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

Thomas Helgerman*

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Abstract

During the 1970s, women's representation in medical schools approximately tripled, mirroring similar increases in dental, law, and business programs. Using a newly constructed dataset on medical school admissions, this paper studies the role of anti-discrimination policy in increasing women's access to medical training. My empirical strategy leverages variation across medical schools in the potential loss of federal funding by failing to comply with the non-discrimination mandate in Executive Order 11246. I find that the threat of sanction led to a sharp increase in the percentage of women enrolled in first-year medical school classes. The effects of this policy were likely amplified by a federal effort to increase enrollment—leveraging the differential timing and size of enrollment increases across institutions in a complementary accounting exercise, I find that these expansions account for around one-third of women's gains from 1970 to 1980.

JEL codes: I28, J16, J78, N32

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1 Introduction

The 1960s witnessed the beginning of the "Quiet Revolution" as women increased their involvement in all aspects of the paid economy and pursued non-traditionally female careers (Goldin 2024). Women's representation soared across medical schools, dental schools, law schools and business schools in the early 1970s (Figure 1a) and occupational segregation fell sharply over the next decade (Blau et al. 2013). Hsieh et al. (2019) argue that these changes also reshaped U.S. economic growth: increases in aggregate productivity due to historically underrepresented groups gaining greater access to knowledge-intensive occupations explains between 20% to 40% of the growth in U.S. output per person between 1960 and 2010.

While much is known about the consequences of these changes (Bailey and DiPrete 2016), why this transformation happened so quickly and why it happened in the early 1970s is poorly understood. An episode of rapid change may suggest a role for public policy (Donohue and Heckman 1991), but the obvious policy changes don't correspond with the timing of women's rapid integration. The Civil Rights Act of 1964 prohibited discrimination by any institution receiving federal funding, but women remained largely excluded from professional schools for the rest of the 1960s. Sex was not included as a protected category for programs that received federal financial assistance (including university admissions), and although sex-based employment discrimination was banned, enforcement was limited in the 1960s (Goldin 1990; Bailey et al. 2024). Title IX of the Education Amendments Act of 1972, the most prominent federal action promoting gender equity in higher education, prohibited discrimination on the basis of sex for any institution receiving federal funding, but the law took effect in 1973—after women's rapid integration into professional schools (Figure 1a).

This paper provides a more detailed account of federal compliance strategies, constructs a novel dataset, and develops a new quasi-experimental design to show that government non-discrimination mandates played a pivotal role in increasing women's access to professional programs. These policies largely operated by leveraging the threat of contract, grant, and funding loss for non-compliant institutions. Accordingly, I focus on medical schools as they are heavily dependent on federal funding provided by the National Institutes of Health (NIH), making them a highly likely place where federal policy would bind. I begin by reexamining the development of federal anti-discrimination policy, arguing that 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 in 1967 with Executive Order 11375 prohibiting federal contractors from discrimination in hiring on the basis of sex. Recognizing that many institutions of higher education were recipients of federal contracts, EO 11375 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). Figure 1b plots the year in which EO 11375 became effective, which is followed almost immediately by a sharp increase in the number and fraction of first-year medical students who are women.

For my empirical analysis, 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. Using this dataset, I estimate the causal effect of anti-discrimination policy on women's enrollment in medical schools. Reviewing action by the women's movement leveraging government policy to end sex discrimination in higher education, I identify a complaint filed by the 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 one standard deviation increase in the log of total research funding received from HEW leads to a 1.3 percentage point increase in the fraction of first-year students who are women, representing a 12% increase over women's baseline representation of 11% among first-year students in 1970; these findings are also robust to a "per capita" measure of federal funding per faculty member. I consider a variety of outcome measures across which my results do not change: I find an increase in women's enrollment in levels, an increase in the percentage of graduating students who are women four years later, and an increase in the difference between the enrollment-to-application ratio, a proxy for the acceptance rate, between women and men.

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. Early work focused on Title VII of the Civil Rights Act of 1964, which prohibited employment and wage discrimination on the basis of sex (and other protected characteristics) for covered employers, finding that these policies improved women's earnings (Beller 1979) and helped their entry into male-dominated professions (Beller 1983). Executive Order 11246, which prohibited federal contractors from discriminating on the basis of race, creed, color, or national origin in 1965, led to higher employment growth for Black workers at covered firms (Leonard 1984, 1990), with similar but small effects for white women (Leonard 1990).[1] There were also several states that implemented fair employment practice and equal pay laws in advance of the federal government; Neumark and Stock (2006) find mixed evidence that these mandates benefited workers, finding earnings gains for Black workers but reduced employment for women.

Recent work has begun to challenge some of these earlier conclusions. Building on work by Kurtulus (2016) leveraging changes in contractor status over time for a panel dataset of firms to evaluate EO 11246, Miller (2017) finds that there are persistent effects of coverage even after a federal contract is completed. Bailey et al. (2024) show that focusing on the Civil Rights Act of 1964 misses an important contribution of the 1963 Equal Pay Act, finding that this was very effective at raising women's earnings, leveraging differential exposure to equal pay policy across states and job types. This paper adds to this collection of evidence by showing how the enforcement of civil rights policy evolved, granting women greater access to medical schools in the 1970s, ultimately helping narrow the gender gap in pay by increasing access to higher paying occupations. The empirical design builds on an important contribution from Rim (2021), who explores the impact of Title IX on changes in women's graduate enrollment.

I extend my analysis with an accounting exercise to explore how anti-discrimination policy interacted with Health Manpower Policy. Starting in the mid-1960s, enrollment at allopathic med-

[1] Executive Order 11357 amended EO 11246 in 1967 to include sex as a protected category.

ical schools roughly doubled between 1965 and 1980 due to legislation that incentivized enrollment growth through construction grants for teaching facilities in conjunction with direct payments to medical schools in exchange for increases in enrollment. This policy interaction is important because, with enrollment capacity simultaneously expanding, anti-discrimination policy did not operate in a zero-sum environment where women's gains necessarily came at men's expense. 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 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.

In summary, I provide new causal evidence that federal anti-discrimination policy—especially EO 11246—played a central role in the episodic change in women's medical school enrollment in Figure 1b, which preceded Title IX's implementation. Further, this policy interacted with other federal initiatives to produce unanticipated impacts: as the barriers to women's entry fell, enrollment at allopathic medical schools underwent a massive expansion, and women were able to fill many of these new seats. I provide some of the first causal evidence verifying the claim in Goldin and Katz (2002) that federal anti-discrimination policy played a role in the growth of women's enrollment in professional schools in the 1970s, adding to a literature that had focused almost exclusively on the role of the Pill in women's labor market integration (Goldin and Katz 2002; Bailey 2006; Hock 2007; Bailey et al. 2012).

Section 2 provides an overview of medical school admissions practices in the 1960s, a descriptive look at how women's representation at medical schools changed across the 1970s, as well as a description of how anti-discrimination policy evolved during this time period. 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, then presents an accounting exercise to estimate its impacts. 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 4, 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 is 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

[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)

AAMC.

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 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).

2.1 Changes in the 1970s

As Figure 1b 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 3a 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.

In addition to representation, we might also be interested in women's access to all institutions: is women's enrollment spread evenly across programs or more concentrated? To explore this, Figure 3b 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

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

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.

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, 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.[4] 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 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

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

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 4a, I plot the share of all medical school revenue 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.[5] 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

[5] 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).

total HEW obligations to medical schools in fiscal year 1969 (HEW 1971).[6] 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 (HEW 1971; Fruen 1983).[7] Figure 4b 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 begun 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 research support received in 1969 from HEW. As Figure 4b makes clear, this is the lion's share of federal funding, directly comparable across institutions, and easily interpretable when scaled by the number of faculty members to account for differences in size across programs.[8] 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. Accordingly, I utilize a log transform of research funding received—the distribution of both research funding and log research funding are presented in Appendix Figure 18a and 18b, respectively. I denote the log of research funding received in 1969 by $d_{i,1969}$, 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. 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.

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

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

[8] Appendix Figure 7 presents a robustness check using a more expansive measure of federal support.

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[9] in years 1966 and 1978-1980. In 1966, this information is 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.[10] Appendix Figure 1 gives a graphical representation of the dataset I've constructed, showing the type of information used for each series in every year. Appendix Section 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 Liaison 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. Finally, I limit my sample to institutions that report a positive number of faculty in 1969. This

[9] This is not equivalent to new entrants as it includes students repeating the first year, though these students represent a minuscule portion of the first year class in medical schools. In Appendix Section A.5, I utilize 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 ($R^2 \approx 1$).

[10] 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 Section A.5, I utilize years where estimated and actual enrollment are observed to verify that these estimates are accurate.

ensures both that I exclude institutions that receive positive HEW funding in 1969 but don't operate until later in the 1970s and also that my sample does not change across my two measures of the intensity of federal support.[11]

3.3 Methodology & Specification

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

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

The outcome, Y_{it}, gives the fraction of all students enrolled as new entrants who are women at institution i in year t. $d_{i,1969}$ is my preferred measure of exposure to the policy, which is the log of the total amount of research funding institution i received from HEW in 1969.[12] This variable 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 the fraction of new entrants who are women. If it was the case that this policy raised women's representation, 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. 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

[11]These include the log of federal research funding received, described in the previous subsection, as well as the log of federal research funding received per faculty member, which is described below.

[12]The distribution of this variable is plotted in Appendix Figure 18b.

particular (Boulis and Jacobs 2008). Accordingly, I include the log 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, I weight by total first-year enrollment to correct for heteroskedasticity, and 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 difference-in-differences design of the form:

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

Here, α_{DiD} is my coefficient of interest, reporting a weighted average of the event coefficients from equation (1). To summarize the policy impact, I report $\alpha_{\text{DiD}} * \sigma_{d_{i,1969}}$, which estimates the increase in the fraction of women enrolled in response to a one standard deviation increase in the log of pre-existing federal research funding.

3.4 Results & Discussion

These results are presented in Figure 5a, and difference-in-differences estimates are reported in columns 1-3 of the first row of Table 1.[13] For the 10 years prior to 1971, we see almost no change in the relationship between HEW funding and the fraction of first-year students who are women. This changes abruptly in 1971, where a one standard deviation increase in the dose distribution is associated with a 1.3 percentage point increase in the fraction of women enrolled, representing around a 12% increase over women's baseline representation of 11% among first-year students in 1970. The event coefficient in 1971 is virtually identical to the difference-in-differences estimate across the 1971-1977 period, suggesting that these gains were permanent and that the policy impact was static and did not change across time. Model 2 accounts for changes in men's application

[13] For easy of interpretation, I only include standard errors and a difference-in-differences estimate for Model 1. Appendix Figure 15 includes these statistics across all designs for reference.

behavior, 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 event study coefficients, but as column 3 of Table 1 demonstrates, I still find a strong, statistically significant response in the fraction of women enrolled.

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 change. First, this empirical design implicitly assumes that the causal response depends on the amount of funding received, not this amount relative to the size of the institution. If larger programs receive more funding, I might simply be picking up a scaling effect as larger programs respond more strongly to increasing demand for medical education from women. To rule this out, I consider a second metric for reliance on federal funding given by the log of research funding per faculty member; intuitively, this gives the research "intensity" of each program rather than total funding accumulated. To construct this measure, I collect the number of faculty members in each medical school as of 1969 from the AAMC Faculty Roster (AAMC 1969). These results are presented in Figure 5b, and difference-in-differences estimates are reported in columns 1-3 of the second row of Table 1. My findings are strongly robust to this change, with very similar event study estimates and a slightly attenuated, statistically significant difference-in-differences estimate across all three specifications.

My second concern is that, with the passage of the Comprehensive Health Manpower Training Act in 1971, better funded schools might have expanded enrollment more rapidly, causing an increase in women's enrollment. Of course, this by itself would not lead to an increase in the fraction of students who are women, but potentially could if, on the margin, women were more likely to fill newer seats. To rule this out, in Appendix Figure 17, I run an identical design with total enrollment on the left-hand side to see if there is a similar response to total enrollment. The results from this exercise show that there is little evidence for any response in total enrollment. The event study coefficients show almost no response in total enrollment between 1970 and 1971, and if anything, schools receiving more funding seem to experience a relative decline in enrollment in the 1970s.

To better understand the institutional response, I utilize a second design with women's and men's enrollment as the outcome variables. An increase in the fraction of women enrolled does not necessarily imply that women gained more seats; it could simply be the case that less men were enrolled as total enrollment was lowered. To explore this, I use the same specification as (1), where the outcome, $Y_{i,t}$, is now the number of women (men) enrolled at institution i in year t. My baseline specification includes both institution fixed-effects γ_i and year fixed effects δ_t, as well as a control for the school's total enrollment. As before, Model 2 adds a control for men's (women's) applications (in levels), and Model 3 includes state-by-year fixed-effects.[14] The results from this design are presented in Figure 6a for women and Figure 6b for men, and difference-in-differences estimates are reported in columns 4-9 of Table 1. Comfortingly, I find that women's enrollment expanded sharply following the policy change; my difference-in-differences estimate implies that a one standard deviation increase in the log of federal research funding was associated with a gain of 3 seats for women. Interestingly, the difference-in-differences estimate for men's enrollment is around -3, suggesting that the seats allotted to women as a result of this policy would have been given to men if not for government intervention.

In Appendix Section C, I show that these results persist through a variety of robustness checks. Appendix Figure 7 replicates Figure 5 using an alternative measure of federal support, defined as 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. I show that my results in Figure 5 are robust to a variety of alternate specifications, including the addition of tuition controls (Appendix Figure 8), control-by-year and type-by-year fixed effects (Appendix Figure 9),[15] the inclusion of unit-specific linear trends (Appendix Figure 10), and including a control for the log of women's applications (Appendix Figure 11). Appendix Figure 12 replicates Figure 5 on a balanced panel of schools operating continuously

[14]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. These designs are unweighted as the rationale for correcting for heteroskedasticity is no longer present without a fraction on the left hand side.

[15]Control denotes public/private status, and type denotes if a program is a medical school, basic sciences school (offering only the first two years of a standard medical education), or a developing program.

since 1960. Following Solon et al. (2015), since my results in Figure 5 are weighted, I include results from an unweighted specification (Appendix Figure 13) as well as a specification with weights fixed to first-year enrollment in 1969 (Appendix Figure 14). Finally, Appendix Figure 16 includes results when standard errors are clustered instead at the state level.

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 an alternative outcome $Y_{i,t+3}$, now the fraction of graduates from institution i in year $t+3$ who are women, adjusted to represent the fact that students admitted in year t will not graduate until year $t+3$, which is weighted by the total number of graduates.[16] Additionally, this is also a convenient 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. Difference-in-differences estimates are in columns 1-3 (row 3) 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. These results are also robust to measuring federal support on a per-faculty basis, as reported in columns 1-3 (row 4) of Table 1.

With information on applications filed, I am able to look at a third outcome of interest. Ideally, I would be able to observe the gender-specific acceptance rate, defined as the ratio of women's (men's) acceptances and women's (men's) applications. Unfortunately, I do not observe acceptances, so I proxy for this using women's (men's) enrollment, which allows me to construct the enrollment-to-application ratio for both women and men. I use this to construct the gender difference in this ratio,

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

defined as the enrollment-to-application ratio for women less the same ratio for men. Using this variable as the outcome $Y_{i,t}$, I estimate an unweighted version of equation 1, where my control for men's applications is replaced by controls for the log of resident and non-resident tuition to adjust for responses in the demand for medical education as its price changes. Difference-in-differences estimates for both definitions of my federal dependence measure are reported in columns 1-3 (rows 6 & 7) of Table 1. These suggest that a one standard deviation increase in the dose distribution is associated with around a 1 percentage point increase in the gender difference in the enrollment-to-application ratio, representing a 9% increase over the average enrollment-to-application ratio across all programs in 1970. Columns 4-9 disaggregate this result by running a similar design with this ratio for women (columns 4-6) and men (columns 7-9) on the left hand side—I find that this result is driven by a strong increase in this ratio for women. All results are robust across both measures of federal dependence.

