University of Ottawa

Doctoral Thesis

Essays in Applied Microeconomics

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A thesis submitted in fulfillment of the requirements for the degree of Doctor of Philosophy

in the

Faculty of Social Sciences
Department of Economics
University of Ottawa

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Declaration of Authorship

I, Lamis El Kattan, declare that this thesis titled, "Essays in Applied Microeconomics" and the work presented in it are my own. Chapter one of this thesis is done by myself jointly with prof. Abel Brodeur. My contribution is equal to his. The second chapter of this thesis is done jointly with Joanne Haddad. My contribution is equal to hers. I, hereby acknowledge the contribution of prof. Abel Brodeur, my thesis supervisor, for the research related to chapter one. I acknowledge the contribution of Joanne Haddad, for the research related to chapter two.
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UNIVERSITY OF OTTAWA

Abstract

Faculty of Social Sciences
Department of Economics

Doctor of Philosophy

Essays in Applied Microeconomics

by Lamis El Kattan

This dissertation includes three essays in applied microeconomics. The first two chapters focus on gender and female labor force participation. The third chapter examines the strategic behavior in politics.

The first chapter examines the impact of male casualties due to World War II on fertility and female employment in the United States. We rely on the number of casualties at the county-level and use a differences-in-differences strategy. While most counties in the U.S. experienced a Baby Boom following the war, we find that the increase in fertility was lower in high casualty rate counties than in low casualty rate counties. Analyzing the channels through which male casualties could have decreased fertility, we provide evidence that county male casualties are positively related to 1950s female employment and household income.

The second chapter examines the impact of gender focused labor legislation on women's labor force participation and economic empowerment. We rely on historical acts passed by state legislatures and exploit whether or not states passed regulatory laws regulating overall and industry specific employment and work conditions for women, night work laws and labor laws requiring provision of seats for working women. We exploit the fact that not all states enacted these laws as well as the variation in the timing of enactment of such laws. Our results show that women in comparison to men in treated states are more likely to be in the labor force after the introduction of seating and night work laws relative to control states. We also document the effect of industry-specific labor policies on women's likelihood to be employed in the affected industry and in higher-wage occupations within the industry of interest. Policy implications of our findings endorse the adoption of labor laws in favor of women to further their empowerment through a higher involvement in the labor market and financial independence.

The third chapter examines strategic timing in the appearance of scandals about elected officials in the United States. In order to minimize negative publicity, politicians may strategically manipulate the timing of uncovering their own unpopular actions to coincide with other important events that are crowding the media and distracting the public. I start by developing a simple voting model to better understand the different mechanisms behind the timing of scandals' appearance. A forward-looking strategy implies that predictable news events may be used by politicians to distort public opinion. Using a novel data set of misconduct episodes from 1970 to 2020 and an instrumental variable strategy, I show that scandals are more likely to appear simultaneously with other foreseeable newsworthy events. I also examine the heterogeneity of different types of scandals and potentially different behavior across political parties. My findings suggest that Republican politicians are behaving especially strategically in timing the revelation of sexual and political misconducts.

Keywords: Female Employment, Fertility, Mass Media, Strategic Timing.

Acknowledgements

I am immensely grateful to my supervisor prof. Abel Brodeur for his dedication, for the time he spent guiding me, for his encouragement and for his constant support. I was privileged to work with Abel, an amazing person, researcher, and academic. He saw the potential of this work from the first day of my doctoral research and kept reminding me of it, even when I could not see it. I could have not asked for a better mentor in these years.

I would like to thank the department of Economics at the University of Ottawa for giving me the opportunity to embark on this journey and for all the feedback received by the faculty members over the years. In particular, I would like to thank prof. Jason Garred for his precious help in developing both my research and my professional experience. I cannot thank prof. Louis-Philippe Morin and prof. Matthew Webb enough for their invaluable help. Finding committee members that guide you to surpass your limits is rare and, in my case, it made all the difference. I also thank prof. Arvind Magesan for agreeing to evaluate my thesis as an external examiner and for his extremely useful comments and suggestions. Finally, I am thankful to the University of Ottawa for funding my doctoral studies and for allowing me to focus on my research.

I would be grateful to have done this PhD just to have found my friend and co-author Joanne. Her support in good and bad periods and the time spent together has often been the highlight of my days in these past years. To Landry, Taylor, Jingjing, Jihad, Fatima, Farzaneh, Mansoureh, and Fabien, this process was made so much better with your presence in it. The collegiality and companionship you all shared with me as we were navigating a journey full of challenges and wonderful moments was one of the biggest highlights of the PhD process.

Lastly, I owe this accomplishment to my parents. Their example, their values, their strong belief in the power of education and their constant faith in my capabilities have been the reason behind all my achievements. This thesis is for them.

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Chapter 1

World War II, the Baby Boom and Employment: County Level Evidence

1.1 Introduction

In today's America, the typical woman has one or two children, is engaged in paid work and enjoys the same legal rights as men. This has not always been the case. At the end of the 19th century, approximately 5 percent of married white women were engaged in paid work and the total fertility rate was about 3.5 (Goldin, 1991; Guinnane, 2011). The decrease in fertility and the increase in female labor supply was not steady and may have been shocked by specific events. For instance, fertility rates declined from 1850 to 1930 and stabilized for several years. This stabilization was followed by exceptionally high birth rates from 1944 to 1961–the Baby Boom– and then a baby bust (Figure 1.1).[1]

A vast literature documents historical shocks that could have led to female economic empowerment in the 20th century (Fernández, Fogli, and Olivetti, 2004; Fernández, 2013; Fernández and Wong, 2014a; Fernández and Wong, 2014b; Goldin, 2006; Goldin and Katz, 2002; Guinnane, 2011). In this study, we reassess the role of one of these potential shocks, World War II (WWII). During WWII, about 16 million men were mobilized and deployed in Asia and Europe, which coincided with a sharp rise in female employment. At the height of the war, women comprised approximately 35 percent of the civilian labor force (Acemoglu, Autor, and Lyle, 2004).

Goldin and Olivetti, 2013, Acemoglu, Autor, and Lyle, 2004 and Doepke, Hazan, and Maoz, 2015 rely on mobilization rates to identify the long run impact of the war on female labor force participation, the wage structure and fertility, respectively. Mobilization rates vary across states because of many factors (e.g., dependents, occupation and fitness to serve) that led to deferments. Using this variation, Goldin and Olivetti, 2013 provide evidence that women's labor supply was shifted during the war and that the effect persisted for some of the women who worked in white-collar positions during WWII. Similarly, Acemoglu, Autor, and Lyle, 2004 provide evidence that women worked more in 1950 in states with a higher mobilization rate and that this increase in female labor supply lowered female and male wages. Doepke, Hazan, and Maoz, 2015 find that states with

[1] The increase in the female labor force participation was steady between 1930 to 1970.

a greater mobilization of men experienced a larger post-war increase in fertility. They argue that the return of men from the war crowded out younger women leading them to opt for marriage and childbearing.

Our purpose is to reexamine the effect of WWII on fertility and women's employment. We first investigate the reproducibility of the results described above. We then complement these studies by relying on novel data on male casualties at a more disaggregated level (i.e., county). Last, we investigate some of the mechanisms through which WWII casualties may have affected fertility, focusing on the labor and marriage markets.

We begin with a replication of Doepke, Hazan, and Maoz, 2015's analyses using a similar empirical model. We successfully replicate their main results and show that state casualty and mobilization rates are positively correlated to fertility post-WWII. One concern with their results is that socioeconomic factors causing differences in male casualties or mobilization could also affect post-war fertility. One key variable is race as the U.S. Army was still segregated when the U.S. entered WWII. Figure 1.2 illustrates the geographic distribution of the casualty rates both across-states and within-states, across-counties. This figure shows that the Deep South overwhelmingly consisted of low-casualty rate counties, partly due to the U.S. military segregation.[2]

We thus enrich Doepke, Hazan, and Maoz, 2015's main specification with the share of black men and additional control variables such as the share of fathers and average educational attainment. Once we control for these additional pre-war characteristics, the point estimates become *negative*, suggesting that higher male causalities and mobilization rates led to lower fertility post-WWII. This sensitivity analysis sheds light on the importance to further control for key determinants of male mobilization (or casualty) and fertility, and taking into account regional trends in fertility.[3]

We then turn to our novel county-level analysis. Our empirical strategy consists of comparing counties with high and low casualty rates, before and after the war. Analyzing the effect of WWII on fertility at the county-level brings the analysis to a more disaggregated level. This has a key advantage over the state-level analysis since counties across states may have very different demographic and socioeconomic characteristics. Additionally, one of the main sources of potential bias in cross-state regressions likely stems from important regional trends in fertility. Relying on the county-level data allows us to account for these trends by controlling for all time-varying state characteristics and solely relying on the within-state variation. County-level data thus strengthens the credibility of the identification strategy.

While most counties in the U.S. experienced a Baby Boom following the war, we find that the increase in fertility was lower in high-casualty rate counties than in low-casualty rate counties in 1950. Our estimates are statistically significant

[2]When black men volunteered for duty or were drafted following the Pearl Harbor attack, they were relegated to segregated divisions and combat support roles, such as cook, quartermaster and grave-digging duty (Bristol and Stur, 2002).

[3]Appendix Figure A1 shows that the U.S. South, in particular, had a unique fertility trajectory. In the late 1940s, the gap between the South and other regions starts to close, suggesting that regions with relatively higher casualty rate had a relatively lower decrease in fertility.

and suggest that women had, on average, about 15% fewer babies in a county that had a one percentage point higher casualty rate during WWII. We show that the sign of the county-level estimates is robust to the inclusion of pre-war county determinants of WWII casualties, as well as a set of extended controls that might have potentially affected fertility levels. We also show that our findings are robust to the exclusion of states with a large share of black residents or large post-war net migration. Of note, we find weak evidence that WWII casualties affect fertility rates in 1960 and no evidence that the war casualties impacted fertility in 1970.

In order to gain insight on the mechanisms through which WWII casualties may have decreased fertility, we follow the literature and test the effect of male casualties on female employment and other socioeconomic outcomes. We reexamine the effect of WWII on female employment at the State Economic Area (SEA) level. A SEA is either a single county or a group of counties. Our results are in line with those of Acemoglu, Autor, and Lyle, 2004 and Goldin and Olivetti, 2013 as we find that SEA-level casualty rates during WWII led to a shift in female employment in 1950. Our findings also suggest that greater casualties reduced individual wages (Acemoglu, Autor, and Lyle, 2004), but that the increase in after-war household income was higher in high casualty rate SEAs than in low casualty rate SEAs.

This set of results suggests an explanation consistent with Becker's theory of fertility (Becker, 1960; Becker and Lewis, 1973; Becker and Barro, 1989). Becker argued that parents derive utility from both the number and the quality of children. When their earnings increase, parents tend to be more likely to invest in their children's human capital and decrease the number of children they have. We provide empirical evidence supporting this theory by relying on infant mortality data and computing the percentage of live births attended by physicians at the hospital. We find that county male casualties are positively associated to the percentage of attended births by physicians at the hospital and negatively related to infant mortality in the after-war period, indicating better health outcomes for newborn kids. These results provide suggestive evidence that parents in high casualty rate counties invest more in the quality of their children during pregnancy and after birth.[4]

Another important channel through which male casualties could have impacted fertility is male-to-female sex ratios and the marriage market. A recent study by Brainerd, 2017 shows that the drastic change in sex ratios caused by World War II in Russia led to lower rates of marriage and fertility and higher non-marital births, and reduced the bargaining power within marriage for women in areas most affected by war casualties.[5] Our fertility results are in line with Brainerd, 2017, but we find no evidence that WWII casualties decreased marriage rates. This could be due to the relatively small decrease in

[4]See Doepke, Hazan, and Maoz, 2015 for a detailed account of the work of Becker on the quantity and quality of children. Becker provides many examples of child quality choices such as giving them dance and music lessons and sending them to private colleges.

[5]Abramitzky, Delavande, and Vasconcelos, 2011 evaluate the impact of war on assortative matching in the marriage market and find that World War I has led to a decrease in the probability of marriage for women in French regions with higher mortality rates.

sex ratios for most U.S. counties (i.e., roughly 1%), compared to a decrease of about 28% in Russia. We also show that WWII casualties led to a significant increase in the age of having a first child for young women aged between 15 and 25 years old in 1950, which can explain the decrease in fertility especially for women in this age category.

Overall, our analysis of the effect of WWII casualties on socioeconomic outcomes provide plausible explanations for the negative relationship between male casualties and fertility in 1950. The absence of men during and after WWII led to an increase in women's employment during and after the war, which increased household income. The combined effects of these two phenomena slowed down the Baby Boom in counties with relatively more casualties. Our findings thus suggest that there was a strong link between young women's labor market and fertility in the baby boom period, and that the labor market was affected strongly by the war (Acemoglu, Autor, and Lyle, 2004; Doepke, Hazan, and Maoz, 2015).

Our study contributes to a large literature providing socioeconomic explanations to the Baby Boom.[6] Previous studies have emphasized the role of parents (Rutherdale, 1999), technological progress in the household sector (Greenwood, Seshadri, and Vandenbroucke, 2005)[7], the Great Depression (Bellou and Cardia, 2014) and ideological and cultural changes (Lesthaeghe and Surkyn, 1988). Albanesi and Olivetti, 2014 also provide evidence that improvements in maternal health contributed to the Baby Boom in the U.S. Using cross-state variation, they show that the decline in maternal mortality is associated with a rise in fertility for women born in the 1920–30s. Our county-level analysis allows us to control for this type of time-varying state-level shocks.

Our paper also contributes to a literature that analyzes the effect of war on fertility and female labor supply (Bellou and Cardia, 2016; Bethmann and Kvasnicka, 2013; Eder, 2016; Boehnke and Gay, 2017; Jaworski, 2014; Vandenbroucke, 2014) and to a vast literature on sex ratios and the marriage market (Carranza, 2014; Grosjean and Khattar, 2019; Lafortune, 2013; Qian, 2008).

We structure the remainder of the paper as follows. In section 1.2, we present the data sources and descriptive statistics. Section 1.3 presents the methodology. Section 1.4 replicates the results of Doepke, Hazan, and Maoz, 2015 at the state-level. In section 1.5, we provide the regression results for fertility at the county-level and a large set of robustness checks. In the subsequent section, we empirically examine the channels, and the last section concludes.

[6] One of the most well-known explanations for the Baby Boom is the "catch-up fertility" hypothesis (Easterlin, 1961). This theory, based on the concept of "relative income," states that people who grew up during the Great Depression had low material well-being and increased their demand for children during the post-WWII economic expansion (Jones and Schoonbroodt, 2016).

[7] Bailey and Collins, 2011 provide empirical evidence that the Amish experienced a Baby Boom and that appliance ownership and electrification are negatively correlated to changes in fertility rate.

1.2 Data

1.2.1 World War II Casualties

During the war, 16 million Americans served in the United States Armed Forces, of whom over 400,000 did not return home. Besides its random component, the variation of male casualties across different counties is affected by the occurrence of draft deferrals. The United States Armed Forces consisted largely of "Citizen Soldiers" drawn from civilian life. The majority, roughly 10 million, joined the military through the draft, and most draftees were assigned to the army. However, the Selective Service granted deferments based on specific factors such as marital status, fatherhood, combat skills including the ability to serve, and medical disabilities. Mobilization rates for fathers were low as they were generally drafted last in the local draft pool. Furthermore, deferments were offered based on occupation. One of the main determinants of draft deferments was farmer status as farming was needed to maintain the level of food supply during the war. Note that most deferments were eliminated during the war. For example, both the wife and child deferment ended in 1943 (Goldin and Olivetti, 2013).

We rely on monographs from the National Archives to construct our measure of military casualties at the county-level. The dataset is compiled by the Department of the Navy and Bureau of Naval Personnel using The Honor List of Dead and Missing for each state in the United States, published by the War Department. This list contains the most complete data available on all military personnel who were killed up until January 31, 1946. Errors in this list were minimized by careful checks by the Casualty Branch of The Adjutant General's Office and by Machine Records Units. Note that civilian casualties are not included in these reports.

We construct casualty rates at the county-level as the number of men killed in action during WWII, divided by the number of men between the ages of 18 and 44 in 1940, and multiplied by 100. Census data on male population in 1940 per age category is retrieved from the National Historical Geographic Information System (NHGIS). Note that the 1940 Selective Service and Training Act required that men between the ages of 21 and 35 register to the draft. Registration was extended a few times during the war, including a new Service Act which made men between 18 and 45 liable for military service. By the end of the war in 1945, 50 million men between 18 and 45 had registered for the draft and 10 million had been inducted in the military. We thus rely on the male population of this age group as a proxy for the number of registered men (Acemoglu, Autor, and Lyle, 2004).

1.2.2 Fertility and Socioeconomic Characteristics

Our analysis uses fertility and socioeconomic characteristics of women and children before and after WWII as the primary outcomes of interest. Data on fertility is from the 1915–2007 U.S. County-Level Natality and Mortality Data (Bailey et al., 2016), retrieved from ICPSR. We rely on the number of total live birth by place of occurrence between 1933 and 1972. Note that data on births by place of occurrence begins to be complete in the dataset starting 1933. Additionally, total births at the county-level are available for the years 1915–1941 from Vital Statistics: Natality & Mortality Data. For the female population, we only consider women of childbearing age (i.e., between 15 and 44 years old) for the years 1915–1972 also from Vital Statistics: Natality & Mortality Data.[8]

For data on the number of children, labor supply, age of first time mothers, personal earnings, and other individual characteristics, we rely on Census data from the 1% Integrated Public Use Microdata Series (IPUMS) of the 1940 and 1950 censuses (Ruggles, McCaa, and Sobek, 2010). Unfortunately, this dataset is not available at the county-level for 1950. We thus rely on data at the SEA-level for this analysis. SEA stands for State Economic Area (see Bogue, 1951 for more details). An SEA is either a single county or a group of counties within the same state that have similar economic characteristics. Importantly, the definitions of SEAs in 1940 and 1950 are similar. The age of a first time mother is computed by subtracting the age of the eldest child from the age of the mother. To measure labor supply, we use a dummy that indicates whether a woman is currently employed, and the total number of weeks worked per year. Personal earnings are considered for employed men and women, and are deflated by the 1990 CPI.

To examine the impact of WWII casualties on infants' health, we rely on the percentage of live births attended by physicians at hospital and infant mortality measured as the death of young children under the age of one. County-level data on births by attendant by place of residence is available between 1939 and 1959 from NHGIS, derived from annual reports of vital statistics for states (including territories) and counties from the U.S. Census Bureau (1939–1944) and the U.S. Public Health Service (1945–1959). For infant mortality, data is available by place of occurrence from 1915 to 1941, and from 1959 to 1972 originally from Vital Statistics: Natality & Mortality Data.[9]

1.2.3 Descriptive Statistics

Table 1.1 displays summary statistics for WWII casualties, fertility and female population by decade for 1940–1970. The average number of casualties by county is about 99. The standard deviation is remarkably large (422) at the county-level. As mentioned previously, the casualty rate is the fraction of registered men who were killed during World War II. The casualty rate is roughly 1 percent and the standard deviation is 0.39. We split the sample into low-casualty and high-casualty counties using the casualty rate mean. There are

[8] Total births and population data are from the Census Bureau's vital statistics annual reports for states (including territories) and counties. Births are limited in geographic extent to the Birth Registration Areas established in each year. Michael Haines at Colgate University provided NHGIS with the source data, which were entered from printed census publications, and NHGIS researchers organized the data into tables and assigned meta-data on topics, categories, etc.

[9] The 1959–1967 data are derived from printed annual reports from the U.S. Public Health Service. The 1968–1972 data are derived from individual-level microdata (either birth certificates or the Compressed Mortality File) from the National Center for Health Statistics.

a total of 3,070 counties, 1,639 of which are in the low casualty rate category. The mean casualty rate for low- and high- casualty rate counties is 0.78 and 1.26 percent, respectively.

The average number of total live births per county increased by approximately 50% between 1940 and 1950, illustrating the Baby Boom period. This increase occurred in both high and low casualty rate counties, although the increase was larger in low casualty rate counties for 1950 and 1960.

Appendix Table A1 reports means and standard deviations for the individual characteristics of interest at the SEA-level. The average age of mothers at the time of first birth, women's employment rate and weekly wages all increased after the war. Of note, women's employment rate increased from 25% in 1940 to 31% in 1950. This increase was larger in high casualty rate counties.

1.3 Identification Strategy

In this section, we first show that casualty and mobilization rates are highly correlated. We then discuss the identification assumption and describe the main specification

1.3.1 State Casualty and Mobilization Rates

Male mobilization during the war might be seen as the best source of exogenous variation across states or counties.[10] However, to the best of our knowledge, data on male mobilization at the county-level is unavailable. Instead, we rely on male casualties as a measure of WWII intensity for our county-level exercise. Our analysis thus differs from Acemoglu, Autor, and Lyle, 2004, Doepke, Hazan, and Maoz, 2015 and Goldin and Olivetti, 2013 along two dimensions. First, we rely on novel county-level data instead of state-level data. This means that we are exploiting within-state across-county variation instead of across-state variation. Second, our identifying variation comes from casualty rates instead of mobilization rates. We provide empirical evidence throughout that the latter dimension does not drive our main conclusions.

Figure 1.3 illustrates that differences in casualty rates mostly reflect differences in mobilization rates. This figure plots both state mobilization and casualty rates. Male casualties are positively and highly correlated with male mobilization at the state-level. The correlation coefficient between mobilization and casualty rates is 0.55.[11]

[10] According to the National WWII Museum, 61.2% of the military were drafted, while only 38.8% were volunteers.

[11] New Mexico faced the highest rate of casualties, exceeding 350 per 100,000 people during WWII, and possibly represents an outlier. Excluding New Mexico makes the positive relationship between male mobilization and casualties stronger, with a correlation coefficient of 0.62 (see Figure 1.3, second panel). Excluding New Mexico from the analysis has no effect on our conclusions. Results available upon request.

1.3.2 Identification Assumption

This paper relies on the parallel trends assumption that in the absence of male war casualties, the average change in birth rates would have not been systematically different between counties with low- and high-casualty rates. This identification assumption could be violated if the casualty rate is related to pre-war demographic and socioeconomic characteristics of counties. We argue in what follows that our results are not driven by pre-war factors.

As shown in Table 1.1, the number of casualties was not uniform across counties. As with mobilization rates, the variation in the cross-county casualty rates could arise from observable economic factors in addition to the random component. Our concern is that socioeconomic factors causing differences in male casualties also affect our outcome variables.

Race The United States military was still segregated during World War II. Even though mobilized men were randomly drafted from the registered pool, less than 4,000 African-Americans were serving in the military and only 12 African-Americans were officers in 1941. In fact, during the war period, the segregation practices of civilian life spilled over into the military. Pressures from the National Association for the Advancement of Colored People led President Roosevelt to pledge that African-Americans would be enlisted according to their percentage in the population.[12] Although this percentage was never actually met during the war, the number of African-Americans in the army grew drastically. Of note, though, blacks were often classified in separate units for combat and were not allowed to fight on the front lines. They were mostly given support duties and were not allowed to be in units with white soldiers. A total of 1.2 million African Americans served in the U.S. Armed Forces in segregated divisions and 708 were killed in action.[13] Below, we show that our main findings are robust to excluding states with a high black population from the analysis.

Education In addition to fitness, the selection of men to serve in the army may have been related to education. Between June and July 1943, 8% of white troops rejected and 34.5% of African-American troops rejected were for educational deficiencies (Jenkins et al., 1944). Men were tested and sent to units based on their educational backgrounds. For instance, servicemen in the infantry branch were less educated as the infantry required a lower logistical burden. Infantry soldiers (also known as foot soldiers) operated under the worst conditions and performed missions that were not assigned to any other units. Even though the infantry branch faced the highest number of casualties, it constituted only 6% of the entire units. Additionally, Appendix Figure A2 shows a weak positive

[12] It was not until 1948 that President Harry S. Truman ordered a desegregation of the Armed Services and equality of treatment and opportunity in the U.S. military without regard to race, color, religion or national origin.

[13] By the end of war, it became more acceptable to have integrated units of both black and white soldiers fighting side by side on the front line in order to maintain the strength of the military (see Sandler, 1992 and Wynn, 1993).

correlation between casualty rates and the county's average years of education for men between 18 and 44 years old in 1940.

Economic Conditions One of the main reasons for deferment was farm occupation in order to maintain the food supply during the WWII period. Acemoglu, Autor, and Lyle, 2004 show that there is a negative correlation between male mobilization and a state's percentage of farmers. With male farmers being deferred, we would expect fewer casualties in rural counties. Additionally, deferments of drafted men could potentially be affected by their economic conditions. Wealthy and connected men could avoid being mobilized or get assigned to non-combat roles. For example, poor men who did not have a record of private medical care may find it harder to obtain medical deferments. However, Appendix Figure A3 shows that the casualty rate and the county's average wage income of men aged between 18 and 44 in 1940 are very weakly correlated.

As a robustness check, we will include the covariates identified by Acemoglu, Autor, and Lyle, 2004 to our baseline model to account for correlates of differences in casualty rates by county that may affect our main outcome. Our results are robust to including 1940 county specific characteristics (such as fathers share, black population share, and average years of education) interacted with the after war year dummies, which suggests that our identification assumption is credible. Additionally, we include county fixed effects in the empirical analysis to control for any county specific time-invariant characteristics that may affect fertility.

1.3.3 Model Specification

Our hypothesis is that the increase in fertility during the Baby Boom period was lower in counties where World War II casualty rates were high. To investigate this hypothesis, we estimate the following specification:

$$y_{ct} = \lambda_c + \delta d_{war} + \mu d_{war} \times R_c + X'_{ct}\omega + \varepsilon_{ct} \tag{1}$$

where y_{ct} is the natural log of total live births or birth rates in county c and year t. We include a full set of county dummies λ_c to control for time-invariant county characteristics. The variable d_{war} equals one for after-war years and zero for the pre-war period. The after war dummy takes the value of zero for 1940 and one for the years 1950, 1960, and 1970 in different regressions, each of which includes data for 1940 and one of these years. This allows us to investigate the short run and the long run effects of war casualties on fertility. The variable R_c is the casualty rate by county. In the binary regressions, it takes the value of one if the county belongs to the high casualty rate category and zero if the county is in the low casualty rate category. In the continuous regressions, this variable is defined as continuous to estimate the impact of a percentage point increase in the casualty rate on fertility. The interaction of d_{war} and R_c shows the effect of the treatment. The coefficient of interest here is thus μ. We cluster standard errors at the county-level.

X_{ct} is a vector of covariates including the natural log of female population of childbearing age (i.e., between 15 and 44 years old). Using the general fertility rate computed as the number of live births by 100 women as the dependent variable yields similar findings (see section 1.5).

In section 1.4, we rely on the state-level variation of these variables in order to reconcile our county-level findings with previous studies at the state level.

As a robustness check, we include state-decade fixed effects to relax the identification assumption. The inclusion of state-decade fixed effects in the model allows us to control for time-varying state policies and shocks such as cross-state variation in pregnancy-related mortality (Albanesi and Olivetti, 2014).

In a set of robustness checks, we control for pre-war county's demographic and socioeconomic characteristics. This is an important specification check since these factors may have caused differences in casualties and fertility and thus lead to a bias in our estimates. More precisely, we interact the "After War" dummy with the following socioeconomic characteristics to allow them to differ by decade: the share of male farmers between the ages of 18 and 44, the share of black men between the ages of 18 and 44, the share of fathers between the ages of 18 and 44 and the average years of education for men between the ages of 18 and 44. We also control for county-level lagged changes between census years 1930–1940 (interacted with the "After War" dummy) in economic or demographic variables that could affect fertility such as age at first marriage (for married women only), Dwelling Ownership (share of those who own a dwelling out of total population for both men and women above the age of 16), and female labor force participation (for women aged between 16 and 64).

1.4 State-Level Results

Before turning our attention to our novel county-level male casualty data, we first replicate the results of Doepke, Hazan, and Maoz, 2015 at the state-level. This exercise serves at least two purposes. First, we believe reproducibility is a key part of the scientific method, and that replications may help to improve our understanding of previous research findings. Second, this exercise may shed some light on the key differences between our state and county analyses.

We first replicate the findings of Doepke, Hazan, and Maoz, 2015 using a similar specification. We then explore the robustness of their results to additional control variables. Our replicated results are presented in Table 1.2, which contains OLS estimates of equation 1 at the state-level. In their analysis, Doepke, Hazan, and Maoz, 2015 rely on pooled census data from 1940 and 1960 and restrict the sample to women aged between 25 and 35 years old.[14] Their main dependent variables are "Children under age of 5" and "Children ever born". For our replication, we rely on the number of children under the age of 5 (columns 1–4) from the pooled micro data from the 1940 and 1960

[14] The authors distinguish between two age groups, women aged 25 to 35 defined as "young" and women aged 45 to 55 defined as "old". We focus on the younger group of women on which the effect of WWII mobilized men is documented.

censuses and complement this analysis using data on the number of total live births (columns 5–8) from Vital Statistics. In the top panel, we rely on the casualty rate, while the bottom panel shows estimates for the mobilization rate. Columns 1, 2, 5 and 6 present estimates for the binary regressions, in which the casualty (or mobilization) rate variable is a dummy that takes the value of one for high-casualty (or mobilization) rate states and zero for low-casualty (or mobilization) rate states. Columns 3, 4, 7 and 8 show estimates for the continuous regressions, in which casualty (or mobilization) rate is a continuous variable.

We successfully replicate the state-level results in Doepke, Hazan, and Maoz, 2015 using both mobilization and casualty rates. In columns 1 and 3, we find that state casualty and mobilization rates are positively correlated with the number of children under the age of 5 for women aged between 25 and 35 years old. The point estimates are strikingly similar in the binary form regressions but differ in magnitude in the continuous form ones. The large difference in magnitudes is due to differences in means as the mean for the casualty rate is around 1%, while the mean for the mobilization rate is around 45%. For instance, Beta coefficients suggest that a one standard deviation increase in casualty rates leads to an increase of 0.03 standard deviation in total births, while a one standard deviation increase in mobilization rates is associated with a 0.04 standard deviation in births (column 3).

We confirm these results from Census data by relying on total births count from Vital Statistics data. In columns 5 and 7, the point estimates for the binary form regressions are positive and statistically insignificant (and of similar magnitude) for both mobilization and casualty rates. The point estimates for the continuous form are also positive, but significant only for mobilization. Overall, these results suggest that Doepke, Hazan, and Maoz, 2015's main results are reproducible and that relying on casualty rather than mobilization leads to similar conclusions.

To further examine the reproducibility of Doepke, Hazan, and Maoz, 2015's conclusions, we add pre-war state demographic and socioeconomic characteristics to the model. More precisely, we add a set of controls to account for state characteristics that may have caused differences in casualties and mobilization: the share of male farmers between the ages of 18 and 44, the share of black men between the ages of 18 and 44, the share of fathers between the ages of 18 and 44 and the average years of education for men between the ages of 18 and 44. All controls are interacted with the "After War" dummy to allow them to differ by decade. The point estimates are presented in columns 2, 4, 6 and 8. We also conduct validation tests for unobservable selection and coefficient stability in columns 2, 4, 6, and 8 following Oster, 2019's methodology.[15]

Conditional on these controls, the estimates at the state-level for casualty and mobilization rates become *negative*, suggesting an omitted variable bias in the state-level estimates. In other words, the estimates for the binary and continuous form regressions for both mobilization and casualty rates have a

[15] Oster, 2019 argues that $|\delta|>1$ leaves a limited scope for unobservables to explain the results. Our reported parameter δ thus suggests that the controlled variables are important counfounders to the relationship between missing men and fertility.

negative sign once we control for selected pre-war state characteristics. Among the included pre-war characteristics, the share of black men is the key variable causing the switch in sign.

These findings provide a major motivation to the use of a finer geographic level for analyzing the impact of WWII casualty on fertility since states may be composed of counties with very different socioeconomic characteristics and with different casualty and fertility rates. In section 1.5, we show that the sign of our county-level estimates is robust to the inclusion of a large set of pre-war county characteristics, as well as state-decade dummies.

To sum up, we successfully replicate Doepke, Hazan, and Maoz, 2015's fertility results using similar specifications, but provide evidence that the estimates are not robust to the inclusion of key pre-war state characteristics. We argue that relying on county-level data strengthens the credibility of the analysis as it provides the possibility to further control for other (potentially) important state-shocks and state-decade fixed effects. Of note, we have shown that the choice of data source (vital statistics versus pooled Census data) and treatment measure for the intensity of war (mobilization versus casualty rate) does not affect our state-level conclusions.

1.5 County-Level Results

In this section, we estimate the treatment effect of war on fertility by using male casualty rates as a measure of war intensity. Our analysis is now at the county-level and our model includes county fixed effects.

1.5.1 Impacts on Fertility

Table 1.3 contains OLS estimates of equation 1. The dependent variable is the natural log of total live births. We cluster standard errors at the county-level. We include county fixed effects and control for the female population in all the regressions. Columns 1, 3 and 5 present estimates for the binary regressions, in which the casualty rate variable is a dummy that takes the value of one for high-casualty rate counties and zero for low-casualty rate counties. Columns 2, 4 and 6 show estimates for the continuous regressions, in which casualty rate is a continuous variable.

What clearly emerges is that county casualty rates are negatively associated with total live births in 1950. Columns 1 and 2 show the estimates for the years 1940–1950. Results indicate that there was a large and highly significant difference in the fertility change between high- and low- casualty rate counties over this time period. The estimate in the binary regression is negative and statistically significant at the 1% level, suggesting that high-casualty rate counties had significantly fewer total live births compared to counties with lower casualty rates. Interestingly, the increase in the total live birth in 1950 seems to be offset by WWII casualties in the high casualty rate counties. In the continuous regression, the point estimate of -0.166 (standard error of 0.04) implies that a one percentage point increase in the male casualty rate during World War II leads to a 15.3% decrease in fertility.

Columns 3 and 4 show comparable estimates for the years 1940–1960. There is weak evidence that WWII casualties affected total live births in 1960. While the coefficient of the binary regression becomes statistically insignificant, the estimate in the continuous regression shows a persistent effect of male war casualties. Columns 5 and 6 suggest that WWII casualties did not affect fertility rates in 1970. The estimates are statistically insignificant and smaller in magnitude than for columns 1–4. This result is not surprising as many women of childbearing age (in 1970) were born after WWII.

To observe how the gaps in fertility have been opening and closing during the pre- and post-war period, we rely on yearly county-level data on births by place of occurrence from 1933 to 1972. We illustrate our results by plotting the estimated coefficients from the continuous regression on the natural log of total live births in Figure 1.4. We omit the year before the entry of the U.S. into WWII, i.e., 1940 is the reference year. We confirm the pre-treatment trends in the period between 1935 and 1939 and a temporary negative effect of war male casualties on total live births during the 1950s and 1960s, with the impact completely fading away during the early 1970s. Of note, the negative effect starts as early as 1941, when the U.S. was not at war. This result is consistent with increased mobilization activity at the time, even in 1940–1941 in advance of U.S. entry into the war. According to the US Army Center of Military History, mobilization evolved from its gradual beginnings in 1940, speeding up in 1941, expanding dramatically in 1942, and reaching its peak in production in 1943. With the German invasion of Poland in September 1939, President Roosevelt proclaimed a limited national emergency and authorized an increase to 227,000 for the Regular Army and to 235,000 for the National Guard. Thus, a sizeable number of men were employed in the army before the U.S. officially entered the war in December 1941.

We check in Table 1.4 whether our findings are driven by time-varying changes at the state-level. Our analysis relies on within-state across-county variation because we control for state-decade fixed effects in our model. The inclusion of state-decade fixed effects has no effect on our main conclusions. Our estimates remain negative and statistically significant, although smaller in magnitude.

In Appendix Table A2, columns 1–2, we test the parallel trend assumption by looking for pre-trends. This assumption requires that in the absence of WWII casualties, the difference between high and low casualty rate counties has a constant trend over time. Unfortunately, county data on total *live* births are not available for 1930. We instead have to rely on total births. Our pre-war estimates indicate that there was no significant relationship between the change in total births from 1930 to 1940 and county casualty rates during WWII. The estimates for both the binary and continuous regressions are very small and statistically insignificant at conventional levels.

So far, our results provide suggestive evidence that war casualties had a negative effect on fertility. In other words, male casualties during WWII may have slowed down the rise of fertility for counties with many war casualties. We check the robustness of our results in the next subsection.

1.5.2 Robustness Checks

In a first set of robustness checks, we examine the endogeneity of our main independent variable by regressing the natural log of total live births in 1940 on the casualty rate at the county-level. Appendix Figure A4 shows that the regression coefficient is equal to zero, suggesting that casualty rates do not explain the pre-war levels of total live births across counties.

In another set of specification checks, we replicate our analysis of Table 1.3, but controlling for pre-war county demographic and socioeconomic characteristics, in Appendix Table A3. As mentioned in Section 1.3, we interact the "After War" dummy with socioeconomic characteristics to allow them to differ by decade. We also control for county-level lagged economic and demographic changes between census years 1930–1940 (interacted with the "After War" dummy).[16] Moreover, due to the endogeneity of the farmers' share, we drop counties with higher than 80% share of male farmers (aged between 18 and 44) in 1940. Our point estimates for 1950 and 1960 are smaller in magnitude, but remain statistically significant at conventional levels. In contrast, our point estimates for 1970 are now larger and significant for the continuous regression.[17]

In Appendix Table A5, we add the set of covariates to the specification including state-decade fixed effects. The point estimates are similar to Table 1.4 and Appendix Table A3, suggesting that the inclusion of state trends or demographic characteristics equally affect the magnitude of our estimates.

