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 argues that 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 creating incentives to admit more women to stabilize enrollment. Using a uniform-adoption difference-in-differences design, we find that women's representation rose by 44% (2 percentage points) in full-time programs relative to part-time programs, which were far less exposed to draft risks. Law schools that lost more first-year men than expected after the change in deferment experienced larger and more sustained increases in women's enrollment share in the following year relative to schools with lower attrition. To understand the labor market impacts of draft disruption, we digitize over 500,000 pages of archival directories of practicing lawyers to document downstream effects on the bar. We show that the draft delayed men's careers and increased women's representation at prestigious law schools, in private practice, and among lawyers who ultimately became firm partners.

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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 geographic variation in draft 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 the first shaded region (1941-45) 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 enrollment 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).

Several authors have highlighted the relationship between wars and the production and gender composition of law students. *American Lawyers* documents that during WWI, female enrollment increased by 27 percent (increasing from 397 to 503) to fill the tuition void left by a 67 percent drop (from 7,477 to 2,443) in male attendance between 1916 and 1918 (Abel 1989, p. 91). This growth was heavily stratified by institutional prestige, declining at elite day schools but surging in mixed and night programs. We find a similar pattern in enrollment changes during WWII and the Korean War. Figure 1b plots the fraction of women enrolled in law schools separately for full-time and part-time programs. While women's representation rose during both conflicts, the segmentation across program types was preserved, as part-time programs continued to enroll a larger share of women. Further, much of this variation across program type was driven by changes in men's enrollment. Relative to pre-war peaks, men's enrollment declined by 11,805 (74%) during WWII and 12,781 (27%) during the Korean War, whereas women's enrollment increased by 239 students (34%) and 80 students (6%) during the corresponding years.

The Vietnam War era presents a striking end to these earlier wartime patterns. As Figure 1b demonstrates, women's representation in full-time programs eclipses that in part-time programs for the first time in 1967, suggesting broadening access to more elite institutions. In a break from previous patterns, this growth was driven by changes in women's enrollment. While administrators feared large declines in male enrollment as a result of the Vietnam Draft, only 1798 fewer men (3%) were enrolled relative to the pre-war peak. Nearly half of this decline was offset by increases in women's attendance, increasing by 798 students (27%), nearly twice the amount of women's growth in law school attendance from the previous three wars combined. Further, women's enrollment continued to increase rapidly while inductions dwindled and the draft ended in 1973, resulting in a decoupling between inductions and women's representation in Figure 1a.

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 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 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. This paper provides the first causal evidence that the Vietnam Draft had a marked impact of women's representation in law schools. Further, we show that this change also mattered for labor market outcomes of women in affected cohorts, leading to greater persistence in the profession and a higher probability of promotion.

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 for this exemption, 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. We find similar results using M.D. students, who retained draft deferments in 1968, as a comparison group in place of part-time law students. Finally, as a complementary design, we compare law schools that lost more first-year men than expected between 1967 and 1968 relative to programs with a lower amount of unexpected attrition. These students would have enrolled in Fall 1967, well before the change in deferment rules in February 1968, when they lost the ability to defer induction until graduation. We find that more surprise attrition, which was almost entirely due to the draft, lead programs to admit more women in Fall 1968.

Figure 2a 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 the growth rate of the share of women 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 percentage points, while we cannot rule out a null response for institutions at the bottom of the volume distribution.

We also digitize and analyze new data on the near universe of practicing lawyers in this time period from the *Martindale-Hubbell* lawyer directories. Because these directories report law school attended and year of first bar admission, which generally occurs upon graduation, we are able to map this labor market information to our policy variation. Using this new data, we first look for evidence that the draft distorted labor market outcomes for men. We find that the number of men graduating law school before they turned 27 drops more than 20% below trend in 1968, suggesting that many younger students either were drafted out of law school or chose to delay applying due to draft risk. However, most of these men appear to have eventually returned to law school, albeit on a delayed schedule, rather than been removed from the profession entirely. We bolster this evidence using variation from draft-lottery numbers chosen in 1969, showing that men assigned numbers called for the draft were 4.1% less likely to graduate before they turned 27. This delay in education translated into worse labor market outcomes as these men were 1.4 percentage points less likely to be a partner relative to an associate (and 2.6 less likely to work at large firms) twelve years after graduation.

This newly constructed data also allows us to study the impact of the draft on women's legal careers. We leverage the fact that many law schools operated only one (either full-time or part-time) program, allowing us to determine the treatment status of this subset of observations. Comparing graduates of full-time and part-time programs, we show that 6 years after graduation, the representation of women in the bar who had graduated from full-time programs increased by 6.2 percentage points relative to their part-time graduate peers. These women were also more likely to be associates at law firms and more likely to achieve partnership at 12-years out. To guard against the possibility that institution quality might have changed around the same time, we run an identical design for men, generally finding muted or null results relative to women's gains during this period.

In addition, we find suggestive evidence that several policy changes reinforced these draft-driven changes to women's representation in law school and in the bar. Title IX requirements (Rim 2021), anti-discrimination lawsuits filed by the Women's Equity Action League (Helgerman 2025), and increased access to contraception (Goldin and Katz 2002) all appear to have helped sustain and perhaps increased women's representation initially sparked by draft-related disruptions.

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, adding to Cross (2024)'s study of gender desegregation in the army in 1972.[1]

We also contribute to the literature on women's access to higher education and human capital accumulation. To do so, we focus 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 women's representation in law schools and the operation of student deferments during the Vietnam Draft. Section 3 provides an overview of the data used and the details of our main empirical design. Section 4 presents results from this design, a battery of robustness checks, evidence on heterogeneity on the basis of prestige, as well as results

[1] Notably, our context and results differ markedly from studies on WWII, which found that WWII decreased women's educational attainment (Jaworski 2014)

for several complementary empirical designs. Section 5 shows how our main results translated to the composition of practicing lawyers. Section 7 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 6.1, 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).[2] 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.[3] 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

[2] 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).

[3] 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).

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 Committee, initially formed in 1948 by the director of the Selective Service System, General Lewis B. Hershey, to devise a principled method for selective deferment.[4] 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.[5] 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 and faced potential conscription if selected by their local draft board. Executive Order

[4] 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).

[5] 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).

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, 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" (The Harvard Crimson 1969) and law school application guidebooks at the time cautioning that "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" (Coons 1967).

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 $t+1$.[6] 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 2b 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.

[6] Formally, let men's enrollment in year t cohort c be given by M_t^c. We calculate aggregate attrition as $\frac{M_t^1 - M_{t+1}^2}{M_t^1}$. 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.

Figure 2c 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 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).[7] 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,

[7] 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).

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.[8] To understand the variation 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.[9]

[8] 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.

[9] 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 situa-

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 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.

tions 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.

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.[10] 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:

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

[10]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 6 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.

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 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 7, 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 C. 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 25), the number of women registered for (Appendix Figure 30) and taking (Appendix Figure 27) the LSAT, the fraction of students registered for (Appendix Figure 32) and taking (Appendix Figure 29) the LSAT who are women, as well as the absence rate (registered but did not attend) for women (Appendix Figure 33).

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 9), as well as all programs that obtain ABA approval by the end of our sample period (Appendix Figure 10). 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 11) or the institution-program level (Appendix Figure 12). We drop institution-program-years that enroll less than 10 students to improve precision; Appendix Figures 13 and 14 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 20 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 8 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 15a. 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 15b and 15c, 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 17 with 95% confidence bands plotted for all models. We allow for potential covariance across time within institution (Appendix Figure 18) as well as state (Appendix Figure 19), 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 16 presents an identical set of results where weights are fixed at total enrollment in 1967, and Appendix Figure 21a 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.[11] We explore these auxiliary outcomes in Appendix Figure 36, 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. Appendix Figure 36a, 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 2b.[12] Then, in the following year (1969), we see a sharp increase in the event coefficient, reflecting

[11]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.

[12]Appendix Figure 38 replicates Figure 2 for women to confirm that there is no corresponding jump in attrition

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. Appendix Figure 36b presents results for third year students. It is a bit more difficult to parse responses across years: the diminished event coefficients relative to Appendix Figure 36a 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.

4.1.1 Medical Schools as a Comparison Group

Our primary difference-in-differences design uses the trend in women's enrollment at part-time law schools as an estimate of how women's enrollment at full-time programs would have evolved in the absence of the policy change. The primary weakness of this design is that there is the potential for spillovers across programs, especially within the same school. Our third specification, eliminating overlap between treatment and control groups, avoids some of this issue, but the movement of men from full-time programs to part-time programs within institution would still push our point estimates upwards without any meaningful change in women's enrollment. This could happen as a result of the same incentives our design relies on: to reduce uncertainty in tuition revenue, admissions committees would be incentivized to move men highly exposed to the draft into part-time programs.

Accordingly, we consider a complementary design using medical schools as a comparison group. Medical students remained eligible for a student deferment in 1968, meaning that they remain untreated, and since law and medical schools are administered separately within institution, the incentive to shuffle enrolled men around programs is not present. Just as with part-time programs, we can look to see how men's attrition changes across time to validate the assumption that medical students are insulated from the draft. To do this, we collect data on enrollment at the institution-year-sex level from *Medical School Admission Requirements* between 1966 and 1968. For cohorts entering in 1966 and 1967, the change in men's attrition between year one and year two for medical schools is -.09 percentage points, compared to a 9.6 percentage point increase at law schools. We obtain institution-by-year data on the fraction of new medical students who are women from Helgerman (2025).[13] The exact statistic varies slightly across time: before 1966, this is the estimated number of new entrants in the spring before students enter in the fall; in 1966, this is first-year students; and after 1967, this is new entrants in the fall.[14] These data sources are compiled to

between years 1 and 2 for women.

[13]There is a subtle distinction between first-year students and new entrants in medicine. New entrants comprises all students entering the institution, whereas first-year students also includes students repeating the first year.

[14]In years in which these variables are both observed, they are highly collinear. See Helgerman (2025) for a detailed discussion of dataset construction.

construct a data series between 1960 and 1970. Since medical schools are impacted by the threat of enforcement action from Executive Order 11246 in 1971 (Helgerman 2025), we do not consider years after 1970.

As before, we utilize a uniform adoption event study design of the form

$$Y_{idt} = \sum_{\tau=1960,\tau\neq 1967}^{\tau=1970} \alpha_\tau D_d \mathbb{1}(t=\tau) + \gamma_{id} + \delta_t + \varepsilon_{idt} \tag{5}$$

The outcome, Y_{idt}, gives the fraction of all first-year students (defined as noted before) who are women at institution i in discipline $d \in \{\text{law}, \text{medicine}\}$ in year t. $D_d = \mathbb{1}(d = \text{law})$ indicates exposure to the policy shock. To improve comparability, we include only ABA-Approved full-time law programs, as medical schools only operate accredited full-time programs. Our baseline specification includes institution-by-discipline fixed effects, γ_{id}, as well as year fixed effects, δ_t, and we consider two additional designs (analogous to M2 and M3 in Section 3.2) to narrow the set of comparisons to guard against differences in how different institutions may be operating. First, we restrict to a "matched" sample that includes only institutions operating both a law school and a medical school. Separately, in a third model, we restrict to the set of law schools operating only a full-time program. Since medical schools do not operate part-time programs, this comparison precludes substitution of men across full-time and part-time programs. Finally, as before, we cluster standard errors at the institution-by-discpline level and weight by total first-year enrollment. To summarize our event study results, we also report estimates from a difference-in-differences design of the form:

$$Y_{idt} = \alpha_{\text{DiD}} D_d \mathbb{1}(t > 1967) + \gamma_{id} + \delta_t + \varepsilon_{idt} \tag{6}$$

The results from this design are presented in Figure 4. Our baseline difference-in-differences estimator is remarkably similar to our estimate in Figure 3a; we estimate that the policy change resulted in a 3 percentage point increase in women's representation. The event study coefficients are very similar across models, and relative to Figure 3a the event study presents more stark evidence of a response in 1968. For all models, the event coefficient in 1968 is slightly below 0.04, implying that women's representation approximately doubled over the 1967 baseline. As before, one clear concern for this design is that this increase might simply represent changes in men's enrollment. Not only are men entering law school in 1968 subject to draft risk, but we might also worry that men's demand for medical education increases relative to other health professional programs, as the student deferment was maintained. To rule this out, we consider a secondary design of the form:

$$Y_{idt} = \sum_{\tau=1960,\tau\neq 1967}^{\tau=1970} \alpha_\tau D_d \mathbb{1}(t=\tau) + \beta E_{idt} + \gamma_{id} + \delta_t + \varepsilon_{idt} \tag{7}$$

The outcome Y_{idt} is now the number of women enrolled at institution i in discipline d in year

t. We include a control for the total number of students enrolled, E_{idt}, to adjust for differential enrollment sizes across institutions. Event coefficients and fixed effects remain identical to those included in equation (5), and we summarize the estimates from this design with a similar summary difference-in-difference specification:

$$Y_{idt} = \alpha_{\mathrm{DiD}} D_d \mathbb{1}(t > 1967) + \beta E_{idt} + \gamma_{id} + \delta_t + \varepsilon_{idt} \tag{8}$$

The results from this exercise are plotted in Figure 4b. This design confirms that there is in fact a strong response in women's enrollment in 1968, suggesting that our results are not driven solely by changes in men's enrollment behavior.

4.1.2 Heterogeneity

Having demonstrated robustness of our main result across a number of checks as well as an alternative design, we return to our main design to characterize heterogeneity of the policy response. 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 B. 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 prestigious programs. If we assume that all schools are drawing from students on the right 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 tastebased 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 yet, 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.[15] Our key causal parameter is the average treatment effect for full-time programs with log volumes V, given by

$$ATT(V|V) = \mathbb{E}[Y_{i,\rho=\text{Full},t=1968}(V) - Y_{i,\rho=\text{Full},t=1968}(0)|V_i = V] \tag{9}$$

It is important to emphasize that we are *not* identifying the causal response in response to an increase in institutional prestige.[16] 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. Additionally, $Y_{i,\rho=\text{Full},t=1968}(0)$ denotes the outcome for institution i if it were untreated by the

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

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

policy change, not the outcome if institution i had an empty library.

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

$$\triangle Y_{i\rho} = \alpha + \mathbb{1}\{D_\rho = 1\} \sum_{k=0}^{3} \beta_k V^k + \varepsilon_{i\rho} \tag{10}$$

Our comparison group remains all part-time programs where $D_\rho = 0$, and we estimate the mean response in this group simultaneously with the constant term α to ensure this uncertainty is reflected in our standard errors. We correct for heteroskedasticity by weighting by first-year enrollment in 1967 and estimating heteroskedasticity-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 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.[17] 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, plotted in Appendix Figure 5b, though this looks far more linear across our measure of selectivity, with substantially less precision among highly selective schools.

[17] 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" (Coons 1968).

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).[18] 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 23.[19] Our finding of a positive relationship between selectivity and the institution-specific ATT is strongly robust to all three of these measures.

In Appendix Figure 24, we also look at heterogeneity by program size. *Ex ante*, it is unclear whether or not we would expect a stronger response from larger programs. Expected loss of tuition revenue would higher for these schools, but the variance of this loss would be lower, and there should be no difference of lost revenue as a fraction of total revenue. Appendix Figure 24a plots estimates of equation 10 where V has been replaced with total first-year enrollment in 1967. We find evidence of a stronger response from larger programs, similar in shape to our estimate of heterogeneity in LSAT/UGPA composite score in Appendix Figure 23c, which may reflect the fact that more prestigious programs tend to also be larger. Since total enrollment in 1967 is included in both the outcome and weights for this result, we also look at heterogeneity in the number of full-time faculty members in 1967 (Appendix Figure 24b) and the estimated number of full-time equivalent faculty in 1967 (Appendix Figure 24c), as the number of faculty members is another indicator of program size that is not mechanically related to enrollment.[20]

4.2 Cross-sectional Measured Attrition

As displayed in Figure 2, just over 20% of the class matriculating between 1960 and 1966 did not make it to the second year of law school. This figure spiked by about 50% for students matriculating in 1967 who became unexpectedly exposed to draft risks. This increase in attrition

[18]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).

[19]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 approximately the same average and spread as the LSAT distribution.

[20]To estimate full-time equivalent faculty, we use the equation $(1/3) * \{\#\text{ Part-Time Faculty}\} + \{\#\text{ Full-Time Faculty}\}$. This is based on Table 6 in Dunham and Wright (1962), which presents counts of all part-time and part-time equivalent faculty at rank instructor and above in the first term of the 1961-62 academic year across all institutions of higher education. The ratio of full-time equivalent part-time faculty to the number of part-time faculty is very close to 1/3 (0.337).

was not evenly spread across institutions and very likely unexpected from the perspective of an admissions committee enrolling the incoming Fall 1967 class in the 1966-67 academic year. Full time programs that had a larger than expected attrition rate in 1968 would face a larger loss of tuition revenue for second year students, and, we posit, would therefore be more likely to enroll a higher share of women. Accordingly, this section presents a second empirical approach that leverages plausibly unanticipated differences across programs in attrition between the first and second year of the program.

For each program ρ at institution i in year $t \in \{1961, \ldots, 1967\}$, we estimate the share of attrition of the first-year class that is unanticipated between year t and year $t+1$. We restrict to institution-program-years where enrollment is observed throughout a 6-year pre-period observation window.[21] Letting $a_{i\rho t}$ denote the fraction of first-year students in year $t-1$ that do not enter the second year in year t, we assume that expected attrition is given by:

$$\mathbb{E}[a_{i,\rho,t+1}|\mathcal{A}_{i,\rho,t}] = \hat{\alpha}_{i,\rho} + \hat{\beta}_{i,\rho}(t+1) \tag{11}$$

Where $\mathcal{A}_{i,\rho,t}$ denotes the history of attrition for program ρ at institution i as of year t. $(\hat{\alpha}_{i,\rho}, \hat{\beta}_{i,\rho})$ are estimated by fitting attrition over the past 6 years to a linear model. Then, $\mathbb{E}[a_{i,\rho,t+1}|\mathcal{A}_{i,\rho,t}]$ is estimated by extrapolating this trend into the next year.

Under this model, the unexpected portion of attrition is defined as $S_{i,\rho,t+1} = a_{i,\rho,t+1} - \mathbb{E}[a_{i,\rho,t+1}|\mathcal{A}_{i,\rho,t}]$, so that $S_{i,\rho,t+1} > 0$ indicates the program experienced steeper enrollment loss in year $t+1$ than its pre-period trend would have implied—consistent with draft-induced male departure from full-time programs. To make the shock comparable across programs that differ in baseline attrition volatility, we standardize $S_{i,\rho,t+1}$ by $\hat{\sigma}_i, \rho, t$, the standard deviation of within-program residuals over the pre-period estimation window, to construct $\tilde{S}_{i,\rho,t+1} = \frac{S_{i,\rho,t+1}}{\hat{\sigma}_{i,\rho,t}}$. The scaled surprise $\tilde{S}_{i,\rho,t+1}$ measures the 1968 attrition shock in units of each program's typical pre-period forecast error, so that programs with historically volatile attrition receive less weight as signals of the draft's enrollment impact. To test that the forecasting model in equation (11) is correct in expectation, we can test whether the standardized residuals $\tilde{S}_{i,\rho,t+1}$ are mean 0 between 1963 and 1967: we are unable to reject the null that $\mathbb{E}_t[\tilde{S}_{i,\rho,t+1}] = 0$ with p-value 0.26.

First, we show that 1968 was indeed a surprising year for attrition in full time programs (but not part time programs) and characterize the cross-sectional patterns of this surprise attrition. Figure 6a plots the average value of $\tilde{S}_{i,\rho,t+1}$ by year and program type between 1963 and 1975. We find that that the average forecast error in 1968 is about 3 times as large as the next largest value of surprise attrition, and more than 3 times the standard deviation of the forecast error in the trailing years between 1963 and 1967. While in most years we are unable to distinguish the reason for student attrition between their first and second years, for students matriculating in 1966, 1967, and 1968, we have data on students who were asked not to return for academic reasons from

[21] To do this, we extend our dataset back through 1955 using data from the *Review of Legal Education*.

the *Pre-Law Handbook*. Figure 6b shows that the two years adjacent to the surprise change in draft policy (students matriculating in 1966 and 1968), the academic dismissal rate hovered around 10% of the class and comprised around 40 and 45% of observed attrition. For students entering in 1967, the academic dismissal rate remained near 10% of the class, while non-academic attrition dramatically increased, a pattern one would expect to see based on the increased draft risk.

Figure 6c plots the women's share of first-year enrollment for full-time programs in the top and bottom quartiles of the 1968 attrition surprise $\tilde{S}_{i,\rho,1968}$. The two groups track each other closely before 1968; beginning with the fall 1968 entering class, women's share at high-surprise schools rises above that at low-surprise schools, and the gap of roughly 4 percentage points persists through 1975, the end of our enrollment sample. This is the school-level pattern the reference-dependence model in Figure 5 predicts: the shock permanently raised exposed schools' baseline share of women, so the 1968 response did not revert once draft pressure ended.

Figure 6d looks more closely at the distribution of unanticipated attrition across schools. We plot this against our preferred measure of institutional prestige, finding that the two are uncorrelated. Figure 6d plots our estimates for 1968. Unlike previous years, we see substantial variation in unanticipated attrition; though there is one outlier, this is also driven by shocks that occur across many institutions. Further, this is positively correlated with prestige, providing a clear rationale for the heterogeneity results in the previous section.

We utilize this measure of unanticipated attrition to construct a second estimator of the impact of the change in draft deferment rules. Instead of relying on parallel trends, we argue that unanticipated attrition is plausibly exogenous, allowing us to recover the causal response of women's enrollment via ordinary least squares (OLS). We restrict our sample to full-time programs, which are directly affected by the change in deferment rules, and adopt an empirical design of the form:

$$\Delta Y_{i\rho t} = \alpha + \beta\, \tilde{S}_{i,\rho,t} + \mathbf{X}_i'\gamma + \varepsilon_{i,\rho,t} \tag{12}$$

Our outcome of interest is $\Delta Y_{i,\rho,t} \equiv Y_{i,\rho,t} - Y_{i,\rho,t-1}$, which gives the one-year change in the share of women in the first-year (1L) class at program i. β is our coefficient of interest, $\mathbf{X}_i$ is a vector of institutional controls, and all regressions are weighted by enrollment size.

We begin by estimating equation (12) in 1968; our estimates $\hat{\beta}$ are reported in Table 2a. Column 1 reports estimates from our baseline specification consisting of a bivariate regression with no institution controls: we find that a one-standard-deviation increase in the standardized attrition shock, interpretable as a program's 1968 attrition exceeded its predicted trend by one pre-period standard deviation, is associated with a 0.21 percentage point increase in women's 1L share, significant at the 1 percent level. It is important to note that this is a different causal estimand than that reported in Section 4.1. Here, we are measuring the change in women's enrollment in response to unexpected attrition between years one and two, whereas our difference-in-differences design captures both responses to unexpected attrition as well as anticipated changes in men's first-year enrollment in 1968.

This estimate is stable across all robustness checks. Controlling for law school prestige via library volumes (Column 2) leaves the coefficient essentially unchanged at 0.19 pp, with the modest sample reduction from 121 to 113 reflecting data availability rather than a substantive shift in the relationship. Winsorizing $\tilde{S}_{i,\rho,t}$ at the 1% to remove the influence of outlier shocks (Column 3) yields a slightly larger estimate of 0.25 pp, also significant at the 1 percent level. Column 4 confirms the result using the raw, unscaled attrition residual; the larger coefficient reflects the change in units rather than a change in the underlying relationship. Dropping the program-specific linear time trend from the pre-period prediction (Column 5) reduces the R^2 from 0.127 to 0.045, consistent with the trend capturing meaningful variation, but the coefficient remains significant at the 5 percent level and is nearly identical in magnitude to the estimate in Column 1. Column 6 applies the same specification to part-time programs, whose students were generally older and carried lower draft exposure. Interestingly, this yields a coefficient of roughly the same magnitude. In principle, there is no reason why a part-time program exposed to a draft-driven positive attrition shock would not also substitute towards enrolling women; our difference-in-differences design depends more so on the assumption that part-time programs did not experience these shocks, as evidence by Figure 6a. It is likely this lack of variation that explains why our estimate in column 6 is statistically indistinguishable from zero.

Table 2b probes whether the 1968 result reflects a pre-existing relationship between attrition surprises and female enrollment growth. We apply the baseline specification to each year between 1963 and 1967 to test whether, before 1968, unexpected attrition would impact women's enrollment. This exercise yields coefficients ranging from -0.0012 to 0.0004, all small relative to the 1968 estimate and none approaching conventional significance levels. The R^2 values are uniformly negligible, indicating that attrition surprises carry essentially no predictive power for female enrollment changes in the pre-draft period. This absence of a pre-1968 relationship rules out the possibility that the core result is driven by a structural tendency for programs experiencing unexpected attrition to differentially admit women, and supports interpreting the 1968 estimate as a response to the draft rather than a latent feature of the data.

5 Translating Enrollment to Employment Outcomes

As shown above, the draft changed the composition of those who attended law school. In this section we show that this propagated into the practicing bar.

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 yearly 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. It also includes a "Biographical Section," which contains longer paid profiles for selected lawyers and firms. We provide representative scans of the Geographic Bar Register in Appendix Figures 3 and 4

Researchers have 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). We produce new data on lawyers for our time period of interest by digitizing hundreds of previously unstudied volumes, processing the images using optical character recognition, and creating machine readable data from this structured text; Appendix A details the digitization pipelines for both sections. Our use of the Martindale–Hubbell volumes builds on this tradition and significantly extends it by leveraging the directories' comprehensive coverage to examine the distribution of lawyers across geographies and organizational settings.

We use the 1965, 1970, 1971, 1972, 1974, 1975, 1976, 1981, 1987, 1991, 1996, and 2000 editions, drawing specifically on the Geographical Bar Register. Contemporaneous writing indicates that more than 90% of practicing lawyers are listed in the Geographical Bar Register during this time period (York and Hale 1973). Importantly, this data includes information on where nearly every lawyer attended law school and when they were first licensed. We also make use of the less representative (biased towards private firms) but more detailed data in the "Biographical Section." Firms could pay to advertise in this section, generally reporting extensive information on lawyers at the firm to signal service quality. This allows us to measure additional information, including the year of law school graduation (which often, but not always, aligns with first licensure), history of military service, position at a particular firm, and birthdate. Throughout, we exclude the directory's foreign-lawyer roster, its U.S. Patent Office cross-index (which duplicates listings from the main state registers), and duplicate listings of the same lawyer across localities (keeping the primary listing).

The directory does not typically record sex. Many ambiguous names are, however, given a parenthetical courtesy title (Mrs./Ms./Miss/Mr.), which we use when available. When not available, we impute sex from first name using the Social Security Administration national name–sex–year frequencies. For each lawyer, we take the first usable name token and calculate the birth-decade-specific probability that a child born with that name is female. This is classified as a female name if this probability exceeds 90%, a male name if this probability is less than 10%, and unclassified otherwise. Over 90% of observed lawyers are unambiguously classified under this relatively conservative procedure.

5.2 The Draft's Footprint in the Practicing Bar

The enrollment results in the previous section establish that the 1968 deferment change pushed women into full-time law programs. The draft evolved into a lottery system in the next year, with lotteries held in every year between 1969 and 1972, before the end of the draft in 1973. While not the focus of our approach so far, it is very plausible that this lottery, as well as the earlier change in deferment rules, both impacted men's careers in law. This section traces these shocks into the profession itself, using the Martindale–Hubbell directories described above. We proceed in the order of the draft's logic: first the men it removed, then the women who entered and what became of their careers.

5.2.1 Drafted Men: Cross-Cohort Evidence

Figure 7a shows the draft's first-order effect on the male side of the profession. Using the year of law school graduation and the graduate's age (implied from date of birth) obtained from Biographical Section of the 1981 directory, we count men who graduated "on time" at ages 22–26, by graduation year, and fit a quadratic trend, excluding the disrupted years 1970–71. The 1970 cohort falls 28% below the trend line and the 1971 cohort 19% below: roughly 1,900 on-time male graduates are missing from firms advertising in the directories. These are likely the men who would have entered law school around 1967–68, when the 2-S deferment was removed, and graduated within 3-4 years. We also find that the missing men were largely diverted, but not eliminated from the bar. Figure 7(b) decomposes male graduates by age at graduation, plotting the number of male graduates between ages 22 and 26 (on time) and between 27 and 40 (older). The deficit of on-time graduates in 1970–71 is mirrored by a surge of *older* (age 27–40) graduates in 1972–74.

