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SHORT RESEARCH ARTICLE

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Childhood exposure to birth registration laws and old-age mortality

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Abstract

This paper studies the effects of the enactment of birth registration laws, as the official universal and uniform method of recording births, across US states in the first decades of the 20th century on old-age longevity for children affected by these laws. We show that establishing birth registration laws has long-term benefits for old-age health. The benefits are primarily driven by states with an effective child labor policy, suggesting that registering births helps the enforcement of child labor laws which in turn operate as the mechanism channel to improve old-age longevity. A treatment-on-treated calculation suggests an increase of 0.6 years of longevity from not working during childhood due to the birth registration law.

KEYWORDS
child labor, historical data, institutions, legal enforcement, longevity, mortality

JEL CLASSIFICATION
H75, I18, N31, N32

1 | INTRODUCTION

The modern birth/death registration system is rooted in the needs of health and medical officials of the 19th century to access accurate statistics of births and deaths in the presence of ever-increasing urbanization and the accompanying epidemics and pandemics (Hetzel, 1997). The registration system in the 20th century was also backed by the ideas pursued by The Federal Children's Bureau as a necessity for enforcement of child labor laws and compulsory attendance laws (Landrum, 2014). The primary purpose of state-wide registration laws was to enable the official record of births, provide a uniform and standardized registration document, and make the registration mandatory and universal. Enactment of birth registration also provides documents of proof of age which has the potential to facilitate the enforcement of child labor laws. Fagernäs (2014) employs the 1910–1930 US census and shows that child labor laws were twice more effective in reducing underage employment if the state had established a birth registration law. She also finds that cohorts born in states with a birth registration law attain roughly 0.1 more years of schooling. Birth and identity registration laws could also facilitate inheritance claims and validate property claims (Higgs, 1996). Szreter (2007) document that identity registration in England between the 17th and 19th centuries played an essential role in accessing social security claims and mobility of labor and capital. Various social security laws in the US are cohort specific and require proof of age. Studies document suggestive impact of social policies on old-age mortality outcomes (Modrek et al., 2022; Noghanibehambari & Engelman, 2022; Snyder & Evans, 2006).

Several studies document that child labor and compulsory attendance laws in the first part of the 20th century were successful in reducing child labor (Moehling, 1999), increasing educational attainments (Lleras-Muney, 2002), and improving adult earnings (Acemoglu & Angrist, 2000; Oreopoulos, 2006). Child labor reduces the incentive and ability to attend school. Arthi (2018) shows that children who experienced the Dust Bowl of the 1930s reveal higher rates of high school completion. She argues that children who would substitute education for work are more likely to attend school in the scarcity of job prospects. The increases in education and income may affect later-life health and longevity in various ways, including improvements in choice of occupation (Fletcher, 2012a), residential location (Barreca et al., 2021), health behavior induced by peer selection (Fletcher, 2012b; Fletcher & Marksteiner, 2017), access to health insurance (Goldin et al., 2021), knowledge of healthy diets and life-style (Loef & Walach, 2012), stress (Nielsen et al., 2008), and generally better access to material resources. A strand of the literature evaluates the direct impact of education and income on later-life health and mortality outcomes and suggest improvements in health outcomes during adulthood and old ages (Fletcher, 2015; Fletcher & Noghanibehambari, 2021; Galama et al., 2018; Lleras-Muney, 2005). A narrow strand of research explores the direct correlation between child labor laws and later-life health outcomes (Edmonds & Pavcnik, 2005). For instance, Roggero et al. (2007) use a cross-country dataset and show that countries with a higher share of working children experience higher adult mortality and higher rates of infectious disease among adults. However, no study explores the effects of birth registration laws on old-age longevity in the US. Our paper aims to fill this gap.

We investigate the effect of enactment and establishment of birth registration laws across states in the first decades of the 20th century on the longevity of exposed cohorts. We find significant intent-to-treat effects that are primarily driven by states that, in addition to birth registration law, also have established a child labor law.

