Online Appendix for "Women's Liberation, Household Revolution"

A New York Times

The *New York Times* (NYT) printed the 1860 New York law, discussed in the main paper Section 3.1, in its entirety upon passage (New York Times, 1860c). However, interest in the topic was so great that they did not only print updates of laws in New York, but rather around the U.S., and indeed the U.K. as well. For instance, in 1852 they reprinted an article entitled "Women's Rights and Wrongs" from the *Detroit Tribune* explaining exactly the difference in legal rights between men and women in the state at the time (New York Times, 1852). They also reprinted an article from the *St. Paul Press* when Minnesota granted women economic rights (New York Times, 1869). It is worth noting that this article refers to the old laws as "barbarous," and explains exactly what rights the new laws do and not convey to each spouse. In 1870, they printed an article explaining to their readers that the "married women of Connecticut suffer injustice," as women had not yet been granted rights in that state, before delving into the efforts in that state to change the law (New York Times, 1870). After women's rights were eventually passed in 1870, they reported on a New-Haven *Journal* article providing a "summary, more complete than we have yet seen, of the provisions of the new law of Connecticut in relation to the property rights of married women . . . " (New York Times, 1877a). The NYT also printed updates on the debates and legal changes in the United Kingdom (New York Times, 1871, 1877b, 1882).

The NYT also updated readers on court cases and legal scholar's opinions on marital property rights. For example, consider the case of *Vandevoort v. Gould*, 36 N.Y 639 (1867), which asked legal questions pertaining to whether married women's property acts applied to property applied before the passage of the act. The NYT covered this case closely, giving readers updates while the case was still on, as well as the final decision and implications (New York Times, 1860a, 1862a,c,b). This court case is not unique. We found several other articles in the *New York Times* covering other cases of importance for women's economic rights (New York Times, 1854, 1860b, 1865a,b, 1866). Again, they did not confine coverage to New York. They reported on court cases in Maine (New York Times, 1868), Illinois (New York Times, 1875), and Missouri (New York Times, 1888). They also went into detail covering public lectures on women's rights, such as given by James T. Brady, Esq. entilted "The Legal Disabilities of Women" (New York Times, 1858).[1] The reviewed scholarly work, such as Ostrogorski (1893), when this work was imported into the United States (New York Times, 1894), and gave summaries of cases where married women did not seem to have their rights enforced (New York Times,

[1]The article covers the first of a series of lectures to be given on women's rights by Mr. Brady, and gives details as to his view on the legal history of women's economic rights. As further evidence as to how interested people were in the topic, the article describes the audience as "large and markedly fashionable."

1879).

B Extra Historical Context, 1850–1920

Between 1850 and 1920, the economic role of women in the United States underwent significant changes, particularly for white non-Hispanic women. This period witnessed the transition from a predominantly agrarian economy to an industrial one, the Demographic Transition (a sustained decline in fertility rates), and the expansion of educational opportunities. We highlight how women's labor force participation evolved by marital status and socioeconomic status, how women's investments in human capital (education) increased, as well as methods people had at their disposal to control their fertility. Throughout this era, marriage remained a highly stable institution for white non-Hispanic women, with divorce being rare by today's standards.

B.1 Labor Force Participation by Marital Status and Socioeconomic Status

In our time period, women's labor force participation was defined by single women. Indeed, Goldin (1980) argues that "[t]he history of the female labor force from 1870 to 1920 was shaped by single women, and, correspondingly, that from 1920 to 1970 by those who were married." (pp. 81-82). She finds that single women dominated the U.S. female workforce in this period, taking jobs in domestic service, manufacturing, and later clerical work. In contrast, once women married, prevailing social norms and the economic structure typically led them to withdraw from paid employment. Around 1900, only a small minority of married white women engaged in gainful employment, whereas a much larger share of single women worked for pay. This disparity reflected the era's gender norms: the husband was expected to be the breadwinner, and a married woman's work was primarily home production and child-rearing. Indeed, many employers imposed formal marriage bars (especially in white-collar occupations emerging in the early 20th century), requiring women to leave their jobs upon marriage, which reinforced the low participation of married women (Goldin, 2006). Married women themselves largely disapproved of the idea that married women should work if their husbands could support them (Fernández, 2013). At the same time, lower-income and working-class women (and widows) often had little choice but to work, so those married women who did participate were disproportionately from economically disadvantaged households. Many of these women engaged in informal or piecework labor (e.g., taking in laundry or boarders) that went undercounted in official statistics. Overall, however, the picture for white non-Hispanic women before 1920 is one of a stark "marital LFP gap": high participation by single women and very low participation by married women. This pattern is emphasized in Claudia Goldin's phase characterization of women's economic history: she terms the era up to the 1920s as that of the "independent female worker," when women typically worked only while single and exited the labor force at marriage (Goldin, 2006). Consistent with this, the aggregate female labor force participation rate changed little in the late 19th century, since rising education and job opportunities mainly affected the timing of work (before marriage) rather than inducing married women to enter the workforce.

It is important to note that these historical statistics refer primarily to white non-Hispanic women, the focus of our paper. Other demographic groups had different experiences. For example, Goldin (1990, p. 27) finds that non-white married women worked at about ten times the rate as white non-Hispanic married women. Within our group of focus, married women in this era rarely worked outside the home, adhering to the prevailing "cult of domesticity" expectations. Indeed, Goldin (1990, p. 16) finds that "[t]he majority of women exited the labor force at or just after marriage ... ". Goldin (2006) finds that the average married woman working was less educated than the average in the population, suggesting it was lower class married women who worked.

B.2 Improvements in Women's Human Capital and Education

Goldin (1999) observes that by the 1840s primary school enrollment in the US was higher than Germany, making Americans among the best educated in the world, but that there were gender gaps in this education rate. However, once schools became publicly funded, the gap closed and eventually reversed.

Secondary education saw especially dramatic growth in the early 20th century. The period 1910-1940 is known as the "high school movement," during which high school enrollment and graduation rates soared across the country (particularly in the American Midwest and West). Significantly, young women embraced secondary education at least as rapidly as young men. In fact, throughout the 1910s and 1920s, female high school enrollment and graduation rates often equaled or exceeded those of males in many states (Goldin, 1998). Goldin (1998) document that by the 1920s, the graduation rate for American girls had pulled slightly ahead of boys, reflecting, among other factors, the view that high school was appropriate for preparing young women for teaching or clerical careers as well as for their roles as mothers. This early closing (and slight reversal) of the gender gap in secondary education is a striking historical finding. It implies that the average white woman coming of age in 1920 was appreciably better educated than her counterpart in 1850, and indeed was as well-educated as her male peers in terms of formal schooling. Higher education, while still the province of a small minority in this era, also saw growing female participation. Goldin et al. (2006) present evidence that women and men born between 1875 and 1910 began college at similar rates, but that men were substantially more likely graduate with a degree.

B.3 Families and Birth Control

Consistent with our findings, Stevenson and Wolfers (2007) show that marriage rates were stable over our time period, and divorce was rare. They also breakdown these findings by race. Perhaps the most dramatic demographic change between 1850 and 1920 was the sharp decline in fertility. In the mid-19th century, white American families were still quite large by modern standards, with total fertility rates often around 5 or more children per woman. By the early 20th century, fertility had fallen significantly: the average white woman born around 1880 had about 3 surviving children, and fertility continued to drop to roughly 2-3 children per woman by the 1920s (Jones and Tertilt, 2008).

Between 1850 and 1920, a wide range of birth control methods was employed in the United States, reflecting both longstanding practices and emerging technological innovations. Early methods included coitus interruptus (withdrawal) and periodic abstinence, but the era also witnessed the increasing use of mechanical barrier devices. With the advent of vulcanization, rubber condoms and diaphragms became more reliable and affordable (Bullough, 1981). Historically, various pessaries made from materials, including cabbage leaves and tar, sponge, and even gold, were employed as contraceptive devices (Fairley, 1990).

Legal and medical debates played a central role in shaping access to these methods. The Comstock Laws, passed in the early 1870s, criminalized not only the distribution of contraceptive devices but also the dissemination of related information, forcing many providers into a clandestine market (Tone, 2000). Medical professionals were divided: some advocated the use of barrier methods (such as condoms and the womb veil) and withdrawal while others, influenced by prevailing moral and religious standards, dismissed these techniques or even condemned them (Cirillo, 1974). The legal framework thus intertwined with medical authority, leading to selective enforcement and, at times, the emergence of a robust black market for contraceptives.

B.4 Legality of Divorce

While divorce was rare by modern standards, as discussed above, it was not illegal in every state. Between 1850 and 1920, obtaining a divorce required navigating a formal legal process defined by state law. A spouse seeking to end a marriage had to file a petition in a state court and prove the existence of statutory grounds for divorce. Throughout this period divorce was *fault-based*: courts would grant a decree dissolving the marriage only upon evidence of legally recognized wrongdoing by the other spouse (such as adultery, desertion, or cruelty). The petitioner bore the burden of proof, often necessitating corroborating testimony of the misconduct. Procedurally, divorce cases followed the norms of civil litigation, but many jurisdictions imposed additional safeguards (for example, residency requirements of six months to a year) to establish the court's jurisdiction and to prevent forum-shopping by couples from out of state (Higdon, 2022). In some states during the early 19th century, divorces could still be obtained by special acts of the legislature, but by the mid-1800s most states had vested this power in the judiciary, making divorce a matter for judges rather than legislators (Hartog, 1991).

Between 1850 and 1920, divorce laws underwent liberalization in many states, even as others remained exceptionally strict. Over the second half of the 19th century, a number of states expanded the grounds for divorce beyond the traditional sole ground of adultery. Activists and jurists formed the New England Divorce Reform League in 1881, reorganized as the National Divorce Reform League in 1885, to advocate more consistent standards and to discourage migratory divorces obtained under lax laws (Riley, 1991, p. 108). Their efforts culminated in the National Congress on Uniform Divorce Laws in 1906 (Riley, 1991, p. 115). No binding uniform law was enacted.

Divorce law variation among states, especially the divide between strict and lenient jurisdictions, led to so-called "divorce mill" states (or territories) (Higdon, 2022). One legal challenge was whether a divorce granted in one state would be recognized elsewhere. Courts grappled with the Constitution's Full Faith and Credit Clause in this context. The U.S. Supreme Court's ruling in *Haddock v. Haddock*, 201 U.S. 562 (1906), exemplified the era's approach: the Court refused to mandate interstate recognition of a divorce decree obtained ex parte (i.e. when only one spouse was domiciled in the forum state), effectively allowing a stricter home state to treat an out-of-state "migratory" divorce as invalid (Higdon, 2022). At the same time, state courts gradually broadened interpretations of what behavior constituted legal grounds for divorce. Judges in the 1880s began to acknowledge mental cruelty (such as persistent emotional abuse or humiliation) as a form of cruelty sufficient to justify divorce, even in the absence of physical violence (Griswold, 1986).

We conclude that divorce, while not a purely static institution, was rare by modern standards for our sample population and time period.

C Construction of County-Border Pairs

The data on the evolution of US historical county boundaries comes from the Integrated Public Use Microdata Series (IPUMS) National Historical Geographic Information System (NHGIS), available at http://www.nhgis.com. Although there are other projects featuring US historical boundaries and spatial data within a Geographic Information Systems (GIS) framework, we use the NHGIS border definitions, as they provide a better fit for mapping US federal census data from IPUMS. We start by obtaining eight geometry file maps corresponding to the 1850-1920 census year boundaries. These shapefiles consist of polygons, each of which is defined by a list of vertices with two-dimensional coordinates. We use QGIS as our primary tool for handling the shapefiles. In order to identify the best topologically continuous set of bordering counties (i.e., counties adjacent to the counties borders from another state) over the entire 1850-1920 period, we develop the following four-step procedure:

Step 1: We identify for every polygon in the shapefile all of its immediate neighbors. A polygon is considered a neighbor of another polygon if they touch or intersect. The script records the unique county (GISJOIN2 variable) and state identifiers of all neighbors. We eliminate counties that are only adjacent to counties from the same state/territory in order to arrive at a sample of county-border pairs. We manually examine the resulting samples and eliminate polygons that correspond to the administrative units that have not been partitioned into counties, such as large territories without political subdivisions.

Step 2: The borders in the year 1920 are the final borders for our study. The borders in earlier decades were unstable due to the evolution of states, as well as counties within the states. We created a stable system of IDs for each region based on the map of 1920. For the earlier decades (1850-1910), we adopt the names given in 1920. Each county's ID in 1850-1910 is defined by the highest intersected area with the IDs in 1920. In other words, each county x in 1850-1910 takes the ID of the county y in 1920 if and only if county y has the highest intersected area with county x across all different intersected counties in 1920.

Step 3: If a county breaks into multiple counties over the course of time, we look at new counties after separation as one cluster based on their borders before separation. This allows us to maintain constant geographic areas as our points of comparison. To be more precise, for each county in decade d, we look at the corresponding counties in previous decades: $t \in [1850, .., d-10]$. If a county from decade t, x_{it} intersects with several counties in decade d, with an intersected area that exceeds 25 percent of the area x_{id}, then all these counties in d are considered as a unique county: x_{it}. We unify overlapping clusters into one.

Step 4: We then develop a stable set of pair-dummies that corresponds to neighboring fixed counties in neighboring states. We proceed as follows: (a) For each county from each decade we find all neighboring counties from other states in the same decade. (b) If the joint border between a pair of neighboring counties from 2 different states is longer than 10 percent of the length of each county's border with the other state, then we constitute a pair-dummy for this pair. (c) If a county-pair was not considered in previous decades– perhaps since the area wasn't well defined or stable– we create a new name for the dummy variable based on the combination of IDs. This step allows us to produce a stable structure of dummies through time. Maps showing our data on borders over time can be seen in Figures A.1 - A.8.

