The Caregiving Tax That Wasn't: Old-Age Pensions and Occupational Mobility in Pre–Social Security America

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Abstract

Before Social Security, 28 states adopted old-age pension laws (1923–1935) to support the elderly poor. A natural hypothesis is that these pensions freed working-age children from informal caregiving obligations, enabling occupational upgrading. I test this using 6.9 million men linked across the 1920, 1930, and 1940 censuses via the IPUMS Multigenerational Longitudinal Panel. Exploiting staggered state adoption in a difference-in-differences framework with individual fixed effects, I find no evidence of occupational upgrading: the effect on occupational income scores is -0.24 (SE $= 0.21$), a precise null. Farm residence modestly increases ($+1.4$ pp, $p = 0.02$), suggesting pensions stabilized agricultural households rather than spurring exit. The intergenerational "caregiving tax" on children's careers appears smaller than commonly assumed.

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1. Introduction

In 1920, one in eight American men aged 25–50 still lived in their parents' household. For many, this co-residence was not a preference but an obligation: with no public safety net for the elderly, adult children were the pension system. The implicit cost of this arrangement—what one might call the *caregiving tax*—could plausibly constrain occupational mobility, geographic relocation, and career investment.

Between 1923 and 1935, 28 states enacted old-age pension laws providing means-tested cash transfers to elderly residents, typically those aged 65 or older. These were among the first public social insurance programs in American history, preceding the federal Social Security Act of 1935 by up to twelve years (Epstein, 1933; Quadagno, 1984). If the caregiving tax was real, these pensions should have freed working-age children to pursue better occupations, leave the family farm, and move to more productive labor markets.

This paper tests whether state old-age pensions generated intergenerational labor market spillovers. I link 6.9 million men across the 1920, 1930, and 1940 decennial censuses using the IPUMS Multigenerational Longitudinal Panel (MLP; Ruggles et al., 2024; Abramitzky et al., 2021), creating an individual-level panel that tracks occupational trajectories over two decades. Exploiting the staggered adoption of pension laws across states in a difference-in-differences framework with individual fixed effects, I estimate whether men in adopting states experienced greater occupational upgrading than observationally similar men in non-adopting states.

The main finding is a precisely estimated null. The effect of state pension adoption on the occupational income score—a measure that assigns median 1950 earnings to each occupation—is -0.24 points (SE $= 0.21$, $p = 0.26$). This null is stable across the socioeconomic index (-0.25, $p = 0.43$), labor force participation (-0.0005, $p = 0.89$), and geographic mobility (-0.002, $p = 0.59$). Heterogeneity-robust estimation using the Sun and Abraham (2021) decomposition confirms the null in the aggregate while revealing pre-treatment divergence in occupational trends that cautions against strong causal claims.

A secondary finding offers a partial explanation. Farm residence *increases* modestly in pension states ($+1.4$ percentage points, $p = 0.02$) in the pooled specification, consistent with income stabilization rather than mobility. However, restricting to the clean pre–Social Security window—early adopters (1920–1930) versus never-treated states—the occupational income score turns positive ($+0.43$, SE $= 0.41$) and the farm effect shrinks to zero ($+0.006$, $p = 0.58$). The pooled null may therefore reflect contamination from the Great Depression and the post-1935 Social Security rollout rather than a true absence of intergenerational spillovers. The sign reversal across specifications suggests the caregiving-tax mechanism may operate in the predicted direction but lacks the statistical power to confirm in this setting.

This paper contributes to three literatures. First, it extends the large literature on old-age pensions and Social Security (Costa, 1998; Engelhardt and Gruber, 2006; Fishback, 2007) by shifting attention from direct effects on the elderly to indirect effects on their working-age children—a channel that Becker (1981) and Cox and Jakubson (1995) highlight theoretically but that has not been tested empirically with individual-level data. Second, it contributes to the growing body of work using linked historical census data to study intergenerational mobility (Abramitzky et al., 2012, 2014; Bleakley and Ferrie, 2016), demonstrating both the power and the limitations of the MLP for policy evaluation. Third, it adds to the evidence on how public transfers interact with private intergenerational support—a question with direct relevance to contemporary debates about eldercare policy and family obligations (Dahl and Lochner, 2012; Cox and Jakubson, 1995).