This research note provides two contributions to the literature. First, this is the first study to document a nexus between old-age longevity, birth registration laws, and child labor laws. Second, we add to the small strand of literature on the health benefits of birth registration and child labor laws. Although registration and child labor is granted in the US, many areas in the world still lack a universal registration system with concerning levels of the child-age working population (UNICEF, 2020).

2 | DATA AND EMPIRICAL METHOD

The primary source of data is restricted-use death records of Vital Statistics over the years 1980–2015. We focus on the post-1980 years as state-of-birth, an important identifier in our setting, is consistently reported for this period. We link this data with the birth registration law database extracted from Fagernäs (2014) based on the state-of-birth of each record. We focus on laws established in the first decades of the twentieth century and hence restrict the sample to cohorts born between 1890 and 1930.[1] Figure 1 depicts the geographic distribution of implementation of birth registration laws across states. Table 1 reports summary statistics of the final sample. The average age-at-death in our sample is 76, and it varies between 40 and 114 years. Roughly 69% of observations are born in state-years with a registration law established. Child labor laws are extracted from Goldin and Katz (2011). We merge their state-year database with mortality data based on individuals' state-of-birth and the year they turn age 14. Following formulation of Goldin and Katz (2011) and Acemoglu and Angrist (2000), we define child labor law as the maximum schooling required before work and the difference between the minimum child's age at work and school entry age. We define a dummy variable (CL_7) to capture the presence and strength of child labor law. The dummy takes a value of one if child labor is more than 7 years (e.g., minimum age of 14 to get a work permit in the case of 7 years of required schooling and age at school entry of 7 years old) and zero otherwise. As reported in Table 1, roughly 55% of observations live in states where CL_7 is one.[2] We should note that our constructed variable is based on a combination of child labor and schooling laws. Therefore, the interpretation of the results expands not only to child labor enforcements per se, but also covers some aspects of compulsory schooling laws.

Our empirical method compares the longevity outcomes of those born in states with a birth registration law to those without a registration law in different years relative to the reform year. Specifically, we implement event-study regressions of the following form:

$$\mathrm{DA}_{isy} = \alpha_0 + \sum_{k=\underline{T}_1}^{\overline{T_2}} \eta_k I((y - \mathrm{BRLY}_s) = k) + \alpha_1 X_i + \alpha_2 Z_{sy} + \zeta_s \times T_y + \xi_y + \varepsilon_{isy} \quad (1)$$

where DA is age-at-death of individual i in state-of-birth s and year-of-birth y. $I(.)$ is an indicator function that equals one if the individual's year of birth is k years away from the state-specific birth registration law year (BRLY). We allow k to vary from

