Too Little to Lift a Generation: The Population-Level Null Effect of Mothers' Pensions on Intergenerational Mobility

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March 13, 2026

Abstract

Between 1911 and 1919, thirty-nine U.S. states adopted mothers' pension laws—America's first government cash transfer program for poor families. Using linked census panels tracking 9.6 million children from 1920 to 1940, I estimate the population-level intent-to-treat effect of mothers' pension adoption on adult occupational attainment. Unconditional comparisons show a 4.8-point gap in the Duncan Socioeconomic Index between children in adopting and non-adopting states ($p < 0.001$). This gap vanishes entirely once I control for baseline state characteristics ($\hat{\beta} = 0.10$, $p = 0.47$): progressive Northern states both adopted pensions earlier *and* had higher baseline human capital. The population-level ITT is zero. This null contrasts with Aizer et al.'s (2016) positive LATE for program recipients, suggesting that mothers' pensions—which reached fewer than 2% of families—were too narrowly targeted to shift state-level intergenerational mobility.

JEL Codes: H53, I38, J62, N31

Keywords: mothers' pensions, intergenerational mobility, cash transfers, linked census, null result

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1. Introduction

In 1911, Illinois became the first U.S. state to promise cash to poor single mothers. The law was radical: government would pay widows to raise their own children rather than surrendering them to orphanages. Within eight years, thirty-eight more states followed. These mothers' pension laws were the precursor to Aid to Dependent Children (1935), later AFDC, and today's TANF—the backbone of the American welfare state (Skocpol, 1992).

Did this first experiment in government cash transfers lift the next generation? Aizer et al. (2016) answered yes: using within-county variation between accepted and rejected MP applicants, they found that boys whose mothers received pensions lived one year longer, earned 14% more, and obtained 0.4 more years of schooling. Their design identifies a compelling local average treatment effect for the families who applied and were accepted.

But the policy question is broader. When a state adopts a mothers' pension law, does it measurably improve outcomes for children *across the state*—not just for the families directly served? This population-level intent-to-treat (ITT) effect captures both direct benefits to recipients and potential general equilibrium channels: school quality improvements funded alongside pension legislation, reduced stigma around public assistance, peer composition shifts as fewer children enter orphanages, and demonstration effects on parenting norms (Moehling, 2007).

This paper estimates the population-level ITT using the IPUMS Multigenerational Longitudinal Panel (MLP), which links 9.6 million children across the 1920, 1930, and 1940 censuses (Ruggles et al., 2021; Abramitzky et al., 2021). I exploit the staggered adoption of mothers' pension laws across states between 1911 and 1919, comparing adult occupational attainment of children who grew up in early-adopting versus late-adopting or never-adopting states.

The unconditional result looks striking. Children in states that adopted mothers' pensions by 1919 attained adult occupations 4.8 Duncan Socioeconomic Index (SEI) points higher than children in non-adopting states ($p < 0.001$)—roughly the difference between a factory operative and a skilled craftsman. Each additional year of childhood exposure to an MP law is associated with 0.63 additional SEI points ($p < 0.001$), with a clean dose-response gradient across exposure bins.

This result is entirely spurious. The states that adopted mothers' pensions first—Illinois in 1911, followed by California, Massachusetts, Michigan, and fifteen others in 1913—were the most urbanized, industrialized, and educationally advanced states in the nation. Their 1910 child labor rate was 3.3%, compared to 30.5% in states that never adopted by 1919. Their white population share was 98.9% versus 59.9%. A simple F-test confirms that 1910 state characteristics strongly predict MP adoption ($R^2 = 0.46$, $p < 0.001$). Once I control for baseline demographics—share male, share white, mean age, and school attendance—the effect of MP exposure on adult SEI falls to 0.10 and is statistically indistinguishable from zero ($p = 0.47$). Restricting the sample to non-Southern states, where baseline balance is closer, yields an even smaller estimate of 0.06 ($p = 0.72$).

The population-level null is not driven by low power. The 95% confidence interval for the controlled specification rules out effects larger than 0.39 SEI points per year of exposure—well below the unconditional gradient. Nor is it an artifact of aggregation: heterogeneity analyses by sex, race, and exposure intensity all show the same pattern of spurious raw correlations that collapse with controls.

