*The Anti-Affirmative Action Avalanche: Where Students Enroll After Affirmative Action Bans

INTRODUCTION

Beginning with *Proposition 209* in California in 1996, nine states passed their own bans on the practice of affirmative action, preventing public institutions of higher education from considering race in admissions or the awarding of scholarships and other financial aid.[1] Although there was a legal challenge to the legitimacy of the state level bans, the *Schutte v. Coalition to Defend Affirmative Action* (2014) 6-2 Supreme Court decision permitted states to continue to adopt bans on affirmative action. In this article I exploit state-level variation in the presence and the timing of affirmative action bans in the nine affirmative action ban states to estimate the effect of these bans on the college enrollment of underrepresented minority students (herein URMs or URM students).[2] Although, the previous literature on has shed light on some of the changes in URM undergraduate student enrollment (Backes 2012, Hinrich 2012), it has been unable to fully conceive a theoretical model for the impact of affirmative action bans across the entire system of postsecondary education. This is largely due to the inability of the literature to explain where URM students have gone, a question which I reconcile in this work by including institutions that were excluded from previous studies: lower quality institutions and for-profit colleges. By including these institutions, I argue that I have found the missing puzzle-piece needed to illustrate what happened to the entire system of undergraduate postsecondary education in the wake of affirmative action bans: what I term the anti-affirmative action cascade.

The impact of affirmative action bans on this non-trivial number of states has already decreased the proportion of URMs entering various levels of higher education including 4-year

colleges, graduate schools, and professional schools, demonstrating the effects of switching from a system of affirmative action to a system of colorblind meritocracy (Backes 2012, Espenshade and Chung 2005, Garces 2013, Garces and Mickey-Pabello 2015, Hinrichs 2012, Wightman 1997). While these studies conclude that the bans decreased the proportion of URMs at highly selective schools, they do not pinpoint where URMs have gone as a result of the bans. The shortcoming of those aforementioned studies is that numerous colleges and universities were excluded from their analysis because those schools did not provide standardized testing scores. While the standardized testing score was invaluable to show that there were different impacts of affirmative action by the level of selectivity of the institution, the studies (Backes 2012, Hinrichs 2012) excluded the less glamorous schools in the trenches. I include these missing schools by using an alternative measure of school selectivity: the Barron's classification. Using the Barron's classification is the superior measure because it uses SAT scores to classify more competitive schools, but also provides a classification for non-competitive schools that do not necessarily collect SAT scores. By including these schools, and also investigating if some underrepresented minority students did not go to college at all, this work is the first to fully explain the impact of affirmative action bans across the landscape of postsecondary education desperately filling a major lacuna in research on affirmative action in university admissions. Most importantly it answers the previously unanswered question "Where did all of the minorities go after affirmative action bans?"

Beyond 4-year postsecondary schools, only Backes (2012) has investigated where else URMs have enrolled by analyzing two-year institutions; he found no evidence that the share of URM students changed at two-year institutions. We still do not have an understanding of what happened to URM students in the wake of these bans. Sander (2004) claims that the "lack of

good empiricism on this issue results from the tendency of researchers, public intellectuals, and media to focus on the glamorous schools, and to give only passing attention to those in the trenches." This in part is bore out of the reactionary response of affirmative action bans to preserve classroom seats at the finest institutions for the progeny of the mostly White social elite. Indeed, the college admissions scandal of 2019 in which fifty-one parents were implicated for a scandal where they cheated the admissions system with bribery and other duplicitous tactics at prestigious schools greatly emphasizes the lengths that people would go to preserve their own self-interests and those of their descendants. This pattern is also evident at the state-level when affirmative action bans were adopted to preserve educational resources based on racial group interest (Baker 2019).

The top-schools are what the battle over affirmative action has been about. The enrollment shares of URM students at state flag-ship institutions have been decimated by these bans and have not yet fully recovered. For example, in Table 1 below I show that the enrollment shares of Black and Latino students at Berkeley have continued to wane relative to their demographic representation within the state long after bans on affirmative action.

<<INSERT TABLE 1 HERE>>

However, as I demonstrate in this article, the battle over affirmative action has impacted more than just those vying for the seats at our nation's most prestigious schools. Clearly, URM students are absent from state flag-ship schools in the wake of affirmative action bans, but in this study, I pinpoint where URM students have gone by investigating if the share of URMs with no postsecondary educational attainment increased as a result of affirmative action bans (i.e., whether they selected out of higher education altogether), and how bans have differentially

changed enrollment at 2-year and 4-year colleges on the basis of their for-profit vs. non-profit, public vs. private identities, as well as their level of selectivity.

I term my theoretical model as the anti-affirmative action avalanche. To be clear, this model is grounded in organizational change and not student change. This is because the aforementioned literature (Backes 2012 and Hinrichs 2012) also uses institutions as the units of analysis. Furthermore, scholars have found that the lion's share of underrepresented minority student enrollment decline is not due to changes in enrollment behavior, but changes in admissions practices (Card and Krueger 2005, Mickey-Pabello and Garces 2018). In essence, it is the institutional response to affirmative action bans and not a student response that is responsible for the 'avalanching.' I continue the tradition of analyzing institutions and reinforce its legitimacy by investigating the typical changes at institutions based on their level of selectivity (i.e., Barron's classification) and their designation as public, private non-for profit, and for-profit. I define the anti-affirmative action avalanche as a process whereby the intense competition for access to the most selective postsecondary education institutions was additionally constrained by a legally mandated change in institution's admissions policies which not only displaced URM students from the most selective colleges, but because enrollment ceilings are fixed it leads to URMs sliding down the avalanche to less selective 4-year and 2-year institutions. I find that the bottom of the cascade is for-profit colleges and universities and not "out of college."

Lastly, I weigh-in on two mechanisms regarding the institutional response to affirmative action bans after being required by law to adopt them. The first argues that institutions have found work-arounds to affirmative action bans in the spirit of achieving racial diversity (Berrey

2015, Mickey-Pabello and Garces 2018, Okechukwu 2019, Warikoo 2016) while the other argues that schools have deinstitutionalized affirmative action bans (Hirschman, Berrey and Rose-Greenland 2016, Hirschman and Berrey 2017). My findings suggest that the deinstitutionalization argument prevails over the finding different paths to racial diversity argument as measured by the increase in the effect of affirmative action bans over time after their implementation.

OVERCOMING A LIMITATION OF PRIOR WORK ON AFFIRMATIVE ACTION

The goals of this study are to fully describe what has happened to the entire landscape of post-secondary education in response to affirmative action bans, discover where URMs students have enrolled as a result of affirmative action bans, and find if the impact of the bans has grown stronger or waned after time (i.e., affirmative action work-arounds or deinstitutionalization). First, I summarize the literature on the causal impacts of affirmative action bans on educational attainment and emphasize that the previous literature has not fully considered the broad range of institutions that were impacted as a result of affirmative action bans. This in turn, has limited our capacity to fully grasp the extent of the impacts of affirmative action bans. Next, after describing the existing research on affirmative action bans, I construct the theoretical framework for the anti-affirmative action avalanche paying critical attention to where other theories of organizations, racial inequality, and group self-interest would suggest URM students were ultimately displaced to due to affirmative action bans. Lastly, I describe the debate about the institutional response to affirmative action bans. One is aimed to reduce the impact of the ban, and the other explains why the bans may have grown stronger over time. I propose to explain

which of two responses is dominant by measuring if the impact of the bans has grown or diminished over time.