To give an example of the difficulties that arise in this process, Figure A.9 shows the evolution of the border between Indiana and Illinois. Each map shows this border in different years,

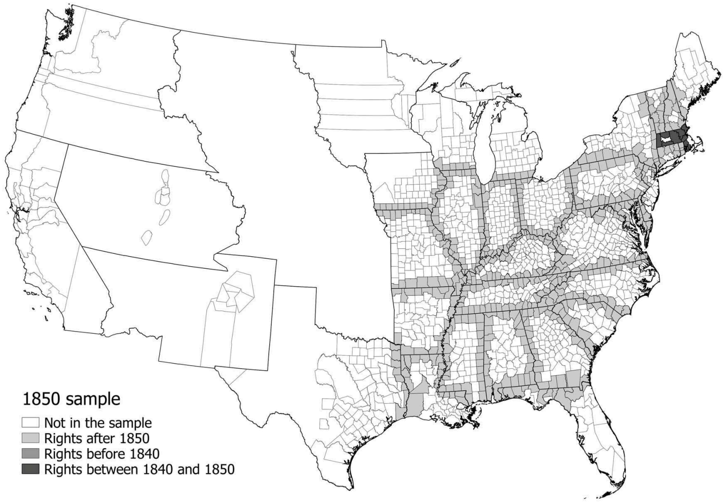

Figure A.1: **State borders, 1850.**

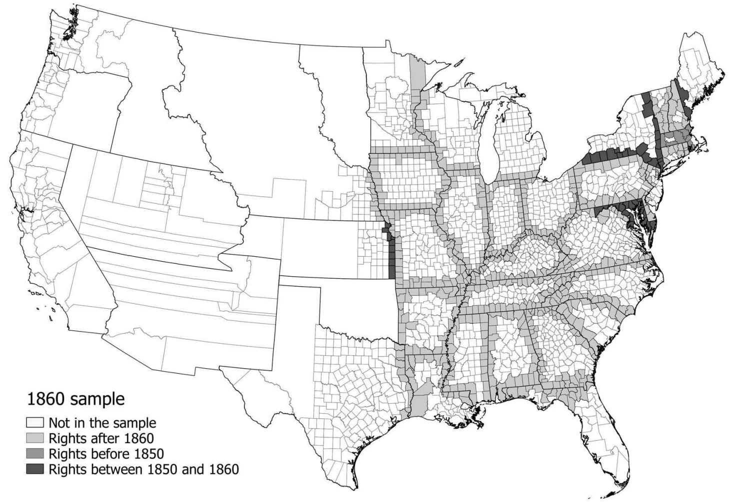

Figure A.2: **State borders, 1860.**

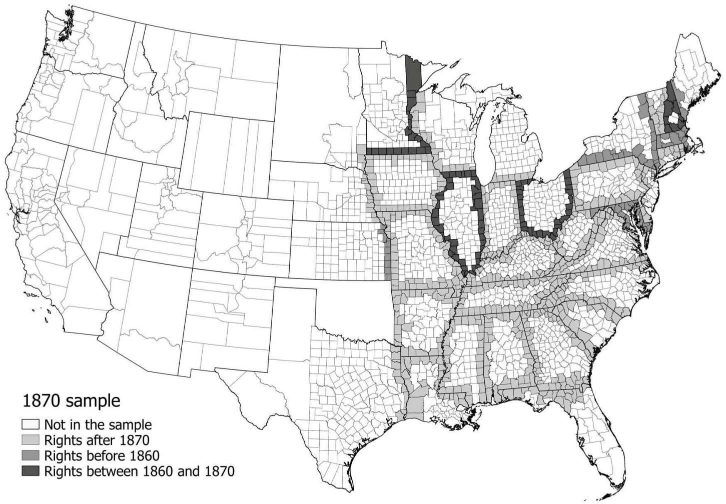

Figure A.3: **State borders, 1870.**

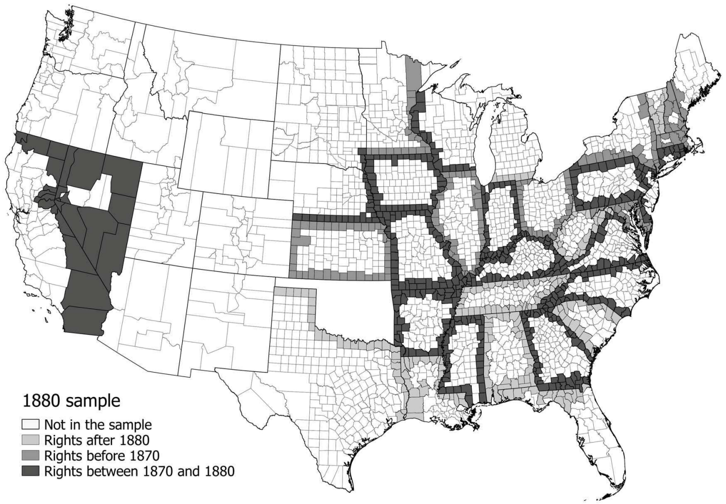

Figure A.4: **State borders, 1880.**

State Borders, 1850-1880

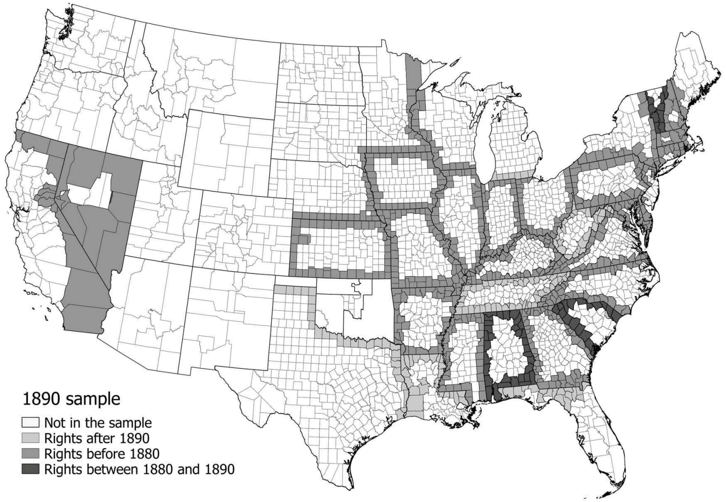

Figure A.5: **State borders, 1890.**

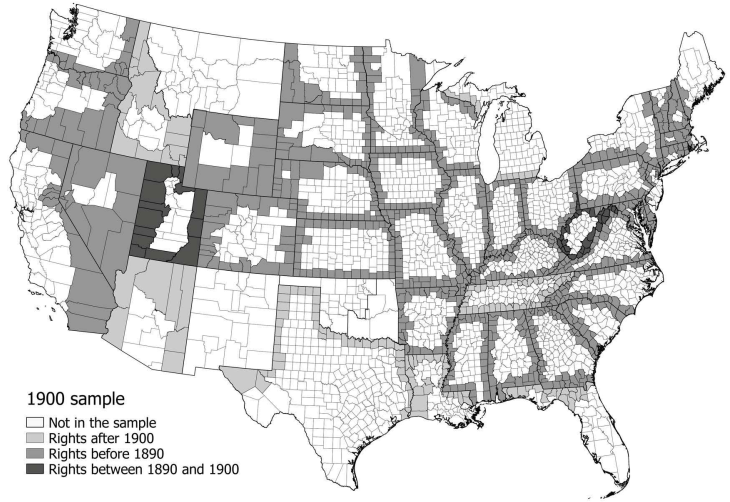

Figure A.6: **State borders, 1900.**

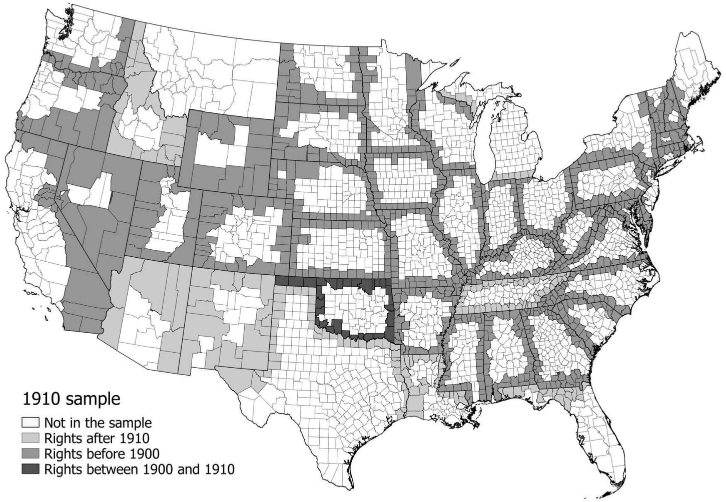

Figure A.7: **State borders, 1910.**

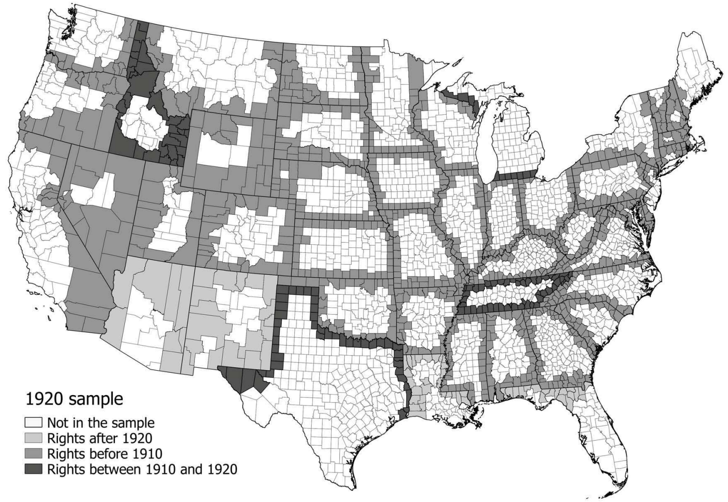

Figure A.8: **State borders, 1920.**

State Borders, 1890-1920

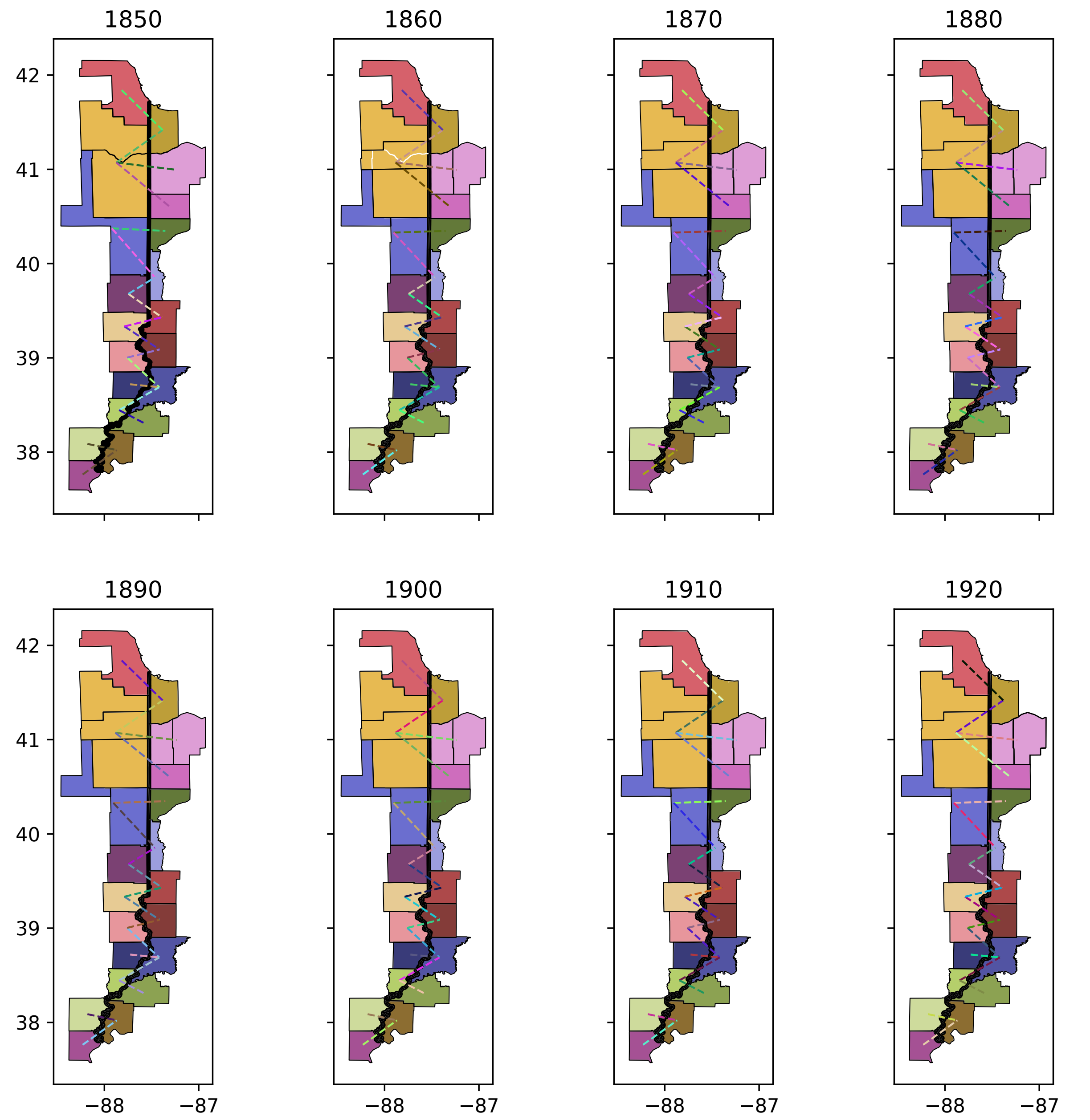

Figure A.9: **State borders, example.** This figure shows the evolution of the county-border pairs between Indiana (right) and Illinois (left) over time.

from 1850 until 1920. The solid black line in the middle, roughly going from north to south, denotes the border between the states. Each polygon represents a county in each year on either side of the state border. Counties in the same state in the same color that touch one another are grouped in a cluster and treated as one county for the purposes of this exercise. The dotted lines show which clusters of counties were compared to which clusters on the other side of the state border. The border between Illinois and Indiana is particularly useful as an example since it allows us to illustrate a number of issues that arose while creating our county-border pair exercises. Many of them have no clear-cut answer as to what to do and require a judgement call. We now go over some issues that arose.

First, whether to clump counties together when they merged. Consider the 3 orange colored counties in Illinois, almost all the way in the north, in 1860. These counties are roughly geographically constant from 1860 until 1920. However, the middle of these 3 counties did not exist in 1850, and was indeed part of the other two counties. We decided to treat this entire area as one county for the entire time period. An alternative strategy would have been to throw out the data from 1850 and have 3 distinct counties from 1860 onwards.

Another example of this is the blue county directly south of these orange counties. In 1850, this region was one large county. Starting in 1860, this region was divided into two counties: one directly on the border with Illinois, and one not directly on the border. We decided that we would consider these two counties as one during the entire time period, and thus include a county not directly on the border, rather than begin the exercise in 1860.

Second, there were small changes in county areas. Consider the most north-western sliver of the blue county adjacent to the orange counties discussed above, in 1850. In 1860, some of this territory is transferred into the orange counties. The area in question is outlined in white in the 1860 map. We decided to ignore this small change in county areas.