We also check whether the negative relationship between fertility and WWII casualties is driven by less populous counties. To rule out this idea, we split the sample by the median share of counties' population in 1940 relative to the state's population to which they belong in Appendix Table A6. The estimates for the binary (continuous) regressions are larger (smaller) in magnitude in less populous counties in 1950 and 1960. As as additional exercise, we also weight counties by this population share in Appendix Table A7.[18] The estimates remain negative and significant. The estimates for the binary regressions are smaller in magnitude for 1950 and 1960, while the estimates for the continuous regressions are larger than in our baseline.

We also show that the negative effect of WWII casualties on births occurred in both rural and urban counties. In Appendix Table A9, we split the sample into two groups of counties based on the share of urban population in 1940, i.e., above and below the median. The estimates for 1950 and 1960 are negative and of similar magnitude for both set of counties. Overall, our findings suggest that the negative impact of war casualties on fertility at the county-level is not solely driven by less populous (or rural) counties.

[16]The inclusion of the extended set of controls in the state-level analysis of Table 1.2 does not affect our findings. See Appendix Table A4.

[17]While the reported Oster, 2019 test highlights the potential importance of unobserved confounders in the baseline models, the additional covariates have limited influence on the estimates as the pattern of our findings remains unchanged.

[18]Weighting counties based on their 1940's population relative to the 1940 U.S. population yields similar results (see Appendix Table A8).

In our main specification, we are controlling for female population. This may be an issue if female population was affected by WWII. Instead of the raw birth counts, we examine the impact of WWII male casualties on birth rates computed as the number of total live births divided by the female population aged between 15 and 44, and multiplied by 100 for each county and decade. Our estimates are presented in Appendix Table A10.[19] Our main findings remain mostly unchanged and suggest that casualty rates are negatively related to fertility in 1950 and 1960.

To provide a check of whether our findings are driven by states with a large share of black residents, we replicate our baseline analysis, but exclude states where the population is more than 20% black. The estimates are presented in Appendix Table A12. Our estimates are slightly smaller in magnitude, but have the same sign and remain statistically significant at conventional levels.

Last, to better understand the distribution of our main independent variable, we illustrate the variation in casualty rates across counties in Appendix Figure A5. The casualty rates' distribution is symmetric and unimodal. Therefore, splitting casualty rates across groups at the median rather than the mean does not affect our main results. (See Appendix Table A13 for the analysis.) The figure also shows a right tail to the distribution suggesting the presence of outliers for the top values of casualty rate. As this might affect our findings when relying on this variable in its continuous form, we exclude the top 1% of casualty rate counties (i.e., 30 counties with casualty rate above 2.11) in Appendix Table A14. Our results in 1950 hold and become larger in magnitude for the continuous form, while the estimate from the 1960 continuous regression slightly decreases in magnitude and loses its significance. Our estimates in columns 5 and 6 are insignificant and approximatively equal to 0, confirming the absence of the effect in 1970.

1.5.3 Migration

We now turn to analysing the role of selective migration after the war. Arguably, men residing in counties with relatively fewer casualties might migrate to counties where sex ratios are more imbalanced. There are plausibly better job opportunities in high casualty rate counties and relatively more unmarried women. In other words, geographical differences in the thickness of the job and marriage markets due to the war could lead to selective migration. Selective migration would "compensate" for missing men in high casualty rate counties. We would thus be underestimating the effect of WWII casualties in our baseline analysis. To examine whether our results are driven by selective migration, we repeat our baseline analysis, but exclude counties with very negative and very positive net migration after the war. More precisely, we exclude counties that are below and above one standard deviation from the mean in total net migration, i.e., we restrict the sample to counties in a bandwidth of total net migration between -30 and 10 net migrants per 100 individuals.

[19] Our results are also robust to controlling for the total population instead of the female population. Appendix Table A11 shows the estimates.

Appendix Table A15 presents our estimates for this subset of counties. This table has the same structure as Table 1.3. The coefficients of interest in columns 1, 3 and 5 are virtually identical to the ones in Table 1.3, while the estimates for the continuous regressions for the years 1950 and 1960 (columns 2 and 4) are larger in magnitude.

1.6 Channels

Our findings so far are intriguing because WWII casualties are negatively correlated to fertility during the 1950s relative to before the war. In this section, we discuss different mechanisms through which male casualties could have impacted fertility. We then rely on data at the SEA-level to test some of these channels.

1.6.1 Fertility

Before turning our attention to the mechanisms, we check whether our fertility results are similar at the SEA-level. Appendix Table A16 provides summary statistics. There is a total of 466 SEAs, 243 of which are categorized as high-casualty rate.

Appendix Table A17 presents estimates of equation 1 where the dependent variable is the natural log of total live births. The structure of the table is the same as Table 1.3. The only differences are that we replace our county fixed effects by SEA fixed effects, and that the standard errors are now clustered at the SEA-level. Our estimates and conclusions are very similar. The estimates are all negative and statistically significant in columns 1–4. The estimated effect remains negative and not statistically significant in 1970, with a smaller magnitude. Of note, estimates from the SEA-level binary measures are not directly comparable to the county-level measures due to the difference in means between groups. Our rescaled coefficient estimate in column 1 suggests that high-casualty rate SEAs had around 8% fewer total live births compared to those with lower casualty rates, which is close in magnitude to our county-level findings.

1.6.2 Mechanisms

The absence of men during WWII could have led to a slowdown in the fertility increase during the Baby Boom period through many channels. A first mechanism through which WWII casualties may affect fertility is the increase in female employment. During WWII, women across the U.S. were highly encouraged to work in different industries and take over jobs previously done by men. “Rosie the Riveter”, a cultural icon of WWII, is now used as a symbol of American feminism and women’s economic power. Acemoglu, Autor, and Lyle, 2004 document that the war induced a large positive shock to the demand for female labor as male mobilization drew many women into the workforce permanently. While men were fighting the war, millions of women were drawn into the labor force and replaced men in factories and offices. In the same line, Goldin and Olivetti, 2013 provide empirical evidence that married women without children experienced the largest increase in labor force participation and weeks worked.

An increase in the female employment both during and after WWII would potentially lead to a decrease in fertility rates due to an increase in the cost of having and raising a child. In high casualty rate counties, the higher share of women who work have less time to raise children, and thus may decide to have fewer children.[20]

In the "quality-quantity" trade-off theory proposed by Becker, 1960,[21] increases in wages induce parents to substitute the quantity of children for higher quality. Despite the fact that Acemoglu, Autor, and Lyle, 2004 found that the shift in female labor force participation increased market competition and lowered individual wages, the existence of an additional source of income within a household would have a positive impact on the couple's earnings. An increase in household wages could tempt married couples to favor quality of children over quantity.[22]

1.6.3 Quality-Quantity Trade-Off

We now test whether WWII casualties affected female employment and household income using individual-level data. An increase in household income could lead parents to substitute the quantity of children for higher quality. The econometric model is as follows:

$$y_{ist} = \lambda_s + \alpha d_{war} + \beta d_{war} \times R_s + X'_{ist}\gamma + \varepsilon_{ist}, \tag{2}$$

where y_{ist} is the outcome variable of interest for individual i from SEA s in year t. R_s is the casualty rate by SEA, while d_{war} equals one in 1950 (post-WWII) and zero in 1940 (pre-WWII). The interaction of d_{war} and R_{ss} shows the effect of WWII casualties on the outcome variable. β is thus our coefficient of interest. X_{ist} is a set of individual characteristics. Our set of (exogenous) individual characteristics includes age, race and ethnicity. We also include the following (potentially endogenous) controls in some specifications: marital status, education and experience. We include SEA fixed effects and report standard errors clustered at the SEA-level.

[20] A recent study by Vandenbroucke, 2014 builds and calibrates a fertility model to fit the birth rate in France from 1800 until World War I (WWI). He finds that the fall in the birth rate during the war is mostly due to the loss of expected income associated with the risk that a wife remains alone. Boehnke and Gay, 2017 also provide evidence that areas with relatively more military casualties in France during WWI experienced a larger increase in female labor throughout the interwar period.

[21] A vast literature focuses on this channel. See Becker and Barro, 1988, Becker and Barro, 1989, Galor and Weil, 2000, Jones, Schoonbroodt, and Tertilt, 2011, Albanesi and Olivetti, 2014, Manuelli and Seshadri, 2009, and Vandenbroucke, 2014.

[22] Higher incomes could also facilitate the adoption of modern household technologies (Bose, Jain, and Walker, 2020). Access to modern household technologies would free women's time from basic housework, which may have led to increased investments in children's health (Lewis, 2018).

Table 1.5 displays the results of this model. Each entry shows the estimate of the interaction term coefficient β for a different specification. Columns 1–3 (4–6) present estimates for the binary (continuous) regressions. Columns 1 and 4 only include SEA fixed effects. In columns 2 and 5, we add to the model our set of exogenous controls. Columns 3 and 6 also control for marital status, education level and experience of the individual.

The first and second panels show the effect of WWII casualties on female employment and weeks worked per year for women of childbearing age. The estimates are statistically significant at the 1% level and suggest that female employment and weeks worked were higher in SEAs with higher casualty rates. The point estimates suggest that female employment in 1950 was 1.1 percentage points higher, and that women of childbearing age worked an additional 1.4 weeks per year, in high casualty rate SEAs. Controlling for age, race and ethnicity does not change the size or significance of the estimates.

These findings provide suggestive evidence that women living in areas with more male casualties were more likely to stay in the labor market (and work more weeks) after the war.[23] These findings are in line with the idea that decreased fertility in high-casualty rate counties is partly due to the induced shifts in female labor supply during and after WWII.

The third and fourth panels test whether the SEA casualty rate is related to wages and household income. Acemoglu, Autor, and Lyle, 2004 show that women worked more after the war in states with higher mobilization rates, which lowered female and male wages. Our estimates support their findings, showing that weekly wages for males and females working for at least 35 hours per week were lower in areas where the casualty rate was high.[24] However, we think it is more relevant to look at the effect of war casualties on the household's income as it relates directly to Becker's quality-quantity trade-off theory. Our estimates show that the effect of WWII on household income was positive in 1950. More precisely, our estimates suggest that household income was about 3% higher in areas where the casualty rate was high.

In Panel five, we restrict the sample of women to those who have at least one child. In our sample, 12.2% (in 1940) and 18.5% (in 1950) of employed women had at least one child. Restricting the sample to mothers allows us to investigate whether WWII increased women's employment only up to the point that they had a first child. We do not find empirical evidence supporting this idea as our estimates suggest that WWII casualties significantly increased female employment for women with children.[25]

[23] It has not been clearly established why so many women stayed in the labor market after the war. Acemoglu, Autor, and Lyle, 2004 hypothesize that it is possibly due to a change in women's preferences, opportunities and/or information about available work. See Mulligan, 1998 for an investigation of the causes of the increase in female labor force participation during WWII.

[24] The sample is restricted to individuals between the age of 15 and 65. Top-coded values are imputed as 1.5 times the censored value. Farmers, self-employed and unpaid family workers are excluded.

[25] Note that when we restrict our sample to women without children, the effect on female employment is larger (around 0.017 in binary form and 0.070 in continuous form), which partially goes in line with Goldin and Olivetti, 2013's findings.

In Table 1.6 and Appendix Tables A18, A19 and A20, we test whether WWII male casualties differently affected younger and older cohorts. These tables show estimates of equation 2 in its continuous and binary forms, separately. We divide the sample into five age groups: 15 to 24, 25 to 34, 35 to 44, 45 to 54 and 55 to 65 years old. For this analysis, we compare women in each age category in 1950 to women in the same age category in 1940. We include SEA fixed effects and our set of exogenous controls. (See Appendix Tables A18 and A20 for the models including the endogenous controls.)

First, we re-examine the impact on fertility at the individual-level. We rely on the continuous variable "Children under age of 5", which indicates the number of own children age 4 and under residing in the household. We restrict the sample to women who gave birth in the period between 1935 and 1940 (pre-war period) versus the period between 1945 and 1950 (post-war period). Our estimates are large, negative and statistically significant for women under the age of 44.[26] In contrast, the estimate is a well-estimated zero for older women. Moreover, we rely on the dummy variable "Having a Child" as an alternative fertility measure. We find that WWII casualties had a significantly negative effect on the probability of having a child for women aged between 25 and 34 years old.[27] (See Appendix Tables A18, A19 and A20.) These results suggest that fertility of women who were of childbearing age during WWII was affected the most. The negative effect on all-age fertility rates in 1950 could still be consistent with Doepke, Hazan, and Maoz, 2015's findings if a large decrease in fertility among older women offset any rise among younger cohorts. This could be the case in the presence of labor market crowd-out for younger cohorts. We investigate the impact of WWII casualties on labor market outcomes by age group in what follows.

For female employment, our estimates are all positive and statistically significant for women aged 15 to 54. Interestingly, our estimates show that younger cohorts were affected the most by WWII casualties. The estimates are twice as large for women between the age of 15 and 24 than for women aged 25 to 44, suggesting that there was no labor market crowd-out for younger cohorts. The estimates are also positive and quite large for weeks worked. In the binary regressions (Appendix Tables A19 and A20), high-casualty rate areas had an increase of 1.7 weeks worked for females aged 25 to 35 years old in comparison to a 1 week increase for those in the age category 45–54.

To examine more closely Becker's trade-off theory and check whether parents in high-casualty rate counties are investing more in the quality of their children, we investigate the impact of WWII casualties on children's health outcomes.[28]

[26]The estimated effect on the number of children under the age of 5 for single women is also negative, suggesting a lower number of out of wedlock children in the high-casualty rates areas.

[27]Restricting the sample to women who had at least one child above 5 years old does not change our results. Estimates are all negative, but statistically significant solely for younger women (aged 15 to 25 years old).

[28]The Veterans Affairs (VA) health care system grew enormously in the decades following WWII. This could be an issue if spouses of veterans could deliver in VA hospitals. The Department of Veterans Affairs confirmed us that (1) women were not uniformly considered as a regular part of the military during WWII, (2) VA did not include maternity benefits

More precisely, we investigate the effect of WWII casualties on the percentage of births attended by physicians at hospital and infant mortality under the age of one.[29] In our sample, 37.3% (79.2%) of births are attended by physicians at hospital in 1940 (1950). A larger percentage of births attended by skilled health personnel would indicate better health conditions for newborn babies.

Table 1.7 shows the estimates of the interaction term coefficient in equation 1 on the percentage of births attended by physicians at hospital (columns 1–4) and the natural log of infant mortality under the age of one (columns 5–6).[30] Results suggest that WWII male casualties had a positive effect on the percentage of births attended by physicians at hospital in 1950 and 1959 (data not available in 1960). Moreover, column 6 shows that a 1 percentage point increase in the male casualty rate during WWII leads to a 21.65% decrease in infant mortality in 1960.

We plot yearly estimates of the effect of WWII casualties on attended births and infant mortality in Appendix Figures A6 and A7, respectively. County-level data on births by attendant by place of residence is available between 1939 and 1959. For infant mortality, data is available by place of occurrence from 1915 to 1941, and from 1959 to 1972. For the impact on births attended by physicians, estimates show that the effect begins in 1941 and decreases in magnitude during the late 1950s. As for the impact on infant mortality, parallel trends can be observed in the period between 1935 and 1939. Again, the effect starts to quick in as early as 1941, is significantly negative in the period between 1959 and 1966, and starts to decrease in magnitude until it fades away completely in late 1960s. These findings suggest that the pattern of the channels at play fits the pattern of our main effect on total live births in Figure 1.4.

To sum up, our findings provide suggestive evidence that WWII casualties increased female employment and household income in 1950. These results could explain the lower increase in fertility in high-casualty rate areas through Becker's quality-quantity trade-off theory. Parents in high-casualty counties earn more and may thus invest to a greater extent in their children's human capital.

1.6.4 Sex Ratios and Marriage Market

Another mechanism through which conflict may affect fertility is sex ratios.[31] The war could have affected women's chance of finding a partner especially in

in the medical benefits package, and (3) male veterans' non-veteran spouses have never been covered by VA hospitals.

[29]In our sample, infant mortality was about 5.5% in 1940 and decreased to approximately 4.1% by 1950.

[30]Note that many counties have missing information for infant mortality under the age of one. We report estimates using a balanced sample of counties. The estimates using an unbalanced sample of counties are strikingly similar.

[31]Another channel through which war may impact fertility is trauma. Festy, 1984 argues that during a troubled period, feasible births decline while desired births remain constant. Caldwell, 2004 empirically examines 13 social crises from the English Civil War in the 1600s to the fall of communism, and finds remarkable drops in birth rates in each of the cases. This direct impact of conflict remains less important in the case of the U.S. in WWII since attacks on American soil were very limited.

areas where mobilization or casualty rates were high, and thus delay the age of having a first child.

In the sixth panel of Table 1.5, we test whether WWII casualties affected the probability of getting married for women of childbearing age (between 15 and 44). To capture the direct effect of WWII on the marriage market outcomes, we restrict the sample to include only women who got married in the last 10 years.[32] The dependent variable in the seventh panel is the age at first birth. In the eighth panel, we focus on first marriages and examine the effect of male casualties on the spousal age gap. Our results suggest that WWII casualties did not significantly affect women's marriage rates, the age of becoming a mother, nor the age difference between married couples.[33] The estimates are all small, positive and statistically insignificant at conventional levels.

We also check the impact of WWII casualties on marital outcomes by age category in Table 1.6 and Appendix Tables A18, A19 and A20. We do not find strong evidence that WWII casualties had any effect on marriage rates and the age gap at marriage for all the age subgroups.[34] One exception is for the age at first birth for women between 15 and 25. Our estimates suggest that WWII casualties led to a significant increase for these women who were most likely unmarried during WWII and reached their childbearing age during or after war (aged 10 to 15 years old in 1945).

Overall, our results suggest that the lower increase in fertility in high-casualty counties is most likely driven by an increase in female employment and household' income rather than male scarcity and lower probability of finding a partner. Our finidngs are in line with Cardoso and Morin, 2018 who argue that a sex ratio imbalance in Portugal has led to an increase in female labor force participation but had no effect on the marriage market.[35]

1.7 Conclusion

The United States underwent major demographic and economic changes following WWII. In an unprecedented rise in fertility, 76 million children were born between 1946 and 1964. In this study, we investigate whether this Baby Boom might have been a consequence of WWII. We identify the impact of WWII using male casualties at the state, county and state economic area-levels and show that the fertility rise in 1950 relative to 1940 was smaller in counties (states) where the casualty (or mobilization) rate was high. The negative effect of casualties on births started to decrease in 1960 and vanished by 1970.

[32] Since most women were married by the age of 30 in 1950, this sample restriction allows us to restrict the treated group to women who got married during or after WWII.

[33] Additionally, we observe no effect of WWII male casualties on divorce.

[34] Examining the effect of WWII casualties on the proportion of women ever married yields similar conclusions as we observe no effect for young women.

[35] A plausible explanation of the absence of the effect on marriage is that WWII casualties had a very small impact on sex ratio imbalance. We compute the sex ratio at the SEA-level in Appendix Table A21 and find that the decrease is low (around 4% in the continuous regression), suggesting that U.S. casualties had a little impact on sex ratio imbalance.

There is considerable evidence that World War II led to a large shift in women's economic role. Based on our findings, it seems that WWII casualties slowed down the Baby Boom, especially since the opportunity cost of having a child was higher for working women. Moreover, our empirical findings suggest that 1950 household incomes increased more in areas with higher casualty rates. Overall, our results are consistent with Becker and Barro, 1989's quality/quantity of children theory. WWII led to an increase in female employment and households' income, which had persistent effects through the 1950s for counties with many missing men.

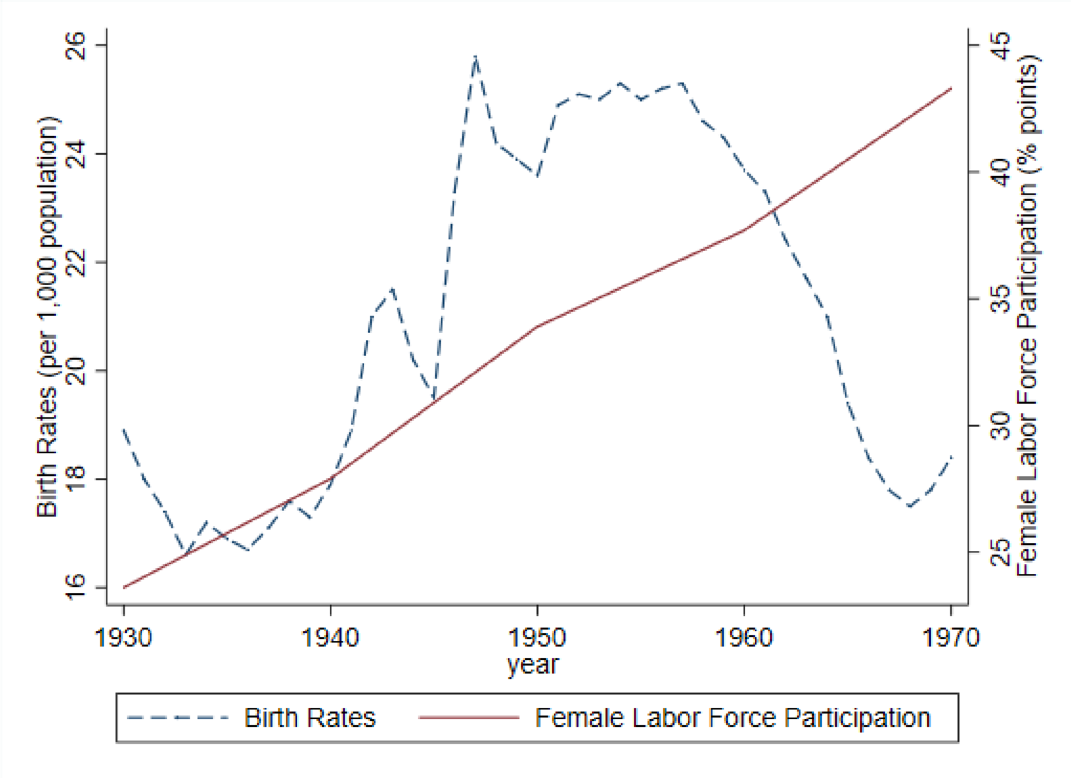


FIGURE 1.1: Birth rates and female labor force participation in the U.S. *Sources*: Vital Statistics for data on births rates. See Blau, Ferber, and Winkler, 2002 for data on female participation rates.

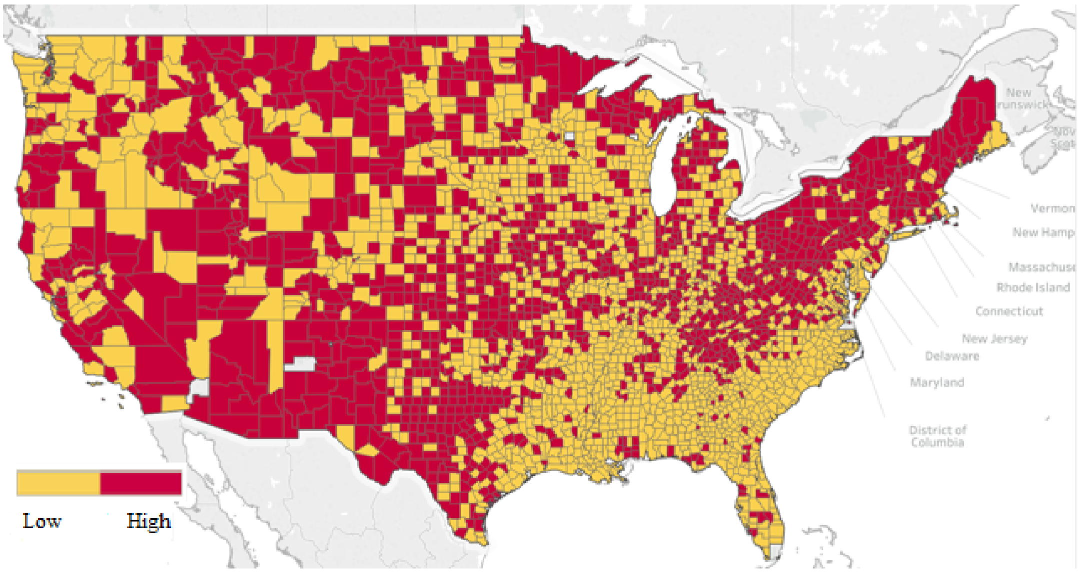
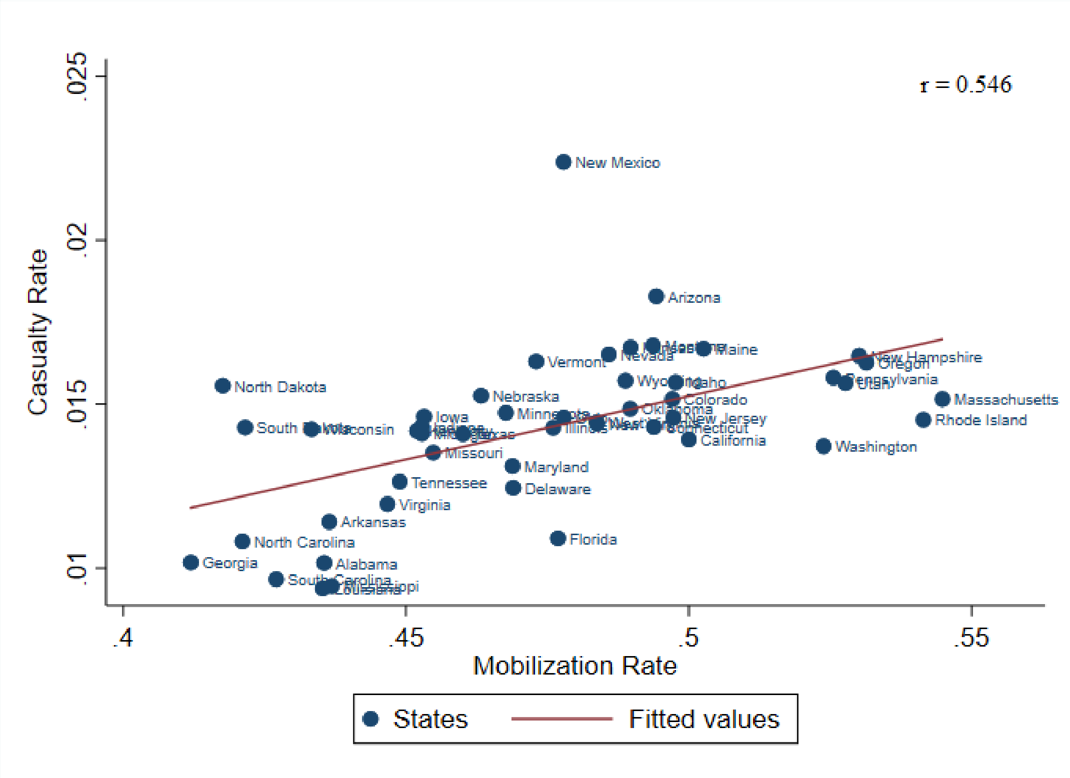


FIGURE 1.2: County-level geographic distribution of low and high casualty rates.



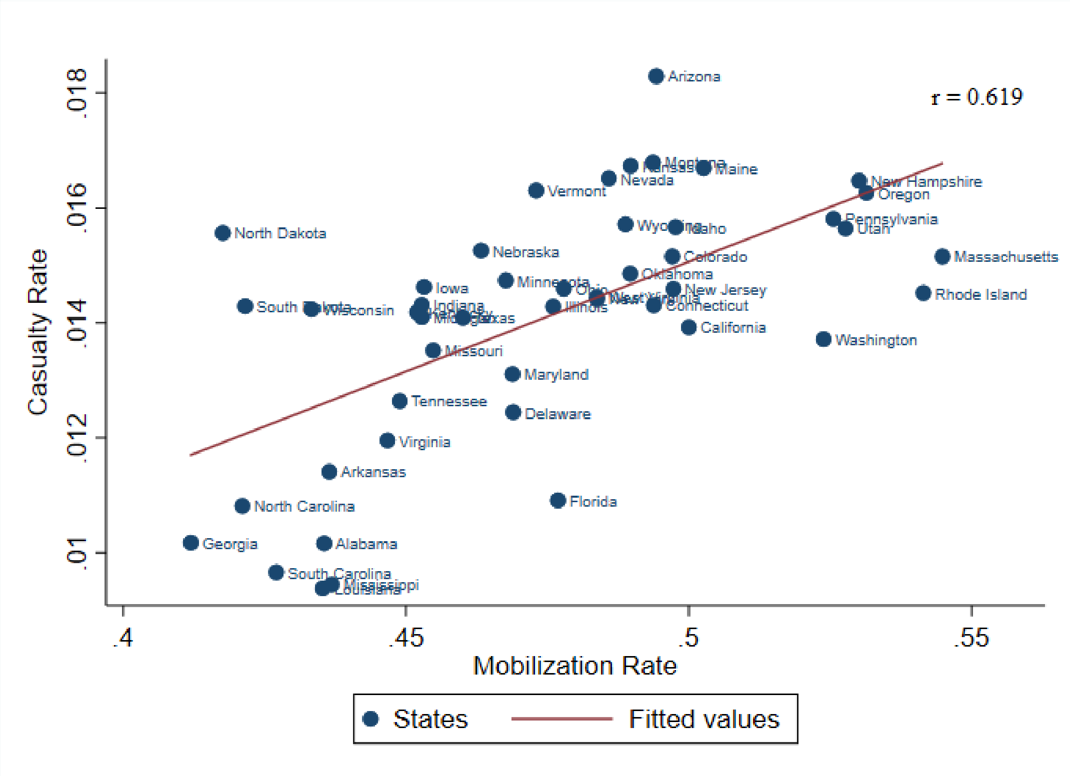


FIGURE 1.3: Correlation between mobilization rate and casualty rate at the state level, with and without New Mexico state. *Source*: Mobilization rates are from Acemoglu, Autor, and Lyle, 2004.

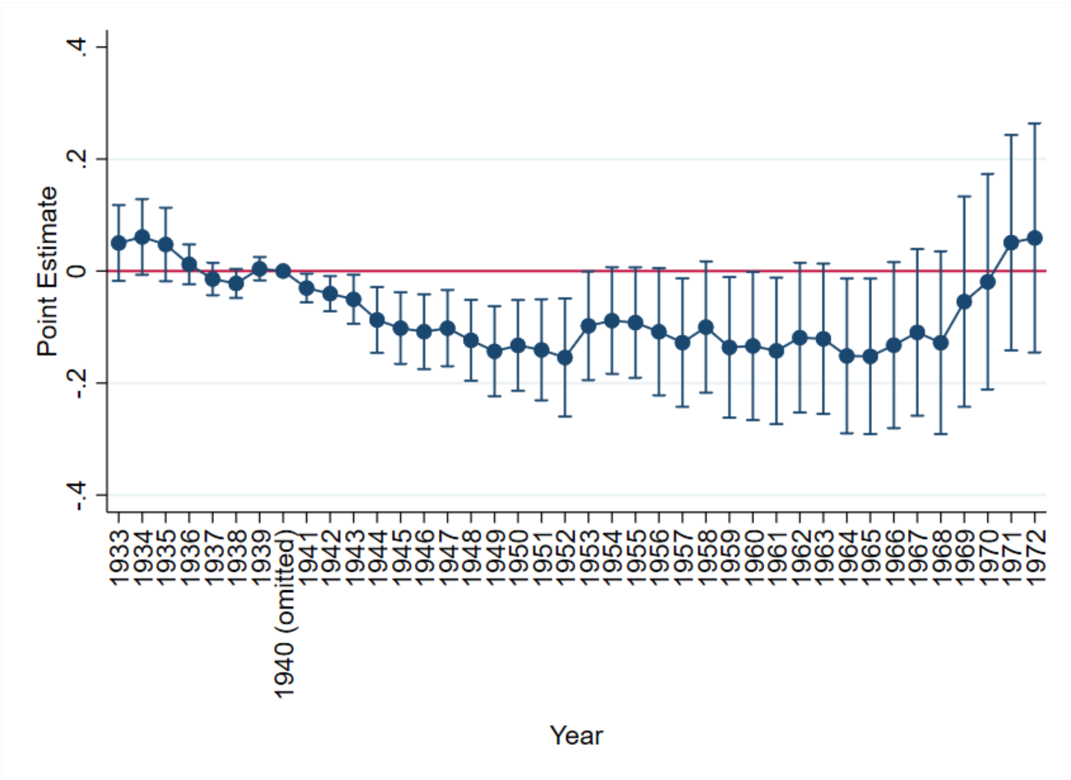


FIGURE 1.4: This figure plots the estimated coefficients from the continuous regression on the natural log of total births by place of occurrence in the United States between 1933 and 1972. The year 1940 is omitted from the regression, i.e., omitted category. *Source*: U.S. County-Level Natality and Mortality Data, 1915-2007 (Bailey et al., 2016). Note that data on births by place of occurrence begins to be complete in the dataset starting 1933.

TABLE 1.1: Demographic Characteristics in Low and High Casualty Rate Counties

	All Counties		Low Rate Counties		High Rate Counties	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Panel A:						
Casualties	98.5	421.7	98.9	542.4	98.0	211.6
Men Population$_{18-44}$	10157	46545	11633	61007	8465	19518
Casualty Rates	1.001	0.391	0.776	0.175	1.260	0.411
Panel B:						
Total Live Births $_{1940}$	758	2,802	853	3,629	650	1,321
Total Live Births $_{1950}$	1,136	4,556	1,289	5,856	961	2,279
Total Live Births $_{1960}$	1,351	5,659	1,534	7,218	1,142	2,990
Total Live Births $_{1970}$	1,185	5,068	1,353	6,446	992	2,724
Panel C:						
Female Population $_{1940}$	10,305	49,306	11,884	64,672	8,497	20,508
Female Population $_{1950}$	10,993	50,717	12,589	66,146	9,165	22,380
Female Population $_{1960}$	11,557	49,600	13,122	63,746	9,761	24,831
Female Population $_{1970}$	13,585	56,129	15,373	71,250	11,534	30,577
Observations	3,070		1,639		1,431	

Notes: Data are from the National Archives monographs and National Historical Geographic Information System. County casualty rate is the number of casualties divided by the number of men aged between 18 and 44 in 1940, multiplied by 100. "Female population" represents the total number of females of childbearing age per county. There are a total of 3,070 counties. We use the casualty rate mean to split the sample into two categories. There are 1,639 counties in the "Low Rate Counties" category and 1,431 counties in the "High Rate Counties" category. Alaska, the District of Columbia and Hawaii are not included in the analysis. The number of casualties is not available for Virginia cities, except for Alexandria city which is included in Arlington county. Bronx, Queens, New York, Richmond, and Kings counties are all included in New York City.

TABLE 1.2: Estimates of the impact of WWII Casualties and Mobilization at the State-Level

	Census Data				Vital Statistics Data			
	Binary		Cont.		Binary		Cont.	
Dependent Variable	(1)	(2)	(3)	(4)	(5).	(6)	(7)	(8)
Panel A: Casualties × After War 1940–1960								
Children Under Age of 5	0.022	-0.023	0.085	-0.121				
	(0.020)	(0.020)	(0.062)	(0.080)				
Log of Total Live Births					0.038	-0.054	0.010	-0.206
					(0.036)	(0.034)	(0.086)	(0.115)
Panel B: Mobilization × After War 1940–1960								
Children Under Age of 5	0.025	-0.041	0.652	-0.290				
	(0.020)	(0.016)	(0.318)	(0.257)				
Log of Total Live Births					0.037	-0.024	0.936	-0.347
					(0.042)	(0.037)	(0.491)	(0.554)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Exogenous Controls	✓	✓	✓	✓				
Endogenous Controls	✓	✓	✓	✓				
Female Population					✓	✓	✓	✓
Black Population$_{1940}$		✓		✓		✓		✓
Years of Education$_{1940}$		✓		✓		✓		✓
Fathers Share$_{1940}$		✓		✓		✓		✓
# of States	48	48	48	48	48	48	48	48
Observations	242,833	242,833	242,833	242,833	96	96	96	96
R-Squared	0.169	0.170	0.169	0.170	0.962	0.979	0.962	0.980
Oster δ for $\beta = 0$		-0.027		-0.021		-0.320		-0.780

Notes: Estimates in columns 1–4 are from separate regressions using pooled micro data from the 1940 and 1960 censuses. Estimates in columns 5–8 are from separate regressions using Vital Statistics data (1940 and 1960). Each outcome variable in panel A is regressed on the World War II casualty rate interacted with a 1960 year indicator variable. Each outcome variable in panel B is regressed on the World War II mobilization rate interacted with a 1960 year indicator variable. The variable "Children Under Age of 5" indicates the number of own children age 4 and under residing with each individual for women aged 25 to 35. Exogenous controls include age, race and ethnicity. Endogenous controls include the education level and marital status. State fixed effects are included for all regressions. We control for pre-war state socioeconomic characteristics that may have caused differences in casualties: "Black Population Share in 1940" as the number of black men aged between 18 and 44 divided by the total population of men in the same age group, "Fathers Share in 1940" as the share of men between 18 and 44 years old who are married and have at least one child, and "Average Years of Education in 1940" as the state-average years of education for men between 18 and 44 years old. Controls are all interacted with the "After War" dummy. Census samples exclude individuals living in institutional group quarters and include person weights used in all calculations. Alaska, the District of Columbia and Hawaii are not included in the analysis. The Oster, 2019 tests in columns 2, 4, 6, and 8 are each with reference to the baseline specification in columns 1, 3, 5, and 7 (respectively) with only basic controls and state fixed effects. R_{max} is set to 1.3R following Oster's suggestion (with upper limit equals to one). Standard errors in parentheses are clustered at the state-level.

