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Long-run intergenerational health benefits of women empowerment: Evidence from suffrage movements in the US

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Abstract
An ongoing body of research documents that women empowerment is associated with improved outcomes for children. However, little is known about the long-run effects on health outcomes. This paper adds to this literature and studies the association between maternal exposure to suffrage reforms and children's old-age longevity. We utilize changes in suffrage laws across US states and over time as a source of incentivizing maternal investment in children's health and education. Using the universe of death records in the US over the years 1979–2020 and implementing a difference-in-difference econometric framework, we find that cohorts exposed to suffrage throughout their childhood live 0.6 years longer than unexposed cohorts. Furthermore, we show that these effects are not driven by preexisting trends in longevity, endogenous migration, selective fertility, and changes in the demographic composition of the sample. Additional analysis reveals that improvements in education and income are candidate mechanisms. Moreover, we find substantial improvements in early-adulthood socioeconomic standing, height, and height-for-age outcomes due to childhood exposure to suffrage movements. A series of state-level analyses suggest reductions in infant and child mortality following suffrage law change. We also find evidence that counties in states that passed the law experienced new openings of County Health Departments and increases in physicians per capita.

KEYWORDS
health, historical data, mortality, parental investment, suffrage, women empowerment

JEL CLASSIFICATION
H75, I18, J16, K38, N31, N32

1 | INTRODUCTION

Interest continues to grow in the positive effects of women empowerment on children's outcomes (Bandiera et al., 2020; Duflo, 2012; Homan, 2017; Kose et al., 2021; Nobles et al., 2010). Much of this work has been dedicated to examining the idea that women are generally more pro-social and prefer higher levels of investment in their children (Alesina & Giuliano, 2011; Araújo et al., 2017; Ashok et al., 2015; Simmons & Emanuele, 2007). On the other end, a growing body of research evaluates the association between early life and childhood conditions with later-life outcomes. Specifically, studies have shown a link between childhood parental investment and old-age longevity and mortality outcomes (Almond et al., 2018; Almond & Currie, 2011; Currie, 2009; Gagnon & Bohnert, 2012; Hayward & Gorman, 2004; Ko & Yeung, 2019; Lazuka, 2019; Lee & Ryff, 2019; Montez & Hayward, 2011; Steptoe & Zaninotto, 2020; van den Berg et al., 2011).

However, despite this voluminous empirical work, research has not yet addressed the link between women empowerment and long-term outcomes of children, such as their old-age health. Shedding light on this link could have important implications for the unintended and unexplored effects of women empowerment. The externalities of women empowerment are detected in factors related to human capital and health capital, including the availability of clean water (Chattopadhyay & Duflo, 2004), infant mortality (Homan, 2017), public school expenditures (Carruthers & Wanamaker, 2015), and maternal mortality (Bhalotra et al., 2017). Since the inequalities often stem from structural laws and regulations, the logical first step in empowering women and reducing gender inequalities is to aim at these laws and systematically alter them. To inspire this first step, we must begin by portraying the outcomes of such law changes. Therefore, our overlooking the influence of the suffrage laws, as a mechanism of women empowerment, on the long-term outcomes of children is an oversight in need of correction.

Therefore, this paper aims to address this gap in the literature by investigating the association between maternal exposure to suffrage laws and children's old-age mortality in the US. Specifically, we ask whether children whose mothers gained the right to vote through suffrage reforms live longer lives compared to women in states without a reform. In other words, are children of politically empowered women better-off in the long-run and enjoy higher longevity? The central thesis of this paper is that the positive implications of empowering women through granting them the right to vote go beyond their own generation. When women are empowered, they are more motivated and enabled to provide their children with better health and educational environments. This improved health and education of children, in turn, improves their longevity.

We provide empirical evidence for our central assertion by searching for longevity effects utilizing the universe of death records in the US between 1979 and 2020 for cohorts born between 1880 and 1940 and who died at ages 39–122. We complement this analysis by looking at mortality outcomes using the National Longitudinal Mortality Study (NLMS) data. We employ a difference-in-difference methodology and find that compared with those who were not exposed to suffrage, those who were exposed to suffrage during childhood benefit from 0.56 additional life years. The results are robust across different specifications, for example, in models that include state-of-birth-by-cohort linear trend or a series of interactions between state-of-birth-by-gender and state-of-birth-by-race dummies. Furthermore, the NLMS analysis suggests that a one-unit change in the share of exposure is associated with an approximately 5 percentage-points decrease in the probability of death, about an 18% reduction from the mean. In addition, we provide evidence for the potential role of improved education and income as the mechanism linking exposure to suffrage to old-age mortality by establishing a significant positive relationship between exposure to suffrage and years of schooling, probability of having any college education, and total family income. Finally, we employ data from World War II enlistment linked to Social Security death records and the full-count 1940-census to search for effects on other health and economic outcomes. We find that children with higher exposure to suffrage laws during childhood are more likely to reside in a neighborhood with a higher socioeconomic index. They have roughly 4% higher occupational income scores in early adulthood. Furthermore, we find small but significant increases in their heigh-for-age, a predictor of overall health and later-life outcomes. A series of state-level analyses suggest reductions in infant and child mortality following suffrage law change. We also find evidence that counties in states that passed the law experienced new openings of County Health Departments and increases in physicians per capita.

This paper makes three important contributions to the literature. First, to our knowledge, this is the first study to explore the effects of the suffrage movements on children's old-age mortality. Specifically, we explore the effects on a key summary measure of old-age health—longevity. Although mortality and longevity are extreme proxies for health, they are more precise measures of well-being than subjective health measures. Moreover, longevity reflects cumulative life-cycle exposures. A large body of literature provides evidence of a strong link between longevity and other economic and health outcomes (Aizer et al., 2016; Buchman et al., 2012; Chetty et al., 2016; Halpern-Manners et al., 2020; Kinge et al., 2019; Lubitz et al., 2003; Sunder, 2005). Second, this study adds to the research on intergenerational aspects of women empowerment, an understudied and overlooked aspect of this literature. In addition, this study adds to the long-run health effects of the childhood environment by providing suggestive evidence of the effectiveness of parental investment on their children's old-age mortality outcomes. Third, this paper also adds to the growing literature that evaluates the relevance of early-life and childhood experiences on old-age health and mortality (Almond et al., 2018; Hayward & Gorman, 2004).

The rest of the paper is organized as follows. Section 2 offers a background review of the suffrage movement and the relevant literature. Section 3 introduces data sources and discusses the sample selection strategy. Section 4 discusses the econometric method. Section 5 reviews the results of the paper. Section 6 investigates potential channels of impact. Finally, we conclude the paper in Section 7.

2 | BACKGROUND

2.1 | Background on suffrage movement in the US

Historical roots of the suffrage movement in the US go back to the first wave of women's voluntary organizations. These voluntary organizations originated in the nineteenth century as a consequence of the rise in industrialization (Flexner & Fitzpatrick, 1996). With men working more and more outside of the home and women becoming the sole person in charge of home responsibilities, women took their responsibilities at home one step forward and expanded the meaning of "home" to "community" (Dorr, 1910). They established several women-led voluntary organizations to advocate "municipal housekeeping," that is, that they are promoters of welfare, health, and hygiene of not only their own house but also the whole community (Dorr, 1910). These voluntary organizations later provided both the ideological foundation and infrastructural means for the women's suffrage movement. The movement officially started with the first women's rights convention (also known as the Seneca Falls Convention) in New York in July 1848 (Skocpol, 1995).

Several voluntary organizations dedicated to the right to vote emerged after the women's rights convention, including the National Woman Suffrage Association, the American Woman Suffrage Association, and the National League of Colored Women. Although the movement saw some early victories in Wyoming and Utah (in 1869 and 1870, respectively), it wasn't until they coordinated their efforts by merging the most important suffrage associations in the 1890s and underlined their municipal housekeeping philosophy that they saw more widespread state-level victories (King et al., 2005). As shown in Figure 1, the movement realized success in several states in the early 1900s. The final catalyst for the movement came with the US entrance into World War I in 1917, as women played an essential role by volunteering as nurses and filling the jobs of men who were deployed to the war (Flexner & Fitzpatrick, 1996). Realizing this essential role, the 19th Amendment, giving women in the US the right to vote, was proposed to Congress in 1918 by President Wilson, and it was ratified in 1920. In this paper, we use the variation in the timing of state-level suffrage law passages, as depicted in Figure 1, to explore the relationship between exposure to maternal suffrage laws during one's childhood and old-age mortality outcomes.

2.2 | Literature review

Women empowerment refers to "the expansion of people's ability to make strategic life choices in a context where this ability was previously denied to them" (Kabeer, 1999). This empowerment can take many forms, such as participation in household decision-making, women's ability to visit important places in their community, women's status (commonly measured as

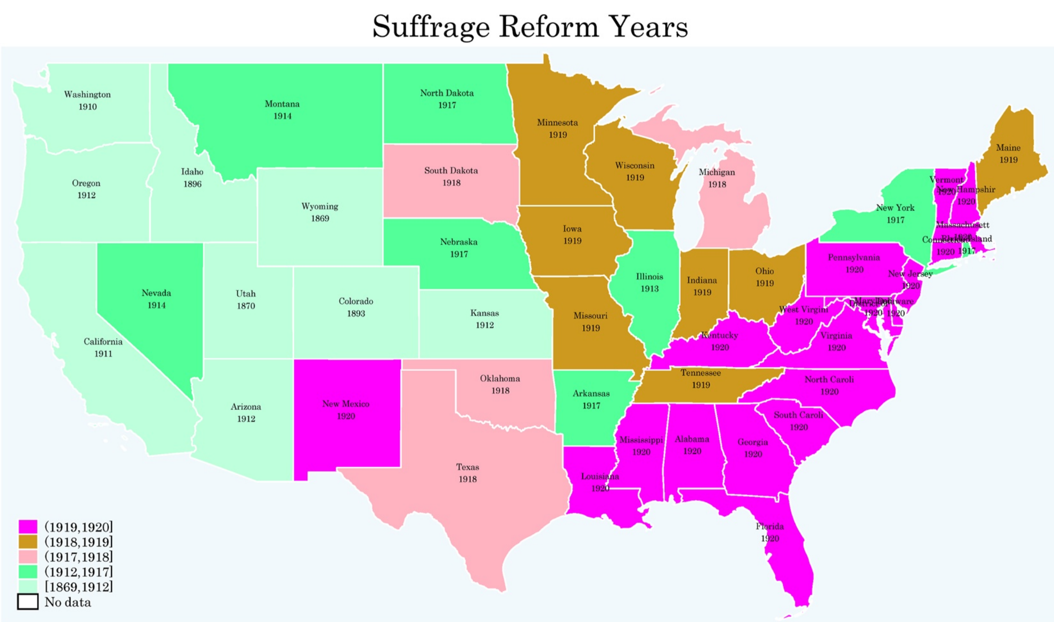

FIGURE 1 Suffrage reform years across states. [Colour figure can be viewed at wileyonlinelibrary.com]

educational level and employment), rights in marriage, control by partner, financial autonomy (Upadhyay et al., 2014), and political empowerment (e.g., the right to vote and parliament representation (Kabeer, 1999)). There is strong evidence establishing beneficial outcomes of women empowerment from the household to the societal level. Drawing on the argument that women are more invested in enhancing public good, are more pro-social, prioritize child health, and favor higher investment in their children (Doepke et al., 2012; Duflo, 2012), scholars have shown significant effects of women empowerment on maternal and child health outcomes (see Pratley (2016) for a review). The documented positive influences of women empowerment include reductions in infant mortality and child mortality (Eswaran, 2002; Hossain, 2015), reductions in fertility rates (Eswaran, 2002; Upadhyay et al., 2014), increases in educational investments in children (Doepke & Tertilt, 2009), increases in occupational mobility of children (Asiedu et al., 2021), better nutritional status of children (measured as height-for-age, weight-for-age, and weight-for-height (Imai et al., 2014)), higher childhood vaccination rates (Wado et al., 2014), longer birth intervals (Upadhyay & Hindin, 2005), and lower rates of unintended pregnancy (see Upadhyay et al. (2014) for a review of reproductive outcomes of women empowerment). For example, Hossain (2015) shows that women's educational level and participation in household decision-making were significantly related to lower levels of infant mortality in Bangladesh.

Political empowerment and the enfranchisement of women have also been linked to several short-run and long-run outcomes, such as infant and child mortality (Bhalotra & Clots-Figueras, 2014; Homan, 2017; Quamruzzaman & Lange, 2016; Swiss et al., 2012), crime and incarceration (Noghanibehambari et al., 2023), public school expenditures (Carruthers & Wanamaker, 2015), measles and DPT immunization (Swiss et al., 2012), usage of prenatal care (Bhalotra et al., 2017), and lifespan of female offspring (Nobles et al., 2010). For instance, Miller (2008) shows that the passage of suffrage laws in the US had a significant relationship with an increase in public health spending, which resulted in a reduction in child mortality due to hygiene-related infectious diseases. The author argues that immediately after the passage of state-level suffrage laws and the 19th Amendment, local and national legislators voted in favor of public health appropriations that were lobbied and campaigned by women. This sudden change was an a-prior reaction to the fear that women won't vote for them later on if they didn't pass women-favored legislations. These public health legislations, and the accompanying increase in public health spending, brought about a significant increase in door-to-door hygiene campaigns. These improvements resulted in a large reduction in child mortality rates (an estimated drop of roughly 20,000 counts annually) caused by hygiene-related infectious diseases.

Suppose maternal exposure to suffrage generates an incentive for increasing investment in infants and children and improvements in early life conditions. In that case, one expects to observe positive gains in old-age health outcomes.[1] The link between early-life parental investment and old-age mortality could operate through several channels. First, mothers may contribute to the initial health endowments of their children by improving the prenatal development environment. In so doing, they may increase material inputs such as better nutrition, increase the utilization of health inputs such as prenatal care, and change their health behavior such as avoiding drinking and smoking. These channels are linked to improved birth outcomes (Abrevaya, 2006; Almond et al., 2011; Cil, 2017; Hoynes et al., 2015; Sonchak, 2015). Several studies document the association between birth outcomes and later-life education and earnings (Almond et al., 2005; Behrman & Rosenzweig, 2004; Black et al., 2007; Maruyama & Heinesen, 2020; Royer, 2009). In postnatal ages, mothers may invest in their offspring's human capital by allocating more time and resources toward their education. For instance, Kose et al. (2021) show that children whose mothers were exposed to suffrage reforms have higher educational attainments during adulthood. Carruthers and Wanamaker (2015) document that up to one-third of the rise in public school spending over the years 1920–1940 can be explained by the 19th Amendment, which enforced suffrage reform to states that had not yet established one. The increases in public education quality could boost overall educational attainments in the short run and improve long-run health outcomes.[2]

Drawing on these studies, one could expect long-term health benefits from political empowerment of women. Suffrage laws may empower women by providing them with incentives for investing more in their children, leading to improved health, developmental outcomes, and education, which could have spillover effects on their lifelong health and be reflected in their longevity in old age. However, this aspect of the theory has gained very little empirical attention. Our study departs from the previous literature by exploring this long-term intergenerational link.

3 | DATA SOURCES

The primary data source is state-identified restricted-access multiple-cause of death data extracted from National Center for Health Statistics (2020) (henceforth NCHS data). The NCHS data reports the universe of death records that occur in the US. Since 1979, death records contain the state-of-birth of an individual, a necessary variable to infer childhood state-level policy exposure. Hence, we use death records data from 1979 to 2020. The data also contains limited individual characteristics, including race, gender, and age at death. We merge the NCHS data with the database of state-level timing of suffrage laws extracted

TABLE 1 Summary statistics.

Variable	Mean	Std. Dev.	Min	Max
Death age	79.19	10.83	39	122
Birth year	1919.33	11.98	1880	1940
Death year	1998.52	11.66	1979	2020
Female	0.52	0.49	0	1
White	0.89	0.30	0	1
Black	0.10	0.30	0	1
Share exposed	0.76	0.34	0	1
Birth state-year characteristics:				
Number of children less than 5 years old	0.44	0.11	0.21	0.83
Share of white-collar workers	0.03	0.009	0.02	0.08
Share of farmers	0.22	0.15	0.01	0.89
Share of other occupations	0.73	0.14	0.08	0.94
Share of literate female	0.65	0.18	0	0.83
Female labor force participation	0.22	0.08	0	0.51
Average socioeconomic index	25.340	4.67	13.58	34.28
Observations	64,845,601			

from Kose et al. (2021). We restrict the sample to post-1880 cohorts to remove those above-100 years old individuals in the control group as their outcomes arguably follow a different path than the treated later-born cohorts. The sample is also restricted to pre-1940 cohorts so as to remove the potential confounding influence of health trends in later cohorts. In addition, we remove individuals born in Wyoming and Utah as the implemented suffrage laws occur years before the first cohorts appear in the data.[3]

The geographic distribution of state-level women's suffrage implementation is depicted in Figure 1. Summary statistics of the final sample are reported in Table 1. The final sample includes roughly 64.85 million observations and covers the death years of 1979–2020 for cohorts born between 1880 and 1940 and who died at ages 39–122. The average childhood exposure to suffrage across individuals is 77%. The average age at death is 79.2 years. Figure 2 shows the density distribution of age at death in the NCHS data over the sample period.

To complement the analysis of longevity, we also use an alternative data source to explore the effects on mortality. We use version-11 of the public-use National Longitudinal Mortality Study extracted from the US Census Bureau (henceforth NLMS). The NLMS is a nationally representative random sample of the non-institutionalized population. Version-11 of NLMS was conducted in 1983 and is linked to the Annual Social and Economic Supplement of the Current Population Survey as well as to death records from National Center for Health Statistics. The advantage of NLMS over the NCHS data is that for a certain (random) sample of cohorts, we observe those who survived until 1983 (from the initial interview in 1973) and those who died, in addition to all demographic and location information necessary for the analysis. This fact allows us to look at the probability of death as opposed to longevity in NCHS data.

For analyses of mechanism and early-adulthood outcomes, we rely on World War II enlistment data extracted from Goldstein et al. (2021). In addition, the decennial census data is extracted from Ruggles et al. (2020). For endogeneity analysis, we use birth registration laws from Fagernäs (2014), state-level prohibition reforms from Law and Marks (2020), and the share of dry counties from Sechrist (2012). Finally, the poll tax and suffrage reforms database is extracted from Fagernäs (2014).