While reasonable people can disagree as to whether the approaches described above are correct or not, both in general and in the specific context of Indiana and Illinois, we hope that it illustrates the need to make a general set of rules and apply our best judgement throughout. Any rule that might seem more/less appropriate on this border, might seem the opposite on a different border.

D Extra Analyses and Robustness Exercises

In this appendix, we do perform extra analyses examining differential effects of women's rights on border and non border regions and robustness exercises on the main exercises reported in the main paper.

D.1 Border Regions

We begin with extra analyses looking at the potential for differential impacts on border regions.

Table A.1 repeats the analyses from Table 3 of the main paper with the following changes. First, we do not do county-border pair exercises.[2] We use all observations, including from interior counties. Second, we interact the event-study variables, $rights_{st}^{k}$ in Equation (1) of the main paper, with an indicator that the county was on a state border.

[2]Notice that this means that we do not include the parameters $\beta_{c,b(c)}$ or $\gamma_{c,b(c)}$ from Equation (1) of the main paper, nor do we duplicate observations in counties with more than one border. We also cluster standard errors only at the state level, rather than double cluster with county-border pairs.

Column 1 of Table A.1 repeats Column 1 of Table 3 of the main paper examining the impact of rights on the probability of giving birth without the "extra controls" for a husband's industry and occupation, while Column 2 includes these controls. Columns 3 and 4 repeat this pattern, but examining the number of children under age 5. In all specifications, there is no evidence of a pretrend in fertility prior to women's rights being granted, and an immediate and dynamic decrease in fertility after rights are granted. The estimates here are larger than their counterparts in Table 3 of the main paper. There is no economically meaningful or statistically significant differential impact of women's rights on border regions.

There is more than one possible way to interpret this finding. One could be that the exercise performed in the main paper does not capture spillovers from county border pairs, which lowers the estimated effect of women's rights.[3] Another possible interpretation is that the impact of rights is better identified in the exercise in the main paper: including county-border pairs presumably allows for a better controlled comparison and more reliable estimates, than a simple cross-state exercise performed here. It is not possible to know which of these hypotheses accounts for the larger estimates documented here.

A third possible explanation for the above findings is that the border counties are systematically different from interior counties.[4] Table A.2 reports the means and standard deviations for the main dependent and independent variables for this exercise by whether the woman lives in a county that is interior or on the border. We find no substantial differences in either the mean or standard deviation for any variable, suggesting that the counties are quite similar. These findings suggest that this third hypothesis is not likely.

Table A.3 does the same exercise for Table 5 of the main paper. That is, we repeat the analysis but include an interaction effect of being married after rights on areas that are on the border of a state. Here, we find that being in a border region reduces the impact of being married after rights by approximately one-third.

D.2 Robustness

We next turn to robustness analyses for our main exercises.

Table A.4 performs our main robustness exercises on the event study results reported in Table 3 of the main paper. Column 1 of Table A.4 repeats Column 1 of Table 3 of the main paper, but without the plausibly endogenous "extra controls" of the husband's occupation and industry.

[3]Notice that just including a dummy variable for being on a border would not capture this spillover: we need to include a county-border pair to know if the other side of the border has rights.

[4]This is as opposed to the discussion below in Appendix E which argues that there is substantial heterogeneity between counties within a state, and thus policy is plausibly exogenous to any given county. Here we argue that the border counties are not systematically different from interior counties, rather than argue that there is little heterogeneity among counties in general.

Table A.1: Cross-State and Border interacted: 1850-1920

	(1)	(2)	(3)	(4)
Dep. Var.	Birth Last Year		# Children Under 5	
$\geq$ 3 Decades Before	0.001	0.002	0.010	0.018
	(0.004)	(0.004)	(0.025)	(0.022)
2 Decades Before	0.000	0.000	0.005	0.006
	(0.003)	(0.003)	(0.019)	(0.019)
1 Decade Before	0	0	0	0
Rights Given	-0.008**	-0.008**	-0.038*	-0.037*
	(0.003)	(0.003)	(0.020)	(0.019)
1 Decade After	-0.016***	-0.016***	-0.080***	-0.082***
	(0.003)	(0.003)	(0.020)	(0.018)
2 Decades After	-0.019***	-0.018***	-0.097***	-0.090***
	(0.004)	(0.004)	(0.023)	(0.022)
$\geq$ 3 Decades After	-0.024***	-0.022***	-0.123***	-0.113***
	(0.005)	(0.004)	(0.027)	(0.024)
$\geq$ 3 Decades Before $\times$	0.002	0.000	0.009	0.000
Border County	(0.002)	(0.002)	(0.012)	(0.009)
2 Decades Before $\times$	-0.003	-0.003	-0.020	-0.019
Border County	(0.003)	(0.002)	(0.015)	(0.013)
1 Decade Before $\times$	-0.002	-0.002	-0.011	-0.009
Border County	(0.002)	(0.002)	(0.011)	(0.010)
Rights Given $\times$	-0.002	-0.002	-0.007	-0.006
Border County	(0.002)	(0.002)	(0.012)	(0.013)
1 Decade After $\times$	0.001	0.002	0.016	0.022*
Border County	(0.002)	(0.002)	(0.014)	(0.011)
2 Decades After $\times$	0.000	0.001	0.005	0.008
Border County	(0.003)	(0.002)	(0.016)	(0.011)
$\geq$ 3 Decades After $\times$	0.000	0.002	-0.002	0.008
Border County	(0.003)	(0.001)	(0.017)	(0.008)
N	26,568,152	26,568,086	26,568,152	26,568,086
Adjusted R^2	0.023	0.027	0.089	0.111
Mean Dep. Var.	0.1903	0.1903	1.1303	1.1303

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the state-year level, are in parentheses. All specifications include fixed effects for each spouse's age interacted with year fixed effects, state fixed effects, birthplace fixed effects, and state linear time trends. Columns 2 and 4 also include fixed effects for the husbands' occupation and industry, interacted with year fixed effects. The sample includes all US born women, age 20-39, with husbands up to age 50.

The estimates are virtually identical to their counterparts in the main analysis. Column 2 repeats Column 1 of Table 3 of the main paper, but drops all county-border pairs between states in the South and not in the South. Column 3 repeats this analysis, but instead drops all borders between community property and non-community property states.[5] Column 4 again repeats this analysis but restricts attention to women who stayed in the same state in which they are born. This has the advantage of improving the precision with which we know which marital rights regime applied to them, but the disadvantage of imposing a selected sample of people who did not migrate. In all of these specifications, the point estimates and standard deviations are remarkably similar to their counterparts in the main analysis. We conclude that our main analysis is robust to all of these potential concerns.

Column 5 performs a slightly different robustness exercise. Our event study analysis makes most sense if we assume that everyone in a treated county was married after rights were granted, as legally these were the people treated by the reform. In reality there were many people who were plausibly married after the reform in states that had recently given rights. Using the 1900 dataset, which asks women for duration of marriage and thus implies age of marriage, we calculate that the median age of marriage for women in our sample was 21. If we assume that any woman who was 21 or younger when rights was granted was married after the reform, while any woman over 21 was married before the reform we can plausibly do an event study where we include people married before the reform as part of our control. Column 5 does this by interacting our event study variables ($rights^k_{st}$ in Equation (1) of the main paper) with an indicator that the woman was 21 or younger at the time of the reform. The estimates in this specification are negative and statistically significant, but smaller than the benchmark exercise. We take this as an indication that the attenuation bias introduced by assuming women who were 21 or younger were not yet married (and those over 21 were married) when the reform passed is large.

To illustrate this point, we redo our main exercises analyzing the impact of being married after rights using the proxy of whether the woman was 21 or younger at the time rights were granted. Table A.5 reports our findings. All specifications find substantially lower estimates than when using the actual variable of whether a woman was married after rights (Table 5 of the main paper). For example, we find with the actual variable that the probability of a woman 20-39 giving birth declined by about 1 p.p. and that the number of kids under age 5 declined by 0.12 children, while the counterparts using the proxy variable are 0 p.p. and 0.07 children, respectively. Thus, attenuation bias dramatically affects our estimates of flow fertility. For

[5]Some states were technically not under coverture. The details of their restrictions of married women's rights were different, but qualitatively the idea that these states granting women rights increased women's bargaining power is the same. For a further discussion of the difference between community law states and other states see Hazan et al. (2019).

women age 45-59, we find that children ever born decreased by 0.19 children, or 0.18 children for women with at least one child, and that the number of surviving children declined by 0.15 using the actual married after rights variable. Using the proxy, these numbers are 0.06, 0.07, and 0.04, respectively. Again, attenuation bias reduces our estimates by approximately two-thirds. This is despite the fact that the variables of "married after rights" and "21 or under when rights were granted" have a correlation of about 0.9. That is, despite the high correlation, the identification of these estimates rely exactly on observations for which the measurement error introduced by assuming all women got married at the same age is large.

Table A.4 Columns 6-10 repeat the pattern of Columns 1-5 for the number of children under age 5 and finds very similar results to their counterparts in the main analysis.

Table A.6 performs robustness analysis on the results comparing people married before and after the reform reported in Table 5. We begin with Panel A. Column 1 repeats Column 1 of Table 5 except does not include the plausibly endogenous controls for a husband's industry and occupation. Column 2 instead drops states in the South. Column 3 instead drops community property states. Column 4 restricts attention to women who stayed in the same state in which they were born. Column 5 drops couples that were married a year before or after the reform. Panel B repeats this pattern for the number of kids under age 5. Panel C repeats this pattern for children ever born. Panel D repeats this pattern for surviving children. In all specifications the estimates are statistically significant and quantitatively similar to those of our main specifications. We conclude that our main results are robust.

Table A.7 breaks down our event studies by age of women. Columns 1-4 repeat Column 1 of Table 3 of the main paper looking at how rights affect the probability of giving birth for different subgroups. Column 1 restricts attention to women age 20-24, Column 2 to ages 25-29, Column 3 to ages 30-34, and Column 4 to ages 35-39. Columns 5-8 repeat this pattern for Column 3 of Table 3 of the main paper looking at how rights affect the number of kids under age 5 for different subgroups. The effect of rights on the probability of women giving birth ages 35-39 is somewhat lower than for all other ages, while the estimates for other ages are all quite similar to each other and to the main effect. There is no major difference when looking at the number of kids under age 5. These results are consistent with the idea that family sizes are largely set by the time women are age 35-39, when fertility rates naturally drop.

Table A.8 does a similar exercise for the exercises comparing people married before and after the reform. Column 1 repeats Column 1 of Table 5 of the main paper, but restricts attention to women age 20-24. Column 2 does this for ages 25-29, while Column 3 does so for 30-34 and Column 4 for 35-39. The largest magnitude effect is on women 20-24, however the statistical significance is only at the 10% level. The estimate on women 25-29 is not significant.

For women 30-34 and 35-39 the estimates are significant at the 1% level. We note that all estimates are within a standard error of the main estimate in Column 1 of Table 5 of the main table. Columns 5-8 repeat this pattern for the number of kids under age 5. Here, all estimates are statistically significant at the 1% level, and the estimates for women 25 and older are all roughly 0.05, which is what we would expect should the true effect be that women reduce the probability of giving birth by 1 p.p. The estimate for women 20-24 is about 3 times larger. This is consistent with the hypothesis expressed in the paper that there is perhaps an effect on delaying fertility. This could also be consistent with the larger reduction in the probability of giving birth for women 20-24.

Table A.9 repeats Columns 4 and 8 of Table 5 of the main paper exploring LFP of married women by age of the woman. Column 1 repeats Column 4 of Table 5 of the main paper, but restricts attention to women 20-24. Column 2 does so for women 25-29. Column 3 does so for women 30-34. Column 4 does so for women 35-39. Column 5 repeats Column 8 of Table 5 of the main paper, but restricts attention to women 40-44. Column 6 does so for women 45-49. Column 7 for women 50-54, and Column 8 for women 55-59. Only Column 1 finds an economically meaningful and statistically significant result of an increase in LFP of 1.3 p.p. This is consistent with women perhaps delaying their fertility a little bit and being in the labor force. However, given that the mean dependent variable is 3.3 p.p., an increase of 1.3 p.p. yields a grand total of 4.6 p.p. of the population working. It is unlikely that this accounts for the decline in fertility we document.

Another interesting exercise is to compare people who live in states without rights, but came from states with rights, with their neighbors. Table A.10 does exactly that. We restrict attention to states without women's rights, and we regress our main fertility variables on whether a woman was born in a state that currently has women's rights. Column 1 does so with whether a woman gave birth last year using our main controls on the county-border pair sample, while Column 2 adds in our "extra controls" of the husband's industry and occupation interacted with year fixed effects. Columns 3 and 4 repeat this pattern for the number of children under age 5. All estimates are statistically significant at the 1% level and quantitatively similar to our findings in the main paper. That is, it seems that migrants from states with rights have fewer children than their neighbors in states without rights. This is consistent with the fact that restricting attention to people who stayed in the state they were born, as reported above, yields similar estimates to not restricting the sample. Migrants do not behave differently, but women's rights matter.

Next, one can ask whether states that granted rights earlier than other states saw a differential impact. Table A.11 looks at the impact of rights on the probability of giving birth. Since we are looking differentially over our time frame, we do a difference in difference exercise, rather than event study, with the variable of interest being "Rights", or whether married women have economic rights. Column 1 does so on the entire sample, using all the controls as in the main exercise in Section 5.1 of the main paper. We find a point estimate of 0.6 p.p. effect of women's rights on the probability of giving birth, similar to the immediate effect of rights in Table 3 of the main paper. Column 2 then adds a variable "Late" for women being granted rights after 1890, as well as an interaction between "Rights" and "Late." Column 3 repeats Column 2, but redefines "Late" as 1873. We choose 1873 as it is the median date of women's rights and 1890 as it is in the middle of our time period.[6] Both specifications yield zero heterogeneous effects of women's rights over time.

Finally, it is natural to ask about heterogeneous effects by culture. While many aspects of local culture are hard to measure, presumably a household's status of having the husband work in agriculture captures much of this variation. Table A.12 reproduces our main results, and then breaks them down by whether a woman's husband works in agriculture. Specifically, Column 1 recreates Column 1 of Table 3 of the main paper. However, here we leave out controls for the husbands occupation and industry since we are selecting the sample on agricultural workers. Column 2 then redoes Column 1 on households where the husband's industry indicates that he is not in agriculture. Column 3 repeats this exercise for those who are in agriculture. Columns 4-6 repeat this exercise for the number of children under age five. Two aspects of the findings stand out. The first is that the point estimates are remarkably similar to one another (and those of Table 3 of the main paper). This indicates that cultural differences between agricultural workers and others probably are not a first order concern for our exercise. However, as is well known, fertility rates among agricultural workers was substantially higher (about 20% here). As such, it is possible to claim that agricultural workers saw a smaller percentage change in their fertility as a result of women's rights.

