Essays on Gender and Labor Economics

by

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“We Made It”

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Out of everything that I have written here, this section was by far the most difficult. It is an impossible task to capture all of the people that have helped me reach this point, and it makes me wonder what exactly my contribution was at all. I have agonized over whether or not I have included everyone who should be listed here, concluding that I have almost certainly failed in this endeavour. The fact is that the amount of people who have inspired me far exceeds what I could hope to write here, but I have tried to at least give a snapshot of what transpired.

This dissertation has gone through several iterations, often times changing wildly in its contents. When I initially wrote my proposal, what is now Chapter 3 was Chapter 1, Chapter 3 was an entirely separate project, and Chapter 1 did not exist. I am wholly indebted to my committee for the transformation that has occurred over the past three years, all of whom have contributed in distinct ways to make this document what it is today. Thank you Betsey, for agreeing to join my committee, at what appeared to be the 11^{th} hour at the time, but continuing on to provide encouragement and advice on all of my work, as well as an invaluable policy-focused perspective that has shaped the way I have thought about these projects. Thank you Pablo, not only for providing much needed assistance on my labor search work, but also for being more than eager to provide very insightful advice on all facets of the dissertation. Thank you Charlie, for agreeing to step in as chair late in my graduate career, guiding this project to completion through rough seas, and for exhaustively pointing out every issue with Chapter 1 so that I would never be surprised on the job market.

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I left for college with a strong conviction that I wanted to study economics with almost no idea of what that entailed. Thankfully, that began to abate once I declared an Economics major—thank you Jane Caldwell for your help navigating both my undergraduate degree as well as the PhD application process. Many faculty at Pitt helped to take what was a budding interest in economic research and shape that into tangible skills I would need down the line.

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ABSTRACT

This dissertation explores the role federal policy can play in rectifying gender inequities in labor market outcomes. The first chapter shows that federal anti-discrimination policy helped to increase women's representation in medical education, contributing to a stark decline in occupational segregation in the second half of the 20th century. The second chapter shows that equal pay policy in the 1960s led to marked increases in women's earnings, staving off an increase in the gender pay gap, especially for lower-earning women. The third chapter makes a theoretical contribution to our understanding of why firms offer parental leave, an amenity at the heart of discussions surrounding the differential labor market impact of childbirth on women and men.

In Chapter 1, I consider the role of federal policy in increasing women's access to medical education. In the 1960s, women comprised under 10% of all medical students, resulting in a lopsided gender imbalance in the medical profession. I find that a host of federal anti-discrimination policies, implemented in the late 1960s and early 1970s, increased women's enrollment by successfully applying pressure to curb sex discrimination in admissions though the threat of revoking federal funding. In addition, this policy amplified the impact of a massive expansion in enrollment through Health Manpower policy on women by allowing them to capture a higher fraction of newly created seats.

In Chapter 2, Martha J. Bailey, Bryan A. Stuart and I consider the success of two landmark statutes—the Equal Pay and Civil Rights Acts—in targeting the long-standing practice of employment discrimination against U.S. women. At the beginning of the 1960s, the gender earnings ratio at the median had dipped below 60%, and the stability of this statistic over the next decade suggested that policy had not done much to alleviate this gap. We revisit this conclusion using two separate causal designs. In our first design, we find that women's wages grew more quickly after 1964 in states that did not have pre-existing equal pay laws, where we would expect the effects of federal policy to be the strongest. Then, in our second design, we find larger wage growth for women working in jobs with a higher wage gap, where pay discrimination is more likely to be present.

In Chapter 3, I consider the question of why firms voluntarily offer parental leave as a benefit to employees. The federal government requires covered employers to provide only

12 weeks of job protected unpaid leave through the Family and Medical Leave Act, but many employers provide additional wage replacement and leave time beyond this statutory requirement. I consider a setting where workers and firms search over a collection of submarkets characterized by the posted wage and likelihood of finding an employment match. Firms are homogenous but are able to choose whether or not they offer a job with parental leave. Workers differ from one another in the amount of time they would spend out of the labor market on leave after childbirth. I find that firms will only offer leave when the duration of expected leave is below a particular threshold. This threshold is given at the point where the benefit of leave, given by hiring savings, is higher than the cost of retaining a worker on leave net of pass through to the employee.

CHAPTER I

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

1.0 Abstract

During the 1970s, women's representation in medical schools grew rapidly from 9.6% of all students in 1970 to 26.5% in 1980. This paper studies the role of federal policy in increasing women's access to medical training through two distinct channels: pressure to curb sex discrimination in admissions and a massive expansion in total enrollment through Health Manpower policy starting in 1963. To study this, I construct a novel school-by-year data set with enrollment and application information from 1960 through 1980. Using a continuous difference- in-differences design, I find that medical schools respond to the threat of losing federal contracts by increasing first year enrollment of women by 3 seats at the mean, which explains 19% of women's gains between 1970 and 1973. Further, I provide evidence that year-to-year expansions explain around 33% of women's gains from 1970 to 1980.

1.1. Introduction

The most prominent federal action pursuing gender equity in higher education is Title IX of the Education Amendments Act of 1972, which broadly prohibited discrimination on the basis of sex for any institution receiving federal funding. However, this was the culmination, rather than beginning, of activist efforts to pressure the government to take action that would continue through the decade. Title VI of the Civil Rights Act of 1964 had prohibited discrimination by any institution receiving federal funding, but sex was not included as a protected category,[1] and educational institutions were explicitly exempt from

[1] This similarity was not accidental—when drafting what would become Title IX, Rep. Edith Green initially wanted to amend Title VI to include sex, but was dissuaded over fears that this would inspire other regressive changes to the law (Suggs, 2006).

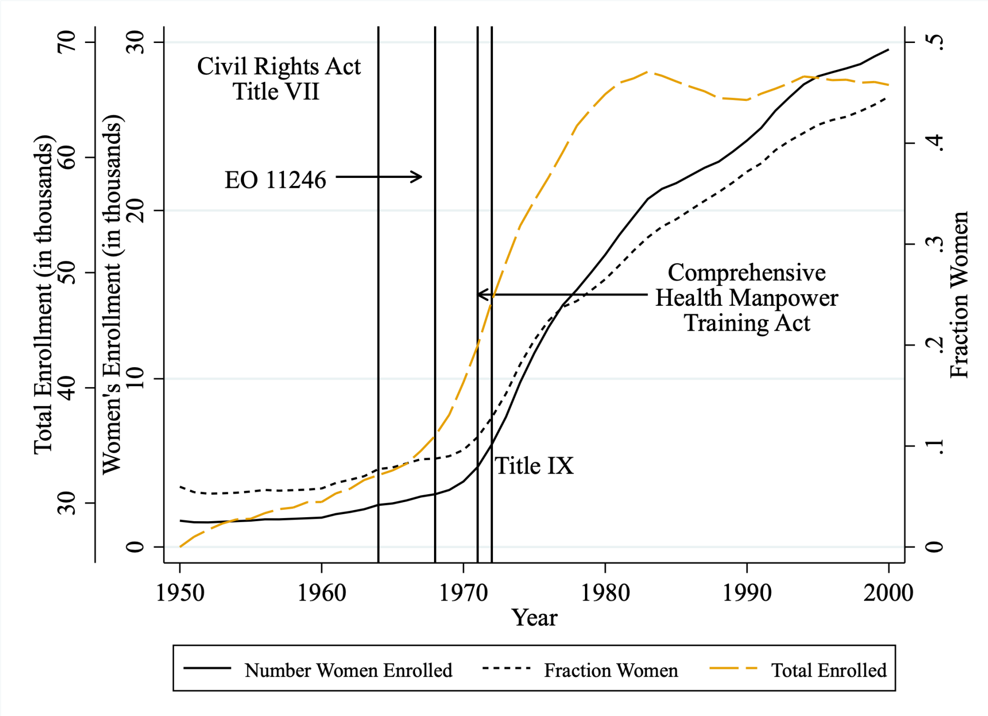
the employment nondiscrimination provisions in Title VII. This would change with Executive Order 11375 in 1967, which amended Executive Order 11246 to prohibit federal contractors from discrimination in hiring on the basis of sex. Recognizing that many institutions of higher education were recipients of federal contracts, EO 11246 was utilized by the Women's Equity Action League (WEAL), led by Bernice Sandler, to file around 250 complaints of noncompliance against colleges and universities, several of which led to investigations resulting in the withholding of federal funding. This paper will argue that it was this push that sparked women's entry into medical schools in the early 1970s, combined with a successful effort to codify sex nondiscrimination through the legislature and amplified by a massive federal push to expand medical school enrollment in the 1970s.

Beginning in the early 1970s, women began to enroll in medical schools at historic rates. Figure 1.1 plots women's enrollment in both levels and as a percentage of total enrollment at all allopathic medical schools from 1950 through 2000. There is a slight uptick in women's enrollment starting in the 1960, but the growth rate changes abruptly around 1970, and there is a drastic increase in both the number of women in medical schools as well as the fraction of all medical students who are women. Figure 1.1 also plots total enrollment throughout this time period—starting in the mid-1960s, enrollment at allopathic medical schools undergoes a massive expansion, essentially doubling between 1965 and 1980. Many of these seats are filled by women: in an accounting sense, women's gains by 1980 comprised 49% of all seats created between 1965 and 1980, representing a 680% increase in women's enrollment. The expansion in total enrollment was the result of several pieces of legislation under the umbrella of Health Manpower Policy that incentivized growth through construction grants for teaching facilities in conjunction with direct payments to medical schools in exchange for increases in enrollment.

It is well known that anti-discrimination mandates likely played a role in women's progress during this time period, but it has been difficult to identify their impact (Goldin, 2005). The time series evidence points to an sudden, episodic change in the early 1970s. Yet it seems to come too early for Title IX, which was not effective until 1973, to be the principal cause. Data restrictions have also played a role: aggregate statistics have revealed changes in women's attendance at professional schools (Goldin and Katz, 2002), but more detailed institutional enrollment data in the Higher Education General Information Survey (HEGIS)[2] is not available at the degree level until the mid 1970s. Finally, while it is well known that total enrollment increased substantially at medical schools as women began to enter (More, 1999; Boulis and Jacobs, 2008), the relaxing of capacity constraints has been underemphasized by the economics literature studying women's enrollment in this time period.

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

Figure 1.1: Trends in Medical School Enrollment, 1950-2000



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

This paper resolves these issues and provides new evidence that anti-discrimination policy played an important role in women's entry into medicine. To do this, I construct a novel school-by-year dataset from 1960 through 1980 with institution-level first-year enrollment and admissions data split by sex. This allows me to characterize changes in the distribution of women across medical schools during their rapid entry in the 1970s. As this is not possible with aggregate data, I contribute to a nascent literature looking more deeply at women's access to professional schools (Katz et al., 2022). Further, this data allows me to utilize causal inference methods to understand the influence of institution-level changes on women's enrollment, adding to Moehling et al. (2019)'s study of women's access to the medical profession during a period of medical school closings from 1900-1960.

I provide causal estimates of the impact of anti-discrimination policy on women's enrollment in medical school. Reviewing action by the women's movement leveraging government policy to end sex discrimination in higher education, I identify a complaint filed by the Women's Equity Action League (WEAL) in October 1970 as the most likely point in time in which anti-discrimination policy would bite for medical schools. I collect data on the amount of funding provided by the Department of Health, Education, and Welfare (HEW) that would be at stake if a school were to violate this policy. Then, using a continuous difference-in-differences strategy, I show that schools with more exposure increase their enrollment of women at higher rates starting in the Fall of 1971. Specifically, I find that a medical school receiving the mean level of funding increases women's first-year enrollment by 3 seats, accounting for 19% of women's gains between 1970 and 1973. This contributes to a growing literature on the effectiveness of anti-discrimination policy in improving labor market outcomes (Beller, 1979, 1983; Leonard, 1989; Manning, 1996; Bailey et al., 2023) and educational outcomes (Rim, 2021) for women.

Finally, I provide evidence that women were able to capture around 20% of newly created seats in the 1970s, accounting for 33% of their progress during this time period. This analysis is motivated by a simple decomposition of the year-to-year change in women's first-year enrollment, allowing me to estimate the contribution of expansions in the number of seats as well as gains conditional on enrollment remaining constant. This also contributes to our understanding of how changes in the supply of college enrollment affects equilibrium outcomes, which has received little attention in the higher education literature (Blair and Smetters, 2021).

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

1.2. Medical Schools in the 1960s

In the 1960s, it was impossible to deny that women were underrepresented in the nation's medical schools—in each year between 1960 and 1969, women did not account for more than 9% of all medical students enrolled. Table 1.1, reproduced from U.S. Congress (1970), pg. 528, gives a snapshot of enrollment at medical schools in 1966. There are a handful of progressive schools in this time period enrolling proportionally more women than the average by a substantial margin, such as Howard University, Boston University and SUNY Downstate. However, the modal medical school is not very different from the average - as this table makes clear, by and large, women constitute a very small fraction of enrollees that does not differ terribly by institution. In other words, there was not an issue of access to a particular set of medical schools, but rather access to any medical school, with the exception of Women's Medical, which exclusively enrolled women.

At the time, analysts tended to point to gender differences in the demand for medical education, rather than discrimination by the admissions committee, as the central reason why women did not take up medicine in greater numbers (Lopate, 1968; Epstein, 1970). Defenders of the *status quo* were quick to point out that acceptance rates for men and women were consistently similar, arguing that this was evidence that admissions committees did not consider sex when evaluating applications. This argument was formalized by Cole (1986), who found that men were not admitted at higher rates from the entire period between 1924 and 1984.[3]

Despite these arguments, it was not at all difficult to establish that some medical schools were discriminating against women. Beginning in 1958, the Association of American Medical Colleges (AAMC) began publishing *Medical School Admission Requirements*, a yearly periodical intended to help prospective students in the application process. Included in each year starting in 1959 is a table containing preferences for each school over applicant characteristics, including sex, race, residency and age in earlier years. In 1960, 21 medical schools (out of 86, excluding Women's Medical) reported that they considered applicant sex in the admissions process; by 1970, this had dropped to 4 schools, but was still being reported by the AAMC.

What was less clear was the extent of the problem. In 1969, Women's Medical first began to consider male applicants, a decision that met resistance from alumni worried that it would compromise opportunities for women to study medicine provided by a women-only institution (U.S. Congress, 1971, pg. 563). To investigate the severity of the problem, the dean of Women's Medical interviewed admissions officers at 25 Northeastern medical schools, finding that 19 "admitted they accepted men in preference to women unless the women were demonstrably superior" (U.S. Congress, 1971, pg. 872), suggesting that many schools acted in a discriminatory manner without admitting formally to preferences over sex.

Lopate (1968) reports that discrimination against women at medical schools manifested in a very particular way: "Prejudice against accepting women continues to exist, except that

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

Table 1.1: Medical School Enrollment by Sex in 1966

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

Reproduced from U.S. Congress (1970), pg. 528.

it is directed toward some future point when the 'minority group' might begin to apply in greater numbers." This was driven by a legitimate concern over an expected shortage of physicians in conjunction with an expectation that women were less likely to practice after graduation. In the words of an admissions officer,

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

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

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

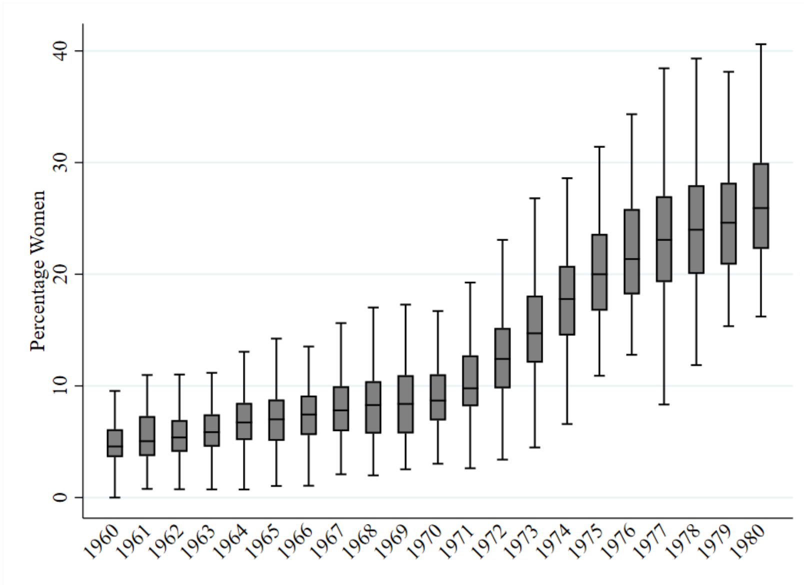
This concern was compounded by higher attrition rates for women, though this was perversely at least partially the result of a male-dominated academic climate that was hostile towards women (Lopate, 1968). Interestingly, though, while attrition for female medical students was higher than their male counterparts,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

1.2.1 Changes in the 1970s

As Figure 1.1 demonstrates, the *status quo* begins to dissolve in the 1970s as women entered medical schools in far greater numbers than before. To characterize the nature of this transition, I begin by establishing several stylized facts. I collect institution-level data on enrollment by sex at every medical school between 1960 and 1980 in the *Journal of the American Medical Association*'s Education Number.[4] Similar to Katz et al. (2022), I characterize entry with respect to two margins: representation among all medical students and overall access to medical education. Figure 1.2 plots the distribution across medical schools of the fraction of their students who are women. We see that women's representation increases across the board at all medical schools between 1970 and 1980, as evidenced by

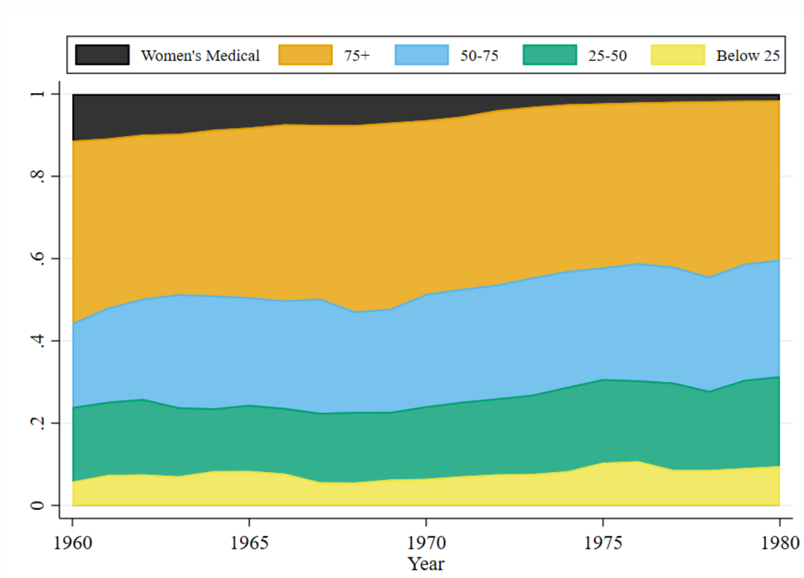
[4] In Appendix A.1.1 I discuss construction of this dataset in more detail.

Figure 1.2: Evolution of Women's Representation



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

Figure 1.3: Evolution of Women's Access



For each year, I calculate the 25$^{\text{th}}$, 50$^{\text{th}}$, and 75$^{\text{th}}$ percentile of the distribution of the number of women in each school. This figure plots the percentage of women enrolled in schools in each quartile of this distribution. Women's Medical is plotted separately as well.

a shift upwards in this distribution. In particular, we see the most rapid changes between 1970 and 1975, with growth slowing in the second half of the 1970s. Simultaneously, we see a large increase in the variance of this distribution - by 1980, some medical schools have almost reached parity, but at others only 15% of students are women.

Table 1.2: Transition Probabilities Between 1966 and 1980

		1980			
	Quartile	1	2	3	4
1966	Below 25th	29%	29%	33%	10%
	25th - 50th	32%	32%	18%	18%
	50th - 75th	33%	19%	19%	29%
	Above 75th	5%	23%	27%	45%

I divide up medical schools into 4 quartiles in 1966 and 1980, ordered by the percentage of their students who are women. Each cell gives the percentage of schools in the row quartile in 1970 that were in the column quartile in 1980.

It is unclear from looking only at distributional changes how individual medical schools are evolving over time. To understand this, I split schools in Table 1.1 into 4 groups, given by which quartile they fall into measured by the proportion of their students that are women.[5] I then calculate the fraction of schools in each group that end up in each quartile, defined similarly, at the end of my sample period in 1980. Table 1.2 reports these transition probabilities for quartiles of this distribution from 1966 to 1980. For schools with relatively high representation of women in 1970, this status is usually maintained in 1980 - 72% of schools in the top quartile in 1966 end up above the median in 1980. However, this pattern is not maintained in lower quartiles; we see a relatively even spread of schools throughout the distribution in 1980 conditional on residing in each quartile in 1966. This is an important pattern to note. As women enter medical schools, we do not see schools with low enrollment of women catch up to their peers; conversely, more women-friendly schools remain this way at the end of the 1970s, with a substantial increase in the variance of women's representation in the remaining institutions.

Figure 1.3 plots the percentage of women enrolled at institutions in each quartile, with Women's Medical plotted in its own category. In 1960, women's access to medical schools was largely determined by a handful of institutions. Women's Medical enrolled around 10% of all women, and 60% of all female medical students were concentrated at 25% of all institutions. However, substantial progress was made throughout my sample period to increase women's enrollments at other institutions. By 1980, the top 25% institutions account for only 40% of women's enrollment driven by increases in women's enrollment across the distribution below

[5]For this exercise, I include only schools that are in my dataset from 1960 through 1980.

the 75^{th} percentile.

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

1.2.2 Development of Policy

The fight against sex discrimination in higher education, which would ultimately lead to the passage of Title IX, was led early on by Bernice Sandler and the Women's Equity Action League (WEAL). As the 1960s came to a close, Sandler realized that there was already federal policy in place that prohibited sex discrimination in the hiring practices of colleges and universities (Suggs, 2006). In 1965, President Johnson issued Executive Order 11246, which prohibited government contractors from discriminating in hiring on the basis of race, color, religion or national origin. However, this was amended in 1967 by Executive Order 11375 to include sex as a protected category, which went into effect in October 1968. Since most universities receive federal contracts, Sandler reasoned that they would be subject to this regulation. A newcomer to political action, Sandler placed a call to the Office of Federal Contract Compliance (OFCC), where she happened to be put in touch with Vincent Macaluso, who not only confirmed that she was correct but also helped Sandler draft complaints to ensure they would be effective (Fitzgerald, 2020). On January 31, 1970, together with the Women's Equity Action League (WEAL), Sandler filed her first complaint under EO 11246, which called for a compliance review of all universities and colleges, with a specific complaint filed against the University of Maryland.

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

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

1.2.2.1 Medical Schools

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

Eventually, Sandler and Green would succeed with the passage of Title IX in 1972, but a similar ban on admissions discrimination was passed a year earlier for health professional schools. The Comprehensive Health Manpower Training Act (CHMTA), passed in November 1971, was the linchpin of a federal push to increase enrollments at medical schools. It involved a host of programs including direct payments to medical schools in exchange for enrollment increases, matching funds for construction projects, and grants to alleviate financial distress at troubled institutions. All of this funding could now be withheld if a medical school utilized discriminatory practices in its admissions process. The stipulation prohibiting sex discrimination in admissions was not in the original bill on the Senate floor, S. 934, but added later as an amendment which was maintained in the final version of the legislation (U.S. Congress 1970). This addition was likely the result of a successful lobbying effort on the part of the Women's Equity Action League (WEAL), which called for such an amendment during the hearings on S. 934.

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

1.2.2.2 Anti-Discrimination Literature

There has been much work trying to understand if EO 11246, along with Title VII of the Civil Rights Act, had improved labor market outcomes for women and Black workers. Early work utilized a difference-in-differences design comparing the progression of employment at firms with and without federal contracts, finding higher employment growth for Black workers at covered firms (Leonard, 1984, 1990), with similar but small effects for white women (Leonard, 1990). However, there was evidence that Title VII (rather than EO 11246) improved women's earnings (Beller, 1979) and helped their entry into male-dominated professions (Beller, 1983).

Recent work has extended this basic design to leverage variation over time in firm exposure to anti-discrimination policy. Kurtulus (2016) utilizes changes in contractor status over time for a panel dataset of firms, finding effects for Black and Native American men and women concentrated in the 1970s and early 1980s. Miller (2017) builds on this strategy, restricting the comparison group to firms that have never been contractors to avoid bias stemming from dynamic treatment effects (Goodman-Bacon, 2021), finding that there are persistent effects of coverage even after a federal contract is completed. Neumark and Stock (2006) leverage changes in state anti-discrimination laws that predate federal action, finding mixed evidence, including earnings gains for Black workers but reduced employment for women.

In this setting, there is no variation over time in medical school coverage, as EO 11246 complaints are filed against every institution simultaneously, and subsequently policy is passed at the national level. Consequently, I leverage differential exposure to affirmative action given by the amount of funding at stake from violating anti-discrimination provisions. This design builds on an important contribution from Rim (2021), who leverages differences across institutions in the amount of federal funding received to measure the impact of Title IX on changes in women's graduate enrollment. I build on this paper by showing that earlier affirmative action policies, in particular EO 11246 and the Comprehensive Health Manpower Training Act, mattered for institutions of higher education, which has been largely unexplored in the economics literature.

1.2.3 Medical School Admissions Decisions

Before moving to my empirical design, I present a model to motivate the choice of specification in the following section. The model is in the spirit of Azevedo and Leshno (2016), where several simplifying assumptions are made to illustrate the most important features of the results. Consider the admissions problem of a single medical school, which faces mass $f(\theta)$ of female applicants and mass $m(\theta)$ of male applicants, and needs to choose some admissions rule to fill a class of E students. I assume that applicant quality, as appraised by this school, is univariate and given by θ. We can now introduce a measure of discrimination, which I operationalize as a penalty to the score of students in a particular group. Assuming a penalty of size $\tau > 0$ so that a female applicant of quality θ receives a score of $\theta - \tau$, this represents a change in the mass distribution of female applicants given by $f(\theta + \tau)$.

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

$$E = \int_P^\infty f(\theta + \tau)\mathrm{d}\theta + \int_P^\infty m(\theta)\mathrm{d}\theta \tag{1.2.1}$$

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

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

Since $\mathrm{d}E/\mathrm{d}\tau = 0$, it is straightforward to show that $\mathrm{d}P/\mathrm{d}\tau = -f(P+\tau)/(f(P+\tau) + m(P))$. Substituting this into the expressions above gives us that

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

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

1.3. Contract Pressure

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

1.3.1 Medical School Finances

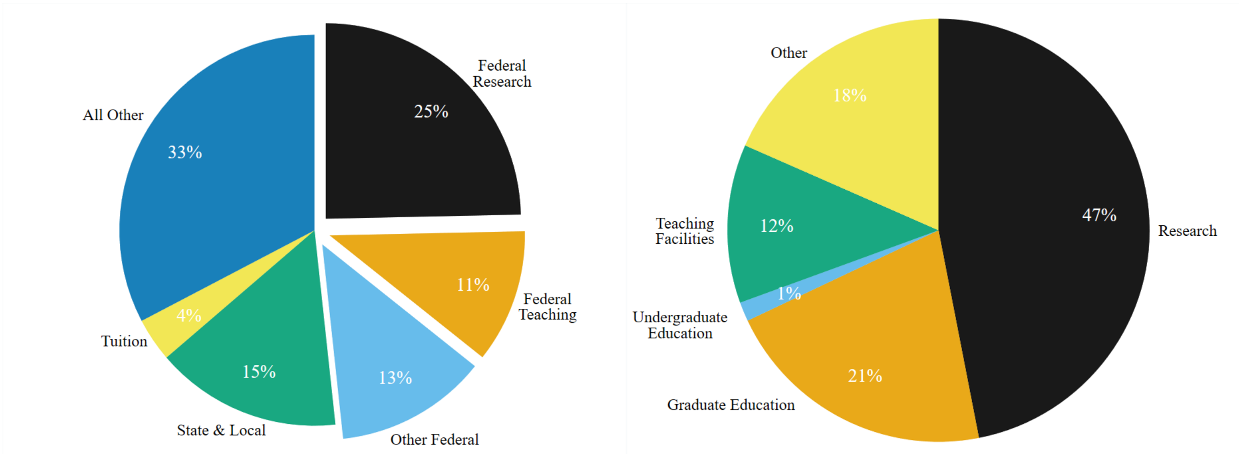
The medical school is a complex entity that has many functions besides classroom education, namely clinical training of both prospective M.D.'s and residents and medical research. These functions are financed through a host of revenue sources, including the federal and state government, tuition payments,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

Institution-level data on revenue is scarce, but aggregate statistics on sources of funding for medical schools are available. In Figure 1.4a, I plot the share of all medical school revenue in 1969 by funding source, taken from Fruen (1983). Funding from the federal government comprises around half of all medical school revenue, with the bulk of this funding provided for research or teaching. This is the most important source of revenue for medical schools, significantly greater than the contribution from state and local government and tuition revenue combined.

Further, by the end of the 1960s, this support had become even more important as an increasing number of medical schools experienced financial distress.[6] The problem had begun

[6] It is worth noting here that raising tuition would likely not have been a viable solution - in 1969, tuition

Figure 1.4: Medical School Finances



(a) Medical School Revenues (b) Source of HEW Support

I plot the percentage of total medical school support by source. All funding from the federal government is "popped out" on the right hand side. The data were collected from Fruen (1983) Table 1, and were collected by this author from the *JAMA* Education Number in various years. I plot the percentage of total medical school HEW support by program. The data were collected from (HEW, 1971).

to reach crisis levels at particular programs, threatening their ability to stay afloat (The New York Times, 1971). To alleviate this, beginning in 1968, the government had been providing financial distress grants for institutions under the health manpower program; by 1970, 61 of the existing 103 medical schools were receiving funding through this program.

To measure institutional reliance on government funding, I collect medical school-level data on the total HEW obligations to medical schools in 1969 (HEW, 1971).[7] This will comprise the bulk, if not all, of federal support to medical schools - in 1969, total HEW obligations of $770m represent 103% of total federal support to medical schools in 1969 (Fruen, 1983; HEW, 1971).[8] Figure 1.4b breaks down this funding by program. The largest funding stream comes through research contracts & grants, which had been the primary way the federal government had supported medical schools for the past several decades (Townsend, 1983). However, as the government pursued its health manpower program in the 1960s, this focus had began to shift to construction support, as evidenced by the funding here for teaching facilities.

My preferred measure of medical school dependence on federal funding is the total amount of HEW support received in 1969, less any construction grants that are given to a school

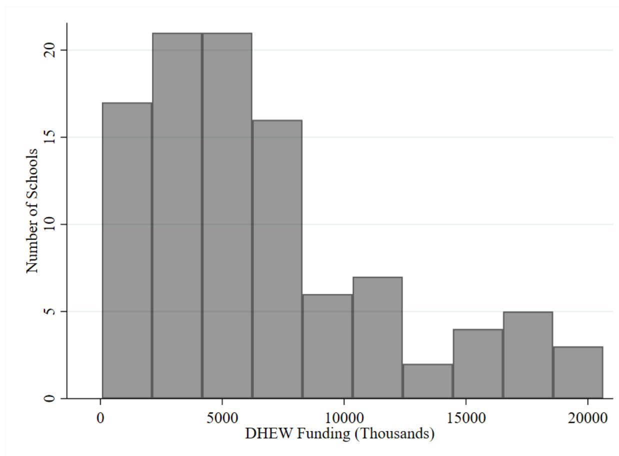
and fee revenue comprised under 4% of medical school financial support (Fruen, 1983).

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

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

in 1969, which are temporary payment that do not reflect continued government support of a school. I plot a histogram of this variable in Figure 1.5. There is substantial variation

Figure 1.5: Distribution of HEW Dose Variable



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

among institutions in the amount of funding received; in particular, this distribution has a right skew, where several institutions receive outsized funding from HEW relative to the mean medical school. Denote this variable $d_{i,1969}$, where i denotes the institutions. To understand if anti-discrimination policy has benefited women's enrollment, I need to measure how the relationship between enrollment and $d_{i,1969}$ has changed over time. However, even if admissions policies adjust rapidly, total enrollment will change slowly, as it is a lagged function of women's admissions. To account for this, I construct an institution-by-year panel of first-year enrollment to obtain a much better metric of changes in medical school enrollment decisions.

1.3.2 Data

I collect a novel institution-by-year dataset from 1960 through 1980. Fortunately, medical schools are unique among health professional schools in that there is consistent historic reporting of institution-level enrollment data. My main source of data is the Study of Applicants published yearly in the *Journal of Medical Education*. From 1967 - 1977, the Study of Applicants reports the number of new entrants, as well as applicants, for each medical school, split by sex. Unfortunately, data reporting from this source stops in 1977, and before 1967, enrollment figures are not split by sex.

Accordingly, to fill a complete panel, I bring in several other sources of data. I am able to collect first-year enrollment[9] in years 1966 and 1978-1980. In 1966, this information is reported in the 1967 *Medical School Admission Requirements*; and in 1978-1980, this is reported in the Education Number, published yearly in the *Journal of the American Medical Association.* To extend the number of pre-periods I can study, I also collect information on estimated new entrants, split by sex, from 1960 - 1965 in the Education Number.[10] Figure 1.6 gives a graphical representation of the dataset I've construction, showing the type of information used for each series in every year. Appendix A.1.2 includes a more detailed discussion of all data sources used.

I summarize some key features of the data in Figure 1.7. I classify observed medical schools as either "established" or "new", in line with HEW designations when awarding grants. The 87 established medical schools include all institutions with positive enrollment in 1960 as well as the California College of Medicine (now the UC Irvine School of Medicine), which I observe enrollment for starting in 1962.[11] The 39 new medical schools report positive enrollment for the first time between 1964 and 1979, leaving me with an unbalanced panel of medical schools. I also plot the percentage of seats in every year that are at established schools. While there is a large push to establish new medical schools, the bulk of seats still remain at established institutions - by 1980, 80% of first-year seats were at institutions that established at the beginning of my sample period.

1.3.3 Methodology & Specification

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

$$Y_{it} = \sum_{\tau=1960,\tau\neq 1970}^{\tau=1977} \alpha_\tau d_{i,1969} \mathbb{1}(t=\tau) + \boldsymbol{\delta}_t' E_{it} + \beta A_{it} + \gamma_i + \delta_{gt} + \delta_{st} + \varepsilon_{it} \tag{1.3.1}$$

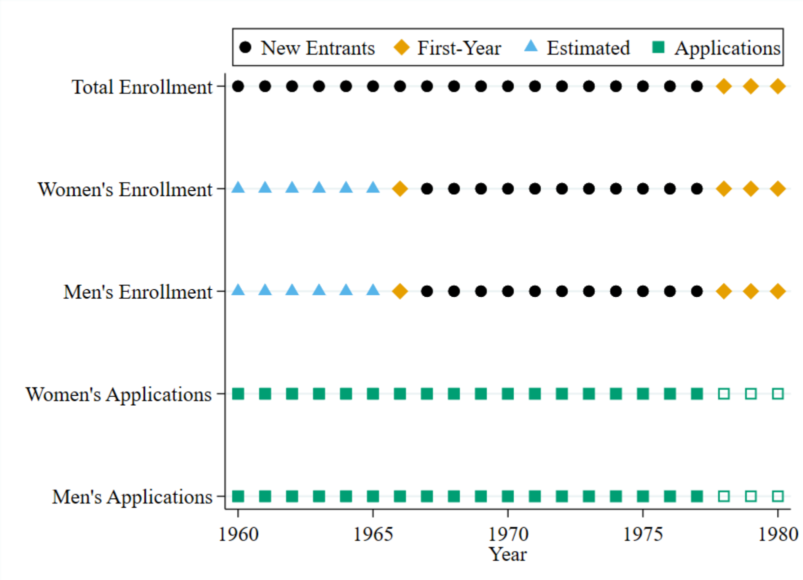
The outcome, Y_{it}, gives the number of women enrolled at institution i in year t. $d_{i,1969}$ is my preferred measure of exposure to the policy, which is interacted with a set of year dummies, omitting 1970. My parameter of interest, α_τ, captures changes in the relationship between HEW funding and women's enrollment. If it was the case that this policy raised women's

[9]This is not equivalent to new entrants as it includes students repeating the first year, though these students represent a miniscule portion of the first year class in medical schools.

[10]These estimates, while published in the Education Number, were first compiled for the *MSAR* in each year. These estimates are made in the spring after a large portion of the application cycle has completed, but there can be differences between these estimates and actual enrollment if, for example, an incoming student drops out. In Appendix A.1.3, I utilize years where estimated and actual enrollment are observed to verify that these estimates are accurate.

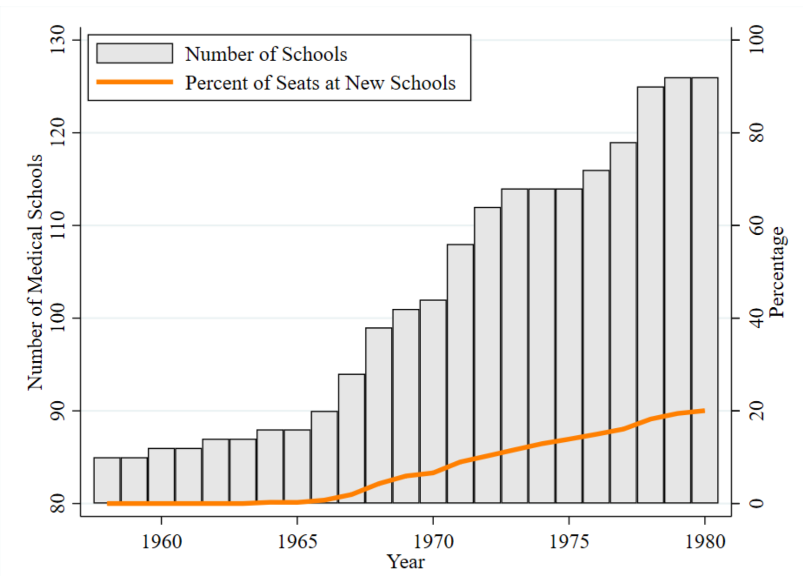
[11]This medical school was established in 1896, but did not become accredited until 1962.

Figure 1.6: Graphical Description of Dataset



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

Figure 1.7: Number of Medical Schools



The bars give the number of medical schools that I observe in every year, where a school is counted if it reports non-missing total enrollment for its first year class. I also include a line indicating the percentage of first-year seats that are at schools I classify as new, which is defined in the text.

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

Recall from the model presented in the previous section that the predicted effects occur only with enrollment held constant. Accordingly, to control for unrelated changes in total enrollment across my sample period, I flexibly control for enrollment by interacting a school's total enrollment E_{it} with year dummies $\boldsymbol{\delta}_t$. My baseline specification includes institution fixed-effects γ_i to control for time-invariant differences in school preferences over women's enrollment and control-by-year (private or public) fixed effects δ_{gt} to account for year-to-year changes in women's demand for medical education that can vary across different types of programs.

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

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

$$\begin{aligned} Y_{it} = {} & \alpha_1^s d_{i,1969}(t-1970) + \alpha_2^s d_{i,1969}(t-1970)\mathbb{1}(t>1970) \\ & + \alpha_3^s d_{i,1969}(t-1970)\mathbb{1}(t>1974) + \boldsymbol{\delta}_t' E_{it} + \beta A_{it} + \gamma_i + \delta_{gt} + \delta_{st} + \varepsilon_{it} \end{aligned} \tag{1.3.2}$$

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

1.3.4 Results & Discussion

These results are presented in Figure 1.8, and transformed spline estimates are reported in Table 1.3. Event coefficient estimates are scaled by the mean of the dose distribution so that they can be interpreted as the number of first-year seats added. For the 10 years prior to 1971, we see almost no change in the relationship between HEW funding and women's enrollment. This changes abruptly in 1971, and gains for women peak in 1973, likely buoyed by the anti-discrimination provisions in the Comprehensive Health Manpower Training Act and Title IX, which are passed in 1971 and 1972, respectively. At the mean, women gain 3 first-year seats as the result of this policy, which is a small but significant increase in enrollment. Across the 101 medical schools, this would create 303 first-years seats, accounting for around 20% of women's gains between 1970 and 1973, which translates roughly to an increase in enrollment of 1200 women across all years of schooling. Model 2 accounts for changes in men's enrollment, which changes the coefficient estimates very little, suggesting that increased demand from men between 1969 and 1973 did not affect women's entry in the early 1970s. Including state-by-year fixed effects introduces a bit of noise into the point estimates, but we still see a gain of almost 4 seats by 1973.

The primary threat to identification in this design is that other institutional characteristics, which correlate with HEW funding, might drive differential responses to an unrelated policy. Specifically, with the passage of the Comprehensive Health Manpower Training Act in 1971, we worry that better funded schools might have expanded enrollment more rapidly, causing an increase in women's enrollment. This hypothesis would also predict increases in men's enrollment in the early 1970s; accordingly, to rule out this explanation, I run an identical design with men's enrollment on the left-hand side.[12]

The results from this design are in Figure 1.9, and spline estimates are reported in columns 4-6 of Table 1.3. Not only does this design rule out enrollment expansion as an alternative explanation, but it also gives insight into the nature of the institutional response. The coefficient for men's enrollment in 1973 is around -3, suggesting that the seats allotted to women as a result of this policy would have been given to men if not for government intervention.

If there is a change in the willingness of medical schools to admit women, does this translate into changes in women's application behavior? There is reason to believe that this information would find its way to prospective applicants. In addition to the formal

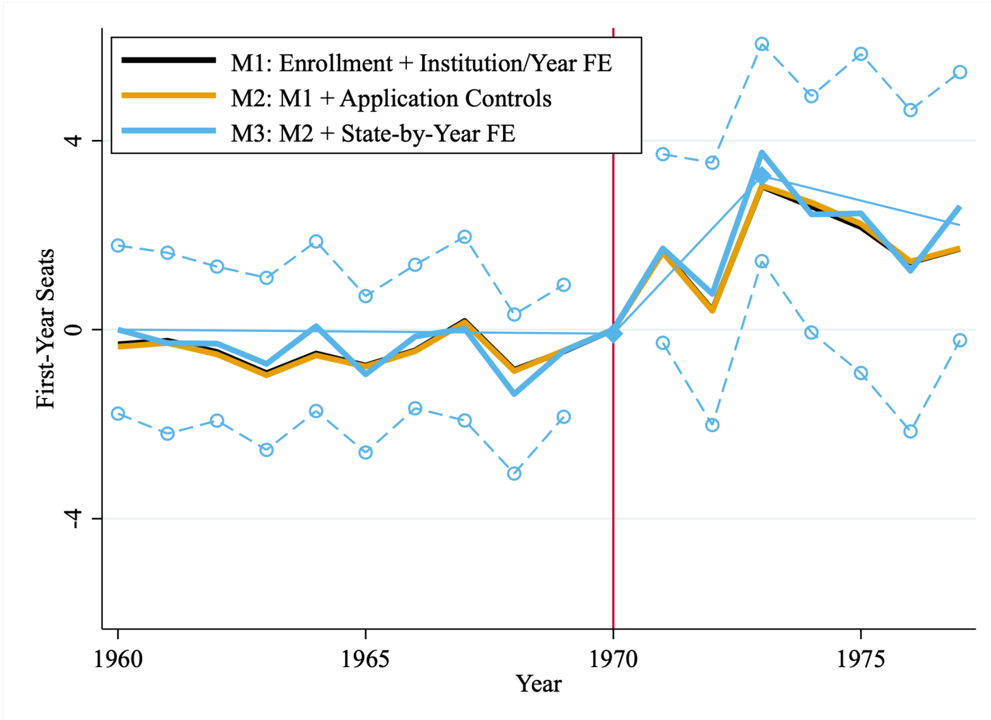
[12] To preserve symmetry, M2 includes the number of applications submitted by women, but since women were not subject to the Vietnam draft, this control does not have the same significance.

Table 1.3: Changes in Enrollment in Response to Anti-Discrimination Policy: Spline Estimates

	Women			Men			Applications	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-Trend Change, 1960-1970	0.027	0.031	-0.009	0.101	0.112	0.089	1.996	1.371
	(0.052)	(0.053)	(0.071)	(0.094)	(0.095)	(0.134)	(1.255)	(1.583)
Spline Estimate in 1973 at Mean Dose	2.839***	2.868***	3.361***	-3.502***	-3.444***	-3.694***	10.532	11.350
	(0.739)	(0.751)	(1.086)	(0.704)	(0.737)	(1.144)	(36.388)	(37.736)
Observations	1701	1701	1299	1701	1701	1299	1702	1299
Total Enrollment	X	X	X	X	X	X	X	X
Men's Applications		X	X		X	X		
State-by-Year Fixed Effects			X			X		X

This table reports transformed estimates from equation (1.3.2). The outcome for columns 1-3 and columns 4-6 are women's first-year enrollment and men's first-year enrollment, respectively, and the outcome for columns 7 & 8 are women's applications. All coefficients are scaled by the mean of the dose distribution so that they give an estimate of the change in seats over a time period attributable to the dose variable. Row 1 reports estimates of the pre-trend slope and Row 2 reports estimates of the cumulative change in seats between 1971 and 1973 adjusted for the pre-trend slope. All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

Figure 1.8: Difference-in-Differences: Results for Women



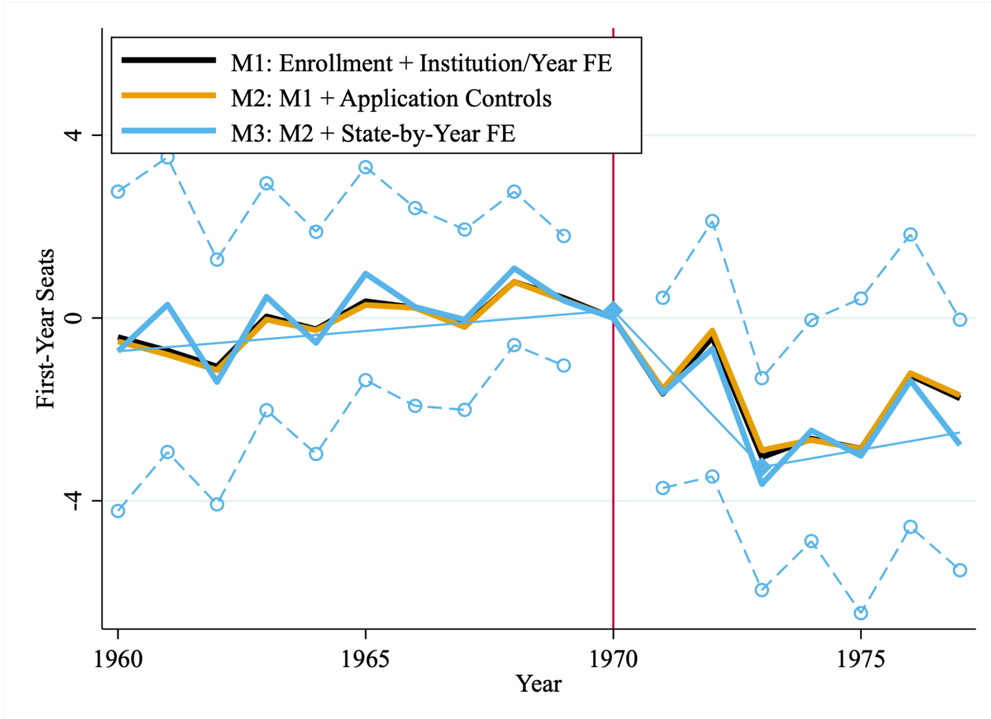
I plot the event study coefficients from equation (1.3.1) scaled by the mean of the dose distribution, where the outcome is women's enrollment. Model 1 includes a flexible control for total enrollment as well as institution and control-by-year fixed effects. Model 2 adds a control for men's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Additionally, I report spline estimates from equation (1.3.2) for model 3. Estimates end in 1977 as application data are not available after this year.

channels mentioned earlier, matriculant data at each school split by sex is generally available in *Medical School Admission Requirements*, which was published for use by prospective students. Further, the introduction of a computerized application system (American Medical College Application Service) in 1971 would have substantially lowered the marginal cost of an additional application, allowing students to respond to institutional changes by filing more applications.

I study changes in the demand for medical education utilizing specification (1.3.1). Y_{it} gives the number of applications filed by women at institution i in year t. My baseline specification includes the same set of controls as model 1 with enrollment as an outcome. This is augmented to include state-by-year fixed effects in a second specification to control for changes in women's educational decisions stemming from differential access to the pill as noted before.[13]Standard errors are clustered at the institution level.

[13]As changes in men's demand do not crowd out women's applications, I do not include men's applications as a control.