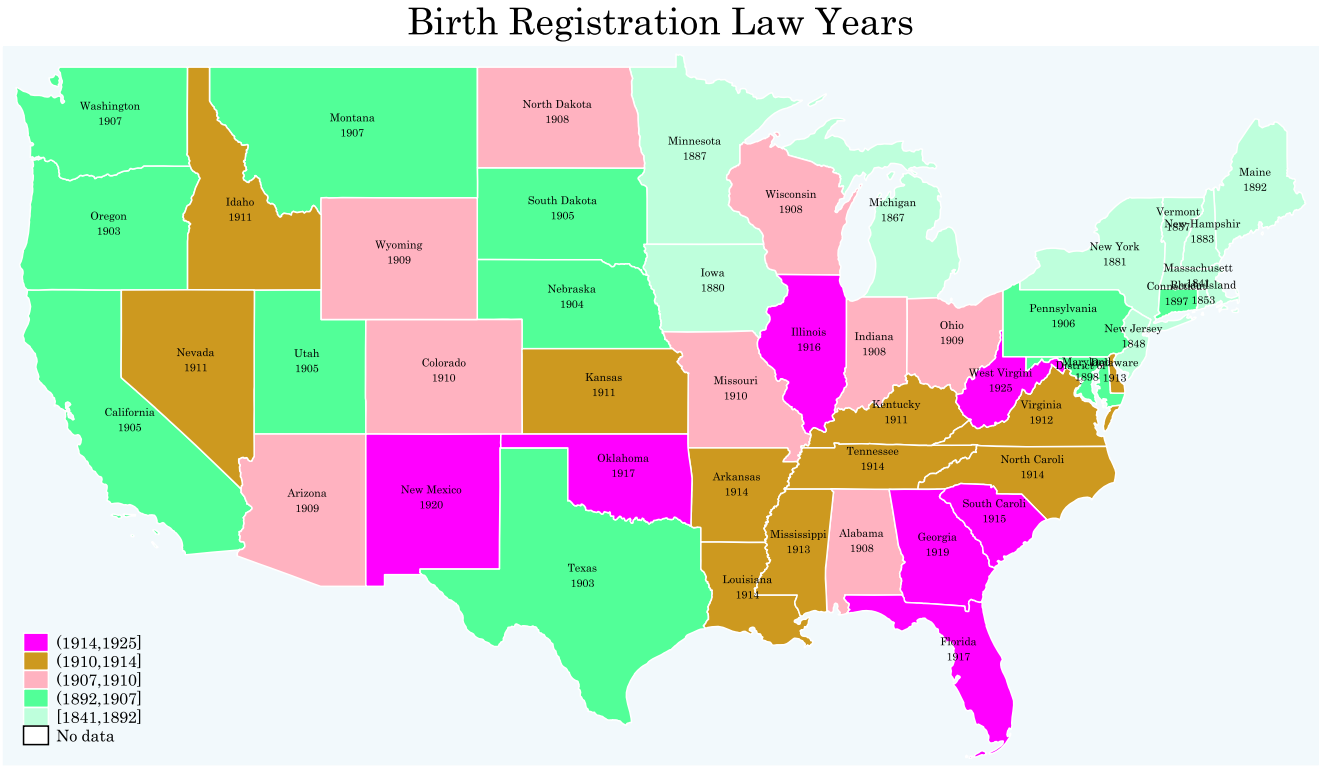

Figure 1 - Birth Registration Law Years across US States

FIGURE 1 Birth registration law years across US states. [Colour figure can be viewed at wileyonlinelibrary.com]

TABLE 1 Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Death age (years)	80.516	9.613	50	118
Birth year	1915.399	9.528	1890	1930
Death year	1995.916	9.789	1980	2015
Female	0.541	0.498	0	1
White	0.897	0.304	0	1
Black	0.099	0.298	0	1
Birth registration exposure dummy	0.586	0.493	0	1
Child labor law>7	0.546	0.498	0	1
Average SEI index	24.353	4.53	13.906	34.005
Share of white-collar wokers	0.034	0.007	0.022	0.058
Share of farmers	0.249	0.15	0.013	0.743
Share of other occupations	0.715	0.144	0.232	0.946
Average male occupational income score	21.75	2.91	14.858	26.855
Female literacy rate	0.722	0.074	0.424	0.834
Observations	44,989,339			

8 years before the state-specific reform up to 18 years post-reform. We remove the coefficients for which $k = 0$ to compare all the estimated effects with respect to these cohorts.

In X, we include as individual covariates race and gender dummies. In Z, we include state-of-birth-by-year-of-birth covariates extracted from full-count decennial censuses 1880–1940 and interpolated for inter-decennial years. These controls include average state-level occupational income score, socioeconomic score, the share of literate people, and the share of people in different occupations. The parameters ζ and ξ represent birth-state and birth-year fixed effects, respectively. We cluster standard errors at the birth-state level to control for serial correlation and birth-year to control for spatial correlation in error terms.

3 | RESULTS

3.1 | Endogeneity concerns

One concern over endogeneity is that registration law reforms may have been accompanied by other changes in demographic and socioeconomic conditions, and the estimated effects of Equation (1) are picking up on those overall changes rather than the effects of reforms. We explore this source by estimating a series of balancing tests within an event study setting, similar to Equation (1) but replacing the outcome with individual race and gender dummies, conditional on a full set of fixed effects. The results are reported in Figure 2. We observe reductions in the female outcome following the law change, although there is no pre-trend in the likelihood of being female (top-left panel). Since females generally have longer lives, to the extent that the share of females drops post-law, we expect that the results underestimate the true effects and our findings provide a lower-bound of true effects. However, there is no consistent and discernible pre- and post-trend in the likelihood of being white, black, and other races The point estimates are small in magnitude and statistically insignificant. In Appendix A, we also use full-count decennial censuses over the years 1880–1940 and show that the passage of registration laws was not associated with several other law changes or changes in sociodemographic characteristics of states. Specifically, we show that there is no pre-and-post trend in several outcomes, including suffrage reforms, prohibition reforms, poll tax reforms, various child labor law and compulsory schooling law measures, changes in the share of different race/ethnicity, changes in various measures of family structure, changes in socioeconomic score, and changes in the composition of different occupations.

