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Universal Pre-K as Economic Stimulus: Evidence from Nine States and Large Cities in the U.S.

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Abstract

While Universal Pre-Kindergarten (UPK) is often proposed as an economic stimulus, its market effects remain uncertain. The researchers analyze UPK programs implemented across nine states and cities from 1995 to 2020, leveraging their staggered adoption for identification. UPK increased Pre-K enrollment and led to a 1.2% rise in labor force participation, a 1.5% increase in employment, and a 1.6% growth in hours worked, resulting in higher aggregate earnings. Employment effects were strongest for mothers but extended to other groups, primarily women. Impacts varied, with the largest effects observed in areas with high public Pre-K enrollment. Notably, each dollar spent on UPK generated between 3 to over 20 dollars in aggregate earnings—enough that tax revenues might fully cover costs.

1 Introduction

While Universal Pre-Kindergarten (UPK) can increase economic output in the long run by increasing worker skill (Gray-Lobe et al., 2022; Berne, 2022), it is also theorized to spur macroeconomic growth in the *short run* by increasing labor supply and enhancing productivity (Daniela Viana Costa, 2021; Belfield and Neveu, 2006). However, while UPK has been shown to increase parental labor supply in some settings (Cascio and Schanzenbach, 2013; Humphries et al., 2024a), it does not do so universally (Fitzpatrick, 2010), leaving uncertainty about how reliably large-scale UPK programs boost maternal labor supply.

There is also uncertainty about UPK's broader labor market impacts. While much of the existing literature focuses on mothers of young children, this may understate the full effects for several reasons. First, increased maternal employment may crowd out jobs for others (Fukui et al., 2023), limiting net gains. Second, since up to half of young children receive informal care—often from female friends and relatives—UPK could impact a wider group than just mothers (Early Edge California, 2024). Third, forward-looking behavior means UPK may influence the labor decisions of prospective parents. Finally, focusing solely on parental employment overlooks demand-side effects, such as increased consumption among families who would otherwise have paid out of pocket for childcare (e.g. Hoynes and Schanzenbach (2009)).

To address uncertainty around both the generalizability of UPK's effects and its broader market implications, we examine labor market impacts beyond directly affected mothers— highlighting a distinct yet important policy dimension. We also help reconcile mixed findings in the literature by formally testing for heterogeneity across settings and assessing whether more ambitious UPK programs yield larger benefits.

Over the past few decades, many states have introduced subsidized Pre-K programs, differing in schedule (part-day vs. full-day), income and age eligibility, and implementation pace. Spending per enrollee varies widely, from \$22,207 in D.C. (the most ambitious program) to \$3,142 in Florida in 2023. We define our sample of Universal Pre-K areas as those offering programs to all students regardless of income or family characteristics and serving at least 50% of four-year-olds as of 2019. To avoid COVID-19 disruptions, we focus on programs established before 2020 in nine states and major cities: Georgia, Oklahoma, West Virginia, Florida, Iowa, Wisconsin, D.C., Vermont, and New York City (Figure 1).[1]

Our analysis use several publicly-available datasets. To isolate the effect of UPK implementation from that of preexisting differences across areas, we compare *changes* in outcomes before and after UPK implementation to changes in areas without UPK. Using this variation, we apply a stacked difference-in-differences approach within an event-study framework. This approach identifies the impact of UPK implementation, assuming no simultaneous forces or policies influenced local markets. While we cannot fully rule this out, we (a) demonstrate similar pre-trends

[1] We exclude New Mexico and Alabama, which, despite universal eligibility, have low enrollment rates (32% and 3% in 2021). Our 50% enrollment threshold also omits California and New Jersey.

between UPK and non-UPK areas, and (b) show our results hold when controlling for strong labor market predictors, and also other policies that might affect aggregate labor market outcomes such as changes in the minimum wages, EITC, Welfare, and Food Stamps. Moreover, we show that (i) demographic groups unlikely to be affected by childcare availability exhibit no significant effects, and (ii) the most pronounced effects occur in areas with the greatest increases in public Pre-K enrollment.

On average, UPK implementation increased public Pre-K enrollment among four-year-olds by 10 percentage points over five years. Despite some crowd-out from private options, total Pre-K enrollment (both full time and part time) rose by 6.6 percentage points. Looking to overall economic activity, the introduction of UPK was linked to a 0.8 percentage point increase in overall labor force participation and a 0.9 percentage point rise in employment. We also find evidence that UPK reduced unemployment, consistent with improved job matching—i.e., expanded childcare access made it easier for job seekers to find or accept employment.

To explore mechanisms, we examine effects across demographic groups. Mothers of young children—often the primary caregivers in the absence of formal care—experience the largest impacts, though they account for less than half of the total. Even excluding this group, we find substantial aggregate effects. Employment increases across all margins for prime-age women, including those without young children, while effects for prime-age men are minimal. These patterns suggest two key mechanisms: informal caregivers—primarily women—are freed from childcare responsibilities, and prospective mothers increase labor market attachment in anticipation of future care access. Consistent with the first mechanism, time diary data show that UPK reduced child caregiving hours for women broadly—not just mothers.

To assess the extent of program-level heterogeneity, we examine how much variation in UPK effects across areas is unexplained by sampling variability. Our findings indicate substantial heterogeneity: while some UPK programs yield minimal employment gains, others increase employment by over 2 percentage points. This variation aligns with differences in estimated effects across existing studies, suggesting that disparate findings reflect genuine treatment heterogeneity rather than flaws in specific studies or differences in research design.

To show that these effects stem from UPK itself rather than other factors correlated with its introduction, we show that employment impacts are strongest in (a) areas with greater public Pre-K enrollment growth and (b) areas with both high Pre-K enrollment and high-quality programming. Moreover, we find that simply having a UPK program—without a significant enrollment increase—has no measurable impact.

To underscore the importance of quality and scale, we conduct an area-specific event study using Washington, D.C., which is home to the most ambitious UPK program in the U.S. Similar to existing literature highlighting the success of the program (Braga et al., 2024), we find substantial aggregate effects: a 1.7 percentage point increase in labor force participation, and a 1.7 percentage point rise in employment —significantly larger than effects observed in less ambitious UPK programs.

We find that UPK had negligible effects on average wages—suggesting highly inelastic labor demand—but led to sizable increases in aggregate income, highlighting meaningful economic benefits. Each dollar spent on public Pre-K generated between 3$andover$20 in additional earnings, enough to offset program costs through higher federal and state tax revenue.

2 Data

Enrollment Data

To analyze Pre-K enrollment trends in the United States, we rely on three complementary datasets: the American Community Survey (ACS), the Current Population Survey (CPS), and data from the National Institute for Early Education Research (NIEER) (U.S. Census Bureau, 2023; U.S. Census Bureau and U.S. Bureau of Labor Statistics, 2023; Friedman-Krauss et al., 2023).

The NIEER data (2002–2023) provides a comprehensive state-level analysis of publicly funded Pre-K programs. Compiled annually, it draws from state education agencies, budget documents, legislative reports, and other sources.[2] NIEER tracks the enrollment of of three- and four-year-olds in public Pre-K. It also tracks spending, reporting total and per-child public Pre-K expenditures (adjusted to 2023 dollars), and assesses adherence to quality benchmarks such as teacher qualifications, class sizes, and instructional time.

Figure 1a shows state-funded Pre-K enrollment in the NIEER data from 2002 to 2022. Over time, enrollment of four-year-olds in publicly-funded Pre-K has generally increased as more states and localities have funded Pre-K programs. Nationally, the average enrollment in publicly-funded Pre-K rose from 15% in 2002 to 37% in 2022. Not surprisingly, states with universal Pre-K (UPK) have the highest enrollment shares; indeed, the top eight states (excluding NYC) are all UPK states. However, many of these states initially had average enrollment levels in the early 2000s but saw substantial increases following UPK implementation.

To extend our analysis beyond publicly-funded Pre-K to overall Pre-K enrollment and to examine outcomes before 2002, we incorporate additional data sources. First, we use the CPS October Supplement, which covers 1976–2023 and tracks long-term Pre-K enrollment trends by surveying families about household members' educational attainment and enrollment. It provides individual-level data distinguishing between public and private Pre-K enrollment by age. However, with a sample of about 60,000 households, the October CPS affords limited precision, particularly for smaller geographic areas or subpopulations. This limitation motivates

[2] State Departments of Education, such as those in California and New York, supply key data on enrollment, funding, and policies. Specialized offices like Connecticut's Office of Early Childhood and Georgia's Department of Early Care and Learning (DECAL) offer further insights. NIEER also reviews budget documents and legislative reports to track funding, occasionally validating state-reported figures with federal data from NCES. These diverse sources enable NIEER to provide detailed, state-level data on enrollment trends, funding, and quality standards for public Pre-K programs.

our use of ACS data which surveys about 3.5 million households annually, offering a much larger dataset than the CPS and enabling more precise enrollment estimates at the state and city levels. However, ACS data only reliably cover 2005–2022, making the CPS the sole option for studying early UPK states.[3] Since the ACS includes the same enrollment variables as the CPS, for states that implemented UPK before 2007, we use CPS data, while for those implementing UPK after 2007 (including sub-state analyses), we use ACS.[4]

During our sample period (1990-2019), approximately 43% of four-year-olds were enrolled in public Pre-K, while total school enrollment (including private preschool) was approximately 67%. As expected, both public and overall Pre-K enrollment rates are higher in UPK areas, with overall enrollment at 69% compared to 66% in non-UPK areas. In Section 4, we show that much of this higher enrollment is driven by UPK implementation.

