When Quality Counts: Early Childhood Regulation and the Long-Run Development of Human Capital

Shreya Bhardwaj*†

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Abstract

This paper provides the first evidence on the long-run effects of stricter childcare regulations, exploiting the staggered adoption of child-to-staff ratio and director education requirements across 31 U.S. states from 1984-1994. Using a difference-in-differences design, I find that exposure to these regulations in early childhood raises annual adult income by approximately $1,400 (2.5%). The gains operate through intensive human capital accumulation—raising graduate and professional degree attainment and high-paying employment—without increasing the likelihood of college attendance or labor force participation. However, these benefits are not universal. The effects are concentrated among the middle-class earners and are significantly smaller for individuals from economically disadvantaged states, underscoring that quality regulations can yield lasting returns but risk leaving behind those least able to access improved care.

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*University of Southern California

†Contact e-mail: shreyab@usc.edu

I Introduction

Early childhood is a pivotal period for human development, where interventions generate substantial and persistent returns for both individuals and society. Seminal research demonstrates that high-quality early childhood experiences significantly improve adult outcomes, from earnings and educational attainment to health and reduced criminal behavior (Heckman et al., 2010; Duncan and Magnuson, 2013). These effects are so profound they can transcend generations, breaking cycles of disadvantage by improving outcomes for the children of those who received early interventions (Barr and Gibbs, 2022; García et al., 2023).

Nearly 40% of children under five spend time in formal non-parental care, making the quality of these environments a central policy concern(U.S. Department of Education, National Center for Education Statistics, 2021). Because parents cannot easily observe quality—since it is weakly signaled by price or convenience (Mocan, 2007)—governments regulate observable inputs such as staff–child ratios, teacher qualifications, and space requirements to ensure minimum safety and developmental standards. Policymakers impose these regulations to ensure that all children—regardless of family resources—have access to safe, developmentally supportive environments that promote school readiness and long-run success. Yet, compliance costs can force providers—especially those in low-income communities—substitute for cheaper inputs or exit the market altogether, potentially reducing access and affordability (Blau, 2003; Hotz and Xiao, 2011). This creates a key policy dilemma: do quality-improving regulations generate long-run benefits that outweigh potential losses in access?

This paper addresses that question by examining the *long-run* effects of stricter childcare regulations on children's adult outcomes. I link variation in state-level regulation changes between 1983 and 2000 to individual outcomes in the 2019–2023 American Community Survey. Using a stacked difference-in-differences design, I exploit differences in the timing of two key regulations—child-to-staff ratios and director education requirements—across 31 states. I compare cohorts exposed to these changes (ages under five) to slightly older cohorts (ages five to ten) within the same state and to young cohorts in states without reforms.

Because these children are now adults aged 25–40, I am able to observe their present-day earnings, employment, educational attainment, and social outcomes. I find that stricter early childhood regulations led to higher adult income and educational attainment, with gains concentrated among middle-income children who retained access to care. These long-run benefits appear to operate through improvements in the quality of early care environments rather than expanded access or parental labor supply.

Specifically, I find that exposure to stricter regulations in early childhood increased annual income by approximately $1,400. Moreover, this cohort had a 1.3 percentage point higher likelihood of being married and a 2.2 percentage point lower likelihood of having had child as a teenager. Treatment also increased the rate of obtaining a graduate or professional degree by 0.5 percentage points and the likelihood of being employed in higher-paying jobs by 0.6 percentage points, both of which are likely drivers of the strong income results.

The effects were not uniform across the income distribution. The largest proportional income gains accrued to those in the middle of the adult earnings distribution. Individuals in the lowest and highest income quintiles showed proportionally smaller and less precise gains. This finding indicates that the benefits of regulation were most pronounced for those who both retained access to formal childcare.

The primary identifying assumption is that, absent of the implementation of stricter regulations, differences in adult outcomes between younger (under five) and older (five to ten) cohorts would have evolved similarly across treatment and control states. Event-study analyses support this assumption, showing parallel pre-trends for older cohorts.

One likely channel for these long-term gains is the enhancement of non-cognitive skills resulting from improved early care environments. The observed improvements in social outcomes and concentration of gains in advanced degrees and managerial roles—fields that reward socio-emotional skills—suggest that regulations may have fostered precisely such development. This explanation is distinct from a purely cognitive story, where test score gains often fade, and aligns with experimental evidence on the persistence of non-cognitive impacts

(Heckman et al., 2013; García et al., 2023).

This paper contributes to three strands of literature. The first builds on research documenting the long-run returns to early childhood interventions. Seminal high-intensity programs, such as the Perry Preschool Project and the Chicago Child-Parent Centers (CPC), have been shown to increase midlife earnings by 7–25% and to produce substantial gains in adult educational attainment and employment (Heckman et al., 2010; Reynolds et al., 2019). Evidence from broader targeted programs like Head Start also shows positive, though more modest, adult impacts (Deming, 2009; Bailey et al., 2021). A smaller volume of research on universal policies, such as K-12 school spending, shows that a 10% increase in education expenditure can increase adult wages of about 7% (Jackson et al., 2016) [1]. My study adds to this literature by examining a different type of large-scale, universal policy: childcare input regulations that apply across states and affect millions of children nationwide each year. By showing that these widely enacted, routine regulations produce lasting improvements in adult income, educational attainment, and social outcomes, I demonstrate that even broad, system-wide quality enhancements can yield meaningful long-run benefits. However, these gains are conditional on access, as evidenced by their concentration among middle-income children.

Second, this paper speaks directly to the mixed evidence on the effects of regulations. By imposing costly compliance burdens, regulations often hinder firm entry, entrepreneurial activity, and employment growth (Bertrand and Kramarz, 2002; Aghion et al., 2008; Garicano et al., 2016). In contrast, health and safety regulations tend to reduce injury rates despite some unintended consequences (Carpenter and Stehr, 2011; Gray and Shimshack, 2011; Levine et al., 2012). In the childcare sector, nearly all papers focus on the market structure and tend to find that stricter regulations reduce the supply of providers while improving the quality of those that remain (Blau, 2003; Currie and Hotz, 2004; Blau and

[1] Literature on long-run outcomes of early childhood interventions also includes influential studies on the Abecedarian Project (ABC) (Campbell et al., 2012, 2014), other analyses of Head Start (Garces et al., 2002), and the effects of teacher quality (Chetty et al., 2011, 2014).

Tekin, 2007; Hotz and Xiao, 2011). Only a handful consider child outcomes, and these focus on short-term measures of safety or early cognitive skills (Blau, 1999; Peisner-Feinberg and Burchinal, 1997; Currie and Hotz, 2004; Garcia-Vazquez, 2023). My paper extends this line of work by shifting the focus from markets to children, providing the first evidence on the long-run effects of stricter childcare regulations and clarifying how ambiguous regulatory trade-offs evolve over time.

Finally, this paper connects to research on the mechanisms driving long-run success, emphasizing the potential role of non-cognitive skills. The lasting improvements in income, education, and social outcomes are more consistent with gains in socio-emotional development than with cognitive effects, which tend to fade-out (Nores et al., 2005; Cunha et al., 2010; Fletcher and Padrón, 2016; Deming, 2017). These benefits appear concentrated among children who retained stable access to regulated care, suggesting that the long-run impact of quality regulation depends on the socioeconomic environments in which it operates (Blau, 1999; Agostinelli and Wiswall, 2025).

The remainder of the paper proceeds as follows. The next section provides additional context on the childcare market and types of regulations common to this context. Section III describes the data and Section IV presents the empirical model. Section V shows the results and Section VI discusses the mechanism. Finally, Section VII concludes.

II Background

The childcare market involves interdependent decisions by parents, providers, policymakers, and children. Parents rely on childcare both to participate in the labor market and to foster their children's development, but they cannot accurately assess quality and often base choices on cost and convenience instead (Mocan, 2007; Bassok et al., 2018a). Low-income parents are particularly price-sensitive, and higher childcare costs can induce them to substitute away from licensed care toward informal arrangements. On the supply side, the childcare sector is highly fragmented, dominated by small providers and characterized by labor-intensive production and narrow profit margins (Helburn and Howes, 1996). Rising operating costs lead providers to adjust prices, modify service quality, or exit the market altogether. Ultimately, children internalize the quality of their early care environments, with potential consequences that unfold throughout their lives.

This combination of information asymmetries on the demand side and positive externalities associated with high-quality early care provides the central economic rationale for government intervention. Because parents cannot fully observe quality or its long-term payoffs, private choices often fall short of the social optimum (Bassok et al., 2018b). Policymakers must therefore set minimum standards for safety and developmental quality while balancing these goals against affordability and access, especially for lower-income households. In the US, state governments administer licensing systems that regulate more than a hundred dimensions of care—including staff qualifications, child–staff ratios, and physical environments. However, both the type of regulations and their stringency differ substantially across states, reflecting variation in policy priorities.

The theoretical effects of stricter childcare regulations on child outcomes are ambiguous. Whether such regulations actually benefit children depends on several critical factors. First, regulations operate indirectly by mandating specific inputs rather than outcomes, and therefore may not improve care quality even when providers comply. This occurs when policies target inputs with weak causal links to quality improvements. Second, weak enforcement or non-binding standards—where most providers already exceed requirements—will produce minimal effects on either quality or access. Third, even when regulations are binding and well-enforced, quality gains come with trade-offs: higher costs reduce affordability and availability, potentially excluding lower-income families from regulated care. The net impact on child outcomes therefore depends on whether quality improvements for children who remain in regulated care outweigh the losses experienced by children whose families can no longer afford it.

These theoretical considerations underscore the importance of understanding whether regulations actually bind in practice. Empirical evidence suggests that the "bite" of regulation is partial and heterogeneous. Blau (2001) studied staff-to-child ratio requirements for a sample of childcare centers across four states in 1993, finding the requirements were binding for only 20-30% of classrooms, with non-compliance remaining a significant issue. More recent papers focusing on the demand for childcare workers report that staff qualification regulations are often binding but with varying levels of enforcement across states (Boyd-Swan and Herbst, 2017; Ali et al., 2024). This partial bindingness implies that the incidence of regulation is uneven, often having a stronger impact in lower-quality or lower-income markets, which can exacerbate inequalities in access and outcomes.

When regulations do bind and raise quality, the effects on children can be substantial. Research examining the relationship between regulated inputs and child outcomes suggests that smaller class sizes and more educated or experienced teachers are associated with gains in children's cognitive, linguistic, and social skills (NICHD Early Child Care Research Network, 1997). While test score gains may fade in the short run, studies show that benefits to non-cognitive skills can persist, re-emerging in adulthood as higher earnings and better social outcomes (Chetty et al., 2011). This evidence suggests that staff ratios and director education requirements serve as meaningful, policy-relevant proxies for the structural quality of the early childhood environment.

Given this background, I focus on staff-to-child ratios and director education requirements for several reasons. First, they represent fundamental, labor-related inputs directly tied to the production of childcare quality (Hotz and Xiao, 2011). Second, they are among the most consistently measured and comparable regulations across states and over time. Third, focusing on these two regulations addresses a practical empirical challenge: childcare policies tend to cluster together, with states that tighten one regulation typically adopting stricter standards across multiple dimensions simultaneously (Blau, 2003; Currie and Hotz, 2004; Hotz and Xiao, 2011). Staff-to-child ratios and director education requirements, however, are nearly universally regulated while exhibiting substantial variation in stringency across states and over time, facilitating identification. Finally, existing research identifies them as particularly consequential for both provider costs and child developmental outcomes (Ali et al., 2024), making them central to the policy debate. The substantial variation in these regulations across states and their staggered adoption over the 1980s and 1990s provides a valuable quasi-experimental setting for identifying their long-run effects.

