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

C. Kirabo Jackson Julia A. Turner Jacob Bastian

March 20, 2025

Abstract

While Universal Pre-Kindergarten (UPK) is often proposed as an economic stimulus, its market effects remain uncertain. We 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.

*Jackson: Northwestern University, 2120 Campus Drive, Evanston IL 60208 (e-mail: kirabo-jackson@northwestern.edu). Turner: Northwestern University, 2120 Campus Drive, Evanston IL 60208 (e-mail: juliaturner2027@u.northwestern.edu), Bastian: Rutgers University, (e-mail: jacob.bastian@rutgers.edu). The authors declare that this research received no financial support from any source. Furthermore, the authors have no financial relationships or other potential conflicts of interest to disclose.

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 around the effect of UPK on markets more broadly. While much of the existing literature has focused on the effect of UPK on the labor-market outcomes of mothers with young children, this may not fully capture UPK's broader labor market effects for several reasons. First, increased maternal employment could crowd out employment for others (Fukui et al., 2023), limiting the overall gains. Second, with up to half of young children receiving informal care (Early Edge California, 2024), UPK's effects likely extend well beyond mothers. Third, because people are forward looking, UPK may affect prospective parents' labor decisions. Finally, focusing only on parental employment misses broader demand-side impacts. That is, increases in in-kind benefits would increase consumption for families who would have paid for childcare out of pocket (Hoynes and Schanzenbach, 2009).

Addressing uncertainty around both (a) the generalizability of the effects of UPK and (b) its broader market implications, we examine the impact of UPK on the labor market beyond the affected mothers, highlighting a distinct but important policy dimension. Additionally, we help reconcile mixed findings in the literature by formally testing for heterogeneity across settings and evaluating whether more ambitious UPK programs generate greater 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. Using data from various sources, 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]

To measure Pre-K enrollment, we use data from the October supplement of the Current Population Survey, the American Community Survey, and data collected by the National Institute for Early Childhood Research (NIEER). We measure labor market outcomes for individuals from both the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS), and the American Community Survey (ACS).

[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.

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 between UPK and non-UPK areas, (b) show our results hold when controlling for strong labor market predictors, and (c) show that our estimated effects are robust to controlling for 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 largely unaffected by childcare availability exhibit no significant effects, and (ii) areas with the largest increases in public Pre-K enrollment experience the most substantial labor market changes.

On average, areas that introduced Universal Pre-K saw about a 10 percentage point increase in public Pre-K enrollment among four-year-olds over five years, compared to other regions. Despite some crowd-out from private Pre-K, overall Pre-K enrollment (both full time and part time) rose by about 6.6 percentage points in these areas.

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. Since employment growth outpaced labor force participation, we examined unemployment and found a decline – suggesting that expanded childcare access improved job matching by making 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 effects but account for less than half of the total impact. A key result is that, even excluding this subgroup, we observe sizable aggregate effects. Employment outcomes increase across all margins for prime-age women, including those without young children, while effects for prime-age men are minimal. These patterns align with two key mechanisms being at play: informal caregivers—primarily women—being relieved of childcare duties and prospective mothers increasing labor market attachment in anticipation of future childcare access.

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 – suggesting that UPK-driven increases in Pre-K enrollment boost economic activity.

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.

In a supply-demand framework, one would expect increased labor supply to lower wages. Consistent with this, we find a small, statistically insignificant wage decline, suggesting highly inelastic demand. Given the negligible average wage changes, while imprecise, UPK led to about sizable increases in total aggregate income - highlighting real overall benefits. To quantify the impact of UPK on the economy, we relate increases in public Pre-K spending to corresponding increases in aggregate earnings. Each dollar spent on public Pre-K generated between 3 to over 20 dollars in additional aggregate earnings— enough to cover program costs through increased federal and state tax revenue.

The remainder of the paper is structured as follows: Section 2 details the data, and Section 3 outlines our identification strategy. Sections 4 and 5 examine the impact of UPK on Pre-K enrollment and aggregate labor market outcomes, respectively. We then explore effect heterogeneity and highlight the substantial impacts of a model UPK program in Section 6.1. Section 7 briefly discusses the effects on aggregate earnings, followed by conclusions in Section 8.

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). Together, these datasets provide a comprehensive view of Pre-K enrollment and public Pre-K funding patterns over time and across geographic areas.

The NIEER data (2002–2023) provides a comprehensive state-level analysis of publicly funded Pre-K programs. Compiled annually in the State of Preschool Yearbooks, it draws from state education agencies, budget documents, legislative reports, and other sources.[2] NIEER

[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.

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. NIEER's focus on public programs and funding makes it a vital resource for understanding state-level early childhood education investments.

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 our use of ACS data. The ACS surveys about 3.5 million households annually, offering a much larger dataset than the CPS, thus enabling more precise enrollment estimates, particularly 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), about 43% of four-year-olds were enrolled in public Pre-K, while overall Pre-K enrollment (including private) 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

[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.

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. While CPS-only results are similar, the ACS's larger sample size improves precision. Appendix Table A.1 presents summary statistics for treated and untreated areas.