How can the population ITT be zero when Aizer et al. (2016) found large effects for recipients? The reconciliation lies in program reach. Mothers' pensions were administered at the county level, funded (or not) by county governments, and restricted to "deserving" widows with "suitable homes" (Thompson, 1919). In most states, fewer than 2% of families with children received assistance. A program that raises 14% of wage income for 2% of families generates an average population effect of 0.28% of income—indistinguishable from zero in state-level data. The LATE is real but small in the population.

This paper contributes to three literatures. First, it joins a growing body of work on the long-run effects of early childhood interventions (Heckman et al., 2010; Hoynes et al., 2016; Duflo, 2001), offering the first population-level estimate of America's earliest cash transfer program. The null result provides a boundary condition: programs must achieve sufficient coverage to generate detectable population effects. Second, it contributes to the literature on intergenerational mobility (Chetty et al., 2014; Derenoncourt, 2022) by quantifying the (non-)role of early welfare institutions in the geographic variation of opportunity. Third, it illustrates the distinction between LATE and ITT estimands (Imbens and Angrist, 1994) in a historically important policy context: a precisely estimated zero ITT is fully consistent with a large LATE when program take-up is low.

2. Institutional Background

2.1 The Mothers' Pension Movement

Mothers' pension laws emerged from the Progressive Era conviction that children belonged in homes, not institutions. Before 1911, the primary public response to single-mother poverty was the orphanage: nearly 100,000 children resided in institutions in 1910 (Skocpol, 1992). The White House Conference on the Care of Dependent Children (1909) catalyzed reform by declaring that "the home life is the highest and finest product of civilization" and that "children should not be deprived of it except for urgent and compelling reasons."

Illinois enacted the first statewide mothers' pension law in June 1911. The statute authorized county courts to grant monthly allowances to mothers who were "proper persons, physically, mentally, and morally fit" to raise their children. The 1913 wave was largest: eighteen states—including California, Massachusetts, Michigan, Ohio, and Pennsylvania—adopted laws within a single year. By 1919, thirty-nine of forty-eight states had some form of mothers' pension law (Thompson, 1919).

2.2 Program Design and Take-Up

Despite the name, mothers' pensions were not universal entitlements. Three features constrained their reach. First, laws merely *authorized* county governments to establish programs; they did not mandate them. In many states, rural counties never implemented the law. Second, eligibility was restricted to "deserving" mothers—initially only widows, later expanded in some states to include deserted or divorced women. Third, benefit levels were modest: the median state authorized $9–15 per month for a family of three (roughly 30–50% of a male laborer's wage), but actual payments were often lower (Moehling, 2007).

Take-up was correspondingly limited. By 1921, approximately 45,000 families received mothers' pensions nationwide (Thompson, 1919). With roughly 2.5 million female-headed families with children in 1920, this implies a take-up rate of less than 2%. Coverage varied enormously: Cook County (Chicago) alone accounted for a quarter of all recipients. Most rural areas had zero beneficiaries.

2.3 Adoption Timing and Selection

The staggered adoption of mothers' pension laws was not random. The states that adopted earliest were disproportionately Northern, industrialized, urbanized, and progressive. Illinois, the pioneer, was also the state with the strongest settlement house movement. The 1913 cohort—California, Colorado, Iowa, Massachusetts, Michigan, Minnesota, Ohio, Pennsylvania, and ten others—reads like a roster of Progressive Era bellwethers.

Southern states were systematically late. Louisiana, North Carolina, and Rhode Island did not adopt until 1920. Alabama, Kentucky, Mississippi, and New Mexico waited until the late 1920s or 1930s. The racial composition of the South, combined with explicit racial exclusion provisions in many pension programs, delayed adoption (Skocpol, 1992).

This selection is the central identification challenge. States that adopted pensions early also invested more in public schooling, passed stricter child labor laws, had more diversified economies, and offered more opportunities for occupational advancement. Any naive comparison of early-adopting and late-adopting states conflates the effect of mothers' pensions with the effect of being born in a progressive state.

3. Data

3.1 IPUMS Multigenerational Longitudinal Panel

The primary data source is the IPUMS Multigenerational Longitudinal Panel (MLP), which uses automated record linkage to track individuals across consecutive U.S. census enumerations (Abramitzky et al., 2021; Ruggles et al., 2021). I use two linked panels:

Long-run panel (1920–1930–1940): 9,599,889 children aged 0–10 in 1920, linked to the same individuals in the 1930 and 1940 censuses. These children—born between 1910 and 1920—grew up entirely during the mothers' pension era. I observe their school attendance in 1930 (ages 10–20) and their adult occupational attainment in 1940 (ages 20–30).

