Essays on Human Capital Investment Decisions: K-12, Higher Education, and the Labor Market

HARVARD
Kenneth C. Griffin

**GRADUATE SCHOOL
OF ARTS AND SCIENCES**

THESIS ACCEPTANCE CERTIFICATE

The undersigned, appointed by the

Department of Education

have examined a dissertation entitled

Essays on Human Capital Investment Decisions:
K-12, Higher Education, and the Labor Market

presented by Samantha Batel Kane

candidate for the degree of Doctor of Philosophy and hereby certify that it is worthy of acceptance.

Signature ______________________

Typed name: Prof. Martin R. West

Signature ______________________

Typed name: Prof. David J Deming

Signature ______________________

Typed name: Prof. Susan Dynarski

Signature ______________________

Typed name: Prof.

Signature ______________________

Typed name: Prof.

Date: April 29, 2025

Essays on Human Capital Investment Decisions: K-12, Higher Education, and the Labor Market

A dissertation presented

by

Samantha Batel Kane

to

Harvard Graduate School of Education

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Education

Harvard University

Cambridge, Massachusetts

April 2025

Dissertation Advisor: Martin West Samantha Batel Kane

Essays on Human Capital Investment Decisions:
K-12, Higher Education, and the Labor Market

Abstract

In this dissertation, I use applied econometric methods to explore a myriad of human capital investments that individuals choose in K-12, higher education, and labor market contexts. The findings provide insight into how public and education policies shape what and where students, particularly women, study and/or work.

The first paper studies the impact of state reproductive rights laws on women's college choices after the U.S. Supreme Court eliminated the constitutional right to abortion in *Dobbs v. Jackson Women's Health Organization* (2022). Using college application data from the Common App, I find that abortion bans caused a significant decrease in the proportion of high-achieving women who applied to a school in states with abortion bans. Effects were larger for applicants from states without abortion bans, as well as for applicants from the most liberal counties in the United States. Measurable effects first emerged in the 2021-22 college application season after several Court actions suggested that it would overturn *Roe v. Wade* (1973) the following year, increased in magnitude in the 2022-23 college application season, and persisted in the 2023-24 college application season.

The second paper examines the effect of a district-level policy change to a K-12 centralized school assignment system on school choice. The policy change, which occurred in a large urban school district in the Northeast, increased families' access to nearby high-quality schools and provided them with new information about school quality. Using district administrative data, I find that families chose higher-quality schools as their preferred first-choice school when their access to these schools increased. I also find suggestive evidence that gaining information about school quality affected school choices above and

beyond improvements to school choice sets. These findings coincided with a decrease in the number of schools that families could choose from, suggesting that school choice decisions can improve as options decrease.

The third paper describes the relationship between non-pecuniary workplace preferences and college majors, occupations, and the gender pay gap. I use data on first-time bachelor's degree-seeking traditional undergraduate students from the 2012/17 Beginning Postsecondary Students Longitudinal Study. First, I find that workplace preferences became less important relative to salary as students approached the workforce. Second, I find that workplace preferences, particularly preferences for helping others as a part of one's job, were positively associated with female-dominated majors and occupations (e.g., education and health care fields) and negatively associated with male-dominated majors and occupations (e.g., business and STEM fields). Third, I find that preferences for helping others explain a significant portion of the early-career gender wage gap; women were more likely to choose jobs within the same field that involved helping others and were associated with lower wages.

Table of Contents

List of Tables

List of Figures

Acknowledgments

My time in graduate school has been a labor of love. Near the start, I heard some advice for those deciding whether to venture down this path that stuck with me – five or six years will go by, regardless, but at the end of those years you could have a PhD. Well, here we are!

This path was lined with people who made my journey possible. My doctoral committee, Martin West, Susan Dynarski, and David Deming, helped to shepherd me along with kind ears and sharp insight. Thank you.

I am grateful for feedback at various stages of my research from a host of faculty mentors, including Christopher Avery, Raj Chetty, Will Dobbie, Claudia Goldin, Thomas Kane, and Eric Taylor. I am incredibly thankful for my PhD colleagues who provided sage advice and friendship, including but not limited to Anthony Bald, Christopher Cleveland, Alexis Gable, Farah Mallah, Christine Mulhern, and Lena Shi, as well as EPPE peers past and present. I am also thankful for the HGSE doctoral program staff, Clara Lau and Eric Zeckman, for their help in navigating PhD milestones and support as I completed my MPP.

Thank you to seminar and conference participants from AEFP, APPAM, the Council of Economic Advisors, CESifo/ifo, the Harvard Economics of Education Workshop, the Harvard Economics and Social Policy Workshop, the Harvard Labor/Public Economics Workshop, and the Office of the Chief Economist at the U.S. Department of Education for helpful comments and discussions. I also gratefully acknowledge Brian Heseung Kim and his colleagues at the Common App, Karen B. and my school district partnership, and the National Center for Education Statistics for access to the data that I use in my research; GSAS, HGSE, the PIER Fellowship, and the Russell Sage Foundation for financial support; and the Harvard Kennedy School Women and Public Policy Program Research Fellowship for its community.

Lastly, I would like to thank my friends and family for being my champions. My mom, for her abounding love and support; my dad, for his confidence in me and for fostering my confidence in myself; and my sister, my sounding board, for paving the way through higher education. My husband, Dan, for his steady hand and endless encouragement. Tucker, the

best boy, who walked the entire path with me. And my daughter, Amelia, whose light got me over the finish line.

Introduction

This dissertation consists of three papers. In each paper, I use applied econometric methods to examine human capital investment decisions in K-12, higher education, and labor market contexts. The findings provide insight into how public and education policies shape what and where students, particularly women, study and/or work. The first paper studies the effect of state abortion bans on women's college choices after the U.S. Supreme Court eliminated the constitutional right to abortion in *Dobbs v. Jackson Women's Health Organization* (2022). The second paper examines school choice in a K-12 centralized assignment system, where a policy change increased families' access to nearby high-quality schools and provided families with new information about school quality. The third paper examines the relationship between workplace preferences and several human capital decisions in higher education and the labor market, including college major, occupation, and wages.

Chapter 1: In the Wake of *Dobbs*: The Effect of State Abortion Bans on Women's College Choices

This paper studies the impact of state reproductive rights laws on women's human capital decisions after the U.S. Supreme Court eliminated the constitutional right to abortion in *Dobbs v. Jackson Women's Health Organization* (2022). Using data from the Common App, the undergraduate college admission application, I implement a difference-in-differences design that compares high-achieving women's college choices to those of their male peers. I find that abortion bans caused a 2.7 percentage point decrease in the proportion of high-achieving women who applied to a school in one of the 13 states with a total ban. Effects were larger for applicants from states without a restriction on abortion, as well as for applicants from the most liberal counties in the United States. Further, treatment effects first emerged in the 2021-22 college application season after several Court actions suggested that it would overturn *Roe v. Wade* (1973) the following year, increased in magnitude in the 2022-23 college application season, and persisted in the 2023-24 college application season.

Chapter 2: Changing Choice Sets: How Parents Respond to Increases in Access to High-Quality Schools

This paper examines the effect on school choice of a centralized school assignment system designed to (1) increase families' access to nearby high-quality schools and (2) provide families with new information about school quality. Using administrative data from a large urban public school district and a difference-in-differences design, I find that families chose higher-quality schools as their preferred first choice for their kindergarten students when their access to these schools increased. I also find suggestive evidence that gaining information about school quality affected parents' choices above and beyond improvements to their school choice sets. These findings coincided with a decrease in the number of schools that families choose from, suggesting that school choice decisions can improve as options decrease.

Chapter 3: Unpacking the Black Box of Tastes: Altruism and the Gender Pay Gap

This paper examines the relationship between gender differences in non-pecuniary workplace preferences and human capital investments. Using survey data on traditional bachelor's degree-seeking first-time undergraduates, I document trends in the relative importance of five workplace preferences (balancing work and family, balancing work and leisure time, being an expert, helping others, and making decisions) to salary. I find that workplace preferences became less important relative to salary as students approached the workforce. Second, I find that workplace preferences, particularly preferences for helping others as a part of one's job, were positively associated with female-dominated majors and occupations (e.g., education and health care fields) and negatively associated with male-dominated majors and occupations (e.g., business and STEM fields). Third, I find that preferences for helping others explain a significant portion (12 percent) of the early-career gender wage gap; women were more likely to choose jobs within the same field that involved helping others and were associated with lower wages.

Chapter 1

In the Wake of *Dobbs*: The Effect of State Abortion Bans on Women's College Choices

1.1 Introduction

Conditional on applying to college, deciding where to apply is one of the most important decisions that applicants make in their college search process. Differences in behavior in the application stage affect where students enroll (Hoxby and Avery, 2013), and where students enroll impacts their human capital and labor market outcomes (Chetty, Deming, and Friedman, 2023; Zimmerman, 2019). This search process is particularly salient for prospective college students who cast a wide net and apply out-of-state. In 2020, for example, a quarter of all first-time undergraduate students enrolled out-of-state (U.S. Department of Education, 2023b).[1]

Applicants have historically based their college decisions on a variety of institution and match-specific factors, such as school quality, cost, and quality (Long, 2004). However, state reproductive rights laws became a new variable for applicants to consider in their college search process after the U.S. Supreme Court overturned *Roe v. Wade* (1973) in *Dobbs v. Jackson Women's Health Organization* (2022). This decision eliminated the federal constitutional right to abortion and set in motion state trigger laws and other restrictions that were previously blocked in court. Press coverage shortly thereafter suggested that these restrictions could affect where students attend college (Anderson, 2022, Bernstein and Horowitch, 2022).

In this paper, I estimate the causal impact of state reproductive rights laws on where women apply to college by examining changes in application behavior to states with and without abortion restrictions before and after the *Dobbs* decision. Using data from the Common Application (Common App), I estimate treatment effects for high-achieving female college applicants relative to their high-achieving male peers. The analysis focuses on high-achieving women for two reasons. First, high-achieving applicants exert more choice (i.e., apply to more out-of-state schools and states) in the college search process than their lower-achieving peers. These applicants are thus more likely to be impacted by out-of-state

[1]This figure is based on the number of domestic degree-seeking undergraduate students who enrolled in four-year public and private not-for-profit institutions of higher education in one of the 50 U.S. States and D.C. within 12-months of graduating high school for the first time.

social policies.[2] Second, state reproductive rights laws are more salient for young women, even if the effect on men is not zero.[3] I thus estimate the impact of abortion bans for a “more” treated group relative to a “less” treated group, which is a conservative approach to identifying their causal impact on women’s college application behavior. Further, high-achieving women and men had similar college application patterns prior to *Dobbs*, which facilitates using a difference-in-differences identification strategy.

To implement the analysis, I conduct a state-by-state review of abortion policy by the 2022-23 college application season, which was the first application season post-*Dobbs*. As shown in the right-hand column of Table 1.1, 24 states passed a law to restrict or ban access to abortion, some of which were blocked by court orders. Of those states, 13 states (marked in bold with a bullet) enforced a total abortion ban. In the remainder of the analysis I refer to all 24 states as those with an “abortion law" and the most restrictive 13 states as those with an “abortion ban". In the left-hand column, 26 states and D.C. did not have a law to restrict or ban access to abortion. I refer to this group of states as those with “no abortion restriction".

[2] According to the Common App, more than half of all applicants apply to five or fewer schools, and the modal applicant applies just once.

[3] In Section 1.8, I provide suggestive evidence that there is no significant treatment effect for men.

Table 1.1: *Abortion Policy by State in the 2022 College Application Season*

No Restriction		Abortion Law	
Alaska	Nebraska	**Alabama**•	Ohio
California	Nevada	Arizona	**Oklahoma**•
Colorado	New Hampshire	**Arkansas**•	South Carolina
Connecticut	New Jersey	Florida	**South Dakota**•
Delaware	New Mexico	Georgia	**Tennessee**•
D.C.	New York	**Idaho**•	**Texas**•
Hawaii	North Carolina	Indiana	Utah
Illinois	Oregon	Iowa	**West Virginia**•
Kansas	Pennsylvania	**Kentucky**•	**Wisconsin**•
Maine	Rhode Island	**Louisiana**•	Wyoming
Maryland	Vermont	Michigan	
Massachusetts	Virginia	**Mississippi**•	
Minnesota	Washington	**Missouri**•	
Montana		North Dakota	

Notes: This table categorizes the 50 U.S. states and D.C. based on their abortion restrictions in the 2022-23 college application season. In the first column, 26 states and D.C. did not have an abortion restriction beyond a 20-week late gestational age ban in Montana and Nebraska. In the second column, 24 states had an early gestational age or complete abortion ban, some of which were blocked by court orders. Of those states, the 13 states with enforceable abortion bans are in **bold** with a bullet(•). Abortion bans in Arizona, Indiana, Michigan, North Dakota, Utah and Wyoming, and early gestational age bans in Iowa, Ohio, and South Carolina were blocked by court orders. Access to abortion in Arizona, Florida, Georgia, and Utah was restricted after 6, 15, or 18 weeks.

To identify the treatment period, I take into consideration both the timing of *Dobbs* and the policy environment leading up to the decision. I consequently define the treatment period as both the 2021-22 and 2022-23 college application seasons, as several Court actions in 2021 suggested that it would overturn *Roe* in 2022 (see Section 2.2). This approach captures changes in the application behavior of women who were making choices in 2021 in anticipation of the Court's decision, as nearly all of the 13 states with an abortion ban passed their laws prior to the 2021-22 college application season,[4] as well as in 2022 after the

[4]West Virginia is the one exception. The state signed a total ban into law in September 2022.

Court's decision was handed down. I extend the analysis to the 2023-24 application season in Section 1.9.

I find that, relative to their high-achieving male peers, high-achieving female applicants were 1.2 percentage points less likely to apply to one of the 24 states with an abortion law in the 2021-22 and 2022-23 application seasons. This estimate is driven by changes in women's application behavior to the subset of 13 states with an enforceable abortion (-2.7 percentage points), with the effect size increasing from -1.4 percentage points in 2021 to -3.7 percentage points in 2022. Further, women from one of the 26 states and D.C. with no abortion restriction had a larger response to abortion bans than their peers from states with an abortion law. Women from states without a ban were less likely to apply to a state with a ban by 1.3 percentage points in 2021 and 4.5 percentage points in 2022. Treatment effects persisted in 2023. On the other hand, there was no effect of *Dobbs* on whether applicants applied out-of-state to a state with no abortion restriction. To my knowledge, these results are the first piece of evidence about the effect of state reproductive rights laws on women's college choices.

I also explore two mechanisms – concerns about access to reproductive health care and political identity – that may drive changes in the application behavior of women from states with no abortion restriction. These two mechanisms are highly correlated (see Section 1.2). I find that these women live too far from states that are landlocked by abortion bans to be sensitive to the travel distance to home to access care, but there is suggestive evidence that they are sensitive to how far schools are from the nearest abortion provider. Further, relative to women from less liberal counties, women from the most liberal counties in states with no abortion restriction were even less likely to apply to a state with an abortion ban compared with their male peers.

These application decisions have important implications for education, as women make up more than half of the college student population (U.S. Department of Education, 2023b). With fewer high-achieving women applying out-of-state to states with bans, colleges and universities may need to either accept lower-performing women from out-of-state, or admit more women from in-state to maintain their gender diversity balance. However, fewer out-of-state students would reduce the social and cultural diversity of learning environments that enhances human capital, and colleges and universities would lose a source of revenue (Mixon and Hsing, 1994). Fewer out-of-state students would also have downstream effects on communities and labor markets. Students often remain in the area where they attended college (Groen, 2004, Groen and White, 2004; Hickman, 2009, Huffman and Quigley, 2002, Winters, 2011a, Winters, 2011b), and larger shares of college graduates drive population growth by raising productivity and wages (Moretti, 2004, Rauch, 1993, Shapiro, 2006) and improving quality of life through greater consumer amenities and tolerance of others (Diamond, 2016, Florida, 2002, Shapiro, 2006).

This research contributes to and connects several different literatures. First, this research extends the literature on college choice. Prospective college students weigh a variety of institution-level characteristics, such as cost, distance, and quality, when deciding whether to cross state lines for college (Adkisson and Peach, 2008, Kyung, 1996, Baryla and Dotterweich, 2006, Mixon and Hsing, 1994, Tuckman, 1970). Geographic amenities such as a favorable climate (Morgan, 1983, Dotzel, 2017) and local economic conditions (Baryla Jr and Dotterweich, 2001, Hsing and Mixon, 1996, Morgan, 1983) also play a role in this decision. To my knowledge, this paper is the first to examine the role of reproductive rights laws in the college choice process.

Second, this project contributes to research on access to reproductive care and women's social and economic outcomes. A body of literature explores the effect of abortion and contraception on fertility (Levine et al., 1999, C. K. Myers, 2017), educational attainment (Angrist and Evans, 1998, Jones, 2021), educational expectations (Jones, 2021), labor force participation (Bailey, 2006, Goldin and Katz, 2002, Kalist, 2004), career outcomes (Jones, 2021), and earnings (Abboud, 2019, Jones, 2021). This paper connects this body of work to the literature on college choice.

Third, this research contributes to the literature on political identity and partisan sorting. Studies find that partisan affiliation affects behavior in economic realms, such as consumer spending (Gerber and Huber, 2009), household financial (Bonaparte, Alok Kumar, and Page, 2017; Meeuwis et al., 2018), and real estate decisions (McCartney, Orellana, and Zhang, 2021), as well as behavior in the labor market (Colonnelli, Neto, and Teso, 2022; Fos, Kempf, and Tsoutsoura, 2022). I build on this literature by exploring the impact of political identity, which is correlated with views on abortion, on women's college location choices.

Lastly, this work contributes to the broader literature on the consequences of state abortion bans. A growing body of work discusses implications for the health care system, including medical education and training (Lambert, Horvath, and Casas, 2023; Orgera, Mahmood, and Grover, 2023; Stephenson-Famy et al., 2023), patient care (Grossman et al., 2023), health inequity (Kimport, 2022), and fertility (Dench, Pineda-Torres, and C. Myers, 2024). On the other hand, abortion protections correlate with stronger indicators of economic well-being (Banerjee, 2023) and job recruitment in Democratic-leaning states and female-dominated jobs (Adrjan et al., 2023).

In the next section, I introduce the conceptual framework for this paper. In Section 2.2, I discuss the history of abortion access in the United States and the current state landscape. I describe the data in Section 3.2, trends in out-of-state college applications and enrollment in Section 2.4, and the empirical strategy in Section 2.5. Section 1.7 reports results, Section 1.8 explores mechanisms, Section 1.9 performs robustness checks and examines treatment effect persistence, and Section 3.4 concludes.

1.2 Conceptual Framework

This papers uses the theoretical framework developed by Sjaastad, 1962, which views migration as an investment in human capital. A student seeks to maximize her expected utility and will migrate if she expects the net present value of moving to exceed the cost. She will thus move if another location offers her a higher future utility than her current location, provided that this difference in utility compensates for the costs of moving. Since preferences vary, students receive different net benefits from a given location and will make different migration choices.

Based on this framework, I hypothesize that state laws on reproductive rights will impact the utility of college applicants. Surveys from the summer and fall following the *Dobbs* decision provide compelling evidence for this hypothesis. According to a BestColleges survey from July 2022, 39 percent (43 percent) of prospective (current) undergraduate students said that the decision will impact where they attend college (Bryant, 2022). Polling from the Lumina Foundation-Gallup 2022 State of Higher Education Study found that state reproductive rights laws were similarly important to 60 percent (72 percent) of unenrolled (enrolled) college adults.

Further, I hypothesize that these laws will affect female college applicants more than male college applicants, causing a differential impact on women's college application behavior. According to the Lumina-Gallup survey, 62 percent (76 percent) of unenrolled (enrolled) women reported that reproductive health laws would affect their college decisions, compared with smaller majorities of unenrolled (57 percent) and enrolled men (68 percent) (Marken and Hrynowski, 2023). These findings persist. The Lumina- Gallup 2024 study found that 89 percent (71 percent) of women (men) would rather attend college in a state with greater access to reproductive healthcare (Marken and Hrynowski, 2024).

Next, I posit that, in response to state reproductive rights laws, women may change where they apply to college for two reasons: (1) concerns about access to reproductive health care and (2) political identity. First, women value access to reproductive health care services to exercise bodily autonomy (Adler et al., 2023). At the same time, partisan affiliation is an important part of an individual's social identity (Green, Palmquist, and Schickler, 2002) that may affect where she chooses to live (Tam Cho, Gimpel, and Hui, 2013; Kinsella, McTague, and Raleigh, 2015; Lang and Pearson-Merkowitz, 2015; McDonald, 2011; A. S. Myers, 2013). Notably, these two mechanisms are highly correlated. According to a 2023 Gallup poll, 84 percent of Democrats identified as pro-choice, compared with just 21 percent of Republicans (Saad, 2023). Further, a 2023 study by Arts & Science Group LLC found that 31 percent (28 percent) of liberal (conservative) students ruled out schools due to state social policies, compared with just 12 percent of students who reported not knowing their party affiliation

(Goebel et al., 2023).

1.3 Policy Context

Prior to 1973, access to abortion care was determined by state-level policy. Five states and the District of Columbia provided legal access to abortion, and 13 states adopted laws that made abortion legal in limited cases. In the remaining states, abortion was generally prohibited except to save a woman's life (C. K. Myers, 2022).

The Supreme Court's landmark decision in *Roe* (1973) held that a woman has the constitutional right to make decisions about her reproductive life, including whether to continue or end her pregnancy before viability.[5] This decision legalized abortion nationwide, and the Court reaffirmed the core holding of *Roe* in *Planned Parenthood of Southeastern Pennsylvania v. Casey* (1992). In the mid-2000s, states began to pass new laws to restrict access to abortion, including late gestational age bans after 20 or 22 weeks[6]; early gestational age bans after 6, 8, 12, or 15 weeks; trigger laws that would ban abortion if the Court overturned *Roe*; and total abortion bans. Most late gestational age bans took effect under *Roe*, but early gestational age and total bans were blocked by court orders.[7,8]

The change in the composition of the Supreme Court and its shift to a conservative majority set the stage for its decision in *Dobbs*. In 2018, then-President Donald Trump appointed Brett Kavanaugh to the Court. This appointment made the Court more conservative than at any other time in modern history with two distinct blocs – five conservatives and four liberals (Liptak, 2018). Notably, more restrictive state abortion bans – including early gestational age bans, trigger laws, and total bans – picked up steam after this appointment,

[5]The current standard for fetal viability is 22-24 weeks (Cha and Roubein, 2021).

[6]There are two main ways to calculate gestational age: postfertilization, which is from the date of conception, and last menstrual period (LMP), which is from the start of the most recent menstrual period. For example, a ban after 20 weeks postfertilization is equivalent to a ban after 22 weeks LMP.

[7]Texas is an exception. The state's 6-week ban took effect in 2021.

[8]This paper focuses on state abortion laws, but states have also enacted targeted restrictions on abortion providers, limitations on insurance coverage of abortion, and limitations on medication abortion.

as Justice Kavanaugh would become, as many predicted, the fifth vote to uphold new limits on abortion (Tavernise, 2019). On June 29, 2020, the Court decided the first major abortion case since the shift in its balance of power. In *June Medical Services LLC v. Russo*, the Court struck down a Louisiana law that required doctors who preform abortions to have admitting privileges at nearby hospitals. Chief Justice John G. Roberts added his fifth vote to those of the Court's four-member liberal bloc, halting anti-abortion momentum (Tavernise and Dias, 2020). However, after Justice Ruth Bader Ginsburg died on September 18, 2020, she was replaced by Justice Amy Coney Barrett. Barrett, who had previously spoken out against abortion, created a six-justice conservative majority on the Court (Liptak, 2021) to make it the most conservative it had been since the 1930s (Biskupic, 2020).

Three major developments in 2021 then signaled the likelihood that the Court would overturn *Roe* – or, at a minimum, further restrict access to abortion – in 2022. First, on May 17, 2021, the Court agreed to hear the *Dobbs* case, which concerned a Mississippi law that banned abortion after 15 weeks of pregnancy. To many, the Court's decision to hear the case signaled the willingness of the justices to revisit – and potentially overturn – its precedent (Liptak, 2021). Second, on September 1, 2021, the Court denied an emergency request to block Texas' 6-week abortion ban, fueling the "hopes of abortion opponents and fears of abortion rights advocates" as the court took up the *Dobbs* case (Liptak, Goodman, and Tavernise, 2021). Texas' law became the most restrictive abortion measure in the United States at that time.[9] Third, on December 1, 2021, the Court heard oral arguments in the *Dobbs* case. Press coverage of the hearings suggested that the majority would uphold the Mississippi law, which banned abortion two months before the viability line determined by *Roe* (Phillips, 2021; Vogue, 2021). Six months later, on May 2, 2022, *Politico* published an initial draft majority opinion for the *Dobbs* case, which was written by Justice Samuel Alito and voted to strike down *Roe* (Gerstein and Ward, 2022). On June 24, 2022, the Court

[9]The Texas Legislature drafted the law to make it difficult to challenge in court. According to *The New York Times*, "Usually, a lawsuit seeking to block a law because it is unconstitutional would name state officials as defendants. However, the Texas law, which makes no exceptions for pregnancies resulting from incest or rape, bars state officials from enforcing it and instead deputizes private individuals to sue anyone who performs the procedure or "aids and abets" it" (Liptak, Goodman, and Tavernise, 2021).

officially decided the case in a 6-3 decision that eliminated the federal constitutional right to abortion. Dozens of state abortion restrictions subsequently went into effect.

In this paper, I group states into three categories based on their abortion policy by the 2022-23 college application season, which was the first application season after the Court's decision in *Dobbs*. The first category includes the 24 states with laws that restricted access to abortion (i.e., an early gestational age ban) and/or banned abortion (i.e., a trigger law or total ban). Some of these laws remained blocked by court orders in the wake of *Dobbs*. The second category includes the 13 of the 24 states where a complete abortion ban was enforced. The third category includes the 26 states and D.C. that did not have a law restricting access to or banning abortion.[10] I subsequently refer to these three categories as states with an abortion law, states with an abortion ban, and states with no abortion restriction. See Table 1.1 and Figure 1.1 for a complete list of states in each category, and Appendix Section A.0.3 for a detailed analysis of state abortion restrictions.

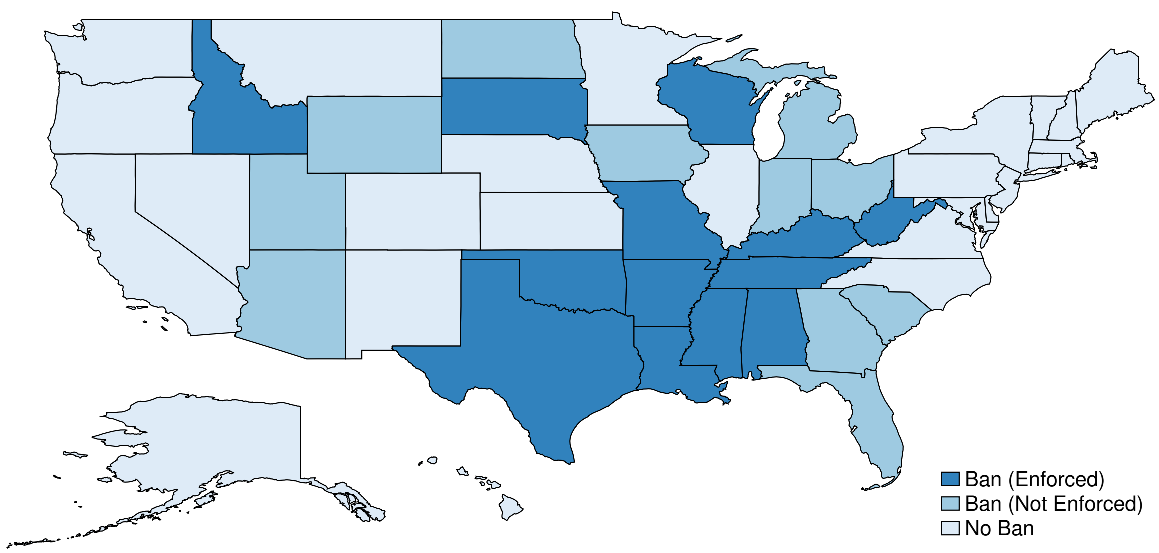


Figure 1.1: *Map of State Abortion Policy in the 2022 College Application Season*

Notes: This figure categorizes the 50 U.S. states and D.C. based on their abortion restrictions in the 2022-23 college application season. See Table 1.1 for more details.

[10]Two states in the "no abortion restriction" category – Montana and Nebraska – had late gestational age bans starting at 20 weeks that were in effect prior to *Dobbs*.

1.4 Data

I use college application data from the Common App from the 2016-17 through 2022-23 college application seasons for the main analysis.[11,12] I interchangeably refer to each application season by the full year (e.g., the 2016-2017 application season) or the fall of that year (e.g., 2016). The data include the de-identified list of schools that applicants applied to via the Common App platform, as well as a set of observable applicant characteristics. I start by identifying a sample of 6,350,153 17- and 18-year-old college applicants who were high school seniors and lived in one of the 50 U.S. states or the District of Columbia.[13] To identify high-achieving applicants, I use the cut score for the 90th percentile of SAT/ACT test-takers in the sample in 2016 to create a subsample of 597,755 applicants.[14] I combine these data with zip-code level covariates from the American Community Survey 5-Year Estimates.

Next, I match each applicant's college applications to the corresponding state abortion policies (abortion law, abortion ban, no abortion restriction) using the coding scheme in Section 2.2. I also identify the state abortion policy in each applicant's state of residence. I then construct three primary binary outcomes – whether an applicant applied to a state with an abortion law, an abortion ban, or no abortion restriction – and collapse the data to the applicant-level.

[11] I exclude three additional years of available Common App data from the 2013-14, 2014-15, and 2015-16 application seasons for two reasons. First, high school class year, which I use to define the sample, is unavailable in those years. Second, the SAT, which I use to define high-achieving applicants in the sample, changed its format in spring 2016.

[12] I bring in data from the 2023-24 college application season in Section 1.9.2.

[13] By construction, applicants who were not high school seniors, 17- or 18-years-old (which cover 98 percent of all observations), and from one of the 50 U.S. states or the District of Columbia are excluded from the sample. For applicants who applied in more than one application season, I use their first observation.

[14] Following Hoxby and Avery, 2013, I define high-achieving students as those who score at or above the 90th percentile on the ACT or SAT. The authors also restrict their definition of high-achieving students to those with a high school grade point average of A- or above, but they note that this criterion hardly matters after conditioning on having test scores in the top 10 percent. I use the cut score for the 90th percentile on the SAT/ACT in 2016 (a score of 1460) in all application seasons to avoid selection into test-taking after institutions of higher education announced test-optional policies in spring 2020. The proportion of applicants in the data who reported an SAT/ACT score to the Common App declined from an average of 72 percent prior to 2020 to an average of 47 percent post-2020.

Table 1.2 summarizes applicant covariates. Of the full sample, 69 percent of applicants were 17-years-old and 57 percent were women. Fifty-three percent identified as White, 12 percent as Black, 17 percent as Hispanic, and 10 percent as Asian. A quarter of applicants were eligible for or received a Common App application fee waiver. Nearly the entire sample consists of U.S. citizens who attended high school in their home state. Nearly 70 percent of applicants were from states with no abortion restriction and approximately one third were from states with an abortion law, of which 11 percent were from states with an abortion ban. Compared with the full sample, high-achieving applicants were less likely to be Black or Hispanic, fee-waiver eligible, or first generation. In addition, high-achieving applicants applied to more schools, were more likely to apply out-of-state, and sent a larger proportion of their applications out-of-state.[15] These application sending patterns suggest that high-achieving applicants are more likely to be impacted by social policy in other states because they cast a wider net.

[15]Further, Figure A.9 shows that the likelihood of applying out-of-state increases with SAT/ACT score.

Table 1.2: *Summary Statistics*

	Full Sample	High-Achieving Applicants
17-years-old	0.69	0.71
18-years-old	0.31	0.29
HS senior	1.00	1.00
Female	0.57	0.45
White	0.53	0.50
Black or African American	0.12	0.02
Hispanic or Latino	0.17	0.07
Asian	0.10	0.28
Two or More Races	0.05	0.06
Unknown Race	0.03	0.07
Fee waiver eligible	0.26	0.08
First generation	0.32	0.08
Attends high school in home state	0.97	0.97
No Restriction Home State	0.68	0.68
Abortion Ban Home State	0.11	0.14
Abortion Law Home State	0.32	0.32
Total number of applications	5.43	7.70
Applied out-of-state	0.71	0.93
Number of states applied out-of-state	2.42	4.75
Prop. of apps sent out-of-state	0.49	0.72
Observations	6350153	597755

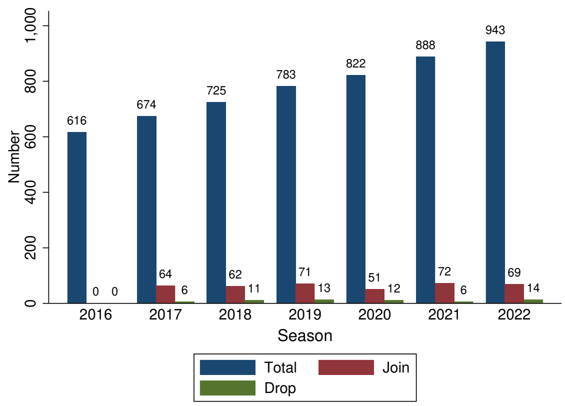
Notes: This table describes the sample of applicants who applied to college using the Common App in application seasons 2016-17 through 2022-23. High-achieving applicants scored at or above the 90th percentile on the SAT/ACT in the sample in 2016.

Between 2016 and 2022, applicants in the sample sent nearly 34.5 million applications to 1,000 schools using the Common App platform, which is limited to public and private not-for-profit four-year institutions of higher education in the United States.[16] Applications to schools that were not sent through the Common App are not captured in the data. For context, public and private not-for-profit four-year colleges and universities received a total of 12,033,078 applications from first-time, degree-seeking undergraduate students for fall 2021 enrollment (U.S. Department of Education, 2023a). Applicants in the sample sent

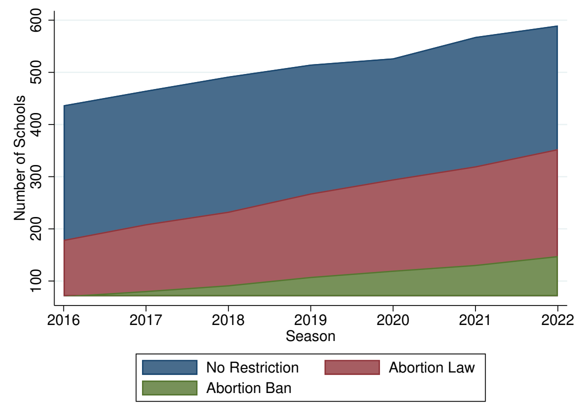
[16] I drop two schools that have an "NA" value for state.

5,440,009 applications (45 percent) in the corresponding application season (2020-2021), making the platform the most comprehensive centralized source of application data.

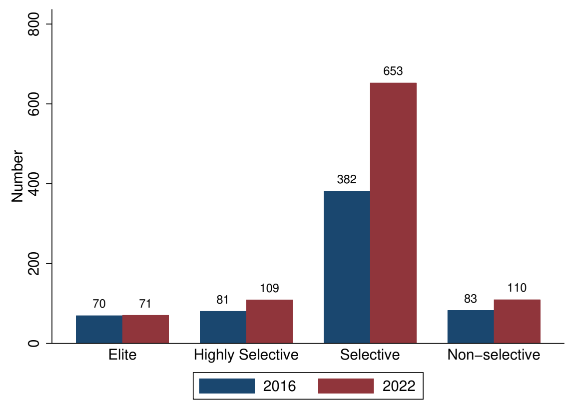
Notably, applicants' school choice sets in the Common App increased from 616 schools in 2016 to 943 schools in 2022 (Figure 1.2a) as Common App membership grew at a similar rate in states with an abortion ban, an abortion law, and no abortion restriction (Figure 1.2b). The growth in the number of Common App members was largely driven by schools that are among the least selective, as shown in Figure 1.2c. Following prior work (Chetty, Friedman, et al., 2017; Deming, Goldin, et al., 2015), I use data from the Barron's 2009 index to classify schools into four tiers based on their selectivity: elite (Ivy-Plus, which includes the Ivy League plus Stanford, MIT, Chicago, and Duke, and Barron's Tier 1 excluding the Ivy-Plus), highly selective (Barron's Tier 2), selective (Barron's Tiers 3-5), and non-selective (Barron's Tier 9, specialized schools, and schools that are otherwise not included in the selectivity index). As the number of selective schools grew, the number of elite schools remained nearly constant over time alongside a small increase in the number of highly selective schools. Further, as the number of Common App members increased, so did the number of applications that applicants sent out-of-state and the number of states that applicants applied to out-of-state (Figure 1.3).



(a)



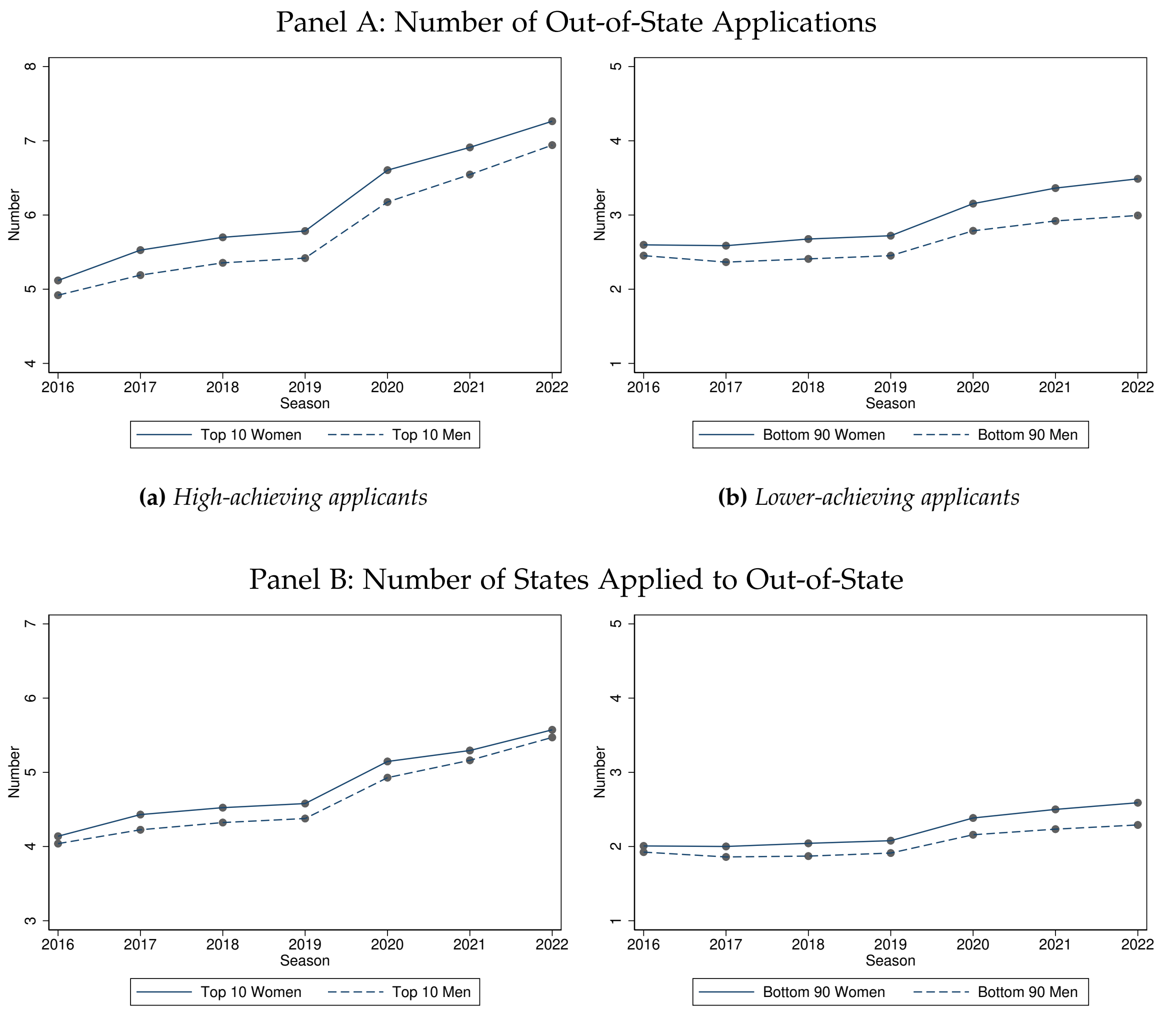
(b)



(c)

Figure 1.2: *Common App Schools*

Notes: This figure uses college application data from seasons 2016-17 through 2022-23. In Figure 1.2a, five schools that exited in one season re-entered in a later season. See Table 1.1 for state categorization scheme used in Figure 1.2b and text for details on the selectivity categories in Figure 1.2c.



(a) *High-achieving applicants* **(b)** *Lower-achieving applicants*

(c) *High-achieving applicants* **(d)** *Lower-achieving applicants*

Figure 1.3: *Out-of-State Application Patterns Over Time*

Notes: This figure shows the number of applications sent out-of-state and the number of states to which applicants applied out-of-state over time using college application data from seasons 2016-17 through 2022-23. High-achieving applicants scored in the top 10 percent of SAT/ACT test-takers in the sample in 2016.

1.5 Trends in Out-of-State Applications and Enrollment

To motivate the analysis, I separately examine institution-level trends in out-of-state applications using data from the Common App, and institution-level trends in out-of-state enrollment using publicly available data on residence and migration from the Integrated Postsecondary Education Data System (IPEDS). I constrain the window of analysis to the pre-period, and focus on college enrollment in the fall of 2016, 2018, and 2020 because IPEDS requires that institutions provide data on residence and migration only in even-numbered years. I identify 569 Common App members with available IPEDS data in all three years.[17] For out-of-state applications, I focus on applicants who met the sample criteria (defined in Section 3.2) and applied to college in 2016 (a proxy for 2015, which is excluded from the sample – see footnote 11), 2017, and 2019 for enrollment in the fall of 2016, 2018, and 2020, respectively. These restrictions identify a sample of 593 schools.

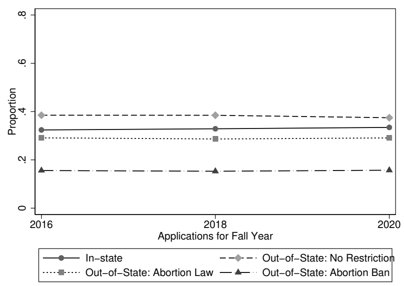
I start by calculating the proportion of each school's applications that were sent from in-state, out-of-state from a state with an abortion ban, out-of-state from a state with an abortion law, and out-of-state from a state with no abortion restriction. I similarly calculate the proportions of each school's student population that enrolled from in-state and out-of-state by abortion policy. (By construction, the proportion of out-of-state applicants and enrolled students from a state with an abortion law encompasses those from a state with an abortion ban.) I then take the average for each outcome across states with an abortion ban, an abortion law, or no restriction.