III Data

III.1 Individual-Level Outcomes Data

My primary source for adult outcomes is the American Community Survey (ACS) 5-year sample covering 2019-2023 (Ruggles et al., 2025). I restrict my sample to individuals aged 25 and older to allow for completion of higher education. The ACS provides detailed information on respondents' state of birth, year of birth, and current socioeconomic characteristics including annual income, employment status, educational attainment, occupation, marital status, and fertility history. This large-scale dataset enables me to link individuals' childhood exposure to daycare regulations (based on state and year of birth) to their adult outcomes approximately 25-40 years later.

I identify treatment based on individuals' age when their birth state implemented stricter daycare regulations. My treated group consists of children aged four years and under at the time of regulatory changes, while my primary control group includes children aged five to ten in the same states who were too old to benefit from improved daycare quality. I additionally use children aged four years and under in states that never changed regulations during my study period as a secondary control group in my stacked difference-in-differences design. For robustness checks, I supplement my analysis with data from the Panel Study of Income Dynamics, public use dataset (2025) (PSID) and a user generated harmonized longitudinal file by Pfeffer et al. (2021) called PSID Stability, History, Early Life, and Family (SHELF), covering 1968-2021 [2]. While the PSID sample of treated individuals is substantially smaller

[2] User guide for the PSID-SHELF data Daumler et al. (2025)

than the ACS, it provides longitudinal data that allows me to track the same individuals over time and observe additional outcomes not available in the ACS.

III.2 Childcare Regulation Data

My treatment variable comes from Hotz and Xiao (2011) paper who use the data originally compiled by Hotz and Kilburn (2010). These data include comprehensive state-year childcare center regulations from 1983-2000. These data identify when states changed their child-to-staff ratio requirements and director education requirements for licensed childcare centers. During my study period, 31 states strengthened at least one of these regulatory dimensions while 10 states maintained unchanged regulations, providing variation in both the location and timing of regulatory changes necessary for identification.

Additionally, to control for contemporaneous changes in all other child care regulations, I use summary indices of the remaining regulations. Including these indices shows that my results are robust to controlling for other childcare regulations.

III.3 State-Level Policy and Contextual Data

To analyze the mechanisms, market responses, and robustness of the main results, I supplement the individual-level data with several state-level datasets. These data are grouped by their analytical purpose below.

Mechanisms and Market Responses: I examine the channels through which regulations operate using four primary sources. First, data from the Quarterly Census of Employment and Wages (QCEW) from 1980-2000 track the number of formal childcare providers to assess the policy's impact on market supply (U.S. Bureau of Labor Statistics, 2025). Second, I use the Current Population Survey (CPS) from the same period to test for changes in maternal labor force participation, which may be affected by shifts in childcare availability (Flood et al., 2025) . Third, to evaluate a direct measure of child safety, I employ Vital Statistics mortality files (1980-2000) to analyze rates of accidental deaths among children. Finally, I incorporate 8th-grade math and reading test scores from the National Assessment of Educational Progress (NAEP) from 1990-2013 to investigate medium-term educational outcomes (U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, 2025).

Robustness Checks: To ensure the robustness of the estimated income effects, I control for key state-level confounders measured in 1980, prior to the regulatory changes. These baseline controls include an indicator for the existence of a state pre-K program (National Institute for Early Education Research, 2019; Cohen-Vogel et al. (2022)), the employment rate of mothers with children under five (USA IPUMS 5% sample), and population density (U.S. Census Bureau). I further test the sensitivity of the results to the inclusion of time-varying policies, namely expenditure per pupil in K-12 education (National Center for Education Statistics) and maximum AFDC benefit levels (Social Security Bulletin; Wexler and Engel (1999)).

Heterogeneity Analysis: To explore differential treatment effects, I construct a state economic disadvantage index using 1980 characteristics. This index combines per capita income (Censuses of Population), the percentage of college graduates (Decennial Census), and state government revenue and expenditure per capita (State and Local Government Finances and Employment).

IV Empirical Strategy

IV.1 Sample Construction

This study estimates the long-run effects of early childhood exposure to stricter childcare regulations by linking adult outcomes from the pooled 2019-2023 American Community Survey (ACS) to historical state-level regulation data from 1983-2000.

I define a state as implementing stricter regulations if it introduces staff-to-child ratio requirements or director education requirements for the first time, or makes either regulation more stringent—for example, by mandating fewer children per caregiver or raising minimum education credentials for directors. The year of first adoption or tightening constitutes the policy year [3]. A total of 31 states enacted such changes between 1984 and 1994, providing the policy variation exploited in my identification strategy.

The analysis sample consists of individuals born in the United States between 1974 and 1994, whom I observe between the ages of 25 and 49. To ensure a consistent comparison, the sample is restricted to individuals who were between less than one and ten years old *at the time of their state's policy change*. This creates a balanced age window around the policy implementation for every individual. For instance, a person born in 1974 was 10 years old at the time of the first policy change in 1984, while a person born in 1994 was an infant (zero years old) during the final policy change in 1994. No individual born in years after the policy was implemented is included to mitigate possible selective migration concerns.

I estimate the intent-to-treat (ITT) effect of these regulatory changes, comparing individuals exposed to stricter standards during early childhood to those who were not, irrespective of their actual childcare enrollment. These estimates therefore capture the policy's population-level impact, encompassing both the direct effects on children in regulated care and any broader equilibrium effects on non-participants.

IV.2 Stacked Difference-in-Differences Model

To estimate the causal effects of stricter regulations, I use a stacked difference-in-differences design (Cengiz et al., 2019) that exploits variation in the timing of regulation adoption across states, with the ten states that made no changes during this period serving as never-treated controls.

In a setting with staggered treatment adoption, a simple two-way fixed effects (TWFE) model can produce biased estimates in the presence of heterogeneous treatment effects (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2023). The problem arises because states that were treated in earlier periods are used as controls for states treated in later periods. If the impact of stricter regulations is different for

[3] To isolate the effects of these specific regulations and control for potential confounding from other policy dimensions, I construct summary indices of all other childcare regulations similar to Blau (2003); Hotz and Xiao (2011). I include these as controls in one of my specifications.

early- and late-adopting states, the TWFE estimate becomes a weighted average of all possible two-group/two-period comparisons, including these invalid ones.

A stacked difference-in-differences design is well-suited for individual-level data with aggregate-level treatment. I define a sub-experiment for each distinct year in which one or more states first implemented a stricter regulation. Let c index these policy adoption years (e.g., $c = 1985$). This method creates sub-experiments, each corresponding to a distinct policy adoption year c between 1984 and 1994. For each of these 9 policy cohorts, I create a sub-dataset that includes:

- Treated Group: Individuals from states that implemented the policy in year c, who were aged four years and under at that time.

- Control Groups:

  1. *Within-state controls:* Individuals from the same treated states who were aged five to ten in year c.

  2. *Geographic controls:* Individuals from states that had not yet implemented the policy by year c, or never did, who were aged four years and under in year c.

These sub-datasets are then stacked for the final analysis. In the final dataset, each individual appears only in sub-experiments where their state qualifies as treated, not-yet-treated, or never-treated. I employ two complementary specifications on the stacked dataset. The first is a pooled specification designed to estimate the treatment effect averaged across all treated early childhood years with maximum statistical power. The main specification for individual i, born in state s, event cohort c

$$Y_{isc} = \beta_{\text{pooled}} \cdot \big(\text{Treated\_State}_s \times \text{Young\_Cohort}_{ic}\big) + \mu_c + \eta_s + \lambda_s \times c + \epsilon_{isc} \qquad (1)$$

where, Treated_State_s is an indicator for being born in an ever-treated state, and Young_Cohort_ic is a dummy variable equal to 1 if the individual was aged four years and under at the time of the policy change in sub-experiment c. The coefficient β_{pooled} thus captures the average intent-to-treat effect of exposure to stricter regulations during early childhood. The preferred specification also includes state-specific linear cohort trends, $\lambda_s \times c$. The second specification replaces the pooled treatment indicator with a more flexible, age-specific specification to examine heterogeneity across ages and to visually assess the parallel trends assumption:

$$Y_{isc} = \sum_{a=0}^{10} \beta_a \cdot \big(\text{Treated\_State}_s \times 1[\text{Age\_at\_policy}_{ic} = a]\big) + \mu_c + \eta_s + \lambda_s \times c + \epsilon_{isc} \qquad (2)$$

In this model, $\text{Age\_at\_policy}_{ic}$ is the individual's exact age (zero to ten years) at the time of the regulatory change. The coefficients β_a for ages 0 through 4 represent the age-specific treatment effects, while the coefficients for ages 5 through 10 serve as placebo tests, verifying that older children who should not be affected by the policy do not show spurious effects. Plotting these coefficients creates an event-study graph that provides a direct test of pre-trends.

The use state-specific event cohort trends is a deliberate one. Event study plot of the specification without the inclusion of state-specific event cohort trends (shown in Figure A.1) reveals a slight upward-sloping pre-trend for the older cohorts (ages 5-10), suggesting that states adopting stricter regulations may have been on a positive trajectory for child outcomes relative to control states. While a joint test of these pre-treatment coefficients fails to reject the null of zero effect (p = 0.143), the pattern indicates a possible violation of the parallel trends-in-levels assumption.

To account for these underlying, state-specific confounders, I relax the identifying assumption to require parallel trends in deviations from a state-specific linear path. Consequently, my preferred specification includes state-specific linear cohort trends. However, in the results section I show that my estimates do not change considerably when using the specification without state-specific event cohort trends.

V Results

V.1 Main Intent-to-Treat Effect on Adult Income

This section presents the core intent-to-treat (ITT) estimate of exposure to stricter childcare regulations on annual adult earnings. The results, derived from the stacked difference-in-differences design, indicate a positive and statistically significant economic return.

V.1.1 *Average Early Childhood Treatment Effect*

The pooled estimate from Equation 1, which provides the average treatment effect across all early childhood years (ages four years and under), shows that individuals exposed to stricter regulations earned $1,392 more per year in adulthood (SE = 365.68) compared to the control groups (Column 1 of Table 2). This represents an increase of approximately 2.5% over the baseline mean. This estimate captures the population-level impact of the policy, reflecting a combination of direct effects on children who experienced higher-quality care and any general equilibrium effects on their peers.

For context, a traditional two-way fixed effects (TWFE) estimation, yields a similar but slightly smaller coefficient of $1,035 (SE = 433.0). The robustness of the point estimate across specifications, and its larger magnitude in the stacked design—which avoids using already-treated units as controls—increases confidence that the preferred specification provides a more reliable assessment of the policy's effect.

The event-study specification from Equation 2, estimates effects for each age of exposure, supports this finding and validates the research design. As shown in Figure 2, the coefficients for the pre-treatment ages (five to ten) are statistically indistinguishable from zero, providing visual evidence supporting the parallel trends assumption. More importantly, the coefficients for the treated ages (four years and under) are uniformly positive, indicating a consistent beneficial effect of exposure during early childhood.

