Women in Law and the Draft*

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Abstract

Between 1964 and 1973, women's representation in full-time law school programs rose five-fold, from 3.7% to 20.1%. This paper examines whether Vietnam War draft policy contributed to this increase. In 1968, men enrolled in law school lost eligibility for 2-S student deferments, threatening law schools' tuition revenues and incentivizing schools to admit more women to stabilize enrollment. To test this mechanism, we construct a school-by-year dataset of enrollment counts split by women/men and full-time/part-time status. Using a uniform adoption difference-in-differences design, we find that women's representation rises by 2 percentage points in full-time programs relative to part-time programs (which were far less exposed to draft risk). This effect represents a 45% increase over women's baseline representation of 4.5% in 1967. We further show that, after the draft, law schools more connected to undergraduate institutions transitioning from all-male to coeducational experienced larger increases in women's enrollment. A shift-share analysis indicates that a one-standard-deviation increase in exposure to these coeducation transitions raised women's first-year enrollment shares by 1-2 percentage points, highlighting the joint role of draft policy and expanding educational opportunity in accelerating women's entry into law.

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1 Introduction

Nationwide disruptions due to the manpower needs of conflict often coincide with drastic, though potentially transient, changes in women's labor force attachment. The most well-known example of this in the U.S. is WWII, where increased demand for women's labor led to both short-run and long-run changes in women's labor supply during subsequent decades (Goldin and Olivetti 2013). The source of this demand shock has been contested in the literature. Earlier studies leveraged geography variation in induction intensity (Acemoglu et al. 2004), while later accounts emphasized the role of increased demand from wartime industries for women's labor (Rose 2018; Shatnawi and Fishback 2018). A markedly similar pattern unfolded in graduate and professional education, pictured for law students in Figure 1a. Women's representation grew sixfold from a baseline of around 4% of full-time students in 1940 to around 25% as inductions peaked in the mid-1940s and law schools struggled to stabilize their tuition base as men were drafted. However, following the war, these seats were quickly returned to men, reflecting the displacement of women. During the Korean War, this trend repeated itself, as women experience a temporary boost in enrollment as inductions swell that once again dissipates after U.S. involvement ends (Fossum 1983).

The Vietnam War era presents a striking end to this pattern, as women's enrollment increased rapidly while inductions dwindled and the draft ended in 1973. Like WWII, the Vietnam War caused substantial disruptions to domestic life. A large literature documents the impact of the draft on eligible men's college enrollment (Card and Lemieux 2001), earnings (Angrist 1990; Angrist et al. 2011), childbearing (Bailey and Chyn 2020), and children (Deza and Mezza 2025; Goodman and Isen 2020). Despite this, there has been little work exploring how these changes impacted women's roles in the labor market, perhaps because the sheer number of inductions was far lower than in earlier wars. Yet observers often interpret the abrupt increase in women's enrollment in law schools beginning in the late 1960s (see Figure 1b) as a consequence of the draft (Fossum 1981, 1983; Strebeigh 2009). During the early stages of the Vietnam War, graduate and professional students were broadly shielded from the draft through the student deferment program that developed throughout the 1950s. However, in 1968, many of these protections were removed; if admissions committees responded by increasing women's enrollment to curb draft risk to their tuition revenue, this change could have played a role in dissolving institutional barriers for women. But this explanation would also struggle to explain why women's enrollment did not fall yet again in the aftermath of the conflict. Accordingly, this paper (i) provides the first causal evidence that the Vietnam Draft had a marked impact of women's representation in law schools, and (ii) shows that law schools enroll more women in response to increases in women's demand for legal education in the aftermath of these changes.

Using digitized archival data in conjunction with a difference-in-differences design, we present new evidence that the 1968 change in draft risk for men caused a large increase in women's representation in law schools. We digitize data from the *Review of Legal Education* (RLE) between

1960 and 1975, which provides information on law school enrollment for every currently enrolled cohort split by sex. To study the impact of changes in draft deferment rules, we leverage the fact that many law schools operate part-time programs, which report enrollment data separately in the RLE. Since the formalization of the student deferment program in 1951, part-time students were largely not eligible to be drafted, providing us with a suitable control group. Accordingly, to estimate the causal impact of the student deferment policy change, we employ a uniform adoption difference-in-differences design, comparing the progression of the fraction of first-year law students who are women at full-time programs relative to part-time programs.

Figure 1b plots the raw data that illustrates our core comparison; it shows the fraction of first-year students that are women across all full-time and part-time programs between 1960 and 1975. In the 1960s, part-time programs enroll a consistently larger fraction of women than full-time programs, but there is strong evidence that the trends in both of these series are parallel. In 1968, following the removal of student deferments for law study, we see a sharp increase in the proportion of full-time students that are women, with no visual break in the trend of this same series for part-time students. Encouragingly, even though this trend changes in the 1970s, it does not differ across program type, providing further evidence of parallel trends in this research design. In our formal difference-in-differences design, we find that this policy change caused a 2 percentage point increase in women's representation between 1967 and 1968, representing a large 44% increase over the baseline value of 4.5% and explaining around two-thirds of the increase in women's representation in full-time programs to 7.4%. We explore this result further by characterizing heterogeneity in this response across programs of differing prestige. Using a continuous difference-in-differences design, we estimate the relationship between the causal response in 1968 and the log number of volumes in the law library, our proxy for institutional prestige. Our results suggest a causal response increasing in prestige: top institutions increase women's enrollment share by almost 4%, while we cannot rule out a null response for institutions at the bottom of the volume distribution.

As Figure 1b makes clear, our estimated policy effects are immediate and static, and so this exercise cannot speak to the rapid increase in women's enrollment that occurs later in the 1970s. We argue that this is best explained by a sharp increase in women's demand for legal education that translates into gains in seats after the eradication of informal quotas in the late 1960s. Since changes in gender-specific demand for education are difficult to measure, we construct a shift-share proxy that is plausibly exogenous to the law school's admission decision. We leverage undergraduate institutions that transition from enrolling only men to coeducational instruction between 1968 to 1975. These programs included many Ivy League and elite private colleges; from the perspective of a law school, women's enrollment at these programs reflects a demand shift insofar as they become more competitive candidates for admission. We measure each law school's exposure to these transitions by digitizing the 1963 *Martindale-Hubbell* lawyer directory, which provides information on the undergraduate institution and law school attendance for the near-universe of barred lawyers in 1963. This allows us to measure the proportion of each law school's graduates who attended any undergraduate program, providing a measure of its admissions network that newly admitted women now have access to. We find that a one standard deviation increase in network exposure to coeducation transitions is associated with around a one percentage point increase in the female share of first-year law students, providing evidence that law schools are responsive to increases in women's demand for legal education following 1968.

This project makes several contributions. First, as noted earlier, there has been much work exploring the unintended consequences of the Vietnam Draft. Earlier in the war, the availability of deferments to avoid service distorted draft eligible men's behavior: hardship deferments increased childbearing rates (Bailey and Chyn 2020), while student deferments improved men's educational attainment through college attendance (Card and Lemieux 2001). Later, this patchwork system was replaced in favor of a draft lottery. Angrist (1990) leverages variation in induction risk based on date of birth, finding that a higher risk of inductions was associated with lower earnings in the short to medium run, with convergence in earnings between veteran and non-veteran earnings in the long run (Angrist et al. 2011). Having an earlier lottery number also had intergenerational impacts: children of fathers with higher induction risk had lower earnings (Goodman and Isen 2020) and engaged in more risky behaviors (Deza and Mezza 2025). Our study is one of the first to look at the impact of the Vietnam draft on women's outcomes, though our results differ markedly from studies on WWII, which found that this conflict decreased women's educational attainment (Jaworski 2014).

We also contribute to the literature on the causes of the sharp increase in women's enrollment in graduate and professional schools in the 1970s. To date, most causal evidence on the role of institutional change in women's access to professional education has focused on either federal anti-discrimination policy, including Title VII of the Civil Rights Act (Beller 1983), Title IX (Rim 2021), and Executive Order 11246 (Helgerman 2025), or the role of the pill in improving women's control over the timing of family formation (Goldin and Katz 2002). Our analysis broadens this literature by highlighting the indirect impact of a government policy not directly tied to higher education. Finally, we contribute to a long literature on the relationship between the demand for legal services and women's entry into law (Katz et al. 2023; Yoon 2017).

Section 2 provides background information on the operation of student deferments during the Vietnam Draft. Section 3 provides an overview of the data used and the details of our empirical design. Section 4 presents results from this design, a battery of robustness checks, and further evidence on heterogeneity on the basis of prestige. Section 5 describes our empirical design leveraging coeducation transitions and presents results from this design. Section 6 concludes.

2 Background

2.1 Law School Admission and Women in Law Schools

Scholars have long examined the entry of women into the legal profession and law schools (Drachman 1998; Lanctot 2020), and a small but growing empirical literature has begun to quantify these trends (Fossum 1983; Katz et al. 2023; Kuehn and Santacroce 2022). Researchers typically divide women's participation into three broad eras: the trailblazing years (1869–1920), a long stagnation (1920–1970), and the period of real progress (1970–2020) (Katz et al. 2023).

During the trailblazing period, women first entered law schools in the late 1860s, with the Midwest leading the way. Belle Mansfield became the first woman admitted to the bar in 1869 in Iowa, and that same year law schools in Illinois and Missouri enrolled their first women students, followed shortly by Michigan (Drachman 1998; Morello 1986; Tokarz 1990). Ada Kepley earned the first woman's law degree in 1870 at what is now Northwestern. Coeducational traditions in the Midwest facilitated these early openings, whereas elite East Coast schools largely remained closed to women—with rare exceptions such as Howard and Boston University (Drachman 1998; Eckhaus 1991; Morello 1986). Despite these precedents, women's enrollment remained extremely low, rising briefly during World War I as male attendance declined but returning to roughly 3% of all law students by 1920 (Drachman 1998).

From 1920 to 1970, women's representation stagnated. Harvard's decision to admit women in 1950 marked an important symbolic milestone, but one that did not immediately generate broader gains. Women remained a small minority of law students and were concentrated in less prestigious institutions. Previous scholarship has pointed towards informal soft quotas to limit the proportion of women in any incoming class (Barnes 1976; Drachman 1998; Lanctot 2020; Morello 1986). As seen in our data, and in previous scholarship, women's share of enrollment rose modestly during World War II and the Korean War, as men left for military service, only to fall when they returned (Abel 1989; Drachman 1998). Meaningful expansion did not begin until the late 1960s. The Vietnam War draft substantially reduced male enrollment in graduate and professional schools, which we will argue, prompted law schools to admit more women. At the same time, the legal and policy environment began to change: Title VII of the Civil Rights Act of 1964 prohibited employment discrimination on the basis of sex, and in 1967 President Johnson extended those prohibitions to federal contractors covering most universities through Executive Order 11375 (Lanctot 2020). The following decades brought far more rapid progress. Federal investigations of university hiring and admissions practices, together with Title IX of the Higher Education Act of 1972, directly challenged the exclusion of women from law schools and faculties (Fossum 1983). The Association of American Law Schools (AALS) responded with resolutions condemning sex discrimination and urging greater inclusion (Lanctot 2020). While less precisely identified than our main empirical results, we explore the potential effects of these policy changes in Section 4.3, but find little evidence of their impacts. By the end of the twentieth century, women accounted for roughly 48 percent of all law students (Katz et al. 2023), and they now comprise a slight majority in most entering classes.

2.2 Vietnam Draft

The draft that would persist through the Vietnam era began with the passage of the Selective Training and Service Act of 1940. Though this would serve as the framework for the draft conducted during World War II (WWII), it was passed before U.S. involvement in the war in response to Germany's occupation of France and siege of England (Flynn 1993, p. 17).[1] Though the details have changed, the basic structure of the draft remained constant throughout its active operation through 1973. Following their 18th birthday, men were required to register for the draft.[2] The draft had a decentralized structure, where an individual registered by reporting to a local draft board comprised of volunteers who lived in their community. The board would provide a classification of draft eligibility for each man. The classification system changed substantially over the course of the draft, but each possibility broadly fit into one of four categories. Men who were fit and available to be inducted were designated Class 1. Men with a deferment due to their occupation were designated Class 2, which included deferments for both graduate and undergraduate education. Men with a deferment due to family responsibilities were designated Class 3, including at times both married men and fathers. Men deemed unfit to serve were designated Class 4, such as those who did not pass a physical examination (US Selective Service System 2022).

When it began, the draft system largely did not provide deferments for college education. Upon passage in 1940, the Act provided students with a deferment until the end of the current academic year, but these men would be eligible as of July 1, 1941 (PL 76-783 1940). Some protections arose during WWII, especially for health professional schools, but students were widely subject to the draft during this time period and college enrollment fell drastically, with graduate enrollment falling by 30 to 40 percent by the Fall of 1943 (Flynn 1993, p. 78). After a brief hiatus in 1947, the draft returned after WWII as an effort to improve national readiness to respond to the Soviet Union in case of a future conflict; in addition, these concerns also prompted calls to provide draft protection for scientific manpower to develop technological capabilities for war (Flynn 1988). This marked the beginnings of a fundamental shift of the draft apparatus from the egalitarian ideals promulgated during WWII to a more "scientific," selective approach, providing deferments to serve economic and strategic interests. As draft calls escalated with U.S. involvement in the Korean War, President Truman introduced the student deferment system with Executive Order 10230 on March 31, 1951.

The initial structure of this system largely reflected the recommendations of the Trytten Com-

[1] Flynn (1993) notes the role of a lobbyist group led by Grenville Clark, motivated by the belief that "it was the moral responsibility of America to help preserve English civilization from a new barbarism" (Flynn 1993, p. 11).

[2] At inception, men were eligible for the draft between the ages of 21 and 36 (PL 76-783 1940), but by the 1960s, the range of eligibility was 18 to 26 (Bailey and Chyn 2020).

mittee, initially formed in 1948 by the director of the Selective Service System, General Lewis B. Hershey, to devise a principled method for selective deferment.[3] A consensus had emerged that student deferments ought be selective, not universally applicable to all students (Flynn 1988), yet the Committee was doubtful of its ability to select "essential" fields of study, with Trytten observing that "in retrospect it seems clear that such a decision in the past would probably have been in substantial error" (Trytten 1952, p. 33). Instead, the Committee recommended providing deferments on the basis of performance on the Selective Service College Qualification Test (SSCQT), as well as class standing.[4] These deferments were only available to students studying full time, though local draft boards did have the ability to bend these rules if they thought a particular student's coursework merited an educational deferment (National Manpower Council 1952).

Over the 1950s, and particularly following the end of the war, lobbying from the scientific and educational establishment to protect students from induction intensified, and by the end of the decade, "there existed a system of deferring virtually all students and teachers" (Flynn 1993, p. 151). This would continue into the next decade but begin to break down once the U.S. began to send troops to Vietnam in 1965. As draft calls increased in the 1960s in response to increased manpower needs from the conflict, the Johnson administration needed to increase the pool of men deemed Class 1-A (available for military service) to meet induction goals. Due in part to demographic shifts as well as the ease of obtaining a deferment, men with a Class 2-S (student) deferment had surged from 210,693 in January 1952 to 1,834,240 in December 1965, representing 10.4% of all registered men (Flynn 1993, p. 199). Given its size, there were increased calls to use this pool of untapped registrants to meet manpower needs. These calls were also driven by public dissatisfaction with class and racial bias inherent in the draft system that had emerged, with student deferments as a focal point. After internal deliberations, the Johnson administration delivered a message to congress detailing its plans to revise the draft on March 6, 1967, recommending the elimination of graduate student 2-S deferments, except in health fields (Flynn 1993, p. 202).

These recommendations were solidified into policy with an announcement from the Selective Service System on February 17, 1968, with the exemption of medical and dental students (Blakey 1968). As a result, incoming law students in the fall of 1968 would not be able to claim 2-S deferment. Executive Order 11360, passed earlier in June, 1967, provided that students who had entered in the fall of 1967 and were in their first year of school would be deferred only to the end of that academic year, while students in their second or third year of study would be allowed to graduate. In the lead-up to this change, the educational establishment warned of dire consequences,

[3] Authority for the draft was supplied repeatedly by Congress, in both the Selective Service Act of 1948 and the Universal Military Training and Service Act of 1951. Implementation, however, was left explicitly to presidential determination (Trytten 1952).

[4] The SSCQT was created by the Educational Testing Service (ETS) with the aim of predicting college achievement, where several scores were targeted to match a specific level of performance on the Army General Classification Test (GCT) (Chauncey 1952). To qualify for a deferment, a graduate student would need to either score a 75 on the SSCQT or graduate in the top half of his senior undergraduate class (National Manpower Council 1952).

with Harvard's president Nathan Pusey claiming that his law school's enrollment of 540 would fall by half if 2-S deferments were eliminated (Strebeigh 2009). These pressures lead to leadership suggesting changes to the draft would change the admission strategies used by law schools. As President Pusey explained, "Harvard law might stay full only by compromising on 'quality or something', " and Congressman John Erlenborn suggested that law schools would adopt "a policy of admitting women, the halt, and the lame" (Strebeigh 2009). In 1969, some reporting suggests that at least some policies had changed, with *The Harvard Crimson* writing that "In response to the financial pressures of fixed costs, graduate schools are changing admission policies to favor women." (No writer attributed 1969)

Using aggregate data on law school enrollment, it is not difficult to spot the impact this change had on men in law. We collect men's full-time and part-time enrollment in all years between 1960 and 1975 from the *Review of Legal Education*. We plot aggregate attrition in each year defined as percentage of students in enrolled in year t that are not enrolled in year $t+1$.[5] We observe enrollment in each year of law school, so we are able to calculate attrition between the first and second year as well as the second and third year of law school. These data are plotted in Figure 2.

Figure 2a plots men's attrition in full-time programs, which are impacted greatly by the policy change. The solid black line plots attrition between the first and second year of the program. Recall that first-year students enrolled in the Fall of 1967 could retain their 2-S deferment until the end of the academic year, but would lose this deferment in the following year. We observe a spike in attrition of around 10 percentage points in 1968 for these students, representing a large increase of roughly 50% over baseline attrition of around 20%. The dashed gray line plots attrition between the second and third year of the program. There appears to be a persistent cohort effect of the policy change, as we observe another spike in attrition in 1968 for this same group of students as they move from their second to third year. In addition, note that there does not seem to be any change in attrition for students enrolled in their second year of law school in the fall of 1967, consistent with EO 11360 allowing these students to retain their 2-S deferment until graduation.

Figure 2b plots men's attrition in part-time programs, which do not appear to be impacted at all by the policy change. In the same years where attrition increases drastically for men in full-time programs, we see a slight decrease in attrition in part-time programs. This suggests that part-time programs are a suitable control group that is not impacted by the policy change, which matches the historical record. Starting with the introduction of the student deferment program in 1951, part-time graduate students were not eligible for a 2-S deferment (Flynn 1988), which persisted into the Vietnam era (Flynn 1993). Further, part-time students were generally more able to obtain different deferments that would shield them from the draft, regardless of their 2-S eligibility.

There were several ways to avoid induction apart from enrolling in school. For one, all men

[5]Formally, let men's enrollment in year t be given by M_t. We calculate aggregate attrition as $\frac{M_t - M_{t+1}}{M_t}$. Note that this is not an exact measure of attrition, as it does not match individuals across years, but in general we expect it to proxy this statistic very closely.

were only eligible through age 26, so aging out of the process was an option. Further, married men and fathers could apply for a Class 3-A hardship deferment, claiming that their absence would harm their dependents, whether that be their wife or their children. While husbands without children received many deferments throughout the existence of the draft, they were removed from 3-A eligibility with Executive Order 11241 in August 1965. Nevertheless, having children remained a reliable way to avoid induction through the Vietnam war (Flynn 1993).[6] While anecdotal, there is evidence that these deferments broadly shielded men enrolled in part-time law programs. Consider this passage from a history of Golden Gate College:

> The students in the evening programs were attending part-time and were already vulnerable to the draft. They were, however, generally older than the full-time students and many were over draft age. Also, many were married with dependents and so would qualify for exemption. Some were employed in vital industries. But we know that the new policy would hit the Law School Day Division hard. (Butz et al. 2008)

It seems likely, then, that the brunt of the increased draft risk fell on full-time law programs.

3 Data and Empirical Design

3.1 Review of Legal Education

For each law school, these data are also reported separately for each type of program the school offers. Beginning in 1971, only two types of programs were used: full-time (called daytime between 1971 and 1973) and part-time. However, in earlier years, programs were differentiated by whether or not they were offered in the morning or evening. Fortunately, it is straightforward to identify the morning program that enrolls full-time students, as this is always listed first below the name of the institution. Following this, many schools operate an evening program, as well as many different morning programs, subsuming both a separate part-time program where students take daytime classes, as well as an additional option for students in the evening program to take daytime classes, and potentially other options we are unaware of. As a result, to construct a consistent series on part-time enrollment, we aggregate all students not enrolled in a full-time daytime program into one part-time enrollment category. We present a graphical description of how these categories are grouped in Appendix Figure 2a.

We make only one sample restriction. Many programs report only a handful of students studying part-time, so we drop all school-program-years with fewer than 10 students to ensure that part-time observations in our dataset reflect actual part-time programs.[7] To understand the variation

[6] There is evidence that men responded so strongly to this policy that the fertility rate increased markedly between 1965 and 1970 (Bailey and Chyn 2020).

[7] Schools reporting, say, only one or two students studying part-time likely reflect students in the full-time program who moved to a part-time course of study after matriculation.

we are leveraging, Appendix Figure 2b plots the percentage of schools in each year that have a specific mix of program types. Around 40% of law schools operate both a full-time and part-time program; the remaining schools generally operate only full-time programs. In 1960, there was a substantial number of programs that operated only a part-time program, but these programs had almost entirely vanished by 1975. Note that this can result from part-time only programs adding a full-time program and not attrition: of the 9 ABA-approved schools that were part-time only in 1960, all 9 were present in 1975.

3.2 Empirical Design

We utilize a uniform adoption difference-in-differences design of the form

$$Y_{ipt} = \sum_{\tau=1960,\tau\neq 1967}^{\tau=1975} \alpha_\tau D_p \mathbb{1}(t=\tau) + \gamma_{ip} + \delta_t + \varepsilon_{ipt} \tag{1}$$

The outcome, Y_{ipt}, gives the fraction of all 1L students who are women at institution i in program p in year t. D_p is an indicator for whether program p is full-time, reflecting exposure to the policy shock, which is interacted with a set of year dummies omitting the year before the policy change, 1967. Our parameter of interest, α_τ, captures the difference in the percentage of women enrolled across full-time and part-time programs relative to 1967. If law schools increased female share of their student body in response to increased draft risk following the removal of 2-S deferments for law students, we would expect that $\alpha_{1968} > 0$, and we estimate α_t through 1975 to test for dynamic responses to the policy. Our empirical design is valid under the parallel trends assumption that the proportion of students in full-time programs follows the same trend as the proportion of students in part-time programs in the absence of a policy change. We estimate pre-period coefficients back through 1960 to test whether this assumption was met in the years before the policy change. We include institution-by-program fixed effects, γ_{ip}, to account for time-invariant differences in school's preferences over the gender mix of enrollees, as well as year fixed effects δ_t to account for year-to-year changes in the aggregate proportion of law students who are women.[8]

Our baseline specification includes all institution-program-years to estimate the impact of the change in 2-S status of men in law school across all exposed programs. In addition, we consider two different models to guard against potential alternative explanations for the results that we find. First, since the mix of programs within institution is endogenous, it might be the case that programs offering full-time and part-time programs differ in a way that leads to heterogeneous responses to the policy of interest that are unrelated to program offerings. To rule this out, we

[8]There has been much work improving the validity of difference-in-differences designs in the presence of staggered adaption of treatment (Goodman-Bacon 2021), continuous treatment variation (Callaway et al. 2024), and in situations when the parallel trends assumption holds only conditional on covariates (Sant'Anna and Zhao 2020). Since our design exhibits none of these features, a simple two-way fixed-effect estimator will properly recover the year-specific average treatment effect on the treated.

consider a restricted design that includes only institutions offering both full-time and part-time programs so that treatment and control are comprised of the same set of institutions. This also results in a "matched" sample where treatment and control groups are comprised of the same set of institutions. However, such a comparison is vulnerable to potential spillovers between the treatment and control groups. So, we also implement a separated design where there is no overlap in institutions between the treatment and control group. To do this, we assign all institutions operating a law school with only a full-time program to the treatment group and all part-time programs to the control group.

We cluster standard errors at the level of treatment (Abadie et al. 2022), which corrects for serial correlation within institution-program across time (Bertrand et al. 2004). Since the variance of our outcome depends on the size of the denominator, we weight by the number of students in each program to reduce heteroskedasticity and improve precision. Unfortunately, these econometric concerns are in tension, as weighting by the number of units in a group can actually increase heteroskedasticity in the presence of clustered errors (Dickens 1990; Solon et al. 2015). Since our clustered errors are robust to heteroskedasticity, this is not a concern in the limit, but we consider several alternative approaches to weighting and clustering in our robustness checks in the appendix to ensure that our results are not sensitive to this decision.

To summarize our event study results, we also report estimates from a difference-in-differences design of the form:

$$Y_{ipt} = \alpha_{\text{DiD}} D_p \mathbb{1}(t > 1967) + \gamma_{ip} + \delta_t + \varepsilon_{ipt} \tag{2}$$

Here, α_{DiD} is our coefficient of interest, reporting a weighted average of the event study coefficients from equation (1) to provide an estimate the impact of the policy change across the entire post-period.

4 Results

4.1 Difference-in-Differences Results

Figure 3a plots the estimates from equation (1), and summary difference-in-differences estimates from equation (2) are reported in the first row of Table 1. In the years leading up to the policy of interest, we see slight evidence of a negative pre-trend, suggesting that full-time programs were enrolling fewer women than part-time programs over time, though none of the event coefficients are statistically different from 0. Starting immediately in the fall of 1968, following the change in 2-S deferment status for law students earlier that year, we see a large, discrete jump in the fraction of students at full-time programs that are women, relative to part-time programs. There is an immediate treatment effect of around 2 percentage points, representing a 45% increase in women's representation over a baseline value of 4.4%. In the aggregate, the fraction of all full-time students that are women rises from 4.4% to 7.4%, so increased draft risk can explain around two-thirds of the rise in women's representation in full-time programs. Our difference-in-differences estimate across the first 5 years after the policy change is almost identical, suggesting that these gains are maintained but do not increase in the following years, consistent with attrition returning to its secular trend in the years after 1968. We find that the pattern of these results does not change in either of the alternative comparisons we consider in model 2 and model 3.

