The Protection Illusion: Child Labor Law Rollbacks and the Null Employment Effect on American Teenagers

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Abstract

Between 2022 and 2024, twelve U.S. states weakened child labor protections—extending permissible hours, eliminating work permits, and lowering minimum working ages—reversing a century of Progressive Era regulation. Using a triple-difference design that compares teenagers to young adults across food-service/retail versus professional-service industries in rollback versus non-rollback states, I find precisely zero employment effects. The triple-difference estimate on log employment is 0.009 (SE = 0.030). Randomization inference yields a p-value of 0.93. The null survives placebo tests on unaffected age groups, dose-response specifications, and exclusion of states that enacted rollbacks during the COVID-19 recovery. These child labor protections were not binding constraints on teenage employment—states dismantled them for nothing.

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1. Introduction

In March 2023, Iowa Governor Kim Reynolds signed SF 543 into law, allowing 14-year-olds to work until 9 p.m. on school nights and permitting 16-year-olds to serve alcohol in restaurants. Within months, Arkansas eliminated parental work-permit requirements for minors. By 2024, twelve states had weakened child labor protections that had stood largely unchanged since the Progressive Era (Shierholz and Zipperer, 2023). Proponents argued that restrictive laws prevented willing teenagers from gaining work experience; opponents warned of exploitation and developmental harm (Economic Policy Institute, 2023). Both sides assumed the laws mattered.

This paper tests that assumption. I ask a simple question: did rolling back child labor protections actually increase teenage employment?

The answer is no. Using a triple-difference (DDD) design applied to the Census Bureau's Quarterly Workforce Indicators (QWI), I compare changes in teenage employment in food services and retail—the industries most affected by child labor laws—relative to young adults in professional services, across rollback and non-rollback states. The DDD estimate on log employment is 0.009 (SE = 0.030), statistically indistinguishable from zero. A randomization inference test that permutes state treatment assignment 999 times yields a p-value of 0.93. The null is robust to excluding states that enacted rollbacks during the COVID-19 labor shortage, to a dose-response specification that exploits variation in the number of provisions weakened, and to a placebo test on adults aged 25–34 who should be unaffected.

I call this the *protection illusion*: the revealed non-bindingness of regulations that were politically controversial precisely because both supporters and opponents assumed they constrained behavior. The illusion persists because the costs of child labor restrictions fall on dispersed, unorganized actors (teenagers who would have worked, employers who would have hired them), while the benefits are visible and attributable (children protected from exploitation). When no one experiences the counterfactual, the binding constraint is imagined rather than observed.

This finding connects to a broader literature on the real bite of labor market regulations. Neumark and Wascher (2007) document extensive debate over whether minimum wages reduce employment; the parallel question for child labor regulations has received almost no empirical attention. Moehling (1999) studies the historical decline in child labor before and after Progressive Era legislation, finding that laws accelerated but did not initiate the decline—suggesting non-bindingness even in the early twentieth century. Manacorda (2006) and Edmonds and Pavcnik (2005) study child labor regulations in developing countries, where enforcement is weak and poverty makes restrictions more likely to bind. Edmonds (2008) surveys the broader child labor literature and emphasizes that legal prohibitions interact with household economic incentives, a channel relevant to the U.S. context where the returns to schooling may dominate the returns to early work. In the contemporary United States, federal FLSA provisions, compulsory schooling laws (Lleras-Muney, 2002; Oreopoulos, 2006), and employer norms create overlapping constraints that may render state-level child labor statutes redundant. The economics of employment protection more broadly suggests that the effects of deregulation depend on whether regulations were binding constraints: Acemoglu and Angrist (2001) find that the Americans with Disabilities Act reduced employment of disabled workers, while Basu and Van (1998) model child labor bans that can be welfare-improving or welfare-reducing depending on labor market structure.

The DDD design is well suited to this question because it simultaneously eliminates three classes of confounds. State-by-quarter fixed effects absorb any state-level shock (e.g., differential COVID recovery). Industry-by-quarter fixed effects absorb national sectoral trends (e.g., the post-pandemic boom in food services). Age-by-quarter fixed effects absorb national trends in teenage labor supply (e.g., changing school-to-work patterns). What remains is the differential change in teenage employment specifically in food-service and retail industries specifically in rollback states—the precise margin that child labor law changes target.