Figure 1.4 shows that there is a sizable amount of student migration across states with an abortion ban, an abortion law, and no restriction. For schools in states with an abortion ban, 40 percent of applicants and nearly a quarter of enrolled students were from states with no abortion restriction. Notably, a greater proportion of enrolled students in these schools were from states with no abortion restriction than from states with an abortion law or ban. Similarly, for schools in states with an abortion law, approximately 35 percent of applicants and a quarter of enrolled students were from states with no abortion restriction. For schools in states with no restrictions, approximately 10 percent of applicants and 8 percent of enrolled students were from states with an abortion law, and approximately 10

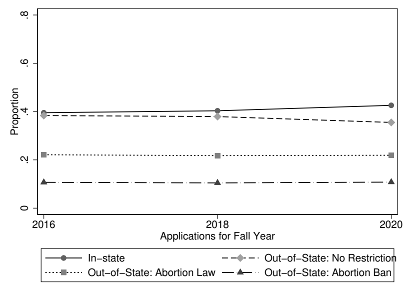
[17] I focus on the residence and migration of first-time degree/certificate-seeking undergraduate students who graduated from high school in the past 12 months across.

percent of applicants and 4 percent of enrolled students were from states with an abortion ban. The majority of out-of-state applicants and enrolled students were from other states with no abortion restriction.

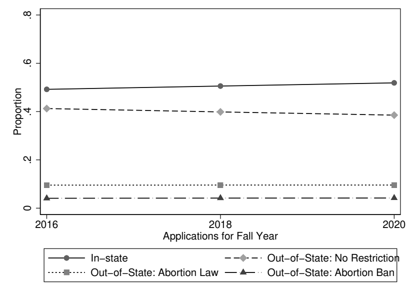
Panel A: Applications



(a) *Abortion Ban States*

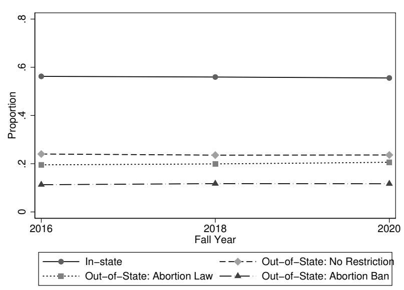


(b) *Abortion Law States*

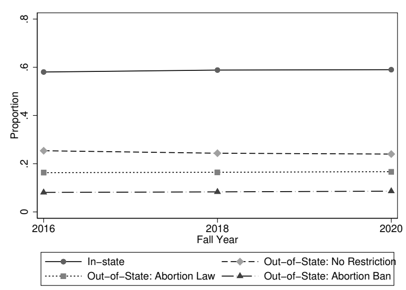


(c) *No Restriction States*

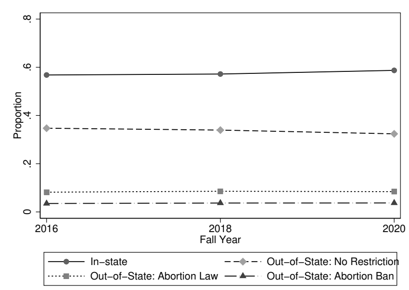
Panel B: Enrollment



(d) *Abortion Ban States*



(e) *Abortion Law States*



(f) *No Restriction States*

Figure 1.4: *Out-of-State Application and Enrollment Patterns Prior to Dobbs*

Notes: Panel A uses institution-level data on college applications from the Common App. Applications are from applicants who met the sample criteria (see Section 3.2) and applied to college in the 2016-17, 2017-19, and 2019-20 application seasons for enrollment the following fall (2016, 2018, and 2020). Application season 2016-17 is a proxy for 2015-16. See Table 1.1 for state categorization scheme and Section 2.4 for more details on sample construction. Panel B uses institution-level data on residence and migration of first-time freshmen from the Integrated Postsecondary Education Data System from 2016-2020 to plot the average proportion of schools' enrolled students by state of residence.

Importantly, because the Common App does not capture all of the applications that a school receives, out-of-state applications by state of residence may be over or understated.

For example, for schools in states with an abortion ban, the proportion of applicants from states with no restrictions would be inflated if non-Common App applications were disproportionately from students in other states with an abortion ban. This feature would attenuate the difference in the proportion of students from states with no abortion restriction who applied to (40 percent) and enrolled in (nearly 25 percent) schools in states with an abortion ban. Nonetheless, these trends suggest that changes in state social policy may have a nontrivial effect on migration to schools in states with an abortion ban or law, given that a significant portion of the students who applied to and enrolled in these schools prior to *Dobbs* were from states with no abortion restriction. On the other hand, changes in state social policy may have less of an effect on migration to schools in states with no abortion restriction, as a smaller portion of the students who applied to and enrolled in these schools were from states with an abortion ban or law.

1.6 Empirical Framework

1.6.1 Identification Strategy

The naïve approach to estimating the causal impact of abortion laws on application behavior would be to compare differences in outcomes between female and male applicants after the *Dobbs* decision. This relationship can be expressed with the following reduced-form equation

$$Y_i = \alpha + \beta Female_i + \mathbf{X}_i + \epsilon_i, \tag{1.1}$$

where Y_i is a measure of an individual's application behavior and $Female_i$ is an indicator for applicant sex.[18] To obtain an unbiased estimate of β, the regression must include all of the elements of $\mathbf{X}_i$ that are correlated with an applicant's gender and her application behavior. However, in practice, many of these covariates are unavailable, and unobserved differences across gender that also induce variation in application behavior would bias the estimate.

Instead, I use a difference-in-differences strategy to compare the change in female

[18] For the years in this analysis, the Common App uses a binary indicator for applicant sex.

applicants' behavior to the change in male applicants' behavior before and after *Dobbs*. The key identifying assumption is that any relative change in application behavior is attributable to state abortion restrictions. I estimate

$$Y_i = \alpha + \beta(Female_i \times After_t) + \gamma Female_i + \delta X_i + \eta_{st} + \nu_{sg} + \epsilon_i, \tag{1.2}$$

where Y_i is a binary indicator for one of three application outcomes (applied to a state with an abortion ban, law, or no restriction). $After_t$ equal to 1 in the 2021 and 2022 application seasons. X_i is a vector of applicant-level covariates, including age, race/ethnicity, Common App fee waiver eligibility, whether the applicant was first generation, SAT/ACT score, whether the applicant attended high school in her home state, high school type, median household income of the applicant's zip code adjusted to 2021 dollars, and percent adults with at least a BA degree in the applicant's zip code. I include state-by-season (η_{st}) and state-by-gender (ν_{sg}) fixed effects to account for state-specific shocks and differences in state of residence by gender.[19] I cluster standard errors by applicant state of residence. I also estimate an event study version of equation (1.2) using

$$Y_i = \alpha + \sum_{\substack{t=-5 \\ t\neq-1}}^{1} \beta_t(Female_i \times \mathbf{1}[\tau = t]) + \gamma Female_i + \delta X_i + \eta_{st} + \nu_{sg} + \epsilon_i, \tag{1.3}$$

which separately estimates effects for every year using indicators for gender, year, and their interactions, with 2020 as the reference year. The coefficients on 2021 and 2022 estimate separate treatment effects by year, whereas the estimate of β from equation (1.2) averages treatment effects across both years. Near-zero coefficients for 2016-2019 provide evidence of parallel trends in the pre-period.

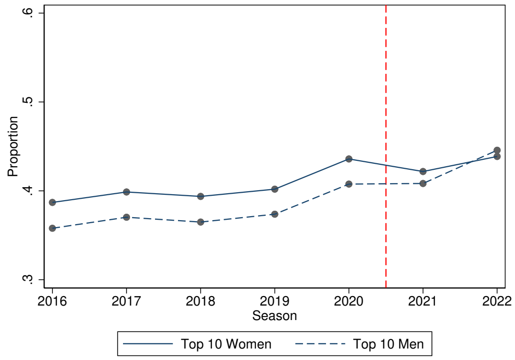
[19] I also estimate a specification that includes include career-by-season and career-by-gender fixed effects to account for academic program-specific shocks and differences in academic programs by gender, using applicants' career interests as a proxy for college major. The Common App does not systematically collect information on applicants' college majors, but applicants can choose from a list of 50 careers. I group these careers into nine occupation categories (plus "other", "non-employment", and "undecided"). However, as students are deciding college and career jointly, it is possible that career decisions are also impacted by state abortion bans. I therefore exclude career-related fixed effects from my preferred specification. Estimates from specifications with these fixed effects are similar to those without, with slight attenuation in 2022.

1.6.2 Evidence on Identifying Assumptions

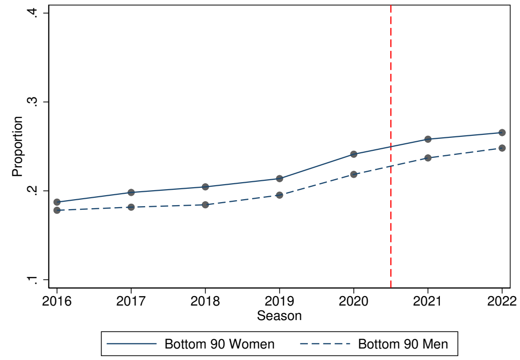
My identification relies on the assumption that women's college application behavior would have trended similarly to men's in the absence of *Dobbs*, where the treatment period is defined as the 2021 and 2022 college application seasons. Defining treatment in this way captures changes in the application behavior of women who were making choices in 2021 in anticipation of the Court's decision, as well as in 2022 after the decision was handed down (see Section 2.2). To assess the plausibility of parallel trends and treatment timing, I examine observed trends in women and men's college application decisions over time.

Figure 1.5a shows that the proportion of high-achieving female and male applicants who applied to a state with an abortion ban tracked each other closely from 2016 to 2020, with a constant gap of roughly 3 percentage points over this period. In 2021, the difference narrows by half, as the proportion of female applicants decreases by approximately 1.5 percentage points. In 2022, the proportion of female applicants drops below the proportion of male applicants by nearly 1 percentage point. Figure 1.5c similarly shows that the proportion of high-achieving female and male applicants who applied to a state with an abortion law tracked each other closely from 2016 to 2020, with a constant gap of approximately 2-3 percentage points over this period. In 2021 and 2022, the gap widened to approximately 4 percentage points. On the other hand, Figure 1.5e shows that similar proportions of high-achieving women and men applied to a state with no abortion restriction over time. Lower-achieving applicants also trended similarly across all three outcomes (see Figures 1.5b, 1.5d, and 1.5f).

Panel A: Applied to a State with an Abortion Ban

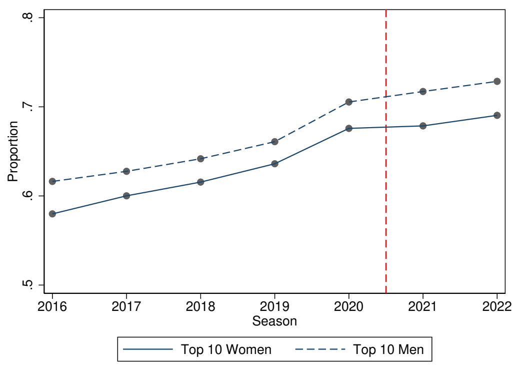


(a) *High-achieving applicants*

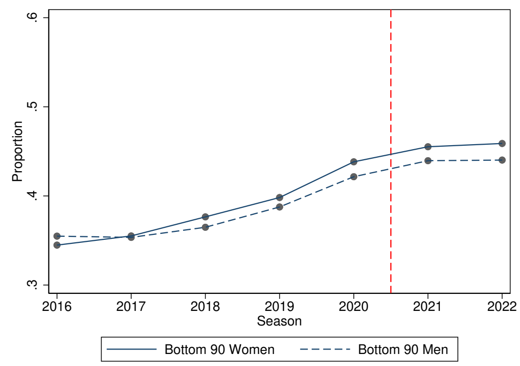


(b) *Lower-achieving applicants*

Panel B: Applied to a State with an Abortion Law

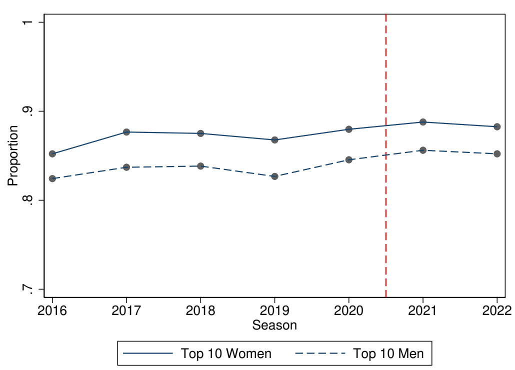


(c) *High-achieving applicants*

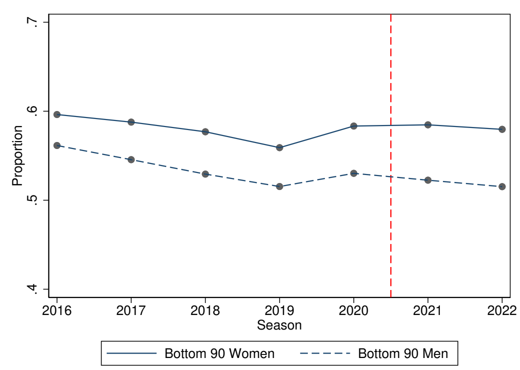


(d) *Lower-achieving applicants*

Panel C: Applied to a State with No Abortion Restriction



(e) *High-achieving applicants*



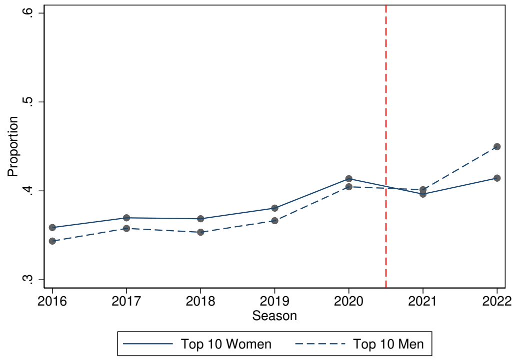
(f) *Lower-achieving applicants*

Figure 1.5: *Application Patterns Over Time, All Applicants*

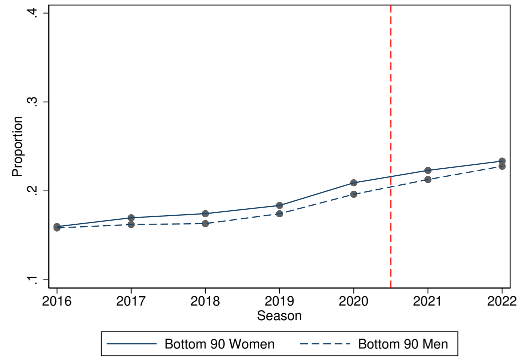
Notes: This figure shows the proportion of applicants who applied to a state with an abortion ban, an abortion law, or no abortion restriction using college application data from seasons 2016-17 through 2022-23. High-achieving applicants scored in the top 10 percent of SAT/ACT test-takers in the sample in 2016. The dashed vertical line indicates the timing of *Dobbs*.

These patterns provide support for common trends in the pre-period as well as suggestive evidence of a *Dobbs* treatment effect on the application behavior of high-achieving women in 2021 and 2022. Changes in the proportion of applicants who applied to a state with an abortion ban were even larger for high-achieving applicants from states with no abortion restriction. Figure 1.6a shows a gap of approximately 1-1.5 percentage points between women and men from these states in 2016 through 2020, after which the proportion of women drop below men by half a percentage point in 2021 and 3.5 percentage points in 2022. Trends in outcomes for applicants from states with an abortion law or ban in Figures 1.7 and 1.8, on the other hand, were more similar over time.

Panel A: Applied to a State with an Abortion Ban

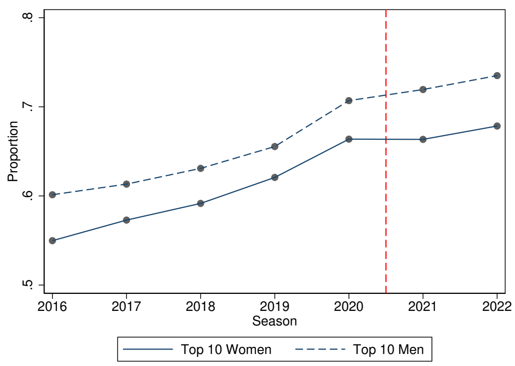


(a) *High-achieving applicants*

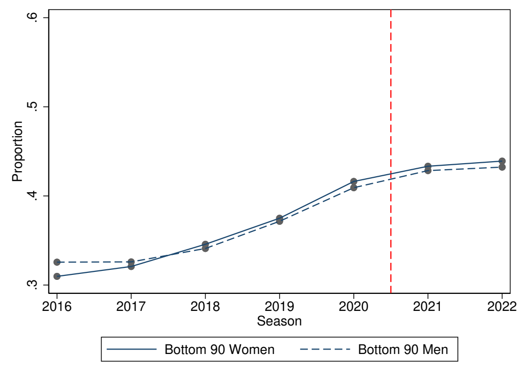


(b) *Lower-achieving applicants*

Panel B: Applied to a State with an Abortion Law

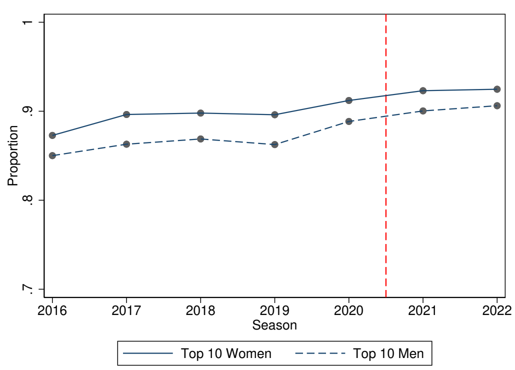


(c) *High-achieving applicants*

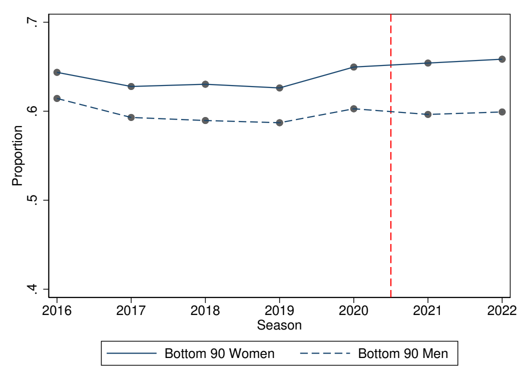


(d) *Lower-achieving applicants*

Panel C: Applied to a State with No Abortion Restriction



(e) *High-achieving applicants*

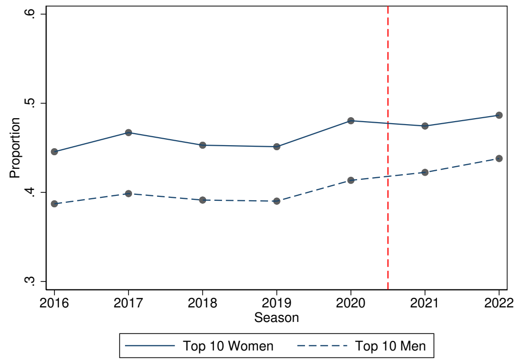


(f) *Lower-achieving applicants*

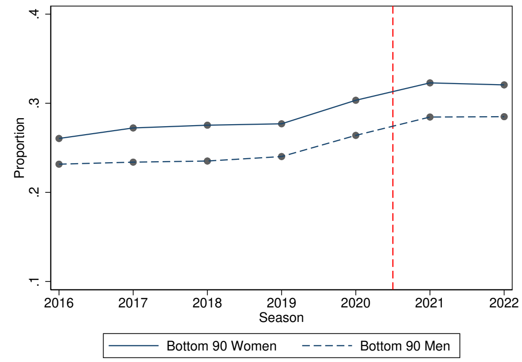
Figure 1.6: *Application Patterns Over Time, Applicants from States with No Abortion Restriction*

Notes: This figure shows the proportion of applicants from states with no abortion restriction who applied to a state with an abortion ban, an abortion law, or no abortion restriction using college application data from seasons 2016-17 through 2022-23. High-achieving applicants scored in the top 10 percent of SAT/ACT test-takers in the sample in 2016. The dashed vertical line indicates the timing of *Dobbs*.

Panel A: Applied to a State with an Abortion Ban

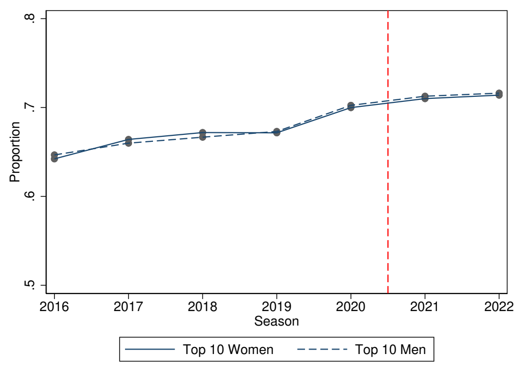


(a) *High-achieving applicants*

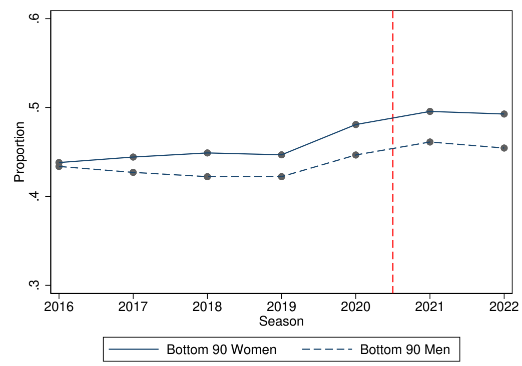


(b) *Lower-achieving applicants*

Panel B: Applied to a State with an Abortion Law

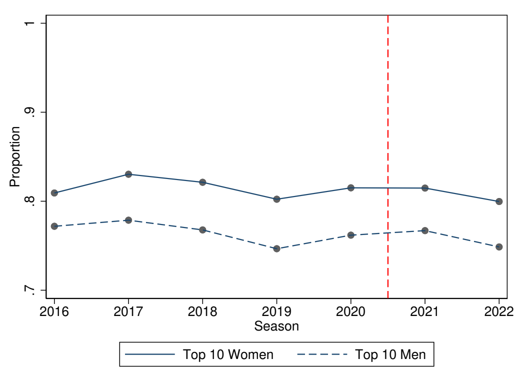


(c) *High-achieving applicants*

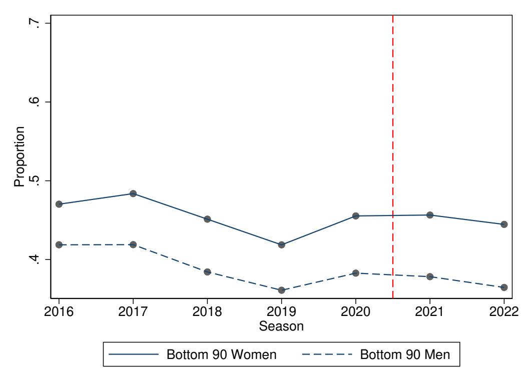


(d) *Lower-achieving applicants*

Panel C: Applied to a State with No Abortion Restriction



(e) *High-achieving applicants*

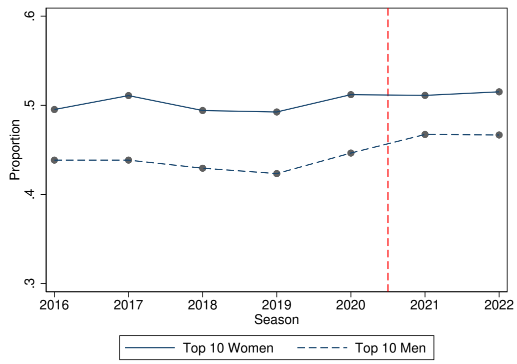


(f) *Lower-achieving applicants*

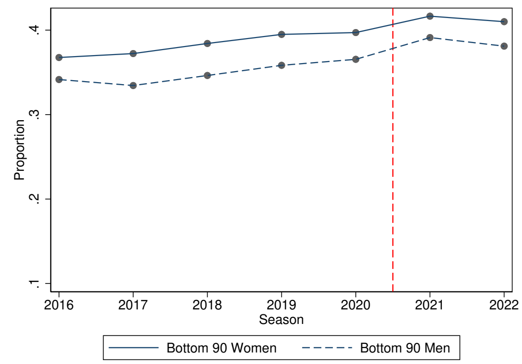
Figure 1.7: *Application Patterns Over Time, Applicants from States with an Abortion Law*

Notes: This figure shows the proportion of applicants from states with an abortion law who applied to a state with an abortion ban, an abortion law, or no abortion restriction using college application data from seasons 2016-17 through 2022-23. High-achieving applicants scored in the top 10 percent of SAT/ACT test-takers in the sample in 2016. The dashed vertical line indicates the timing of *Dobbs*.

Panel A: Applied to a State with an Abortion Ban

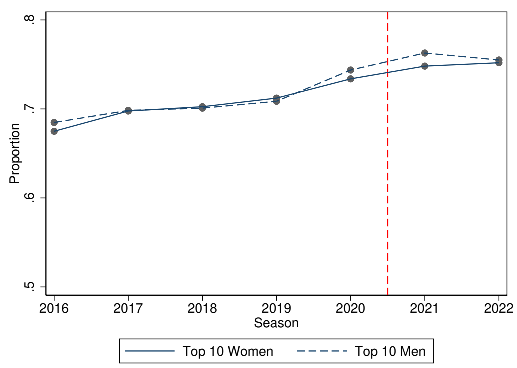


(a) *High-achieving applicants*

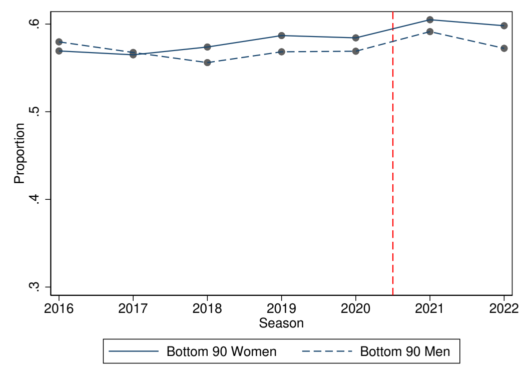


(b) *Lower-achieving applicants*

Panel B: Applied to a State with an Abortion Law

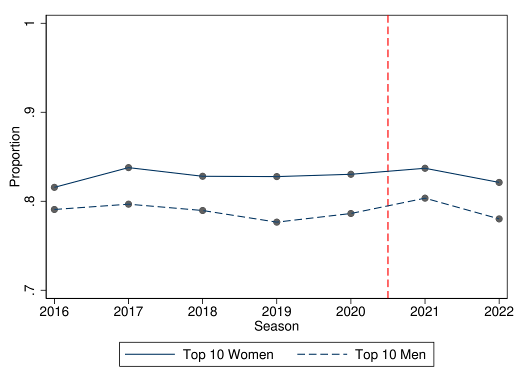


(c) *High-achieving applicants*

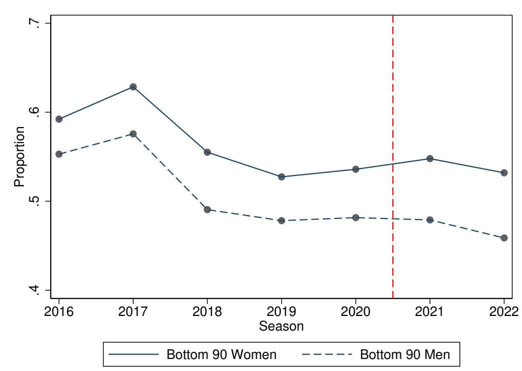


(d) *Lower-achieving applicants*

Panel C: Applied to a State with No Abortion Restriction



(e) *High-achieving applicants*



(f) *Lower-achieving applicants*

Figure 1.8: *Application Patterns Over Time, Applicants from States with an Abortion Ban*

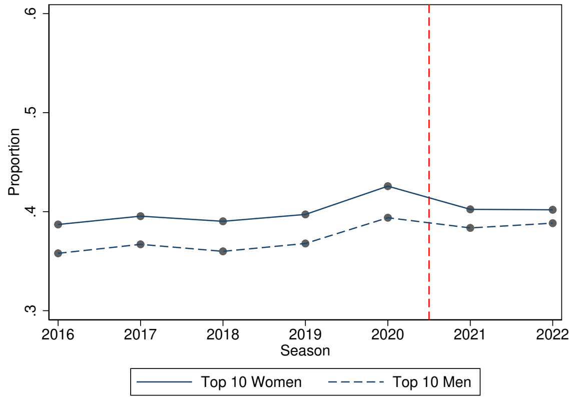
Notes: This figure shows the proportion of applicants from states with an abortion ban who applied to a state with an abortion ban, an abortion law, or no abortion restriction using college application data from seasons 2016-17 through 2022-23. High-achieving applicants scored in the top 10 percent of SAT/ACT test-takers in the sample in 2016. The dashed vertical line indicates the timing of *Dobbs*.

Notably, Figures 1.5 and 1.6 show an upward trend in the proportion of high-achieving women who applied to a state with an abortion ban or law, even as the proportion of women decreased relative to men in 2021 and 2022. These patterns mirror the increase in the number of Common App member institutions in these states in Figure 1.2b. In Figure 1.9, I restrict applications to the subset of Common App members that received at least one application in each season. This subsample includes application data for 590,363 high-achieving applicants applying to the set of 580 schools that were always Common App members between 2016 and 2022.[20] This subsample thus may not capture a state that an applicant applied to if she only applied to a school in the state that was not always a Common App member.[21] However, this subsample has the benefit of estimating changes in application behavior for a constant set of schools over time. Using these data, Figure 1.9 shows that the proportion of high-achieving women who applied to a state with an abortion ban decreased in both treated years relative to the proportion of women in 2020, and the proportion of high-achieving women who applied to a state with an abortion law flattened out.

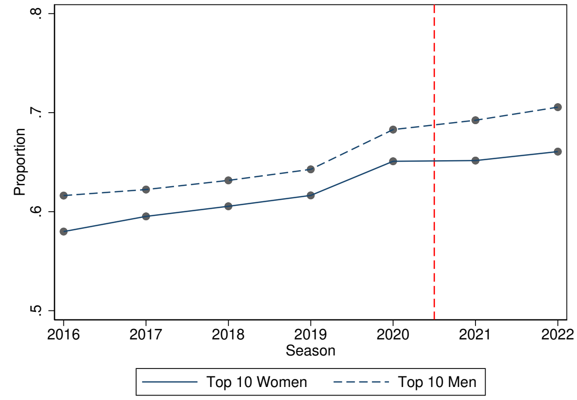
[20]Applicants who only applied to schools that were not Common App members in all application seasons are subsequently dropped.

[21]For example, the modified sample excludes applications to North Dakota and Wyoming. One school in each state joined the Common App after 2016. See Table A.1.

Panel A: High-Achieving Applicants

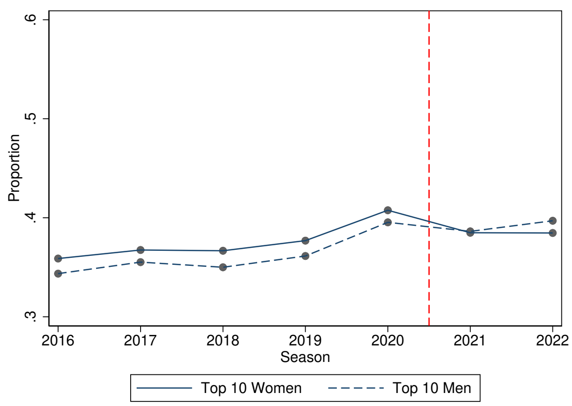


(a) *Applied to a state with an abortion ban*

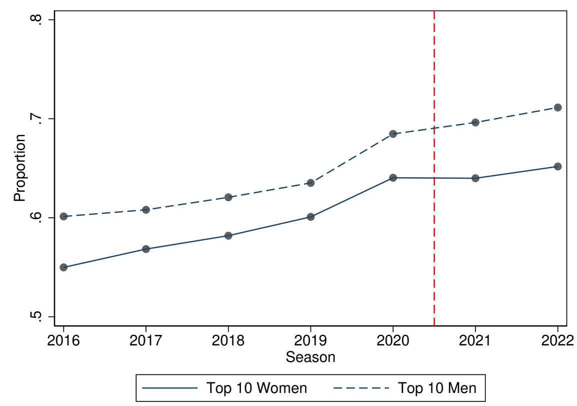


(b) *Applied to a state with an abortion law*

Panel B: High-Achieving Applicants from States with No Abortion Restriction



(c) *Applied to a state with an abortion ban*



(d) *Applied to a state with an abortion law*

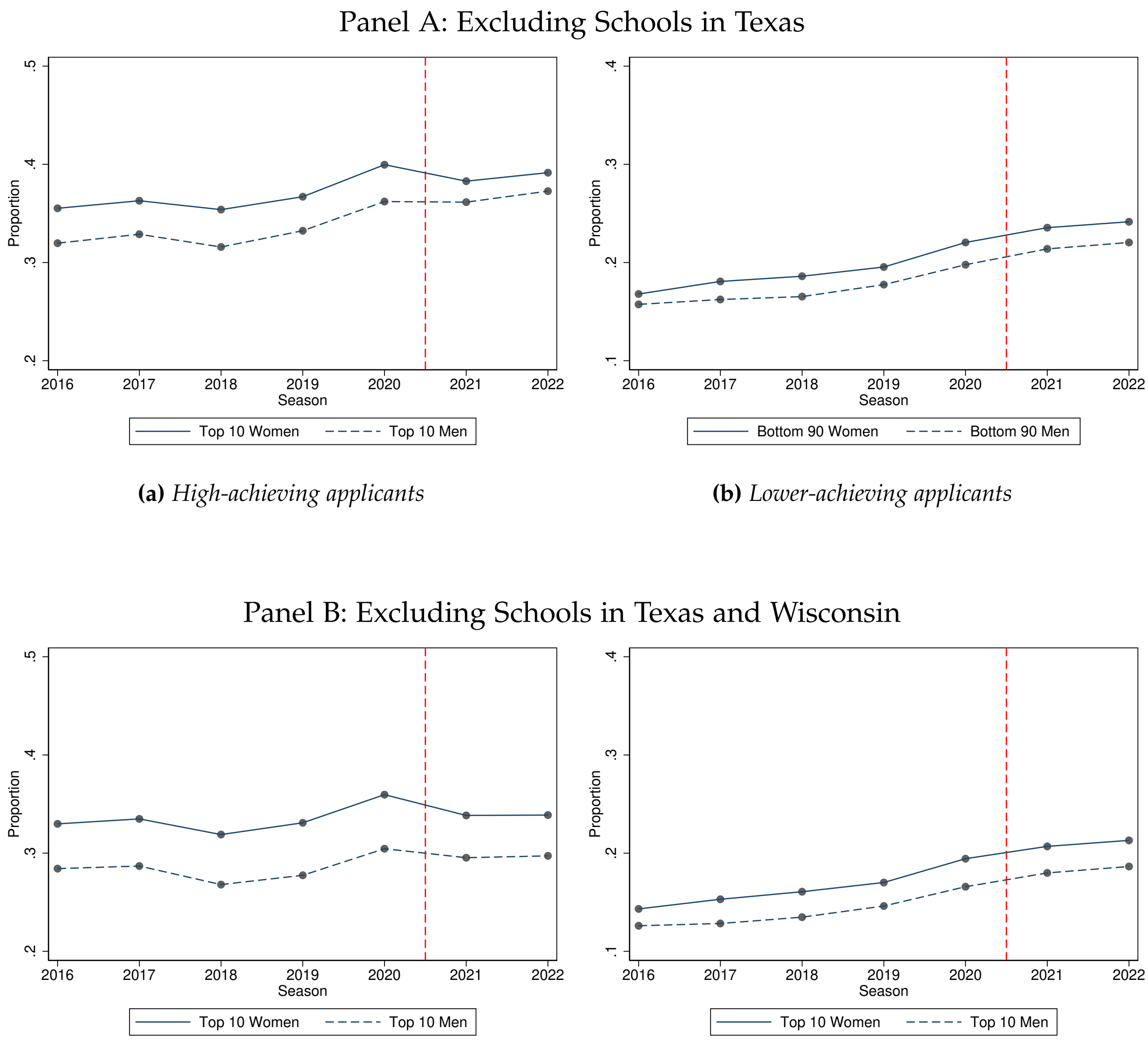
Figure 1.9: *Application Patterns for High-Achieving Applicants to Always Common App Members*

Notes: This figure shows the proportion of high-achieving applicants who applied to a state with an abortion law or an abortion ban using college application data from seasons 2016-17 through 2022-23. The sample is restricted to applications to schools that were Common App members in all application seasons. High-achieving applicants scored in the top 10 percent of SAT/ACT test-takers in the sample in 2016. The dashed vertical line indicates the timing of *Dobbs*.

A close inspection of Figures 1.5a and 1.6a also suggests that there was an uptick in the proportion of high-achieving men who applied to a state with an abortion ban in 2022 beyond the trend line. This uptick is largely driven by new Common App members in Texas, including its two flagship universities that joined in 2022 (Steele, 2022). In Figure

1.10a, I plot the proportion of high-achieving applicants who applied to a state with an abortion ban, excluding schools in Texas. This figure eliminates the uptick in the proportion of high-achieving men who applied to a state with an abortion ban in 2022, and shows an attenuated increase in the proportion of high-achieving women.[22] Figures 1.9a and 1.9c, which use application data for schools that are always Common App members and thus similarly exclude the new Texas schools, also eliminate this uptick.

[22]New Common App members in Wisconsin that appeal particularly to high-achieving applicants also entered the sample over time. Figure 1.10c plots application trends to states with an abortion ban excluding schools in both Texas and Wisconsin.



(a) *High-achieving applicants*

(b) *Lower-achieving applicants*

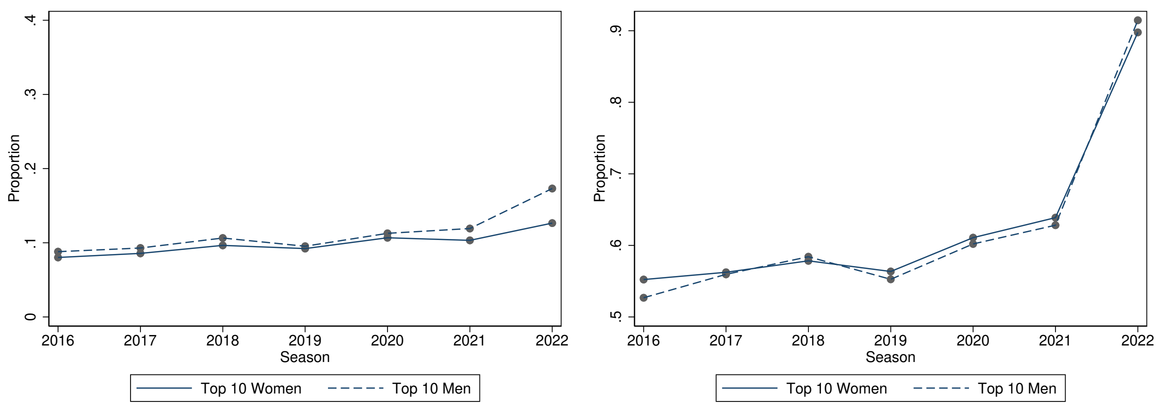
(c) *High-achieving applicants*

(d) *Lower-achieving applicants*

Figure 1.10: *Application Patterns Over Time to States with an Abortion Ban Excluding Schools in Texas and Wisconsin*

Notes: This figure shows the proportion of applicants who applied to a state with an abortion ban – excluding schools in Texas or Texas and Wisconsin – using college application data from seasons 2016-17 through 2022-23. High-achieving applicants scored in the top 10 percent of SAT/ACT test-takers in the sample in 2016. The dashed vertical line indicates the timing of *Dobbs*.

Importantly, for the main identifying assumption to hold, the proportion of high-achieving women who applied to a state with an abortion ban or law would have had to increase at a similar rate as men as new Common App members joined the sample in the post-period. Figures 1.5 and 1.6, as previously discussed, provide support for this assumption. Further, Figure 1.11 shows similar pre-period trends in the proportion of men and women who applied in- and out-of-state to Texas, as well as a sharp increase in 2022 in the proportion of men and women who applied in-state to Texas. These data suggest that, similar to men, more out-of-state women would have applied to Texas in 2022 after the state's flagship universities joined the Common App if not for the state's abortion ban.



(a) *Applied Out-of-State to Texas*

(b) *Applied In-State to Texas*

Figure 1.11: *Application Patterns of High-Achieving Applicants to Texas Over Time*

Notes: This figure shows the proportion of applicants who applied in- and out-of-state to Texas in the 2016-17 through 2022-23 college application seasons. High-achieving applicants scored in the top 10 percent of SAT/ACT test-takers in the sample in 2016.

Lastly, to provide further support for the timing of the treatment – which I define as the 2021 and 2022 college application seasons – I explore trends in Google searches on the topic of abortion in Figure 1.12.[23] Numbers on the Y-axis represent search interest relative to the highest point on the chart for the given region and time. A value of 100 is the peak popularity for the term, which corresponds with the *Dobbs* decision in June 2022. There is also a notable spike in spring 2019, which is when states passed restrictive laws with greater intensity, and in fall 2021, when Texas' 6-week ban went into effect. Importantly, Figures 1.10a and 1.10c provide evidence that women changed their application behavior

[23] A Google Trends topic is a group of search terms that share the same concept or entity, in any language.

in response to more than Texas' 6-week ban in 2021, as the proportion of high-achieving women who applied to a state with an abortion ban or law – excluding Texas – declined relative to men in both 2021 and 2022.

