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. These admissions were more concentrated in elite programs and those that lost more men to the draft. To understand the labor market impacts of draft disruption, we digitize over 600,000 pages of archival directories of practicing lawyers to document downstream effects on the bar. We show that the draft increased women's representation at prestigious law schools, in private practice, and as law firm partners.

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1 Introduction

Until the late 20th century, changes in women's representation in U.S. law schools were driven almost entirely by manpower needs for the military. During periods of conflict, men's enrollment in higher education often declined substantially as young men volunteered for service, were drafted, or entered wartime industries in lieu of enrolling in school (Flynn 1993). As a result, women's share of incoming students swelled as law schools struggled to stabilize enrollment. This pattern is evident in Figure 1a, which plots women's representation in full-time and part-time law programs against the number of inductions in the previous year. Before 1967, women's representation in law schools was highly correlated with inductions: women comprise around 5% of students in peacetime, but around 25% of law students at full-time programs as inductions reach their peak during WWII; similarly, during the Korean War, women experience a temporary boost in representation as inductions swell in the early 1950s (Fossum 1983).[1] However, the Vietnam War era presents a striking end to these earlier wartime patterns, as women's representation continues to grow even as inductions subside. Further, this growth was driven by increases in women's enrollment, while most of the prior wartime improvements in women's representation were almost entirely due to declines in men's enrollment.

The rapid integration of women into the nation's law schools during the 1970s and 1980s has received much attention from economists. This was part of the "Quiet Revolution" where women entered many traditionally male-dominated professional occupations in large numbers, which had major ramifications for economic growth (Hsieh et al. 2019). Further, law is an occupation in which gender representation is of particular interest, as lawyers disproportionately occupy positions of power: Bonica (2017) finds that, relative to the average citizen, lawyers are 99 times more likely to be elected to Congress, and they comprise the near entirety of the judiciary. The existing literature attempting to explain the growth of women in law has emphasized the role of diffusion of the pill in providing women more control over fertility timing, which lowered the cost of entering careers requiring a large initial educational investment (Bailey et al. 2012; Goldin and Katz 2002), as well as federal anti-discrimination efforts in employment and higher education, including Title VII of the Civil Rights Act (Beller 1983), Title IX (Rim 2021) and Executive Order 11246 (Helgerman 2025). Yet these explanations, which all would have impacted women entering law school in the 1970s, cannot explain why, as Figure 1a clearly demonstrates, women's entry into law begins abruptly in 1968.[2]

This paper provides the first causal evidence that the Vietnam draft had a marked impact on women's representation in law schools, corresponding directly to the break in this time series in 1968. During the early stages of the Vietnam War, graduate and professional students were

[1] Though we are not able to locate regularly available data before 1940, Abel (1989) reports an identical pattern during WWI.

[2] In fact, the legal education literature has often interpreted this fact as evidence that women's integration into law schools was a consequence of the draft, though no formal evidence has been provided (Fossum 1981, 1983; Strebeigh 2009). This possibility was also noted by Goldin and Katz (2002, fn. 56).

broadly shielded from the draft through the student deferment program that developed throughout the 1950s. However, in 1968, many of these protections were removed. We argue that admissions committees responded by increasing women's enrollment to curb draft risk to their tuition revenue, and that this change likely played a role in dissolving institutional barriers for women. To show this, 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 that women's representation rose by 44% (2 percentage points) in full-time programs relative to part-time programs in 1968. Further, we use the same design to explore whether this change mattered for women's careers once they enter the bar. We digitize the *Martindale–Hubbell* lawyer directory in almost every year between 1960 and 2000, generating novel employer-employee matched data on the near-universe of barred lawyers. We find that the change in deferment rules had significant consequences for early and mid-career outcomes for women, leading to increases in the likelihood of women graduating from full-time programs working as associates 6 years after bar admission and making partner 13 years after bar admission.

Figure 1b plots the raw data that illustrate 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. To complement this design, we build and calibrate a toy model of the admissions decision using available data on sex-specific performance on the LSAT. We find that, on the margin, women are positively selected relative to men, suggesting that women's increased enrollment at top programs reflected more talented women entering law school, rather than gender differences in the cutoff for applicant credentials.

This design assumes that part-time programs will be unaffected by changes in deferment policy, but if women who would have enrolled in part-time programs enrolled instead as full-time students, our results would overstate the true policy effect. To rule this out, we run a similar design using M.D. students as a comparison group, as they retain draft deferments in 1968 but have vastly different admission requirements relative to law schools, finding very similar results. In addition, we consider a third, complementary design that leverages plausibly exogenous differences in exposure across treated full-time programs. 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, led programs to admit more women in Fall 1968.

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. To do this, we 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 immediately after 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 for students graduating in 1970 and 1971, 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 removed from the profession entirely. 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 lawyers from full-time-only and part-time-only schools, we estimate a 6.45 percentage point increase in women's representation six years after bar admission for the 1972–79 cohorts relative to 1956–69, excluding the 1970–71 transition cohorts. Women from full-time-only schools were also more likely to be associates six years after bar admission and to hold large-firm partnership positions thirteen years after bar admission. The corresponding increases

for men across institutions are far smaller, suggesting that these changes cannot be explained by differential changes in the relative stature of full-time and part-time schools.

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 increase women's representation perhaps initially sparked by draft-related disruptions.

1.1 Related Literature

This project makes several contributions. First, we contribute to a large literature documenting how the Vietnam War caused substantial disruptions to domestic life. This includes impacts on men's educational attainment (Angrist and Chen 2011; Angrist and Krueger 1992; Card and Lemieux 2001; Schmitz and Conley 2017), earnings (Angrist 1990; Angrist et al. 2011; Berger and Hirsch 1983; Goldberg and Warner 1987), employment (Autor et al. 2011; Johnson and Conley 2019), health (Angrist et al. 2010; Dobkin and Shabani 2009; Eisenberg and Rowe 2009; Grimard and Parent 2007; Grossman et al. 2018; Hearst et al. 1986; Schmitz and Conley 2016; Wang et al. 2025), criminal behavior and incarceration (Kuziemko 2010; Lindo and Stoecker 2014; Rohlfs 2010; Wang and Flores-Lagunes 2022), and migration behavior (Malamud and Wozniak 2012); as well as fertility and marriage decisions (Bailey et al. 2021; Bailey and Chyn 2020; Bitler and Schmidt 2012; Kutinova 2009), household stability (Conley and Heerwig 2011), and outcomes for children of veterans (Deza and Mezza 2025; Goodman and Isen 2020). However, there has been little work exploring how these changes impacted women's roles in the labor market. This topic has received much attention in the context of WWII, as drafted men leaving the labor force (Acemoglu et al. 2004; Goldin and Olivetti 2013), as well as increased demand from wartime industries (Rose 2018; Shatnawi and Fishback 2018), led to changes in women's labor supply.[3] The only work we are aware of in the Vietnam era is Cross (2024), which shows that the gender desegregation of the army, a response to a shortage of recruits as the draft ended, boosted women's entry into nontraditional occupations.

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. As we noted earlier, most causal evidence on the role of institutional change in women's access to professional education has focused on either federal anti-discrimination policy (Beller 1983; Helgerman 2025; Rim 2021), or the role of the pill in improving women's control over the timing of family formation (Bailey et al. 2012; Goldin and Katz 2002). Our analysis broadens this literature by highlighting the indirect impact of a government

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

policy not directly tied to higher education. Finally, we contribute to a 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 for several complementary empirical designs. Section 5 shows how our main results translated to the composition of practicing lawyers. Section 6 examines changes in the 1970s that likely reinforced the impacts from the draft. Section 7 concludes.

2 Background

2.1 Law School Admission and Women in Law Schools

Throughout the first half of the 20th century, women's representation at law schools tended to vary inversely with prestige. At the turn of the century, women's enrollment in law schools began to grow due to the increased number of part-time law schools (Chester 1985). These programs tended to lack approval from the American Bar Association (a marker of prestige), where women were overrepresented. In 1930, when 57.4% of all men enrolled in law schools were at unapproved programs, 76.3% of women were at similar programs (Hummer 1976), with roughly 30% of all women enrolled at Portia Law School, a part-time unapproved school that enrolled women exclusively.

The only time this pattern was undone was during wartime, though it was reasserted once the conflict had ended (Abel 1989). This is apparent for WWII in Figure 1a, where women's representation in full-time programs briefly eclipses that in part-time programs, reaching around 25%. However, this was more of a mirage than anything, reflecting drastic changes in men's enrollment, not improvements in access for women. This was evident during WWI: women's representation among law students rose from 5% to 17%, but women's enrollment at schools only operating full-time programs actually decreased, while women's enrollment at schools only operating part-time programs rose by 92.4% (Abel 1989).[4] Beginning in 1940, when enrollment data are more consistently available, we formalize this analysis by decomposing changes in women's representation at full-time programs into the sum of changes due to shifts in women's full-time enrollment and men's full-time enrollment. A derivation is presented in Appendix Section F.1, and the results are plotted in Appendix Figure 1. Most notably, the rise in women's representation in full-time programs during WWII is almost entirely attributable to a drastic fall in men's enrollment; in fact, women's enrollment increases the most rapidly when law schools expand after the war to accommodate a sharp rise in men's enrollment. A similar pattern, while much less drastic, is present during the Korean War. As noted before, the Vietnam War presents a distinct departure from this pattern,

[4] At schools operating both, women's enrollment rose by 32.4%.

as changes in women's enrollment drive up representation starting in the 1960s.

That said, this increase in enrollment was initially very small and took place at a time when legal education was difficult for most women to access. As law school enrollment surged in the years after WWII, several programs continued to refuse to enroll women, who did not gain access to Harvard until 1950, Notre Dame until 1969, and Washington and Lee until 1972 (Epstein 1993). Even when these formal barriers fell, women were prevented from enrolling in greater numbers through a system of informal quotas that continued to operate. These were never documented formally, but it was "common knowledge" among law faculty that these limited women's enrollment through the 1960s (Epstein 1993, p. 52). Schwartz (1976), recounting her experience applying to Columbia Law, gives a sense of how admissions committees operated. Schwartz, then an undergraduate student at Brown, heard from her Dean that the men who had applied to Columbia had heard back while she was still waiting on a decision:

> The Dean called Columbia's admissions office and then told me: "It seems that they've already made their decisions on what men to take and they are meeting this afternoon to discuss the applications from women." (Schwartz 1976, p. 83)

This would not have been out of place among admissions committees for professional programs at the time: Smith (2011) reports in detail the admissions process of the Cornell School of Veterinary Medicine, where offers to men, for a prescribed number of seats, would be made before women's applications, sitting in a separate pile, would be considered. In any case, law school administrators were not exactly shy about their admission preferences; Dinerman (1969) notes that law schools did admit to vetting women's applications more carefully. When Harvard Law announced that it would enroll women, the Dean told *The New York Times*, "Opportunities for women in the law are still limited ... It is our expectation that we will admit only a small number of unusually qualified women students" (The New York Times 1949). Application statistics are scarce, but there is evidence that this constraint did bind. In 1951, 19 women were accepted out of 36 applicants to Harvard Law; in 1965, applications had increased four-fold to 136, but only 22 women had been accepted (Harvard Law Record 1965).

Further, once enrolled, the environment women law students found themselves in was hostile towards their presence. The space itself was often not structured for women's involvement. Living quarters were generally male-only, which excluded women from the study groups that formed there (Epstein 1993). When women were first admitted, integration was haphazard: Schwartz (1976) was invited to live in the all-male dorm upon acceptance to Columbia, where students admitted earlier had reported one women's bathroom in the entire law building, which was a men's bathroom with "wo" painted on (Morello 1986). In the classroom, male classmates reacted in ways that varied from "indifference to hostility," but the harshest treatment came from male faculty (Epstein 1993). The most egregious example of this was the widespread occurrence of "Ladies' Day," where women were singled out and asked questions designed to mock or embarrass them. At Harvard Ladies'

Day, one professor quizzed women on the rights wives have over their husbands' property were they to pass away (Epstein 1993); at Columbia, another would tell students, "better get back to the kitchen" if they answered incorrectly (Schwartz 1976). This was a specific instance of a male-dominated climate that pervaded legal education. The Dean of NYU (another program that had a Ladies' Day) lamented "jokes or illustrations in class offensive to women; embarrassment of female students by differentiated treatment; and even expressions that might suggest male superiority" (McKay 1971). This was reflected in law textbooks as well, with Ginsburg (1971) noting an example in a property casebook, which stated that "land, like woman, was meant to be possessed." With this context, we now move to characterizing the draft and how it changed in the late 1960s, before discussing its impact on law schools and potential role in this transition.

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 the Battle of Britain (Flynn 1993, p. 17).[5] Though the details have changed, the basic structure of the draft remained constant throughout its active operation until 1973. Following their 18th birthday, men were required to register for the draft.[6] The draft had a decentralized structure, where an individual registered by reporting to a local draft board composed 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 study. 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

[5] 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).

[6] 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).

case of a future conflict; in addition, these concerns also prompted calls to provide draft protection for scientific manpower to develop technological capabilities for war (Flynn 1988). This marked the beginnings of a fundamental shift of the draft apparatus from the egalitarian ideals promulgated during WWII to a more "scientific," selective approach, providing deferments to serve economic and strategic interests. As draft calls escalated with U.S. involvement in the Korean War, President Truman introduced the student deferment system with Executive Order 10230 on March 31, 1951.

The initial structure of this system largely reflected the recommendations of the Trytten 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.[7] A consensus had emerged that student deferments ought to 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.[8] 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).

[7] 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).

[8] 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 (AGCT) (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).

These recommendations were solidified into policy with the passage of the Military Selective Service Act of 1967, which limited graduate deferments to cover only programs "necessary to the maintenance of the national health, safety, or interest" (PL 90-40 1967). This was passed on June 30, 1967, but in an executive order implementing this legislation, the Johnson administration postponed designating fields that would qualify until the National Security Council issued its recommendation (Flynn 1993). The resulting decision was announced by the Selective Service System on February 16, 1968, removing all graduate and professional deferments with the exception of medical and dental students (The New York Times 1968).[9] 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 11360, issued 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.

2.3 Impact on Law Schools

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 first-year enrollment of 540 would fall by half if 2-S deferments were eliminated (Strebeigh 2009). These pressures led to leadership suggesting that 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, writing at the time suggests that at least some policies had changed, with *The Harvard Crimson* reporting 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 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 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 1968).

2.3.1 Evidence on Men's Attrition

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

[9] In our analysis, we date this to when this announcement was widely reported on the next day (February 17, 1968), which is sometimes when the announcement is dated (Blakey 1968). This difference in timing is completely inconsequential to the results in the paper.

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 $t+1$.[10] We observe enrollment in each year of law school, so we are able to calculate attrition between the first and second year as well as the second and third year of law school. These data are plotted in Figure 2.

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

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

There were several ways to avoid induction apart from enrolling in school. For one, all men 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).[11] 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 vul-

[10] 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.

[11] 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).

> nerable 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 knew 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.

2.3.2 Evidence on Women's Enrollment

Many impacts of the draft on law school enrollment are evident in descriptive statistics. Figure 1b plots the fraction of first-year students in full-time and part-time programs who are women. In the fall of 1968, when the change in deferment rules takes effect, we see a sharp increase in women's representation in full-time programs, with no change in the same statistic for part-time programs, relative to trend. It is unclear that this represents an improvement in women's access however; it could be the case, as in WWII, that this is driven by changes in men's enrollment. To explore this possibility, we plot the first-difference in women's first-year enrollment for full-time students in ABA-Approved programs in Figure 2c.[12] In 1968, there is a sharp increase in women's enrollment of 516 students, around three times higher than the highest previous increase in the 1960s. This increase may seem small, especially when compared to increases in the 1970s, but represents a 58% increase over women's first-year enrollment of 894 students in 1967. Throughout our sample period, this represents the largest single-year percentage increase in women's first-year enrollment.

The other change that is apparent in the aggregate data is that, in Figure 1b, women's representation in full-time and part-time programs moves in lockstep following the convergence in 1968. Given our understanding of how admissions committees were operating in the 1960s, one possible interpretation is that, given the pressures on men's enrollment, law schools abandoned their informal quotas in 1968, which were never reimposed. Unfortunately, as we noted earlier, it is difficult to verify that these quotas existed at all, let alone any numerical targets that we could bring to the data. The closest we have been able to come is a letter to the editor of the *Harvard Law Record* from a 1967 alumna of the law school, describing an experience from her first year of law school in 1964:

> The explanation from the Dean's mouth during my first year that Harvard Law had then reached an enrollment for women of 5% for each class; that Harvard would probably not go above that 5% level since that was Yale Law School's percentage; and that, after all, there could never be a great influx of women into the school (Harvard Law Record 1969)

We plot the fraction of first-year students in Harvard and Yale Law Schools who were women in Figure 2d. It is, of course, impossible to conclude definitively, but the data are strongly consistent

[12] We restrict to ABA-Approved programs so that the set of schools (and thus women's enrollment) does not change depending on which schools reported enrollment figures in which years; ABA-Approved programs report enrollment figures in every year, but this was optional for unapproved programs.

with the hypothesis that women's representation was clustered around 5% between 1960 and 1967. This pattern breaks in 1968, as women's representation rises substantially across both programs, where it continues to climb across the 1970s. This reflects the conclusion reached by Fossum (1981), who argues that draft pressure breaking informal quotas at law schools, combined with women's surging interest in legal education as the women's movement accelerated, resulted in the steep climb in enrollment in Figure 1a. We contribute to this discussion by introducing several new pieces of causal evidence, which we now turn to.

3 Data and Empirical Design

3.1 Review of Legal Education

Our primary data source is the *Review of Legal Education* (RLE), which is published in every year in our sample period. In each year, the RLE publishes enrollment counts for every law school that responds to its survey. This includes the universe of American Bar Association (ABA) approved law schools, as well as a subset of non-ABA approved institutions that respond as well. These counts are reported separately for each cohort; we denote these years using the standard nomenclature 1L, 2L, 3L, and 4L for students in their first, second, third, and fourth year, respectively. For each cohort, the total number of enrolled students is reported, as well as the total number of women enrolled, allowing us to construct the fraction of students who are women, our main dependent variable of interest.

For each law school, these data are also reported separately for each type of program the school offers. Beginning in 1971, only two types of programs were used: full-time (called daytime between 1971 and 1973) and part-time. However, in earlier years, programs were differentiated by whether or not they were offered in the morning or evening. Fortunately, it is straightforward to identify the morning program that enrolls full-time students, as this is always listed first below the name of the institution. Following this, many schools operate an evening program, as well as many different morning programs, subsuming both a separate part-time program where students take daytime classes, as well as an additional option for students in the evening program to take daytime classes, and potentially other options we are unaware of.[13] 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.[14] To understand the variation

[13] An illustrative example of a school entry in the RLE is provided in Appendix Figure 33.

[14] Schools reporting, say, only one or two students studying part-time likely reflect students in the full-time program

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. Between 1960 and 1975, the number of schools operating only a part-time program declines substantially, but it is important to note that this can result from the addition of 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 the 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_τ 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 who are women in full-time programs follows the same trend as the proportion of students who are women 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 program-specific institutional 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.[15]

Our baseline specification (model 1) 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 institutions offering both full-time and part-time programs differ in a way that leads to hetero-

who moved to a part-time course of study after matriculation.

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

geneous 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 composed of the same set of institutions (model 2). This also results in a "matched" sample where treatment and control groups are composed of the same set of institutions. However, such a comparison is vulnerable to potential spillovers between the treatment and control groups within institutions. So, we also implement a separated design where there is no overlap in institutions between the treatment and control group (model 3). 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. 2023), 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 of the impact of the policy change across the entire post-period.

4 Results

4.1 Difference-in-Differences Results

Figure 3a plots the estimates from equation (1), and summary difference-in-differences estimates from equation (2) are reported in the first row of Table 1. In the years leading up to the policy of interest, we see slight evidence of a negative pre-trend, suggesting that full-time programs were enrolling fewer women than part-time programs over time, though none of the event coefficients are statistically different from 0. Starting immediately in the fall of 1968, following the change in 2-S deferment status for law students earlier that year, we see a large, discrete jump in the fraction of students at full-time programs that are women, relative to part-time programs. There is an immediate treatment effect of around 2 percentage points, representing a 44% increase in women's representation over a baseline value of 4.5%. In the aggregate, the fraction of all full-time students

that are women rises from 4.5% 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 only slightly higher, 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.[16] Event coefficients and fixed effects remain identical to those included in equation (1), and we summarize the estimates from this design with a similar summary difference-in-differences specification:

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

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

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

[16] 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 3 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.

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 4, 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) taking the exam 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.1. 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 5), the number of women registered for (Appendix Figure 6) and taking (Appendix Figure 7) the LSAT, the fraction of students registered for (Appendix Figure 8) and taking (Appendix Figure 9) the LSAT who are women, as well as the absence rate (registered but did not attend) for women (Appendix Figure 10).

These results survive a battery of robustness checks, which are presented 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 11), as well as all programs that obtain ABA approval by the end of our sample period (Appendix Figure 12). 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 13) or the institution-program level (Appendix Figure 14). We drop institution-program-years that enroll fewer than 10 students to improve precision; Appendix Figures 15 and 16 show that our results are not sensitive to the size of this restriction and are unaffected by dropping institution-program-years with fewer than 20 and 50 students, respectively. Appendix Figure 17 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 18 presents results with city-by-year fixed effects to rule out any idiosyncratic variation that might be occurring at this level. This results in significantly more variation, especially in the more restricted models 2 & 3, but the results from model 1 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 19a. 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 Appendix Figures 19b and 19c, our 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 presented in Appendix Figure 20 with 95% confidence bands plotted for all models. We allow for potential covariance across time within institution (Appendix Figure 21) as well as state (Appendix Figure 22), 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 23 presents results where weights are fixed at total enrollment in 1967 across multiple thresholds for removing smaller programs (as in Appendix Figure 19), and Appendix Figure 24 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.[17] We explore these auxiliary outcomes in Appendix Figure 25, and difference-in-differences estimates are presented in the 2L and 3L panels 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 25a, which presents results for second year students, appears odd at first glance—all three models show a slight increase in the fraction of women enrolled in the second year in 1968, one year before we should see a causal effect. This is not a statistical artifact but actually reflects the impact of increased attrition for men between years 1 and 2 we saw in Figure 2a.[18] Then, in the following year (1969), we see a sharp increase in the event coefficient, reflecting women's increased 1L enrollment in 1968 translating into the following year. The difference between the 1969 and 1968 event coefficients [0.025 (0.008)] is slightly higher than the 1968 event coefficient in the 1L design [0.022 (0.005)], suggesting that differential attrition between men and women did not attenuate the policy impact. Appendix Figure 25b 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 25a 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.

[17] 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 the 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 required across program type. Accordingly, we opt to use persistence to 3L as our main proxy for graduation.

[18] Appendix Figure 26 replicates Figures 2a and 2b for women to confirm that there is no corresponding jump in attrition between years 1 and 2 for women.

4.1.1 Alternative Designs

In the Appendix, we present two alternative causal designs that depend on different assumptions than our core design, and we show that our results remain robust in both. 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, which would violate the Stable Unit Treatment Value Assignment (SUTVA) assumption. 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. We present difference-in-differences results in Appendix Section D.1, which rely on the assumption that the fraction of first-year enrollees who are women in full-time programs in law and medicine would have evolved in parallel in the absence of the change in deferment policy. The results, presented in Appendix Figure 27, are remarkably similar to our baseline estimates in Figure 3.

In addition, we consider a design that does not depend on a parallel trends assumption and only leverages comparisons across full-time programs (also removing the need for SUTVA to hold for part-time programs). As displayed in Figure 2a, 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 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 therefore should be more likely to enroll a higher share of women.

Accordingly, in Appendix Section D.2, we develop a second empirical approach that leverages plausibly unanticipated differences across programs in attrition between the first and second year of the program. Using program-specific data on attrition between years one and two, we develop a year t prediction of attrition in time $t+1$. Then, using observed attrition in year $t+1$, we interpret the residual as unanticipated attrition. Averaged across programs within year, this measure is far above 0 only in 1968, consistent with the draft pulling men out of law school after their first

year. We then regress the change in women's first-year enrollment between 1967 and 1968 on unanticipated attrition, finding that a program's 1968 attrition exceeding its predicted trend by one pre-period standard deviation (1962-1967) is associated with a 0.21 percentage point increase in women's 1L share, significant at the 1 percent level.

4.2 Heterogeneity

Having demonstrated robustness of our main result across a number of checks as well as two alternative designs, 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 its 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 homogeneous, reflecting a shared (discriminatory) norm. If, instead, we used a taste-based discrimination model, where the utility cost was higher at 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. In 1969, the American Bar Association began an annual survey of law libraries, which recorded the number of volumes held by each library. We collect this information from the Fall 1970 survey (Lewis 1971). 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 first-year students who 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 $\triangle Y_{ip}$, 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.[19] Our key causal parameter is the average treatment effect for full-time programs with log volumes V, given by

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

It is important to emphasize that we are *not* identifying the causal effect of an increase in institutional prestige.[20] 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, $Y_{i,p=\text{Full},t=1968}(0)$ denotes the outcome for institution i if it were untreated by the 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 ($\hat{\mathbb{E}}[\triangle Y_{ip}|D_p = 0]$), then using the first-difference transformation $\triangle Y_{ip} - \hat{\mathbb{E}}[\triangle Y_{ip}|D_p = 0]$ as the outcome of a flexible regression

[19] We utilize the log number of volumes as the number of volumes is right-skewed.

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

on V. Given that we have 136 observations, we opt for a parametric regression on a cubic in standardized log volumes, given by

$$\triangle Y_{ip} = \alpha + \mathbb{1}\{D_p = 1\} \sum_{k=0}^{3} \beta_k \tilde{V}^k + \varepsilon_{ip} \tag{6}$$

where $\tilde{V}$ denotes the standardized value of V.[21] Our comparison group remains all part-time programs where $D_p = 0$, 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 4a, 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 p.p.; 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. 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 Figure 4b, though this looks far more linear across our measure of selectivity, with substantially less precision among highly selective schools.

Another natural choice for measuring selectivity would be the average LSAT Score or Undergraduate GPA of entering students, but there are several issues with these metrics. For one, the LSAT was not nearly as central to the admissions process as it is today—the vast increase in demand for legal education in the late 1960s and 1970s resulted in higher use of the LSAT by admissions committees (Kidder 2003).[22] In addition, the first report of these statistics (that the authors are

[21] To avoid collinearity between polynomial terms, we standardize V for estimation.

[22] In fact, as Kidder (2003) notes, survey evidence from the 1960s indicates that admissions committees tended to

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 can only 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 28.[23] Our finding of a positive relationship between selectivity and the institution-specific ATT is strongly robust to all three of these measures. Estimates of other types of heterogeneity, including program and faculty size, are presented in Appendix Section F.2.

4.3 Who was the Marginal Student?

Given the heterogeneity results, before moving to employment outcomes, it is important to establish characteristics about the marginal student in the years following the policy change in 1968. We have established that women begin to have access to top law programs, but it is not clear if this access stems from changes in admission standards or applicant credentials (or both). In a 1972 survey to members of the Association of American Law Schools (AALS), Bysiewicz (1973) finds that the acceptance rate for law students fell from 44% in 1969-70 to 18% in 1972-73. However, over the same period, the acceptance rate for women fell from 55% to 43%, over twice the rate of men for the class entering in the 1972-73 academic year. Similarly, while applications rose approximately threefold over these three years, applications for women rose 14 times. One way this difference in acceptance rates could arise is if the credentials of women applying exceed those of men; this is a story where there were many talented women "waiting in the wings" now able to attend law schools. However, it could also be the case that law schools had lowered admissions standards for women, perhaps in an attempt to avoid violating anti-discrimination provisions (see Section 6.1). We draw on the existing statistical data on applicant credentials to distinguish between these two explanations.

But first, it is helpful to briefly describe how law school admissions committees operated in the 1960s and 1970s. Turnbull et al. (1972) conduct a descriptive study of law school admissions, surveying programs about their admissions process and exploring this process in detail for five schools. They find that most programs use some combination of LSAT score and Undergraduate GPA (UGPA) aggregated into an "admissions index" (Turnbull et al. 1972).[24] Applicants are then sorted into three groups: probable acceptances (high index values), probable rejections (low index values), and a marginal group that requires more committee discussion. In general, once an

weight undergraduate GPA more highly than LSAT Score (Lunneborg and Radford 1965).

[23] The composite score is given by $(1/2)(\text{LSAT}) + (1/2)(200 \times \text{UGPA})$, 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.

[24] Lunneborg and Radford (1965) report the weights on these components to construct this admissions index used by 88 law schools in an earlier survey.

applicant has been labelled as a probable acceptance or probable rejection, they rarely leave that category (Turnbull et al. 1972). Since these two indices are used widely in the application process, we focus on differences in LSAT Scores and UGPA by sex for applicants and matriculants.

Wightman (1974) provide detailed statistics on LSAT scores by sex produced separately by exam date between November 1969 and July 1973. The tabulations report averages over applicants (as opposed to exams), so that the reported score for repeat takers is the average of all of their scores at or before that date. Using the sex-specific mean and standard deviation of the LSAT distribution on each exam date, we plot the mean difference in scores by sex as well as a 95% confidence interval in Appendix Figure 29.[25] The data provide strong evidence that, among LSAT takers, women were positively selected relative to men throughout this time period: on every exam date, women's mean score exceeded that for men, with most statistical tests rejecting the hypothesis that there was no sex difference in exam scores.

Further, in Appendix Section C.2, we develop a simple model of admission into law school which allows us to use observable features of the 1972-73 incoming class to back out the expected LSAT score of the marginal applicant. In many years, we observe the number of LSAT takers, the number of enrollees, and the mean and standard deviation of LSAT scores, all separately by sex. This allows us to characterize the applicant pool, but we can say little about the students who end up attending law school. However, Bysiewicz (1973) reports the LSAT average for both women and all incoming students in Fall 1972;[26] as a result, under certain assumptions on the nature of selection into law school conditional on LSAT score, this allows us to back out the expected LSAT score of the marginal applicant. We estimate that this was 561.25 for women relative to 557.46 across all applicants, suggesting that even at the margin, women were positively selected relative to men.

Data on undergraduate GPA are not collected systematically for applicants and difficult to compare across universities and areas of study, but data on enrollees show that women have persistently higher grades, both in the early and mid-1970s (Bysiewicz 1973; White and Roth 1979). Taken together, this evidence suggests that the women who entered law school in the 1970s were highly qualified for study in legal education and not negatively selected relative to the applicant pool. With this in mind, we turn to career outcomes to understand how improved access to law schools, and elite law schools in particular, impacted women in law.

5 Translating Enrollment to Employment Outcomes

We have documented that the Vietnam draft changed the composition of those who attended law school. In this section we show that this propagated into the practicing bar.

[25] Since the mean and standard deviation are reported as whole numbers, this is a close approximation of, but not the true, sex differential and confidence interval values.

[26] This average is taken across the 81 schools that respond to the 1972 AALS Survey reported in Bysiewicz (1973).

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. Versions of these directories were first published in 1868. The directory was designed to provide yearly systematic listings of attorneys and law firms, organized in a "Geographic Bar Roster," so that readers could find local legal representation. The directories are organized 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. Because these profiles are purchased, this section disproportionately represents lawyers at private firms. We provide representative scans of the Geographic Bar Roster and Biographical Section in Appendix Figures 30 and 31, respectively.

Researchers have long recognized the value of *Martindale–Hubbell* directories for studying the legal profession. The American Bar Foundation relied on *Martindale–Hubbell* listings to construct the official counts of U.S. lawyers in its *Lawyer Statistical Reports* between 1951 and 2005 (American Bar Foundation 2005). More recently, empirical studies have begun to use small subsets of 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). However, our digitization efforts allowed us to dramatically expand and enrich the data that can be derived from these listings. 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 provides details on the digitization pipelines for both sections.