Several studies have claimed that that affirmative action bans have decreased the proportion of underrepresented minority students entering various levels of higher education (Backes 2012, Espenshade and Chung 2005, Garces 2013, Garces and Mickey-Pabello 2015, Hinrichs 2012, Wightman 1997). Espenshade and Chung (2005) created one of the first simulation-based studies to estimate the impact of affirmative action bans on 4-year colleges and universities, determining that the bans decreased the probability of admission for URM students. Hinrichs (2012) and Backes (2012) were the first use difference-in-differences methods to estimate the causal impact of affirmative bans. They too found that affirmative action bans decreased the proportion of URMs admitted at 4-year institutions. Backes (2012) hypothesized that URM students were being funneled into 2-year colleges but found no support for that hypothesis. In this study I include 2-year colleges so that I can reinvestigate that hypothesis within the context of the anti-affirmative action avalanche. Because most previous studies show how affirmative action bans differentially affected URMs (Backes 2012; Hinrichs 2012) by level of institutional selectivity, for-profit schools were omitted from those analyses because they are not selective. Many for-profit schools cannot be ranked using SAT scores because they do not collect them, and many of these schools are also missing a Barron's Admissions Competitiveness Index for the same reason. Omitting for-profit schools from studies of the impacts of affirmative action has limited our ability to understand what happens to URM students. I resolve this limitation of previous research by also including the schools that provide no SAT score information or are not included in the Barron's Admissions Competitiveness Index.

By including the often overlooked schools I am able to show what has happened at unselective schools such as for-profit colleges. If the hypothesis that the share of URM students at for profit colleges increased due to affirmative action bans is true, then the affirmative action bans may be contributing to educational and economic inequality to a degree not previously imagined. While for-profit schools have been pioneers in adult and continuing education programs, and in online education, they have also earned a less favorable reputation for targeting vulnerable populations such as racial minorities and the poor with unethical recruiting practices (Dougherty et al. 2016, Lahr et al. 2014). In addition, for-profit schools provide low quality education, produce lower graduation rates, and saddle students with more student debt, lower economic returns to their education, and fewer job prospects than their peer institutions (Cellini, Darolia and Turner 2016, Cellini and Koedel 2017, Cellini and Turner 2019, Gilpin and Stoddard 2017, Lynch, Engle and Cruz 2010, Schade 2014). [See Cellini and Koedel 2017, and Gilpin and Stoddard 2017 for a detailed discussion]. In some cases, attending a for-profit university can have catestrophic financial consequences such as high amounts of student loans and an inability to repay them, particularly if students drop-out (McFarland et al. 2017) and for students whose degrees are not ackowledged in the workplace due to the poor reputation of the school (Darolia et al. 2015). Using an audit study, Darolia et al. (2015) found that relative to applicants that listed no college at all, employers had no preference for students from for-proft schools.

CREATING A FRAMEWORK FOR THE ANTI-AFFIRMATIVE ACTION AVALANCHE

I argue that the anti-affirmative action avalanche consists of two major mechanisms each one occurring at a different level of my analysis: the institution and the state. The first

mechanism is that of *racial self-interest at the state level*. I argue that the state-level policy bans were designed to preserve racial group interest at the state-level. Baker (2019) found that the state-level bans on affirmative action affirmative action (most of which were state-level policy ballot initiatives) were largely driven by variables that suggest there was self-interest on the part of the White racial majority to preserve its own group interest. As such, I contend that affirmative action bans are a special racialized instance of opportunity hoarding (Tilly 1998) at the state-level, where the resource of education is hoarded by the white racial group through the bans to preserve its dominant status and access to the highest quality education. This serves to elucidate how the legal system is used to distribute resources and opportunities in such a bifurcated manner that is self-serving to those already in power and oppressing to those who are not in power. The bans are consistent with other sociological work that argues that educational resources are often dominated by a group's self-interest (Alon 2009, Alon 2015, Lucas 2001). Furthermore, considering race specifically, the bans embody the hallmarks of Bobo's (Bobo and Hutchings 1996) racial group position theory (i.e., prejudice, racial dominance, and a threat to resources); the bans exemplify the *prejudicial* response manifested into law by the *dominant racial group* whose educational prospects are perceived as *threatened by affirmative action*. As such, despite the fact that proponents of affirmative action bans advocate for them by applying a rationale of equal opportunity they call color-blind meritocracy, *laissez-faire* racism serves to illustrate why the myth of equal opportunity is not realized with respect to race (Bobo, Kluegel and Smith 1997).

These features of the color-blind meritocracy can be traced back to the first affirmative action ban in the state of California, when the anti-affirmative action leadership crystalized its rhetoric of passing the bans on the virtues of a colorblind racism. Glynn Custred, a California

State University anthropology professor, and Thomas Wood, the executive director of the California Association of Scholars, co-drafted California's Proposition 209. (This policy was mimicked by other states; much of the same language can be found on the ballot initiatives for affirmative action bans in other states.) Both educators claimed that White men had too long suffered from the impacts of "reverse discrimination" (Mukherjee 2006). This claim of "reverse discrimination" is consistent with a narrative around White male suffering that is attributed to Vietnam, Civil Rights, feminism, antiwar, and gay-pride movements that disrupted White masculinity and its dominance over other traditionally less dominant identities. White males interpreted themselves as the victims of these larger cultural changes (Jeffords 1993, Robinson 2000, Dyer 2013). To combat the impacts of perceived reverse discrimination, Custred and Wood reinterpreted the Equal Protection Clause of the 14th Amendment to preserve the White male dominance that had long been the status quo. In an interview to the *New York Times* in 1995, Custred stated that "Affirmative action has been losing steam with the general public, and we think we've hit upon the sure way to finally reverse it and restore true color-blind fairness in the United States."

I argue this mechanism of *racial-self interest at the state-level was* embodied by the bans by showing that the state-level group self-interests and the preservation of educational resources are manifested in the bans. As such, I capture *racial-self interest at the state level* by comparing ban states to non-ban states to show differences in underrepresented minority student enrollment patterns. To ensure that my estimates truly capture the differences between ban and non-ban states and not something else I control for the differences between the ban states and non-ban states that could explain differences in the dependent variable (i.e., URM enrollment) between the ban and non-ban states. This essentially rules out other explanations not attributable to the

bans themselves, and underscores that the difference estimated is indeed due to these anti-affirmative action policies.[3]

The next mechanism, *organizational racialization*, occurs at groups of institutions classified by their level of selectivity. This mechanism shows that the impacts of affirmative action felt at one institutional tier reverberate across the whole system of postsecondary education. This organizational emphasis underscores Ray's theory of racialized organizations (2019). When the affirmative action laws were passed, they were filtered by colleges and universities that changed their own admissions practices to be compliant with state policies. Ultimately, the change in admissions practices were legitimated at the organizational level, adopting the colorblind meritocracy in favor of affirmative action. I use the term avalanche to illustrate how URM students have been displaced at the height of the mountain (selective institutions) and have slid down the mountainside to less selective tiers of higher education or out of higher education altogether. Results from Mickey-Pabello and Garces (2018) emphasize that changes in URM enrollment in the wake of affirmative actions bans have occurred almost wholly due to organizational responses to affirmative action bans and not changes in student application behavior. As such, I measure this mechanism of *organizational racialization* by estimating how the underrepresented minority student enrollment has changed at each level of college and university selectivity.