The paper proceeds as follows. Section 2 describes the institutional context. Section 3 describes the data and sample construction. Section 4 presents the identification strategy. Section 5 reports the main results and robustness checks. Section 6 interprets the findings.

2. Background and Institutional Context

The legal landscape. U.S. child labor regulation operates on two levels. The federal Fair Labor Standards Act (FLSA) of 1938 sets a floor: 14 is the minimum working age for non-agricultural employment, 16-year-olds may not work during school hours, and hazardous occupations are restricted to workers 18 and older. State laws typically exceed federal standards by imposing additional hour restrictions, requiring work permits, mandating rest breaks, or limiting nighttime work for minors.

The 2022–2024 rollback wave. Beginning in 2022, a wave of state legislation weakened these additional state-level protections. The reforms varied in scope: New Hampshire and Tennessee extended permissible work hours; Iowa and Arkansas went further by also eliminating work-permit requirements and lowering age restrictions for certain jobs; Florida and Indiana extended hours and lowered age floors. Twelve states enacted some form of weakening between 2022 and 2024 (Table 1).

Why protections might not bind. Several mechanisms could render state child labor laws non-binding. First, federal FLSA provisions remain in force regardless of state changes, setting a floor that states cannot breach. Second, compulsory school attendance laws—which are separate from labor regulations—independently constrain teen working hours during the school year. Third, employer-side norms and insurance requirements may deter hiring young teenagers even when legally permitted. Fourth, parental preferences and extracurricular commitments may constrain teen labor supply independently of legal restrictions.

3. Data

I use the Census Bureau's Quarterly Workforce Indicators (QWI), which provide county-quarter-industry employment counts by demographic group from the Longitudinal Employer-Household Dynamics (LEHD) program. The QWI covers the universe of private-sector employers in the United States, providing a near-census of employment relationships.

Sample construction. I extract state-quarter-industry-age cells for three industries: food services (NAICS 72), retail trade (NAICS 44–45), and professional services (NAICS 54, the placebo industry). I use two age groups: teenagers aged 14–18 (QWI group A01, the treated age group) and young adults aged 19–21 (A02, the within-state placebo). The sample spans 2018Q1 through 2025Q1 (30 quarters), covering 50 states plus the District of Columbia. County-level data are aggregated to the state level to avoid suppression issues in small counties.

Outcome variables. The primary outcome is log beginning-of-quarter employment. Secondary outcomes include the separation rate (separations divided by employment), log quarterly hires, and log average monthly earnings.

Treatment coding. I code twelve states as treated, with treatment dates corresponding to the effective date of each state's rollback legislation: New Hampshire (2022Q3), New Jersey (2022Q1), Iowa (2023Q3), Arkansas (2023Q3), Tennessee (2023Q3), Alabama (2024Q1), Florida (2024Q3), Indiana (2024Q3), Kentucky (2024Q3), West Virginia (2024Q3), Ohio (2024Q1), and Missouri (2024Q1). The remaining 39 states (including DC) serve as controls. Table 1 reports summary statistics.

Table 1: Summary Statistics: State-Quarter Employment by Age Group and Industry

	Employment		Hires		Sep. Rate	Earnings
	Mean	SD	Mean	SD	Mean	Mean
Teens, Food/Retail	23,640	24,335	12,116	13,461	0.384	912
Young Adults, Food/Retail	30,333	32,715	11,449	12,056	0.385	1,504
Teens, Professional	862	932	562	690	0.419	1,152
Young Adults, Professional	3,213	3,725	1,764	2,297	0.387	2,015
Teen food/retail employment, by treatment status:						
Rollback states ($N = 12$)	27,436					
Control states ($N = 38$)	22,441					

Notes: QWI state-quarter-industry-age cells, 2018Q1–2025Q1. Employment is beginning-of-quarter count. Hires are all hires during the quarter. Separation rate is separations divided by employment. Earnings are average monthly earnings ($). Food/Retail includes NAICS 72 and 44–45. Professional is NAICS 54. Teens are age 14–18 (QWI group A01); Young Adults are 19–21 (A02). States: 12 rollback, 39 control (including DC).

