Health Womanpower

The Role of Federal Policy in Women's Entry into Medicine

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Abstract

During the 1970s, women's representation in medical schools grew rapidly from 9.6% of all students in 1970 to 26.5% in 1980. This paper studies the role of federal policy in increasing women's access to medical training through two distinct channels: pressure to curb sex discrimination in admissions and a massive expansion in total enrollment through Health Manpower policy starting in 1963. To study this, I construct a novel school-by-year dataset with enrollment and application information from 1960 through 1980. Using a continuous difference-in-differences design, I find that medical schools respond to the threat of losing federal contracts by increasing first year enrollment of women by 4 seats at the mean, which explains 25% of women's gains between 1970 and 1973. Further, leveraging the differential timing and size of enrollment increases across institutions, I conduct an accounting exercise that suggests that year-to-year expansions explain around 33% of women's gains from 1970 to 1980, with stronger effects later in the decade.

JEL codes: I28, J16, J78, N32

Key words: women in medicine, women and affirmative action, health manpower policy, medical school enrollment

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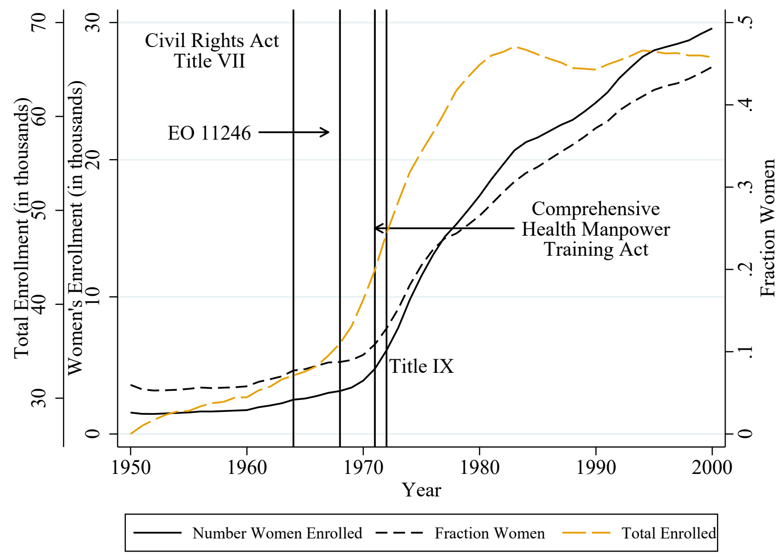
1 Introduction

Beginning in the early 1970s, women began to enroll in medical schools at historic rates. Figure 1 plots women's enrollment in both levels and as a percentage of total enrollment at all allopathic medical schools from 1950 through 2000. The growth rate of both time series changes abruptly around 1970 as there is a drastic increase in both the number of women in medical schools as well as the fraction of all medical students who are women. It is well known that anti-discrimination mandates likely played a role in women's progress during this time period, but it has so far been difficult to identify their impact (Goldin 2005). As Figure 1 illustrates, the time series evidence points to a sudden, episodic change in the early 1970s, indicating the potential role of public policy (Donohue III and Heckman 1991), but the obvious candidates do not seem to fit the bill. Title VI of the Civil Rights Act of 1964 had prohibited discrimination by any institution receiving federal funding, but sex was not included as a protected category, and educational institutions were explicitly exempt from the employment non-discrimination provisions in Title VII. Title IX of the Education Amendments Act of 1972, the most prominent federal action pursuing gender equity in higher education, broadly prohibited discrimination on the basis of sex for any institution receiving federal funding. Yet the change point in women's enrollment seems to come far too early for Title IX, which was not effective until 1973, to be the principal cause.

However, Title IX represented the culmination, rather than beginning, of activist efforts to pressure the government to take action. Sex discrimination was first integrated into the federal affirmative action effort with Executive Order 11375 in 1967, which amended Executive Order 11246 to prohibit federal contractors from discrimination in hiring on the basis of sex. Recognizing that many institutions of higher education were recipients of federal contracts, EO 11246 was utilized by the Women's Equity Action League (WEAL) to file around 250 complaints of non-compliance against colleges and universities, several of which led to investigations resulting in the withholding of federal funding (Suggs 2006). This paper will argue that it was this push that sparked women's entry into medical schools in the early 1970s, combined with a successful effort to codify sex non-discrimination through the legislature and amplified by a massive federal push to expand medical school enrollment in the 1970s.

The surge in women's enrollment in the 1970s was preceded by a vast increase in total enrollment that is also plotted in Figure 1. Starting in the mid-1960s, enrollment at allopathic medical schools undergoes a massive expansion, essentially doubling between 1965 and 1980. The increase in total enrollment was the result of several pieces of legislation under the

Figure 1: Trends in Medical School Enrollment, 1950-2000



This figure plots the total number of women enrolled, the total number of students enrolled, and their ratio at U.S. allopathic medical schools from 1950 through 2000. Data are collected from the *Journal of the American Medical Association*'s Education Number in various years between 1950 and 2001. In addition, I mark the passage of several important anti-discrimination policies—note that EO 11246 is dated in 1968, when sex was officially added to the list of protected classes. Women's enrollment in 1958 is unavailable and interpolated in the figure.

umbrella of Health Manpower Policy that incentivized growth through construction grants for teaching facilities in conjunction with direct payments to medical schools in exchange for increases in enrollment. This relaxation of capacity constraints is thought to be an important explanatory factor for increases in women's enrollment (Boulis and Jacobs 2008)—In an accounting sense, women's gains by 1980 comprised 49% of all seats created between 1965 and 1980, representing a 680% increase in women's enrollment. Accordingly, in this paper I consider how the totality of federal policy, including both anti-discrimination mandates as well as enrollment expansions, impacted women's entry into medical schools in the 1970s.

To do this, I construct a novel school-by-year dataset from 1960 through 1980 with institution-level first-year enrollment, graduates, and applicant counts split by sex. This allows me to characterize changes in the distribution of women across medical schools during their rapid entry in the 1970s, contributing to a nascent literature looking more deeply at women's access to professional schools (Katz et al. 2023). Aggregate statistics have revealed changes in women's attendance at graduate and professional programs (Goldin and Katz 2002), but more detailed institutional enrollment data in the Higher Education General Information Survey (HEGIS)[1] is not available at the degree level until the mid 1970s. These data allow me to utilize causal inference methods to understand the influence of institution-level changes on women's enrollment, adding to Moehling et al. (2019)'s study of women's access to the medical profession during a period of medical school closings from 1900-1960. Importantly, unlike previous work, I pool information across multiple data sources to construct a panel of first-year enrollment, allowing me to precisely estimate the timing of policy aimed at admissions policies.

In the first part of the paper, I provide causal estimates of the impact of anti-discrimination policy on women's enrollment in medical school. Reviewing action by the women's movement leveraging government policy to end sex discrimination in higher education, I identify a complaint filed by the Women's Equity Action League (WEAL) in October 1970 as the most likely point in time in which anti-discrimination policy would bite for medical schools. I collect data on the amount of funding provided by the Department of Health, Education, and Welfare (HEW) that would be at stake if a school were to violate this policy. Then, using a continuous difference-in-differences strategy, I show that schools with more exposure increase their enrollment of women at higher rates starting in the Fall of 1971. Specifically, I find that a medical school receiving the mean level of funding increases women's first-year enrollment by 4 seats, accounting for 25% of women's gains between 1970 and 1973. This design builds on an important contribution from Rim (2021), who leverages differences across institutions in the amount of federal funding received to measure the impact of Title IX on changes in women's graduate enrollment, and provides some of the first causal evidence verifiying the claim in Goldin and Katz (2002) that federal anti-discrimination played a role in the growth of women's enrollment in professional schools in the 1970s.

In the second part of the paper, I conduct an accounting exercise to estimate the fraction of seats created through Health Manpower Policy that are captured by women. Using a first-differences design, I regress the year-to-year change in women's enrollment on the change in total enrollment, leveraging variation across institutions in the timing and size of enrollment expansions. I find that enrollment expansions are most important for the growth in women's enrollment in the late 1970s, after the direct effects of anti-discrimination policy have subsided. My estimates suggest that between 1975 and 1980, women capture around 25% of newly created seats in the year that the expansion occurs, and up to 40% of these seats in the three years following an expansion. Further, my dataset allows me to study the heterogeneous impact of capacity creation across new and existing medical schools. Contrary

[1]This was the predecessor to the Integrated Postsecondary Education Data System (IPEDS).

to what has been asserted in the policy literature (More 1999; Boulis and Jacobs 2008), I do not find that additional seats at new medical schools were more likely to be filled by women than enrollment expansions at existing programs. This analysis also contributes to our understanding of how changes in the supply of college enrollment affects equilibrium outcomes, which has received little attention in the higher education literature (Blair and Smetters 2021).

My findings provide a clear picture of the role of federal policy in women's entry to medicine in the 1970s. In the first half of the decade, anti-discrimination policy begins to bind, allowing women to fill seats that men had previously held. I provide direct evidence for this claim by generating causal estimates of the effect of anti-discrimination policy on women's *and* men's enrollment, finding that men lose a similar number of seats at the exact same time women gain them. However, in later years, federal policy benefits women through incentivizing expansions in first-year enrollment. Women fill many of these new seats due to more effective anti-discrimination legislation in conjunction with a surge in demand for medical education. As a result, even though this push for expanded medical school enrollment was called Health "Manpower" Policy at the time, it proved important for giving women access to health professional training.

1.1 Related Literature

This paper contributes to a growing literature on the effectiveness of anti-discrimination policy in improving labor market outcomes (Beller 1979, 1983; Leonard 1989; Manning 1996; Bailey et al. 2024) and educational outcomes (Rim 2021) for women. There has been much work trying to understand if EO 11246 had improved labor market outcomes for women and Black workers. Early work utilized a difference-in-differences design comparing the progression of employment at firms with and without federal contracts, finding higher employment growth for Black workers at covered firms (Leonard 1984, 1990), with similar but small effects for white women (Leonard 1990). Recent work has extended this basic design to leverage variation over time in firm exposure to anti-discrimination policy. Kurtulus (2016) utilizes changes in contractor status over time for a panel dataset of firms, finding effects for Black and Native American men and women concentrated in the 1970s and early 1980s. Miller (2017) builds on this strategy, restricting the comparison group to firms that have never been contractors to avoid bias stemming from dynamic treatment effects (Goodman-Bacon 2021), finding that there are persistent effects of coverage even after a federal contract is completed.

There has also been much work on other anti-discrimination programs. Early work provided evidence that Title VII (rather than EO 11246) improved women's earnings (Beller 1979) and helped their entry into male-dominated professions (Beller 1983). However, leveraging changes in state anti-discrimination laws that predate federal action, Neumark and Stock (2006) find mixed evidence that these mandates benefited workers, finding earnings gains for Black workers but reduced employment for women. Bailey et al. (2024) utilize similar variation to study the impact of federal Equal Pay policy, finding that this was very effective at raising women's earnings, which was verified in a second design leveraging differential exposure to equal pay policy across job types. This paper adopts a similar empirical strategy in a panel setting, but instead focuses on educational outcomes, building on Rim (2021)'s study of the impact of Title IX.

Section 2 provides an overview of the state of medical school admissions practices in the 1960s, a descriptive look at how women's representation and access at medical schools changed across the 1960s and 1970s, as well as a description of how anti-discrimination policy evolved during this time period and its expected effects. Section 3 describes my empirical strategy to estimate the impact of federal anti-discrimination policy as well as my main dataset and presents results and discussion. Section 4 pivots to focus on Health Manpower policy, providing an overview of how this program developed and its expected impacts, then presents an accounting exercise to estimate its impacts as well as results and discussion. Section 5 concludes.

2 Medical Schools in the 1960s

In the 1960s, it was impossible to deny that women were underrepresented in the nation's medical schools—in each year between 1960 and 1969, women did not account for more than 9% of all medical students enrolled. Appendix Table E.3, reproduced from U.S. Congress (1970), pg. 528, gives a snapshot of enrollment at medical schools in 1966. There are a handful of progressive schools in this time period enrolling proportionally more women than the average by a substantial margin. However, the modal medical school is not very different from the average—as this table makes clear, by and large, women constitute a very small fraction of enrollees that does not differ terribly by institution. In other words, there was not an issue of access to a particular set of medical schools, but rather access to any medical school, with the exception of Women's Medical, which exclusively enrolled women.

At the time, analysts tended to point to gender differences in the demand for medical education, rather than discrimination by the admissions committee, as the central reason why women did not take up medicine in greater numbers (Lopate 1968; Epstein 1970). Defenders of the *status quo* were quick to point out that acceptance rates for men and women were consistently similar, arguing that this was evidence that admissions committees did not consider sex when evaluating applications. This argument was formalized by Cole (1986), who found that men were not admitted at higher rates from the entire period between 1924 and 1984.[2]

Despite these arguments, it was not at all difficult to establish that some medical schools were discriminating against women. Throughout the 1960s, the Association of American Medical Colleges (AAMC) would publish *Medical School Admission Requirements*, a yearly periodical intended to help prospective students in the application process. Included in each year starting in 1959 is a table containing preferences for each school over applicant characteristics, including sex, race, residency and age. In 1960, 21 medical schools (out of 86, excluding Women's Medical) reported that they considered applicant sex in the admissions process; a sample of the table in this year is presented in Figure 2. By 1970, this had dropped to 4 schools, but was still being reported by the AAMC.

Figure 2: Admissions Preferences in 1960

Table 5

Factors Considered in Medical Student Selection

Medical school	Some admission preference on the basis of each factor						Age preference	
	Residence	M.S. or other advanced degree	Undergraduate work at parent university	Sex	Race	Religion	Range	Exceptions
Cornell	No	No	No	No	No	No	20-25	Occasional
Creighton	No	No	Yes	No	No	Yes	20-30	Occasional
Dartmouth	No	No	Yes	No	No	No	20-26	Occasional
Duke	Yes	No	Yes	Yes	Yes	No	20-25	Occasional
Einstein (Yeshiva)	No	Yes	No	No	No	No	—	No policy
Emory	Yes	No	Yes	Yes	No	No	21-26	Occasional
Florida	Yes	Yes	Yes	Yes	No	No	21-29	Occasional
Georgetown	No	No	No	Yes	No	Yes	21-30	Occasional
George Washington	No	Yes	No	Yes	No	No	20-27	Occasional
Georgia	Yes	Yes	No	No	No	No	21-25	Occasional

An excerpt from *Medical School Admissions Requirements* in 1960. I include the header of the table as well as a snippet of ten rows.

What was less clear was the extent of the problem. In 1969, Women's Medical first began to consider male applicants, a decision that met resistance from alumni worried that

[2]Interestingly, women's advocates utilized this exact same statistic to conclude that there must be discrimination; in their letter to Congress, WEAL argues that this could not be the case unless admissions committees were utilizing information on sex to ensure admissions rates were identical (U.S. Congress 1971, pg. 874)

it would compromise opportunities for women to study medicine provided by a women-only institution (U.S. Congress 1971, pg. 563). To investigate the severity of the problem, the dean of Women's Medical interviewed admissions officers at 25 Northeastern medical schools, finding that 19 "admitted they accepted men in preference to women unless the women were demonstrably superior" (U.S. Congress 1971, pg. 872), suggesting that many schools acted in a discriminatory manner without admitting formally to preferences over sex.

Lopate (1968) reports that discrimination against women at medical schools manifested in a very particular way: "Prejudice against accepting women continues to exist, except that it is directed toward some future point when the 'minority group' might begin to apply in greater numbers." This was driven by a legitimate concern over an expected shortage of physicians in conjunction with an expectation that women were less likely to practice after graduation. In the words of an admissions officer,

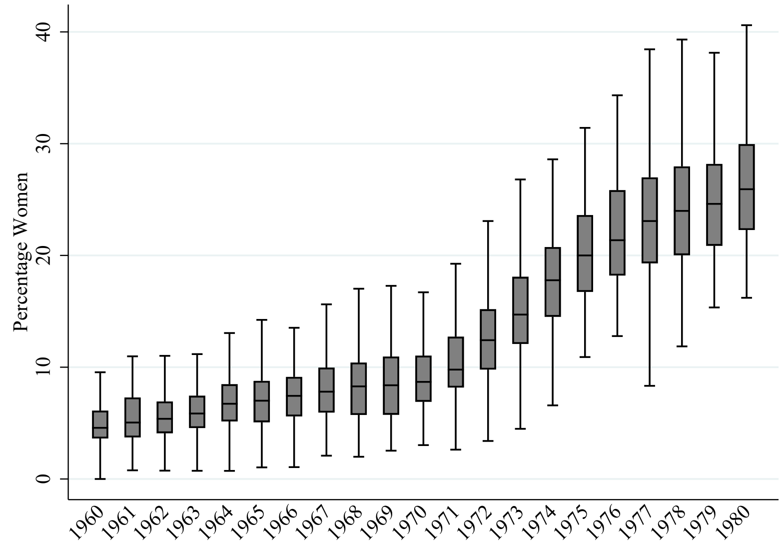
> With the predicted shortage of the 1970's we have to produce as many physicians as we can who will guarantee sufficient practice. If we accept a woman, we'd better make sure she will practice after she gets out. This year I had to insist that we only accept better-than-average women. (qtd. in Lopate 1968)

The expectation that women are less likely to practice was directly tied to family decisions. This line of reasoning is demonstrated succinctly by Bernice Sandler, here discussing all graduate admissions:

> If a woman is not married, she'll get married. If she is married, she'll probably have children. If she has children, she can't possibly be committed to a profession. If she has older children, she is too old to begin training. (U.S. Congress 1970)

This concern was compounded by higher attrition rates for women, though this was perversely at least partially the result of a male-dominated academic climate that was hostile towards women (Lopate 1968). Interestingly, though, while attrition among medical students was higher for women than for men, overall attrition in medical schools was far lower than other advanced degrees. Between 1948 and 1958, 8.69% of admitted students did not receive an M.D., with gender-specific attrition rates of 8.28% for men and 15.51% for women; for comparison, similar figures at law and engineering schools for overall attrition during this time period were 40% and 51%, respectively (Johnson and Hutchins 1966).

Figure 3: Evolution of Women's Representation



This figure plots a box and whisker plot summarizing the distribution of women's representation in medical schools in each year, excluding Women's Medical. I calculate the fraction of total enrollees who are women at each medical school in every year. For each year, the box plots the 25$^{\text{th}}$, 50$^{\text{th}}$, and 75$^{\text{th}}$ percentile of this distribution. The whiskers plot the upper and lower adjacent values.

2.1 Changes in the 1970s

As Figure 1 demonstrates, the *status quo* begins to dissolve in the 1970s as women entered medical schools in far greater numbers than before. To characterize the nature of this transition, I begin by establishing several stylized facts. I collect institution-level data on enrollment by sex at every medical school between 1960 and 1980 in the *Journal of the American Medical Association*'s Education Number.[3] Similar to Katz et al. (2023), I characterize entry with respect to two margins: representation among all medical students and overall access to medical education. Figure 3 plots the distribution across medical schools of the fraction of their students who are women. We see that women's representation increases across the board at all medical schools between 1970 and 1980, as evidenced by a shift upwards in this distribution. In particular, we see the most rapid changes between 1970 and 1975, with growth slowing in the second half of the 1970s. Simultaneously, we see a large increase in the spread of this distribution—by 1980, some medical schools have almost reached parity, but at others only 15% of students are women.

It is unclear from looking only at distributional changes how individual medical schools

[3] In Appendix A.1 I discuss construction of this dataset in more detail.

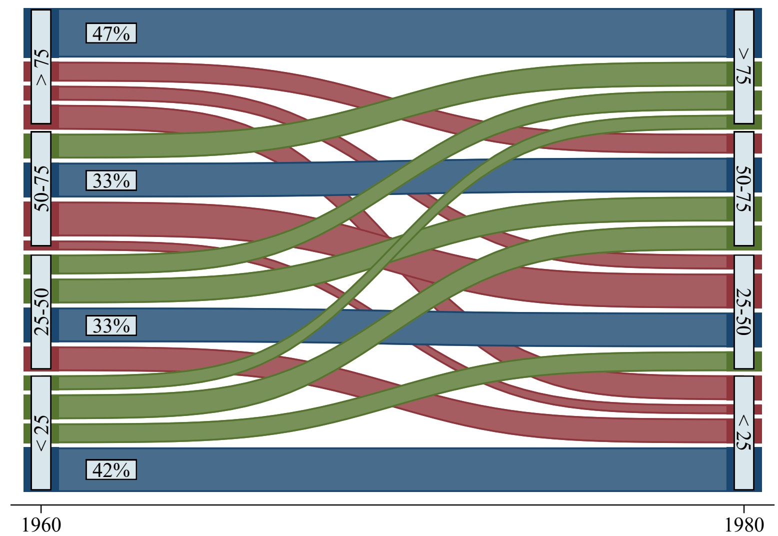
are evolving over time. To understand this, I split schools in Figure 3 into 4 groups, given by which quartile they fall into measured by the proportion of their students that are women in 1960.[4] I then calculate the fraction of schools in each group that end up in each quartile, defined similarly, at the end of my sample period in 1980. Figure 4 plots how schools flow from quartile to quartile between 1960 and 1980. A blue flow represents schools that remain at the same quartile between 1960 and 1980; a green flow represents schools that end up in a higher quartile (higher percentage of women students relative to other institutions) in 1980; and a red flow represents schools that end up in a lower quartile (lower percentage of women students relative to other institutions). At the tails of the distribution, there is evidence of persistence—almost half of schools in the top or bottom quartile in 1960 remain there in 1980. However, what is most striking is the preponderance of movement between quartiles, especially in the middle of the distribution. While indirect, this is another piece of evidence that the incentives to enroll a larger number of women might have changed between 1960 and 1980, in a way that is potentially orthogonal to existing admissions preferences.