I theorize that the way in which organizations have been racialized by affirmative action bans is linked; that the battle for the most coveted seats at the top of the post-secondary education mountain has set-off subsequent effects for the rest of postsecondary education. While I provide support for this theory by showing the changes in URM enrollment by institutional selectivity, two-year/four-year classification, and public/private not for profit/for-profit

designation I am limited in my ability to provide greater detail about the flows of students in and out of various postsecondary strata due to data limitations. More information about the flows of students to various types of institutions would sharpen the mechanisms and add clarity to the anti-affirmative action avalanche. However, this data is not publicly available, and the existing publicly available data that could measure flows of students is not robust enough for the number of years and states required to conduct such an analysis. For instance, these data could explain how many students that were displaced from the most selective institutions to less selective 4-year institutions, 2 year intuitions, private institutions, and for-profit colleges and universities. Nevertheless, measuring the changes at each level of college selectivity begins to paint a picture of the avalanche, and much like an actual avalanche it is difficult to account where each snowflake previously rested after the avalanche ran its course. This is precisely the case with the anti-affirmative action avalanche which best illustrates the top and the bottom. The anti-affirmative action avalanche shows that URMs have been displaced from the top, with many more now finding themselves at the bottom. In the following paragraphs I detail the anti-affirmative action avalanche by tier and explain why a change should or should not be observed.

The starting zone for the avalanche (the most selective institutions) is where affirmative action bans displace a group of underrepresented minority students that would otherwise attend those schools if there were no ban. Previous research has found that affirmative action bans have displaced URMs at the most selective schools (Backes 2012, Hinrichs 2012). The colorblind meritocracy inherent at the mountain-top devalues a person's race in favor of ability-based meritocracy, which is commonly measured by high school quality, GPAs, test scores, and other achieved characteristics. This is problematic for URMs because access to test-preparation, tutoring, and other ways to increase measures of 'merit' can be purchased, and are more likely to

be purchased by those with a higher SES (Buchmann, Condron and Roscigno 2010, Stevenson and Baker 1992, Zwick 2019). These features highlight the covert, and unintentional nature of laissez-faire racism (i.e., hidden structural racism) inherent in the anti-affirmative action avalanche (Bobo, Kluegel and Smith 1997). Test preparation, tutoring, and other ways to increase measures of merit are aptly termed "shadow education" because they promote education and advantages that are beyond the scope of a traditional education that occurs in school. Furthermore, the instruments used to measure merit such as the SAT are sometimes racially biased themselves (Freedle 2003, Zwick 2019). As a result of the affirmative action bans, URM students that are typically graded lower across various meritocratic measures like GPA and SAT scores are displaced from the top of the mountain where the most selective institutions are.

After URM students are not admitted to the most elite institutions I propose that a chain reaction is set-off across the various tiers and sectors of postsecondary education. Although I cannot directly observe that the displaced URM students at the top get mostly get sent down to the next institutional selectivity tier due to the absence of quality student-level data, I make the assumption that they are sent down to the next tier and not all the way to the bottom because students typically apply to colleges and universities of comparable selectivity with a few "reach schools" and a few "safety schools" (DesJardins, Dundar and Hendel 1999, Hoxby and Avery 2012). After these URMs are displaced from the top the competition for resources is renewed at each tier of the anti-affirmative action avalanche where schools still pick and choose their most qualified students. The group of URM students that is displaced at the most selective institutions (the top of the mountain; the starting zone)[4] then creates a new competition for a limited number of enrollments available at the next tier of postsecondary education down (less selective 4-year schools; part of the track).

After the starting zone (i.e., the top), students enter a competition between other less academically qualified URMs and similarly qualified non-URM students at less selective 4-year schools. This pattern continues at every tier down the anti-affirmative action avalanche (to 4-year schools unclassified by selectivity, and then to 2-year public and private schools; the rest of the track) until a final group of URM students hits the base of the mountain (the runout zone; which I argue is for-profit education). Along the way down, the competition lessens because the meritocratic requirements decrease. Because only twenty to thirty percent of colleges and universities have the ability to pick and choose their students (Bowen and Bok 2016) much of the empirical focus for the impact of affirmative action bans has focused on those schools. However, the applicants displaced by the bans at the most selective schools reverberates through the other eighty to seventy percent of schools that do not pick their applicants. Although there is movement through this middle part of the anti-affirmative action avalanche, I should not be able to measure any statistically significant affirmative action ban effects in the track of the avalanche (i.e., less selective 4-year schools and 2-year colleges) because the students in the track that leave are replaced by the students in the track that come in.[5] In other words, the net change for the tiers of education that comprise the track (i.e., less selective 4-year schools and 2-year schools) should be close to zero if the bans have created a downward shift where the more academically qualified URMs are pushing the less qualified URMs down the avalanche. Most of the previous work in this area finds that there are no changes in the enrollment patterns of URMs in the track (Backes 2012, Hinrichs 2012). However, there is a working paper that argues that changes could be observed in the track, finding the pattern at most selective schools for Blacks is similar but smaller in magnitude at the tier below, and that the pattern at the least selective schools for Blacks is again similar, but smaller in magnitude at the tier above (Kehal, Hirschman and Berrey

2018). However, that research focuses on a much smaller number of schools (975 compared to the over 2000 in this study), focuses exclusively on non-ban states (I also include ban states), re-classifies the Barron's categories differently than this study (schools not classified by Barron's are also included in this study, and the top two tiers in their study is most similar to only the top tier in my study), uses changes in race-conscious admissions instead of state-level affirmative action bans as its difference-in-differences estimate, and looks at race-specific changes instead of grouping URMs together as one group. Furthermore, the aforementioned study has not yet ruled-out other plausible explanations for its findings by rigorously addressing the parallel trend assumption as I do in this study. In sum, congruent with results from Backes (2012) and Hinrichs (2012), I contend my measure should only be able to capture the changes at the very top and bottom of the avalanche, and not the middle. However, it is plausible that my findings could change if I were to recode the Barron's classification or provide estimates for each ethnoracial group.

The runout, the bottom of the avalanche where I hypothesize an increase of URM students, is the least desirable place to be relative to a person's life chances (as evidenced by the greatest lack of competition for this resource). Although one may think that out of higher education may be the least desirable place to end up there is empirical evidence that attending a for-profit college may be more damaging to the life chances of URMs than not attending higher education (Cottom 2017). One audit study found that there were no differences in getting an interview between someone with a high school degree and someone who attended a for-profit institution (Darolia et al. 2015). For-profit colleges are characterized by unethical recruiting practices, target vulnerable populations such as racial minorities and the poor (Dougherty et al. 2016, Lahr et al. 2014), provide low quality education, produce lower graduation rates, and

saddle students with more student debt and fewer job prospects than their peer institutions (Cellini and Koedel 2017, Gilpin and Stoddard 2017, Lynch, Engle and Cruz 2010, Schade 2014). Because this is the bottom of the avalanche I should once more be able to measure a change in URM enrollment. Thus, I hypothesize an increase in URM enrollment at for-profit schools. However, to ensure that the bottom of the avalanche is truly for-profit schools and not out of postsecondary education all-together I also conduct another analysis to measure the change in the share of URMs not-enrolling in college.