We digitize approximately 650,000 pages from 40 annual *Martindale–Hubbell* directories from 1960 to 2000 (excluding 1988, which we were unable to digitize), with most of our analysis drawing specifically on the Geographic Bar Roster. Contemporaneous writing indicates that more than 90% of practicing lawyers are listed in the Geographic Bar Roster during this time period (York and Hale 1973), a statistic supported by comparison to census surveys. To validate our dataset, we also digitize the 1940 and 1950 directories and compare our extraction to the full count microdata from the U.S. Census. In 1940, our total number of lawyers is 95% of the count in the 1940 Census (and within 1% of the 1% sample derived count), and we find similar ratios when comparing our counts to other decennial censuses through 1970. After this point, coverage declines modestly by 1990 with counts derived from *Martindale–Hubbell* digitization representing about 83% of the Census-derived statistic. Importantly, these data include 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, history of military service, position at a particular firm, and birthdate.[27]

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 name 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 Legal Practice Before the Vietnam War

Using our newly digitized data from the *Martindale–Hubbell* directories, we are able to confirm many of the patterns noted by those who described the organization of legal work at the time. Appendix Table 1 reports the number and share of men and women in the 1969 *Martindale–Hubbell* Directory listed at different types of employers. We are able to recover usable information on 302,532 lawyers in the United States. A large share, well more than a third, of lawyers practicing in 1969 were working as solo practitioners. About one-third of lawyers were working for law firms with at least two lawyers, and about 8% were partners at firms with at least 7 lawyers (a top 10% firm in terms of size). About 8.5% of lawyers were working as in-house counsel at various businesses. About 10% worked for the government in some fashion, and about 3% were judges.

As we have documented, a small percentage of the entering class prior to the Vietnam War were women. Of the 302,532 lawyers listed in 1969, we are able to infer from listed names that 9,121, or about 3.2% of those who we can reliably assign sex to, were women. Figure 5b plots women's share of newly barred lawyers between 1958 and 1979 – as this figure demonstrates, representation remained near 3% during the 1960s, only beginning to increase in the early to mid-1970s. Men and women were not distributed equally throughout the profession. As we show in Figure 5c, relative to their overall share of working lawyers, women in 1969 were more likely to be working for the federal government, as a solo or unclassified lawyer, or especially in legal aid. Women were less represented in roles that often came with higher pay or political power like working at the elite large law firms (especially as partners) and in the judiciary.

The pathway to the most sought-after positions generally ran through top programs, which functioned as a large barrier to entry for women. As Epstein (1993) observed,

> For women aspiring to law careers in New York City, restrictions or quotas on their

[27] 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). This arises often when a lawyer is a part of the bar in one city or state but practicing (and listed) in another.

> entrance meant that channels to the larger firms served by Columbia, Yale, and Harvard were cut off, and channels to the other large firms that comprised the country's principal legal community and to posts in government and leading corporations were also severely limited. (Epstein 1993, p. 51)

Reflecting this landscape, we find that where lawyers went to law school is highly predictive of where they end up working. At the largest (and considered most "elite") 10% of law firms, a vast majority of the attorneys graduated from the most elite schools as shown in Figure 5a, where we order schools by 1970 law-library holdings to reflect prestige.[28] Harvard alone accounts for 12.8% of these jobs. The 10, 25, and 50 highest-ranked schools in the figure account for 36.6%, 57.8%, and 75.8% of all recorded large-firm placements, respectively. Further reflecting this hierarchy, students attending part-time programs were less likely to be recruited to firms, especially the top firms, and more likely to engage in solo practice and in-house positions as shown in Figure 5d.[29]

Importantly, if it was the case that career outcomes were highly dependent on law school attended, our heterogeneity results from Section 4.2 suggest that increases in women's access to elite programs might translate to improved job prospects and promotion outcomes, which we test with this dataset.

5.3 The Draft's Footprint Among Men in the Bar

Earlier in the paper, to justify the assumption that full-time programs were impacted by the change in draft deferment rules, we demonstrated a spike in the attrition rate that corresponded with the policy timing. In this section, we show that this change also disrupted the legal careers of men entering the profession during this time period. To do so, we rely on the 1981 *Martindale–Hubbell* Biographical Section, which contains rich information on graduation years, birth dates, and reported military service, which allows us to better understand the impact of the draft on the impacted cohorts.

Figure 6 presents several descriptive figures for male graduates from the 1965–75 bar admission cohorts. We would expect the change in draft deferment rules in February 1968 to impact 1L students in 1967-68, as well as students entering law school in 1968-69, who would have been admitted to the bar in 1970 and 1971, respectively, had they graduated on time (in 3 years) and been admitted to the bar after graduating. To test for this, Figure 6a counts men graduating "on time," at ages 22–26, and compares these counts with a quadratic trend fitted outside 1970–71. The 1970 and 1971 cohorts fall 29.1% and 20.4% below the fitted trend, respectively, implying a

[28] For five schools missing from the 1970 survey, we use their 1969 holdings increased by 5.6%, the median growth among the 127 schools in our *Martindale–Hubbell* sample observed in both surveys. These schools are Howard University, the University of Buffalo, the University of Utah, Seton Hall University, and North Carolina College at Durham. We apply the full-precision growth factor before taking logs or ordering schools.

[29] Writing about law in the 1920s and 1930s, at a time when women's enrollment was limited primarily to part-time and unapproved programs, Chester (1985, pp. 7-8) notes that "instead of a single open and mobile profession, law in fact consists of several subprofessions, stratified largely on the basis of law school attended."

combined shortfall of approximately 2,000 on-time graduates within this sample. Following their service, these men could have decided not to return to law school or could have returned to their studies.[30] It appears as though many of these men missing from "on time" graduation would appear as older graduates in subsequent years (and appear with recorded military service), at least in an accounting sense. Figure 6b shows that the decline in on-time graduation coincides with an increase in graduation at ages 27–40 during 1972–74. This pattern appears consistent with delayed and non-permanent disruptions to legal training and entry to the profession.

We are able to look more concretely at the nature of these disruptions by studying self-reported military service within the lawyer biographies. Figure 6c plots the rate of lawyers reporting any military service by bar admission year for individuals who were admitted between ages 22-26 ("on time") as well as ages 27-40. Rates of service for both groups are very similar (though slightly higher for older graduates) before 1968. However, beginning in 1969, these series begin to decouple, with substantially higher rates of service among older graduates. In all, between 1968 and 1975, this gap rises from under 5 p.p. to over 15 p.p. Among the 7,029 older graduates in 1972–74, 1,577 report military service and 1,359 have a usable last reported service year. Of the latter group, 67.9% report their latest service year in 1969–71, as demonstrated in Figure 6d. This concentration is consistent with military service preceding later law-school graduation.

In 1969, the draft apparatus was replaced with a lottery system, which would have placed a different set of students at risk of induction. Following Angrist (1990), Appendix Section F.4 presents complementary within-cohort evidence of disruption to men's legal careers, where we leverage the fact that the *Martindale–Hubbell* Biographical Section reports date of birth. Using variation in draft risk from lottery assignment, we document that men assigned lower draft numbers were significantly more likely to serve in the military, more likely to graduate at later ages, and less likely to be partners at large firms, though as before, we do not find evidence that they were removed from the bar altogether.

5.4 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 Geographic Bar Roster 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 compare students at schools enrolling primarily full-time students (defined as having

[30] Accounts from the time give suggestive evidence that veterans who were drafted out of law school following their first year of study returned to the same program in their second year. See, for example, Williamson (2018) recounting the experience of UVA Law.

more than 98% of students enrolled in full-time programs) to students at schools enrolling only part-time students.[31]

Using the Geographic Bar Rosters from 1960-2000, we gather data on all cohorts admitted to the bar between 1956 and 1979, observing each cohort's outcomes at a fixed follow-up period. 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 the same career age. We consider two time horizons: 6 years out as well as 13 years out.[32] Given how few women are observed in our data, we present results for two-year cohort bins to improve precision.

We estimate an event study design of the form:

$$Y_{isc} = \sum_{\substack{\tau \in \mathcal{T} \\ \tau \neq 1968}} \alpha_\tau D_s \mathbb{1}\{c \in \{\tau, \tau + 1\}\} + \gamma_s + \delta_c + \varepsilon_{isc}, \tag{7}$$

where $\mathcal{T} = \{1956, 1958, \ldots, 1978\}$ indexes the first year of each two-year bar-admission cohort bin. The omitted reference bin includes both the 1968 and 1969 admission cohorts, and D_s indicates attendance at a full-time-only school, with part-time-only schools serving as the control group. Y_{isc} is the outcome for lawyer i from school s in bar-admission cohort c, measured at the outcome-specific follow-up horizon $K \in \{6, 13\}$. We control for school fixed effects, γ_s, and annual bar-admission cohort fixed effects, δ_c, and cluster standard errors by school. For career outcomes, we estimate event studies separately for women and men. Each coefficient α_τ measures the change in the FT-only minus PT-only outcome gap relative to the omitted 1968–69 cohorts, which precede the documented disruptions to graduation timing. The event studies retain all 1956–79 cohorts, including 1970–71.

We also estimate summary difference-in-differences estimates that compare the 1956–69 admission cohorts with the 1972–79 cohorts, excluding both 1970 and 1971. Formally, our specification is given by:

$$Y_{isc} = \alpha_{\text{DiD}} D_s \mathbb{1}\{c \geq 1972\} + \gamma_s + \delta_c + \varepsilon_{isc}, \tag{8}$$

where α_{DiD} is our parameter of interest, and individuals in the 1970 and 1971 bar admission cohorts are excluded. This exclusion separates the later cohorts from the transition cohorts in which the shortfall of graduating men is especially pronounced. For career outcomes, we also estimate a pooled triple difference-in-differences design that interacts full-time-only school attendance, admission in 1972–79, and an indicator for being female, using the same sample restriction for men and women. Formally, this design is given by:

$$Y_{iscg} = \alpha_{\text{DDD}} D_s \mathbb{1}\{c \geq 1972\} \mathbb{1}\{g = \text{Female}\} + \beta D_s \mathbb{1}\{c \geq 1972\} + \gamma_{sg} + \delta_{cg} + \varepsilon_{iscg}, \tag{9}$$

[31] Our results are similar if we require 100% instead.

[32] This is close to, but not exact: we were unable to digitize the 1988 volume, so we substitute 1987. This means the 1975 cohort uses a 12-year follow-up horizon.

where the 1970 and 1971 bar admission cohorts remain excluded. This specification also includes school-by-sex and annual cohort-by-sex fixed effects. The triple-interaction coefficient α_{DDD} measures the women-minus-men difference in the estimated draft effect and equals the difference between the separate-sex DiD estimates. As a robustness check, we add school-by-cohort fixed effects, comparing women and men from the same school and admission cohort.

Figure 7 presents the results. We begin by measuring the impact on representation in the medium run, where Y_{isc} is an indicator for individual i having a female name, measured six years after bar admission in year c. Figure 7a presents the event study results from equation (7), as well as a dashed line giving the summary difference-in-differences estimates for the 1972-79 cohorts. For the 1972–73 admission cohorts, women's share is 3.2 percentage points higher at FT-only schools relative to PT-only schools than for the 1968–69 reference cohorts ($p = 0.040$). The estimated increase reaches 8.3 percentage points for the 1976–77 cohorts, and the pooled difference-in-differences estimate is 6.5 percentage points ($p = 0.006$). This is a little over twice the size of our difference-in-differences estimate of the impact on enrollment share, implying that there is also greater attachment to the bar among women attending full-time programs relative to part-time programs across these treated cohorts. For our career outcomes below, we are able to use difference-in-differences results for men as a proxy for the secular change in outcome differences between full-time and part-time programs, but such a design is not feasible here. Accordingly, to rule this out, in Appendix Section F.3, we use our measures of institutional prestige to model this explicitly, finding that this matters little for our findings here.

Our remaining results focus on the impact on women's careers. First, we analyze job outcomes in the medium run, defining Y_{isc} as an indicator for associate status measured six years after bar admission (Figure 7b), denoting employment at a private firm, as well as an indicator for employment at a medium-to-large (at or above the 75th percentile in firm size) private firm (Figure 7c), considered a more prestigious placement. For each outcome, we present event study estimates from equation (7) for women and men, summary difference-in-differences estimates from equation (8) for women and men, as well as the triple differences estimate from equation (9). In Figure 7b, we find an increase in the probability of being listed as a firm associate six years after bar admission of 10.3 percentage points for women ($p = 0.002$) and 4.1 points for men ($p = 0.014$), yielding a women-minus-men effect of 6.3 points ($p = 0.007$). In Figure 7c, we find an increase in the probability of being employed at a medium-to-large firm six years after bar admission of 12.1 percentage points for women ($p < 0.001$) and 3.5 points for men ($p = 0.114$); the triple difference is 8.6 points ($p = 0.002$). Recall the evidence presented in Section 5.2 showing that, as of 1969, women were overrepresented among solo practitioners, federal lawyers, and in legal aid work. These results imply that increased admission to full-time programs (and more prestigious schools within this group) translated to improved access to private firms for impacted women.

We also examine long-run career outcomes, reflecting the likelihood of promotion within firms.

Our outcome of interest is large-firm partnership thirteen years after admission, using non-associate status at a national firm at or above the annual 90th size percentile as our measure of obtaining these elite jobs. We present the same set of statistics for this long-run outcome in Figure 7d. We find an increase in the probability of partnership at a large firm of 10.0 percentage points for women ($p < 0.001$) and 3.8 points for men ($p = 0.003$), yielding a women-minus-men effect of 6.2 points ($p = 0.007$). These estimates suggest that the gains associated with attendance at draft-exposed schools extend beyond initial placement to longer-run career outcomes. Table 2 presents results for a broader set of labor market outcomes, reporting difference-in-differences from equation (8) for women and men, as well as triple differences results from equation (9), both with and without school-by-cohort fixed effects, which serves as a demanding robustness check. The triple differences results are negative for federal government employment (-3.6 percentage points), state or local government employment (-1.8), in-house work (-4.3), and patent practice (-2.4), suggesting that women substituted away from these positions towards private firms. All in all, these results strongly support the hypothesis that improved access to full-time programs, and in particular prestigious full-time programs, had a marked impact on medium- and long-run career outcomes for women, reflecting the prominent role of law school pedigree in the legal labor market.

6 Reinforcing Policies

Throughout the paper, we have leveraged the fact that the 1968 change in draft deferment rules impacted full-time programs but not part-time programs. Comparing enrollment and career outcomes across these types of programs over time, we have found that this change led to a sharp, immediate increase in women's share of first-year enrollees in law school. After graduation, these changes led to improved access to the bar and jobs at (large) private firms for women, as well as a marked increase in the probability of making partner. However, returning to Figure 1a, it is apparent from the time series evidence that women's representation at both full-time and part-time programs increased drastically over the 1970s, a trend that this design cannot speak to directly.

Accordingly, we now turn to contextualizing our finding by evaluating the existing explanations in the literature, applied to women in law, to understand the extent to which they contributed to the gains women made during the 1970s. In doing so, we find much evidence that several other factors also spurred increases in women's representation in law schools, implying that these explanations in total can explain a significant portion of the secular increase in Figure 1a. That said, what is unique about the draft shock we study is that it occurs early, in 1968, and coincides with Harvard and Yale both departing from a realized enrollment share for women that closely tracks 5% (see Figure 2d). Thus, we believe the evidence is most consistent with pressure from draft exposure forcing elite programs to abandon informal quotas, creating room for complementary impacts from many other sources to drastically increase women's representation in law in the 1970s, though it is important to note that we are not able to provide direct causal evidence for this mechanism.

6.1 Title IX

Most accounts of the impact of anti-discrimination policy on women's enrollment in law schools focus on Title IX of the Education 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 law, as measured by the share of institutional revenue received from the federal government, after its passage in 1972.

We limit our sample to law schools that are identified in the Higher Education General Information Survey (HEGIS), which reports institution-level financial statistics. This sample is by no means representative, as we are only able to match 12 law schools that operate independently, listed in Appendix Table 2. That said, it is a particularly instructive sample, as several institutions receive no federal funding, and so would not face any penalty for violating Title IX, providing us with an untreated group. Further, among schools receiving federal funding, the maximum federal funding share is around 4%, suggesting that these are comparable law programs. Appendix Section E.1 presents a formal difference-in-differences design; here, we plot the raw fraction of first-year students who are women in the treated and untreated groups between 1960 and 1980 in Figure 8a, where the date of passage (June 1972) and date that compliance regulations were signed into law (May 1975) are marked by vertical dashed lines between 1971/1972 and 1974/1975, respectively. The trend in both groups is essentially identical between 1960 and 1974, including the sharp acceleration of women's representation that occurs around 1970, suggesting that there was little change in response to the law's implementation in 1973. However, these trends decouple sharply beginning in 1975; in particular, it seems like the growth in women's enrollment slows markedly in programs not subject to these regulations. Our difference-in-differences estimate suggests that Title IX lifted women's representation by 4.3 percentage points (significant at the 10% level).

6.2 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.[33]

In April 1971, the Professional Women's Caucus filed a complaint of EO 11246 violation against all law schools (Sandler 1973). However, before this point, WEAL had filed complaints against 41 institutions with law schools (including complaints filed in April 1971), beginning with the University of Maryland on January 31, 1970. Accordingly, we leverage variation in the timing at which an institution received its first complaint; more precisely, we compare law schools at an institution that received an "early" complaint in calendar year 1970, identified in Appendix Table 3, against those that received a complaint in April 1971 at the latest. Appendix Section E.2 presents a formal difference-in-differences analysis; here, we plot the raw fraction of first-year students who are women in programs treated early and later in Figure 8b. A sizable gap between these groups opens in the early 1970s, suggesting that women's share of enrollment accelerated more quickly in programs treated earlier. However, this gap had closed by the end of the decade, and our long-run difference-in-differences coefficient (covering 1975-1980) is not statistically different from zero. Taken together with our policy evaluation of Title IX, 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.

6.3 Access to Contraception

The other primary explanation in the literature is women's increased access to oral contraception ("the pill") in the late 1960s and early 1970s as states lowered the age of majority, allowing young, unmarried women to access contraceptive services. Goldin and Katz (2002) argue that the pill substantially lowered the cost of long, uninterruptible professional training for young women by vastly reducing uncertainty about the pregnancy consequences of sex. They estimate for each birth cohort the fraction of women who had used the pill before age 21 and the fraction of women who would have had access to the pill before age 21, finding a positive relationship between cohort-level usage and access to the pill and entry into professional occupations, including law. We replicate the design in Goldin and Katz (2002) leveraging birth cohort variation in access to the pill with our

[33] Appendix Figure 43 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.

Martindale–Hubbell dataset to estimate the impact of increased availability of the pill on women's representation in the bar.

Appendix Section E.3 presents details on the legal coding of state-level access to the pill for young women, our cohort-level measures of exposure, and dataset construction.[34] Appendix Table 4 presents a Martindale–Hubbell analogue of Goldin and Katz (2002)'s age-by-year analysis in Table 5 using all 40 editions from 1960 to 2000, excluding 1988.[35] Following Goldin and Katz (2002), we evaluate our regression coefficient at the mean level of pill access for birth cohorts in the mid-1950s (0.98), finding that diffusion of the pill led to an increase of 0.27 women lawyers per 100 BAs awarded to women, significant at the 1% level. This finding is robust using an alternative coding of young women's legal access to the pill in Bailey (2006). Further, we augment this specification to reflect the core mechanism in this paper, including a control for men's exposure to the Vietnam draft in each cohort. Mirroring our results, we find that for the 1945 birth cohort with peak draft risk (0.175), displacement of men led to an increase of 0.29 women lawyers per 100 BAs awarded to women. Further, in this specification, the impact of diffusion of the pill approximately doubled, to 0.53 women lawyers per 100 BAs awarded to women. This reflects a negative correlation between cohort exposure to the pill and the draft (conditional on age and edition fixed effects, this correlation is -0.73), likely driven by the draft winding down in the early 1970s as the pill becomes widely available, suggesting that the results in Goldin and Katz (2002) are an underestimate of the true impact of the pill on women's careers. That said, these are, in a sense, two sides of the same coin; as Goldin and Katz (2002, p. 739) note, "the war in Vietnam was the main reason for the passage of the Twenty-sixth Amendment and the changes in the age of majority that preceded and followed its ratification."

7 Conclusion

In this paper, we have focused primarily on the ways in which a sudden change in deferment rules for the Vietnam draft led to an abrupt increase in women's access to legal education. However, once women were able to enroll in law school in greater numbers, they began to effect institutional change, compounding this initial impact. There are numerous examples of this in the history of legal education.

At first, changes began in the law schools themselves. Ladies' Day at Harvard ended in 1968 when women in the incoming class, prepared for what they would encounter, embarrassed their instructor during his prepared questioning (Epstein 1993, p. 67). This year also marked the for-

[34] Goldin and Katz (2002) focus on legal changes between 1969 and 1974, though some states liberalized earlier in the 1960s (Bailey 2006; Bailey et al. 2011). This means that the diffusion of the pill coincides with our treatment window. Accordingly, in Appendix Section E.3.2, we show that our core design in Section 5 is not driven by this variation.

[35] Our replication does not include a control for the fraction of women living in a state with legalized abortion at age 18 as this coefficient was not significant in the original analysis.

mation of the first women's group in law school: the Women's Rights Committee at NYU Law. The Committee had an immediate impact on students at NYU, campaigning successfully for the prestigious Root-Tilden Scholarship, then awarded only to men, to open up to women in 1969 (Epstein 1993). The Committee served as a model for students across the country; by 1972, 70% of institutions had an active association of women law students (Bysiewicz 1973), with most accredited law schools reporting a women's group in their course catalog (Ginsburg 1978). Many of these groups played an active role in recruiting students, sending information and even speakers to undergraduate institutions, as well as corresponding with women who had applied or been accepted (Bysiewicz 1973). The Assistant Dean of Admissions at Harvard even commended the efforts of the school's Women's Law Association as women's share of enrollment neared 30% by the end of the 1970s (Harvard Law Record 1978).

But the impact of women law students was not limited to the classroom; rather, they played an active role in legal developments throughout the 1970s. In June 1971, the NYU Women's Rights Committee, together with women at Columbia, filed a complaint with the New York City Human Rights Commission, alleging discriminatory practices at ten NYC firms (Epstein 1993; The New York Times 1972). The basis for the complaint was the documented experiences of 13 women law students at NYU and Columbia during the placement process in the previous year; these students would have entered law school in the Fall of 1968. From these 13 incidents, two would be chosen to move forward as lawsuits, both of which led to settlements where the defendant law firms agreed to change their hiring practices (Epstein 1993; The New York Times 1976). Epstein (1993) reports that this litigation had a broader impact, as all of the 10 firms named in the initial complaint adopted company policies similar to the terms of the settlement. By then, legal action had spread beyond these initial complaints, with three additional law firms agreeing to settlements in response to allegations of discrimination against women in that same year (The New York Times 1976).

This paper provides new causal evidence on women's access to law schools beginning in 1968 that immediately precedes essentially all of these changes. Further, we find that the Vietnam War reshaped access to the legal profession in ways that extended far beyond entry into law school. 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. These changes also altered the composition of the practicing bar, as women entering full-time programs in the 1970s were more likely to be practicing and employed at private firms 6 years after bar admission. We also find evidence of improvements in promotion, as women graduating from full-time programs were more likely to make partner 13 years after bar admission.

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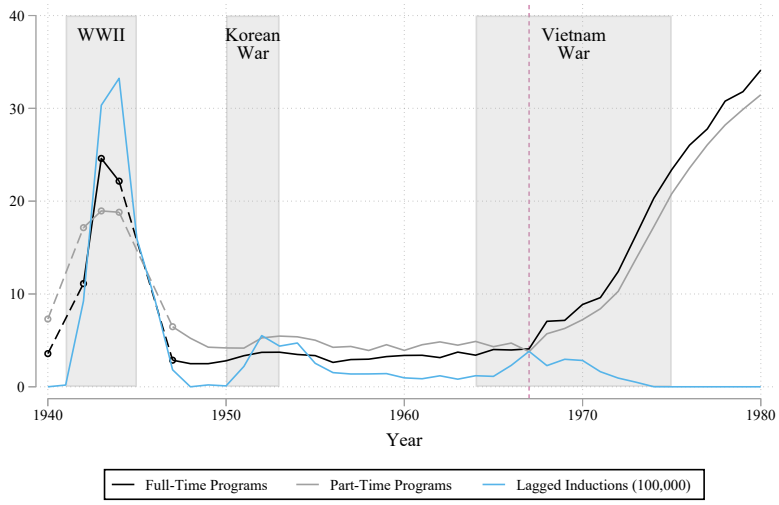
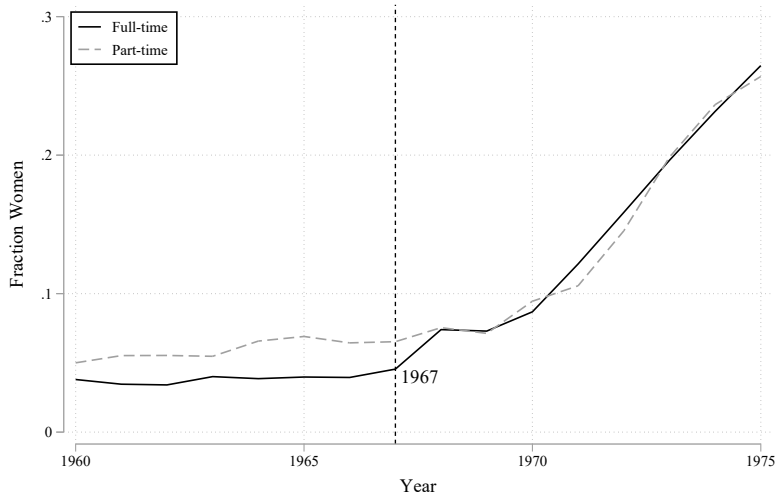
Figure 1: Enrollment Statistics



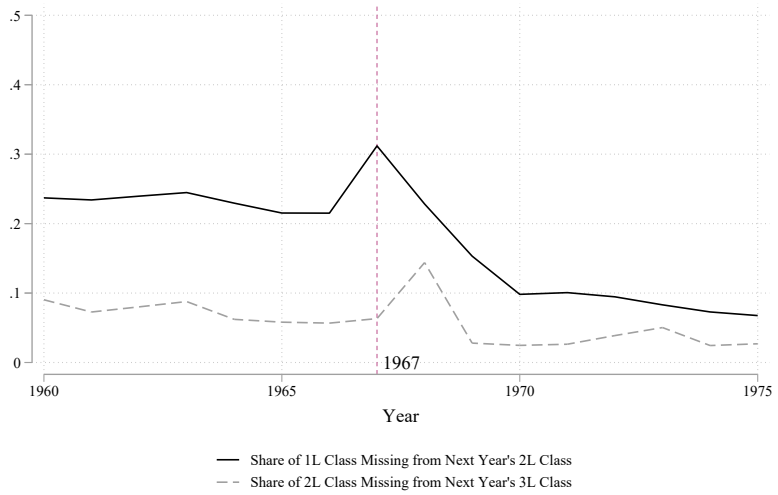
(a) Draft Inductions and Women's Representation in Law School



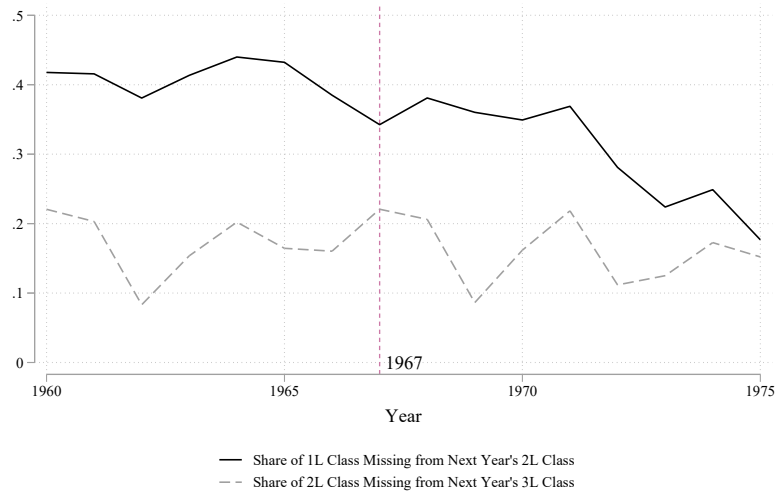
(b) Fraction Women in Full-Time and Part-Time Programs as a 1L

Notes. Figure 1a plots the percentage of all law students who are women between 1940 and 1980 in full-time and part-time programs 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. Data on law school enrollment from 1943 and 1947-1980 are collected from the *Review of Legal Education* in various years; data for 1940 and 1942 are collected from Gulliver (1943); data for 1944 are estimated from National Conference of Bar Examiners (1945) using schools running only full-time or part-time programs in the 1944 *Review of Legal Education*; we interpolate with the dashed line for years when data are missing. These statistics are calculated over all operating law schools that report to the RLE with the exception of 1940, 1942, and 1944, where they are calculated over all ABA approved law schools responding to the enrollment survey. Shaded bars plot U.S. involvement in WWII (1941-1945), the Korean War (1950-1953), and the Vietnam War (1964-1975), collected from `department.va.gov/americas-wars/`. Figure 1b plots the fraction of all first-year students who were women at U.S. law schools that reported to the *Review of Legal Education* from 1960 through 1975 separately for full-time and part-time programs. Data are collected from the *Review of Legal Education* in every year (raw enrollment levels across all program types are plotted in Appendix Figures 32a and 32b). In both plots, we mark the year before student deferments from the Vietnam draft were revoked (1967).

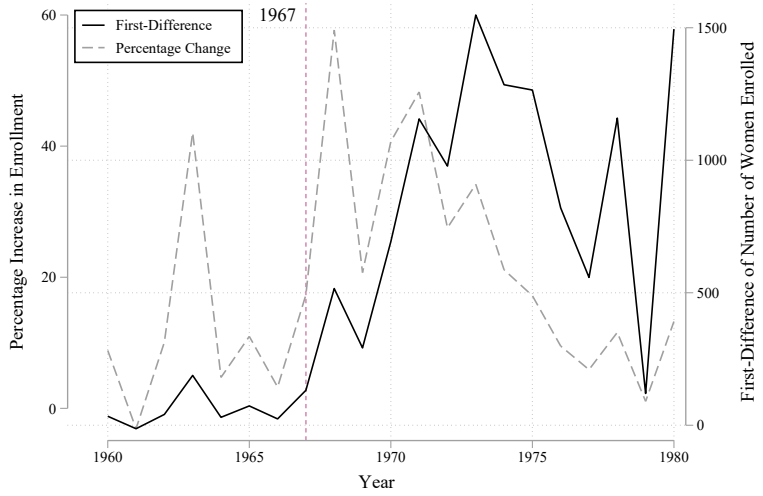
Figure 2: Descriptive Evidence on the Draft's Impact



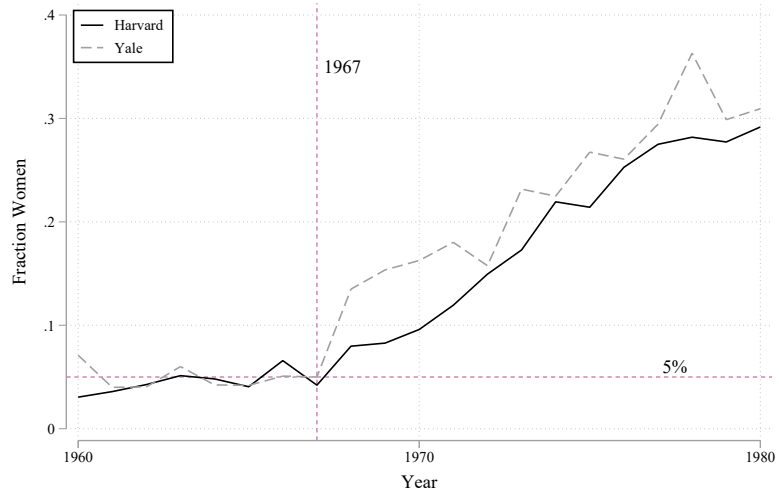
(a) Full-Time Programs



(b) Part-Time Programs



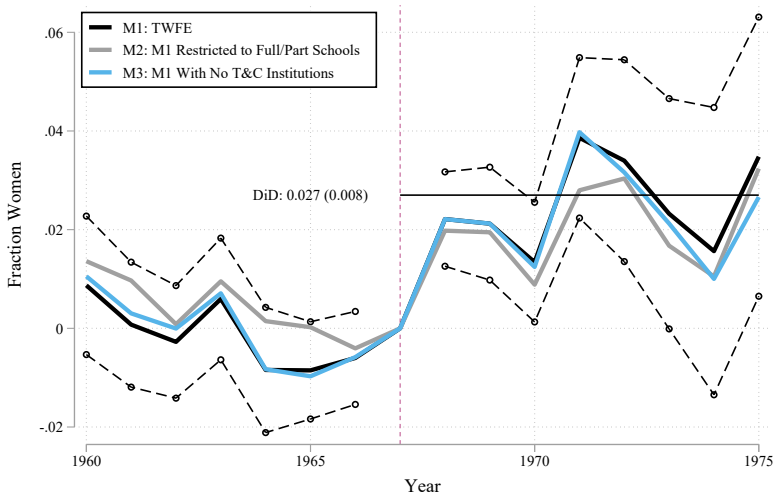
(c) First-Difference of Number of Women Enrolled



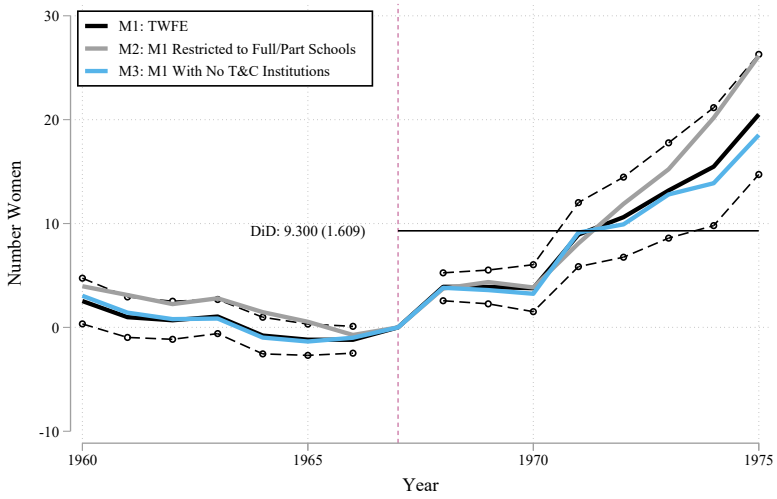
(d) Harvard/Yale 1L Enrollment

Notes. Figures 2a and 2b 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 2a plots these series for full-time programs. Figure 2b plots these series for part-time programs. Figure 2c plots the first-difference of the number of women enrolled in the first year of law school in both levels and as a percentage of last year's enrollment. This sample is restricted to ABA-approved programs for comparability of levels across time. Figure 2d plots the fraction of first-year students at Harvard and Yale who are women. In all plots, data are collected from the *Review of Legal Education* in various years, and we mark the year before student deferments from the Vietnam draft were revoked (1967).