UPK and non-UPK areas are largely similar. Among individuals over the age of 18, both treatment and control states have employment rates near 62% and labor force participation rates around 67%.[6] 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 data from the University of Kentucky Center for Poverty Research (UKCPR, 2025), which tracks demographic shifts, policy changes, labor market conditions, and local political dynamics across the U.S. Specifically, we include state-level data on welfare benefits, poverty rates, governor political affiliation, state Earned Income Tax Credit (EITC) rates, minimum wage levels, and participation in Supplemental Security Income (SSI), Medicaid, and WIC.

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, allowing five years of post-implementation observation for all areas. We ensure a balanced panel by examining outcomes four years before and after UPK implementation.

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. 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

[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.

[6]Both ACS and CPS analyses use employment indicators reflecting employment in the previous week.

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, which includes a mini-dataset for each treatment-area group. We 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, and each state-year observation is weighted by population size. 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, it is important that the control group states closely resemble the UPK areas to ensure they follow similar pre-period trends and experience comparable time shocks, satisfying the common trends assumption. As such, to further account for regional differences, our preferred specification also includes region-by-year fixed effects–capturing shared regional shocks like economic fluctuations or policy changes that broader time controls may miss. This adjustment effectively matches each UPK state with others in its region, improving comparability.[7] Additionally, we estimate all models with a full set of policy controls to account for the potential effects of coinciding policies.

[7]We also test a Bartik-style control using a shift-share approach. Bartik controls multiply 1990 state-level industry shares (pre-UPK) by national industry growth rates in employment and labor force participation, then aggregate them at the state-year level. These predict labor market shifts based on each state's 1990 industrial structure and national trends. Table A.4 presents results with both region-by-year fixed effects and Bartik controls. We prefer the region-by-year specification for its parsimony, as results are similar with and without these controls.

4 Effect on Pre-K Enrollment

Figure 2 presents event-study plots illustrating how outcomes change in areas that implemented UPK compared to states that did not. The figure displays event study estimates spanning four years before and four years after UPK implementation. We also present the average effects for the four pre-treatment years and the five post-treatment years, along with their respective confidence intervals. We also report the pre-post change and the standard error of this change. The pre- and post-treatment averages are calculated as precision-weighted averages of the respective estimates, and the reported change represents the difference between these two precision-weighted averages.

Figure 2 panel (a) shows an event study of public Pre-K enrollment for four-year-olds using CPS-ACS data, covering all UPK states and the full set of controls. While the data are somewhat noisy pre-UPK, public Pre-K enrollment increases by about 10 percentage points post-UPK (p-value< 0.01). Importantly, there were no differential trends in public Pre-K enrollment before UPK implementation in UPK areas, and there was a sudden increase at implementation. This is consistent with results using data from NIEER (see Appendix Figure A.1 for the event study) which is available after 2002 summarized in Table 1 column (8) showing a 14.5 percentage point increase (p-value< 0.01) in publicly-funded Pre-K.[8] Both datasets show a significant increase in public (or publicly funded) pre-K enrollment following UPK implementation. Moreover, as shown in Table 1 Panel A, estimates remain consistent across models and datasets, regardless of the data source or the inclusion of the full set of controls—region-by-year effects and welfare controls.

Since many children may shift from private to public Pre-K once it becomes available—as seen in Boston (Weiland et al., 2020) and New Haven (Humphries et al., 2024b)—we estimate effects on the proportion of four-year-olds enrolled in *any* school. In Table 1 and Figure 2, CPS/ACS data show that, like public Pre-K enrollment, overall Pre-K enrollment is noisy without a clear trend before UPK. However, after UPK implementation, enrollment increases by 6.6 percentage points (p-value< 0.01). To shed further light on where children enroll, Table 1 shows that, overall, UPK increases part-time Pre-K enrollment by 3.6 percentage points ($p-value = 0.23$) and full-time Pre-K enrollment by 3.7 percentage points ($p-value = 0.10$).

Overall, the evidence links UPK implementation to significant public Pre-K enrollment increases of about 10 percentage points. However, crowd-out effects limit the net gain in total Pre-K enrollment among four-year-olds to about 6.6 percentage points, with increases for both part-time and full-time enrollment. We show that the results are robust to using different data sources (when possible) and in models with and without controls (see Table 1).

[8] When we restrict the ACS/CPS analysis to the events covered by the NIEER, the estimated increase is 12.0 percent – similar to the NIEER estimate. Note that NIEER measures "publicly funded" Pre-K, covering state-subsidized private centers and voucher-funded programs—such as in D.C., where the district funds private child-care centers through community-based organizations (DC, 2025). This is not included in the ACS/CPS measure of public Pre-K enrollment. As such, while these two measures are similar, they are not identical.

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 ($p-value < 0.01$) and is about a 1.2 percent relative increase. Importantly, there is no evidence of any differential pre-trending in aggregate outcomes for areas that introduced Universal Pre-K. 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.