Labor Market Data

Our analysis uses labor market data from the CPS Annual Social and Economic Supplement (ASEC), (which surveys approximately 75,000 households in March) and the ACS. Together these provide detailed information on employment, labor force participation, and wages at both individual and household levels. We construct a labor market panel spanning UPK introductions from 1995 onward, using CPS data for pre-2005 outcomes and ACS data thereafter, given its larger sample size.[5]

We aggregate data into a state-by-year panel from 1990 to 2019, treating New York City (NYC) as a separate state. Within New York, we designate NYC as a UPK area while treating the rest of the state as a non-UPK area. For UPK introductions before 2007 (GA, OK, WV, FL, and IA), we rely solely on CPS data, ensuring each "stack" (as discussed below) uses CPS for all outcome years. For areas implementing UPK in 2008 or later, where ACS data provide pre- and post-implementation observations, we use ACS for all outcome years. Appendix Table A.1 presents summary statistics for treated and untreated areas.

UPK and non-UPK areas are similar. Among people over the age of 18, both treatment and control states have employment rates near 62% and labor force participation rates around 67%. The share of full-time workers was about 54%, with average weekly hours worked close to 27. Treated areas have slightly higher wages ($43,000 vs. $40,200) and a higher share of Black residents (18% vs. 12%), but overall, UPK and non-UPK areas resemble the U.S. as a whole.

Policy Controls

To account for policies that may affect our outcomes, we use state-level data on welfare benefits, poverty rates, governor political affiliation, EITC rates, minimum wages, and participation in SSI, Medicaid, and WIC—all from the University of Kentucky Center for Poverty

[3] ACS data exist from 2001 but were experimental until 2005.

[4] All analyses are robust to using only CPS data but gain precision with the larger ACS sample.

[5] Table A.2 shows that both data sources yield comparable estimates. Additional results using only ACS or CPS data are provided in the Appendix.

Research (UKCPR, 2025).

3 Empirical Strategy

Our analytic approach compares changes in outcomes before and after Universal Pre-K (UPK) implementation, using areas without UPK during the same period as a comparison group. To isolate UPK effects from the pandemic, we limit our analysis to data through 2019. This allows for a balanced panel with four years of post-implementation observation for all areas.

To avoid including areas already treated in the comparison group, we employ a stacked difference-in-differences model where each treated area is compared to all untreated areas. That is, for each treated area, we create a stack that includes the treated area along with all areas that remained untreated throughout the sample period.[6] This approach compares the average changes in outcomes in treated areas to those in untreated areas over the same time period. While control states lack defined UPK programs, many offer childcare subsidies, and NIEER data indicate some four-year-old enrollment in state childcare programs. Thus, our results reflect changes in UPK areas relative to potentially smaller shifts in comparison areas.

Each stack (one for each treated unit, g) represents a dataset. These group g datasets are then appended to create a single stacked dataset We then estimate models as below.

$$Y_{sgt} = \sum_{\tau=-4}^{4} \beta_\tau (UPK_s \times 1_\tau) + \gamma_s + \gamma_{t,g} + \varepsilon_{sgt} \tag{1}$$

Y_{sgt} is the outcome for area s, in treated group g, in year t, and UPK_s equals 1 if area s implemented UPK during the sample period. For treated areas, τ represents years relative to UPK implementation (event time), while for untreated areas, $\tau = -7$. The indicator 1_τ equals 1 for all UPK observations in year τ. In equation (1), subscript g denotes the stack g. To (a) account for differing time effects across areas and (b) ensure never-treated areas serve as the comparison group, we include stack-by-year fixed effects γ_{gt}. The area-level error term ε_{sgt} varies by stack for comparison areas. Standard errors are clustered at the area level. This is the stacked difference-in-differences model employed in Cengiz et al. (2019), Deshpande and Li (2019), and Jackson (2023).

The β_τ map out the evolution of outcomes in the UPK areas relative to the comparison areas before and after implementation of UPK. To estimate an average treatment effect, we compute the difference between the average effect for the four years before the implementation of UPK (i.e., $t-4$ through $t-1$), and that for the four years after (i.e., $t=0$ through $t+4$).

The event study framework accounts for a range of potentially confounding factors with state and year fixed effects, which control for time-invariant differences across states as well as common nationwide shocks. However, to further account for regional differences, our preferred

[6] We also drop Alabama and New Mexico from the control group, as these states are often considered to have UPK programs (by other definitions) although they do not meet our standard for classification. All results are robust to their inclusion.

specification also includes region-by-year fixed effects–capturing shared regional shocks like economic fluctuations or policy changes that broader time controls may miss.[7] Additionally, we estimate all models with a full set of policy controls to account for the potential effects of potentially coincident policies.

4 Effect on Pre-K Enrollment

Figure 2 presents event-study plots showing how outcomes changed in areas that implemented UPK compared to those that did not, spanning four years before and after implementation. The figure also reports precision-weighted averages for the pre- and post-treatment periods, along with their confidence intervals. The reported pre-post change and its standard error reflect the difference between these two weighted averages.

Figure 2, panel (a), presents an event study of public Pre-K enrollment for four-year-olds using CPS-ACS data, covering all UPK states and including the full set of controls. While pre-UPK data are somewhat noisy, enrollment increases by about 10 percentage points after UPK implementation (p-value < 0.01), with no evidence of differential pre-trends and a clear jump at implementation. These results are consistent with estimates from NIEER data (Appendix Figure A.1), available from 2002 onward, and summarized in Table 1, column (8), which show a 14.5 percentage point increase in publicly funded Pre-K enrollment (p-value < 0.01).[8] Both datasets show a significant increase in public (or publicly funded) Pre-K enrollment following UPK adoption. As shown in Table 1, Panel A, these estimates are robust across models, data sources, and specifications, including those with region-by-year and welfare controls.

Since many children may shift from private to public Pre-K once it becomes available—as shown in Boston (Weiland et al., 2020) and New Haven (Humphries et al., 2024b)—we also examine effects on the share of four-year-olds enrolled in any school. Using CPS/ACS data, Table 1 and Figure 2 show that overall enrollment rises by 6.6 percentage points following UPK implementation (p-value < 0.01). To better understand enrollment patterns, Table 1 also shows that UPK increases part-time Pre-K enrollment by 3.6 percentage points (p-value = 0.23) and full-time enrollment by 3.7 percentage points (p-value = 0.10).

Importantly, we show that the patterns of increased public Pre-K and overall enrollment are robust across data sources and hold in models both with and without controls (see Table 1).

[7] We also estimate models using a Bartik-style shift-share control, which combines 1990 state-level industry shares with national industry growth rates in employment and labor force participation to predict labor market shifts by state and year. Table A.5 shows results with both region-by-year fixed effects and Bartik controls. Given the similar results, we prefer the more parsimonious region-by-year specification.

[8] Restricting the CPS/ACS sample to years covered by NIEER yields a 12.0 percentage point increase—closely aligning with the NIEER estimate. Note that NIEER defines "publicly funded" Pre-K more broadly, including state-subsidized private centers and voucher-supported programs (e.g., D.C.'s partnerships with community-based providers (DC, 2025)), which differs slightly from the CPS/ACS definition.

5 UPK Effects on Aggregate Labor Market Outcomes

Looking to aggregate labor market outcomes, Figure 2 presents event study effect of UPK, showing compelling evidence of increases in labor market participation and employment overall. Panel (c) shows a 0.8 percentage point increase in labor force participation in the full population ($p-value < 0.01$) — a relative increase of about 1.2 percent. Panel (d) plots the effects for the aggregate employment rate showing an increase of about 0.9 percentage points ($p-value < 0.01$)— a relative increase of about 1.5 percent. Importantly, there is no evidence of any differential pre-trending in aggregate outcomes for areas that introduced UPK.

The employment rate increase slightly exceeds the rise in labor force participation, which may seem counterintuitive but can be explained by shifts from unemployment to employment. Employment gains come from two sources: those entering from outside the labor force ($N \Rightarrow E$) and those moving from unemployment ($U \Rightarrow E$). Indeed, Figure A.2 shows a reduction unemployment after the implementation of UPK – indicating that both flows contribute to the larger employment effect relative to labor force participation. This pattern may reflect a reduction in job search costs for caregivers following UPK implementation. Caregivers often face both lower incentives and higher costs to search for work—barriers that are especially pronounced in high-cost childcare settings (Novoa and Workman, 2021). Consistent with this, Philippe and Skandalis (2023) finds that mothers submit fewer job applications and are more selective about wages and amenities, with application rates falling by over 20% when children are not in school.

We also find strong evidence of intensive margin effects. Panel (e) of Figure 2 shows that UPK increases average weekly hours worked by about 0.42 hours, and Table 1 reports a 1.1 percentage point increase in full-time employment. This increase exceeds that in overall employment, suggesting that some new entrants took full-time jobs, and that some existing workers shifted from part-time to full-time work—highlighting UPK's impact on labor flows.