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 fraction 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 representation among applicants, and I include year fixed effects to account for national-level changes in 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, and 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. All models are weighted by total applications filed to correct for heteroskedasticity, and standard errors are clustered at the institution level. The results from this exercise are presented in Figure 7. I find little evidence of a disproportionate response in women's application filing behavior relative to men's, with a difference-in-differences estimate that is precise and close to zero. This result is unchanged across all models and measures of federal dependence.

Finally, Appendix Table 3 presents a series of heterogeneity results, though my ability to detect differences among subgroups is substantially limited by the small number of observations. I report difference-in-differences results for Model 1 for all three outcome variables and two measures of federal dependence I consider in Table 1. I find stronger effects at public programs (column 2) relative to private medical schools (column 3), with magnitudes similar to that at university-affiliated programs (column 4). Comfortingly, I find evidence of stronger effects in the Northeast (column 5) and West (column 8) Census Regions, verifying that my results are not due solely to north-south differences. Interestingly, I do not find significant effects in the Midwest (column 6), and moderate but imprecise effects in the South (column 8).

I also consider differences across the quality distribution. 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. For schools with a perceived quality score, I present results for programs below (column 9) and above (column 10) the median.[17] The results suggest that this mechanism is operating above the median, with the important implication that women are able to access high quality medical education as a result of anti-discrimination policy. That said, 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.[18]

[17] Median perceived quality score is calculated over all medical schools in my sample, not the overall median in 1971.

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

4 Expansionary Policy

In the previous section, I found that anti-discrimination policy led to a sharp increase in women's enrollment immediately following 1970. This inquiry was motivated by the presence of several important pieces of federal policy that were implemented around the time that the rate of women's entry into medical schools increased (Figure 1a and 1b). In addition, the surge in women's enrollment in the 1970s was preceded by a vast increase in total enrollment (Figure 8a). Starting in the mid-1960s, enrollment at allopathic medical schools undergoes a massive expansion, essentially doubling between 1965 and 1980. If enrollment had remained constant in the 1970s, gains for women would have necessarily come at the expense of men; however, since capacity was expanding at the same time, women were able to enter in larger numbers even as men's enrollment in the 1970s rose (Figure 8a) amid an increase in men's demand for medical education.[19]

This increase in total enrollment was the result of several pieces of legislation under the umbrella of Health Manpower Policy that included a variety of programs aimed at both medical programs and students designed to boost enrollment. The progression of Health Manpower Policy is summarized in Figure 8b, 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 between 1965 and 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 to 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.

Given the magnitude and timing of this expansion, it seems very plausible that, following a successful effort to codify sex non-discrimination through the legislature with Title IX in 1972, Health Manpower Policy could have amplified these efforts to increase women's representation in medical schools. Accordingly, to understand in a more complete manner how federal policy mattered

[19] Men's total applications to medical schools increase more than two-fold between 1961 and 1975 (Boulis and Jacobs 2008, Figure 2.1).

for women's entry into medicine, I now consider the impact of policies aimed at expanding total enrollment. I begin by describing the progression of Health Manpower Policy over the 1960s and 1970s before turning to an accounting exercise to estimate its impact.

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 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 two-thirds of the cost of building an educational facility for a new school or expanding facilities in 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,[20] 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

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

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 additional 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

We can get a crude estimate of the success of Health Manpower policy by using the 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.[21] 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 understand 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 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. My data and sample remain the same as in Section 3,

[21] 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.

with the only change being the inclusion of medical schools that do not report a positive number of faculty in 1969 —the opening of new medical schools is a crucial component of Health Manpower Policy, so it is important to include these programs when estimating its impact.

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 first-year 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.[22] 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.[23]

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

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

[23]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.

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 I find no dynamic impact of anti-discrimination policy in Section 3. Formally, define $\mathcal{B}_t$ as the vector of year bins and $\boldsymbol{\alpha}$ 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 2 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 2. I omit the 1965-1970 term in my vector of 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 2. 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 1975-1980 relative to 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, 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:

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

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

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

Estimation results are contained in the top section of Table 2 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 3.2 percentage points between $k = 1$ and $k = 3$ for enrollment expansions occurring in 1965-1970; this figure is similar at 4.9% for 1970-1975, but far larger at 12.0% for 1975-1980.

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 (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 underrepresented 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 9a and 9b 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 9a. As these new programs began to increase enrollment, they became an increasingly large part of the production 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 9b 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.[24]

The results in Table 2 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 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 3. 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. 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 9b was that all seats at new schools were created in a recent enrollment expansion.

5 Conclusion

Considering what caused the abrupt change in the entry rate of women into professional occupations (pictured in Figure 1a), Goldin (2005) lays out three probable candidates for this change: (1) government mandates prohibiting discrimination in employment practice and educational programs; (2) social changes following the emergence of second wave feminism in the 1960s; and (3) the introduction and spread of oral contraception ("the Pill"). Among these, the literature has leveraged the staggered rollout of the Pill across states to identify its labor market impacts (Goldin and Katz 2002; Bailey 2006; Hock 2007; Bailey et al. 2012), but almost no microeconomic evidence exists

[24] 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.

testing the hypotheses that government mandates or social changes played a causal role. This paper provides one of the first pieces of evidence that federal anti-discrimination policy mattered for this transition, finding that it played an important role in women's entry into medical schools.

Leveraging variation across medical schools in their exposure to federal anti-discrimination policy, I find that a one standard deviation increase in the log of total research funding received from HEW lead to a 1.3 percentage point increase in the fraction of women enrolled in the first year. My results suggest that this policy served to open the door for women in medicine, as their numbers were buoyed in later years by a massive expansion in medical school capacity under the umbrella of Health Manpower Policy. The key limitation of this study, of course, is that it looks only at medical programs; other professional schools that are less reliant on federal funding might have been less responsive. That said, for programs that operate as part of a broader university or college, federal threats of contract loss for the entire organization could lead to indirect pressure for changes in admission policy from the institution's administration.

This study also has important implications for understanding the ways in which educational institutions, and organizations more broadly, respond to federal policy. Put in the simplest terms, my results show that incentives matter. I find that organizations that had more to lose from violating federal anti-discrimination policy responded more strongly and, in the case of Health Manpower Policy, that providing direct payments to medical schools in exchange for enrollment increases seems to have been an effective way to increase enrollment. While the policies I explore were implemented several decades ago in the midst of the emergence of federal civil rights policy, this lesson is still policy relevant today. The infrastructure in place that governs the way the federal government attempts to influence how educational institutions behave has remained remarkably stable since the mid-1970s, with core policies like the Civil Rights Act (1964), Higher Education Act (1965), Title IX (1972), and the Family Educational Rights and Privacy Act (1974) still in place. While the way in which these policies have impacted higher education has changed across time—the debate over Title IX did not feature much discussion about sexual harassment or access to athletic programs, for example—the way in which they incentivize organizational changes has not.

In March 2025, the federal government announced the cancellation of $ 400 million in federal grants and contracts to Columbia University following an investigation of violations to Title VI of the Civil Rights Act, of which $ 250 million was provided by the NIH (Columbia Spectator 2025). Funding was eventually restored after Columbia agreed to a series of payments to the federal government along with changes in campus policy (PBS News 2025). While priorities may change from one presidential administration to the next, federal policy levers have remained essentially unchanged. Following an HEW probe following allegations of sex discrimination at the University of Michigan in 1970, the federal government held up $4 million in contracts ($32m in June 2025 dollars) until the university agreed to federal demands, including back pay for discrepancies in pay (Harvard Crimson 1971). My study does not focus on individual investigations but rather the threat of federal action; accordingly, my findings suggest that a marked change in federal priorities, backed by the credible threat of the loss of federal funding, could have broad impacts on organizational behavior beyond the handful of investigations conducted at elite institutions.

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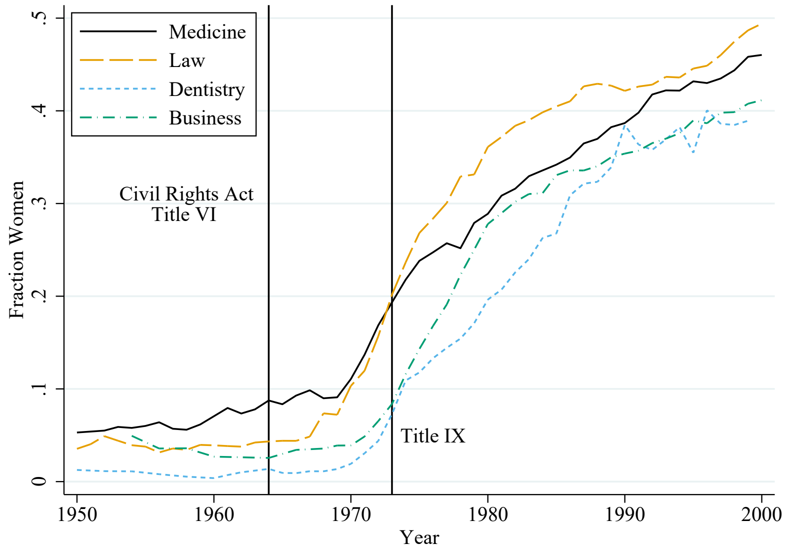
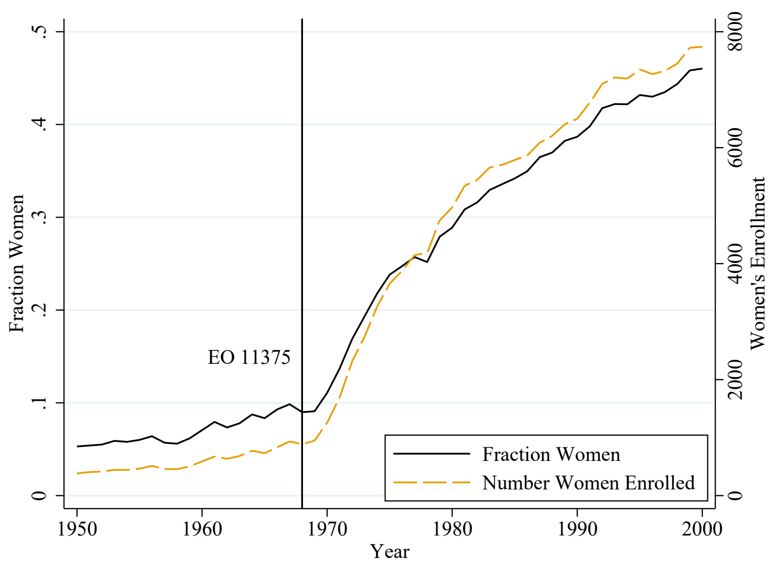
Figure 1: Women's Enrollment in Professional Occupations



(a) Trends in Professional School Enrollment, 1950-2000



(b) Trends in Medical School Enrollment, 1950-2000

Figure 1a plots the fraction of first-year students who are women across programs in medicine, law, dentistry, and business between 1950 and 2000. Data on allopathic medical schools is collected from Potthoff (1960), Dubé (1973), and the 1980, 1990, and 2001 Education Numbers of the *American Medical Association*. Data on American Bar Association approved law schools is collected from the *Review of Legal Education* between 1950 and 1995 and the *ABA Guide* between 1998 and 2002. Following Goldin (2006), I collect data on dental degrees awarded by academic year from NCES (2005), Table 257 and lag this by four years. Similarly, I collect data on business degrees awarded by academic year from NCES (2005), Table 278 and lag this by two years. In addition, I mark the year in which the Civil Rights Act of 1964 and Title IX of the Educational Amendments of 1972 were effective (1964 and 1973, respectively). Figure 1b plots the fraction and number of first-year students who are women at U.S. allopathic medical schools between 1950 and 2000. In addition, I mark the year in which Executive Order 11246 first applied to women (1968).

Figure 2: Admissions Preferences in 1960

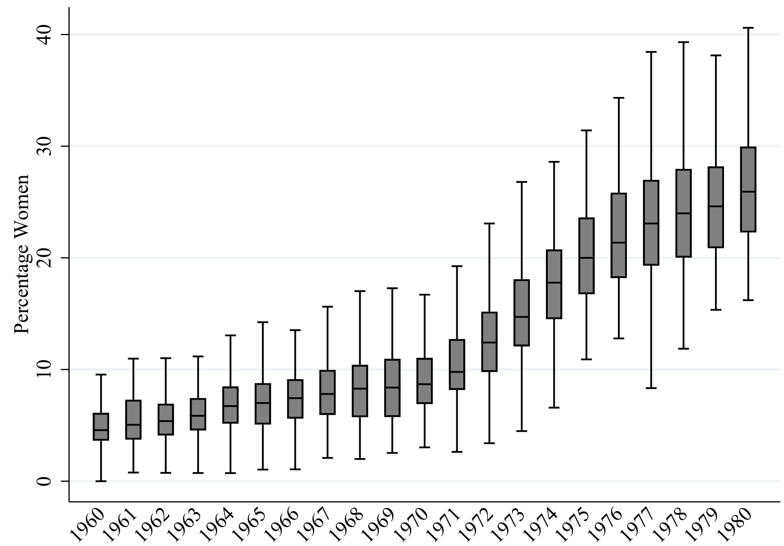
Table 5

Factors Considered in Medical Student Selection

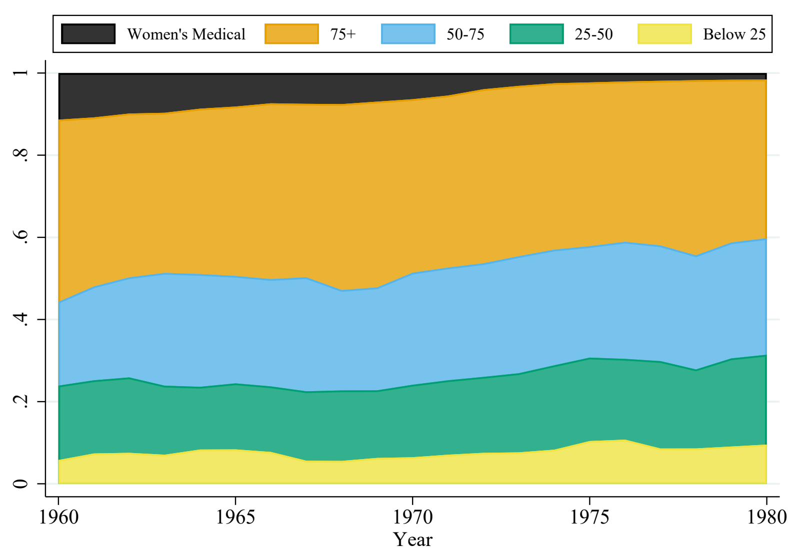
Medical school	Some admission preference on the basis of each factor						Age preference	
	Residence	M.S. or other advanced degree	Undergraduate work at parent university	Sex	Race	Religion	Range	Exceptions
Cornell	No	No	No	No	No	No	20-25	Occasional
Creighton	No	No	Yes	No	No	Yes	20-30	Occasional
Dartmouth	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.