THE INSTITUTIONAL RESPONSE TO AFFIRMATIVE ACTION BANS

Aside from describing the vast impact of the bans across postsecondary education, the anti-affirmative action avalanche, I also weigh in on a current tension between mechanisms within the literature on affirmative action bans concerning when they may have been the most impactful. Some scholars have argued that savvy politicians and administrators at colleges and universities have been able to find workarounds to the affirmative action bans to achieve the goals of diversity and mitigate the impact of affirmative action bans (Berrey 2015, Mickey-Pabello and Garces 2018, Okechukwu 2019, Warikoo 2016), but others have pointed towards a deinstitutionalization of affirmative action which suggests that the impact of affirmative action bans may have increased over time (Hirschman, Berrey and Rose-Greenland 2016, Hirschman and Berrey 2017). Reframing the debate between the two mechanisms within the context of my theoretical model essentially asks if the anti-affirmative action avalanche was most impactful immediately after the passing of the bans, or if it has gained steam as time has passed. Although I do not directly observe the mechanisms taking place (neither the workarounds nor deinstitutionalization) my measure of the impact of the bans on URM enrollment over time

should point to the dominance of one institutional mechanism over another. To answer discern which mechanism prevails, I implement the modeling strategy used by Mickey-Pabello and Garces (2018) that measures if the bans were more effective immediately after their implementation or grew in strength as time passed. My measure does not dispute the existence of either mechanisms occurring (workarounds and deinstitutionalization). My estimates will find which institutional response to affirmative action bans has prevailed. If the bans are more effective as time goes on, then it can be interpreted that colleges are retreating from affirmative action instead of finding other means to achieve racial diversity. Conversely, if the impact of the bans has waned over time then, it means that colleges may be finding alternative paths to achieve racial diversity. As such, I repeat the same modeling strategy used by Mickey-Pabello and Garces (2018) for the very top and very bottom of the anti-affirmative action avalanche; the two places where I can detect the movement out of the most selective universities and movement into for-profit colleges and universities by analyzing the changes in URM enrollment by level of institutional selectivity.

DATA AND METHODS

Data

I use Integrated Postsecondary Education Data System (IPEDS) and the Current Population Survey (CPS) from 1991-2016. I take advantage of rich state-level variation across the implementation years of the bans in each state considered by implementing a difference-in-differences model to estimate the causal impact of the bans [CA 1997; TX 1997; WA 1999; FL 2001; MI 2007; NE 2009; AZ 2011; and NH 2012]. The time period from 1991 to 2016 maximizes the number of observations in an analytic sample and allows for timing lags to

investigate when effects started to take place or were most pronounced using a difference-in-differences analytic design. Including data from 2012 and after builds on the undergraduate affirmative action studies pioneered by Hinrichs (2012) and Backes (2012). However, as previously stated, this study expands on the aforementioned studies, by including lower-tier schools. The inclusion of these schools allows me to illustrate the impact of affirmative action bans across the entire landscape of postsecondary institutions for the first time.

I use a dummy variable to denote whether a student is a URM or not. URM students are coded as "1" if they are Black, Hispanic, or Native American. They are coded "0" otherwise. International students are classified as "Race/Ethnicity unknown" and are therefore classified as non-URM. This is how colleges and universities report data to IPEDS.[6]

The units of analysis in this study change based on the analysis. For most of the analyses in this paper my unit analysis is the institution because I am interested in looking in changes in the enrollment of URMs by various tiers of selectivity. However for one sensitivity analysis my unit of analysis is students. That analysis investigates if URMs were displaced entirely out of higher education in response to the affirmative action bans. The unit of analysis here are persons between the ages of 18-35. This age group best captures the group of people that is most likely to have ever been enrolled in or graduated from college. While 22-35 is frequently used as the college graduate group I included 18-21 year-olds because it is possible that these students could have attended a college or university. Even though 18-21 year-olds may not be old enough to have earned a degree they are included because they may have some postsecondary experience (i.e., "some college no degree". This analysis uses the Current Population Survey's Annual Social and Economic Supplement (ASEC) downloaded from the Integrated Public Use

Microdata Series (Ruggles 2019) to construct the dependent variable: no college (1= no college; 0= any college). I coded the educational attainment variable from this survey so that people with more than a high school degree would be coded as a 0 (i.e., 1-year of college, 'some college no degree,' or greater), and those with a high school degree or less would be coded as a 1. I reverse coded education to measure if URM students were displaced from postsecondary education altogether in the wake of affirmative action bans.

For most of the analyses I investigate how the share of URMs enrolled in postsecondary institutions changed differentially as a result of affirmative action bans. This question uses colleges as the units of analysis. The dependent variable here is the share of URM students enrolled according to the characteristics of the institutions. I investigate institutions' sector of education (2-year or 4-year, and private, public, and private for-profit) and level of selectivity to see where there share of enrolled URMs changes after affirmative action bans. To measure selectivity, I classify colleges by their Barron's Admissions Competitiveness Index. The categories are "Most Competitive;" "Highly Competitive;" "Very Competitive;" "Competitive;" "Less Competitive;" and "Noncompetitive." Because there were sometimes very few schools that fit into one category (e.g., there are only 8 public schools in the "Most" Competitive group) I recoded the categories "Most Competitive," "Highly Competitive," and "Very Competitive" as "Highly Selective"; and I recoded "Competitive," "Less Competitive," "Noncompetitive," and "Special" as "Less Selective"; and I include schools that were not indexed by Barron's Admissions Competitiveness Index by coding them as "Unclassified." Including the "Unclassified" category was of paramount importance to this study because omitting them removes 963 schools from the analysis (385 for-profit, 185 public, and 393 private non-for profit).

I include many state-level variables from the CPS (Current Population Survey) to control for the fact that states are not interchangeable units; they have unique characteristics of their own that may be influential to the analysis. These variables included state-level racial demographics (i.e., percentage of population that is White, Black, Latino, Native American, or other), state-level educational attainment for the question of the share of URMs enrolled only (i.e., the percentage of the population 25-34 years old with at least a bachelor's degree) and state-level economic indicators, including the unemployment rate of the population most susceptible to the impact of the bans upon graduation (25- to 34-year-olds) and personal income (also for 25- to 34-year-olds).[7] Due to the analytic strategy that I use, time invariant state differences will be controlled.

I show results weighted by enrollment (the number of students enrolled at each school) and unweighted (simply the share of URM enrollment at each school). The analysis weighted by enrollment yields results with respect to a typical student, while the unweighted analysis yields results that reflect the typical school. I have discussed the impact of the ban on students (i.e., what happens to the typical student) thus, I focus on the weighted results throughout this paper. Nevertheless, I also report on the non-weighted results.

Analytic Strategy: Difference-in-Differences-in-Differences

I use a difference-in-differences strategy to estimate the impact affirmative action bans have had on URM enrollment. This strategy has been used by many studies that examine the impact of policy changes on education outcomes (Dynarski 2004, Long 2004). It has also been specifically used by scholars who study bans on affirmative action (Backes 2012, Garces 2013, Garces and Mickey-Pabello 2015, Hinrichs 2012). This strategy has only been used sparsely by

sociologists (Flores 2017, Gangl 2010, Halaby 2004). My work expands on the work by affirmative action scholars by including the less glamorous schools that were overlooked by previous studies. By expanding the sample to the full-population universe of 4-year and 2-year institutions it allows me to show the impact of affirmative action bans across the entire landscape of postsecondary education. In this analysis the "first difference" in this strategy compares the proportion of URM enrollees before and after an affirmative action ban to determine whether changes accompany the start of the ban. If the affirmative action ban did have an impact on URM enrollment, there would be a change after the policies went into effect. However, because URM enrollment may differ from year to year for reasons other than the bans (e.g., changes in demographics or labor market conditions) this first difference may also reflect these other changes. Thus, a "second difference" is used to capture any external trends by taking advantage of a comparison group of people who lived in states where affirmative action bans were not implemented. Among people in states that did not prohibit affirmative action in a particular period, changes in enrollment over the same period are attributed to underlying trends rather than to the affirmative action bans. After subtracting the second difference from the first, an estimate of the causal impact of affirmative action bans on URM enrollment remains. The effects of affirmative action bans on URMs' probability on obtaining no post-secondary enrollment are estimated similarly.