To summarize these labor market effects and account for false discovery, following Kling et al. (2007), we create a labor index with is the average of the standardized outcomes. As expected, given that the effect on each component for the adult population is significant, the labor index effect is also significant, with a coefficient of 0.031 and a p-value below 0.001.

5.1 Effects by Subgroup: Evidence on Mechanisms

While one expects UPK to directly affect mothers of young children, its influence likely extends to other adults through three channels: (a) relieving informal caregivers (caregiver effect), (b) increasing labor market attachment in anticipation of future access (anticipatory effect), and (c) boosting disposable income among families which stimulates demand (demand-side effect). To test these mechanisms, we exploit differences across demographic groups more or less likely to experience these effects.

To anchor our analysis, we begin with prime-age men (ages 18 to 64). This group is unlikely to experience significant caregiving effects, as such responsibilities typically fall to women

(Susman-Stillman and Banghart, 2008), and younger men are less likely than older men (e.g., grandfathers) to serve as informal caregivers (tak Joo et al., 2024). They are also less affected by anticipatory effects, as men's labor market decisions tend to be less responsive to childcare availability (Schmitz, 2020; Houter and Barry, 2024). Because all groups—including younger men—may be exposed to demand-side effects, we use prime-age men as a baseline to help isolate these broader economic impacts from caregiving and anticipatory channels.

To analyze prime-age men, we re-estimate our main models using data for this group alone. Panel B, Column (6) of Table 1 shows that UPK has small, statistically insignificant effects on their labor force participation, employment, and hours worked, with a modest 1 percentage point increase in full-time status ($p < 0.05$). The labor market index yields an estimated effect of 0.015 ($p = 0.096$), and we do not reject the null of no effect. These findings suggest (a) limited demand-side effects and (b) that aggregate labor market impacts operate primarily through women—highlighting the likely importance of caregiving and anticipatory channels.

To isolate caregiving and anticipatory effects, we estimate a triple-differences model comparing UPK's impact across demographic groups relative to prime-age men, as shown in Figure 3.[9] Double-difference results for each group are also presented in Table 1. We begin with mothers of young children (under age six), who are most directly affected by UPK. Relative to prime-age men, they experience a 1.1 percentage point increase in labor force participation, a 1.4 percentage point increase in employment, a 1.2 percentage point rise in full-time status, and an increase of 0.47 hours in weekly work. All differential effects have p-values less than 0.01. These results align with *ex ante* expectations and closely match prior estimates based on age-based Pre-K eligibility among mothers of young children (Ilin et al., 2021).

Mothers of young children represent only 6.0% of the adult population, so their effects cannot account for the full aggregate effects. To assess this formally, we estimate UPK's impact *excluding* this group. As shown in Table 1, column (4), labor force participation increases by 0.3 percentage points (not significant), employment rises by 0.4 percentage points ($p < 0.05$), full-time status increases by 0.9 percentage points ($p < 0.01$), and weekly hours rise by 0.29 ($p < 0.01$). Summarizing these outcomes, the labor index excluding mothers of young children increases by 0.020 ($p = 0.0043$), down from 0.031 in the full sample. This suggests that mothers of young children account for roughly one-third of the total effect. Figure 3 shows the corresponding event-study plot, with post-UPK increases and no differential pre-trends—supporting causal effects beyond this subgroup.

We also examine effects for prime-age women, who may be influenced by two overlapping mechanisms: (a) the informal childcare channel, as caregiving is often provided by female relatives, friends, and neighbors, and (b) anticipatory effects related to expected future access to childcare. While we cannot fully disentangle these mechanisms, both would lead to stronger

[9] We implement this using a stacked dataset, estimating a single regression with all variables interacted with demographic group indicators. Reported coefficients reflect the interaction between group indicators and the pre-post difference.

effects for prime-age women than for prime-age men. To isolate these channels from the direct impact on mothers, we focus on prime-age women who are not mothers of young children. Compared to prime-age men, this group shows little differential change in labor force participation. However, employment increases by 0.5 percentage points ($p > 0.05$), full-time status increases by 0.9 percentage points ($p < 0.01$), and hours worked increases by 0.28 hours per week ($p < 0.01$). The labor index rejects the null of no differential effect relative to prime-age men at the 1% level, indicating that UPK had meaningful labor market effects for this group.

To test the caregiver mechanism directly, we use the 2003–2018 American Time Use Survey (BLS, 2019), which provides time-use data for a subsample of CPS respondents. Results are shown in Figure 3. If increased employment among non-mothers reflects a shift from caregiving to formal labor market work, we should observe a corresponding decline in time spent on childcare. In a triple-difference model relative to prime-age men, UPK reduces time spent with children by 4.51 hours per week for mothers of young children ($p = 0.018$) and by 1.54 hours for women without young children ($p < 0.01$).[10] Both effects are large enough to explain all of the observed relative increases in work hours for these groups, indicating that the caregiver mechanism was active for many women—including those without young children.

The results confirm that UPK improves labor market outcomes for mothers of young children, consistent with prior research. Importantly, we also find that UPK increases labor market attachment among *other* women. We hypothesize that these effects operate through a combination of caregiving and anticipatory channels, and provide time-use evidence consistent with the caregiver channel. While we do not reject the null of no effect for prime-age men, the estimates are consistent with small effects for this group.

6 Exploring Heterogeneity

As noted by Fitzpatrick (2010) and Cascio (2021), UPK effects vary. To measure and quantify these differences, we exploit the stacked data structure to estimate state- and city-specific effects on labor market participation and employment – comparing each UPK area to all non-UPK areas. This yields a UPK effect and a standard error for each UPK location.[11] Because estimated effect across locations can vary due to sampling variability, we use the DerSimonian and Laird (1986) method to adjust the overall variability in estimated effects for sampling variability. We estimate the standard deviation of true effects at around 1.1 percentage point for both outcomes. Given an average effect on employment of 0.9 percentage points, this indicates that about two-thirds of true effects will range between 0 and 2 percentage points.

To help explain this heterogeneity, we examine whether UPK's labor market effects vary with enrollment levels. We estimate state-level UPK effects on public Pre-K enrollment and labor market outcomes, then assess their bivariate relationship. The bivariate plots are presented

[10]Because the ATUS sample is too small to identify cities, we exclude New York State from the time-use analysis. See Appendix table A.3 for regression estimates under different models.

[11]Appendix Figure A.3 presents these estimates with 95% confidence intervals.

in the top panel of Figure 4. Each data point presents the UPK effect on the outcomes for a given state or city, along with the 95 percent confidence interval of that estimate.

Before examining the relationship with enrollment, the estimates on labor force participation (panel (a)) and employment (panel (b)), and their standard errors, provide useful insights. There are significant gains in labor force participation and employment in Washington, D.C., Florida, and New York City, with all having positive point estimates for both outcomes. In contrast, Vermont and Georgia show declines, though these are not statistically significant, as indicated by confidence intervals that include zero. The lack of overlap in confidence intervals across areas (e.g., D.C. and Vermont) reinforce the existence of true treatment heterogeneity beyond sampling variability. Additionally, our state-level estimates align with past research, showing weak or null effects in Georgia (Fitzpatrick, 2010) and positive effects in recent technical reports on Washington, D.C. (Malik, 2018) and New York City (Marifian, 2021). This consistency suggests that mixed findings in the literature may reflect true treatment heterogeneity rather than methodological differences or bias. We now turn to the relationship between UPK's effect on Pre-K enrollment and those on employment outcomes.

While enrollment estimates are noisy, potentially attenuating the estimated relationships, the patterns are informative. The top panel of Figure 4 plots UPK effects on labor force participation and employment against effects on public Pre-K enrollment (measured in the ACS and CPS). The relationship is positive but imprecisely estimated, likely due to sample limitations and measurement noise (see Section 4). Interpreted directly, a 10-percentage-point increase in public Pre-K enrollment among four-year-olds corresponds to a 0.23 percentage point increase in labor force participation and a 0.4 percentage point increase in employment.

To avoid noise in state-level enrollment effects from ACS/CPS data, one could use NIEER data, but its coverage is limited to states after 2002. To balance coverage and precision, we correlate UPK labor market effects with NIEER's 2023 access rating, which ranks areas by the percentage of four-year-olds in public Pre-K.[12] Panels (c) and (d) shows a clear linear relationship— areas with higher public Pre-K enrollment saw stronger UPK impacts on labor force participation and employment. We reject the null hypothesis of zero effects at the 5% level– suggesting that UPK-driven employment gains stem from increased public Pre-K enrollment.

The Exemplar Case of D.C.

UPK's impact depends on key implementation details, including whether programs are full- or half-day, delivered through public schools, and paired with aftercare. Washington, D.C.'s UPK program stands out on all fronts, allocating $22,207 per child in 2023—more than twice that of any other state.[13] D.C. offers full-day Pre-K for all three- and four-year-olds, unlike many states that restrict access to four-year-olds (e.g., Florida, Georgia) or provide only half-

[12] NYC's NIEER rating is imputed from New York State data (see Table 1b).

