A Structural Investigation of Laissez Faire Racism: The Intended and Unintended Consequences of Affirmative Action Bans

by

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subfield" needs to be seriously considered because it informs classic sociological debates, and show why it is a mistake to overlook it. This subtle conversation led to an "Ah-ha!" moment that inspired the theoretical reframing of my dissertation.

Next, I thank my dissertation committee for the face-to-face meetings, and countless reviews and suggestions they have made throughout this process. It was frustrating at times to write this dissertation because no one on the committee was a subject matter expert on affirmative action nor sociology of education, but as strong sociologists and social scientists they had the intuition to know that my work was not ready; it did not feel right. There are so many times when I wish I could have been directly led to the right answers, particularly in my early attempts to theoretically frame this dissertation, and to be honest, I sometimes felt that I was being hazed because I was trying hard, but somehow falling short of their lofty expectations. I chose my dissertation committee because they really cared about my welfare, wanted to see me succeed, and were committed to having me realize my true potential as a scholar. Ultimately, I have prevailed- largely because they wanted the best for me and would not let me settle for anything but the very best. The quality of this dissertation is a testament of their belief in me and their relentlessness to make me a better scholar.

My dissertation chair, Sarah Burgard was instrumental to the existence of this dissertation. Very early on in the process there were questions about subject matter of my dissertation. Some people felt that my work was too narrow, felt more like public policy than sociology, that my dissertation was just a subfield of a subfield, and that I should focus on sociology of education more broadly. However, I was stubborn, and saw that my dissertation held much more promise and ability to contribute to mainstream sociological thinking- although I had trouble conveying those points before I wrote much of the dissertation. I do not know how

convincing I was to Sarah early on in the process, but I cannot thank her enough for the diplomacy she showed from the early stages of the process to the end. She was willing to let me live or die by my own sword, let me fight for my ideas, and wrestled the control of the dissertation back into my hands. While I was writing my dissertation, my funding was also starting to run out. Sarah let me pursue a 9-5 job, because I begged her that I needed it, and that I would quit immediately if it ever got in the way of writing my dissertation. It meant pulling a double-shift almost every weekday for two years and working on weekends. While this decision was wildly unpopular with my committee Sarah again let me be in control of my own fate. I cannot thank her enough for letting this be *my* dissertation and for helping me persevere through the many challenges I faced along the way.

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Barbara Anderson and I first met during my sophomore year at the University of Michigan when I enrolled in Sociology 430, an introductory course in demography. I enrolled in the class because I was working on project at the National Forum on Higher Education for the

Public Good where I felt like I was producing a lot of demographic data but had no training in demography whatsoever. I never knew how fortuitous enrolling in that class would be. When I was a master's student, I enrolled in Barbara's graduate level demography two-course sequence. I'm not sure if Barbara knows this, but my experience with demography and her course helped me make the decision that I would apply to sociology programs for my PhD instead of higher education programs where I had a masters. Barbara was also instrumental in helping me land an internship at the U.S. Census Bureau where I had my first work experience as a formal demographer. Although, much of my work to date would fall under the label of sociology of education or sociology of race, Barbara also influenced the content of my dissertation in a meaningful way. When I was first ruminating about ideas for my dissertation I first thought of affirmative action, but the pivotal moment was when I asked myself "How can my training as a sociologist and a demographer contribute to our understanding of affirmative action?" It was then that I came up with the idea for the fourth chapter in this dissertation where I connect affirmative action bans to a decline in interracial marriage. It is safe to say that without Barbara's influence what I feel is the most important chapter of my dissertation never would have been written.

When I was a master's student at the Center for the Study of Higher and Postsecondary Education at the University of Michigan, I decided to enroll in the PhD level statistics course sequence to see if I had the ability to be a competitive students and complete that coursework at the graduate level. I enrolled in Steve DesJardins's Education 795 course, where I first learned about regression techniques and STATA, and Education 799, where I learned categorical and limited dependent variable modeling. After my first semester of my freshman year of college I had resigned myself from quantitative coursework after a bad experience with Calculus. Steve convinced me that I had thrown the towel in too early. Steve took me under his wing and inspired me to strengthen my foundational skills in statistics. As a PhD student in sociology I backtracked some to take Calculus II, Calculus III, Matrix Algebra, and take graduate coursework in inferential statistics in the Department of Biostatistics. This heavy dose of quantitative training made me a good graduate student instructor under the aegis of Deirdre Bloome and gave me a great foundation to learn new statistical methodologies. Even in the early stages of my dissertations process my committee most likely agreed that my methodological skills were strong. Much of this was attributable to Steve DesJardins, and the quality of his instruction and mentorship. Steve also suggested that I include triple diff in my dissertation.

Lastly, I owe perhaps the most tremendous amount of gratitude to John Burkhardt. Without him I probably never would have written a dissertation, let alone gone to graduate school. Some of those who have never met John may have questioned why he, a white man, was the figurehead of the National Center for Institutional Diversity for several years. The truth, however, is that John has been a better mentor to students of color than any faculty member of color I can think of. John is a bastion of social justice, and an antithetical aberration to the white male stereotype that "they are in it for themselves and not the betterment of society."

When I first met John I was a sophomore at the University of Michigan working at a restaurant on North Campus. Our very first encounter was at Carson's American Bistro where I worked. In between the time that I had written him a letter asking to work with him and the time when we had our first formal meeting at his office he came into the restaurant by happenstance. Perhaps it was a foreshadowing that I, his server on that evening, would be served by him and learn so many lessons about servant leadership by him and the Legendary Musicians of the National Forum on Higher Education for the Public Good. I had written a letter to him and several University of Michigan faculty members at the Center for the Study of Higher and Postsecondary Education asking them if they had any interest in an undergraduate student working with them. There is no undergraduate concentration in higher education, but I wanted that experience because I had spent a lot of time in student leadership positions and had learned that many of them had master's degrees in higher education. No one, but John, responded to my email. Ironically, it was my "Dear John" letter that would lead to a cherished friendship, and meaningful mentor-mentee relationship (although John would surely turn the tables and say he learned more from me than I ever did from him). I cannot begin to thank John enough for all he has done for me over the past decade. Even though I left the higher education program for sociology several years ago John and I have met nearly every semester, and he has invited me to his holiday gathering every year (although I have not been able to attend many times to be with family).

I have dreamed of this moment for so long so I could finally thank everyone else that has made an investment in my education. Thank you so much for helping to make my dream a reality. It is because of you that I can now go out into the world and apply my scholarship and leadership to ensure that the American Dream and path to upward social mobility is available for marginalized populations.

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Abstract

This dissertation frames affirmative action bans as the policy manifestation of colorblind, *laissez faire* racism; a term describing the hidden and often unintentional nature of racism. I explore several outcomes of affirmative action bans using difference in differences methodologies to test hypotheses that the racialized control of education ultimately leads to racial inequality in educational outcomes and later life. Chapter 2, "The Affirmative Action Ban Cascade: Where Students Enroll After Affirmative Action Bans," finds that affirmative action bans have not only displaced underrepresented minority students from the most selective institutions, but they have also created a complex 'cascade' effect wherein underrepresented minority students have increasingly enrolled at for-profit colleges. Chapter 3, "The Machinations of Laissez Faire Racism in Action: Differences in Underrepresented Student STEM and non-STEM Degree Attainment in the Wake of Affirmative Action Bans," finds that underrepresented minority student completion of undergraduate degrees in STEM was impacted more by affirmative action bans than their completion of undergraduate degrees in non-STEM majors. Because a degree in a STEM field is more prestigious and competitive than one in a non-STEM field, this result is consistent with the hypothesis that competition for coveted resources leads to reduced opportunities for underrepresented minority students. Chapter 4, "The Unintended Consequences of Affirmative Action: How Affirmative Action Bans Have Shaped Interracial Marriage," finds that affirmative action bans have decreased interracial marriage; largely as a result of a decrease in White and Asian intermarriage. Together the chapters of this dissertation examine how *laissez faire* racism operates through affirmative action bans to produce racial inequality.

Chapter 1: Introduction

Competition for Selective Education Comes to a Crossroads with Affirmative Action

The attainment of postsecondary education credentials has long been associated with upward social mobility (Arrow 1973, Spence 1978, Dale and Krueger 2002, Bills 2004, Brand and Xie 2010, Dale and Krueger 2014). Arrow (1973) and Spence (1978) argued that educational institutions provide a credential to their alumni that is communicated and signaled to potential employers. Sorokin (1959) best describes the mechanisms underpinning Arrow's signaling theory when he writes that schools "test the abilities of individuals, which sifts them, selects them, and decides their prospective social position." Dale and Kruger's studies on the returns to education (2002 and 2014) confirm that the more educated a person is, the higher their income is. Furthermore, they find that more selective schools (i.e., more competitive schools) yield higher returns to education. The American public has also viewed higher education as a crucial ingredient for achieving the American Dream for several decades, but that has in turn made it more competitive and sought after (Putnam 2000)

This competitiveness can first be traced to the period between 1955 and 1970 known as the "Tidal Wave" in college admissions which raised the collegiate enrollment rate by almost ten percentage points due to the coming of age of the Baby Boomers generation (Duffy and Goldberg 2014). During this time, the admission acceptance percentages at Harvard and Yale, two of the nation's most prestigious universities, declined by twenty percentage points (from the 40s to the 20s). However, during the 1970s the competitiveness subsided some due to worsening economic conditions, a decrease in returns to postsecondary education, and a drop in the college-aged population (Thelin 2011, Duffy and Goldberg 2014). In the 1980s college competitiveness rankings were introduced. The *Barron's Profiles of American Colleges* debuted in 1981 and the *U.S. News and World Report* rankings debuted in 1983. Furthermore, the returns to postsecondary also increased during the 1980s. For the past forty years, these conditions have intensified the competition for access to postsecondary education, particularly at the most selective institutions ((NACAC) 2006, Alon and Tienda 2007). Selective colleges also play into this culture of selectivity and exclusion where their own prestige and reputation is commensurate with having the best students. For this reason, many colleges make the academic and non-academic achievements of their students public knowledge. Many describe the research publications and awards of students, boast how many students were awarded prestigious fellowships, and list their job placements or admissions acceptances to other universities (Brewer, Gates et al. 2002). Recently, Harvard College proudly claimed that its Class of 2023 was its most selective in its history- with an admissions rate of four and a half percent (Caldera 2019). In contrast, conscious that this feature of exclusivity at highly selective colleges may be societally toxic, Stanford University stated it would no longer publish its acceptance rate (Bliss 2018).

Indeed, the allure of these highly prized credentials have produced malfeasance and corruption in higher education. 2019 featured the largest ever college admissions prosecution by the U.S. Department of Justice when it charged 50 people for taking part in illegal college admissions practices. Highly selective colleges such as Stanford, Yale, and the University of Southern California admitted students that had either cheated on standardized tests, bribed college admissions administrators, or made false statements on admissions documents claiming that they were highly talented athletes, when in fact they played no sport (Rooney and Smith

2019). While this may be an extreme example, many scholars argue that affluent parents are heavily invested in the educational attainment of their offspring and will direct resources towards them to give them a competitive edge in educational attainment (Lareau and Horvat 1999, Lareau 2000, Karabel 2006). This is done in part to assure that their children will not become downwardly mobile (Killgore 2009). The bottom line is that people will go to great lengths to secure access to a prestigious education.

During the "Tidal Wave," a disparate social movement within education was also occurring. In the late 1960s Princeton, Harvard, and Yale implemented affirmative action policies by increasing the enrollment numbers of African American students on the heels of racial riots across the United States (Karabel 2005). There were particularly violent riots in Los Angeles (1965), Detroit (1967), Newark (1967), and Washington (1968). During this time Title VI of the Civil Rights Act of 1964 was also passed to prohibit discrimination on the basis of race, color, and national origin in programs and activities receiving federal financial assistance. Together this institutional response to racial riots, and the passage of the Civil Rights Act of 1964 increased the enrollment of African American students across the United States by over two-hundred percent between 1964 and 1973 (Anderson 2002). From 1974 to 1984 the enrollment of African American students stagnated in number and declined as a percentage share of the total higher education enrollment (Kane 1994, Anderson 2002). From 1985 to 1997 there was a 48 percent increase in African American enrollment in higher education (Anderson 2002). Between 1976 and 2008 Black enrollment rose from 943,000 to 2,269,000. Similarly, Hispanic enrollment rose from 350,000 to 2,103,000 during the same period, although the growth in Hispanic enrollment has risen much faster than the African American enrollment since the start of the new millennium (Planty, Hussar et al. 2008). From 2000 to 2016 the percentage of eighteen to twenty-four year-olds enrolled in college by race and ethnicity changed from thirty-one percent to thirty-six percent for African Americans and twenty-two percent to thirty-nine percent for Hispanics (de Brey, Musu et al. 2019).

The increased competitiveness in college admissions came into conflict with the rise of underrepresented minority student enrollment in 1978 during the *Regents of the University of California v. Bakke* Supreme Court case.[1] The plaintiff in the case, Allan Bakke, a White applicant to the University of California, Davis medical school, was rejected on two separate occasions. Bakke brought a suit against the university, believing that he had been discriminated against under the Equal Protection Clause of the Fourteenth Amendment, a clause that prevents institutions from practicing racial discrimination, and had, until that point, been used to protect marginalized racial groups from racial segregation. The Court ruled in favor of affirmative action by allowing race to be considered as one of several factors in college admissions, but it deemed that racial quotas (i.e., allocating a certain number of admissions for African American students) were impermissible. Several more court cases have challenged affirmative action since *Bakke*, and as a result it stands as one of the most controversial and heavily litigated issues in the history of the United States court system.

Ensuing Litigation After Bakke

In 1996 the first successful challenge to racial preferences in student admissions occurred with *Hopwood v. Texas*, when four students were rejected from the University of Texas at Austin's School of Law. The U.S. Fifth Circuit court, which has jurisdiction over Louisiana, Mississippi, and Texas, wrote the opinion that the law school could not use race as a factor in

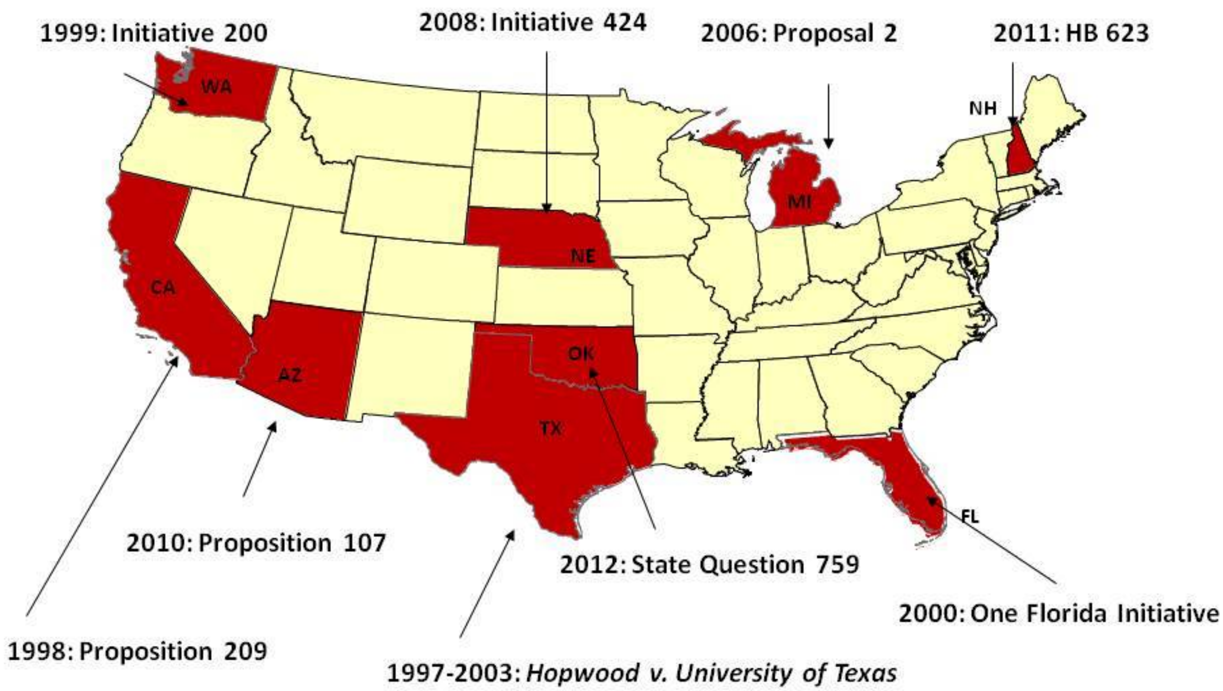
[1] Oddly enough, it came during a time when African American enrollment in postsecondary education had stagnated.

admissions. However, this decision was later abrogated (i.e., repealed) by *Grutter v. Bollinger,* where the Supreme Court ruled in a 5-4 decision that the University of Michigan Law School, which had a compelling interest in promoting diversity, could continue to use race-conscious admissions. A separate decision, *Gratz v. Bollinger,* concluded that the University of Michigan's undergraduate admissions process that had granted extra points in admission on the sole basis of racial identity was unconstitutional. In 2013 and 2016, *Fisher v. University of Texas* and *Fisher II* further supported the earlier decisions in *Grutter v. Bollinger* and *University of California v. Bakke* that the use of race in college admissions was legal. In 2015 however, the Supreme Court ruled in *Schutte v. Coalition to Defend Affirmative Action* that states had the right to implement their own bans on the practice of affirmative action. The next court case that is likely to reach the Supreme Court is *Students for Fair Admissions v. Harvard*, which argues that Harvard's use of 'personal metrics' in which Asian Americans score poorly is discriminatory against Asian Americans.

The *Schutte* case is central to this dissertation because it ruled that states have the right to implement their own bans on affirmative action. The first such state ban was passed in California in 1996 with Proposition 209, and Texas shortly followed, outlawing the practice of affirmative action when after the U.S. Fifth District Court ruling in 1997 that was ultimately repealed by *Grutter v. Bollinger* in 2003. Washington, Florida, Michigan, Nebraska, Arizona, New Hampshire and Oklahoma also passed bans on affirmative action. Washington, Michigan, Nebraska, Arizona, and Oklahoma introduced the law through ballot initiatives. Colorado also put affirmative action on the ballot in 2008, but voters rejected the ballot measure, making it the lone state to have had a ban proposed on a ballot and not pass it. Florida introduced its ban via

Executive Order when then Governor Jeb Bush introduce his One Florida Initiative in 2001. Lastly, New Hampshire introduced its ban on affirmative action through House Bill 623 in 2011.

Figure 1.1: A State Map of Affirmative Action Bans



Almost all of the anti-affirmative action measures (with the exception of *Bakke* and New Hampshire House Bill 623) involved Edward Blum, Ward Connerly, and the organizations that they are affiliated with (e.g., American Civil Rights Institute, the Center for Equal Opportunity, and the Pacific Legal Foundation). Edward Blum, who is currently spearheading the Harvard affirmative action lawsuit, has also been involved with twelve "equal rights" related lawsuits, of which six have reached the Supreme Court. He has had many powerful allies backing his position including former University of California Regent, Ward Connerly, former U.S. Attorney General, Edwin Meese, and Clint Bolick, the former Vice President of the Goldwater Institute, a conservative think tank, who in 1998 launched a campaign in which all 50 state attorneys general were contacted to eliminate racial preferences in affirmative action. Ward Connerly led the charge against affirmative action in California, and would later help campaign against affirmative action in Washington, Florida, Michigan, Colorado (where it was rejected as a ballot initiative), Nebraska, and a few other states where affirmative action bans failed to reach a ballot.

During the first affirmative action ban in the state of California, 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 would be mimicked; much of the same language can be found on the ballot initiatives for affirmative action bans in other states. Both of these 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."

Framing Colorblind Ideology as Laissez Faire Racism

Bonilla-Silva (2006) argued that this colorblind ideology - where ostensibly meritocracy dictates the distribution of resources without the consideration of race - is simply racism by another name. He also argues that a person holding that ideology would not identify themselves as racist. He applies four frames for interpreting color-blind ideology: abstract liberalism, naturalization, cultural racism, and minimization of racism. Specifically, within the context of this dissertation, I explore abstract liberalism because it "involves using ideas associated with political liberalism (e.g., equal opportunity) and economic liberalism (e.g., choice, individualism) in an abstract manner to explain racial matters" (Bonilla-Silva 2006). The ideology of color-blind meritocracy ties perfectly into this frame because meritocratic features such as academic ability (measured by GPAs and standardized test scores) emphasize the economic liberalism component because it situates students as the sole agents responsible for their destinies, while the color-blind feature embodies equal opportunity and qualifies as economic liberalism component by erasing any advantage (*but not disadvantage*) on the basis of race.

Bobo et al. (1997) contend that colorblind ideology is the most salient feature of *laissez-faire* racism, the era of racism that we are currently under. Unlike the preceding Jim Crow era that is characterized by overt racism, legalized segregation, bigotry, and the prevailing belief that the White racial group is genetically and intellectually superior to all other races, this new era, termed *laissez-faire* racism, describes an era where overt acts of racism are few and far between, and instead there is subtler subconscious and systemic racism. We live in a society of laissez faire racism where society often thinks of itself as post-racial and denies the existence of racism, but is still fraught with issues of racial inequality throughout the life course, in wealth and income, in housing, in the criminal justice system, in healthcare, and in education (Keister and Moller 2000, Kao and Thompson 2003, Pettit and Western 2004, DiPrete and Eirich 2006, Pager and Shepherd 2008, Williams and Sternthal 2010). The concept of laissez-fair racism suggests that racism still exists because there is measurable racial inequality between racial groups that are dominated by the White racial group. A Marxian interpretation of law is that law is merely the instrument of class interest; an ideological reflection and mechanism used to preserve class interests that exists not as a structure, but as a superstructure (Harris 2001).[2] As such, I contend that affirmative action bans are specialized instance racialized instance of opportunity hoarding (Tilly 1998), where the resource of education is hoarded by the white racial group through the bans to preserve its dominant status. This example serves to elucidate how the legal system distributes 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. It embodies 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, *laissez-faire* racism serves to illustrate why the myth of equal opportunity is not realized with respect to race.

While these ideas of *laissez faire* racism (Bobo 1997) and racism without racists (Bonilla-Silva) have become an incredibly powerful tools to illustrate modern day racism, they are subject to several criticisms. The largest critique of Bonilla-Silva's work is that much of his theory is *a priori*, and when he uses *a posteriori* logic he sometimes uses convenient, almost anecdotal examples to support his theory; some contend that these examples suffer from confirmation bias. One example (Wimmer 2015) is when Bonilla-Silva ignores control variables entirely when deducing that the average incomes of each racial group dictate which groups will

[2] The traditional difference between structure and superstructure argues that structure defines social relations, while superstructure refers to symbolic and ideational relations (e.g., the law).

be allowed by Whites to have ‘this much’ income or education in the future. Bobo’s work focuses on how socio-psychological measures of racial attitudes, beliefs, and schemas are moderated (Bobo and Licari 1989) or how they impact structural outcomes such as bussing or residential segregation (Bobo 1983, Zubrinsky and Bobo 1996). While these studies are on better methodological footing they lack a causal explanation to provide the strongest evidence that racial attitudes lead to structural outcomes. Certainly, the causal argument may exist even when the empirical proof is not fully developed, but more recently, a host of audit studies have forged a strong causal link between racial identity and structural outcomes like employment, credit, and housing that could be explained by *laissez faire* racism (Pager 2003, Quillian 2006). These studies have evidenced that a person’s racial identity plays a key role in determining their life’s chances despite the absence of overt discrimination. These studies reinforce that racism oftentimes occurs out of plain view: unconsciously or covertly.

The Contribution this Dissertation Makes to Our Understanding of Laissez-Faire Racism

This dissertation advances our understanding of the relationship between social policy and racism by applying a more rigorous methodology, broader empirical outcomes than those previously studied, and a sociological perspective to frame affirmative action bans. In these studies, I apply difference in differences methodologies to explain how the application of colorblind racism in postsecondary education resulted in racial inequality in education and intermarriage. I do this by exploiting the dynamic nature of affirmative action bans implementation over states and time over the past twenty-five years. The phenomenon produced the required variation in policy and timing that make affirmative action bans ideal candidates for difference in differences methods. Chapter 2, “The Affirmative Action Ban Cascade: Where Students Enroll After Affirmative Action Bans,” explores how various sectors of postsecondary education (i.e., 2 year and 4-year schools, and public, private, and for-profit schools) have been impacted by affirmative action bans. Chapter 3, "The Machinations of Laissez Faire Racism in Action: Differences in Underrepresented Student STEM and non-STEM Degree Attainment in the Wake of Affirmative Action Bans," explores the impact *laissez faire* racism, as embodied by affirmative action bans, on STEM and non-STEM education. Because STEM is more prestigious and competitive than non-STEM, this study explores how the racialized control of resources produces racial inequality. Chapter 4, "The Unintended Consequences of Affirmative Action: How Affirmative Action Bans Have Shaped Interracial Marriage," highlights the depth to which a policy that enforces a racialized redistribution of resources can have unanticipated and unintended impacts on society. This chapter, more than the other two chapters, advances the literature methodologically, theoretically, and empirically. Methodologically, audit studies are incapable of exploring an outcome such as interracial marriage, and in investigating how shifts in racial schemas produce unintended or unanticipated consequences descriptive studies would have the difficult task of overcoming a relationship that is potentially spurious. This chapter also makes a theoretical contribution to *laissez-faire* racism or the understanding of racism without racists because it provides insight into empirically unchartered waters of institutional racism by focusing on the unintended and unanticipated consequences of racialized policy. Identifying unintended and unanticipated consequences of an abrupt change in racial schemas gives social scientists the ability to illustrate the depths of institutional racism.