Table 1.3: Impact of World War II Casualties on Fertility

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.093	0.215	0.006	0.178	-0.605	-0.509
	(0.016)	(0.041)	(0.027)	(0.068)	(0.036)	(0.088)
Casualty Rate ×	-0.097	-0.166	-0.050	-0.194	-0.013	-0.103
After War	(0.025)	(0.040)	(0.047)	(0.067)	(0.054)	(0.086)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	3,069	3,069	3,066	3,066	3,062	3,062
Observations	6,138	6,138	6,132	6,132	6,124	6,124
R-Squared	0.194	0.197	0.246	0.249	0.339	0.339

Notes: The dependent variable is the natural log of total live births. In the binary regressions (columns 1, 3 and 5), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regressions (columns 2, 4 and 6), "Casualty Rate" is a continuous variable expressed as the number of casualties per 100 men. "After War" is a dummy that takes the value of one in 1950, 1960 or 1970. County fixed effects and the natural log of female population are included in all regressions. Alaska, the District of Columbia and Hawaii are not included in the analysis. The number of casualties is not available for most Virginia cities, except for Alexandria city which is included in Arlington county. Bronx, Queens, New York, Richmond, and Kings counties are all included in New York City. The decrease in the number of observations is due to missing data on total live births for some counties in 1950, 1960 and 1970. Moreover, 1970 data on female population is missing for Carson City and Ormbsy in Nevada. Standard errors in parentheses are clustered at the county-level.

Table 1.4: Impact of World War II Casualties on Fertility: State-Decade FE

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.259	0.331	0.323	0.449	-0.217	-0.159
	(0.087)	(0.092)	(0.088)	(0.104)	(0.159)	(0.180)
Casualty Rate × After War	-0.059	-0.108	-0.021	-0.144	-0.023	-0.071
	(0.027)	(0.040)	(0.043)	(0.065)	(0.055)	(0.089)
Female Population	✓	✓	✓	✓	✓	✓
State-Decade FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	3,069	3,069	3,066	3,066	3,062	3,062
Observations	6,138	6,138	6,132	6,132	6,124	6,124
R-Squared	0.237	0.238	0.309	0.311	0.419	0.419

Notes: The dependent variable is the natural log of total live births. In the binary regressions (columns 1, 3 and 5), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regressions (columns 2, 4 and 6), "Casualty Rate" is a continuous variable. "After War" is a dummy that takes the value of one in 1950, 1960 or 1970. County fixed effects and the natural log of female population are included in all regressions. Additionally, all specifications include state times decade fixed effects. Alaska, the District of Columbia and Hawaii are not included in the analysis. The number of casualties is not available for most Virginia cities, except for Alexandria city which is included in Arlington county. Bronx, Queens, New York, Richmond, and Kings counties are all included in New York City. The decrease in the number of observations is due to missing data on total live births for some counties in 1950, 1960 and 1970. Moreover, 1970 data on female population is missing for Carson City and Ormbsy in Nevada. Standard errors in parentheses are clustered at the county-level.

TABLE 1.5: Impact of World War II Casualties on Women's Economic Outcomes

		1940–1950 Binary			1940–1950 Continuous	
Dependent Variables	(1)	(2)	(3)	(4)	(5)	(6)
Employed	0.011	0.011	0.015	0.050	0.047	0.072
	(0.004)	(0.004)	(0.005)	(0.012)	(0.012)	(0.014)
N		742,824			742,824	
Weeks Worked	1.429	1.410	1.619	6.154	6.017	7.337
	(0.382)	(0.387)	(0.438)	(0.724)	(0.737)	(0.844)
N		742,824			742,824	
Log Individual	-0.017	-0.014	-0.013	-0.214	-0.143	-0.143
Weekly Wages	(0.017)	(0.015)	(0.015)	(0.068)	(0.055)	(0.053)
N		292,306			292,306	
Log Household	0.038	0.032	0.044	0.104	0.113	0.157
Income	(0.016)	(0.014)	(0.013)	(0.046)	(0.042)	(0.039)
N		286,625			286,625	
Employed	0.008	0.008	0.007	0.046	0.041	0.039
with Child	(0.004)	(0.004)	(0.004)	(0.015)	(0.015)	(0.015)
N		407,998			407,998	
Married	0.003	0.002	0.003	0.029	0.028	0.034
	(0.009)	(0.008)	(0.008)	(0.026)	(0.025)	(0.025)
N		236,265			236,265	
Age at First Birth	0.016	0.010	0.003	-0.063	-0.022	-0.066
	(0.074)	(0.080)	(0.069)	(0.209)	(0.224)	(0.192)
N		609,278			609,278	
Age Gap in Marriage	0.011	0.015	0.023	0.303	0.267	0.284
	(0.072)	(0.071)	(0.074)	(0.291)	(0.285)	(0.292)
N		127,781			127,781	
SEA FE	✓	✓	✓	✓	✓	✓
Exogenous Controls		✓	✓		✓	✓
Endogenous Controls			✓			✓

Notes: Each estimate is from a separate regression of pooled micro data from the 1940 and 1950 censuses. Each outcome variable is regressed on the World War II casualty rate interacted with a 1950 year indicator variable. All samples exclude individuals living in institutional group quarters. The sample for the dependent variables "Employed" and "Weeks Worked" is restricted to women in their childbearing age (between 15 and 44). For the "Employed with Child" variable, we further restrict the sample to only include women with at least one child. The sample for "Individual Income" is restricted to both males and females working for at least 35 hours per week between the age of 15 and 65. For the "Household Income" outcome, we consider individual income for single individuals and the average partners' income in each household for couples. We exclude farmers, self-employed and unpaid family workers for individual and household income. Top-coded values are imputed as 1.5 times the censored value for individual and household income. The variable "Married" equals one for women of childbearing age (between 15 and 44 years old)who got married in the last 10 years and zero for women who never got married. "Age of First Birth" is computed by subtracting the age of the eldest child from the mother's age. We restrict the sample to the "Age of First Birth" values between 15 and 45 to account for measurement errors. The variable "Age Gap in Marriage" is computed for married women as the difference between their partner's age and their own age. In the binary regressions (columns 1–3), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regressions (columns 4–6), "Casualty Rate" is a continuous variable. Exogenous controls include the age (except for the "Age at First Birth"), race and ethnicity. Endogenous controls include the education level, marital status (except for the "Married" and "Household Income" outcomes), and experience for all income outcomes. Incomes are deflated by the 1990 CPI. SEA fixed effects are included for all regressions. Census person weights are used in all calculations. Standard errors in parenthesis are clustered at the SEA-level.

TABLE 1.6: Impact of World War II Casualties on Women's Economic Outcomes by Age Categories: Continuous Regressions

	Females 15 to 24	Females 25 to 34	Females 35 to 44	Females 45 to 54	Females 55 to 65
Dependent Variables	(1)	(2)	(3)	(4)	(5)
Children Under Age of 5	-0.055	-0.081	-0.052	-0.001	
	(0.030)	(0.026)	(0.021)	(0.008)	
N	266,102	254,399	222,323	162,182	
Having a Child	-0.008	-0.026	-0.014		
	(0.016)	(0.016)	(0.017)		
N	266,102	254,399	222,323		
Employed	0.078	0.037	0.037	0.064	0.024
	(0.018)	(0.022)	(0.017)	(0.026)	(0.021)
N	266,102	254,399	222,323	162,182	118,371
Weeks Worked	5.298	6.330	6.410	6.676	2.735
	(0.987)	(1.105)	(0.921)	(1.160)	(1.134)
N	266,102	254,399	222,323	162,182	118,371
Married	0.033	-0.011	-0.049	-0.107	0.034
	(0.028)	(0.027)	(0.088)	(0.109)	(0.083)
N	209,449	92,140	29,189	14,983	10,214
Age at First Birth	0.319	0.117	-0.042		
	(0.115)	(0.184)	(0.208)		
N	56,326	174,204	167,977		
Age Gap in Marriage	0.011	-0.283	-0.041	0.226	-0.055
	(0.479)	(0.427)	(0.613)	(0.642)	(0.667)
N	16,412	37,332	30,674	22,188	15,038
SEA FE	✓	✓	✓	✓	✓
Exogenous Controls	✓	✓	✓	✓	✓

Notes: Each outcome variable is regressed on the World War II casualty rate interacted with a 1950 year indicator variable. The casualty rate variable is considered as continuous in all the regressions (continuous regressions). The outcome "Children Under Age of 5" indicates the number of own children age 4 and under residing with each individual. The binary variable "Having a Child" takes the value of one for women having at least one child (based on the number of children ever born to each woman). The sample for "Children Under Age of 5", "Having a Child", "Employed", and "Weeks Worked" includes the original sample of women based on their age category. For the "Married" outcome, we keep the sample of women who are married (treated) or single (comparison group) and exclude those who were divorced or widowed. For the "Age at First Birth", we adjust for measurement errors and remove observations for which the calculated age at first birth is lower than 15 or larger than 45. We restrict the sample to married women only for the regression on the "Age Gap in Marriage". See notes of Table 1.5 for additional details. Exogenous controls include race and ethnicity. SEA fixed effects are included for all regressions. Census person weights are used in all calculations. Standard errors in parenthesis are clustered at the SEA-level.

TABLE 1.7: Impact of World War II Casualties on Children's Quality

	Dependent Variable: % of Live Births Attended Physicians at Hospital				Dependent Variable: Log of Infant Mortality	
	1940–1950		1940–1959		1940–1960	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	39.66	35.60	52.65	49.75	-0.372	-0.186
	(0.456)	(1.653)	(0.570)	(1.330)	(0.022)	(0.066)
Casualty Rate × After War	3.010	5.417	0.761	3.238	-0.118	-0.244
	(0.667)	(1.630)	(0.813)	(1.263)	(0.035)	(0.068)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	3,069	3,069	3,066	3,066	1,564	1,564
Observations	6,138	6,138	6,132	6,132	3,128	3,128
R-Squared	0.841	0.842	0.862	0.862	0.535	0.540

Notes: The dependent variable in columns 1 to 4 is computed as the number of live births attended by a physician at the hospital divided by the total number of live births by place of residence multiplied by 100. In columns 5 and 6, the dependent variable is the natural log of infant mortality (under one) by place of occurrence. In the binary regressions (columns 1, 3 and 5), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regressions (columns 2, 4 and 6), "Casualty Rate" is a continuous variable expressed as the number of casualties per 100 men. "After War" is a dummy that takes the value of one in 1950, 1959 or 1960. County fixed effects and the natural log of female population are included in all regressions. Standard errors in parenthesis are clustered at the county-level. See notes of Table 1.3 for sample details.

Chapter 2

Labor Legislation and Female Economic Empowerment: Evidence from Night Work, Regulatory and Seating Laws

2.1 Introduction

Female labor force participation patterns differ significantly across countries. Economic development (Boserup, 1970; Goldin, 1995) and cultural differences in gender norms arising from historical agricultural practices (Alesina, Giuliano, and Nunn, 2013; Hansen, Jensen, and Skovsgaard, 2015) or family cultures (Alesina and Guiliano, 2010) or ties to religious roots (Nunn, 2014) have been shown to be crucial factors contributing to the understanding of these patterns. In addition, gender focused policies including maternity leave, child benefits and tax credits favoring women as well as policies granting them legal rights or financial incentives to invest in female education have been presented as key determinants of women's participation on the labor market.[1]

The purpose of this paper is to assess whether public policies, specifically labor legislations for women, have a direct effect on female economic empowerment. We focus on the context of the United States and restrict our analysis to the period 1860 to 1940. This setting is attractive for at least two reasons.

First, it allows us to study a large number of legislative acts and gender based labor laws enacted during that period. More precisely, we examine three types of laws: regulatory, night work and seating laws. Regulatory laws include laws that aim at enhancing the well-being of employed women by improving their labor conditions and banning their employment in certain types of occupations that may include unfavourable work conditions. For instance, working in mines, especially coal mines, involves detrimental settings potentially affecting maternal capacity or women's health. It was thus deemed illegal for women to occupy these types of occupations. Other laws organized work conditions in specific occupations by banning women from carrying heavy burdens (see Section 2.2 for details). Night work laws are labor regulations enacted by state legislatures that prohibit work at night for women in certain industries or occupations or

[1]See Jayachandran, 2015 and Heath and Jayachandran, 2017 for an overview of this literature for developing countries.

restrict the number of hours that women are allowed to work at night. Lastly, seating laws made it compulsory that "suitable" seating accommodations were provided to female employees.

Second, focusing on the 1860–1940 U.S. context is interesting because the long time period studied allows us to examine the short and medium run effects of these labor laws targeting women. This is especially important given that gender norms are hard to change and it might take years or decades for the enactment and implementation of labor legislation to impact women's economic empowerment.

Our aim is to examine whether the introduction of labor laws targeting women had an effect on their likelihood to be in the labor force, to occupy jobs in targeted industries and to be employed in higher wage occupations within affected industries. We also examine whether gender focused labor laws have indirect policy effects on women's marital, divorce and fertility status.

To examine causal impacts of these labor laws, we rely on a differences in differences research design that allows us to compare changes in female labor force participation before and after labor laws were introduced in treated states (i.e. those that passed labor legislation for women) in comparison to control states (i.e. those that did not pass any of the labor laws targeting women). This strategy exploits geographical and temporal variation in the adoption of labor laws favoring women. This is because not all states adopted regulatory, night work or seating laws. Moreover, the states that did enact any of these labor laws, did not adopt them at the same time. The enactment of female focused labor laws happened rather gradually across states between 1872 and 1931.

A differences in differences estimation strategy requires assumptions that may not hold for a number of reasons. First, states that passed labor legislation for women might be systematically different than states that did not. Second, female labor force participation might have upward general trends in both types of states. Finally, other state specific shocks might have occurred and that could explain changes in female labor force participation.

We estimate leads and lags of the treatment over five decades which leverages variation in the timing of gender based labor laws' introduction across different states. We investigate regulatory, seating and night work laws simultaneously, and classify states in each decade into treated or control states depending on whether or not the state passed one of the labor laws targeting women in that specific decade. Parallel pre-trends provide suggestive evidence in support of parallel trends in the absence of the treatment, i.e. identification assumption.

When we examine women and men separately, we provide weak evidence in support of higher female labor force participation in the states that implemented these policies in comparison to the states that did not. The estimates for men are close to zero indicating that labor legislation targeting women does not impact men's labor force participation. This is suggestive that men can be used as an alternative counterfactual group in examining the impact of gender focused labor laws. Our findings from carrying out this analysis indicate that night work laws increased women's labor force participation by around 2 percentage points in comparison to men in treated in comparison to control states. Our results are robust to the inclusion of potential state specific confounding factors including laws granting women voting, property and earning rights.

We also provide a heterogeneity analysis by exploiting variation in electricity access and urbanization level. We document a positive and statistically significant impact of seating laws on female labor force participation, in comparison to their male counterparts, with higher electricity access and urbanization level.

Next, we consider industry specific labor laws' effects. Our results show that women are more likely to be employed in the manufacturing industry and less likely to occupy personal services related jobs when laws prohibiting night work shifts for women are introduced. We also document a higher likelihood of employment for women in manufacturing related occupations as regulatory laws enhancing work conditions for women are enacted. The decrease in women's employment in the personal services industry is likely due to the fact that this industry requires night work shifts which are prohibited to women.

In addition to examining industry specific employment effects, we investigate whether women are more likely to be employed in higher wage occupations within affected industries. We find evidence in support of this for the personal services industry. When we examine the impact of labor laws favoring women on women's marriage and fertility status, our results imply that there were no such effects suggesting that direct policies might be more efficient.

Our paper offers an important contribution to the debate on public policies targeting female economic empowerment by documenting positive effects induced by the introduction of labor laws on female participation in the labor market through an amelioration of their working conditions. Our findings appear to provide evidence in support of emerging policies in developing countries where, for instance, India just implemented a seating law in 2018. This policy is believed to grant women greater freedom in taking factory jobs. Moreover, our research is also relevant to the controversial issues in the garment industry in Bangladesh and other developing countries. According to the World Bank, approximately 80 percent of garment workers in 2017 were women, most of them working in detrimental conditions and having serious difficulties finding alternative employment. Furthermore, numerous developing countries have gender-unequal laws and policies restricting gender equal legal rights, including property or inheritance rights, and that hinder female employment. Additional restrictions to female employment include husbands' right to prevent their wives from working or women not being allowed to head households. Gonzales et al., 2015 documents strong evidence in support of a cross-country correlational relationship between removing such discriminatory laws and increases in female employment. Causal evidence on the effect of gender based laws is still, however, weak.

This paper contributes to the literature in three ways. First, to our knowledge, we are the first paper to formally investigate the causal impacts of gender based labor legislation on female labor force participation. Second, we provide suggestive evidence that such labor laws regulating formal labor market conditions in favor of women fail to have significant indirect effects on marriage or fertility choices for women. Finally, our results have policy implications endorsing the adoption of labor laws in favor of women to increase female empowerment through a higher involvement in the labor market and financial independence.

The rest of the paper is structured as follows. In Section 2.2, we provide a detailed overview of labor legislations for women in the United States. In Section 2.3, we describe our data sources. Section 2.4 outlines our empirical strategy. In Section 2.5, we discuss our results and present a battery of robustness checks. We briefly conclude in Section 2.6.

2.2 Development of Labor Legislation for Women

"When the spinning, the weaving, the making of clothes and shoes, and all the other industrial pursuits once followed in the home were taken away and developed into factory industries, women were called upon still to conduct many of the operations, and thereby became part of the army of those gainfully employed" (Women's Bureau U.S. Department of Labor, Bulletin No. 3).

Prior to World War I, the number of women employed in industries gradually increased, but the need to recruit women then peaked as men vacated jobs to fight in the war. The urgent need for workers allowed women to fulfill jobs traditionally held by men. There was also a necessity for laws regulating the efficiency and the health of working women. *"The physical well-being of woman becomes an object of public interest and care in order to preserve the strength and vigor of the race"* (Cushman, 1998). Additionally, because women had been in a weaker economic position with respect to men, there was a great need for control of the standards of women's employment. During war, both federal and state agencies were responsible for building up standards for working women. After WWI, U.S. states' responsibility increased and the need for protecting the health of female workers was recognized locally as a vital economic and social measure.

Standards for women's employment in industries, drawn up with the advice of both employers and workers, were published by the "Women in Industry Group" created by the Department of Labor which then became the United States Department of Labor Women's Bureau. These standards were eventually incorporated into labor laws at the state and federal levels. *"It was the first time the federal government had taken a practical stand on conditions of employment for women, and although the standards were only recommendations and had no legal force, they were a very important statement of policy and were widely used in all parts of the country"* (Mary Anderson, the first Director of the Women's Bureau).

Recommendations included posture at work: *"Continuous standing and continuous sitting are both injurious. A chair should be provided for every woman and its use encouraged."* Other recommendations covered comfort and sanitation: *"State labor laws and industrial codes should be consulted with reference to provisions for comfort and sanitation"* (Women's Bureau U.S. Department of Labor, Bulletin No. 173).

Governments of different states passed legislative acts regulating women's labor on the basis of these standards.[2] Our data on the chronological development of state level labor legislation for women is based on a digitalized archived bulletin published by the United States Department of Labor Women's Bureau. The establishment of this bureau in the Department of Labor was enacted during a congress assembly of the Senate and House of Representatives of the United States. The role of the Women's Bureau as dictated in the bulletin is to *"formulate standards and policies which shall promote the welfare of the wage-earning women, improve their working conditions, increase their efficiency, and advance their opportunities for profitable employment"* (Public Law 66-259, 66th Congress, H.R. 13229). Moreover, this bureau had investigative authority over any issue concerning women's welfare. The bureau subsequently sent its report to the Department of Labor, which had the authority to make the results of investigations available to the public.

For our empirical analysis, we rely on Bulletin number 66 Part II revised in December 1931 and published in 1932 that was prepared and published by the United States Department of Labor Women's Bureau. It presents the history of the evolution of all labor laws for women enacted from 1872 to 1931. One part of this bulletin (Part I) provides a detailed overview of women's labor legislation in only three states (California, Massachusetts and New York). The second part (Part II) provides a compressed overview of labor laws for women across all states.

We are the first to rely on this data to formally examine the effect of gender focused labor legislation favoring women. We are interested in three types of laws that affect women's labor (and not men's). First, laws prohibiting certain kinds of employment for women or regulating the conditions under which they may be performed. Legislative acts passed by legislatures of some states exclude women from employment in specific occupations. These acts mainly prohibit women from working in mines, specifically coal mines. Other bans forbid women from working as baggage handlers for instance, or working in the manufacturing or handling of nitro and amido compounds or any other dry substance (i.e. highly solid based substances). More broadly, labor laws banned women from working in occupations which involved harmful and detrimental work conditions or affected women's potential capacity for motherhood or health. Other laws banned employment for women immediately before and after childbirth. Additional regulatory laws organized and controlled the conditions under which specific occupations could be performed rather than banning them for women. For instance, women were not permitted to carry heavy burdens, clean out moving machinery, etc.

In summary, regulatory legislation included laws with the stated aim to improve the well-being of women at work and ban their employment in certain

[2] The first law regulating the hours of work for both men and women was enacted in 1847 in New Hampshire. While hours of work and minimum wage laws were controversial issues in the U.S., regulations related to work conditions for women (except minimum wage laws) were enacted and implemented rather easily. Woloch, 2015 offers a good historical narrative for the roots of protective laws for working women such as maximum hours laws, minimum wage laws, and night work laws, including popular reformers' rationales and the legislative and judicial progression of protective policies in the United States.

types of occupations that could include detrimental work conditions for them. The majority of these laws were enacted in a narrow range of only a few states. The distribution of regulatory laws by state is as follows: about 21 states did not enact any regulatory legislation as of 1931 whereas 13 states passed a single regulation.[3] A total of six states passed two regulatory regulations, two states passed three and two states enacted four regulations. Finally, the highest number of regulations enacted were passed by three states where one state passed six regulations, one passed 13 and one passed 23 regulations respectively. Only two states had an exhaustive list of restrictions prohibiting women from working in many occupations that were deemed legal and women were free to occupy in all other states.

Second, we are interested in seating laws. These laws made it mandatory that "suitable" seats should be provided to female employees. The extent to which these seats could be used varied across states. Some states specified the purpose as to allow working women to *"sit when not actively engaged in their duties or when sitting does not interfere with the proper discharge of duties"* (Bulletin number 66 Part II, p. 173). Other states dictated that such seats could be used as it is necessary to preserve the health of workers. Almost all states except Mississippi introduced seating laws, however, the occupations or industries in which these laws were applicable varied significantly. For instance, seating laws were applicable to mercantile occupations exclusively in Alabama, Maryland, North Dakota and South Carolina. Moreover, the number of seats required varied across states and/or occupations. In this paper, we only focus on whether or not states passed seating laws, and the type of industries in which these laws were applicable.

Lastly, we focus on night work laws. These laws prohibit night work for women in certain industries or occupations or limit the number of hours that women are allowed to work at night. In total, 16 states banned female work at night in specific industries or occupations. Three of those states prohibited night work shifts for women in manufacturing industries and one state in mercantile; two states ban their night work in one occupation each. The remaining ten states specified at least two targeted industries or occupations for night work laws. Few states allowed women to work at night but instead limited shifts to eight or ten hours. Some states combined these two types of laws by imposing a restrictive number of hours for female night work in certain occupations while banning work at night for women in other occupations. Most states specified 10 p.m. to 6 a.m. as the period during which night work is banned.

Appendix Figures A1, A2 and A3 describe the chronological timing of passage of regulatory, night work and seating laws across states. States enacted regulatory laws between 1880 and 1940, while night work laws and seating laws were enacted between 1900 and 1940. Appendix Figure A1 documents that only one state had enacted regulatory laws in 1880, and by 1940, about 21 states had

[3] A total of seven out of these 13 states banned employment of women in coal mines, one state prohibited their employment in messenger services, two states regulated unemployment periods for women before and after childbirth, one state forbid heavy lifting for women, one state banned the cleaning of moving machinery and one state regulated their work on moving abrasives.

not passed any, while the remaining states had passed at least one regulatory law. Appendix Figure A2 describes the timeline of passage of night work laws showing that by 1940 few states had enacted such laws. Appendix Figure A3 shows that by 1940, all but two states had passed seating laws.

We disregard other type of labor laws enacted, such as hours laws that provided daily/weekly limits for female hours worked, because these laws varied significantly across different states, making it hard to measure and identify relevant variation. This is especially true given that almost all states passed such laws but regulated the hours of work for women differently.[4] We also do not examine the equal-pay law that made it unlawful to discriminate in any way in the payment of wages between sexes (Megdal and Ransom, 1985). This is because during our period of study, only two states (Michigan and Montana) passed such a law (in 1919).

2.3 Data

In this section, we describe our data sources for state level labor legislation, female labor force participation, and other outcomes of interest. We also provide some detailed descriptive statistics.

2.3.1 Data on State Level Labor Legislation for Women

In order to obtain data on state level labor legislation for women in the United States, we rely on a digitalized archived bulletin published by the United States Department of Labor Women's Bureau. Bulletin number 66 part II published in 1932 presents an overview of the evolution of all labor laws for women enacted from 1872 to 1931.

When we focus on regulatory laws, we consider a state that passed a regulatory law of any kind targeting any industry as a treated state in the corresponding decade. By 1940, 28 states had passed regulatory laws while 21 had not (Appendix Figure A1). Regarding night work laws, 20 states enacted such laws by 1940 and 29 states did not (Appendix Figure A2). As discussed above, while night work laws in some states banned women from working at night in specific industries, other states dictated the number of hours during which women could work at night. In our paper, we do not differentiate between different types of night work laws. We label any state passing night work laws of any kind as a treated state in our sample. Regulatory laws were enacted across U.S. states between 1872 and 1919 and night work laws were implemented between 1889 and 1921.

Finally, regarding seating laws, i.e. laws requiring provision of seats for working women, our treated group in 1940 (the last year in our sample) comprises 47 states and our control group includes two states. For the purpose of this paper, we do not focus on the exact wording of these seating laws, which

[4]Some states regulated hours worked for women to eight hours, other states enacted 8.5 hour laws, etc. Few states passed weekly hours laws. In many states, the industries or occupations targeted by these hours laws were very limited.

diverges across different states, nor the specific number of seats required for example. In our sample, a state is considered as treated if a seating law of any kind is enacted. The first seating law was implemented in 1881 and the last one in 1931 (see Appendix Figure A1).

2.3.2 IPUMS Data - Individual Level

We rely on 1% weighted samples from the United States Census data (1860–1940) available from the Integrated Public Use Microdata Series (IPUMS).[5] IPUMS provides access to U.S. census microdata and includes a wide range of variables related to education, work, income and demographic characteristics. We carry out our analysis at the individual level and we collect information about individual's age, race and ethnicity. We gather data on labor force status using a dichotomous variable that indicates whether the person is in the labor force or not.

We also rely on information about the occupational income score for women from IPUMS. This is a constructed two digit numeric variable that allocates a value representing the median total income (in hundreds of 1950 dollars) to occupations in all years. This variable allows for the classification of higher wage occupations. For our analysis, we restrict the sample to women aged 16–65 years old. IPUMS also contains information about the marital and fertility status of women.

Table 2.1 shows summary statistics by decade for our sample of women. We report statistics for the following decades: 1860, 1880, 1900, 1920 and 1940, in columns (1) to (5) of Table 2.1 respectively. The share of women in the labor force increased from about 14% in 1860 to almost 29% in 1940. The average age in our sample of females increases from 33 to 37 between 1860 and 1940. The majority of women in our sample are married and the average number of own children decreased from about two to one child between 1860 and 1940.[6] In column (5), we compute descriptive statistics across all decades. The average share of women that are in the labor force is about 24%.

2.4 Identification Strategy

In this section, we describe in detail our identification strategy, identification assumption, main specifications and controls. The chronological variation in implementation of state-level policies, in addition to the large span of data we have on female labor force participation covering both pre and post policy introduction periods, provide us with an ideal framework to carry a differences in differences analysis with many treatment and control groups.

[5]Note that Census data is missing for 1890 from all sources. We are thus excluding this decennial year from our analysis. Also, we rely on a person weight variable available from IPUMS which gives the population represented by each individual in the sample.

[6]Marital status variable is not included in the U.S. Census before 1880.

2.4.1 Outline of Identification Strategy

Identifying the causal effect of labor laws targeting women on their labor force participation is challenging for a number of reasons. First, the states that passed labor legislation for women might be systematically different than states that did not. Moreover, female labor force participation might have upward general trends in both type of states irrespective of such policies. Finally, other state specific shocks (policies) might have occurred (been introduced) that can explain the increase in women's labor force participation.

We thus rely on an identification strategy that exploits across states variation in the type of labor laws targeting women that were enacted. This is because not all states prohibited night work for women for instance. Moreover, while some states had no laws regulating occupation specific employment for women, other states had regulations prohibiting women from working in specific occupations. Finally, not all states enacted laws requiring seats provision to working women. We furthermore exploit the chronological nature of implementation. This is because not all states enacted labor laws at the same time, rather, implementation happened gradually between 1872 and 1931. This gradual state-level policy implementation, along with the fact that not all states enacted women labor laws, allows us to carry a differences in differences analysis with many treatment and control groups varying by decade.

We examine the impact of the introduction of seating, regulatory and night work laws by categorizing states in each decade into treated or control states depending on whether or not the state had any of the labor laws targeting women. We thus generate binary indicator variables that classify states into treated and control states. The indicators take the value 1 for treated states, i.e. states that had a given type of labor law passed in a given decade, as of that specific decade. The indicators take the value 0 for control states i.e. states that never had a labor law targeting women or treated states in decades before the passage of these laws. This research design allows us to identify the causal effect of introducing labor laws targeting women by comparing changes in female labor force participation before and after the introduction of labor legislations in treated states in comparison to control states.

2.4.2 Model Specification

Differences in Differences Estimation

The objective is to investigate the impact of the passage of gender based labor laws. Our identification strategy relies on a direct comparison between states that passed one of the labor laws targeting women and those that did not. We carry out our analysis at the individual level which allows us to control for individuals' characteristics. We also account for decade specific and time-invariant state specific characteristics. We estimate the following differences in differences specification:

$$y_{isd} = \alpha_s + \beta_d + \gamma SeatingLaw_{sd} + \delta RegulatoryLaw_{sd} + \omega NightWorkLaw_{sd} + \lambda X'_{isd} + \theta_{isd} \quad (2.1)$$

where y_{isd} is the labor force status of woman i residing in state s in decennial year d. It is a binary variable which takes the value one if woman i residing in state s in decennial year d is in the labor force and zero otherwise. $Seating\ Law_{sd}$, $Regulatory\ Law_{sd}$ and $Night\ Work\ Law_{sd}$ are indicators that equal one for states that had any seating, regulatory or night work laws respectively in decennial year d and zero otherwise. These are binary indicator variables that classify states into treated and control states. Our coefficients of interest are γ, δ and ω which capture our treatment effects. α_s and β_d are state and decade fixed effects, X'_{isd} is a list of individual level controls including age, ethnicity and race and θ_{isd} is our error term. Our standard errors are clustered at the state level. We also report wild cluster bootstrap p-values given the relatively small number of unbalanced clusters contributing to the identifying variation (MacKinnon and Webb, 2017, Roodman et al., 2019).

Our identification assumption is the parallel trends in outcome variables between treated and control states in the absence of the treatment. In other words, trends in female labor force participation would have continued to be the same in treated and control states had these labor laws targeting women not been introduced. We thus compare states that implemented women's labor laws to control states, i.e. those that had not implemented such laws in a given decade and we argue that these form a valid counterfactual group.

In an alternative specification, we rely on men as our counterfactual group and conduct a triple differences analysis that allows us to compare women's labor force participation in comparison to men in treated relative to control states. More precisely, our sample in this case is pooled to include both men and women but our dependent variable of interest remains labor force status. We interact our independent variables of interest $Seating\ Law_{sd}$, $Regulatory\ Law_{sd}$ and $Night\ Work\ Law_{sd}$ with a dummy "woman" that takes the value 1 if the individual is a female and zero otherwise.

When we investigate industry specific labor laws' effects, a state is considered treated in a given decennial year only if it had one of the three types of labor laws for the industry of interest. For instance, when we focus on targeted regulatory laws effects on the manufacturing industry, only a state that passed a law regulating female employment in manufacturing occupations is considered a treated state. Thus, our analysis examines the impacts of gender based labor laws on female employment in targeted industries. The same analysis applies to night work laws or seating laws targeting only specific occupations, which we classify into the corresponding industry. The dependent variable of interest, female employment, is a dummy variable that takes the value of 1 if a woman is employed in the indicated industry, and 0 otherwise. This type of analysis allows us to investigate whether there exists a direct effect on female employment in specific industries from targeted policies rather than examining the impact on the overall labor force participation of women.

Leads and Lags of the Treatment

In an alternative specification, we estimate treatment impacts at different dates. We thus generate dummies for leads and lags of the binary indicator variables for the passage of different labor laws for two decades before and three decades after the passage of laws. For the analysis, we omit the first decade preceding the year of the policy implementation *(d-1)*.

We thus estimate the following specification:

$$y_{isd} = \lambda_s + \gamma_d + \Sigma_{\kappa=-2}^{3} \alpha_\kappa StateLaw_{s(d+\kappa)} + \beta OtherStateLaw_{sd} + \delta X'_{isd} + \varepsilon_{isd} \tag{2.2}$$

where y_{isd} is the labor force status of woman i residing in state s in decennial year d. It is a binary variable which takes the value one if woman i residing in state s in decennial year d is in the labor force and zero otherwise. $State\ Law_{sd}$ is set to equal one for states that had a given labor law in decennial year d and zero otherwise. The leads provide a falsification test of the main identification assumption, which is that trends in female labor force participation would have continued to be the same in treated and control states in the absence of the laws' passage by examining pre-treatment outcomes (i.e. for κ equals -2). The lags in our model allow us to examine whether the effect of the given labor law fades away or grows over time (for κ being equal to 1, 2, and 3, respectively). Because we do this for one type of law at a time, $Other\ State\ Law_{sd}$ indicators are two sets of dummies that control for the two other types of labor laws. These dummies take the value one if other laws are in place in state s in decennial year d and zero otherwise. Our coefficients of interest are α_κ which capture treatment impacts at different dates. λ_s and γ_d are state and decade fixed effects, X'_{isd} is a list of individual level controls including age, ethnicity and race and ε_{isd} is our error term. Our standard errors are clustered at the state level.

2.5 Main Results

2.5.1 Main Findings

Table 2.2 reports the effects of the three types of state level labor legislation for women estimated using Equation 2.1. We first examine seating laws in columns (1) and (2); then regulatory laws in columns (3) and (4) and lastly we report the analysis for night work laws in columns (5) and (6). All state laws are included in the same specification to account for the overlap that might occur between the three different policies. Thus, in column (1) we examine the impact of seating laws while controlling for regulatory and night work laws passage as well as state and decade fixed effects. Column (2) further controls for individual level exogenous characteristics including age, race and ethnicity. In column (3), we examine the impact of regulatory laws while controlling for seating and night work laws passage then add individual level controls in column (4). Finally, in column (5), we report our estimates for night work laws and we add individual level controls in column (6).

In Panel A, we analyze the impact of gender focused labor laws on labor force participation for both women and men separately. We find no evidence that female labor force participation in states that implemented these policies is higher than in those that did not. Our estimates on women's LFP are positive but not statistically significant. While investigating the effect on men's labor force participation, we find that our estimates are close to zero with a slightly negative impact of night work laws. These results indicate that labor legislation for women did not impact male labor force participation which suggests that men can be used as a potential counterfactual in studying the impact of these policies on women's LFP.

Therefore, we mimic Equation 2.1 but we pool men and women and add a "women" dummy - that takes the value of one if individual i is a woman - as an additional interaction term to each of our state law dummy indicator variables. The triple differences specification in Panel B allows us to assess the impact of state law policies on the change in labor force participation of women compared to men in treated compared to control states. Our results suggest that night work laws increased women's labor force participation by around 2 percentage points compared to men, respectively.

Given that the states of Michigan and Montana passed equal pay laws during our period of study (in 1919), we exclude them from our analysis as a robustness exercise. Our results, reported in Appendix Table A1, remain robust to the exclusion of these states and are of a similar magnitude as in Table 2.2.

Moreover, we carry a robustness analysis where we additionally control for state specific time trends. Appendix Table A2 displays the results we obtain from doing this specification. Our findings for the triple differences estimates are of similar magnitude to those in Table 2.2, confirming our previous conclusions.

