the categorical variable was not something included in their model, but rather was a way to separate comparison groups into states or regions rather than using the nationwide sample as the only comparison group.

An additional struggle to replicate the design is that despite using year fixed effects, Flores and Shepherd obtain parameter estimates for the *AFTER* variable, which should be perfectly collinear with the fixed effects. I also notice that Flores and Shepherd's results tables contain a label for "groups." This "groups" label is obtained in Stata when using random effects, but the number of groups is not consistent with a random effect for states. Rather, the number is consistent with a random effect for institutions. Therefore, I include random effects for institutions in my model. When I do so, Stata does not omit the *AFTER* variable, but rather omits an additional indicator that is part of the year fixed effects. That is, rather than only omitting the year 2001 as the reference group, it also omits 2006, thereby giving an estimate for *AFTER*. I conclude that these random effects, causing the omission of the 2006 indicator, are the reason that Flores and Shepherd obtain their estimates on the *AFTER* variable. The random effects help to isolate a causal effect by taking into account the unobserved differences between institutions, which could cause omitted variable bias without the random effects.

b. Regression discontinuity extension

In my RD extension, I use the same years in my sample, 2001 through 2006, but I only use institutions in Texas, as those are the treated institutions during the second half of the time period. I conduct this analysis because the lack of parallel pre-policy trends in the dependent

variables causes me to believe a DD design is not robust for this analysis because the comparison groups are not viable. By using only institutions in Texas in my analysis, I eliminate the need to find a comparison group from other regions. However, one problem with this design is that since my sample is small, I do not have the power to detect a policy effect with a high level of confidence. Regardless, I conduct the RD analysis in order to see if my results differ substantially from the DD analysis.

In my RD analysis I use three dependent variables: tuition and fees, first-time Hispanic student enrollment, and first-time black student enrollment. I estimate a model with tuition and fees as the dependent variable in order to confirm that the deregulation policy had a causal effect on tuition costs, and these rising costs were not just part of a larger trend in Texas. I estimate the following model:

$$Y = ß_0 + ß_1 AFTER + ß_2(Year - 2004) + ß_3[AFTER \times (Year - 2004)] + ß_4 X + \varepsilon$$

where *Y* is the dependent variable, *AFTER* is an indicator equal to one for observations after the deregulation policy was implemented, *(Year - 2004)* controls for the time trend, *[AFTER x (Year – 2004)]* is an interaction term that allows the time trend to vary after the policy is implemented, and *X* is two institutional controls: first-time undergraduate enrollment and admissions rate. Like Flores and Shepherd did in their DD analysis, I cluster standard errors at the institution level to account for serial correlation across years within institutions.

I decide not to include tuition and fees as controls in the models estimating enrollment, as Flores and Shepherd did in their DD design, because I believe tuition costs are a major mechanism through which the policy impacts minority enrollment. Therefore, controlling for tuition would negate any effect the policy has through its impact on tuition costs, which would bias the estimated effect. Including tuition costs in the DD design is, I believe, a major weakness

of Flores and Shepherd's analysis. While some aspects of tuition costs are unrelated to the deregulation policy and should thus be controlled for, these aspects can be controlled for in other ways. For example, the main way they control for aspects of tuition unrelated to the policy is through state appropriations, which is likely to be the most important factor that will impact tuition differently at different institutions, regardless of a deregulation policy.

Figures 3, 4, and 5 present the discontinuities visually for each of the three dependent variables. Examining these figures, it appears that tuition and fees and black student enrollment are discontinuous at the cutoff with a positive jump in 2004. It is less clear that Hispanic student enrollment is discontinuous at the cutoff. A discontinuity may exist, but the trend may simply be continuous and exponential. Furthermore, RD relies on the assumption that units cannot self-select on which side of the cutoff they are. In this case, the law was implemented in 2004 and institutional governing boards then had tuition-setting authority. No institutions had this authority before 2004 and all had the authority after 2004. Thus, there is no self-selection.

After conducting my main RD analysis, I conduct two falsification tests. In these tests, I use the same models with one exception: I test for discontinuities in 2003 and 2005. If I find a discontinuity in either of these years, it would cast doubt on the validity of any causal effect I find in the main analysis.

IV. Results

a. Difference-in-differences replication

Table 2 presents the results of the DD analysis of Hispanic student enrollment, with both the original and replicated estimates. In the specification using their main comparison group, the Southwest including California and Nevada, Flores and Shepherd find that the tuition deregulation policy caused a statistically significant decrease in first-time Hispanic enrollment of

318 students, all else equal. With the descriptive statistics showing average first-time Hispanic enrollment at 517 in Texas from 2001 to 2003 and 609 from 2004 to 2006, this 318 student decrease is a massive change in enrollment. In my replication, which uses the same sample size in this specification, I estimate an increase of 21 students, all else equal, but the effect is not statistically significant. There are a number of possible reasons for this discrepancy. First, my model lacks two variables from the original analysis due to data availability. Furthermore, since I use a different data source for state controls, and I know from the summary statistics that the observations are not exactly the same in the two data sources, the differences in these observations would likely cause me to obtain a different estimate. However, as I describe above, the time trends of the state controls in my replication lead me to believe that my estimate should not differ by such a large magnitude. Therefore, the discrepancy must be due to, at least in part, a methodological difference that was not described in the original paper. I conducted the analysis in a number of different ways in an attempt to find out what this methodological difference could be. For example, I tried including state random effects and omitting different additional years in the year fixed effects, but these efforts did not substantially change my estimates.

