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SHORT NOTE

Social externalities of women empowerment: Evidence from suffrage movements of late nineteenth and early twentieth century United States

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Abstract

Previous literature suggests that empowering women is associated with children's improved outcomes. However, little is known about its effects on children's later-life crime and incarceration. We argue that women empowerment through suffrage law changes during the late nineteenth and early twentieth centuries in the US generated incentives for women to invest in their children's human capital. The accumulated human capital then has the potential to reduce future incarceration of children. We use full-count censuses 1920–1930, implement a difference-in-difference framework, and empirically show that childhood exposure to suffrage laws is associated with considerable reductions in incarceration. The effects appear to be primarily driven by decreases in male and Black incarceration. The balancing tests rule out the concern that the effects are driven by demographic compositional changes or endogenous changes in other state-level characteristics. Furthermore, an event-study analysis rejects the concern that the effects are driven by preexisting trends in incarceration among exposed cohorts. The findings of this research note offer informative implications for overlooked externalities of women empowerment in a historical setting.

KEYWORDS
crime, externality, historical data, incarceration, political economy, racial gap, suffrage, women empowerment

JEL CLASSIFICATION
J18, K42, N41, N42, P48

1 | INTRODUCTION

Several streams of research in various settings document that empowering women through conventional legal system channels such as suffrage reforms or community behavior channels leads to higher investment in children and improvement in their outcomes (Bozzano, 2017; Duflo, 2012; Homan, 2017; Kose et al., 2021; Nobles et al., 2010). For instance, Kose et al. (2021) exploit the space–time variations in suffrage laws in the late nineteenth and early twentieth century United States and show that children exposed to suffrage reveal improvements in education and future labor market outcomes. They argue that a large portion of observed improvements can be explained by suffrage-induced growth in public school spending. Carruthers and Wanamaker (2015) show that female voter enfranchisement during the early part of the twentieth century was responsible for about one third of increases in public education spending between 1920 and 1940. Furthermore, several strands of studies document positive relationships between political empowerment of women and children's health outcomes, including immunization status (Swiss et al., 2012), child nutritional status (Santoso et al., 2019), and child mortality rates (Miller, 2008; Moehling & Thomasson, 2012).

On the other hand, studies show that childhood conditions are important determinants of later-life outcomes (Almond et al., 2018; Case et al., 2005; Currie, 2009; Goodman-Bacon, 2021b; Hayward & Gorman, 2004; Jayachandran & Lleras-Muney, 2009). For instance, there is evidence that childhood health endowment, human capital accumulation, and education can influence criminal behavior and determine incarceration outcomes (Akee et al., 2014; Åslund et al., 2018; Campaniello et al., 2016; Chalfin & Deza, 2019; Cook & Kang, 2016; Fella & Gallipoli, 2014; Garces et al., 2002; He & Barkowski, 2020; Lochner & Moretti, 2004; Machin et al., 2011; Noghanibehambari & Tavassoli, 2022; Wen et al., 2017). For instance, Bailey et al. (2019) explore the effects of the Head Start program and public school spending during K-12 education on later-life outcomes. They find that Head Start is associated with increases in education and earnings and reductions in incarceration. Moreover, the effects of Head Start are larger when they are accompanied by rises in K-12 school spending. Noghanibehambari (2022) explores the direct link between public school spending and juvenile crime rates. The author argues that increases in school spending generate incentives among students to finish high school. The higher expected (future) wage premium resulting from higher education increases the opportunity cost of crime and leads to lower juvenile crime rates.

Taking these streams of research together, one would expect that there are potential intergenerational externalities in women empowerment, specifically for later-life criminal behavior. However, the empirical literature is limited in this aspect of the research. This is unfortunate because looking at the long-run effects of political empowerment of women can stimulate more actions aiming at reducing gender inequality at the societal level. As empowering women reverberates through generations, often the greatest gains will not be seen if one does not expand one's viewpoint.

To fill this gap, we explore the association between childhood exposure to suffrage laws as a source of women empowerment that incentivizes mothers to invest more in their children's human capital on later-life incarceration. The suffrage movement in the United States, which started with a women's rights convention in New York in July of 1848 and resulted in the ratification of women's right to vote in 1920, was accompanied by significant exogenous variation in the timing of state-level extension of suffrage rights to women. To test the effects of women empowerment through suffrage laws on later-life incarceration of children, we employ a large dataset

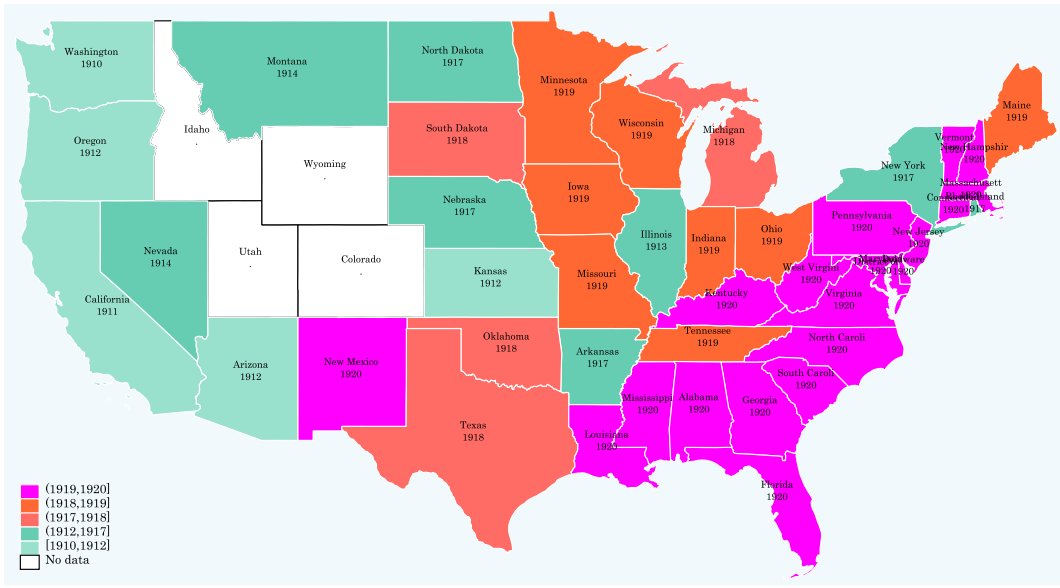

FIGURE 1 Suffrage reform years across states.

covering the population of the United States in the decennial years 1920–1930 and rely on the aforementioned exogeneity of state-year changes in suffrage laws. We implement a difference-in-difference estimation strategy and show that exposure to suffrage during childhood is associated with reductions in incarceration. Moreover, we find that the effects are primarily driven by reductions in the incarceration of males and Black people.