The primary threat to identification in this design is that concurrent changes in men's demand for legal education could be driving our results. If admission policies remained unchanged in response to increased draft risk, a resulting drop in men's enrollment would mechanically drive an increase in women's representation, even if women did not acquire increased access to legal education. To rule this out, we consider a secondary design to verify that we are measuring an increase in women's enrollment and not simply a decline in men's enrollment. The empirical design is given by:

$$Y_{ipt} = \sum_{\tau=1960,\tau\neq 1967}^{\tau=1975} \alpha_\tau D_p \mathbb{1}(t=\tau) + \beta E_{ipt} + \gamma_{ip} + \delta_t + \varepsilon_{ipt} \tag{3}$$

The outcome Y_{ipt} is now the number of women enrolled at institution i in program p in year t. We include a control for the total number of students enrolled, E_{ipt}, to adjust for differential enrollment sizes across institutions.[9] Event coefficients and fixed effect remain identical to those included in equation (1), and we summarize the estimates from this design with a similar summary difference-in-difference specification:

$$Y_{ipt} = \alpha_{\text{DiD}} D_p \mathbb{1}(t > 1967) + \beta E_{ipt} + \gamma_{ip} + \delta_t + \varepsilon_{ipt} \tag{4}$$

The results from this exercise are plotted in Figure 3b, and summary difference-in-differences estimates from equation (4) are reported in the second row of Table 1. We find a sharp increase in women's enrollment of 4 seats in 1968, which grows over time reflecting the slight drift upwards of the treatment effect estimated in Figure 3a as well as the 32% increase between 1967 and 1975 in the average size of a full-time program in our dataset. While it is likely that men's enrollment also declined as a result of increased draft risk, these results imply that the drop in men's enrollment was at least partially offset by an immediate increase in women's enrollment.

Of secondary concern is confounding driven by concurrent changes in women's demand for legal education. Any concerning variation would shift women's demand for full-time legal education relative to part-time programs. It is plausible that the introduction of the pill, in giving women better control over the timing of family formation, could lead to a change on this margin, as it is well known to have increased women's attainment of professional degrees (Goldin and Katz 2002). To

[9] Several recent papers have documented issues that may arise when time-varying covariates are included in a TWFE difference-in-differences design (Caetano and Callaway 2024; Karim and Webb 2024). Appendix Figure 5 demonstrates that these results are (largely) robust to an entropy weighting design where, instead, treatment and control groups are balanced on enrollment size in 1967.

adjust for this potential confounder, we consider a design that includes state-by-year fixed effects δ_{st} to absorb changes as states liberalized access to oral contraception in different years. These results, presented in Appendix Figure 6, can safely rule out the role of the pill in driving changes in women's enrollment, as our results are very robust to including state-by-year fixed effects. We also collect aggregate statistics on LSAT administration between the 1966-67 and 1969-70 law school application cycles to test directly for unusual changes in the number of women (or men) and the fraction of students sitting for the exam who are women, as well as changes in the average LSAT score by sex that might indicate a change in the aggregate qualifications of law school applicants. All statistics are detrended using estimates of application cycle and monthly fixed effects, and we use a standard z-test to test for changes in mean LSAT score and outlier detection techniques from Belotti et al. (2024) to test for changes in test taking behavior—these procedures are described in detail, along with our results, in Appendix Section B. We find little evidence to suggest that there is a policy-induced response in women's test taking behavior affecting average LSAT score (Appendix Figure 22), the number of women registered for (Appendix Figure 27) and taking (Appendix Figure 24) the LSAT, the fraction of students registered for (Appendix Figure 29) and taking (Appendix Figure 26) the LSAT who are women, as well as the absence rate (registered but did not attend) for women (Appendix Figure 30).

These results survive a battery of robustness checks, which are described in the Appendix. We show that our results persist across multiple different sample and specification definitions. We find similar results after restricting our sample to ABA Approved programs (Appendix Figure 8), as well as all programs that obtain ABA approval by the end of our sample period (Appendix Figure 9). We also find similar results after restricting our sample to a balanced panel, whether or not that panel is balanced at the institutional level (Appendix Figure 10) or the institution-program level (Appendix Figure 11). We drop institution-program-years that enroll less than 10 students to improve precision; Appendix Figures 12 and 13 show that our results are not sensitive to the size of this restriction and are unaffected by dropping institution-program-years with less than 20 and 50 students, respectively. Appendix Figure 19 shows similar effects after excluding schools in the south, ruling out that these results are driven by north-south differences. Building on the inclusion of state-by-year fixed effects discussed earlier, Appendix Figure 7 presents results with city-by-year fixed effects to rule out any idiosyncratic variation that might be occurring at the this level. This results in significantly more variation, especially in the more restricted models 2 & 3, but the results from model remain essentially unchanged.

We also show that our results do not depend substantively on our choices about weighting, clustering, or alternative ways to assign treatment. As Solon et al. (2015) recommend, we present results for specification (1) without weights in Appendix Figure 14a. The pattern of event coefficients remains the same, but our results are attenuated, as we would expect from not correcting noisier outcomes from smaller programs. As we remove smaller programs from this sample in figures

14b and 14c, our estimated difference-in-differences estimate for model 1 increases markedly, which is also consistent with this hypothesis. To correct for the potential correlation across time that weighting can induce (Dickens 1990), our baseline design clusters standard errors at the institution-program level—these results are also plotted in Appendix Figure 16 with 95% confidence bands plotted for all models. We allow for potential covariance across time within institution (Appendix Figure 17) as well as state (Appendix Figure 18), which does not seem to impact precision much; most importantly, we find a statistically significant 1968 event coefficient across all models and choices of clustering. Finally, Appendix Figure 15 presents an identical set of results where weights are fixed at total enrollment in 1967, and Appendix Figure 20a fixes program mix at its 1967 value to assign treatment status. These exercises allow us to rule out that endogenous changes in either total enrollment or program mix could be driving our results.

In addition to increases in first-year enrollment, we might be interested in whether or not these students persist through later years in the program.[10] We explore these auxiliary outcomes in Figure 4, and difference-in-differences estimates are presented in Sections 2 and 3 of Table 1. Each design uses a similar specification to (1), where the outcome is defined as the fraction of 2L (3L) students who are women, and the regression is weighted by the total 2L (3L) enrollment. Figure 4a, which presents results for second year students, appears odd on first glance—all three models show a slight increase in the fraction of women enrolled in the second year in 1968, one year before we should see a causal effect. This is not a statistical artifact but actually reflects the impact of increased attrition for men between years 1 and 2 we saw in Figure 2a.[11] Then, in the following year (1969), we see a sharp increase in the event coefficient, reflecting women's increased 1L enrollment in 1968 translating into the following year. The difference between the 1969 and 1968 event coefficients [0.025 (0.008)] is slightly higher than the 1968 event coefficient in the 1L design [0.022 (0.005)], suggesting that differential attrition between men and women did not attenuate the policy impact. Figure 4b presents results for third year students. It is a bit more difficult to parse responses across years: the diminished event coefficients relative to Figure 4a suggest that attrition between years 2 and 3 might have mattered, though the difference-in-differences coefficient for model 1 is essentially identical across 1L and 3L results, suggesting that over time any existing differential attrition dissipated.

[10] Ideally, we would like to observe whether or not these students graduate, or even better, pass the bar exam upon graduating. However, bar exam passage rates are not reported at all during sample period, and the number of graduating men and women is only reported starting in the 1970 *Review of Legal Education.* Even persistence to the last year of the program is difficult to measure in a comparable way, since programs can differ in years of length across program type. Accordingly, we opt to use persistence to 3L as our main proxy for graduation.

[11] Appendix Figure 35 replicates Figure 2 for women to confirm that there is no corresponding jump in attrition between years 1 and 2 for women.

4.2 Heterogeneity

The key metric of heterogeneity that we seek to understand is how this shock translated to gains for women across the law school reputation (and thus market power) distribution. Of interest is whether or not this shock allowed women to access highly ranked law schools, or whether their access was limited to institutions with lower prestige. It is easy to imagine that institutional responses could be higher at both the upper and lower end of the reputation distribution. To fix ideas, we develop a simple model of the admission committee's decision in Appendix Section A. We consider an admissions committee selecting which applicants to admit to meet a fixed enrollment constraint. The committee would like to maximize it's average (unidimensional) matriculant credentials less a convex cost term that bites for enrolling a high fraction of women relative to other programs.

Assuming that draft risk is orthogonal to applicant credentials, each program will expect to lose a certain proportion of matriculants that it will need to replace. The degree to which the admissions committee is incentivized to enroll more women depends on the drop in average student credentials it would face by lowering the admissions threshold for men. Accordingly, the magnitude of the institutional response should be determined by the density of men applying who are at the margin for acceptance. If there are many applicants who have credentials at the margin, the admissions committee will not feel much pressure to enroll more women. More precisely, this response will depend on the enrollment elasticity of men's credentials. If marginal credentials decline sharply with increases in men's enrollment, the marginal benefit of enrolling relatively more women must be higher. This implies that we would expect to see a stronger response from more prestigous programs. If we assume that all schools are drawing from students on the high side of a normal LSAT distribution, for example, it follows immediately that this elasticity will be higher for programs with a higher cutoff score.

On the other hand, this result assumes that the reputation cost of enrolling a high fraction of women is homogenous, reflecting a shared (discriminatory) norm. If, instead, we used a taste-based discrimination model, where the utility cost was higher are more prestigious institutions, conditional on having the same drop in applicant credentials, more prestigious programs would enroll relatively fewer women. Put together, the predicted impact would be ambiguous, depending on the strength of each channel.

Constructing a measure of law school reputation in the 1960s is not a trivial task, since modern ranking systems, like U.S. News, did not exist let, let alone more sophisticated revealed preference measures (Rothstein and Yoon 2024). In fact, the idea that the reputation of graduate and professional programs could be measured through faculty surveys was just developing in the mid-1960s, with the 1966 Cartter Report being the first to publish a ranking collected in this manner (Cartter 1966). Unfortunately, these early rankings generally do not include professional programs. Several studies in the 1970s applied this approach to professional schools (including law) but they generally only report a subset of "top" programs, with a cutoff varying across studies (Blau and Margulies

1974; Margulies and Blau 1973). Further, there is a clear endogeneity concern that, as these ranking systems became more common, programs might manipulate the inputs to boost their standing.

Accordingly, to construct a useful measure of program reputation, we leverage the result in Blau and Margulies (1974) that estimated law school reputation is highly correlated ($r = 0.86$) with the number of books in the program's library. This proxy for prestige has several advantages. First and foremost, it is directly observable for almost all programs in our sample. Beginning in 1969, the American Bar Association began an annual survey of law libraries to collect statistical data on their operation, including the number of volumes held by each library. We collect this information from the inaugural Fall 1969 survey (Lewis 1969). In addition, we are able to obtain a measure of prestige very close to our event year of 1968; by contrast, U.S. news only begins publishing a ranking for law schools in 1990 (Rothstein and Yoon 2024).

We test for heterogeneity using a continuous difference-in-differences design, following Callaway et al. (2024). Our outcome of interest remains Y_{ipt}, which gives the percentage of students that are women enrolled in program p in institution i in year t. We consider the change in this variable between 1967 and 1968, given by $\triangle Y_{i\rho}$, to focus on identifying heterogeneity in this response across the continuously distributed log number of volumes in the law library at institution i, given by V_i.[12] Our key causal parameter is the average treatment effect for full-time programs with volumes V, given by

$$ATT(V|V) = \mathbb{E}[Y_{i,\rho=\text{Full},t=1968}(V) - Y_{i,\rho=\text{Full},t=1968}(0)|V_i = V] \tag{5}$$

It is important to emphasize that we are *not* identifying the causal response in response to an increase in institutional prestige.[13] Rather, we are characterizing the way in which the causal response to the draft shock varies across the prestige distribution, as proxied by log volumes.

To identify $ATT(V|V)$ as a continuous function of V, Callaway et al. (2024) recommend estimating the average change in outcomes among an untreated group ($\hat{\mathbb{E}}[\triangle Y_{i\rho}|D_\rho = 0]$), then using the first-difference transformation $\triangle Y_{i\rho} - \hat{\mathbb{E}}[\triangle Y_{i\rho}|D_\rho = 0]$ as the outcome of a flexible regression on V. Given that we have 136 observations, we opt for a parametric regression on a cubic in log volumes, given by

$$\triangle Y_{i\rho} = \alpha + \mathbb{1}\{D_\rho = 1\} \sum_{k=0}^{3} \beta_k V^k + \varepsilon_{i\rho} \tag{6}$$

Our comparison group remains all part-time programs where $D_\rho = 0$, and we estimate the mean response in this group simultaneously to ensure this uncertainty is reflected in our standard errors. We correct for heteroskedasticity by weighting by first year enrollment in 1967 and estimating heteroskediasticity-robust standard errors.

The results of this exercise are plotted in Figure 5a, where we plot point estimates of $ATT(V|V)$ as well as pointwise and uniform confidence bands, where the latter are calculated using the method

[12] We utilize the log number of volumes as the number of volumes has a right skew distribution.

[13] This would correspond to the average causal response parameter in Callaway et al. (2024).

proposed in Montiel Olea and Plagborg-Møller (2019). These results suggest that the causal response is almost piece-wise linearly related to our prestige measure. Institutions around the top half of the log volume distribution increase their share of enrolled women by around 4%; this response then falls as we consider lower prestige programs, and we cannot rule out a non-response by institutions near the bottom of the distribution. The magnitude of the response from prestigious institutions is striking: relative to the baseline proportion of women at 4.5% in 1967, this represents an approximate doubling of representation, affirming that women were able to access highly ranked programs.

We complement this with a more direct measure of institutional selectivity. The first *Pre-Law Handbook*, published during the 1968-69 academic year, reported a variety of statistics to help prospective students with the application process. Among these are the number of applications filed for each school and the number of acceptances issued, both measured in the 1967-68 academic year.[14] We utilize these to construct the applicant-to-acceptance ratio (the inverse of the acceptance rate), which is increasing in measured selectivity. We also opt for a log transformation for this variable to consider variation in percentage changes in the number of students per admission slot across programs. We find strong support for an increasing relationship between selectivity and the response to the draft shock, though this looks far more linear across our measure of selectivity, with substantially less precision among highly selective schools.

Another natural choice for measuring selectivity would be the average LSAT Score or Undergraduate GPA of entering students, but there are several issues with these metrics. For one, the LSAT was not nearly as central to the admissions process as it is today—the vast increase in demand for legal education in the late 1960s and 1970s resulted in higher use of the LSAT by admissions committees (Kidder 2003).[15] In addition, the first report of these statistics (that the authors are aware of) at the program level is in the 1969-70 *Pre-Law Handbook*. Consequently, we can only measure this for the 1968-69 incoming class, where scores may have been affected by the change in draft deferment policy. Further, many schools did not opt to share scores, and for those that did, we only can observe counts within a set of score bins, necessitating an imputation of average score. Accordingly, while we prefer our measures of prestige and selectivity, we also report results for (imputed) average LSAT Score, Undergraduate GPA, and their composite in Appendix Figure 21.[16] Our finding of a positive relationship between selectivity and the ATT is strongly robust to

[14]The preface to the data contained in this handbook provides an interesting piece of evidence for the impact of the draft on law school admissions: "The information included for each school is for the fall term of 1967; however, it is being made available as guidelines in the 1968-69 academic year, primarily for students who will wish to enter law school in the fall term of 1969. Because of the uncertainty of the Selective Service program, largely arising from and the discontinuance of deferments for graduate students, significant changes may have occurred for a given school in the basic size and range of admission criteria. The reader must be somewhat cautious in interpreting those areas of the data relating to number of students, admissions and class sizes."

[15]In fact, as Kidder (2003) notes, survey evidence from the 1960s indicates that admissions committees tended to weight undergraduate GPA more highly than LSAT Score (Lunneborg and Radford 1965).

[16]The composite score is given by $(1/2) * \text{LSAT} + (1/2) * (200 * \text{UPGA})$, inspired by the observation in Lunneborg and Radford (1965) that this transformation of GPA has the same average and spread as the LSAT distribution.

all three of these measures.

4.3 Testing Additional Explanations

To put these results in context and to test if other policy changes have polluted our estimates, we test several alternative explanations for the growth in women's enrollment that occurred in the Vietnam War era. Helgerman (2025) studies the impact of federal anti-discrimination policy on medical schools, emphasizing the role of Executive Order 11246. Goldin and Katz (2002) find that improved state-level access to the pill in the late 1960s is associated with higher rates of graduate education in the following decades. Using similar variation from Bailey (2006), we do not find that law schools located in states that liberalize access earlier increase the fraction of women enrolled at a faster rate than states that move more slowly. Finally, Rim (2021) finds that Title IX, passed in 1972, led to a large increase in women's representation across many graduate and professional programs. However, leveraging differences across institutions in reliance on federal funding, we do not find that law schools affiliated with institutions more reliant on this funding increase the fraction of women enrolled at a faster rate than their less reliant peers.

4.3.1 Access to the Pill

The increased availability of oral contraception has been found to be a factor in the rise of women's enrollment in professional degree programs, including law school. Goldin and Katz (2002) argued that by lowering the risk of unintended pregnancy, the pill enabled young women to delay marriage and reliably plan for "long-duration" educational investments. This control over fertility, gained through technological and legal changes, may have helped to precipitate the surge in female attendance at law schools observed during our study period. This work was reinforced by Bailey (2006), who also used state-level variation in contraceptive access to show that the pill led to significant, broad increases in women's human capital investment and lifetime labor supply.

We follow Bailey (2006) and study the earliest year in which an unmarried, childless woman under the age of 21 could legally obtain medical treatment without a parent or spouse consenting. This was accomplished in two main ways: either by lowering the legal age of majority from 21 to 18 or 19, or by enacting specific laws that carved out exceptions for medical treatment. These specific exceptions included "mature minor" doctrines, which allowed minors to consent if they were deemed mature enough to understand the treatment, and "comprehensive family planning" statutes, which governed how physicians could treat minors.

As the policy instrument we are studying was implemented in a staggered setting, we implement the estimation method proposed in Callaway and Sant'Anna (2021). Neither the raw data (Appendix Figure 33a) or the formal event study (Appendix Figure 33b) reveals any systematic earlier increase in women's share of enrollment at schools in states liberalizing access earlier. If anything, this design suggests lower enrollment. Given potential threats to identification surrounding receiving care in states other than where the law school is located, we present these results with caution about over-interpretation.

4.3.2 Title IX

Title IX of the Educational Amendments of 1972 prohibited any educational institution receiving federal funding from discriminating on the basis of sex for graduate or professional admissions. Rim (2021) finds that this law contributed to a subsequent rise in women's enrollment, finding larger increases in women's enrollment in programs with a larger incentive to comply with the program, as measured by the share of institutional revenue received from the federal government. To explore this potential mechanism, we collect institutional financial data from the 1968-69 Financial Statistics portion of the Higher Education General Information Survey (HEGIS) via ICPSR. We match each ABA-Approved law school in our sample to their parent institution (or institution, in the case of independent law schools) in the HEGIS survey to measure the parent institution's reliance on federal funding. Following Rim (2021), we defined the federal share of funding as the sum of federal appropriations, federal funding for sponsored research and other sponsored programs, and federal student aid, divided by total educational revenue and student aid grants.

Appendix Figure 34a splits programs into terciles given by their federal funding share and plots the average share of women in the first-year class for schools in each tercile, following Rim (2021). These time series give little indication that Title IX in improving women's enrollment: while we do observed the institutions with a higher federal funding share tend to enroll a higher fraction of women, this difference emerges in the late 1960s, around five years before Title IX is implemented. Following Helgerman (2025), to more rigorously test for a policy response, we also use a continuous difference-in-differences design of the form:

$$Y_{it} = \sum_{\tau=1960,\tau\neq 1971}^{\tau=1975} \alpha_\tau d_{i,1968} \mathbb{1}(t=\tau) + \gamma_i + \delta_t + \varepsilon_{it} \tag{7}$$

Here, Y_{it} is the fraction of first-year law students at institution i in year t who are women, $d_{i,1968}$ is a measure of reliance of institution i on federal funding measured in the 1968-69 HEGIS survey, and γ_i and δ_t are institution and year fixed effects respectively. Our parameter of interest, α_τ, captures changes in the relationship between reliance on federal funding and the fraction of first-year law students who are women. Our estimates are weighted by total first year enrollment and we cluster our standard errors at the institution level.

Our results are presented in Appendix Figure 34b. We use two different measures of reliance on federal funding: the log of total federal funding (following Helgerman (2025)) and the federal funding share (following Rim (2021)). All coefficients are multiplied by the standard deviation of the distribution of $d_{i,1968}$ so that they can be interpreted as the change in the fraction of women enrolled in response to a one standard deviation increase in $d_{i,1968}$. The pattern of parameter estimates are stable across design, and both lack compelling evidence of a treatment effect. If anything, it appears that there was a slight decline in women's enrollment at schools with higher reliance on federal funding. We advise caution in interpreting this as strong evidence of a null results: whereas Helgerman (2025) is able to measure directly federal funding given to medical schools, we do not have access to an analogous measure of federal funding provided to law schools. In government reports of funding distributed by HEW that we have been able to locate, the only programs listed separately within institution are health professional schools.

5 Changing Applicant Pools

Our preferred estimates of the policy impact of changes in the requirements for obtaining a deferment suggest that this led to an immediate 2 percentage point increase women's enrollment, which grows slightly between 1968 and 1975, but stays relatively constant. This explains the 1968 break in the trend for women's representation in full-time law schools that we observe in Figure 1b but cannot explain the secular increase in this same time series in the following years. As this figure illustrates, women's representation in both full-time and part-time programs increases in a markedly positive and parallel manner in the early 1970s. As Figure 1a shows, this stands in marked contrast to the fall in women's enrollment that occurred following previous surges in inductions during both WWII and the Korean. Accordingly, we turn next to understanding what led to this difference.

Aggregate statistics on enrollment suggest an obvious culprit: a surge in demand for legal education from women. Reporting results from surveys conducted by the Committee on Women in Legal Education of the Association of American Law Schools (AALS) in 1972, Bysiewicz (1973) finds that the number of women applicants climbed 14-fold between 1969 and 1972, reflecting data on LSAT administration reporting a 3-fold increase in the number of women taking the exam over the same period (Wightman 1974). Studying the role of demand changes, however, involves two difficult obstacles. First, any type of data that might get at the demand for legal education are not available across our sample period—Freeland (2015) notes that even consistent *aggregate* data on the number of men and women applying to law school is not available until 1985. Second, even if these data were available at the institutional level, they would of course not be an exogenous measure of the demand for legal education, as women's willingness to apply to law school likely also depends on anticipated discrimination and a program's reputation for enrolling a higher fraction of women students.[17]

To tackle these challenges, we digitize a rich new source of data on law school enrollment networks and exploit differences across programs in how their applicant pipeline moderates common national shocks to women's enrollment. The shocks we consider are the decision of formerly all-

[17] New York University Law School is often cited as an example of a particularly progressive program (Eckhaus 1991), becoming the first institution to admit a class where at least a quarter of the students were women (Epstein 1993, p. 54).

male undergraduate colleges and universities to transition to co-education. These transitions should result in an increase in women's competitiveness in the law school admissions process, especially insofar as they give undergraduate women access to highly selective programs. However, the degree to which each law school is impacted is moderated by existing enrollment networks, as students do not search randomly across law schools in the application process. We measure these networks using data from the 1963 *Martindale-Hubbell* Lawyer Directory, and use this information to calculate exposure to co-education transitions, which we argue below comprises an exogenous change in women's demand for legal education from the perspective of the law school admissions committee.

5.1 Data

Our primary data source is the *Martindale–Hubbell Law Directory*, a set of annual volumes that, for much of the twentieth century, served as the de facto national roster of lawyers in the United States and Canada. First published in 1868, the directory expanded to provide systematic listings of attorneys and law firms, organized in a "Geographical Bar Register" that proceeded state by state, and then city by city, across the country. Within each locality, Martindale–Hubbell recorded the names of firms and individual practitioners, along with their office addresses, contact information, and (for many entries) peer-reviewed ratings of professional ability and ethical standards.

We use the 1963 edition, drawing specifically on the Geographical Bar Register. This section of the volume was intended to provide comprehensive coverage of lawyers within each jurisdiction, distinguishing it from the "Biographical Section," which contained longer paid profiles for selected lawyers and firms. The Geographical Bar Register thus provides the most complete census-like listing of the organized bar available for this period. Contemporaneous writing indicates that more than 90% of practicing lawyers are listed in Martindale–Hubbell during this time period (York and Hale 1973). Importantly, this data includes information on where nearly every lawyer graduated from for both undergraduate training and law school.

Researchers have long recognized the value of Martindale–Hubbell directories for studying the legal profession. The American Bar Foundation relied on Martindale–Hubbell listings to construct the official counts of U.S. lawyers in its *Lawyer Statistical Reports* between 1951 and 2005 (American Bar Foundation 2005). More recently, empirical studies have used the directories to analyze the evolution of law firms, mobility of lawyers, and the role of reputation ratings in professional markets (Chilton et al. 2024; Farhang 2006; Partow 2023). Our use of the 1963 Geographical Bar Register builds on this tradition by leveraging its comprehensive coverage to examine the distribution of lawyers across geographies and organizational settings at mid-century.

5.1.1 Timing of Co-education Transitions

To identify institutions that transition from all-male education to coeducation, we use the Coeducation College Database, compiled by Goldin and Katz (2011) and graciously shared with us by the authors. We begin with the sample of 88 institutions identified by Truffa and Wong (2025) that make this transition between 1960 and 1990. We restrict our sample to institutions that (i) made this transition between 1968 and 1975 and (ii) are identified in the list of institutions in the 1963 *Martindale-Hubbell* directory. We choose this time period because, in addition to occurring immediately following the change in deferment rules, it is also a time of particular importance in the history of coeducation. 62 institutions transition in these 8 years, including many ivy league colleges and elite private schools. Of these 62 schools, we are only able to match 55 institutions to the *Martindale-Hubbell* list; all institutions, the timing of their transition, as well as their inclusion in *Martindale-Hubbell* are presented in Appendix Table 2. Goldin and Katz (2011) note that, outside of Catholic institutions, this period is not so remarkable in terms of the number of programs that begin to enroll women, but given their stature, we hypothesize that they might be of particular importance for law school admissions, where the reputation of the undergraduate institution likely matters for the probability of acceptance.