Figure 3: Difference-in-Differences Results



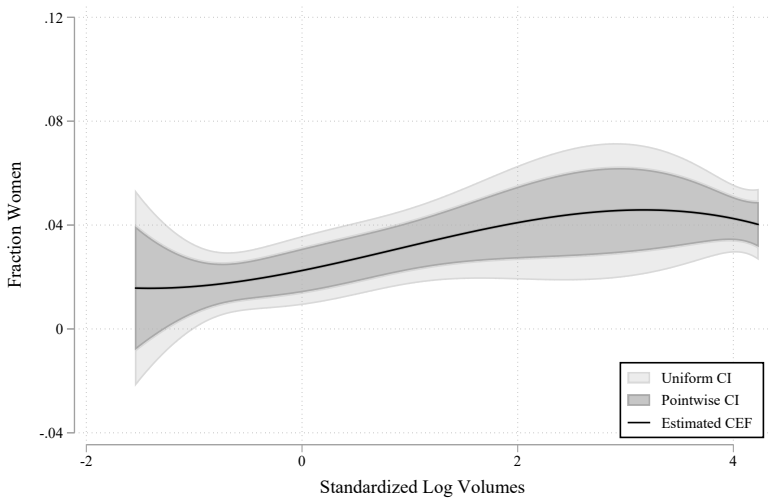
(a) Fraction Women



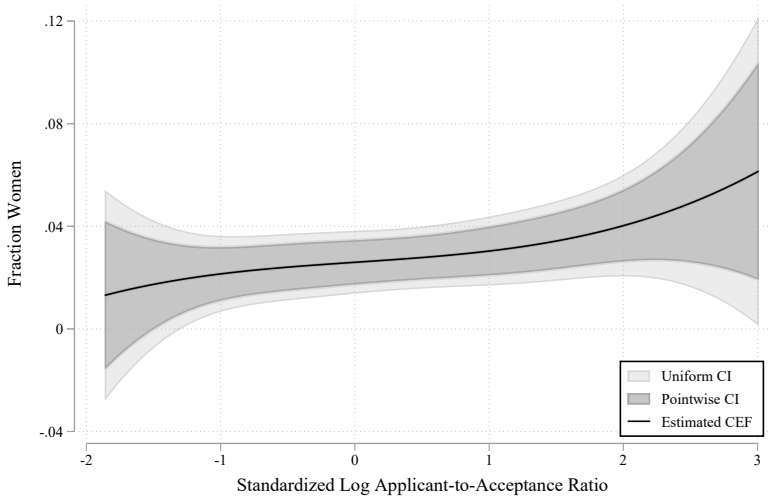
(b) Number Women

Notes. 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. Standard errors are clustered at the institution-program level.

Figure 4: Continuous Difference-in-Differences Results



(a) Log Volumes in Law Library

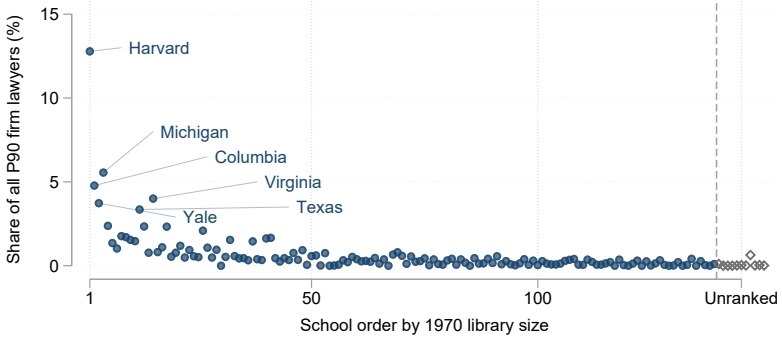


(b) Log Applications per Acceptance

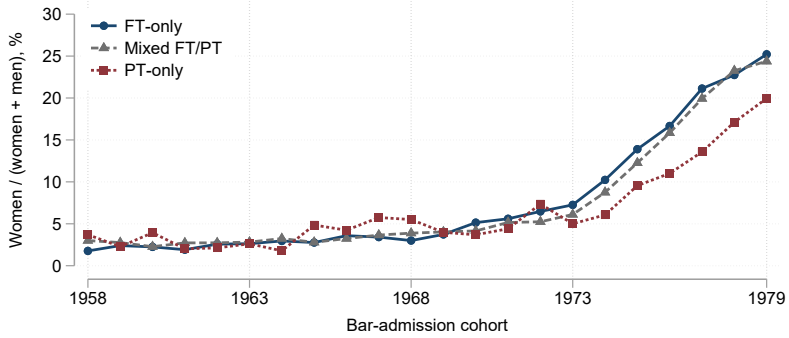
Notes. This figure plots estimates from equation (6), where the outcome is the change in the fraction of first-year students who are women between 1967 and 1968, the regression is weighted by total first-year enrollment in 1967, and standard errors are robust to heteroskedasticity. After estimating regression coefficients, we construct a grid of 500 points evenly spaced between the minimum and maximum values of $\tilde{V}$ and plot $\hat{Y}_{ip}$ for treated units ($D_p = 1$) at each of these 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 5: Women's Entry and Employment in the Legal Profession Across Law Schools

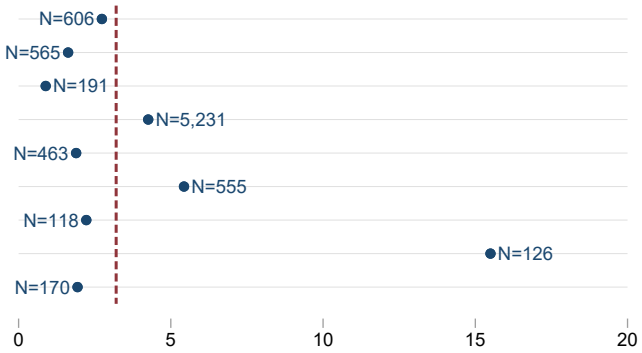
(a) School origins of large-firm lawyers



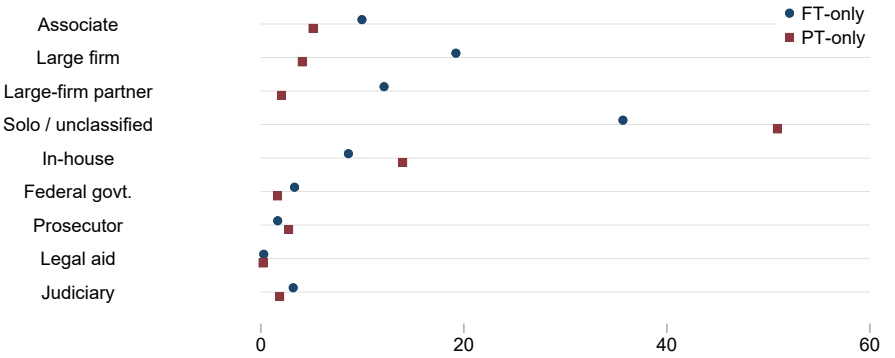
(b) Women's share by school program



(c) Women among occupation holders (%), 1969

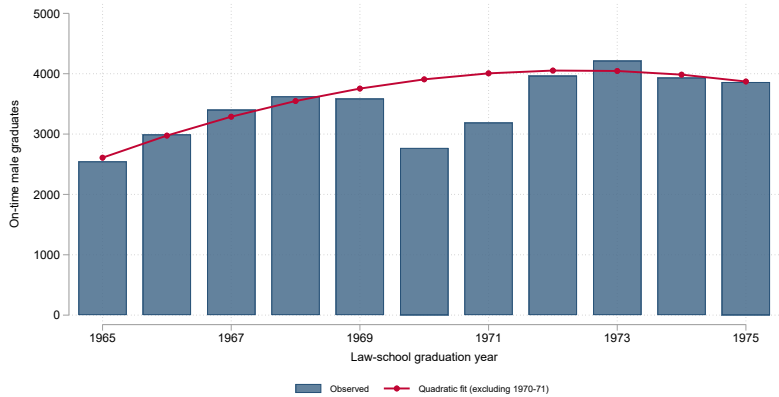


(d) Within-program occupation shares (%), 1969

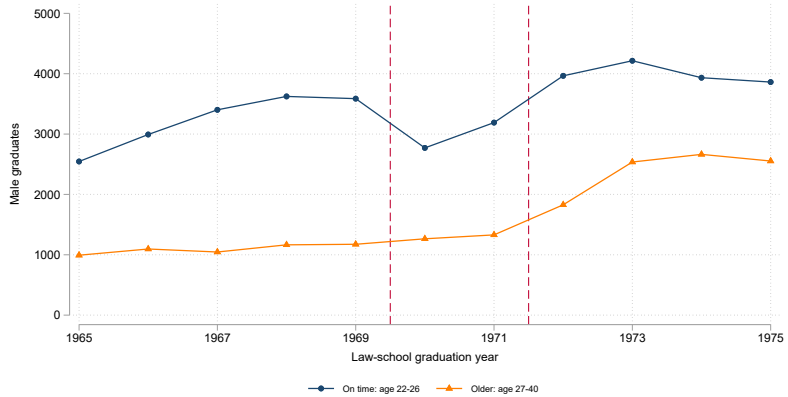


Notes. Source: *Martindale–Hubbell* Geographic Bar Roster. Panel (a) uses the 1963–67 editions and lawyers observed exactly two years after admission (1961–65 cohorts). Each point is a school's share of all 7,995 recorded jobs at national firms at or above the annual 90th size percentile (P90); the denominator includes missing or unmapped schools (4.23%). Schools are ordered by 1970 law-library volumes; five missing values use 1969 holdings multiplied by 1.05566, the median growth factor among 127 overlapping *Martindale–Hubbell* schools. Unranked schools appear last. Labels identify the six largest school contributions to P90 employment. Panel (b) shows women/(women+men) for the 1958–79 admission cohorts at exactly two years. Panels (c)–(d) use the full 1969 cross-section of 302,532 lawyers and the same nine overlapping categories. Panel (c) reports women's share among role holders of known sex and labels each dot with the number of women; the dashed line is women's share of all lawyers of known sex (3.21%). Panel (d) reports employment shares within FT-only and PT-only school groups, including unresolved sex and excluding unknown outcomes. School types describe offerings with any positive enrollment in 1966–72, not individual attendance modes. National size counts distinct linked lawyers across domestic offices; annual percentiles weight resolved private firms equally and include ties. Large-firm partner is the P90 non-associate proxy, including of counsel. Solo/unclassified includes residual listings without an identified affiliation.

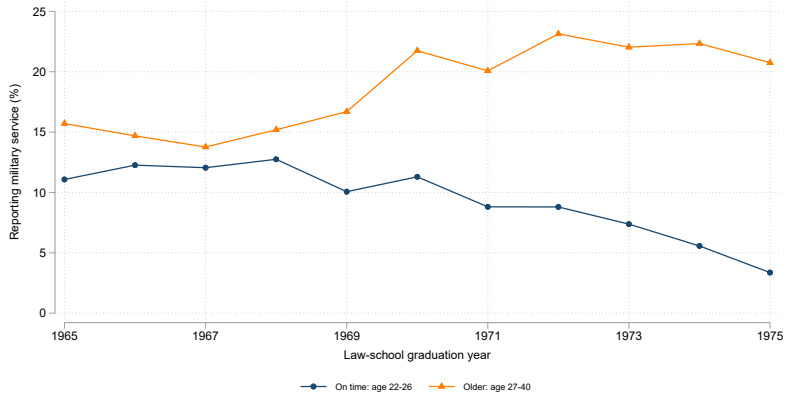
Figure 6: The Draft's Footprint Among Men Practicing Law



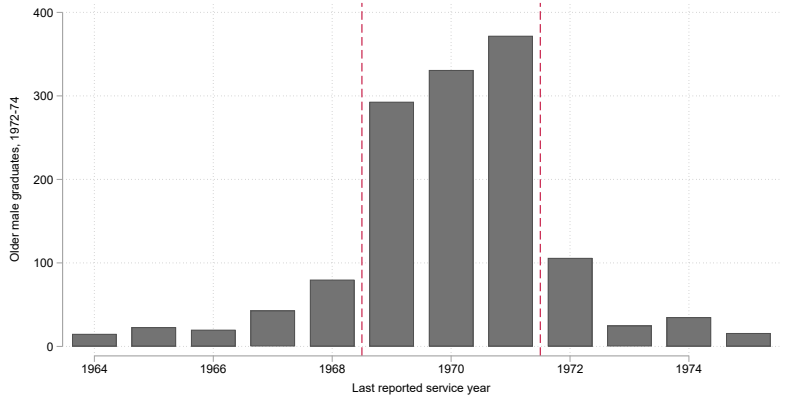
(a) On-Time Graduates



(b) Graduates by Age at Graduation



(c) Reported Military Service by Age Group

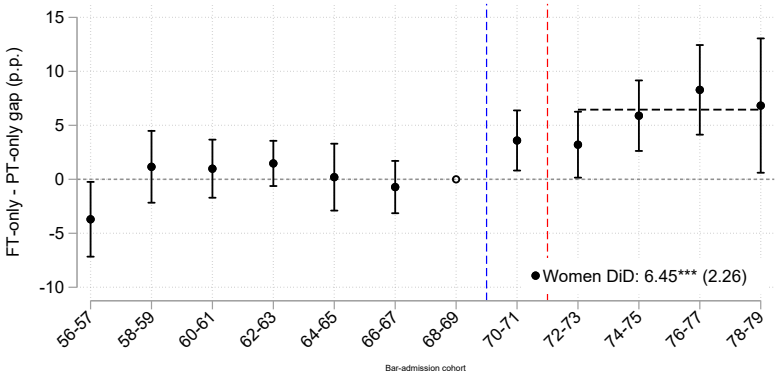


(d) Last Reported Service Year: Older 1972–74 Graduates

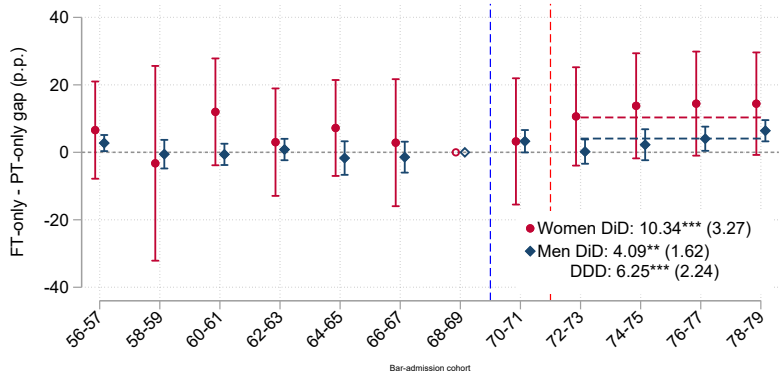
Notes. The sample consists of male lawyers in the domestic 1981 *Martindale–Hubbell* Biographical Section, with identified duplicate biographies consolidated. Age at graduation is graduation year minus birth year; on-time graduates are ages 22–26 and older graduates are ages 27–40. Panel (a) plots on-time counts and a quadratic fitted to the 1965–75 cohorts excluding 1970–71. The combined predicted shortfall is 1,952. Panel (b) plots the number of male graduates between 1965 and 1975 across both age groups. Panel (c) reports the percentage mentioning military service in each age group; absence of a report is coded zero. Panel (d) displays the last reported service year for 1,359 older 1972–74 graduates whose latest reported military year is in 1964–75; undated and explicitly ongoing service are excluded. Dashed lines mark the 1970 & 1971 graduation cohorts in panel (b) and service years between 1969 and 1971 in panel (d).

Figure 7: Draft Exposure and Women's Careers

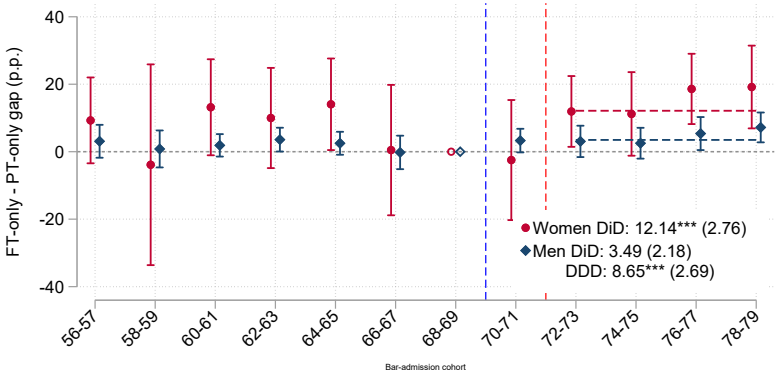
(a) Women's share, six years after admission



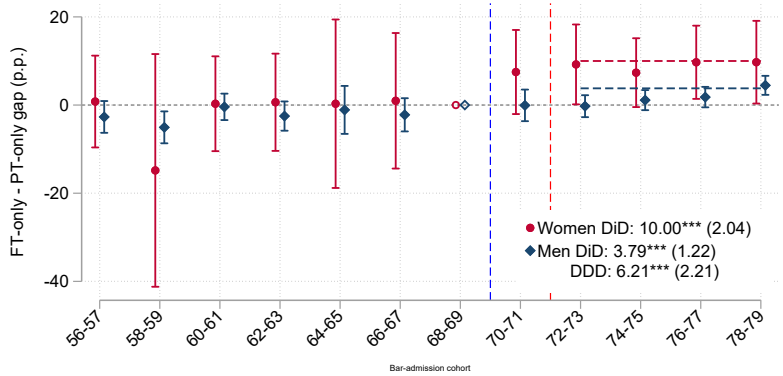
(b) Associate, six years after admission



(c) Medium-to-large firm employment (P75), six years after admission

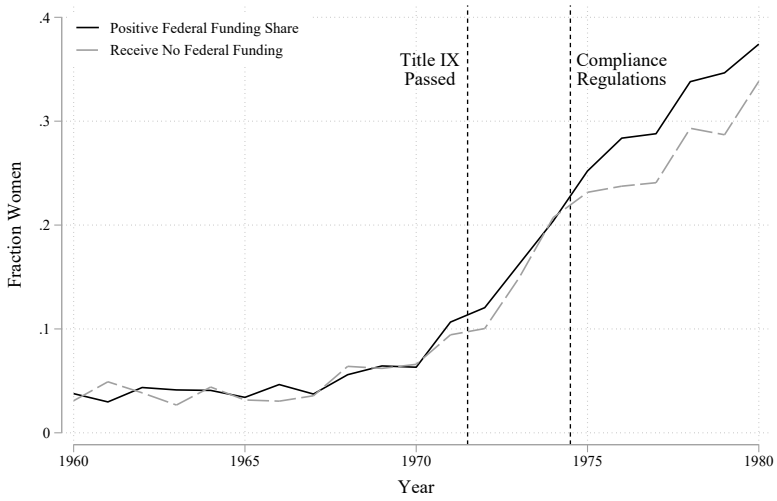


(d) Large-firm partner (P90), thirteen years after admission

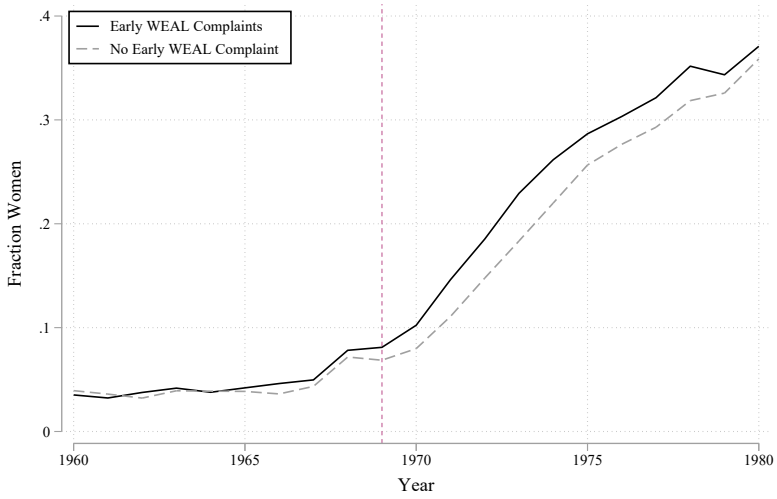


Notes. Each panel presents event study results from equation (7) comparing 48 FT-only with 17 PT-only schools for the 1956–79 admission cohorts. The panel title indicates the outcome variable and the time, relative to the year of bar admission, in which it is observed. For panel (a), the outcome is an indicator of having a female-coded name; for panel (b), the outcome is being listed as an associate; for panel (c), the outcome is being employed at a medium-to-large firm (at or above the 75th percentile of firm size); and for panel (d), the outcome is our proxy for large-firm partnership status: a non-associate at a firm at or above the annual 90th size percentile. The partnership panel uses +12 for the 1975 cohort because the 1988 edition is unavailable. Point estimates are FT-only minus PT-only gaps relative to the omitted 1968–69 bin, with school and annual-cohort fixed effects. Hollow markers identify this normalization; error bars are school-clustered 95% intervals. Horizontal lines and legends report the pooled difference-in-differences estimate from equation (8) comparing 1956–69 with 1972–79 admission cohorts, excluding 1970–71 from estimation; in panel (a), this is done across all observations, but in panels (b), (c), and (d), this is done separately for men and women. Observations after the red dashed line are included in the difference-in-differences estimate as post-treatment years; observations before the blue dashed line are included in the difference-in-differences estimate as pre-treatment years; and observations after the blue line but before the red line are excluded. Career panels (b,c,d) also report the women-minus-men triple difference-in-differences estimate using specification (9). Legend entries give school-clustered standard errors in parentheses across all designs; $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Figure 8: Anti-Discrimination Time Series



(a) Raw Enrollment By Federal Funds Receipt Status



(b) Raw Enrollment By Timing of WEAL Complaint

Notes. Figure 8a 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. This is limited to programs directly identifiable in the HEGIS data; see text for details on sample selection. The vertical dashed lines denote when Title IX was passed (June 1972; plotted between Fall 1971 and Fall 1972) and when compliance regulations were signed into law (May 1975; plotted between Fall 1974 and Fall 1975). Figure 8b 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.

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	
No T&C			X

Notes. This table presents difference-in-differences results from equations (2) and (4). Each section header denotes the class of interest for that section, which is either first-year students (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 class of interest, and the regression is weighted by the total number of students in the class 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 class 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. Standard errors are clustered at the institution-program level. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$

Table 2: Draft Exposure and Employment Outcomes: Difference-in-Differences Results

Outcome	Years	FT means (%): 1968–69 cohorts		Effects (p.p.)				
		Women	Men	All	Women	Men	Women minus men	Within-school DDD
Women	+6	—	—	+6.5*** (2.3)	—	—	—	—
Practice setting and specialty								
Solo practitioner / unclassified	+6	35.5	25.7	−5.7*** (1.8)	−5.9 (6.9)	−6.2*** (1.7)	+0.3 (6.4)	+6.1 (5.4)
In-house	+6	6.2	8.0	+4.1*** (0.9)	0.0 (1.7)	+4.3*** (0.8)	−4.3** (1.9)	−4.5** (1.8)
Academic	+13	5.5	2.0	−0.3* (0.2)	−1.7 (1.3)	−0.4** (0.2)	−1.3 (1.3)	−1.1 (1.3)
Patent	+6	0.3	0.6	+1.6** (0.7)	−0.7* (0.3)	+1.7** (0.7)	−2.4*** (0.8)	−2.5*** (0.7)
Public service								
Federal government	+6	10.2	4.8	+0.8 (0.5)	−3.0** (1.2)	+0.6 (0.4)	−3.6*** (1.2)	−3.3** (1.3)
State or local government	+6	7.0	4.4	+0.3 (1.1)	−1.4 (1.3)	+0.4 (1.1)	−1.8 (1.3)	−2.7 (1.7)
Judiciary	+13	2.7	2.0	+0.2 (0.2)	−3.1** (1.3)	+0.2 (0.2)	−3.3** (1.4)	−4.1** (1.5)
Prosecutor	+6	5.2	3.0	+0.1 (0.7)	−0.9 (0.9)	+0.2 (0.7)	−1.1 (1.0)	−2.0 (1.3)
Public defender	+6	0.3	0.6	−0.1 (0.2)	+0.4 (0.8)	−0.1 (0.2)	+0.5 (0.9)	+0.8 (0.9)
Legal aid	+6	4.5	1.6	+0.7*** (0.2)	+0.7 (1.1)	+0.5*** (0.2)	+0.2 (0.9)	+0.7 (1.0)
Firm roles and organization								
Associate	+6	18.6	25.1	+3.7** (1.7)	+10.3*** (3.3)	+4.1** (1.6)	+6.3*** (2.2)	+3.3 (2.6)
Of counsel	+6	0.0	0.1	−0.1 (0.1)	−0.4* (0.2)	0.0 (0.1)	−0.4 (0.3)	−0.3 (0.4)
Named partner (recorded marker)	+13	11.6	30.3	−3.3*** (1.0)	+3.3 (3.0)	−2.3** (0.9)	+5.6* (3.0)	+4.0* (2.4)
Large Firm partner	+13	11.8	25.2	+2.7** (1.1)	+10.0*** (2.0)	+3.8*** (1.2)	+6.2*** (2.2)	+4.3* (2.5)
Multi-office	+6	0.3	1.6	0.0 (0.2)	−0.2 (1.0)	+0.1 (0.3)	−0.3 (1.1)	−0.5 (0.9)
Ratings								
Any rating	+6	7.9	17.3	−8.6*** (2.0)	+1.0 (3.6)	−8.8*** (1.9)	+9.7*** (3.3)	+7.2** (3.6)
Top rating (a v)	+13	6.5	12.1	−0.7 (1.1)	−1.0 (1.4)	−0.2 (1.2)	−0.8 (1.3)	−1.6 (1.5)

Notes. Each row of this table denotes a set of results. The outcome for each row is given in column 1, where the outcome is an indicator for the employment status listed. Column 2 gives the number of years after bar admission when this outcome is measured. Follow-up is +13 for judiciary, academic, both partner measures, and top rating, and +6 otherwise; the 1975 cohort uses +12 for long-horizon outcomes because the 1988 edition is unavailable. Columns 3 & 4 give the baseline mean of the outcome at the specified follow-up period for the 1968 and 1969 cohorts. The final five columns give results from various empirical designs; these effects are given in percentage points, and school-clustered standard errors are in parentheses. The estimation sample compares the 1956–69 bar admission cohorts with the 1972–79 bar admission cohorts, excluding 1970–71, from 48 FT-only and 17 PT-only schools. Columns 5, 6, and 7, denoted All/Women/Men respectively, give difference-in-differences results from equation (8), where the column title denotes the population over which the equation was estimated over. Column 8, denoted "Women minus men," gives the triple difference-in-differences results from equation (9). The final column augments this specification by also including school-by-bar-admission-cohort fixed effects. Large-firm partner is a non-associate, including of counsel, at a national firm at or above the annual 90th size percentile; Solo/unclassified includes residual listings without an identified affiliation; roles can overlap. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix

A 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 *Geographic Bar Roster* 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 extraction procedures matched to the two layouts. We use the open-source PaddleOCR engine for the tightly formatted register lines and reconcile Mistral document-OCR and PaddleOCR extractions of the free-form biographical cards. This appendix describes the construction of each source.

A.1 The Geographic Bar Roster

Source material and OCR. We digitized the Geographic Bar Roster for 42 editions: 1940, 1950, and each year from 1960 through 2000 except 1988. 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.

Enrichment and repair. A sequence of deterministic post-passes then (i) cleans name and firm strings and splits names into parts; (ii) imputes sex from Social Security Administration first-name

[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.

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 mapped 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 for person-level outcomes. Firm size counts distinct lawyers across the firm's observed offices nationwide; a lawyer listed at multiple offices of the same firm contributes once to its national headcount. Large-firm thresholds use the distribution of national headcounts across firms in each edition.

A.2 The Biographical Section

OCR and structured extraction. The 1981 Biographical Section comprises 24,207 page images across seven volumes. Its firm cards contain free-form paragraphs for individual lawyers and often continue across pages. We extract the cards with Mistral's document-OCR model using a schema for firm names and addresses, lawyer names, birth dates, admissions, education, positions, and military service. Continuation flags and location checks join cards spanning several pages. A separate rule-based parser extracts these fields from PaddleOCR text. We reconcile the two extractions using page location, names, and compatible biographical information, requiring a unique match. Fields reported by only one extractor are retained; disagreements are flagged, with the PaddleOCR value retained when unresolved. Explicit position and military-service text from matched records are preserved for the measures described below.

Sample and person identification. The reconciled file contains 201,898 source records. We restrict attention to current biographies in the fifty states and the District of Columbia, excluding historical entries, nonperson records, fragments, and invalid name strings. We consolidate repeated listings using normalized full name and full birth date. When the full date is unavailable, the fallback key requires name, birth year, law-school code, and graduation year. A partial-date listing joins a full-date identity only when these fields identify a unique birth date. We additionally consolidate same-name, same-birth-year biographies that uniquely link to the same register lawyer. Conflicting full birth dates are treated as unknown for lottery assignment; conflicting graduation years or partner/associate classifications are unknown for the corresponding outcomes. Incomplete identities that cannot be resolved retain separate records, so this procedure need not identify every repeated biography. The resulting sample contains 169,339 identified people.

Demographics and graduation. Sex is assigned from Social Security Administration first-name

frequencies by birth decade, using the pooled distribution when the decade-specific distribution is unavailable. We classify a name as female when its female probability exceeds 0.9 and as male when that probability is below 0.1; generational suffixes such as Jr. and III also classify a record as male. Other names remain unclassified. The sample contains 146,924 men, 10,714 women, and 11,701 people whose sex is unclassified. Birth dates must be valid calendar dates and consistent with any separately reported birth year. To address retroactively reissued J.D. degrees, a graduation year more than one year after first bar admission is replaced with an eligible LL.B. year from the education text, or with the first admission year when no such year is available. Ambiguous or conflicting graduation years remain missing. Graduation age is graduation year minus birth year; we retain ages 20–45, define on-time graduation as ages 22–26, and define older graduates as ages 27–40. Unknown graduation ages remain missing. In the analysis sample, 94.6 percent have a birth year, 89.8 percent a full birth date, 95.3 percent an admission year, and 93.4 percent a graduation year after these rules.

Military service. The service indicator records a positive report in the biography; absence of a report is coded zero and does not establish absence of service. We expand abbreviated date ranges, so that, for example, "1967–70" ends in 1970, and record the latest reported military year. Explicitly ongoing spells have no known last year. This variable measures the latest year mentioned in the service history, which need not be the end of active duty. Positive reports are retained across repeated listings. In total, 16,721 people, or 9.9 percent of the analysis sample, report military service.

