Appendix A: *Ban States, Policy Names, Ratification Method, and Dates*

Below, Table A lists the ban states, the name of the policy, the method by which the ban came into existence, and the various years for the ban ratification, implementation, and the first cohort impacted by the bans. Ratification marks when the ban became law. Implementation marks when admissions decisions became race neutral. Fall cohort refers to the cohort of entering first year students first affected by changes in admissions policies. The timing span from ratification to fall cohort spans between two to three years and varies by state - suggesting that the "onset of treatment" is complex. Ratification, and fall cohort are included in Appendix C; they are excluded from the main text because all of the specifications are robust to the main findings of this work. These results are shown in Appendix C because Appendix B provides an assessment of the parallel trends assumption. The analysis in Appendix B finds anticipation effects which are further explored by Appendix C when the different specifications for the ban onsets are considered.

Table A Ban Timing: Ratification, Implementation, and First Fall Cohort

State	Affirmative Action Ban Policy	Method	Ratification	Implementation	Fall Cohort
California	Proposition 209	Ballot Proposition	1996	1997	1998
Texas	*Hopwood v. Texas*	Court Case- U.S. Court of Appeals (5th Circuit)	1996	1997	1997
Washington	Initiative 200	Ballot Initiative	1998	1999	1999
Florida	"One Florida Initiative" - E.O. 99-281	Executive Order (Governor)	1999	2000	2001
Michigan	"Michigan Civil Rights Initiative" - Proposal 2	Ballot Initiative	2006	2007	2008
Nebraska	"Affirmative Action Initiative" - Nebraska Measure 424	Ballot Measure	2008	2009	2009
Arizona	Proposition 107	Ballot Proposition	2010	2011	2011
New Hampshire	House Bill 0623	Legislative Vote	2011	2012	2013
Oklahoma	State Question 759	Ballot Question	2012	2013	2013
Idaho	House Bill 440	Legislative Vote	2020	2020	2021
Non-Ban States	*Johnson v Board of Regents of the University of Georgia*	Court Case- U.S. Court of Appeals (11th Circuit)	2001	2002	2003

Notes: R University of California Regents approved an affirmative action ban in 1995 and implemented it in 1998. Abrogated by Grutter v Bollinger in 2003. Although the 5th Circuit includes Louisianna and Missippi, federal consent decreese required these states to address racial segregation. Thus, they are not included as ban states. The years after 2003 are treated as missing in the study for Texas. In 2019 Washington passed Initiative 1000 through legislature, ending the impact of Initiative 200. (Initiative 1998 went into effect on Dec 3, 1998- in this study the implementation is rounded to 1999). 2019 is after the scope of this study (2016). Florida State University's cohort was impacted by the ban in 2000, but the University of Florida's unsuccessful challenge of the ban delayed the impact to its first cohort to 2001. Proposal 2 went into effect on Dec 22, 2006- in this study the implementation is rounded to 2007. Idaho is not included in this study because its ban occurs after the analytic period of this study. Alabama and Florida are under the jurrisdiction of the 11th circuit court, but it is unclear that they they were ever impacted after the Grutter v. Bollinger decision in 2003. The University of Georgia continued to not use race in admissions after the decision.

Appendix B: *Parallel Trend Assumption*

Because the difference-in-differences estimator compares the differences in the dependent variable between the ban and non-ban states over time as specified by the difference-in-differences estimator (i.e., this is β_1 in Equation 1):

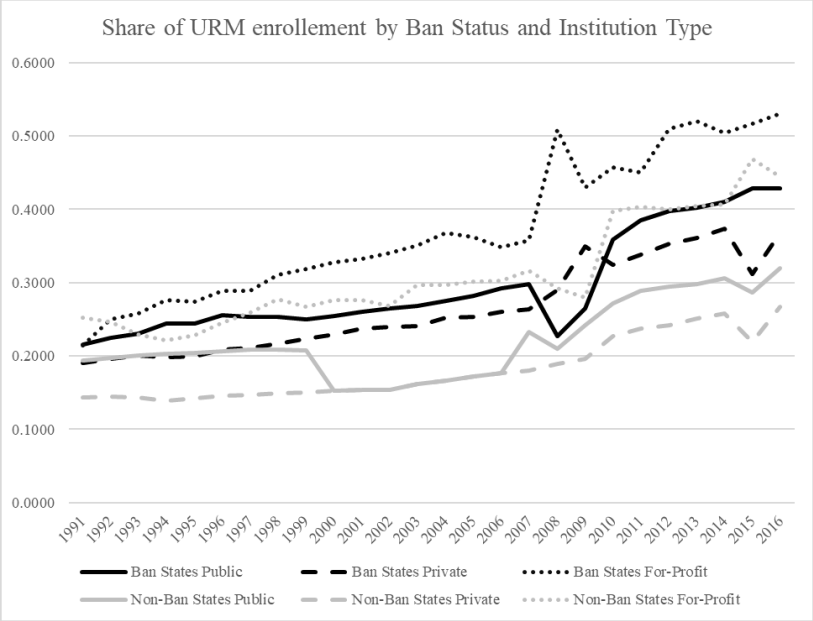
$$\delta = \left(\bar{Y}_{BanAfter} - \bar{Y}_{BanBefore}\right) - \left(\bar{Y}_{NonBanAfter} - \bar{Y}_{NonBanBefore}\right) \quad \text{(B1)}$$