Figure 3: Changes in the 1970s



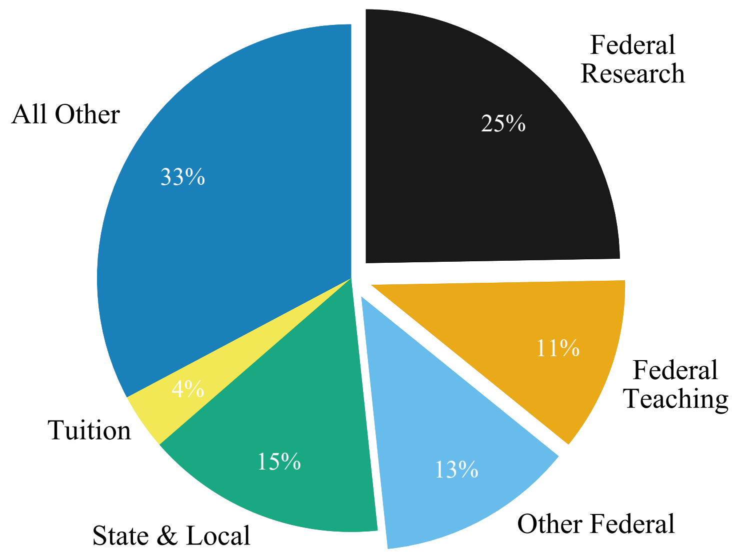
(a) Evolution of Women's Representation



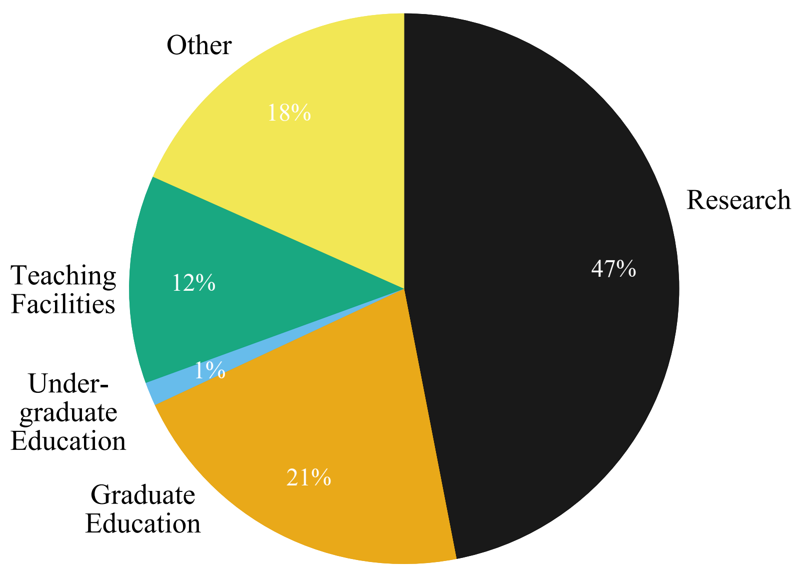
(b) Evolution of Women's Access

Figure 3a 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^{th}, 50^{th}, and 75^{th} percentile of this distribution. The whiskers plot the upper and lower adjacent values. In Figure 3b, for each year, I calculate the 25^{th}, 50^{th}, and 75^{th} 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. Women's Medical, an all-women's medical school until 1970, is plotted separately as well.

Figure 4: Medical School Finances



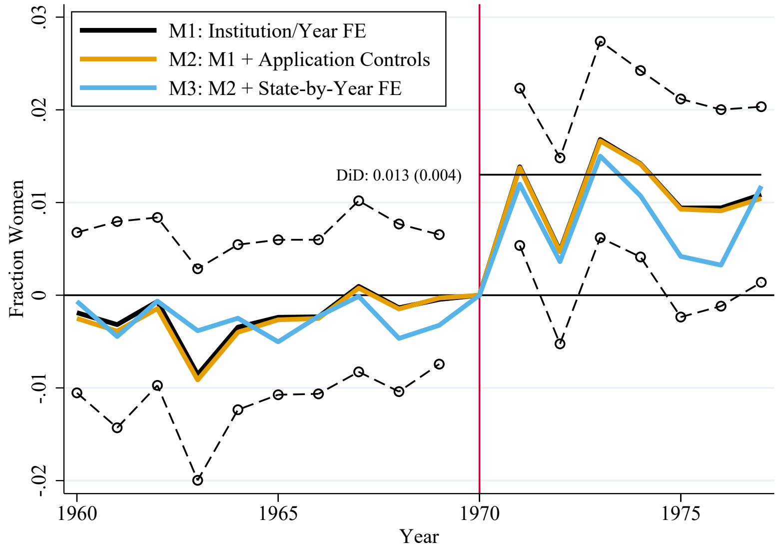
(a) Evolution of Women's Representation



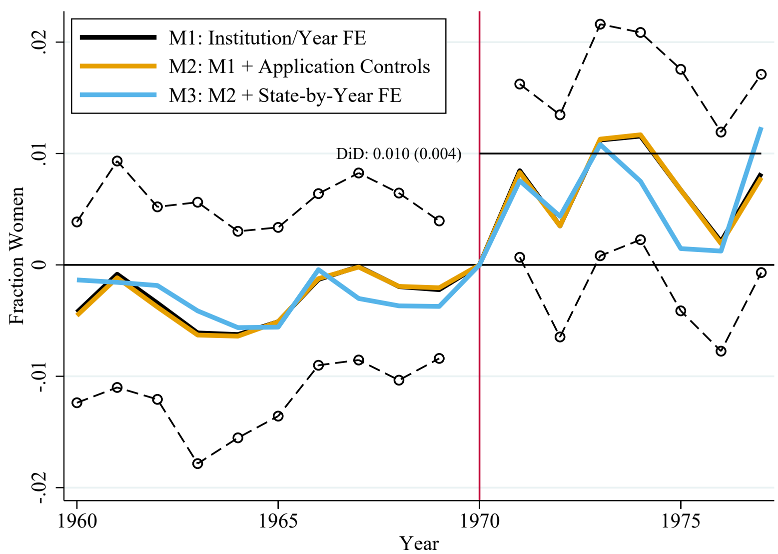
(b) Source of HEW Support

In Figure 4a, 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 4b, I plot the percentage of total medical school HEW support by program in fiscal year 1969. The data were collected from HEW 1971.

Figure 5: Difference-in-Differences Results



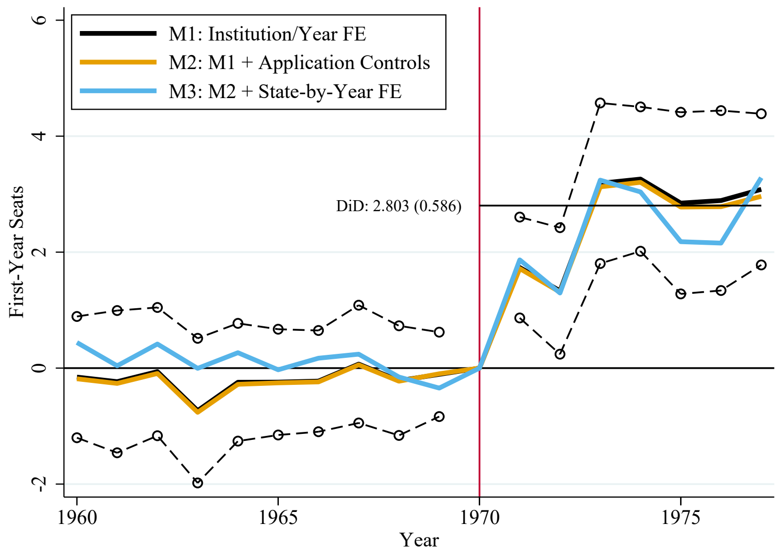
(a) Log Research Funding



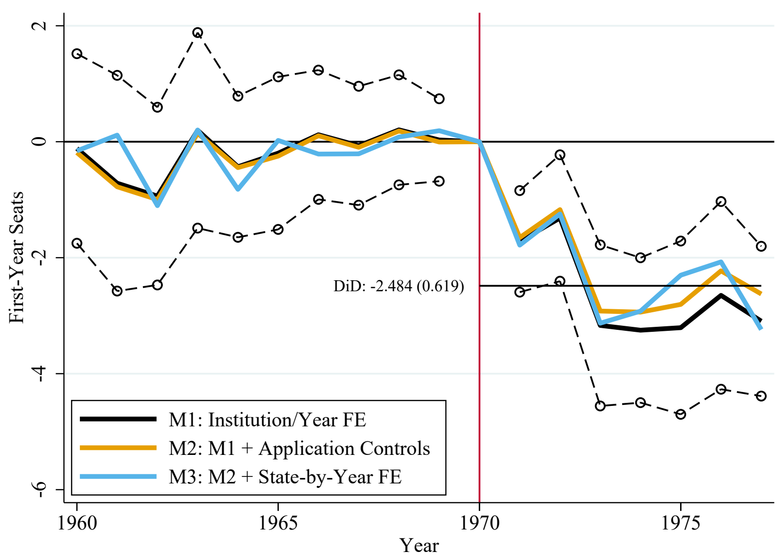
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 5a and the log of total research funding per faculty member in Figure 5b. Model 1 includes only institution and year fixed effects. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Figure 6: Results for New Entrants



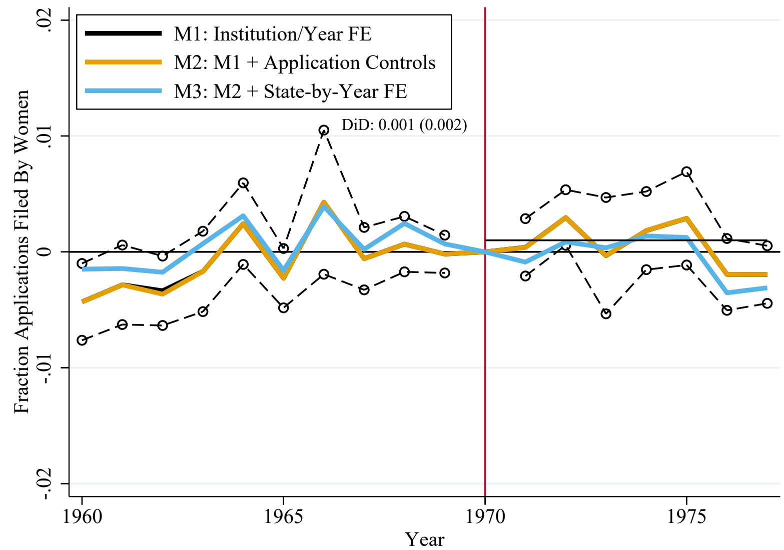
(a) Women



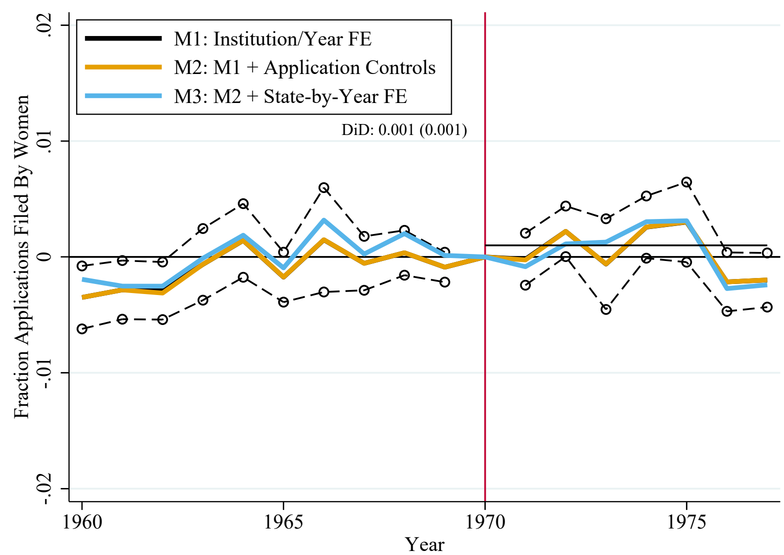
(b) Men

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is women's (Figure 6a) or men's (Figure 6b) 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 6a) or women's (Figure 6b) applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Figure 7: Application Results



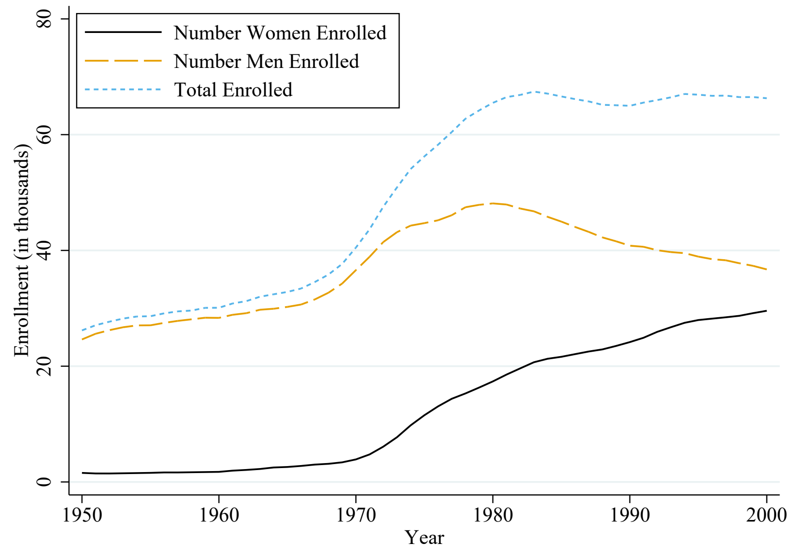
(a) Log Research Funding



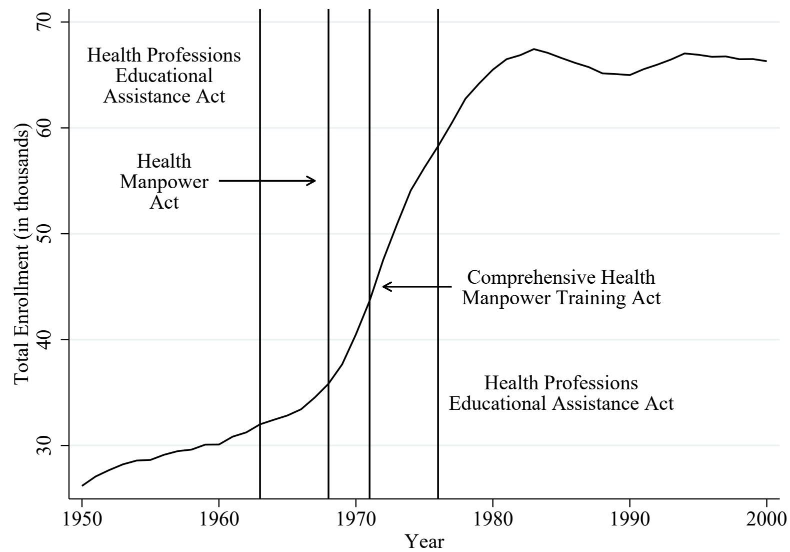
(b) Log Research Funding Per Faculty

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of applications submitted by women and the regression is weighted by total applications. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 5a and the log of total research funding per faculty member in Figure 5b. Model 1 includes only institution and year fixed effects. Model 2 adds a control for the log of resident tuition and the log of non-resident tuition. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Figure 8: Health Manpower Policy and Women's Enrollment