Firm positions and national firm size. Partner/member and associate designations require explicit evidence from card section headings or the lawyer's entry. A default member classification without such evidence does not establish partnership. The conditional partner outcome compares partners and members with associates; other or unknown roles are excluded. We link biographies to the 1981 Geographic Bar Roster using exact normalized full name, birth year, and state, rejecting pairs with conflicting observed admission years or school codes and requiring a unique compatible register identity. This links 144,065 people, or 85.1 percent of the sample. The linked register record supplies the person's firm and its national headcount, measured across all observed offices. The large-firm partnership outcome restricts the conditional role sample to firms at or above the 1981 90th percentile: eight distinct lawyers, with ties included. The cutoff is calculated across all 35,786 private firms in the register, rather than only firms purchasing biographies. Unmatched people and unknown firm sizes remain missing for this outcome.

Analysis samples. The graduation-cohort figures use 56,322 men reporting law-school graduation in 1965–75. The lottery analysis requires a valid complete birth date and classified sex; 61,100 people born in 1944–53 meet these conditions, including 40,119 men born in 1944–50. Sample sizes vary by outcome because missing graduation information, explicit role requirements, and firm linkage restrict different measures. These analyses describe people appearing in the 1981 biographies.

Inclusion in a purchased biography and subsequent employment may both depend on earlier draft exposure, so conditional comparisons do not by themselves identify effects for all lawyers or all men exposed to the draft.

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 34 demonstrates this graphically. Appendix Figure 34a 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 34b 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 less credentialed 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 34c. 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 34d. 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 34e, and consequently the marginal benefit curve in Appendix Figure 34f 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 out 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 more colorful descriptions 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.'" Congressman John Erlenborn added that removing graduate deferments would lead to "a policy of admitting women, the halt, and the lame" (Strebeigh 2009).

C LSAC Data

C.1 Responses to Policy Announcement

To understand potential demand-side responses to policy announcements, we digitize summary statistics on differences by sex 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 sex.[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 sex 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, where all statistics are rounded to the nearest whole number.

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 5a and for men in Appendix Figure 35a. 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 passage of the Military Selective Service Act of 1967 on June 30, 1967, where Congress instructed the executive to curtail graduate

[6] At the beginning of the report, the authors note that LSAC requested that sex-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 do the 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 for this analysis, we only use data from the first report.

[9] The 1968-69 Pre-Law Handbook indicates all schools apart from Brooklyn, Chicago, Creighton, Fordham, and Howard required the exam.

deferments, and the execution of this policy change with the removal of deferments for law students on February 17, 1968.[10] It is clear from the data that average scores vary cyclically both across and throughout the application cycle, and it is 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 sex 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 sex, 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 5b and for men in Appendix Figure 35b, 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 have 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.

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

[11] It is likely that LSAT scores themselves are approximately normally distributed – we provide evidence on this in Appendix Section C.2.2. Regardless, our tests here do not depend on this assumption.

To induce normality, we use a log transformation for count variables, and a logit transformation 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 sex 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 substantive assumptions as the number and fraction of women taking the LSAT are 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

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

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 follows:

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

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

[12] Well known variance stabilizing transformations include the square root for a Poisson random variable and $\sqrt{(n)}\sin^{-1}\sqrt{\hat{p}}$ 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 39 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.

Here, a_n and b_n are finite sample corrections to ensure these estimators are unbiased,[14] and $k = \binom{\lfloor n/2 \rfloor + 1}{2}$ 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 7b) and men (Appendix Figure 36b) taking the LSAT, the fraction of individuals taking the LSAT who are women (Appendix Figure 9b), the number of women (Appendix Figure 6b) and men (Appendix Figure 37b) registered to take the LSAT, the fraction of individuals registered to take the LSAT who are women (Appendix Figure 8b), and the fraction of women (Appendix Figure 10b) and men (Appendix Figure 38b) 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 8) and take (Appendix Figure 9) 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 7) and men (Appendix Figure 36) taking the LSAT in November of 1969 such that the fraction of test takers who are women (Appendix Figure 9) is unaffected. These patterns are also borne out in our registration statistics, but in any case, are occurring far too late to influence our 1968 event coefficient.

C.2 Backing out the Marginal Applicant

We are able to observe the LSAT distribution of applicants, the share of applicants that enroll, and the LSAT average of enrollees. If it were the case that all applicants that score above some threshold LSAT score enrolled, these would be mechanically related, but in practice many students who score well on the LSAT do not enroll, lowering the average of enrolled students relative to what this would predict. Accordingly, we construct a simple selection model to back out the expected LSAT score of the marginal applicant.

Let μ and σ denote the observed mean and standard deviation of the LSAT distribution. For simplicity, we consider the standardized LSAT score, given by $Z = (\text{LSAT} - \mu)/\sigma$. We assume that $\text{LSAT} \sim \mathcal{N}(\mu, \sigma^2)$ so that Z is standard normal; we present empirical evidence that the distribution of LSAT scores is approximately normally distributed at the end of this section. Law schools observe both Z as well as some other application component ε that is unobserved to the econometrician.

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

We assume that this information is standardized as well, so that $\mathbb{E}[\varepsilon] = 0$ and $\mathbb{V}[\varepsilon] = 1$, and we assume without loss of generality that ε is uncorrelated with Z, in that law schools use information about all correlates of the LSAT score. Law schools apply some set of weights to construct an admissions index of the form:

$$V^* = \alpha Z + (1-\alpha)\varepsilon$$

Notice that scaling both of the weights by some constant, positive value will not affect the ordering over candidates. So, we scale each weight by the standard deviation of V^*, $\sqrt{\alpha^2 + (1-\alpha)^2}$, allowing us to rewrite this index as

$$V = \rho Z + \left(\sqrt{1-\rho^2}\right)\varepsilon$$

where $\rho = \alpha/\sqrt{\alpha^2 + (1-\alpha)^2}$. Under the assumption that ε is standard normal, it follows that $(Z\,\varepsilon)'$ is a bivariate standard normal. Then, we can write $(Z\,V)'$ as

$$\begin{pmatrix} Z \\ V \end{pmatrix} = \begin{pmatrix} 1 & 0 \\ \rho & \sqrt{1-\rho^2} \end{pmatrix} \begin{pmatrix} Z \\ \varepsilon \end{pmatrix}$$

and it follows immediately that $(Z\,V)'$ is jointly normal with mean zero and variance-covariance matrix given by

$$\begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}$$

We assume that law schools use some threshold rule for admission so that applicants with $V > c$ are admitted. Then, it must be the case that the enrollment share of applicants p is equal to $\mathbb{P}(V > c)$. Since the marginal distribution of V is standard normal, c is identified by $c = \Phi^{-1}(1-\hat{p})$, where $\hat{p}$ denotes the observed enrollment share of applicants. We also know the average LSAT score of admitted applicants, which allows us to calculate $\hat{\mathbb{E}}[Z|V > c] = (\hat{\mathbb{E}}[\text{LSAT}|V > c] - \hat{\mu})/\hat{\sigma}$; by standard results (Heckman 1976), this pins down ρ, as

$$\mathbb{E}[Z|V > c] = \rho\lambda(c)$$

where $\lambda(\cdot)$ is the inverse Mills ratio. Now, we can estimate the expected LSAT score of the marginal applicant, which follows directly from the formula for the conditional expectation of a bivariate standard normal:

$$\mathbb{E}[Z|V = c] = \rho c$$

Transforming this to be in terms of observables,

$$\mathbb{E}[LSAT|V = c] = \mu + \rho c\sigma$$

C.2.1 Empirical Estimates

For the entering class in 1972-73, we are able to obtain these moments from a couple of different data sources:

- $\hat{p}$: Share of LSAT takers who enroll
  - Number of LSAT takers in 1971-72 (Wightman 1974)
  - First-year students at approved programs in Fall 1972 (1972 *Review of Legal Education*)
- $\hat{\mu}, \hat{\sigma}$: Mean & standard deviation of LSAT takers (Wightman 1974)
- $\hat{\mathbb{E}}[\text{LSAT}|V > c]$: LSAT average of enrolled students (Bysiewicz 1973)

C.2.2 Are LSAT Scores Approximately Normally Distributed?

To understand if a normal distribution is a good approximation to the LSAT score distribution, we use data reported from the initial calibration of the exam by Swineford (1949). For each administration we study in the paper, we are able to observe the mean and standard deviation of test takers. However, for a set of administrations in 1948, Swineford (1949) reports these statistics alongside the number of students who scored within each 25-point bin. In Appendix Figure 40, we plot the histogram of binned scores against a normal distribution with the mean and standard deviation of scores for each administration. Visually, these plots suggest the normal distribution calibrated to the exam mean and standard deviation is a very reasonable approximation of the score distribution. Further, many of the observed deviations are likely due to a small number of test takers: for the May 8 administration, which has around three times as many students as the next largest administration on November 13 (2940 relative to 952), we observe the best distributional fit.

D Alternative Causal Designs

D.1 Medical Schools as a Comparison Group

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 -0.09 percentage points, compared to a 9.6 percentage point increase at full-time programs in law schools. We obtain institution-by-year data on the fraction of new medical students who are women from Helgerman (2025).[15] 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 1966, this is new entrants in the fall.[16] These data sources are compiled to 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

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

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 model 2 and model 3 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:

$$Y_{idt} = \alpha_{\text{DiD}} D_d \mathbb{1}(t > 1967) + \gamma_{id} + \delta_t + \varepsilon_{idt} \tag{D.2}$$

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

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

The results from this design are presented in Appendix Figure 27. 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:

$$Y_{idt} = \sum_{\tau=1960, \tau \neq 1967}^{\tau=1970} \alpha_\tau D_d \mathbb{1}(t=\tau) + \beta E_{idt} + \gamma_{id} + \delta_t + \varepsilon_{idt} \tag{D.3}$$

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 (D.1), and we summarize the estimates from this design with a similar summary difference-in-difference specification:

$$Y_{idt} = \alpha_{\text{DiD}} D_d \mathbb{1}(t > 1967) + \beta E_{idt} + \gamma_{id} + \delta_t + \varepsilon_{idt} \tag{D.4}$$

The results from this exercise are plotted in Appendix Figure 27b. 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.

D.2 Cross-sectional Measured Attrition

For each program p 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.[17] Letting a_{ipt} 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:

$$\mathbb{E}[a_{i,p,t+1} | \mathcal{A}_{i,p,t}] = \hat{\alpha}_{i,p} + \hat{\beta}_{i,p}(t+1) \tag{D.5}$$

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

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

Under this model, the unexpected portion of attrition is defined as $S_{i,p,t+1} = a_{i,p,t+1} - \mathbb{E}[a_{i,p,t+1}|\mathcal{A}_{i,p,t}]$, so that $S_{i,p,t+1} > 0$ 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 $S_{i,p,t+1}$ by $\hat{\sigma}_{i,p,t}$, the standard deviation of within-program residuals over the pre-period estimation window, to construct $\tilde{S}_{i,p,t+1} = \frac{S_{i,p,t+1}}{\hat{\sigma}_{i,p,t}}$. The scaled "surprise" $\tilde{S}_{i,p,t+1}$ 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 impact. To test that the forecasting model in equation (D.5) is correct in expectation, we can test whether the standardized residuals $\tilde{S}_{i,p,t+1}$ are mean 0 between 1963 and 1967: we are unable to reject the null that $\mathbb{E}_t[\tilde{S}_{i,p,t+1}] = 0$ 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. Appendix Figure 41a plots the average value of $\tilde{S}_{i,p,t+1}$ by year and program type between 1963 and 1975. We find 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 1962 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 the *Pre-Law Handbook*. Appendix Figure 41b shows that in 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 45% and 40% of observed attrition in each year, respectively. 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.

Appendix Figure 41c plots the women's share of first-year enrollment for full-time programs in the top and bottom quartiles of the 1968 attrition surprise $\tilde{S}_{i,p,1968}$. 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-dependent model in Appendix Figure 34 predicts: the shock permanently raised exposed schools' baseline share of women, so the 1968 response did not revert once draft pressure ended.

Appendix Figure 41d looks more closely at the distribution of unanticipated attrition in 1968 across schools, plotted against our preferred measure of institutional prestige. 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 Section 4.2.

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:

$$\Delta Y_{ipt} = \alpha + \beta \, \tilde{S}_{i,p,t} + \mathbf{X}_i' \gamma + \varepsilon_{i,p,t} \tag{D.6}$$

Our outcome of interest is $\Delta Y_{i,p,t} \equiv Y_{i,p,t} - Y_{i,p,t-1}$, which gives the one-year change in the share of women in the first-year (1L) class at institution i in program p. 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 (D.6) in 1968; our estimates $\hat{\beta}$ are reported in Appendix Table 6a. 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 exceeding 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 $\tilde{S}_{i,p,t}$ at the 1st and 99th percentiles 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 similar 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 evidenced by Appendix Figure 41a. It is likely this lack of variation that explains why our estimate in column 6 is statistically indistinguishable from zero.

Appendix Table 6b probes whether the 1968 result reflects a pre-existing relationship between attrition surprises and women's 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 -0.12 p.p. to 0.04 p.p., 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 women's 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.

E Reinforcing Mechanisms Causal Designs

E.1 Title IX

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.[18] We match law schools appearing in the 1967 *Review of Legal Education* to the institution they are affiliated with 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 2.

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 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:

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

Here, Y_{ipt} is the fraction of first-year law students at institution i in program p 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 signed into law in May 1975 (Rim 2021).[19] We use a standard TWFE design with institution-program (γ_{ip}) 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

[18] 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.

[19] Funding is measured in advance of January 1970, when the Women's Equity Action League first drew attention to the fact that universities receiving federal contracts should be subject to EO 11246 (Helgerman 2025).

difference-in-differences estimator of the form:

$$Y_{ipt} = \alpha_{\text{DiD}} \mathbb{1}(d_{i,1968} > 0)\mathbb{1}(t \geq 1975) + \gamma_{ip} + \delta_t + \varepsilon_{ipt} \tag{E.2}$$

Our results are presented in Appendix Figure 42a. 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 no change in the difference in women's representation across exposed and unexposed schools.

E.2 Earlier Activist Pressure

The complaints made by WEAL 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 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 1971 (U.S. Congress 1971). We assume this report, filed by WEAL, is up to date as of the beginning of the hearing, which begins on May 26, 1971 – the latest complaint reported is on May 24, 1971. We identify 41 complaints filed against institutions that include law schools, which are reported in Appendix Table 3. In addition, in April 1971, the Professional Women's Caucus filed a complaint against all law schools (Sandler 1973).[20] 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

[20]This is included in Appendix Table 3, though it was not included in WEAL's report to Congress.

as treated early if it receives a complaint from WEAL (or an allied organization) during calendar year 1970. We use this treatment definition as part of an event study design of the form:

$$Y_{it} = \sum_{\tau=1960, \tau \neq 1969}^{\tau=1980} \alpha_\tau \text{Early}_i \mathbb{1}(t = \tau) + \gamma_i + \delta_t + \varepsilon_{it} \tag{E.3}$$

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 p. 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 Appendix Figure 42b. 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:

$$Y_{it} = \alpha^{\text{SR}}_{\text{DiD}} \text{Early}_i \mathbb{1}(t \in [1970, 1974]) + \alpha^{\text{LR}}_{\text{DiD}} \text{Early}_i \mathbb{1}(t \in [1975, 1980]) + \gamma_i + \delta_t + \varepsilon_{it} \tag{E.4}$$

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.

E.3 Contraceptive Access

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

E.3.1 Data

Legal access codings. Between 1960 and 1976, legal access to oral contraception expanded for unmarried women under 21 through changes in the age of majority, mature-minor doctrines, family-

planning statutes, and court decisions. From Table 2 and the accompanying discussion in Goldin and Katz (2002), we construct two measures. The *GK strict* coding dates access under a nonrestrictive rule that allowed 16-year-olds to obtain contraception without parental consent. For the *GK broad* coding, we use the first year in which Goldin and Katz (2002) Table 2 reports that someone under 21 could obtain the pill without parental consent. The *Bailey* coding uses the independently collected dates in Table I of Bailey (2006), which records the first year unmarried, childless women under 21 could legally obtain the pill without parental consent.

E.3.2 Employment Result Robustness to Diffusion of the Pill

Two tests address how these changes might interact with our estimates on the impact of the Vietnam draft on women's representation in the bar, estimated using equation (8), which we show in Appendix Table 7. First, exploiting the staggered state liberalization of contraceptive access for unmarried women under age 21, we interact the full-time × post term with the pill-access regime of each school's state in Panel B: 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, and the triple differences term is not statistically distinguishable from 0, suggesting a similar impact in states treated earlier. Second, because state laws cannot capture nationally diffusing access, in Panel A 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 changes little across both legal codings, and the pill interaction is a precise zero.

E.3.3 A Martindale–Hubbell Analogue of Goldin and Katz's (2002) Table 5

The career analysis in Goldin and Katz (2002) relates professional employment among U.S.-born college-graduate women aged 30–49 to cohort-level pill access using age-by-year cells from the 1970, 1980, and 1990 censuses. We apply an age-by-edition design to the expanded Martindale–Hubbell panel. The sample contains 690 cells from all 40 editions between 1960 and 2000, except for 1988, retaining ages 30–49 and birth cohorts 1921–1960. A lawyer may appear at different ages in successive editions.

Following Goldin and Katz (2002), we estimate an OLS specification of the form:

$$Y_{at} = \gamma P_{c(a,t)} + \alpha_a + \delta_t + \varepsilon_{at} \tag{E.5}$$

where Y_{at} is 100 times the number of women lawyers in the age-by-year cell divided by female BA conferrals in year $c + 22$.[21] γ is the main coefficient of interest, measuring how the outcome varies

[21] Specifically, for birth cohort c, we use annual bachelor's-degree conferrals to women in year $c + 22$. BA conferrals

with cohort-level exposure to the pill. $P_{c(a,t)}$ measures this exposure, given by 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. We include age fixed effects α_a as well as directory year fixed effects δ_t, and standard errors clustered at cohort level.[22]

Appendix Table 4 reports results for our replication of the headline design in Goldin and Katz (2002); column 1 uses the legal coding from Goldin and Katz (2002), and column 2 uses the legal coding from Bailey (2006). Following Goldin and Katz (2002), we evaluate our regression coefficient at the mean level of pill access for birth cohorts in the mid-1950s (0.98), finding that diffusion of the pill led to an increase of 0.27 women lawyers per 100 BAs awarded to women, significant at the 1% level. Our estimate is essentially unchanged using the legal coding in Bailey (2006); we find here that diffusion of the pill led to an increase of 0.31 women lawyers per 100 BAs awarded to women. We augment this design to control for cohort-level exposure to the Vietnam draft, reflecting the core mechanism in this paper. We use the following specification:

$$Y_{at} = \gamma P_{c(a,t)} + \eta D_{c(a,t)} + \alpha_a + \delta_t + \varepsilon_{at} \tag{E.6}$$

Here, we add a Vietnam draft-risk index $D_{c(a,t)}$ formed by allocating annual inductions in 1964–1973 uniformly across ages 19–24 and dividing each birth cohort's total inductions by estimated male births, so that D_c is inductions per male birth (at most 0.175, for the 1945 cohort). Column (3) reports γ and η for women. This approximately doubles $\hat{\gamma}$, suggesting that the result in Goldin and Katz (2002) is attenuated due to the exclusion of this covariate. Evaluated at peak cohort exposure to the draft (0.175), we find that men's exposure led to an increase of 0.29 women lawyers per 100 BAs awarded to women; this is about half the magnitude of the scaled increase of 0.53 women lawyers due to pill exposure in the same specification.

in 1943, 1945, and 1947 are linearly interpolated.

[22] Notably, relative to the specification in Goldin and Katz (2002), we omit controls for state-level changes in abortion legalization.

F Additional Results

F.1 Decomposing Changes in Women's Representation

We leverage a simple decomposition to determine the impact of changes in women's and men's enrollment on women's representation in law schools. Let W and M denote women's and men's enrollment, respectively, where representation $R = W/(W+M)$. Taking the total derivative,

$$\mathrm{d}R = \left(\frac{W}{W+M}\right)\left(\frac{M}{W+M}\right)\left\{\frac{\mathrm{d}W}{W} - \frac{\mathrm{d}M}{M}\right\}$$

As a result, we can decompose the change in representation into the following pieces:

$$\mathrm{d}R = \underbrace{\left(\frac{W}{W+M}\right)\left(\frac{M}{W+M}\right)\frac{\mathrm{d}W}{W}}_{\text{Increase in Women's Enrollment}} + \underbrace{\left(\frac{W}{W+M}\right)\left(\frac{M}{W+M}\right)\frac{-\mathrm{d}M}{M}}_{\text{Decrease in Men's Enrollment}}$$

We use a discrete approximation to plot this in each year. Let W_t, M_t, R_t denote enrollment figures in year t. We linearly interpolate between years to determine the leading share terms and use logs to approximate percentage changes in enrollment. More precisely, letting

$$S_t = \left(\frac{W_t}{W_t+M_t}\right)\left(\frac{M_t}{W_t+M_t}\right)$$

We estimate

$$\text{Increase in Women's Enrollment} \approx (0.5 \times S_t + 0.5 \times S_{t-1}) \times (\ln(W_t) - \ln(W_{t-1})) \tag{F.1}$$

$$\text{Decrease in Men's Enrollment} \approx (0.5 \times S_t + 0.5 \times S_{t-1}) \times (\ln(M_{t-1}) - \ln(M_t)) \tag{F.2}$$

Then, a positive value for each term will indicate a contribution to an increase in women's representation, whereas a negative term will indicate a contribution to a decrease in women's representation. These results are plotted in Appendix Figure 1.

F.2 Additional Heterogeneity Results

In Appendix Figure 44, we 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 be higher for these schools, but the variance of this loss would be lower, and there should be no difference in lost revenue as a fraction of total revenue. Appendix Figure 44a plots estimates of equation (6) 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 28c, 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 44b) and the estimated number of full-time equivalent faculty in 1967 (Appendix Figure 44c), as the number of faculty members is another indicator of program size that is not mechanically related to enrollment.[23]

F.3 Robustness to Prestige Differences

Our previous results suggest that it is important to consider differences in responses across the prestige distribution. In Appendix Table 8, we examine how our estimates of equation (8) interact with school prestige. As before, we measure presence in the bar six years after bar admission, comparing the 1972–79 cohorts with 1956–69 and excluding 1970–71. We use the same prestige measure as our earlier exercises, the log of the number of volumes in the law library in 1970. The first row reproduces the result from our baseline specification in the text. The second row adds linear cohort trends interacted with observed log prestige, with an indicator for missing prestige, to control for linear changes in the difference between full-time and part-time outcomes across time. The third row excludes the most prestigious full-time schools, operationalized as programs in the top tercile of prestige, to rule out that our results are being driven solely by movement at prestigious programs. Similarly, in row four, we include only full-time programs in the bottom half of the prestige distribution, so that prestige is roughly similar in the treatment and comparison groups. Our estimated share of women entering the bar shrinks very modestly to 5.7 percentage points after adding a linear trend in prestige (standard error 2.8), compared with 6.5 points in our baseline specification. Excluding the more prestigious full-time programs yields an estimate of 6.3 points, while excluding the top half by prestige yields a coefficient estimate of 5.9 points. In summary, our estimates attenuate modestly but remain statistically significant at the 5% level across all three checks.

F.4 Draft-Lottery Associations Among Lawyers Observed in 1981

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

[23] 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).

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 45 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. For our causal analysis, we restrict to the 40,119 men born in 1944–50. To estimate the impact of induction risk, we use the following specification:

$$Y_{i,dmy} = \alpha + \beta \text{eligible}_{dmy} + \gamma_m + \delta_y + \varepsilon_{i,dmy} \tag{F.3}$$

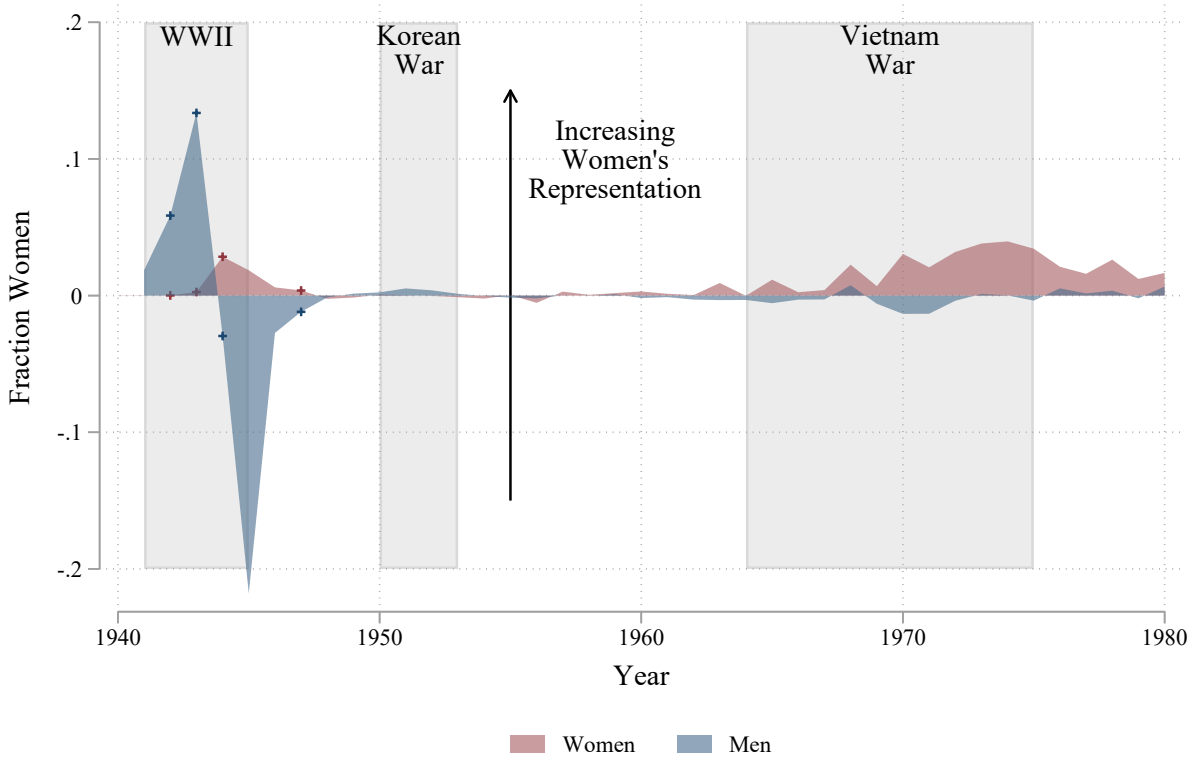
Here, $Y_{i,dmy}$ is our outcome of interest for individual i, born on day d month m and year y, and eligible_{dmy} indicates that day dm was below the ceiling in year y. If we were analyzing one birth cohort and numbers were truly randomly assigned, it would be enough to calculate the difference in means between eligible and ineligible men ($\hat{\beta}$). However, in practice, the lottery assigned lower 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 adjust for this. Additionally, when we pool across cohorts, we include birth-year fixed effects δ_y. Standard errors are adjusted for heteroskedasticity.

Among men born in 1944–50, eligibility is associated with a 2.7-percentage-point increase in reported military service, relative to an ineligible mean of 10.3%. Eligible men graduate 0.15 years later and are 4.1 percentage points less likely to graduate at ages 22–26. Among 14,661 men explicitly identified as partners, members, or associates at national firms at or above the 1981 90th size percentile, eligibility is associated with a 2.8-percentage-point lower partner share (standard error 0.8), relative to an ineligible mean of 57.0%. National firm size counts distinct lawyers across each firm's observed offices in the Geographic Bar Roster; the inclusive percentile cutoff is eight lawyers. Appendix Table 9 replaces the eligibility indicator with lottery-number groups, using numbers 231–366 as the reference. Relative to the omitted group, lottery numbers that were eventually called (1-195) are associated with an increase in the likelihood of reporting military service, a decrease in on-time graduation, an increase in age at graduation, and a decrease in the likelihood of large-firm partnership. We find 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 Section 5.3, the lottery moved men's career

timing and careers but not their presence in the bar. Appendix Figure 46 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.

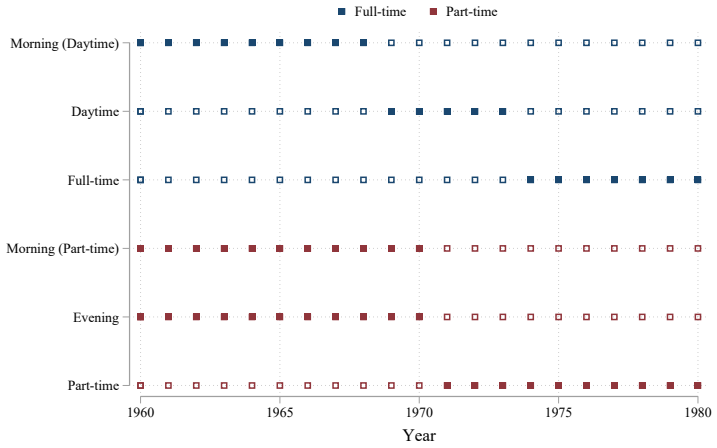
Appendix Figure 1: Relative Contributions to Changes in Women's Representation in Full-Time Programs



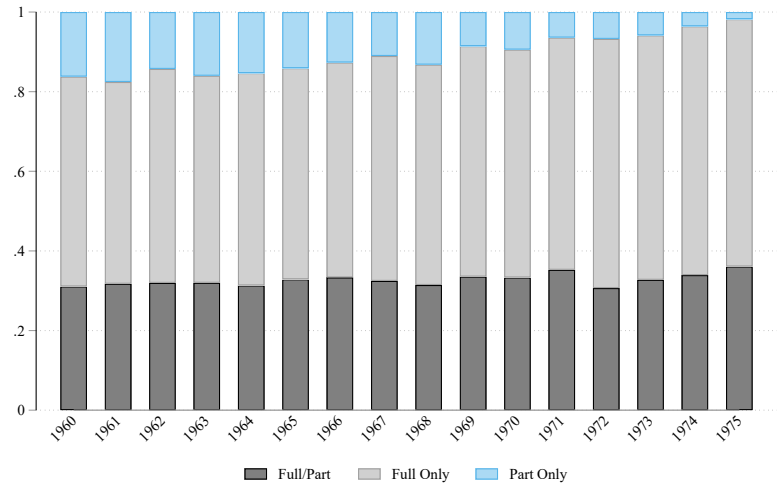
Appendix Figure 1 plots estimates of the contributions of changes in women's full-time enrollment (F.1) and men's enrollment (F.2) to changes in women's representation in law school between 1940 and 1980. The area in red represents the estimated increase in women's representation that resulted from increases in women's enrollment, and the area in blue represents the estimated increase in women's representation that resulted from decreases in men's enrollment. These can take negative values to plot the decrease in women's representation that resulted from decreases in women's enrollment and increases in men's enrollment, respectively. Between 1940 and 1947, markers are used to indicate the years in which figures for men's and women's enrollment are available or estimable from available enrollment data. These data are available in 1940, 1942, 1943, and 1947. In 1944, we observe women's and men's total enrollment (across full-time and part-time programs), and estimate full-time enrollment by multiplying each respective value by the fraction of all women (men) in a full-time program in 1943. All missing years in this time period are estimated via linear interpolation; all years of data are available between 1947 and 1980.