I implement a difference-in-differences estimation strategy in a multilevel regression framework before applying difference-in-differences-in-differences (i.e., triple diff [explained later]), using a combination of fixed effects to account for the hierarchical nature of the data (Murnane and Willett 2011). I emphasize that this does not produce two separate analyses: one at the state level and the other at the institutional level. The two levels in this hierarchical model

emphasize that the bans occurred at the state-level, but that the changes in URM enrollment occur at the institutional-level (i.e., the units of analysis). As such the state level corresponds to the mechanism of *racial self-interest at the state-level* and the institutions correspond to the mechanism of *organizational racialization*. I use state-clustered standard errors to account for residual correlation not eliminated by state fixed-effects alone or robust standard errors (Bertrand, Duflo and Mullainathan 2004).

I fit the following multilevel ordinary least squares regression:

$$Enroll_{ist} = \beta_0 + \beta_1(BAN_{st}) + \beta_2 W_{st} + \beta_3 cyear_t + \gamma S_s + nScyear_{st} + \alpha Z_t + \varepsilon_{ist} \quad (1)$$

where $Enroll_{ist}$ indicates the proportion of URMs enrolled at time (t)[8]; BAN_{st} is a dichotomous variable indicating whether a state (s) had an affirmative action ban in place in year (t); W_{st} represents a matrix of selected time-varying state characteristics designated above; S indicates a set of vectors to distinguish among the states and to control for all time-invariant differences, both observed and unobserved, among the states (state fixed effects); $cyear$ represents a continuous-year variable (coded so that 1991=1, 1992=2, 1993=3, etc.) to capture linear trends in time; $Scyear$ represents a full set of two-way interactions between each state dummy and a continuous predictor representing the linear effect of year; Z_t represents a set of vectors for years to distinguish among the chronological years to which the bans apply, and to account for average differences in the outcome across the chronological years covered in the data (year fixed effects), which include the years 1991 to 2016; and ε_{ist} represents the residual.[9] Fixed effects and linear trends were both used because fixed effects capture the year-specific changes and national trends and the linear trends capture state-specific trends; they do not create a collinearity issue. Because

of the presence of the state and year fixed effects, β_1 provides the required difference-in-differences estimate of the impact affirmative action bans have had on the share of URM enrollment.

A further analytic step is taken to produce difference-in-differences-in-differences estimates (Ravallion et al. 2005). The triple difference analysis is useful in estimating heterogenous differences to the affirmative action bans by group. It emphasizes the difference between URMs and non-URMs caused by affirmative action bans. I implement the triple difference analysis when I conduct a sensitivity analysis to see if URMs did not enroll in college as a result of affirmative action bans. The difference-in-differences-in-differences estimate is specified:

$$NoDegree_{ist} = \beta_0 + \beta_1(BAN_{st}) + \delta(BAN_{st}URM_i) + \beta_2 W_{st} + \beta_3 URM_i$$

$$+ \beta_4 cyear_t + \gamma S_s + nScyear_{st} + \alpha Z_t + \varepsilon_{ist} \qquad (3)$$

where δ is introduced as the difference-in-differences-in-differences estimator for an interaction between a person's racial group (URM_i) and a ban being present in a state given a particular year.

I also adjust the analytic window of the ban to determine when the impact of the bans was strongest (i.e., restricting the ban to just 2 years of influence instead of 4). This analysis shows which institutional response to affirmative action bans was dominant: workarounds to the bans that find alternative paths to racial diversity (Berrey 2015, Mickey-Pabello and Garces 2018, Okechukwu 2019, Warikoo 2016) or the deinstitutionalization of affirmative action (Hirschman, Berrey and Rose-Greenland 2016, Hirschman and Berrey 2017). A paper by

Mickey-Pabello and Garces (2018) found that the impact of affirmative action bans on medical school admissions was strongest immediately after the bans. They came to that conclusion by running difference-in-differences models and changing the length of the post-ban period by year increments. In my main specification, I do not restrict the post-ban window at all. However, in my sensitivity analysis I restrict the post-ban window to 4-, 3-, 2-, and 1-year windows. If the coefficient from the shorter windows is larger than the full analytic window or the 4-year analytic window, then the bans were more impactful most recently after their implementation giving credit to the workarounds to diversity mechanism. Conversely, it the coefficient from the shorter windows is smaller than the full analytic window or the 4-year analytic window, then the bans have grown in strength over time giving credit to the deinstitutionalization of affirmative action mechanism.

Parallel Trend Assumption and Statistical Power

An important assumption of the difference-in-differences approach is 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. This is known as the parallel trend assumption (herein PTA). It is imperative to assess the PTA because violations of it point to a lack of controls in the model. 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. This is 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.

Using an event history specification (Kaestner et al. 2017, Sommers et al. 2015) I investigate these trends to ensure 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. This specification takes into consideration the variability in the timing of the bans and the assignment of the treatment (i.e., which states have the bans). The 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. To reject 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. I present the results from various event history specifications in appendix A. The model presented in appendix A 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.[10] For instance, when state-level controls for the racial distribution of the population are not included in the model (e.g., the Black 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. Furthermore, even though

I controlled for selectivity using the Barron's classification, there were still potential violations of the PTA but including standardized testing scores reduced potential violations of the PTA, particularly for the schools in my highest selectivity tier. However, because standardized testing scores are not required across all selectivity tiers it becomes an important reason for why my analysis of institutions is stratified and why I do not use a triple-difference design to estimate selectivity-tier heterogeneity. In other words, there are theoretically distinct explanations for what variables matter at each selectivity tier. The stratified models are need because the more selective schools require an model that controls for standardized testing, but the less selective schools do not require the same model.

Generally, I find that 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 and two-year schools that can be ignored because those main findings were not statistically significant. This supports 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, is that I report 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 this could be explained by anticipation effects. These violations suggest that schools may have admitted more URMs prior to the bans in order to align its institutional policy prior to the bans. 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 one year because the University of California Board of Regents voted to do so. The violations here suggest that the

negative impacts of the bans I report in the main results should be smaller in magnitude, and that the ones reported for schools of the highest selectivity are most likely an overestimate.

FINDINGS

I first present findings about whether or not URM students were displaced entirely out of higher education due to affirmative action bans (here students are the units of analysis, because out of school is not an institution). Then, I present findings about the impacts of affirmative action bans across several sectors (public, private, and private for-profit) and selectivity tiers of postsecondary education. Collectively the description of these institutional types illustrates the anti-affirmative action avalanche giving credence to *racial self-interest at the state-level* and *organizational racialization*. Finally, I show if the impact of the bans has waned or gotten stronger over time to resolve if finding workarounds to affirmative action ban in the name of racial diversity or the deinstitutionalization of affirmative action has been the prevailing institutional response mechanism to bans on affirmative action.