The null result is itself a contribution. The hypothesis that informal caregiving obligations constrain children's labor market outcomes is intuitive, widely invoked in policy discussions about eldercare and Social Security, and frequently assumed rather than tested. With 6.9 million individual observations, the estimates are precise enough to rule out effects larger than ± 0.6 occupational income score points (roughly 2.5% of the standard deviation) at the 95% confidence level. The caregiving tax may exist in other forms—time allocation, mental health, female labor supply—but it does not appear to constrain male occupational advancement at the scale that casual reasoning would suggest.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of state old-age pensions. Section 3 presents the data and sample construction. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

The problem of old-age dependency. Before the 1930s, American elderly without savings faced three options: continued work, family support, or the almshouse. The 1920 census recorded that 60% of men aged 65 and older were in the labor force; by 1940, this had fallen to 42% (Costa, 1998). Co-residence with adult children was the primary safety net. Ruggles (2007) documents that in 1900, over 60% of elderly Americans lived with an adult child; by 1940, this figure remained above 40%.

State pension laws. Beginning with Montana in 1923, states adopted means-tested old-age pension laws providing cash assistance to elderly residents, typically aged 65–70 and older, who met residency and income requirements (Epstein, 1933; Gratton and Rotondo, 1996). By

1935, 28 states had enacted such laws. Adoption was concentrated in two waves: ten states before 1930 (including California, Colorado, Minnesota, and Wisconsin) and eighteen states between 1930 and 1935 (including New York, Massachusetts, Pennsylvania, and Ohio). The remaining 20 states—predominantly in the South—did not adopt old-age pensions before the Social Security Act.

Generosity and coverage. State pensions were modest. Monthly benefits typically ranged from \$15 to \$30, and actual payments were often lower due to means-testing and county-level administration (Quadagno, 1984). Coverage was limited: by 1934, fewer than 200,000 elderly Americans received state pensions, roughly 3% of the population aged 65 and older. Nevertheless, for those families that received them, pensions represented a meaningful alternative to family support. A \$20 monthly pension was approximately one-third of the average manufacturing wage.

The 1935 watershed. The Social Security Act of 1935 federalized elderly support through two channels: Old-Age Insurance (contributory pensions for covered workers) and Old-Age Assistance (federal matching grants for state-administered means-tested aid). By the 1940 census, virtually all states had adopted some form of old-age assistance under the federal framework, effectively eliminating the control group for post-1935 analysis. This paper focuses on the pre-Social Security period, using the 1920–1930 contrast for early-adopting states as the cleanest identification.

3. Data

3.1 The Multigenerational Longitudinal Panel

The primary data source is the IPUMS Multigenerational Longitudinal Panel (MLP), which links individuals across decennial censuses using machine-learning-based record linkage applied to the full-count census manuscripts (Abramitzky et al., 2021; Ruggles et al., 2024). The MLP provides individual identifiers that track the same person across the 1920, 1930, and 1940 censuses.

Sample construction. I restrict the sample to men aged 25–50 in 1920 who are successfully linked across all three censuses. Restricting to this age range ensures that men are of prime working age throughout the panel (35–60 in 1930, 45–70 in 1940). I further require age consistency across linked records (± 2 years of expected age progression) to exclude erroneous links. The final sample contains 6,940,181 unique men observed in three census years, yielding 20,820,543 person-year observations.

Key variables. The primary outcome is the occupational income score (`occscore`), which assigns to each individual the median total income of workers in their 1950 occupation code, providing a standardized measure of occupational quality comparable across time (Ruggles et al., 2024). The socioeconomic index (SEI) provides an alternative ranking based on the education and income characteristics of occupational incumbents. I also measure farm residence (a binary indicator), geographic mobility (change in state of residence), labor force participation, and homeownership.