4 | EMPIRICAL METHOD

The econometric method exploits the differences in the differential adoption of suffrage law across states and over time. We operationalize this comparison in a two-way fixed effect difference-in-difference model as follows:

$$y_{icsr} = \alpha_0 + \alpha_1 ShareExp_{cs} + + \alpha_2 X_i + \alpha_3 Z_{cs} + \xi_{cr} + \zeta_s + \varepsilon_{icsr} \tag{1}$$

Where y is the outcome (longevity in NCHS and mortality in NLMS analysis) of individual i who belongs to birth cohort c and is born in state s located in census region r. In X, we include as individual controls gender and race dummies. In Z, we include birth-state-by-birth-year covariates, including female literacy rate, female labor force participation rate, average

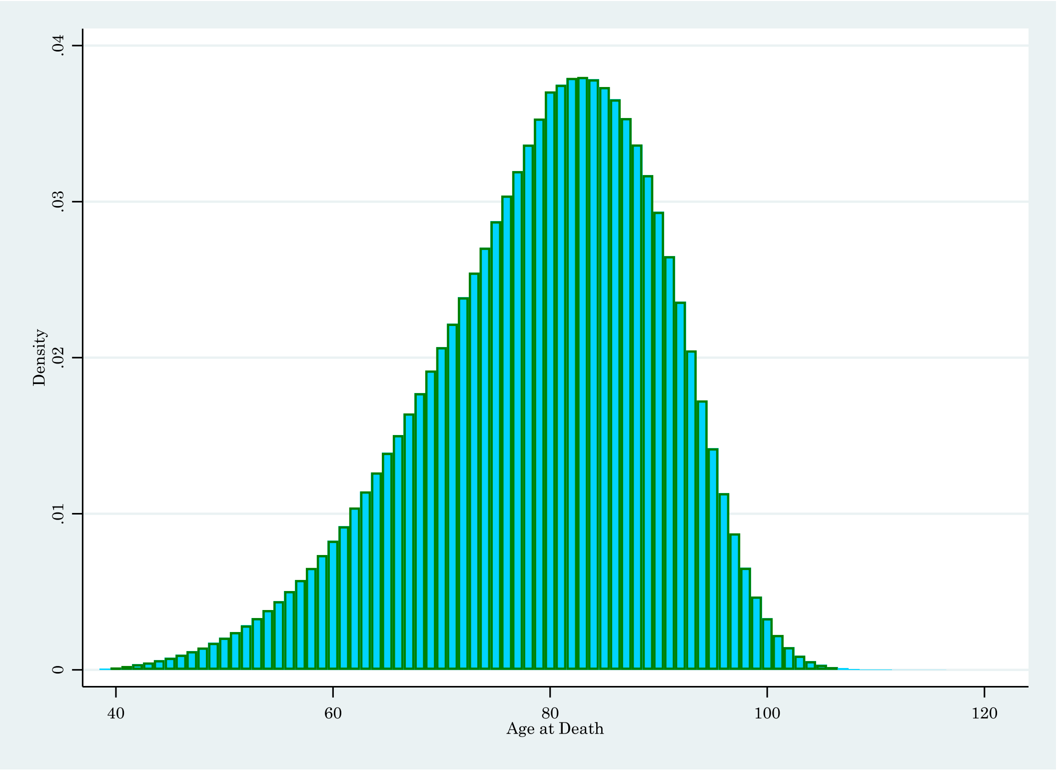

FIGURE 2 Density distribution of old-age longevity over the years 1979–2020 and for birth cohorts of 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

socioeconomic index, the average number of children, and share of employees in different occupation groups. These covariates are extracted from the full-count decennial census data (1880–1940) and interpolated for non-decennial years. To control for convergence in health outcomes of different cohorts across census regions, we include region-of-birth-by-birth-year fixed effects represented by ξ.[4] The parameter ζ represents state-of-birth fixed effects. The variable of interest, *ShareExp*, measures the share of time during a person's childhood up to age 17 that their mother was exposed to the implementation of suffrage laws. Thus, for those who turned 18 at the time of suffrage, the variable takes a value of zero. Also, for those born before the suffrage reform, it equals one. For a person who turned nine at the suffrage reform year, it equals 0.5. Finally, ε is a disturbance term. To account for serial correlation in the error term across the place of birth, we cluster standard errors at the birth-state level.

4.1 | Identification challenges

There are several threats to the identification strategy of Equation (1), which we list below. First, states that pass the laws earlier might have already been on a path of social and economic reforms with potential influence on health and longevity and the effects could pick up on those pre-existing trends in longevity. Our event study analyses (discussed in Section 5.1) rules out this concern. Second, suffrage law changes might result in outflow/inflow of migration and, through this channel, alter the sociodemographic composition of states. This endogenous change in the populations' characteristics could bias the regressions as there are differences in longevity of different sociodemographic subpopulations. We extensively discuss this in Section 5.5 and Appendix J and do not find empirical evidence to support this concern.

Third, the staggered-adoption setting of the econometric method raises the concerns of endogeneity in ordinary least square estimations (Goodman-Bacon, 2021a; Sun & Abraham, 2021). In Section 5.7, we show that the results are robust to alternative difference-in-difference techniques. Fourth, the death window of NCHS data (1979–2020) is truncated from left and right. In Section 5.8, we implement a series of analyses and simulations to provide evidence that this truncation is likely to underestimate the results and the main effect of the paper is a lower bound of the true effects (extensively discussed in Appendix F).

5 | RESULTS

5.1 | Event-study results

We start our analysis by showing the event-study results of childhood exposure to suffrage reform and longevity.5 The results are illustrated in Figure 3. The event-time coefficients in these event studies are year the individual turns 17 relative to the suffrage law year. We drop the coefficient of those who were 17–18 years at the time of state-specific suffrage (event-time = [−1,0]), so other coefficients are measured relative to longevity of these cohorts. Therefore, the pre-trend coefficients represent older cohorts or the unexposed cohorts (event-time coefficients of ≤−2). Similarly, coefficients of post-trend represent younger cohorts or exposed cohorts. These coefficients vary between 1 and 17 for partially exposed cohorts and ≥18 for fully exposed cohorts.

The pre-trend coefficients are close to zero and statistically insignificant, which rules out the concern over preexisting trends in the outcome. The coefficients start to rise for those who turn 15–16 years old at the time of suffrage (event-time = [1,2]) and continue to increase for other partially exposed cohorts. The coefficients become relatively (at least relative to the rising trend for partially exposed cohorts) stable for fully exposed cohorts. Moreover, all post-trend coefficients are statistically significant at 5% level.

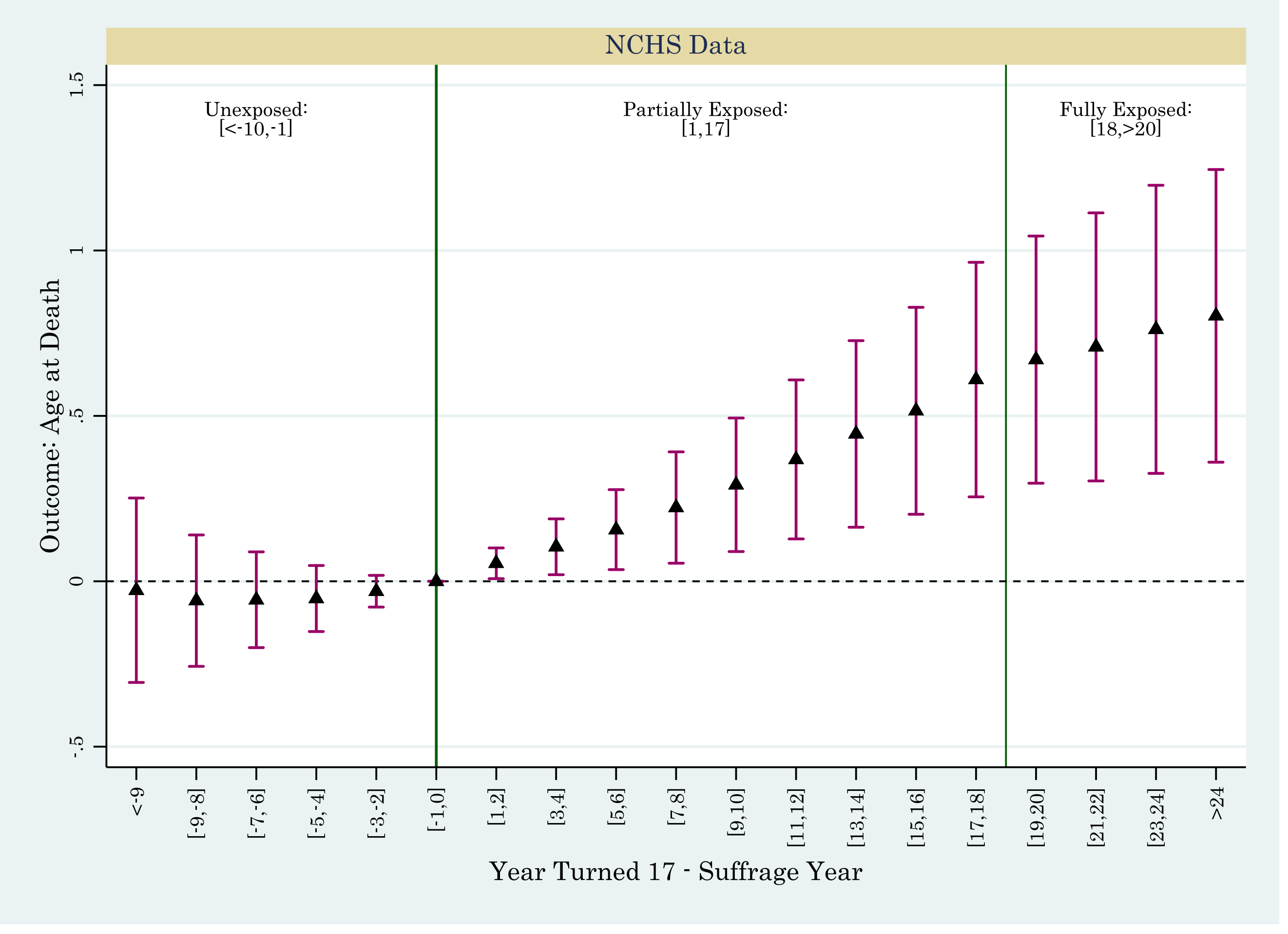

FIGURE 3 Event-study to explore the association between childhood exposure to suffrage laws and old-age longevity using NCHS mortality data. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. Standard errors are clustered at the state level. The data covers the years 1979–2020 for cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

5.2 | Main results

The main results of the paper are reported in Table 2 across subsamples in consecutive panels. The first column within each panel shows the effects conditional on fixed effects, and the second panel adds individual and state-level controls. The full specification of the full sample suggests that compared with those who turned 18 at the time of suffrage (share of exposure = 0), being exposed to suffrage during all years of childhood (share of exposure = 1) is associated with 0.56 additional life years (column 2).[6] This effect is slightly larger among males than females (comparing columns 4 and 6). However, the effects are significantly larger among blacks compared to whites. Among blacks, the difference in the longevity of fully-exposed versus unexposed cohorts is about 1.2 years, equivalent to a roughly 1.5% rise from the mean. This racial difference in the effects of suffrage is also documented in the previous literature. For instance, Kose et al. (2021) show that children fully exposed to suffrage reveal improvements in educational attainments and that these benefits appear to be significantly larger among black children than white children. They also find modestly larger effects among males than females, consistent with our reduced-form findings on longevity.

To understand the magnitude of the effects, we can compare them with other determinants of longevity. Chetty et al. (2016) explore the income-longevity relationship in the US over the years 1999–2014. They employ data from Social Security Administration death records linked with individual tax records. They find that each additional income percentile is associated with about 1.9 months of increases in longevity. Using the sample's median income, this is equivalent to an increase of $8000 in income (in 2020 dollars). Therefore, an increase of 0.56 years is equivalent to the impact of roughly $28,000 additional income. Aizer et al. (2016) explore the impacts of childhood exposure to the Mothers' Pension (MP) program on old-age longevity. MP was a government program to transfer cash benefits over a 3-year period to poor single mothers that was equivalent to about 30–40% of mothers' pre-transfer earnings. They focus on cohorts born between 1900 and 1925 and died between 1965 and 2012. They find that receipt of MP increased children's longevity by about 1 year. Therefore, the intent-to-treat exposure to suffrage has about half the effects of a relatively large transfer to poor families. Noghanibehambari and Engelman (2022) explore the effects of in-utero and early-life exposure to social spending under the New Deal programs of the 1930s on old-age longevity. They focus on birth cohorts of 1929–1940 who died between 1988 and 2005. They find that a 100% rise in city-level spending is associated with roughly 3.5 months of additional life. Therefore, childhood exposure to suffrage is equivalent to a 160% rise in per capita social spending, off a mean of about $1000 (in 2020 dollars).[7]

5.3 | Heterogeneity across subsamples

In accompanying the suffrage laws, states also enacted literacy tests. Therefore, one would a priori expect to observe larger effects in places with higher female literacy rates. Moreover, several studies suggest that women empowerment in society can also be attained by increasing the share of women in the labor force (Togeby, 2016). In this view, female labor force participation can operate as a dynamic complementarity factor to boost the effects of suffrage laws. Therefore, we would expect to observe larger effects in places with higher initial female labor force participation. To examine these potential heterogeneities, we replicate the main results across states below/above the median of female literacy, female socioeconomic index, and female labor force participation rate in the year of the suffrage law change.[8] These results are reported in Table 3. As expected, the effects are stronger for states with higher female literacy and higher initial female labor force participation.

Another argument is that the effects might be concentrated in one specific region. In columns 7–10 of Table 3, we replicate the main results across census regions. The effects are not uniform across regions, but we don't observe that a specific region drives them. For instance, the coefficient of Midwest-born people suggests 0.37 years increase in longevity, while this effect is 0.94 years for Southern-born individuals.

5.4 | Using an alternative outcome

In this subsection, we show that the effects do not appear solely for longevity outcomes. We document that comparing cross-cohorts and across states leads us to observe lower mortality of suffrage-exposed individuals. In so doing, we replicate the main results for the NLMS sample and replace the outcome with a dummy variable indicating whether or not a person was dead by the year of the interview in 1983. We start our analysis by showing that the NLMS sample does not reveal a demographic change resulting from suffrage law change by showing the balancing-test type event-study analysis in Appendix A. Then, we implement a similar event-study as in Figure 3, using all fixed effects and covariates discussed in Equation (1). The results are

TABLE 2 Main results: The association between childhood exposure to suffrage laws and old-age longevity.

	Outcome: Age at death (Years), samples:									
	Full sample		Females		Males		Blacks		Whites	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Share of exposure	0.34451**	0.56683***	0.42861***	0.54216***	0.24562	0.60377**	1.1677***	1.16278***	0.428***	0.51913***
	(0.16849)	(0.20648)	(0.15632)	(0.18987)	(0.23162)	(0.24264)	(0.31969)	(0.36384)	(0.15185)	(0.17138)
Observations	64,889,135	64,845,601	34,265,048	34,236,092	30,624,087	30,609,509	6,578,661	6,576,240	58,006,804	57,967,325
R-squared	0.24295	0.26734	0.25526	0.25765	0.21204	0.21465	0.28426	0.30426	0.23365	0.25598
Mean DV	79.193	79.186	81.302	81.295	76.833	76.828	76.136	76.131	79.547	79.540
%Change	0.435	0.716	0.527	0.667	0.320	0.786	1.534	1.527	0.538	0.653
Birth state FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓		✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE 3 Heterogeneity of the main results across subsamples.

	Outcome: Age at death (Years), samples:									
	Female literacy		Female socioeconomic index		Female labor force participation		Census region			
	Below median	Above median	Below median	Above median	Below median	Above median	Northeast	Midwest	South	West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Share of exposure	0.22152	0.5408***	0.35728*	0.76025***	0.36704**	1.69046***	0.98749***	0.3794***	0.94703***	0.20315
	(0.35805)	(0.13335)	(0.18167)	(0.23626)	(0.14948)	(0.48655)	(0.18194)	(0.12038)	(0.28757)	(0.27937)
Observations	32,660,474	32,185,127	33,460,508	31,385,093	33,486,498	31,359,103	16,456,316	20,172,040	23,923,332	4,293,913
R-squared	0.27072	0.25946	0.28279	0.24877	0.27036	0.26375	0.23757	0.26961	0.28143	0.23651
Mean DV	78.597	79.784	78.844	79.551	79.378	78.982	79.552	80.033	78.239	79.086
%Change	−0.282	0.678	0.453	0.956	0.462	2.140	1.241	0.474	1.210	0.257
Birth state FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

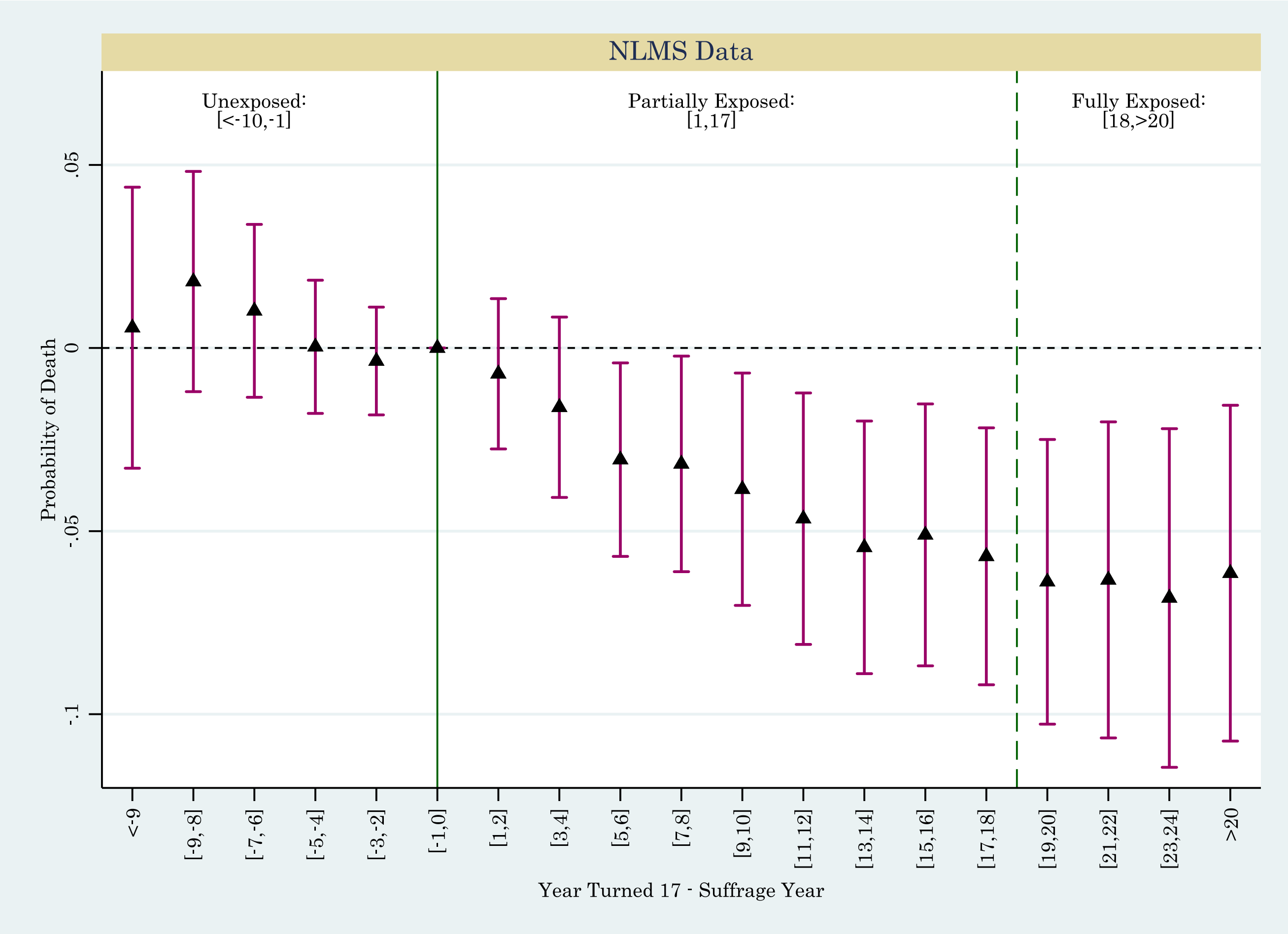

FIGURE 4 Event-Study to Explore the Association between Childhood Exposure to Suffrage Laws and Old-Age Mortality Using National Longitudinal Mortality Study. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. Standard errors are clustered at the state level. The data covers cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

illustrated in Figure 4. Pre-treatment coefficients are statistically and economically indistinguishable from zero revealing no preexisting trend in the mortality of unexposed cohorts. The effects start to rise (in magnitude) for partially exposed cohorts and become quite stable for fully exposed cohorts.