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Chapter 2: The Affirmative Action Ban Cascade: Where Students Enroll After Affirmative Action Bans

INTRODUCTION

Affirmative action was a term first introduced in the policies of the Kennedy and Johnson administrations in the United States as part of the Civil Rights movement. These policies aimed to create favorable conditions for people that had historically suffered from discrimination. The first policies emphasized equal opportunity in employment, but then spread to higher education. Colleges first adopted affirmative action policies in the early 1960s in response to non-violent civil rights protests. A second wave of affirmative action programs in college admissions emerged as the result of student protests in the late 1960s (Stulberg and Chen 2014). However, legal pushback against these policies began with *Regents of the University of California v. Bakke* (1978), in which a medical school applicant argued that the use of a racial quota had prevented him from accessing medical school. *Bakke* deemed the racial quota system unconstitutional; however, the decision permitted the use of race as a factor in admissions. Since the *Bakke* case, the Supreme Court has revisited affirmative action four more times, making it one of the most litigated issues in this country over the past fifty years.

Beginning with *Proposition 209* in California in 1996, nine states passed their own bans on the practice of affirmative action, preventing public institution of higher education from considering race in admissions or the awarding of scholarships and other financial aid.

The *Schutte v. Coalition to Defend Affirmative Action* (2014) decision permitted states to adopt bans on affirmative action. I exploit state-level variation in the presence and the timing of affirmative action bans to estimate the effect of these bans on the college enrollment of underrepresented minority students (herein URMs or URM students).

Many studies indicate that affirmative action bans have decreased the proportion of URMs entering various levels of higher education including 4-year colleges, graduate schools, and professional schools (Wightman 1997, Espenshade and Chung 2005, Backes 2012, Hinrichs 2012, Garces 2013, Garces and Mickey-Pabello 2015). While these studies conclude that the bans decreased the proportion of URMs at various levels of higher education, they do not pinpoint where URMs have gone as a result of the 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. 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." In this study, I pinpoint where URM students have gone by investigating (a) if URMs' probability of having no postsecondary educational attainment increased as a result of affirmative action bans (i.e., whether they selected out of higher education altogether), and (b) 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. My findings add to the existing corpus of studies on the causal impacts of affirmative action bans on URM students by fully fleshing-out, for the first time, what I call the affirmative action ban cascade. I define the affirmative action cascade as a process whereby the intense competition for access to the most selective postsecondary education institutions was additionally constrained by affirmative action bans which not only displaced URM students from the most selective colleges but has also lead to URMs trickling down the cascade to less selective 4-year and 2-year institutions. Somewhat unexpectedly, I find that the cascade has led to an increase in postsecondary education enrollment for URM students. My findings suggest that the increase in postsecondary education enrollment for URM students is in part attributable to a rise in their enrollment in for-profit universities. This finding implies that a) students that otherwise would have gone to a more selective institution went to for-profit schools instead due to the bans, and b) students who otherwise would not have enrolled in college were drawn to these institutions as well.

HOW AFFIRMATIVE ACTION BANS IMPACT UNDERREPRESENTED RACIAL MINORITIES

The main goal of this study is to discover where URMs students have enrolled as a result of affirmative action bans. I summarize the literature on the causal impacts of affirmative action bans on educational attainment and highlight that the previous literature has not fully considered the broad range of institutions that were impacted as a result of affirmative action bans.

Several studies have claimed that that affirmative action bans have decreased the proportion of underrepresented minority students entering various levels of higher education (Wightman 1997, Espenshade and Chung 2005, Backes 2012, Hinrichs 2012, Garces 2013, Garces and Mickey-Pabello 2015). 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 affirmative action ban cascade.

Because most previous studies how affirmative action bans differentially affected URMs (Backes 2012; Hinrichs 2012) by level of institutional selectivity, for-profit schools were omitted from those analyses. 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 them has limited our ability to understand what happens to URM students.

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 (Lahr, Pheatt et al. 2014, Dougherty, Jones et al. 2016). 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 (Lynch, Engle et al. 2010, Schade 2014, Cellini, Darolia et al. 2016, Cellini and Koedel 2017, Gilpin and Stoddard

2017, Cellini and Turner 2019). [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, Hussar et al. 2017) and for students whose degrees are not ackowledged in the workplace due to the poor reputation of the school (Darolia, Koedel 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.

I use the metaphor of an affirmative action cascade to illustrate how URM students have been displaced at the height of the cascade (selective institutions) and have trickled down the cascade to less selective tiers of higher education or out of higher education altogether.[3] The cascade starts out at the most selective institutions where affirmative action bans displace a group of underrepresented minority students that would otherwise attend those schools if there were no ban. The colorblind meritocracy inherent at the height of the affirmative action ban cascade 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 (Stevenson and Baker 1992, Buchmann, Condron et al. 2010). These features highlight the covert, and unintentional nature of laissez-faire racism in the affirmative action ban cascade. 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

[3] This study adds to the *en vogue* water metaphors for higher education such as Bowen and Bok's *Shape of the River* and Massey, Charles, et al.'s *Source of the River*.

traditional education that occurs in school. Furthermore, the instruments used to measure merit such as the SAT are sometimes racially biased themselves (Freedle 2003). 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 affirmative action cascade where the most selective institutions are. Because being an underrepresented racial minority no longer has an advantage under the colorblind meritocracy imposed by affirmative action bans, Whiteness becomes a credential.

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. The competition of resources is renewed at each tier of the affirmative action cascade until the bottom is reached. The group of URM students that is displaced at the most selective institutions (the top of the cascade) 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). They 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 affirmative action cascade (to 4-year schools unclassified by selectivity, and then to 2-year public and private schools) until a final group of URM students hits a bottom (which can be thought of as either out of postsecondary education or 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 affirmative action cascade, we should not observe affirmative action ban effects in this middle part of the cascade (i.e., less selective 4-year schools and 2-year colleges) because the students in the middle that leave are replaced by the students that come in. In other words, the net change for the tiers of education in the middle (i.e., less selective 4-year schools and 2-year schools) should be close to zero if the bans have created a trickling down where the more academically qualified URMs are pushing the less qualified URMs down the cascade.

The bottom (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). However, given that a person's life chances without higher education and a person's life chances at for-profit universities are both lower than they would be if they had more desirable college degrees, it is possible that the share of URMs could increase either among people without any postsecondary education, with for-profit education, or both.

DATA AND METHODS

Data

I used 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).

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.[4]

The units of analysis in this study change based on the question being answered. The first question investigates if the probability of not participating in any postsecondary education increased for URMs as a result of the 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. 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 recoded the educational attainment variable from this survey so that people with more than a high school degree would

[4] 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).

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.

The second question investigates 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). However, I also include the "Most Competitive" group alone to confirm that URMs were being displaced from the most selective tier of post-secondary education affirmative action as theorized by the affirmative action ban cascade.

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).[5] 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, Hinrichs 2012,

[5] 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.

Garces 2013, Garces and Mickey-Pabello 2015). 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 a 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 (observations over time, nested within states) (Murnane and Willett 2011). I use state-clustered standard errors to account for residual correlation not eliminated by state fixed-effects alone or robust standard errors (Bertrand, Duflo et al. 2004).

I fit the following multilevel ordinary least squares regression (the levels are states and colleges or individuals depending on the question being modeled):

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

where $Enroll_{ist}$ indicates the proportion of URMs enrolled at time (t)[6]; 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.[7] 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, Galasso et al. 2005) so that the difference in the proportion of not earning a

[6] *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.
[7] 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.

college degree between URMs and non-URMs can be estimated for affirmative action bans. Triple-differencing is necessary for the first research question, but not the second research question in this study. The first question does not have a race specific dependent variable (no postsecondary education), but the second question does have a race specific dependent variable (the share of URMs enrolled at an institution). Triple differencing is required for the first question specifically because we expect there to be a difference in the probability between a URM and non-URM in having no postsecondary education and because the URM feature is captured by an independent variable and not within the dependent variable. 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). 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 the same model 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 a 4-year analytic window, then the bans were more impactful most recently after their implementation.

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. I compare 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.[8] Figure 2.1 presents graphs for the proportion of URM enrollees in each of the states that ultimately implemented an affirmative action ban, and for the proportion of URM enrollees in each of the 32 states in the comparison group during the years investigated by this study. It is necessary to look at the states individually because the timing of the ban varies from state to state. Overall, the trends are sufficiently similar in the pre-ban periods between the dotted and undotted lines. There is no movement in the trends between ban and non-ban states that indicate that pre-ban differences between states was caused by something other than the bans. There are some issues of small sample size in New Hampshire, Arizona, Washington, and Nebraska that make these graphs look less stable over-time than they really are (i.e., it looks like there is too

[8] A problem with resolving the parallel assumptions trend comes when the treatment is too dynamic. In the case of affirmative action bans the bans (i.e., the treatment) were implemented at several points in time. The graphs shown are cumbersome, difficult to read, and can be misleading when there are a small number of cases that cause the graphs to vary more on a year-to-year basis than they should. A model proposed by Mora and Reggio (2015) accounts for the dynamic nature of this implementation. Because there are multiple pre-treatment and post-treatment periods, multiple tests on the equality of the effect on pre- and post-treatment periods are calculated. The statistic of the equivalence of the parallel assumptions is the estimated effect on the last pre-treatment period. A limitation of this dissertation is not including this estimate, but future drafts of the three empirical chapters will include these more formal tests of the parallel assumptions trend.

much year-to-year variation), making them poor candidates to test the parallel trend assumption on a state-by-state basis. However, it should be noted that the difference in difference analysis looks at all the states together, and not individual states. I have included Figure 2.2 to demonstrate that the trend in ban states when considered as a group is indeed more stable over calendar years, however Figure 2.2 does not demonstrate the variation in the year of implementation for the bans.

Figure 2.1 Parallel Trend Assumption: No Postsecondary Education

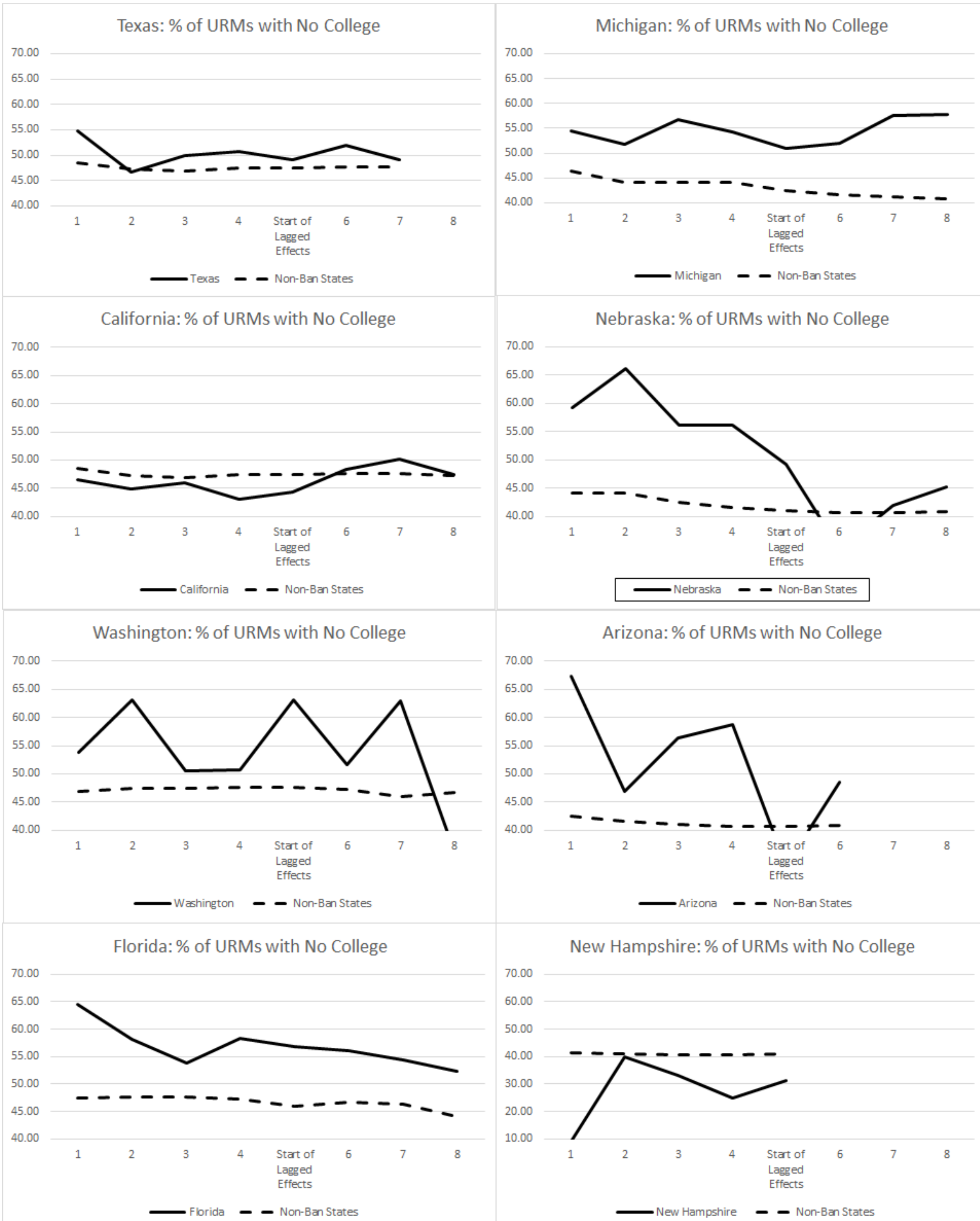
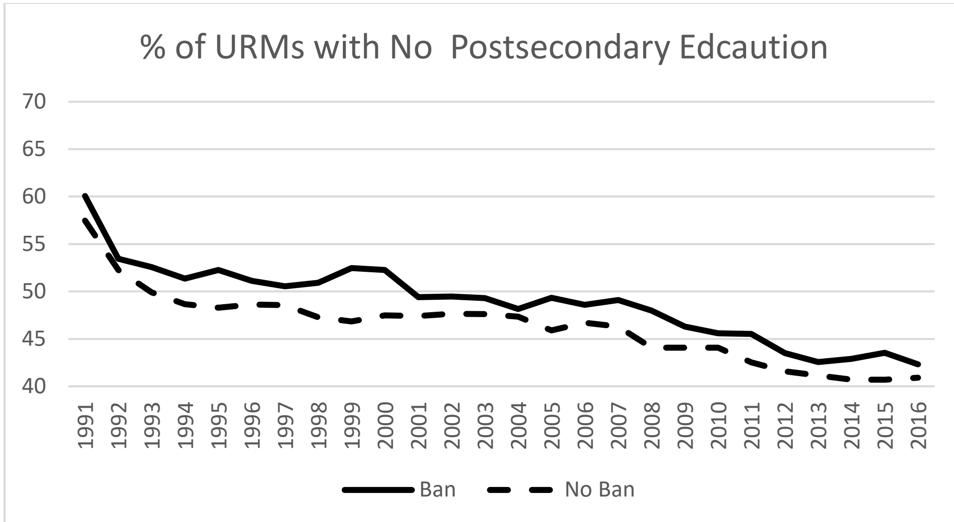


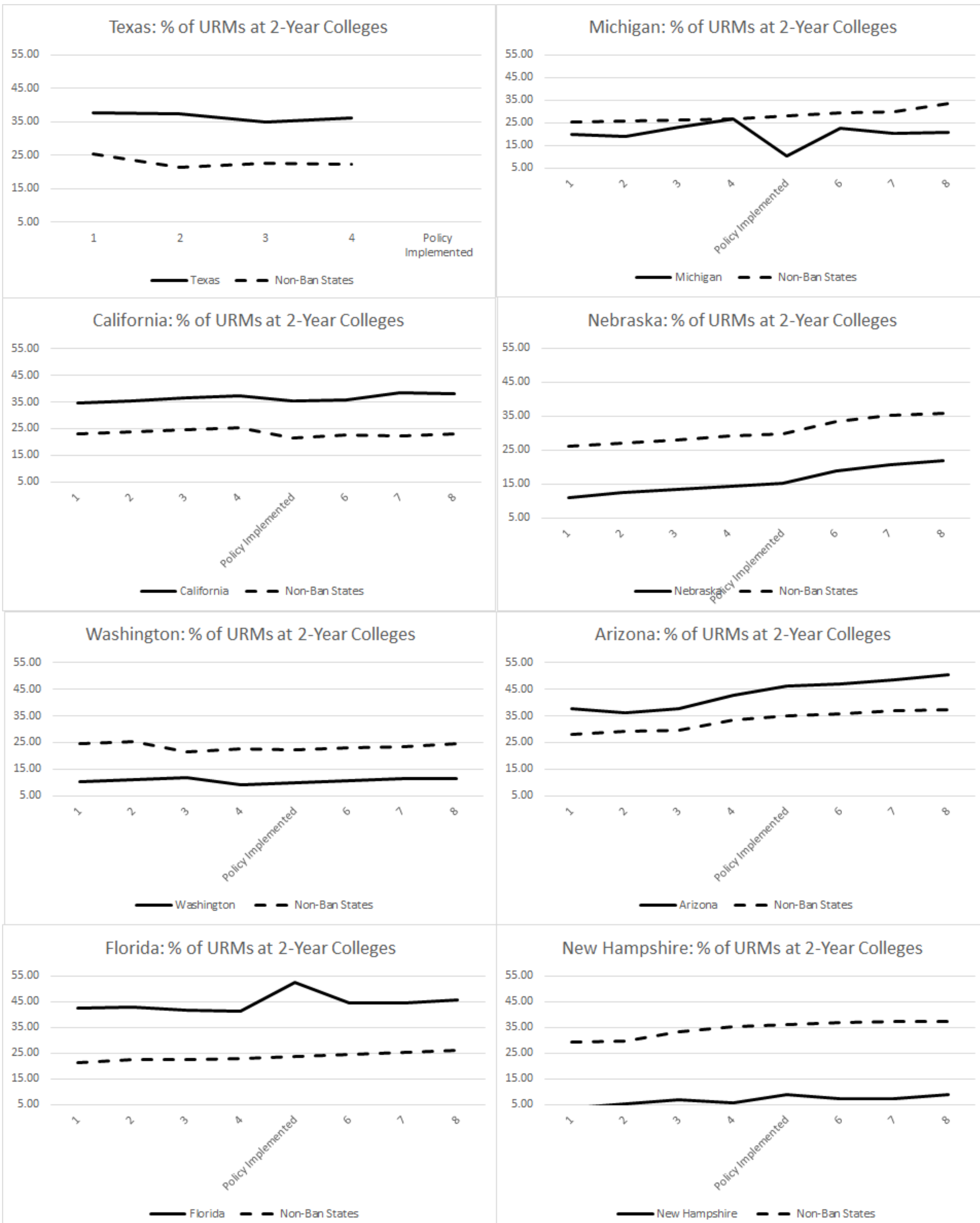
Figure 2.2 The Percentage of URMs with No Postsecondary Education



The share of URM enrollment at 4-year institutions is shown in Figure 2.3 and the share of URM enrollment at 2-year institutions is shown in Figure 2.4. Again, in Figure 2.3 below the trends are sufficiently similar in the pre-ban periods between the dotted and undotted lines. There is no movement in the trends between ban and non-ban states that indicate that pre-ban differences between states were caused by something other than the bans.

Figure 2.3 Parallel Trend Assumption: Share of URM Students at 4-Year Postsecondary Institutions

Figure 2.4 Parallel Trend Assumption: Share of URM Students at 2-Year Postsecondary Institutions



FINDINGS

I present the results for the first research question pertaining to the share of URMs not attaining any postsecondary education in Table 2.1. 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) in Panel B tells us how URMs were impacted by the bans relative to non-URMs. Although the coefficient here is negative (-0.0436), the impact is to increase the share of not completing college because the dependent variable is inverted (i.e., 1=not attending college and 0= attending college). 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 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 oddly more likely to enroll in college due to affirmative action bans. However, 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 can in part explain why there is an increase in the share of URM students enrolling in college.

Because this result was somewhat surprising, I took the further step to include a spuriousness check. To give greater confidence to my 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. If sex 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 is questionable). My spuriousness checks indicate that triple difference coefficient is not spurious (the interaction effect for sex is not significant); hence my findings support that URM degree attainment was impacted by affirmative action bans.

Table 2.1 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 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.

In Table 2.2 I present the findings for the second research question, 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. 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%). 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 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. Stratifying the 4-year institutions allows for the detection of movement along the affirmative action cascade. Consistent with the theoretical model the only discernable movement in the share of URMs at 4-year institutions comes at the top of the cascade- at the most selective schools in the country (-0.1028 or a decrease of 38%).

Table 2.2 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

In Table 2.3 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 replaced by students that were displaced from further up on the cascade.

Table 2.3 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	

WHAT DOES THE AFFIRMATIVE CASCADE REALLY LOOK LIKE?

The results of this study help to solve the puzzle of what has happened to URM students as a result of affirmative action bans. Particularly, they illustrate what the bottom of the affirmative action ban cascade looks like. 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. Backes (2012) further found that there was no change in the enrollment pattern of URMs at 2 year institutions. My results not only confirm the these results, but also stratify schools by sector (public, private, and for-profit) and selectivity, and include results for people that never went to college. My results support previous findings that URMs where displaced from the most selective institutions, and previous findings that URMs were not filtered into private schools, but also impact private schools (Backes 2012, Hinrichs 2012). More importantly, I answer the question of where URMs were displaced as a result of affirmative action bans. My results suggest that the typical URM student is more likely to enroll in postsecondary education as a result of affirmative action bans. Furthermore, because there was a positive coefficient for the causal impact of affirmative action bans on for profit schools generally, while there were no such results for public or private schools, I argue the increased enrollment of the typical URM student in postsecondary education was driven, in part, by their increased enrollment in for-profit schools.

I argue that colorblind meritocracy was embedded into postsecondary organizations through anti-affirmative action policies passed in various states. The affirmative action cascade was created as a result of schools in ban states applying new schemas about race that are tied to organizational resources (mainly being admitted to college). At the top is 4-year undergraduate education (ordered from more selective to less selective), then a lower tier (e.g., 2-year institutions) and towards the bottom of the cascade is a confluence of those that outside of higher education altogether and those attending for-profit universities. I speculated that it was possible that the affirmative action ban cascade would end with URM students being removed from higher education altogether. However, the probability of a URM enrolling at a postsecondary institution increased, and the affirmative action ban cascade actually ends with URM students being displaced into for-profit universities. The theory I posited suggested that URMs would be displaced from the top of the cascade and resurface at the bottom, where their life-chances were the worst and there was the least amount of competition. There is some evidence in the literature on for-profit colleges to support this position.

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

In sum, I argue that opportunity hording 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 disenfranchising to those who are not in power. Under the guise of colorblind meritocracy these affirmative action bans exemplify the extent to which *laissez faire* racism operates in our institutions. The affirmative action ban cascade 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.

CONCLUSION

The affirmative action ban cascade 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 top of the cascade exists because the policy preserves 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. And further down the affirmative action ban cascade URM some 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.

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.

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. 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.

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Chapter 3: Laissez-Faire Racism in Action: Differences in Underrepresented Student STEM and Non-STEM Degree Attainment in the Wake of Affirmative Action Bans

INTRODUCTION

In recent years there have been calls from the National Academies (Vest and President Emeritus 2010), the President's Council of Advisors on Sciences and Technology (Olson and Riordan 2012), the Congressional Research Service (Kuenzi 2008), and the National Science Foundation (2010) to increase an emphasis on STEM education. Though the United States' system of postsecondary education is highly regarded, the country ranks poorly relative to other nations in terms of the number of student completing STEM degrees. The aforementioned groups are concerned because the country's economic and political power is associated with the breakthroughs it has made in science and technology (Goldin and Katz 2009). Both gender and racial inequalities are commonly cited as significant causes of low STEM attainment in the United States (Xie et al. 2015). This study focuses on ethnic/racial inequalities. In 2013-14, 17% of all bachelor's degrees granted to White students were STEM degrees, compared to 11% among Blacks, 14% among Hispanics, 31% among Asians, 15% among Pacific Islanders, 14% among American Indians or Alaskan Natives, and 18% among those that identified with more than one racial category (Snyder, de Brey et al. 2016). Although Blacks and Hispanics trail

Whites by only 6 and 3 percentage points respectively, the National Research Council finds these disparities particularly troubling because the populations that are the most underrepresented in STEM are also the fastest growing populations in the United States (Groups, Science et al. 2010).

There are two chief explanations for racial inequality in STEM. The first is that it is an issue of individual agency and achieved characteristics. This explanation argues that lower levels of pre-college achievement such as lower standardize test scores and high school GPAs make entry into higher education more difficult for underrepresented minority students (herein URMs) (Riegle-Crumb and Grodsky 2010, Board 2015). However, other aspects of individual agency, such as drive, motivation, and levels of interest in STEM are not part of the explanation. For example, Riegle-Crumb et al. (2011) show that URM student interest in science is on par with that of their White counterparts. The second explanation is that there are ascribed characteristics and structural impediments for URM students that limit their participation in STEM. Several studies have shown that after controlling for ascribed characteristics such as family income, parental resources, and parent's educational attainment, the racial achievement gap in math and science is greatly reduced, although not eliminated (Kao and Thompson 2003, Downey 2008, Jencks and Phillips 2011). Furthermore, Xie and colleagues (2015) mentions several structural impediments to STEM education that occur more frequently for UMR students in primary and secondary education such as poor teacher quality, lower per-pupil expenditure levels, and racial mis-matches between students and teachers. Yet within postsecondary education, the study of structural impediments to racial STEM equality has been limited to social-psychological factors of school integration and self-belonging. No prior research (to the author's knowledge), has focused on structural impediments that arise from policy and the subsequent racialized distribution of resources within postsecondary education. In this study, I add novel evidence for structural explanations at the postsecondary level, finding that affirmative action bans have restricted the STEM pipeline for URM students. As such, this study breaks new ground on the issue of STEM attainment for URM students by investigating the role affirmative action bans have played.