This important assumption of the difference-in-differences approach is known as the parallel trend assumption (herein PTA). In terms of the model specified in this paper it requires that the proportion of URM enrollment trends in each of the target states before the introduction of the affirmative action bans is sufficiently similar to trends in the comparison states over the same period. It is imperative to assess the PTA because violations of it point to a lack of controls in the model, alternative explanations, or that an appropriate counterfactual has not been chosen. This lack of controls may lead to differences in the dependent variable (i.e., URM enrollment) between the ban and non-ban states that are not fully accounted for in the model. A violation would be highly problematic because the difference-in-differences estimator (i.e., the impact of the affirmative action bans) would be biased. Violations are best captured during the pre-ban periods for the non-ban states because there should be no differences between the ban and non-ban states that are not explainable by controls. A violation could occur after the bans as well, but it would have to be strongly argued theoretically that the violation (i.e., the difference between trends) occurring is exogenous to the ban, and not a byproduct of the ban.

Using an event history specification (note: this is not the same as event history analysis) ensures that whatever change in the pattern is observed between the ban and non-ban states after the bans were implemented are attributable to having implemented a ban and not some other factor that is not accounted for in the model (Kaestner et al. 2017, Sommers et al. 2015). While

some empirical work with difference-in-differences with one control and one treated group have often explored violations of the PTA by showing trend graphs of the dependent variables and variables for the treatment and control groups that could potentially threaten the validity of the difference-in-differences design (Card and Krueger 1993), such a straightforward approach is not possible due to the complexity and dynamic implementation of the bans over time. This complexity is made evident by Figure B.1 below. Some studies have attempted to overcome this limitation by showing graphs of each ban state relative to non-ban states however, the limitation with the presentation is that it fails to illustrate the typical pattern.

Figure B.1 URM Enrollment Shares by Ban Status and Institution Type



The event history specification resolves this limitation because it takes into consideration the variability in the timing of the bans and the assignment of the treatment (i.e., which states have the bans). For the event history specification, a placebo ban is created with a proxy treatment variable that starts at various points during the pre-ban period. The years that the ban really went into effect in treated states are treated as missing, so only the pre-trend period is evaluated, and unbiased by years where the ban was actually in place. If the treatment variable is

statistically significant at any point during the pre-treatment period, then it is argued that there is a violation of the PTA. If there are no violations of the PTA, the ban should not be statistically significant during any point in the pre-ban period. If it were statistically significant, it would mean that there is something outside of the controls being used in the model that can explain differences in the control and treatment states during the pre-ban period, the PTA would be violated, and the results for the difference-in-differences model would be biased. In other words, there would be a systematic difference between the treated and untreated states that would bias the estimates produced by the difference-in-differences model.

Results from event history specifications for the probability of URMs not attending college (Table B.1 and Table B.2) and 4-year schools (Table B.3 and Table B.4) are presented. However, no table for 2-year schools is presented because those findings are not statistically significant to begin with (they comprise the middle of the avalanche). Table B.1 presents an event history model without person-level covariates. Here the interaction term at every pre-ban interval suggests that there are violations of the PTA. In other words, there are systematic differences between the ban and non-ban states that influence URM enrollment patterns between the two states are not being explained by the model. To correct this bias, person-level covariates such as racial identity (in addition to URM identity), age, marital status, and citizenship are included in Table B.2. These variables help explain the differences in URM enrollment between the ban and non-ban states. However, the fact that the ban is significant one year before the ban is not problematic (see last row of Table B.3). This is an anticipation effect – or 'Ashenfelter's dip' (Ashenfelter and Card 1984). It indicates that the pattern of URMs enrolling more in college relative to their White peers began when the bans were being debated, and not when the bans were passed.