Further analysis of the event-study coefficients reveals no statistically significant gradient in the treatment effect across ages 0 through 4 (F = 0.84, p = 0.3635). A plausible explanation for this pattern lies in differential enrollment rates: while infants (under one year of age) who are enrolled accumulate more years of regulated care, they represent a much smaller fraction of their cohort than the four-year-olds. The strong effect at age 4 thus likely reflects the power of broad exposure, even if cumulative duration is shorter. The lack of a strong dose-response relationship indicates that the fact of exposure to a regulated environment during these early years is what matters most for the long-run outcome, rather than the cumulative duration of exposure.

V.1.2 *Distributional Effects*

The benefits were not uniformly distributed across the adult income spectrum. Figure 3 plots the results of quintile regression using log of income as the outcome variable to produce percentage treatment effects across the distribution of own adult earnings. The pattern reveals a pronounced inverted U-shape: the largest proportional gains accrued to individuals in the lower-middle and middle of the income distribution (25th through 50th percentile). In contrast, the effects for the lowest (bottom 10%) and highest (top 10%) earners were smaller and slightly noisier. This indicates that the primary beneficiaries were lower-middle and middle-income earners, not those at the very top or bottom of the economic ladder. Since the outcome variable here is log-transformed, the non-workers who earn zero income are dropped and the estimates are larger than the level-effects.

V.1.3 *Robustness of the Main Estimates*

The positive effect of early childhood exposure to stricter regulations on adult income is highly robust to a variety of model specifications and the inclusion of potential confounders. Table 2 presents results for different specifications. Column 1 presents the baseline estimate from the preferred stacked difference-in-differences specification, which includes state-specific event cohort fixed effects. The stability of this estimate is a primary indicator of its robustness. Columns 2 and 3 add policy covariates and 1980 state characteristics to the preferred specification, respectively. In Column 2, I add time-varying state policy controls: K-12 expenditure per pupil and maximum AFDC generosity. Column 3 includes the existence of a public pre-K program; the employment rate of mothers with children under age five; and population density, all for the year 1980 (prior to any regulatory changes). In both cases the coefficients remain quite stable as compared to the baseline specification.

Column 4 adds controls for all other childcare regulations. My dataset consists of over 40 regulations applicable to childcare centers. I construct summary indices for staff qualifications and training requirements using regulations showing strong thematic coherence. For the regulatory variables that did not cluster into reliable thematic indices, I use principal component analysis to parsimoniously capture regulatory variation. Again, the coefficient of interest remains relatively stable, suggesting the results are not driven by concurrent changes in other child care regulations.

In Column 5, I exclude state-specific event-cohort trends but include age, survey year, and birth year fixed effects. The result is again very similar to the estimates found in Columns 1-4. Next, in Column 6, the point estimate is somewhat larger yet still robust to the exclusion of both state-specific event-cohort trends and age, survey year, and birth year fixed effects. Finally, I also present the results of regression using the traditional Two-Way Fixed Effects (TWFE) model in Column 7, which also produces similar income estimates.

V.2 Effects on Other Dimensions of Adult Success

This section examines the impact of stricter childcare regulations on a broader set of adult outcomes to provide a more comprehensive picture of their long-run effects and to identify the specific channels through which the observed income gains were achieved. I analyze labor supply, educational attainment, occupational choice, and social outcomes. Table 4 presents the results for these outcomes using both the preferred stacked difference-in-differences (DiD) estimator (Panel A) and a simple DiD specification (Panel B) for robustness.

V.2.1 *Labor Supply*

Figure 4 shows the treatment effect on likelihood of employment; number of weeks worked in the previous year; and usual weekly hours worked. The results suggest that the regulations had no statistically significant effect on either the extensive or intensive margins of labor supply. Results suggest that the regulations had no statistically significant effect on neither extensive nor intensive margins of labor supply. These null findings imply that the regulation's benefits, concentrated among middle-income earners, stemmed from gains in worker productivity and human capital rather than from changes in labor force participation or the quantity of work supplied.

V.2.2 *Educational Attainment*

Results on different education outcomes show an interesting pattern. Figure 5 shows the treatment effect relative to the five-year olds at the time of policy. Although the effects on high school and college graduation rates are positive, they are small and imprecise. However, exposure to stricter regulations in early childhood led to a statistically significant 0.5 percentage-point increase in the rate of obtaining a graduate or professional degree. This represents a substantial intensive-margin gain, indicating that the policy provided a catalyst for individuals at the threshold of pursuing advanced education to complete their graduate degrees.

V.2.3 *Professional and Social Choices*

Figure 6 shows the results for professional and social choices. Treated individuals were 0.6 percentage points more likely to be employed in managerial, scientific, or professional occupations, which tend to be higher-paying jobs that leverage advanced skills [4]. Moreover, the regulations significantly influenced social outcomes, increasing the probability of being married by 1.3 percentage points and reducing the likelihood of a teenage pregnancy by 2.2

[4] As defined by ACS PUMS Occupation Code: 0010 - 3550 https://usa.ipums.org/usa/volii/c2ssoccup.shtml

percentage points.

Thus, the benefits of stricter childcare regulations manifest not in the quantity of work or basic educational milestones, but in the quality of professional and personal outcomes. The significant increases in graduate degree attainment, high-skilled employment, and marital rates, coupled with a sharp decline in teenage pregnancy, likely reflects that the policy fostered the development of valuable non-cognitive skills such as patience, forward-planning, and risk-aversion, which yielded long-term economic and social dividends.

V.3 Heterogeneity in the Income Effect

Income effect averaged across treated ages masks significant heterogeneity in the treatment effect across individual demographics, income distribution, state-level economic context.

Table 3 shows the results of heterogeneous effects on adult income by gender and race. The standard errors for individual race groups are rather large, suggesting power issue. Even based on gender, the estimates are not precise enough to detect statistically significant differences.

Next, the economic context of a state moderates the impact of regulations. I construct a state economic disadvantage index using 1980 baseline characteristics (per capita income, percentage of college graduates, education expenditure per pupil, and government revenue per capita). The results indicate that a one standard deviation increase in this index reduces the treatment effect by approximately \$688. Consequently, the effect was largest and most significant in the least disadvantaged states (\$2,614), while it was small and statistically indistinguishable from zero in the most disadvantaged states (\$935). This pattern provides indicates that the long-run returns to regulation were associated with state's underlying economic capacity, reflecting the role of enforcement in deriving quality benefits.

Next, I examine whether the economic context of a state moderates the impact of the regulations on long-term earnings. To systematically capture a state's underlying economic health at the baseline period preceding the regulation, I construct a State Economic Disadvantage Index using key characteristics from the 1980 census. This index is designed to capture multiple dimensions of a state's fiscal and human capital resources. I collected a wide variety of state-level characteristics and, through principal component analysis, identified four factors most relevant to a state's economic condition: per capita income (PCI), the percentage of college graduates, education expenditure per pupil, and government revenue per capita. The final index is created by averaging the z-scores of these four variables, providing a standardized and comprehensive measure of a state's relative economic disadvantage.

The results, presented in Table 6, indicate that a state's economic capacity is a likely moderator of the regulation's effect. The negative and weakly significant coefficient in Column (1) shows that a one standard deviation increase in the State Economic Disadvantage Index reduces the positive treatment effect on income by approximately $688. To illustrate the economic magnitude of this heterogeneity, the effect was largest and most significant in the least disadvantaged states ($2,614), while it was small and statistically indistinguishable from zero in the most disadvantaged states ($935).

To ensure that this finding is not driven by a single component of the index, Columns (2) through (5) present the results of separate regressions where the key interaction term is interacted with each individual sub-index. The results show that no one factor alone drives the overall result; the coefficients for the individual interactions with per capita income, education expenditure, percent college graduates, and government revenue are all positive but statistically insignificant and notably smaller in magnitude than the effect of the composite index.

Finally, I examine whether the treatment effect varies by the level of state urbanization. Center-based childcare providers tend to be concentrated in urban areas. Therefore, a concern is that more urban states may be driving the income results. However, I find that the interaction between the treatment indicator and the state's percentage of urban population is small and statistically insignificant Table 7, the interaction between the treatment indicator and the state's percentage of urban population is small and statistically insignificant (Panel A). A subgroup analysis confirms this pattern, revealing positive and significant income effects in both high-urban and low-urban states, with point estimates of \$1,530 and \$1,248, respectively (Panel B). While the effect is moderately larger in more urban states—potentially reflecting better enforcement infrastructure or a denser market for formal childcare—the absence of a statistically significant interaction indicates that the core benefit of stricter regulations was not uniquely dependent on a highly urbanized context. This suggests that the policy's human capital returns were broadly generalizable across different geographic settings.

These findings suggests that the regulations were more effective in states with greater underlying capacity, likely due to better infrastructure and stronger enforcement mechanisms that ensured the policy's intended benefits were realized.

V.4 PSID and PSID-SHELF Analysis

To assess the robustness of the primary findings derived from the ACS sample, I also replicate the empirical strategy using PSID and PSID-SHELF data, which are collected from a nationally representative longitudinal survey. The advantage of using these data sources is that they contains rich retrospective information on an individual's childhood environment, including the state in which they grew up, as well as detailed data on their parents' socioeconomic characteristics, such as education and income levels. This allows for a direct measure of the family background controls that were proxied using state-level information in the main ACS analysis.

Despite this significant benefit, the PSID and PSID-SHELF are not the preferred dataset for the primary analysis due to a critical limitation: sample size. Filtering out the individuals who do not fit the specified birth year and state restrictions, the final sample only consists of 5,422 distinct individuals. This is several orders of magnitude smaller than the ACS sample of over 6 million individuals. The consequence of this limited sample is a substantial reduction in statistical power. As expected, the estimates generated from the PSID analysis are considerably noisier, reflected in larger standard errors and less precise coefficient estimates.

Notwithstanding this limitation, the same stacked DiD design is implemented on these data. The key results for adult outcomes are presented in Table 5. The coefficient of interest,

$Treated\_state \times Young\_cohort$, shows directional alignment with the main findings for several outcomes, though statistical significance is attenuated due to the high variance. For instance, the point estimates for years of education are positive and statistically significant at the 10% level, suggesting a positive effect of the regulation on educational attainment, consistent with the ACS results. The estimates for earnings, employment, current marital status are positive but statistically indistinguishable from zero. Similar to results found in ACS sample, teenage pregnancy rates among the treated individual is lower, though again the estimates are imprecise.

To further explore heterogeneity in the treatment effects, I conducted additional analyses using the PSID's detailed family background data. These analyses examine how the effect of the regulation varied across subgroups defined by childhood family income, parental education, race, and gender. The results of these subgroup analyses are provided in the appendix tables A.3, A.4, and A.5 .

V.5 Effects on Migration Choice

A potential concern is that the estimated effects are driven by selective migration rather than the direct impact of childcare regulations. Families might have moved across state lines to access better childcare, or treated individuals might have relocated as adults to pursue better economic opportunities, thereby confounding the comparison between treated and control states.

To test this, I examine whether exposure to stricter regulations affected subsequent migration patterns. The outcomes are defined using an individual's state of birth and current state of residence in the ACS: the likelihood of currently living in one's birth state; and, for those who moved, the likelihood of living in a never-treated state or another treated state. It is important to note that this variable identifies whether an individual's current state of residence differs from their state of birth. It does not reveal when the move occurred, how many times they moved, or their reasons for moving.

The results, presented in Table 8, show that exposure to stricter regulations is associated with a very small, 0.6 percentage points, increase in the probability of migrating out of one's birth state. However, there is no clear pattern in the choice of destination among those who moved; the point estimates for moving to a control state or another treated state are small and statistically insignificant.