5.2 Methodology

We use a first-differences specification of the form:

$$\triangle Y_i = \alpha + \beta S_i + \boldsymbol{\gamma}'\mathbf{X}_i + \varepsilon_i \tag{8}$$

Here, $\triangle Y_i$ gives the change in the fraction of first-year students who are women between 1967 and 1976. We suppress the program subscript, ρ, as we only consider full-time enrollment in this design, since part-time programs are no longer used as a comparison group. Our main parameter of interest, β, measures the response of the outcome to the fraction of lawyers we observe with a degree from institution i who graduated from an all-male institution that transitions to co-education between 1967 and 1976. $\mathbf{X}_i$ is a vector of observed controls. Note that this specification is consistent with that used in equation (1); institution-program fixed effect are removed by first-differencing, and the difference in year fixed effects between 1967 and 1977 is estimated with the constant term α.

Formally, we can decompose our shift-share instrument S_i as follows:

$$S_i = \sum_{c=1}^{C} L_{ic}^{63} W_c \tag{9}$$

Here, L_{ic} is our measure of the fraction of lawyers with a law degree from institution i that received a bachelor's degree from college c as measured in the 1963 *Martindale-Hubbell* lawyer directory.[18] Our ideal statistic would measure the fraction of lawyers that matriculate at institution i after graduating from college c, but we expect this to be a reasonable proxy of these recruiting networks.

[18] This is only an estimate of the true flow, as our measure is impacted by non-response to the *Martindale-Hubbell* survey and errors in optical character recognition (OCR). As noted above, non-response is thought to be very low, and OCR errors are likely to be orthogonal to our flow estimates.

W_c is an indicator for college c transitioning to co-education between 1967 and 1975, our time period of interest.

We visualize these flows for a subset of our sample in Appendix Figures 36 and 37. As can be seen from these basic flows, there is considerable variation in where law schools are drawing their classes. Some schools like the University of Minnesota Law School (Appendix Figure 36a) recruit mostly local students within state. Other schools, like Boston University School of Law (Appendix Figure 36b), were drawing in students regionally from the northeast. Still others had a more national reach like Georgetown (Appendix Figure 37a) and Harvard (Appendix Figure 37b). Despite differences across institutions in the geographical scope of which undergraduate institutions their students graduate from, there is one clear commonality: the largest contributor to each law school is the undergraduate program that it is attached to. As we discuss below, this could confound our results, so we develop an alternative definition of S_i that does not include these within-institution flows as well.

5.2.1 Identification

We utilize a reduced form version of a shift-share design, where we are interested directly in the relationship between S_i and our outcome. Causal identification hinges on the exogeneity of either the shifts or the shares utilized in such a design (Borusyak et al. 2025). It is clear in our setting that the shifts are not exogenous; far from the ideal random experiment, in 1967 only a handful of schools in operation enrolled men only and would be able to transition to co-education. In light of this, we rely on exogeneity of the shares. Goldsmith-Pinkham et al. (2020) show that S_i can be written as the sum of K separate instruments, where K is the number of shocks, or schools that transition to co-education in our context. Our identifying assumption is that, for each college that transitions to co-education, law schools with differing exposure to this change, as measured by our network variable L_{ic}^{63}, would have experienced parallel trends in outcomes in the absence of this change.

Following the recommendation of Goldsmith-Pinkham et al. (2020), we begin by describing the variation each instrument is leveraging. We can write our estimate of the coefficient of interest from (8) as $\hat{\beta} = \sum_c \hat{\alpha}_c \hat{\beta}_c$, where $\hat{\beta}_c$ is the two-stage least squares estimate resulting from instrumenting for S_i using L_{ic}^{63} and $\hat{\alpha}_c$ is the Rotemberg weight. Accordingly, $\hat{\alpha}_c$ reports the contribution of the IV regression leveraging variation across programs in their exposure to the co-education transition at college c. Table 2 presents the Rotemberg weights for the 26 schools taking a weight value above the median, listed in order of magnitude.[19] The year in which the school transitions is included, and Appendix Figure 38 plots these weights across years, in addition to the total weight in each year. We find that 1969, 1970, and 1972 drive around 75% of the variation in total, with 1972 driven by Notre Dame, which carried the highest Rotemberg weight. To understand why, Table 2 also

[19]This represents around 93% of the total weight and thus captures almost all of the relevant variation.

plots the mean, standard deviation, and number of non-zero entries for each college's instrument. Schools with a higher Rotemberg weight tend to send more students to law school (higher μ) and also have more variation in where their students matriculate (higher σ). Further, the importance of Notre Dame in particular is likely explained by its extensive network, sending students to 101 of the law schools in our sample, the most of any formerly all-male institution.

5.3 Results

Results from this exercise are presented in Panel A of Table 3. Each column gives an estimate of the parameter of interest, $\hat{\beta}$, multiplied by the standard deviation of the shift-share measure S_i, so that the value reported represents the estimated increase in the fraction of women enrolled in the first year in response to a one standard deviation increase in exposure to coeducation transitions. Column 1 presents our baseline results, where we find that a one standard deviation increase in exposure leads to a 1.1% increase in the percentage of women enrolled. This finding is robust to allowing correlation in the errors across state clusters, which is reported in Column 2. Following the recommendation in Borusyak et al. (2025), Column 3 reports a specification with a variety of institutional controls included to relax the assumption of share exogeneity to hold conditional on these controls. We are able to match 114 of the 116 law schools in our sample to the 1968-69 *Pre-Law Handbook*, which includes information about institutions in the 1967-68 academic year. We include indicators for AALS membership, ABA approval, LLB awarded, JD awarded, having a law review, having a legal aid program, requiring an undergraduate degree for admission, requiring an undergraduate degree for graduation, requiring the LSAT, admitting outside of the Fall term, admitting special students (e.g. summer only), allowing accelerated study by taking summer courses, hosting summer courses for their own students, and hosting summer courses for other students.[20] Conditional on program characteristics, we find evidence of a stronger institutional response—a one standard-deviation increase in exposure to co-education transitions is associated with a 1.9% increase in the percentage of women enrolled in the first year.

The main threat to identification in this setting is that, among law schools located within institutions that themselves adopt coeducation during this time period, institutional pressure to admit women might drive both this decision as well as the law school's admissions decision. Indeed, as Goldin and Katz (2011) point out, undergraduate institutions often begin to enroll women due to financial pressure from a shrinking pool of highly qualified men to pull from; as we know from the results in our previous section, a very similar pressure applied to law schools during the Vietnam War. This concern, together with the descriptive finding noted earlier that it is generally the case that the institution sending the most students to each law school is the undergraduate program at the same institution, imply that endogeneity of this nature could very well be driving our results. To rule this out, we run a separate design where we drop own-institution flows from our shift share

[20] We only include binary controls as they are defined for all programs we are able to match to the handbook data.

variable. Formally, we modify equation (9) so that

$$S_i' = \sum_{c=1, c\neq i}^{C} L_{ic}^{63} W_c \tag{10}$$

Column 4 of Panel A reports results from equation (8) using S_i' as the independent variable. We find essentially no difference in our coefficient estimate, suggesting that our findings are not being driven by within-institution flows from undergraduate to legal education, and our results remain robust to clustering errors at the state level (Column 5) and the inclusion of institutional controls (Column 6).

Finally, we conduct a pre-period test to understand if S_i predicts increases in women's enrollment before 1967, which would suggest that women's representation would not have evolved in parallel across more or less exposed institutions in response to the shock we measure. To do this, we estimate equation (8) where the outcome is now the change in the share of women enrolled in the first year between 1960 and 1967. Panel B presents the same collection of results as Panel A with this new outcome variable. We find little evidence of any pre-existing relationship, as all estimated coefficients are not significant at traditional levels. Our baseline estimate in column 1 does not rule out that there might be a weak, imprecise pre-existing positive relationship, but comfortingly this disappears almost entirely with the inclusion of institutional controls (column 3) or removing within-institution flows (columns 4-6).

6 Conclusion

This paper provides new causal evidence that the Vietnam War reshaped access to the legal profession in ways that extended beyond men's draft exposure. When male law students lost eligibility for 2-S deferments in 1968, law schools faced a sudden risk of enrollment shortfalls. Using a difference-in-differences design that exploits the differential exposure of full-time and part-time programs to this policy change, we show that women's representation in full-time programs rose by about two percentage points, about a 44 percent increase over baseline levels. The effect was concentrated among more prestigious institutions, suggesting that women's entry accelerated most at the top of the law-school hierarchy. We further show that, in the years that followed the draft, law schools began to respond to rising demand from women as coeducation spread across elite undergraduate institutions. Law schools more closely connected to these newly coeducational programs admitted more women, suggesting that the erosion of supply-side barriers interacted with expanding female demand for legal education.

Taken together, our results highlight how external shocks, in this case a military manpower policy not directed at higher education, altered the gender composition of professional schools and helped open the doors of the legal profession to women. We also underscore that institutional change in access to professional education often arises from the interaction of policy shocks and evolving applicant pools, rather than from formal equality mandates alone. While we do not analyze employment outcomes in this paper, future research may build on these findings by linking individual-level lawyer records to subsequent career outcomes, examining whether women who entered law schools during the Vietnam era persisted in the profession and how their entry reshaped firm composition, practice areas, and the long-run structure of the bar.

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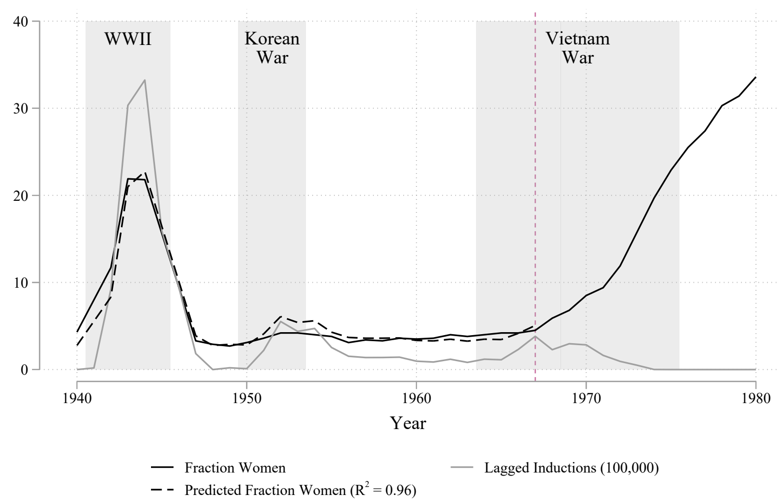
Figure 1: Enrollment statistics

(a) Draft Inductions and Women's Representation in Law School



(b) Fraction Women in Full-Time and Part-Time Programs as a 1L

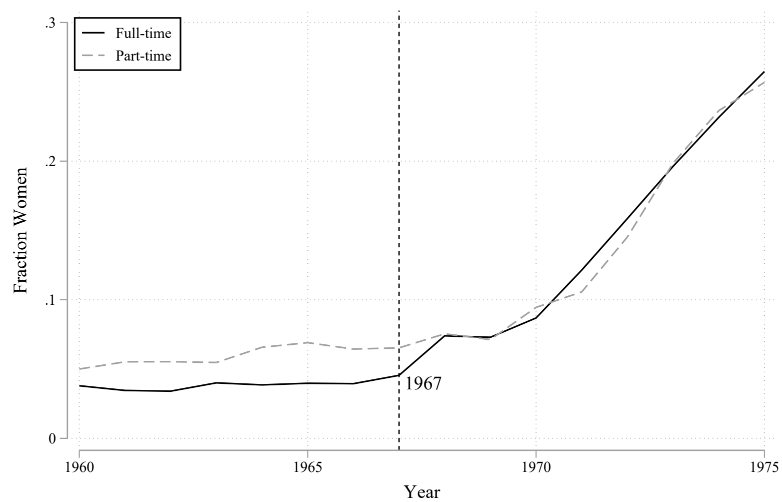
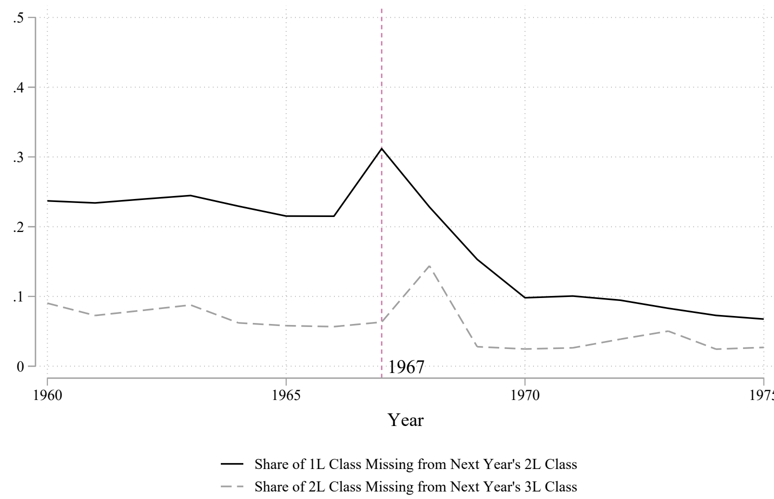


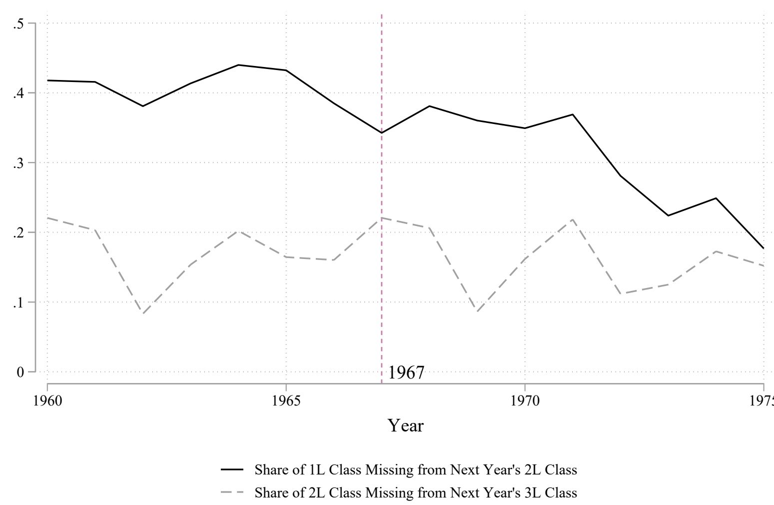
Figure 1a plots the percentage of all law students who are women between 1940 and 1980, collected from Table 27 of Abel (1989), against the total number of inductions conducted by the Selective Service System in the previous year between 1940 and 1973, collected from `www.sss.gov/history-and-records/induction-statistics/`. For years after the draft ended in 1973, this time series is set equal to 0. We regress the percentage of law students who are women on lagged inductions between the 1940 and 1967, plotting the predicted value and reporting the R^2 statistic. Shaded bars plot U.S. involvement in WWII (1941-1945), the Korean War (1950-1953), and the Vietnam War (1964-1975), collected from `department.va.gov/americas-wars/`. Figure 1b plots the fraction of all first-year students who were women at U.S. law schools that reported to the *Review of Legal Education* from 1960 through 1975 separately for full-time and part-time programs. Data are collected from the *Review of Legal Education* in every year. In both plots, we mark the year before student deferments from the Vietnam Draft were revoked (1967).

Figure 2: Aggregate Attrition for Men

(a) Full-Time Programs

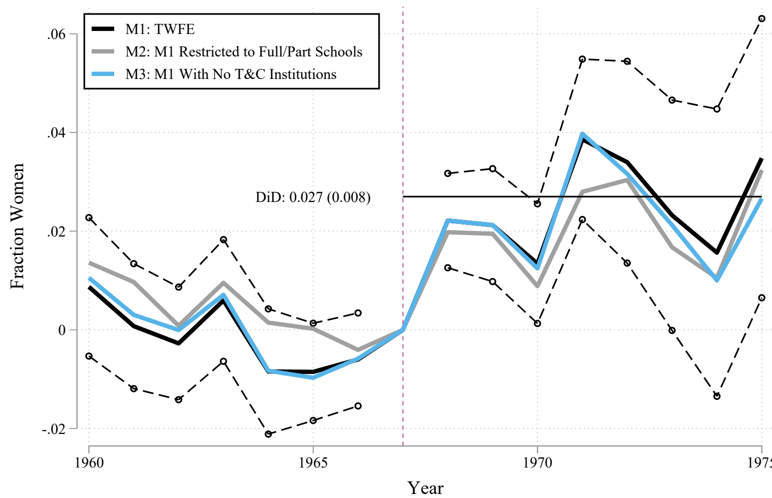


(b) Part-Time Programs

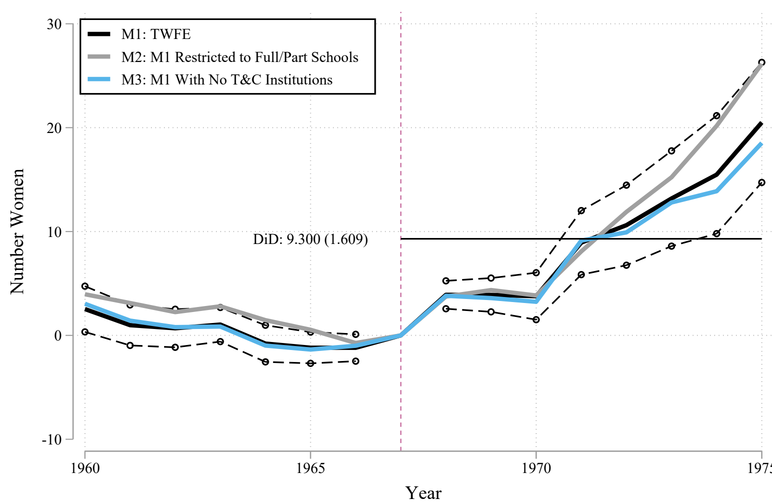


Each figure plots estimates of men's attrition from law schools in each year. To do this, we calculate the percentage drop in men's enrollment from year to year. These calculations are plotted in the baseline year; for example, in 1967, we plot the percentage of 1L students in 1967 that are missing in 1968. The solid black line plots the share of the 1L class missing from next year's 2L class; the dashed grey line plots the share of the 2L class missing from the next year's 3L class. Figure 2a plots these series for full-time programs. Figure 2b plots these series for part-time programs. In both plots, we mark the year before student deferments from the Vietnam Draft were revoked (1967).

Figure 3: Difference-in-Differences Results



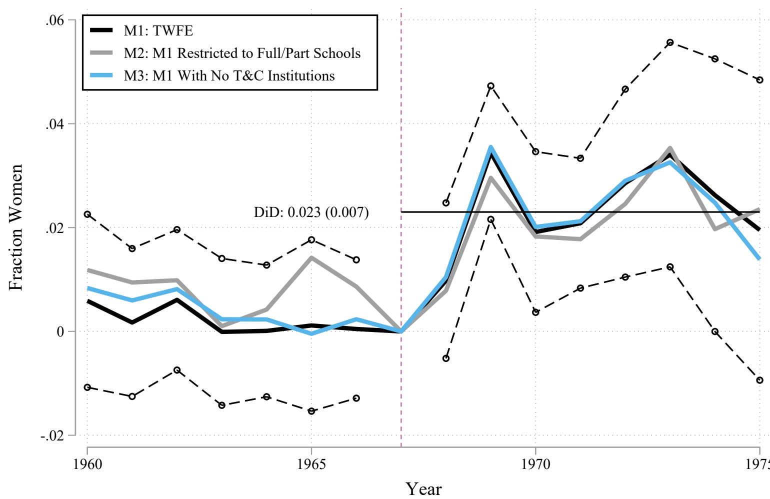
(a) Fraction Women



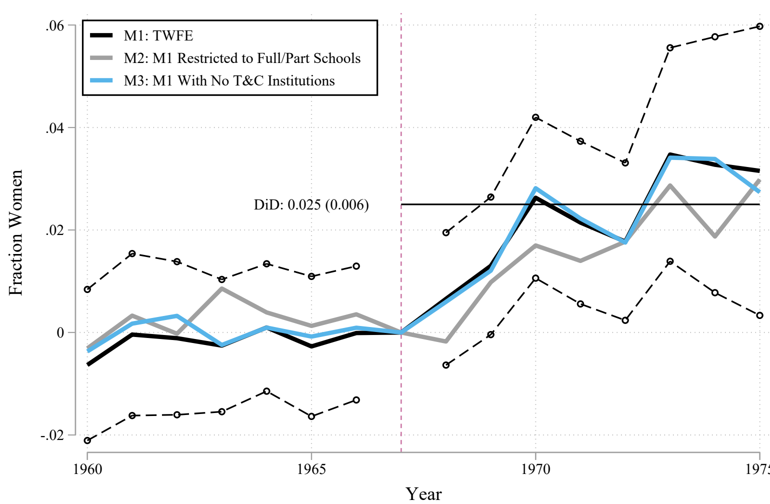
(b) Number Women

Figure 3a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Figure 3b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Figure 4: Difference-in-Differences Results: L2 and L3



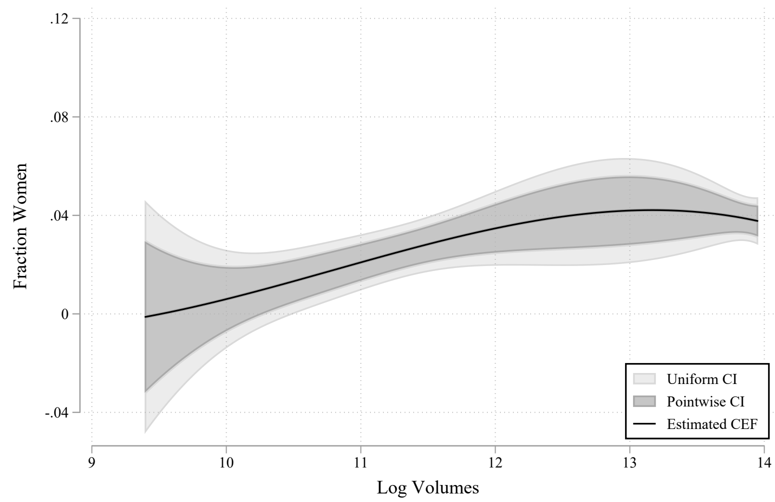
(a) Fraction Women (L2)



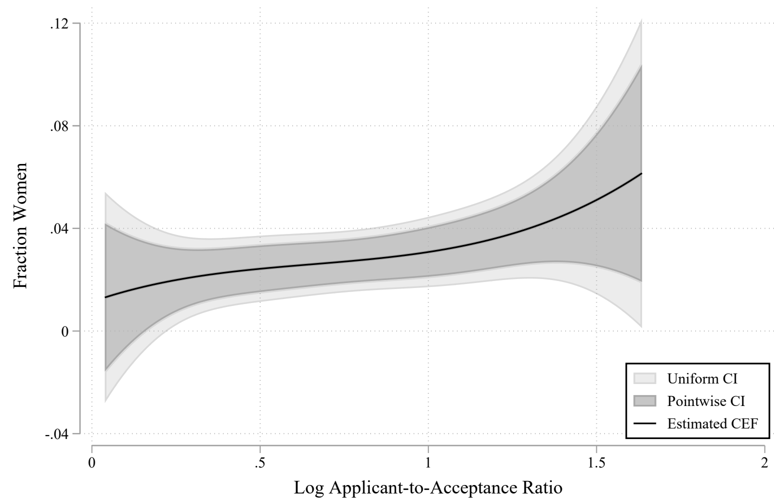
(b) Fraction Women (L2)

Figures 4a and 4b plot coefficient estimates from equation (1), where the outcome is the fraction of second-year (Figure 4a) or third-year (Figure 4b) students who are women and the regression is weighted by total second-year (Figure 4a) or third-year (Figure 4b) enrollment. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Figure 5: Continuous Differences-in-Differences Results



(a) Log Volumes in Law Library



(b) Log Applications per Acceptance

This figure plots estimates from equation (6), where the outcome is the change in the fraction of first-year students who are women between 1967 and 1968 and the regression is weighted by total first-year enrollment in 1967. After estimating regression coefficients, we construct a grid of 500 points evenly spaced between the minimum and maximum dose values and plot $\hat{Y}_{i\rho}$ for treated units ($D_{i\rho} = 1$) at each of these dose values. We also plot two sets of 95% confidence bands across our grid: a pointwise confidence interval using the standard error of the predicted value at a given level of the dose, as well as a uniform confidence interval across the entire grid using the method proposed in Montiel Olea and Plagborg-Møller (2019).

Table 1: Difference-in-Differences Results

	(1)	(2)	(3)
1L			
Fraction Women	0.027***	0.018*	0.024***
	(0.008)	(0.010)	(0.009)
Number Women	9.300***	8.913***	8.749***
	(1.609)	(2.035)	(1.967)
Observations	3288	1610	2468
2L			
Fraction Women	0.023***	0.015	0.020**
	(0.007)	(0.009)	(0.008)
Number Women	5.343***	5.345***	4.918***
	(1.138)	(1.438)	(1.331)
Observations	3238	1583	2439
3L			
Fraction Women	0.025***	0.016*	0.023***
	(0.006)	(0.008)	(0.007)
Number Women	3.899***	3.228***	3.688***
	(0.925)	(1.024)	(1.127)
Observations	3180	1553	2402
TWFE	X	X	X
Full/Part		X	X
No T&C			X

This table presents difference-in-differences results from equations (2) and (4). Each section header denotes the year of interest for that section, which is either first-year (1L), second-year students (2L), or third-year students (3L). The first row within each section presents results from equation (2), where the outcome is the fraction of students who are women in the year of interest, and the regression is weighted by the total number of students in the year of interest. The second row within each section presents results from equation (4), where the outcome is the number of students who are women in the year of interest. Column 1 presents results from Model 1, a standard two-way fixed-effects design. Column 2 presents results from Model 2, which restricts our sample to include only institutions operating both a full-time and a part-time program. Column 3 presents results from Model 3, which restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.
*** $p < .01$, ** $p < .05$, * $p < .10$

Table 2: Rotemberg Weights

Institution	Rotemberg Weight	Transition Year	μ	σ	# Non-Zero
University Of Notre Dame	0.145	1972	0.012	0.060	99
Tulane University Of Louisiana	0.099	1969	0.006	0.053	50
Boston College	0.088	1970	0.006	0.038	30
Villanova University	0.066	1968	0.004	0.028	37
Fordham University	0.063	1974	0.005	0.030	40
St John's University-New York	0.063	1971	0.004	0.035	32
Yale University	0.061	1969	0.009	0.029	93
University Of Virginia-Main Campus	0.056	1970	0.006	0.034	74
College Of The Holy Cross	0.049	1972	0.004	0.014	46
Saint Joseph's University	0.031	1970	0.002	0.014	20
Georgetown University	0.029	1969	0.005	0.018	75
Princeton University	0.027	1969	0.006	0.011	79
La Salle University	0.020	1970	0.001	0.009	17
Manhattan College	0.016	1973	0.001	0.005	27
Dartmouth College	0.015	1972	0.004	0.006	85
Providence College	0.015	1970	0.002	0.006	28
Johns Hopkins University	0.014	1972	0.002	0.014	33
University Of Scranton	0.012	1973	0.001	0.005	31
Bowdoin College	0.011	1971	0.002	0.015	43
Loyola College	0.011	1971	0.001	0.009	19
Loyola Marymount University	0.011	1973	0.002	0.021	17
Rutgers University New Brunswick	0.009	1970	0.002	0.009	51
Amherst College	0.009	1975	0.002	0.004	79
Lafayette College	0.008	1970	0.001	0.004	41
Franklin And Marshall College	0.008	1969	0.001	0.004	34
Williams College	0.007	1970	0.002	0.003	57
Lehigh University	0.007	1971	0.001	0.003	42
Mount St Mary's University	0.006	1971	0.001	0.002	29

This table displays Rotemberg weights ($\hat{\alpha}_c$) and summary statistics for the 28 institutions with weights above the median, representing 96% of the total weight. The weights measure the contribution of each institution's specific instrument to the overall coefficient estimate. Transition year denotes the first year in which the formerly all-male college admitted women as recorded in Goldin and Katz (2011). μ and σ denote the mean and standard deviation of the shares. # Non-Zero is the count of law schools to which the institution sends students.