The contribution of this research to the literature is twofold. First, it adds to the literature on women empowerment by documenting the effects on an unexplored and understudied outcome in this literature. Specifically, this study is the first to establish a link between women suffrage laws and children's later-life incarceration. Second, this study contributes to the literature on childhood conditions and future outcomes by showing a link between childhood parental behavior and future incarceration.

2 | ECONOMETRIC METHOD AND DATA SOURCES

To explore the effect of suffrage laws on incarceration, we use the full-count decennial census data of 1920–1930 extracted from Ruggles et al. (2020). We restrict the sample to individuals between 10 and 60 years old.[1] We merge this data based on state-of-birth with the database of suffrage law reforms taken from Kose et al. (2021). We remove four states that adopted the law in the nineteenth century, that is, Idaho, Wyoming, Utah, and Colorado. The outcome of this study is a dummy variable that equals one if the person is reported to reside in a correctional institution and zero otherwise. Figure 1 shows the geographic distribution of suffrage law reform years across states. More discussion on the sample description is provided in Appendix A.

Our empirical analysis relies on cross-states and over-time variations in suffrage laws and follows a difference-in-difference[2] framework as follows:

[1]In Appendix C, we show the robustness of the results to samples that focus on partially treated cohorts, states with full exposure, and alternative age restrictions.

[2]In Appendix D, we show that the results are not driven by differential weighting of OLS-produced estimates by showing the robustness of the event study to the method suggested by Sun and Abraham (2021).

$$y_{isrct} = \beta_0 + \beta_1 ShareExp_{src} + \beta_2 X_{isrc} + \gamma_s + \lambda_{rc} + \theta_t + \epsilon_{isrc} \quad (1)$$

where y is the outcome (being incarcerated) of individual i born in state s region r who belongs to cohort c and observed in year t. The parameters γ, λ, and θ represent birth-state fixed effects, region-of-birth-by-birth-cohort fixed effects, and year fixed effects, respectively. In X, we include as individual covariates dummies for race and gender. The parameter *ShareExp* is the share of one's childhood (between 0 and 17 years of age) that they were exposed to the implementation of suffrage law. Therefore, the parameter β_1 is the coefficient of interest that captures the effect of being fully exposed to suffrage law during childhood (vs no exposure) on the probability of being incarcerated later in life. The standard errors are clustered at the state-of-birth level.

3 | RESULTS

3.1 | Balancing tests

One concern in interpreting the estimated coefficients of equation 1 is suffrage-induced migration issues. For instance, Black people may perceive suffrage as a sign of social change and move to early adopter states. Since Black people have higher incarceration rates (due to unobservable reasons not simply captured by a black dummy), the estimated effects understate the true effects. To explore this source of endogeneity, we implement a series of balancing test type event-study estimations in which the event time is the year individual turns 17 minus the year of suffrage implementation. The results, reported in Figure 2, do not provide evidence to support this concern. Specifically, there is no pre/post suffrage trend in the share of whites, Black people, and Hispanics.[3] However, we observe some reductions in the share of females post-event. To the extent that the results are affected by this change in the gender composition of the sample, we expect that the results provide only a lower bound of true effects and underestimate the true effects since females' incarceration rates are generally lower than males.

3.2 | Event-study analysis

Another endogeneity concern is the presence of preexisting trends in incarceration and that the overall difference-in-difference effect is riding on these trends rather than capturing the exogenous changes. To explore this issue, we implement an event-study analysis using the full specification of equation 1. The results are reported in Figure 3. The event coefficients of unexposed cohorts (those who turned 18-and-above after the suffrage) are close to zero and statistically insignificant. The coefficients start to rise (in magnitude) for partially exposed cohorts (those who turned 1–17 at the time of suffrage) and become relatively stable for fully exposed cohorts (those who were born after the suffrage). This event-study rules out the pre-trend concern and shows the evolution of effects across cohorts.

[3]We further discuss the endogeneity issue in Appendix B and offer evidence that the results are arguably not driven by changes in other state-level laws or evolution of other state-level characteristics. Moreover, other studies to examine short-run and long-run effects of suffrage movements examine many other potential confounders. Their results, in-line with the results of Appendix B, lend to the exogenity of these laws (Carruthers & Wanamaker, 2015; Kose et al., 2021; Miller, 2008).