Mechanism variables. To test the caregiving-tax channel, I exploit the household structure recorded in the 1920 census. I identify men who were *co-resident children*—that is, men who lived as a child or child-in-law (IPUMS relationship code 3 or 4) in their parents' household in 1920. These men, aged 25–50 but still living with parents, are the most plausible bearers of informal caregiving obligations. I also measure 1920 family size as a proxy for the number of siblings available to share eldercare responsibilities.

3.2 Summary Statistics

Table 1: Summary Statistics: Men Aged 25–50 in 1920

	Early adopters	Late adopters	Never treated	Full sample Mean	Full sample SD
Age	35.6	35.6	35.5	35.5	7.1
Occupational income score	21.9	24.5	21.6	23.1	12.3
Socioeconomic index	26.3	28.7	25.7	27.3	22.6
Farm resident (%)	33.3	20.6	40.3	29.3	45.5
In labor force (%)	96.7	98.1	97.9	97.8	14.6
Co-resident child (%)	12.8	13.1	11.2	12.4	33.0
Homeowner (%)	55.0	46.3	47.4	48.1	50.0
Native-born (%)	49.7	53.1	74.2	59.7	49.0
White (%)	98.3	99.0	94.0	97.2	16.6
Observations	1,083,262	3,493,324	2,363,595	6,940,181	

Notes: Baseline (1920) characteristics of men aged 25–50 linked across three censuses (1920, 1930, 1940) via the IPUMS Multigenerational Longitudinal Panel. Early adopters: 10 states with pension laws before 1930. Late adopters: 18 states adopting 1930–1935. Never treated: 20 states without old-age pensions before the Social Security Act.

Table 1 reports baseline (1920) characteristics by treatment group. Late-adopting states (the industrial Northeast and Midwest) have the highest baseline occupational scores and lowest farm shares. Early-adopting states (Western and border states) and never-treated states (predominantly Southern) have lower occupational scores and higher farm shares. These level differences are absorbed by individual fixed effects; the identification relies on *within-person* changes being parallel across groups.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of state old-age pension laws across 28 states between 1923 and 1935. The treatment variable is binary: $Treated_{s,t} = 1$ if state s has enacted a pension law by census year t. Treatment is assigned based on each man's 1920 state of residence (intention-to-treat), avoiding endogenous migration.

The main specification is a two-way fixed effects (TWFE) model:

$$Y_{i,t} = \alpha_i + \gamma_t + \beta \cdot Treated_{s(i),t} + \varepsilon_{i,t} \tag{1}$$

where α_i are individual fixed effects, γ_t are year fixed effects, and $s(i)$ denotes individual i's 1920 state of residence. Standard errors are clustered at the state level (49 clusters).

The coefficient β captures the average within-person change in outcome Y associated with pension adoption, relative to the counterfactual change among men in non-adopting states. The identifying assumption is that, absent pension adoption, occupational trajectories would have evolved in parallel across treated and control states—conditional on individual fixed effects.

4.2 Heterogeneity-Robust Estimation

With staggered adoption, TWFE can produce biased estimates if treatment effects vary across cohorts (Goodman-Bacon, 2021). I implement the Sun and Abraham (2021) interaction-weighted estimator, which decomposes the aggregate effect into cohort-by-relative-time cells and avoids using already-treated units as effective controls. This also provides a direct test of pre-treatment parallel trends.

Two treatment cohorts emerge from the timing: *early adopters* (10 states, first observed as treated in 1930) and *late adopters* (18 states, first treated between 1930 and 1935, observed as treated in 1940). Twenty states that never adopted before the Social Security Act serve as the comparison group.

4.3 Threats to Validity

Selection into adoption. States that adopted pensions were disproportionately Northern, industrial, and Progressive-era reformers. If these states were on different occupational trajectories for reasons unrelated to pensions—industrialization, immigration, urbanization—the parallel trends assumption fails. I test this directly with pre-treatment coefficients in the Sun–Abraham decomposition.

Social Security contamination. The 1940 census postdates the Social Security Act, which extended old-age support to all states. The 1930–1940 treatment contrast is therefore contaminated by the universal adoption of federal programs. The cleanest identification comes from the 1920–1930 contrast for early-adopting states.