Like GPAs, standardized test scores, and parental resource differences, affirmative action bans impede access to postsecondary education for underrepresented minority students. By law the bans reverse many of the benefits to URMs that had been created by affirmative action policies in the domains of admissions, financial aid, and other university services. Previous research has shown that affirmative action bans have decreased the proportion of URM students at college entry in undergraduate (Espenshade and Chung 2005, Backes 2012, Hinrichs 2012), graduate (Garces 2013), and medical education (Garces and Mickey-Pabello 2015, Mickey-Pabello and Garces 2018). I build on these past studies of college and professional school enrollment by measuring how much the share of URM students graduating from STEM majors has changed due to the bans. I compare the change in the share of URM students graduating with STEM degrees to the change in the share of URM students graduating from non-STEM majors and find that the declines in STEM degrees are greater. These findings implicate affirmative action as a new component of the racial inequality that affects the STEM pipeline. I also investigate whether students in more selective schools are more impacted by the bans, if public or private schools are more impacted by the bans, and when the bans had their greatest impact (right after the bans, or after a period of time had passed). The results indicate that the bans were more impactful after a period of time had passed and supports that private schools may have also been impacted by affirmative action bans.

This study provides insight into the application of a new racialized policy in postsecondary education. Due to the colorblind nature of these policies I assert that the applications of the policy by institutions are ultimately machinations of *laissez-faire* racism (Bobo, Kluegel et al. 1997). In the section below, I review the empirical literature on the causal impact of affirmative action bans but reframe it through the lens of *laissez-fair* racism and theory on racialized organizations. Not only is this reframing important because it provides context for institutions' applications of racialized laws, but also because I contest and reevaluate several existing findings later in the paper and illustrate how sensitive difference-in-differences models can be to slight variations in model specification. After familiarizing the reader with the affirmative action ban literature, I review the literature on URM STEM degree attainment and contextualize the impact of affirmative action bans on STEM degree attainment. Combining these two previously disparate literatures on STEM and affirmative action provides insight into how affirmative action bans have led to racial inequality in STEM.

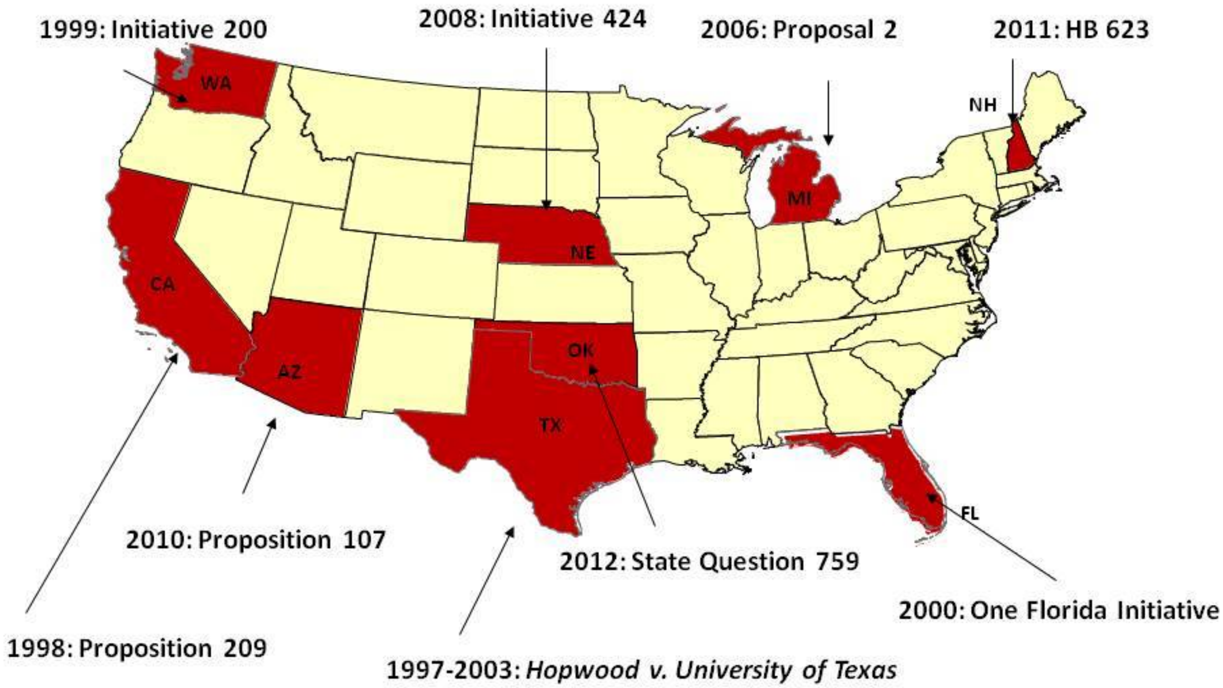
AFFIRMATIVE ACTION BANS AS LAISSEZ FAIRE RACISM

Affirmative action was a term first introduced in the policies of the Kennedy and Johnson administrations in the United States as part of the Civil Rights movement. These policies were aimed at creating favorable conditions for those who had historically suffered from the effects of discrimination. These policies, including Kennedy's Executive Order 10925 which first made mention of 'affirmative steps,' were directed towards equal opportunity in employment to help correct for economic inequalities. The policy schemas of affirmative action used to benefit people of color in the workplace then diffused via isomorphism to postsecondary education, but were largely adopted in response to student protests in the 1960s (Stulberg and Chen 2014).

racial quota system in admissions had prevented him from gaining access to medical school. While use of a racial quota system was deemed unconstitutional, the use of race as a factor in admissions was permitted.

Since the *Bakke* case the Supreme Court has revisited affirmative action four more times, making it one of the most controversial issues in this country over the past fifty years. The most notable of those court cases for the context of this paper is *Schutte v. Coalition to Defend Affirmative Action* because it has ultimately led to state-level variation in affirmative action policies. The *Schutte* case highlighted the pattern that emerged because there was no national consensus around the issue; instead, some states, beginning with *Proposition 209* in California in 1996, passed their own bans on the practice of affirmative action. In the twenty years following California's ballot initiative, eight other states adopted their own policy that prevented public institutions of higher education from considering race in admissions or the awarding of scholarships and other financial aid. Figure 3.1 depicts California and all of the other states banning affirmative action by year and the name of the policy. Most of the states passed bans on affirmative action through ballot initiatives; Colorado was the lone state to have introduced a ballot initiative to ban affirmative action but failed to pass it.

Figure 3.1 Affirmative action policy map



A handful of studies have investigated the causal impacts of affirmative action bans on URM enrollment. However, because the overwhelming body of research comes from economics or education policy it has not been interpreted with sociological theories such as *laissez-faire* racism (Bobo, Kluegel et al. 1997) or the very recent theory of racialized organizations (Ray 2019). The sociological lens provides a richer explanation for the role of race, and how organizations and the U.S. system of postsecondary education have responded to affirmative action bans. *Laissez-faire* racism is the key feature of this lens. It characterizes an era that has at times been erroneously thought of as a post-racial era due the lack of hallmark features of the Jim Crow era such as overt bigotry, *de jure* segregation, and the belief that Blacks (and many other racial-ethnic groups) are genetically and intellectually inferior to Whites.[9] We now live in a society that often thinks of itself as post-racial and denies the existence of racism, but is still

[9] Although the term laissez-faire racism typically refers to racism against Blacks and the boundary that represents the Black-White color line several scholars now contend that American color lines are more complex (Lee and Bean 2004). For this reason, I am more inclusive about the racial and ethnic groups that suffer from such racism.

fraught with issues of racial inequality throughout the life course, in wealth and income, in housing, in the criminal justice system, in healthcare, and in education (Keister and Moller 2000, Kao and Thompson 2003, Pettit and Western 2004, DiPrete and Eirich 2006, Pager and Shepherd 2008, Williams and Sternthal 2010). I contend that affirmative action bans are the epitome of *laissez-faire* racism because the proponents of these policies advocate for a colorblind meritocracy that denies the existence of racism through willful ignorance. What makes this policy racist is not that it overtly discriminates against people of color through *de jure* segregation, but that through the veil of color-blind meritocracy there are *de facto* implications for people of color. These span socio-psychological implications such as underrepresented minority students internalizing stereotypes about their lower academic performance (Steele and Aronson 1995, Crosby, Iyer et al. 2003, Dixon and Rosenbaum 2004, Fischer and Massey 2007) to structural implications such as decreased college accessibility for underrepresented minority students, which I discuss in depth below.

To capture the structural machinations of *laissez-faire* racism in higher education I frame the *de facto* implications of affirmative action bans using Ray's tenets for his theory of racialized organizations (2019). In adopting affirmative action bans higher education organizations (i.e., colleges and universities) had to reformulate their own racial schemas to conform with a racially charged policy that adopted the view of color-blind meritocracy. The forced adoption of that policy, or willful adoption under the coercive threat of litigation, embedded the unequal ethnoracial distribution of access to higher education into the sinews of postsecondary access to education and is justified by the *laissez-fair* racism of colorblind ideology. Ray's first tenet (racialized organizations enhance or diminish the agency of groups) is met because through affirmative action bans, postsecondary educational institutions have enhanced the agency of

White and Asian students and diminished the agency of Blacks and Hispanics by increasing the enrollment of the former and decreasing the enrollment of the later (Backes 2012; Hinrichs 2012).

The second tenet – that racialized organizations legitimate the unequal distribution of resources - is met because postsecondary institutions have done so. This is exemplified by the fact that not only have the affirmative action bans impacted students of color at their *de jure* targets; public postsecondary institutions, but studies have also found some mixed evidence that the impact of the bans has a *de facto* extension beyond public institutions (Backes 2012; Hinrichs 2012). (The results are mixed because sometimes there are fewer URMs admitted to these institutions and other times there is no statistically significant difference.) There has not been an empirical spill-over effect of URM students leaving public schools and enrolling in private schools as one might expect (Backes 2012, Hinrichs 2012). Therefore, I argue that the legitimization of the adoption of affirmative action bans was so powerful that another class of institutions not obligated by law to adopt it also has. Hirschman and Berrey (Hirschman and Berrey 2017) speculate that public universities may have walked away from affirmative action due to the threat of potential litigation. Another characteristic that legitimates the unequal distribution of resources by race is that the more selective an institution is, the larger its impact on URM enrollment is (Backes 2012, Hinrichs 2012, Arcidiacono, Aucejo et al. 2014). This was the case when Harvard first adopted affirmative action in 1961 and several institutions followed suit; through institutional isomorphism the educational organizations with the highest public visibility impact what other organizations do (DiMaggio and Powell 1983, Van Vught 2008, Stulberg and Chen 2014). To evaluate this tenant, I investigate a hypothesis that affirmative action bans impacted both private and public schools, and I investigate the hypothesis that increased selectivity is associated with larger declines in STEM and non-STEM attainment by URMs.

The third tenet is that Whiteness is a credential. Because the conditions of a 'color-blind' meritocracy White students are able to leverage their meritocratic advantages over people of color and through virtue of stereotype are positively viewed with respect to their academic credentials. This tenet needs no evaluation; it is a symptom of color-blind racism and several studies have demonstrated that unconscious or implicit racism (hallmarks of *laissez-faire* racism) benefits Whites (Greenwald, McGhee et al. 1998, Quillian 2008).

The fourth and final tenet of the theory of racialized organizations is that the decoupling of formal rules from organizational practice is often racialized. This is best exemplified by a recent finding by Mickey-Pabello and Garces (2018) on the impact of affirmative action bans at medical schools, which revealed that the impact on medical school admissions was most impactful immediately after the bans were initiated, and that the effect waned over time. Mickey-Pabello and Garces (2018) speculated that medical schools combated the losses in the proportion of URM medical school students by placing a stronger emphasis on diversity and life experiences as part of a holistic admissions process because racial diversity was aligned within the schools' priorities and various governing bodies of medical education such as the Association of American Medical Colleges. Therefore, despite the existence of the affirmative bans, organizations and their agents may have decoupled from ban to achieve racial diversity through other means. I evaluate this final tenet by conducting a sensitivity analysis to investigate when the impact of the ban has been strongest for STEM and non-STEM URM students. I hypothesize that formal rules have decoupled themselves actual practice so the impact of the ban should be strongest early on and wane after time. In the next section, I describe the STEM pathway and isolate where along the pathway the mechanisms I detailed occur.

WHY STEM DEGREE ATTAINMENT IS MORE DIFFICULT FOR UNDERREPRESENTED MINORITY STUDENTS

The challenges to attaining a bachelor's degree in a STEM field begin before college for underrepresented minority students.[10] URM students are less likely to graduate from high school (Musu-Gillette, Robinson et al. 2016), and on average perform lower in mathematics, putting them at a disadvantage in terms of skills valued in STEM (Smyth and McArdle 2004, Adelman 2006, Riegle-Crumb, Moore et al. 2011). They are more likely to come from K-12 school districts with a higher percentage of URMs (Flores 2007, Musu-Gillette, Robinson et al. 2016) that have fewer math and science courses available (Adelman 2006), and that are more likely to be underfunded (Logan, Minca et al. 2012, Musu-Gillette, Robinson et al. 2016). Their teachers are more likely to be less qualified (Clotfelter, Ladd et al. 2005, Goldring, Gray et al. 2013), are less likely to be of the same racial group (National Science Board 2016), which is problematic because Black and Hispanic students are perceived by their mostly non-URM teachers to be less capable to take on difficult coursework (Oakes 1995, Ferguson 1998), are downwardly tracked in schools (Gamoran and Mare 1989), and their counseling is less likely to be directed towards college preparation (Musu-Gillette, Robinson et al. 2016). On average, their parents are also less educated. The percentage of children who had at least one parent earn a bachelor's degree is substantially lower for Black and Hispanic children (lower than 20%) than is for White (44%) and Asian (59%) children (Musu-Gillette, Robinson et al. 2016). Furthermore, URM students are

[10] In this section I detail the entire pathway to a student becoming a STEM graduate. Although affirmative action only directly impacts the admission of a student into a school I detail the whole pathway to contextualize the issues for URMs in STEM.

less likely to have a parent working in a STEM field (Wang 2013), which is additionally burdensome because children who have parents working in a STEM field are more likely to select a STEM major in college (Chute 2009).

The next set of challenges arises at college entry. This is precisely where this study enters the picture. The proportions of 18-22 year old Blacks, Hispanics, and American Indians in any college are already much lower than the proportions of Asians and Whites in any college (Musu-Gillette, Robinson et al. 2016), and groups of URM students are even less represented at more selective institutions (Massey, Charles et al. 2011, Bowen and Bok 2016). Affirmative action bans impact all URM student's (Backes 2012, Hinrichs 2012) making access to higher education more difficult. As previously mentioned, the bans have also made more difficult for URM students to get into the most selective universities (Backes 2012, Hinrichs 2012), its *de jure* impact on public schools may also extend as a *de facto* impact on private schools (Backes 2012, Hinrichs 2012), and that at least for medical schools the impact of the bans was most notable immediately after the onset of the bans and waned after time. I argue that anti-affirmative action legislation has hit STEM harder than non-STEM majors using Tilly's concept of opportunity hoarding. The crux of the URM-STEM argument is that racialized control of resources is greater in STEM than non-STEM fields because they carry higher levels of prestige and economic value (Killewald 2012, Hershbein and Kearney 2014), hence opportunity hoarding explains the drive to acquire STEM resources for one's own racial group. I use opportunity hoarding to describe a larger macro-level phenomenon where anti-affirmative action policy leverages access to education away from URMs and into the hands of non-URM.

After college entry, the final hurdles these students face occur once they are STEM majors. There are no differences in the proportion of racial groups selecting into STEM majors [relative to non-STEM majors] between White, Black, and Hispanic students in STEM fields, but Asians do select into STEM majors in higher proportion (Anderson and Kim 2006, Chen 2009, Ma 2009). Attrition from STEM is a serious problem for URM students. As was the case with entry into STEM, high school academic preparation also affects persistence within STEM (Griffith 2010, Price 2010), because having higher SAT scores and having taken more AP courses increase STEM persistence (Kokkelenberg and Sinha 2010, Ost 2010). Departure from STEM by switching major is also a major issue. Riegle-Crumb et al. (2019) find that Black and Latina/o students are significantly more likely than their White peers to switch and earn a degree in another field. Riegle-Crumb et al. (2019) argue that opportunity hoarding also occurs for STEM on a micro-level that aggregates to the meso-level of college. They argue that racial macroaggressions along with the relative lack of support and inclusion on the part of predominantly White faculty and fellow classmates are expressions of opportunity hoarding that ultimately creates a toxic environment and drives underrepresented students away from STEM (Gurin, Dey et al. 2002, Hurtado, Eagan et al. 2011). One of the causes for this behavior appears to lie in the intensified competition for seats in the classroom. Because STEM fields do not have enough seats in classrooms to accommodate all interested students, "weed-out" culture is created where the grading curves and/or the material being taught in courses are aimed at increasing attrition to yield the highest quality students and to meet their enrollment management limits. One of the ways that this competitiveness can be measured is by comparing the lower GPAs of STEM majors to the higher GPAs of non-STEM majors (Johnson 2006). URM students are particularly vulnerable in this culture because of their lack of academic preparation, and the schema and stereotype that they are less academically prepared makes them targets in STEM. As a result, URM students are also more likely to experience racial microaggressions in STEM (Solorzano, Ceja et al. 2000, Brown, Henderson et al. 2016). In addition to the weed-out culture and racial microaggressions, there are also fewer URM faculty members in STEM fields (Li and Koedel 2016), and that is problematic because URM students are more successful when matched to an instructor of the same race or ethnicity (Ehrenberg, Goldhaber et al. 1995, Dee 2004). Due to the uninviting socio-cultural conditions in STEM, some studies have shown that many URM students formed supportive peer networks within STEM majors to buffer themselves from the intense climate (Harper 2010, Hurtado, Newman et al. 2010, Gasiewski, Eagan et al. 2012), but it is still not enough to overcome the racial gaps in STEM.

In the two previous sections, I articulated my hypotheses and contextualized them around the literature on affirmative action bans, racial issues in STEM attainment, laissez faire racism, opportunity hoarding, and a racialized theory of organizations. For ease of readability, I recapitulate the aforementioned hypotheses below:

1. Affirmative action bans have led to a decline in the share of URM STEM completion (i.e. the proportion of STEM graduates that are URM students).
2. There is a greater impact of the ban on URM STEM majors compared to non-URM STEM majors.
3. The more selective a postsecondary institution is the more it is impacted by affirmative action bans.
4. Only public institutions experience a decrease in shares of URM degree completion due to affirmative action bans.

5. Affirmative action bans have the greatest impact 4 years after going into effect (because it takes time for the impacted students to graduate) and wane after time.

DATA AND METHODS

Data

This study uses data from the Integrated Postsecondary Education Data System (IPEDS) and the Current Population Survey (CPS) from 1991-2016, taking advantage of rich state-level variation that captures the implementation years of the bans in each state. This time period maximizes the number of observations in the analytic sample and allows for timing lags to investigate when effects became observable after a state's ban went into effect using a difference-in-differences analytic design. Importantly I include the years 2012 to 2016, building on the undergraduate affirmative action studies pioneered by Hinrichs (2012) and Backes (2012). Doing so leverages the influence of the data points from schools in Texas and California to include more years for other states that have passed affirmative action bans and to states that have passed bans since the time of those previous studies (e.g., Arizona). However, to appropriately contrast my findings to those of Backes and Hinrichs, I perform several sensitivity analyses which I describe in this section that retrofit the data to limit the analysis to the years of to those available to Backes and Hinrichs to assess how the analysis differs when including more data points. I also demonstrate how variations in the model specification change the results, and that my findings would be similar to theirs by using specifications similar to the ones they used.

The units of analysis for this study are 4-year bachelor's degree-granting postsecondary institutions. Because the aim of this study is to observe how affirmative action bans affected the attainment of STEM degrees for URM students, the dependent variables used in this study are the proportion of URM STEM graduates (i.e., the proportion of STEM graduates that are underrepresented minority students) and the proportion of URM non-STEM graduates (i.e. the proportion of non-STEM graduates that are underrepresented minority students) at each institution. This allows for a comparison between STEM and non-STEM students. The numerator is the number of URM STEM graduates (i.e., URM students who have graduated with a STEM degree) and the denominator is the total number of STEM graduates (same process is followed to produce the non-STEM proportion). The STEM/non-STEM distinction was based on the CIP codes provided by the Department of Homeland Security (2016) to classify STEM students (See Appendix A). I also conduct another analysis using the National Science Foundation's (2019) classification (See Appendix B). I made the decision to use the Department of Homeland Security's (DHS) measure because I felt it had stronger content validity. The difference between the two is that the NSF includes Architecture as STEM, but excludes Science Technologies and Technicians, Psychology, and Health Professions and Related Programs. My preference was strongly weighted by the choice to include a STEM measure that also captured Science Technologies and Technicians; a category which I believe strongly defines STEM. While I use the STEM measure provided by DHS I also include results based on the NSF version in Appendix C (Table 3.C). I do this because multiple measures of STEM have led to different narratives around the role of STEM in education and occupations (Stevenson 2014). The U.S. Department of Commerce has one for the workplace, the Department of Homeland Security has another one directly relating to a student's eligibility for an F-1 visa through the STEM OPT extension, and the National Science Foundation has separate classifications for STEM majors and STEM occupations. Furthermore, other scholars create their own STEM classifications (e.g., Riegle-Crumb et. al. 2019).

Many state-level variables from the CPS (Current Population Survey) are included to control for state-level effects on URM STEM and non-STEM attainment and they can be found in Table 3.1. Adjusting for state-level characteristics in a multivariate analysis addresses the fact that states are not interchangeable units of analysis; they have unique characteristics that may be influential to the analysis. They include state-level racial demographics (a percentage variable for each population that is White, Black, Latino, Native American, and other), state-level educational attainment as measured by the percentage of the population 25-34 years old with at least a bachelor's degree and state-level economic indicators: the unemployment rate of the population most susceptible to the impact of the bans upon graduation (25- to 34-year-olds) and median personal income (also for 25- to 34-year-olds).

Table 3.1 State-Level Control Variables for Ban and Non-Ban States

Ban States	Percent White	Percent Asian	Percent Latino	Percent Black	Percent Native American	Percent 25-34 with a Bachelor's degree	Unemployment rate for 25-34 year-olds
California	39.06%	15.26%	38.93%	6.20%	0.53%	25.18%	5.27%
Texas	43.41%	4.97%	39.05%	12.18%	0.37%	21.77%	4.34%
Florida	55.74%	3.05%	24.87%	16.04%	0.29%	21.04%	4.92%
Washington	71.51%	9.82%	12.44%	4.67%	1.53%	26.24%	4.36%
Michigan	76.42%	3.29%	4.95%	14.60%	0.71%	21.63%	5.57%
Nebraska	80.48%	2.62%	10.66%	5.31%	0.91%	30.60%	2.68%
Arizona	56.47%	0.036	30.94%	4.74%	4.23%	19.54%	5.36%
New Hampshire	91.80%	2.84%	3.52%	1.53%	0.28%	28.36%	2.93%
Comparison States							
Arkansas	74.00%	1.90%	7.31%	15.94%	0.82%	16.62%	5.30%
Colorado	69.78%	3.56%	21.31%	4.56%	0.77%	29.93%	3.60%
Connecticut	68.54%	4.80%	15.72%	10.65%	0.26%	28.82%	6.19%
DC	37.08%	4.40%	10.92%	47.30%	0.27%	35.31%	3.90%
Hawaii	25.81%	60.92%	10.37%	2.63%	0.25%	23.93%	2.52%
Illinois	62.43%	5.68%	17.04%	14.64%	0.20%	28.80%	5.15%
Indiana	80.50%	2.38%	6.78%	10.04%	0.27%	22.62%	4.12%
Iowa	87.06%	2.67%	5.82%	4.10%	0.33%	25.37%	2.56%
Kansas	77.54%	3.18%	11.64%	6.62%	0.99%	24.17%	3.40%
Maryland	52.52%	6.90%	9.75%	30.52%	0.29%	26.40%	4.88%
Massachusetts	73.94%	6.86%	11.46%	7.50%	0.22%	32.64%	3.91%
Minnesota	81.70%	5.15%	5.24%	6.67%	1.21%	30.01%	3.32%

Missouri	80.78%	2.26%	4.11%	12.29%	0.54%	23.80%	4.67%
Nevada	51.48%	9.53%	28.45%	9.54%	0.98%	16.76%	6.04%
New Jersey	56.47%	9.94%	19.97%	13.41%	0.18%	30.07%	5.02%
New Mexico	38.79%	1.66%	48.52%	2.15%	8.85%	16.20%	6.05%
New York	56.50%	9.00%	18.97%	15.15%	0.35%	29.20%	5.11%
North Carolina	64.44%	3.05%	9.18%	22.11%	1.19%	23.22%	5.56%
North Dakota	86.04%	1.65%	3.63%	3.20%	5.45%	27.34%	3.73%
Ohio	80.41%	2.36%	3.65%	13.30%	0.25%	22.84%	5.00%
Oklahoma	69.23%	2.52%	10.29%	8.45%	9.49%	20.46%	5.06%
Oregon	78.19%	5.25%	12.76%	2.40%	1.38%	24.39%	4.56%
Pennsylvania	77.84%	3.56%	7.04%	11.36%	0.17%	26.97%	5.06%
Rhode Island	74.34%	3.67%	14.89%	6.58%	0.50%	29.42%	4.47%
South Dakota	83.52%	1.58%	3.71%	2.34%	8.83%	26.76%	4.32%
Utah	80.00%	3.74%	13.77%	1.37%	1.09%	25.22%	3.28%
Vermont	94.25%	1.87%	1.86%	1.56%	0.43%	28.73%	3.13%
Virginia	63.66%	7.03%	9.10%	19.89%	0.30%	26.42%	4.29%
West Virginia	93.30%	0.92%	1.54%	4.00%	0.21%	15.84%	7.96%

Another set of variables accounts for characteristics of the bachelor's degree-granting institution (enrollment size as a weight and not a covariate, public/private status, and selectivity). Enrollment size is used to weigh the variables in the analysis to reflect what the affirmative action bans mean with respect to a typical student, and not what the affirmative action bans mean with respect the typical school (which is what the unweighted analysis does). [This distinction is clarified in Table 3.3 when I present both weighted and unweighted results]. Although I present both results in the table, the findings are written to reflect the typical student (i.e., the weighted results). The public/private distinction allows one to investigate whether the ban extended beyond its *de jure* targets: public schools. Whether the college was public or private was coded as a binary variable where 0 indicates "private" and 1 indicates "public." To measure selectivity colleges are classified by their Barron's Admissions Competitiveness Index with categories of "Most Competitive;" "Highly Competitive;" "Very Competitive;" "Competitive;" "Less Competitive;" and "Noncompetitive." An additional sensitivity analysis uses SAT scores instead of the Barron's index, as in previous studies. The main results use the Barron's data because they include a larger sample of schools- not all schools submit their SAT scores or require them for admission.