Further, military service is often reported in the Biographical Section, allowing us to corroborate this mechanism directly. Figure 7c plots the estimated military service rate among on-time and older graduates. For students graduating in the late 1960s, the service rate is slightly higher for older graduates relative to on-time graduates, but this difference is stable across years. This gap widens substantially for the 1970 and 1971 graduating cohorts that would have been impacted by the change in deferment rules, with older graduates serving at roughly twice the rate of younger graduates in 1970. Interestingly, this gap stabilizes while service declines across both groups in the mid-1970s, suggesting that later cohorts were impacted as well. We can test this directly as we are also able to observe the last year of service for lawyers who served. Figure 7d plots the distribution of the last year of service for men graduating at an older age (between 27-40) between 1972 and 1974. The year of last service is highly concentrated in 1969, 1970, and 1971 (comprising rough

3/4 of all older 1972-1974 graduates), consistent with a spike in induction intensity in 1968.

5.2.2 Drafted Men: Within-Cohort Evidence

In 1969, the draft moved to a lottery system, beginning with the first lottery in December 1969. Each lottery randomly assigned every day of the year to a number between 1 and 366, indicating the order in which induction would proceed, with the last number called to be determined by manpower needs. The lottery was held multiple times, with each drawing corresponding to a set of birth cohorts it would apply to. The December 1969 lottery applied to men born from 1944–1950, of which numbers up through 195 were eventually called; the 1970 and 1971 drawings did the same for men born 1951 (ceiling 125) and 1952 (ceiling 95); and the 1972 drawing assigned numbers to men born 1953, but no one was ever called (Angrist 1990). Because the Biographical Section records full birth dates, we can attach to each male lawyer the draft-lottery number assigned to his birthday. Within a birth cohort, the assigned number is (approximately) random, allowing us to compare draft-eligible men (numbers at or below the ceiling) with their cohort-mates above it to identify the impact of draft risk on career outcomes for men.

We observe 53,875 men born from 1944–1953 in the 1981 Biographical Section. Appendix Figure 39 plots lottery number means across several outcomes of interest, including percentage reporting military service, age at graduation, and percentage of firm members and associates listed as partners. To estimate the impact of induction risk, we use the following specification:

$$Y_{i,dmy} = \alpha + \beta \text{eligible}_{dmy} + \gamma_m + \delta_y \tag{13}$$

Here, $Y_{i,dmy}$ is our outcome of interest for individual i both on day d month m and year y, and $texteligible_{dmy}$ indicates that day dm was below the ceiling in year y. If we were analyzing one birth cohort and numbers were truly randomly assigned, it would be enough to calculate the difference in means between eligible and ineligible men ($\hat{\beta}$). However, in practice, the lottery assigned higher numbers to days in December due to an error in the mixing process (Fienberg 1971; Rosenblatt and Filliben 1971), so we include birth-month fixed effects γ_m to adust for this. Additionally, when we pool across cohorts, we include birth-year fixed effects δ_y. Standard errors are adjusted for heteroskedasticity.

Table 3 reports results for a variety of outcomes of interest. Draft-eligible men born 1944–50 report 2.4 p.p. higher rates of military service relative to a base of 9.8% (Panel A Row 1). These men were also 2.2 p.p. more likely to have service extending into the 1970s (Panel A Row 2), suggesting that this increase in service took place primarily in the 1970s. Draft-eligible men graduated from law school 0.15 years older (Panel A Row 3) and were 4.1 percentage points less likely to graduate on time (Panel A Row 4). In 1981, twelve years after the drawing, the impact of the draft disruption is still visible on the partnership ladder: draft-eligible men are less likely to be listed as members of their firms (Panel B Row 5) and more likely to remain associates (Panel B

Row 6), and the gap grows as we restrict the sample to men further up the career ladder. Across our entire sample, draft-eligible men are 1.4 p.p. less likely to be partner (Panel B Row 5); among all men who are associates, draft-eligible men are 1.9 p.p. less likely to be partner (Panel B Row 7); and among men listed at firms with ten or more listed lawyers, where the partnership tournament is most structured, draft-eligible men are 2.6 p.p. less likely to have made partner (Panel B Row 8). Each 100 additional (safer) lottery numbers raise big-firm partnership by roughly one percentage point, and estimates by lottery-number group (Appendix Table 5) show effects in every group that was actually called, while the never-called "near miss" group (numbers 196–230) is indistinguishable from the safest numbers, suggesting that much of the penalty operates through actual calls, not the expectation of being inducted.

Just as we found in our cross-cohort comparisons in the previous section, the lottery moved men's career timing and careers but not their presence in the bar. Appendix Figure 40 plots the average number of men across several bins of birth dates, and we find no evidence of a relationship between draft number and the count of men present in the 1981 Biographical Section. Finally, as a check on the validity of our empirical design, we replicate the design for the 1971 lottery on the cohort born 1953 and find null effects (almost entirely) throughout (Table 3, column 3); the single nominally significant placebo coefficient, age at graduation, is less than half the treated estimate and is not echoed in the on-time rate.

5.3 Women's Entry and Careers at Draft-Exposed Schools

We now use our earlier identification strategy comparing women at full-time compared to part-time programs to evaluate the impact of the change in draft deferment rules on women's representation in the practicing bar. The Geographical Bar Register records each lawyer's law school, but not the program division attended, so we limit our sample to schools that can be identified from the RLE data as full-time only (daytime program only; draft-exposed), both, or part-time only (evening-only night schools; control). Because we do not observe the program a student is enrolled in at a given school, we limit our comparison to students at schools with only full-time programs to students at schools with only part-time programs.

5.3.1 Women's Entry

Using the Geographic Bar Register in 1974, 1975, 1976, and 1981, we gather data on all cohorts admitted to the bar between 1958–78 and run the following event study design:

$$Y_{iscd} = \sum_{\tau=1958,\tau\neq 1971}^{\tau=1978} \alpha_\tau D_s \mathbb{1}(c=\tau) + \gamma_s + \delta_c + \eta_d + \varepsilon_{iscd}, \tag{14}$$

Y_{iscd} is an indicator for lawyer i at school s, admission cohort c, and directory edition d, having a female name. D_s indicates that school s operates a full-time program only, which we interact with a set of directory year indicators, omitting 1970, as the first bar-admission cohort of the fall-1968 entering class would be 1971. Our event coefficients, α_τ, measure the difference across full-time programs and part-time programs of the likelihood of a bar-admitted lawyer having a female name relative to 1971. We include school, cohort, and directory year fixed effects and cluster standard errors by school. We also summarize our results with a difference-in-differences design of the form:

$$Y_{iscd} = \alpha_{\text{DiD}} D_s \mathbb{1}(c \geq 1971) + \gamma_s + \delta_c + \eta_d + \varepsilon_{iscd}, \tag{15}$$

Figure 8b displays the result as the bar-side analogue of the enrollment event study. We find little evidence that the differential gender composition across full-time only and part-time only programs changes between bar admission cohorts between 1958 and 1970, with the event coefficient in 1958 close to 0. Our event coefficients after 1970 are all positive, climbing to +4 to +8 percentage points across the 1974–77 cohorts. Our summary difference-in-differences estimate suggests that, across the 1971-1978 graduating cohorts, the policy change let to a 4.9 p.p. increase in women's representation ($p < 0.001$). This is evident from the raw data as well. Figure 8a plots the fraction of bar admission cohorts who are women across full-time only and part-time only schools between 1958 and 1978. This statistic is virtually identical for the two groups in 1970, with the gap emerging and widening with the 1971 cohort. We also plot the fraction of women who graduated from schools that operated both a full-time and part-time program. This closely tracks women's representation at full-time only programs, suggesting that they were impacted by the policy change in a similar way.

5.3.2 Robustness to Prestige Differences.

The main threat to identification in this design is that women's representation in full-time only programs might have trended differently than part-time programs, even in the absence of the policy change. In particular, full-time only schools are generally more prestigious than part-time only programs, so our contrast risks capturing an elite-school trend rather than the draft. We run several robustness checks that augment our difference-in-differences design in equation (15), reporting the results in the first column of Appendix Table 4. We measure prestige as before, using the log of the number of volumes in the law library in 1969. We include a cohort trend in the log number of volumes (Row 2), drop the most prestigious third of full-time only schools (Row 3), and keep only the bottom third of full-time only schools across the prestige distribution (Row 4). Our results survive all of these adjustments.

5.3.3 Women's Careers

In addition to simply measuring representation in the bar, we observe many career-related outcomes in the directory data. With these outcomes, however, our concerns about differences across full-time only and part-time only programs are magnified, as institutional prestige might have an outsize impact on the probability of making partner, for example. Accordingly, to correct for any school-specific unobserved differences in trend, we run a triple difference-in-differences design, differencing out any estimated policy impact for men to remove potential bias from our DiD estimator.[22] We are able to measure 20 separate career outcomes, listed in the rows of Appendix Table 4. Each outcome is an indicator Y_{iscd} that lawyer i is observed to have some characteristic (e.g. working as an associate, working for a private firm) in directory year d. For each outcome, we first estimate Eq. (14) separately by sex, and then report the triple DiD estimator, given by

$$\begin{aligned} Y_{iscd} = {} & \tau \left(D_s \times \mathbb{1}[c \geq 1971] \times \text{Fem}_i\right) \\ & + \lambda_1 \big(D_s \times \mathbb{1}[c \geq 1971]\big) + \lambda_2 \text{Fem}_i \times D_s + \lambda_3 \text{Fem}_i \times \mathbb{1}[c \geq 1971] + \\ & + \lambda_4 \text{Fem}_i + \gamma_s + \delta_c + \eta_d + \varepsilon_{iscd}, \end{aligned} \tag{16}$$

τ is our coefficient of interest, which reports the gender difference in the exposure effect, net of any program-type×time trend common to both sexes. $\boldsymbol{\lambda}$ indexes the additional terms needed to ensure that α_{DiDiD} has the proper interpretation. Table 4 reports the full battery, of which we highlight the most prominent results here. The draft induced women to move into the *private-firm* track ($\tau = +0.163$) and *federal* government ($\tau = +0.024$), and away from solo practice ($\tau = -0.103$), state and local government ($\tau = -0.046$), and the judiciary ($\tau = -0.018$). They also attained seniority there, by every measure we can construct: the top peer-review rating ($\tau = +0.038$), named partnership as printed on the firm's display card ($\tau = +0.082$), and a size-based partnership measure defined as practicing as a non-associate in an office of more than ten lawyers ($\tau = +0.060$).

5.3.4 Career outcomes at defined follow-up periods

Our main design in this section pools all observations across multiple directories. This uses all available information, but might introduce bias because of the mechanical relationship between bar admission cohort and career length (for fully attached workers). For example, for a fixed directory year, earlier cohorts will be observed at a later career stage than later cohorts, potentially confounding cross-cohort comparisons. To correct for this, we run an additional design using all years of data we have collected and obtaining information for each cohort at a fixed follow-up time. Specifically, for a chosen horizon K and bar admission cohort C, we collect labor market information from the directory closest to year $C + K$, so every cohort is observed at approximately

[22] Since women's representation and men's representation are collinear, we cannot use this design in the previous section.

the same career age.[23] We consider two time horizons: 6 years out as well as 13 years out.

This allows us to estimate an event study design of the form:

$$Y_{isc} = \sum_{\tau=1958,\tau\neq1971}^{\tau=1978} \alpha_\tau D_s \mathbb{1}(c=\tau) + \gamma_s + \delta_c + \varepsilon_{isc} \tag{17}$$

Y_{isc} is now an indicator for an outcome of interest for lawyer i graduating from school s in bar admission cohort c, where this outcome is measured in the directory year closest to $c+K$. The event study coefficients have the same interpretation as before, and we no longer need to include directory year fixed effects with a fixed follow-up period.

These results are presented in Figure 9, which reads as a career arc in four panels. Figure 9a reports results for an outcome indicating that lawyer i has a female name, measuring persistence in the bar six years after admission. As before, we find a clear increase in event coefficients from a flat pre-period to +8.5pp by the 1977 cohort, with a summary difference-in-differences estimate of +6.2pp ($p = 0.001$). Figure 9b presents results for an outcome indicating that lawyer i holds a firm associate job six years after bar admission. This model, as well as the other outcomes in Figures 9c and 9d, are estimated separately by sex, and the event coefficients are binned in groups of two years (relative to the 1969 and 1970 admission cohorts) to improve precision. We estimate a large increase in the probability of women holding an firm associate job: over the entire post-period, we estimate a 12 p.p. ($p = 0.001$) increase for women, relative to a statistically insignificant 2.1 p.p. increase for men. The gap between event coefficients for women and men is roughly constant across years, suggesting that this impact appears almost immediately in the early 1970s. Figure 9c presents results for an outcome indicating that lawyer i works at a private firm six years after bar admission. This corroborates our finding in Table 4, as we find that women at draft-exposed schools are 14.4pp ($p = 0.02$) more likely to be working at a private firm after 6 years, against a 1.1pp decline in this probability for men. Finally, Figure 9d looks at a long run career outcome, thirteen years after admission to the bar, reporting results for an outcome indicating that lawyer i is listed as partner. We estimate a small but significant increase for men of +2.3p.p. ($p = 0.01$), suggesting that the premium for attending a more prestigious law school might be increasing over this time period. However, our estimate for women of a 7.0p.p. ($p = 0.01$) increase suggests that increased access to full-time programs, as a result of draft pressure, likely lifted the probability of women making partner, a highly desirable career outcome for lawyers.

[23] Due to data limitations, we cannot obtain an exact match for every cohort, but this approach still drastically reduces the cross-cohort comparison problem.

6 Reinforcing Policies

6.1 Did Federal Anti-Discrimination Policy Matter?

Through our empirical designs, we find substantial evidence that the 1968 change in deferment status for full-time law students mattered for women's enrollment, but across all designs, these gains are largely limited to the late 1960s. Thus, this change has limited direct explanatory power for the secular rise in women's representation we see in Figure 2a during the 1970s. Accordingly, we now turn to analyze the (potential) complementary role of anti-discrimination policy.

6.1.1 Title IX

Most accounts of the impact of anti-discrimination policy on women's enrollment in law schools focus on Title IX of the Educational Amendments of 1972, which prohibited any educational institution receiving federal funding from discriminating on the basis of sex for graduate or professional admissions. Fossum (1983)[p. 229] calls 1972 "the critical year in terms of the governmental prohibition against sex discrimination in the admission of students to the nation's law schools," but concludes that it is difficult to say definitively that Title IX caused the rapid rise in women's enrollment, noting the preponderance of changes occurring around the same time, including the removal of deferments that we study here. More recently, Rim (2021) studies the impact of Title IX on graduate education writ large, finding that this law contributed to a subsequent rise in women's enrollment. In particular, Rim (2021) notes 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, after its passage in 1972.

To explore this potential mechanism, we collect institution-level data on the mix of funding from the 1968-69 Higher Education General Information Survey (HEGIS), accessed via ICPSR. Following Rim (2021), we use the share of revenue provided by the federal government as our preferred measure of exposure. This is defined as revenue received from federal appropriations, sponsored programs (including research), and student aid, as a fraction of the sum of educational and general revenues and student aid grants.[24] We match law schools appearing in the 1967 *Review of Legal Education* to the institution they are affiliated to in the HEGIS survey. While this is a very useful measure of institutional reliance on federal funding, it is a weak metric for a given law school's reliance, as it pools both revenue and federal funding across the entire institution. Accordingly, we restrict our sample to all institutions where the law school itself is identified in the HEGIS data. We report all schools included in this analysis in Appendix Table 3.

It is important to note that this sample is extremely limited relative to our previous analysis - we are able to include only 12 law schools in this analysis. Further, these programs have very

[24] This is calculated from the HEGIS survey form as follows: (Line Item 4 + Line Item 10 + Line Item 16 + Line Item 27) / (Line Item 1 + Line Item 26). See Rim (2021) for details on what is included in the survey measure of revenue and student aid.

little dependence on federal funding, as the maximum federal funding share is around 4%. However, several institutions receive no federal funding, which we would expect to be unaffected by the policy, providing us with an untreated group. This allows us to run an event study design of the form:

$$Y_{i\rho t} = \sum_{\tau=1960, \tau\neq 1974}^{\tau=1980} \alpha_\tau \mathbb{1}(d_{i,1968} > 0)\mathbb{1}(t = \tau) + \gamma_{i\rho} + \delta_t + \varepsilon_{i\rho t} \tag{18}$$

Here, Y_{it} is the fraction of first-year law students at institution i in program ρ in year t who are women, and $d_{i,1968}$ is the amount of federal funding institution i received as measured in the 1968-69 HEGIS survey. α_τ measures how the response of institutions receiving positive federal funding in academic year 1968 relative to those receiving no federal funding changes relative to the difference in Fall 1974. While Title IX was passed in 1972, we use the Fall of 1974 as the omitted year, as this is the last cohort that enters before compliance regulations were finalized in May 1975 (Rim 2021). We use a standard TWFE design with institution-program ($\gamma_{i\rho}$) and year (δ_t) fixed effects, respectively. Our estimates are weighted by total first-year enrollment, and we cluster our standard errors at the institution level. We summarize the event study results with a difference-in-differences estimator of the form:

$$Y_{i\rho t} = \alpha_{DiD} \mathbb{1}(d_{i,1968} > 0)\mathbb{1}(t \geq 1975) + \gamma_{i\rho} + \delta_t + \varepsilon_{i\rho t} \tag{19}$$

Our results are presented in Appendix Figure 10b. The standard errors on our event coefficients are large due to the limited number of institutions, but the pattern of the point estimates is indicative of a sizable impact of Title IX. Across the 14 years prior to policy regulations coming into force, we see change in the difference in women's representation across exposed and unexposed schools. However, the pre-period coefficients arguably understate the similarity of these trends. We plot the raw fraction of students in the treated and untreated group between 1960 and 1980 in Figure 10a. The trend in both groups is essentially identical between 1960 and 1974, including the sharp acceleration of women's representation that occurs around 1970. These trends only decouple beginning in 1975, reflected by a rise in the event coefficients in Figure 10b in the same year. Our power is limited, but our difference-in-differences estimate suggests that Title IX lifted women's representation by 4.3 percentage points (significant at the 10% level).

6.1.2 Earlier Activist Pressure

As Helgerman (2025) notes, Title IX was the encapsulation, rather than the beginning, of activist pressure to curb sex discrimination in higher education. Before it was enacted in 1972, the Women's Equity Action League (WEAL) emerged as the most active national organization challenging sex discrimination in higher education. Lacking a statutory framework directed specifically at education, WEAL strategically leveraged Executive Order 11246 (amended by EO 11375), which prohibited federal contractors from discriminating on the basis of sex in employment, as an indirect but potent enforcement tool. Because most major universities received substantial federal research funding, the federal government had meaningful leverage over university employment practices. Beginning in 1970, WEAL filed many formal complaints with the Department of Labor's Office of Federal Contract Compliance (OFCC), targeting elite campuses such as Harvard, Yale, Michigan, Wisconsin, and the University of California system, as well as many regional and state institutions. These filings constituted one of the earliest systematic efforts to challenge gender discrimination in academia through federal law.

The complaints documented a wide array of discriminatory practices affecting women faculty—and, indirectly, women students. They typically alleged the systematic exclusion of women from tenure-track positions, disproportionate assignment to low-rank or non-tenure appointments, unequal salaries for equivalent work, opaque or biased promotion procedures, and the use of marital or anti-nepotism rules that effectively barred many married women from academic employment. Many complaints alleged quota-like ceilings on women's admissions or hiring, rigid maternity-leave and benefits policies, and the near absence of women in senior leadership roles. WEAL often supplemented these allegations with its own surveys, anonymous letters from women faculty, and data compiled from publicly available catalogs, faculty directories, and institutional reports. While these complaints focused on hiring discrimination, they often involved allegations of admissions discrimination, particularly for first professional programs like law, where degree programs function as a barrier to entry to the profession. To evaluate the role of this pressure, we leverage the timing of complaints and the institutions targeted. Additionally, since these complaints target institutions housing a large number of law schools, this allows us to expand the limited sample in the previous section to evaluate the impact anti-discrimination policy across a far more representative set of institutions.

We collect information on complaints of violations of EO 11246 that were filed by WEAL and allied organizations from congressional hearings on proposed amendments to the Fair Labor Standards Act in 1970 (U.S. Congress 1971). We assume this report, filed by WEAL, is up to date as of the beginning of the hearing; this begins on May 26, 1971, and the latest complaint reported is on May 24, 1971. We identify 42 complaints filed against institutions that include law schools, which are reported in Appendix Table 2. In addition, in April 1971, the Professional Women's Caucus files a complaint against all law schools (Sandler 1973).[25] Consequently, even though there is variation across time in the date of complaint, this variation is very limited, as all law schools are effectively "treated" in April 1971. Consequently, we consider a simple empirical design, leveraging whether or not an institution is treated earlier or later. To operationalize this, we code a law school as treated early if it receives a complaint from WEAL (or an allied organization) during calendar

[25] This is included in Appendix Table 2, though it was not included in WEAL's report to congress.

year 1971. We use this treatment definition as part of an event study design of the form:

$$Y_{it} = \sum_{\tau=1960,\tau\neq 1969}^{\tau=1980} \alpha_\tau \text{Early}_i \mathbb{1}(t=\tau) + \gamma_i + \delta_t + \varepsilon_{it} \tag{20}$$

Our outcome remains the fraction of women enrolled at institution i in year t; we include only full-time programs for this design and drop the program subscript ρ. Early_i is our treatment indicator, defined in the previous paragraph, and we exclude an indicator for Fall 1969 as this was the last cohort that began classes before the first WEAL complaint was filed in January 1970. We utilize a simple two-way fixed effects design that is weighted by total enrollment and clustered at the institution level.

Our results are presented in Figure 10d. We take this as suggestive, but not definitive, evidence that WEAL complaints prompted an increase in women's enrollment in the early 1970s. There is a subtle pre-trend, but we find a marked increase in women's representation at programs treated early relative to those treated later in the first half of the 1970s. However, later in the decade, this effect attenuates and the event coefficient in 1980 is around 0. We suspect this is likely a result of the crystallization of the enforcement apparatus of Title IX in 1975, given our previous result and the clear drop in the event coefficient. To formalize this result, we summarize our event study coefficients with a difference-in-differences design of the form:

$$Y_{it} = \alpha^{\text{SR}}_{\text{DiD}} \text{Early}_i \mathbb{1}(t \in [1970, 1974]) + \alpha^{\text{LR}}_{\text{DiD}} \text{Early}_i \mathbb{1}(t \in [1975, 1980]) + \gamma_i + \delta_t + \varepsilon_{it} \tag{21}$$

Here, we split the post-treatment period into a short run coefficient ($\alpha^{\text{SR}}_{\text{DiD}}$) measuring the policy response between 1970 and 1974, and a long run coefficient ($\alpha^{\text{LR}}_{\text{DiD}}$) measuring the policy response between 1975 and 1980. We find a short run increase in women's representation of 2.6 percentage points, but a long run coefficient that is not statistically distinguishable from 0, suggesting that receiving an early WEAL complaint prompted earlier compliance activity, but as admissions non-discrimination (on the basis of sex) was legislated through Title IX, enforcement activity eliminated this difference by 1980. This is evident in the raw data, plotted in Figure 10c, showing the fraction of students who were women enrolled in institutions receiving early complaints relative to those treated later. A sizable gap between these groups opens in the 1970s, closing by the end of the decade.[26]

Taken together, these results suggest that anti-discrimination policy did play an important role in improving women's representation in law. Title IX provided a significant boost once its regulations were finalized, though these impacts were "brought forward" into the early 1970s at programs targeted by EO 11246 complaints from WEAL.

[26] Appendix Figure 22 demonstrates robustness of our main design to adjusting for the institutional receipt of an early WEAL complaint by including this term as a control interacted with year fixed effects.

6.2 Contraception access

A possible competing explanation is that oral contraception, rather than the draft, generated women's differential entry into full-time programs: Goldin and Katz (2002) argue that the pill made long, uninterruptible professional training rational for young women, and its diffusion coincides with our treatment window. Appendix D details the legal codings, exposure measures, and underlying data construction, reports the full estimates (Appendix Tables 6 and 7), and replicates the law career results of Goldin and Katz (2002) in the Martindale-Hubbell directory data. We document in Appendix Table 7 that cohort-level pill access is associated with a 68 percent increase in women's lawyer production per college woman and a 13 percentage point rise in women's share of the bar, which is the same direction as, and only modestly below, the roughly twofold increase in professional employment that Goldin and Katz's census estimates imply (with no corresponding movement for men, and robust to both legal codings and to population and college-graduate denominators).

Two tests address how these changes might interact with our estimates on the impact of the Vietnam draft, which we show in Appendix Table 6. First, exploiting the staggered state liberalization of contraceptive access for unmarried women under age 21, we interact the full-time $\times$ post term with the pill-access regime of each school's state: if the pill mechanism drove the full-time/part-time differential, it should be concentrated where young women had early legal access. It is not. The effect obtains in late-liberalizing states (+4.7 percentage points, $p = .006$, under the Goldin–Katz coding; +3.8 points, $p = .016$, under the coding of Bailey 2006), cohorts who should not have been exposed to increased access. Second, because state laws cannot capture nationally diffusing access, we add to the baseline specification the interaction of full-time-only status with each lawyer's cohort-level probability of legal pill access before age 21. The draft coefficient is unmoved (+4.88 to +4.83 points), the pill interaction is a precise zero.

7 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. Initial draft-driven gains were reinforced by federal anti-discrimination policies and access to contraception.

These changes altered the composition of the practicing bar. Men who faced higher draft risks were more likely to graduate at a later age, and these delays in education are echoed in worse career trajectories, including fewer firm partners. Newly entering women were able to join firms and become partners.

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.