In Table 4, we show the regression results for two models: ordinary-least-square and logit regressions. Both models suggest considerable reductions in the mortality of exposed individuals. The OLS results suggest that a one-unit change in the share of exposure (comparing fully-exposed to unexposed cohorts) is associated with a roughly 5 percentage-points decrease in the probability of death, equivalent to an 18% drop from the mean of mortality in the sample. The logit model suggests that for a one-unit increase in the share of exposure, the odds of death decrease by 18.5%.

5.5 | Endogeneity concerns

The idea behind the empirical methodology is that the changes in suffrage laws are uncorrelated with other determinants of longevity. In other words, the underlying assumption is that in the absence of the reforms, the outcome of treated and control cohorts would have followed the same path and were influenced by the same factors. However, several reasons pose doubts about this assumption which we discuss below.

First, suffrage law movements might induce migrations and subsequent changes in sociodemographic composition of states. Further, the policy reforms may also reflect other policy changes or results in future reforms in non-suffrage related areas.

TABLE 4 The association between exposure to suffrage laws and old age mortality using national Longitudinal mortality study.

	Outcome: Individual is Dead (Dummy)	
	LPM	**Logit**
	(1)	**(3)**
Share of exposure	−0.05028***	−0.20408***
	(0.0163)	(0.059)
Observations	381,141	381,141
R-squared	0.2864	0.25152
Mean DV	0.274	0.274
Birth state FE	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓
Controls	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Therefore, one concern is that the results pick up on these contemporaneous state-level changes in policy, demographic, and socioeconomic characteristics. In Appendix J, we empirically investigate this concern and find no evidence for these concerns.

The second concern is regarding the survival of infants and children to NCHS data. Suppose suffrage imposes a condition on survival that differs by gender/race/ethnicity. In that case, the regression estimations are contaminated since there are differences in longevity by gender, race, and ethnicity that cannot be captured by including these as control variables. To explore this source of bias, we implement some balancing-test type event-study analysis in which the event-time is the year a person turns 17 relative to the year of suffrage.[9] The results are reported in four panels of Figure 5. All the pre-post coefficients' point estimates are indistinguishable from zero, statistically and economically.

To complement this section and show that the effects are indeed driven by exposure during a specific age range, that is, childhood, we implement several placebo tests to explore the association between exposure to suffrage laws at ages that individuals likely moved out of their original household. These results are reported and discussed in Appendix H. Appendix Table H1 reports the effects for different age group comparisons. For instance, column 1 compares the outcomes of those individuals who experienced suffrage when they were 19–20 years old to those who were 21–23. If the association between childhood exposure to suffrage and longevity were driven by overall improvement in health outcomes in early-adopter states versus later-adopter states, we would observe strong associations in this table. However, the estimated effects are quite small in magnitude and statistically insignificant.

5.6 | Robustness across specifications

We explore the robustness of the results in Appendix I. We show that the results are quite robust when we add birth-state-by-race and birth-state-by-gender fixed effects, birth state trend, death-state fixed effects, death-month fixed effects, and an array of additional state controls. Moreover, we show the robustness of the results when we replace the outcome with log of age at death and a dummy indicating longevity of beyond 75 years. Finally, we show that the results remain significant when we use robust standard errors and a two-way clustering level by birth-state and region-year level.

5.7 | Robustness to alternative difference-in-difference estimations

Our empirical methodology is primarily an OLS-produced difference-in-difference (DD) strategy. The recent development in the econometrics of DD analysis, specifically for staggered adoption in policy analysis, suggests that the DD coefficient is a combination of 2-by-2 DD comparisons between post/pre and treatment/control groups (Goodman-Bacon, 2021a). For instance, the OLS compares early suffrage adopter states to those states yet to adopt the law, as well as later adopter states to those who

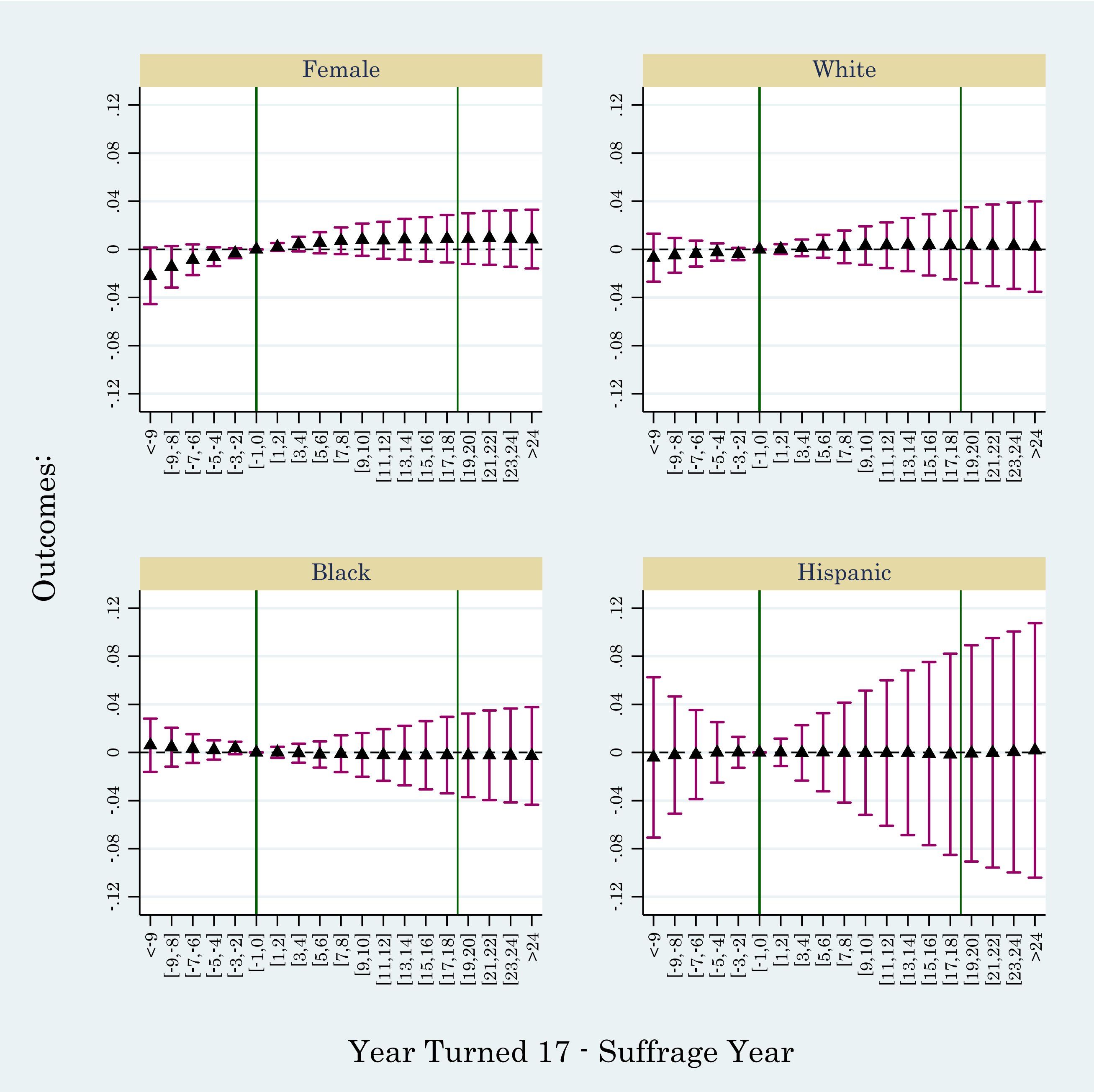

FIGURE 5 Event-study to explore the association between childhood exposure to suffrage laws and observable characteristics in NCHS mortality data. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The data covers the years 1979–2020 for cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

adopted the law earlier. In this case, the already treated observations are in a different trajectory as suffrage has changed their longevity trends and are not an appropriate control group for newly treated groups. To explore this heterogeneity in different comparison sets, we implement bacon-decomposition and discuss the results in Appendix E (Goodman-Bacon, 2021a). We observe that comparing later treated versus already treated cohorts reveals a negative overall coefficient while the other comparisons and the overall DD coefficient are positive. Therefore, we would expect that the OLS contaminations only under-bias the overall effects, and the true effects could be even larger.

As an alternative analysis, we replicate the event-study analysis of de Chaisemartin and D'Haultfœuille (2020), which attempts to modify the OLS estimates by removing contaminations of later-treated versus earlier-treated comparisons. These results are depicted in Appendix Figure E3. The absence of a pre-trend and the rise in coefficients for treated cohorts reveal a pattern similar to those of Figure 3. However, the marginal effects of treated cohorts and specifically fully-exposed cohorts are only modestly larger than those of the OLS-produced event-study. Overall, we find our initial findings relatively robust after excluding the contaminant comparison sets. In Appendix E, we also show the robustness of the event-study using the method developed by Sun and Abraham (2021).

5.8 | Truncation issue

The NCHS data covers the entire deaths that occurred in the US over the period of the study. Although we are using all available death years during which birth-state is reported, there are still concerns about truncation issues. To address the left-truncation concern, we use earlier deaths in the NCHS database. The issue with earlier death data, as discussed in section 3, is that they do not report state-of-birth, the key variable to measure childhood exposure to law changes. We use the death-state as a proxy for the birth-state and add death data from 1960 to 1978 to our final sample. We report and discuss these results in Appendix F. We first examine the effects in a subsample of those that died between 1979 and 2020 data but using death-state as a proxy for birth-state. We find a drop of about 12 percentage-points (off a mean of 79.2). We then compare the results with those extracted from the analysis of the sample of 1960–2020 death records (with death-state as a proxy of birth-state). The resulting coefficient drops by about 9 percentage-points (off a mean of 79.2). This small reduction suggests a small role of the confounding influence of left truncation.

Another concern is deaths that occurred post-2020. Although we cannot fully address this concern, we can understand to what extent this could be problematic by restricting the scope of birth cohorts. Specifically, we focus on a subsample of 1880–1920 and 1880–1915 cohorts, those who are less likely to have survived to 2020. We report and discuss these results in Appendix F. We find effects that, although smaller than the main results, are economically and statistically significant. We should note that by restricting birth cohorts, we lose variations in suffrage laws specifically for those exposed to the federal act of 1920. Therefore, some of the reductions could be due to decreases in suffrage variations and fewer fully-exposed cohorts in the sample. We should also note that conditional on survival up to the year 1980, these restricted subsamples have a high coverage rate. For instance, based on the 1980 census, cohorts born between 1880 and 1915 add up to about 25 million individuals. The subsample of 1880–1915 cohorts in the NCHS data covers 23.3 million deaths. Therefore, this subsample covers about 93% of these cohorts that survived up to 1980.

As a final step, we implement a series of simulation analyses. We generate random observations across cohorts and states similar to our final sample. We assign the longevity of each cohort based on cohort-specific life expectancy and an error term We then assign a certain treatment based on share of exposure of each cohort and implement regressions similar to Equation (1). The results are reported and discussed in Appendix F. We re-run this regression for samples truncated at various years. We observe reductions in effects for more truncated samples, suggesting that having access to a full mortality history would have provided us with larger marginal effects. We should note that the suggested rise in magnitude based on these simulations is relatively large. For instance, comparing the longevity of all individuals from birth to death with those truncated after 1980, we find a drop of about 68% (panel B Appendix Table F3s). This fact suggests that the impact of suffrage could be as large as 2 years had we had information on the full history of longevity of all individuals.

5.9 | Additional analysis

In Appendix C, we replicate the event-study for different causes of death. The longevity benefits appear to be stronger for diseases related to Malignant Neoplasm, Cardiovascular diseases, Chronic Lower Respiratory diseases, Influenza, Pneumonia, and Nephritis. We also replicate the event-study for subsamples based on race and gender.

6 | POTENTIAL MECHANISMS

So far, the results suggest improvements in old-age health due to exposure to empowering women during childhood. However, these results are reduced-form impacts and are not informative regarding the potential mechanisms. In this section, we discuss several potential mechanisms and try to use historical data to empirically test these mediatory channels.

6.1 | Bargaining power

Empowering women through changes in political institutions may change the intrahousehold bargaining power of women (Duflo, 2012). Some studies suggest that the bargaining power at the household level is both the result and the cause of bargaining power at the political level (Hiller & Touré, 2021). Several studies document that increases in intrahousehold bargaining power of women can improve children's nutrition intake, general health outcomes, and their human capital (Kulkarni et al., 2021; Lépine & Strobl, 2013; Park, 2007; Tommasi, 2019). Although this is a plausible mechanism, we are aware of no data for this period that contains intrahousehold bargaining status or changes in within-household resource allocation.

6.2 | Health and health spending

Mortality provides an extreme but precise measure of health. For the period of the study, we extract state-level mortality rates from Kose et al. (2021). It covers age-gender-specific death counts for 45 states between 1900 and 1930. We focus on several age groups for which we have data pre-1920, to allow for state-specific variations. We merge this data with suffrage database based on state and year and implement regressions that include state fixed effects and state-trends. Since in each year there are very few numbers of states in the sample, we avoid adding region-year fixed effects (as we do in the main results). We report the results in columns 1–4 of Table 5. The independent variable is a dummy indicating state-specific post-suffrage. We observe a reduction in infant mortality rate and mortality rate of 10–14 years old children by about 6.6 and 7.7%, respectively (columns 1–2).[10] The results also suggest decreases in mortality rates of those aged 15–19, but the effects are statistically insignificant (column 3). We argue that these improvements in mortality is the result of individual and collective investment in children's health. As placebo test, we show the effects on log mortality rate of those aged 35–49 in column 4. The estimated effects are small and statistically significant.

TABLE 5 Exploring mechanisms using historical data on mortality, health spending, and compulsory schooling.

	Outcomes:							
	Log mortality rate under 1-year	**Log mortality rate 10–14 Years old**	**Log mortality rate 15–19 Years old**	**Log mortality rate 35–49 Years old**	**County health departments is present in the county**	**Log per capita physicians**	**Log per capita property tax collection**	**Years of required compulsory schooling**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Exposure	−0.06608**	−0.07702**	−0.05092	−0.03855	0.03254***	0.01183***	0.0203***	0.25369***
	(0.02988)	(0.03578)	(0.03478)	(0.02634)	(0.00541)	(0.00079)	(0.00489)	(0.08763)
Observations	794	794	794	794	55,167	55,167	55,094	1567
R-squared	0.95341	0.87023	0.92759	0.87805	0.73766	0.99379	0.98924	0.94814
Mean DV	5.219	2.954	3.440	7.158	0.075	2.268	3.019	5.504
%Change	−1.266	−2.607	−1.480	−0.539	43.385	0.521	0.672	4.609
Years of data coverage	1900–1930	1900–1930	1900–1930	1900–1930	1910–1930	1910–1930	1910–1930	1880–1912
No of states of data coverage	45	45	45	45	39	39	39	48
Region-of-Birth-by-Birth-Year FE					✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE interacted with year trend	✓	✓	✓	✓				✓
County FE interacted with year trend					✓	✓	✓	

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

The collective bargaining power of women and its benefits can appear in changes in allocation of resources toward health-related spending and public health infrastructures (Chattopadhyay & Duflo, 2004; Duflo, 2012; Miller, 2008). We add to these studies by showing the impact of suffrage on the establishment of County Health Departments (CHD). The CHDs were designed to provide sanitations and child health services. Hoehn-Velasco (2018) find that CHDs decreased rural infant mortality rate by about 10%. We use the replication data of Hoehn-Velasco (2018) to construct a county-year panel that covers 39 states over the years 1910–1930. We implement regressions that include cunty fixed effects, region-year fixed effects, and a county-trend to explore the effects of suffrage law change on the likelihood of a county having a CHD. The results are reported in column 5 of Table 5. We observe an increase in the probability of a CHD being established in the county by about 3.2 percentage-points, off a mean of 0.075. In column 6, we show that suffrage law change is associated with 1.2% increase in per capita physicians at the county-level. In column 7, we examine the effects on per capita property tax. During the early decades of the 20th century, a large portion of school spending, health spending, and even policing spending was extracted from local property taxes, although in later decades the share of state funds increased. Therefore, property taxes are a proxy for overall social spending, public finance, and infrastructure investments. We find a significant increase of 2% in per capita property taxes. The combination of the results of columns 5–7 are in line with findings of Miller (2008) that suffrage movements was associated with increases in public health spending.

Finally, we also examine the effects on changes in educational laws. In so doing, we employ data on historical compulsory attendance laws extracted from Clay et al. (2021). The data contains data on 48 states for the period of 1880–1912. We employ regressions that include region-year fixed effects and state fixed effects interacted with a linear year trend. We report the results in column 8 of Table 5. Suffrage law change is associated with 0.25 years increase in compulsory schooling, off a mean of 4.6. Several studies suggest that early 20th century schooling laws were effective to improve education, occupational choice, occupational mobility, and labor market outcomes (Clay et al., 2021; Lleras-Muney, 2002; Rauscher, 2016). Improvements in income, education, and occupational choice can be translated into improvements in health and longevity (Chetty et al., 2016; Fletcher, 2012; Fletcher et al., 2021; Malamud et al., 2021).

6.3 | Education-income profile

As we discussed in section 2.2, the literature documents suffrage-induced increases in school spending and rises in public education among exposed children. In addition, a strand of the literature argues, though inconclusively, that improvements in education and labor market outcomes are associated with increases in longevity and reductions in mortality (Cutler et al., 2016; Fletcher, 2015; Huebener, 2019; Lleras-Muney, 2005; Meghir et al., 2018).