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 ($U \Rightarrow E$). Although the unemployment effect is imprecisely estimated (see Figure A.2), our evidence suggests both flows contribute to the larger employment effect relative to labor force participation. This could be explained by UPK lowering job search costs for caregivers, and is consistent with Philippe and Skandalis (2023) who finds that mothers submit fewer job applications and are more selective about wages and amenities, with application rates dropping over 20% when children are not in school. This suggests parents with young children face both lower incentives and higher costs to job search, a challenge amplified in high childcare cost settings (Novoa and Workman, 2021).

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, and Table 1 indicates a 1.1 percentage point rise in full-time employment. Notably, this increase exceeds the rise in overall employment, suggesting that some new employees took full-time jobs, and that some existing workers shifted from part-time to full-time—highlighting UPK's impact on labor flows. To summarize these labor market effects and to account for false discovery, following Kling et al. (2007), we create a labor index with is the average of the standardized outcomes. Given that each of the effects for the adult population was significant, as one would expect, the effect on the labor index is significant – with a coefficient of 0.031 and a $p-value$ below 0.001.

5.1 Effects by Subgroup: Evidence on Mechanisms

While UPK is expected to impact mothers of young children, we posit that its influence extends to a broader set of adults through three channels: (a) relieving informal caregivers of childcare responsibilities (caregiver effect), (b) increasing labor market attachment among those anticipating future childcare access (anticipatory effect), and (c) stimulating economic activity through boosting disposable income for some parents (demand-side effect). To test these mechanisms, we leverage demographic differences, recognizing that some groups are more likely than others to provide informal care or experience anticipatory effects.

To anchor our analysis, we focus first on prime-age men (ages 18 to 64). This group is

unlikely to experience significant caregiving effects, as such responsibilities primarily fall on women (Susman-Stillman and Banghart, 2008), and younger men are less likely than older men (e.g., grandfathers) to be informal caregivers (tak Joo et al., 2024). They are also less susceptible to anticipatory effects, as men's labor market decisions are generally less influenced by childcare availability (Schmitz, 2020; Houter and Barry, 2024). Since all groups, including younger men, are subject to demand-side effects, we use prime-age men as a baseline to help isolate broader demand-side influences from caregiving and anticipatory effects.

To analyze prime-age men, we estimate our main models using data from this group alone. Panel B, Column (6) of Table 1 shows that UPK's effects on their labor force participation, employment, and hours worked are small and not statistically significant, and there is a 1 percentage point increase in full-time status ($p < 0.05$). Looking at the overall labor market index, we find an estimated effect of 0.015 with a p-value of 0.096 and do not reject the null hypothesis of no effects for prime-age men. These findings suggest (a) minimal demand-side effects and (b) the aggregate effects operate primarily through women—underscoring the likely role of caregiving and anticipatory effects.

To isolate caregiving and anticipatory effects, we estimate a triple-differences model comparing UPK's impact on other demographic groups relative to prime-age men in Figure 3.[9] We also present double difference results for each group in Table 1. We begin with mothers of young children (under age six), the group most directly affected by UPK. Relative to prime-age men, after UPK implementation, mothers of young children experience a 1.1 percentage point increase in labor force participation, a 1.4 percentage point increase in employment, a 1.2 percentage point increase in full-time status and 0.47 hours more work per week. All differential effects have $p - value < 0.01$. These estimates align with *ex ante* expectations and are similar to estimates based on age-based Pre-K eligibility variation among mothers of young children (Ilin et al., 2021).

Mothers of young kids make up only 6.0% of the adult population, so their contribution cannot explain all the observed aggregate effects. To formally assess this, we estimate the aggregate UPK effect *excluding* these mothers. The results presented in 1 column (4) show a 0.003 increase in labor force participation (not significant), a 0.004 rise in employment ($p < 0.05$), a 0.9 percentage point increase in full time status ($p < 0.01$), and a 0.29 increase in weekly hours ($p < 0.01$). Summarizing these effects overall, the labor index excluding moms of young kids increases by 0.2 after UPK implementation ($p - value = 0.0043$). The reduction in the index effect from 0.031 to 0.02 when excluding mothers of young children suggests that they account for approximately one-third of the aggregate impact. Figure 3 presents the event-study plot for this group, showing post-UPK increases with no differential pre-trends–consistent with causal effects that go beyond mother of young children.

[9]This is implemented in a single regression using a stacked dataset for all groups, estimating the main model while interacting all variables with demographic group indicators. We report the coefficient on the interaction between the group indicator and the pre-post difference.

We also examine effects for prime-age women. While we cannot disentangle the two, this group is likely influenced by (a) the informal childcare channel, as female friends, neighbors, and relatives often provide care, and it may also be affected by (b) the anticipatory effect associated with having childcare in the future. If both mechanisms are at play, we expect larger effects for prime-age women than for prime-age men. To remove the mother effect, we examine outcomes for prime-age women who are not mothers of young children. Relative to prime-age men, UPK has little differential effect on labor force participation among prime-age women without young children. 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 rises by 0.28 hours ($p < 0.01$). The labor index strongly rejects the null hypothesis that the overall labor market effects for prime-age women without young children are the same as those for prime-age men, with significance at the one percent level.