Figures 2.1 and 2.2 show our analysis for the leads and lags of the treatment effect on women's and men's labor force participation respectively. These results are computed using Equation 2.2 where we estimate treatment impacts at different dates. Our leads estimates provide a falsification test of our main identification assumption. The fact that our estimates are close to zero and not statistically significant before policy enactment bolsters the credibility of our identification strategy.

The lags in our model allow us to examine how the effects of the different labor laws changed over time. This is especially important in our context since it may have taken several years for the policies to have an effect on gender roles and labor markets. Figure 2.1 shows that while the impact of seating laws remain statistically insignificant over the full period analyzed, the effect of regulatory laws became significantly positive on women's LFP one decade after the policy implementation *(d+1)*. The effect of night work laws were also not immediate; rather, we document a positive statistically significant estimate three decades after its implementation *(d+3)*. The increasing effect over time is probably due to the fact that these state laws were being reinforced over time. One way to reinforce these laws was by increasing the penalties for violations.

Figure 2.2 shows our leads and lags treatment effects on male labor force participation. Results in this figure confirm that female specific labor laws did not have an impact on men's labor outcomes.

2.5.2 Industry Specific Analysis

We now consider industry specific labor laws' effects. We use definitions for industries as they appear in the digitalized archived bulletin published by the United States Department of Labor Women's Bureau. These industry classifications appear in the same way in our data source, IPUMS. Our analysis includes four industries mentioned in the bulletin: mercantile, manufacturing, personal services and mining. To gain more insight on the effect in the manufacturing industry, we divide it into two subcategories: manufacturing of durable goods and manufacturing of non-durable goods.

We rely on a specification similar to Equation 2.1 but instead we change our definition of treated and control groups to become industry based. A state is considered treated in a given decennial year only if it had one of the three labor laws and the law targets the industry of interest. Our analysis thus allows us to investigate the impact of labor laws on women's employment by targeted industry, and therefore we are able to examine whether there exists a direct effect on women's employment in specific industries using targeted policies. Recall that, regulatory laws include laws that prohibit women from working in specific occupations (specifically related to the mining industry) and regulations that enhance work conditions by banning women from doing specific tasks such as heavy lifting which might have detrimental consequences for their health. Since all occupation bans are related to the mining industry, carrying out an industry specific analysis and examining labor laws' effects by targeted industries is one way to distinguish between regulatory laws and examine the impacts of occupation and within-occupation task bans separately.

We investigate the impact of each labor law and our results are reported in Table 2.3. While women's employment is not significantly higher in states that implemented seating laws in specific industries, we document that women are more likely to be employed in manufacturing industry (more precisely in durable goods manufacturing) when regulatory laws are implemented. The increase in employment likelihood for women in manufacturing is likely due to the fact that regulatory laws enhanced work conditions for women by forbidding heavy weight lifting for instance. The effect of night work laws vary based on the industries they were implemented in. We find that women are more likely to work in manufacturing (specifically in non-durable goods manufacturing) but less likely to occupy personal services related jobs when laws prohibiting night work shifts for women are introduced. We argue that this decrease in women's likelihood in occupying personal services jobs is due to the nature of this industry that might require night work shifts which are prohibited to women.

In Appendix Table A3, we repeat the same analysis to examine the effect of these policies on men's employment in different industries. All our estimates are not statistically significant. These findings provide suggestive evidence that gender focused policies targeting women's employment in specific industries are not crowding out men from specific jobs in targeted industries.

As an additional exercise, we analyse the effect of each type of labor legislation on women's occupational score within industries (i.e. the likelihood to be employed in higher wage occupations within affected industries). We thus focus on each industry separately, and restrict our sample to women occupying jobs in the industry of interest. Our aim is to check whether such policies improve women's position within industries, i.e. whether women are more likely to be occupying high paying jobs. While seating laws that made it mandatory to provide seating accommodations to women improve their work conditions, in Appendix Table A4, we show that this policy actually led to a 0.08 standard deviation (SD) decrease in women's occupational score in manufacturing and manufacturing of non-durable goods industries.

Night work laws however increased this score by about 0.02 SD in the personal services industry. We previously showed, in our industry specific analysis, that women's overall employment in personal services jobs decreased after night work laws' enactment. However, our results on women's occupational income score within the personal services industry show that women are now more likely to be employed in higher wage occupations.

2.5.3 Marriage and Fertility Outcomes

In Table 2.4, we present results from estimating Equation 2.1, to examine the impact of labor laws for women on marriage, divorce and fertility outcomes. We refer to these effects as indirect policy impacts, with the direct effect of these policies being on women's labor force participation. Similar to Table 2.2, we first examine seating laws in columns (1) and (2); then regulatory laws in columns (3) and (4) and lastly we report the analysis for night work laws in columns (5) and (6). All three types of labor laws are included in the same regression. Each row displays a different outcome. First, we analyze the impact of gender focused labor laws on the likelihood of marriage for women, then the likelihood of being divorced. To study fertility choices, we focus on the number of children and the age of having the first child for women (i.e. the age of being a first time mother). In columns (1)–(6), we include state and decade fixed effects. We add individual level controls including age, race and ethnicity in columns (2), (4) and (6).

When we examine indirect policy effects, we find that all state law policies have no effect on marriage, divorce or fertility choices. This is suggestive that more direct policies might be more successful in influencing marriage or fertility choices than policies affecting women's labor.

2.5.4 Robustness Checks and Heterogeneity Analysis

Women's Suffrage and Financial Liberation

In this section, we examine the robustness of our analysis to the inclusion of other potential state specific confounding factors. We first consider the passage of suffrage laws enfranchising women at different points in time across difference states. Some states passed suffrage rights for women prior to the 1920 passage of the Nineteenth Amendment, a federal mandate for women's voting rights which made it mandatory for all states. We thus exploit variation in the timing of women's suffrage laws across states and examine whether female labor laws' effects remain robust to controlling for this shock. We rely on data on suffrage laws' passage from Lott and Kenny, 1999 and Miller, 2008.

We also examine the timing of granting property rights and earnings rights to women in the United States. Data on the timing of women's financial liberation by state is obtained from Geddes and Dean, 2002. This can be perceived as a positive shock increasing women's incentive to work.

Table 2.5 shows the effect of state labor laws on our main outcome variables after controlling for women's suffrage and financial liberation. Panel A shows the effect of seating laws on each of our outcomes of interest: women's labor force participation, men's labor force participation, and labor force participation in a triple differences framework. In Panel B, we investigate regulatory laws and Panel C reports the analysis for night work laws. Column (1) of Table 2.5 repeats our findings from column (2) of Table 2.2 with individual level controls and state and decade fixed effects. In column (2) of Table 2.5, we control for women's financial liberation. We then introduce women's suffrage in column (3) and lastly in column (4) of Table 2.5, we simultaneously control for women's suffrage and financial liberation. Comparing our estimates in column (4) to column (1) suggests that our results are robust to the inclusion of these state level shocks.

Electrification and Urbanization

In this section, we account for electricity access in the early 1900s as an important potential confounding factor that could have affected female labor force participation during that era. Electrification can have an impact on female employment and employment characteristics through many channels. It has been argued that electricity access reduces the time burden of housework stemming from the use of electrical appliances and thus results in an increase in female labor force participation (see Gordon, 2016, Greenwood, Seshadri, and Vandenbroucke, 2005 and Ramey, 2009). This channel is particularly important in our context.

We rely on electrification data from Vidart, 2020.[7] To examine whether the effect of labor policies implemented by state legislatures differs across states based on their access to electricity during that era, we modify Equation 2.1 to carry a heterogeneity analysis. Precisely, we add an additional term to our $Seating\ Law_{sd}$, $Regulatory\ Law_{sd}$ and $Night\ Work\ Law_{sd}$ variables, that is the natural log of total capacity in 1919 (total generation capacity in Kilowatt within state boundaries).[8]

Estimates in Panel A of Table 2.6 show the effect of the three types of policies interacted with the natural log of total electrical capacity on our main outcomes. Our differences in differences results suggest that electricity access had no impact on the change in female labor force participation in states that

[7] We thank Daniela Vidart for sharing this data with us. Vidart, 2020 relies on the "Central station directory: a complete list of electric light and power companies with data" (McGraw Publishing Company (1911, 1919)) to build a new dataset in 1911 and 1919 by digitalizing historical documents containing information on 5,409 and 5,631 generators, respectively.

[8] As Table 2.1 shows, almost all state policies were implemented by 1920.

implemented gender based labor laws compared to those that didn't. Additional effects on male labor force participation remain absent. When we carry out our heterogeneous triple differences analysis, depending on electricity access, we document an impact of seating laws on women's labor force participation. Our estimates in column (2) Panel A of Table 2.6 show that a one percentage point increase in electricity access leads to a positive and statistically significant additional effect of seating laws on female labor force participation compared to their males' counterpart, with a magnitude around 1 percentage point. Of note, the average of the natural logarithm of total electrical capacity is close to 12.

Additionally, we argue that urbanization can have an effect on women's labor force status for structural and social reasons. Besides the expansion of the industrial and service sectors in metropolitan areas, urbanization has been seen as one part of the social transformation that has affected the occupational prospects of women.[9] To further investigate the additional effect of urbanization, we interact the $\textit{Seating Law}_{sd}$, $\textit{Regulatory Law}_{sd}$ and $\textit{Night Work Law}_{sd}$ variables with the share of urban population at the state level in 1920.

Panel B of Table 2.6 shows the estimates for our interaction terms. Results suggest that a rise in the share of urban population increases the effect of seating laws on women's labor force participation. Moreover, the effect of seating laws policies on women's LFP compared to men's LFP is larger in urban areas. More precisely, we document that a one percentage point increase in the share of urban population leads to an additional effect of around 7 percentage points on women's LFP compared to men's LFP, in treated states compared to control states. The average share of urban population is around 45%.

2.6 Conclusion

This paper assesses whether public policies, specifically, labor legislations targeting women had direct effects on their likelihood to be in the labor force, to occupy jobs in targeted industries and to be employed in higher wage occupations within affected industries. We examine three types of laws targeting women's labor conditions: regulatory, night work and seating laws. These laws were manifested in regulations that banned women from performing certain tasks that might involve detrimental work conditions, laws that banned them from working at night and regulations that made it mandatory to provide seating accommodations for women.

Our differences in differences estimates provide weak evidence in support of higher women's involvement in the formal labor market after introduction of labor laws by state legislatures in treated states in comparison to control states. However, when we pool our sample to include both men and women and carry out a triple differences analysis, we find that night work laws increased women's labor force participation in comparison to men in treated relative to

[9] Women's employment was even used as an index of urbanism in Shevsky and Bell, 1955 in their social area analysis.

control states. Our results remain robust to the inclusion of other state level laws granting women voting, property and earning rights.

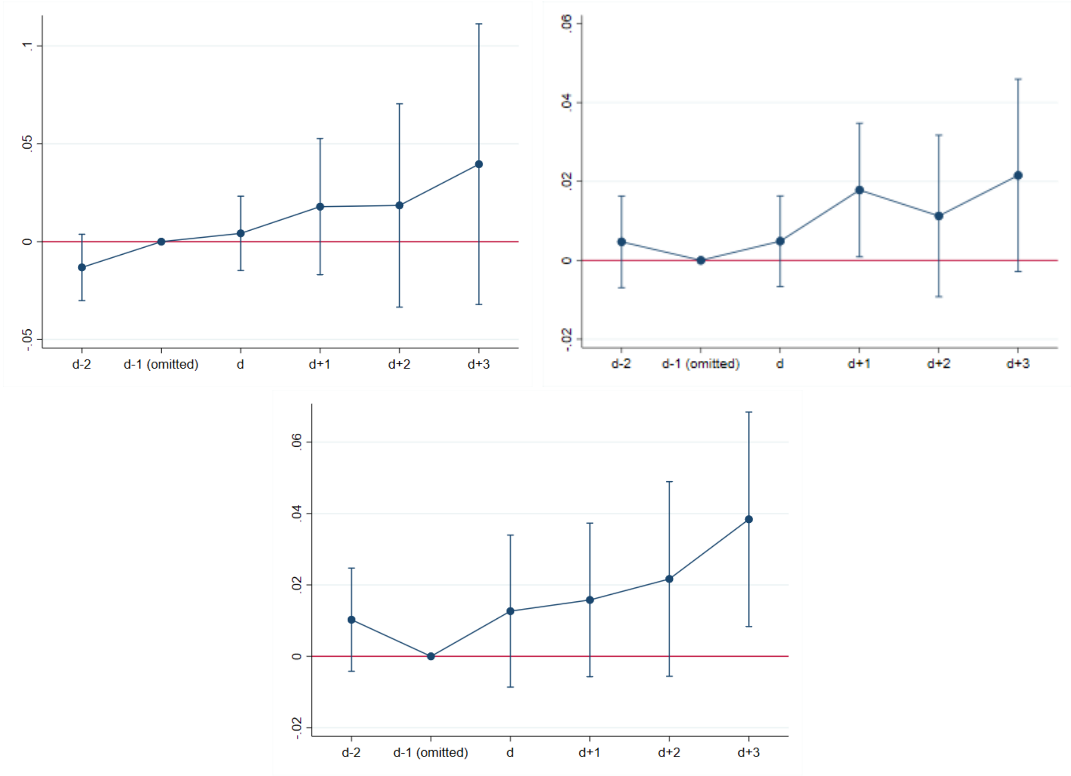
We document a positive impact on women's employment in the manufacturing industry, and a negative impact on employment in personal services related jobs, when laws prohibiting night work shifts for women in these specific industries are introduced. Our estimates also show a higher likelihood of employment for women in manufacturing related occupations as regulatory laws enhancing industry-specific work conditions for women are enacted. Moreover, we examine whether women are more likely to be employed in higher wage occupations within targeted industries and we find evidence in support of this for the personal services industry.

When we carry a heterogeneity analysis, we document a positive and statistically significant impact of seating laws on female labor force participation, in comparison to their male counterparts, with higher electricity access and urbanization level.

Our analysis of policy repercussions on marriage, divorce or fertility choices shows no significant impacts suggesting that more direct policies might be more successful for marriage or fertility choices than policies affecting women's labor.

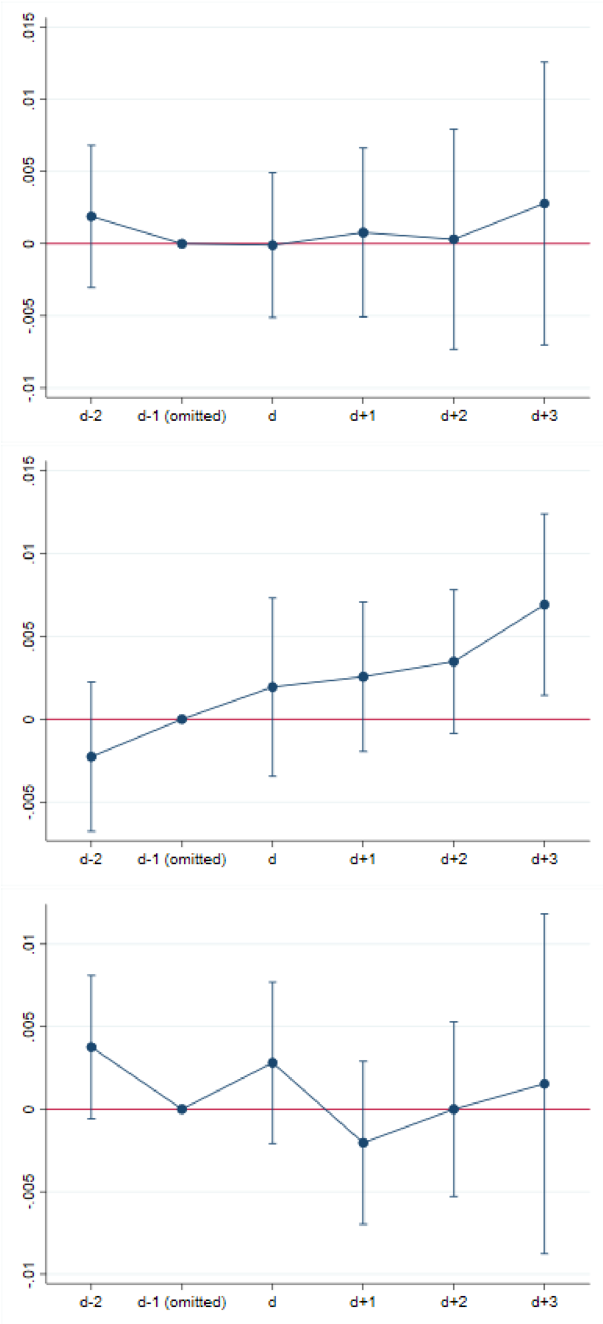
Our empirical findings make important contributions. First, to our knowledge, we are the first paper to formally document the causal impacts of gender focused labor laws favoring women on female labor force participation. Second, we provide suggestive evidence that such labor laws regulating formal labor market conditions in favor of women fail to have significant indirect effects on their marriage or fertility choices. Finally, our results have policy implications endorsing the adoption of labor laws in favor of women to increase women's economic empowerment through higher involvement in the labor market and financial independence, especially in countries where work conditions for women are still detrimental or their involvement is still low.

FIGURE 2.1: Leads and lags regression on Female LFP



Notes: This figure plots the estimated coefficients of the leads and lags regression on Female LFP for seating, regulatory, and night work laws, respectively. All three policies are included in the same specification. The first decade before policy implementation is omitted from the regression, i.e. the omitted category. *Source*: Individual level data are from Census IPUMS 1% samples for decades between 1860 and 1940.

Figure 2.2: Leads and lags regression on Male LFP



Notes: This figure plots the estimated coefficients of the leads and lags regression on Male LFP for seating, regulatory, and night work laws, respectively. All three policies are included in the same specification. The first decade before policy implementation is omitted from the regression, i.e. the omitted category. *Source*: Individual level data are from Census IPUMS 1% samples for decades between 1860 and 1940.

Table 2.1: Descriptive Statistics: Females' Sample

	1860 (1)	1880 (2)	1900 (3)	1920 (4)	1940 (5)	All (6)
In the labor force	0.144	0.156	0.202	0.237	0.289	0.236
	(0.351)	(0.363)	(0.401)	(0.425)	(0.453)	(0.425)
Age	33	34	34	35	37	35
	(12)	(13)	(13)	(13)	(13)	(13)
White	0.980	0.875	0.886	0.898	0.899	0.896
	(0.141)	(0.331)	(0.318)	(0.303)	(0.301)	(0.305)
Hispanic	0.007	0.007	0.006	0.010	0.015	0.011
	(0.082)	(0.083)	(0.079)	(0.100)	(0.120)	(0.101)
Married	.	0.735	0.728	0.767	0.773	0.759
	(.)	(0.441)	(0.444)	(0.423)	(0.419)	(0.428)
Number of Children	1.980	1.866	1.690	1.536	1.281	1.537
	(2.194)	(2.126)	(2.056)	(1.911)	(1.734)	(1.929)
N	67,512	129,316	203,045	292,626	416,559	1,811,437
Age First Time Mother	24.052	24.213	24.370	24.390	24.503	24.397
	(5.750)	(5.885)	(5.646)	(5.580)	(5.662)	(5.677)
N	40,881	76,769	114,326	163,991	217,901	1,006,066
Nb of States	49	49	49	49	49	49
States: Seating Law	0	0	27	45	47	47
States: Regulatory Law	0	1	12	28	28	28
States: Night work Law	0	0	5	17	20	20

Notes: Individual level data are from Census IPUMS 1% samples for decades between 1860 and 1940 (except for 1890 due to missing census data). Data covers all U.S. states except for Hawaii and Alaska. We report means and standard deviations in parentheses for each variable of interest for the decades 1860, 1980, 1900, 1920, 1940, and all decades combined. The labor force status and individual characteristics are for women between the age of 16 and 65. "Married" is a dummy variable that takes the value of one if a woman over the age of 16 ever got married and zero if she never got married. The "Number of Children" variable counts the number of own children (of any marital status) residing with each individual. "Age of First Time Mother" is computed by subtracting the age of the eldest child from the mother's age. We restrict the sample to the "age of first time mother" values between 15 and 45 to account for measurement errors. Census person weights are used in all calculations. The table also reports the number of states that implemented each policy in a specific decade.

Table 2.2: Effects of State Law Policies

	Seating Laws		Regulatory Laws		Night work Laws	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: DID estimates on $StateLaw_{sd}$						
Women's LFP	-0.003	0.001	0.007	0.008	0.015	0.013
	(0.008)	(0.008)	(0.007)	(0.006)	(0.010)	(0.009)
	[0.768]	[0.888]	[0.352]	[0.246]	[0.156]	[0.177]
N	1,811,237					
Men's LFP	-0.001	-0.001	0.003	0.003	-0.004*	-0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	[0.638]	[0.731]	[0.188]	[0.171]	[0.100]	[0.086]
N	1,818,771					
Panel B: DDD estimates on $StateLaw_{sd} \times Women$						
LFP	-0.002	0.003	0.004	0.005	0.019*	0.017*
	(0.008)	(0.008)	(0.007)	(0.007)	(0.010)	(0.009)
	[0.852]	[0.765]	[0.595]	[0.524]	[0.076]	[0.080]
N	3,630,008					
States	49	49	49	49	49	49
Individual Controls	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Panel A displays estimates for the State Law Policies for men and women separately. In Panel B, we pool the sample of men and women and show estimates for the interaction between each State Law Policy and a dummy variable "women" that takes the value of one if the individual "i" is a woman. The dependent variables thus are "Women's labor force participation", "Men's labor force participation" and "Labor force participation" respectively. Each row displays a separate weighted estimation and each value displays an estimate for a different labor law. Standard errors in parentheses are clustered at the state-level. Wild cluster bootstrap p-values are reported in square brackets. Bootstrap errors are clustered by state (the level at which they are clustered in the cluster-robust variance estimator (CRVE)). We report estimates for our variables of interest $Seating\ Law_{sd}$, $Regulatory\ Law_{sd}$ and $Night\ Work\ Law_{sd}$ that equal one for states that had any seating, regulatory or night work laws respectively as of decennial year d and zero otherwise. Individual controls include age, race and ethnicity. State and decade fixed effects are included in all regressions. As for the significance of the results: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 2.3: Effects of State Law Policies on Women's LFP in Affected Industries

	Mercantile	Manufacturing All	Manufacturing Durable Goods	Manufacturing Non-durable	Personal Services	Mining
	(1)	(2)	(3)	(4)	(5)	(6)
Seating Laws	-0.003	0.000	-0.000	0.000	-0.005	
	(0.004)	(0.004)	(0.002)	(0.003)	(0.006)	
	[0.696]	[0.968]	[0.923]	[0.906]	[0.424]	
Regulatory Laws		0.002	0.006**	-0.004		0.000
		(0.004)	(0.003)	(0.003)		(0.000)
		[0.596]	[0.155]	[0.247]		[0.414]
Night work Laws	-0.003	0.010***	0.000	0.010***	-0.020***	
	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	
	[0.343]	[0.009]	[0.923]	[0.007]	[0.036]	
Ind. Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1,811,238	1,811,238	1,811,238	1,811,238	1,811,238	1,811,238
Nb of States	49	49	49	49	49	49

Notes: The sample is restricted to women aged 16–65. Each column shows estimates from separate weighted regressions for a different indicated industry. The dependent variable is thus women's employment, a dummy variable that takes the value of 1 if a woman is employed in the indicated industry, and 0 otherwise. We report estimates for our variables of interest $Seating\ Law_{sd}$, $Regulatory\ Law_{sd}$ and $Night\ Work\ Law_{sd}$. These are now dummy variables that take the value of one if the state had a labor law targeting the specific industry. Missing estimates indicate that the policy in question did not target the industry of interest. Individual controls include age, race and ethnicity. Standard errors are clustered at the state level. Wild cluster bootstrap p-values are reported in square brackets. Bootstrap errors are clustered by state (the level at which they are clustered in the cluster-robust variance estimator (CRVE)). State fixed effects and decade fixed effects are included in all regressions. As for the significance of the results: *** p<0.01, ** p<0.05, * p<0.1

TABLE 2.4: Effects of State Law Policies on Women's Marriage and Fertility

	Seating Laws		Regulatory Laws		Night work Laws	
	(1)	(2)	(3)	(4)	(5)	(6)
Marriage	-0.003	-0.004	0.004	0.001	0.006	0.003
	(0.003)	(0.003)	(0.005)	(0.005)	(0.004)	(0.005)
	[0.360]	[0.236]	[0.474]	[0.778]	[0.230]	[0.543]
N	1,646,586					
Divorce	-0.000	-0.000	0.002	0.002	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.961]	[0.983]	[0.215]	[0.217]	[0.991]	[0.887]
N	1,121,360					
Nb of Children	-0.073	-0.075	-0.005	-0.005	0.038	0.039
	(0.046)	(0.047)	(0.040)	(0.041)	(0.048)	(0.048)
	[0.176]	[0.167]	[0.909]	[0.906]	[0.479]	[0.471]
N	1,811,238					
Age at First Birth	0.040	0.012	-0.035	-0.015	-0.130	-0.040
	(0.061)	(0.040)	(0.074)	(0.042)	(0.082)	(0.055)
	[0.569]	[0.796]	[0.639]	[0.749]	[0.190]	[0.566]
N	1,005,972					
States	49	49	49	49	49	49
Individual Controls	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variables are "Marriage", "Divorce", "Number of Children" and "Age at First Birth". See notes of Table 2.1 for details of sample and variables. Each row shows estimates from separate weighted regressions for different indicated outcomes. Standard errors in parentheses are clustered at the state level. We report estimates for our variables of interest *Seating Law*$_{sd}$, *Regulatory Law*$_{sd}$ and *Night Work Law*$_{sd}$. Each law is a dummy variable that takes the value of one if the state passed the indicated law. Individual controls include age, race and ethnicity. Wild cluster bootstrap p-values are reported in square brackets. Bootstrap errors are clustered by state (the level at which they are clustered in the cluster-robust variance estimator (CRVE)). State fixed effects and decade fixed effects are included in all regressions. As for the significance of the results: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 2.5: Effects of State Law Policies - Women Liberation and Suffrage Controlled

	(1)	(2)	(3)	(4)
Panel A: Seating Law				
Women's LFP	0.001	0.002	0.001	0.002
	(0.008)	(0.008)	(0.008)	(0.008)
	[0.888]	[0.816]	[0.874]	[0.792]
N		1,811,237		
Men's LFP	-0.001	-0.001	-0.001	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)
	[0.731]	[0.580]	[0.726]	[0.589]
N		1,818,771		
LFP	0.003	0.003	0.003	0.003
	(0.008)	(0.008)	(0.008)	(0.008)
	[0.765]	[0.761]	[0.756]	[0.758]
N		3,630,008		
Panel B: Regulatory Law				
Women's LFP	0.008	0.008	0.007	0.007
	(0.006)	(0.006)	(0.006)	(0.006)
	[0.246]	[0.261]	[0.277]	[0.272]
N		1,811,237		
Men's LFP	0.003	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
	[0.171]	[0.247]	[0.173]	[0.147]
N		1,818,771		
LFP	0.005	0.005	0.005	0.005
	(0.007)	(0.007)	(0.007)	(0.007)
	[0.524]	[0.524]	[0.523]	[0.520]
N		3,630,008		
Panel C: Night work Law				
Women's LFP	0.013	0.013	0.013	0.013
	(0.009)	(0.010)	(0.009)	(0.010)
	[0.177]	[0.247]	[0.170]	[0.244]
N		1,811,237		
Men's LFP	-0.004*	-0.003	-0.004*	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)
	[0.086]	[0.168]	[0.085]	[0.167]
N		1,818,771		
LFP	0.017*	0.017*	0.017*	0.017*
	(0.009)	(0.009)	(0.009)	(0.009)
	[0.080]	[0.081]	[0.085]	[0.078]
N		3,630,008		
Financial Liberation	No	Yes	No	Yes
Suffrage Rights	No	No	Yes	Yes
States	49	49	49	49
Individual Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes

Notes: See notes of Table 2.2. For the Labor Force Participation (LFP) outcome, we show estimates for the interaction between State Law policy and a dummy variable "women" that takes the value of one if the individual "i" is a woman. In addition to previous controls, we control for Women's Suffrage and Women's Financial Liberation. We rely on data on suffrage laws passage from Lott and Kenny, 1999 and Miller, 2008. Data on the timing of women's financial liberation by state is obtained from Geddes and Dean, 2002. We control for women's suffrage by adding a dummy variable that takes the value of one if a given state s had a law allowing women to vote as of a given decade d, and zero otherwise. Similarly for women's financial liberation.

Table 2.6: Effects of State Law Policies - Heterogeneity Analysis

	Seating Law		Regulatory Law		Night work Law	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Electrical Capacity						
Women's LFP	0.009	0.010	0.001	-0.001	-0.001	-0.003
	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
N	1,811,237					
Men's LFP	-0.000	-0.000	-0.001	-0.001	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
N	1,818,771					
LFP	0.009	0.011**	0.002	0.002	0.002	-0.001
	(0.006)	(0.005)	(0.007)	(0.006)	(0.007)	(0.006)
N	3,630,008					
Panel B: Share of Urban Population						
Women's LFP	0.052	0.062**	0.002	-0.005	0.030	0.010
	(0.036)	(0.029)	(0.027)	(0.028)	(0.071)	(0.068)
N	1,811,237					
Men's LFP	-0.005	-0.005	0.003	0.003	-0.022*	-0.018
	(0.011)	(0.010)	(0.008)	(0.007)	(0.013)	(0.011)
N	1,818,771					
LFP	0.056*	0.071***	0.000	-0.007	0.052	0.031
	(0.029)	(0.022)	(0.026)	(0.027)	(0.067)	(0.062)
N	3,630,008					
States	49	49	49	49	49	49
Individual Controls	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: See notes in Table 2.2. For the Labor Force Participation (LFP) outcome, we show estimates for the interaction between State Law policy and a dummy variable "women" that takes the value of one if the individual "i" is a woman. Each row displays a different estimate for the outcome of interest. In Panel A, we report coefficients on the interaction term between each of our variables of interest *Seating* Law_{sd}, *Regulatory* Law_{sd} and *Night Work* Law_{sd} and the natural log of Total Electrical Capacity in 1919. Each law is a dummy variable that takes the value of one if the state passed the indicated law. Total capacity is calculated as the average capacity across counties in a given state. In Panel B, we report coefficients for the interaction term between each of our variables of interest *Seating* Law_{sd}, *Regulatory* Law_{sd} and *Night Work* Law_{sd} and the share of urban population in 1920 for each state. Individual controls include age, race and ethnicity.

Chapter 3

Strategic Timing in the Appearance of Scandals

3.1 Introduction

Media coverage of scandals about public officials have been shown to increase the likelihood of resignation, decrease the voting shares for candidates, and reduce the likelihood of incumbents to seek re-election (Dobratz and Whitfield, 1992; Shea, 1999; Basinger, 2013; and Garz and Sörensen, 2017). While several studies argue that the effects of scandals depend on the timing of their appearance (Praino, Stockemer, and Moscardelli, 2013; Doherty, Dowling, and Miller, 2014; Mitchell, 2014; and Pereira and Waterbury, 2019), little is known about why they appear at some times and not others. Anecdotal evidence suggests that reporting of certain scandals could be orchestrated through under-the-table negotiations and strategic timing (Garz and Sörensen, 2021; Gratton, Holden, and Kolotilin, 2018; and Cook, Hardin, and Levi, 2005). To minimize their negative publicity, politicians might want to see their own misconduct revealed at times when other newsworthy events are crowding the media and distracting the public. The underlying assumption is that minimizing exposure to negative media attention is better than dragging it out or having it repeated with higher frequency (Gibson, 1999).

The practice of timing unpopular actions may be part of the folk wisdom of politics and appears in many real-world examples.[1] Perhaps the most striking evidence appeared in a leaked memo written by the special adviser of Stephen Byers; the transport secretary under Tony Blair's administration. On the afternoon of September 11, 2001, Jo Moore said that it was "a very good day to get out anything we want to bury".[2] There also exists some systematic evidence on the use of such tactics in politics. For instance, DellaVigna, 2009 finds that the president is less likely to sign a non-controversial executive order or law on a Friday; and Djourelova and Durante, 2021 find that executive orders by U.S. presidents are more likely to be signed when the news is dominated by other important stories that can crowd out media coverage. The contribution of this

[1] "It's a time-honored practice where the president's trying to talk about what he wants to talk about and push the subjects that maybe he doesn't want to talk as much about into a time when people aren't paying as much attention," said Dee Dee Myers, press secretary during Clinton's first two years in office.

[2] The Guardian, 09 October 2001.

paper is to consider whether strategic timing is also observed for scandals, a very different type of political event over which politicians may have much less direct control.

This paper relies on a novel list of misconduct by elected officials and aims to examine the strategic timing of scandals' appearance in the United States. The argument is made that elected officials anticipate the structure of the news cycle to release potentially harmful information, aiming to reap the benefit of uninformed voters during the next election.

A simple theoretical framework is first developed to better understand the different mechanisms behind the timing of scandals' appearance. The study relies on a rational learning model and introduces voter exposure to news as a function of news pressure, opening the possibility of strategic timing.[3] The presence or absence of other newsworthy events can affect the coverage of a specific issue and, consequently, the probability that a voter finds information about it. It is assumed that news cannot be fabricated; the only strategy available to politicians is thus to orchestrate the timing of bad news' appearance.

I allow politicians to capture the media and propose a setup where they can partially anticipate the level of news congestion. The strategy of elected officials is to increase the share of voters informed about the opposition's bad news and minimize voters' exposure to their own misconduct. On the other hand, media outlets select political coverage to maximize profits. The optimal strategy for the media is to release a scandal when the number of newsworthy events is low. This is due to the limited supply of high-audience news and relatively lower revenues accruing from scandal coverage.

A forward-looking strategy allows only predictable news events to be used by politicians to distort public opinion. For instance, it is documented that Tony Blair's government had a weekly diary, "The Grid", filled with all sorts of forthcoming events so that all government members knew which days were good or bad to plan anything else. "The news is not left to chance", said journalist Peter Oborne, suggesting that this list of events provides a strong tool for public officials to manipulate the public's exposure to their actions.[4] The same process, however, is less likely to occur with unpredictable events given the short time available for politicians to arrange coverage of a specific news story.

For the empirical analysis, I rely on a list of keywords for each politician and his/her episodes of misconduct from The Political Graveyard and conduct an automated search from newspaper electronic archives (newspapers.com) to build the outcome variable on scandal media coverage. Data on scandals' first appearance in newspapers is combined with a weekly measure of "news pressure".[5] To capture strategic timing in the relationship between scandals' appearance and news pressure, I implement an instrumental variable strategy in which the level

[3]In this class of models, media matters because it provides information to voters (Enikolopov, Petrova, and Zhuravskaya, 2011; DellaVigna and Kaplan, 2007; and Gerber, Karlan, and Bergan, 2009)) and increases voters' responsiveness to news about politicians (Ferraz and Finan, 2008 and Larreguy, Marshall, and Snyder, 2020).

[4]BBC News Online Magazine, 25 May 2004.

[5]Following Eisensee and Strömberg, 2007, this measure is derived from the total amount of time devoted to the top three stories featured on the evening newscast of NBC, ABC, and CBS.

of weekly news pressure is predicted by the occurrence of predicted events that have the capacity to crowd out the news and the public's attention. "Predicted events" consist of U.S. military attacks and famous U.S. trials. Information is also collected on the timing and characteristics of natural and human-made disasters to construct an "unpredicted events" category to employ in a placebo analysis.

I document and discern a large, positive, and statistically significant effect of news pressure, instrumented by military attacks and famous trials on the timing of scandal release for all politicians. In contrast, I find no evidence that scandals are timed to unpredictable events.

Given that media coverage might vary for different types of scandals (Doherty, Dowling, and Miller, 2011; Basinger, 2013; and Doherty, Dowling, and Miller, 2014), I disaggregate the dependent variable into sexual, financial, political, and other types of scandals.[6] Previous studies also find that people might respond differently to "office-related" (e.g., financial or political) and "moral" (e.g., sexual) scandals. Moral scandals are often emotionally charged and speak to the character of a politician. They are thus expected to negatively influence the personal evaluations of a politician. In contrast, office-related scandals are more likely to involve illegal behavior and may thus reveal more about an individual's competence as a government official (Carlson, Ganiel, and Hyde, 2000). Therefore, these types of office-related scandals tend to affect a politician's job evaluations as well as his/her personal evaluations.[7]

The effect of news pressure on all scandals seems to be driven by the timing of the appearance of sexual and political scandals. Despite the potential negative reaction from the public to financial scandals, I find no evidence that the timing of their appearance is strategic. This could be due to the fact that financial scandals usually appear following a legal process and may involve multiple politicians simultaneously, which makes their occurrence harder to manipulate.

In order to gain insights on heterogeneity amongst different political parties, I split the sample of politicians into Republicans and Democrats and examine whether there exists any differential effect of news pressure (induced by the occurrence of predicted events) on the likelihood of scandals' appearance. Both political parties are found to adopt similar strategies as their scandals tend to appear when news pressure is high due to the occurrence of military attacks and famous trials. The findings, however, suggest that Republican politicians are behaving more strategically in timing the revelation of sexual and political misconduct.

Finally, I examine the impact of scandals and the level of news pressure on the election outcomes of the politicians involved. While all types of scandals

[6]This classification was previously adopted by Basinger, 2013; Hamel and Miller, 2019; and Pereira and Waterbury, 2019. Despite the importance of these distinctions, further data disaggregation would leave too few media scandals of each type to analyze empirically.