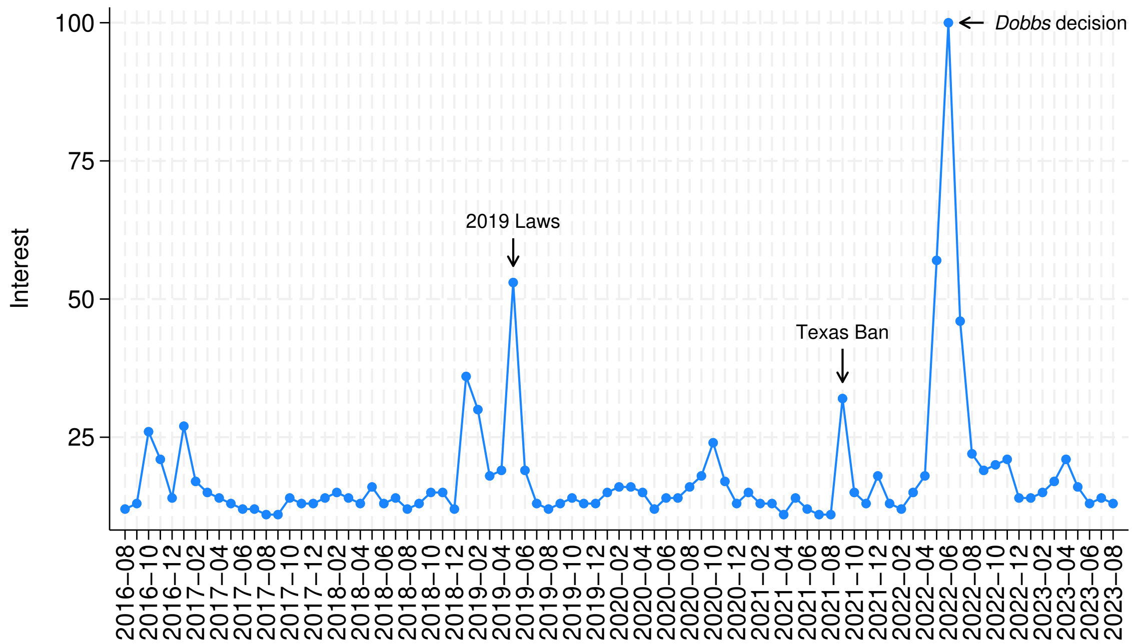


Figure 1.12: *Interest in Abortion Topic Over Time*

Notes: This figure uses GoogleTrends data on the topic "Abortion" in the United States from August 2016 through August 2023. A topic is a group of search terms that share the same concept or entity, in any language. Numbers represent search interest relative to the highest point on the chart for the given region and time. A value of 100 is the peak popularity for the term. A value of 50 means that the term is half as popular. A score of 0 means there was not enough data for this term. An improvement in the data collection system was applied from 1/1/2022.

1.7 Results

Table 1.3 reports the proportion of high-achieving women and men who applied to a state with an abortion ban, an abortion law, or no restriction in the pre- and post-periods. For example, in the pre-period in Panel A, 40.4 percent of high-achieving women applied to a state with an abortion ban, relative to 37.6 percent of high-achieving men. In the post-period in Panel A, approximately 43 percent of both men and women applied to a state with an abortion ban. The last column takes the difference between these two differences, for an implied effect of -2.5 percentage points. Panel B reports a -1.1 percentage point effect on applying to a state with an abortion law and Panel C reports a near zero effect on applying to a state with no abortion restriction.

The differences in means in Table 1.3 correspond with columns 1, 3, and 5 in Table 1.4. The preferred specification in columns 2, 4, and 6 in Table 1.4 includes applicant covariates and fixed effects. Relative to high-achieving men, high-achieving women were 2.7 percentage points less likely to apply to a state with an abortion ban (column 2) and 1.2 percentage points less likely to apply to a state with an abortion law (column 4). There is a small, negative effect on applying to a state with no abortion restriction (column 6) when effects are averaged across both treated years, but Table 1.5 reports null effects in both 2021 and 2022.

Table 1.3: *Proportion of High-Achieving Applicants who Applied to a State with an Abortion Ban, Abortion Law, or No Restriction Before and After Dobbs*

Panel A: Abortion Ban

	Women	Men	Difference
Before	0.404	0.376	0.029
After	0.430	0.427	0.003
Difference	0.026	0.051	-0.025

Panel B: Abortion Law

	Women	Men	Difference
Before	0.625	0.653	-0.028
After	0.684	0.723	-0.039
Difference	0.059	0.070	-0.011

Panel C: No Restriction

	Women	Men	Difference
Before	0.871	0.835	0.037
After	0.885	0.854	0.031
Difference	0.014	0.019	-0.005

Notes: This table shows the proportion of high-achieving applicants who applied to a state with an abortion ban, an abortion law, or no restriction in the pre- and post-periods. The third column (Difference) is the difference between the proportion of women and men.

Table 1.4: *The Effect of Dobbs on High-Achieving Women's College Application Decisions: Applied to a State with an Abortion Ban, Abortion Law, or No Restriction*

	Abortion Ban		Abortion Law		No Restriction	
	(1)	(2)	(3)	(4)	(5)	(6)
Female x After	-0.025***	-0.027***	-0.011***	-0.012***	-0.005*	-0.006**
	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.002)
Female	0.029***		-0.028***		0.037***	
	(0.007)		(0.006)		(0.004)	
After	0.051***		0.070***		0.019*	
	(0.009)		(0.010)		(0.011)	
Applicant covariates	No	Yes	No	Yes	No	Yes
State-by-season FE	No	Yes	No	Yes	No	Yes
State-by-gender FE	No	Yes	No	Yes	No	Yes
R-squared	0.002	0.076	0.005	0.081	0.003	0.097
N	597755	596526	597755	596526	597755	596526

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the difference-in-differences estimates of the impact of *Dobbs* on high-achieving female college applicants relative to high-achieving male college applicants. The outcome is whether an applicant applied to one of 13 states with an enforceable abortion ban (columns 1-2); one of 24 states with an abortion law (columns 3-4), which includes those with an enforceable ban; or one of 26 states and D.C. with no abortion restriction (columns 4-6). The first estimate for each outcome uses the baseline specification without any controls. The second estimate for each outcome controls for applicant-level covariates (see text for details) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

Table 1.5: *Event Study Estimates for High-Achieving Applicants: Applied to a State with an Abortion Ban, Abortion Law, or No Restriction*

	Abortion Ban		Abortion Law		No Restriction	
	(1)	(2)	(3)	(4)	(5)	(6)
Female x After x 2022	-0.035***	-0.037***	-0.009	-0.010**	-0.004	-0.005
	(0.008)	(0.007)	(0.005)	(0.005)	(0.003)	(0.003)
Female x After x 2021	-0.015***	-0.014***	-0.009*	-0.009*	-0.003	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
Female x After x 2019	-0.000	0.002	0.005	0.006	0.007**	0.006**
	(0.004)	(0.004)	(0.005)	(0.005)	(0.003)	(0.003)
Female x After x 2018	0.001	0.002	0.003	0.004	0.002	0.003
	(0.005)	(0.005)	(0.006)	(0.005)	(0.003)	(0.003)
Female x After x 2017	0.000	0.003	0.002	0.005	0.005*	0.004
	(0.005)	(0.005)	(0.006)	(0.005)	(0.003)	(0.003)
Female x After x 2016	0.001	0.002	-0.007	-0.005	-0.006	-0.007
	(0.007)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
Applicant covariates	No	Yes	No	Yes	No	Yes
State-by-season FE	No	Yes	No	Yes	No	Yes
State-by-gender FE	No	Yes	No	Yes	No	Yes
R-squared	0.003	0.076	0.008	0.081	0.003	0.097
N	597755	596526	597755	596526	597755	596526

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the event study estimates of the impact of *Dobbs* on high-achieving female college applicants relative to high-achieving male college applicants. The outcome is whether an applicant applied to one of 13 states with an enforceable abortion ban (columns 1-2); one of 24 states with an abortion law (columns 3-4), which includes those with an enforceable ban; or one of 26 states and D.C. with no abortion restriction (columns 4-6). The first estimate for each outcome uses the baseline specification without any controls. The second estimate for each outcome controls for applicant-level covariates (see text for details), and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

These results indicate that women had the greatest behavioral response to the subset of 13 states with an enforceable abortion ban, and the magnitude of this effect increased after the *Dobbs* decision was handed down. Column 2 of Table 1.5 reports that women were 1.4 percentage points less likely to apply to these states in 2021, and 3.7 percentage points less likely to apply to these states in 2022. Further, women from states with no abortion restriction largely drove the estimated treatment effects. Column 2 of Table 1.6 reports that, relative to women from states with an abortion law, women from states with no abortion restriction were less likely to apply to a state with an abortion ban by approximately 2 percentage points more than their male peers, for a total effect of -3.3 percentage points across both treated years. In 2022, this subset of women was 4.5 percentage points less likely to apply to a state with an abortion ban, as reported in column 2 of Table 1.7. See Figure 1.15 for the corresponding event studies for all applicants and for those from states with no abortion restriction.[24]

[24]See Figure A.2 for event studies for applicants from states with an abortion ban or law.

Table 1.6: *The Effect of Dobbs on High-Achieving Women's College Application Decisions by State of Residence: Applied to a State with an Abortion Ban, Abortion Law, or No Restriction*

	Abortion Ban		Abortion Law		No Restriction	
	(1)	(2)	(3)	(4)	(5)	(6)
Female x After x No Restriction	-0.020**	-0.019**	-0.013*	-0.012*	-0.007	-0.008
	(0.009)	(0.009)	(0.007)	(0.006)	(0.006)	(0.006)
Female x After	-0.013*	-0.014*	-0.003	-0.003	-0.002	-0.001
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
Female x No Restriction	-0.050***		-0.041***		-0.022***	
	(0.008)		(0.009)		(0.008)	
No Restriction x After	0.025		0.040**		0.042**	
	(0.017)		(0.017)		(0.019)	
No Restriction	-0.031		-0.028		0.103***	
	(0.034)		(0.030)		(0.028)	
Female	0.063***		0.001		0.051***	
	(0.007)		(0.008)		(0.007)	
After	0.033**		0.043***		-0.007	
	(0.013)		(0.012)		(0.017)	
Applicant covariates	No	Yes	No	Yes	No	Yes
State-by-season FE	No	Yes	No	Yes	No	Yes
State-by-gender FE	No	Yes	No	Yes	No	Yes
R-squared	0.005	0.076	0.007	0.081	0.023	0.097
N	597755	596526	597755	596526	597755	596526

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the triple difference-in-differences estimates of the impact of *Dobbs* on high-achieving female college applicants from states with no abortion restriction relative to their high-achieving male peers.The outcome is whether an applicant applied to one of 13 states with an enforceable abortion ban (columns 1-2); one of 24 states with an abortion law (columns 3-4), which includes those with an enforceable ban; or one of 26 states and D.C. with no abortion restriction (columns 4-6). The first estimate for each outcome uses the baseline specification without any controls. The second estimate for each outcome controls for applicant-level covariates (see text for details), and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

Table 1.7: *Event Study Estimates for High-Achieving Applicants from States with No Abortion Restriction: Applied to a State with an Abortion Ban, Abortion Law, or No Restriction*

	Abortion Ban		Abortion Law		No Restriction	
	(1)	(2)	(3)	(4)	(5)	(6)
Female x After x 2022	-0.044***	-0.045***	-0.013**	-0.013***	-0.005	-0.005
	(0.007)	(0.007)	(0.005)	(0.005)	(0.004)	(0.004)
Female x After x 2021	-0.014**	-0.013**	-0.013*	-0.012*	-0.001	-0.001
	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)	(0.003)
Female x After x 2019	0.005	0.005	0.009	0.009	0.010***	0.010***
	(0.005)	(0.004)	(0.006)	(0.006)	(0.003)	(0.003)
Female x After x 2018	0.006	0.006	0.004	0.004	0.006	0.006*
	(0.006)	(0.005)	(0.006)	(0.005)	(0.003)	(0.003)
Female x After x 2017	0.003	0.005	0.003	0.004	0.010***	0.009***
	(0.005)	(0.005)	(0.007)	(0.006)	(0.002)	(0.003)
Female x After x 2016	0.006	0.008	-0.008	-0.006	-0.001	-0.000
	(0.009)	(0.007)	(0.007)	(0.005)	(0.004)	(0.004)
Applicant covariates	No	Yes	No	Yes	No	Yes
State-by-season FE	No	Yes	No	Yes	No	Yes
State-by-gender FE	No	Yes	No	Yes	No	Yes
R-squared	0.003	0.081	0.012	0.088	0.005	0.075
N	406627	406058	406627	406058	406627	406058

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the event study estimates of the impact of *Dobbs* on high-achieving female college applicants from states with no abortion restriction relative to high-achieving male college applicants. The outcome is whether an applicant applied to one of 13 states with an enforceable abortion ban (columns 1-2); one of 24 states with an abortion law (columns 3-4), which includes those with an enforceable ban; or one of 26 states and D.C. with no abortion restriction (columns 4-6). The first estimate for each outcome uses the baseline specification without any controls. The second estimate for each outcome controls for applicant-level covariates (see text for details), and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

However, the *Dobbs* decision did not affect the application behavior of women from states with an abortion law to states with no abortion restriction. Column 6 of Table 1.6 reports a null effect for this population, indicating that treatment effects are asymmetric – women from states with no abortion restriction were less likely to apply to a state with an abortion ban, but women from states that signed an abortion restriction into law were no more or less likely to apply to a state with no restriction. Women from states with no restriction similarly did not shift their applications toward states with no restriction as they decreased their applications to states with a ban.

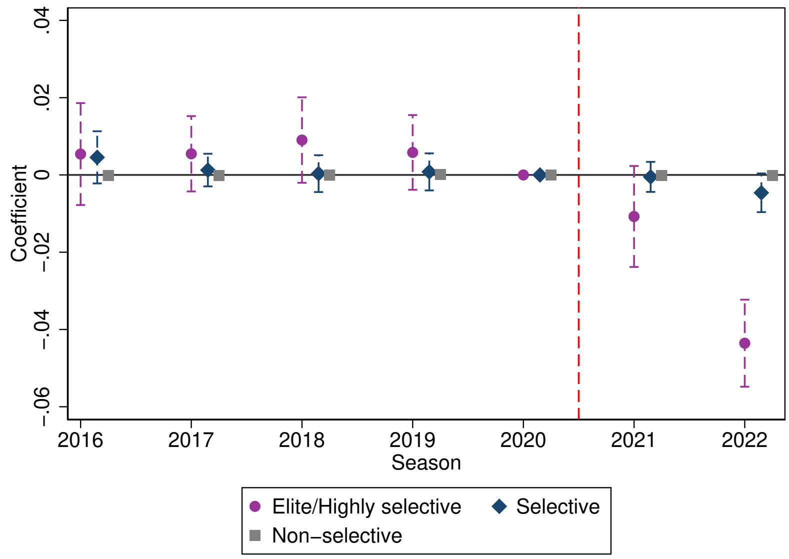
These results report changes in application behavior for whether an applicant applied to any school in one of the 13 states with an abortion ban. However, as schools vary in selectivity, they may also vary in their appeal to high-achieving college applicants. Panel A of Table 1.8 provides a break down of school selectivity in each state with an abortion ban for schools that were ever Common App members during the sample window (see Section 3.2 for a discussion of school selectivity categorization). There are four elite Common App members, including one in Louisiana, Missouri, Tennessee, and Texas, and 26 highly selective members spread out across Arkansas, Kentucky, Louisiana, Oklahoma, Tennessee, Texas, and Wisconsin. More than two-thirds of schools fall in the selective category, and a small number are non-selective.

Table 1.8: *School Selectivity in States with an Abortion Ban*

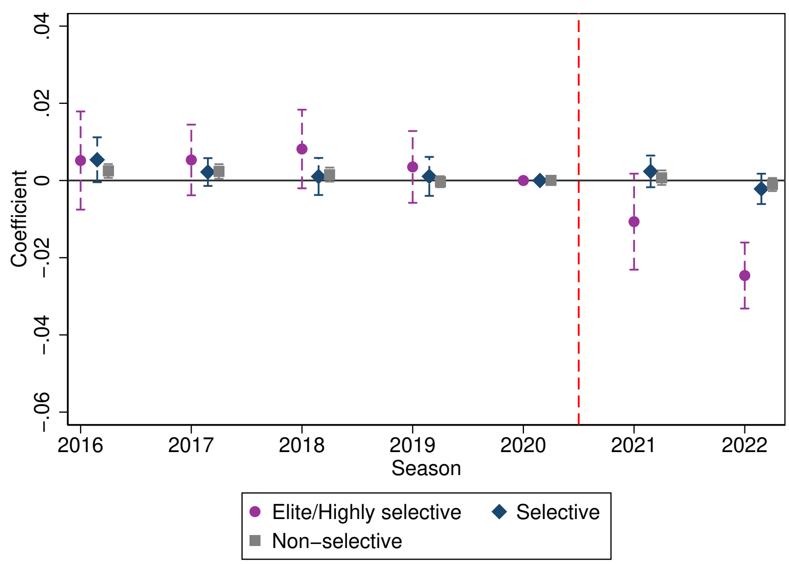
	Panel A: Ever a Common App member			
	Elite	Highly Selective	Selective	Non-selective
Alabama	-	-	8	1
Arkansas	-	1	1	2
Idaho	-	-	3	-
Kentucky	-	1	8	3
Louisiana	1	1	5	1
Mississippi	-	-	5	-
Missouri	1	-	24	1
Oklahoma	-	1	3	-
South Dakota	-	-	4	-
Tennessee	1	2	10	1
Texas	1	9	18	3
West Virginia	-	-	9	-
Wisconsin	-	11	10	-
Total	4	26	108	12
	Panel B: Always a Common App member			
	Elite	Highly Selective	Selective	Non-selective
Alabama	-	-	4	-
Arkansas	-	1	-	1
Idaho	-	-	3	-
Kentucky	-	1	2	-
Louisiana	1	1	3	-
Mississippi	-	-	2	-
Missouri	1	-	8	1
Oklahoma	-	1	2	-
South Dakota	-	-	1	-
Tennessee	1	2	5	-
Texas	1	6	4	1
West Virginia	-	-	4	-
Wisconsin	-	5	7	-
Total	4	17	45	3

Notes: This table categorizes schools using the Barron's selectivity index: elite schools (Ivy-Plus, which includes the Ivy League plus Stanford, MIT, Chicago, and Duke, and Barron's Tier 1 excluding the Ivy-Plus), highly selective schools (Barron's Tier 2), selective schools (Barron's Tiers 3-5), and non-selective schools (Barron's Tier 9 and colleges that are not included in the selectivity index). Panel A includes schools that were ever a Common App member between the 2016-17 and 2022-23 application seasons. Panel B includes schools that were always Common App members between the 2016-17 and 2022-23 application seasons.

Event study estimates in Figure 1.13a show significant treatment effects for applying to an elite or highly selective school, both of which drew the largest proportion of high-achieving applicants (35.5 percent) in the pre-period. Applications to selective schools, which drew a much smaller proportion of high-achieving applicants in the pre-period (3.7 percent), were largely unaffected. There is a precise null for non-selective schools, which drew just 0.01 percent of high-achieving applicants in the pre-period. These results suggest that changes in application behavior to the most competitive schools in states with an abortion ban drive the main findings, largely because those are the schools to which high-achieving applicants apply.



(a) *Ever Common App members*



(b) *Always Common App members*

Figure 1.13: *Event Study Estimates by School Selectivity*

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interaction terms between year and gender, where the outcome is an indicator for whether an applicant applied to a school by selectivity category. The sample is restricted to applicants from states with no abortion restriction. The first figure uses schools that were ever Common App members between the 2016 and 2022 college application seasons. The second figure uses schools that were always Common App members between the 2016 and 2022 college application seasons. The dashed vertical line indicates the start of the treatment period. Regressions control for applicant covariates (see text), and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

Indeed, in the pre-period, significantly more high-achieving applicants from states with no abortion restriction applied out-of-state to the five states – Louisiana, Missouri, Tennessee, Texas, and Wisconsin (see Figure A.1) – that are home to all of the elite schools and nearly 90 percent of the highly selective schools in states with a ban. As a result, restricting the outcome to whether an applicant applied to one of these five states in Figure A.3a recovers similar estimates to the main findings.[25]

Notably, Panel A (schools that were ever Common App members in the sample window) and Panel B (schools that were always Common App members in the sample window) of Table 1.8 show that the number of elite schools remained constant over time, while the number of highly selective schools increased due to nine schools in Texas and Wisconsin becoming members. Figure 1.13b plots the event study estimates by school selectivity for the subsample of application data to schools that are always Common App members in the sample window. The pattern of results is consistent with those in Figure 1.13a. There is a smaller treatment effect in 2022 with the exclusion of the highly selective schools that joined the Common App in 2022, particularly the flagship universities in Texas. Tables A.2 and A.3 report average treatment effects and event study estimates across all schools for this subsample.

1.8 Mechanisms

In this section, I explore two mechanisms that I hypothesize may drive changes in women's application behavior to states with an abortion ban: concerns about access to reproductive health care and political identity. I restrict the analytic sample to applicants from states with no abortion restriction.

I start by considering the sensitivity of women's college choices to the nearness of available reproductive services. Women attending college in a state with an abortion ban would have to travel out-of-state for care, and travel distance has a significant impact on

[25]Figure A.3 also shows how treatment effects vary when each of the five states is excluded from the outcome.

abortion access. Lindo et al., 2020, for example, find substantial and nonlinear effects of travel distance on abortion rates in Texas – an increase in travel distance from 0–50 miles to 50–100 miles reduces abortion rates by 16 percent, but the effects of increasing distance are smaller when the nearest clinic is already more than 50 miles away. Of the 13 states with an abortion ban, three states – Arkansas, Louisiana, and Mississippi – do not share a border with a state where abortion is legal. I posit that if women were to apply to and attend school in one of these states, they would subsequently be likely to travel back home to access a clinic.

Arkansas, Louisiana, and Mississippi are clustered together in the U.S. South, geographically far from states with no abortion restriction (see Figure 1.1). I measure the distance (in miles) between the center of population of each of the three states and the centroid of each applicant's home zip code. I then calculate the average distance-to-home across the three states for each applicant, and take the average by applicant home state.[26] Applicants live at least nearly 500 miles from Arkansas, Louisiana, and Mississippi, and an average of nearly 1200 miles. With these significant distances to home, applicants are unlikely to be sensitive to incremental changes in proximity. Results in Table 1.9 confirm this hypothesis. There is no incremental effect of living in the second or third tercile of distance-to-home relative to the first on whether an applicant applies to a school in Arkansas, Louisiana, or Mississippi. Importantly, this result does not imply that women do not care about access to reproductive care, but rather that they live too far from the states that are landlocked by abortion bans to be sensitive to the travel distance to home.

[26] I exclude applicants from Alaska and Hawaii from this analysis.

Table 1.9: *The Effect of Dobbs on High-Achieving Women's College Application Decisions by Distance to Home*

	Applied to AR, LA, or MS	
	(1)	(2)
Female x After x Second Tercile	0.003	0.002
	(0.004)	(0.004)
Female x After x Third Tercile	0.004	0.004
	(0.004)	(0.003)
Female x Second Tercile	-0.002	
	(0.003)	
Female x Third Tercile	-0.005	
	(0.003)	
After x Second Tercile	-0.009**	
	(0.003)	
After x Third Tercile	-0.005*	
	(0.003)	
Female x After	-0.012***	-0.011***
	(0.002)	(0.002)
Second Tercile	0.015*	
	(0.008)	
Third Tercile	0.001	
	(0.007)	
Female	0.030***	
	(0.003)	
After	-0.011***	
	(0.003)	
Applicant covariates	No	Yes
State-by-season FE	No	Yes
State-by-gender FE	No	Yes
R-squared	0.005	0.025
N	404254	403713

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the triple difference-in-differences estimates of the impact of *Dobbs* on high-achieving female college applicants relative to their high-achieving male peers by distance-to-home. The sample is restricted to applicants from states with no abortion restriction. The outcome is whether an applicant applied to Arkansas, Louisiana, or Mississippi. The first estimate uses the baseline specification without any controls. The second estimate controls for applicant-level covariates (see text for details) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

In addition to school-home distance, I explore whether applicants are sensitive to how far schools are from the nearest abortion provider. I focus on changes in application behavior to elite and highly selective schools, which draw the largest proportion of high-achieving applicants (see Section 1.7).[27] These schools are located in eight of the 13 states with an abortion ban, including Arkansas, Kentucky, Louisiana, Missouri, Oklahoma, Tennessee, Texas, and Wisconsin. I match each school's geocoordinates to the corresponding county and each county to public-use data based on the Myers Abortion Facility Database, which is a panel of United States county-by-month distances to the nearest abortion provider (C. Myers, 2024). I use data on abortion providers in the August of each year, which marks the start of the Common App application season. Following Lindo et al., 2020, I then group schools into five categories based on their distance to the nearest abortion provider in 2022: 0-50 miles, 50-100 miles, 100-150 miles, 150-200 miles, and more than 200 miles.[28]

The event study in Figure 1.14a shows that treatment effects are largest for the schools that are more than 200 miles from the nearest abortion clinic, which are concentrated in Louisiana and Texas. The effect size for this subset of schools is larger in magnitude than the main results, particularly in 2022, which is largely driven by application behavior to the highly selective schools in Texas that joined the Common App in 2022.[29,30] This analysis provides suggestive evidence that college-going women *are* sensitive to access to reproductive care when making application decisions; however, they may also be responding to other characteristics about the abortion ban policies or environments that are specific to

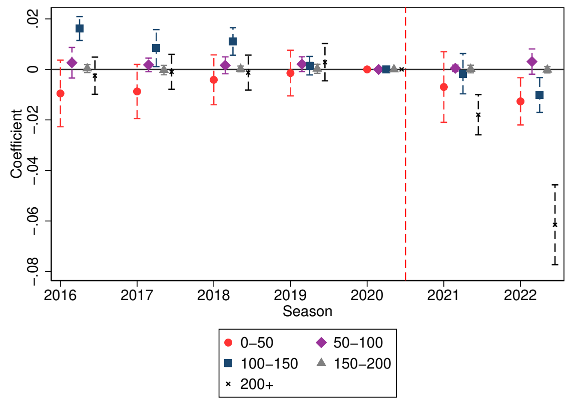
[27]Limiting the analysis to elite and highly selective schools also avoids potentially conflating student preferences for selectivity with student preferences for distance to abortion providers if more selective schools are systematically closer to abortion clinics than their less selective peer institutions.

[28]There are 30 elite or highly selective schools in states with an abortion ban (see Table 1.8), which are not equally distributed across the distance bins. There are four schools in the 0-50 mile bin, eight schools in the 50-100 mile bin, two schools in the 100-150 mile bin, 4 schools in the 150-200 mile bin, and 12 schools in the more than 200 mile bin.

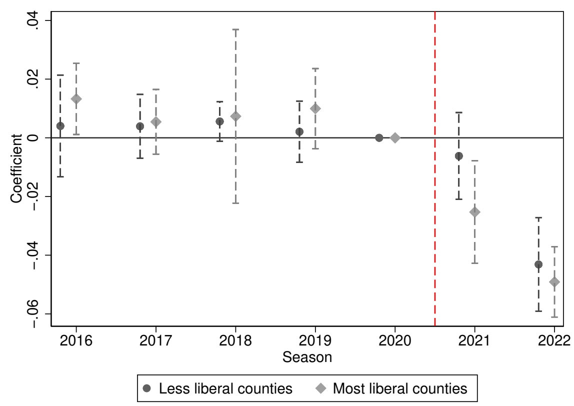
[29]Importantly, omitting applications decisions to Louisiana and Texas from the main outcome in Section 1.7 – a binary indicator for whether an applicant applied to a state with an abortion ban – only slightly attenuates the main results, providing evidence that changes in application behavior to these two states do not solely drive the main findings.

[30]Effect sizes are similar in magnitude to the main results in Section 1.7 for the subsample of elite and highly selective schools that are always Common App members.

these two states and correlated with clinic distance.



(a) *Nearness to Abortion Provider (in miles)*



(b) *Political Ideology*

Figure 1.14: *Event Studies for Mechanisms*

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interaction terms between year and gender. In the first figure, the outcome is an indicator for whether an applicant applied to an elite or highly selective school based on distance to the nearest abortion provider. In the second figure, the outcome is an indicator for whether an applicant applied to a state with an abortion ban. The sample is restricted to applicants from states with no abortion restriction. The dashed vertical line indicates the start of the treatment period. Regressions control for applicant covariates (see text), and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

Next, I examine the impact of political identity on women's college choices. Given the relationship between political ideology and views on abortion, I hypothesize that treatment effects will be larger for more liberal women. To test this mechanism, I match each applicant's zip code to its county-level vote share in the 2016 presidential election using data from the MIT Election Data and Science Lab, 2018.[31] I then cut Democratic vote share into terciles. Vote share for Hillary Clinton ranged from approximately 7-55 percent in the first tercile, 55-68 percent in the second tercile, and 68-91 percent in the third tercile. I create a binary indicator for applicants from counties in the top tercile (the most liberal counties).

Results in Table 1.10 show that, relative to women from less liberal counties, women from the most liberal counties were less likely to apply to a state with an abortion ban by nearly 2 percentage points more than their male peers. In particular, women from the most liberal counties had a significantly larger behavioral response in 2021 in anticipation of *Dobbs* (see Figure 1.14b). Notably, according to the *The New York Times*, 11 of the 13 states with a ban tended to vote much more Republican than the country as a whole ("2016 Presidential Election Results" 2017). Women from the most liberal counties in the U.S. were thus less likely to apply to schools in the the most conservative states with abortion bans.

[31] Zip codes may span more than one county. To allocate vote share data from the county to the zip code, I weight by the ratio of residential addresses when collapsing.

Table 1.10: *The Effect of Dobbs on High-Achieving Women's College Application Decisions by County Politics: Applied to a State with an Abortion Ban*

	Abortion Ban	
	(1)	(2)
Liberal x Female x After	-0.014**	-0.017**
	(0.006)	(0.006)
Female x After	-0.028***	-0.027***
	(0.006)	(0.005)
Liberal x After	0.008	-0.002
	(0.013)	(0.010)
Liberal x Female	-0.010	-0.007
	(0.006)	(0.005)
Liberal	0.045**	0.002
	(0.021)	(0.008)
Female	0.016***	
	(0.005)	
After	0.056***	
	(0.008)	
Applicant covariates	No	Yes
State-by-season FE	No	Yes
State-by-gender FE	No	Yes
R-squared	0.004	0.081
N	406200	405750

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the triple difference-in-differences estimates of the impact of *Dobbs* on high-achieving female college applicants from liberal counties relative to their high-achieving male peers. The sample is restricted to applicants from states with no abortion restriction. The outcome is whether an applicant applied to a state with an abortion ban. The first estimate uses the baseline specification without any controls. The second estimate controls for applicant-level covariates (see text for details) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

However, it is plausible that men also changed their application behavior due to political ideology. If so, the estimated treatment effects would understate the impact of state reproductive rights law on women's college choices. I explore this possibility by separately plotting the proportion of men and women from the most (the top tercile) and less (the bottom two terciles) liberal counties. Figure A.7 shows that the proportion of men in more liberal counties who applied to a state with an abortion ban trended similarly to men in less liberal counties, with no relative change in application behavior in 2021 and 2022. However, for women, those from more liberal counties changed their behavior relative to those from less liberal counties during the treatment period. These descriptive data provide suggestive evidence that political identity motivated changes in women's but not men's application behavior.

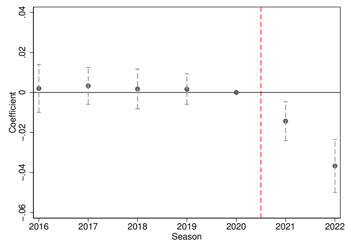
1.9 Robustness and Persistence

1.9.1 Alternative Explanations and Specification Checks

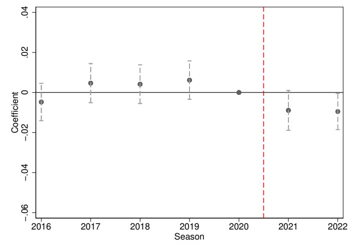
In this section, I test alternative explanations for the main findings and perform sensitivity checks for the preferred specification. I start by examining whether treatment effects could be driven by state policies on COVID-19. People living in states won by Donald Trump in the 2016 election – which include all of the 24 states with an abortion law – were less likely to live under mask mandates, stay-at-home orders, or limitations on social gatherings compared with people in states won by Hillary Clinton (Rothwell and Makridis, 2020). The results in Section 1.7 could be driven by a response to COVID-19 if, relative to men, women from states with no abortion restriction – which were largely won by Hillary Clinton – were deferentially impacted by COVID-19 protocols.

I provide two pieces of evidence to rule out this alternative explanation. First, the 2020-21 college application season began in the summer immediately following the COVID-19 pandemic outbreak in spring 2020. Panel B of Figure 1.15 provides no evidence of differential trends in the 2016-2020 application seasons for applicants from states with no abortion restriction. Second, Figure A.6 presents null effects for whether applicants from states with no abortion restriction applied to one of the six states (Alaska, Kansas, Pennsylvania, Montana, Nebraska, and North Carolina) with no abortion restriction that voted for Donald Trump in the 2016 election.

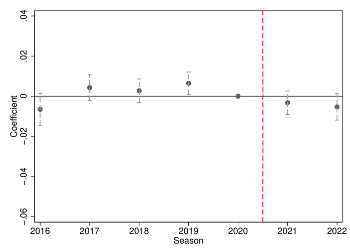
Panel A: All Applicants



(a) *Abortion ban*

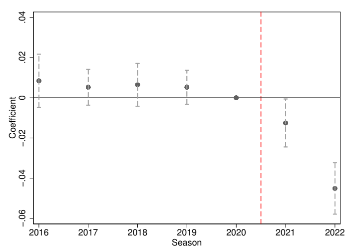


(b) *Abortion law*

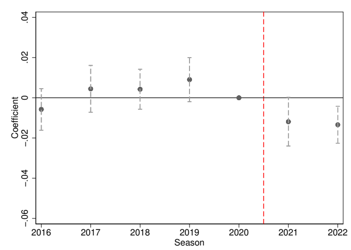


(c) *No abortion restriction*

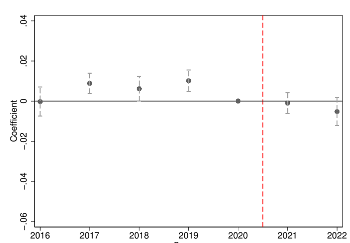
Panel B: Applicants from States with No Abortion Restriction



(a) *Abortion ban*



(b) *Abortion law*



(c) *No abortion restriction*

Figure 1.15: *The Effect of Dobbs on High-Achieving Women's College Application Decisions*

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interaction terms between year and gender, where the outcome is an indicator for whether an applicant applied to a state with an abortion ban, abortion law, or no abortion restriction. Year 2020 is the omitted category. The dashed vertical line indicates the start of the treatment period. Regressions control for applicant covariates (see text for details) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

Next, I assess whether the main findings are sensitive to how I define the outcomes, which are based on state laws passed by the 2022-23 application season. It is possible, though improbable, that a state's decision to pass an abortion law is endogenous to changes in the proportion of it's out-of-state college applicants. I define two new variables for whether an applicant applied to a state with an abortion law or ban based on states that passed a law by the 2021-22 college application season, which was the first treated year. Consistent with the main findings, Figure A.8 shows a negative treatment effect in 2021 and a larger negative treatment effect in 2022 for both outcomes.[32] I also assess the sensitivity of the main findings to applications to Indiana and West Virginia, which signed abortion laws on August 5 and September 16, 2022, respectively, after the start of the Common App season on August 1. For high-achieving applicants, nearly 100 percent of applications to Indiana were sent after August and approximately 98 percent of applications to West Virginia were sent after September, indicating that nearly all application decisions were made after the states passed their laws.[33]

I then show that the main findings are not sensitive to how I define high-achieving applicants. To motivate this analysis, Figure A.9 shows that the proportion of applicants who applied out-of-state is increasing in SAT/ACT score.[34] The proportion starts to level off after a score of 1460, which is the cut score for the 90th percentile of SAT/ACT test-takers in the sample in 2016, which I apply in all years. In Table A.4, I estimate the preferred specification for high-achieving students based on an SAT/ACT cut score of 1300, 1350, or 1400.[35] Results are similar in magnitude to the main results in Table 1.4.

[32]These results should not be surprising, as nearly all of the states with an abortion ban passed their corresponding laws prior to 2022. Indeed, the outcome for applying to a state with an abortion ban by 2021 excludes only West Virginia and the outcome for applying to a state with an abortion law by 2021 excludes Florida, Indiana, West Virginia, and Wyoming. Figure A.1 confirms that a small proportion of high-achieving applicants applied to these states in the pre-period.

[33]Over 95 percent of applications from all high-achieving applicants were sent between October and January across all application seasons. Thirty-five percent of applicants' first applications were submitted in October and 47 percent of applicants' first applications were submitted in January of their respective application seasons.

[34]For this figure, I restrict the sample to 2016-2020 to avoid capturing an effect of treatment. However, the pattern is unchanged if I use all application seasons.

[35]According to the College Board, in 2016 a score of 1290 corresponded with the 90th percentile of a nationally

Next, I test the sensitivity of the main findings to alternative sample selection criteria by predicting the likelihood of applying out-of-state using a logit regression with applicant and zip code covariates.[36] I cut the predicted propensity to apply out-of-state into deciles and re-estimate treatment effects by decile for applying to a state with an abortion ban. Figure A.5 compares the characteristics of applicants in the top decile of likelihood to the high-achieving sample of applicants. A greater proportion of applicants in the top decile are women, White, and Black, and fewer applicants are Asian. These applicants are more likely to be from states with an abortion ban. They are slightly more likely to apply out-of-state than their high-achieving peers (96 percent vs. 93 percent), apply to fewer out-of-state states (4.59 states vs. 4.75 states), and send a greater proportion of their applications out-of-state (83 percent vs. 72 percent).

Figures A.10a and A.10b plot the treatment effects in 2021 and 2022, respectively, by decile.[37] Effect sizes are increasing in decile, meaning that applicants who are more likely to apply out-of-state experienced larger treatment effects. Figure A.10c plots event study estimates for the top decile of propensity to apply out-of-state. These results are similar to Figure 1.15a in Panel A, which plots event study estimates for the top decile by achievement.

Lastly, I use a synthetic difference-in-differences (SDID) research design to compare changes in the college application behavior of women from states with and without abortion restrictions. To implement the SDID, I restrict my sample to high-achieving women and collapse the data to applicant state of residence by application season cells. The advantage of SDID is that, by combining features of synthetic control and difference-in-differences,

---

representative sample of U.S. students in the 11th and 12 grade weighted to represent all U.S. students in thoes grades, regardless of whether they typically take the SAT. A score of 1340 corresponded with the 90th percentile of a nationally representative sample of U.S. college bound students, weighted to represent students who typically take the SAT last as 11th- or 12th-graders.

[36] I restrict the logit regressions to the pre-period application seasons (2016-2020) and then predict applicants' propensity to apply out-of-state in all seasons. Logit regressions include race, gender, age, Common App fee waiver eligibility, first generation status, SAT score, whether an applicant attended high school in her home state, the type of high school the applicant attended, median household income of the applicant's zip code adjusted to 2021 dollars, percent adults with at least a BA degree in the applicant's zip code, applicant home state, and application season.

[37] Note that for applicants with a lower propensity to apply out-of-state, particularly those in the first three deciles, event studies reveal significant pre-trends.

it consistently estimates the causal effect of treatment even without the parallel trends assumption holding between all treatment and control unit on average.[38] This feature allows me to estimate the effect of *Dobbs* on changes in women's application behavior, independent of changes in men's. However, unlike the main analysis, which weights data at the applicant-level, collapsing the data gives equal weight to each state and decreases precision.

I model this analysis on work by Dench, Pineda-Torres, and C. Myers, 2024, who estimate the effect of *Dobbs* on fertility. Using SDID, the authors compare changes in birth rates in the 13 states with a total abortion ban (treated states) to those in the 24 states and D.C. where abortion was protected (control states). The remaining 13 states with gestational age limits or blocked bans are excluded from the control states. The authors' classification scheme closely aligns with my organization of state abortion restrictions with minimal exceptions.[39] However, in the context of college applications, as reported in Section 1.7, *Dobbs* induced women from states without an abortion restriction to change their behavior, but not women from states with a restriction. I therefore compare changes in the application behavior of women from the 26 states and D.C. with no restriction on abortion (treated states) to those of women from the 24 states with an abortion law (control states). I also restrict the group of control states to the 13 states with an abortion ban.

To estimate treatment effects, I use

$$\left(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = argmin \tau, \mu, \alpha, \beta \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\} \tag{1.4}$$

with a balanced panel of N states and T application seasons, where the outcome for state i in application season t is denoted by Y_{it}. The binary treatment, which is equal to 1 for states in the treated group in the 2021-22 and 2022-23 application seasons and zero otherwise, is

[38]SDID re-weights and matches pre-exposure trends to weaken the reliance on parallel trends like synthetic control, and is invariant to additive unit-level shifts and allows for valid large-panel inference like difference-in-differences (Arkhangelsky et al., 2021).

[39]There are four exceptions. Dench, Pineda-Torres, and C. Myers, 2024 classify Michigan as a protected state, and Nebraska, North Carolina, and Pennsylvania as an excluded state.

denoted by W_{it}. The average treatment effect on the treatment is generated from a two-way fixed effects regression with optimally chosen weights, $\hat{\omega}_i^{sdid}$ and $\hat{\lambda}_t^{sdid}$. The selection of unit weights, ω, seeks to ensure that comparison is made between treated and control units that approximately followed parallel trends prior to treatment. The selection of time weights, λ, seeks to draw more weight from pre-treatment periods that are more similar to post-treatment periods. In comparison, difference-in-differences procedures estimate the effect of treatment by solving the same two-way fixed effects regression without the time or unit weights (Arkhangelsky et al., 2021; Clarke et al., 2023). I rely on block bootstrap methods for statistical inference using 1,000 replications and follow Clarke et al., 2023 to estimate SDID event studies with confidence intervals.

The first two columns of Panel A of Table A.6 provide evidence that high-achieving women from states with no abortion restriction were less likely to apply to a state with an abortion ban relative to women from states with an abortion law (-1.3 percentage points) and relative to women from states with an abortion ban (-2.6 percentage points, significant at the 10 percent level). There was no effect on applying to a state with an abortion law or no restriction. In Panel B, I restrict the sample to the pool of applications to schools that were Common App members in the 2019-2020 through 2021-2022 application seasons. I narrow the estimation window to include more Common App member schools in the sample, which is made possible by SDID requiring a minimum of two pre-treatment periods to determine control units (Clarke et al., 2023), and estimates are similar to those in Panel A. Figure A.11 presents corresponding event study estimates for applying to a state with an abortion ban with the control group restricted to states with an abortion ban.

1.9.2 Treatment Effect Persistence

A natural next question is whether treatment effects persisted beyond the 2022-23 college application season. To answer this question, I incorporate preliminary college application data from the 2023-24 application season into the analysis and focus on application behavior to states with an abortion ban.[40] Notably, as described in Appendix A.0.3, several states made changes to their abortion policy post-2022. Abortion policy changes in three states – Indiana, North Dakota, and Wisconsin – are the most relevant for this analysis. Indiana and North Dakota began enforcing complete abortion bans in the spring and summer of 2023, while abortion access resumed in Wisconsin in the fall of 2023. As a result, 14 states had a total abortion ban in the 2023-24 college application season, compared with 13 states in the 2022-23 season.