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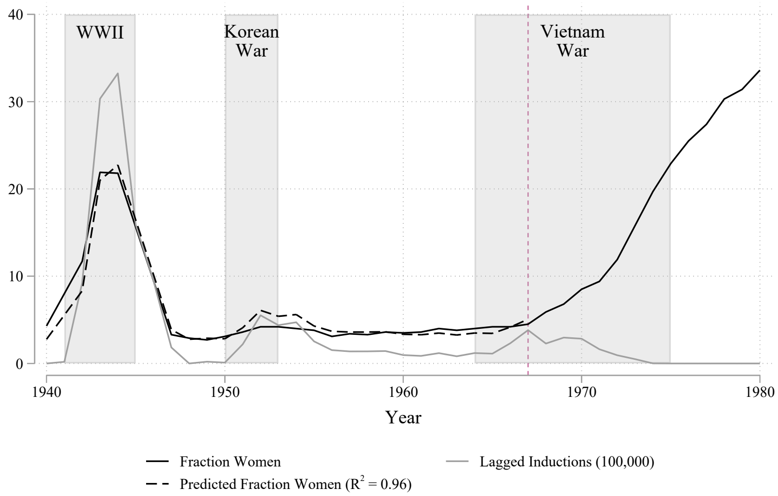
Figure 1: Enrollment statistics

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



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

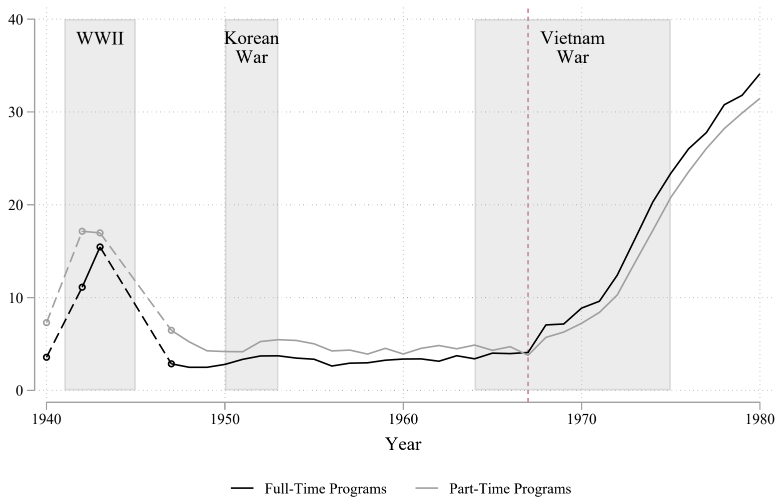
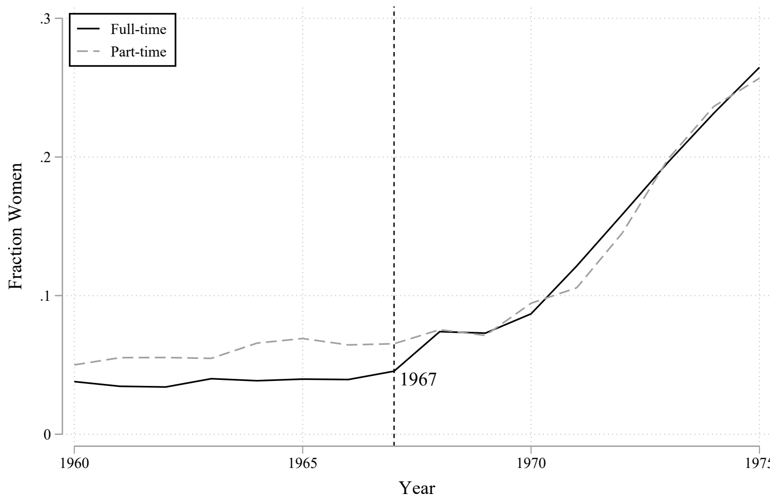


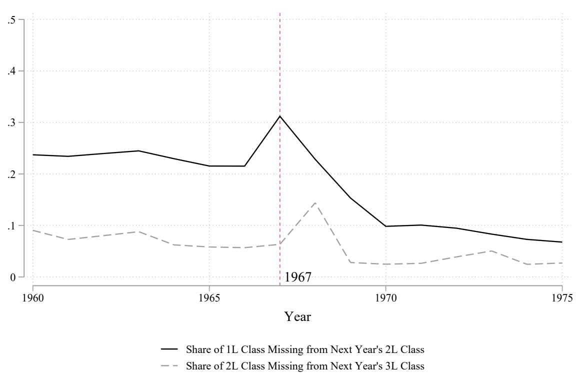
Figure 1a plots the percentage of all law students who are women between 1940 and 1980, collected from Table 27 of Abel (1989), against the total number of inductions conducted by the Selective Service System in the previous year between 1940 and 1973, collected from `www.sss.gov/history-and-records/induction-statistics/`. 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 students who were women at U.S. law schools separately for full-time and part-time programs. Data are collected from the *Review of Legal Education* between 1947 and 1980. Before 1947, data are only available in 1940, 1942, and March 1943 (plotted in 1943); we interpolate with the dashed line for years when data are missing. Data for Fall 1940 and Fall 1942 are collected from Gulliver (1943), and data for March 1943 are collected from National Conference of Bar Examiners (1943). In both plots, we mark the year before student deferments from the Vietnam Draft were revoked (1967).

Figure 2: Aggregate Attrition for Men

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



(b) Full-Time Programs



(c) Part-Time Programs

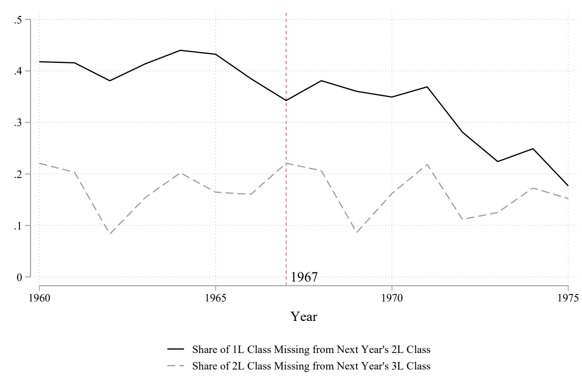
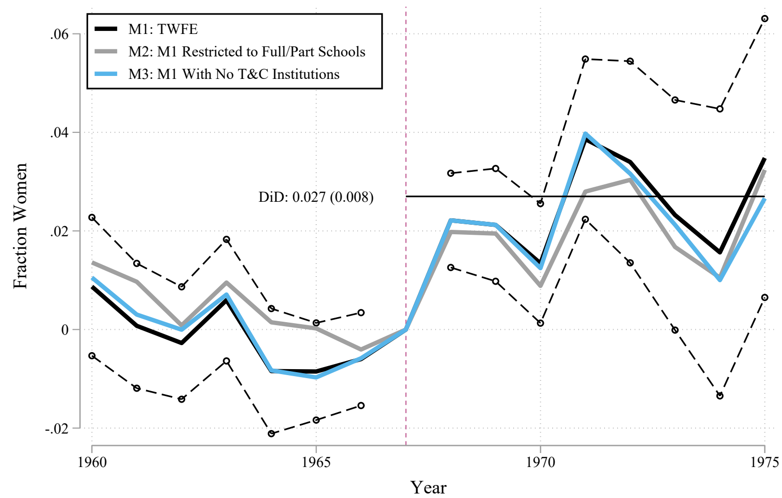
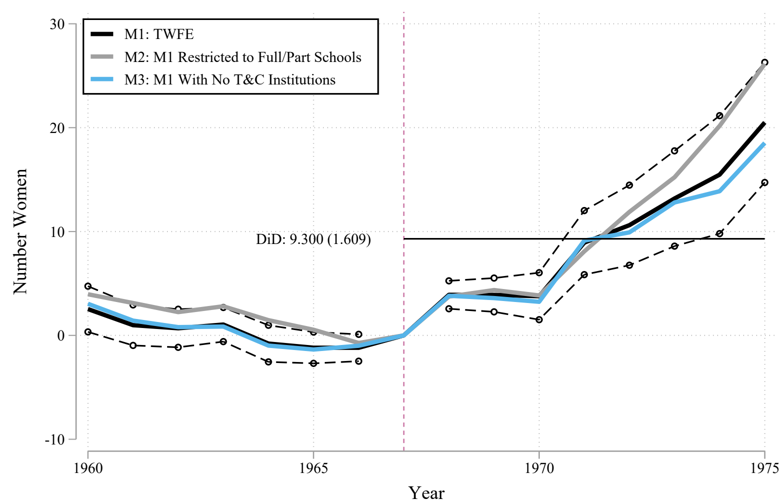


Figure 2a plots the fraction of all first-year students who were women at U.S. law schools that reported to the *Review of Legal Education* from 1960 through 1975 separately for full-time and part-time programs. Data are collected from the *Review of Legal Education* in every year (raw levels are plotted in Appendix Figures 37a and 37b). Figures 2b and 2c plot 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 2b plots these series for full-time programs. Figure 2c plots these series for part-time programs. In all plots, we mark the year before student deferments from the Vietnam Draft were revoked (1967).

Figure 3: Difference-in-Differences Results



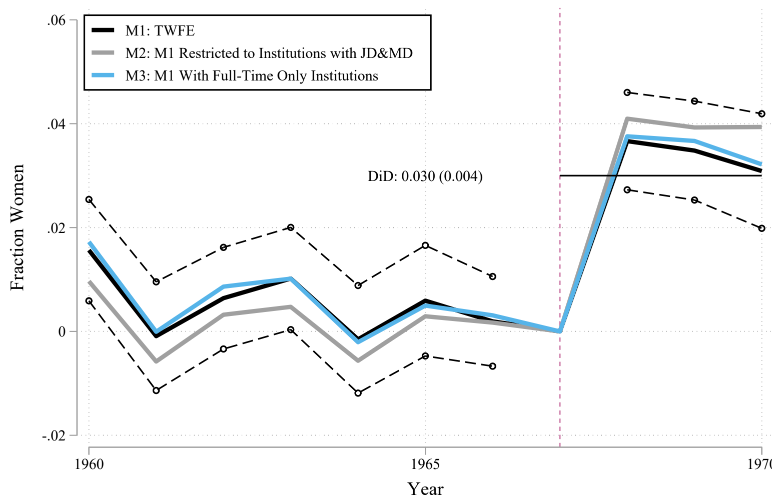
(a) Fraction Women



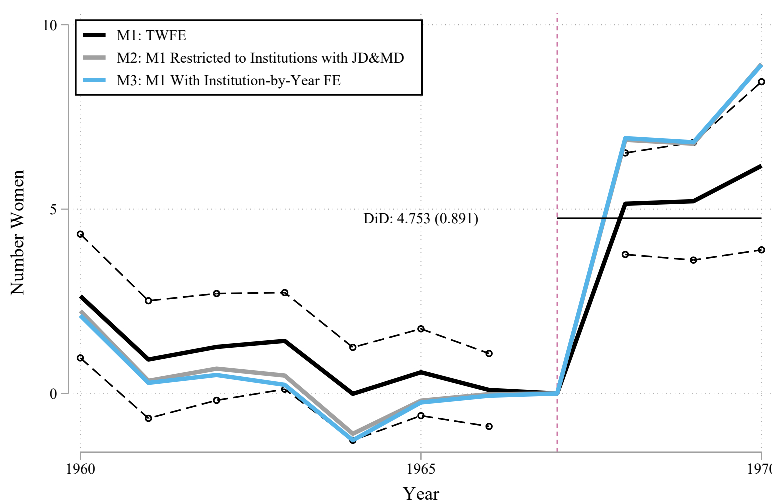
(b) Number Women

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: Law-Medicine Difference-in-Differences Results



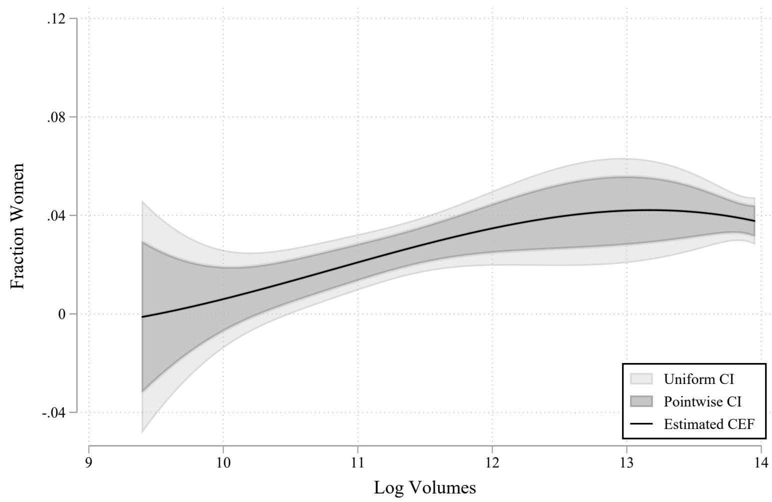
(a) Fraction Women



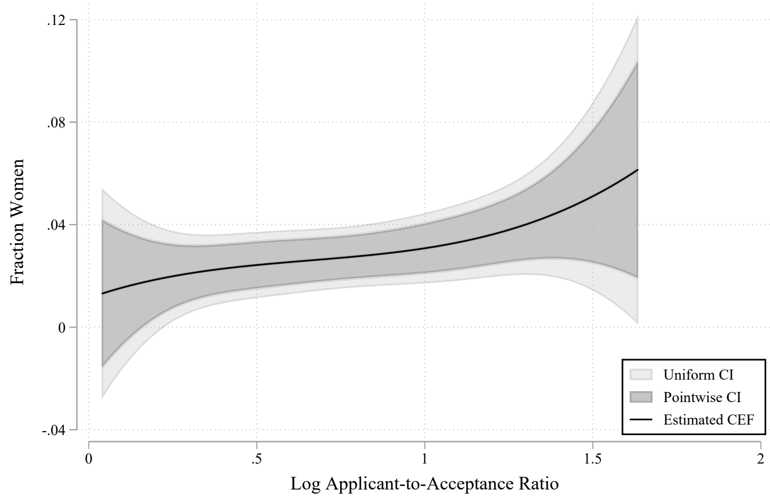
(b) Number Women

Figure 4a plots coefficient estimates from equation (5), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Figure 4b plots coefficient estimates from an alternative specification, where the outcome is the number of first-year students who are women and a control for total enrollment is included. 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. In both figures, Model 2 restricts our sample to include only institutions operating both a medical school and a law school. Model 3 keeps this restriction and adds institution-by-year fixed effects.

Figure 5: Continuous Differences-in-Differences Results



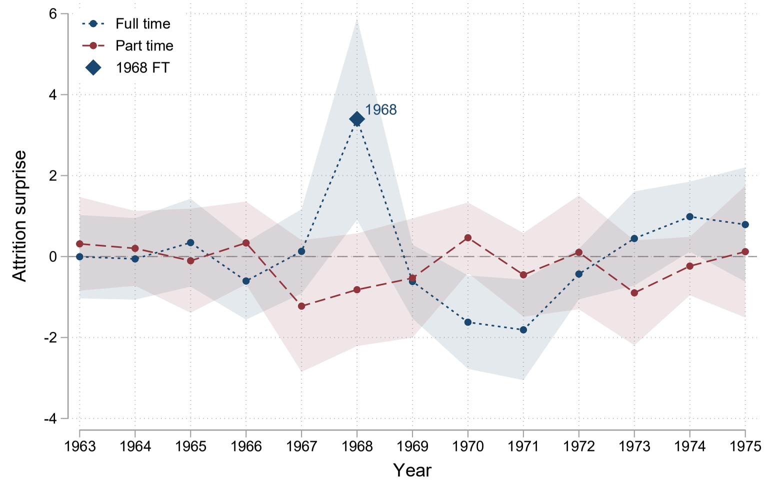
(a) Log Volumes in Law Library



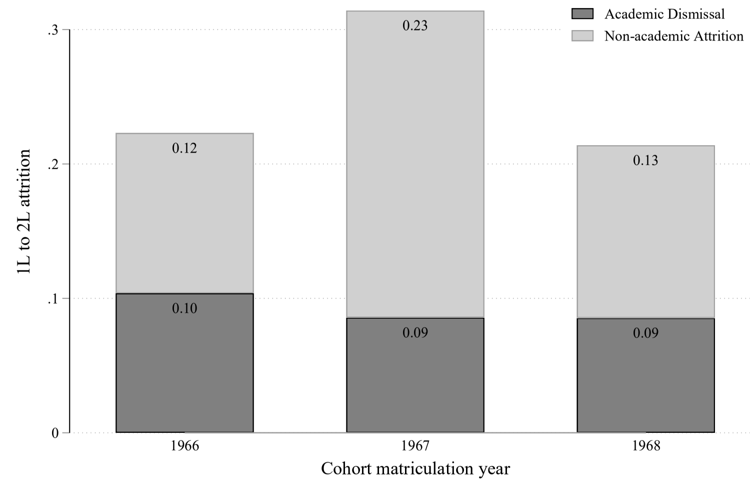
(b) Log Applications per Acceptance

This figure plots estimates from equation (10), 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).

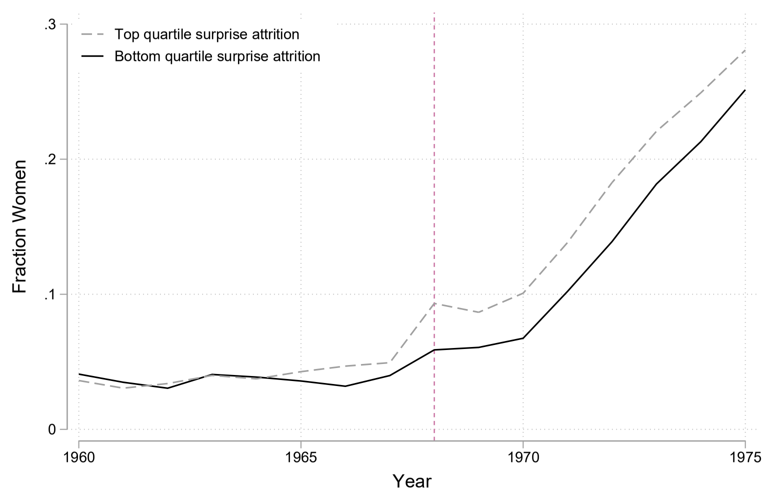
Figure 6: Variation in Attrition Surprises and Prestige



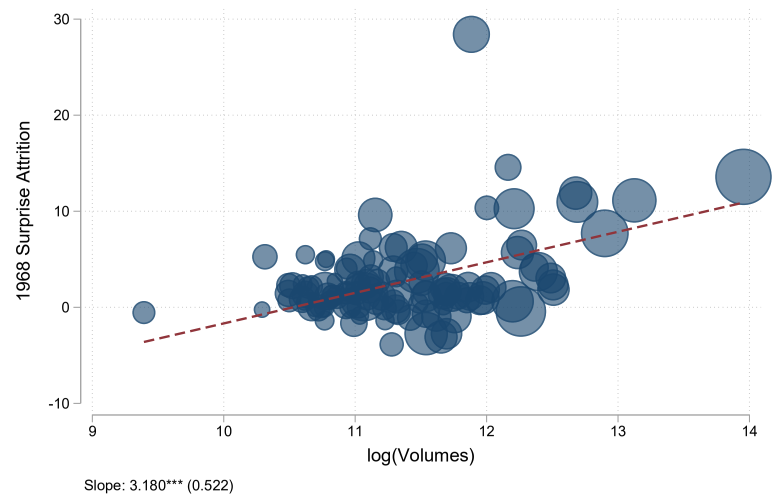
(a) Attrition Surprises Over Time



(b) Academic Attrition



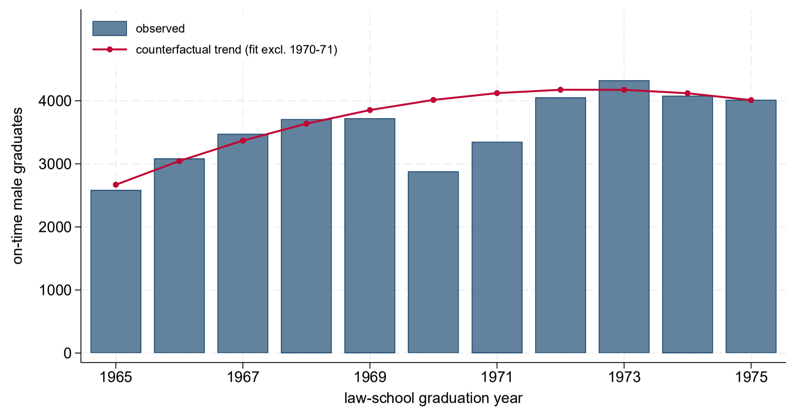
(c) Women's 1L Share by Surprise Attrition Quartile



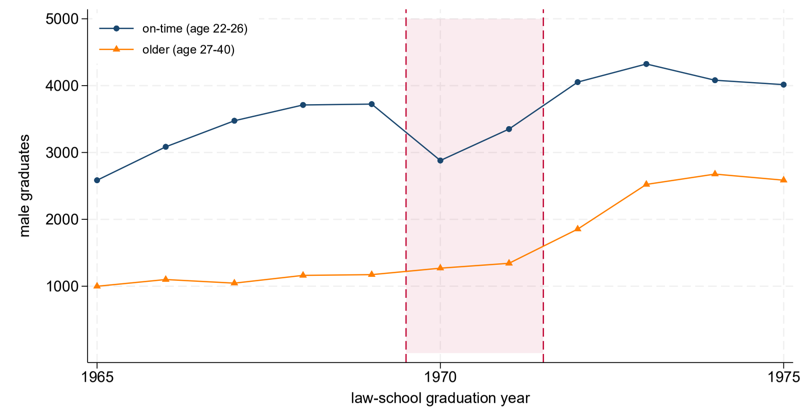
(d) Attrition Surprises and Prestige in 1968

Panel (a) plots the mean standardized attrition surprise $\tilde{S}_i$ for full-time programs by year, estimated using a rolling six-year trailing window, together with a ± 1 standard deviation band. Panel (b) decomposes first-year attrition for the entering classes of 1966, 1967, and 1968 into academic and non-academic components using data from the *Pre-Law Handbook*. Panel (c) shows the fraction of women enrolled in full-time programs that experienced surprise 1968 attrition in the top and bottom quartiles. Panel (d) plot the standardized attrition surprise against log library volumes, with marker area proportional to lagged enrollment and a weighted least-squares fitted line superimposed.

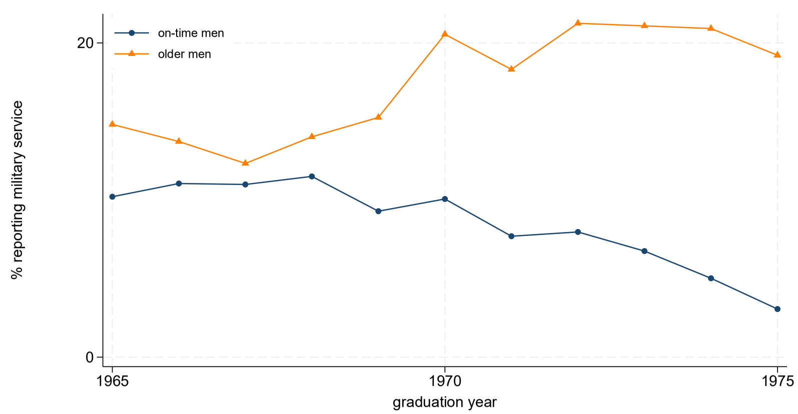
Figure 7: The Draft's Footprint Among Men Graduating in Law



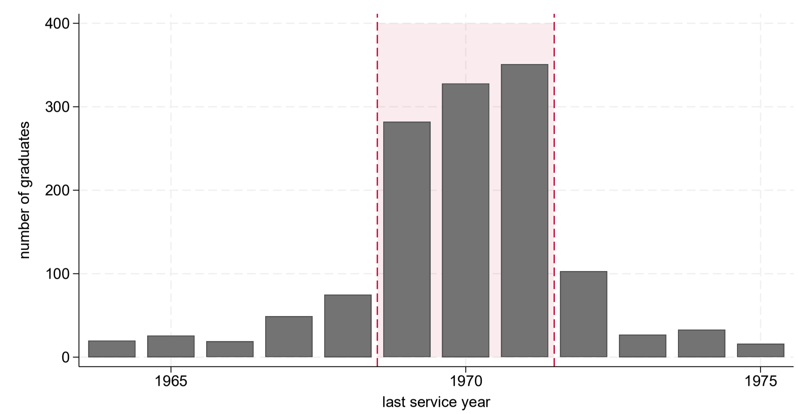
(a) On-time Graduates



(b) On-time Graduates and Older Graduates surge.



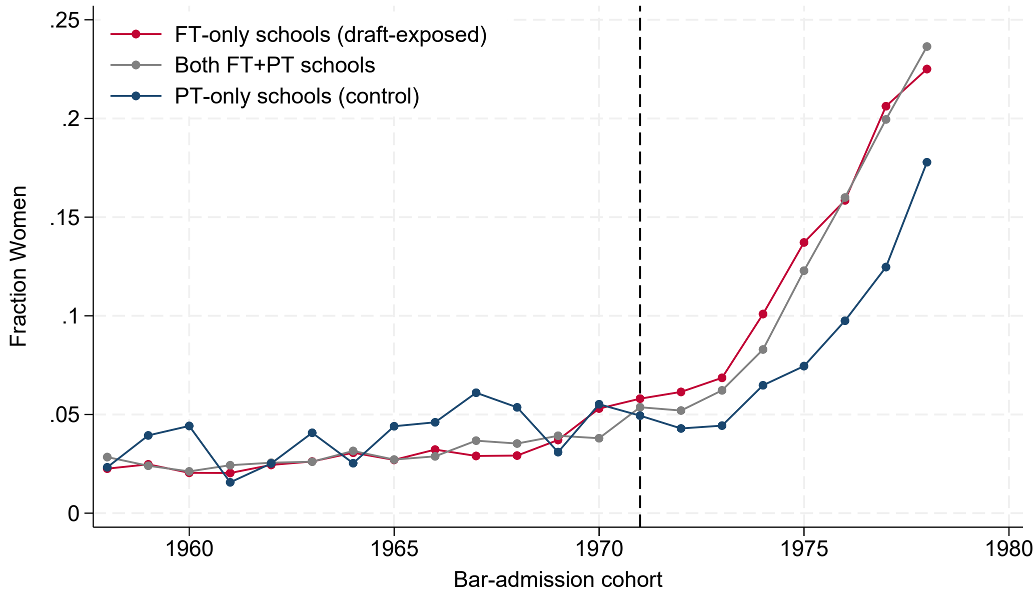
(c) Military Service Rate by Age Group



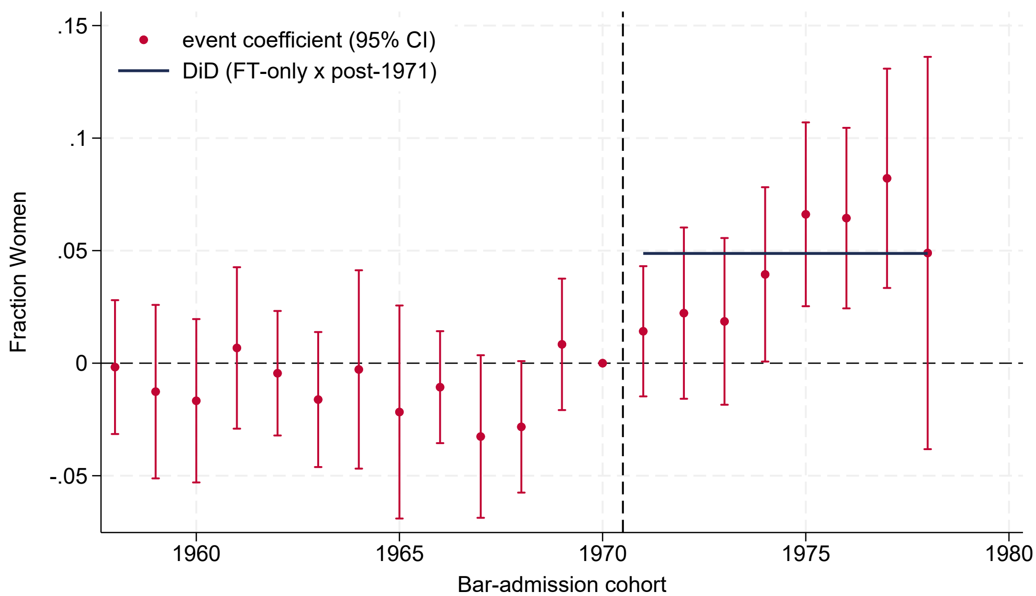
(d) last Year of Service for Older 1972–74 Graduates Who Served

Notes. Panel (a): on-time (age 22–26) male law-school graduates by graduation year (bars) and a quadratic counterfactual fit excluding 1970–71 (line); the implied 1970–71 deficit is ≈1,904 men. Panel (b): male graduates by age at graduation; the on-time deficit is offset by an older-graduate (age 27–40) surge in 1972–74. Panel (c): military-service rate by age group. Panel (d): last year of service for the older 1972–74 graduates who served. Dashed lines and shading mark the disrupted 1970–71 graduation years (panel b) and the 1969–71 peak-draft service years (panel d). Source: 1981 Martindale–Hubbell Biographical Section.

Figure 8: Full-time and part-time women's share in the bar



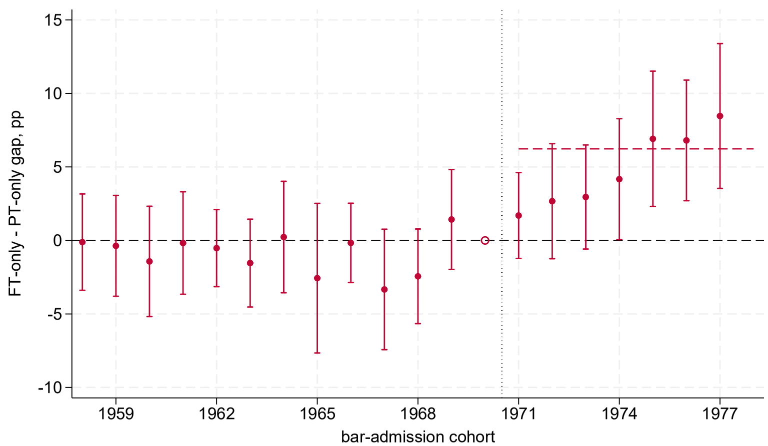
(a) Women's share by cohort and school type, 1981 edition.