[13] The next highest, New Jersey, spends about $16,000 per child but lacks a UPK program. Other high-spending states (e.g., Oregon, California, Arkansas, Washington) enroll fewer than 25% of four-year-olds in Pre-K.

day programs. Even in New York, where NYC guarantees full-day UPK, availability is limited elsewhere in the state. Full-day schooling is crucial, as shown by Humphries et al. (2024b) and Gibbs et al. (2024), who find that full-day kindergarten expansions increased parental labor supply and reduced commuting time. D.C.'s program also uses a mixed-delivery model—including public schools, charter schools, and community-based organizations—enhancing flexibility and access. It is additionally recognized for its high quality, ranking first among all states and territories on observed classroom and center quality measures (NIEER, 2024).

While Figure 4 shows large labor market effects in D.C., it is important to confirm that these reflect causal impacts rather than differential pre-trends. To assess this, the lower panel of Figure 4 presents an event study for labor market outcomes in D.C., showing effect sizes nearly double those in the full sample. Panel (a) reports a 1.7 percentage point increase in labor force participation following UPK implementation ($p < 0.01$), and Panel (b) shows a nearly identical employment effect. Crucially, as in the full sample, we find no evidence of differential pre-trends in D.C. relative to comparison areas, reinforcing that the observed heterogeneity is real. These strong labor supply effects underscore the importance of accessible, high-quality UPK programs. While D.C. plays a prominent role in the analysis, it does not drive the overall results—Appendix Table A.4 shows that excluding D.C. yields similar UPK effects.

6.1 Potential Confounding

Without random implementation of UPK across locations (which is unrealistic), one concern is that the observed effects may stem from broader labor market trends coinciding with UPK implementation. We address this in several ways. First, we incorporate a Bartik predictor as well as region-by-year fixed effects to account for labor market shocks. Second, we control for several policies that could influence employment outcomes, including SNAP/TANF benefits, the poverty rate, a Democratic governor indicator, EITC receipt, the state minimum wage, SSI, Medicaid, and WIC receipt. Note that our results are similar with and without these controls.

Also, the absence of meaningful effects for prime-age men—who are unlikely to be directly impacted by UPK—supports the argument that UPK was not introduced alongside broader employment-boosting policies or economic shifts. Additionally, the bivariate relationship between UPK effects on enrollment and labor market outcomes (Section 6) indicates that the labor market effects stem from Pre-K enrollment rather than other coincident factors. That is, the linear relationship predicts near-zero labor market effects when there is no enrollment change, reinforcing the notion that the labor market impacts are driven by Pre-K availability rather than unrelated events coinciding with UPK expansion. While no single test is dispositive in isolation, taken together, the results support a causal interpretation.

7 Effects on Aggregate Earnings

To assess market-wide effects, we analyze aggregate wages using CPS and ACS data. Because aggregate earnings reflect overall economic output and can be influenced by population growth, we control for the log of population.[14] We begin by examining average wages and find no effect, suggesting highly inelastic labor demand.[15] So far, results show that UPK increased employment by about 1.5% and hours worked by 1.6%. Holding wages constant, the confidence interval for hours worked implies expected aggregate wage effects between around 1% and 4%. Consistent with this range, Figure 2 shows that UPK increased total real wages by 5.5%, with a 95% confidence interval ranging from 0.8% to 10%. While the data are consistent with a relatively wide range of positive effects, the evidence shows that UPK functions as a supply-side stimulus, boosting overall economic activity.

The pre-treatment average of aggregate wages was $45 billion, so the 95% confidence interval implies an increase of $315 million to $4.5 billion in additional earnings. For comparison, we estimate that state spending on Pre-K increased by $78–$99 million following UPK implementation (see Table 1). This suggests that each dollar spent on Pre-K generated between $3 and over $20 in additional earnings—consistent with Humphries et al. (2024b), who find $10 in added parental earnings per dollar spent on UPK in New Haven.

With an average income tax rate of 20%, each dollar spent generates between $0.60 and over $4.00 in tax revenue—suggesting that, *on the margin*, UPK programs could be self-sustaining even in the short run. However, because much of this revenue accrues to the federal government, states may not fully recoup their costs, creating a fiscal externality. This highlights the efficiency of federal subsidies for state UPK programs, compared to relying solely on state or local funding (Agrawal et al., 2022).

8 Conclusions

This paper shows that UPK programs have broad economic effects beyond their direct impact on parents of preschool-aged children—an important and policy-relevant finding. We show that UPK increases labor supply on both the extensive and intensive margins, raising employment across a wide range of workers and boosting aggregate earnings.

We also document substantial heterogeneity in effects across states, helping to reconcile prior mixed findings. Our analysis suggests that program scale and quality matter: more ambitious models—such as Washington, D.C.'s—yield significantly greater economic benefits.

To summarize the overall impact, we estimate that each dollar spent on UPK generates much more than one dollar in additional earnings, implying a high short-run benefit-cost ratio. Taken together, our results suggest that high-quality UPK is not only an investment in children's development, but also a potent supply-side stimulus.

[14] UPK states may experience faster population growth post-implementation.

[15] The estimated UPK effect on hourly wages is 0.0005 (se = 0.0007), a precisely estimated null.

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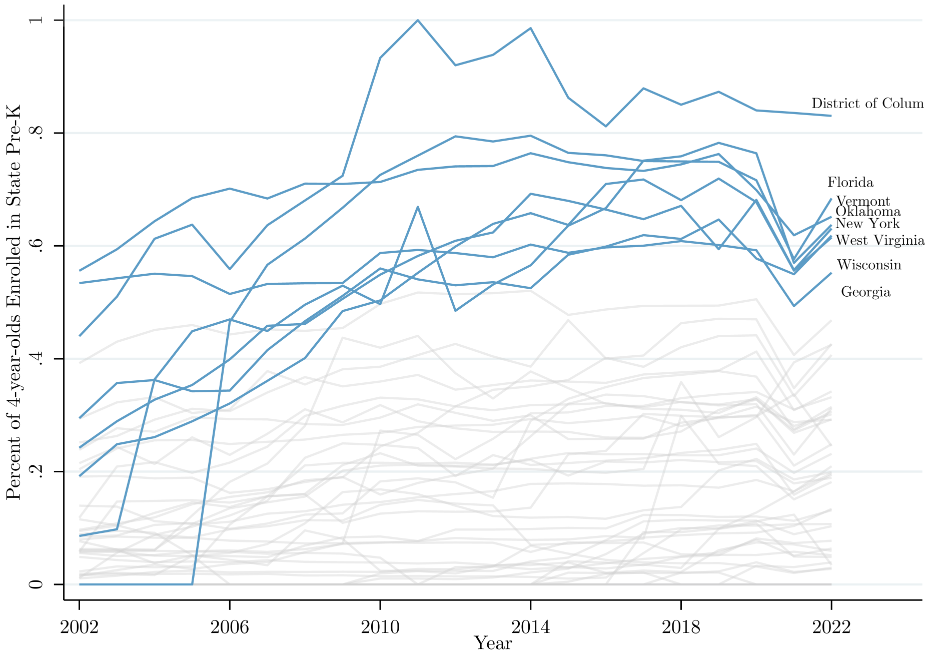
Figures and Tables

Figure 1: State Pre-K Programs and Enrollment Trends



(a) Enrollment in State Pre-K By State (2002-2022)

State/City	% 4y/o Enrolled	% Districts Offering Pre-K	State Spending Per Enrolled	NIEER Ranking	Year Implemented
Georgia	56	100	$5,646	9	1995
Oklahoma	67	100	$4,798	3	1998
West Virginia	67	100	$7,053	4	2002
Florida	67	100	$3,142	2	2005
Iowa	67	98	$3,705	5	2007
Wisconsin	63	99	$3,831	7	2008
District of Columbia	88	100	$22,207	1	2009
Vermont	71	100	$8,244	6	2014
New York City*	67	100	$6,101	3.5	2014

(b) Details of States and Cities Pre-K Programs (as of 2023)

Notes: **Panel (a)**: Share of four-year-olds enrolled in state-sponsored Pre-K from 2002 to 2022. Highlighted states are in the treatment group; others are in gray. Data from NIEER. "New York" refers to New York City.
Panel (b): Details of state and city Pre-K programs as of 2023. Implementation year refers to when the policy took effect or was funded. NIEER rankings reflect four-year-old access in 2022-23.
Key State Notes: - Georgia: Expanded in 1995 (NIEER, pg. 58). - Oklahoma: Statewide funding in 1998 (NIEER, pg. 124). - West Virginia: Rollout began in 2002 (NIEER, pg. 158). - Wisconsin: Incentivized district participation via 4K startup grants (2008). - District of Columbia: Pre-K Enhancement Act implemented in 2009. - New York City: Expanded via SUFDPK in 2014 (NIEER, pg. 116).
*** NYC Data**: NYC enrollment is imputed from state data. In 2023, NYC accounted for 50% of state Pre-K enrollment and 40% of the population (ACS). Its enrollment rate is estimated as 1.2x the state rate, with NIEER ranking assigned based on similar states.