[6] Technically, 1885 would be the middle of our time period, but given our "round up" approach, 1890 is our midpoint.

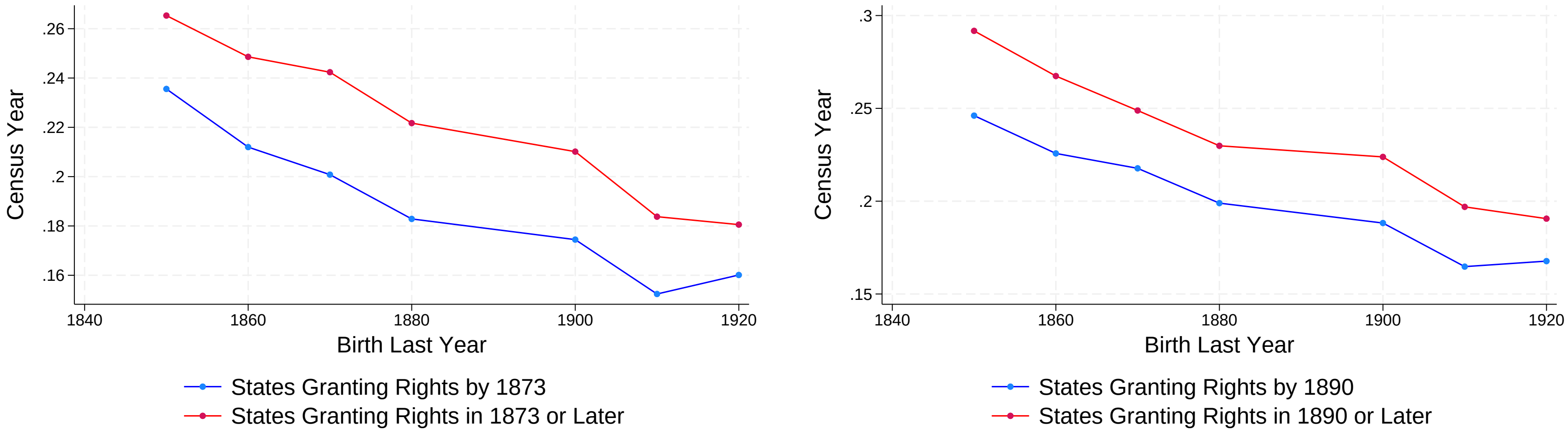

Figure A.10: **Fertility by Timing of Rights.** This figure shows the fertility rates of married white non-Hispanic women, 20-39 years old. The figures show the fraction of women who gave birth in the previous year over time by whether the woman was from a state that gave rights early or late. The left figure defines early as before 1873 while the right figure defines it as before 1890.

Table A.2: Mean and (Standard Deviation) by Year, Comparing Interior and Border Counties

	1850		1860		1870		1880		1900		1910		1920	
Border County	Interior	Border	Interior	Border	Interior	Border	Interior	Border	Interior	Border	Interior	Border	Interior	Border
	Women age 20-39													
Birth Last Year	0.257	0.249	0.237	0.228	0.222	0.217	0.204	0.200	0.195	0.192	0.169	0.168	0.168	0.170
	(0.437)	(0.432)	(0.425)	(0.420)	(0.416)	(0.412)	(0.403)	(0.400)	(0.396)	(0.394)	(0.375)	(0.374)	(0.374)	(0.376)
# Children < 5	1.469	1.421	1.407	1.358	1.261	1.236	1.264	1.247	1.121	1.109	1.061	1.055	0.977	0.990
	(1.035)	(1.033)	(1.025)	(1.019)	(0.995)	(0.997)	(1.024)	(1.024)	(1.030)	(1.028)	(1.012)	(1.010)	(0.984)	(0.985)
LFP			0.061	0.053	0.010	0.012	0.027	0.027	0.023	0.026	0.042	0.046	0.037	0.041
			(0.239)	(0.223)	(0.100)	(0.109)	(0.161)	(0.162)	(0.150)	(0.158)	(0.201)	(0.210)	(0.190)	(0.199)
Age	28.97	29.09	28.92	29.04	28.92	29.01	28.98	29.05	29.56	29.61	29.63	29.68	29.84	29.83
	(5.451)	(5.452)	(5.438)	(5.446)	(5.446)	(5.438)	(5.428)	(5.421)	(5.427)	(5.416)	(5.416)	(5.424)	(5.373)	(5.371)
Spouse's Age	33.54	33.59	33.77	33.81	33.87	33.84	33.69	33.61	34.23	34.16	34.00	34.01	33.99	33.99
	(6.675)	(6.657)	(6.685)	(6.709)	(6.929)	(6.939)	(6.736)	(6.742)	(6.695)	(6.676)	(6.676)	(6.676)	(6.647)	(6.656)
N	806,251	633,320	1,046,749	773,338	1,282,904	917,757	1,820,576	1,279,620	2,731,616	1,959,472	3,487,503	2,483,681	4,326,564	3,018,801

Notes: Summary statistics for our main dependent and independent variables by year and whether the woman lived in a border county or an interior county. The sample is married white non-Hispanic women 20-39 years old with husbands up to 50 years old.

Table A.3: Fertility, Married After Rights 1900-1910 – Potential Spillovers at the Border

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep Var:	Birth Last Year	#Children Under 5	LFP	CEB	CEB>0 Children	Surviving Ratio	Surviving	LFP
	Women Age 20-39			Women Age 45-59				
Married After Rights	-0.012***	-0.132***	-0.000	-0.217***	-0.212***	-0.168***	-0.000	-0.000
	(0.003)	(0.025)	(0.001)	(0.060)	(0.057)	(0.050)	(0.001)	(0.001)
Married After Rights×Border	0.006**	0.031***	0.001	0.065**	0.068***	0.049**	-0.000	0.001
	(0.002)	(0.011)	(0.001)	(0.026)	(0.024)	(0.021)	(0.001)	(0.001)
N	10,040,159	10,040,159	10,040,159	3,485,362	3,191,435	3,485,362	3,191,435	3,485,362
Adj. R^2	0.052	0.201	0.053	0.261	0.234	0.242	0.027	0.035
Mean Dep. Var.	0.180	1.072	0.034	4.844	5.290	3.796	0.807	0.036

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level are in parentheses. All specifications include county-year fixed effects, wife's age and husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects interacted with year fixed effects. They also include husband's occupation and husband's industry fixed effects, interacted with year fixed effects and birthplace fixed effects. Border is a binary indicator for whether a county shares a border with another state.

Table A.4: Fertility Robustness 1850-1920

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var.	Birth Last Year					# Children Under 5				
	No Extra	No South	No CP	Stayers	21	No Extra	No South	No CP	Stayers	21
≥ 3 Decades Before	0.002	0.002	0.002	0.001	0.001	0.010	0.017	0.011	0.008	0.004
	(0.003)	(0.003)	(0.003)	(0.005)	(0.003)	(0.015)	(0.013)	(0.013)	(0.020)	(0.013)
2 Decades Before	-0.001	-0.001	-0.001	-0.001	0.000	-0.007	-0.006	-0.007	-0.014	-0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.012)	(0.012)	(0.011)	(0.016)	(0.011)
1 Decade Before	0	0	0	0	0	0	0	0	0	0
Rights Given	-0.006***	-0.006***	-0.006***	-0.007***		-0.028***	-0.026***	-0.027***	-0.029*	
	(0.002)	(0.002)	(0.002)	(0.002)		(0.010)	(0.010)	(0.010)	(0.016)	
1 Decade After	-0.013***	-0.013***	-0.012***	-0.011***		-0.053***	-0.059***	-0.051***	-0.040**	
	(0.003)	(0.003)	(0.003)	(0.004)		(0.014)	(0.012)	(0.013)	(0.017)	
2 Decades After	-0.012***	-0.013***	-0.011***	-0.012***		-0.062***	-0.065***	-0.058***	-0.059***	
	(0.003)	(0.003)	(0.003)	(0.004)		(0.017)	(0.016)	(0.018)	(0.021)	
≥ 3 Decades After	-0.015***	-0.016***	-0.013***	-0.015***		-0.084***	-0.084***	-0.076***	-0.081***	
	(0.003)	(0.003)	(0.003)	(0.004)		(0.019)	(0.018)	(0.020)	(0.023)	
Rights Given×21					-0.003					-0.007
					(0.002)					(0.009)
1 Decade After×21					-0.007***					-0.022***
					(0.002)					(0.008)
2 Decades After×21					-0.005**					-0.027**
					(0.002)					(0.012)
≥ 3 Decades After×21					-0.006**					-0.037**
					(0.003)					(0.015)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
N	21,973,593	17,926,262	20,624,476	14,438,955	21,973,593	21,973,593	17,926,262	20,624,476	14,438,955	21,973,593
Adj. R^2	0.026	0.029	0.028	0.027	0.028	0.110	0.125	0.123	0.120	0.123
Mean Dep. Var.	0.198	0.194	0.198	0.201	0.198	1.173	1.152	1.174	1.186	1.173

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are double clustered at the county-border pair and state levels, in parentheses. Controls include county-border pair fixed effects and county-border pair linear time trend, fixed effects for both the wife's and husband's ages, interacted with year fixed effects, as well as state fixed effects and birthplace fixed effects. Extra control include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. "21" is a dummy variable equal to 1 if a woman was aged 21 or younger when rights were granted.

Table A.5: Fertility, 21 or Under At Rights, 1900-1910

	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var:	Birth Last Year	# Children Under 5	CEB	CEB>0	Surviving Children	Surviving Ratio
	Women Age 20-39		Women Age 45-59			
21 or Under At Rights	-0.002	-0.069***	-0.061**	-0.071**	-0.038**	0.000
	(0.002)	(0.024)	(0.025)	(0.027)	(0.019)	(0.001)
N	10,040,159	10,040,159	3,485,362	3,191,435	3,485,362	3,191,435
Adj. R^2	0.052	0.201	0.261	0.234	0.242	0.027
Mean Dep Var	0.180	1.072	4.844	5.290	3.796	0.807

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level are in parentheses. All specifications include county-year fixed effects, wife's age and husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects interacted with year fixed effects. They also include husband's occupation and husband's industry fixed effects, interacted with year fixed effects, and birthplace fixed effects. Our variable of interest is whether a woman was 21 or younger when rights were granted.

Table A.6: Robustness: Fertility, Married After Rights, 1900-1910

	(1)	(2)	(3)	(4)	(5)
Sample	All	No South	No CP	Stayers	Donut
	Panel A: Dep Var: Birth Last year, age 20-39				
Married After	-0.010***	-0.010**	-0.009***	-0.009***	-0.010***
Rights	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)
N	10,040,168	7,171,256	9,060,253	7,233,579	9,969,410
Adj. R^2	0.050	0.051	0.052	0.052	0.052
Mean Dep. Var.	0.180	0.164	0.179	0.186	0.180
	Panel B: Dep Var: # of Kids Under Age 5, age 20-39				
Married After	-0.121***	-0.114**	-0.121***	-0.137***	-0.132***
Rights	(0.024)	(0.051)	(0.027)	(0.039)	(0.026)
N	10,040,168	7,171,256	9,060,253	7,233,579	9,969,410
Adj. R^2	0.187	0.178	0.197	0.0202	0.200
Mean Dep. Var.	1.072	0.975	1.065	1.100	1.071
	Panel C: Dep Var: Children Ever Born, age 45-59				
Married After	-0.186***	-0.155***	-0.202***	-0.226**	-0.238***
Rights	(0.058)	(0.055)	(0.062)	(0.100)	(0.071)
N	3,485,383	2,537,399	3,212,896	2,203,297	3,260,133
Adj. R^2	0.240	0.218	0.254	0.263	0.261
Mean Dep. Var.	4.844	4.391	4.789	4.840	4.777
	Panel D: Dep Var: Surviving Children, age 45-59				
Married After	-0.146***	-0.104**	-0.156***	-0.174***	-0.184***
Rights	(0.049)	(0.045)	(0.052)	(0.083)	(0.059)
N	3,485,383	2,537,399	3,212,896	2,203,297	3,260,133
Adj. R^2	0.222	0.201	0.236	0.244	0.243
Mean Dep. Var.	3.796	3.447	3.756	3.805	3.745

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level are in parentheses. All specifications include county-year fixed effects, wife's age and husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects interacted with year fixed effects. They also include birthplace fixed effects. Columns (2)-(6) also include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. The sample in Panel A includes all U.S. born, age 20-39, married to men up to age 50. The sample in Panel B includes all U.S. born, age 45-59, married to men up to age 70. The "Donut" sample excludes women who were married one year before, during, or one year after the year rights were granted.

Table A.7: Fertility by Age Groups, Event Study 1850-1920

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.	Birth Last Year				# Children under 5			
Age Group	20-24	25-29	30-34	35-39	20-24	25-29	30-34	35-39
$\geq$ 3 Decades Before	-0.003	0.006*	0.004	0.002	0.000	0.017	0.012	0.012
	(0.003)	(0.003)	(0.004)	(0.003)	(0.012)	(0.016)	(0.014)	(0.015)
2 Decades Before	-0.006**	0.001	0.000	0.002	-0.015	-0.007	-0.004	0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.012)	(0.013)	(0.012)	(0.012)
1 Decade Before	0	0	0	0	0	0	0	0
Rights Given	-0.008***	-0.007***	-0.007***	-0.004*	-0.022**	-0.029**	-0.033***	-0.032***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.010)	(0.011)	(0.012)	(0.011)
1 Decade After	-0.014***	-0.013***	-0.013***	-0.010***	-0.035***	-0.055***	-0.061***	-0.066***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.011)	(0.015)	(0.016)	(0.014)
2 Decades After	-0.013***	-0.014***	-0.013***	-0.008**	-0.037***	-0.063***	-0.079***	-0.077***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.014)	(0.017)	(0.023)	(0.022)
$\geq$ 3 Decades After	-0.013***	-0.016***	-0.017***	-0.011***	-0.044***	-0.082***	-0.103***	-0.096***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.016)	(0.019)	(0.025)	(0.025)
N	4,892,141	6,276,676	5,781,700	5,023,076	4,892,141	6,276,676	5,781,700	5,023,076
Adj. R^2	0.013	0.017	0.023	0.028	0.112	0.096	0.113	0.130
Mean Dep. Var.	0.247	0.223	0.178	0.137	1.129	1.349	1.195	0.967

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are double clustered at the county-border pair and state levels, in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend, fixed effects for both the wife's and husband's ages, interacted with year fixed effects, as well as husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Additionally they include state fixed effects and birthplace fixed effects.