One potential solution is to incorporate changes in state policy into the outcome variable in the main analysis, which is a binary indicator for whether an applicant applied to a state with a ban. In Section 1.7, applicants in the 2016-17 through 2022-23 application seasons were coded as a 1 if they applied to one of the 13 states with an abortion ban and 0 otherwise. Applying this logic forward, applicants in the 2023-24 application season would be coded as a 1 if they applied to one of the 14 states with an abortion ban and 0 otherwise.

However, men and women's application behavior differed across these two pools of states. In Figure 1.5a, the proportion of high-achieving women who applied to one of the 13 states with an abortion ban in the 2022-23 application season outpaced their male peers at every point in time in the pre-period. On the other hand, Figure A.4 shows opposite trends – the proportion of high-achieving men who applied to one of the 14 states with an abortion ban in the 2023-24 application season outpaced their female peers at every point in time in the pre-period. Using differences in men and women's application behavior to the 13 states with a ban as the counterfactual for differences in application behavior to the 14 states with a ban will thus bias the estimated treatment effect in 2023-24 upwards.

Alternatively, I incorporate the new application data into the analysis and estimate two separate event studies. In the first specification, I use the original outcome variable (whether an applicant applied to one of the 13 states with a ban).[41] In the second specification, I

[40] These data include applications from August 2023 through March 2024.

[41] I also estimate a specification where the outcome is whether an applicant applied to one of 12 states with a ban, where I exclude Wisconsin from the outcome because the state resumed access to abortions in 2023. Results are nearly identical.

define the outcome based on whether an applicant applied to one of the 14 states with a ban. Figure A.5 shows that the magnitude of the treatment effect in the 2023-24 application season was the same as that in the 2022-23 application season. This finding provides evidence of persisting changes in high-achieving women's college application behavior in response to state abortion bans.

1.10 Conclusion

In this paper, I estimate the impact of state laws on reproductive rights on women's human capital decisions. I use variation in state abortion restrictions from the timing of the *Dobbs* case, which the U.S. Supreme Court agreed to hear in May 2021 and decided in June 2022. Using a difference-in-differences identification strategy, I show that state abortion restrictions, particularly total bans that were enforced, impacted high-achieving women's college choices relative to men.

High-achieving female college applicants were 2.7 percentage points less likely to apply to a state that enforced a total abortion ban and 1.2 percentage points less likely to apply to a state with any abortion law relative to their male peers. Effects on applying to a state with an abortion ban were larger in 2022 (-3.7 percentage points) than in 2021 (-1.4 percentage points) and persisted in 2023. Further, effects were driven by high-achieving women from states with no abortion restriction – these applicants were 1.3 percentage points less likely to apply to a state with an abortion ban in 2021 and 4.5 percentage points less likely to apply to a state with an abortion in 2022, for a combined effect of -3.3 percentage points. There was no effect on whether women, including those from states with an abortion law or ban, applied to a state with no abortion restriction, implying that effects are asymmetric.

Further, I posited that women from states with no abortion restriction changed their application behavior because of their concerns about access to reproductive health care and/or political identity. Given the politics of abortion, the two mechanisms are likely highly correlated for women from primarily "blue" states. In trying to disentangle the two, I find that the distance between a woman's home state and a state with an abortion ban (i.e., the distance she would need to travel back home to access reproductive services) does not have an incremental effect on whether she applied to a school in that state, but that the distance between schools in states with an abortion ban and the nearest abortion clinic does. Further, women from the most liberal counties were less likely to apply to a state with an abortion ban relative to their peers from less liberal counties.

Notably, the main finding that women were less likely to apply to a state with an abortion ban is largely driven by changes in application behavior to the most competitive schools, as these schools draw a large proportion of high-achieving applicants. This result has economic implications for students. The average annual out-of-state tuition fee for elite and highly selective schools in states with an abortion ban was approximately $42,000 in 2022, compared with an average out-of-state fee of approximately $49,000 in states with no abortion restriction. Women are thus willing to forgo applying to a less expensive schooling option – a potential $7,000 savings, not including differences in the cost of living – based on state abortion policy.

Beyond the economic cost to students, these results also have significant policy implications. First, these findings indicate that state social policy impacts where women apply out-of-state to college, which may translate into changes in college learning environments. Second, this result has potential downstream implications for labor markets, as individuals are more likely to work in the states where they attended college. If fewer high-achieving women from liberal states migrate to more conservative states, for example, the U.S. will experience an increase in partisan sorting by gender. Third, these findings suggest there is a need for additional research on the effect of state abortion bans on other migration decisions in education (e.g., college enrollment and transfers, graduate school applications and enrollment) and beyond (e.g., migration more broadly).

Chapter 2

Changing Choice Sets: How Parents Respond to Increases in Access to High-Quality Schools[1]

[1]The research reported here was supported, in whole or in part, by the Institute of Education Sciences, U.S. Department of Education, through grant R305B150012 to Harvard University. The opinions expressed are those of the authors and do not represent the views of the Institute or the U.S. Department of Education.

2.1 Introduction

To facilitate school choice, several major cities have created centralized school choice systems where parents rank their top choice schools. Families weigh a myriad of school factors when making these choices, including academic quality, distance to home, and peer composition (Abdulkadiroğlu, Pathak, et al., 2020; Glazerman and Dotter, 2017; Glazerman, 1998; Harris and Larsen, 2015; Hastings, Kane, and Staiger, 2006). However, parents' choices may not purely reflect their preferences due to constraints on access to schools, asymmetric information, or other search frictions.

A policy change in a large urban public school district in the Northeast (LUDNE) provides a unique opportunity to explore the effect of changes to constraints on parents' school choices. In school years prior to 2014-15, the district divided schools into three geographic zones, and parents could rank any school in their home zone in the lottery. Starting in school year 2014-15, the district implemented a new assignment plan that gave families access to a set of schools based on new, district-determined measures of school quality. The district designed the policy to provide families with a guaranteed minimum access to nearby high-quality schools based on a four-tier rating system.

This policy change had several important implications. First, all families received access to at least two nearby top quality schools, which was not guaranteed under the former policy. Second, families gained new information about the schools in their choice sets through the district's tier ratings. Third, the new assignment plan reduced the number of schools that parents could choose from by decreasing the size of their choice sets. The new policy thus relaxed constraints on access to nearby high-quality schools while also reducing search frictions.

In this paper, I estimate the effect of changes to these constraints on the academic quality of schools, as measured by the district's tier ratings, that parents ranked as their first choice for their prospective kindergarten students. To do so, I follow a panel of geocodes over time; new students participated in the lottery each year, but the same set of geocodes – the smallest unit of address aggregation in the district – are repeated in the data. To define the treatment, I create a summative measure for the average quality of school choice sets by geocode and policy period. I then implement a difference-in-differences empirical strategy, where the treatment is defined at the geocode-level and the outcome is at the student-level.

I focus on two plausible mechanisms in the analysis: changes in access to high-quality schools (what I refer to as the “choice set quality effect") and changes in access to school quality information (what I refer to as the “information effect"). To identify the first channel, I divide geocodes into quartiles based on the average quality of families' school choice sets prior to the policy change. Only the families who lived in geocodes in the first quartile experienced a large increase in average choice set quality after the policy change. I can then compare changes in the quality of first-choice schools for families in the first quartile to those for families in the rest of the district before and after the policy change. To identify the second channel, I use the measure of average choice set quality by geocode and policy period. Changes in access to high-quality schools varied by geocode, but families across all geocodes gained access to school quality information after the policy change. I can then estimate the differential effect of increasing the average quality of school choice sets after the policy change relative to before the policy change on the quality of first-choice schools (i.e., how the relationship between school choice set quality and first-choice school quality changes), which I argue is the effect of gaining access to school quality information.

I find that changes in access to high-quality schools as well as changes in access to information about school quality affected parents' choices. First, I find that families chose higher-quality schools when their access to these schools increased. The choice set quality effect was particularly salient for historically underserved families in the district, including Black and Hispanic families. Second, I find that increasing choice set quality induced even higher quality first-choice schools after parents gained access to school tier ratings. The information effect was the largest for White families and not detectable for Black families. Third, for families who gained access to higher quality schools, I find that parents' choices translated into higher quality school assignments and enrollments. Changes in information had a smaller effect on school assignments and enrollments. Notably, these three sets of findings coincided with a decrease in the number of schooling options available to families, suggesting that choice outcomes can improve as options decrease.

This paper makes three significant contributions to the literature. First, this paper adds to the research on revealed preferences and school choice. Much of this work uses discrete choice models to infer how families trade off different school attributes in district-wide systems of choice (Glazerman, Nichols-Barrer, et al., 2018; Glazerman and Dotter, 2017; Harris and Larsen, 2015; Hastings, Kane, and Staiger, 2006; Hastings, Kane, and Staiger, 2009). These studies describe preferences based on ranked choices under a given policy regime at a point in time. Several predict changes in school choices by simulating alternative scenarios to the studied context. These studies also find that preferences are heterogeneous, with wealthier families more likely to choose higher-performing schools (Harris and Larsen, 2015; Hastings, Kane, and Staiger, 2009). However, some of the variation in preferences may arise from differences in access rather than differences in taste (Burgess et al., 2015).

Second, this paper contributes to the literature on the role of information in school choice. Several survey experiments study how information affects perceptions of school quality (Barrows et al., 2016; Glazerman, Nichols-Barrer, et al., 2018; Houston and Henig, 2021; Jacobsen, Snyder, and Saultz, 2014; Schneider et al., 2018). There is also a body of work on experimental informational interventions in K-12 school choice (Arteaga et al., 2022; Cohodes et al., 2022; Corcoran et al., 2018; Hastings and Weinstein, 2008; Weixler et al., 2020). These studies contribute to the literature in economics, psychology, and education that find that providing decision-makers with information improves their educational choices, including choosing schools with better test scores, graduation rates, or placement rates. These approaches are particularly important for less advantaged decision-makers. Third, this paper contributes to the literature on decision-making and choice overload. Research in behavioral economics finds that people have difficulty making decisions when faced with a large set of options (e.g., Thaler and Sunstein, 2009).

The remainder of the paper proceeds as follows. Section 2.2 reviews the policy context. Section 3.2 provides a description of the data and Section 2.4 describes motivating evidence for the treatment. Section 2.5 describes the empirical strategy. Section 2.6 reports results, Section 2.7 includes specification checks, and Section 3.4 discusses and concludes.

2.2 Policy Context

LUDNE is one of the most diverse school districts in the nation and serves more than 50,000 students. How the district assigns students to schools has evolved over time. Once court ordered to desegregate, the city later divided the district's schools into geographic zones. Families could rank at least three schools in order of preference in their zone or within one mile of their home in the district's lottery. According to the district, this policy resulted in substantial transportation costs without resolving persisting inequities in access to nearby high-quality schools. In the 2014-15 school year, the district implemented a new assignment plan that replaced the zones with individualized school choice sets based on a student's home address and the quality of nearby schools.

To implement the policy, the district created new measures of school quality, referred to as tiers; the district did not calculate school ratings prior to the 2014-15 school year. The district created one tier system for the 2014-15 and 2015-16 school years and a second tier system for the 2016-17 and 2017-18 school years. The district assigned these tiers based on a percentile ranking of school-level performance on state exams in reading and math. These performance scores were from school years prior to the implementation of the new policy, a feature that ensures that the policy change did not affect the tier ratings during the window of this study.[2]

The district used these tiers to determine students' choice sets. Students were given access to the two nearest Tier 1 (highest-quality) schools, the four nearest Tier 1 or 2 schools, and the six nearest Tier 1, 2, or 3 schools. Students also had access to schools within a mile radius of their home, schools with special programs, schools that their siblings attended, and higher capacity schools (often Tier 4) that were close to home. Students ranked their

[2]The first tier system was based on school test scores from 2011 and 2012. The second tier system was based on school test scores from 2013 and 2014.

preferred choices in a lottery and the district used a deferred acceptance algorithm to assign seats.

The district provided several opportunities for families to learn about the schools in their choice sets before ranking their preferences, including a kickoff event where families could meet with school principals, several school preview days, and district-led information sessions.[3] In addition, all families who participated in the lottery met with district staff to fill out registration paperwork, review their list of school choices, and submit their preferences. LUDNE also created an online search engine to provide parents with information about the schools in their choice sets, such as school quality tier, distance to home, and available programs and facilities.

2.3 Data

I use district administrative data on student school choice sets, ranked school choices and school assignments, and school enrollment from school years 2011-12 through 2017-18. I refer to school years 2011-12 through 2013-14 as the pre-period and school years 2014-15 through 2017-18 as the post-period. The sample includes 19,116 prospective kindergarten students (K1 and K2) who participated in the district's school choice lottery. I focus on kindergarten students for two reasons. First, kindergarten is the main entry point into the district. Second, the new assignment plan was phased in over six school years, starting with kindergarten students.

I include in the sample students who were new to the district, meaningfully participated in the lottery for the first time, ranked a school that received a school quality rating, reported a home address, and had at least ten schools in their choice set. See Section B.0.1 for comprehensive details on data construction. In Section 2.7, I show that my results are not sensitive to the decisions that I make to construct the data.

Table 2.1 reports descriptive statistics for in- and out-of-sample students. Thirty-eight

[3]Source: Director of Assignment, LUDNE, October 7, 2019.

percent of students in the sample identified as Hispanic, 26 percent were Black, and 23 percent were White. Thirty-seven percent of students were English learners. More than half of students in the sample had a sibling in the district. Nearly two thirds (62 percent) of the sample participated in the lottery in K1. The sample appears to have more White students than those out-of-sample, although more out-of-sample students were missing race/ethnicity data.[4]

Table 2.1: *Sample Descriptive Statistics*

	Sample	Not in Sample	All
White	22.77	10.53	18.70
Asian	8.25	4.27	6.94
Black	26.40	33.65	28.75
Hispanic/Latino	38.35	34.90	37.17
Native	0.22	0.32	0.025
Mixed/Other	3.82	2.30	3.32
Missing race/ethnicity	0.18	14.04	4.87
Non-ELL	61.76	62.72	62.08
ELL	37.35	36.13	36.95
Sibling	57.76	59.64	58.35
K1	61.95	57.36	60.44
K2	38.05	42.64	39.56
Observations	19116	9402	28518

Notes: This table reports percentages across all school years in the window of the study.

2.4 Motivating Evidence

The policy change affected the composition of students' choice sets by guaranteeing minimum access to nearby high-quality schools. The district defined minimum access as the two nearest Tier 1 schools, the four nearest Tier 1 or 2 schools, and the six nearest Tier 1, 2, or 3 schools. To summarize how this guarantee changed the composition of choice

[4]Only the data file on student choice sets contain students' race/ethnicity, and nearly all out-of-sample students who were missing race/ethnicity (99 percent) did not appear in that file.

sets, I start by calculating the average school quality of each student's choice set. I flip the orientation of the tiers so that they are increasing in higher quality schools by coding Tier 1 schools as a 4, Tier 2 schools as a 3, Tier 3 schools as a 2, and Tier 4 schools as a 1.[5] I then calculate the average school quality of each students' choice sets, and take the mean within each geocode and policy period. For simplicity, I refer to this measure as "average choice set quality," which ranges from a value of 2.0 to 3.2.

Next, I divide geocodes into quartiles based on average choice set quality in the pre-period.[6] Figure 2.1 depicts average choice set quality by quartile over time. All four quartiles trended similarly in the pre-period and the first quartile (Q1) experienced the largest increase in average choice set quality in the post-period. These data suggest that geocodes in Q1 were the geocodes that were most treated by the policy. Figure 2.2 shows that students in these geocodes were mostly Black and Hispanic.

[5]The district had one tier system for schools in the 2014-15 and 2015-16 school years and a second tier system for schools in the 2016-17 and 2017-18 school years. I apply the first tier system to schools in years prior to the policy change; the data used to calculate the first set of tiers overlap with these years and are thus a plausible representation of school quality in the years in which the district did not assign school tiers.

[6]The choice of quartiles is supported by a decile plot of geocodes' average choice set quality in the pre-period. See Figure B.3. The first three deciles experienced a clear jump in average choice set quality after the policy change.

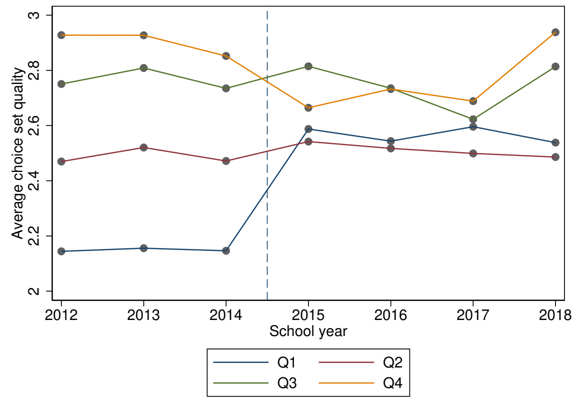


Figure 2.1: *Average Choice Set Quality by Geocode Quartile and Year*

Notes: This figure depicts average choice set quality by quartile and school year. Geocodes are divided into quartiles based on average choice set quality prior to the policy change in school year 2014-15.

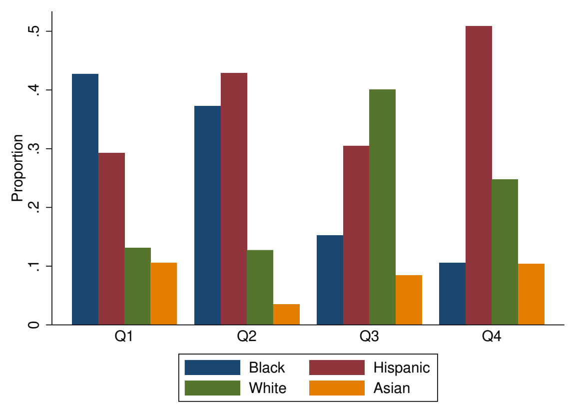


Figure 2.2: *Proportion of Students by Race/Ethnicity in Each Geocode Quartile*

Notes: This figure reports the proportion of students by race/ethnicity in each quartile. Geocodes are divided into quartiles based on average choice set quality prior to the policy change in school year 2014-15.

How did the policy change students' choice sets? Overall, students had fewer schools of all tiers in their choice sets in the post-period, as the policy change broadly reduced choice set size. The average number of schools in students' choice sets in Q1 decreased from approximately 27 to 15 schools. Nearly 50 percent of students in geocodes in Q1 did not have access to a Tier 1 school within a mile from home in either period. (Under both policy regimes, students had access to all schools within a one-mile walk zone.) However, in the post-period, more students had access to Tier 1 schools and fewer students had access to Tier 4 schools within 1.5 or 2 miles of home.

As students' choice sets changed, so too did their first-choice schools. Prior to the treatment period, Figure 2.3 shows that all four quartiles trended similarly in the quality of schools that families ranked first. In the post-period, geocodes in Q1 – which experienced the largest increase in average choice set quality – also experienced the largest improvement in first-choice school quality. This metric is the outcome of interest, as it summarizes families' preferred school choices.[7] Together, these trends provide suggestive evidence of a treatment effect.

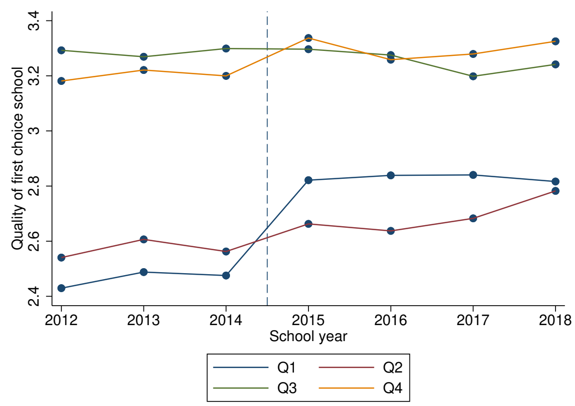


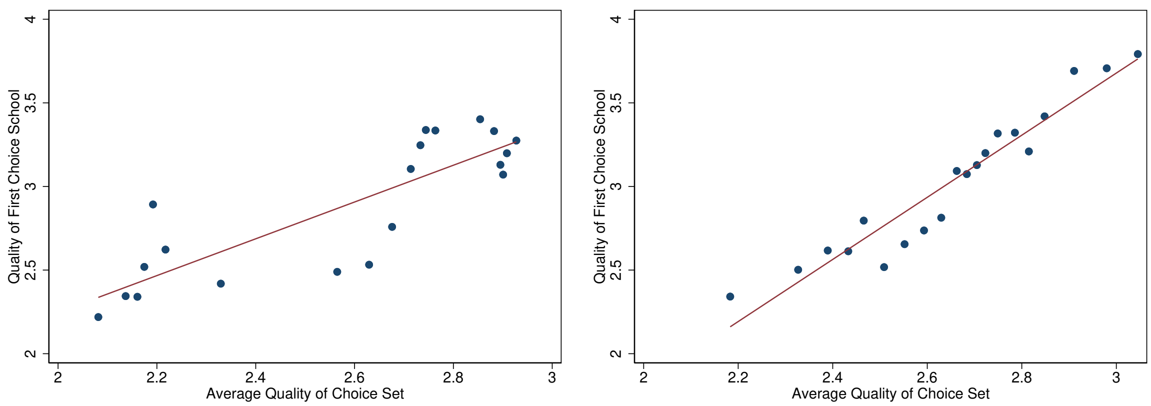
Figure 2.3: *Quality of First-Choice Schools by Geocode Quartile and Year*

Notes: This figure depicts the quality of students' first-choice schools by quartile and school year. Geocodes are divided into quartiles based on average choice set quality prior to the policy change in school year 2014-15.

Importantly, in addition to changes in choice set composition, families in the post-period also gained access to district-determined measures of school quality. This information appears to have affected parents' ranked choices above and beyond changes in their choice sets. Using data from all geocodes in the sample, binned scatter plots with the quality of

[7] I similarly flip the orientation of the tiers so that the outcome is increasing in higher quality schools.

first-choice schools on the x-axis and average choice set quality on the y-axis show a change in the relationship between the two before and after the policy change. First, as shown in Figure 2.4, there is a rightward shift in the distribution in the post-period. This shift occurs because average choice set quality increased after the policy change. Second, the slope steepens, suggesting that as the average quality of choice sets increased, the quality of first-choice schools increased more in the post-period than in the pre-period. This finding implies that increasing average choice set quality had a greater impact on the outcome when families had access to school quality information.



(a) *Before*

(b) *After*

Figure 2.4: *Average Choice Set Quality and Quality of First-Choice Schools*

Notes: Binned scatter plots show the relationship between the quality of students' first choice schools and the average quality of students' choice sets by geocode in each policy period.

Going forward, for ease of exposition I standardize average choice set quality and the quality of students' first-choice schools to each have a mean of 0 and a standard deviation of 1. A one standard deviation increase in average choice set quality is like moving from an average choice set quality of 2.6 to 2.9. A one standard deviation increase in the quality of first-choice school is like moving from a Tier 3 school at the 50th percentile in school-level performance to a Tier 1 school at the 80th percentile.[8]

[8]This comparison uses the 2016-17 and 2017-18 tier rankings. The district assigned tier rankings based on

2.5 Empirical Strategy

My goal is to estimate the causal effect of two mechanisms on the quality of first-choice schools: (1) increasing average choice set quality and (2) providing information about school quality. To do so, I follow a panel of neighborhood regions (geocodes) over time using data on successive cohorts of prospective kindergarten students.

I leverage the quartiles in Figure 2.1, which are based on average choice set quality in the pre-period, to test the first mechanism. All four quartiles received an information treatment in the post-period, but only the first quartile (Q1) experienced a significant increase in the average quality of school choice sets. I use this variation to isolate the effect of increasing average choice set quality on the quality of first-choice schools. The main identifying assumption is that geocodes in Q1 would have similar trends in students' ranked choices as geocodes in Q2-Q4 if not for changes to average choice set quality. I subsequently estimate

$$Y_{i(g)} = \alpha + \beta(Q1_g \times After_t) + \delta Q1_g + \theta After_t + \epsilon_{i(g)} \tag{2.1}$$

where i indexes the student in geocode g, and t indexes the policy period. $Q1$ is an indicator variable equal to 1 for geocodes in the first quartile and 0 otherwise. $After$ is an indicator variable that is equal to 1 if the student participated in the lottery beginning in the 2014-15 school year and 0 otherwise. The primary dependent variable is the school quality tier of students' first-choice schools and β is the parameter of interest, which I refer to as the choice set quality effect.[9] I cluster standard errors at the geocode-level.

Next, I use a difference-in-differences specification with a continuous treatment measure to test the second mechanism. I estimate

$$Y_{i(g)} = \alpha + \beta(Quality_{gt} \times After_t) + \delta Quality_{gt} + \theta After_t + \epsilon_{i(g)} \tag{2.2}$$

school-level proficiency and growth percentiles. Tier 1 schools were in the top 25th percentiles, Tier 2 schools were in the 26-59th percentiles, Tier 3 schools were in the 51-75th percentiles, and Tier 4 schools were in the 76th-100th percentiles. I standardize these percentiles to be mean 0, standard deviation 1.

[9]To define the dependent variable, I apply the first tier system to all pre-policy years.

where *Quality* is average choice set quality by geocode and policy period, as previously defined. Here, the parameter of interest, β, estimates the difference in the linear relationship between the treatment and outcome in the post-period relative to the pre-period. In other words, $\hat{\beta}$, tells us the differential effect of increasing the average quality of school choice sets after the policy change relative to before the policy change. I refer to this estimate as the information effect.

I also estimate event study versions of equations (2.1) and (2.2) using

$$Y_{i(g)} = \alpha + \sum_{\substack{t=-3 \\ t\neq -1}}^{3} \beta_t (Q1_g \times \mathbf{1}[\tau = t]) + \delta Q1_g + \nu_t + \epsilon_{i(g)} \tag{2.3}$$

$$Y_{i(g)} = \alpha + \sum_{\substack{t=-3 \\ t\neq -1}}^{3} \beta_t (Quality_{gt} \times \mathbf{1}[\tau = t]) + \delta Quality_{gt} + \nu_t + \epsilon_{i(g)}, \tag{2.4}$$

where I regress the outcome (the quality of students' first-choice schools) on a set of school year dummy indicators interacted with the treatment ($Q1$ or *Quality*), controlling for treatment ($Q1$ or *Quality*) and school year fixed effects. School year 2013-14 is the omitted category. I report results for these specifications in Section 2.7.

Lastly, I estimate reduced form effects for the quality of the schools to which students were assigned at the end of the lottery and the schools in which they subsequently enrolled. To do so, I estimate equations (2.1)-(2.4) with the quality of a student's assigned or enrolled school as the dependent variable.

2.6 Results

Table 2.2 reports results from equation (2.1). The first column reports an effect size of 0.267σ, meaning a one standard deviation increase in average choice set quality in geocodes in Q1 increased the quality of first-choice schools by 0.267σ. To put this into context, a 0.3σ effect size is equivalent to moving from a first-choice Tier 3 school at the 50th percentile in school-level performance to a first-choice Tier 2 school at nearly the 60th percentile.[10] This result is similar in magnitude to changes in peer composition for school choice lottery winners from low-quality neighborhood schools in North Carolina (0.36σ higher eighth-grade math scores, Deming, Hastings, et al., 2014) and the effect of Massachusetts charter schools on student achievement (about 0.3σ/year in math and 0.2σ/year in English Language Arts, Abdulkadiroğlu, Angrist, et al., 2011; Angrist, Dynarski, et al., 2010; Angrist, Dynarski, et al., 2012). The estimated effect does not meaningfully change with the addition of a set of controls in the second column, including student-level covariates (race/ethnicity, English learner status, grade level, and whether the student has a sibling in the district) and the student covariates interacted with the post-period indicator to allow for their effect to vary over time.[11] In the third column, I include geocode and year fixed effects, which subsume the main effect for *Q1* and the post-period indicator. The magnitude of the estimated effect increases slightly but is meaningfully the same.

[10]This comparison uses the 2016-17 and 2017-18 tier rankings. The district assigned tier rankings based on school-level proficiency and growth percentiles. Tier 1 schools were in the top 25th percentiles, Tier 2 schools were in the 26-59th percentiles, Tier 3 schools were in the 51-75th percentiles, and Tier 4 schools were in the 76th-100th percentiles. I standardize these percentiles to be mean 0, standard deviation 1.

[11]Free or reduced-price lunch status and gender are available only for students who enroll in LUDNE.

Table 2.2: *Choice Set Quality Effect*

	(1)	(2)	(3)
Q1 x After	0.267***	0.277***	0.304***
	(0.039)	(0.037)	(0.036)
Q1	-0.510***	-0.427***	
	(0.054)	(0.046)	
After	0.067***	-0.097***	
	(0.020)	(0.033)	
Student covariates	No	Yes	Yes
Geocode FE	No	No	Yes
Year FE	No	No	Yes
R-squared	0.034	0.138	0.325
N	19116	19116	19087

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors are clustered at the geocode-level. Column 3 has fewer observations because of the geocode fixed effects; 29 students are in geocodes with single observations.

Notably, as shown in Figure 2.2, the majority of families in geocodes in Q1 were Black or Hispanic. Indeed, across all years, 37 percent of students in geocodes in Q1 were Black and 43 percent were Hispanic. These percentages match those prior to the implementation of the new policy, suggesting that there were no significant demographic shifts in geocodes in Q1 starting in the 2014-15 school year. The estimated choice set quality effect in Table 2.2 is thus particularly relevant for families who were historically underserved in the district.

Next, Table 2.3 reports results from equation (2.2). The first column reports an effect of 0.186σ, meaning that a one standard deviation increase in average choice set quality in the post-period increased the average quality of first-choice schools by 0.186σ more than in the pre-period. A 0.2σ effect size is equivalent to moving from a first-choice Tier 3 school at the 50th percentile to a first-choice Tier 2 school at approximately the 56th percentile.[12] The

[12]This comparison uses the 2016-17 and 2017-18 tier rankings. The district assigned tier rankings based on school-level proficiency and growth percentiles. Tier 1 schools were in the top 25th percentiles, Tier 2 schools were in the 26-59th percentiles, Tier 3 schools were in the 51-75th percentiles, and Tier 4 schools were in the 76th-100th percentiles. I standardize these percentiles to be mean 0, standard deviation 1.

estimated effect is slightly smaller but not meaningfully different with the inclusion of the set of student-level controls in the second column. For this specification, I do not include geocode and year fixed effects; two-way fixed effects are not necessary for identification when the policy change occurs at one time for all units, and their inclusion may result in a biased estimate for a continuous treatment measure (Callaway, Goodman-Bacon, and Sant'Anna, 2021; Chaisemartin et al., 2022).

Table 2.3: *Information Effect*

	Choice		Random	Best
	(1)	(2)	(3)	(4)
Quality x After	0.186***	0.155***	-0.005	0.026***
	(0.015)	(0.023)	(0.013)	(0.006)
Quality	0.271***	0.233***	0.240***	0.000***
	(0.002)	(0.017)	(0.009)	(0.000)
After	0.034	-0.293***	0.004	0.041***
	(0.024)	(0.034)	(0.044)	(0.014)
Student covariates	No	Yes	Yes	Yes
R-squared	0.034	0.138	Yes	0.232
N	19116	19087	19116	19087

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The first two columns report results from the main specification. The third and fourth columns report results from different placebo tests (see text for details). Robust standard errors are clustered at the geocode-level.

The third column in Table 2.3 reports results from a placebo test. I randomly choose a school from each student's choice set to define school quality for the dependent variable, estimate equation (2.1), and repeat this exercise 50 times. I expect the estimated coefficient on $Quality \times After$ to be zero, as this process estimates the mechanical effect of changing choice set quality on the quality of first-choice schools; there should be no difference in this relationship between periods (e.g., no information effect) when schools are randomly chosen. Indeed, the estimated coefficient on the interaction term is nearly zero and not statistically significant.

The fourth column in Table 2.3 reports results from selecting the highest quality school in each student's choice set for the dependent variable. The estimate of 0.026σ represents the mechanical change in the relationship between increasing average choice quality and the quality of the top school in a student's choice set in the post-period. This estimate is small in magnitude, indicating that the estimates in the first two columns are largely the result of active selection.

I next estimate equation (2.2) by race/ethnicity to unpack potential heterogeneity in the information effect. The interaction term for Black families is small and not statistically significant in Table 2.4. The interaction terms for Hispanic and Asian families are similar in magnitude to the estimate for the full sample. The interaction term for White families is nearly double that for the full sample. A visual inspection of these relationships in binned scatter plots in Figure 2.5 shows no change in the relationship between the quality of first-choice schools and average choice set quality in the post-period for Black families, and a steep change in slope for White families. These findings suggest that access to school information through the tier ratings did not have the same impact on all families' school choices.

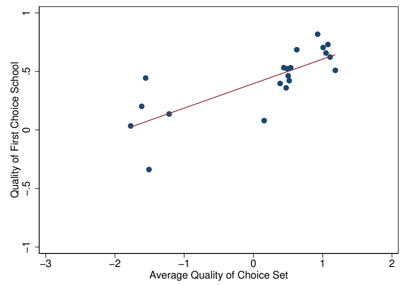
Table 2.4: *Information Effect by Race/Ethnicity*

	(1) Black	(2) Hispanic	(3) White	(4) Asian
Quality × After	-0.003	0.160^{***}	0.330^{***}	0.177^{***}
	(0.038)	(0.031)	(0.041)	(0.061)
Quality	0.321^{***}	0.185^{***}	0.209^{***}	0.200^{***}
	(0.025)	(0.023)	(0.037)	(0.032)
After	0.098^{***}	0.110^{***}	-0.321^{***}	-0.092^{*}
	(0.038)	(0.031)	(0.034)	(0.047)
R-squared	0.09	0.06	0.17	0.11
N	5047	7331	4353	1578

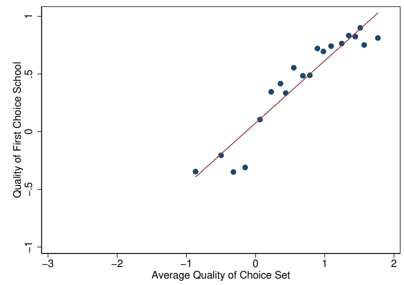
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

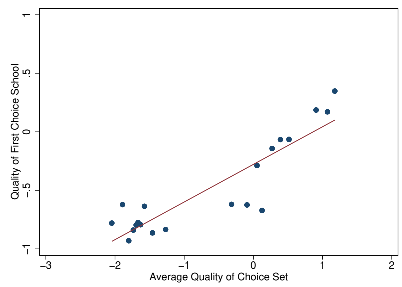
Notes: Regressions are estimated separately for each subgroup of students. Robust standard errors are clustered at the geocode-level.



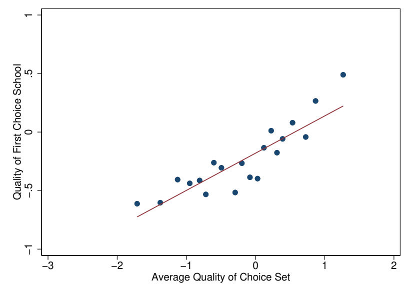
(a) *White Families Before*



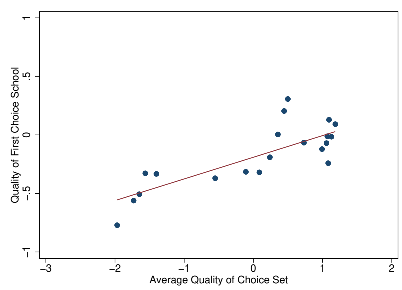
(b) *White Families After*



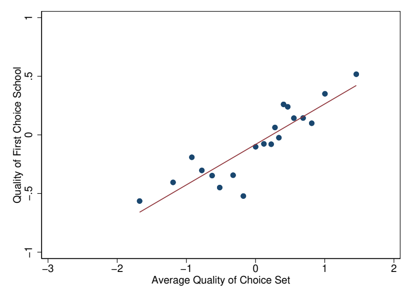
(c) *Black Families Before*



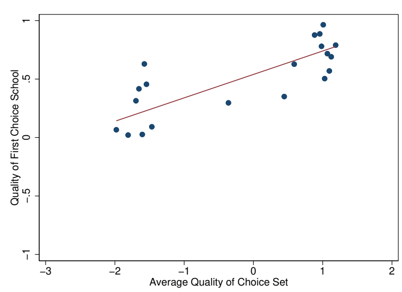
(d) *Black Families After*



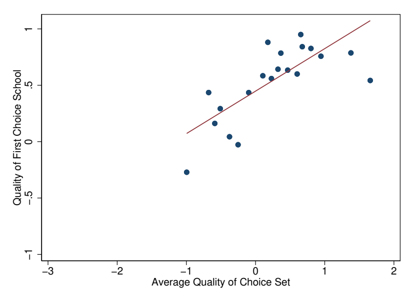
(e) *Hispanic Families Before*



(f) *Hispanic Families After*



(g) *Asian Families Before*



(h) *Asian Families After*

Figure 2.5: *Average Choice Set Quality and Quality of First-Choice Schools by Race/Ethnicity*

Notes: Binned scatter plots show the relationship between the quality of students' first choice schools and the average quality of students' choice sets by geocode in each policy period by student race/ethnicity.

Lastly, I estimate equations (2.1) and (2.2) for the quality of schools to which students were assigned at the end of the lottery and the quality of the schools in which students enrolled the following school year. The effects of increasing average choice set quality on the quality of assigned and enrolled schools in Table 2.5 are similar in magnitude to those for the quality of first-choice schools in Table 2.2. This finding suggests that parents' choices in geocodes in Q1 largely translated into higher-quality school assignments and enrollments. The effects of school quality information on the quality of assigned and enrolled schools in Table 2.6 are similar, but slightly smaller with the inclusion of student covariates, than that for the quality of first-choice schools in Table 2.3.

Table 2.5: *Choice Set Quality Effect: Assigned/Enrolled Schools*

	Assigned Schools			Enrolled Schools		
	(1)	(2)	(3)	(4)	(5)	(6)
Q1 x After	0.237***	0.265***	0.283***	0.169***	0.211***	0.241***
	(0.041)	(0.041)	(0.038)	(0.043)	(0.043)	(0.040)
Q1	-0.632***	-0.558***		-0.619***	-0.545***	
	(0.044)	(0.041)		(0.045)	(0.041)	
After	-0.061**	-0.115***		0.002	-0.063	
	(0.025)	(0.039)		(0.026)	(0.039)	
Student covariates	No	Yes	Yes	No	Yes	Yes
Geocode FE	No	No	Yes	No	No	Yes
Year FE	No	No	Yes	No	No	Yes
R-squared	0.050	0.146	0.326	0.055	0.165	0.352
N	14345	14345	14310	12878	12878	12832

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome in Panel A is the quality of schools to which students are assigned at the end of the lottery. The outcome in Panel B is the quality of schools in which students enroll. The samples are restricted to students who were assigned to a school with a school quality rating at the end of the lottery or to students who enrolled in a district school with a school quality rating, respectively. Robust standard errors are clustered at the geocode-level.

Table 2.6: *Information Effect: Assigned/Enrolled Schools*

	Assigned Schools		Enrolled Schools	
	(1)	(2)	(3)	(4)
Quality x After	0.109***	0.065***	0.145***	0.088***
	(0.022)	(0.022)	(0.024)	(0.023)
Quality	0.339***	0.308***	0.329***	0.297***
	(0.017)	(0.016)	(0.017)	(0.017)
After	-0.082***	-0.285***	-0.036	-0.254***
	(0.028)	(0.042)	(0.029)	(0.043)
Student covariates	No	Yes	No	Yes
R-squared	0.142	0.208	0.147	0.225
N	14345	14345	12878	12878

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome in Panel A is the quality of schools to which students are assigned at the end of the lottery. The outcome in Panel B is the quality of schools in which students enroll. The samples are restricted to students who were assigned to a school with a school quality rating at the end of the lottery or to students who enrolled in a district school with a school quality rating, respectively. Robust standard errors are clustered at the geocode-level.

Results in Table 2.7 report information effects for the quality of assigned and enrolled schools results by race/ethnicity. Information effects are small and not significant for Black families, similar to the main finding for first-choice school quality. The information effect on school assignments is smaller than that for first-choice school quality for Hispanic and Asian families. Information effects for assigned and enrolled schools for White families are similar in magnitude to those for first-choice school quality.

Table 2.7: *Treatment Effects: Quality of Assigned and Enrolled Schools by Race/Ethnicity*

	(1) Black	(2) Hispanic	(3) White	(4) Asian
Panel A: Assigned Schools				
Quality × After	-0.015	0.063*	0.155***	0.054
	(0.041)	(0.033)	(0.038)	(0.052)
Quality	0.291***	0.274***	0.373***	0.335***
	(0.027)	(0.021)	(0.035)	(0.028)
After	-0.093**	0.027	-0.323***	-0.174***
	(0.045)	(0.038)	(0.42)	(0.053)
R^2	0.08	0.09	0.18	0.17
N	3779	5579	3151	1242
Panel B: Enrolled Schools				
Quality × After	-0.030	0.076**	0.218***	0.143**
	(0.043)	(0.035)	(0.042)	(0.062)
Quality	0.297***	0.264***	0.343***	0.326***
	(0.028)	(0.021)	(0.037)	(0.028)
After	-0.093**	0.052	-0.302***	-0.066
	(0.045)	(0.038)	(0.045)	(0.061)
R-squared	0.08	0.08	0.20	0.20
N	3437	5252	2581	1132

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome in Panel A is the quality of schools to which students are assigned at the end of the lottery. The outcome in Panel B is the quality of schools in which students enroll. The samples are restricted to students who were assigned to a school with a school quality rating at the end of the lottery or to students who enrolled in a district school with a school quality rating, respectively. Robust standard errors are clustered at the geocode-level.

2.7 Specification Checks

In this section, I implement several specifications checks. I start with the event studies. Figures B.1a and B.1b plot the coefficients and 95 percent confidence intervals for the choice set quality effect and information effect, respectively. In each figure, there are no statistically significant pre-trends for the quality of first-choice schools before the 2014-15 school year. There are significant, positive effects each year after the district implemented the new policy.

Figure B.2 plots the coefficients from the event study regressions with the quality of school assignments or enrollments as the dependent variable. In Figures B.2a and B.2c, which use $Q1$ as the treatment, there are no statistically significant pre-trends and there are positive effects each year after the district implemented the new policy. In Figures B.2b and B.2d, which use $Quality$ as the treatment, there are also no statistically significant pre-trends; effects in 2016 are not statistically significant because of the smaller number of in-sample students that year.

Next, I estimate equations (2.1) and (2.2) using a more conservative approach to creating students' choice sets. I limit students' choice sets to schools that appear in their choice set data in the first round and year in which they participate in the lottery. For example, if a student first participated in the lottery in Round 1 in the 2013-14 school year, I include in their choice set only the schools that were listed in their choice set data in Round 1. This process excludes schools that were listed in the choice set data in the second, third, and fourth rounds as well as schools from students' ranked choice lists – which come from a separate data file (see Section B.0.1) – that did not appear in their choice set data. This approach necessarily restricts the sample to students whose first-choice schools appeared in their choice set data, which excludes 517 students. Results in column 1 of Table B.1 are nearly unchanged.