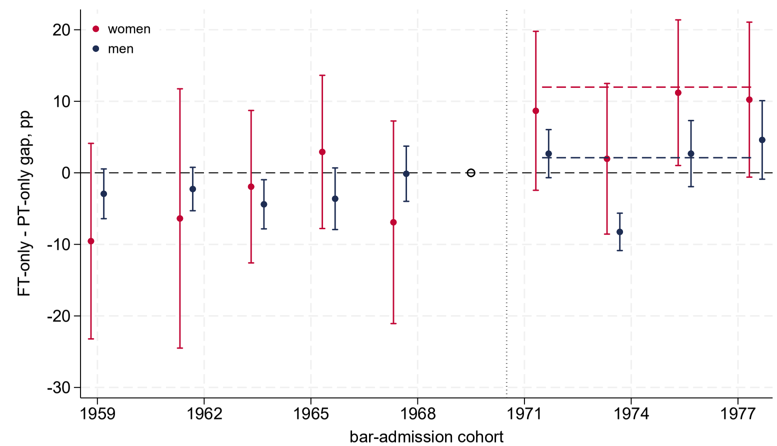
(b) Event study (pooled 1974–81 editions).

Notes. Panel (a): women's share of each admission cohort by school type in the 1981 edition. Panel (b): event-study coefficients from the two-way fixed-effects model in Eq. (14) with cohort-specific treatment interactions, omitted cohort 1970; the horizontal line is the summary difference-in-differences (+0.049; wild-cluster $p = 0.001$).

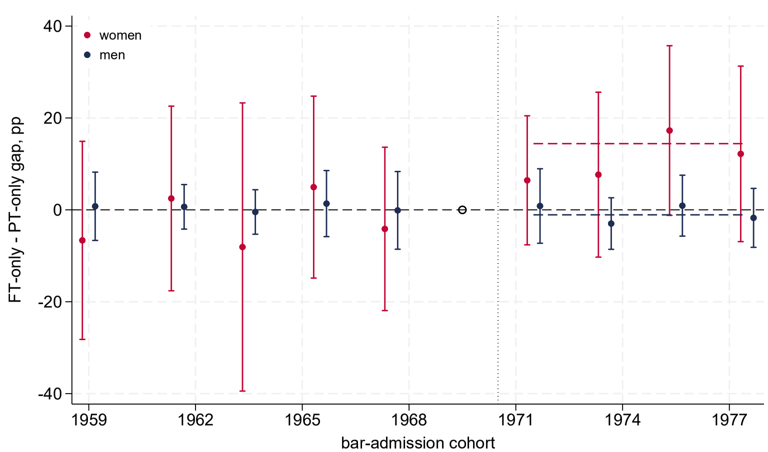
Figure 9: The draft cohorts' careers at fixed follow-up: entry at six years, partnership at thirteen



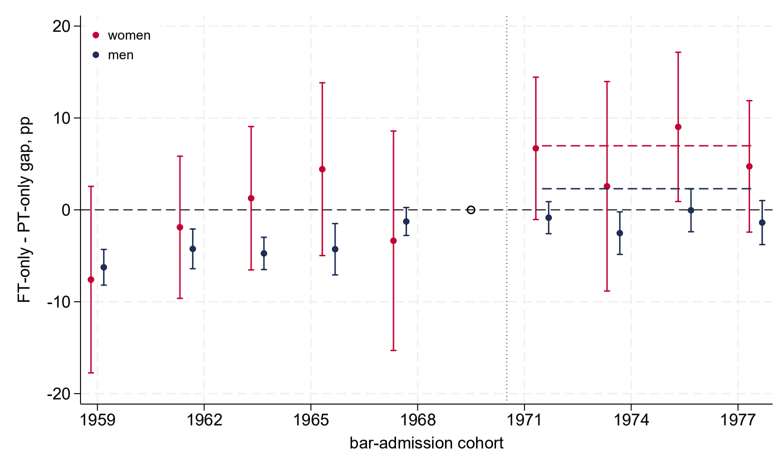
(a) Composition: women's share (6 years out)



(b) Associate (6 years out)



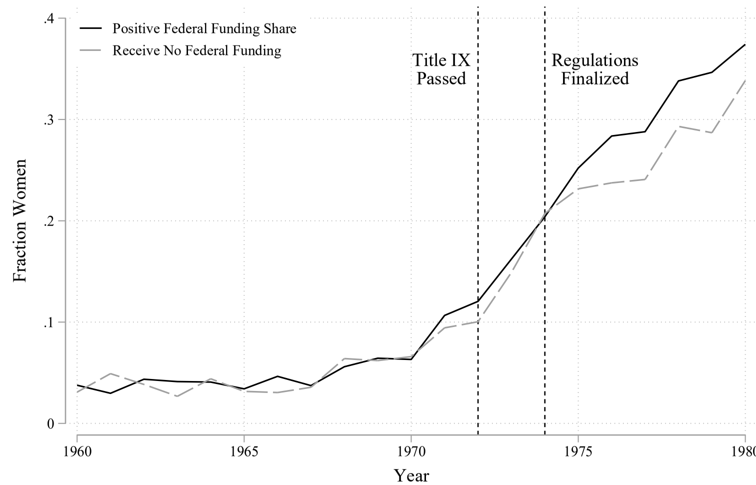
(c) Private firm (6 years out)



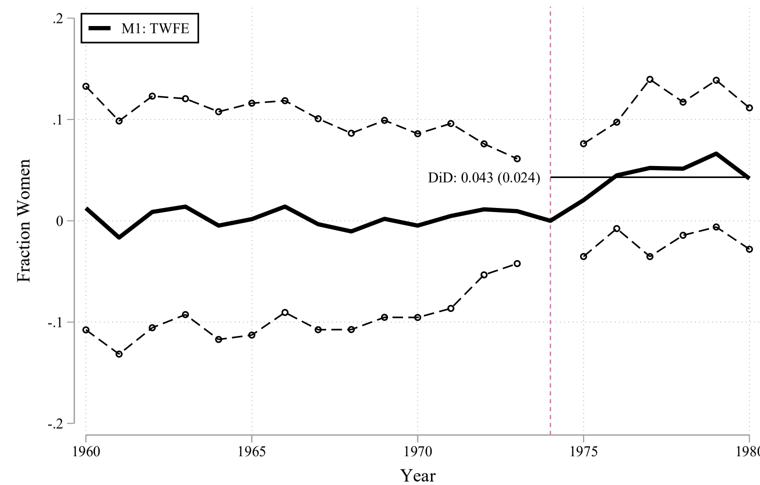
(d) Partner (13 years out)

Notes. Each admission cohort is observed in the single directory edition nearest admission+6 years (panels a–c; edition gap $\leq$ 2 years) or +13 years (panel d; gap $\leq$ 3). Panel (a): composition (women's share, all lawyers), per-cohort coefficients, omitted cohort 1970. Panels (b)–(d): outcomes by sex in two-year cohort buckets, omitted bucket 1969–70. Two-way fixed effects (school and cohort; edition fixed effects absorbed by cohort fixed effects), FT-only vs. PT-only schools, admission cohorts 1958–78, SE clustered by school; dashed lines are the summary difference-in-differences, with wild-cluster bootstrap p-values reported in the text.

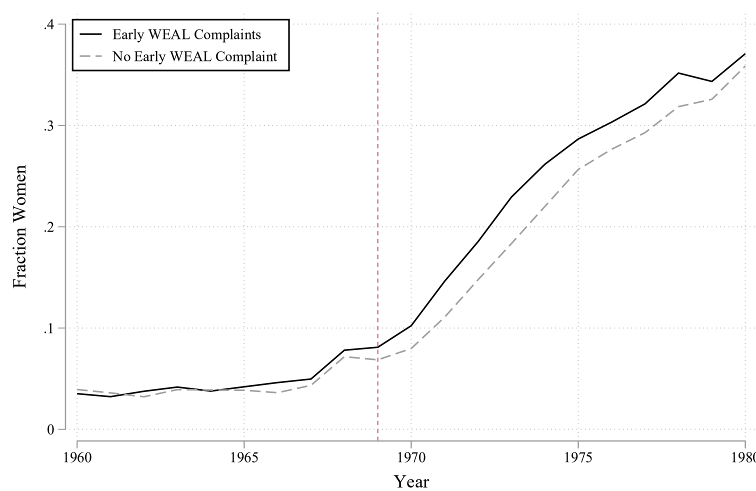
Figure 10: Anti-Discrimination Results



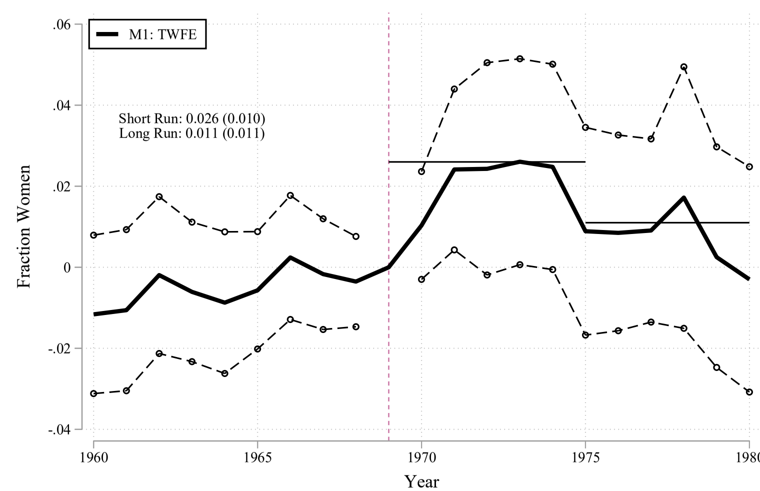
(a) Raw Enrollment By Federal Funds Receipt Status



(b) Title IX Event Study Results



(c) Raw Enrollment By Timing of WEAL Complaint



(d) WEAL Complaint Event Study

Figure 10a plots the fraction of first-year students enrolled who are women at law programs identified in the 1968-69 Financial Statistics portion of HEGIS separately for institutions receiving and not receiving federal funding. Figure 10b presents difference-in-differences estimates from equation (19). Figure 10c plots the fraction of first-year students enrolled who are women at schools receiving and not receiving an early WEAL complaint (see text for details on what determines if a complaint is early). Figure 10d presents difference-in-differences estimates from equation (20)

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: Draft-Induced Attrition and Changes in Women's First-Year Enrollment

(a) Attrition Surprises and Change in Women's 1L Enrollment Share

	(1) Core	(2) + Prestige	(3) Winsorized	(4) Unscaled	(5) No Trend	(6) Part Time
Attrition surprise	0.0021***	0.0019***	0.0025***	0.0745***	0.0025**	0.0019
	(0.001)	(0.001)	(0.001)	(0.018)	(0.001)	(0.001)
Observations	121	113	121	121	121	49
R^2	0.127	0.130	0.123	0.127	0.045	0.032

(b) Impact of Attrition Surprises Before Draft Shock

	(1) 1963	(2) 1964	(3) 1965	(4) 1966	(5) 1967
Attrition surprise	0.0002	0.0004	-0.0008	-0.0012	-0.0001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	116	115	118	120	120
R^2	0.000	0.001	0.007	0.012	0.000

Notes. Both panels estimate $\Delta w_{i,t} = \alpha + \beta\,\tilde{S}_{i,t} + X_i'\gamma + \varepsilon_{i,t}$ by OLS, where $\Delta w_{i,t} \equiv w_{i,t} - w_{i,t-1}$ is the one-year change in the share of women in the first-year (1L) class at program i, $\tilde{S}_{i,t} = S_{i,t}/\hat{\sigma}_i$ is the standardized attrition surprise defined in the text, and X_i collects additional controls included only in column (2). Observations are weighted by 1L enrollment throughout.
Panel A restricts the sample to full-time programs and sets $t = 1968$. Column (1) is the baseline specification with no additional controls. Column (2) adds log library volumes ($\ln \mathrm{Vol}_i$) to absorb variation in attrition shocks attributable to institutional prestige. Column (3) replaces $\tilde{S}_i$ with a version winsorized at the 1st and 99th percentiles to limit the influence of outlier shocks. Column (4) replaces the scaled surprise with the unscaled residual $S_{i,t}$, omitting the division by $\hat{\sigma}_i$; the coefficient is therefore in units of raw percentage-point attrition deviation rather than pre-period standard deviations. Column (5) estimates the attrition surprise from a specification with program fixed effects only, omitting the program-specific linear time trend; $\tilde{S}_i$ therefore captures each program's deviation from its historical *mean* attrition rather than its mean *trend*. Column (6) applies the baseline specification to part-time programs.
Panel B applies the baseline specification to full-time programs for each year $t \in \{1963, \ldots, 1967\}$, replacing $\tilde{S}_{i,1968}$ with the analogous attrition surprise $\tilde{S}_{i,t}$ computed from the six-year trailing window ending in year $t-1$.
Standard errors in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table 3: The draft lottery and male lawyers' careers, 1981 Biographical Section

	Ineligible mean	Draft-eligible, born 1944–50	Placebo: born 1953
Panel A: Service and timing			
Military service	0.098	$+0.024^{***}$ (0.003)	$+0.001$ (0.003)
Service extending into the 1970s	0.080	$+0.022^{***}$ (0.003)	$+0.002$ (0.003)
Age at law-school graduation	26.29	$+0.150^{***}$ (0.021)	$+0.065^{**}$ (0.028)
Graduated on time (age 22–26)	0.642	-0.041^{***} (0.005)	-0.011 (0.010)
Panel B: Position in 1981			
Partner (member of firm)	0.525	-0.014^{***} (0.005)	-0.006 (0.015)
Still an associate	0.268	$+0.016^{***}$ (0.005)	$+0.016$ (0.018)
Partner vs. associate	0.662	-0.019^{***} (0.005)	-0.012 (0.018)
at firms with 10+ listed lawyers	0.566	-0.026^{***} (0.008)	-0.015 (0.022)

Notes. Each cell reports the coefficient on draft eligibility (lottery number at or below the call ceiling) from a separate regression with birth-month fixed effects and, in the pooled 1944–50 cohort, birth-year fixed effects; heteroskedasticity-robust standard errors in parentheses. "Ineligible mean" is the outcome mean among men born 1944–50 with numbers above the 195 ceiling. The placebo column repeats the design for men born 1953, whose 1972 lottery assigned numbers but never issued calls (pseudo-ceiling 95). $N = 39{,}332$ men born 1944–50 and 4,194 born 1953. Outcomes are fractions except age at graduation (years). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: FT-only × post-1971 on employment outcomes: DiD by group and triple-difference.

Outcome	Mean (%)	All	Women	Men	Triple (FT×post×F)
Female (composition)	5.6	+0.049***	—	—	—
Government (any)	8.2	+0.032***	+0.006	+0.030***	−0.029
Federal	3.8	+0.020***	+0.035**	+0.016***	+0.024
State / local	3.8	+0.011**	−0.027	+0.013**	−0.046***
Private firm	49.4	−0.018	+0.120***	−0.016	+0.163***
Solo	36.2	−0.021	−0.108**	−0.020	−0.103**
Legal services (aid/PD)	1.6	+0.011***	+0.000	+0.010***	−0.017
In-house	2.3	−0.008**	+0.003	−0.009**	+0.011
Prosecutor	3.0	+0.003	−0.003	+0.004	−0.016
Public defender	0.5	+0.002	−0.012	+0.003	−0.018
Legal aid	1.1	+0.010***	+0.017*	+0.008***	+0.006
Judiciary	1.2	+0.001	−0.017***	+0.001	−0.018***
Academic	0.8	−0.002	−0.005	−0.002	−0.006
Patent	0.7	+0.003	−0.000	+0.003	+0.001
Of counsel	0.3	−0.003***	−0.007**	−0.002***	−0.004
Associate	15.3	+0.137***	+0.160***	+0.138***	+0.008
Named partner (card)	20.4	−0.100***	−0.026*	−0.099***	+0.082***
Partner, size-based	8.9	−0.061***	−0.011	−0.061***	+0.060***
Multi-office	1.1	+0.000	−0.003	+0.001	−0.002
Top rating ($a\,v$)	4.8	−0.051***	−0.019**	−0.051***	+0.038***
Any rating	29.2	−0.106***	−0.025	−0.106***	+0.071*

Notes. The All/Women/Men columns report $\hat{\alpha}_{\text{DiD}}$ from the DiD in Eq. (15); the last column reports $\hat{\tau}$ from the triple-difference Eq. (16) — the *women-minus-men* difference in the exposure effect, with male classmates as the within-school, within-cohort control. FT-only vs. PT-only schools, cohorts 1958–78, pooled 1974–81 editions (a lawyer can appear in up to four editions), school/cohort/edition fixed effects, SE clustered by school. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix

A Data Appendix: Digitizing the *Martindale–Hubbell* Directories

The lawyer-level data in Section 5 come from historical print editions of the *Martindale–Hubbell Law Directory*. Each edition has two components that we use. The *Geographical Bar Register* is a state-by-city roster that aims to list every lawyer admitted in the United States, one dense line per lawyer. The *Biographical Section* ("Professional Biographies") contains card entries that law firms purchase, listing their lawyers with full professional biographies. We scanned the print volumes and built two digitization pipelines matched to the two layouts: a pipeline based on the open-source PaddleOCR engine for the tightly formatted register lines, and a pipeline based on Mistral's document-OCR model for the free-form biographical cards. This appendix describes each in turn.

A.1 The Geographical Bar Register

Source material and OCR. We digitized the register from twenty-two edition years spanning 1965–2000 (1965, 1967, 1970–1972, 1974–1976, 1978, 1981, 1983, 1987, and annually 1991–2000). Scanned pages were split into single-column images (the register prints two columns per page); the 1981 edition alone comprises seven volumes of roughly 1,700–2,100 column images each. Each column image is processed with PaddleOCR (v3.7.0) using the pretrained PP-OCRv6 medium detection and recognition models, which return, for every text box, the recognized string, a confidence score, and a bounding box.[1]

Parsing register lines. A register entry packs a lawyer's record into one or two lines, for example

△ Alper, Harvey M. '46 '71 b v 3 g C&L.260 B.S., J.D. [A. & W.]

which reads: American Bar Association member (the triangle); name; birth year (1946); year of first bar admission (1971); the confidential peer-review rating (a letter legal-ability rating and general-recommendation rating, with an optional numeric grade); the numeric college and law-school code (here college and law school share code 260); degrees earned; and the firm affiliation in brackets, where a circled A before the firm name marks an associate rather than a member. Further symbols mark lawyers with multiple offices, retired or single-client status, and active Armed Forces service. Extraction proceeds page by page: text boxes are filtered and grouped into lines; lines are classified as state banners, city or county headers, lawyer entries, or continuations; geography is carried forward across pages; entries split across lines are re-glued; and the fields above are parsed with rule-based patterns into one row per listing, carrying flags for uncertain year tokens, out-of-range school codes, and OCR-suspect strings.

[1] We also evaluated a recognition model fine-tuned on directory pages; the stock pretrained model performed better on these scans (the fine-tune introduced character doubling and corrupted the special symbols described below), so all production runs use the pretrained models.

Enrichment and repair. A sequence of deterministic post-passes then (i) cleans name and firm strings and splits names into parts; (ii) imputes gender from Social Security Administration first-name distributions by birth decade, supplemented by honorifics; (iii) maps school codes to institution names through a crosswalk built from the directory's own school index; (iv) classifies employment into sectors and roles following the Curran et al. (1985)-style taxonomy (government and its level, prosecutor, public defender, judiciary, in-house counsel, academic, patent practice); and (v) repairs structural errors at the edition level: state assignments are re-anchored to the printed volume order, state boundaries are snapped to the divider pages the directory prints between states, and two rosters that would otherwise contaminate counts are flagged and excluded from analysis — the Canadian and international roster at the end of each edition and the U.S. Patent Office cross-index, which duplicates lawyers already listed in their home states. Lawyers listed at several offices are resolved to the largest office. Cleaned and localized firm affiliations yield an office-size measure: the count of listed lawyers per firm and city.

A.2 The Biographical Section

OCR and structured extraction. The Biographical Section's layout which is more of a free-form, multi-page firm cards with per-lawyer paragraphs is more challenging for line-based parsing, so we digitized it with Mistral's dedicated document-OCR model. Every page image is submitted with an extraction schema and a fixed set of extraction rules instructing the model to return, for that page only, structured firm objects (name, address, organizational form, telephone, representative clients) containing lawyer objects: name parts; birth place and full birth date; bar and court admissions with years; undergraduate, graduate, and law education with institutions, degrees, and years; position at the firm; memberships, honors, and languages; and military service. The schema also requires explicit flags marking cards that continue from or onto neighboring pages. Lawyers' positions are subsequently overwritten deterministically from the section headers printed on the card itself (MEMBERS OF FIRM, ASSOCIATES, and so on), so a lawyer's recorded rank never rests on model inference. For 1981, the edition the paper uses, the section comprises 24,207 page images across seven volumes, with page coverage above 99.9 percent in every volume.

Reassembly and repair. Post-processing reassembles multi-page cards by chaining the continuation flags, with safeguards against over-merging (same-city checks and tests against the running page headers); deduplicates firms within volume on name, city, state, and telephone; cleans and splits names; and corrects retroactively reissued law degrees: where a reported law-graduation year postdates the lawyer's earliest bar admission by more than a year — the signature of schools that reissued J.D. degrees to LL.B. alumni — the graduation year is corrected from the education text or the admission year.

Coverage and completeness. The result for 1981 is a file of 190,581 biographical records. Completeness is high for the fields the paper uses: 90 percent of records carry a birth year and 88 percent a full birth date — month, day, and year, the basis for the draft-lottery design of Section 5 — 91 percent a bar-admission year, 89 percent a law school, 86 percent a law-graduation year, and 83 percent an explicit position at the firm; 8.3 percent report military service. Gender is imputed from SSA name distributions exactly as in the register pipeline. Beyond 1981, the same pipeline has digitized fourteen edition years (1965–1999, 3.6 million biographical records), which we use for corroborating cross-sections.

B 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.[2] 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.[3] 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 5 demonstrates this graphically. Appendix Figure 5a 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.[4] Appendix Figure 5b 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

[2]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).

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

[4]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 5c. 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 5d. This immediately implies an increase in f^* and f^{opt}, the first substantive prediction of our model.[5]

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 5e, and consequently the marginal benefit curve in Appendix Figure 5f 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.

[5]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).

C 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.[6] These data are reported at the "monthly" level,[7] 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.[8] 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.[9] 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 25a and for men in Appendix Figure 26a. 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.[10] It's clear from the data that

[6] 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.

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

[8] 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.

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

[10] 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:

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

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.[11] Our results for women are plotted in Appendix Figure 25b and for men in Appendix Figure 26b, 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

[11] 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.[12] Specifically, we use equation (C.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

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

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).[13] These are defined as followed:

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

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

Here, a_n and b_n are finite sample corrections to ensure these estimators are unbiased,[14] and $k =$

[12] 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 35 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.

[13] 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.

[14] 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 $\tilde{\varepsilon}_{g,t}^{\text{Studentized}}$, $\tilde{\varepsilon}_{g,t}^{\text{MAD}}$, and $\tilde{\varepsilon}_{g,t}^{\text{Q}}$ for every LSAT administration against a threshold of $z_{0.025} = 1.96$ for the number of women (Appendix Figure 27b) and men (Appendix Figure 28b) taking the LSAT, the fraction of individuals taking the LSAT who are women (Appendix Figure 29b), the number of women (Appendix Figure 30b) and men (Appendix Figure 31b) registered to take the LSAT, the fraction of individuals registered to take the LSAT who are women (Appendix Figure 32b), and the fraction of women (Appendix Figure 33b) and men (Appendix Figure 34b) 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 32) and take (Appendix Figure 29) 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 27) and men (Appendix Figure 28) taking the LSAT in November of 1969 such that fraction of test takers who are women (Appendix Figure 29) 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).

D Robustness to Contraceptive Access: Data and Results

This section details the data construction behind the contraception-access robustness tests reported in the main text and presents the full estimates.

Legal access codings. Between 1960 and 1976 the states extended to unmarried women under 21 the ability to obtain oral contraception without parental consent, through changes in the age of majority, mature-minor doctrines, comprehensive family-planning statutes, and court decisions. We use the two codings of this legal history in the literature: the dates in Table 2 of Goldin and Katz (2002), in both their strict ("nonrestrictive") and broad (earliest legal age below 21) forms, and the independently collected dates in Table I of Bailey (2006). The results under both codings do not depend on any single reading of the legal record. Each law school is assigned the state of its main campus from its MH directory name, and a school's state is "early" under a given coding if its access year is 1971 or earlier.

Exposure measures. For the national test we construct, for each lawyer, the probability that her birth cohort had legal access before age 21: P_c equals the 1970-population-weighted share of states whose access year falls at or before the year cohort c turned 20, set to zero for cohorts that turned 21 before the pill's FDA approval in 1960.

Specifications and results. Panel A of Appendix Table 6 augments the baseline linear probability model (an indicator for female regressed on full-time-only $\times$ post-1971 with law-school, bar-cohort, and directory fixed effects, standard errors clustered by school) with the full-time-only $\times P$ interaction. Panel B interacts the full-time $\times$ post term with the early-access regime of the school's state, allowing regime-specific cohort profiles; the early main effect is absorbed by the school fixed effects. Two features of the design are worth noting. First, under the Goldin–Katz coding the national access curve has essentially no support before bar cohort 1973, so the sharper national test is the Bailey-coded one, in which 43 percent of pre-break observations carry positive access; the pill interaction there is negative. Second, the timing is informative on its own: the full-time/part-time feminization gap opens over bar cohorts 1971–1975, whereas the access curve for these birth cohorts rises only after 1976. These tests exclude the pill-access profile specifically; they do not adjudicate between a discrete 1971 break and a smooth post-1968 ramp in the differential itself, which in any case grows before pill access rises.

D.1 Replicating Goldin and Katz's (2002) Table 5

The career evidence in Goldin and Katz (2002) is an age-by-census-year design: the professional share of college-graduate women, ages 30–49, across the 1970–1990 censuses, regressed on cohort-level pill access with full age and year profiles. The directories allow the same design in a complete census of one profession. We build 218 age $\times$ edition cells (single ages 30–49; twelve editions, 1965–2000; birth cohorts 1921–1960) and estimate the same specification for a ladder of outcomes:

the female share of lawyers; log counts by sex; counts per person of the relevant sex, using July 1 national population estimates by single year of age and sex on a consistent resident-plus-armed-forces-overseas concept (Census Bureau intercensal series, 1935–2000); and counts per bachelor's degree conferred to the cohort's sex in year $c + 22$ (NCES annual series) — the last being the closest analogue of Goldin and Katz's outcome. In estimates not shown, a Vietnam draft-risk control (Selective Service inductions at ages 19–24 per male birth) addresses the possibility that draft-era dynamics, rather than the pill, shape the male comparison: the female coefficients remain positive and significant, while the male log-count decline is absorbed by draft exposure. Appendix Table 7 reports the grid: the per-college-woman estimate implies a 68 percent increase in women's lawyer production per college woman as cohort access goes from zero to universal, in line with the roughly twofold increase implied by Goldin and Katz's estimate — and, as the notes detail, identified from the same cohort-level contrast, with the same limitations, as theirs.

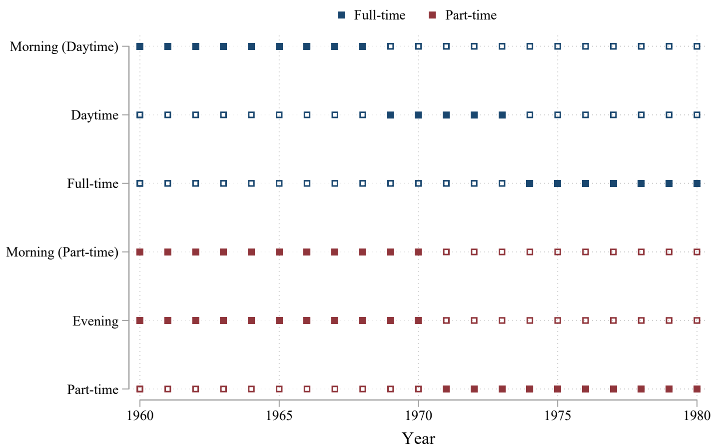
Appendix Figure 1: Raw *Review of Legal Education* Scans

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

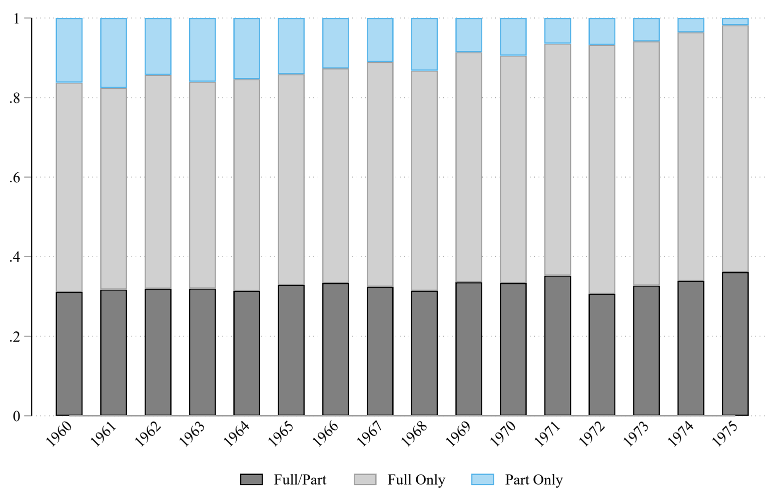
Appendix Figure 1 provides an example of the scans used to construct the RLE data.