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

(a) Coding Full-Time and Part-Time

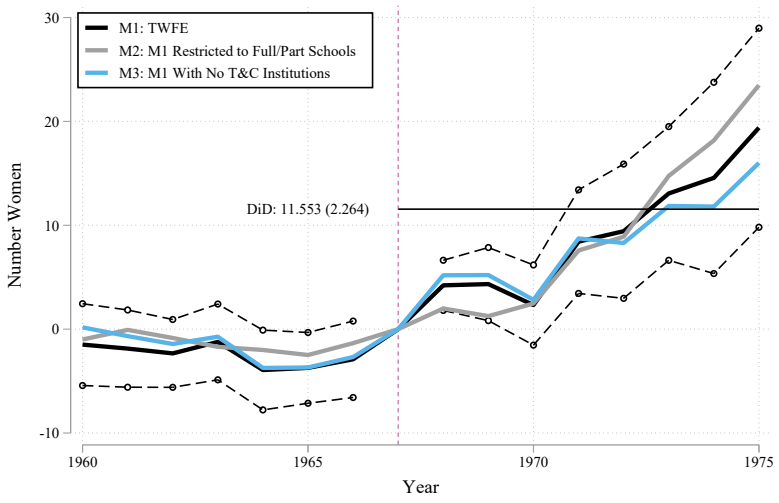


(b) Program Mix Across Law Schools



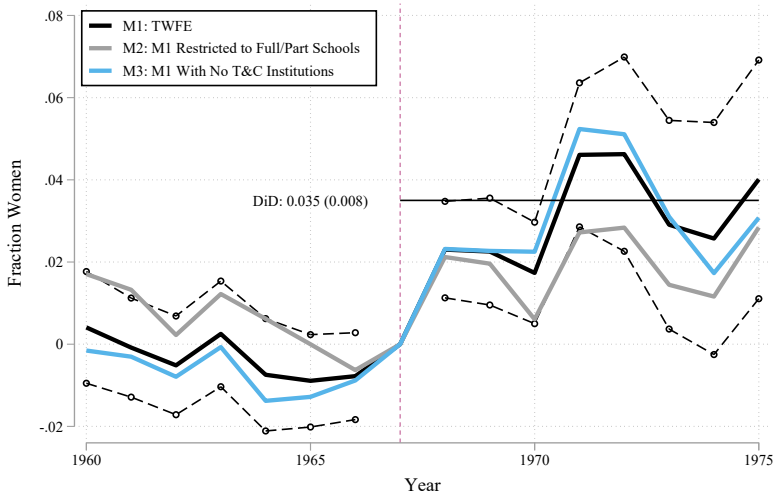
Appendix Figure 2 provides metadata on our dataset. Appendix Figure 2a demonstrates how we sort each year's differential coding of program types into our consistent full-time and part-time coding scheme. Using this scheme, Appendix Figure 2b shows the fraction of institutions in each year operating both 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: Robustness to Entropy Weighting

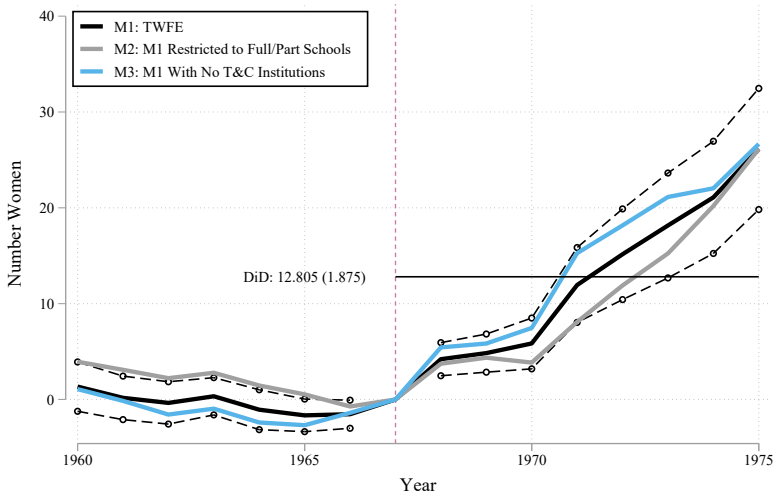


Appendix Figure 3 plots coefficient estimates from equation (3), where the outcome is the number of first-year students who are women and the enrollment control is omitted. Instead, we estimate entropy weights in 1967 such that weighted average total enrollment is equal in the treatment and control groups (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 (4) with the enrollment control omitted. 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 4: Robustness to State-by-Year Fixed Effects



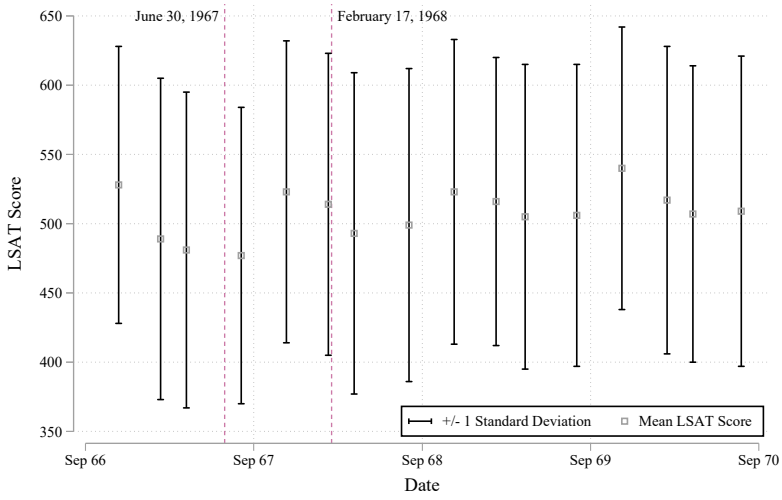
(a) Fraction Women



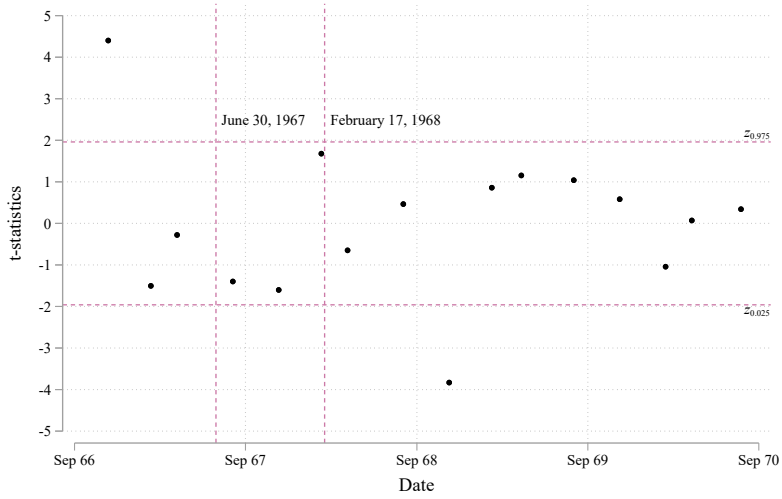
(b) Number Women

Appendix Figure 4a 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) augmented with state-by-year fixed effects. Appendix Figure 4b 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) augmented with state-by-year fixed effects. 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 5: Women's LSAT Scores



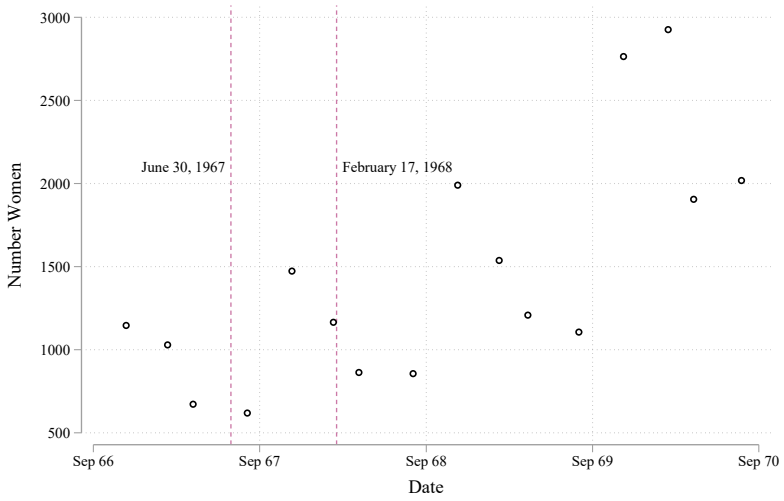
(a) Raw Data



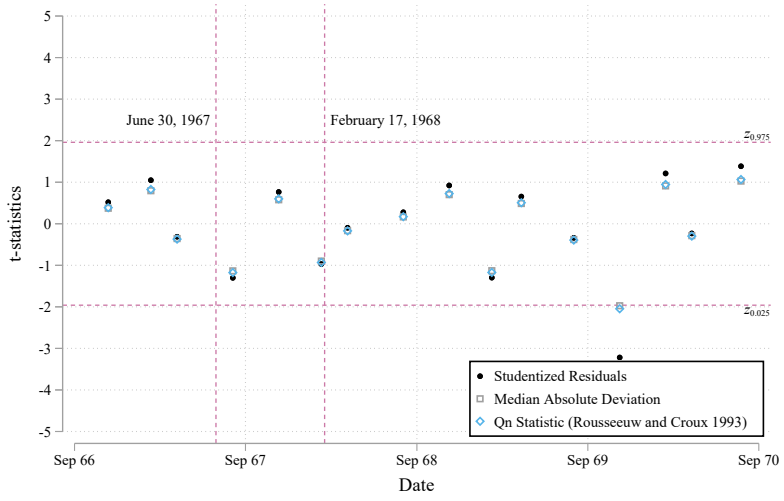
(b) Statistical Tests

Appendix Figure 5a 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 5b 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 6: Number of Women Registered to Take the LSAT



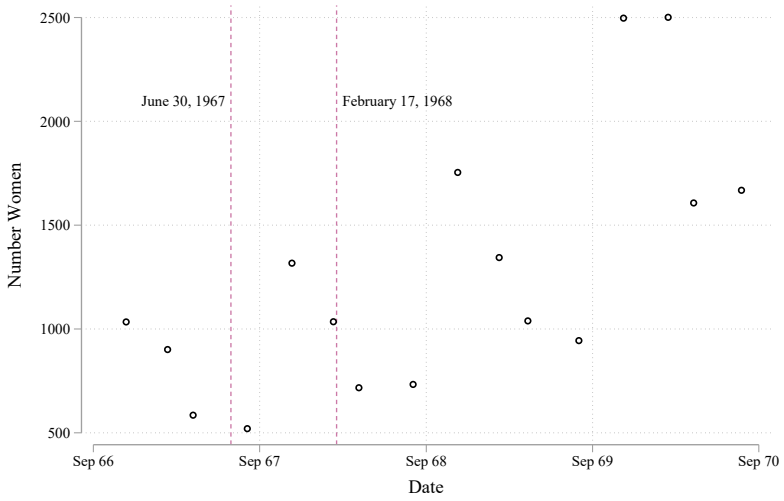
(a) Raw Data



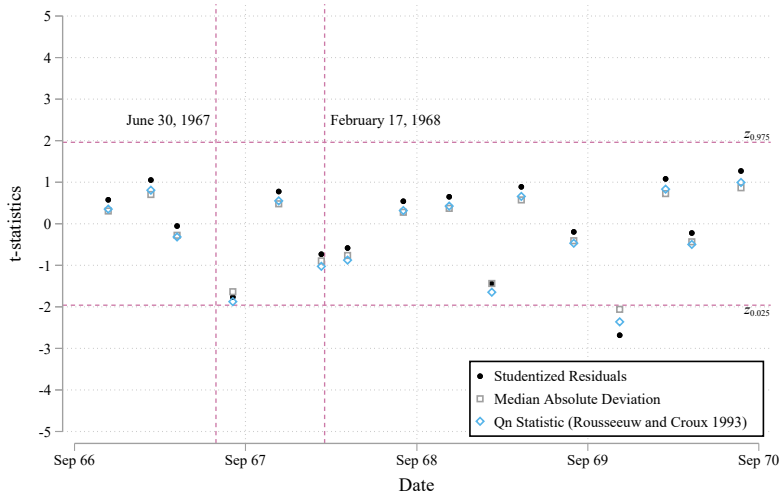
(b) Statistical Tests

Appendix Figure 6a plots the raw number of women who registered for each LSAT exam on the day of each exam administration. Appendix Figure 6b 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 equations (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 7: Number of Women Taking the LSAT



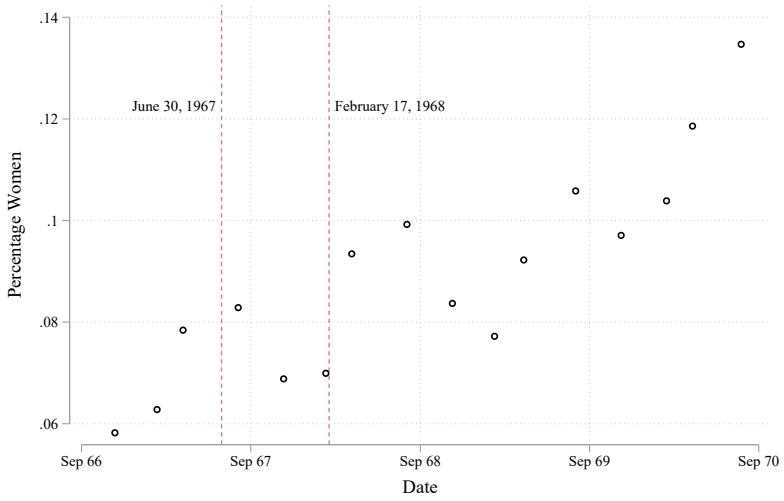
(a) Raw Data



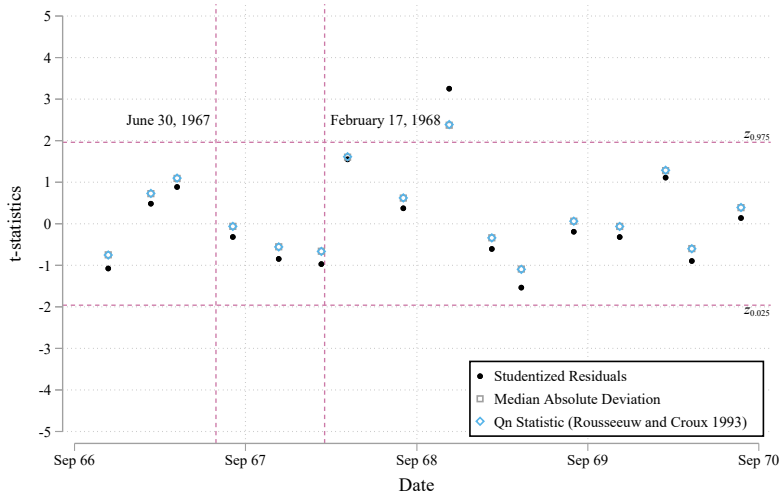
(b) Statistical Tests

Appendix Figure 7a plots the raw number of women who sat for each LSAT exam on the day of each exam administration. Appendix Figure 7b 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 equations (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 8: Fraction of Students Registered to Take the LSAT who are Women



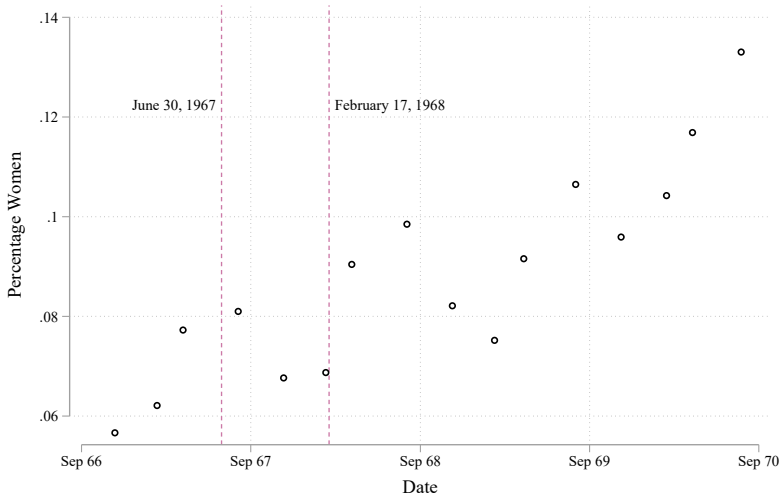
(a) Raw Data



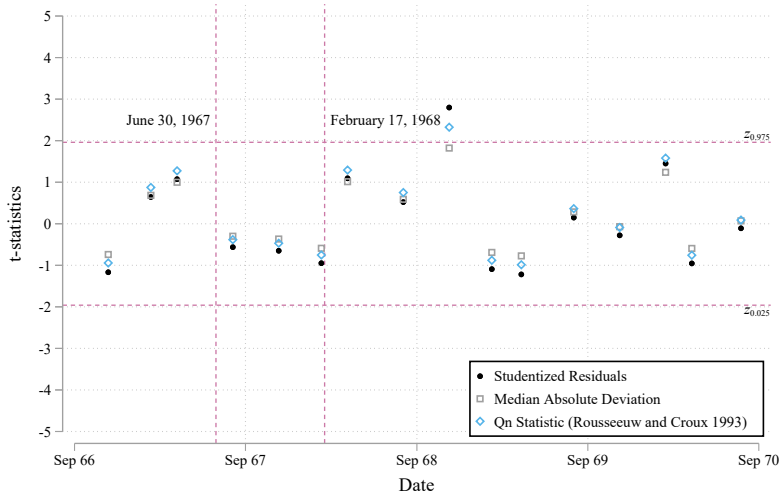
(b) Statistical Tests

Appendix Figure 8a plots the raw fraction of students who registered for each LSAT exam who were women on the day of each exam administration. Appendix Figure 8b 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 equations (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 9: Fraction of Students Taking the LSAT who are Women



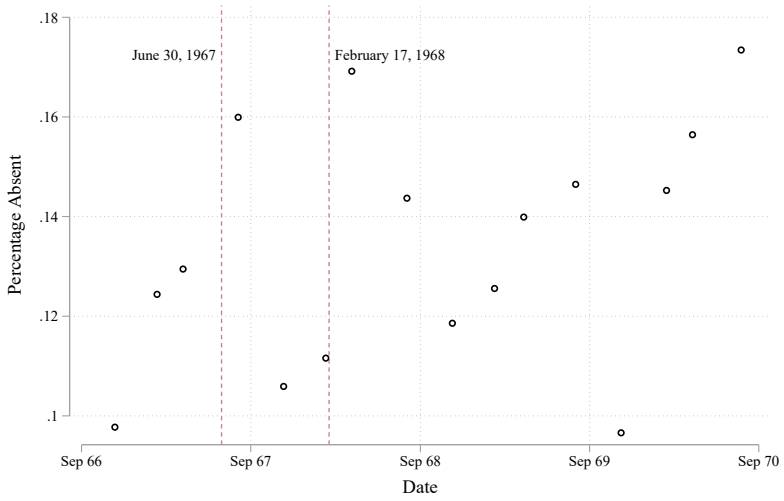
(a) Raw Data



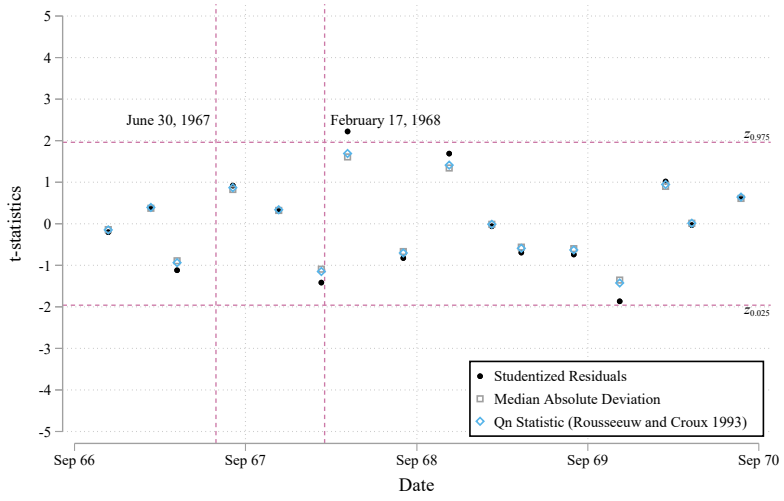
(b) Statistical Tests

Appendix Figure 9a plots the raw fraction of students who sat for each LSAT exam who were women on the day of each exam administration. Appendix Figure 9b 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 equations (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 10: Fraction of Women Absent from LSAT



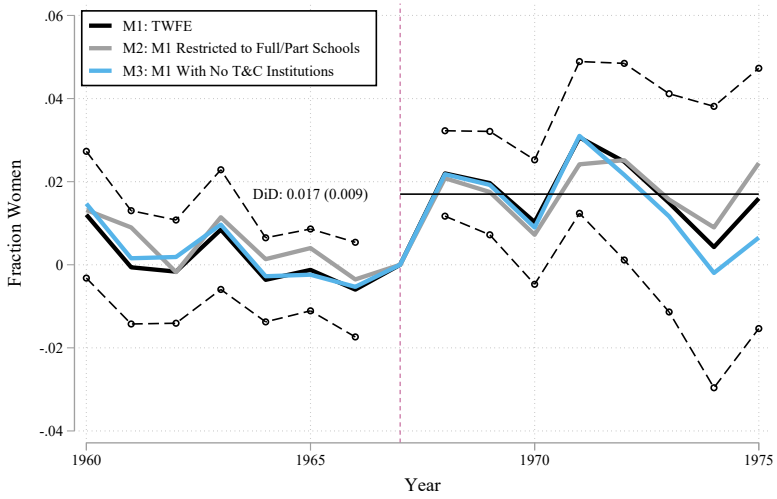
(a) Raw Data



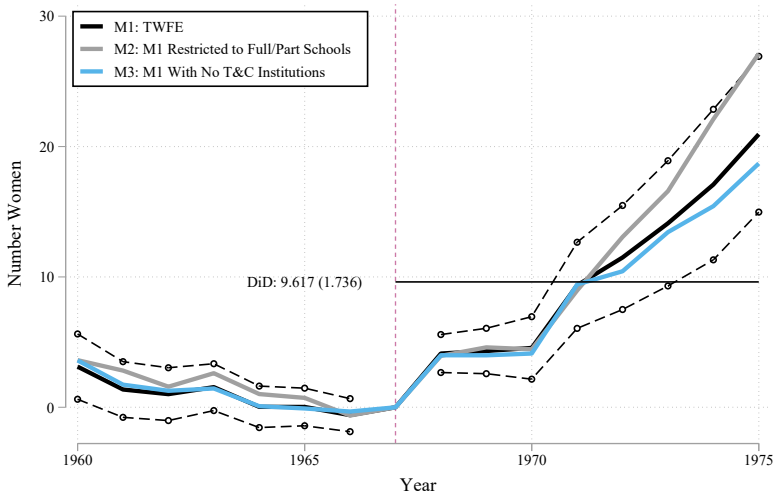
(b) Statistical Tests

Appendix Figure 10a 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 10b 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 equations (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 11: Restricting to ABA-Approved Schools



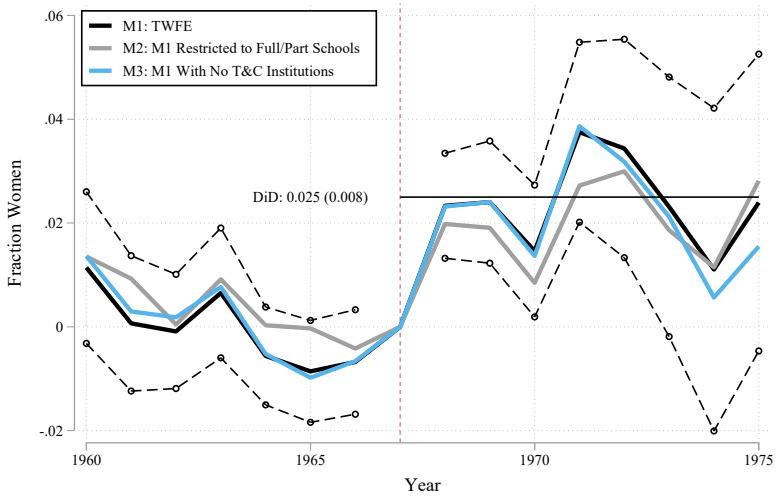
(a) Fraction Women



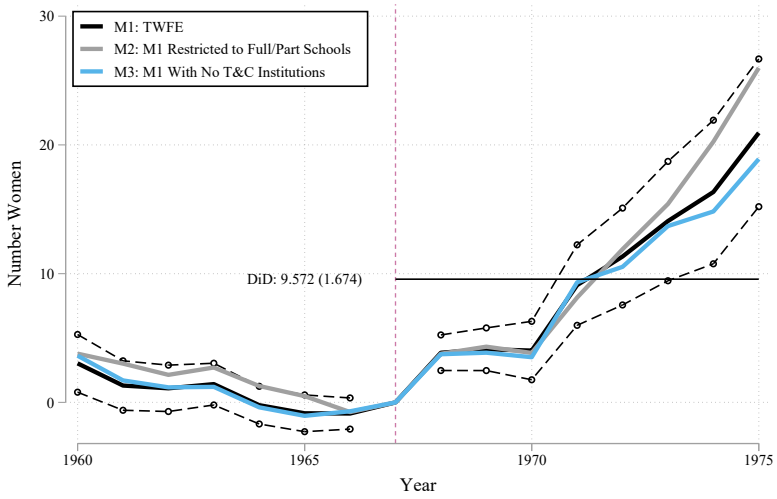
(b) Number Women

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

Appendix Figure 12: Restricting to Schools ABA-Approved by 1975



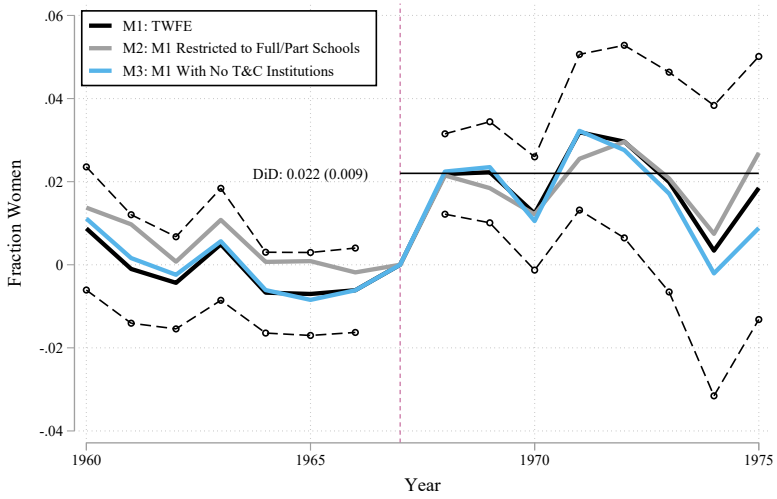
(a) Fraction Women



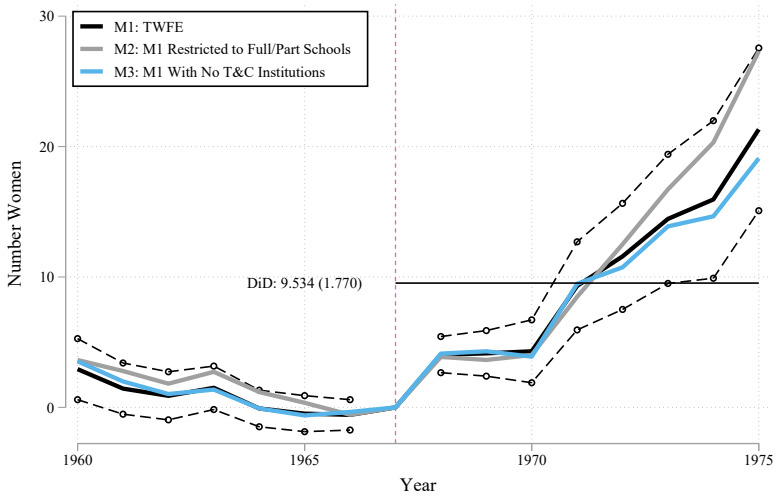
(b) Number Women

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 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 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: Institution-Balanced Panel



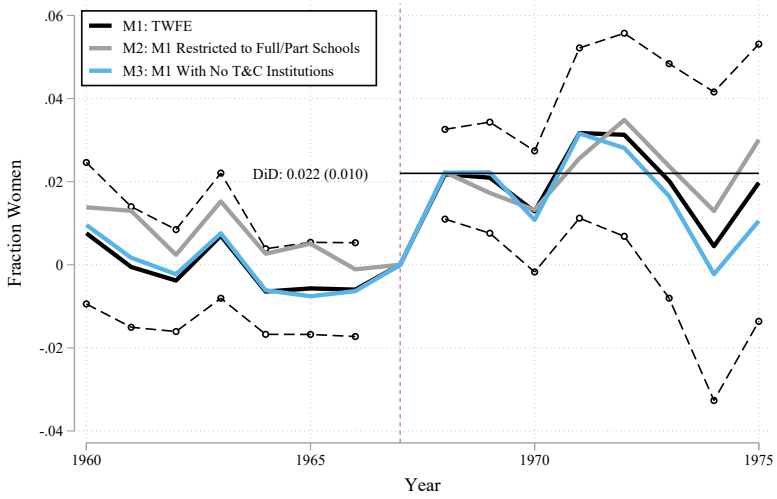
(a) Fraction Women



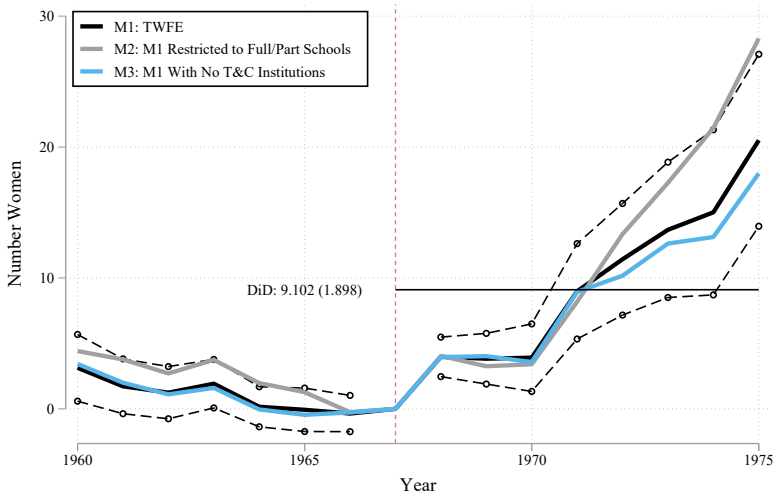
(b) Number Women

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 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 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: Institution-Program-Balanced Panel



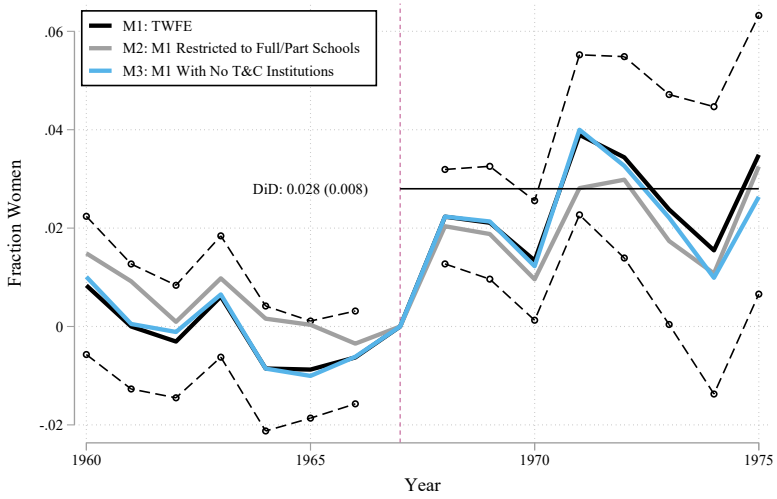
(a) Fraction Women



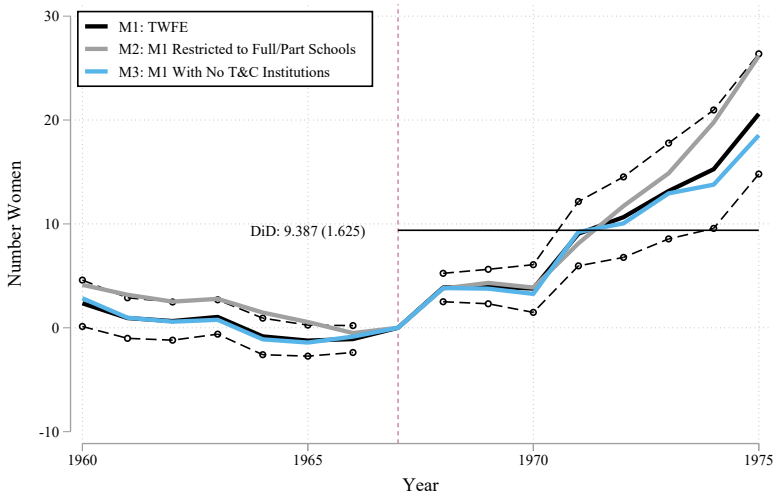
(b) Number Women

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



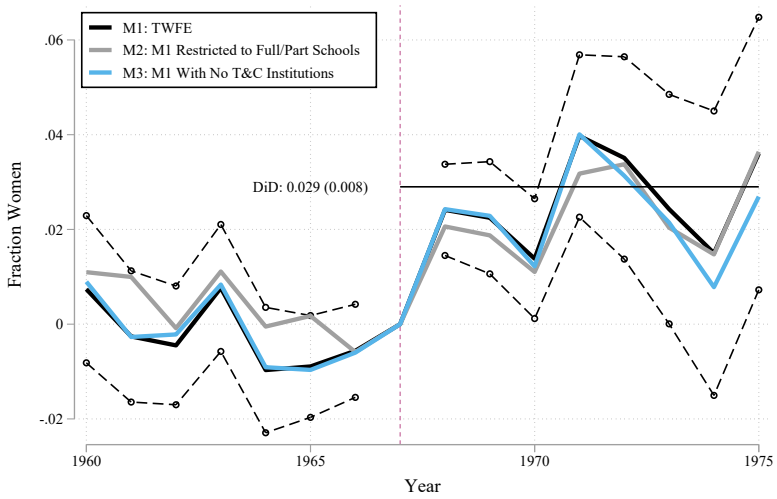
(a) Fraction Women



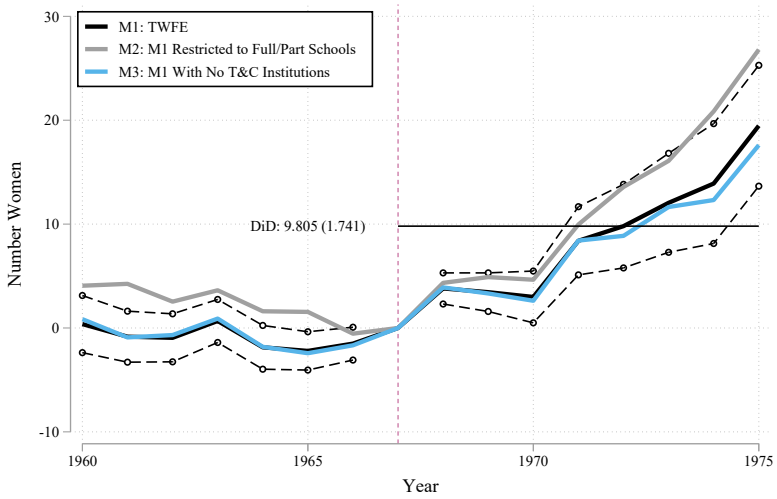
(b) Number Women

Appendix Figure 15a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. The sample is restricted to institution-program-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 15b 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 16: Restricting to Institution-Program-Years with at least 50 Students