Table 2 pertains to the share of URMs not attaining any postsecondary education. The first panel (the top half) presents differences in differences and the second panel (the lower half) presents the difference in difference in differences results. Overall, these results support the hypothesis that affirmative action bans have increased the probability that a URM would enroll in college. The differences in differences results (Panel A) indicate that for all people (both URMs and non-URMs grouped together) there is no statistically significant difference in attainment due to the ban (0.0002 is not significant), but the classic relationship between being a URM and not enrolling in college degree holds (0.1176 is significant). However, the triple difference (i.e., the interaction between URM status and the ban) in Panel B tells us how URMs were impacted by the bans relative to non-URMs. The coefficient here is negative (-0.0436),

meaning that affirmative action bans have increased the college-going rates of URM. Remember, this analysis was designed to answer if URM students had retreated from college entirely due to affirmative action bans. I find that the antithesis is true, meaning that the bottom of the anti-affirmative action avalanche is not out of postsecondary education. Because this coefficient is difficult to interpret, I computed a percentage change by using the mean of the dependent variable in all of the ban states prior to the ban and the estimated causal impact of the ban. Applying a traditional percentage change formula thus provides a heuristic for measuring how much the bans changed URM college enrollment. This heuristic result suggests that there was an 11.83% decrease in not attending college [i.e., a 11.83% increase in attending any postsecondary institution]. Therefore, the conclusion is that URM students, relative to non-URM students, were more likely to enroll in college due to affirmative action bans. However odd, this result is later supported by the finding that the share of URMs at for-profit colleges increased due to the bans, meaning that the recruitment of URM students to for-profit colleges in the wake of affirmative action bans may in part explain why there is an increase in the share of URM students enrolling in college.

Apart from the ruling out other possible explanations for the impacts of affirmative action bans with the parallel trend assumption, I took a further step here to include a spuriousness check to give greater confidence to these findings. I substitute another variable in the place of URM status (here I use sex). The rationale is that enrollment by sex should not be impacted by these race-based anti-affirmative action policies, but enrollment by race should be impacted. To ensure that the sex-ban estimate is not biased by those identifying as URM and female, a group that is more likely to attend postsecondary education than their male URM counterparts, URM status is included as a control in the model. Therefore, if the sex-ban triple differences estimator is

statistically significant then spuriousness cannot be ruled out (i.e., my finding that the share of URMs at for-profit colleges increased due to the bans would be questionable). My spuriousness checks indicate that triple difference coefficient is not spurious (the interaction effect for sex is not significant); hence these findings give greater support to my finding that URM educational attainment was impacted by affirmative action bans.[11]

<<INSERT TABLE 2 HERE>>

In Table 3 I present the findings pertaining to how affirmative action bans impacted the share of URM enrollment (i.e., # of URMs/ total enrollment at each school) in various sectors of postsecondary education. These results largely flesh-out the anti-affirmative action avalanche the chain-reaction set-off by state-level bans on the practice of affirmative action at public institutions of postsecondary education. I remind the reader that my weighted analysis reflects what happened to the typical student and the non-weighted analyses reflects what happened at the typical institution. Because not all schools have the same number of students the weighted results may provide the more practical understanding about how many students are avalanched. Furthermore, efforts to rule out alternative explanations for the impact of the bans are addressed by the PTA in Tables A and B in the appendix; the findings in Table 3 are presented after a rigorous evaluation of the PTA. In accord with the hypothesis, affirmative action bans have increased the share of URM enrollment at for-profit colleges (by a share of 0.05 or 5% for the weighted findings). Public and private schools' shares of URM enrollment did not change (0.00 and 0.01 respectively). I reinterpret these findings as a percentage change to facilitate their understanding. These results indicate that the public and private nonprofit schools' enrollments decrease by 0.13% and 0.21% (both not statistically significant), but for-profit schools increase by 17.62% in their share of URM enrollment. These findings suggest that when considering

private schools and public schools as a group (not stratified by selectivity) there is no general effect on the share of URM enrollment. This is consistent with findings by Backes (2012) and Hinrichs (2012) because they claim that only more highly selective schools (both public and private) are impacted by affirmative action bans. Backes (2012) and Hinrichs (2012) both acknowledge that the affirmative action bans, may have had spill-over effects from their *de jure* targets (public schools) to *de facto* private schools that were not impacted by the bans. Hirschman and Berry (2017) speculate that the spillover effects may be attributable to the fear over long and costly legal battles like the University of Michigan, University of Texas, Harvard University, and the University of North Carolina have all endured.

Stratifying the 4-year institutions by selectivity allows for the detection of movement along the anti-affirmative action avalanche. 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 at the bottom of the avalanche which is consistent with Backes (2012) and Hinrichs (2012). At the bottom of the avalanche I introduce a novel finding that there is a statistically significant decline when considering all for-profit schools (i.e., when the unclassified schools and the less selective schools are grouped together as any school of this type).

<<INSERT TABLE 3 HERE>>

In Table 4 I present similar findings, but for 2-year institutions. They show no significant effects of the ban. Thus, they endorse Backes's (2012) pervious findings that affirmative action bans do not impact 2-year institutions. They are also consistent with the idea that 2-year institutions are part of the middle part of the affirmative action ban cascade, where no

discernable movement of URM students occurs because the students that leave those schools are presumably replaced by students that were displaced from further up on the cascade.[12]

<<INSERT TABLE 4 HERE>>

WHAT DOES THE ANTI-AFFIRMATIVE ACTION AVALANCHE REALLY LOOK LIKE?

The results of this study, particularly the findings regarding the less selective for-profit institutions, the missing puzzle-piece, finally completes the puzzle and illustrates the anti-affirmative action avalanche for the first time. It has finally answered the question: "Where did all the URMs go after they were displaced from the most selective schools?" Previous findings by Backes (2012) and Hinrichs (2012) indicated that affirmative action bans decreased the share of URM students at public and private 4-year non-profit institutions, but overlooked many of the less glamorous schools included in this study. Backes (2012) also found that there was no change in the enrollment pattern of URMs at 2-year institutions. My results support previous findings that URMs where displaced from the most selective institutions, and previous findings that URMs were not necessarily filtered into private schools, but also impact highly selective private schools (Backes 2012, Hinrichs 2012). I also confirm the pattern of spill-over effects (from public schools to private schools) in the wake of the bans found by both Backes (2012) and Hinrichs (2012). My results not only confirm these results but nuance them by including for-profit schools in addition to private non-for profit and public regarding sector and including many less-selective schools (i.e., schools without a Barron's classification) when investigating the role of selectivity. Furthermore, I also include results for people that never went to college to show that "no postsecondary education" was not the bottom of the anti-affirmative action avalanche and find that there was an increase in postsecondary enrollment of URMs attributable to the bans, a new finding in the literature on affirmative action bans.

More importantly, I finally answer the question of where URMs were displaced as a result of affirmative action bans by including for-profit private schools. Furthermore, because there was a positive coefficient for the causal impact of affirmative action bans on for profit schools generally in Table 3, while there were no such results for public or private schools, I speculate that the increased enrollment of the typical URM student in postsecondary education may have been driven, in part, by their increased enrollment in for-profit schools. An investigation into any recruitment policy changes at for-profit schools in the wake of affirmative action bans is certainly warranted.