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 part-time program (Full/Part), only a full-time program (Full Only) and only a part-time program (Part Only).

Appendix Figure 3: Raw *Martindale-Hubbell* Geographical Bar Register Scan Example

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.
Madison Street Office Bldg., P.O. Box 492, 35010
Telephone: 234-4226; Area Code 205.
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.
201 Court House, 35010
Telephone: 234-2547; Area Code 205.
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.]

Wilbanks, Wilbanks and Dillon, a v
300 Wilbanks Bldg., P.O. Box 698, 35010
Telephone: 234-3446; Area Code 205.
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 Geography scans used to construct the Martindale-Hubbell-based data.

Appendix Figure 4: Raw *Martindale-Hubbell* Biographical Scan Example

62B ALABAMA

HARE, WYNN, NEWELL AND NEWTON (Continued)

lon Delta. Member: Board of Directors, Campbell Moot Court, 1969-1970; Bench and Bar. Editor, Alabama Law Reporter, 1969-1970. *Member:* Birmingham and American Bar Associations; Alabama State Bar; Alabama Trial Lawyers Association (Member, Board of Governors, 1979—); The Association of Trial Lawyers of America; American Judicature Society.

David Leon Ashford, born Athens, Alabama, May 12, 1948; admitted to bar, 1973, Alabama, U.S. Supreme Court and U.S. District Courts, Northern and Middle Districts of Alabama. Preparatory education, University of Alabama (B.S., 1970); legal education, University of Alabama (J.D., 1973). *Fraternity:* Phi Delta Phi. Member, Bench and Bar. Assistant Attorney General, 1973-1974. *Member:* Birmingham Bar Association (Member, Executive Committee, 1976-1979, President, 1980, Young Lawyers Section); Alabama State Bar (Member, Executive Committee, Young Lawyers Section, 1979); Alabama Trial Lawyers Association; The Association of Trial Lawyers of America.

John W. Haley, born Birmingham, Alabama, November 16, 1949; admitted to bar, 1975, Alabama and U.S. Court of Appeals, 5th Circuit; 1976, U.S. District Court, Southern District of Alabama. Preparatory education, Tulane University of Louisiana (B.A., 1971); legal education, University of Alabama and Cumberland School of Law of Samford University (J.D., magna cum laude, 1975). *Fraternity:* Phi Delta Phi. Editor, Alabama Trial Lawyers Journal, 1976—. *Member:* Birmingham and American Bar Associations; Alabama State Bar; Alabama Trial Lawyers Association.

Stephen D. Heninger, born Flora, Illinois, June 23, 1950; admitted to bar, 1977, Alabama. Preparatory education, University of Illinois (B.A., 1972); legal education, Cumberland School of Law of Samford University (J.D., summa cum laude, 1977). *Fraternity:* Phi Delta Phi. Editor in Chief, Alabama Bar Foundation Real Property Newsletter, 1976. Member, Moot Court Board. Law Clerk to U.S. District Judge James H. Hancock, Northern District of Alabama, 1977-1978. *Member:* Birmingham and American Bar Associations; Alabama State Bar; The Association of Trial Lawyers of America; American Judicature Society; National Order of Barristers. [1st. Lt., U.S. Army, Adjutant General, 1972-1974]

REFERENCE: First National Bank of Birmingham.

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328-1112

MEMBERS OF FIRM

Jack H. Harrison, born Birmingham, Alabama, August 22, 1933; admitted to bar, 1957, Alabama; 1975, Florida. Preparatory education, Birmingham-Southern College and University of Alabama (B.S., 1956); legal education, University of Alabama (LL.B., 1957). *Fraternity:* Beta Gamma Sigma. City Attorney: Homewood, 1968-1972; Hoover, 1968—; Trussville, 1973-1974. Member, Panel of Arbitrators, American Arbitration Association. *Member:* Birmingham and American Bar Associations; Alabama State Bar; The Florida Bar.

William A. Jackson, born Tremont, Mississippi, October 21, 1934; admitted to bar, 1961, Alabama. Preparatory education, Jacksonville State University (B.A., with honors, 1956); legal education, University of Alabama (J.D., 1961). *Fraternity:* Sigma Delta Kappa. Member, Scabbard & Blade. Member, Bench & Bar. Legal Advisor to Governor George C. Wallace, 1972-1979. Chairman, State of Alabama Industrial Revenue Bond Advisory Council, 1979—. *Member:* Alabama State Bar; American Bar Association. [2nd Lt., U.S. Army, Infantry, 1956—; Col., 20th Special Forces Group, Alabama National Guard]

Roger W. Lee, born Anniston, Alabama, October 22, 1949; admitted to bar, 1976, Florida; 1977, Alabama. Preparatory education, University of Alabama (B.S., 1972); legal education, University of Alabama (J.D., 1976). *Fraternities:* Omicron Delta Kappa; Phi Eta Sigma. Member, Jasons. Editor: University of Alabama Law and Psychology Review, 1975-1976. President, Student Government Association, University of Alabama, 1973. *Member:* Birmingham and American (Member: Insurance, Negligence and Compensation Law Section; Litigation Section) Bar Associations; Alabama State Bar (Member, Young Lawyers Section; Member, Committee on Prepaid Legal Services, 1980—); Alabama Trial Lawyers Association.

REPRESENTATIVE CLIENTS: Employers Life Insurance Company of Alabama; University of Montevallo; City of Hoover; Madden Coal Co.; Viva Chumbler Realty Co.; Alabama Title Co., Inc.; Dunhill of Birmingham, Inc.; Parker Supply Co.; City of Trussville Industrial Development Board.

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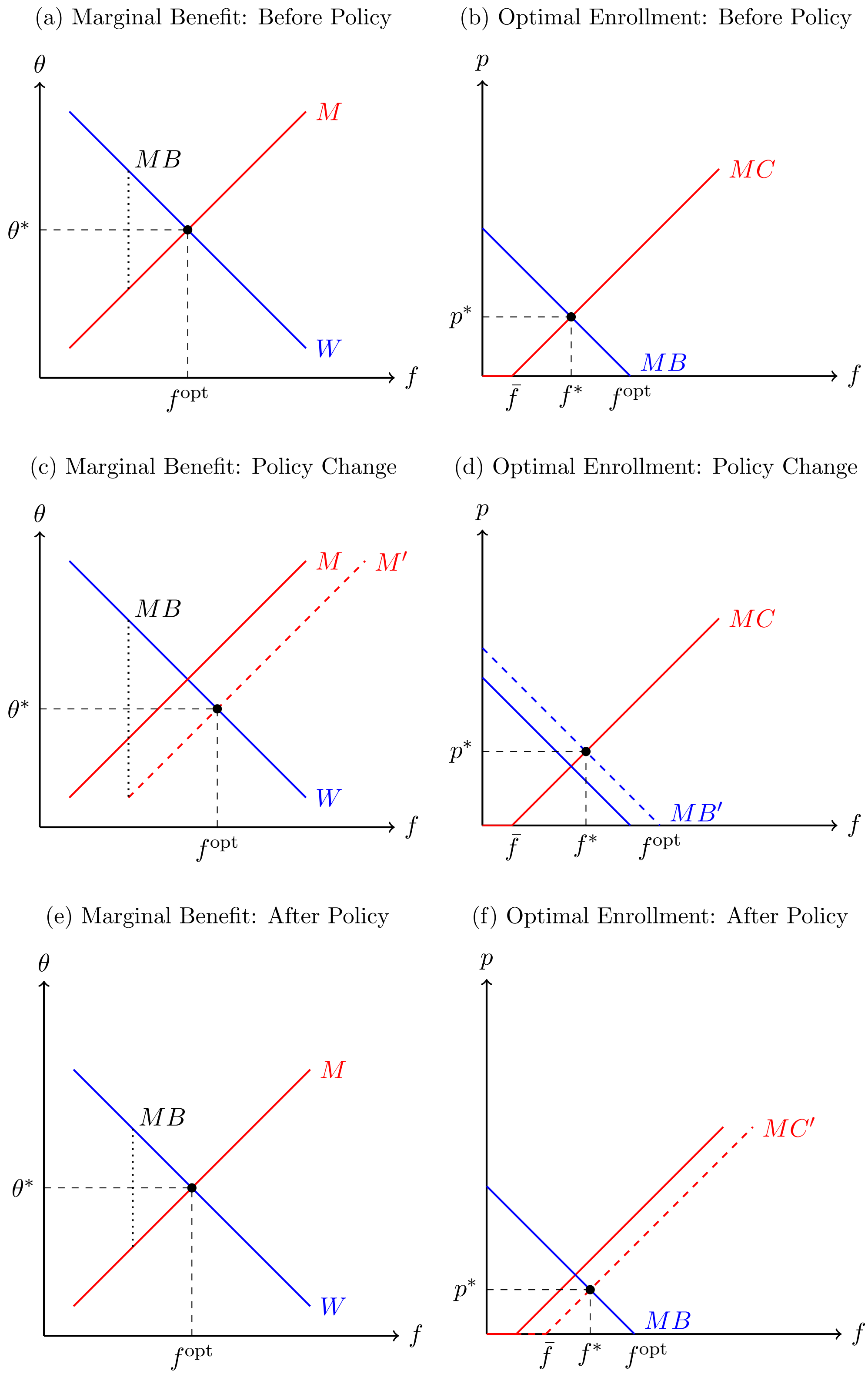
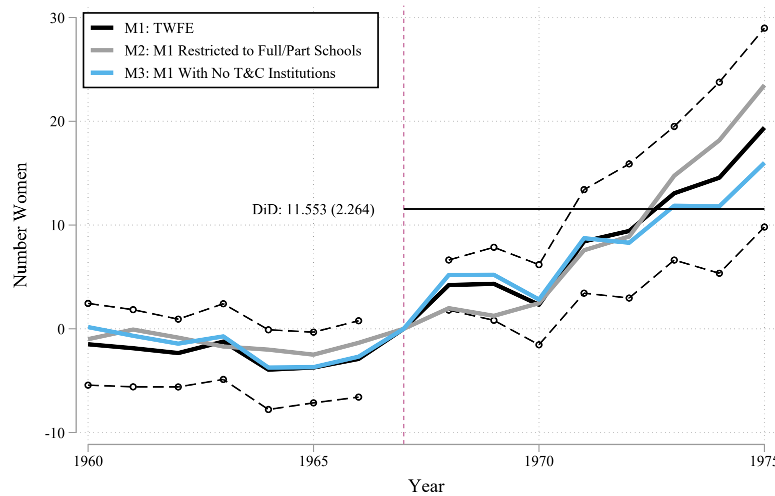
Appendix Figure 4 provides an example of the Biography scans used to construct the Martindale-Hubbell-based data.

Appendix Figure 5: Model of Law School Admissions



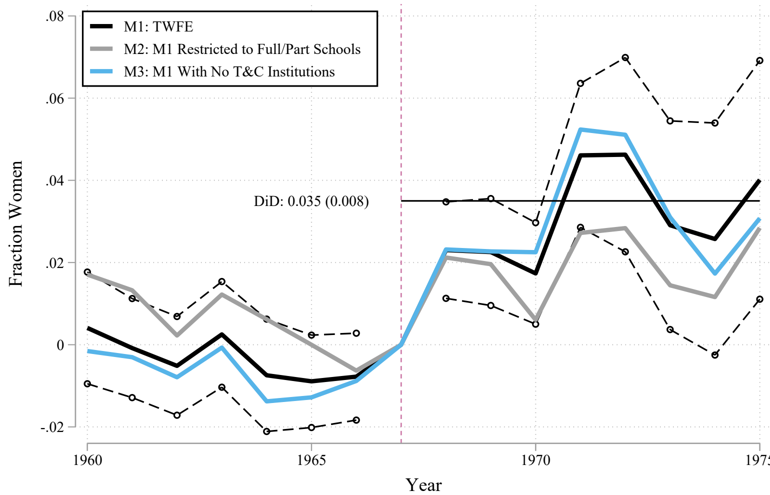
Appendix Figure 5 provides a graphical illustration of our model of law school admissions. See Appendix Figure 5 for details.

Appendix Figure 6: Robustness to Entropy Weighting

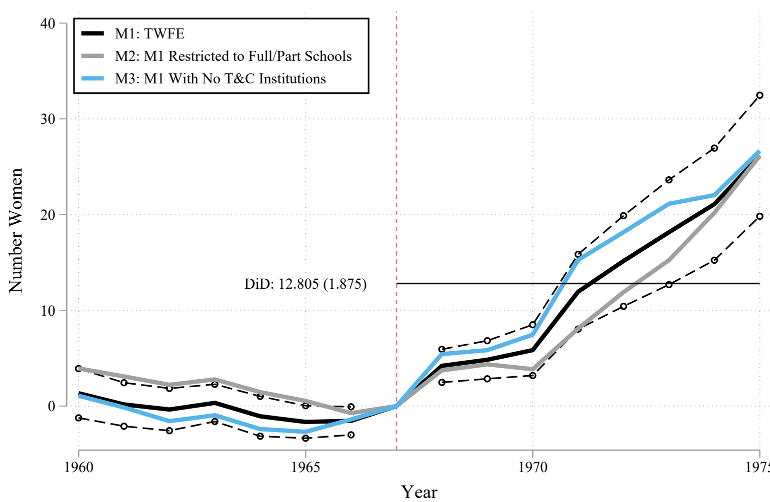


Appendix Figure 6 plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women and the enrollment control is omitted. Instead, we estimate entropy weights in 1967 such that weighted average total enrollment is equal in the treatment and control groups (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 7: Robustness to State-by-Year Fixed Effects



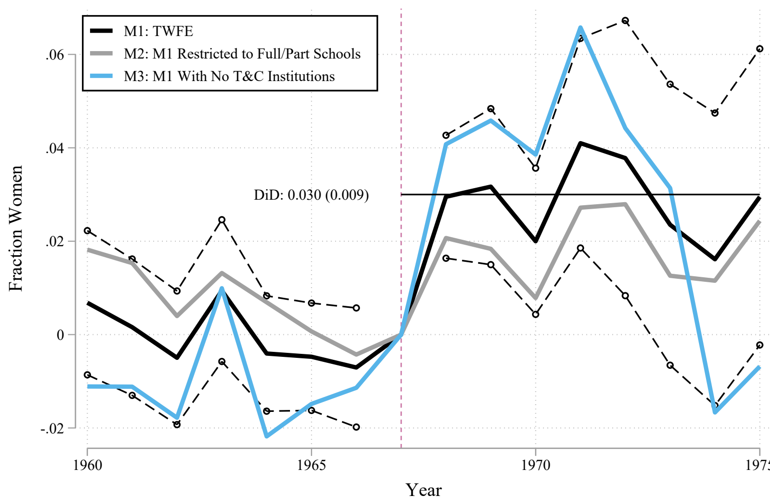
(a) Fraction Women



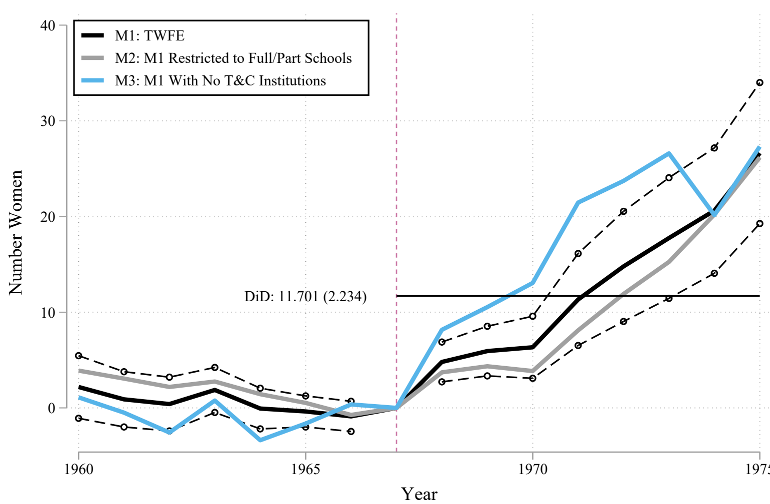
(b) Number Women

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 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 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 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 8: Robustness to City-by-Year Fixed Effects



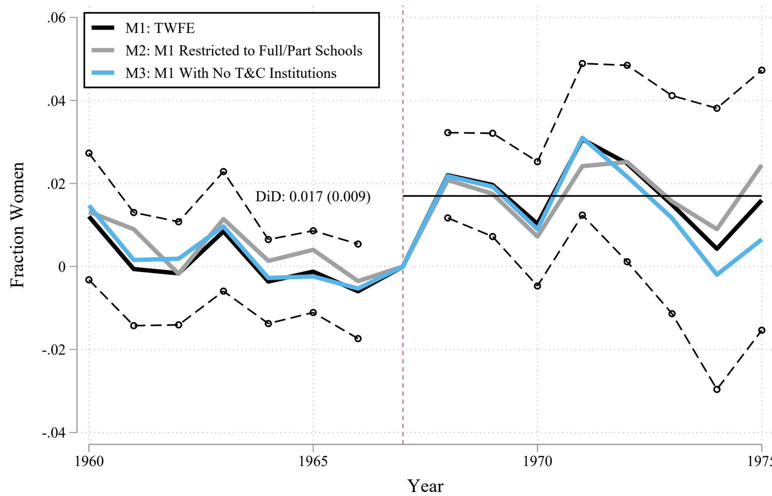
(a) Fraction Women



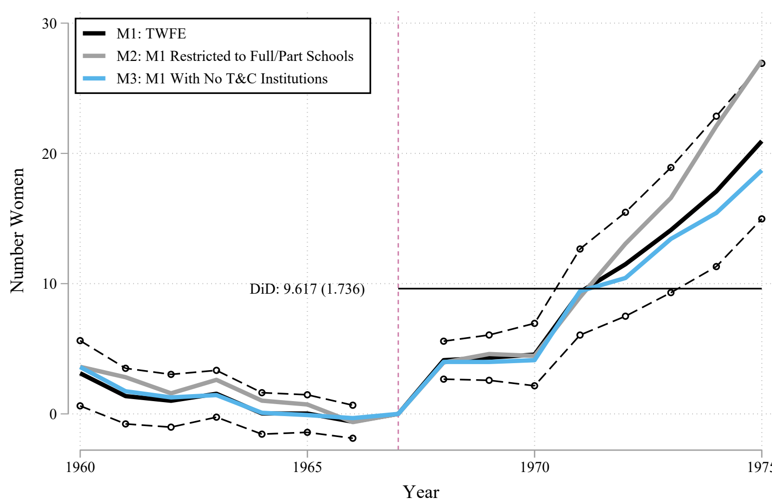
(b) Number Women

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. 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 8b 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 9: Restricting to ABA-Approved Schools



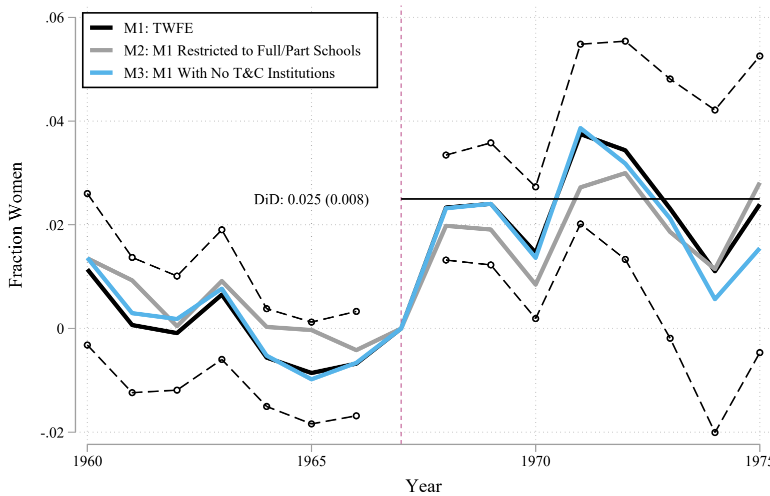
(a) Fraction Women



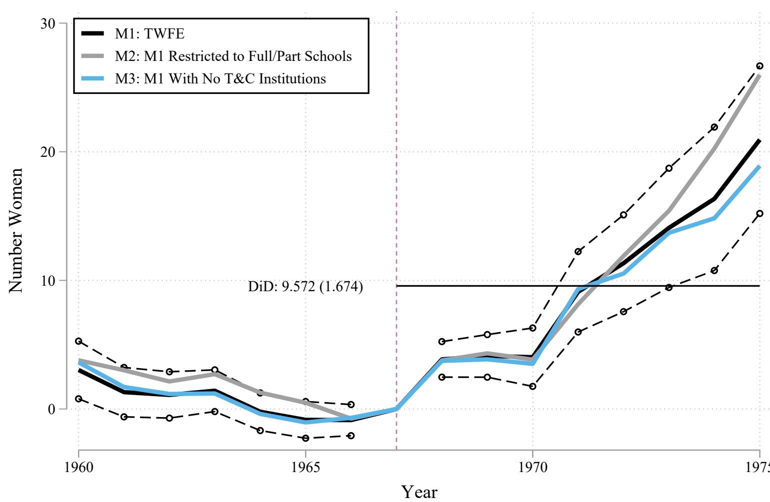
(b) Number Women

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 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 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: Restricting to Schools ABA-Approved by 1975



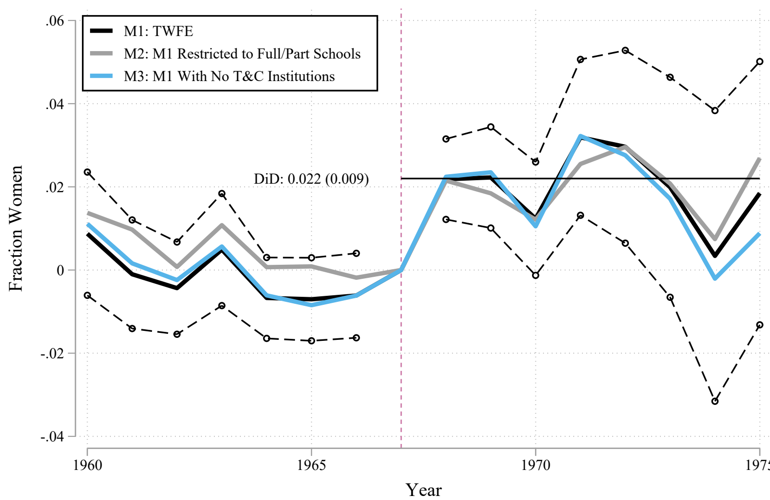
(a) Fraction Women



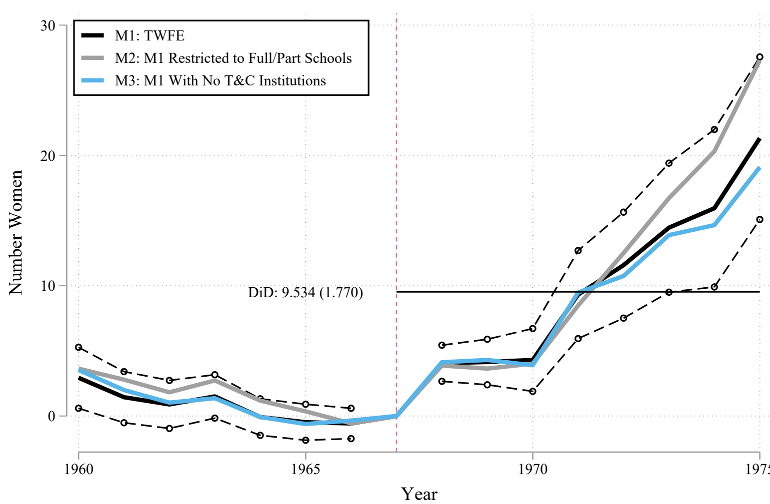
(b) Number Women

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 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 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-Balanced Panel



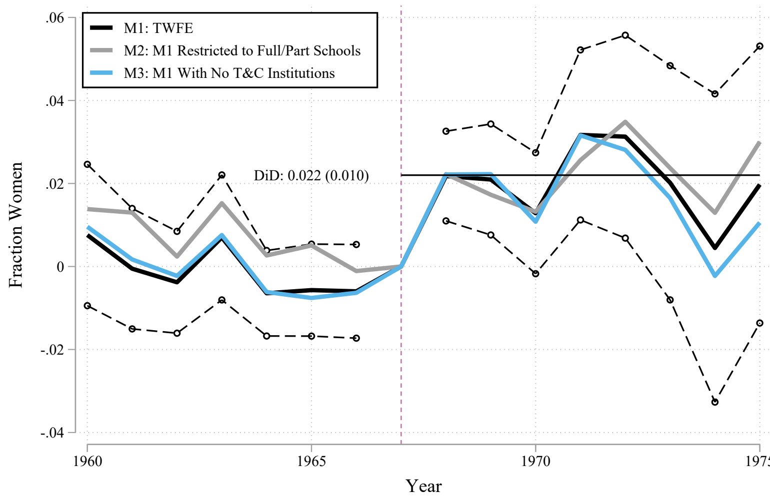
(a) Fraction Women



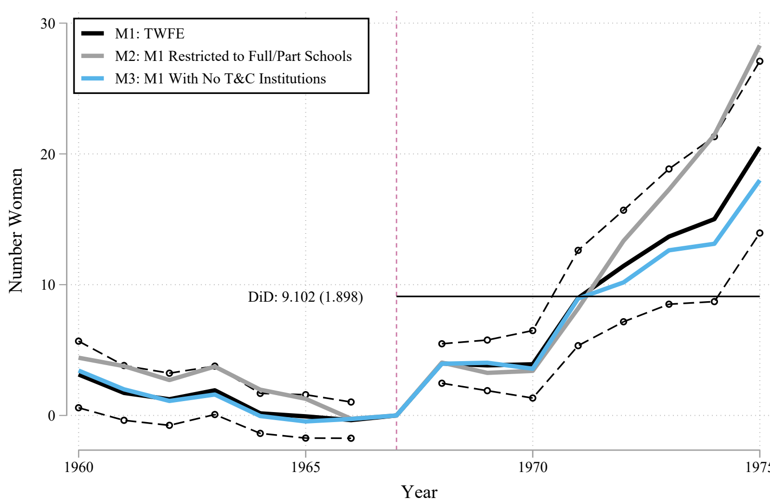
(b) Number Women

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 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 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: Institution-Program-Balanced Panel