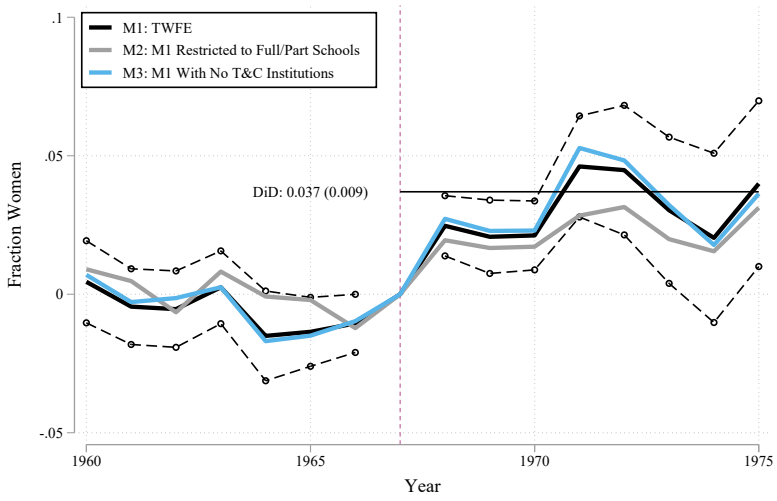
(a) Fraction Women



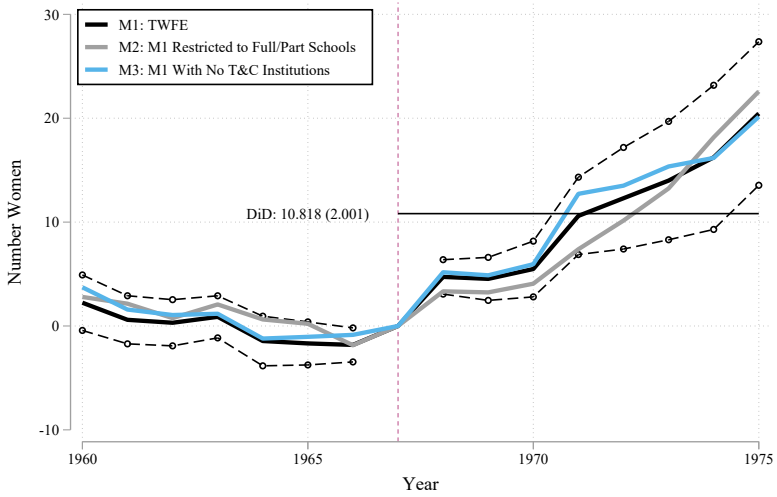
(b) Number Women

Appendix Figure 16a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. The sample is restricted to institution-program-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 16b 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 17: Excluding the South



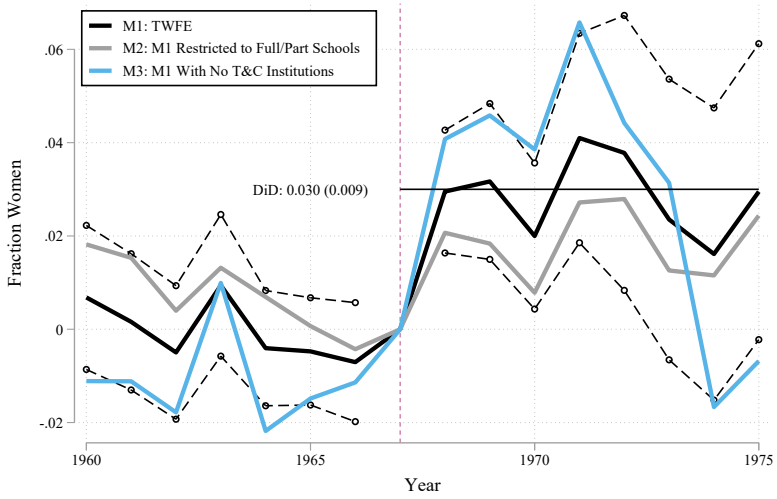
(a) Fraction Women



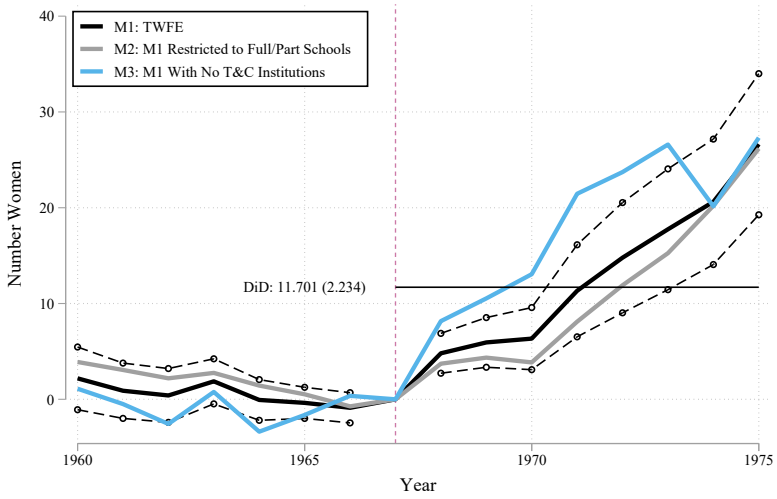
(b) Number Women

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. 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 17b 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 18: Robustness to City-by-Year Fixed Effects



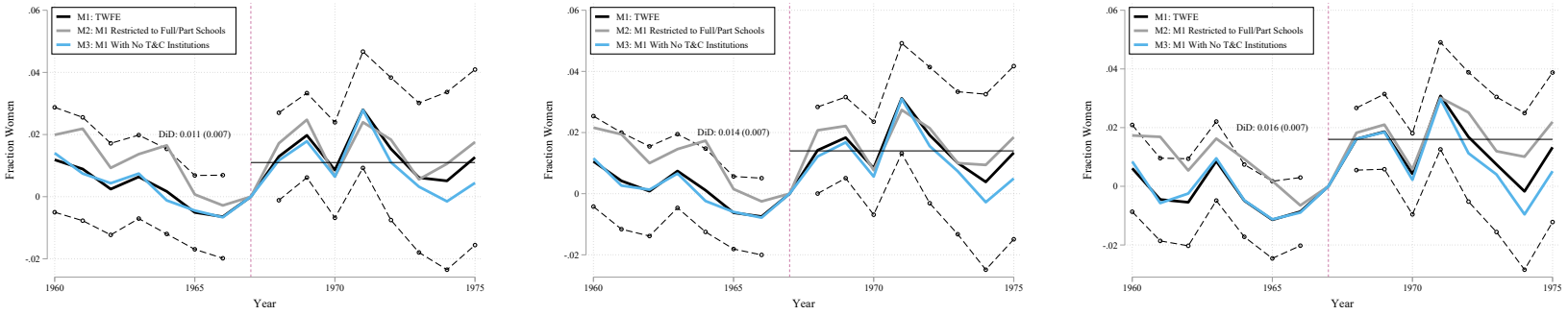
(a) Fraction Women



(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. 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), augmented with city-by-year fixed effects. Appendix Figure 18b 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), augmented with city-by-year fixed effects. 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.

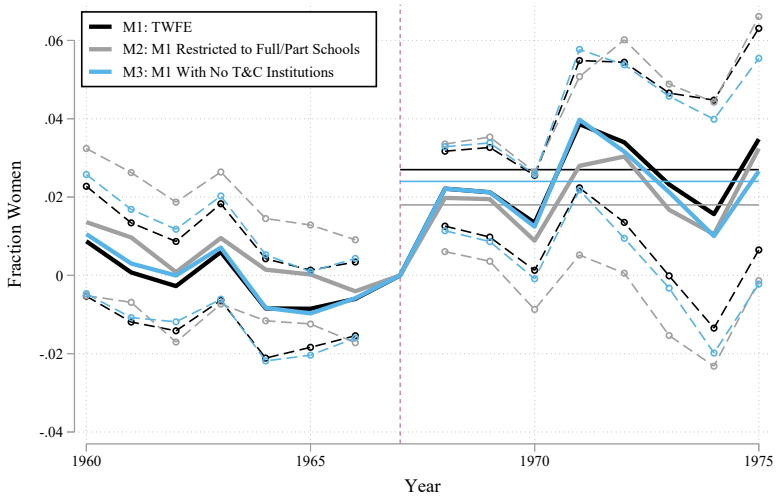
Appendix Figure 19: No Weighting



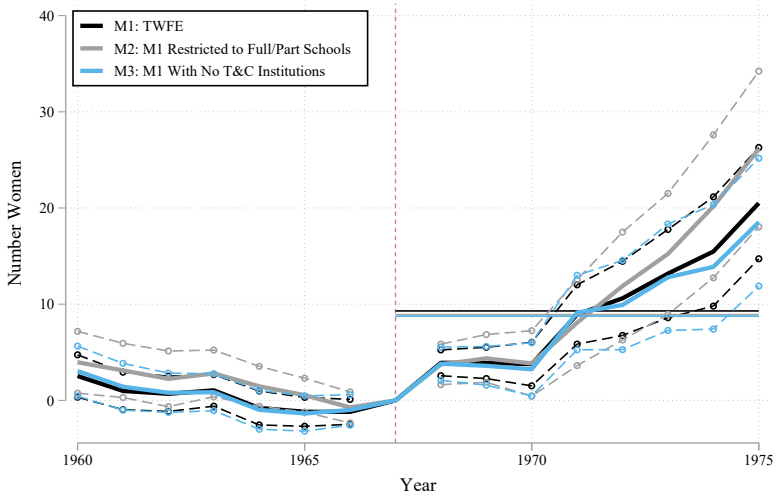
(a) Fraction Women (1L 10+) (b) Fraction Women (1L 20+) (c) Fraction Women (1L 50+)

Appendix Figure 19a 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 19b replicates this figure estimated on a sample restricted to institution-program-years with at least 20 students enrolled. Appendix Figure 19c replicates this figure estimated on a sample restricted to institution-program-years with at least 50 students enrolled.

Appendix Figure 20: Clustering at the Institution-Program Level



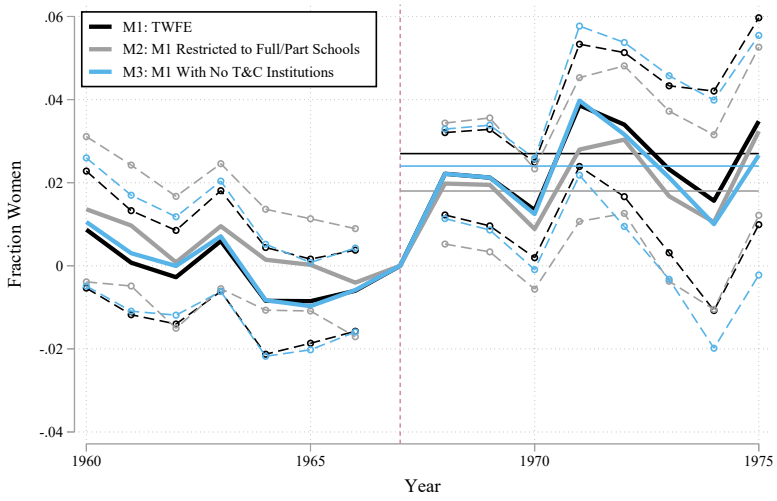
(a) Fraction Women



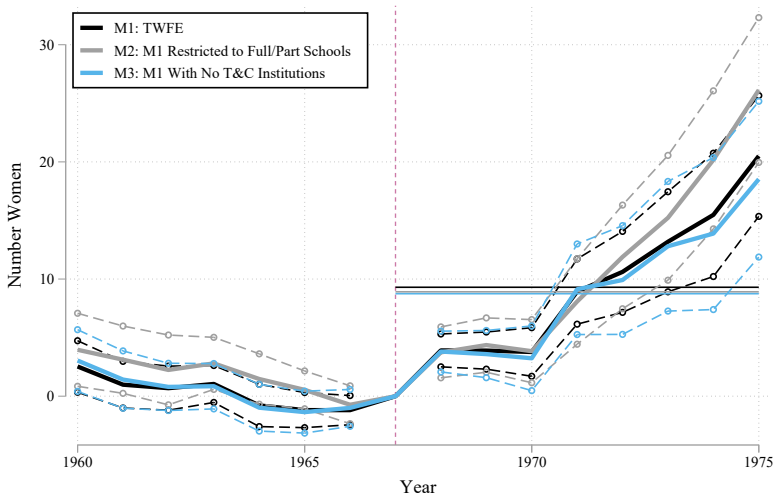
(b) Number Women

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. Appendix Figure 20b 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 20a) or equation (4) (Appendix Figure 20b).

Appendix Figure 21: Clustering at the Institution Level



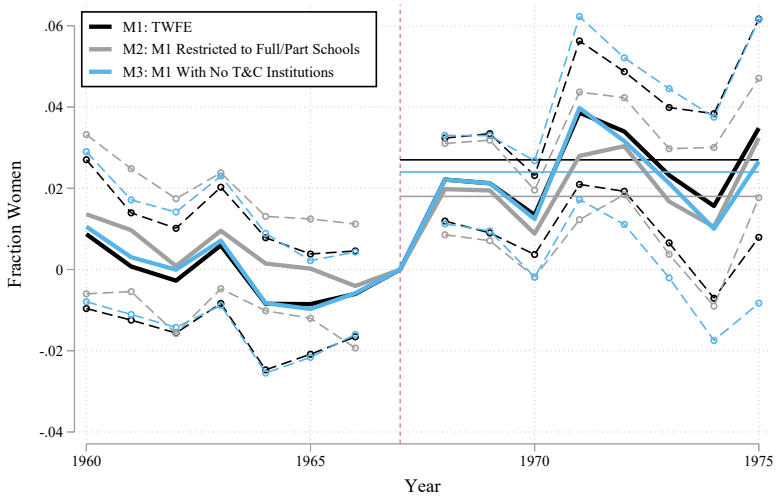
(a) Fraction Women



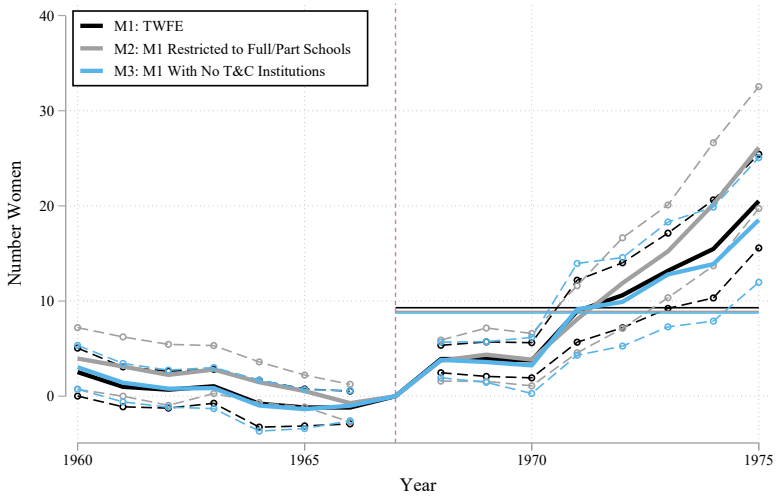
(b) Number Women

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. Appendix Figure 21b 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 21a) or equation (4) (Appendix Figure 21b).

Appendix Figure 22: Clustering at the State Level



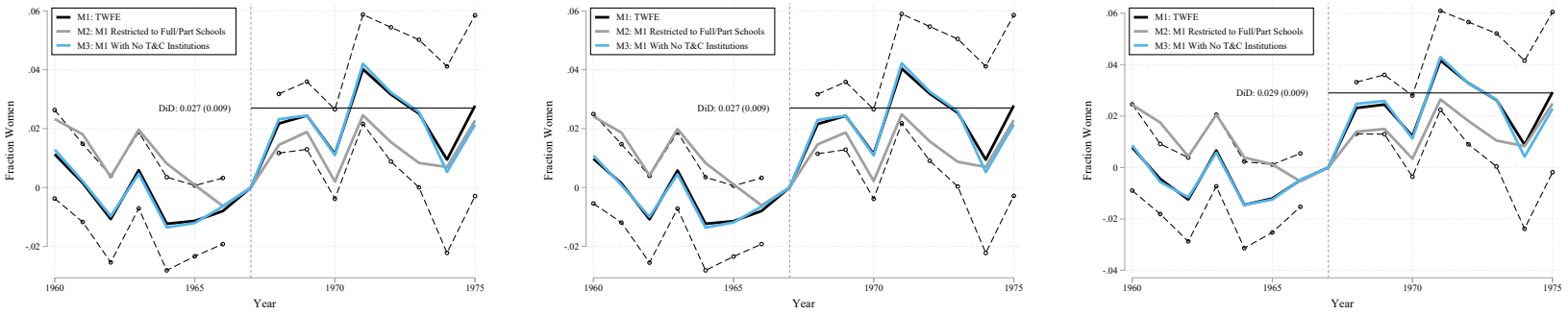
(a) Fraction Women



(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. Appendix Figure 22b 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 22a) or equation (4) (Appendix Figure 22b).

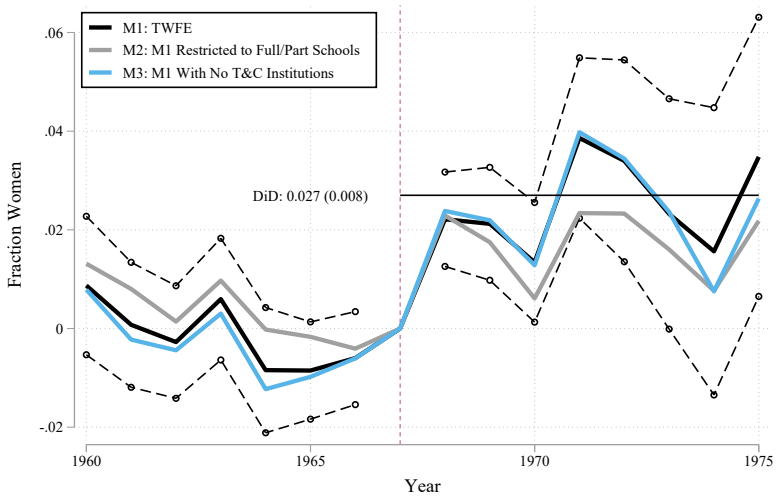
Appendix Figure 23: 1967 Weights



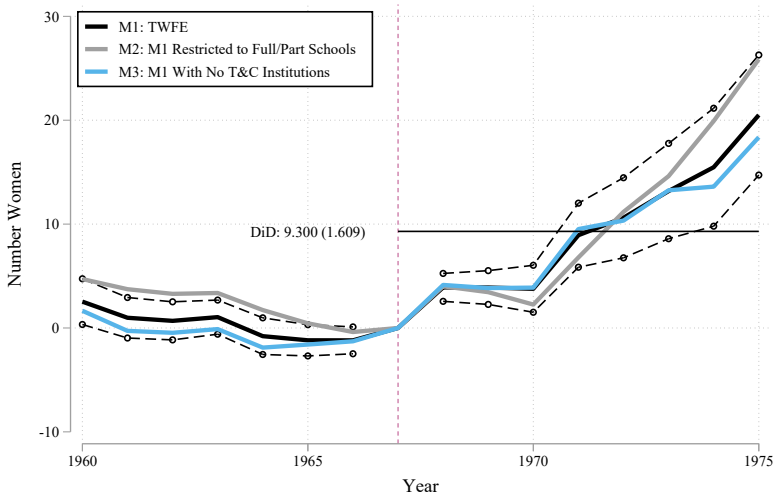
(a) Fraction Women (1L 10+) (b) Fraction Women (1L 20+) (c) Fraction Women (1L 50+)

Appendix Figure 23a 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 23b replicates this figure estimated on a sample restricted to institution-program-years with at least 20 students enrolled. Appendix Figure 23c replicates this figure estimated on a sample restricted to institution-program-years with at least 50 students enrolled.

Appendix Figure 24: Fixing Program Mix at 1967 Value



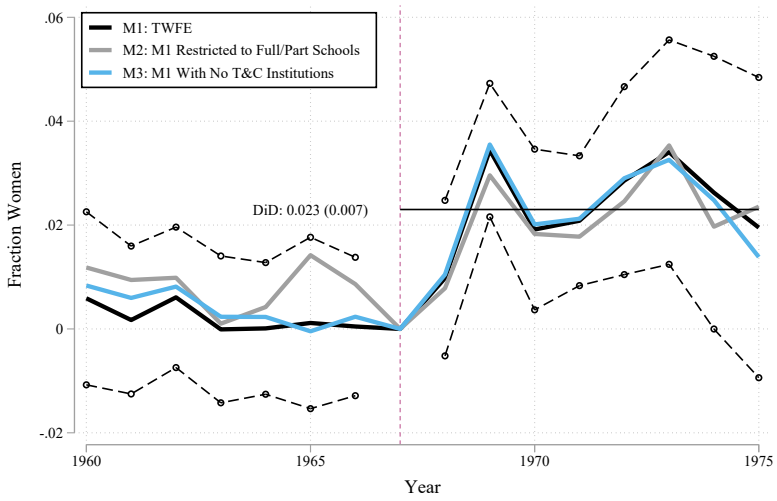
(a) Fraction Women



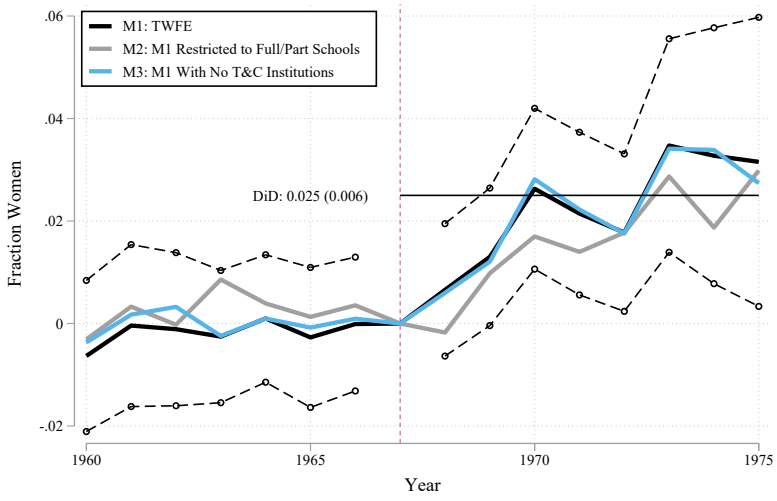
(b) Number Women

Appendix Figure 24a 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 24b 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. Treatment assignment across all models is fixed to an institution's 1967 program mix.

Appendix Figure 25: Difference-in-Differences Results: 2L and 3L



(a) Fraction Women (2L)

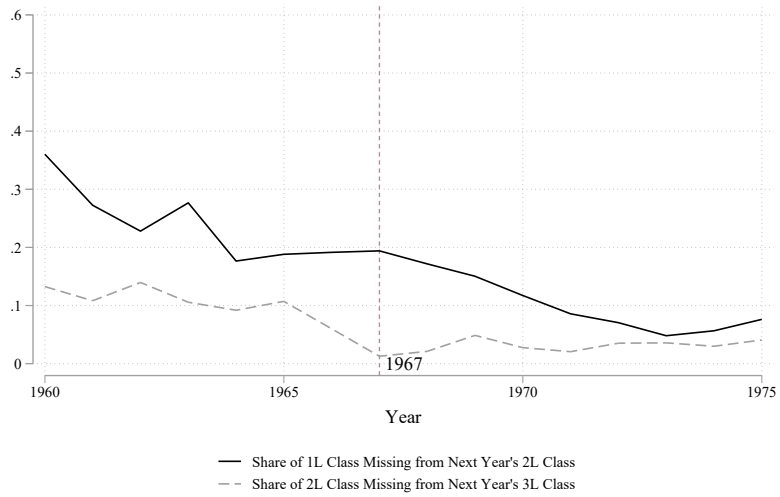


(b) Fraction Women (3L)

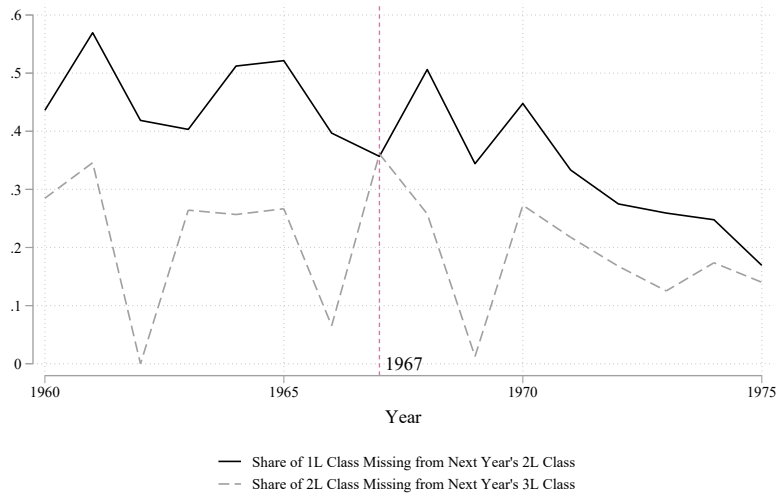
Appendix Figure 25a and Appendix Figure 25b plot coefficient estimates from equation (1), where the outcome is the fraction of second-year (Appendix Figure 25a) or third-year (Appendix Figure 25b) students who are women and the regression is weighted by total second-year (Appendix Figure 25a) or third-year (Appendix Figure 25b) 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 26: Aggregate Attrition for Women

(a) Full-Time Programs

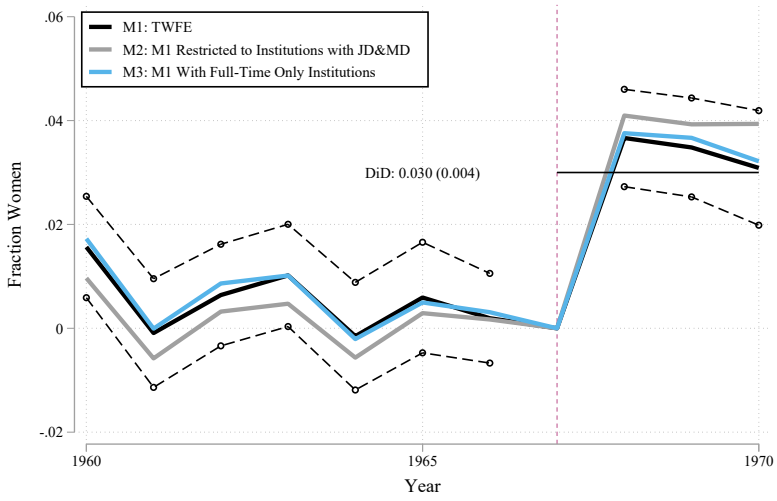


(b) Part-Time Programs

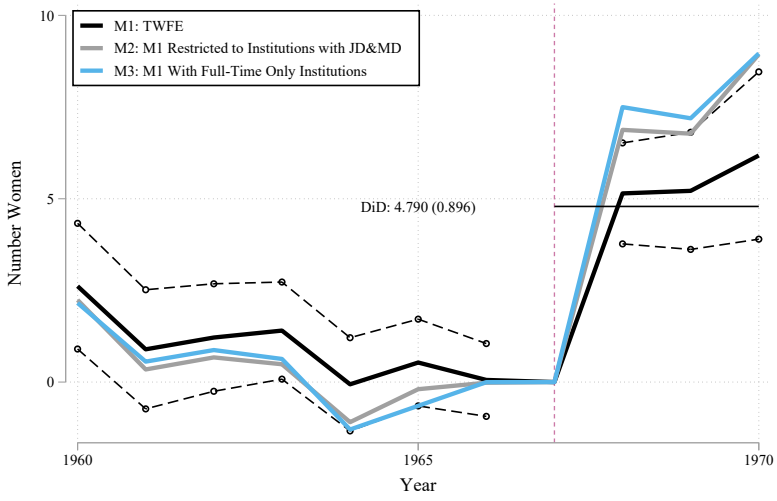


Each figure plots estimates of women's attrition from law schools in each year. To do this, we calculate the percentage drop in women's enrollment from year to year. These calculations are plotted in the baseline year; for example, in 1967, we plot the percentage of 1L students in 1967 that are missing 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 26a plots these series for full-time programs. Appendix Figure 26b plots these series for part-time programs.

Appendix Figure 27: Law-Medicine Difference-in-Differences Results



(a) Fraction Women

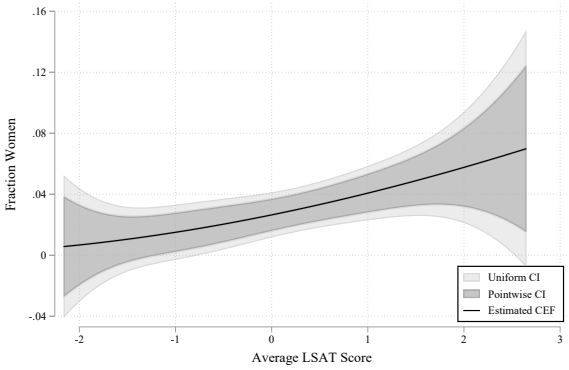


(b) Number Women

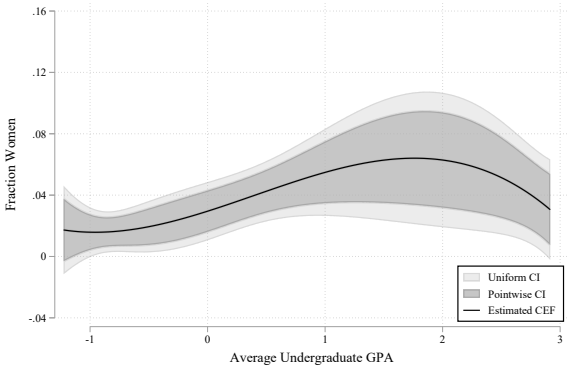
Figure 27a plots coefficient estimates from equation (D.1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Model 1 uses a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (D.2). Figure 27b plots coefficient estimates from equation (D.3), where the outcome is the number of first-year students who are women and a control for total enrollment is included. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (D.4). In both figures, Model 2 restricts our sample to include only institutions operating both a medical school and a law school, and Model 3 restricts the law schools in our sample to those operating only a full-time program.