Figure 1.9: Difference-in-Differences: Results for Men

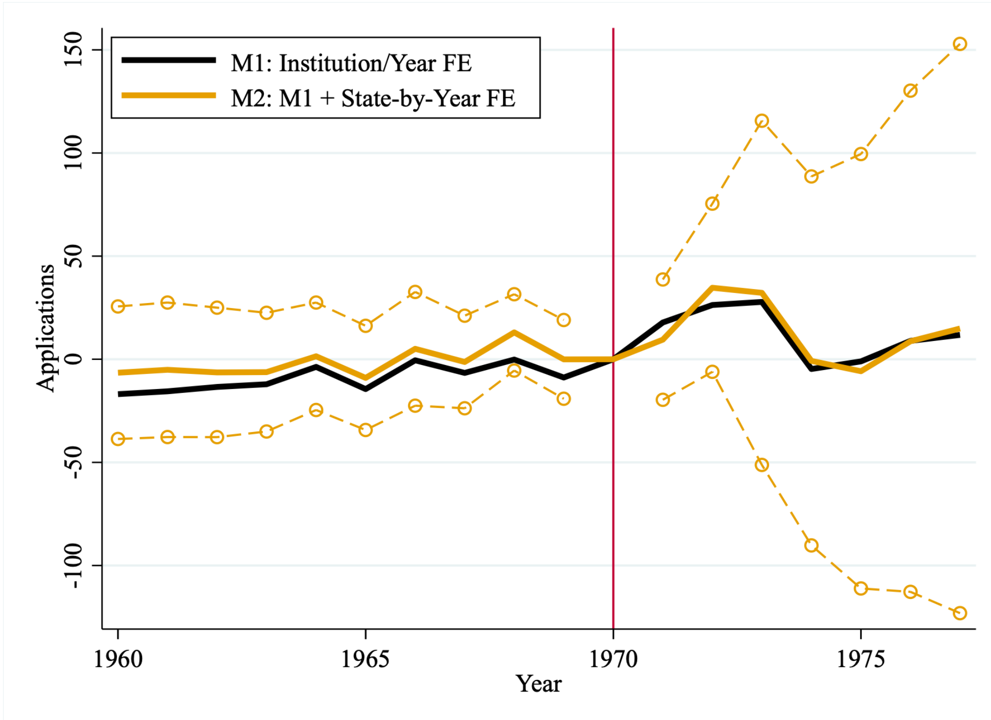


I plot the event study coefficients from equation (1.3.1) scaled by the mean of the dose distribution, where the outcome is men's enrollment. Model 1 includes a flexible control for total enrollment as well as institution and control-by-year fixed effects. Model 2 adds a control for women's applications. Model 3 adds state-by-year fixed effects. I plot a 95% confidence interval for model 3, where standard errors are clustered at the institution level. Additionally, I report spline estimates from equation (1.3.2) for model 3. Estimates end in 1977 as application data are not available after this year.

The results from this exercise are given in Figure 1.10, and spline estimates are reported in columns 7-8 of Table 1.3. Both specifications suggest that women increased application effort at medical schools where women's enrollment jumped by a larger amount in response to the policy, but this effect had subsided by 1975, and inference is hindered by the large amount of noise in my estimates after 1970. In sum, then, what I've found is that anti-discrimination policy both increased women's enrollment at and possibly directed women's applications towards medical schools that were more dependent on federal funding in the short term.

However, in addition to increases in the number of women enrolled in medical schools, we might also care about access to high quality schooling. To look at this, I bring in data from Cole and Lipton (1977), who conduct a survey of medical school faculty in 87 out of the 94 AMA-approved medical schools in 1971. For each medical school, they produce a "perceived quality score," which utilizes this survey data to order schools based on their quality as reported by medical faculty across the country, which I take as a reasonable metric of medical school quality. If there are differential effects across the quality distribution, it

Figure 1.10: Women's Applications



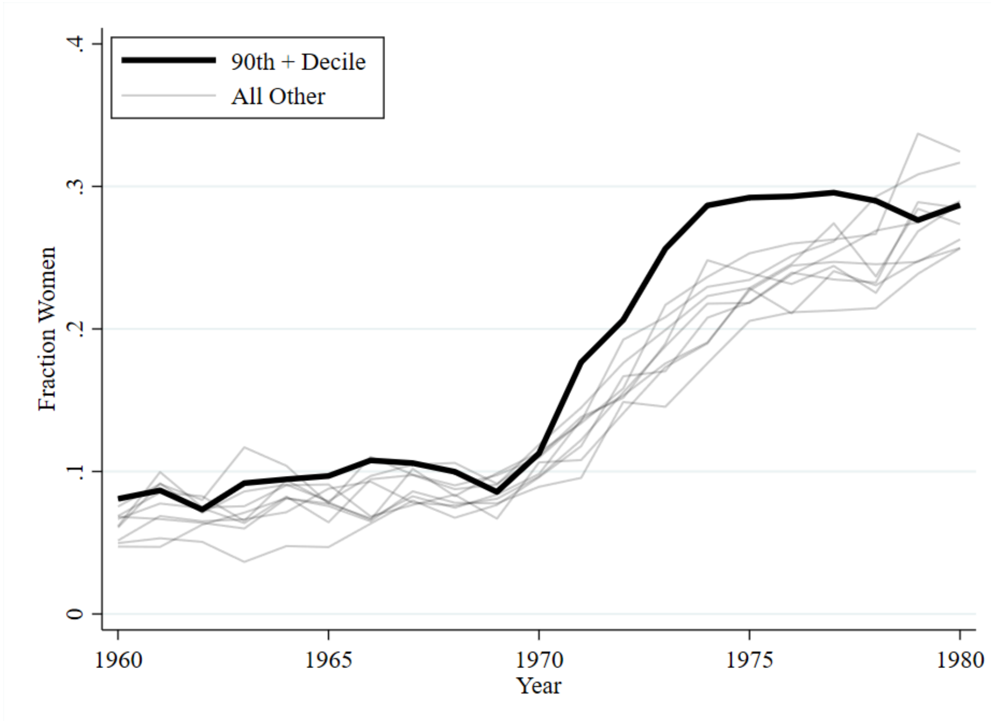
I plot the event study coefficients from equation (1.3.1) scaled by the mean of the dose distribution, where the outcome is women's applications. Model 1 includes a flexible control for total enrollment as well as institution and control-by-year fixed effects. Model 2 adds state-by-year fixed effects. I plot a 95% confidence interval for model 2, where standard errors are clustered at the institution level.

is unclear *ex ante* where these would obtain. So, I look first at the time series and divide schools into 10 groups, based on which decile they fall into in the quality score distribution. For each group, in each year, I calculate the fraction of students across all institutions who are women—Figure 1.11 plots this time series for each decile group across my sample period.

One decile appears to display a different pattern than other schools and is highlighted in Figure 1.11: medical schools scoring in the top decile of the quality distribution, which includes the University of California at San Francisco, Columbia, Cornell, Duke, Harvard, Johns Hopkins, the University of Michigan, Stanford, Washington University in St. Louis, and Yale. Women very quickly make advances in representation between 1970 and 1975, but convergence seems to stall during the second half of the 1970s. On the other hand, for all other deciles, progress is more limited before 1975 but sustained through the end of the decade.

I return to my causal specification to explore heterogeneity in effects for schools at differing points in the quality distribution. To do so, I estimate different event coefficients for "elite" medical schools scoring above the 90$^{\text{th}}$ percentile and "non-elite" medical schools below the

Figure 1.11: Change's Women's Representation Across the Quality Distribution



I divide schools into 10 groups, based on which decile they fall into in the quality score distribution, where the score is taken from Cole and Lipton (1977). Each line plots the fraction of students in each decile who are women between 1960 and 1980. The top decile (90% and above) is in bold.

90^{th} percentile by interacting my linear spline specification in (1.3.2) with a dummy for elite status. The results are given in Table 1.4.

These estimates support the time series evidence—anti-discrimination policy had a larger impact at elite medical schools, allocating around 4 seats for women, as opposed to 1.5 seats at all other institutions. It is important to note that these results are, in some sense, mechanical - as research quality is an input into the perceived quality of a medical school, if institutions that produce better research receive more federal funding, high quality medical schools should receive relatively more federal funding. However, this should not dilute the importance of these results; rather, due to the way in which federal anti-discrimination policy leverages research contracts, we can be assured that women are not kept out of the nation's best institutions.

1.4. Expansionary Policy

In the previous section, I found that anti-discrimination policy increased women's enrollment by around 1200 seats, which explains around 19% of women's gains between

Table 1.4: Differences Across the Quality Distribution: Spline Estimates

	(1)	(2)	(3)
Non-Elite: Pre-Trend Change, 1960-1970	0.025	0.030	0.004
	(0.065)	(0.066)	(0.084)
Non-Elite: Spline Estimate in 1973 at Mean Dose	1.594*	1.593*	1.611
	(0.844)	(0.853)	(1.282)
Elite: Pre-Trend Change, 1960-1970	0.007	0.011	-0.005
	(0.058)	(0.059)	(0.070)
Elite: Spline Estimate in 1973 at Mean Dose	4.125***	4.240***	4.383***
	(0.982)	(0.968)	(0.930)
Observations	1598	1598	1242
Total Enrollment	X	X	X
Men's Applications		X	X
State-by-Year Fixed Effects			X

This table reports transformed estimates from equation (1.3.2), where each spline coefficient is interacted with a dummy for a school scoring above the 90th percentile on the quality score from Cole and Lipton (1977). Rows 1 & 2 give results for institutions below the 90^{th} percentile (non-elite), and Rows 3 & 4 give results for institutions above the 90^{th} percentile (elite). The outcome for all columns is women's first-year enrollment. All coefficients are scaled by the overall mean of the dose distribution so that they give a comparable estimate of the change in seats over a time period attributable to the dose variable. Row 1 reports estimates of the pre-trend slope and Row 2 reports estimates of the cumulative change in seats between 1971 and 1973 adjusted for the pre-trend slope. All standard errors are clustered at the institution level.
*** $p < .01$, ** $p < .05$, * $p < .10$

1970 and 1973. While an important driver of growth during this time period, women's entry continues through the second half of the 1970s, which leaves plenty of room for complementary explanations. I now turn to exploring the role of policy aimed at expanding the capacity of existing medical schools and constructing new medical schools and the interaction between these policies and anti-discrimination legislation.

1.4.1 Development of Policy

Recognizing that in order to increase the supply of health professionals in the 1970s the nation would have to act far earlier, Congress passed the Health Professions Educational Assistance (HPEA) Act in 1963. This legislation created what would become two pillars of health manpower policy: assistance for medical schools, though the provision of construction grants, and aid for medical students by providing student loans. The federal government had, by this point, become involved in the funding of medical schools, but this represented a fundamental shift away from research grants, which comprised the lion's share of federal support by the start of the 1960s (Townsend, 1983). Under the construction grant program, the Department of Health, Education, and Welfare (HEW) would provide funding for 2/3 of the costs for building a new school or expanding an existing one in exchange for several promises from the institution, including that the building would be used for teaching purposes for at least 10 years and a small increase in first-year enrollment (MacBride, 1973b). In addition, the HPEA provided student loans, jointly with medical schools, to defray the increasing costs of medical education.

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

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

At the same time, the financial position of medical schools had become markedly worse, with many schools receiving financial distress grants through the Health Manpower Act. Consequently, Congress looked for a "comprehensive" solution that would stabilize the financial situation of medical schools while incentivizing an increase in enrollment (MacBride, 1973a). This policy took the form of the Comprehensive Health Manpower Training Act (CHMTA) of 1971, where the focus of federal support shifted to Capitation Grants, which provide schools with a set amount of funding dependent on their enrollment, type of enrollment,[14] and number of graduates. As before, to receive this funding, an institution was also required to increase its first-year enrollment by a given amount. In addition, all forms of funding in the CHMTA are tied to a requirement that a school "will not discriminate on the basis of sex in the admission of individuals to its training programs."

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

1.4.1.1 Impact on Medical School Enrollment

The totality of Health Manpower policy is summarized in Figures 1.12 and 1.13. Figure 1.12 plots total enrollment across all medical schools between 1950 and 2000. While Health Manpower Policy is actively supporting medical schools from 1965 - 1980, there is a historic rise in enrollment, with the total number of students approximately doubling during this time period. This stands in stark contrast to period from 1980 - 2000 where total enrollment remains constant after federal support for enrollment increases abates. Figure 1.13 plots the major components of federal funding provided specifically to medical schools. For the first 7 years, the focus was on building new facilities, as most funding was directed towards construction grants. With the passage of the CHMTA in 1971, this switched to Capitation Grants, and special project grants grew in importance as well.

It is difficult to tie observed enrollment increases directly to federal programs, but the time series strongly suggests that medical schools responded strongly to federal incentives to increase enrollment. Construction grants provided by the Bureau of Health Manpower (BHM) were tied to a specific number of first-year seats that a medical school would maintain and increase as a result of the new building: in total, these grants implied an increase of 4,873 seats, accounting for 56% of the observed increase of 8,650 seats between 1965 and

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

1980. Almost every medical school increased enrollment to obtain capitation grant funding in response to the CHMTA: the average school would have to have increased first-year enrollment by at least 10 students, leading to the creation of 1,020 seats through this program alone. Given the difficulties of estimating the direct association between federal programs and enrollment increases, I focus on identifying the reduced form relationship between enrollment changes and women's enrollment.

1.4.2 Returning to the Model

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

Totally differentiating the equation for enrollment (1.2.1) gives us that

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

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

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

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

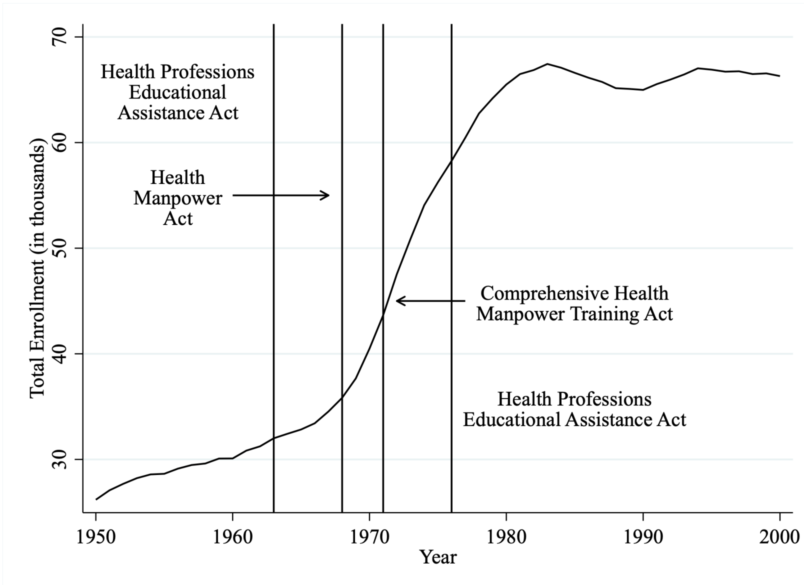
$$\mathrm{d}F = \frac{f(P+\tau)}{f(P+\tau)+m(P)}\mathrm{d}E \tag{1.4.1}$$

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

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

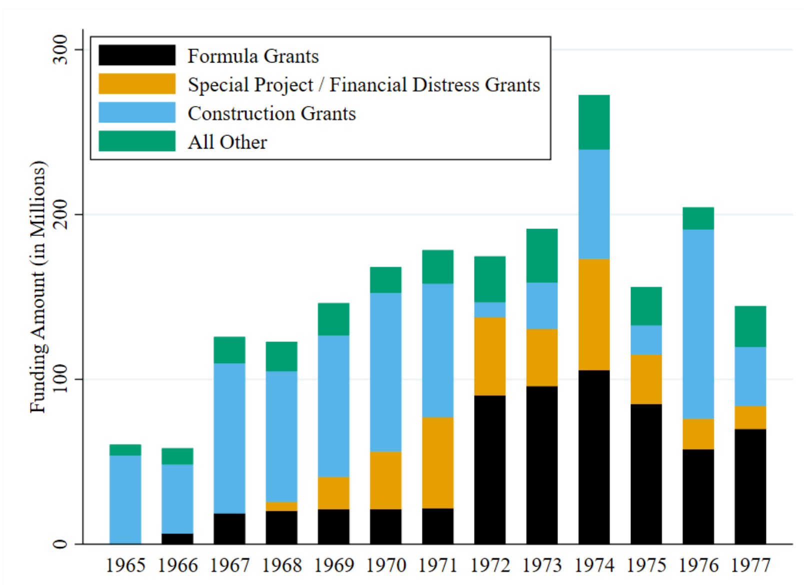
It follows that women should capture more seats in the absence of discrimination as long as $f(\cdot)$ is decreasing, a prediction I test in the following section. Notice, unlike in the previous case, we expect a change in the coefficient on $\mathrm{d}E$ to be persistent. That is, successful anti-discrimination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

Figure 1.12: Health Manpower Policy Timeline: Total Enrollment



I plot total enrollment at allopathic medical schools from 1950-2000. The main pieces of Health Manpower Legislation are denoted with vertical lines.

Figure 1.13: Health Manpower Policy Funding



In each year, I plot the amount of funding for formula grants, special project grants (including financial distress grants in years when these are counted separately), construction grants, and all other sources of funding, collected from HEW BHM, 1977. From 1965-1968, this funding comes from the HPEA; from 1969-1971, from the Health Manpower Act; from 1972-1976, from the CHMTA & continued resolutions; and from 1977-1980, from the 1976 HPEA & continued resolutions.

1.4.3 Empirical Specification and Results

The previous derivation, and in particular equation (1.4.1), suggests that changes in women's enrollment should be a linear function of changes in enrollment. Consider the statistical model specified in the previous section; setting aside the event terms, I assume that women's enrollment is given by:

$$F_{it} = \boldsymbol{\delta}_t' E_{it} + \beta A_{it} + \gamma_i + \delta_{gt} + \delta_{st} + \varepsilon_{it}$$

We can take first differences of this equation to obtain a more useful specification:

$$\triangle F_{it} = \delta_t E_{it} - \delta_{t-1} E_{it-1} + \beta \triangle A_{it} + \tilde{\delta}_{gt} + \tilde{\delta}_{st} + \triangle \varepsilon_{it}$$

Finally, by adding and subtracting $\delta_t E_{i,t-1}$, we see that

$$\triangle F_{i,t} = \delta_t \triangle E_{i,t} + (\delta_t - \delta_{t-1}) E_{i,t-1} + \beta \triangle A_{it} + \tilde{\delta}_{gt} + \tilde{\delta}_{st} + \triangle \varepsilon_{it} \tag{1.4.2}$$

Specification (1.4.2) also provides a (conditional) decomposition of women's enrollment gains in each period that is simple to interpret: The year-over-year change in women's enrollment $\triangle F_{i,t}$ can be decomposed into gains due to enrollment expansions $\delta_t \triangle E_{i,t}$ and gains due to capturing more seats conditional on enrollment $(\delta_t - \delta_{t-1}) E_{i,t-1}$, conditional on the included controls.

I estimate the decomposition given in (1.4.2) with the following baseline specification:

$$\triangle F_{it} = \alpha + \beta_t \triangle E_{it} + \theta_t E_{it-1} + \tilde{\delta}_{gt} + \nu_{it} \tag{1.4.3}$$

Here, β_t estimates the role of enrollment expansions in every period, θ_t estimates changes in the share of total enrollment that women capture in the absences of expansions, and ν_{it} is an error term. Ideally, I would estimate each coefficient in every period, but these estimates are too noisy, so I utilize two year bins to improve precision. In addition, I estimate the average value of β_t and θ_t before and after anti-discrimination legislation begins with the following summary specification:

$$\begin{aligned} \triangle F_{it} = \alpha &+ \beta_t^{pre} \mathbb{1}(t < 1971) \triangle E_{it} + \theta_t^{pre} \mathbb{1}(t < 1971) E_{it-1} \\ &+ \beta_t^{post} \mathbb{1}(t \geq 1971) \triangle E_{it} + \theta_t^{post} \mathbb{1}(t \geq 1971) E_{it-1} \\ &+ \tilde{\delta}_{gt} + \nu_{it} \end{aligned} \tag{1.4.4}$$

I begin by plotting estimates of β_t in Figure 1.14a, averaged over two year periods, and summary estimates from (1.4.4) are given in Table 1.5. There is a drastic change in the role of enrollment expansions throughout my sample period. In the 1960s, women capture around 10% of seats created, but this increases to almost 30% in the late 1970s. Interestingly, there is not a sharp uptick in women's gains in the early 1970s. This can be explained by changes in θ_t, which are plotted in Figure 1.14b. From 1971-1974, there is a sharp increase in the change in the proportion of seats filled by women absent any change in enrollment; this explains why we still see a rise in women's enrollment in the time series in this time period and is fully consistent with my earlier finding that women capture seats that would have been filled by men as the result of anti-discrimination policy.

Table 1.5: Summary Estimates of the Impact of Enrollment Changes on Women's Enrollment

	(1)	(2)	(3)
Enrollment Change, 1960s	0.064***	0.064***	0.067**
	(0.024)	(0.024)	(0.026)
Lagged Enrollment, 1960s	0.004**	0.004**	0.004**
	(0.001)	(0.001)	(0.002)
Enrollment Change, 1970s	0.222***	0.200***	0.153***
	(0.026)	(0.029)	(0.028)
Lagged Enrollment, 1970s	0.013***	0.016***	0.015***
	(0.001)	(0.002)	(0.003)
Observations	2035	1668	1293
Application Controls		X	X
State-by-Year Fixed Effects			X

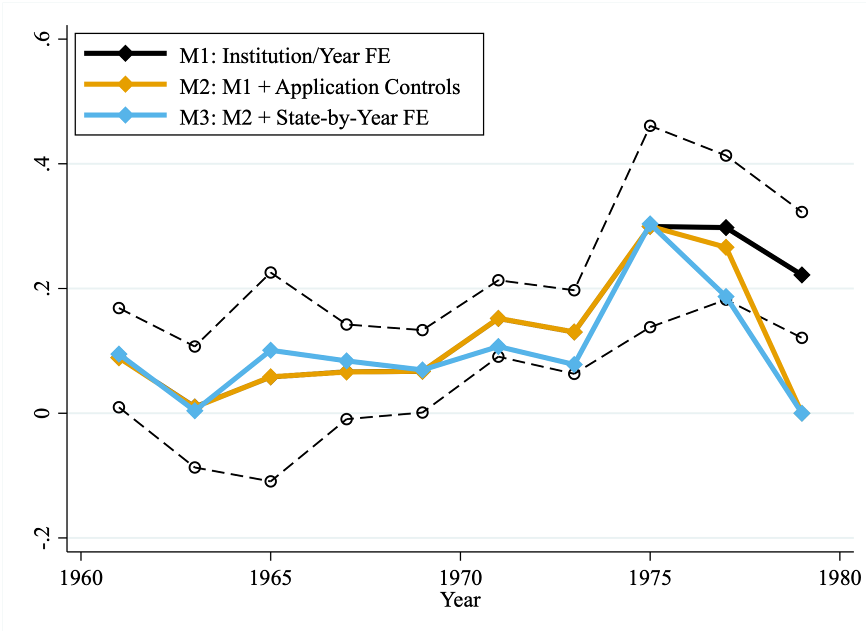
This table plots estimates from equation (1.4.4). Enrollment change refers to estimates of β_t, and Lagged enrollment refers to estimates of θ_t. These coefficients are binned separately for 1961-1970 (1960s) and 1971-1980 (1970s). Model 1 does not include any other right hand side variables, and 95% confidence intervals are plotted using standard errors clustered at the institution level to correct for serial correlation. Model 2 adds year fixed effects, and Model 3 adds institution fixed effects.
*** $p < .01$, ** $p < .05$, * $p < .10$

To explore robustness of this specification, I consider two other designs. My second specification includes controls for the year to year change in men's applications filed, controlling for potential crowd-out of women when new seats are available:

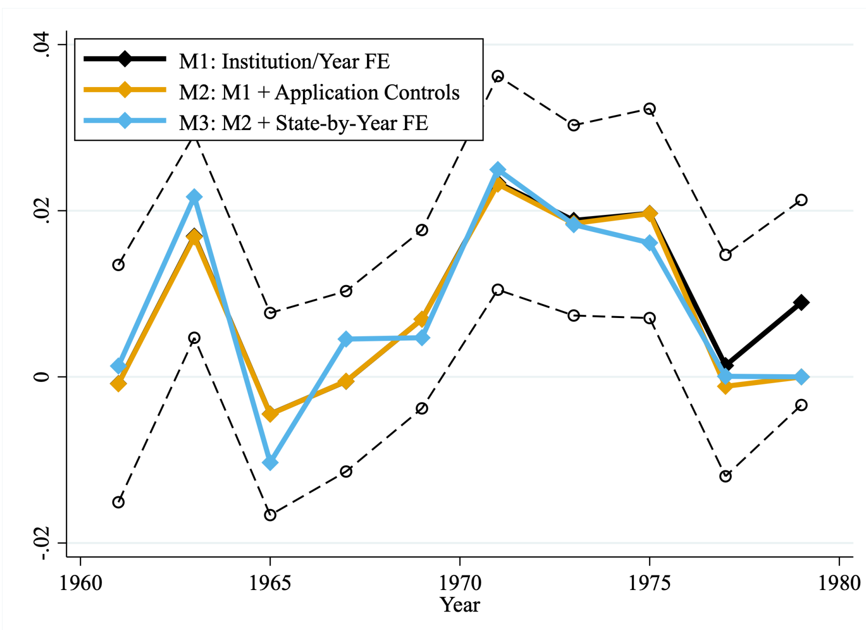
$$\triangle F_{it} = \alpha + \beta_t \triangle E_{it} + \theta_t E_{it-1} + \beta \triangle A_{it} + \tilde{\delta}_{gt} + \tilde{\delta}_{st} + \nu_{it} \tag{1.4.5}$$

The results of this specification are plotted in Figures 1.14a and 1.14b. As before, my results are extremely robust to the inclusion of application controls, with estimates of β_t and θ_t not

Figure 1.14: Role of Enrollment Expansions in Women's Enrollment



(a) Enrollment Expansions



(b) Gains Conditional on Enrollment

This figure plots results from Equation (1.4.3), where I estimate β_t (Figure 1.14a) and θ_t (Figure 1.14b) within two-year bins to reduce noise in the estimates. Model 1 does not include any other right hand side variables, and 95% confidence intervals are plotted using standard errors clustered at the institution level to correct for serial correlation. Model 2 adds year fixed effects, estimating equation (1.4.5). Model 3 adds institution fixed effects, estimating equation (1.4.6).

changing much. My last specification adds state-by-year fixed effects to control for changes in women's demand for medical education:

$$\triangle F_{it} = \alpha + \beta_t \triangle E_{it} + \theta_t E_{it-1} + \beta \triangle A_{it} + \tilde{\delta}_{gt} + \tilde{\delta}_{st} + \nu_{it} \tag{1.4.6}$$

Even with this demanding specification, the pattern across all models remains consistent. I confirm my finding earlier that women make outsized gains in the early 1970s. However, by the end of the decade, gains conditional on enrollment appear to have fallen back to their trend 1960s. Additional gains in the late 1970s seem to be much better explained by enrollment increases rather than capturing an increased fraction of existing seats. A quick back of the envelope calculate suggests that enrollment expansions are an important part of women's entry during the 1970s. Between 1970 and 1980, 6,035 new first-year seats were created; the OLS results from above suggest that women captured 1,207 of these, representing roughly 33% of their gain of 3,742 seats during this time period.[15]

1.5. Conclusion

In her 2006 Ely lecture, Claudia Goldin opens by stating that "women's increased involvement in the economy was the most significant change in labor markets during the past century" (Goldin, 2006a). Women's entry into professional schools was a core part of the last phase of this transition, beginning in the early 1970s and continuing through the new millennium. This paper contributes to our understanding of this era of history by quantifying the role of federal policy in women's entry into medicine, a small part of a much broader story. I find that federal policy began to matter in 1971, when anti-discrimination policy was first directed effectively at medical schools. Aspiring women were helped further by large increases in enrollment spurred by Health Manpower policy in the second half of the 1970s and filled many of these new seats. This was just the first chapter in a long process of change: in 2017, women comprised the majority of first-year medical students for the first time, becoming the majority of all enrollees shortly afterwards in 2019 (AAMC, 2019).

These changes have had a massive impact on U.S. economic progress. Hsieh et al. (2019) find that changes in the occupational distribution explain anywhere from 20% to 40% of the growth in U.S. output per person between 1960 and 2010. One of the key frictions in their model that was relaxed during this time period was barriers to human capital formation; I provide microeconomic evidence that federal policy played an important role in breaking

[15]One restriction on this analysis is that an enrollment expansion only creates new seats for women in the year that it occurs. In Appendix A.2, I consider a case study where I relax that restriction and find that women continue to gain seats (relative to the no-expansion counterfactual) for several years afterwards, suggesting that this might be an underestimate of the true effect.

these barriers. Since medicine and many other professional occupations are licensed, there is direct link between access to schooling and work, suggesting that educational frictions play an outsized role in women's access to these jobs. Future work should be directed at understanding changes in non-health professional occupations, such as the legal profession, which were unaffected by health manpower policy. Medicine (and other health professions) are unique in that education is capital-intensive, requiring not only lecture halls and classroom labs, but also hospitals for clinical training and research laboratories to fund the medical school. For this reason, the supply of legal education seems to be much more elastic than medical education, suggesting a bigger role for changes in women's (and men's) demand for seats.

Finally, there is likely much more to be gleaned about women's contributions to medicine as the 1970s came to a close. There is still a long road between graduation and practice—where did these newly minted M.D.'s go? And did the differential preferences of women over specialties and locations help fill gaps in healthcare provision and improve outcome for patients? These interesting and important questions are left for future work.

CHAPTER II

How the 1963 Equal Pay Act and 1964 Civil Rights Act Shaped the Gender Gap in Pay

With Martha J. Bailey and Bryan A. Stuart

2.0 Abstract

In the 1960s, two landmark statutes—the Equal Pay and Civil Rights Acts—targeted the long—standing practice of employment discrimination against U.S. women. For the next 15 years, the gender gap in median earnings among full-time, full-year workers changed little, leading many scholars and advocates to conclude the legislation was ineffectual. This paper uses two different research designs to show that women's relative wages grew rapidly in the aftermath of this legislation. The data show little short-term changes in women's employment but some evidence that firms reduced their hiring and promotion of women in the medium to long term.

2.1. Introduction

In the 1960s, two landmark pieces of legislation targeted the long-standing practice of employment discrimination against U.S. women. The Equal Pay Act of 1963 became the first piece of federal legislation to mandate equal pay for equal work through an amendment to the Fair Labor Standards Act (FLSA) (P.L. 88-38). The following year, Title VII of the Civil Rights Act of 1964 went further to ban sex-based discrimination in hiring, firing, and promotion (P.L. 88-352). In the context of the 1960s, these Acts were nothing short of revolutionary: according to the 1963 Occupational Wage Survey (OWS), women earned around 17 log points less than men working in the same narrowly defined jobs (U.S. Department of Labor, 1963).

Today, few histories conclude that the legislation succeeded, at least in its early years.

Annual estimates reported for decades by the Census Bureau show that—among full-time, full-year workers—women's median annual wage earnings hovered around 60 percent of men's for 15 years after the legislation passed (Figure 2.1a).[1] Goldin (1990) argues that "equal pay for equal work has been ... a rather weak doctrine to combat discrimination" (p. 201) and that "Title VII of the 1964 Civil Rights Act has also been weak in counteracting pay inequities that arise from differences in jobs and promotion" (p. 209). Given high rates of occupational segregation (Blau, 1977; Groshen, 1991), the legal standard of "equal work" meant that firms could segregate workers across occupations or establishments to comply with the letter of the law, while maintaining discriminatory pay practices. Gunderson (1989) notes that, "because differences in pay across establishments and industries account for a substantial portion of the gap, this severely restricts the scope of policies like equal pay and comparable worth, both of which are limited to comparisons within the same establishment" (p. 68). In addition, there is little evidence of enforcement of Title VII for sex discrimination until the 1970s (Simchak, 1971), which has led research on the law's consequences to focus on this later period (Beller, 1979, 1982a,b). Blau and Kahn's (2017) article in the Journal of Economic Literature summarizes the professional consensus: "we see no indication of a notable improvement in women's relative earnings in the immediate post-1964 period that might be attributable to the effects of the government's antidiscrimination effort; the gender pay ratio remained basically flat through the late 1970s or early 1980s, after which it began to increase" (p. 848).

Yet a closer examination of the earnings distribution for a broader set of workers hints that the legislation mattered more than previously believed. Figure 2.1b shows that the gender gap converged rapidly after 1964 for lower-wage workers if one broadens the Census Bureau's sample to include full-time women working at least 27 weeks—a sample more similar to modern analyses (Blau and Beller, 1988; Bailey et al., 2021). The historical record supports this conclusion as well. The Department of Labor reported great success with the Equal Pay Act's enforcement (Moran, 1970), and the Wall Street Journal celebrated ten years of the legislation, headlining that $475 million (2022 dollars) had been awarded to 140,000 workers in the legislation's first decade (Hyatt, 1973). Although few contemporaries claimed that Title VII affected sex discrimination before 1971, the law's timing and potential role in strengthening and broadening the Equal Pay Act make its effects difficult to rule out.

This paper reexamines the combined effects of the Equal Pay Act and Title VII on women's labor market outcomes in the 1960s using two complementary approaches. Motivated by

[1] The Census Bureau has reported the gender gap at the median for full-time, full-year workers for decades in order to characterize pay gaps for individuals with a similar level of labor-market attachment. However, full-time full-year women workers comprised only 45 percent of working women in 1964.

Neumark and Stock (2006), our first approach is based on the logic that federal anti-discrimination legislation—if effective—should have larger effects in the 28 states without pre-existing equal pay laws. Drawing on the 1950-1960 Decennial Census and 1962-1975 Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS), we find that women's weekly wages rose by around 9 percent (8.7 log points) more in states without pre-existing equal pay laws after the federal legislation took effect. These estimates are robust to controlling for state-by-birth-cohort fixed effects, which flexibly account for cohort-level shifts in women's aspirations and skills (Goldin et al., 2006; Goldin, 2006a,b), as well as industry-by-year and occupation-by-year fixed effects, which flexibly account for national changes in the economy and help focus the analysis on the narrowly defined types of discrimination targeted by Equal Pay legislation. While this research design has the advantage of characterizing broad changes in the labor market, its internal validity is limited to the extent that unobserved forces may have differentially affected labor markets in states without pre-existing equal pay laws.

Our second approach addresses this concern by examining within-state changes in women's weekly wages following the passage of the legislation. This approach follows Card's (1992) influential work on the minimum wage, which exploits the fact that a national policy has greater incidence in areas where more individuals are affected. Although we do not observe sex discrimination in the data, this paper hypothesizes that the observed gender gap in pay within industry-occupation-state-group cells is correlated with this latent variable. If this hypothesis holds and federal anti-discrimination legislation was somewhat effective, we expect women's wages to rise more quickly after 1964 in job cells with larger pre-existing gender gaps. An advantage of this research design is that it permits the inclusion of state-by-year fixed effects to absorb potentially confounding time-varying state-level factors that could compromise the internal validity of the first research design.

Consistent with federal legislation narrowing gender gaps, we find that women's weekly wages grew more quickly after 1964 in job cells with larger pre-existing gender gaps—an effect equivalent to 11 percent (10 log points) at the mean gender gap. Noteworthy is that effect sizes do not differ for White and Black women, which suggests that the estimates are not driven by the Civil Rights Act's effects on racial discrimination. In addition, the research design recovers no effects of the legislation on men's wages, which ameliorates concerns that alternative labor-market shocks or policies drive these findings.

Heterogeneity tests underscore the complementarity and validity of the two empirical approaches. In states without pre-existing equal pay laws—where federal anti-discrimination legislation should have been more effective—women's weekly wages grew by 18 percent at the mean after 1964, whereas women's wages grew by one-third that amount (6 percent) in states with pre-existing equal pay laws. Recentered-influence-function (RIF) regressions show that the largest effects of the legislation accrued to women in the lowest percentiles of the wage distribution (Firpo et al., 2009), which connects these findings to the large wage growth among women earning below the median after 1964 in Figure 2.1b. These patterns are consistent with pay equalization being greater in jobs where the "equality of work" was more easily judged and where the Wage and Hour Division (WHD)—the agency tasked with enforcing the Equal Pay Act—focused its investigations of compliance with the minimum wage.

A final analysis investigates how federal anti-discrimination legislation affected women's employment. Consistent with firms having some monopsonistic power to set wages, the data provide little evidence that women's employment or annual hours fell in response to wage increases in the short run—findings that align closely with Manning's (1996) study of the Equal Pay Act in the United Kingdom. In the long run, however, we find some evidence that women's employment grew more slowly in more affected job cells, which is consistent with Neumark and Stock's (2006) study of state-level anti-discrimination legislation before 1960. Contemporary accounts provide direct evidence as to why this might have been the case. After the passage of the Equal Pay Act but prior to the Civil Rights Act (which made the practice illegal), employers told journalists that they planned to "segregate male and female job classifications" and "downgrade job classifications for women and assign higher-paying duties to men" in response to the Equal Pay Act (Washington Post, 1964).

In summary, these results imply an important role for the Equal Pay Act and Title VII in reducing pay discrimination against U.S. women in the 1960s. The magnitudes of our findings suggest that federal anti-discrimination legislation reduced the within-job gender gap in pay by at least 58 percent between 1964 and 1968 but may have slowed the integration of women into higher-paying, historically male jobs in the longer term. These findings contribute to a long but mixed literature on the role of anti-discrimination legislation in reducing the gender gap in the U.S., which has focused on the effects of affirmative action after 1967 or the later expansion or enforcement of Title VII after 1970 (Beller, 1979, 1982a,b; Leonard, 1984; Carrington et al., 2000; Holzer and Neumark, 2006; Kurtulus, 2012; Helgerman, 2023). Little evidence exists regarding the effects of the 1963 Equal Pay Act, and studies of equal pay initiatives in other countries suffer from a dearth of data, limited internal validity, and differences in policies and implementation (Gunderson, 1989). This paper develops two new empirical strategies to show that the implementation of the Equal Pay Act, which was potentially strengthened by Title VII, reduced the gender gap in pay in the mid-1960s across the U.S. labor market.

2.2. A History of the Equal Pay Act and Title VII of the Civil Rights Act

In the early 1960s, sex discrimination in labor markets was not only widely accepted, it was also institutionalized and legal. State laws mandated different minimum wage, break, and rest requirements for men and women and placed different restrictions on the jobs men and women could hold (Moran, 1970; Marchingiglio and Poyker, 2021). Union contracts delineated different pay schedules by sex for the same job (Eaton, 1965). Newspapers posted help-wanted advertisements for male and female jobs (Pedriana and Abraham, 2006), along with explicitly different pay scales for what appear to be the same jobs.[2] Firms often fired women when they got married (Goldin, 1991) and more routinely when they became pregnant (Gruber, 1994).

After World War II opened many jobs to women, their labor-force participation rates grew rapidly, rising from around 26 percent to 35 percent between 1940 and 1960 (Goldin, 1990, pg. 17). The rise of scheduled part-time work in the 1940s and 1950s pulled significant numbers of married women into the labor force, many of whom worked fewer than 35 hours per week. Changes in part-time work were particularly pronounced in certain sectors. For example, only 14 percent of the female sales sector worked part-time in 1940 but 40 percent did by 1960 (Goldin, 2006a). The increase in part-time work also reinforced the segregation of women into certain jobs. Women tended to work as secretaries, teachers, nurses, librarians, and social workers. In the 1960 Census, approximately 83 percent of male workers were employed in occupations in which no more than 20 percent of the workers were female (Blau, 1977, pg. 12), but some women were entering male-dominated fields: 58 percent of women worked in occupations where they comprised more than 80 percent of the workers, with the other 42 percent working in more integrated occupations (Ibid).

Between 1950 and 1960, men's weekly wages grew by 32 log points, whereas women's weekly wages only grew by around half that figure, increasing the gap in pay by around 16 log points (Appendix Table B.1). A Kitagawa-Blinder-Oaxaca decomposition using basic demographic variables (log hours worked per week, years of schooling, years of potential experience, an indicator for being married, and an indicator for race) shows that only 13 percent (=0.022/0.164) of the growing gender gap is explained by changes in women and men's characteristics (primarily hours worked). Much of the gender gap, however, is explained by occupational segregation. Adding detailed indicators for industry (143 categories) and occupation (263 categories) raises the explained share of the increase in the gender gap to 60 percent.

[2] In an analysis of these advertisements, Hunt and Moehling (2021) find an advertised gender wage gap of 38 log points in three cities in 1960, 28 log points of which corresponds to within-agency differences in pay.

2.2.1 State and Federal Equal Pay Acts

Within this broader context of a rising gender pay gap, the 1963 Equal Pay Act represented a watershed moment following decades of advocacy. Federal equal pay legislation was first introduced to Congress in 1945 after wage studies showed pervasive wage inequality between women and men in wartime industries. The Women's Bureau in the Department of Labor documented multiple examples of sex-based pay discrimination, including discrepancies in entry wages and pay for more experienced workers in identical jobs (Fisher, 1948).[3] Although federal legislation failed to pass for two decades, 22 states passed equal pay laws before 1963 (U.S. Congress, 1963). State equal pay laws were primarily in the Northeast, Midwest, and West (Figure 2.2), where their aim was often to keep women from undercutting men's wages rather than raising women's earnings. Arkansas was the sole state in the South to pass equal pay legislation.

State equal pay laws varied in their language and enforcement. Michigan and Montana, the two states that passed the first equal pay laws in 1919, illustrate these differences well. While Montana's law applied to nearly any enterprise employing men and women, Michigan's law applied only to employees in manufacturing. A common thread across these two states is that neither one went beyond making a "general declaration of law," which made these laws difficult to enforce (Fisher, 1948, pg. 54). In making the case for a national Equal Pay Act to Congress, the Women's Bureau noted that state laws "leave large groups of workers out, and often have inadequate provisions for administration and enforcement" (U.S. Congress, 1963, pg. 20).

The momentum to pass federal anti-discrimination legislation in the 1960s grew out of President John F. Kennedy's Commission on the Status of Women. The Equal Pay Act was first introduced to Congress in August of 1961 and managed to pass in both houses, but the business lobby undermined the bill during the reconciliation process (Harrison, 1989). Esther Peterson, the Assistant Secretary of Labor and Director of the U.S. Women's Bureau under Kennedy, redoubled her efforts and revived the Equal Pay Act as an amendment to the FLSA (P.L. 75-718). In addition to producing detailed reports to document pay differences (U.S. Congress, 1962), Peterson used her Congressional testimony to describe pervasive sex discrimination in employment. Analyzing pay differences among similarly experienced bank tellers working comparable hours, the Department of Labor found that women had lower weekly earnings in every city studied (U.S. Congress, 1963, pg. 31). Furthermore, surveys found that men outearned women with the same title in nearly all establishments (U.S.

[3] Fisher (1948) reports one particularly egregious example: "In the gun manufacturing industry ... where experienced men and women worked on five different types of machines, the lowest rate for men was at least ten cents above the highest wage paid to women" (Fisher, 1948, pg. 51).

Congress, 1963, pg. 30,37).[4]

To quantify the gender gap in pay within narrowly defined jobs just before the Equal Pay Act passed, we digitized the 1963 Occupational Wage Survey (OWS), which contains weekly or hourly wage observations by sex from 82 cities and 58 narrowly defined job classifications (U.S. Department of Labor, 1963). The OWS show a 32-log-point gap in pay across all cities and jobs in 1963 (Appendix Table B.3), which is similar to the gap in weekly wages in the Census and ASEC. When including fixed effects for detailed job classifications, the within-job gap in weekly pay is 17 log points—a sizable wage gap within jobs that could be targeted by the Equal Pay Act. Jobs with hourly pay show a larger total gender gap in pay of 44 log points but a similar within-job difference in pay of 18 log points. The Labor Department noted that differences in pay occurred mostly in “large department stores, banks, airline reservation offices, chain stores, and other firms where men and women customarily perform similar work” (Eaton, 1965).

Peterson’s report also cited a National Office Management Association survey of employers in the U.S. and Canada, which asked, “Do you have a double standard pay scale for male and female office workers?” (U.S. Congress, 1963, pg. 27), where one third of employers answered, “Yes.” In discussions with members of Congress, Peterson often cited a personal anecdote as well, noting that a manager told her, “We pay them less because we can get them for less” (quoted in Harrison 1989, pg. 95).

Under Peterson’s stewardship, the revised equal pay bill was introduced on February 14, 1963, and—after replacing the phrase “comparable work” with “equal work”—passed into law on June 10, 1963. The Equal Pay Act prohibited sex-based wage discrimination between men and women in the same establishment who perform jobs that require substantially equal skill, effort, and responsibility under similar working conditions.[5] For workers not covered under collective bargaining agreements, the Equal Pay Act took effect on June 10, 1964. For the 13 percent of women who were unionized in the early 1960s (LeGrande, 1978), the Act took effect the following year on June 10, 1965. As an amendment to the FLSA, the Equal Pay Act only applied to workers covered under the FLSA.[6]

[4]Appendix Table B.2 reprints tabulations of gender differences in average hourly earnings across several industry-occupation categories in Chicago, Winston-Salem, and Philadelphia.

[5]Sex discrimination can take many different forms, including women being paid less than their productivity solely due to their sex, being hired less or receiving different job assignments, and receiving different promotion opportunities. The Equal Pay Act only addresses sex discrimination to the extent that it manifests as unequal pay for equal work.

[6]Not all workers are covered under the FLSA, but its coverage was expanded in the 1961 and 1966 Amendments and in the 1972 Educational Amendments. The 1961 Amendments extended coverage to employees in retail or service, local transit, construction, and gasoline service stations. The 1966 Amendments expanded coverage to include employees on large farms, federal service contracts, federal wage board employees, and certain Armed Forces employees (e.g., post exchanges). It also narrowed or repealed exemptions for

2.2.2 Title VII of the Civil Rights Act

Just one year after the Equal Pay Act passed, Congress enacted the 1964 Civil Rights Act. Title VII of the Civil Rights Act largely overlapped with the Equal Pay Act in its coverage of pay discrimination but also extended its provisions by (1) expanding coverage to many workers not covered under the FLSA and (2) prohibiting sex-based discrimination in employment, including hiring, firing, and promotions. Coverage was not universal: Title VII did not apply to public sector employees until 1972 (Posner, 1989), and the legislation covered only employers with at least 100 employees as of July 1965, a threshold that was gradually reduced to 25 employees by 1968.

The goal of the Civil Rights Act had little to do with gender equality, and the initial legislation did not include sex among the protected classes of race, color, religion, and national origin. "Sex" was added to Title VII's protected classes just one day before the final vote by a segregationist, Representative Howard Smith (D-Virginia), who opposed the Act's passage. Many commentators believe Smith intended to make the bill unpassable (Harrison, 1989). Thomas (2016) explains how Rep. Smith played his amendment for laughs, claiming a letter from his constituent had asked him to "protect our spinster friends." One of the twelve women House Representatives, Martha Griffiths (D-Michigan), silenced the laughter, saying, "if there had been any necessity to point out that women were a second-class sex, the laughter would have proved it" (Thomas, 2016, pg. 102). The next day the legislation passed, codifying prohibitions of sex-based employment discrimination into federal law.

2.2.3 The Effectiveness of Anti-Discrimination Legislation in the 1960s

As an amendment to the FLSA, the enforcement of the Equal Pay Act fell to the WHD in the Department of Labor, which monitors and enforces compliance with the FLSA (P.L. 75-718). Based on the WHD's long reputation, firms knew that non-compliance could be punished by mandating the payment of back wages and criminal prosecution, and courts had already settled many points of interpretation. Following the Equal Pay Act's effective date in 1964, the WHD instructed its field staff to check for compliance with the new equal pay

employees of hotels, restaurants, laundries and dry cleaners, hospitals, nursing homes, schools, auto and farm implement dealers, small loggers, local transit and taxi companies, agricultural processing, and food services. Finally, the 1966 FLSA included an indirect expansion of coverage through its reduction in the enterprise volume test from $1 million (in the 1961 Amendments) to $250,000. See Bailey et al. (2021) for a discussion of changes in coverage and minimum wages in the 1960s. Another quirk of the FLSA is that section 13(a)(1) carves out an exemption to the minimum wage and overtime provisions for any worker employed in a bona fide executive, administrative, or professional (EAP) capacity. Consequently, when the Equal Pay Act Amendment prohibited discrimination on the basis of sex by amending the minimum wage provisions of the FLSA, EAP-exempt workers were not covered. In 1972, Title IX of the Educational Amendments amended Section 13(a) to remove the EAP exemption.

provisions as part of all investigations under the FLSA (U.S. Department of Labor, 1965). In addition, the Labor Department filed suits signaling its intent to enforce the law. *Wirtz v. Basic Incorporated* (1966) challenged an employer's claim that a male analyst was entitled to more money because he had greater experience and responsibility. The court supported the Labor Department's claim of discrimination, noting that the work of three employees (one man and two women) was the same and that the man's greater experience was not a requirement of the job. The ruling emphasized that the statutory requirement of "differences in working conditions" could not be established by job title alone and that the burden of proof for any exceptions to equal pay lay with the employer.

The Department of Labor continued to enforce compliance with the Equal Pay Act, both reviewing labor union contracts and bringing multiple lawsuits. By the end of 1964, investigators had found $55,000 in discriminatory wage payments owed to women, and one firm voluntarily paid $227,000 in back pay when the WHD began checking for discrimination (in 2022 dollars). By 1965, around 80 percent of sex-discrimination complaints had led to back payments to workers. Likely due to the WHD's enforcement, Secretary Wirtz reported to Congress that "voluntary" compliance with the Equal Pay Act was high (U.S. Department of Labor, 1966, pg. 18). Many unions and employers made voluntary changes to eliminate contractual differences in wage rates, welfare and pension plans, sick leave, rest periods, and "marriage provisions" that dictated the loss of seniority and possible dismissal for women getting married. At the same time, the courts strengthened the law by issuing rulings to eliminate employer justifications for unequal pay (Eaton, 1965; Washington Post, Times Herald, 1964).

Building on the federal Equal Pay Act, many states extended existing fair employment practice laws to prohibit pay discrimination on the basis of sex, while others passed new equal pay legislation. These state measures supplemented the federal law by extending the equal pay principle to areas not covered by federal statutes (Simchak, 1971). By the end of the 1960s, some contemporaries concluded that the Equal Pay Act had been successful in achieving its aims (Moran, 1970). Hole and Levine (1971) argue that "the Equal Pay Act [is] the only law dealing with sex discrimination that is anywhere near properly enforced" (Hole and Levine, 1971, pg. 29).