Table B.1 Event History Specification without Covariates- No College

Difference in Difference in Differences	Ban	Sig.	URMxBAN Interaction	Sig.	N=	# of Group
Interaction Effect	-0.0018(0.0035)		-0.0597(0.0047)	***	1,057,316	99,167
4 Years Before	0.0147(0.0056)	**	-0.0359(0.0083)	***	864,943	94,369
3 Years Before	0.0103(0.0057)		-0.0294(0.0096)	**	864,943	94,369
2 Years Before	0.0017(0.0060)		-0.0267(0.0118)	*	864,943	94,369
1 Year Before	0.0129(0.0070)		0.0772 (0.0173)	***	864,943	94,369

Note: * p <0.05, ** p<0.01, and *** p<0.001

Table B.2 Event History Specification with Covariates- No College

Difference in Difference in Differences	Ban	Sig.	URMxBAN Interaction	Sig.	N=	# of Group
Interaction Effect	-0.0019(0.0033)		-0.0418(0.0044)	***	1,057,316	99,167
4 Years Before	0.0020 (0.0070)		-0.0143(0.0089)		735,738	93,201
3 Years Before	0.0014(0.0071)		-0.0149(0.0098)		735,738	93,201
2 Years Before	-0.0006(0.0070)		-0.0160(0.0121)		735,738	93,201
1 Year Before	-0.0201(0.0081)		-0.0561(0.0174)	***	735,738	93,201

Note: * p <0.05, ** p<0.01, and *** p<0.001

The results from various event history specifications for 4-year schools are shown in Table B.3 (event history specification without covariates) below. Here, one can see that when state-level controls for the racial distribution of the population are not included in the model (e.g., the Black or Hispanic share of the population) the difference-in-differences estimator is statistically significant during the entire pre-ban period across the full range of institutional types investigated. Hence, the statistically significant ban-coefficient is capturing differences between ban and non-ban states that are related to the enrollment of URMs and thus, appears to violate the PTA. The model presented in Table 2 (main results) is a model that rigorously teased out the potential threats to the validity of the differences-in-differences model showing almost no potential violations of the PTA. This was done by including state-level covariates as shown by comparing Table B.4 and Table B.5. More covariates than included in Table B.5 were tested

(i.e., standardized testing scores and in-state and out-of-state costs of tuition) but omitted because their inclusion led to over-fitting the model. Furthermore, when other covariates were replaced by these covariates it was found that these covariates were not necessary to rule-out violations of the parallel trend assumption.

Table B.3 Event History Specification without Covariates

No College Education

Difference in Difference in Differences	Ban	Sig.	URMxBAN Interaction	Sig.	N=	# of Group
Interaction Effect	-0.0019(0.0033)		-0.0418(0.0044)	***	1,057,316	99,167
4 Years Before	0.0020 (0.0070)		-0.0143(0.0089)		735,738	93,201
3 Years Before	0.0014(0.0071)		-0.0149(0.0098)		735,738	93,201
2 Years Before	-0.0006(0.0070)		-0.0160(0.0121)		735,738	93,201
1 Year Before	-0.0201(0.0081)		-0.0561(0.0174)	***	735,738	93,201

Note: * p <0.05, ** p<0.01, and *** p<0.001

The remaining violations of the parallel assumption trend are not problematic and can be reasonably explained by other empirical research. There are violations for unclassified schools in Table B.4 (event history specification with covariates) that can be ignored because those main findings were not statistically significant and correcting for them would likely leave the main finding statistically insignificant because the size of the coefficient is small. This is congruent with existing research that finds no statistically significant movement along the track of the anti-affirmative action avalanche (Backes 2012, Hinrichs 2012). Of greater importance, however, are violations for four-year schools of the highest selectivity in the 2-year and 3-year periods before affirmative action. When the bans are significant just before, or a few years before the bans were implemented it can be explained by anticipation effects, as was the case in Table B.2, where URM enrollment increased in the year before the bans went into effect. The potential violations in Table B.4 suggest that schools admitted more URMs prior to the bans because they would be unable to do so after the bans went into place. However, the fact that the ban was not statistically

significant in the year proceeding the bans is odd if explained by an anticipation effect. The empirically driven reason for this explanation aligns with the decision making of the University of California system regarding affirmative action. According to Kidder and Gándara (2016) the shift in the University of California system preceded the state-wide ban that would later be implemented by the state of California by a year before California's ban on affirmative action because the University of California Board of Regents voted to do so. This means the ban occurred 1 year before the real ban and means that the anticipation effect would have taken place 2 or 3 years before the ban which is congruent with these results and those found in Bleemer who adjusts his ban "onset" for his analysis of University of California schools to offset the anticipation effect (Bleemer 2020). This is important because the most selective tier includes 4-year public schools in California such as UCLA, which retreated from affirmative action earlier than other schools impacted by California's state-level ban on affirmative action. However, outside of the most selective tier there are no other meaningful anticipation effects.