Appendix Figure 28: Alternative Continuous Difference-in-Differences Results

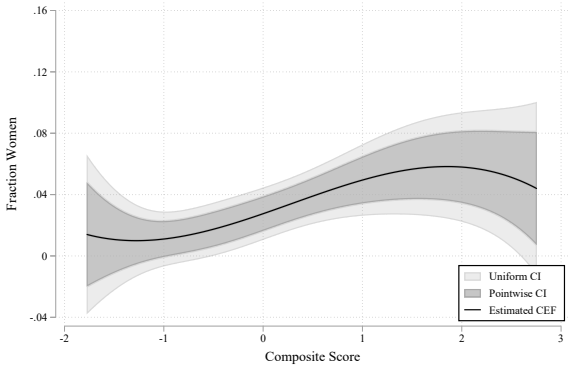
(a) Average LSAT Score



(b) Average Undergraduate GPA

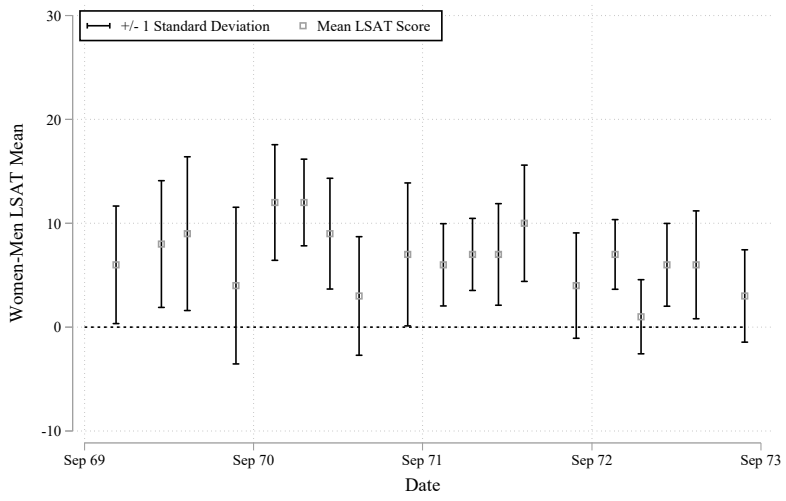


(c) Composite Score



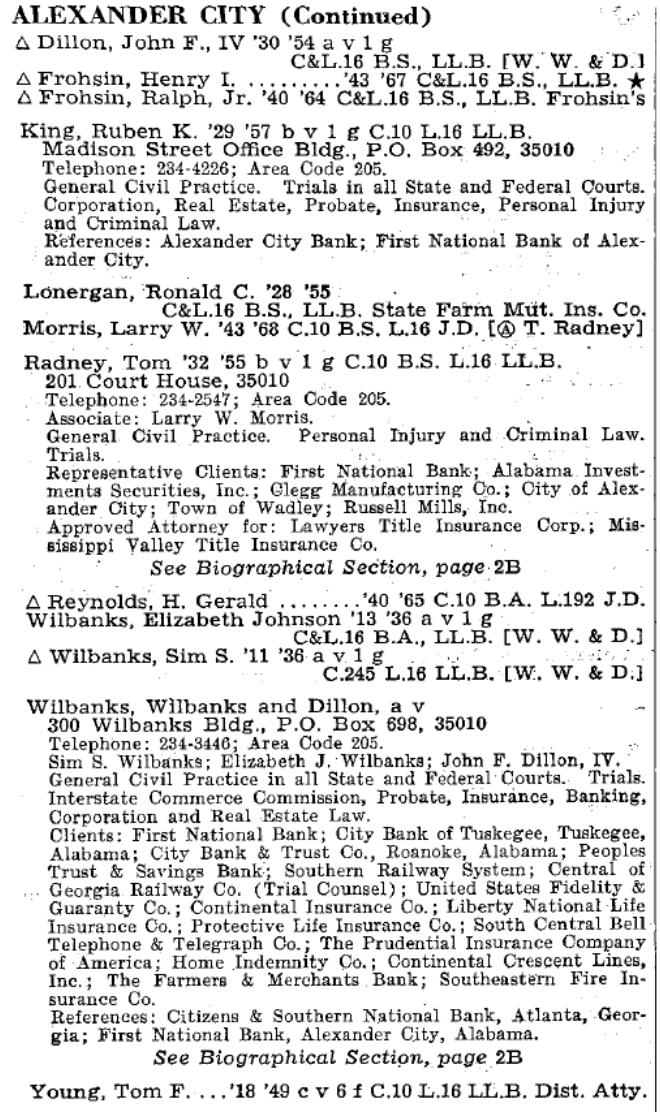
This figure plots estimates from equation (6), where the outcome is the change in the fraction of first-year students who are women between 1967 and 1968 and the regression is weighted by total first-year enrollment in 1967. The dose variable is imputed average LSAT score for the 1968-69 incoming class in Appendix Figure 28a, imputed average undergraduate GPA for the 1968-69 incoming class in Appendix Figure 28b, and the composite score for the 1968-69 incoming class in Appendix Figure 28c, where the composite score is computed as $(1/2)(\text{LSAT}) + (1/2)(200 \times \text{UGPA})$. To impute estimated means, we assign each observation the value of its bin's midpoint (or endpoint for top and bottom bins); the midpoint values are given in Appendix Table 5. LSAT and UGPA means are estimated from binned counts by multiplying the fraction of enrolled students in a given bin by that bin's midpoint and taking the sum. We drop one program with an unusually low imputed average LSAT (< 400) and GPA (< 2.0). 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 $\hat{Y}_{ip}$ for treated units ($D_p = 1$) 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 29: Differences in LSAT Scores by Sex, 1969-1973



Using data from Wightman (1974), we plot the difference between men's and women's average LSAT score by date of administration between November 1969 and July 1973, where date of administration was collected from the University of Chicago law school bulletins, as well as a 95% confidence interval using the sex-specific standard deviations. Note that these statistics are all reported in whole numbers, so these statistics are all a close approximation.

Appendix Figure 30: Raw *Martindale–Hubbell* Geographic Bar Roster 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.
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Telephone: 234-4226; Area Code 205.
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References: Alexander City Bank; First National Bank of Alexander City.

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

Radney, Tom '32 '55 b v 1 g C.10 B.S. L.16 LL.B.
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Telephone: 234-2547; Area Code 205.
Associate: Larry W. Morris.
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Approved Attorney for: Lawyers Title Insurance Corp.; Mississippi Valley Title Insurance Co.
See Biographical Section, page 2B

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

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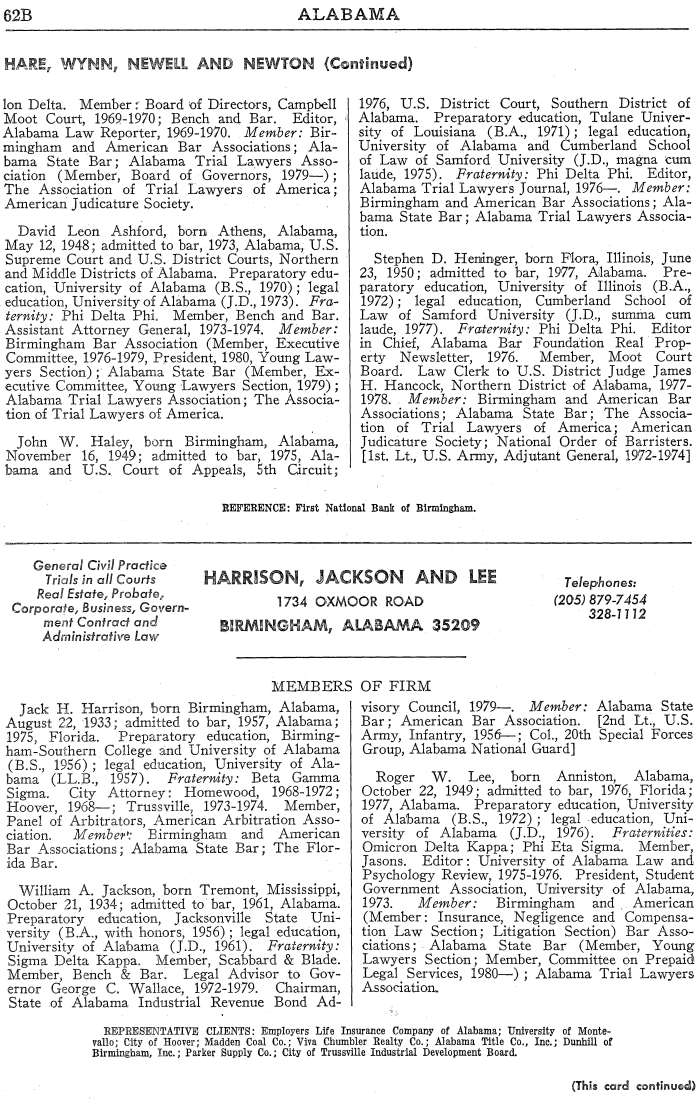
Sim S. Wilbanks; Elizabeth J. Wilbanks; John F. Dillon, IV.
General Civil Practice in all State and Federal Courts. Trials. Interstate Commerce Commission, Probate, Insurance, Banking, Corporation and Real Estate Law.
Clients: First National Bank; City Bank of Tuskegee, Tuskegee, Alabama; City Bank &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

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

[Appendix Figure 30](#) provides an example of the Geographic Bar Roster scans used to construct the *Martindale–Hubbell* dataset.

Appendix Figure 31: 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.

General Civil Practice
Trials in all Courts
Real Estate, Probate, Corporate, Business, Government Contract and Administrative Law

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BIRMINGHAM, ALABAMA 35209

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(205) 879-7454
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.; Vira Chumbler Realty Co.; Alabama Title Co., Inc.; Dunhill of Birmingham, Inc.; Parker Supply Co.; City of Trussville Industrial Development Board.

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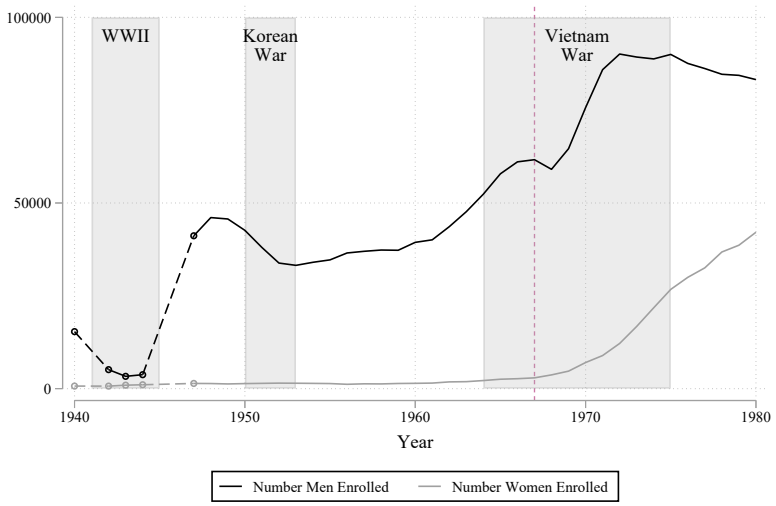
Appendix Figure 31 provides an example of the Biographical Section scans used to construct the *Martindale–Hubbell* dataset.

Appendix Figure 32: Enrollment Statistics in Levels

(a) Historical Enrollment

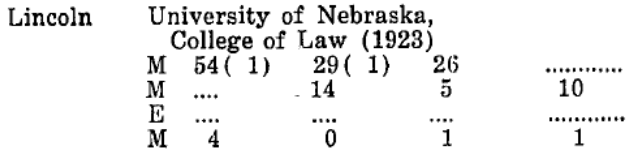


(b) Enrollment by Program Type



Appendix Figure 32a plots the number of all enrolled law students who are men and women between 1940 and 1980, imputed from Table 27 of Abel (1989). 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/. Appendix Figure 32b 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.

Appendix Figure 33: Raw *Review of Legal Education* Scans

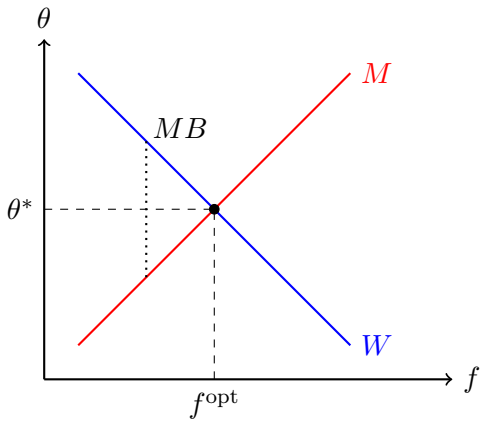


Lincoln	University of Nebraska, College of Law (1923)			
M	54(1)	29(1)	26	.....
M	....	14	5	10
E	....	....	....	.....
M	4	0	1	1

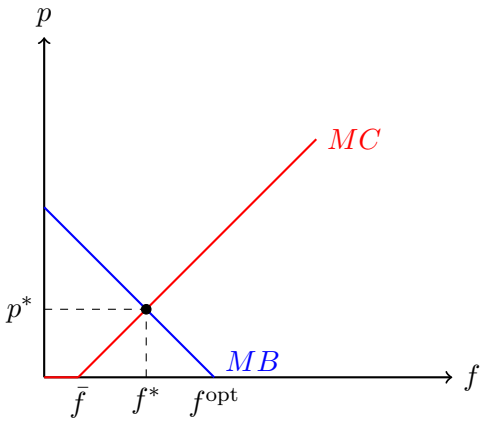
Appendix Figure 33 provides an example of the data entries in the *Review of Legal Education*.

Appendix Figure 34: Model of Law School Admissions

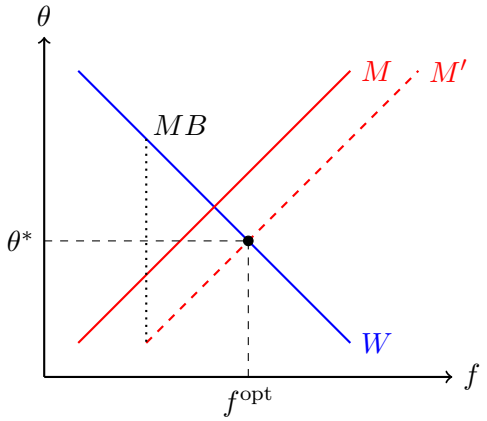
(a) Marginal Benefit: Before Policy



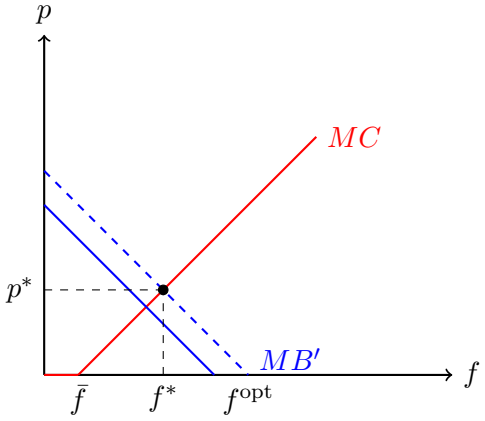
(b) Optimal Enrollment: Before Policy



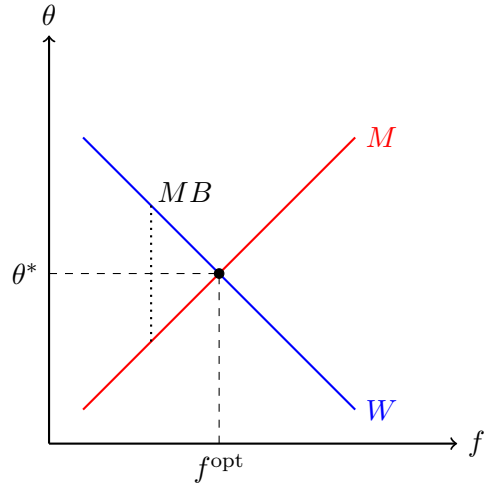
(c) Marginal Benefit: Policy Change



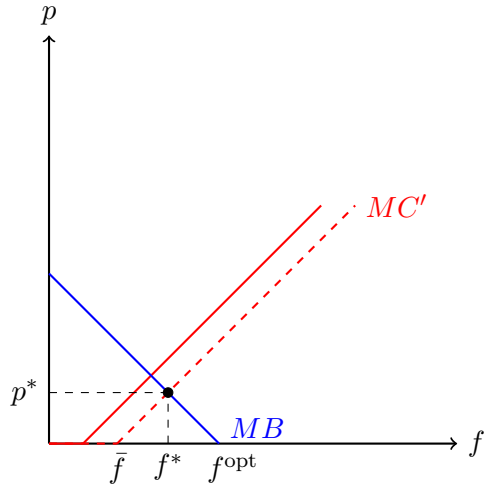
(d) Optimal Enrollment: Policy Change



(e) Marginal Benefit: After Policy

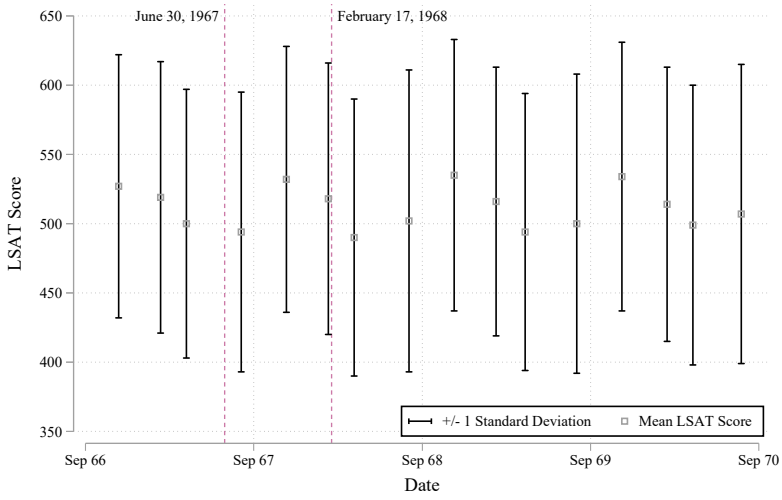


(f) Optimal Enrollment: After Policy

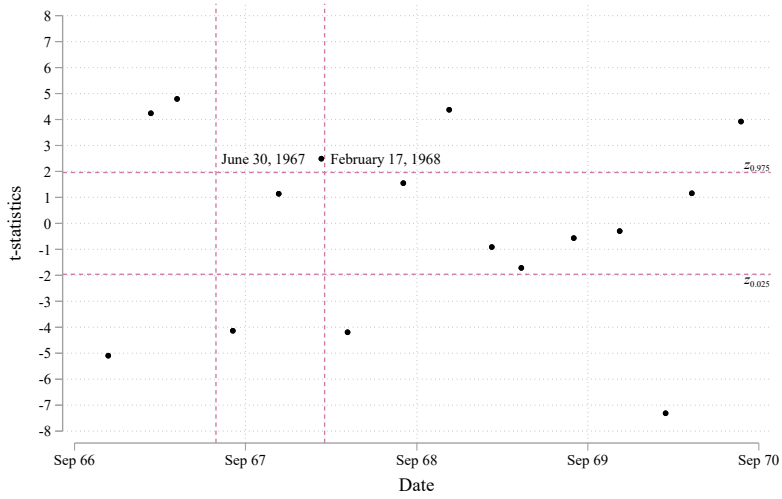


Appendix Figure 34 provides a graphical illustration of our model of law school admissions. See Appendix Section B for details.

Appendix Figure 35: Men's LSAT Scores



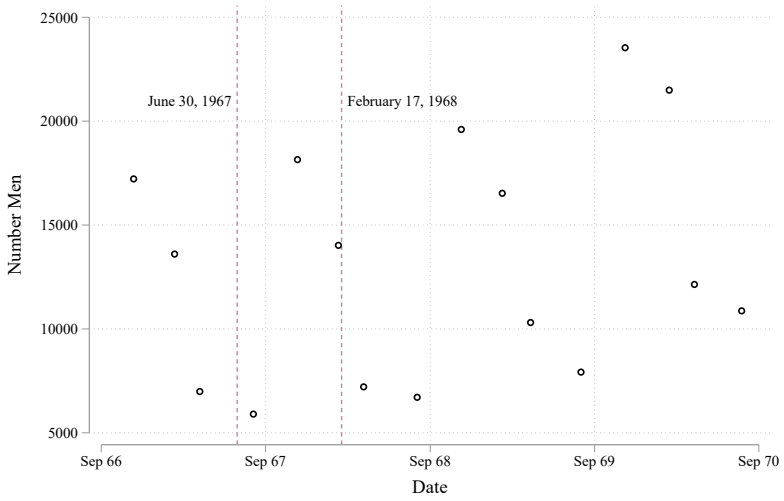
(a) Raw Data



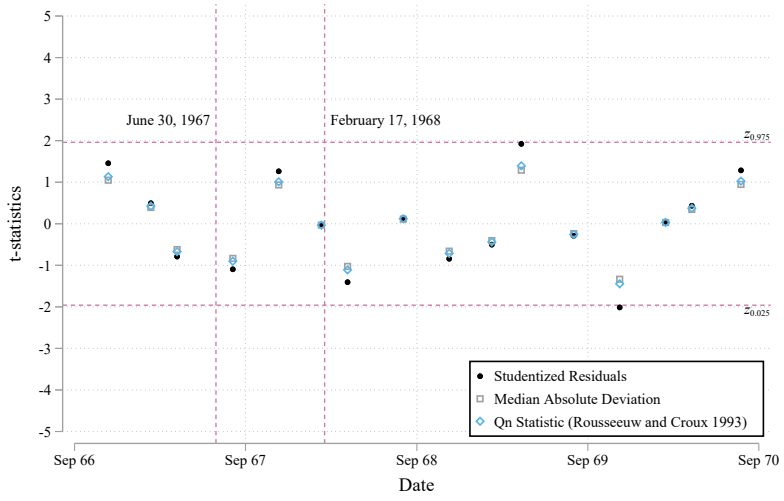
(b) Statistical Tests

Appendix Figure 35a 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 35b 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 36: Number of Men Taking the LSAT



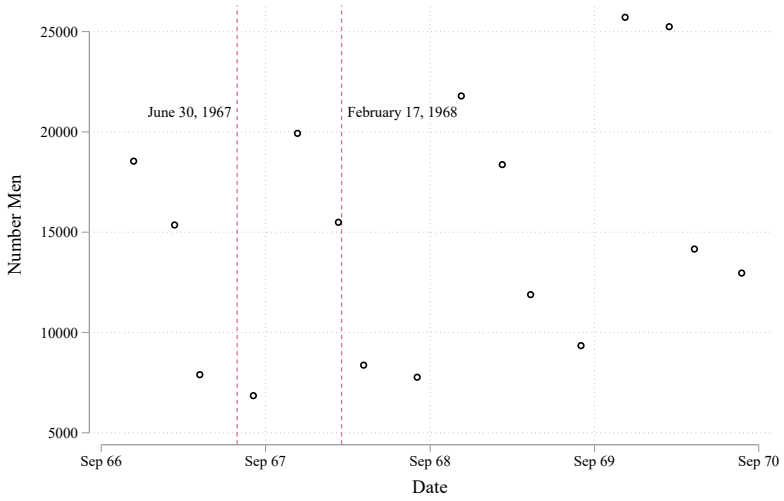
(a) Raw Data



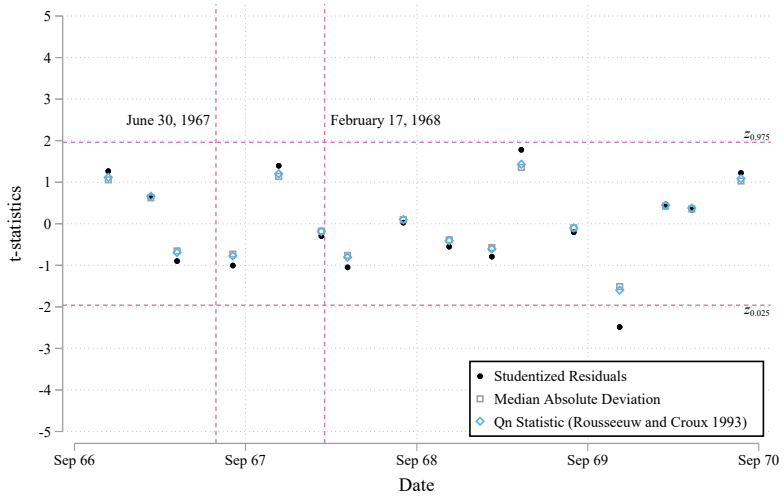
(b) Statistical Tests

Appendix Figure 36a plots the raw number of men who sat for each LSAT exam on the day of each exam administration. Appendix Figure 36b 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 equations (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 37: Number of Men Registered to Take the LSAT



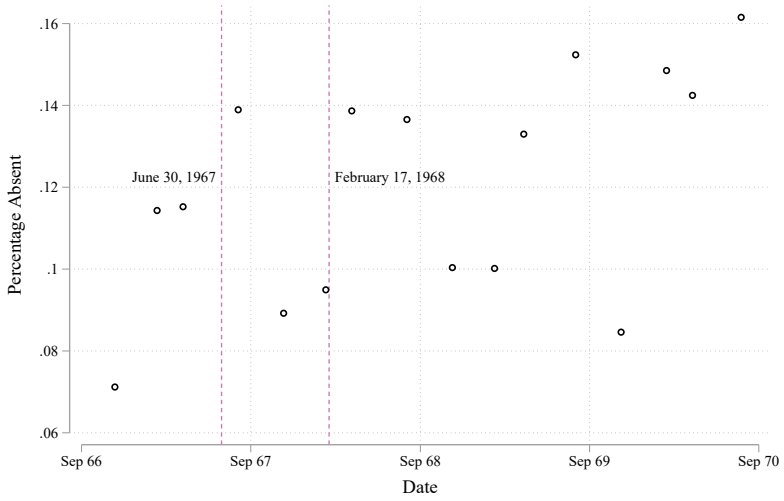
(a) Raw Data



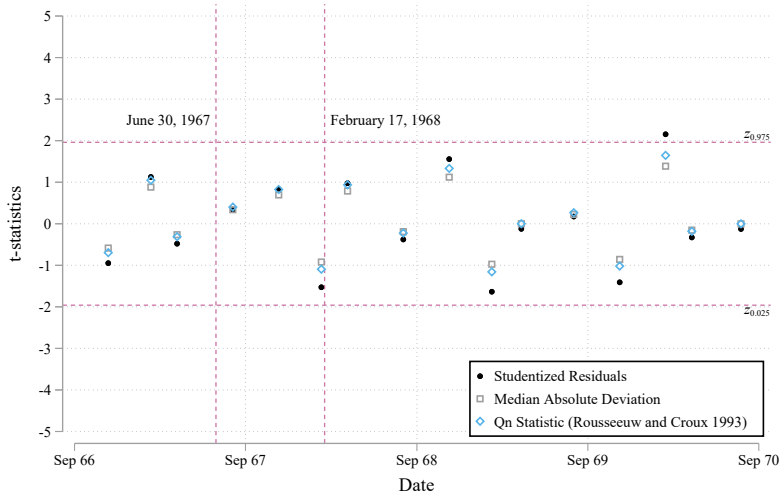
(b) Statistical Tests

Appendix Figure 37a plots the raw number of men who registered for each LSAT exam on the day of each exam administration. Appendix Figure 37b 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 equations (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 38: Fraction of Men Absent from LSAT



(a) Raw Data

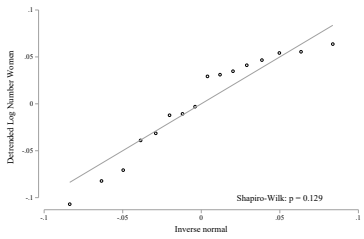


(b) Statistical Tests

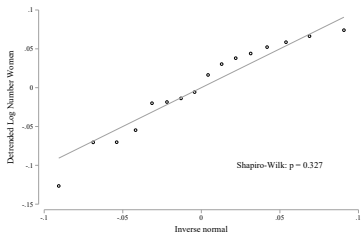
Appendix Figure 38a plots the raw fraction of men who registered for each LSAT exam that did not attend on the day of each exam administration. Appendix Figure 38b plots the studentized residual of the logit transformation of this variable, as well as the normalized deviation from the median utilizing the median absolute deviation and the Q statistic (Rousseeuw and Croux 1993) as the measure of scale, calculated using equations (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 39: QQ Plots

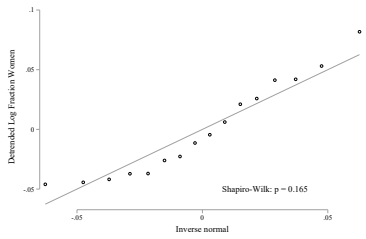
(a) Number Women Attended



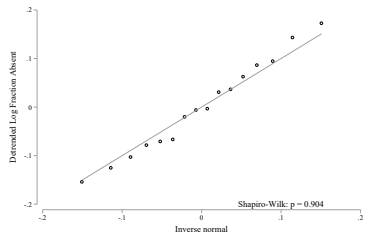
(b) Number Women Registered



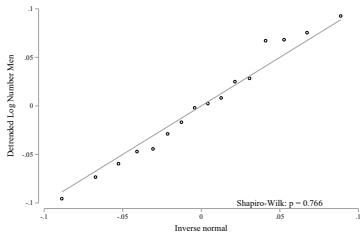
(c) Fraction Women Attended



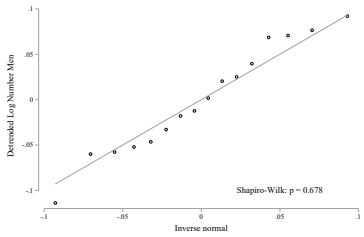
(d) Fraction Women Absent



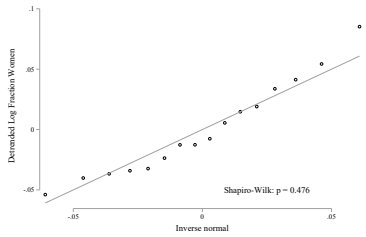
(e) Number Men Attended



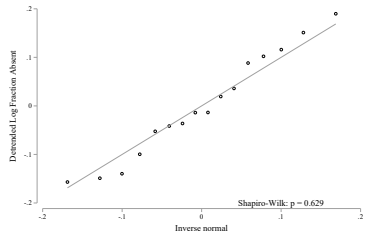
(f) Number Men Registered



(g) Fraction Women Registered

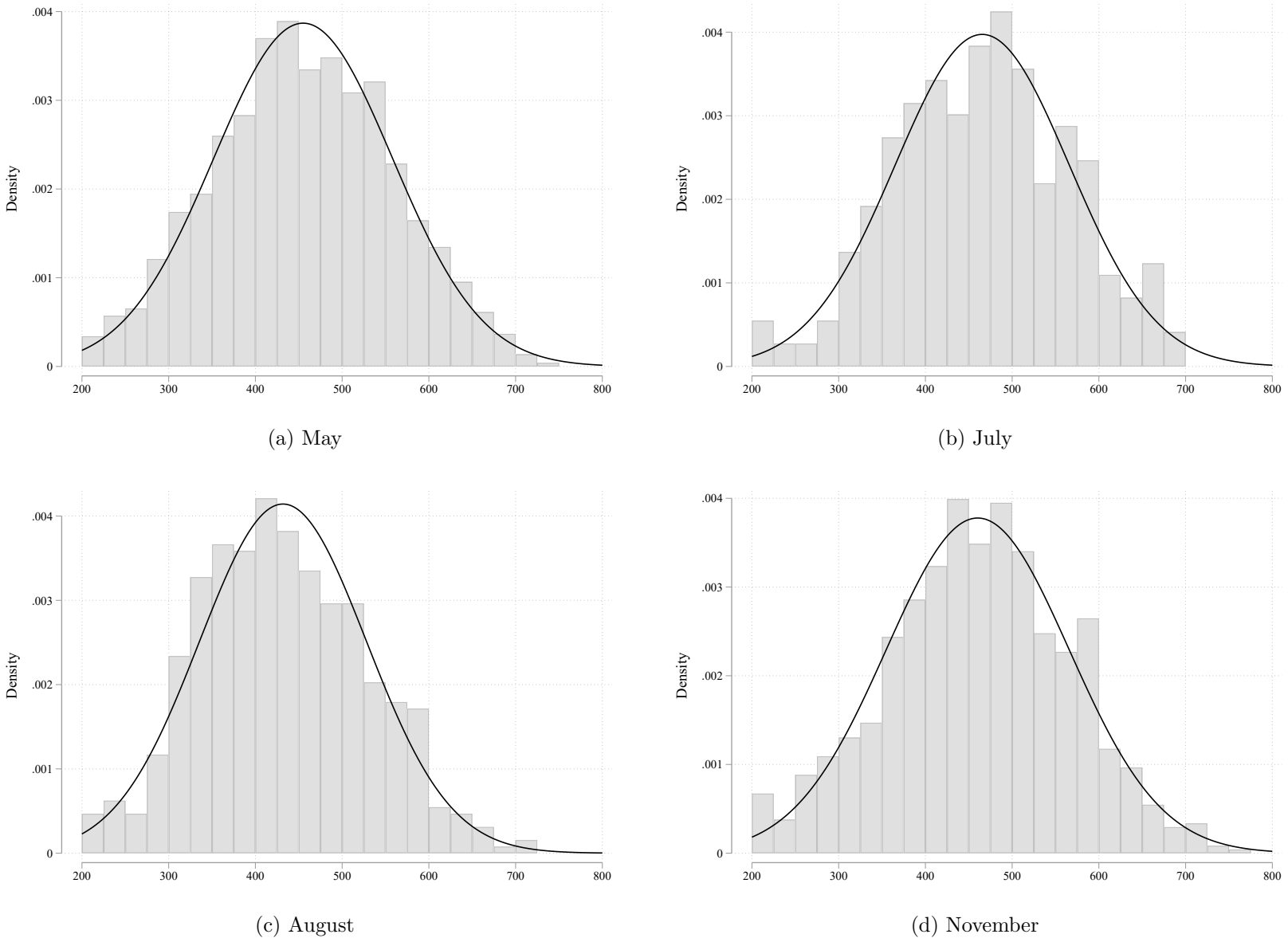


(h) Fraction Men Absent



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 39a), the detrended log number of men attending each LSAT exam (Appendix Figure 39e), the detrended log number of women registered to take each LSAT exam (Appendix Figure 39b), the detrended log number of men registered to take each LSAT exam (Appendix Figure 39f), the detrended logit transformation of the fraction of LSAT exam attendees who are women (Appendix Figure 39c), the detrended logit transformation of the fraction of students registered to take the LSAT exam who are women (Appendix Figure 39g), the detrended logit transformation of the fraction of women who register for but do not take each LSAT exam (Appendix Figure 39d), and the detrended logit transformation of the fraction of men who register for but do not take each LSAT exam (Appendix Figure 39h).