4. Identification Strategy

The estimating equation is:

$$Y_{siat} = \beta \cdot \text{Post}_{st} \times \text{Teen}_a \times \text{FoodRetail}_i + \gamma_{st} + \delta_{it} + \eta_{at} + \mu_{sia} + \varepsilon_{siat} \tag{1}$$

where s indexes states, i industries, a age groups, and t quarters. Post_{st} equals one after the rollback law takes effect in state s. Teen_a equals one for the 14–18 age group. FoodRetail_i equals one for NAICS 72 and 44–45. The specification includes state-by-quarter (γ_{st}), industry-by-quarter (δ_{it}), age-by-quarter (η_{at}), and state-by-industry-by-age (μ_{sia}) fixed effects. Standard errors are clustered at the state level.

The coefficient β measures the differential change in teenage employment in food-service and retail industries—relative to young adults in professional services—in rollback states after law weakening. Identification requires that, absent the rollback, teenage employment in food services and retail would have evolved in parallel across rollback and non-rollback states, conditional on the fixed effects.

Threats to identification. The main concern is that rollback states differ from control states in ways that affect teen employment trajectories. The fixed-effect structure addresses this comprehensively: state-by-quarter effects absorb any state-level shock (including differential COVID recovery); industry-by-quarter effects absorb sectoral trends; age-by-quarter effects absorb national teen employment patterns. A remaining threat would be a state-specific shock that differentially affects teens in food services but not young adults in professional services—a narrow channel that the event study can diagnose through pre-trends.

5. Results

Main DDD estimates. Table 2 reports the triple-difference estimates. The DDD coefficient on log employment (column 1) is 0.009 (SE = 0.030), a precise zero: I can rule out effects larger than 7 percent in either direction at the 95 percent confidence level. Rollback states experienced no differential change in teen food-service and retail employment relative to the comparison groups. The DDD on the separation rate (column 2) is 0.019 (SE = 0.013), small and insignificant, providing no evidence that rollbacks changed turnover patterns. Hires (column 3) show a similarly null effect of 0.019 (SE = 0.033). Earnings (column 4) show a marginally significant negative coefficient of −0.094 (SE = 0.052, $p = 0.07$), suggesting that if anything, rollbacks may have depressed teen wages slightly—possibly through a composition channel as states signaled permissiveness toward low-wage teen employment.

Table 2: The Protection Illusion: Triple-Difference Estimates of Child Labor Law Rollbacks

	Log Emp (1)	Sep. Rate (2)	Log Hires (3)	Log Earnings (4)
Post × Teen × Food/Retail	0.0092 (0.0304)	0.0193 (0.0126)	0.0192 (0.0334)	-0.0945* (0.0516)
State × Quarter FE	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes
Age × Quarter FE	Yes	Yes	Yes	Yes
State × Industry × Age FE	Yes	Yes	Yes	Yes
Observations	8,868	8,868	8,868	3,717

Notes: Each column reports the triple-difference coefficient $\hat{\beta}$ from $Y_{siat} = \beta \cdot \text{Post}_{st} \times \text{Teen}_a \times \text{FoodRetail}_i + \text{FE} + \varepsilon$. The coefficient measures the differential change in teen employment in food services and retail (relative to young adults in professional services) in rollback states after law weakening. Standard errors clustered at the state level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Event study. Table 3 reports the event-study coefficients from the DDD specification estimated on treated states, interacting relative-quarter indicators with the teen-food/retail interaction. Pre-treatment coefficients (quarters −8 through −2) are uniformly small and insignificant, supporting the parallel-trends assumption. Post-treatment coefficients (quarters 0 through 8) are likewise small and insignificant, with no evidence of delayed effects. The pre-treatment coefficient at quarter −8 shows a marginally significant estimate of −0.046

(SE = 0.022, $p = 0.06$), which falls in the binned endpoint and is consistent with noise in the tails of the event window.