Table A.8: Fertility by Age Groups, Married After Rights, 1900-1910

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.	Birth Last Year				# Children under 5			
Age Group	20-24	25-29	30-34	35-39	20-24	25-29	30-34	35-39
Married After Rights	-0.013*	-0.006	-0.008***	-0.007***	-0.142***	-0.069***	-0.058***	-0.044***
	(0.007)	(0.004)	(0.003)	(0.002)	(0.046)	(0.016)	(0.011)	(0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,075,074	2,858,910	2,718,239	2,843,723	2,075,074	2,858,910	2,718,239	2,843,723
Adj. R^2	0.075	0.033	0.025	0.025	0.363	0.206	0.130	0.120
Mean Dep. Var.	0.241	0.206	0.160	0.112	1.033	1.258	1.094	0.825

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level are in parentheses. All specifications include county-year fixed effects, wife's age and husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects interacted with year fixed effects. They also include husband's occupation and husband's industry fixed effects, interacted with year fixed effects and birthplace fixed effects.

Table A.9: LFP by Age Groups, Married After Rights, 1900-1910

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.	Labor Force Participation							
Age Group	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59
Married After Rights	0.013***	0.002	0.001	0.002	0.003*	0.000	0.002***	-0.000
	(0.004)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,087,047	2,892,270	2,797,964	2,565,698	2,118,831	1,658,838	1,148,226	783,103
Adj. R^2	0.075	0.059	0.048	0.041	0.037	0.036	0.035	0.035
Mean Dep. Var.	0.033	0.033	0.035	0.039	0.039	0.039	0.035	0.031

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level are in parentheses. All specifications include county-year fixed effects, wife's age and husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects interacted with year fixed effects. They also include husband's occupation and husband's industry fixed effects, interacted with year fixed effects and birthplace fixed effects.

Table A.10: States without Rights, including Migrants from States with Rights

	(1)	(2)	(3)	(4)
Dep. Var.	Birth Last Year		# Children under 5	
Rights Origin	-0.007***	-0.005***	-0.042***	-0.027***
	(0.002)	(0.001)	(0.008)	(0.007)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
N	4,783,413	4,783,373	4,783,413	4,783,373
Adj. R^2	0.020	0.022	0.107	0.117
Mean Dep. Var.	0.231	0.231	1.357	1.357

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are double clustered at the county-border pair and state levels, in parentheses. Rights Origin is a dummy variable equal to 1 if married women had rights in the state of origin during the census year, and 0 otherwise. Controls include county-border pair fixed effects and county-border pair linear time trend, fixed effects for both the wife's and husband's ages, interacted with year fixed effects. Extra controls include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. All specifications include state fixed effects, birthplace fixed effects, and year fixed effects. The sample consists of all married, non-Hispanic white women ages 20-39, born in the U.S. and residing in counties bordering adjacent states.

Table A.11: Fertility 1850-1920, by Timing of Rights

	(1)	(2)	(3)
Dep. Var.		Birth Last Year	
Rights	-0.006***	-0.007***	-0.009***
	(0.002)	(0.002)	(0.002)
Late		0.000	0.000
		(0.000)	(0.000)
Rights×Late		0.001	0.004
		(0.004)	(0.003)
Late Year Cutoff		1890	1873
# of Late Obs		2,282,872	11,302,279
N	21,973,593	21,973,593	21,973,593
Adj. R^2	0.028	0.028	0.028
Mean	0.198	0.198	0.198

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are double clustered at the county-border pair and state levels, in parentheses. "Late" is a dummy variable that is equal to 1 if rights were granted in or after 1890 (1873) in column 2 (3), and 0 otherwise. All specifications include county-border pair fixed effects and county-border pair linear time trend, fixed effects for both the wife's and husband's ages, interacted with year fixed effects, as well as husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Additionally they include state fixed effects, birthplace fixed effects, and year fixed effects. The sample includes all married, white non-Hispanic women born in the US.

Table A.12: Fertility, 1850-1920. Agriculture/Non Agriculture.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable		Birth Last Year			# Children Under 5	
$\geq$ 3 Decades Before	0.002	0.002	-0.000	0.010	0.009	-0.006
	(0.003)	(0.003)	(0.004)	(0.015)	(0.016)	(0.013)
2 Decades Before	-0.001	-0.002	-0.001	-0.007	-0.003	-0.019
	(0.002)	(0.002)	(0.002)	(0.012)	(0.010)	(0.014)
1 Decade Before	0	0	0	0	0	0
Rights Given	-0.006***	-0.006***	-0.005**	-0.028***	-0.028**	-0.021
	(0.002)	(0.002)	(0.002)	(0.010)	(0.011)	(0.013)
1 Decade After	-0.013***	-0.012***	-0.010***	-0.053***	-0.050**	-0.038***
	(0.003)	(0.004)	(0.003)	(0.014)	(0.020)	(0.013)
2 Decades After	-0.012***	-0.011**	-0.010***	-0.062***	-0.054**	-0.046**
	(0.003)	(0.005)	(0.004)	(0.017)	(0.028)	(0.021)
$\geq$ 3 Decades After	-0.015***	-0.012**	-0.013***	-0.084***	-0.074**	-0.062***
	(0.003)	(0.005)	(0.004)	(0.019)	(0.032)	(0.019)
Sample	All	Non-Agri	Agri	All	Non-Agri	Agri
N	21,973,593	13,365,296	8,608,297	21,973,593	13,365,296	8,608,297
Adj. R^2	0.026	0.025	0.019	0.110	0.089	0.098
Mean Dep Var	0.198	0.173	0.233	1.173	1.025	1.384

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are double clustered at the county-border pair and state levels, in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend, fixed effects for both the wife's and husband's ages, interacted with year fixed effects. Additionally they include state fixed effects, birthplace fixed effects, and year fixed effects. The sample includes all married, white non-Hispanic women born in the US. The first column uses the probability of giving birth as the dependent variable and includes the entire sample, while the second (third) column restricts attention to women who's husbands worked in non-agriculture (agriculture). Columns 4-6 repeat the same pattern, but switch the dependent variable to the number of children under the age of 5.

E County Heterogeneity Within State

As discussed in Section 4.3 of the main paper, state-level law changes are plausibly exogenous to individual counties in the state, allowing our event-study exercise to capture the causal effects of women's rights on households. However, this argument is invalid if all the counties within a state are similar. If this is the case, then state legislatures pass laws that all counties "agree" on, and reverse causality becomes a concern. In this appendix, we address this concern by studying heterogeneity within states during our sample period.

Specifically, for each year, we calculate our main flow fertility measures in our sample for each county in each state. We then regress these averages on state fixed effects and report the R^2 and adjusted R^2. These measures reflect how much of the county-level heterogeneity can be accounted for by states.

Table A.13 reports the results. Panel A reports the results when the dependent variable is the probability a woman gave birth in the previous year. The number of counties in the sample increases from 1,612 to 3,063 over the course of the sample. The R^2 (adjusted R^2) ranges from about 0.2 (0.18) to 0.46 (0.44), suggesting that about 54-80% of variation between counties cannot be explained by state fixed effects. Panel B repeats this exercise for the number of children under age 5 and finds that the R^2 (adjusted R^2) ranges from about 0.40 (0.39) to 0.57 (0.56), suggesting that about 43-61% of variation between counties cannot be explained by state fixed effects.

Table A.13: R^2 and Adjusted R^2: Regressing County level outcome on State Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	1850	1860	1870	1880	1900	1910	1920
				Panel A: Birth Last Year			
N	1,612	2,042	2,257	2,529	2,804	2,959	3,065
R^2	0.2936	0.1976	0.2257	0.2437	0.4613	0.4480	0.3513
Adjusted R^2	0.2774	0.1811	0.2092	0.2294	0.4519	0.4386	0.3410
				Panel B: # of Kids Under Age 5			
N	1,612	2,042	2,257	2,529	2,804	2,959	3,065
R^2	0.4958	0.4214	0.4020	0.5384	0.5722	0.5719	0.4603
Adjusted R^2	0.4843	0.4096	0.3893	0.5296	0.5647	0.5645	0.4517

Since the R^2 and adjusted R^2 for these exercises are low, we conclude that there is substantial heterogeneity between counties within states in our sample. Thus, it is reasonable to conclude that state policies are exogenous to individual counties within our sample.

F Randomization Exercises

This appendix documents the randomization exercises discussed in the main paper. In these exercises, we do the following. (1) Randomly assign a date that each state granted women rights. The date is uniformly drawn between 1850 and 1920. (2) Rerun our estimation procedure on these fake dates of women's rights. (3) Repeat steps one and two 1,000 times.

The idea behind the exercise is twofold. First, if the estimates from these randomized regressions are centered at zero, it suggests that there is no bias in our regression specifications. Second, the percent of these estimates that give a larger effect of women's rights gives a second type of "p-value". This p-value can be interpreted as a measure of how unlikely a random set of years would be to generate our results, and thus indicates the significance of the actual years in which women were granted rights in the US.

Table A.14 documents the results of these exercises when looking at the event-study approach to fertility, as discussed in Section 5.1 of the main paper. In particular, we randomize the main specifications of Tables 3 of the main paper, though for computational reasons we remove the controls for the husband's industry and occupation.[7] The results of these exercises for the probability of giving birth and the number of kids under age 5 are reported in Panels A and B, respectively. Columns 1-6 report the mean, standard deviation, minimum value, maximum value, the value from the regression on the actual dates ("Our Estimate"), and the fraction of random regressions that gave a larger estimate than our estimate ("p-value"), in absolute value.

Beginning with Panel A, the randomized sample of all of our outcome variables (estimates on 3 decades before rights through 3 decades after rights) are centered at zero, and have minimum and maximum values approximately 3 standard deviations from zero. We conclude that the specification of the relevant regression is not biased. The "p-value" on estimates before rights is high: 68.9% for three decades before rights and 39.2% for 2 decades before rights. This reinforces the notion that there was no detectable pre-trend in fertility prior to rights being granted. The "p-value" for the impact of rights is 1.2%, one decade after rights is 0%, two decades after rights is 1.1% and three decades after rights is 0%. Thus we conclude that it is highly unlikely that a random set of dates would generate results similar to those we document in this paper.

Turning to Panel B, again the randomized sample of estimates is centered on zero, with the minimum and maximum approximately 3 standard deviations from zero. Again our estimates

[7] We do so as these exercises are computationally demanding. Having fewer control variables substantially speeds up the computational process. Technically, this specification is as in Column 1 of Table A.4. Given how similar these findings are to those in Table 3 of the main paper, it is likely that these randomization exercises would yield similar results if performed on other specifications.

Table A.14: Event Study Randomization Exercise, Birth & # of Kids Under 5, 1850-1920

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Dependent Variable is Birth					
	Mean	Std. Dev.	Min	Max	"Our Estimate"	*p-value*
$\geq$ 3 Decades Before	0.000	0.004	-0.013	0.012	0.002	0.689
2 Decades Before	-0.000	0.003	-0.010	0.009	-0.001	0.392
Right Given	- 0.000	0.003	-0.009	0.008	-0.006	0.012
1 Decade After	-0.000	0.004	-0.012	0.013	-0.013	0.000
2 Decades After	-0.000	0.005	-0.015	0.017	-0.012	0.011
$\geq$ 3 Decades After	-0.000	0.007	-0.023	0.023	-0.015	0.000
	Panel B: Dependent Variable is # of Kids Under 5					
	Mean	Std. Dev.	Min	Max	"Our Estimate"	*p-value*
$\geq$ 3 Decades Before	0.001	0.020	-0.067	0.055	0.010	0.677
2 Decades Before	-0.000	0.013	-0.037	0.038	-0.007	0.277
Right Given	-0.000	0.014	-0.044	0.045	-0.028	0.023
1 Decade After	-0.001	0.021	-0.059	0.056	-0.053	0.005
2 Decades After	-0.001	0.026	-0.081	0.077	-0.062	0.007
$\geq$ 3 Decades After	-0.002	0.036	-0.105	0.100	-0.084	0.011

Notes: Distribution of 1,000 estimates on randomly assigned dates between 1850 and 1920 that each state gave rights, and rerun the estimates from Columns (1) and (3) in Table 3 of the main paper. "Our estimate" is the estimated parameter value using the dates that women were actually granted rights. *p-value* is the fraction of estimates in the randomization exercise that are equal or smaller than "our estimate".

have large p-values prior to rights being granted (67.7% 3 decades before rights and 27.7% 2 decades before rights), and low p-values after rights have been granted (2.3% when rights are granted, 0.5% a decade later, 0.7% two decades later, and 1.1% three decades later). We thus again conclude that our regression specification is not biased, and that it is highly unlikely that a random set of dates would generate results similar to those we document in this paper.

Table A.15 does a similar set of exercises on the regressions estimating the effect of being married after rights, as in Section 5.3 of the main paper.

We note that all of the distributions of the measures of fertility are centered at zero and have minimum and maximum values about 2-3 standard deviations around zero. The p-value of our estimates for the probability a woman gave birth last year, the number of kids under 5, children ever born, and the number of surviving children are 1.5%, 0.1%, 1%, and 0.9%, respectively. Accordingly, we conclude that our regression specifications are not biased, and that it is highly unlikely that a random set of dates would generate results similar to those we document in this paper.

Table A.15: Randomization Exercise, Married After Rights: Fertility

	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Std. Dev.	Min	Max	"Our Estimate"	*p-value*
Birth	-0.000	0.004	-0.013	0.010	-0.009	0.015
# of Kids Under 5	-0.003	0.034	-0.119	0.083	-0.116	0.001
Children Ever Born	-0.001	0.086	-0.234	0.301	-0.189	0.010
Surviving Children	-0.000	0.068	-0.180	0.271	-0.147	0.009

Notes: Distribution of 1,000 estimates on randomly assigned dates that each state gave rights. It reruns the estimates from Columns (1), (2), (4), and (6) in Table 5 of the main paper. "Our estimate" is the estimated parameter value using the dates that women were actually granted rights. *p-value* is the fraction of estimates in the randomization exercise that are equal or smaller than "our estimate".

G Block Bootstrap Standard Errors

In this appendix, we describe our block-bootstrapping procedure for calculating standard errors in our two step estimators.