Table 3 shows the DD results for the effect of tuition deregulation on first-time Black student enrollment. While I do not exactly match the original estimates in this analysis, my results are more consistent with Flores and Shepherd's than in the analysis of Hispanic enrollment. Using their main comparison group, the Southeast including Florida, Flores and Shepherd find that all else equal, the tuition deregulation policy causes an increase in first-time Black enrollment of 127 students. The result is marginally significant. In my replication, which uses a slightly larger sample, I estimate the deregulation policy causes a statistically significant increase in Black enrollment of 52 students, all else equal. The same data limitations apply to

this analysis, meaning my obtaining state controls from different data sources and lacking two institutional controls is likely to cause a discrepancy in my results.

Although I take the same steps outlined by Flores and Shepherd in creating my sample of Southeastern institutions, including dropping West Virginia and Virginia from the region, my sample size for this model is slightly larger. This difference in sample could also be a reason for the discrepancy in results. Upon contacting Shepherd about the sample in the original analysis, I learn that Flores and Shepherd took further steps to drop some observations that seemed to be outliers. However, I am not able to learn specifically which observations they excluded or which variables on which they based any outlier decisions. Thus, I am not able to exactly replicate their sample. I also attempted to make some of the same methodological changes as in the analysis of Hispanic enrollment in order to try to match the results more closely, but these attempts were not successful.

After investigating the original data sources further, I find a potential weakness in Flores and Shepherd's analysis that could explain my failure to replicate the analysis. Flores and Shepherd use state controls from the ACS. However, the ACS was not launched until 2005, meaning it could not provide them with data from 2001 to 2004 (U.S. Census Bureau 2019). Based on the descriptive statistics, they do have data on state controls prior to 2004, but they never state how they obtain this data. These further discrepancies between the original data and my data could explain the differences in our results.

b. Regression discontinuity extension

Table 4 presents the results of the three models in my regression discontinuity extension. I find that on average, holding all else equal, the deregulation policy caused a statistically significant increase in tuition and fees of $742. This finding supports the hypothesis that the

deregulation policy did have a positive effect on tuition costs. In my other two models, I find a positive effect on first-time Hispanic enrollment and a negative effect on first-time Black enrollment. However, these two estimates are not statistically significant.

One curious finding from these three analyses is the differences in R-squared values. The R-squared values obtained from the models estimating the effect on tuition and fees and Hispanic enrollment are much greater than the one obtained from the model estimating the effect on Black enrollment. This disparity means that the independent variables explain much more of the variation in tuition costs and Hispanic enrollment than Black enrollment, suggesting other unobserved factors may be more likely to explain Black enrollment. This lower R-squared causes me to believe Black enrollment is more difficult to model than Hispanic enrollment, which could explain why my RD analysis provides a negative estimate (though not statistically significant), but my DD analysis provides a positive estimate.

To test the robustness of the results of my RD analysis, I run two falsification tests. In these tests, I conduct the RD analyses as if the deregulation policy was implemented in 2003 or 2005 rather than 2004. Since the policy was not implemented in those years, I should find no significant estimates. The results of these tests are presented in Table 5. For the model estimating tuition and fees, my falsification tests find no significant effect, which makes me confident that the deregulation policy has a positive causal impact on tuition and fees. In my test for 2005, I do find a marginally significant positive impact on Hispanic enrollment. The result of this test is somewhat similar to the result of my initial RD analysis for Hispanic enrollment, though the effect is found with a larger magnitude and higher level of confidence. This falsification test causes me to believe Hispanic enrollment may be changing in Texas due to reasons other than the deregulation policy.

V. Discussion

The requirements embedded in the tuition deregulation law could plausibly have caused these public research institutions to attempt to increase, rather than decrease, their minority enrollment numbers. As a reminder, two of these requirements are (1) reserving a percentage of in-state tuition to help offset the increased costs and (2) improving performance on student diversity metrics. These two requirements could plausibly work together to increase enrollment of underrepresented students. Reserving revenue to use as additional institutional grant aid could help institutions enroll more Hispanic and Black students, as underrepresented minority students are more likely to be lower-income, meaning they can benefit from the additional grant aid and their net price could be the same or less than it would have been without the deregulation policy.

Hispanic enrollment and population trends in Texas support the hypothesis that these requirements in the law help to increase the enrollment of minority students. Figure 6 presents a descriptive visual of a comparison between the trend in first-time Hispanic enrollment in Texas and the Hispanic population aged 15-19 in Texas. The two measures increase at a similar rate between 2001 and 2003, but Hispanic enrollment increases at a much faster rate than the young Hispanic population between 2004 and 2006. Of course, this visual is not a causal analysis, so we cannot conclude the increase is caused by the deregulation policy. However, it certainly provides a piece of descriptive evidence in opposition to Flores and Shepherd's hypothesis and finding, which is that the tuition deregulation policy reduced college access for Hispanic students at public research institutions. This figure also shows a trend consistent with my RD analysis and falsification tests. That is, I find an increase in Hispanic enrollment in 2004 that is not statistically significant, then a larger increase in 2005 that is marginally significant.

One possible reason that we see a larger increase in Hispanic enrollment in 2005 than 2004 is there may be a lagged positive effect of the deregulation policy on Hispanic enrollment. If there is a positive effect due to the requirements embedded in the policy, this lag could be due to the fact that it takes time for institutions to adjust their recruitment strategies to appeal to Hispanic students, something they may have wanted to do in order to help comply with the new requirements. These adjustments in recruiting could cause much of the policy effect to lag behind implementation, which would explain the modest uptick in Hispanic enrollment we see in 2004, before a sharper increase in 2005.