[7]Related studies tend to reach different conclusions about which type of scandal induces a stronger negative reaction from the public. For instance, Carlson, Ganiel, and Hyde, 2000 find that people respond more negatively to office-related scandals, whereas other studies tend to either find no difference between the two or the reverse relationship (Welch and Hibbing, 1997; Brown (2001) and Brown (2006); and Thapa and Brown, 2007).

lead to a decrease in the share of votes and increase the likelihood of resignation or loss, I find that the likelihood of exit is 13 percentage points lower for politicians whose scandal appeared on high news pressure weeks relative to low news pressure weeks. This finding suggests that revealing officials' misconduct when the public is distracted by other newsworthy events may potentially lead to lower political accountability.

The key obstacle to research on scandals so far is the difficulty of defining and measuring scandals themselves. Most studies related to this topic usually rely on a specific national newspaper (e.g., the Washington Post and the New York Times) and use the word "scandal" as a keyword for their search. But successfully identifying all scandals is a difficult challenge, especially since outlets might use the term "scandal" differently based on their audiences' preferences (Gentzkow and Shapiro, 2010 and Puglisi and Snyder Jr, 2011).[8] One of the main contributions of this paper is that it relies on a novel dataset of misconduct episodes. Although it does not provide an exhaustive list of scandals, it provides a rigid list of keywords, on both politicians and scandals, that facilitate a search for related press articles. Thus, relying on this list allows us to identify the time when each scandal was uncovered in newspapers. Additionally, the newspaper archives I use contain a large number of local newspapers, allowing me to observe reported scandals involving lower-level or local government officials. To the best of my knowledge, this paper is the first to provide a convincing causal effect on the possible manipulation in politics when it comes to revealing misconducts, as well as heterogeneity of different types of scandals and variation across political parties.

This paper relates to a number of literatures. First, it contributes to the literature on strategic behavior in politics, the rational inattention and the strategic release of information (e.g. Patell and Wolfson, 1982; Damodaran, 1989; DellaVigna, 2009; Doyle and Magilke, 2009; and DeHaan, Shevlin, and Thornock, 2015). It provides systematic evidence that these tactics are employed by high- and low-level officials to manipulate negative publicity stemming from scandals. Second, this study contributes to the literature on political accountability and mass media (Besley, Burgess, and Prat, 2002; Ferraz and Finan, 2008; Prat and Strömberg, 2013; Snyder and Strömberg, 2010; and Sobbrio, 2014). This paper's finding that politicians strategically time the appearance of bad news reveals their ability to manipulate voters' information about elected officials. Finally, the methodology employed relates to recent studies on the use of strategic timing by corporations (DellaVigna, 2009), NGOs (Couttenier and Hatte, 2016), the military (Durante and Zhuravskaya, 2018), and U.S. presidents (Djourelova and Durante, 2021). It provides empirical evidence that strategic timing is adopted by elected officials in the revelation of their misconduct.[9]

I structure the remainder of the paper as follows. In Section 3.2, I develop a simple theoretical framework capturing the different mechanisms behind a

[8] In fact, this might be the reason why most studies related to this topic limit their analysis to a specific case study or a specific public office (e.g., presidents or governors).

[9] While other studies examine the timing of scandals' release for U.S. presidents (Nyhan, 2015) and U.S. governors (Nyhan, 2017), they do not study the sort of forward-looking strategic behavior that this analysis documents for multiple levels of elected officials.

scandal's appearance in the media. I present the data sources and descriptive statistics in Section 3.3. Section 3.4 presents the methodology. In Section 3.5, I provide empirical evidence of strategic timing and examine the impact of scandals on electoral outcomes of politicians in the sample. Finally, the last section concludes.

3.2 Conceptual Framework

The aim of this section is to develop a simple model suggesting the mechanisms and the expected outcomes of any relationship between the appearance of scandals and the existence of newsworthy events in the media. Precisely, the objective of this model is to generate insights on the actual time of a scandal's first appearance with respect to news pressure.

3.2.1 Model Setup

I rely on a rational learning model in which voters are rational and media matters because it transmits information. In this standard model of informative media, there are three classes of actors: voters; politicians; and the media. This setup was first used by Strömberg, 2001 and has subsequently been used in a number of papers focusing on the effect of media access, coverage, bias, and capture.[10] I introduce voter exposure to news on politicians as a function of news pressure to highlight on the importance of strategic timing. News pressure is defined as the level of news congestion and varies with the availability of newsworthy material.

A two-party political system is employed where incumbent i belongs to political party R and incumbent j belongs to political party D. Politicians have important reasons for trying to get covered in the news media. Media attention allows them to set the political agenda (Sellers, 2000), to boost their position within the party (Davis, 2010), and – perhaps most importantly – it increases the chance that citizens will vote for them (Erkel, Aelst, and Thijssen, 2020). I allow politicians to manipulate the timing of news' appearance.

There are two possible types of news that media outlets can disclose about politicians. Let $\theta \in \{b, g\}$, where g stands for "good" and b for "bad". Since information is a public good, voters become informed if at least one outlet reports a news item. Media in this context is defined as a single player.

All media outlets are identical and face two possible sources of profit - commercial profits and profits from collusion with politicians. Commercial profits are broadly audience driven. They can take the form of sales, subscriptions, or advertising. Audiences increase if the media outlet reports interesting information. Profits from collusion with politicians are various; at one extreme are direct monetary payments (e.g., bribes).[11] They can however take a more

[10] See, for instance, Besley and Prat, 2006; Hellwig and Veldkamp, 2009; Prat and Strömberg, 2013.

[11] Manipulating the media through bribes might however be very costly. Mcmillan and Zoido, 2004 provide evidence that silencing the media with bribes is expensive.

subtle and indirect form of influence, such as an administrative decision or a legislative intervention that benefits a firm controlled by the media owner.

Moreover, each outlet has a limited number of journalists and a finite amount of space in which to present stories.

3.2.2 Information and Voting

Voters are dependent on political campaigns and mass media for most of their political information. For instance, evidence on learning from newspapers consistently showed significant effects (Berelson, Lazarsfeld, and McPhee, 1954; Neuman, Neuman, and Marion R. Just, 1992; Norris and Sanders, 2003) and newspapers are seen as key providers of political information (Snyder and Strömberg, 2010).[12]

Mass media informs voters, and this information affects their voting decision.[13] Citizens are assumed to form attitudes based on considerations that are most accessible; and the media coverage of an issue improves access to information about that issue.[14] Importantly, it is not information about the issue that has an effect; it is whether the issue has received a certain amount of processing time and attention (Scheufele and Tewksbury, 2007). In fact, press releases and press conferences are most successful when they are circulated at times when the journalistic demand for "new" information is high, i.e., in the absence of big events, or low news pressure (Eisensee and Strömberg, 2007).

Voters are modeled as a unitary block. They try to elect politicians who will give them the most utility. Media matters because it provides relevant information about each incumbent and allows voters to form and/or update their beliefs. Their expected utility from incumbent i winning depends on the information received and is expressed as:

$$E[u_i(\theta)] = w_g(n)u_i(g) + w_b(n)u_i(b)$$

where $\theta \in \{b, g\}$ represents news on politician i and $u_i(\theta)$ its valuation. In this model, news on politicians represent a perfect signal and fully reveals politicians' character and voters have a negative valuation for bad news (i.e., $u_i(b) < 0$). $w_\theta \in [0, 1]$ is the voters' exposure parameter to news on politicians and n the amount of news pressure N, with $n \in [0, 1]$.[15]

From the presentation above, it is safe to assume that $w_\theta(n)$ is decreasing in n. That is, when there exists a high number of newsworthy events (i.e., high

[12] Some might argue that the influence of traditional news media on the public has decreased because people increasingly get their information from social media and alternative outlets. Yet, as Bennett (2012, p. 144) remarks: "The mainstream media continues to play a key role in shaping public perceptions simply because it still sends out the loudest signal."

[13] Voters' responsiveness to media news is found in studies involving new, surprising information; such as corruption and scandals (Ferraz and Finan, 2008; Larreguy, Marshall, and Snyder, 2020).

[14] This hypothesis is adopted in memory-based models and derived from the agenda setting theory (McCombs and Shaw, 1972) and the priming theory (Iyengar and Kinder, 1992). The memory processes underlying these theories are supported by research on memory by biologists and psychologists (e.g., Schacter, 1996).

[15] The parameters $w_g(n)$ and $w_b(n)$ are probabilities with $w_g(n) + w_b(n) = 1$.

news pressure), voters are more distracted and less likely to notice the media coverage of a politician. In fact, the presence or absence of other newsworthy events affects the coverage of a specific issue and, consequently, the likelihood that a voter finds information about it. The impact of bad news is thus amplified with a higher exposure.

The condition for voter v to choose incumbent i over incumbent j is as follows:

$$E[u_i(\theta)]) - E[u_j(\theta)] \geq \tilde{\beta_v}$$

where $E[u_i(\theta)]$ and $E[u_j(\theta)]$ are the expected utilities from incumbent i and j winning, respectively, and $\tilde{\beta_v}$ is an exogenous preference parameter.

The following aspect of the study focuses solely on bad news limited to scandals. Scandals in this context are interpreted as resulting from the disclosure of official misconduct (Apostolidis and Williams, 2004). As for the timing, this retrospective voting model consists of two periods. At the start of the game, an incumbent is in power. Under-table negotiations *(if any)* between media and incumbents about the timing of a misconduct revelation take place during the first period. At the end of the first period, media outlets make their report on the scandal available to voters. If no agreement between media and incumbents is formed in period one, the media outlet unilaterally chooses the timing of a scandal appearance. Voters only observe the news; they do not observe whether a media coalition was built or not. In the second period, voters receive information from media, update their preferences, and vote.

3.2.3 Strategies and Predictions

The model's equilibria are driven from two building blocks: political competition and media's optimal strategy. Politicians try to get re-elected, and perhaps enjoy political rents, and the media selects political coverage to maximize profits.

I begin by introducing the first block - the role of political competition in the timing of scandals' appearance. Since media attention is such a powerful resource for politicians, they might make increasingly professionalized efforts to manipulate the media and thus manipulate the political process (Strömbäck and Esser, 2017). For instance, bad news on politician i will decrease his/her chances of getting re-elected, while bad news on his/her opponent j will increase it. Politicians would want to increase the share of voters informed about the opposition's bad news and to minimize voters' exposure to their own misconduct. I assume that news cannot be fabricated; the only strategy available to politicians is thus to manipulate the timing of a bad news' appearance.[16] They can orchestrate the timing of an appearance by releasing information they already have or abusing their power and bribing the media. Politicians can also pressure the media by offering preferential news access to friendly outlets (Besley and Prat, 2006).

[16]This assumption is adopted in most media capture models. See, for instance, Besley and Prat, 2006.

I thus study the occurrence of media scandals as a political event and argue that the timing of "disclosure" of politicians' related news is non-random. Politicians attempt to influence the timing of information release intending to maximize their political benefits. This is partly achieved by reducing the public's attention, hence the impact, of bad news.[17] Politicians want to increase their chances of getting re-elected by minimizing the exposure of their bad news and/or maximizing the exposure of the opposite party's bad news.[18]

Events can be classified according to whether their timing is predictable or foreseeable. Politicians can time the appearance of scandals by observing, a priori, the occurrence of events that would potentially capture the media attention.

I now begin to characterize the subgame equilibria in period $t = 2$. Let S_i be a scandal on politician $i \in \{R\}$, and $P(S_i)$ the probability that it appears in media M. This appearance is defined to be conditional on news pressure N. The probability that a scandal on politician i appears at a specific level of news pressure $N = n$ is thus $p_i = P(S_i|N = n) = g(n, S_i)$, where $p_i \in [0, 1]$ is a conditional probability that satisfies all the Kolmogorov axioms.

Lemma 1. *If the timing of a misconduct disclosure is decided by politician $i \in \{R\}$ (i.e., $g(n, S_i) = g_R(n, S_i)$), the likelihood of a scandal appearance is increasing in n.*

Proof. See Appendix.

This result establishes that releasing a scandal when there is a high number of newsworthy events is an optimal strategy for politician i if he/she succeeded in capturing the media and manipulating the time of release. The intuition is as follows. Politicians will attempt, in so far as it is possible, to steer their bad news into the time where they will receive less attention in order to minimize negative publicity. Politician i can reduce the attention given to bad news about him/her if he/she can arrange to have the bad news appear on days when there is strong competition from other important stories (i.e., high news pressure). This strategy can be adopted not only by politician i but also by his/her political party R.[19]

Lemma 2. *If the timing of misconduct disclosure is decided by politician $j \in \{D\}$ (i.e., $g(n, S_i) = g_D(n, S_i)$), the likelihood of a scandal appearance is decreasing in n.*

Proof. See Appendix.

[17]The underlying assumption is that minimizing the amount or duration of negative media attention is better than dragging it out or having it repeated with greater frequency (Gibson, 1999).

[18]Such behavior is argued to be part of the folk wisdom of politics. For instance, it seems common for politicians to take advantage of low newspaper readership on Saturdays, by dumping bad news on Fridays (Norris, 2005; Walsh and Austin, 2013).

[19]Theoretically, a scandal on a politician can plausibly affect the political party to which he/she belongs. For instance, Daniele, Galletta, and Geys, 2020 find empirical evidence that scandals can become transmitted across politicians and levels of government through the partisan cues embedded in party affiliations.

The interpretation of this result mirrors the previous one. Politician j (or the political party D) aims to maximize the negative publicity of its opposition. He/she can increase the voters' exposure to the opposition's bad news by capturing the media and trying to release the information on days (or weeks) when competition from other stories is weak.

The second building block of the equilibria is the media's strategy when it comes to timing a scandal release. Due to limited news space and audience attention, the occurrence of other newsworthy events can crowd out information that is relevant to evaluate government behavior.[20] For instance, when few major stories are available, media outlets are more likely to investigate and report surprising news on politicians and to give more prominent placement to those stories. By contrast, when the news agenda is congested, the opportunity costs of covering a given story will increase.[21] This is likely to occur especially since scandals about politicians are shown to generate relatively low levels of public interest - and thus lower revenues - compared to other types of news stories (Robinson, 2007).

Lemma 3. *If the timing of a misconduct disclosure is decided by the media outlet (i.e., $g(n, S_i) = g_M(n, S_i)$), the likelihood of a scandal appearance is decreasing in n.*

This result establishes that releasing a scandal when there is a low number of newsworthy events is an optimal strategy for media outlets if they are the ones deciding on the time of release. The intuition is as follows. Since the supply of high-audience news is limited and scandals about politicians are shown to generate lower revenues compared to other types of big events, media outlets would rather publish a scandal when news pressure is low. In addition, competition from other news stories can reduce the incentive to pursue allegations of wrongdoing, or crowd them off of the front page, relative to 'slow news' periods in which the opportunity costs of pursuing scandal coverage are lower. As a result, media scandals will be more likely to emerge when the news agenda is less congested.[22]

Equilibria The timing of media scandals about politicians can be interpreted as a joint production of the different political parties and media (Nyhan, 2015).[23] The possible outcomes of the under-table negotiations at the end of period $t = 2$ are discussed below. A scandal appearance at a specific time is either the result of the media's business motives, or the media being captured by politicians from different political parties.

[20] Eisensee and Strömberg, 2007 demonstrate this effect in a study showing that external events frequently crowd out media coverage of foreign disasters.

[21] Entman, 1989 shows that the attention devoted to a particular story depends on the context in which it occurs.

[22] Even when a scandal is about a high-ranking politician and is believed to generate higher revenues, a media outlet would rather publish the related report in a way to optimally capture public attention and maximize its profits. This is achieved when news pressure is low.

[23] Corneo, 2006 also previously considered a model with a heterogeneous electorate where the media can collude with various interest groups.

The sign of the relationship between the likelihood of scandals' appearance and news pressure levels will allow us to determine partially which political party succeeded in strategically timing the information release. The model's plausible equilibria and their implications are thus characterized as follows:

Proposition 1. *A scandal regarding politician i appearing when news pressure is high is driven by the politician i, or the political party R, capturing the media.*

and,

Proposition 2. *A scandal regarding politician i appearing when news pressure is low is driven by either (1) the media's business motives or (2) the opposition party capturing the media.*

Proof. See Appendix.

The first plausible equilibrium suggests that politician i, or the political party R, succeeded in minimizing the public's exposure to the scandal and thus minimizing negative publicity. This indicates that politician i was able to capture the media and strategically time the appearance of his/her own scandal. While it is hard to disentangle between the two factors in the second equilibrium, the literature suggests that political motives of actors involved in the production of scandals are more relevant for the timing of scandals than the business motives by the newspapers (Garz and Sörensen, 2021).

3.3 Data Sources and Main Variables

In the empirical analysis, data are used on (1) episodes of misconduct by elected political figures, including details on both politicians and misconduct episodes; (2) the timing of scandals' first appearance in newspapers and the amount of coverage; (3) all stories that appeared on evening US TV news, including information about their order, length and topic; (4) the timing of conflict-related events; and (5) the occurrence of natural and human-induced disasters.

3.3.1 Scandals and Media Coverage

This paper relies on a list of misconduct derived from The Political Graveyard between 1970 and 2020. The Political Graveyard includes a political biography of around 300,000 politicians from the United States arranged by name, date, office, and geographic location. This website also compiles and categorizes politicians along dimensions such as whether they received a special recognition or distinction (e.g., Nobel Prize) or were in trouble or disgrace during their life. The latter category provides insight to code whether a politician with a misconduct was still in office when the scandal appeared in the media. To be listed in the category Trouble and Disgrace, what is required is "a public scandal, or some kind of formal action, such as censure, disbarment, impeachment, recall or expulsion from office, including resignation under fire; or some kind of brush with the criminal justice system, including arrest, indictment, conviction, or imprisonment. In a few cases uncontested evidence of wrongdoing emerged after death (The Political Graveyard)."

I exclude presidents and vice presidents; activists, and politicians who never held a public office; diplomats; judicial positions; and appointed cabinet members.[24] For each politician, I check whether the scandal appeared when he/she was still in office. The aim is to keep politicians in elected positions and whose misconduct may affect their future election outcomes. The final sample includes 233 episodes of misconduct, of which 200 are about politicians who were still in office when the scandal appeared in media. Table 3.1 shows summary statistics for the main sample of misconduct.

The dataset provides details on each politician, such as the year of birth, race, gender, political party, the office positions, the state of office, as well as details on their misconduct. Office positions are classified into four major office types: federal legislative (U.S. senators and U.S. representatives), state legislative (state senators and state representatives), state executive (governors, state comptrollers and state treasurers), and local government (mayors, sheriffs, borough or parish presidents, and county executives).

The classification system is adopted from Basinger, 2013 and divides scandals into four categories. *Sex scandals* includes extramarital affairs, solicitation of a prostitute, sodomy, sexual assault, and sexual harassment. The *financial scandals* category includes inadequate financial disclosures, embezzlement, tax evasion or fraud, and improper employment practices (e.g., payroll kickbacks). *Political scandals* include, conspiracies, perjury, neglect of duty, election fraud, and campaign finance violations (e.g., inadequate disclosure of donations and solicitation of donations from illegitimate sources, such as foreigners). Misuse of campaign donations belongs to this category if the funds were not converted to personal use. *Other scandals* is a residual category that includes illicit drug use, speeding, assaults, driving while intoxicated, plagiarism, and gambling. Appendix Table A1 shows summary statistics by type of scandal.

For each politician, I rely on a list of keywords from The Political Graveyard and conduct an automated search for related press articles from newspaper electronic archives (newspapers.com) to build the outcome variable on scandal media coverage.[25] I identify the date on which the misconduct was first mentioned and build the main variable of "Scandals' Appearance" as a dummy variable that takes the value of one on the week when the scandal appeared.[26] I also record the daily number of newspaper articles covering each scandal until 30 days after the first appearance. The paper relies on the variation in timing of the first appearance to capture strategic timing of a scandal release. Of note, the first media appearance can cover an investigation made public, a newspaper

[24] The appearance of scandals about presidents could be affected by their decision of launching a military attack, which violates the exclusion restriction of the adopted identification strategy. Presidents are thus excluded from the analysis.

[25] Newspapers.com is one of the largest newspaper electronic archives in the U.S. It includes archives of around 21,600 national and local newspapers from the 1700s till now.

[26] A scandal might first appear in one or multiple newspapers. The large number of newspapers included in the archives allows a more accurate capture of the date of the first appearance.

report, or a politician being indicted, charged, or arrested.[27]

The model of capture developed in Section 3.2 has focused so far on media bias that is induced by media capture. Other potential sources of media bias exist; for instance, partisan media bias (also known as ideological bias) is seen as the most common charge leveled against the U.S. media, as its coverage could systematically favor one party over the other. Partisan bias can thus affect the timing of a scandal release.

To examine whether any partisan bias exists in the newspapers sample, I collect data on the number of appearances for each politician (using their full name) 30 days before and 30 days after the scandal's appearance. Table 3.2 shows the average number of appearances splitting the sample by political affiliation. While there exists a significant increase in the number of times a politician is mentioned after a scandal, the data suggest that there is no systematic difference in politicians' appearances by their political party affiliation, either before or after a scandal.[28] This finding is in line with previous suggestive evidence that most U.S. media outlets are centrist (see, for instance, Groseclose and Milyo, 2005; Ho and Quinn, 2008; Puglisi and James M. Snyder, 2015).[29]

3.3.2 News Pressure

To measure news congestion, I rely on data from the Vanderbilt Television News Archives (VTNA) for the 1968–2020 period.[30] VTNA contains more than 30,000 individual network evening news broadcasts and 700,000 news stories from the major U.S. national broadcast networks. I rely on daily data of evening news broadcasts on the top three US networks: NBC, ABC, and CBS.[31] For each day and each network, information is available on the order of appearance, the length in seconds, and a summary of the topic. This information is used to construct a measure of the presence of newsworthy events.

I follow Eisensee and Strömberg, 2007 and define "News Pressure" for a given network as the time (in minutes/10) devoted to the top three stories that are unrelated to scandals (if any) in the evening newscast on NBC, ABC, and CBS, which are the three networks with 30-minute evening editions. News related

[27]The variation in the timing of misconduct disclosure exists even within legally prosecuted cases. For instance, a U.S. representative, Louis Stokes (D-Ohio), was arrested for driving under the influence on March 25, 1983, but the story appeared in newspapers a week later.

[28]Appendix Figure A1 displays the number of articles covering scandals by politicians' political party affiliation for up to 30 days from the first appearance. I also find that there is no difference in the amount of scandal coverage for Democrat and Republican politicians.

[29]Gentzkow, Glaeser, and Goldin, 2006 also show that media market competition led successful newspapers to develop a reputation for independent information provision rather than favoring their political patrons.

[30]Data retrieved from the Harvard Dataverse on March 4, 2021: https://doi.org/10.7910/DVN/BP2JXU.

[31]CNN is excluded as it features news around the clock, and data are only available starting in October 1995.

to scandals is excluded to avoid reverse causality (i.e., a scandal can appear in newspapers because it appeared on TV).[32]

The daily news pressure measure is the median number (across broadcasts in a given day) of minutes a news broadcast devoted to the top three new segments in a day. I rely on the median value among the three networks to reduce the influence of measurement error. From September 1968 until December 2019, daily news pressure ranges between 0.7 and 30 minutes, with an average of around 8 minutes. Appendix Figure A2 shows the distribution of the daily news pressure measure. The main measure is the "Weekly News Pressure" computed as the weekly average of the daily news pressure variable.

This paper argues that this news pressure indicator is a valid measure of news congestion in newspapers. Previous studies have shown that TV news pressure reflects well the presence or absence of significant competing stories for all forms of mainstream outlets including main and local newspapers (see Crouse, 1973; Sabato, 1991; Bennett, 2004). Therefore, the added value of relying on newspapers to measure news pressure is likely to be limited.

3.3.3 Conflicts, Famous Trials, and Disasters

I also gather data on a list of predicted and unpredicted events to predict the variation in news pressure. While both types of events are likely to capture the media attention, foreseeable ones create some ex-ante expectations and would potentially reflect the strategic timing involved in a scandal release.

The variable "Predicted events" represents U.S. military attacks and famous trials, and the variable "unpredicted events" represents natural and human-induced disasters. I argue that U.S. military attacks are planned ahead and thus serve as predicted events. For instance, the War Powers Resolution[33] is a federal law intended to check the U.S. president's power to commit the United States to an armed conflict without the consent of the U.S. Congress within at least 48 hours of committing armed forces to military action. In addition, most of the launched military attacks required the congress approval on bills funding the operations.[34] For famous trials, I focus on the last day of hearing during which the sentencing took place. This date is the most likely to be known to the public and thus capture media attention the best. These events are plausibly exogenous with respect to scandals in the sense that they do not affect the appearance of scandals about politicians except via the channel of news pressure.

[32] The news pressure measure without excluding scandals is highly correlated with the main measure. The correlation coefficient between daily news pressure and daily news pressure corrected for scandals is of 0.99.

[33] Also known as the War Powers Resolution of 1973; or the War Powers Act. The resolution was adopted in the form of a United States congressional joint resolution. It provides that the president can send the U.S. Armed Forces into action abroad only by declaration of war by Congress, "statutory authorization", or in case of "a national emergency created by attack upon the United States, its territories or possessions, or its armed forces".

[34] For instance, while the War Powers Resolution has been violated during the bombing campaign in Kosovo in 1999, President Bill Clinton's legal team opined that its actions were consistent with the War Powers Resolution because Congress had approved a bill funding the operation, which they argued constituted implicit authorization.

For the list of major conflict related events, I rely on data from "Cline Center Historical Phoenix Event Data" from the University of Illinois Data Bank.[35] I focus on events for which the U.S. military was the main actor and identify dates on which the U.S. got involved in international material conflicts, such as battles and/or unconventional mass violence. The sample is restricted to the period of the analysis (i.e., September 1969 until December 2019).[36]

For famous trials, I rely on "Famous Trials" compiled by Professor Douglas O. Linder and supported by the University of Missouri-Kansas City Law School. The selection of famous trials is based on some specific characteristics. First, the trial must have grabbed the public's attention. Additionally, the trial must also have shaped history in some significant way or, alternatively, be an especially good window for observing and understanding a particular time.

The sample of famous trials is restricted to those that took place in the U.S. from 1969 to 2019. I exclude trials related to American domestic politics such as: Chicago 8 (or Chicago 7), John Hinckley's trial, Falwell vs Flynt, and Bill Clinton's impeachment trial. The final sample thus consists of a total of 19 trials. I focus on the last day of trial during which the sentencing took place.

Data on natural and human-induced disasters are drawn from the Spatial Hazard Events and Losses Database for the United States (EM-DAT database).[37] I focus on disasters that took place in the U.S. between 1968 and 2019 and led to at least one casualty.

3.4 Empirical Strategy

3.4.1 Predicted vs Unpredicted Events

The aim of the empirical analysis is to examine the timing of misconduct disclosure or the appearance of media scandals. This could be established by identifying the relationship between scandals' appearances and news pressure. To verify whether a given scandal is strategically timed by any of the different actors, I examine its relationship with news pressure induced by predicted events. In other words, a politician would be able to time a scandal appearance to news pressure only through events that he/she was able to anticipate

[35] The dataset (1945–2019) includes around 8.2 million political events extracted from 21.2 million news stories coded from the NYTimes (1945–2018), the Wall Street Journal (1945–2006), BBC Monitoring's Summary of World Broadcasts (1979–2019) and the Central Intelligence Agency's Foreign Broadcast Information Service (1995–2004). Retrieved from `https://databank.illinois.edu/datasets/IDB-2796521` on March 26, 2020.

[36] The sample on military attacks contains around a thousand event including invasions (e.g., Panama, Afghanistan, Iraq), military interventions (e.g., Bosnia and Herzegovina, Libya, Iraq, Syria), and major operations in wars (e.g., Persian Gulf War, War in Northwest Pakistan, Somalia and Northeastern Kenya, Ocean Shield operation vs. Somali Pirates, Lord's Resistance Army in Uganda, and the Yemeni Civil War).

[37] Also known as the International Disasters Dataset, compiled by the Center for Research on the Epidemiology of Disasters at the Catholic University of Louvain. Retrieved from `http://www.emdat.be/database` on March 26, 2020.

and to expect that they will have an impact on the level of news pressure.[38] There should be, however, no link between the likelihood of a scandals' appearance and the level of news pressure induced by unpredicted events. Recall that "predicted events" include U.S. military attacks and famous trials, whereas the "unpredicted events" category consists of natural and human-induced disasters.

3.4.2 Model Specification

I implement an instrumental variable strategy in which the first stage predicts the level of weekly news pressure by the occurrence of predicted events. I then use this predicted news pressure measure in the second stage to estimate the impact of news pressure on the likelihood of scandals' appearances. The following specification is estimated:

$$NewsPressure_{tmy} = \alpha PredictedEvents_{tmy} + \lambda_m + \gamma_y + \omega_{tmy}$$

$$S_{itmy} = \mu \widehat{NewsPressure}_{tmy} + X_i' \beta + \lambda_m + \gamma_y + \varepsilon_{itmy} \tag{3.1}$$

where S_{itmy} is a discrete variable that takes the value of one if a scandal regarding politician i appeared on week t of month m of year y, and zero otherwise. $PredictedEvents_{tmy}$ equals one if a predicted event occurred during a specific week. $\widehat{NewsPressure}_{tmy}$ is the predicted news pressure from the first-stage regression. X_i' is a vector of time invariant control variables containing information on politicians including their year of birth, gender, race, office type, and state of office. I also include full sets of year dummies γ_y and month dummies λ_m to capture seasonality. Standard errors are clustered at the year-month level.

This specification exploits variation in the occurrence of predicted events to obtain variation in news pressure level. The parameter of interest μ is thus identified if news pressure is restricted to affect the likelihood of scandals' appearance only through the channel of the existence of predicted events. These types of events create some ex-ante expectations and the effect could then potentially reflect the strategic timing involved in a scandal's release. The IV strategy thus allows one to estimate the local average effect of news pressure on the likelihood of the appearance of scandals only for the variation in news pressure stemming from the occurrence of predicted events.

In this sense, only predictable events may be strategically used to sway public opinion. Hence, I can uncover evidence about a strategic timing when the estimate of the coefficient of interest is large and statistically significant. If the coefficient is significantly positive, I argue that the politician is strategically timing the appearance of his/her scandal when the attention of the public is captured by other events. In contrast, if the coefficient is negative, the opposition might be trying to time the politician's scandal to low news pressure levels. The opposition's aim is to avoid newsworthy events in order to not crowd out the attention to the reported scandal. Theory's predictions on media's strategic

[38]This methodology has been widely adopted in the literature on strategic timing. See for instance, Durante and Zhuravskaya, 2018; and Djourelova and Durante, 2021.

behavior imply that media outlets are more likely to release scandals to fill low-activity weeks (i.e., when news pressure is low). Empirically, this would lead to a downward bias in the estimates if a positive effect of news pressure on the likelihood of scandals' appearance is detected.

The main sample includes 200 politicians whose scandals appeared when they were still in office. For each politician, I include all weeks within six months of the scandal's first appearance.[39]

To examine heterogeneity, I split the sample into Republican and Democrat politicians. Additionally, I pool both political parties and interact $News\widehat{Pressure}_{tmy}$ with "*Democrat*" to examine the relationship for Democrats relative to Republicans. "*Democrat*" is thus a dummy variable that takes the value of one if politician i is a Democrat, and zero otherwise. Unlike the full main sample, there are no independent politicians in this pooled one.

I rely on "unpredicted events" to instrument for "News pressure" in a placebo check. Even though politicians would want to time the appearance of scandals to this type of events, they would have limited time (and thus limited ability) to orchestrate any misconduct's disclosure.

3.5 Findings

In this section, I present the empirical findings. I first show the estimates of equation 3.1, and then discuss some suggestive evidence on how scandals affected the political outcomes of politicians in the sample.

3.5.1 Evidence on Strategic Timing

Table 3.3 presents the first-stage relationship between predicted events and the weekly level of news pressure. In column 3, the regression includes month and year fixed effects. The instrument is a very strong predictor of news pressure: news pressure is, on average, between 0.3 and 0.8 minutes higher during weeks that coincide with the considered military attacks and famous trials. The first-stage relationship between news pressure and unpredicted events is reported in Appendix Table A2. News pressure is, on average, between 0.2 and 0.6 minutes higher on weeks where a human-made or natural disaster took place.[40]

The value of the reported F-statistics, conditional on controls, is larger than 10, and thus suggests that the instruments that are employed are valid. In other words, the selected "predicted events" have the capacity to crowd out the news. The magnitude of the estimates is in line with previous studies predicting the variation in the news pressure measure by the occurrence of specific events. For

[39] In Section 3.5, I show that the main findings are robust to removing this sample restriction.

[40] Unpredicted events are seen as weak instruments conditional on controlling for underlying observable and unobservable systematic differences between the observed months and years. They are however used as placebo for the main analysis.

instance, Durante and Zhuravskaya, 2018 find that political and sports events are associated with 1.8 minutes higher news pressure for the 2000–2011 period.[41]

The results of the second stage regressions are presented in Table 3.4. All columns include the set of controls for politicians' time-invariant characteristics, as well as month and year fixed effects. I find a strong positive and significant effect of news pressure instrumented by military attacks and famous trials on the timing of scandal releases (panel A). The results listed in Column 1 suggest that a 1-minute increase in news pressure due to predicted events is associated with a 3 percentage point increase in the likelihood of a scandal appearance.

The effect on all scandals seems to be driven by the timing of appearances of sexual and political scandals (columns 2 and 4). Elected officials are aware that these types of scandals are expected to negatively influence their personal reputations. Additionally, politics-related scandals would reveal that the concerned politician is incompetent as a government official. Politicians would thus aim to strategically limit voter exposure to these scandals. The appearance of financial scandals are somewhat different than the other types of scandals, since there is often an ongoing legal process, and many politicians may be involved at the same time (e.g. Abscam and the Keating Five scandals). Their time of appearance is thus harder to manipulate, which might explain the absence of strategic timing for this type of scandal despite the potential strong negative reaction from the public.[42]

Appendix Table A4 displays estimates of the reduced form regressions of "predicted events" on the likelihood of a scandal appearance. What clearly emerges is that the occurrence of a predicted event is positively associated with scandal appearance.[43]

In panel B of Table 3.4, estimates indicate that there is no significant relationship between the timing of a scandal release and news pressure instrumented by human-made and natural disasters. The coefficient for all scandals (column 1) is close to zero; estimates for different types of scandals (columns 2–5) are small and statistically insignificant at conventional levels.

I also examine the heterogeneous effect of news pressure induced by predicted events on the different political party affiliations of politicians in the sample. In the first and second panels of Table 3.5, I split the sample into scandals about Democrat and Republican politicians, respectively. In the last panel, politicians from both political parties are pooled together, and the relationship for Democrats relative to Republicans using an interaction term between News Pressure and Democrats is examined. The point estimates on all scandals in the

[41]The analysis in this paper cannot rely on political events, as any domestic-politics related occasion might be endogenous to the appearance of scandals. Additionally, including sports events during the study period to the "predicted events" category gives a lower F-statistics value (see Appendix Table A3). I thus exclude sports indicators for events from the analysis.

[42]Of note, newspapers' business motives from reporting sexual scandals are high as controversial stories are most likely to capture public attention and thus generate higher sales. Newspapers might want to report sexual scandals when news pressure is low, meaning the coefficient on sex scandals might be underestimated.

[43]Results for the ordinary least square regressions of news pressure on scandals' appearance are presented in Appendix Table A5. I find almost no relationship between news pressure and likelihood of scandals' appearance.

first two panels are positive for both subsamples (although statistically insignificant at conventional levels), suggesting that both Republicans and Democrats are behaving strategically in timing the revelation of misconduct to coincide with other foreseeable newsworthy events that distract the public's attention. The estimate for the interaction term is small and statistically insignificant, suggesting that there is not a significant difference between Democrats and Republicans.

Estimates in columns 2 to 5 focus on sex, financial, political, and other types of scandals, respectively. The estimated effect is positive and statistically significant for Republicans for sex and political scandals (columns 2 and 4), while the point estimates are all statistically insignificant for Democrats (potentially due to low statistical power). The interaction term confirms that there is a significant difference for political scandals between Republicans and Democrats, while the difference between political parties is not statistically significant for sex scandals. These findings suggest that Republican politicians are behaving strategically in timing the revelation of certain types of misconduct. Put differently, Republicans seem to strategically time the appearance of political scandals significantly better than Democrats.

To further examine the theory's predictions, a focus on battleground states is applied. Politicians should theoretically be more concerned about their public image in these states, as they witness stronger competition. The effect of news pressure on scandals' appearance is expected to be larger. I compute the average margin of victory during the 1972–2020 Presidential Elections (i.e., elections that took place during the analysis period) and define battleground states as those in which the average margin of victory was lower than 20% or 15% on average.[44] Table 3.6 presents the estimates for all states (column 1), 32 states who had on average a margin of victory lower than 20% (column 2), and 20 states who had on average a margin of victory lower than 15% (column 3). I find some evidence that scandal appearance is more likely to be timed to predicted events in battleground states, with a coefficient of 0.363 (s.e. 0.196) in the third column versus a coefficient of 0.295 (0.159) in column 1, although the difference is not statistically significant at the 10% level.