Table B.4 Event History Specification with Covariates

Share of first year URM students enrolled		For-Profit		Public		Private	
		Ban(S.E.)	Sig.	Ban(S.E.)	Sig.	Ban(S.E.)	Sig.
Any Selectivity	Unweighted	0.0109(0.0161)		-0.0003(0.0122)		-0.0163(0.0184)	
	4 Years Before	0.0034(0.0200)		0.0019(0.0048)		-0.0100(0.0149)	
	3 Years Before	-0.0021(0.0195)		0.0018(0.0047)		-0.0044(0.0107)	
	2 Years Before	-0.0096(0.0211)		-0.0012(0.0052)		-0.0025(0.0065)	
	1 Years Before	-0.0102(0.0227)		0.0033(0.0062)		-0.0010(0.0065)	
	N=	5,720		16,385		28,985	
	# of Schools	406		692		1299	
Barron's Highest Selectivity	Unweighted	N/A		-0.1036(0.0113)	***	-0.0220(0.0089)	*
	4 Years Before	N/A		0.0244(0.0105)		0.0072(0.0056)	
	3 Years Before	N/A		0.0323(0.0106)	*	-0.0022(0.0061)	
	2 Years Before	N/A		0.0390(0.0106)	*	-0.0069(0.0068)	
	1 Years Before	N/A		-0.0021(0.0150)		-0.0076(0.0068)	
	N=	N/A		200		1,785	
	# of Schools	N/A		8		72	
Highly Selective	Unweighted	N/A		-0.0144(0.0126)		-0.0105(0.0058)	
	4 Years Before	N/A		0.0049(0.0031)		0.0008(0.0041)	
	3 Years Before	N/A		0.0032(0.0037)		-0.0011(0.0035)	
	2 Years Before	N/A		0.0021(0.0040)		-0.0035(0.0030)	
	1 Years Before	N/A		-0.0018(0.0062)		-0.0059(0.0031)	
	N=	N/A		3,118		8,207	
	# of Schools	N/A		125		334	
Less Selective	Unweighted	-0.0072(0.0723)		-0.0139(0.0172)		-0.0059(0.0138)	
	4 Years Before	-0.0487(0.0337)		0.0009(0.0068)		0.0119(0.0077)	
	3 Years Before	-0.0280(0.0444)		0.0014(0.0065)		0.0139(0.0075)	
	2 Years Before	-0.0364(0.0452)		0.0014(0.0062)		0.0064(0.0060)	
	1 Years Before	0.0245(0.0901)		0.0028(0.0061)		0.0021(0.0894)	
	N=	495		9,284		13,914	
	# of Schools	20		382		572	
Unclassified	Unweighted	-0.0044(0.0133)		-0.0044(0.0065)		-0.0453(0.0524)	
	4 Years Before	-0.0134(0.0197)		-0.0096(0.0092)		-0.0980(0.0694)	
	3 Years Before	0.0202(0.0194)		-0.0109(0.0086)		-0.0640(0.0431)	
	2 Years Before	-0.0299(0.0235)		-0.0173(0.0078)	*	-0.0361(0.0314)	
	1 Years Before	-0.0362(0.0217)		-0.0044(0.0103)		-0.0109(0.0289)	
	N=	5,200		3,983		6,864	
	# of Schools	385		185		393	

Note: * p <0.05, ** p<0.01, and *** p<0.001

Table B.5 Event History Specification without Covariates

Share of first year URM students enrolled		For-Profit Ban(S.E.)	Sig.	Public Ban(S.E.)	Sig.	Private Ban(S.E.)	Sig.
Any Selectivity	Unweighted	0.0324 (0.0119)	**	0.0263 (0.0086)	**	0.0119 (0.0153)	
	4 Years Before	-0.0082 (0.0116)		0.0111 (0.0051)	*	0.0011 (0.0129)	
	3 Years Before	-0.0111 (0.0105)		0.0077 (0.0046)	*	0.0048 (0.0098)	
	2 Years Before	-0.0136 (0.0105)		0.0107 (0.0047)		0.0063 (0.0061)	
	1 Year Before	-0.0146 (0.0169)		0.0117 (0.0050)	*	0.0060 (0.0062)	
	N=	5,823		17,028		30,087	
	# of Schools	406		692		1260	
Barron's Highest Selectivity	Unweighted	N/A		-0.0933 (0.0055)	***	-0.0194 (0.0111)	
	4 Years Before	N/A		0.0192 (0.0024)	***	0.0058 (0.0058)	
	3 Years Before	N/A		0.0233 (0.0025)	***	-0.0019 (0.0059)	
	2 Years Before	N/A		0.0289 (0.0026)	***	-0.0067 (0.0064)	
	1 Year Before	N/A		-0.0042 (0.0046)		-0.0066(0.0057)	
	N=	N/A		208		1,855	
	# of Schools	N/A		8		72	
Highly Selective	Unweighted	N/A		0.0008 (0.0142)		0.0043 (0.0059)	
	4 Years Before	N/A		0.0043 (0.0061)		0.0044 (0.0036)	
	3 Years Before	N/A		0.0026 (0.0064)		0.0026 (0.0031)	
	2 Years Before	N/A		0.0014 (0.0066)		0.0003 (0.0029)	
	1 Year Before	N/A		-0.0021 (0.0084)		-0.0015 (0.0025)	
	N=	N/A		3,241		8,537	
	# of Schools	N/A		125		334	
Less Selective	Unweighted	0.0395 (0.0394)		0.0225 (0.0098)	*	0.0122 (0.0149)	
	4 Years Before	0.0753 (0.0141)	***	0.0129 (0.0080)		0.0126 (0.0088)	
	3 Years Before	-0.0555 (0.0190)	*	0.0129 (0.0077)		0.0144 (0.0088)	
	2 Years Before	-0.0611 (0.0208)	*	0.0124 (0.0070)	*	0.0077 (0.0086)	
	1 Year Before	-0.0160 (0.0576)		0.0131 (0.0069)	*	0.0035 (0.0117)	
	N=	514		9,657		14,462	
	# of Schools	20		382		572	
Unclassified	Unweighted	0.0222 (0.0127)		0.0147 (0.0117)		0.0067 (0.0542)	
	4 Years Before	-0.0061 (0.0104)		0.0045 (0.0081)		-0.0494 (0.0633)	
	3 Years Before	-0.0105 (0.0093)		0.0028 (0.0082)		-0.0281 (0.0416)	
	2 Years Before	-0.0158 (0.0125)		-0.0027 (0.0076)		-0.0041 (0.0281)	
	1 Year Before	-0.0231 (0.0165)		0.0082 (0.0098)		0.0164 (0.0163)	
	N=	5,283		4,130		7,088	
	# of Schools	385		185		393	