Table 1: Effects of UPK Introduction on Enrollment and Labor Market Outcomes

Panel A. Pre-K Enrollment Outcomes

	ACS+CPS		CPS only		ACS only		NIEER	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enrollment in Public Pre-K	0.093	0.101	0.130	0.120	0.054	0.055	0.160	0.145
	(0.019)	(0.017)	(0.014)	(0.012)	(0.027)	(0.011)	(0.037)	(0.031)
Enrollment in Private Pre-K	-0.036	-0.039	-0.059	-0.070	-0.017	-0.003	--	--
	(0.025)	(0.020)	(0.026)	(0.028)	(0.015)	(0.016)	--	--
Enrollment in Any Pre-K	0.059	0.066	0.071	0.047	0.041	0.040	--	--
	(0.029)	(0.030)	(0.034)	(0.034)	(0.016)	(0.022)	--	--
Part-Time Enrollment in Pre-K	--	--	0.047	0.036	--	--	--	--
	--	--	(0.031)	(0.030)	--	--	--	--
Full-Time Enrollment in Pre-K	--	--	0.033	0.037	--	--	--	--
	--	--	(0.014)	(0.023)	--	--	--	--
Public Pre-K Spending ($M)	--	--	--	--	--	--	99.4	77.9
(NIEER)	--	--	--	--	--	--	(21)	(19)
Controls Included		X		X		X		X

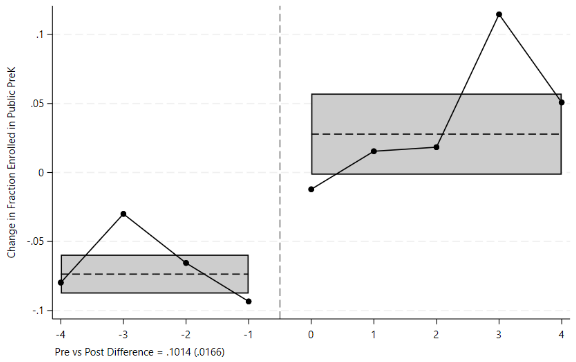
Panel B. Labor Market Outcomes

	All Individuals		Excluding Moms of Young Kids		Prime-Age Men		Young Moms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Labor Force Participation	0.006	0.008	0.004	0.003	-0.004	-0.001	0.020	0.026
	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.009)	(0.010)
Employment	0.009	0.009	0.004	0.004	0.003	0.003	0.021	0.029
	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.006)	(0.009)
Full-time Status	0.012	0.011	0.009	0.009	0.008	0.010	0.014	0.015
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.008)	(0.007)
Hours Worked	0.422	0.419	0.256	0.290	0.199	0.239	0.633	0.664
	(0.090)	(0.093)	(0.096)	(0.106)	(0.167)	(0.178)	(0.380)	(0.358)
Labor Index	0.030	0.031	0.017	0.020	0.012	0.015	0.059	0.076
	(0.005)	(0.006)	(0.005)	(0.007)	(0.008)	(0.009)	(0.027)	(0.027)
Log Total Wages	0.078	0.055	--	--	--	--	--	--
	(0.023)	(0.024)	--	--	--	--	--	--
Controls Included		X		X		X		X

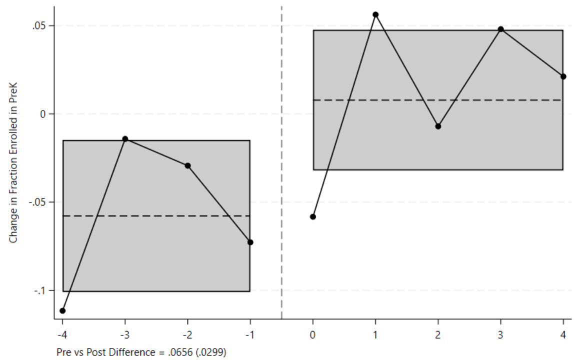
Notes: Pre-K enrollment differences compare the four years before UPK implementation to the year of implementation plus four years after. Public Pre-K spending values are in millions of 2023 dollars. Labor market estimates are from pre-post event studies. ”Not Mothers of Young Kids” includes all individuals except women aged 18–65 with a child under 5. UPK introductions before 2008 (GA, OK, WV, FL, IA) use CPS data; later stacks (WI, DC, VT, NYC) use ACS data. Each stack includes one treatment group and 42 control groups (treated states are left out of control group). Across 9 stacks, 387 state-by-year level observations are used in the analysis. Specifications with controls include region by year fixed effects along with the full set of policy controls described in Section 2. Log total wages results include control for log state population.

Figure 2: Effect of UPK Implementation on Enrollment and Employment Outcomes

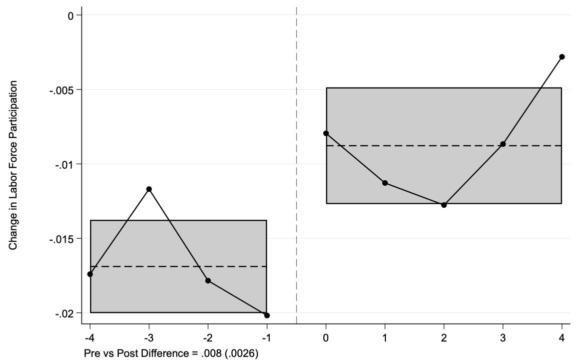
(a) ACS-CPS Data (Public Pre-K)



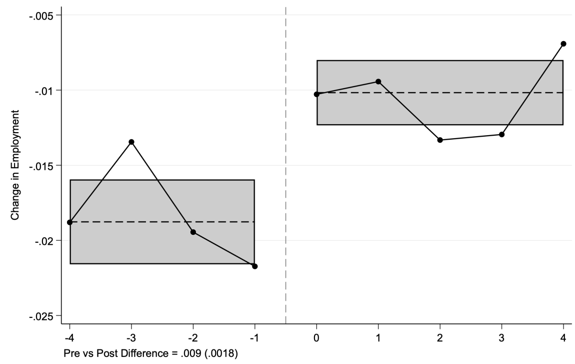
(b) ACS-CPS Data (Any Pre-K: Public or Private)