The enforcement of Title VII was a different story. The Equal Employment Opportunity Commission (EEOC)—the newly created agency tasked with the enforcement of the 1964 Civil Rights Act—had limited will and authority to enforce the law's sex-based provisions (Munts and Rice, 1970). The EEOC regarded its primary mission as reducing racial discrimination, maintaining that "the addition of sex to the law had been illegitimate—merely a ploy to kill the bill" (Harrison, 1989, pg. 187).[7] Another complication was that Title VII challenged decades of state protective legislation that explicitly set different standards by sex. Because the 1965 EEOC did not see "any clear Congressional intent to overturn all of these [state] laws" (Harrison, 1989, pg. 187), it created a task force to provide states with guidelines—a process that took years (Munts and Rice, 1970). Unlike the Labor Department, the EEOC was initially unable to bring its own lawsuits and could only refer cases to the Department of Justice. Consequently, the EEOC had pursued very few sex discrimination cases by 1970. Simchak (1971) notes, "Of the total number of court cases filed by the Department of Justice to date (approximately fifty) under all the discrimination criteria in Title VII, only one has pertained to sex discrimination" (Simchak, 1971, pg. 555).

Ambivalence about sex discrimination outside the Labor Department is also evident in President Johnson's 1965 Executive Order 11246, an affirmative action mandate that omitted "sex" entirely (Johnson, 1965b). The order prohibited the federal government and federal contractors from employment discrimination on the basis of race, color, religion, or national origin only. This inaction galvanized women's groups and advocacy efforts and eventually resulted in Executive Order 11375 in 1967, which amended Order 11246 to include "sex" (Johnson, 1967; Harrison, 1989). But the EEOC's active enforcement of Title VII's sex provisions did not increase in earnest until after the U.S. Supreme Court's first decision in *Phillips v. Martin Marietta Corporation* (1971), which ruled that an employer cannot hire men with young children while maintaining a policy to prohibit hiring women with young children.[8] Title VII was strengthened further by the amendments in the Equal Employment Opportunity Act of 1972, which gave the EEOC the authority to pursue independent lawsuits and expanded the Act's coverage of individuals employed by the government and smaller firms (P.L. 92-261).

Overall, the historical record provides a mixed picture of the success of the Equal Pay Act and Title VII in addressing labor-market discrimination against women in the 1960s. While the Equal Pay Act's provisions were seriously enforced starting in 1964 and extended through state legislation, the law's effects were likely limited by "equal work" requirements, which failed to address pay discrimination arising from differential hiring, assignment, and

[7]When a reporter asked Franklin D. Roosevelt, Jr., the EEOC's first commissioner, "What about sex?" Roosevelt joked, "I'm all for it." Similarly, the EEOC's second executive director, Herman Edelsberg, dismissed the sex provision as a "fluke" that was "conceived out of wedlock" (Thomas, 2016) Title VII became known as the "Bunny Law," named after a satirized case in which Playboy turned down a man for a job as a Playboy bunny.

[8]Following *Phillips v. Martin Marietta Corporation* (1971), considerable ambiguity about sex discrimination remained. For instance, the U.S. Supreme Court in *General Electric Co. v. Gilbert* (1976) held that Title VII did not guarantee pregnant women equal coverage under employee benefit plans covering non-occupational sickness and accidents, which Congress remedied with the Pregnancy Discrimination Act of 1978 (Posner, 1989).

promotion of men and women. Title VII's provisions were broader, but the EEOC's reluctance to enforce the law's sex provisions and the EEOC's limited enforcement authority likely curbed the statute's effectiveness until the 1970s. Consistent with this history, research on the implications of Title VII for sex discrimination focuses on this later period (Beller, 1979, 1982a,b).

2.3. Data and Research Design 1: Variation in the Incidence of Anti-Discrimination Legislation due to State Equal Pay Laws

Our analysis complements these historical accounts by quantifying the effect of the Equal Pay Act and Title VII on women's wages and employment. We combine the one-percent sample of the 1950 Decennial Census, the five-percent sample of the 1960 Decennial Census, and the 1962 to 1975 CPS ASEC to document labor-market outcomes for non-agricultural wage earners ages 25 to 64 in nationally representative data (Flood et al., 2022; Ruggles et al., 2023). Some analyses also use the combined one-percent Form 1 and Form 2 state samples of the 1970 Decennial Census, as well as the full count 1940 Decennial Census (Ruggles et al., 2021).

2.3.1 Data Processing and Sample Restrictions

Our sample focuses on prime-age wage earners and excludes individuals under age 25 who may not have completed their schooling. To increase consistency between the ASEC and censuses, we restrict the censuses to individuals not in the Armed Forces or institutionalized. We additionally require that observations have non-missing data for industry, occupation, and state group of residence, which are critical for our empirical strategy. Our analysis uses nine industries (n), eight occupations (o), and 21 state groups (s).[9] We exclude individuals working in agriculture by dropping individuals with the occupation of "farmer" or "farm laborer" or the industry of "agriculture, forestry, and fishing." We also exclude individuals if they report being self-employed in the survey reference week or if the ratio of their self-employment and farm income to labor income exceeds 10 percent in absolute value (Lemieux, 2006).

We convert annual wage earnings into 2022 dollars using the CPI-U. The census and ASEC ask about annual earnings and weeks worked in the year before the survey, so we index wages

[9] The nine industries are mining, construction, manufacturing, transport/communications/electric/gas/sanitary services, wholesale trade, retail trade, finance/insurance/real estate, services, and public administration. The eight occupations are professional/technical, managers/officials/proprietors, clerical, sales, craftsmen, operatives, service, and non-farm laborers. The public ASEC only identifies 21 state groups consistently in our period of interest, which dictates our use of 21 "state groups."

and employment to the appropriate year (e.g., the 1965 ASEC provides information about wages and employment in 1964). We construct log weekly wages by subtracting from log annual wage earnings the mean log number of weeks worked within each reported interval.[10] Because weekly wage earnings are measured with error due to (1) the aggregation of weeks worked into intervals and (2) misreporting by respondents about wage earnings and weeks worked, we evaluate the sensitivity of our results to using annual earnings and hourly wage earnings (see Appendix B.1) and to winsorizing the lowest ten percentiles (see Appendix B.2).

Figure 2.3 describes the evolution of mean log weekly wages in states with and without pre-existing equal pay laws for both women and men. Several features of these plots stand out. First, weekly wages show a dip in the early 1960s relative to the 1960 Census, which likely reflects changes in the CPS sampling frame between 1961 and 1963.[11] The dip in weekly wages is slightly larger for women and in states without equal pay laws, which should be kept in mind when interpreting our estimates. Second, states without equal pay laws tended to have lower average weekly wage earnings, which is not surprising given that the standards of living were lower in the South and western Midwest, which were less likely to have equal pay laws (Figure 2.2). Third, women's wages in states without pre-existing equal pay laws converge on those of women in states with equal pay laws after the mid-1960s—a pattern less evident among men.

2.3.2 Research Design 1: Pre-existing State Equal Pay Laws

Our first research design posits that anti-discrimination legislation should have larger effects in areas with more sex discrimination. Motivated by Neumark and Stock (2006), we test whether women's wages grew more quickly after 1964 in the 28 states that did not have pre-existing equal pay laws. This would be the case if state equal pay laws had already somewhat lowered sex discrimination, so that federal anti-discrimination legislation would have smaller effects in these states.

[10] The 1960 Census and 1962-1975 ASEC report weeks worked last year in categories (1-13, 14-26, 27-39, 40-47, 48-49, and 50-52 weeks), whereas the 1976-1979 ASEC report weeks worked in integers. We use the 1976-1979 ASEC to estimate the mean log number of weeks worked within each category in the 1962-1975 ASEC by sex, race, and 10-year age bin. Similarly, the 1960 Census reports hours worked in categories. For this year, we use the mean log hours worked within each category estimated from the 1962-1979 ASEC by sex, race, and 10-year age bin.

[11] Changes to the sampling frame reflect changes in the population size and distribution as well as the industrial mix between areas as revealed in the 1960 Census. Interested readers may find a history of the CPS here, https://www2.census.gov/programs-surveys/cps/methodology/Techincal%20paper%2066%20chapter%202%20history.pdf (accessed December 30, 2021).

2.3.2.1 Event-Study Specification

We estimate the following event-study specification using ordinary least squares:

$$Y_{it} = \sum_{\tau=1949,\tau\neq 1964}^{1974} \alpha_\tau D_\tau \text{NoEPL}_{s(i)} + X_{it}'\beta + \gamma_{n(i)o(i)s(i)} + \delta_{s(i)b(i)} + \delta_{n(i)t} + \delta_{o(i)t} + \varepsilon_{it}mm \quad (2.3.1)$$

The outcome, Y_{it}, is log weekly wage earnings of individual i in calendar year $t = 1949, 1959, 1961-1974$. The independent variable of interest, NoEPL_s, is equal to 1 if a state group did not have an equal pay law as of January 1, 1963. In the three state groups containing states with and without equal pay laws, we use the share of workers residing in states without an equal pay law rather than a 0/1 coding.[12] We identify whether states had an equal pay law using statutory coding from U.S. Congress, 1963, which agrees with Neumark and Stock (2006, Table 2). Note that NoEPL_s does not vary across time—it captures a state's legal environment as of 1963.

We interact NoEPL_s with a set of year indicator variables, D_τ, omitting 1964—the year the Equal Pay Act took effect. Our parameter of interest, α_τ, captures the combined effects of the Equal Pay Act and Title VII on women's wages. If (1) sex discrimination in pay was larger in 1963 in states without state-level equal pay legislation and (2) national anti-discrimination legislation reduced sex discrimination in pay, we expect that $\alpha_\tau > 0$ for $\tau > 1964$. If the parallel trends assumption holds and states without equal pay laws were trending similarly before the Equal Pay Act and Title VII took effect, then we expect $\alpha_\tau = 0$ for $\tau < 1964$. To the extent that the federal legislation affected discrimination in states with pre-existing equal pay laws, this approach will understate the legislation's effects on states without equal pay laws—a point we revisit with the second research design. Changes in state laws after 1964 that target labor market discrimination tended to bring states into accord with federal law, and we regard these changes as part of the treatment effect of the federal legislation.

We include additional covariates to account for changes in workforce composition and improve precision. The vector X_{it} includes log hours worked in the reference week, an indicator variable for nonwhite race, and a quadratic in the worker's age.[13] Fixed effects for

[12] We calculate the share of workers within a state group that live in a state without an equal pay law using the 1960 Census. In Arkansas-Louisiana-Oklahoma, 76 percent of wage earners were in a state without an equal pay law (Louisiana, Oklahoma). In Arizona-Colorado-Idaho-Montana-Nevada-New Mexico-Utah-Wyoming, 40 percent of wage earners were in a state without an equal pay law (Idaho, Nevada, New Mexico, Utah). In Maine-Massachusetts-New Hampshire-Rhode Island-Vermont, 5 percent of wage earners were in a state without an equal pay law (Vermont). Appendix Table B.4 reports summary statistics by states' pre-existing equal pay law status.

[13] The grouping of "nonwhite" is an aggregation necessitated by the data. Detailed race/ethnicity coding that would be used today is not consistently reported during the 1960s. Hispanic/Latinx origin is not available in the ASEC until 1971.

single-digit industry n by single-digit occupation o by state-group s, γ_{nos}, account for average differences in wages across job classifications and labor markets. While these fixed effects focus the analysis on within industry-occupation-state-group wage changes, these cells are broader than the within-firm jobs targeted by the Equal Pay Act. To the extent that men shifted to higher-paying jobs within industry-occupation-state-group cells, our results may understate the wage effects of the legislation within the same jobs. We view this as a feature: the research design recovers changes in women's pay net of these potentially offsetting shifts in employment as long as they occur within a single-digit industry-occupation-state group cell.

Although this specification cannot include state-by-year fixed effects to account for time-varying within-state changes in labor markets or policies (Chay, 1998; Almond et al., 2003; Cascio et al., 2010; Bailey and Duquette, 2014; Bailey and Goodman-Bacon, 2015; Goodman-Bacon, 2018), it can accommodate other flexible controls. In some specifications, we include state-group-by-birth-year (b) fixed effects, δ_{sb}, which flexibly account for cohort-level shifts in women's aspirations and skills (Goldin, 2006a,b) as well as differential state-level changes in labor-market skills (including educational quantity and quality, potential labor-market experience, and other unobserved cohort characteristics). Industry-year and occupation-year fixed effects, δ_{nt} and δ_{ot}, capture unobserved, national changes that affect all workers in these groups.[14]

A triple-differences specification (DDD) accounts for gender neutral labor-demand or supply shocks by using men as an additional comparison group. To the extent that the Equal Pay Act and Title VII reduced men's wages (either as a means for firms to comply with the law or in response to general increases in the cost of labor), this specification may overstate the resulting gains in women's wages. On the other hand, this specification could understate the effect on women's wages if the legislation caused firms to increase men's responsibilities (and pay) to maintain pre-existing wage hierarchies. Consequently, this exercise provides a complementary characterization of labor-market adjustments, rather than a falsification test. This specification interacts all variables in equation (2.3.1) with an indicator variable for sex, which allows the relationship of all covariates and fixed effects to differ between men and women.

[14] Educational attainment is available in all years except the 1963 ASEC. We omit this covariate from our main specifications to avoid dropping 1963 as a pre-treatment observation. Including education as a covariate changes the estimates very little (see Appendix Figures B.5 and B.13).

2.3.2.2 Employment Outcomes

Equation (2.3.1) cannot be estimated using employment as an outcome, because industry and occupation tend to be reported only for individuals who are employed. To test for the legislation's employment effects, we define the dependent variable as the log of the survey-weighted number of employees or annual hours worked in a sex-specific industry-occupation-state-group (*nos*) cell in year t, where annual hours worked is the survey-weighted sum of the number of weeks worked last year multiplied by the number of hours worked in the reference week.[15] We estimate the following specification, which is similar to equation (2.3.1) with several modifications:

$$Y_{nost} = \sum_{\tau=1949,\tau\neq1964}^{1974} \alpha_\tau D_\tau \text{NoEPL}_s + X'_{nost}\beta + \gamma_{nos} + \delta_{nt} + \delta_{ot} + \varepsilon_{nost} \qquad (2.3.2)$$

The first modification is that we replace the individual covariates with *nos* cell averages, including a quadratic in age and the share of workers that are nonwhite (we omit hours worked). Second, we make two further adjustments to minimize the importance of small *nos* cells. We limit the employment regressions to *nos* cells that have at least one wage earner in each of our years of interest and weight by the product of each cell's share of observations in the 1960 Census and the total number of observations in each survey year. These two adjustments maintain the representation of different cells over time and account for year-to-year changes in census and ASEC sample sizes. This approach places higher weight on cells which have more observations in 1960 or come from survey years with larger total sample sizes, which reduces the influence of small, noisy cells (Solon et al., 2015). The weight does not depend on the number of industry-occupation-state observations in each survey year, as this would generate weights that reflect shifts in employment which might be driven by the legislation.

2.3.2.3 Spline Specification

Although the event-study specification provides a highly flexible and transparent description of the data, the estimates for individual years are often noisy. We, therefore, complement the event-study with a three-part spline specification with knots in 1964 and 1968, which summarizes the event-study estimates and improves precision. Using log weekly wage earnings

[15]We first construct annual hours worked for individuals by multiplying the level of weeks worked by hours worked, where the level is calculated using the procedure described in footnote 10. Then, we aggregate to the nos cell.

as an outcome, the spline specification is,

$$
\begin{aligned}
Y_{it} = {} & \tilde{\alpha}_0 \text{NoEPL}_{s(i)} t + \tilde{\alpha}_1 \mathbb{1}(t > 1964) \text{NoEPL}_{s(i)} t + \tilde{\alpha}_2 \mathbb{1}(t > 1968) \text{NoEPL}_{s(i)} t \\
& + X_{it}' \tilde{\beta} + \tilde{\gamma}_{n(i)o(i)s(i)} + \tilde{\delta}_{n(i)t} + \tilde{\delta}_{o(i)t} + \tilde{\varepsilon}_{it}
\end{aligned} \tag{2.3.3}
$$

The first three terms interact linear time trends, t, with the NoEPL_s variable as well as with indicator variables for the post-1964 and post-1968 period.[16] Thus, the spline succinctly summarizes trends in the data without placing too much emphasis on one (potentially noisy) point estimate or year. The remaining covariates correspond to those defined in equation (2.3.1). The spline provides a parsimonious method to test and, if necessary, adjust for pre-trends, as captured in $\tilde{\alpha}_0$.[17] The coefficient, $\tilde{\alpha}_1$, and corresponding standard error also admit a formal test for a trend break in outcomes after 1964, when the federal legislation first took effect. The coefficient, $\tilde{\alpha}_2$, allows the effects of the legislation to differ in the longer (1969-onwards) and the shorter terms (1965-1968). Specifications for employment outcomes are analogous but estimated at the aggregated *nos* level as previously described.

2.3.2.4 Standard Error Calculations

In all regressions for research design 1, we cluster standard errors to correct for heteroskedasticity and account for an arbitrary covariance structure at the state-group level (Huber, 1967; White, 1980; Arellano, 1987; Bertrand et al., 2004). Because we only have 21 state groups, our tables also report p-values for tests of two null hypotheses, $\tilde{\alpha}_0 = 0$ and $\tilde{\alpha}_1 = 0$, from a wild cluster bootstrap procedure with 499 replications (Cameron et al., 2008).

2.4. Results: Using Pre-Existing State Equal Pay Laws to Quantify the Effects of the Federal Anti-Discrimination Legislation on Labor-Market Outcomes

Figure 2.4 presents event-study estimates for three different specifications: one that includes only industry-occupation-state-group fixed effects, year fixed effects, and demographic controls (model 1), one that adds industry-year and occupation-year fixed effects to model 1 (model 2), and one that adds state-group-by-year-of-birth fixed effects to model 2 (model 3). The estimates are highly robust to additional controls. The three models show that wages grew more slowly for women in states without equal pay laws between 1949 and 1963 relative to states with equal pay laws, but this pattern reversed after 1964. The event-study

[16] Note that the terms, $\tilde{\alpha}_3 t + \tilde{\alpha}_4 \mathbb{1}(t > 1964)t + \tilde{\alpha}_5 \mathbb{1}(t > 1968)t$, are not identified due to the inclusion of year fixed effects.

[17] For a discussion of pre-trend adjustments, see Freyaldenhoven et al. (2019) and (Rambachan and Roth, 2023).

coefficients in Figure 2.4a show that women's wages in states without equal pay laws rose by 7.3 log points (s.e. 1.9) more than in other states between 1964 to 1965, followed by more gradual gains through the late 1960s.[18]

The timing of effects helps alleviate concerns that our results are driven by several other factors, such as the differential effects of the 1961 FLSA amendments, which raised the minimum wage and increased coverage (Bailey et al., 2021),[19] and the adoption of Executive Order 11375, which prohibited sex-based discrimination by the federal government after November 1967 and federal contractors after October 1968. The timing of these effects also alleviates concerns that our results are driven by the 1966 Amendments to the FLSA (effective in 1967), which increased the minimum wage and expanded its coverage, or 1967 revisions to the ASEC sampling frame and definition of employment: the estimates show little change between 1966 and 1967.

[18]This estimate is the event-study coefficient on the year 1965 for the model 2 specification (Appendix Table B.5). Appendix Figure B.2 shows that results are similar when examining log hourly or annual wages instead of log weekly wages, which addresses the concern that our results might be driven by measurement error in the weeks or hours worked variables. We construct log hourly wages as log annual wages minus the sum of log weeks worked and log hours worked, using the procedure described in footnote 10 to calculate mean log weeks and hours within categories when necessary. In addition, Appendix Figure B.3 shows the robustness of our findings to winsorizing up to the tenth percentile of the 1960-1964 wage distribution for women, which is equivalent to around one-half of the 1964 minimum wage, which covered fewer workers and was higher relative to more recent periods. One-half the minimum wage is similar to Katz and Murphy (1992) and more aggressive than Blau and Kahn (2017), whose average "too-low-wage" is 29 percent of the federal minimum wage. Appendix Figure B.4 shows that our results are similar when limiting to a sample of more attached workers, Appendix Figure B.5 shows that our estimates are robust to controlling for education, and Appendix Figure B.6 provides a similar conclusion when dropping states that adopted equal pay laws between 1959 and 1962.

[19]The 1961 FLSA raised the minimum wage for previously covered workers from \$1 to \$1.15 an hour effective in September 1961 and \$1.25 per hour in September 1963. If our estimates capture the fact that women were disproportionately affected by the 1961 FLSA's minimum wage hikes, we expect to see gains in their wages in 1962 and 1964. Instead, Figure 2.4a shows gains in 1965, which occurred in the aftermath of the Equal Pay Act's implementation. In addition, the 1961 FLSA extended coverage to around 663,000 workers who were paid less than the minimum wage and worked primarily in large retail enterprises and construction (Martin, 1967). For previously uncovered workers, a minimum wage of \$1 per hour was implemented in September 1961, raised to \$1.15 per hour in September 1964, and again raised to \$1.25 per hour in September of 1965. If our empirical strategy is capturing the fact that women's wages were disproportionately affected by the FLSA's expansion in coverage, one would expect to see gains in their wages in each of the three years when the minimum wage for this group was raised: 1962, 1965, and 1966. Instead, Figure 2.4a shows only one large increase in their wages in 1965, which occurred in the aftermath of the Equal Pay Act's implementation. In addition, the estimated wage increases are nearly identical when excluding individuals employed in retail trade and construction (Appendix Figure B.7), the industries which experienced the largest expansion in coverage under the 1961 FLSA (Martin, 1967) Moreover, if increases in the minimum wage or FLSA coverage are driving these findings, we would expect to find some increases for men's wages in the years of these changes. The estimates, however, show little evidence of a trend-break in men's wages in 1965 overall or below the median (Appendix Figure B.8). Regarding the role of the 1966 FLSA, Bailey et al. (2021) and Derenoncourt and Montialoux (2021) find effects of the legislation in 1967 after it was implemented. However, Figure 2.4a shows striking wage gains for women in 1965 before this legislation took effect and little change in 1967. In summary, the evidence is inconsistent with the 1961 or 1966 FLSA driving the results.

Our three-part linear spline specification averages across small ASEC samples (and noisy estimates) in the early 1960s, which Table 2.1 presents and Figure 2.4 plots for our preferred model. The event-study estimate for 1968 (Appendix Table B.5) is almost identical to the spline estimate of 8.7 log points (s.e. 2.1, Table 2.1, column 1). The spline also admits a formal pre-trend test, which shows no differential change in women's wages (column 1). Finally, the spline estimates confirm a statistically significant, positive trend-break in women's wages after 1964 in states without equal pay laws (2.2 log points, s.e. 0.5).

These estimates do not include changes after 1968, which are also noteworthy although more tenuously related to the 1964 implementation of the Equal Pay Act and 1965 implementation of Title VII of the Civil Rights Act. The event-study estimates show a slight increase in women's wages around 1972, which corresponds to changes in the coverage and enforcement of anti-discrimination legislation. For example, Title IX of the 1972 Educational Amendments amended the Equal Pay Act to include executive, administrative, and professional workers (who were initially excluded from the federal law's coverage as an amendment to the FLSA). The EEOC's active enforcement of Title VII's sex provisions increased in earnest after the U.S. Supreme Court's first decision in *Phillips v. Martin Marietta Corporation* (1971), which ruled that an employer cannot hire men with young children while maintaining a policy to prohibit hiring women with young children. The amendments to the Equal Employment Opportunity Act in 1972 also gave the EEOC the authority to pursue independent lawsuits and expanded Title VII coverage of individuals employed by the government and smaller firms (P.L. 92-261).

The absence of similar changes in men's wages helps rule out the hypothesis that broad changes in labor markets or policies—rather than federal anti-discrimination legislation—are driving these results. Using the same specification and men's wages as the dependent variable, we find some evidence of gains in men's wages in states without equal pay laws after the mid-1960s (consistent with Figure 2.3b). However, gains in men's wages are entirely absent between 1964 and 1965 when the effects for women are largest. Figure 2.4b shows that men's wages in states without equal pay laws rose slightly before the legislation took effect (in 1963), failed to grow between 1964-1965 after the anti-discrimination legislation was implemented, and increased slightly in 1967 following the implementation of the 1966 FLSA amendments. Highlighting the benefits of event-study analyses, these mistimed effects show up in the spline estimates as a positive trend-break for men after 1964 (Table 2.1, column 2), but with a magnitude about half as large as for women. For completeness, we report estimates from a triple-differences specification that uses men as an additional comparison group. However, the pre-treatment gains for men in the event-study suggest that this approach may understate women's wage gains.

The lack of wage changes among men also helps rule out that the Civil Rights Act's provisions to combat racial discrimination are driving these results (Heckman and Payner, 1989; Donohue and Heckman, 1991). While Southern states were less likely to have pre-existing equal pay laws, an obvious counterpoint is that the timing of women's wage gains, which occur between 1964 and 1965 (Figure 2.4a), largely pre-date the Civil Rights Act, which took effect in July of 1965, and are absent among men (Figure 2.4b), who show no wage gains between 1965 and 1966. It seems unlikely that the Civil Rights Act's race provisions would have such large effects between July and December 1965 but smaller effects in the subsequent years, when the legislation was in place for the full 12 months covered in the ASEC earnings question. A third piece of evidence is that the estimates are not statistically different for White women (8.4, s.e. 2.0) and Black women (8.5, s.e. 5.1) (Appendix Table B.6, columns 3 and 4).

Altogether, the results suggest that the Equal Pay Act and Title VII boosted wages of working women—a group accounting for roughly one third of the U.S. labor force in 1960. If labor markets were perfectly competitive and women were being paid their marginal product, differentials in pay would arise due to differences in men and women's skills. Consequently, mandating equal pay would encourage firms to lay women off, reduce their hours, and hire more men. However, if women's labor-supply to a firm is not perfectly elastic, firms might counterintuitively respond to the equal pay act by increasing the employment of women in response to higher mandated wages for them (Manning, 1996).

Figure 2.5 describes the evolution of the log of the number of employees and the log of annual hours worked by states' equal pay law status. The time series show different pre-trends in both outcomes for both sexes, as employment in states without equal pay laws caught up with the rest of the country. The event-study estimates in Figure 2.6 formalize these comparisons and also adjust for covariates, which also illustrates a violation of the parallel-trends assumption necessary for valid inference using a standard differences-in-differences estimator. (A differences-in-differences estimator would attribute the increase in the average difference in employment after 1964 to federal anti-discrimination policy, even though it is driven by a positive pre-trend, which is why we favor the spline in this context). Consistent with the visual impression in Figure 2.6, we find no trend-break after 1964 in women's employment or hours worked relative or relative to these outcomes for men, suggesting the legislation had little effect on women's employment at the extensive or intensive margins (Table 2.1).

In summary, these findings suggest that the Equal Pay Act and Title VII increased women's wages rapidly. To put these effect sizes in perspective, our preferred wage estimate from column 1 of Table 2.1 (8.7 log points) is just over half of the average within-occupation weekly wage gap (17 log points) in the 1963 OWS (Appendix Table B.3, column 3). There is little evidence from this first empirical strategy of a decline in women's employment, which is consistent with Manning's (1996) findings of labor-market monopsony for women in the U.K. As state-level variation in pre-existing equal pay laws limits our ability to rule out alternative hypotheses, we use a second and complementary research design to narrow the scope for omitted variables.

2.5. Research Design 2: Variation in the Incidence of Anti-Discrimination Legislation using the 1960 Gender Pay Gap

Our second research design also hypothesizes that the Equal Pay Act and Title VII—if effective—should have larger effects after 1964 in jobs with more pre-existing sex discrimination. Under the assumption that a larger 1960 gender gap in pay is correlated with more sex discrimination, we expect larger relative wage gains after 1964 for women in jobs with larger gender gaps. An additional benefit of this approach is that it allows us to account for state-level shifts in labor demand or supply, policies, and economic conditions, which could confound the state equal pay law design.

2.5.1 The 1960 Gender Gap as a Proxy for Labor-Market Discrimination

We do not observe jobs or establishments in the censuses or ASEC, but we compute the gender gap in single-digit industry (n), occupation (o), and state group (s) "job cells." We rely on the 1960 Census (rather than the 1964 ASEC), because the census offers a much larger sample size which yields more reliable gender wage gap estimates for a larger number of industry-occupation-state-group cells and mitigates concerns about mean reversion.[20] Nine single-digit industries, eight single-digit occupations, and 21 state groups yield 1,512 potential job cells. We exclude from our analysis 562 cells that have fewer than ten women or ten men working full-time in the 1960 Census and eight that have no observations in the ASEC during our period of interest.[21] Our final sample consists of 942 industry-occupation-state-group job cells. For each job cell, we construct the unconditional gender wage gap in mean log hourly wages using the 1960 Census, $\widehat{\text{Gap}}_{nos} = \ln(W^m_{nos}) - \ln(W^w_{nos})$, where m denotes

[20]The 1960 Census has over 600,000 women in the wage earner sample, whereas the 1964 ASEC has around 6,000 such women, allowing us to construct only 75 job cells. If a high gender gap (due to lower women's wages) in a job cell in the 1964 ASEC reflects sampling variation, these job cells would tend to see higher wage growth for women in the year afterwards due to mean reversion. Using the 1960 Census to measure the gender wage gap eliminates this mechanical relationship.

[21]Included job cells are listed in Appendix Table B.8 and excluded job cells are listed in Appendix Table B.9. Appendix Table B.10 describes the number of observations by sex, year, occupation, and industry.

men and w women, and the variable describes the extent to which men out-earn women.[22]

2.5.2 Descriptive Evidence that Federal Legislation Was More Effective in Jobs with Larger 1960 Gender Gaps

A key assumption of our approach is that a larger gender gap in wages in 1960 is correlated with greater sex discrimination. It is difficult to verify this assumption directly. However, if this assumption does not hold or the federal legislation was ineffective, we should find no association between the 1960 gender gap and subsequent growth in women's wages. We begin by presenting descriptive evidence from the 1960 and 1970 Censuses regarding the association between the gender gap, $\widehat{\text{Gap}}_{nos}$, women's wages, and their representation in different job cells. Figure 2.7a shows that the share of employees that are women differs considerably across industries and occupations, but there is little relationship between the female employment share in a job cell and the 1960 gender gap. On the other hand, Figure 2.7b shows that the gender gap tends to be much larger in lower paying job cells, many of which were in services and retail sales (slope coefficient: -1.9, s.e. 0.2). (Of course, this relationship is not causal and could reflect some selection of women with more skill into better paying jobs, and vice versa.) Reassuringly, these findings hold when accounting for sampling variation using a split sample instrumental variables (IV) approach (slope coefficient: -1.9, s.e. 0.2; Inoue and Solon 2010), or when accounting for transitory wage shocks using the 1940 gender wage gap as an IV (slope coefficient: -2.0, s.e. 0.2).[23] Of course, this relationship is not causal and could reflect some selection of women with more skill into better paying jobs, and vice versa.

To motivate our research design, Figure 2.8a plots the change in women's relative wages over the 1960s against the 1960 gender gap in wages. Each point represents the difference in outcomes between women and men for an industry-occupation-state-group cell, and the size of each point represents the number of women working in the cell in 1960. Consistent with the Equal Pay Act and Title VII ameliorating pay discrimination and increasing women's relative wages, we find that women's wages grew more than men's during the 1960s in job cells with larger gender gaps at the start of the decade. The similarity of the results when using the split sample IV (slope coefficient: 0.35, s.e. 0.04) or 1940 gender gap IV (slope coefficient:

[22]We use the sample of full-time workers to calculate the gender wage gap. The gender wage gap is nearly identical when we control for individuals' demographic and education characteristics using a quadratic in age, an indicator for workers of a nonwhite race, and a set of indicators for each year of schooling. The correlation between the unadjusted gender gap and the covariate-adjusted gender gap is 0.97 (Appendix Figure B.9a), so we use the unadjusted gender gap for simplicity. Appendix Figure B.9b shows that the gender gap in hourly wages is very similar to the gender gap in weekly wages (correlation of 0.98), and Appendix Figure B.9c shows that the gender gap in weekly wages is nearly identical after controlling for demographics and hours worked (correlation of 0.97).

[23]We use the full-count 1940 Decennial Census to compute the gender gap in wages (Ruggles et al., 2021)

0.42, s.e. 0.04) provides reassurance that these patterns are not driven by mean reversion due to measurement error or real transitory shocks to the labor market. Moreover, Figure 2.8d shows that this relationship did not exist in the 1950s, before federal anti-discrimination legislation should have affected sex discrimination in pay. In the 1960s, women's employment and annual hours grew more slowly than men's in job cells where women's relative wages grew more quickly (Figures 2.8b-2.8c). As with wages, these patterns depart from the 1950s, where the gender gap was not predictive of changes in employment (Figures 2.8e-2.8f).

2.5.3 Event-Study and Spline Specifications

We use the following event-study specification to test whether these changes align with the passage of the Equal Pay Act and Title VII:

$$Y_{it} = \sum_{\tau=1949,\tau\neq 1964}^{1974} \theta_\tau D_\tau \widehat{\text{Gap}}_{n(i)o(i)s(i)} + X'_{it}\beta + \gamma_{n(i)o(i)s(i)} + \delta_{s(i)t} + \delta_{n(i)t} + \delta_{o(i)t} + \varepsilon_{it} \quad (2.5.1)$$

The dependent variable, Y_{it}, is log weekly wages of individual i in calendar year $t = 1949, 1959, 1961 - 1974$, and $\widehat{\text{Gap}}_{nos}$ is as defined previously. We interact $\widehat{\text{Gap}}_{nos}$ with a set of year indicator variables, D_τ, and omit 1964, the year the Equal Pay Act became effective in June. Because $\widehat{\text{Gap}}_{nos}$ varies within state groups, the addition of state-group-by-year fixed effects, δ_{st}, allows the analysis to account for unobserved state-level changes in labor markets and policies. The remaining notation remains as described previously. Specifications for employment outcomes are analogous to equation (2.3.2) but replace NoEPL_s with $\widehat{\text{Gap}}_{nos}$ on the right side in equation (2.5.1) and add state-group-by-year fixed effects. Standard errors are corrected for heteroskedasticity and arbitrary correlation within industry-occupation-state-group cells (Huber, 1967; White, 1980; Arellano, 1987).[24]

Our parameters of interest, θ_τ, capture changes across time in the correlation of women's wages with the gender pay gap in 1960. If federal legislation reduced labor-market discrimination against women, we expect women's wages to increase more after 1964 in job cells with a larger gender gap (i.e., $\theta_\tau > 0$ for $\tau > 1964$). Testing for changes in this correlation before 1964 also helps rule out potential confounders and assess the validity of the parallel-trends assumption. For instance, if women's productivity and work intensity were increasing differentially in jobs with larger gender gaps pre-dating the legislation, we would expect θ_τ to increase in years prior to 1964, leading us to reject the parallel trends assumption.

[24] Appendix B.3 uses a combination of a parametric bootstrap and a Bayesian bootstrap to show that accounting for sampling variability in estimates of the gender gap variable leads to standard errors that are very similar to those reported in the main tables.

We summarize the event-study estimates using a three-part spline, or

$$Y_{it} = \tilde{\theta}_0 \widehat{\text{Gap}}_{n(i)o(i)s(i)} t + \tilde{\theta}_1 \mathbb{1}(t > 1964) \widehat{\text{Gap}}_{n(i)o(i)s(i)} t + \tilde{\theta}_2 \mathbb{1}(t > 1968) \widehat{\text{Gap}}_{n(i)o(i)s(i)} t \\ + X'_{it} \tilde{\beta} + \tilde{\gamma}_{n(i)o(i)s(i)} + \tilde{\delta}_{s(i)t} + \tilde{\delta}_{n(i)t} + \tilde{\delta}_{o(i)t} + \tilde{\varepsilon}_{it} \tag{2.5.2}$$

where notation remains as previously defined.

2.6. Results: Using the 1960 Gender Gap in Wages to Quantify the Effects of the Federal Anti-Discrimination Legislation on Labor-Market Outcomes

Figure 2.9a presents the event-study results for women, and Table 2.2 summarizes the event-study estimates using the spline. Point estimates and confidence intervals are scaled by the mean gender gap in the 1960 Census (equal to 0.374).[25] Model 1 includes demographic covariates and industry-occupation-state-group and year fixed effects. Model 2 adds state-group-by-year fixed effects to model 1, and model 3 adds industry-year and occupation-year fixed effects to model 2.

Consistent with the Equal Pay Act and Title VII reducing labor-market discrimination against women, the data show that women's weekly wages increased by 10 log points (s.e. 2.3) between 1964 and 1968 in job cells with the average 1960 gender gap in pay (Table 2.2, column 1). The magnitude of this estimate is equivalent to 58 percent of the average within-occupation weekly wage gap in the 1963 OWS (Appendix Table B.3, column 3). Wages rise almost immediately following the legislation and remain stable between 1967 and 1970. Although changes in women's wages are not correlated with the gender gap after the implementation of the 1966 FLSA in 1967, the correlation again increases between 1970 and 1973. This timing is reminiscent of similar patterns in our first research design and corresponds to the Education Amendments broadening the coverage of the Equal Pay Act and the Supreme Court's 1971 decision and the Equal Employment Opportunity Act of 1972 strengthening and expanding the legal basis for enforcing Title VII's sex provisions.

These estimates are not only robust across specifications, they are also robust to using annual or hourly wage earnings (Appendix Figure B.10), winsorizing low wage levels (Appendix Figure B.11), limiting the sample to more attached workers (Appendix Figure B.12), controlling for education (Appendix Figure B.13), accounting for measurement error or mean reversion following transitory labor-market changes in the 1950s or early 1960s (Appendix Figure B.14), excluding industries that saw substantial increases in minimum wage coverage under the 1961 FLSA (Appendix Figure B.15), and including state-by-birth-cohort fixed effects (Appendix Figure B.16). In contrast, we find no evidence of wage gains for men

[25]See Appendix Table B.11 for the event-study coefficients and standard errors in numerical form.

(Figure 2.9b; Table 2.2, column 2), which narrows the scope for alternative labor-market or policy explanations. Recent work on differences-in-differences estimators highlights difficulties in interpreting the magnitudes of event-study regressions with a continuous treatment variable and treatment-effect heterogeneity, even in settings like ours without a staggered treatment timing (Callaway et al., 2021). Considering this issue, evidence of limited treatment effect heterogeneity for nos cells with average wages above and below the *nos*-cell median is reassuring (Appendix Figure B.17).

We also explore the heterogeneity in women's wage gains to shed light on the mechanisms for these effects. Following Firpo et al. (2009), we estimate RIF regressions to understand the effects of federal anti-discrimination legislation on the unconditional percentiles of women's log weekly wages. Figure 2.10a shows results, which are scaled by the mean gender gap in the 1960 Census. We find large increases in women's wages at the 10th and 25th percentiles after the legislation took effect (31 and 18 log points in 1968, respectively; Appendix Table B.12), which is consistent with the legislation benefiting the lowest-earning women, for whom the gender gap in wages was largest (Figure 2.7b) and for whom convergence in the gender gap was the most rapid in the 1960s (Figure 2.1b). RIF-regressions using only the 1950, 1960, and 1970 Decennial Censuses yield similar results (displayed as single points), which ameliorates concerns that the estimates are driven by revisions in the ASEC sampling frame. In contrast, percentiles above the median show little evidence of a trend break after 1964 or any change through the 1970s. The same specification for men's wages shows little change at any point in the distribution (Figure 2.10b), which mitigates concerns that the results are driven by broad labor-market trends or policies. These findings suggest that the federal anti-discrimination legislation reduced the gender wage gap and also the wage gap in earnings between the highest and lowest paid women.[26]

As a final check on the validity of the results, we bring both research designs together to examine whether women's relative wages changed differently after 1964 in jobs with a higher gender pay gap in states without a pre-existing equal pay law. If state equal pay laws were somewhat effective in reducing sex discrimination, we expect women's wages to increase by more in job cells that had the same 1960 gender wage gap in states without pre-existing equal pay laws relative to states with equal pay laws. Said another way, effective prior legislation implies that the correlation of the same gender gap in pay in 1960 with sex discrimination should be weaker. Columns 4-5 of Table 2.2 confirm this prediction. In the 22 states with pre-existing equal pay laws, we find women's relative wages grew by 6.0 log points

[26]Appendix Table B.13 examines effect heterogeneity across other population subgroups. The results show that the within-job cell wage gains for women following the Equal Pay Act and Title VII were pervasive. Wage increases are evident for White workers, which addresses the concern that our results are driven by provisions in the Civil Rights Act targeting racial discrimination.

at the mean gender gap (s.e. 3.9, column 4). This finding is consistent with the legislation having a meaningful effect in states with pre-existing equal pay laws and the first research design understating the effects of the legislation in states without pre-existing equal pay laws. In states without equal pay laws, we find women's relative wages grew by much more after 1964—an increase of 16.2 log points by 1968 (s.e. 3.4, column 5). Altogether, this evidence suggests an important role for anti-discrimination legislation—at the state level and then at the federal level—in reducing the gender gap in wages.

In light of these large wage gains for women, how did the legislation affect their employment? Some direct evidence on this question comes from reports around the time the Equal Pay Act was passed. On June 14, 1964, the *Washington Post* interviewed different employers and reported:

> ...the head of a new Virginia manufacturing plant put it: "We had planned to employ women in some of our light manufacturing jobs, but we decided against it because of anticipated complications arising from the equal pay law." An Ohio manufacturer said his plant would downgrade some job classifications for women and reassign higher-level, higher-paying duties to men...
>
> Many employers said they would hike women's wages to bring them into line with men's. Some firms said they would equalize salaries now, but in the future would segregate male and female job classifications.

Although Title VII would make this type of behavior illegal the following year, honest reporting before it passed provides important context. Notably, no employer said they would fire women in response to the Equal Pay Act—which is consistent with our findings when examining employment responses using state equal pay laws. However, employers indicated that they planned to change job classifications and hiring, which could show up as industry-occupation level changes in women's employment in the longer term.

Figure 2.11 tests this prediction using the event-study and spline specifications.[27] In 1966, when women's wages soared in jobs with higher 1960 gender gaps, the number of female or male employees or annual hours worked changed little. Although Table 2.2 reveals a larger trend-break after 1964 for women than men, which translates into a reduction in employment of 11.8 log points by 1968 at the mean (s.e. 4.7, column 1) for women versus a 6.2-log-point decline for men (s.e. 2.9, column 2), the difference between the two groups is not statistically significant (column 3). The decline in women's employment in states without pre-existing equal pay laws is larger (where women's wages grew more quickly), but neither estimate is statistically significant at conventional levels. In these states, the number of female employees

[27]See Appendix Table B.14 for the event-study coefficients and standard errors in numerical form.

relative to male employees experienced a sizable and marginally statistically significant decline of 11.2 log points (s.e. 6.9, column 5), although their relative number of annual hours did not fall discernibly.[28] In contrast, in states with pre-existing equal pay laws where wages grew by less than one-third the amount by 1968, the trend break in employment and annual hours worked was much smaller and statistically insignificant.[29]

In summary, this evidence strongly suggests that the Equal Pay Act and Title VII lifted the wages of working women with some evidence that their employment fell in the longer term. Similar to what was reported in the *Washington Post*, different employers likely varied in their response to the legislation, which is difficult to detect without more information on jobs and establishments.

2.7. How the Equal Pay Act and Title VII Affected the Gender Gap in Wages

Almost 60 years after the Equal Pay Act and Title VII passed, little quantitative work suggests this legislation reduced pervasive pay discrimination against women in the 1960s. Studies have noted the roles of Title VII and federal affirmative action mandates under Executive Order 11375 in facilitating women's wage and employment gains and increasing their enrollment in colleges and professional schools in the 1970s and later (Beller, 1979, 1982a,b; Leonard, 1984; Carrington et al., 2000; Kurtulus, 2012; Blau and Kahn, 2017; Helgerman, 2023). This paper provides new evidence that federal anti-discrimination legislation—especially the Equal Pay Act—had direct and larger effects on sex discrimination in the 1960s than previously understood.

Using two complementary research designs, we find that federal legislation prohibiting sex-based pay and employment discrimination led to large increases in women's wages, especially in lower-paying jobs where the "equality of work" was more easily measured and federal investigations of compliance with the minimum wage were focused. After the legislation took effect, women's wages grew by around 11 percent in jobs with the average gender gap, with most of these effects benefitting women in the lower half of the weekly wage distribution. Importantly, anti-discrimination legislation appears to have had little effect on median wages among full-time, full-year workers, which has been the focal statistic released annually by the Census Bureau (Figure 2.1a). However, our estimates of larger gains among lower-wage

[28] The p-values on the test of the null hypothesis that estimates in columns 4 and 5 of Table 2 are equal are 0.20 in panel A (wages), 0.07 in panel B (employment), and 0.19 in panel C (annual hours worked).

[29] Appendix Table B.13 shows that the employment effects of the Equal Pay Act and Title VII are large but imprecise across subgroups. Employment fell by 5.0 log points (s.e. 5.0, panel B, column 3) at the mean gender gap for White women. For Black women, the point estimate implies a decline in employment of 75 log points, but the standard error is very large (32 log points), leaving considerable uncertainty about the true effect. Employment among women with less than 12 years of education also experienced a large decrease with a large standard error.

workers in the mid-1960s correspond well to the gains below the median in the timeseries during this period (Figure 2.1b) (Bailey et al., 2021). Consistent with firms having some monopsony power, the Equal Pay Act and Title VII had little effect on women's employment in the short run. In the longer-term, however, historical evidence suggests that some firms shifted their hiring away from women workers, which tracks with contemporary reports and scholars' critiques of the legislation.

The stability of the gender earnings ratio at the mean and median during the 1960s masks two opposing trends—an observation that helps reconcile the magnitudes in this study with those in the aggregate time series in Figure 2.1. First, economic forces pre-dating the legislation put downward pressure on women's relative pay in the 1960s. After World War II, strong economic growth drove up wages, but it raised wages for men faster than for women. Trends pre-dating the 1960s imply that the gender wage ratio would have fallen rather than stabilizing in the absence of federal legislation. We are not the first to point this out. Beller (1979) argues that Equal Employment Opportunity laws staved off a larger 7-point increase in the earnings gap in the 1970s, and others, notably Blau and Kahn (2017), suggest that the increase in female labor-force participation during the 1960s may have masked the effects of the legislation in the aggregate time series.

Second, the estimates using the gender gap design reflect large changes in the within-job component of the gender gap, which is smaller than the overall gender gap. A Kitagawa-Blinder-Oaxaca decomposition shows that 69.7 percent of the 1960 gender gap in hourly earnings is attributable to differences within-industry-occupation-state-group cells used in our analysis.[30] Assuming the legislation had little effect on the allocation of workers across job cells, our estimate of a 10-log-point increase at the mean gender gap within job cells (Table 2.2) would translate into a 7.0 point gain in the aggregate gender gap.

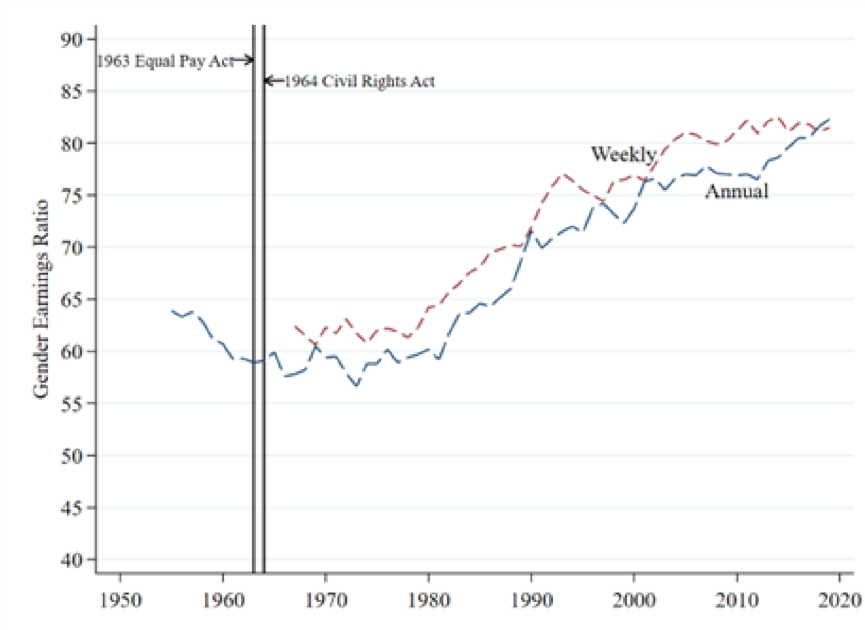
Ignoring shifts in workers across jobs, these two countervailing changes imply a net gain of 5.2 log points at the mean (7.0 less 1.8 log points due to the pre-trend). But this change is still larger than observed in the timeseries, likely because changes in firm hiring and promotion behavior, selection, and larger shifts in the economy worked to offset women's wage gains

[30]We calculate this number as the sum over industry-occupation-state-group cells of the difference in the mean log wage for men and women, multiplied by the share of men employed in the cell. This calculation is 62.5 percent when multiplying the within-cell gender wage gap by the share of women employed in the cell. This share is not directly comparable to estimates of occupational segregation because our occupation/industry cells are larger groupings than job classifications. Polachek (1987) similarly finds that only 17-21 percent of gender differences in annual wage earnings in 1960 and 1970 can be explained by occupational segregation, which is similar to the conclusion of Goldin (1990, pg. 71-73). Blau (1977) finds that intra-firm pay differences are a small share of the total gender wage gap in 1970 in office occupations in three Northern cities for establishments with at least 50 employees (Tables 4-6). Using data from 1974 to 1983, Groshen (1991) finds that wage gaps from establishment and job segregation are around 6 percent, whereas occupational segregation accounts for a gap of 11 percent.