Note: * p <0.05, ** p<0.01, and *** p<0.001

Appendix C: *Variation in Ban "Onset" Timing*

The main results of this paper (Table 2) use ban implementation as the onset of the bans. This onset time describes when admissions offices changed their adminisions policies to race neutral. Table B.4 in the previous section of the Appendix finds some evidence of anticipation effects in the event history analysis. The anticipation effects were found to be positive for the highest selectivity public schools two- and three- years before the bans on affirmative action were implemented. Because the "pseuedo-bans" were positive this means that there was an increase in URM enrollment prior to the implementation of the bans. In other words, schools appear to have admitted more URMs than they normally would because they knew the bans would make it more difficult to admit these students. However, its also possible that imprecision of the ban onset timing could produce this result. Thus, two alternatives are presented in Tables C.1 and C.2 to account for possible variation in ban onset. The ban ratification and the first cohorts impacted by the bans are presented in Appendix A. The ban ratification represents the year when the ban was passed, but had not yet become law. The first cohort represents the first enrollment cohorts to be impacted by the bans. Table C.2 shows the model for ratifications. Because the ban onset moves to an earlier time-point it absorbs some of the "anticipation effects" shown in Table B.4. There is confidence that it absorbs this effect (around +0.03) because the main model reports an unweighted effect of -0.1036 and the ratification model reports an effect of -0.0783.

The fall cohort coefficients in Table C.2 are more negative (-0.1135) than those reported by Table B.4 (-0.1036). This suggests that the implementation at fall cohort onset provides the highest estimate of the impact of the bans.

All models are robust to the main findings of this paper: decreases in URM enrollment at public highest selectivity institutions, and increases in URM enrollment at for-profit colleges in response to the state-level affirmative action bans. However, these patterns are not congruent. These additional analyses show an increase in magnitude for public highest selective schools as the onset time moves from earlier to later in time, but show a V-shaped pattern for for-profit schools. This V-shapped pattern for for-profits requires some speculation. The pattern implies that for-profit schools may have also agressively tageted URM students prior to the bans (just like the highest selectivity public schools did- potentially out of fear that it would be harder to enroll them later), for-profts slowed their enrollment of URMs closer to the implementation (by admissions offices) of the ban (perhaps due to the fear of potential litigation or regulatory action), and then continued their increased enrollment of URM enrollment (once their fears may have subsided).

Table C.1. Ban Onset at Ratification

Share of first year URM students enrolled		For-Profit		Public		Private	
		Ban(S.E.)	Sig.	Ban(S.E.)	Sig.	Ban(S.E.)	Sig.
Any Selectivity	Weighted	0.0957(0.0428)	*	0.0027(0.0095)		0.0098(0.0131)	
	Unweighted	-0.0008(0.0170)		0.0006(0.0102)		-0.0130(0.0161)	
	N=	5,720		16,385		28,985	
	# of Schools	406		692		1,299	
Barron's Highest Selectivity	Weighted	N/A		-0.0735(0.0110)	***	-0.0224(0.0160)	
	Unweighted	N/A		-0.0783(0.0103)	***	-0.0191(0.0077)	*
	N=	N/A		200		1,785	
	# of Schools	N/A		8		72	
Highly Selective	Weighted	N/A		-0.0096(0.0107)		-0.0061(0.0072)	
	Unweighted	N/A		-0.0121(0.0115)		-0.0097(0.0048)	*
	N=	N/A		3,118		8,207	
	# of Schools	N/A		125		334	
Less Selective	Weighted	0.0779(0.2066)		-0.0065(0.0114)		0.0009(0.0177)	
	Unweighted	-0.0131(0.0546)		-0.0111(0.0132)		-0.0046(0.0124)	
	N=	495		9,284		13,914	
	# of Schools	20		382		572	
Unclassified	Weighted	-0.0014(0.0180)		0.0080(0.0082)		0.0505(0.0359)	
	Unweighted	-0.0179(0.0123)		-0.0029(0.0072)		-0.0351(0.0457)	
	N=	5,200		3,983		6,864	
	# of Schools	385		185		393	