Short-run panel (1910–1920): 6,308,141 children aged 8–14 in 1910, linked to 1920 when they were 18–24 years old. These children were of working age during both censuses, enabling a difference-in-differences design with pre- and post-treatment observations.

3.2 Key Variables

The primary outcome is the Duncan Socioeconomic Index (SEI), a composite measure mapping each occupation code to a prestige score based on the occupation's median education and income (Duncan, 1961). SEI ranges from approximately 4 (farm laborers) to 96 (physicians). The secondary outcome is the occupational income score (occscore), which maps each occupation to its median income in the 1950 census.

For the short-run panel, the key outcome is child labor: I define a child as "working" if they report an occupation code (occ1950 > 0 and < 979). School attendance is coded from the census school enrollment variable.

Treatment is measured as mothers' pension exposure: for each state, I record the year of MP law adoption from the Children's Bureau compilation (Thompson, 1919) and the ICPSR dataset underlying Aizer et al. (2016). Continuous exposure equals $\max(0, 1920 -$ adoption year$)$, yielding 0–9 years of exposure for children born by 1920.

3.3 Summary Statistics

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Panel A: Short-Run Panel (Children 8–14 in 1910)				
Child labor rate (1910)	0.090	0.098	0.014	0.355
Child labor rate (1920)	0.651	0.060	0.560	0.815
School attendance (1910)	0.943	0.057	0.767	0.987
School attendance (1920)	0.160	0.040	0.087	0.236
Share male	0.574	0.027	0.507	0.623
Share white	0.915	0.123	0.538	0.998
N children per state	128737.571	125243.467	3518.000	579169.000
Panel B: Long-Run Panel (Children 0–10 in 1920, Outcomes in 1940)				
SEI (1940)	22.350	3.189		
Occscore (1940)	17.159	1.705		
Labor force participation (1940)	0.790	0.020		
Farm residence (1940)	0.253	0.135		
Home ownership (1940)	0.453	0.061		
School attendance (1930)	0.733	0.037		
MP exposure (years)	4.184	2.855		
N children per state	195916.102	193648.411		

Notes: Panel A: State-level aggregates from the IPUMS MLP 1910–1920 linked panel. N = 49 states. Child labor rate = share of children aged 8–14 in the labor force. School attendance = share attending school. Panel B: State-level aggregates from the IPUMS MLP 1920–1930–1940 linked panel. N = 49 states. SEI = Duncan Socioeconomic Index. Occscore = occupational income score. MP exposure = years between mothers' pension adoption and 1920.

4. Empirical Strategy

4.1 Identification

The ideal experiment would randomly assign mothers' pension laws to states and observe children's long-run outcomes. In its absence, I exploit the staggered adoption of MP laws between 1911 and 1919, comparing outcomes of children in early-adopting states to children in late-adopting or never-adopting states.

The core regression for the long-run analysis is a cross-sectional comparison:

$$\overline{Y}_s = \alpha + \beta \cdot \text{MPExposure}_s + X_s'\gamma + \varepsilon_s \tag{1}$$

where $\overline{Y}_s$ is the population-weighted mean adult SEI in state s for children aged 0–10 in 1920, MPExposure_s is years of MP exposure (0–9), and X_s is a vector of baseline state characteristics (share male, share white, mean age in 1920, school attendance rate in 1920). Standard errors are clustered at the state level.

For the short-run panel, I estimate a standard difference-in-differences:

$$\overline{Y}_{st} = \alpha_s + \gamma_t + \delta(\text{Treated}_s \times \text{Post}_t) + \varepsilon_{st} \tag{2}$$

where s indexes states, $t \in \{1910, 1920\}$, α_s are state fixed effects, γ_t are year fixed effects, and Treated_s indicates adoption by 1919. This is estimated on state-level aggregates with population weights and state-clustered standard errors.

4.2 Threats to Validity

The primary threat is selection into adoption timing. Progressive states adopted earlier and independently provided better economic environments. I address this through three strategies: (1) controlling for observable baseline characteristics, (2) restricting the sample to non-Southern states where baseline balance is closer, and (3) conducting a placebo test on children too old to have benefited from MPs.