Our process is as follows. (1) Randomly draw, with replacement, a set of 47 states. (2) Keep all counties on the borders between these states and their neighbors. If a state is drawn more than once, it receives a new fixed effect every time it is drawn. (3) Rerun our two-step estimator. (4) Repeat steps 1-3 two hundred times. The standard error reported is the standard deviation of the estimates across these 200 estimates.

H Extra

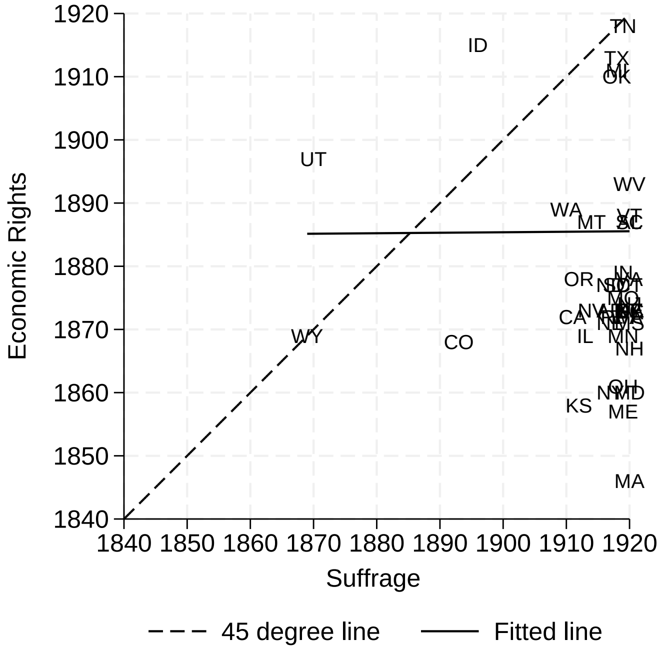

Figure A.11: **Women's economic rights and suffrage.**

We begin with extra figures relating to our discussion on the timing of rights. Figure A.11 compares the timing of women's rights by state with the timing of women's suffrage. We find no relationship, suggesting that feminism was not a driving force behind women's rights, as discussed in Section 3.3 of the main paper.

We continue by showing that people did not time their marriages around when rights were given. In Figure A.12, we provide evidence that couples did not time their marriage around the granting of women's rights. In particular, the top-left panel shows the fraction of people getting married relative to the year their state gave rights in the 1900 US census, when limited to white non-Hispanic couples where the wife is 20-39 years old. The top-right panel does the

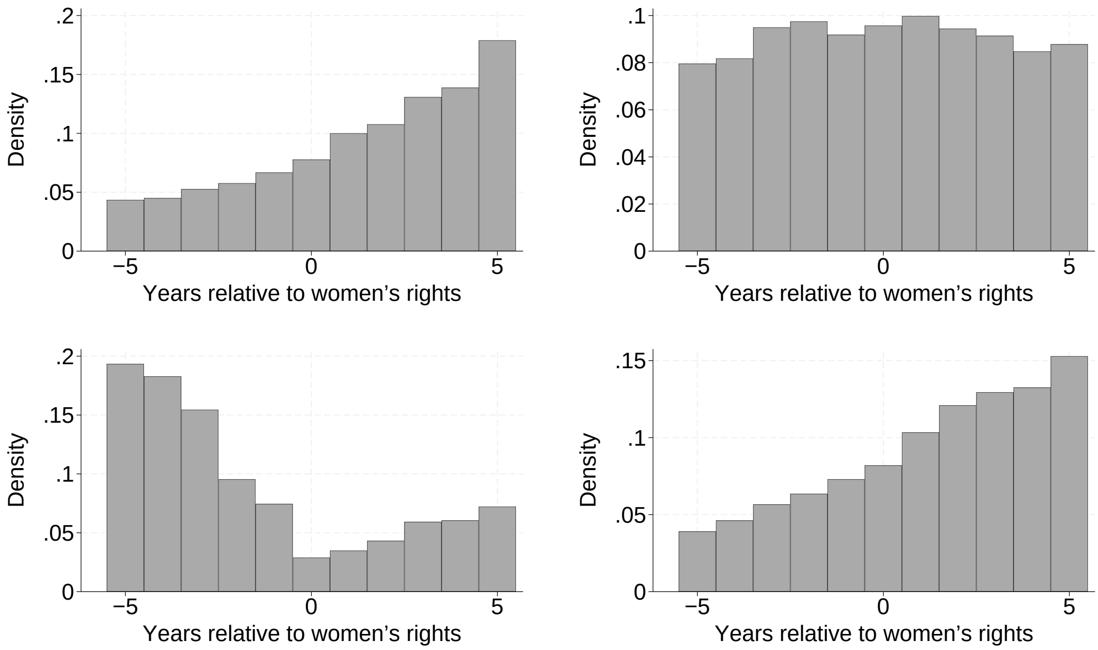

Figure A.12: The top left panel shows the number of people getting married, relative to the year their state gave rights, in the 1900 US census, when limiting to white non-Hispanic couples, where the wife is 20-39 years old. The top right panel does the same for couples where the wife is 45-59. The bottom left and bottom right repeat this patter using the 1910 census.

same for couples where the wife is 45-59. The bottom-left and bottom-right panels repeat this pattern using the 1910 census. In all cases, except for couples where the wife is 20-39 years old in 1910, there is clearly no break in the data around the year a state gave rights, nor is there any bunching behavior. The 1910 data for couples where the wife is 20-39 in 1910 is noisy, and thus harder to interpret. This is due to the small sample of states that gave rights in the relevant time frame.[8]

Turning to our fertility exercises, we next show figures illustrating the results of our event study analyses. Figure A.13 visualizes the results of the exercise described in Section 5.3 estimating how the parity of households changed with the granting of rights. As can be seen, the probability that a household has 1-6 children increased, while the probability that a household had 7-15 children decreased. Finally, Figure A.14 shows the raw data on children ever born (left figure) and surviving children (right figure) by whether or not the parents were married before rights were granted. As can be seen, those married after rights were granted have lower completed fertility rates, by both measures. This figure motives the analysis of these measures of completed fertility in Section 5.3 of the main paper.

[8]Assuming that couples got married between ages 20 and 40, there were only 2 (small) states that gave rights in the relative time period prior to 1910: West Virginia (1893) and Utah (1897).

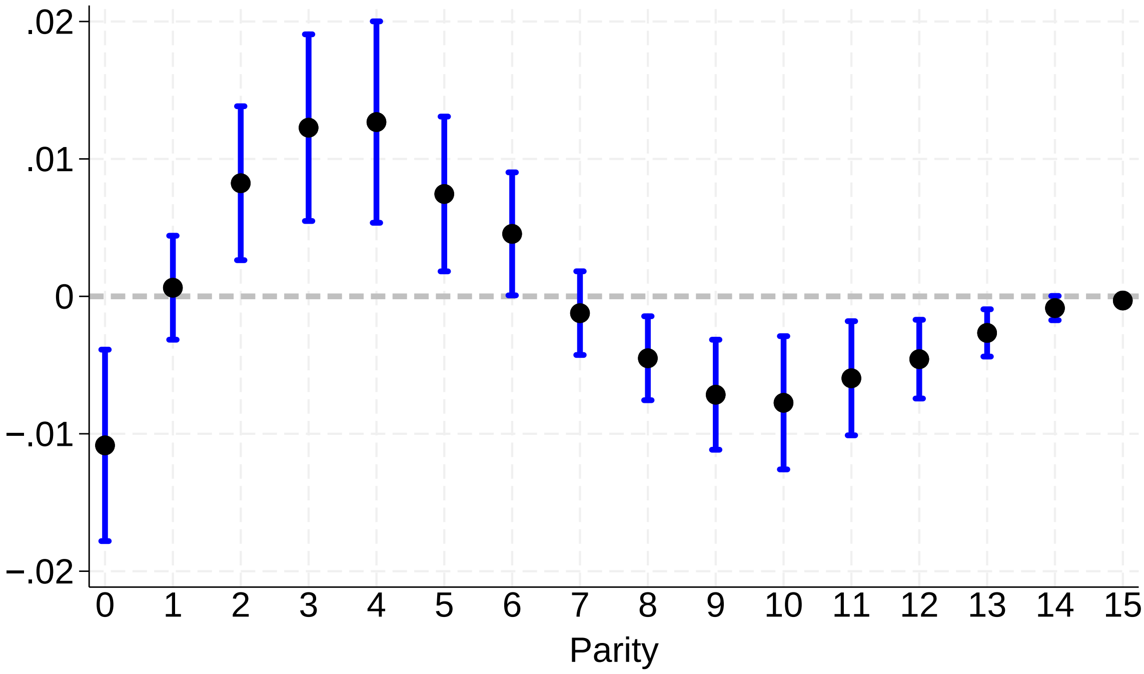

Figure A.13: **Changes in Parity** The X-axis is the parity of completed fertility, measured by children ever born, for women age 45-59 in 1900 and 1910. The Y-axis measures the difference in the fraction of households of a given parity for those married after rights as compared to those married before rights, after controls, as described in Section 5.3 of the main paper. For those married after rights, there is a rise in smaller households and a decline in larger households.

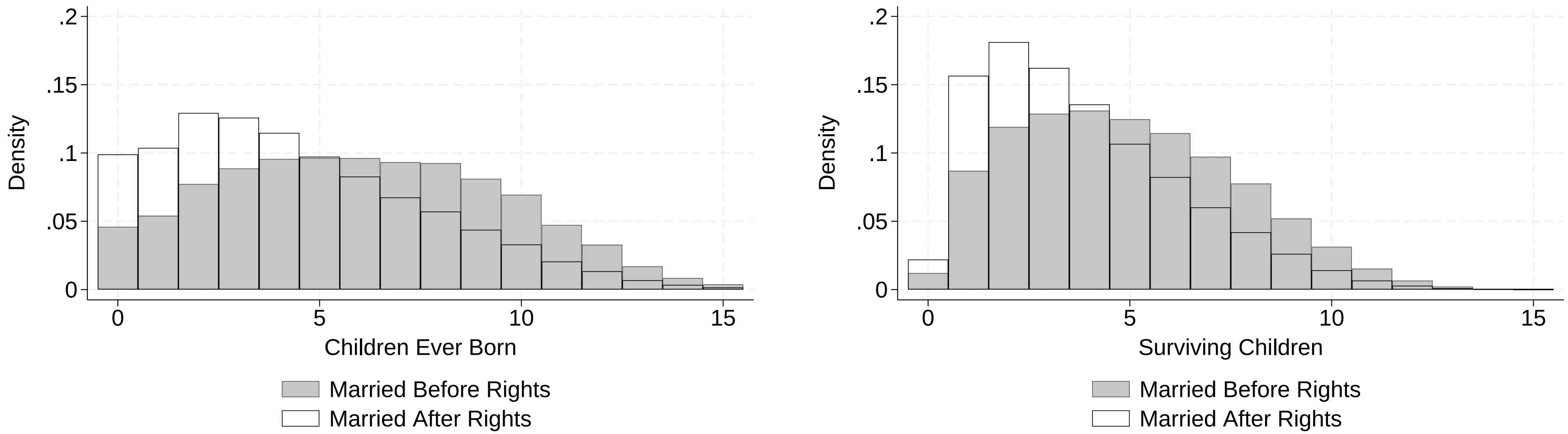

Figure A.14: The left panel plots the density of children ever born to white, non-Hispanic married women age 45-59 in 1900 and 1910. The plot is done separately by whether these women were married with economic rights or not. The right panel repeats this exercise for the number of surviving children.

I Theoretical Model

Here we lay out a simple cooperative household model with two spouses (a wife and a husband) who decide jointly on the number of children and their individual private consumptions. The key feature of our setup is that the wife bears an additional health cost (or risk) from childbearing, which we model as a negative term in her utility from having children. Our main goal is to analyze a Nash bargaining framework, where the disagreement point in bargaining is a breakdown of cooperation (Lundberg and Pollak, 1993). However, our approach to doing so is to first solve a Pareto problem, and then show equivalence between that Pareto problem and the Nash problem. Our approach is thus in the spirit of Greenwood et al. (2017); Doepke and Kindermann (2019).

That is, our approach is to solve the model under two frameworks:

1. A *collective (Pareto)* approach, in which a parameter μ directly captures the wife's weight in the household's maximand.
2. A *Nash-bargaining* approach, in which the outside option of each spouse depends on exogenously given shares of household wealth.

We show that in both frameworks, *raising the wife's bargaining power* (i.e. increasing μ or improving her outside option) *reduces the equilibrium number of children*, provided that the wife's effective valuation of children (net of her health cost) is lower than the husband's valuation.

In addition, the model predicts that this negative relationship between women's bargaining power and fertility is *stronger* in contexts with higher maternal mortality or morbidity risks (i.e. larger health costs). Similarly, we allow the health cost function $h(\cdot)$ to increase with household wealth W, implying that wealthier women value life more. This gives that the fertility-reducing effect of women's empowerment is magnified for wealthier women.

These predictions directly inform our main empirical findings: we show that legal changes that grant women more rights (and hence increase their bargaining power) lead to fewer children, *especially* in places with higher maternal mortality risk and among higher-wealth households as shown in Section 6.1 of the main text.

I.1 The Setup

Consider a household composed of two individuals: a wife (denoted by f) and a husband (denoted by m). Their total household wealth (or income) is exogenously given by W. The price of one unit of the public good (children) is p. Each individual's private consumption is c_i, for $i \in \{f, m\}$.

Preferences. Each individual derives utility from private consumption and from the number of children n, a public good. We allow for the possibility that bearing children imposes a higher health cost on the wife. Formally, we assume

$$U_f = \ln(c_f) + \big(v_f - h(W)\big)\ \ln(n), \quad U_m = \ln(c_m) + v_m\ \ln(n).$$

Here, v_f and v_m capture how each spouse values children, while $h(W) \geq 0$ captures the additional disutility (or risk) borne by the wife. For instance, a larger $h(W)$ can represent higher morbidity/mortality risk from childbirth. We assume $h(\cdot)$ is weakly increasing in W, reflecting that wealthier women may value quality of life (and thus the adverse health consequences of childbirth) more strongly.

Budget Constraint. The household faces a single budget constraint:

$$c_f + c_m + p\,n \;=\; W.$$

All allocations (c_f, c_m, n) must satisfy this feasibility constraint.