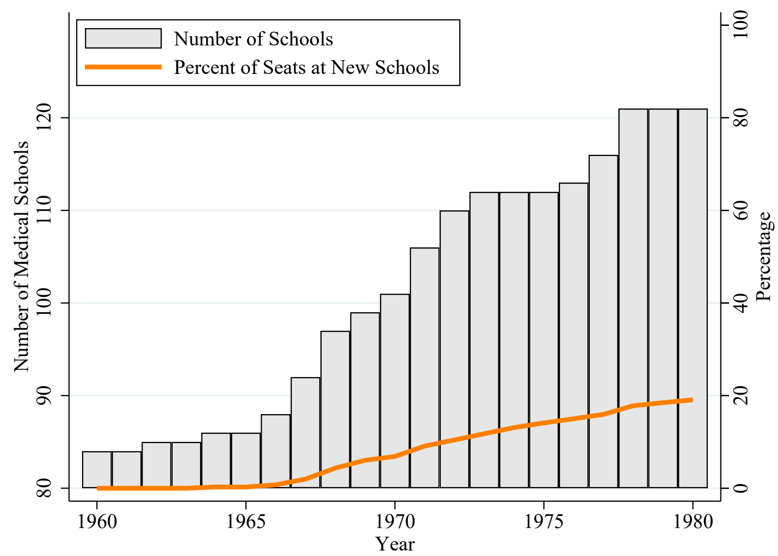
(a) Trends in Medical School Enrollment, 1950-2000



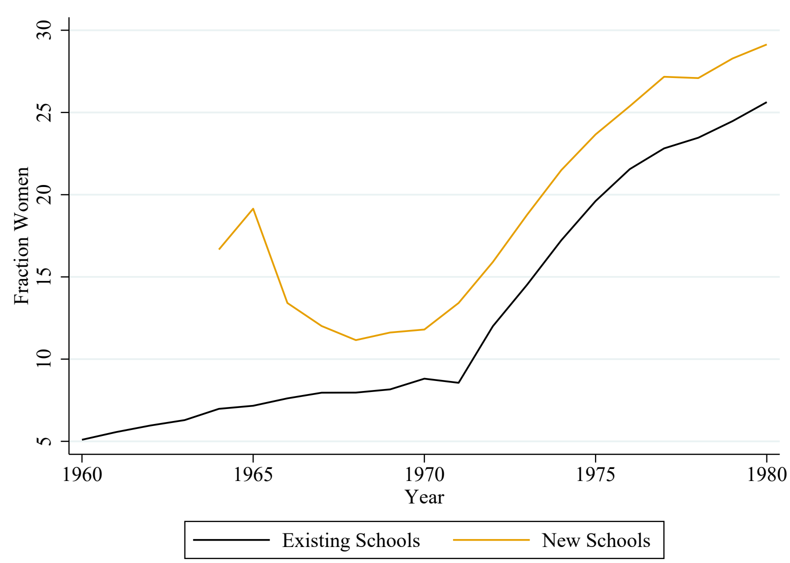
(b) Health Manpower Policy Timeline

Figure 8a plots women's, men's, and total enrollment across all allopathic medical schools in the U.S. Data are collected from the *Journal of the American Medical Association*'s Education in various years between 1950 and 2001. Figure 8b plots total enrollment from this same data series along with vertical lines denoting the timing of the main pieces of Health Manpower Legislation.

Figure 9: New and Existing Medical Schools



(a) Number of Medical Schools



(b) Women's Representation

Figure 9a: 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 on or after 1963, when Health Manpower policy begins. Figure 9b: 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 1: Changes in Enrollment in Response to Anti-Discrimination Policy: Difference-in-Differences Estimates

	Fraction Women			Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
First-Year Entrants									
Log Research Funding	0.013***	0.013***	0.011**	2.803***	2.751***	2.381***	-2.484***	-2.143***	-2.237***
	(0.004)	(0.004)	(0.005)	(0.586)	(0.585)	(0.768)	(0.619)	(0.602)	(0.841)
Log Research Funding Per Faculty	0.010***	0.010***	0.009**	1.359**	1.309**	1.564**	-1.241**	-0.944*	-1.522**
	(0.004)	(0.004)	(0.005)	(0.564)	(0.565)	(0.648)	(0.564)	(0.548)	(0.689)
Observations	1580	1580	1248	1580	1580	1248	1580	1580	1248
Graduates									
Log Research Funding	0.011***	0.011***	0.010*	2.843***	2.867***	2.393***	-2.843***	-2.624***	-2.358***
	(0.004)	(0.004)	(0.005)	(0.619)	(0.635)	(0.746)	(0.619)	(0.620)	(0.766)
Log Research Funding Per Faculty	0.008**	0.008**	0.008*	1.481***	1.449***	1.600**	-1.481***	-1.216**	-1.478**
	(0.003)	(0.003)	(0.004)	(0.531)	(0.544)	(0.641)	(0.531)	(0.531)	(0.649)
Observations	1570	1563	1247	1570	1563	1247	1570	1563	1247
Enrollment-to-Application Ratio									
Log Research Funding	0.009**	0.010***	0.010**	0.017***	0.017**	0.015**	0.008	0.007	0.005
	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.005)
Log Research Funding Per Faculty	0.008**	0.009**	0.008**	0.017**	0.017**	0.015*	0.009*	0.008	0.007
	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.008)	(0.005)	(0.005)	(0.006)
Observations	1579	1577	1244	1579	1577	1244	1580	1578	1245
TWFE	X	X	X	X	X	X	X	X	X
Applications		X	X		X	X		X	X
State-by-Year Fixed Effects			X			X			X

This table reports transformed difference-in-differences estimates from equation (2). Section 1 uses data on first-year entrants to calculate the outcome variables. For columns 1-3, the outcome variable is the fraction of first-year entrants who are women; for columns 4-6, the outcome variable is the number of women enrolled; and for columns 7-9, the outcome variable is the number of men enrolled. In Section 2, the outcome variables remain the same, but they are calculated for graduates, where the data are lagged by 3 years as discussed in the text. In Section 3, the outcome variable is some transformation of the ratio of students enrolled in the first year to applications filed. For columns 1-3, the outcome variable is the difference between this ratio for women and men; for columns 4-6, the outcome variable is this ratio for women; and for columns 7-9, the outcome variable is this ratio for men. Model 1 includes institution and year fixed effects (Column 1), as well as a control for total enrollment (Columns 4&7 only). Model 2 (Columns 2, 5, & 8) includes controls for applications filed by the opposite sex for first-year enrollment/graduate outcomes and tuition controls for enrollment-to-application ratio outcomes. Model 3 (Columns 3, 6, & 9) adds state-by-year fixed effects. For each row of coefficient estimates, the row title indicates the dose used. All coefficients are scaled by the standard deviation of the dose distribution. All standard errors are clustered at the institution level.

*** $p < .01$, ** $p < .05$, * $p < .10$

Table 2: 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.
*** $p < .01$, ** $p < .05$, * $p < .10$

Table 3: 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 on or 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 2. Standard errors are clustered at the institutional level.
*** $p < .01$, ** $p < .05$, * $p < .10$

Online Appendix

A Data Appendix

A.1 Total Enrollment Data

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 Appendix Figure 2.

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

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 Appendix Figure 3. 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. In 1966, I collect this information from the *MSAR*. 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 preceding 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. Appendix Table 1 reports the results from (A.1), (A.2), (A.3), and (A.4). The estimated coefficients 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.

A.6 Faculty Data

I collect counts of the number of faculty at each medical school from the 1969 AAMC Medical School Faculty Table (AAMC 1969). Even though the data are historical, schools are organized by the name of the institution at present. This is generally a non-issue, with the exception of Drexel University. This medical school was created after the merger of Woman's Medical College and

Hahnemann Medical College, which occurred after my sample period. To correct this, I count the number of faculty in the Hahnemann Medical College 1969 yearbook. I count 124 total, exactly half of the 248 faculty reported at Drexel in this year.

B 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,[25] and the time series for enrollment verifies this. Appendix Figure 4a 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 4b. 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

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

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 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. Appendix Table 2 summarizes the results of my estimation procedure, which constructs a synthetic University of Cincinnati from four medical schools.

Appendix Figure 5 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 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. Appendix Figure 6 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.

C Robustness Checks

The appendix 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:

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

The outcome, Y_{it}, gives the fraction of all students enrolled as new entrants who are women at institution i in year t. $d_{i,1969}$ is my measure of exposure to the policy; as in the text, I run two designs for two separate measures of exposure. First, I use my preferred measure of exposure, given by the log of the total amount of research funding institution i received from HEW in 1969; then, I run a secondary design, where this measure is the log of the total amount of research funding per faculty member. This variable 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 the fraction of new entrants who are women. 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. I include two additional specifications to contend with potential confounders to my design, sequentially including controls for log 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). Observations are weighted by the total number of first-year students enrolled to correct for heteroskedasticity.

To summarize my event study results, I also estimate a difference-in-differences design of the form:

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

Here, α_{DiD} is my coefficient of interest, reporting a weighted average of the event coefficients from equation (C.1). To summarize the policy impact, I report $\alpha_{\text{DiD}} * \sigma_{d_{i,1969}}$, which estimates the increase in the fraction of women enrolled in response to a one standard deviation increase in the log of pre-existing federal research funding.

Appendix Figure 7 replaces $d_{i,1969}$ with an alternative measure of exposure to federal policy, given by 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.[26] This measure is broader that research support, but as demonstrated in Appendix Figure 7a, does not qualitatively alter my results. The results for a per faculty member measure, given in Appendix Figure 7b, are much noisier, as this measure does not have a clear interpretation, since faculty might not be involved in obtaining funding for non-research activities, but my difference-in-differences estimates remain very similar to my main result.

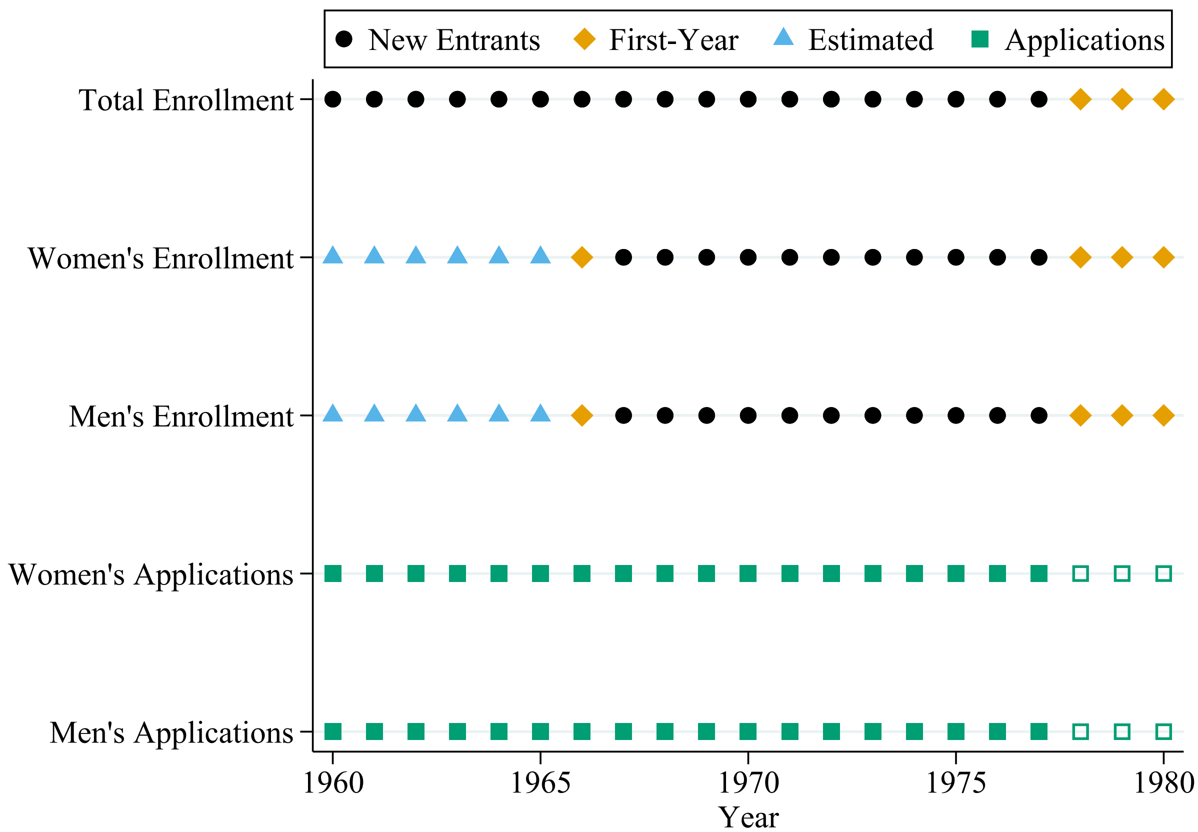
I explore robustness to several alternative specifications. Appendix Figure 8 adds controls for the log of resident tuition and non-resident tuition. Appendix Figure 9 adds control-by-year (public/private) and type-by-year (medical/basic/developing) fixed effects. Appendix Figure 10 adds institution-specific linear trends, where 1960 is also dropped from the year indicators interacted with $d_{i,1969}$ to allow for identification of the event coefficients. Appendix Figure 11 includes a control for the log of women's applications filed. Appendix Figure 12 estimates my main specifications on a balanced panel, where medical schools must report positive first-year enrollment beginning in 1960.

My main design includes weights for total enrollment to improve precision due to larger variance in the fraction of first-year enrollees who are women in programs with lower total enrollment. However, as Solon et al. (2015) note, Dickens (1990) showed that weighting like this, designed to deal with heteroskedasticity, can actually harm precision in the presence of serial correlation. Since my setting also almost certainly exhibits serial correlation, I follow the recommendation from Solon et al. (2015) and present results from an unweighted design in Appendix Figure 13. Appendix Figure 14 also presents results from a design where weights are fixed at total enrollment in 1969 so that my results are unaffected by the changing size of medical schools across time.

I include weights in my main specifications because I am able to estimate standard errors robust to serial correlation within cluster. I cluster at the institution level, and Appendix Figure 15 shows standard errors for all models. In Appendix Figure 16, I consider a more conservative design that clusters at the state level, which widens my standard errors slightly.