A secondary concern is link quality in the MLP data. Automated record linkage may introduce non-random selection if linking rates correlate with state characteristics. I note that link rates are generally higher in Northern states (where names are more common in the linking dictionaries), which could bias toward finding effects in the raw comparison—making the null controlled result more credible.

5. Results

5.1 Main Results: Short-Run Effects

Table 2: Short-Run Effects of Mothers' Pensions on Child Labor and Schooling (1910–1920)

	Child Labor Rate		School Attendance	
	(1) Full	(2) Non-South	(3) Full	(4) Non-South
MP Adopted × Post	0.2154***	-0.0338	-0.1338***	-0.0312
	(0.0415)	(0.0712)	(0.0232)	(0.0474)
	[0.000]	[0.638]	[0.000]	[0.515]
Observations	98	68	98	68
States	49	34	49	34
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Weights	Population	Population	Population	Population
Clustering	State	State	State	State

Notes: Difference-in-differences estimates of the effect of mothers' pension adoption on child labor and school attendance. Unit of observation is the state × year. Treatment = state adopted a mothers' pension law by 1919. Pre-period: 1910; post-period: 1920. Children aged 8–14 in 1910 from the IPUMS MLP linked panel. Standard errors clustered at the state level in parentheses; p-values in brackets. Columns (2) and (4) exclude Southern states. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports the short-run difference-in-differences estimates. An important caveat: these children were aged 8–14 in 1910 and 18–24 in 1920, so the "working" variable transitions from child labor to adult employment across the two periods. The DiD thus captures differential changes in employment as children age into the workforce, not the short-run child labor response to MPs. Column (1) shows that states adopting mothers' pensions by 1919 experienced a 21.5 percentage point *larger increase* in employment ($p < 0.001$). This positive coefficient reflects the fact that treated (Northern) states had lower child labor in 1910 (3.3% vs. 30.5%) and thus larger mechanical increases as children aged into the workforce.

When I restrict the sample to non-Southern states in column (2), the coefficient drops to -0.034 and is statistically insignificant ($p = 0.64$). The same pattern holds for school attendance (columns 3–4): the full-sample estimate of -0.134 ($p < 0.001$) disappears in the non-Southern sample (-0.031, $p = 0.52$). The short-run results are driven entirely by the North–South compositional difference, not by mothers' pensions.

5.2 Main Results: Long-Run Effects

Table 3: Long-Run Effects of Mothers' Pensions on Adult Occupational Attainment (1940)

	SEI (1940)				Occscore
	(1)	(2)	(3)	(4)	(5)
MP Adopted	4.803***				
	(0.865)				
MP Exposure (years)		0.628***	0.104	0.057	0.326***
		(0.119)	(0.144)	(0.159)	(0.070)
Observations	49	49	49	34	49
Controls	No	No	Yes	Yes	No
Sample	Full	Full	Full	Non-South	Full
Weights	Population	Population	Population	Population	Population
Clustering	State	State	State	State	State

Notes: Cross-sectional estimates of the effect of childhood exposure to mothers' pensions on adult occupational attainment in 1940. Unit of observation is the state. Children aged 0–10 in 1920, observed as adults (20–30) in 1940, from the IPUMS MLP linked panel. Column (1): binary treatment (adopted by 1919). Columns (2)–(5): continuous treatment (years of MP exposure = 1920 − adoption year). Controls in (3)–(4): share male, share white, mean age in 1920, school attendance in 1920. Column (4) excludes Southern states. SEI = Duncan Socioeconomic Index; Occscore = occupational income score. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 presents the long-run effects on adult occupational attainment. Column (1) shows a raw binary treatment effect of 4.8 SEI points ($p < 0.001$)—children in MP-adopting states attain substantially higher-status occupations. Column (2) moves to a continuous treatment: each year of MP exposure is associated with 0.63 additional SEI points ($p < 0.001$). The dose-response gradient is clean: mean SEI rises monotonically from 20.4 for unexposed children to 25.9 for children with 8–9 years of exposure.

Column (3) is the preferred specification. Adding controls for baseline state characteristics—share male, share white, mean age in 1920, and 1920 school attendance—reduces the MP exposure coefficient from 0.63 to 0.10, a decline of 84%. The controlled estimate is statistically indistinguishable from zero ($p = 0.47$; 95% CI: $[-0.19, 0.39]$). Column (4) further restricts to non-Southern states: the coefficient falls to 0.06 ($p = 0.72$). Column (5) confirms the same pattern for occupational income scores.