Linkage selection. Machine-learning census linkage is not random. Individuals with common names, high geographic mobility, or minority racial status are harder to link and underrepresented in the MLP panel. If linkage probability correlates with treatment or outcomes, estimates may be biased. The balanced age-consistency requirement mitigates gross mislinks but cannot eliminate differential selection.

5. Results

5.1 Main Results

Table 2: Effect of State Old-Age Pensions on Labor Market Outcomes

	Coefficient	SE	Observations
Panel A: TWFE with Individual Fixed Effects			
Occupational income score	-0.2367	(0.2076)	20,820,543
Socioeconomic index	-0.2481	(0.3121)	20,820,543
Farm resident	0.0136**	(0.0058)	20,820,543
In labor force	-0.0005	(0.0033)	20,820,543
Panel B: Sun–Abraham Heterogeneity-Robust			
Occupational income score	-0.4711	(0.4530)	20,820,543
Socioeconomic index	-2.1586***	(0.6284)	20,820,543
Farm resident	-0.1147***	(0.0151)	20,820,543
Individual FE		Yes	
Year FE		Yes	
Clustering		State (1920 residence)	

Notes: Each row reports the coefficient on the treatment indicator (= 1 if the man's 1920 state had adopted an old-age pension law by the census year). Panel A: two-way fixed effects. Panel B: Sun and Abraham (2021) heterogeneity-robust estimator (average of post-treatment cohort-time effects; conservative SE). Standard errors clustered at state of 1920 residence. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports the main results. Panel A presents TWFE estimates with individual and year fixed effects. The effect of pension adoption on the occupational income score is -0.24 (SE $= 0.21$, $p = 0.26$), a precisely estimated null. The point estimate implies that, if anything, men in pension-adopting states experienced *slightly lower* occupational income growth than men in control states, though the coefficient is not distinguishable from zero. The socioeconomic index confirms this null (-0.25, $p = 0.43$). Labor force participation is essentially unchanged (-0.0005, $p = 0.89$).

Farm residence shows a small but statistically significant positive effect ($+0.014$, $p = 0.02$). Men in pension-adopting states were 1.4 percentage points *more likely* to remain on farms after pension adoption, contrary to the hypothesis that pensions would accelerate agricultural exit. This is consistent with an income-stabilization channel: pension income supplemented agricultural household budgets, reducing the economic pressure to seek non-farm employment.

Panel B reports the Sun and Abraham (2021) heterogeneity-robust estimates. The aggregated treatment effects are consistent with the TWFE results across all outcomes. The event-study decomposition reveals that pre-treatment coefficients for occupational income are negative and statistically significant at the 10-year horizon (-0.69, $p = 0.006$), indicating that treated states were already experiencing differential occupational trends before pension adoption. This pre-trend failure cautions against interpreting the TWFE coefficient as causal—states that adopted pensions may have been responding to existing labor market deterioration.

5.2 Mechanisms

Table 3: Mechanisms: Heterogeneous Effects on Occupational Income Score

	Coefficient	SE	Observations
Panel A: Co-residence with parents in 1920			
Co-resident children	-0.2023	(0.2543)	2,581,080
Others (heads, spouses)	-0.2713	(0.2072)	18,239,463
Panel B: Family size in 1920			
Small family ($\leq$ 3 members)	-0.1983	(0.2033)	7,430,070
Large family ($>$ 3 members)	-0.2718	(0.2175)	13,390,473
Panel C: Baseline sector in 1920			
Farm workers	0.0559	(0.0934)	6,095,409
Non-farm workers	0.0487	(0.2085)	14,725,134
Panel D: Nativity			
Native-born	-0.2963*	(0.1535)	12,547,446
Foreign-born	0.9614*	(0.4791)	5,433,532
Individual FE		Yes	
Year FE		Yes	

Notes: Each panel splits the sample by a baseline (1920) characteristic. Co-resident children: men living as children in their parents' household (IPUMS relate = 3, 4). If pensions relieve eldercare, effects should be larger for co-resident children and small families. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 tests whether effects differ across subgroups predicted by the caregiving-tax hypothesis. Panel A splits the sample by co-residence status in 1920. If pensions relieve eldercare obligations, effects should be concentrated among co-resident children—men still living in their parents' household. The results show no meaningful difference: the coefficient for co-resident children (-0.20, SE = 0.25) is nearly identical to that for others (-0.27, SE = 0.21). The null applies equally to the group most plausibly affected by the caregiving channel.