We reevaluate the association between suffrage and education for cohorts similar to those in our study sample. In so doing, we focus on the 1980 census for two reasons. First, it contains all the necessary information required for our empirical strategy, in addition to education and income. Also, it is the last year that the census asks about years of schooling.[11] Second, our sample starts from 1979. If treated cohorts are more likely to survive in future years, as the main results suggest, looking at post-1980 censuses may overestimate the effects as we are observing healthier and probably better-educated individuals.

We impose similar cohort and state choices as discussed in section 3 and implement regressions similar to the full specification of Equation (1) while replacing the education-income profile as the outcomes. The results are reported in Table 6 for different outcomes across different columns. A one-unit increase in the share of exposure ($ShareExp = 1$ vs. $ShareExp = 0$) is associated with an increase of 0.24 years of schooling, 3.3% points increase in the probability of having any college education, 5.9% higher total family income, and roughly 17% reduction in total welfare receipt. These effects are statistically significant, economically considerable, and consistent with previous findings (Carruthers & Wanamaker, 2015; Kose et al., 2021). However, education seems to be a modest and partial channel if we compare the magnitudes with the findings of education-longevity studies. For instance, Halpern-Manners et al. (2020) Show that an additional year of schooling increases longevity by about 0.34 years. Using this figure and combing the effects of column 1 of Table 6 and column 2 of Table 2, we can deduce that increases in years of schooling can explain only 15% of rises in longevity. Therefore, other health investments during childhood (that do not appear in education and income) could also play a role in linking suffrage exposure and longevity.

6.4 | Early-adulthood health and socioeconomic status

Another path to examine mechanisms of impact is to search for other health and socioeconomic outcomes in earlier years. In so doing, we use World War II enlistment data linked to Social Security Administration Death Master Files (DMF) extracted

TABLE 6 Exploring mechanisms of impact: The association between exposure to suffrage laws and education-income outcomes using census 1980.

	Outcome:				
	Years of schooling	Education less than high school	Education: College and more	Log total family income	Log total welfare income
	(1)	(2)	(3)	(4)	(5)
Share of exposure	0.23794**	−0.01625	0.03328***	0.05948*	−0.17108**
	(0.11405)	(0.01251)	(0.00966)	(0.03469)	(0.06744)
Observations	3,541,076	3,541,076	3,541,076	3,454,789	3,541,076
R-squared	0.14189	0.07208	0.04287	0.18785	0.04233
Mean DV	11.269	0.037	0.251	10.862	0.398
%Change	2.111	−43.927	13.259	0.548	−42.986
Birth state FE	✓	✓	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE 7 Exploring mechanisms of impact using DMF-WWII-enlistment data.

	Outcome:					
	Migrant (birth state different than 1940 state)	Neighborhood attainment	Occupational income score	Height	Log height	Heigh-for-age (standardized)
	(1)	(2)	(3)	(4)	(5)	(6)
Share of exposure	0.06985***	0.04451**	0.87471**	0.28856***	0.00417***	0.02505**
	(0.02045)	(0.02072)	(0.43055)	(0.08924)	(0.00128)	(0.01144)
Observations	368,602	368,602	335,466	367,478	367,478	367,478
R-squared	0.08284	0.05482	0.07543	0.04279	0.04095	0.92369
Mean DV	0.229	0.476	22.266	68.282	4.223	0.001
%Change	30.501	9.350	3.928	0.423	0.099	-
Birth state FE	✓	✓	✓	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

from CenSoc Project (Goldstein et al., 2021). There are three advantages to using this data. First, it is linked to the full-count 1940-census at the individual level. Therefore, we gain access to granular geographic information on place-of-residence in 1940. This is useful as we can explore the effects of suffrage on migration and the quality of residential location as a pathway to improved economic and health outcomes that can be reflected in later-life longevity (Derenoncourt, 2022). Second, since it is based on death records over the years 1975–2005, we know that individuals survived to their old ages and hence mitigate the concerns over selective survival. Moreover, this makes the sample similar to our main analysis sample. Third, it contains information about individuals' height, an indicator of health, and an outcome of childhood health investments (Bozzoli et al., 2009; Deaton, 2007).

We use the DMF-enlistment data and merge it with the full-count 1940-census extracted from Ruggles et al. (2020). We then merge this data with the suffrage database and other birth-state covariates. We implement regressions similar to Equation (1). The results are reported in Table 7. Column 1 reports the effects on migration which is defined as a dummy that equals one if the birth-state is different from the state-of-residence in 1940. We observe a large effect of exposure on migration, roughly an increase of 30% from the mean. Next, we construct a dummy to measure neighborhood attainment. In so doing, we calculate the average socioeconomic score at the county-level using the full-count 1940-census. The neighborhood attainment dummy takes a value of 1 if the individual's county-of-residence in 1940 is above-median of socioeconomic score in the nation and zero otherwise. The results suggest that childhood exposure to suffrage laws increases the probability of living in a wealthier neighborhood by about 4.5 percentage-points, off a mean of 0.48. We also observe increases in occupational income score of about 0.9 units, off a mean of 22.3 (column 3).

In column 4, we observe small but significant increases in height. To mitigate the influence of outliers and mismeasurements of enumerators, we also show the results for the log of height. These estimates are reported in column 5. We observe an increase of about 0.4%. Finally, we adjust the height by the age of individuals at the time of enlistment. We calculate the standardized value of height-for-age with respect to the mean and standard deviation of the sample and report the results in column 6. The effect suggests a significant rise of about 2.5% of a standard deviation with respect to the sample mean. Although height is an outcome and reflects other conditions, several studies document the correlations between height and later-life mortality outcomes (Jousilahti et al., 2000; Spijker et al., 2012; Wilson, 2019).

7 | CONCLUSION

While the developed countries started a path toward a more gender-equal society, issues of the gender gap still possess a wide range of outcomes (Doepke et al., 2012). Moreover, in many developing countries, the inequalities stem from the structural design of legal systems and cultural platforms (e.g., Godefroy (2019)). While the research has offered potential benefits of women empowerment and its spillover effects, fewer studies have looked at the long-run externalities. This paper added to this literature by documenting the long-run longevity improvements among children whose mothers were exposed to suffrage law changes in the United States during the late 19th century and early 20th century.

We extensively discussed the potential endogeneity concerns and ruled out issues regarding migration, endogenous fertility, and demographic compositional changes. A series of placebo tests combined with event-study analyses ruled out the concern that the observed effects ride on the preexisting trend and cross-cohort differentials in longevity. The main results suggested that cohorts fully exposed to suffrage during childhood compared with unexposed cohorts live roughly 0.56 years longer. These effects were stronger among blacks but somewhat similar between males and females. The gains also appeared to be larger in states with higher initial female literacy rates and higher initial female labor force participation. Additional analyses suggested that increases in education and income could have, though partially, operated as underlying mechanisms. These findings added to the ongoing literature on the long-run and intergenerational health benefits of women empowerment.

The positive and significant reduced-form effects on longevity is in-line with a strand of research that documents the positive externalities of women empowerment and specifically suffrage law movements in the US (Carruthers & Wanamaker, 2015; Kose et al., 2021; Miller, 2008; Moehling & Thomasson, 2012). Further, our results are also in-line with the growing body of empirical research that documents the relevance of early-life environment and childhood exposures in determining later-life outcomes and specifically old-age health and longevity (Almond et al., 2018; Cook et al., 2019; Fletcher, 2018; Montez & Hayward, 2011).

The difference in life expectancy in the US increased from 39.4 for cohorts born in 1880 to 62.1 for those born in 1940. Our results suggest that longevity improvements due to childhood exposure to suffrage laws account for 2.5% of these cohorts' overall rise in life expectancy.

The average exposure to suffrage in our sample is roughly 76%. Using this value, the marginal effect of column 2 of Table 2, and the number of individuals in the final sample, we can calculate a back-of-an-envelope estimation of 27.5 million life-years gained due to suffrage movements.

To put these findings into perspective, one method is to use the concept of Value of Statistical Life (VSL) estimates. By taking into account the difference of approximately 1 year in the average age at death between the final sample of individuals exposed to the intervention and the average lifespan of all cohorts born between 1880 and 1940 who died within the years 1979–2020, and using a discount rate of 3%, we can estimate a Value of Statistical Life at age 79 (VSL_{79}) of roughly \$1.5 million (Colmer, 2020; Kniesner & Viscusi, 2019; Viscusi, 2018). Based on a back-of-an-envelope estimation of the number of life-years saved and the VSL_{79}, we can conclude that the total savings amount to around \$40 trillion.

We should note that the economic and social development of many developing countries represent that of early decades of the 20th century US. In many developing countries, there are large gender gaps in various aspects of life, including labor force participation, political participation, and education (Schwab et al., 2017). Moreover, these countries reveal substantially lower life expectancy than other developing countries (Roser et al., 2013). The results of this paper could be useful for policymakers in these countries and provide insights on the long-run health benefits of women empowerment through creating a more gender-inclusive culture with the help of policy instruments.

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CONFLICT OF INTEREST STATEMENT

The authors claim no conflict of interest to report.

DATA AVAILABILITY STATEMENT

The data used in this study is confidential. The replication codes are available upon request.

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ENDNOTES

[1] A growing body of research evaluates the early-life parental investment and childhood conditions on later-life outcomes, including cognitive development (Abufhele et al., 2017; Thomas et al., 2022), education (Case et al., 2005; de Haan and Leuven, 2020; Smith, 2009), labor market outcomes (Flores & Kalwij, 2014; Goodman-Bacon, 2021b; Schiman et al., 2019), disability (Arthi, 2018; Muchomba & Chatterji, 2020; Schiman et al., 2019), hospitalization (Miller & Wherry, 2019; Scholte et al., 2015), diabetes (Sotomayor, 2013), respiratory function (Bartley et al., 2012), psychological stress (Daly & Delaney, 2013; Darin-Mattsson et al., 2018), old-age mental health (Adhvaryu et al., 2019; Singhal, 2019), and mortality outcomes (Hayward & Gorman, 2004; Ko & Yeung, 2019; Smith et al., 2014; Steptoe & Zaninotto, 2020; Van Den Berg et al., 2006, 2009, 2011). For instance, Smith et al. (2014) employ Utah Population Database (UPDB) to explore the effects (and the mediatory channels) of early-life parental death on old-age mortality. They control for a wide array of early-life economic and social conditions as well as contemporary covariates, including socioeconomic status and marital status. They find modest but significant effects on mortality risks in ages above 65. They argue that contemporaneous economic conditions do not offset the early-life effects.

[2] There is also a growing literature that examines the role of education in health outcomes and specifically old-age mortality (Braakmann, 2011; Buckles et al., 2016; Cutler et al., 2015; Cutler & Lleras-Muney, 2006; Fletcher, 2015; Fletcher & Noghanibehambari, 2021; Galama et al., 2018; Lacroix et al., 2019; Lleras-Muney, 2005; Lleras-Muney et al., 2020). For instance, Halpern-Manners et al. (2020) explore the effects of education on old-age longevity. They implement a twin-fixed effect strategy to control for unobserved innate abilities and shared exposures during childhood and find that an additional year of schooling is associated with 0.3 additional life years. Fletcher and Noghanibehambari (2021) argue that the accessibility and availability of colleges generate incentives for individuals to attend college. They examine the impact of new college openings in the county of residence during adolescence on old-age longevity. They show that new college openings increase education and longevity. Their treatment-on-treated calculations suggest that having earned any college education raises age at death by 1–1.6 years.

[3] Wyoming implemented the law in 1869 and Utah in 1870. Moreover, as we build the matrix of covariates based on decennial census and since the census did not cover Hawaii and Alaska (up to 1940), we remove individuals in these states, too.

[4] While in the main results we include region-of-birth-by-birth-cohort fixed effects, in Appendix D we show that the results are robust to excluding this double-interaction of fixed effects.

[5] Specifically, we implement regressions of the following forms using ordinary-least-square: $y_{ics} = \alpha_0 + \sum_{i=\underline{T}}^{\overline{T'}} \beta_i (I((BY_{ics} + 17 - Suff_{cs}) = i)) + \alpha_2 X_i + \alpha_3 Z_{cs} + \xi_{cr} + \zeta_s + \varepsilon_{ics}$, where all covariates and fixed effects are as in Equation (1). The event times (denoted by i) are grouped in 2-year chunks. For illustrative purposes, the event times are grouped for less than $\underline{T}$ (<−9) and more than $\overline{T'}$ (>24) into two single dummies. The parameter $I(.)$ represent unit function that takes one if its argument is true and zero otherwise.

[6] Comparing columns 1 and 2, we observe a jump in the coefficient. In Appendix G, we add one covariate at a time detect the one with largest impact. We find that female literacy rate and share of blue-collar occupation workers are the two important controls that drive the observed rise in the coefficient.

[7] Although these comparisons imply the relevance and significance of women empowerment and suffrage laws for later-life longevity, we should exercise caution in our interpretations. The main reason is that their focus on cohorts and death coverage is slightly different than our sample (1880–1940). The birth cohort selection of our study is much wider than Aizer et al. (2016) (1900–1925) and Noghanibehambari and Engelman (2022) (1929–1940). Moreover, death year coverage of our paper is also different than Aizer et al. (2016) (1965–2012) and Noghanibehambari and Engelman (2022) (1988–2005). However, in Appendix F, we provide evidence of robustness of these results to birth cohort selection and also to death year selection.

[8] We use the year of suffrage as the initial year so as to avoid the potential endogenous responses of people to suffrage law changes.

[9] The logic behind age 17 cut-off is that children leave their home after this age and that, if we believe education is a likely channel as we discuss in section 2.2, this is the usual cut-off age for completing K-12 education. However, in Appendix B, we show the robustness of the results to alternative age restrictions.

[10] Our data source of mortality of toddlers covers death post-1920. Thus, we are unable to estimate the regressions.

[11] In census-1990-onwars, we only observe education in categories.

[12] We employ similar sample selections as those explained in section 3.

[13] This is calculated as reducing the effect of column 3 by the percentage change between columns 1 and 2.

[14] Similar to Equation (1), we include region-by-year and state fixed effects in all these regressions. The standard errors are clustered at the state level.

[15] The argument rests on the assumption that single mothers, on average, have fewer available material resources. See, for instance, Duriancik and Goff (2019), Taanila et al. (2002), and Waldfogel et al. (2010).

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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APPENDIX A

In the main text, we implement the balancing tests through a series of event studies. In Appendixs Table A1, we show the balancing test through regressions similar to Equation (1), where the outcome is individual observable characteristics. We also replicate this practice with the NLMS sample in Appendix Table A2. Moreover, we replicate the event-study similar to the balancing test of Figure 5 for the NLMS sample and report the results in Appendix Figure A1. Overall, these results do not provide a strong and statistically significant association between samples' demographic compositional change due to suffrage laws.

TABLE A1 Balancing test: The association between childhood exposure to suffrage laws and observable characteristics in NCHS data.

	Outcome:			
	Female	**White**	**Black**	**Hispanic**
	(1)	**(2)**	**(3)**	**(4)**
Share of exposure	−0.00672	0.00565	0.0033	−0.00427
	(0.00458)	(0.00584)	(0.004)	(0.01274)
Observations	64,889,135	64,889,135	64,889,135	64,889,135
R-squared	0.01555	0.19061	0.20185	0.19382
Mean DV	0.528	0.894	0.101	0.275
%Change	−1.273	0.632	3.263	−1.554
95% confidence intervals	[−0.014 0.001]	[−0.004 0.015]	[−0.003 0.010]	[−0.026 0.017]
Birth state FE	✓	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE A2 Balancing test: The association between childhood exposure to suffrage laws and observable characteristics in NLMS data.

	Outcome:			
	Female	**White**	**Black**	**Hispanic**
	(1)	**(2)**	**(3)**	**(4)**
Share of exposure	0.01176	0.0092	−0.00678	0.00476
	(0.02028)	(0.00924)	(0.00621)	(0.01759)
Observations	381,375	381,375	381,375	381,375
R-squared	0.00456	0.1503	0.17204	0.14549
Mean DV	0.539	0.903	0.088	0.060
%Change	2.181	1.019	−7.704	7.939
95% confidence intervals	[−0.022 0.046]	[−0.006 0.025]	[−0.017 0.004]	[−0.025 0.034]
Birth state FE	✓	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

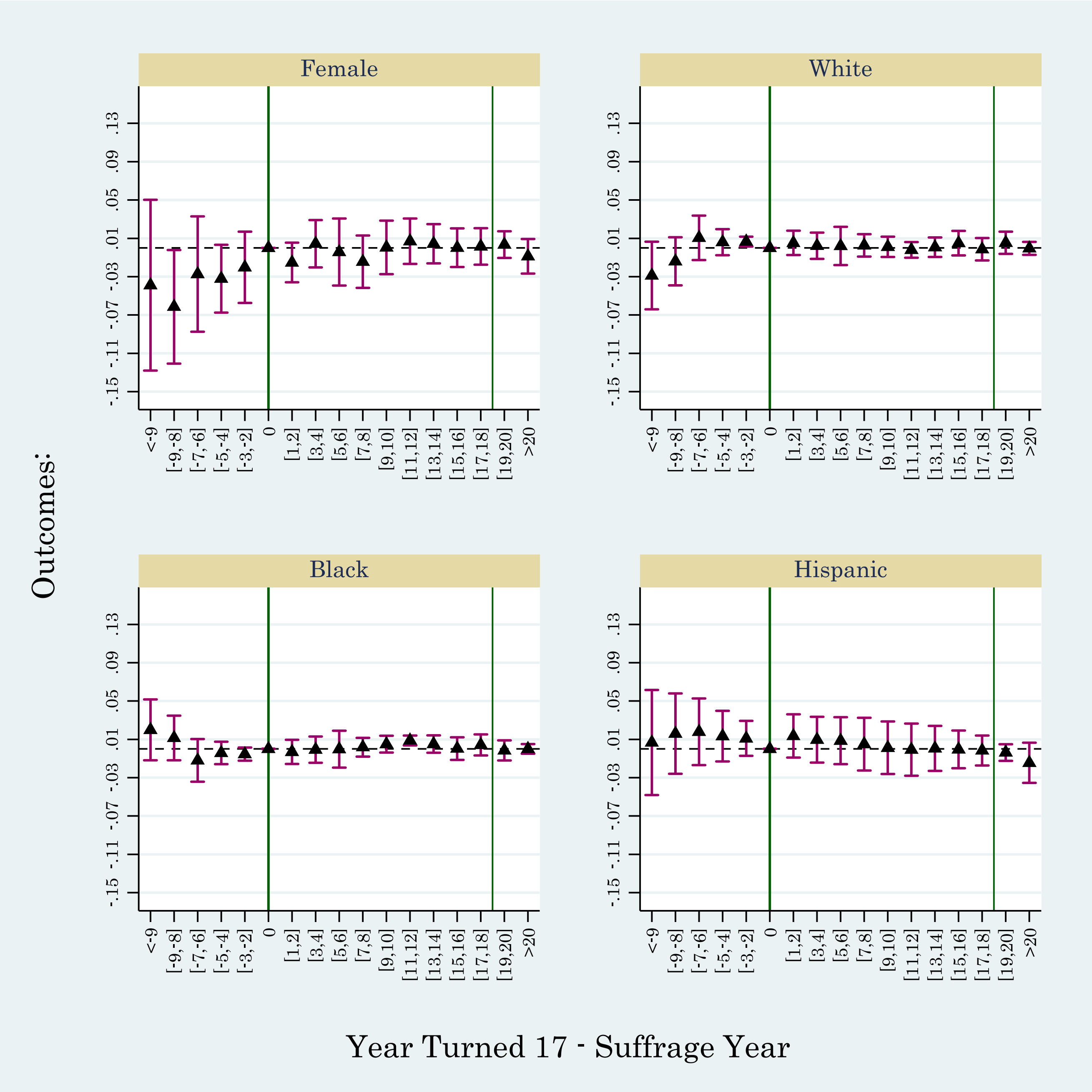

FIGURE A1 Event-study to explore the association between childhood exposure to suffrage laws and observable characteristics in NLMS data. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The data covers cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

APPENDIX B

In the main results, we considered childhood exposure up to age 17, assuming that children leave households at this age and have completed K-12 education by this age. Appendix Table B1 shows that the results are robust to alternative cut-off ages. However, the effects become only modestly smaller if we assume exposure up at ages 10, 12, and 15.