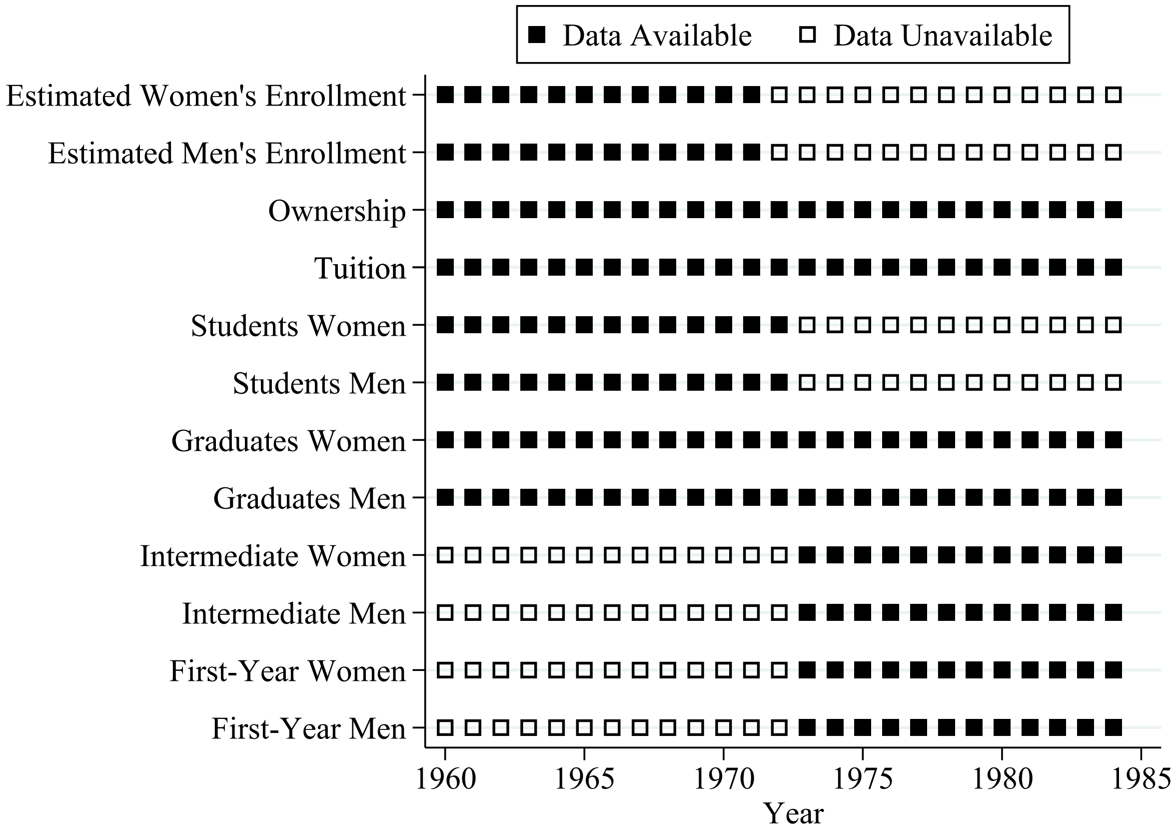
[26] Histogram plots of dose variables used in this robustness check are plotted in Appendix Figure 20 and 21.

Appendix Figure 1: Graphical Description of Dataset



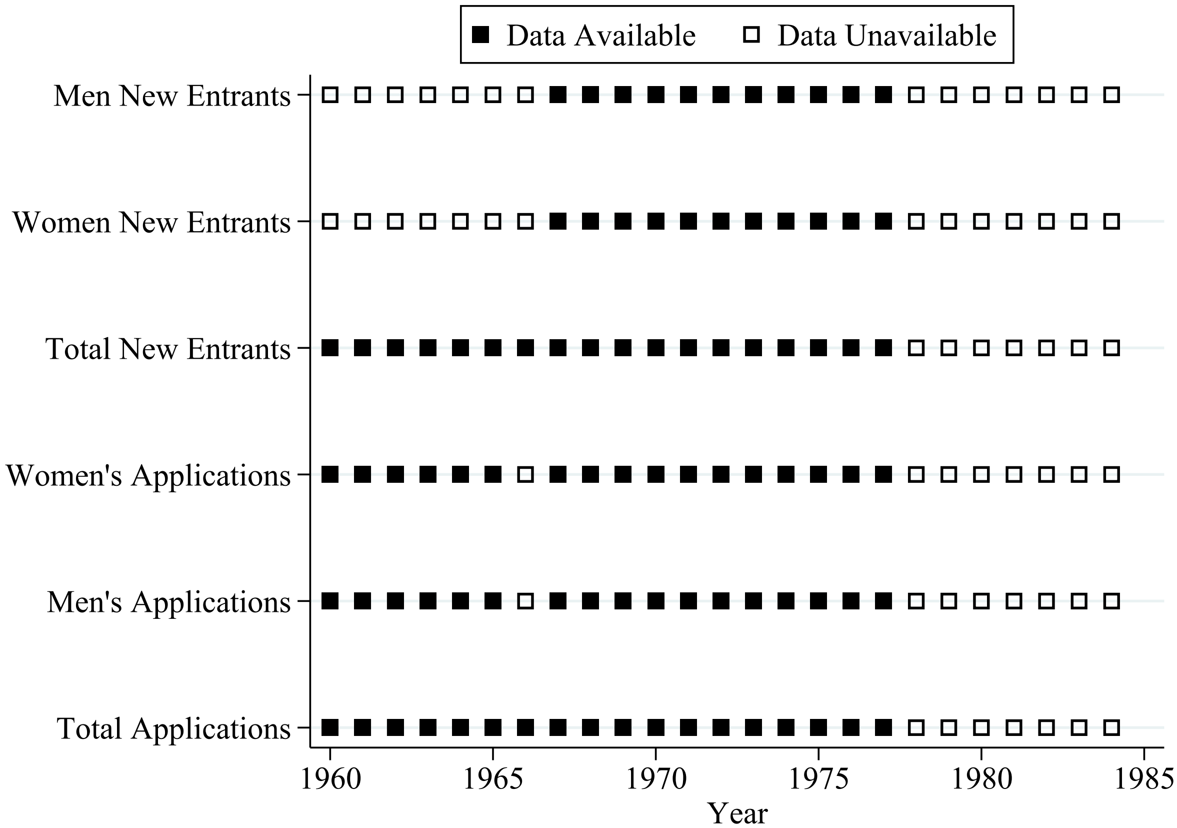
This figure gives a visual description of how my panel dataset is constructed. For each main variable of interest, the marker in a given year indicates if the data from that year pertains to new entrants, all first-year students (new entrants and repeat students), or is estimated (in the spring of the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

Appendix Figure 2: *Journal of the American Medical Association* Education Number



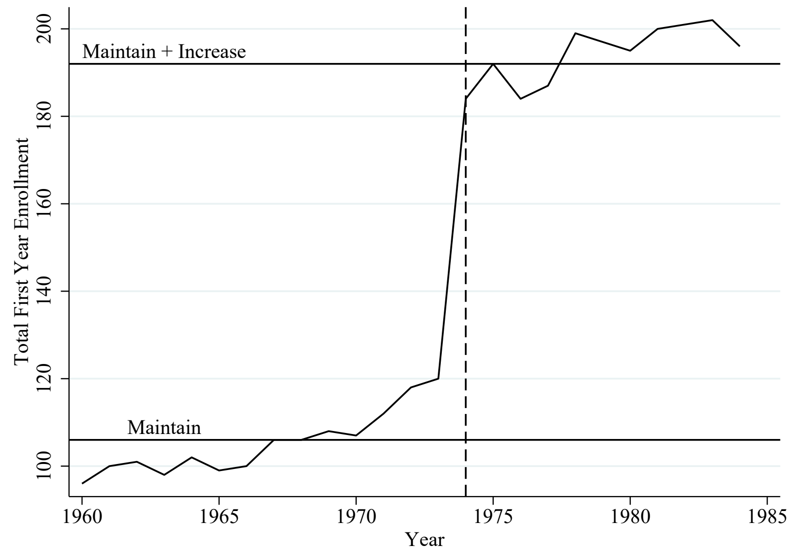
This figure gives a visual description of variable availability in the *Journal of the American Medical Association* Education Number. The label on the y-axis indicates an available variable, and the x-axis indicates a particular year. If a variable is available in a given year, the square at that point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

Appendix Figure 3: *Journal of Medical Education*

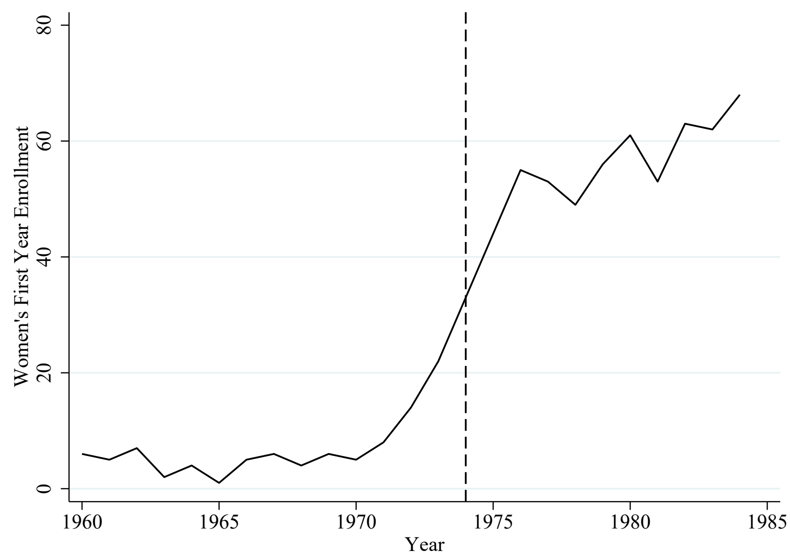


This figure gives a visual description of variable availability in the *Journal of Medical Education* Study of Applicants. The label on the y-axis indicates an available variable, and the x-axis indicates a particular year. If a variable is available in a given year, the square at that point is filled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

Appendix Figure 4: University of Cincinnati First-Year Enrollment, 1960-1980



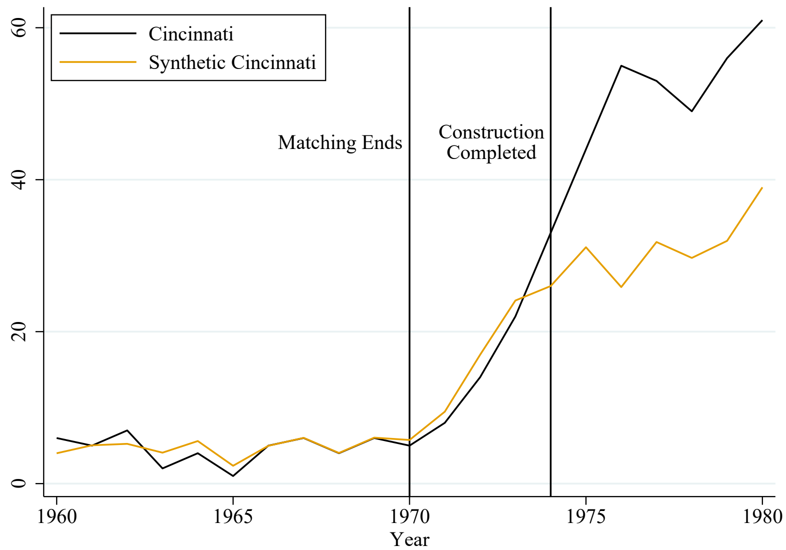
(a) Total First-Year Enrollment



(b) Women's First-Year Enrollment

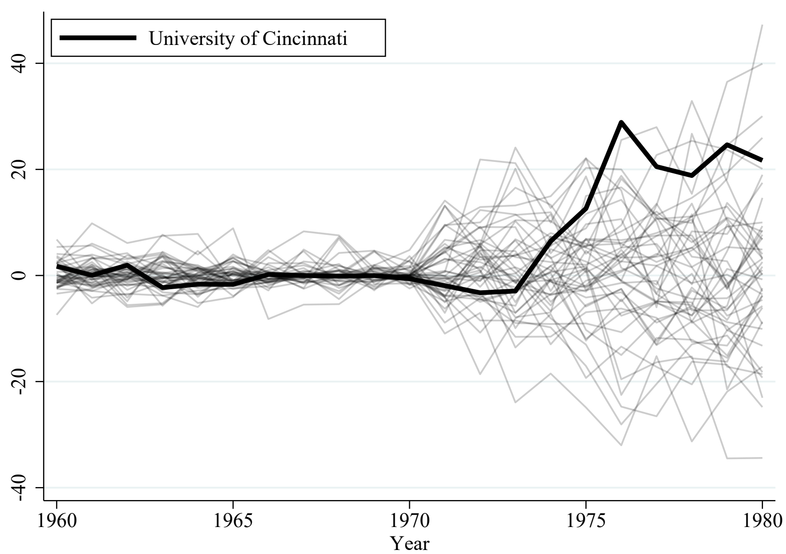
These figures plot the time series of total (Figure 4a) and women's first-year enrollment (Figure 4b) at the University of Cincinnati medical school from 1960 through 1980. The vertical dashed line at 1974 indicates completion of construction of a new basic science building. This building was funded 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).

Appendix Figure 5: Synthetic Control And Observed Enrollment



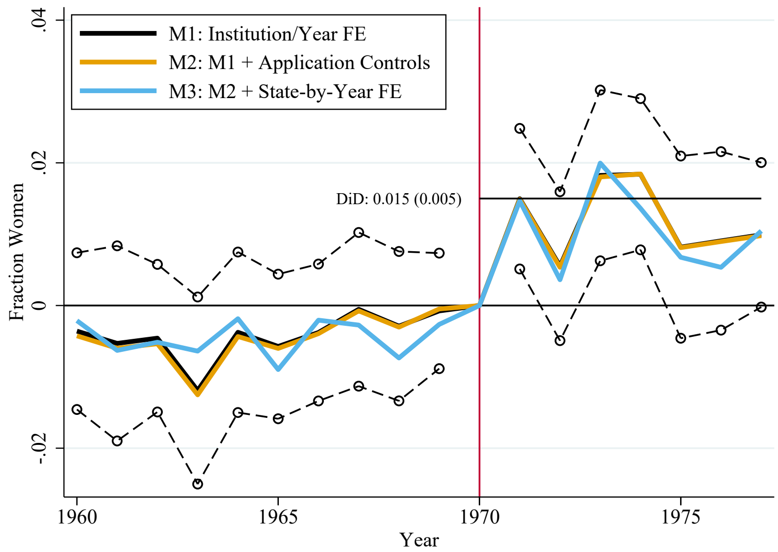
This figure plots women's first-year enrollment for the University of Cincinnati against the same time series for my synthetic control. This is constructed by taking a weighted average of women's first-year enrollment at other medical schools, where weights are given in Appendix Table 2

Appendix Figure 6: Placebo Test

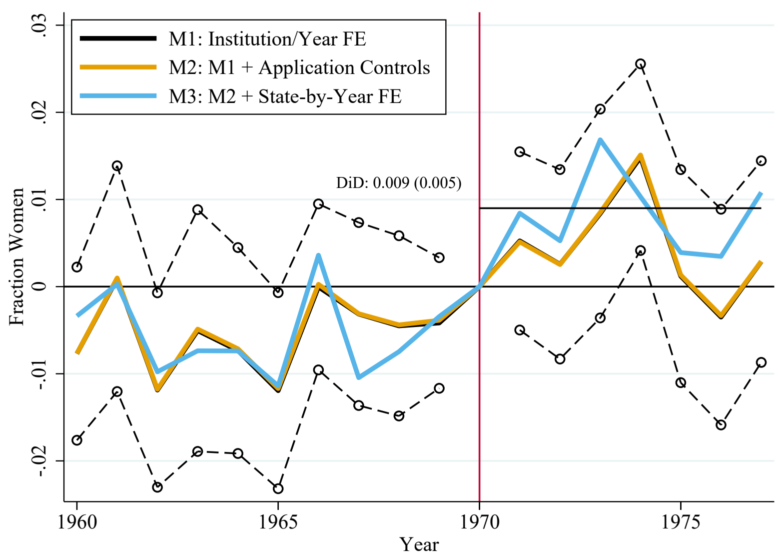


This figure plots the results of the placebo test outline in Abadie et al. (2010). Each series here plots the estimated treatment effect for each unit in my donor pool, as well as Cincinnati, which is in bold. This is calculated by constructing a synthetic control for each unit and taking the difference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