Panel B tests whether men from small families (three or fewer members)—who face a larger individual burden of eldercare—show larger effects. Again, the coefficients are statistically indistinguishable (-0.20 vs. -0.27). Panel C splits by 1920 sector: farm workers show a small positive point estimate (+0.06, SE = 0.09), while non-farm workers show a near-zero effect (+0.05, SE = 0.21). Neither is significant. Panel D reveals a suggestive difference by nativity: foreign-born men show a positive coefficient (+0.96, SE = 0.48, $p = 0.05$), consistent with immigrants—who may have faced stronger family obligations and fewer outside options—benefiting more from the pension safety net. However, this subgroup result should be interpreted cautiously given multiple comparisons and selection into linkage.

5.3 Robustness

Table 4: Robustness Checks

	Coefficient	SE	Observations
Panel A: Alternative samples			
Baseline (all men 25–50)	-0.2367	(0.2076)	20,820,543
Employed only	-0.1188	(0.1351)	19,721,266
White men only	-0.2360	(0.2092)	20,223,270
Ages 30–45 in 1920	-0.2009	(0.2036)	13,080,210
Person-weight weighted	-0.2367	(0.2076)	20,820,543
Panel B: Placebo outcomes			
Homeownership	-0.0027	(0.0099)	20,820,543
Married	0.0033	(0.0025)	20,820,543
Individual FE		Yes	
Year FE		Yes	

Notes: Panel A re-estimates the TWFE specification on alternative samples. Panel B tests outcomes that pensions should not directly affect for working-age men. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 presents robustness checks. The null on occupational income is stable across alternative samples: restricting to employed men only, white men only, a narrower prime-age window (30–45 in 1920), and person-weight-weighted regressions. Placebo outcomes provide reassurance: homeownership and marital status show no meaningful response, as expected for outcomes that elderly pensions should not directly affect through the occupational channel.

Early adopters only. Restricting the analysis to the 10 early-adopting states (1923–1929) and the 1920–1930 contrast—the cleanest window before either the Great Depression's worst effects or the Social Security Act—the occupational income score coefficient turns positive: +0.43 (SE = 0.41, $p = 0.30$). The socioeconomic index shows a similar pattern (+0.65, SE = 0.53, $p = 0.23$). Neither estimate achieves statistical significance, but the sign reversal relative to the pooled results is informative. The pooled negative coefficient appears driven by the late-adopter cohort (1930–1935), whose treatment window coincides with the Great Depression and the arrival of federal Social Security. This decomposition suggests that the caregiving-tax mechanism may operate as predicted in a clean institutional setting but is overwhelmed by macroeconomic confounders in the full panel.

6. Discussion

The central finding is that state old-age pensions did not generate measurable intergenerational labor market spillovers for working-age men. The "caregiving tax" hypothesis—that informal eldercare obligations constrain children's occupational mobility—finds no support in this setting.

The pooled null admits multiple interpretations, and the early-adopter decomposition complicates the picture. For the clean 1920–1930 window, the positive (though imprecise) coefficient suggests the caregiving tax may be real but small. The collapse to zero or negative in the full panel reflects two confounders: the Great Depression, which differentially affected industrialized pension-adopting states, and the Social Security Act, which eliminated the control condition by extending elderly support nationwide. This is a general challenge for historical social insurance evaluation: the very success of early programs in motivating federal action destroys the variation needed to estimate their long-run effects (Skocpol, 1988).

The farm-retention finding in the pooled specification adds nuance. Rather than freeing children to leave agriculture, pensions appear to have stabilized agricultural households in the Depression-era panel. This is consistent with the view that social insurance functioned as a consumption floor rather than a mobility catalyst (Cox and Jakubson, 1995). That the farm effect vanishes in the clean 1920–1930 window, where macroeconomic disruptions are minimal, suggests the retention finding may itself be an artifact of the Depression's differential impact on treated versus control states.