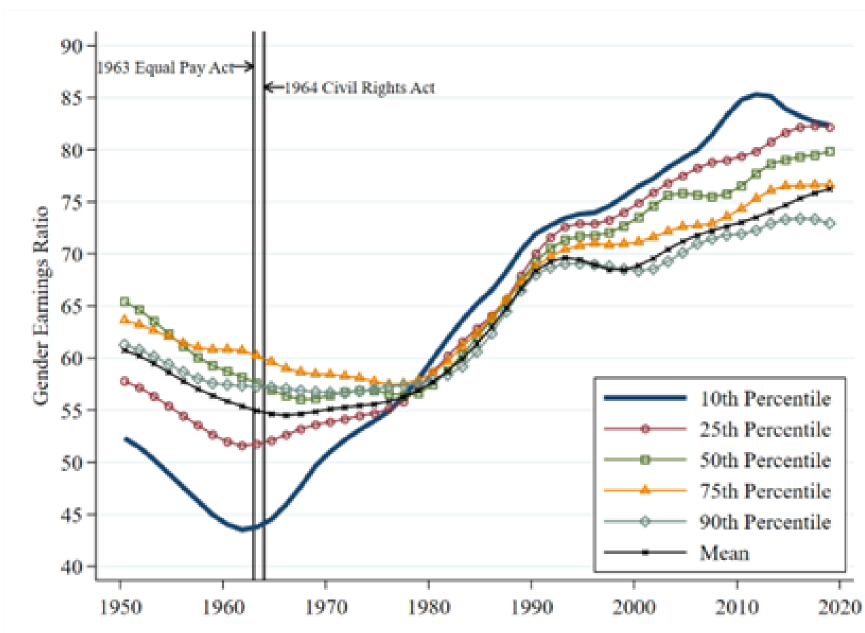
within jobs.

In conclusion, our findings claim an important role for the Equal Pay Act, strengthened by Title VII, in reducing pay discrimination against U.S. women in the 1960s. Yet they also provide a cautionary tale: targeting pay discrimination without sufficient protections against employment discrimination provided leeway for firms to shift how they discriminated, reshaping the gender gap and leading the literature in economics to focus on occupational segregation and litigation to focus on strengthening Title VII over the next sixty years.

Figure 2.1: Estimates of the U.S. Gender Gap in Wage Earnings



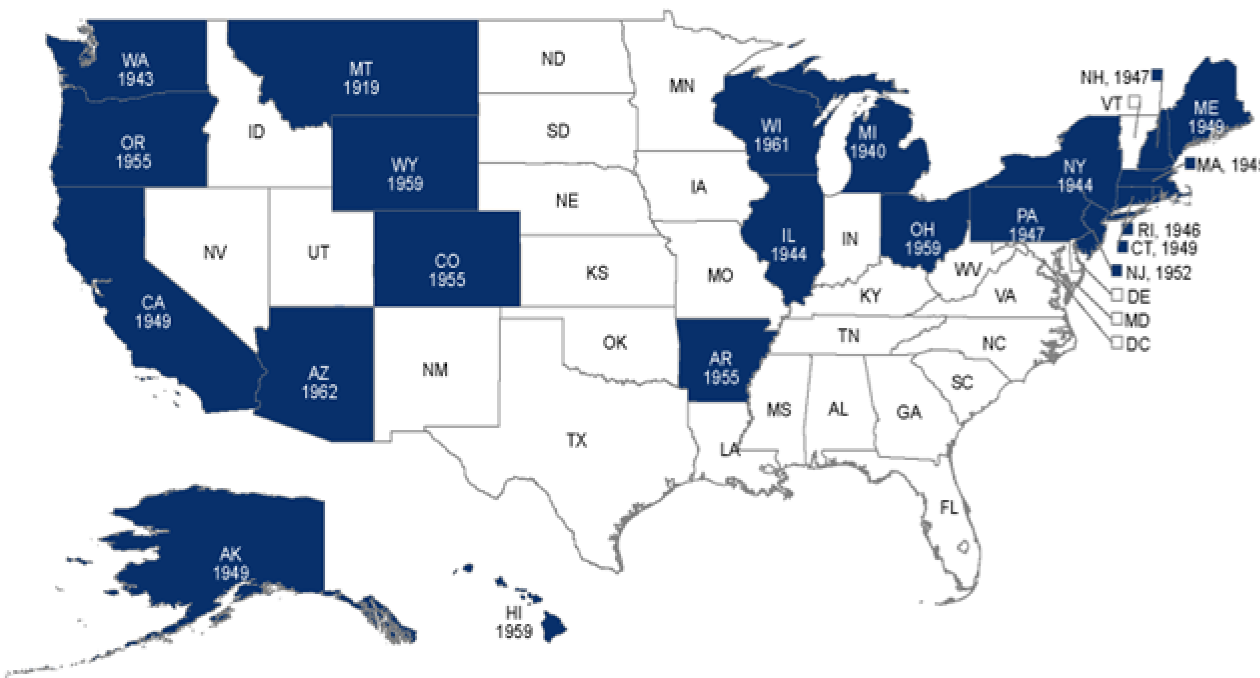
(a) Census Bureau Estimates for Full-Time, Full-Year Workers at the Median



(b) Census/CPS Estimates for Full-Time Workers with at least 27 Weeks of Work in the Previous Year

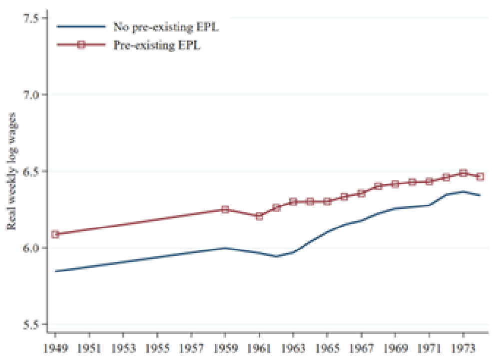
Figure 2.1a plots data on the ratio of median annual and weekly wage and salary earnings of full-time, full-year workers for women relative to men from the following sources: the Census Bureau's Consumer Income (P60) series for 1955 through 1960 (U.S. Census Bureau, 1956, 1958a,b, 1960, 1961, 1962); the female-to-male annual earnings ratio for full-time, full-year workers from DeNavas-Walt and Proctor (2015); and Shrider et al. (2021) for 2015 through 2019. Data on the female-to-male ratio of usual weekly earnings for full time wage and salary workers come from Mellor (1984) for 1967 through 1978, the U.S. Department of Labor (2015) for 1979 through 2014; and Proctor et al. (2016) and U.S. Department of Labor (2021) for 2015 through 2019. Panel B uses a sample of 25–64-year-old, full-time workers working at least 27 weeks in the previous year. We plot the gender earnings ratio at the pth percentile/mean by taking the ratio of the pth percentile/mean of the wage distribution for women over the pth percentile/mean of the wage distribution for men. Panel B sources include the 1950 and 1960 Decennial Censuses and the 1962 to 2020 ASEC (Flood et al., 2022; Ruggles et al., 2023). We linearly extrapolate values for earnings years 1950–1958 and 1960, when Census and CPS data are not available. We smooth the series using a local linear regression with a bandwidth of 2 years.

Figure 2.2: Map of State Equal Pay Laws as of 1963

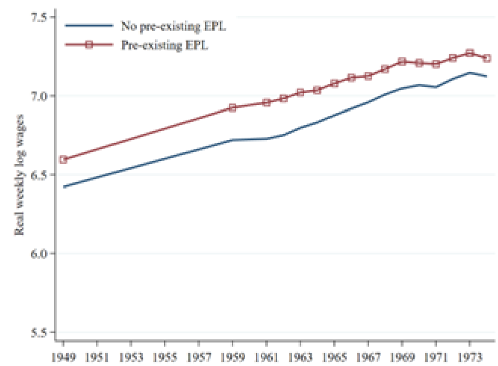


The figure plots the 22 states with equal pay laws in the U.S. as of 1963 (dark blue) and those without such a law (U.S. Congress, 1963). The states with equal pay laws in 1963 are Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Hawaii, Illinois, Maine, Massachusetts, Michigan, Montana, New Jersey, New Hampshire, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Washington, Wisconsin, and Wyoming. The year listed next to each state indicates the year when the state enacted its equal pay law. See also Neumark and Stock (2006).

Figure 2.3: The Evolution of Women's and Men's Weekly Wages in States with and without Pre-Existing Equal Pay Laws



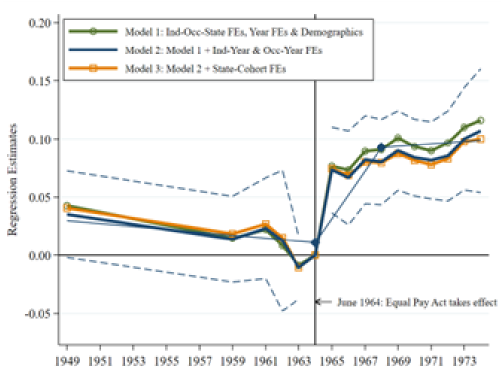
(a) Women



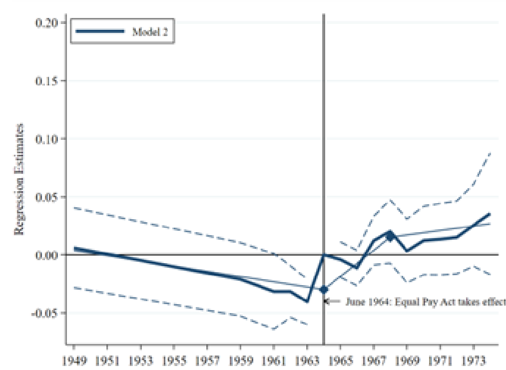
(b) Men

The figure plots the mean of log of weekly wages for women and men in state groups that did not have an equal pay law as of January 1, 1963, and state groups where at least one state did have such a law. *Sources*: Authors' calculations using the 1% sample of the 1950 Decennial Census, 5% sample of the 1960 Decennial Census, and the 1962 to 1975 CPS ASEC (Flood et al., 2022; Ruggles et al., 2023). See text for details on sample selection and exclusion criteria.

Figure 2.4: The Effect of the Equal Pay Act and Title VII on Weekly Wages using Pre-Existing State Equal Pay Laws



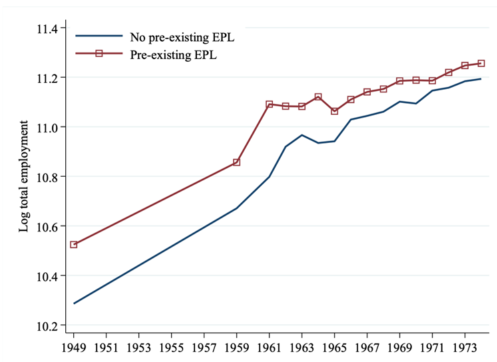
(a) Women



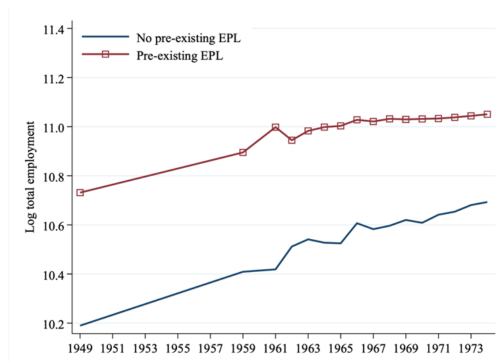
(b) Men

The figure plots the event-study coefficients from equation (2.3.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and an arbitrary correlation within state group (Huber, 1967; White, 1980; Arellano, 1987). The spline specification is based on model 2 of equation (2.3.3). See Appendix Table B.5 for the individual point estimates and standard errors. *Sources*: See Figure 2.3.

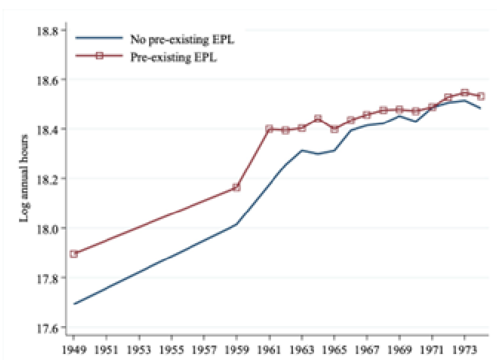
Figure 2.5: The Evolution of Women's and Men's Employment and Annual Hours in States with and without Pre-Existing Equal Pay Laws



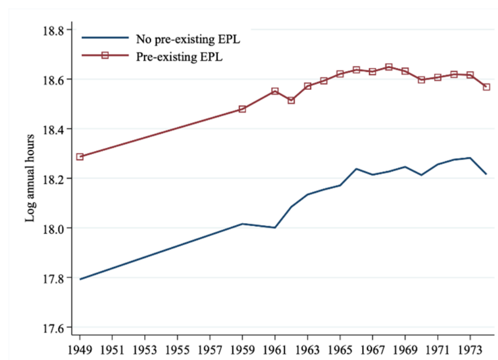
(a) Female Employees



(b) Male Employees



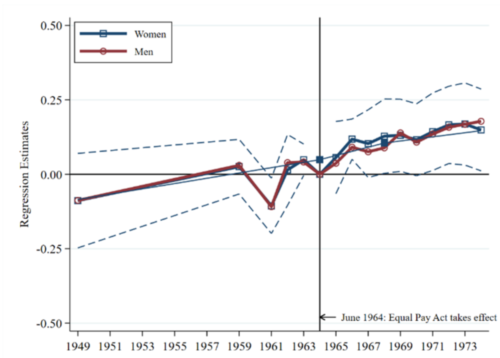
(c) Annual Hours Worked by Women



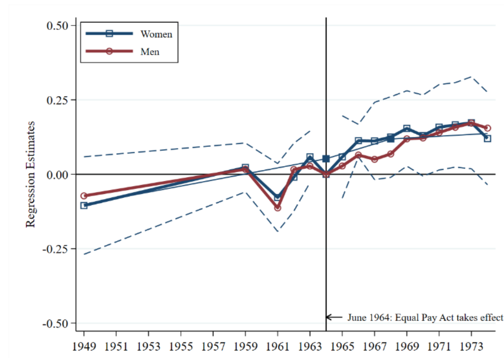
(d) Annual Hours Worked by Men

Figures 2.5a and 2.5b plot the mean of log sum of employees (total employment) within an industry-occupation-state-group job cell for women and men in state groups that did not have an equal pay law as of January 1, 1963, and state groups where at least one state did have such a law. Because the total counts are depressed in 1961-1962 and, to a lesser extent, in 1963-1964, due to issues around whether variables were included in the February CPS, we inflate employment by the inverse of the fraction of observations in each year coded as a February-March match. Figures 2.5c and 2.5d show analogous results for the mean of log annual hours worked, which are adjusted using the same inflation factor. *Sources*: See Figure 2.3.

Figure 2.6: The Effect of the Equal Pay Act and Title VII on Employment using Pre-Existing State Equal Pay Laws



(a) Log Number of Employees

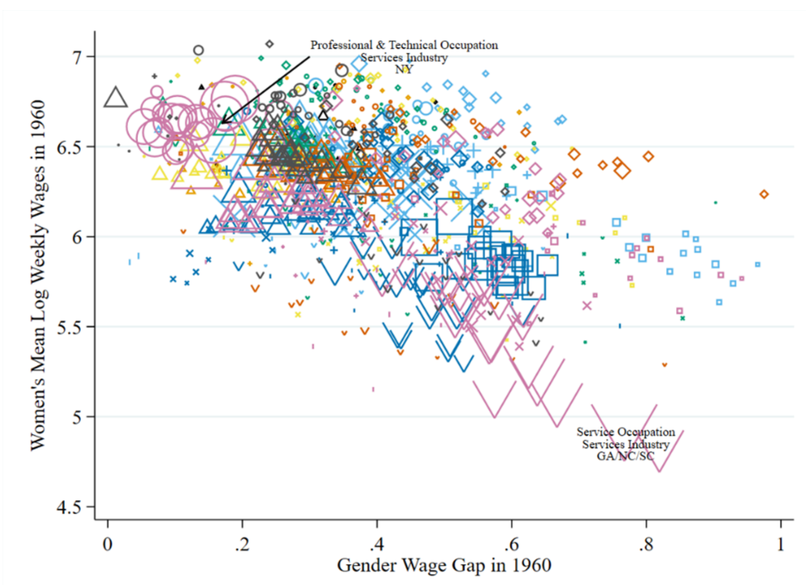


(b) Log Annual Hours Worked

The figure plots the event-study coefficients from model 2 of equation (2.3.2). Dependent variables are indicated in subtitles. Dashed lines are 95-percent, pointwise confidence intervals for women, where standard errors have been corrected for heteroskedasticity and an arbitrary correlation within state group (Huber, 1967; White, 1980; Arellano, 1987). See Appendix Table B.7 for the individual point estimates and standard errors. *Sources*: See Figure 2.3.

Figure 2.7: The Correlation of Women's Representation and Wages with the 1960 Gender Wage Gap, by Industry, Occupation, and State-Group Cell

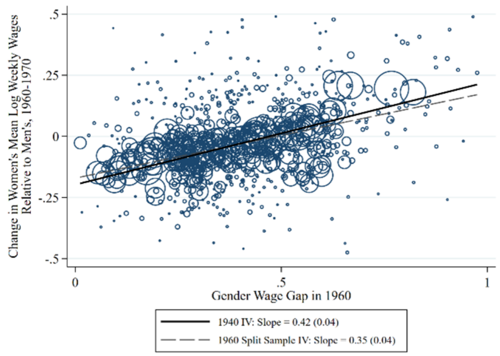
(a) Share of Employees that are Women in 1960



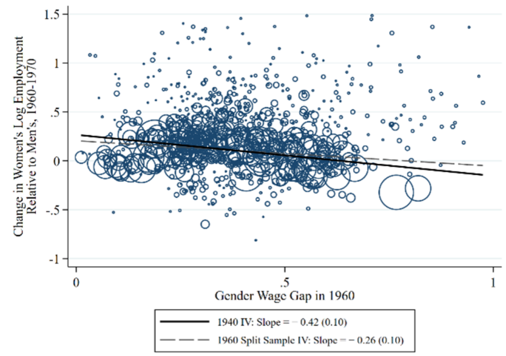
(b) Women's Weekly Wages in 1960

Each marker represents an industry-occupation-state-group job cell. The size of the marker represents the number of women working in the cell in 1960. The color of each marker captures the industry, and the marker shape captures the occupation as shown in the legend. The x-axis is the gender wage ratio (*Gap*), which is calculated as the difference in average log hourly wages for men and women working full time in 1960. The y-axis in Figure 2.7a is the share of employees in each cell in the 1960 Census who are women and in Figure 2.7b is the average log weekly wages for women in the 1960 Census. *Sources*: 5% sample of the 1960 Decennial Census.

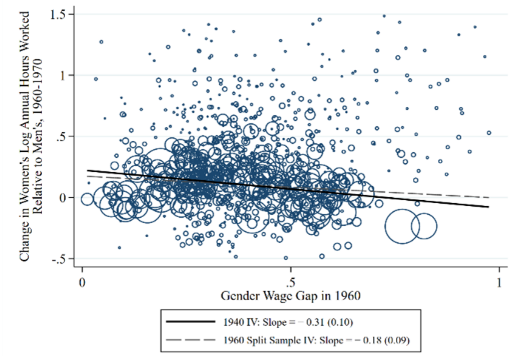
Figure 2.8: Correlation of Changes in Relative Wages and Employment and the Gender Gap in Wages, by Industry, Occupation, and State-Group Cell



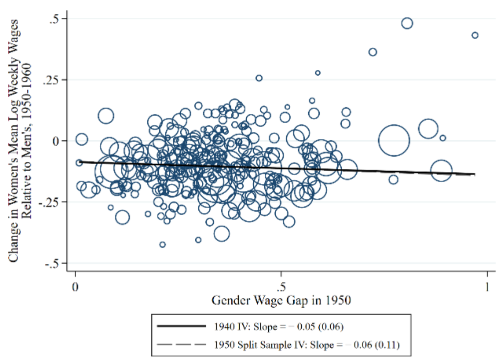
(a) Weekly Wages, 1960-1970



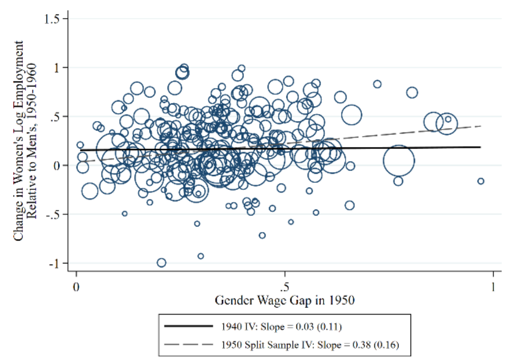
(b) Number of Employees, 1960-1970



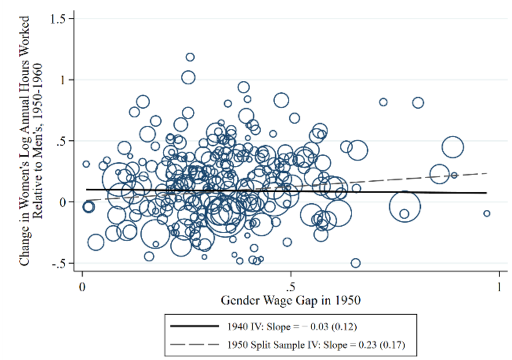
(c) Annual Hours Worked, 1960-1970



(d) Weekly Wages, 1950-1960



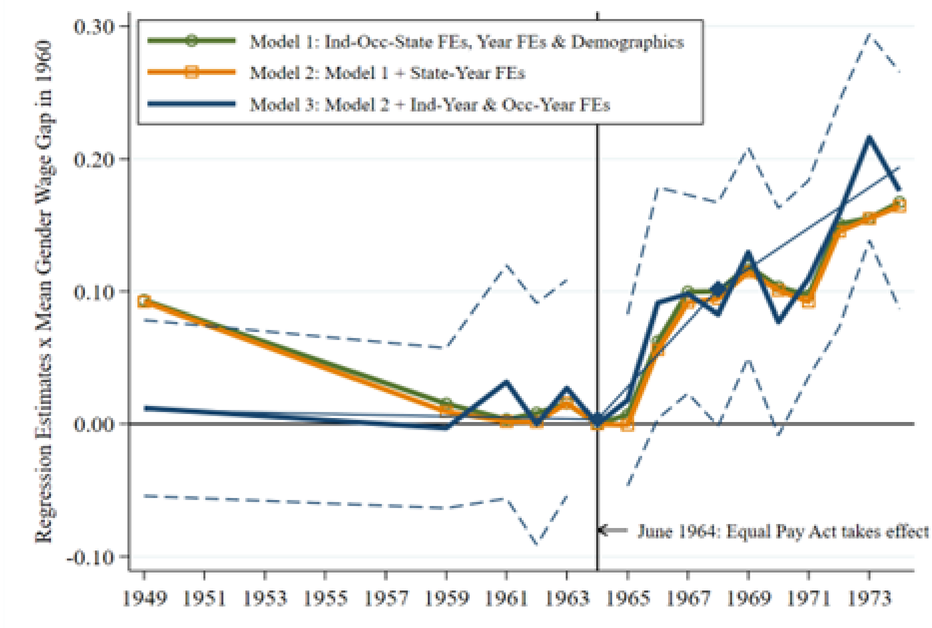
(e) Number of Employees, 1950-1960



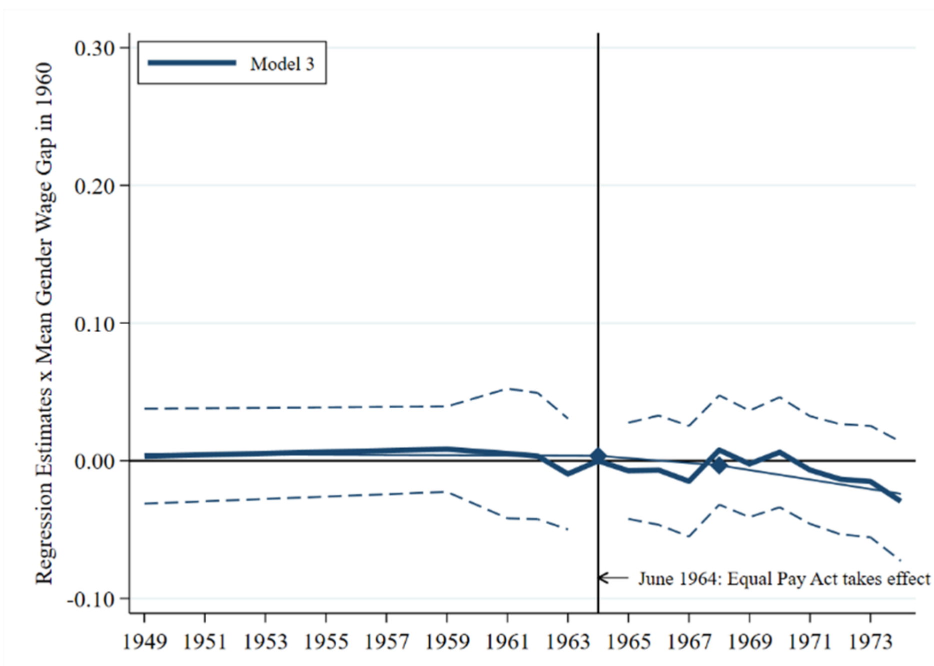
(f) Annual Hours Worked, 1950-1960

Each marker represents the difference in outcomes between women and men for the industry-occupation-state-group cell. The dependent variable in Figures 2.8a - 2.8c is $(Y^f_{nos,70} - Y^f_{nos,60}) - (Y^m_{nos,70} - Y^m_{nos,60})$, where $Y^g_{nos,t}$ is the outcome (average log weekly wages, log number of employees, log sum of annual hours worked) for sex g in year t, where g is either female (f) or male (m). The dependent variable in Figures 2.8d - 2.8f is constructed similarly but uses the change between 1950 and 1960. The size of each marker represents the number of women working in the cell in 1960 (figures 2.8a - 2.8c) or 1950 (figures 2.8d - 2.8f). Figures are limited to cells with variables in the indicated ranges, but regressions are estimated on all observations. The slope coefficient and heteroskedasticity-robust standard error are calculated using a bivariate regression of the outcome on the y-axis against the gender wage gap with weights equal to the number of women in each cell in 1960 (figures 2.8a - 2.8c) or 1950 (figures 2.8d - 2.8f). As described in the text, we use a split sample instrumental variable procedure or use the 1940 gender wage gap as an instrument for the 1960 gender wage gap. *Sources*: Authors' calculations using the 1% sample of the 1950 Decennial Census, 5% sample of the 1960 Decennial Census, and the combined one-percent Form 1 and Form 2 state samples of the 1970 Decennial Census (Ruggles et al., 2023).

Figure 2.9: The Effect of the Equal Pay Act and Title VII on Weekly Wages using the 1960 Gender Wage Gap



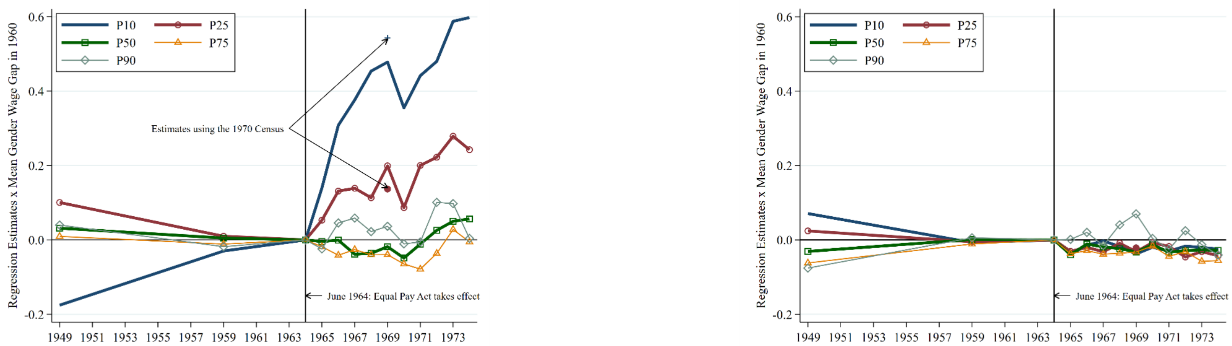
(a) Women's Weekly Wages



(b) Men's Weekly Wages

The figure plots the event-study coefficients from equation (2.5.1) as well as 95-percent, pointwise confidence intervals based on standard errors corrected for heteroskedasticity and an arbitrary correlation within industry-occupation-state-group (Huber, 1967; White, 1980; Arellano, 1987). Dependent variables are indicated in subtitles. The solid thin lines correspond to model 3 spline estimates of equation (2.5.2). Point estimates and confidence intervals are multiplied by the average gender wage gap in the 1960 Census for the relevant sample of women (equal to 0.374). See Appendix Table B.11 for the individual point estimates and standard errors. *Sources*: See Figure 2.3.

Figure 2.10: The Effect of the Equal Pay Act and Title VII on the Distribution of Wages using the 1960 Gender Wage Gap

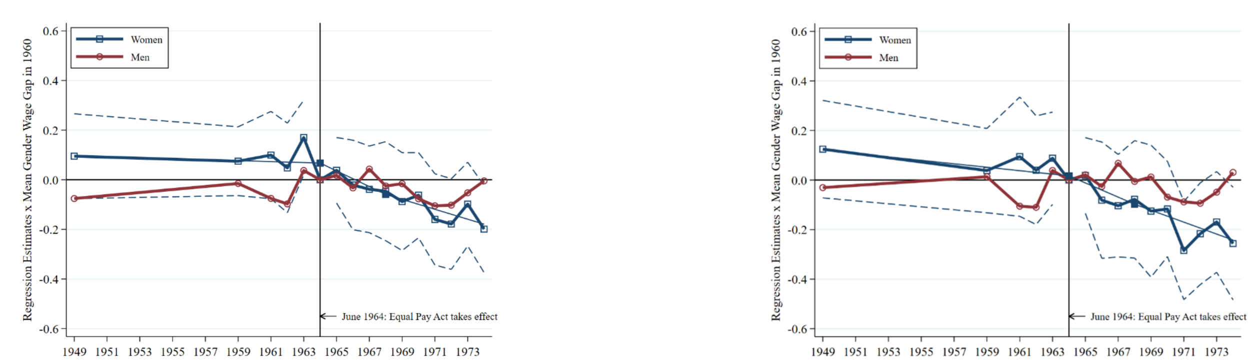


(a) Women's Weekly Wages

(b) Men's Weekly Wages

The figure plots estimates of model 3 of equation (2.5.1) where the dependent variable is the RIF for weekly log wages for women (figure 2.10a) and men (figure 2.10a). Because sample sizes are much smaller in the early ASEC years and because this is a demanding specification, we pool 1959 and 1962-1964 into a single event-study coefficient. Coefficients are scaled by the average gender wage gap (equal to 0.374). Estimates for the 1970 Census are shown for the 10th and 25th percentiles, from a regression estimated using only the 1950, 1960, and 1970 Censuses. See Appendix Table B.12 for the estimates and standard errors. *Sources*: See Figure 2.3 notes and the combined one-percent Form 1 and Form 2 state samples of the 1970 Decennial Census.

Figure 2.11: The Effect of the Equal Pay Act and Title VII on Female Employment using the 1960 Gender Wage Gap



(a) Log Number of Employees

(b) Log Annual Hours Worked

These figures plot the event-study coefficients from model 3 of equation (2.5.1) run on data aggregated at the industry-occupation-state-group-level. Dependent variables are indicated in subtitles. Point estimates and confidence intervals are multiplied by the average gender wage gap (equal to 0.374). Dashed lines are 95-percent, pointwise confidence intervals for women and based on standard errors corrected for heteroskedasticity and an arbitrary correlation within industry-occupation-state-group (Huber, 1967; White, 1980; Arellano, 1987). See Appendix Table B.14 for the individual point estimates and standard errors. *Sources*: See Figure 2.3 notes.

Table 2.1: The Effects of the Equal Pay Act and Title VII on Wages and Employment using Pre-Existing State Equal Pay Laws

	(1) Women	(2) Men	(3) Women - Men
A. Log weekly wage			
Spline estimate in 1968	0.087	0.054	0.033
	(0.021)	(0.018)	(0.010)
p-value, wild cluster bootstrap	[0.000]	[0.006]	[0.006]
Trend-break in 1964	0.022	0.014	0.008
	(0.005)	(0.004)	(0.003)
Pre-trend slope, 1949-1964	-0.001	-0.002	0.001
	(0.001)	(0.001)	(0.001)
p-value, wild cluster bootstrap	[0.168]	[0.074]	[0.449]
R-squared	0.398	0.331	0.501
Mean log wage in 1960, 2022 dollars	6.16	6.86	–
Mean wage in 1960, 2022 dollars	$595	$1,089	–
B. Log number of employees			
Spline estimate in 1968	0.020	-0.018	0.038
	(0.068)	(0.057)	(0.027)
p-value, wild cluster bootstrap	[0.784]	[0.796]	[0.166]
Trend-break in 1964	0.005	-0.005	0.010
	(0.017)	(0.014)	(0.007)
Pre-trend slope, 1949-1964	0.009	0.009	-0.000
	(0.005)	(0.006)	(0.003)
p-value, wild cluster bootstrap	[0.116]	[0.172]	[0.900]
R-squared	0.982	0.987	0.986
Mean nos cell log number of employees in 1960	11.06	10.97	–
Mean nos cell number of employees in 1960	90,282	103,153	–
C. Log number of annual hours worked			
Spline estimate in 1968	0.026	0.003	0.023
	(0.069)	(0.059)	(0.025)
p-value, wild cluster bootstrap	[0.739]	[0.962]	[0.319]
Trend-break in 1964	0.006	0.001	0.006
	(0.017)	(0.015)	(0.006)
Pre-trend slope, 1949-1964	0.010	0.007	0.003
	(0.006)	(0.006)	(0.003)
p-value, wild cluster bootstrap	[0.178]	[0.347]	[0.307]
R-squared	0.977	0.985	0.983
Mean nos cell log number of annual hours in 1960	18.38	18.59	–
Mean nos cell number of annual hours in 1960	132 M	202 M	–
Observations	800,345	1,561,633	2,361,978
Sex-Industry-Occupation-State-Year Cells	5,264	10,640	15,904

Table presents the spline estimates for model 2 as described in the text. Dependent variables are indicated in panel subtitles. In column 3, demographic controls and fixed effects are allowed to vary by sex. Standard errors in parentheses are corrected for heteroskedasticity and arbitrary correlation within state-group (Huber, 1967; White, 1980; Arellano, 1987). Wild cluster bootstrap p-values using 499 replications are in brackets. *Sources*: See Figure 2.3 notes.

Table 2.2: The Effects of the Equal Pay Act and Title VII on Wages and Employment using 1960 Gender Wage Gaps

	(1)	(2)	(3)	(4)	(5)
				Equal Pay Law	
				State Law	No State Law
	Women	Men	Women - Men	Women - Men	Women - Men
A. Log weekly wage					
Spline estimate in 1968 at mean Gap	0.100	-0.007	0.107	0.060	0.162
	(0.023)	(0.011)	(0.025)	(0.039)	(0.034)
Trend-break in 1964	0.067	-0.004	0.071	0.041	0.103
	(0.015)	(0.008)	(0.017)	(0.027)	(0.022)
Pre-trend slope, 1949-1964	-0.001	-0.000	-0.001	0.007	-0.012
	(0.004)	(0.002)	(0.004)	(0.006)	(0.006)
R-squared	0.399	0.327	0.511	0.476	0.538
Mean log wage in 1960, 2022 dollars	6.17	6.89	–	–	–
Mean wage in 1960, 2022 dollars	$599	$1,114	–	–	–
B. Log number of employees					
Spline estimate in 1968 at mean Gap	-0.118	-0.062	-0.056	-0.009	-0.112
	(0.047)	(0.029)	(0.049)	(0.077)	(0.069)
Trend-break in 1964	-0.079	-0.041	-0.038	-0.006	-0.072
	(0.031)	(0.019)	(0.032)	(0.053)	(0.044)
Pre-trend slope, 1949-1964	-0.005	0.015	-0.020	-0.000	-0.033
	(0.011)	(0.005)	(0.011)	(0.014)	(0.020)
R-squared	0.989	0.991	0.990	0.991	0.989
Mean nos cell log number of employees in 1960	11.06	10.97	–	–	–
Mean nos cell number of employees in 1960	90,345	103,153	–	–	–
C. Log number of annual hours worked					
Spline estimate in 1968 at mean Gap	-0.087	-0.047	-0.039	-0.060	-0.046
	(0.052)	(0.030)	(0.054)	(0.095)	(0.081)
Trend-break in 1964	-0.058	-0.032	-0.026	-0.041	-0.030
	(0.034)	(0.020)	(0.036)	(0.065)	(0.052)
Pre-trend slope, 1949-1964	-0.019	0.008	-0.026	-0.003	-0.047
	(0.013)	(0.005)	(0.012)	(0.016)	(0.021)
R-squared	0.984	0.989	0.987	0.989	0.985
Mean nos cell log annual hours in 1960	18.38	18.59	–	–	–
Mean nos cell number of annual hours in 1960	132 M	202 M	–	–	–
Observations	797,272	1,362,199	2,159,471	1,435,264	724,204
Sex-Industry-Occupation-State-Year Cells	5,264	10,640	15,904	9,904	5,968

Table presents the spline estimates for model 3 of equation (2.5.2). The spline estimates and standard errors in 1968 are scaled by the mean gender gap in the 1960 Census (equal to 0.374). Columns 4 and 5 split the sample into state groups where at least one state had an equal pay law as of January 1, 1963, and state groups that did not (U.S. Congress, 1963). We use separate values of the mean gender gap for these two columns (equal to 0.364 for column 4 and 0.392 for column 5). Standard errors are corrected for heteroskedasticity and an arbitrary correlation within industry-occupation-state-group (Huber, 1967; White, 1980; Arellano, 1987).

CHAPTER III

Firm Provision of Parental Leave in Competitive Equilibrium

3.0 Abstract

The United States guarantees considerably less parental leave than most other OECD countries. The federal government requires covered employers to provide only 12 weeks of job protected unpaid leave through the Family and Medical Leave Act but does not require private sector employers to provide any paid wage replacement. As a result, provision of this benefit, particularly paid leave, is largely based on the decision of individual firms. This paper presents a theoretical reason for why firms might offer parental leave: avoiding potentially large hiring costs by retaining an existing worker. I begin by presenting a standard undirected labor search model on parental leave. I show that this model can be extended to model the firm decision to provide leave and some form of wage replacement. I then construct a novel directed search model of parental leave with worker heterogeneity in their desired length of leave. I solve a full information setting where workers and firms can write contracts conditional on worker type, showing that leave is always provided above some threshold of worker type, given by the cost of hiring a new worker relative to having a worker on leave.

3.1. Introduction

The amount of job protected parental leave (both paid and unpaid) required by federal law in the United States has lagged far behind that in other rich countries (OECD, 2022). Firms have filled this void by voluntarily offering job protected leave to their employees. In 1993, the Family and Medical Leave Act was passed, mandating that firms with 50 or more employees provide 12 weeks of unpaid leave to all qualifying workers. However, even before this law was enacted, the Bureau of Labor Statistics reported that 37% of workers at private sector firms with 100+ employees had access to unpaid maternity leave (Waldfogel, 1999). More recently, in 2022 the BLS reported that 24% of workers at private sector firms had access to paid leave (Bureau of Labor Statistics, 2022), despite no federal policy requiring private employers to provide paid leave. Most research on parental leave has focused on the effectiveness & incidence of policy mandates (see Olivetti and Petrongolo (2017) and Rossin-Slater (2018) for a summary of existing evidence), but new work has endeavored to understand why firms are choosing to offer leave policies in the absence of government intervention.

There are likely many reasons a firm might offer job protected leave to its workers. Goldin et al. (2020) propose a model with firm-specific human capital investments and gender differences in the value of non-market time. Here, firms only provide paid leave if employees value it highly enough and return at sufficiently high rates after having children; since women are assumed to have a higher value of non-market time, this creates a negative relationship between leave provision and the fraction of workers who are women at a firm. In a similar vein, Liu et al. (2023) document a robust negative relationship between the percentage of highly skilled women in an industry and firm maternity benefit ratings, though their explanation is that firms use amenities more highly valued by women to attract and retain talented workers when female talent is scarce. I focus on the "retention" side of this hypothesis, rooted in labor search theory. It is time-consuming and costly for a firm to find and hire a new worker; as a result, if it is less costly to have a worker take leave than to find a new employee, the firm will be more likely to offer leave as a benefit.

This paper is closely related to a growing literature concerned with understanding the theoretical impact of parental leave policy. I begin with the baseline model of parental leave provision with search and matching frictions in Del Rey et al. (2017b). In this set-up, leave duration is set by the government, and considered exogenous from the firm's perspective; the authors derive the impact of mandated leave on wages and employment. I extend this model to allow firms to decide whether or not to provide leave to workers, holding fixed the leave duration, and I find that the firm's decision of what type of job to offer depends on a very simple expression:

$$\frac{pc}{q(\theta)} > \frac{p\psi}{\gamma}$$

The left-hand side gives the cost of opening a new vacancy: c is the flow cost of having a vacancy open and $1/q(\theta)$ is the average duration of a vacancy; the right-hand side gives the cost of having a worker on leave: ψ is the flow cost of having a worker on leave and $1/\gamma$ is the expected duration of leave offered to the worker; and all costs are given in output units p. This result is identical to that derived in Lemma 3.1 of Miyazaki (2023) (despite slight differences in assumptions), a paper concerned with the efficiency implications of mandated parental leave when firms are not offering it.

This paper departs from the simplifying assumption of random search and models parental leave in a directed search setting. I begin by showing that the standard model can be recast as a directed search model, where all solutions remain identical with the elasticity of the matching function $\eta(\theta)$ taking the place of the bargaining parameter β, mirroring the relationship between the standard undirected (Pissarides, 2000) and directed (Moen, 1997) search models. I then present a novel model of directed search where firms have the option to provide either a job with or without job-protected leave upon childbirth and workers are heterogeneous in their preferred duration of leave.

I solve for a competitive equilibrium in a full information setting where firms can observe worker type. I show that there is a unique equilibrium for every worker type, indexed by a submarket wage w, vacancy-unemployment ratio θ and leave duration $1/\gamma$. Further, I find that there is a threshold leave duration $1/\gamma^*$ where firms provide leave to workers with leave type $1/\gamma \leq 1/\gamma^*$ and do not provide leave to workers with leave type $1/\gamma > 1/\gamma^*$. In contrast with Miyazaki (2023), in equilibrium I find that this switching point is given by γ^* solving

$$\frac{pc}{q(\theta)} = \frac{(1-\eta(\theta))(1-\alpha)p\psi}{\gamma^*}$$

The additional terms here reflect the fact that firms are able to pass off some fraction of leave costs to workers in the wage bargaining state. $1-\eta(\theta)$ gives firm bargaining power and $1-\alpha$ defines the extent to which workers value the myopic benefit of leave taking.

Section 2 presents the baseline model in Del Rey et al. (2017b) and extensions. Section 3 builds a model of directed search and presents results. Section 4 concludes. Detailed derivations are kept in the appendix for the motivated reader.

3.2. Baseline Model

To begin, I start with the baseline model of labor search with parental leave in Del Rey et al. (2017b). This extends the standard model to add exogenous fertility and job protected parental leave following childbirth.

3.2.1 Set-Up

All standard assumptions of a continuous time model of labor search are maintained (Pissarides, 2000). There are a continuum of risk neutral workers and firms, each of measure 1, who face an exogenous interest rate r. Firms decide whether or not to post a costly vacancy in order to hire a worker, and workers must engage in a costly search process in order to find a job. A matching function $m(\cdot)$ describes the technology by which workers meet firms. The match rate m is a function of the stock of unemployed workers u searching for jobs and the stock of posted vacancies v, so that $m = m(u, v)$. Since we have normalized the size of the labor force to be 1, u and v also give the unemployment rate and vacancy rate, respectively. The matching function $m(\cdot)$ is assumed to be increasing in both arguments, concave, and homogenous of degree 1. The ratio of the vacancy and unemployment rate, $\theta = v/u$, referred to as market tightness, serves as a useful parameter to describe the match rate in the labor market. Posted vacancies are filled at the rate m/v; utilizing homogeneity of the matching function, the vacancy match rate can be described by the function $q(\theta)$, where $q(\theta) := m/v = m(u, v)/v = m(\theta^{-1}, 1)$. Similarly, unemployed workers find vacancies at the rate $m/u = m(u, v)/u = m(1, \theta) = \theta q(\theta)$. Using the fact that the matching function is increasing in both arguments, we can show that $q'(\theta) < 0$ and $\mathrm{d}\theta q(\theta)/\mathrm{d}\theta > 0$.

3.2.2 Firms

Let V_l denote the value to the firm of posting a vacancy for a job with leave, let J_l denote the value of this job once it is filled, and let X_l denote the value of having a worker on leave.[1] Labor productivity is constant across workers and given by p in output units; while employed, the firm pays the workers wage w_l in output units. While a vacancy is open, the firm incurs a flow cost that is proportional to worker productivity, given by pc. The firm receives a worker match at rate $q(\theta_l)$. The value of a vacancy is given by

$$rV_l = -pc + q(\theta_l)(J_l - V_l) \tag{3.2.1}$$

Once created, jobs can be destroyed for two reasons.[2] First, I assume that jobs are exogenously destroyed at Poisson rate λ due to product demand shocks. Additionally, workers have children at Poisson rate σ, at which point the employer must hold this position open and cannot post a new vacancy. Let X_l denote the value of having a worker on leave. Accordingly, the value of an employed worker is given by

$$rJ_l = p - w_l + \lambda(V_l - J_l) + \sigma(X_l - J_l) \tag{3.2.2}$$

With a worker on leave, firms incurs a flow cost proportional to worker productivity given by $p\psi$, and the worker returns to the job at rate γ. Thus, we can write the value of having a

[1] The subscript is superfluous at this stage but will be important for differentiating jobs later.
[2] I assume that the likelihood of these events occurring at the same time is negligible.

worker on leave as

$$rX_l = -p\psi + \gamma(J_l - X_l) \tag{3.2.3}$$

To round out the firm side, I make the standard assumption that firms will enter the market until no profit can be made from opening a new vacancy. Formally,

$$V_l = 0 \tag{Assumption 1: Free Entry}$$

Utilizing (3.2.1), we can substitute $V_l = 0$ to obtain that $J_l = pc/q(\theta_l)$. As in a standard search model, the marginal benefit of opening a job must be equal to the marginal cost of posting a vacancy. Since, $q(\theta_l)$ is the Poisson rate at which the vacancy transitions to a filled job, $1/q(\theta_l)$ gives the expected duration of a vacancy. Accordingly, the expected cost of a vacancy is given by $pc/q(\theta_l)$.

To derive the Job Creation condition for a job with leave, we can first solve the system (3.2.2) & (3.2.3) to obtain an expression for the marginal benefit of a filled job with leave. Substituting this solution into the optimality condition gives:

$$\frac{(r+\gamma)(p-w_l) - \sigma p\psi}{r(r+\lambda+\sigma) + \gamma(r+\lambda)} = \frac{pc}{q(\theta_l)} \tag{3.2.4}$$

The left hand side of this expression gives the expected payoff from a filled position, which must be equal to the expected cost of opening a vacancy under a no profit assumption.

3.2.3 When is Leave Offered?

At this point, we can conduct a simple partial equilibrium exercise to understand when firms are willing to offer leave to their employees. To facilitate this analysis, I develop a simple extension of the standard model of job search in Pissarides (2000) in the appendix. In this setup, there is exogenous fertility but not job protected leave, so both job destruction and childbirth move the firm from a filled job to a vacancy.

The job creation condition describes a line in (w, θ) space that gives a zero-profit condition for opening a new vacancy. I show in the appendix that this optimality condition for the job without leave is given by

$$p - \left(w_{nl} + (r+\lambda+\sigma)\frac{pc}{q(\theta_{nl})} \right) = 0 \tag{3.2.5}$$

This tells us that, with the free entry of firms, the marginal benefit of a job (given by output p) must be equal to the marginal cost, given by the sum of the wage w_{nl} and the capitalized hiring cost $(r+\lambda+\sigma)(pc)/q(\theta)$.

With job protected leave, an additional term changes the job creation condition. We can rearrange (3.2.4) to obtain:

$$p - w_l - (r + \lambda + \sigma)\frac{pc}{q(\theta_l)} + \frac{\sigma\gamma}{r + \gamma}\left\{\frac{pc}{q(\theta_l)} - \frac{p\psi}{\gamma}\right\} = 0 \tag{3.2.6}$$

As the subscriptions make clear, comparisons between these expressions are hindered by the fact that, in equilibrium, different wage rates and market tightness will occur for different types of jobs. For this partial equilirbium exercise, we set $w_l = w_{nl} = w$ and $\theta_l = \theta_{nl} = \theta$ to consider when a firm might want to deviate to a different type, holding wages and market tightness fixed. Comparison of these expressions shows that the marginal benefit of a job with leave lies above that of a job without leave if and only if

$$\frac{c}{q(\theta)} > \frac{\psi}{\gamma}$$

This expression has a simple interpretation. We know that the term on the left hand side is the expected cost of opening a new vacancy. The term on the right side is the expected cost of having a worker on leave: we are multiplying the flow cost of having a worker on leave, ψ, by the expected duration of leave, $1/\gamma$. Consequently, the firm will prefer to offer leave if it is costlier to hire a new worker than to have an existing worker go on leave.