Overall, the results confirm that UPK improves labor market outcomes for mothers of young children, aligning with existing research. However, we also find that UPK strengthens labor market attachment among women of childbearing age. We hypothesize that these effects arise from a combination of caregiving and anticipatory mechanisms. While we do not reject the null hypothesis of no effect for prime-age men, the estimates do not rule out the possibility of small effects for this group.

6 Exploring Heterogeneity

As noted by Fitzpatrick (2010) and Cascio (2021), UPK effects vary. To examine 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.[10] 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 explore 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 in the top panel of Figure 4. Each data point presents the UPK effect on the outcomes for a given state or cite, 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,

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

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 reduce 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.[11] 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.

6.1 The Exemplar Case of D.C.

UPK's impact depends on key implementation details, including full-day vs. half-day programs, public school provision, and aftercare availability. Washington, D.C.'s UPK program stands out in all aspects, allocating $22,207 per child in 2023—more than twice any other state program.[12] D.C. also 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-day programs. Even in New York, where NYC guarantees full-day UPK, availability remains limited in other parts of 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 significantly increased parental labor supply and reduced commuting time. D.C.'s UPK program also operates through a mixed-delivery system, including public schools, charter schools, and community-based or-

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

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

ganizations, enhancing flexibility and accessibility for families. It is also recognized for its high quality—ranking highest among all states and territories on observable 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 estimates reflect causal effects rather than differential pre-trends obscured by aggregating all UPK states and cities. To this aim, the lower panel of Figure 4 presents the event study for labor market outcomes in D.C., showing effect sizes nearly twice those in the full sample. Panel (a) reports a 1.7 percentage point increase in labor force participation following UPK's introduction ($p < 0.01$), while Panel (b) shows a nearly identical employment effect. Crucially, as in the full sample, there is 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 highlight the importance of accessible, high-quality UPK programs. While D.C. is central to our analysis, it does not drive our overall results—Appendix Table A.3 shows that excluding D.C. yields similar UPK effects.

6.2 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 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. Aggregate earnings reflect overall economic output, and since they can be influenced by population growth, we control for the log of population.[13] It is helpful to establish a reasonable range of expected estimates. First, we examine average wages and find no impact, suggesting highly

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

inelastic labor demand.[14] 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 suggests aggregate wage effects between 1% and 3.6%. Consistent with this range, Figure 2 shows UPK led to a 5.5% increase in total real wages. While this is statistically significantly different from zero, it has a 95 percent confidence interval between 0.8 and 10 percent. That is, while the precise effect is unclear, these findings indicate that UPK acts as a supply-side stimulus, boosting overall economic activity.

The pre-treatment average of aggregate wages was $45 billion, so the 95 percent confidence interval indicates an increase of between 315 million and 4.5 billion in additional earnings. To put this in perspective, we estimate that state spending on Pre-K rose by $99–$78 million following UPK implementation (see Table 1). This implies that each dollar spent on Pre-K generates between $3 to over $20 in additional earnings – in line with Humphries et al. (2024b), who report $10 in additional 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 to over $4.00 in tax revenue, suggesting that, *on the margin*, UPK programs could be self-sustaining even in the short run. However, since much of this revenue goes to the federal government, states may not fully recoup their costs, creating a fiscal externality. This underscores the efficiency of federal subsidies for state UPK programs over exclusive reliance on state or even 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. This is an important and policy-relevant result. We show that UPK increases labor supply on both extensive and intensive margins, boosting employment across a wide range of workers and driving higher aggregate earnings.

This study also demonstrates true heterogeneity in effects across states, helping reconcile inconsistencies in prior findings. To help explain this heterogeneity, we show that program scale and quality matter—more ambitious models, such as Washington, D.C.'s, yield significantly greater economic benefits.

To summarize overall benefits, we estimate that each dollar spent on UPK generates much more than a dollar in additional earnings, indicating a high benefit-cost ratio in the short run. Taken together, our results suggest that high-quality UPK may not only be a worthwhile investment in the skills of children —but also a potent supply-side economic intervention.

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

References

David R. Agrawal, William H. Hoyt, and Tidiane Ly. A New Approach to Evaluating the Welfare Effects of Decentralized Policies, January 2022. URL https://papers.ssrn.com/abstract=4009872.

Clive R Belfield and Andre Neveu. The macroeconomics of pre-schooling: Simulating the effects of universal early childhood education on the u.s. economy, 2006.

Jordan S Berne. The long-run impacts of universal pre-k: Evidence from the first statewide program the long-run impacts of universal pre-k: Evidence from the first statewide program, 2022. URL https://doi.org/10.26300/k4bh-0114.

Breno Braga, Justin B. Doromal, Erica Greenberg, Tomas Monarrez, Leonardo Restrepo, and Rachel Lamb. The effects of public pre-k for 3-year-olds on early elementary school outcomes: Evidence from the dc centralized lottery, August 2024. URL http://www.edworkingpapers.com/ai24-1019.