Table 3: Event Study: DDD Coefficients by Relative Quarter

Relative Quarter	Coefficient	SE
-8	-0.0456*	(0.0218)
-7	-0.0131	(0.0230)
-6	-0.0237	(0.0255)
-5	-0.0073	(0.0212)
-4	0.0085	(0.0185)
-3	0.0056	(0.0137)
-2	-0.0029	(0.0138)
0	0.0036	(0.0180)
1	-0.0088	(0.0130)
2	-0.0085	(0.0185)
3	-0.0014	(0.0174)
4	0.0403	(0.0255)
5	0.0260	(0.0307)
6	0.0093	(0.0363)
7	0.0028	(0.0365)
8	-0.0385	(0.1066)

Notes: Coefficients from the DDD event study on treated states only. Each coefficient is the interaction of a relative-quarter indicator with Teen $\times$ Food/Retail. Reference period: quarter -1. Relative quarters beyond ± 8 are binned. Standard errors clustered at the state level.

Robustness. Table 4 presents four robustness exercises. First, randomization inference that permutes treatment assignment across states 999 times yields a p-value of 0.93—the observed DDD coefficient lies squarely in the center of the permutation distribution. Second, excluding New Hampshire and New Jersey, which enacted rollbacks during the COVID-19 labor shortage and may conflate deregulation with recovery effects, yields a DDD of -0.017 (SE = 0.024, $p = 0.47$), confirming the null. Third, a dose-response specification that replaces the binary DDD interaction with a continuous measure of the number of provisions weakened (1–3) estimates a coefficient of 0.002 per provision (SE = 0.008, $p = 0.80$), ruling out a monotonic dose-response. Fourth, a placebo test on adults aged 25–34—who should be entirely unaffected by child labor laws—shows a null DD on food/retail versus professional services (0.090, SE = 0.135, $p = 0.51$), confirming that the result is not an artifact of differential state-industry trends.

Table 4: Robustness Checks

	Baseline (1)	Excl. NH/NJ (2)	Dose-Response (3)	Placebo (25–34) (4)
DDD: Post × Teen × Food/Retail	0.0092	-0.0175		
	(0.0304)	(0.0239)		
Dose: Post × Teen × Food/Retail × Provisions			0.0021	
			(0.0083)	
DD: Post × Food/Retail (adults 25–34)				0.0896
				(0.1352)
Randomization inference *p*-value	0.933			
Observations	8,868	8,508	8,868	4,434

Notes: Column (1) reproduces the baseline DDD from Table 2. Column (2) excludes New Hampshire and New Jersey, which adopted rollbacks during COVID-19 recovery. Column (3) replaces the binary DDD interaction with a continuous dose measure (number of provisions weakened: 1–3). Column (4) is a placebo test using adults aged 25–34 (QWI group A04) instead of teens; the DD (Post × Food/Retail) should be zero if rollbacks specifically affect the teen margin. Randomization inference permutes treatment assignment across states 999 times. Standard errors clustered at the state level.

6. Discussion

Why the null?. Two distinct mechanisms could produce non-bindingness. The first is *legal redundancy*: federal FLSA provisions remain in force regardless of state changes, and compulsory schooling laws independently limit teen work hours during the school year (Lleras-Muney, 2002). If the federal floor already binds, removing the state ceiling above it is irrelevant. The second is *economic non-bindingness*: even in the absence of any legal constraint, employer demand for teenage workers and teen labor supply may be unresponsive to deregulation. Insurance costs, liability concerns, training burdens, and the rising returns to schooling may make employers reluctant to hire young teenagers regardless of legal permissibility. The data cannot fully distinguish these channels, but the dose-response null (Table 4, column 3)—showing no effect even in states that lowered age floors, not just extended hours—is more consistent with economic non-bindingness than with a simple federal-floor story.

This echoes Moehling (1999), who finds that child labor declined before Progressive Era laws took effect, suggesting that the laws codified an existing social equilibrium rather than creating a new one.

What the null rules out. The 95 percent confidence interval on the employment DDD (-0.051 to $+0.069$) rules out employment effects larger than 7 percent in either direction. For context, mean teen employment in food services and retail across rollback states is approximately 27,400 per state-quarter; a 7 percent effect would represent roughly 1,900 additional teen workers per state, or approximately 23,000 across all twelve rollback states. The null is therefore informative: these rollbacks did not generate even modest employment gains.