In an additional exercise, I rely on the same average share of votes of opposite political parties to divide states into Democrat dominant states and Republican dominant states. Democrat dominant states are those in which Democrat politicians were on average more likely to win over the analysis period. Republican dominant states are similarly defined. For Democrat politicians in the first panel of Table 3.7, the estimate is significantly positive in states where Republicans are more likely to win. In the last panel, the estimate of the first column is significantly negative, suggesting that Republicans are strategically timing their scandal appearances relatively better than Democrats in Democrat dominant states. These findings imply that politicians are more likely to strategically time their scandal appearance to predicted newsworthy events in states where the opposite political party is dominant. Interestingly, this pattern is similar for all types of scandals (see Appendix Table A6).

[44] Data on the shares of votes is from Dave Leip's Atlas of U.S. Presidential Elections: https://uselectionatlas.org/.

3.5.2 Robustness Checks

In a first set of robustness checks, I replace the outcome variable by continuous measures of scandals' appearance in Appendix Table A7. In columns 1–5 (6–10), I examine the impact of news pressure instrumented by predicted events on the number (ratio) of appearances during the seven days following the first appearance. The point estimates on all scandals are positive and statistically significant for the two continuous measures (columns 1 and 6), suggesting again that scandals are more likely to be covered when news pressure is high. The pattern of findings for different types of scandals is also similar to the main results for the likelihood of scandals' appearance.[45]

In another set of specification checks, I replicate the analysis of Table 3.4 (panel A) in Appendix Table A9 and replace the set of politicians' controls by politicians fixed effects. As in the main results, I find a positive and significant effect of news pressure instrumented by military attacks and famous trials on the timing of scandal releases for all politicians. For Republicans (3rd panel), the impact is significantly positive on the appearance of sex and political scandals. All points estimates are strikingly similar in magnitude to those for the main findings.

I also rely on the sample of politicians who were no longer in office (i.e. a total of 33 politicians) when their misconduct was released to the media. Estimates in Appendix Table A10 suggest that there is no significant effect of news pressure instrumented by predicted events on the likelihood of scandals appearing (column 1), nor on the continuous measures of scandal appearance (columns 2 and 3). This could be due to the fact that former politicians care less about timing the release of their misconduct, as it no longer affects their political outcomes.

To examine whether the findings are driven by the sample restriction of six months within the scandal appearance, I replicate Table 3.4 and include the full sample (i.e. all weeks between September 1968 and December 2019). Appendix Table A11 shows that the main findings are in line with the original findings. The point estimates in panel A remain positive and statistically significant, although smaller in magnitude. The effect of news pressure instrumented by unpredicted events remains absent (panel B).

Finally, media and opposition parties may be as much or more interested in arranging news events to the disadvantage of competing opposition parties before elections.[46] To understand the dynamics during elections cycles, I replace the month and year fixed effects by quarter fixed effects in panel A of Appendix Table A12. The point estimates become larger in magnitude in all specifications. Note that only 38 scandals in the sample (i.e., 19% of scandals) appeared during the elections cycle. The larger positive effect might thus be driven by scandals that appeared in non-election periods. In panel B, I exclude scandals that

[45] I find no effect of news pressure predicted by human-made and natural disasters on the continuous measures of scandals' appearance (see Appendix Table A8).

[46] For instance, Garz and Sörensen, 2021 show that scandals are more likely to occur during elections period in Germany; and Gratton, Holden, and Kolotilin, 2018 provide empirical evidence that while real scandals are released as they are discovered, fake scandals are strategically delayed and concentrated towards the end of the election campaign.

appeared during the last part of the election cycles (i.e., last quarter of every four years when congressional and presidential elections take place). Again, the estimates are larger in magnitude, suggesting that politicians are more likely to time the coverage of their own misconduct in non-election periods.

3.5.3 Effect of Scandals on Political Outcomes

One key role of elections is to allow voters to punish politicians who perform poorly in office. While the impact of scandal on involved politicians has been widely studied, I highlight the extent to which election results of politicians in the sample were affected. I thus construct a panel data set on the vote share for each politician for two elections prior and one election after the scandal appearance.[47]

I first display the change in the share of votes induced by the scandal appearance. Figure 3.1 presents the change in vote share for all politicians in the sample.[48] The panel data are unbalanced in the sense that the last election results exclude politicians who resigned or lost in primaries (i.e., did not survive the scandal and did not participate in the elections right after the scandal appearance). I check whether there exists a pre-trend prior to the scandal and find that pre-scandal vote share is constant as there seems to be no change in the share of votes for politicians between the two elections before their scandal appeared. However, scandals appear to lead to a drastic decrease in the average share of votes for politicians involved (i.e., before versus after a scandal).[49]

A scandal does not only affect the share of votes, but can lead voters to completely remove an involved politician from office. In Appendix Table A13, I show that a scandal appearance increases the likelihood of politicians exiting politics (i.e., not participating in the next election), and the likelihood of politicians losing the post-scandal election. These results highlight the seriousness of the scandals in the sample and makes it hard to argue the assertion that bad publicity is good publicity in this context.

I also briefly examine the impact of news pressure level during the week of a scandal's first appearance on political outcomes. Table 3.8 shows OLS estimates of news pressure on the likelihood of exit (columns 1–3) and the share of votes (columns 4–6). Conditional on all controls, the likelihood of exit is 13 percentage points lower for politicians whose scandal appeared on high news pressure weeks relative to low news pressure weeks. While the effect of a scandal on the share of votes is negative, there seems to be no significant differential effect for the level of news pressure. One can thus argue that exposing scandals when the public is distracted by other newsworthy events may potentially lead to lower political accountability.

[47] Data on the share of votes is from ourcampaigns.com, retrieved in August, 2021.

[48] Three politicians were removed from this analysis due to the lack of data on their elections results.

[49] This is true for all types of scandals (see Appendix Figure A3).

3.6 Conclusion

Little is known on why scandals appear in the media at specific times and not others. This is not just a matter of evidence, but is also related to political agendas and contexts (Shaw, 1999 and Reeves, 2011). If politicians are aware that media coverage has a significant impact on their career, they might make a concerted effort to manipulate the media and thus the political process. One potential means of influence is through the strategic timing of news about themselves. Due to limited news space and audience attention, the occurrence of especially newsworthy events can crowd out other information, opening the possibility of burying reported scandals when they are less likely to draw attention.

This paper is the first to study the strategic timing in scandals' appearance. It relies on a novel list of politicians' misconduct episodes and provides empirical evidence that the timing of scandals could be orchestrated by elected officials in order to minimize their political costs. More precisely, I find that politicians strategically manipulate the timing of their own unpopular actions to coincide with other important (predictable) events that distract the mass media and public. Finally, I show that media coverage of scandals at times with high level of "news pressure" lessens the electoral consequences of politicians' misconduct. As media plays "a crucial role as watchdogs, informing citizens about any improper conduct by those in power" (Puglisi and Snyder Jr (2011, p. 931)), shedding light on this issue is crucial to understand to what extent strategic behavior by elected officials can limit political accountability.

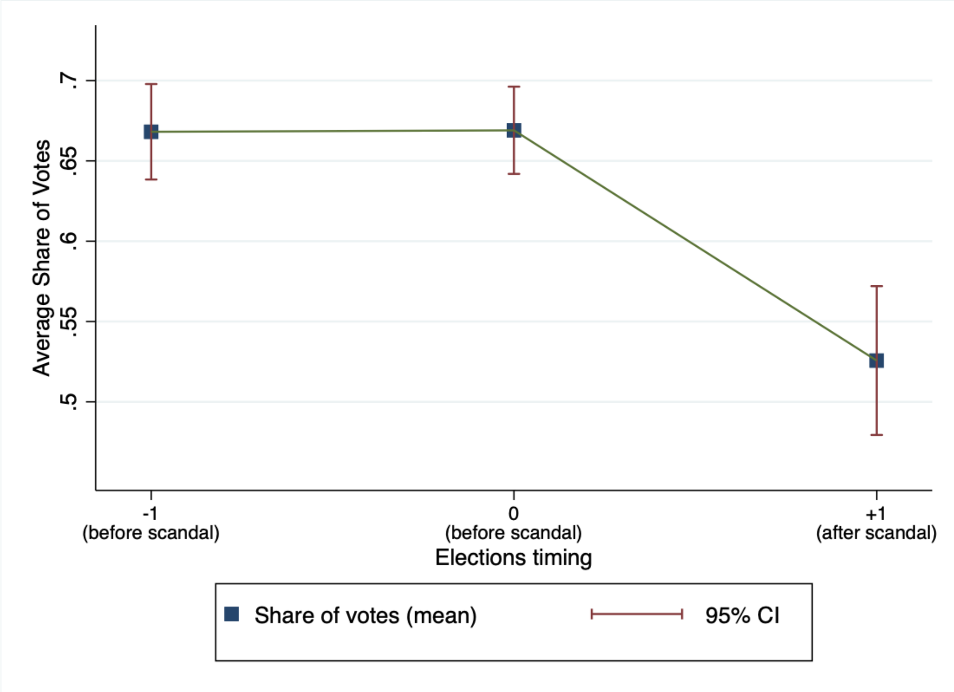


FIGURE 3.1: Change in Share of Votes for all Politicians (N=187; unbalanced)
Source: ourcampaigns.com

Table 3.1: Summary statistics – Episodes of Misconduct

	Frequency	Percent
Panel A: By Time of Misconduct		
1970 – 1979	39	19.50
1980 – 1989	55	27.50
1990 – 1999	35	17.50
2000 – later	71	35.50
Total	200	100
Panel B: By Political Party		
Democrat	130	65.00
Republican	64	32.00
Other/Unknown	6	3.00
Total	200	100
Panel C: By Office Type		
Federal legislative	82	41.00
State legislative	40	20.00
State executive	28	14.00
Local government	50	25.00
Total	200	100
Panel D: By Scandal Type		
Sex Scandal	41	20.50
Financial Scandal	94	47.00
Political Scandal	34	17.00
Other Scandal	31	15.50
Total	200	100

Notes: The main sample is limited to politicians who were still in office when the misconduct appeared in the media.

TABLE 3.2: Average Appearance of Politicians' Name – by Political Party

		30 days before	30 days after	**Difference**
Republican	*mean*	155.969	827.109	**671.141*** **
	se	(40.819)	(186.781)	[0.001]
	N	64	64	128
Democrats	*mean*	182.362	812.608	**630.246*** **
	se	(50.442)	(144.133)	[0.000]
	N	130	130	260
Difference	*mean*	**-26.393**	**14.502**	**40.894**
	p-value	[0.734]	[0.953]	[0.85]
	N	194	194	388

Notes: This table shows the average number of appearances of the politician's name in all newspapers within 30 days from the media scandal appearance. The sample includes politicians who belong to either Democrat or Republican party. Standard errors are reported in parentheses and p-values in brackets.

TABLE 3.3: First Stage – Predicted Events

	Weekly News Pressure		
	(1)	(2)	(3)
Predicted Events	0.084***	0.084***	0.032***
	(0.010)	(0.010)	(0.009)
N	13,201	13,201	13,201
F-statistics, excluded instruments	71.69	72.49	13.99
R-Squared	0.051	0.067	0.263
Month FE		✓	✓
Year FE			✓

Notes: The table presents first stages of the 2SLS regressions with the weekly news pressure as dependent variables and predicted events as regressor. Predicted events include military attacks and famous trials. Each column represents a separate regression. The sample includes all weeks within six months of the scandal's first appearance. I report the F-statistic of a joint test on whether all excluded instruments (i.e., military attacks and famous trials) are significantly different from zero. Standard errors are clustered by month × year and reported in parenthesis. * p< 0.10, ** p< 0.05, *** p<0.010.

Table 3.4: 2SLS – All Politicians

	Outcome: Likelihood of scandals' appearance				
	All Scandals	Sex	Financial	Political	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Predicted Events as Instrument					
News Pressure	0.295*	0.074*	0.092	0.074*	0.056
(All Politicians)	(0.159)	(0.044)	(0.122)	(0.043)	(0.043)
N	13,201	13,044	13,097	13,035	13,034
Panel B: Unpredicted Events as Instrument (placebo)					
News Pressure	0.001	-0.083	0.030	0.001	0.075
(All Politicians)	(0.143)	(0.098)	(0.137)	(0.076)	(0.095)
N	13,201	13,044	13,097	13,035	13,034
Politician Controls	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the likelihood of scandals' appearance as dependent variable and the weekly news pressure as the main explanatory variable. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks within six months of the scandal's first appearance. In panel A, predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In panel B, unpredicted events are used to instrument for weekly news pressure. It includes human-made and natural disasters. Politicians' controls include the year of birth, gender, race, office type, and state of office. Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * $p< 0.10$, ** $p< 0.05$, *** $p<0.010$.

Table 3.5: 2SLS – Predicted Events – By Political Affiliation

	Outcome: Likelihood of scandals' appearance				
	All Scandals	Sex	Financial	Political	Other
	(1)	(2)	(3)	(4)	(5)
News Pressure	0.380	0.028	0.230	0.020	0.099
(Democrats)	(0.247)	(0.049)	(0.207)	(0.047)	(0.066)
N	8,652	8,544	8,590	8,543	8,541
News Pressure	0.204	0.141*	-0.103	0.164**	0.010
(Republicans)	(0.152)	(0.080)	(0.096)	(0.078)	(0.054)
N	4,282	4,236	4,243	4,229	4,228
News Pressure	0.105	-0.046	0.162	-0.081*	0.066
$\times$ Democrats	(0.145)	(0.054)	(0.115)	(0.047)	(0.050)
N	12,934	12,780	12,833	12,772	12,769
Politician Controls	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the likelihood of scandals' appearance as dependent variable and the weekly news pressure as the main explanatory variable. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks within six months of the scandal's first appearance. Predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In the first and second panel, the sample is restricted to Republican and Democrat politicians, respectively. In the last panel, politicians from both political party are pooled together. "Democrat" is a binary variable that takes the value of 1 if the scandal is on a Democrat politician, and 0 on a Republican politician. Politicians' controls include the year of birth, gender, race, office type, and state of office. Each estimate represents a separate regression. Standard errors are clustered by month $\times$ year and reported in parenthesis. * $p< 0.10$, ** $p< 0.05$, *** $p<0.010$.

TABLE 3.6: 2SLS – Predicted events – Battleground states

	Likelihood of app.		
	Main Sample	20% margin of victory	15% margin of victory
	(1)	(2)	(3)
News Pressure	0.295*	0.329*	0.363*
(All Politicians)	(0.159)	(0.171)	(0.196)
N	13,201	12,130	7,124
News Pressure	0.380	0.428	0.509*
(Democrats)	(0.247)	(0.261)	(0.284)
N	8,652	8,183	4,577
News Pressure	0.204	0.224	0.200
(Republicans)	(0.152)	(0.166)	(0.206)
N	4,282	3,680	2,347
News Pressure	0.105	0.122	0.266
× Democrats	(0.145)	(0.150)	(0.217)
N	12,934	11,863	6,924
Nb of States	39	32	20
Nb of scandals	200	183	107
Politician Controls	✓	✓	✓
Month FE	✓	✓	✓
Year FE	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the likelihood of a scandals' appearance as dependent variable and the weekly news pressure as the main explanatory variable. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks within six months of the scandal's first appearance. Predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In the second and third panel, the sample is restricted to Republican and Democrat politicians, respectively. In the last panel, politicians from both political party are pooled together. "Democrat" is a binary variable that takes the value of 1 if the scandal is on a Democrat politician, and 0 on a Republican politician. Politicians' controls include the year of birth, gender, race, office type, and state of office. In column 2 and 3, the sample is restricted to scandals about politicians in states where the average margin of victory during the 1972–2020 presidential elections is <=15% and <=20%, respectively. Data on the share of votes is form Dave Leip's Atlas of U.S. Presidential election. Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * p< 0.10, ** p< 0.05, *** p<0.010.

TABLE 3.7: 2SLS – Predicted Events – States by Average Political Party Dominance

	Outcome: Likelihood of scandals' appearance	
	Democrat dominant	Republican dominant
	(1)	(2)
News Pressure	-0.356	0.707*
(Democrats)	(0.352)	(0.371)
N	3,206	5,179
News Pressure	0.332	0.113
(Republicans)	(0.258)	(0.190)
N	2,408	1,874
News Pressure	-0.647*	0.361
× Democrats	(0.367)	(0.244)
N	5,614	7,053
Politician Controls	✓	✓
Month FE	✓	✓
Year FE	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the likelihood of scandals' appearance as dependent variable and the weekly news pressure as the main explanatory variable. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks within six months of the scandal's first appearance. Predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In the first and second panel, the sample is restricted to Republican and Democrat politicians, respectively. In the last panel, politicians from both political party are pooled together. "Democrat" is a binary variable that takes the value of 1 if the scandal is on a Democrat politician, and 0 on a Republican politician. Politicians' controls include the year of birth, gender, race, office type, and state of office. In column 1 (2), the sample includes scandals about politicians in states where the average share of votes during the 1972–2020 presidential elections was higher for Democrats (Republicans). Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * $p< 0.10$, ** $p< 0.05$, *** $p<0.010$.

Table 3.8: Effect of Scandals' Appearance on Political Outcomes

	Outcome: Exit			Outcome: Share of votes		
	(1)	(2)	(3)	(4)	(5)	(6) IV
High newspressure	-0.150** (0.071)	-0.140* (0.072)	-0.134* (0.075)			
Scandal				-0.164*** (0.0274)	-0.268* (0.140)	-0.236 (0.770)
Scandal × High newspressure					0.142 (0.180)	0.098 (1.043)
N	200	200	187	206	206	206
Time Controls		✓	✓	✓	✓	✓
Type of Scandal		✓	✓			
Politicians Controls		✓	✓			
Lagged share of voted			✓			
Politician FE				✓	✓	✓

Notes: The table presents estimates for ordinary least square regressions. In columns 1–3, the outcome "exit" is a binary variable equals to 1 if the politician died, resigned, or lost elections after a scandal appearance, and 0 otherwise. The sample in columns 4–6 is limited to the 103 politicians who did not exit politics. "High newspressure" is a dummy equals to 1 if the scandal appeared on a week where weekly news pressure is above the mean, 0 otherwise. Scandal is also a dummy equals to 0 for pre-scandal period and 1 for post-scandal period. In column 6, I rely on "predicted events" to instrument for news pressure. Politicians' controls include the year of birth, gender, race, office type, and state of office. Each estimate represents a separate regression. Standard errors are reported in parentheses, clustered by month × year. * $p< 0.10$, ** $p< 0.05$, *** $p<0.010$.

Appendix A

Appendix – Chapter 1

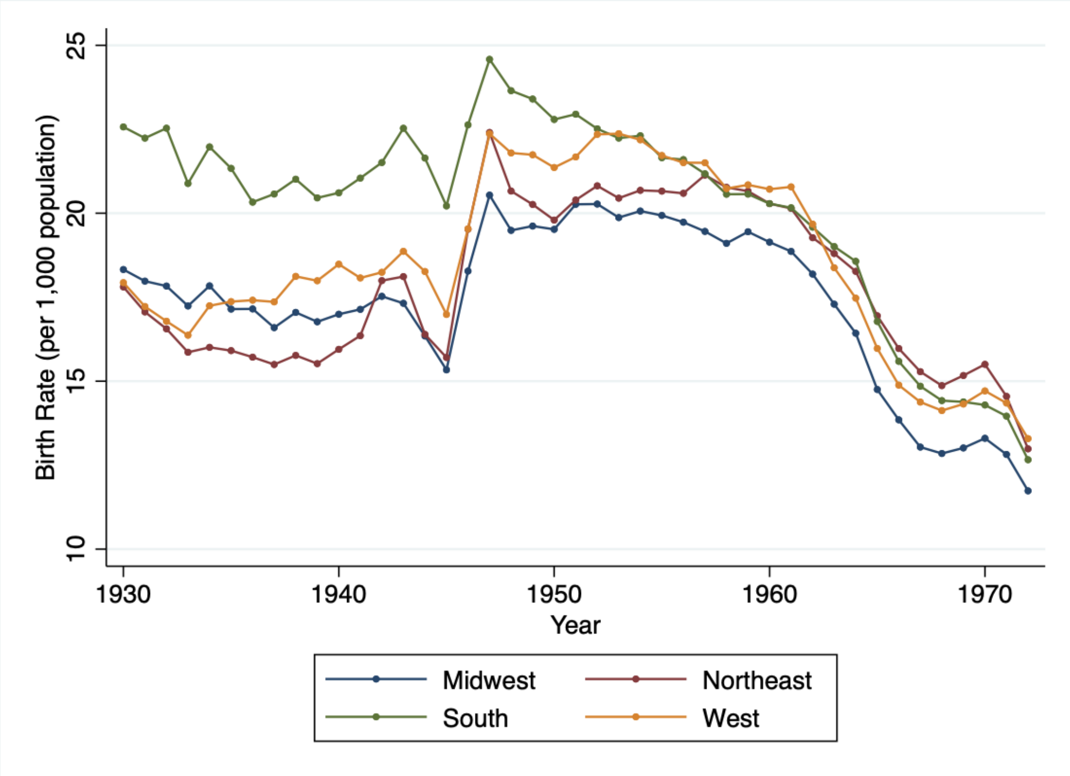


FIGURE A1: Fertility trends by U.S. regions. *Source*: IPUMS-USA

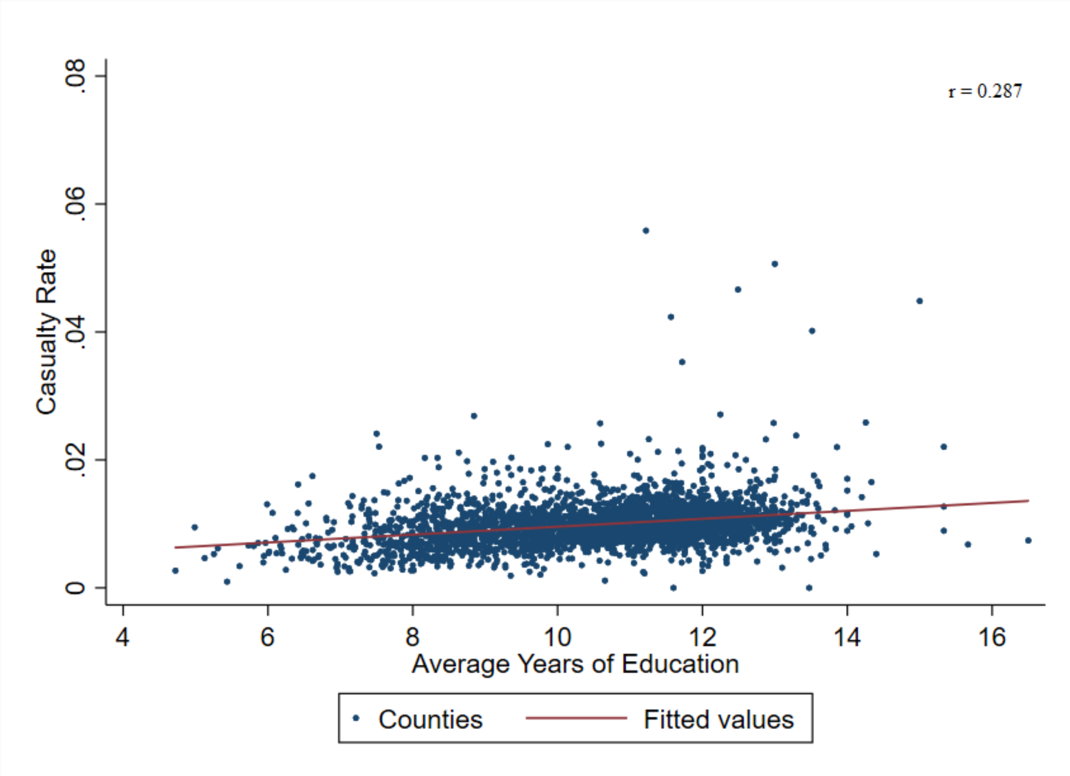


Figure A2: Correlation between casualty rates and average years of education in 1940 for men between 18 and 44 years old per county. *Source*: IPUMS-USA

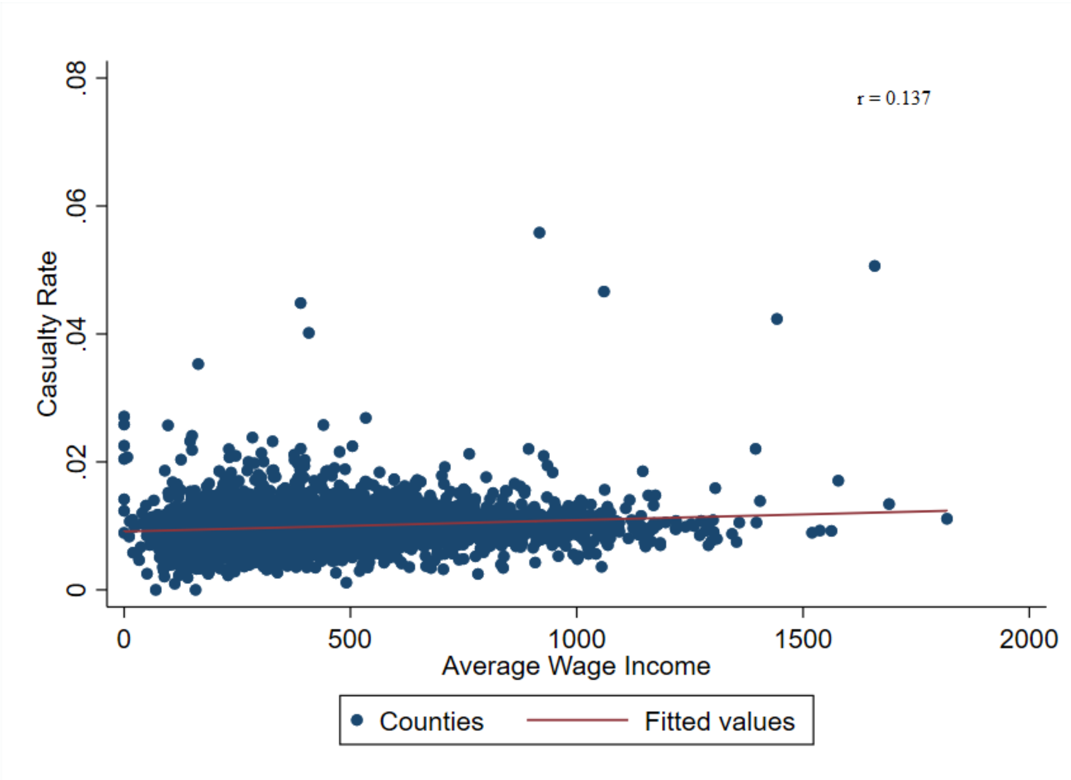


FIGURE A3: Correlation between casualty rates and average wage income in 1940 for men between 18 and 44 years old per county. *Source*: IPUMS-USA

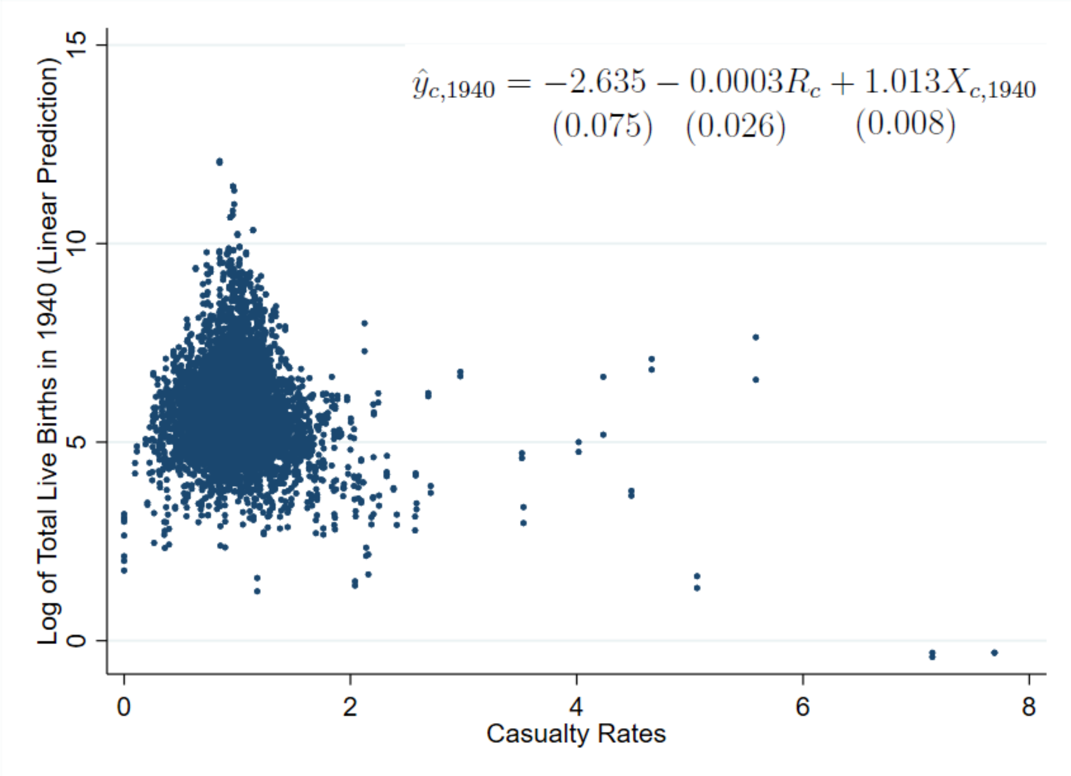


Figure A4: Linear Regression of the natural log of total live births in 1940 on county-level casualty rates. Standard errors are clustered at the county level.

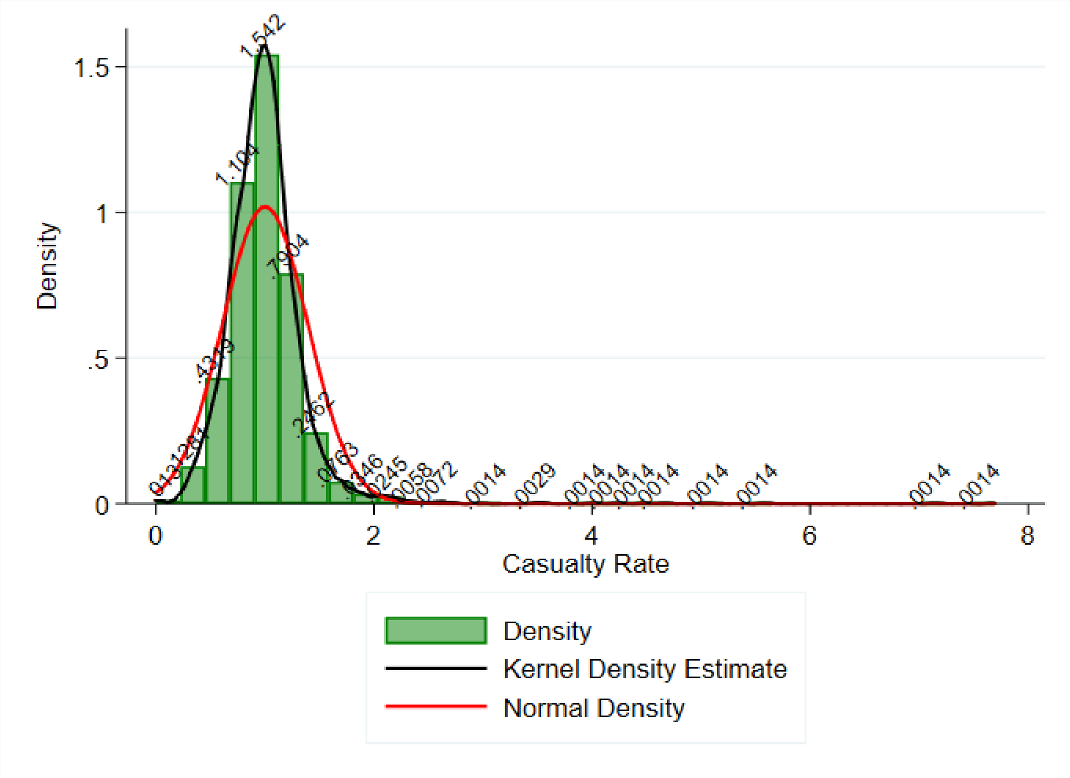


FIGURE A5: County-level casualty rates distribution.

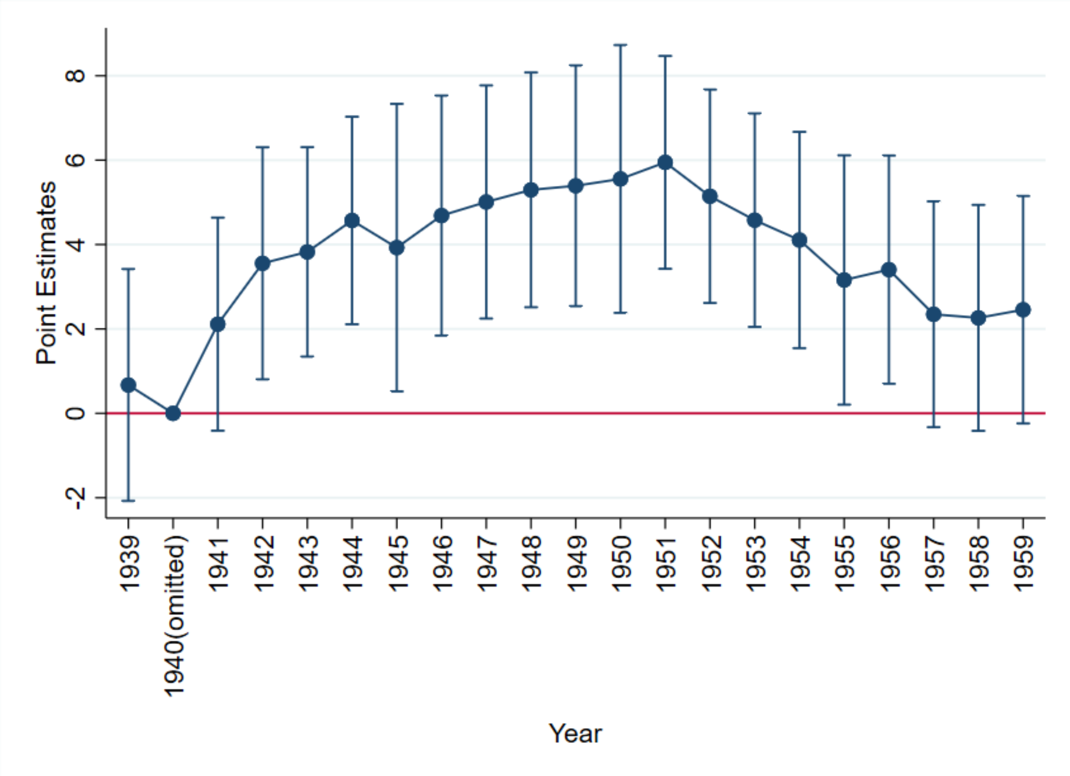


FIGURE A6: This figure plots the estimated coefficients from the continuous regression on the percent of live births attended physicians at hospital computed as the number of live births attended by a physician at the hospital divided by the total number of live births by place of residence multiplied by 100. Data is available at the county level in the United States between 1939 and 1959. The year 1940 is omitted from the regression, i.e., omitted category. *Source*: IPUMS-USA

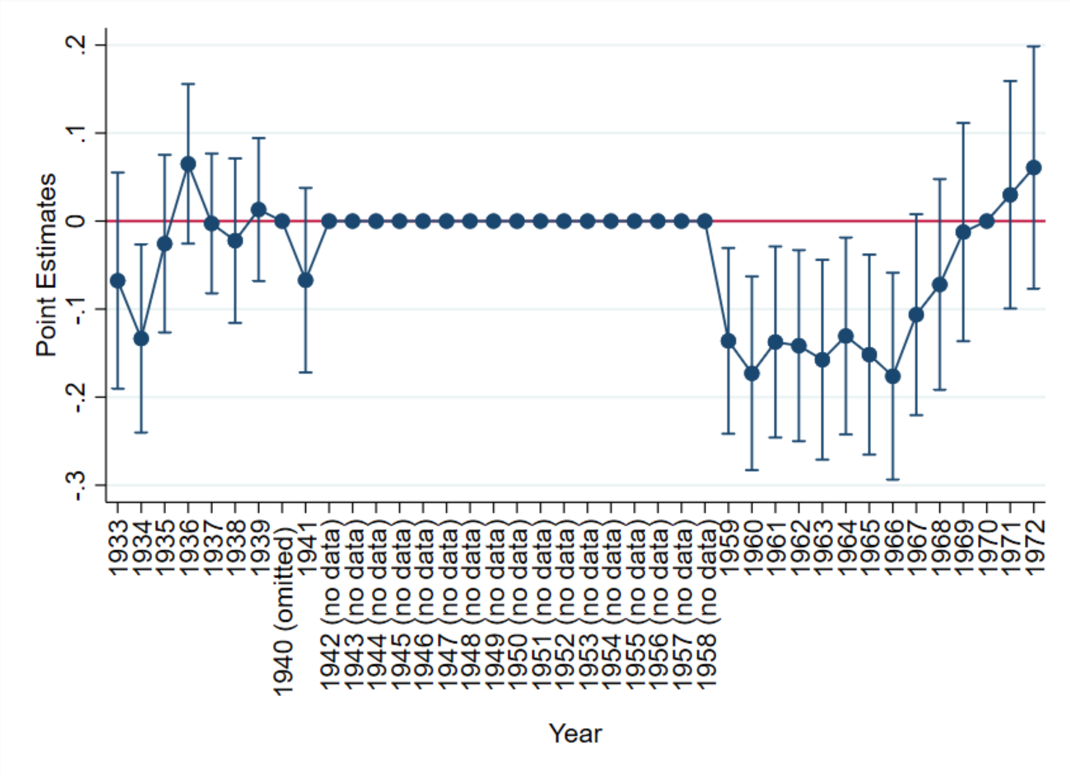


Figure A7: This figure plots the estimated coefficients from the continuous regression on the natural log of infant mortality (under one) by place of occurrence in the United States between 1933 and 1972 with missing data between 1942 and 1958. The year 1940 is omitted from the regression, i.e., omitted category. *Source*:IPUMS-USA

Table A1: Socioeconomic Characteristics in Low and High Casualty Rate SEAs Before and After World War II

	All SEAs		Low Rate SEAs		High Rate SEAs	
	1940	1950	1940	1950	1940	1950
Married	0.216	0.523	0.221	0.526	0.210	0.519
	(0.412)	(0.449)	(0.415)	(0.499)	(0.407)	(0.500)
Age First Birth	23.63	24.30	23.67	24.34	23.57	24.25
	(5.534)	(5.494)	(5.622)	(5.578)	(5.415)	(5.376)
Employed	0.254	0.314	0.272	0.326	0.230	0.295
	(0.435)	(0.464)	(0.445)	(0.469)	(0.421)	(0.456)
Log-Weekly	3.126	3.970	3.119	3.972	3.135	3.966
Individual Wage	(0.632)	(0.522)	(0.651)	(0.531)	(0.603)	(0.507)
Log-Household	8.607	9.194	8.637	9.206	8.565	9.175
Income	(0.865)	(1.002)	(0.870)	(0.998)	(0.856)	(1.009)

Notes: Data are from Census IPUMS 1 percent samples for 1940 and 1950. SEA stands for State Economic Area (Bogue, 1951). The 1950 census did not create SEAs for Alaska and Hawaii, and the analysis excludes those in the District of Columbia. The county components of the 1940 and 1950 SEAs are the same. There are a total of 466 SEAs. We use the casualty rate mean to split the SEAs into two categories. There are 223 SEAs in the "Low Rate SEAs" category and 243 SEAs in the "High Rate SEAs" category. "Married" is a dummy variable that takes the value of one if a woman of childbearing age (between 15 and 44 years old) got married in the last 10 years and zero if she never got married. The age of having a first child is computed as the age of the eldest child subtracted from the age of the mother. The employment status is for women between the age of 15 and 65. "Individual Weekly Income" is for both males and females working for at least 35 hours per week between the age of 15 and 65. For the "Household Income" outcome, we consider individual income for single individuals and the average partners' income in each household for couples. Farmers, self-employed and unpaid family workers are excluded from the sample for weekly individual and household income. Top-coded values are imputed as 1.5 times the censored value for individual and household income. All samples exclude individuals living in group quarters (such as institutions and other group living arrangements). Incomes are deflated by the 1990 CPI. Census person weights are used in all calculations.