Table 3.2 below shows the distribution of Barron's index selectivity scores across the states with and without affirmative action bans and public and private institutions. 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), the categories "Most Competitive," "Highly Competitive," and "Very Competitive" were recoded as "Highly Selective"; and "Competitive," "Less Competitive," "Noncompetitive," and "Special" were recoded as "Less Selective"; and schools that were not indexed by Barron's Admissions Competitiveness Index were recoded as "Unclassified."

Table 3.2 Number of Institutions by Barron's Classification

Barron's Selecitvity Index	Public Institutions: Most Competative	Highly Competative	Very Competative	Competative	Less Competative	Non-competative	Special	Not Indexed	Total (Public)
Ban States	*1*	*10*	*17*	*51*	*15*	*9*	*1*	*92*	*196*
California	1	4	3	17	5	0	0	14	44
Texas	0	3	0	12	8	6	0	12	41
Florida	0	2	5	4	0	0	0	29	40
Washington	0	0	2	4	0	0	0	25	31
Michigan	0	1	4	9	1	0	0	6	21
Nebraska	0	0	1	2	0	3	0	0	6
Arizona	0	0	1	2	0	0	0	5	8
New Hampshire	0	0	1	1	1	0	1	1	5
Comparison States	*7*	*17*	*53*	*138*	*38*	*33*	*9*	*77*	*295*
Arkansas	0	0	3	4	0	2	0	1	10
Colorado	1	1	2	7	2	0	0	3	16
Connecticut	0	2	0	4	0	0	1	3	10
DC	0	0	0	0	0	0	0	0	0
Hawaii	0	0	1	1	0	0	0	2	4
Illinois	0	1	2	7	0	0	0	2	12
Indiana	0	0	2	3	8	0	0	2	15
Iowa	0	0	2	1	0	0	0	0	3
Kansas	0	0	1	4	0	2	0	1	8
Maryland	1	2	3	5	0	0	1	1	13
Massachusetts	0	0	0	11	1	0	2	0	14
Minnesota	0	1	2	6	1	0	1	1	12
Missouri	0	0	6	3	0	2	1	1	13
Nevada	0	0	0	0	0	1	0	5	6
New Jersey	1	2	5	6	0	0	1	0	15
New Mexico	0	1	0	2	2	1	0	3	9
New York	1	4	8	17	1	4	1	4	40
North Carolina	1	0	4	4	6	0	1	0	16
North Dakota	0	0	1	1	2	2	0	3	9
Ohio	0	1	1	5	1	8	0	18	34
Oklahoma	0	0	2	5	2	3	0	3	15
Oregon	0	0	0	6	0	1	0	1	8
Pennsylvannia	0	1	1	15	6	1	0	18	42
Rhode Island	0	0	0	2	0	0	0	0	2
South Dakota	0	0	1	4	1	1	0	0	7
Utah	0	0	1	1	0	2	0	3	7
Vermont	0	0	1	3	1	0	0	0	5
Virginia	2	1	4	7	1	0	0	0	15
West Virginia	0	0	0	4	3	3	0	2	12
Total (by private/public and selectivity)	***8***	***27***	***70***	***189***	***53***	***42***	***10***	***169***	***491***
% Selectivity within Public/Private	**1.63%**	**5.50%**	**14.26%**	**38.49%**	**10.79%**	**8.55%**	**2.04%**	**34.42%**	**100.00%**

Barron's Selecitvity Index	Private Institutions: Most Competative	Highly Competative	Very Competative	Competative	Less Competative	Non-competative	Special	Not Indexed	Total (Private)	Total (All Schools)
Ban States	*12*	*19*	*27*	*72*	*20*	*12*	*13*	*117*	*292*	*488*
California	8	7	9	17	2	3	8	42	96	140
Texas	1	6	4	14	7	1	0	20	53	94
Florida	1	1	3	14	6	1	2	34	62	102
Washington	1	2	2	4	0	3	1	2	15	46
Michigan	0	3	6	10	3	3	1	6	32	53
Nebraska	0	0	3	6	1	1	1	5	17	23
Arizona	0	0	0	2	0	0	0	5	7	15
New Hampshire	1	0	0	5	1	0	0	3	10	15
Comparison States	*53*	*48*	*128*	*257*	*50*	*14*	*34*	*239*	*823*	*1118*
Arkansas	0	2	3	2	1	1	0	3	12	22
Colorado	0	1	2	2	0	0	1	6	12	28
Connecticut	3	2	1	5	2	0	0	3	16	26
DC	1	1	1	1	1	0	1	2	8	8
Hawaii	0	0	2	0	0	0	0	5	7	11
Illinois	2	6	8	22	3	0	3	14	58	70
Indiana	2	0	10	13	1	1	1	6	34	49
Iowa	0	1	9	11	3	2	0	6	32	35
Kansas	0	0	0	12	1	1	0	8	22	30
Maryland	1	2	4	5	0	0	1	5	18	31
Massachusetts	9	11	5	15	8	0	6	10	64	78
Minnesota	2	2	6	7	0	0	1	8	26	38
Missouri	1	0	9	13	0	0	2	15	40	53
Nevada	0	0	1	0	0	0	0	0	1	7
New Jersey	1	1	2	6	5	0	0	11	26	41
New Mexico	0	1	0	1	0	0	0	0	2	11
New York	10	9	21	26	5	1	5	63	140	180
North Carolina	3	1	3	20	5	0	1	13	46	62
North Dakota	0	0	0	2	0	0	0	3	5	14
Ohio	3	1	9	22	3	1	7	15	61	95
Oklahoma	0	1	2	3	0	2	0	5	13	28
Oregon	1	0	3	7	0	1	1	5	18	26
Pennsylvannia	10	4	15	33	6	3	3	11	85	127
Rhode Island	1	1	2	2	0	0	1	2	9	11
South Dakota	0	0	1	3	1	1	0	0	6	13
Utah	0	0	2	0	0	0	0	8	10	17
Vermont	1	1	3	4	2	0	0	1	12	17
Virginia	2	0	4	13	3	0	0	9	31	46
West Virginia	0	0	0	7	0	0	0	2	9	21
Total (by private/public and selectivity)	***65***	***67***	***155***	***329***	***70***	***26***	***47***	***356***	***1115***	***1606***
% Selectivity within Public/Private	**5.83%**	**6.01%**	**13.90%**	**29.51%**	**6.28%**	**2.33%**	**4.22%**	**31.93%**	**100.00%**	

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

I used a difference-in-differences strategy to estimate the impact of affirmative action bans on the proportion of URM STEM graduates (i.e. the proportion of STEM graduates that are underrepresented minority students). This strategy has been used by prominent studies that examine the impact of policy changes on education outcomes (Dynarski 2004, Long 2004) and has also been specifically used by scholars who study bans on affirmative action (Backes 2012, Hinrichs 2012, Garces 2013, Garces and Mickey-Pabello 2015). The "first difference" in this strategy compares the proportion of URM STEM graduates before and after an affirmative action ban was put in place, to determine whether changes are associated with the start of the ban. If the affirmative action ban did have an impact on the proportion of URM STEM graduates, there would be a decline after the policy went into effect. However, because the proportion of URM STEM graduates may differ from year to year for other reasons (e.g., period effects, 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, which take 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 STEM graduation 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 the proportion of URM STEM graduation remains.

I implement a differences-in-differences estimation strategy in a multilevel regression framework and then apply difference-in-differences-in-differences, using fixed to account for the hierarchical nature of the data (observations over time, nested within states) (Murnane and Willett 2011). Furthermore, to account for the standard errors that are often poorly estimated in difference-in-differences models (i.e., not as well estimated by state fixed effects alone or robust standard errors) I use state-clustered standard errors to provide more conservative estimate for the impact of affirmative action bans (Bertrand, Duflo et al. 2004).

The following multilevel ordinary least squares regression was fitted (the levels are colleges and states [s]):

$$STEM_{ist} = \beta_0 + \beta_1(BAN_{st}) + \beta_2 W_{st} + \beta_3 I_{it} + \beta_4 cyear_t + \gamma S_s + nScyear_{st} + \alpha Z_t + \varepsilon_{ist}$$

(1)

where $STEM_{ist}$ indicates the proportion of STEM (or non-STEM) degrees awarded; BAN_{st} is a dichotomous variable indicating whether a state (*s*) had an affirmative action ban in place in year (*t*). Here year *t* is lagged by four years because the college freshmen that were first impacted by affirmative action need at least four years to graduate. This is essential, because it is unlikely that changes in the proportion of URM STEM graduates would change immediately after the bans.[11] ; W_{st} represents a matrix of selected time-varying state characteristics, such as state-level racial demographics (i.e., percentage of population that is White, Black, Latino, Native American, or other), state-level educational attainment (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 (25- to 34-year-

[11] A sensitivity analysis was conducted that changed the timing of the ban. Several iterations where there was no lag, and three-, four-, five- and six-year lags were performed to measure when the impact of the bans was most prominent. The results indicated that the affirmative action bans did not immediately impact college completion, and that the four-year lags were the optimal choice.

olds). These state level characteristics are used to control for the fact that states are not interchangeable units. I_{it} [not to be confused with the identity matrix] represents a matrix of institutional characteristics (i.e., enrollment size, percentage of students receiving financial aid, institutional selectivity, and cost of tuition); S_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); $cyear_t$ represents a continuous-year variable (coded so that 1993=1, 1994=2, 1995=3, etc.); $Scyear_{st}$ represents a full set of two-way interactions between each state dummy and a continuous predictor representing the linear effect of year;[12] 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 1993 to 2016; and ε_{ist} represents the residual.[13] 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; including both does 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 proportion of URM STEM college completions (i.e., the proportion of STEM graduates who are underrepresented minorities).

I further estimate difference-in-differences-in-differences estimates (Ravallion et al. 2005) to estimate the heterogeneity in influence of affirmative action bans produced by school

[12] To avoid collinearity with the continuous variable *cyear,* Equation (2) does not include year dummies.
[13] This specification of the multilevel model uses a combination of fixed and random efforts 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.

selectivity. This third difference is important because affirmative action bans are hypothesized to impact the most coveted highly competitive resources the most. This third difference subtracts the difference-in-differences result for the baseline group ("very competitive plus") from other groups classified by selectivity (e.g., "competitive"). The difference-in-differences-in-differences estimate is specified:

$$STEM_{ist} = \beta_0 + \beta_1(BAN_{st}) + \delta(BAN_{st}Selectivity_i) + \beta_2 W_{st} + \beta_3 cyear_t + \beta_4 I_{it} + \gamma S_s$$

$$+ n_s Scyear_{st} + \alpha Z_t + \varepsilon_{isj} \tag{3}$$

where δ is introduced as the difference-in-differences-in-differences estimator for an interaction between a school's selectivity ($Selectivity_i$) and a ban being present in a state given a particular year. The $Selectivity_i$ component of the interaction is also present in *I* which represents a matrix of institutional characteristics. This analysis helps to answer the question of whether higher levels of selectivity at 4-year institutions correspond to higher impacts of affirmative action bans.

Lastly, to investigate if the ban was most impactful shortly after or long after the bans were implemented, as Mickey-Pabello and Garces (2018) have done, purposeful right-censoring is used to determine when the impact of the bans was strongest (i.e., restricting the ban to just 2 years of influence: 6 years after implementation minus 4 years of lag to allow students to graduate). Sensitivity analyses such as these are generally required when using difference-in-differences and difference-in-differences-in-differences because they are quasi-experimental methods subject to alternative explanations that need to be considered or ruled out (Shadish, Cook et al. 2002).[14]

Parallel Trend Assumption and Statistical Power

An important assumption of the difference-in-differences approach is that the proportion of STEM and non-STEM URM college completion 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. I compare these trends to ensure that whatever change in the pattern is observed between the ban and non-ban states after the bans were implemented is attributable to having implemented a ban and not some other factor that is not accounted for in the model.[15] Figure 3.2 presents graphs for the proportion of URM STEM and non-STEM graduates in each of the states that ultimately implemented an affirmative action ban, and for the proportion of URM STEM and non-STEM graduates in each of the 32 states in the comparison group during the years investigated by this study. Each graph shows a particular ban state in the years prior to and after the implementation of the bans and contrasts it to the comparison states during those same years. The comparison states' trend lines look different between graphs for two reasons. First the axes are different between states and were adjusted so that changes in the trend lines

[14] This analysis did not change over time. The results were consistent 4-years, 5-years, and 6-years after the bans. The results here are in contrast to Mickey-Pabello and Garces (2018) who concluded that the impact of affirmative action bans had waned over time for medical schools.

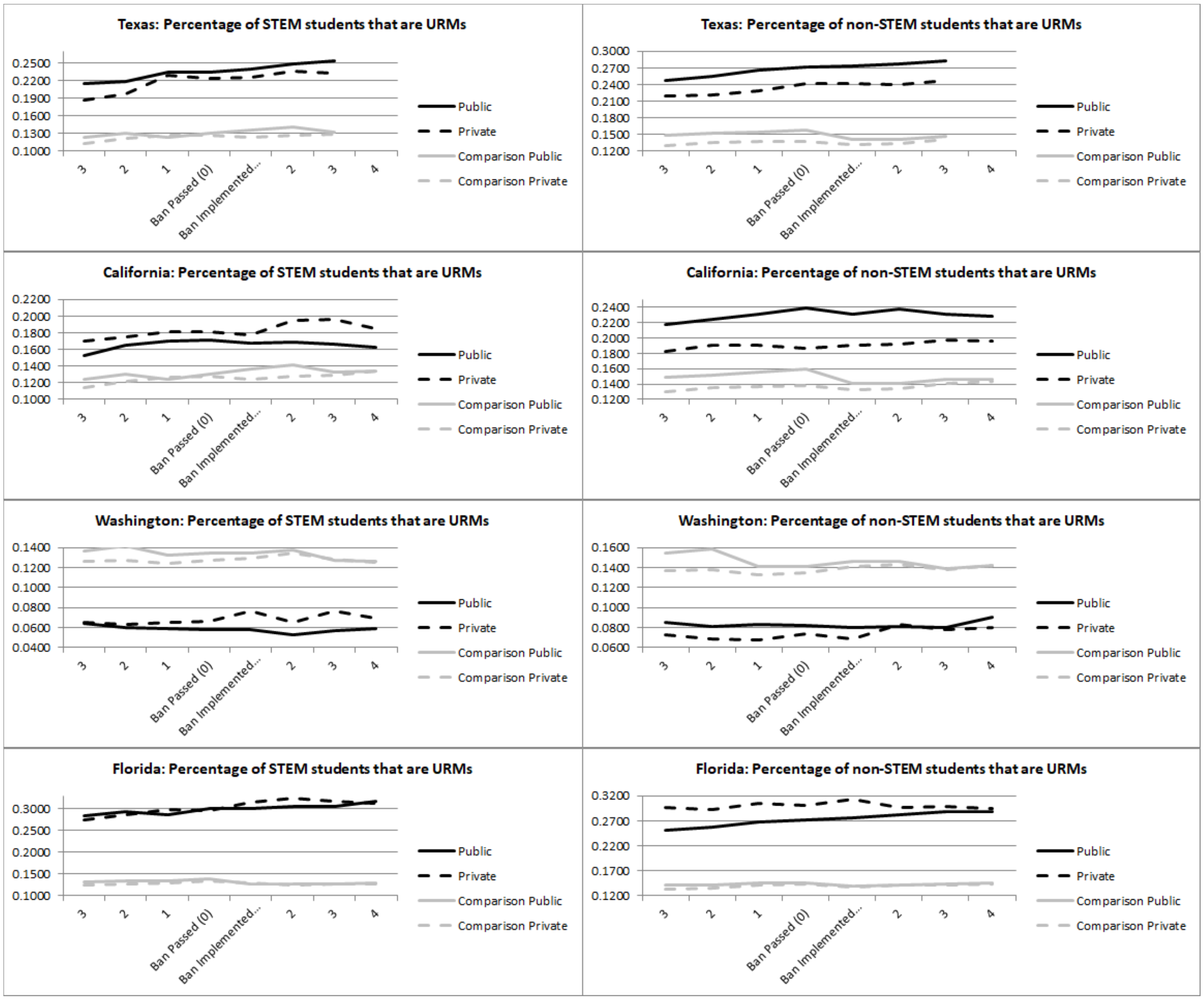
[15] A problem with resolving the parallel assumptions trend comes when the treatment is too dynamic. In the case of affirmative action bans the bans (i.e., the treatment) were implemented at several points in time. The graphs shown are cumbersome, difficult to read, and can be misleading when there are a small number of cases that cause the graphs to vary more on a year-to-year basis than they should. A model proposed by Mora and Reggio (2015) accounts for the dynamic nature of this implementation. Because there are multiple pre-treatment and post-treatment periods, multiple tests on the equality of the effect on pre- and post-treatment periods are calculated. The statistic of the equivalence of the parallel assumptions is the estimated effect on the last pre-treatment period. A limitation of this dissertation is not including this estimate, but future drafts of the three empirical chapters will include these more formal tests of the parallel assumptions trend.

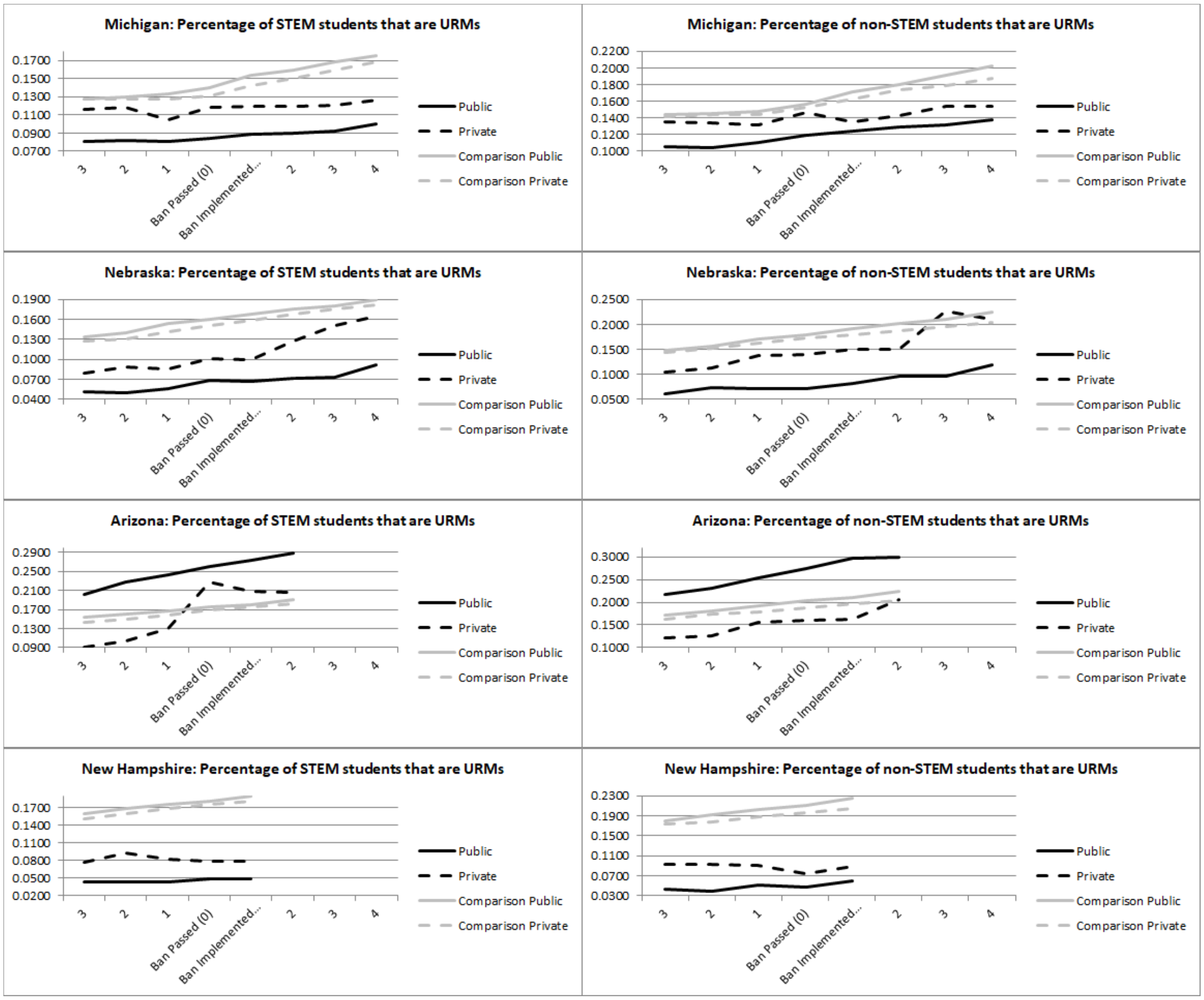
could be best observed. Secondly, the bans occurred at different times for different states. For instance, the ban was implemented in California in 1997 (passed in 1996) and implemented in Michigan in 2007 (passed in 2006).

Because I hypothesize that the difference-in-difference result will show a decline in URM graduation (for STEM or non-STEM majors), then for each state we should observe a decline in the percentage of URM graduation in ban states relative to non-ban states in the period following the ban. In other words, if there is a gain in URM graduation in ban states then we should expect the gain in the non-ban states to be greater. Likewise, if there is a decrease in the URM graduation in the ban states then we should expect the decrease in the non-ban states to be small. California, Washington, and Michigan follow the hypothesized pattern. Nebraska also follows the expected pattern, but only for public schools. Arizona also follows the pattern, but only for non-STEM majors. It is too difficult to discern a clear pattern for New Hampshire, because it only contributes one year of post-ban data to the analysis. Texas runs contrary to the expected pattern; the gain in public and private STEM and non-STEM URM graduates outpace any gain (or decline) in the comparison states. This pattern was also true for Florida to a greater extent which makes it the best candidate to see how removing it or excluding it impacts the results from the analysis.[16]

[16] To further investigate how any violation of the parallel assumption trend could have impacted the results of this study I conducted several sensitivity analyses where individual states were removed from the analysis. Even for Florida, the most problematic state, the results of the study were robust to the choice include or exclude it. This in short, also addresses the sixth hypothesis posed in this paper, although as I discuss support for this argument is mixed.

Figure 3.2. Parallel Trends Assumption for STEM and non-STEM





FINDINGS

The main motivation for this paper was to explore whether anti-affirmative action policy, the policy manifestation of *laissez-faire* racism, impacts an already restricted STEM pipeline for URM students. The findings (as presented in Table 3.3 below) indicate that the share of URM STEM college completion has decreased as the result of affirmative action bans. I present these findings as both unweighted [emphasizing the typical school] and weighted by enrollment [emphasizing the typical student]. The unweighted results show that at the typical school of any level of selectivity the share of URM STEM college completion significantly decreased for public schools (-0.0180) and private schools (-0.0106). Similarly, the weighted results show that the shares significantly decrease for both public and private schools (-0.0255 and -0.0145 respectively). I also provide an alternative measure of STEM degrees in Appendix C (Table 3.C) based upon the NSF's classification of STEM, but the results shown in Table 3.3 below represent the Department of Homeland Security's STEM classification.