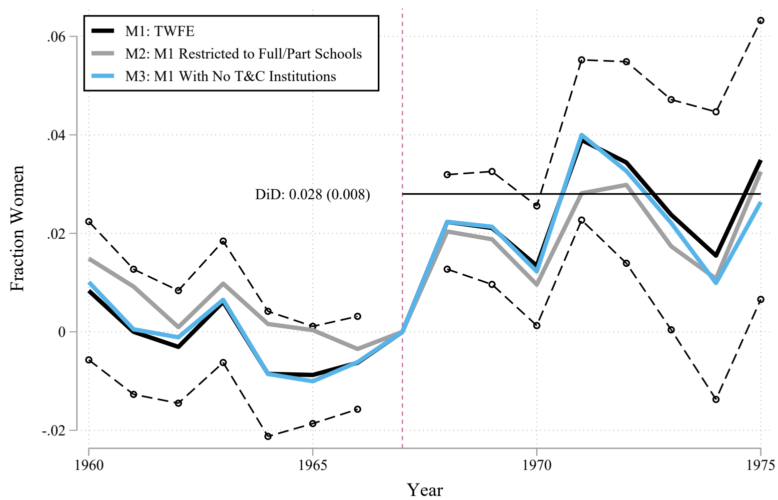
(a) Fraction Women



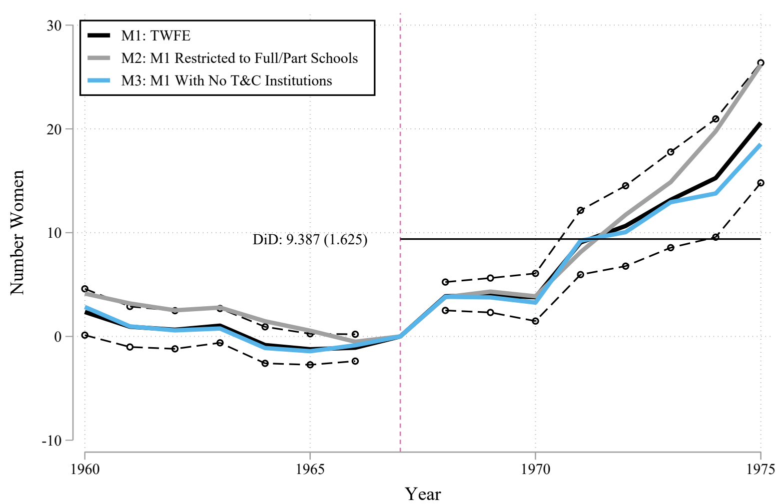
(b) Number Women

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 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 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 20 Students



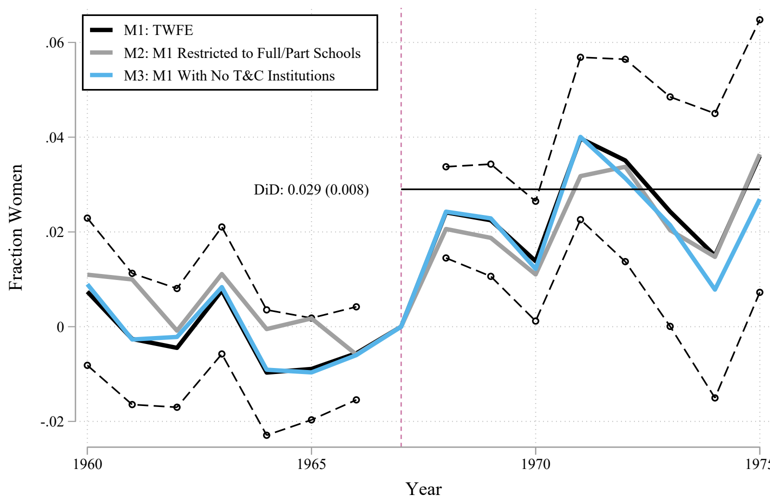
(a) Fraction Women



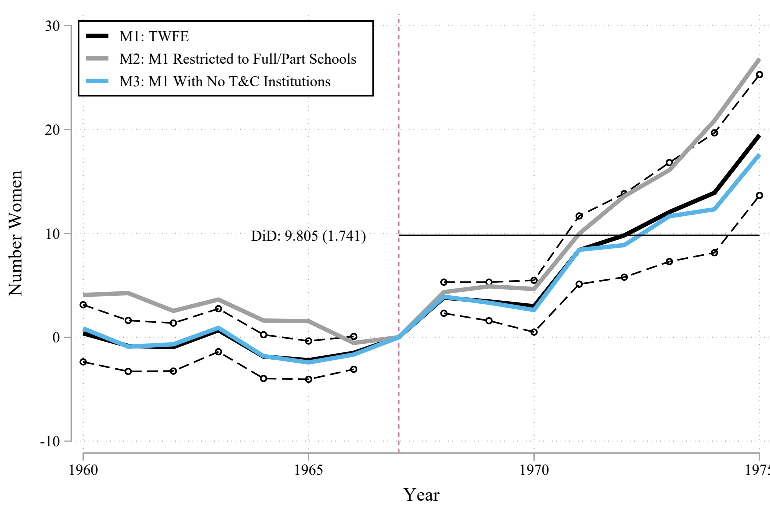
(b) Number Women

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



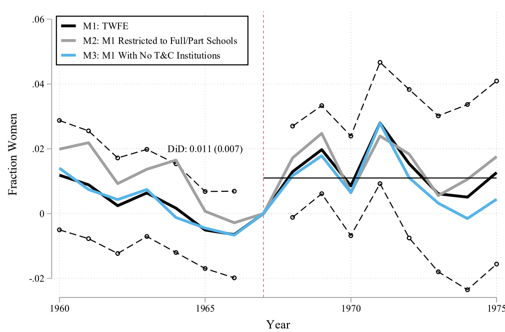
(a) Fraction Women



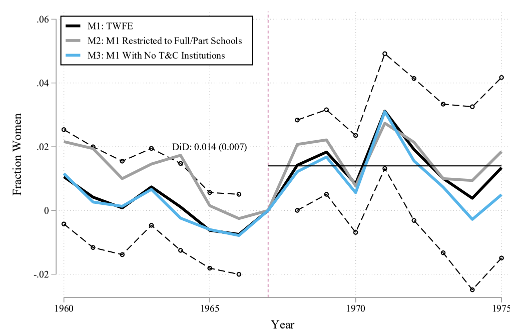
(b) Number Women

Appendix Figure 14a 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 14b 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.

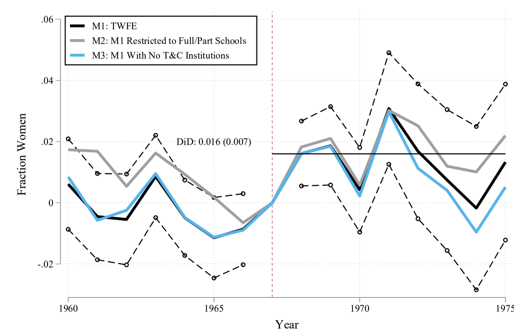
Appendix Figure 15: No Weighting



(a) Fraction Women (L1 10+)



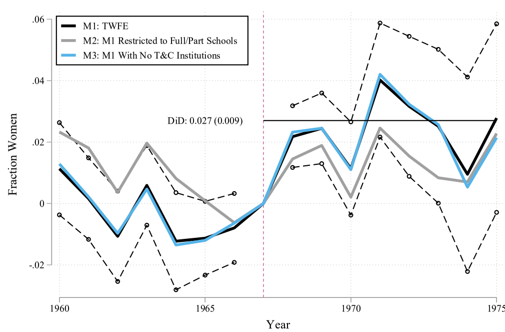
(b) Fraction Women (L1 20+)



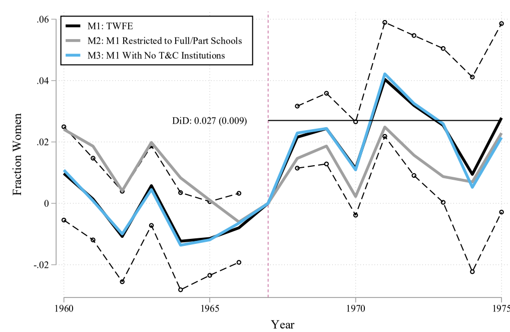
(c) Fraction Women (L1 50+)

Appendix Figure 15a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women. 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.

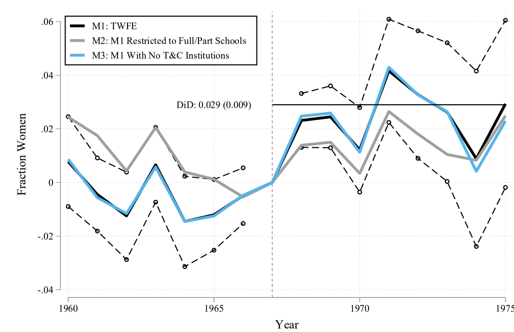
Appendix Figure 16: 1967 Weights



(a) Fraction Women (L1 10+)



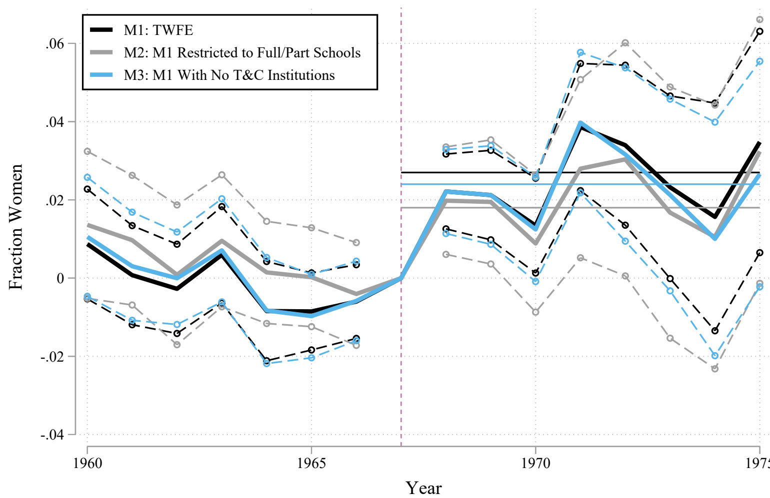
(b) Fraction Women (L1 20+)



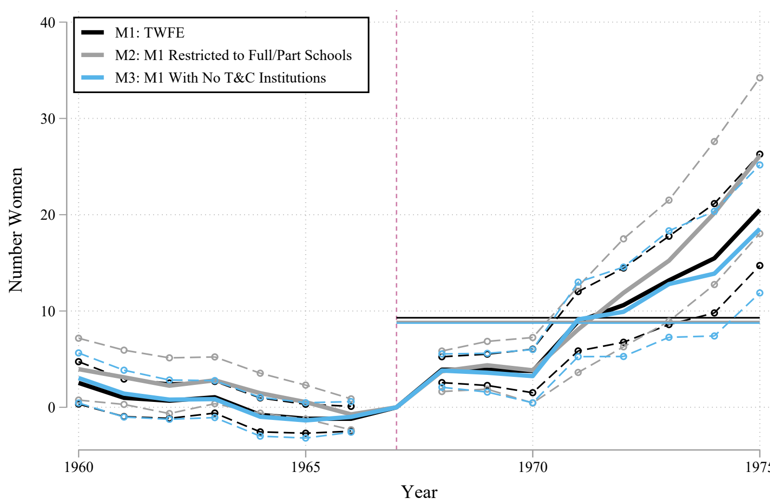
(c) Fraction Women (L1 50+)

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 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 16b replicates this figure estimated on a sample is restricted to institution-programs-years with at least 20 students enrolled. Appendix Figure 16c replicates this figure estimated on a sample is restricted to institution-programs-years with at least 50 students enrolled.

Appendix Figure 17: Clustering at the Institution-Program Level



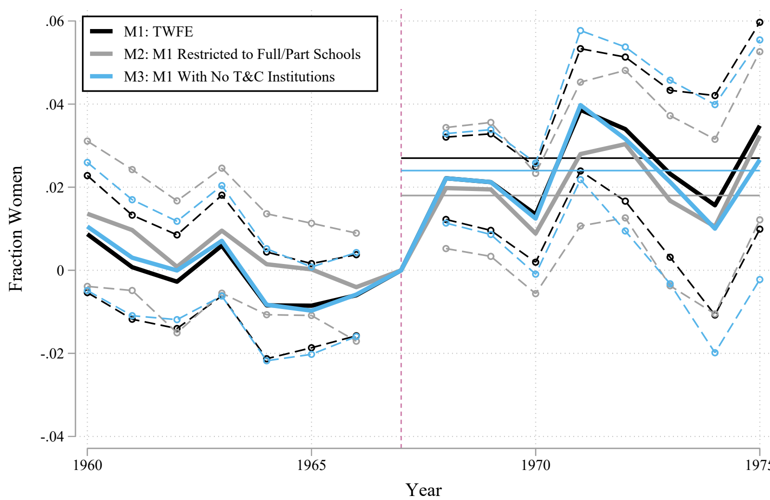
(a) Fraction Women



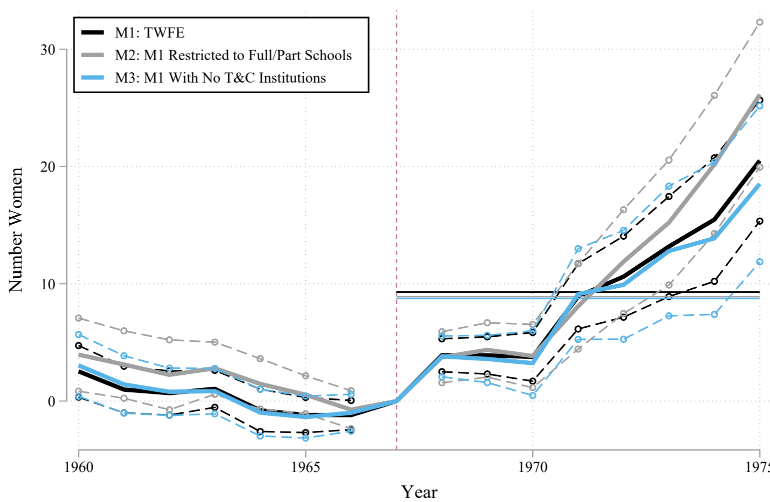
(b) Number Women

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-program 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 Institution Level



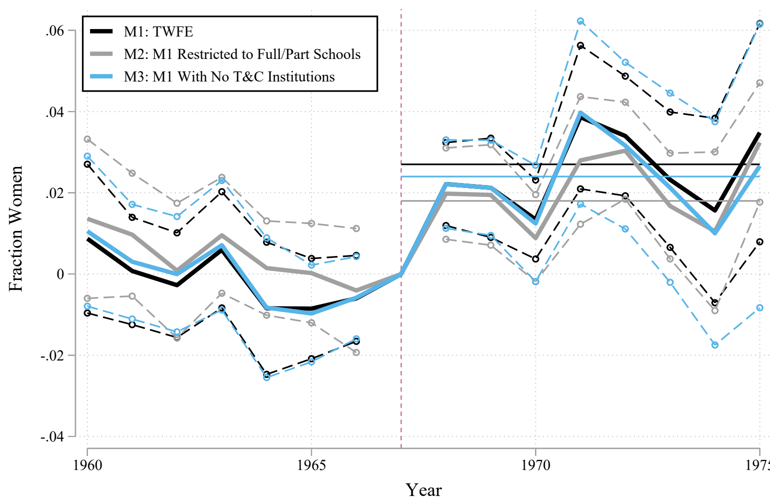
(a) Fraction Women



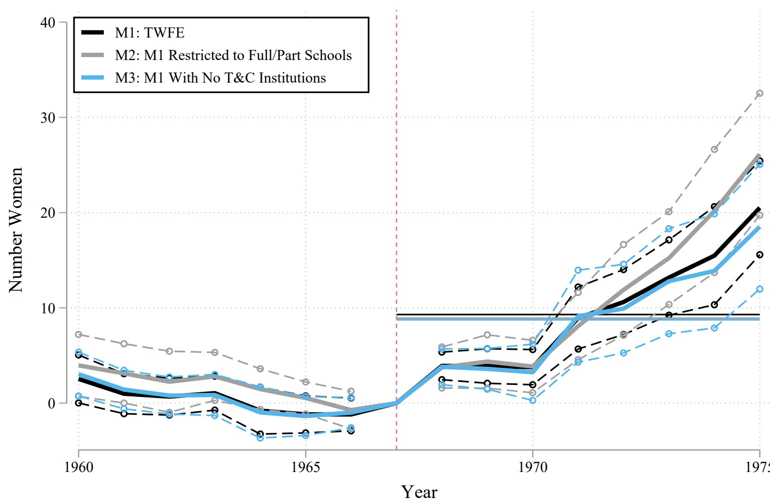
(b) Number Women

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 institution 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: Clustering at the State Level



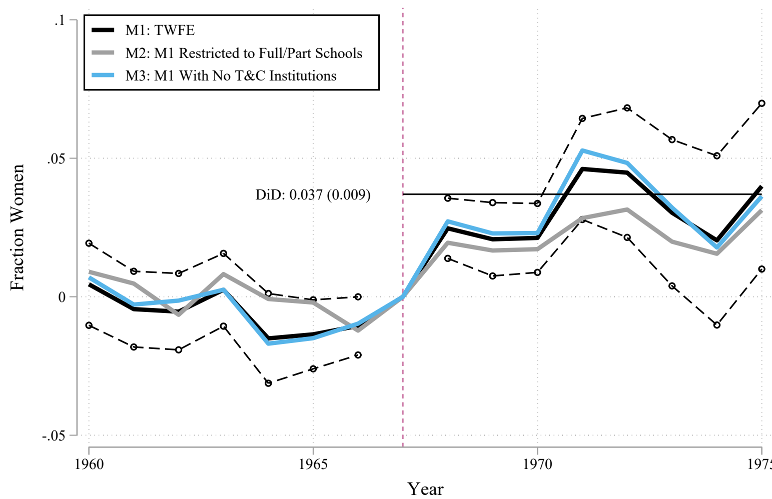
(a) Fraction Women



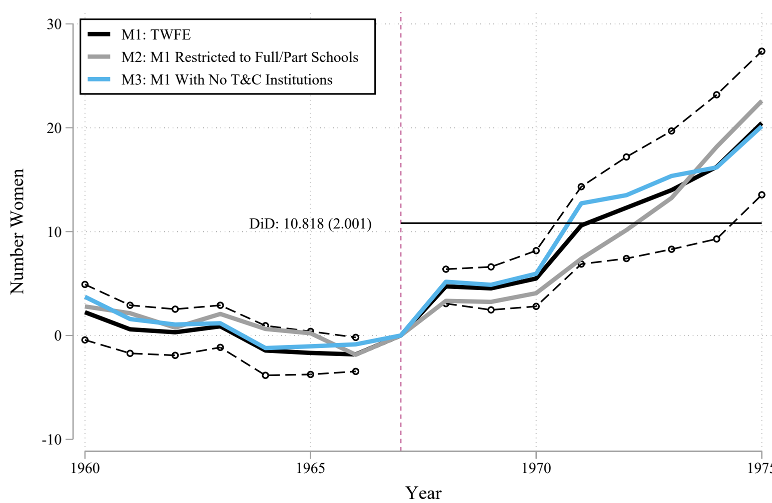
(b) Number Women

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. Appendix Figure 19b 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 19a) or equation (4) (Appendix Figure 19b).

Appendix Figure 20: Excluding the South



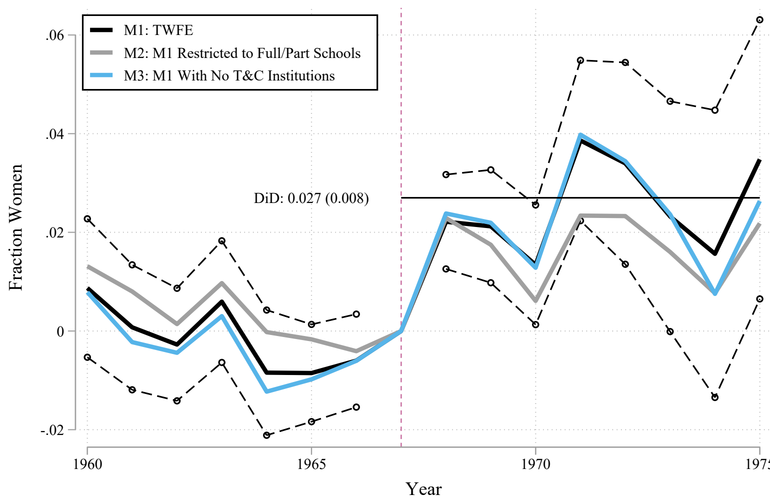
(a) Fraction Women



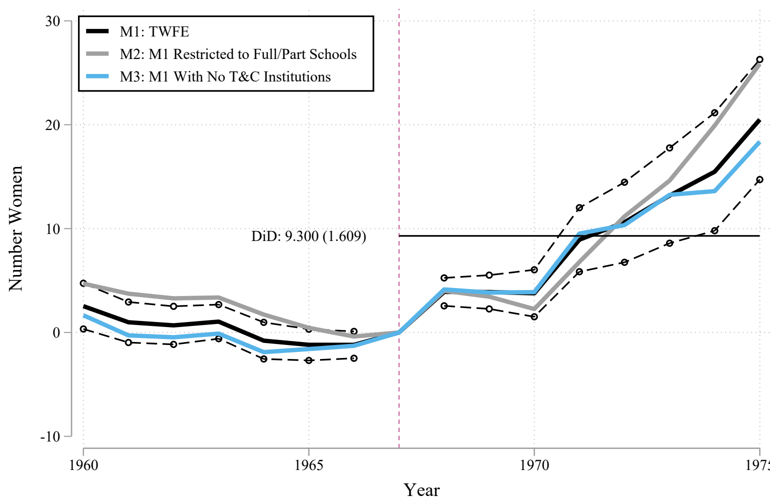
(b) Number Women

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. 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 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.

Appendix Figure 21: Fixing Program Mix at 1967 Value



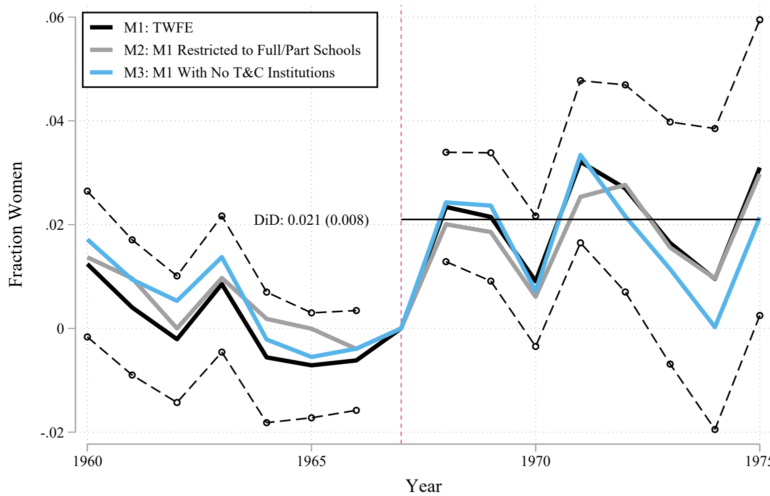
(a) Fraction Women



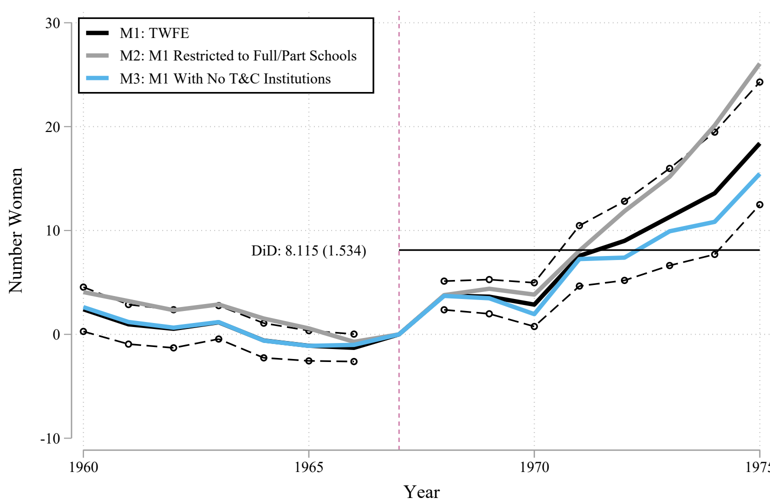
(b) Number Women

Appendix Figure 21a 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 21b 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 22: Adjusting for WEAL Complaints



(a) Fraction Women

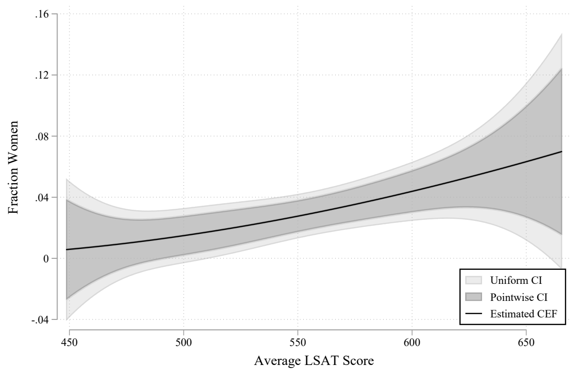


(b) Number Women

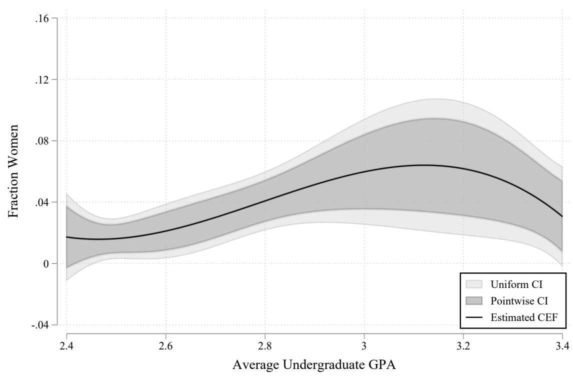
Appendix Figure 22a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 22b 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. Across all models, an indicator for institutions receiving an early WEAL complaint has been interacted with year fixed effects. The institutions coded as receiving an early complaint are listed in Appendix Table 2.