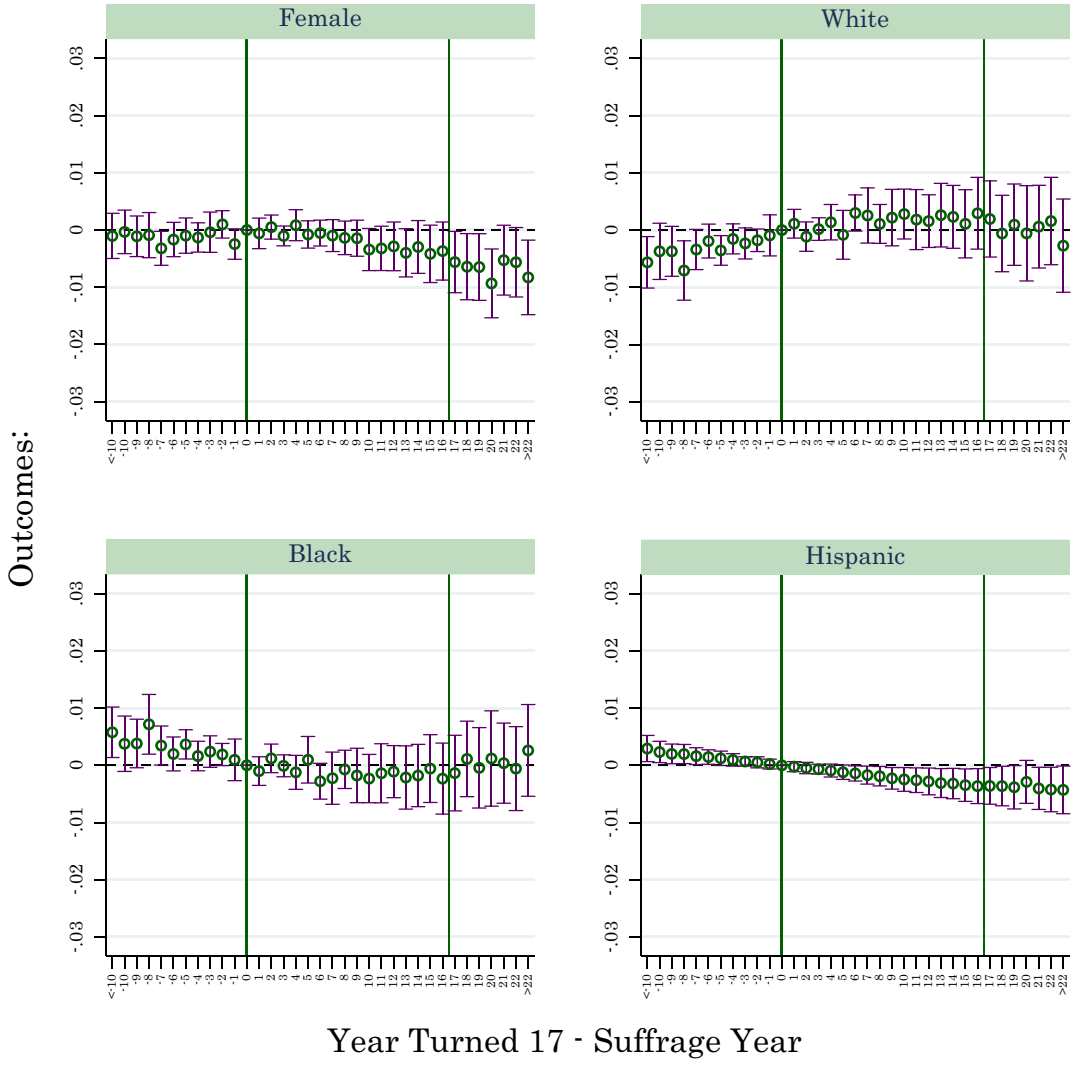

FIGURE 2 Event-study analysis to explore the correlation between exposure to suffrage reform and observable characteristics. Point estimates and 90% confidence intervals are depicted. The regression includes year fixed effects, birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. Standard errors are two-way clustered at the birth-state and birth-year levels.

3.3 | Main results

The main results of the article are reported in Table 1. Being exposed to suffrage throughout childhood (*ShareExp*=1 vs *ShareExp*=0) is associated with roughly five basis-points reduction in the likelihood of being incarcerated later in life (column 1). This effect is equivalent to about a 28% reduction relative to the mean of the outcome.

We explore the heterogeneity of the results by gender and race by interacting a dummy for male and a dummy for Black with the independent variable of interest (columns 2 and 3, respectively). The effects primarily are driven by changes in male incarceration as the coefficient of share of exposure is economically and statistically zero

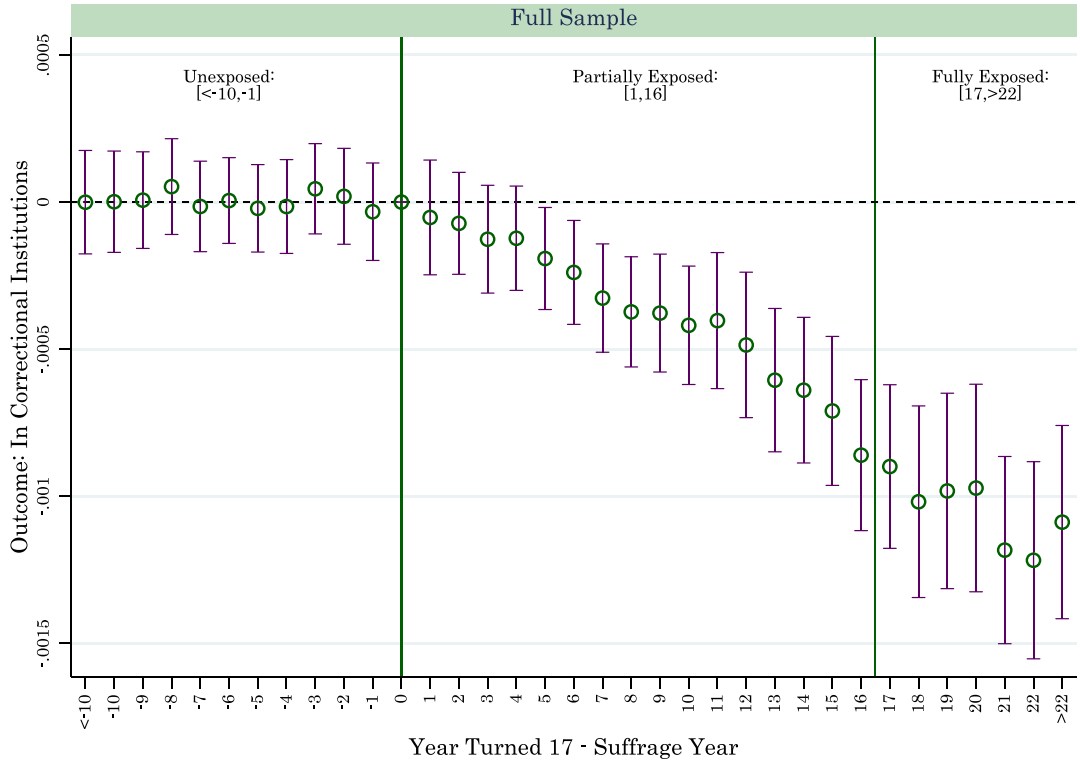

FIGURE 3 Event-study analysis to explore the childhood exposure to suffrage laws and the likelihood of being incarcerated. Point estimates and 90% confidence intervals are depicted. The regression includes year fixed effects, birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes dummies for gender, race, and ethnicity. Standard errors are two-way clustered at the birth-state and birth-year levels.