Table 3.3 Difference in Differences Results: Impact of Affirmative Action Bans on URM Degree Attainment

	STEM								Non-STEM							
	Public				Private				Public				Private			
	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.
Selectivity by Barron's (w/o FL)																
All States (baseline)	-0.0156	***	-0.0147	***	-0.0048		0.0002		-0.0130	***	-0.0137	***	-0.0092	***	-0.0075	***
Any Selectivity (including Unclassified)	-0.0180	***	-0.0237	***	-0.0106	*	-0.0236	***	-0.0156	***	-0.0171	***	-0.0094	***	-0.0122	***
Highly Selective (w/o unclassified)	-0.0198	***	-0.0210	***	-0.0084		-0.0072		-0.0149	***	-0.0154	***	-0.0068	***	-0.0041	***
Less Selective (w/o unclassified)	-0.0188	***	-0.0253	***	-0.0055		-0.0243	***	-0.0171	***	-0.0200	***	-0.0046	***	-0.0089	***
Unclassified Only	-0.0115	***	-0.0227	***	-0.0077		-0.0050		-0.0035	*	-0.0161	***	0.0031	*	0.0108	***
Timing Considered (Any Selectivity w/o FL)																
2-Year Post-Ban Analytic Period	-0.0076	***	-0.0009		0.0107		-0.0147		-0.0067	***	0.0006		-0.0021	*	-0.0131	***
3-Year Post-Ban Analytic Period	-0.0096	***	-0.0053	***	0.0101	*	-0.0100		-0.0089	***	-0.0030	*	0.0003		-0.0080	***
4-Year Post-Ban Analytic Period	-0.0122	***	-0.0092	***	0.0085		0.0045		-0.0089	***	-0.0070	**	-0.0012		-0.0023	
5-Year Post-Ban Analytic Period	-0.0115	***	-0.0093	***	0.0058		-0.0102		-0.0103	***	-0.0064	***	-0.0019	*	-0.0092	***
6-Year Post-Ban Analytic Period	-0.0125	***	-0.0115	***	0.0022		-0.0139	*	-0.0109	***	-0.0081	***	-0.0033	***	-0.0101	***
9-Year Post-Ban Analytic Period	-0.0130	***	-0.0154	***	0.0006		-0.0100		-0.0110	***	-0.0109	***	-0.0024	***	-0.0065	***
Full Window	-0.0180	***	-0.0237	***	-0.0106	*	-0.0236	***	-0.0156	***	-0.0171	***	-0.0094	***	-0.0122	***
Timing Considered (Highly Selective w/o FL & Excluding Unclassified)																
2-Year Post-Ban Analytic Period	-0.0094	**	-0.0044		0.0125		0.0044		-0.0052		-0.0019		-0.0017		-0.0052	*
3-Year Post-Ban Analytic Period	-0.0116	***	-0.0082	**	0.0113		0.0057		-0.0077	**	-0.0052	*	-0.0003		-0.0018	
4-Year Post-Ban Analytic Period	-0.0122	***	-0.0092	***	0.0085		0.0045		-0.0089	***	-0.0070	**	-0.0012		-0.0023	
5-Year Post-Ban Analytic Period	-0.0131	***	-0.0109	***	0.0063		0.0030		-0.0095	***	-0.0080	***	-0.0031	*	-0.0042	**
6-Year Post-Ban Analytic Period	-0.0143	***	-0.0126	***	0.0023		-0.0004		-0.0104	***	-0.0093	***	-0.0044	**	-0.0055	***
9-Year Post-Ban Analytic Period	-0.0148	***	-0.0158	***	0.0007		0.0005		-0.0103	***	-0.0110	***	-0.0034	**	-0.0023	
Full Window	-0.0198	***	-0.0210	***	-0.0084		-0.0072		-0.0149	***	-0.0154	***	-0.0068	***	-0.0103	***

To understand the magnitude of the bans I take the average share of URM STEM college completions in states with an affirmative action ban in the years before the bans went into effect for public schools (0.1331) and private schools (0.1201) (for all states and any level of selectivity) and add or subtract the estimate produced from the models to create an estimate of the average post-ban share of URM students at schools to create an estimated percentage difference. I report these results in Table 3.4 below. I repeat the same process for non-STEM majors. Interpreting the findings in this manner, there is a decline of 12% in the share of URM students completing college at public schools with a STEM major, a decline of 4% in the share of URM students completing college at private schools with a STEM major. The declines are 8% and 6% for public and private schools respectively for non-STEM majors.[17] These results confirm the first hypothesis that affirmative action bans have led to a decline in the share of URM STEM completion, and also confirm that affirmative action bans have led to a decline in the share of URM non-STEM completion.

Table 3.4 Understanding the Magnitude of the Bans

STEM

	Pre-Ban	Ban	Estimate	Percentage Difference
Public	0.1331	-0.0163	0.1168	-12.2464
Private	0.1201	-0.0048	0.1153	-3.9967

Note: The private ban estimate for STEM was not statistically significant in Table 3. The estimates in this table are based on the ones from the "All States Considered" part of Table 3.4.

The next hypothesis investigates if the decline in the shares of URM *STEM* and URM *non-STEM* college completion is greater for URM *non-STEM* students. When selectivity level is not taken into consideration the declines for STEM (-0.0190, -0.0255, -0.0063, and -0.0145) were greater than the declines for non-STEM (-0.0132, -0.0184, -0.0002, and -0.0058)

[17] The percentage differences are not estimated for every single difference-in-differences estimate because they are tautological. The difference-in-differences estimate and the percentage difference both indicate the direction and magnitude of the impact of affirmative action bans. Table 3.4 serves as tool to assist readers unfamiliar with difference-in-differences estimates.

respectively, for public unweighted and weighted, and private unweighted and weighted (the findings are not significant for unweighted private schools). These findings strongly support that affirmative action bans more heavily impacted the shares of URM STEM college completion than the shares of URM non-STEM completion (their magnitude is consistently greater for STEM than non-STEM). This confirms the second hypothesis.

The third hypothesis states that the more selective an institution, the more affirmative action bans would decrease the college access of URM students. The findings for the difference-in-differences analysis run contrary to expectation: it is not the most selective institutions that have the highest decreases in the share of URM graduates. Instead, it is mostly when all of the institutions are grouped by any level of selectivity that the findings become statistically significant (see row 1 of Table 5). Furthermore, of all of the coefficients where selectivity is considered, only the non-STEM private unweighted and weighted coefficients are statistically significant for schools classified as non-competitive and less competitive (-0.0014 [not significant], -0.0052 [significant], -0.0050 [significant], and -0.0078 [significant]). Beyond those statistically significant coefficients there are none, suggesting that selectivity does not play a meaningful role (See Table 5 below).

Table 3.5. Difference in Differences in Differences Results: Does Selectivity Matter?

	STEM								Non-STEM							
	Public				Private				Public				Private			
	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.
Ban	-0.0226	***	-0.0289	***	-0.0039		-0.0042		-0.017	***	-0.0219	***	-0.0013		-0.0046	***
Selectivity by Barron's (w/o FL; reference Ban*Most Competitive)																
Special	0.0043		0.0021		0.0005		0.0049		0.0056		0.0031		-0.0010		0.0038	
Unclassified	0.0013		0.0013		-0.0007		-0.0004		-0.0091		-0.0076		0.0000		0.0015	
Noncompetitive	0.0049		0.0094		-0.0072		-0.0271		-0.0033		0.0017		-0.0014		-0.0052	**
Less Competitive	0.0025		0.0012		-0.0055		-0.0227		0.0014		0.0010		-0.0050	*	-0.0078	**
Competitive	0.0028		0.0036		-0.0032		-0.0084		0.0026		0.0034		-0.0020		-0.0012	
Very Competitive	0.0038		0.0052		-0.0044		-0.0077		0.0041		0.0051		-0.0019		0.0004	
Highly Competitive	0.0016		0.0017		-0.0036		-0.0045		0.0011		0.0015		-0.0020		0.0009	
N	15,955		15,530		30,509		27,960		15,955		15,530		30,513		27,964	

The fourth hypothesis examines whether the *de jure* impact of affirmative action bans applies only to public schools and expects no impact of the affirmative action bans on URM completion at private schools. However, the results indicate that there are impacts on students attending private schools. For nearly every sensitivity test in Table 3, the share of URM non-STEM college completions at private schools significantly decreased as a result of the bans (the association is positive in one case, and not statistically significant in 3), but the decrease was only statistically significant in some conditions for URM STEM college completions at private schools. These results suggest that there is a *de facto* impact of affirmative action bans at private schools. However, the impacts at private schools are smaller in magnitude than the impacts at public schools. In every sensitivity analysis, with the exception of the one considering the 2-year post-ban analytic period, the absolute value of the coefficient at public schools was greater than or equal to the absolute value of the coefficient at private schools.

The fifth and final hypothesis considers when the ban was most effective. I hypothesized that the impact of the ban would be most effective early on, based on Mickey-Pabello and Garces (2018) findings about the impacts of affirmative action on medical schools. However, the exact opposite was true: the impact of the ban, as measured by the magnitude of the ban coefficient grew as the post-ban analytic period lengthened (see Table 3: *Timing Considered*). This was the case for schools of any selectivity and for the most highly selective schools. This suggests that Ray's (2019) tenet of institutional decoupling from a racial policy is not always true. Organizational networks are complicated. The results from Pabello and Garces (2018) study on medical schools, may be different from this one on undergraduate degree attainment not because of anything having to do with the selectivity of medical schools. Instead the difference in results may be due to the fact that accreditation violations have been given to medical schools for not being racially diverse (e.g., Wayne State University Medical School), but such sanctions are not common in undergraduate education.

I summarize all of the findings in the table below (see Table 6). The first hypothesis about the share of URM STEM completions declining was supported, as was the second hypothesis about the share of URM STEM completions being impacted more than the share of URM non-STEM completions. However, findings for the remaining three hypotheses are more intriguing because they clash with previous findings. In the remainder of this section I provide additional evidence for why previous studies may have arrived at findings different from my own.

Table 3.6. Hypothesis Summary

	Hypotheses	Supported / Unsupported	Notes
1	Affirmative action bans have led to a decline in the share of URM STEM completion	Supported	
2	Because STEM is so competitive relative to non-STEM majors there should be a greater magnitude in the decrease of URM STEM majors compared to non-URM STEM majors	Supported	
3	The more selective a school is the greater it is impacted by affirmative action	Unsupported	Affirmative action bans reduce the share of URM STEM and non-STEM college completions across all selectivity categories for 4-year private and public schools.
4	Only public institutions experience a decrease in shares of URM degree completion	Unsupported	Private institutions also experience a decline, but it is not as large as the decline for public institutions. Where else do students go? For- profits?

5	The results of the ban are most impactful early on and wane after time	Unsupported	The results of the ban are most impactful when post-ban period considered is the largest.

The first controversial finding in this paper is that highly selective schools were not impacted the most by affirmative action bans [*contra* (Backes 2012, Hinrichs 2012, Arcidiacono, Aucejo et al. 2014)]. There are few distinct ways in which previous studies differed from this one methodologically, in ways that could explain the difference: there are fewer units of analysis in other studies, other studies place a greater emphasis on Texas and particularly California, and selectivity was operationalized differently. Hinrich's (2012) study places more weight on results in California and Texas because they contributed more "school-years" [observations] relative to other bans states in his study because his analytic window spanned from 1995 to 2007 and because Texas and California have more schools than the other states. Backes' study (2012) is similar in emphasizing Texas and California because it spans from 1990 to 2009. Arcidiacono et al. use a wider analytic window, but only focus on California (2014). Additionally, Backes uses SAT quantiles to measure selectivity. He uses four groups: the first, second, and third deciles of highest SAT scores, and a fourth group for the lower seven deciles that yielded only 526 schools (far less than the 2,097 used in this study). To engage with the different methodological applications above I produce the following analyses in the table below. First, I exclude all of the ban states except California and Texas. Next, I reduce my analytic period from 1991 to 2009 to replicate the time periods used by previous studies. Finally, I use SAT deciles like the previous studies instead of the Barron's Admissions Competitiveness Index to measure selectivity.

There is a notable difference in the results between all states and just Texas and California using Barron's Admissions Competitiveness Index. For the weighted public STEM results for all states the negative effects of the ban are greater in highly selective schools than less selective ones, but when including all the ban states in the analysis the effects of the bans are lesser in highly selective schools than less selective ones. In summation, Texas and California exhibit a selectivity pattern not found in other states.

Changing the analytic period was also important. In the full-analytic period (through 2016) using Barron's Admissions Competitiveness Index and investigating California and Texas, highly selective schools were the least impacted schools, followed by less selective schools, and unclassified schools. However, when the analysis is right censored to 2009 (as was done in the aforementioned studies), highly selective schools are the most impacted by the bans, followed by less selective schools, and unclassified schools. This inversion of order also indicates that the selectivity pattern exhibited by other studies may be sensitive to the time period considered.

Lastly, the choice to use the Barron's Admissions Competitiveness Index or SAT changes the findings. For all the years of analysis for Texas and California using Barron's Admissions Competitiveness Index the negative impact of the ban for weighted public STEM schools are smallest for highly selective schools, larger for less selective schools, and largest for unclassified schools. When SAT scores are used instead of the Barron's Admissions Competitiveness Index, the results were more negative in the top decile than they were in the second or third deciles but were most negative for all other schools that submitted SAT scores to IPEDs. Furthermore, using SAT scores restricts the analysis to a smaller number of schools (in 2016 when using SAT scores, the number of schools in the analysis is 1,219, and using the Barron's Competitiveness index yields 1,435 [out of 2,436 4-year degree granting schools

Table 3.7. Difference in Differences Results: Investigating Controversial Findings

	STEM								Non-STEM							
	Public				Private				Public				Private			
	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.
Selectivity by Barron's (TX and CA only)																
Any Selectivity (including Unclassified)	-0.0310	***	-0.0617	***	0.1255	*	0.0731	***	-0.0312	***	-0.0583	***	0.0029	***	0.0041	**
Highly Selective (w/o unclassified)	-0.0315	***	-0.0530	***	0.0251	*	0.0316	**	-0.0290	***	-0.0466	***	-0.0089	***	-0.0795	***
Less Selective (w/o unclassified)	-0.0258	***	-0.0580	***	0.0354	***	0.1327	***	-0.0297	***	-0.0596	***	-0.0013		0.0160	***
Unclassified Only	-0.0382	***	-0.0668	***	-0.0021		0.0040		-0.0363	***	-0.0645	***	0.0010		0.0207	***
1991-2009 (TX and CA only)																
Any selectivity	-0.0177	***	-0.0337	***	0.0078		0.0414	***	-0.0149	***	-0.0279	***	0.0001		-0.0125	***
Highly Selective (w/o unclassified)	-0.0200	***	-0.0356	***	0.0293	*	0.0152		-0.0110	***	-0.0221	***	-0.0098	***	-0.0171	***
Less Selective (w/o unclassified)	-0.0157	***	-0.0333	***	0.0240	*	0.0603	*	-0.0149	***	-0.0285	***	-0.0072	***	-0.0089	***
Unclassified Only	-0.0166	***	-0.0203	***	-0.0045		-0.0034		-0.0164	***	-0.0320	***	-0.0018		-0.0024	
1991-2009 (All states)																
Any selectivity	-0.0200	***	-0.0203	***	0.0110	*	0.0361	***	-0.0101	***	-0.0124	***	-0.0024	***	-0.0146	***
Highly Selective (w/o unclassified)	-0.0209	***	-0.0218	***	0.0030		0.0036		-0.0088	***	-0.0114	***	-0.0105	***	-0.0146	***
Less Selective (w/o unclassified)	-0.0186	***	-0.0264	***	-0.0079		0.0579	***	-0.0135	***	-0.0204	***	-0.0119	***	-0.0146	***
Unclassified Only	-0.0146	***	-0.0164	***	0.0192	*	-0.0049		-0.0062	***	-0.0092	***	-0.0007		-0.0056	*
Selectivity by SAT (All Years & States)																
Top decile	-0.0227	***	-0.0200	***	-0.0109	**	-0.0082		-0.0189	***	-0.0182	***	-0.0185	***	0.0088	***
2nd decile	-0.0088	***	-0.0070	**	-0.0172	**	-0.0137	***	-0.0053	**	-0.0045	**	-0.0062	**	-0.0046	*
3rd decile	-0.0118	*	-0.0146	*	-0.0016		-0.0016		-0.0162	***	-0.0204	***	-0.0095	***	-0.0118	***
All other schools	-0.0148	***	-0.0215	***	0.0128	*	0.0321	***	-0.0127	***	-0.0192	***	-0.0071	***	-0.0036	**
All Years (TX and CA only)																
Top decile	-0.0433	***	-0.0728	***	-0.0085		0.0135		-0.0388	***	-0.0642	***	-0.0193	***	-0.0144	***
2nd decile	-0.0310	***	-0.0382	***	-0.0192		-0.0198	**	-0.0234	***	-0.0337	***	-0.0144	***	-0.0144	***
3rd decile	-0.0078		-0.0057		0.0166		0.0164		-0.0109		-0.0192		-0.0001		-0.0107	*
All other schools	-0.0231	***	-0.0610	***	0.0400	***	0.0931	***	-0.0256	***	-0.0567	***	0.0005		0.0186	***
1991-2009 (TX and CA only)																
Top decile	-0.0231	***	-0.0423	***	-0.0026		0.0062		-0.0143	***	-0.0225	***	-0.0115	***	-0.0190	***
2nd decile	-0.0157		-0.0199	*	0.0027		-0.0123		-0.0078		-0.0162	**	-0.0143	**	-0.0195	***
3rd decile	-0.0063		-0.0126		0.0066		0.0086		-0.0102	*	-0.0232	***	0.0021		-0.0105	*
All other schools	-0.0166	***	-0.0392	***	0.0209		0.0313		-0.0123	***	-0.0245	***	-0.0098	***	-0.0133	***

The choice of states, the specificity of time, and the choice of which variable to control for selectivity produced conditions in earlier studies to conclude that the more selective a school was, the more it would be impacted by affirmative action. The results of this study suggest that institutional selectivity is not as meaningful in determining what has happened to URM students in the wake of affirmative action bans as previously thought.

The second hypothesis that I contest is that only public institutions experience a decrease in the shares of URM degree completion. In most cases the affirmative action bans only applied to public schools, but in Texas they applied to private schools as well. In previous empirical affirmative action ban studies (Backes 2012; and Hinrichs 2012), private schools were only included in the analysis by Backes (2012) with the intent to see if underrepresented minority students were going to private schools instead of public schools [they were not]. The main findings of this paper (See Table 3) suggest that the shares of URM STEM and non-STEM graduates decreased at private schools, and only slightly increased or decreased for non-STEM students at private schools. Backes (2012) arrives at a similar conclusion noting that the shares of Black and Hispanic undergraduate students decreased at private schools for some levels of selectivity but remain unchanged in others. This may in part be due to the fact that both public and private colleges and universities have retreated from race-explicit admissions policies. Hirschman and Berrey (2017) believe that the general affirmative action ban movement diffused not only to public schools, but also private schools due to the fear of legal action.[18] Furthermore, Backes (2012) found no evidence that these students were enrolling at two-year institutions instead.

[18] I emphasize speculation here due to the authors' loose causal language. This was a descriptive paper in which causal conclusions were formulated.

The final unsupported hypothesis was that affirmative action bans have become more impactful as time elapsed from the bans with respect to the decline of URM STEM and non-STEM students. This contrasts a recent finding by Mickey-Pabello and Garces (2018) on the impact of affirmative action at medical schools that revealed that the impact of affirmative action on medical schools was most impactful immediately after the bans.

DISCUSSION AND CONCLUSION

This study sheds light on an example of *laissez faire* racist policies: affirmative action bans. I illustrate that this policy of colorblind meritocracy has implications for racial inequality in the STEM pipeline. I found that the proportion STEM and non-STEM graduates who are URMs were both impacted by the affirmative action bans. However, as anticipated, the proportion of STEM graduates that are URMs appear to be impacted to a greater extent than the proportion of non-STEM graduates that are URMs. This uneven impact may be explained by the findings of Riegle Crumb and colleagues (2019), who speculate that the social-psychological factors URM students experience in STEM (Carlone and Johnson 2007, Change et al 2011, Change et al 2014) lead to increased attrition in STEM. These social-psychological factors are intensified in the presence of affirmative action bans, and help explain the differences between STEM and non-STEM degree attainment by URMs.

The uneven impact may also have implications on career choice and returns to education. STEM careers have a high level of prestige and high salaries (Killewald 2012, Rothwell 2013). Affirmative action bans may exacerbate income inequalities between URMs and non-URMs. Further study to establish a link between affirmative action bans and their impact on returns to education is certainly warranted.

When considering the public/private designation of a school the affirmative action bans do not appear to have a spillover effect in terms of the URMs leaving public schools for private schools. A strict reading of the laws may lead the layperson to believe that only public schools were impacted by affirmative action bans because the laws technically only apply to public schools. However, results of this study suggest that the *de jure* impact at public schools extends to the *de facto* impact at private schools: private schools have also adopted the bans. This is perhaps due to the threat of potential lawsuits as Hirschman and Berry suggest (2017), alternatively it may be the cause of institutional isomorphism, or some combination of both effects.

The timing of the ban for undergraduate education also differs from the pattern previously found in the literature by Mickey-Pabello and Garces (2018). The impact of the bans has not waned over time for undergraduate education like it has for medical schools. Instead, it appears to have gotten stronger over time. One may think that the extreme selectivity of medical schools may be a feature that causes the two patterns to differ. However, when even the most highly selective undergraduate institutions are considered the pattern remains inconsistent. I speculate that the reason is not due to selectivity, but rather it is due to forces that facilitate or block against a college or university espousing a ban on affirmative action. One mechanism which may partly explain this discrepancy is that universities not only have their own mission statements to follow but are also pressured by their professional organizations and accreditation bodies. An institution may have separate accreditation bodies for medical schools (Liaison Committee for Medical Education and the Commission on Osteopathic College Accreditation) and for undergraduate education (several agencies). For example, Wayne State University's medical school in the state of Michigan was cited for 12 accreditation violations in 2015. The most pertinent violation concerning the context of this study was a lack of recruiting sufficient minority students. The violation was earned 8 years after bans on affirmative action had gone into effect in Michigan. Despite affirmative action bans, institutions of higher education must still adhere to their accreditation standards. For medical schools it appears as though these standards may have curtailed the impacts of affirmative action bans while at 4-year bachelor's degree granting institutions they have not, however further work should seek to find empirical support for this speculation.

More areas of investigation for affirmative action bans would come from the opportunity to completely tease out all of the mechanisms discussed in the theory section of this paper. For example, not all college students have a major when they matriculate to college. Thus, the STEM major is often not selected until later in a student's career, and the timing for the choice of major varies by school and within the school. This is problematic because Riegle-Crumb et al (2019) have shown that attrition may be an additional mechanism which leads to differences between STEM and non-STEM college completion. I would have like to include results for affirmative action attrition by asking the question "Have affirmative action bans increased levels of attrition from college for URM students?" It could enhance our understanding of why the more competitive nature of STEM may produce a smaller share of URM STEM graduates than the share of URM non-STEM graduates. I suspect URM STEM students to be more impacted by this type of attrition because URM students are more likely to be the casualties of the competitive nature of STEM programs. The rigor of competition drives individuals to seek support, and when that support is not there from peers of the same race the likelihood of attrition should be greater. In other words, if other URMs are integral to the success of other URMs because they create a closer-knit sense of community, then has the dearth of URMs created by affirmative action bans lead to increased attrition?

In summary, I have provided evidence for the effects of a *laissez-fair* racist policy, affirmative action bans. The results indicate that URM students are hindered by such policies, most notably in STEM education. If the United States wishes to move towards a society that embraces equal opportunity and rejects racial inequality, it should abandon its quest for the color-blind meritocracy sought out through anti-affirmative action policies.