At this point, there is not anything we can say on the intensive margin - that is, conditional on offering leave, how long should firms offer leave for? This is because the model assumes that workers will always return to their previous job if leave is offered. In this circumstance, it is always optimal to provide the least possible leave and let $1/\gamma \to 0$, as this minimizes the cost of leave and maximizes the capitalized value of hiring savings. We can see this in two steps. First, it is clear that γ will never fall below the point at which $\frac{c}{q(\theta)} = \frac{\psi}{\gamma}$, as this would imply losses from providing leave. Above this threshold, the job creation condition is always increasing in γ, so the firm will choose $\gamma = \infty$.[3]

We can break this mechanical link by introducing a reasonable trade-off for the firm. Suppose instead that there is some function $\pi(\gamma) \in [0, 1]$ that gives the probability that a worker provided with leave γ returns to work after their leave has concluded. Assume that $\pi'(\gamma) < 0$ and that $\pi(\gamma) \to 0$ as $\gamma \to \infty$. Then, we can rewrite the value of a worker on leave as

$$rX_l = -p\psi + \gamma[\pi(\gamma)(J_l - X_l) + (1 - \pi(\gamma))(V_l - X_l)]$$

[3] Again, this is a partial equilibrium result in that we do not consider how θ and w vary with γ.

This leads to the expected change in the job creation equation:

$$p - w_l - (r + \lambda + \sigma)\frac{pc}{q(\theta_l)} + \frac{\sigma\gamma}{r+\gamma}\left\{\pi(\gamma)\frac{pc}{q(\theta_l)} - \frac{p\psi}{\gamma}\right\} = 0$$

Where a less generous leave policy (higher γ) now lowers the probability of realizing hiring savings in addition to lowering the cost of having a worker on leave. I return to the firm decision of when to provide leave in the next section, but now we turn to workers to round out the baseline model.

3.2.4 Workers

Let U_l denote the asset value of unemployment to the worker, and let E_l denote the asset value of employment. I assume that the worker receives flow utility b while unemployed. Additionally, recall that workers find jobs at the Poisson rate $\theta_l q(\theta_l)$. Using all of this, we can write the value of unemployment as

$$rU_l = b + \theta_l q(\theta_l)(E_l - U_l) \tag{3.2.7}$$

After matching to a job, the worker receives a flow wage benefit w_l. The job match can be destroyed for two reasons: a product demand shock to the firm (λ) or a fertility shock to the worker (σ). Letting L_l denote the value of currently being on leave, we can write the value of employment as

$$rE_l = w_l + \lambda(U_l - E_l) + \sigma(L_l - E_l) \tag{3.2.8}$$

While on leave, I assume that the worker receives some flow benefit z. At this juncture, we can think of $z - b$ loosely as the additional myopic value of job protection to a worker, though I will discuss this parameter in detail later. While on leave, the worker receives an opportunity to return to work at Poisson rate γ, and I assume that the worker always exercises this option. The value of taking leave is given by

$$rL_l = z + \gamma(E_l - L_l) \tag{3.2.9}$$

At this point, we can solve for the prevailing wage, using a standard Nash bargaining approach. Letting worker bargaining power be given by $\beta \in (0,1)$, I assume that w_l is given by the solution to the problem

$$\arg\max_{w_l} \quad (E_l^i - U_l)^{\beta}(J_l^j - V_l)^{1-\beta}$$

Where the superscript i denotes a value function specific to the worker (i) and firm (j) engaging in bargaining. In the appendix, I show that the solution to this problem is given by[4]

$$w_l = (1-\beta)b + \beta p + \beta pc\theta_l + \frac{\sigma}{r+\gamma}\left\{\beta pc\theta_l - [(1-\beta)(z-b) + \beta p\psi]\right\} \tag{3.2.10}$$

The first three terms are identical to the wage in the standard model of labor search (see Pissarides (2000) equation 1.20). Adding in parental leave introduces a fourth term, which is a dynamic version of what a simple static theory of mandated benefits would predict (Summers, 1989).

To understand this, consider the term $(1-\beta)(z-b) + \beta p\psi$. $z-b$ is the flow benefit to workers of being on leave (relative to unemployment), and $p\psi$ is the flow cost to firms of having a worker on leave. Suppose that workers to not value leave at all ($z-b=0$): in this case, leave functions fully as a tax on employers of $p\psi$, of which the fraction β is passed along to workers through lower wages. At the other extreme, suppose that workers value leave at exactly the value of its cost ($z-b=p\psi$): in this case, the full cost of the leave cost $p\psi$ will be passed to workers through a lower wage. These two predictions match the cases outlined in Summers (1989). There is, however, an additional dynamic term here: $pc\theta_l$ reflects changes to hiring costs for firms due to the presence of workers on leave, of which workers are able to capture β. I discuss plausible parameter values for α in the next section, but for now we briefly consider paid leave in the framework before moving on.

3.2.5 Adding Paid Leave

At this juncture, we can think about the role of paid leave in this model. For simplicity, assume that the only cost the firm incurs during leave is a direct transfer to workers of the amount τ, so that $p\psi = \tau$. Further, let the only myopic benefit to workers on leave be given by this same term so that $z-b=\tau$. In this case, the wage paid changes to

$$w_l = (1-\beta)b + \beta p + \beta pc\theta_l + \frac{\sigma}{r+\gamma}\left\{\beta pc\theta_l - \tau\right\}$$

Paid leave in this case functions solely as a intertemporal transfer for the worker. While working, the worker receives a wage lowered by the expected amount they would receive if on leave. This is given by the expected value of the leave payment, $\tau/(r+\gamma)$, multiplied by the probability of going on leave σ. Since $w > b$ by the participation constraint, this functions like an insurance product for a fertility shock, a mechanism with empirical support (Rodgers,

[4]My solution is different that that given in Del Rey et al. (2017b). The appendix also provides a justification for this difference.

2020).

3.3. Directed Search

I depart from the baseline model by moving to a setting with directed search. There have been several models featuring both search frictions and parental leave, far more intricate than what I am presenting here, including Erosa et al. (2010) and Xiao (2021), but this is the first paper to consider parental leave provision where workers and firms are not randomly matched to one another.

3.3.1 Extending the Baseline Model

The setting remains similar, except I now allow workers to search in specific submarkets, denoted by a wage-tightness pair (w_l, θ_l). To begin, I show that a directed version of the model presented in Del Rey et al. (2017b) yields similar predictions to their undirected model, analagous to standard models of labor search (Pissarides, 2000; Moen, 1997).

The value functions for firms remain the same as before, with the only difference being that they now must hold in every submarket (w_l, θ_l). As before, I assume there is free entry which requires that $V_l = 0$, and we can write the value of leave X_l as a function of J_l. We can substitute these into (3.2.2) to obtain an expression for the wage:

$$w_l = p - (r + \lambda + \sigma)\frac{pc}{q(\theta_l)} + \frac{\sigma\gamma}{r+\gamma}\frac{pc}{q(\theta_l)} - \frac{\sigma p \psi}{r+\gamma} \tag{3.3.1}$$

This is simply another more convenient way of writing the job creation condition, which ensures that firms are optimizing in submarket (w_l, θ_l).

The value functions for workers also remain the same but now must hold in every submarket. In the appendix, I show that this system of equations, (3.2.7), (3.2.8), and (3.2.9), can be used to solve for $E_l - U_l$:

$$E_l - U_l = \frac{(r+\sigma+\gamma)(w_l - b) - \sigma(w_l - z)}{(r+\sigma+\gamma)(r+\lambda+\theta q(\theta_l)) - \sigma\lambda}$$

Which we can substitute into the value function for an unemployed worker:

$$rU_l = b + \theta q(\theta_l)\left[\frac{(r+\sigma+\gamma)(w_l - b) - \sigma(w_l - z)}{(r+\sigma+\gamma)(r+\lambda+\theta q(\theta_l)) - \sigma\lambda}\right] \tag{3.3.2}$$

Equation (3.3.2) gives the return to a worker of searching in submarket (w_l, θ_l). Ignoring b, which does not depend on the submarket, the expected return of searching in a specific submarket is given by the product of the probability of matching to a job, $\theta_l q(\theta_l)$, multiplied by the expected return conditional on job match. Utilizing a market utility approach, we can then solve for the equilibrium submarket (w_l, θ_l) by maximizing worker utility subject to the constraint that the employer wage equation (3.3.1) holds. As it does not matter for the solution, I let the workers post (Wright et al., 2021). Formally, I solve the problem

$$\begin{aligned} \max_{w_l,\theta_l} \quad & b + \theta_l q(\theta_l)\left[\frac{(r+\sigma+\gamma)(w_l - b) - \sigma(w_l - z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda}\right] \\ \text{s.t.} \quad & w_l = p - (r+\lambda+\sigma)\frac{pc}{q(\theta_l)} + \frac{\sigma\gamma}{r+\gamma}\frac{pc}{q(\theta_l)} - \frac{\sigma p\psi}{r+\gamma} \end{aligned}$$

In the appendix, I show that the first order condition of this problem is identical to that of the bargaining solution in the baseline model, where the bargaining weight β has been replaced by the elasticity of the matching function $\eta(\theta) = -q'(\theta)\theta/q(\theta)$. It follows that the wage here is given by

$$w_l = (1-\eta(\theta_l))b + \eta(\theta_l)p + \eta(\theta_l)pc\theta_l + \frac{\sigma}{r+\gamma}\left\{\eta(\theta_l)pc\theta_l - [(1-\eta(\theta_l))(z-b) + \eta(\theta_l)p\psi]\right\} \tag{3.3.3}$$

Now that we have solved for a competitive equilibrium in the baseline model, I introduce a new type of job where firms do not offer leave to their employees.

3.3.2 Job Without Leave

The model in the previous section assumed that leave of length $1/\gamma$ must be provided to the worker. In this section, I interpret this parameter instead as the length of leave that a worker will take upon childbirth. Upon viewing this parameter, the employer will decide whether or not to provide leave and maintain this worker or to not provide leave and post a vacancy instead. To begin, I consider a separate economy where the firm does not offer leave and solve for the firm's zero profit condition and equilibrium wages. Following this, I will allow firms to choose between either job, and using what we know about equilibria in both economies, we can solve for the equilibrium submarket as a function of worker type with firm choice of leave provision.

3.3.2.1 Firms

Let V_{nl} denote the value to the firm of posting a vacancy for a job with no leave, and let J_{nl} denote the value of this job once it is filled. There is no corresponding function X_{nl} as the firm does not offer leave in this type of job.

All other assumptions in the previous section are maintained. As a result, the value of posting a vacancy will have the same expression:

$$rV_{nl} = -pc + q(\theta)(J_{nl} - V_{nl}) \tag{3.3.4}$$

However, now childbirth moves a firm from having a filled job to posting a vacancy, instead of having a worker on leave. Accordingly, the value of a filled job becomes:

$$rJ_{nl} = p - w_{nl} + (\lambda + \sigma)(V_{nl} - J_{nl}) \tag{3.3.5}$$

For use later, I solve for the new job creation condition here. Using the free entry assumption that $V_{nl} = 0$, we know from (3.3.4) that $J_{nl} = pc/q(\theta)$. Substituting this into (3.3.5) yields the following:

$$\frac{p - w_{nl}}{r + \lambda + \sigma} = \frac{pc}{q(\theta)} \tag{3.3.6}$$

This is identical to the job creation condition in a standard model of labor search (Pissarides, 2000), except the rate of job destruction is now given by $\lambda + \sigma$.

3.3.2.2 Workers

The value functions for workers remain similar, though I change the return that a worker receives while out of the labor force following childbirth, which I discuss below.

All other assumptions in the previous section are maintained. As a result, the value of unemployment will have the same expression:

$$rU_{nl} = b + \theta_{nl} q(\theta_{nl})(E_{nl} - U_{nl}) \tag{3.3.7}$$

After childbirth, I still maintain that workers move to a leave state L_{nl}, but this will of course be different than a job-protected leave state L_l. Given this, the expression for the value of employment will also remain the same:

$$rE_{nl} = w_{nl} + \lambda(U_{nl} - E_{nl}) + \sigma(L_{nl} - E_{nl}) \tag{3.3.8}$$

Upon childbirth, workers still take leave from the workforce (similar to Miyazaki (2023)), but following this they enter unemployment instead of re-entering their previous job. Thus, the value of leave is given by

$$rL_{nl} = b + \gamma(U_{nl} - L_{nl}) \tag{3.3.9}$$

The modelling decision of what myopic worker utility while on leave should be is not an innocuous one, even though the objects of interest in this model are firms, not workers. I show in the appendix, under different set-ups, how this decision affects equilibrium wages, and in turn what portion of the surplus firms are able to recover.

A natural choice is to assume that a worker on leave receives the same flow return z whether or not they are returning to a job or moving to unemployment. In this case, wages will be given by:

$$w_{nl} = (1-\eta(\theta_{nl}))b + \eta(\theta_{nl})p + \eta(\theta_{nl})pc\theta_{nl} + \frac{\sigma}{r+\gamma}\left\{\eta(\theta_{nl})pc\theta_{nl} - (1-\eta(\theta_{nl}))(z-b)\right\} \quad (3.3.10)$$

This expression is strange in that workers are paying some fraction $(1-\eta(\theta_{nl}))$ of their benefit from leave $z-b$ back to the firm, despite the fact that no amenity is being offered. This occurs because of the simplifying assumption that workers cannot have children while unemployed, so the amenity value of being able to have children is priced into a job where leave is not offered. Suppose instead that workers move directly to unemployment following childbirth. In this case, the model is identical to a standard directed search model with the probability of job destruction $\lambda + \sigma$, and wages will be given by the same expression:

$$w_{nl} = (1-\eta(\theta_{nl}))b + \eta(\theta_{nl})p + \eta(\theta_{nl})pc\theta_{nl} \quad (3.3.11)$$

While avoids the problem outlined previously, it leads to the paradoxical result that workers might prefer a job without leave to one with leave if they are able to re-enter employment faster, which occurs if the job match rate $\theta_{nl}q(\theta_{nl})$ is sufficiently higher than γ. To avoid this, I set the flow return of leave to be identical to the flow return of unemployment, and wages are given by

$$w_{nl} = (1-\eta(\theta_{nl}))b + \eta(\theta_{nl})p + \eta(\theta_{nl})pc\theta_{nl} + \frac{\sigma}{r+\gamma}\eta(\theta_{nl})pc\theta_{nl} \quad (3.3.12)$$

Where the change in hiring cost due to the introduction of leave is priced in appropriately.

3.3.2.3 Submarket Equilibrium

Now, we solve for the equilibrium (w_{nl}, θ_{nl}) pair in a submarket with no leave. Using the free entry assumption that $V_{nl} = 0$, we know from (3.3.4) that $J_{nl} = pc/q(\theta)$. Substituting this into equation (3.3.5), we can then solve for wages:

$$w_{nl} = p - (r + \lambda + \sigma)\frac{pc}{q(\theta_{nl})} \quad (3.3.13)$$

Taking first differences of (3.3.7), (3.3.8) and (3.3.9) produces a system of equations that we can solve for $E_{nl} - U_{nl}$:

$$E_{nl} - U_{nl} = \frac{(r+\gamma)(w_{nl} - b)}{(r+\gamma)(r+\lambda+\sigma) + (r+\sigma)\theta_{nl}q(\theta_{nl}) + \sigma\gamma\theta_{nl}q(\theta_{nl})} \tag{3.3.14}$$

Substituting this into (3.3.7), we can write down the market utility problem as before:

$$\begin{aligned} \max_{w_{nl},\theta_{nl}} \quad & b + \theta_{nl}q(\theta_{nl})\frac{(r+\gamma)(w_{nl} - b)}{(r+\gamma)(r+\lambda+\sigma) + (r+\sigma)\theta_{nl}q(\theta_{nl}) + \sigma\gamma\theta_{nl}q(\theta_{nl})} \\ \text{s.t.} \quad & w_{nl} = p - (r+\lambda+\sigma)\frac{pc}{q(\theta_{nl})} \end{aligned}$$

Utilizing the relationship between the undirected and directed solutions, I show in the appendix that equilibrium wages are given by (3.3.12).

3.3.3 Equilibrium With Symmetric Information

With an undertstanding of equilibria for an economy with each type of job, I now move towards a more general setting. Firms are homogenous and able to offer either type of job in any submarket. Workers are heterogeneous, differing in the expected duration of leave they will take following childbirth, $1/\gamma$. I assume some distribution of workers over the type space given by $F(\gamma)$ with support over $(0, \infty)$. Worker type is observed and can be contracted over, so we are searching for an equilibrium submarket (w, θ, γ) indexed by worker type. Further, for this equilibrium to be stable, it must be the case that firms are both indifferent across submarket and do not want to deviate to a different type of job within submarket.

I characterize equilibrium in this model in two steps. First, having solved for wages as a function of market tightness θ and worker type γ, I solve for equilibrium tightness as a function of worker type for an economy with only jobs with leave $\theta_l(\gamma)$ and an economy with only jobs without leave $\theta_{nl}(\gamma)$. Then, given a submarket defined by worker type γ, we have two potential equilibria: $(\gamma, \theta_l(\gamma), w(\theta_l(\gamma), \gamma))$ and $(\gamma, \theta_{nl}(\gamma), w(\theta_{nl}(\gamma), \gamma))$. I then show that only one equilibrium is stable and does not involve a profitable deviation to a different type of job.

To make this problem tractable, I first need to impose additional assumptions. First, I assume that the elasticity of the matching function is constant as a function of θ:

$$\eta(\theta) = \eta \qquad \text{(Assumption 2a: Constant Elasticity)}$$

This assumption is consistent with many common functional forms (such as Cobb-Douglas) that match the data well (Petrongolo and Pissarides, 2001). Since $\eta(\theta)$ is the sharing parameter in a competitive equilibrium, this facilitates comparisons across equilibria with different market tightness. Next, I add that

$$(r+\gamma)[(1-\eta(\theta))(p-b)-\eta(\theta)pc\theta]-\sigma\eta(\theta)pc\theta-\sigma(1-\eta(\theta))(1-\alpha)p\psi>0 \tag{Assumption 2b: Gains to Trade}$$

This ensures that there is a positive benefit to opening both types of jobs and rules out extreme parameter values like $\eta(\theta)=1$. Finally, without loss of generality, we can write the worker mypoic returns to leave $(z-b)$ as $\alpha p\psi$. Then, losing some generality, I restrict α so that

$$\alpha \in (0,1) \tag{Assumption 2c: Costly Leave}$$

This assumption maintains that the myopic benefit to the worker from leave is lower than the cost to the firm. This is important for establishing a single crossing property but is not essential for the existence of equilibrium.[5]

3.3.3.1 Solving for Market Tightness

So far, for both the leave and no leave economies, I have solved for the firm profit maximization condition and the wage equation. Substituting the latter into the former will yield an expression that gives the solution for market tightness as a function of worker type:

$$\frac{(r+\gamma)[(1-\eta(\theta_{nl}))(p-b)-\eta(\theta_{nl})pc\theta_{nl}]}{(r+\gamma)(r+\lambda+\sigma)}-\frac{\sigma\eta(\theta_{nl})pc\theta_{nl}}{(r+\gamma)(r+\lambda+\sigma)}=\frac{pc}{q(\theta_{nl})} \tag{3.3.15}$$

$$\begin{aligned}\frac{(r+\gamma)[(1-\eta(\theta_l))(p-b)-\eta(\theta_l)pc\theta_l]}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}-\frac{\sigma\eta(\theta_l)pc\theta_l}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}-\\ \frac{\sigma(1-\eta(\theta_l))(1-\alpha)p\psi}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}=\frac{pc}{q(\theta_l)}\end{aligned} \tag{3.3.16}$$

Let $\theta_l(\gamma)$ denote equilibrium in the economy with leave and $\theta_{nl}(\gamma)$ denote equilibrium in the economy without leave. In the appendix, I show that the following properties of these solution functions can be shown:

1. $\lim_{\gamma\to\infty}\theta_l(\gamma)>\lim_{\gamma\to\infty}\theta_{nl}(\gamma)$
2. $\theta_l'(\gamma),\theta_{nl}'(\gamma)>0$
3. $\lim_{\gamma\to 0}\theta_l(\gamma)<\lim_{\gamma\to 0}\theta_l(\gamma)$

[5] If $\alpha>1$, the firm receives a net *return* on offering leave, as its losses during leave are recouped by lower wage payments. This is an interesting counterfactual to consider but introduces a complex set of equilibria.

Taken together, I show in the appendix that these results imply a single crossing property of the solution functions, as illustrated in Figure 3.1a. This is key for determining which job will be provided for a worker of type γ.

The single crossing property implies that there will be a unique γ^* such that $\theta_l(\gamma^*) = \theta_{nl}(\gamma^*)$. Consider some γ such that $1/\gamma > 1/\gamma^*$. Let $\mathrm{MB}_t(\theta_t(\gamma), \gamma)$ denote the LHS of the job creation condition and $\mathrm{MC}_t(\theta_t(\gamma))$ denote the RHS, where $t \in \{l, nl\}$. Free entry ensures that these are equal. Consider a potential equilibrium where workers of type γ are offered leave. It is easy to show that $\partial \mathrm{MB}_t(\theta_t(\gamma), \gamma)/\partial\theta_t < 0$ and $\partial \mathrm{MC}_t(\theta_t(\gamma))/\partial\theta_t > 0$; intuitively, a higher vacancy-unemployment ratio increases the average duration of a vacancy and lowers returns conditional on a match as workers capture some of these higher hiring costs. By inspection of Figure 3.1a, we see that it must be the case that $\theta_{nl}(\gamma) > \theta_l(\gamma)$. It follows immediately that $\mathrm{MB}_{nl}(\theta_l(\gamma), \gamma) > \mathrm{MC}_{nl}(\theta_l(\gamma))$, providing firms with a profitable deviation to a job with no leave. We also know that $\mathrm{MB}_l(\theta_{nl}(\gamma), \gamma) < \mathrm{MC}_l(\theta_{nl}(\gamma))$, so no such deviation exists from the no-leave equilibrium.

Symmetric arguments establish that for γ such that $1/\gamma < 1/\gamma^*$, only the equilibrium with leave is not subject to a profitable deviation to the other type of job. Putting this together, we see that equilibrium tightness as a function of worker type can be given by the upper envelope of Figure 3.1a, as shown in Figure 3.1b. For the submarket with worker type γ^*, firms will be indifferent between providing both types of jobs, and this will also be the threshold at which the job offered switched from no leave to leave. By equating the job creation conditions, we can solve for this value:

$$\gamma^* = \left\{(1-\eta(\theta))(1-\alpha)p\psi\right\}\left\{\frac{pc}{q(\theta}\right\}^{-1}$$

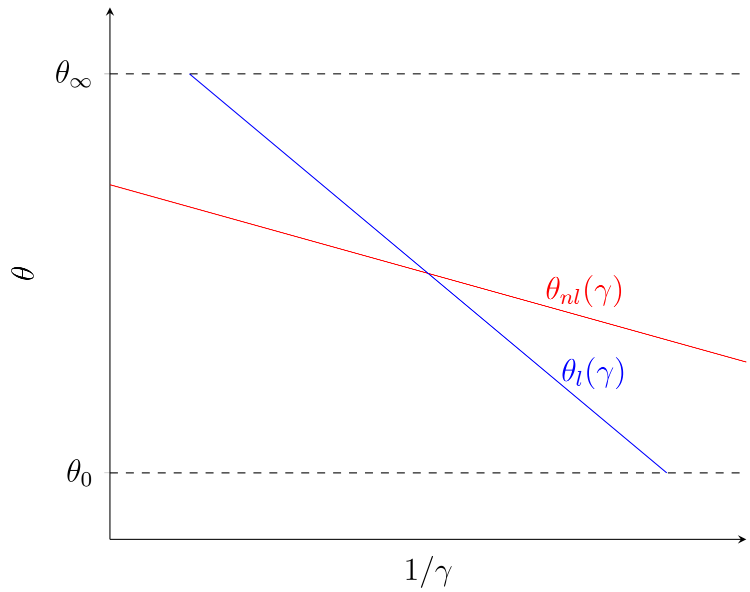
Importantly, by rearranging this equation, we find that γ^* solves

$$\frac{pc}{q(\theta)} = \frac{(1-\eta(\theta))(1-\alpha)p\psi}{\gamma^*} \tag{3.3.17}$$

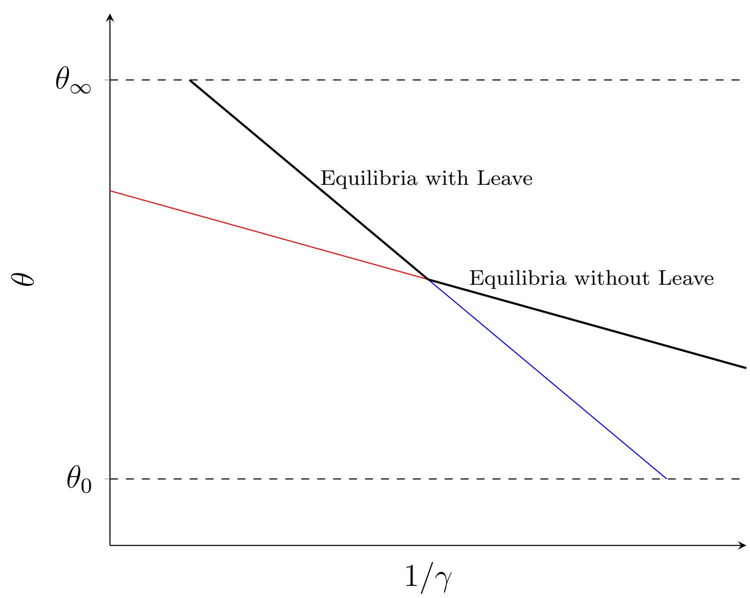
Firms provide leave up until the point where the expected cost of leave, given by the fraction of leave cost paid by the firm multiplied by the expected duration of leave, equals the expected cost of a vacancy. In equilibrium, the jobs in which the firm is willing to provide leave depends on the degree to which the worker values leave, given by $\alpha \in (0, 1)$. If we think there are gender differentials in the valuation of parental leave, this could help to explain differences in firm offering of leave to men and women.

Figure 3.1: Graphical Depiction of Equilibria

(a) Single Crossing of $\theta_l(\gamma)$,$\theta_{nl}(\gamma)$



(b) Upper Envelope



3.4. Conclusion

In this paper, I demonstrate a strong theoretical rationale for firm provision of parental leave: to save on hiring costs by avoiding search frictions. This focus on retention ignores the attraction of workers, which is likely also an important channel. Not only might firms offer certain benefits to attract particular workers (Liu et al., 2023), but adverse selection might prevent them from offering leave even in the presence of potential gains (Bastani et al., 2019). Future work should explore these mechanisms in the context of search frictions.

APPENDICES

APPENDIX A

Appendix to Chapter 1

A.1. Data

A.1.1 Total Enrollment Data

To construct time series evidence on changes in women's enrollment over time, I collect institution-level information on total enrollment, split by sex. In every year, the *Journal of the American Medical Association* publishes its Education Number, which includes reports and statistics on medical education. Between 1960 and 1972, the Education Number includes information on the number of current students and graduates from each medical school, reported separately by sex. Starting in 1973, students are split into three categories: first-year students, intermediate students, and graduates. Intermediate students include students in years 2-3 at 4-year programs, students in year 2 at 3-year programs, as well as students in year 2 at 2-year basic science schools.

To construct a comparable time series throughout my sample period, I utilize data on the number of students in each year from 1960-1972. From comparing total enrollment figures to sums of the variables provided here, it appears each year's graduates are included in the count of total students. From 1973-1980, I construct information on total enrollment by sex by adding first-year, intermediate, and graduate enrollment.

There are two known issues with these data. First, enrollment of full-time students is reported from 1960-1962, while data on all students is reported from 1963 - 1980. Since most medical students are full-time, I am able to measure almost all enrollment in every year; further, since the data are consistent starting in 1973, I am able to capture important trend breaks around 1970 without worrying about this change in reporting. Second, I am missing information on one institution in 1973.

A.1.2 First-Year Enrollment Data

Table A.1 summarizes the source of all variables that I collect to construct my dataset. I collect data from three sources:

JME *Journal of Medical Education*

MSAR *Medical School Admission Requirements*

JAMA EN *Journal of the American Medical Association* Education Number

The *Journal of Medical Education* published its "Study of Applicants" in every year from 1960 through 1977. In every year, I collect information on total new entrants, male applicants and female applicants for each institution. Unfortunately, information on new entrants split by sex is only available starting in 1967. To supplement this, I collect information on first-year enrollments in 1966 as well as 1978-1980. First-year enrollments differ slightly from new entrants, as this count includes students repeating the first year, but it is generally very close to the number of new entrants. From 1978-1980, I collect this data from the *JAMA* Education Number in each year that it is reported. Information on the 1966-67 entering class is published in the 1968-69 *MSAR*, but unfortunately earlier copies of the *MSAR* do not publish this data series.

Accordingly, to extend my panel back to 1960, I utilize estimated enrollment data. This is published in the *MSAR* and then reprinted in the *JAMA* Education Number during my years of interest, which is where I collect it. Medical schools are surveyed in the spring before a class enters in the next fall for an estimate of the gender composition of their incoming students. Generally, this is a highly accurate estimate, as many applicants have committed to enroll in the following year by spring. Interestingly, starting with the 1971-72 *MSAR*, medical schools begin estimating the in-state/out-of-state composition of their incoming class instead of the sex composition.

A.1.3 Accuracy of Estimated Enrollment

Fortunately, there are several years where I observe both estimated new entrants and actual new entrants, which allows me to evaluate the ability to which medical schools are able to accurately estimate the sex distribution of their incoming class. I utilize the following set of variables:

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

Table A.1: Data Sources and Availability

Year	First-Year Enrollment	New Entrants (Total)	New Entrants (By Sex)	Applications	Estimated Enrollment
1960		*JME*		*JME*	*JAMA* EN
1961		*JME*		*JME*	*JAMA* EN
1962		*JME*		*JME*	*JAMA* EN
1963		*JME*		*JME*	*JAMA* EN
1964		*JME*		*JME*	*JAMA* EN
1965		*JME*		*JME*	*JAMA* EN
1966	*MSAR*	*JME*		*JME*	
1967		*JME*	*JME*	*JME*	
1968		*JME*	*JME*	*JME*	
1969		*JME*	*JME*	*JME*	
1970		*JME*	*JME*	*JME*	
1971		*JME*	*JME*	*JME*	
1972		*JME*	*JME*	*JME*	
1973		*JME*	*JME*	*JME*	
1974		*JME*	*JME*	*JME*	
1975		*JME*	*JME*	*JME*	
1976		*JME*	*JME*	*JME*	
1977		*JME*	*JME*	*JME*	
1978	*JAMA* EN				
1979	*JAMA* EN				
1980	*JAMA* EN				

This table presents the data sources for my institution-by-year panel dataset on first-year enrollment. Each column gives a different data series, and each row gives a year. If a particular data series is utilized in my dataset in a particular year, in that cell I include the abbreviation for the data source it is collected from, where all abbreviations are listed at the top of section A.1.2.

- M_{it}^{EST}: Estimated new entrants for institution i in year t that are men

To evaluate to predictive value of F_{it}^{EST} and M_{it}^{EST}, I run the following bivariate regressions:

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

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

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

Table A.2: Accuracy of Estimated Enrollment

	(1)	(2)
New Entrants (Men)	1.011***	
	(0.006)	
New Entrants (Women)		1.027***
		(0.015)
Observations	485	485
R^2	0.991	0.944

Column 1 gives estimates of β from equation (A.1) (for women), and column 2 gives estimates of β from equation (A.2) (for men). Standard errors are clustered at the institution level to correct for institution-specific errors in reporting.

Table A.2 reports the results from (A.1) and (A.2). The primary statistic of interest is R^2: I am able to explain 94% of variation in actual enrollment for women and 99% of variation in actual enrollment for men, suggesting that estimated enrollment functions as an excellent proxy for true enrollment.

A.2. Treatment Effect Dynamics

Specification (1.4.3) implicitly imposes a constraint that increases in women's enrollment must occur in the period in which enrollment changes. We might expect, however, that it takes several years for enrollment expansions to translate into gains for women. To illustrate this, I utilize a large expansion in enrollment capacity at the University of Cincinnati as a case study to show that the gains can take several years to materialize.

A.2.0.1 Case Study: University of Cincinnati

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

To understand the potential dynamics of women's entry, I consider a case study of a grant given to the University of Cincinnati. This medical school received a grant in Fiscal Year 1970 for $32m to construct a basic science building. In exchange, the university would maintain 106 existing seats and add 86 new seats. The university's website reports that this building was completed in 1974,[1] and the time series for enrollment verifies this. Figure A.1 plots first-year enrollment for the University of Cincinnati during my sample period, and there is a clear discrete jump in enrollment when the new Medical Sciences Building opens in 1974 of around 60 students.

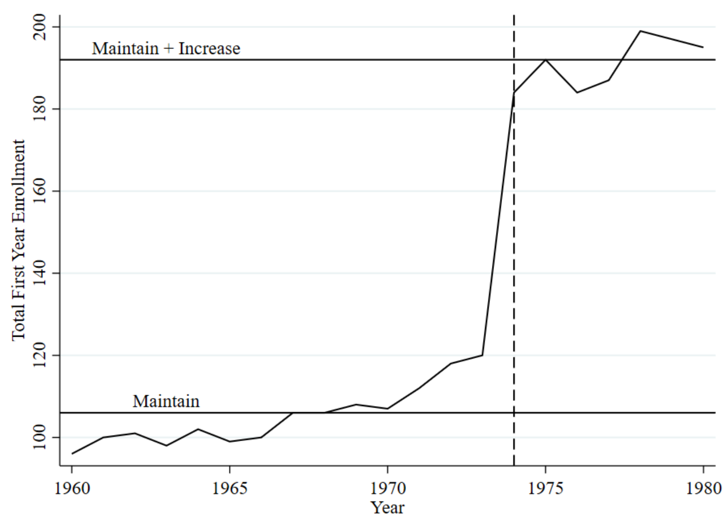
It is less clear that women benefit from this enrollment expansion; women's enrollment at the University of Cincinnati is plotted in Figure A.2. Women's enrollment is increasing over this entire time period, but it is unclear to what extent this increase is due to a specific increase in teaching capital or part of a previous rise in women's enrollment. To disentangle the impact of this expansion on women's enrollment, I construct a synthetic University of Cincinnati in the years leading up to this expansion in order to directly estimate the counterfactual where the university does not expand (Abadie et al., 2010).

I utilize a donor pool of all medical schools that did not receive a construction grant after 1969, which includes 45 institutions after dropping Women's Medical.[2] To construct a synthetic control, we search for a weighted average of schools in the donor pool that minimize the distance to the treated unit for a collection of pre-intervention covariates, which are left to researcher discretion. I utilize women's enrollment and total enrollment from 1966 through 1970; this prevents potential over-fitting from matching on the entire pre-intervention period and ensures that my estimates are not sensitive to measurement error in estimated enrollment data before 1966. Further, since construction is not completed until 1974, the treatment effect estimate in 1971 through 1973 should be close to zero if it is the case that my synthetic control accurately estimates the latent factors driving women's enrollment. By

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

[2] Women's Medical is an all-women's school until 1970, so I drop the school from my donor pool to prevent this from confounding my results.

Figure A.1: University of Cincinnati First-Year Enrollment, 1960-1980



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

Figure A.2: University of Cincinnati Women's First-Year Enrollment, 1960-1980

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

not matching on these years, I allow for a simple graphical placebo test along these lines. Table A.3 summarizes the results of my estimation procedure, which constructs a synthetic University of Cincinnati from four medical schools.

Table A.3: Synthetic University of Cincinnati

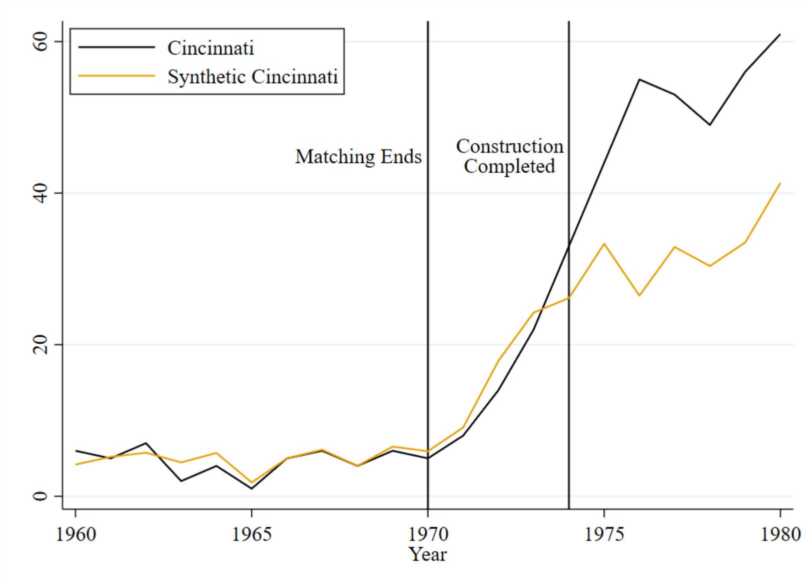
School	Weight	School	Weight	School	Weight
Albany	0.166	Indiana	0	Puerto Rico	0
Albert Einstein	0	Jefferson	0	Rochester	0
Boston	0	Johns Hopkins	0	SUNY-Buffalo	0
Bowman Gray	0	Kentucky	0	SUNY-Downstate	0
California-San Francisco	0	Loma Linda	0	SUNY-Upstate	0
Case Western Reserve	0	Loyola (Stritch)	0	South Dakota	0
Chicago Medical	0	Maryland	0	Southern California	0
Chicago-Pritzker	0	Medical College of GA	0.562	Stanford	0
Colorado	0	Michigan	0	Temple	0
Columbia	0	Missouri-Columbia	0	Tennessee	0
Cornell	0.121	New Jersey Medical	0	Utah	0.151
Duke	0	North Dakota	0	Vermont	0
Georgetown	0	Northwestern	0	Washington-St. Louis	0
Hahnemann	0	Oregon	0	West Virginia	0
Harvard	0	Pittsburgh	0	Yale	0

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

Figure A.3 plots the synthetic control against observed enrollment. Even though I do not match on 1971 through 1973, I am able to match the rise in women's enrollment well with an estimated treatment effect around 0, suggesting that my synthetic control has matched well on latent factors determining women's enrollment. Starting in 1974, I find a distinct break between these series - by 1977, three years after construction is completed, I estimate that the University of Cincinnati enrolls around 20 more women than it would have if it had not construction a new teaching facility. This point estimate of 20 students is stable through the end of my sample period.

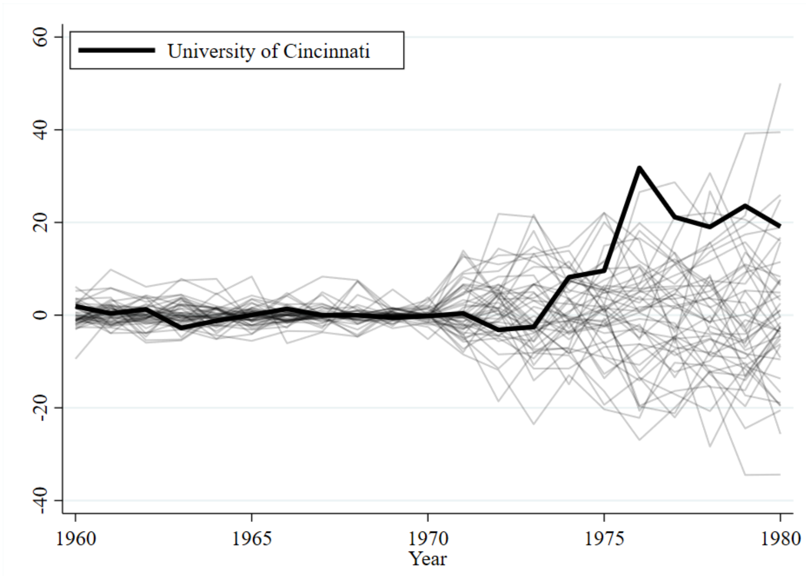
I perform the standard placebo test recommended in Abadie et al. (2010). I add the University of Cincinnati back into my donor pool, and run an identical procedure for all 46 medical schools. Figure A.4 plots the treatment effect estimate for every medical school, with results for the University of Cincinnati in bold; a graphical analysis confirms that my findings are extreme relative to the distribution plotted here. I confirm this by running the standard statistical test recommended by Abadie (2021)—I calculate a p-value of 0.022.

Figure A.3: Synthetic Control And Observed Enrollment



This figure plots women's first year enrollment for the University of Cincinnati against the same time series for my synthetic control. This is constructed by taking a weighted average of women's enrollment at other medical schools, where weights are given in Table A.3

Figure A.4: Placebo Test



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

APPENDIX B

Appendix to Chapter 2

B.1. Comparison of Impacts on Hourly, Weekly, and Annual Earnings

Our preferred measure of earnings is the log weekly wage, which equals log annual earnings last year divided by weeks worked in the last year. This variable has the advantage of not relying on reports of the number of hours worked during the week before the survey, which is measured for a different time period than annual earnings and the number of weeks worked.

To account for intensive margin labor supply adjustments, we include log hours worked per week as a covariate when the dependent variable is log weekly wages; we include as a covariate log hours worked per week and log weeks worked per year when the dependent variable is log annual wage earnings.

This appendix shows that our results are similar when estimating impacts on hourly, weekly, or annual earnings. Appendix Figure 2 shows results for the state-level equal pay law research design (research design 1) for our preferred specification (model 2). Appendix Figure 10 shows results for the industry-occupation-state-group gender wage gap design (research design 2) for our preferred specification (model 3). For each of the three dependent variables and both research designs, our estimates are not only very similar but statistically indistinguishable.

B.2. Sensitivity to Low Earnings Amounts in the CPS

During the 1960s, the CPS changed its sampling design, which resulted in a large number of low hourly earnings observations. In addition, many workers were not covered by the federal minimum wage law. As a result, it is difficult to know whether some of the reported wages below the statutory minimum wage are real or due to measurement error in reports of annual earnings, weeks worked, or hours worked. We explore the robustness of our results to winsorizing hourly, weekly, and annual earnings for both men and women at the lowest 10 percentiles of the earnings distribution for women from 1960 to 1964. The real wage level used for winsorization is fixed across years to avoid introducing changes over time that reflect changes in the CPS sampling frame.

Appendix Table 15 lists the dollar values, in 1964 and 2022 dollars, of the first 10 percentiles of women's hourly, weekly, and annual wages. The first percentile of the hourly wage distribution is \$0.17 in 1964 dollars, which amounts to 14 percent of the minimum wage in January 1964 for workers who had FLSA coverage before the 1961 amendments. The tenth percentile is \$0.65, which is 52 percent of this minimum wage. Winsorizing up to the 10th percentile is in line with the literature, especially given the fact that our analysis is focused on a period with less extensive coverage of the minimum wage and a much higher real minimum wage. For comparison, Derenoncourt and Montialoux (2021) study the effects of the FLSA expansions on the Black-White wage gap in the 1960s and winsorize the annual wage earnings data at the 5-percent level. Blau and Kahn (2017) study the gender gap in earnings and identify wages as being "too low" if they are lower than \$2 per hour in 2010 dollars, which amounts to \$0.29 in January 1964 dollars, or between the 2nd and 3rd percentile in 1964. Katz and Murphy (1992) and Autor et al. (2008) identify wages as being "too low" if they are below one-half of the 1982 minimum wage level for full-time workers, which is equivalent to $\$3.35 \times 0.50 \times 40$ hours = \$67 in 1982 dollars. Their use of a weekly minimum arises from their focus on full-time workers. Because our sample includes women working less than full-time, the hourly wage provides a more natural benchmark, and half of the \$3.35 minimum wage in 1982 amounts to \$0.55 in 1964 dollars, which falls just above the 7^{th} percentile for our sample.

Appendix Figure 3 displays event-study estimates for hourly, weekly, and annual earnings for the state equal pay law research design when winsorizing low wage levels. The post-1964 wage increases are smaller when winsorizing, but the winsorized point estimates fall within the 95-percent confidence interval of our main results. The spline estimate in 1968 for women's weekly wages is 0.087 (0.021) when not winsorizing and 0.070 (0.017) when winsorizing at the 7th percentile (p-value on the test of the difference = 0.004). Appendix Figure 11 shows comparable results for the 1960 gender wage gap research design. The spline estimate at the mean in 1968 for women's weekly wages is 0.100 (0.023) when not winsorizing and 0.065 (0.018) when winsorizing at the 7th percentile (p-value on the test of the difference = 0.001). In summary, the appendix shows that the paper's main results are smaller but survive winsorization of very low wages.

B.3. Adjusting Standard Errors for Estimates of the Gender Wage Gap

For our second research design, the key explanatory variable is a generated regressor: the estimated gender wage gap between men and women in 1960. It is possible that our standard errors are too small because they do not account for uncertainty in this estimate.

A common approach to accounting for generated regressors is to use a pairs bootstrap. In our setting, this would amount to re-sampling industry-occupation-state-group cells with replacement, keeping all observations within an industry-occupation-state cell together. However, because our gender gap variable is defined at the industry-occupation-state-level, the gender gap variable would be identical for each resampled cell. As a result, the pairs bootstrap cannot address the generated regressor issue in this setting.

Instead, we use an approach that combines the parametric bootstrap and the Bayesian bootstrap (Rubin, 1981). First, we estimate the heteroskedasticity-robust standard error on each industry-occupation-state cell's gender wage gap by regressing log hourly wages on an indicator for being a man among observations in a given cell in 1960. The point estimate from this regression is identical to the gender wage gap used in our main analysis, and the standard error reflects uncertainty in the gender wage gap estimate due to a finite sample size. Second, we implement a clustered version of the Bayesian bootstrap by drawing industry-occupation-state-group-cell-specific weights following the procedure described in the appendix to Angrist et al. (2017). In isolation, the Bayesian bootstrap produces similar results as clustering standard errors by industry-occupation-state-group. However, we can combine the Bayesian bootstrap with the parametric bootstrap by generating a normally-distributed gender wage gap variable with mean and standard deviation given by the first-step regression estimate. By generating a new gender wage gap variable in each bootstrap sample, this "parametric clustered Bayesian bootstrap" procedure accounts for uncertainty in the generated regressor.

Appendix Table 16 reports our main estimates from Table 2.2, with cluster-robust standard errors based on asymptotic approximations in parentheses, alongside standard errors from the parametric clustered Bayesian bootstrap in brackets. Likely owing to the fact that our generated regressor is calculated using a fairly large sample (the 5% sample of the 1960 Census), the two sets of standard errors are very similar. This provides some reassurance that our conclusions are not materially affected by sampling variability in the gender wage gap variable.

Table B.1: Kitawaga-Blinder-Oaxaca Decomposition of Changes in the Gender Gap in Weekly Wages, 1949 to 1959

	(1)	(2)	(3)
Year	1949	1959	1949 to 1959
A. Selected characteristics (mean)			
Log weekly wages			
Men	6.549	6.872	0.323
Women	6.012	6.170	0.159
Gender gap (men minus women)	0.537	0.701	0.164
Log hours worked in week before survey			
Men	3.735	3.732	-0.002
Women	3.607	3.511	-0.096
Gender gap (men minus women)	0.127	0.221	0.094
Years of education			
Men	9.684	10.516	0.832
Women	10.326	10.819	0.493
Gender gap (men minus women)	-0.642	-0.303	0.339
B. Decomposition 1: Without occupation or industry			
Explained gender wage gap	0.067	0.089	0.022
Unexplained gender wage gap	0.470	0.613	0.142
Components of explained gap (differences in characteristics)			
Log hours worked	0.011	0.037	0.027
Education	-0.028	-0.018	0.010
Potential experience	0.007	-0.005	-0.012
Married	0.064	0.061	-0.003
Nonwhite	0.014	0.014	-0.000
Components of unexplained gap (differences in coefficients)			
Log hours worked	-0.907	-1.104	-0.196
Education	-0.079	-0.197	-0.118
Potential experience	0.155	0.084	-0.070
Married	0.111	0.199	0.088
Nonwhite	0.015	0.009	-0.007
Constant	1.176	1.622	0.446
C. Decomposition 2: With occupation and industry			
Explained gender wage gap	0.171	0.270	0.099
Unexplained gender wage gap	0.366	0.431	0.065
Components of explained gap (differences in characteristics)			
Log hours worked	0.015	0.041	0.025
Education	-0.021	-0.013	0.008
Potential experience	0.006	-0.005	-0.011
Married	0.047	0.045	-0.002
Nonwhite	0.009	0.009	-0.000
Occupation	0.033	0.056	0.024
Industry	0.081	0.138	0.056
Components of unexplained gap (differences in coefficients)			
Log hours worked	-0.616	-0.597	0.019
Education	0.068	0.092	0.024
Potential experience	0.084	0.022	-0.062
Married	0.090	0.165	0.074
Nonwhite	-0.009	-0.018	-0.009
Occupation	-0.175	0.388	0.563

Industry	-0.026	-0.049	-0.023
Constant	0.950	0.429	-0.521

Table reports the difference in average hourly earnings (women minus men) from the Hearings on the Equal Pay Act in April of 1963, inflated to January 2022 dollars using the CPI-U. *Sources*: Hearings on the Equal Pay Act in April of 1963. Data on Furniture Manufacturing taken from Table 15 p. 38; Data on Power Laundries taken from Table 9 p. 33. Data on Eating and Drinking Places Taken from Table 12 p.36 (U.S. Congress, 1963).