This slight increase in mobility alone does not establish selective migration as a key channel. The critical test is whether the treatment effects on income and human capital are larger for those who moved, which would suggest that moving to access better opportunities is a driving mechanism. As shown in Appendix Table A.2, the interaction between the treatment indicator and an indicator for having moved is small and statistically insignificant for all key outcomes—income, employment, and graduate degree attainment. This indicates no meaningful difference in returns between those who moved and those who stayed.

In conclusion, while the regulations may have induced a minor increase in geographic mobility, the absence of heterogeneous treatment effects by migration status indicates that selective migration does not account for the improved adult outcomes I observe.

VI Mechanism

The long-run benefits of early childhood regulation on adult income, advanced degrees, and occupational standing are clear for a significant segment of the treated cohort. However, these gains were not universal, showing heterogeneity by socioeconomic background. This section explores the mechanisms underlying these patterns, examining the short-run market disruptions, the behavioral responses of families, and how these factors may have collectively shaped human capital development through channels that plausibly include non-cognitive skill formation.

VI.1 Short-Run Market Adjustments and Heterogeneous Supply Shock

The implementation of stricter regulations induced a non-trivial contraction in the supply of formal childcare centers. My analysis for contemporaneous outcomes uses the de Chaisemartin and D'Haultfœuille (2023) tw-way fixed effects estimator. My results indicate an overall reduction in the number of establishments between 6% and 16%, though these estimates are somewhat noisy. This finding is consistent with more precise establishment-level data from Hotz and Xiao (2011), who document a significant 9-11% decline in centers following similar regulatory changes.

Crucially, this supply shock was not evenly distributed. The decline was disproportionately larger in areas with higher pre-existing unemployment rates and a higher percentage of Black residents (see Table 9). This heterogeneous impact suggests that the initial market disruption exacerbated existing disparities in access to regulated care. While the raw number of centers decreased, it is important to note that surviving and merging centers may have rapidly expanded their capacity, leaving the net effect on total enrollment ambiguous. Nevertheless, the geographic pattern of closures is a key factor in understanding the heterogeneity of long-run effects, as the communities most affected by the initial loss of access appear to have benefited less in adulthood.

VI.2 Family and Care Substitution Responses

A critical piece of the mechanistic puzzle is the absence of a significant negative impact on maternal labor market outcomes. Figure 7 shos the event study results. Neither the event studies, nor the two-way fixed effects estimates show meaningful change in maternal employment (average effect: 0.005) or earnings (average effect: $740). This null result implies that the reduction in center-based supply may have been offset by a substitution toward alternative childcare arrangements.

I interpret this as evidence of crowd-out from regulated centers to other forms of care, likely including licensed family childcare homes and unlicensed care provided by relatives, neighbors, or nannies. This interpretation is supported by Hotz and Xiao (2011), who found an increase in revenues for non-employer (typically home-based) childcare establishments following regulatory tightening, indicating increased enrollment in these settings.

This crowd-out dynamic is central to the mechanism. The children who moved from closing centers may not have simply dropped out of formal care; instead, they transitioned to alternative arrangements. It is plausible that for a subset of children, the quality of care in these new settings—whether due to more stable parental care, smaller group sizes in family homes, or more attentive relatives—was superior to the quality of the centers that closed. This potential for a net quality improvement for some children, driven by market-induced substitution, is a plausible pathway through which regulation could generate long-run benefits even amid a reduction in the overall number of centers.

VI.3 A Suggestive Interpretation: The Role of Non-Cognitive Skill Formation

Tying together the pattern of long-run results—gains in income, graduate degrees, and high-status occupations, but not in college attendance or employment—points toward a mechanism that operates beyond pure academic aptitude. The evidence is consistent with the development of non-cognitive skills, such as persistence, self-regulation, and interpersonal effectiveness.

Persistence and Goal Completion: The lack of effect on college attendance coupled with a significant increase in graduate degree completion suggests that treated individuals were not more likely to enter college but were more likely to persist and attain advanced degrees once there. This pattern is a hallmark of tenacity and long-term goal orientation.

Interpersonal and Organizational Skills: The shift into managerial and other higher-paying occupations, conditional on being employed, indicates advantages in skills that are valued in the labor market but not directly measured by test scores, such as communication, teamwork, and organization (Lindqvist and Vestman, 2011; Deming, 2017).

Self-Control and Forward Planning: The significant reductions in teen pregnancy and increases in marriage rates in adulthood are life outcomes strongly associated with greater self-control, planning ability, and delayed gratification (Fletcher and Padrón, 2016).

It is important to emphasize that this interpretation is suggestive. I lack direct measures of non-cognitive skills from childhood to conclusively prove this channel. However, the constellation of null results in some domains (e.g., baseline employment, college entry) and positive results in others that require sustained effort and savvy (e.g., graduate degrees, career advancement) is difficult to explain through cognitive gains alone. The regulatory environment, by potentially shifting children into care settings that fostered these behavioral skills, may have laid a foundation for success that manifested precisely in the domains of adult life where non-cognitive skills are most decisive.

VII Conclusion

This paper presents the first causal evidence on the long-term adult outcomes from exposure in childhood to stricter childcare regulation. By exploiting quasi-experimental variation in the timing of regulatory changes across 31 states, I find that these policies generated a meaningful, permanent return on investment, increasing adult earnings annually by about \$1,400. This positive average net effect masks important heterogeneity, however, which is crucial to understanding the true impact of such regulations.

The legacy of childcare regulation is one of transformed trajectories, not expanded access. The benefits were distributed primarily to children from upper middle-income families, who presumably continued to enjoy access to the formal childcare market. The regulations helped them by fostering than further upward mobility, in their increased attainment of advanced degrees, promotion into better-paid managerial jobs, enhanced social stability through increased marriage rates combined with lower rates of teenage pregnancy, etc. By contrast, children from the lowest income families, who would have been disadvantages by lack of access due to provider exit, do not evidence any of these benefits.

This set of results suggests that the primary channel through which these regulations operated was the increased development of non-cognitive skills in very young children. The null effect on college graduation as well as employment, together with strong effects on graduate degrees, managerial jobs, and wiser social choices denotes improvement in traits like persistence, conscientiousness, self-control, etc. This explanation is consistent with a model in which stricter regulations improved the quality of the process of child care—the richness of the social interactions and environment of the learning process — for those who remained in the system.

My results provide an important nuance to the literature on investments in early childhood. Whereas landmark studies of large intensive interventions, aimed at the disadvantaged, find improvements for the most disadvantaged children in their cognitive and noncognitive abilities by reducing their deficiencies in both, I have shown that, in such a large regulatory policy as this, it is not the most disadvantaged children who gain. Rather the beneficiaries of this large scale regulatory activity were children of the middle classes. For them, whose cognitive development may already be favored by their otherwise well provisioned homes, the binding constraint to obtaining elite results appears to have been noncognitive ability. The intervention of better quality formal care seemed to remove this constraint (Bassok et al., 2008).

The policy implications are twofold and reflect a deep-seated trade-off between contrasting sets of policy prescriptions. On the one hand it can be said that improved quality by way of input based regulations can indeed lead to very considerable long run returns, thus rendering such policies valuations as desirable investments in human capital. On the other hand, there is no universal redistribution of the benefits which might ensue, and above all these benefits can by-pass completely the most disadvantage children, unless the implementation of such a policy of quality improvement were not simultaneously to restrict supply in low income areas. Hence the optimal way of devising child care policy must not be merely concentrated on the quality. How is it, that if the promise of the early starting scheme is to be available to all, the quality regulations, have to be combined with subsidies, or direct public provision being given, to be available for the low income family, and to protect against the emergence of the two tier system where quality is the privilege of the stable middle class.

In this way this study can draw the conclusion that the early childhood environment constituted by public policy mingles, historically, a season of very considerable length of time. The further fact is, that the growth of regulatory methods has produced this result that the additional opportunities have not been expanded for the purpose, but the effectiveness of the existing opportunities have become much increased. Instead of the opportunity pipeline becoming bigger, the output has become bigger, and the good students have become exceptional students, and the decent careers into lucrative careers. The problem of future policy is to produce a system that will do both.

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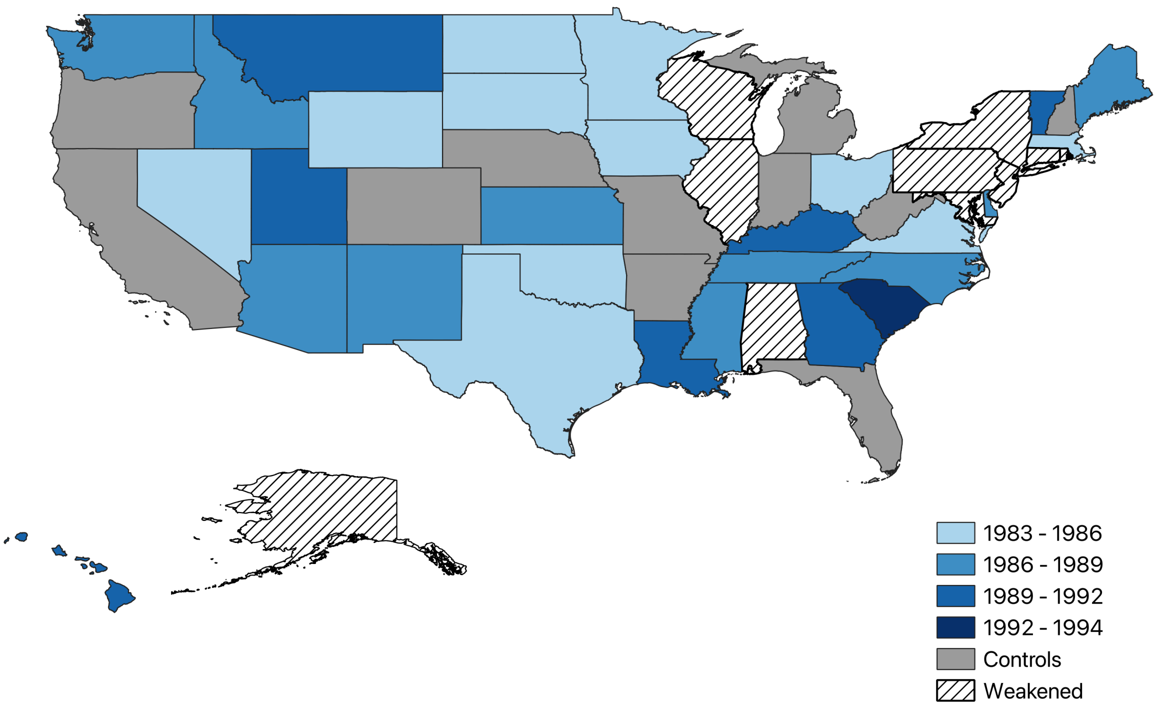
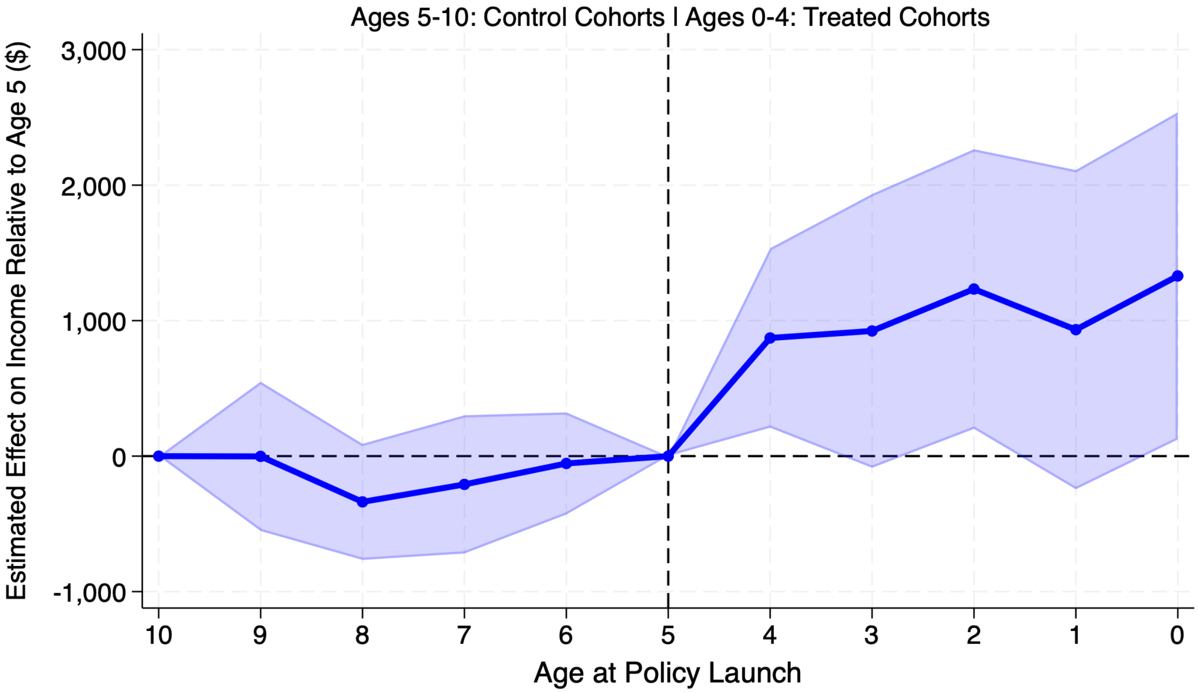
Figures