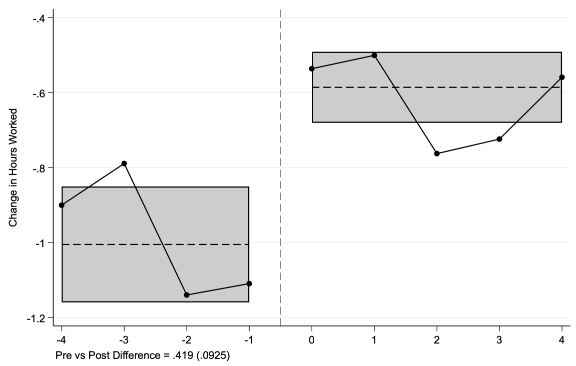
(c) Labor Force Participation Rate



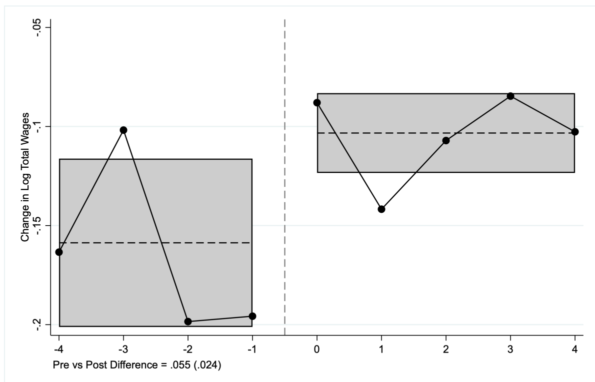
(d) Employment Rate



(e) Hours Worked



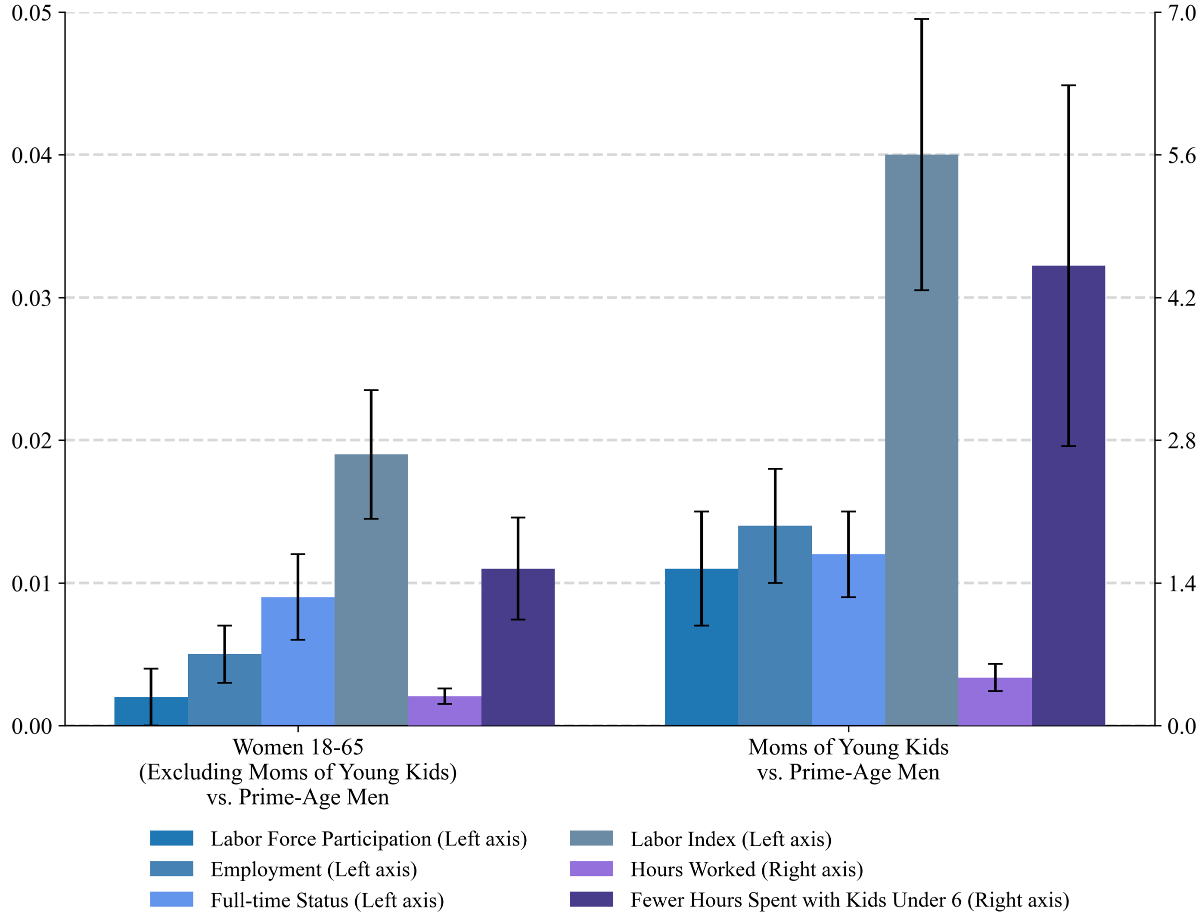
(f) Aggregate Earnings



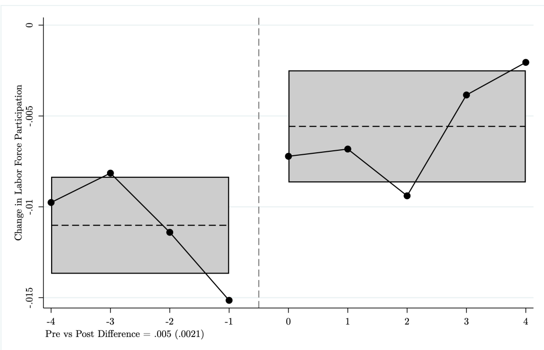
Notes: Panels (a) and (b) show the effect of UPK introduction on Pre-K enrollment on ACS-CPS data. Panel (a) shows publicly funded Pre-K; panel (b) shows total Pre-K enrollment (public and private). Panels (c)–(e) show the effect of UPK on employment outcomes: (c) labor force participation, (d) employment, and (e) average hours worked. Panel (f) shows the effect of UPK introduction on log total real wages across all UPK states, estimated using ACS/CPS data. All panels include region-year controls as well as policy controls discussed in Section 2. Panel (f) also includes a control for state-level log population. UPK introductions before 2008 (GA, OK, WV, FL, and IA) use CPS data. Later introductions (WI, DC, VT, NYC) use ACS data. Each stack includes one treatment group and 42 control groups (treated states are left out of control group). Across 9 stacks, 387 state-by-year level observations are used in the analysis.

Figure 3: Effects by Group

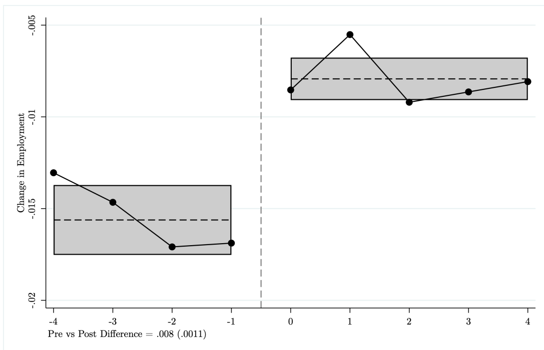
(a) Effects by Group: Relative to Prime Age Men



(b) Labor Force Participation Rate: Excluding Mothers of Young Kids



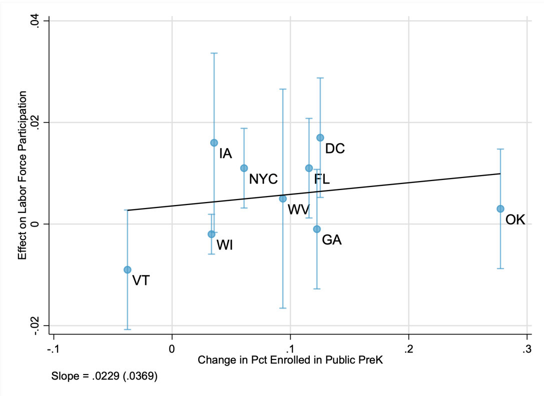
(c) Employment Rate: Excluding Mothers of Young Kids



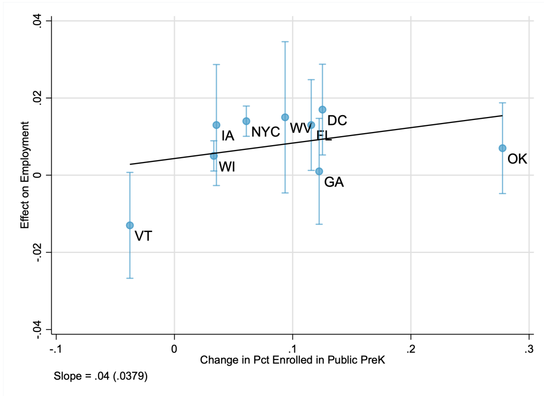
Note: Panel (a) shows point estimates from triple difference specifications that compare the specified treatment group to prime age (18-64) men in treated versus untreated states before and after UPK introduction. Model includes region-by-year fixed effects along with policy controls discussed in Section 2. Effects on hours worked and time spent with kids under age 6 are shown on the right axis of this figure. Results for time spent are measured in hours (sign reversed); the specification is described in greater detail in the Appendix. Panels (b) and (c) show the effect of UPK introduction on labor force participation and employment, respectively, for individuals over 18 who are *not* mothers of young children. UPK introductions before 2008 (GA, OK, WV, FL, and IA) use CPS data, while later introductions (WI, DC, VT, and NYC) rely on ACS data.

Figure 4: Effect of UPK Introduction on Employment and Labor Market Outcomes

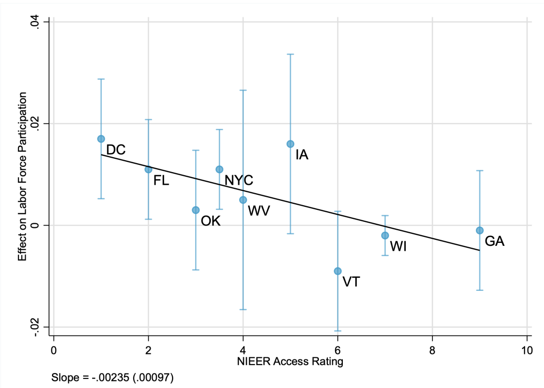
(a) Labor Force Participation Rate



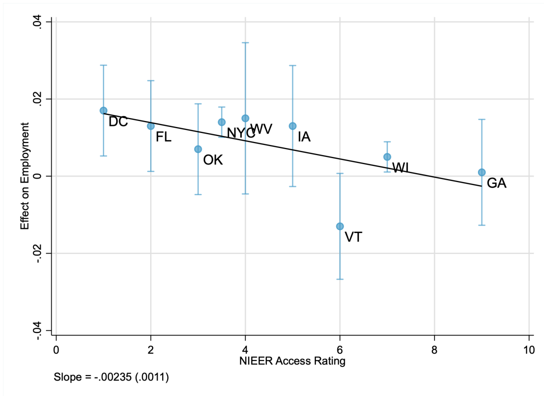
(b) Employment Rate



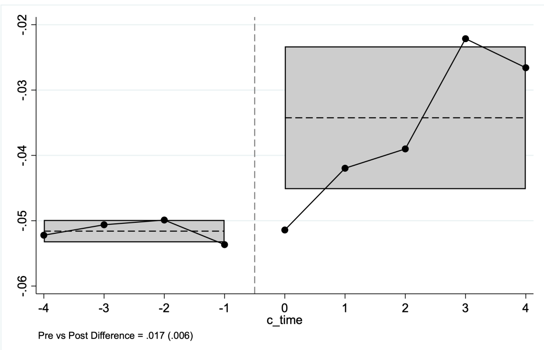
(c) Labor Force Participation Rate (NIEER Ranking)



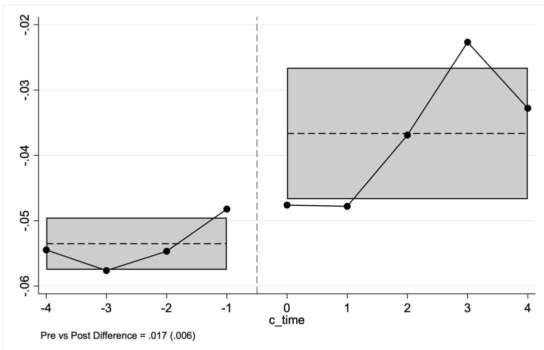
(d) Employment Rate (NIEER Ranking)



(e) Labor Force Participation Rate (DC)



(f) Employment Rate (DC)



Note: Each panel of the figure shows labor market outcomes before and after UPK introduction plotted against Pre-K enrollment before and after UPK introduction (panels a and b) and NIEER rankings of UPK program quality (in 2022). Labor market outcomes and public Pre-K enrollment use ACS and CPS data. **Panels (e) and (f):** The first row presents the estimated effects of UPK introduction on labor force participation and employment rates in Washington, DC, for individuals aged 18 and over using ACS data. Specifications include region by year fixed effects along with policy controls discussed in Section 2.