In addition to representation, we might also be interested in women's access to all institutions: is women's enrollment spread evenly across institutions or more concentrated? To explore this, Figure 5 divides schools into quartiles based on the number of women enrolled, then plots the percentage of all women students enrolled within each quartile between 1960 and 1980, with Women's Medical (an all women's medical school) plotted in its own category. In 1960, women's access to medical schools was largely determined by a handful of institutions. Women's Medical enrolled around 10% of all women, and 60% of all female medical students were concentrated at 25% of all institutions. However, substantial progress was made throughout my sample period to increase women's enrollments at other institutions. By 1980, the top 25% institutions account for only 40% of women's enrollment driven by increases in women's enrollment across the distribution below the 75$^{\text{th}}$ percentile.

Both of these figures paint a distinct picture: women's enrollment increases in the aggregate because of changes across the distribution in women's admission to medical schools, rather than schools with low enrollment "catching up" to schools that had enrolled more women. As a result, women had access to a larger set of medical schools, with concentration at more female-friendly institutions decreasing between 1960 and 1980. Now, I turn to the task of determining what drove these changes, starting by describing the progression of federal anti-discrimination policy that occurred throughout the 1960s and early 1970s.

[4] For this exercise, I only include a balanced panel of schools that are in my dataset from 1960 through 1980, excluding Women's Medical.

Figure 4: Institutional Changes in Representation Across Time

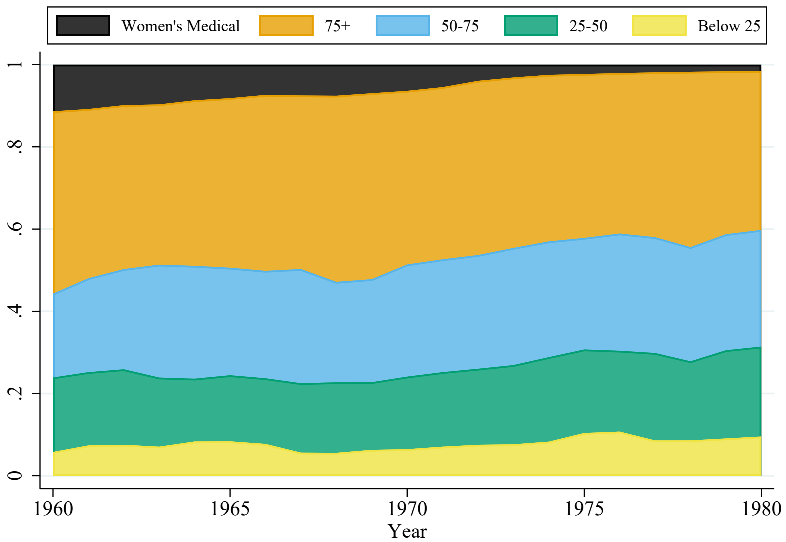


In 1960 and 1980, I calculate the 25^{th}, 50^{th}, and 75^{th} percentile of the distribution of the percentage of women enrolled in each school, excluding Women's Medical. I divide all medical schools that are in operation in both years into groups determined by which quartile they fall into in each year. This figure plots this data in a Sankey diagram depicting the flow of institutions between quartiles from 1960 to 1980. The width of each flow is proportional to the number of medical schools falling in that transition category. The color of each flow is determined by the nature of the transition: blue denotes programs staying in the same quartile, green denotes programs that transition to a higher (more representative) quartile, and red denotes programs that transition to a lower (less representative) quartile. For each quartile in 1960, I print the percentage of institutions that are in the same quartile in 1980.

2.2 Development of Policy

The fight against sex discrimination in higher education, which would ultimately lead to the passage of Title IX, was led early on by Bernice Sandler and the Women's Equity Action League (WEAL). As the 1960s came to a close, Sandler realized that there was already federal policy in place that prohibited sex discrimination in the hiring practices of colleges and universities (Suggs 2006). In 1965, President Johnson issued Executive Order 11246, which prohibited government contractors from discriminating in hiring on the basis of race, color, religion or national origin. However, this was amended in 1967 by Executive Order 11375 to include sex as a protected category, which went into effect in October 1968. Since most universities receive federal contracts, Sandler reasoned that they would be subject to this regulation. A newcomer to political action, Sandler placed a call to the Office of Federal Contract Compliance (OFCC), where she happened to be put in touch with Vincent Macaluso,

Figure 5: Evolution of Women's Access



For each year, I calculate the 25th, 50th, and 75th percentile of the distribution of the number of women in each school. This figure plots the percentage of women enrolled in schools in each quartile of this distribution. Woman's Medical, an all-women's medical school until 1970, is plotted separately as well.

who not only confirmed that she was correct but also helped Sandler draft complaints to ensure they would be effective (Fitzgerald 2020). On January 31, 1970, together with the Women's Equity Action League (WEAL), Sandler filed her first complaint under EO 11246, which called for a compliance review of all universities and colleges, with a specific complaint filed against the University of Maryland.

This complaint was passed along to the Department of Health, Education and Welfare (HEW), which was responsible for enforcement. By this point, HEW had been involved in enforcement of the racial non-discrimination provision of EO 11246; compliance guidelines were issued by the OFCC in 1968, and HEW was in the midst of several compliance investigations by the end of the decade (Fitzgerald 2020). Over the next two years, Sandler and WEAL continued to file EO 11246 complaints against around 250 institutions (Suggs 2006). HEW took these complaints seriously and began examining several universities—by the end of 1970, investigations were ongoing at the University of Maryland, recipient of the initial complaint, as well as Harvard, Loyola (Chicago), George Washington, the University of Pittsburgh, the University of Southern Illinois, and the University of Michigan (The New York Times 1970).

While initially attention was focused on hiring, action was broadened to include allegations of admissions discrimination at both the undergraduate and graduate level (Fitzgerald 2020). WEAL argued that graduate and professional admissions policies were subject to the executive order as they are analogous to training and apprenticeship programs, which are explicitly covered (Walsh 1971). These investigations were often lengthy battles between HEW and administration officials, involving the disclosure of relevant data by the university as well as negotiations over remedial action if a university was found to be in non-compliance, and HEW proved willing to withhold funding at any stage of this process. Institutions often did not want to provide data on hiring and admissions, but when Harvard refused to do so at the onset of a review, HEW held up millions in funding until the data were released (Harvard Crimson 1971). Further, the conclusion of these investigations resulted in the suspension of contracts for several institutions in the late 1970s and early 1971 until they complied with HEW demands (Bazell 1970).

2.2.1 Medical Schools

As WEAL continued to file complaints of EO 11246 violations, Sandler shifted her attention to the legislature, working as a consultant for Rep. Edith Green's Subcommittee on Higher Education (Suggs 2006). In June 1970, Green led a series of federal hearings on discrimination against women, in which medical schools featured prominently. Admissions data and several studies of admissions committees were presented, and testimony went as far as naming an explicit list of schools where "female enrollment figures are consistently, patently, discriminatory" (U.S. Congress 1970, pg. 512). Accordingly, it was no surprise when in October 1970, WEAL filed EO 11246 complaints against all medical schools in the country citing sex discrimination (More 1999).

Eventually, Sandler and Green would succeed with the passage of Title IX in 1972, but a similar ban on admissions discrimination was passed a year earlier for health professional schools. The Comprehensive Health Manpower Training Act (CHMTA), passed in November 1971, was the centerpiece of a federal push to increase enrollments at medical schools. It involved a host of programs including direct payments to medical schools in exchange for enrollment increases, matching funds for construction projects, and grants to alleviate financial distress at troubled institutions.[5] All of this funding could now be withheld if a medical school utilized discriminatory practices in its admissions process. The stipulation prohibiting sex discrimination in admissions was not in the original bill on the Senate floor, S. 934, but added later as an amendment which was maintained in the final version of the

[5] Details on these programs are provided in Section 4.

legislation (U.S. Congress 1970). This addition was likely the result of a successful lobbying effort on the part of the Women's Equity Action League (WEAL), which called for such an amendment during the hearings on S. 934.

Once enacted, enforcement fell to the Bureau of Health Manpower (BHM) of the Department of Health, Education and Welfare. From their report to congress, it appears that the BHM took this seriously, stating the requirement of non-discrimination as one of the "assurances" that must be provided by institutions before receiving a capitation grant (HEW BHM 1976). The BHM had access to admissions data through the grant application process, and it was given the power to visit medical schools to check on their progress on grants provided for special projects.

3 Contract Pressure

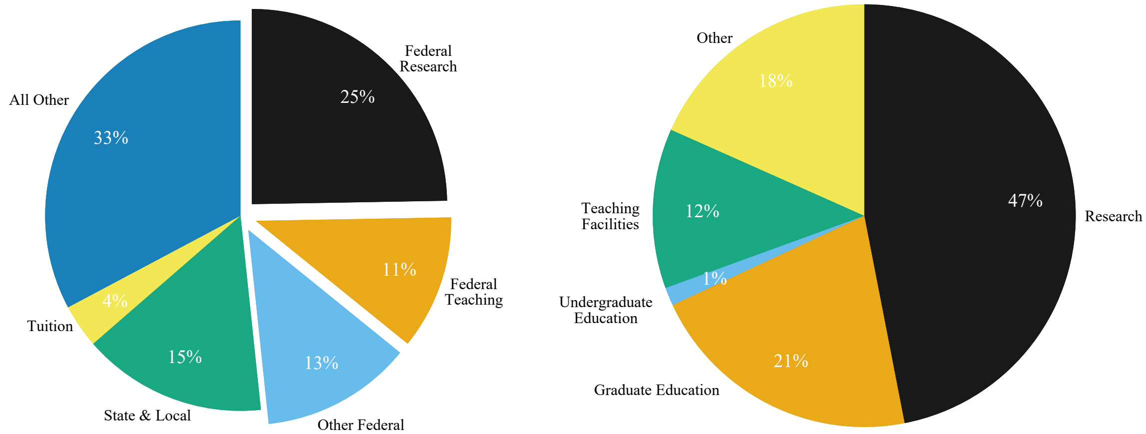
The "stick" wielded by the federal government in this context is its ability to delay funding to medical schools. The identifying assumption of my design is that medical schools receiving more of this funding should increase their enrollment of women by a greater amount in order to remain compliant with this law. Before introducing a formal empirical specification, I begin by providing some brief background on how medical schools are financed. I show that federal funding provides around half of total operations support, suggesting that the hold-up of this funding would pose a serious threat to the viability of an institution. After describing my preferred measure of federal dependence, I describe the data I utilize to test the hypothesis that anti-discrimination policy improved women's enrollment at medical schools. Following this, I introduce my main specification and provide results and discussion.

3.1 Medical School Finances

The medical school is a complex entity that has many functions besides classroom education, including clinical training of both prospective M.D.'s and residents, medical research, as well as providing care. These functions are financed through a host of revenue sources, including the federal and state government, tuition payments, as well as compensation for patient care in affiliated hospitals. Consequently, it is extremely difficult to tie a source of revenue to a particular function of the medical school (Townsend 1983), and I consider all funding as potentially at stake.

Institution-level data on revenue is scarce, but aggregate statistics on sources of funding for medical schools are available. In Figure 6a, I plot the share of all medical school revenue

Figure 6: Medical School Finances



(a) Medical School Revenues

(b) Source of HEW Support

In Figure A, I plot the percentage of total medical school support (across all institutions) in 1969 by source. All funding from the federal government is "popped out" on the right hand side. The data were collected from Fruen (1983) Table 1 and originated from the *JAMA* Education Number in various years. In Figure B, I plot the percentage of total medical school HEW support by program in fiscal year 1969. The data were collected from (HEW 1971).

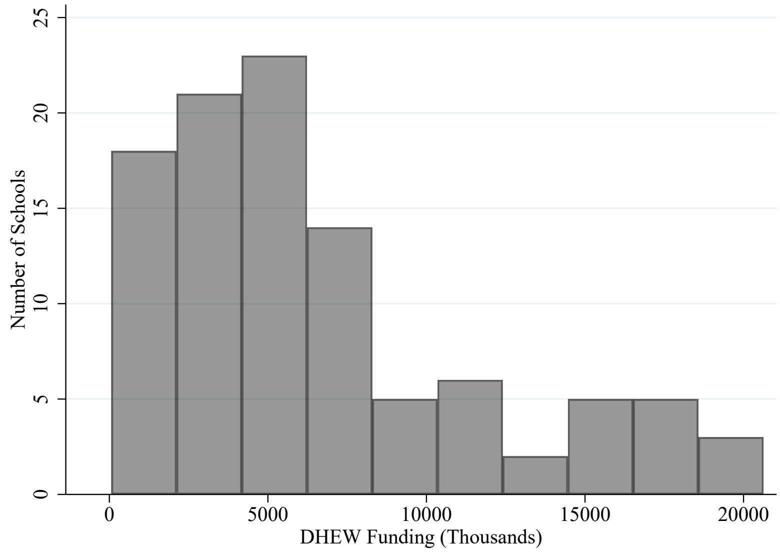
in 1969 by funding source, collected from Fruen (1983). Funding from the federal government comprises around half of all medical school revenue, with the bulk of this funding provided for research or teaching. This is the most important source of revenue for medical schools, significantly greater than the contribution from state and local government and tuition revenue combined. Further, by the end of the 1960s, this support had become even more important as an increasing number of medical schools experienced financial distress.[6] The problem had begun to reach crisis levels at particular programs, threatening their ability to stay afloat (The New York Times 1971). To alleviate this, beginning in 1968, the government had been providing financial distress grants for institutions under the health manpower program; by 1970, 61 of the existing 103 medical schools were receiving funding through this program.

To measure institutional reliance on government funding, I collect medical school-level data on total HEW obligations to medical schools in fiscal year 1969 (HEW 1971).[7] This will comprise the bulk, if not all, of federal support to medical schools—in 1969, total HEW obligations of $770m represent 103% of total federal support to medical schools in 1969 (Fruen

[6]It is worth noting here that raising tuition would likely not have been a viable solution—in 1969, tuition and fee revenue comprised under 4% of medical school financial support (Fruen 1983).

[7]Data is collected in 1969 instead of 1970 because of data availability restrictions.

Figure 7: Distribution of HEW Dose Variable



I plot a histogram of the distribution of my dose variable, which is the amount of total HEW funding provided to a school in 1969 less the amount designated for teaching facilities.

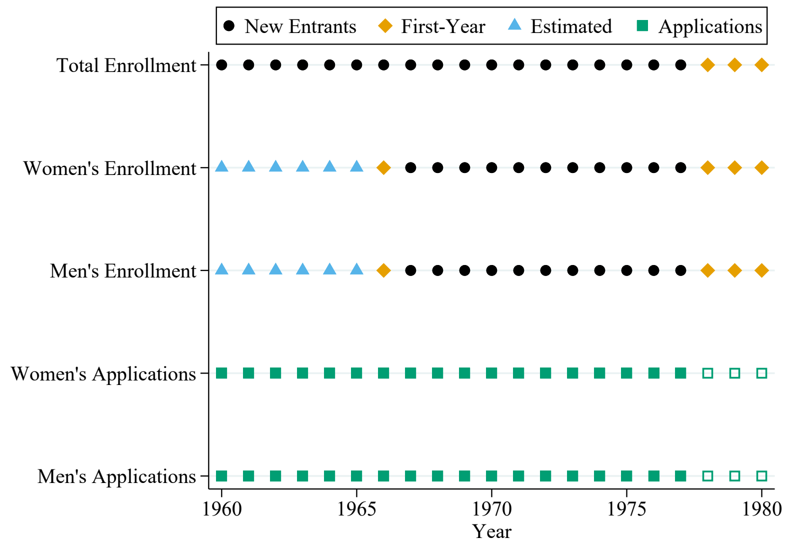
1983; HEW 1971).[8] Figure 6b breaks down this funding by program. The largest funding stream comes through research contracts and grants, which had been the primary way the federal government supported medical schools for the past several decades (Townsend 1983). However, as the government pursued its health manpower program in the 1960s, this focus had began to shift to construction support, as evidenced by the funding here for teaching facilities.

My preferred measure of medical school dependence on federal funding is the total amount of HEW support received in 1969, less any support for teaching facilities (e.g. construction grants), which are a temporary payment that do not necessarily reflect continued government support of a school.[9] I plot a histogram of this continuous variable in Figure 7. There is substantial variation among institutions in the amount of funding received; in particular, this distribution has a right skew, where several institutions receive outsized funding from HEW relative to the mean medical school. Denote this variable $d_{i,1969}$, where i identifies the institution. To understand if anti-discrimination policy has benefited women's enrollment, I need to measure how the relationship between enrollment and $d_{i,1969}$ has changed over time.

[8] This proportion is over 100% as obligations are not always paid in the same fiscal year as they are appropriated.

[9] I conduct a robustness check in Appendix Section D.1 using a simpler measure of research support.

Figure 8: Graphical Description of Dataset



This figure gives a visual description of how my panel dataset is constructed. For each main variable of interest, the marker in a given year indicates if the data from that year pertains to new entrants, all first-year students (new entrants and repeat students), or is estimated (in the spring of the previous year). Application information is included as well, where a hollow marker indicates that data is missing.

However, even if admissions policies adjust rapidly, total enrollment will change slowly, as it is a lagged function of women's admissions. To account for this, I construct a novel institution-by-year panel of first-year enrollment between 1960 and 1980 to obtain a much better metric of changes in medical school enrollment decisions.

3.2 Data and Sample

Fortunately, medical schools are unique among health professional schools in that there is consistent historic reporting of institution-level enrollment data. My main source of data is the Study of Applicants published yearly in the *Journal of Medical Education.* From 1967 - 1977, the Study of Applicants reports the number of new entrants, as well as applicants, for each medical school, split by sex. Unfortunately, data reporting from this source stops in 1977, and before 1967, enrollment figures are not split by sex.

Accordingly, to fill a complete panel, I bring in several other sources of data. I am able to collect first-year enrollment[10] in years 1966 and 1978-1980. In 1966, this information is

[10]This is not equivalent to new entrants as it includes students repeating the first year, though these students represent a miniscule portion of the first year class in medical schools. In Appendix A.5, I utilize

reported in the 1967 *Medical School Admission Requirements (MSAR)*; and in 1978-1980, this is reported in the Education Number, published yearly in the *Journal of the American Medical Association*. To extend the number of pre-periods I can study, I also collect information on estimated new entrants, split by sex, from 1960 - 1965 in the Education Number.[11] Figure 8 gives a graphical representation of the dataset I've constructed, showing the type of information used for each series in every year. Appendix A.2 includes a more detailed discussion of all data sources used.

I am able to collect data on the universe of institutions accredited by the Liason Committee on Medical Education (LCME), but I make a few sample restrictions. First, I drop all medical schools outside of the 50 United States, which excludes accredited schools in Canada and Puerto Rico. Second, and more importantly, I exclude Woman's Medical College of Pennsylvania, an all-women's medical school that became co-educational in 1970, as I am primarily interested in the entry of women into previously male-dominated institutions. I also exclude the Uniformed Services University of the Health Sciences, which educates students in the United States Uniformed Services.

3.3 Methodology & Specification

Using this panel dataset, I estimate a continuous difference-in-differences design with an event study specification:[12]

$$Y_{it} = \sum_{\tau=1960,\tau\neq 1970}^{\tau=1977} \alpha_\tau d_{i,1969} \mathbb{1}(t=\tau) + \boldsymbol{\beta}'\mathbf{X}_{it} + \gamma_i + \delta_{st} + \varepsilon_{it} \tag{1}$$

The outcome, Y_{it}, gives the number of women enrolled as new entrants at institution i in year t. $d_{i,1969}$ is my preferred measure of exposure to the policy, which is the total amount of federal funding institution i received from HEW in 1969.[13] This variable is interacted with a set of year dummies, omitting 1970. My parameter of interest, α_τ, captures changes

years where both new entrants and first-year enrollment are observed to verify that first-year enrollment explains almost all of the variation in new entrants ($R^2 \approx 1$).

[11] These estimates, while published in the Education Number, were first compiled for the *MSAR* in each year. These estimates are made in the spring after a large portion of the application cycle has completed, but there can be differences between these estimates and actual enrollment if, for example, an incoming student drops out. In Appendix A.5, I utilize years where estimated and actual enrollment are observed to verify that these estimates are accurate.

[12] In Appendix Section C, I present a simple theoretical model of the admissions decision to both motivate this specification and derive predictions on the expected policy impact.

[13] The distribution of this variable is plotted in Figure 7.

in the relationship between HEW funding and women's enrollment. If it was the case that this policy raised women's enrollment, we would expect that this relationship would change abruptly in 1971 and that $\alpha_{1971} > 0$. I include a long pre-period extending back to 1960 in order to check for pre-existing trends in this relationship, and I estimate dynamic effects through 1977, as this is the latest year in which all covariates are available.

My baseline specification includes institution fixed-effects γ_i to control for time-invariant differences in school preferences over women's enrollment and year fixed effects δ_t to account for year-to-year changes in women's demand for medical education. My baseline control $\mathbf{X}_{it}$ is the school's total enrollment, which adjusts for changes in women's enrollment attributable to total enrollment growth across institutions. I include two additional specifications to contend with potential confounders to my design. First, we might be concerned that women's enrollment is affected by changes in men's demand for medical education. Previous work has shown that the announcement of the Vietnam Wartime Draft by President Nixon in 1969 led to increased educational attainment by men (Card and Lemieux 2001), and the end of the draft in 1973 has been suggested as a cause of the increase in women's enrollment in medical school in particular (Boulis and Jacobs 2008). Accordingly, I include the number of applications filed by men to control for institution-specific changes in men's demand for medical education. Second, the introduction of oral contraception in 1960 had wide-reaching implications for U.S. women, leading to changes in fertility decisions (Bailey 2006) and age at first marriage (Goldin and Katz 2002). My third specification includes state-by-year fixed effects δ_{st} to control for differential access to the pill as states liberalized access at different times. For all designs, standard errors are clustered at the medical school level to correct for serial correlation (Bertrand et al. 2004).

To summarize my event study results, I also estimate a three-part linear spline of the form:

$$\begin{aligned} Y_{it} = {} & \alpha_1^s d_{i,1969}(t-1970) + \alpha_2^s d_{i,1969}(t-1970)\mathbb{1}(t>1970) \\ & + \alpha_3^s d_{i,1969}(t-1970)\mathbb{1}(t>1973) + \boldsymbol{\beta}'\mathbf{X}_{it} + \gamma_i + \delta_{st} + \varepsilon_{it} \end{aligned} \tag{2}$$

Here, I interact the dose $d_{i,1969}$ with event time $t-1970$ and estimate the slope of my event coefficients before 1970 ($\hat{\alpha}_1^s$), between 1970 and 1973 ($\hat{\alpha}_2^s$) and after 1973 ($\hat{\alpha}_3^s$). My main coefficient of interest, $\hat{\alpha}_2^s$, measures the break in slope after the EO 11246 filing, adjusting for an estimated pre-trend $\hat{\alpha}_1^s$. To summarize short-run effects, I report $3 * \hat{\alpha}_2^s * \bar{d}_{i,1969}$, which estimated the cumulative number of seats given to women between 1971 and 1973, relative to any pre-trend, at the mean of the dose distribution $\bar{d}_{i,1969}$.

3.4 Results & Discussion

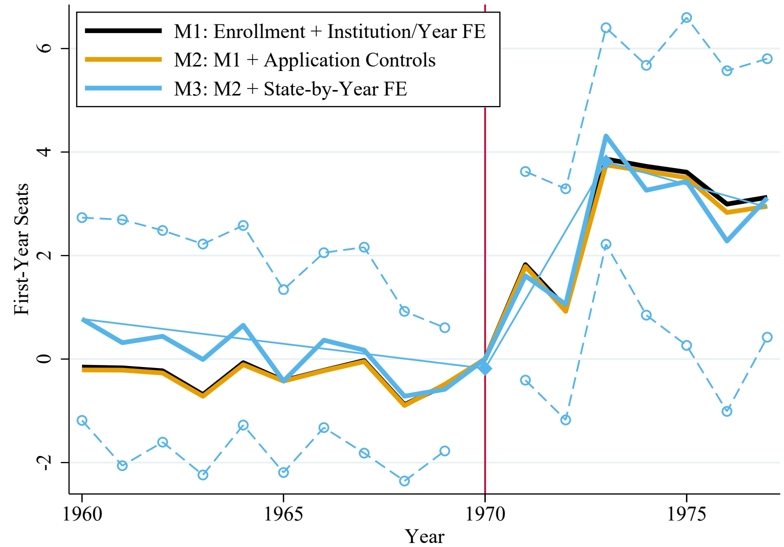
These results are presented in Figure 9a, and transformed spline estimates are reported in columns 1-3 of the first two rows of Table 1. Event coefficient estimates are scaled by the mean of the dose distribution so that they can be interpreted as the number of first-year seats added. For the 10 years prior to 1971, we see almost no change in the relationship between HEW funding and women's enrollment. This changes abruptly in 1971, and gains for women peak in 1973, likely buoyed by the anti-discrimination provisions in the Comprehensive Health Manpower Training Act and Title IX, which are passed in 1971 and 1972, respectively. At the mean, women gain 4 first-year seats as the result of this policy, which is a small but significant increase in enrollment. Across the 101 medical schools, this would create 404 first-years seats, accounting for around 25% of women's gains between 1970 and 1973, which translates roughly to an increase in enrollment of 1600 women across all years of schooling. Model 2 accounts for changes in men's applications, which changes the coefficient estimates very little, suggesting that increased demand from men between 1969 and 1973 did not affect women's entry in the early 1970s. Including state-by-year fixed effects introduces a bit of noise into the point estimates, but we still see a statistically significant gain of around 4 seats by 1973.

The primary threat to identification in this design is that other institutional characteristics, which correlate with HEW funding, might drive differential responses to an unrelated policy. Specifically, with the passage of the Comprehensive Health Manpower Training Act in 1971, we worry that better funded schools might have expanded enrollment more rapidly, causing an increase in women's enrollment. I test for this in two separate ways. First, in Appendix Section D.6, I run an identical design with total enrollment on the left-hand side to see if there is a similar response to total enrollment.[14] The results from this design suggest that there is little evidence for any response in total enrollment, and if anything, schools receiving more funding seem to experience a decline in enrollment in the 1970s. Second, this hypothesis would also predict increases in men's enrollment in the early 1970s; accordingly, to rule out this explanation, I run an identical design with men's enrollment on the left-hand side.[15] The results from this design are in Figure 9b, and spline estimates are reported in columns 4-6 of Table 1. Not only does this design rule out enrollment expansion as an alternative explanation, but it also gives insight into the nature of the institutional response.

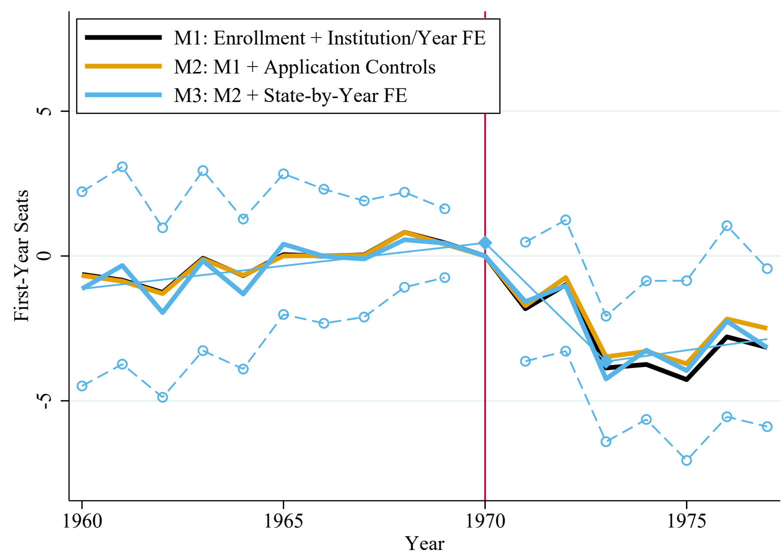
[14] Total enrollment is excluded as a control in these regressions.

[15] To preserve symmetry, M2 includes the number of applications submitted by women, but since women were not subject to the Vietnam draft, this control does not have the same significance.

Figure 9: Results for New Entrants



(a) Women



(b) Men

I plot the event study coefficients from equation (1) scaled by the mean of the dose distribution, where the outcome is women's (Figure 9a) or men's (Figure 9b) enrollment. Model 1 includes a control for total enrollment as well as institution and year fixed effects. Model 2 adds a control for men's (Figure 9a) or women's (Figure 9b) applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Additionally, I report spline estimates from equation (2) for model 3. Estimates end in 1977 as application data are not available after this year.

Table 1: Changes in Enrollment in Response to Anti-Discrimination Policy: Spline Estimates

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
First-Year Entrants						
Pre-Trend Change, 1960-1970	-0.049	0.008	-0.957	1.317	1.337	1.586
	(0.672)	(0.674)	(0.891)	(1.051)	(1.050)	(1.444)
Spline Estimate in 1973 at Mean Dose	3.896***	3.777***	4.281***	-4.534***	-4.144***	-4.569***
	(0.808)	(0.810)	(1.023)	(0.807)	(0.805)	(1.088)
Observations	1683	1683	1299	1683	1683	1299
Graduates						
Pre-Trend Change, 1960-1970	0.991	1.085	0.265	-0.991	-1.040	-0.401
	(0.680)	(0.677)	(0.849)	(0.680)	(0.668)	(0.823)
Spline Estimate in 1973 at Mean Dose	2.911***	2.828***	3.345***	-2.911***	-2.646***	-3.263***
	(0.899)	(0.919)	(1.147)	(0.899)	(0.925)	(1.165)
Observations	1642	1634	1287	1642	1634	1287
Applications						
Pre-Trend Change, 1960-1970	4.653	-14.679	-15.050	-90.545	-145.397**	-202.982*
	(15.039)	(17.249)	(25.223)	(76.524)	(70.646)	(111.959)
Spline Estimate in 1973 at Mean Dose	85.942**	78.221**	55.956*	191.711	172.922	160.982
	(34.380)	(34.838)	(28.782)	(148.142)	(149.744)	(145.776)
Observations	1684	1669	1280	1684	1669	1280
Total Enrollment	X	X	X	X	X	X
Applications/Tuition		X	X		X	X
State-by-Year Fixed Effects			X			X

This table reports transformed estimates from equation (2). The header of each section denotes the outcome variable: first-year entrants for section 1, graduates for section 2, and applications for section 3. Columns 1-3 report results for women, and Columns 4-6 report results for men. Model 1 (Columns 1&4) includes institution and year fixed effects, as well as a control for total enrollment. Model 2 controls for applications filed by the opposite sex for first-year enrollment/graduate outcomes and tuition controls for application regressions. Model 3 adds state-by-year fixed effects. All coefficients are scaled by the mean of the dose distribution so that they give an estimate of the change in seats/graduates/applications over a time period attributable to the dose variable. Within each section, Row 1 reports estimates of the pre-trend slope and Row 2 reports estimates of the cumulative change between 1971 and 1973 adjusted for the pre-trend slope. All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

The coefficient for men's enrollment in 1973 is around -4, suggesting that the seats allotted to women as a result of this policy would have been given to men if not for government intervention. This also verifies the prediction from the model in Appendix Section C that, conditional on total enrollment, a reduction in discrimination should result in an increase in women's enrollment matched by an equal decrease in men's enrollment.

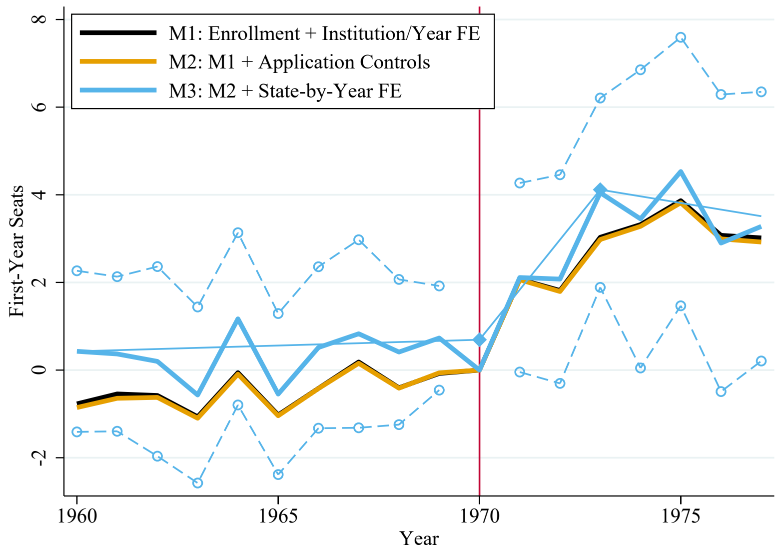
In the appendix, I show that these results persist through a variety of robustness checks. In Section B.1, I consider robustness to specifying the outcome as the percentage of enrolled first-year students who are women. While my spline estimates are robust to this change, the event study coefficients are substantially noisier. After limiting my sample to schools that experienced a smaller increase in enrollment across my sample period, I recover precision, suggesting that enrollment shocks (affecting the denominator of this outcome variable) are introducing noise into this design. In Section B.2, I consider recent advances in the difference-in-differences literature concerned with how selection into dose intensity can bias causal estimates (Callaway et al. 2024). Though I am limited by the nature of my identification strategy in what I can establish here, I show that HEW funding is unrelated with my variable of interest in 1969 (conditional on total enrollment), suggesting that it is unlikely that admission committee preferences for women are correlated with my measure of policy exposure. In addition, I show that my results survive the discritezation of my dose measure, where I use a set of schools receiving a low amount of HEW funding as control units. Finally, Appendix Section D provides a battery of robustness checks. These results are robust to a variety of alternate specifications (Section D.2), restricting my sample to a balanced panel (Section D.3), weighting by total enrollment (Section D.4), and including separate enrollment controls by year (Section D.5). Finally, Appendix Table E.2 presents a series of heterogeneity results. I find similar magnitudes of effects across public and private programs, as well as at university-affiliated schools. Most importantly, I find that similar effects when limiting my sample to the Northeast, Midwest, and West Census Regions, verifying that my results are not due solely to north-south differences.[16]

While changes in first-year enrollment are the clear place to look for an institutional response, they are likely not the most policy-relevant outcome; instead, we might be more interested in the production of medical school graduates, a better proxy for physician production. Accordingly, I consider a second design where my outcome $Y_{i,t+3}$ is now the number of women graduates from institution i in year $t+3$, adjusted to represent the fact that students admitted in year t will not graduate until year $t+3$.[17] Additionally, this is also a conve-

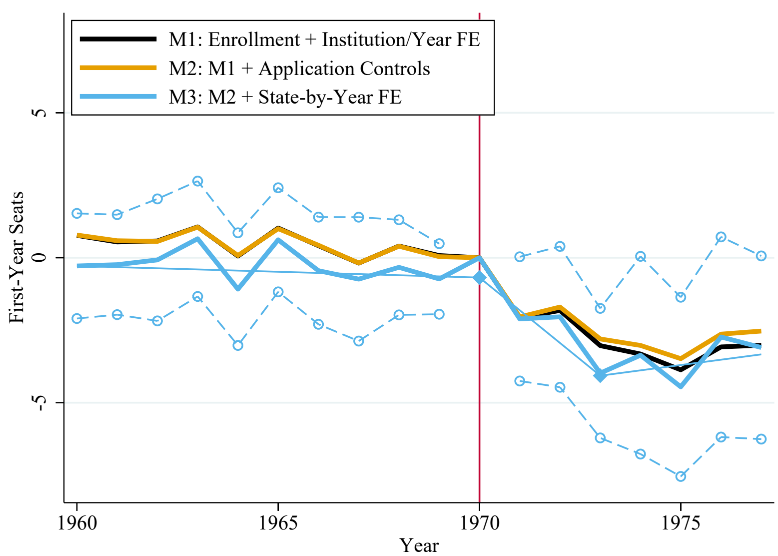
[16]Interestingly, I do not find significant effects in the south.

[17]This adjustment is 3 and not 4 years because my data are reported by academic year. A student admitted

Figure 10: Results for Graduates



(a) Women



(b) Men

I plot the event study coefficients from equation (1) scaled by the mean of the dose distribution, where the outcome is women's (Figure 10a) or men's (Figure 10b) graduates in $t+3$ years. Model 1 includes a control for total graduates in $t+3$ years as well as institution and year fixed effects. Model 2 adds a control for men's (Figure 10a) or women's (Figure 10b) applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Additionally, I report spline estimates from equation (2) for model 3. Estimates end in 1977 as application data are not available after this year.

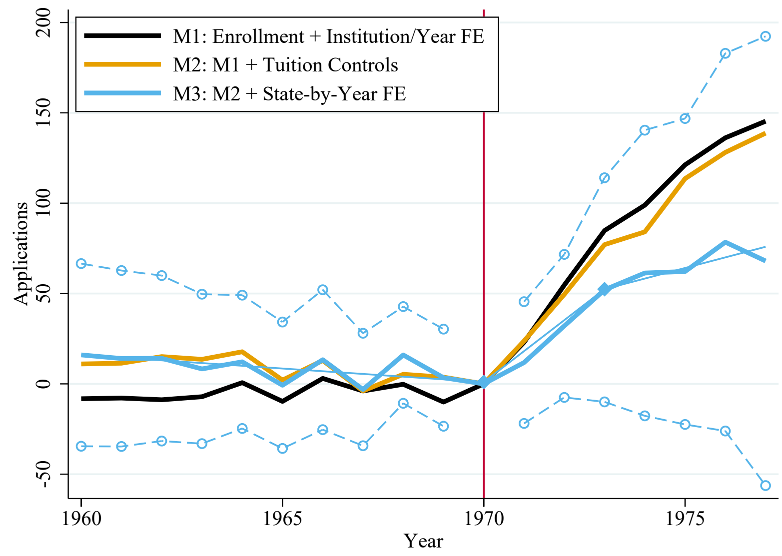
nient check on the use of multiple data sources to construct a consistent panel of first-year enrollment; I collect information on graduates from the Education Number in every year in my sample period, so these results should not be impacted by changes in the data source. These results are plotted in Figures 10a (Women) and 10b (Men), and the summary spline estimates are in rows 3-4 of Table 1. Point estimates here suggest that the increase in first-year seats filled has a direct impact on graduates. There is no guarantee that these estimates will be identical—not only is there attrition, but students from two-year programs generally transfer to a four-year program after completing the basic science curriculum, a process that could also be affected by government policy. My results suggest that changes in first-year enrollment are driving increases in women's graduation at more exposed institutions.

If there is a change in the willingness of medical schools to admit women, does this translate into changes in women's application behavior? There is reason to believe that this information would find its way to prospective applicants. Matriculant data at each school split by sex is generally available in *Medical School Admission Requirements*, which was published for use by prospective students. Further, the introduction of a computerized application system (American Medical College Application Service) in 1971 would have substantially lowered the marginal cost of an additional application, allowing students to respond to institutional changes by filing more applications. I study changes in the demand for medical education utilizing specification (1), where Y_{it} now gives the number of applications filed by women at institution i in year t. I include institutional fixed effects γ_i to account for pre-existing differences in women's application filing, and I include year fixed effects to account for national-level changes in women's application behavior. This is augmented to include controls for both resident and non-resident tuition in a second specification to adjust for changes in demand due to tuition increases.[18] Finally, I include state-by-year fixed effects in a third specification to control for changes in women's educational decisions stemming from differential access to the pill as noted before. Standard errors are clustered at the institution level. The results from this exercise are given in Figure 11, and spline estimates are reported in rows 5-6 of Table 1. All specifications suggest that women increased application effort at medical schools where women's enrollment jumped by a larger amount in response to the policy. However, the spline estimates in columns 4-6 indicate a substantial (though noisy) increase in application filing for men as well. In sum, then, I find limited,

in the year $t, t+1$ academic year will graduate at the end of their $t+3, t+4$ academic year, denoted year $t+3$ in my dataset. More detail on this data is given in Appendix Section A.3.

[18] As changes in men's demand do not crowd out women's applications, I do not include men's applications as a control. Details on the collection and construction of tuition data are given in Appendix Section A.4.

Figure 11: Applications: Results for Women



I plot the event study coefficients from equation (1) scaled by the mean of the dose distribution, where the outcome is women's applications. Model 1 includes institution and year fixed effects. Model 2 adds controls for resident and non-resident tuition. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level.

but suggestive evidence that this policy induced an increase in women's applications to more highly affected schools.

In addition to increases in the number of women enrolled in medical schools, we might also care about access to high quality schooling. To look at this, I bring in data from Cole and Lipton (1977), who conduct a survey of medical school faculty in 87 out of the 94 AMA-approved medical schools in 1971. For each medical school, they produce a "perceived quality score," which utilizes this survey data to order schools based on their quality as reported by medical faculty across the country, which I take as a reasonable metric of medical school quality. If there are differential effects across the quality distribution, it is unclear *ex ante* where these would obtain. In light of this, I explore the heterogeneity of my results across the quality distribution by a sequence of sample splits reported in Table 2.

Column 1 presents results for women using Model 2 from Table 1 estimated over all schools with a perceived quality score.[19] To begin, I estimate effects both above and below the median. Column 2 reports spline estimates from model 2 using only schools below the

[19] Model 2 is my preferred specification for this exercise as estimating effects across the quality distribution requires subsetting the data, and I lose precision quickly with the inclusion of state-by-year fixed effects.

Table 2: Differences Across the Quality Distribution: Spline Estimates

	(1) All Schools	(2) Below Median	(3) Above Median	(4) First Tercile	(5) Second Tercile	(6) Third Tercile
First-Year Entrants						
Pre-Trend Change, 1960-1970	-0.006	0.288	-0.111	0.354	-0.327	-0.204
	(0.069)	(0.227)	(0.101)	(0.357)	(0.296)	(0.136)
Spline Estimate in 1973 at Mean Dose	3.738***	5.468*	4.394***	9.556***	2.481	4.629**
	(0.862)	(2.884)	(1.063)	(3.259)	(2.485)	(1.797)
Observations	1580	765	815	508	512	560
Graduates						
Pre-Trend Change, 1960-1970	0.095	0.576**	-0.042	0.626	0.225	-0.117
	(0.073)	(0.237)	(0.121)	(0.380)	(0.386)	(0.176)
Spline Estimate in 1973 at Mean Dose	2.351**	2.421	3.547***	7.776***	-3.300	3.582*
	(0.952)	(2.568)	(1.268)	(2.594)	(2.478)	(2.096)
Observations	1569	764	805	509	501	559
Applications						
Pre-Trend Change, 1960-1970	-2.154	11.499	-1.596	7.221	-9.528	1.725
	(2.100)	(11.687)	(2.888)	(13.881)	(14.976)	(2.815)
Spline Estimate in 1973 at Mean Dose	56.757	-101.610	27.159	-120.587	-75.655	22.283
	(38.616)	(115.513)	(47.781)	(170.461)	(147.192)	(57.306)
Observations	1576	761	815	507	509	560

This table reports transformed estimates from equation (2). The header of each section denotes the outcome variable: first-year entrants for section 1, graduates for section 2, and applications for section 3. Column 1 gives results for all schools that have a perceived quality score from Cole and Lipton (1977). Columns 2 & 3 report results of the same regression model where the sample has been restricted to programs below and above the median perceived quality score, respectively. Columns 4-6 report results of the same regression model where the sample has been restricted to programs in the first, second, and third tercile of the perceived quality score distribution, respectively. All coefficients are scaled by the mean of the dose distribution so that they give an estimate of the change in seats/graduates/applications over a time period attributable to the dose variable. Within each section, Row 1 reports estimates of the pre-trend slope and Row 2 reports estimates of the cumulative change between 1971 and 1973 adjusted for the pre-trend slope. All specifications include institution and year fixed effects, as well as controls for total enrollment and men's applications (first-year enrollment/graduate outcomes) or resident and non-resident tuition (application outcomes), equivalent to M2 in Table 1). All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

median of the perceived quality distribution, and Column 3 reports identical estimates for schools above the median. Comfortingly, my results are present for both higher and lower quality schools, suggesting that women are not only given access to lower quality medical schools. However, this split masks some interesting heterogeneity. In columns 4-6, I consider a similar split by tercile. This reveals that the strongest effects are present and the upper and lower parts of the quality distribution. It is important to note that these results are, in some sense, mechanical—as research quality is an input into the perceived quality of a medical school, if institutions that produce better research receive more federal funding, high quality medical schools should receive relatively more federal funding.[20] Interestingly, I also find very strong effects for medical schools below the first tercile of perceived quality, which could reflect women's greater enrollment growth at less well established medical schools, which is explored in more depth in the next section. In Appendix Table E.1, I present estimates split at each quartile, which broadly confirm this pattern; past this point, there is a substantial loss in precision of my estimates.

4 Expansionary Policy

In the previous section, I found that anti-discrimination policy increased women's enrollment by around 1600 seats, which explains around 25% of women's gains between 1970 and 1973. While an important driver of growth during this time period, women's entry continues through the second half of the 1970s, which leaves plenty of room for complementary explanations. I now turn to exploring the role of policy aimed at expanding the capacity of existing medical schools and constructing new medical schools.

4.1 Development of Policy

By the start of the 1960s, the federal government was increasingly concerned about a projected shortage of physicians in the coming decades. Recognizing that in order to increase the supply of health professionals in the 1970s the nation would have to act far earlier, Congress passed the Health Professions Educational Assistance (HPEA) Act in 1963. This legislation created what would become two pillars of health manpower policy: assistance for medical schools, though the provision of construction grants, and aid for medical students by providing student loans. The federal government had, by this point, become involved

[20] I multiply all point estimates by the overall mean of the dose distribution so differences across columns do not arise due to this mechanical relationship.

in the funding of medical schools, but this represented a fundamental shift away from research grants, which comprised the lion's share of federal support by the start of the 1960s (Townsend 1983). Under the construction grant program, the Department of Health, Education, and Welfare (HEW) would provide funding for 2/3 of the costs for building an educational facility to a new school or expanding an existing one in exchange for several promises from the institution, including that the building would be used for teaching purposes for at least 10 years and a small increase in first-year enrollment (MacBride 1973b). In addition, the HPEA provided student loans, jointly with medical schools, to defray the increasing costs of medical education.

The HPEA was amended in 1965 to both extend the existing programs and add three more: the government would provide additional assistance to medical schools through basic and special improvement grants, as well as further aid to students through a new scholarship program. Basic improvement grants, which would later be more aptly called "capitation grants," provided institutions with a grant consisting of a baseline payment in addition to further funding for each enrolled student. In exchange, the institution would be required to implement a small increase in first-year enrollment. Any appropriated funds left over after these payments were made would be put towards Special Improvement Grants, which were provided to fund specific types of projects that schools would pitch in an application (Kline 1971). Finally, student assistance was broadened with the introduction of a scholarship program in addition to loan provision.

These programs were extended and modified by the Health Manpower Act of 1968, but remained reasonably constant through the end of the decade. In 1961, during hearings on what would become the HPEA, then HEW secretary Abraham Ribicoff stated that the U.S. would have to increase medical school admissions to 12,000 per year in order to stabilize the physician-to-population ratio (U.S. Congress 1962). Taking stock in 1970, a report to the President on the effectiveness of these policies noted that first-year places had risen from 9,213 in 1963 to a projected 11,500 in 1970 (HEW 1970), very close to Ribicoff's stated threshold. Despite this progress, however, concerns about a shortage of health professionals persisted. An October 1970 report from *The Carnegie Commission on Higher Education* reiterated the severity of the problem, citing an estimate from then HEW secretary Roger Egeberg that the U.S. needed approximately 50,000 more physicians at the beginning of the 1970s (Carnegie Commission on Higher Education 1970).

At the same time, the financial position of medical schools had become markedly worse, with many schools receiving financial distress grants through the Health Manpower Act.

Consequently, Congress looked for a "comprehensive" solution that would stabilize the financial situation of medical schools while incentivizing increases in enrollment (MacBride 1973a). This policy took the form of the Comprehensive Health Manpower Training Act (CHMTA) of 1971, where the focus of federal support shifted to capitation grants, which provided schools with a set amount of funding dependent on their enrollment count, type of enrollment,[21] and number of graduates. As before, to receive this funding, an institution was also required to increase its first-year enrollment by a given amount. In addition, all forms of funding in the CHMTA are tied to a requirement that a school "will not discriminate on the basis of sex in the admission of individuals to its training programs" (P.L. 92-157 1971).

The last important piece of Health Manpower legislation was passed in 1976, also named the Health Professions Educational Assistance Act. By this point in time, emphasis had shifted from producing more M.D.'s to directing newly minted doctors to primary care specialties and areas with a shortage of health professionals (Korper 1980). Accordingly, the conditions for receiving capitation grants were changed to align better with these new priorities and new types of special project grants were introduced. Nevertheless, previous sources of funding were largely maintained, and first year enrollment continued to rise through 1980. However, as the new decade began, support for health manpower policy began to fade quickly as newer projections showed a physician surplus in place of a shortage (Congressional Quarterly 1981). Eventually, a new piece of legislation was passed in 1981, but focus had shifted again almost entirely towards student support and away from institutional aid (Congressional Quarterly 1982).

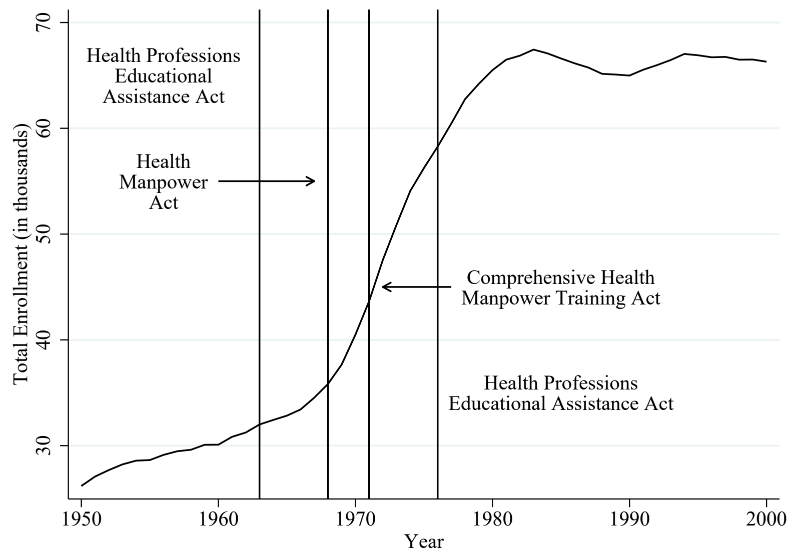
4.1.1 Impact on Medical School Enrollment

The totality of Health Manpower policy is summarized in Figure 12, which plots total enrollment across all medical schools between 1950 and 2000 as well as the timing for the four core pieces of legislation. While Health Manpower Policy is actively supporting medical schools from 1965 - 1980, there is a historic rise in enrollment, with the total number of students approximately doubling during this time period. This stands in stark contrast to the period from 1980 - 2000 where total enrollment remains constant after federal support for enrollment increases abates. It is difficult to tie observed enrollment increases directly to federal programs, but the time series strongly suggests that medical schools responded promptly to federal incentives to increase enrollment.

We can get a crude estimate of the success of Health Manpower policy by using the

[21] Bonuses were given for students enrolled in 3-Year programs.

Figure 12: Health Manpower Policy Timeline



I plot total enrollment at allopathic medical schools from 1950-2000. Data are collected from the *Journal of the American Medical Association*'s Education in various years between 1950 and 2001. The main pieces of Health Manpower Legislation are denoted with vertical lines.

public record of grant provision. Construction grants provided by the Bureau of Health Manpower (BHM) were tied to a specific number of first-year seats that a medical school would maintain and increase as a result of the new building: in total, these grants implied an increase of 4,880 seats (HEW BHM 1980, p. 23), accounting for 56% of the observed increase of 8,650 seats between 1965 and 1980.[22] In addition, almost every medical school increased enrollment to obtain capitation grant funding in response to the CHMTA: the average school would have to have increased first-year enrollment by at least 10 students, leading to the creation of 1,020 seats through this program alone.

To get a better sense of how enrollment expansions contributed to the growth in women's enrollment, I conduct an OLS accounting exercise to estimate the fraction of the change in women's enrollment over time that can be explained by year-to-year changes in total enrollment. This strategy leverages the differential timing and size of enrollment expansions across institutions to estimate the reduced form relationship between changes in women's enrollment and changes in total enrollment. With these estimates in hand, a back-of-the-envelope

[22] In general, medical schools do meet their promise of increased enrollment, but not always. In particular, near the end of the sample period, several schools do not meet their promised enrollment expansion. It is unclear if HEW relaxes their requirements or if this increase is met after the sample period ends.

calculation using the total change in enrollment during this time period will provide an estimate of the proportion of women's enrollment growth that can be explained by enrollment growth.

4.2 Accounting Exercise

I use a first differences design to estimate the share of enrollment expansions captured by women over time using the following specification:

$$\triangle F_{it} = \triangle \delta_t + \alpha \triangle E_{it} + \nu_{it} \tag{3}$$

The outcome, $\triangle F_{it}$, gives the change in the number of women enrolled in the first-year class at institution i in year t. The independent variable of interest is the change in total enrollment, given by $\triangle E_{it}$. I estimate the share of new seats seats that are filled by women, given by α, as both the outcome and explanatory variable are given in first differences. I include year fixed effects $\triangle \delta_t$ to capture national-level changes in women's enrollment that are not due to enrollment expansions, including the direct anti-discrimination policy effects that were documented in the previous section.[23] My baseline specification does not include any additional controls to measure the relationship between enrollment increases and changes in women's enrollment. However, changes in women's enrollment are likely also impacted by changes in the demand for medical education. As a result, I include an additional specification with controls for changes in both resident and non-resident tuition $\triangle \mathbf{X}_{it}$, as well as a further specification with state-by-year fixed effects to adjust for the staggered introduction of the pill as before.[24]

To understand how α changes over time, I interact $\triangle E_{it}$ with bins for the years 1965-1970, 1970-1975, and 1975-1980, estimating a separate coefficient for each time period. 1965-1970 represents the five years between when funding from Health Manpower policy was first disbursed and when federal anti-discrimination policy began to bite. 1970-1975 represents the five years when a flurry of anti-discrimination policy occurred (namely, the CHMTA and Title IX) and when I find strong direct effects of anti-discrimination policy with my difference-in-differences design. 1975-1980 are the last five years in my sample period, where we continue to see large growth in women's enrollment but no direct impact of anti-discrimination policy.[25]

[23] Note that any institution fixed effects are removed by the first differencing.

[24] I cannot control directly for changes in applications filed, as this data series ends in 1977, and I am interested in particular in women's gains in the second half of the 1970s.

[25] In Appendix Section C.3, I extend the model from the previous section to derive the expected impact

Formally, define $\mathcal{B}_t$ as the vector of year bins and α as the corresponding parameter vector, given by

$$\mathcal{B}_t = \langle \mathbb{1}(t \in [1965, 1970), \mathbb{1}(t \in [1970, 1975)), \mathbb{1}(t \in [1975, 1980) \rangle'$$
$$\boldsymbol{\alpha} = \langle \alpha_{[1965,1970)}, \alpha_{[1970,1975)}, \alpha_{[1976,1980)} \rangle'$$

So, in sum, my most stringent specification is given by:

$$\triangle F_{it} = \triangle \delta_{st} + \boldsymbol{\alpha}' \mathcal{B}_t \triangle E_{it} + \boldsymbol{\beta}' \triangle \mathbf{X}_{it} + \nu_{it} \tag{4}$$

Estimation results are contained in the top section of Table 3 in Columns 1-3. Two patterns are apparent in these estimates. First, as we would expect, the fraction of seats captured by women is increasing over time. Estimates from Column 1, which captures the linear relationship between enrollment growth and changes in women's enrollment, show that women capture 6.8% of new seats between 1965 and 1970, which jumps to 17.0% in 1970-1975, and then to 26.8% in 1975-1980. However, once we account for changes in women's demand for medical education, a different pattern emerges. Estimates from Column 3, which include the most stringent set of covariates, show little growth between 1965-1970 and 1970-1975, but a large change between 1970-1975 and 1975-1980.

I introduce a formal statistical comparison of these parameter estimates in the second section of Table 3. I omit the 1965-1970 term in my year bins and parameter vector:

$$\tilde{\mathcal{B}}_t = \langle \mathbb{1}(t \in [1970, 1975)), \mathbb{1}(t \in [1975, 1980) \rangle'$$
$$\tilde{\boldsymbol{\alpha}} = \langle \tilde{\alpha}_{[1970,1975)}, \tilde{\alpha}_{[1976,1980)} \rangle'$$

Then, with the following specification, $\tilde{\boldsymbol{\alpha}}$ will estimate the difference in the fraction of seats captured by women across year bins:

$$\triangle F_{it} = \triangle \delta_{st} + \tilde{\boldsymbol{\alpha}}' \tilde{\mathcal{B}}_t \triangle E_{it} + \alpha \triangle E_{it} + \boldsymbol{\beta}' \triangle \mathbf{X}_{it} + \nu_{it} \tag{5}$$

The estimates of $\tilde{\boldsymbol{\alpha}}$ from Equation 5 are reported in the second part of Table 3. These tests confirm the comparisons in the previous paragraph: In column 3, I cannot reject the hypothesis that the fraction of seats captured by women differs across the 1965-1970 and 1970-1975 bins, but I find strong evidence that women capture substantially more seats in

of an enrollment expansion following successful anti-discrimination policy.

Table 3: Estimates of the Impact of Enrollment Changes on Women's Enrollment

	One Year			Two Years			Three Years		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Within-Bin Results									
1965-1970	0.068**	0.068**	0.081***	0.111***	0.111***	0.191***	0.115***	0.115***	0.113**
	(0.028)	(0.028)	(0.028)	(0.037)	(0.037)	(0.047)	(0.033)	(0.033)	(0.048)
1970-1975	0.170***	0.170***	0.119***	0.218***	0.219***	0.185***	0.231***	0.232***	0.168***
	(0.027)	(0.027)	(0.027)	(0.035)	(0.035)	(0.052)	(0.045)	(0.046)	(0.061)
1975-1980	0.268***	0.267***	0.240***	0.313***	0.314***	0.266***	0.397***	0.395***	0.360***
	(0.047)	(0.047)	(0.044)	(0.041)	(0.042)	(0.044)	(0.073)	(0.074)	(0.065)
Across-Bin Results									
1970-1975	0.102**	0.102**	0.038	0.107**	0.108**	-0.006	0.117**	0.117**	0.055
	(0.039)	(0.039)	(0.041)	(0.049)	(0.049)	(0.069)	(0.054)	(0.054)	(0.067)
1975-1980	0.199***	0.199***	0.160***	0.202***	0.203***	0.075	0.282***	0.280***	0.247***
	(0.058)	(0.058)	(0.054)	(0.053)	(0.053)	(0.066)	(0.073)	(0.073)	(0.073)
Observations	1586	1582	1268	1586	1582	1268	1586	1582	1268
Tuition Controls		X	X		X	X		X	X
State-by-Year FE			X			X			X

This table plots estimates from equations (6) and (7), where the outcome is the change in women's first-year enrollment between years t and $t+k$. Columns 1-3 give results for a one-year difference ($k=1$), columns 4-6 give results for a two-year difference ($k=2$),and columns 7-9 give results for a three year difference ($k=3$). The first section gives ordinary least squares results for equation (6), reporting estimates by year group for 1965-1970 (row 1), 1970-1975 (row 2), and 1975-1980 (row 3). The second section gives ordinary least squares results for equation (7), reporting estimates by year group for 1970-1975 (row 1), and 1975-1980 (row 2) relative to 1965-1970. Model 1 includes year fixed effects; Model 2 adds controls for resident and non-resident tuition; and Model 3 adds state-by-year fixed effects. All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

1975-1980 relative to the 1965-1970.

Accordingly, growth in women's enrollment between 1970 and 1975 seems best explained by changes in women's demand for medical education (likely spurred by the women's movement) and the effectiveness of anti-discrimination policy. Increases in total enrollment became an important part of the picture in the second half of the decade, likely magnified by the success of anti-discrimination policy in "opening the door" for women to access medical schools.

Specification 4 implicitly imposes a constraint that increases in women's enrollment must occur in the period in which enrollment changes. We might expect, however, that it takes several years for enrollment expansions to translate into gains for women. To explore this, in Appendix Section B.3, I utilize a large discrete expansion in enrollment capacity at the University of Cincinnati as a case study. Using a pool of medical schools that did not receive a construction grant after 1969, I construct an untreated synthetic control to compare to the realized path of women's enrollment at the University of Cincinnati. I find that this program enrolls around 20 more women than it would have had it not constructed new teaching facilities and that it took around three years for these gains to be realized. Following this, I augment my previous specification to estimate gains to women's enrollment that accrue two and three years later:

$$\triangle_k F_{it} = \triangle_k \delta_{st} + \boldsymbol{\alpha}' \mathcal{B}_t \triangle_1 E_{it} + \boldsymbol{\beta}' \boldsymbol{\triangle_k} \mathbf{X}_{it} + \nu_{it} \tag{6}$$

Here, I define $\triangle_k V_{i,t} = V_{i,t+k} - V_{i,t}$ for any variable $V_{i,t}$, and the parameter k indicates the number of years over which I estimate the change in women's enrollment. Note that tuition controls are also adjusted to reflect the increase in tuition between years t and $t+k$. Across-bin differences are estimated directly using the dynamic analog of Equation 5, given by

$$\triangle_k F_{it} = \triangle_k \delta_{st} + \tilde{\boldsymbol{\alpha}}' \tilde{\mathcal{B}}_t \triangle_1 E_{it} + \alpha \triangle_1 E_{it} + \boldsymbol{\beta}' \boldsymbol{\triangle_k} \mathbf{X}_{it} + \nu_{it} \tag{7}$$

Estimation results are contained in the top section of Table 3 in Columns 1-3 ($k = 1$), 4-6 ($k = 2$), and 7-9 ($k = 3$). For each year grouping, there is evidence that women gain seats from enrollment expansions in subsequent years. Further, like before, these dynamic gains are most pronounced in the 1975-1980 time period. Comparing columns 3 and 9, which utilize the full set of controls, I find that the proportion of seats women capture grows by 3.3% between $k = 1$ and $k = 3$ for enrollment expansions occurring in 1965-1970; this figure is similar at 5.1% for 1970-1975, but far larger at 11.8% for 1975-1980. To see the importance of enrollment expansions, compare the magnitude of these effects with the proportion of all medical students who are women: this figure rose from 20.5% in 1975 to 26.5% in 1980, far below the 35.9% of seats women fill following enrollment expansions during this same time period.

4.3 The Role of New Medical Schools

The results from the previous section document that the creation of new capacity at medical schools was particularly important for women's entry, which had been highlighted in the policy literature (see Boulis and Jacobs (2008, p. 26)). This evidence, however, does not distinguish between existing and newly created medical schools, which could have been particularly important for women's enrollment, as More (1999) argues. We might expect that, absent the legacy of an established admissions policy, newly established schools could have been more willing to admit historically underrepesented groups, and the drastic increase in the number of programs in the 1960s and 1970s could magnify these differences enough to matter in the aggregate.

Figures 13a and 13b lay out some of the descriptive facts supporting this position. Between 1963 and 1980, 37 new medical schools began enrolling students, increasing the total number of schools from 85 to 122, displayed in Figure 13a. As these new programs began to increase enrollment, they became an increasingly large part of the prodution of M.D.'s —by 1980, almost 20% of all first-year medical students were enrolled at one of these newer programs. Additionally, these programs generally enrolled more women. Figure 13b plots the fraction of all students who were women at new and existing schools. While both types of programs follow a similar trend, it is clear that new schools consistently enroll a larger proportion of women.[26]

The results in Table 3 provide support for the proposition that newly created seats were important for women's enrollment growth. Between 1970 and 1975, 3,741 new first-year seats were created, and between 1975 and 1980, 2,294 new first-year seats were created; my one-year estimates imply that women captured around 1,250 of these, representing 33% of their gain of 3,742 seats during this period. To test whether or not newly created programs played a more prominent role in this expansion, I estimate Equation 6 separately for both types of schools. To avoid the large loss in sample size (especially for new programs) resulting

[26]There is a notable amount of noise for new schools in the mid-1960s. Not only did few institutions contribute to this average, but budding programs generally enroll very small classes in their first year before scaling to their desired enrollment target.

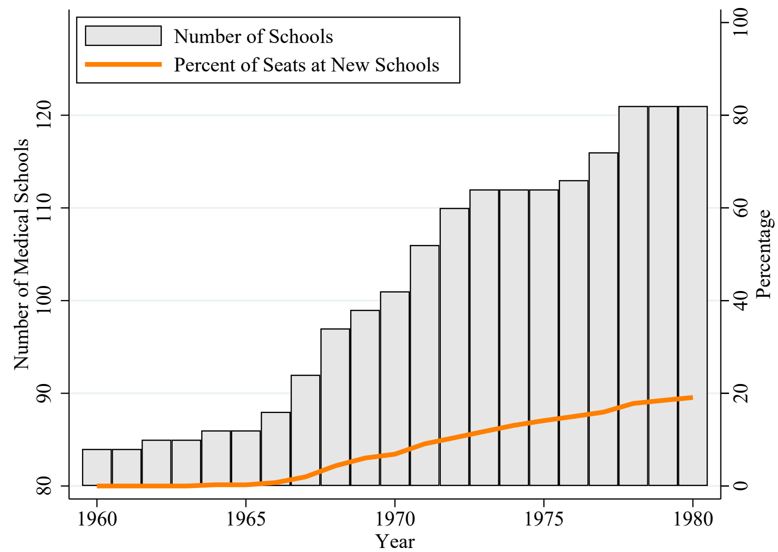
from state-by-year fixed effects, I utilize model 1, which captures the relationship between enrollment expansions and changes in women's enrollment. The results from this exercise are presented in Table 4. Between 1965 and 1970, my point estimates provide some evidence for this hypothesis. After 3 years, women capture 17.6% of enrollment expansions at new schools, compared with 10% at existing ones. However, after 1970, I find that this relationship has disappeared—by and large, the point estimates for new and existing schools are similar, if not slightly larger for existing programs. It is important to note, as before, that these point estimates are generally much higher than the proportion of students at existing programs that are women. This affirms that the creation of new seats was a key driver of growth in women's enrollment at both types of programs; perhaps the driver of the difference in Figure 13b was that all seats at new schools were created in a recent enrollment expansion.

5 Conclusion

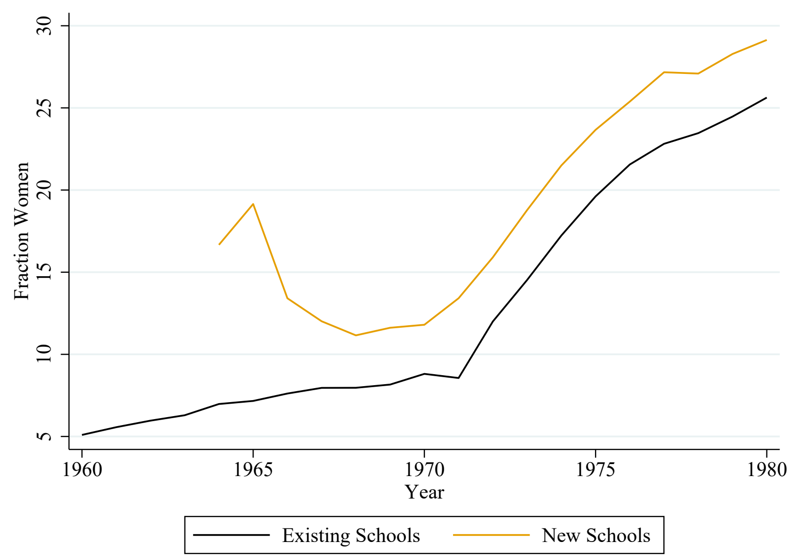
In her 2006 Ely lecture, Claudia Goldin opens by stating that "women's increased involvement in the economy was the most significant change in labor markets during the past century" (Goldin 2006). Women's entry into professional schools was a core part of the last phase of this transition, termed the "Quiet Revolution." This began with increasing expectations among young women of the years they would spend in the labor market, interacting with the introduction of the pill; better control over the timing of fertility allowed women to make costly educational investments after college, like medical school (Goldin 2024). This led to a drastic, episodic increase in women's representation across medical schools, dental schools, law schools and business schools, beginning in the early 1970s and continuing through the new millennium.

This paper contributes to our understanding of this era of history by quantifying the role of federal policy in women's entry into medicine, a small part of a much broader story. I find that federal policy began to matter in 1971, when anti-discrimination policy was first directed effectively at medical schools by the Women's Equity Action League. Women's enrollment was lifted by around 4 seats at the mean between 1971 and 1973, explaining approximately 25% of women's gains during this time period. Aspiring women were helped further by large increases in enrollment spurred by Health Manpower policy in the second half of the 1970s and filled many of these new seats. Ultimately, this was just the first chapter in a long process of change: in 2017, women comprised the majority of first-year allopathic medical students for the first time, becoming the majority of all enrollees shortly

Figure 13: New and Existing Medical Schools



(a) Number of Medical Schools



(b) Women's Representation

Figure 13a: The bars give the number of medical schools that I observe in every year, where a school is counted if it reports non-missing total enrollment for its first-year class. I also include a line indicating the percentage of first-year seats that are at schools I classify as new. A medical school is considered new if it first reports positive first-year enrollment after 1963, when Health Manpower policy begins. The sample is identical to my main analysis sample described in Section 3.2. Figure 13b: This figure plots the percentage of all students at new and existing schools that are women. A medical school is considered new if it first reports positive first-year enrollment after 1963, when Health Manpower policy begins. The sample is identical to my main analysis sample described in Section 3.2

Table 4: Heterogeneity in the Impact of Enrollment Changes on Women's Enrollment Across New and Existing Medical Schools

	Existing Schools			New Schools		
	(1) One Year	(2) Two Years	(3) Three Years	(4) One Year	(5) Two Years	(6) Three Years
Within-Bin Results						
1965-1970	0.038	0.109**	0.101**	0.160***	0.112***	0.176***
	(0.027)	(0.048)	(0.041)	(0.037)	(0.030)	(0.043)
1970-1975	0.159***	0.223***	0.265***	0.232***	0.236***	0.174**
	(0.038)	(0.042)	(0.056)	(0.035)	(0.065)	(0.072)
1975-1980	0.346***	0.336***	0.383***	0.189***	0.291***	0.407***
	(0.070)	(0.080)	(0.123)	(0.026)	(0.038)	(0.088)
Across-Bin Results						
1970-1975	0.121***	0.113*	0.164**	0.072	0.124	-0.001
	(0.044)	(0.061)	(0.066)	(0.059)	(0.075)	(0.085)
1975-1980	0.307***	0.226**	0.283**	0.030	0.179***	0.232**
	(0.077)	(0.087)	(0.118)	(0.044)	(0.055)	(0.091)
Observations	1275	1275	1275	310	310	310

This table plots estimates from equation (6) and (7), where the outcome is the change in women's first-year enrollment between years t and $t+k$. In columns 1-3, the sample is restricted to existing medical schools, and in columns 4-6, the sample is restricted to new medical schools. A medical school is considered new if it first reports positive first-year enrollment after 1963, when Health Manpower policy begins. Columns 1 & 4 give results for a one-year difference ($k=1$), columns 2 & 5 give results for a two-year difference ($k=2$),and columns 3 & 6 give results for a three year difference ($k=3$). The first section gives ordinary least squares results for equation (6), , reporting estimates by year group for 1965-1970 (row 1), 1970-1975 (row 2), and 1975-1980 (row 3). The second section gives ordinary least squares results for equation (7), reporting estimates by year group for 1970-1975 (row 1), and 1975-1980 (row 2) relative to 1965-1970. All models include year fixed effects, equivalent to the first specification in Table 3. Standard errors are clustered at the institutional level.
*** $p < .01$, ** $p < .05$, * $p < .10$

afterwards in 2019 (AAMC 2019).

These changes have had a massive impact on U.S. economic progress. Hsieh et al. (2019) find that changes in the occupational distribution explain anywhere from 20% to 40% of the growth in U.S. output per person between 1960 and 2010. One of the key frictions in their model that was relaxed during this time period was barriers to human capital formation; I provide microeconomic evidence that federal policy played an important role in breaking these barriers. Since medicine and many other professional occupations are licensed, there is direct link between access to schooling and work, suggesting that educational frictions play an outsized role in women's access to these jobs. Accordingly, future work should be directed at understanding changes in non-health professional occupations, such as the legal profession, which were unaffected by health manpower policy and thus beyond the scope of this paper.

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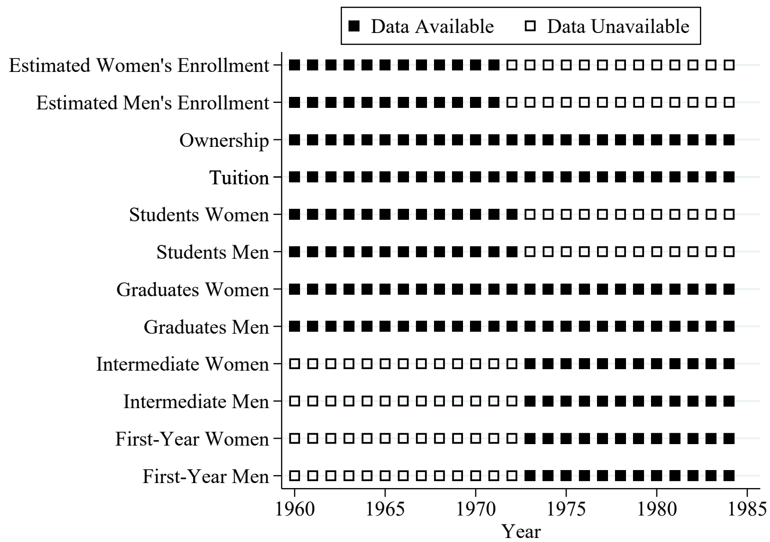
Online Appendix

A Data Appendix

A.1 Total Enrollment Data

Figure A.1: *Journal of the American Medical Association* Education Number



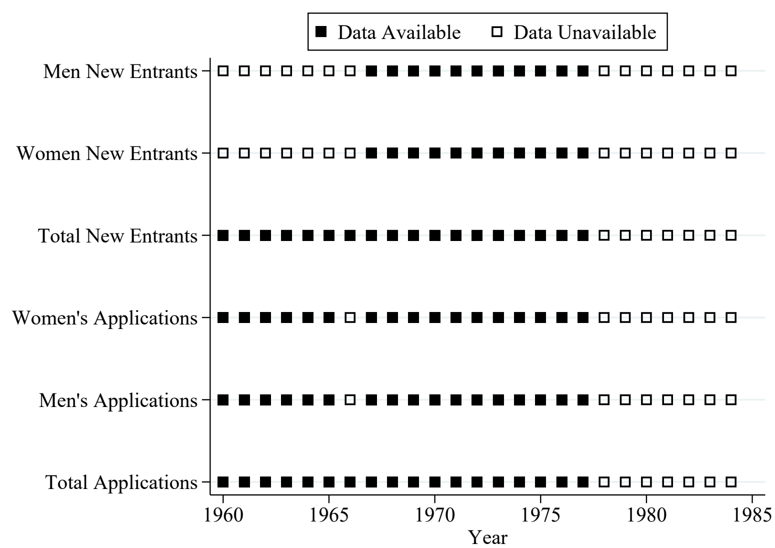
This figure gives a visual description of variable availability in the *Journal of the American Medical Association* Education Number. The label on the y-axis indicates an available variable, and the x-axis indicates a particular year. If a variable is available in a given year, the square at that point is filled in; if not, it is empty.

To construct time series evidence on changes in women's enrollment over time, I collect institution-level information on total enrollment, split by sex. In every year, the *Journal of the American Medical Association* publishes its Education Number, which includes reports and statistics on medical education. Between 1960 and 1972, the Education Number includes information on the number of current students and graduates from each medical school, reported separately by sex. Starting in 1973, students are split into three categories: first-year students, intermediate students, and graduates. Intermediate students include students in years 2-3 at 4-year programs, students in year 2 at 3-year programs, as well as students in year 2 at 2-year basic science schools. To construct a comparable time series throughout my sample period from 1960-1980, I utilize data on the number of students in each year from 1960-1972. From comparing total enrollment figures to sums of the variables provided here, it appears each year's graduates are included in the count of total students. From 1973-1980, I construct information on total enrollment by sex by adding first-year, intermediate, and graduate enrollment. Availability of all data in the *JAMA* Education Number is plotted in Figure A.1.

There is one small issue with the data that I will note here. Enrollment of full-time students is reported from 1960-1962, while data on all students is reported from 1963 - 1980. Since most medical students are full-time, I am able to measure almost all enrollment in every year; further, since the data are consistent starting in 1963, I am able to capture important trend breaks around 1970 without worrying about this change in reporting.

A.2 First-Year Enrollment Data

Figure A.2: *Journal of Medical Education*



This figure gives a visual description of variable availability in the *Journal of Medical Education* Study of Applicants. The label on the y-axis indicates an available variable, and the x-axis indicates a particular year. If a variable is available in a given year, the square at that point is filled in; if not, it is empty.

My primary source for this dataset is the *Journal of Medical Education*'s "Study of Applicants," published in every year from 1960 through 1977. Variable availability for this source is plotted in Figure A.2. In every year that this is published, I collect information on total new entrants and total applications filed for each institution in each year. Information on applicants split by sex is available in every year except 1966. Information on new entrants split by sex is only available starting in 1967, so I am only able to collect number of men and women that are new entrants in each year between 1967 and 1977.

To supplement this, I collect information on first-year enrollments in 1966 as well as 1978-1980. First-year enrollments differ slightly from new entrants, as this count includes students repeating the first year, but it is generally very close to the number of new entrants. From 1978-1980, I collect this data from the *JAMA* Education Number in each year that it is reported. Information on the 1966-67 entering class is published in the 1968-69 *MSAR*, but unfortunately earlier copies of the *MSAR* do not publish this data series. To extend my panel back to 1960, I utilize estimated enrollment data. This is published in the *MSAR* and then reprinted in the *JAMA* Education Number between 1960 and 1971, where I collect it between 1960 and 1965. Medical schools are surveyed in the spring before a class enters in the next fall for an estimate of the gender composition of their incoming students. Generally, this is a highly accurate estimate, as many applicants have committed to enroll in the following year by spring, which I confirm in the next section. Interestingly, starting with the 1973-74 *MSAR*, medical schools begin estimating the in-state/out-of-state composition of their incoming class instead of the sex composition.

A.3 Adjusting Graduate Data

To construct meaningful panel data on graduates, I make several adjustments to the observed data. First, in many years while schools are in operation but before any students graduate, they report having 0 graduates. I code these as missing instead to mirror the fact that schools do not report any new entrants until the year that they are in operation. In addition, in a handful of years, an abnormally low number of graduates are also reported; this is treated as erroneous and recoded as missing. Most importantly, as noted in the text, I lag graduates by three years to reflect the estimated year in which they enrolled. This not only eases interpretation of the event study (we would expect to see effects in the same year as first-year enrollment), but also allows the use of the same covariates. Since my data are collected and analysed at the level of the academic year, this is the correct lag for a 4-year program.

There are several details to note here. First, in my data, there are a handful of basic science schools, which enroll students for the first two years of medical school, who then transfer to a 4-year program to complete their degree. These schools do not report graduates, meaning that students are not double counted. Second, I find that it is often the case that new programs enroll an initial class of both first and third year students, leading to graduates in the second year of operation. Even so, the lag I use still reflects the academic year in which these students first enrolled (at a different institution). Lastly, the only potential concern is the introduction of three-year accelerated programs. I do not adjust for this as it is difficult to measure when schools begin and phase out their three-year program. It is also unclear that this would impact my timing at all, since graduates in an academic year are counted starting in July of the preceeding summer, which might still capture graduates of accelerated programs.

A.4 Constructing Tuition Data

To construct meaningful panel data on tuition, I make several adjustments to the observed data. First, many schools only report resident tuition (and not non-resident tuition) if they only enroll in-state students. For these school-years, I code non-resident tuition as equal to resident tuition to avoid dropping these observations in my regression analysis. For private schools that charge one tuition rate to all students, I make the same correction if resident tuition is ever missing. Second, many public schools charge only fees to in-state students; in most years, these costs are recorded, but in others, they are recorded as "fees only." I code these observations as missing and utilize a simple interpolation procedure to estimate them, as described below. Finally, for most programs, tuition for the academic year is reported, but at times tuition for different units of time is reported instead. To convert to the academic year, I make the following adjustments: tuition by 6 week module is multiplied by 5 to convert to a 30 week academic year; tuition for the entire curriculum is divided by 4 to convert to one academic year in a 4-year program; tuition by quarter is multiplied by 3 to convert to a typical academic year on a quarter system.

To match tuition data with the academic year in which enrollment is collected, I lead my collected tuition by two academic years. This is due to the timing in which my data are reported. Consider information on academic year $t, t+1$. This is generally published in the education number in the fall of year $t+1$, after this academic year has been completed. The tuition data published here is usually an estimate of what tuition will be in the following academic year, $t+2, t+3$. Accordingly, tuition data collected in the education number covering academic year $t, t+1$ is attributed to academic year $t+2, t+3$. It is important to note that, as a result, this is estimated tuition, but likely the relevant metric, as this is what students would expect to pay while applying in the fall for the upcoming academic year.

Unfortunately, at times, estimation tuition information is unavailable. In some cases, tuition from a previous year is reported in its stead; I always use reported information in these cases. However, in a handful of school-years, no tuition information not available. Instead of dropping these observation, I linearly interpolate tuition values in the years they are missing to estimate cost increases across these years.

A.5 Comparing Different Measures of Enrollment

To construct a full panel of first-year students, I have to rely on several slightly different measures of enrollment. My preferred variable is the number of new entrants, which is available from 1967-1977. Outside of this time period, I sometimes need to use estimated new entrants, as well as first-year students, which includes new entrants as well as students repeating the first year. Fortunately, there are several years where these variables overlap. From 1967-1971, I observe both estimated and realized new entrants, which allows me to evaluate the ability to which medical schools are able to accurately estimate the sex distribution of their incoming class. Additionally, from 1973-1977, I observe both new entrants and first-year students, allowing me to evaluate the degree to which the latter is a reasonable measure of the former.

In sum, I have the following set of variables:

- F_{it}: New entrants for institution i in year t that are women
- M_{it}: New entrants for institution i in year t that are men
- F_{it}^{EST}: Estimated new entrants for institution i in year t that are women
- M_{it}^{EST}: Estimated new entrants for institution i in year t that are men
- F_{it}^{FY}: First-year students for institution i in year t that are women
- M_{it}^{FY}: First-year students for institution i in year t that are men

To evaluate to predictive value of F_{it}^{EST} and M_{it}^{EST}, I restrict my data to the years in which both are observed (1967-1971), and I run the following bivariate regressions:

$$F_{it} = \beta F_{it}^{EST} + \varepsilon_{it} \tag{A.1}$$

$$M_{it} = \beta M_{it}^{EST} + \varepsilon_{it} \tag{A.2}$$

To evaluate to predictive value of F_{it}^{FY} and M_{it}^{FY}, I restrict my data to the years in which both are observed (1973-1977), and I run the following bivariate regressions:

$$F_{it} = \beta F_{it}^{FY} + \varepsilon_{it} \tag{A.3}$$

$$M_{it} = \beta M_{it}^{FY} + \varepsilon_{it} \tag{A.4}$$

Notice that I do not include a constant, so $\beta = 1$ indicates a correct predictor. Standard errors are clustered at the institution level to correct for institution-specific errors in reporting.

Table A.1 reports the results from (A.1), (A.2), (A.3), and (A.4). The estimated coefficients

Table A.1: Accuracy of Estimated and First-Year Enrollment

	(1)	(2)	(3)	(4)
New Entrants (Men)	1.011***			
	(0.006)			
New Entrants (Women)		1.027***		
		(0.015)		
First-Year Students (Men)			0.968***	
			(0.005)	
First-Year Students (Women)				0.960***
				(0.006)
Observations	485	485	576	576
R^2	0.991	0.944	0.997	0.994

Column 1 gives estimates of β from equation (A.1), where the independent variable is women's estimated first-year enrollment; column 2 gives estimates of β from equation (A.2), where the independent variable is men's estimated first-year enrollment); Column 3 gives estimates of β from equation (A.3), where the independent variable is women's first-year enrollment; and Column 4 gives estimates of β from equation (A.4), where the independent variable is men's first year enrollment. In Columns 1 & 3, the outcome is women's new entrants, and in Columns 2 & 4, the outcome is men's new entrants. All specifications are estimated only over the years in which the dependent and independent variable are both available—see Figures A.1 and A.2 for data availability. Standard errors are clustered at the institution level to correct for institution-specific errors in reporting.
*** $p < .01$, ** $p < .05$, * $p < .10$

show that medical schools slightly overestimate new entrants ($\beta > 1$) and that there tend to be slightly fewer new entrants than first-year students in each year ($\beta < 1$) due to repeated students. The primary statistic of interest is R^2: I am able to explain almost all of the variation ($R^2 \sim 1$) for all but one proxy (estimated new entrants that are women), for which I still am able to replicate around 95% of the variation, suggesting that these are excellent proxies for my preferred measure of enrollment.

B Additional Results

B.1 Percentage Women as Outcome

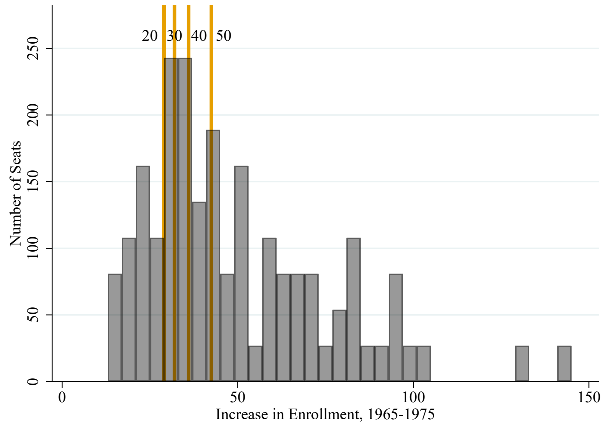
Instead of estimating the change in women's and men's enrollment while controlling for enrollment, we could alternatively use the percentage of women students as the outcome

Table B.1: Percentage of New Entrants who are Women: Summary Estimates

	Percentage Women		
	(1)	(2)	(3)
First-Year Entrants			
Difference-in-Doses Estimate	0.013**	0.013**	0.010
	(0.005)	(0.005)	(0.007)
Observations	1683	1683	1299
Graduates			
Difference-in-Doses Estimate	0.013**	0.013**	0.011
	(0.005)	(0.005)	(0.007)
Observations	1634	1634	1287
Applications			
Difference-in-Doses Estimate	-2.686	-2.493	2.074
	(2.707)	(2.510)	(1.857)
Observations	1684	1669	1280
Total Enrollment	X	X	X
Men's Applications		X	X
State-by-Year Fixed Effects			X

This table reports transformed estimates from equation (B.2). The header of each section denotes the outcome variable: the percentage of first-year entrants who are women for section 1, the percentage of graduates who are women for section 2, and the percentage of applications that are filed by women for section 3. Model 1 (Column 1) includes a control for the log of total enrollment as well as institution and year fixed effects. Model 2 (Column 2) adds a control for the log of men's applications. Model 3 (Column 3) adds state-by-year fixed effects. All coefficients are scaled by the mean of the dose distribution so that they give an estimate of the change in the percentage women of seats/graduates/applications between 1971 and 1977. All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

Figure B.1: Distribution of Enrollment Growth Between 1965 and 1975



For all medical schools that report positive total new entrants in both 1965 and 1975, I take the difference between enrollment in both years and plot the distribution in the histogram here. The vertical bars give the 20th, 30th, 40th, and 50th percentiles of this distribution and are labelled accordingly.

variable. To study this, I utilize the event study specification:

$$Y_{it} = \sum_{\tau=1960,\tau\neq1970}^{\tau=1977} \alpha_\tau d_{i,1969} \mathbb{1}(t=\tau) + \boldsymbol{\beta}'\mathbf{X}_{it} + \gamma_i + \delta_{st} + \varepsilon_{it} \tag{B.1}$$

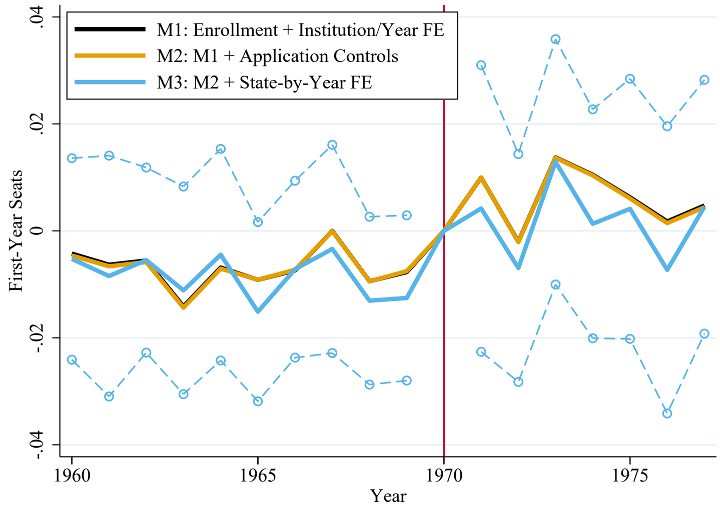
The outcome, Y_{it}, gives the percentage of new entrants who are women at institution i in year t. $d_{i,1969}$ remains my preferred measure of exposure to the policy, which is interacted with a set of year dummies, omitting 1970. I adjust the covariates to reflect the transformation of the outcome variable. In my baseline specification, I include institution fixed effects γ_i, year fixed effects δ_t, as well as the log of total enrollment, following Moehling et al. (2019).[27] To control for shocks to men's demand from the Vietnam draft, I include an additional specification with the log of men's applications. My final specification includes state-by-year fixed effects as before.

Figure B.2 plots the results. There is evidence of an increase in the percentage of women around 1970, but the results are much noisier than before, as no event coefficient is different from 0. To provide a summary estimate, I pool all pre-1970 and post-1970 event coefficients to produce a difference-in-differences estimate with the following specification:

$$Y_{it} = \alpha_{DID} d_{i,1969} \mathbb{1}(t > 1970) + \boldsymbol{\beta}'\mathbf{X}_{it} + \gamma_i + \delta_{st} + \varepsilon_{it} \tag{B.2}$$

[27] This is motivated by the fact that the derivative of log enrollment is the percentage change in enrollment.

Figure B.2: Difference-in-Differences: Percentage of New Entrants who are Women



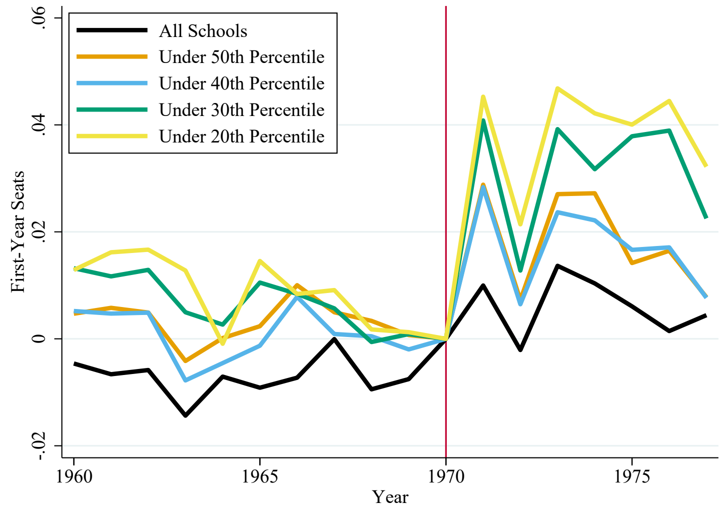
I plot the event study coefficients from equation (B.1) scaled by the mean of the dose distribution, where the outcome is the percentage of new entrants who are women. Model 1 includes a control for the log of total enrollment as well as institution and year fixed effects. Model 2 adds a control for the log of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Estimates end in 1977 as application data are not available after this year.

Here, α_{DID} is my coefficient of interest, which represents a standard continuous difference-in-differences estimator and replaces the event terms; all other variables remain the same. Results are presented in Table B.1. The difference-in-differences estimate is statistically significant for models 1 and 2, suggesting a 1.3% increase in the percentage of women enrolled, or a 12% increase over the baseline 10% average enrollment rate for women in 1970. I find similar difference-in-difference results for graduates; neither are significant when state-by-year fixed effects are included, though the point estimate remains around the same magnitude.

I prefer using women's and men's enrollment as an outcome for two reasons. First, it allows me to test the implication of the model that a drop in discrimination should lead to an increase in women's enrollment *and* a decrease in men's enrollment, as I derive in the model presented in Appendix Section C. However, in addition, it is much less sensitive to shocks to total enrollment. As I show in Section 4, throughout my sample period, there are large increases in enrollment at many medical schools, which largely accrue to men. These shocks affect the denominator of my outcome variable in this section, which I argue here introduces noise into my event study estimates.

To illustrate this, I present results with the same outcome where my sample is restricted

Figure B.3: Difference-in-Differences: Results for Schools with Low Enrollment Growth



I plot the event study coefficients from equation (B.1) scaled by the mean of the dose distribution, where the outcome is the percentage of new entrants who are women. I plot results estimated over my entire sample, as well as over all schools under the 50th, 40th, 30th, and 20th percentile of the distribution of the difference in total new entrants between 1965 and 1975. All specifications include a control for the log of total enrollment, the log of men's applications, as well as institution and year fixed effects. Estimates end in 1977 as application data are not available after this year.

to schools that experience small increases in enrollment to understand how enrollment shocks are affecting my results. To do this, I limit my sample to medical schools that have positive enrollment in 1965 and 1975 and calculate the increase in total enrollment between these two dates. Intuitively, I would like to only keep programs with a "small" enrollment increase between these two years, but it is unclear *ex ante* what a "small" increase is. In light of this, I present results for four groups: schools below the 50th, 40th, 30th, and 20th percentile of the distribution of enrollment increase between 1965 and 1975. To limit the drop in observations, I include controls for the log of total enrollment and the log of men's applications but not state-by-year fixed effects. The distribution of enrollment increases and these percentiles are plotted in Figure B.1.

The event study coefficients from this exercise are plotted in Figure B.3, and the summary difference-in-differences estimates are included in Table B.2. As the plots make clear, no matter the percentile cutoff used, there is a large, sharp increase in the percentage of women enrolled in 1971. While small in absolute terms, a 4% increase in the percentage of women enrolled represents a 40% increase in representation over the mean of 10% in 1970. Further, while there is a notable down-tick in 1972, these effects are largely persistent throughout the mid-1970s—all summary estimates are significant, with effects increasing in precision as the enrollment change cutoff is lowered. The evidence here strongly suggests that federal anti-discrimination policy had a marked positive impact on the percentage of women enrolled, and the baseline results presented in Figure B.3 plausibly contain substantial noise resulting from enrollment shocks.

B.2 Alternate Causal Parameters

To discuss the estimation of causal parameters precisely, I borrow the set-up and terminology from Callaway et al. (2024). Their framework considers a setting where there are two time periods, $t = 1$ and $t = 2$, where all units are untreated in period 1 and treated in period 2. The intensity of treatment is given by a dose variable D, and potential outcomes for unit i in period t are given by $Y_{it}(D_i)$.

Using a continuous difference-in-differences design, I am able to recover an average causal response (ACR) parameter, given by

$$\beta^{twfe} = \int_{d_L}^{d_U} w_1^{acr}(l) ACRT(l|l) \mathrm{d}l$$

Where $ACRT(l|l) = \frac{\partial ATT(l|d)}{\partial l}\Big|_{l=d}$ is the derivative of the average treatment effect with respect to the dose for units that received treatment d. However, as Callaway et al. (2024) point out, this parameter is only recovered if a strong parallel trends condition holds; that is, $\forall d \in \mathcal{D}$,

$$\mathbb{E}[Y_{t=2}(d) - Y_{t=1}(0)] = \mathbb{E}[Y_{t=2}(d) - Y_{t=1}(0)|D = d]$$

In effect, this limits the amount of treatment heterogeneity that can be present, as the path of outcomes for units receiving each dose d must be identical to the average path of outcomes for all units, if they had received the same dose in period 2.

It does not seem immediately implausible that strong parallel trends could hold in this context. From the relatively flat pre-trends test in Figure 9a, we know that the relationship between federal funding in 1969 and women's enrollment varies little in the 1960s, but this does not rule out that these variables are, at baseline, strongly related. However, the opposite is true—Table B.3 shows that, conditional on total enrollment, there is no relationship between women's enrollment and my measure of federal support in 1969. Further, there is no relationship between the fraction of women enrolled and federal support in 1969, both conditional on enrollment and unconditionally. To the extent that using financial support as a policy lever in Civil Rights era was novel, we might expect that attitudes towards women's admission (and, subsequently, the path of potential outcomes of women's enrollment) is plausibly homogenous with respect to pre-existing federal support.

However, we want to ensure that the results are robust to this strong assumption. A simple alternative proposed by Callaway et al. (2024) is to run a standard difference-in-differences estimator, where the treatment group is comprised of all units receiving a positive dose, while the control group is all units receiving a 0 dose. Under a standard parallel trends assumption, this will recover a weighted average of $ATT(d|d)$ parameters, given by

$$ATT^o = \mathbb{E}[ATT(D|D)|D > 0]$$

Unfortunately, in this context, I cannot recover this parameter, since there are no medical schools that receive 0 federal funding.[28] However, as Callaway et al. (2024) note, there is a natural alternative estimand in this context - the difference in $ATT(d|d)$ parameters, given by $ATT(d|d) - ATT(d_L|d_L)$ for any dose d. While this difference might also include selection bias, it is a reasonable robustness check that will demonstrate that the result still holds even if a substantial amount of dose variation is thrown out. Since I have no lowest dose d_L, I binarize at the 20th percentile and compare units above and below this point to estimate $\int_{D>F_D^{-1}(.2)} ATT(D|D) - \int_{D<F_D^{-1}(.2)} ATT(D|D)$. For comparability with my main results, I estimate the event study specification:

$$Y_{it} = \sum_{\tau=1960,\tau\neq 1970}^{\tau=1977} \alpha_\tau \mathbb{1}(d_{i,1969} > D_{1969}^{p20})\mathbb{1}(t=\tau) + \boldsymbol{\beta}'\mathbf{X}_{it} + \gamma_i + \delta_{st} + \varepsilon_{it} \tag{B.3}$$

The only change here is that treatment is now binary, given by $\mathbb{1}(d_{i,1969} > D_{1969}^{p20})$, where D_{1969}^{p20} denotes the 20th percentile of the dose distribution. Figure B.4 plots the results, as well as a series of alternate percentile thresholds to establish that my preferred specification was not "cherry-picked." Qualitatively, these results are strikingly similar to my main results in Figure 9a. To provide a summary estimate, I pool all pre-1970 and post-1970 event coefficients to produce a difference-in-differences estimate with the following specification:

$$Y_{it} = \alpha_{DID}\mathbb{1}(d_{i,1969} > D_{1969}^{p20})\mathbb{1}(t > 1970) + \boldsymbol{\beta}'\mathbf{X}_{it} + \gamma_i + \delta_{st} + \varepsilon_{it} \tag{B.4}$$

[28] And, even if there were, it is unlikely that they would form a plausible control group.

Here, α_{DID} is my coefficient of interest, which represents a standard continuous difference-in-differences estimator and replaces the event terms; all other variables remain the same. Table B.4 reports these estimates, which are a couple of seats larger than my preferred continuous specification. Note that this difference can arise solely because I am estimating a different causal parameter.

B.3 Case Study: University of Cincinnati

In addition to capitation grants, the main way the government funded enrollment expansions was through providing grants for the construction of new teaching facilities (and the renovation of existing capital). These grants were attached to a specific number of first-year places that a medical school would add as a condition of receiving this funding. I collect data on all grants given to medical schools between 1965, when the HPEA began distributing funds, and 1979.

To understand the potential dynamics of women's entry, I consider a case study of a grant given to the University of Cincinnati. This medical school received a grant in Fiscal Year 1970 for $32m to construct a basic science building. In exchange, the university would maintain 106 existing seats and add 86 new seats. The university's website reports that this building was completed in 1974,[29] and the time series for enrollment verifies this. Figure B.5 plots first-year enrollment for the University of Cincinnati during my sample period, and there is a clear discrete jump in enrollment when the new Medical Sciences Building opens in 1974 of around 60 students.

It is less clear that women benefit from this enrollment expansion; women's enrollment at the University of Cincinnati is plotted in Figure B.6. Women's enrollment is increasing over this entire time period, but it is unclear to what extent this increase is due to a specific increase in teaching capital or part of a previous rise in women's enrollment. To disentangle the impact of this expansion on women's enrollment, I construct a synthetic University of Cincinnati in the years leading up to this expansion in order to directly estimate the counterfactual where the university does not expand (Abadie et al. 2010).

To construct a donor pool, I begin by restricting my sample to a balanced panel of medical schools that report positive first-year enrollment between 1960 and 1980. I make the same sample restrictions as in the text, but I do not exclude the University of Puerto Rico Medical School. I utilize all medical schools that did not receive a construction grant

[29] https://med.uc.edu/education/systems-biology-and-physiology-graduate-program/about/program-facilities (Accessed August 10, 2023).

Table B.2: Results for Schools with Low Enrollment Growth: Summary Estimates

	(1)	(2)	(3)	(4)	(5)
Difference-in-Doses Estimate	0.013**	0.015**	0.017*	0.026***	0.031***
	(0.005)	(0.008)	(0.008)	(0.007)	(0.005)
Observations	1683	772	556	376	304

This table reports transformed estimates from equation (B.2), where the outcome is the percentage of new entrants who are women. Each column estimates results over a subset of my sample, determined by the distribution of the difference in total new entrants between 1965 and 1975. Column 1 presents results for all observations, Column 2 presents results for all medical schools below the 50th percentile, Column 3 presents results for all medical schools below the 40th percentile, Column 4 presents results for all medical schools below the 30th percentile, and Column 5 presents results for all medical schools below the 20th percentile. All specifications include a control for the log of total enrollment, the log of men's applications, as well as institution and year fixed effects. All coefficients are scaled by the mean of the dose distribution so that they give an estimate of the change in the percentage women of seats between 1971 and 1977. All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

Table B.3: Raw Correlations

	Women's Enrollment		Fraction Women Enrolled	
	(1)	(2)	(3)	(4)
Adjusted Federal Funding	0.000277***	0.000100	0.000000	0.000001
	(0.000101)	(0.000078)	(0.000001)	(0.000001)
Total Enrollment		0.082654***		
		(0.009251)		
Log Total Enrollment				-0.017759**
				(0.008553)
Constant	6.749632***	-0.541520	0.085293***	0.162000***
	(0.863106)	(1.036813)	(0.006817)	(0.037546)
Observations	98	98	98	98

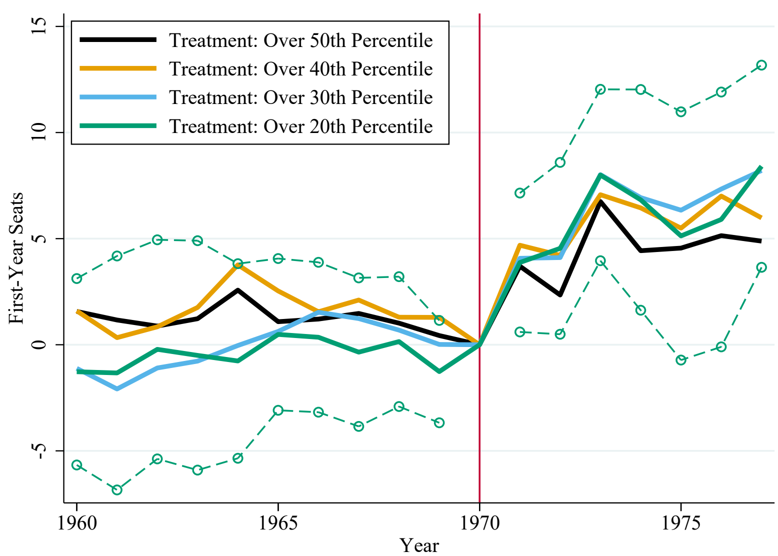
All estimates presented are for a sample restricted to the academic year beginning in 1969. In Columns 1 & 2, the outcome is the number of new entrants who are women, and in Columns 3 & 4, the outcome is the percentage of new entrants who are women. My independent variable of interest is titled "Adjusted Federal Funding," which is the total HEW funding received by a medical school in fiscal year 1969, less the amount given for teaching facilities. Columns 1 & 3 present regression coefficients for a specification including only this variable and a constant; Columns 2 & 4 add a control for total enrollment and the log of total enrollment, respectively.
*** $p < .01$, ** $p < .05$, * $p < .10$

Table B.4: Binarized Dose: Summary Estimates

	(1)	(2)	(3)	(4)
Difference-in-Differences Estimate	3.494**	4.409***	6.341***	6.481***
	(1.596)	(1.604)	(1.980)	(1.740)
Observations	1384	1384	1384	1384

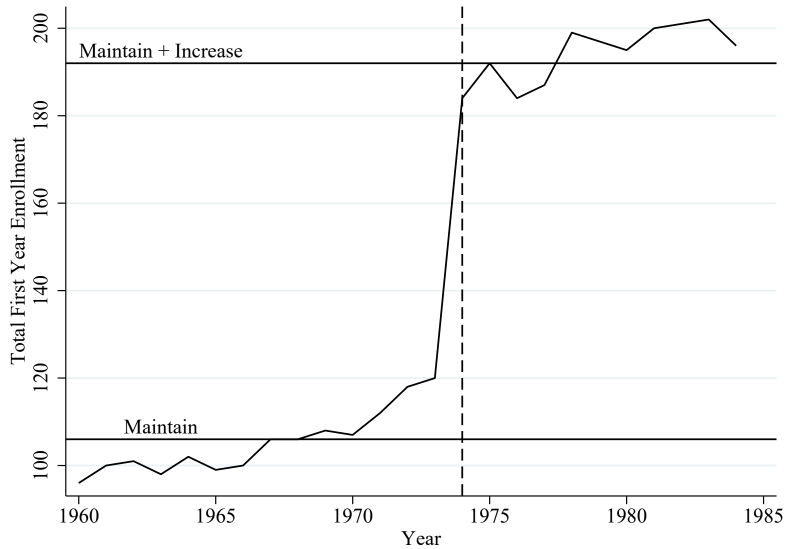
This table reports transformed estimates from equation (B.4), where the outcome is the number of new entrants who are women. Each column estimates results for a different definition of the treatment variable. In Column 1, the units considered treated are over the 50th percentile of the dose distribution; in Column 2, the units considered treated are over the 40th percentile of the dose distribution; in Column 3, the units considered treated are over the 30th percentile of the dose distribution; and in Column 4, the units considered treated are over the 20th percentile of the dose distribution. All specifications include a control for total enrollment, a control for men's applications, as well as institution and state-by-year fixed effects. All coefficients are scaled by the mean of the dose distribution so that they give an estimate of the change in the percentage women of seats between 1971 and 1977. All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

Figure B.4: Difference-in-Differences: Binarized Dose



I plot the event study coefficients from equation (B.3) scaled by the mean of the dose distribution, where the outcome is women's enrollment. I plot results for several definitions of the treatment variable, where the units considered treated are over the 50th percentile of the dose distribution, 40th percentile of the dose distribution, 30th percentile of the dose distribution, and 20th percentile of the dose distribution. All specifications include a control for total enrollment, a control for men's applications, as well as institution and state-by-year fixed effects. I plot a 95% confidence interval for treatment defined at the 20th percentile of the dose distribution, where standard errors are clustered at the institution level. Estimates end in 1977 as application data are not available after this year.

Figure B.5: University of Cincinnati First-Year Enrollment, 1960-1980



This figure plots the time series of total first-year enrollment at the University of Cincinnati medical school from 1960 through 1980. The vertical dashed line at 1974 indicates completion of construction of a new basic science building. This building was funded by a federal grant, in exchange for which Cincinnati promised to maintain 106 seats (lower solid line) and increase enrollment by 86 seats to a total of 192 seats (upper solid line).

after 1969, which includes 45 institutions. To construct a synthetic control, we search for a weighted average of schools in the donor pool that minimize the distance to the treated unit for a collection of pre-intervention covariates, which are left to researcher discretion. I utilize women's enrollment and total enrollment from 1966 through 1970; this prevents potential over-fitting from matching on the entire pre-intervention period and ensures that my estimates are not sensitive to measurement error in estimated enrollment data before 1966. Further, since construction is not completed until 1974, the treatment effect estimate in 1971 through 1973 should be close to zero if it is the case that my synthetic control accurately estimates the latent factors driving women's enrollment. By not matching on these years, I allow for a simple graphical placebo test along these lines. Table B.5 summarizes the results of my estimation procedure, which constructs a synthetic University of Cincinnati from four medical schools.

Figure B.7 plots the synthetic control against observed enrollment. Even though I do not match on 1971 through 1973, I am able to match the rise in women's enrollment well with an estimated treatment effect around 0, suggesting that my synthetic control has matched

Table B.5: Synthetic University of Cincinnati

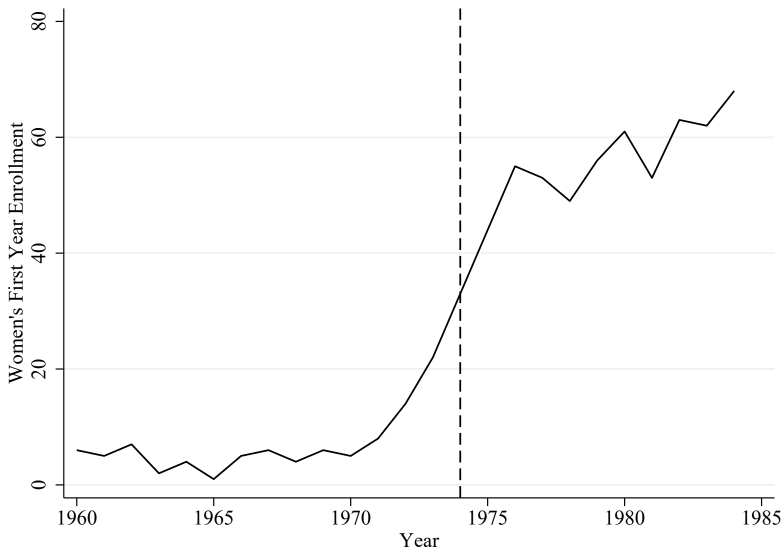
School	Weight	School	Weight	School	Weight
Albany	0.157	Indiana	0	Puerto Rico	0
Albert Einstein	0	Jefferson	0	Rochester	0
Boston	0	Johns Hopkins	0	SUNY-Buffalo	0
Bowman Gray	0	Kentucky	0	SUNY-Downstate	0
California-San Francisco	0	Loma Linda	0	SUNY-Upstate	0
Case Western Reserve	0	Loyola (Stritch)	0	South Dakota	0
Chicago Medical	0	Maryland	0	Southern California	0
Chicago-Pritzker	0	Medical College of GA	0.441	Stanford	0
Colorado	0	Michigan	0	Temple	0
Columbia	0	Missouri-Columbia	0	Tennessee	0
Cornell	0.169	New Jersey Medical	0	Utah	0.197
Duke	0.036	North Dakota	0	Vermont	0
Georgetown	0	Northwestern	0	Washington-St. Louis	0
Hahnemann	0	Oregon	0	West Virginia	0
Harvard	0	Pittsburgh	0	Yale	0

This table includes entries for all medical schools in my donor pool. I include the weight on each medical school which comprises my synthetic control. The only institutions with positive weights are Albany, Cornell, Duke, the Medical College of Georgia, and Utah.

well on latent factors determining women's enrollment. Starting in 1974, I find a distinct break between these series - by 1977, three years after construction is completed, I estimate that the University of Cincinnati enrolls around 20 more women than it would have if it had not constructed a new teaching facility. This point estimate of 20 students is stable through the end of my sample period.

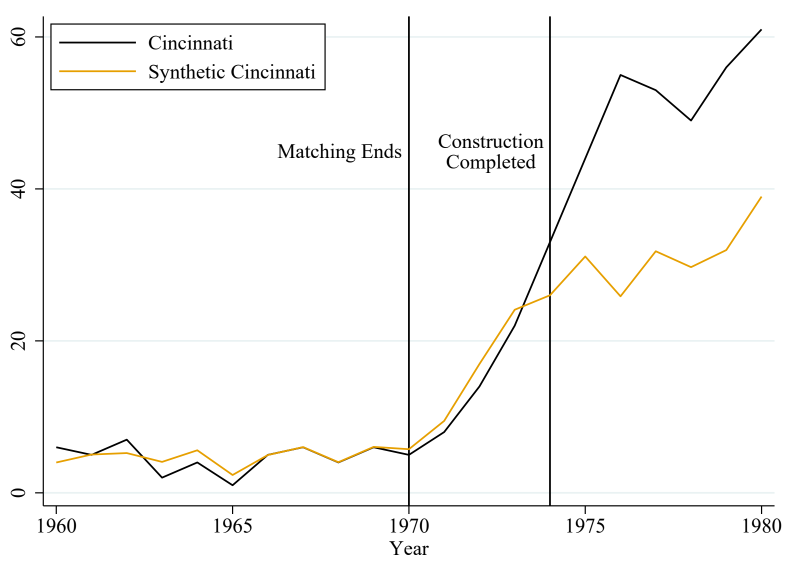
I perform the standard placebo test recommended in Abadie et al. (2010). I add the University of Cincinnati back into my donor pool, and run an identical procedure for all 46 medical schools. Figure B.8 plots the treatment effect estimate for every medical school, with results for the University of Cincinnati in bold; a graphical analysis confirms that my findings are extreme relative to the distribution plotted here. I confirm this by running the standard statistical test recommended by Abadie (2021)—I calculate a p-value of 0.043.

Figure B.6: University of Cincinnati Women's First-Year Enrollment, 1960-1980



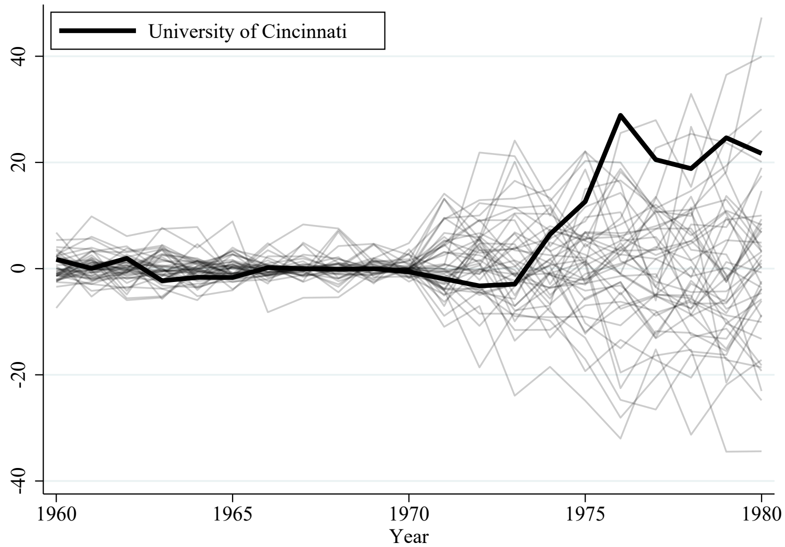
This figure plots the time series of women's first-year enrollment at the University of Cincinnati medical school from 1960 through 1980. The vertical dashed line at 1974 indicates completion of construction of a new basic science building.

Figure B.7: Synthetic Control And Observed Enrollment



This figure plots women's first-year enrollment for the University of Cincinnati against the same time series for my synthetic control. This is constructed by taking a weighted average of women's first-year enrollment at other medical schools, where weights are given in Table B.5

Figure B.8: Placebo Test



This figure plots the results of the placebo test outline in Abadie et al. (2010). Each series here plots the estimated treatment effect for each unit in my donor pool, as well as Cincinnati, which is bolded. This is calculated by constructing a synthetic control for each unit and taking the difference between actual and synthetic enrollment.

C Model

In this section, I present a simple model of the medical school admissions decision to motivate the choice of specifications in the paper and generate some falsifiable predictions. The model is in the spirit of Azevedo and Leshno (2016), where several simplifying assumptions are made to illustrate the most important features of the results.

C.1 Set Up

Consider the admissions problem of a single medical school, which faces mass $f(\theta)$ of female applicants and mass $m(\theta)$ of male applicants, and needs to choose some admissions rule to fill a class of E students. I assume that applicant quality, as appraised by this school, is univariate and given by θ. We can now introduce a measure of discrimination, which I operationalize as a penalty to the score of students in a particular group. Assuming a penalty of size $\tau > 0$ so that a female applicant of quality θ receives a score of $\theta - \tau$, this represents a change in the distribution of female applicants, given by $f(\theta + \tau)$, from the perspective of the admissions committee.

Azevedo and Leshno (2016) show that a stable matching between a discrete set of medical schools and a continuum of students can be represented by each school posting some minimum admissions threshold P, given in units of a student's type at that institution. Given this threshold, enrollment E must be equal to

$$E = \int_P^\infty f(\theta + \tau)\mathrm{d}\theta + \int_P^\infty m(\theta)\mathrm{d}\theta \tag{C.1}$$

C.2 Anti-Discrimination Policy

Now, we can solve for changes in enrollment when discrimination is eased. Let F and M denote women's and men's enrollment, respectively. Differentiating with respect to τ,

$$\begin{aligned} \frac{\mathrm{d}F}{\mathrm{d}\tau} &= -f(P+\tau)\frac{\mathrm{d}P}{\mathrm{d}\tau} + \int_P^\infty f'(\theta+\tau)\mathrm{d}\theta = -\left(1 + \frac{\mathrm{d}P}{\mathrm{d}\tau}\right)f(P+\tau) \\ \frac{\mathrm{d}M}{\mathrm{d}\tau} &= -m(P)\frac{\mathrm{d}P}{\mathrm{d}\tau} \end{aligned}$$

Where the first equality follows by the Leibniz rule and the second equality (for women's enrollment) follows by the fundamental theorem of calculus, assuming that $f(\theta)$ goes to 0 as $\theta \to \infty$. Since we are in a static environment where enrollment does not change ($\mathrm{d}E/\mathrm{d}\tau = 0$), we can totally differentiate equation C.1 with respect to τ. With a bit of algebra, it is straightforward to show that $\mathrm{d}P/\mathrm{d}\tau = -f(P+\tau)/(f(P+\tau)+m(P))$. Substituting this into the expressions above gives us that

$$\frac{\mathrm{d}F}{\mathrm{d}\tau} = -\frac{m(P)f(P+\tau)}{f(P+\tau)+m(P)} = -\frac{\mathrm{d}M}{\mathrm{d}\tau}$$

This theoretical exercise leaves us with two clear predictions. First, a reduction in discrimination should lead to an increase in women's enrollment, and a decrease in men's enrollment of the same magnitude, conditional on total enrollment remaining constant. Second, if policy is successful in reducing discrimination, there should be a one-off change in enrollment that does not grow over time. Put differently, once all schools have responded to the policy change, relative movement in women's enrollment across programs should be driven by changes in student quality and the demand for medical education, not past responses to anti-discrimination policy.

C.3 Enrollment Expansion

In the previous section, I assume that the admissions committee regards enrollment as fixed, and chooses which students to admit to fill a class of size E. I now consider how women's enrollment changes in response to a shock to total enrollment and, in addition, how this depends on discriminatory practices. Recall that total enrollment is given by E, the admissions threshold is given by P, the discriminatory penalty to women's applications is given by τ, and women's and men's enrollment are given by F and M, respectively.

Totally differentiating the equation for enrollment (C.1) gives us that

$$\mathrm{d}E = -f(P+\tau)\mathrm{d}P - m(P)\mathrm{d}P$$

We can solve this equation to determine the change in the admissions threshold in response to a shock to enrollment:

$$\frac{\mathrm{d}P}{\mathrm{d}E} = -\frac{1}{f(P+\tau)+m(P)}$$

Using this change, we can solve for the impact of a shock to enrollment on women's enrollment:

$$\mathrm{d}F = \frac{f(P+\tau)}{f(P+\tau)+m(P)}\mathrm{d}E \tag{C.2}$$

Women capture some fraction of the newly available seats, determined by the fraction of students that were marginally rejected who are women. Importantly, this fraction should change with a reduction in discrimination. Differentiating this fraction with respect to τ,

$$\frac{\partial}{\partial\tau}\frac{f(P+\tau)}{f(P+\tau)+m(P)} = \frac{m(P)f'(P+\tau)}{[f(P+\tau)+m(P)]^2}$$

It follows that women should capture more seats in the absence of discrimination as long as $f(\cdot)$ is increasing, a prediction I test in the following section. Notice, unlike in the previous case, we expect a change in the coefficient on $\mathrm{d}E$ to be persistent. That is, successful anti-discrimination policy should lead to a lasting increase in the fraction of each enrollment expansion that women capture.

D Robustness Checks

This section contains a collection of a variety of robustness checks for my main anti-discrimination result in the text. I estimate a continuous difference-in-differences design with an event study specification:

$$Y_{it} = \sum_{\tau=1960,\tau\neq 1970}^{\tau=1977} \alpha_\tau d_{i,1969} \mathbb{1}(t=\tau) + \boldsymbol{\beta}'\mathbf{X}_{it} + \gamma_i + \delta_{st} + \varepsilon_{it} \tag{D.1}$$

The outcome, Y_{it}, gives the number of women enrolled in the first year at institution i in year t. $d_{i,1969}$ is my preferred measure of exposure to the policy, which is interacted with a set of year dummies, omitting 1970. My parameter of interest, α_τ, captures changes in the relationship between HEW funding and women's enrollment. My baseline specification includes institution fixed-effects γ_i to control for time-invariant differences in school preferences over women's enrollment and year fixed effects δ_t to account for year-to-year changes in women's demand for medical education. My baseline control $\mathbf{X}_{it}$ is the school's total enrollment, which adjusts for changes in women's enrollment attributable to total enrollment growth across institutions. I include two additional specifications to contend with potential confounders to my design, sequentially including controls for men's applications and state-by-year fixed effects. For all designs, standard errors are clustered at the medical school level to correct for serial correlation (Bertrand et al. 2004).