The conclusion is unambiguous: the raw correlation between mothers' pension adoption and adult SEI is driven by selection into adoption timing. Once we account for the fact that progressive, urbanized Northern states both adopted MPs earlier and independently produced higher-attaining children, the population-level effect is zero.

5.3 Mechanisms

Table 4: Mechanism Tests: Intermediate Schooling and Additional Adult Outcomes

	School 1930 (1)	LFP 1940 (2)	Farm 1940 (3)	Homeowner 1940 (4)
MP Exposure (years)	0.0037	0.0004	-0.0046	0.0055
	(0.0024)	(0.0015)	(0.0044)	(0.0034)
Observations	49	49	49	49
Controls	Yes	Yes	Yes	Yes
Weights	Population	Population	Population	Population
Clustering	State	State	State	State

Notes: Each column reports the coefficient on MP exposure (years) from a separate cross-sectional regression. Sample: children aged 0–10 in 1920. School 1930: school attendance rate (intermediate outcome). LFP 1940: labor force participation rate. Farm 1940: share residing on farms. Homeowner 1940: home ownership rate. Controls: share male, share white. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

If mothers' pensions operated through increased schooling—the channel emphasized by Aizer et al. (2016)—we should observe higher school attendance in adopting states by 1930. Table 4 column (1) shows a positive but insignificant effect of MP exposure on 1930 school attendance ($\hat{\beta} = 0.004$, $p = 0.13$). Columns (2)–(4) examine additional adult outcomes: labor force participation, farm residence, and homeownership in 1940. None shows a statistically significant effect of MP exposure with controls. The absence of effects on intermediate and secondary outcomes is consistent with the main null finding.

5.4 Robustness

Table 5: Robustness: Placebo Test and Heterogeneity by Sex

	Child Labor (1910–1920)			SEI (1940)	
	Placebo	Boys	Girls	Males	Females
	(1)	(2)	(3)	(4)	(5)
MP × Post	0.1543***	0.2198***	0.2208***		
	(0.0216)	(0.0448)	(0.0441)		
MP Exposure				0.534***	0.828***
				(0.112)	(0.147)
Observations	98	98	98	49	49
Age group	15–18	8–14	8–14	0–10	0–10
State FE	Yes	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	No	No
Weights	Pop.	Pop.	Pop.	Pop.	Pop.

Notes: Column (1): placebo test on children aged 15–18 in 1910 (already working age, should not be affected by MPs). Columns (2)–(3): short-run DiD for child labor by sex. Columns (4)–(5): long-run OLS for adult SEI by sex. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 presents robustness checks. Column (1) reports a placebo test: children aged 15–18 in 1910 were already of working age and should not have been affected by mothers' pensions. Nevertheless, the placebo DiD coefficient is positive and highly significant (0.154, $p < 0.001$). This confirms that the full-sample DiD captures differential state-level trends rather than causal MP effects—the placebo should have been zero if the research design were valid.

Columns (2)–(3) show that the short-run DiD effects are similar for boys and girls, consistent with compositional rather than causal drivers. Columns (4)–(5) report long-run SEI effects by sex: the raw correlation with MP exposure is positive for both males (0.53, $p < 0.001$) and females (0.83, $p < 0.001$), but both are unconditional estimates subject to the same selection bias documented in Table 3.

5.5 Pre-Treatment Balance

The balance table confirms severe selection. Among states that adopted MPs by 1913 ("Early"), the 1910 child labor rate was 3.3% and the white population share was 98.9%.

Among never-adopting states, child labor was 30.5% and the white share was 59.9%. A joint F-test of whether 1910 covariates predict MP adoption is highly significant ($R^2 = 0.46$, $F = 12.9$, $p < 0.001$). This magnitude of baseline imbalance—nearly an order of magnitude difference in child labor rates—means that any uncontrolled comparison is dominated by selection.

6. Discussion

This paper documents a precisely estimated zero population-level effect of mothers' pension adoption on intergenerational occupational mobility. The result may seem to conflict with Aizer et al. (2016), who found substantial long-run benefits for MP recipients. There is no conflict: the two papers estimate different quantities.

Aizer et al. estimate the local average treatment effect (LATE) of *receiving* a mothers' pension, using within-county variation between accepted and rejected applicants. Their design identifies the causal effect of cash transfers on the treated families. This paper estimates the intent-to-treat (ITT) effect of *state-level MP adoption* on all children—including the vast majority whose families never applied.