Figure 1: Adoption Year of Stricter Day care Regulations Across States



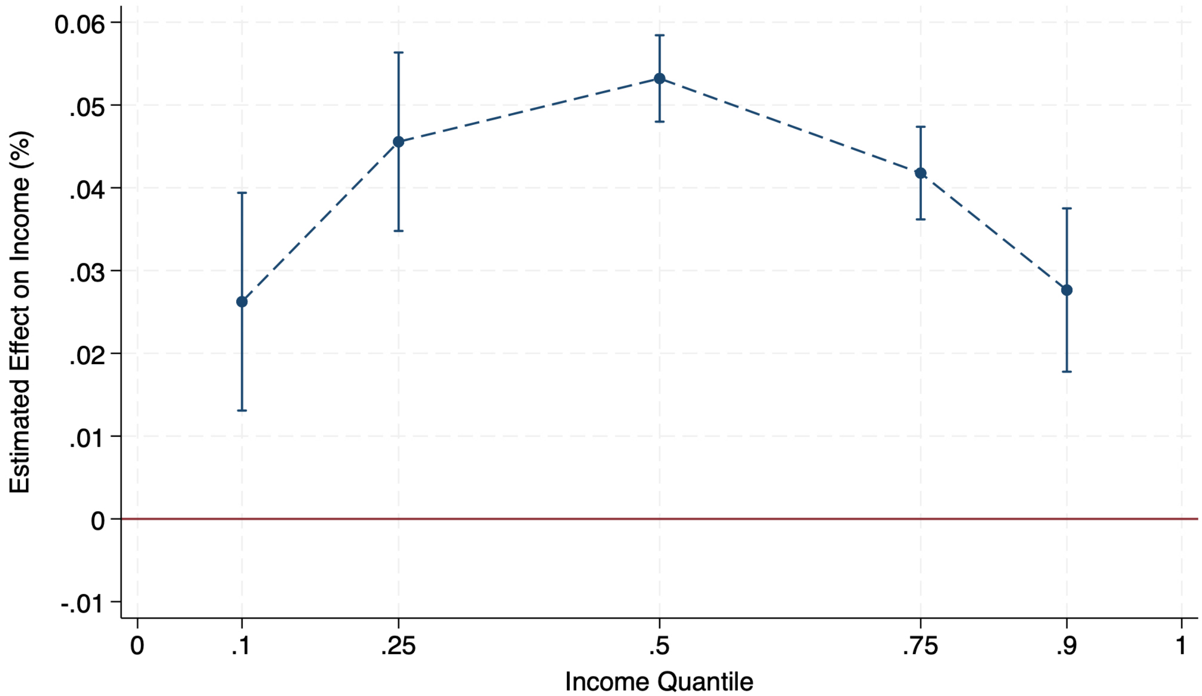
Notes: The map shows the range of years within which states adopted either stricter child-to-staff ratio (CSR) or higher education requirement for center directors, or both. Lighter blue states adopted stricter regulation earlier and darker blue adopted later. The ten gray states did not change either regulation during this period and therefore serve as my pure control states. The shaded states weakened one or both of these regulations during this period and are therefore not part of the analysis. Data are from Hotz and Kilburn (2010)

Figure 2: Effect on Annual Adult Income



Notes: The above figure shows the ITT effect on annual income for treated cohorts (ages four years and under), following the equation 2. X-axis represents the cohorts of children by their age at the time of policy. The 5 year olds at the time of policy are the baseline group. Y-axis shows the difference in adult income between treated and control groups relative to the 5 year old cohort in their respective states. Income data are from ACS 5-year 2019-2023 sample. All monetary values in 2023 dollars.

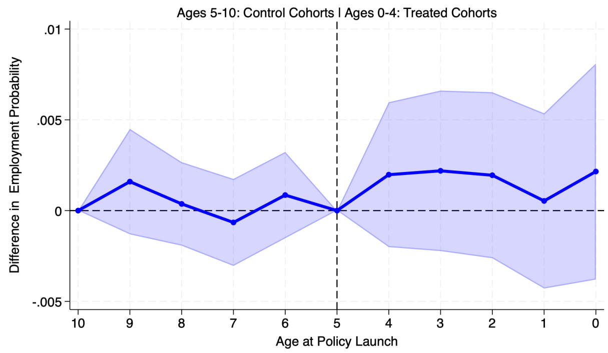
Figure 3: Effects of the Regulation Across the Income Distribution



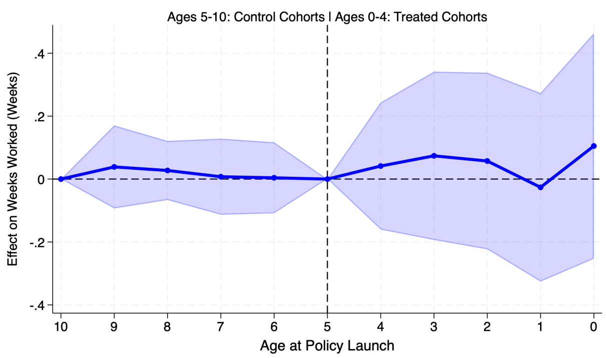
Notes: This figure presents the percentage treatment effects of regulations across the distribution of adult earnings. The estimates are derived from a quantile regression of the log of annual income. Since long-transformation drops non-workers earning zero income, these proportional effects are larger than the level-effect estimates presented elsewhere in the paper. Income data are from ACS 5-year 2019-2023 sample.

Figure 4: Effect on Labor Supply

(a) Employment Rate



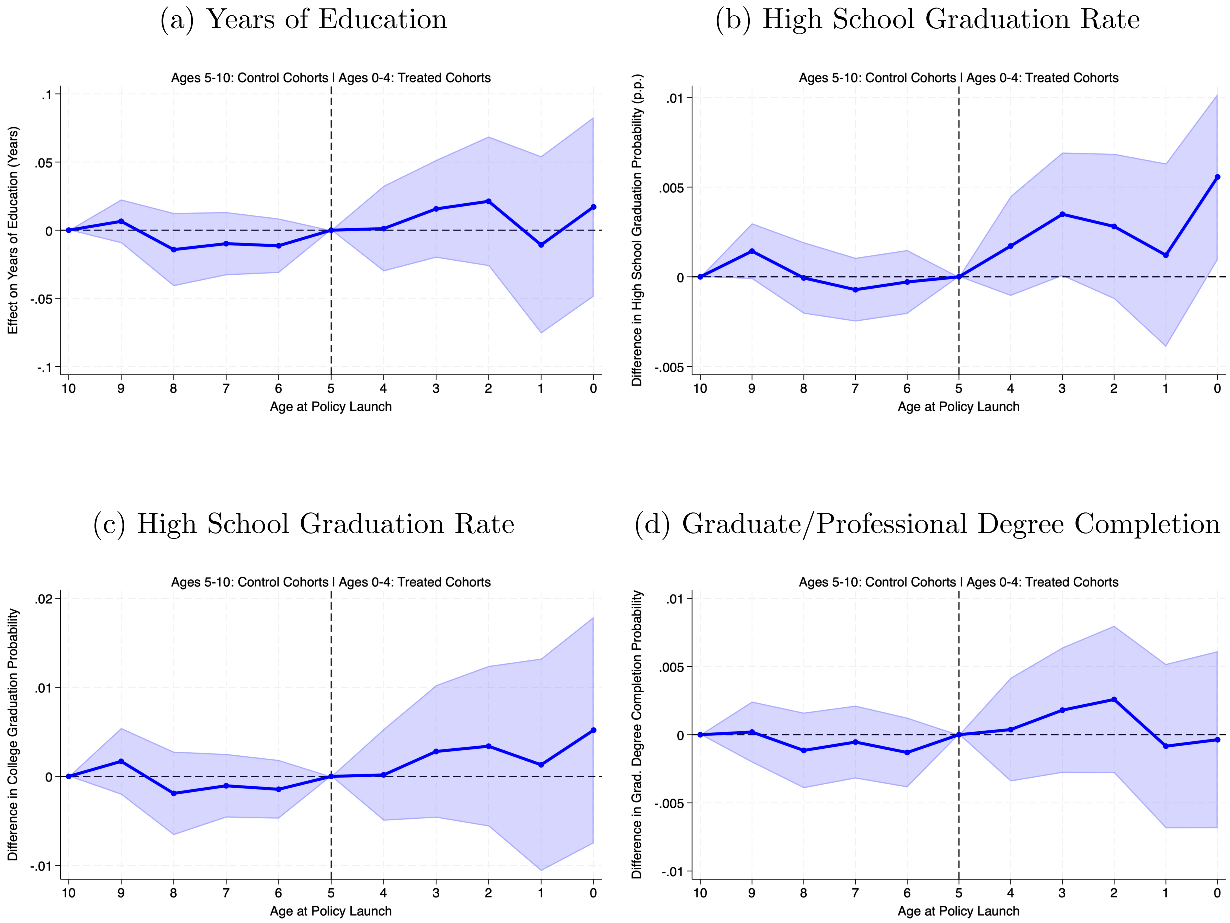
(b) Weeks Worked Last Year



(c) Usual Weekly Hours Worked

Notes: Each figure shows the effect of regulations on a different measure of labor supply by age at treatment implementation, following equation 2. Figure (a) shows the effects on likelihood of being employed; figure (b) shows the effects for the number of weeks worked last year; and figure (c) shows the effect on usual weekly hours worked. Data are from ACS 5-year 2019-2023 sample.