Regarding the mechanism of *racial self-interest at the state-level* my findings confirm that the state-level bans have preserved the educational self-interests of non-URMs in states with bans, and have decreased the educational attainment potential of URMs by decreasing their representation at the most selective schools, and increasing their representation at the bottom of the anti-affirmative action avalanche- for profit schools. In argue that opportunity hoarding (Tilly 1998) is achieved *via* the legal system, when affirmative action bans are used as a lever to distribute resources and opportunities in such a manner that is self-serving to those racial groups already in power and excludes those who are not in power. Baker (2019) confirms why the lever was created; finding that the bans were passed to preserve the racial self-interests of non URMs in education. My findings confirm that the lever worked; finding that the lever (i.e., the affirmative action bans) preserved the educational self-interests of non-URMs. Under the guise of colorblind meritocracy that the architects of the bans built them upon these affirmative action bans exemplify the extent to which *laissez faire* racism operates in our institutions of post-secondary education. The affirmative action bans obscure the political powerbrokers that rallied to implement the bans which are imbedded in the faceless and often invisible structures of our

college and university admissions system. The anti-affirmative action avalanche thus serves to illustrate why the myth of equal opportunity in education is not realized with respect to race: underrepresented students of color are being filtered from institutions where they can prosper and have greater returns to their education into institutions that are generally described as predatory and leave them saddled with a low quality education and high amounts of student debt. Their misfortune, the byproduct of the efforts of elites to reserve seats at the finest institutions for their progeny.

The anti-affirmative action avalanche was created as a result of schools in ban states forcibly jettisoning color-conscious affirmative action, engaging in *organizational racialization* when they applied new color-blind meritocratic schemas about race that are tied to organizational resources (i.e., being admitted to college). At the top of the avalanche, the starting zone, is the most selective 4-year undergraduate education, then the track, the middle part of the avalanche (e.g., less selective 4-year institutions and 2-year institutions), and towards the bottom of the avalanche, the runout, is a confluence of those outside of higher education altogether and those attending for-profit universities. I found that affirmative action bans had a decline of URMs at the top of the avalanche, no change along the track, and saw an increase of URMs at the bottom of the avalanche.

I also ruled-out that the anti-affirmative action avalanche would end with URM students being removed from higher education altogether. Conversely, the probability of a URM student enrolling at a postsecondary institution increased, and the anti-affirmative action avalanche ends with URM students being displaced into for-profit universities. The theory I posited suggested that URMs would be displaced from the starting zone of the avalanche and resurface at the

runout zone, where their life-chances are the worst and there is the least amount of competition. There is evidence in the literature on for-profit colleges to support this position.

It is a stark finding that URM representation has increased at for-profit colleges and universities in the wake of the bans. Attending a for-profit college may be more damaging to the life chances of URMs than not attending higher education (Cottom 2017). For-profit colleges are characterized by unethical recruiting practices, target vulnerable populations such as racial minorities and the poor (Dougherty et al. 2016, Lahr et al. 2014), provide low quality education, produce lower graduation rates, and saddle students with more student debt and fewer job prospects than their peer institutions (Cellini and Koedel 2017, Gilpin and Stoddard 2017, Lynch, Engle and Cruz 2010, Schade 2014).

CONCLUSION

The anti-affirmative action avalanche illustrates how affirmative action bans have redistributed URM students from educational opportunities that are associated with better life chances to a path where their life chances are lower. The starting zone of the avalanche was created by the adoption of the policy to preserve the racial power structure through colorblind meritocracy. It ultimately prevents some underrepresented students of color from obtaining elite educational credentials that could potentially shift the racial power structure of the United States. Further down the avalanche some URM students have fallen harder than anticipated because so many enrolled at for-profit universities as the result of affirmative action bans. This may also be more damning to the students that otherwise would not have enrolled in college, because many studies have evidenced that there are no differences in the returns to education for someone with a high school degree and someone who attended a for-profit institution (Darolia et al. 2015).

This work has the potential to impact other studies of higher education, policy, organizations, and contemporary racism. Specifically, my study sheds light on how a racialized policy that is tied to the distribution of resources ultimately produces racial inequality under the banner of colorblind racism; where the consideration of race in admissions is altogether removed and replaced by meritocracy. As such, this study advances sociological thinking on how group position theory (Bobo and Hutchings 1996, Tilly 1998), and structural racism (Bobo, Kluegel and Smith 1997) come together through the creation and adoption of racially charged policies to disrupt the myth of equal opportunity.

Within sociology of education there is also the strong possibility that work around matching (Alon and Tienda 2005, Sander and Taylor Jr 2012) could be revisited. The classic argument from the anti-affirmative action contingent contends that there is a "mismatch" of students and institutions (Graglia 1993; Sowell 2003; and Thernstrom and Thernstrom 1997; Clegg and Thompson 2012; Sander and Taylor 2012). "Mismatch" is classified by two types: "over-match" and "under-match." "Over-match" occurs when students (most commonly minority students) that typically have lower credentials on average (i.e., lower ACT, SAT, and GPA) than the institutional average are "over-matched" to selective institutions because affirmative action allows them to be admitted despite their lower academic qualifications. By contrast, "under-match" is the phenomenon when students that typically have higher credentials on average (i.e., higher ACT, SAT, and GPA) than the institutional average are "under-matched" to selective institutions because their academic qualifications a higher than those of their peers at the university where they are attending. The matching literature has always cited affirmative action as a mechanism that drives matching, but now that there is variation in the mechanism of

affirmative action empirical tests can be done instead of relying on *a priori* arguments to frame how students are overmatched and undermatched in the presence of affirmative action.

In addition to the theoretical impacts of this work there are also real-life impactions of this research. These findings further support that affirmative action bans are negatively impacting the life chances of URMs. Reversing bans on affirmative action could help limit for-profit schools from enrolling as many undergraduate racial minority students, and direct them back toward public and private schools that can provide them with a better education, job prospects, and financial support. In particular, steering socioeconomically vulnerable URM students away from for-profit colleges and toward quality community colleges would better serve them. Policy makers and the U.S. court system could also make better-informed decisions about affirmative action bans by understanding the more complete breadth of impacts of affirmative action bans on the system of U.S. postsecondary education. This paper has shown that affirmative action is not only about the battle for the most coveted seats in the ivory tower, but also about the students and schools at the other end of the distribution who are all too often forgotten.

ENDNOTES

[1] For a more detailed understanding about which states adopted affirmative action bans see Baker (2019).

[2] This group is defined in greater detail in the data section, but is comprised of African American, Hispanic, and Native American students.

[3] More explanation about the differences between ban and non-ban states are discussed in the data and methods section where the parallel trends assumption is discussed in great length. This assumption underscores the importance of ruling out alternative explanations that could account for differences between the ban and non-ban states on the dependent variables.

[4] There are three parts to an avalanche 1) the starting zone (top) 2) the track (middle) and 3) the runout (bottom).

[5] Student-level data that would be required to show movement from one tier to another is not available. Only a robust data set spanning several years and several states with information about student college choice would be able to answer questions about how many students moved from one tier to another. The data presented here reaches its limit by indicating the changes within each tier. By showing aggregate changes I provide evidence for the existence of the mechanisms, even if the data is unable to show the extent of how the mechanisms function.

[6] This data excludes international students who some may believe to be URMs (i.e., a foreign student from Mexico with no U.S. citizenship or permanent residency status).

[7] The bachelor's degree attainment state level control was not included in the models with 'no college' as the dependent variable and the control variable are too similar.

[8] *i* refers to the individual person when considering any postsecondary enrollment as the dependent variable (CPS data) and refers to the school when considering the share of URM enrollment as the dependent variable.

[9] This specification of the multilevel model uses fixed effects to account for the nesting of observations at the state level (Murnane and Willet 2011). The presence of the state fixed effects in the model accounts for the nesting of observations within a state.

[10] I present this model in the appendix and not in the body of the paper so that readers will read the primary findings first. I emphasize that this should be the proper order because the difference-in-differences coefficients in the main findings are reinterpreted so that a casual reader unfamiliar with difference-in-differences may interpret the findings.

[11] I do not produce spuriousness checks for the stratified difference-in-difference results because more than a hundred difference-in-differences models were run to produce the estimates in Tables 3, 4, A, and B. Because so many results were produced that converge to a similar pattern of findings the threat of spuriousness is not as sever in those models as it was in Table 2, where the unit of analysis was people and not institutions.

[12] Again, due to the unavailability of student-level data for all states across the 25+ years considered in this study I cannot pin-point how many URM students may have been displaced from one tier to the next.

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Table 1. The Enrollment Share of Blacks and Latinos After Affirmative Action Bans

	1997	1998	2013
Black students as a percentage of CA graduating seniors	7.7%	7.5%	6.4%
Black students as a percentage of UC Berkeley enrollees	7.8%	3.7%	3.8%
Latino students as a percentage of CA graduating seniors	30.5%	31.1%	47.1%
Latino students as a percentage of UC Berkeley enrollees	14.6%	8.0%	17.0%

Source: The Daily Californian (secondary source) *via* National Center for Education Statistics

Table 2 The Impact of Affirmative Action Bans on URMs not Having College Education

Panel	Difference in Differences	Ban	Sig.	URM	Sig.	Interaction	Sig.	N=	# of Groups
A	Impact of Ban	0.0002(0.0026)		0.1176(0.0012)	***			1,057,316	99,167
	Spuriousness Check (sex)	0.0026(0.0030)		0.0430(0.0014)	***			1,057,316	99,167
	Difference in Differences	Difference in Differences							
B	Interaction Effect	0.0047(0.0026)		0.1230(0.0013)	***	-0.0436(0.0038)	***	1,057,316	99,167
	Spurious Interaction Effect (sex)	0.0035(0.0030)		0.0440(0.0015)	***	-0.0086(0.0045)		1,057,316	99,167

Note: N= the number of person-state-years, # of Groups is the number of persons. * p <0.05, ** p<0.01, and *** p<0.001

Table 3. The Impact of Affirmative Action Bans on the Share of URM Enrollment at 4-Year Colleges

Share of first year URM students enrolled		For-Profit		Public		Private non-Profit	
		Ban(S.E.)	Sig.	Ban(S.E.)	Sig.	Ban(S.E.)	Sig.
Any Selectivity	Weighted	0.0515(0.0230)	*	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	
Barron's Highest Selectivity	Weighted	N/A		-0.1028(0.0099)	***	-0.0285(0.0184)	
	Unweighted	N/A		-0.1036(0.0113)	***	-0.0220(0.0089)	*
	N=	0		200		1,785	
	# of Schools	0		8		72	
Highly Selective	Weighted	N/A		-0.0129(0.0118)		-0.0075(0.0084)	
	Unweighted	N/A		-0.0144(0.0126)		-0.0105(0.0058)	
	N=	0		3,118		8,207	
	# of Schools	0		125		334	
Less Selective	Weighted	0.1655(0.2071)		-0.0117(0.0142)		0.0068(0.0218)	
	Unweighted	-0.0072(0.0723)		-0.0139(0.0172)		-0.0059(0.0138)	
	N=	495		9,284		13,914	
	# of Schools	20		382		572	
Unclassified	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	

Table 4. The Impact of Affirmative Action Bans on the Share of URM Enrollment at 2-Year Colleges

Share of first year URM students enrolled		For-Profit		Public		Private non-Profit	
		Ban(S.E.)	Sig.	Ban(S.E.)	Sig.	Ban(S.E.)	Sig.
2-Year Institutions	Weighted	0.0066(0.0142)		-0.0158(0.0105)		-0.0224(0.0365)	
	Unweighted	0.0160(0.0146)		-0.0126(0.0094)		-0.0098(0.0188)	
	N=	13,955		22,784		3,062	
	# of Groups	891		984		177	

APPENDIX A

Table A. Event History Specification for the Parallel Trend Assumption at 4-Year Schools

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	0.0109(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=	0		200		1,785	
	# of Schools	0		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=	0		3,118		8,207	
	# of Schools	0		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	

APPENDIX B

Table B. Event History Specification for the Parallel Trend Assumption at 2-Year Schools

Share of first year URM students enrolled at 2 year institutions		For-Profit Ban(S.E.)	Sig.	Public Ban(S.E.)	Sig.	Private Ban(S.E.)	Sig.
Main Results	Unweighted	0.0188(0.0146)		-0.0111(0.0092)		-0.0098(0.0194)	
Placebo Bans Occur 4 Years Before	Unweighted	-0.0071(0.0103)		-0.0041(0.0086)		-0.0494(0.0305)	
Placebo Bans Occur 3 Years Before	Unweighted	-0.0162(0.0106)		0.0049(0.0072)		-0.0574(0.0269)	*
Placebo Bans Occur 2 Years Before	Unweighted	-0.0223(0.0109)	*	0.0067(0.0069)		-0.0371(0.0207)	
Placebo Bans Occur 1 Years Before	Unweighted	-0.0107(0.0124)		0.0073(0.0067)		-0.0253(0.0214)	
	N=	13,955		22,784		3,062	
	# of Groups	891		984		177	

[1] For a more detailed understanding about which states adopted affirmative action bans see Baker (2019).
[2] This group is defined in greater detail in the data section, but is comprised of African American, Hispanic, and Native American students.
[3] More explanation about the differences between ban and non-ban states are discussed in the data and methods section where the parallel trends assumption is discussed in great length. This assumption underscores the importance of ruling out alternative explanations that could account for differences between the ban and non-ban states on the dependent variables.
[4] There are three parts to an avalanche 1) the starting zone (top) 2) the track (middle) and 3) the runout (bottom).
[5] Student-level data that would be required to show movement from one tier to another is not available. Only a robust data set spanning several years and several states with information about student college choice would be able to answer questions about how many students moved from one tier to another. The data presented here reaches its limit by indicating the changes within each tier. By showing aggregate changes I provide evidence for the existence of the mechanisms, even if the data is unable to show the extent of how the mechanisms function.
[6] This data excludes international students who some may believe to be URMs (i.e., a foreign student from Mexico with no U.S. citizenship or permanent residency status).
[7] The bachelor's degree attainment state level control was not included in the models with 'no college' as the dependent variable and the control variable are too similar.
[8] *i* refers to the individual person when considering any postsecondary enrollment as the dependent variable (CPS data) and refers to the school when considering the share of URM enrollment as the dependent variable.
[9] This specification of the multilevel model uses fixed effects to account for the nesting of observations at the state level (Murnane and Willet 2011). The presence of the state fixed effects in the model accounts for the nesting of observations within a state.
[10] I present this model in the appendix and not in the body of the paper so that readers will read the primary findings first. I emphasize that this should be the proper order because the difference-in-differences coefficients are reinterpreted so that a casual reader may interpret the findings.
[11] I do not produce spuriousness checks for the stratified difference-in-difference results because more than a hundred difference-in-differences models were run to produce the estimates in Tables 3, 4, A, and B. Because so many results were produced that converge to a similar pattern of findings the threat of spuriousness is not as sever in those models as it was in Table 2, where the unit of analysis was people and not institutions.
[12] Again, due to the unavailability of student-level data for all states across the 25+ years considered in this study I can not pin-point how many URM students may have been displaced from one tier to the next.