FIGURE 2 Balancing test in an event-study framework. The point estimates and 95% confidence intervals are reported. The regressions include state-of-birth fixed effects, year-of-birth fixed effects, and a state-of-birth linear trend. Standard errors are two-way clustered at the birth-state and birth-year level. [Colour figure can be viewed at wileyonlinelibrary.com]

3.2 | Main results

The event-study results are reported across two panels of Figure 3. The top-left panel shows the ordinary-least-square regressions. The negative event-time coefficients are small and insignificant, suggesting no concern for preexisting trends in longevity to drive the results. The coefficients start to rise for post-reform exposures and become statistically significant. The slow rise in the effects suggests that there were delays after establishing the reforms until they became universal and fully effective (Fagernäs, 2014).[3]

The coefficients stabilize for cohorts born roughly 11–12 years after the establishment of laws. The overall post-versus-pre difference-in-difference coefficient suggests that cohorts born in states with a registration law established enjoy roughly 0.08 years higher longevity (see Appendix Table B1).

Several studies suggest that OLS-generated difference-in-difference estimates could be biased in settings with staggered treatment (de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021). The bias arises from two-by-two comparisons between early-adopters and later-adopters, that is, states that passed the registration law earlier in comparison with those who enacted it later and vice versa. To explore the robustness of our event-study to excluding such bias-generating comparisons, we implement the method developed by Sun and Abraham (2021). The results are reported in the bottom panel of Figure 3. We observe very similar sets of pre-and-post marginal effects that are quite comparable with those of OLS results, implying that the results are not confounded by OLS-produced biases.[4]

FIGURE 3 OLS and Sun and Abraham (2021) event study estimates to explore the effect of birth registration laws on longevity. The point estimates and 95% confidence intervals are reported. The regressions include state-of-birth fixed effects, year-of-birth fixed effects, and a state-of-birth linear trend. The regressions also include state-of-birth and individual controls. Standard errors are two-way clustered at the birth-state and birth-year level. [Colour figure can be viewed at wileyonlinelibrary.com]