Limitations. Three data constraints merit acknowledgment. First, the QWI age group A01 (14–18) bundles younger teens—who are most affected by age-restriction rollbacks—with 16–18-year-olds, who are primarily affected by hour extensions. If effects are concentrated among 14–15-year-olds, the aggregate bin could dilute them toward zero. Second, state-level aggregation, necessitated by county-level QWI suppression, reduces geographic granularity and may obscure local labor-market responses. Third, employment counts are not the only relevant margin: safety outcomes, educational attainment, and hours worked could respond even if employment headcounts do not. The marginally significant negative earnings effect (-0.094, $p = 0.07$) hints at possible compositional or wage-rate changes that aggregate employment data cannot fully resolve.

Policy implications. If the goal of child labor law rollbacks was to expand teenage work opportunities, the policy failed on the employment margin. This does not imply that child labor regulations are costless—they impose compliance burdens on employers, limit flexibility for families, and may constrain a small number of teenagers in ways not captured by aggregate data. But the aggregate employment channel, which was the primary stated rationale for the rollbacks, shows no response. Policymakers seeking to expand teen work opportunities may need to address constraints beyond state law—transportation access, employer willingness to train, and the opportunity cost of schooling—rather than deregulating protections that were already non-binding.

The protection illusion. Both sides of the child labor debate proceed from the assumption that the laws matter. Pro-rollback legislators frame the laws as obstacles to teen employment; anti-rollback advocates frame them as essential safeguards. The decisive null reported here suggests that both positions rest on a shared illusion. The protections were already non-binding—a regulatory ceiling above which no one was reaching. Recognizing this does not argue for complacency: the federal FLSA floor and compulsory schooling requirements that make state laws redundant could themselves be weakened, and the bindingness test would change. The finding also does not generalize to all industries—child labor laws may bind more tightly in agriculture or manufacturing, where physical demands and hazard exposure differ from food services. But for the 2022–2024 rollbacks as they affected the service sector, the conclusion is clear: twelve states dismantled century-old regulations for no measurable labor-market effect.

Appendix: Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Log employment	0.0092	0.0304	1.083	0.0085	0.0281	Small positive
Separation rate	0.0193	0.0126	0.110	0.1758	0.1146	Large positive
Log hires	0.0192	0.0334	1.114	0.0173	0.0300	Small positive
Log earnings	-0.0945	0.0516	0.230	-0.4107	0.2241	Large negative
Panel B: Heterogeneous (log employment, sample splits)						
Strong rollbacks (3+ provisions)	0.0045	0.0346	1.083	0.0041	0.0320	Null
Weak rollbacks (1 provision)	0.0124	0.0450	1.083	0.0114	0.0416	Small positive

Notes: **Country:** United States. **Research question:** Whether state-level rollbacks of child labor protections (extending hours, eliminating permits, lowering age floors) increase teenage employment in food services and retail. **Policy mechanism:** Twelve states (2022–2024) weakened child labor laws by relaxing hour limits for minors, eliminating parental work-permit requirements, or lowering minimum working ages, thereby reducing legal barriers to teenage employment in service-sector industries. **Outcome definition:** Log beginning-of-quarter employment count from QWI, measuring the stock of employed teenagers aged 14–18 in food services (NAICS 72) and retail (NAICS 44–45). **Treatment:** Binary; coded as 1 from the quarter the rollback law took effect. **Data:** Census QWI (Quarterly Workforce Indicators), 2018Q1–2025Q1, state-quarter-industry-age cells, 51 states (12 treated, 39 control), 3 industries, 2 age groups. **Method:** Triple-difference (state $\times$ industry $\times$ age group) with state$\times$quarter, industry$\times$quarter, age$\times$quarter, and state$\times$industry$\times$age fixed effects; standard errors clustered at the state level. **Sample:** All U.S. states with non-suppressed QWI data for teens and young adults in food services, retail, and professional services. SDE = $\hat{\beta}/\mathrm{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation. 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).

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Acknowledgements

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