Note: * p <0.05, ** p<0.01, and *** p<0.001

Table C.2. Ban Onset at Fall Cohort

Share of first year URM students enrolled		For-Profit		Public		Private	
		Ban(S.E.)	Sig.	Ban(S.E.)	Sig.	Ban(S.E.)	Sig.
Any Selectivity	Weighted	0.0764(0.0307)	*	0.0006(0.0113)		0.0178(0.0167)	
	Unweighted	0.0171(0.0141)		0.0001(0.0119)		-0.0118(0.0164)	
	N=	5,720		16,385		28,985	
	# of Schools	406		692		1,299	
Barron's Highest Selectivity	Weighted	N/A		-0.1203(0.0096)	***	-0.0307(0.0185)	
	Unweighted	N/A		-0.1135(0.0123)	***	-0.0185(0.0085)	*
	N=	N/A		200		1,785	
	# of Schools	N/A		8		72	
Highly Selective	Weighted	N/A		-0.0113(0.0114)		-0.0061(0.0078)	
	Unweighted	N/A		-0.0136(0.0128)		-0.0101(0.0056)	
	N=	N/A		3,118		8,207	
	# of Schools	N/A		125		334	
Less Selective	Weighted	0.2816(0.2193)		-0.0096(0.0123)		0.0140(0.0239)	
	Unweighted	0.0296(0.0864)		-0.0142(0.0166)		0.0030(0.0142)	
	N=	495		9,284		13,914	
	# of Schools	20		382		572	
Unclassified	Weighted	-0.0053(0.0229)		0.0060(0.0098)		0.0566(0.0442)	
	Unweighted	0.0013(0.0117)		-0.0010(0.0071)		-0.0316(0.0429)	
	N=	5,200		3,983		6,864	
	# of Schools	385		185		393	

Note: * p <0.05, ** p<0.01, and *** p<0.001

Appendix D: *Non-Recoded Main Results for 4-Year Institutions*

Fully stratifying the 4-year institutions by selectivity, as is done in Table D allows for more precision along the anti-affirmative action avalanche. Wild bootstraps were not calculated here, because this model does not impact the aggregated findings in Table 2. Consistent with the theoretical model, the only discernable movement in the share of URMs at 4-year institutions comes at the top of the avalanche- at the most selective schools in the country (-0.1028 or a decrease of 38% and statistically significant in terms of the typical student and -0.1036 concerning the typical school), and spills over-to the typical private school (-0.0220 and statistically significant), but not the typical student at "Most Competitive" public schools (-0.0285 and not statistically significant) which is consistent with Backes (2012) and Hinrichs (2012). Furthermore, this displacement effect extends to public schools at the next tier down, concerning both the typical student (-0.0397 and statistically significant) and the typical school (-0.0463 and statistically significant). However, at this next tier down, there is no spillover into public schools. Additionally, this model shows that there is a displacement of URMs from "Competitive" for-profit schools. This finding suggests that at the top-tier (where estimates could be produced for for-profit schools), students are also displaced like they are in the "Most Competitive" and "Highly Competitive" tiers of public schools. Some tiers for for-profit schools had too few schools to produce a statistically meaningful result. Congruent with results along the track (i.e., the middle section of the avalanche) found by Backes (2012), Hinrichs (2012), and Bleemer (2020), there are no statistically significant results for URM enrollment. However, at the bottom of the avalanche, novel evidence shows an increase in URM enrollment at 4-year non-competitive public schools for both the typical student (0.0378 and statistically significant) and the typical school (0.0312 and statistically significant). Another novel finding illustrates a

statistically significant decline for all for-profit schools but only for the typical student (0.0749 and statistically significant).

Table D. The Impact of Affirmative Action Bans on the Share of URM Enrollment at 4-Year Colleges (Not Recoded)