Figure 7 presents a visual for first-time Black enrollment trends in Texas relative to the trends in the young Black population. This figure shows a very similar trend to Figure 6 between 2001 and 2005, with the only major difference showing up in 2006. Although these trends are similar for most years in the sample, Flores and Shepherd find a different effect of the policy on Black students relative to Hispanic students. In my RD analysis, though my estimates are not statistically significant, I also find different effects. Therefore, one option for future research is to explore first-time Black enrollment trends, how they respond to tuition policy and cost changes, and why they may respond differently than enrollment trends of other underrepresented groups.

Overall, this replication and extension casts doubt on the results found in Flores and Shepherd's analysis. While Flores and Shepherd find a negative impact of the deregulation policy on Hispanic enrollment and a marginally significant positive impact on Black enrollment, my analysis finds that there could be a positive impact on Hispanic enrollment, and there could be lagged effect of the policy. Future research is needed to investigate not only Black enrollment trends, but also potential lagged effects of state tuition deregulation policies. Research in these areas could lead to a better understanding of how tuition deregulation impacts college access.

Appendix: Tables and Figures

Table 1. Descriptive Statistics: Averages by Region and Time

	Original						Replication					
	Texas		Nationwide		Florida		Texas		Nationwide		Florida	
	2001-03	2004-06	2001-03	2004-06	2001-03	2004-06	2001-03	2004-06	2001-03	2004-06	2001-03	2004-06
First-time Black undergraduate enrollment	267.05 (174.53)	311.86 (195.60)	245.23 (214.60)	260.57 (228.91)	527.22 (135.35)	548.89 (141.30)	267.05 (174.53)	311.86 (195.60)	256.44 (259.48)	273.09 (279.71)	527.22 (135.35)	548.89 (141.30)
First-time Hispanic undergraduate enrollment	516.76 (319.08)	609.05 (388.65)	220.59 (279.57)	263.15 (357.01)	784.72 (442.39)	1022.94 (761.52)	516.76 (319.08)	609.05 (388.65)	214.96 (276.91)	256.41 (353.27)	784.72 (442.39)	1022.94 (761.52)
First-time undergraduate enrollment (in thousands)	4.01 (2.23)	4.05 (2.24)	3.29 (1.62)	3.43 (1.71)	4.64 (1.65)	5.06 (1.62)	4.01 (2.23)	4.05 (2.24)	3.25 (1.62)	3.39 (1.71)	4.64 (1.65)	5.06 (1.62)
Admissions rate	69.27 (10.48)	66.31 (10.64)	70.98 (15.34)	69.53 (15.54)	61.30 (7.62)	53.58 (9.96)	69.27 (10.48)	66.31 (10.64)	70.95 (15.33)	69.73 (15.48)	61.30 (7.62)	53.58 (9.96)
Tuition and fees (in thousands)	4.11 (0.66)	5.83 (0.87)	5.11 (1.76)	6.28 (2.47)	2.98 (0.21)	3.24 (0.28)	3.68 (0.62)	5.65 (0.92)	4.61 (1.72)	6.07 (2.40)	2.66 (0.21)	3.14 (0.29)
Unemployment	7.20 (0.87)	7.57 (0.46)	7.07 (1.26)	6.83 (1.19)	6.87 (0.41)	6.30 (0.67)	6.30 (0.25)	5.13 (0.59)	5.70 (0.87)	4.95 (1.01)	5.40 (0.50)	3.70 (0.51)
Per capita income (in thousands)	23.06 (0.22)	22.86 (0.28)	24.52 (3.22)	24.67 (3.14)	24.68 (0.15)	25.27 (0.12)	29.14 (0.25)	33.24 (1.77)	29.77 (3.97)	33.51 (4.73)	29.73 (0.42)	34.51 (1.70)
Population aged 15-19 (in thousands)	1554.40 (3.57)	1653.41 (84.67)	637.68 (628.93)	684.39 (680.07)	1010.69 (19.38)	1120.09 (43.91)	1683.41 (6.91)	1728.1 (21.95)	699.23 (670.99)	725.14 (703.38)	1071.82 (18.09)	1159.85 (27.53)
Population aged 20-24 (in thousands)	1525.87 (40.73)	1648.88 (69.59)	630.38 (621.21)	673.58 (665.91)	949.41 (38.30)	1094.91 (43.77)	1690.61 (44.44)	1782.63 (12.19)	695.78 (680.24)	722.00 (701.75)	1055.72 (36.98)	1166.27 (20.84)
Black population aged 15-19 (in thousands)	190.26 (3.91)	216.41 (14.73)	91.82 (72.66)	101.41 (81.22)	217.18 (8.23)	236.75 (12.48)	235.80 (3.46)	254.66 (9.20)	110.29 (94.46)	118.73 (101.70)	249.85 (3.95)	272.45 (7.85)

Black population aged 20-24 (in thousands)	163.45 (6.58)	193.28 (18.29)	81.86 (63.49)	89.93 (72.52)	185.54 (6.43)	216.27 (14.82)	228.38 (7.38)	248.48 (7.21)	104.59 (89.81)	109.76 (94.10)	233.52 (10.18)	263.28 (5.85)
Hispanic population 15-19 (thousands)	625.30 (7.87)	657.56 (31.67)	150.99 (280.31)	165.52 (305.19)	205.32 (5.92)	237.47 (14.64)	647.00 (4.37)	677.26 (14.59)	157.81 (288.17)	169.63 (309.41)	217.93 (5.84)	246.98 (10.46)
Hispanic population 20-24 (thousands)	653.66 (25.86)	690.66 (12.83)	165.63 (298.47)	172.86 (306.39)	222.64 (9.05)	257.12 (12.84)	678.72 (19.46)	713.69 (1.74)	170.97 (303.20)	176.93 (313.48)	239.52 (9.04)	268.86 (7.36)
Number of institutions	7		120		6		7		124		6	

Notes: Standard deviations appear in parentheses. Nationwide includes all states except Virginia.