Table 3: Shift-Share Results

Panel A. 1968–1976

	Including Own Flows			Excluding Own Flows		
	(1)	(2)	(3)	(4)	(5)	(6)
Shift Share	0.011**	0.011**	0.018***			
	(0.004)	(0.004)	(0.006)			
Shift Share (No Own Flows)				0.013**	0.013**	0.022***
				(0.006)	(0.006)	(0.008)
Observations	112	112	110	112	112	110
State Clusters		X	X		X	X
Institution Controls			X			X

Panel B. 1960–1967

	Including Own Flows			Excluding Own Flows		
	(1)	(2)	(3)	(4)	(5)	(6)
Shift Share	0.004**	0.004**	0.001			
	(0.002)	(0.002)	(0.002)			
Shift Share (No Own Flows)				0.002	0.002	0.001
				(0.003)	(0.002)	(0.002)
Observations	107	107	106	107	107	106
State Clusters		X	X		X	X
Institution Controls			X			X

This table presents results from equation (8). The outcome for Panel A is the long difference in the fraction of first-year students who are women between 1976 and 1968. Panel B is pre-trends analysis using the 1967 to 1960 long difference. Columns 1-3 use our shift-share dependent variable calculated in equation (9). This gives the historic share of graduates who attend an undergraduate institution that transitions from all men to coeducation between 1967 and 1975. Column 4-6 present results from the same design, where the shift-share variable excludes flows from an undergraduate program at the same institution as the law school, as given by equation (10). Columns 2 and 5 cluster errors at the state level. Columns 3 and 6 include institutional controls documented in the 1967-68 academic year: AALS membership, ABA approval, LLB awarded, JD awarded, having a law review, having a legal aid program, requiring an undergraduate degree for admission, requiring an undergraduate degree for graduation, requiring the LSAT, admitting outside of the Fall term, admitting special students (e.g. summer only), allowing accelerated study by taking summer courses, hosting summer courses for their own students, and hosting summer courses for other students. All coefficient estimates and standard errors are multiplied by the standard deviation of the shift-share variable. Standard errors are robust to heteroskedasticity.
*** $p < .01$, ** $p < .05$, * $p < .10$

Appendix

A Theoretical Framework

We consider the problem of a law school deciding which applicants to enroll. The admissions committee receives a set of applications of mass M men and mass F women, from which they need to choose a set of E students to satisfy an enrollment constraint. Students are distinguished by their qualifications θ_i, which we assume are of a single dimension.[1] The committee cares about the average credentials of its admitted students, and so $\bar{\theta}_i$ among its incoming class enters directly into the committee's objective function. However, we also assume that the school will pay a prestige cost by admitting more women than the average program. This cost is given by $C(f - \bar{f})$, where $\bar{f}$ was the fraction of all students in the previous year that were women. We assume that $C(x) = 0$ if $x < 0$, and that $C(\cdot)$ is convex for $x > 0$. Intuitively, a program only pays this prestige cost if it enrolls more women relative to the average, and this cost is increasing in that deviation. In sum, the objective function for the admissions committee is given by $\bar{\theta}_i - C(f - \bar{f})$

Since enrollment is fixed, the admissions committee's decision depends solely on the fraction of students it admits that are women, given by f.[2] If the same standard for admission is set across all students, average credentials will be maximized, with an implied gender mix f^{opt}. Changing this mix necessitates a different admissions threshold for each group and must lower average credentials but could be preferred by the committee if it lowers the prestige cost of an incoming cohort with a large fraction of women. Appendix Figure 4 demonstrates this graphically. Appendix Figure 4a plots the credential θ of the marginal man (M) and woman (W) as the fraction of women enrolled (f) changes. Consider a law school that enrolls only men. When this program enrolls its first woman, it experiences an increase in its average cohort credentials, as it enrolls the highest credentialed woman in its applicant pool in place of the marginally admitted man. For each change in f, this difference (MB) is given by the difference between the marginal woman applicant and marginal man applicant; graphically, this is the vertical distance between these two curves.[3] Appendix Figure 4b plots the positive portion of this marginal benefit (MB) curve against the marginal cost to prestige by enrolling a higher than average fraction of women. The point at which these intersect, f^*, is the solution to the committee's problem, which will be less than f^{opt} as long as the school is constrained and $f^{\text{opt}} > \bar{f}$.

Now, we consider how the policy change affects the admissions decision. From the perspective of the committee, this amounts to a random shock across men's applications, where a certain proportion of these students are unable to matriculate when they are drafted. As a result, the curve giving the marginal credentials for men should shift to the right, as demonstrated in Appendix

[1] This is a very reasonable approximation. Many law schools at the time considered a weighted average of LSAT score and undergraduate GPA (Lunneborg and Radford 1965).

[2] This depends on the innocuous assumption that, conditional on gender, the committee would prefer a more credentialed student to a lower one.

[3] To speak more precisely, the marginal benefit is the distance between these curves multiplied by $1/E$, but this does not change the analysis.

Figure 4c. To enroll the same number of men, the credential of the marginal man enrolled must be lower as a result of the draft shock. Since E is fixed, it follows that for each f, the marginal benefit of enrolling more women must increase, represented by the increased vertical distance between the credential curves. This translates to an outwards shift of the marginal benefit curve, which is illustrated in Appendix Figure 4d. This immediately implies an increase in f^* and f^{opt}, the first substantive prediction of our model.[4]

Next, we turn to the question of whether a one-off change can have a persistent impact on women's enrollment. To do so, we assume that in the year after the policy change, the threat to men's enrollment dissipates. This is unlikely but represents an upper bound on the endogenous response in men's demand for legal education: strategic behavior to avoid the draft through means other than student deferment likely buoyed men's applications in the years following 1968 to partially offset the increased draft risk. Accordingly, we let all credential curves return to their original position after the policy in Appendix Figure 4e, and consequently the marginal benefit curve in Appendix Figure 4f also returns to its initial position. However, as a result of the policy response in the previous period, $\bar{f}$ has increased, lowering the marginal cost of enrolling a higher share of women and pushing our the marginal cost curve. This can result in a higher equilibrium fraction of women even after the draft shock has dissipated.

[4]This model prediction is consistent with the the more colorful description of some administrators at the time: 'Testifying before Congress to oppose the change, President Nathan Pusey of Harvard predicted that his law school's entering class of 540 would be 'reduced by close to a half.' Harvard law might stay full only by compromising on 'quality or something.' " and Congressman John Erlenborn: "a policy of admitting women, the halt, and the lame" (Strebeigh 2009).

B LSAC Data

To understand potential demand-side responses to policy announcements, we digitize summary statistics on gender differences in LSAT performance from Hussein and Wightman (1971), an internal report generated by the Law School Admission Test Council (LSAC). These data begin in November 1966 and, as far as the authors are aware, comprise the earliest available LSAT statistics that are reported separately by gender.[5] These data are reported at the "monthly" level,[6] and they are organized by application cycle, with four exams taking place in each cycle, in November, February, April, and August. In the 1970-71 application cycle, the number of exams is expanded to five and the months in which they are administered are adjusted to October, December, February, April, and July. Since we need to remove cyclicality from our data and are only interested in immediate changes in test taking behavior following policy announcements in 1967 and 1968, we drop this cycle to obtain a dataset on LSAT aggregates by gender between the 1966-67 and 1969-70 application cycles with 16 observations.[7] For each exam administration, we observe the number of registered test takers, the number of actual test takers, and the mean/standard deviation of LSAT score, all reported separately by sex.

Our objective is to understand if any of these administration statistics show evidence of a shift in exam taker composition. In particular, we want to rule out that there was a positive shock to women's attendance or performance following either of the relevant policy announcements that could explain their rise in enrollment in the Fall of 1968. At this time, a vast majority of law schools required an LSAT score for admission.[8] With data on LSAT scores, this is relatively straightforward to test. For each exam administration, we have information on mean LSAT score, standard deviation of LSAT score, and number of test takers, separately by sex. To begin, we plot raw data on the mean and standard deviation of LSAT score for women in Appendix Figure 22a and for men in Appendix Figure 23a. To observe timing precisely, we collect LSAT administration dates from the University of Chicago Law School Announcements between 1966-67 and 1969-70, and plot the two policy announcements we are concerned with: the 1967 Draft Act on June 30, 1967, and the removal of deferments for law students on February 17, 1968.[9] It's clear from the data that

[5] At the beginning of the report, the authors note that LSAC requested that gender-specific reports be made available at its December 12, 1970 meeting. The report, published on November 18, 1971, seems to be the fulfillment of this request, and there is no indication of an earlier set of results available.

[6] In 1970, the "August" LSAT is actually administered in late July.

[7] Data between the 1969-70 and 1972-73 application cycles are available in a later report (Wightman 1974). However, not only does number and timing of the exams differ in these years, but the way in which the data are reported changed across time. This second report takes the basic unit of observation to be the candidate and not the score, replacing repeated exams with the student's average score. This change in reporting makes it difficult to compare statistics reported between the two reports, and since we prefer results at the score level, we only use data from the first report.

[8] The 1968 Law School Handbook indicates all schools apart from Brooklyn, Chicago, Creighton, Fordham, Howard, and Texas South. required the exam.

[9] Note, in particular, that the February 1968 LSAT takes place on February 10, 7 days before the policy announcement on February 17.

average scores vary cyclically both across and throughout the application cycle, and it's difficult to tell from this plot alone that there is any impact of the announcement. So, to run a formal statistical test, we first remove month and year effects using the following regression specification:

$$Y_{g,t} = \alpha_{\text{month}(t)} + \delta_{\text{year}(t)} + \varepsilon_{g,t} \tag{B.1}$$

Where $Y_{g,t}$ is mean LSAT score for gender g on date t, and $\alpha_{\text{month}(t)}$ and $\delta_{\text{year}(t)}$ are month and year fixed effects, respectively. Our object of interest is the residual from this model, $\hat{\varepsilon}_{g,t}$, estimated separately by gender, that gives the deviation of each LSAT mean from its expected value given our estimated month and year effects.

We run a simple one-sample z-test to test whether or not this residual has mean zero in a given year, leveraging the fact that we observe all of the statistics for each exam needed to run such a statistical test.[10] Our results for women are plotted in Appendix Figure 22b and for men in Appendix Figure 23b, where we include horizontal lines at the 5% significance level for our test statistic. We find little evidence that women's scores respond to policy announcements as evidenced by a measured increase in their average LSAT score. We are unable to reject the null that the residualized mean is zero in the two administrations following each policy announcement, as well as the first November exam following each policy announcement, which is generally the most well attended. In fact, for many of these exams, we estimate a drop in women's mean score relative to trend. For men, many of our estimates are significant, but we are hesitant to draw any definite conclusions given our limited ability to detrend. That said, our estimates suggest a drop in men's average score in the administration after a policy announcement, implying a response consistent with men's decreased enrollment in the Fall of 1968.

While LSAT statistics are easiest to test, we are also interested in whether or not there is an increase in registration or attendance by women in response to the policy announcements—given our null results on scoring, a boost in women's exam taking behavior would be enough to boost enrollment in 1968. Unfortunately, this is less straightforward to test, as we are observing only 16 observations of counts during our sample period, as opposed to 16 means across many individual observations of LSAT scores. In light of this, we follow best practices from the literature on outlier detection. Belotti et al. (2024) recommend the following strategy:

1. Transform the variable of interest to induce normality in its empirical probability density function.
2. Set robust thresholds to identify the outlier region.

To induce normality, we use a log transformation for count variables, and a logit transformation

[10] It is likely that LSAT scores themselves are approximately normally distributed. Amabebe (2020) notes that the modern LSAT is norm-referenced, where raw scores are translated to test scores such that the distribution follows a bell curve that remains similar across exams to allow for comparability. We don't have clear documentation of how LSAT scores were normalized during our sample period, but it seems likely that a similar, if not more crude, method of normalization was used. Regardless, our tests here do not depend on this assumption.

for fraction variables (e.g. the fraction of test takers who are women). These are not necessarily the optimal transformations to induce normality, but they allow us to remove cyclicality from the transformed variables with interpretable fixed effects.[11] Specifically, we use equation (B.1), where $Y_{g,t}$ is the transformed variable of interest for gender g on date t, and we again consider the residuals $\varepsilon_{g,t}$ from designs detrended separately by sex. For count variables, the log transformation assumes that within-application cycle deviations are constant and additive in percentage terms, and the logit transformation assumes that these deviations are constant and multiplicative in the odds ratio. These are important assumptions to consider as the number and fraction of women taking the LSAT is growing substantially between 1966-67 and 1969-70.

We consider three alternative methods to set robust thresholds for outlier detection. The framework for detection is very straightforward: we need to choose a measure of location μ and scale σ and set a threshold for detection z_α such that $\hat{\varepsilon}_{g,t}$ is flagged as an outlier if $|(\hat{\varepsilon}_{g,t}-\mu)/\sigma| > z_\alpha$ (Belotti et al. 2024). Our simplest method uses the studentized residual $\tilde{\varepsilon}_{g,t}$ as the test statistic, which is given by

$$\tilde{\varepsilon}_{g,t}^{\text{Studentized}} = \frac{\hat{\varepsilon}_{g,t} - 0}{\sqrt{\text{MSE}_{(g,t)}}\sqrt{1-h_{g,t}}} \tag{B.2}$$

Where $\text{MSE}_{(g,t)}$ is the Mean Squared Error calculated without observation (g,t) and $h_{g,t}$ is the leverage of observation (g,t). Here, the mean is our measure of location (constrained to be 0 by OLS), and the denominator is the standard error of observation (g,t). Since the mean is not very robust to outliers, our other methods utilize the median as our measure of location, and we consider two robust measures of scale: the median absolute deviation (MAD) and Q statistic (Rousseeuw and Croux 1993).[12] These are defined as followed:

$$\tilde{\varepsilon}_{g,t}^{\text{MAD}} = \frac{\hat{\varepsilon}_{g,t} - \text{Med}(\hat{\varepsilon})}{\text{MAD}_n(\hat{\varepsilon})} = \frac{\hat{\varepsilon}_{g,t} - \text{Med}(\hat{\varepsilon})}{a_n \text{Med}|\hat{\varepsilon} - \text{Med}(\hat{\varepsilon})|} \tag{B.3}$$

$$\tilde{\varepsilon}_{g,t}^{\text{Q}} = \frac{\hat{\varepsilon}_{g,t} - \text{Med}(\hat{\varepsilon})}{\text{Q}_n(\hat{\varepsilon})} = \frac{\hat{\varepsilon}_{g,t} - \text{Med}(\hat{\varepsilon})}{b_n\{|\hat{\varepsilon}_{g,t} - \hat{\varepsilon}_{g,t'}|; t < t'\}_{(k)}} \tag{B.4}$$

Here, a_n and b_n are finite sample corrections to ensure these estimators are unbiased,[13] and $k =$

[11] Well known variance stabilizing transformations include the square root for a Poisson random variables $\sqrt(n)\sin^{-1}\sqrt{\hat{p}}$ for a Binomial random variable with n draws and observed success rate $\hat{p}$ (Yu 2009), with refinements proposed by Anscombe (1948) and Freeman and Tukey (1950). We could also apply the Box-Cox transformation to induce normality and remove heteroskedasticity (Box and Cox 1964). In practice, Belotti et al. (2024) suggest using the transformation that maximizes goodness of fit, but as this would lead to differential transformations being applied across different variables, we opt for a log transformation as it is uniform and easily interpretable in our context. Appendix Figure 32 presents QQ plots of our empirical quantiles against what we would observe were the data exactly normally distributed along with the p-value from the Shapiro-Wilk test of normality, which fails to reject any transformation we consider.

[12] The Q statistic, proposed by Rousseeuw and Croux (1993), matches the finite sample breakdown point of the MAD (50%), a measure of the percentage of data that can be corrupted without the bias of the statistic diverging (see Huber (1984) for details). However, it demonstrates much higher asymptotic efficiency, implying potential gains to precision without sacrificing robustness.

[13] The finite sample correction for the MAD is taken from Park et al. (2022) and the correction for Q is taken from

$\binom{\lfloor n/2 \rfloor + 1}{2}$ defines the order statistic used by the Q statistic. These are calculated separately by sex.

We calculate and plot $\tilde{\varepsilon}^{\text{Studentized}}_{g,t}$, $\tilde{\varepsilon}^{\text{MAD}}_{g,t}$, and $\tilde{\varepsilon}^{\text{Q}}_{g,t}$ for every LSAT administration against a threshold of $z_{0.025} = 1.96$ for the number of women (Appendix Figure 24b) and men (Appendix Figure 25b) taking the LSAT, the fraction of individuals taking the LSAT who are women (Appendix Figure 26b), the number of women (Appendix Figure 27b) and men (Appendix Figure 28b) registered to take the LSAT, the fraction of individuals registered to take the LSAT who are women (Appendix Figure 29b), and the fraction of women (Appendix Figure 30b) and men (Appendix Figure 31b) who registered for but did not take the LSAT. For comparison, the raw data used for these calculations are plotted in the first panel of each figure as well. We find little to suggest that there is an outsized change in women's LSAT taking behavior. Of note is an increase in the fraction of women who both register for (Appendix Figure 29) and take (Appendix Figure 26) the LSAT in November of 1968, which is consistent with the hypothesis that there is a policy-induced increase in demand for legal education from women but would not impact our estimated effects in September of 1968. We also find evidence that women are more likely to register for but not attend the LSAT in April of 1968; to the extent that this is evidence of a change in demand, an increased absence rate would imply a drop in demand for legal education, biasing our 1968 event coefficient downward. Last, though we are unsure why, there seems to be a notable drop in both the number of women (Appendix Figure 24) and men (Appendix Figure 25) taking the LSAT in November of 1969 such that fraction of test takers who are women (Appendix Figure 26) is unaffected. These patterns are also bourne out in our registration statistics, but in any case, are occurring far too late to influence our 1968 event coefficient.

Akinshin (2022).

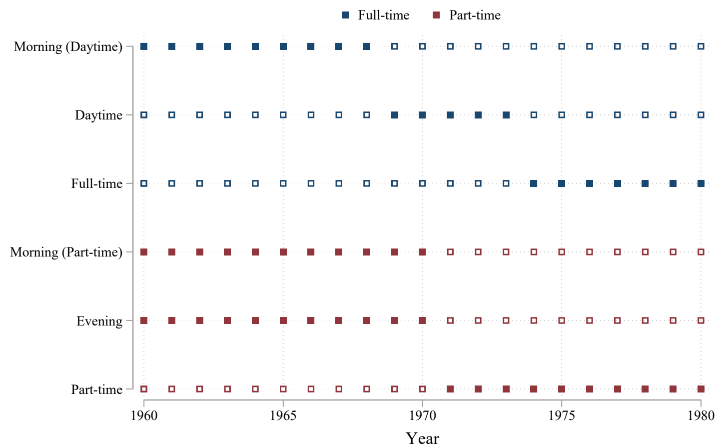
Appendix Figure 1: Raw *Review of Legal Education* Scans

Lincoln	University of Nebraska, College of Law (1923)				
	M	54(1)	29(1)	26	....
	M	....	14	5	10
	E	....	....	....	....
	M	4	0	1	1

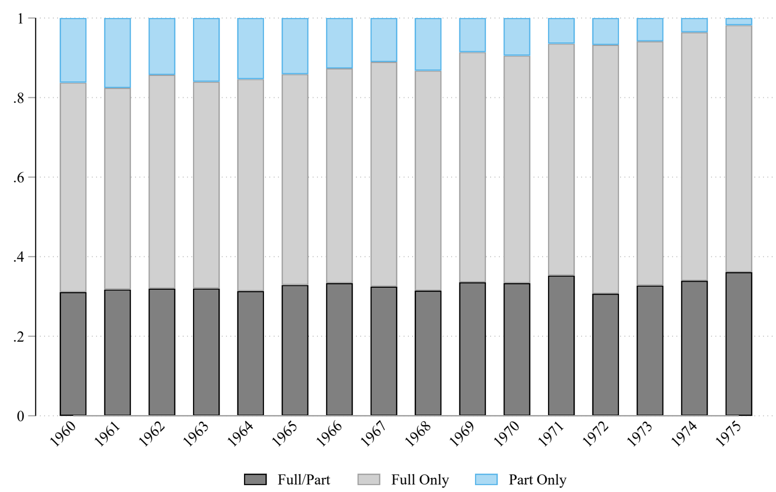
Appendix Figure 1 provides an example of the scans used to construct the RLE data.

Appendix Figure 2: Constructing *Review of Legal Education* Dataset

(a) Coding Full-Time and Part-Time



(b) Program Mix Across Law Schools



Appendix Figure 2 provides metadata on our dataset. Figure 2a demonstrates how we sort each year's differential coding of program types into our consistent full-time and part-time coding scheme. Using this scheme, Figure 2b shows the fraction of institutions in each year operating both a full-time and part0time program (Full/Part), only a full-time program (Full Only) and only a part-time program (Part Only).

Appendix Figure 3: Raw *Martindale-Hubbell* Scans

ALEXANDER CITY (Continued)

△ Dillon, John F., IV '30 '54 a v 1 g
C&L.16 B.S., LL.B. [W. W. & D.]
△ Frohsin, Henry I. ....'43 '67 C&L.16 B.S., LL.B. ★
△ Frohsin, Ralph, Jr. '40 '64 C&L.16 B.S., LL.B. Frohsin's

King, Ruben K. '29 '57 b v 1 g C.10 L.16 LL.B.
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General Civil Practice. Trials in all State and Federal Courts. Corporation, Real Estate, Probate, Insurance, Personal Injury and Criminal Law.
References: Alexander City Bank; First National Bank of Alexander City.

Lonergan, Ronald C. '28 '55
C&L.16 B.S., LL.B. State Farm Mut. Ins. Co.
Morris, Larry W. '43 '68 C.10 B.S. L.16 J.D. [Ⓐ T. Radney]

Radney, Tom '32 '55 b v 1 g C.10 B.S. L.16 LL.B.
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Associate: Larry W. Morris.
General Civil Practice. Personal Injury and Criminal Law. Trials.
Representative Clients: First National Bank; Alabama Investments Securities, Inc.; Glegg Manufacturing Co.; City of Alexander City; Town of Wadley; Russell Mills, Inc.
Approved Attorney for: Lawyers Title Insurance Corp.; Mississippi Valley Title Insurance Co.
See Biographical Section, page 2B

△ Reynolds, H. Gerald ....'40 '65 C.10 B.A. L.192 J.D.
Wilbanks, Elizabeth Johnson '13 '36 a v 1 g
C&L.16 B.A., LL.B. [W. W. & D.]
△ Wilbanks, Sim S. '11 '36 a v 1 g
C.245 L.16 LL.B. [W. W. & D.]

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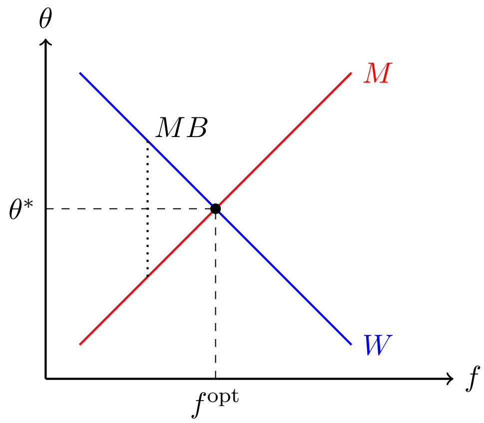
Sim S. Wilbanks; Elizabeth J. Wilbanks; John F. Dillon, IV.
General Civil Practice in all State and Federal Courts. Trials. Interstate Commerce Commission, Probate, Insurance, Banking, Corporation and Real Estate Law.
Clients: First National Bank; City Bank of Tuskegee, Tuskegee, Alabama; City Bank & Trust Co., Roanoke, Alabama; Peoples Trust & Savings Bank; Southern Railway System; Central of Georgia Railway Co. (Trial Counsel); United States Fidelity & Guaranty Co.; Continental Insurance Co.; Liberty National Life Insurance Co.; Protective Life Insurance Co.; South Central Bell Telephone & Telegraph Co.; The Prudential Insurance Company of America; Home Indemnity Co.; Continental Crescent Lines, Inc.; The Farmers & Merchants Bank; Southeastern Fire Insurance Co.
References: Citizens & Southern National Bank, Atlanta, Georgia; First National Bank, Alexander City, Alabama.
See Biographical Section, page 2B

Young, Tom F. ...'18 '49 c v 6 f C.10 L.16 LL.B. Dist. Atty.