Share of first year URM students enrolled		For-Profit Ban(S.E.)	Sig.	Public Ban(S.E.)	Sig.	Private Ban(S.E.)	Sig.
Any Selectivity	Weighted	0.0749 (0.0320)	*	0.0000(0.0120)		0.0132(0.0158)	
	Unweighted	0.0109 (0.0161)		-0.0003(0.0122)		-0.0163(0.0184)	
	N=	5,720		16,385		28,985	
	# of Schools	406		692		1299	
Most Competitive	Weighted	N/A		-0.1028(0.0099)	***	-0.0285(0.0184)	
	Unweighted	N/A		-0.1036(0.0113)	***	-0.0220(0.0089)	*
	N=	N/A		200		1,785	
	# of Schools	N/A		8		72	
Highly Competitive	Weighted	N/A		-0.0397 (0.0114)	**	-0.0014 (0.0092)	
	Unweighted	N/A		-0.0463 (0.0105)	***	-0.0077 (0.0077)	
	N=	N/A		740		1,860	
	# of Schools	N/A		31		78	
Very Competitive	Weighted	N/A		-0.0027 (0.0106)		0.0034 (0.0100)	
	Unweighted	N/A		-0.0038 (0.0119)		-0.0085 (0.0088)	
	N=	N/A		2,178		4,562	
	# of Schools	N/A		86		184	
Competitive	Weighted	-0.1482 (0.0550)	*	-0.0152 (0.0142)		-0.0142 (0.0198)	
	Unweighted	-0.1239 (0.0336)	*	-0.0211 (0.0180)		-0.0169 (0.0112)	
	N=	172		6,138		9,777	
	# of Schools	7		250		399	
Less Competitive	Weighted	N/A		-0.0105 (0.0193)		0.0290 (0.0360)	
	Unweighted	N/A		-0.0140 (0.0238)		0.0306 (0.0372)	
	N=	N/A		1,784		2,026	
	# of Schools	N/A		75		85	
Non-competitive	Weighted	N/A		0.0378 (0.0133)	**	0.0141 (0.0358)	
	Unweighted	N/A		0.0312 (0.0113)	*	0.0066 (0.0370)	
	N=	N/A		1,175		840	
	# of Schools	N/A		48		35	
Special	Weighted	N/A		0.0128 (0.0183)		0.0158 (0.0337)	
	Unweighted	N/A		-0.0034 (0.0386)		-0.0075 (0.0186)	
	N=	N/A		187		1,271	
	# of Schools	N/A		9		53	
Not Classified	Weighted	-0.0026 (0.0219)		0.0047 (0.0084)		0.0535 (0.0464)	
	Unweighted	-0.0044 (0.0133)		-0.0044 (0.0065)		-0.0453 (0.0524)	
	N=	5,200		3,983		6,864	
	# of Schools	385		185		393	

Note: * p <0.05, ** p<0.01, and *** p<0.001

Appendix E: *Bias in Staggered Difference in Differences Models*

The Goodman-Bacon decomposition (Figures E.1, E.2, and E.3) results indicate that there is likely heterogeneity in the treatment, which would produce biased results. However, the decomposition provides only a limited scope of the tiers studied in the analysis. There are insufficient observations for several analyses (For Profit: every level except Less Selective; Public: Unclassified; Private: Any, Highly Selective, and Unclassified).