I then estimate equations (2.1) and (2.2) restricting the analysis to school years 2011-12 through 2015-16. This window includes all of the school years pre-policy change and the two school years post-policy change. This approach circumvents the change in tier system in the 2016-17 and 2017-18 school years. To implement this check, I drop observations after school year 2015-16 (5,944 students), redefine average choice set quality in each period, and cut geocodes into quartiles using data from the pre-period. Results in column 2 of Table B.1 are similar to the main estimates in Tables 2.2 and 2.3.

I also estimate equations (2.1) and (2.2) with two alternative samples. First, I use a sample that is not restricted to students with at least ten schools in their choice sets. The new sample includes 2,632 additional students, the majority of which participated in the lottery in the 2015-16 school year. Second, I use a sample that drops all observations from the 2015-16 school year (808 students) because of the small choice set sizes in that year. Estimates in columns 3 and 4 in Table B.1 are similar in magnitude to the main estimates in Tables 2.2 and 2.3.

Lastly, I examine the proportion of students who ranked a first-choice school with a quality rating before and after the policy change. From a simple difference in means, a similar proportion of students ranked a first-choice school with a quality rating before (81 percent) and after (79 percent) the policy change.[13] Descriptively, these data suggest that the policy change itself did not induce more students to rank schools with an assigned tier as their first choice.[14]

2.8 Discussion and Conclusion

Centralized systems of school choice are increasingly the way in which families choose schools in large urban centers. These systems simplify the assignment process and have the potential to increase equity by improving historically underserved families' access to high-quality schools. Yet parent participation may nonetheless be constrained by limited access to their preferred schools or asymmetric information.

This paper leverages a district-wide policy shock to estimate the effect of changes to constraints in the school choice process on the quality of parents' first-choice schools. The policy shock changed the composition of students' choice sets and provided families with district-determined school quality ratings for the first time. I consequently examine two mechanisms that may have affected parents' school choices: changes in access to high-quality schools and changes in access to school quality information.

First, I find that increasing access to high-quality schools drove selection of those schools,

[13]This statistic uses the full population of 28,518 in- and out-of-sample K1 and K2 students.

[14]I also reconstruct the average choice set quality measure to include choice set data from students who did not rank a school with a quality rating as their first-choice school. This check mitigates concerns that these students' choices are a function of a difference in their choice sets. Results are not sensitive to the inclusion of these students.

particularly for families who previously lacked access. This result largely applied to Black and Hispanic families in the district. Second, I find that increasing choice set quality induced even higher quality first-choice schools after parents had access to school tier ratings. This effect was the largest for White families, and not detectable for Black families. Lastly, I find that increasing access to high-quality schools increased the quality of the schools to which students were assigned at the end of the lottery and the schools in which they subsequently enrolled. School choice outcomes can thus improve as options decrease, as the policy shock also reduced the number of schools in students' choice sets.

The findings in this study underscore the role of school quality in parent decision-making, as parents choose higher quality schools as these options become available. This paper also has important implications for the construction and communication of school quality information. The school ratings leveraged in this paper were based on school-level academic performance in reading and math, which may offer a limited understanding of school quality. Yet, as shown, summary school performance measures had a significant effect on demand-side pressures for schooling. Beyond education, school ratings have also been shown to affect housing values and the demographic composition of communities (Figlio and Lucas, 2004; Hasan and Anuj Kumar, 2019). The creation and dissemination of salient school quality information thus has reverberating consequences that go beyond school choice.

Chapter 3

Unpacking the Black Box of Tastes: Altruism and the Gender Pay Gap

3.1 Introduction

Tomorrow's income is the canonical motivation for human capital investment decisions today. Seminal work by Becker, 1962 focuses on the investment activities that "influence future real income" through the "imbedding of resources in people." Although earnings remain important, there is increasing interest in other determinants of human capital investments, particularly preferences for job attributes that may impact one's day-to-day work experience and/or quality of life.

One reason for this growing attention is because student preferences or tastes better explain major choice (Arcidiacono, 2004; Beffy, Fougère, and Maurel, 2012; Wiswall and Zafar, 2015) and major sorting by gender (Gemici and Wiswall, 2014; Turner and Bowen, 1999) than expected earnings and ability. Yet major choice remains remains a key determinant of future wages (Altonji, Blom, and Meghir, 2012; Hastings, Neilson, et al., 2016; Kirkebøen, Leuven, and Mogstad, 2014). Further, not only do college-educated women sort into majors that systemically lower their potential earnings relative to men, even when men and women sort into the same major, women sort into occupations with lower potential wages (Sloane, Hurst, and Black, 2021).

An emerging body of literature consequently examines the non-pecuniary job characteristics that influence key human capital decisions and expected income. Studies find that students, particularly women, consider factors such as work flexibility, job stability, and future family life when choosing a college major in anticipation of their future occupation (Bronson, 2015; Zafar, 2013; Wiswall and Zafar, 2018; Wiswall and Zafar, 2021). These preferences are then borne out in job choices and measurable salary differences (Blau and Kahn, 2017; Daymont and Andrisani, 1984; Fortin, 2008; Sloane, Hurst, and Black, 2021).[1] Indeed, the substantial and enduring gender wage gap underscores the need for more research to understand what drives pre-market investments as well as labor market decisions (Sloane,

[1]Other work examines traits such as competitiveness (Manning and Swaffield, 2008; Reuben, Wiswall, and Zafar, 2017), locus of control (Nyhus and Pons, 2012; Semykina and Linz, 2007), and the Big Five personality traits (Mueller and Plug, 2006) in accounting for the gender wage gap.

Hurst, and Black, 2021).

In this paper, I use data on first-time bachelor's degree-seeking traditional college students from the 2012/17 Beginning Postsecondary Students Longitudinal Study to describe the relationship between workplace preferences and college majors, occupations, and the gender pay gap. I focus on the set of five surveyed job attributes that students reported preferences for relative to salary, including (1) balancing work and family, (2) balancing work and leisure, (3) being an expert, (4) helping others, and (5) making decisions. I find that workplace preferences were not constant over time. Rather, they became relatively less important than salary as students approached the workforce. Preferences for work/family balance far outpaced the other four job attributes, but the gender disparity was largest for preferences for helping others, which a significantly greater proportion of women preferred than men. Next, I find that preferences for helping others were positively associated with female-dominated majors and occupations (e.g., education and health care fields) and negatively associated with male-dominated majors and occupations (e.g., business and STEM fields). Third, I find that preferences for helping others accounted for 12 percent of the early-career gender wage gap, conditional on occupation and major controls.

This paper makes two main contributions. First, I add to the body of work on human capital investments and non-pecuniary job attributes, such as family, job flexibility, and job satisfaction (Wiswall and Zafar, 2018; Wiswall and Zafar, 2021; Zafar, 2013). I examine job attributes across several dimensions, such as altruism and job flexibility. Preferences for these attributes are reported relative to salary, which makes them particularly well-suited for analyses of their role in human capital investments. I also draw data from a national study; extant research in the field commonly use data from one university, which is often a selective private institution (e.g., NYU) (Arcidiacono et al., 2020; Wiswall and Zafar, 2015; Wiswall and Zafar, 2018; Wiswall and Zafar, 2021; Zafar, 2013).

Second, I contribute to the literature on the gender pay gap. Selected studies find that differences in psychological traits account for approximately three to 30 percent of the gender pay gap (see Blau and Kahn (2017) for a review). These papers examine a variety of factors, such as the the Big Five personality traits, self-esteem, risk, and competitiveness. Most closely related to the workplace preferences I examine in this study are those in Fortin (2008), which examines composite variables for the importance of money/work and the importance of people/family in addition to self-esteem and locus of control. Fortin (2008) finds that the importance of money/work is the noncognitive factor that plays the largest role in the gender wage gap, accounting for roughly 5 percent among young adults. I find that preferences for helping others as a part of one's job fully explain the role of workplace preferences in the early-career gender pay gap. This finding is largely explained by the fact that women chose lower-paying jobs within the same field where helping others was an important activity or personal characteristic to perform the job. This result underscores Wiswall and Zafar (2018), which finds that gender differences in preferences for earnings, work hours, and work flexibility cause women to choose systemically different jobs within the same field, accounting for at least a quarter of the gender gap in expected early-career earnings.

The remainder of the paper is organized as follows. In the next section, I describe the data along with respondents' workplace preferences for the five non-pecuniary job attributes, college majors, and occupations in 2017. I also describe how workplace preferences relate to choice of major and occupation. In Section 3.3, I decompose the gender wage gap using these factors. Section 3.4 concludes.

3.2 Data and Descriptive Statistics

I use restricted-use data from the 2012/17 Beginning Postsecondary Students Longitudinal Study (BPS:12/17) conducted by the National Center for Education Statistics (NCES). BPS:12/17 is the second follow-up of students who began postsecondary education in the 2011–12 academic year. The survey draws from the 2011–12 National Postsecondary Student Aid Study (NPSAS:12) to create a nationally representative sample of first-time beginning undergraduate students. Students were surveyed three times, beginning in their first year of postsecondary education and followed up at the end of their third and sixth years after entry into postsecondary education. I use the BPS:12/17 panel weight for all analyses.[2]

I identify a sample of 8,320 traditional undergraduate students, defined as those who were younger than 24 years and pursuing bachelor's degrees at four-year institutions of higher education in the 2011-12 school year.[3] Just over half of students in the sample were female (57 percent) and nearly two-thirds were white (63 percent) (column 1 of Table 3.1). Nearly 60 percent of respondents' parents earned at least a bachelor's degree, and the average respondent income was approximately $90,000 in 2012. Nearly all students (96 percent) attended a public or private nonprofit first institution of higher education; 4 percent attended a private for-profit college or university. More than two-thirds of the sample (70 percent) earned a bachelor's degree by 2017. Five percent of students earned an associate's degree or a certificate, and 4 percent earned an associate's degree, a certificate, or both, in addition to their bachelor's degree.

[2]The BPS:12/17 panel weight was created for all BPS:12/17 respondents who also responded in the 2012/14 Beginning Postsecondary Students Longitudinal Study (BPS:12/14). The cross-sectional weight was created for BPS:12/17 interview respondents regardless of their NPSAS:12 or BPS:12/14 response status. The cross-sectional sample includes approximately 900 more respondents. Results are not meaningfully sensitive to choice of weight.

[3]The sample size is unweighted and rounded to the nearest 10.

Table 3.1: *Descriptive Statistics*

	Sample	Employed	Unemployed
Female	0.57	0.59	0.54
White	0.63	0.68	0.53
Black	0.12	0.10	0.16
Hispanic/Latino	0.13	0.11	0.15
Asian	0.07	0.06	0.10
Other minority	0.05	0.05	0.06
Age	18.42	18.38	18.49
Parent(s): at least bachelor's degree	0.58	0.59	0.54
Income in 2012 (in thousands)	91.43	97.15	81.60
Public or private nonprofit	0.96	0.97	0.95
Bachelor's degree	0.70	0.78	0.56
Associate's degree	0.03	0.02	0.05
Certificate	0.02	0.01	0.03
Multiple degrees	0.04	0.04	0.05
No degree	0.25	0.19	0.36
Employed	0.63	1.00	0.00
Observations	8320	5300	3020

Notes: This table reports descriptive statistics. The first column includes all respondents in the sample. The second and third columns include employed and unemployed respondents, respectively. Data (excluding N-sizes) are weighted using the BPS:12/17 panel weight and N-sizes are rounded to the nearest 10. "Multiple degrees" includes at least a bachelor's degree.

I organize the college majors that respondents pursued into six categories in Figure 3.1, including science, technology, engineering, and mathematics (STEM); business; education; health care; humanities and other; and undecided and undeclared.[4] These categories are based on the ten major fields of study of the bachelor's degree a respondent was pursuing when last enrolled as of June 2017.[5] The STEM category includes majors in the

[4]The literature on human capital investments and workplace preferences similarly organizes majors into broad categories. For example, Arcidiacono, 2004 organizes majors into four categories: natural science (including math and engineering), business (including economics), social sciences/humanities, and education. Wiswall and Zafar, 2015 and Wiswall and Zafar, 2018 organize majors into four categories: economics/business, engineering (or engineering/computer science), humanities/arts (or humanities/social sciences), and natural sciences or (natural sciences/math). I separate out education and health care fields, which are predominately female-dominated majors, to more acutely describe gender differences in the relationship between workplace preferences and major choice.

[5]A small proportion (less than 2 percent) of respondents attained more than one bachelor's degree by this date. Students who did not formally declare a major are included. The categories use the NCES major classification scheme, which is based on the U.S. Department of Education's Classification of Instructional Programs (CIP 2010).

fields of computer and information sciences; engineering and engineering technology; and biological and physical sciences, science technology, mathematics, and agriculture. The business, education, and health care categories include majors in those respective fields. The humanities and other category includes majors in the fields of humanities, social sciences, general studies and other, and other applied.

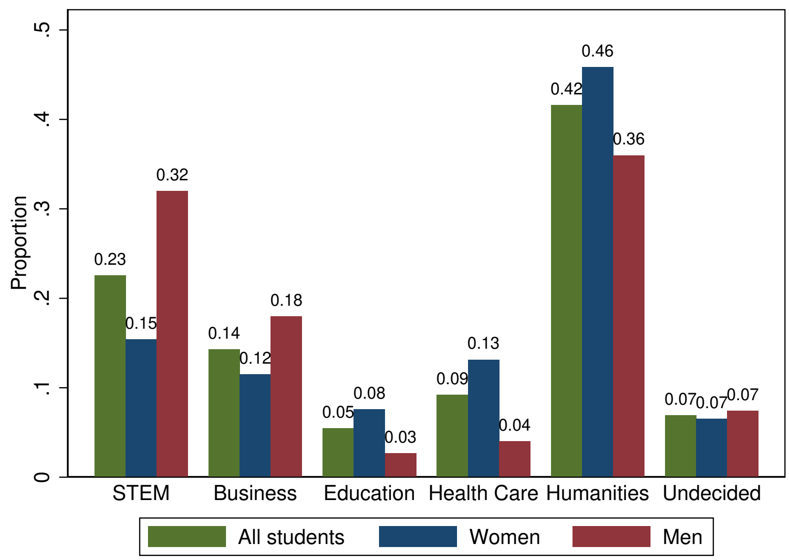


Figure 3.1: *Major Choice*

Notes: This figure includes all respondents in the sample. The six categories are based on the ten major fields of study of the bachelor's degree a respondent was pursuing when last enrolled as of June 2017. Data are weighted using the BPS:12/17 panel weight.

Figure 3.1 shows that the largest proportion of students pursued a degree in humanities/other (42 percent), followed by STEM (23 percent) and business (14 percent). Men were more than twice as likely than women to pursue a degree in STEM and 1.5 times as likely to pursue a degree in business. On the other hand, women were more than twice as likely than men to pursue a degree in education and three times as likely to pursue a degree in a health care field. Women also outpaced men in the humanities/other field by 10 percentage points. There was relative parity in the proportion of women and men who were undecided.

Figure 3.2 shows the degree or certificate attainment and enrollment of respondents over the six years. Attainment peaked in the 2014-15 academic year, four years after students first enrolled in a bachelor's degree program, with 44 percent earning a degree or certificate. Approximately 20 percent of students did not earn a degree that year and were either enrolled for less than 8 months (6 percent) or not enrolled (11 percent). The remaining 39 percent of students were still working toward a degree. Degree attainment dropped to 22 percent in the 2015-16 school year and to 9 percent in the 2016-17 school year. Trends in attainment and enrollment were similar for women and men, with more women earning a degree each year through the 2014-15 academic year, after which men slightly outpaced women (see Figure C.1).

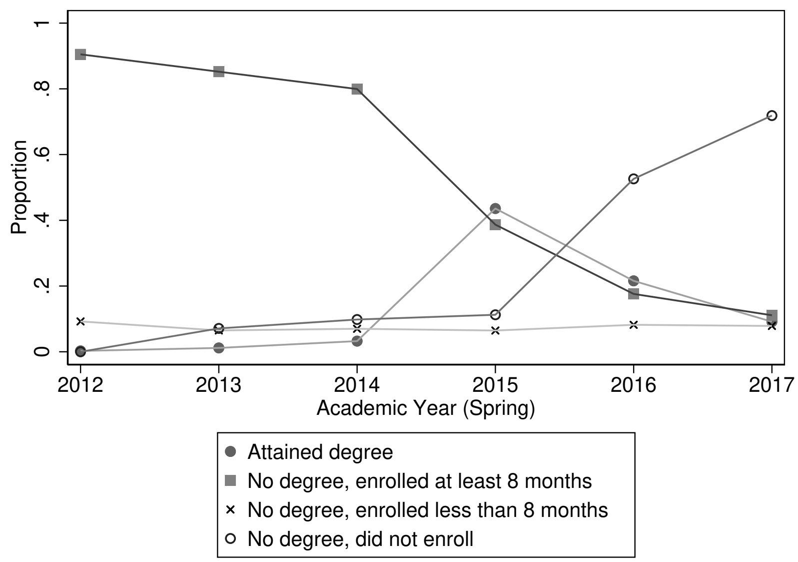


Figure 3.2: *Degree Attainment and Enrollment Over Time*

Notes: This figure includes the full sample of respondents. Data are weighted using the BPS:12/17 panel weight.

Figure 3.3 visualizes the proportion of highest degrees attained at the end of the six years. More women (72 percent) than men (66 percent) earned a four-year degree. Similar proportions of men and women (less than 5 percent) earned an associate's degree or certificate as their highest degree. A larger proportion of men (29 percent) than women (22 percent) did not earn any degree. Among students who never earned a degree or certificate, the proportion of students who dropped out steadily increased over time. The proportion of women and men who dropped out followed similar trajectories, with a slightly larger proportion of men reporting to neither be enrolled nor have earned a degree in the academic years prior to 2014-15 (see Figure C.2). By the 2016-17 academic year, there was relative gender parity, with 54 percent of male dropouts and 57 percent of female dropouts neither earning a degree nor enrolling that year.

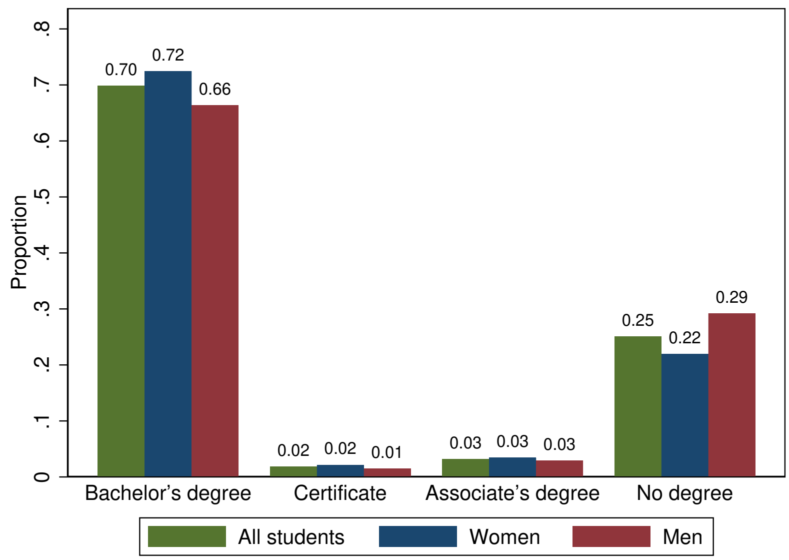


Figure 3.3: *Highest Degree Attained through June 2017*

Notes: This figure includes all respondents in the sample. Data are weighted using the BPS:12/17 panel weight.

In the final survey year, nearly two-thirds (63 percent) of respondents reported that they were employed at any time from February to June 2017 (see column 1 of Table 3.1). Of the remaining respondents in the sample, approximately half were neither employed during this time frame nor enrolled, and the other half was still enrolled in a degree program after January 2017. I group these respondents together as "unemployed" in column 3 of Table 3.1. A larger proportion of respondents who were employed (column 2 of Table 3.1) were female and earned a bachelor's degree compared to respondents who were unemployed. Of those who were unemployed, respondents were more likely to not have earned any type of degree.

I organize respondents' occupations into six categories in Figure 3.4, including STEM, business, education, health care fields, all other fields, and unemployed.[6] These categories are based on the 2010 Standard Occupation Classification (SOC) System, which organizes occupations into 23 groups. I define STEM occupations as those in computer, engineering, and science; architecture and engineering; and life, physical, and social science.[7] I include healthcare practitioners and technical operations occupations and healthcare support occupations in health care fields.[8] I define business occupations as those in business and financial operations. I include remaining occupations in the all other fields category.[9] I also identify the geographic location of respondent's employers by matching employer zip codes to U.S. Census region and metropolitan area using data from the U.S. Census Bureau and the American Community Survey 5-year estimates. I also bring in work characteristics from the O*NET database and national occupational annual mean wage estimates from 2017 from the U.S. Bureau of Labor Statistics (BLS) in Section 3.3.

[6] I organize occupations in this way to echo the five categories for college major. These categories indicate the occupations that respondents had in their current or most recent job as of 2017.

[7] I include four additional occupations – two from management occupations and one from sales and related occupations – that are coded as STEM in the 2010 Census occupation classification system, including computer and information systems managers, architectural and engineering managers, natural science managers, and sales engineers. I do not exclude the one occupation from the architecture and engineering occupations that is coded as STEM-related.

[8] I include one additional occupation from management occupations – medical and health services managers – that is coded as STEM-related in the 2010 Census occupation classification system.

[9] These occupations include those in management; community and social service; legal; arts, design, entertainment, sports, and media; protective service; food preparation and serving related; building and grounds cleaning and maintenance; personal care and service; sales and related; office and administrative; farming, fishing, and forestry; construction and extraction; installation, maintenance, and repair; production, transportation and material moving, and military specific.

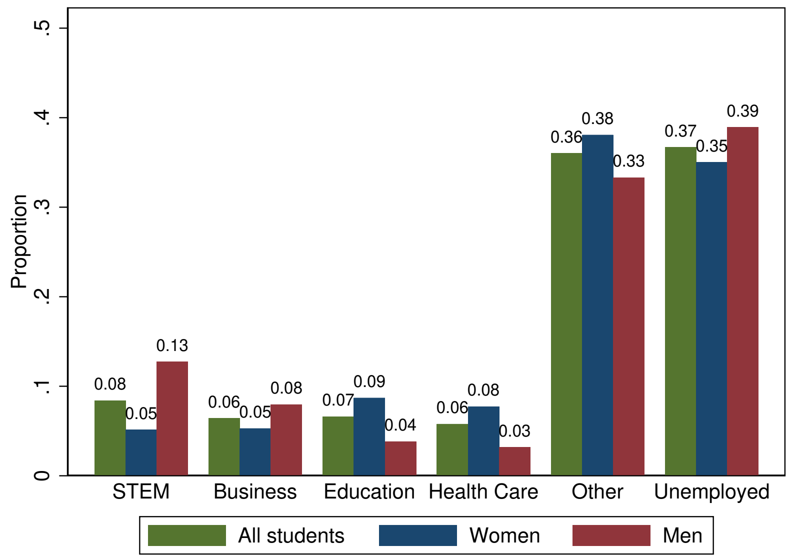


Figure 3.4: *Occupational Choice*

Notes: This figure includes all respondents in the sample. The six categories are based on the 23 occupations of respondents' current or most recent job as of 2017. Data are weighted using the BPS:12/17 panel weight.

Figure 3.4 shows that there was relative parity in the proportion of respondents who were employed in STEM, business, education, and health care fields (6-8 percent). However, men were more than twice as likely than women to be employed in a STEM occupation, whereas women were more than twice as likely to work in education or health care fields. Men also outpaced women in business occupations. These gender patterns across occupations largely mirrored those across majors, with a greater proportion of women in education and health care fields and a greater proportion of men in STEM and business fields. More than one-third of all respondents (36 percent) chose an occupation in the all other fields category.

As respondents made decisions about major, enrollment/degree attainment, and employment, they also reported their preferences for a handful of non-monetary job attributes relative to salary. These attributes included (1) balancing work and family, (2) balancing work and leisure time, (3) being seen as an expert in your field, (4) helping others as part of your job, and (5) making your own decisions about how to get your work done. Reporting a job attribute as less important than salary was coded as a 1, as important as salary was coded as a 2, and more important than salary was coded as a 3.[10]

Table 3.2 reports the pairwise correlation coefficients between students' baseline workplace preferences in 2012. The coefficients, which range in magnitude from 0.104 to 0.388, indicate that there are low to moderately strong relationships between preferences for job attributes.[11] That no two attributes are strongly correlated is particularly helpful in this context, as it suggests that the attributes successfully capture different dimensions of students' workplace preferences. The highest degree of correlation is between balancing work/family and balancing work/leisure, which both measure time demands. The lowest degrees of correlation are between being an expert and both balancing work/family and balancing work/leisure.

Table 3.2: *Workplace Preference Correlations*

	Work/Family	Work/Leisure	Being an Expert	Helping Others	Making Decisions
Work/Family	1.000				
Work/Leisure	0.388	1.000			
Being an Expert	0.104	0.131	1.000		
Helping Others	0.228	0.156	0.252	1.000	
Making Decisions	0.178	0.328	0.395	0.220	1.000

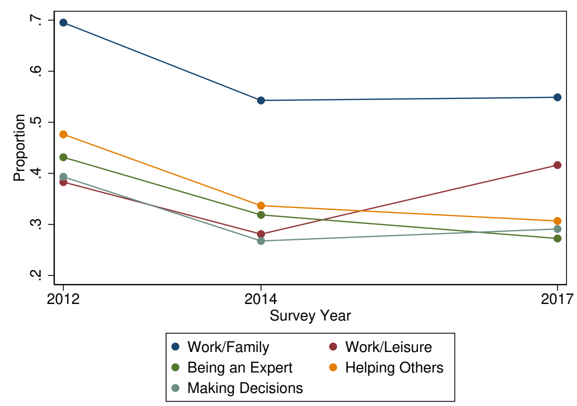
Notes: This table reports pairwise correlation coefficients for student workplace preferences in 2012 for all respondents in the sample. Data are weighted using the BPS:12/17 panel weight.

Figure 3.5 plots the proportion of students who reported a job attribute to be more important than salary over time. The largest proportion of students reported balancing work/family to be more important than salary in 2012 (70 percent), followed by helping

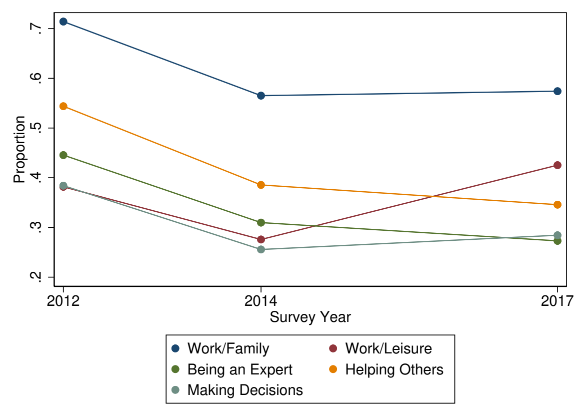
[10]Table C.1 reports the proportion of imputed responses. Results for the wage decomposition analysis are similar in magnitude to the reported findings in Section 3.3 when I restrict the sample to respondents whose workplace preferences are not imputed in 2012 and 2014. This subsample of respondents also has similar observable characteristics to the full sample.

[11]To compute the pairwise coefficients, I treat the five variables for workplace preferences as continuous variables. Results are similar if I compute the pairwise correlations using the five dummy variables for workplace preferences that are "more important than salary".

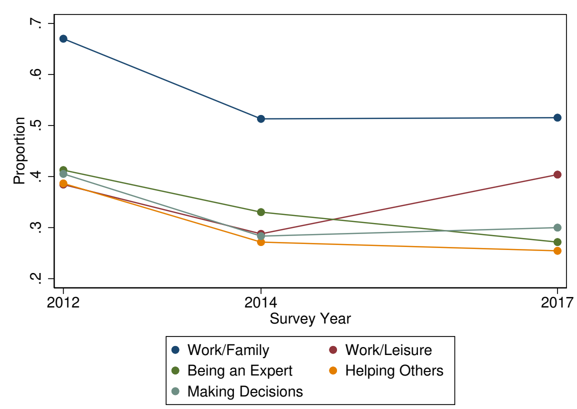
others (48 percent), being an expert (43 percent), making decisions (39 percent), and balancing work/leisure (38 percent). The proportion of students who reported each attribute to be more important than salary decreased between 2012 and 2017, except for work/leisure balance, suggesting that salary became more important to respondents as they approached the workforce. However, a sizable gap of approximately 20 percentage points remained between balancing work/family and the other workplace preferences, on average, by the end of the study.



(a) *All Students*



(b) *Women*



(c) *Men*

Figure 3.5: *Workplace Preferences Over Time (More Important Than Salary)*

Notes: This figure includes all respondents in the sample and plots the proportion of respondents who reported a job attribute to be more important than salary in each survey year. Data are weighted using the BPS:12/17 panel weight.

Figure 3.5 also shows that workplace preferences trended similarly for men and women, except for a notable difference in the proportion of men and women who reported helping others to be more important than salary. In 2012, 54 percent of women reported helping others to be more important than salary, compared with 39 percent of men. The gap narrowed over time but nonetheless persisted as the biggest difference in workplace preferences between women and men. By 2017, 35 percent of women reported helping others to be more important than salary, compared with 25 percent of men.

Another way to quantify gender differences in workplace preferences is with Duncan-Duncan segregation indices, which measure the share of women that would have had to change their preferences to equate the distributions between women and men for a given job attribute. I compute

$$D_j = \frac{1}{2}\sum |f_i - m_i|, \tag{3.1}$$

where f_i and m_i are the shares of women and men, respectively, with preference $i \in$ {less important than salary, as important as salary, more important than salary} for job attribute j. The index would be 0 if there were no differences in preferences and 1 if there was complete segregation. Figure 3.6 shows that the magnitude of the index between women and men is the largest for helping others (an average of 0.12), followed by balancing work and family (an average of 0.05). As a result, even though a greater proportion of students reported balancing work/family to be more important than salary relative to the other job attributes, there was greater gender segregation among preferences for helping others. The indices for the other attributes are relatively smaller in magnitude.

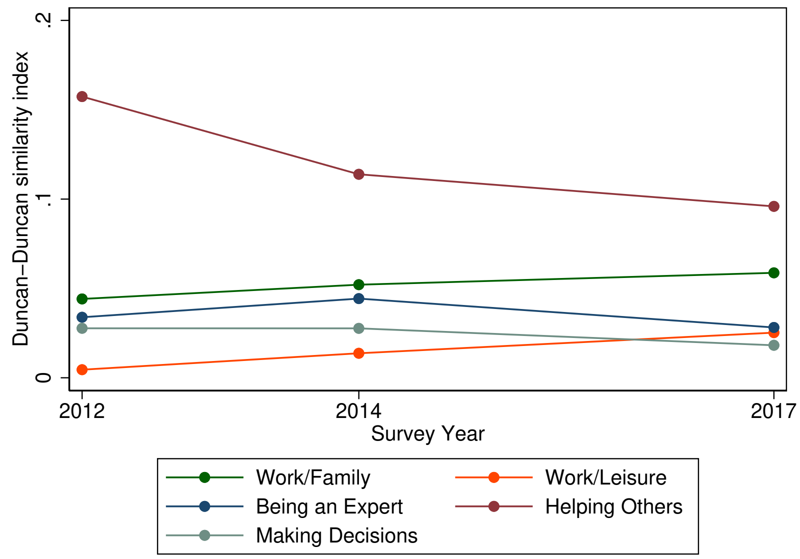
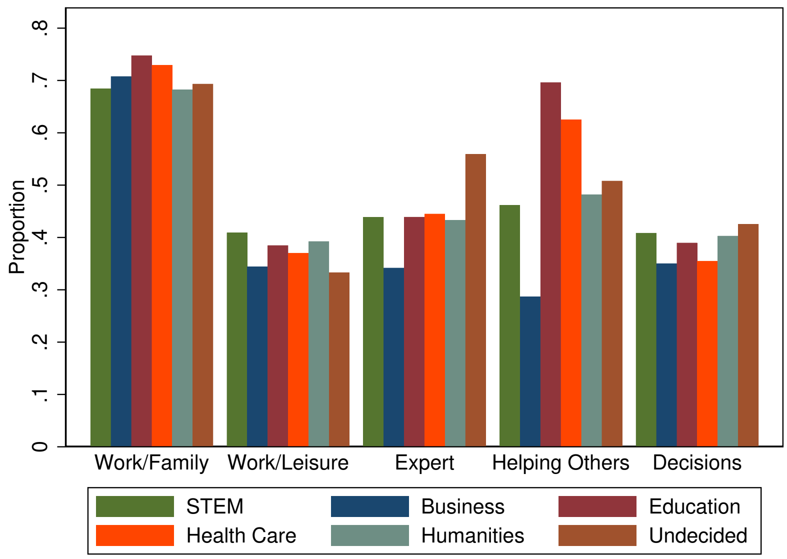


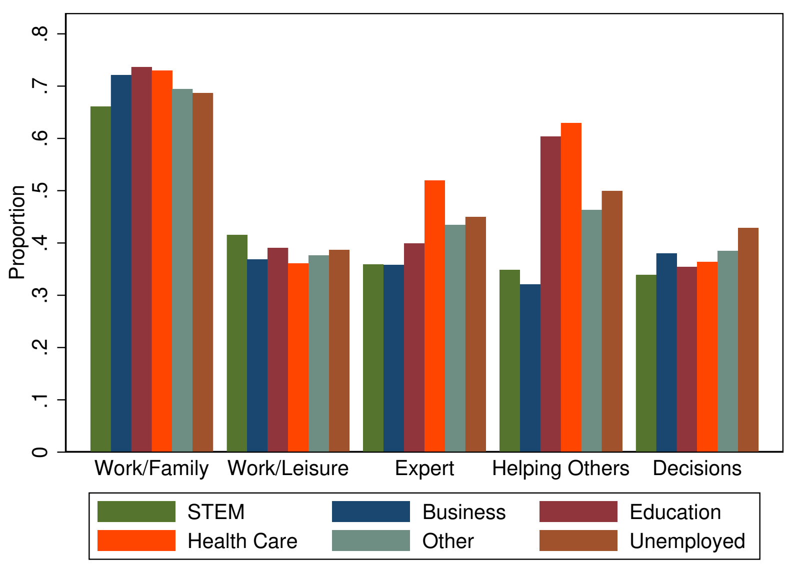
Figure 3.6: *Duncan-Duncan Dissimilarity Index for Workplace Preferences*

Notes: This figure includes all respondents in the sample. Data are weighted using the BPS:12/17 panel weight.

Reported baseline preferences for helping others were also associated with respondents' choice of college major and occupation. Figure 3.7 shows that, among the five job attributes, there was the greatest variation in preferences for helping others across majors. The smallest proportion of respondents who chose a business major (29 percent) reported helping others to be more important than salary in 2012. On the other hand, the largest proportion of respondents who chose a major in education or a health care field (70 and 63 percent, respectively) reported helping others to be more important than salary. This pattern was similar for respondents' occupations in 2017. Fewer respondents who chose a business or STEM occupation (32 and 35 percent, respectively) reported helping others to be more important than salary, compared to a larger proportion of respondents in education and health care fields (60 and 63 percent, respectively).



(a) *Majors*

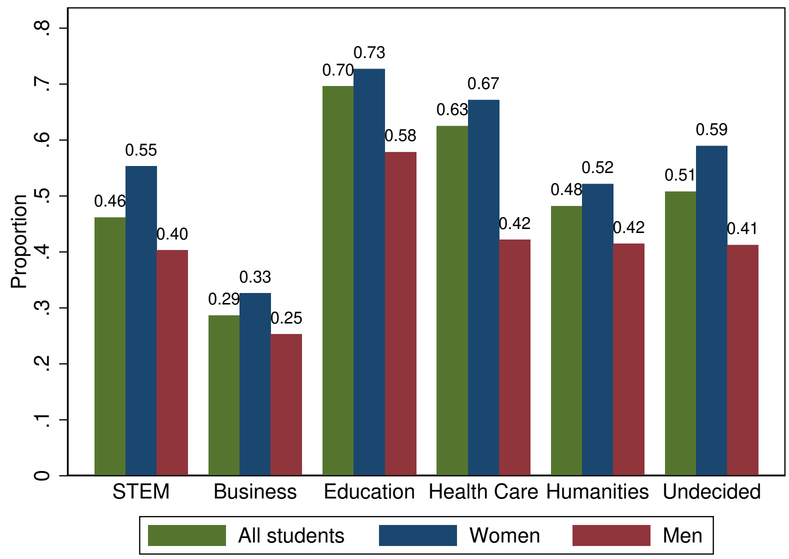


(b) *Occupations*

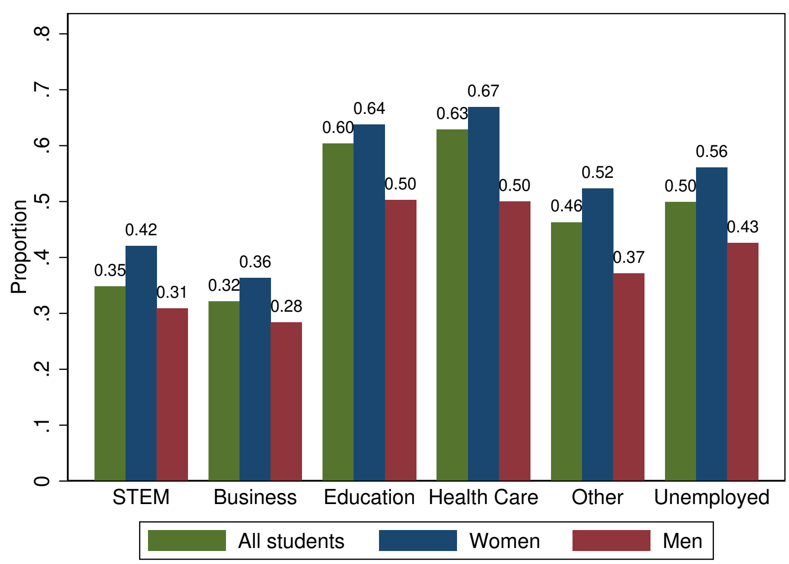
Figure 3.7: *Major/Occupational Choice and Workplace Preferences (More Important Than Salary)*

Notes: This figure includes all respondents in the sample and plots the proportion of respondents who reported a job attribute to be more important than salary in 2012 by major or occupation. Data are weighted using the BPS:12/17 panel weight.

Figure 3.8 disaggregates the relationship between preferences for helping others and major/occupation by gender, showing similar patterns for both women and men. This finding suggests that preferences for helping others were positively associated with entry into female-dominated fields and negatively associated with entry into male-dominated fields for all respondents. As a result, even though there were larger absolute gender differences in preferences for helping others, as shown in Figure 3.6, these differences did not necessarily lead to gender segregation across major and occupational fields. However, gender differences in workplace preferences may still explain a portion of the gender pay gap if they are related to segregation within fields. I explore this question in the next section.



(a) *Majors*



(b) *Occupations*

Figure 3.8: *Major/Occupational Choice and Workplace Preferences for Helping Others (More Important Than Salary)*

Notes: This figure includes all respondents in the sample and plots the proportion of respondents who reported helping others to be more important than salary in 2012 by major or occupation. Data are weighted using the BPS:12/17 panel weight.

3.3 Decomposition of the Gender Wage Gap

In this section, I use the Oaxaca-Blinder (OB) decomposition method to estimate how much of the mean difference in wages between female and male respondents is accounted for by gender differences in workplace preferences, conditional on major and occupational controls. The OB method, which is commonly used to decompose the mean differences in wages between two groups, is based on linear regression models.[12] Below, the model for men is indexed by M and the model for women is indexed by W,

$$Y_M = X_M'\beta_M + \epsilon_M \quad \text{and} \quad Y_W = X_W'\beta_W + \epsilon_W. \tag{3.2}$$

The outcome, Y, is respondents' wages in 2017; X is a vector containing workplace preferences and other predictors, and a constant; and β contains the slope parameters and intercept. The mean difference in wages between men and women is

$$\Delta E(Y) = E(Y_M) - E(Y_W) = E(X_M)'\beta_M - E(X_W)'\beta_W \tag{3.3}$$

which can be rearranged to a threefold decomposition[13]

$$\Delta E(Y) = \{E(X_M) - E(X_W)\}'\beta_W + E(X_W)'(\beta_M - \beta_W) + \{E(X_M) - E(X_W)\}'(\beta_M - \beta_W) \tag{3.4}$$

from the perspective of women. The first component of equation (3.4), $\{E(X_M) - E(X_W)\}'\beta_W$, is the differential that is due to gender differences in predictors (the "endowment effect"). In other words, the endowment effect is the mean change in women's wages if they had the values of the explanatory variables of men. The second component, $E(X_W)'(\beta_M - \beta_W)$,

[12]Another popular decomposition method is the Kitagawa method. Oaxaca and Sierminska (2023) distinguishes between the OB and Kitagawa methods, stating that the Kitagawa method is a special case of the OB methodology when the dependent outcome variables are binary, the covariates are categorical variables, and the OB decomposition uses the OLS estimated linear probability model.

[13]Alternatively, the twofold decomposition expresses the difference between mean outcomes using two terms. The first term is the same as in the threefold approach, and it captures the portion of the difference in means that is attributable to the explanatory variables (the "explained portion"). The second term captures the differences in the coefficients (the "unexplained portion"). The interaction term is not separated from the endowment and coefficients.

measures the contribution of differences in the coefficients, or the mean change in women's wages when applying men's coefficients to women's predictors (the "coefficient effect"). The third component, $\{E(X_M) - E(X_W)\}'(\beta_M - \beta_W)$, measures the simultaneous effect of differences in endowments and coefficients (the "interaction") (Jann, 2008).[14]

To decompose the wage gap, I restrict the sample to wage earners in 2017 (see column 2 of Table 3.1). For the predictors, I use workplace preferences from 2012 and 2014 to avoid the reverse causality problem between reported preferences in 2017 and wages. Further, given the close proximity between when preferences and wages were measured, preferences for these non-pecuniary job factors are relatively unaffected by direct feedback from the labor market (Fortin, 2008). I include a set of dummy variables for preferences for each job attribute relative to salary (as important as salary, same as salary, more important than salary) in 2012 and 2014. I also control for the following: demographics (race/ethnicity, age, parental education, and income in 2012), education (GPA, field of study,[15] and degree attainment), work experience (years of work experience prior to 2012 and employment status in 2014), occupation in 2017,[16] and location of employer.[17]

Table 3.3 reports the summary results of the decomposition and Figure 3.9 visualizes these results with approximate standard errors (Jann, 2008).[18] The weighted mean of log wages is 10.373 for men and 10.201 for women, yielding a wage gap of 0.172 log points. Differences in endowments explain the largest portion of the wage gap (0.135 log

[14] I use the oaxaca command in Stata (Jann, 2008) to execute the analysis.