Table A2: Test for the Parallel Trend Assumption – Total Births

	Dependent Variable: Log of Total Births	
	Placebo: 1930–1940	
	Binary	Continuous
	(1)	(2)
1940	-0.108	-0.096
	(0.007)	(0.014)
Casualty Rate × 1940	-0.003	-0.013
	(0.009)	(0.013)
Female Population	✓	✓
County FE	✓	✓
# of Counties	2,747	2,747
Observations	5,494	5,494
R-Squared	0.309	0.310

Notes: The dependent variable is the natural log of total births. In the binary regression (column 1), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regression (column 2), "Casualty Rate" is a continuous variable. The dummy variable "1940" takes the value of one in 1940 and zero in 1930. County fixed effects and the natural log of female population are included in all regressions. Alaska, the District of Columbia and Hawaii are not included in the analysis. The number of casualties is not available for Virginia cities, except for Alexandria city which is included in Arlington county. Bronx, Queens, New York, Richmond, and Kings counties are all included in New York City. The sample size is smaller than the main one due to missing observation on total births in 1930. Standard errors are clustered at the county-level.

TABLE A3: Estimates of the Impact of World War II Casualties on Fertility – Extended Controls – Below 80% Farmers Share in 1940

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	-0.049	0.056	-1.295	-1.132	-3.155	-3.014
	(0.149)	(0.159)	(0.247)	(0.260)	(0.340)	(0.348)
Casualty Rate × After War	-0.049	-0.137	-0.008	-0.186	-0.051	-0.181
	(0.027)	(0.050)	(0.045)	(0.087)	(0.058)	(0.109)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Black Population$_{1940}$ × After War	✓	✓	✓	✓	✓	✓
Years of Education$_{1940}$ × After War	✓	✓	✓	✓	✓	✓
Fathers Share$_{1940}$ × After War	✓	✓	✓	✓	✓	✓
Δ FLFP$_{1930-40}$ × After War	✓	✓	✓	✓	✓	✓
Δ Age at Marriage$_{1930-40}$ × After War	✓	✓	✓	✓	✓	✓
Δ Dwelling Ownership$_{1930-40}$ × After war	✓	✓	✓	✓	✓	✓
# of Counties	2,912	2,912	2,910	2,910	2,908	2,908
Observations	5,824	5,824	5,820	5,820	5,816	5,816
R-Squared	0.209	0.211	0.254	0.256	0.353	0.353
Oster δ for β= 0	0.524	0.183	0.034	0.384	-0.279	-0.540

Notes: See notes for Table 1.3. Data on 1940s controls is missing for: Gila (Arizona); Alpine, Mendocino, and Yuba (California); Grand, Gunnison, Hinsdale, and Huerfano (Colorado); Power, Shoshone, and Teton (Idaho); Dawson, Deer Lodge, and Yellow National Park (Montana); Arthur (Nebraska); Elko, Esmeralda, Eureka, and Ormsby (Nevada); Coos and Polk (Oregon), Armstrong (South Dakota); Aransas, Archer, Armstrong, Atascosa, Deaf Smith Delta, Hartley, Haskell, Palo Pinto, Somervell, Starr, and Stephens (Texas); Richmond and Roanoke (Virginia); Grant (Washington); Yellowstone National Park (Wyoming). "Black Population Share" is the number of black men aged between 18 and 44 divided by the total population of men in the same age group. "Farmers Share" is the share of active men between 18 to 44 owning a farm or working on a farm. "Fathers Share" is the share of men between 18 and 44 years old who are married and have at least one child. We also control for county-level lagged changes between census years 1930–1940 in economic or demographic variables that could affect fertility such as Age at First Marriage (for married women only), Dwelling Ownership (share of those who own a dwelling out of total population for both men and women above the age of 16), and Female Labor Force Participation (for women aged between 16 and 64). Moreover, due to the endogeneity of the farmers' share, we drop counties with higher than 80% share of male farmers (aged between 18 and 44) in 1940. A total of 96 counties is dropped. The Oster, 2019 tests in columns 1–6 are each with reference to the baseline specification in Table 1.3 including the female population control and county fixed effects. R_{max} is set to 1.3R following Oster's suggestion (with upper limit equals to one). Standard errors are clustered at the county-level.

TABLE A5: Estimates of the Impact of World War II Casualties on Fertility – Extended Controls and State-Decade FE – Below 80% Farmers Share in 1940

	Dependent Variable: Log of Total Live Births					
	1940–1950		940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.135	0.194	-0.788	-0.712	-2.479	-2.464
	(0.196)	(0.198)	(0.301)	(0.302)	(0.430)	(0.435)
Casualty Rate × After War	-0.040	-0.114	0.017	-0.125	0.006	-0.022
	(0.027)	(0.052)	(0.044)	(0.081)	(0.057)	(0.112)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
State-Decade FE	✓	✓	✓	✓	✓	✓
Black Population$_{1940}$ × After War	✓	✓	✓	✓	✓	✓
Years of Education$_{1940}$ × After War	✓	✓	✓	✓	✓	✓
Fathers Share$_{1940}$ × After War	✓	✓	✓	✓	✓	✓
Δ FLFP$_{1930-40}$ × After War	✓	✓	✓	✓	✓	✓
Δ Age at Marriage$_{1930-40}$ × After War	✓	✓	✓	✓	✓	✓
Δ Dwelling Ownership$_{1930-40}$ × After war	✓	✓	✓	✓	✓	✓
# of Counties	2,912	2,912	2,910	2,910	2,908	2,908
Observations	5,824	5,824	5,820	5,820	5,816	5,816
R-Squared	0.257	0.258	0.317	0.318	0.424	0.424
Oster δ for β= 0	0.344	0.451	-0.175	0.523	-0.520	0.208

Notes: See notes for Table A3. Additionally, all specifications include state times decade fixed effects. The Oster, 2019 tests in columns 1–6 are each with reference to the baseline specification in Table 1.4 including the female population control, county fixed effects, and state-decade fixed effects. R_{max} is set to 1.3R following Oster's suggestion (with upper limit equals to one).

TABLE A4: Estimates of the impact of WWII Casualties and Mobilization at the State-Level

	Census Data				Vital Statistics Data			
	Binary		Cont.		Binary		Cont.	
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Casualties × After War 1940–1960								
Children Under Age of 5	0.022	-0.018	0.085	-0.118				
	(0.020)	(0.024)	(0.062)	(0.071)				
Log of Total Live Births					0.038	-0.029	0.010	-0.122
					(0.036)	(0.039)	(0.086)	(0.101)
Panel B: Mobilization × After War 1940–1960								
Children Under Age of 5	0.025	-0.035	0.652	-0.291				
	(0.020)	(0.016)	(0.318)	(0.254)				
Log of Total Live Births					0.037	-0.010	0.936	-0.322
					(0.042)	(0.032)	(0.491)	(0.445)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Exogenous Controls	✓	✓	✓	✓				
Endogenous Controls	✓	✓	✓	✓				
Female Population					✓	✓	✓	✓
Extended Controls		✓		✓		✓		✓
# of States	48	48	48	48	48	48	48	48
Observations	242,833	242,833	242,833	242,833	96	96	96	96
R-Squared 0.169	0.170	0.169	0.170	0.962	0.983	0.962	0.983	
Oster δ for β= 0		-0.018		-0.017		-0.287		-0.996

Notes: Estimates in columns 1–4 are from separate regressions using pooled micro data from the 1940 and 1960 censuses. Estimates in columns 5–8 are from separate regressions using Vital Statistics data (1940 and 1960). Each outcome variable in panel A is regressed on the World War II casualty rate interacted with a 1960 year indicator variable. Each outcome variable in panel B is regressed on the World War II mobilization rate interacted with a 1960 year indicator variable. The variable "Children Under Age of 5" indicates the number of own children age 4 and under residing with each individual for women aged 25 to 35. Exogenous controls include age, race and ethnicity. Endogenous controls include the education level and marital status. State fixed effects are included for all regressions. Extended Controls are all interacted with the "After War" dummy and include pre-war state socioeconomic characteristics that may have caused differences in casualties: "Black Population Share in 1940" as the number of black men aged between 18 and 44 divided by the total population of men in the same age group, "Fathers Share in 1940" as the share of men between 18 and 44 years old who are married and have at least one child, and "Average Years of Education in 1940" as the state-average years of education for men between 18 and 44 years old. We also control for state-level lagged changes between census years 1930–1940 in economic or demographic variables that could affect fertility such as Age at First Marriage (for married women only), Dwelling Ownership (share of those who own a dwelling out of total population for both men and women above the age of 16), and Female Labor Force Participation (for women aged between 16 and 64). Census samples exclude individuals living in institutional group quarters and include person weights used in all calculations. Alaska, the District of Columbia and Hawaii are not included in the analysis. The Oster, 2019 tests in columns 2, 4, 6, and 8 are each with reference to the baseline specification in columns 1, 3, 5, and 7 (respectively) with only basic controls and state fixed effects. R_{max} is set to 1.3R following Oster's suggestion (with upper limit equals to one). Standard errors in parentheses are clustered at the state-level.

TABLE A6: Estimates of the Impact of World War II Casualties on Counties Above and Below the 50th Percentile of Population Share in 1940

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Above Median						
After War	0.218	0.391	0.209	0.479	-0.203	0.082
	(0.017)	(0.055)	(0.028)	(0.099)	(0.032)	(0.149)
Casualty Rate × After War	-0.046	-0.199	-0.015	-0.282	-0.079	-0.331
	(0.028)	(0.059)	(0.041)	(0.102)	(0.054)	(0.159)
# of Counties	1,535	1,535	1,534	1,534	1,531	1,531
Observations	3,070	3,070	3,068	3,068	3,062	3,062
Panel B: Below Median						
After War	-0.060	-0.014	-0.232	-0.186	-1.075	-1.149
	(0.029)	(0.053)	(0.047)	(0.087)	(0.065)	(0.117)
Casualty Rate × After War	-0.103	-0.094	-0.027	-0.058	0.141	0.140
	(0.040)	(0.047)	(0.066)	(0.079)	(0.089)	(0.102)
# of Counties	1,534	1,534	1,532	1,532	1,531	1,531
Observations	3,068	3,068	3,064	3,064	3,062	3,062
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓

Notes: See notes for Table 1.3. We split the sample into counties above and below the 50th percentile (median) of population share in 1940 in panels A and B, respectively. A county's population share is obtained by dividing its population in 1940 by the population of the state to which it belongs.

Table A7: Impact of World War II Casualties on Fertility: Weighted by County Population

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.266	0.434	0.325	0.544	-0.070	0.135
	(0.017)	(0.046)	(0.034)	(0.083)	(0.024)	(0.071)
Casualty Rate ×	-0.069	-0.202	-0.0684	-0.253	-0.127	-0.266
After War	(0.025)	(0.046)	(0.043)	(0.079)	(0.048)	(0.099)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	3,069	3,069	3,066	3,066	3,062	3,062
Observations	6,138	6,138	6,132	6,132	6,124	6,124
R-Squared	0.431	0.435	0.407	0.410	0.380	0.380

Notes: The dependent variable is the natural log of total live births. In the binary regressions (columns 1, 3 and 5), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regressions (columns 2, 4 and 6), "Casualty Rate" is a continuous variable. "After War" is a dummy that takes the value of one in 1950, 1960 or 1970. County fixed effects and the natural log of female population are included in all regressions. Counties are weighted by their population in 1940 where each county's weight is obtained by dividing its population in 1940 by the population of the state to which it belongs. Alaska, the District of Columbia and Hawaii are not included in the analysis. The number of casualties is not available for most Virginia cities, except for Alexandria city which is included in Arlington county. Bronx, Queens, New York, Richmond, and Kings counties are all included in New York City. The decrease in the number of observations is due to missing data on total live births for some counties in 1950, 1960 and 1970. Moreover, 1970 data on female population is missing for Carson City and Ormbsy in Nevada. Standard errors in parentheses are clustered at the county-level.

Table A8: State versus County Analysis: Weighted by County Population - Relative to US Population

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.312	0.483	0.420	0.714	0.051	0.292
	(0.019)	(0.044)	(0.040)	(0.101)	(0.064)	(0.164)
Casualty Rate × After War	-0.079	-0.210	-0.131	-0.357	-0.178	-0.322
	(0.024)	(0.040)	(0.046)	(0.093)	(0.067)	(0.145)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	3,069	3,069	3,066	3,066	3,062	3,062
Observations	6,138	6,138	6,132	6,132	6,124	6,124
R-Squared	0.533	0.536	0.452	0.455	0.342	0.340

Notes: The dependent variable is the natural log of total live births. In the binary regressions (columns 1, 3 and 5), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regressions (columns 2, 4 and 6), "Casualty Rate" is a continuous variable. "After War" is a dummy that takes the value of one in 1950, 1960 or 1970. County fixed effects and the natural log of female population are included in all regressions. Counties are weighted by their population in 1940 relative to the US population where each county's weight is obtained by dividing its population by the US 1940's population. Alaska, the District of Columbia and Hawaii are not included in the analysis. The number of casualties is not available for Virginia cities, except for Alexandria city which is included in Arlington county. Bronx, Queens, New York, Richmond, and Kings counties are all included in New York City. The decrease in the number of observations is due to missing data on total live births for some counties in 1960 and 1970. Moreover, 1970 data on female population is missing for Carson City and Ormbsy in Nevada. Standard errors in parentheses are clustered at the county-level.

TABLE A9: Robustness Checks: Estimates of the Impact of World War II Casualties on Counties Above and Below the 50th Percentile of Urban Population Share in 1940

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Above Median						
After War	0.299	0.475	0.359	0.581	-0.014	0.201
	(0.013)	(0.064)	(0.020)	(0.094)	(0.026)	(0.138)
Casualty Rate × After War	-0.090	-0.220	-0.078	-0.259	-0.086	-0.256
	(0.024)	(0.066)	(0.033)	(0.096)	(0.041)	(0.140)
# of Counties	1,519	1,519	1,519	1,519	1,518	1,518
Observations	3,038	3,038	3,038	3,038	3,036	3,036
Panel B: Below Median						
After War	-0.112	0.025	-0.357	-0.175	-1.179	-1.135
	(0.031)	(0.064)	(0.051)	(0.099)	(0.065)	(0.128)
Casualty Rate × After War	-0.133	-0.196	-0.087	-0.220	-0.054	-0.068
	(0.042)	(0.060)	(0.068)	(0.092)	(0.092)	(0.120)
# of Counties	1,539	1,539	1,539	1,539	1,539	1,539
Observations	3,078	3,078	3,078	3,078	3,078	3,078
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓

Notes: See notes for Table 1.3. Additionally, we split the sample into counties above and below the 50th percentile (median) of urban population share in 1940.

Table A10: Estimates of the Impact of World War II Casualties on Fertility - Birth Rates

	Dependent Variable: Log of Birth Rates					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.091	0.162	0.04	0.144	-0.348	-0.327
	(0.012)	(0.029)	(0.017)	(0.043)	(0.020)	(0.049)
Casualty Rate ×	-0.064	-0.101	-0.021	-0.106	0.003	-0.020
After War	(0.018)	(0.028)	(0.026)	(0.042)	(0.030)	(0.047)
County FE	✓	✓	✓	✓	✓	✓
# of Counties	3,069	3,069	3,066	3,066	3,062	3,062
Observations	6,138	6,138	6,132	6,132	6,124	6,124
R-Squared	0.018	0.020	0.003	0.005	0.153	0.153

Notes: See notes for Table 1.3. The dependent variable is the natural log of birth rates. County birth rates are computed as the number of total live births divided by the female population aged between 15 and 44, multiplied by 100.

Table A11: Estimates of the Impact of World War II Casualties on Fertility - Controlling for Total Population

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	-0.030	0.093	-0.287	-0.129	-0.871	-0.793
	(0.016)	(0.037)	(0.026)	(0.067)	(0.036)	(0.087)
Casualty Rate ×	-0.085	-0.164	-0.028	-0.171	0.002	-0.077
After War	(0.025)	(0.037)	(0.039)	(0.066)	(0.053)	(0.085)
Total Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	3,069	3,069	3,066	3,066	3,062	3,062
Observations	6,138	6,138	6,132	6,132	6,124	6,124
R-Squared	0.220	0.224	0.279	0.281	0.365	0.365

Notes: See notes for Table 1.3. In addition to the discussed missing observations, data on total population is missing for Adams county in Wisconsin State in 1970. The dependent variable is the natural log of total live births. We control for log of total population.

Table A12: Robustness Checks: Excluding States with More than 20 Percent Black Population

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.058	0.153	-0.017	0.142	-0.603	-0.474
	(0.022)	(0.042)	(0.035)	(0.070)	(0.044)	(0.094)
Casualty Rate × After War	-0.066	-0.122	-0.023	-0.164	-0.027	-0.136
	(0.029)	(0.036)	(0.046)	(0.062)	(0.060)	(0.084)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	2,524	2,524	2,521	2,521	2,517	2,517
Observations	5,048	5,048	5,042	5,042	5,034	5,034
R-Squared	0.200	0.202	0.254	0.257	0.342	0.343

Notes: The dependent variable is the natural log of total live births. In the binary regressions (columns 1, 3 and 5), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regressions (columns 2, 4 and 6), "Casualty Rate" is a continuous variable. "After War" is a dummy that takes the value of one in 1950, 1960 or 1970. County fixed effects and the natural log of female population are included in all regressions. Alaska, the District of Columbia and Hawaii are not included in the analysis. The number of casualties is not available for Virginia cities, except for Alexandria city which is included in Arlington county. Bronx, Queens, New York, Richmond, and Kings counties are all included in New York City. The decrease in the number of observations is due to missing data on total live births for some counties in 1960 and 1970. Moreover, 1970 data on female population is missing for Carson City and Ormbsy in Nevada. Additionally, we drop states with more than 20% black population: Alabama, Delaware, Georgia, Louisiana, Maryland, Mississippi, North Carolina and South Carolina. Standard errors in parentheses are clustered at the county-level.

Table A13: Impact of World War II Casualties on Fertility – Split by Median

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.087	0.215	0.001	0.178	-0.615	-0.509
	(0.017)	(0.041)	(0.028)	(0.068)	(0.037)	(0.088)
Casualty Rate ×	-0.080	-0.166	-0.036	-0.194	0.007	-0.103
After War	(0.025)	(0.040)	(0.040)	(0.067)	(0.054)	(0.087)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	3,069	3,069	3,066	3,066	3,062	3,062
Observations	6,138	6,138	6,132	6,132	6,124	6,124
R-Squared	0.193	0.197	0.246	0.249	0.339	0.339

Notes: See notes for Table 1.3. In the binary regressions (columns 1, 3 and 5), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties based on the casualty rate median instead of the mean.

Table A14: Impact of World War II Casualties on Fertility – Removing top 1% Casualty Counties

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.092	0.263	0.008	0.109	-0.605	-0.604
	(0.016)	(0.054)	(0.027)	(0.077)	(0.036)	(0.101)
Casualty Rate ×	-0.089	-0.216	-0.031	-0.117	0.008	0.003
After War	(0.025)	(0.055)	(0.040)	(0.077)	(0.054)	(0.101)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	3,039	3,039	3,036	3,036	3,032	3,032
Observations	6,078	6,078	6,072	6,072	6,064	6,064
R-Squared	0.186	0.189	0.249	0.250	0.341	0.341

Notes: See notes for Table 1.3. Additionally, we remove the top 1% of casualties counties. A total of 30 counties is dropped from the analysis.

TABLE A15: Robustness Checks: Estimates of the Impact of World War II Casualties - Excluding Counties with High Migration

	Dependent Variable: Log of Total Live Births					
	Bandwidth: -30 to +10 net migrants per 100 individuals					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.113	0.293	0.044	0.254	-0.603	-0.508
	(0.018)	(0.050)	(0.030)	(0.078)	(0.041)	(0.107)
Casualty Rate × After War	-0.115	-0.231	-0.052	-0.229	0.013	-0.088
	(0.027)	(0.049)	(0.044)	(0.077)	(0.059)	(0.105)
Female Population	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
# of Counties	2,598	2,598	2,598	2,598	2,597	2,597
Observations	5,196	5,196	5,196	5,196	5,194	5,194
R-Squared	0.160	0.166	0.198	0.202	0.312	0.312

Notes: See notes for Table 1.3. Additionally, we drop counties whose total net migration rate is higher than 10 or lower than -30 per 100 individuals in the 1950s. Data on migration at the county-level were produced by the Applied Population Lab at the University of Wisconsin-Madison in a study supported by the National Institutes of Health (Winkler et al., 2013). In our sample, the average of the total net migration rate is -8.76 per 100 individuals with a standard deviation of 20.04. We construct the bandwidth of the total net migration in this analysis by adding and subtracting the standard deviation from the mean. We end up excluding 472 counties.

TABLE A16: Demographic Characteristics in Low and High Casualty Rate SEAs

	All SEAs		Low Rate SEAs		High Rate SEAs	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Panel A:						
Casualties	648.9	1,233.4	706.8	1,640.7	595.8	669.4
Men Population *18-44*	67,172	135,710	81,287	183,818	54,219	63,570
Casualty Rates	0.9866	0.1797	0.8445	0.1179	1.1170	0.1166
Panel B:						
Birth Rates $_{1940}$	0.0811	0.015	0.080	0.015	0.082	0.016
Birth Rates $_{1950}$	0.110	0.018	0.110	0.019	0.109	0.017
Birth Rates $_{1960}$	0.119	0.020	0.121	0.021	0.118	0.020
Birth Rates $_{1970}$	0.087	0.018	0.088	0.020	0.087	0.015
Observations	466		223		243	

Notes: Data are from the National Archives monographs and National Historical Geographic Information System. SEA stands for State Economic Area (Bogue, 1951). The 1950 census did not create SEAs for Alaska and Hawaii, and the analysis excludes those in the District of Columbia. The county components of the 1940 and 1950 SEAs are the same. Casualty rate is the number of casualties divided by the number of men aged between 18 and 44 in 1940, multiplied by 100. Birth rates are the number of total live births divided by female population of childbearing age per county. There are a total of 466 SEAs. We use the casualty rate mean to split the SEAs into two categories. There are 223 SEAs in the "Low Rate Counties" category and 243 SEAs in the "High Rate SEAs" category.

Table A17: Estimates of the Impact of World War II Casualties on Fertility at the SEA-Level

	Dependent Variable: Log of Total Live Births					
	1940–1950		1940–1960		1940–1970	
	Binary	Cont.	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
After War	0.315	0.456	0.399	0.523	0.057	0.154
	(0.014)	(0.049)	(0.020)	(0.074)	(0.026)	(0.099)
Casualty Rate ×	-0.043	-0.167	-0.045	-0.149	-0.033	-0.116
After War	(0.019)	(0.050)	(0.027)	(0.074)	(0.033)	(0.098)
Female Population	✓	✓	✓	✓	✓	✓
SEA FE	✓	✓	✓	✓	✓	✓
# of SEAs	466	466	466	466	466	466
Observations	932	932	932	932	932	932
R-Squared	0.892	0.895	0.900	0.900	0.840	0.841

Notes: An SEA stands for State Economic Area (Bogue, 1951). The 1950 census did not create SEAs for Alaska and Hawaii, and the analysis excludes those in the District of Columbia. The county components of the 1940 and 1950 SEAs are the same. In the binary regressions (columns 1, 3 and 5), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regressions (columns 2, 4 and 6), "Casualty Rate" is a continuous variable. SEA fixed effects and log of female population are included in all the regressions. Standard errors in parentheses are clustered at the SEA-level.

Table A18: Impact of World War II Casualties on Different Age Categories - Continuous Regressions

	Females 15 to 24	Females 25 to 34	Females 35 to 44	Females 45 to 54	Females 55 to 65
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Children Under Age of 5	-0.086	-0.100	-0.056	-0.000	
	(0.031)	(0.023)	(0.021)	(0.008)	
N	266,102	254,399	222,323	162,182	
Having a Child	-0.029	-0.048	-0.027		
	(0.015)	(0.012)	(0.017)		
N	266,102	254,399	222,323		
Employed	0.103	0.051	0.040	0.051	0.020
	(0.022)	(0.023)	(0.018)	(0.023)	(0.022)
N	266,102	254,399	222,323	162,182	118,371
Weeks Worked	6.378	7.590	6.893	6.259	2.556
	(0.897)	(1.249)	(0.780)	(0.894)	(1.128)
N	266,102	254,399	222,323	162,182	118,371
Married	0.038	-0.013	-0.052	-0.118	0.041
	(0.026)	(0.028)	(0.088)	(0.108)	(0.081)
N	209,449	92,140	29,189	14,189	10,214
Age at First Birth	0.407	0.308	0.081		
	(0.108)	(0.160)	(0.199)		
N	56,326	174,204	167,977		
Age Gap in Marriage	-0.128	-0.300	-0.003	0.234	-0.042
	(0.481)	(0.430)	(0.623)	(0.649)	(0.667)
N	16,412	37,332	30,674	22,188	15,038
SEA FE	✓	✓	✓	✓	✓
Exogenous Controls	✓	✓	✓	✓	✓
Endogenous Controls	✓	✓	✓	✓	✓

Notes: Each outcome variable is regressed on the World War II casualty rate interacted with a 1950 year indicator variable. “Casualty Rate” is a continuous variable. Exogenous controls include race and ethnicity. Endogenous controls include education and marital status. Standard errors in parentheses are clustered at the SEA-level. See notes of Table 1.6 for more details.

Table A19: Impact of World War II Casualties on Different Age Categories - Binary Regressions

	Females 15 to 24	Females 25 to 34	Females 35 to 44	Females 45 to 54	Females 55 to 65
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Children Under Age of 5	-0.013	-0.013	-0.012	-0.002	
	(0.010)	(0.008)	(0.008)	(0.002)	
N	266,102	254,399	222,323	162,182	
Having a Child	-0.005	-0.010	-0.005		
	(0.006)	(0.005)	(0.005)		
N	266,102	254,399	222,323		
Employed	0.016	0.013	0.008	0.009	-0.004
	(0.006)	(0.008)	(0.005)	(0.007)	(0.007)
N	266,102	254,399	222,323	162,182	118,371
Weeks Worked	1.185	1.702	1.263	1.002	0.156
	(0.509)	(0.494)	(0.306)	(0.362)	(0.352)
N	266,102	254,399	222,323	162,182	118,371
Married	0.001	-0.001	-0.001	0.000	0.026
	(0.010)	(0.009)	(0.026)	(0.034)	(0.023)
N	209,449	92,140	29,189	14,189	10,214
Age at First Birth	0.055	-0.008	0.044		
	(0.042)	(0.062)	(0.066)		
N	56,326	174,204	167,977		
Age Gap in Marriage	0.108	-0.189	-0.109	0.029	-0.028
	(0.152)	(0.110)	(0.151)	(0.174)	(0.166)
N	16,412	37,332	30,674	22,188	15,038
SEA FE	✓	✓	✓	✓	✓
Exogenous Controls	✓	✓	✓	✓	✓

Notes: Each outcome variable is regressed on the World War II casualty rate interacted with a 1950 year indicator variable. "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. Exogenous controls include race and ethnicity. Endogenous controls are not included. Standard errors in parentheses are clustered at the SEA-level. See notes of Table 1.6 for more details.

Table A20: Impact of World War II Casualties on Different Age Categories - Binary Regressions

	Females 15 to 24	Females 25 to 34	Females 35 to 44	Females 45 to 54	Females 55 to 65
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Children Under Age of 5	-0.014	-0.015	-0.012	-0.001	
	(0.010)	(0.008)	(0.008)	(0.002)	
N	266,102	254,399	222,323	162,182	
Having a Child	-0.005	-0.012	-0.006		
	(0.005)	(0.004)	(0.006)		
N	266,102	254,399	222,323		
Employed	0.018	0.015	0.008	0.008	-0.004
	(0.007)	(0.008)	(0.005)	(0.006)	(0.007)
N	266,102	254,399	222,323	162,182	118,371
Weeks Worked	1.258	1.901	1.333	1.008	0.113
	(0.497)	(0.546)	(0.283)	(0.286)	(0.342)
N	266,102	254,399	222,323	162,182	118,371
Married	0.001	-0.001	-0.002	-0.004	0.028
	(0.009)	(0.009)	(0.026)	(0.033)	(0.023)
N	209,449	92,140	29,189	14,189	10,214
Age at First Birth	0.070	0.031	0.066		
	(0.042)	(0.052)	(0.061)		
N	56,326	174,204	167,977		
Age Gap in Marriage	0.093	-0.176	-0.101	0.031	-0.025
	(0.149)	(0.113)	(0.155)	(0.176)	(0.165)
N	16,412	37,332	30,674	22,188	15,038
SEA FE	✓	✓	✓	✓	✓
Exogenous Controls	✓	✓	✓	✓	✓
Endogenous Controls	✓	✓	✓	✓	✓

Notes: Each outcome variable is regressed on the World War II casualty rate interacted with a 1950 year indicator variable. "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. Exogenous controls include race and ethnicity. Endogenous controls include education and marital status. Standard errors in parentheses are clustered at the SEA-level. See notes of Table 1.6 for more details.

Table A21: Impact of World War II Casualties on Sex Ratio – SEA Level

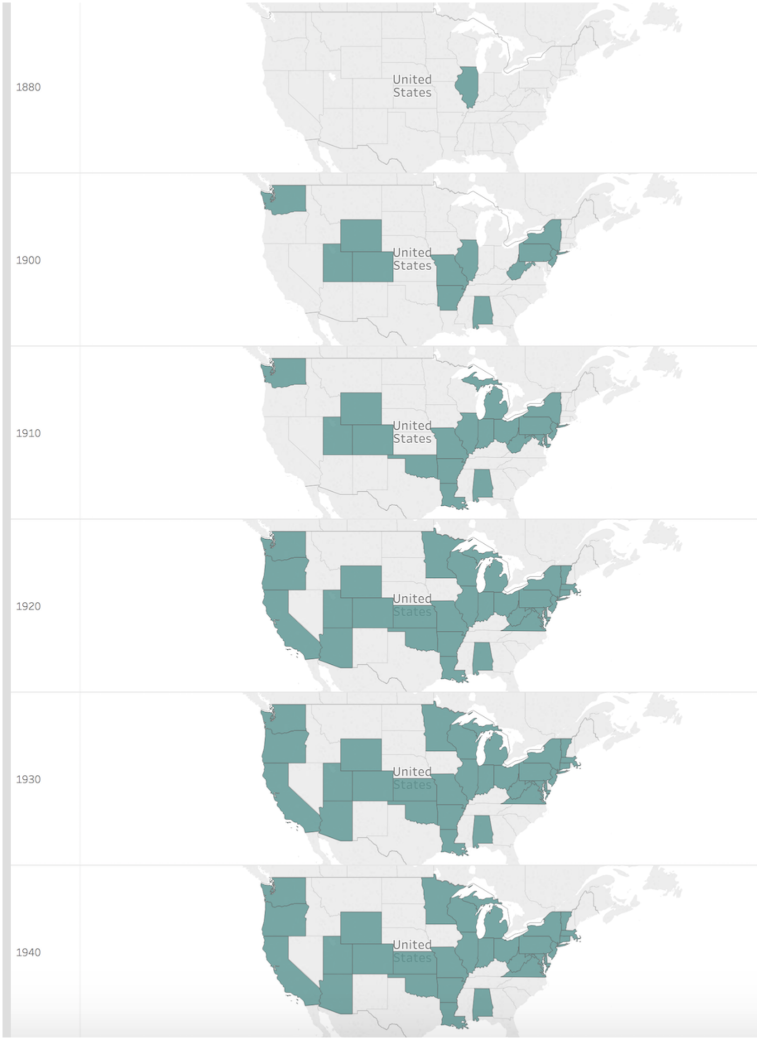
	Dependent Variable: Sex Ratio 1940–1950			
	Individual-Level Data		Full Count Data	
	Binary	Cont.	Binary	Cont.
	(1)	(2)	(3)	(4)
After War	-0.033	0.004	-0.029	-0.003
	(0.005)	(0.020)	(0.004)	(0.016)
Casualty Rate × After War	-0.010	-0.043	-0.009	-0.030
	(0.007)	(0.021)	(0.006)	(0.016)
SEA FE	✓	✓	✓	✓
# of SEAs	466	466	466	466
Observations	932	932	932	932

Notes: The dependent variable is the sex ratio computed at the SEA level. Columns 1 and 2 rely on weighted individual level data for individuals above 16 years old. Columns 3 and 4 rely on the Census full count data for individuals above 16 years old. In the binary regressions (columns 1 and 3), "Casualty Rate" is a dummy that takes the value of one for high casualty rate counties and zero for low casualty rate counties. In the continuous regressions (columns 2 and 4), "Casualty Rate" is a continuous variable. "After War" is a dummy that takes the value of one in 1950. SEA fixed effects are included in all regressions. Standard errors in parentheses are clustered at the SEA-level.