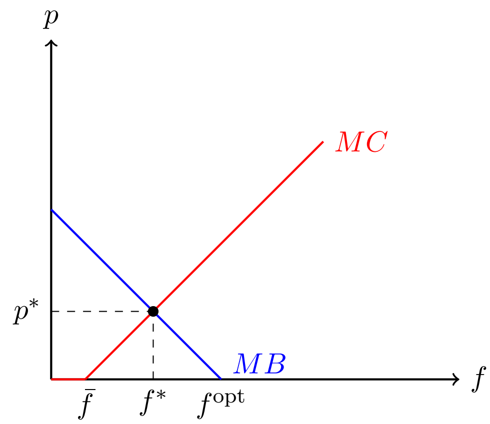
Appendix Figure 3 provides an example of the scans used to construct the Martindale-Hubbell-based data.

Appendix Figure 4: Model of Law School Admissions

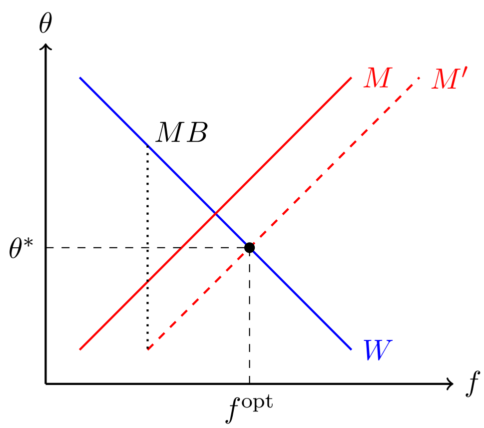
(a) Marginal Benefit: Before Policy



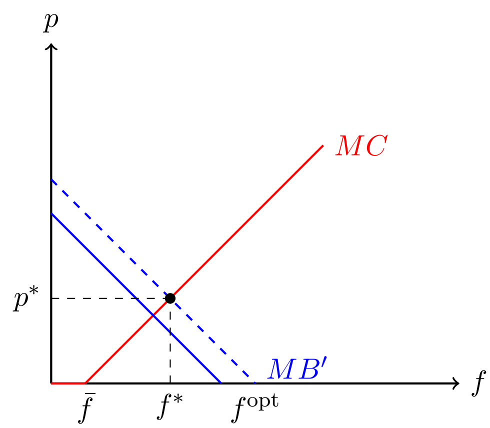
(b) Optimal Enrollment: Before Policy



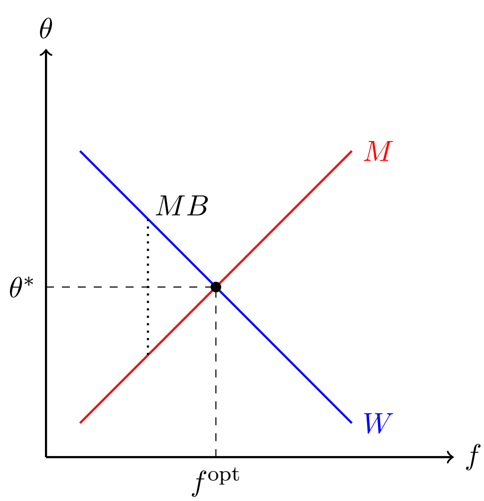
(c) Marginal Benefit: Policy Change



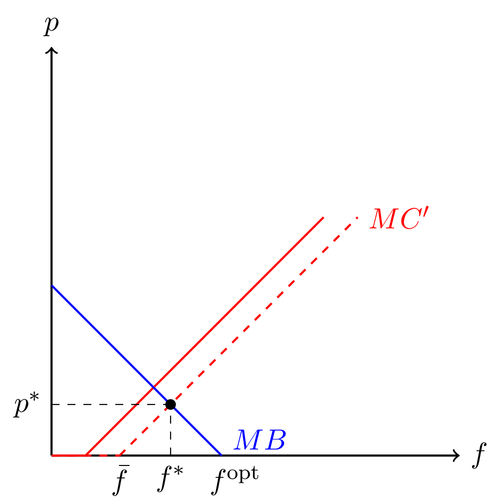
(d) Optimal Enrollment: Policy Change



(e) Marginal Benefit: After Policy

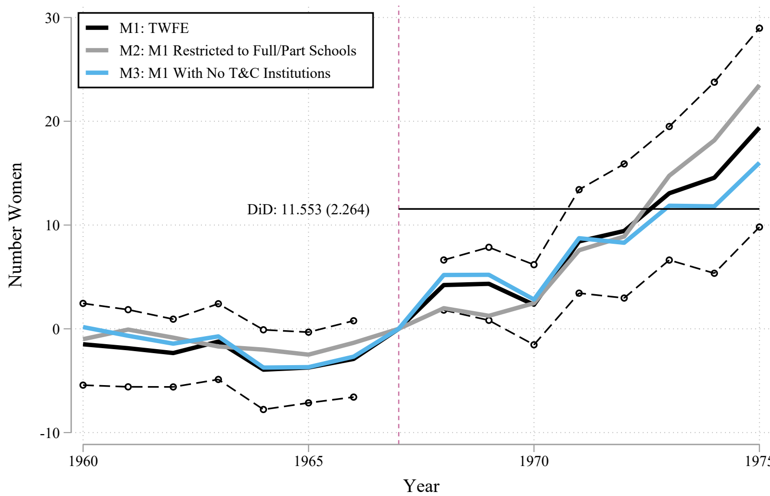


(f) Optimal Enrollment: After Policy



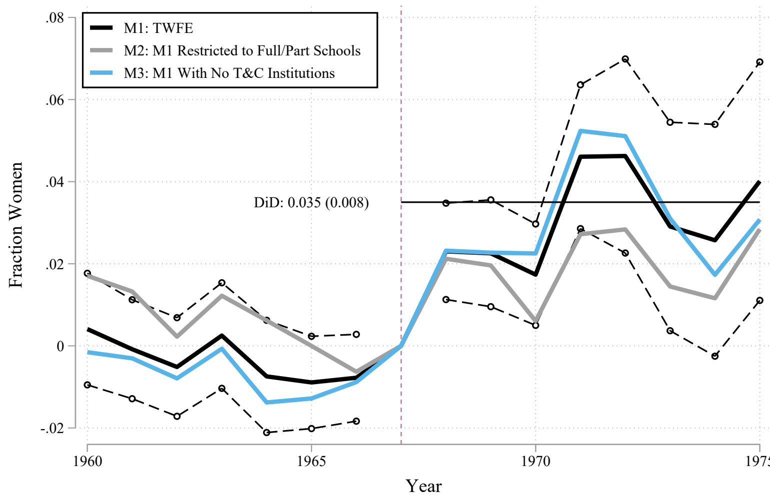
Appendix Figure 4 provides a graphical illustration of our model of law school admissions. See Appendix Figure 4 for details.

Appendix Figure 5: Robustness to Entropy Weighting

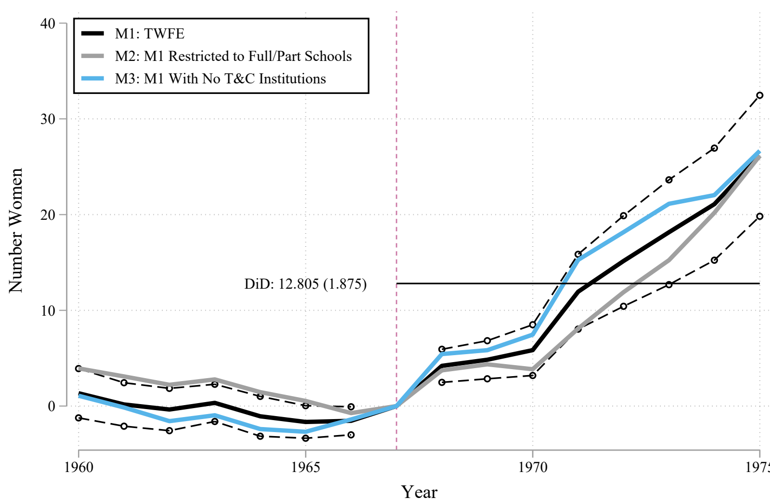


Appendix Figure 5 plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women and the enrollment control is omitted. Instead, we estimate entropy weights in 1967 such that weighted average total enrollment is equal in the treatment and control groups (Hainmueller 2012). Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program. Entropy weights are constructed separately for each model to ensure proper balance.

Appendix Figure 6: Robustness to State-by-Year Fixed Effects



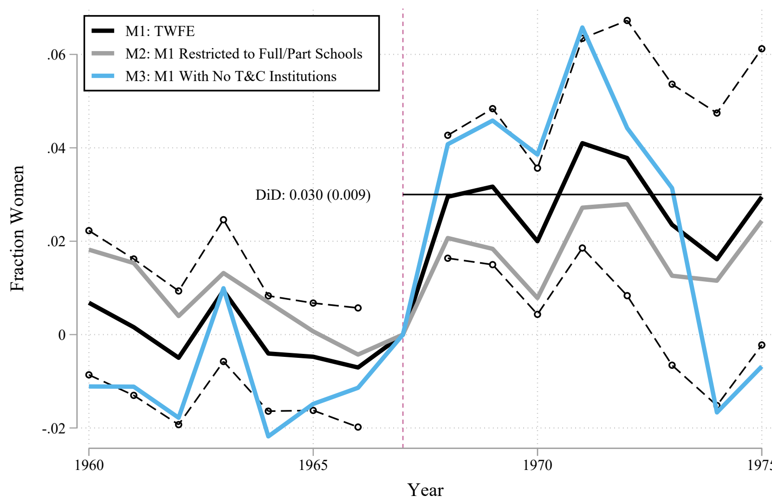
(a) Fraction Women



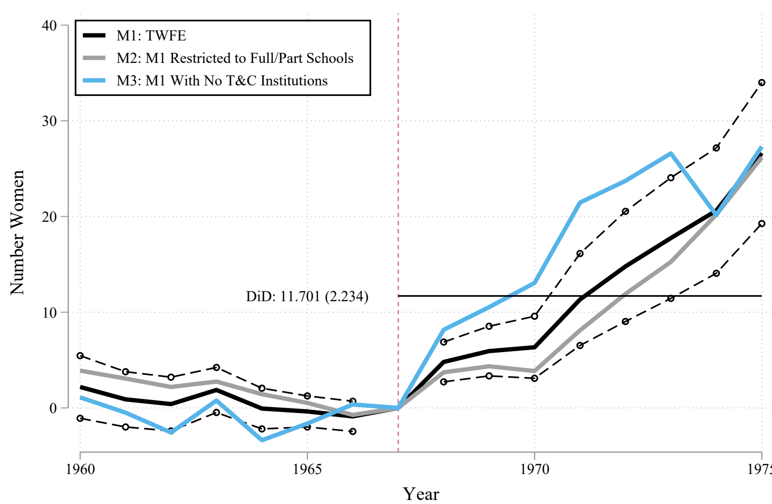
(b) Number Women

Appendix Figure 6a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Model 1 includes institution-by-program fixed effects and state-by-year fixed effects, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 6b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 includes institution-by-program fixed effects and state-by-year fixed effects, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Appendix Figure 7: Robustness to City-by-Year Fixed Effects



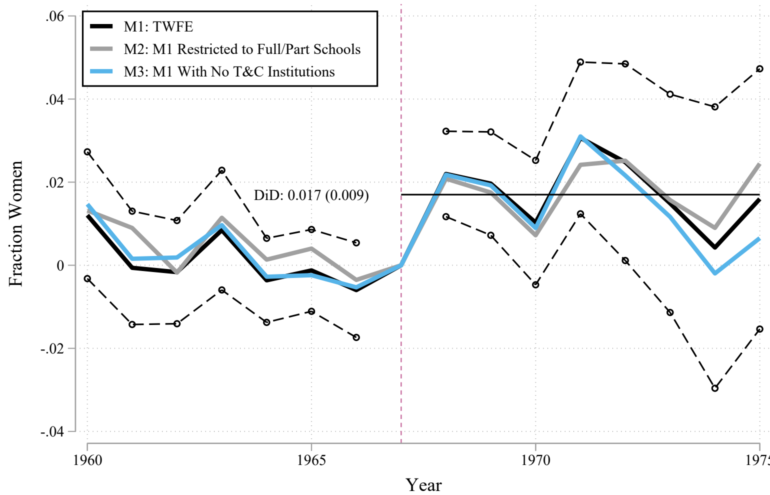
(a) Fraction Women



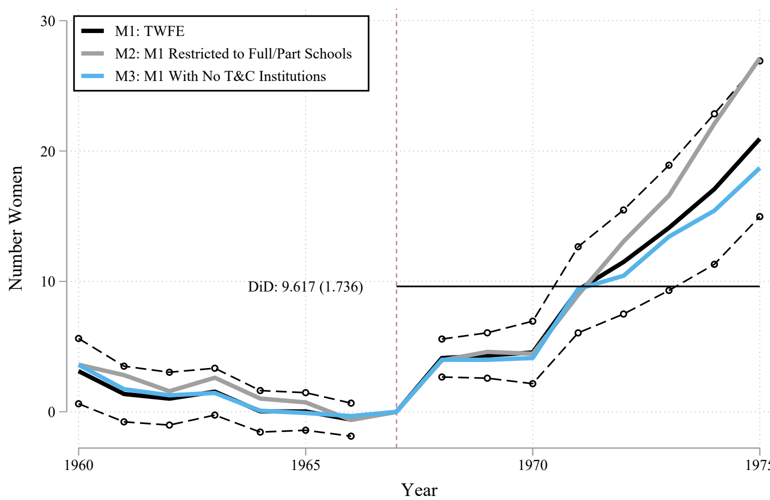
(b) Number Women

Appendix Figure 7a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Model 1 includes institution-by-program fixed effects and city-by-year fixed effects, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 7b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 includes institution-by-program fixed effects and city-by-year fixed effects, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Appendix Figure 8: Restricting to ABA-Approved Schools



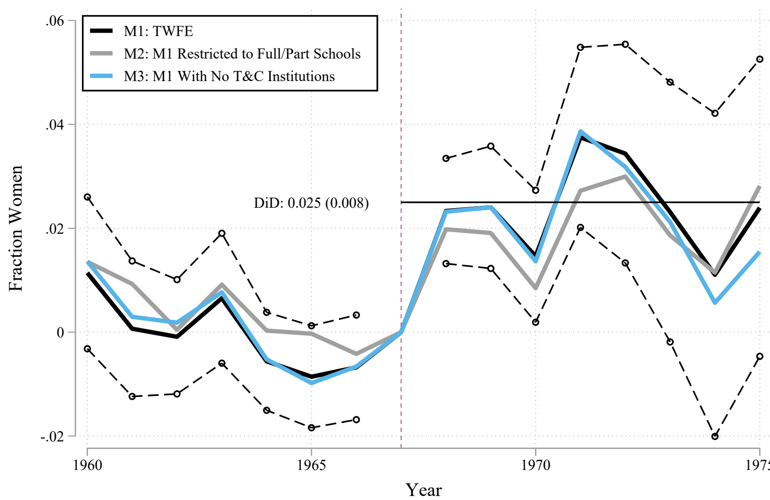
(a) Fraction Women



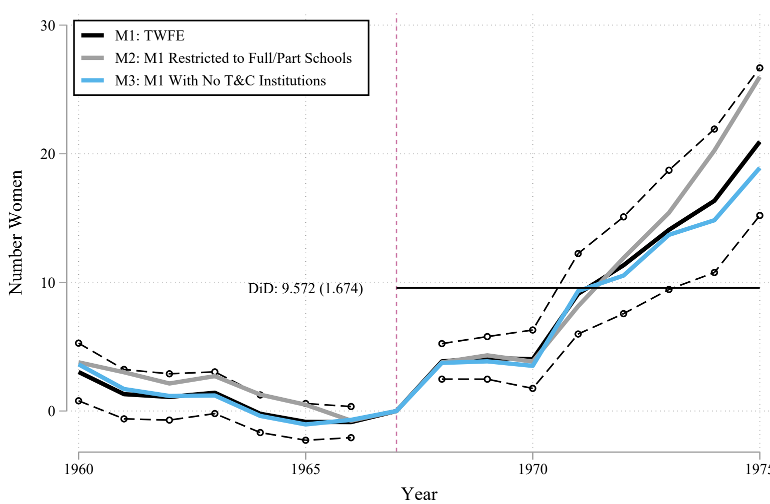
(b) Number Women

Appendix Figure 8a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. The sample in each year is restricted to the set of schools that are ABA-Approved. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 8b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Appendix Figure 9: Restricting to Schools ABA-Approved by 1975



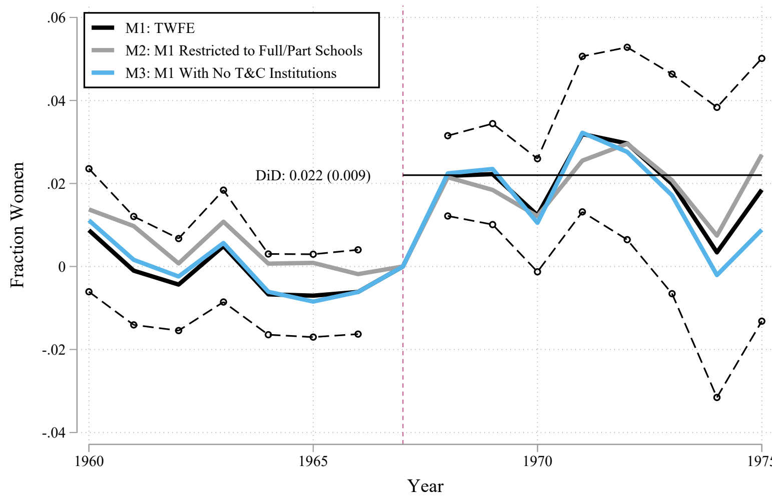
(a) Fraction Women



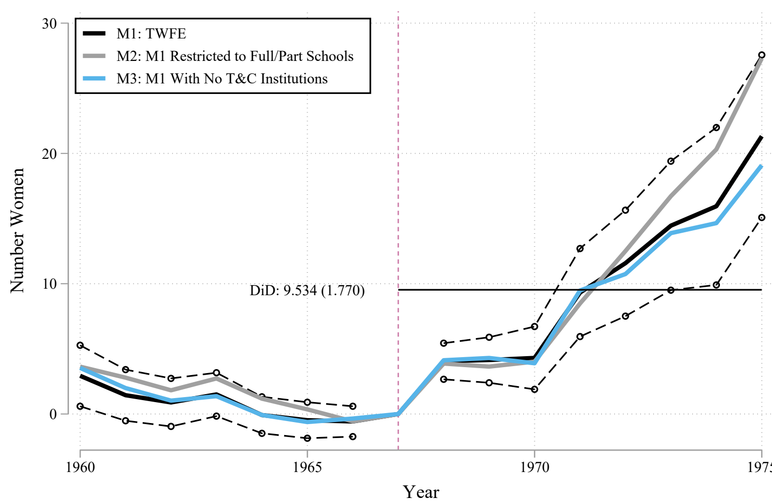
(b) Number Women

Appendix Figure 9a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. The sample in each year is restricted to the set of schools that receive ABA-Approval by 1975, the end of our sample period. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 9b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Appendix Figure 10: Institution-Balanced Panel



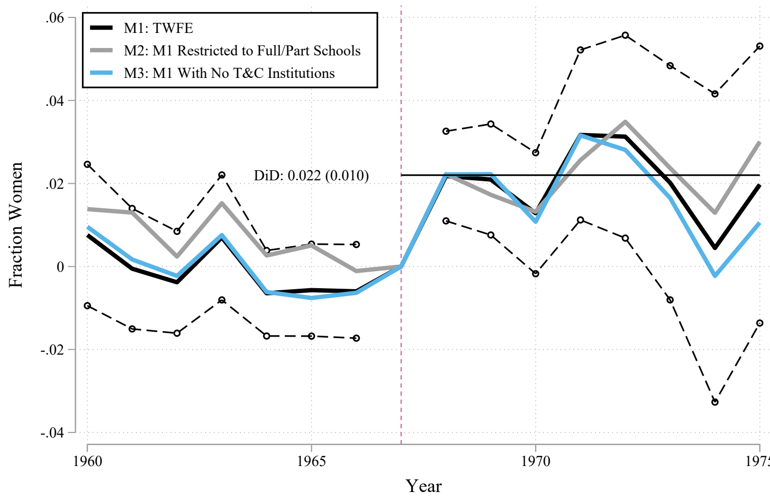
(a) Fraction Women



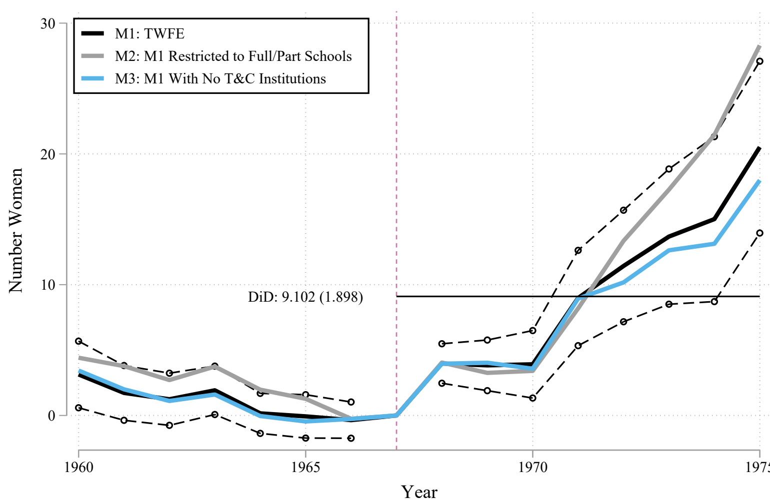
(b) Number Women

Appendix Figure 10a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. The sample is restricted to a panel balanced at the institution level, so that every institution included (regardless of program type offered) appears in every year. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 10b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Appendix Figure 11: Institution-Program-Balanced Panel



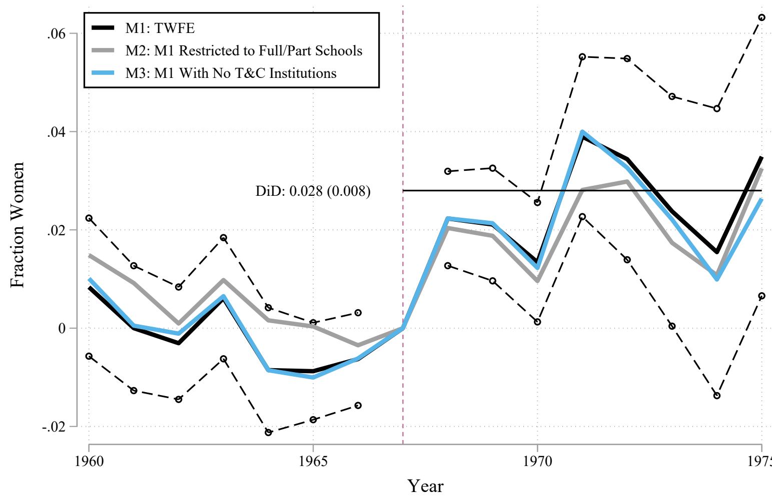
(a) Fraction Women



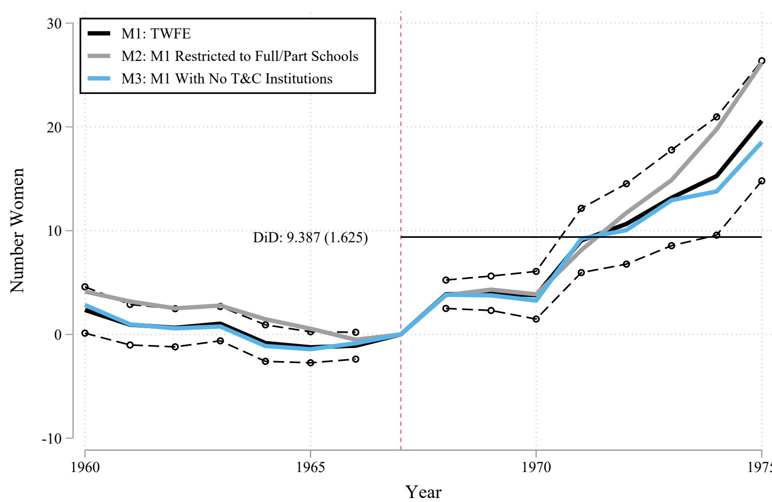
(b) Number Women

Appendix Figure 11a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. The sample is restricted to a panel balanced at the institution-program level. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 11b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Appendix Figure 12: Restricting to Institution-Program-Years with at least 20 Students



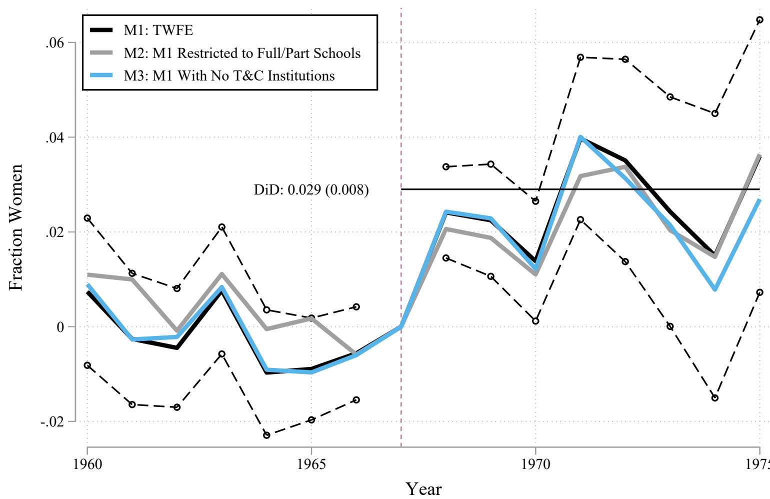
(a) Fraction Women



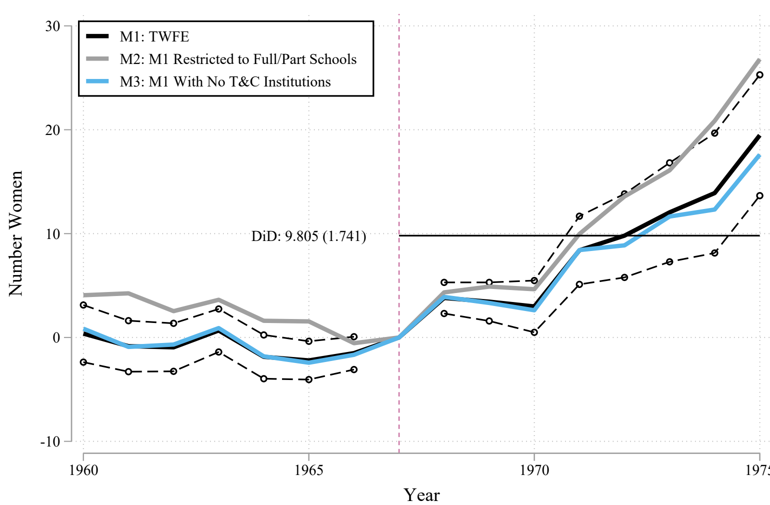
(b) Number Women

Appendix Figure 12a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. The sample is restricted to institution-programs-years with at least 20 students enrolled. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 12b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Appendix Figure 13: Restricting to Institution-Program-Years with at least 50 Students