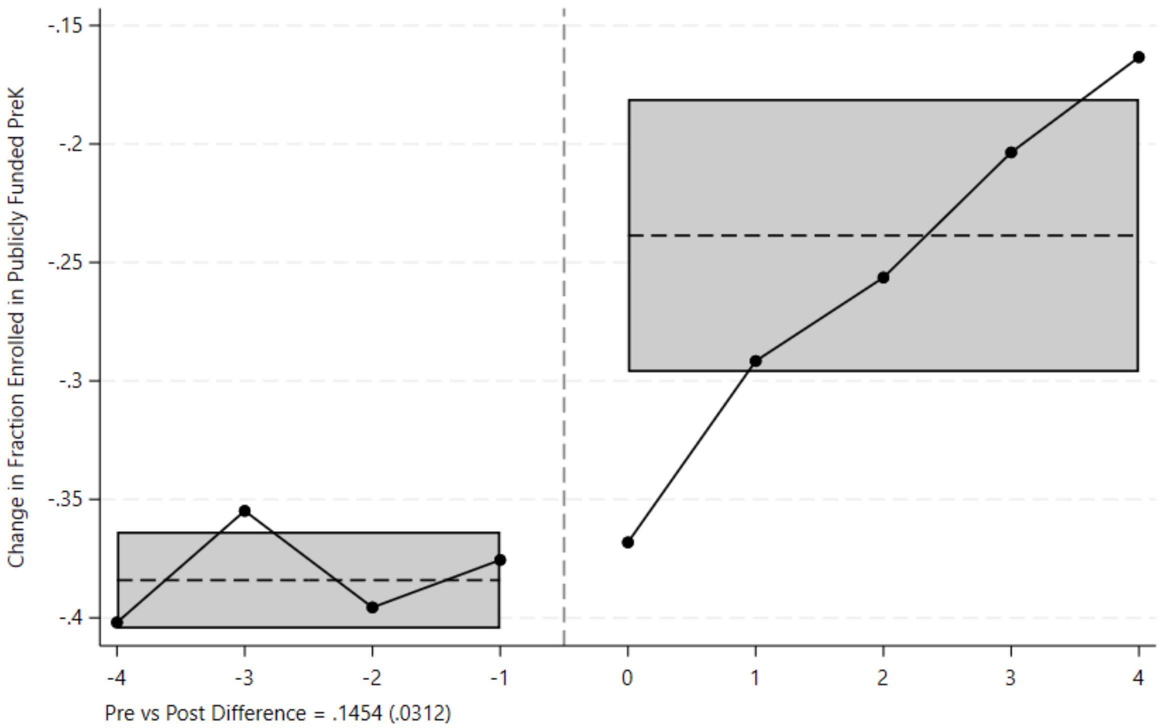
A Appendix Tables and Figures

Table A.1: Summary Statistics for Treated and Non-Treated Groups

	Treated	Not Treated
	(1)	(2)
Employment (last week)	0.62	0.61
	[0.24]	[0.23]
Labor Force Participation	0.67	0.66
	[0.25]	[0.25]
Full-time Employment Status	0.54	0.54
	[0.23]	[0.24]
Hours Worked	26.77	26.80
	[10.94]	[11.06]
Annual Average Wages	42,934	40,226
	[21,906]	[23,105]
White	0.75	0.80
	[0.43]	[0.40
Black	0.18	0.12
	[0.38]	[0.32]
Asian	0.04	0.05
	[0.21]	[0.22]
Female	0.51	0.51
	[0.50]	[0.50]
Age	40.86	40.72
	[13.48]	[13.47]
Percent in Pre-K	0.69	0.66
	[.09]	[.09]
Percent in Public Pre-K	0.46	0.41
	[0.11]	[0.10]
Percent in Private Pre-K	0.23	0.25
	[.08]	[.08]
Percent in Part-Time Pre-K	0.27	0.32
	[0.10]	[0.10]
Percent in Full-Time Pre-K	0.43	0.33
	[0.11]	[.10]

Notes: Table shows 2000–2019 summary statistics for treated and non-treated states using the CPS ASEC sample. Standard deviations are given in brackets. Treated states are DC, FL, GA, IA, NYC, OK, VT, WV, and WI . Appendix Table A.2 shows similarities between the CPS and ACS.

Figure A.1: Effect of UPK on Publicly Funded Pre-K Enrollment among Four-Year-Olds (NIEER Data)



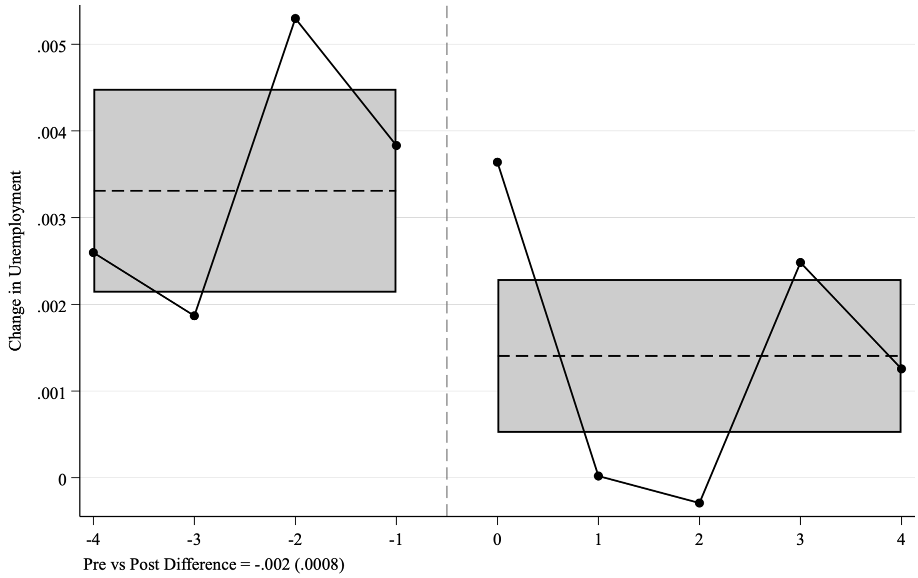
Note: Event study shows the effect of UPK introduction on publicly funded Pre-K enrollment using NIEER data. Since NIEER data starts in 2002, UPK states include IA, WI, DC, VT, and NYC. Specification is identical to panels a and b in Figure 2. Specification includes region-year controls as well as policy controls discussed in Section 2.

Table A.2: Employment Statistics: ACS and CPS Comparison

	Employment Rate		LFPR		Hours Worked		Average Total Income		Average Total Wages	
	ACS	CPS	ACS	CPS	ACS	CPS	ACS	CPS	ACS	CPS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1990	60.1%	60.4%	66.1%	66.4%	27.9	24.0	45,951	45,566	34,802	33,317
2000	61.6%	62.9%	67.0%	68.0%	29.2	25.1	53,481	52,022	40,773	38,895
2001	61.1%	62.5%	67.1%	67.9%	28.7	24.7	53,910	53,911	40,830	40,938
2002	60.4%	61.2%	67.3%	67.4%	28.4	24.1	53,871	54,263	40,920	41,794
2003	60.2%	60.9%	67.2%	67.2%	28.1	23.9	52,551	52,517	39,939	40,746
2004	60.2%	60.7%	67.1%	66.7%	28.1	23.9	53,372	52,601	40,374	40,291
2005	60.5%	61.0%	67.1%	66.5%	28.2	24.0	53,599	52,146	40,619	40,043
2006	59.9%	61.2%	66.2%	66.8%	27.8	24.0	52,024	52,789	39,190	40,181
2007	59.8%	61.8%	66.1%	67.2%	27.7	24.3	53,343	53,807	40,034	40,977
2008	61.1%	61.5%	67.2%	67.0%	27.4	24.1	52,754	52,600	39,751	40,229
2009	58.4%	58.6%	66.7%	66.5%	26.4	22.5	51,754	53,021	38,940	40,646
2010	57.2%	57.5%	65.9%	65.9%	25.5	22.0	49,288	50,974	36,767	38,593
2011	57.2%	57.5%	65.4%	65.2%	25.3	22.0	48,487	49,399	36,164	37,272
2012	57.7%	57.6%	65.3%	65.0%	25.4	22.1	48,566	49,759	36,233	37,722
2013	58.0%	57.6%	65.0%	64.5%	25.5	22.3	49,366	50,209	36,896	38,139
2014	58.4%	58.1%	64.6%	64.2%	25.5	22.4	49,679	51,346	37,118	38,502
2015	58.8%	58.5%	64.5%	63.9%	25.7	22.7	51,851	52,511	38,571	39,409
2016	59.2%	59.1%	64.5%	64.1%	25.8	22.9	52,892	54,473	39,576	41,043
2017	59.5%	59.0%	64.5%	64.1%	25.9	22.8	53,496	55,729	40,101	41,918
2018	59.9%	59.4%	64.6%	64.0%	26.0	23.0	54,393	56,465	40,670	42,171
2019	60.3%	59.9%	64.8%	64.2%	25.9	23.3	56,332	57,797	41,604	43,100
2020	57.4%	57.9%	63.7%	63.8%	25.7	22.3	57,155	61,620	41,975	45,321
2021	58.5%	56.9%	64.3%	62.7%	25.4	22.1	56,908	59,751	41,984	43,203
2022	60.3%	59.2%	64.7%	63.4%	25.9	23.0	56,251	58,176	41,880	42,443