(column 2). Moreover, the double-interaction term on Black people suggests that the reduction in incarceration is more pronounced among Black people. Among Black people compared to whites, a full exposure is associated with 21 basis-points reduction in incarceration. This is equivalent to about 48.6% reduction from the mean of incarceration among Black. However, suffrage also has a positive effect among whites as the main effect of exposure share is negative and statistically significant (column 3, row 1).

Kose et al. (2021) suggest an increase of about 0.9 years of schooling among Black people as a result of a full exposure to suffrage laws (off a mean of 5.2 years). Using their estimation combined with column 3 of Table 1, one can calculate an elasticity of incarceration with respect to education of about 2.8 among Black people. One interesting avenue to understand this number is to compare with other policy-driven interventions. For instance, Eriksson (2018) explore the effects of Rosenwald school construction during Jim Crow South on education and incarceration of Black people. She finds increases in education and reductions in later-life incarceration. Her estimations suggest an elasticity of incarceration with respect to education of about 3.13.[4] This elasticity is very similar to the implied elasticity based on our results.

[4]She finds a reduction in incarceration of about 2 percentage-points, off a mean of 0.025. She also finds increases in education by 1.3 years, an increase of about 25% with respect to the mean.

TABLE 1 Main results: The association between maternal exposure to suffrage reforms and children's later-life probability of incarceration.

	Outcome: In correctional institutions		
	(1)	(2)	(3)
Exposure share	−0.0005*** (0.00013)	0.0001 (0.00014)	−0.0007*** (0.00013)
Exposure share × Male		−0.0012*** (0.00013)	
Exposure share × Black			−0.00209*** (0.00019)
Male	0.00289*** (0.00004)	0.00315*** (0.00005)	0.00289*** (0.00004)
Black	0.0027*** (0.00014)	0.0027*** (0.00014)	0.0031*** (0.00015)
White	−0.00078*** (0.00012)	−0.00078*** (0.00012)	−0.00078*** (0.00012)
Observations	134,511,086	134,511,086	134,511,086
R-squared	0.00209	0.00211	0.00211
Year fixed effects	✓	✓	✓
Birth state fixed effect	✓	✓	✓
Region-of-birth-by-birth-year fixed effect	✓	✓	✓

Notes: Standard errors, two-way clustered at the birth-state and birth-year level, are reported in parentheses. The data cover the full-count decennial censuses 1910–1920 and cohorts born between the years 1870 and 1920.
*** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

4 | CONCLUSION

The literature in various fields suggests that empowering women leads to improvements in children's outcomes as women have a higher preference for investing in their offspring's health, education, social interaction, and general human capital (Carruthers & Wanamaker, 2015; Duflo, 2012; Eswaran, 2002; Homan, 2017; Hossain, 2015; Kose et al., 2021). However, only a handful of studies have explored beyond contemporaneous short-run outcomes, specifically later life criminal behavior. This study contributed to the ongoing research on women empowerment and children outcomes by documenting significant reductions in later-life incarceration of children whose mothers were exposed to suffrage laws during the late nineteenth and early twentieth century United States. We find that women empowerment through suffrage laws can reduce roughly 28% of incarceration among fully exposed versus nonexposed children. Furthermore, the results indicate that these effects are largely driven by reductions in the incarceration of males and Black people. These findings provide nontrivial implications for policymakers who attempt to influence the criminal behavior of individuals through preventive methods and nonconventional measures.

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APPENDIX A

Summary statistics of the final sample are reported in Appendix Table A1. The average share of exposure, the independent variable of interest in the analysis, is 22%, with a standard deviation of 32%. On average, 0.18% of individuals are in correctional institutions. In addition, Appendix Figure A1 shows the geographic distribution of the share of the state-level population who are in correctional institutions.

APPENDIX B

This appendix discusses how state-level characteristics evolve pre/post suffrage reforms. We implement a series of event-study analyses where the event is the year of suffrage, and the event-time is the time relative to the passage of suffrage law in the state. We use decennial censuses 1870–1930 to construct some state-level variables for decennial years. We also use data on the dry status of counties extracted from Sechrist (2012) to calculate the share of dry counties. We use data from Kose et al. (2021) to build a dummy to indicate whether Poll tax is effective in a state or not. We implement regressions that include state fixed effects, year fixed effects, and region-by-year fixed effects. Finally, we cluster standard errors at the state level.

We show the results of these event studies in eight panels in Appendix Figure B1 and Appendix Figure B2. Virtually all pre/post event coefficients are economically and statistically indistinguishable from zero. Specifically,

TABLE A1 Summary statistics.

Variables	Mean	SD	Min	Max
In Correctional Institutions	0.0018	0.0422	0	1
Year of birth	1897.3642	13.3795	1870	1920
Female	0.5005	0.5	0	1
White	0.8804	0.3245	0	1
Black	0.1167	0.321	0	1
Hispanic	0.0017	0.0417	0	1
Share of exposure	0.2199	0.3223	0	1
Observations	134,511,086			