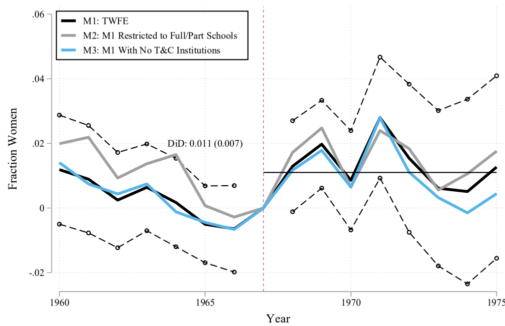
(a) Fraction Women



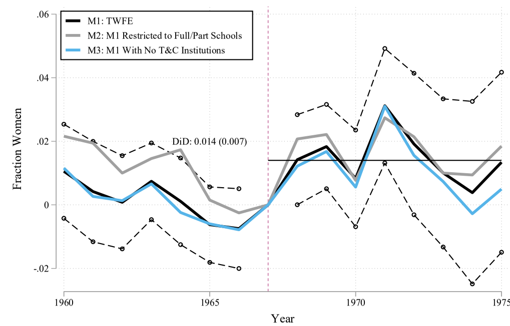
(b) Number Women

Appendix Figure 13a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. The sample is restricted to institution-programs-years with at least 50 students enrolled. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 13b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

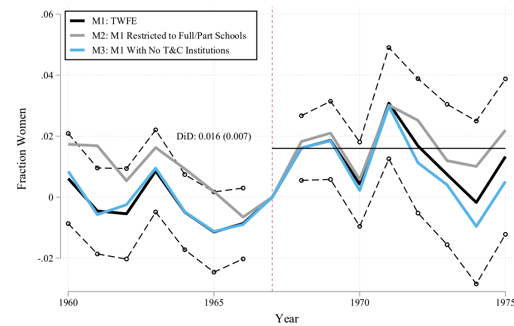
Appendix Figure 14: No Weighting



(a) Fraction Women (L1 10+)



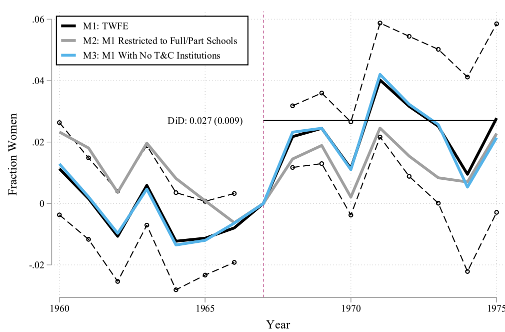
(b) Fraction Women (L1 20+)



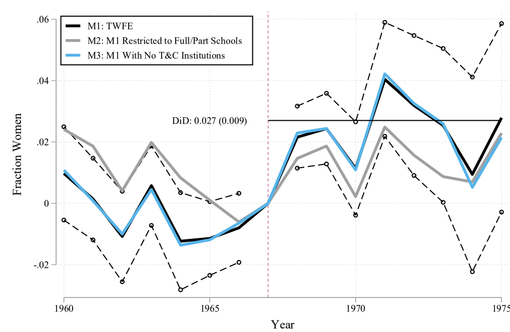
(c) Fraction Women (L1 50+)

Appendix Figure 14a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program. Appendix Figure 14b replicates this figure estimated on a sample is restricted to institution-programs-years with at least 20 students enrolled. Appendix Figure 14c replicates this figure estimated on a sample is restricted to institution-programs-years with at least 50 students enrolled.

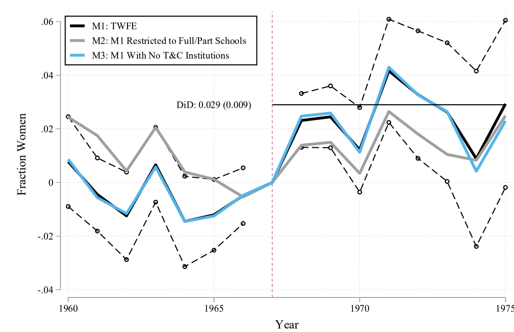
Appendix Figure 15: 1967 Weights



(a) Fraction Women (L1 10+)



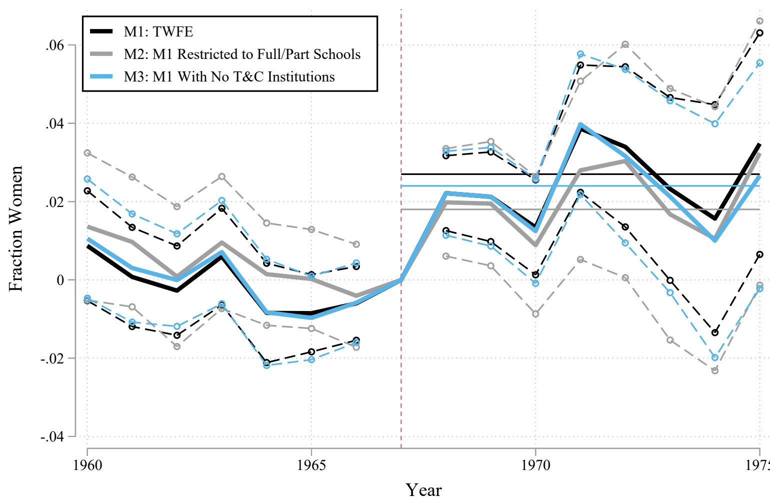
(b) Fraction Women (L1 20+)



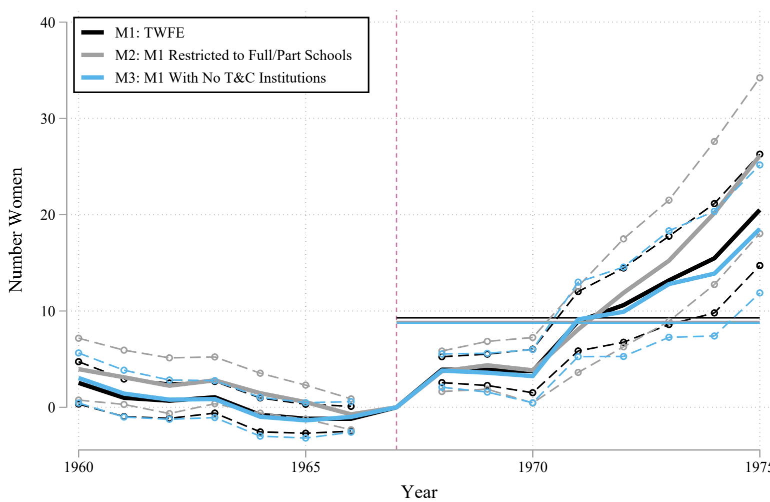
(c) Fraction Women (L1 50+)

Appendix Figure 15a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment in 1967. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program. Appendix Figure 15b replicates this figure estimated on a sample is restricted to institution-programs-years with at least 20 students enrolled. Appendix Figure 15c replicates this figure estimated on a sample is restricted to institution-programs-years with at least 50 students enrolled.

Appendix Figure 16: Clustering at the Institution-Program Level



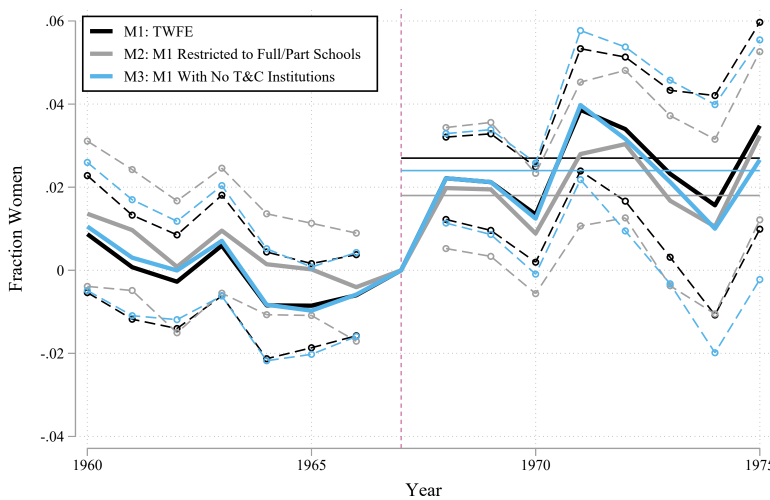
(a) Fraction Women



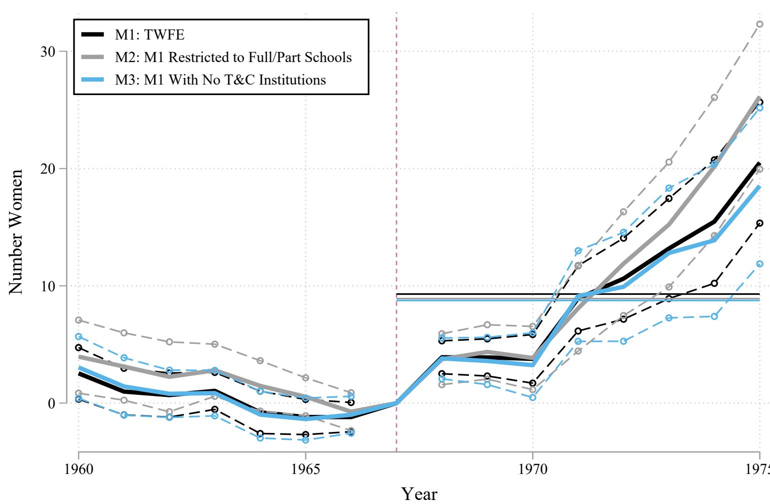
(b) Number Women

Appendix Figure 16a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Appendix Figure 16b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. For both figures, Model 1 uses a standard two-way fixed-effects design, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program, and Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program. For all models, a 95% confidence interval is plotted for every event coefficient, where standard errors are clustered at the institution-program level. The horizontal lines plot the difference-in-differences estimate from each model, estimated with equation (2) (Appendix Figure 16a) or equation (4) (Appendix Figure 16b).

Appendix Figure 17: Clustering at the Institution Level



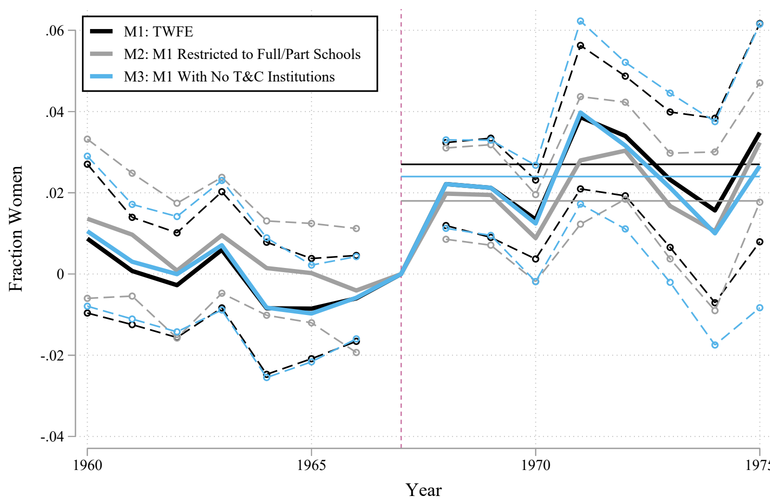
(a) Fraction Women



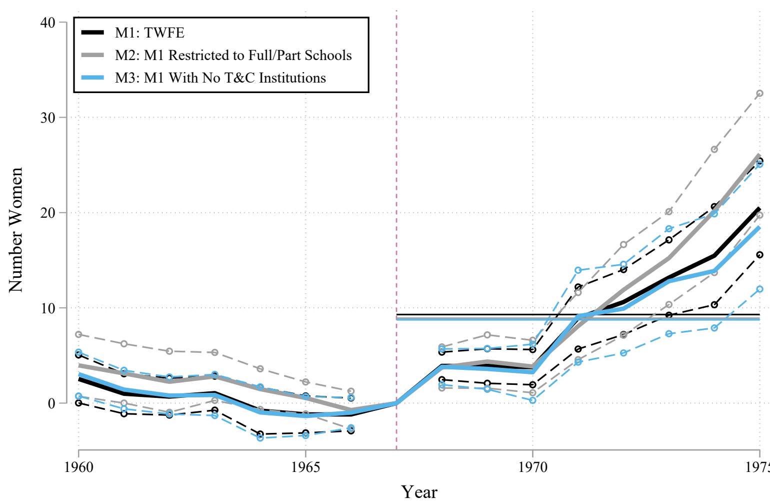
(b) Number Women

Appendix Figure 17a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Appendix Figure 17b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. For both figures, Model 1 uses a standard two-way fixed-effects design, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program, and Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program. For all models, a 95% confidence interval is plotted for every event coefficient, where standard errors are clustered at the institution level. The horizontal lines plot the difference-in-differences estimate from each model, estimated with equation (2) (Appendix Figure 17a) or equation (4) (Appendix Figure 17b).

Appendix Figure 18: Clustering at the State Level



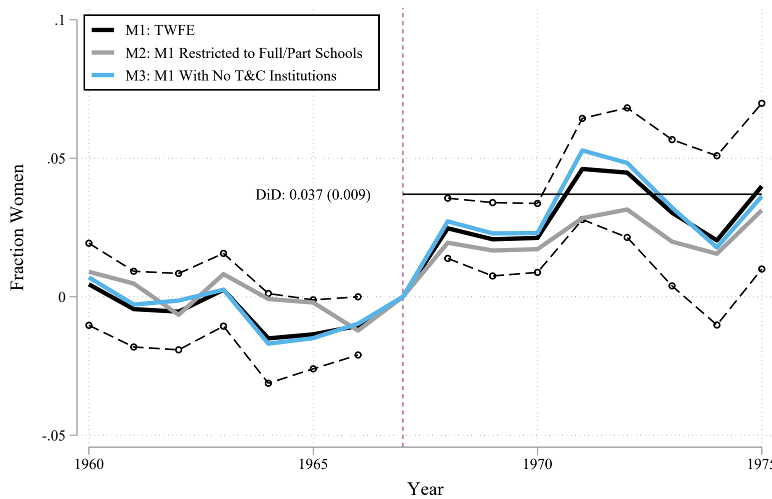
(a) Fraction Women



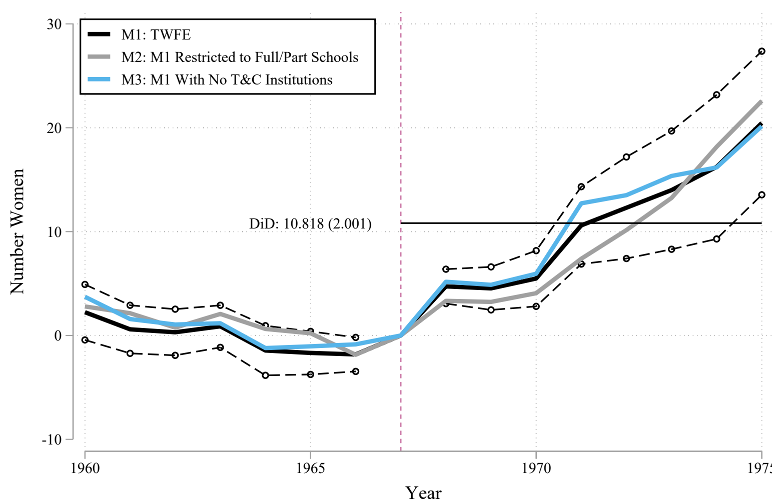
(b) Number Women

Appendix Figure 18a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Appendix Figure 18b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. For both figures, Model 1 uses a standard two-way fixed-effects design, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program, and Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program. For all models, a 95% confidence interval is plotted for every event coefficient, where standard errors are clustered at the state level. The horizontal lines plot the difference-in-differences estimate from each model, estimated with equation (2) (Appendix Figure 18a) or equation (4) (Appendix Figure 18b).

Appendix Figure 19: Excluding the South



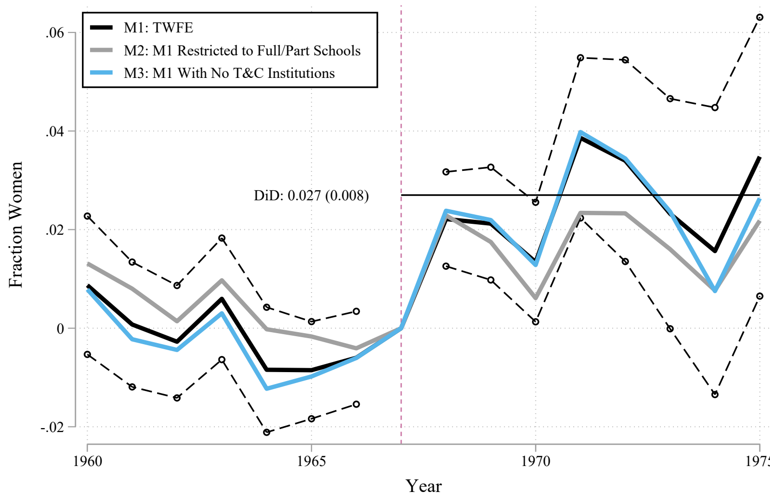
(a) Fraction Women



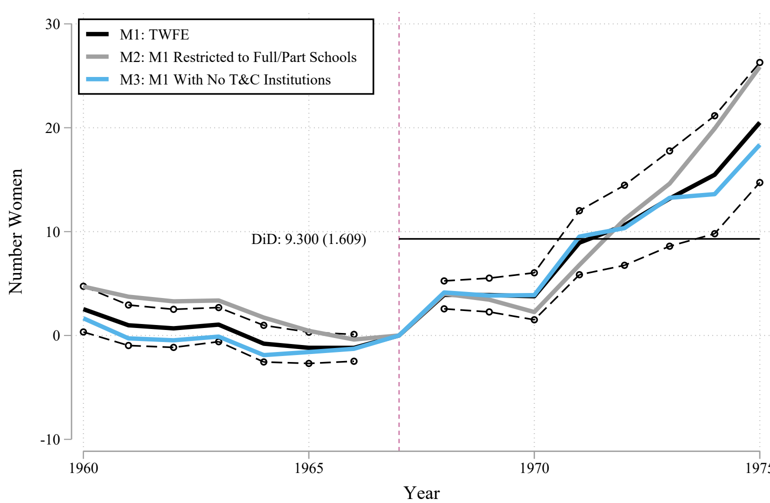
(b) Number Women

Appendix Figure 19a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. The sample is restricted to institutions not located in the Southern Census Region. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 19b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program.

Appendix Figure 20: Fixing Program Mix at 1967 Value



(a) Fraction Women

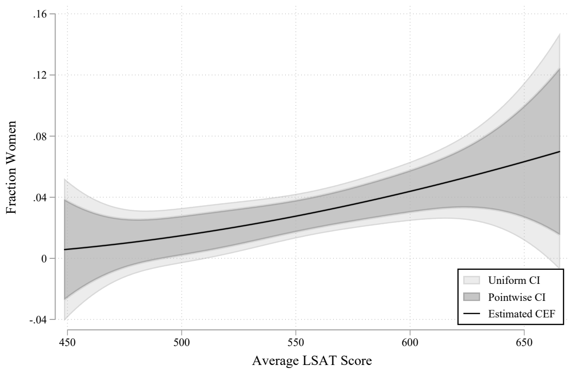


(b) Number Women

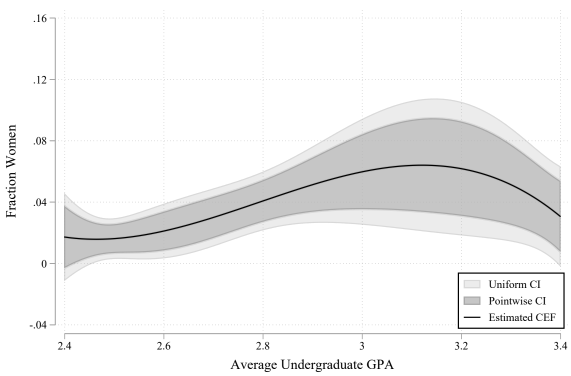
Appendix Figure 20a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 20b plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (4). In both figures, Model 2 restricts our sample to include only institutions operating both a full-time and a part-time program. Model 3 restricts the treatment group to institutions operating only a full-time program and the control group to any institution operating a part-time program. Assignment across all models is fixed to an institution's 1967 program mix.

Appendix Figure 21: Alternative Continuous Differences-in-Differences Results

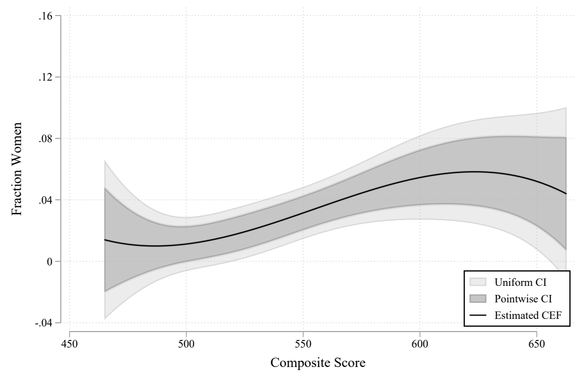
(a) Average LSAT Score



(b) Average Undergraduate GPA

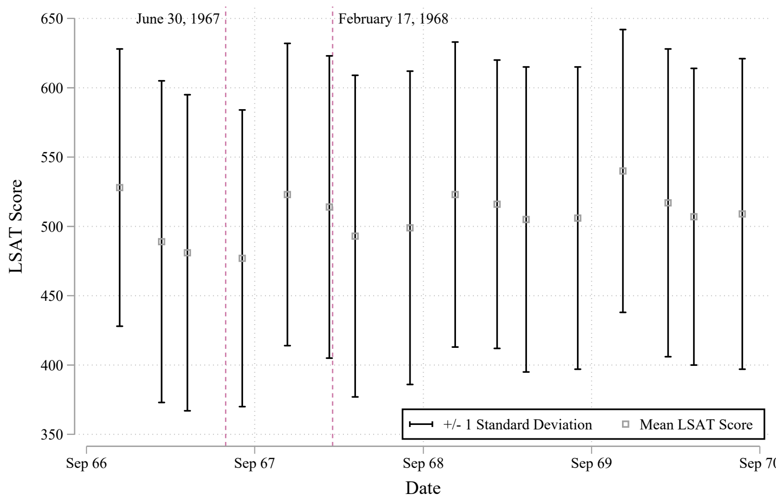


(c) Composite Score

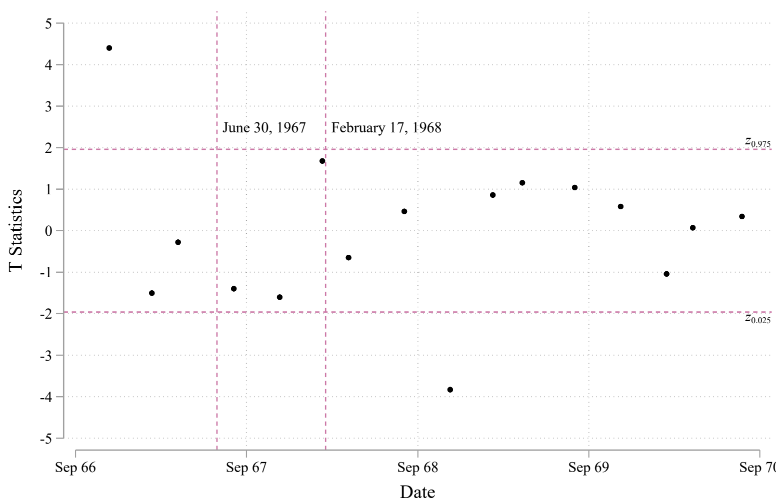


This figure plots estimates from equation (6), where the outcome is the change in the fraction of first-year students who are women between 1967 and 1968 and the regression is weighted by total first-year enrollment in 1967. The dose variable is imputed average LSAT score for the 1967-68 incoming class in Appendix Figure 21a, imputed average undergraduate GPA for the 1967-68 incoming class in Appendix Figure 21b, and the composite score for the 1967-68 incoming class in Appendix Figure 21c, where the composite score is computed as $(1/2) * \text{LSAT} + (1/2) * (200 * \text{UGPA})$. For each figure, after estimating regression coefficients, we construct a grid of 500 points evenly spaced between the minimum and maximum dose values and plot $\hat{Y}_{i\rho}$ for treated units ($D_{i\rho} = 1$) at each of these dose values. We also plot two sets of 95% confidence bands across our grid: a pointwise confidence interval using the standard error of the predicted value at a given level of the dose, as well as a uniform confidence interval across the entire grid using the method proposed in Montiel Olea and Plagborg-Møller (2019).