TABLE B1 Robustness of the results to different thresholds of childhood exposure.

	Outcome: Age at death, exposure up to:		
	Age 10	**Age 12**	**Age 15**
	(1)	**(2)**	**(3)**
Share of exposure	0.37397***	0.43414***	0.51468***
	(0.12728)	(0.15254)	(0.18404)
Observations	64,845,601	64,845,601	64,845,601
R-squared	0.26734	0.26734	0.26734
Mean DV	79.186	79.186	79.186
%Change	0.472	0.548	0.650
Birth state FE	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓
Controls	✓	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

APPENDIX C

One potential heterogeneity is due to differences in longevity of those who die of certain causes of death. To explore this, we replicate the event-study analysis for subsamples of individuals who die from specific causes. These results are reported across eight panels of Appendix Figure C1 and Appendix Figure C2. The gains in old-age longevity appear to be stronger in deaths due to Malignant Neoplasm, Cardiovascular diseases, Chronic Lower Respiratory diseases, Influenza, Pneumonia, and Nephritis.

We continue the heterogeneity analysis of event studies by reporting the event-study results across subsamples by race in Appendix Figure C3 and gender in Appendix Figure C4. For instance, comparing the bottom and top panels of Appendix Figure C3, one can observe the relatively larger rises in post-suffrage coefficients.

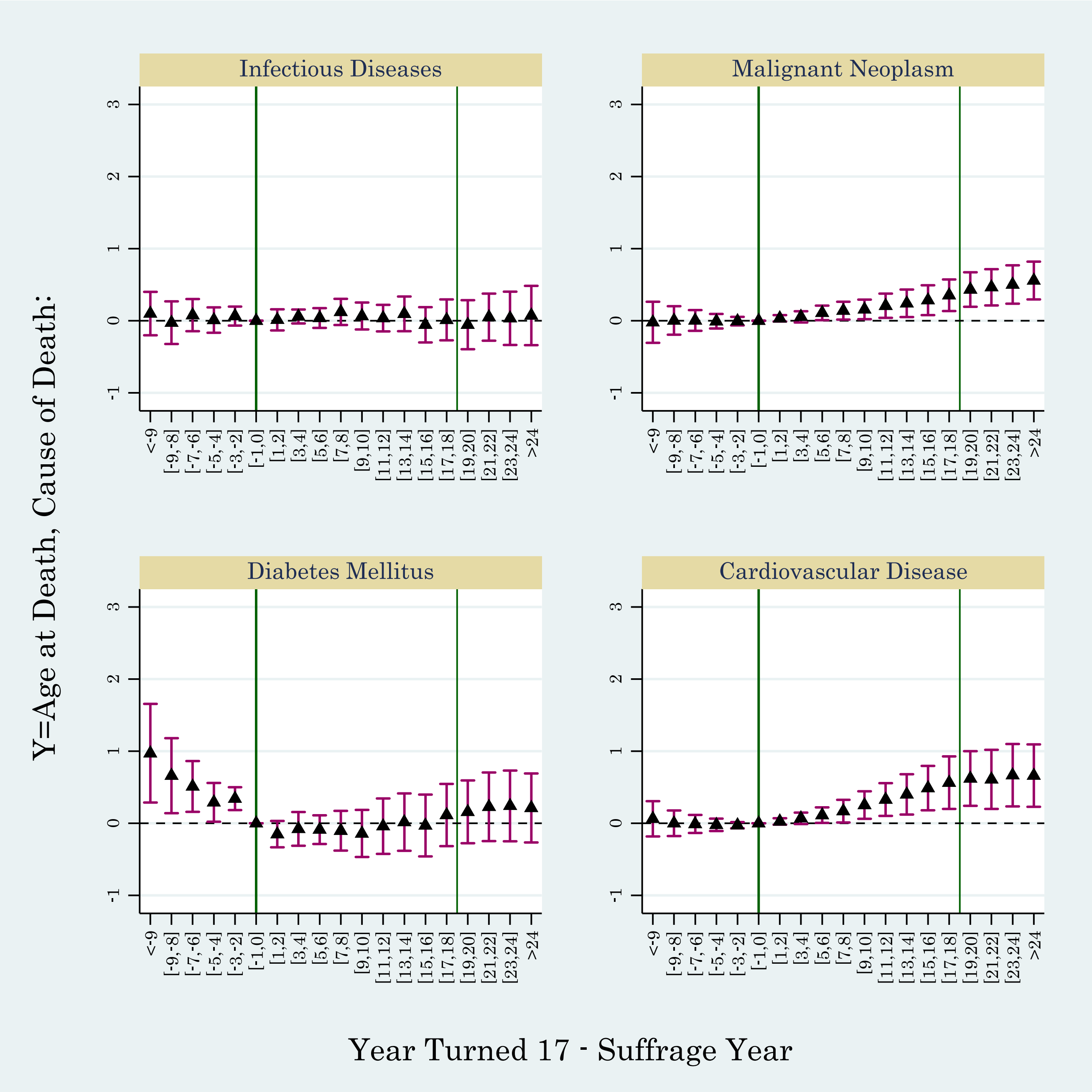

FIGURE C1 Event-study to explore the association between childhood exposure to suffrage laws and old-age longevity using NCHS mortality data for different cause of deaths. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. sStandard errors are clustered at the state level. The data covers the years 1979–2020 for cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

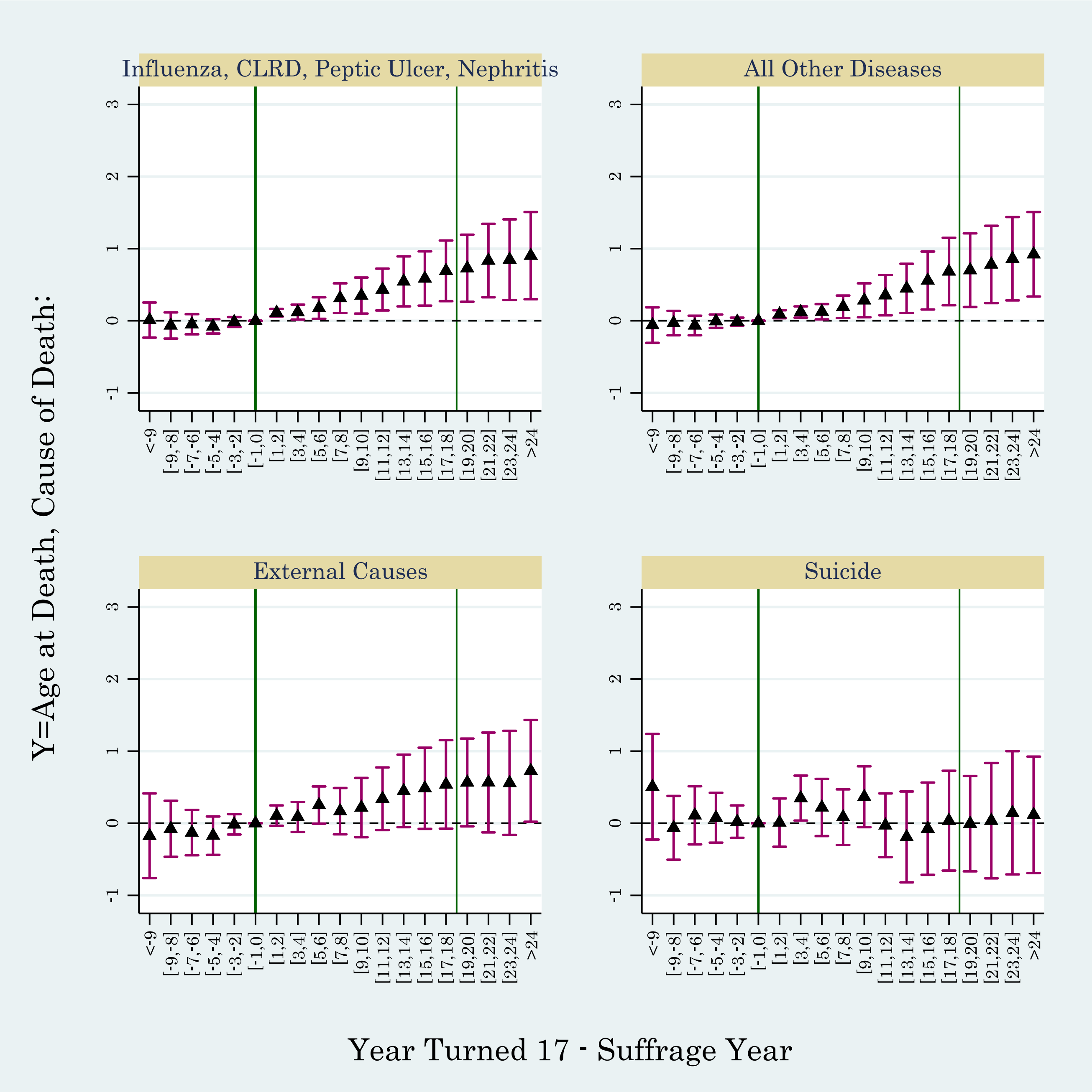

FIGURE C2 Event-study to explore the association between childhood exposure to suffrage laws and old-age longevity using NCHS mortality data for different cause of deaths. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. Standard errors are clustered at the state level. The data covers the years 1979–2020 for cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

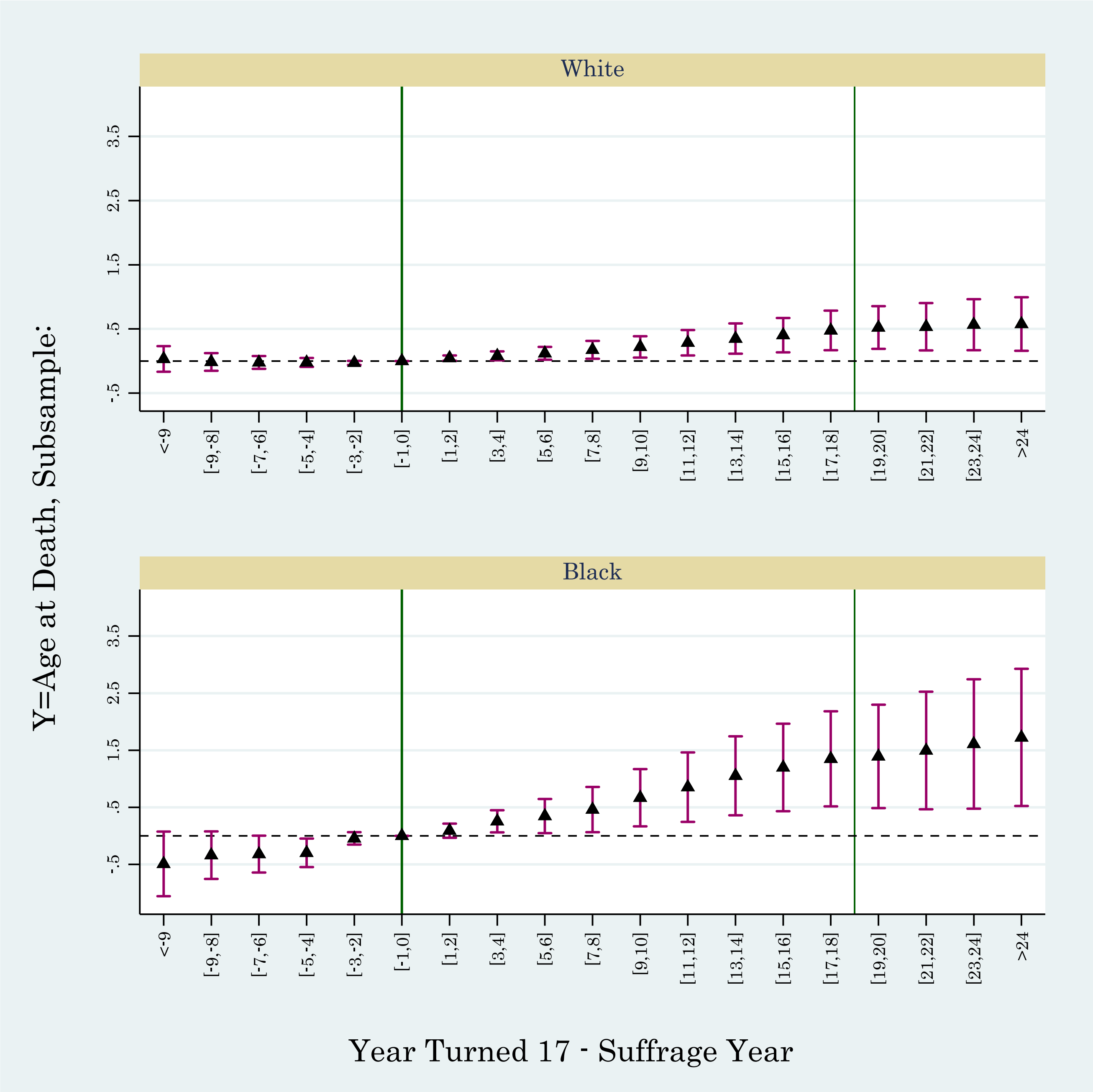

FIGURE C3 Event-study to explore the heterogeneity of the association between childhood exposure to suffrage laws and old-age longevity using NCHS mortality data for whites and blacks. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. Standard errors are clustered at the state level. The data covers the years 1979–2020 for cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

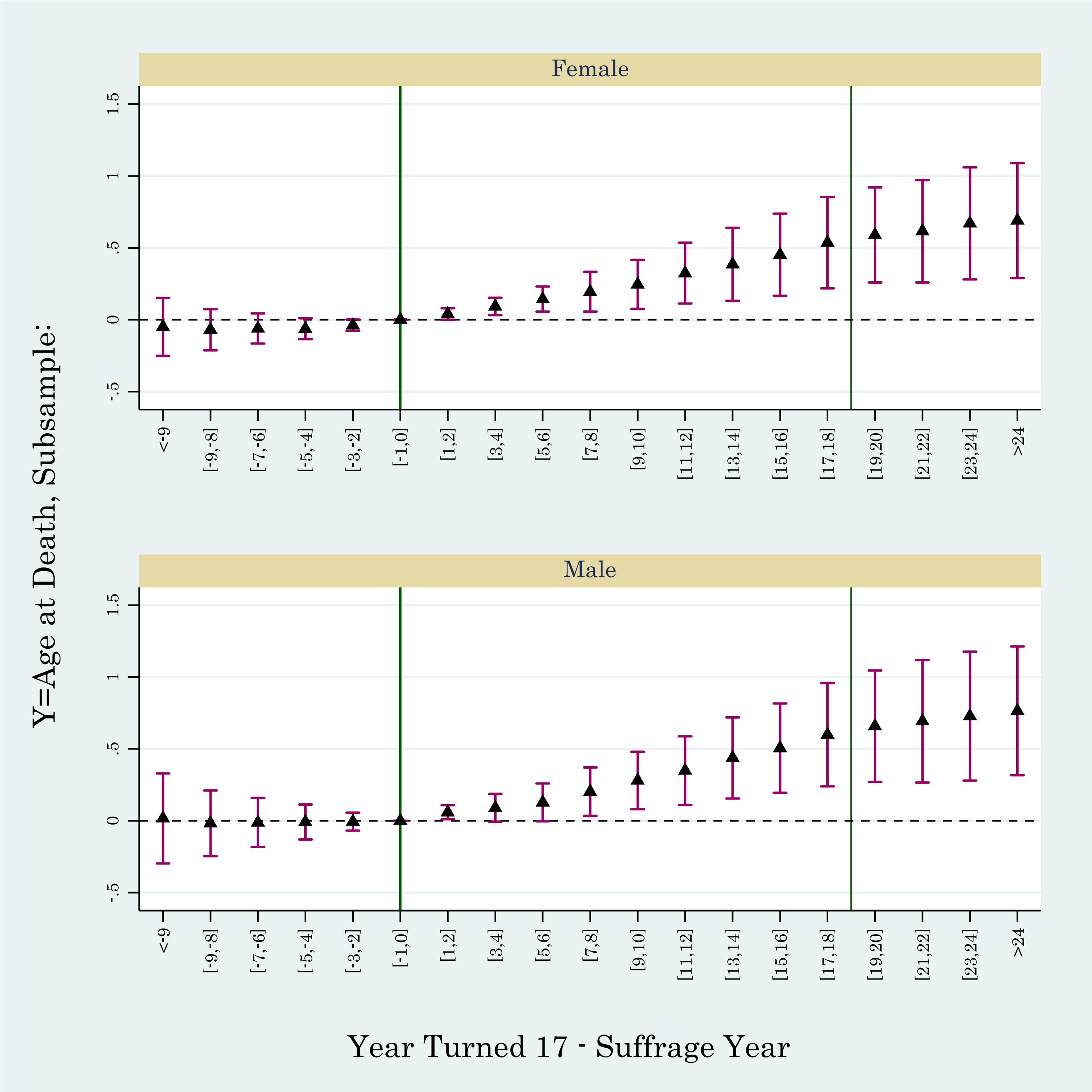

FIGURE C4 Event-study to explore the heterogeneity of the association between childhood exposure to suffrage laws and old-age longevity using NCHS mortality data among Males and Females. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. Standard errors are clustered at the state level. The data covers the years 1979–2020 for cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

APPENDIX D

In this appendix, we show additional robustness checks. First, we show that the event-study estimate is relatively robust to excluding the set of region-cohort dummies. These results are reported in Appendix Figure D1. It seems that including region-cohort dummies help control for preexisting trends in longevity, although in the current event study, all the pre-trend coefficients are statistically insignificant. We also add a state-of-birth-by-birth-cohort linear trend and replicate the event-study in Appendix Figure D2. All pre-trend coefficients are economically and statistically zero. The effects start to rise for partially exposed cohorts and become relatively stable for fully exposed cohorts, a similar pattern and similar coefficients as observed in Figure 3.