Appendix Figure 23: Alternative Continuous Differences-in-Differences Results

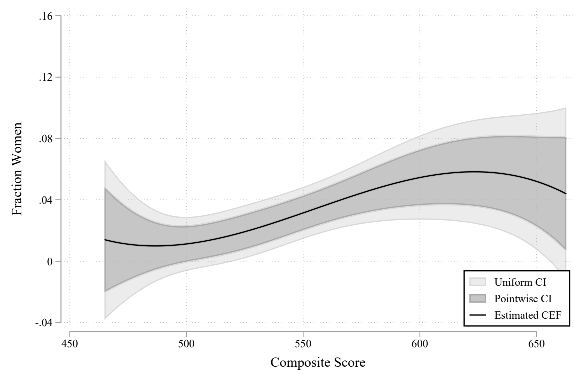
(a) Average LSAT Score



(b) Average Undergraduate GPA



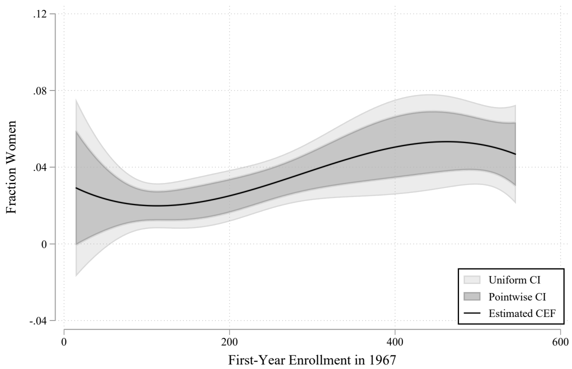
(c) Composite Score



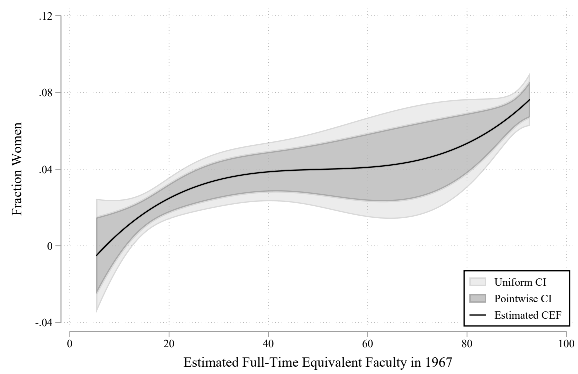
This figure plots estimates from equation (10), where the outcome is the change in the fraction of first-year students who are women between 1967 and 1968 and the regression is weighted by total first-year enrollment in 1967. The dose variable is imputed average LSAT score for the 1967-68 incoming class in Appendix Figure 23a, imputed average undergraduate GPA for the 1967-68 incoming class in Appendix Figure 23b, and the composite score for the 1967-68 incoming class in Appendix Figure 23c, 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 24: Heterogeneity in Law School Size

(a) First-Year Enrollment



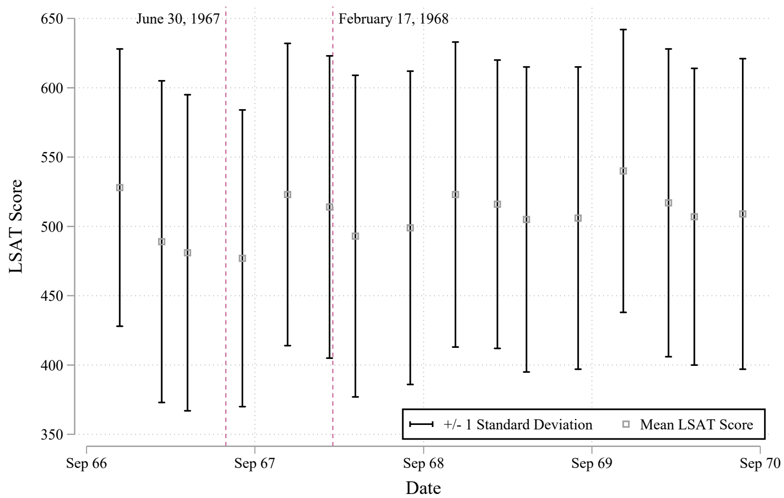
(c) Estimated Full-Time Equivalent Faculty



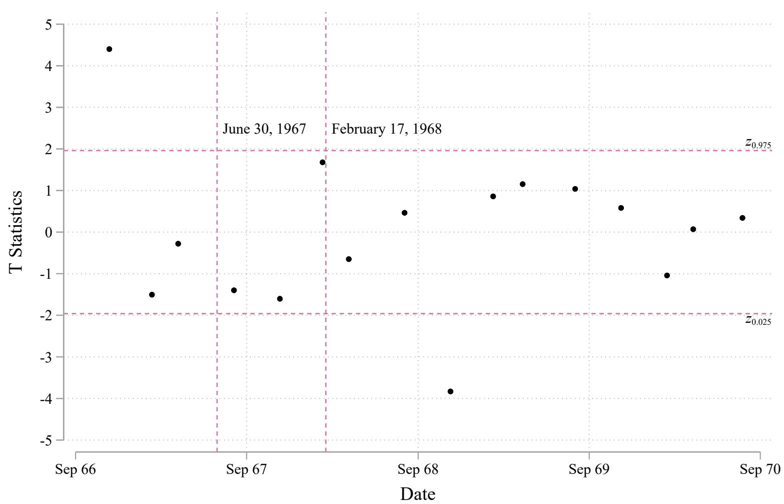
(b) Full-Time Faculty

This figure plots estimates from equation (10), 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 total L1 enrollment in 1967 in Appendix Figure 24a, the number of full-time faculty members in 1967 in Appendix Figure 24b, and the estimated number of full-time equivalent faculty members in 1967 in Appendix Figure 23c, where the estimate is computed as $\text{Full-Time Faculty} + (1/3) * \text{Part-Time Faculty}$. 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 25: Women's LSAT Scores



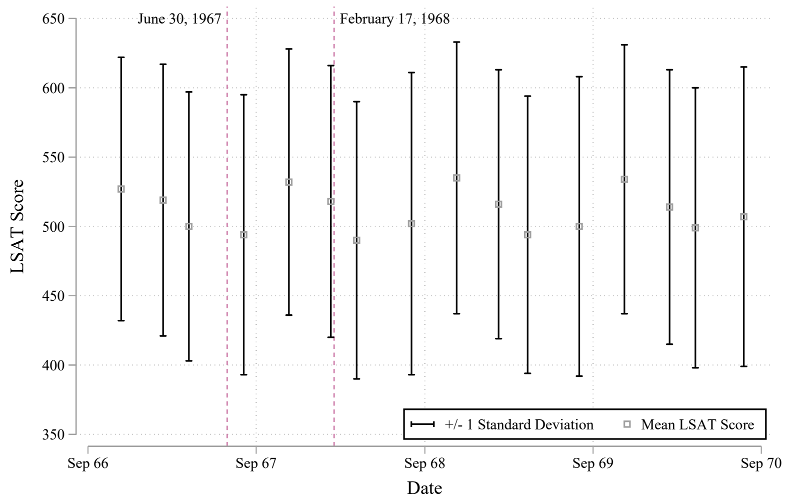
(a) Raw Data



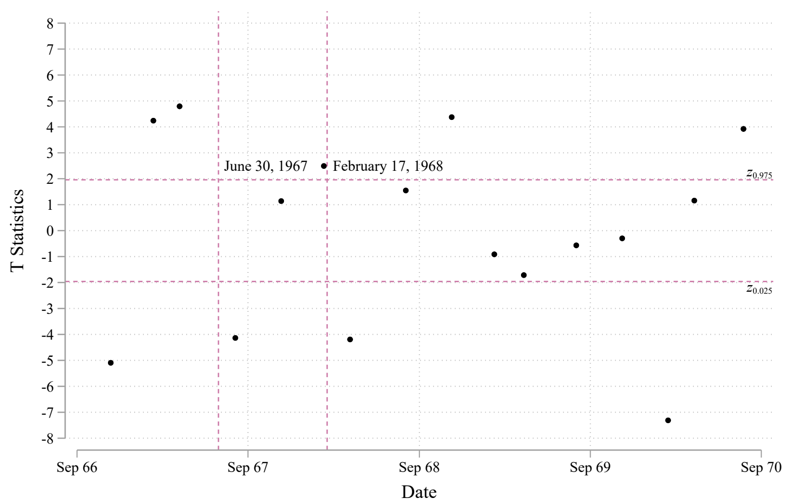
(b) Statistical Tests

Appendix Figure 25a 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 25b 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 26: Men's LSAT Scores



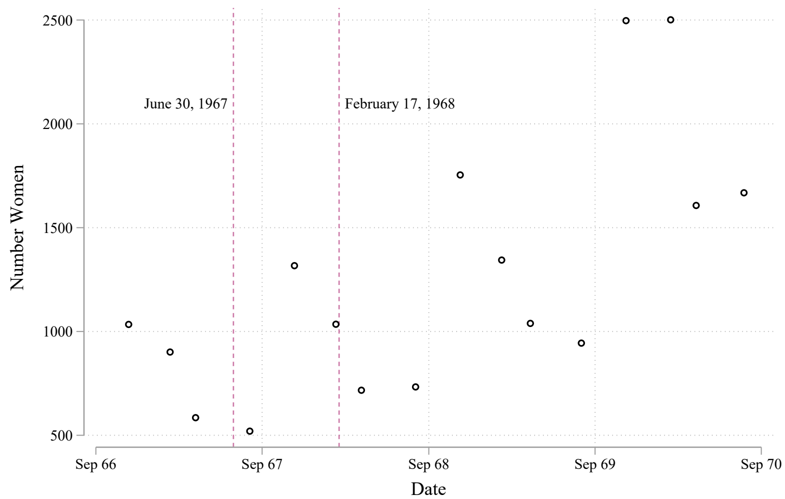
(a) Raw Data



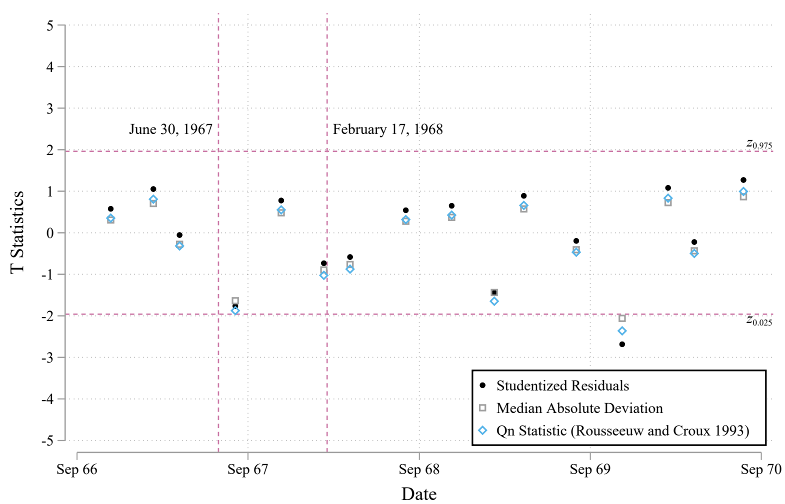
(b) Statistical Tests

Appendix Figure 26a 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 26b 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 27: Number of Women Taking the LSAT



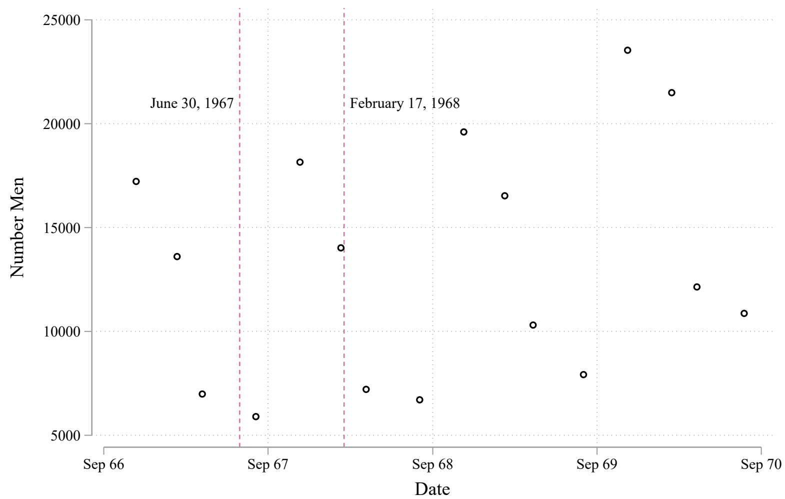
(a) Raw Data



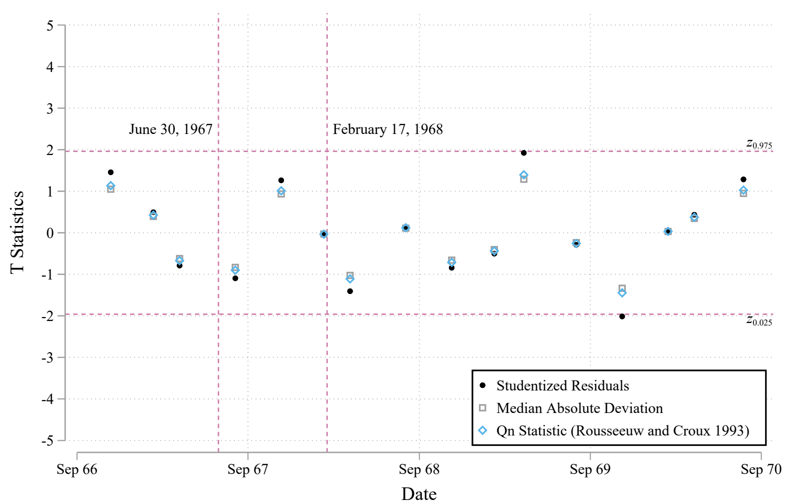
(b) Statistical Tests

Appendix Figure 27a plots the raw number of women who sat 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 (C.2), (C.3), and (C.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 Taking the LSAT



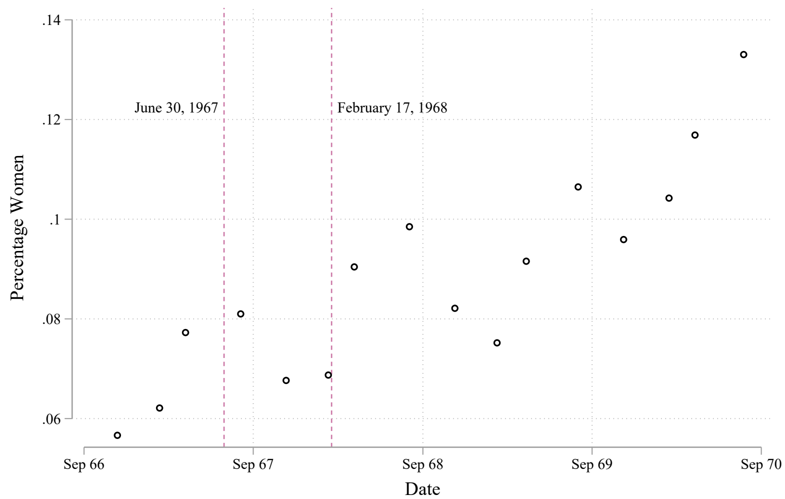
(a) Raw Data



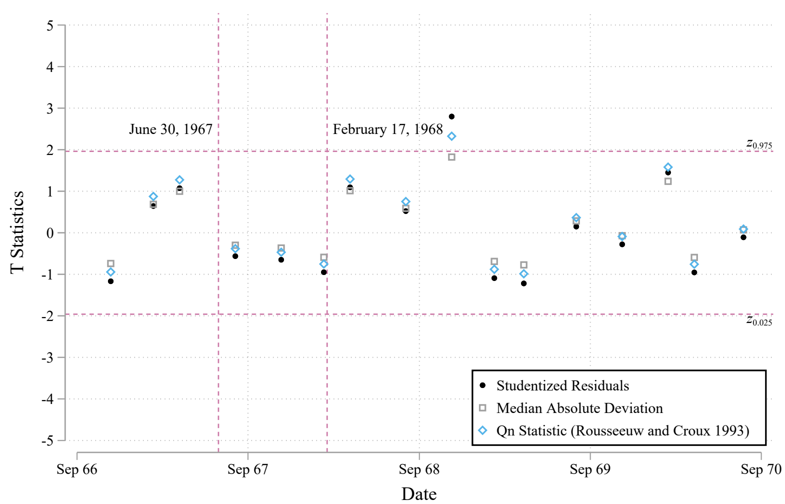
(b) Statistical Tests

Appendix Figure 28a plots the raw number of men who sat 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 (C.2), (C.3), and (C.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 Taking the LSAT who are Women



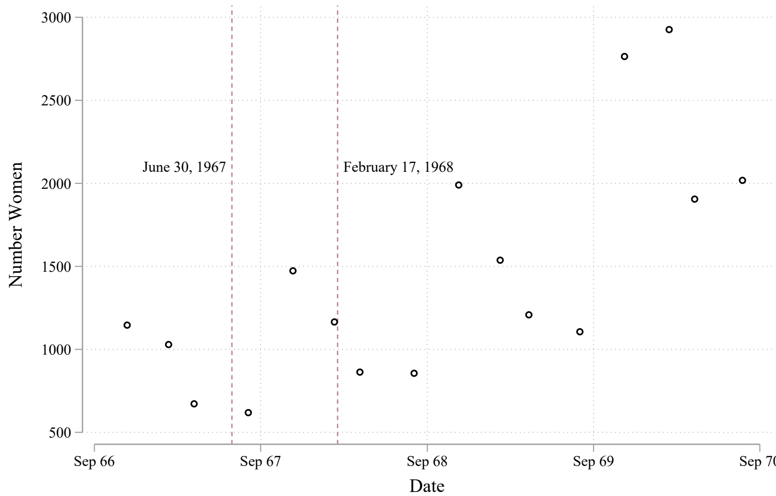
(a) Raw Data



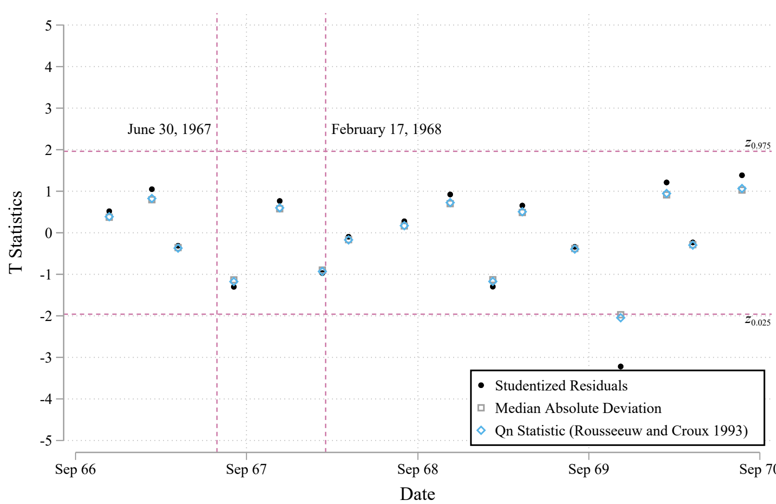
(b) Statistical Tests

Appendix Figure 29a plots the raw fraction of students who sat 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 (C.2), (C.3), and (C.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: Number of Women Registered to Take the LSAT



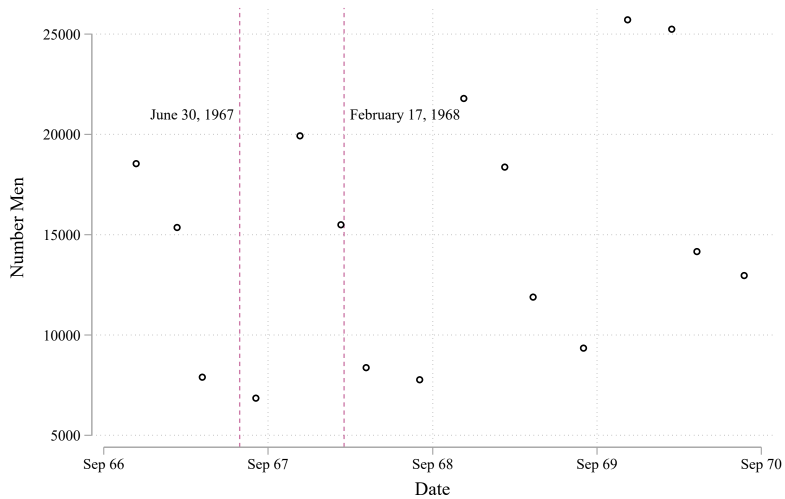
(a) Raw Data



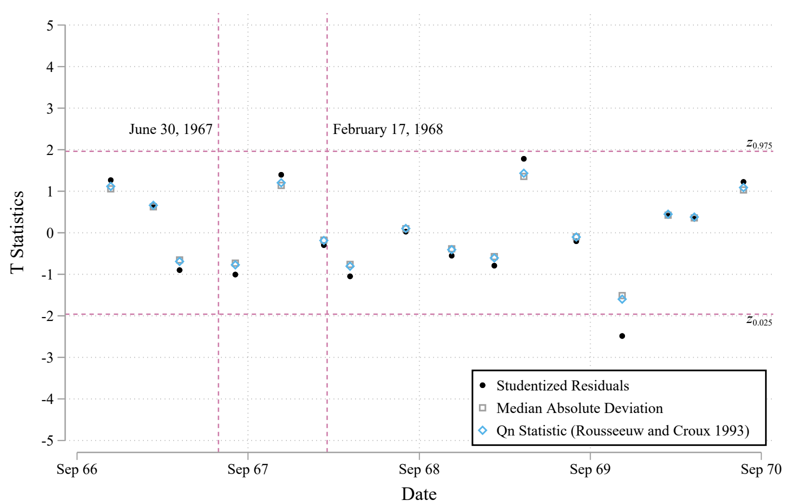
(b) Statistical Tests

Appendix Figure 30a plots the raw number of women who registered for each LSAT exam on the day of each exam administration. Appendix Figure 30b 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 (C.2), (C.3), and (C.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: Number of Men Registered to Take the LSAT



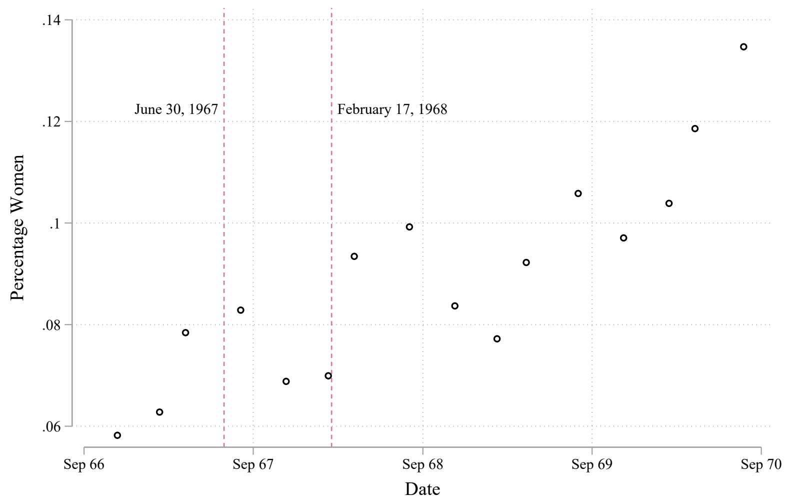
(a) Raw Data



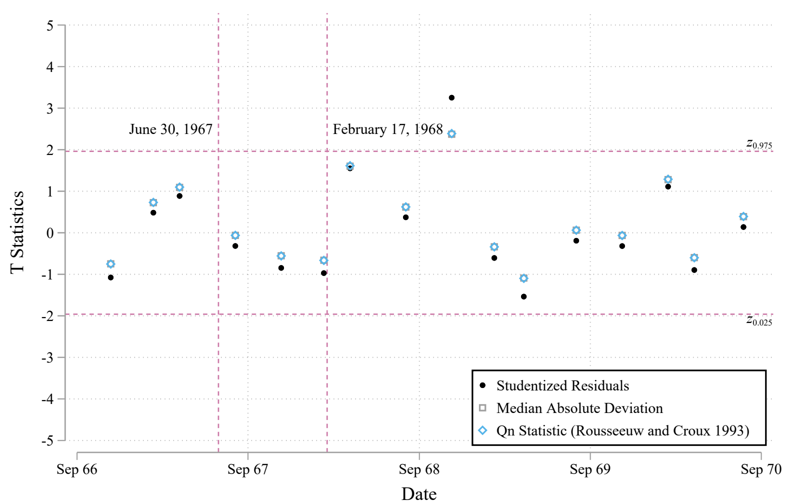
(b) Statistical Tests

Appendix Figure 31a plots the raw number of men who registered for each LSAT exam on the day of each exam administration. Appendix Figure 31b 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 (C.2), (C.3), and (C.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: Fraction of Students Registered to Take the LSAT who are Women



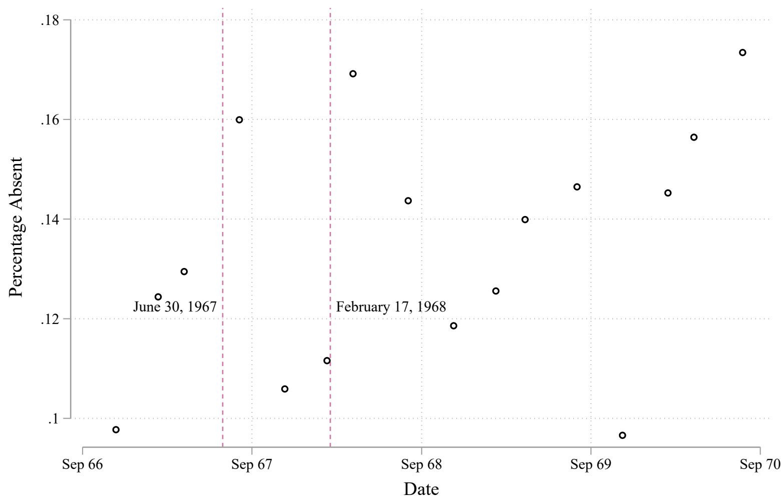
(a) Raw Data



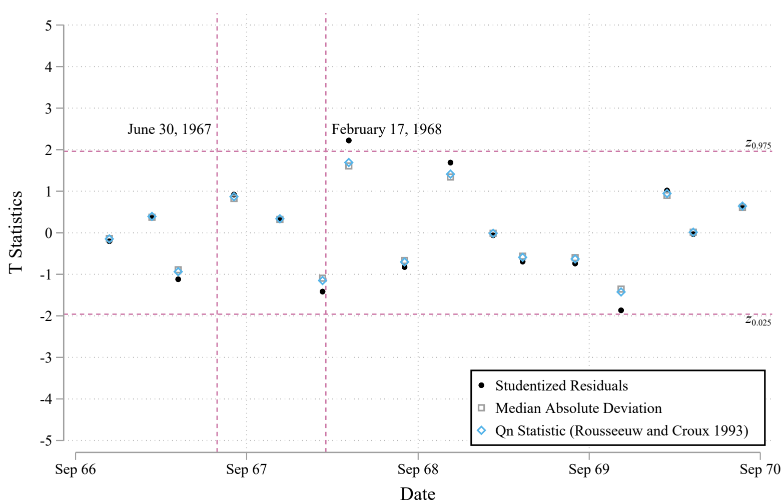
(b) Statistical Tests

Appendix Figure 32a plots the raw fraction of students who registered for each LSAT exam who were women on the day of each exam administration. Appendix Figure 32b 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 (C.2), (C.3), and (C.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 33: Fraction of Women Absent from LSAT



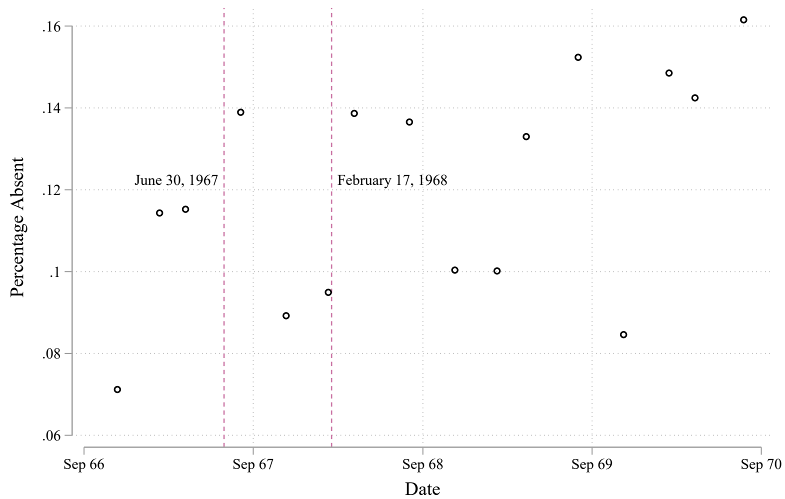
(a) Raw Data



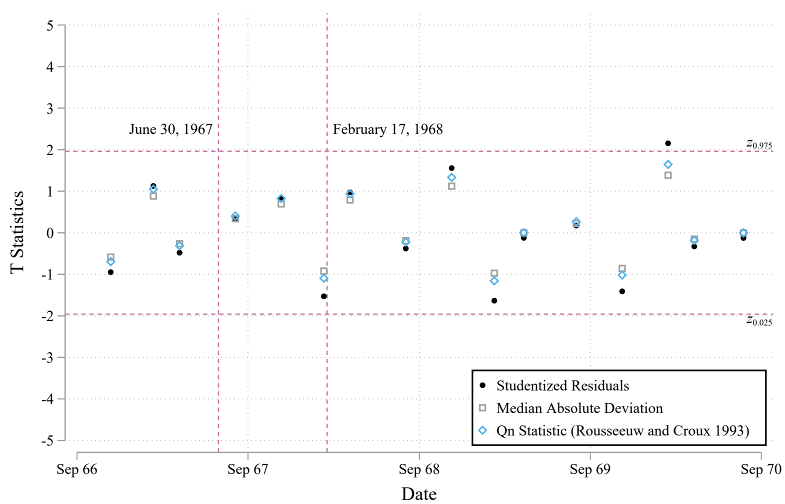
(b) Statistical Tests

Appendix Figure 33a 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 33b 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 (C.2), (C.3), and (C.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 34: Fraction of Men Absent from LSAT



(a) Raw Data

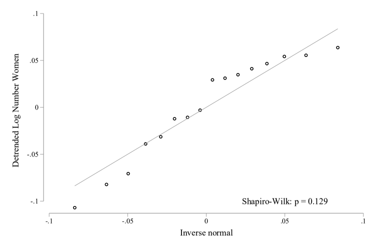


(b) Statistical Tests

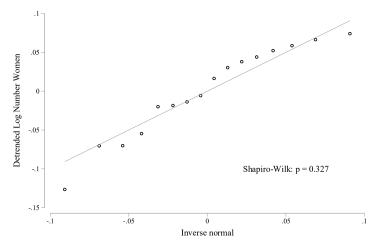
Appendix Figure 34a plots the raw fraction of men who registered for each LSAT exam that did not attend on the day of each exam administration. Appendix Figure 34b plots the studentized residual of the logit transformation of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equation (C.2), (C.3), and (C.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 35: QQ Plots