I.2 Pareto (Collective) Household Framework

We begin by characterizing the solution under a collective (Pareto) model, in which the household solves

$$\max_{c_f, c_m, n}\ \mu\, U_f \;+\; (1-\mu)\, U_m \quad \text{subject to} \quad c_f + c_m + p\,n = W,$$

where $\mu \in (0,1)$ is the bargaining weight of the wife. One can interpret μ as capturing how much the wife's preferences matter in the joint decision—a higher μ means greater influence for the wife.

I.2.1 First-Order Conditions and Optimal Allocation

Step 1: Substitute the budget constraint. Since $c_m = W - p\,n - c_f$, the maximization problem becomes:

$$\max_{c_f,\, n}\ \Big\{\mu\big[\ln(c_f) + (v_f - h(W))\ln(n)\big] \;+\; (1-\mu)\big[\ln\big(W - p\,n - c_f\big) + v_m\ \ln(n)\big]\Big\}.$$

Step 2: Take partial derivatives with respect to c_f and n.

$$\frac{\partial}{\partial c_f}\big(\mu\, U_f + (1-\mu)\, U_m\big) \;=\; \mu\,\frac{1}{c_f} \;-\; (1-\mu)\,\frac{1}{W - p\,n - c_f} \;=\; 0.$$

This implies:

$$\mu \frac{1}{c_f} = (1-\mu)\frac{1}{W - p\,n - c_f},$$

or equivalently,

$$\mu\left(W - p\,n - c_f\right) = (1-\mu)\,c_f. \tag{1}$$

Next, the derivative with respect to n is:

$$\frac{\partial}{\partial n}\left(\mu\,U_f + (1-\mu)\,U_m\right) = \mu\,(v_f - h(W))\,\frac{1}{n} + (1-\mu)\,v_m\,\frac{1}{n} - (1-\mu)\,\frac{p}{W - p\,n - c_f} = 0.$$

Factor out $\frac{1}{n}$:

$$\frac{1}{n}\left[\,\mu\,(v_f - h(W)) + (1-\mu)\,v_m\right] = \frac{(1-\mu)\,p}{W - p\,n - c_f}.$$

Define

$$t \equiv \mu\left(v_f - h(W)\right) + (1-\mu)\,v_m.$$

Then the above equation becomes

$$\frac{t}{n} = \frac{(1-\mu)\,p}{W - p\,n - c_f}. \tag{2}$$

Step 3: Solve the system. From equation (1):

$$c_f = \mu\left(W - p\,n\right). \tag{3}$$

Substitute (3) into (2). Note that

$$W - p\,n - c_f = W - p\,n - \mu\,(W - p\,n) = (1-\mu)\left(W - p\,n\right).$$

Hence, (2) becomes

$$\frac{t}{n} = \frac{(1-\mu)\,p}{(1-\mu)\left(W - p\,n\right)} \implies t\left(W - p\,n\right) = p\,n.$$

Rearrange to solve for n. We get:

$$t\,W - t\,p\,n = p\,n \implies t\,W = p\,n\left(1+t\right).$$

Hence the equilibrium number of children,

$$n^* = \frac{t\,W}{p\,(1+t)}, \tag{4}$$

And then $c_f^* = \mu\,(W - p\,n^*)$ follows from (3), while $c_m^* = W - p\,n^* - c_f^*$.

I.2.2 Comparative Statics

We are interested in:

1. Fertility declining with women gaining bargaining power. This is the main result of the paper (see Section 5 of the main paper.
2. The fertility decline is stronger when people are wealthier.[9] This corresponds to the finding that higher wealth households responded more to women's rights. Moreover, we show below that, the percent decline in fertility is higher for higher wealth people if people care about maternal mortality risk more when their wealth increases (i.e. $h'(W) > 0$). See Table 6 of the main paper.
3. The decrease of fertility with women's bargaining power is greater when risk is higher (i.e. when h is higher). This corresponds to the finding that higher maternal mortality risk states saw greater reductions in fertility. See Table 7 of the main paper.

Proposition 1. ***Effect of Bargaining Power and Maternal Mortality Risk on Fertility***

Let a household choose the number of children $n^* = n^*(\mu, W, h)$ according to:

$$n^*(\mu, W, h) \;=\; \frac{W}{p} \cdot \frac{t(\mu, h)}{1 + t(\mu, h)},$$

Assume $v_f - h < v_m$ so that the wife's net valuation of children is lower than the husband's. *Then:*

(i) $\dfrac{\partial n^*}{\partial \mu} < 0.$ Increasing the wife's bargaining power reduces fertility in levels.

(ii) The sign of $\dfrac{\partial n^*}{\partial W}$ *can be* positive, negative, or zero*, depending on parameter magnitudes.*

Hence higher wealth may raise or reduce fertility, due to offsetting income and health-cost effects.

(iii) $\dfrac{\partial^2 n^*}{\partial \mu\, \partial W} < 0.$ As wealth increases, the negative effect of μ on n^* becomes strictly more negative in levels.

(iv) $\dfrac{\partial}{\partial W}\Big(\dfrac{\partial \ln n^*}{\partial \mu}\Big) < 0.$ The **percentage** decline in fertility when μ rises is larger at higher W.

[9] Whether fertility increases or decreases in wealth depends on whether an income effect (richer people want more kids) or a health effect (richer women care more about maternal mortality risk) dominates. Our point here is that higher wealth households have a stronger decline in fertility with women's rights both in both absolute and percentage terms.

(v) $\dfrac{\partial^2 n^*}{\partial\mu\,\partial h} < 0.$ If maternal-risk cost h increases, then the negative impact of μ on n^* grows even stronger.[10]

Proof. **Notation and Setup.** By hypothesis,

$$n^*(\mu, W, h) \;=\; \frac{W}{p} \cdot \frac{t(\mu,h)}{1+t(\mu,h)},$$

We keep W and h as separate parameters. One can think of h as a shift capturing maternal mortality/morbidity risk that lowers the wife's net valuation of children $(v_f - h)$.

(i) $\frac{\partial n^*}{\partial\mu} < 0$.

First, observe that

$$t(\mu,h) \;=\; \mu\,(v_f - h) \;+\; (1-\mu)\,v_m \;\;=\;\; v_m + \mu\,(v_f - h - v_m).$$

Then

$$n^* \;=\; \frac{W}{p} \cdot \frac{t(\mu,h)}{1+t(\mu,h)}.$$

We compute

$$\frac{\partial n^*}{\partial\mu} = \frac{W}{p} \cdot \frac{\partial}{\partial\mu}\Big(\frac{t(\mu,h)}{1+t(\mu,h)}\Big).$$

Since

$$\frac{t(\mu,h)}{1+t(\mu,h)} \;=\; \phi\big(t(\mu,h)\big), \quad \text{where } \phi(x) = \frac{x}{1+x},$$

we know $\phi'(x) = \frac{1}{(1+x)^2}$. Also $\frac{\partial t}{\partial\mu} = v_f - h - v_m$. Hence the product rule yields

$$\frac{\partial n^*}{\partial\mu} = \frac{W}{p}\,\phi'(t(\mu,h))\,\big(\tfrac{\partial t}{\partial\mu}\big) \;=\; \frac{W}{p} \cdot \frac{v_f - h - v_m}{\big[1+t(\mu,h)\big]^2}.$$

Given $v_f - h < v_m$, we have $(v_f - h - v_m) < 0$. Therefore

$$\frac{\partial n^*}{\partial\mu} < 0.$$

This shows that *increasing the wife's bargaining power (raising μ) strictly reduces fertility n^*.*

(ii) $\frac{\partial n^*}{\partial W}$ can have any sign.

Now consider

$$n^*(\mu, W, h) = \frac{W}{p}\,\frac{t(\mu,h)}{1+t(\mu,h)}.$$

[10] In many applications, one might interpret h as $h(W)$, i.e. an increasing function of W. But for clarity, we list h explicitly as a separate parameter so that $\partial/\partial h$ makes sense.

When W increases, there is a *direct* positive effect (the factor W/p grows, suggesting more resources to afford children). Formally, if $v_f - h$ is viewed as decreasing in W (for instance, through a function $h(W)$), that can offset or reverse the positive resource effect.

Hence, in general,

$$\frac{\partial}{\partial W}\Big(\frac{W}{p}\Big) > 0 \quad \text{but} \quad \frac{\partial}{\partial W}\big[t(\mu, h)\big] \leq 0 \text{ as } h \text{ rises with } W.$$

Thus the net sign of $\partial n^*/\partial W$ may be positive, negative, or zero, depending on parameter values. *Hence an unambiguous sign is not determined by the model.*

(iii) $\frac{\partial^2 n^*}{\partial \mu \, \partial W} < 0$.

Whereas $\partial n^*/\partial W$ is indeterminate in sign, the cross-partial

$$\frac{\partial^2 n^*}{\partial \mu \, \partial W} \;=\; \frac{\partial}{\partial W}\Big(\frac{\partial n^*}{\partial \mu}\Big)$$

is strictly negative.

From part (i) we have

$$\frac{\partial n^*}{\partial \mu} = \frac{W}{p} \cdot \frac{v_f - h - v_m}{\big[1 + t(\mu, h)\big]^2}.$$

If h strictly increases with W, then $v_f - h$ is *more negative* at higher W, so the expression $(v_f - h - v_m)$ is decreasing in W. Furthermore, the term $t(\mu, h)$ depends negatively on h, so the denominator can also shift. By direct (but somewhat tedious) partial differentiation, below, one obtains

$$\frac{\partial^2 n^*}{\partial \mu \, \partial W} < 0$$

provided that $v_f - h < v_m$ and $h'(W) > 0$. Intuitively, *the gap in net child valuation between husband and wife widens at higher W, so giving the wife more power reduces n^* more strongly.*

Continued: Detailed derivation of $\dfrac{\partial^2 n^*}{\partial \mu \, \partial W} < 0$. **Step 1.** We differentiate $\partial n^*/\partial \mu$ with respect to W to obtain the cross-partial:

$$\frac{\partial^2 n^*}{\partial \mu \, \partial W} \;=\; \frac{\partial}{\partial W}\Big(\frac{\partial n^*}{\partial \mu}\Big).$$

From before we have:

$$\frac{\partial n^*}{\partial \mu}(\mu, W) \;=\; \frac{W}{p} \cdot \frac{\big[v_f - h(W)\big] - v_m}{\big[1 + t(\mu, W)\big]^2}.$$

Applying the product rule in W:

$$\frac{\partial^2 n^*}{\partial\mu\,\partial W} = \underbrace{\frac{\partial}{\partial W}\left(\frac{W}{p}\right)}_{=\frac{1}{p}} \times \frac{\left[v_f - h(W)\right] - v_m}{\left[1 + t(\mu, W)\right]^2} \quad + \quad \frac{W}{p} \times \frac{\partial}{\partial W}\left(\frac{\left[v_f - h(W)\right] - v_m}{\left[1 + t(\mu, W)\right]^2}\right).$$

Step 2. Let us set

$$\psi(W) \;=\; \left[v_f - h(W)\right] - v_m, \quad \text{so that} \quad \psi'(W) = -\,h'(W).$$

Then

$$\text{Term B} \;=\; \frac{\psi(W)}{\left[1 + t(\mu, W)\right]^2}, \quad \text{and} \quad t(\mu, W) \;=\; \mu\left[v_f - h(W)\right] \;+\; (1-\mu)\,v_m \;=\; \mu\left[\psi(W) + v_m\right] + (1-\mu)\,v_m.$$

Thus

$$\frac{\partial t}{\partial W} \;=\; -\,\mu\,h'(W) \leq 0 \text{if } h \text{ rises with } W.$$

We now differentiate Term B w.r.t. W:

$$\frac{\partial}{\partial W}\left(\frac{\psi(W)}{\left[1 + t(\mu, W)\right]^2}\right) \;=\; \underbrace{\psi'(W)}_{=-h'(W)} \cdot \frac{1}{\left[1 + t(\mu, W)\right]^2} \;+\; \psi(W)\,\frac{\partial}{\partial W}\left(\left[1 + t(\mu, W)\right]^{-2}\right).$$

Since $\frac{d}{dx}(1+x)^{-2} = -2\,(1+x)^{-3}$, we get

$$\frac{\partial}{\partial W}\left[(1+t)^{-2}\right] \;=\; -2\,(1+t)^{-3}\,\frac{\partial t}{\partial W} \;=\; -2\,(1+t)^{-3}\left[-\mu\,h'(W)\right] \;=\; 2\,\mu\,h'(W)\,(1+t)^{-3}.$$

Hence:

$$\begin{aligned} \frac{\partial}{\partial W}\left(\frac{\psi(W)}{(1+t)^2}\right) &= \frac{-\,h'(W)}{(1+t)^2} \;+\; \psi(W)\left[2\,\mu\,h'(W)\,(1+t)^{-3}\right] \\ &= \frac{-\,h'(W)}{(1+t)^2} \;+\; \frac{2\,\mu\,h'(W)\,\psi(W)}{(1+t)^3}. \end{aligned}$$

Putting it all together,

$$\begin{aligned} \frac{\partial^2 n^*}{\partial\mu\,\partial W} &= \frac{1}{p}\,\frac{\psi(W)}{(1+t)^2} \;+\; \frac{W}{p}\left[\frac{-\,h'(W)}{(1+t)^2} \;+\; \frac{2\,\mu\,h'(W)\,\psi(W)}{(1+t)^3}\right] \\ &= \frac{1}{p\left[1 + t(\mu, W)\right]^3}\left[\psi(W)\,(1+t) \;+\; W\,h'(W)\left(-\left[1+t\right] \;+\; 2\,\mu\,\psi(W)\right)\right], \end{aligned}$$

where $(1+t)$ abbreviates $(1 + t(\mu, W))$.

Step 3. Sign under $v_f - h(W) < v_m$ and $h'(W) > 0$.

- We have $h'(W) > 0$ by assumption (the health-risk term rises with wealth). - We also have $\psi(W) = \left[v_f - h(W)\right] - v_m < 0$ (since $v_f - h(W) < v_m$). - One can verify by direct expansion that, inside the last bracket, the negative sign of $\psi(W)$ and the positivity of $h'(W)$ together ensure $\dfrac{\partial^2 n^*}{\partial\mu\,\partial W} < 0$.

Thus, *the higher is W, the larger the difference between v_m and $v_f - h(W)$, so giving more weight to the wife (μ) reduces n^* **even more** strongly in higher-wealth households.* □

(iv) The percentage reduction in n^* from raising μ increases with W.