To summarize my event study results, I also estimate a three-part linear spline of the form:

$$\begin{aligned} Y_{it} = {} & \alpha_1^s d_{i,1969}(t-1970) + \alpha_2^s d_{i,1969}(t-1970)\mathbb{1}(t>1970) \\ & + \alpha_3^s d_{i,1969}(t-1970)\mathbb{1}(t>1973) + \boldsymbol{\beta}'\mathbf{X}_{it} + \gamma_i + \delta_{st} + \varepsilon_{it} \end{aligned} \tag{D.2}$$

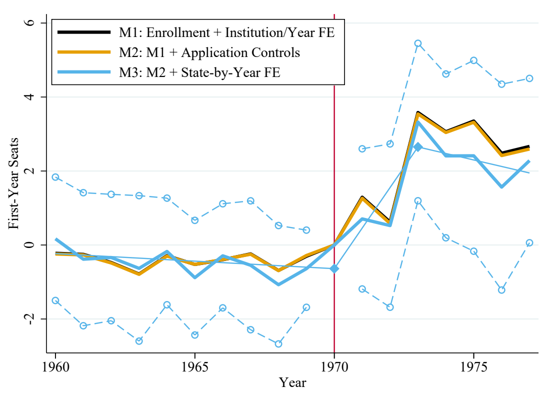
Here, I interact the dose $d_{i,1969}$ with event time $t-1970$ and estimate the slope of my event coefficients before 1970 ($\hat{\alpha}_1^s$), between 1971 and 1973 ($\hat{\alpha}_2^s$) and after 1973 ($\hat{\alpha}_3^s$).

Robustness Checks:

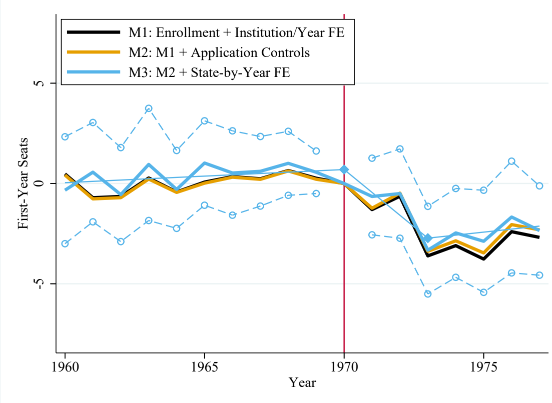
1. Section D.1 replaces $d_{i,1969}$ with a simpler measure of exposure, given by the amount of research funding received by institution i in 1969 (in thousands).
2. Section D.2 considers a variety of alternative specifications
3. Section D.3 restricts my sample to a balanced panel of schools, reporting positive first-year enrollment in 1960
4. Section D.4 estimates a weighted OLS design where each program is weighted by its total enrollment
5. Section D.5 interacts my enrollment control with an indicator for each year
6. Section D.6 replaces Y_{it} with total first-year enrollment for a placebo test

D.1 Alternate Dose Measure

Figure D.1: Outcome: First-Year Enrollment



(a) Women



(b) Men

I plot the event study coefficients from equation (D.1) scaled by the mean of the dose distribution, where the outcome is women's/men's enrollment. For these estimates, I utilize an alternative dose measure which only includes the amount of research funding given by HEW. Model 1 includes a control for total enrollment as well as institution and year fixed effects. Model 2 adds a control for men's/women's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Additionally, I report spline estimates from equation (D.2) for model 3. Estimates end in 1977 as application data are not available after this year.

D.2 Specification Choice

Figure D.2: Outcome: First-Year Enrollment

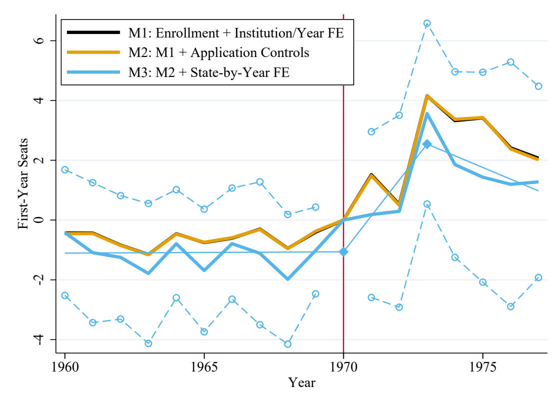
(a) Women

(b) Men

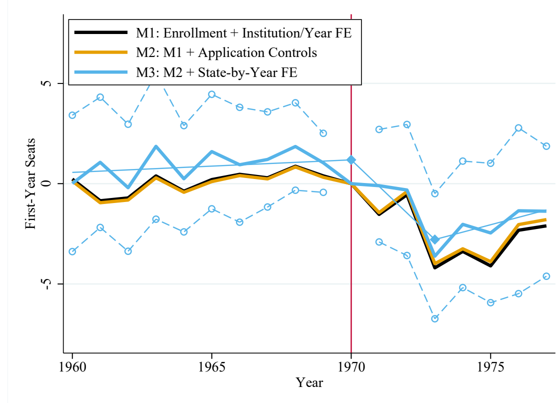
I plot the event study coefficients from equation (D.1) scaled by the mean of the dose distribution, where the outcome is women's/men's enrollment. Each specification considers a particular variant of the three models I utilize in Section 3, which I repeat here for clarity. Model 1 includes a control for total enrollment as well as institution and year fixed effects. Model 2 adds a control for men's/women's applications. Model 3 adds state-by-year fixed effects. In this figure, specification 1 includes only institution and year fixed effects (model 0). Specification 2 augments model 1 to include controls for resident and non-resident tuition. Specification 3 augments model 3 to include control-by-year and type-by-year fixed effects. Specification 4 augments model 3 to include institution-specific linear trends. Specification 5 augments model 3 to include own applications. Estimates end in 1977 as application data are not available after this year.

D.3 Balanced Panel

Figure D.3: Outcome: First-Year Enrollment



(a) Women

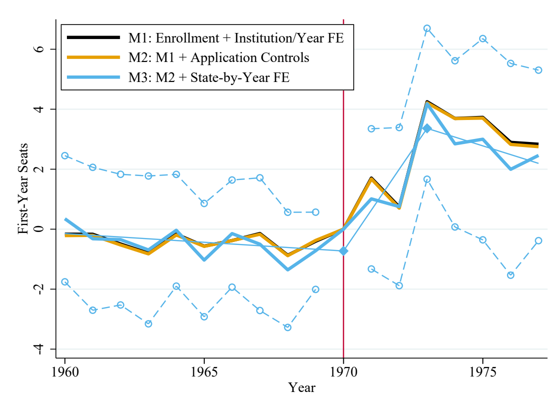


(b) Men

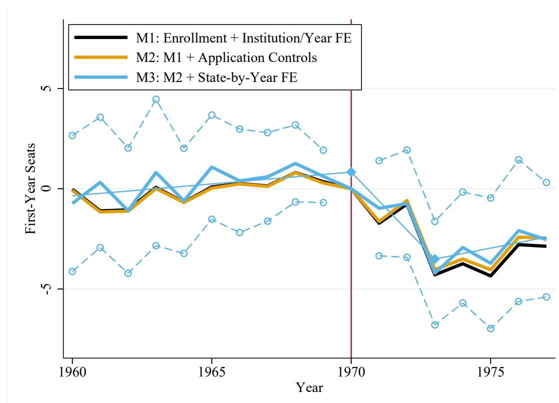
I plot the event study coefficients from equation (D.1) scaled by the mean of the dose distribution, where the outcome is women's/men's enrollment. The data are restricted to a balanced panel, where I require medical schools to report positive enrollment beginning in 1960. Model 1 includes a control for total enrollment as well as institution and year fixed effects. Model 2 adds a control for men's/women's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Additionally, I report spline estimates from equation (D.2) for model 3. Estimates end in 1977 as application data are not available after this year.

D.4 Weights

Figure D.4: Outcome: First-Year Enrollment



(a) Women

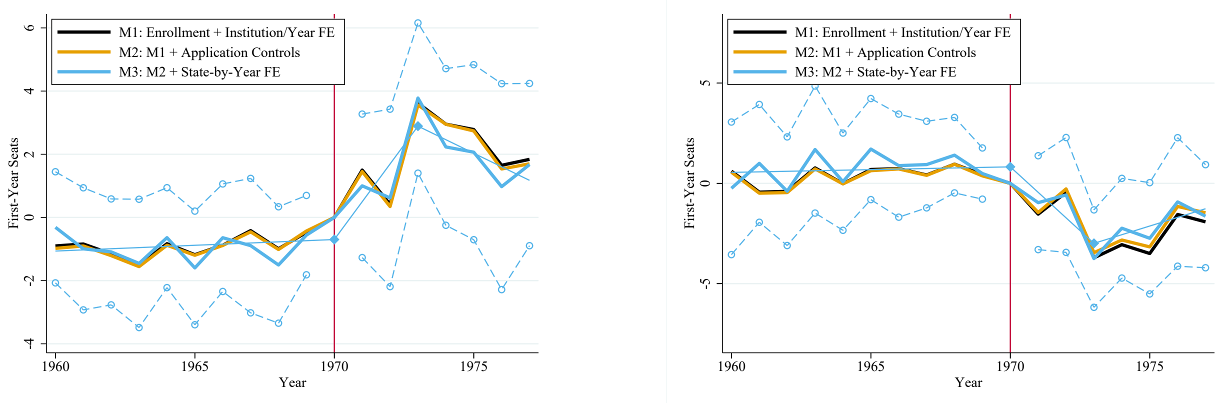


(b) Men

I plot the event study coefficients from equation (D.1) scaled by the mean of the dose distribution, where the outcome is women's/men's enrollment. All specifications are weighted by the total number of new entrants. Model 1 includes a control for total enrollment as well as institution and year fixed effects. Model 2 adds a control for men's/women's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Additionally, I report spline estimates from equation (D.2) for model 3. Estimates end in 1977 as application data are not available after this year.

D.5 Enrollment by Year

Figure D.5: Outcome: First-Year Enrollment

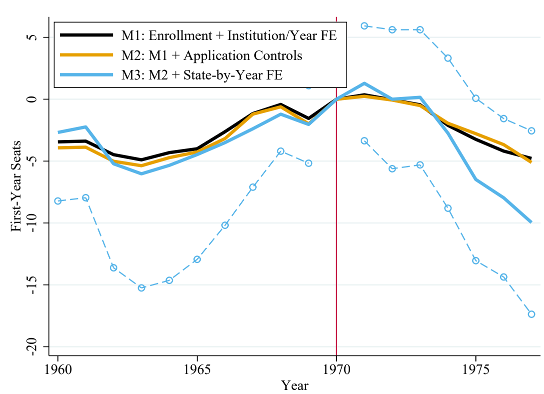


(a) Women (b) Men

I plot the event study coefficients from equation (D.1) scaled by the mean of the dose distribution, where the outcome is women's/men's enrollment. Model 1 includes a control for total enrollment interacted with year dummies, as well as institution and year fixed effects. Model 2 adds a control for men's/women's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Additionally, I report spline estimates from equation (D.2) for model 3. Estimates end in 1977 as application data are not available after this year.

D.6 Placebo Test

Figure D.6: Outcome: Total Enrollment



I plot the event study coefficients from equation (D.1) scaled by the mean of the dose distribution, where the outcome is total enrollment. Model 1 includes institution and year fixed effects. Model 2 adds controls for total applications filed, as well as resident and non-resident tuition. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Estimates end in 1977 as application data are not available after this year.

E Additional Tables

This section contains a collection of additional tables. Tables E.1 and E.2 provide additional heterogeneity results for the anti-discrimination design in Section 3, and Table E.3 reproduces U.S. Congress 1970, pg. 528.

Table E.1: Differences Across the Quality Distribution: Additional Spline Estimates

	(1) All Schools	(2) First Quartile	(3) Second Quartile	(4) Third Quartile	(5) Fourth Quartile
First-Year Entrants					
Pre-Trend Change, 1960-1970	-0.006	0.942*	-0.240	-0.608***	-0.098
	(0.069)	(0.477)	(0.297)	(0.192)	(0.188)
Spline Estimate in 1973 at Mean Dose	3.738***	5.741	6.484	2.278	5.886**
	(0.862)	(4.835)	(3.920)	(2.394)	(2.627)
Observations	1580	392	373	417	398
Graduates					
Pre-Trend Change, 1960-1970	0.095	1.123*	0.227	-0.360	-0.011
	(0.073)	(0.547)	(0.274)	(0.352)	(0.239)
Spline Estimate in 1973 at Mean Dose	2.351**	6.122	1.218	-1.379	5.105
	(0.952)	(3.857)	(3.497)	(2.398)	(3.233)
Observations	1569	393	371	408	397
Applications					
Pre-Trend Change, 1960-1970	3.475***	16.813	18.418***	7.770**	5.248***
	(1.103)	(13.375)	(5.341)	(2.932)	(1.525)
Spline Estimate in 1973 at Mean Dose	34.448*	168.186	49.928	-198.018***	-11.586
	(19.498)	(168.068)	(55.990)	(62.252)	(27.999)
Observations	1581	393	373	417	398

This table reports transformed estimates from equation (2). The header of each section denotes the outcome variable: first-year entrants for section 1, graduates for section 2, and applications for section 3. Column 1 gives results for all schools that have a perceived quality score from Cole and Lipton (1977). Columns 2-5 report results of the same regression model where the sample has been restricted to programs in the first, second, third, and fourth quartile of the perceived quality score distribution, respectively. All coefficients are scaled by the mean of the dose distribution so that they give an estimate of the change in seats/graduates/applications over a time period attributable to the dose variable. Within each section, Row 1 reports estimates of the pre-trend slope and Row 2 reports estimates of the cumulative change between 1971 and 1973 adjusted for the pre-trend slope. All specifications include institution and year fixed effects, as well as controls for total enrollment and men's applications (first-year enrollment/graduate outcomes) or resident and nonresident tuition (application outcomes), equivalent to M2 in Table 1). All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

Table E.2: Changes in Enrollment in Response to Anti-Discrimination Policy: Heterogeneity

	(1) All Schools	(2) Public	(3) Private	(4) University	(5) Northeast	(6) Midwest	(7) South	(8) West
First-Year Entrants								
Pre-Trend Change, 1960-1970	0.001	-0.072	-0.009	0.014	0.005	-0.020	0.133	0.048
	(0.067)	(0.128)	(0.075)	(0.067)	(0.094)	(0.086)	(0.099)	(0.124)
Spline Estimate in 1973 at Mean Dose	3.777***	4.190***	3.531***	4.017***	4.559***	3.711	0.361	3.712***
	(0.810)	(1.205)	(1.155)	(0.880)	(1.258)	(2.409)	(1.962)	(1.170)
Observations	1683	888	747	1291	465	435	550	233
Graduates								
Pre-Trend Change, 1960-1970	0.099	0.059	0.094	0.113	0.111	0.096	0.253***	0.127
	(0.073)	(0.125)	(0.092)	(0.076)	(0.150)	(0.081)	(0.090)	(0.133)
Spline Estimate in 1973 at Mean Dose	2.214**	2.941***	1.353	2.176**	2.670	1.526	-1.904	3.231**
	(0.935)	(1.061)	(1.305)	(1.061)	(1.823)	(2.248)	(1.525)	(1.500)
Observations	1634	854	733	1257	446	408	550	230
Applications								
Pre-Trend Change, 1960-1970	2.855**	1.128	4.512***	2.869**	1.188	5.436*	1.265	2.520
	(1.108)	(1.510)	(1.355)	(1.216)	(1.795)	(2.885)	(1.739)	(2.564)
Spline Estimate in 1973 at Mean Dose	35.351*	1.061	58.170**	57.670***	27.958	15.575	8.531	19.383
	(18.203)	(15.376)	(24.566)	(21.710)	(22.542)	(38.499)	(39.256)	(31.019)
Observations	1684	889	747	1292	465	435	550	234

This table reports transformed estimates from equation (2). The header of each section denotes the outcome variable: first-year entrants for section 1, graduates for section 2, and applications for section 3. Column 1 gives results for all schools. Columns 2 & 3 give results for public and private schools, respectively. Column 4 gives results for all schools affiliated with a university, with affiliation collected from Darley (1965). Columns 5-8 give results for medical schools in each Census Region. All coefficients are scaled by the mean of the dose distribution so that they give an estimate of the change in seats/graduates/applications over a time period attributable to the dose variable. Within each section, Row 1 reports estimates of the pre-trend slope and Row 2 reports estimates of the cumulative change between 1971 and 1973 adjusted for the pre-trend slope. All specifications include institution and year fixed effects, as well as controls for total enrollment and men's applications (first-year enrollment/graduate outcomes) or resident and nonresident tuition (application outcomes), equivalent to M2 in Table 1). All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

Table E.3: Medical School Enrollment by Sex in 1966

			Women					Women	
Rank	Medical School	Men	Number	Percent	Rank	Medical School	Men	Number	Percent
0	Total	30,652	2,771	8.3	45	University of Texas, Southwestern	371	30	7.5
1	Woman's Medical College	0	204	100	46	University of Iowa	451	36	7.4
2	University of Puerto Rico	166	49	22.8	47	University of Florida	219	17	7.2
3	Howard University	322	79	19.7	48	University of Texas, medical branch	549	42	7.1
4	Rutgers State University	13	3	18.8	49	University of Maryland	452	34	7
5	Boston University	244	42	14.7	50	New Jersey College of Medicine, Seton Hall University	282	21	6.9
6	State University of New York, Downstate	652	102	13.5	51	Hahnemann Medical College	398	29	6.8
7	University of California, San Francisco	428	61	12.5	52	University of North Dakota	85	6	6.6
8	University of New Mexico	58	8	12.1	53	University of Oregon	314	22	6.5
9	Case Western Reserve University	310	41	11.7	54	Ohio State University	543	38	6.5
10	New York University	423	55	11.5	55	University of Minnesota	601	42	6.5
11	University of Chicago	250	30	10.7	56	Tufts University School of Medicine	416	29	6.5
12	Loma Linda University	303	36	10.6	57	University of Missouri	305	21	6.4
13	University of Wisconsin	353	42	10.6	58	University of Tennessee	629	43	6.4
14	Columbia University	422	50	10.6	59	University of California, Los Angeles	280	19	6.4
15	Temple University	493	58	10.5	60	Jefferson Medical College	621	42	6.3
16	Stanford University	275	32	10.5	61	Vanderbilt University	193	13	6.3
17	University of Kentucky	248	28	10.1	62	University of Vermont	183	12	6.2
18	Albert Einstein College of Medicine	349	39	10.1	63	Cornell University	317	21	6
19	Albany Medical College	225	24	9.6	64	University of Oklahoma	375	24	6
20	New York Medical College	447	47	9.5	65	University of North Carolina	267	17	6
21	Meharry Medical College	212	22	9.4	66	State University of South Dakota	83	5	5.7
22	Yale University	290	30	9.4	67	Medical College of Virginia	354	21	5.6
23	University of Illinois	695	70	9.2	68	Bowman-Gray School of Medicine	202	12	5.6
24	California College of Medicine	289	29	9.1	69	University of Louisville	343	20	5.5
25	Wayne State University	469	47	9.1	70	University of Kansas	422	24	5.4
26	West Virginia University	210	21	9.1	71	University of Arkansas	351	19	5.1
27	Harvard Medical School	488	48	9	72	University of Washington	299	16	5.1
28	University of Colorado	310	30	8.8	73	Georgetown University	427	22	4.9
29	State University of New York at Buffalo	353	34	8.8	74	Loyola University Stritch School of Medicine	322	16	4.7
30	Louisiana State University	471	45	8.7	75	Emory University	267	13	4.6
31	State University of New York, Upstate	357	34	8.7	76	Marquette University	376	18	4.6
32	University of Pittsburgh	348	33	8.7	77	University of Nebraska	324	15	4.4
33	University of Michigan	713	67	8.6	78	University of Pennsylvania	480	22	4.4
34	Northwestern University	490	46	8.6	79	University of Rochester	263	12	4.4
35	Dartmouth Medical School 2	86	8	8.5	80	Tulane University	489	21	4.1
36	Indiana University	750	68	8.3	81	Baylor University	330	14	4.1
37	University of Southern California	256	23	8.2	82	Medical College of Georgia	369	14	3.7
38	Johns Hopkins University	336	30	8.2	83	University of Virginia	286	9	3.1
39	University of Miami	287	25	8	84	Medical College of South Carolina	299	9	2.9
40	St. Louis University	404	35	8	85	University of Cincinnati	377	11	2.8
41	Washington University	303	26	7.9	86	University of Utah	231	6	2.5
42	George Washington University	373	32	7.9	87	Medical College of Alabama	305	7	2.2
43	Duke University	298	25	7.7	88	Chicago Medical School	279	3	1.1
44	University of Mississippi	275	23	7.7	89	Creighton University	280	3	1.1

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