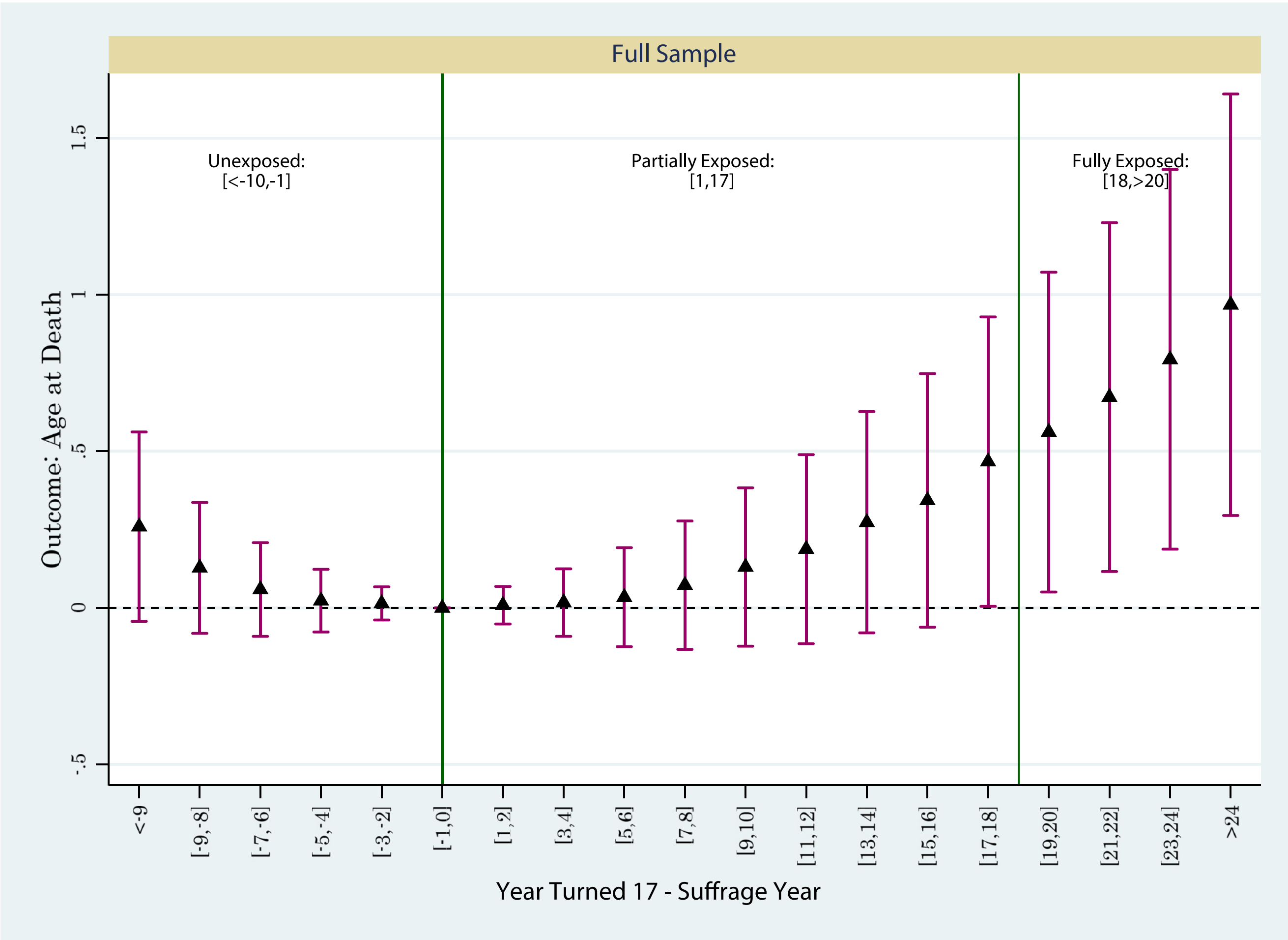

FIGURE D1 Event-study to explore the association between childhood exposure to suffrage laws and old-age longevity using NCHS mortality data, excluding region-cohort fixed effects. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects and birth-year fixed effects. The regression also includes a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. Standard errors are clustered at the state level. The data covers the years 1979–2020 for cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

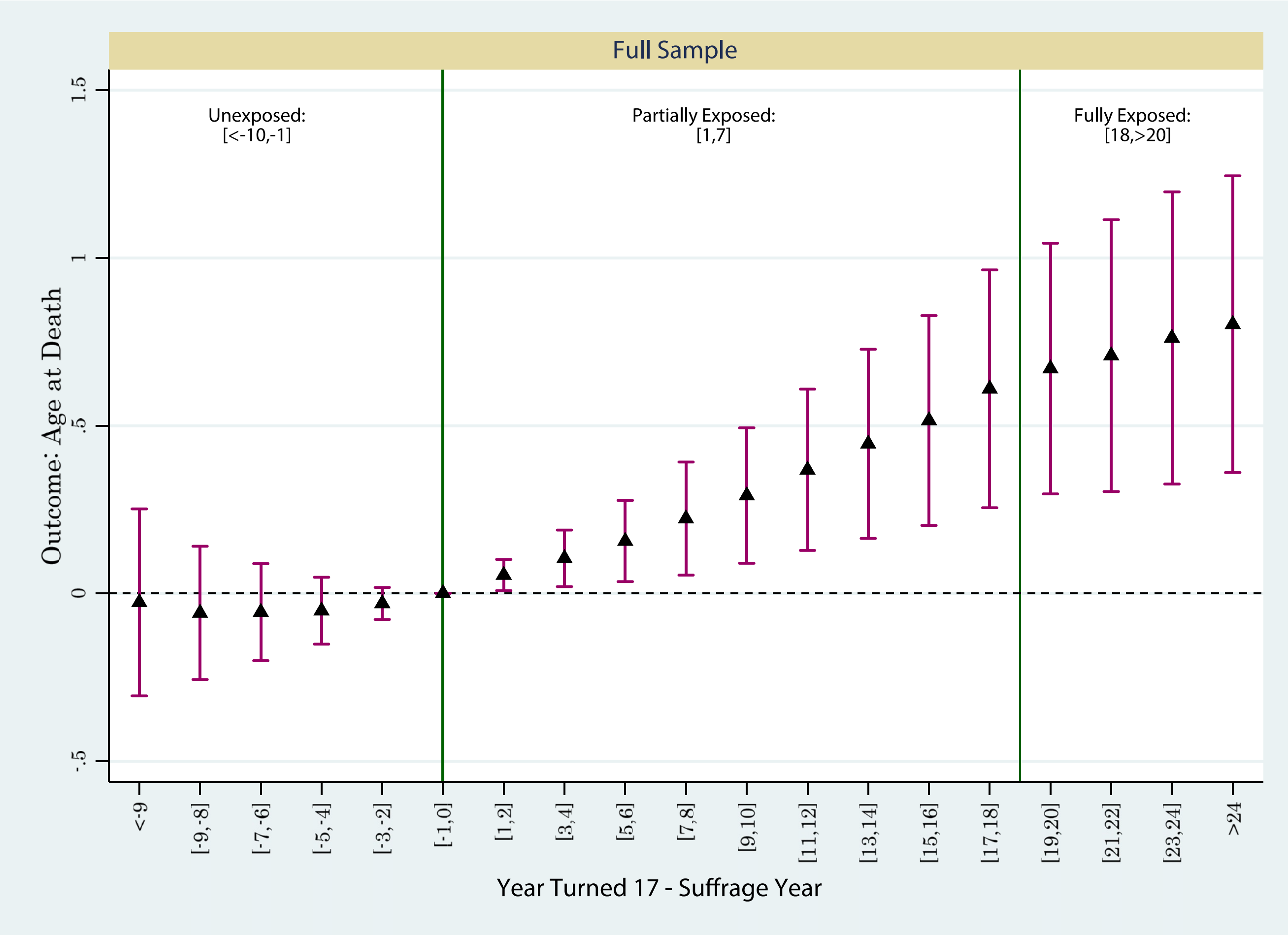

FIGURE D2 Event-study to explore the association between childhood exposure to suffrage laws and old-age longevity using NCHS mortality data, including birth-state by birth year trend. Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year -by-birth-region fixed effects, and birth-state by birth year linear trend. The regression also includes a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. Standard errors are clustered at the state level. The data covers the years 1979–2020 for cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

APPENDIX E

Appendix Figure E1 shows the bacon-decomposition of the 2-by-2 difference-in-difference comparisons. We should note that in this figure, the sample is collapsed at the birth-year and state-of-birth level, and comparisons lack any controls (a restriction imposed by the bacon decomposition command). The overall DD coefficient is 0.13 additional life years. The overall DD coefficient in earlier treated versus later control (treatment = early adopters; control = later adopters) is 0.4 with a weight (in calculating overall DD) of 0.3. The DD effect of later treatment and earlier control (treatment = later adopters; control = earlier adopters) is 0.2 with a weight of 0.4. The effects are so far consistent with a positive impact across comparison groups. The only contamination appears in the comparison set of treated versus already treated. It provides an overall effect of −0.3 with a weight of 0.27. We believe that this is contamination in OLS-produced DD effects throughout the paper since the already treated states (early suffrage adopters) are in a distorted and different trajectory, and it does not offer a well-behaved control group that satisfies the exogeneity criteria. Therefore, we believe that the true effects could be even larger than those reported in the text.

To further check for validity of OLS estimates of the paper, we also implement difference-in-difference method of Sun and Abraham (2021). The results, reported in Appendix Figure E2, reveal a very similar pattern as the event-study of Figure 5. Further, we also show the event-study using the technique developed by de Chaisemartin & D'Haultfœuille (2020) in Appendix Figure E3.

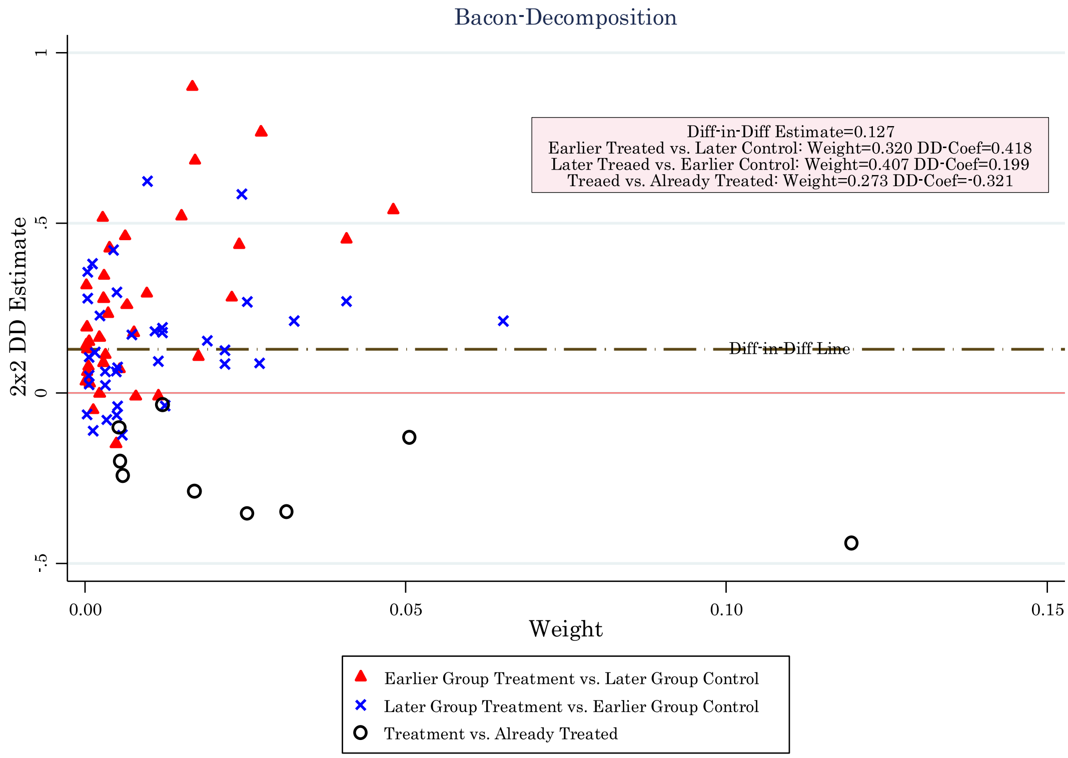

FIGURE E1 Bacon decomposition of the association between childhood exposure to suffrage laws and old-age longevity using NCHS mortality data. [Colour figure can be viewed at wileyonlinelibrary.com]

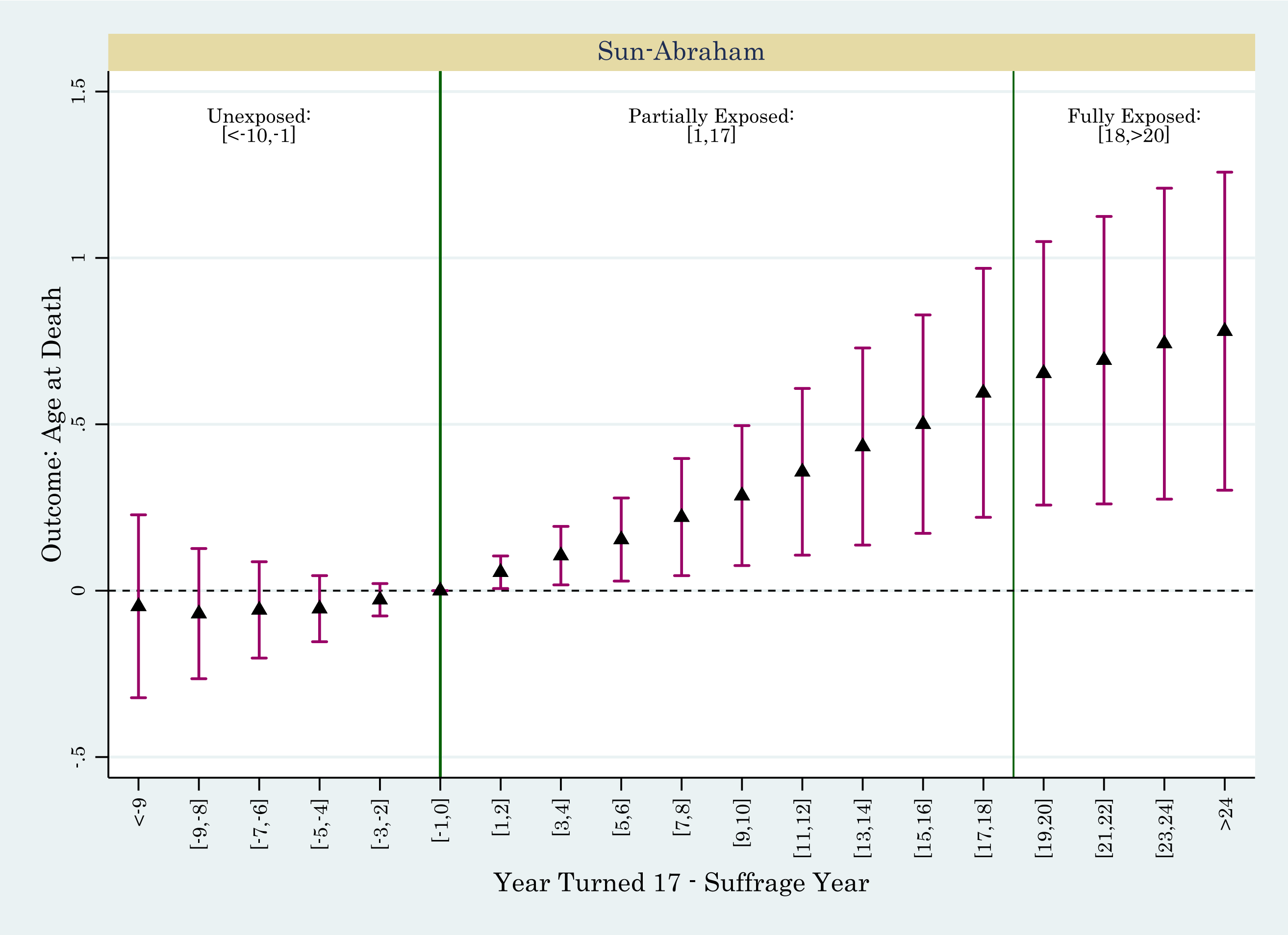

FIGURE E2 Event-study to explore the association between childhood exposure to suffrage laws and old-age longevity using NCHS mortality data and implementing Sun and Abraham (2021) estimates. [Colour figure can be viewed at wileyonlinelibrary.com]

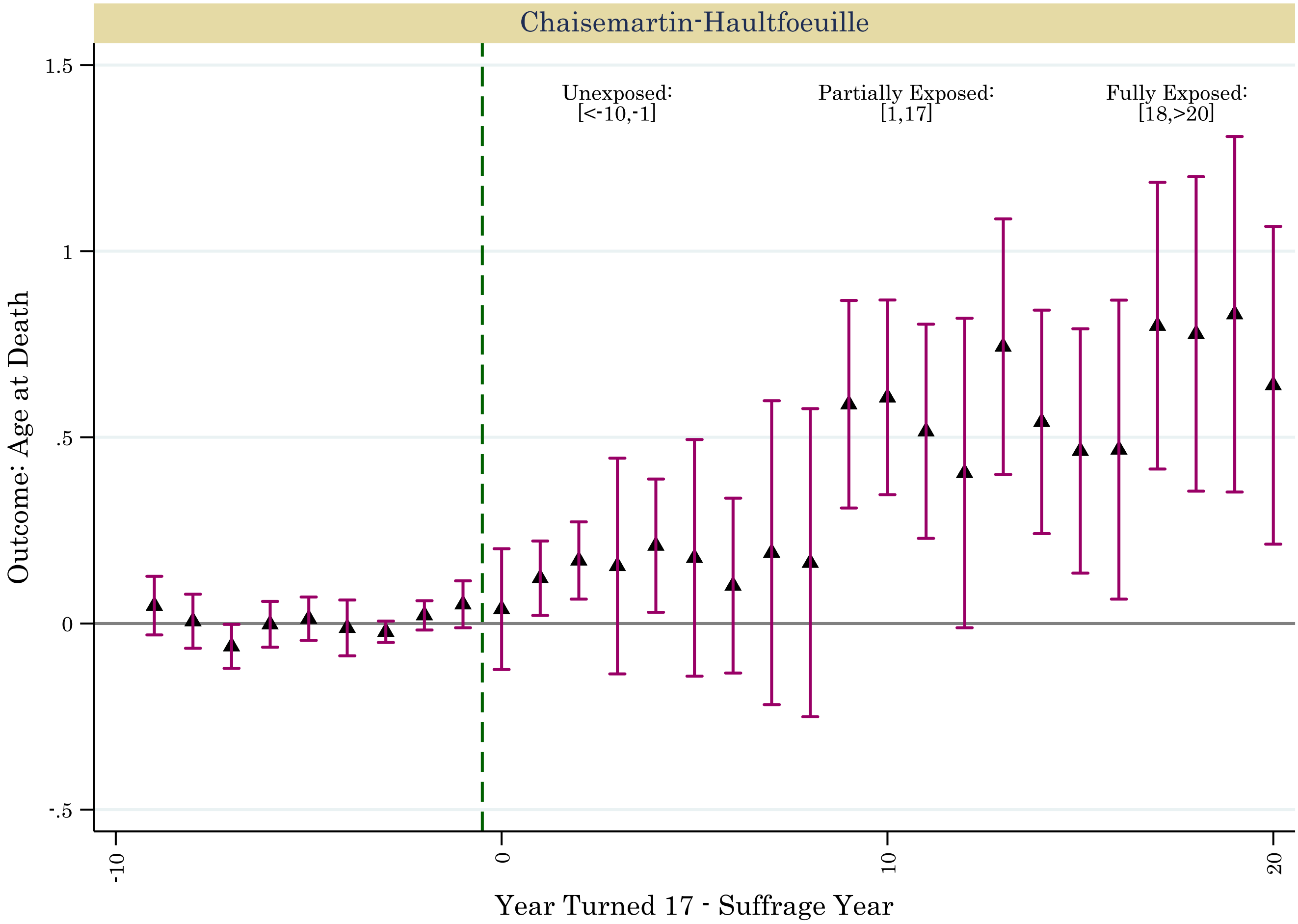

FIGURE E3 Event-study to explore the association between childhood exposure to suffrage laws and old-age longevity in NCHS data using the approach of de Chaisemartin and D'Haultfœuille (2020). Notes. Point estimates and 90 percent confidence intervals are illustrated. The regression includes birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. Standard errors are clustered at the state level. The data covers the years 1979–2020 for cohorts born in years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

APPENDIX F

One concern in the main analysis of the paper is the truncation issue as the NCHS data reports state-of-death for post-1979 death years. In this appendix, we explore the issues related to truncation. We implement four sets of analyses to understand the problems and the degree of this issue, explained below.