Table 1 Continued

	Original						Replication					
	California		Southeast		Southwest		California		Southeast		Southwest	
	2001-03	2004-06	2001-03	2004-06	2001-03	2004-06	2001-03	2004-06	2001-03	2004-06	2001-03	2004-06
First-time Black undergraduate enrollment	124.89 (56.67)	122.26 (56.17)	391.31 (216.63)	408.26 (222.83)	127.78 (68.75)	152.04 (90.25)	124.89 (56.67)	122.26 (56.17)	454.13 (365.32)	479.79 (394.18)	127.78 (68.75)	152.04 (90.25)
First-time Hispanic undergraduate enrollment	551.04 (155.71)	624.37 (200.61)	48.96 (38.54)	67.50 (44.53)	473.00 (365.67)	561.96 (417.47)	551.04 (155.71)	624.37 (200.61)	47.07 (38.61)	64.05 (44.82)	473.00 (365.67)	561.96 (417.47)
First-time undergraduate enrollment (in thousands)	3.92 (0.42)	4.03 (0.61)	2.72 (1.16)	2.90 (1.28)	3.46 (1.60)	3.65 (1.79)	3.92 (0.42)	4.03 (0.61)	2.70 (1.14)	2.88 (1.26)	3.46 (1.60)	3.65 (1.79)
Admissions rate	54.43 (20.34)	50.57 (18.52)	71.60 (17.87)	70.31 (14.39)	83.61 (7.22)	80.89 (12.51)	54.43 (20.34)	50.57 (18.52)	70.60 (17.62)	70.30 (14.31)	83.61 (7.22)	80.89 (12.51)
Tuition and fees (in thousands)	5.16 (1.31)	6.66 (1.31)	4.17 (0.59)	4.86 (0.84)	3.30 (0.44)	4.24 (0.49)	4.63 (1.22)	6.44 (1.25)	3.98 (1.75)	4.77 (1.18)	2.96 (0.43)	4.12 (0.52)
Unemployment	7.83 (0.69)	7.13 (0.42)	7.81 (1.05)	7.64 (0.84)	7.20 (0.93)	6.34 (0.80)	6.60 (0.17)	5.27 (0.39)	5.91 (0.60)	5.46 (1.02)	5.44 (0.49)	4.35 (0.44)
Per capita income (in thousands)	26.90 (0.11)	27.25 (0.30)	21.27 (1.90)	21.45 (1.81)	22.47 (1.84)	23.17 (1.95)	33.17 (0.36)	37.68 (1.77)	25.78 (2.03)	28.81 (2.48)	27.04 (2.37)	31.38 (3.47)
Population aged 15-19 (in thousands)	2384.11 (11.92)	2566.74 (129.20)	330.45 (129.10)	354.57 (155.89)	234.75 (99.05)	253.82 (114.11)	2571.28 (20.45)	2695.34 (43.63)	378.99 (146.16)	391.50 (165.11)	253.84 (107.50)	268.25 (118.47)
Population aged 20-24 (in thousands)	2364.06 (61.18)	2514.53 (132.48)	340.50 (137.90)	354.50 (151.09)	234.00 (109.14)	258.03 (118.55)	2619.00 (42.28)	2687.03 (4.69)	391.36 (155.93)	400.07 (165.03)	261.45 (118.58)	276.73 (120.99)
Black population aged 15-19 (in thousands)	168.20 (6.83)	188.53 (12.46)	100.62 (48.81)	108.63 (57.85)	11.35 (6.22)	13.79 (6.14)	236.04 (4.86)	259.34 (6.73)	118.69 (53.12)	124.57 (59.71)	17.64 (8.13)	20.57 (8.46)
Black population aged 20-24 (in thousands)	136.11 (5.10)	156.49 (12.88)	93.79 (44.39)	100.30 (52.39)	10.62 (6.59)	12.38 (5.72)	217.72 (3.68)	227.79 (2.59)	114.50 (51.77)	119.48 (55.59)	17.69 (8.21)	20.19 (8.54)

Hispanic population 15-19 (thousands)	986.68 (10.66)	1084.22 (54.14)	11.71 (11.12)	15.05 (14.87)	68.26 (44.82)	77.17 (51.03)	1033.61 (12.76)	1116.95 (33.87)	16.16 (14.43)	17.87 (15.78)	72.58 (45.44)	81.07 (51.85)
Hispanic population 20-24 (thousands)	1059.79 (28.03)	1080.91 (35.24)	21.15 (23.10)	22.61 (21.45)	72.78 (48.11)	78.78 (50.42)	1091.97 (18.63)	1123.57 (0.73)	25.27 (24.31)	24.40 (22.31)	78.11 (50.00)	84.41 (53.16)
Number of institutions	9		24		9		9		25		9	

Notes: Standard deviations appear in parentheses. Southeast includes Alabama, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and West Virginia. Southwest includes Arizona, Nevada, New Mexico, and Oklahoma.