Appendix Figure 40: Normal Approximation to LSAT Distribution



(a) May

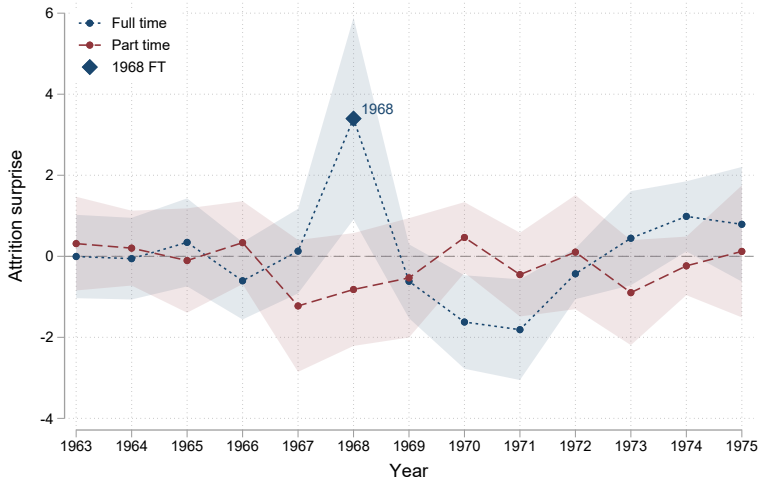
(b) July

(c) August

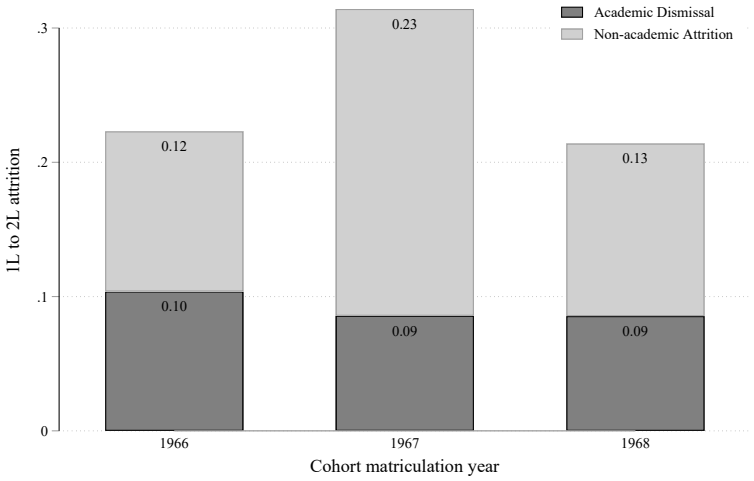
(d) November

Notes. This figure plots data from Swineford (1949), which reports LSAT scores within 25-point bins across four exam administrations in 1948: May 8, July 31, August 14, and November 13. For each exam, the histogram plots the density of exam takers who scored within each bin. The plotted curve is a normal distribution with the mean and standard deviation calibrated to student performance on that exam.

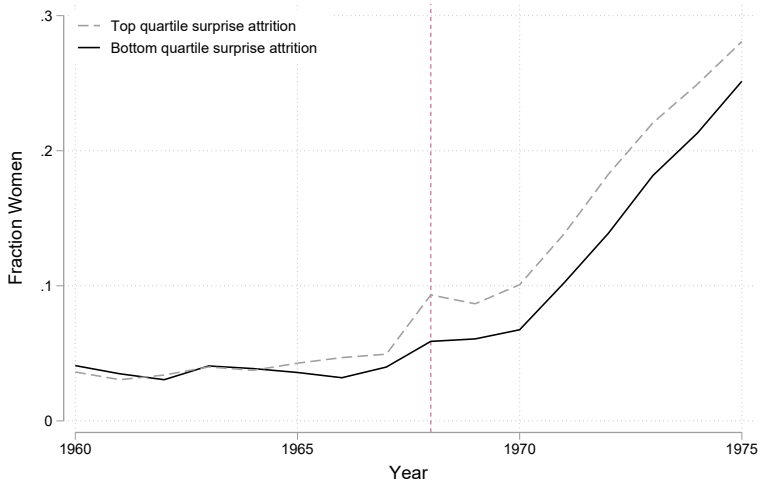
Appendix Figure 41: Variation in Attrition Surprises and Prestige



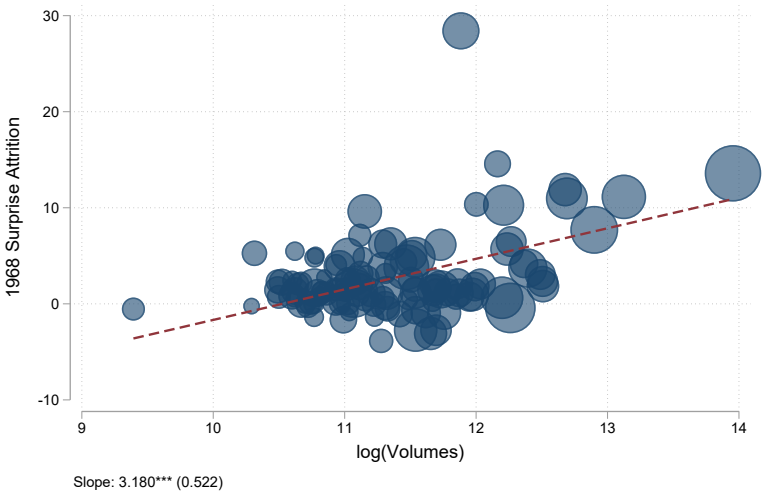
(a) Attrition Surprises Over Time



(b) Academic Attrition



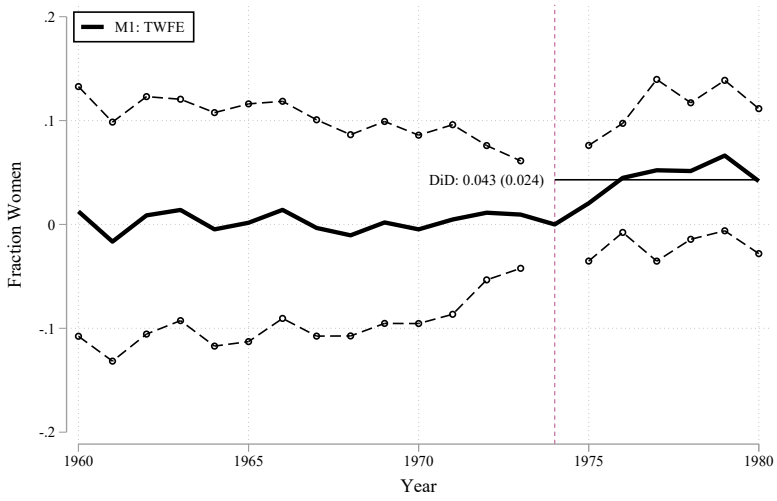
(c) Women's 1L Share by Surprise Attrition Quartile



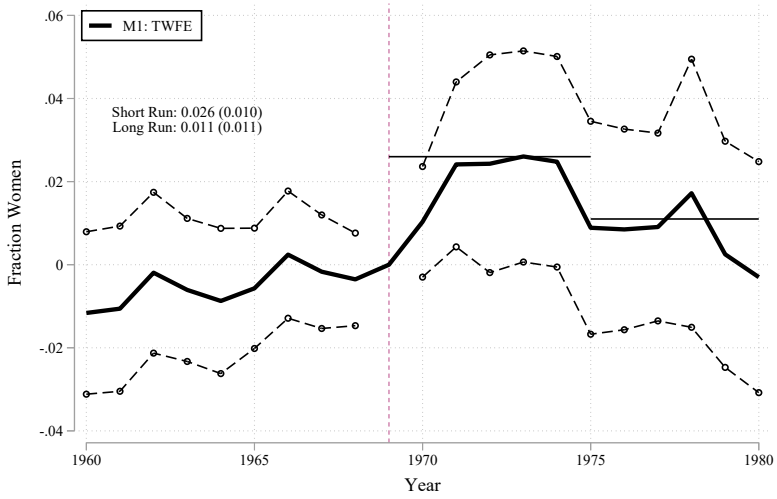
(d) Attrition Surprises and Prestige in 1968

Panel (a) plots the mean standardized attrition surprise $\tilde{S}_i$ for full-time programs and part-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) plots the standardized attrition surprise in 1968 against the log of library volumes in 1970, with marker area proportional to 1967 enrollment and a weighted least-squares fitted line superimposed.

Appendix Figure 42: Anti-Discrimination Causal Results



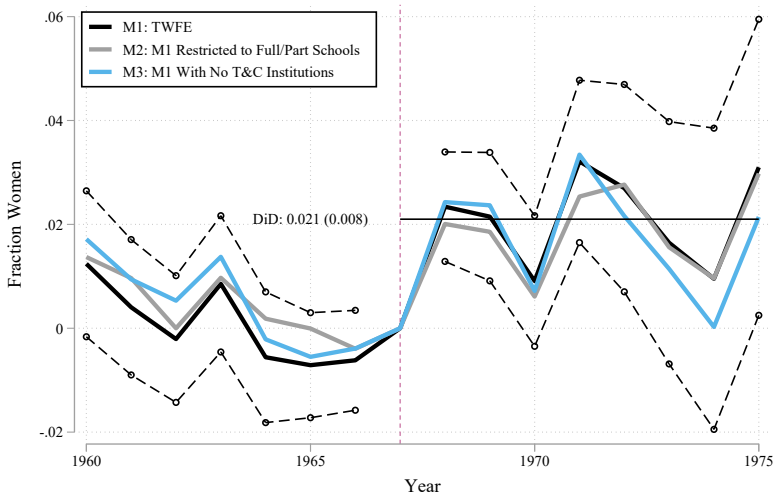
(a) Title IX Event Study Results



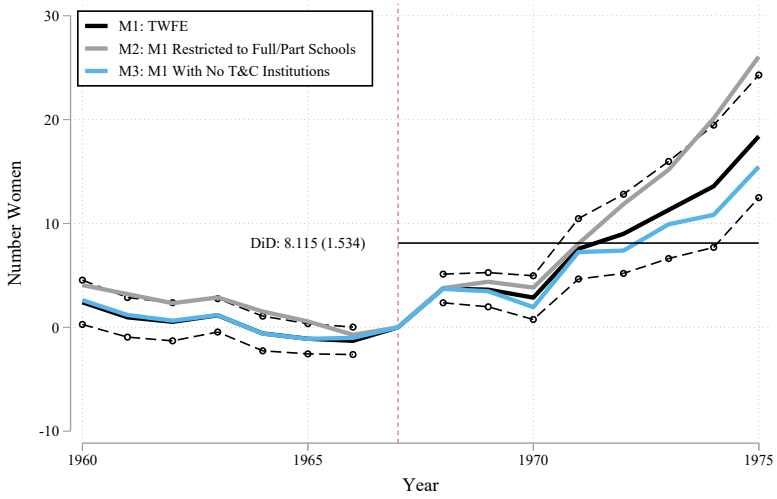
(b) WEAL Complaint Event Study

Appendix Figure 42a plots coefficient estimates from equation (E.1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. We use 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 (E.2). Appendix Figure 42b plots coefficient estimates from equation (E.3), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. We use a standard two-way fixed-effects design, where a 95% confidence interval is plotted for every event coefficient. The horizontal lines plot the short-run (1970-1974) and long-run (1975-1980) difference-in-differences estimates from this model, estimated with equation (E.4).

Appendix Figure 43: Adjusting for WEAL Complaints



(a) Fraction Women



(b) Number Women

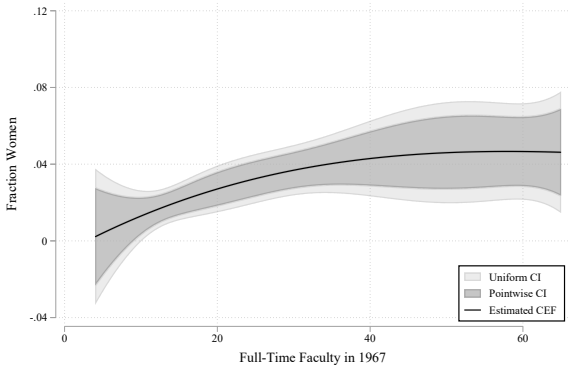
Appendix Figure 43a plots coefficient estimates from equation (1), where the outcome is the fraction of first-year students who are women and the regression is weighted by total first-year enrollment. Model 1 uses institution-program fixed effects and an indicator for institutions receiving an early WEAL complaint interacted with year fixed effects, where a 95% confidence interval is plotted for every event coefficient. The institutions coded as receiving an early complaint are listed in Appendix Table 3. The horizontal line plots the difference-in-differences estimate from this model, estimated with equation (2). Appendix Figure 43b 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 44: Heterogeneity in Law School Size

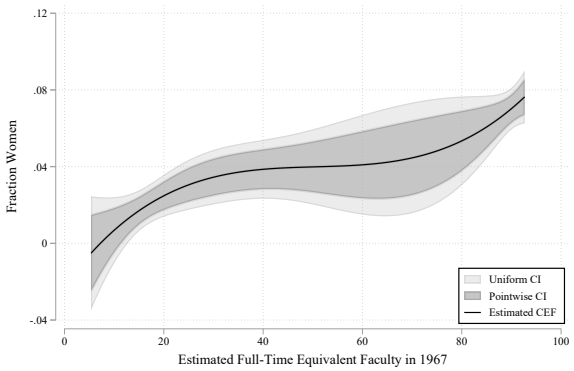
(a) First-Year Enrollment



(b) Full-Time Faculty

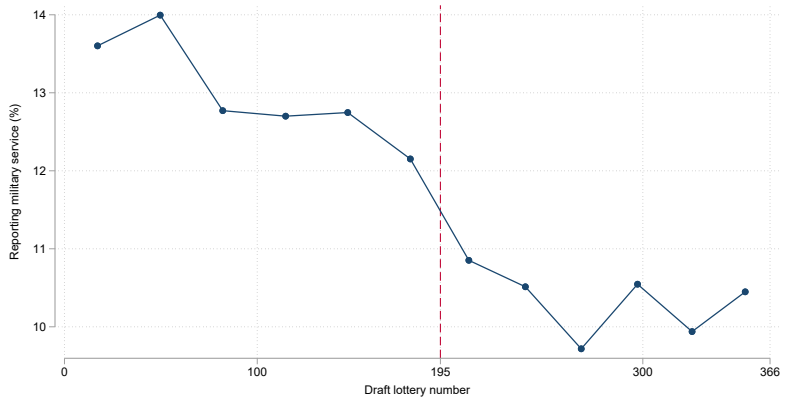


(c) Estimated Full-Time Equivalent Faculty

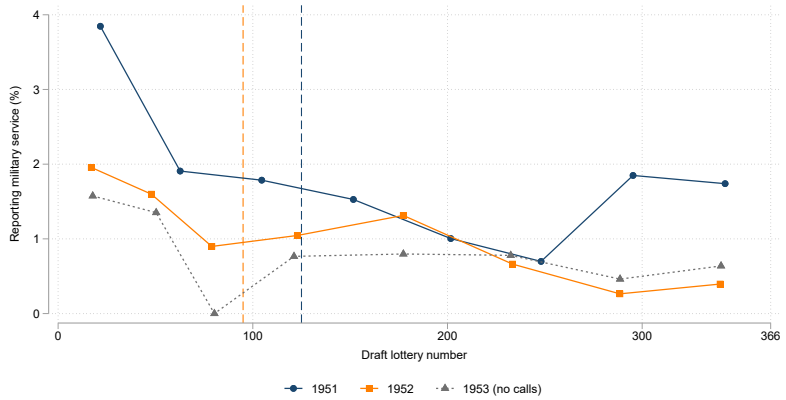


This figure plots estimates from equation (6), where the outcome is the change in the fraction of first-year students who are women between 1967 and 1968 and the regression is weighted by total first-year enrollment in 1967. The dose variable is total first-year enrollment in 1967 in Appendix Figure 44a, the number of full-time faculty members in 1967 in Appendix Figure 44b, and the estimated number of full-time equivalent faculty members in 1967 in Appendix Figure 44c, where the estimate is computed as # Full-Time Faculty + (1/3) × # 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 $\hat{Y}_{ip}$ for treated units ($D_p = 1$) 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).

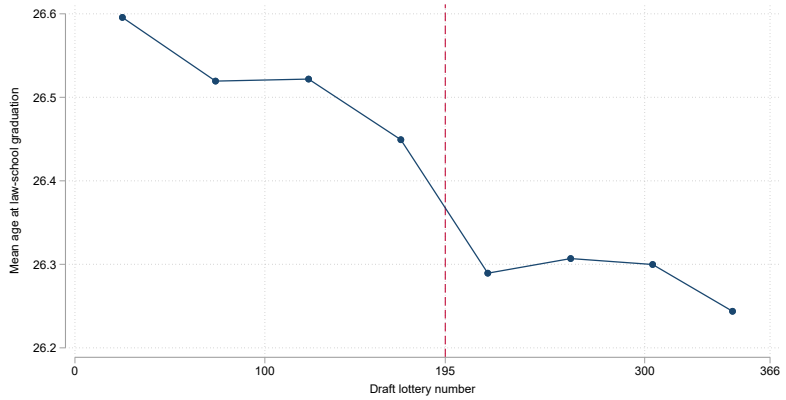
Appendix Figure 45: The Draft Lottery in the 1981 Bar: Reported Service, Graduation Timing, and Partnership



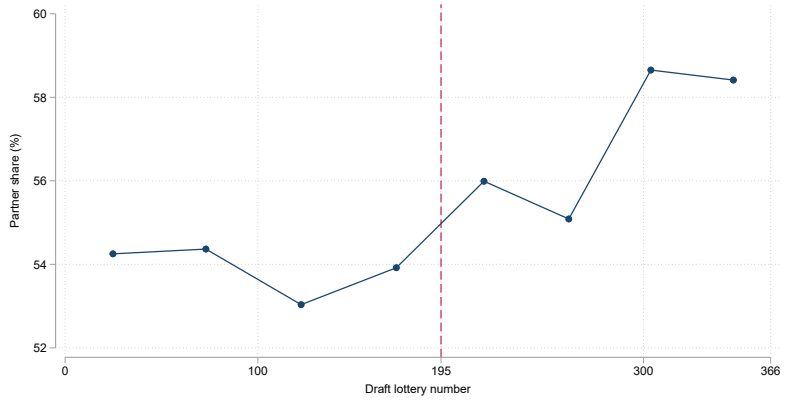
(a) Reported Military Service, Men Born 1944–50



(b) Reported Military Service, Single-Year Birth Cohorts



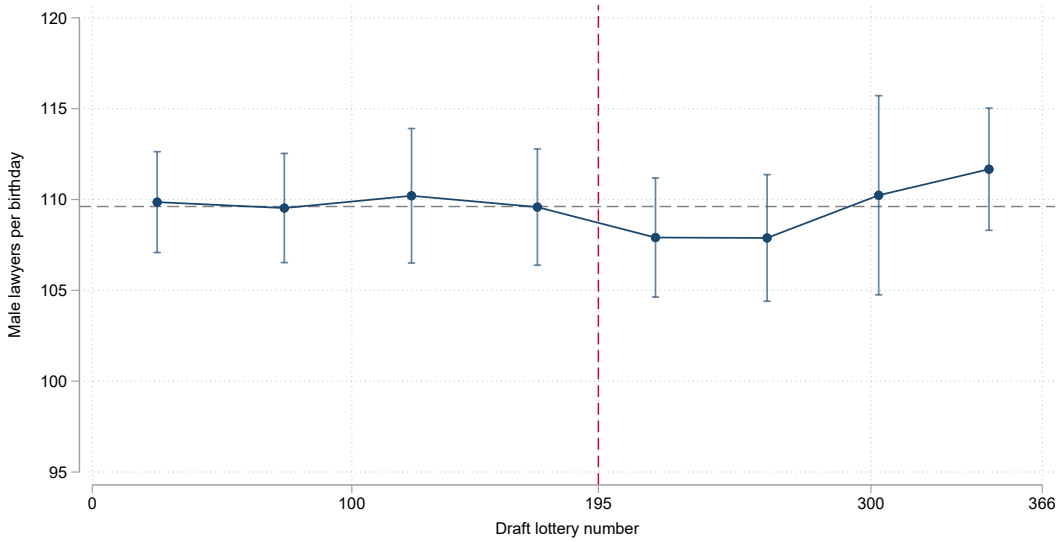
(c) Age at Graduation, Men Born 1944–50



(d) Partner Share at National-P90 Firms, Men Born 1944–50

Notes. Each point is an unadjusted mean within bins containing approximately equal numbers of lawyers, formed separately on each side of the relevant cutoff and only grouping numbers together if they were either both called or both not called in a given year. Dashed lines mark the induction ceilings. Panel (a) uses the 1969 drawing for men born in 1944–50 (ceiling 195). Panel (b) uses the drawings for men born in 1951 (ceiling 125), 1952 (ceiling 95), and 1953 (no actual calls; a comparison cutoff of 95 defines the bins). Panel (c) plots average age at graduation for men born between 1944-1950 by 1969 lottery number bin. Panel (d) plots the partner/member share among men in the same cohorts explicitly classified as members, partners, or associates at firms with at least eight distinct lawyers across observed national offices, the inclusive 1981 90th percentile of private-firm size in the Geographic Bar Roster. Unknown roles or firm sizes are excluded from panel (d). The sample comprises domestic *Martindale–Hubbell* 1981 biographies with valid full birth dates and identified duplicate biographies consolidated. Absence of a military-service report is coded zero.

Appendix Figure 46: The Draft Lottery in the 1981 Bar: Number of Lawyers



Counts pool male lawyers born in 1944–50 and observed in the 1981 *Martindale–Hubbell* Biographical Section by calendar birthday (month and day). The sample contains 40,119 men across all 366 birthdays; February 29 retains its observed count from the two leap-year birth cohorts. Each point averages these pooled counts within a bin. Bins contain approximately equal numbers of distinct lottery numbers on each side of 195 (the vertical dashed line), with membership determined independently of lawyer counts. The horizontal dashed line is the overall mean of 109.61 lawyers per birthday. Error bars are 95% confidence intervals using birthday-clustered standard errors; the pooled data contain at least one observation per birthday. A regression of pooled counts on eligibility with birth-month fixed effects gives 0.03 lawyers per birthday (standard error 1.24). The comparison concerns inclusion in these biographies and does not identify the population effect on entry to law.

Appendix Table 1: Employment in the Legal Profession, 1969

	Shares (%)			Counts			
Outcome	All	Women	Men	Total	Men	Women	Unknown sex
Women (composition)	3.21	—	—	—	—	—	—
Practice setting and specialty							
Solo practitioner / unclassified	43.71	57.35	42.72	132,227	117,686	5,231	9,310
In-house	8.51	5.08	8.72	25,742	24,032	463	1,247
Academic	1.00	1.60	1.00	3,031	2,753	146	132
Patent	1.68	0.57	1.75	5,068	4,829	52	187
Public service							
Federal government	3.56	6.23	3.57	10,603	9,662	555	386
State or local government	2.79	2.86	2.80	8,304	7,597	255	452
Judiciary	3.22	1.86	3.13	9,730	8,610	170	950
Prosecutor	1.85	1.29	1.88	5,601	5,188	118	295
Public defender	0.13	0.13	0.14	400	377	12	11
Legal aid	0.28	1.38	0.25	845	687	126	32
Firm roles and organization							
Associate	7.54	6.64	7.82	22,807	21,546	606	655
Of counsel	0.51	0.19	0.49	1,539	1,355	17	167
Named partner (recorded marker)	13.67	4.35	14.10	41,347	38,850	397	2,100
Large Firm partner	7.69	2.14	7.96	22,607	21,284	191	1,132
Multi-office	1.32	0.50	1.37	3,996	3,772	46	178
Ratings							
Any rating	32.04	13.43	32.31	96,920	89,011	1,225	6,684
Top rating (a v)	11.79	2.94	11.73	35,669	32,318	268	3,083
Total listed lawyers	—	—	—	302,532	275,460	9,121	17,951

Notes. Full eligible domestic cross-section from the 1969 Martindale–Hubbell Geographic Bar Roster, with one representative listing per linked lawyer and no school, admission-cohort, or career-length restriction. All employment shares include unresolved sex and exclude unknown outcomes; women's and men's shares use the corresponding sex-specific evaluable denominator. Women's composition excludes unresolved sex. Counts are recorded positives; categories overlap, so the final row is the full sample rather than their sum. Solo practitioner/unclassified includes residual listings without an identified affiliation. Large-firm partner is a non-associate, including of counsel, at or above national P90 (seven distinct linked lawyers in 1969). National size combines domestic offices and includes percentile ties; unresolved size remains unknown unless another status establishes a zero. Unmatched reviewed firm cards count as zero for the named-partner marker. Unknown government level is excluded from federal and state/local shares.

Appendix Table 2: 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 3: 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 26, 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 Michigan	May 28, 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 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, pp. 336–338) that apply to a law school in our sample. We code a complaint as occurring early if 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. *The coding for LSU law is ambiguous; the complaint is filed against LSU in New Orleans, but the law school is in Baton Rouge. We code this as treated but there is some uncertainty here.

Appendix Table 4: A Martindale–Hubbell analogue of Goldin and Katz (2002)'s Table 5

	(1) GK broad	(2) Bailey	(3) GK + draft risk	
Outcome	Pill access	Pill access	Pill access	Draft risk
Women lawyers per 100 female BA conferrals	0.274***	0.313***	0.545***	1.647***
	(0.068)	(0.068)	(0.097)	(0.524)
Age and edition fixed effects	Yes	Yes	Yes	
Age × edition cells	690	690	690	
Birth-cohort clusters	40	40	40	
Editions	40	40	40	

Notes. Unless otherwise noted, each entry is the coefficient on cohort pill access P_c for birth cohorts 1921–1960 from a separate regression on 690 age-by-edition cells: ages 30–49 and 40 editions in 1960–2000, excluding 1988. Each linked lawyer is counted once per edition. The sample includes the 50 states and DC and requires an observed clean birth year and resolved sex. P_c is the fixed-1970-population-weighted share of states coded as providing access by the year a cohort turned 20; birth cohorts 1939 and earlier are assigned zero. Column (1) uses the dates provided in Goldin and Katz (2002)'s Table 2, assigning each state's treatment as the earliest year in which women under 21 could access contraceptive services without parental consent; column (2) uses treatment dates from Table 1 in Bailey (2006). Column (3) adds a draft-risk index, constructed as follows. Using 1964–1973 induction counts (including calls for the Vietnam War), we allocate each year's count uniformly across cohorts aged 19–24 in that year. Then, for each birth cohort, our draft-risk index is calls per male birth (maximum 0.175, for the 1945 cohort). We report the coefficients on both P_c and the draft-risk index. All models include single-age and edition effects. Our outcome of interest is the number of women lawyers per 100 same-sex BA conferrals. For each birth cohort, BA counts are annual conferrals in year $c + 22$, with 1943/45/47 interpolated, and do not measure college-graduate stocks. Appendix text discusses the degree-definition and age-timing limitations. Standard errors in parentheses cluster by birth cohort; stars use $t(39)$ inference: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Table 5: 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 6: 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 equation (D.6) via OLS, 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 ($\ln \text{Vol}_i$) 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. $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Appendix Table 7: 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

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. 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). The *GK strict* coding dates access under a nonrestrictive rule that allowed 16-year-olds to obtain contraception without parental consent. For the *GK broad* coding, we use the first year in which Goldin and Katz (2002) Table 2 reports that someone under 21 could obtain the pill without parental consent. The *Bailey* coding uses the independently collected dates in Table I of Bailey (2006), which records the first year unmarried, childless women under 21 could legally obtain the pill without parental consent.

Appendix Table 8: School Prestige and the FT/PT Difference in Women's Representation

Specification	Women's share DD (p.p.)	FT schools
All-school baseline	6.5***	48
	(2.3)	
Prestige/missing trends	5.7**	48
	(2.8)	
Omit top FT tercile	6.3**	34
	(2.4)	
Bottom FT half versus PT	5.9**	22
	(2.6)	

Notes. The outcome is women's share among lawyers of known sex, measured six years after bar admission with one observation per lawyer. The estimates compare the FT-only versus PT-only difference between the 1956–69 and 1972–79 admission cohorts, excluding 1970–71. All models include school and annual bar-admission-cohort fixed effects. Effects are in percentage points, with school-clustered standard errors in parentheses.
Prestige is log 1970 law-library holdings. Five schools missing from that survey use 1969 holdings increased by the median growth among 127 overlapping MH schools (5.6%); other missing values remain unassigned. The trend specification retains all 65 schools and interacts a linear cohort trend with centered observed log prestige and a missing-prestige indicator. FT terciles and halves are defined among the 43 FT schools with prestige data, giving each school equal weight. Omitting the top FT tercile excludes 14 ranked FT schools and retains unranked FT schools. The bottom-half comparison includes the 22 FT schools at or below the median and excludes unranked FT schools. All specifications retain all available PT schools. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Table 9: Draft-Lottery Associations by Lottery-Number Group, Men Born 1944–50

Lottery numbers	Military service	On time	Graduation age	Partner vs. associate: national firm P90+
1–95	3.2***	−4.7***	0.19***	−2.7***
	(0.4)	(0.6)	(0.03)	(1.0)
96–125	2.3***	−4.3***	0.14***	−3.7**
	(0.6)	(0.9)	(0.04)	(1.5)
126–160	2.6***	−4.1***	0.15***	−3.0**
	(0.6)	(0.9)	(0.04)	(1.4)
161–195	1.8***	−2.6***	0.09**	−3.3**
	(0.6)	(0.9)	(0.04)	(1.4)
196–230	0.3	−0.2	0.03	−1.2
	(0.6)	(0.9)	(0.04)	(1.5)

Notes. Each column reports coefficients on lottery-number-group indicators from one regression, with numbers 231–366 as the omitted group, birth-month and birth-year fixed effects, and heteroskedasticity-robust standard errors in parentheses. Binary coefficients and standard errors are percentage points; graduation age is in years. The sample consists of men born in 1944–50 observed in the domestic 1981 Biographical Section with a valid full birth date; identified duplicate biographies are consolidated. Military service is a reported-service indicator, and on-time graduation means ages 22–26. National-P90 partnership is the partner/member share among explicitly classified members, partners, or associates at firms with at least eight distinct lawyers across observed national offices in the 1981 Geographic Bar Roster. The cutoff is the inclusive 90th percentile across private firms. Sample sizes are 40,119 for service, 39,782 for each graduation outcome, and 14,661 for partnership. Numbers 196–230 exceed the final induction ceiling of 195. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.