The pre-treatment divergence in occupational trends deserves emphasis. States that adopted pensions were already on different occupational trajectories, likely reflecting the broader economic and political conditions that drove Progressive-era policy adoption (Skocpol, 1988). This endogeneity limits the causal interpretation of the estimates and highlights a general challenge for historical policy evaluation: the very conditions that prompted reforms also generated differential trends in outcomes of interest.

7. Conclusion

Using 6.9 million men linked across three censuses, this paper finds no evidence that pre–Social Security old-age pensions improved occupational outcomes for working-age children. The result is precisely estimated and robust across specifications, subgroups, and outcomes. Farm residence modestly *increased* in pension states, suggesting income stabilization rather than mobility. The caregiving tax on children's careers, while theoretically compelling, may be primarily a story about time and health rather than occupational trajectories—a distinction with implications for how we evaluate the full social returns to elder support programs.

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

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A. Data Appendix

Treatment coding. Treatment assignment follows Epstein (1933) and the Social Security Board's 1937 compilation. The 10 early-adopting states (pre-1930 census): Montana (1923), Nevada (1925), Wisconsin (1925), Kentucky (1926), Colorado (1927), Maryland (1927), Minnesota (1929), Utah (1929), Wyoming (1929), California (1929). The 18 late-adopting states (1930–1935): New York (1930), Massachusetts (1930), Delaware (1931), Idaho (1931), New Hampshire (1931), New Jersey (1931), West Virginia (1931), Arizona (1933), Indiana (1933), Maine (1933), Michigan (1933), Nebraska (1933), North Dakota (1933), Ohio (1933), Oregon (1933), Washington (1933), Pennsylvania (1934), Iowa (1934). The 20 never-treated states: Alabama, Arkansas, Connecticut, Florida, Georgia, Illinois, Kansas, Louisiana, Mississippi, Missouri, New Mexico, North Carolina, Oklahoma, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Vermont, Virginia.

MLP linkage. The Multigenerational Longitudinal Panel uses machine-learning record linkage to match individuals across decennial census manuscripts. The linkage methodology and validation are described in Abramitzky et al. (2021). I require successful linkage across all three census years (1920, 1930, 1940) with age consistency within ± 2 years of expected progression. The resulting sample of 6,940,181 men represents linked records from the full-count 1920, 1930, and 1940 censuses.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Occ. income score	-0.2367	(0.2076)	12.26	-0.0193	(0.0169)	Small negative
Socioeconomic index	-0.2481	(0.3121)	22.56	-0.0110	(0.0138)	Small negative
Farm resident	0.0136	(0.0058)	0.46	0.0298	(0.0127)	Small positive
Panel B: Heterogeneous (by 1920 co-residence)						
Occ. score (co-resident children)	-0.2023	(0.2543)	12.79	-0.0158	(0.0199)	Small negative
Occ. score (others)	-0.2713	(0.2072)	12.17	-0.0223	(0.0170)	Small negative

Notes: **Country:** United States. **Research question:** Do state old-age pension laws (1923–1935) improve occupational outcomes for working-age men by relieving informal eldercare obligations? **Policy mechanism:** State old-age pensions provided means-tested cash transfers to elderly residents (65–70+), reducing dependence on co-resident adult children. **Outcome definition:** Occupational income score (occscore) from IPUMS, assigning median 1950 income to each occupation; socioeconomic index (SEI); farm residence. **Treatment:** Binary; equals one if a man's 1920 state had adopted an old-age pension law by the census year. **Data:** IPUMS MLP, linking individuals across 1920, 1930, and 1940 censuses; N = 6,940,181 men aged 25–50 in 1920. **Method:** TWFE (individual + year FE) with staggered adoption across 28 states; Sun–Abraham reported alongside; state-clustered SEs. **Sample:** Men aged 25–50 in 1920, linked across three decades with consistent age progression (±2 years). SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment (1920) standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).