Appendix Figure 22: Women's LSAT Scores



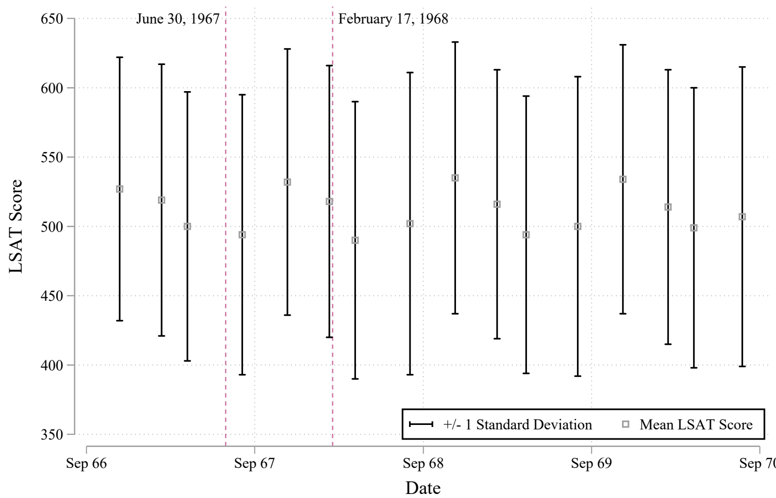
(a) Raw Data



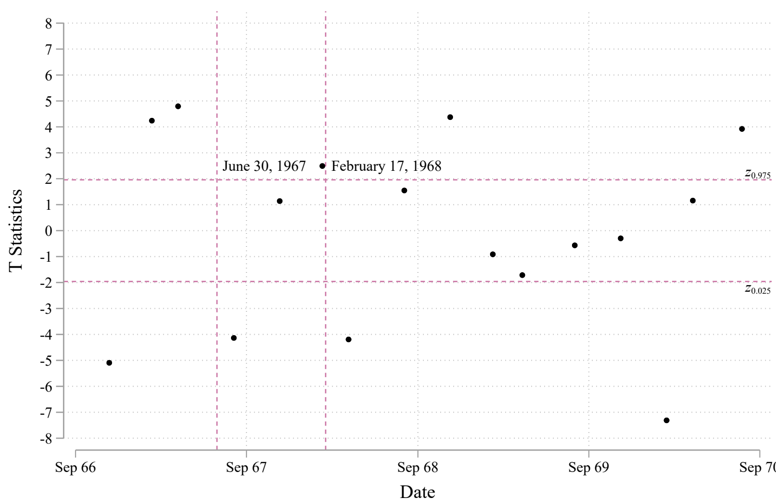
(b) Statistical Tests

Appendix Figure 22a plots the raw aggregate data on women's LSAT scores. On the day of each exam administration, we plot the mean as well as bars indicating +/- 1 standard deviation from the mean. Appendix Figure 22b plots the z-statistic estimated from a statistical test of whether or not the detrended mean scores are statistically different from 0. The horizontal lines indicate the critical values for a two-sided test with a 5% significance level. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

Appendix Figure 23: Men's LSAT Scores



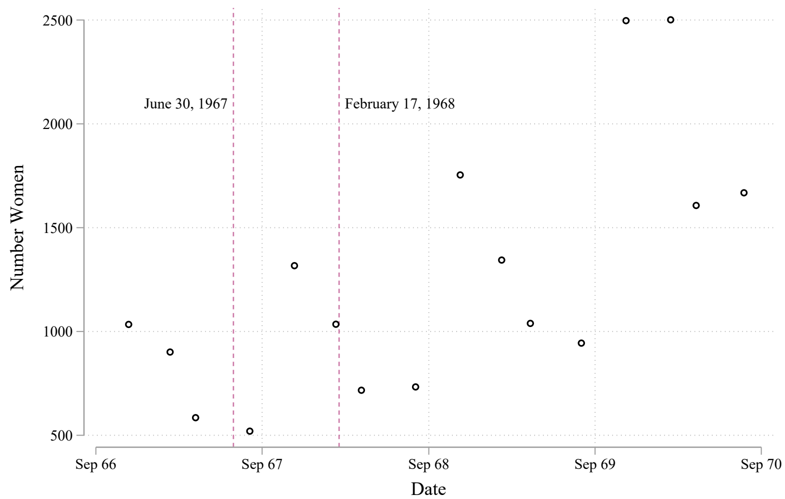
(a) Raw Data



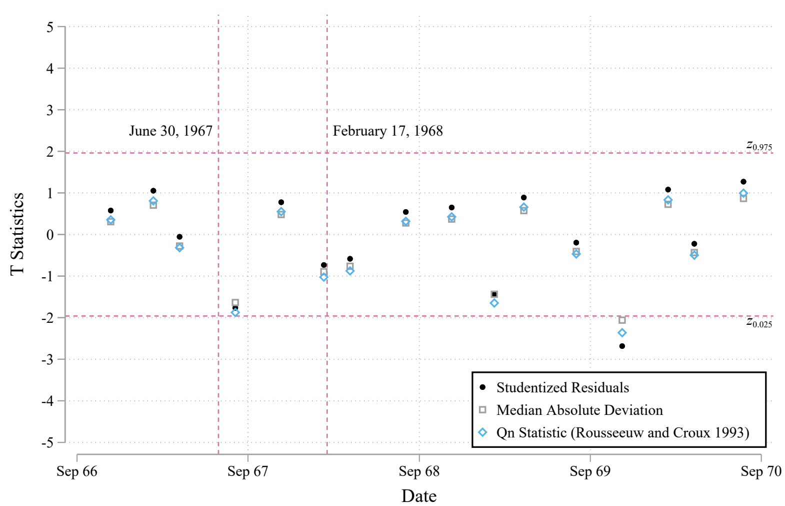
(b) Statistical Tests

Appendix Figure 23a plots the raw aggregate data on men's LSAT scores. On the day of each exam administration, we plot the mean as well as bars indicating +/- 1 standard deviation from the mean. Appendix Figure 23b plots the z-statistic estimated from a statistical test of whether or not the detrended mean scores are statistically different from 0. The horizontal lines indicate the critical values for a two-sided test with a 5% significance level. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

Appendix Figure 24: Number of Women Taking the LSAT



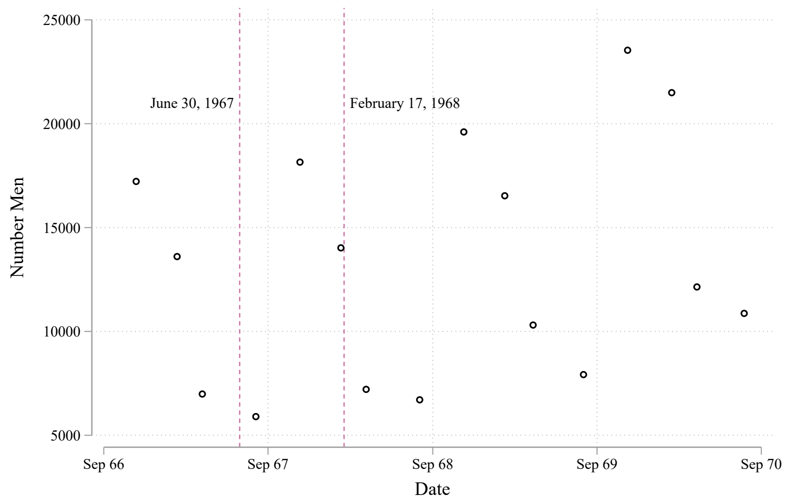
(a) Raw Data



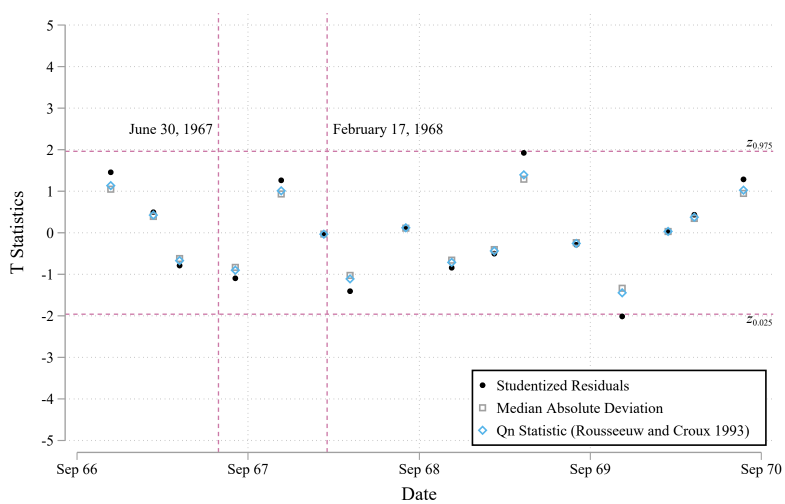
(b) Statistical Tests

Appendix Figure 24a plots the raw number of women who sat for each LSAT exam on the day of each exam administration. Appendix Figure 24b plots the studentized residual of the log of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equation (B.2), (B.3), and (B.4), respectively. The horizontal lines indicate the critical values for a two-sided z-test with a 5% significance level, which we use as our threshold for an outlier value. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

Appendix Figure 25: Number of Men Taking the LSAT



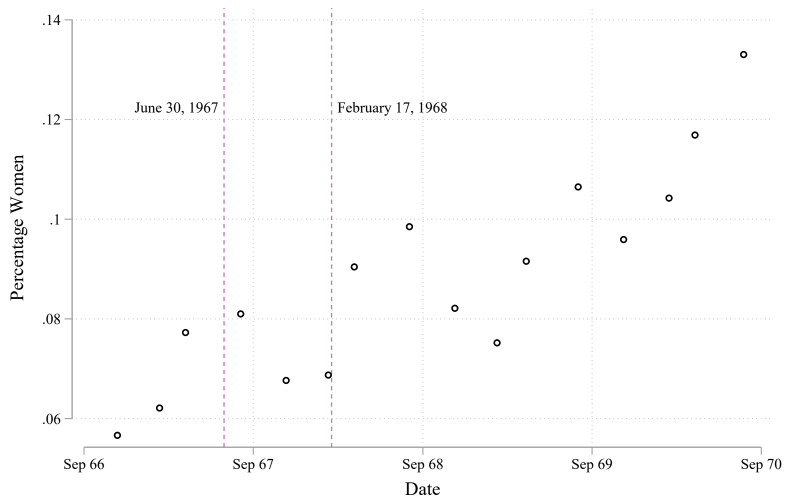
(a) Raw Data



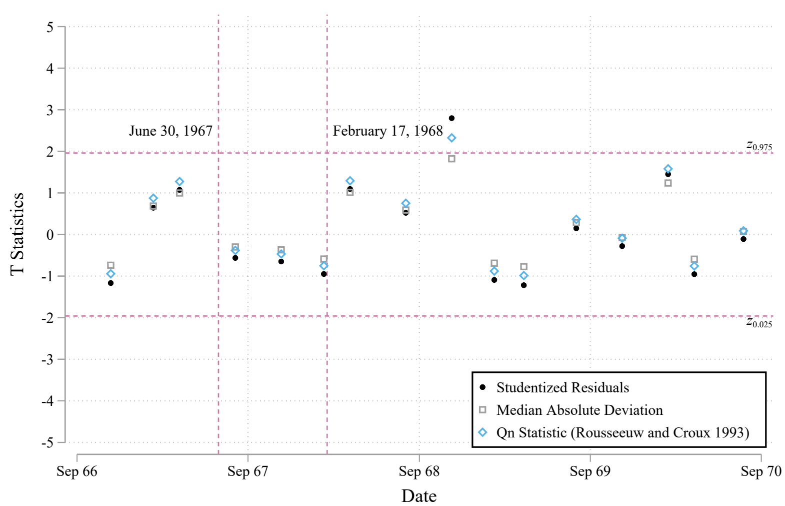
(b) Statistical Tests

Appendix Figure 25a plots the raw number of men who sat for each LSAT exam on the day of each exam administration. Appendix Figure 25b plots the studentized residual of the log of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equation (B.2), (B.3), and (B.4), respectively. The horizontal lines indicate the critical values for a two-sided z-test with a 5% significance level, which we use as our threshold for an outlier value. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

Appendix Figure 26: Fraction of Students Taking the LSAT who are Women



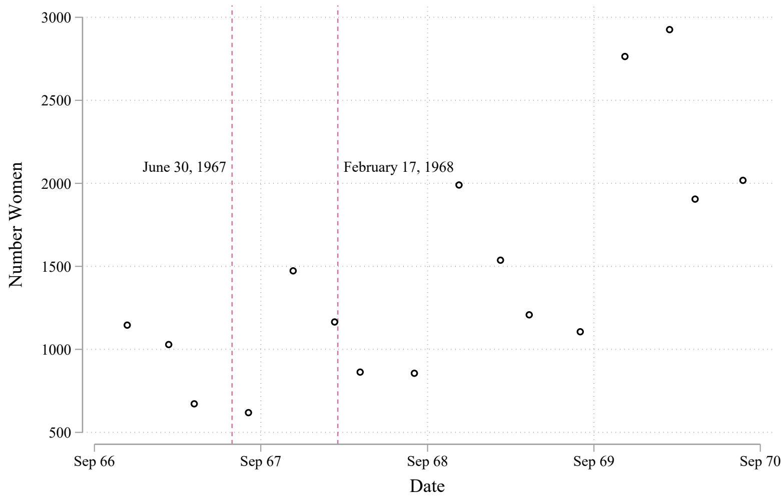
(a) Raw Data



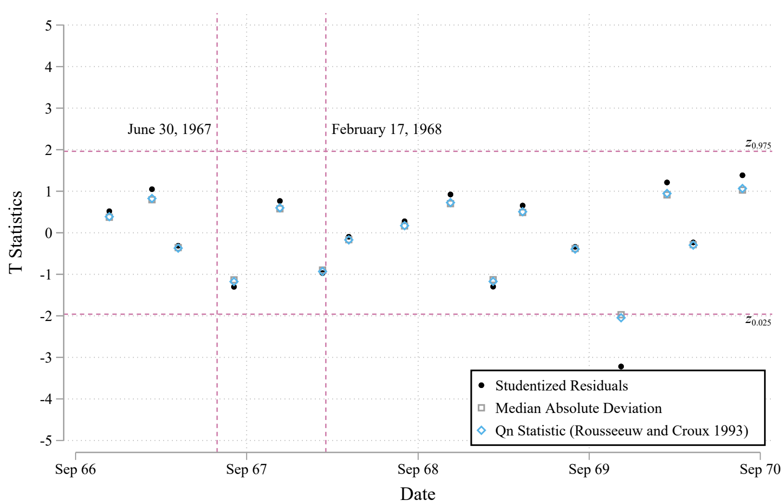
(b) Statistical Tests

Appendix Figure 26a plots the raw fraction of students who sat for each LSAT exam who were women on the day of each exam administration. Appendix Figure 26b plots the studentized residual of the logit transformation of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equation (B.2), (B.3), and (B.4), respectively. The horizontal lines indicate the critical values for a two-sided z-test with a 5% significance level, which we use as our threshold for an outlier value. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

Appendix Figure 27: Number of Women Registered to Take the LSAT



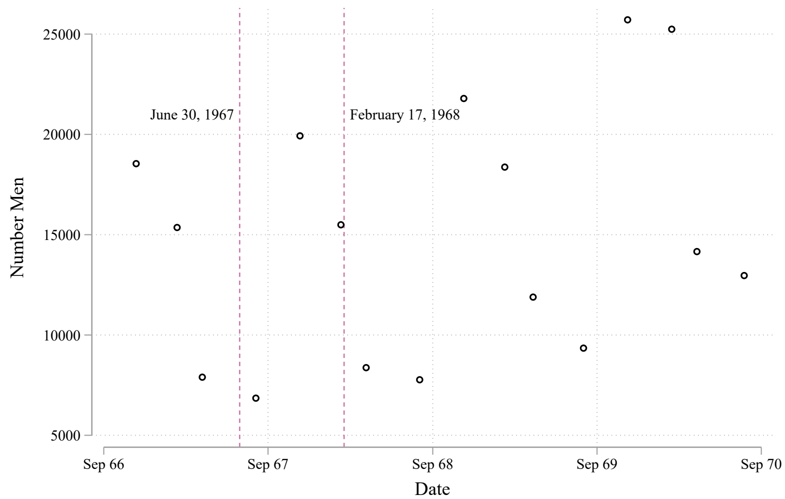
(a) Raw Data



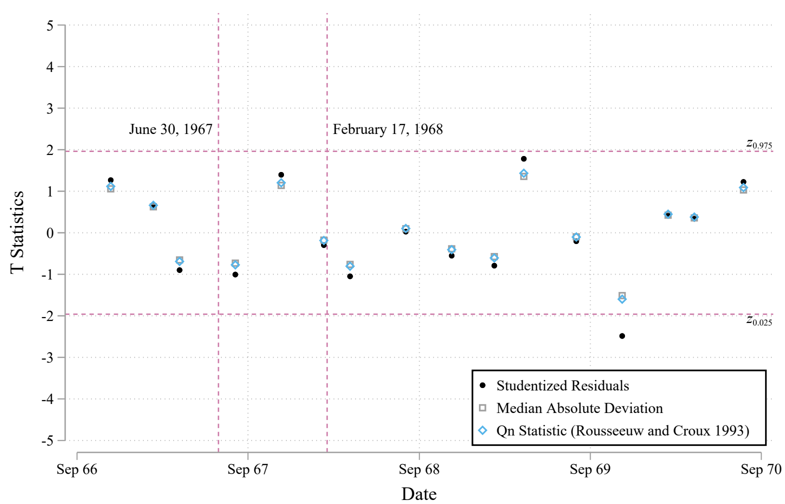
(b) Statistical Tests

Appendix Figure 27a plots the raw number of women who registered for each LSAT exam on the day of each exam administration. Appendix Figure 27b plots the studentized residual of the log of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equation (B.2), (B.3), and (B.4), respectively. The horizontal lines indicate the critical values for a two-sided z-test with a 5% significance level, which we use as our threshold for an outlier value. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

Appendix Figure 28: Number of Men Registered to Take the LSAT



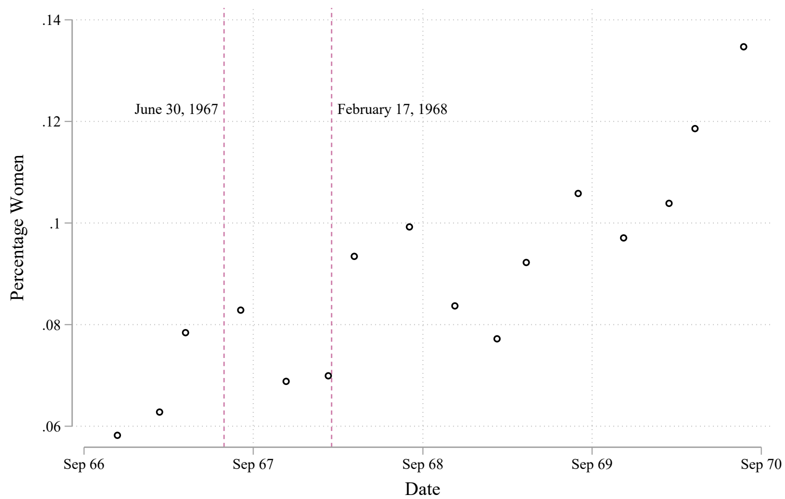
(a) Raw Data



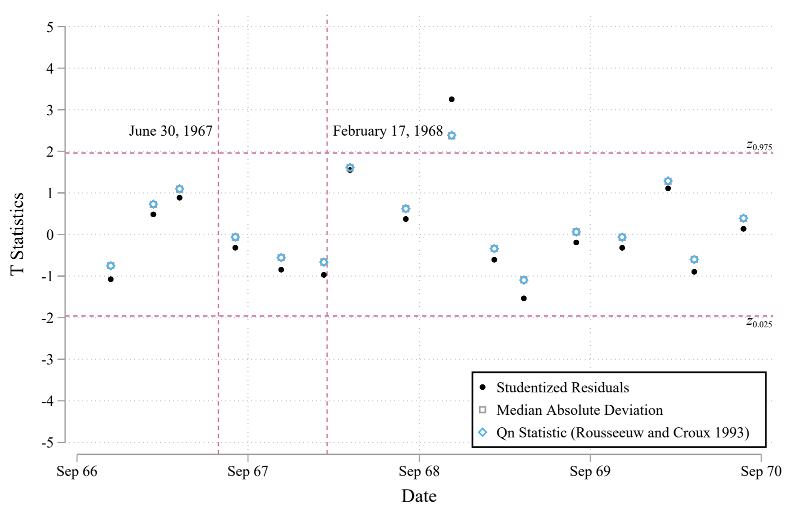
(b) Statistical Tests

Appendix Figure 28a plots the raw number of men who registered for each LSAT exam on the day of each exam administration. Appendix Figure 28b plots the studentized residual of the log of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equation (B.2), (B.3), and (B.4), respectively. The horizontal lines indicate the critical values for a two-sided z-test with a 5% significance level, which we use as our threshold for an outlier value. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

Appendix Figure 29: Fraction of Students Registered to Take the LSAT who are Women



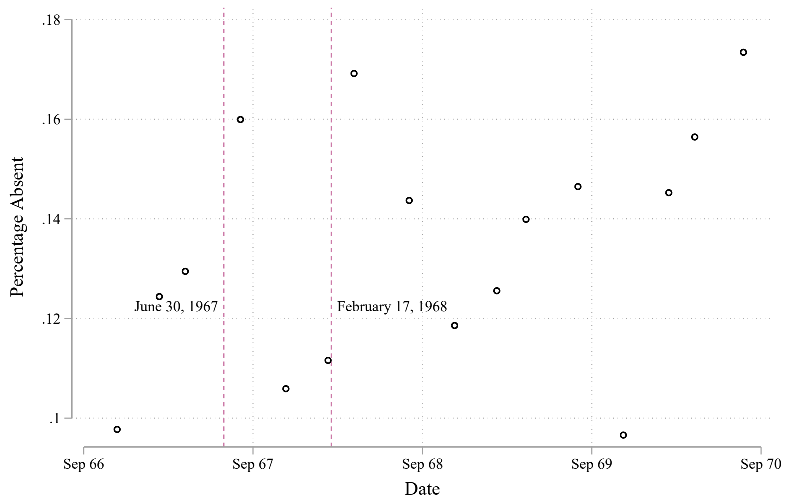
(a) Raw Data



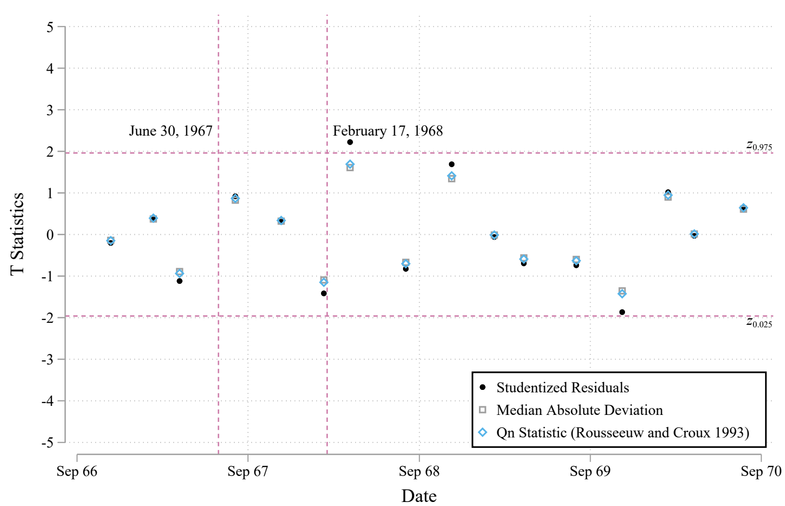
(b) Statistical Tests

Appendix Figure 29a plots the raw fraction of students who registered for each LSAT exam who were women on the day of each exam administration. Appendix Figure 29b plots the studentized residual of the logit transformation of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equation (B.2), (B.3), and (B.4), respectively. The horizontal lines indicate the critical values for a two-sided z-test with a 5% significance level, which we use as our threshold for an outlier value. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

Appendix Figure 30: Fraction of Women Absent from LSAT



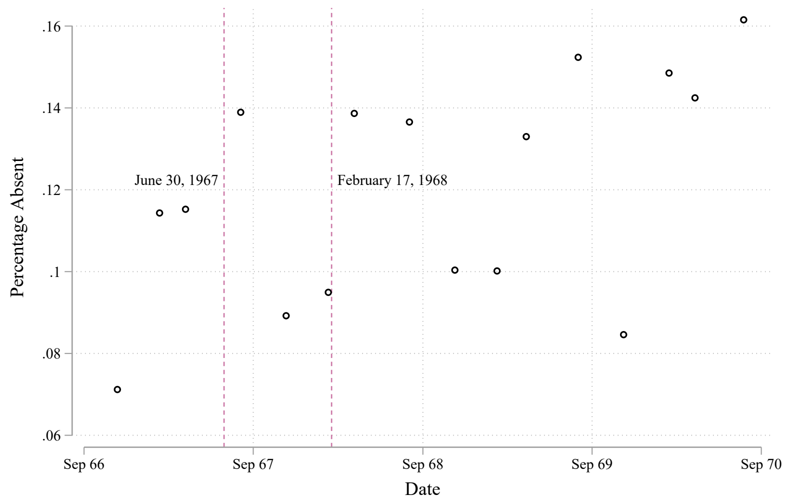
(a) Raw Data



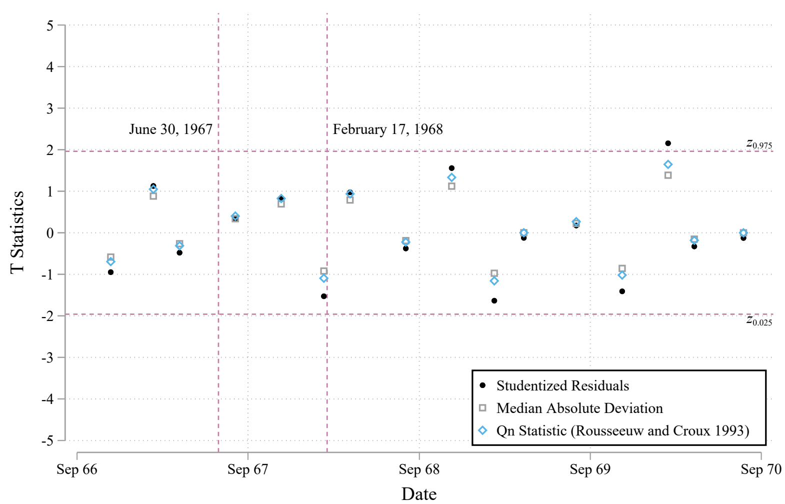
(b) Statistical Tests

Appendix Figure 30a plots the raw fraction of women who registered for each LSAT exam that did not attend on the day of each exam administration. Appendix Figure 30b plots the studentized residual of the logit transformation of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equation (B.2), (B.3), and (B.4), respectively. The horizontal lines indicate the critical values for a two-sided z-test with a 5% significance level, which we use as our threshold for an outlier value. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

Appendix Figure 31: Fraction of Men Absent from LSAT



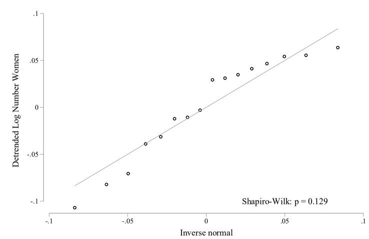
(a) Raw Data



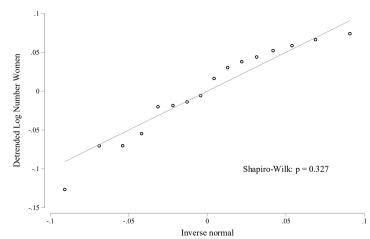
(b) Statistical Tests

Appendix Figure 31a plots the raw fraction of men who registered for each LSAT exam that did not attend on the day of each exam administration. Appendix Figure 31b plots the studentized residual of the logit transformation of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equation (B.2), (B.3), and (B.4), respectively. The horizontal lines indicate the critical values for a two-sided z-test with a 5% significance level, which we use as our threshold for an outlier value. In both plots, vertical lines denote the two policy announcements of interest: the Military Selective Service Act of 1967, passed on June 30, 1967; and the removal of deferments for law students by the Selective Service on February 17, 1968.