Figure 5: Effect on Education



(a) Years of Education

(b) High School Graduation Rate

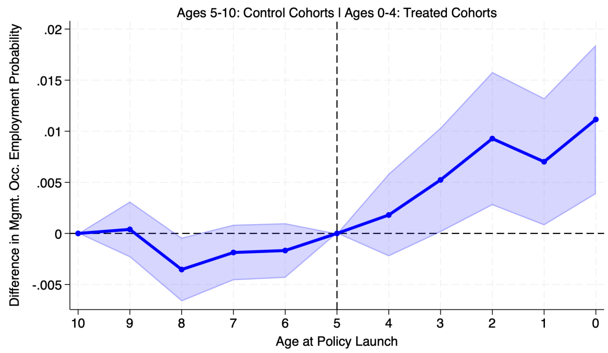
(c) High School Graduation Rate

(d) Graduate/Professional Degree Completion

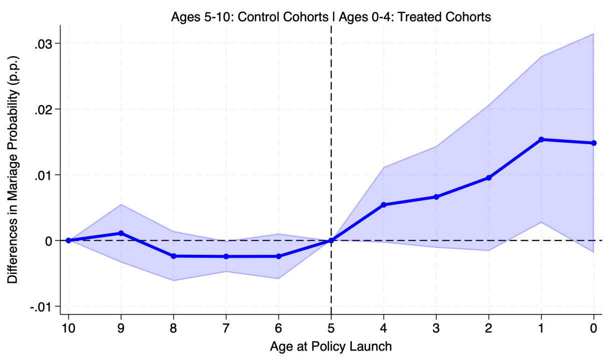
Notes: Each figure shows the effect of regulations on a different education outcome by age at treatment implementation, following equation 2. Figure (a) shows the effects on the total years of education completed; figure (b) shows the effects on the rate of high school completion; figure (c) presents the effects on college graduation rates; and figure (d) shows the effects on graduate or professional degree completion. Data are from ACS 5-year 2019-2023 sample.

Figure 6: Effect on Professional and Social Choices

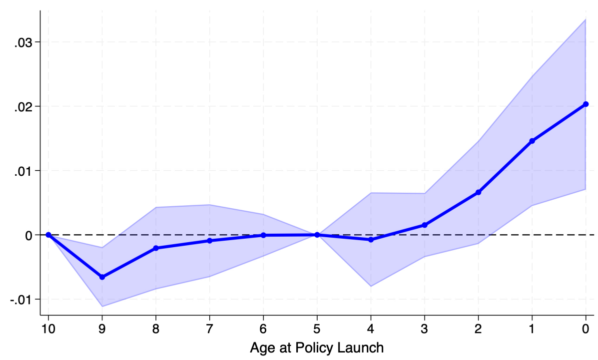
(a) Employment in Management Occupations



(b) Marriage Rate



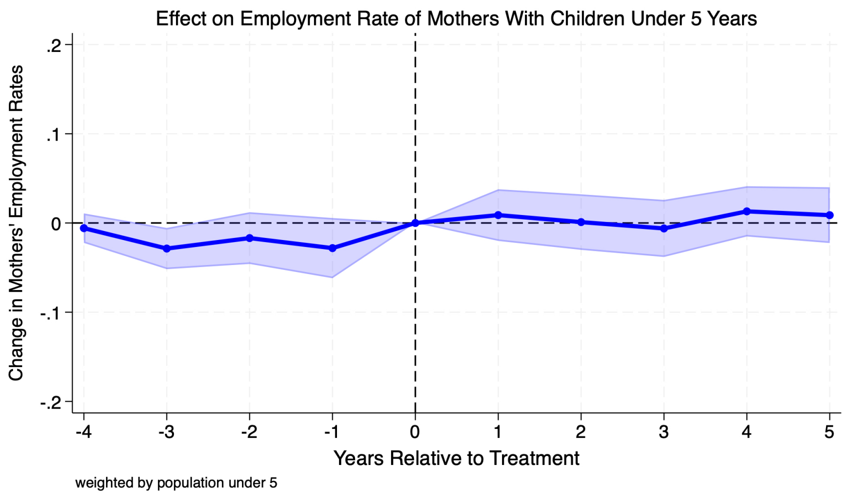
(c) Teenage Pregnancy



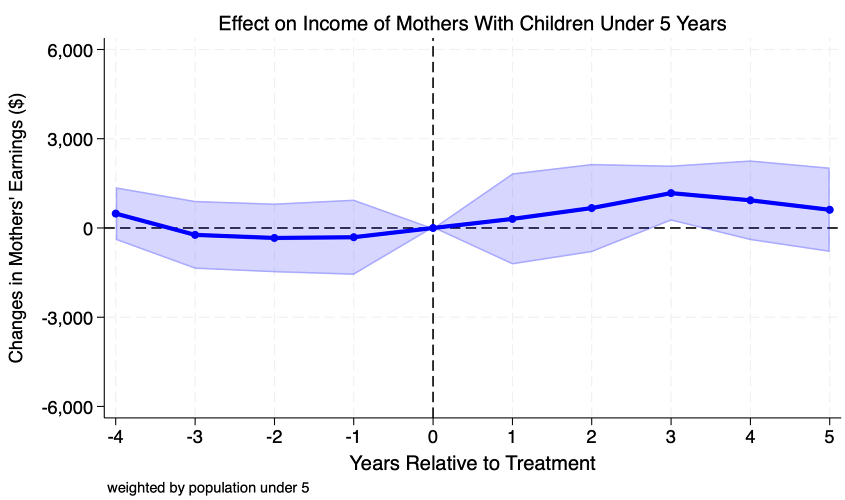
Notes: Each figure shows the effect of regulations on a different professional or social outcome by age at treatment implementation, following equation 2. Figure (a) shows the effects on the likelihood of being employed in a managerial/scientific/professional occupation; figure (b) shows the effects on marriage rate; and figure (c) presents the impact on rates of teenage pregnancy. Data are from ACS 5-year 2019-2023 sample.

Figure 7: Effect on Maternal Labor Supply

(a) Maternal employment rate

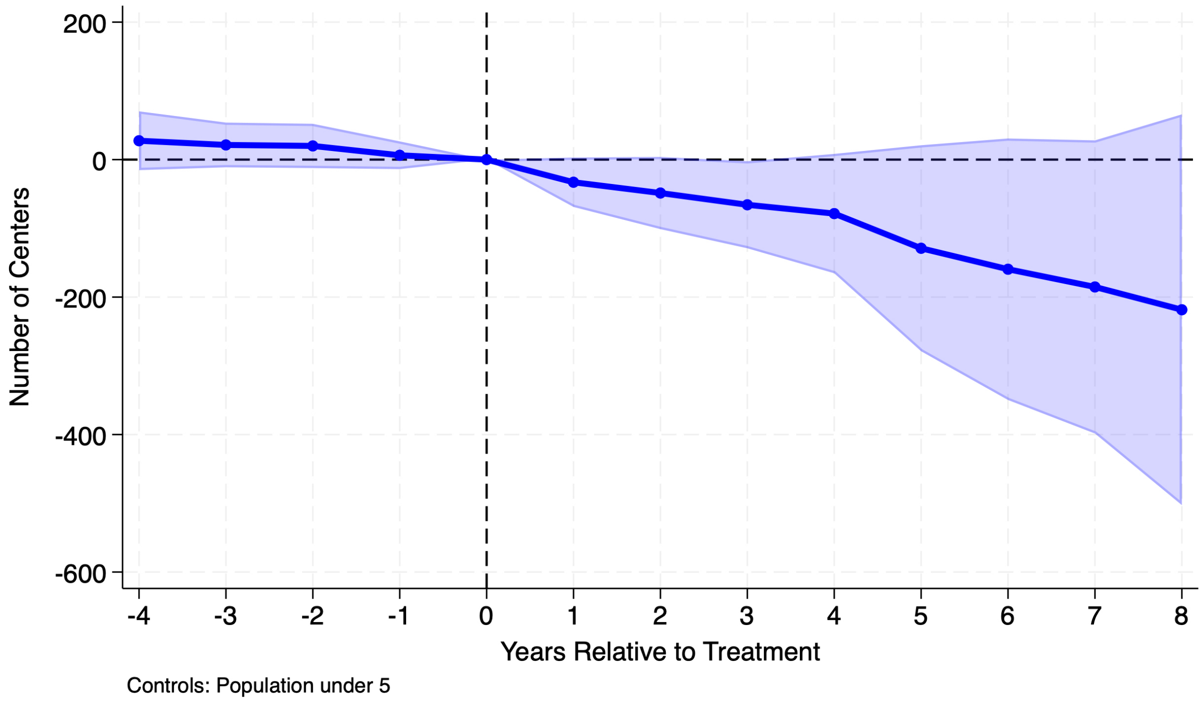


(b) Maternal earnings



Notes: Each figure shows the effect of regulations on mothers' labor supply outcomes. Figure (a) shows the effects on likelihood of being employed; figure (b) shows the effects on earnings in 2023 dollars. Each regression is weighted by number of children under 5 for the state. Data are from IPUMS CPS 1980-2000 sample.

Figure 8: Effect on the Number of Childcare Centers



Notes: The above figure shows the effect of regulations on the number of total childcare centers. Regression uses de Chaisemartin and D'Haultfœuille (2023) estimator. The regression controls for the number of children under 5 years old. Data are from QCEW 1980-2000.

Tables

Table 1: Baseline Descriptive Statistics for Treated and Never-Treated States

	(1)	(2)	(3)
Covariates	Control	Treated	Difference
Total population	5,558,180	3,401,592	-2,156,588
Population under 5	408,760	254,042	-154,718
Per capita income	$27,796.29	$26,763.98	-$1,032.31
% Non-white	0.10	0.17	0.07
% High school graduates	0.37	0.35	-0.021
% College graduates	0.156	0.163	0.007
Democrat governor	0.6	0.63	0.03
% urban	64.57	65.82	1.25
Birth rate†	30.89	17.42	-13.47
Fertility rate△	69.72	74.67	4.95
Average AFDC per participant	342.592	304.410	-38.182
Median home value	$166,530.5	$167,142.02	$611.52
Education expenditure per pupil	8,396.50	8,160.51	-235.991
Unemployment rate	7.63	6.32	-1.31*
Public pre-k⋇	0.1	0.065	-0.035

Notes: Data are from various sources described in Section III. All dollar amounts in 2023 values.
† Birth rate is defined as births per 1,000 population.
△ Fertility rate is defined as births per 1,000 women aged 15-44.
⋇ Only 3 states (1 control and 2 treated) had introduced public pre-K program as defined by NIEER 2019 State of Preschool Yearbook
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2: Robustness of Income Estimates to Different Specifications

Outcome Variable: Income	Stacked DiD						TWFE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated_state × Young_cohort	1,392.4***	1,329.7***	1,392***	1,639***	1,237.535**	2,670***	1,035**
	(356.7)	(358.1)	(356.6)	(435.52)	(443.8)	(542.2)	(433.0)
Covariates		✓					
Baseline Controls			✓				
Summary Indices of All Regulations				✓			
Age, Survey year and Birth year FE					✓		
Event-cohort trends	✓	✓	✓	✓			

Notes: Each cell reports the coefficient from a separate regression model for the interaction term: Treated_state × Young_cohort. Columns (1-4) all include event-cohort trends following Equation 1. Columns (2) adds time-varying state policy covariates: K-12 expenditure per pupil and maximum AFDC generosity. Column (3) includes select 1980 state characteristics: the existence of a public pre-K program; the employment rate of mothers with children under age five; and population density. Column (4) controls for all other childcare regulations: two summary indices (staff qualifications and training requirements) plus eight principal components extracted from remaining regulations. Column (5) drops event-cohort trends and instead includes age, survey year, and birth year fixed effects. Column (6) only includes the standard two-way fixed effects from sub-experiment and event-cohort. Column (7) shows the result from simple DiD estimation. Data comes from 2019-2023 5-year ACS sample. Robust standard errors clustered at state of birth level are presented in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Treatment Heterogeneity Across Gender and Race