Table B.2: Evidence Presented at 1963 Equal Pay Senate Hearings

City, Industry, and Occupation	Difference in Average Hourly Earnings (Women - Men) (2022$)
A. Chicago	
Furniture Manufacturing	
Assemblers, case goods	-2.25
Off-bearers, machine	0
Packers, furniture	-1.69
Sanders, furniture, hand	-2.91
Power Laundries	
Assemblers	-0.75
Clerks, retail, receiving	-5.57
Identifiers	-2.17
Pressers, machine (dry cleaning)	-2.83
Tumbler operators (laundry)	-2.26
Wrappers, bundle	-2.55
B. Winston-Salem	
Furniture Manufacturing	
Assemblers, case goods	-1.03
Packers, furniture	-1.12
Rubbers, furniture, hand	-0.09
Rubbers, furniture, machine	-0.37
Sanders, furniture, hand	-0.84
Sprayers	-1.22
C. Philadelphia	
Eating and Drinking Places	
Bus girls and boys	1.32
Counter attendants	-1.23
Pantry workers	0
Power Laundries	
Assemblers	2.83
Identifiers	0
Tumbler operators (laundry)	-0.75
Wrappers, bundle	-0.66

Panel A reports averages of the indicated variables using the paper's sample restrictions. Panels B and C report Kitagawa-Blinder-Oaxaca decompositions of the difference in mean log weekly wages between men and women. We weight the difference in observed characteristics by the coefficients for men. For each panel, the decomposition is estimated separately for each year in columns 1 and 2, and column 3 reports the difference between each term from 1949 to 1959. *Sources*: 1% sample of the 1950 Decennial Census, 5% sample of the 1960 Decennial Census (Ruggles et al., 2023). See text for details on sample selection and exclusion criteria.

Table B.3: Estimates of the Gender Gap using the 1963 Occupational Wage Survey

	(1)	(2)	(3)
	Dependent variable: Log weekly wage		
A. All jobs			
Women	-0.321	-0.314	-0.172
	(0.022)	(0.021)	(0.010)
Observations	4,337	4,337	4,337
R-squared	0.273	0.416	0.927
B. Jobs reporting hourly wages			
Women	-0.440	-0.433	-0.183
	(0.024)	(0.025)	(0.016)
Observations	1,843	1,843	1,843
R-squared	0.174	0.412	0.929
Covariates			
City FE		X	X
Narrow job classification FE			X

Coefficient on women captures the difference in log weekly wages earned by women relative to men (omitted). In panel A, we combine jobs reporting weekly wages and hourly wages by converting hourly wages into weekly equivalents (multiplying the hourly wage by 40 hours). In all regressions in panel A, we include a dummy variable equal to 1 for these jobs. In panel B, we examine the pay differential in log hourly wages in hourly wage jobs only and omit this covariate. Column 2 includes city fixed effects, and column 3 adds fixed effects for detailed occupational classes. Regressions are weighted by the number of employees in each sex-city-job observation. Heteroskedasticity robust standard errors are reported in parentheses. *Sources*: Data are from the Bureau of Labor Statistics' Occupational Wage Survey (U.S. Department of Labor, 1963).

Table B.4: Summary Statistics, by State Pre-Existing Equal Pay Law Status

	(1)	(2)	(3)	(4)
	Women		Men	
	No EPL	EPL	No EPL	EPL
Mean weekly wage in 1959 (2022 dollars)	525	634	975	1145
Mean weekly wage in 1964 (2022 dollars)	569	679	1087	1275
Mean nos cell number of employees in 1959	84,172	93,630	69,395	118,204
Mean nos cell number of employees in 1964	91,828	103,820	74,739	116,922
Mean nos cell number of annual hours in 1959	125 M	136 M	139 M	229 M
Mean nos cell number of annual hours in 1964	141 M	156 M	158 M	239 M
Mean nos cell gender wage gap in 1960	0.394	0.367	0.403	0.403

Table reports summary statistics for the main dependent and independent variables of interest for state groups that did not have an equal pay law (EPL) as of January 1, 1963 (columns 1 and 3) and state groups where at least one state did have such a law (columns 2 and 4). Gender wage gap in 1960 is calculated by industry-occupation-state-group cell. *Sources*: 5% sample of the 1960 Decennial Census and the 1965 CPS ASEC (Flood et al., 2022; Ruggles et al., 2023).

Table B.5: The Effect of the Equal Pay Act and Title VII on Weekly Wages using Pre-Existing State Equal Pay Laws, Event-Study Estimates

	(1)	(2)	(3)	(4)
		Women		Men
Year	Model 1	Model 2	Model 3	Model 2
1949	0.043	0.035	0.041	0.006
	(0.019)	(0.019)	(0.019)	(0.018)
1959	0.015	0.014	0.018	-0.021
	(0.019)	(0.019)	(0.019)	(0.016)
1961	0.022	0.023	0.027	-0.032
	(0.021)	(0.022)	(0.022)	(0.017)
1962	0.008	0.013	0.015	-0.032
	(0.030)	(0.031)	(0.030)	(0.011)
1963	-0.009	-0.011	-0.011	-0.040
	(0.014)	(0.014)	(0.014)	(0.010)
1964 (omitted)				
1965	0.076	0.073	0.074	-0.004
	(0.019)	(0.019)	(0.019)	(0.008)
1966	0.073	0.067	0.069	-0.011
	(0.020)	(0.021)	(0.021)	(0.008)
1967	0.089	0.082	0.080	0.012
	(0.019)	(0.019)	(0.019)	(0.011)
1968	0.091	0.080	0.079	0.020
	(0.019)	(0.019)	(0.019)	(0.014)
1969	0.101	0.090	0.087	0.003
	(0.017)	(0.017)	(0.017)	(0.014)
1970	0.093	0.084	0.081	0.012
	(0.016)	(0.017)	(0.017)	(0.015)
1971	0.090	0.082	0.078	0.013
	(0.017)	(0.017)	(0.017)	(0.016)
1972	0.097	0.085	0.083	0.015
	(0.021)	(0.020)	(0.019)	(0.016)
1973	0.110	0.100	0.098	0.025
	(0.024)	(0.022)	(0.023)	(0.018)
1974	0.116	0.107	0.100	0.035
	(0.029)	(0.027)	(0.026)	(0.027)
Observations	800,345	800,345	800,344	1,561,633

Table presents the event-study coefficients and standard errors from equation (2.5.1) presented in Figure 2.4. The standard errors have been corrected for heteroskedasticity and an arbitrary correlation within state group. See notes to Figure 2.4.
Sources: See Figure 2.3 notes.

Table B.6: Heterogeneity in the Effects of the Equal Pay Act and Title VII on Wages and Employment of Women using Pre-Existing State Equal Pay Laws

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All wage earners	Full-time wage earners	White	Black	Less than 12 years education	At least 12 years education	Age 25-44	Age 45-64	Married	Unmarried
A. Log weekly wage, mean 1960 level:	*$595*	*$646*	*$625*	*$387*	*$482*	*$700*	*$586*	*$607*	*$575*	*$631*
Spline estimate in 1968	0.087	0.079	0.084	0.085	0.112	0.074	0.072	0.101	0.092	0.066
	(0.021)	(0.019)	(0.020)	(0.051)	(0.029)	(0.018)	(0.026)	(0.023)	(0.024)	(0.025)
p-value, wild cluster bootstrap	[0.000]	[0.000]	[0.000]	[0.188]	[0.000]	[0.002]	[0.018]	[0.002]	[0.002]	[0.034]
Trend-break in 1964	0.022	0.020	0.002	-0.005	-0.004	0.004	-0.002	0.005	0.002	-0.001
	(0.005)	(0.005)	(0.007)	(0.013)	(0.010)	(0.006)	(0.008)	(0.008)	(0.008)	(0.006)
Pre-trend slope, 1949-1964	-0.001	-0.003	0.005	0.005	0.005	0.004	0.006	0.005	0.007	0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
B. Log employees, mean 1960 level:	*90,282*	*55,710*	*77,544*	*63,140*	*85,057*	*47,738*	*51,055*	*47,123*	*60,355*	*38,588*
Spline estimate in 1968	0.020	-0.001	0.007	0.046	0.143	-0.094	-0.017	0.002	0.034	-0.002
	(0.068)	(0.068)	(0.073)	(0.085)	(0.080)	(0.067)	(0.065)	(0.068)	(0.064)	(0.045)
p-value, wild cluster bootstrap	[0.784]	[0.992]	[0.912]	[0.635]	[0.120]	[0.160]	[0.790]	[0.988]	[0.637]	[0.984]
Trend-break in 1964	0.005	-0.000	-0.017	-0.020	-0.019	-0.009	-0.015	-0.014	-0.011	0.016
	(0.017)	(0.017)	(0.018)	(0.017)	(0.022)	(0.018)	(0.015)	(0.016)	(0.015)	(0.010)
Pre-trend slope, 1949-1964	0.009	0.012	0.021	-0.001	0.020	0.011	0.018	0.014	0.010	-0.004
	(0.005)	(0.007)	(0.005)	(0.005)	(0.006)	(0.007)	(0.006)	(0.007)	(0.004)	(0.006)
C. Log annual hours worked, mean 1960 level:	*132 M*	*111 M*	*115 M*	*83 M*	*119 M*	*73 M*	*73 M*	*70 M*	*83 M*	*62 M*
Spline estimate in 1968	0.026	0.006	0.010	0.060	0.153	-0.098	-0.015	0.008	0.027	0.016
	(0.069)	(0.006)	(0.073)	(0.107)	(0.085)	(0.073)	(0.071)	(0.067)	(0.065)	(0.051)
p-value, wild cluster bootstrap	[0.739]	[0.938]	[0.888]	[0.663]	[0.126]	[0.176]	[0.858]	[0.926]	[0.697]	[0.780]
Trend-break in 1964	0.006	0.001	-0.025	-0.024	-0.025	-0.019	-0.023	-0.024	-0.016	0.015
	(0.017)	(0.017)	(0.017)	(0.020)	(0.022)	(0.019)	(0.016)	(0.016)	(0.015)	(0.012)
Pre-trend slope, 1949-1964	0.010	0.012	0.024	0.001	0.024	0.016	0.023	0.018	0.013	-0.005
	(0.006)	(0.007)	(0.007)	(0.005)	(0.007)	(0.008)	(0.008)	(0.007)	(0.005)	(0.007)
Observations	800,345	550,813	695,541	98,485	354,690	441,614	443,988	356,230	514,032	286,184
Industry-Occupation-State-Year Cells	5,264	4,640	4,992	863	2,460	4,395	3,888	3,584	4,319	3,008

Notes: Table presents the spline estimates and standard errors for women. Column 1 replicates column 1 of Table 2.1. Columns 2 limits the sample to full-time wage earners (at least 35 hours per week and 27 or more weeks per year). Columns 3-4 restrict the sample to White and Black workers (race covariate excluded in these specifications). Columns 5-6 restrict the sample to individuals with less than or at least 12 years of education. Columns 7-8 restrict the sample to workers of different ages (age covariates excluded). Columns 9-10 restrict the sample to married and unmarried individuals. Individual observations are reported for Panel A, and the number of job cells are reported for Panels B and C. See Table 2.1 notes and text for details. *Sources*: See Figure 2.3 notes.

Table B.7: The Effect of the Equal Pay Act and Title VII on Employment using Pre-Existing State Equal Pay Laws, Event-Study Estimates

	(1)	(2)	(3)	(4)
	Log Number of Employees		Log Annual Hours Worked	
Year	Women	Men	Women	Men
1949	-0.088	-0.088	-0.105	-0.072
	(0.081)	(0.098)	(0.084)	(0.104)
1959	0.026	0.030	0.023	0.017
	(0.047)	(0.057)	(0.042)	(0.063)
1961	-0.106	-0.109	-0.078	-0.113
	(0.047)	(0.048)	(0.058)	(0.049)
1962	0.015	0.039	-0.010	0.016
	(0.061)	(0.052)	(0.058)	(0.059)
1963	0.049	0.041	0.059	0.027
	(0.027)	(0.034)	(0.044)	(0.042)
1964 (omitted)				
1965	0.056	0.037	0.058	0.028
	(0.062)	(0.044)	(0.071)	(0.048)
1966	0.118	0.091	0.113	0.065
	(0.035)	(0.027)	(0.028)	(0.027)
1967	0.103	0.076	0.112	0.050
	(0.058)	(0.047)	(0.066)	(0.050)
1968	0.128	0.089	0.125	0.068
	(0.064)	(0.052)	(0.069)	(0.055)
1969	0.131	0.139	0.154	0.119
	(0.062)	(0.051)	(0.065)	(0.050)
1970	0.116	0.108	0.130	0.122
	(0.062)	(0.047)	(0.069)	(0.047)
1971	0.143	0.135	0.158	0.140
	(0.066)	(0.057)	(0.073)	(0.057)
1972	0.166	0.158	0.166	0.158
	(0.066)	(0.050)	(0.072)	(0.052)
1973	0.169	0.168	0.173	0.172
	(0.070)	(0.060)	(0.079)	(0.059)
1974	0.149	0.178	0.120	0.155
	(0.070)	(0.064)	(0.079)	(0.071)
Observations	5,264	10,640	5,264	10,640

Notes: Table presents the event-study coefficients and standard errors from model 2 of equation (2.3.2) presented in Figure 2.6. The standard errors have been corrected for heteroskedasticity and an arbitrary correlation within state group. See notes to Figure 2.6. *Sources*: See Figure 2.3 notes.

Table B.8: Cells Included in the 1960 Gender Wage Gap Analysis, by Occupation and Industry

Occupation	Industry	Number of State Groups
Professional, Technical	Mining	2
	Construction	6
	Manufacturing	21
	Transportation, Communications, Electric, Gas & Sanitary Services	19
	Wholesale Trade	5
	Retail Trade	19
	Finance, Insurance & Real Estate	19
	Services	21
	Public Administration	21
Managers, Officials and Proprietors	Mining	1
	Construction	10
	Manufacturing	21
	Transportation, Communications, Electric, Gas & Sanitary Services	18
	Wholesale Trade	19
	Retail Trade	21
	Finance, Insurance & Real Estate	21
	Services	21
	Public Administration	21
Clerical	Mining	13
	Construction	21
	Manufacturing	21
	Transportation, Communications, Electric, Gas & Sanitary Services	21
	Wholesale Trade	21
	Retail Trade	21
	Finance, Insurance & Real Estate	21
	Services	21
	Public Administration	21
Sales	Manufacturing	20
	Transportation, Communications, Electric, Gas & Sanitary Services	3
	Wholesale Trade	17
	Retail Trade	21
	Finance, Insurance & Real Estate	21
	Services	18
Craftsmen	Construction	2
	Manufacturing	21
	Transportation, Communications, Electric, Gas & Sanitary Services	15
	Wholesale Trade	4
	Retail Trade	20
	Finance, Insurance & Real Estate	1
	Services	19
	Public Administration	5
Operatives	Manufacturing	21
	Transportation, Communications, Electric, Gas & Sanitary Services	18
	Wholesale Trade	20
	Retail Trade	21
	Finance, Insurance & Real Estate	2
	Services	21
	Public Administration	14
Service Workers	Manufacturing	20
	Transportation, Communications, Electric, Gas & Sanitary Services	19
	Wholesale Trade	1
	Retail Trade	21
	Finance, Insurance & Real Estate	20
	Services	21
	Public Administration	20
Non-farm Laborers	Manufacturing	20
	Transportation, Communications, Electric, Gas & Sanitary Services	6
	Wholesale Trade	2
	Retail Trade	9
	Services	10
	Public Administration	2

Total	**942**

Table reports the number of industry-occupation-state-group cells included in the analysis. The final column reports the number of state-groups within each occupation-industry pair. *Sources*: 5% sample of the 1960 Decennial Census (Ruggles et al., 2023).

Table B.9: Cells Excluded from the 1960 Gender Wage Gap Analysis, by Occupation and Industry

Occupation	Industry	Number of State Groups
Professional, Technical	Mining	19
	Construction	15
	Transportation, Communications, Electric, Gas & Sanitary Services	2
	Wholesale Trade	16
	Retail Trade	2
	Finance, Insurance & Real Estate	2
Managers, Officials and Proprietors	Mining	20
	Construction	11
	Transportation, Communications, Electric, Gas & Sanitary Services	3
	Wholesale Trade	2
Clerical	Mining	8
Sales	Mining	21
	Construction	21
	Manufacturing	1
	Transportation, Communications, Electric, Gas & Sanitary Services	18
	Wholesale Trade	4
	Services	3
	Public Administration	21
Craftsmen	Mining	21
	Construction	19
	Transportation, Communications, Electric, Gas & Sanitary Services	6
	Wholesale Trade	17
	Retail Trade	1
	Finance, Insurance & Real Estate	20
	Services	2
	Public Administration	16
Operatives	Mining	21
	Construction	21
	Transportation, Communications, Electric, Gas & Sanitary Services	3
	Wholesale Trade	1
	Finance, Insurance & Real Estate	19
	Public Administration	7
Service Workers	Mining	21
	Construction	21
	Manufacturing	1
	Transportation, Communications, Electric, Gas & Sanitary Services	2
	Wholesale Trade	20
	Finance, Insurance & Real Estate	1
	Public Administration	1
Non-farm Laborers	Mining	21
	Construction	21
	Manufacturing	1
	Transportation, Communications, Electric, Gas & Sanitary Services	15
	Wholesale Trade	19
	Retail Trade	12
	Finance, Insurance & Real Estate	21
	Services	11
	Public Administration	19
Total		**570**

Table reports the number of industry-occupation-state-group cells excluded from the analysis. The final column reports the number of state-groups within each occupation-industry pair that are dropped from the analysis because fewer than 10 men and women wage earners are observed in 1960 or there are no observations in the ASEC during our period of interest. We also drop cells in the agriculture industry or farmer and farm-laborer occupations. *Sources*: 5% sample of the 1960 Decennial Census (Ruggles et al., 2023).

Table B.10: Observation Counts by Sex for Year, Industry, and Occupation

	Observations	
Year, Industry, and Occupation	Men	Women
A. Year (of wage or weeks observation, or survey year - 1)		
1949	68,888	28,639
1959	1,329,790	674,676
1961	11,078	5,819
1962	7,811	4,249
1963	11,373	6,109
1964	11,331	6,215
1965	24,093	13,234
1966	15,408	8,493
1967	23,981	14,149
1968	24,300	14,393
1969	23,437	14,146
1970	23,322	14,305
1971	22,354	13,900
1972	21,740	13,734
1973	21,334	13,778
1974	20,935	13,948
B. Industry		
Mining	32,695	1,424
Construction	160,298	6,432
Manufacturing	649,683	223,953
Transportation, Communications, Electric, Gas & Sanitary Services	185,461	31,865
Wholesale Trade	76,048	20,241
Retail Trade	171,370	154,370
Finance, Insurance & Real Estate	59,935	47,009
Services	204,015	330,382
Public Administration	121,670	44,111
C. Occupation		
Professional, technical	198,752	125,208
Managers, Officials and Proprietors	160,075	26,786
Clerical	132,717	265,676
Sales	107,363	64,267
Craftsmen	416,512	12,589
Operatives	408,780	174,146
Service Workers	110,528	186,145
Non-farm Laborers	126,448	4,970

Table reports the number of observations in our wage earner sample by sex for each year, industry, and observation. *Sources*: See Figure 2.3 notes.

Table B.11: The Effect of the Equal Pay Act and Title VII on Weekly Wages using the 1960 Gender Wage Gap, Event-Study Estimates

	(1)	(2)	(3)	(4)
		Women		Men
Year	Model 1	Model 2	Model 3	Model 3
1949	0.093	0.092	0.012	0.003
	(0.021)	(0.020)	(0.034)	(0.018)
1959	0.015	0.009	-0.003	0.008
	(0.020)	(0.020)	(0.031)	(0.016)
1961	0.003	0.002	0.032	0.005
	(0.031)	(0.031)	(0.045)	(0.024)
1962	0.008	0.001	-0.000	0.003
	(0.033)	(0.030)	(0.046)	(0.023)
1963	0.016	0.016	0.027	-0.010
	(0.025)	(0.024)	(0.042)	(0.021)
1964 (omitted)				
1965	0.007	-0.001	0.018	-0.007
	(0.027)	(0.022)	(0.033)	(0.018)
1966	0.062	0.056	0.091	-0.007
	(0.035)	(0.031)	(0.045)	(0.020)
1967	0.099	0.092	0.098	-0.015
	(0.030)	(0.027)	(0.038)	(0.020)
1968	0.100	0.094	0.083	0.008
	(0.031)	(0.027)	(0.043)	(0.020)
1969	0.119	0.114	0.129	-0.002
	(0.031)	(0.027)	(0.040)	(0.020)
1970	0.103	0.101	0.077	0.006
	(0.031)	(0.027)	(0.044)	(0.020)
1971	0.097	0.092	0.110	-0.007
	(0.030)	(0.027)	(0.038)	(0.020)
1972	0.151	0.145	0.158	-0.013
	(0.035)	(0.031)	(0.043)	(0.020)
1973	0.155	0.155	0.216	-0.015
	(0.034)	(0.028)	(0.040)	(0.021)
1974	0.167	0.164	0.176	-0.029
	(0.037)	(0.030)	(0.046)	(0.022)
Observations	797,272	797,272	797,272	1,362,199

Table presents the event-study coefficients and standard errors from equation (2.5.1) presented in Figure 2.9. The standard errors have been corrected for heteroskedasticity and an arbitrary correlation within industry-occupation-state-group. Point estimates and standard errors are multiplied by the average gender wage gap in the 1960 Census (equal to 0.374). See notes to Figure 2.9. *Sources*: See Figure 2.3 notes.

Table B.12: The Effects of the Equal Pay Act and Title VII on The Distribution of Weekly Wages using 1960 Gender Wage Gaps

	(1) P10	(2) P25	(3) P50	(4) P75	(5) P90
A. Women					
Spline estimate in 1968 at mean Gap	0.314	0.184	-0.036	-0.032	0.085
	(0.099)	(0.040)	(0.029)	(0.024)	(0.037)
Trend-break in 1964	0.079	0.046	-0.009	-0.008	0.021
	(0.025)	(0.010)	(0.007)	(0.006)	(0.009)
Pre-trend slope, 1949-1964	0.016	-0.006	-0.002	-0.001	-0.004
	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
B. Men					
Spline estimate in 1968 at mean Gap	0.052	0.014	-0.046	-0.057	-0.031
	(0.023)	(0.020)	(0.012)	(0.014)	(0.025)
Trend-break in 1964	0.013	0.004	-0.012	-0.014	-0.008
	(0.006)	(0.005)	(0.003)	(0.004)	(0.006)
Pre-trend slope, 1949-1964	-0.007	-0.003	0.002	0.004	0.006
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Covariates					
Demographics, Ind-Occ-State FEs, Year FEs	X	X	X	X	X
Ind-Year FEs, Occ-Year FEs, State-Year FEs	X	X	X	X	X

Table reports estimates of equation (2.5.2), where the dependent variable is the recentered influence function for weekly log wages and all regressions include as covariates an indicator for nonwhite race, a quadratic function in age, and fixed effects for industry-occupation-state-group, industry-year, occupation-year, and state-group-year. Spline estimates at mean *Gap* are multiplied by the average gender gap (equal to 0.374). As in Figure 2.9, we pool years 1959 and 1961-1963 because of small sample sizes in the CPS. See notes to Table 2.2 for details on sample and specification. *Sources*: See Figure 2.3 notes.

Table B.13: Heterogeneity in the Effects of the Equal Pay Act and Title VII on Wages and Employment of Women using 1960 Gender Wage Gaps

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All wage earners	Full-time wage earners	White	Black	Less than 12 years education	At least 12 years education	Age 25-44	Age 45-64	Married	Unmarried
A. Log weekly wage										
Spline estimate in 1968 at mean Gap	0.100	0.140	0.079	0.045	0.093	0.095	0.096	0.102	0.090	0.111
	(0.023)	(0.023)	(0.024)	(0.059)	(0.030)	(0.028)	(0.027)	(0.035)	(0.030)	(0.031)
Trend-break in 1964	0.067	0.098	0.056	0.022	0.052	0.077	0.065	0.065	0.060	0.074
	(0.015)	(0.016)	(0.017)	(0.029)	(0.017)	(0.023)	(0.018)	(0.022)	(0.020)	(0.021)
Pre-trend slope, 1949-1964	-0.001	-0.010	0.001	-0.008	-0.002	-0.005	0.002	-0.002	0.003	-0.006
	(0.004)	(0.004)	(0.005)	(0.009)	(0.005)	(0.006)	(0.004)	(0.007)	(0.005)	(0.005)
B. Log number of employees										
Spline estimate in 1968 at mean Gap	-0.118	-0.060	-0.050	-0.752	-0.239	-0.005	-0.160	-0.115	-0.085	-0.213
	(0.047)	(0.055)	(0.050)	(0.321)	(0.085)	(0.068)	(0.056)	(0.073)	(0.067)	(0.069)
Trend-break in 1964	-0.079	-0.042	-0.035	-0.372	-0.133	-0.004	-0.108	-0.073	-0.057	-0.143
	(0.031)	(0.039)	(0.035)	(0.159)	(0.047)	(0.055)	(0.038)	(0.046)	(0.045)	(0.046)
Pre-trend slope, 1949-1964	-0.005	-0.010	0.024	0.059	0.000	0.026	-0.016	0.007	-0.016	0.016
	(0.011)	(0.016)	(0.012)	(0.030)	(0.010)	(0.018)	(0.017)	(0.015)	(0.014)	(0.015)
C. Log annual hours worked										
Spline estimate in 1968 at mean Gap	-0.087	-0.086	-0.063	-0.573	-0.157	-0.021	-0.130	-0.079	-0.058	-0.135
	(0.052)	(-0.086)	(0.058)	(0.502)	(0.081)	(0.076)	(0.056)	(0.077)	(0.076)	(0.080)
Trend-break in 1964	-0.058	-0.060	-0.044	-0.284	-0.088	-0.017	-0.088	-0.050	-0.038	-0.090
	(0.034)	(0.039)	(0.041)	(0.249)	(0.045)	(0.062)	(0.038)	(0.049)	(0.051)	(0.053)
Pre-trend slope, 1949-1964	-0.019	-0.016	0.012	0.019	-0.019	0.017	-0.026	-0.001	-0.025	-0.004
	(0.013)	(0.015)	(0.014)	(0.016)	(0.011)	(0.018)	(0.017)	(0.017)	(0.015)	(0.018)
Group mean *Gap*	0.374	0.358	0.356	0.505	0.449	0.309	0.370	0.394	0.374	0.374
Observations	797,272	548,891	693,141	97,935	353,102	440,266	442,429	354,843	512,242	285,029
Industry-Occupation-State-Year Cells	5,264	4,640	4,976	847	2,430	4,395	3,888	3,568	4,319	3,008

Table presents the spline estimates and standard errors for women. Column 1 replicates column 1 of Table 2.2. See notes to Appendix Table B.6 for descriptions of samples in remaining columns. The spline estimates in 1968 are scaled using the mean gender gap for the group, whose value in the data is reported in the third-to-last row. Individual observations are reported for Panel A, and the number of job cells are reported for Panels B and C. See Table 2.2 notes and text for details. *Sources*: See Figure 2.3 notes.

Table B.14: The Effect of the Equal Pay Act and Title VII on Employment using the 1960 Gender Wage Gap, Event-Study Estimates

	(1)	(2)	(3)	(4)
	Log Number of Employees		Log Annual Hours Worked	
Year	Women	Men	Women	Men
1949	0.095	-0.076	0.125	-0.030
	(0.087)	(0.043)	(0.100)	(0.045)
1959	0.075	-0.015	0.038	0.013
	(0.071)	(0.037)	(0.087)	(0.039)
1961	0.099	-0.075	0.094	-0.105
	(0.090)	(0.056)	(0.123)	(0.062)
1962	0.048	-0.097	0.040	-0.110
	(0.092)	(0.055)	(0.112)	(0.060)
1963	0.170	0.037	0.088	0.039
	(0.077)	(0.042)	(0.095)	(0.044)
1964 (omitted)				
1965	0.038	0.018	0.018	0.021
	(0.067)	(0.043)	(0.078)	(0.045)
1966	-0.021	-0.033	-0.081	-0.026
	(0.092)	(0.048)	(0.120)	(0.049)
1967	-0.039	0.043	-0.104	0.067
	(0.089)	(0.046)	(0.105)	(0.048)
1968	-0.046	-0.025	-0.078	-0.005
	(0.102)	(0.046)	(0.121)	(0.048)
1969	-0.088	-0.016	-0.125	0.012
	(0.101)	(0.045)	(0.136)	(0.049)
1970	-0.062	-0.077	-0.117	-0.069
	(0.088)	(0.048)	(0.098)	(0.052)
1971	-0.160	-0.105	-0.284	-0.088
	(0.094)	(0.047)	(0.101)	(0.048)
1972	-0.178	-0.102	-0.217	-0.093
	(0.093)	(0.047)	(0.105)	(0.049)
1973	-0.098	-0.052	-0.169	-0.049
	(0.086)	(0.047)	(0.104)	(0.048)
1974	-0.199	-0.004	-0.256	0.031
	(0.089)	(0.049)	(0.116)	(0.056)
Observations	5,264	10,640	5,264	10,640

Table presents the event-study coefficients and standard errors from model 3 of equation (2.5.1) presented in Figure 2.11. The standard errors have been corrected for heteroskedasticity and an arbitrary correlation within industry-occupation-state-group. Point estimates and standard errors are multiplied by the average gender wage gap in the 1960 Census (equal to 0.374). See notes to Figure 2.11. *Sources*: See Figure 2.3 notes.

Table B.15: Dollar Values of Percentiles of Hourly, Weekly, and Annual Wages for Women

	Hourly Wage		Weekly Wage		Annual Wage	
Percentile	Value in 1964$	Value in 2022$	Value in 1964$	Value in 2022$	Value in 1964$	Value in 2022$
1	0.17	1.53	3.92	35.63	68.44	622.68
2	0.25	2.32	6.02	54.76	121.97	1109.79
3	0.32	2.92	8.00	72.74	182.96	1664.69
4	0.38[a]	3.44	9.93	90.31	218.43	1987.43
5	0.43[b]	3.87	11.75	106.91	274.78	2500.14
6	0.48	4.36	12.81	116.57	316.26	2877.52
7	0.52[c]	4.76	14.69	133.68	373.29	3396.42
8	0.57	5.16	15.82	143.95	414.76	3773.80
9	0.61	5.55	17.56	159.74	484.81	4411.12
10	0.65	5.90	18.82	171.23	518.46	4717.25

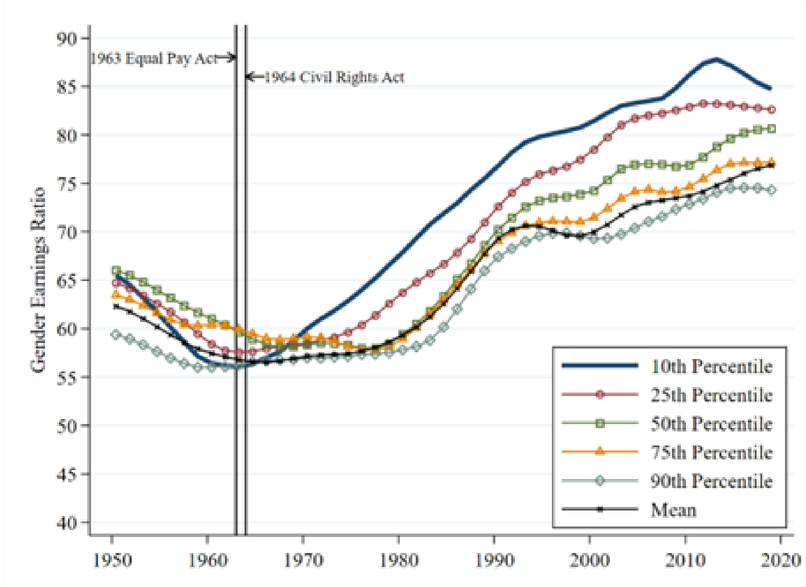
Percentiles are calculated separately for each outcome using the 1960 Census and 1962-1964 CPS. [a]Blau and Kahn (2017) exclude workers earning less than 29% of the minimum wage, or around \$0.36 per hour in 1964 dollars (\$1.25 $\times$ 0.29 = \$0.36). [b]Derenoncourt and Montialoux (2021) study the effects of the FLSA expansions on the Black-White wage gap in the 1960s and winsorize the annual wage data at the 5-percent level. [c]Katz and Murphy (1992) exclude workers earning less than 50% of the 1982 minimum wage (\$3.35), which is equivalent to around \$0.55 per hour in 1964. *Sources*: 1% sample of the 1960 Decennial Census, 1962-1964 CPS ASEC (Flood et al., 2022; Ruggles et al., 2023). See text for details on sample selection and exclusion criteria.

Table B.16: The Effects of the Equal Pay Act and Title VII on Wages and Employment using 1960 Gender Wage Gaps, Robustness to Accounting for the Gender Gap as a Generated Regressor

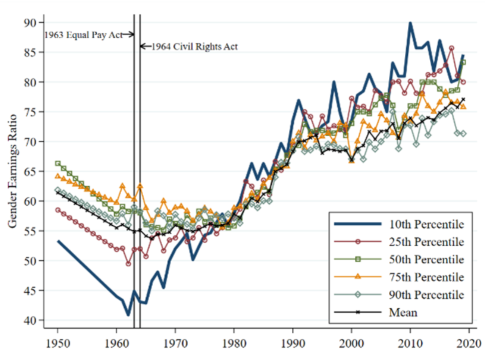
	(1)	(2)	(3)
	Women	Men	Women-Men
A. Log weekly wage			
Spline estimate in 1968 at mean Gap	0.100	-0.007	0.107
	(0.023)	(0.011)	(0.025)
	[0.023]	[0.010]	[0.025]
Trend-break in 1964	0.067	-0.004	0.071
	(0.015)	(0.008)	(0.017)
	[0.015]	[0.007]	[0.016]
Pre-trend slope, 1949-1964	-0.001	-0.000	-0.001
	(0.004)	(0.002)	(0.004)
	[0.004]	[0.002]	[0.004]
B. Log number of employees			
Spline estimate in 1968 at mean Gap	-0.118	-0.062	-0.057
	(0.047)	(0.029)	(0.048)
	[0.044]	[0.026]	[0.051]
Trend-break in 1964	-0.079	-0.041	-0.038
	(0.031)	(0.019)	(0.032)
	[0.030]	[0.017]	[0.034]
Pre-trend slope, 1949-1964	-0.005	0.015	-0.020
	(0.011)	(0.005)	(0.011)
	[0.010]	[0.005]	[0.011]
C. Log number of annual hours worked			
Spline estimate in 1968 at mean Gap	-0.087	-0.047	-0.039
	(0.052)	(0.030)	(0.054)
	[0.053]	[0.027]	[0.056]
Trend-break in 1964	-0.058	-0.032	-0.026
	(0.034)	(0.020)	(0.036)
	[0.035]	[0.018]	[0.038]
Pre-trend slope, 1949-1964	-0.019	0.008	-0.026
	(0.012)	(0.005)	(0.012)
	[0.012]	[0.005]	[0.012]
Observations	797,272	1,362,199	2,159,471
Sex-industry-occupation-state-year cells	5,264	10,640	15,904
Covariates			
Demographics, Ind-Occ-State FEs, Year FEs	X	X	X
Ind-Year FEs, Occ-Year FEs, State-Year FEs	X	X	X

Table presents the spline estimates and asymptotic standard errors clustered by industry-occupation-state-group in parentheses. The standard errors in brackets are based on the parametric clustered Bayesian bootstrap described in Appendix B.3, which accounts for the fact that *Gap* is a generated regressor. See notes to Table 2.2. *Sources*: See Figure 2.3 notes.

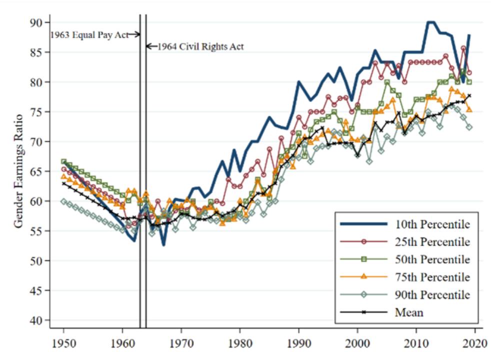
Figure B.1: Additional Estimates of the U.S. Gender Gap in Wage Earnings



(a) Smoothed Census/CPS Estimates for Full-Time, Full-Year Workers



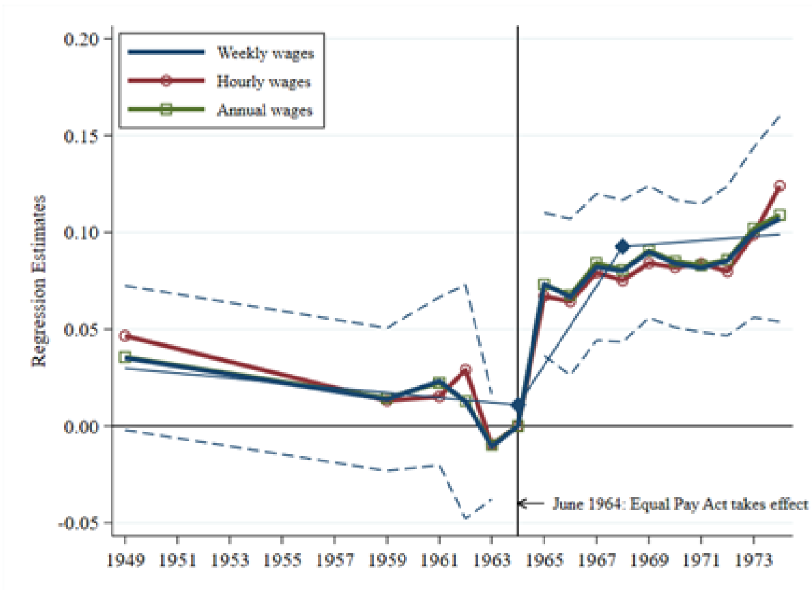
(b) Unsmoothed Census/CPS Estimates for Full-Time Workers with at least 27 Weeks of Work in the Previous Year



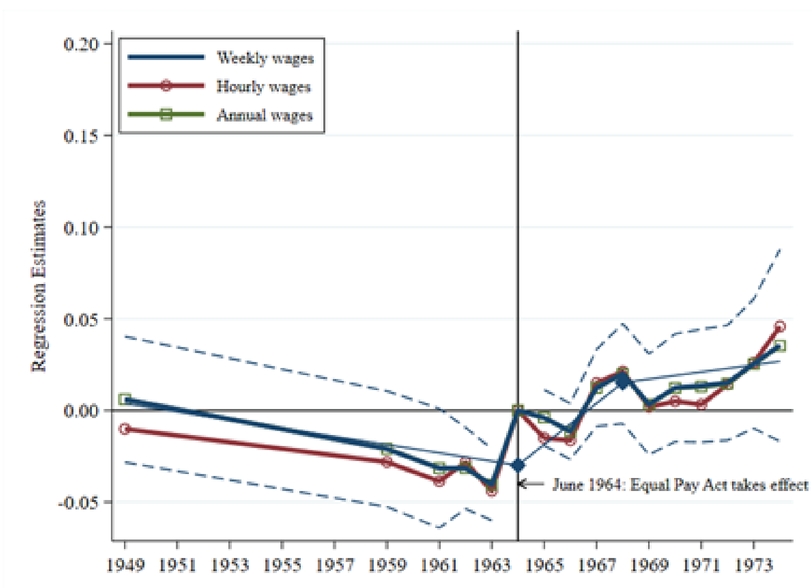
(c) Unsmoothed Census/CPS Estimates for Full-Time, Full-Year Workers

Figure uses the 1950 and 1960 Decennial Censuses and the 1962 to 2020 ASEC. We linearly extrapolate values for earnings years 1950-1958 and 1960 when Census and CPS data are not available. The sample in figures B.1a and B.1c consists of wage and salary workers ages 16-64 who work full-time (35+ hours), full-year (50+ weeks worked), and report positive wage income in the previous year. The sample in figure B.1b consists of 25–64-year-old, full-time workers working at least 27 weeks in the previous year. In figure B.1a, we smooth the series using a local linear regression with a bandwidth of 2 years. We plot the gender earnings ratio at the *pth* percentile/mean by taking the ratio of the *pth* percentile/mean of the wage distribution for women over the *pth* percentile/mean of the wage distribution for men. See notes to Figure 2.1 for additional details on the sample.

Figure B.2: The Effect of the Equal Pay Act and Title VII on Wages using Pre-Existing State Equal Pay Laws, Robustness to Using Hourly and Annual Wages



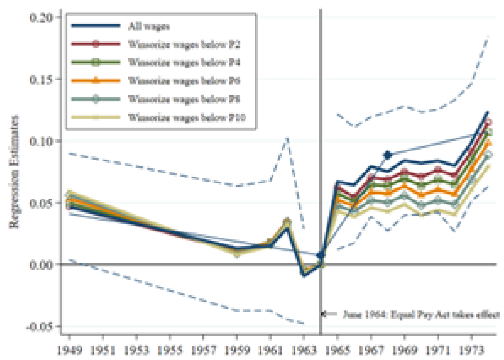
(a) Women



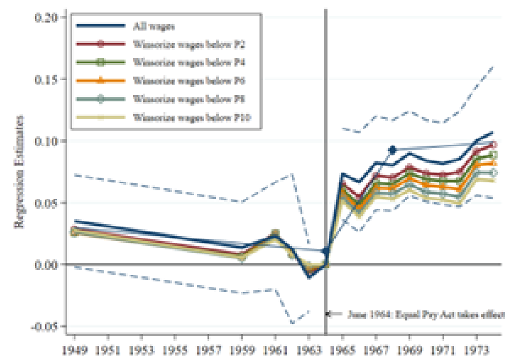
(b) Men

Figure plots the event-study coefficients from equation (2.3.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation within state-group. The dependent variable is either the log weekly wage (our preferred approach), log hourly wage, or log annual wage. Log hourly wage is log annual wage earnings less log weeks worked last year and log hours worked in the reference week. The spline (equation (2.3.3)) is shown for the log weekly wage. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, and fixed effects for industry-occupation-state-group, industry-year, and occupation-year. We include log hours worked as a covariate when the dependent variable is log weekly wages and log hours worked and log weeks worked when the dependent variable is log annual wages. See description in Appendix B.1. *Sources*: See Figure 2.3.

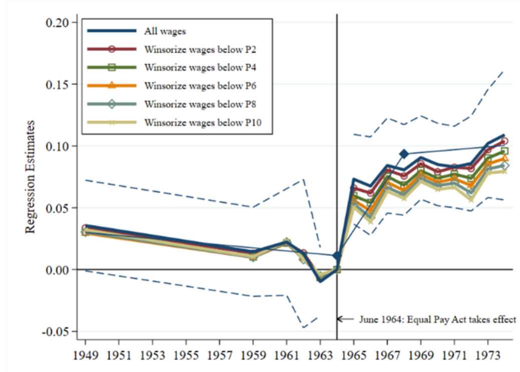
Figure B.3: The Effects of the Equal Pay Act and Title VII on Wages using Pre-Existing State Equal Pay Laws, Robustness to Winsorizing Low Wages



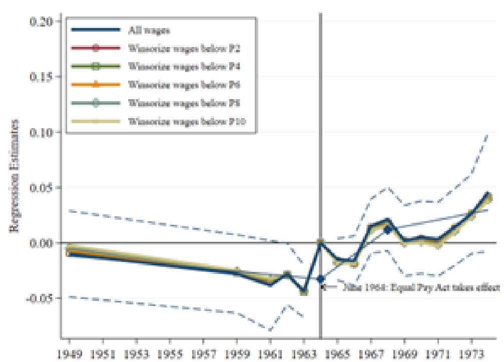
(a) Hourly Wages, Women



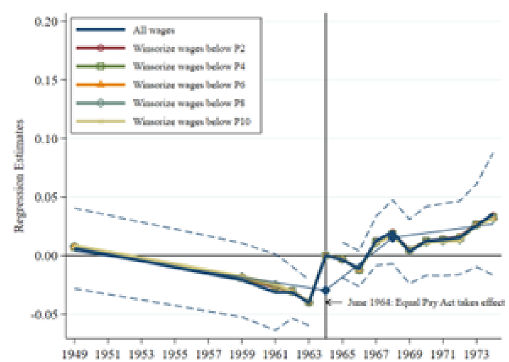
(b) Weekly Wages, Women



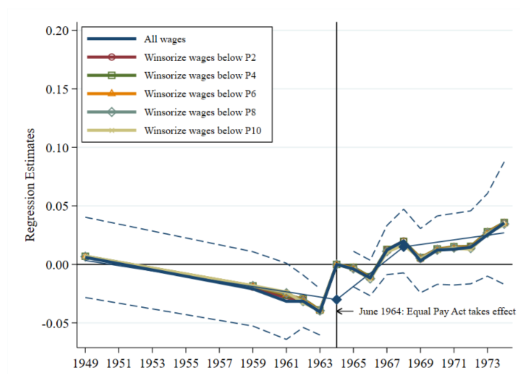
(c) Annual Wages, Women



(d) Hourly Wages, Men



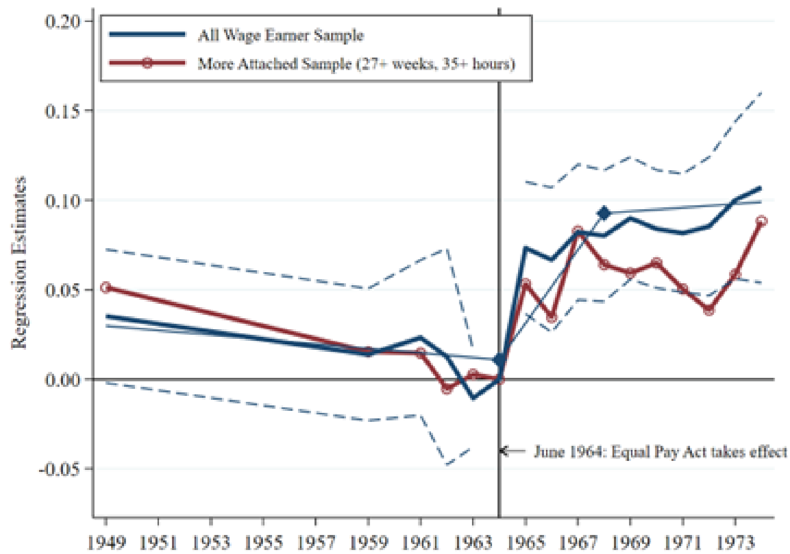
(e) Weekly Wages, Men



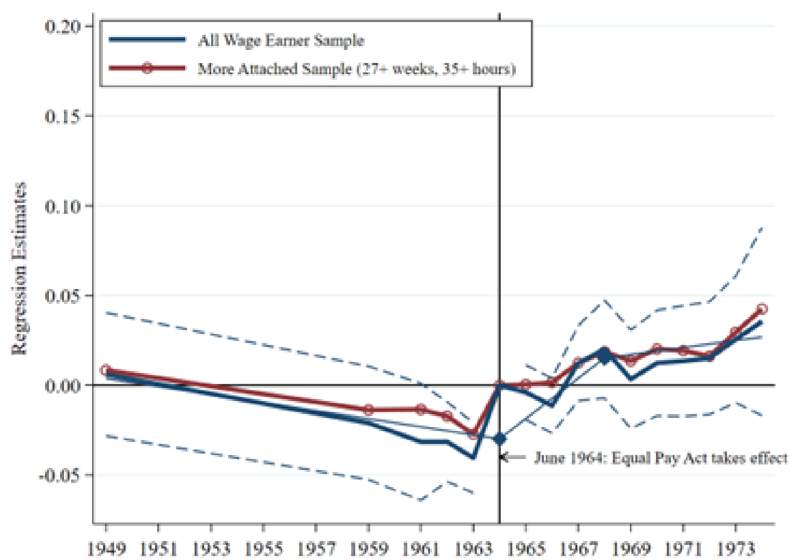
(f) Annual Wages, Men

Figure plots the event-study coefficients from equation (2.3.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation within state-group. The dependent variables are the unwinsorized log hourly, weekly, or annual wage ("all wages") and their winsorized counterparts at the indicated percentile (see Appendix Table B.15 for the value in levels). The spline (equation (2.3.3)) is shown for the unwinsorized dependent variable. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, and fixed effects for industry-occupation-state-group, industry-year, and occupation-year. We include log hours worked as a covariate when the dependent variable is log weekly wages and log hours worked and log weeks worked when the dependent variable is log annual wages. See description in Appendix B.2 for details on winsorizing and Appendix B.1 for details on dependent variables. *Source*: See Figure 2.3.