Appendix Figure 7: Alternate Dose Measure



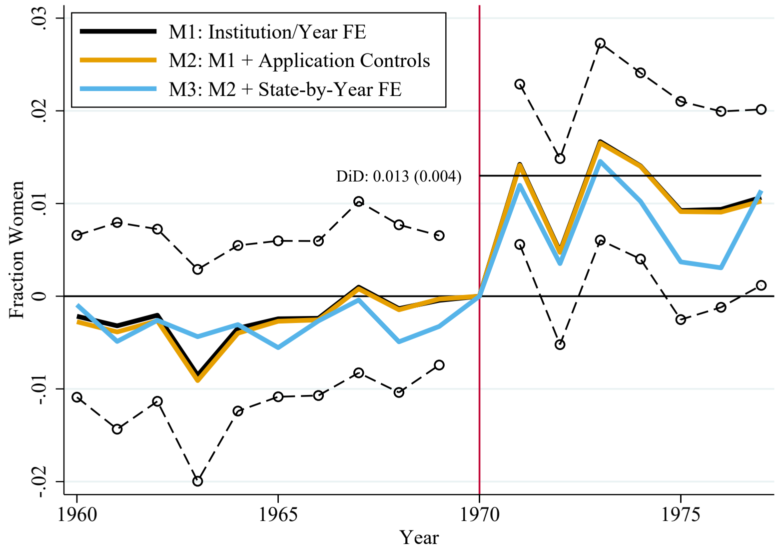
(a) Log Total Adjusted Funding



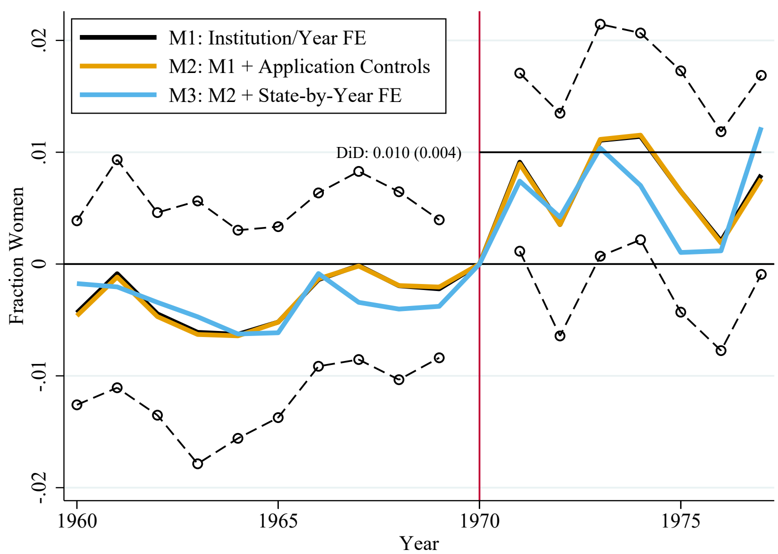
(b) Log Total Adjusted Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment. The measure of exposure to federal anti-discrimination policy is the log of the total amount of HEW support received in 1969, less any support for teaching facilities Figure 7a and the log of total amount of HEW support received in 1969, less any support for teaching facilities, per faculty member in Figure 7b. Model 1 includes only institution and year fixed effects. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 8: Adding Tuition Controls



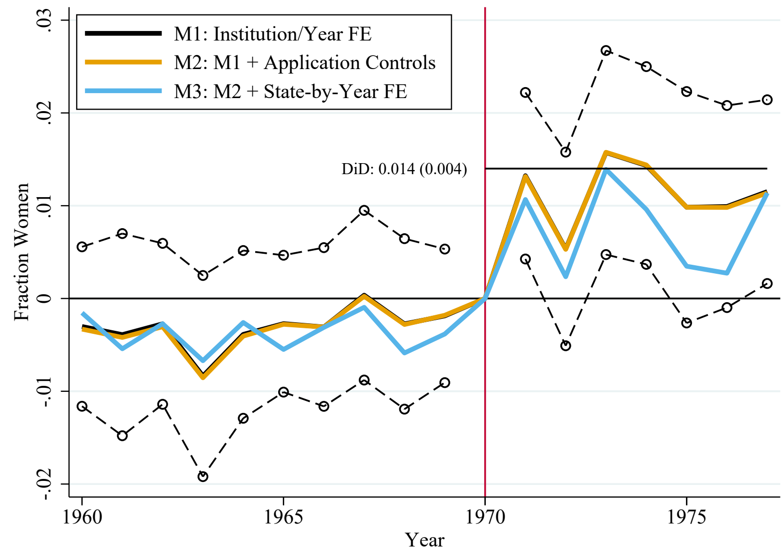
(a) Log Research Funding



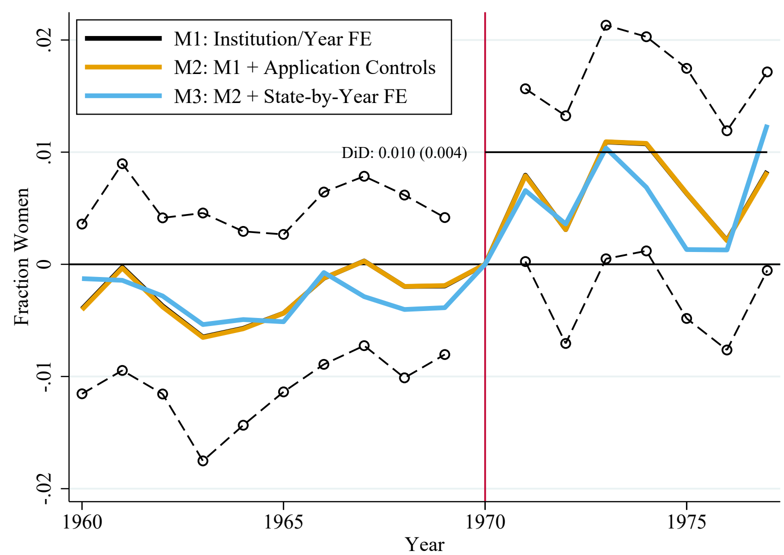
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 8a and the log of total research funding per faculty member in Figure 8b. Model 1 includes only institution and year fixed effects as well as controls for the log of resident tuition and non-resident tuition. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 9: Control-by-Year and Type-by-Year Fixed Effects



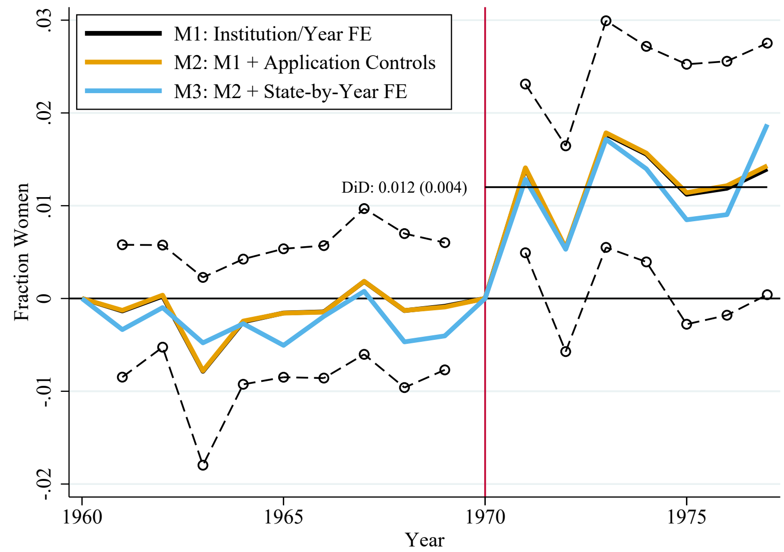
(a) Log Research Funding



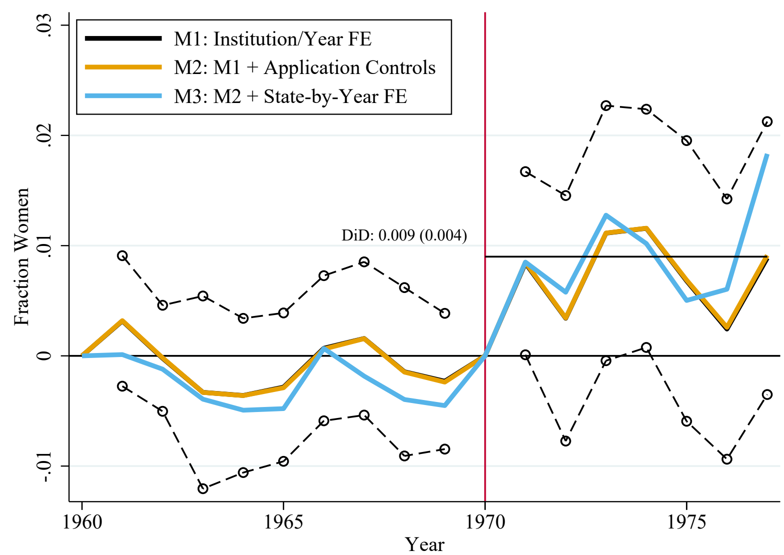
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 9a and the log of total research funding per faculty member in Figure 9b. Model 1 includes only institution and year fixed effects as well as control-by-year and type-by-year fixed effects. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 10: Unit-Specific Linear Trends



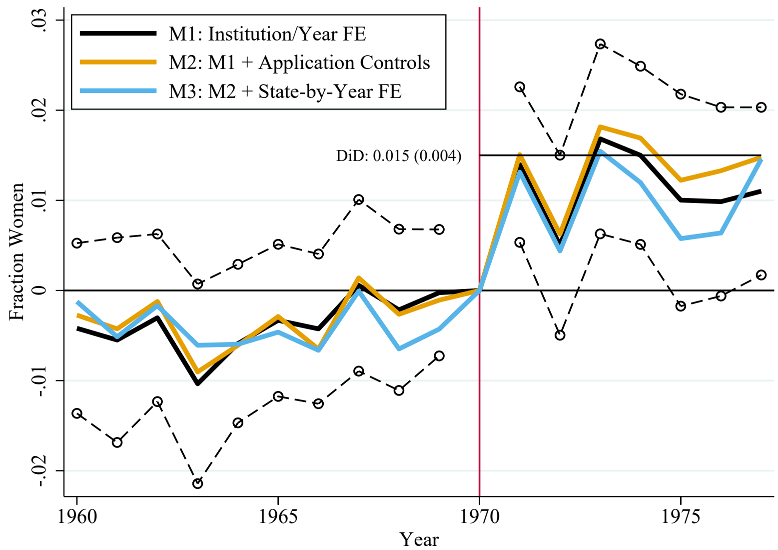
(a) Log Research Funding



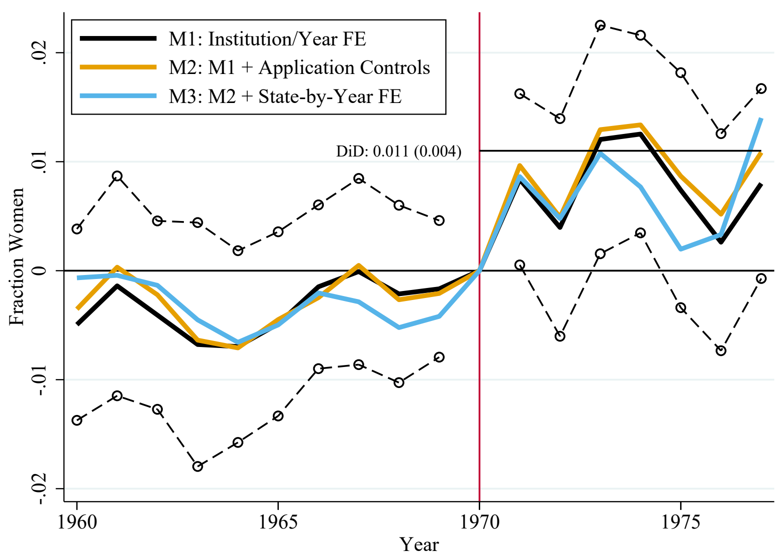
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment. Note that the 1960 event coefficient is dropped to identify event coefficients relative to the estimated trend. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 10a and the log of total research funding per faculty member in Figure 10b. Model 1 includes only institution and year fixed effects as well as program-specific linear trends. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 11: Controlling for Women's Applications



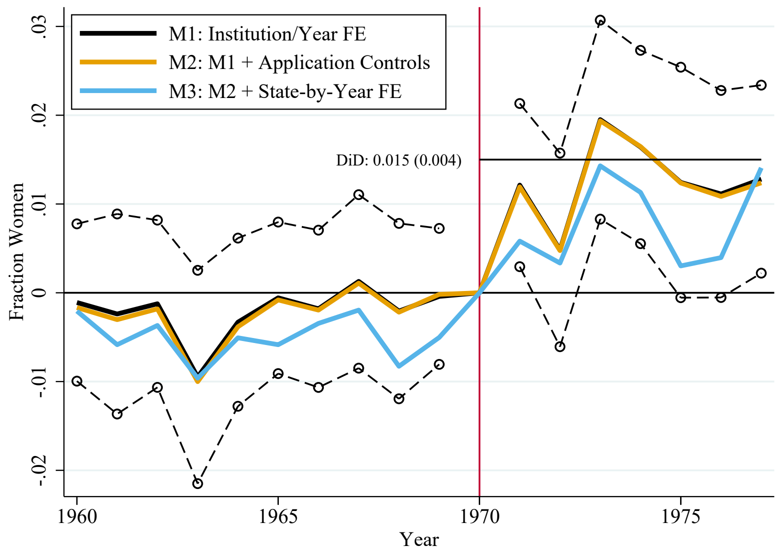
(a) Log Research Funding



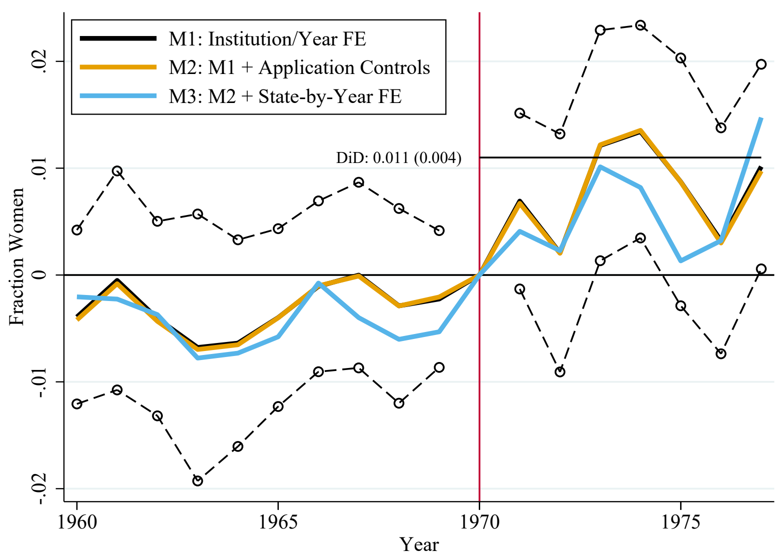
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 11a and the log of total research funding per faculty member in Figure 11b. Model 1 includes only institution and year fixed effects as well as a control for the log number of women's applications. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 12: Balanced Panel