One concern in the main results is the right truncation as our death data ends in 2020, the last year data was available at the time of the study. We address this issue by using a more restricted cohorts for whom there are fewer people survived to 2020. We focus on a subsample of cohorts born between 1880–1920 and 1880–1915. The issue with this analysis is that we lose variations of suffrage laws. Specifically, we eliminate those that were exposed to the federal act of 1920. However, the results provide an insight on to what extent truncation is problematic. We report the results in Appendix Table F1. We observe reductions in the magnitude of the marginal effects for both subsamples. This reduction could be due to restricting the suffrage variations or due to right truncation. However, the effects are still economically meaningful and statistically significant.

As a second step, we explore to what degree including earlier deaths could bias the estimates. In so doing, we proxy state-of-birth with state-of-death. We should note that this is an unrealistic assumption specifically given the evidence of Table 7 that provides a relationship between suffrage exposure and migration. Nonetheless, using this proxy allows us to employ mortality data starting from the year 1960, covering 2 decades of earlier deaths. The results are reported in panel A of Appendix Table F2. In column 1, we replicate the results of column 2 of Table 2 as the benchmark comparison. In column 2, we use death records of 1979–2020 (similar to column 1) but use death-state as a proxy for birth-state. The marginal effect is drops by about 21% relative to column 1. In column 3, we employ all death data from 1960–2020.[12] Relative to column 2, the marginal effect drops by about 20%. Since the second column (using death-state proxy) likely underestimate the effects, we expect to observe a larger impact for column 3 if we had access to birth-state for the 1960–1978 period. This leaves a small over-estimation in column 1. However, the estimated effect would still be economically meaningful.

During the late 19th century, life expectancy at birth lied below 50 years. Therefore, those who were born before 1900 must live well beyond the average expected longevity of their cohorts to reach the NCHS death data. This fact leaves concerns as to what extent they are representative of their original population and to what extent that might affect truncation issue. To address this, we replicate the exercise of panel A of Appendix Table F2 and restrict the cohorts to those born between 1890 and 1940. These results are reported in panel B of Appendix Table F2. We observe smaller changes across columns. Next, we replicate these results for birth cohorts of 1900–1940 and report the results in panel C of Appendix Table F2. On the contrary to previous tables, we observe an increase of about 60% as we move to the second column (using death-state proxy). Compared with the second column, the third column (deaths of 1960–2020) provides an increase of about 31% in the marginal effect. Using this cross-column comparisons, we can do a back-of-an-envelope calculation and estimate an effect of about 0.4.[13] This is comparable to the main results and remains economically significant.

Fourth, we implement a simulation using a randomly generated fake data. The purpose of this simulation is to have a sample of death data that covers all years from birth to death for all cohorts. We then use this comprehensive data to explore what happens when we truncate the sample. We start by generating a random sample of 10M observations. We randomly generate birth cohort variable that varies between 1880 and 1940. Each cohort is assigned the average life expectancy of that cohort (extracted from O'Neill (2021)) in addition to a random error term. We create a random variable for birth state and use the birth-state-year information to detect exposure to suffrage. For the fully exposed people, we assign a treatment of one. For the partially exposed people, we assign a treatment equivalent to the share of years of exposure. We then implement regressions similar to the main results of the paper. These results are reported in panel A of Appendix Table F3. As expected, the full sample suggests an effect of 1 year of longevity (column 1). Across the following columns, we restrict the sample to those died after 1940, 1950, 1960, 1970, 1980, and 1990 (columns 2–7). We observe reductions in the effects until column 6. Column 7 breaks this trend and suggest slight rises in the magnitude compared with column 6. Comparing column 6 (death years similar to the final sample of the paper) and column 1, one can deduce considerable reductions due to truncation. Therefore, our results might underestimate the true effects and we may observe larger longevity impacts had we had access to the full mortality history of individuals. We observe similar patterns as use a 2-units and a 0.5-units of treatment effects in panels B and C of Appendix Table F3.

TABLE F1 Replicating the main results using a restricted cohort sample.

	Outcome: Age at death (Years)	
	Birth cohorts of 1880–1920	Birth cohorts of 1880–1915
	(1)	(2)
Share of exposure	0.35413***	0.41064**
	(0.12105)	(0.17122)
Observations	32,631,859	23,277,034
R-squared	0.19683	0.21764
Mean DV	83.163	84.396
%Change	0.426	0.487
Birth state FE	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓
Controls	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE F2 Exploring the left truncation issue using earlier death records with death-state as a proxy for birth-state.

	Outcome: Age at Death (Years)		
	Column 2 Table 2	Death records 1979–2020 with death-state as the proxy for birth-state	Death records 1960–2020 with death-state as the proxy for birth-state
	(1)	(2)	(3)
Panel A. Cohorts of 1880–1940			
Share of exposure	0.56683***	0.44737	0.35027
	(0.20648)	(0.50448)	(0.51684)
Observations	64,845,601	64,806,127	89,802,453
R-squared	0.26734	0.27769	0.15901
Mean DV	79.186	79.183	76.381
%Change	0.716	0.565	0.459
Panel B. Cohorts of 1890–1940			
Share of exposure	0.56134***	0.46026	0.3982
	(0.20713)	(0.49038)	(0.45498)
Observations	64,322,773	64,291,946	84,229,125
R-squared	0.25343	0.26425	0.13346
Mean DV	79.052	79.051	75.813
%Change	0.710	0.582	0.525
Panel C. Cohorts of 1900–1940			
Share of exposure	0.5288**	0.8404**	1.08971**
	(0.22102)	(0.40243)	(0.40935)
Observations	60,687,934	60,678,946	73,392,660
R-squared	0.20965	0.22149	0.11559
Mean DV	78.422	78.424	75.101
%Change	0.674	1.072	1.451
Birth state FE	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓
Controls	✓	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE F3 The results of simulation analysis using a 1-unit artificially induced treatment effect to explore the truncation issue.

	Outcome: Age at Death (Years)						
	Full sample	Death year ≥ 1940	Death year ≥ 1950	Death year ≥ 1960	Death year ≥ 1970	Death year ≥ 1980	Death year ≥ 1990
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Using a 1-unit artificially induced treatment effect							
Share of exposure	0.98319***	0.81774***	0.61341***	0.40635***	0.30495***	0.27469***	0.44921***
	(0.05633)	(0.05394)	(0.06112)	(0.05984)	(0.04371)	(0.08986)	(0.14809)
Observations	10,000,000	9,612,234	9,002,742	7,922,401	6,330,758	4,417,274	2,531,570
R-squared	0.17868	0.12415	0.08654	0.05256	0.035	0.06359	0.14154
Mean DV	55.689	56.497	57.458	58.953	61.058	63.770	67.310
%Change	1.766	1.447	1.068	0.689	0.499	0.305	0.667

(Continues)

TABLE F3 (Continued)

	Outcome: Age at Death (Years)						
	Full sample	Death year ≥ 1940	Death year ≥ 1950	Death year ≥ 1960	Death year ≥ 1970	Death year ≥ 1980	Death year ≥ 1990
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B. Using a 2-unit artificially induced treatment effect							
Share of exposure	2.08082***	1.77888***	1.36622***	0.95935***	0.64726***	0.63459***	0.34203*
	(0.03538)	(0.03692)	(0.03682)	(0.05448)	(0.03959)	(0.06682)	(0.19091)
Observations	10,000,000	9,616,688	9,024,603	7,991,375	6,468,403	4,600,052	2,706,695
R-squared	0.19795	0.13921	0.09619	0.05615	0.03255	0.05482	0.12958
Mean DV	56.461	57.287	58.251	59.725	61.785	64.449	67.899
%Change	3.685	3.105	2.345	1.606	1.048	0.985	0.504
Panel C. Using a 0.5-unit artificially induced treatment effect							
Share of exposure	0.48319***	0.39009***	0.27226***	0.15961***	0.16604***	0.14598**	0.28898**
	(0.05633)	(0.05269)	(0.05593)	(0.05448)	(0.04211)	(0.07382)	(0.12931)
Observations	10,000,000	9,609,832	8,990,791	7,884,478	6,259,245	4,325,640	2,445,796
R-squared	0.16942	0.11709	0.08224	0.05121	0.03673	0.06826	0.1474
Mean DV	55.305	56.105	57.064	58.574	60.700	63.438	67.022
%Change	0.874	0.695	0.477	0.272	0.274	0.183	0.431
Birth state FE	✓	✓	✓	✓	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

APPENDIX G

From column 1–2 of Table 2, we observe a jump of about 64% as we add controls to our regressions. In this appendix, we slightly add controls to our regressions. The results are reported in Appendix Table G1. We observe a jump from column 6–7, when we add the state-level share of blue-collar workers, and from column 7–8, as we add female literacy rate.

TABLE G1 Exploring the main results by adding incremental covariates.

	Outcome: Age at death (Years)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Share of exposure	0.34425**	0.32494*	0.31585*	0.34011**	0.34136**	0.33366**	0.42712**	0.58312***	0.58112***	0.59732***
	(0.16862)	(0.18258)	(0.17825)	(0.161)	(0.15817)	(0.15697)	(0.1875)	(0.17941)	(0.17674)	(0.20721)
Observations	64,845,601	64,845,601	64,845,601	64,845,601	64,845,601	64,845,601	64,845,601	64,845,601	64,845,601	64,845,601
R-squared	0.2425	0.26456	0.2673	0.2425	0.2425	0.2425	0.24251	0.24255	0.24255	0.24255
Mean DV	79.186	79.186	79.186	79.186	79.186	79.186	79.186	79.186	79.186	79.186
%Change	0.435	0.410	0.399	0.430	0.431	0.421	0.539	0.736	0.734	0.754
Birth state FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls:										
Female		✓	✓	✓	✓	✓	✓	✓	✓	✓
Nonwhite			✓	✓	✓	✓	✓	✓	✓	✓
County covariates:										
Socioeconomic index				✓	✓	✓	✓	✓	✓	✓
Share of white-collar occupations					✓	✓	✓	✓	✓	✓
Share of farmers						✓	✓	✓	✓	✓
Share of blue-collar occupations							✓	✓	✓	✓
Female literacy rate								✓	✓	✓
Female labor force participation rate									✓	✓
Number of children <5										✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

APPENDIX H

In Appendix Table H1, we implement a placebo test and assign treatment to subpopulations that were potentially unaffected by suffrage laws. For instance, in column 1, we assign treatment to groups that were 19–20 years old and use individuals aged 21–23 as the control group. We observe small and insignificant coefficients which further lends to the validity of the main results.

APPENDIX I

In Appendix Table I1, we explore the sensitivity of the main results to alternative model specifications. In column 1, we replicate the full specification results of column 2 of Table 2 as our benchmark comparison. In column 2, we add to column 1 a series of interactions between state-of-birth-by-gender and state-of-birth-by-race dummies to allow for the state effects to vary for each subpopulation. The coefficient remains virtually constant.

In column 3, we absorb all state-level observable and unobservable characteristics that evolve linearly across cohorts. Adding a state-of-birth-by-birth-year linear trend drops the magnitude of the effect by roughly 32%, while the marginal effect is still significant at 5% level. In the main analysis, we avoid using a linear trend due to the concerns raised in the literature about unit-specific trends, the fact that it may over-control the model, and that it may generate artificial variations in the treatment variable (Chou et al., 2006; Goodman-Bacon, 2021a; Gruber & Frakes, 2006; Meer & West, 2016).

In the main analysis, we avoid controlling for state-of-death as the choice of state later in life can also be determined by early life events and is an endogenous control (Xu et al., 2020). In column 4, we show that controlling for death-state fixed effects does not alter the magnitude or statistical significance of the coefficient.

Several studies suggest seasonality in health and mortality (Marti-Soler et al., 2014; Vaiserman, 2021). To account for the confounding influence of this seasonal pattern, column 5 adds month-of-death fixed effects. The effect is almost identical to that of column 1.

TABLE H1 Placebo tests: Exposure to suffrage laws among potentially unaffected cohorts.

	Outcome: Age at death (Years)			
	Suffrage age 19–20 VS 21–23	**Suffrage age 24–26 VS 27–29**	**Suffrage age 30–32 VS 33–35**	**Suffrage age 35–36 VS 37–40**
	(1)	**(2)**	**(3)**	**(4)**
Share of exposure	−0.01159	−0.01251	−0.0048	0.02229
	(0.01914)	(0.01292)	(0.03225)	(0.09203)
Observations	2,494,734	1,469,923	417,821	89,343
R-squared	0.09702	0.13194	0.18291	0.20715
Mean DV	88.078	91.172	95.046	98.624
%Change	−0.013	−0.014	−0.005	0.023
Birth state FE	✓	✓	✓	✓
Region-of-Birth-by-Birth-Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Note: Standard errors, clustered at the birth-state level, are reported in parentheses. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

In column 6, we add a wide array of additional birth-state-level controls, including prohibition law dummies, the share of dry counties, birth registration law dummies, poll tax dummies, literacy test dummies, the share of people in different age groups, the share of immigrants, the share of homeowners, average occupational income score, and share of married women. The effect drops in magnitude but remains economically and statistically significant.

To check the sensitivity of the functional form to the linear outcome, we replace the outcome with the log of age at death. The result is reported in column 7. It suggests that fully exposed cohorts (relative to non-exposed cohorts) have a 0.8% higher age at death. This is almost identical to the percentage change from the mean reported in row 5 of column 2 of Table 2. In column 8, we replace the outcome with a binary indicator that equals one if the individual lived beyond age 75 and zero otherwise. We find that a full exposure (exposure of 1 vs. 0) is associated with a 1.6 percentage-points higher probability of living past age 75, off a mean of 0.61.

Finally, while we cluster standard errors at the birth-state level in column 1, we show that the estimated standard error is considerably smaller if we use the Huber-White robust method (column 9). Moreover, the standard errors are smaller if we cluster them at the region-cohort and birth-state level (two-way clustering, column 10). Thus, the birth-state level is a relatively more conservative level of clustering of standard errors.

TABLE I1 Robustness checks across specifications.

	Column 2 Table 2	Adding birth-state by Race/Gender FE	Adding birth-state by birth-year linear trend	Adding death state FE	Adding month of death FE
	(1)	(2)	(3)	(4)	(5)
Share of exposure	0.56683***	0.55902***	0.38653**	0.57263***	0.5666***
	(0.20648)	(0.20522)	(0.15429)	(0.20125)	(0.20653)
Observations	64,845,601	64,845,601	64,845,601	64,838,585	64,845,601
R-squared	0.26734	0.26788	0.26738	0.26979	0.26747
	Adding more state controls	**Outcome in log**	**Outcome: Age at death >75**	**Huber-white robust SE**	**Two-way clustering SE at birth-state and region-by-birth-year level**
	(6)	(7)	(8)	(9)	(10)
Share of exposure	0.38472**	0.00786***	0.01571**	0.56683***	0.56683***
	(0.14371)	(0.00283)	(0.00773)	(0.06012)	(0.02269)
Observations	64,845,601	64,845,601	64,845,601	64,845,601	64,845,601
R-squared	0.26735	0.25963	0.16481	0.26734	0.26734

Note: Standard errors, clustered at the birth-state level (except for columns 9–10), are reported in parentheses. All regressions include birth state fixed effects, region-of-birth by birth-year fixed effects, and a full set of controls. Controls include a dummy for gender, a dummy for race, and average birth-state-by-birth-year covariates including female literacy rate, female labor force participation rate, average socioeconomic index, average number of children under 5 years old, and share of workers in different occupations. Column 6 adds more birth-state-level covariates, including prohibition law dummies, share of dry counties, birth registration law dummies, poll tax dummies, literacy test dummies, share of people in different age groups, share of immigrants, share of homeowners, average occupational income score, and share of married women. The outcome in all regressions (except columns 7–8) is age at death (in years).

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

APPENDIX J

In this appendix, we examine changes in state-level socioeconomic and demographic characteristics before and after suffrage law movements. We argue four threats of endogeneity that these changes might pose to our identification and address each one below.

First, suffrage reforms could be accompanied by other state-level law changes that marginally affect later-life health outcomes. For instance, several studies show that state-level alcohol consumption ban during prohibition movements was associated with improvements in later-life education and health (Evans et al., 2016; Jacks et al., 2021; Law & Marks, 2020). Other studies point to the effectiveness of state entrance into the birth registration areas to enforce child labor laws and consequent improvements in educational outcomes (Fagernäs, 2014). In order to explore whether the suffrage reforms followed or were proceeded with other potential law changes, we implement a series of event-study analyses in which the event is the suffrage law reform, and event-time is the distance (in year groups) from the year of the law change.[14] These results are depicted in four panels of Appendix Figure J1. There is no discernible pattern that suffrage laws were enacted following other laws or followed other changes, specifically prohibition reforms, the share of dry counties, the enactment of birth registration law, and the introduction of poll taxes. All point estimates are indistinguishable from zero.