Figure 1. First-Time Hispanic Enrollment Trends

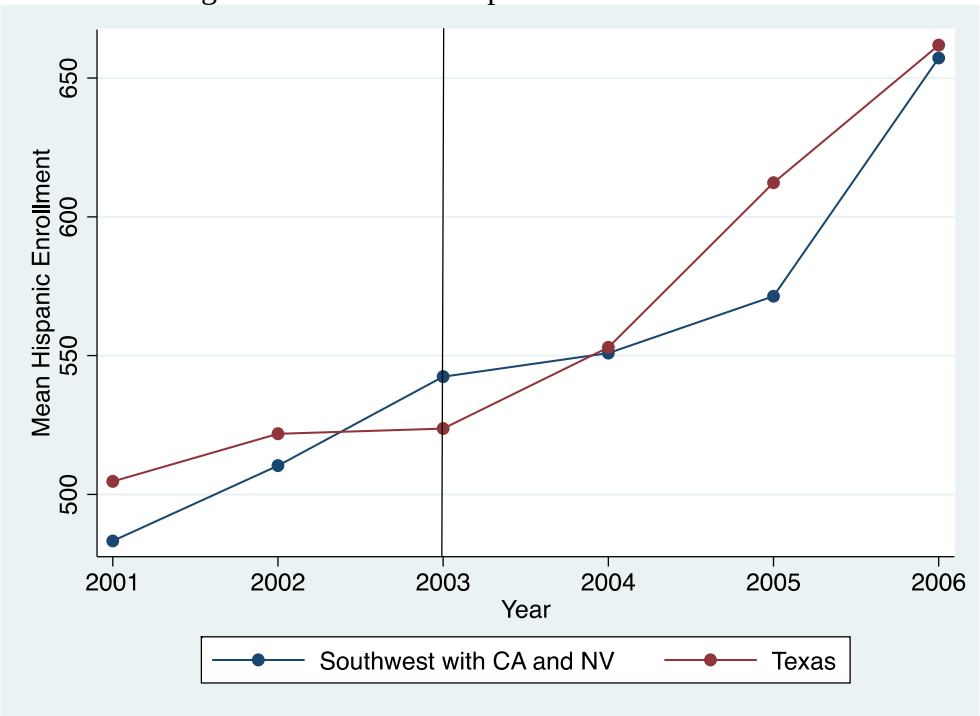


Figure 2. First-Time Black Enrollment Trends

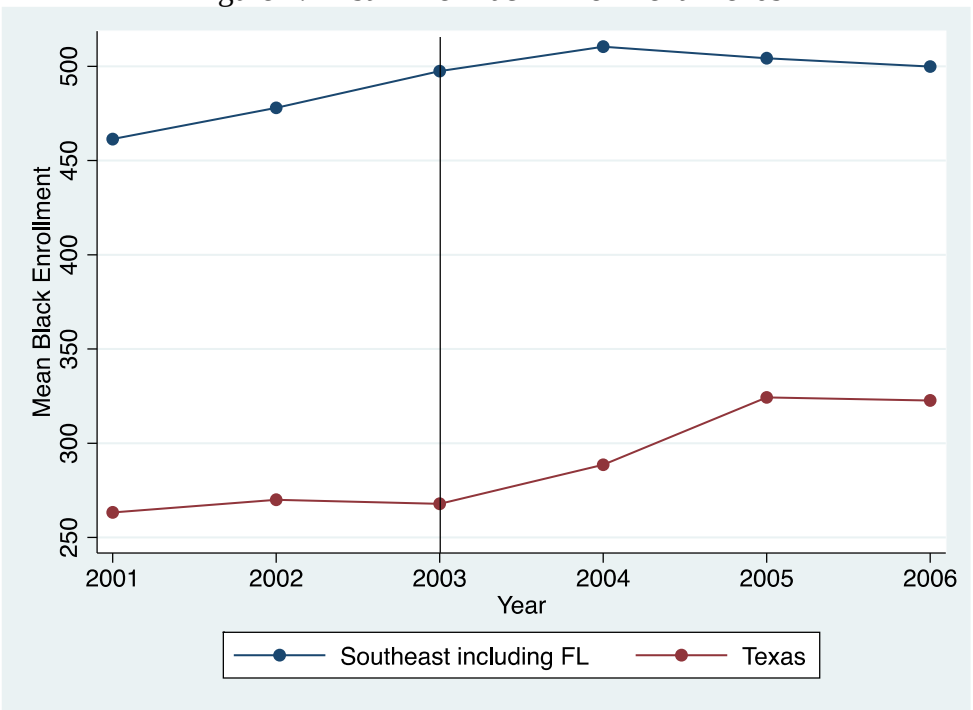


Table 2. Effect of Tuition Deregulation on First-Time Hispanic Enrollment

	Original			Replication		
	Nationwide	Southwest with CA & NV	Southwest with CA, NV, & FL	Nationwide	Southwest with CA & NV	Southwest with CA, NV, & FL
Texas x After deregulation (DD estimator)	83.29	-318.17**	-235.53*	45.84	21.16	-4.34
	(39.74)	(110.49)	(102.03)	(40.16)	(37.90)	(41.96)
After deregulation	106.56**	366.48**	323.54**	1.65	302.51*	148.05
	(28.30)	(91.38)	(85.46)	(21.68)	(145.00)	(129.74)
Texas	84.97	-14.31	14.37	-22.33	-90.87	-98.78
	(89.49)	(194.35)	(276.84)	(102.41)	(106.74)	(128.89)
Admissions rate	-1.39	3.34	-4.02	0.02	0.75	1.12
	(1.97)	(2.66)	(5.74)	(0.22)	(0.85)	(0.73)
Tuition and fees (in thousands)	-71.40**	-75.36*	-54.55	-4.81	-0.20	-6.97
	(22.17)	(33.64)	(38.59)	(4.62)	(17.57)	(16.44)
First-time undergraduate enrollment	0.07**	0.13**	0.11**	0.09**	0.14**	0.21**
	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)	(0.06)
Hispanic population 15-19 (thousands)	0.37	0.34	-0.27	0.43	0.30	-0.64
	(0.81)	(1.06)	(1.22)	(0.46)	(0.50)	(0.76)
Hispanic population 20-24 (thousands)	-0.01	-0.14	0.10	0.10	-0.08	0.75
	(0.76)	(1.05)	(1.07)	(0.47)	(0.46)	(0.70)
Unemployment	-0.12	312.40**	244.15*	-18.48	5.29	-30.94
	(10.48)	(93.45)	(95.55)	(9.74)	(18.24)	(30.78)
Per capita income (in thousands)	13.02*	-33.94	-6.22	3.71	-30.74	-9.60
	(5.22)	(37.59)	(45.16)	(3.43)	(19.39)	(18.22)
State controls	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Constant	45.01	-1372.92	-442.77	-166.47	707.28	74.88
	(288.78)	(873.31)	(1150.01)	(126.71)	(587.24)	(481.95)
R-squared	0.47	0.55	0.29	0.40	0.44	0.19
Observations	674	150	186	744	150	186
Groups	115	25	31	124	25	31