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APPENDIX A

Table 3.A Department of Homeland Security CIP crosswalk

CIP Code Two Digit Series	2010 CIP Code	CIP Code Title
01	01.0308	Agroecology and Sustainable Agriculture
01	01.0901	Animal Science, General
01	01.0902	Agricultural Animal Breeding
01	01.0903	Animal Health
01	01.0904	Animal Nutrition
01	01.0905	Dairy Science
01	01.0906	Livestock Management
01	01.0907	Poultry Science
01	01.0999	Animal Science, Other
01	01.1001	Food Science
01	01.1002	Food Technology and Processing
01	01.1099	Food Science and Technology, Other
01	01.1101	Plant Sciences, General
01	01.1102	Agronomy and Crop Science
01	01.1103	Horticultural Science
01	01.1104	Agricultural and Horticultural Plant Breeding
01	01.1105	Plant Protection and Integrated Pest Management
01	01.1106	Range Science Management
01	01.1199	Plant Sciences, Other
01	01.1201	Soil Science and Agronomy, General
01	01.1202	Soil Chemistry and Physics
01	01.1203	Soil Microbiology
01	01.1299	Soil Sciences, Other
03	03.0101	Natural Resources/Conservation, General
03	03.0103	Environmental Studies
03	03.0104	Environmental Science
03	03.0199	Natural Resources Conservation and Research, Other
03	03.0205	Water, Wetlands, and Marine Resources Management
03	03.0502	Forest Sciences and Biology
03	03.0508	Urban Forestry
03	03.0509	Wood Science and Wood Products/Pulp and Paper Technology
03	03.0601	Wildlife, Fish and Wildlands Science and Management
04	04.0902	Architectural and Building Sciences/Technology

09	09.0702	Digital Communication and Media/Multimedia
10	10.0304	Animation, Interactive Technology, Video Graphics and Special Effects
11	11.0101	Computer and Information Sciences, General
11	11.0102	Artificial Intelligence
11	11.0103	Information Technology
11	11.0104	Informatics
11	11.0199	Computer and Information Sciences, Other
11	11.0201	Computer Programming/Programmer, General
11	11.0202	Computer Programming, Specific Applications
11	11.0203	Computer Programming, Vendor/Product Certification
11	11.0299	Computer Programming, Other
11	11.0301	Data Processing and Data Processing Technology/Technician
11	11.0401	Information Science/Studies
11	11.0501	Computer Systems Analysis/Analyst
11	11.0701	Computer Science
11	11.0801	Web Page, Digital/Multimedia and Information Resource Design
11	11.0802	Data Modeling/Warehousing and Database Administration
11	11.0803	Computer Graphics
11	11.0804	Modeling, Virtual Environments and Simulation
11	11.0899	Computer Software and Media Applications, Other
11	11.0901	Computer Systems Networking and Telecommunications
11	11.1001	Network and System Administration/Administrator
11	11.1002	System, Networking, and LAN/WAN Management/Manager
11	11.1003	Computer and Information Systems Security/Information Assurance
11	11.1004	Web/Multimedia Management and Webmaster
11	11.1005	Information Technology Project Management
11	11.1006	Computer Support Specialist
11	11.1099	Computer/Information Technology Services Administration and Management, Other
13	13.0501	Educational/Instructional Technology
13	13.0601	Educational Evaluation and Research
13	13.0603	Education Statistics and Research Methods
14	14.XXXX	Engineering
15	15.000	Engineering Technology, General
15	15.0101	Architectural Engineering Technology/Technician
15	15.0201	Civil Engineering Technology/Technician
15	15.0303	Electrical, Electric and Communications Engineering Technology/Technician
15	15.0304	Laser and Optical Technology/Technician
15	15.0305	Telecommunications Technology/Technician
15	15.0306	Integrated Circuit Design

15	15.0399	Electrical and Electronic Engineering Technologies/Technicians, Other
15	15.0401	Biomedical Technology/Technician
15	15.0403	Electromechanical Technology/Electromechanical Engineering Technology
15	15.0404	Instrumentation Technology/Technician
15	15.0405	Robotics Technology/Technician
15	15.0406	Automation Engineering Technology/Technician
15	15.0499	Electromechanical and Instrumentation and Maintenance Technologies/Technicians, Other
15	15.0501	Heating, Ventilation, Air Conditioning and Refrigeration Engineering Technology/Technician
15	15.0503	Energy Management Systems Technology/Technician
15	15.0505	Solar Energy Technology/Technician
15	15.0506	Water Quality and Wastewater Treatment Management and Recycling Technology/Technician
15	15.0507	Environmental Engineering Technology/Environmental Technology
15	15.0508	Hazardous Material Management and Waste Technology /Technician
15	15.0599	Environmental Control Technologies/Technicians, Other
15	15.0607	Plastics and Polymer Engineering Technology/Technician
15	15.0611	Metallurgical Technology/Technician
15	15.0612	Industrial Technology/Technician
15	15.0613	Manufacturing Engineering Technology/Technician
15	15.0614	Welding Engineering Technology/Technician
15	15.0615	Chemical Engineering Technology/Technician
15	15.0616	Semiconductor Manufacturing Technology
15	15.0699	Industrial Production Technologies/Technicians, Other
15	15.0701	Occupational Safety and Health Technology/Technician
15	15.0702	Quality Control Technology/Technician
15	15.0703	Industrial Safety Technology/Technician
15	15.0704	Hazardous Materials Information Systems Technology /Technician
15	15.0799	Quality Control and Safety Technologies/Technician
15	15.0801	Aeronautical Engineering Technology/Technician
15	15.0803	Automotive Engineering Technology/Technician
15	15.0805	Mechanical Engineering/Mechanical Technology/Technician
15	15.0899	Mechanical Engineering Related Technologies/Technicians, Other
15	15.0901	Mining Technology/Technician
15	15.0903	Petroleum Technology/Technician
15	15.0999	Mining and Petroleum Technologies/Technicians, Other
15	15.1001	Construction Engineering Technology/Technician
15	15.1102	Survey Technology/Surveying
15	15.1103	Hydraulics and Fluid Power Technology/Technician

15	15.1199	Engineering -Related Technologies, Other
15	15.1201	Computer Engineering Technology/Technician
15	15.1202	Computer Technology/Computer Systems Technology
15	15.1203	Computer Hardware Technology/Technician
15	15.1204	Computer Software Technology/Technician
15	15.1299	Computer Engineering Technologies/Technicians, Other
15	15.1301	Drafting and Design Technology/Technician, General
15	15.1302	CAD/CADD Drafting and /or Design Technology/Technician
15	15.1303	Architectural Drafting and Architectural CAD/CADD
15	15.1304	Civil Drafting and Civil Engineering CAD/CADD
15	15.1305	Electrical/Electronics Drafting and Electrical/Electronics CAD/CADD
15	15.1306	Mechanical Drafting and Mechanical Drafting CAD/CADD
15	15.1399	Drafting/Design Engineering Technologies/Technicians, Other
15	15.1401	Nuclear Engineering Technology/Technician
15	15.1501	Engineering/Industrial Management
15	15.1502	Engineering Design
15	15.1503	Packaging Science
15	15.1599	Engineering-Related Fields, Other
15	15.1601	Nanotechnology
26	15.9999	Engineering Technologies and Engineering-Related Fields, Other
27	26.XXXX	Biological and Biomedical Sciences
28	27.XXXX	Mathematics and Statistics
28	28.0501	Air Science/Airpower Studies
28	28.0502	Air and Space Operational Art and Science
29	28.0505	Naval Science and Operational Studies
29	29.0201	Intelligence, General
29	29.0202	Strategic Intelligence
29	29.0203	Signal/Geospatial Intelligence
29	29.0204	Command & Control (C3, C41) Systems and Operations
29	29.0205	Information Operations/Joint Information Operations
29	29.0206	Information/Psychological Warfare and Military Media Relations
29	29.0207	Cyber/Electronic Operations Warfare
29	29.0299	Intelligence, Command Control and Information Operations, Other
29	29.0301	Combat Systems Engineering
29	29.0302	Directed Energy Systems
29	29.0303	Engineering Acoustics
29	29.0304	Low-Observables and Stealth Technology
29	29.0305	Space Systems Operations
29	29.0306	Operational Oceanography
29	29.0307	Undersea Warfare
29	29.0399	Military Applied Sciences, Other

29	29.0401	Aerospace Ground Equipment Technology
29	29.0402	Air and Space Operations Technology
29	29.0403	Aircraft Armament Systems Technology
29	29.0404	Explosive/Ordinance/Bomb Disposal
29	29.0405	Joint Command/Task Force (C3, C41) Systems
29	29.0406	Military Information Systems Technology
29	29.0407	Missile and Space Systems Technology
29	29.0408	Munitions Systems/Ordinance Technology
29	29.0409	Radar Communications and Systems Technology
29	29.0499	Military Systems and Maintenance Technology, Other
30	29.9999	Military Technologies and Applied Sciences, Other
30	30.0101	Biological and Physical Sciences
30	30.0601	Systems Science and Theory
30	30.0801	Mathematics and Computer Science
30	30.1001	Biopsychology
30	30.1701	Behavioral Sciences
30	30.1801	Natural Sciences
30	30.901	Nutrition Sciences
30	30.2501	Cognitive Science
30	30.2701	Human Biology
30	30.3001	Computational Science
30	30.3101	Human Computer Interaction
30	30.3201	Marine Sciences
40	30.3301	Sustainability Studies
41	40.XXXX	Physical Sciences
41	41.0000	Science Technologies/Technicians, General
41	41.0101	Biology Technician/Biotechnology Laboratory Technician
41	41.0204	Industrial Radiologic Technology Technician
41	41.0205	Nuclear/Nuclear Power Technology/Technician
41	41.0299	Nuclear and Industrial Radiologic Technologies/Technicians, Other
41	41.0301	Chemical Technology/Technician
41	41.0303	Chemical Process Technology
41	41.0399	Physical Science Technologies/Technicians, Other
42	41.9999	Science Technologies/Technicians, Other
42	42.2701	Cognitive Psychology and Psycholinguistics
42	42.2702	Comparative Psychology
42	42.2703	Developmental Psychology
42	42.2704	Experimental Psychology
42	42.2705	Personality Psychology
42	42.2706	Physiological Psychology/Psychobiology
42	42.2707	Social Psychology

42	42.2708	Psychometrics and Quantitative Psychology
42	42.2709	Psychopharmacology
43	42.2799	Research and Experimental Psychology, Other
43	43.0106	Forensic Science and Technology
45	43.0116	Cyber/Computer Forensics and Counterterrorism
45	45.0301	Archeology
45	45.0603	Econometrics and Quantitative Economics
49	45.0702	Geographic Information Science and Cartography
51	49.0101	Aeronautics/Aviation/Aerospace Science and Technology, General
51	51.1002	Cytotechnology/Cytotechnologist
51	51.1005	Clinical Laboratory Science/Medical Technology/Technologist
51	51.1401	Medical Scientist
51	51.2003	Pharmaceutics and Drug Design
51	51.2004	Medicinal and Pharmaceutical Chemistry
51	51.2005	Natural Products Chemistry and Pharmacognosy
51	51.2006	Clinical and Industrial Drug Development
51	51.2007	Pharmacoeconomics/Pharmaceutical Economics
51	51.2009	Industrial and Physical Pharmacy and Cosmetic Sciences
51	51.2010	Pharmaceutical Sciences
51	51.2202	Environmental Health
51	51.2205	Health/Medical Physics
51	51.2502	Veterinary Anatomy
51	51.2503	Veterinary Physiology
51	51.2504	Veterinary Microbiology and Immunobiology
51	51.2505	Veterinary Pathology and Pathobiology
51	51.2506	Veterinary Toxicology and Pharmacology
51	51.2510	Veterinary Preventative Medicine, Epidemiology, and Public Health
51	51.2511	Veterinary Infectious Diseases
52	51.2706	Medical Informatics
52	52.1301	Management Science
52	52.1302	Business Statistics
52	52.1304	Actuarial Science
52	52.1399	Management Science and Quantitative Methods, Other

Source: https://www.ice.gov/sites/default/files/documents/Document/2016/stem-list.pdf

APPENDIX B

Table 3.B National Science Fondation CIP crosswalk

CIP Code Two Digit Series	2010 CIP Code	CIP Code Title
01	01.09	Animal Sciences
01	01.10	Food Science and Technology
01	01.11	Plant Sciences
01	01.12	Soil Sciences
01	01.99	Agriculture, Agriculture Operations and Related Sciences, Other
03	03.01	Natural Resources Conservation and Research
03	03.02	Natural Resources Management and Policy
03	03.03	Fishing and Fisheries Sciences and Management
03	03.05	Forestry
03	03.06	Wildlife and Wildlands Sciences and Management
03	03.99	Natural Resources and Conservation, Other
11	11.01	Computer and Information Sciences, General
11	11.02	Computer Programming
11	11.04	Information Sciences/Studies
11	11.07	Computer Science
11	11.08	Computer Software and Media Applications
14	14.01	Engineering, General
14	14.02	Aerospace, Aeronautical and Astronautical Engineering
14	14.03	Agricultural Engineering
14	14.04	Architectural Engineering
14	14.06	Ceramic Sciences and Engineering
14	14.07	Chemical Engineering
14	14.08	Civil Engineering
14	14.09	Computer Engineering
14	14.10	Electrical, Electronics and Communications Engineering
14	14.11	Engineering Mechanics
14	14.12	Engineering Physics
14	14.13	Engineering Science
14	14.14	Environmental/ Environmental Health Engineering
14	14.18	Materials Engineering
14	14.19	Mechanical Engineering
14	14.20	Metallurgical Engineering
14	14.21	Mining and Mineral Engineering
14	14.22	Naval Architecture and Marine Engineering
14	14.23	Nuclear Engineering

14	14.24	Ocean Engineering
14	14.25	Petroleum Engineering
14	14.27	Systems Engineering
14	14.28	Textile Sciences and Engineering
14	14.32	Polymer/Plastics Engineering
14	14.33	Construction Engineering
14	14.35	Industrial Engineering
14	14.36	Manufacturing Engineering
14	14.37	Operations Research
14	14.38	Survey Engineering
14	14.39	Geological/Geophysical Engineering
14	14.40	Paper Science and Engineering
14	14.41	Electromechanical Engineering
14	14.42	Mechatronics, Robotics, and Automation Engineering
14	14.43	Biomechanical Engineering
14	14.44	Engineering Chemistry
14	14.45	Biological/Biosystems Engineering
14	14.99	Engineering, Other
15	15.00	Engineering Technology, General
15	15.10	Construction Engineering Technologies
15	15.11	Engineering-Related Technologies
15	15.15	Engineering-Related Fields
15	15.16	Nanotechnology
26	26.01	Biology, General
26	26.02	Biochemistry, Biophysics and Molecular Biology
26	26.03	Botany/Plant Biology
26	26.04	Cell/Cellular Biology and Anatomical Sciences
26	26.05	Microbiological Sciences and Immunology
26	26.07	Zoology/Animal Biology
26	26.08	Genetics
26	26.09	Physiology, Pathology and Related Sciences
26	26.11	Biomathematics, Bioinformatics, and Computational Biology
26	26.12	Biotechnology
26	26.13	Ecology, Evolution, Systematics, and Population Biology
26	26.15	Neurobiology and Neurosciences
26	26.99	Biological and Biomedical Sciences
27	27.01	Mathematics
27	27.03	Applied Mathematics
27	27.05	Statistics
27	27.99	Mathematics and Statistics, Other
30	30.01	Biological and Physical Sciences

30	30.06	Systems Science and Theory
30	30.08	Mathematics and Computer Science
30	30.10	Biopsychology
30	30.18	Natural Sciences
30	30.19	Nutritional Sciences
30	30.27	Human Biology
30	30.30	Computational Science
30	30.32	Marine Science
40	40.01	Physical Sciences
40	40.02	Astronomy and Astrophysics
40	40.04	Atmospheric Sciences and Meteorology
40	40.05	Chemistry
40	40.06	Geological and Earth Sciences/Geosciences
40	40.08	Physics
40	40.10	Material Science
40	40.99	Physical Sciences, Other
52	52.13	Management Sciences and Quantitative Methods, Other

Source: https://www.lsamp.org/help/help_stem_cip_2015.cfm

APPENDIX C

Table 3.C Difference in differences results: impact of affirmative action bans on URM degree attainment

	STEM (NSF Version)								Non-STEM (NSF Version)							
	Public				Private				Public				Private			
	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.
All States Considered (Any Selectivity)																
All States (baseline)	-0.0137	***	-0.0089	*	-0.0164	***	0.0046		-0.0082	**	-0.0073		-0.0195	***	-0.0074	*
Geographic Proximity (Comparison Group)	-0.0247	***	-0.0131	**	-0.0283	***	-0.0147	*	-0.0188	***	-0.0109	*	-0.0215	***	-0.0214	***
Most Media Mentions (Comparison Group)	-0.0217	***	-0.0164	**	-0.0235	***	-0.0083		-0.0214	***	-0.0187	***	-0.0215	***	-0.0179	***
Without States Facing Desegregation Litigation (Comparison Group)	-0.0141	***	-0.0094	*	-0.0131	**	0.0050		-0.0105	***	-0.0072		-0.0153	***	-0.0083	*
Without FL (Ban Group)	-0.0140	***	-0.0118	**	-0.0137	**	0.0055		-0.0088	**	-0.0077	*	-0.0169	***	-0.0053	
Without AZ (Ban Group)	-0.0156	***	-0.0118	**	-0.0139	**	0.0039		-0.0111	***	-0.0089	*	-0.0156	***	-0.0087	*
Without NE (Ban Group)	-0.0156	***	-0.0118	**	-0.0140	**	0.0039		-0.0111	***	-0.0090	*	-0.0158	***	-0.0088	*
All States Considered (Highly Selective Only & Excluding Unclassified)																
All States (baseline)	-0.0092	*	-0.0067		-0.0176	***	-0.0102	*	-0.0094	*	-0.0039		-0.0241	***	-0.0194	***
Geographic Proximity (Comparison Group)	-0.0151	***	-0.0123	*	-0.0285	***	-0.0301	***	-0.0191	***	-0.0096		-0.0286	***	-0.0293	***
Most Media Mentions (Comparison Group)	-0.0214	***	-0.0219	**	-0.0291	***	-0.0356	***	-0.0309	***	-0.0233	**	-0.0300	***	-0.0392	***
Without States Facing Desegregation Litigation (Comparison Group)	-0.0093	*	-0.0073		-0.0170	***	-0.0091		-0.0098	*	-0.0044		-0.0238	***	-0.0196	***
Without FL (Ban Group)	-0.0160	***	-0.0164	***	-0.0161	***	-0.0055		-0.0150	**	-0.0141	***	-0.0239	***	-0.0153	***
Without FL (Ban Group/Less Selective)	-0.0150	**	-0.0114	*	-0.0118		0.0118		-0.0095	**	-0.0141	***	-0.0135	***	0.0005	
Without AZ (Ban Group)	-0.0102	**	-0.0084		-0.0176	***	-0.0102	*	-0.0105	*	-0.0058		-0.0241	***	-0.0194	***
Without NE (Ban Group)	-0.0102	**	-0.0085		-0.0176	***	-0.0102	*	-0.0106	*	-0.0059		-0.0241	***	-0.0194	***

Chapter 4: Unintended Consequences of Affirmative Action: How affirmative action bans have shaped interracial marriage

INTRODUCTION

Interracial marriage has had a tumultuous history in the United States. When slavery was still prevalent and the plantation economy was booming, the states developed many anti-miscegenation laws that aimed to prevent the intermarriage of Blacks and Whites, and also prevented the marriage of several racial groups with each other. Twelve states placed restrictions on Native Americans, fourteen on Asian Americans, and nine targeted Filipinos (Pascoe 2009). These laws trace their origins to Maryland in 1664 when first anti-miscegenation law in the United States between Blacks and Whites was passed. Forty-one states or colonies had enacted miscegenation laws by 1967, when the Supreme Court ruled in *Loving v. Virginia* that anti-miscegenation laws were unconstitutional. The Court condemned anti-miscegenation, claiming it was "designed to maintain White Supremacy."

After the ruling in the *Loving v Virginia* court case, interracial marriage began to rise. According to the U.S. Census Bureau, the percent of interracial marriages increased from 0.4% of all marriages in 1960 to 0.7% in 1970, and to 2.0% by 1980 (1998). The percentage of interracial marriage has more than doubled since then, from 6.7% of new marriages in 1980 to 15.1% in 2010 (Wang 2012).

Although anti-miscegenation laws have been obsolete for the last 50 years and interracial marriage is on the rise, laws that ban the practice of affirmative action, a policy aimed to support and prevent discrimination for racial groups that have been historically discriminated against, are only about twenty years old. In this work I argue that affirmative action bans resemble these anti-miscegenation laws because although they do not have a *de jure* impact on interracial marriage they have a *de facto* impact of declining interracial marriage. Affirmative action bans have arisen recently in response to race based affirmative policies that were introduced in the United States in 1961, and predated the Civil Rights Act by three years and the ruling of *Loving v Virginia* by six years. The first policy to use the term affirmative action was President Kennedy's Executive Order 10925. It sought to ensure that employment practices were free from racial bias. Since then, numerous employers and educational institutions have sought to pursue affirmative action, and in doing so have added racial diversity to the workplace and schools. However, a reversal began in 1996 when the state of California placed a ban on the practice of affirmative action. This ban on affirmative action disallowed preferential treatment on the basis of race in the operation of public employment, the awarding of public contracts, or public education.

Affirmative action bans have implications for interracial marriage because many people meet their future spouses through school (Rosenfeld and Thomas 2012). One study indicates that 28% of Facebook users who are married college graduates attended the same college (Macskassy 2013). Hence, if affirmative action bans have shaped where people go to school, they could also impact the rates of interracial marriage because colleges are where many unmarried people meet. If interracial marriages decline as a result of affirmative action bans, then these bans could effectively be viewed as a modern day relative of anti-miscegenation laws, although much more covert, unintended, or unanticipated.[19]

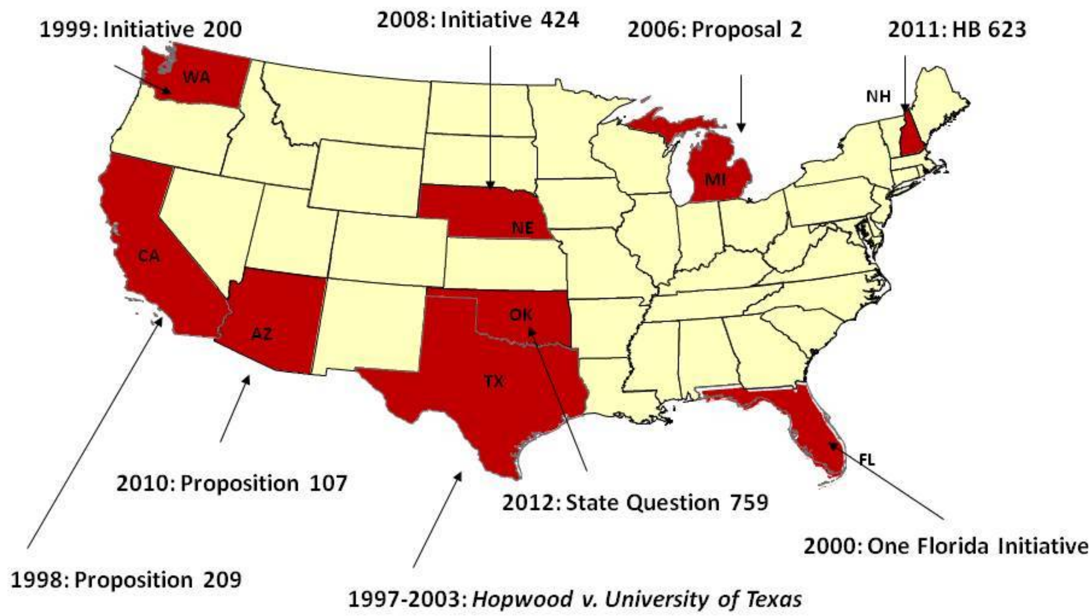
[19] Interracial marriage and not cohabitation is used as the dependent variable in this study because of the parallels affirmative action bans create relative to anti-miscegenation. However, cohabitation could be the focus of further study.

This study investigates how the shock of affirmative action bans change individuals' partner selection. I first investigate whether affirmative action bans caused declines in interracial marriage [research question 1: main effects]. I then study the types of interracial couples that changed in the wake of these bans [research question 2: differential effects]. I provide an overview of the literature on the causal impacts of affirmative action bans on higher education. Because schools common places were couples meet even in the presence of online dating I investigate how their racial redistribution in the wake of the bans has impacted interracial marriage. I draw on two theories of racial segregation and social ties in order to understand how affirmative action bans may have shaped interracial marriage. Blau, Blum, and Shwartz (1982) posit that more racially diverse environments yield more interracial marriages. Consequently, we might expect that in the wake of affirmative bans there are more White and Asian students in higher education so White and Asian interracial marriages could become more common. On the other hand there White and Asian student might self-segregate thereby homogenizing the two groups; this could lead to a pattern of relationship formation where there is less interracial marriage between Whites and Asians (Mollica, Gray et al. 2003, Marmaros and Sacerdote 2006, Mayer and Puller 2008, Wimmer and Lewis 2010). This study explores those theories in the context of the changing racial composition of colleges that affirmative action creates and uses difference in differences approaches to estimate the causal impact of affirmative action bans on interracial marriage.

State Level U.S. Affirmative Action Bans

After the introduction of affirmative action legislation during the 1960s Civil Rights legislation, there was no notable pushback on affirmative action, presumably because the Voting Rights Act of 1965 created a political environment where pushback could result in a loss of votes from a large new block of voters (Skrentny 2006). More than ten years later, the Supreme Court Case *Regents of the University of California v. Bakke* (1978) emerged. Bakke, a medical school student, argued that the use of a racial quota system had prevented him from gaining access to medical school. The Court deemed the racial quota system was unconstitutional but allowed the use of race as a factor in admissions. Since the *Bakke* case the Supreme Court has revisited affirmative action four more times making it one of the most controversial issues in this country over the past fifty years. The most notable of those court cases for the context of this paper is *Schutte v. Coalition to Defend Affirmative Action* (2013) because it highlights the lack of consensus around these policies. Because there was no law that outlawed the practice of affirmative action at the national level some states beginning with *Proposition 209* in California in 1996 passed their own bans on the practice of affirmative action. In the twenty years following California's ballot initiative many other states adopted their own policy that would prevent colleges and universities from considering race in admissions or the awarding of scholarships. The map below illustrates all of the states where affirmative action bans have been passed, the years they passed, and the name of the legislation (See Figure 4.2).

Figure 4.1 Affirmative Action Policy Map



ASSORTATIVE MATING IN THE CONTEXT OF AFFIRMATIVE ACTION BANS

To help explain how assortative mating could be altered by a ban on affirmative action I draw on theories of assortative mating, organizations, and marriage. Theories of assortative mating claim that both *selection* and *socialization* work together to achieve a good marital match (Oppenheimer 1988, Schwartz 2013). Individuals *select* each other by matching on similar or complimentary traits that they (or sometimes their parents) value. Adaptive *socialization* during courtship or after the marriage modifies the existing traits of one or both partners in order to improve the quality of the match achieved via the selection process alone (Oppenheimer 1988). This paper does not observe any socialization processes (i.e., partners' behaviors in their relationship building phase or afterwards), but it does concern itself with the selection process, particularly as affirmative action ban altered the environment for a match.[20] I argue that the

[20] Data about the dating and relationship-building phase would be required to assess socialization, and this is a limitation of this study. However, research shows that the selection characteristics are more important than socialization characteristics (Watson et al. 2004). While socialization is important in creating a bond such as a

affirmative action bans have altered the racial distribution of people in a major institution of American society, the university, ultimately leading to declines in interracial marriage.

It is also possible, that affirmative action bans could have changed peoples' assortative mating preferences (non-structural), but there is no existing literature on how affirmative action bans changed them. As such, I treat that mechanism as part of an affirmative action black box. I describe this black box in Figure 4.4 just before the methods section of the paper to emphasize that the analytic strategy I employ can capture the causal impact of affirmative action bans on interracial marriage despite not being able to analyze some of the mechanisms in-depth.