Figure B.4: The Effect of the Equal Pay Act and Title VII on Wages using Pre-Existing State Equal Pay Laws, Robustness to Limiting Sample to More Attached Workers



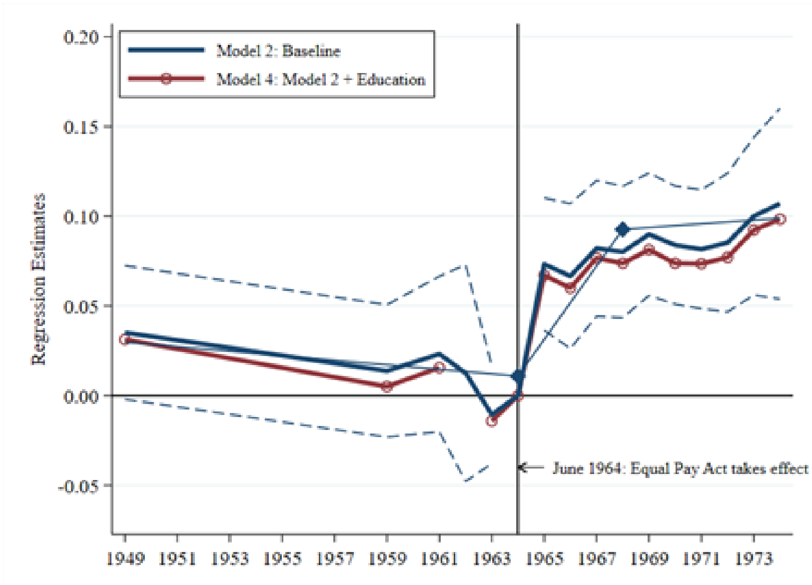
(a) Women



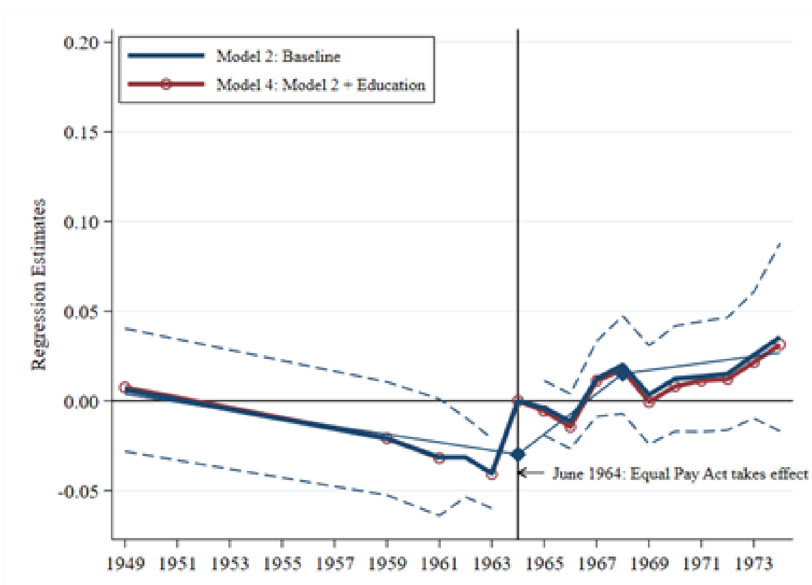
(b) Men

Figure plots the event-study coefficients from equation (2.3.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and an arbitrary correlation within state group. The thin lines correspond to spline estimates of equation (2.3.3) for the baseline specification. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, log hours worked, and fixed effects for industry-occupation-state-group, industry-year, and occupation-year. The estimates in red are based on a sample of individuals who worked at least 27 weeks in the previous year and at least 35 hours in the reference week. *Sources*: See Figure 2.3.

Figure B.5: The Effect of the Equal Pay Act and Title VII on Wages using Pre-Existing State Equal Pay Laws, Robustness to Controlling for Education



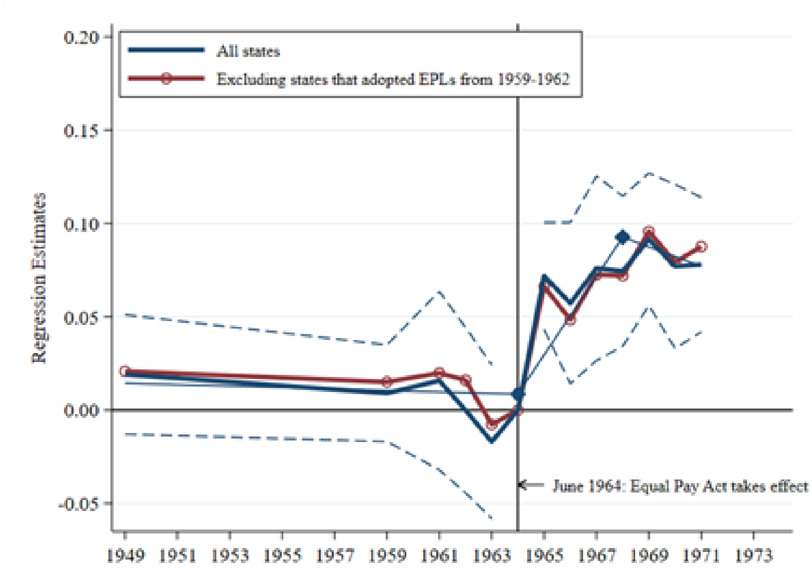
(a) Women



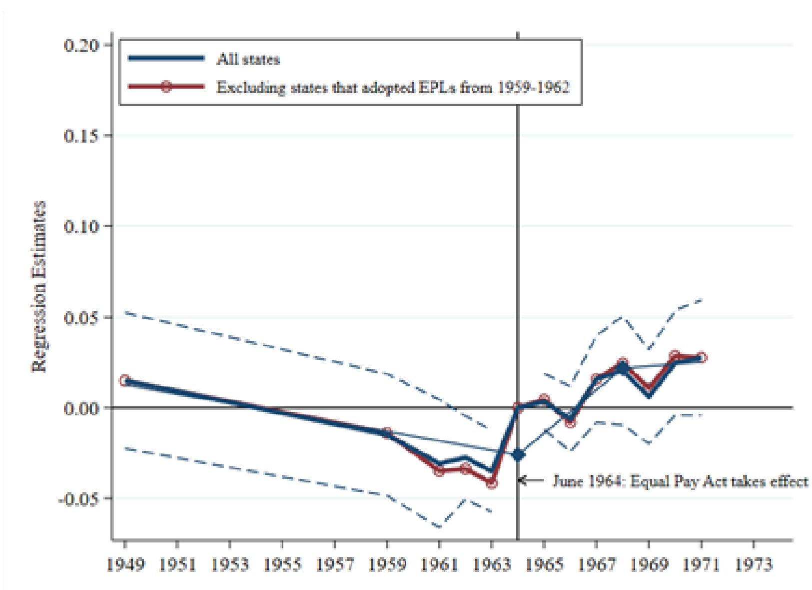
(b) Men

Figure plots the event-study coefficients from equation (2.3.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and an arbitrary correlation within state group. The thin lines correspond to spline estimates of equation (2.3.3) for the baseline specification. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, log hours worked, and fixed effects for industry-occupation-state-group, industry-year, and occupation-year. The estimates in red include years of education as a covariate. We omit earnings year 1962 from the regression because education is not available in that year. *Sources*: See Figure 2.3.

Figure B.6: The Effect of the Equal Pay Act and Title VII on Wages using Pre-Existing State Equal Pay Laws, Robustness to Dropping States that Enacted Equal Pay Laws from 1959-1962



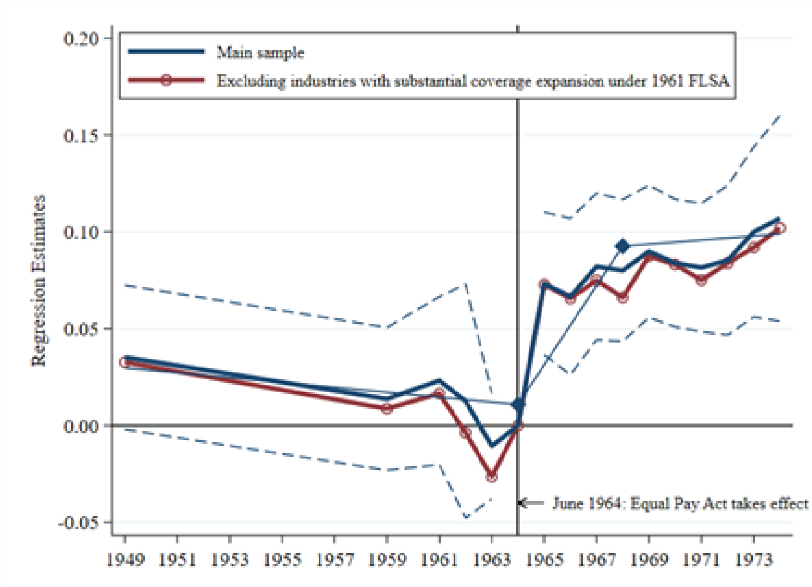
(a) Women



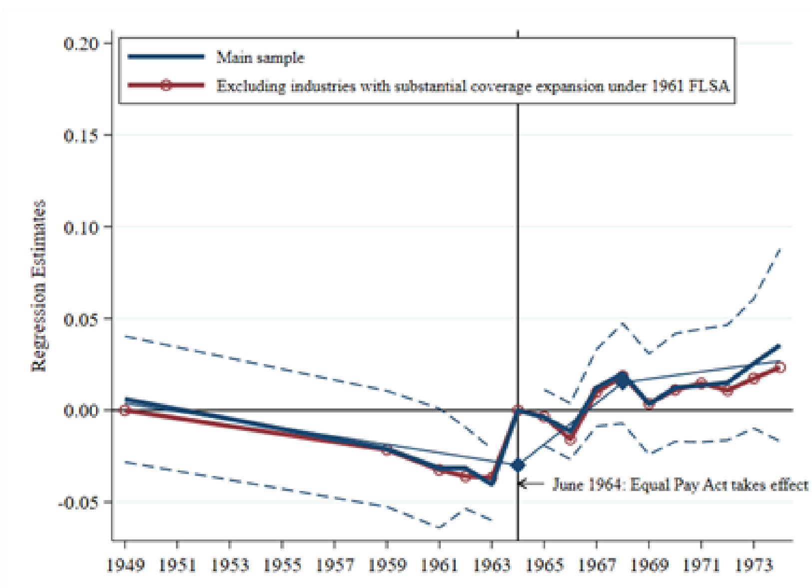
(b) Men

Figure plots the event-study coefficients from equation (2.3.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation within state-group. We present results for a sample of wage earners in all states, as well as an auxiliary sample that excludes individuals living in state groups that adopted or expanded equal pay laws between 1959 and 1962 (Arizona-Colorado-New Mexico, Alaska-Hawaii-Washington, Michigan-Wisconsin, Ohio, Idaho-Montana-Nevada-Utah-Wyoming). We end the analysis with the 1972 CPS to obtain more detailed state group definitions for this robustness check. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, and fixed effects for industry-occupation-state-group, industry-year, and occupation-year. See text for more details. *Sources*: See Figure 2.3

Figure B.7: The Effect of the Equal Pay Act and Title VII on Wages using Pre-Existing State Equal Pay Laws, Robustness to Excluding Industries Newly Covered under the 1961 FLSA Amendments



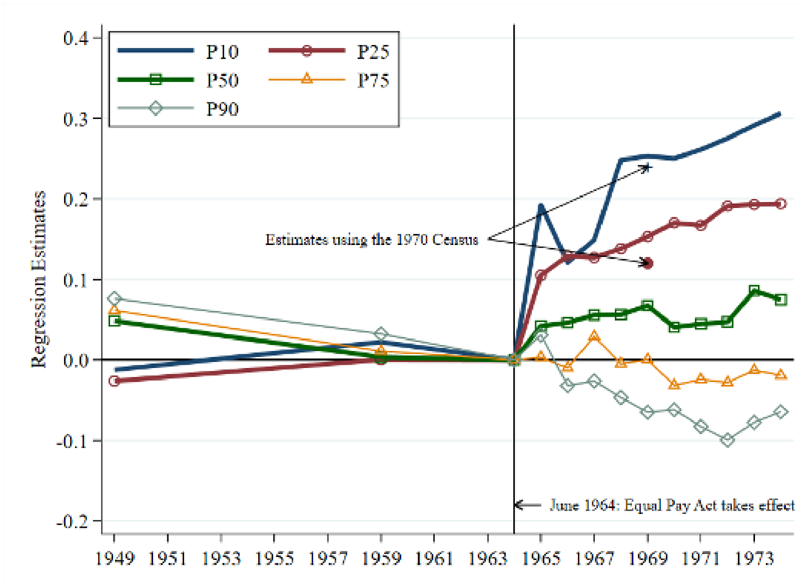
(a) Women



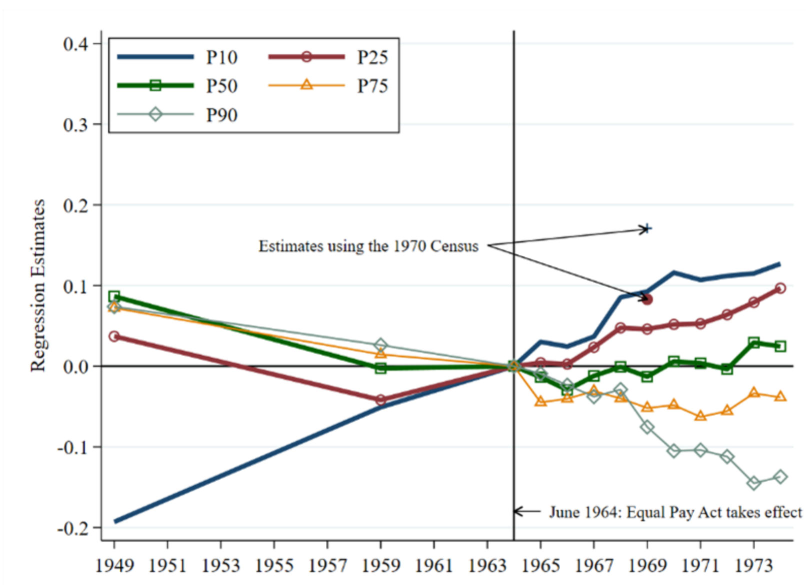
(b) Men

Figure plots the event-study coefficients from equation (2.3.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation within state-group. We present results for all industry-occupation-state-group cells in our main sample in blue and results when excluding industries where coverage of the minimum wage expanded under the 1961 FLSA amendments in red. These industries are retail trade and construction. The spline (equation (2.3.3)) is shown for the main sample. All regressions use the covariates from model 2. *Sources*: See Figure 2.3.

Figure B.8: The Effect of the Equal Pay Act and Title VII on the Distribution of Wages using Pre-Existing State Equal Pay Laws



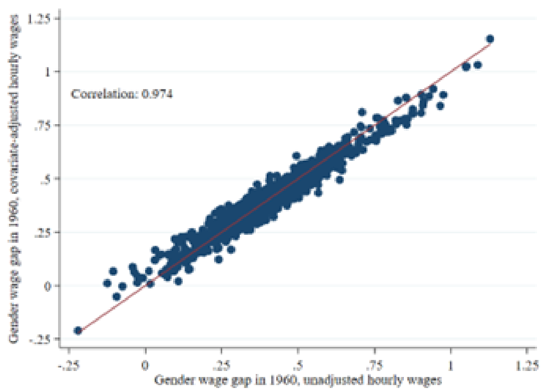
(a) Women's Weekly Wages



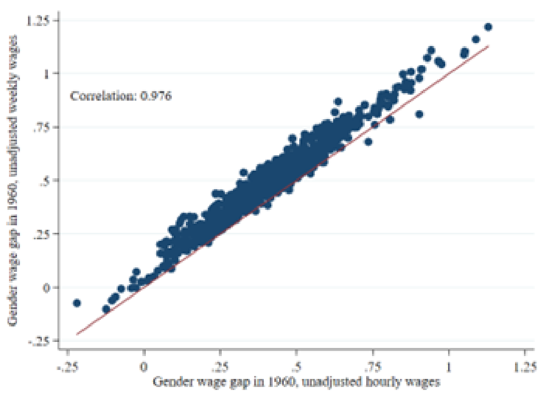
(b) Men's Weekly Wages

The figure plots estimates of model 2 of equation (2.3.1) where the dependent variable is the RIF for weekly log wages for women (figure B.8a) and men (figure B.8b). Because sample sizes are much smaller in the early ASEC years and because this is a demanding specification, we pool 1959 and 1962-1964 into a single event-study coefficient. Estimates for the 1970 Census are shown for the 10^{th} and 25^{th} percentiles, from a regression estimated using only the 1950, 1960, and 1970 Censuses. *Sources*: See Figure 2.3 notes and the combined one-percent Form 1 and Form 2 state samples of the 1970 Decennial Census.

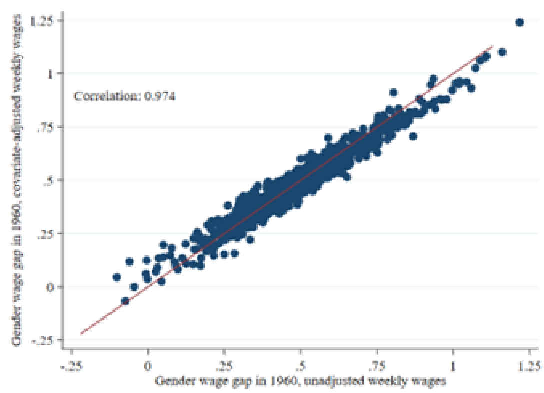
Figure B.9: Comparison of Different Measures of 1960 Gender Wage Gap



(a) Unadjusted Hourly Wage Gap to Demographic-Adjusted Hourly Wage Gap



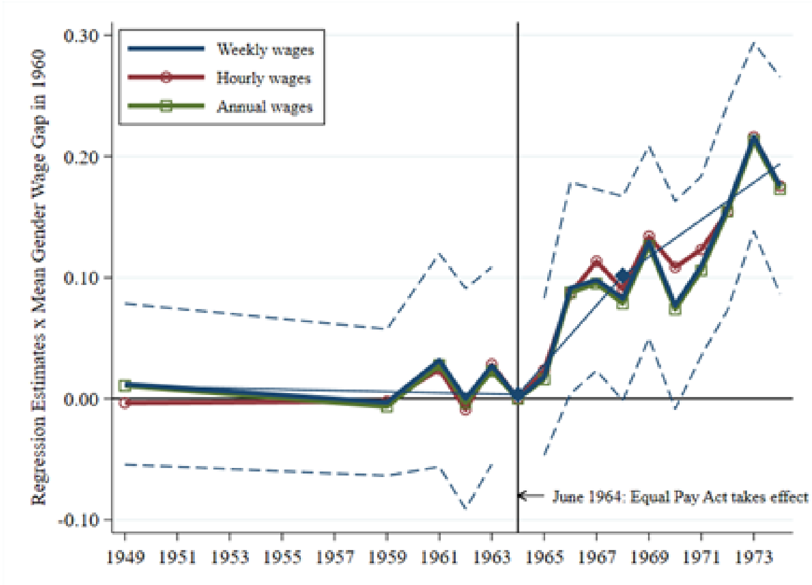
(b) Unadjusted Hourly Wage Gap to Unadjusted Weekly Wage Gap



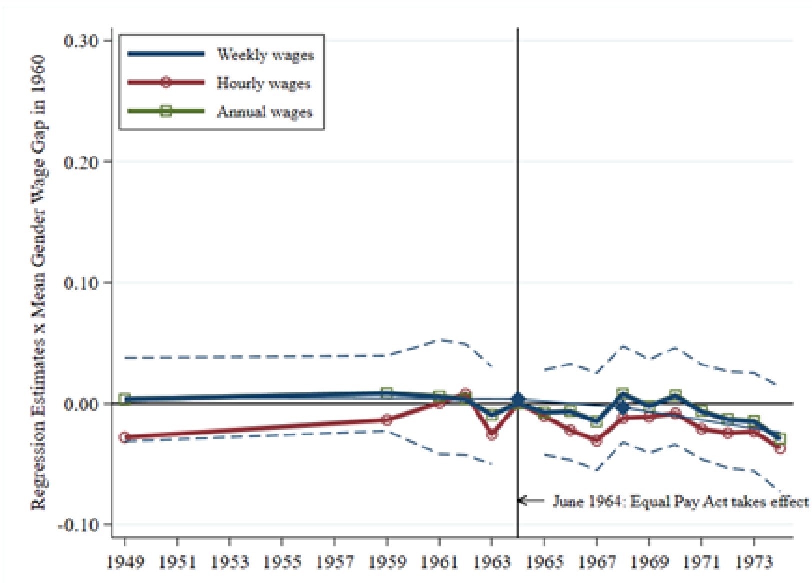
(c) Unadjusted Weekly Wage Gap to Demographic and Hours-Adjusted Weekly Wage Gap

Each point represents the gender wage gap in an industry-occupation-state-group cell. We construct the unadjusted hourly and weekly wage gaps as the difference between mean log wages of men and women as described in the text. The covariate-adjusted hourly wage gap is estimated from a regression that pools women and men and includes as covariates an indicator for nonwhite race, a quadratic in age, and indicators for educational attainment (with all covariates assumed to have the same coefficient for women and men). The covariate-adjusted weekly wage gap also includes the log of usual hours worked as a covariate in the regression. *Sources*: 5% sample of the 1960 Decennial Census (Ruggles et al., 2023).

Figure B.10: The Effect of the Equal Pay Act and Title VII on Wages using 1960 Gender Wage Gaps, Robustness to Using Hourly and Annual Wages



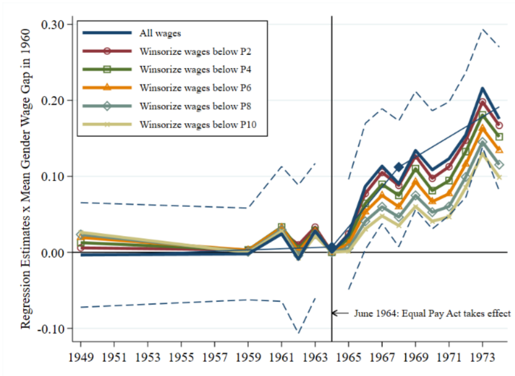
(a) Women



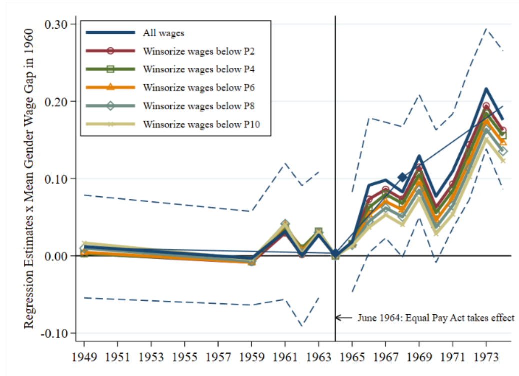
(b) Men

Figure plots the event-study coefficients from equation (2.5.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation within industry-occupation-state-group. The dependent variable is either the log weekly wage (our preferred approach), log hourly wage, or log annual wage. Log hourly wage is the log annual wage earnings less log weeks worked last year and log hours worked in the reference week. The spline (equation (2.5.2)) is shown for the log weekly wage. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, and fixed effects for industry-occupation-state-group, industry-year, occupation-year, and state-group-year. We include log hours worked as a covariate when the dependent variable is log weekly wages and log hours worked and log weeks worked when the dependent variable is log annual wages. Point estimates and confidence intervals are multiplied by the average gender wage gap in the 1960 Census for women (equal to 0.374). See description in Appendix B.1. *Sources*: See Figure 2.3.

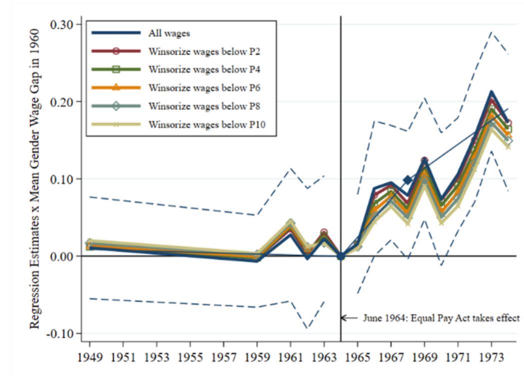
Figure B.11: The Effects of the Equal Pay Act and Title VII on Wages using Pre-Existing State Equal Pay Laws, Robustness to Winsorizing Low Wages



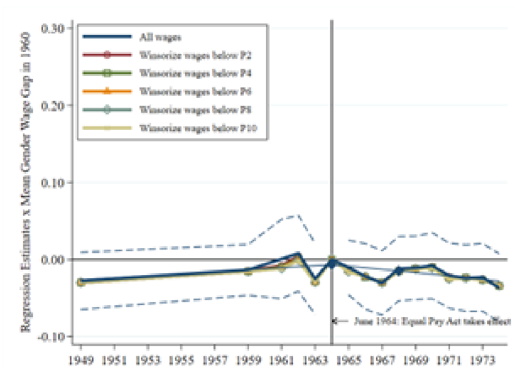
(a) Hourly Wages, Women



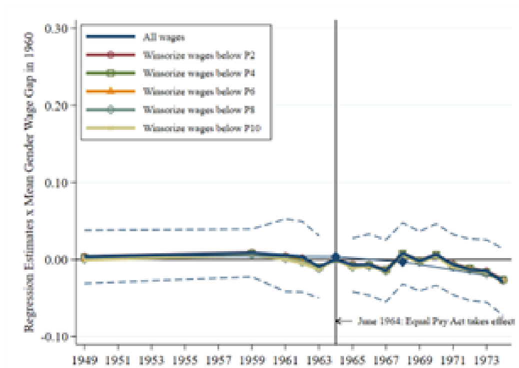
(b) Weekly Wages, Women



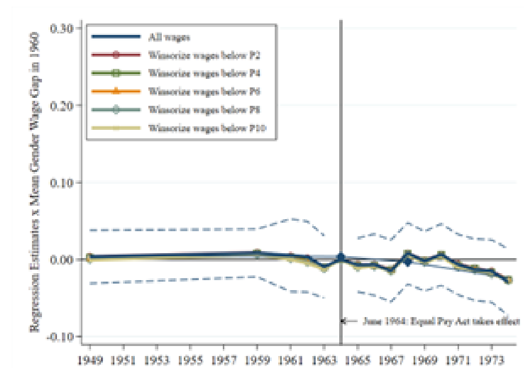
(c) Annual Wages, Women



(d) Hourly Wages, Men



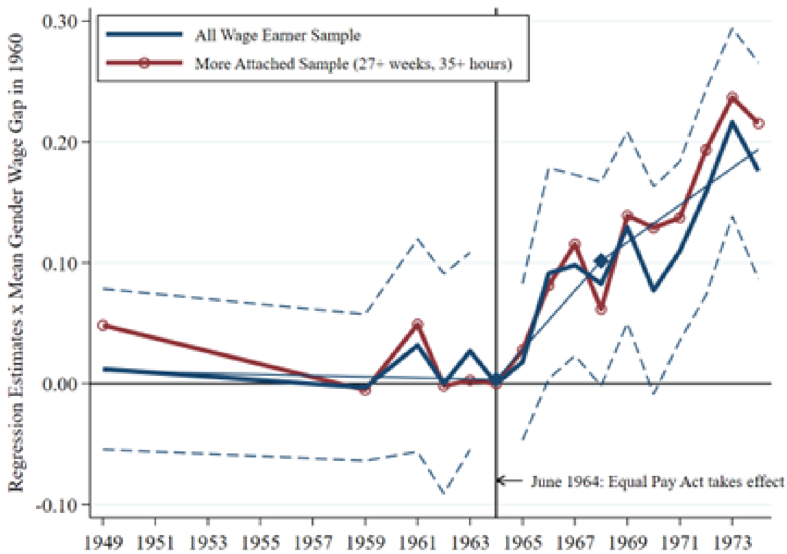
(e) Weekly Wages, Men



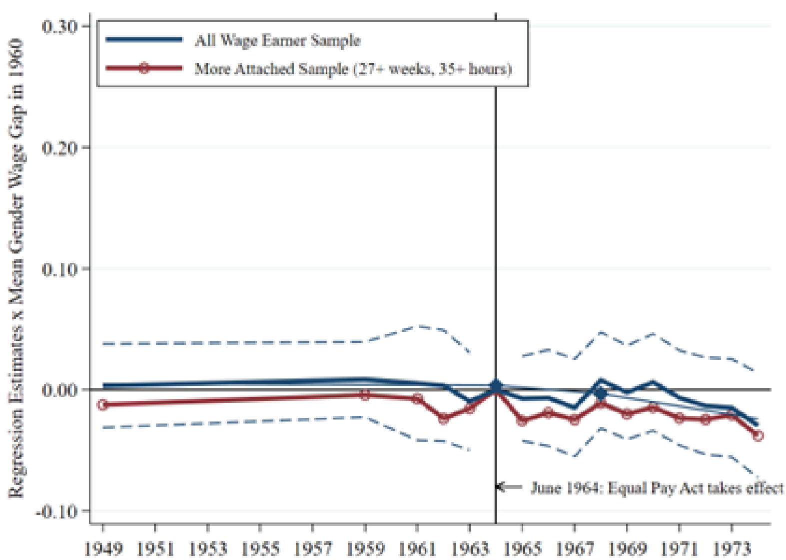
(f) Annual Wages, Men

Figure plots the event-study coefficients from equation (2.5.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation within industry-occupation-state-group. The dependent variables are the unwinsorized log hourly, weekly, or annual wage ("all wages") and their winsorized counterparts at the indicated percentile (see Appendix Table B.15 for the value in levels). The spline (equation (2.5.2)) is shown for the unwinsorized dependent variable. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, and fixed effects for industry-occupation-state-group, industry-year, occupation-year, and state-group-year. We include log hours worked as a covariate when the dependent variable is log weekly wages and log hours worked and log weeks worked when the dependent variable is log annual wages. Point estimates and confidence intervals are multiplied by the average gender wage gap in the 1960 Census for women (equal to 0.374). See description in Appendix B.2 for details on winsorizing and Appendix B.1 for details on dependent variables. *Source*: See Figure 2.3.

Figure B.12: The Effect of the Equal Pay Act and Title VII on Wages using 1960 Gender Wage Gaps, Robustness to Limiting Sample to More Attached Workers



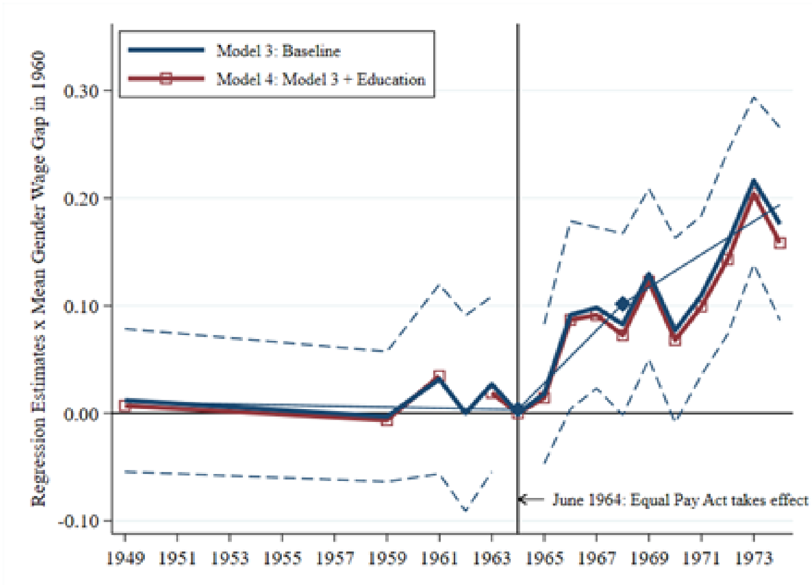
(a) Women



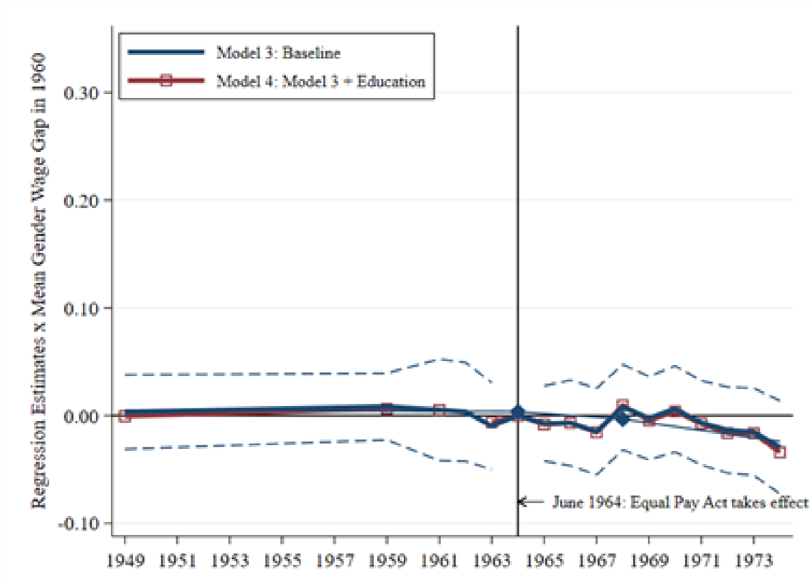
(b) Men

Figure plots the event-study coefficients from equation (2.5.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation by industry-occupation-state-group. The thin lines correspond to spline estimates of equation (2.5.2) for the baseline specification. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, log hours worked, and fixed effects for industry-occupation-state-group, industry-year, occupation-year, and state-group-year. The estimates in red are based on a sample of individuals who worked at least 27 weeks in the previous year and at least 35 hours in the reference week. Point estimates and confidence intervals are multiplied by the average gender wage gap in the 1960 Census for women (equal to 0.374). *Sources*: See Figure 3.

Figure B.13: The Effect of the Equal Pay Act and Title VII on Wages using 1960 Gender Wage Gaps, Robustness to Controlling for Education



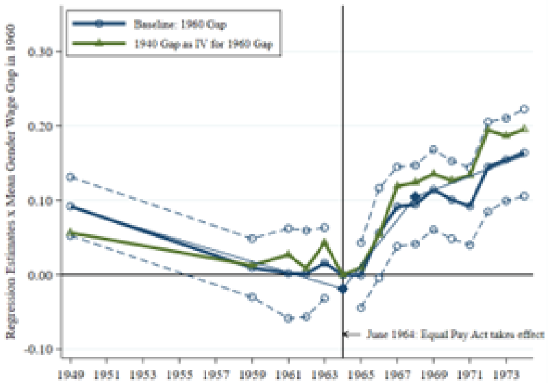
(a) Women



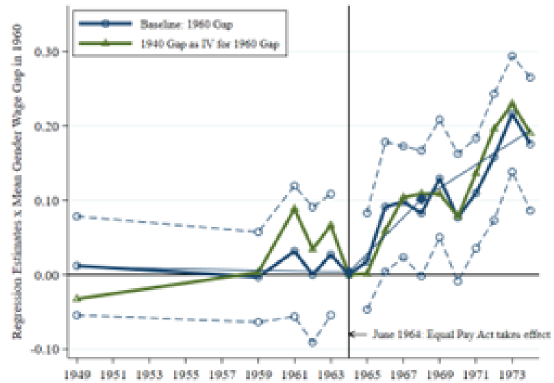
(b) Men

Figure plots the event-study coefficients from equation (2.5.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation by industry-occupation-state-group. The thin lines correspond to spline estimates of equation (2.5.2) for the baseline specification. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, log hours worked, and fixed effects for industry-occupation-state-group, industry-year, occupation-year, and state-group-year. The line in red adds years of education as a covariate. We omit earnings year 1962 from the regression because education is not available in that year. Point estimates and confidence intervals are multiplied by the average gender wage gap in the 1960 Census for women (equal to 0.374). *Sources*: See Figure 2.3.

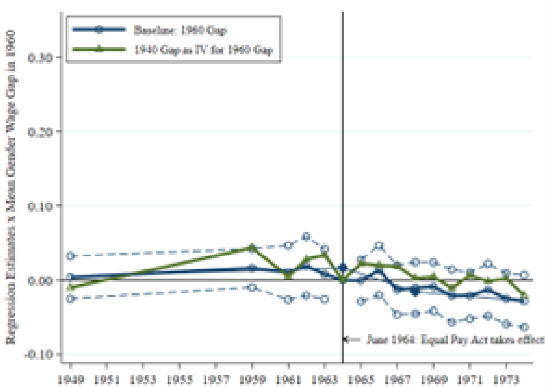
Figure B.14: The Effect of the Equal Pay Act and Title VII on Wages using 1960 Gender Wage Gaps, Robustness to Using the 1940 Gender Wage Gap as an Instrumental Variable



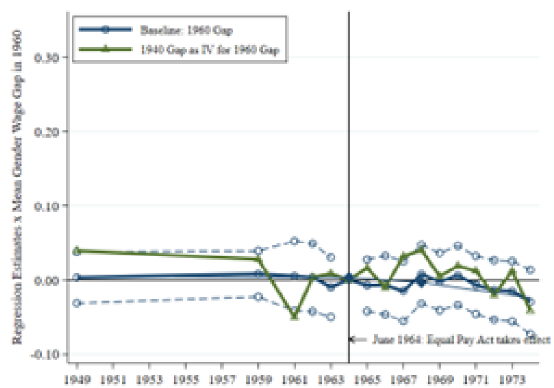
(a) Women, Using 1940 Gap as an IV, Specification 2



(b) Women, Using 1940 Gap as an IV, Specification 3



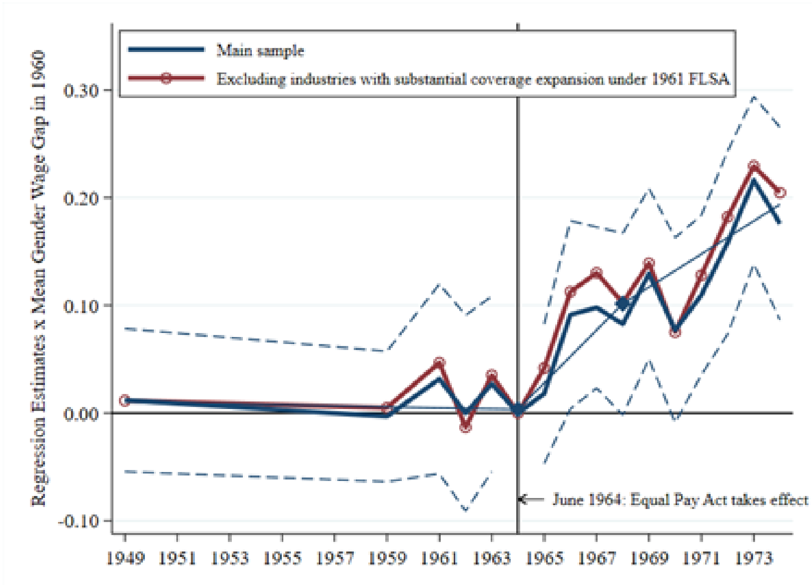
(c) Men, Using 1940 Gap as an IV, Specification 2



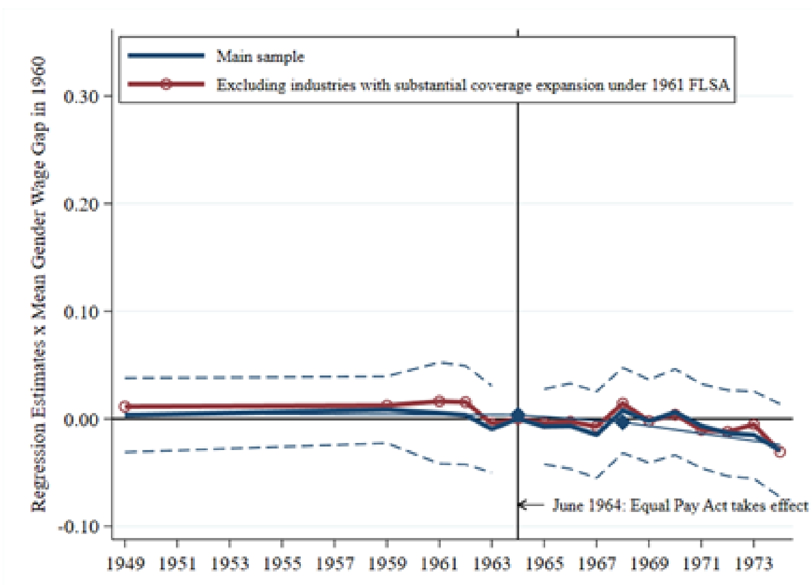
(d) Men, Using 1940 Gap as an IV, Specification 3

Figure plots the event-study coefficients from equation (2.4) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation within industry-occupation-state-group. The blue line displays our baseline results, which use the 1960 gender wage gap as the key explanatory variable. The green line displays results in which the 1940 gender wage gap is an instrumental variable for the 1960 gender wage gap. The spline (equation (2.5)) is shown for the baseline approach. Figures B.14a and B.14c use the covariates from model 2 of Figure 2.9a, and figures B.14b and B.14d use the covariates from model 3 of Figure 2.9a. Point estimates and confidence intervals are multiplied by the average gender wage gap in the 1960 Census for women (equal to 0.374). See text for more details. *Sources*: Full count of the 1940 Decennial Census, 1% sample of the 1950 Decennial Census, 5% sample of the 1960 Decennial Census and the 1965 CPS ASEC (Flood et al., 2022; Ruggles et al., 2023).

Figure B.15: The Effect of the Equal Pay Act and Title VII on Wages using 1960 Gender Wage Gaps, Robustness to Excluding Industries with Coverage Expansions under the 1961 FLSA Amendments



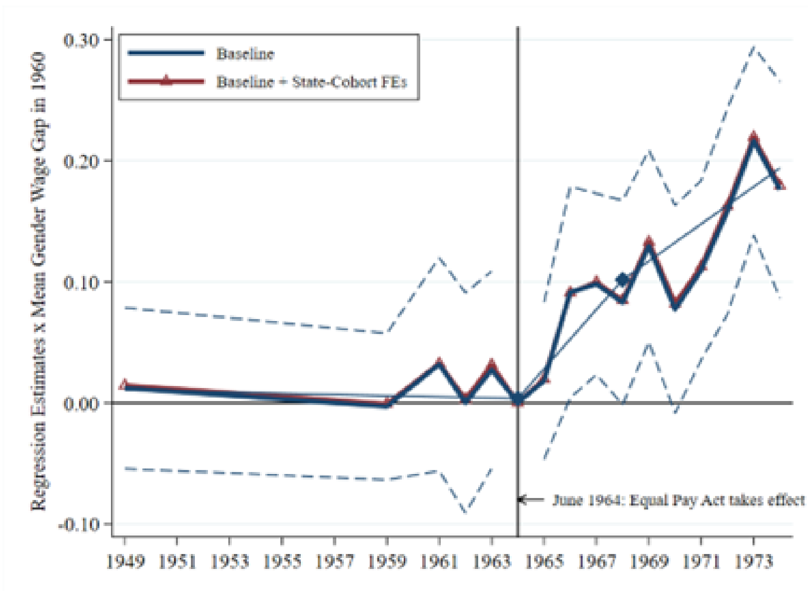
(a) Women



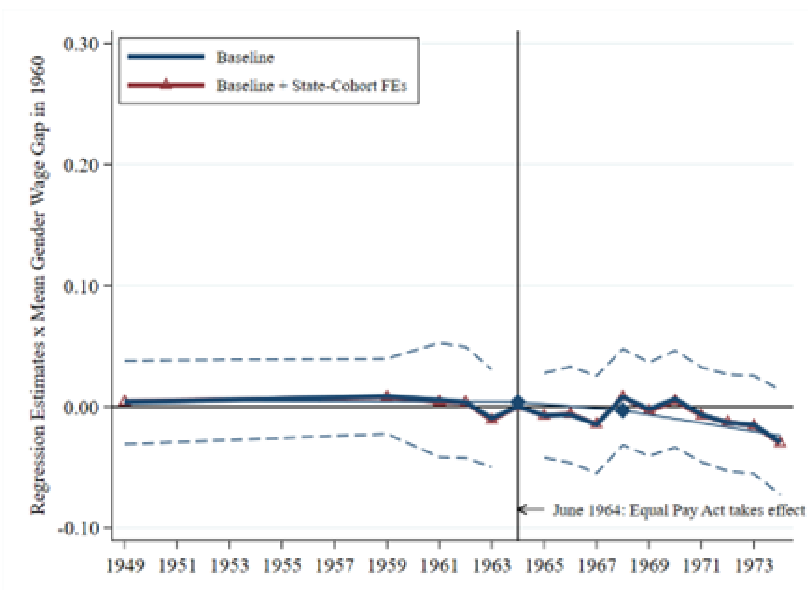
(b) Men

Figure plots the event-study coefficients from equation (2.5.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation within industry-occupation-state-group. We present results for all industry-occupation-state-group cells in our main sample in blue and results when excluding industries where coverage of the minimum wage expanded under the 1961 FLSA amendments in red with circle markers. These industries are retail trade and construction. The spline (equation (2.5.2)) is shown for the main sample. All regressions use the covariates from model 3. Point estimates and confidence intervals are multiplied by the average gender wage gap in the 1960 Census for women (equal to 0.374). *Sources*: See Figure 2.3.

Figure B.16: The Effect of the Equal Pay Act and Title VII on Wages using 1960 Gender Wage Gaps, Robustness to Adding State-by-Cohort Fixed Effects



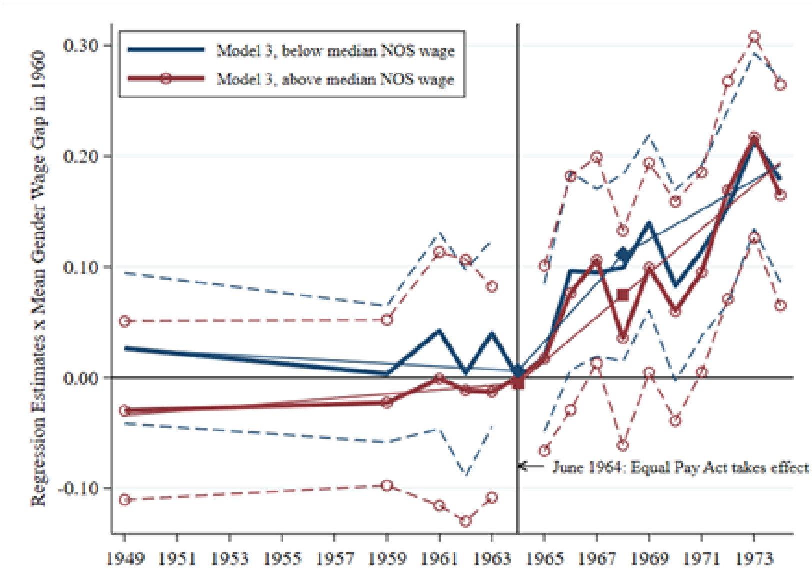
(a) Women



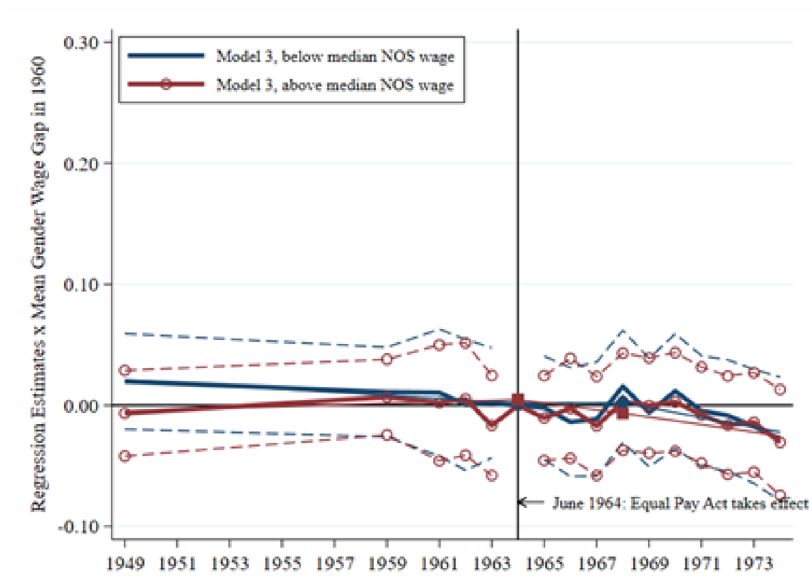
(b) Men

Figure plots the event-study coefficients from equation (2.5.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation by industry-occupation-state-group. The thin lines correspond to spline estimates of equation (2.5.2) for the baseline specification. All regressions include as covariates an indicator for nonwhite race, a quadratic function in age, log hours worked, and fixed effects for industry-occupation-state-group, industry-year, occupation-year, and state-group-year. The line in red adds fixed effects for state-by-birth-year as a covariate. Point estimates and confidence intervals are multiplied by the average gender wage gap in the 1960 Census for women (equal to 0.374). *Sources*: See Figure 2.3.

Figure B.17: The Effect of the Equal Pay Act and Title VII on Wages using 1960 Gender Wage Gaps, by 1960 Average Wage Level in Industry-Occupation-State-Group-Cell



(a) Women



(b) Men

Figure plots the event-study coefficients from equation (2.5.1) as well as 95-percent, pointwise confidence intervals using standard errors that have been corrected for heteroskedasticity and arbitrary correlation within industry-occupation-state-group. The thin lines correspond to spline estimates of equation (2.5.2). We allow the event-study coefficients to differ based on whether an industry-occupation-state-group cell has a 1960 average wage that is above or below the median cell-level average wage. Otherwise, this specification is the same as model 3, shown in Figure 2.9a. Point estimates and confidence intervals are multiplied by the average gender wage gap in the 1960 Census for women (equal to 0.374). *Sources*: See Figure 2.3.

APPENDIX C

Appendix to Chapter 3

C.1. Baseline Model

C.1.1 Set Up

C.1.2 Firms

Substituting that $V_l = 0$ (Free Entry Condition) and moving terms to the LHS of (3.2.2),

$$(r + \lambda + \sigma)J_l = p - w_l + \sigma X_l$$

We can also solve (3.2.3) for X:

$$X_l = \frac{1}{r + \gamma}\{-p\psi + \gamma J_l\}$$

Substituting this expression into the first equation gives us that

$$(r + \lambda + \sigma)J_l = p - w_l + \frac{\sigma}{r + \gamma}\{-p\psi + \gamma J_l\}$$

To solve this, first multiply through by $r + \gamma$:

$$(r + \gamma)(r + \lambda + \sigma)J_l = (r + \gamma)(p - w_l) + \sigma\{-p\psi + \gamma J_l\}$$

Subtracting $\sigma\gamma J_l$ and dividing by the term multiplying J_l on the LHS gives us an expression for the marginal benefit of a job with leave:

$$J_l = \frac{(r + \gamma)(p - w_l) - \sigma p\psi}{(r + \gamma)(r + \lambda + \sigma) - \sigma\gamma} = \frac{(r + \gamma)(p - w_l) - \sigma p\psi}{r(r + \lambda + \sigma) + \gamma(r + \lambda)} \tag{C.1}$$

C.1.3 When is Leave Offered?