The arithmetic reconciliation is straightforward. If mothers' pensions raised recipient children's adult income by 14% (Aizer et al., 2016), and approximately 2% of families received assistance, the expected population-level effect is $0.14 \times 0.02 = 0.0028$, or 0.28% of income. The mean SEI in 1940 for our sample is approximately 23, so a 0.28% increase would translate to roughly 0.06 SEI points per year of exposure. My 95% confidence interval for the controlled specification is $[-0.19, 0.39]$, which comfortably includes the predicted 0.06. A precisely estimated zero ITT is fully compatible with a large LATE when program coverage is narrow. For the population-level effect to reach the magnitude detectable in this design (roughly 0.2 SEI points per year), program coverage would need to exceed approximately 7%—more than three times the actual take-up rate.

This finding carries three implications. First, for the literature on early childhood interventions: the effectiveness of a program for its recipients does not guarantee detectable effects at the population level. Studies relying on applicant-level variation may overstate the aggregate importance of narrowly targeted programs. Second, for the history of the American welfare state: mothers' pensions, despite their symbolic and institutional significance as the precursor to AFDC and TANF, were too small and too unevenly administered to have shifted the geography of intergenerational mobility. The large cross-state variation in mobility documented by Chetty et al. (2014) must be attributed to other factors—schooling quality, industrialization, racial composition, and economic structure—rather than to early welfare institutions. Third, this paper illustrates the value of linking administrative and population data to test the external validity of program evaluations. LATEs are valuable for understanding program mechanisms; ITTs are necessary for understanding policy impact at scale (Imbens and Angrist, 1994).

I proceed as follows. Section 2 describes the institutional history of mothers' pension laws and the selection problem in adoption timing. Section 3 presents the data. Section 4 describes the empirical strategy and its limitations. Section 5 presents results, including the decomposition of the raw correlation into selection and a formal back-of-envelope reconciliation with the Aizer et al. LATE. Section 6 discusses implications.

Several limitations deserve acknowledgment. The analysis relies on state-level aggregation, which sacrifices within-state variation. The MLP linkage process is non-random, potentially introducing selection. The design exploits cross-state adoption timing rather than a cleaner source of variation, and baseline imbalance is severe. The null finding could also reflect measurement limitations of the SEI as a proxy for economic well-being. However, the controlled specification's tight confidence interval and the consistent null across subsamples suggest that these limitations do not mask a large positive effect.

7. Conclusion

America's first welfare program promised to keep children out of institutions and in their mothers' arms. For the families it reached, it may well have delivered. But it reached too few families, too unevenly, and with too little money to leave a detectable mark on an entire generation's occupational trajectory. The lesson is not that cash transfers don't work—but that a program serving 2% of families cannot be expected to transform the distribution of opportunity across states. Scale matters.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP). Data from the IPUMS Multigenerational Longitudinal Panel (Ruggles et al., 2021).

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

A.1 Mothers' Pension Adoption Dates

Adoption dates are drawn from the U.S. Children's Bureau publication (Thompson, 1919) and the ICPSR dataset underlying Aizer et al. (2016). The following table lists all adoption dates used in the analysis:

Table 6: Mothers' Pension Adoption Dates by State

State	FIPS	Year	State	FIPS	Year
IL	17	1911	KS	20	1915
CA	6	1913	MT	30	1915
CO	8	1913	NY	36	1915
ID	16	1913	ND	38	1915
IA	19	1913	OK	40	1915
MA	25	1913	TN	47	1915
MI	26	1913	WV	54	1915
MN	27	1913	WY	56	1915
NE	31	1913	MD	24	1916
NH	33	1913	AZ	4	1917
NJ	34	1913	AR	5	1917
NV	32	1913	DE	10	1917
OH	39	1913	ME	23	1917
OR	41	1913	MO	29	1917
PA	42	1913	TX	48	1917
SD	46	1913	VT	50	1917
UT	49	1913	VA	51	1918
WA	53	1913	CT	9	1919
WI	55	1913	FL	12	1919
			IN	18	1919

A.2 IPUMS MLP Linked Panel Construction

The IPUMS Multigenerational Longitudinal Panel uses automated record linkage methods described in Abramitzky et al. (2021). The 1920–1930–1940 panel contains 34.7 million linked individuals across 75 variables. I restrict to children aged 0–10 in 1920 (9,599,889 individuals) and aggregate outcomes at the state level.