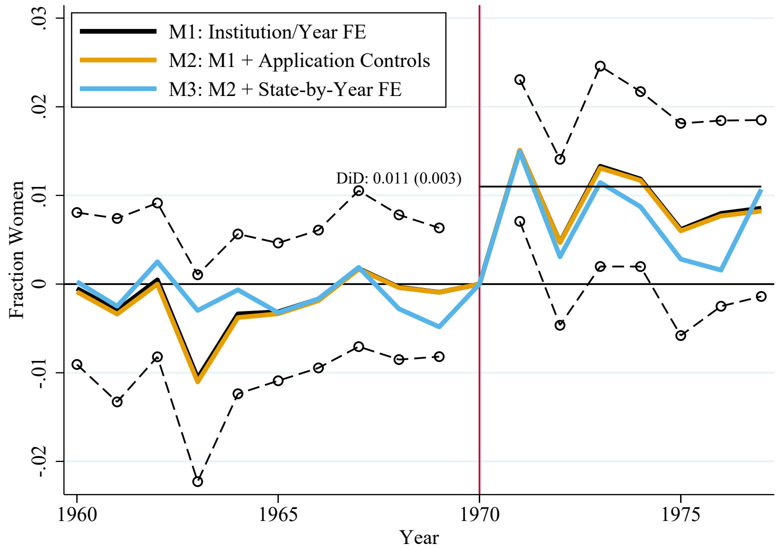
(a) Log Research Funding



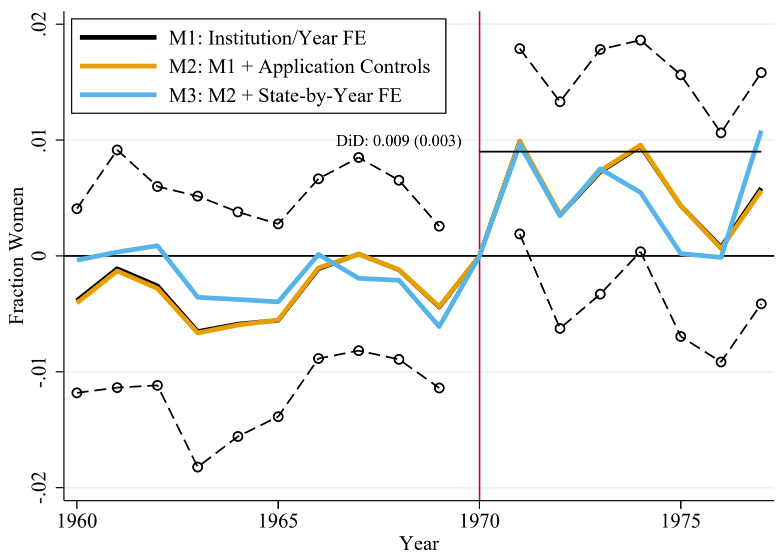
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment. My sample is restricted to a balanced panel, where medical schools are only included if they report positive first-year enrollment beginning in 1960. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 12a and the log of total research funding per faculty member in Figure 12b. Model 1 includes only institution and year fixed effects. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 13: Unweighted



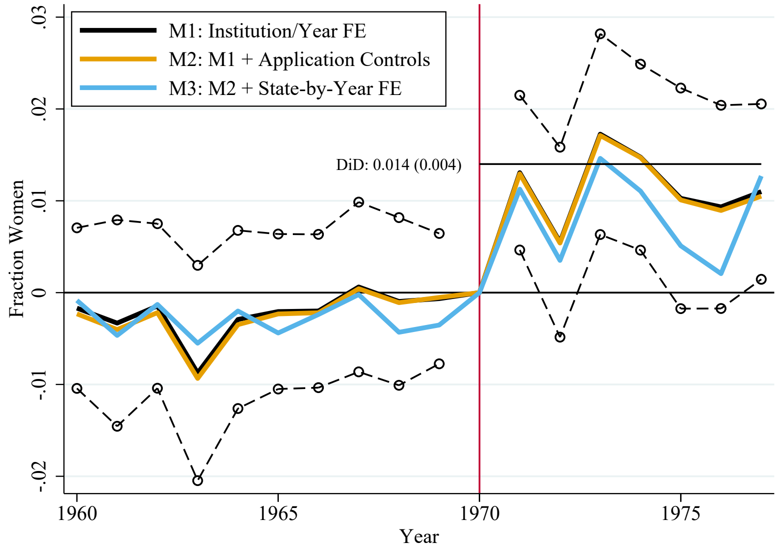
(a) Log Research Funding



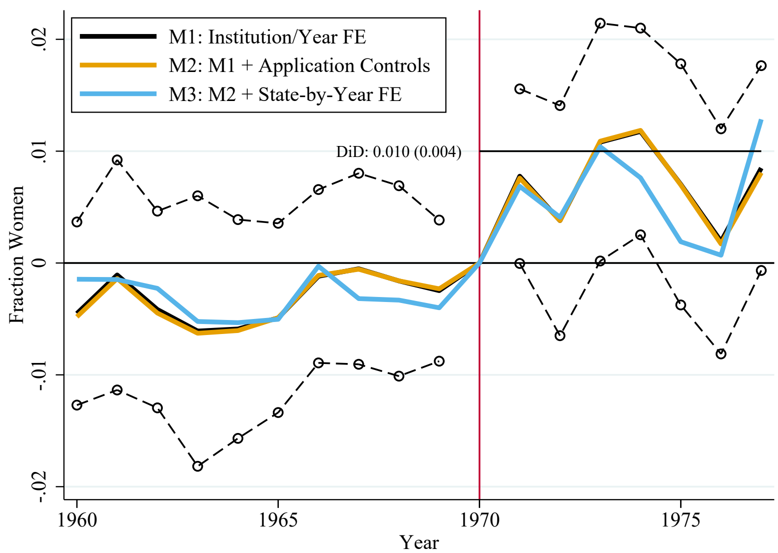
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 13a and the log of total research funding per faculty member in Figure 13b. Model 1 includes only institution and year fixed effects. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 14: Weights Fixed At 1969 First-Year Enrollment



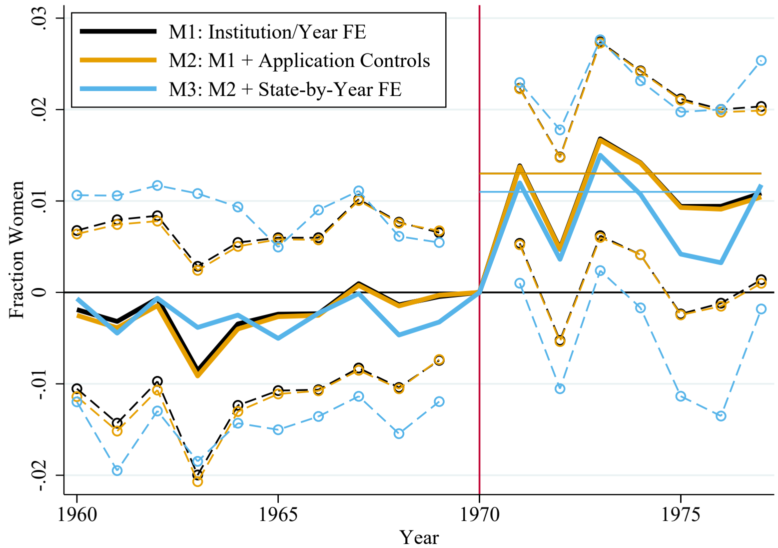
(a) Log Research Funding



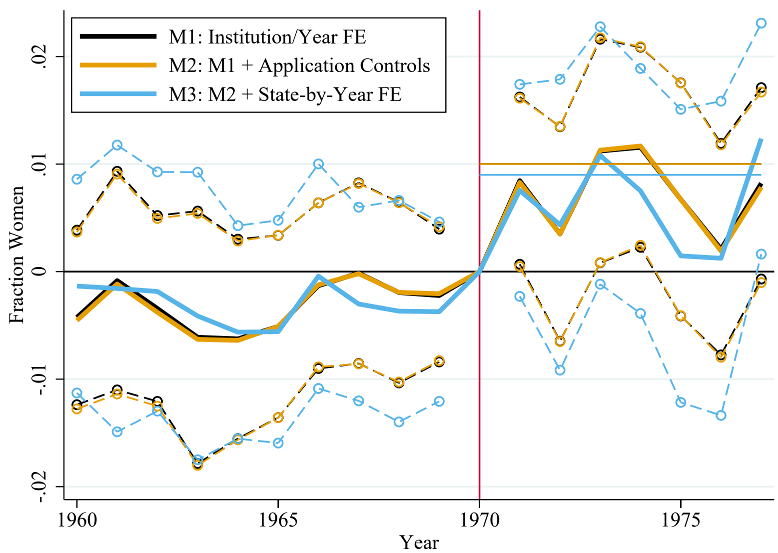
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment in 1969. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 14a and the log of total research funding per faculty member in Figure 14b. Model 1 includes only institution and year fixed effects. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 15: All Standard Errors Shown (School Clusters)



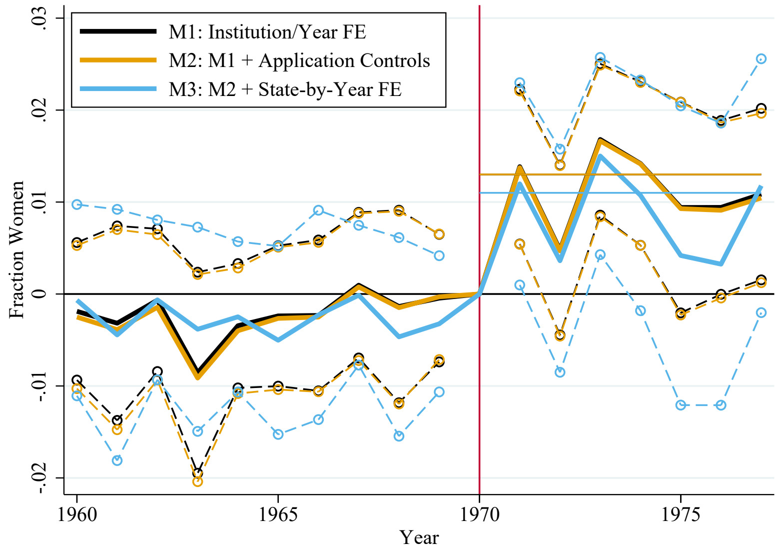
(a) Log Research Funding



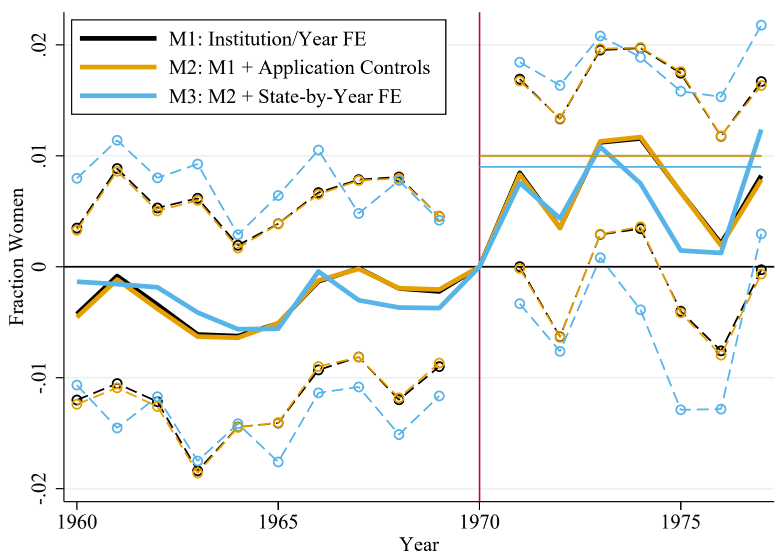
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 15a and the log of total research funding per faculty member in Figure 15b. Model 1 includes only institution and year fixed effects. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for models 1, 2 & 3, where standard errors are clustered at the institution level. Additionally, I plot the difference-in-differences estimate for all models. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 16: All Standard Errors Shown (State Clusters)



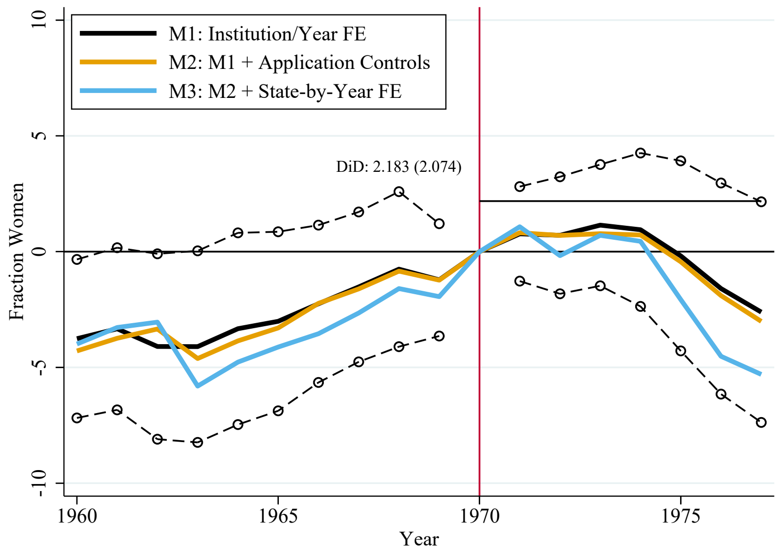
(a) Log Research Funding



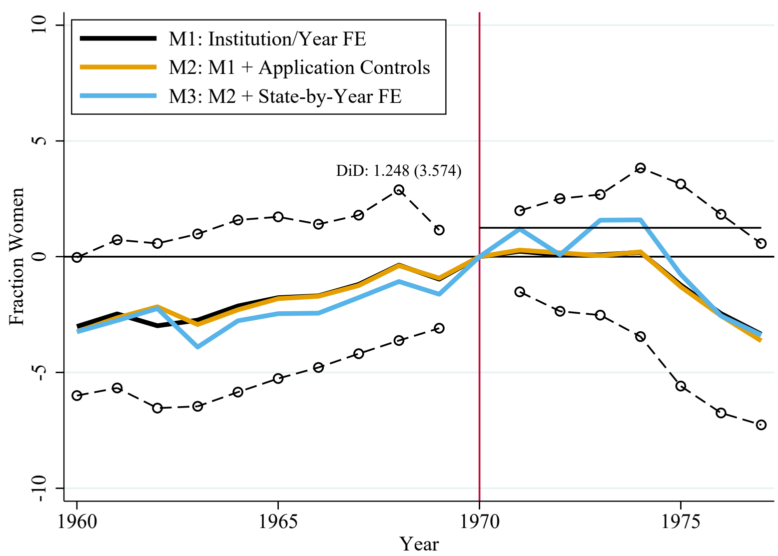
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is the fraction of first-year enrollees who are women and the regression is weighted by total enrollment. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 16a and the log of total research funding per faculty member in Figure 16b. Model 1 includes only institution and year fixed effects. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for models 1, 2 & 3, where standard errors are clustered at the state level. Additionally, I plot the difference-in-differences estimate for all models. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 17: Placebo Test: Total Enrollment As Outcome