Elizabeth Cascio. Early childhood education in the united states: What, when, where, who, how, and why, 4 2021. URL https://papers.ssrn.com/abstract=3834162.

Elizabeth Cascio and Diane Schanzenbach. The impacts of expanding access to high-quality preschool education, 2013. URL https://www.brookings.edu/articles/the-impacts-of-expanding-access-to-high-quality-preschool-education/.

Doruk Cengiz, Arindrajit Dube, Attila Lindner, and Ben Zipperer. The Effect of Minimum Wages on Low-Wage Jobs. *The Quarterly Journal of Economics*, 134(3):1405–1454, August 2019. ISSN 0033-5533. doi: 10.1093/qje/qjz014. URL https://doi.org/10.1093/qje/qjz014.

Youran Wu Daniela Viana Costa, Maddison Erbabian. Economic effects from preschool and childcare programs — penn wharton budget model, 8 2021. URL https://budgetmodel.wharton.upenn.edu/issues/2021/8/23/economic-effects-preschool-and-childcare-programs.

My School DC. Find schools outside my school dc, 2025. URL https://www.myschooldc.org/find-schools/school-options-outside-my-school-dc. Accessed: 2025-01-30.

Rebecca DerSimonian and Nan Laird. Meta-analysis in clinical trials. *Controlled Clinical Trials*, 7:177–188, 9 1986. ISSN 0197-2456. doi: 10.1016/0197-2456(86)90046-2.

Manasi Deshpande and Yue Li. Who Is Screened Out? Application Costs and the Targeting of Disability Programs. *American Economic Journal: Economic Policy*, 11(4):213–248, November 2019. ISSN 1945-7731. doi: 10.1257/pol.20180076. URL https://www.aeaweb.org/articles?id=10.1257/pol.20180076.

Early Edge California. Increasing Access to Family, Friend, and Neighbor Caregiver Wages through California's Child Care Subsidy System, 2024. URL https://earlyedgecalifornia.org/new-policy-brief-increasing-access-to-family-friend-and-neighbor-caregiver-wages-th

Maria Donovan Fitzpatrick. Preschoolers enrolled and mothers at work? the effects of universal prekindergarten. *Journal of Labor Economics*, 28:51–85, 1 2010. ISSN 0734306X. doi: 10.1086/648666.

Masao Fukui, Emi Nakamura, and Jón Steinsson. Women, Wealth Effects, and Slow Recoveries. *American Economic Journal: Macroeconomics*, 15(1):269–313, January 2023. ISSN 1945-7707. doi: 10.1257/mac.20200312. URL https://www.aeaweb.org/articles?id=10.1257/mac.20200312.

Chloe Gibbs, Jocelyn Wikle, and Riley Wilson. A matter of time? measuring effects of public schooling expansions on families, 2024.

Guthrie Gray-Lobe, Parag A. Pathak, and Christopher R. Walters. The long-term effects of universal preschool in boston. *The Quarterly Journal of Economics*, 138:363–411, 12 2022. ISSN 0033-5533. doi: 10.1093/QJE/QJAC036. URL https://dx.doi.org/10.1093/qje/qjac036.

Kate Den Houter and Karen Guggenheim Kristin Barry. More than a program: A culture of women's wellbeing at work, 12 2024. URL https://www.gallup.com/workplace/653843/program-culture-women-wellbeing-work.aspx.

Hilary Hoynes and Diane Whitmore Schanzenbach. Consumption responses to in-kind transfers: Evidence from the introduction of the food stamp program. *American Economic Journal: Applied Economics*, 1:109–39, 10 2009. ISSN 1945-7782. doi: 10.1257/APP.1.4.109. URL http://www.aeaweb.org/articles.php?doi=10.1257/app.1.4.109http://www.aeaweb.org/articles.php?doi=10.1257/app.1.4.109.

John Eric Humphries, Christopher Neilson, Xiaoyang Ye, and Seth D. Zimmerman. Parents' earnings and the returns to universal pre-kindergarten. *NBER Working Papers*, 10 2024a. doi: 10.3386/W33038. URL https://www.nber.org/papers/w33038.

John Eric Humphries, Christopher Neilson, Xiaoyang Ye, and Seth D. Zimmerman. Parents' Earnings and the Returns to Universal Pre-Kindergarten, October 2024b. URL https://www.nber.org/papers/w33038.

Elias Ilin, Samantha Shampine, and Ellie Terry. Does Access to Free Pre-Kindergarten Increase Maternal Labor Supply?, November 2021.

C. Kirabo Jackson. When Does School Autonomy Improve Student Outcomes, July 2023.

Jeffrey R Kling, Jeffrey B Liebman, and Lawrence F Katz. Experimental analysis of neighborhood effects. *Econometrica*, 75:83–119, 1 2007.

Rasheed Malik. The effects of universal preschool in washington, d.c., 9 2018. URL https://www.americanprogress.org/article/effects-universal-preschool-washington-d-c/.

Elise A Marifian. The spillover effects of universal pre-k: Modern evidence on mothers' labor supply responses, 2021. URL https://www.boston.gov/news/15-million-investment-.