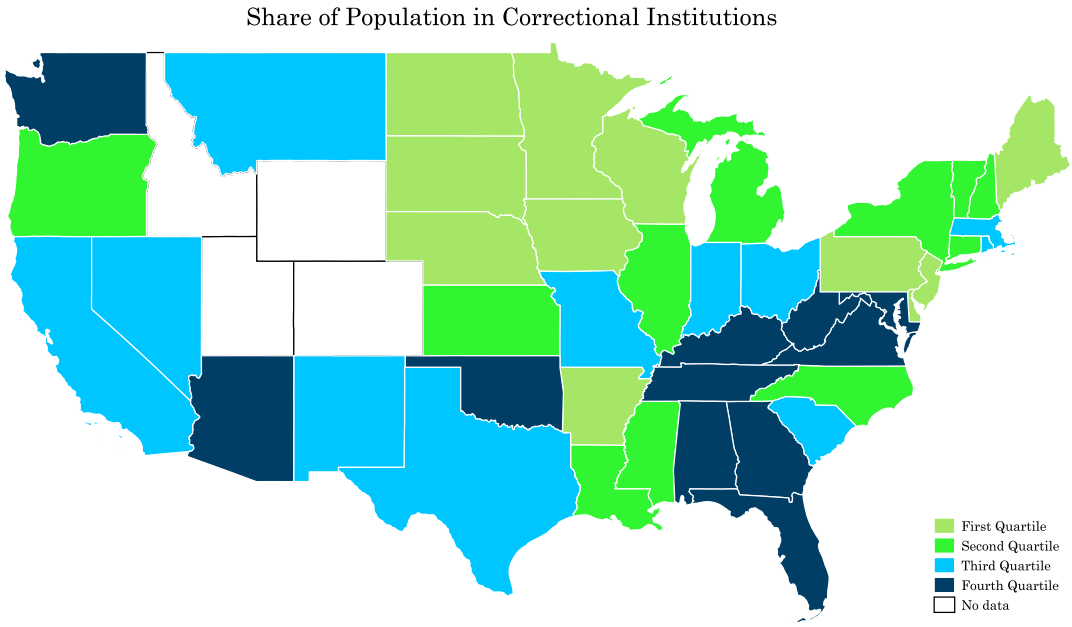

FIGURE A1 Geographic distribution of prisoners by state over the years 1870–1920.

there is no association between changes in suffrage laws and the share of dry counties, Poll tax status, the share of females, whites, Black people, Hispanics, married women, and average socioeconomic index. These figures further lend to the exogeneity assumption of women suffrage laws.

APPENDIX C

Many states adopt the suffrage law in the year 1920, as mandated by the federal amendment. Moreover, we observe incarceration in decennial censuses 1920–1930. This could create a problem as we do not observe a full set of years of exposure across all states. To gauge the relevance of this issue, we implement the empirical method for two alternative subsamples. First, we focus only on cohorts that were partially exposed, that is, dropping those with a full exposure. The event-study results for this subsample is reported in Appendix Figure C1. The difference-in-difference results are reported in column 1 of Appendix Table C1. Second, we focus on cohorts with

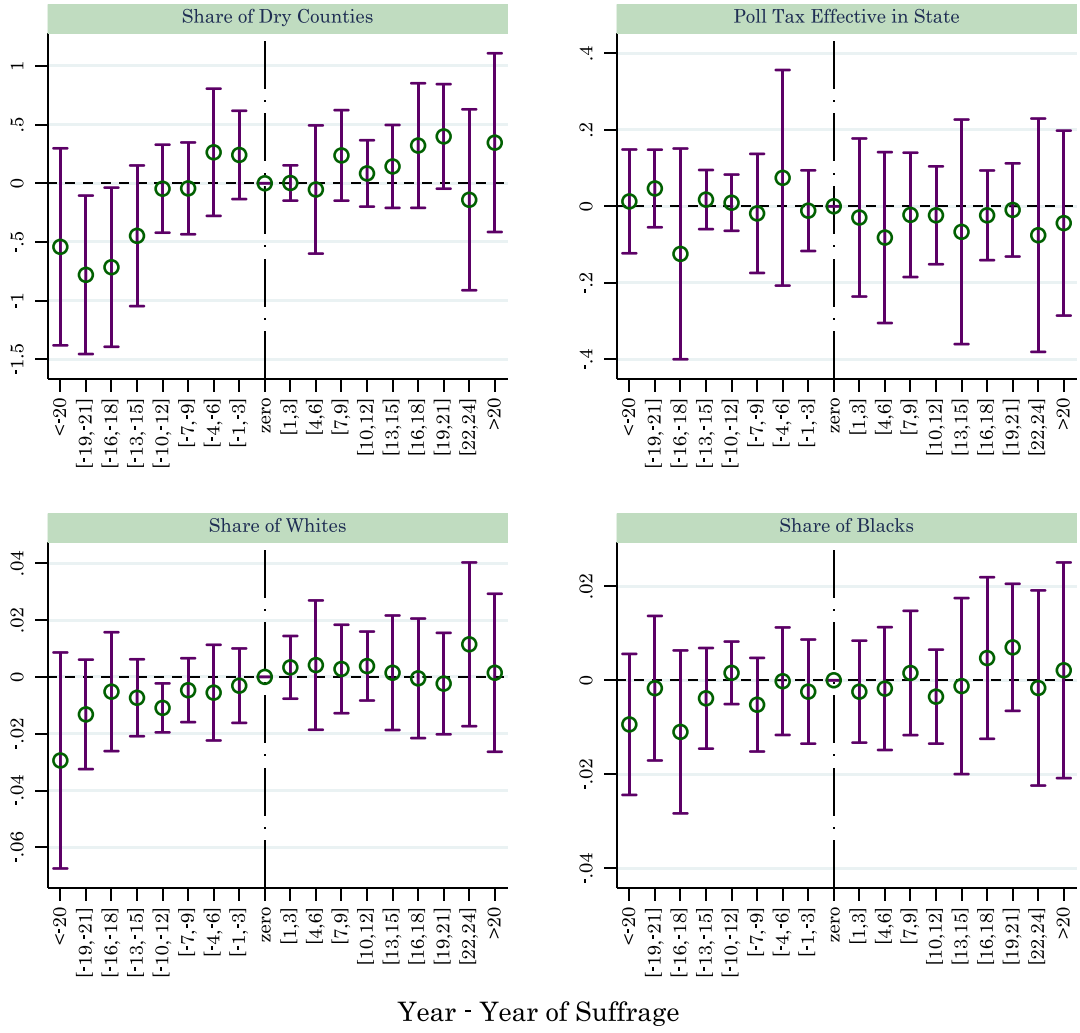

FIGURE B1 Event-study analysis for state-level covariates prior to and following the suffrage implementation. Point estimates and 90% confidence intervals are illustrated. Outcomes are shown in each panel's title. The event-time is the distance between each census year (in which the outcome is observed) and the year of suffrage reform. All regressions include state fixed effects, year fixed effects, and region-by-year fixed effects. Standard errors are two-way clustered at the birth-state and birth-year levels. The data cover the decennial years 1880–1940.

an exposure of −10 to 22 years. These set of results are reported in Appendix Figure C2 and column 2 of Appendix Table C1. The results are generally comparable to the main findings of the article.