Notes: Standard errors appear in parentheses and are clustered at the institution level. *p < 0.05, **p < 0.01

Table 3. Effect of Tuition Deregulation on First-Time Black Enrollment

	Original			Replication		
	Nationwide	Southeast excluding FL	Southeast including FL	Nationwide	Southeast excluding FL	Southeast including FL
Texas x After deregulation (DD estimator)	-36.59 (24.68)	101.38 (58.28)	127.16 (67.39)	36.80* (18.00)	40.67 (25.00)	52.36* (26.75)
After deregulation	-10.19 (19.12)	135.91 (67.03)	127.16 (67.39)	25.25 (34.59)	-37.49 (65.35)	-5.58 (59.83)
Texas	-186.10* (87.26)	-262.24 (145.27)	-283.13* (129.76)	-149.70 (101.39)	-451.27* (219.26)	-313.47* (156.40)
Admissions rate	-1.18 (1.76)	-3.91 (2.95)	-3.31 (2.75)	0.03 (0.36)	-0.55 (0.69)	-0.12 (0.65)
Tuition and fees (in thousands)	-9.78 (14.26)	-65.09 (53.56)	-82.69 (43.90)	-3.62 (2.90)	-7.52 (9.51)	-6.78 (9.30)
First-time undergraduate enrollment	0.03** (0.01)	0.01 (0.02)	0.03 (0.02)	0.07** (0.02)	0.12** (0.04)	0.12** (0.03)
Black population 15-19 (in thousands)	-1.92 (1.77)	-1.81 (1.42)	-0.94 (1.52)	0.95 (0.96)	4.96 (2.95)	4.43 (2.66)
Black population 20-24 (in thousands)	3.39 (2.00)	1.56 (1.50)	0.66 (1.81)	-0.25 (1.06)	-4.63 (3.04)	-4.80 (2.89)
Unemployment	41.59* (19.76)	-1.41 (36.06)	7.24 (30.26)	5.42 (7.41)	8.24 (17.57)	5.50 (15.26)
Per capita income (in thousands)	-10.81 (7.22)	-1.67 (23.12)	-1.33 (22.42)	-3.04 (4.25)	10.29 (9.95)	5.87 (9.62)
State controls	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Constant	236.75 (313.83)	1107.68 (679.31)	979.91 (682.73)	18.87 (154.80)	-170.08 (406.70)	-35.81 (377.40)
R-squared	0.34	0.34	0.35	0.11	0.00	0.01
Observations	637	176	212	744	186	222
Groups	111	30	36	124	31	37

Notes: Standard errors appear in parentheses and are clustered at the institution level. Southeast region does not include Virginia or West Virginia. *p < 0.05, **p < 0.01