[15] I include a set of dummy variables for the 23 fields of study that respondents were pursuing when last enrolled in a bachelor's degree program as of June 2017. Students who did not formally declare a major are included.

[16] I include a set of dummy variables for the 23 occupations that respondents had in their current or most recent job as of 2017.

[17] Following Blau and Kahn (2017), I exclude marital status and children from the analysis because these are likely to be endogeneous with respect to women's labor force decisions. These variables are also expected to increase male wages but decrease female wages. Results are similar when I include these variables in the decomposition.

[18] Etezady et al. (2021) note that most studies using this method do not include statistical test results. One reason may be because we cannot equate a decomposition term that has low statistical significance to zero, otherwise the terms would not sum to the total gap.

points), or 78 percent. The coefficients term accounts for a narrowing of the wage gap by 8 percent (-0.014 log points) and the interaction of the endowments and coefficients explains approximately 30 percent (0.05 log points) of the wage gap.

Table 3.3: *Decomposition of the Gender Wage Gap*

	Log Points	Share of the Gap
Mean log wages: Women	10.201	
Mean log wages: Men	10.373	
Gender wage gap	0.172	100%
Endowments	0.135	78.49%
Occupation	0.054	31.40%
Education	0.058	33.72%
Workplace Preferences	0.018	10.47%
Demographics	-0.001	-0.58%
Work Experience	0.002	1.16%
Region	0.003	1.74%
Coefficients	-0.014	-8.14%
Occupation	-0.099	-57.56%
Education	-0.088	-51.16%
Workplace Preferences	-0.057	-33.14%
Demographics	-0.405	-235.47%
Work Experience	-0.122	-70.93%
Region	0.020	11.63%
Intercept	0.738	429.07%
Interaction	0.050	29.1%
Occupation	0.070	40.70%
Education	-0.030	-17.44%
Workplace Preferences	-0.000	-0.00%
Demographics	0.002	1.16%
Work Experience	0.005	2.9%
Region	0.003	1.74%
Observations		
All	5300	
Women	3140	
Men	2170	

Notes: This table reports the mean of log wages for female and male respondents who reported employment in 2017. The gap in wages is then decomposed into three components: endowments, coefficients, and the interaction between the two. See Section 3.3 for more details. Data (excluding N-sizes) are weighted using the BPS:12/17 panel weight and N-sizes are rounded to the nearest 10. Parts may not sum to totals due to rounding.

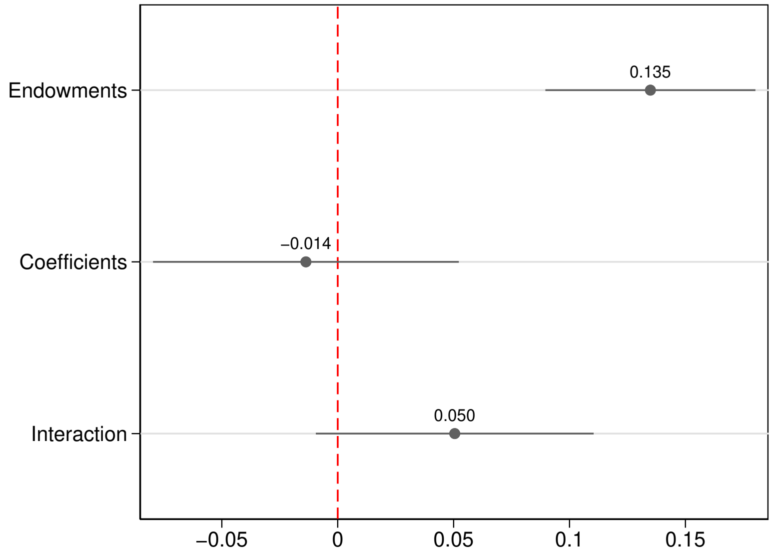


Figure 3.9: *Wage Decomposition*

Notes: This figure includes the respondents in the sample who were employed in 2017. The horizontal lines denote the 95 percent confidence intervals. Data are weighted using the BPS:12/17 panel weight.

I am chiefly interested in the endowment effect, which describes the mean change in women's wages if women had the same characteristics as men, or how much of the wage gap is accounted for by differences in women and men's predictors. Table 3.3 and Figure 3.10 show that gender differences in occupation (0.054 log points) and education (0.058 log points) explain approximately 80 percent of the endowment effect and two-thirds of the wage gap. Workplace preferences (0.018 log points) also explain a sizable portion of both the endowment effect (13 percent) and the wage gap (10 percent). Figure 3.11 further disaggregates how each of the five workplace preferences contributes to the endowment effect, showing that differences in preferences for helping others (0.020 log points) fully explain the contribution. We would therefore expect the wages of women to increase if their preferences for helping others relative to salary matched men's, closing the wage gap by as much as 12 percent.

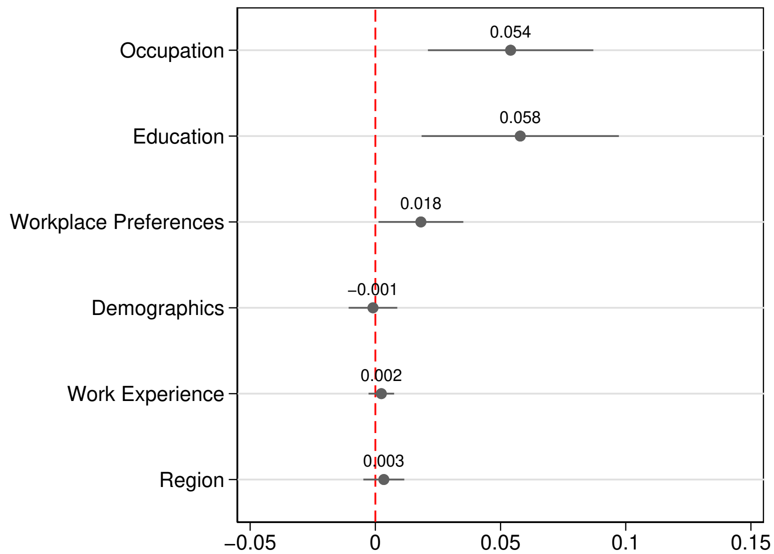


Figure 3.10: *Wage Decomposition (Endowments)*

Notes: This figure includes the respondents in the sample who were employed in 2017. The horizontal lines denote the 95 percent confidence intervals. Data are weighted using the BPS:12/17 panel weight.

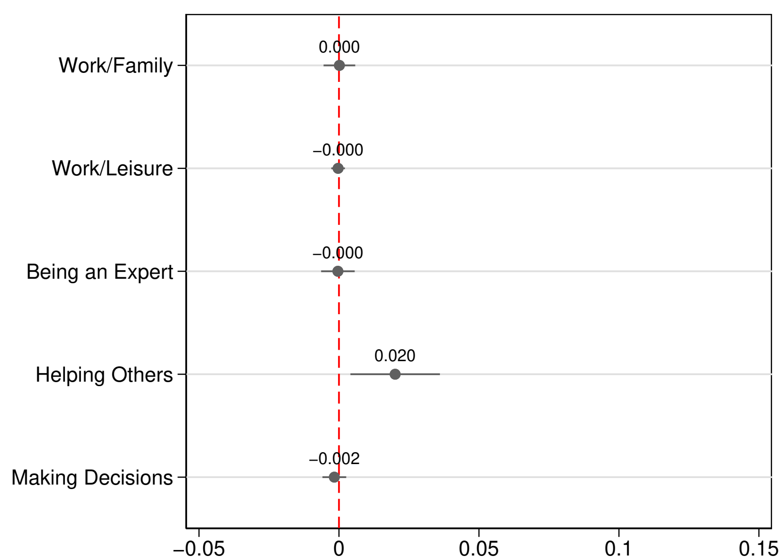


Figure 3.11: *Wage Decomposition (Endowments: Workplace Preferences)*

Notes: This figure includes the respondents in the sample who were employed in 2017. The horizontal lines denote the 95 percent confidence intervals. Data are weighted using the BPS:12/17 panel weight.

The results from the wage decomposition indicate that, even conditional on major and occupation, women's preferences for helping others as a part of their job explain a significant portion of the wage gap early in their careers. Given this finding, I posit that women chose jobs within fields that were more likely to be in service of others and yield lower wages. To test this hypothesis, I create an index that measures the extent to which a job involves helping others. I take the average of the "importance" rating of two work characteristics from O*NET for respondents' six-digit occupation codes,[19] including:

1. Concern for others: Job requires being sensitive to others' needs and feelings and being understanding and helpful on the job.

2. Assisting and caring for others: Providing personal assistance, medical attention, emotional support, or other personal care to others such as coworkers, customers, or patients.

O*NET classifies the first item as a work style, which is a personal characteristic that can affect how well someone performs a job. The second item is a work activity, or general type of job behavior. I then estimate

$$Y_i = \alpha + \beta Female_i + \lambda_o + \epsilon_i, \tag{3.5}$$

where Y_i is the standardized helping others index for student *i's* occupation in 2017, $Female_i$ is a dummy indicator for sex, and λ_o is a set of fixed-effects for the 23 occupational categories. $\hat{\beta}$ is the difference in the average value of the helping others index for women's occupations relative to men's, conditional on occupation category. The analysis includes the subsample of respondents who reported a detailed occupation code.

The estimate in the first column of Table 3.4 indicates that, on average, women chose jobs that were higher on the helping index by 0.09 standard deviations. The relationship is slightly smaller in magnitude (0.05 standard deviations) and marginally significant in

[19] I use the O*NET SOC 2010 to SOC 2019 crosswalk to match occupations in the NCES data to job characteristics from the O*NET database. I take the average of the "importance" rating for each matched pair and then collapse the data by detailed occupation code.

the second column after controlling for the student-level covariates used in the wage decomposition, including demographics, education, and work experience.[20] This sorting translates into lower earnings. Column three reports results from equation (3.5) where Y_i is national occupational annual mean wage estimates in 2017 from BLS, and the helping others index is included on the right-hand side. A one standard deviation increase in the helping others index is associated with a roughly $5,600 decrease in annual mean wages, conditional on occupation, sex, and other student-level covariates. Women's preferences for helping others thus led to lower-paying jobs within the same field that involved concern, assisting, and caring for others, contributing to the early-career gender pay gap.

Table 3.4: *Results for Helping Others Index*

	(1) Helping Others Index	(2) Helping Others Index	(3) Annual Mean Wages
Female	0.094^{***}	0.050^{*}	-401.215
	(0.029)	(0.029)	(933.096)
Helping Others Index			-5581.246^{***}
			(829.148)
Covariates	No	Yes	Yes
N	4150	4150	4010

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports results from an OLS model. The outcome in the first two columns is the helping others index (see Section 3.3 for details). The outcome in the third column is annual mean wages. In the first two columns, the sample is restricted to employed respondents with a detailed occupation code. In the third column, the sample is restricted to employed respondents whose detailed occupation code matched to occupational wage estimates from the U.S. Bureau of Labor Statistics. Data (excluding N-sizes) are weighted using the BPS:12/17 panel weight and N-sizes are rounded to the nearest 10.

[20]The coefficient is similar in magnitude and remains statistically significant at the 5 percent level when using the BPS:12/17 cross-sectional weight, which includes approximately 900 more respondents.

3.4 Conclusion

In this paper, I examine the relationship between workplace preferences and human capital investments. I use longitudinal survey data on first-time college-going students from BPS:12/17 to explore how workplace preferences change over time and how they relate to college majors, occupations, and the gender wage gap. I examine a set of five job attributes for which students report their preferences relative to salary, including (1) balancing work and family, (2) balancing work and leisure, (3) being an expert, (4) helping others, and (5) making decisions.

First, I find that workplace preferences became relatively less important than salary as students progressed through college and entered the workforce. Preferences for work/family balance over salary far outpaced the other four job attributes, but the gender disparity was the largest for the importance of helping others, which a significantly greater proportion of women preferred than men. Second, I find that preferences for helping others were positively associated with female-dominated majors and occupations (e.g., education and health care fields) and negatively associated with male-dominated majors and occupations (e.g., business and STEM fields) for all respondents. Third, I find that preferences for helping others accounted for 12 percent of the early-career gender wage gap, conditional on occupation and major controls, because women were more likely to choose lower-paying jobs that involved concern, assisting, and caring for others.

This set of results advances the growing literature that finds that students, particularly women, experience a trade-off between earnings and job characteristics, and that they are willing to take a wage penalty for their workplace preferences. I find that workplace preferences for helping others as a part of one's job are particularly salient for young workers. Using data from BPS:12/17 on individuals who are early in their careers is advantageous for identifying the impact of workplace preferences on job selection without significant feedback from the labor market. Future research that examines how these preferences change and relate to choice of occupation further in individuals' careers is crucial to understanding the evolution of the gender wage gap.

Appendix A

Appendix to Chapter 1

A.0.1 Tables

Table A.1: *Number of Schools in Sample by State and Year*

	2016	2017	2018	2019	2020	2021	2022
Alabama	4	6	5	5	7	9	8
Alaska	1	1	1	1	1	1	1
Arizona	2	4	4	4	4	4	3
Arkansas	2	2	2	4	5	4	3
California	38	41	40	41	43	44	42
Colorado	8	9	9	8	11	14	14
Connecticut	17	18	18	18	18	18	18
Delaware	1	1	1	1	1	2	3
District of Columbia	4	4	4	4	4	4	4
Florida	20	22	26	29	31	33	35
Georgia	10	11	11	12	14	14	17
Hawaii	1	1	2	2	2	2	2
Idaho	3	3	3	3	3	3	3
Illinois	25	27	29	33	35	48	50
Indiana	15	20	21	23	23	27	31
Iowa	11	11	13	15	16	16	16
Kansas	1	1	3	5	5	5	5
Kentucky	4	4	6	9	10	10	12
Louisiana	5	6	7	7	7	7	8
Maine	16	18	18	18	18	18	18
Maryland	14	13	14	16	17	19	20
Massachusetts	47	48	49	51	50	53	55
Michigan	14	15	17	22	25	29	32
Minnesota	11	14	15	16	17	18	18
Mississippi	2	2	4	4	4	5	5
Missouri	10	14	18	22	23	25	26
Montana	2	2	3	2	2	2	2
Nebraska	3	3	4	4	4	4	4
Nevada	1	2	2	2	2	1	1
New Hampshire	10	10	11	10	10	10	10
New Jersey	20	22	22	22	23	23	23
New Mexico	1	1	1	1	1	1	2
New York	93	95	98	101	96	100	100
North Carolina	13	18	21	23	24	29	39
North Dakota	.	.	.	1	1	1	1
Ohio	31	39	43	44	46	46	48
Oklahoma	3	3	3	3	3	4	4
Oregon	8	10	12	11	11	13	14
Pennsylvania	55	56	64	72	76	81	78
Rhode Island	10	10	10	10	10	10	10
South Carolina	4	4	4	7	12	16	19
South Dakota	1	1	1	3	3	3	4
Tennessee	9	10	11	10	12	13	14
Texas	12	13	13	15	18	21	31
Utah	1	1	1	2	2	2	2
Vermont	14	15	12	12	11	11	9
Virginia	15	17	20	22	25	26	30
Washington	8	8	9	9	10	11	18
West Virginia	4	5	5	7	7	7	9
Wisconsin	12	12	14	16	18	20	21
Wyoming	.	1	1	1	1	1	1
Total	616	674	725	783	822	888	943

Notes: Data include all Common App member schools that applicants applied to in seasons 2016-17 through 2022-23.

Table A.2: *The Effect of Dobbs on High-Achieving Women's College Application Decisions: Applied to a State with an Abortion Ban, Abortion Law, or No Restriction using Always Common App Members*

	Abortion Ban		Abortion Law		No Restriction	
	(1)	(2)	(3)	(4)	(5)	(6)
Female x After	-0.014***	-0.015***	-0.014***	-0.015***	0.003	0.002
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
Female	0.030***		-0.029***		0.036***	
	(0.007)		(0.006)		(0.004)	
After	0.016*		0.058***		0.007	
	(0.009)		(0.011)		(0.009)	
Applicant covariates	No	Yes	No	Yes	No	Yes
State-by-season FE	No	Yes	No	Yes	No	Yes
State-by-gender FE	No	Yes	No	Yes	No	Yes
R-squared	0.001	0.071	0.004	0.080	0.003	0.093
N	590363	589171	590363	589171	590363	589171

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the difference-in-differences estimates of the impact of *Dobbs* on high-achieving female college applicants relative to high-achieving male college applicants. The sample is restricted to applications to schools that were Common App members in all application seasons between 2016-17 and 2022-23. The outcome is whether an applicant applied to one of 13 states with an enforceable abortion ban (columns 1-2); one of 24 states with an abortion law (columns 3-4), which includes those with an enforceable ban; or one of 26 states and D.C. with no abortion restriction (columns 4-6). The first estimate for each outcome uses the baseline specification without any controls. The second estimate for each outcome controls for applicant-level covariates (see text for details) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

Table A.3: *Event Study Estimates for High-Achieving Applicants: Applied to a State with an Abortion Ban, Abortion Law, or No Restriction using Always Common App Members*

	Abortion Ban		Abortion Law		No Restriction	
	(1)	(2)	(3)	(4)	(5)	(6)
Female x After x 2022	-0.018***	-0.019***	-0.013***	-0.014***	-0.001	-0.003
	(0.006)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Female x After x 2021	-0.013**	-0.013**	-0.009	-0.009*	0.005	0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
Female x After x 2019	-0.003	-0.001	0.006	0.006	0.001	0.000
	(0.004)	(0.004)	(0.005)	(0.005)	(0.003)	(0.003)
Female x After x 2018	-0.002	-0.001	0.006	0.006	-0.001	-0.001
	(0.005)	(0.005)	(0.006)	(0.005)	(0.004)	(0.003)
Female x After x 2017	-0.003	-0.001	0.005	0.006	0.002	0.001
	(0.005)	(0.005)	(0.007)	(0.005)	(0.003)	(0.004)
Female x After x 2016	-0.003	-0.002	-0.004	-0.003	-0.009**	-0.010**
	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
Applicant covariates	No	Yes	No	Yes	No	Yes
State-by-season FE	No	Yes	No	Yes	No	Yes
State-by-gender FE	No	Yes	No	Yes	No	Yes
R-squared	0.001	0.071	0.005	0.080	0.003	0.093
N	590363	589171	590363	589171	590363	589171

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the event study estimates of the impact of *Dobbs* on high-achieving female college applicants relative to high-achieving male college applicants. The sample is restricted to applications to schools that were Common App members in all application seasons between 2016-17 and 2022-23. The outcome is whether an applicant applied to one of 13 states with an enforceable abortion ban (columns 1-2); one of 24 states with an abortion law (columns 3-4), which includes those with an enforceable ban; or one of 26 states and D.C. with no abortion restriction (columns 4-6). The first estimate for each outcome uses the baseline specification without any controls. The second estimate for each outcome controls for applicant-level covariates (see text for details) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

Table A.4: *The Effect of Dobbs on High-Achieving Women's College Application Decisions by SAT/ACT Score: Applied to a State with an Abortion Ban, Abortion Law, or No Restriction*

	Abortion Ban			Abortion Law			No Restriction		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female x After	-0.020***	-0.025***	-0.028***	-0.008*	-0.010***	-0.011***	-0.001	-0.002	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)
SAT/ACT cut score	1300	1350	1400	1300	1350	1400	1300	1350	1400
Applicant covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.109	0.099	0.089	0.116	0.104	0.092	0.175	0.148	0.128
N	1667112	1263607	984852	1667112	1263607	984852	1667112	1263607	984852

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the difference-in-differences estimates of the impact of *Dobbs* on high-achieving female college applicants relative to high-achieving male college applicants. The outcome is whether an applicant applied to one of 13 states with an enforceable abortion ban (columns 1-2); one of 24 states with an abortion law (columns 3-4), which includes those with an enforceable ban; or one of 26 states and D.C. with no abortion restriction (columns 4-6). Columns 1, 4, and 7 estimate treatment effects for applicants who scored at or above a 1300 on the SAT/ACT. Columns 2, 5, and 8 estimate treatment effects for applicants who scored at or above a 1350 on the SAT/ACT. Columns 3, 6, and 9 estimate treatment effects for applicants who scored at or above a 1400 on the SAT/ACT. Regressions include applicant-level covariates (see text for details) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

Table A.5: *Summary Statistics for Mobile Applicants*

	Likely to Apply Out-of-State (Top Decile)	High-Achieving
17-years-old	0.67	0.71
18-years-old	0.33	0.29
Female	0.60	0.45
White	0.55	0.50
Black or African American	0.09	0.02
Hispanic or Latino	0.08	0.07
Asian	0.13	0.28
Two or More Races	0.08	0.06
Unknown Race	0.06	0.07
Fee waiver eligible	0.11	0.08
First generation	0.06	0.08
Attends high school in home state	0.94	0.97
Min SAT/ACT	400	1460
Mean SAT/ACT	1416	1511
Missing SAT/ACT	0.12	0
No Restriction Home State	0.72	0.68
Abortion Ban Home State	0.22	0.14
Abortion Law Home State	0.28	0.32
Total number of applications	7.13	7.70
Applied out-of-state	0.96	0.93
Number of states applied out-of-state	4.59	4.75
Prop. of apps sent out-of-state	0.83	0.72
Observations	637929	597755

Notes: This table describes two different samples of applicants who applied to college using the Common App in application seasons 2016-17 through 2022-23. The left-hand column describes the sample of applicants who were most likely to apply out-of-state based on their observable characteristics. The right-hand column describes the sample of high-achieving applicants who scored at or above the 90th percentile on the SAT/ACT in the sample in 2016.

Table A.6: *The Effect of Dobbs on High-Achieving Women's College Application Decisions: Synthetic Differences-in-Differences Estimates*

Panel A: All Application Seasons

	Abortion Ban		Abortion Law		No Restriction	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Group	-0.013	-0.026*	0.005	-0.005	0.002	0.004
	(0.012)	(0.015)	(0.011)	(0.012)	(0.008)	(0.012)
Comparison Group States	Abortion Law	Abortion Ban	Abortion Law	Abortion Ban	Abortion Law	Abortion Ban
N	357	280	357	280	357	280

Panel B: Application Seasons 2019-2022

	Abortion Ban		Abortion Law		No Restriction	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Group	-0.010	-0.021*	-0.000	-0.005	0.005	0.009
	(0.010)	(0.011)	(0.011)	(0.011)	(0.007)	(0.009)
Comparison Group States	Abortion Law	Abortion Ban	Abortion Law	Abortion Ban	Abortion Law	Abortion Ban
N	204	160	204	160	204	160

Notes: This table shows the synthetic difference-in-differences estimates of the impact of *Dobbs* on high-achieving female college applicants. Standard errors are calculated using block bootstrap inference.

A.0.2 Figures

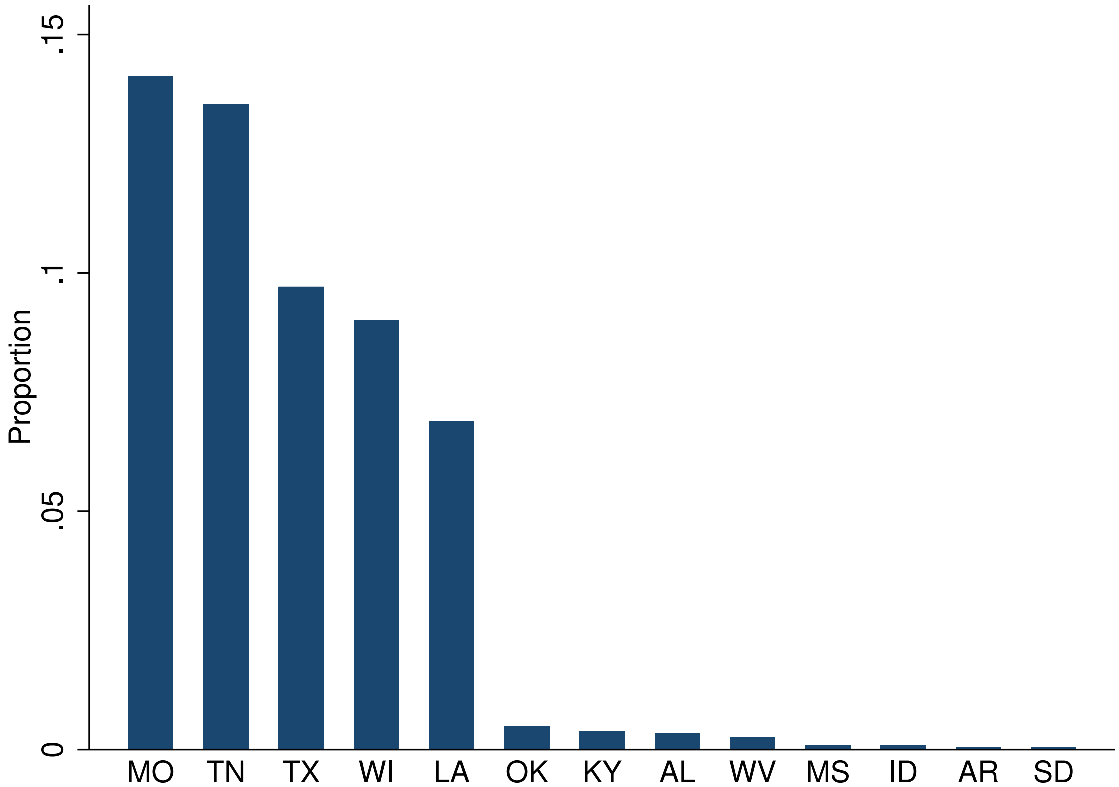
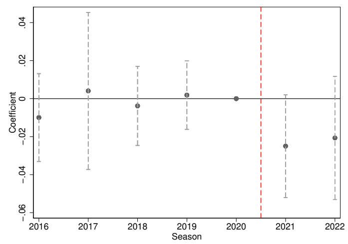


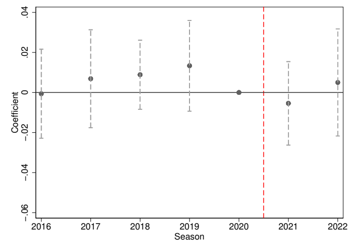
Figure A.1: *Application Behavior to States with an Abortion Ban prior to Dobbs*

Notes: This figure shows the proportion of high-achieving applicants from states with no abortion restriction who applied to each state with an abortion ban prior to the 2021-22 and 2022-23 college application seasons.

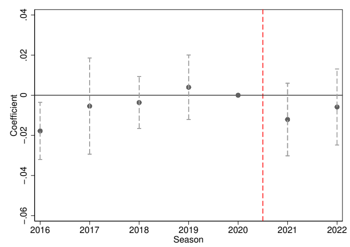
Panel A: Applicants from States with an Abortion Ban



(a) *Abortion ban*

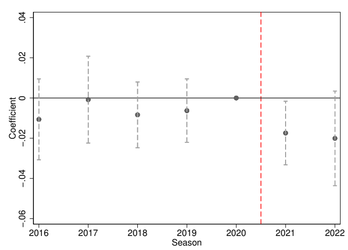


(b) *Abortion law*

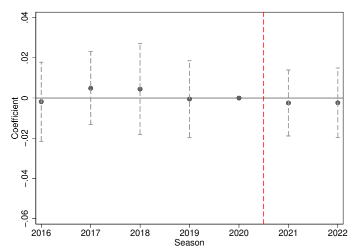


(c) *No abortion restriction*

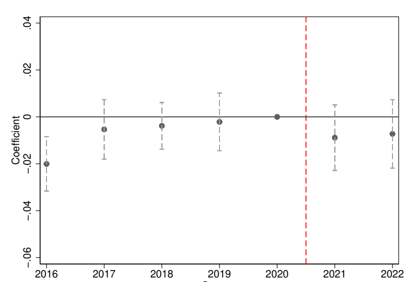
Panel B: Applicants from States with an Abortion Law



(a) *Abortion ban*



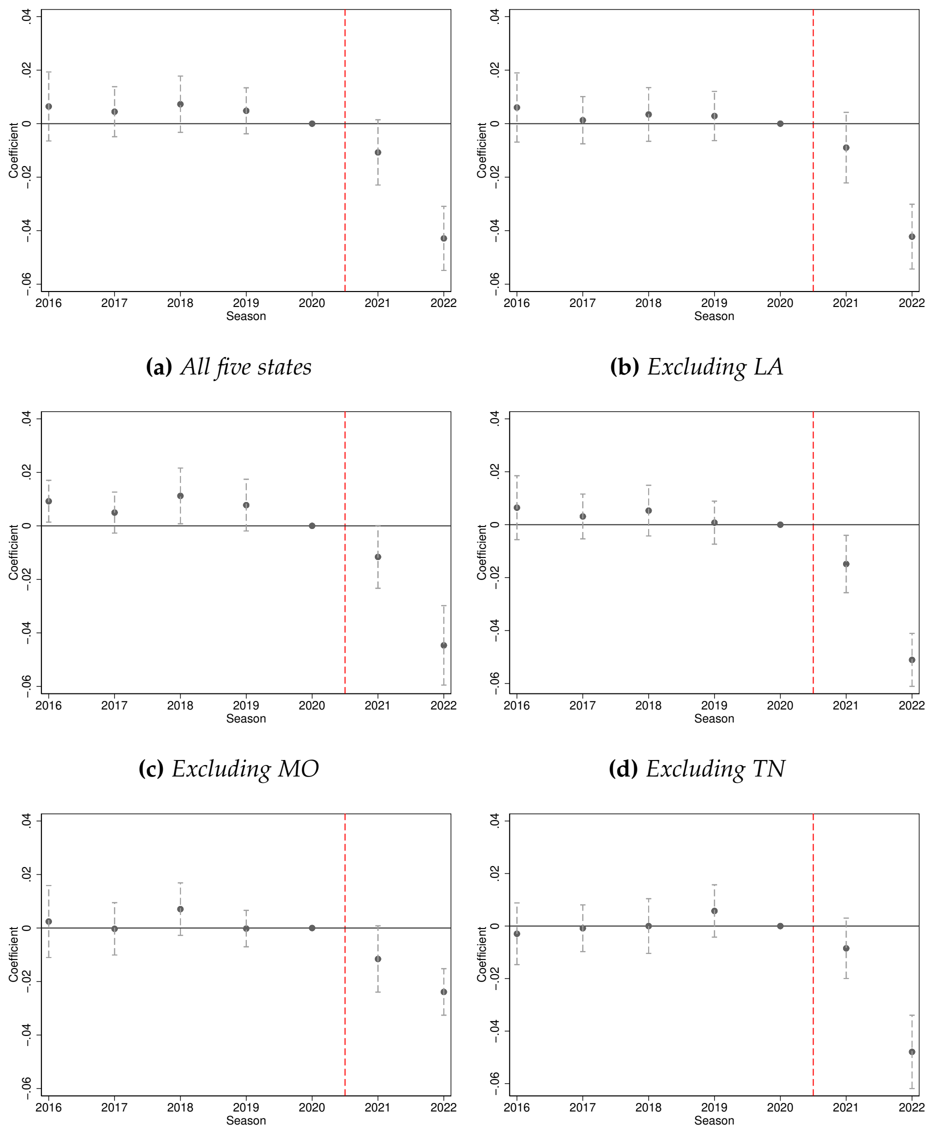
(b) *Abortion law*



(c) *No abortion restriction*

Figure A.2: *The Effect of Dobbs on High-Achieving Women's College Application Decisions*

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interaction terms between year and gender, where the outcome is an indicator for whether an applicant applied to a state with an abortion ban, abortion law, or no abortion restriction. Year 2020 is the omitted category. The dashed vertical line indicates the start of the treatment period. Regressions control for applicant covariates (see text for details) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.



(a) *All five states* **(b)** *Excluding LA*

(c) *Excluding MO* **(d)** *Excluding TN*

(e) *Excluding TX* **(f)** *Excluding WI*

Figure A.3: *Event Study Estimates for Five States with an Abortion Ban*

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interaction terms between year and gender. The sample includes high-achieving applicants from states with no abortion restriction who applied to college during the 2016-17 through 2022-23 application seasons. The outcome in the first figure is an indicator for whether an applicant applied to at least one of five states (Louisiana, Missouri, Tennessee, Texas, and Wisconsin) with a ban based on state abortion policy in the 2022-23 college application season. These fives states received the largest proportion of applicants prior to treatment (see Figure A.1). The subsequent figures systematically exclude one state from the outcome. Regressions control for applicant covariates (see text) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

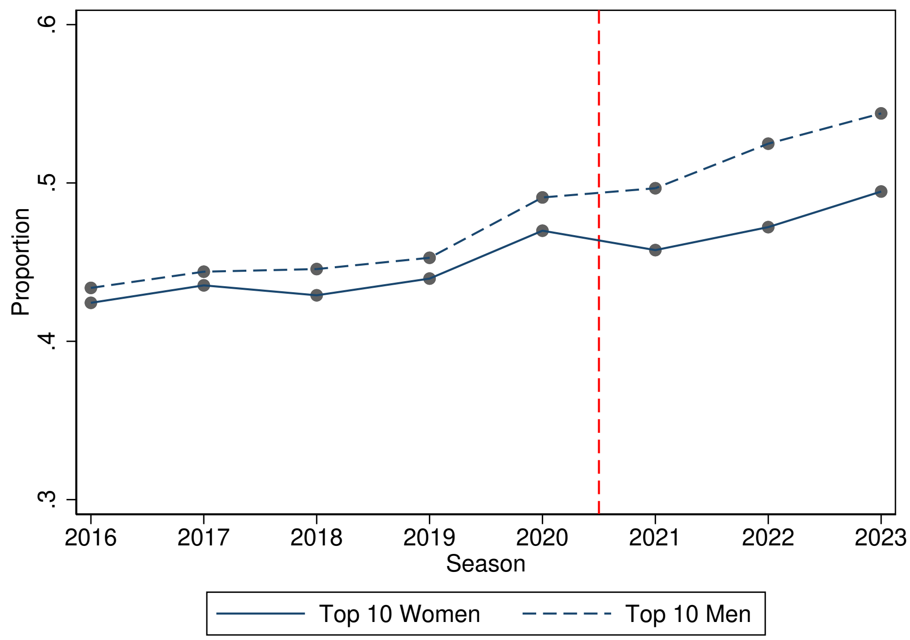
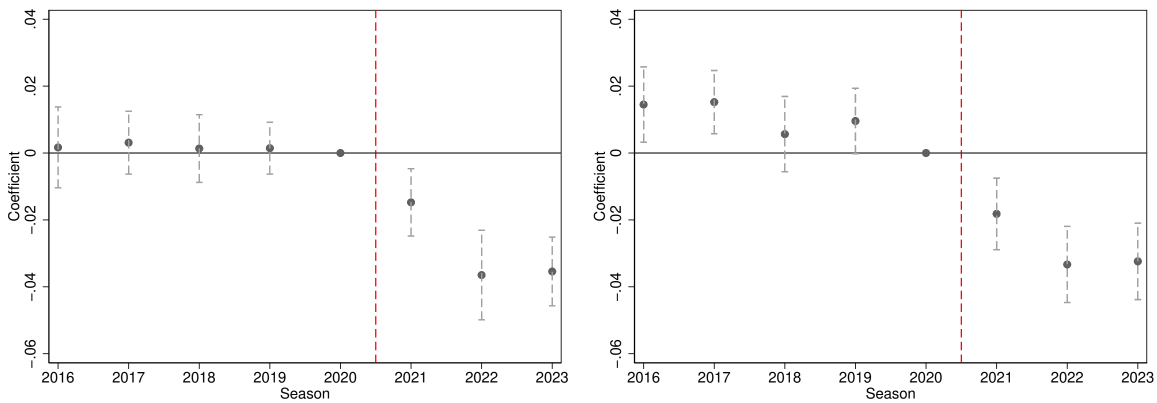


Figure A.4: *Proportion of High-Achieving Applicants who Applied to at least One of 14 States with an Abortion Ban in 2023*

Notes: This figure shows the proportion of high-achieving male and female applicants who applied to at least one of the 14 states with an abortion ban (see text for details) in the 2023-24 application season. The dashed vertical line indicates the start of the treatment period.



(a) *Outcome based on 13 states with a ban*

(b) *Outcome based on 14 states with a ban*

Figure A.5: *Persisting Treatment Effects in the 2023-24 Application Season*

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interaction terms between year and gender. The sample includes high-achieving applicants who applied to college during the 2016-17 through 2022-23 application seasons. The outcome in the first figure is an indicator for whether an applicant applied to at least one of 13 states with a ban based on state abortion policy in the 2022-23 college application season. The outcome in the second figure is an indicator for whether an applicant applied to at least one of 14 states with a ban based on state abortion policy in the 2023-24 college application season. Year 2020 is the omitted category. The dashed vertical line indicates the start of the treatment period. Regressions control for applicant covariates (see text) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

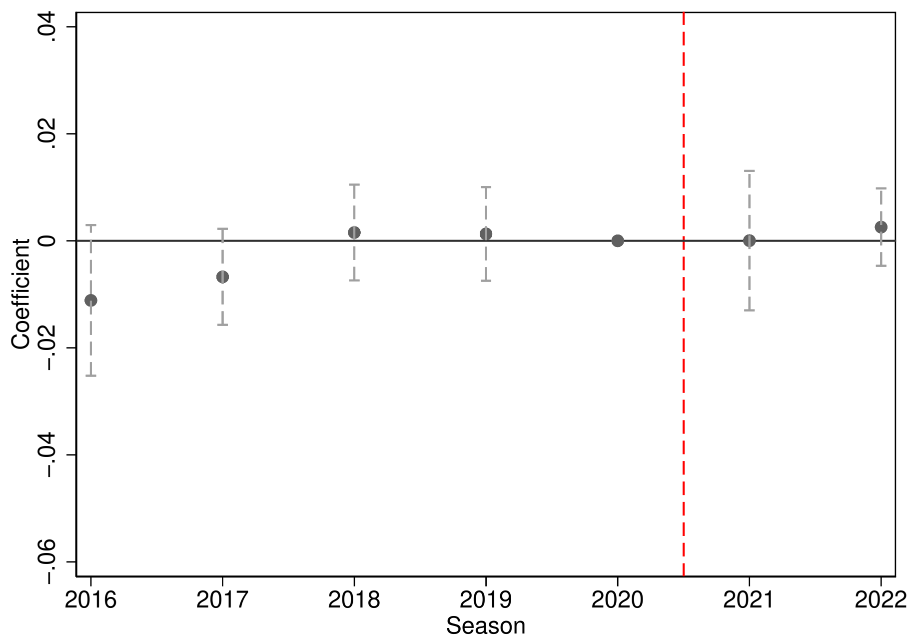
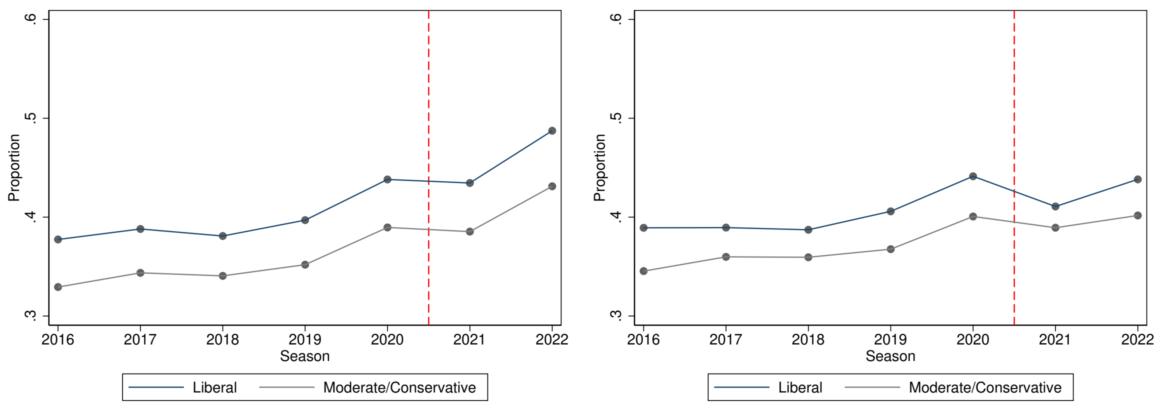


Figure A.6: *Event Study for Applying to a Red State with No Abortion Restriction*

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interaction terms between year and gender. The outcome is whether an applicant applied to a state that voted for Donald Trump in the 2016 election that does not have an abortion restriction. The sample is restricted to applicants from states with no abortion restriction. The dashed vertical line indicates the start of the treatment period. Regressions control for applicant covariates (see text for details) and state-by-season and state-by-gender fixed effects. Standard errors are clustered by applicant state of residence.

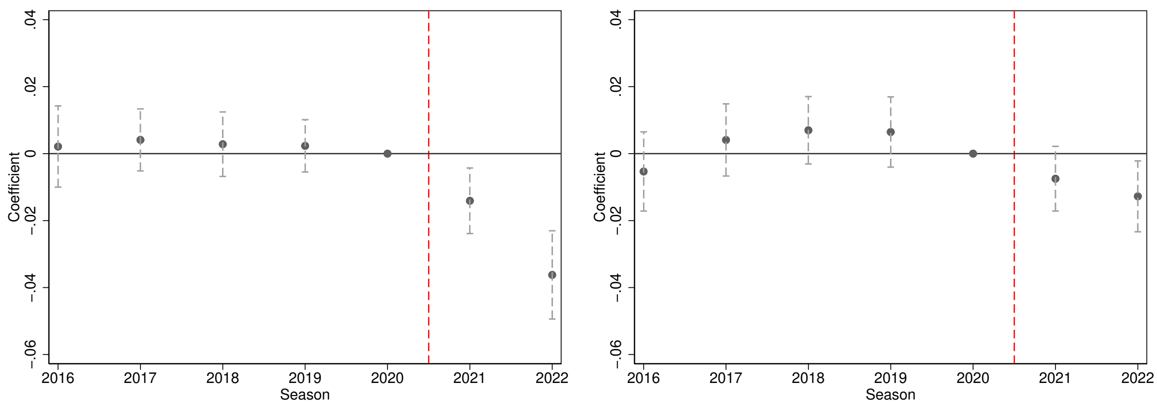


(a) *Men*

(b) *Women*

Figure A.7: *Application Patterns to States with an Abortion Ban by County Partisanship*

Notes: This figure shows the proportion of high-achieving men and women from states with no abortion restriction that applied to a state with an abortion ban in the 2016-17 through 2022-23 college application seasons. County partisanship is defined based on presidential vote share in the 2016 election (see text for details).



(a) *Applied to a state with an abortion ban*

(b) *Applied to a state with an abortion law*

Figure A.8: *The Effect of Dobbs on High-Achieving Women's College Application Decisions (Abortion Restrictions by the 2021-22 College Application Season)*

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interaction terms between year and gender, where the outcome is an indicator for whether an applicant applied to a set of states with an abortion law or ban by 2021, and year 2020 is the omitted category. The dashed vertical line indicates the start of the treatment period. Regressions control for applicant covariates (see text for details) and state-by-season, state-by-gender. Standard errors are clustered by applicant state of residence.