C.1.3.1 Firms

Let V_{nl} denote the value to the firm of posting a vacancy for a job with no leave, and let J_{nl} denote the value of this job once it is filled. Labor productivity is constant across workers and given by p in output units; while employed, the firm pays the workers wage w_{nl} in output units.

Jobs can be destroyed for two reasons. First, I assume that jobs are exogenously destroyed at Poisson rate λ due to product demand shocks. Additionally, workers have children at rate σ—since this job has no leave protection, the job is destroyed upon childbirth and the firm must search again for a worker. Now, we can write the value of a filled job as

$$rJ_{nl} = p - w_{nl} + (\lambda + \sigma)(V_{nl} - J_{nl})$$

Utilizing the free entry assumption that $V_{nl} = 0$, we can rearrange this expression to generate a zero profit condition:

$$p - w_{nl} - (r + \lambda + \sigma)J_{nl} = 0$$

While a vacancy is open, the firm incurs a flow cost that is proportional to worker productivity, given by pc. The firm receives a worker match at rate $q(\theta_{nl})$. The value of a vacancy is given by

$$rV_{nl} = -pc + q(\theta_{nl})(J_{nl} - V_{nl})$$

Again utilizing a free entry assumption, we obtain that $J_{nl} = pc/q(\theta_{nl})$. We can substitute this into the zero profit condition to derive an expression independent of J_{nl}:

$$p - \left(w_{nl} + (r + \lambda + \sigma)\frac{pc}{q(\theta_{nl})} \right) = 0$$

This optimality condition is used in equation (3.2.5) in the text.

C.1.3.2 Re-arranging the job creation condition

Starting with (3.2.4), we can break the fraction on the LHS into two parts:

$$\frac{(r + \gamma)(p - w_l)}{r(r + \lambda + \sigma) + \gamma(r + \lambda)} - \frac{\sigma p \psi}{r(r + \lambda + \sigma) + \gamma(r + \lambda)} = \frac{pc}{q(\theta_l)}$$

Multiplying both sides by $\frac{r(r+\lambda+\sigma)+\gamma(r+\lambda)}{(r+\lambda+\sigma)(r+\gamma)}$ gives us that

$$\frac{p-w_l}{r+\lambda+\sigma} - \frac{\sigma p\psi}{(r+\lambda+\sigma)(r+\gamma)} = \frac{pc}{q(\theta_l)}\frac{r(r+\lambda+\sigma)+\gamma(r+\lambda)}{(r+\lambda+\sigma)(r+\gamma)}$$

We can rewrite the RHS fraction multiplying $pc/q(\theta_l)$:

$$\begin{aligned}\frac{p-w_l}{r+\lambda+\sigma} - \frac{\sigma p\psi}{(r+\lambda+\sigma)(r+\gamma)} &= \frac{pc}{q(\theta_l)}\frac{(r+\lambda+\sigma)(r+\gamma)-\gamma\sigma}{(r+\lambda+\sigma)(r+\gamma)} \\ &= \frac{pc}{q(\theta_l)} - \frac{pc}{q(\theta_l)}\frac{\gamma\sigma}{(r+\lambda+\sigma)(r+\gamma)}\end{aligned}$$

Rearranging and collecting terms yields

$$\frac{p-w_l}{r+\lambda+\sigma} + \frac{\sigma\gamma}{(r+\lambda+\sigma)(r+\gamma)}\left\{\frac{pc}{q(\theta_l)} - \frac{p\psi}{\gamma}\right\} = \frac{pc}{q(\theta_l)}$$

Finally, we can multiply through by $r+\lambda+\sigma$ to obtain

$$p - w_l - (r+\lambda+\sigma)\frac{pc}{q(\theta_l)} + \frac{\sigma\gamma}{r+\gamma}\left\{\frac{pc}{q(\theta_l)} - \frac{p\psi}{\gamma}\right\} = 0$$

C.1.3.3 Adding Worker Exit from Job

The model remains the same as before except the equation giving the value of a worker on leave (3.2.3) is now given by

$$rX_l = -p\psi + \gamma[\pi(\gamma)(J_l - X_l) + (1-\pi(\gamma))(V_l - X_l)]$$

Utilizing the free entry assumption that $V_{nl} = 0$, we can solve for X_l:

$$X_l = \frac{1}{r+\gamma}\{-p\psi + \gamma\pi(\gamma)J_l\}$$

Substituting this into (3.2.2) and moving some terms to the LHS,

$$(r+\lambda+\sigma)J_l = p - w_l + \frac{\sigma}{r+\gamma}\{-p\psi + \gamma\pi(\gamma)J_l\}$$

As before, substituting the free entry condition into (3.2.1) gives us that $J_l = pc/q(\theta_l)$. We can substitute this into our expression and rearrange a bit to arrive at the expression in the text:

$$p - w_l - (r+\lambda+\sigma)\frac{pc}{q(\theta_l)} + \frac{\sigma\gamma}{r+\gamma}\left\{\pi(\gamma)\frac{pc}{q(\theta_l)} - \frac{p\psi}{\gamma}\right\} = 0$$

C.1.4 Workers

C.1.4.1 Deriving the Wage Equation

I assume that the worker and firm engage in Nash bargaining over the surplus of a job match, where worker bargiaining power is given by $\beta \in (0,1)$. In particular, w_l is the solution to

$$\text{argmax}_{w_l}(E_l^i - U_l)^\beta (J_l^j - V_l)^{1-\beta} \tag{C.2}$$

for worker i and firm j.[1] Taking the log of the expression in (C.2) yields an equivalent maximization problem:

$$\text{argmax}_{w_l}\beta \ln\Big(E_l^i - U_l\Big) + (1-\beta)\ln\Big(J_l^j - V_l\Big)$$

We can solve directly for the terms that depend on the bargained wage. Solving (3.2.8) gives us that

$$E_l^i = \frac{1}{r+\lambda+\sigma}[w_l + \lambda U_l + \sigma L_l^i]$$

We need to also substitute for L_l^i as the value of being on leave depends on the wage negotiated upon return. From (3.2.9), we know that $L_l^i = (r+\gamma)^{-1}[z + \gamma E_l^i]$, which we can substitute into the above equation:

$$E_l^i = \frac{1}{r+\lambda+\sigma}[w_l + \lambda U_l + \frac{\sigma}{r+\gamma}(z + \gamma E_l^i)]$$

Solving this for E_l^i gives our final expression for workers:

$$E_l^i = \frac{r+\gamma}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}[w_l + \lambda U_l + \frac{\sigma z}{r+\gamma}]$$

Turning to firms, solving (3.2.2) yields

$$J_l^j = \frac{1}{r+\lambda+\sigma}[p - w_l + \lambda V_l + \sigma X_l^j]$$

From (3.2.3) we know that $X_l^j = (r+\gamma)^{-1}[-p\psi + \gamma J_l^j]$. Substituting this into our equation yields

$$J_l^j = \frac{1}{r+\lambda+\sigma}[p - w_l + \lambda V_l + \frac{\sigma}{r+\gamma}[-p\psi + \gamma J_l^j]]$$

[1] Superscripts are used here to distinguish that the value of employment is specific to the wage bargained here, whereas the value of unemployment & vacancies are at their equilibrium value.

Solving this for J_l^j gives our final expression for firms:

$$J_l^j = \frac{r+\gamma}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}[p - w_l + \lambda V_l - \frac{p\psi\sigma}{r+\gamma}]$$

Differentiating the objective function with respect to w_l, we arrive at the first order condition:

$$\beta\frac{\frac{r+\gamma}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}}{E_l^i - U_l} - (1-\beta)\frac{\frac{r+\gamma}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}}{J_l^j - V_l} = 0$$

A couple of steps of algebra gives us the following:

$$\beta(J_l^j - V_l) = (1-\beta)(E_l^i - U_l) \tag{C.3}$$

Imposing the free entry condition simplifies this expression slightly:

$$\beta J_l^j = (1-\beta)(E_l^i - U_l)$$

We've already solved for J_l^j in the text, so now we derive an expression for $E_l^i - U_l$. Taking the difference between (3.2.8) and (3.2.9) as well as the difference between (3.2.8) and (3.2.7) gives a system of two equations with two unknown differences:

$$\begin{aligned} rE_l^i - rL_l^i &= (w_l - z) - \lambda(E_l^i - U_l) - (\sigma+\gamma)(E_l^i - L_l^i) \\ rE_l^i - rU_l &= (w_l - b) - (\lambda + \theta_l q(\theta_l))(E_l^i - U_l) - \sigma(E_l^i - L_l^i) \end{aligned}$$

Pulling like terms to the LHS simplifies this to:

$$\begin{aligned} (r+\sigma+\gamma)(E_l^i - L_l^i) &= (w_l - z) - \lambda(E_l^i - U_l) \\ (r+\lambda+\theta_l q(\theta_l))(E_l^i - U_l) &= (w_l - b) - \sigma(E_l^i - L_l^i) \end{aligned}$$

We can solve the first equation for $E_l^i - L_l^i$:

$$E_l^i - L_l^i = \frac{(w_l - z) - \lambda(E_l^i - U_l)}{r+\sigma+\gamma}$$

And then substitute this expression into the second equation to obtain:

$$(r+\lambda+\theta_l q(\theta_l))(E_l^i - U_l) = (w_l - b) - \sigma\frac{(w_l - z) - \lambda(E_l^i - U_l)}{r+\sigma+\gamma}$$

Then, multiplying through by $r+\sigma+\gamma$,

$$(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))(E_l^i-U_l) = (r+\sigma+\gamma)(w_l-b) - \sigma(w_l-z) + \sigma\lambda(E_l^i-U_l)$$

$$\{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda\}(E_l^i-U_l) = (r+\sigma+\gamma)(w_l-b) - \sigma(w_l-z)$$

We can now obtain a steady state expression for $E-U$:

$$E_l^i - U_l = \frac{(r+\sigma+\gamma)(w_l-b) - \sigma(w_l-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda} \tag{C.4}$$

Before solving for wages, I re-arrange the numerator and denominator for computational convenience later:

$$\begin{aligned}
E_l^i - U_l &= \frac{(r+\sigma+\gamma)w_l - (r+\sigma+\gamma)b - \sigma(w_l-z)}{r(r+\sigma+\gamma) + \lambda(r+\sigma+\gamma) + \theta_l q(\theta_l)(r+\sigma+\gamma) - \sigma\lambda} \\
&= \frac{(r+\gamma)w_l - (r+\sigma+\gamma)b + \sigma z}{r(r+\sigma+\gamma) + \lambda(r+\gamma) + \theta_l q(\theta_l)(r+\sigma+\gamma)} \\
&= \frac{(r+\gamma)w_l - (r+\sigma+\gamma)b + \sigma z}{r(r+\sigma) + r\gamma + \lambda r + \lambda\gamma + \theta_l q(\theta_l)(r+\sigma+\gamma)} \\
&= \frac{(r+\gamma)w_l - (r+\sigma+\gamma)b + \sigma z}{r(r+\sigma+\lambda) + \gamma(r+\lambda) + \theta_l q(\theta_l)(r+\sigma+\gamma)}
\end{aligned}$$

From (C.1) we know that

$$J_l^j = \frac{(r+\gamma)(p-w_l) - \sigma p\psi}{r(r+\lambda+\sigma) + \gamma(r+\lambda)}$$

Substituting these expressions into the wage bargaining solution yields

$$(1-\beta)\frac{(r+\gamma)w_l - (r+\sigma+\gamma)b + \sigma z}{r(r+\sigma+\lambda) + \gamma(r+\lambda) + \theta_l q(\theta_l)(r+\sigma+\gamma)} = \beta\frac{(r+\gamma)(p-w_l) - \sigma p\psi}{r(r+\lambda+\sigma) + \gamma(r+\lambda)}$$

Multiplying through by the LHS denominator gives

$$\begin{aligned}
(1-\beta)\{(r+\gamma)w_l - (r+\sigma+\gamma)b + \sigma z\} &= \beta\frac{r(r+\sigma+\lambda) + \gamma(r+\lambda)}{r(r+\lambda+\sigma) + \gamma(r+\lambda)}\{(r+\gamma)(p-w_l) - \sigma p\psi\} \\
&\quad + \beta\frac{\theta_l q(\theta_l)(r+\sigma+\gamma)}{r(r+\lambda+\sigma) + \gamma(r+\lambda)}\{(r+\gamma)(p-w_l) - \sigma p\psi\} \\
&= \beta\{(r+\gamma)(p-w_l) - \sigma p\psi\} \\
&\quad + \beta\frac{\theta_l q(\theta_l)(r+\sigma+\gamma)}{r(r+\lambda+\sigma) + \gamma(r+\lambda)}\{(r+\gamma)(p-w_l) - \sigma p\psi\}
\end{aligned}$$

Now, we divide both sides by $(r+\sigma+\gamma)$

$$(1-\beta)\left\{\frac{(r+\gamma)w_l}{r+\sigma+\gamma}-b+\frac{\sigma}{r+\sigma+\gamma}z\right\}=\beta\left\{\frac{(r+\gamma)(p-w_l)}{r+\sigma+\gamma}-\frac{\sigma}{r+\sigma+\gamma}p\psi\right\}$$
$$+\beta\theta_l q(\theta_l)\underbrace{\frac{(r+\gamma)(p-w_l)-\sigma p\psi}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}}_{J_l^j}$$

Noticing that the underlined term here is J_l^j, we can utilize the firm optimality condition $(J_l^j = pc/q(\theta_l))$:

$$(1-\beta)\left\{\frac{(r+\gamma)w_l}{r+\sigma+\gamma}-b+\frac{\sigma}{r+\sigma+\gamma}z\right\}=\beta\left\{\frac{(r+\gamma)(p-w_l)}{r+\sigma+\gamma}-\frac{\sigma}{r+\sigma+\gamma}p\psi\right\}+\beta\theta_l pc$$

Now, we multiply through by $(r+\sigma+\gamma)/(r+\gamma)$:

$$(1-\beta)\left\{w_l-b\frac{r+\sigma+\gamma}{r+\gamma}+\frac{\sigma}{r+\gamma}z\right\}=\beta\left\{(p-w_l)-\frac{\sigma}{r+\gamma}p\psi\right\}+\beta\theta_l pc\frac{r+\sigma+\gamma}{r+\gamma}$$

Isolating wages on the LHS and simplifying,

$$w_l=(1-\beta)\left\{b\left(1+\frac{\sigma}{r+\gamma}\right)-\frac{\sigma}{r+\gamma}z\right\}+\beta\left\{p-\frac{\sigma}{r+\gamma}p\psi\right\}+\beta\theta_l pc\left(1+\frac{\sigma}{r+\gamma}\right)$$

Finally, collecting like terms gives our expression for wages:

$$w_l=(1-\beta)b+\beta p+\beta pc\theta_l+\frac{\sigma}{r+\gamma}\left\{\beta pc\theta_l-[(1-\beta)(z-b)+\beta p\psi]\right\}$$

C.1.4.2 Reconciling Wage Results

This expression for w_l is different than that reached in Del Rey et al. (2017b). I attempt to reconcile this difference here. Start with the expression for $E_l^i - U_l$ in (C.4),

$$E_l^i-U_l=\frac{(r+\sigma+\gamma)(w_l-b)-\sigma(w_l-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}$$

Re-arranging the numerator and denominator,

$$E_l^i-U_l=\frac{(r+\gamma)(w_l-b)+\sigma(z-b)}{(r+\sigma+\gamma)(r+\theta_l q(\theta_l))+(r+\gamma)\lambda}$$

Substituting this expression, along with the expression above for J_l^j in (C.1), into the wage bargaining solution (C.3), gives

$$(1-\beta)\frac{(r+\gamma)(w_l-b)+\sigma(z-b)}{(r+\sigma+\gamma)(r+\theta_l q(\theta_l))+(r+\gamma)\lambda}=\beta\frac{(r+\gamma)(p-w_l)-\sigma p\psi}{r(r+\lambda+\sigma)+\gamma(r+\lambda)} \tag{C.5}$$

Besides for some slight differences in notation, this equation is identical to equation A.5 in Del Rey et al. (2017a). I replicate their subsequent wage equation from this equality with one algebra error, which I note below. To start, multiply through by the LHS denominator:

$$\begin{aligned}
&(1-\beta)\{(r+\gamma)(w_l-b)+\sigma(z-b)\}\\
&\qquad=\beta\{(r+\gamma)(p-w_l)-\sigma p\psi\}\frac{(r+\sigma+\gamma)(r+\theta_l q(\theta_l))+(r+\gamma)\lambda}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}\\
&\qquad=\beta\{(r+\gamma)(p-w_l)-\sigma p\psi\}\frac{(r+\sigma+\gamma)\theta_l q(\theta_l)+\sigma r+(r+\gamma)r+(r+\gamma)\lambda}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}\\
&\qquad=\beta\{(r+\gamma)(p-w_l)-\sigma p\psi\}\left\{\frac{(r+\sigma+\gamma)\theta_l q(\theta_l)+\sigma r}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}+\frac{(r+\gamma)(r+\lambda)}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}\right\}
\end{aligned}$$

Here, the second and third equalities are simply re-arranging the RHS. Now, divide both sides by $r+\gamma$ to obtain

$$\begin{aligned}
&(1-\beta)\{(w_l-b)+\frac{\sigma}{r+\gamma}(z-b)\}\\
&\qquad=\beta\{(p-w_l)-\frac{\sigma}{r+\gamma}p\psi\}\left\{\frac{(r+\sigma+\gamma)\theta_l q(\theta_l)+\sigma r}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}+\underbrace{\frac{(r+\gamma)(r+\lambda)}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}}_{\text{Assume }=1}\right\}
\end{aligned}$$

Going forward, I assume that the term in brackets is equal to 1 - notice that for $\sigma>0$ this is not a true statement. Substituting this into the expression, we obtain

$$\begin{aligned}
(1-\beta)\{(w_l-b)+\frac{\sigma}{r+\gamma}(z-b)\}&=\beta\{(p-w_l)-\frac{\sigma}{r+\gamma}p\psi\}\left\{\frac{(r+\sigma+\gamma)\theta_l q(\theta_l)+\sigma r}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}+1\right\}\\
&=\beta\{(p-w_l)-\frac{\sigma}{r+\gamma}p\psi\}+\beta\{(p-w_l)-\frac{\sigma}{r+\gamma}p\psi\}\frac{(r+\sigma+\gamma)\theta_l q(\theta_l)+\sigma r}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}\\
&=\beta\{(p-w_l)-\frac{\sigma}{r+\gamma}p\psi\}+\frac{\beta}{r+\gamma}\underbrace{\frac{(r+\gamma)(p-w_l)-\sigma p\psi}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}}_{J_l^j}\{(r+\sigma+\gamma)\theta_l q(\theta_l)+\sigma r\}
\end{aligned}$$

Here, the second equality splits the RHS into two terms, and the third equality re-arranges the second term. Following this, we can substitute the bracketed expression using the firm optimality condition $J_l^j = pc/q(\theta_l)$. This gives us

$$(1-\beta)\{(w_l - b) + \frac{\sigma}{r+\gamma}(z-b)\} = \beta\{(p-w_l) - \frac{\sigma}{r+\gamma}p\psi\} + \frac{\beta}{r+\gamma}\frac{pc}{q(\theta_l)}\{(r+\sigma+\gamma)\theta q(\theta_l) + \sigma r\}$$

Isolating wages on the LHS,

$$w_l = (1-\beta)\{b - \frac{\sigma}{r+\gamma}(z-b)\} + \beta\{p - \frac{\sigma}{r+\gamma}p\psi\} + \beta\frac{pc}{q(\theta_l)}\frac{(r+\sigma+\gamma)\theta_l q(\theta_l) + \sigma r}{r+\gamma}$$

Finally, collecting similar terms yields

$$\begin{aligned} w_l &= (1-\beta)b + \beta p + \frac{\sigma}{r+\gamma}\{-(1-\beta)(z-b) - \beta p\psi\} + \beta\frac{pc}{q(\theta_l)}\frac{(r+\gamma)\theta_l q(\theta_l) + (r+\theta_l q(\theta_l))\sigma}{r+\gamma} \\ &= (1-\beta)b + \beta p + \beta pc\theta_l + \frac{\sigma}{r+\gamma}\left\{\beta\left(\underbrace{\frac{pc(r+\theta_l q(\theta_l))}{q(\theta_l)}}_{B} - p\psi\right) - (1-\beta)(z-b)\right\} \end{aligned}$$

Which is identical (up to notation) to the wage equation (15) in Del Rey et al. (2017b). The main difference between our expressions is the bracketed B term. In the wage equation I derive above, $B = pc\theta_l$

C.2. Directed Search

C.2.1 Extending the Baseline Model

C.2.1.1 Solving for wage as a function of market tightness

For completeness, I reproduce all firm value functions here:

$$rV_l = -pc + q(\theta_l)(J_l - V_l) \tag{3.2.1}$$

$$rJ_l = p - w_l + \lambda(V_l - J_l) + \sigma(X_l - J_l) \tag{3.2.2}$$

$$rX_l = -p\psi + \gamma(J_l - X_l) \tag{3.2.3}$$

Utilizing (3.2.3), we can solve for the value of leave as a function of J_l:

$$X_l = \frac{1}{r+\gamma}\{-p\psi + \gamma J_l\}$$

Now, we can move terms to the left side of (3.2.2):

$$(r+\lambda+\sigma)J_l = p - w_l + \lambda V_l + \sigma X_l$$

And, utilizing our work above and the free entry assumption $V_l = 0$, we can write:

$$(r+\lambda+\sigma)J_l = p - w_l + \frac{\sigma}{r+\gamma}\{-p\psi + \gamma J_l\}$$

Solving for w_l and substituting $J_l = pc/q(\theta_l)$ gives us a firm zero profit condition for the wage:

$$w_l = p - (r+\lambda+\sigma)\frac{pc}{q(\theta_l)} + \frac{\sigma\gamma}{r+\gamma}\frac{pc}{q(\theta_l)} - \frac{\sigma p\psi}{r+\gamma}$$

C.2.1.2 Solving for equilibrium submarket

We can substitute the constraint into the objective function to obtain a simpler optimization problem, where $w_l(\theta_l)$ gives the constraint:

$$\max_{\theta_l} \quad b + \theta_l q(\theta_l)\left[\frac{(r+\sigma+\gamma)(w_l(\theta_l)-b) - \sigma(w_l(\theta_l)-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda}\right]$$

The first order condition for this problem is given by:

$$\begin{aligned}
&[q(\theta_l)+\theta_l q'(\theta_l)]\left[\frac{(r+\sigma+\gamma)(w_l(\theta_l)-b) - \sigma(w_l(\theta_l)-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda}\right]+\\
&\theta_l q(\theta_l)\Bigg[\frac{\{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda\}\{(r+\gamma)w_l'(\theta_l)\}}{\{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda\}^2}-\\
&\frac{\{(r+\sigma+\gamma)(w_l(\theta_l)-b) - \sigma(w_l(\theta_l)-z)\}\{(r+\sigma+\gamma)[q(\theta_l)+\theta_l q'(\theta_l)]\}}{\{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda\}^2}\Bigg] = 0
\end{aligned}$$

Cancelling terms and rearranging,

$$\begin{aligned}
&[q(\theta_l)+\theta_l q'(\theta_l)]\left[\frac{(r+\sigma+\gamma)(w(\theta_l)-b) - \sigma(w_l(\theta_l)-z)}{(r+\sigma+\gamma)(r+\lambda+\theta q(\theta_l)) - \sigma\lambda}\right]+\\
&\theta q(\theta_l)\Bigg[\frac{(r+\gamma)w_l'(\theta_l)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda}-\\
&\frac{(r+\sigma+\gamma)(w_l(\theta_l)-b) - \sigma(w_l(\theta_l)-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda}\frac{(r+\sigma+\gamma)[q(\theta_l)+\theta_l q'(\theta_l)]}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l)) - \sigma\lambda}\Bigg] = 0
\end{aligned}$$

Collecting like terms,

$$\left[\frac{(r+\sigma+\gamma)(w_l(\theta_l)-b)-\sigma(w_l(\theta_l)-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]\times$$
$$\left[q(\theta_l)+\theta_l q'(\theta_l)-\theta_l q(\theta_l)\frac{(r+\sigma+\gamma)[q(\theta_l)+\theta_l q'(\theta_l)]}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]+$$
$$\theta_l q(\theta_l)\left[\frac{(r+\gamma)w_l'(\theta_l)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]=0$$

Multiplying by 1 in the second term to obtain the same denominator,

$$\left[\frac{(r+\sigma+\gamma)(w_l(\theta_l)-b)-\sigma(w_l(\theta_l)-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]\times$$
$$\left[\frac{[q(\theta_l)+\theta_l q'(\theta_l)][(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda]}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}-\frac{\theta_l q(\theta_l)(r+\sigma+\gamma)[q(\theta_l)+\theta_l q'(\theta_l)]}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]+$$
$$\theta_l q(\theta_l)\left[\frac{(r+\gamma)w_l'(\theta_l)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]=0$$

Simplifying,

$$\left[\frac{(r+\sigma+\gamma)(w_l(\theta_l)-b)-\sigma(w_l(\theta_l)-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]\left[\frac{[q(\theta_l)+\theta_l q'(\theta_l)][(r+\sigma+\gamma)(r+\lambda)-\sigma\lambda]}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]+$$
$$\theta_l q(\theta_l)\left[\frac{(r+\gamma)w_l'(\theta_l)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]=0$$

From the constraint, we know that

$$w_l'(\theta_l)=pc\left\{\frac{\sigma\gamma}{r+\gamma}-(r+\lambda+\sigma)\right\}\frac{-q'(\theta_l)}{q(\theta_l)^2}$$

Multiplying by $(r+\gamma)$ and rearranging the RHS,

$$(r+\gamma)w_l'(\theta_l)=\frac{pc}{q(\theta_l)}\left\{(r+\lambda+\sigma)(r+\gamma)-\sigma\gamma\right\}\frac{q'(\theta_l)}{q(\theta_l)}$$

Using (3.2.4), we can substitute for $pc/q(\theta_l)$:

$$(r+\gamma)w_l'(\theta_l)=\frac{(r+\gamma)(p-w_l)-\sigma p\psi}{r(r+\lambda+\sigma)+\gamma(r+\lambda)}\left\{(r+\lambda+\sigma)(r+\gamma)-\sigma\gamma\right\}\frac{q'(\theta_l)}{q(\theta_l)}$$

Which simplifies to

$$(r+\gamma)w_l'(\theta_l) = [(r+\gamma)(p-w_l) - \sigma p\psi]\frac{q'(\theta_l)}{q(\theta_l)}$$

We can substitute this into the equation above:

$$\left[\frac{(r+\sigma+\gamma)(w_l(\theta_l)-b)-\sigma(w_l(\theta_l)-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]\left[\frac{[q(\theta_l)+\theta_l q'(\theta_l)][(r+\sigma+\gamma)(r+\lambda)-\sigma\lambda]}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right] =$$
$$-\theta_l q(\theta_l)\left[\frac{[(r+\gamma)(p-w_l)-\sigma p\psi]\frac{q'(\theta_l)}{q(\theta_l)}}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right]$$

Multiplying by $[\frac{[q(\theta_l)+\theta_l q'(\theta_l)][(r+\sigma+\gamma)(r+\lambda)-\sigma\lambda]}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}]^{-1}$,

$$\left[\frac{(r+\sigma+\gamma)(w_l(\theta_l)-b)-\sigma(w_l(\theta_l)-z)}{(r+\sigma+\gamma)(r+\lambda+\theta_l q(\theta_l))-\sigma\lambda}\right] =$$
$$-\frac{\theta_l q(\theta_l) q'(\theta_l)}{[q(\theta_l)+\theta_l q'(\theta_l)]q(\theta_l)}\left[\frac{[(r+\gamma)(p-w_l)-\sigma p\psi]}{(r+\sigma+\gamma)(r+\lambda)-\sigma\lambda}\right]$$

Finally, rewriting a bit and dividing by $q(\theta_l)/[q(\theta_l)+\theta_l q'(\theta_l)]$,

$$(1-\eta(\theta_l))\frac{(r+\gamma)w_l(\theta_l)-(r+\sigma+\gamma)b+\sigma z}{r(r+\sigma+\lambda)+\gamma(r+\lambda)+\theta_l q(\theta_l)(r+\sigma+\gamma)} = \eta(\theta_l)\frac{(r+\gamma)(p-w_l)-\sigma p\psi}{r(r+\sigma+\lambda)+\gamma(r+\lambda)}$$

Notice that this equality is identical to the bargaining solution (C.5) with the sharing parameter β replaced by the elasticity of the matching technology $\eta(\theta_l) = -q'(\theta_l)\theta_l/q(\theta_l)$. It follows immediately that the solution to this equation must be the wage from the undirected model with β replaced accordingly.

C.2.2 Job Without Leave

C.2.2.1 Firms

For completeness, I reproduce all firm value functions here:

$$rV_{nl} = -pc + q(\theta_{nl})(J_{nl} - V_{nl}) \tag{3.3.4}$$
$$rJ_{nl} = p - w_{nl} + (\lambda+\sigma)(V_{nl} - J_{nl}) \tag{3.3.5}$$

As before, I solve for w_{nl} as a function of θ_{nl} to ensure firm optimization. Using the free entry assumption that $V_{nl} = 0$, we know from (3.3.4) that $J_{nl} = pc/q(\theta_{nl})$. Substituting this and

$V_{nl} = 0$ into (3.3.5) and rearranging gives us that

$$w_{nl} = p - (\lambda + \sigma + r)\frac{pc}{q(\theta_{nl})}$$

C.2.2.2 Workers

To show the various wage results, I consider a more general model where (3.3.9) is instead given by

$$rL_{nl} = z + \gamma(U_{nl} - L_{nl}) \tag{C.6}$$

We can take the first differences (3.3.8) - (3.3.7), (3.3.8) - (C.6), and (C.6) - (3.3.7) to obtain the following system of three differences:

$$rE_{nl} - rU_{nl} = (w_{nl} - b) - (\lambda + \theta_{nl}q(\theta_{nl}))(E_{nl} - U_{nl}) - \sigma(E_{nl} - L_{nl}) \tag{C.7}$$

$$rE_{nl} - rL_{nl} = (w_{nl} - z) - \lambda(E_{nl} - U_{nl}) - \sigma(E_{nl} - L_{nl}) + \gamma(L_{nl} - U_{nl}) \tag{C.8}$$

$$rL_{nl} - rU_{nl} = (z - b) - \gamma(L_{nl} - U_{nl}) - \theta_{nl}q(\theta_{nl})(E_{nl} - U_{nl}) \tag{C.9}$$

Using (C.9), we can write $L_{nl} - U_{nl}$ as a function of $E_{nl} - U_{nl}$:

$$L_{nl} - U_{nl} = \frac{1}{r+\gamma}[(z - b) - \theta_{nl}q(\theta_{nl})(E_{nl} - U_{nl})]$$

Substituting this into (C.8), we can write $rE_{nl} - rL_{nl}$ as a function of $E_{nl} - U_{nl}$:

$$E_{nl} - L_{nl} = \frac{1}{r+\sigma}\left\{(w_{nl} - z) - \lambda(E_{nl} - U_{nl}) + \frac{\gamma}{r+\gamma}[(z - b) - \theta_{nl}q(\theta_{nl})(E_{nl} - U_{nl})]\right\}$$

Finally, substituting this into (C.7), we can solve for $E_{nl} - U_{nl}$:

$$\begin{aligned} rE_{nl} - rU_{nl} = &(w_{nl} - b) - (\lambda + \theta_{nl}q(\theta_{nl}))(E_{nl} - U_{nl}) \\ &- \frac{\sigma}{r+\sigma}\left\{(w_{nl} - z) - \lambda(E_{nl} - U_{nl}) + \frac{\gamma}{r+\gamma}[(z - b) - \theta_{nl}q(\theta_{nl})(E_{nl} - U_{nl})]\right\} \end{aligned}$$

Combining like terms and multiplying by $(r + \sigma)(r + \gamma)$,

$$\begin{aligned} (r+\sigma)(r+\gamma)(r+\lambda+\theta_{nl}q(\theta_{nl}))(E_{nl} - U_{nl}) = &(r+\sigma)(r+\gamma)(w_{nl} - b) \\ &- \sigma(r+\gamma)\{(w_{nl} - z) - \lambda(E_{nl} - U_{nl})\} \\ &- \gamma\sigma[(z - b) - \theta_{nl}q(\theta_{nl})(E_{nl} - U_{nl})] \end{aligned}$$

Again moving like terms to the LHS,

$$
\begin{aligned}
[(r+\sigma)(r+\gamma)(r+\lambda+\theta_{nl}q(\theta_{nl})) - \lambda\sigma(r+\gamma) - \gamma\sigma\theta_{nl}q(\theta_{nl})](E_{nl}-U_{nl}) &= (r+\sigma)(r+\gamma)(w_{nl}-b) \\
&\quad - \sigma(r+\gamma)(w_{nl}-z) \\
&\quad - \gamma\sigma(z-b)
\end{aligned}
$$

Isolating $E_{nl} - U_{nl}$ on the LHS:

$$
\begin{aligned}
(E_{nl}-U_{nl}) &= \frac{(r+\sigma)(r+\gamma)(w_{nl}-b) - \sigma(r+\gamma)(w_{nl}-z) - \gamma\sigma(z-b)}{(r+\sigma)(r+\gamma)(r+\lambda+\theta_{nl}q(\theta_{nl})) - \lambda\sigma(r+\gamma) - \gamma\sigma\theta_{nl}q(\theta_{nl})} \\
&= \frac{r(r+\gamma)w_{nl} - r(r+\gamma+\sigma)b + r\sigma z}{r(r+\gamma)(r+\lambda+\theta_{nl}q(\theta_{nl})) + \sigma(r+\gamma)(r+\theta_{nl}q(\theta_{nl})) - \gamma\sigma\theta_{nl}q(\theta_{nl})} \\
&= \frac{r(r+\gamma)w_{nl} - r(r+\gamma+\sigma)b + r\sigma z}{r(r+\gamma)(r+\lambda+\theta_{nl}q(\theta_{nl})) + \sigma r(r+\theta_{nl}q(\theta_{nl})) + \sigma\gamma r} \\
&= \frac{(r+\gamma)w_{nl} - (r+\gamma+\sigma)b + \sigma z}{(r+\gamma)(r+\lambda+\theta_{nl}q(\theta_{nl})) + \sigma(r+\theta_{nl}q(\theta_{nl})) + \sigma\gamma} \\
&= \frac{(r+\gamma)w_{nl} - (r+\gamma+\sigma)b + \sigma z}{(r+\gamma)(r+\lambda+\sigma) + \theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)} \\
&= \frac{(r+\gamma)(w_{nl}-b) + \sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma) + \theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)}
\end{aligned}
$$

C.2.2.3 Submarket Equilibrium

We can find for the equilibrium (w_{nl}, θ_{nl}) by solving the following maximization problem:

$$
\begin{aligned}
\max_{w_{nl},\,\theta_{nl}} \quad & b + \theta_{nl}q(\theta_{nl})\frac{(r+\gamma)(w_{nl}-b) + \sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma) + \theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)} \\
\text{s.t.} \quad & w_{nl} = p - (r+\lambda+\sigma)\frac{pc}{q(\theta_{nl})}
\end{aligned}
$$

Differentiating with respect to θ_{nl} yields the first order condition:

$$
\begin{aligned}
&[q(\theta_{nl}) + \theta_{nl}q'(\theta_{nl})]\left[\frac{(r+\gamma)(w_{nl}-b) + \sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma) + \theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)}\right] + \\
&[\theta_{nl}q(\theta_{nl})]\frac{[(r+\gamma)(r+\lambda+\sigma) + \theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)][(r+\gamma)w'_{nl}(\theta_{nl})]}{[(r+\gamma)(r+\lambda+\sigma) + \theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]^2} - \\
&[\theta_{nl}q(\theta_{nl})]\frac{[(r+\gamma)(w_{nl}-b) + \sigma(z-b)]\{[q(\theta_{nl}) + \theta_{nl}q'(\theta_{nl})](r+\gamma+\sigma)\}}{[(r+\gamma)(r+\lambda+\sigma) + \theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]^2} = 0
\end{aligned}
$$

Collecting like terms and rearranging,

$$\begin{aligned}&\left[\frac{(r+\gamma)(w_{nl}-b)+\sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)}\right]\times\\ &\left[[q(\theta_{nl})+\theta_{nl}q'(\theta_{nl})]-[\theta_{nl}q(\theta_{nl})]\frac{\{[q(\theta_{nl})+\theta_{nl}q'(\theta_{nl})](r+\gamma+\sigma)\}}{[(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]}\right]+\\ &[\theta_{nl}q(\theta_{nl})]\frac{[(r+\gamma)w'_{nl}(\theta_{nl})]}{[(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]}=0\end{aligned}$$

Multiplying by 1 to obtain the same denominator on line 2,

$$\begin{aligned}&\left[\frac{(r+\gamma)(w_{nl}-b)+\sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)}\right]\times\\ &\left[\frac{[q(\theta_{nl})+\theta_{nl}q'(\theta_{nl})][(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]}{[(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]}-\right.\\ &\left.\frac{\{[\theta_{nl}q(\theta_{nl})][q(\theta_{nl})+\theta_{nl}q'(\theta_{nl})](r+\gamma+\sigma)\}}{[(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]}\right]+\\ &[\theta_{nl}q(\theta_{nl})]\frac{[(r+\gamma)w'_{nl}(\theta_{nl})]}{[(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]}=0\end{aligned}$$

Simplifying,

$$\begin{aligned}&\left[\frac{(r+\gamma)(w_{nl}-b)+\sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)}\right]\left[\frac{[q(\theta_{nl})+\theta_{nl}q'(\theta_{nl})][(r+\gamma)(r+\lambda+\sigma)]}{[(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]}\right]+\\ &[\theta_{nl}q(\theta_{nl})]\frac{[(r+\gamma)w'_{nl}(\theta_{nl})]}{[(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]}=0\end{aligned}$$

Multiplying through by $\left[\frac{[q(\theta_{nl})+\theta_{nl}q'(\theta_{nl})][(r+\gamma)(r+\lambda+\sigma)]}{[(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]}\right]^{-1}$,

$$\begin{aligned}&\frac{(r+\gamma)(w_{nl}-b)+\sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)}+\\ &[\theta_{nl}q(\theta_{nl})]\frac{[(r+\gamma)w'_{nl}(\theta_{nl})]}{[q(\theta_{nl})+\theta_{nl}q'(\theta_{nl})][(r+\gamma)(r+\lambda+\sigma)]}=0\end{aligned}\tag{C.10}$$

From the constraint, we know that

$$w'_{nl}(\theta_{nl})=(\lambda+\sigma+r)\frac{pcq'(\theta_{nl})}{q(\theta_{nl})^2}$$

Multiplying by $(r+\gamma)$ and rearranging the RHS,

$$(r+\gamma)w'_{nl}(\theta_{nl}) = (r+\gamma)(\lambda+\sigma+r)\frac{pc}{q(\theta_{nl})}\frac{q'(\theta_{nl})}{q(\theta_{nl})}$$

Using (3.3.6), we can substitute for $pc/q(\theta_{nl})$:

$$(r+\gamma)w'_{nl}(\theta_{nl}) = (r+\gamma)(\lambda+\sigma+r)\frac{p-w_{nl}}{r+\lambda+\sigma}\frac{q'(\theta_{nl})}{q(\theta_{nl})}$$

Which simplifies to

$$(r+\gamma)w'_{nl}(\theta_{nl}) = (r+\gamma)(p-w_{nl})\frac{q'(\theta_{nl})}{q(\theta_{nl})}$$

We can substitute this into (C.10),

$$\frac{(r+\gamma)(w_{nl}-b)+\sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)} = -[\theta_{nl}q(\theta_{nl})]\frac{(r+\gamma)(p-w_{nl})\frac{q'(\theta_{nl})}{q(\theta_{nl})}}{[q(\theta_{nl})+\theta_{nl}q'(\theta_{nl})][(r+\gamma)(r+\lambda+\sigma)]}$$

Rewriting a bit,

$$\frac{q(\theta_{nl})+\theta_{nl}q'(\theta_{nl})}{q(\theta_{nl})}\frac{(r+\gamma)(w_{nl}-b)+\sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)} = \frac{-\theta_{nl}q'(\theta_{nl})}{q(\theta_{nl})}\frac{p-w_{nl}}{r+\lambda+\sigma}$$

Noting that $\eta(\theta) = -q'(\theta)\theta/q(\theta)$, this becomes

$$(1-\eta(\theta_{nl}))\frac{(r+\gamma)(w_{nl}-b)+\sigma(z-b)}{(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)} = \eta(\theta_{nl})\frac{p-w_{nl}}{r+\lambda+\sigma}$$

Cross multiplying,

$$\begin{gathered}(1-\eta(\theta_{nl}))(r+\lambda+\sigma)[(r+\gamma)(w_{nl}-b)+\sigma(z-b)] = \\ \eta(\theta_{nl})(p-w_{nl})[(r+\gamma)(r+\lambda+\sigma)+\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)]\end{gathered}$$

Rearranging,

$$\begin{aligned}(r+\lambda+\sigma)(r+\gamma)w_{nl} &= (1-\eta(\theta_{nl}))(r+\lambda+\sigma)(r+\gamma)b - (1-\eta(\theta_{nl}))(r+\lambda+\sigma)\sigma(z-b) \\ &\quad + \eta(\theta_{nl})p(r+\gamma)(r+\lambda+\sigma) + \eta(\theta_{nl})(p-w_{nl})\theta_{nl}q(\theta_{nl})(r+\gamma+\sigma)\end{aligned}$$

Dividing through by $(r + \lambda + \sigma)(r + \gamma)$,

$$
\begin{aligned}
w_{nl} &= (1 - \eta(\theta_{nl}))b + \eta(\theta_{nl})p + \eta(\theta_{nl})\frac{p - w_{nl}}{r + \lambda + \sigma}\theta_{nl} q(\theta_{nl}) \\
&\quad + \frac{\sigma}{r + \gamma}\left\{\eta(\theta_{nl})\frac{p - w_{nl}}{r + \lambda + \sigma}\theta_{nl} q(\theta_{nl}) - (1 - \eta(\theta_{nl}))(z - b)\right\}
\end{aligned}
$$

Finally, using the job creation condition (3.3.6), we arrive at (3.3.10):

$$
w_{nl} = (1 - \eta(\theta_{nl}))b + \eta(\theta_{nl})p + \eta(\theta_{nl})pc\theta_{nl} + \frac{\sigma}{r + \gamma}\left\{\eta(\theta_{nl})pc\theta_{nl} - (1 - \eta(\theta_{nl}))(z - b)\right\}
$$

Taking the limit as $\gamma \to \infty$ gives (3.3.11), and setting $z = b$ gives (3.3.12).

C.2.3 Equilibrium With Symmetric Information

C.2.3.1 Proving the Single Crossing Property

To simplify analysis, I write $\eta(\theta_{nl}) = \eta(\theta_l) = \eta$, as this elasticity does not depend on θ by (Assumption 2a: Constant Elasticity). I show that each property holds in turn.

1. $\lim_{\gamma \to \infty} \theta_l(\gamma) > \lim_{\gamma \to \infty} \theta_{nl}(\gamma)$

Taking the limit of (3.3.15) and (3.3.16) as $\gamma \to \infty$, this system of equations becomes

$$
\begin{aligned}
\frac{[(1 - \eta)(p - b) - \eta pc\theta_{nl}]}{(r + \lambda + \sigma)} - \frac{pc}{q(\theta_{nl})} &= 0 && \text{No Leave} \\
\frac{[(1 - \eta)(p - b) - \eta pc\theta_{l}]}{(r + \lambda)} - \frac{pc}{q(\theta_{l})} &= 0 && \text{Leave}
\end{aligned}
$$

Under (Assumption 2b: Gains to Trade) we know that the numerator of the first term in both equations is positive, so it follows that, if $\theta_{nl} = \theta_l$,

$$
\frac{[(1 - \eta)(p - b) - \beta pc\theta_{nl}]}{(r + \lambda + \sigma)} - \frac{pc}{q(\theta_{nl})} < \frac{[(1 - \eta)(p - b) - \beta pc\theta_{l}]}{(r + \lambda)} - \frac{pc}{q(\theta_{l})}
$$

Further, since $q'(\theta) < 0$, we know that the LHS of both equations is decreasing in θ. It follows immediately that $\theta_l > \theta_{nl}$ at their solution.

2. $\theta_l'(\gamma), \theta_{nl}'(\gamma) > 0$

We can rewrite (3.3.15) and (3.3.16):

$$
\begin{aligned}
F_{nl} &\equiv \frac{[(1-\eta)(p-b)-\eta pc\theta_{nl}]}{(r+\lambda+\sigma)} - \frac{\sigma\eta pc\theta_{nl}}{(r+\gamma)(r+\lambda+\sigma)} - \frac{pc}{q(\theta_{nl})} = 0 \\
F_l &\equiv \frac{(r+\gamma)[(1-\eta)(p-b)-\eta pc\theta_l]}{r(r+\lambda+\sigma)+\gamma(r+\lambda)} - \frac{\sigma\eta pc\theta_l}{r(r+\lambda+\sigma)+\gamma(r+\lambda)} - \\
&\qquad \frac{\sigma(1-\eta)(1-\alpha)p\psi}{r(r+\lambda+\sigma)+\gamma(r+\lambda)} - \frac{pc}{q(\theta_l)} = 0
\end{aligned}
$$

We can use the implicit function theorem to sign these derivatives. Recalling that η is a constant function of θ and that $q'(\theta) < 0$, it is clear that $\partial F_{nl}/\partial\theta_{nl} < 0$ and $\partial F_l/\partial\theta_l < 0$. In addition, it is easy to show that $\partial F_{nl}/\partial\gamma > 0$, and noting that

$$
\frac{\partial}{\partial\gamma}\frac{r+\gamma}{r(r+\lambda+\sigma)+\gamma(r+\lambda)} = \frac{r\sigma}{[r(r+\lambda+\sigma)+\gamma(r+\lambda)]^2} > 0
$$

We have that $\partial F_l/\partial\gamma > 0$ as well. Then, the implicit function theorem gives us that

$$
\begin{aligned}
\theta_l'(\gamma) &= -\frac{\partial F_l/\partial\gamma}{\partial F_l/\partial\theta_l} > 0 \\
\theta_{nl}'(\gamma) &= -\frac{\partial F_{nl}/\partial\gamma}{\partial F_{nl}/\partial\theta_{nl}} > 0
\end{aligned}
$$

3. $\lim_{\gamma\to 0}\theta_l(\gamma) < \lim_{\gamma\to 0}\theta_{nl}(\gamma)$

Taking the limit of (3.3.15) and (3.3.16) as $\gamma \to 0$, this system of equations becomes

$$
\begin{aligned}
&\frac{r[(1-\eta)(p-b)-\eta pc\theta_{nl}]}{r(r+\lambda+\sigma)} - \frac{\sigma\eta pc\theta_{nl}}{r(r+\lambda+\sigma)} - \frac{pc}{q(\theta_{nl})} = 0 \\
&\frac{r[(1-\eta)(p-b)-\eta pc\theta_l]}{r(r+\lambda+\sigma)} - \frac{\sigma\eta pc\theta_l}{r(r+\lambda+\sigma)} - \\
&\qquad \frac{\sigma(1-\eta)(1-\alpha)p\psi}{r(r+\lambda+\sigma)} - \frac{pc}{q(\theta_l)} = 0
\end{aligned}
$$

By (Assumption 2c: Costly Leave), we know that $1-\alpha > 0$, so it must be the case that $\frac{\sigma(1-\eta(\theta))(1-\alpha)p\psi}{r(r+\lambda+\sigma)} > 0$. It follows that, if $\theta_{nl} = \theta_l$,

$$
\begin{aligned}
&\frac{r[(1-\eta)(p-b)-\eta pc\theta_l]}{r(r+\lambda+\sigma)} - \frac{\sigma\eta pc\theta_l}{r(r+\lambda+\sigma)} - \frac{pc}{q(\theta_l)} - \frac{\sigma(1-\eta)(1-\alpha)p\psi}{r(r+\lambda+\sigma)} < \\
&\frac{r[(1-\eta)(p-b)-\eta pc\theta_{nl}]}{r(r+\lambda+\sigma)} - \frac{\sigma\eta pc\theta_{nl}}{r(r+\lambda+\sigma)} - \frac{pc}{q(\theta_{nl})}
\end{aligned}
$$

Since the LHS of both equations is decreasing in θ, it follows that $\theta_l < \theta_{nl}$ at their solution.

Now, with these lemmas, we can prove the single crossing property. Properties 1 and 3, together with continuity of $\theta_l(\gamma), \theta_{nl}(\gamma)$, immediately imply that there exists some γ^* such that $\theta_l(\gamma^*) = \theta_{nl}(\gamma^*)$. Suppose for contradiction that there are multiple γ such that $\theta_l(\gamma) = \theta_{nl}(\gamma)$, and let γ^* denote the smallest such γ. At any solution, (3.3.15) and (3.3.16) imply that the following condition must hold:

$$\frac{pc}{q(\theta)} = \frac{(1-\eta(\theta))(1-\alpha)p\psi}{\gamma}$$

Since γ^* is the smallest γ, any other solution γ' must be greater than γ^*. Since $\theta_l'(\gamma), \theta_{nl}'(\gamma) > 0$, it must be the case that the corresponding tightness $\theta' > \theta$. However, at the above equation the LHS is increasing in θ while the RHS is decreasing in γ, so it must be the case that

$$\frac{pc}{q(\theta')} > \frac{(1-\eta)(1-\alpha)p\psi}{\gamma'}$$

Thus we have a contradiction and uniqueness of the solution.

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