NIEER. The State of Preschool 2023: State Preschool Yearbook, 2024. URL https://nieer.org/sites/default/files/2024-08/2023_nieer_yearbook_8-9-24.pdf.

Cristina Novoa and Simon Workman. Growing the economy through affordable child care. Technical report, Center for American Progress, Washington, D.C., 2021. URL https://www.americanprogress.org/article/growing-economy-affordable-child-care/.

Arnaud Philippe and Daphne Skandalis. Motherhood and the Cost of Job Search, December 2023.

Sophia Schmitz. The impact of publicly funded childcare on parental well-being: Evidence from cut-off rules. *European Journal of Population*, 36:171–196, 4 2020. ISSN 0168-6577. doi: 10.1007/S10680-019-09526-Z). URL http://link.springer.com/10.1007/s10680-019-09526-z.

Amy Susman-Stillman and Patti Banghart. Demographics of family, friend, and neighbor child care in the united states. Technical report, Reviews of Research Research Brief, 2008. URL www.researchconnections.org.

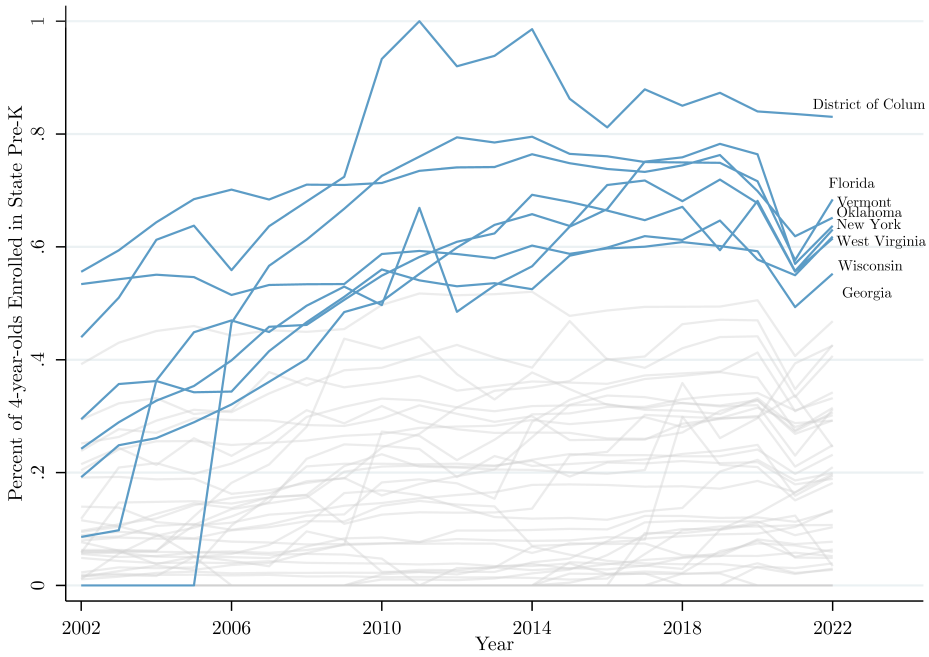
Won tak Joo, Felix Elwert, and Martin D. Munk. Labor market consequences of grandparenthood. *Sociological Science*, 11:600–625, 8 2024. ISSN 2330-6696. doi: 10.15195/V11.A22. URL www.sociologicalscience.com.

UKCPR. UKCPR national welfare data, 1980–2023, January 2025. Lexington, KY. Available at https://cpr.uky.edu/resources/national-welfare-data (accessed March 19, 2025).

Christina Weiland, Rebecca Unterman, Anna Shapiro, Sara Staszak, Shana Rochester, and Eleanor Martin. The Effects of Enrolling in Oversubscribed Prekindergarten Programs Through Third Grade. *Child Development*, 91(5):1401–1422, 2020. ISSN 1467-8624. doi: 10.1111/cdev.13308. URL https://onlinelibrary.wiley.com/doi/abs/10.1111/cdev.13308. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/cdev.13308.

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

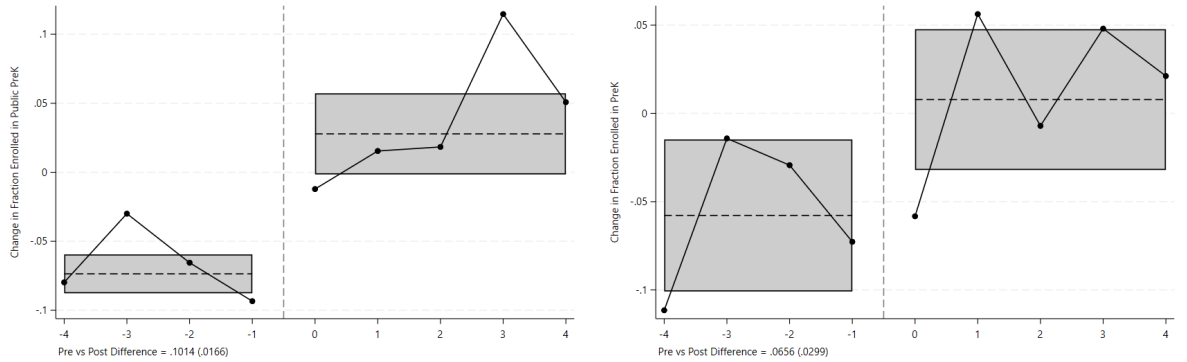
	All Individuals		Excluding Moms of Young Kids		Prime-Age Men		Young Moms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Labor Force Participation	0.006	0.008	0.004	0.003	-0.004	-0.001	0.020	0.026
	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.009)	(0.010)
Employment	0.009	0.009	0.004	0.004	0.003	0.003	0.021	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, weighted by population size. 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