Another concern with the results is that as incarceration decreases by age, the results may be driven by young cohorts in states that passed the law earlier. To check for this concern, we limit the sample to cohorts in the age range of 15–40 and replicate the main results. These set of results are reported in Appendix Figure C3 and column 3 of Appendix Table C1. The results suggest slightly larger effects.

APPENDIX D

Another concern in interpreting the OLS-produced results of the article is the different weights OLS assign to various two-by-two difference-in-difference comparisons (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021a). To address the robustness of our OLS-produced difference-in-difference method, we replicate the event-study using the method provided by Sun and Abraham (2021). The results, reported in Appendix Figure D1, reveal very similar pattern of effects as the main event-study of the article.

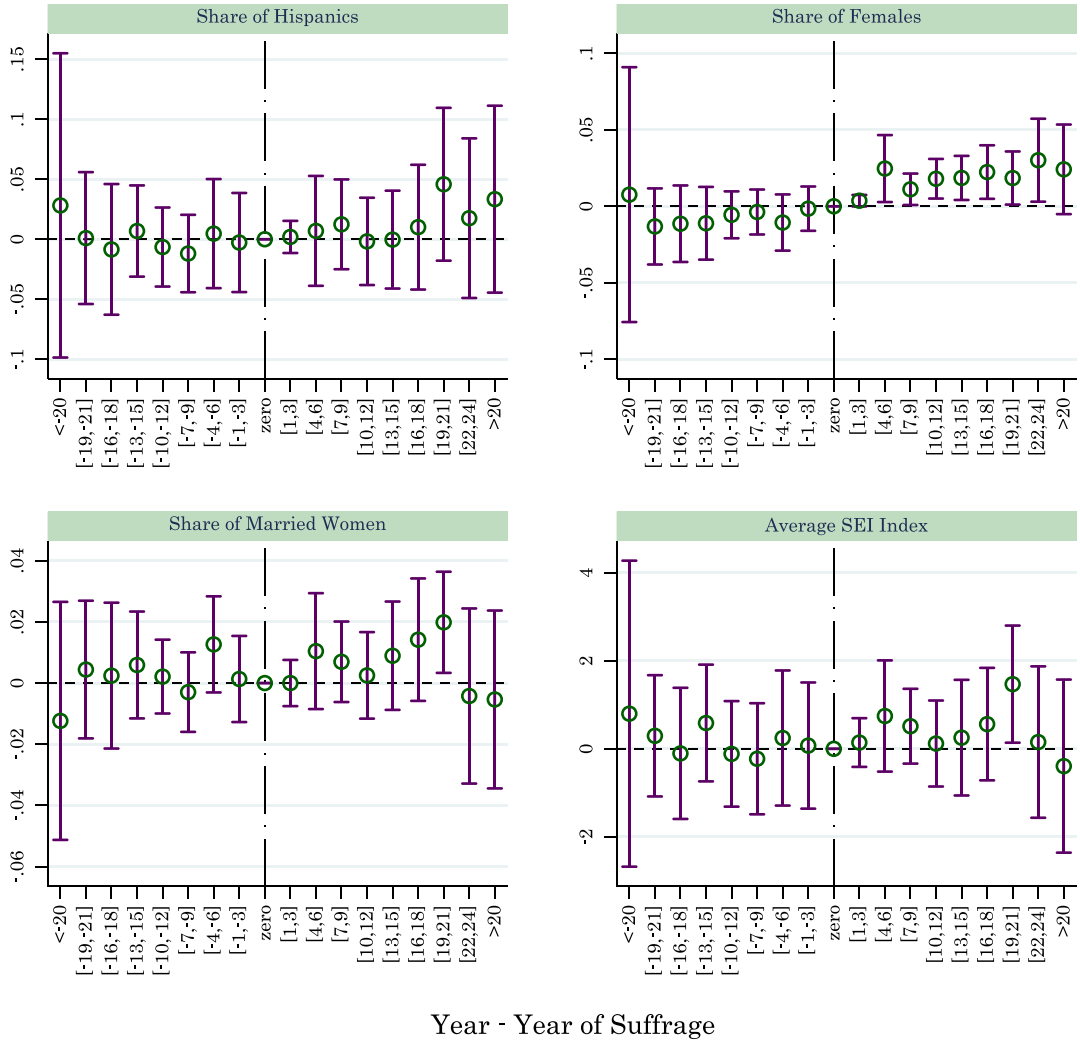

FIGURE B2 Event-study analysis for state-level covariates prior to and following the suffrage implementation. Point estimates and 90% confidence intervals are illustrated. Outcomes are shown in each panel's title. The event-time is the distance between each census year (in which the outcome is observed) and the year of suffrage reform. All regressions include state fixed effects, year fixed effects, and region-by-year fixed effects. Standard errors are two-way clustered at the birth-state and birth-year levels. The data cover the decennial years 1880–1940.

TABLE C1 Robustness of the results across three subsamples.

	Outcome: In correctional institutions		
	(1)	(2)	(3)
Exposure share	−0.00043*** (0.00013)	−0.00034** (0.00016)	−0.00059*** (0.00017)
Male	0.003*** (0.00004)	0.00333*** (0.00006)	0.00369*** (0.00006)
Black	0.00255*** (0.00016)	0.00303*** (0.0002)	0.00344*** (0.00021)
White	−0.00101*** (0.00014)	−0.00102*** (0.00016)	−0.00109*** (0.00018)
Observations	127,995,133	90,165,835	89,384,803
R-squared	0.0021	0.00231	0.00269
Year fixed effects	✓	✓	✓
Birth State fixed effect	✓	✓	✓
Region-of-birth-by-birth-year Fixed effect	✓	✓	✓

Notes: Standard errors, two-way clustered at the birth-state-birth-year level, are reported in parentheses. The data cover the full-count decennial censuses 1910–1920 and cohorts born between the years 1870 and 1920.