Figure 3. Mean Tuition and Fees in Texas, by Year

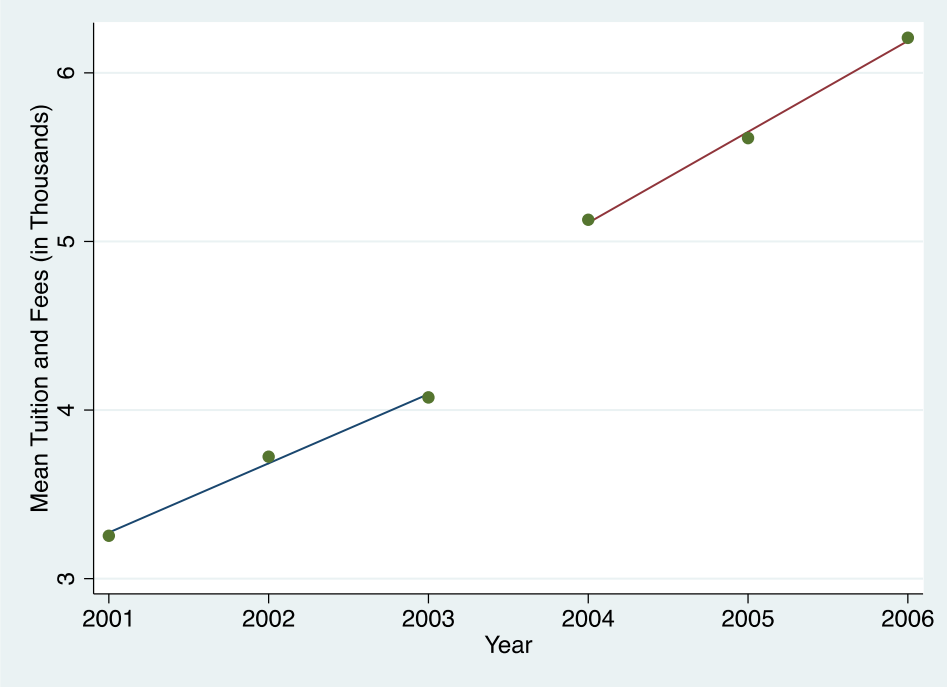


Figure 4. Mean First-Time Hispanic Enrollment in Texas, by Year

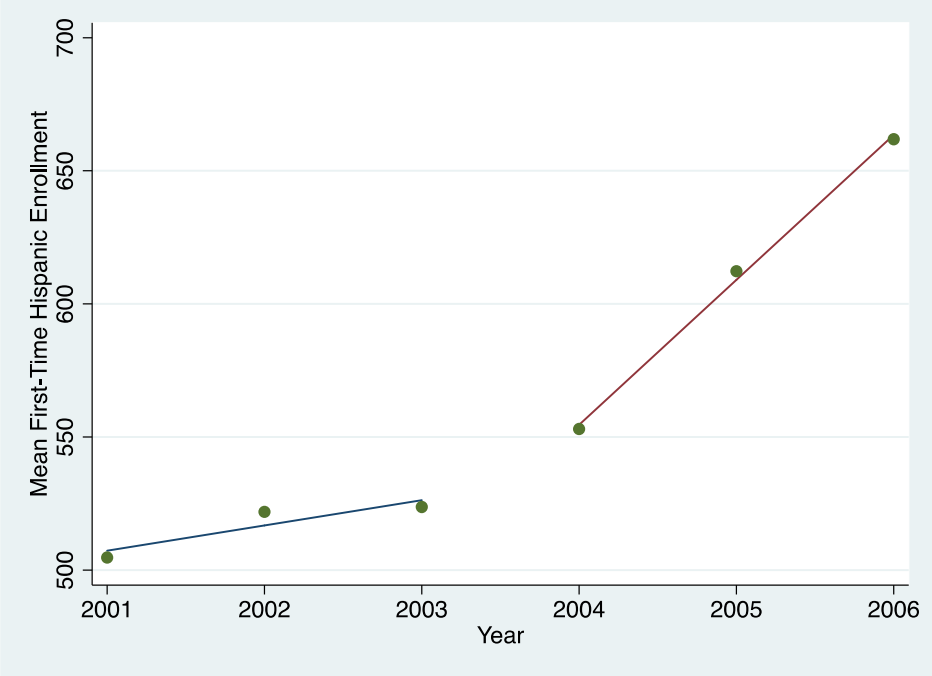


Figure 5. Mean First-Time Black Enrollment in Texas, by Year

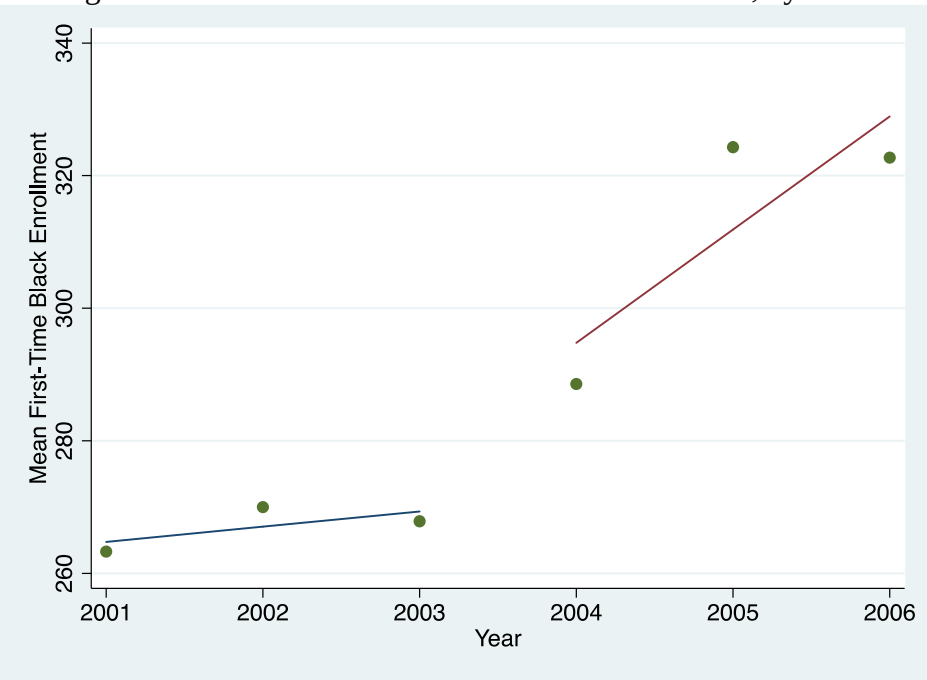


Table 4. Regression Discontinuity Results

	Dependent Variables		
	Tuition and fees (in thousands)	First-time Hispanic enrollment	First-time Black enrollment
After deregulation (RD estimator)	0.742**	23.748	-13.757
	(0.202)	(42.406)	(23.146)
(Year – 2004)	0.318*	13.984	30.320
	(0.104)	(12.508)	(15.493)
After deregulation x (Year – 2004)	0.169	23.925	-4.592
	(0.251)	(10.412)	(12.580)
First-time undergraduate enrollment	0.000	0.141**	0.015
	(0.000)	(0.024)	(0.021)
Admissions rate	-0.032*	1.053	9.612
	(0.010)	(6.231)	(5.431)
R-squared	0.87	0.79	0.31
Observations	42	42	42