		Interaction with Race		
	Female	Black	Non-White Hispanic	All Other Races
	(1)	(2)	(3)	(4)
Outcome Variable: Income	-1,493.05	233.32	-627.10	1,970.51
	(993.23)	(907.12)	(1037.43)	(1552.41)

Notes: Each cell reports the coefficient from a separate regression model for the triple interaction term: Treated_state × Young_cohort × Group. The demographic group for the interaction is listed in the column header. Data comes from 2019-2023 5-year ACS sample. Robust standard errors clustered at state of birth level are presented in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: All Outcomes by Stacked DiD and Simple DiD

	Income	Emp	Weeks Worked	Weekly Hrs. Worked	Edu Years	HS Grad	BS Grad	Grad Degree	Mgmt. Occ	Married	Teenage Pregnancy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Stacked DiD											
Treated_state	1,392.4***	0.002	0.06	0.16	0.03	0.001	0.004	0.005**	0.006**	0.01***	-0.022***
× Young_cohort	(356.68)	(0.002)	(0.11)	(0.1)	(0.01)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Panel B: Simple DiD											
Treated Cohort	1,035**	0.0000	-0.04	-0.05	0.01	0.002	0.001	0.005**	0.004*	0.01	-0.01
	(433.0)	(0.003)	(0.12)	(0.1)	(0.02)	(0.002)	(0.002)	(0.001)	(0.002)	(0.01)	(0.01)

Notes: Panel A shows results of stacked DiD with various outcomes, following Equation 1. Treated_state refers to states with increased regulation strictness. Young_cohort is indicator variable for ages 0–4 at the time of treatment. Stacked DiD model includes state specific linear time trends. Panel B shows results of traditional difference-in-difference model with age, birth state, birth cohort, survey year, and birth state by survey year fixed effects. Treatment Cohort indicator variable for ages 0–4 in treated states. Following is the description of each outcome by column number: Column (1) annual income, including zero income for non-workers; Column (2) employment indicator; Column (3) number of weeks worked last year; Column (4) usual hours worked in a week; Column (5) number of years of education; Column (6) high school graduation indictor ; Column (7) college graduation indicator; Column (8) graduate/professional degree completion indicator; Column (9) indicator for employment in managerial/scientific/professional occupation as defined by (ACS PUMS Occupation Code: 0010 - 3550); Column (10) indicator for currently being married; Column (11) indicator for females who had their first child as a teenager (ages 13-19). Data are from 2019-2023 5-year ACS sample. Robust standard errors clustered at state of birth level are presented in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: The Effect of Regulation on Adult Outcomes: PSID data

	Earnings		Employment		Yrs. of Education		Married		Teenage Pregnancy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated_state × Young_cohort	3,518.5	2,486.4	-0.0078	-0.0096	0.54*	0.45*	0.052	0.047	-0.084	-0.076
	(3817.6)	(3626.5)	(0.0223)	(0.0211)	(0.212)	(0.216)	(0.054)	(0.054)	(0.042)	(0.042)
Controls		✓		✓		✓		✓		✓

Notes: Dependent variables listed in the column header. The interaction term "Treated_state × Young_cohort" represents the stacked difference-in-differences estimator. Columns 2, 4, 6, 8 and 10 present the results of regression with controls. Controls include childhood family income, parental education, and race. Data comes from Panel Study of Income Dynamics, public use dataset (2025) and Pfeffer et al. (2021). Robust standard errors clustered at state of birth level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: Heterogeneity by State Economic Disadvantage Index

Outcome Variable	Disadvantage Index	Interaction with Sub-indices			
		PCI	Edu. Exp	% College	Revenue
	(1)	(2)	(3)	(4)	(5)
Income	-687.88*	525.15	506.28	606.75	424.84
	(319.4)	(360.8)	(297.17)	(460.40)	(329.43)

Notes: Each cell reports the coefficient from a separate regression model for the triple interaction term. Column (1) interacts Treated_state × Young_cohort with state's disadvantage index. Columns (2)-(5) run separate regressions, interacting Treated_state × Young_cohort with individual components of the disadvantage index. Individual sub-indices are standardized. Income data are from 2019-2023 5-year ACS sample. State level urban population data come from Decennial Census. Robust standard errors clustered at state of birth level are presented in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Heterogeneity by Urbanicity

		Outcome Variable: Income
Panel A: Interaction with % Urban		
	Treated_state × Young_cohort × % Urban	16.37 (22.79)
Panel B: Sub-group Analysis		
High-urban States	Treated_state × Young_cohort	1,530.3**
		(406.5)
Low-urban States	Treated_state × Young_cohort	1,248.1*
		(604.9)

Notes: Panel A shows the result of heterogeneity analysis by urbanicity. The coefficient are from the triple interaction term: Treated_state × Young_cohort × percentage of population living in urban areas. Panel B presents results from sub-group analysis. "High-urban State" the treatment effect on income for states with above median percentage of population residing in urban areas. "Low-urban State" provides results for states with below median percentage of population living in urban areas. Data comes from 2019-2023 5-year ACS sample. Robust standard errors clustered at state of birth level are presented in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Effects on Migration Status

	(1) Moves out of Birth State	(2) Moves to Control State	(3) Moves to Treated State
Treated × Young Cohort	0.006*	0.002	0.004
	(0.003)	(0.002)	(0.002)

Notes: Results are from running the main stacked difference-in-difference regressions following Equation 1 with various migration variables as outcomes. Column (1) shows the likelihood of currently residing in a state that is not the state of birth. Column (2) reports the likelihood of currently residing in a control state, regardless of the treatment status of birth state. Column (3) presents the likelihood of currently residing in a control state if birth state was treated. Robust standard errors clustered at state of birth level are presented in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 9: Differential Rates of Decline in Childcare Centers by State Sub-Group

Sub-group	Effect on Log Childcare Centers
Above median total population	-0.067
	(0.035)
Above median % Black	-0.086***
	(0.03)
Above median unemployment rate	-0.088***
	(0.03)
Above median number of daycares	-0.081***
	(0.028)
Below median birth rate	-0.08**
	(0.03)
Below median home value	-0.08
	(0.04)
Below median PCI	-0.037
	(0.039)
Below median % College Grads	-0.005
	(0.004)

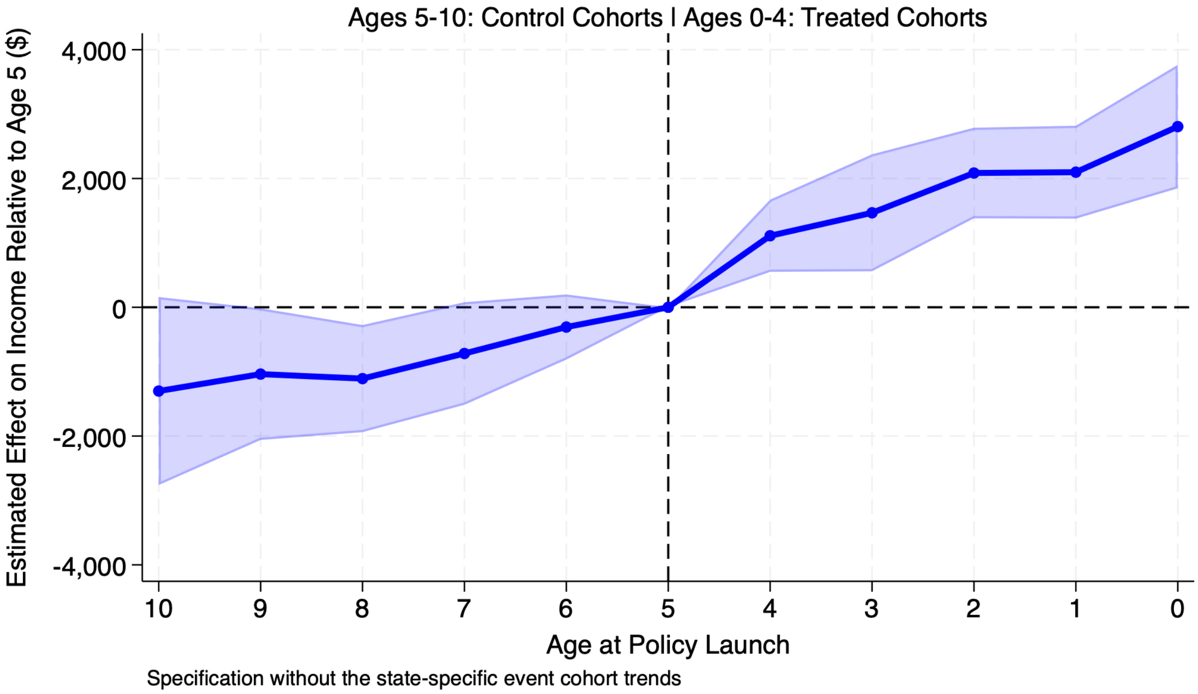
Notes: The dependent variable is the log number of daycare establishments. The table shows the effect on the number of childcare establishments by state-level sub-groups. Each row identifies the result of running the regression on the set of states that follow the description. Each regression controls for population of children under the age of 5. Data on the number of establishments are from QCEW. Data on state characteristics are from sources described in III. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Panel A: Maximum Child-to-Staff Ratio							
New Policy Level				**Change in Strictness (Fewer Infants)**			
3:1	4:1	5:1	6:1	1	2	3	6
243.69***	230.80	448.06**	-188.38	655.83*	413.63*	-302.93***	-285.90***
(10.30)	(386.02)	(147.48)	(140.08)	(285.43)	(194.50)	(53.28)	(55.98)
Panel B: Minimum Director Education							
New Policy Level				**Change in Strictness (Additional Years)**			
12	13	14	16	1	2		
-1,483.75**	913.92	204.59	30.22	286.87***	-557.99		
(521.89)	(571.05)	(197.32)	(257.32)	(30.31)	(708.12)		

Notes: The dependent variable is adult income in 2023 dollars. Panel A shows the results of estimating the effect of introducing and tightening child-to-staff ratio on adult income. Columns I-IV present the results of introducing the child-to-staff regulation at various levels in a state for the first time on income. Columns V-VIII report the effect on income of tightening existing regulations by reducing the number of infants allowed per caregiver. Panel B reports the results of estimating the effect of introducing and tightening directorial educational requirements on adult income. Columns I-IV show the impact on income of introducing a minimum on the years of educational attainment by daycare directors in a state for the first time. Columns V-VIII report present the results of increasing the minimum number of years of education required of directors in states with existing regulations on income. Data are from ACS 5 years 2019-2023. Robust standard errors clustered at the state of birth level are presented in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Appendix

Figure A.1: Alternative Specification: Effect on Annual Adult Income



Notes: The above figure shows the ITT effect on annual income for treated cohorts (ages four years and under), for specification without state-specific event cohort trends. X-axis represents the cohorts of children by their age at the time of policy. The 5 year olds at the time of policy are the baseline group. Y-axis shows the difference in adult income between treated and control groups relative to the 5 year old cohort in their respective states. A joint test of these pre-treatment coefficients fails to reject the null of zero effect (p = 0.143). Income data are from ACS 5-year 2019-2023 sample. All monetary values in 2023 dollars.