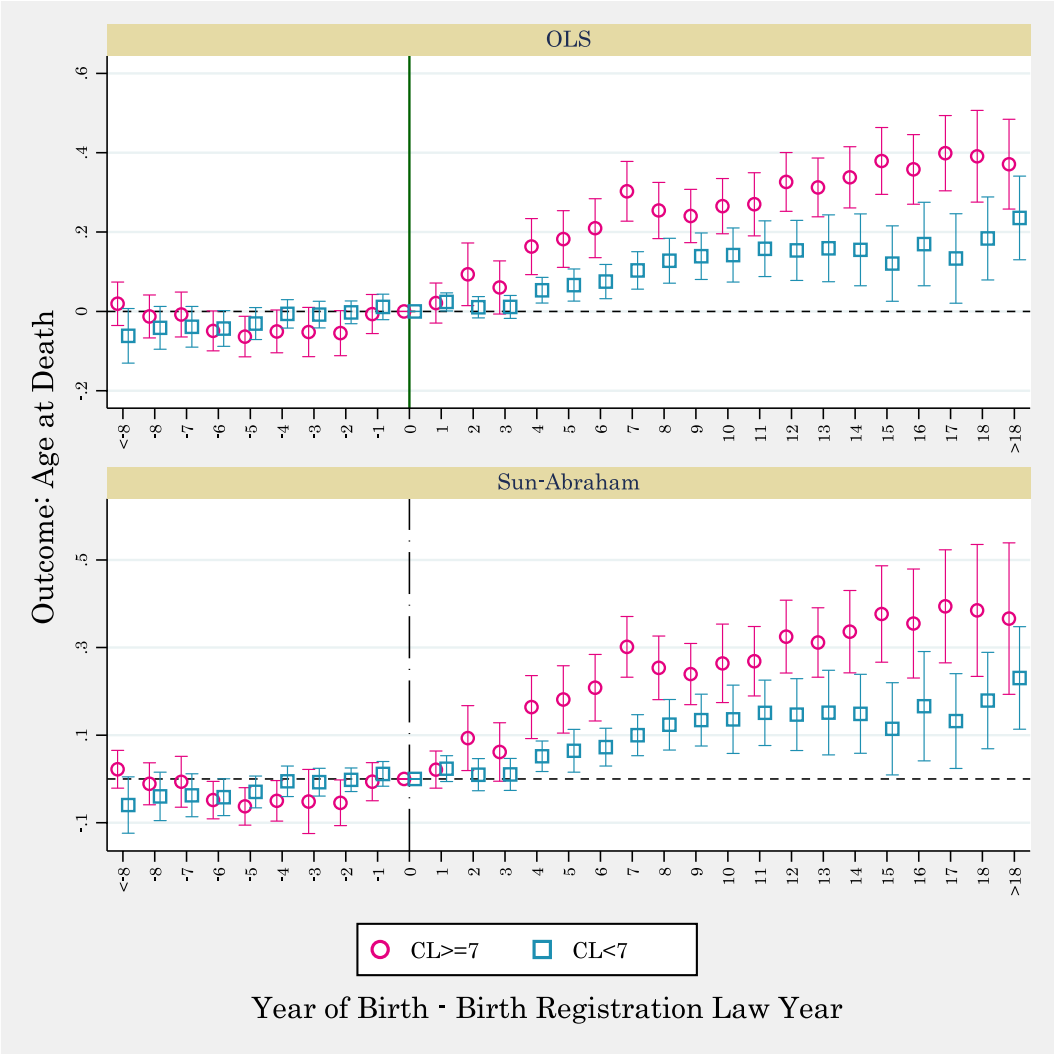

FIGURE 4 OLS and Sun and Abraham (2021) event study estimates to explore the effect of birth registration laws in states with strong/weak child labor law on longevity. The point estimates and 95% confidence intervals are reported. The regressions include state-of-birth fixed effects, year-of-birth fixed effects, and a state-of-birth linear trend. The regressions also include state-of-birth and individual controls. Standard errors are two-way clustered at the birth-state and birth-year level. [Colour figure can be viewed at wileyonlinelibrary.com]

Birth registration laws are expected to enable the enforcement of child labor laws more efficiently by offering documentation of age. We replicate the main event-study results for a subset of states with $CL\_7 = 1$ and set of states with $CL\_7 = 0$. The results are reported in Figure 4 for OLS and Sun-Abraham estimates in top and bottom panels, respectively. While pre-trend coefficients do not provide strong and consistent pattern of preexisting trend concern, the post-trend coefficients rise for both set of states. Specifically, we observe larger increases among states with stronger child labor law compared to states with weaker laws. This fact suggests that the registration law effects on longevity may operate primarily through enforcement of child labor laws.

3.3 | Robustness checks

In Appendix B, we show the reduced-form difference-in-difference results of exposure to registration laws on longevity and then explore the robustness of these estimates. We show that the results are robust when we add state-of-death-by-state-of-birth fixed effects, month-of-death fixed effects, and allow for state-of-birth fixed effects to vary flexibly by gender and race. We also investigate the robustness of the functional form by replacing the outcome with the log of age-at-death and a dummy indicating longevity beyond 80 years. The results are quite insensitive and robust to these alternative models. Furthermore, we show that standard errors are robust to alternative clustering levels, including clustering only at state-of-birth.