Where Spouses Meet

Prior research has shown that people often meet their partners at school (Rosenfeld and Thomas 2012, and Macskassy 2013). Though more than one third of relationships that result in marriages between 2005 and 2012 started on the Internet (Cacioppo, Cacioppo et al. 2013). Online dating research has indicated that certain racial groups and combinations of racial groups (for multi-racial/ethnics) are preferred to others (Curington, Lin et al. 2015).

Despite the growing presence of online dating, people continue to meet each other through school (Rosenfeld and Thomas 2012). Indeed, many mechanisms for meeting a person from the same school exist in the realm of online dating. Dating websites and applications such as Tinder, Bumble, OkCupid, Plenty of Fish, Match.com, and eHarmony often ask their users to list their educational affiliations. Furthermore, many schools have alumni associations across several cities that host social events. This increases the chances that a person may select a partner

marriage (Demo and Hughes 1990; and Bryan and Jacobson, 2005) affirmative action bans influence the environment for selection.

from their alma mater. Other social networks and professional organizations also increase the number of opportunities for similarly educated individuals to meet.

How Affirmative Action Can Disrupt Assortative Mating in Colleges

Empirically based studies have concluded that affirmative action bans have decreased the proportion of underrepresented minority students (herein URM) entering various levels of higher education [i.e., undergraduate, graduate, and professional schools] (Wightman 1997, Espenshade and Chung 2005, Backes 2012, Hinrichs 2012, Garces 2013, Garces and Mickey-Pabello 2015). Whites and Asians (the non-URM groups) are seen as the beneficiaries of affirmative action bans because their proportions grow while the proportions of Blacks, Latinos, and Native Americans (the URM) decrease. Thus, because affirmative action bans increase the racial homogenization of colleges (more Whites and Asians), the bans cause people of different racial groups to interact in college. Furthermore, they are also less likely to meet because their college affiliation impacts them later on in life through online dating profiles and alumni functions.

Sorting Characteristics in Partner Choice

Not all characteristics for assortative mating are weighed equally or in the same way for different individuals. People have greater preferences for certain attributes, and what are a desirable characteristic in a male partner, may not be as desirable in a female partner. For instance, men place a greater emphasis on weight, youth, and physical attractiveness in a potential partner, while women place emphasis on financial resources, and other resources that are beneficial in childrearing (Buss 1989, Gangestad and Cousins 2001). Despite these sex differences most studies indicate positive assortment or homophily: where similarity leads to connection (McPherson, Smith-Lovin et al. 2001). Marital scholars have also shown that educational homogamy (i.e., married couples with similar levels of education) has continued to rise since 1960 (Schwartz and Mare 2005).

Overwhelmingly, people match on similar characteristics instead of disparate ones (Buss 1985). In general, people assort with race as the most important characteristic (Fisman, Iyengar et al. 2008), followed by age, religion, education, occupation, and gender (McPherson, Smith-Lovin et al. 2001). For example, racial homophily dominates the impacts of educational homogamy: people prefer partners of their same race, over partners of their same level of educational attainment (Lin and Lundquist 2013). Based on the 2010 American Community Survey -among newlyweds- only 9.4% of Whites, 17.1% of Blacks, 25.7% of Hispanics, and 27.7% of Asians chose to marry someone who race or ethnicity was different than their own (Wang and Taylor 2012).

Assortative Mating in a Changing Racial Environment

We know that colleges have become less heterogeneous with respect to race due to affirmative action bans (Hinrichs 2012, Backes 2012). A negative change in the graduation of URMs (underrepresented minority students) has been estimated as high as 12 percent (Mickey-Pabello 2019). Since colleges became less racially heterogeneous as a result of affirmative action bans, then it is reasonable to expect that there should be fewer interracial marriages than there would have been in the counterfactual condition where there were no affirmative action bans. Endogamy (i.e., marriage among persons with similar identities such as race, ethnic group, class, or religion) has been shown to be negatively related to higher levels of heterogeneity in a population (Blau, Blum et al. 1982, Kalmijn 1998).

McPherson, Smith-Lovin, and Cook (2001) argue homophily in race creates the strongest divides in our personal environments. More recently, scholars have found that racial homophily is the dominant feature of assortative mating (Lin and Lundquist 2013). Racial homophily has also been well documented in non-dating social networking contexts in both colleges (Mollica, Gray et al. 2003, Marmaros and Sacerdote 2006, Mayer and Puller 2008, Wimmer and Lewis 2010). In the transition from high school to college, research has found that interracial friendships increase for Whites, decrease for Blacks, and do not change significantly for Hispanics and Asians (Stearns, Buchmann et al. 2009).

This pattern of racial homophily is in conflict with Blau, Blum, and Schwartz's theorem of heterogeneity. The heterogeneity theorem posits that if the environments (i.e., colleges) are less heterogeneous then there should be less interracial marriages and more heterogeneous environments should yield more interracial marriages. The theorem fails because it does not account for the fact that people will often self-segregate themselves within institutions, instead this theorem assumes that interactions with others occur at random. In the case of affirmative action bans I contend that the bans altered structural and cultural mechanisms that ultimate lead to declines in interracial marriage. These mechanisms are underscored by Figure 4.4 below.

Figure 4.2. The Causal Impact of Affirmative Action Bans on Interracial Marriage

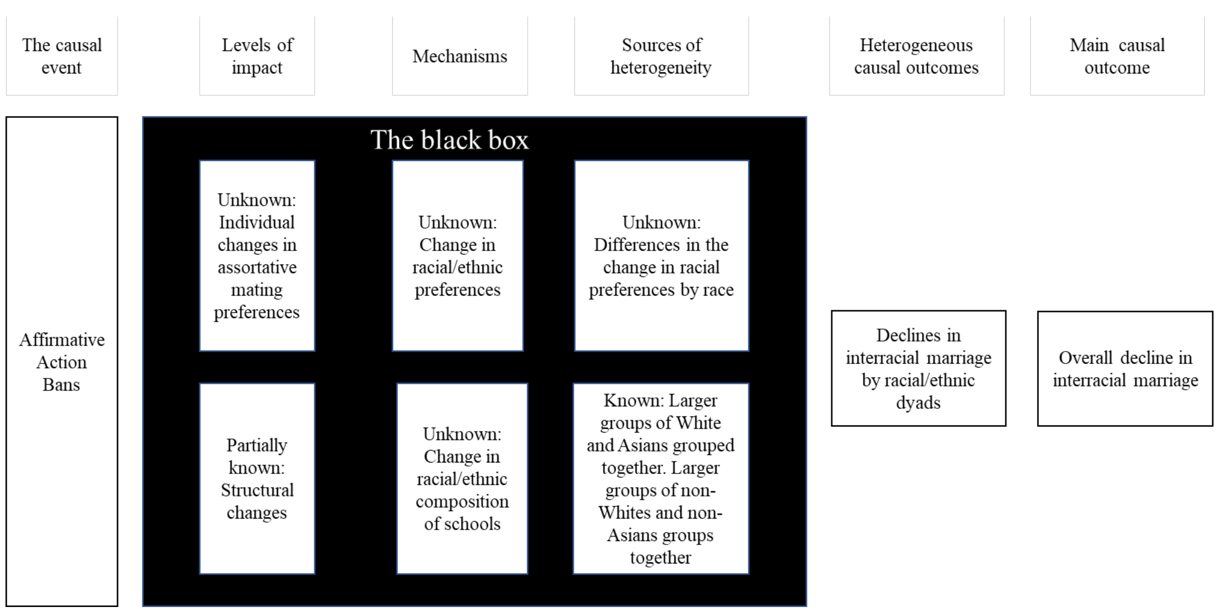


Figure 4.3 suggests that affirmative action bans may have impacted both structural and cultural elements that could lead to declines in interracial marriages. I include both the known and unknown features of the impacts of affirmative action black box together for two reasons. First, the unknown mechanisms are unknown so we do not know how they work or how much they may be influencing interracial marriage. Second, the known mechanisms are partially known and partially unknown. For example, we know how much affirmative action bans impacted the racial composition of schools, but we do not really know to what extent that mechanism is interacting with an unknown change in racial/ethnic assortative mating preferences (if at all).

I want to reemphasize what is known first: the impact of affirmative action bans on the change in the racial and ethnic composition of schools. To recapitulate, the mechanism that explains the decline in interracial marriage is that there was increased racial homogeneity in schools. We know that the beneficiaries of affirmative action bans are White and Asian students because they gain the spots that otherwise would have gone to a member of an underrepresented racial minority group if affirmative action was practiced (Wightman 1997, Espenshade and Chung 2005, Hinrichs 2012, Backes 2012). URMs in contrast, are displaced from more selective schools (Hinrichs 2012, Backes 2012). This means that there is an over-all pattern of increased racial homogeneity, but where people are filtered to (e.g., the selectivity of an institution) differs on the basis of their racial identity. What is unknown is how affirmative action bans have impacted peoples' assortative mating preferences. However, even though we do not know if affirmative action bans caused changes in preferences, we can confirm that the preferences play a role. If racial preferences do not play a role, then we should expect that two racial groups that were at increased exposure to each other would see an increase in their level of interracial marriage (i.e., increased White and Asian interracial marriage). If racial preferences play a role, then we should expect that two racial groups that were at increased exposure to each other would not see an increase in their level of interracial marriage (i.e., decreased White and Asian interracial marriage).

This is why the White-Asian dyad is of particular theoretical interest. Both of these groups benefit from affirmative action bans. White-Asian interracial marriage is the second most common type of interracial marriage (Wang 2012).[21]We also know that the overwhelming preferences of these racial groups (or any racial group) is to marry within the same race (how much so depends on the racial group). Therefore, we should expect that the proportion of White-Asian interracial marriages declines as a result of the bans (as a racial homophily framework would suggest) and not an increase in the proportion of interracial marriages between Whites and Asians (as the theorem of heterogeneity argues). Instead of marrying a person of an opposite race, because White and Asians now have exposure to a larger pool of Whites and Asians due to affirmative action bans, we should expect them to act on those preferences to find a partner of the same race. Thus, even if I cannot directly show evidence of the mechanism at work I can argue through induction that the mechanism may exist. If there is a decline in White-Asian interracial marriage, it is not only due to the racial composition of individuals within society's institutions, but it is also due to racial preferences. If they act on their preferences and choose mates of their own race, then I show that preferences are part of the black box. In summation, the impact of the ban on White-Asian interracial marriage should test [by induction] whether or not preferences are a mechanism in this phenomenon of affirmative action bans and interracial marriage.

[21] White-Hispanic is the most common interracial marriage.

Relationship 1: States impacted by affirmative action bans have experienced decreases in the proportion of interracial marriage.

Relationship 2: There is variation in the pattern of decline in interracial marriage related to the racial composition of the marriage dyad (e.g., White-Asian or White-Black). Each racial group is subject to its own compositional change and assortative mating preferences. These preferences, when combined with the shift in the racial composition of the environment at institutions should drive the increases/decreases in the proportions of interracial marriage. For example, I expect White-Asian interracial marriage decline.

DATA AND METHODS

Data

This study defines interracial marriage, the dependent variable, as conventionally framed in the literature (Qian 1997): marriages between a Hispanic and a non-Hispanic, or marriages between non-Hispanic spouses who come from the following different racial groups: White, Black, Asian, or American Indian. Data from the Current Population Study (CPS) provide information about which people in the household are married to each other, and also provides the ethnic and racial characteristics of each person in the household making it possible to identify if a couple in the household constituted an interracial marriage. The analysis was restricted to households where there was a marriage [unit of analysis]. A first dependent variable was constructed which identifies if a married couple is in an interracial marriage (0 = "No" 1 = "Yes"). I constructed several dependent variables for interracial marriage were constructed to indicate different types of marriage dyads: Black-Hispanic, Black-White, Black-Asian, Hispanic-White, Hispanic-Asian, White-Asian, White-Native American, White-Multiracial, Asian-Multiracial, and Multiracial-Multiracial. Constructing these variables gives insight into which types of interracial marriages were driving differences in interracial marriage. The reference group for each of these interracial marriage types is married couples who are not in an interracial marriage, and the reference group for a cumulative category of "any interracial marriage" is also married couples who are not in an interracial marriage (non-married couples were excluded from the study). The reference group was selected in this way so that the results from various diff-in-diff equations could be "roughly" compared to each other. The inclusion of the White-Asian group in particular, not only allows me to see the heterogeneity of the changing in interracial marriage due to affirmative action bans by race, but it also allows me to test through induction if the mechanism of assortative mating is an important part of the black-box.

Eight states are excluded from the analysis [leaving 42] because they had some characteristic that made them poor candidates for a counterfactual comparison. Alabama, Georgia, Louisiana, Mississippi, Tennessee, Kentucky, and South Carolina were all excluded because public institutions (i.e., schools) in those states faced desegregation litigation during the period of investigation. New Hampshire is excluded from the study [although it is a ban state; leaving 41 states] because it is predominantly White. Ninety-six percent of people in New Hampshire are White, hence there are so few interracial marriages that comparing this state to others counterfactually would not make sense. Removing those eight states allowed for a more reasonable estimate of the causal impact of affirmative action bans.

To increase the precision of the estimates, control variables (also from the CPS) were used to represent selected time-varying state characteristics, such as racial demographics (percentage of the population by race/ethnicity), educational attainment (as measured by the percentage of the population in a state 25- to 34 with a bachelor's, master's, or doctoral degree), and economic indicators (unemployment rates for 25- to 34-year-olds and per-capita income).

Table 4.1 shows the differences between the ban states and non-ban states on these dimensions. All of these variables were controlled for because they give credence to the fact that states are not interchangeable units of analysis; they have characteristics that make them unique. It is important to control for those state characteristics because they could confound the impact of affirmative action bans.

Household-level characteristics are also used (age, educational attainment, sex, and household size). Age is a continuous measure; educational attainment is measured by the number of partners that earned bachelor's degree ("Both", "One", "Neither"); sex is measured dichotomously ("Male" or "Female"); and household size is treated pseudo-continuously.[22]

Table 4.1. Racial, economic, and social differences in states with and without affirmative action bans

Variable	No Ban States	Ban States
White	75.12%	61.71%
Black	11.64%	9.02%
Asian	4.00%	5.75%
Hispanic	7.91%	22.36%
Native American	1.31%	1.11%
Employed	78.13%	76.57%
Unemployed	5.10%	5.09%
Not in Labor Force	16.76%	18.32%
Bachelor's Degree	24.84%	24.96%
Master's Degree	7.84%	7.46%
Professional Degree	1.37%	1.46%
Doctoral Degree	1.24%	1.20%
N=	673,838	271,904

Note: The percentages above are based on person-year state averages for those aged 25-34 from 1991-2015.

[22] Household size is a discrete variable, with a limited number of persons in each household. However, in the analysis the variable is treated as a continuous variable.

There are also various considerations about who experiences the impacts of affirmative action bans (see Table 4.2). There are people that had already graduated by the time the bans took place in certain states were not impacted by the bans. For example, the admission of a student that started college in 2006 at the University of Michigan was not impacted by the ban starting in 2007. These people should not be considered as being impacted by the ban in school. However, that same person could be impacted by the ban in the workplace (under the assumption they did not move out of state after graduating). Several sensitivity analyses are conducted (and detailed later) to try to capture the right age groups and take migration into account. Many of these considerations vastly reduce the number of cases that can be used for analysis thereby rendering several variable-rich datasets that only number in the tens of thousands ineffective. That is why a large dataset such as the Current Population Survey that spans the time period that the bans were passed and can provide enough state-level variation before and after a ban is ideal for this study.

Table 4.2. The population at risk to bans in education

Description	Frequency
College educated persons at highly selective schools in states with bans.	574,154

Additional data is also utilized by various sensitivity analyses in this study. The additional data sets, and variables, along with the rationale for using them are provided in those sections. Those analyses consider various populations at risk to the effects of affirmative action ban in workplaces, colleges, or both.

Analytic Strategy: Difference-in-Differences

A difference-in-differences strategy is used to estimate the causal impact affirmative action bans have on interracial marriage in states that have such bans. This strategy has been used in a number of prominent studies that examine the impact of policy changes (Bailey and Dynarski 2011) and is well-suited for this study. The "first difference" in this strategy compares interracial marriages before and after an affirmative action ban was put in place to determine whether changes are associated with the introduction of the ban. If the affirmative action ban did have an impact on interracial marriage, we would see a decline in such marriages after the policies went into effect. However, because the percentage of interracial marriage may differ from year to year for other reasons (e.g., period effects, 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, which take advantage of a comparison group of married couples who lived in states where affirmative action bans were not implemented. Among married couples in states that did not prohibit affirmative action in a particular period, changes in interracial marriages over the same period were attributed to underlying trends rather than to the affirmative action bans. After subtracting the second difference from the first, the estimated causal impact of affirmative action bans on interracial marriages remains.

As a quasi-experimental method, this analytic strategy has limited ability to support causal claims. Thus, plausible alternative explanations for the findings must be considered and ruled out (Shadish, Cook, and Campbell, 2002). For this reason, an exhaustive set of sensitivity analyses are conducted to rule out these alternative explanations. I examine how sensitive the results are to the onset of the post-ban period that best captures the timing of marriage. I also test that the sex of the first person in the household did not change due to affirmative action bans. This helps to rule-out spuriousness. A difference-in-differences estimation strategy is implemented in a multilevel regression framework, using a combination of fixed and random effects to account for the hierarchical nature of the data (observations over time, nested within states) (Murnane and Willett, 2011).

The following multilevel ordinary least squares regression was fitted (levels are household and state):

$$IRM_{ist} = \beta_0 + \beta_1(BAN_{st}) + \beta_2 W_{st} + \beta_3 cyear_s + \beta_4 P_i + \gamma S_t + \ nScyear_{st} + \rho Z_t + \varepsilon_{ist} \quad (1)$$

where IRM_{ist} indicates the whether or not a person was in an interracial marriage in a given year (t); BAN_{st} is a dichotomous variable indicating whether a state (s) had an affirmative action ban in place in year (t); W_t represents a matrix of selected time-varying state characteristics, such as state-level racial demographics (i.e., percentage of population that is White, Black, Latino, Native American, or other and White is the reference group), state-level educational attainment (i.e., the percentage of the population 25 years and older with a bachelor's degree) and state-level economic indicators, including the unemployment percentage of the age group most likely to marry (25- to 34-year-olds); adding state-specific linear time trends ($cyear_s$) to allow trends in the dependent variable over time to differ by state (coded so that 1993=1, 1994=2, 1995=3, etc.); P_i represents a matrix of personal characteristics (i.e., age, educational attainment, sex, and household size); S_t indicates a set of vectors to distinguish among the states' dichotomies and to control for all time-invariant differences, both observed and unobserved, among the states (state fixed effects); Z_t represents a set of vectors for year dichotomies 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 1993 to 2015; $Scyear_{st}$ represents a full set of two-way interactions between each state dummy and a continuous predictor representing the linear effect of year;[23] and ε_{ist} represents the residual.[24] 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; including both does 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 interracial marriage. A model is repeated for each dependent variable (i.e., separate models for each dyad: Black-White; Black-Asian, Black-Hispanic, etc.). Because state-level difference in differences models frequently provide lower standard errors than they should, I use state-clustered standard errors (Bertrand, Duflo et al. 2004)

Parallel Trend Assumption and Statistical Power

An important assumption of the difference in differences approach is that interracial marriage trends in each of the target states before introduction of the affirmative action bans is sufficiently similar to trends in the comparison states over the same period. This ensures that if not for treatment (the affirmative action bans) the treatment and control states would change in the same way, thus any difference in changes can be attributed to the treatment. Table 2 above shows that the ban and non-ban states are sufficiently similar during the years of analysis under study across a host of control variables. Figure 4.5 (below) presents each affirmative action ban state compared to the group of comparison states in the years prior-to and after the bans in each state.[25] The graphs have different axis for two reasons. First, I readjust the Y-axis from graph to

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[23] To avoid collinearity with the continuous variable *cyear,* Equation (2) does not include year dummies.

[24] This specification of the multilevel model uses a combination of fixed and random efforts 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.

[25] A problem with resolving the parallel assumptions trend comes when the treatment is too dynamic. In the case of affirmative action bans the bans (i.e., the treatment) were implemented at several points in time. The graphs shown are cumbersome, difficult to read, and can be misleading when there are a small number of cases that cause the graphs to vary more on a year-to-year basis than they should. A model proposed by Mora and Reggio (2015)

graph so that the trend can be best visualized. Second, although I centered the X-axis to 0, the X-axis corresponds to different points in time for each figure. For example, the ban was implemented in California in 1997 (passed in 1996) and implemented in Michigan in 2007 (passed in 2006). Although the pre-ban trends are different for a few of the states like Nebraska and Arizona, as a whole, the group of graphs evidence that ban and non-ban states were sufficiently similar on the dependent variable of interracial marriage before the implementation of the ban. Hence, a violation of the parallel assumption trend appears unlikely.

Because I argue that the difference-in-difference result is expected to be a decline in interracial marriage then for each state we should observe a decline in the share of interracial in ban states relative to non-ban states in the period following the ban. In other words, if there is a gain in interracial marriage in ban states then we should expect the gain in the non-ban states to be greater. Likewise, if there is a decrease in interracial marriage in the ban states then we should expect the decrease in the non-ban states to be small. We can also see a decrease in interracial marriage in a ban state while there is an increase in interracial marriage in non-ban states. Florida best illustrates the pattern of a gain in interracial marriage not being as great as the gain in comparison states. Michigan and Nebraska show a pattern where interracial marriage is declining while there is a growth in interracial marriage in comparison states. These graphs suggest that a decline in interracial marriage attributable to affirmative action bans is likely to be estimated from a difference-in-differences model.

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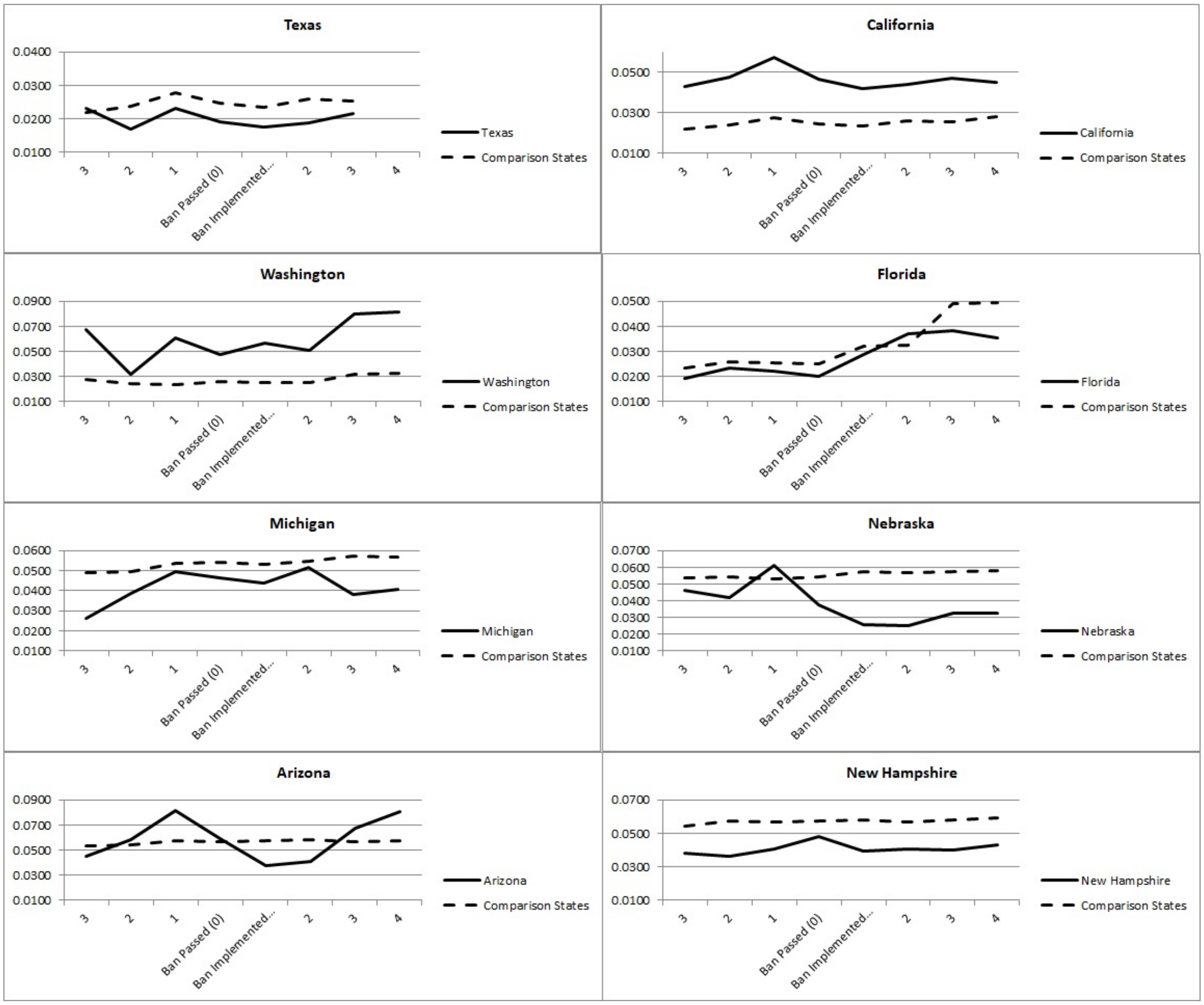
accounts for the dynamic nature of this implementation. Because there are multiple pre-treatment and post-treatment periods, multiple tests on the equality of the effect on pre- and post-treatment periods are calculated. The statistic of the equivalence of the parallel assumptions is the estimated effect on the last pre-treatment period. A limitation of this dissertation is not including this estimate, but future drafts of the three empirical chapters will include these more formal tests of the parallel assumptions trend.