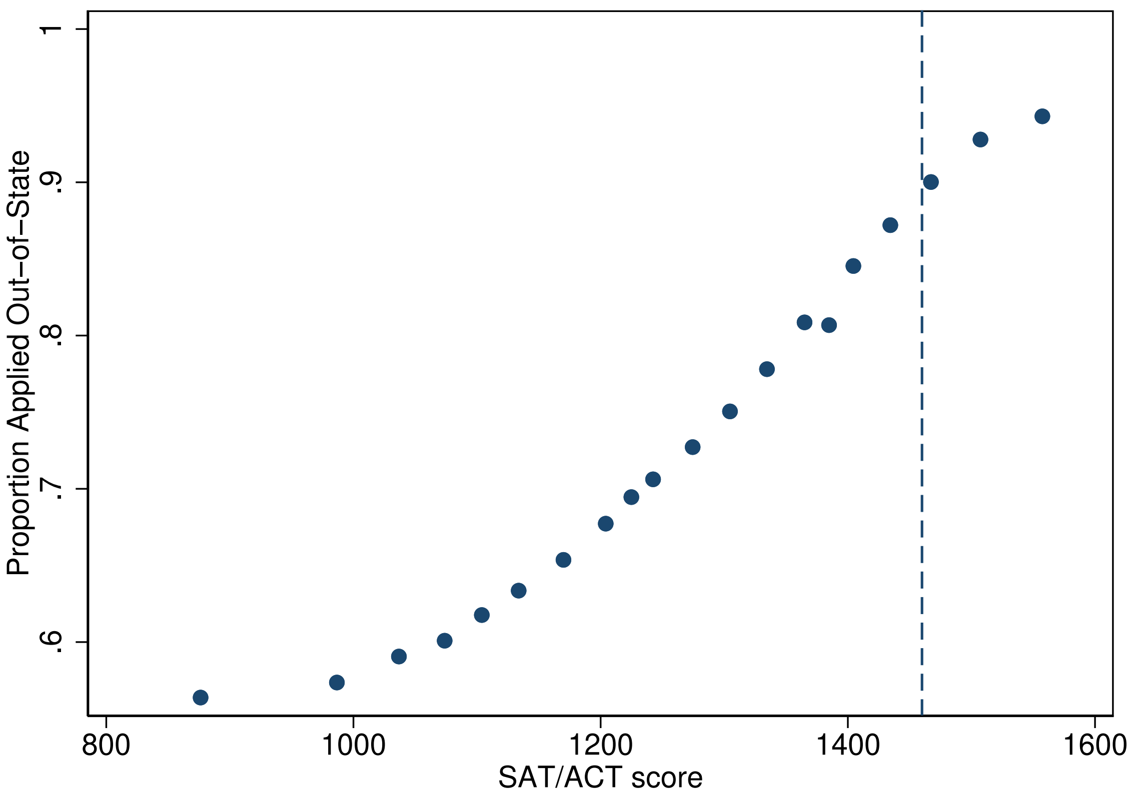
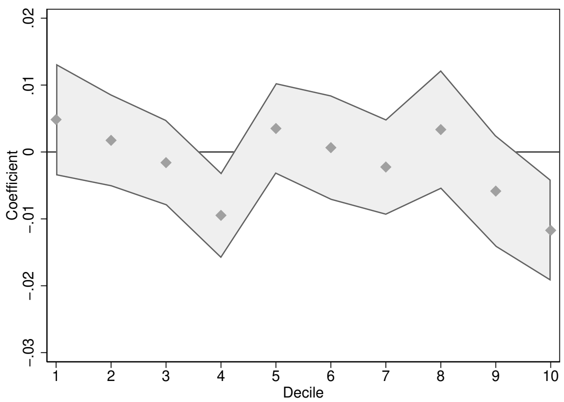
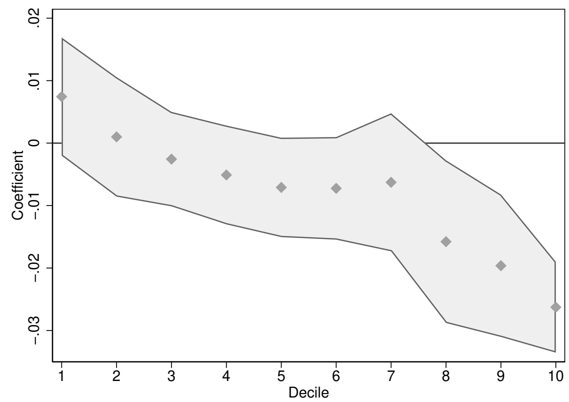


Figure A.9: *Proportion of Applicants who Applied Out-of-State by SAT/ACT Score*

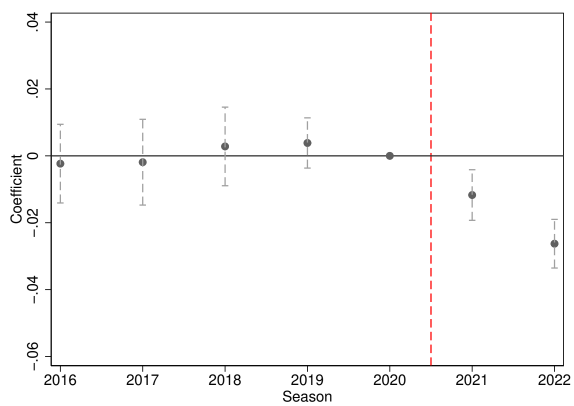
Notes: This figure shows a binscatter plot of the proportion of applicants in the sample who applied out-of-state by SAT/ACT score between the 2016 and 2020 application seasons. The dotted line is at an SAT/ACT score of 1460, which defines the top 10 percent of SAT/ACT test-takers in the sample in 2016.



(a) *Treatment effects in 2021 by decile*



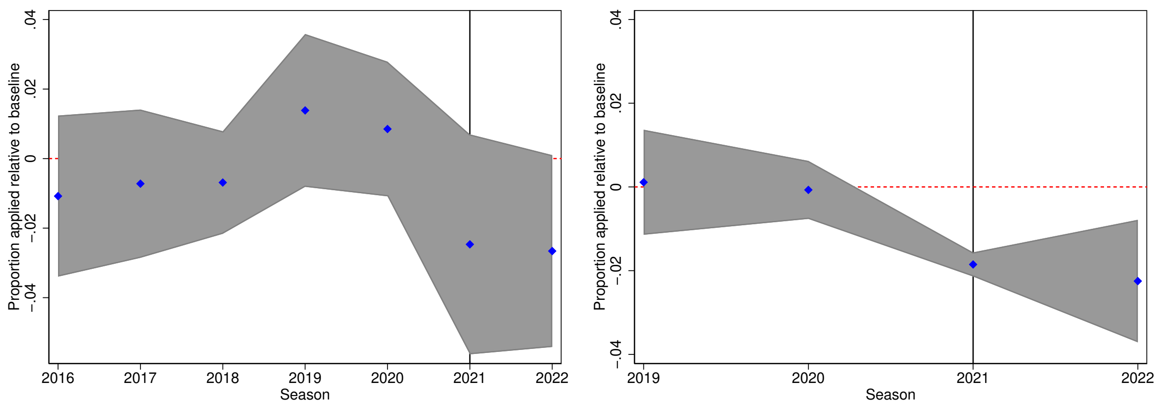
(b) *Treatment effects in 2022 by decile*



(c) *Treatment effects for top decile*

Figure A.10: *Estimates for Applying to a State with an Abortion Ban by Likelihood of Applying Out-of-State (in deciles)*

Notes: The first two figures plot treatment effects in 2021 and 2022, respectively, for deciles of predicted likelihood that an applicant applies out-of-state. The third figure plots event study estimates for the top decile. See text for sample selection details.



(a) *All application seasons*

(b) *Application seasons 2019-2022*

Figure A.11: *Synthetic Difference-in-Differences Event Study Estimates*

Notes: This figure shows synthetic difference-in-differences event study estimates. The outcome is whether an applicant applied to a state with an abortion ban. The control group includes states with an abortion ban and the treatment group includes states with no abortion restriction. The event study estimates are produced using the procedure outlined in Clarke et al., 2023.

A.0.3 Analysis of State Abortion Policy

I draw on a combination of primary and secondary sources for my analysis of state abortion policy, including state legislation, news articles, and organizations such as the Center for Reproductive Rights, the Guttmacher Institute, and KFF. My analysis reflects the state abortion landscape as of the 2022-23 college application season, and I focus on state laws that intended to severely restrict access to abortion based on gestational age (early gestational age bans) or completely ban access to abortion (trigger laws and total abortion bans). Laws in 24 states meet these criteria, and of those states, 13 states enforced a total abortion ban in the 2022-23 college application season. These 13 states, which are marked with a bullet (•), include Alabama, Arkansas, Idaho, Kentucky, Louisiana, Mississippi, Missouri, Oklahoma, South Dakota, Tennessee, Texas, West Virginia, and Wisconsin.

In addition to their more restrictive laws, many of the 24 states passed late gestational age bans at 20 or 22 weeks in prior years, including Alabama, Arizona, Arkansas, Georgia, Idaho, Indiana, Iowa, Kentucky, Louisiana, Mississippi, North Dakota, Ohio, Oklahoma, South Carolina, South Dakota, Texas, West Virginia, and Wisconsin. Four states without restrictive abortion policies also previously passed late gestational age bans, including Kansas, Montana, Nebraska, and North Carolina. Many, but not all, of these laws took effect under *Roe*. In this analysis, I focus on the most restrictive abortion laws – limited to those based on gestational age or total bans – that took effect after *Dobbs*. Texas and Oklahoma are two exceptions. Texas' 6-week ban took effect in September 2021, and Oklahoma's 6-week and total bans took effect in May 2022.

It is also helpful to understand some technical details included in my review. First, there are two common ways to calculate the gestational age of a pregnancy, which is relevant for the severity of abortion restrictions as codified in state law. These definitions include post-fertilization, which is based on the date of conception, and last menstrual period (LMP), which is based on the first day of a woman's most recent menstrual period. Gestational age based on LMP is roughly two weeks more than gestational age based on post-fertilization. I refer closely to the language in state legislation in my analysis. Second, I use the term “medical emergency" – which is the most common exception in legislation that restricts access to abortion – to broadly capture various definitions regarding the health of a pregnant person, such as to save the pregnant person’s life or to prevent serious risk to the pregnant person’s physical health.

Alabama•

On May 15, 2019, Governor Kay Ivey signed H.B. 314 made performing an abortion a Class A felony punishable by up to 99 years in prison, and attempting to perform an abortion a Class C felony punishable by up to 10 years in prison, except in the case of a medical emergency. The law, which was scheduled to take effect on November 15, 2019, was blocked by a federal judge on October 29, 2019. The law went into effect on June 24, 2022.

Arizona

On March 30, 2022, Governor Doug Ducey signed S.B. 1164, which made performing an abortion after 15 weeks LMP, except in the case of a medical emergency, a Class 6 felony, punishable by fines, probation, and possible prison time between months and up to 5 years. The law went into effect on September 24, 2022. One day prior, on September 23, 2022, a state judge allowed a pre-statehood law from 1864 that banned abortion except in the case of a medical emergency, punishable by two to five years in prison, to take effect. The law was blocked by an appeals court on October 7, and on October 27, 2022 the state attorney general agreed to not enforce the law until at least 2023. However, a ruling on December 30, 2022 held that the state’s total ban did not take precedence over the state’s other laws and regulations.

Arkansas•

On February 19, 2019, Governor Asa Hutchinson signed S.B. 149, which made performing an abortion an unclassified felony, except in the case of a medical emergency, if the U.S. Supreme Court overruled *Roe v. Wade*, punishable by up to 10 years in prison or a fine of up to $100,000. The law went into effect on June 24, 2022.

The state also passed three other pieces of legislation to severely restrict access to abortion that were blocked by court orders. On March 6, 2013, the state senate overrode

Governor Mike Beebe's veto of S.B. 134, which banned abortion starting at twelve weeks LMP except in the case of a medical emergency, rape, or incest. The law, which was set to take effect on August 16, 2013, was blocked by a federal judge on May 17, 2013 and struck down by a federal judge on March 15, 2014 and a federal appeals court on May 27, 2015. On March 15, 2019, Governor Hutchinson signed H.B. 1439, which made performing an abortion after 18 weeks LMP a Class D felony, except in the case of a medical emergency, rape, or incest, punishable by up to six years in prison and a fine of up to $10,000. The law, which was set to take effect on July 24, 2019, was blocked by a federal judge that day and on August 6, 2019, as well as by a federal appeals court on January 5, 2021. On March 9, 2021, Governor Hutchinson signed S.B. 6, which made performing an abortion an unclassified felony, except in the case of a medical emergency, punishable up to 10 years in prison or a fine of up to $100,000. The law, which was set to take effect on July 28, 2021, was blocked by a federal judge on July 20, 2021.

Florida

On April 14, 2022, Governor Ron DeSantis signed H.B. 5, which banned abortion after 15 weeks LMP except in the case of a medical emergency or lethal fetal abnormality. The law, which was scheduled to take effect on July 1, 2022, was blocked by a state judge on June 30, 2022 and reinstated by the state on July 5, 2022.

Georgia

On May 7, 2019, Governor Brian Kemp signed H.B. 481, which banned abortion after the detection of a fetal heartbeat except in the case of a medical emergency, rape, incest, or medically futile pregnancy. The law, which was scheduled to take effect on January 1, 2020, was blocked by a federal judge on October 1, 2019. The law was reinstated by a federal appeals court on July 20, 2022, blocked by a federal judge on November 15, 2022, and reinstated by the state's supreme court on November 23, 2022.

Idaho•

On March 19, 2020, Governor Brad Little signed S.B. 1385, which made performing an abortion a felony, except in the case of a medical emergency, rape, or incest if the U.S.

Supreme Court overturned *Roe v. Wade*, punishable by two to five years in prison. On August 24, 2022, a federal judge partially blocked the law, which then went into effect the next day on August 25, 2022.

The state also passed two other pieces of legislation to severely restrict access to abortion that were blocked by court orders. On April 27, 2021, Governor Brad Little signed H.B. 366, which made performing an abortion after the detection of a fetal heartbeat a felony, except in the case of a medical emergency, rape, or incest, if the U.S. Supreme Court overturned *Roe v. Wade*. On March 23, 2022, Governor Little signed S.B. 1309, which expanded on H.B. 366 to allow relatives of the person who received an abortion to sue the medical professional for damages of at least $20,000. The law, which was scheduled to take effect on April 22, 2022, was blocked by the state's supreme court on April 8, 2022.

Indiana

On August 5, 2022, Governor Eric Holcomb signed S.B. 1, which made performing an abortion a Level 5 felony, except in the case of a medical emergency, rape, incest, or lethal fetal anomaly, punishable by one to six years in prison and a fine of up to $10,000. The law, which took effect on September 15, 2022, was blocked by a state judge on September 22, 2022 and December 2, 2022.

Iowa

On May 4, 2018, Governor Kim Reynolds signed S.F. 359, which made performing an abortion after the detection of a fetal heartbeat a Class C felony, except in the case of a medical emergency, rape, incest, or lethal fetal abnormality, punishable by a maximum two-year prison sentence and a $4,000 fine. The law, which was scheduled to take effect on July 1, 2018, was blocked by a state judge on June 1, 2018 and January 22, 2019. A state judge declined to lift the block on December 12, 2022.

Kentucky•

On March 15, 2019, Governor Bevin signed S.B. 9, which made abortion after the detection of a fetal heartbeat a Class D felony except in the case of a medical emergency, punishable by up to five years in jail. The law, which was scheduled to take effect immediately, was blocked by a federal judge that same day. On March 26, 2019, Governor Bevin signed H.B. 148, which made abortion a Class D felony, except in the case of a medical emergency, if the U.S. Supreme Court overruled *Roe v. Wade*. The law went into effect on June 24, 2022. A state judge blocked S.B. 9 and H.B. 148 on June 30, 2022, but they were reinstated on August 2, 2022.

In addition to these two laws, on April 14, 2022, the state legislature overrode Governor Andy Beshear's veto of H.B. 3, which banned abortion starting at 15 weeks post-fertilization except in the case of a medical emergency. The law, which was scheduled to take effect immediately, was blocked by a federal judge on April 21, 2022. The federal judge lifted her injunction on July 14, 2022.

Louisiana•

On June 17, 2006, Governor Kathleen Blanco signed S.B. 33, which banned abortion except in the case of a medical emergency if the U.S. Supreme Court overruled *Roe v. Wade*, and made the punishment for performing an abortion imprisonment at hard labor for between one and ten years and fines between $10,000 and $100,000. The law went into effect on June 24, 2022. It was blocked by a state judge on June 27, 2022, reinstated by a state judge on July 8, 2022, blocked by a state judge on July 12, 2022 and July 21, 2022, and reinstated by a state appeals court on July 29, 2022.

The state has also passed four other pieces of legislation to severely restrict access to abortion. On June 18, 1991, the state legislature overrode a veto by Governor Buddy Roemer to enact a law that banned abortion except in the case of a medical emergency, rape, or incest, punishable by up to 10 years in prison and fines up to $100,000. The law was blocked by a federal judge on August 8, 1991 and ruled unconstitutional by a federal appeals court on September 22, 1991. On May 23, 2018, Governor John Bel Edwards signed S.B. 181, which banned abortion after 15 weeks LMP if a federal court upheld a similar law (S.B. 1510) in Mississippi, with similar punishment provisions as S.B. 33. On May 30, 2019, Governor Bel Edwards signed S.B. 184, which banned abortion after the detection of a fetal heartbeat, except in the case of a medical emergency, if a federal court upheld a similar law (S.B. 2116) in Mississippi. On June 17, 2022, Governor Bel Edwards signed S.B. 342, which expanded the exceptions in S.B. 33 to include medical futility and ectopic pregnancies.

Michigan

On May 17, 2022, a state judge blocked a 1931 law that made performing an abortion a felony except in the case of a medical emergency, punishable by up to four years in prison. On August 1, 2022, a state judge granted Governor Gretchen Whitmer's request to temporarily block enforcement of the law. On September 7, 2022, a state court order declared the law unconstitutional. Voters approved Proposal 3, an amendment to protect abortion rights, on November 8, 2022.

Mississippi•

On March 22, 2007, Governor Haley Barbour signed S.B. 2391, which made performing an abortion a felony if the U.S. Supreme Court overruled *Roe v. Wade* except in the case of a medical emergency or rape, punishable by one to ten years in prison. The law went into effect on July 7, 2022.

The state also passed two other pieces of legislation to severely restrict access to abortion, including the law at the center of the *Dobbs* case. On March 19, 2018, Governor Phil Bryant signed H.B. 1510, which made performing an abortion after 15 weeks LMP a misdemeanor, except in the case of a medical emergency or severe fetal abnormality, punishable by up to six months in prison and a fine of $1,000. The law was blocked by a federal judge on March 20, 2018 and November 20, 2018. On March 21, 2019, Governor Bryant signed S.B. 2116, banned abortion after the detection of a fetal heartbeat except in the case of a medical emergency. The law was blocked by a federal judge on May 24, 2019.

Missouri•

On May 24, 2019, Governor Mike Parson signed H.B. 126, which made performing an abortion a Class B felony (1) starting at eight weeks LMP except in the case of a medical emergency and (2) if the U.S. Supreme Court overruled *Roe v. Wade* except in the case of a medical emergency, punishable by between five and 15 years in prison. The 8-week ban was blocked by a federal judge on August 27, 2019, the day before it was scheduled to take effect. The trigger law took effect on June 24, 2022.

North Dakota

On April 26, 2007, Governor John Hoeven signed H.B. 1466, which made performing an abortion a Class C felony, except in the case of a medical emergency, rape, or incest, if the U.S. Supreme Court overruled *Roe v. Wade*, punishable by up to 5 years in prison and a fine of $10,000. The law was blocked by a state judge on August 25, 2022, the day before it was scheduled to take effect.

Previously, on March 26, 2013, Governor Jack Dalrymple signed H.B. 1456, which made performing an abortion after the detection of a fetal heartbeat a Class C felony except in the case of a medical emergency. The law, which was scheduled to take effect on August 1, 2013, was blocked by a federal judge on July 22, 2013 and April 16, 2014, and a federal appeals court on July 22, 2015.

Ohio

On April 11, 2019, Governor Mike DeWine signed S.B. 23, which made performing an abortion after the detection of a fetal heartbeat a fifth degree felony except in the case of a medical emergency or ectopic pregnancy, punishable by by between 6 and 12 months in prison and a fine of up to $2,500. The law, which was scheduled to take effect on July 10, 2019, was blocked by a federal judge on July 3, 2019. The law went into effect on June 27, 2022, but it was blocked by a state judge on September 14, 2022 and October 7, 2022.

Oklahoma•

On May 3, 2022, Governor Kevin Stitt signed S.B. 1503, which banned abortion after the detection of a fetal heartbeat, except in the case of a medical emergency. The law went into effect immediately, and offered damages of at least $10,000 to anyone who successfully sues an abortion provider or anyone who aids and abets someone seeking abortion. On May 25, 2022, Governor Stitt signed H.B. 4327, which banned abortion, except in the case of a medical emergency, rape, or incest, with similar enforcement provisions as S.B. 1503. The law took effect immediately, making Oklahoma the only state to successfully outlaw abortion under *Roe*.

On April 27, 2021, Governor Stitt signed S.B. 918, which banned abortion except in the case of a medical emergency if the U.S. Supreme Court overruled *Roe v. Wade*. On April 29, 2022, Governor Stitt signed S.B. 1555, which amended the trigger language in S.B. 918 to allow the state to enforce a 1910 abortion ban. The law, which went into effect on June 24, 2022, made performing an abortion a felony, except in the case of a medical emergency, if the Court overruled *Roe*, punishable by up to five years in prison.

On April 12, 2022, Governor Stitt signed S.B. 612, which made performing an abortion a felony, except in the case of a medical emergency, punishable by up to 10 years in prison and fine of up to $100,000. The law went into effect on August 25, 2022.

South Carolina

On February 18, 2021, Governor Henry McMaster signed S.B. 1, which made performing an abortion after the detection of a fetal heartbeat a felony except in the case of a medical emergency, rape, incest, or lethal fetal abnormality, punishable by up to 2 years in prison and a $10,000 fine. The law was blocked by a federal judge the following day and reinstated on June 27, 2022. The law was blocked by the state supreme court on August 17, 2022.

South Dakota•

On March 22, 2005, Governor Mike Rounds signed H.B. 1249, which made performing an abortion a Class 6 felony except in the case of a medical emergency if the U.S. Supreme Court overruled *Roe v. Wade*, punishable by up to 2 years in prison and a fine of $4,000. The law went into effect on June 24, 2022.

Tennessee•

On May 10, 2019, Governor Bill Lee signed S.B. 1257, which made performing an abortion a Class C felony except in the case of a medical emergency if the U.S. Supreme Court overruled *Roe v. Wade*, punishable by 3 to 15 years in prison and fines up to $10,000.. The law went into effect on August 25, 2022.

On July 13, 2020, Governor Lee signed H.B. 2263, which made performing abortion after the detection of a a fetal heartbeat a Class C felony except in the case of a medical emergency. The law was blocked by a federal judge that same day and instated by a federal appeals court on June 28, 2022.

Texas•

On May 19, 2021, Governor Greg Abbott signed S.B. 8, which banned abortion after the detection of a fetal heartbeat except in the case of a medical emergency. The law went into effect on September 1, 2021, and offered damages of at least $10,000 to anyone who successfully sues an abortion provider or anyone who aids and abets someone seeking abortion. On June 16, 2021, Governor Abbott signed H.B. 1280, which made performing an abortion a first degree felony except in the case of a medical emergency if the U.S. Supreme Court overruled *Roe v. Wade*, punishable by up to life in prison and a fine of up to $10,000. The law went into effect on August 25, 2022. Notably, the state supreme court allowed a pre-*Roe* abortion ban from 1925, which took effect on June 24, 2022 and blocked by a state judge on June 28, 2022, to take effect on July 1, 2022. The law made performing an abortion punishable by two to 10 years in prison.

Utah

On March 25, 2019, Governor Gary Herbert signed H.B. 136, which made performing an abortion after 18 weeks LMP a second degree felony except in the case of a medical emergency, rape, incest, or lethal fetal anomaly, punishable by a 1 to 15-year prison sentence and fines up to $10,000. The law was blocked by a federal judge on April 18, 2019 and took effect on June 28, 2022.

The state passed two other pieces of legislation to severely restrict access to abortion. On January 25, 1991, Governor Norman Bangerter signed a law that made performing an abortion a class 3 felony except in the case of a medical emergency, rape, incest, or lethal fetal anomaly, punishable by a $5,000 fine and up to five years in jail. The law was ruled unconstitutional by a federal judge in December 2022. On March 28, 2020, Governor Herbert signed S.B. 174, which made performing an abortion a second degree felony except in the case of medical emergency, rape, incest, or lethal fetal anomaly. The law took effect on June 24, 2022 but was blocked by a state judge on June 27, 2022.

West Virginia•

On September 16, 2022, Governor Jim Justice signed H.B. 302, which made performing an abortion a felony except in the case of a medical emergency, nonmedically viable fetus, or ectopic pregnancy, punishable by three to ten years in prison. The law immediately took effect.

Wisconsin•

On June 24, 2022, abortion law in the state reverted back to an 1849 law that made performing an abortion a Class H or Class E felony except in the case of a medical emergency, punishable by up to six years in prison and a fine up to $10,000 or up to 15 years in prison and a fine up to $50,000.

Wyoming

On March 15, 2022, Governor Mark Gordon signed H.B. 92, which made performing an abortion a felony, except in the case of medical emergency, rape, or incest, if the U.S. Supreme Court overruled *Roe v. Wade*, punishable by up to 14 years in prison. The law was blocked by a state judge on July 27, 2022, the day it was scheduled to take effect.

Post-2022 Changes in State Abortion Policy

Eleven states made changes to their abortion laws or policies after the start of the 2022-23 college application season.[1] However, these changes either did not impact or have a minimal impact on the state categorization in my analysis. The status of abortion access did not change in three states (Iowa, Florida, and Wyoming) or impact the categorization of one state (South Carolina). Three states moved from no restriction to an abortion law (Montana, Nebraska, and North Carolina). Two states (Indiana and North Dakota) moved from an abortion law to an abortion ban. One state (Michigan) moved from an abortion law to no restriction and one state (Wisconsin) moved from an abortion ban to no restriction.

On June 30, 2023, the **Indiana** Supreme Court vacated the injunction on S.B. 1. The law, which bans abortion, took effect on August 1, 2023.

[1]Note: Some laws were passed in the spring of 2023, which was during the 2022-23 college application season. However, it is unlikely that these laws would have affected applicants' decisions in that season because the majority of applications were submitted by February 2023.

On July 14, 2023, **Iowa** Governor Reynolds signed H.F. 597, which banned abortion after the detection of a fetal heartbeat except in the case of a medical emergency, rape, incest, or lethal fetal anomaly. The law, which took effect upon enactment, was blocked by a state judge on July 17, 2023.

On April 13, 2023, **Florida** Governor DeSantis signed S.B. 300, which banned abortion after six weeks LMP except in the case of a medical emergency or lethal fetal abnormality, or rape or incest within the first 15 weeks of pregnancy. The law takes effect thirty days after the Florida Supreme Court rules on the case challenging the state's 15-week ban.

On November 8, 2022, **Michigan** voters approved Proposal 3, an amendment to protect abortion rights. On April 5, 2023, Governor Whitmer signed into law two pieces of legislation, H.B. 4006 and 4032, which repealed the 1931 law.

On May 3, 2023, **Montana** Governor Greg Gianforte signed S.B. 154, which stated that the right to privacy under the state's constitution does not include the right to abortion. On May 12, 2023, the Montana Supreme Court reaffirmed that the state constitution guarantees the right to seek abortion care. On May 16, 2023, Governor Gianforte signed H.B. 721, which banned the most common method of abortion that is used after approximately 15 weeks of pregnancy. The law was blocked by a state judge on May 18, 2023.

On May 22, 2023, **Nebraska** Governor Jim Pillen signed L.B. 574, which banned abortion starting at 12 weeks LMP, except in the case of a medical emergency, sexual assault, or incest. The law immediately took effect.

On May 16, 2023, the **North Carolina** state legislature overrode Governor Roy Cooper's veto of S.B. 20, which banned abortion after 12 weeks of pregnancy. A federal judge allowed the ban to take effect on July 1, 2023.

On April 24, 2023, **North Dakota** Governor Doug Burgum signed S.B. 2150, which made abortion a class C felony, punishable by up to five years in prison and a fine of $10,000. Exceptions include medical emergencies, and sexual assault or incest within the first six weeks of pregnancy. The law immediately took effect.

On May 25, 2023, **South Carolina** Governor McMaster signed S.B. 474, which made performing an abortion after the detection of a fetal heartbeat a felony except in the case of a medical emergency, rape, incest, or lethal fetal abnormality, punishable by up to 2 years in prison and a $10,000 fine. On August 23, 2023, the South Carolina Supreme Court upheld S.B. 474, which had been blocked by a state judge on May 26, 2023. The law is nearly identical to S.B. 1, which the state supreme court permanently struck down on January 5, 2023.

On September 18, 2023, abortion access resumed in **Wisconsin** after a state judge ruled on July 17, 2023 that the 1849 law did not apply to abortions.

On March 17, 2023, **Wyoming** Governor Gordon allowed H.B. 0152 to become law, which made performing an abortion a felony, except in the case of a medical emergency, rape, incest, or lethal fetal anomaly, punishable by up to 5 years in prison and a fine of $20,000. The law, which went into effect on March 19, 2023, blocked by a state judge on March 22, 2023.

Appendix B

Appendix to Chapter 2

B.0.1 Data Construction

I combine three sources of district administrative data: student school choice sets, student ranked school choice lists and school assignments, and student school enrollments. I use data from school years 2011-12 through 2017-18. Beginning in the 2018-19 school year, the district added additional metrics to its school quality framework. For comparability, I consequently restrict the analysis to school years that use only test score data to define school quality tiers.

The data include 36,880 students across three levels of early childhood programming: K0, K1, and K2. School assignment is guaranteed for all students who apply for full-day K2. Assignment is not guaranteed for K0 or K1, but K1 has increasingly become an entry point to the district. I subsequently drop K0 students, which removes 8,362 students. Of the remaining 28,518 students, 19,116 students meet the criteria for sample selection.

I start by defining students' choice sets and their ranked school choices. The district maintains separate data files for the schools that were in students' choice sets by school year and lottery round (which I refer to as choice set data), and the schools that students ranked in the lottery by school year and lottery round and were subsequently assigned (which I refer to as school assignment data). I use the choice set data to identify all of the schools (regardless of the lottery round) that students could rank in their first year of lottery participation. I use the school assignment data to identify the schools that students ranked in the first round in which they participated in the lottery.[1] I combine these data to create each student's complete choice set, in an effort to include all of the schools that students may have seen at the time of their ranked choices. I then identify all unique schools (schools may have appeared more than once in students' choice sets because of school programs). In Section 2.7, I use a more "conservative" version of choice set construction that limits choice sets to the schools that were in the choice set data in the first round and year in which students participated in the lottery.

To identify student race/ethnicity, I start with the race/ethnicity of each student in their initial year of lottery participation. If missing, I then assign student race/ethnicity based on data in future years when only one race/ethnicity is subsequently reported. The latter rule informs less than 0.5 percent of observations in the out-of-sample group.

I then restrict the sample using several criteria. First, I include only students who were new to the district and meaningfully participated in the lottery for the first time between 2011-12 and 2017-18. This excludes students who participated in the lottery in prior school years, who were previously enrolled in the district, and who did not rank at least one school. At the advice of the district, I also exclude from the analysis students with two special education codes because their school assignment process may be different than non-special education students. Second, I restrict the analysis to students who ranked a school with sufficient data to receive a school quality rating. This restriction removes students who ranked an early learning center or a school with insufficient data as their first choice and thereby will not have a measurable outcome. Third, the sample excludes students who were missing a home address in the data.

I use several decision rules to identify students' home addresses, which I need to calculate school-home distances. The choice set data file and the assignment data file each contain student addresses. I start with data from the assignment data file. I use student geocodes from the assignment data file and the corresponding X/Y address coordinates. I use the

[1] I use ranked choices from the first round of lottery participation as the best estimate of true preferences.

corresponding X/Y address coordinates from the choice set data file if the assignment data file is missing students' X/Y address coordinates and the choice set data and assignment data list the same geocode. If the assignment data file is missing student addresses, I use the geocode and corresponding X/Y coordinates from the choice set data file. I use the geocode centroids for students who are missing X/Y coordinates. I exclude students with multiple or missing addresses.

Fourth, I remove from the sample students with fewer than ten schools in their choice sets, which I use as an approximate floor in expected choice set size. According to district policy, every family should have a choice set with at least six schools, but most will have between ten and fourteen under the new assignment plan. Further, the district's lottery data reports up to the tenth choice in all school years.

There is significant overlap across the exclusion criteria. Of the 9,402 out-of-sample students, 21 percent was previously enrolled in the district, 11 percent previously participated in the lottery, and 20 percent did not rank at least one school. Nearly two-thirds (61 percent) of out-of-sample students did not rank a school with sufficient data to receive a school quality rating and approximately one-third (34 percent) of students had small choice sets. A very small percentage of out-of-sample students had special education codes (0.05 percent) or were missing a home address (0.1 percent).

Table B.2 shows how the composition of students changes based on the three main exclusion criteria. The first column includes all K1 and K2 students. The second column excludes students who were not new to the district and/or who did not meaningfully participate in the district's school choice lottery. The third column further excludes students who did not rank a school with sufficient data to receive a school quality rating. The fourth column, which includes all of the students in the final sample, further excludes students who had small choice sets. The composition of students is similar across all four columns of the table.

B.0.2 Tables

Table B.1: *Treatment Effects with Alternative Specifications*

	(1)	(2)	(3)	(4)
Panel A: Choice Set Quality Effect				
Q1 $\times$ After	0.268***	0.272***	0.270***	0.264***
	(0.040)	(0.045)	(0.037)	(0.040)
Q1	-0.518***	-0.513***	-0.592***	-0.509***
	(0.054)	(0.054)	(0.051)	(0.054)
After	0.077***	0.063***	0.047***	0.069***
	(0.020)	(0.022)	(0.018)	(0.020)
R-squared	0.04	0.04	0.04	0.04
N	18599	13172	21748	18308
Panel B: Information Effect				
Quality $\times$ After	0.182***	0.182***	0.159***	0.182***
	(0.025)	(0.030)	(0.022)	(0.025)
Quality	0.275***	0.295***	0.309***	0.273***
	(0.020)	(0.022)	(0.020)	(0.020)
After	0.038	0.024	-0.033	0.033
	(0.025)	(0.024)	(0.023)	(0.025)
R-squared	0.12	0.12	0.15	0.12
N	18599	13172	21748	18308

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

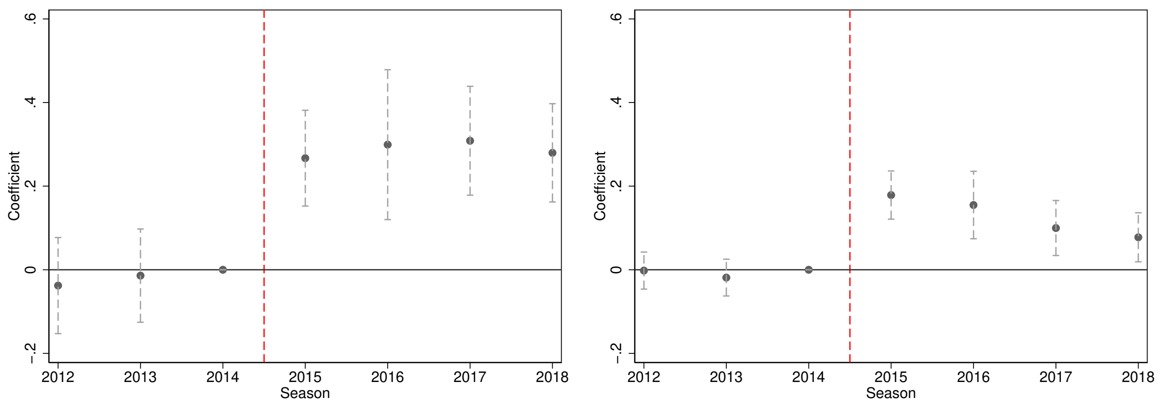
Notes: This table reports estimates from analyses that use alternative samples. In the first column, choice sets are limited to schools that appear in students' choice set data in the first round and year in which they participate in the lottery. In the second column, regressions are based on data from school years 2011-12 through 2015-16. In the third column, the sample does not remove students with fewer than then schools in their choice sets. In the fourth column, the sample excludes observations from school year 2015-16. Robust standard errors are clustered at the geocode-level.

Table B.2: *Student Composition Based on Selection Criteria*

	(1)	(2)	(3)	(4)
White	0.19	0.20	0.23	0.23
Asian	0.07	0.07	0.08	0.08
Black	0.29	0.29	0.26	0.26
Hispanic/Latino	0.37	0.38	0.38	0.38
Native	0.00	0.00	0.00	0.00
Mixed/Other	0.03	0.04	0.04	0.04
Missing race/ethnicity	0.05	0.02	0.02	0.00
ELL	0.37	0.36	0.36	0.37
Non-ELL	0.63	0.64	0.64	0.63
Sibling	0.58	0.59	0.58	0.58
K1	0.60	0.62	0.61	0.62
K2	0.40	0.38	0.39	0.38
Geographic coverage	1.00	0.99	0.99	0.99
Observations	28518	25384	21748	19116

Notes: This table shows how the composition of the sample changes based on the decision rules used to define the sample. The first column includes all K1 and K2 students. The second column excludes students who were not new to the district and/or who did not meaningfully participate in the lottery. The third column further excludes students who did not rank a school with sufficient data to receive a school quality rating. The fourth column, which includes all of the students in the final sample, further excludes students who had small choice sets. See Section B.0.1 for more details on data construction.

B.0.3 Figures



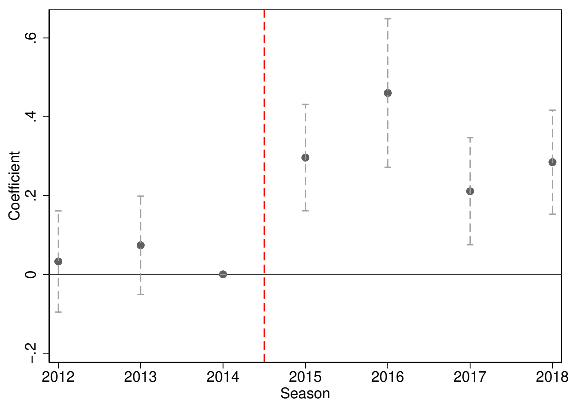
(a) *Choice Set Quality Effect*

(b) *Information Effect*

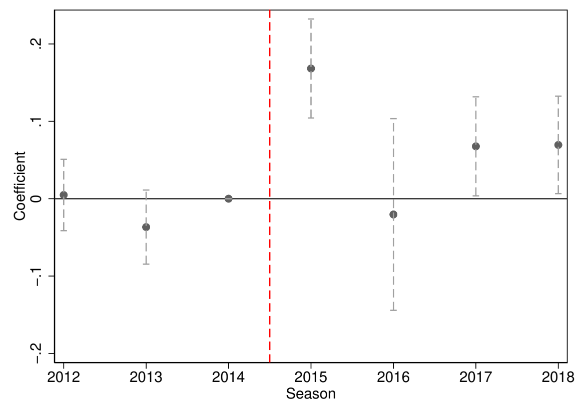
Figure B.1: *Event Studies (First-Choice Schools)*

Notes: The plotted coefficients correspond to interactions between the treatment (Q1 or Quality) and a set of dummy indicators interacted with the treatment. The outcome is the quality of students' first-choice schools. Regressions include a main effect for treatment school year fixed effects. School year 2013-14 is the omitted category. Robust standard errors are clustered at the geocode-level.

Panel A: Assigned Schools

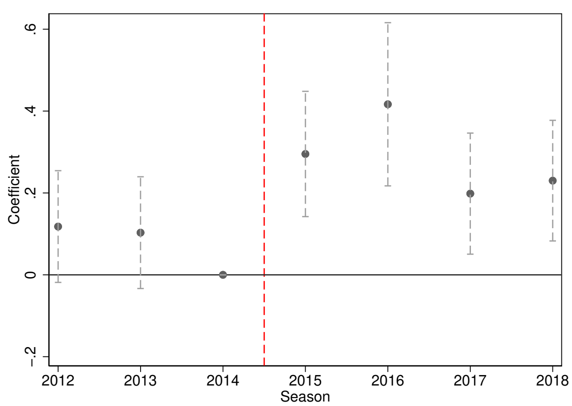


(a) *Choice Set Quality Effect*

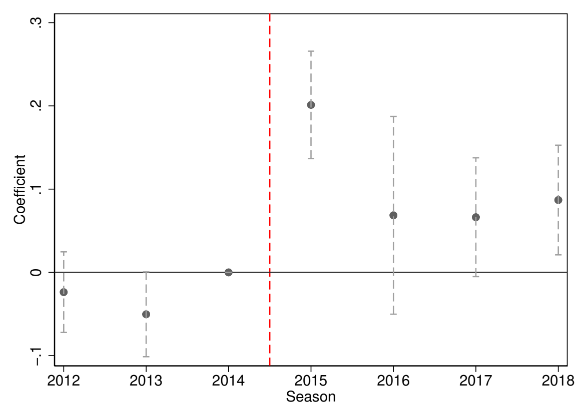


(b) *Information Effect*

Panel B: Enrolled Schools



(c) *Choice Set Quality Effect*



(d) *Information Effect*

Figure B.2: *Event Studies (Assigned and Enrolled Schools)*

Notes: The plotted coefficients correspond to interactions between the treatment (Q1 or Quality) and a set of dummy indicators interacted with the treatment. The outcome is the quality of students' first-choice schools. Regressions include a main effect for treatment school year fixed effects. School year 2013-14 is the omitted category. Robust standard errors are clustered at the geocode-level.