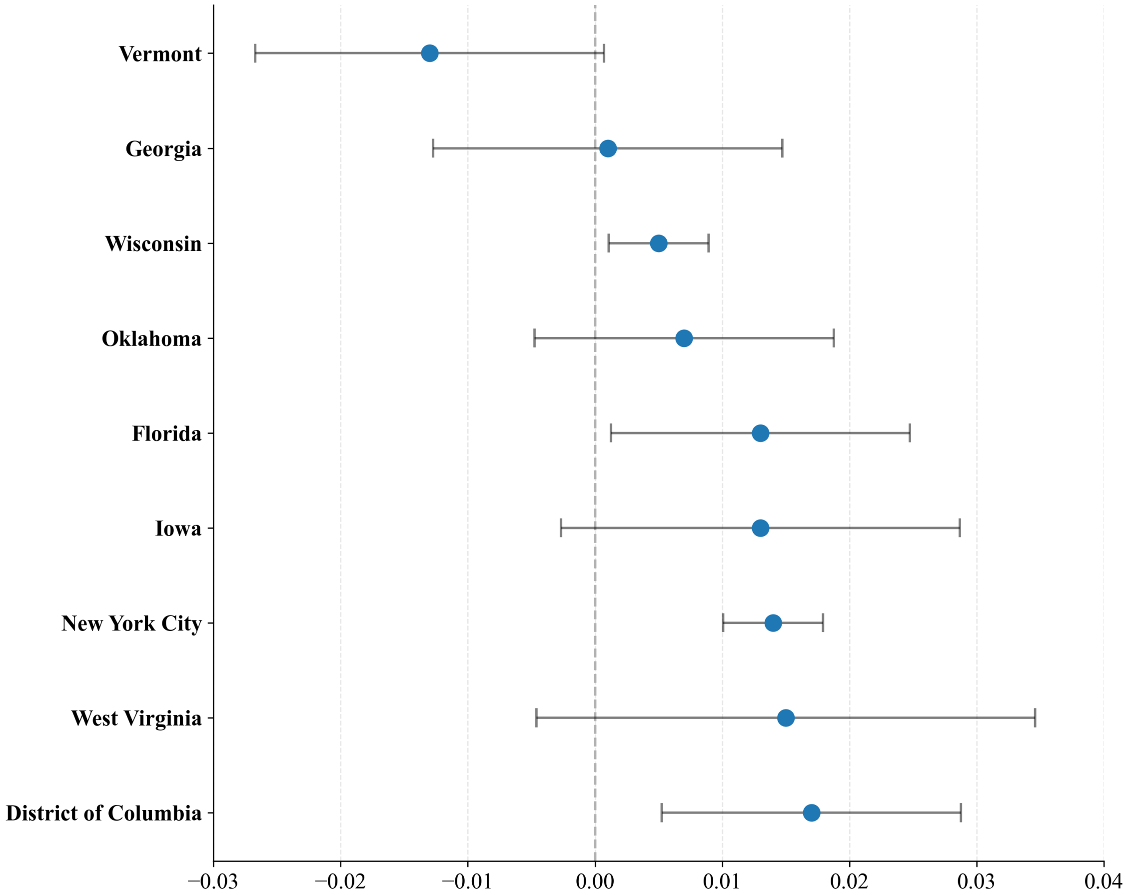
Notes: Summary statistics presented above use full samples (18 and over) from both ACS and CPS. Average of each variable are computed using perwt in ACS and asecwt in CPS (ASEC sample used).

Figure A.2: Effect of UPK Introduction on Unemployment Rate



Note: The figure shows the effect of UPK introduction on unemployment rate. The effect is estimated for all individuals aged 18-65. UPK introductions that occur before 2008 (GA, OK, WV, FL, and IA) use CPS data in their stack. All other stacks (WI, DC, VT, and NYC) use ACS data. Model includes policy specific controls discussed in Section 2.

Figure A.3: Effect of UPK Introduction on Employment by State



Note: The figure shows the effect of UPK introduction on employment by state. UPK introductions that occur before 2008 (GA, OK, WV, FL, and IA) use CPS data in their stack. All other stacks (WI, DC, VT, and NYC) use ACS data. Models do not include controls, but results are comparable when region-year and policy controls are included.

Table A.3: Effects of UPK on Time with Young Children

	NY Not Included				NY Included			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Moms of young kids	-4.65	-4.56	-4.40	-4.51	-2.10	-2.50	-2.38	-2.67
	(1.85)	(1.70)	(1.68)	(1.77)	(1.96)	(1.86)	(1.85)	(1.76)
Women without young kids	-1.50	-1.53	-1.45	-1.54	-1.58	-1.68	-1.62	-1.74
	(0.50)	(0.53)	(0.55)	(0.50)	(0.23)	(0.30)	(0.31)	(0.29)
Aggregated	X				X			
State Controls		X		X		X		X
Region-Year			X	X			X	X
Number of Kids				X				X

Notes: Table shows estimates from pre-post event studies using specified samples. Not moms of young kids sample includes all individuals *except* women who have a child under 5. Columns (1) and (5) show aggregated results, which are analogous to columns (2) and (6), but aggregate data to the state-year-stack level to mimic main results. Columns (5) through (8) include New York state in place of NYC - which introduces some measurement error and attenuation bias. Columns (1) through (4) omit New York altogether - which is our preferred approach.

Table A.4: Effects of Policy on Labor Market Outcomes (Excluding DC)

	All		Moms of Young Kids		Not Moms of Young Kids	
	(1)	(2)	(3)	(4)	(5)	(6)
Labor Force Participation	0.006	0.009	0.020	0.030	0.001	0.004
	(0.002)	(0.003)	(0.008)	(0.009)	(0.002)	(0.002)
Employment	0.008	0.009	0.020	0.027	0.005	0.005
	(0.002)	(0.002)	(0.011)	(0.010)	(0.001)	(0.002)
Hours Worked	0.407	0.436	0.602	0.703	0.297	0.304
	(0.110)	(0.091)	(0.327)	(0.365)	(0.113)	(0.105)
Full-time Status	0.013	0.011	0.016	0.016	0.011	0.009
	(0.002)	(0.002)	(0.008)	(0.007)	(0.003)	(0.003)
Controls Included		X		X		X

Notes: Table shows estimates from pre-post event studies using specified samples. Not moms of young kids sample includes all individuals *except* women between the ages of 18 and 65 who have a child under 5. UPK introductions that occur before 2008 (GA, OK, WV, FL, and IA) use CPS data in their stack. All other stacks (WI, VT, and NYC) use ACS data. Each stack includes one treatment group and 42 control groups (treated states are left out of control group). Across 8 stacks (leaving out DC), 344 state-by-year level observations are used in the analysis. Specifications that include controls include region-year fixed effects as well as policy specific controls discussed in Section 2.

Table A.5: Effects on Labor Market Outcomes Including Extra Controls

	All Individuals		Excluding Moms of Young Kids		Young Moms	
	(1)	(2)	(3)	(4)	(5)	(6)
Labor Force Participation	0.006	0.003	0.004	0.001	0.020	0.026
	(0.002)	(0.003)	(0.001)	(0.003)	(0.009)	(0.010)
Employment	0.009	0.007	0.004	0.008	0.021	0.029
	(0.002)	(0.003)	(0.001)	(0.002)	(0.006)	(0.009)
Full-time Status	0.012	0.019	0.009	0.017	0.014	0.028
	(0.002)	(0.002)	(0.003)	(0.003)	(0.008)	(0.014)
Hours Worked	0.422	0.487	0.256	0.551	0.633	0.837
	(0.090)	(0.077)	(0.096)	(0.123)	(0.380)	(0.563)
Bartik and Region Controls		X		X		X

Notes: Table shows estimates from pre-post event studies using specified samples. Columns (1), (3), and (5) include no controls (as in Table 1). Columns (2), (4), and (6) include Bartik-style and region-year controls. Policy controls are not included in this model and can be seen in Table 1. Not moms of young kids sample includes all individuals *except* women between the ages of 18 and 65 who have a child under 5. UPK introductions that occur before 2008 (GA, OK, WV, FL, and IA) use CPS data in their stack. All other stacks (WI, DC, VT, and NYC) use ACS data. Each stack includes one treatment group and 42 control groups (treated states are left out of control group). Across 9 stacks, 387 state-by-year level observations are used in the analysis.