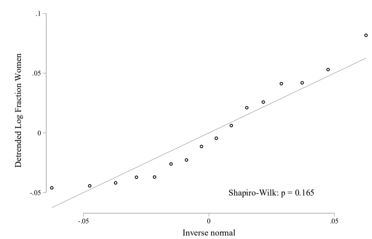
Appendix Figure 32: QQ Plots



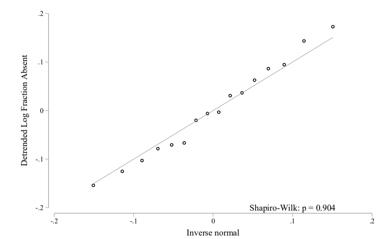
(a) Number Women Attended



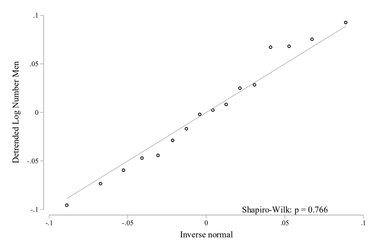
(b) Number Women Registered



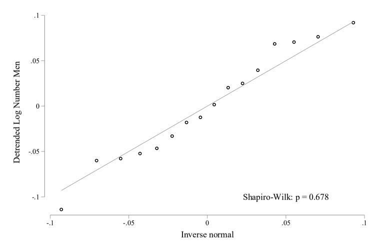
(c) Fraction Women Attended



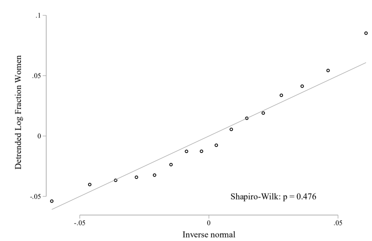
(d) Fraction Women Absent



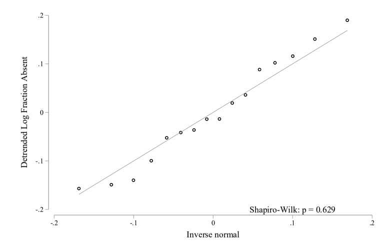
(e) Number Men Attended



(f) Number Men Registered



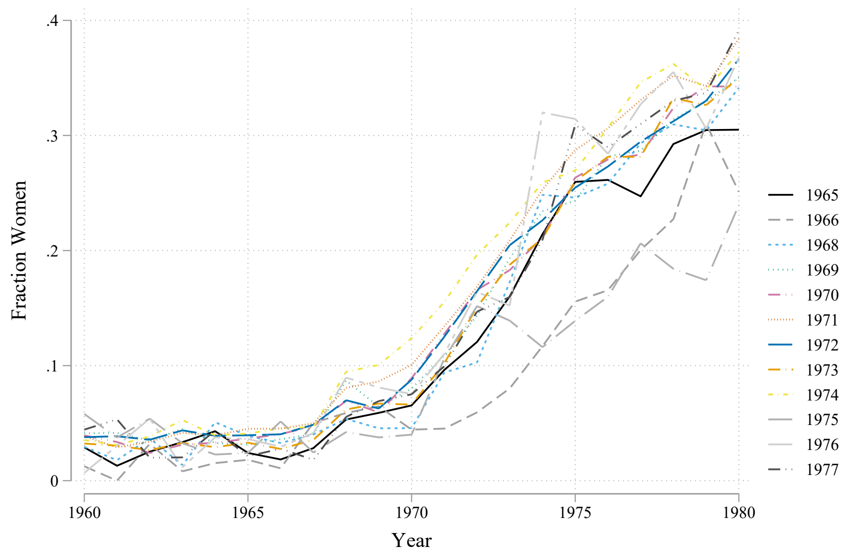
(g) Fraction Women Registered



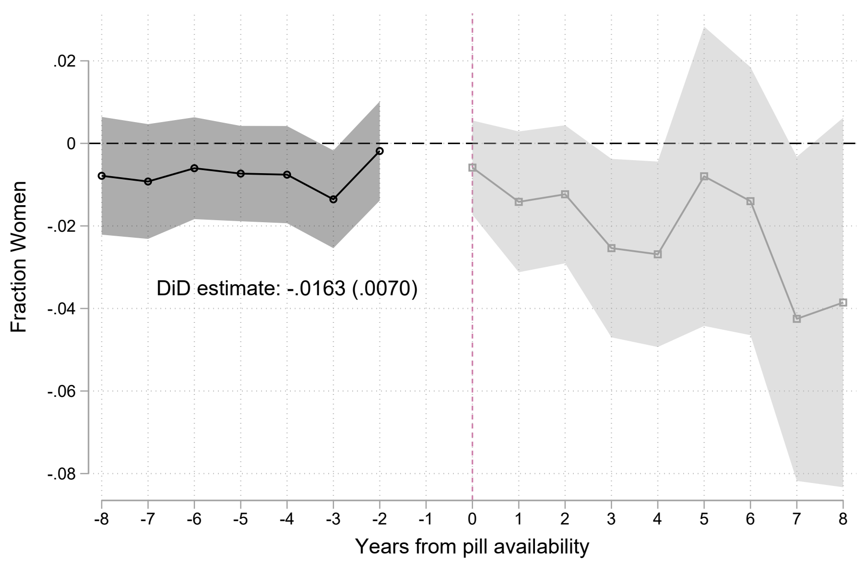
(h) Fraction Men Absent

Each subfigure is comprised of a QQ plot for a certain variable, plotting the observed distribution against the value from the standard normal that would be observed at the empirical quantile of each observation. We also report the *p*-value from a Shapiro-Wilk test of normality. We plot results for the detrended log number of women attending each LSAT exam (Appendix Figure 32a), the detrended log number of men attending each LSAT exam (Appendix Figure 32e), the detrended log number of women registered to take each LSAT exam (Appendix Figure 32b), the detrended log number of men registered to take each LSAT exam (Appendix Figure 32f), the detrended logit transformation of the fraction of LSAT exam attendees who are women (Appendix Figure 32c), the detrended logit transformation of the fraction of students registered to take the LSAT exam who are women (Appendix Figure 32g), the detrended logit transformation of the fraction women women who register for but do not take each LSAT exam (Appendix Figure 32d), and the detrended logit transformation of the fraction women women who register for but do not take each LSAT exam (Appendix Figure 32h).

Appendix Figure 33: Pill Liberalization



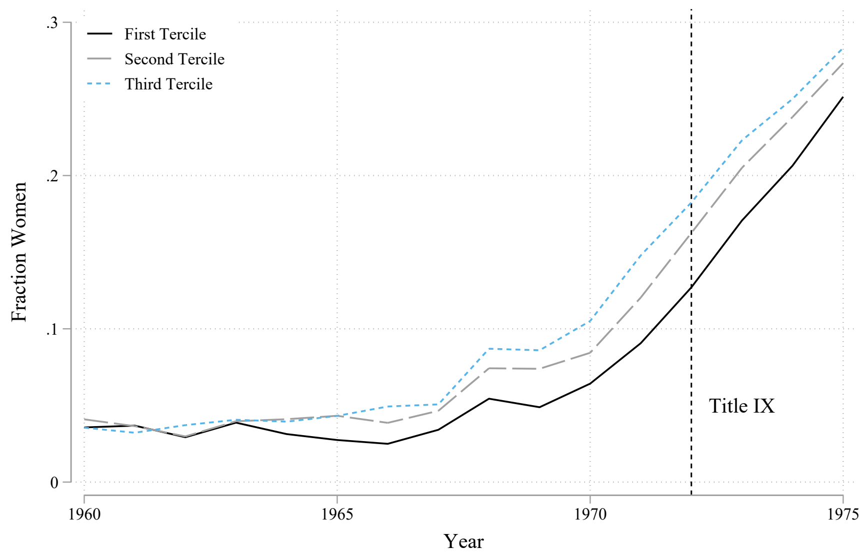
(a) Raw Enrollment By Pill Liberalization



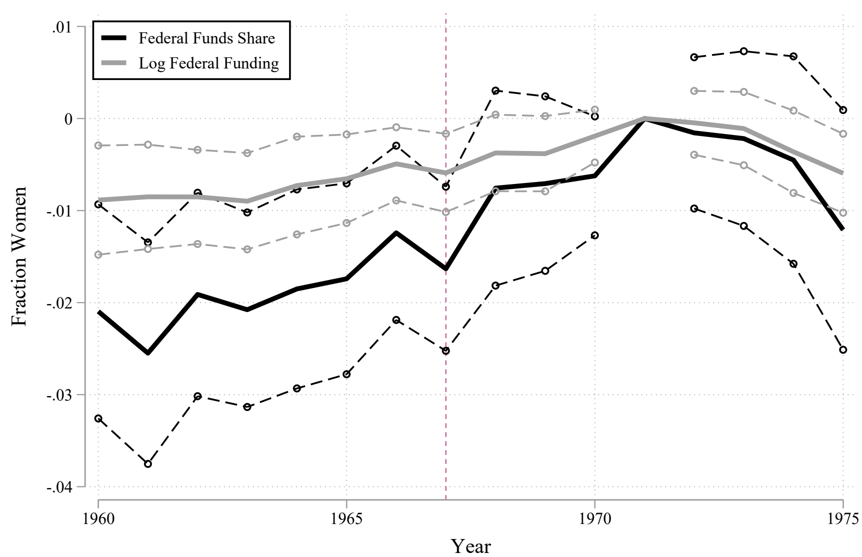
(b) Pill Liberalization Event Study

Appendix Figure 33a plots the total enrollment weighted average of the fraction of first-year students enrolled who are women within each treatment cohort. Treatment here is defined as the first year in which an unmarried, childless woman under the age of 21 could legally obtain medical treatment without a parent or spouse consenting in the state in which a law school resides. Appendix Figure 33b presents difference-in-differences estimates of the impact of this treatment on the fraction of women enrolled in the first-year at the law school associated with the institution that received the complaint. Event study coefficients are calculated using the method developed in Callaway and Sant'Anna (2021).

Appendix Figure 34: Title IX



(a) Raw Enrollment By Federal Funds Share Tercile

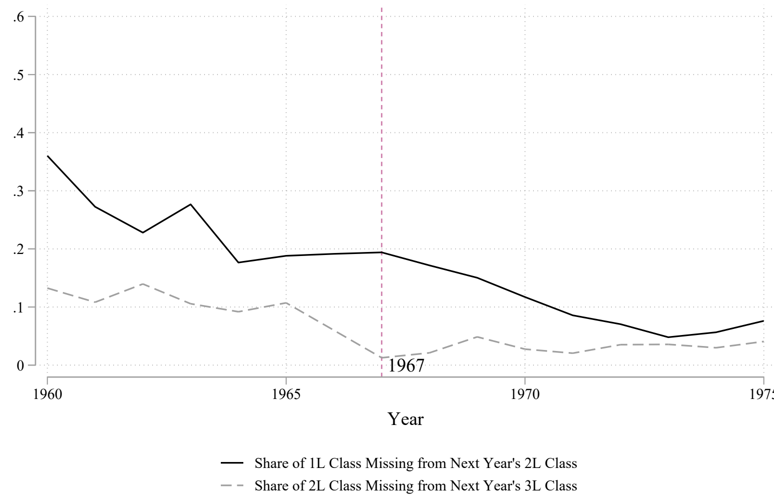


(b) Event Study Results

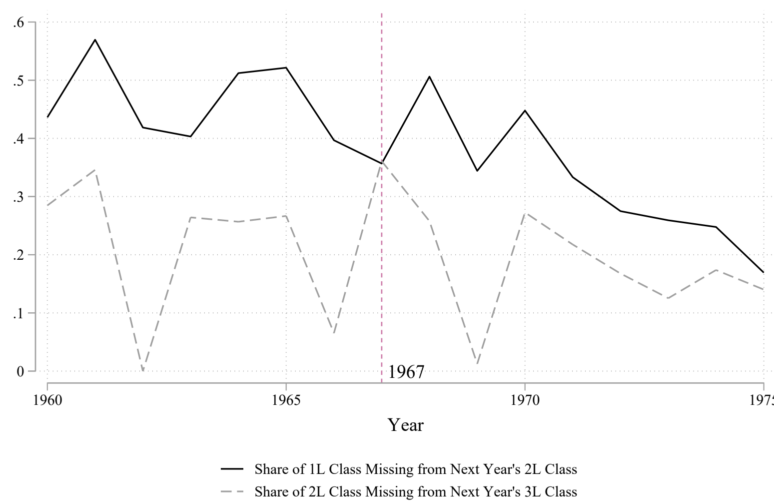
Appendix Figure 34a plots the total enrollment weighted average of the fraction of first-year students enrolled who are women within each tercile of the institutional federal funding share as measured in the 1968-69 Financial Statistics portion of HEGIS. The federal funding share is defined as the fraction of an institution's total revenue that is received from the federal government. Appendix Figure 34b presents continuous difference-in-differences estimates from equation (7). The outcome is the fraction of first-year law students who are women. The solid black line plots results where the independent variable is the federal funds share, and the solid grey line plots results where the independent variable is the log of federal funding. Standard errors are clustered at the institution level.

Appendix Figure 35: Aggregate Attrition for Women

(a) Full-Time Programs



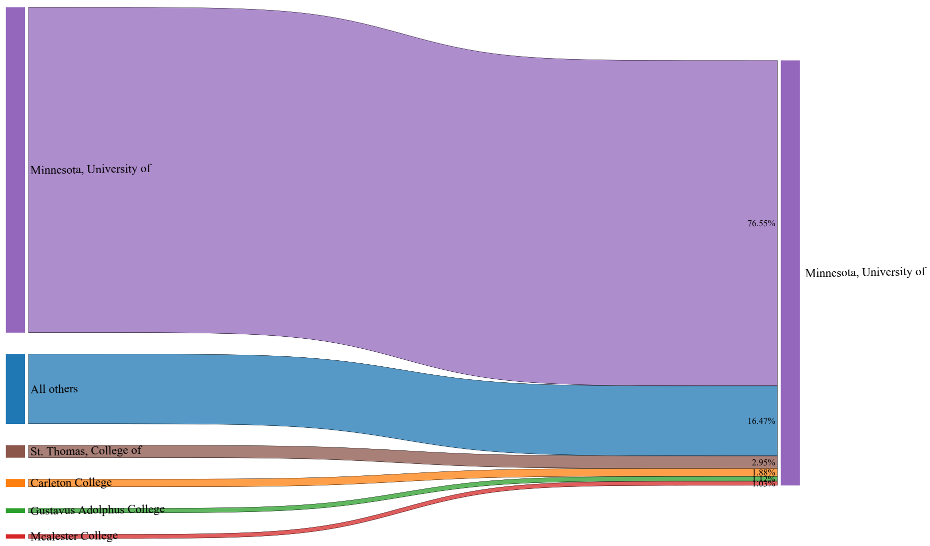
(b) Part-Time Programs



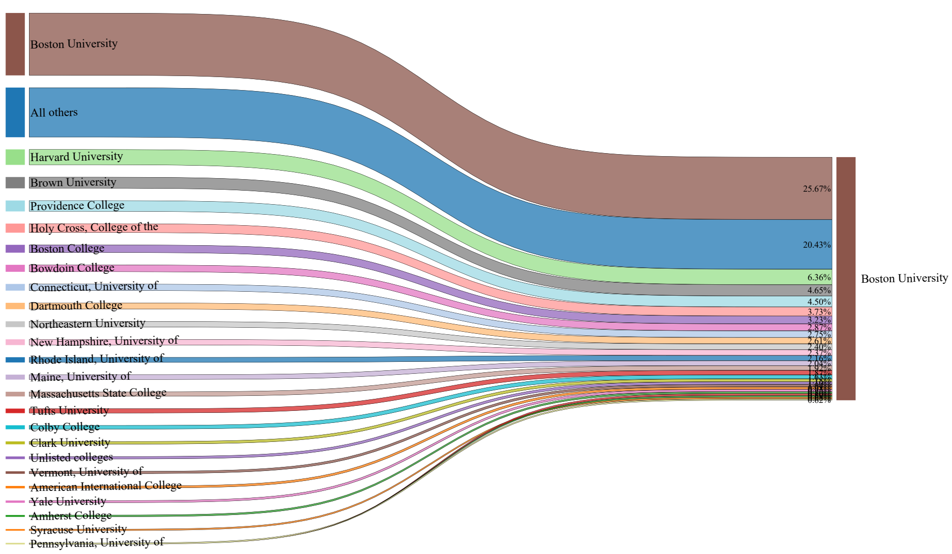
Each figure plots estimates of women's attrition from law schools in each year. To do this, we calculate the percentage drop in women's enrollment from year to year. These calculations are plotted in the baseline year; for example, in 1967, we plot the percentage of 1L students in 1967 that are missing in 1968. The solid black line plots the share of the 1L class missing from next year's 2L class; the dashed grey line plots the share of the 2L class missing from the next year's 3L class. Appendix Figure 35a plots these series for full-time programs. Appendix Figure 35b plots these series for part-time programs

Appendix Figure 36: Undergraduate to Law School Flow Plots - Local and Regional

(a) University of Minnesota



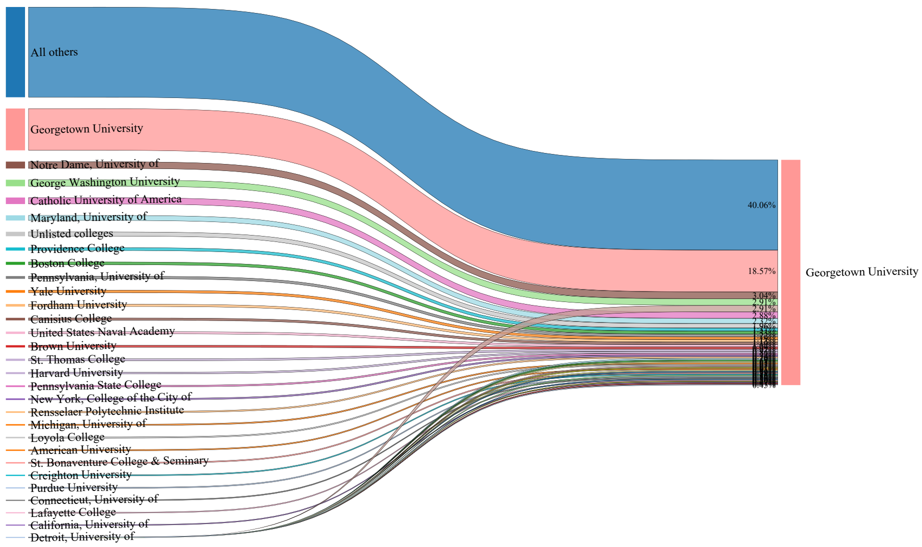
(b) Boston University



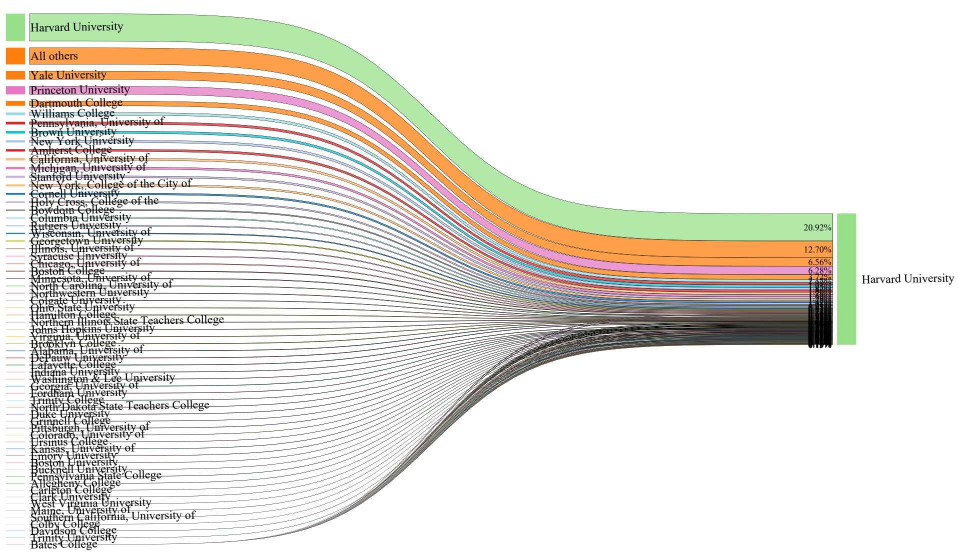
Each subfigure is comprised of a flow plot for a law school. Undergraduate institutions sending at least 20 students to each law school are shown individually, while all schools sending fewer students are summed and displayed in the "all others" category. The share of graduates with known undergraduate education are displayed.

Appendix Figure 37: Undergraduate to Law School Flow Plots - National

(a) Georgetown

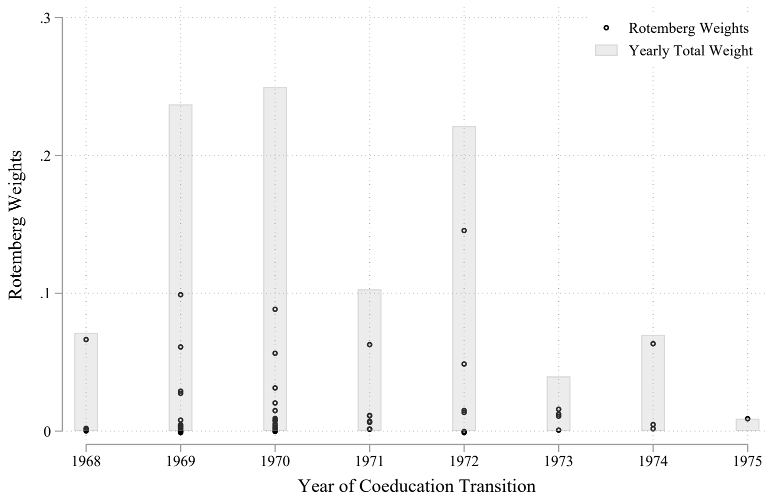


(b) Harvard



Each subfigure is comprised of a flow plot for a law school. Undergraduate institutions sending at least 20 students to each law school are shown individually, while all schools sending fewer students are summed and displayed in the "all others" category. The share of graduates with known undergraduate education are displayed.

Appendix Figure 38: Rotemberg Weights



Appendix Figure 38 plots Rotemberg weights for the shift-share instrument constructed in equation (9). The weight for each undergraduate institution is plotted against the year in which it transitions to coeducation. The grey bars plot the total weight in each transition year.

Appendix Table 1: Bin Midpoints Used to Impute Mean LSAT and GPA Scores

LSAT Score Bin	**Imputed Value**
Under 450	450
450–500	475
501–550	525
551–600	575
601–650	625
651–700	675
Over 700	700
GPA Bin	**Imputed Value**
Under 2.00	2.00
2.00–2.50	2.25
2.51–2.75	2.625
2.76–3.00	2.875
3.01–3.25	3.125
3.26–3.50	3.375
3.51–4.00	3.75

This table presents the bin midpoints used to impute Mean LSAT and GPA scores for the incoming class of a given enrollment year. The left-hand column gives the reported ranges in our data source, and the right-hand column gives the imputed value that we used as an estimate of the within-bin average.

Appendix Table 2: Coeducation Transitions

Institution	Transition Year	Identified in *Martindale-Hubbell*
Babson College	1968	No
Biscayne College	1968	No
St. Procopius College	1968	Yes
John Carroll University	1968	Yes
Marist College	1968	No
Regis College	1968	Yes
Siena College	1968	Yes
Villanova University	1968	Yes
Delaware Valley College of Science & Agriculture	1969	No
Franklin And Marshall College	1969	Yes
Georgetown University	1969	Yes
Kenyon College	1969	Yes
Princeton University	1969	Yes
Rockhurst College	1969	Yes
Saint Mary's College	1969	Yes
Trinity College	1969	Yes
Tulane University Of Louisiana	1969	Yes
University Of The South	1969	Yes
Washington & Jefferson College	1969	Yes
Wesleyan University	1969	Yes
Xavier University	1969	Yes
Yale University	1969	Yes
Boston College	1970	Yes
Christian Brothers College	1970	Yes
Colgate University	1970	Yes
Fairfield University	1970	Yes
La Salle University	1970	Yes
Lafayette College	1970	Yes
Nichols College	1970	No
Providence College	1970	Yes
Rutgers University New Brunswick	1970	Yes
Saint Edward's University	1970	Yes
Saint Joseph's University	1970	Yes
Saint Mary's College Of California	1970	Yes
Saint Michael's College	1970	Yes
Union College	1970	Yes
University Of Virginia-Main Campus	1970	Yes
Williams College	1970	Yes
Bowdoin College	1971	Yes
Lehigh University	1971	Yes
Loras College	1971	Yes

Institution	Transition Year	Identified in *Martindale-Hubbell*
Loyola College	1971	Yes
Menlo College	1971	No
Mount St Mary's University	1971	Yes
Randolph-Macon College	1971	Yes
Saint John Fisher College	1971	No
St John's University-New York	1971	Yes
Stevens Institute Of Technology	1971	Yes
College Of The Holy Cross	1972	Yes
Dartmouth College	1972	Yes
Davidson College	1972	Yes
Johns Hopkins University	1972	Yes
University Of Notre Dame	1972	Yes
Wofford College	1972	Yes
Loyola Marymount University	1973	Yes
Manhattan College	1973	Yes
Saint Mary's Seminary and University	1973	Yes
University Of Scranton	1973	Yes
Fordham University	1974	Yes
Norwich University	1974	Yes
Saint Anselm College	1974	Yes
Amherst College	1975	Yes

Appendix Table 2 lists all 62 undergraduate institutions that transition to coeducation between 1967 and 1975. For each, we provide the year of transition, taken from Goldin and Katz (2011), as well as an indicator if that institution is identified in the 1963 *Martindale-Hubbell* list of colleges and universities.