Figure E.1. Goodman-Bacon decomposition for For-Profit 4-year schools

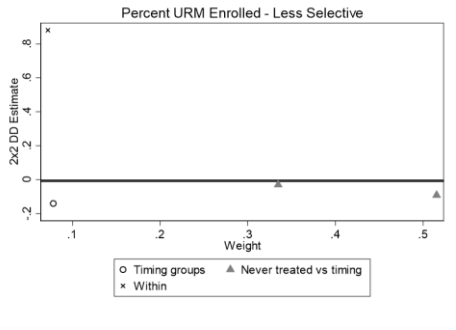


Figure E.2 Goodman-Bacon decomposition for Public 4-year schools

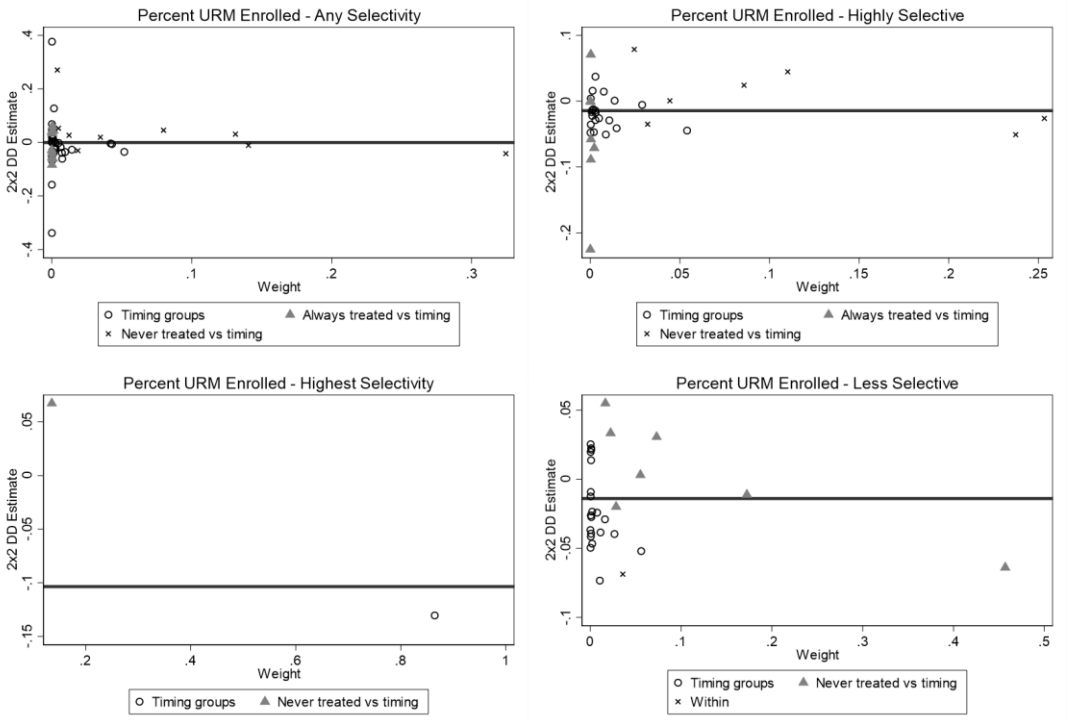
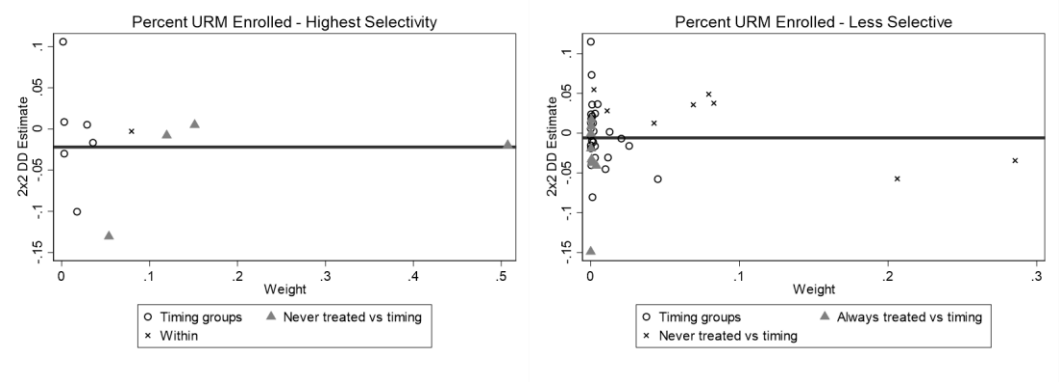


Figure E.3 Goodman-Bacon decomposition for Private 4-year schools



A further limitation of this decomposition is that it does not include any covariates. This is problematic because the inclusion of covariates largely corrects for differences between ban and non-ban states. Tables B.4 and B.5 show that the impact of the ban for any public school was 0.0263 and statistically significant without the inclusion of state-level covariates but including them changed the result to a not significant -0.0003. However, because the Goodman-Bacon decomposition does not allow for the inclusion of covariates it is unclear if the fully fitted model would also produce negative weights. The unbiased estimator results in Table E.1, based on the work of Chaisemartin and d'Haultfoeuille (2020), include covariates and have some discrepancies from the main models.

Table E.1 Chaisemartin and d'Haultfoeuille's Unbiased DID Estimator

Share of first year URM students enrolled		For-Profit Ban(S.E.)	Sig.	Public Ban(S.E.)	Sig.	Private Ban(S.E.)	Sig.
Any Selectivity	Unweighted	0.0245(0.0461)		-0.0075(0.0055)		-0.0259(0.0414)	
	N	1,923		3,996		7,819	
Barron's Highest	Unweighted	NA		-0.0517(0.0109)	***	-0.0158(0.0123)	
	N			8		258	
Highly Selective	Unweighted	NA		-0.0071(0.0068)		-0.0016(0.0093)	
	N			753		1773	
Less Selective	Unweighted	-0.0694(0.0240)	*	0.0014(0.0068)	*	0.0072(0.0095)	
	N	38		2,333		3,489	
Unclassified	Unweighted	0.0275(0.0467)		0.0207(0.0147)		-0.0862(0.0781)	
	N	1533		777		1926	

These models produce the well-known effect that URM enrollment declines at highly selective public schools. However, it also suggests that there may be a decrease at for-profit schools and an increase at public schools which are “Less Selective” and an increase at “Less Selective” public schools. Unfortunately, this unbiased estimator only estimates the treatment effect in the groups that switch treatment at the time when they switch and does not account for the longer-term impacts of the bans. This is problematic because an analysis of influential for-profit schools in this study (Table 4) finds that there were large changes nearly ten years after the passage of affirmative action bans for some for-profit schools. Thus, these changes would not be captured by these decomposition model. Furthermore, because the packages used to estimate these results do not accommodate the weighted regressions used in the main analysis these sensitivity analyses cannot remark on how the typical student at a given institutional type has been impacted by the bans. They can only remark on the unweighted analysis of how the composition of students changes as a result of the bans. Ultimately, these tests do not invalidate the existence of causal effects but suggest that the findings be treated with caution.

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