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

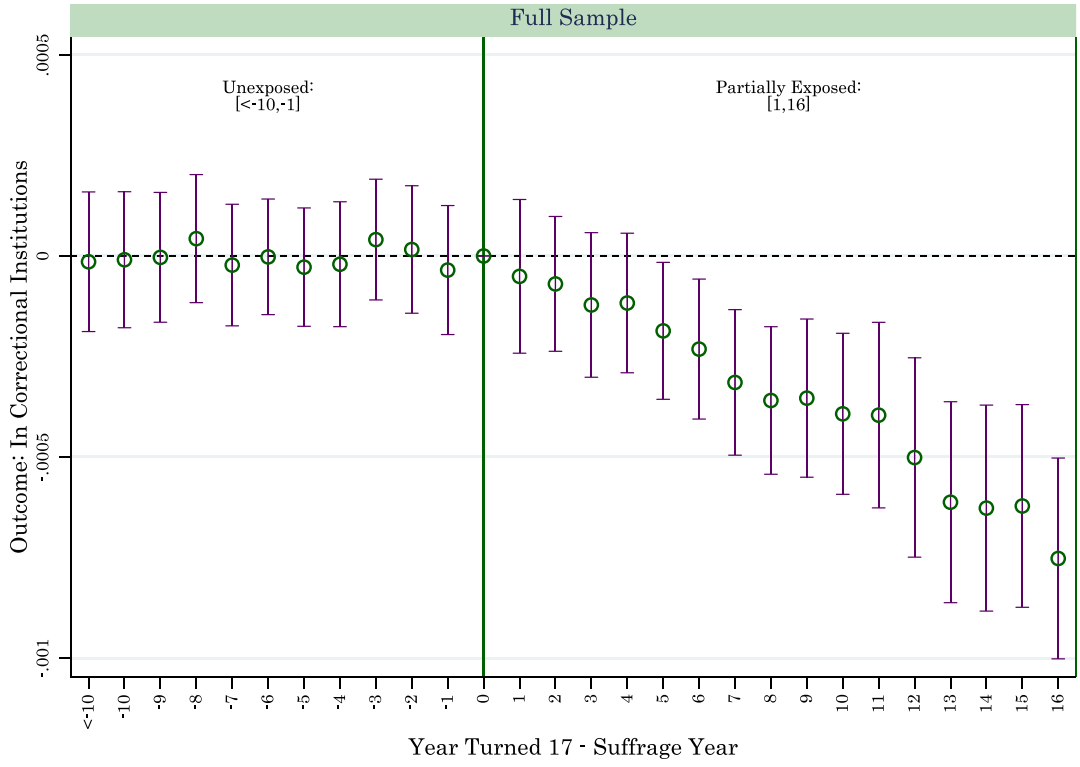

FIGURE C1 Robustness of the event-study results for partially treated cohorts only. Point estimates and 90% confidence intervals are depicted. The regression includes year fixed effects, birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes dummies for gender, race, and ethnicity. Standard errors are two-way clustered at the birth-state and birth-year levels.

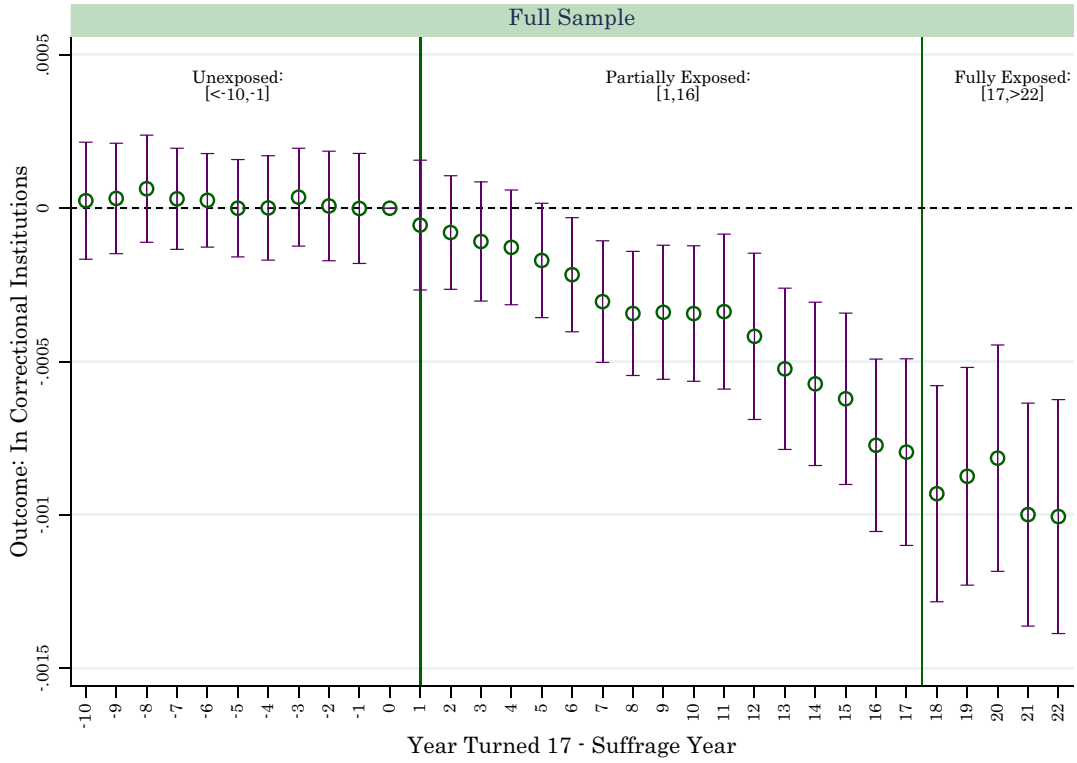

FIGURE C2 Robustness of the event-study results for states that cover cohorts with −10 to 22 years of exposure. Point estimates and 90% confidence intervals are depicted. The regression includes year fixed effects, birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes dummies for gender, race, and ethnicity. Standard errors are two-way clustered at the birth-state and birth-year levels.