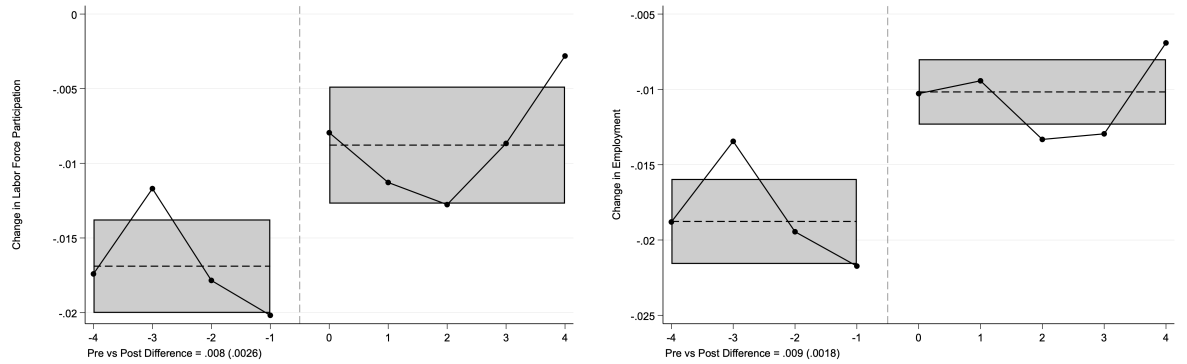
(a) ACS-CPS Data (Public Pre-K)