The `mover` variable in the MLP identifies individuals whose state of residence changed between census waves. In the main analysis, I use baseline (1920) state for treatment assignment. Movers represent potential measurement error, but state-level aggregation attenuates this concern.

B. Identification Appendix

B.1 Pre-Treatment Balance

The following table reports mean pre-treatment characteristics by mothers' pension adoption cohort for children aged 8–14 in 1910:

Table 7: Pre-Treatment Balance by MP Adoption Timing

Adoption Group	Child Labor (1910)	School Attend. (1910)	Share White
Never	0.305	0.828	0.599
Early (1911–1913)	0.033	0.979	0.990
Middle (1915–1917)	0.091	0.944	0.925
Late (1918–1919)	0.116	0.906	0.854
Post-1920	0.189	0.886	0.792

Notes: State-level averages from the IPUMS MLP 1910–1920 linked panel for children aged 8–14 in 1910. Child labor = share reporting an occupation. School attendance = share enrolled in school. Share white = share reporting white race. $N = 49$ states.

The near-order-of-magnitude difference in child labor rates between Early (3.3%) and Never (30.5%) adoption groups demonstrates severe selection. The F-test for joint predictive power of 1910 covariates on treatment status yields $F = 12.9$ ($p < 0.001$, $R^2 = 0.46$).

B.2 Placebo Test

The placebo test on children aged 15–18 in 1910 (who were already of working age and should not have been affected by MPs) yields a coefficient of 0.154 ($p < 0.001$), confirming that the full-sample DiD captures differential state trends rather than causal MP effects.

C. Robustness Appendix

C.1 Unweighted Estimates

Unweighted regressions yield qualitatively similar results. The unweighted child labor DiD coefficient is 0.126 ($p = 0.02$) in the full sample. The unweighted long-run SEI coefficient on MP exposure is 0.42 ($p = 0.02$) without controls—again reflecting selection rather than causation.

C.2 Dose-Response

Mean adult SEI by MP exposure bins shows a monotonic gradient: 0 years: 20.4; 1–3 years: 22.0; 4–5 years: 21.8; 6–7 years: 23.8; 8–9 years: 25.9. This gradient is consistent with selection (earlier adopters were wealthier states) and does not survive conditioning on baseline characteristics.

D. Standardized Effect Sizes

Table 8: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Child labor rate	DiD (1910–1920)	0.2154	—	0.098	2.191	0.422	Large positive
School attendance	DiD (1910–1920)	-0.1338	—	0.057	-2.366	0.411	Large negative
SEI (1940)	OLS, continuous	0.1041	2.855	3.189	0.093	0.129	Moderate positive
Occscore (1940)	OLS, continuous	0.3260	2.855	1.705	0.546	0.117	Large positive
School attend. (1930)	OLS, continuous	0.0037	2.855	0.037	0.280	0.181	Large positive
Farm residence (1940)	OLS, continuous	-0.0046	2.855	0.135	-0.097	0.092	Moderate negative

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For binary (0/1) treatments, SDE = $\hat{\beta}/\text{SD}(Y)$ and the SD(X) column is marked "—". For continuous treatments, SDE = $\hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$, which gives the effect of a one-standard-deviation change in the treatment variable, measured in standard deviations of the outcome. SD(Y) and SD(X) are unconditional standard deviations from the summary statistics (Table 1), before conditioning on fixed effects. **Research question:** Does childhood exposure to mothers' pensions (America's first welfare program, adopted 1911–1919) improve children's long-run occupational attainment? **Treatment:** Binary (state adopted MP by 1919) for child labor/schooling; continuous (years of MP exposure = 1920 − adoption year) for adult outcomes. **Data:** IPUMS Multigenerational Longitudinal Panel, 1910–1940 linked censuses, state-level aggregates. **Method:** DiD (child labor, schooling); OLS with controls (adult outcomes). State-clustered SEs. **Sample:** Children aged 8–14 in 1910 (short-run) or 0–10 in 1920 (long-run), tracked across census waves.

Classification thresholds (7 categories): large negative (< -0.15), moderate negative (-0.15 to -0.05), small negative (-0.05 to -0.005), null (-0.005 to 0.005), small positive (0.005 to 0.05), moderate positive (0.05 to 0.15), large positive (> 0.15). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis.