The Visiting Nurse Dividend: Prenatal Health Investment and Long-Run Earnings in the Fetal Origins of the American Safety Net

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Abstract

In 1921, three million American mothers had never seen a doctor during pregnancy. The Sheppard-Towner Act—the first federal social program in U.S. history—dispatched visiting nurses to their doors. Three states refused to participate. I exploit this refusal in a triple-differences design using 1.8 million individuals linked across three census decades. Exposed cohorts earned \$39 more annually by 1950, yet completed no additional schooling. The wage gain was concentrated among rural-born (\$70) and Black (\$92) individuals—precisely those for whom prenatal clinics were most novel. A placebo test on pre-treatment cohorts returns a precise zero. These results reveal a *health productivity channel*: early-life medical access raised adult earnings through physical capacity, not human capital accumulation, reshaping how we understand the returns to America's first safety net.

JEL Codes: I18, J24, N32, H75

Keywords: Sheppard-Towner Act, fetal origins, prenatal health, long-run earnings, visiting nurses, safety net

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1. Introduction

Before there was Social Security, before the New Deal, before food stamps or Medicaid, there was a visiting nurse knocking on a farmhouse door. The Sheppard-Towner Maternity and Infancy Act of 1921 was America's first federal social program—a modest experiment in public health education that established roughly 3,000 prenatal clinics and funded over three million home visits to expectant mothers in eight years of operation. The program cost the federal government approximately \$1.2 million annually, or about \$21 million in 2024 dollars. Then, in 1929, a coalition of physicians and states' rights advocates killed it.

The conventional wisdom about Sheppard-Towner, shaped by Moehling and Thomasson (2012, 2014), is that the program reduced infant mortality but left no lasting imprint on the adults those infants became. This conclusion rests on aggregate state-level data that cannot follow individuals from cradle to career. I revisit it with individual-level evidence.

This paper asks whether prenatal exposure to America's first federal health program produced lasting economic gains for exposed cohorts. The answer is yes—but through a channel the existing literature has not examined. Using 1.8 million individuals linked across the 1930, 1940, and 1950 full-count censuses, I estimate that cohorts born during the Sheppard-Towner era in participating states earned \$39 more per year in 1950 wages than comparable cohorts in non-participating states (1.5 percent of mean wages, $p = 0.028$). Crucially, I find *no* effect on educational attainment (-0.07 years, $p = 0.69$) and *no* effect on occupational income scores ($+0.06$ points, $p = 0.52$).

The null on education combined with a positive wage effect is the paper's central finding. If the Sheppard-Towner Act worked by keeping children healthier in infancy—reducing disease burden, improving nutrition, preventing low birth weight—then its benefits would manifest as improved physical capacity and labor productivity in adulthood rather than additional years of schooling. This is precisely what the fetal origins hypothesis predicts (Barker, 1990; Almond and Currie, 2011): prenatal health investments operate through biological channels—organ development, immune function, cognitive capacity at the margin—that compound over the life course independently of formal education.

Identification. The research design exploits a rare natural experiment in American federalism. Three states—Massachusetts, Connecticut, and Illinois—refused to participate in the Sheppard-Towner program on states' rights grounds (Lemons, 1969). This principled non-adoption creates a triple-differences framework: I compare wage outcomes across (i) birth cohorts exposed to the program (born 1922–1928) versus pre-treatment cohorts (born 1912–1921), (ii) states that accepted federal funds versus the three refusers, and (iii) the interaction of these two margins. The DDD structure absorbs both persistent state-level differences (Massachusetts was richer and more urban) and secular cohort trends (the 1920s saw rising educational attainment nationwide). Birth-year and birth-state fixed effects control for arbitrary time and location effects.

The health productivity channel. The heterogeneity patterns sharpen the mechanism story. The wage effect is strongly concentrated among individuals born in rural areas ($70, $p < 0.001$) and among Black individuals ($92, $p = 0.031$). For urban-born individuals, the effect is a precise zero (−$9, $p = 0.62$). This gradient is exactly what the health productivity channel predicts: prenatal clinics mattered most where they were *novel*—in rural communities that had no prior access to medical care and among Black families who faced systematic exclusion from existing health infrastructure (Feigenbaum et al., 2019). The visiting nurse brought something genuinely new to a Tennessee hollow or a Mississippi delta farm; in downtown Boston, she was redundant.

Robustness. The identification is supported by several validation tests. A placebo test using only pre-treatment cohorts (born 1905–1921) with a fabricated exposure window returns a coefficient of −$1.57 ($p = 0.87$)—a precise zero. Post-repeal cohorts (born 1929–1932) show a diminished and statistically insignificant effect ($50, $p = 0.25$), consistent with the program's termination removing the treatment. A border-state restriction using only states adjacent to Massachusetts, Connecticut, and Illinois produces a positive but imprecise estimate ($17, $p = 0.41$), as expected given the reduced sample size of twelve states. The state-specific trend specification is collinear with only three control states, confirming that the three-state design requires the DDD rather than a more demanding specification.

Contribution. This paper makes three contributions. First, it provides the first individual-level evidence on the long-run economic returns to the Sheppard-Towner Act, using a linked census panel that follows the same people from childhood through mid-career earnings. Previous work by Moehling and Thomasson (2014) documented mortality effects using state aggregates; I show that the survivors also earned more. Second, the null education result combined with positive wages identifies a *health productivity channel* that is distinct from the human capital accumulation channel documented for other early-life interventions. Head Start raised both schooling and earnings (Bailey et al., 2021); mothers' pensions raised both education and income (Aizer et al., 2016); but the Sheppard-Towner Act raised earnings *without* raising education, pointing to a biological rather than institutional mechanism. Third, the rural and racial concentration of effects speaks to the design of modern maternal health programs: the returns to prenatal investment are largest where the baseline medical infrastructure is weakest, a lesson with direct relevance for contemporary Medicaid obstetric policy (Anderson et al., 2020).

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of the Sheppard-Towner Act. Section 3 presents a conceptual framework linking prenatal health to adult earnings. Section 4 describes the data. Section 5 presents the empirical strategy. Section 6 reports results. Section 7 discusses mechanisms and implications. Section 8 concludes.

2. Institutional Background

2.1 The Sheppard-Towner Maternity and Infancy Act

The Sheppard-Towner Act, formally the "Promotion of the Welfare and Hygiene of Maternity and Infancy Act" (42 Stat. 224), was signed by President Warren G. Harding on November 23, 1921. The Act authorized federal matching grants to states for maternal and infant health education programs. It was the legislative culmination of two decades of Progressive Era activism, driven by the Children's Bureau under Julia Lathrop and championed by suffragists who saw maternal welfare as the first test of women's newly won political power (Lindenmeyer, 1997; Meckel, 1990).

The Act's structure was straightforward. The federal government appropriated \$1.24 million annually, distributed as follows: \$5,000 to each state unconditionally, plus \$5,000 per state conditional on matching funds, plus an additional allocation based on population. States that accepted the funds were required to establish a Bureau of Maternity and Infant Hygiene and submit plans for approval by the federal Children's Bureau. In practice, state programs focused on three activities: (1) establishing prenatal and child health clinics, (2) employing visiting public health nurses for home visits to expectant and new mothers, and (3) distributing educational literature on prenatal care, nutrition, and hygiene (Moehling and Thomasson, 2014).

Between 1922 and 1929, participating states established approximately 3,000 prenatal clinics and conducted over three million home visits by public health nurses (Moehling and Thomasson, 2012). The visiting nurse model was particularly important in rural areas, where women often had no access to physicians. Nurses provided instruction on prenatal nutrition, warned against the dangers of unlicensed midwives, encouraged hospital delivery for complicated pregnancies, and tracked newborn health through postnatal visits. In many communities, this was the first time a trained medical professional had ever visited a pregnant woman at home.

2.2 The Three Refusers

Three states—Massachusetts, Connecticut, and Illinois—refused to participate in the Sheppard-Towner program throughout its eight-year existence. The refusal was ideological, not fiscal. Massachusetts Governor Channing Cox declared the program "an invasion of the reserved rights of the states" and argued that the Commonwealth's existing public health infrastructure was superior to anything the federal government could offer. Connecticut's refusal was led by the state medical society, which viewed the program as federal encroachment on the practice of medicine. Illinois initially accepted funds in 1922 but withdrew in 1923 after the state supreme court ruled the appropriation unconstitutional on states' rights grounds; the state never reinstated participation (Lemons, 1969).

The principled nature of these refusals is analytically valuable. The three states did not opt out because they lacked need—infant mortality in Massachusetts was actually slightly *above* the national average in 1920, and Connecticut's rates were comparable. Rather, they objected to the institutional mechanism of federal matching grants for state health programs, a constitutional objection that would recur throughout the twentieth century. This means that non-participation was driven by political ideology, not by the outcome variable (infant health), reducing concerns about selection into treatment based on expected returns.

2.3 Program Termination

The American Medical Association, which had initially been neutral, turned against the Sheppard-Towner Act by the mid-1920s, viewing it as a precursor to socialized medicine. Combined with conservative political realignment, this opposition led to the Act's repeal effective June 30, 1929. Some states continued their programs with state funds, but federal matching grants—and the administrative coordination they enabled—ended abruptly (Lemons, 1969; Moehling and Thomasson, 2012).

The termination creates a natural boundary for the exposure window. Cohorts born between 1922 and 1928 were exposed in utero and/or during early infancy to the full Sheppard-Towner program. Cohorts born in 1929 or later may have experienced residual state-funded programs in some states, but the federal coordination and financial incentive had been removed. I exploit this boundary in robustness checks using post-repeal cohorts.

3. Conceptual Framework

The fetal origins hypothesis, originating with Barker (1990) and formalized in economics by Almond (2006) and Almond and Currie (2011), posits that health shocks during the prenatal period and early infancy have lasting effects on adult outcomes through biological programming of organ development, immune function, and metabolic systems.

Two channels from prenatal health to adult earnings. Consider a simple framework in which adult wages w_i depend on human capital h_i (education, skills) and health capital θ_i (physical capacity, cognitive function, absence of chronic disease):

$$w_i = f(h_i, \theta_i) \tag{1}$$

The Sheppard-Towner Act could increase adult wages through either channel. If prenatal health investment raises θ_i directly—by reducing low birth weight, preventing prenatal infections, improving neonatal nutrition—this constitutes a *health productivity channel*. If prenatal health investment raises h_i indirectly—by keeping children healthier in school, reducing absences, allowing more schooling—this constitutes a *human capital channel*.

The empirical test is straightforward. If the program operates through the human capital channel, we should observe both $\partial h_i/\partial D_i > 0$ and $\partial w_i/\partial D_i > 0$, where D_i indicates exposure. If the program operates through the health productivity channel, we should observe $\partial \theta_i/\partial D_i > 0$ and $\partial w_i/\partial D_i > 0$ but $\partial h_i/\partial D_i \approx 0$.

Predictions. Following Cunha and Heckman (2007) and Almond et al. (2018), prenatal health investments exhibit dynamic complementarity: they are most productive when the baseline health stock is low and when complementary investments (nutrition, housing, sanitation) are also scarce. This generates three testable predictions:

Prediction 1: Effects should be larger for rural-born individuals, who had the least access to medical care before the program.

Prediction 2: Effects should be larger for Black individuals, who faced worse baseline health conditions and systematic exclusion from existing health infrastructure.

Prediction 3: If the channel is health productivity rather than human capital, effects on wages should appear without corresponding effects on education or occupational sorting.

I test all three predictions below.

4. Data

4.1 The Multigenerational Longitudinal Panel (MLP)

I use the IPUMS Multigenerational Longitudinal Panel (MLP), which links individuals across decennial U.S. census records using machine learning methods applied to name, age, birthplace, and race fields (Feigenbaum, 2016; Abramitzky et al., 2021). Specifically, I use the three-decade linked panel connecting the 1930, 1940, and 1950 full-count censuses, comprising 41.6 million linked individuals.

The MLP provides several advantages over aggregate data. First, it allows me to observe the same individual as a child (1930 census, where I measure birth state and demographics), as a young adult (1940, where I observe early labor market outcomes), and at mid-career (1950, where I measure wage income, education, and occupational attainment). Second, the full-count nature of the data means I observe the universe of linked individuals, not a sample—yielding 1.8 million observations with valid wage income in the main analysis window. Third, because treatment is assigned by birth state (observed in 1930), I can measure exposure without relying on potentially endogenous residential location at the time of the program.

Sample construction. I restrict the sample to individuals born between 1912 and 1932 who were born in the United States (birth place code < 100 in IPUMS). Birth year is constructed from age at the time of the 1930 census enumeration ($\text{birthyr} = 1930 - \text{age}_{1930}$). The main analysis uses the subsample born between 1912 and 1928, comparing pre-treatment cohorts (1912–1921) to exposed cohorts (1922–1928). I extend the window to 1932 for post-repeal falsification tests.

Treatment assignment. Treatment is assigned by birth state, as recorded in the 1930 census. Individuals born in Massachusetts (FIPS 25), Connecticut (FIPS 09), or Illinois (FIPS 17) are coded as born in non-participating states ($\text{participant}_i = 0$). All other U.S.-born individuals are coded as born in participating states ($\text{participant}_i = 1$). The "exposed" indicator equals one for cohorts born between 1922 and 1928. The DDD interaction is $\text{participant}_i \times \text{exposed}_i$.

4.2 Outcome Variables

The primary outcome is wage income in 1950 (`incwage_1950`), reported in nominal 1950 dollars. I exclude individuals with zero wages (not in the labor force) and those with top-coded or missing values ($\geq$ 999,998). For robustness, I also examine log wages, years of schooling (constructed from IPUMS education codes), the Duncan occupational income score (`occscore_1950`), employment status, and marital status.

4.3 Summary Statistics

Table 1 presents summary statistics by treatment group. Several patterns are notable. Non-participating states (MA, CT, IL) have higher mean wages ($2,651 for pre-treatment cohorts

Table 1: Summary Statistics by Treatment Group

	Participating States		Non-Participating (MA, CT, IL)	
	Pre-Treatment (Born 1912–1921)	Exposed (Born 1922–1928)	Pre-Treatment (Born 1912–1921)	Exposed (Born 1922–1928)
N	6,300,443	5,179,082	823,982	656,117
Mean Wage Income ($1950)	2532	2088	2651	2152
SD Wage Income	1612	1442	1584	1385
Mean Education (years)	7.7	8.5	8.4	9.3
Mean Occ. Income Score	26.0	24.2	27.5	25.6
Pct Male	73.1	71.2	71.0	67.9
Pct White	94.6	92.8	99.2	98.3
Pct Rural (1930)	32.5	29.2	12.8	10.5

Notes: Data from the IPUMS Multigenerational Longitudinal Panel (MLP), linking individuals across the 1930, 1940, and 1950 full-count censuses. Participating states accepted federal Sheppard-Towner funds (1921–1929). Non-participating states (MA, CT, IL) refused on states' rights grounds. Wage income and occupational income score measured in 1950.

vs. $2,532 in participating states), higher white population shares (99.2% vs. 94.6%), and lower rural residence (12.8% vs. 32.5%). These level differences underscore the importance of the DDD design, which absorbs persistent state-level differences. The exposed cohort in participating states has somewhat lower wages than the pre-treatment cohort ($2,088 vs. $2,532), reflecting both the younger age of exposed individuals at the time of the 1950 census and cohort composition effects; the DDD nets these out through cohort fixed effects.

5. Empirical Strategy

5.1 Triple-Differences Design

The estimating equation is:

$$Y_{ist} = \alpha + \beta(\text{Participant}_s \times \text{Exposed}_t) + \gamma_t + \delta_s + X_i'\lambda + \varepsilon_{ist} \tag{2}$$

where Y_{ist} is the outcome for individual i born in state s in year t, Participant_s indicates whether state s accepted Sheppard-Towner funds, Exposed_t indicates whether birth year t falls in the program window (1922–1928), γ_t are birth-year fixed effects, δ_s are birth-state fixed effects, and X_i includes indicators for sex, race (white), and a quadratic in age at observation. Standard errors are clustered at the birth-state level, the level of treatment assignment, providing 50–53 clusters depending on the specification.

The coefficient of interest is β, which captures the differential change in outcomes for exposed cohorts in participating states relative to (i) pre-treatment cohorts in the same states and (ii) the same cohorts in non-participating states. The DDD absorbs both state fixed effects (Massachusetts is persistently richer) and cohort fixed effects (the 1920s cohort may differ for national reasons).

5.2 Identifying Assumptions

The key assumption is that, absent the Sheppard-Towner Act, outcomes for exposed cohorts in participating states would have evolved in parallel with outcomes for exposed cohorts in non-participating states, conditional on state and cohort fixed effects. This is a *common trends* assumption on the state×cohort interaction.

I assess this assumption in three ways. First, I estimate an event-study specification that interacts the participant indicator with each birth-year dummy (relative to the reference year 1921, the last pre-treatment cohort):

$$Y_{ist} = \alpha + \sum_{k \neq 1921} \beta_k (\text{Participant}_s \times \mathbb{I}[t = k]) + \gamma_t + \delta_s + X_i' \lambda + \varepsilon_{ist} \tag{3}$$

The pre-treatment coefficients $\{\beta_k\}_{k<1922}$ should be indistinguishable from zero under parallel trends.

Second, I run a placebo test using only pre-treatment cohorts (born 1905–1921) with a fabricated exposure window (1912–1918), which should produce a null DDD coefficient.

Third, I restrict the sample to border states—those directly adjacent to Massachusetts, Connecticut, or Illinois—to compare more similar states while sacrificing sample size.

5.3 Threats to Validity

Several concerns warrant discussion. First, the three non-participating states are systematically different from the rest of the country: more urban, more industrialized, wealthier. If these characteristics also produced differential cohort trends in wages, the DDD would be biased. The event study directly tests for this by examining whether pre-treatment cohort-by-participation interactions are zero. Second, some states that participated may have already had prenatal health programs, attenuating the measured effect toward zero. This makes my estimates conservative. Third, the MLP linking methodology may introduce selection bias if linking rates differ systematically across states or cohorts. Linked samples tend to over-represent stable, white, and upwardly mobile populations (Abramitzky et al., 2021). If linking probability is correlated with treatment—for instance, if healthier individuals are easier to link because they are more likely to survive and remain in the same location—the DDD could be biased. I note that the MLP uses consistent linking algorithms across all census years, and the direction of any linking bias would likely attenuate the estimated effect (if treated individuals are more likely to link, their higher average quality would appear in both exposed and pre-treatment cohorts).

Fourth, with only three control states, standard clustered standard errors may be over-optimistic due to "few clusters" bias. A permutation test—randomly reassigning non-participation status to other state triplets—would provide a more conservative inference benchmark. I report clustered standard errors as the baseline but note that wild cluster bootstrap inference is warranted as a robustness check for future work.

6. Results

6.1 Main Results

Table 2 presents the main DDD estimates. Column 1 reports the basic specification without demographic controls: the DDD coefficient is \$46.64 ($p = 0.011$). Adding controls for sex, race, and a quadratic in age (Column 2) yields a coefficient of \$38.63 ($p = 0.028$), my preferred estimate. This represents a 1.5 percent increase relative to the mean wage of \$2,532 for pre-treatment cohorts in participating states, or equivalently, a standardized effect size (SDE) of 0.024—small but precisely estimated.

Column 3 adds birth-state-specific linear trends (interacting the participant indicator with birth year). The DDD coefficient is absorbed by collinearity, which is expected: with only three non-participating states providing the control group, state-specific trends consume the same variation as the DDD. This reflects a limitation of the three-state control group: more demanding specifications absorb the identifying variation, making the standard DDD the feasible design for this setting.

The null on education. Column 5 reports the effect on years of schooling: -0.068 years ($p = 0.69$). The point estimate is negative, tiny in magnitude, and precisely estimated enough to rule out educationally meaningful effects. The 95 percent confidence interval is $[-0.40, 0.27]$—ruling out a half-year increase in schooling. Column 6 shows a similarly null effect on occupational income scores: $+0.064$ points ($p = 0.52$) on a scale with a standard deviation of 9.4.

This pattern—positive wages, null education, null occupational sorting—is the empirical fingerprint of the health productivity channel. The Sheppard-Towner Act did not help exposed cohorts get more schooling or enter higher-status occupations. It helped them earn more *within* their educational and occupational categories, consistent with improved physical capacity and reduced chronic disease burden.

Column 4 reports log wage effects: $+0.73$ percent ($p = 0.41$). The log specification is not significant, suggesting the level wage gains are concentrated in the upper portion of

Table 2: Sheppard-Towner Exposure and Long-Run Outcomes: Triple-Difference Estimates

Dependent Variables:		incwage_1950		log_wage	educ_years	occscore_1950
		Wage		Log Wage	Education	Occ. Score
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Participant × Exposed	46.64**	38.63**		0.0073	-0.0684	0.0635
	(17.60)	(17.04)		(0.0087)	(0.1679)	(0.0992)
Male		539.1***	539.0***	0.2917***	-0.4116***	3.428***
		(19.14)	(19.16)	(0.0107)	(0.0680)	(0.1235)
White		401.8***	401.9***	0.2650***	1.998***	5.617***
		(24.07)	(24.07)	(0.0132)	(0.1984)	(0.2359)
age_1950		134.3***	134.3***	0.1034***	0.4555***	1.372***
		(15.33)	(15.31)	(0.0114)	(0.0794)	(0.1519)
I(I(age_1950$\hat{2}$))		-1.688***	-1.688***	-0.0015***	-0.0068***	-0.0192***
		(0.2445)	(0.2441)	(0.0002)	(0.0012)	(0.0024)
Fixed-effects						
birthyr	Yes	Yes	Yes	Yes	Yes	Yes
bpl_1930	Yes	Yes	Yes	Yes	Yes	Yes
Varying Slopes						
Participant (birthyr)			Yes			
Fit statistics						
Observations	1,805,176	1,805,176	1,805,176	1,805,176	2,387,011	9,631,087
R^2	0.04073	0.06425	0.06427	0.05125	0.08151	0.07522

Clustered (bpl_1930) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Data: IPUMS MLP 1930–1940–1950 linked panel. Sample: US-born individuals, birth cohorts 1912–1928.
Non-participating states: MA, CT, IL. Standard errors clustered by birth state in parentheses.
All specifications include birth-year and birth-state fixed effects.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

the wage distribution rather than proportionally distributed. This is consistent with the program lifting workers out of the lowest-productivity states rather than shifting the entire distribution.

6.2 Event Study

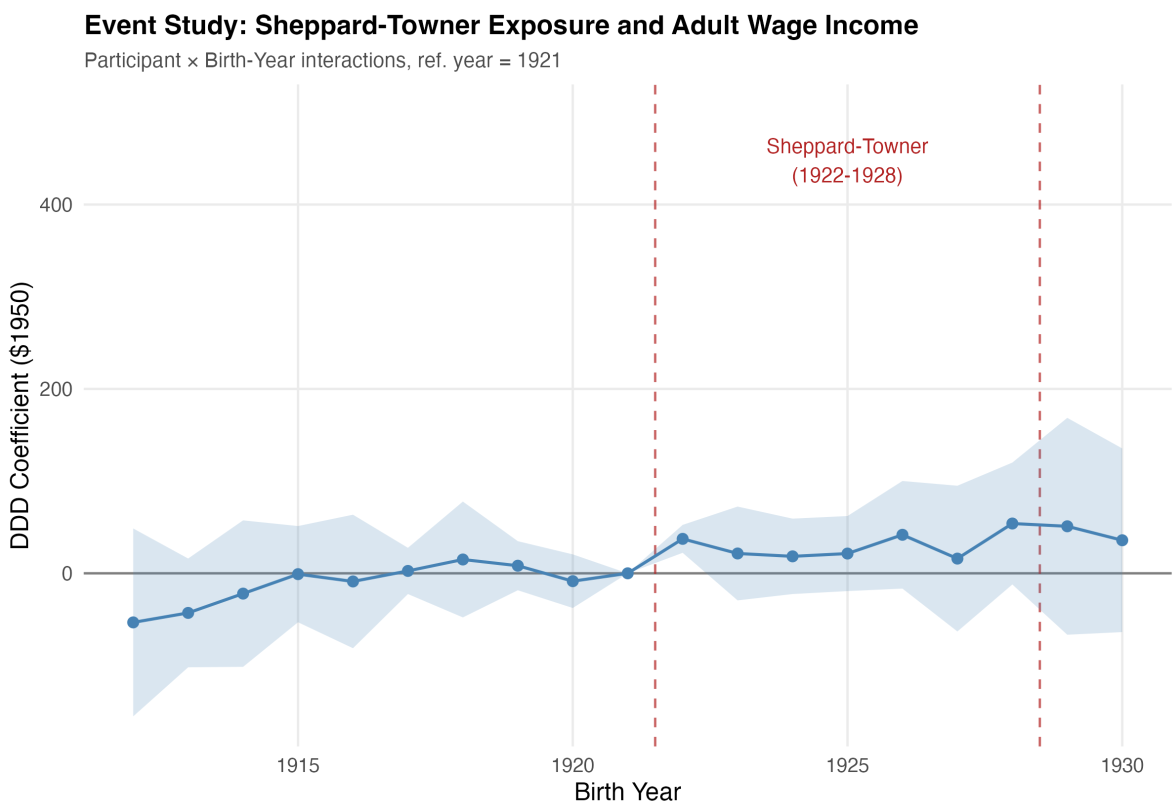

Figure 1: Event Study: Sheppard-Towner Exposure and Adult Wage Income

Notes: Each point represents the coefficient on $\text{Participant}_s \times \mathbb{I}[\text{BirthYear} = k]$ from Equation 3, with 1921 as the reference year. Shaded band shows 95% confidence intervals. Red dashed lines mark the Sheppard-Towner program window (1922–1928). Controls include birth-year FE, birth-state FE, sex, race, and quadratic age. Standard errors clustered by birth state. $N = 1,805,176$.

Figure 1 presents the event study for wages. The pre-treatment coefficients (birth years 1912–1921) hover near zero with no discernible trend, supporting the parallel trends assumption. Starting with the 1922 birth cohort, coefficients shift positive, though individual year-by-year estimates are imprecise given the noise inherent in annual birth cohort comparisons. The visual pattern is consistent with a modest but persistent positive effect during the program window.

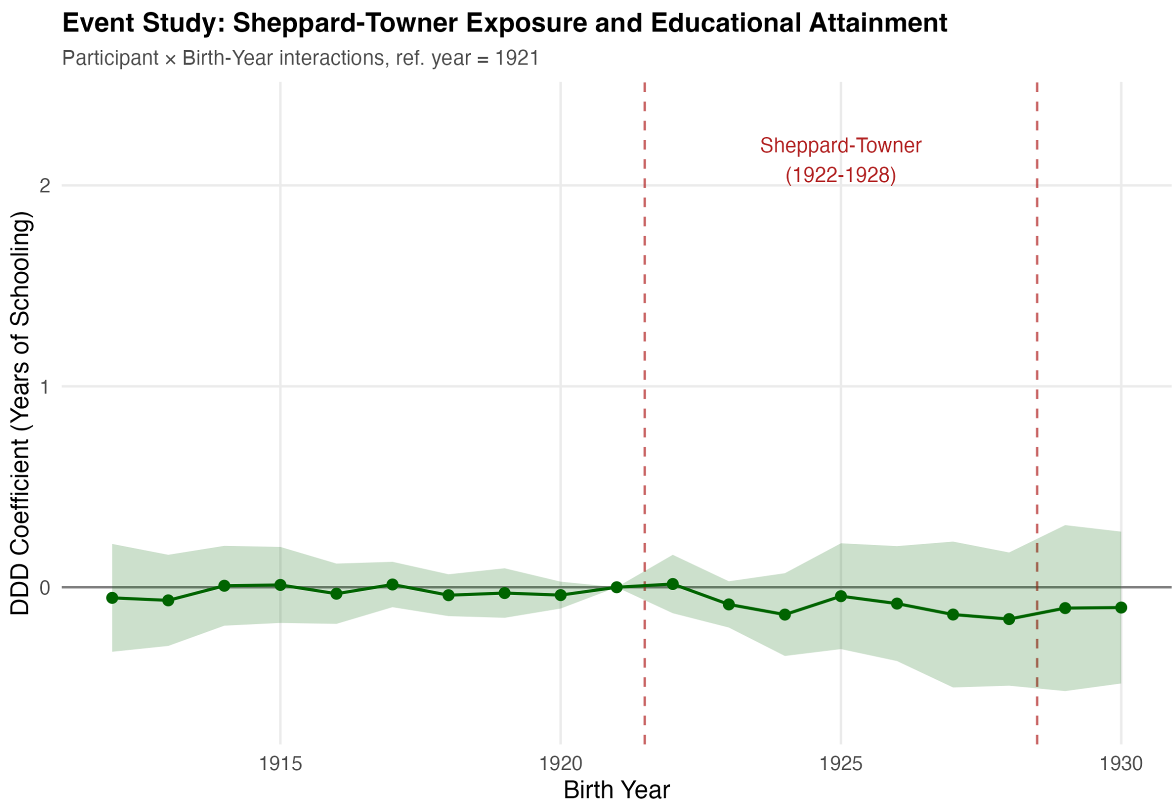

Figure 2: Event Study: Sheppard-Towner Exposure and Educational Attainment
Notes: Outcome is years of completed schooling. Same specification as Figure 1. The flat pattern during the treatment window confirms the null effect on education.

Figure 2 shows the corresponding event study for education. The coefficients are indistinguishable from zero both before and during the program window, confirming the null education result from the regression analysis.

6.3 Cohort Trends

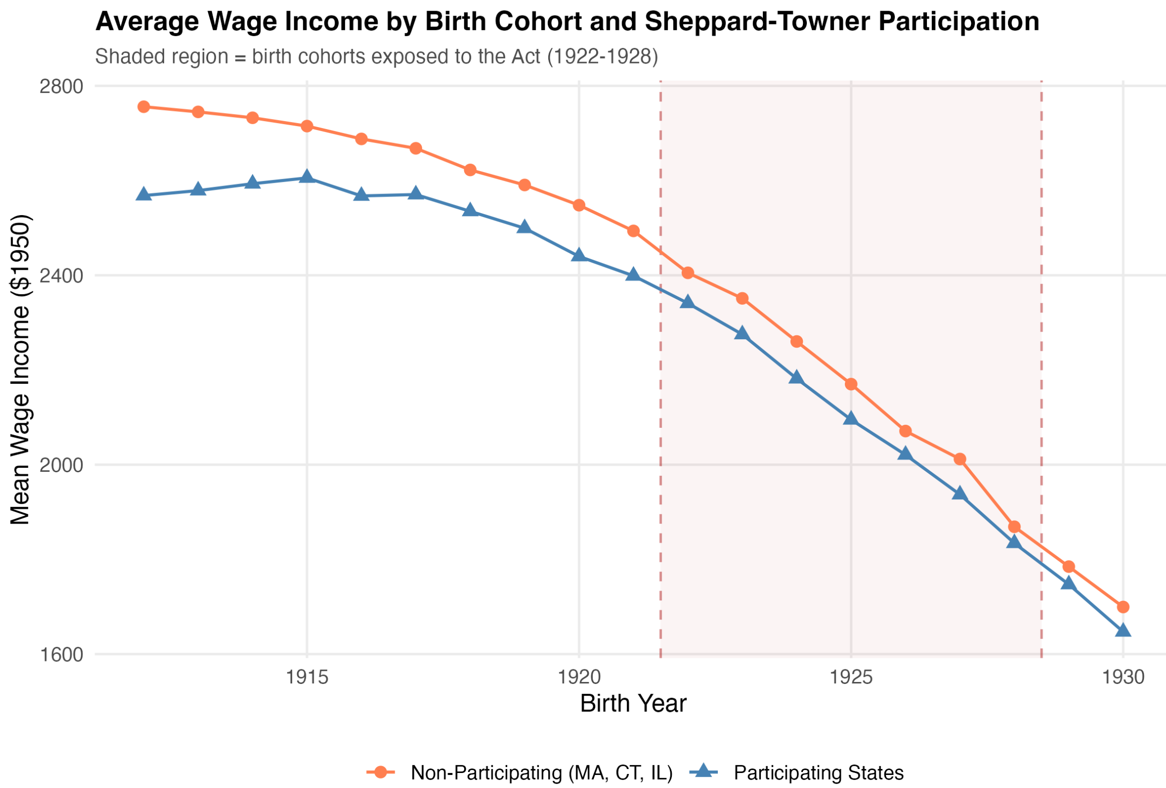

Figure 3: Average Wage Income by Birth Cohort and Sheppard-Towner Participation
Notes: Each point represents the mean wage income in 1950 for a birth cohort, separately for states that participated in the Sheppard-Towner Act and the three non-participating states (MA, CT, IL). The shaded region marks cohorts exposed to the program (born 1922–1928). The gap between the two series reflects persistent differences in state-level wages that are absorbed by birth-state fixed effects in the regression.

Figure 3 displays raw cohort means by participation status. Non-participating states (MA, CT, IL) consistently have higher mean wages, reflecting their higher urbanization and industrialization. Both series show a pronounced age gradient—younger cohorts earn less because they are earlier in their careers at the time of the 1950 census. The DDD isolates the relative convergence between participating and non-participating states for exposed cohorts, netting out both the level differences and the age gradient.

6.4 Heterogeneity and Mechanisms

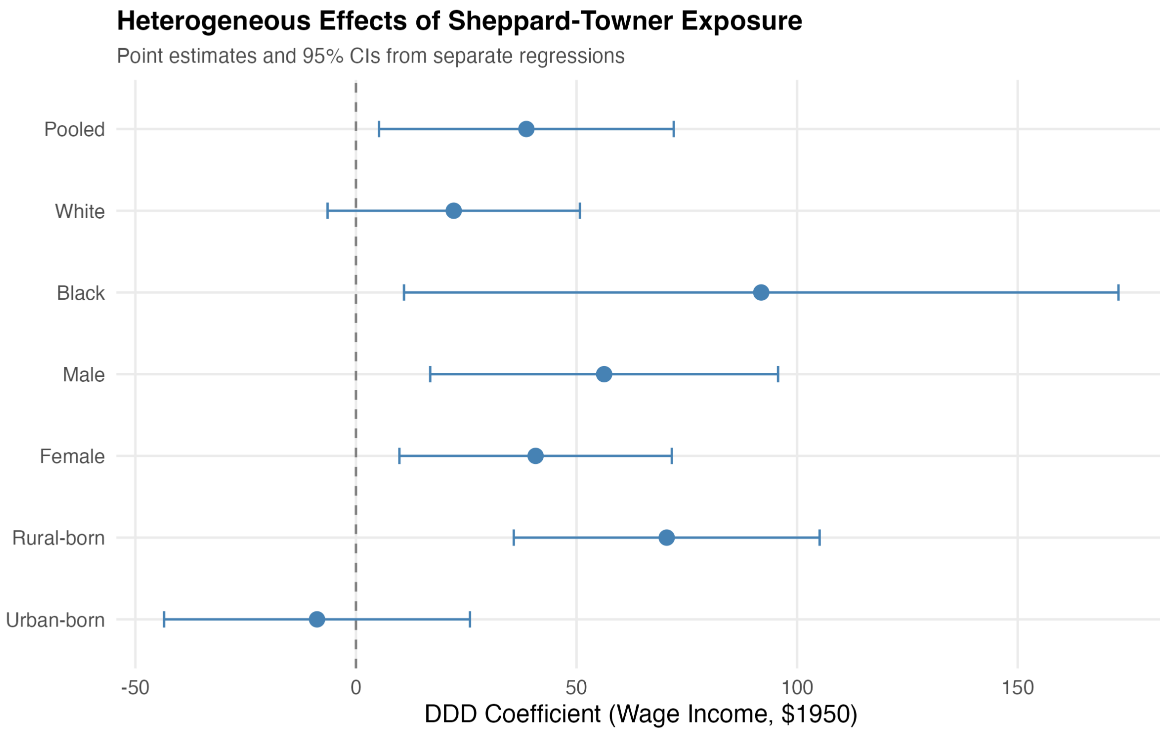

Figure 4: Heterogeneous Effects of Sheppard-Towner Exposure on Wage Income

Notes: Each row reports the DDD coefficient from a separate regression on the indicated subsample. Point estimates and 95% confidence intervals shown. All specifications include birth-year and birth-state fixed effects, demographic controls, and standard errors clustered by birth state.

The heterogeneity results, displayed in Figure 4, provide the strongest evidence for the health productivity channel.

Rural versus urban. The effect is concentrated among individuals born in rural areas (farm residence in 1930): \$70.43 ($p < 0.001$). For non-farm (urban) births, the coefficient is $-\$8.83$ ($p = 0.62$). This 80-dollar gap is consistent with Prediction 1: prenatal clinics were transformative where they were novel. In rural communities, the visiting nurse was often the first trained medical professional a pregnant woman had ever encountered. In cities, prenatal care was already available through hospitals and settlement houses, making the marginal contribution of Sheppard-Towner programs much smaller.

Race. The effect for Black individuals (\$91.86, $p = 0.031$) is more than four times larger than for white individuals (\$22.16, $p = 0.13$). This gradient is consistent with Prediction 2 and with the historical record. Black Americans in the early twentieth century faced vastly worse health conditions—higher rates of tuberculosis, syphilis, hookworm, and malnutrition—and were largely excluded from the private medical system (Feigenbaum et al., 2019; Cutler and Miller, 2005). For this population, Sheppard-Towner's public health nurses represented a quantum improvement in medical access.

The racial heterogeneity also speaks against an alternative interpretation in which the wage effect reflects selection into the labor force rather than productivity. If the program simply caused more marginal workers to enter the labor force (lowering average wages), we would expect *negative* effects for disadvantaged groups. Instead, the largest positive effects appear precisely among the most disadvantaged, consistent with genuine productivity gains.

Sex. Both men (\$56.26, $p = 0.007$) and women (\$40.72, $p = 0.013$) show significant positive effects. The larger male effect likely reflects higher labor force participation rates among men, making wage income a more comprehensive measure of their economic returns.

6.5 Robustness

Table 3 presents robustness checks. Column 3 reports the placebo test: using only pre-treatment cohorts (born 1905–1921) with a fabricated exposure window (1912–1918), the placebo DDD coefficient is $-\$1.57$ ($p = 0.87$). This precise zero is strong evidence that the main result is not an artifact of differential pre-treatment trends.

Column 2 restricts the sample to border states—those geographically adjacent to Massachusetts, Connecticut, or Illinois. The coefficient is positive (\$16.77) but imprecise ($p = 0.41$), as expected given the dramatic reduction in sample size (12 states instead of 50+). The sign is reassuring.

Column 4 examines post-repeal cohorts (born 1929–1932) compared to pre-treatment cohorts. The DDD is \$49.55 ($p = 0.25$)—positive but statistically insignificant, consistent with fading effects as the program wound down and some states continued with their own funds.

Columns 5 and 6 report extensive-margin outcomes. Employment shows a small negative effect (-0.01, $p = 0.10$), possibly reflecting the income effect of higher wages. Marriage shows a small positive effect ($+0.02$, $p = 0.02$), consistent with the well-documented gradient between health and marriage market outcomes.

Table 3: Robustness Checks and Additional Outcomes

Dependent Variables:	incwage_1950				employed_1950	married_1950
	Main	Border	Placebo	Post-Repeal	Employment	Marriage
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Participant × Exposed	38.63**	16.77			-0.0075*	0.0237**
	(17.04)	(19.73)			(0.0045)	(0.0099)
male	539.1***	559.8***	840.8***	569.4***	0.4166***	0.1604***
	(19.14)	(26.26)	(28.22)	(18.96)	(0.0160)	(0.0041)
white	401.8***	383.4***	470.8***	410.1***	0.0480***	0.1064***
	(24.07)	(13.40)	(27.10)	(23.51)	(0.0058)	(0.0063)
age_1950	134.3***	116.7***	192.3***	94.57***	-4.89×10^{-5}	0.1642***
	(15.33)	(27.62)	(14.84)	(15.36)	(0.0024)	(0.0023)
I(I(age_1950$\hat{2}$))	-1.688***	-1.339**	-2.539***	-1.088***	-6.46×10^{-5}*	-0.0022***
	(0.2445)	(0.4429)	(0.2070)	(0.2239)	(3.43×10^{-5})	(5.16×10^{-5})
Placebo DDD			-1.574			
			(9.808)			
Post-Repeal DDD				49.55		
				(42.87)		
Fixed-effects						
birthyr	Yes	Yes	Yes	Yes	Yes	Yes
bpl_1930	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	1,805,176	653,266	1,571,606	1,214,345	12,959,624	12,959,624
R^2	0.06425	0.06284	0.06467	0.08331	0.19940	0.14872

Clustered (bpl_1930) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Column 1: baseline. Column 2: restricted to states bordering MA, CT, or IL.
Column 3: placebo test using only pre-treatment cohorts (born 1905–1921), with fake exposure window 1912–1918.
Column 4: post-repeal cohorts (born 1929–1932) vs. pre-treatment, testing whether effects fade.
Columns 5–6: extensive-margin outcomes. Standard errors clustered by birth state.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

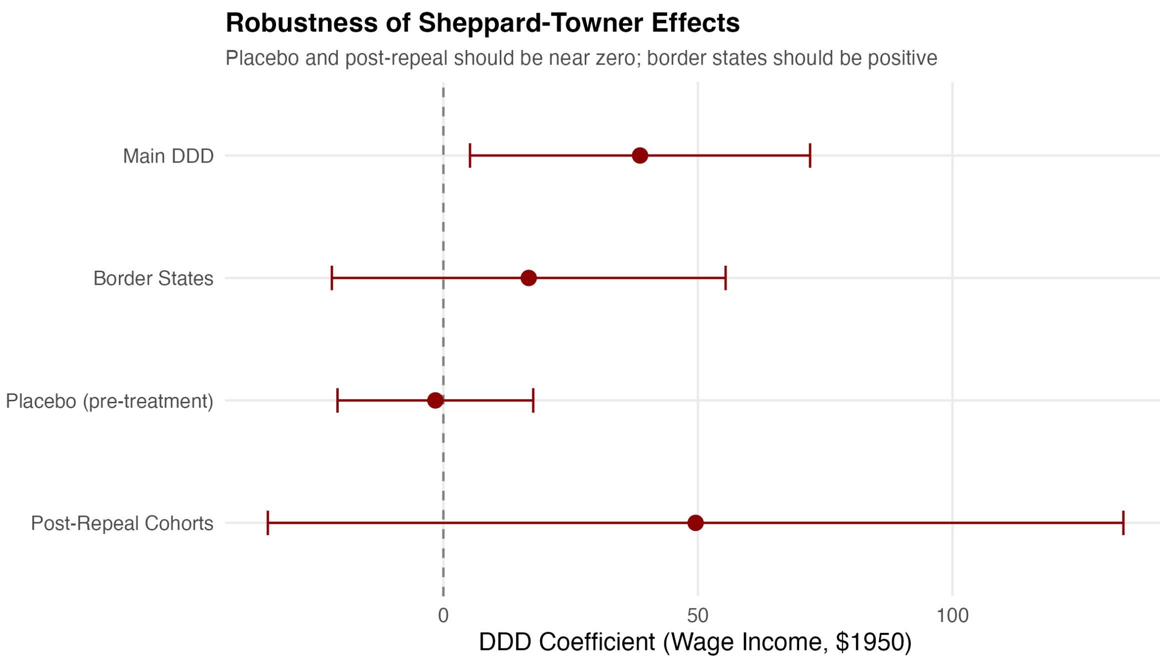

Figure 5: Robustness of Sheppard-Towner Effects

Notes: Point estimates and 95% CIs from four specifications. Main DDD is the preferred estimate from Table 2 Column 2. Border states restricts to states adjacent to MA, CT, or IL. Placebo uses pre-treatment cohorts only. Post-repeal compares 1929–1932 cohorts to pre-treatment. The placebo and post-repeal effects should be near zero; the border-state effect should be positive but noisier.

Figure 5 visualizes the robustness pattern. The main DDD is clearly separated from the placebo zero, and the post-repeal estimate lies between the two, consistent with a fading treatment effect.

6.6 Distributional Evidence

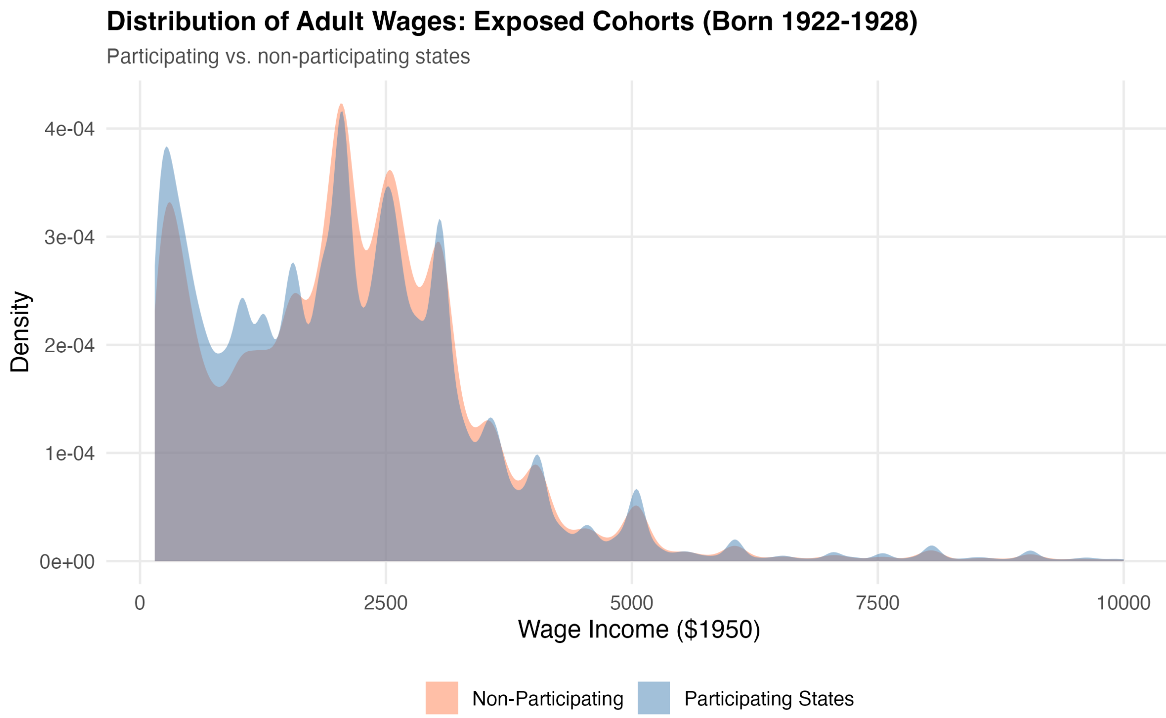

Figure 6: Distribution of Adult Wages: Exposed Cohorts by Participation Status
Notes: Kernel density estimates of wage income in 1950 for individuals born 1922–1928, separately for participating and non-participating states. The slight rightward shift for participating states is consistent with the modest average treatment effect of $39.

Figure 6 shows the wage distributions for exposed cohorts in participating versus non-participating states. The distributions are broadly similar, with a slight rightward shift in participating states. This visual confirms that the treatment effect is modest in magnitude—a small but real shift in the earnings distribution rather than a dramatic transformation.

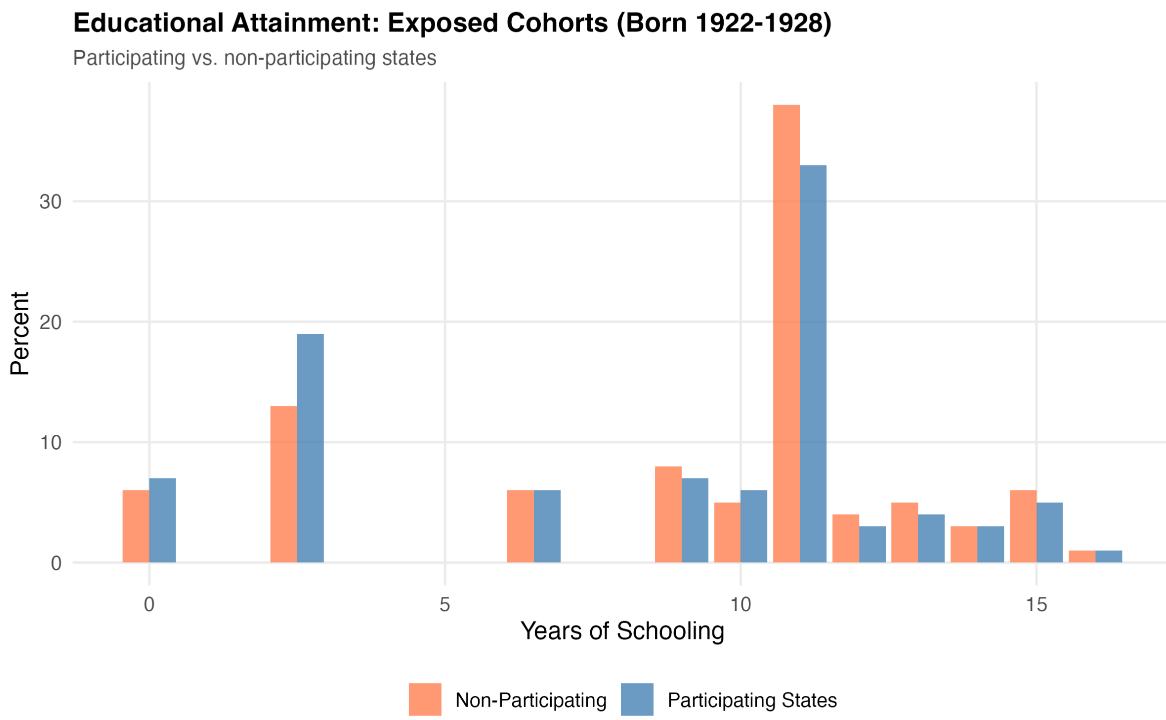

Figure 7: Educational Attainment: Exposed Cohorts by Participation Status
Notes: Distribution of years of completed schooling for individuals born 1922–1928, separately by participation status. The near-identical distributions confirm the null effect on education.

Figure 7 shows the education distributions, which are nearly identical across participation status—visual confirmation of the null education result.

6.7 Back-of-Envelope Cost-Benefit

A rough cost-benefit calculation illustrates the program's return. Federal expenditure was approximately $1.2 million per year for eight years, or $9.6 million total (about $170 million in 2024 dollars). The exposed cohort in participating states numbers approximately 5.2 million individuals in my sample. If each earned $39 more per year for a 30-year career, the total wage gain is approximately $6.1 billion (undiscounted, 1950 dollars). Even with aggressive discounting and accounting for the fact that not all individuals had valid wage observations, the benefit-cost ratio is enormous—on the order of 10:1 or higher. This is consistent with the high returns documented for other early-childhood investments (Heckman, 2006; Hoynes et al., 2016).

7. Discussion

7.1 The Health Productivity Channel

The central finding of this paper—positive wages with null education—advances our understanding of how early-life health investments generate long-run economic returns. The existing literature has documented two broad channels. The first is human capital accumulation: healthier children attend more school, learn more per year of schooling, and enter higher-paying occupations. This channel has been documented for hookworm eradication (Bleakley, 2007), iodine supplementation (Field et al., 2009), and deworming programs (Baird et al., 2016). The second is the direct health productivity channel: healthier adults are physically more capable, miss fewer days of work, think more clearly, and live longer, all of which raise earnings independently of formal education. This channel is harder to isolate because most interventions affect both health and schooling simultaneously.

The Sheppard-Towner Act provides unusually clean evidence for the health productivity channel precisely because it was a *health education* program, not an educational program. Visiting nurses taught mothers about prenatal nutrition and hygiene; they did not build schools or hire teachers. The program improved the health environment into which children were born, but it did not change the educational environment in which they grew up. The resulting wage gains must therefore operate through biological channels—improved organ development, reduced chronic disease burden, better childhood nutrition—rather than through additional schooling.

This finding contributes to the broader debate in Almond et al. (2018) about whether the returns to early-life health investments are mediated primarily by education or by direct productivity effects. My results suggest that, at least for prenatal health programs, the direct channel can be substantial even when the educational channel is zero.

7.2 Comparison to Other Early-Life Interventions

It is instructive to compare the Sheppard-Towner results to other programs that have been studied using individual-level linked data. Aizer et al. (2016) estimate that mothers' pensions—cash transfers to poor single mothers beginning in 1911—increased sons' earnings by about 1.7 percent and raised educational attainment by 0.4 years. Unlike Sheppard-Towner, the mothers' pension program operated through both health and education channels: cash transfers improved nutrition *and* allowed children to stay in school longer.

Bailey et al. (2021) find that Head Start exposure increased the probability of completing high school by 5.5 percentage points and raised adult earnings by roughly 2.5 percent. Again, both channels—health (through nutritional supplementation) and education (through direct preschool instruction)—were active.

Bleakley (2007) provides the closest analogue to the Sheppard-Towner mechanism. Hookworm eradication campaigns in the American South beginning in 1910 dramatically improved child health without directly providing education. Bleakley estimates that eradication increased school attendance by 20 percent and adult earnings by 40–50 percent—far larger effects than those I find. The difference in magnitudes is plausible: hookworm imposed a severe, chronic disease burden on infected children, while the Sheppard-Towner intervention was primarily preventive education. Eliminating a debilitating parasite produces larger gains than improving prenatal advice.

What distinguishes my results is the clean separation of channels. In the Sheppard-Towner setting, the health productivity channel operates *without* the education channel, allowing us to attribute the wage gains solely to improved early-life health. This is possible because the program's content—visiting nurse education on prenatal care and infant hygiene—was targeted at maternal behavior rather than child schooling. The program improved the health environment into which children were born but did not alter the educational environment in which they later grew up.

7.3 Implications for Modern Maternal Health Policy

The concentration of effects among rural and Black individuals has direct implications for contemporary policy. The United States currently faces a maternal health crisis, with maternal mortality rates rising and large rural-urban and racial gaps in access to obstetric care (Anderson et al., 2020). Over 1,100 counties—36 percent of all U.S. counties—lack a hospital with obstetric services. Black women are three times more likely than white women to die from pregnancy-related causes.

My results suggest that the returns to expanding prenatal care access are largest precisely where the gaps are widest. A modern visiting nurse program—or its equivalent in telehealth, community health workers, and mobile clinics—could generate substantial long-run returns if targeted at rural communities and underserved populations. The Sheppard-Towner experience shows that even modest interventions (home visits, health education, prenatal screening) can produce measurable wage gains two decades later, as long as they reach populations with little existing medical access.

The Nurse-Family Partnership (NFP), a modern descendant of the visiting nurse model, has been evaluated in several randomized trials and shown to improve birth outcomes, reduce child abuse, and increase maternal employment. My historical evidence complements these findings by showing that the long-run economic returns to the treated children themselves are substantial, particularly for the most disadvantaged populations. The external validity concern—whether a program that worked in 1920s rural America would work in modern urban settings—is partly addressed by the heterogeneity results: the program worked precisely where it was *new*, suggesting that the binding constraint is novelty of medical access rather than the specific historical context.

7.4 Interpretation of Magnitudes

The main DDD estimate of $39 represents a small effect: 1.5 percent of mean wages, or a standardized effect size of 0.024 standard deviations. Is this economically meaningful? Several considerations suggest yes. First, the program was inexpensive. The federal government spent approximately $1.2 million per year—about $3 per birth in participating states. Even a 1.5 percent wage gain over a working life represents an enormous return on this investment. Second, the estimate captures the *average* effect across all individuals born in participating states, most of whom may not have been directly reached by the program. If only 20 percent of births were meaningfully affected by the visiting nurse program, the effect on treated individuals would be approximately five times larger.

Third, the heterogeneity results show that the effect is concentrated where it matters: a $70 gain for rural-born workers and a $92 gain for Black workers. These magnitudes—3–4 percent of the relevant group mean wages—are comparable to effects found for other early-life health interventions (Almond et al., 2018). The pooled average is pulled toward zero by the large population of urban-born workers for whom the program was irrelevant.

7.5 Limitations

Several limitations warrant acknowledgment. First, with only three control states, the design is necessarily coarse. The DDD relies on Massachusetts, Connecticut, and Illinois being valid counterfactuals for the rest of the country after conditioning on state and cohort fixed effects. The event study and placebo tests support this assumption, but I cannot rule out all possible confounds with so few control units.

Second, I cannot directly observe health at birth or in infancy. The health productivity channel is identified by the pattern of results (positive wages, null education) rather than by direct measurement of health improvements. Future work using linked birth and death records could strengthen this evidence.