Another concern is that certain subpopulations may value suffrage laws in a way to move to states which enacted the law earlier. Similarly, people may interpret suffrage laws as predictors of upcoming changes and incipient social and economic movements. For instance, blacks may observe suffrage as a step toward a more socially equal society and move to states that pass the law. Since blacks have lower longevity for reasons that cannot be simply absorbed by race dummies, the estimated effects of Equation (1) are biased. To explore this endogeneity issue, we implement event-study analyses to evaluate the decennial evolution of demographic features as a response to suffrage reforms. Specifically, we regress the decennial census (1880–1940) share of state-level people in each race/ethnicity on a dummy indicating the passage of suffrage. The results are reported in four panels of Appendix Figure J2. As the small and insignificant event coefficients suggest, there is no significant change in the share of whites, blacks, and Hispanics during pre-post suffrage years.

The third concern is related to changes in the share of females, family structure, and endogenous fertility. For instance, if states with earlier adoption of suffrage attract single mothers, the coefficients of Equation (1) may underestimate the true effects, as single parenthood could also be associated with lower health endowment and lower later-life longevity.[15] However, Appendix Figure J3 provides no evidence that suffrage reforms were accompanied by changes in the share of females, married women, the percentage of households with a child less than 5 years old, and the total number of children in the family.

Fourth, the reforms may follow structural social and economic changes that can also be observed in economic variables. For instance, children born in early-adopter states may also experience improvements in economic conditions and reveal improvements in longevity as a result of the latter rather than the former. To explore this, we look at the decennial census changes in the average socioeconomic index, the average occupational prestigious score, the share of white-collar employees, and the percentage of farmers. The results of event-studies, reported in four panels of Appendix Figure J4, reveal no pattern of any pre-post changes in these outcomes due to the suffrage law change.

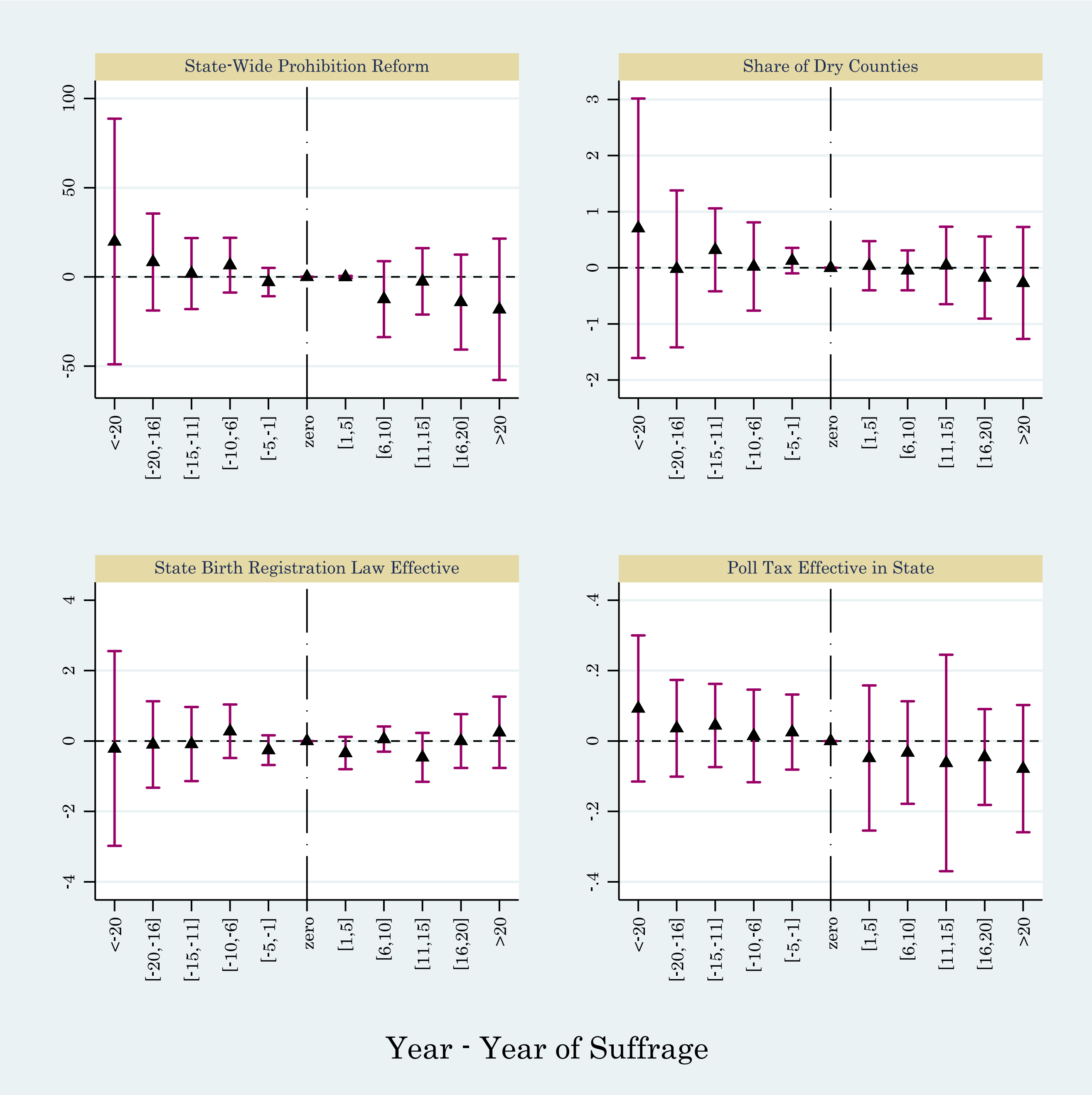

FIGURE J1 Event-study to explore the endogenous changes in state-level characteristics before and after the suffrage reforms. Notes. Point estimates and 90 percent confidence intervals are illustrated. Outcomes are shown in each panel's title. The event-time is the distance between each census year (in which the outcome is observed) and the year of suffrage reform. All regressions include state fixed effects, year fixed effects, and region-by-year fixed effects. Standard errors are clustered at the state level. The data covers the decennial years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

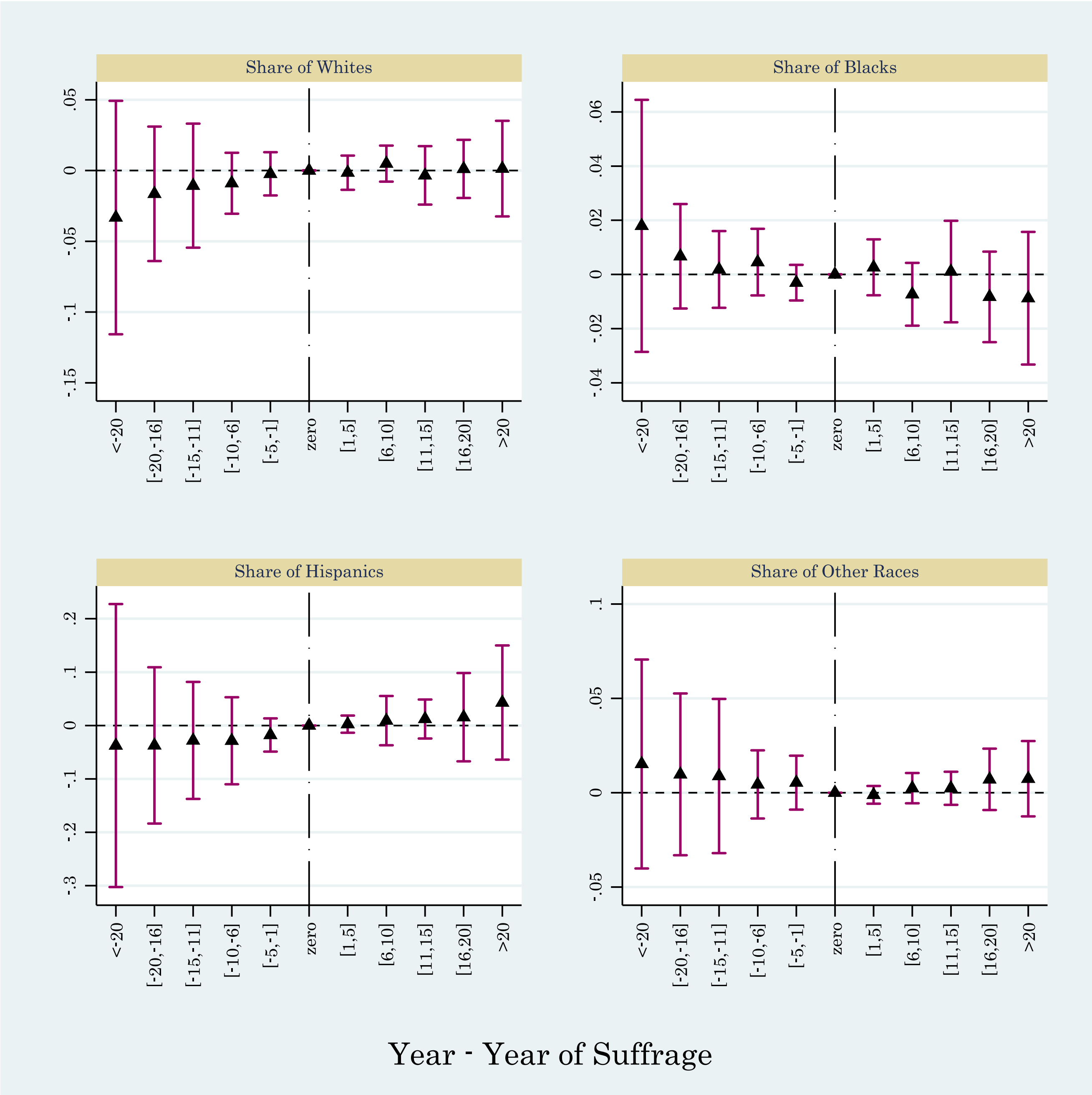

FIGURE J2 Event-study to explore the endogenous changes in state-level characteristics before and after the suffrage reforms. Notes. Point estimates and 90 percent confidence intervals are illustrated. Outcomes are shown in each panel's title. The event-time is the distance between each census year (in which the outcome is observed) and the year of suffrage reform. All regressions include state fixed effects, year fixed effects, and region-by-year fixed effects. Standard errors are clustered at the state level. The data covers the decennial years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

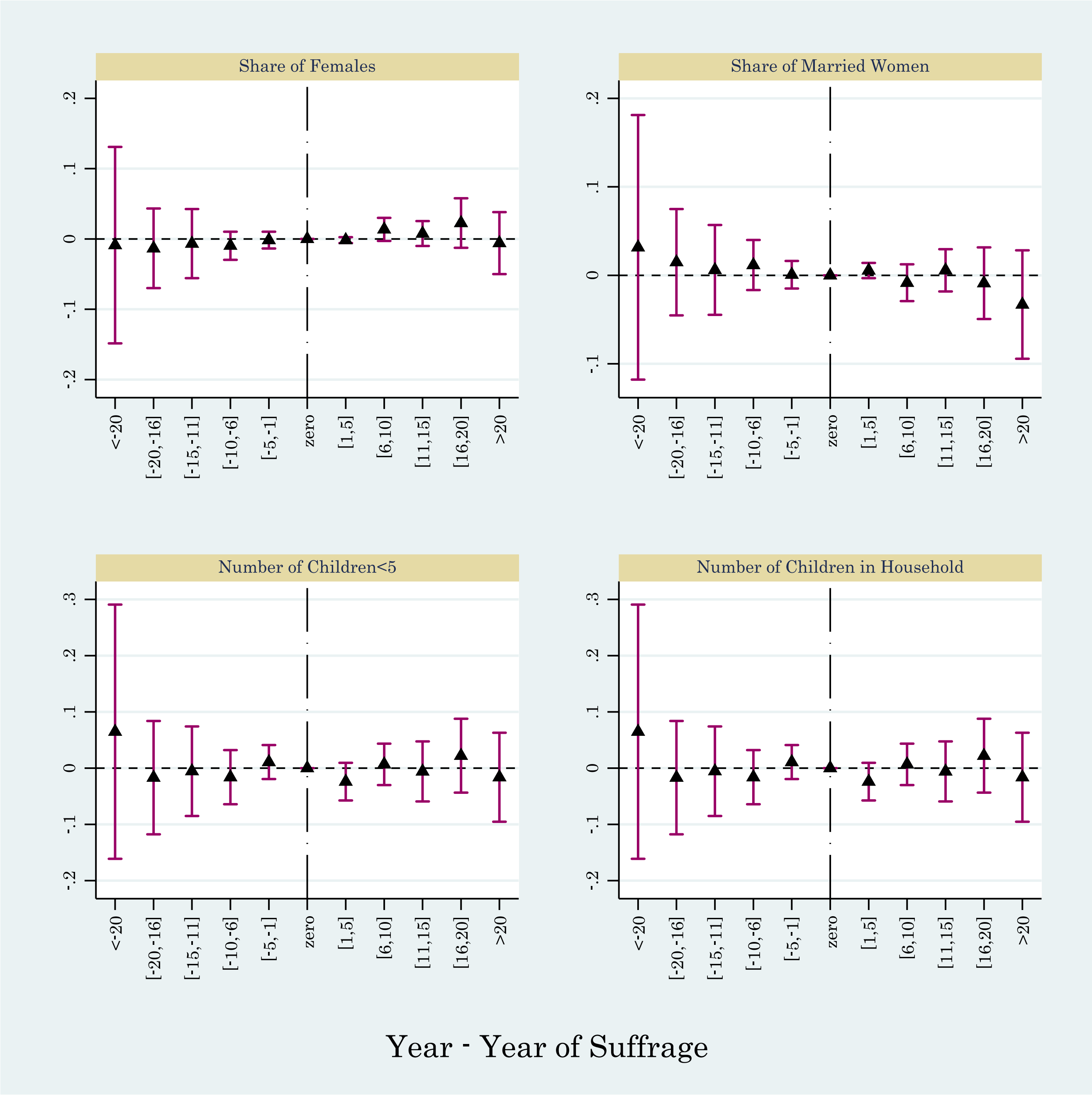

FIGURE J3 Event-study to explore the endogenous changes in state-level characteristics before and after the suffrage reforms. Notes. Point estimates and 90 percent confidence intervals are illustrated. Outcomes are shown in each panel's title. The event-time is the distance between each census year (in which the outcome is observed) and the year of suffrage reform. All regressions include state fixed effects, year fixed effects, and region-by-year fixed effects. Standard errors are clustered at the state level. The data covers the decennial years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]

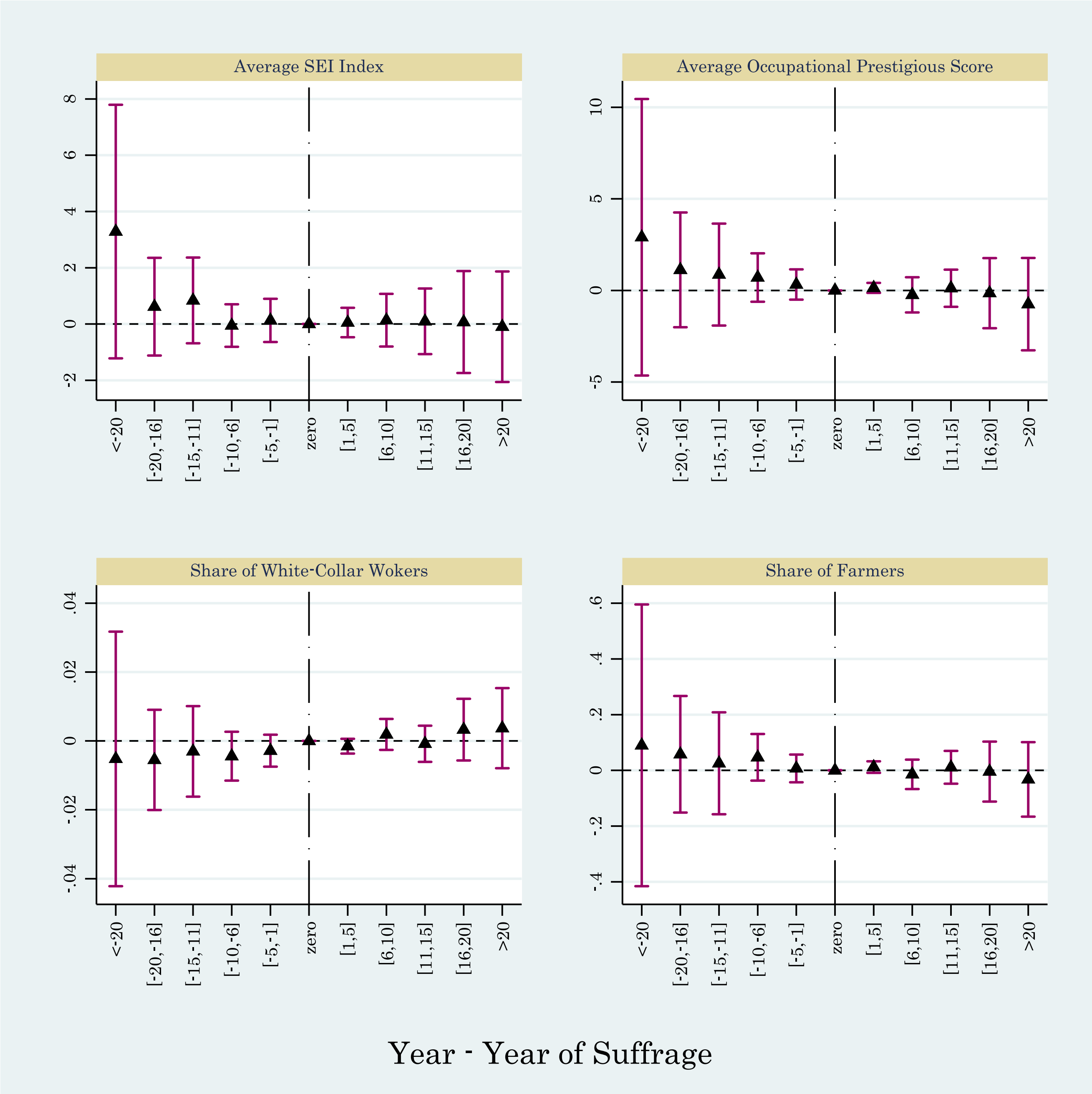

FIGURE J4 Event-study to explore the endogenous changes in state-level characteristics before and after the suffrage reforms, Notes. Point estimates and 90 percent confidence intervals are illustrated. Outcomes are shown in each panel's title. The event-time is the distance between each census year (in which the outcome is observed) and the year of suffrage reform. All regressions include state fixed effects, year fixed effects, and region-by-year fixed effects. Standard errors are clustered at the state level. The data covers the decennial years 1880–1940. [Colour figure can be viewed at wileyonlinelibrary.com]