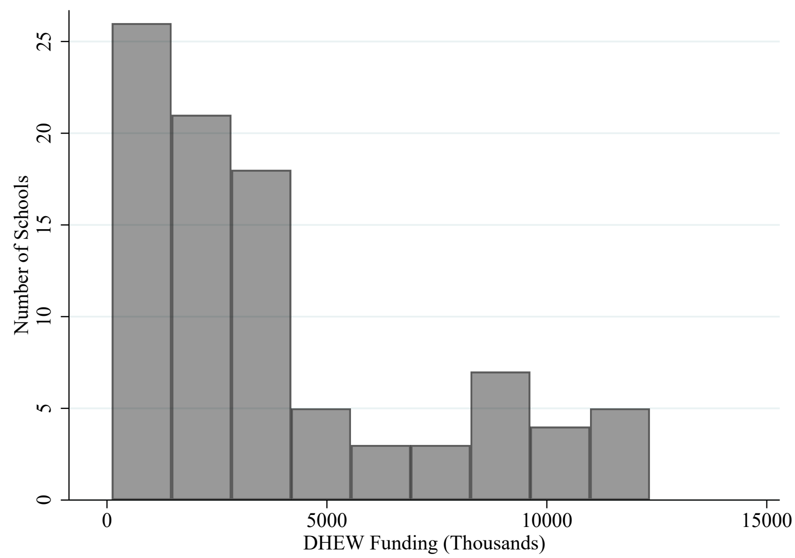
(a) Log Research Funding



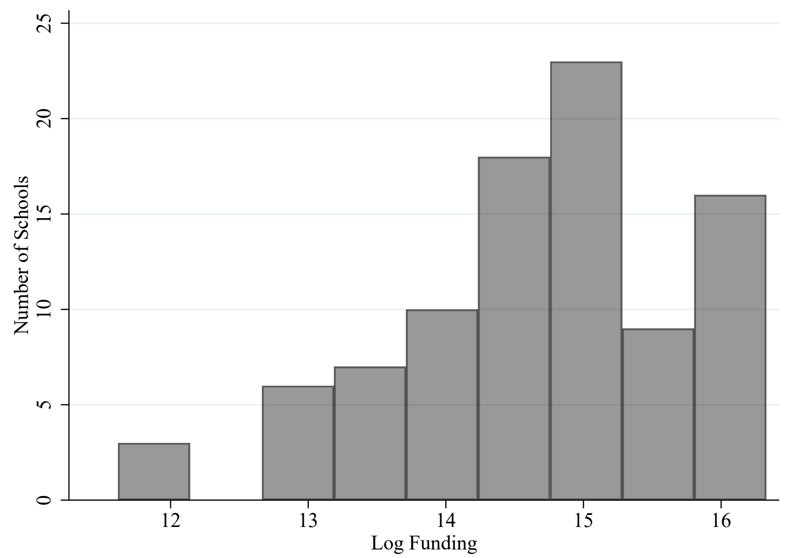
(b) Log Research Funding per Faculty Member

I plot the event study coefficients from equation (1) scaled by the standard deviation of the dose distribution, where the outcome is total first-year enrollment. The measure of exposure to federal anti-discrimination policy is the log of total research funding received from HEW in Figure 17a and the log of total research funding per faculty member in Figure 17b. Model 1 includes only institution and year fixed effects. Model 2 adds a control for the log number of men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 1, where standard errors are clustered at the institution level. Additionally, I report the difference-in-differences estimate and standard error from equation (2) for model 1. Estimates end in 1977 as application data are not available after this year.

Appendix Figure 18: Distribution of HEW Dose Variable



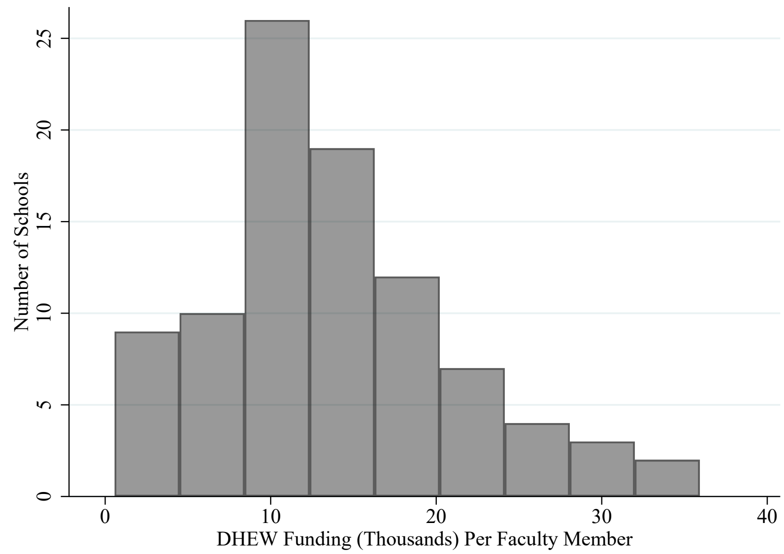
(a) Distribution of Total RD Funding



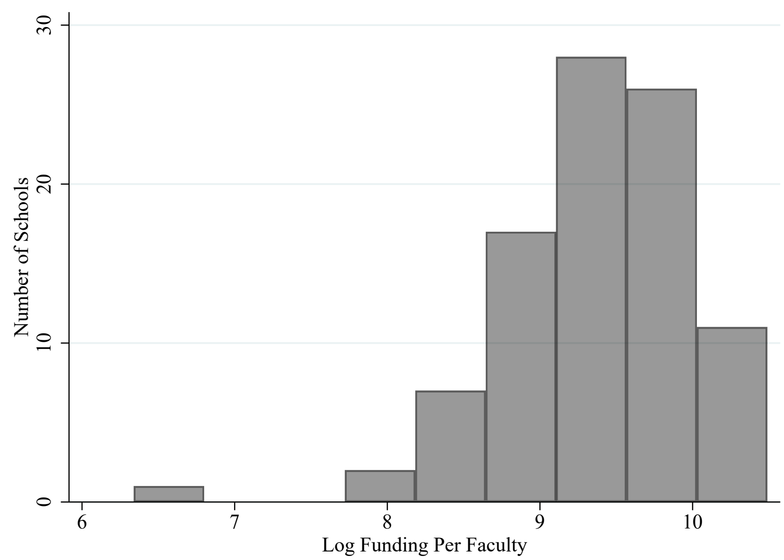
(b) Distribution of Log Total RD Funding

I plot a histogram of the distribution of my dose variable, which is the amount of total HEW research funding provided to a school in 1969. Figure 18a gives the raw distribution, while Figure 18b gives the log-transformed distribution.

Appendix Figure 19: Distribution of HEW Dose Per Faculty Variable



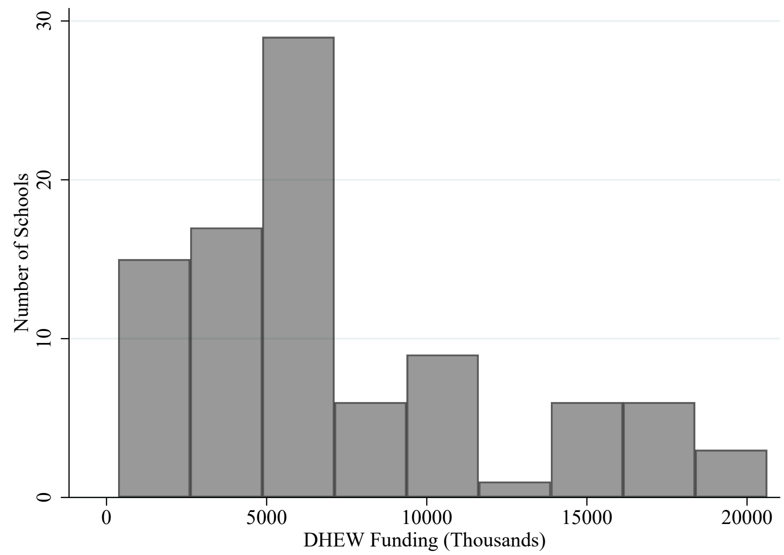
(a) Distribution of Total RD Funding Per Faculty Member



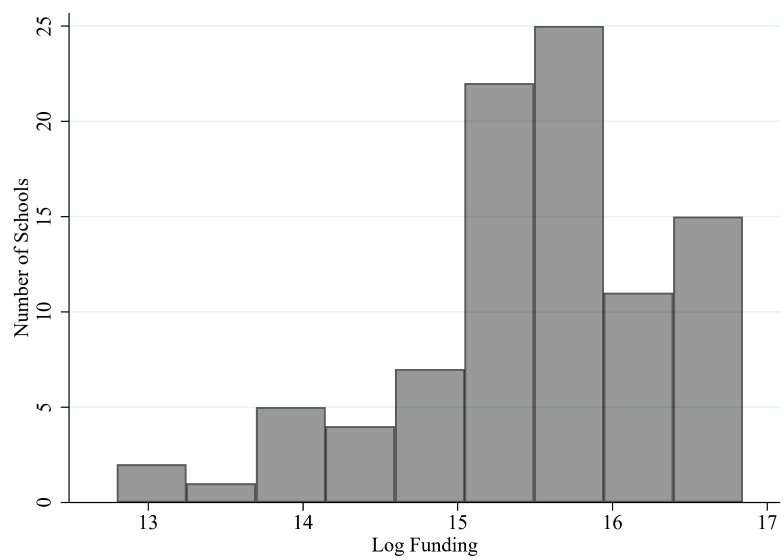
(b) Distribution of Log Total RD Funding Per Faculty Member

I plot a histogram of the distribution of my dose variable adjusted for program size, which is the amount of total HEW research funding provided to a school in 1969 per faculty member. Figure 18a gives the raw distribution, while Figure 18b gives the log-transformed distribution.

Appendix Figure 20: Distribution of Alternative HEW Dose Variable



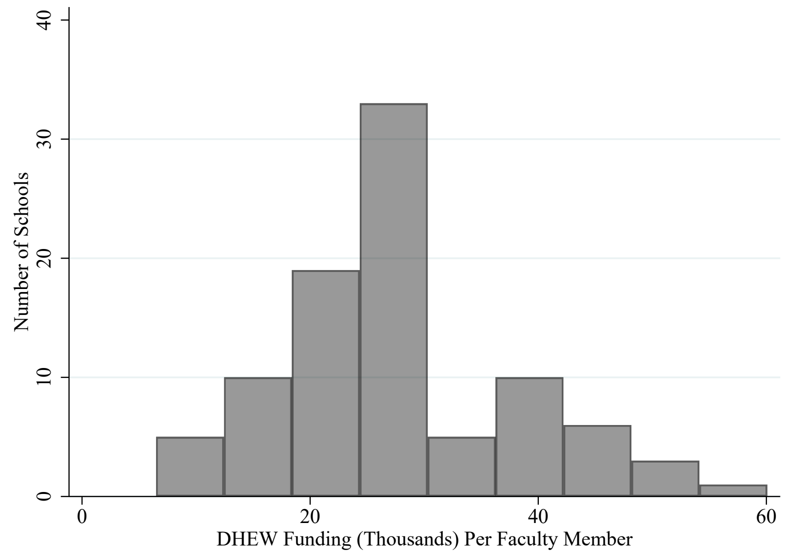
(a) Distribution of Total RD Funding



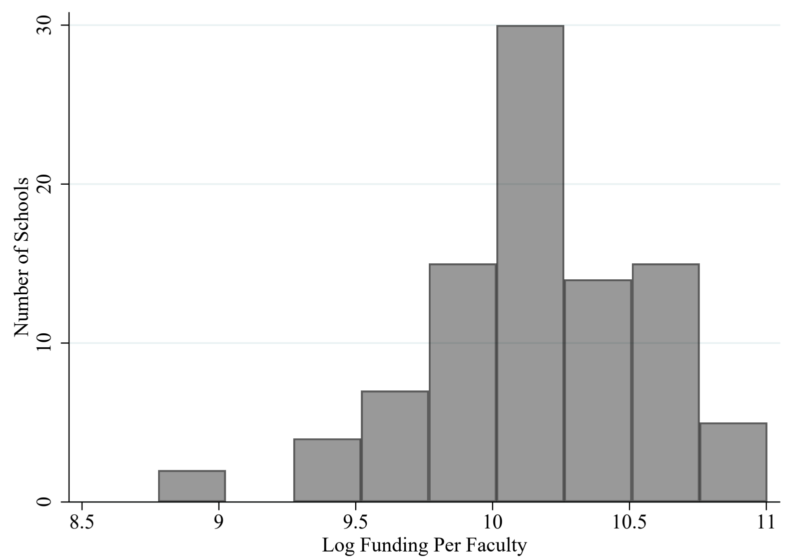
(b) Distribution of Log Total RD Funding

I plot a histogram of the distribution of my alternative dose variable, which is the amount of total HEW funding provided to a school in 1969 less the amount designated for teaching facilities. Figure 20a gives the raw distribution, while Figure 20b gives the log-transformed distribution.

Appendix Figure 21: Distribution of Alternative HEW Dose Per Faculty Variable



(a) Distribution of Total RD Funding Per Faculty Member



(b) Distribution of Log Total RD Funding Per Faculty Member

I plot a histogram of the distribution of my alternative dose variable adjusted for program size, which is the amount of total HEW funding provided to a school in 1969, less the amount designated for teaching facilities, per faculty member. Figure 21a gives the raw distribution, while Figure 21b gives the log-transformed distribution.

Appendix Table 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 Appendix Figures 2 and 3 for data availability. Standard errors are clustered at the institution level to correct for institution-specific errors in reporting.
*** $p < .01$, ** $p < .05$, * $p < .10$

Appendix Table 2: 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.

Appendix Table 3: 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	(9) Below Median	(10) Above Median
First-Year Entrants										
Log Research Funding	0.013***	0.016***	0.012**	0.016***	0.019*	0.002	0.009	0.022**	0.002	0.013*
	(0.004)	(0.006)	(0.005)	(0.005)	(0.011)	(0.004)	(0.009)	(0.009)	(0.006)	(0.007)
Log Research Funding Per Faculty	0.010***	0.015***	0.007	0.012***	0.007	0.002	0.010	0.024***	0.003	0.004
	(0.004)	(0.005)	(0.004)	(0.004)	(0.016)	(0.003)	(0.008)	(0.007)	(0.004)	(0.009)
Observations	1580	819	714	1255	403	405	550	222	735	845
Graduates										
Log Research Funding	0.011***	0.016***	0.008	0.014***	0.018	0.001	0.004	0.020**	-0.003	0.015
	(0.004)	(0.006)	(0.005)	(0.005)	(0.013)	(0.004)	(0.009)	(0.007)	(0.005)	(0.010)
Log Research Funding Per Faculty	0.008**	0.014**	0.004	0.009**	0.001	0.002	0.005	0.021***	0.001	0.003
	(0.003)	(0.006)	(0.004)	(0.004)	(0.018)	(0.003)	(0.007)	(0.007)	(0.003)	(0.009)
Observations	1570	803	714	1242	400	393	552	225	741	829
Enrollment-to-Application Ratio										
Log Research Funding	0.009**	0.013**	0.009*	0.012**	0.010**	-0.001	0.021**	0.015	0.001	0.005
	(0.004)	(0.006)	(0.005)	(0.005)	(0.004)	(0.003)	(0.009)	(0.009)	(0.007)	(0.004)
Log Research Funding Per Faculty	0.008**	0.015***	0.006	0.010**	0.006	0.001	0.020**	0.014	0.002	0.004
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.002)	(0.009)	(0.008)	(0.005)	(0.005)
Observations	1579	819	713	1255	402	405	550	222	734	845

This table reports transformed difference-in-differences estimates from equation (2). For section 1, the outcome variable is the fraction of first-year entrants who are women; for section 2, the outcome variables is the fraction of graduates who are women, where the data are lagged by 3 years as discussed in the text; in section 3, the outcome variable is the difference between the enrollment-to-application ratio for women and men. All specifications include institution and year fixed effects. For each row of coefficient estimates, the row title indicates the dose used. The column header indicates which subsample this design is estimated over. Column 1 reports results for all schools; columns 2 & 3 report results for public and private institutions, respectively; column 4 reports results for all medical schools affiliated with a university; columns 5, 6, 7, & 8 report results within each of the four Census Divisions; and columns 8 & 9 report results for programs below and above the median perceived quality score collected from Cole and Lipton (1977). All coefficients are scaled by the standard deviation of the dose distribution. All standard errors are clustered at the institution level.

*** $p < .01$, ** $p < .05$, * $p < .10$

Appendix Table 4: 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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