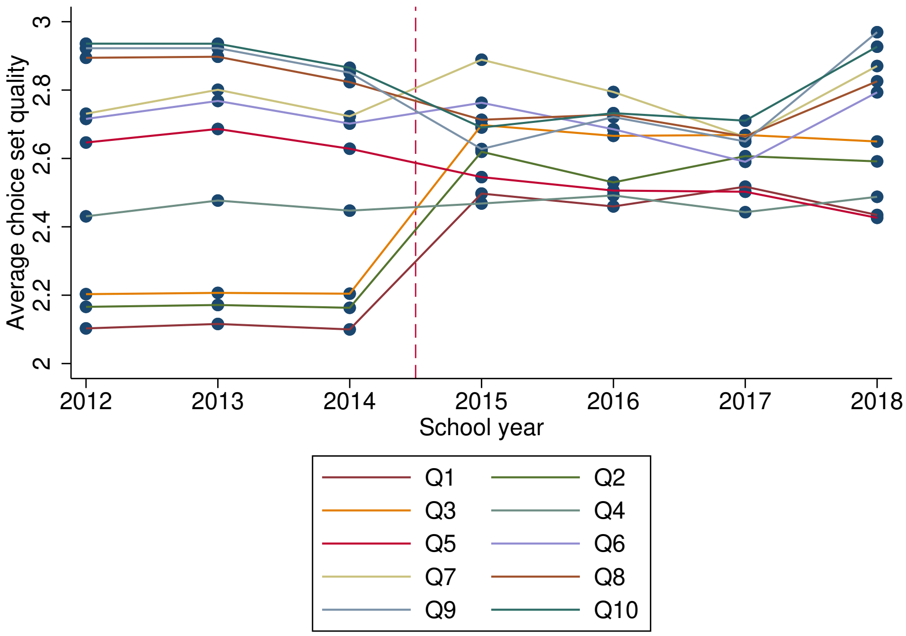


Figure B.3: *Average Choice Set Quality by Geocode Decile and Year*

Notes: This figure depicts average choice set quality by decile and school year. Geocodes are divided into deciles based on average choice set quality prior to the policy change in school year 2014-15..

Appendix C

Appendix to Chapter 3

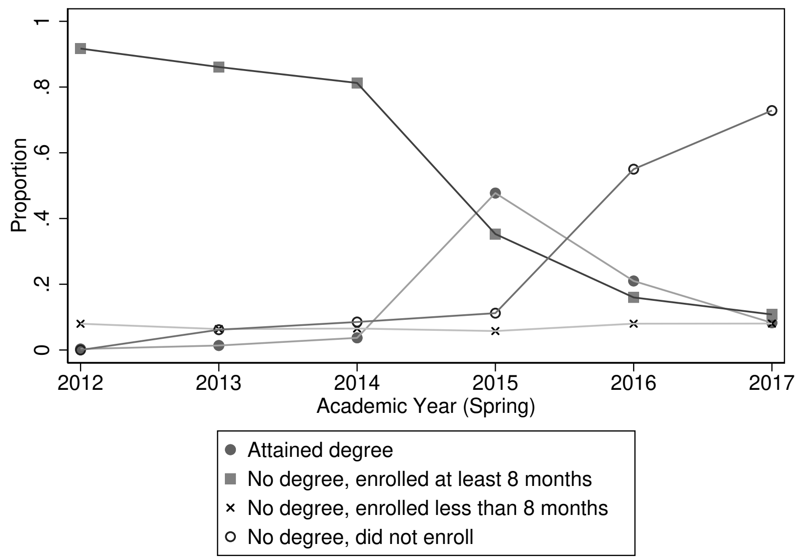
C.0.1 Tables

Table C.1: *Proportion of Imputed Workplace Preferences*

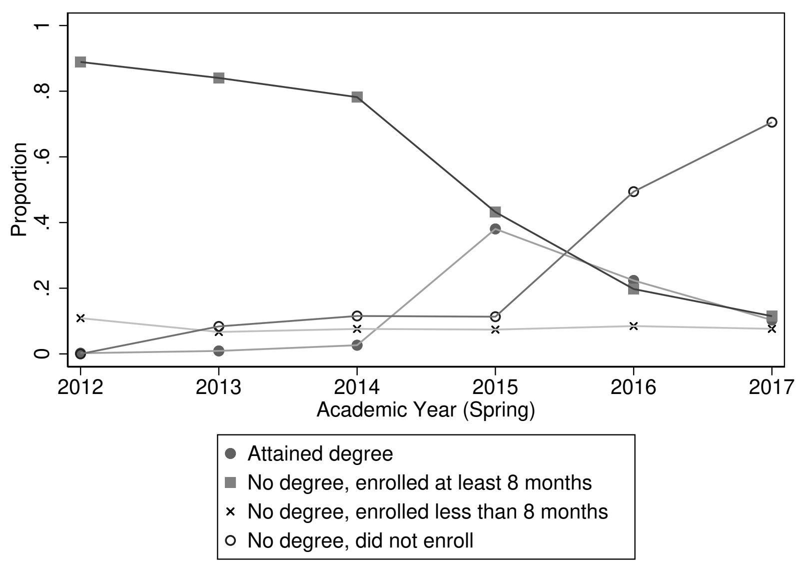
	(1)	(2)	(3)
Work/Family	0.000	0.080	0.147
Work/Leisure	0.000	0.079	0.147
Being an Expert	0.000	0.080	0.146
Helping Others	0.000	0.080	0.146
Making Decisions	0.000	0.081	0.148
Observations	8320	8320	8320

Notes: This table includes all respondents in the sample and reports the proportion of workplace preferences that were imputed each survey year. The first, second, and third columns correspond to the 2012, 2014, and 2017 survey years, respectively. Data (excluding N-sizes) are weighted using the BPS:12/17 panel weight and N-sizes are rounded to the nearest 10.

C.0.2 Figures



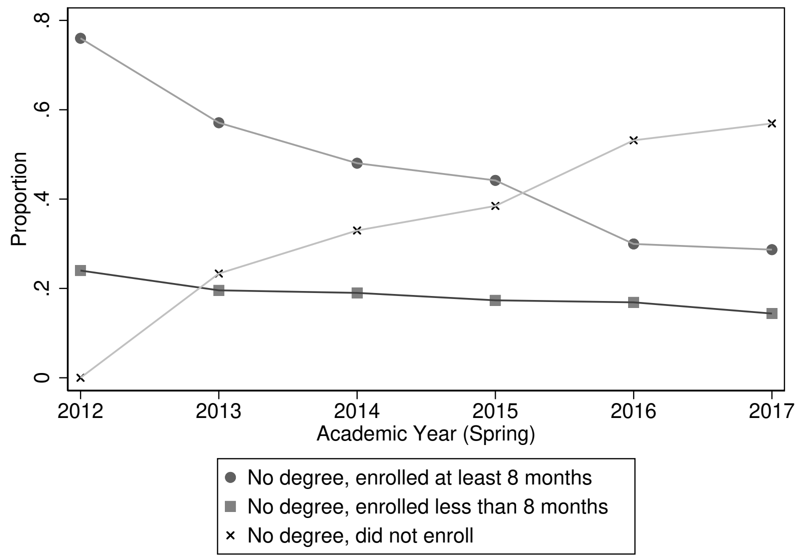
(a) *Women*



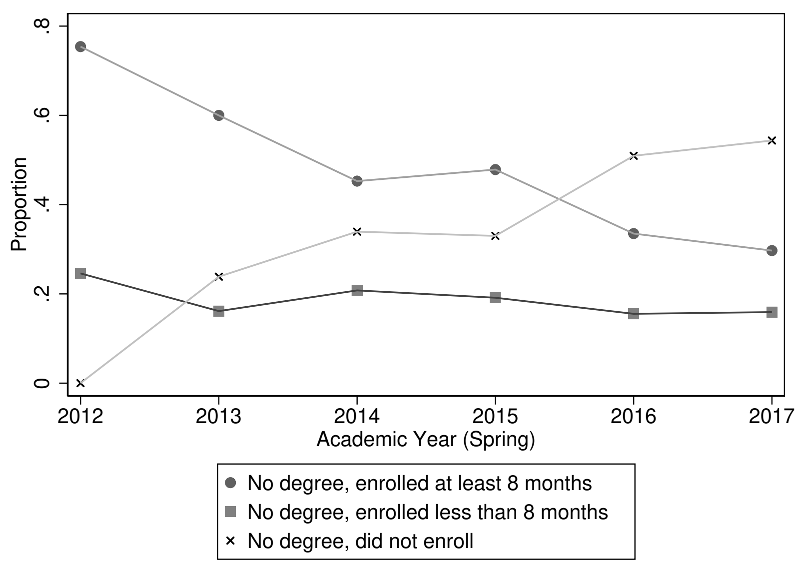
(b) *Men*

Figure C.1: *Degree Attainment and Enrollment Over Time by Gender*

Notes: This figure includes all respondents in the sample. Data are weighted using the BPS:12/17 panel weight.



(a) *Women*



(b) *Men*

Figure C.2: *Enrollment Over Time For Respondents Who Did Not Earn a Degree*

Notes: This figure includes all respondents in the sample. Data are weighted using the BPS:12/17 panel weight.

References

"2016 Presidential Election Results" (2017). *The New York Times*. URL: https://www.nytimes.com/elections/2016/results/president.

Abboud, Ali (2019). *The Impact of Early Fertility Shocks on Women's Fertility and Labor Market Outcomes*. SSRN Scholarly Paper. URL: https://papers.ssrn.com/abstract=3512913.

Abdulkadiroğlu, Atila, Joshua D. Angrist, Susan M. Dynarski, Thomas J. Kane, and Parag A. Pathak (2011). "Accountability and Flexibility in Public Schools: Evidence from Boston's Charters And Pilots". *The Quarterly Journal of Economics* 126.2, 699–748. URL: https://doi.org/10.1093/qje/qjr017.

Abdulkadiroğlu, Atila, Parag A. Pathak, Jonathan Schellenberg, and Christopher R. Walters (2020). "Do Parents Value School Effectiveness?" *American Economic Review* 110.5, 1502–1539. URL: https://pubs.aeaweb.org/doi/10.1257/aer.20172040.

Adkisson, Richard V. and James T. Peach (2008). "Non-resident enrollment and non-resident tuition at land grant colleges and universities". *Education Economics* 16.1, 75–88. URL: https://doi.org/10.1080/09645290701563156.

Adler, Aliza, M. Antonia Biggs, Shelly Kaller, Rosalyn Schroeder, and Lauren Ralph (2023). "Changes in the Frequency and Type of Barriers to Reproductive Health Care Between 2017 and 2021". *JAMA Network Open* 6.4, e237461. URL: https://doi.org/10.1001/jamanetworkopen.2023.7461.

Adrjan, Pawel, Svenja Gudell, Emily Nix, Allison Shrivastava, Jason Sockin, and Evan Starr (2023). "We've Got You Covered: Employer and Employee Responses to Dobbs v. Jackson". URL: https://www.iza.org/publications/dp/16360/weve-got-you-covered-employer-and-employee-responses-to-dobbs-v-jackson.

Altonji, Joseph G., Erica Blom, and Costas Meghir (2012). "Heterogeneity in Human Capital Investments: High School Curriculum, College Major, and Careers". *Annual Review of*

Economics 4.1, 185–223. URL: http://www.annualreviews.org/doi/10.1146/annurev-economics-080511-110908.

Anderson, Nick (2022). "College-shopping students have a new query: Is abortion legal there?" *Washington Post*. URL: https://www.washingtonpost.com/education/2022/07/22/state-abortion-access-college-decision/.

Angrist, Joshua D., Susan M. Dynarski, Thomas J. Kane, Parag A. Pathak, and Christopher R. Walters (2010). "Inputs and Impacts in Charter Schools: KIPP Lynn". *American Economic Review* 100.2, 239–243. URL: https://pubs.aeaweb.org/doi/10.1257/aer.100.2.239.

— (2012). "Who Benefits from Kipp?" *Journal of Policy Analysis and Management* 31.4, 837–860. URL: https://www.jstor.org/stable/41653798.

Angrist, Joshua D. and William N. Evans (1998). "Children and Their Parents' Labor Supply: Evidence from Exogenous Variation in Family Size". *The American Economic Review* 88.3, 450–477. URL: http://www.jstor.org/stable/116844.

Arcidiacono, Peter (2004). "Ability sorting and the returns to college major". *Journal of Econometrics* 121.1-2, 343–375. URL: https://linkinghub.elsevier.com/retrieve/pii/S030440760300263X.

Arcidiacono, Peter, V. Joseph Hotz, Arnaud Maurel, and Teresa Romano (2020). "Ex Ante Returns and Occupational Choice". *Journal of Political Economy* 128.12, 4475–4522. URL: https://www-journals-uchicago-edu.ezp-prod1.hul.harvard.edu/doi/full/10.1086/710559.

Arkhangelsky, Dmitry, Susan Athey, David A. Hirshberg, Guido W. Imbens, and Stefan Wager (2021). "Synthetic Difference-in-Differences". *American Economic Review* 111.12, 4088–4118. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20190159.

Arteaga, Felipe, Adam J Kapor, Christopher A Neilson, and Seth D Zimmerman (2022). "Smart Matching Platforms and Heterogeneous Beliefs in Centralized School Choice". *The Quarterly Journal of Economics* 137.3, 1791–1848. URL: https://doi.org/10.1093/qje/qjac013.

Bailey, Martha J. (2006). "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply". *The Quarterly Journal of Economics* 121.1, 289–320. URL: https://doi.org/10.1093/qje/121.1.289.

Banerjee, Asha (2023). *The economics of abortion bans*. Tech. rep. Economic Policy Institute. URL: https://www.epi.org/publication/economics-of-abortion-bans/.

Barrows, Samuel, Michael Henderson, Paul E. Peterson, and Martin R. West (2016). "Relative Performance Information and Perceptions of Public Service Quality: Evidence From American School Districts". *Journal of Public Administration Research and Theory* 26.3, 571–583. URL: https://doi.org/10.1093/jopart/muw028.

Baryla, Edward A. and Douglas Dotterweich (2006). "Institutional focus and non-resident student enrollment". *International Journal of Educational Management* 20.4, 239–248. URL: https://doi.org/10.1108/09513540610665360.

Baryla Jr, Edward A. and Douglas Dotterweich (2001). "Student Migration: Do Significant Factors Vary by Region?" *Education Economics* 9.3, 269–280. URL: http://ezp-prod1.hul.harvard.edu/login?url=https://search.ebscohost.com/login.aspx?direct=true&db=aph&AN=5596082&site=ehost-live&scope=site.

Becker, Gary S. (1962). "Investment in Human Capital: A Theoretical Analysis". *Journal of Political Economy* 70.5, 9–49. URL: https://www.jstor.org/stable/1829103.

Beffy, Magali, Denis Fougère, and Arnaud Maurel (2012). "Choosing the Field of Study in Postsecondary Education: Do Expected Earnings Matter?" *The Review of Economics and Statistics* 94.1, 334–337. URL: https://www.jstor.org/stable/41349179.

Bernstein, Sharon and Rose Horowitch (2022). "Abortion bans force U.S. students to rethink college plans". *Reuters*. URL: https://www.reuters.com/world/us/some-us-students-re-think-college-plans-states-with-abortion-bans-2022-07-11/.

Biskupic, Joan (2020). "The Supreme Court hasn't been this conservative since the 1930s". *CNN*. URL: https://www.cnn.com/2020/09/26/politics/supreme-court-conservative/index.html.

Blau, Francine D. and Lawrence M. Kahn (2017). "The Gender Wage Gap: Extent, Trends, and Explanations". *Journal of Economic Literature* 55.3, 789–865. URL: https://www.aeaweb.org/articles?id=10.1257/jel.20160995.

Bonaparte, Yosef, Alok Kumar, and Jeremy K. Page (2017). "Political climate, optimism, and investment decisions". *Journal of Financial Markets* 34, 69–94. URL: https://linkinghub.elsevier.com/retrieve/pii/S1386418117301155.

Bronson, Mary Ann (2015). "Degrees Are Forever: Marriage, Educational Investment, and Lifecycle Labor Decisions of Men and Women". *Department of Economics, Georgetown University*. Working Paper, 66. URL: https://economics.sas.upenn.edu/sites/default/files/filevault/event_papers/BronsonMajors.pdf.

Bryant, Jessica (2022). *Roe v. Wade Ruling Impacts College Choice for 39% of Prospective Students*. URL: https://www.bestcolleges.com/research/roe-v-wade-impacts-college-students/.

Burgess, Simon, Ellen Greaves, Anna Vignoles, and Deborah Wilson (2015). "What Parents Want: School Preferences and School Choice". *The Economic Journal* 125.587, 1262–1289. URL: https://onlinelibrary.wiley.com/doi/abs/10.1111/ecoj.12153.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro H. C. Sant'Anna (2021). *Difference-in-Differences with a Continuous Treatment*. URL: http://arxiv.org/abs/2107.02637.

Cha, Ariana Eunjung and Rachel Roubein (2021). "Fetal viability is at the center of Mississippi abortion case. Here's why." *Washington Post*. URL: https://www.washingtonpost.com/health/2021/12/01/what-is-viability/.

Chaisemartin, Clément de, Xavier D'Haultfoeuille, Félix Pasquier, and Gonzalo Vazquez-Bare (2022). *Difference-in-Differences Estimators for Treatments Continuously Distributed at Every Period*. URL: http://arxiv.org/abs/2201.06898.

Chetty, Raj, David J. Deming, and John N. Friedman (2023). *Diversifying Society's Leaders? The Causal Effects of Admission to Highly Selective Private Colleges*. Working Paper 31492. National Bureau of Economic Research. URL: https://www.nber.org/papers/w31492.

Chetty, Raj, John N. Friedman, Emmanuel Saez, Nicholas Turner, and Danny Yagan (2017). *Mobility Report Cards: The Role of Colleges in Intergenerational Mobility*. Working Paper 23618. National Bureau of Economic Research. URL: https://www.nber.org/papers/w23618.

Clarke, Damian, Daniel Pailañir, Susan Carleton Athey, and Guido W. Imbens (2023). *Synthetic Difference-in-Differences Estimation*. Discussion Paper 15907. IZA Journal of Labor Economics. URL: https://www.ssrn.com/abstract=4346540.

Cohodes, Sarah, Sean Corcoran, Jennifer Jennings, and Carolyn Sattin-Bajaj (2022). *When Do Informational Interventions Work? Experimental Evidence from New York City High School*

Choice. Working Paper 29690. National Bureau of Economic Research. URL: http://www.nber.org/papers/w29690.

Colonnelli, Emanuele, Valdemar Pinho Neto, and Edoardo Teso (2022). *Politics At Work*. Working Paper 30182. National Bureau of Economic Research. URL: https://www.nber.org/papers/w30182.

Corcoran, Sean P., Jennifer L. Jennings, Sarah R. Cohodes, and Carolyn Sattin-Bajaj (2018). *Leveling the Playing Field for High School Choice: Results from a Field Experiment of Informational Interventions*. Working Paper 24471. National Bureau of Economic Research. URL: https://www.nber.org/papers/w24471.

Daymont, Thomas N. and Paul J. Andrisani (1984). "Job Preferences, College Major, and the Gender Gap in Earnings". *The Journal of Human Resources* 19.3, 408–428. URL: https://www.jstor.org/stable/145880.

Deming, David J., Claudia Goldin, Lawrence F. Katz, and Noam Yuchtman (2015). "Can Online Learning Bend the Higher Education Cost Curve?" *American Economic Review* 105.5, 496–501. URL: https://www.aeaweb.org/articles?id=10.1257/aer.p20151024.

Deming, David J., Justine S. Hastings, Thomas J. Kane, and Douglas O. Staiger (2014). "School Choice, School Quality, and Postsecondary Attainment". *American Economic Review* 104.3, 991–1013. URL: http://pubs.aeaweb.org/doi/10.1257/aer.104.3.991.

Dench, Daniel, Mayra Pineda-Torres, and Caitlin Myers (2024). "The effects of post-Dobbs abortion bans on fertility". *Journal of Public Economics* 234, 105124. URL: https://www.sciencedirect.com/science/article/pii/S0047272724000604.

Diamond, Rebecca (2016). "The Determinants and Welfare Implications of US Workers' Diverging Location Choices by Skill: 1980–2000". *American Economic Review* 106.3, 479–524. URL: https://pubs.aeaweb.org/doi/10.1257/aer.20131706.

Dotzel, Kathryn R. (2017). "Do natural amenities influence undergraduate student migration decisions?" *The Annals of Regional Science* 59.3, 677–705. URL: https://doi.org/10.1007/s00168-016-0765-6.

Etezady, Ali, F. Atiyya Shaw, Patricia L. Mokhtarian, and Giovanni Circella (2021). "What drives the gap? Applying the Blinder–Oaxaca decomposition method to examine generational differences in transportation-related attitudes". *Transportation* 48.2, 857–883. URL: https://doi.org/10.1007/s11116-020-10080-5.

Figlio, David N and Maurice E Lucas (2004). “What’s in a Grade? School Report Cards and the Housing Market”. *American Economic Review* 94.3, 591–604. URL: https://pubs.aeaweb.org/doi/10.1257/0002828041464489.

Florida, Richard (2002). “Bohemia and economic geography”. *Journal of Economic Geography* 2.1, 55–71. URL: https://www.jstor.org/stable/26160379.

Fortin, Nicole M. (2008). “The Gender Wage Gap among Young Adults in the United States: The Importance of Money versus People”. *The Journal of Human Resources* 43.4, 884–918. URL: https://www.jstor.org/stable/40057374.

Fos, Vyacheslav, Elisabeth Kempf, and Margarita Tsoutsoura (2022). *The Political Polarization of Corporate America*. Working Paper 30183. National Bureau of Economic Research. URL: https://www.nber.org/papers/w30183.

Gemici, Ahu and Matthew Wiswall (2014). “Evolution of Gender Differences in Post-Secondary Human Capital Investments: College Majors”. *International Economic Review* 55.1, 23–56. URL: https://www.jstor.org/stable/24517161.

Gerber, Alan S. and Gregory A. Huber (2009). “Partisanship and Economic Behavior: Do Partisan Differences in Economic Forecasts Predict Real Economic Behavior?” *American Political Science Review* 103.3, 407–426. URL: https://www.cambridge.org/core/journals/american-political-science-review/article/partisanship-and-economic-behavior-do-partisan-differences-in-economic-forecasts-predict-real-economic-behavior/9643FC0E94207743C7B1A192AFFFBFAB.

Gerstein, Josh and Alexander Ward (2022). “Exclusive: Supreme Court has voted to overturn abortion rights, draft opinion shows”. *POLITICO*. URL: https://www.politico.com/news/2022/05/02/supreme-court-abortion-draft-opinion-00029473.

Glazerman, Steven (1998). “School Quality and Social Stratification: The Determinants and Consequences of Parental School Choice”. URL: https://eric.ed.gov/?id=ED425520.

Glazerman, Steven and Dallas Dotter (2017). “Market Signals: Evidence on the Determinants and Consequences of School Choice From a Citywide Lottery”. *Educational Evaluation and Policy Analysis* 39.4, 593–619. URL: https://doi.org/10.3102/0162373717702964.

Glazerman, Steven, Ira Nichols-Barrer, Jon Valant, and Alyson Burnett (2018). *Presenting School Choice Information to Parents: An Evidence-Based Guide. NCEE 2019-4003*. Tech.

rep. National Center for Education Evaluation and Regional Assistance. URL: https://eric.ed.gov/?id=ED589101.

Goebel, Craig, David Strauss, Nanci Tessier, and Richard Hesel (2023). "Politics, Policies, and Student Perspectives - The Impact of State Social Policies on College Choice". *Art & Science Group LLC* 16.1. URL: https://indd.adobe.com/view/f7cb9b98-22ad-433e-bd68-ef225093ab27.

Goldin, Claudia and Lawrence F. Katz (2002). "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions". *Journal of Political Economy* 110.4, 730–770. URL: https://www-journals-uchicago-edu.ezp-prod1.hul.harvard.edu/doi/full/10.1086/340778.

Green, Donald, Bradley Palmquist, and Eric Schickler (2002). *Partisan Hearts and Minds: Political Parties and the Social Identities of Voters*. Yale University Press. ISBN: 978-0-300-09215-8. URL: https://www.jstor.org/stable/j.ctt1npp6m.

Groen, Jeffrey A. (2004). "The effect of college location on migration of college-educated labor". *Journal of Econometrics*. Higher education (Annals issue) 121.1, 125–142. URL: https://www.sciencedirect.com/science/article/pii/S0304407603002525.

Groen, Jeffrey A. and Michelle J. White (2004). "In-state versus out-of-state students: the divergence of interest between public universities and state governments". *Journal of Public Economics* 88.9, 1793–1814. URL: https://www.sciencedirect.com/science/article/pii/S0047272703000768.

Grossman, Daniel, Carole Joffe, Shelly Kaller, Katrina Kimport, Elizabeth Kinsey, Klaira Lerma, Natalie Morris, and Kari White (2023). "Care Post-Roe: Documenting cases of poor-quality care since the Dobbs decision". *Advancing New Standards in Reproductive Health*. URL: https://psnet.ahrq.gov/issue/care-post-roe-documenting-cases-poor-quality-care-dobbs-decision.

Harris, Doug and Matthew Larsen (2015). *What Schools Do Families Want (And Why)? · Publications*. Tech. rep. URL: https://educationresearchalliancenola.org/publications/policy-brief-what-schools-do-families-want-and-why.

Hasan, Sharique and Anuj Kumar (2019). *Digitization and Divergence: Online School Ratings and Segregation in America*. SSRN Scholarly Paper. Rochester, NY. URL: https://papers.ssrn.com/abstract=3265316.

Hastings, Justine S., Thomas J. Kane, and Douglas O. Staiger (2006). *Parental Preferences and School Competition: Evidence from a Public School Choice Program*. Working Paper 11805. National Bureau of Economic Research. URL: https://www.nber.org/papers/w11805.

— (2009). *Heterogeneous Preferences and the Efficacy of Public School Choice*. Working Paper. National Bureau of Economic Research. URL: https://www.povertyactionlab.org/sites/default/files/research-paper/840%20Heterogeneous%20Preferences%20May%202009.pdf.

Hastings, Justine S., Christopher A. Neilson, Anely Ramirez, and Seth D. Zimmerman (2016). "(Un)informed college and major choice: Evidence from linked survey and administrative data". *Economics of Education Review*. Access to Higher Education 51, 136–151. URL: https://www.sciencedirect.com/science/article/pii/S0272775715000813.

Hastings, Justine S. and Jeffrey M. Weinstein (2008). "Information, School Choice, and Academic Achievement: Evidence from Two Experiments*". *The Quarterly Journal of Economics* 123.4, 1373–1414. URL: https://doi.org/10.1162/qjec.2008.123.4.1373.

Hickman, Daniel C. (2009). "The effects of higher education policy on the location decision of individuals: Evidence from Florida's Bright Futures Scholarship Program". *Regional Science and Urban Economics* 39.5, 553–562. URL: https://linkinghub.elsevier.com/retrieve/pii/S0166046209000362.

Houston, David M and Jeffrey R Henig (2021). "The Effects of Student Growth Data on School District Choice: Evidence from a Survey Experiment". *American Journal of Education* 127.4. URL: https://www.journals.uchicago.edu/doi/abs/10.1086/715003#:~:text=Providing%20growth%20data%20leads%20participants,provision%20of%20achievement%20data%20alone.

Hoxby, Caroline and Christopher Avery (2013). "The Missing "One-Offs": The Hidden Supply of High-Achieving, Low-Income Students". *Brookings Papers on Economic Activity* 44.1 (Spring), 1–65. URL: https://ideas.repec.org//a/bin/bpeajo/v44y2013i2013-01p1-65.html.

Hsing, Yu and Franklin G. Mixon (1996). "A Regional Study of Net Migration Rates of College Students". *Review of Regional Studies* 26.2, 197–209. URL: https://rrs.scholasticahq.com/article/8951-a-regional-study-of-net-migration-rates-of-college-students.

Huffman, David and John M. Quigley (2002). “The role of the university in attracting high tech entrepreneurship: A Silicon Valley tale”. *The Annals of Regional Science* 36.3, 403–419. URL: http://link.springer.com/10.1007/s001680200104.

Jacobsen, Rebecca, Jeffrey W. Snyder, and Andrew Saultz (2014). “Informing or Shaping Public Opinion? The Influence of School Accountability Data Format on Public Perceptions of School Quality”. *American Journal of Education* 121.1, 1–27. URL: https://www-journals-uchicago-edu.ezp-prod1.hul.harvard.edu/doi/full/10.1086/678136.

Jann, Ben (2008). “The Blinder–Oaxaca Decomposition for Linear Regression Models”. *The Stata Journal* 8.4, 453–479. URL: https://doi.org/10.1177/1536867X0800800401.

Jones, Kelly (2021). *At a Crossroads: The impact of abortion access on future economic outcomes*. American University. URL: https://ideas.repec.org/p/amu/wpaper/2021-02.html.

Kalist, David E. (2004). “Abortion and female labor force participation: Evidence prior to Roe v. Wade”. *Journal of Labor Research* 25.3, 503–514. URL: https://doi.org/10.1007/s12122-004-1028-3.

Kimport, Katrina (2022). “Abortion after Dobbs: Defendants, denials, and delays”. *Science Advances* 8.36, eade5327. URL: https://www.science.org/doi/10.1126/sciadv.ade5327.

Kinsella, Chad, Colleen McTague, and Kevin N. Raleigh (2015). “Unmasking geographic polarization and clustering: A micro-scalar analysis of partisan voting behavior”. *Applied Geography* 62, 404–419. URL: https://www.sciencedirect.com/science/article/pii/S0143622815001071.

Kirkebøen, Lars, Edwin Leuven, and Magne Mogstad (2014). *Field of Study, Earnings, and Self-Selection*. Working Paper 20816. National Bureau of Economic Research. URL: https://www.nber.org/papers/w20816.

Kyung, Wonseon (1996). “In-Migration of College Students to the State of New York”. *The Journal of Higher Education* 67.3, 349–358. URL: https://www.jstor.org/stable/2943848.

Lab, MIT Election Data and Science (2018). *County Presidential Election Returns 2000-2020*. URL: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/VOQCHQ.

Lambert, Stephanie J., Sarah K. Horvath, and Rachel S. Casas (2023). "Impact of the Dobbs Decision on Medical Education and Training in Abortion Care". *Women's Health Issues* 33.4, 337–340. URL: https://www.whijournal.com/article/S1049-3867(23)00079-8/fulltext.

Lang, Corey and Shanna Pearson-Merkowitz (2015). "Partisan sorting in the United States, 1972–2012: New evidence from a dynamic analysis". *Political Geography* 48, 119–129. URL: https://www.sciencedirect.com/science/article/pii/S0962629814001000.

Levine, P B, D Staiger, T J Kane, and D J Zimmerman (1999). "Roe v Wade and American fertility." *American Journal of Public Health* 89.2, 199–203. URL: https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1508542/.

Lindo, Jason M., Caitlin Knowles Myers, Andrea Schlosser, and Scott Cunningham (2020). "How Far Is Too Far? New Evidence on Abortion Clinic Closures, Access, and Abortions". *Journal of Human Resources* 55.4, 1137–1160. URL: http://muse.jhu.edu/pub/19/article/771324.

Liptak, Adam (2018). "Confirming Kavanaugh: A Triumph for Conservatives, but a Blow to the Court's Image". *The New York Times*. URL: https://www.nytimes.com/2018/10/06/us/politics/conservative-supreme-court-kavanaugh.html.

— (2021). "Supreme Court to Hear Abortion Case Challenging Roe v. Wade". *The New York Times*. URL: https://www.nytimes.com/2021/05/17/us/politics/supreme-court-roe-wade.html.

Liptak, Adam, J. David Goodman, and Sabrina Tavernise (2021). "Supreme Court, Breaking Silence, Won't Block Texas Abortion Law". *The New York Times*. URL: https://www.nytimes.com/2021/09/01/us/supreme-court-texas-abortion.html.

Long, Bridget (2004). "How have college decisions changed over time? An application of the conditional logistic choice model". *Journal of Econometrics*. Higher education (Annals issue) 121.1, 271–296. URL: https://www.sciencedirect.com/science/article/pii/S0304407603002549.

Manning, Alan and Joanna Swaffield (2008). "The Gender Gap in Early-Career Wage Growth". *The Economic Journal* 118.530, 983–1024. URL: https://doi.org/10.1111/j.1468-0297.2008.02158.x.

Marken, Stephanie and Zach Hrynowski (2023). "Reproductive Health Laws Factor Into Many College Decisions". *Gallup.com*. URL: https://news.gallup.com/poll/474365/reproductive-health-laws-factor-college-decisions.aspx.

— (2024). "State Reproductive Policies Important to Enrollment Decisions". *Gallup.com*. URL: https://news.gallup.com/poll/611453/state-reproductive-policies-important-enrollment-decisions.aspx.

McCartney, W. Ben, John Orellana, and Calvin Zhang (2021). *Sort Selling: Political Polarization and Residential Choice*. SSRN Scholarly Paper. Rochester, NY. URL: https://papers.ssrn.com/abstract=3799220.

McDonald, Ian (2011). "Migration and Sorting in the American Electorate: Evidence From the 2006 Cooperative Congressional Election Study". *American Politics Research* 39.3, 512–533. URL: https://doi.org/10.1177/1532673X10396303.

Meeuwis, Maarten, Jonathan A. Parker, Antoinette Schoar, and Duncan I. Simester (2018). *Belief Disagreement and Portfolio Choice*. Working Paper 25108. National Bureau of Economic Research. URL: https://www.nber.org/papers/w25108.

Mixon, Franklin G. and Yu Hsing (1994). "The determinants of out-of-state enrollments in higher education: A tobit analysis". *Economics of Education Review* 13.4, 329–335. URL: https://www.sciencedirect.com/science/article/pii/S0272775705800561.

Moretti, Enrico (2004). "Estimating the social return to higher education: evidence from longitudinal and repeated cross-sectional data". *Journal of Econometrics*. Higher education (Annals issue) 121.1, 175–212. URL: https://www.sciencedirect.com/science/article/pii/S0304407603002653.

Morgan, James N. (1983). "Tuition Policy and the Interstate Migration of College Students". *Research in Higher Education* 19.2, 183–195. URL: https://www.jstor.org/stable/40195561.

Mueller, Gerrit and Erik Plug (2006). "Estimating the Effect of Personality on Male and Female Earnings". *ILR Review* 60.1, 3–22. URL: https://doi.org/10.1177/001979390606000101.

Myers, Adam S. (2013). "Secular geographical polarization in the American South: The case of Texas, 1996–2010". *Electoral Studies* 32.1, 48–62. URL: https://www.sciencedirect.com/science/article/pii/S0261379412001229.

Myers, Caitlin (2024). *County-by-month travel distances to nearest abortion provider, Vintage January, 1, 2024*. URL: osf.io/pfxq3.

Myers, Caitlin Knowles (2017). "The Power of Abortion Policy: Reexamining the Effects of Young Women's Access to Reproductive Control". *Journal of Political Economy* 125.6, 2178–2224. URL: https://www.journals.uchicago.edu/doi/full/10.1086/694293.

— (2022). "Confidential and legal access to abortion and contraception in the USA, 1960–2020". *Journal of Population Economics* 35.4, 1385–1441. URL: https://doi.org/10.1007/s00148-022-00902-4.

Nyhus, Ellen K. and Empar Pons (2012). "Personality and the gender wage gap". *Applied Economics* 44.1, 105–118. URL: https://doi.org/10.1080/00036846.2010.500272.

Oaxaca, Ronald L. and Eva Sierminska (2023). "Oaxaca-Blinder Meets Kitagawa: What is the Link?" *SSRN Electronic Journal*. URL: https://www.ssrn.com/abstract=4464602.

Orgera, Kendal, Hasan Mahmood, and Atul Grover (2023). "Training Location Preferences of U.S. Medical School Graduates Post Dobbs v. Jackson Women's Health Organization Decision". *AAMC Research and Action Institute*. URL: https://www.aamc.org/advocacy-policy/aamc-research-and-action-institute/training-location-preferences.

Phillips, Amber (2021). "4 takeaways from the Supreme Court arguments on abortion". *Washington Post*. URL: https://www.washingtonpost.com/politics/2021/12/01/takeaways-scotus-mississippi-abortion/.

Rauch, James E. (1993). "Productivity Gains from Geographic Concentration of Human Capital: Evidence from the Cities". *Journal of Urban Economics* 34.3, 380–400. URL: https://www.sciencedirect.com/science/article/pii/S0094119083710429.

Reuben, Ernesto, Matthew Wiswall, and Basit Zafar (2017). "Preferences and Biases in Educational Choices and Labour Market Expectations: Shrinking the Black Box of Gender". *The Economic Journal* 127.604, 2153–2186. URL: http://onlinelibrary.wiley.com/doi/abs/10.1111/ecoj.12350.

Rothwell, Jonathan and Christos Makridis (2020). *Politics is wrecking America's pandemic response*. Tech. rep. The Brookings Institution. URL: https://www.brookings.edu/articles/politics-is-wrecking-americas-pandemic-response/.

Saad, Lydia (2023). “Broader Support for Abortion Rights Continues Post-Dobbs”. *Gallup.com*. URL: https://news.gallup.com/poll/506759/broader-support-abortion-rights-continues-post-dobbs.aspx.

Schneider, Jack, Rebecca Jacobsen, Rachel S. White, and Hunter Gehlbach (2018). “The (Mis)measure of Schools: How Data Affect Stakeholder Knowledge and Perceptions of Quality”. *Teachers College Record* 120.5, 1–40. URL: https://doi.org/10.1177/016146811812000507.

Semykina, Anastasia and Susan J. Linz (2007). “Gender differences in personality and earnings: Evidence from Russia”. *Journal of Economic Psychology* 28.3, 387–410. URL: https://www.sciencedirect.com/science/article/pii/S0167487006000420.

Shapiro, Jesse M. (2006). “Smart Cities: Quality of Life, Productivity, and the Growth Effects of Human Capital”. *The Review of Economics and Statistics* 88.2, 324–335. URL: https://www.jstor.org/stable/40042998.

Sjaastad, Larry A. (1962). “The Costs and Returns of Human Migration”. *Journal of Political Economy* 70.5, 80–93. URL: https://www.jstor.org/stable/1829105.

Sloane, Carolyn M., Erik G. Hurst, and Dan A. Black (2021). “College Majors, Occupations, and the Gender Wage Gap”. *Journal of Economic Perspectives* 35.4, 223–248. URL: https://www.aeaweb.org/articles?id=10.1257/jep.35.4.223.

Steele, Emma (2022). *Common App launches 2022-2023 application with more than 1,000 colleges and universities*. URL: https://www.commonapp.xyz/blog/common-app-launches-2022-2023-application-more-1000-colleges-and-universities.

Stephenson-Famy, Alyssa, Tammy Sonn, Laura Bacher-Lind, Rashmi Bhargava, Katherine Chen, Angela Fleming, Helen Kang Morgan, Christopher M. Morosky, Jonathan A. Schaffier, Shireen Madani Sims, Jill M. Sutton, and Celeste S. Royce (2023). “The Dobbs Decision and Undergraduate Medical Education: The Unintended Consequences and Strategies to Optimize Reproductive Health and a Competent Workforce for the Future”. *Academic Medicine* 98.4, 431–435(5). URL: https://www-ingentaconnect-com.ezp-prod1.hul.harvard.edu/content/wk/acm/2022/00000098/00000004/art00006.

Tam Cho, Wendy K., James G. Gimpel, and Iris S. Hui (2013). “Voter Migration and the Geographic Sorting of the American Electorate”. *Annals of the Association of American Geographers* 103.4, 856–870. URL: http://www.tandfonline.com/doi/abs/10.1080/00045608.2012.720229.

Tavernise, Sabrina (2019). "'The Time Is Now': States Are Rushing to Restrict Abortion, or to Protect It". *The New York Times*. URL: https://www.nytimes.com/2019/05/15/us/abortion-laws-2019.html.

Tavernise, Sabrina and Elizabeth Dias (2020). "The Supreme Court Stopped Anti-Abortion Momentum. For Now." *The New York Times*. URL: https://www.nytimes.com/2020/06/29/us/abortion-supreme-court.html.

Thaler, Richard and C. Sunstein (2009). *NUDGE: Improving Decisions About Health, Wealth, and Happiness*. Vol. 47. ISBN: 978-0-14-104001-1.

Tuckman, Howard P. (1970). "Determinants of College Student Migration". *Southern Economic Journal* 37.2, 184–189. URL: https://www.jstor.org/stable/1056128.

Turner, Sarah E. and William G. Bowen (1999). "Choice of Major: The Changing (Unchanging) Gender Gap". *Industrial and Labor Relations Review* 52.2, 289–313. URL: https://www.jstor.org/stable/2525167.

U.S. Department of Education, National Center for Education Statistics (2023a). *Admissions: How many applications for admission from first-time, degree/certificate-seeking undergraduate students were received by postsecondary institutions in the fall?* Admissions component final data (fall 2014-2020) and provisional data (fall 2021). URL: https://nces.ed.gov/ipeds/TrendGenerator/app/answer/10/101.

— (2023b). *Residence and Migration - How many degree-seeking undergraduate students are enrolled in postsecondary institutions for the first time in the fall?* Fall Enrollment component final data (2004 - 2018) and provisional data (2020). URL: https://nces.ed.gov/ipeds/TrendGenerator/app/answer/11/150.

Vogue, Ariane de (2021). "Takeaways from the historic Supreme Court arguments on abortion rights | CNN Politics". *CNN*. URL: https://www.cnn.com/2021/12/01/politics/takeaways-supreme-court-abortion/index.html.

Weixler, Lindsay, Jon Valant, Daphna Bassok, Justin B. Doromal, and Alica Gerry (2020). "Helping Parents Navigate the Early Childhood Education Enrollment Process: Experimental Evidence From New Orleans". *Educational Evaluation and Policy Analysis* 42.3, 307–330. URL: http://journals.sagepub.com/doi/10.3102/0162373720922237.

Winters, John V. (2011a). "Human Capital and Population Growth in Nonmetropolitan U.S. Counties: The Importance of College Student Migration". *Economic Development Quarterly* 25.4, 353–365. URL: https://doi.org/10.1177/0891242411418221.

Winters, John V. (2011b). “Why Are Smart Cities Growing? Who Moves and Who Stays”. *Journal of Regional Science* 51.2, 253–270. URL: http://onlinelibrary.wiley.com/doi/abs/10.1111/j.1467-9787.2010.00693.x.

Wiswall, Matthew and Basit Zafar (2015). “Determinants of College Major Choice: Identification using an Information Experiment”. *The Review of Economic Studies* 82.2 (291), 791–824. URL: https://www.jstor.org/stable/43551547.

— (2018). “Preference for the Workplace, Investment in Human Capital, and Gender”. *The Quarterly Journal of Economics* 133.1, 457–507. URL: https://doi.org/10.1093/qje/qjx035.

— (2021). “Human Capital Investments and Expectations about Career and Family”. *Journal of Political Economy* 129.5, 1361–1424. URL: https://www-journals-uchicago-edu.ezp-prod1.hul.harvard.edu/doi/10.1086/713100.

Zafar, Basit (2013). “College Major Choice and the Gender Gap”. *The Journal of Human Resources* 48.3, 545–595. URL: https://www.jstor.org/stable/23799096.

Zimmerman, Seth D. (2019). “Elite Colleges and Upward Mobility to Top Jobs and Top Incomes”. *American Economic Review* 109.1, 1–47. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20171019.