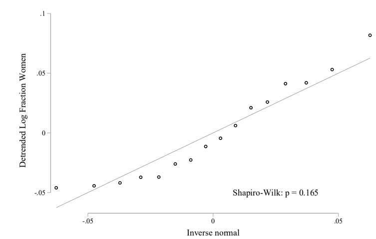
(a) Number Women Attended



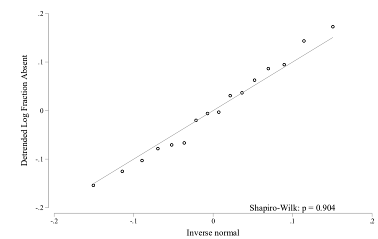
(b) Number Women Registered



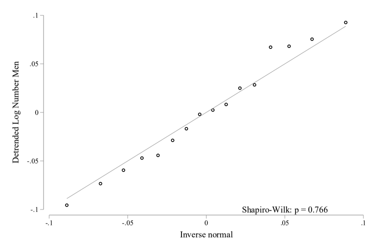
(c) Fraction Women Attended



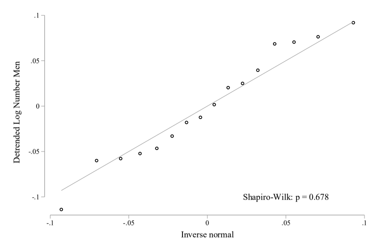
(d) Fraction Women Absent



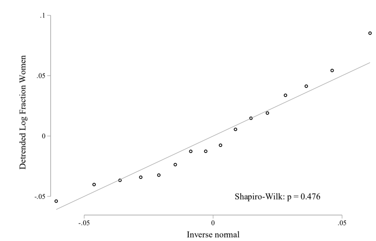
(e) Number Men Attended



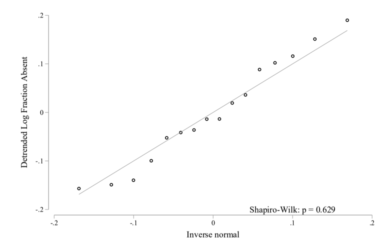
(f) Number Men Registered



(g) Fraction Women Registered

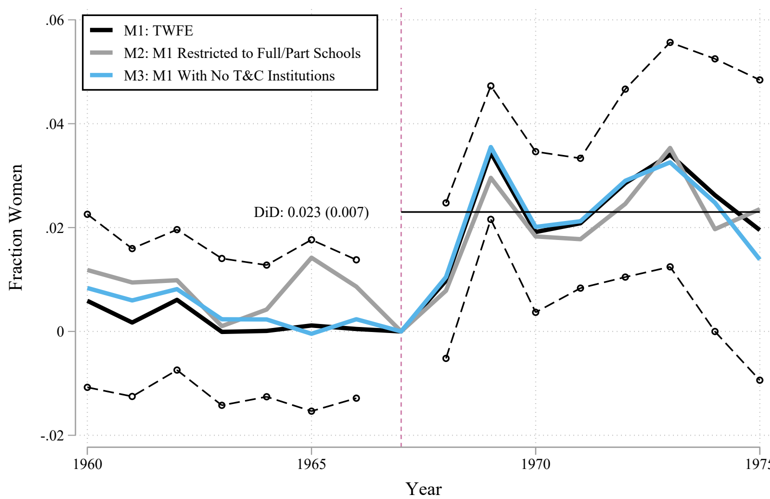


(h) Fraction Men Absent

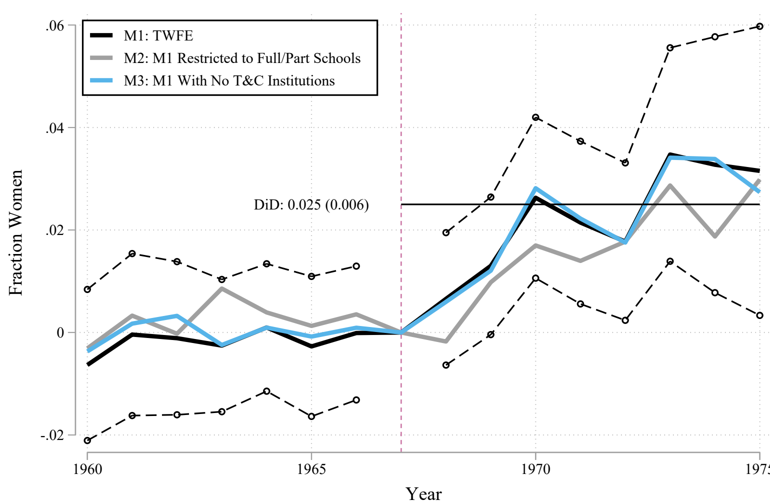


Each subfigure is comprised of a QQ plot for a certain variable, plotting the observed distribution against the value from the standard normal that would be observed at the empirical quantile of each observation. We also report the *p*-value from a Shapiro-Wilk test of normality. We plot results for the detrended log number of women attending each LSAT exam (Appendix Figure 35a), the detrended log number of men attending each LSAT exam (Appendix Figure 35e), the detrended log number of women registered to take each LSAT exam (Appendix Figure 35b), the detrended log number of men registered to take each LSAT exam (Appendix Figure 35f), the detrended logit transformation of the fraction of LSAT exam attendees who are women (Appendix Figure 35c), the detrended logit transformation of the fraction of students registered to take the LSAT exam who are women (Appendix Figure 35g), the detrended logit transformation of the fraction women women who register for but do not take each LSAT exam (Appendix Figure 35d), and the detrended logit transformation of the fraction women women who register for but do not take each LSAT exam (Appendix Figure 35h).

Appendix Figure 36: Difference-in-Differences Results: 2L and 3L



(a) Fraction Women (2L)

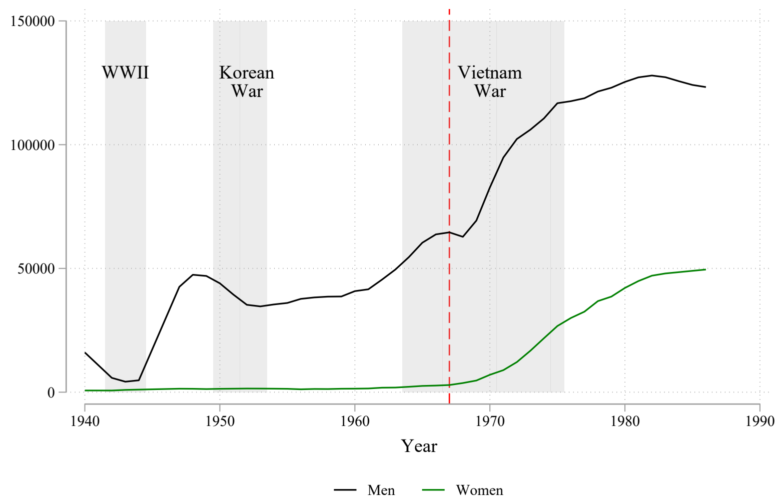


(b) Fraction Women (3L)

Figures 36a and 36b plot coefficient estimates from equation (1), where the outcome is the fraction of second-year (Figure 36a) or third-year (Figure 36b) students who are women and the regression is weighted by total second-year (Figure 36a) or third-year (Figure 36b) 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.

Appendix Figure 37: Enrollment statistics in levels

(a) Historical Enrollment



(b) Enrollment by Program Type

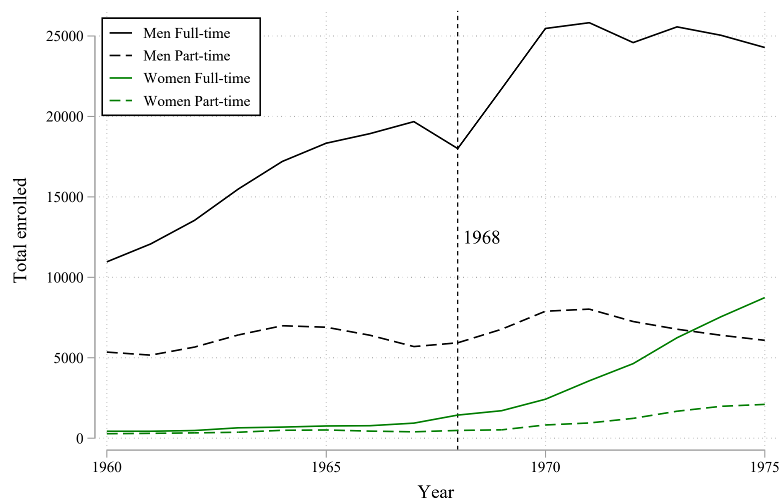
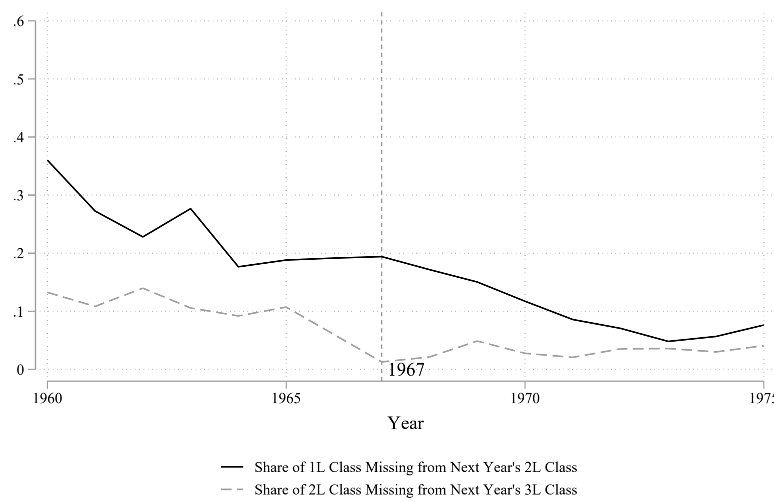


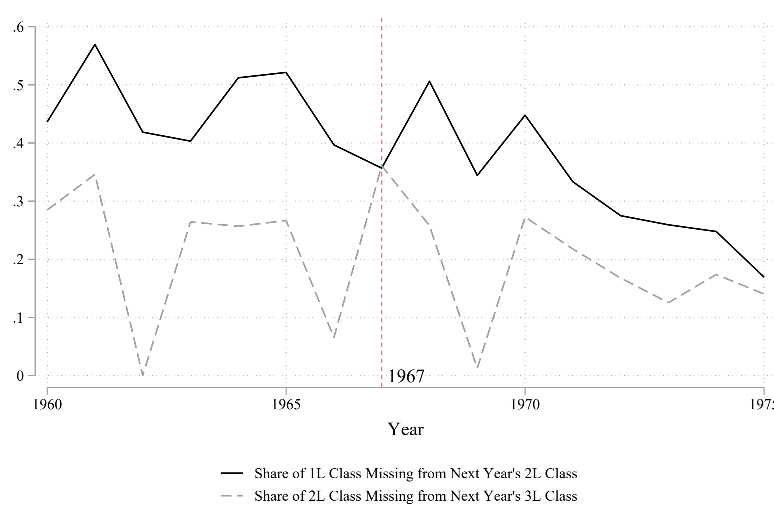
Figure 37a plots the percentage of all law students who are men and women between 1940 and 1986, collected from Table 27 of Abel (1989). Figure 37b plots the number of all first-year students who were men or women at U.S. law schools that reported to the *Review of Legal Education* from 1960 through 1975 separately for full-time and part-time programs. Data are collected from the *Review of Legal Education* in every year. Fractions are displayed in Figures 1a and 2a.

Appendix Figure 38: Aggregate Attrition for Women

(a) Full-Time Programs

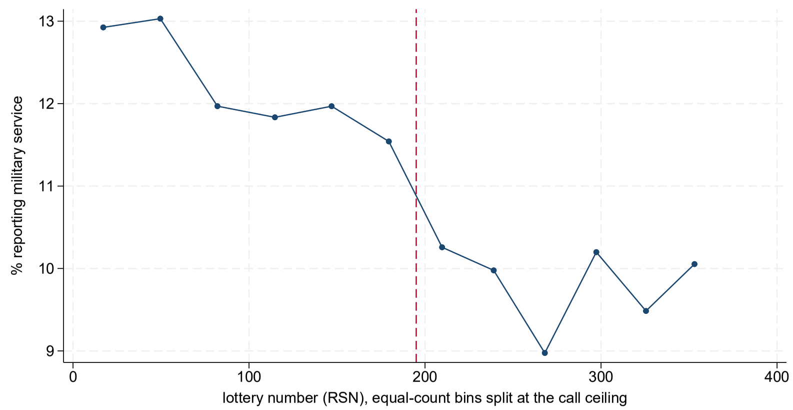


(b) Part-Time Programs

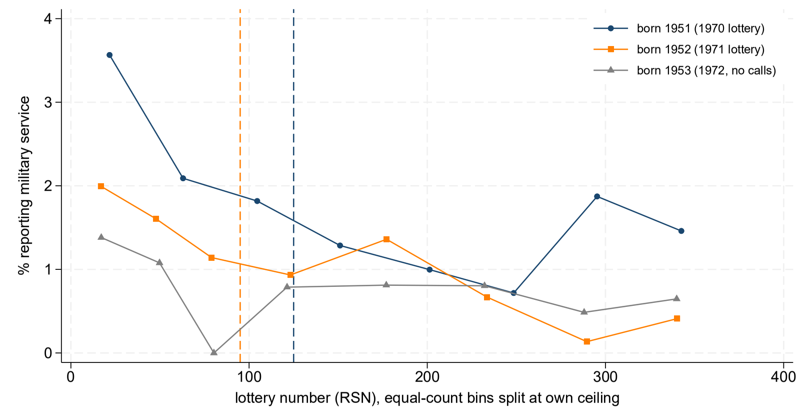


Each figure plots estimates of women's attrition from law schools in each year. To do this, we calculate the percentage drop in women's enrollment from year to year. These calculations are plotted in the baseline year; for example, in 1967, we plot the percentage of 1L students in 1967 that are missing 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 38a plots these series for full-time programs. Appendix Figure 38b plots these series for part-time programs

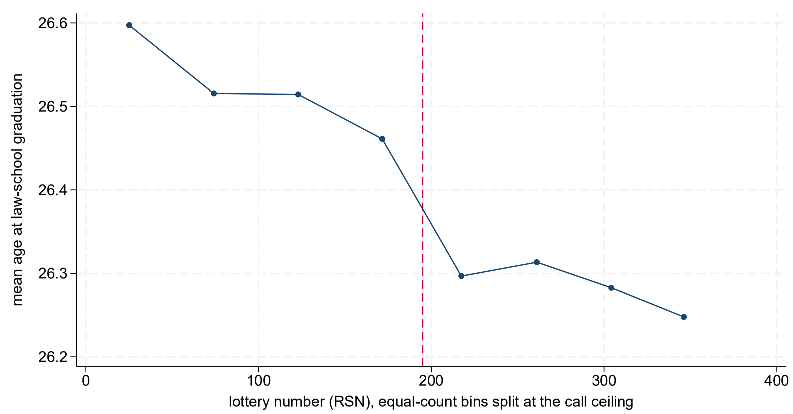
Appendix Figure 39: The draft lottery in the 1981 bar: service, timing, and partnership by lottery number



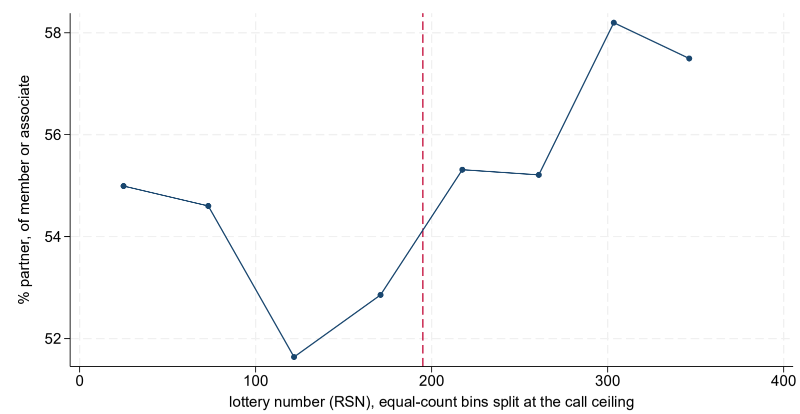
(a) Military service, men born 1944–50.



(b) Military service, single-year cohorts.



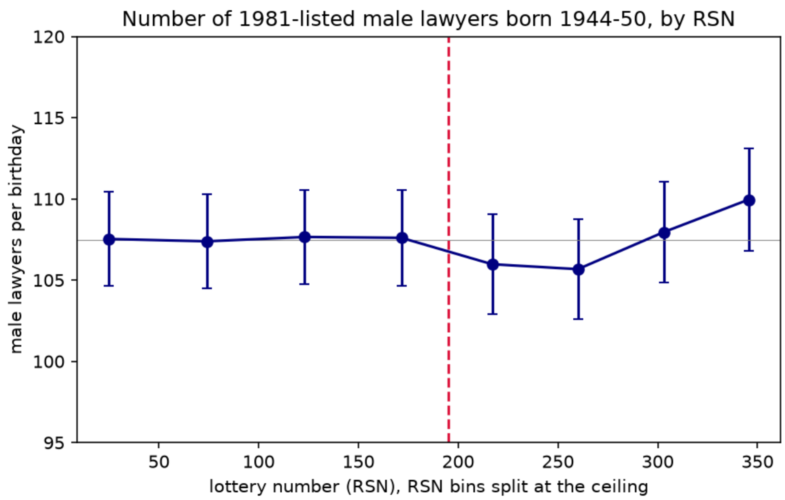
(c) Age at law-school graduation, born 1944–50.



(d) Partner vs. associate at firms with 10+ lawyers, born 1944–50.

Notes. Each point is the mean outcome within equal-count bins of the draft-lottery number, split exactly at the call ceiling (dashed lines) so no bin straddles the cutoff. Panel (a): share of men born 1944–50 reporting military service, by 1969-lottery number (ceiling 195). Panel (b): the same for men born 1951 (1970 lottery, ceiling 125) and 1952 (1971 lottery, ceiling 95), and for the placebo cohort born 1953, whose 1972 lottery assigned numbers but issued no calls; each series is binned around its own ceiling. Panel (c): mean age at law-school graduation. Panel (d): share listed as a member of the firm, among men holding an explicit member or associate designation at firms with ten or more listed lawyers. Sample: US listings in the 1981 Martindale–Hubbell Biographical Section with parseable birth dates.

Appendix Figure 40: The draft lottery in the 1981 bar: number of lawyers



Appendix Figure 40 *Notes.* Each point is the mean number of male lawyers in the 1981 Martindale–Hubbell Biographical Section per birthdate, among men born 1944–50 (the cohorts assigned numbers in the December 1969 drawing), within bins of the random sequence number (RSN); bins split exactly at the call ceiling of 195 (vertical line), so no bin straddles the eligibility cutoff. Error bars are 95 percent confidence intervals treating per-date counts as Poisson; the horizontal line is the overall mean. If draft exposure had pushed men out of the profession (or out of the directory), birthdates with low numbers would supply fewer listed lawyers; the profile is flat. In a regression of lawyers per birthdate on eligibility with birth-month fixed effects, the eligibility coefficient is -0.3 ($= -0.3$ percent of the ineligible mean of 107.4 per date, $t = 0.25$), ruling out extensive-margin losses larger than about 2.5 percent; the same null holds separately in every birth year 1944–53, for the never-called 1953 placebo cohort, and for women assigned pseudo-numbers..

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: WEAL Complaints by School and Date

School	**Date WEAL Complaint Filed**
Panel A: Early Complaints (1970)	
University of Maryland	January 31, 1970
University of North Carolina	March 16, 1970
Harvard University	March 25, 1970
University of Pittsburgh	March 25, 1970
University of Tennessee	April 22, 1970
George Washington University	April 22, 1970
Southern Illinois University	April 22, 1970
Rutgers University (Camden)	April 22, 1970
Rutgers University (Newark)	April 22, 1970
Boston College	April 26, 1970
Northeastern University	April 26, 1970
Boston University	April 26, 1970
University of Connecticut	April 26, 1970
University of Miami	April 26, 1970
Columbia University	May 11, 1970
University of Georgia	May 11, 1970
Florida State University	May 25, 1970
Florida A.&M. University	May 25, 1970
University of Florida	May 25, 1970
University of California (Berkeley)	June 1, 1970
University of California (Davis)	June 1, 1970
University of California (Los Angeles)	June 1, 1970
University of California (Hastings)	June 1, 1970
University of Michigan	May 28, 1970
University of Minnesota	June 30, 1970
Wayne State University	July 1, 1970
University of Wisconsin	July 5, 1970
University of Buffalo	July 1970
William & Mary	November 23, 1970
University of South Carolina	November 23, 1970
University of Chicago	November 23, 1970
Loyola University (New Orleans)	December 29, 1970
Louisiana State University*	December 29, 1970
Southern Methodist University	December 29, 1970
Panel B: Other Complaints (1971)	
Yale University	February 1, 1971
University of Hawaii	February 1, 1971
University of Missouri (Columbia)	February 26, 1971
Professional Women's Caucus (All Law Schools)	April 1971
University of Bridgeport	April 27, 1971
Drake University	April 27, 1971
University of Detroit	April 27, 1971
Willamette University	May 24, 1971

This table displays all complaints cataloged by the Women's Equity Action League and presented in U.S. Congress 1971[p. 336-338]. We code a complaint as occurring early is it was filed in calendar year 1970 (Panel A) and not early if it was filed in calendar year 1971 (Panel B), including a complaint against all law schools filed by the Professional Women's Caucus in April 1971.

Appendix Table 3: Law Schools Identified in HEGIS

Law School	**HEGIS Institution Name**
University of San Diego	U SAN DIEGO SCH OF LAW
Chicago-Kent College of Law	CHICAGO KENT COL OF LAW
John Marshall Law School	JOHN MARSHALL LAW SCHOOL
Detroit College of Law	DETROIT COLLEGE OF LAW
William Mitchell College of Law	WM MITCHELL COL LAW
Albany Law School (Union University)	ALBANY LAW SCHOOL
Brooklyn Law School	BROOKLYN LAW SCHOOL
New York Law School	NEW YORK LAW SCHOOL
Salmon P. Chase College of Law	SALMN P CHASE COLLEGE-LAW
Cleveland-Marshall Law School	CLEVELND MARSHALL LAW SCH
Dickinson School of Law	DICKINSON SCHOOL OF LAW
South Texas College of Law	SOUTH TEXAS COLLEGE LAW

This table displays all law schools that we are able to identify in the HEGIS data. We list the name of the school as well as the institution name as listed in the 1968-69 *Education Directory*.

Appendix Table 4: Prestige robustness of the FT/PT effect.

Specification	Composition (women's share)	Career (women→firm triple-diff)
Baseline	$+0.049^{***}$	$+0.165^{***}$
+ log-volume × cohort	$+0.042^{***}$	$+0.162^{***}$
Drop top prestige tercile (FT)	$+0.046^{***}$	$+0.123^{**}$
Prestige-matched (low-FT vs. PT)	$+0.028^{**}$	$+0.092$ ($p = 0.12$)

*Notes.*Prestige = log 1969 law-library volumes; PT-only night schools without survey data are imputed at 30,000 volumes (the bottom of the range), and FT-only schools missing from the survey are dropped. SE clustered by school. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Table 5: Draft-lottery dose response: outcomes by lottery-number group, men born 1944–50

Lottery numbers	Military service	On time	Age at graduation	Partner vs. assoc. (firms 10+)
1–95 (certain early call)	+0.029*** (0.004)	−0.047*** (0.006)	+0.186*** (0.026)	−0.019** (0.010)
96–125 (certain call)	+0.020*** (0.006)	−0.037*** (0.010)	+0.133*** (0.038)	−0.036** (0.014)
126–160 (mid-year call)	+0.024*** (0.006)	−0.043*** (0.009)	+0.158*** (0.037)	−0.045*** (0.013)
161–195 (late call)	+0.017*** (0.006)	−0.032*** (0.009)	+0.104*** (0.037)	−0.035*** (0.014)
196–230 (near miss)	+0.001 (0.006)	−0.002 (0.009)	+0.040 (0.037)	−0.016 (0.014)

Notes. Coefficients on lottery-number-group indicators (omitted group: numbers 231–366, never at risk) from one regression per outcome, with birth-month and birth-year fixed effects; heteroskedasticity-robust SE in parentheses. Groups follow the 1969 drawing's call schedule: numbers 1–95 were called early in 1970 and 96–195 through the rest of the year (the final ceiling was 195), while numbers just above the ceiling (196–230) were never reached—a near-miss falsification group that should, and does, resemble the omitted safe group. Sample as in Table 3; the partner outcome conditions on an explicit member or associate designation at firms with ten or more listed lawyers. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Table 6: Pill-access robustness of the full-time-only draft break

Panel A: national horse race, person-level pill exposure

	(1) Baseline	(2) +GK pill	(3) +Bailey pill
FT-only × post-1971	4.88***	4.83***	5.37***
	(1.19)	(1.19)	(1.20)
FT-only $\times P^{GK}$		0.31	
		(1.66)	
FT-only $\times P^{Bailey}$			-2.58
			(1.97)
Observations	349,373	349,373	349,373
School clusters	75	75	75

Panel B: state triple-difference, early legal access ≤ 1971

	(1) GK strict	(2) GK broad	(3) Bailey
FT-only × post (late-access states)	4.68***	5.84***	3.84**
	(1.69)	(0.86)	(1.60)
FT-only × post × early	0.39	-1.27	2.69
	(2.28)	(1.66)	(2.16)
Observations	349,330	349,330	349,330

Notes: Coefficients × 100 (percentage points); standard errors clustered by law school in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Linear probability models of an indicator for female on the reported interactions with law-school, bar-cohort, and directory-edition fixed effects. Sample: one row per lawyer listing, pooled 1974/1975/1976/1981 Martindale–Hubbell editions, bar-admission cohorts 1958–1978, full-time-only (draft-exposed) versus part-time-only (control) law schools. P is the 1970-population-weighted share of states in which the lawyer's birth cohort had legal access to the pill before age 21 (zero for cohorts turning 21 before FDA approval in 1960), under the Goldin–Katz broad coding (column 2) or Bailey's (2006) Table I dates (column 3). In Panel B, early = school-state early-legal-access year ≤ 1971 (GK strict / GK broad / Bailey codings).

Appendix Table 7: Replicating Goldin and Katz's (2002) Table 5 in the Martindale–Hubbell lawyer census

Outcome (age × edition cell)	Coefficient on cohort pill access P_c (1) Goldin–Katz coding	(2) Bailey coding
Panel A: Women		
Female share of lawyers (WLS)	0.126***	0.132***
	(0.012)	(0.014)
ln women lawyers	0.564***	0.660***
	(0.170)	(0.168)
ln women lawyers, controlling ln men	1.293***	1.307***
	(0.111)	(0.118)
ln women lawyers per woman	0.309**	0.372***
	(0.125)	(0.125)
ln women lawyers per female BA	0.521***	0.595***
	(0.111)	(0.108)
Panel B: Men (placebos)		
ln men lawyers	−0.369***	−0.342***
	(0.087)	(0.088)
ln men lawyers per man	−0.623***	−0.629***
	(0.070)	(0.072)
ln men lawyers per male BA	−0.238	−0.197
	(0.159)	(0.173)
Age and edition fixed effects	X	X
Cells (age × edition)	218	218
Birth-cohort clusters	40	40

Notes: Each cell of the table reports the coefficient on P_c from a separate regression on 218 age × edition cells (single ages 30–49; twelve Martindale–Hubbell editions, 1965–2000; birth cohorts 1921–1960), where each cell is a near-complete census count of American lawyers by sex. P_c is the 1970-population-weighted fraction of states in which the birth cohort c had legal access to oral contraception before age 21, set to zero for cohorts reaching 21 before the pill's 1960 FDA approval; column (1) uses the Goldin–Katz (2002, Table 2) broad coding and column (2) the Bailey (2006, Table I) coding. All regressions include single-age and edition fixed effects; standard errors, in parentheses, are clustered by birth cohort. The share regression is weighted by cell size; log-outcome regressions are OLS. Population denominators are July 1 national estimates by single age and sex (resident plus armed forces overseas); the BA denominator is bachelor's degrees conferred to the cohort's sex in year $c+22$ (results are similar at $c+21$ and $c+23$, with the per-BA coefficient ranging from 0.41 to 0.62 in column (1)). Significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.