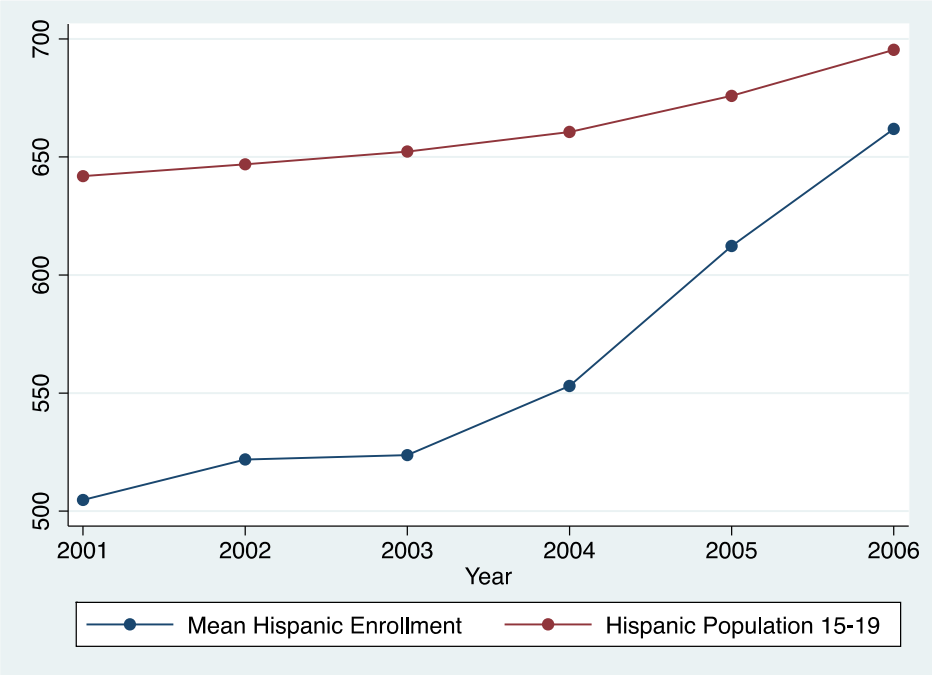
Notes: Standard errors appear in parentheses and are clustered at the institution level. *p < 0.05, **p < 0.01

Table 5. RD Falsification Tests

	Dependent Variables		
	Tuition and fees (in thousands)	First-time Hispanic enrollment	First-time Black enrollment
RD estimator: 2003 test	0.134	31.228	-14.547
	(0.161)	(36.080)	(46.546)
RD estimator: 2005 test	0.180	42.359*	-0.587
	(0.365)	(20.779)	(20.579)
Observations	42	42	42

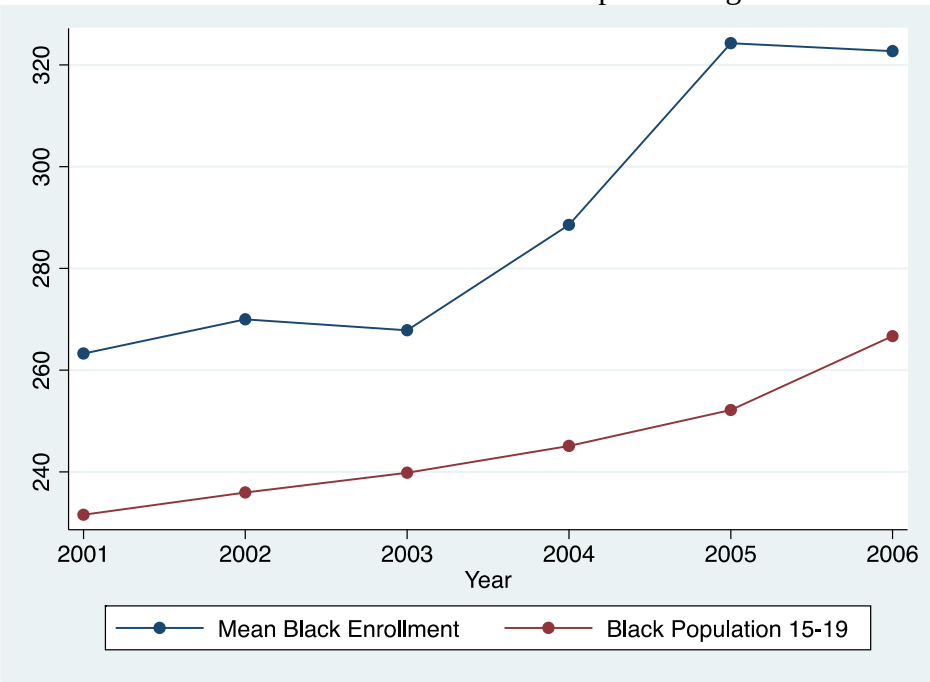
Notes: Standard errors appear in parentheses and are clustered at the institution level. *p < 0.10

Figure 6. Mean First-Time Hispanic Enrollment and Hispanic Population aged 15-19 in Texas, by Year



Note: Population measured in thousands.

Figure 7. Mean First-Time Black Enrollment and Black Population aged 15-19 in Texas, by Year



Note: Population measured in thousands.

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