4 | CONCLUSION

This study is the first to show the long-term benefits of enacting birth registration laws on old-age longevity. We document that the observed effect primarily operates through enforcing child labor laws. The balancing tests and endogeneity tests rule out the concern that changes in the sample's demographic composition and a wide range of state sociodemographic characteristics confound the estimated effects. We present our results in a series of event-study results. The simple difference-in-difference estimation suggests an intent-to-treat effect of 0.08 years.

Halpern-Manners et al. (2020) implement a twin-fixed-effect strategy and show that an additional year of schooling is associated with 0.34 years higher longevity. Fagernäs (2014) finds that introduction of birth registration laws increased the schooling of affected cohorts by roughly 0.1 years. Combining the results of these papers, one would expect an increase of 0.03 years of life. The fact that we observe relatively larger reduced-form effects on longevity (0.08 years) suggests that the effects only partially operate through child-labor/educational channels (~40%) and may also operate through non-educational channels, such as better access to social security claims, validated property right access, facilitating age-specific vaccinations, proof of citizenship and employment eligibility in the presence of immigrants, etc. We can also use the estimated effects of Fagernäs (2014) on child-age employment as the first stage results to scale up the reduced-form effects. She documents that in states with a child labor law, cohorts exposed to the establishment of birth registration law are 5%-points less likely to work during their childhood, off a mean of 0.15. Therefore, a back-of-an-envelope calculation suggests that for cohorts who would have otherwise been working during childhood, being born in states with a birth registration law is associated with 0.6 years of additional lifespan.[5,6]

Aizer et al. (2016) examine the effects of the Mothers' Pension (MP) program, a conditional cash transfer program to single mothers implemented between 911 and 1935, on children's later-life longevity. They show that the program transferred about 30–40% of pre-transfer maternal income over a three-year period. They find that children of accepted mothers reveal increases in longevity of about 11.6 months. Our back-of-an-envelop treatment-on-treated calculation of birth registration is about 62% of long-term impact of childhood exposure to cash transfers under the MP program.

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CONFLICT OF INTEREST

The authors claim that they have no conflict of interest to report.

DATA AVAILABILITY STATEMENT

Our data is confidential. The replication codes can be made available upon request from the corresponding author.

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ENDNOTES

[1] In Appendix Table B7, we show the results for cohorts born between 1890–1920 and 1890–1915. While we lose much of the variation in birth registration laws, it allows examining across much older cohorts for which the right truncation issues are more restricted.

[2] Acemoglu and Angrist (2000) show that CL_7, CL_8, and CL_9 have significant effects in their first stage analysis on educational outcomes. The dummy variable CL_7, as we defined here, combines all the three measures into one. However, in Appendix Table B2, we show the results using other thresholds, a continuous measure of child labor law, and a dummy that indicates work permit age is more than 14 years.

[3] In considering the dynamics of the effects and the observed delays, we should note that not all births were registered following the establishment of birth registration laws. The take-up rate was gradual both due to nonconformity issues and the fact that not all births occurred in places under official scrutiny. Moreover, there were reports of data quality, measurement issues, and coverage that were heterogeneous across subpopulations and concentrated among disadvantaged people who were more likely benefit from birth registration laws (Brumberg et al., 2012).

[4] In Appendix Table B3, we show the difference-in-difference results of post-versus-pre average difference using Sun and Abraham (2021) estimator and Callaway and Sant'Anna (2021) proposed method. Both estimators are also similar to the OLS-produced estimates of Appendix Table B1.

[5] This is calculated using the coefficient of exposure in column 1 of Appendix Table B1 (the difference-in-difference coefficient = 0.079) divided by the first stage effect of Fagernäs (2014) (0.05) and multiplied by back-of-an-envelope estimate that about 40% of effects operate through child-labor/education channels, hence 0.6 years.

[6] There are two concerns in these calculations. First, although we multiply the number with an estimate of the extent to which the effects could have operated through child labor laws, there are still concerns that this share underestimate/overestimate the true values, hence biasing the overall number. Second, a better approach would be to have cohorts' information on their childhood and adulthood work history and implement comparisons among those who avoided child labor due to birth registration to those who didn't. Then, we could compare the difference in their longevity as the numerator in footnote 4. However, since the death records data lack this information, we are left with using first stage regressions of Fagernäs (2014).

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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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