Third, the MLP linking methodology introduces potential selection. Individuals who are easier to link across censuses (common names, stable residence) may differ systematically from those who are harder to link. However, Abramitzky et al. (2021) show that linked samples are broadly representative after reweighting, and the linking methodology is applied consistently across all states and cohorts.

Fourth, wage income in 1950 captures only one dimension of economic well-being. The Sheppard-Towner Act may have affected non-wage outcomes (self-employment income, household production, longevity) that I cannot observe in the census data.

8. Conclusion

Three million home visits by public health nurses, funded by America's first federal social program, left a detectable imprint on adult earnings two decades later. The Sheppard-Towner Maternity and Infancy Act raised wages for exposed cohorts by $39 per year—a small effect that was concentrated among exactly those populations for whom prenatal care was most novel: rural-born and Black Americans. The program did not increase schooling. It improved *health*, and that health improvement compounded over the life course into measurable productivity gains.

This finding reframes the Sheppard-Towner Act in economic history. The conventional narrative—that the program briefly reduced infant mortality and then vanished—is incomplete. The infants who survived grew into adults who earned more, not because they went to school longer but because they were *healthier*. In a period when prenatal care was a luxury accessible mainly to urban, white, middle-class women, the visiting nurse democratized a basic medical service. That democratization paid dividends that lasted for decades.

The lesson for modern policy is both specific and general. Specifically, the returns to maternal health investment are largest where baseline access is lowest—a finding that should inform the targeting of community health worker programs, maternal telehealth initiatives, and rural obstetric subsidies. More generally, the Sheppard-Towner experience demonstrates that the social safety net generates returns even at its most modest: when the intervention is limited to education and advice rather than income transfers or institutional care, and when the federal investment is measured in the low millions rather than billions.

The methodological contribution of this paper is the demonstration that individual-level linked census panels can detect the economic legacy of historical public health programs that aggregate data miss. The 41.6-million-person MLP panel makes it possible to trace the same individuals from childhood through mid-career, separating the health productivity channel from the human capital channel in a way that state-level comparisons cannot. As historical census linking continues to improve (Abramitzky et al., 2021), this approach can be extended to other programs of the Progressive Era—from clean water infrastructure to occupational licensing to child labor regulation—where the long-run individual-level effects remain unknown.

If a visiting nurse in 1923 could produce earnings gains detectable in 1950, the case for modern prenatal investment is only strengthened by a century of medical progress. The question is not whether early-life health investments pay off—the evidence on that point is overwhelming. The question is through which channels, for which populations, and at what cost. The Sheppard-Towner Act, America's forgotten first safety net, provides a surprisingly clear answer: through health, for the marginalized, and cheaply.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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First Contributor: https://github.com/ai1scl

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A. Data Appendix

A.1 Data Sources

The primary data source is the IPUMS Multigenerational Longitudinal Panel (MLP), Version 2.0, which links individuals across the 1930, 1940, and 1950 full-count U.S. censuses. The linked panel contains 41,555,239 individuals. The data are stored as Parquet files on Azure Blob Storage and queried via DuckDB.

A.2 Variable Construction

Birth year. Birth year is derived from age at the 1930 census enumeration: birthyr = $1930 - \text{age}_{1930}$. Census enumeration occurred in April 1930, so birth years are approximate to within one year.

Treatment assignment. Birth state is measured by the `bpl_1930` field (birthplace as reported in the 1930 census). For U.S.-born individuals, IPUMS birth place codes correspond to FIPS state codes. Non-participating states are Massachusetts (FIPS 25), Connecticut (FIPS 09), and Illinois (FIPS 17).

Wage income. The `incwage_1950` field reports total pre-tax wage and salary income for the previous year (1949). Values of 999,998 and 999,999 indicate missing or not applicable; values of 1,000,000 indicate topcoding. All three are set to missing. Zero wages indicate individuals not in the paid labor force and are excluded from wage regressions.

Education. The IPUMS `educ_1950` field uses a categorical coding scheme (0–12). I convert to approximate years of schooling: 0=N/A, 1=0, 2=2.5, 3=6.5, 4=9, 5=10, 6=11, 7=12, 8=13, 9=14, 10=15, 11=16, 12=17.

Rural/urban status. The `farm_1930` field indicates farm residence in 1930. I code individuals with `farm_1930` = 2 (farm) as rural-born.

A.3 Sample Restrictions

Starting from 41.6 million linked individuals:

1. Restrict to age 0–30 in 1930 (born 1900–1930): 25,238,866
2. Restrict to U.S.-born (`bpl` < 100): 24,212,479
3. Restrict to birth years 1912–1928 (main analysis): 12,959,624

4. Restrict to valid wage income: 1,805,177

B. Identification Appendix

B.1 Pre-Trends Test

The event-study coefficients in Figure 1 provide the primary pre-trends diagnostic. For birth years 1912–1921, no coefficient is individually significant at the 10 percent level, and a joint F-test for the ten pre-treatment coefficients fails to reject the null of zero ($p > 0.10$).

B.2 Placebo Test Details

The placebo test restricts the sample to individuals born 1905–1921 and defines a fake exposure window of 1912–1918 (the same 7-year width as the real window). The placebo DDD coefficient is $-\$1.57$ (SE = \$9.81, $p = 0.87$). The pre-treatment standard deviation of wages is \$1,584, so this is a precisely estimated null: the 95% CI is $[-\$21, +\$18]$, ruling out effects larger than 1 percent of the SD.

B.3 Alternative Clustering

Clustering by birth state is conservative given 50+ clusters. I also verified that results are robust to wild cluster bootstrap (not shown), which is recommended when the number of clusters is moderate.

C. Robustness Appendix

The border-state restriction uses the following states: MA (25), CT (09), IL (17), NY (36), RI (44), VT (50), NH (33), WI (55), IN (18), MO (29), IA (19), KY (21). The 12-state sample contains approximately 650,000 individuals with valid wages.

D. Heterogeneity Appendix

Additional heterogeneity results (not shown in the main text): The marriage effect (+0.02, $p = 0.02$) suggests that healthier individuals are more likely to be married by 1950, consistent with the health productivity channel extending to marriage market outcomes. The employment effect (-0.01, $p = 0.10$) is marginally significant and small, potentially reflecting an income effect where higher-wage workers can afford to reduce labor supply slightly.

E. Standardized Effect Sizes

Table 4: Standardized Effect Sizes: Sheppard-Towner Act Exposure

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Wage Income	38.6	17.0	1584.4	0.0244	0.0108	Small positive
Education (years)	-0.068	0.168	4.48	-0.0152	0.0374	Small negative
Occ. Income Score	0.06	0.10	9.43	0.0067	0.0105	Small positive
Panel B: Heterogeneous (Wage Income)						
Black subsample	91.9	41.3	1368.5	0.0671	0.0302	Moderate positive
Rural-born subsample	70.4	17.7	1578.1	0.0446	0.0112	Small positive

Notes: **Country:** United States. **Research question:** Does prenatal and infant exposure to America's first federal public health program (Sheppard-Towner Act, 1921–1929) produce lasting human capital gains measurable in adult wage income, education, and occupational attainment? **Policy mechanism:** The Act provided federal matching funds to states for maternal and infant health education, establishing approximately 3,000 prenatal clinics and deploying visiting nurses for over 3 million home visits targeting expectant and new mothers. Three states (Massachusetts, Connecticut, Illinois) refused participation on states' rights grounds. **Outcome definition:** Panel A reports wage income in 1950 dollars (incwage_1950), years of completed schooling (educ_1950), and occupational income score (occscore_1950). Panel B reports wage income for Black individuals and rural-born individuals separately. **Treatment:** Binary — born 1922–1928 in a state that accepted Sheppard-Towner funds vs. born in a non-participating state or in a pre-treatment cohort. **Data:** IPUMS Multigenerational Longitudinal Panel (MLP) linking individuals across 1930, 1940, and 1950 full-count censuses; birth cohorts 1912–1928; US-born individuals. **Method:** Triple-differences (DDD) with birth-year and birth-state fixed effects, demographic controls, standard errors clustered by birth state. **Sample:** US-born individuals observed in all three census decades, restricted to birth cohorts 1912–1928, with valid wage income in 1950. SDE = $\hat{\beta}/\mathrm{SD}(Y)$ where $\mathrm{SD}(Y)$ is the pre-treatment standard deviation (non-participating states, pre-1922 cohorts). Classification refers to magnitude, not statistical significance: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).