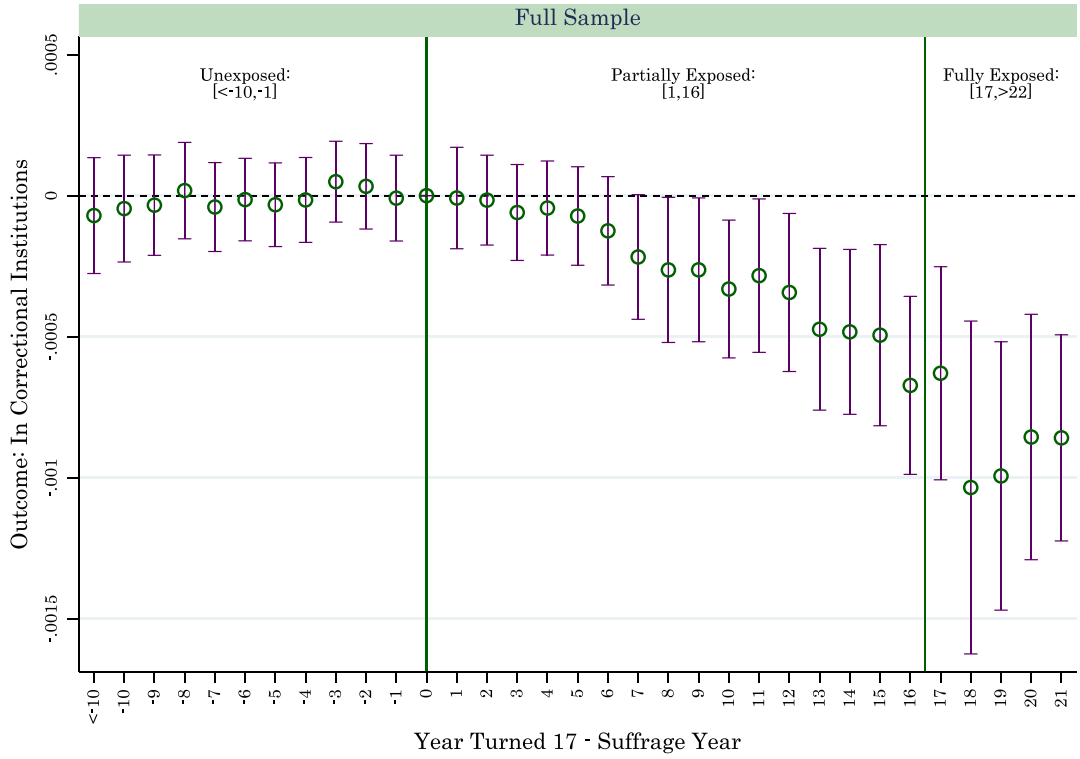

FIGURE C3 Robustness of the event-study results to subsample of aged 15–40. Point estimates and 90% confidence intervals are depicted. The regression includes year fixed effects, birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes dummies for gender, race, and ethnicity. Standard errors are two-way clustered at the birth-state and birth-year levels.

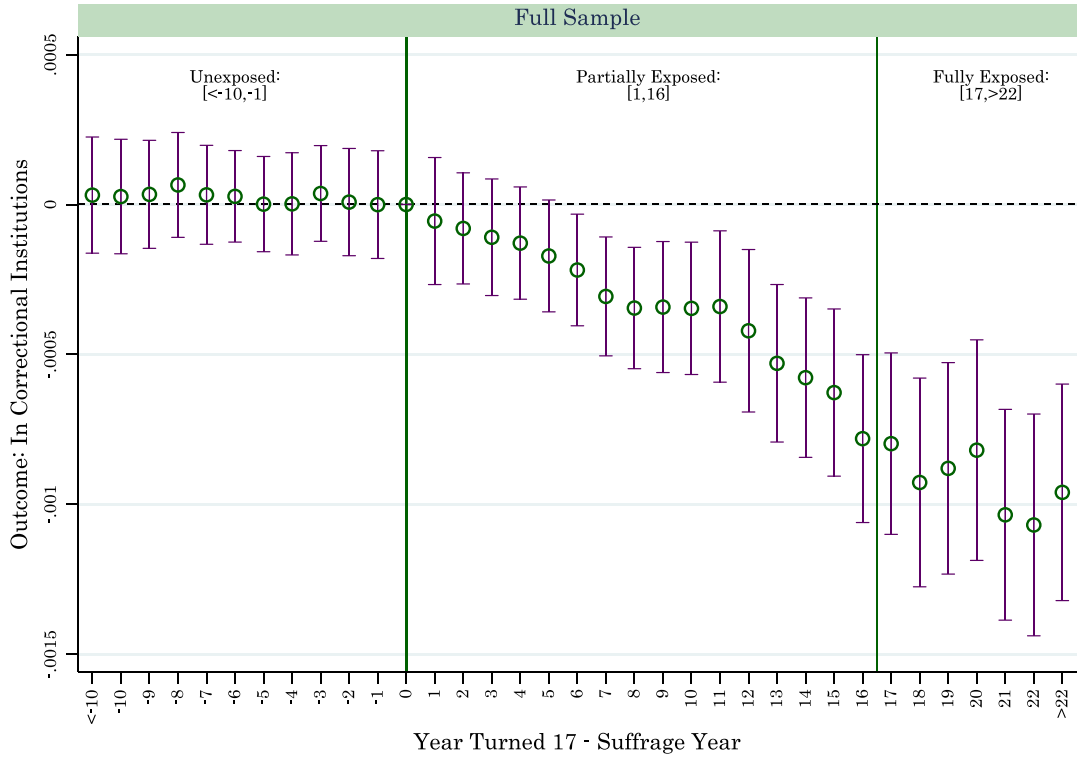

FIGURE D1 Robustness of the event-study results to Sun and Abraham (2021) estimation. Point estimates and 90% confidence intervals are depicted. The regression includes year fixed effects, birth-state fixed effects, birth-year fixed effects, and birth-region-by-birth-year fixed effects. The regression also includes dummies for gender, race, and ethnicity. Standard errors are two-way clustered at the birth-state and birth-year levels.