Appendix B

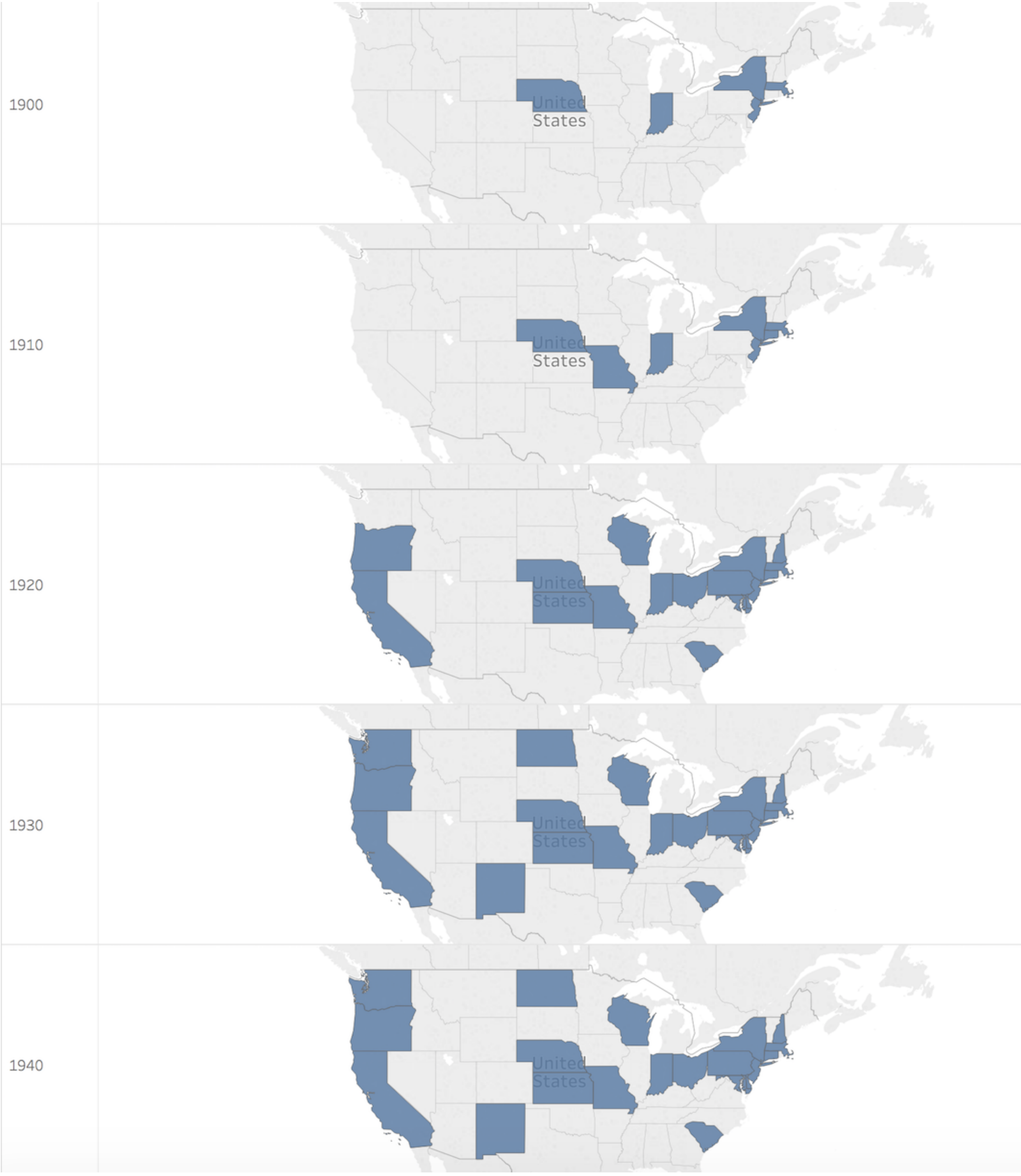
Appendix – Chapter 2

FIGURE A1: Timing of passage of regulatory laws



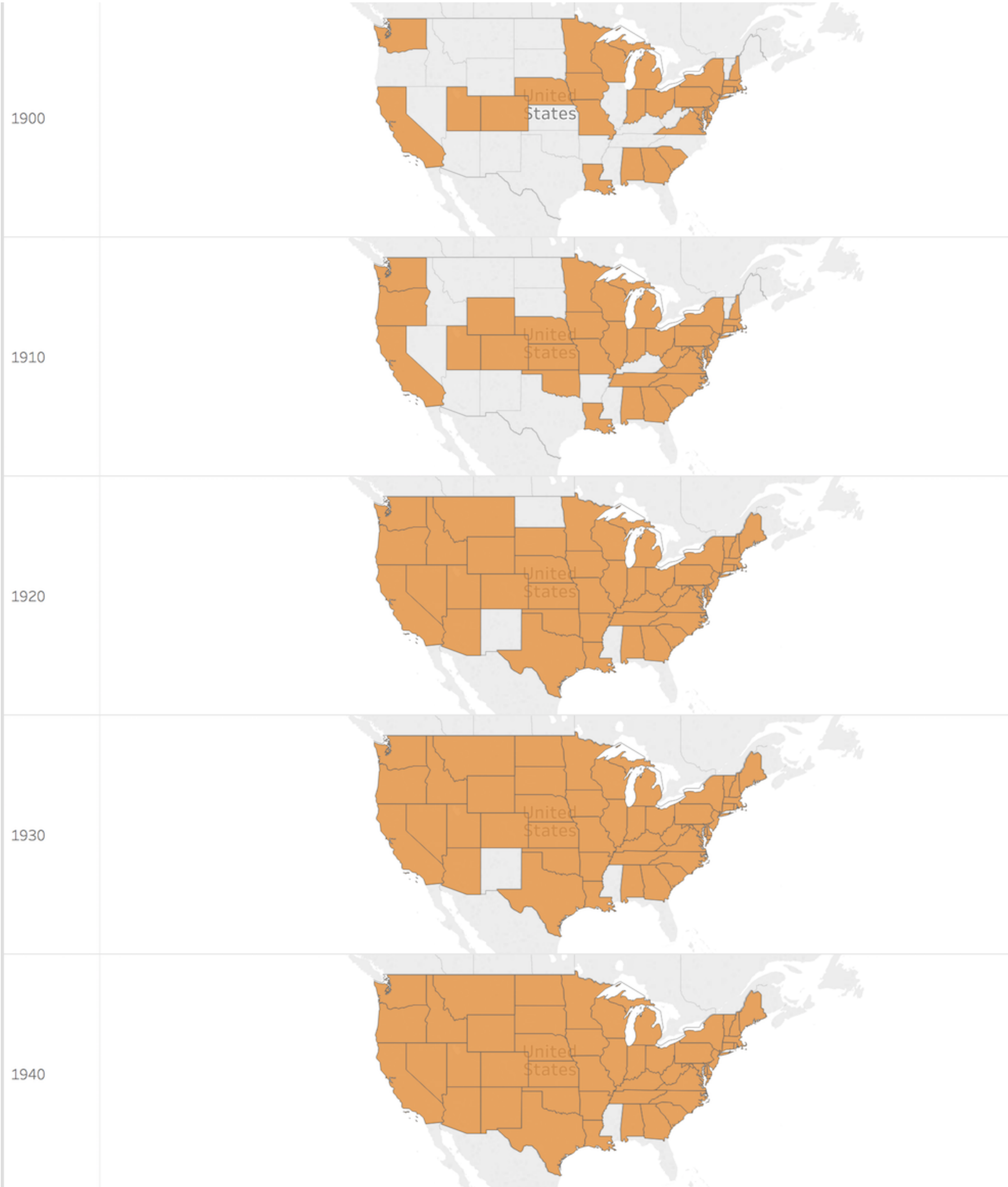
Notes: Chronological timing of regulatory laws' passage across states. Green indicates that a state had a regulatory law. *Source*: Authors' compilation.

Figure A2: Timing of passage of night work laws



Notes: Chronological timing of night work laws' passage across states. Blue indicates that a state had a night work law. *Source*: Authors' compilation.

FIGURE A3: Timing of passage of seating laws



Notes: Chronological timing of seating laws' passage across states. Orange indicates that a state had a seating law. *Source*: Authors' compilation.

TABLE A1: Effects of State Law Policies – Excluding Michigan and Montana

	Seating Laws		Regulatory Laws		Night work Laws	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: DID estimates on $StateLaw_{sd}$						
Women's LFP	-0.002	0.001	0.006	0.008	0.017	0.014
	(0.008)	(0.008)	(0.007)	(0.007)	(0.010)	(0.010)
	[0.806]	[0.904]	[0.409]	[0.269]	[0.157]	[0.202]
N			1,742,091			
Men's LFP	-0.001	-0.000	0.003	0.003	-0.004	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	[0.680]	[0.835]	[0.222]	[0.214]	[0.147]	[0.140]
N			1,744,258			
Panel B: DDD estimates on $StateLaw_{sd} \times Women$						
LFP	-0.001	0.003	0.004	0.005	0.020*	0.017*
	(0.008)	(0.008)	(0.007)	(0.007)	(0.010)	(0.010)
	[0.877]	[0.759]	[0.631]	[0.516]	[0.077]	[0.099]
N			3,486,349			
Seating Law	No	No	Yes	Yes	Yes	Yes
Regulatory Law	Yes	Yes	No	No	Yes	Yes
Night work Law	Yes	Yes	Yes	Yes	No	No
States	47	47	47	47	47	47
Individual Controls	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: See notes of Table 2.2. We exclude the states of Michigan and Montana from the analysis.

TABLE A2: Effects of State Law Policies – With State Trends

	Seating Laws		Regulatory Laws		Night work Laws	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: DID estimates on $StateLaw_{sd}$						
Women's LFP	-0.012*	-0.009	-0.003	-0.001	0.005	0.003
	(0.007)	(0.006)	(0.006)	(0.005)	(0.008)	(0.007)
	[0.099]	[0.160]	[0.590]	[0.821]	[0.564]	[0.686]
N			1,811,237			
Men's LFP	-0.003	-0.002	0.007***	0.008***	0.002	0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
	[0.334]	[0.416]	[0.020]	[0.019]	[0.454]	[0.248]
N			1,818,771			
Panel B: DDD estimates on $StateLaw_{sd} \times Women$						
LFP	-0.002	0.003	0.004	0.005	0.019*	0.016*
	(0.008)	(0.008)	(0.007)	(0.007)	(0.009)	(0.009)
	[0.838]	[0.767]	[0.582]	[0.512]	[0.079]	[0.093]
N			3,630,008			
Seating Law	No	No	Yes	Yes	Yes	Yes
Regulatory Law	Yes	Yes	No	No	Yes	Yes
Night work Law	Yes	Yes	Yes	Yes	No	No
States	49	49	49	49	49	49
Individual Controls	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
State Trends	Yes	Yes	Yes	Yes	Yes	Yes

Notes: See notes of Table 2.2. Additionally, we control for state trends.

Table A3: Effects of State Law Policies on Men's LFP in Affected Industries

	Mercantile	Manufacturing All	Manufacturing Durable Goods	Manufacturing Non-durable	Personal Services	Mining
	(1)	(2)	(3)	(4)	(5)	(6)
Seating Laws	0.001	-0.001	-0.001	0.000	0.000	
	(0.006)	(0.007)	(0.005)	(0.005)	(0.001)	
	[0.810]	[0.874]	[0.774]	[0.966]	[0.817]	
Regulatory Laws		0.0234	0.031	-0.007		0.009
		(0.023)	(0.022)	(0.006)		(0.008)
		[0.446]	[0.377]	[0.272]		[0.244]
Night work Laws	-0.004	-0.025	-0.024	-0.001	0.002	
	(0.004)	(0.021)	(0.020)	(0.005)	(0.001)	
	[0.488]	[0.396]	[0.387]	[0.904]	[0.396]	
Ind. Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1,818,771	1,818,771	1,818,771	1,818,771	1,818,771	1,818,771
Nb of States	49	49	49	49	49	49

Notes: See notes of Table 2.3. The sample here is restricted to men aged between 16 and 65.

Table A4: Effects of State Law Policies on Women's Occupational Income Score within Affected Industries

	Mercantile	Manufacturing All	Manufacturing Durable Goods	Manufacturing Non-durable	Personal Services	Mining
	(1)	(2)	(3)	(4)	(5)	(6)
Seating Laws	0.041	-0.076**	-0.044	-0.081**	0.009	0.017
	(0.034)	(0.035)	(0.042)	(0.036)	(0.008)	(0.015)
	[0.256]	[0.083]	[0.371]	[0.064]	[0.316]	
Regulatory Laws		0.0021	0.021	-0.000		-0.002
		(0.017)	(0.023)	(0.015)		(0.123)
		[0.920]	[0.451]	[0.994]		[0.987]
Night work Laws	0.022	0.004	-0.007	0.006	0.024*	
	(0.024)	(0.016)	(0.023)	(0.014)	(0.014)	
	[0.421]	[0.830]	[0.762]	[0.705]	[0.102]	
Ind. Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
N	60,277	82,316	17,815	64,501	132,145	466
Nb of States	49	49	49	49	49	49

Notes: The sample is restricted to women aged 16–65. Each column shows estimates from a separate weighted regression on women's occupational score (standardized) for different indicated industry. The occupational score variable is a constructed two digit numeric variable that allocates a value representing the median total income (in hundreds of 1950 dollars) to occupations in all years. In each column, we restrict the sample to the indicated industry. We report estimates for our variables of interest *Seating* Law_{sd}, *Regulatory* Law_{sd} and *Night Work* Law_{sd}. Each law is a dummy variable that take the value of one if the state passed a labor law targeting the specific industry. Missing estimates indicate that the policy in question did not target the industry of interest. Individual controls include age, race and ethnicity. Standard errors are clustered at the state level. Wild cluster bootstrap p-values are reported in square brackets. Bootstrap errors are clustered by state (the level at which they are clustered in the cluster-robust variance estimator (CRVE)). State fixed effects and decade fixed effects are included in all regressions. As for the significance of the results: *** p<0.01, ** p<0.05, * p<0.1

Appendix C

Appendix – Chapter 3

Proof of Lemma 1:

The aim of politician i is to increase his share of votes. To achieve this he would want to maximize voters' expected utility. Maximizing voters' expected utility from incumbent i winning is equivalent to minimizing bad publicity on i or $w_{\theta=b}(n)u_i(\theta = b)$.

Politicians have no control over voters' valuation of bad news. Their only strategy is thus to minimize their exposure to it. This could be achieved if he can arrange scandals to appear on periods when there is strong competition from other important stories (i.e., high news pressure).

Since $w_\theta(n)$ is decreasing in n, exposure is minimized when news pressure is high:

$\lim_{n\to 1} w_{\theta=b}(n) = 0$

Politician i would thus want to increase the likelihood of a scandal appearance when $n \to 1$. Precisely:

$\lim_{n\to 1} g_R(n, S_i) = 1$

Which defines a positive relationship between the likelihood of a scandal appearance and news pressure.

Proof of Lemma 2:

The aim of politician j is to decrease the share of votes of politician i. To achieve this he would want to minimize voters' expected utility. Minimizing voters' expected utility from incumbent i winning is equivalent to maximizing bad publicity on i or $w_{\theta=b}(n)u_i(\theta = b)$.

This could be achieved if he can arrange scandals to appear on periods when there is no competing news stories to distract the public (i.e., low news pressure).

Since $w_\theta(n)$ is decreasing in n, exposure is maximized when news pressure is high:

$\lim_{n\to 0} w_{\theta=b}(n) = 1$

Politician j would thus want to increase the likelihood of a scandal appearance when $n \to 0$. Precisely:

$\lim_{n\to 0} g_D(n, S_i) = 1$

Which defines a negative relationship between the likelihood of a scandal appearance and news pressure.

Proof of Proposition 1 and 2:

At the end of period $t = 2$, three possible events are occurring:

1. Politician $i \in R$ exposes S_i and $g(n, S_i) = g_R(n, S_i)$
2. Politician $j \in D$ exposes S_i and $g(n, S_i) = g_D(n, S_i)$
3. Media M exposes S_i and $g(n, S_i) = g_M(n, S_i)$

I assume that these events are independent and define $p_i = g(n, S_i)$ as the probability that at least one of these events happens. The plausible equilibria are thus defined as follow:

1. If the equilibrium is driven by the optimal strategy of politician $i \in R$ in Lemma 1 then $g(n, S_i) = g_R(n, S_i)$ and $\frac{\partial g_R(n,S_i)}{\partial n} > 0$. That is, S_i is more likely to appear on a high news pressure.
2. If the equilibrium is driven by the optimal strategy of politician $j \in D$ in Lemma 2 then $g(n, S_i) = g_D(n, S_i)$ and $\frac{\partial g_D(n,S_i)}{\partial n} < 0$. That is, S_i is more likely to appear on a low news pressure.
3. If the equilibrium is driven by the media's optimal strategy in Lemma 3 then $g(n, S_i) = g_M(n, S_i)$ and $\frac{\partial g_M(n,S_i)}{\partial n} < 0$. That is, S_i is more likely to appear on a low news pressure.

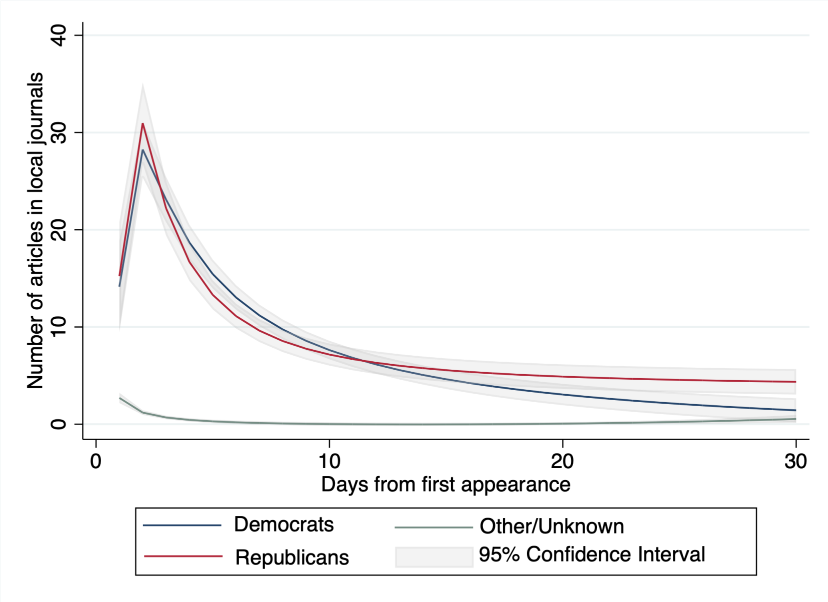


Figure A1: Polynomial Fit of Scandal Coverage by Political Party
Source: newspapers.com

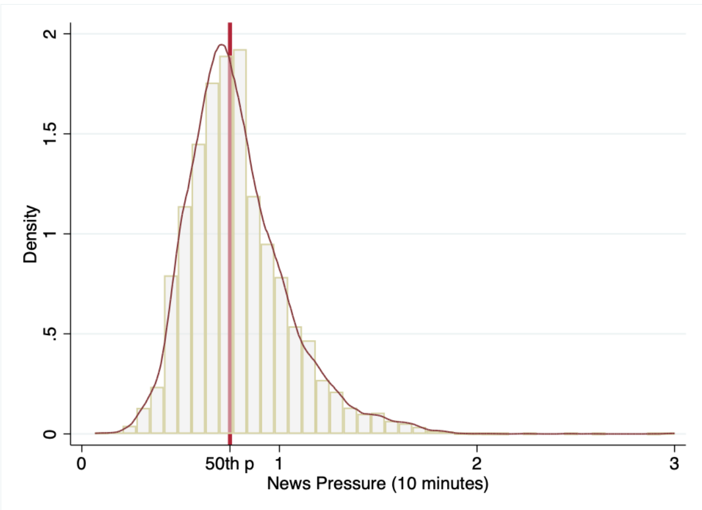


Figure A2: Kernel Distribution of “Daily News Pressure” Measure
Source: Vanderbilt Television News Archives (VTNA)

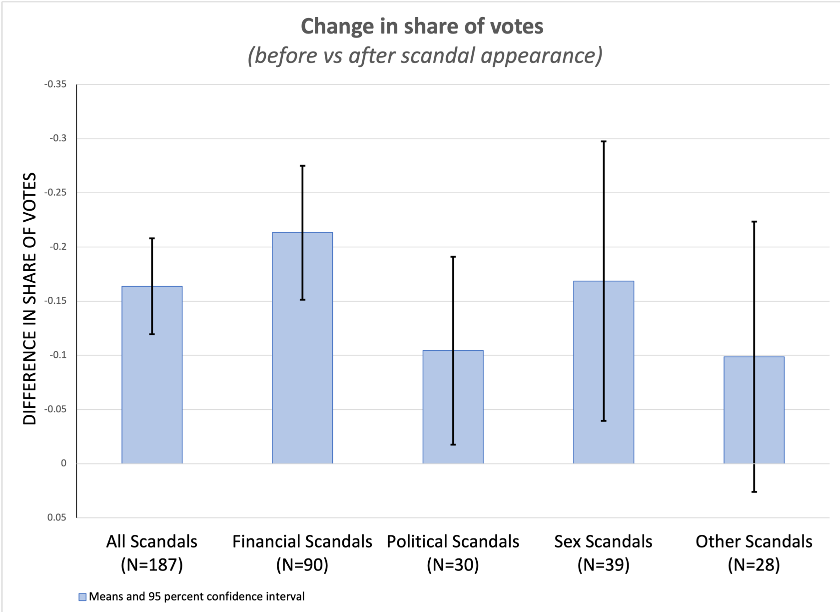


Figure A3: Change in Share of Votes Before vs. After a Scandal Appearance
Source:Ourcampaigns.com

TABLE A1: Descriptive Stats – By Scandal Type

	Sex Scandals	Financial Scandals	Political Scandals	Other Scandals
Panel A: By Political Party				
Democrat	22	68	21	19
Republican	18	25	11	10
Other/Unknown	1	1	2	2
Total	41	94	34	31
Panel B: By Office Type				
Federal legislative	27	38	9	8
State legislative	2	19	7	12
State executive	5	16	7	0
Local government	7	21	11	11
Total	41	94	34	31

Notes: The main sample is limited to politicians who were still in office when the misconduct appeared in the media. See Section 3.3 for details on office and scandals types.

TABLE A2: First Stage – Unpredicted Events

	Weekly News Pressure		
	(1)	(2)	(3)
Unpredicted Events	0.058***	0.060***	0.020**
	(0.010)	(0.010)	(0.009)
N	13,201	13,201	13,201
F-statistics, excluded instruments	30.85	33.65	4.93
R-Squared	0.016	0.033	0.259
Month FE		✓	✓
Year FE			✓

Notes: The table presents first stages of the 2SLS regressions with the weekly news pressure as dependent variables and unpredicted events as regressor. Unpredicted events include human-made and natural disasters. Each column represents a separate regression. The sample includes all weeks within six months of the scandal's first appearance. I report the F-statistic of a joint test on whether all excluded instruments (i.e., human-made and natural disasters) are significantly different from zero. Standard errors are clustered by month × year and reported in parenthesis. * p< 0.10, ** p< 0.05, *** p<0.010.

Table A3: First Stage – Predicted Events

	Weekly News Pressure		
	(1)	(2)	(3)
Predicted Events	0.072***	0.075***	0.026***
	(0.009)	(0.009)	(0.008)
N	13,201	13,201	13,201
F-statistics, excluded instruments	60.08	64.19	10.77
R-Squared	0.040	0.058	0.261
Month FE		✓	✓
Year FE			✓

Notes: The table presents first stages of the 2SLS regressions with the weekly news pressure as dependent variables and predicted events as regressor. Predicted events include military attacks, famous trials, and sports events. Each column represents a separate regression. The sample includes all weeks within six months of the scandal's first appearance. I report the F-statistic of a joint test on whether all excluded instruments (i.e., military attacks and famous trials) are significantly different from zero. Standard errors are clustered by month × year and reported in parenthesis. * p< 0.10, ** p< 0.05, *** p<0.010.

Table A4: Reduced Form – Predicted events

	Outcome: Likelihood of scandals' appearance				
	All Scandals	Sex	Financial	Political	Other
	(1)	(2)	(3)	(4)	(5)
Predicted Events	0.009**	0.002**	0.003	0.002*	0.002
(All Politicians)	(0.004)	(0.001)	(0.004)	(0.001)	(0.001)
N	13,201	13,044	13,097	13,035	13,034
Predicted Events	0.011*	0.001	0.006	0.001	0.003*
(Democrats)	(0.006)	(0.001)	(0.005)	(0.001)	(0.002)
N	8,652	8,544	8,590	8,543	8,541
Predicted Events	0.007	0.005**	-0.003	0.005**	0.0003
(Republicans)	(0.005)	(0.002)	(0.003)	(0.002)	(0.002)
N	4,282	4,236	4,243	4,229	4,228
Predicted Events	0.003	-0.002	0.007	-0.004**	0.003
× Democrats	(0.006)	(0.002)	(0.005)	(0.002)	(0.002)
N	12,934	12,780	12,833	12,772	12,769
Politician Controls	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Notes: The table presents reduced form regressions with the likelihood of a scandals' appearance as dependent variable and predicted events as the main explanatory variable. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks within six months of the scandal's first appearance. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. Politicians' controls include the year of birth, gender, race, office type, and state of office. Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * p< 0.10, ** p< 0.05, *** p<0.010.

Table A5: OLS Regressions

	Likelihood of scandals' appearance				
	(1)	(2)	(3)	(4)	(5)
News Pressure	-0.009	-0.010	-0.010	-0.010	-0.010
(All Politicians)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
N	13,333	13,201	13,201	13,201	13,201
News Pressure	-0.008	-0.010	-0.010	-0.010	-0.011
(Democrats)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
N	8,652	8,652	8,652	8,652	8,652
News Pressure	-0.009	-0.010	-0.010	-0.011	-0.010
(Republicans)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)
N	4,282	4,282	4,282	4,282	4,282
News Pressure	0.0007	0.0004	0.0003	0.0003	0.0009
× Democrats	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)
N	12,934	12,934	12,934	12,934	12,934
Basic Controls		✓	✓	✓	✓
Office Type			✓	✓	✓
Office State				✓	✓
Month FE					✓
Year FE					✓

Notes: The table presents ordinary least square regressions with the likelihood of scandals' appearance as dependent variable and weekly news pressure as the main explanatory variable. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks within six months of the scandal's first appearance. Basic controls include the year of birth, gender, and race. Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * $p< 0.10$, ** $p< 0.05$, *** $p<0.010$.

TABLE A6: 2SLS – Predicted Events – States by Average Political Party Dominance – by Type of Scandal

	Likelihood of scandals' appearance							
	Sex Scandals		Financial Scandals		Political Scandals		Other Scandals	
	Dem.	Rep.	Dem.	Rep.	Dem.	Rep.	Dem.	Rep.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
News Pressure	-0.029	0.049	-0.115	0.429	-0.251	0.105	0.050	0.125*
(Democrats)	(0.037)	(0.072)	(0.194)	(0.291)	(0.162)	(0.068)	(0.173)	(0.070)
N	5,115	3,166	3,177	5,147	3,168	5,111	3,169	5,109
News Pressure	0.249*	0.044	-0.048	-0.162	0.277*	0.076	0.071	-0.035
(Republicans)	(0.143)	(0.067)	(0.163)	(0.132)	(0.150)	(0.076)	(0.109)	(0.0598)
N	1,856	2,380	2,387	1,856	2,380	1,849	2,377	1,851
News Pressure	-0.119	-0.066	-0.058	0.351*	-0.488**	0.009	-0.035	0.112*
× Democrats	(0.102)	(0.073)	(0.199)	(0.189)	(0.202)	(0.054)	(0.167)	(0.060)
N	6,971	5,546	5,564	7,003	5,548	6,960	5,546	6,960
Politician Controls	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the likelihood of scandals' appearance as dependent variable and the weekly news pressure as the main explanatory variable. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks within six months of the scandal's first appearance. Predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In the first and second panel, the sample is restricted to Republican and Democrat politicians, respectively. In the last panel, politicians from both political party are pooled together. "Democrat" is a binary variable that takes the value of 1 if the scandal is on a Democrat politician, and 0 on a Republican politician. Politicians' controls include the year of birth, gender, race, office type, and state of office. In columns 1, 3, 5, and 7 (2, 4, 6, and 8), the sample includes scandals about politicians in states where the average share of votes during the 1972–2020 presidential elections was higher for Democrats (Republicans). Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * p< 0.10, ** p< 0.05, *** p<0.010.

Table A7: 2SLS Regressions – Predicted Events – Continuous Measures

	Log of nb of app. in 7 days					Log of ratio of app. in 7 days				
	All Scandals	Sex	Financial	Political	Other	All Scandals	Sex	Financial	Political	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
News Pressure	1.353*	0.392	0.519	0.240**	0.210	0.542*	0.146	0.182	0.116*	0.102
(All Politicians)	(0.759)	(0.239)	(0.621)	(0.122)	(0.153)	(0.304)	(0.089)	(0.245)	(0.0605)	(0.0709)
N	13,201	13,044	13,097	13,035	13,034	13,201	13,044	13,097	13,035	13,034
News Pressure	1.771	0.180	1.197	0.025	0.356	0.756	0.0546	0.494	0.0211	0.182
(Democrats)	(1.209)	(0.279)	(1.056)	(0.090)	(0.233)	(0.492)	(0.101)	(0.421)	(0.061)	(0.115)
N	8,652	8,544	8,590	8,543	8,541	8,652	8,544	8,590	8,543	8,541
News Pressure	0.938	0.739*	-0.370	0.591**	0.014	0.288	0.283*	-0.245	0.270**	-0.007
(Republicans)	(0.604)	(0.404)	(0.316)	(0.293)	(0.187)	(0.253)	(0.158)	(0.171)	(0.126)	(0.088)
N	4,282	4,236	4,243	4,229	4,228	4,282	4,236	4,243	4,229	4,228
News Pressure	0.496	-0.251	0.807	-0.323*	0.242	0.266	-0.096	0.371*	-0.143*	0.127
× Democrats	(0.650)	(0.277)	(0.522)	(0.165)	(0.166)	(0.273)	(0.108)	(0.222)	(0.076)	(0.084)
N	12,934	12,780	12,833	12,772	12,769	12,934	12,780	12,833	12,772	12,769
Politician Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the weekly news pressure as the main explanatory variable. The outcome variable in columns 1–5 (6–10) is the natural log of the number (ratio) of appearances during the 7 days following the scandal's first appearance. The sample includes all weeks within six months of the scandal's first appearance. Predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In the first and second panel, the sample is restricted to Republican and Democrat politicians, respectively. In the last panel, politicians from both political party are pooled together. "Democrat" is a binary variable that takes the value of 1 if the scandal is on a Democrat politician, and 0 on a Republican politician. Politicians' controls include the year of birth, gender, race, office type, and state of office. Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * p< 0.10, ** p< 0.05, *** p<0.010.

TABLE A8: 2SLS Regressions – Unpredicted Events – Continuous Measures

	Log of nb of app. in 7 days					Log of ratio of app. in 7 days				
	All Scandals	Sex	Financial	Political	Other	All Scandals	Sex	Financial	Political	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
News Pressure	-0.137	-0.096	-0.014	-0.012	-0.011	-0.015	-0.041	0.034	-0.007	-0.001
All Politicians	(0.559)	(0.385)	(0.308)	(0.172)	(0.181)	(0.248)	(0.147)	(0.165)	(0.086)	(0.0902)
N	13,201	13,044	13,097	13,035	13,034	13,201	13,044	13,097	13,035	13,034
Politician Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the weekly news pressure as the main explanatory variable. The outcome variable in columns 1–5 (6–10) is the natural log of the number (ratio) of appearances during the 7 days following the scandal's first appearance. The sample includes all weeks within six months of the scandal's first appearance. Unpredicted events are used to instrument for weekly news pressure. It includes human-made and natural disasters. Politicians' controls include the year of birth, gender, race, office type, and state of office. Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * $p< 0.10$, ** $p< 0.05$, *** $p<0.010$.

TABLE A9: 2SLS – Predicted events – Fixed Effects

	Outcome: Likelihood of scandals' appearance				
	All Scandals	Sex	Financial	Political	Other
	(1)	(2)	(3)	(4)	(5)
News Pressure	0.290*	0.078*	0.091	0.064	0.058
(All Politicians)	(0.164)	(0.046)	(0.126)	(0.043)	(0.045)
N	13,333	13,174	13,227	13,167	13,164
News Pressure	0.384	0.027	0.234	0.019	0.100
(Democrats)	(0.254)	(0.050)	(0.211)	(0.048)	(0.0672)
N	8,652	8,544	8,590	8,543	8,541
News Pressure	0.203	0.141*	-0.103	0.163**	0.0100
(Republicans)	(0.152)	(0.079)	(0.098)	(0.077)	(0.0538)
N	4,282	4,236	4,243	4,229	4,228
News Pressure	0.167	-0.089	0.298	-0.128*	0.078
× Democrats	(0.224)	(0.081)	(0.185)	(0.073)	(0.0688)
N	12,934	12,780	12,833	12,772	12,769
Politician FE	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the likelihood of scandals' appearance as dependent variable and the weekly news pressure as the main explanatory variable. "Likelihood of scandal appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks within six months of the scandal's first appearance. Predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In the first and second panel, the sample is restricted to Republican and Democrat politicians, respectively. In the last panel, politicians from both political party are pooled together. "Democrat" is a binary variable that takes the value of 1 if the scandal is on a Democrat politician, and 0 on a Republican politician. All columns include politicians fixed effects. Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Table A10: 2SLS – Predicted events – Former Politicians

	All Scandals		
	Likelihood of scandals' appearance	Log of nb app. in 7 days	Log of ratio app. in 7 days
	(1)	(2)	(3)
News Pressure	0.123	0.194	0.019
All Politicians	(0.272)	(0.718)	(0.347)
N	2,081	2,081	2,081
News Pressure	0.033	0.139	-0.079
Democrats	(0.395)	(1.085)	(0.518)
N	1,410	1,410	1,410
News Pressure	0.143	0.165	0.034
Republicans	(0.339)	(0.859)	(0.420)
N	536	536	536
News Pressure	0.110	0.045	-0.013
× Democrats	(0.329)	(0.841)	(0.416)
N	1,946	1,946	1,946
Politician Controls	✓	✓	✓
Month FE	✓	✓	✓
Year FE	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions for former politicians. The outcome variable in column 1 is the likelihood of scandals' appearance. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The outcome variable in column 2 (3) is the natural log of the number (ratio) of appearances during the 7 days following the scandal's first appearance. The sample includes all weeks within six months of the scandal's first appearance and is restricted to 33 scandals about politicians who were no longer in office when the scandal first appeared. Predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In the first and second panel, the sample is restricted to Republican and Democrat politicians, respectively. In the last panel, politicians from both political party are pooled together. "Democrat" is a binary variable that takes the value of 1 if the scandal is on a Democrat politician, and 0 on a Republican politician. Politicians' controls include the year of birth, gender, race, office type, and state of office. Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * $p< 0.10$, ** $p< 0.05$, *** $p<0.010$.

Table A11: 2SLS – All Politicians – Full Sample

	Outcome: Likelihood of scandals' appearance				
	All Scandals	Sex	Financial	Political	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Predicted Events as Instrument					
News Pressure	0.006*	0.002*	0.002	0.002*	0.001
All Politicians	(0.003)	(0.001)	(0.003)	(0.001)	(0.001)
N	628,650	628,493	628,546	628,484	628,483
Panel B: Unpredicted Events as Instrument					
News Pressure	0.000	-0.001	0.000	0.000	0.001
All Politicians	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
N	628,650	628,493	628,546	628,484	628,483
Politician Controls	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the likelihood of scandals' appearance as dependent variable and the weekly news pressure as the main explanatory variable. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks between September, 1969 and December, 2019. In panel A, predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In panel B, unpredicted events are used to instrument for weekly news pressure. It includes human-made and natural disasters. Politicians' controls include the year of birth, gender, race, office type, and state of office. Each estimate represents a separate regression. Standard errors are clustered by month × year and reported in parenthesis. * $p< 0.10$, ** $p< 0.05$, *** $p<0.010$.

Table A12: 2SLS – Predicted Events – Election Periods

	Outcome: Likelihood of scandals' appearance				
	All Scandals	Sex	Financial	Political	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Predicted Events as Instrument					
News Pressure	0.354*	0.076	0.084	0.100*	0.096*
All Politicians	(0.202)	(0.051)	(0.143)	(0.059)	(0.055)
N	12,126	11,982	12,031	11,971	11,968
Politician Controls	✓	✓	✓	✓	✓
Quarters FE	✓	✓	✓	✓	✓
Panel B: Predicted Events as Instrument					
News Pressure	0.393*	0.074	0.161	0.082	0.079
All Politicians	(0.212)	(0.053)	(0.164)	(0.052)	(0.050)
N	11,036	10,905	10,950	10,897	10,894
Politician Controls	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Notes: The table presents second stages of the 2SLS regressions with the likelihood of scandals' appearance as dependent variable and the weekly news pressure as the main explanatory variable. "Likelihood of scandals' appearance" is a binary variable that takes the value of 1 on the week the scandal appeared for the first time, and 0 otherwise. The sample includes all weeks within six months of the scandal's first appearance. Predicted events are used to instrument for weekly news pressure. "Predicted events" is a binary variable that takes the value of 1 if a predicted event appeared on a specific week, and 0 otherwise. It includes military attacks and famous trials. In panel A, I replace month and year fixed effects by quarters fixed effects. Standard errors are also clustered at the quarter level. In panel B, the sample excludes 38 politicians whose scandals appeared in the pre-elections period. Politicians' controls include the year of birth, gender, race, office type, and state of office. Each estimate represents a separate regression. Standard errors in panel B are clustered by month $\times$ year and reported in parenthesis. * $p< 0.10$, ** $p< 0.05$, *** $p<0.010$.

Table A13: Descriptive Stats – Scandal appearance on likelihood of Exit & Loss

	Probability of Exit			Probability of Loss		
	Mean	SD	N	Mean	SD	N
All Scandals	0.49	0.50	200	0.45	0.45	103
Financial Scandals	0.48	0.50	94	0.53	0.50	49
Political Scandals	0.44	0.50	34	0.32	0.48	19
Sex Scandals	0.66	0.48	41	0.50	0.52	14
Other Scandals	0.32	0.48	31	0.33	0.48	21

Notes: The table presents descriptive statistics on the probability of exit (columns 1–3) and the probability of loss (columns 4–6) for politicians in the sample. Probability of loss is restricted to politicians who did not exit politics (i.e., resigned or died) after a scandal appearance.

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