Figure 4.3 Parallel Trend Assumption



Additional Sensitivity Analyses: Lagging for timing of first marriage and graduation

An additional set of sensitivity analyses were performed for the subset of couples where both of the people had a bachelor's degree or more. Those models censored the time for 4, 5, 6 and 7 years after the bans. The first model adjusted for the 4 years that it would take an individual to complete college, and that students may have taken longer to graduate. These models also consider that the timing to first marriage (27 for women and 29 for men) usually occurs years after most people finish college (22 years old). These lags shift the window of the post ban period. For example, if a ban was implemented in 2000, the 4-year lag would shift the start of the ban to 2004. The rationale here was to see how sensitive the models were to the timing of marriage and graduation. However, the models performed similarly to the base model, meaning that the timing of graduation and the age of first marriage do not have a noticeable impact on these models. These models are included in Appendix B (Table 3.B).

FINDINGS

Table 4.3, the base model for difference-in-differences results, indicates that there was a decline in interracial marriage in affirmative action ban states due to the bans. Because the dependent variable is a proportion from 0.00 to 1.00 the coefficient of 0.0054 amounts to half of a percentage point. For ease of interpretation, I calculated the percentage change (i.e., the change between interracial marriage prior to the ban, and the causal-impact estimate of interracial marriage after the ban). Interpreting the impact of the ban in this way means the bans created a 12-percentage point decline. In other words, although the change interracial marriage is small (0.0054) because the incidence of any intermarriage is also small in the affirmative action ban states prior to the ban (0.043), the relative change of interracial marriage due to the bans is substantial at 12%. The bans the Hispanic-Asian, and White-Asian interracial marriages that are driving the overall interracial marriage decline. They explain most of the 12-percentage point change in interracial marriage. These findings confirm the first two relationships posed by this paper: 1) interracial marriage declined in ban states as a result of affirmative action bans and 2) the racial composition of the dyad is important; the various racial groups are disproportionately impacted by affirmative action bans. To check for spuriousness, I added sex to this model. The sex of the first person listed in the household should not be impacted by affirmative action in any way. This variable is not statistically predictable from the bans; the coefficient is small in magnitude (0.0040 relative to its average near 0.50). This successful placebo test allows us to feel more confidence in the results where interracial marriage is the dependent variable.

Table 4.3: The causal impact of affirmative action on interracial marriage

	Base Model			
Marriage type	Coef (SE)	Sig	N	Percentage Change
Any	-0.0054(0.0013)	***	917,380	-12.64
Black-Hispanic	0.0002(0.0001)		875,712	20.03
Black-White	0.0002(0.0005)		879,375	4.86
Black-Asian	0.0002(0.0001)		875,700	40.99
Hispanic-White	-0.0001(0.0002)		875,960	-10.50
Hispanic-Asian	-0.0004(0.0001)	**	875,600	-63.78
White-Asian	-0.0021(0.0007)	***	883,788	-26.05
Sex (Spuriousness check)	0.0040(0.0029)		917,380	0.80

Table 4 shows that affirmative action bans not only impact individuals that went to college; they also impact couples where neither individual has a college degree. While the bans seem to impact couples with no bachelor's degrees and couples were both persons have at least a bachelor's degree similarly as far as any interracial marriage is concerned, there is a clear difference on the impact of the White-Asian dyad; the impact is more pronounced when both partners have at least a bachelor's degree (a statistically significant coefficient of -0.0048). These two racial groups (White and Asians) comprise the non-URM group in many studies that have estimated the causal impact of affirmative action on various levels of higher education (graduate school, undergraduate, and professional schools). They have both increased in proportion after the ban at college campuses, and their preference (as observed by real marriages) is to marry within their racial group. This explains why their coefficient is so much larger when both partners have earned a bachelor's degree as opposed to when neither partner has earned a bachelor's degree.

Table 4.4: The impact of the ban across educational attainment

	No Bachelor's			Only One Bachelor's or Greater			Both Bachelor's or Greater		
Marriage type	Coef (SE)	Sig	N	Coef (SE)	Sig	N	Coef (SE)	Sig	N
Any	-0.0064(0.0017)	***	549,360	0.0027(0.0033)		184,305	-0.0061(0.0030)	*	176,339
Black-Hispanic	-0.0002(0.0002)		526,113	-0.0006(0.0004)		135,822	0.0000(0.0003)		167,518
Black-White	-0.0000(0.0005)		528,262	0.0008(0.0011)		180,813	0.0002(0.0011)		168,210
Black-Asian	0.0002(0.0002)		526,051	0.0003(0.0004)		174,729	0.0001(0.0003)		116,289
Hispanic-White	-0.0004(0.0002)		526,252	0.0006(0.0004)		174,754	0.0000(0.0004)		137,040
Hispanic-Asian	-0.0005(0.0001)	**	526,018	-0.0001(0.0003)		174,678	-0.0005(0.0003)		167,528
White-Asian	-0.0023(0.0007)	***	529,290	-0.0016(0.0017)		176,983	-0.0048(0.0020)	*	170,139

DISCUSSION

The findings of this paper indicate that affirmative action bans have led to a decline in interracial marriage, and that pattern has largely been led by the decline in interracial marriage between Whites and Asians and has more so been the result of the effect of affirmative action bans on higher education than the effect of affirmative action bans on the workplace. The first of these findings, the decline in interracial marriage due to affirmative action bans, is delaying the onset one of the most well-known demographic moments to occur in the 21st century: the U.S. is expected to become a majority minority society by 2045 (Frey 2018). During that year, Whites will comprise 49.7 percent of the population. Because interracial marriage declines as a result of these bans it is likely that there will be fewer bi-racial and multi-racial people born from these marriages. Furthermore, this paper posited that if the bans led to a decrease in interracial marriage then the bans could be thought of as antithetical to *Loving v. Virginia*, the Supreme Court decision that legalized interracial marriage. Although affirmative action bans have no *de jure* impacts on interracial marriage their *de facto* impacts on interracial marriage are tangible. Affirmative action bans are not anti-miscegenation *per se*, but there are certainly similarities to it; while *Loving v. Virginia* focused on black-white interracial marriage, the affirmative action bans mainly impact White-Asian interracial marriage.

The second finding in this paper found that it was largely intermarriage among the beneficiaries of affirmative action policies (Whites and Asians) that led the declines in interracial marriages. This finding suggests that the effects of changing population composition on interracial marriage may depend on the level of racial homophily. This finding further advances that argument because it is a causal example of population change influencing intergroup relations. Elsewhere in the relationship selection literature (marriages, friendships, and otherwise) the studies are mostly descriptive in nature (Lin and Lundquist 2013; Marmaros and Sacerdote 2006; Mayer and Puller 2008; Mollica, Gray, and Trevino 2003; Wimmer and Lewis 2010 Reskin, McBrier, and Kmec 1999; Tomaskovic-Devey 1993; Tomaskovic-Devey, Thomas, and Johnson 2005). One of the most advanced causal studies of intergroup relationship relies on games that are artificial (Abascal 2015). This study (to the author's knowledge) is the only empirical study that has focused on population change causally impacting relationship selection. Similar to how Abascal (2015) positions Hispanics as part of a racial triangle with Blacks and Whites, this study comments on an Asian triangle of racial relations with Blacks and Whites (Kim 1999). While Black-White and Black-Asian intermarriage did not change with a statistically significant result Asian and White intermarriage did. I largely attribute this to a compositional effect: there were relatively few Blacks in postsecondary education even prior to while there were numerous Asians and Whites in higher education. This study furthers Kim's argument that the location of Asian American's has continued to reinforce White racial power. Kim and others argue that Asians have attempted to "be White" in order to get ahead (Tuan 1998), disidentify with Blacks, and oppose affirmative action. Affirmative action bans create a divide between Whites and Asians that preserves their racial boundaries and maintains a social distance between each group. This social distance, albeit unintentional serves to preserve White racial power, and racial homogeneity.

The targeting of postsecondary institutions by affirmative action policies that has given rise to the multitude of impacts of affirmative action bans. They have created a redistribution of people across society's institutions that not only impact the composition of the institutions themselves but will also impact the demographic composition of the United States. Before this study, it was only theorized that affirmative action bans had the anticipated and intended consequences of decreasing the racial share of underrepresented minorities at colleges and universities. Now affirmative action bans have been shown to have both unanticipated and unintended consequences that have broader impacts on the demographic composition of the United States. Hopefully, this article gives rise to additional work on the unintended and unanticipated impacts of various policies [including affirmative action] on major life course events such as birth, intermarriage, and death. It is difficult to imagine that this policy could have had this effect when it was conceived, but perhaps better understanding the true scope of its impacts will lead to different discussions about its impact and efficacy.

Lastly, I offer some limitations in this research. More detail about a person's educational history (e.g., college attended, selectivity of a school attended), employment (e.g., public or private) and migration history (e.g., state born, state where they attended college, state(s) they have worked in), and racial preferences in dating would have been invaluable to study the impact of the bans on interracial marriage. These variables would have helped to create a stronger narrative for the mechanisms leading to declines in interracial marriage. The structural variables would have allowed me to stratify between private schools and public schools. This would have allowed me to more accurately capture the impact of the bans. The assortative mating variables would have allowed me to investigate if people's preferences (beyond what is observed through marriage) were changing due to the bans. While some data sets provide this level of detail none of them provide that level of detail across the number of cases and the number of states that would be required to answer the research questions posed by this study. Better quality data would have led to more traditional analysis with a stronger focus on mechanisms and less reliance on inductive reasoning to verify the existence of mechanisms.

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APPENDIX A: Affirmative action bans and college selectivity

Highly selective schools are not impacted the most by affirmative action bans [*contra* (Backes 2012, Hinrichs 2012, Arcidiacono, Aucejo et al. 2014)]. There are shortcomings with the previous studies: there are fewer units of analysis in previous studies, previous studies place a greater emphasis on Texas and particularly California, and selectivity was operationalized differently. Hinrich's (2012) study places more weight on results in California and Texas because they contributed more "school-years" [observations] relative to other bans states in his study because his analytic window spanned from 1995 to 2007 and because Texas and California have more schools than the other states. Backes' study (2012) is similar in emphasizing Texas and California because it spans from 1990 to 2009. Arcidiacono et al. use a wider analytic window, but only focus on California (2014). Additionally, Backes uses SAT quantiles to measure selectivity. He uses four groups: the first, second, and third deciles of highest SAT scores, and a fourth group for the lower seven deciles that yielded only 526 schools (far less than used here). To engage with the different methodological applications above I produce the following analyses in the table below using the same IPEDS database as Hinrichs and Backes used in their studies. First, I exclude all of the ban states except California and Texas. Next, I reduce my analytic period from 1991 to 2009 to replicate the time periods used by previous studies. Finally, I use SAT deciles like the previous studies instead of the Barron's Admissions Competitiveness Index to measure selectivity.

In Table 4.A below I there is a notable difference in the results between all states compared to Texas and California using Barron's Admissions Competitiveness Index. For the weighted public STEM results for all states the negative effects of the ban are greater in highly selective schools than less selective ones, but when including all the ban states in the analysis the effects of the bans are lesser in highly selective schools than less selective ones. Texas and California exhibit a selectivity pattern not found in other states.

Changing the analytic period was also important. In the full-analytic period (through 2016) using Barron's Admissions Competitiveness Index and investigating California and Texas, highly selective schools were the least impacted schools, followed by less selective schools, and unclassified schools. However, when the analysis is right censored to 2009 (as was done in the aforementioned studies), highly selective schools are the most impacted by the bans, followed by less selective schools, and unclassified schools. This inversion of order also indicates that the selectivity pattern exhibited by other studies may be sensitive to the time period considered.

Lastly, the choice to use the Barron's Admissions Competitiveness Index or SAT changes the findings. For all the years of analysis for Texas and California using Barron's Admissions Competitiveness Index the negative impact of the ban for weighted public STEM schools are smallest for highly selective schools, larger for less selective schools, and largest for unclassified schools. When SAT scores are used instead of the Barron's Admissions Competitiveness Index the results were more negative in the top decile than they were in the second or third deciles, but were most negative for all other schools that submitted SAT scores to IPEDs. Furthermore, choosing SAT scores restricts the analysis to a smaller number of schools (in 2016 when using SAT scores the number of schools in the analysis is 1,219, and using the Barron's Competitiveness index yields 1,435 [out of 2,436 4-year degree granting schools]).

In summary, the choice of states, the specificity of time, and the choice of which variable to control for selectivity produced conditions in earlier studies to conclude that the more selective a school was, the more it would be impacted by affirmative action. The results here suggest that institutional selectivity is not as meaningful in determining what has happened to URM students in the wake of affirmative action bans as previously though

Appendix A

Table 4.A Why selectivity is not as important as previously thought

	STEM								Non-STEM							
	Public				Private				Public				Private			
	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.	Unweighted	Sig.	Weighted	Sig.
Selectivity by Barron's (TX and CA only)																
Any Selectivity (including Unclassified)	-0.0310	***	-0.0617	***	0.1255	*	0.0731	***	-0.0312	***	-0.0583	***	0.0029	***	0.0041	**
Highly Selective (w/o unclassified)	-0.0315	***	-0.0530	***	0.0251	*	0.0316	**	-0.0290	***	-0.0466	***	-0.0089	***	-0.0795	***
Less Selective (w/o unclassified)	-0.0258	***	-0.0580	***	0.0354	***	0.1327	***	-0.0297	***	-0.0596	***	-0.0013		0.0160	***
Unclassified Only	-0.0382	***	-0.0668	***	-0.0021		0.0040		-0.0363	***	-0.0645	***	0.0010		0.0207	***
1991-2009 (TX and CA only)																
Any selectivity	-0.0177	***	-0.0337	***	0.0078		0.0414	***	-0.0149	***	-0.0279	***	0.0001		-0.0125	***
Highly Selective (w/o unclassified)	-0.0200	***	-0.0356	***	0.0293	*	0.0152		-0.0110	***	-0.0221	***	-0.0098	***	-0.0171	***
Less Selective (w/o unclassified)	-0.0157	***	-0.0333	***	0.0240	*	0.0603	*	-0.0149	***	-0.0285	***	-0.0072	***	-0.0089	***
Unclassified Only	-0.0166	***	-0.0203	***	-0.0045		-0.0034		-0.0164	***	-0.0320	***	-0.0018		-0.0024	
1991-2009 (All states)																
Any selectivity	-0.0200	***	-0.0203	***	0.0110	*	0.0361	***	-0.0101	***	-0.0124	***	-0.0024	***	-0.0146	***
Highly Selective (w/o unclassified)	-0.0209	***	-0.0218	***	0.0030		0.0036		-0.0088	***	-0.0114	***	-0.0105	***	-0.0146	***
Less Selective (w/o unclassified)	-0.0186	***	-0.0264	***	-0.0079		0.0579	***	-0.0135	***	-0.0204	***	-0.0119	***	-0.0146	***
Unclassified Only	-0.0146	***	-0.0164	***	0.0192	*	-0.0049		-0.0062	***	-0.0092	***	-0.0007		-0.0056	*
Selectivity by SAT (All Years & States)																
Top decile	-0.0227	***	-0.0200	***	-0.0109	**	-0.0082		-0.0189	***	-0.0182	***	-0.0185	***	0.0088	***

2nd decile	-0.0088	***	-0.0070	**	-0.0172	**	-0.0137	***	-0.0053	**	-0.0045	**	-0.0062	**	-0.0046	*
3rd decile	-0.0118	*	-0.0146	*	-0.0016		-0.0016		-0.0162	***	-0.0204	***	-0.0095	***	-0.0118	***

Table 4.A continued

All other schools	**-0.0148**	***	**-0.0215**	***	**0.0128**	*	**0.0321**	***	**-0.0127**	***	**-0.0192**	***	**-0.0071**	***	**-0.0036**	**
All Years (TX and CA only)																
Top decile	-0.0433	***	-0.0728	***	-0.0085		0.0135		-0.0388	***	-0.0642	***	-0.0193	***	-0.0144	***
2nd decile	-0.0310	***	-0.0382	***	-0.0192		-0.0198	**	-0.0234	***	-0.0337	***	-0.0144	***	-0.0144	***
3rd decile	-0.0078		-0.0057		0.0166		0.0164		-0.0109		-0.0192		-0.0001		-0.0107	*
All other schools	-0.0231	***	-0.0610	***	0.0400	***	0.0931	***	-0.0256	***	-0.0567	***	0.0005		0.0186	***
1991-2009 (TX and CA only)																
Top decile	-0.0231	***	-0.0423	***	-0.0026		0.0062		-0.0143	***	-0.0225	***	-0.0115	***	-0.0190	***
2nd decile	-0.0157		-0.0199	*	0.0027		-0.0123		-0.0078		-0.0162	**	-0.0143	**	-0.0195	***
3rd decile	-0.0063		-0.0126		0.0066		0.0086		-0.0102	*	-0.0232	***	0.0021		-0.0105	*
All other schools	-0.0166	***	-0.0392	***	0.0209		0.0313		-0.0123	***	-0.0245	***	-0.0098	***	-0.0133	***

Appendix B

Table 4.B

	4-year lags			5-year lags			6-year lags			7-year lags		
Marriage type	Coef (SE)	Sig	N	Coef (SE)	Sig	N	Coef (SE)	Sig	N	Coef (SE)	Sig	N
Any	-0.0052(0.0047)		169,675	-0.0093(0.0045)	*	167,759	-0.0141(0.0063)	*	165,927	-0.0044(0.0084)		164,057
Black-Hispanic	-0.0005(0.0004)		161,164	-0.0004(0.0004)		159,362	-0.0001(0.0003)		161,514	-0.0000(0.0003)		159,731
Black-White	0.0005(0.0014)		163,936	0.0002(0.0016)		160,020	-0.0015(0.0019)		158,270	-0.0002(0.0027)		156,483
Black-Asian	-0.0003(0.0005)		161,193	-0.0000(0.0005)		159,388	0.0000(0.0006)		157,649	0.0003(0.0004)		159,756
Hispanic-White	0.0006(0.0006)		131,501	0.0006(0.0006)		159,422	-0.0001(0.0004)		161,578	-0.0002(0.0005)		159,792
Hispanic-Asian	-0.0009(0.0004)	*	161,177	-0.0007(0.0005)		159,375	-0.0011(0.0006)	*	157,636	-0.0016(0.0008)	*	155,853
White-Asian	-0.0050(0.0025)	*	163,694	-0.0084(0.0031)	**	159,770	-0.0091(0.0036)	**	160,080	-0.0043(0.0049)		158,274
White-Native American	0.0024(0.0008)	**	161,365	0.0013(0.0010)		159,555	0.0008(0.0011)		157,813	-0.0008(0.0016)		156,026
White-Multiracial	-0.0013(0.0015)		161,926	-0.0001(0.0018)		160,121	0.0004(0.0021)		158,377	0.0010(0.0028)		156,576
Asian-Multiracial	-0.0005(0.0005)		161,199	-0.0001(0.0003)		163,287	-0.0001(0.0004)		161,549	-0.0011(0.0002)	***	159,766
Multiracial-Multiracial	-0.0057(0.0045)	*	169,057	-0.0088(0.0051)	*	167,152	-0.0151(0.0061)	**	165,323	-0.0067(0.0082)		163,462

Chapter 5: Conclusion

The results of this dissertation provide compelling evidence for the presence of systemic institutional racism that is often associated with *laissez faire* racism. This dissertation finds that the colorblind ideology manifested in affirmative action bans produce institutional inequality by shaping access to education. Furthermore, the impacts of the ban extend beyond institutional walls and reach the societal, by decreasing interracial marriage. As such, this work highlights the crossroads of affirmative action bans and the laissez-faire racism.

A Reminder of the Real World Implications of this Research

It is important to remember that these empirical findings suggest important impacts for real people. Millions of people have been impacted by the bans on affirmative action. My results suggest that more URMs would have gone to prestigious highly selective colleges and universities if not for these bans. Fewer URMs would have attended for-profit colleges. URM degree attainment in STEM would be higher. There would also be more interracial marriages un absence of the bans; a trend that has surely slowed-down the countdown to the most important demographic moment in the twenty first century when the United States becomes a majority minority society. Together these findings reinvigorate classic moral and political debates around the question "who is college for?" They have the potential to inform the ongoing public debate around Harvard's current affirmative action related litigation and subsequent litigation concerning the nature of affirmative action in the U.S. court system. *Particularly, these findings illustrate an egregious irony regarding anti-affirmative action policy; the premise of equal* *opportunity in education on which colorblind ideology is built upon crumbles under the sheer weight of racial inequality as the antithesis of equal opportunity.* The new outcomes I have studied suggest that price of racial control over the resource of selective higher education reinforced through affirmative action bans is far greater than previously reported in the literature on the causal impact of affirmative action bans (Wightman 1997, Espenshade and Chung 2005, Backes 2012, Hinrichs 2012, Garces 2013, Garces and Mickey-Pabello 2015). We need to do more to understand many of the potential unintended consequences of affirmative action bans and other social policies.

The Sociological Contributions of This Dissertation

This dissertation has also made contributions to sociology and the study of social policy. First, it adds to the vast canon on racial inequality in the sociology of education (Campbell, Coleman et al. 1966, Jencks 1979, Roscigno and Ainsworth-Darnell 1999, Lareau 2000, Kao and Thompson 2003, Alon and Tienda 2007, Brand and Xie 2010, Jencks and Phillips 2011) by showing how a change in educational policy can lead to racial inequality in access to education. The affirmative action ban cascade also adds to the rich organizational literature within postsecondary education (Meyer 1977, Meyer and Rowan 1977, Bastedo and Gumport 2003, Stevens, Armstrong et al. 2008, Van Vught 2008), because it shows that although the frame of colorblind racism may have been more universally applied in higher education through institutional isomorphism (DiMaggio and Powell 1983) it fails to have had a consistent effect across all universities due to the variation in school selectivity and lack of admissions competitiveness at numerous colleges and universities (Bowen and Bok 2016). This feature helps to explain why URM enrollment at for-profit universities increased due to the bans. Secondly, this dissertation informs racial policy and race relations by redefining and extending the empirical boundary for studies of institutional racism to include unintended and unanticipated consequences, building on the work of Bobo (1997), Bonilla-Silva (2006), and Ray (2019). Thirdly, this study makes an important contribution to the study of marriage and family by showing how shifts in racial compositions of institutions have the potential to reshape the marriage market. This contribution is akin to that of Bobo, who has shown that racial schemas are tied to institutional inequality. Bobo (1983, 1996) and I advanced the discourse by showing how the adoption of affirmative action bans led to a causal change in institutional inequality. The assortative mating literature has also mostly been descriptive in connecting racial schemas to interracial marriage patterns (Kalmijn 1993, Qian 1997, Kalmijn 1998, Qian and Lichter 2007, Qian and Lichter 2011, Rosenfeld and Thomas 2012, Lin and Lundquist 2013, Curington, Lin et al. 2015). I advance that literature by showing how the shift in racial schemas (i.e., switching from affirmative action to a colorblind meritocracy) led to a causal change in interracial marriage. Lastly, although race and education, race and marriage, or education and marriage are frequently studied together this dissertation brings all three subfields together to enhance our understanding of inequality. The strongest example of this is the reframing of anti-miscegenation policy under *laissez faire* racism. While anti-miscegenation laws are part of a group of laws emphasizing the *de jure* racial inequality and racial segregation of the Jim Crow era, affirmative action bans are similar to anti-miscegenation in the era of *laissez faire* racism not because of their *de jure* impact on postsecondary impacts, but because they lead to *de facto* racial inequality as measured by the declines in interracial marriage that ultimately preserve the dominance of the White racial group. This point is underscored the finding that White and Asian interracial marriage declined due to affirmative action bans.

Directions for Future Work: Filling the Sociological Void of Affirmative Action

More importantly sociological research on affirmative action is scant. There is only a handful of research on the topic and most of it considers general attitudes about affirmative action (Sidanius, Pratto et al. 1996, Skrentny 2006, Skrentny 2018), affirmative action the workplace (Kelly and Dobbin 1998, Kalev, Dobbin et al. 2006) and not education. Within the sociology of education, literature on mismatch speculates that affirmative action bans may have produced inequality, but offers no empirical evidence to single out affirmative action as a culprit (Alon and Tienda 2005, Fischer and Massey 2007, Kurlaender and Grodsky 2013). Researchers studying the race-gap in higher education also cite it as a potential mechanism for producing racial inequality, but again fail to model how it contributes to racial inequality beyond speculation (Massey, Charles et al. 2011, Bowen and Bok 2016, Ciocca Eller and DiPrete 2018). Hallinan (2001) best describes the paucity of the literature when she writes "Compared to the fairly large body of literature on the effects of desegregation at elementary and secondary levels, research on the consequences of affirmative action policies for blacks and other minority students and the effects of multiracial colleges on student outcomes is scarce." While there is hope because there is a new sociological literature on affirmative action, it lags ten-years behind similar work in economics (Long 2004). The newer sociological literature promotes a work-around to affirmative action bans by substituting socioeconomic status for racial identity in college admissions (Alon 2015, Reardon, Baker et al. 2017). I insist that there is much more work to be done in sociology to understand the impacts of affirmative action bans on personal, organizational, institutional, and societal levels and firmly believe that the empirical findings contained within this dissertation are just the tip of the iceberg. It is my hope that this study leads to more fruitful discussions of public sociology and policy sociology that have been lacking in the discipline for decades (Burawoy 2005, Clawson, Zussman et al. 2007).

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