Table A.1: Heterogeneous Effects on Annual Income by Baseline Characteristics

Above Median Group	Interaction with Above Median Group
Total population	-458.2 (1020.75)
Per capita income	578.87 (680.58)
Employment rate of mother with kids <5 years	655.87 (720.5)
% Black	-423.4 (863.79)
Number of Daycare centers	-1,068.61 (924.22)
Number of Daycare centers per 1000 kids under 5	-405.39 (972.74)
% Non-white	-265.75 (741.36)
% College graduates	970.85 (835.72)
% urban	-22.62 (743.35)
Birth rate	-711.23 (724.49)
Fertility rate	209.97 (771.1)
Average AFDC per participant	-573.04 (703.33)
Median home value	1,189.02 (681.43)
Education expenditure per pupil	802.77 (664.84)
Unemployment rate	65.83 (696.98)
Presence of Public pre-k	3,978.61*** (430.16)
% Counties with Head Start	308.51 (770)
Democrat Governor	898.11 (761.85)

Notes: The table shows the coefficients on triple interaction term: Treated_state × Young_cohort × Above Median Baseline Characteristic. Outcome variable is annual income. Data are from various sources described in Section III. All dollar amounts in 2023 values. Birth rate is defined as births per 1,000 population. Fertility rate is defined as births per 1,000 women aged 15-44. Presence of public pre-k is based on NIEER's 2019 State of Preschool Yearbook. Only 3 states (1 control and 2 treated) had introduced public pre-K program as 1980. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.2: Heterogeneity Effects of Migration

Dependent Variable:	Income	Employment	Grad Degree
Treated_state × Young_cohort × Moved Out of Birth State	-997.25	-0.005	0.003
	(1445.13)	(0.004)	(0.003)

Notes: The results show heterogeneity analysis by likelihood of migrating out of birth state. The coefficient are from the triple interaction term–Treated State × Young Cohort × Moved Out of Birth State–on various adult outcomes. Data are from ACS 5-year 2019-2023 sample. Robust standard errors clustered at state of birth level are presented in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.3: Heterogeneity By Family Background, Race, and Gender: PSID

	Interaction with Group			
Outcome Variable	Below Median Family Income	Below Median Family Education	Non-White	Female
Earnings	-5,814	-4,356	550	-3,618
	(4486)	(4776)	(5496)	(5569)
Employment	-0.007	-0.03	0.02	0.02
	(0.04)	(0.03)	(0.05)	(0.045)
Yrs. of Education	0.02	0.34	0.1	-0.11
	(0.30)	(0.24)	(0.35)	(0.28)
Married	-0.04	-0.04	0.06	0.02
	(0.05)	(0.07)	(0.05)	(0.09)
Teenage Pregnancy	0.04	-0.18**	0.11	
	(0.07)	(0.08)	(0.11)	

Notes: The table presents stacked difference-in-difference estimates of daycare regulation on various adult outcomes using Panel Study of Income Dynamics, public use dataset (2025) and Pfeffer et al. (2021) data. Each cell reports the coefficient from a separate regression model for the triple interaction term: Treated State $\times$ Young Cohort $\times$ Group. The demographic group for the interaction is listed in the column header.Robust standard errors clustered at state of birth level are presented in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.4: The Effect of Regulation on Adult Outcomes, by Family Background: PSID

Sub-group	Earnings	Employment	Yrs. of Education	Married	Teenage Pregnancy
High Income	10,456	0.012	0.45	0.02	-0.086
	(5766)	(0.026)	(0.296)	(0.07)	(0.052)
Low Income	-1,199	-0.016	0.332	0.06	-0.008
	(4039)	(0.041)	(0.38)	(0.07)	(0.057)
High Education	13,314.6*	0.017	0.59	0.04	0.015
	(5214)	(0.029)	(0.32)	(0.06)	(0.03)
Low Education	-7,908*	-0.041	0.53	0.08	-0.075
	(3469)	(0.035)	(0.36)	(0.09)	(0.062)

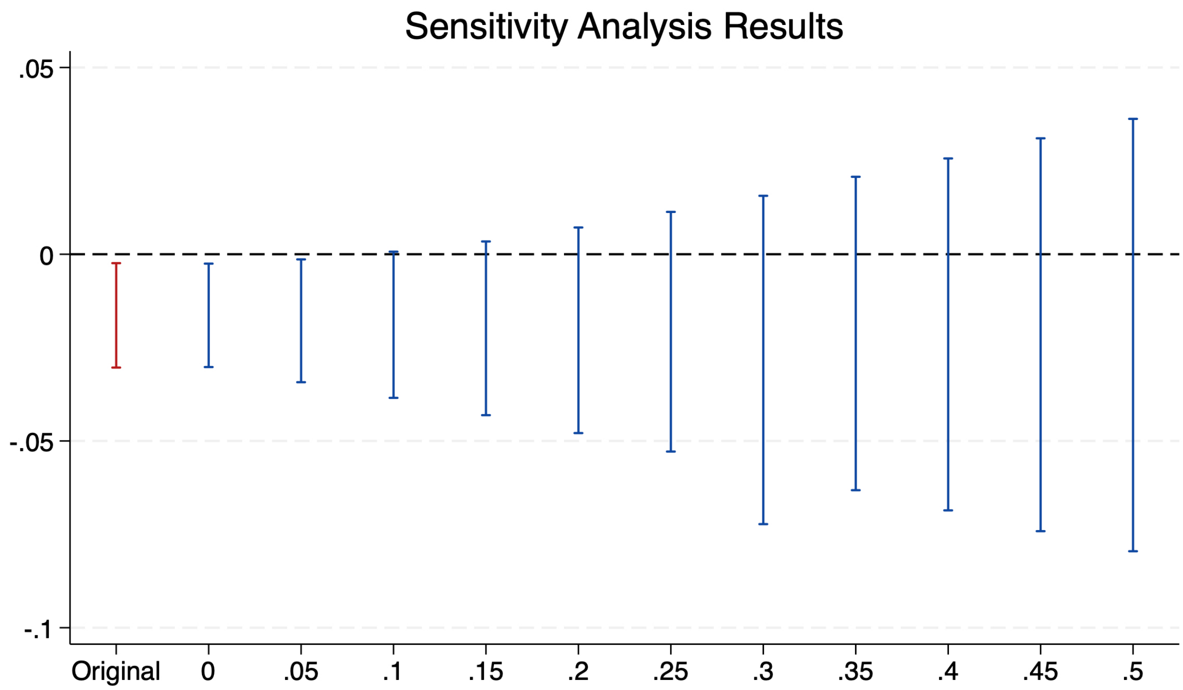
Notes: The table presents stacked difference-in-difference estimates of daycare regulation on various adult outcomes using Panel Study of Income Dynamics, public use dataset (2025) and Pfeffer et al. (2021) data. Dependent variables listed in the column header. Robust standard errors clustered at state of birth level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.5: The Effect of Regulation on Adult Outcomes, by Gender and Race: PSID

Sub-group	Earnings	Employment	Yrs. of Education	Married	Teenage Pregnancy
Female	7,223	-0.01	0.76	0.06	
	(6403)	(0.039)	(0.426)	(0.06)	
Male	2,028	-0.005	0.27	0.02	
	(8122)	(0.026)	(0.287)	(0.06)	
Non-white	-1,736	-0.047	0.34	0.08	0.017
	(3854)	(0.041)	(0.25)	(0.08)	(0.074)
White	6,310	0.015	0.52	0.02	-0.08
	(4613)	(0.024)	(0.26)	(0.06)	(0.058)

Notes: The table presents stacked difference-in-difference estimates of daycare regulation on various adult outcomes using Panel Study of Income Dynamics, public use dataset (2025) and Pfeffer et al. (2021) data. Dependent variables listed in the column header. Robust standard errors clustered at state of birth level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

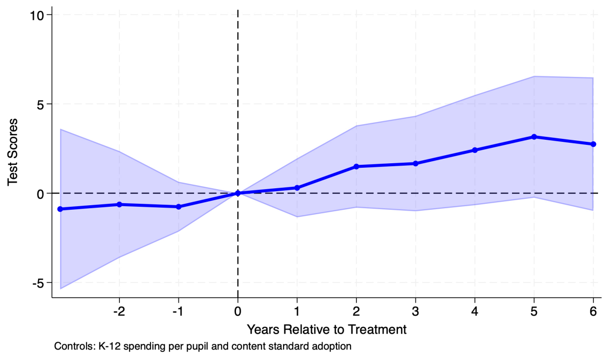
Figure A.2: Sensitivity Analysis Using HonestDiD



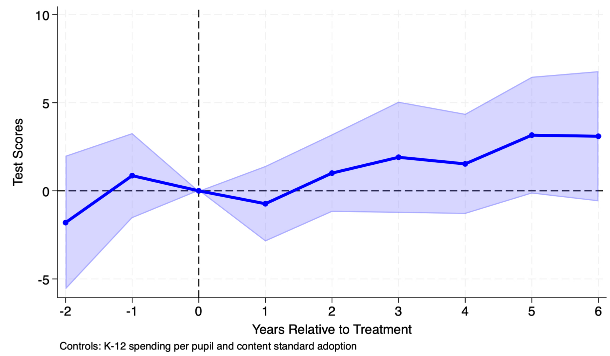
Notes: This figure presents the results from running sensitivity analysis from Honest DiD on the log number of childcare establishments. Establishment data are from QCEW 1980-2000 childcare industry.

Figure A.3: Effect on State-Level 8th Grade Test Scores

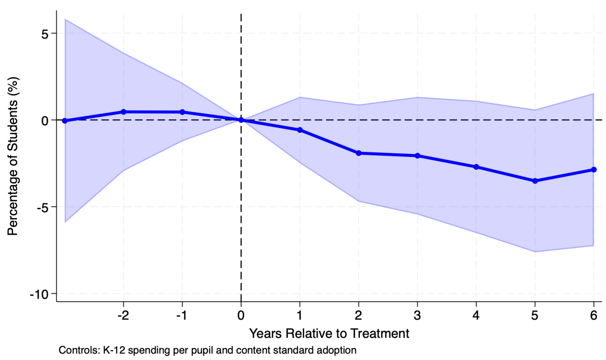
(a) Average Math Scores



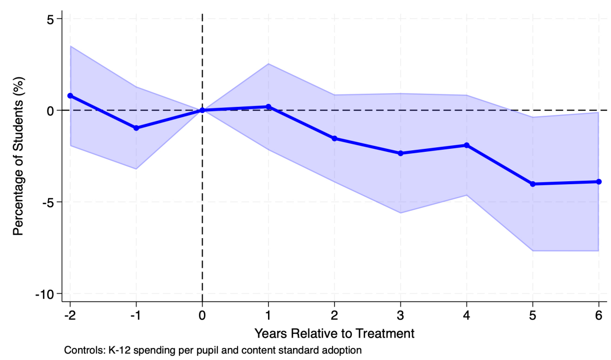
(b) Average Reading Scores



(c) Students Scoring Below Basic Level in Math



(d) Students Scoring Below Basic Level in Reading



Notes: Each figure shows the effect of regulations on state-level Math and Reading test score related outcome for 8th graders. Figure (a) shows the effects on the total years of education completed; figure (b) shows the effects on the rate of high school completion; figure (c) presents the effects on college graduation rates; and figure (d) shows the effects on graduate or professional degree completion. Data are from National Assessment of Educational Progress (NAEP) from 1990-2013.