Examine

$$\frac{\partial \ln n^*}{\partial\mu} = \frac{1}{n^*}\,\frac{\partial n^*}{\partial\mu}.$$

A more convenient form is to write:

$$\ln\bigl(n^*(\mu, W, h)\bigr) = \ln\Bigl(\tfrac{W}{p}\Bigr) + \ln\bigl[t(\mu,h)\bigr] - \ln\bigl[1 + t(\mu,h)\bigr].$$

Then

$$\frac{\partial \ln n^*}{\partial\mu} = \frac{\partial}{\partial\mu}\bigl[\ln t(\mu,h)\bigr] \; - \; \frac{\partial}{\partial\mu}\bigl[\ln\bigl(1 + t(\mu,h)\bigr)\bigr].$$

Since $\frac{\partial t}{\partial\mu} = v_f - h - v_m < 0$, we get a negative value overall (given $v_f - h < v_m$). One can then check how this expression changes with W. Because h is strictly increasing in W (if so assumed), $(v_f - h)$ becomes more negative, strengthening the negative fraction and making $\left|\frac{\partial \ln n^*}{\partial\mu}\right|$ larger.

Hence $\frac{\partial}{\partial W}\Bigl(\frac{\partial \ln n^*}{\partial\mu}\Bigr) < 0$. In words, *the percentage drop in n^* from raising μ is strictly bigger at higher wealth W.*

(v) $\frac{\partial^2 n^*}{\partial\mu\,\partial h} < 0$.

Finally, to see how maternal-risk cost h influences *the effect* of μ on n^*, we compute

$$\frac{\partial}{\partial h}\Bigl(\frac{\partial n^*}{\partial\mu}\Bigr) = \frac{\partial^2 n^*}{\partial\mu\,\partial h}.$$

Recall

$$\frac{\partial n^*}{\partial\mu} = \frac{W}{p} \cdot \frac{v_f - h - v_m}{\bigl[1 + t(\mu,h)\bigr]^2}.$$

Taking the partial derivative w.r.t. h:

$$\frac{\partial^2 n^*}{\partial\mu\,\partial h} = \frac{W}{p}\,\frac{\partial}{\partial h}\Bigl[\frac{v_f - h - v_m}{\bigl(1 + t(\mu,h)\bigr)^2}\Bigr].$$

One sees immediately that $\frac{\partial}{\partial h}(v_f - h - v_m) = -1$, while $\frac{\partial}{\partial h}\bigl(t(\mu,h)\bigr) = -\mu$. By carefully applying the quotient rule (or product rule if you rewrite it suitably), we get

$$\frac{\partial^2 n^*}{\partial \mu\, \partial h} < 0$$

so long as $v_f - h < v_m$ and $\mu > 0$. Intuitively, an increase in h (greater maternal mortality/morbidity risk) *lowers the wife's net valuation of children*, making her more eager to reduce n, so an increase in her bargaining power μ has an even stronger negative impact on n^*.

Thus, *the negative effect of μ on n^* becomes strictly larger in absolute value if maternal-risk h rises*, concluding part (v).

Summary. We have established (i) $\partial n^*/\partial \mu < 0$, (ii) the sign of $\partial n^*/\partial W$ is ambiguous, (iii) $\partial^2 n^*/(\partial \mu\, \partial W) < 0$, (iv) the percentage reduction in n^* from raising μ is larger at higher W, and (v) $\partial^2 n^*/(\partial \mu\, \partial h) < 0$, so that a higher maternal-risk h amplifies the negative impact of μ on fertility.

□

I.3 Nash Bargaining Framework

We now show that the same qualitative results obtain if we assume a Nash bargaining setup rather than an arbitrary Pareto weight μ. In particular, suppose each spouse's outside option (threat point) is determined by $\alpha \in (0, 1)$, the fraction of total household wealth W controlled by the wife in case of disagreement:

$$U_f^0 = \ln(\alpha\, W), \quad U_m^0 = \ln\big((1-\alpha)\, W\big).$$ [11]

Then the household solves:

$$\max_{c_f, c_m, n} \big(U_f - U_f^0\big)\,\big(U_m - U_m^0\big), \quad \text{subject to} \quad c_f + c_m + p\, n = W.$$

Formally,

$$\max_{c_f, c_m, n} \Big[\ln(c_f) + (v_f - h(W))\,\ln(n) - \ln(\alpha\, W)\Big]\,\Big[\ln(c_m) + v_m\,\ln(n) - \ln\big((1-\alpha)\, W\big)\Big].$$

I.3.1 First-Order Conditions in the Nash Product

Denote

$$\Delta U_f \;=\; U_f - U_f^0 \;=\; \ln(c_f) + (v_f - h(W))\,\ln(n) \;-\; \ln(\alpha\, W),$$

[11] Notice that this framework implicitly assumes that, even when disagreeing, there is exactly one kid. That is, upon agreeing the marry the couple confirms that they will have 1 kid unless otherwise specified by agreement. This is a reasonable assumption: virtually all married couples had at least one child, and children were often seen as the reason to get married.

$$\Delta U_m \;=\; U_m - U_m^0 \;=\; \ln(c_m) + v_m \,\ln(n) \;-\; \ln\big((1-\alpha)\,W\big).$$

The objective is $G = \Delta U_f \cdot \Delta U_m$. From the budget constraint, $c_m = W - p\,n - c_f$.

Step 1: Derivatives with respect to c_f and n.

$$\frac{\partial G}{\partial c_f} \;=\; \Delta U_m \,\frac{\partial \Delta U_f}{\partial c_f} \;+\; \Delta U_f \,\frac{\partial \Delta U_m}{\partial c_f}.$$

But

$$\frac{\partial \Delta U_f}{\partial c_f} \;=\; \frac{1}{c_f}, \quad \frac{\partial \Delta U_m}{\partial c_f} \;=\; \frac{\partial}{\partial c_f}\big[\ln(W - p\,n - c_f)\big] \;=\; -\,\frac{1}{W - p\,n - c_f}.$$

Setting $\frac{\partial G}{\partial c_f} = 0$ yields

$$\Delta U_m \,\frac{1}{c_f} \;-\; \Delta U_f \,\frac{1}{W - p\,n - c_f} \;=\; 0,$$

$$\implies \quad \Delta U_m \,(W - p\,n - c_f) \;=\; \Delta U_f \, c_f. \tag{5}$$

Similarly, for n:

$$\frac{\partial G}{\partial n} \;=\; \Delta U_m \,\frac{\partial \Delta U_f}{\partial n} \;+\; \Delta U_f \,\frac{\partial \Delta U_m}{\partial n}.$$

We have

$$\frac{\partial \Delta U_f}{\partial n} \;=\; \frac{v_f - h(W)}{n}, \quad \frac{\partial \Delta U_m}{\partial n} \;=\; \frac{v_m}{n} \;-\; \frac{p}{W - p\,n - c_f}.$$

Thus

$$\frac{\partial G}{\partial n} = \Delta U_m \,\frac{v_f - h(W)}{n} \;+\; \Delta U_f \left(\frac{v_m}{n} - \frac{p}{W - p\,n - c_f}\right) = 0.$$

Rearranging,

$$\frac{1}{n}\big[\Delta U_m \,(v_f - h(W)) + \Delta U_f \, v_m\big] \;=\; \Delta U_f \,\frac{p}{W - p\,n - c_f}. \tag{6}$$

I.3.2 Equivalence to a Pareto Weight Formulation

We claim that the above system is equivalent to a collective model with a certain μ_0. Let

$$\mu_0 \;\equiv\; \frac{\Delta U_m}{\Delta U_m + \Delta U_f}.$$

We show that if (c_f^*, c_m^*, n^*) solves the Nash problem, then it also solves

$$\max_{c_f,\, c_m,\, n} \Big\{\mu_0 \, U_f + (1 - \mu_0)\, U_m\Big\},$$

subject to $c_f + c_m + p\,n = W$.

Step 2 : Rewrite conditions (5) and (6). From (5):

$$\Delta U_m\,(W - p\,n - c_f) \;=\; \Delta U_f\,c_f \;\Longrightarrow\; \frac{\Delta U_m}{\Delta U_m + \Delta U_f} \;=\; \frac{c_f}{c_f + (W - p\,n - c_f)}.$$

Hence

$$\mu_0 \;=\; \frac{c_f}{W - p\,n}.$$

Equivalently,

$$c_f = \mu_0\,(W - p\,n), \quad c_m = (1-\mu_0)\,(W - p\,n).$$

Next, condition (6) can be rearranged similarly to the Pareto FOCs we had earlier (compare with our Step 2 in the Pareto model). One obtains

$$\Big[\mu_0\,(v_f - h(W)) + (1-\mu_0)\,v_m\Big]\,(W - p\,n - c_f) \;=\; \mu_0\,(W - p\,n)\left[\frac{v_m}{n}\right] \;\Longrightarrow\; \dots$$

and this exactly matches the FOCs from the Pareto approach, with $\mu = \mu_0$. Therefore, $\left(c_f^*, c_m^*, n^*\right)$ solves both the Nash product problem and the Pareto problem with weight μ_0.

LEMMA 1. *μ_0 is strictly increasing in α. Consequently, an increase in α (improving the wife's outside option) leads to an increase in the implicit bargaining weight μ_0.*

Proof. By definition,

$$\mu_0 \;=\; \frac{\Delta U_m}{\Delta U_m + \Delta U_f} \;=\; \frac{U_m - U_m^0}{(U_m - U_m^0) + (U_f - U_f^0)}.$$

Since $U_m^0 = \ln\big((1-\alpha)\,W\big)$ decreases as α increases, it follows that $\Delta U_m = U_m - U_m^0$ *increases* with α. Similarly, ΔU_f may also change with α; however, one can verify (by direct differentiation of μ_0 with respect to α) that the net effect is strictly positive as long as ΔU_m and ΔU_f remain finite. A standard derivation confirms that better fallback for the wife shifts μ_0 upward. ☐

I.3.3 Main Comparative Statics Under Nash Bargaining

Once we have established the equivalence to a Pareto model with $\mu = \mu_0(\alpha)$, the same sign analysis applies. Specifically, if α increases (granting the wife greater control of wealth in the outside option), then μ_0 increases, which in turn reduces n^* if $v_f - h(W) < v_m$. Furthermore,

1. $\frac{\partial^2 n}{\partial \mu \partial W} < 0$, or $\frac{\partial n}{\partial W}$ is decreasing with μ. Considering that the mapping between the Nash and Pareto problems is such that μ increases as α increases, $\frac{\partial^2 n}{\partial W \partial \alpha} < 0$.

2. $\frac{\partial^2 n}{\partial \mu \partial h} < 0$, or $\frac{\partial n}{\partial h}$ decreases with μ. Again, this implies that $\frac{\partial^2 n}{\partial \alpha \partial h} < 0$.

That is, the mapping between the two problems is such that the comparative statics on μ and α are very similar. Thus the effect of α on fertility is larger when $h(W)$ is larger or when W is larger.

Proposition 2. *Under the Nash bargaining setup, let α denote the wife's share of wealth in the outside option. Suppose $v_f - h(W) < v_m$. Then:*

(i) n^* is a decreasing function of α. Formally, $\frac{\partial n^*}{\partial \alpha} < 0$.

(ii) As in the Pareto model, the magnitude of this fertility reduction is larger for higher $h(W)$ and/or higher W.

I.4 Interpretation and Figurative Solution

Figures A.15(*left*) and A.15(*right*) illustrate the difference between:

- A movement along the Pareto frontier (left panel), showing how the wife's utility U_f and the husband's utility U_m vary as the bargaining weight μ shifts.
- The analogous Nash-bargaining outcome (right panel), in which the threat points (U_f^0, U_m^0) shift with α. An increase in α moves the "bargaining lens" and again leads to a solution favoring the wife.

In both frameworks, raising the wife's bargaining power (either by exogenously raising μ or raising α) reduces fertility as long as the wife's effective preference for children is lower, i.e. $v_f - h(W) < v_m$.

I.5 Concluding Remarks

In both the collective and Nash-bargaining frameworks, we conclude:

- **Granting more power to the wife** (via a higher Pareto weight μ or a more favorable outside option α) **reduces fertility** whenever the wife's marginal utility for children (net of health/mortality cost) is lower than the husband's.
- **The fertility decline is larger** when $\mathbf{h}(\mathbf{W})$ is larger or if **W** is larger and $h(\cdot)$ is increasing in wealth.

These results align with the empirical notion that empowering women (especially where maternal mortality or morbidity risks are significant) can reduce fertility.

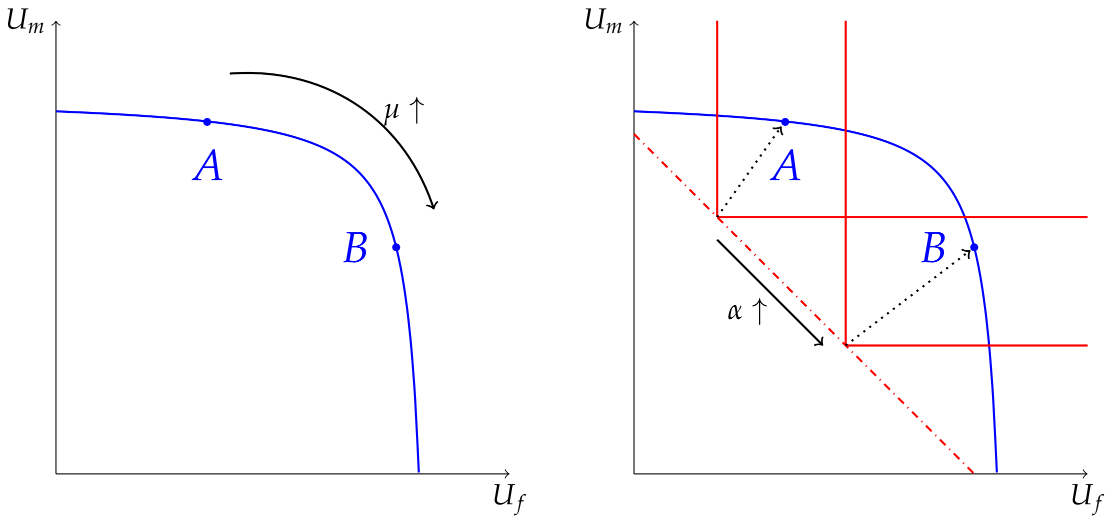

Figure A.15: Set of Pareto-optimal allocations (left panel) and Nash-bargaining solution (right panel). When we grant more "rights" or outside-option resources to the wife, either by increasing μ (left) or α (right), the solution moves in a way that raises U_f relative to U_m and (under $v_f - h(W) < v_m$) reduces n. The minimum utility is normalized to zero for both agents.

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