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



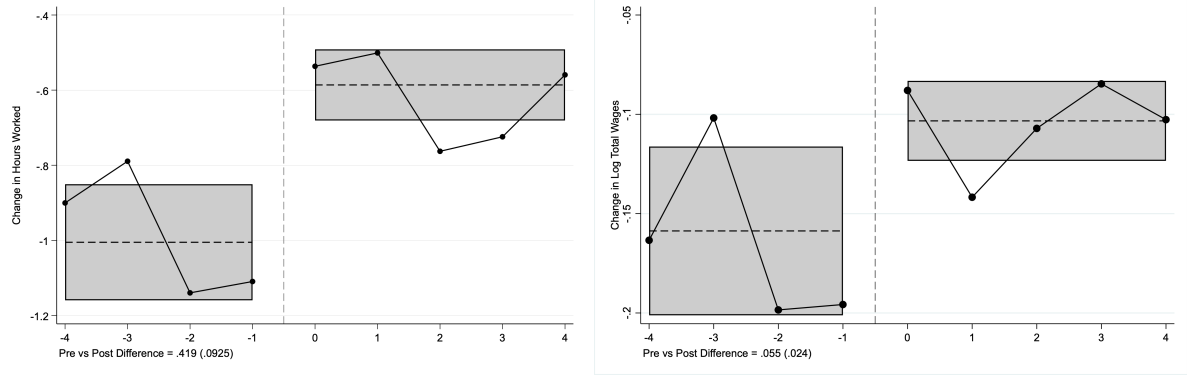
(c) Labor Force Participation Rate

(d) Employment Rate



(e) Hours Worked Last Week

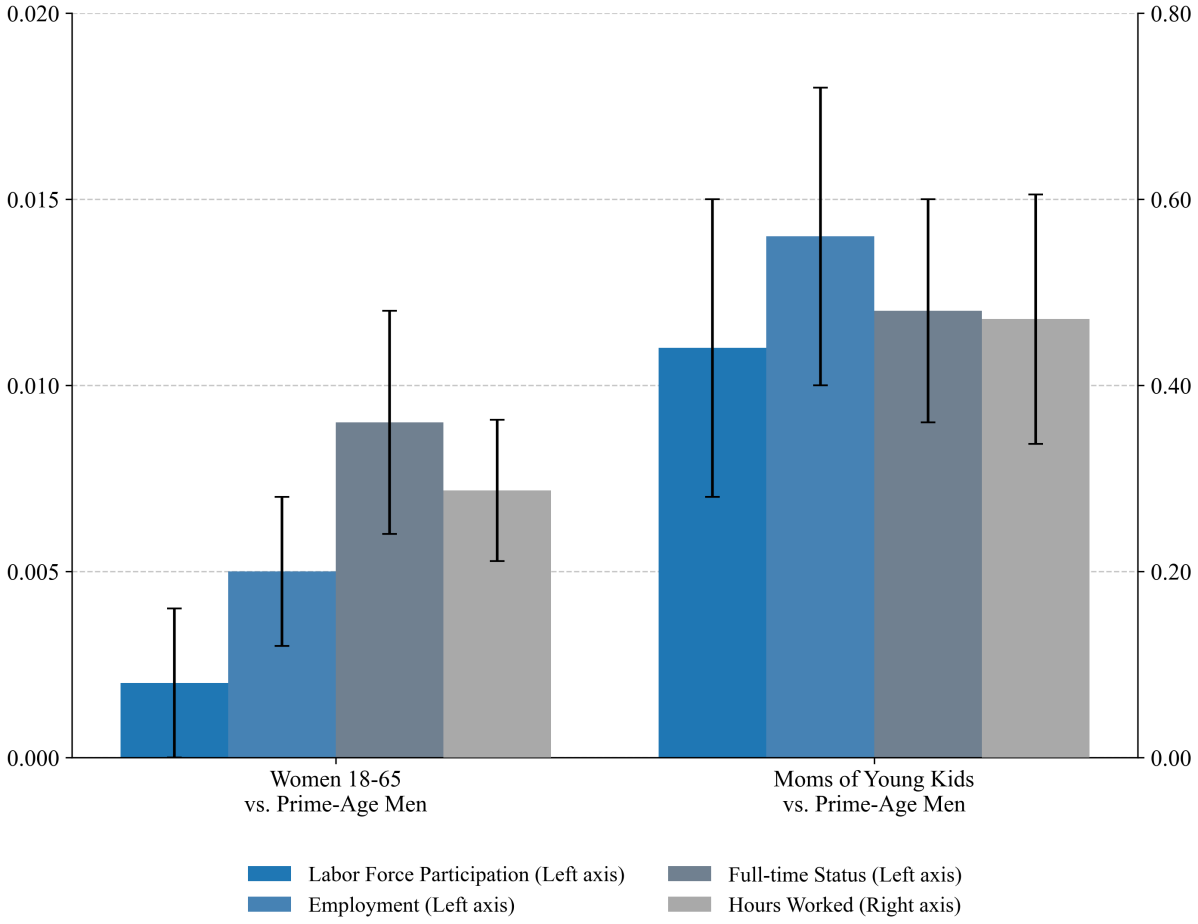
(f) Aggregate Earnings



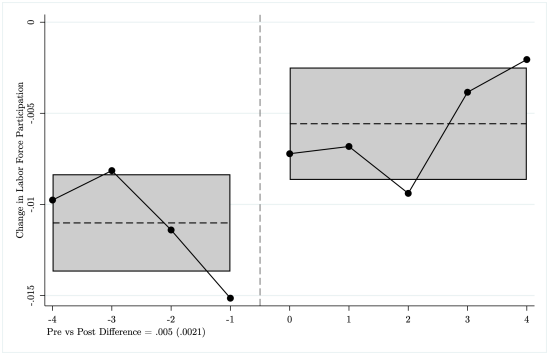
Notes: Panels (a) and (b) show the effect of UPK introduction on Pre-K enrollment on ACS-CPS data. Panel (a) shows publicly funded Pre-K; panel (b) shows total Pre-K enrollment (public and private). Panels (c)–(e) show the effect of UPK on employment outcomes: (c) labor force participation, (d) employment, and (e) average hours worked last week. 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, weighted by population size.

Figure 3: Effects by Group

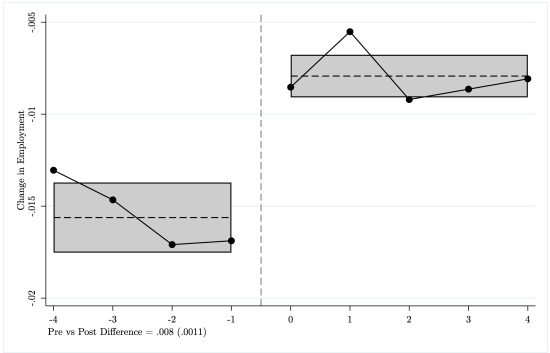
(a) Effects by Group: Relative to Prime Age Men



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



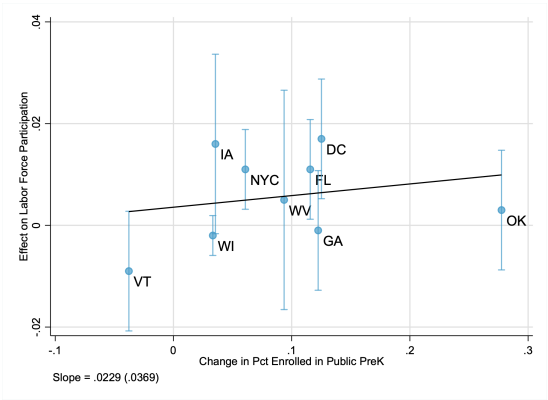
(c) Employment Rate: Excluding Mothers of Young Kids



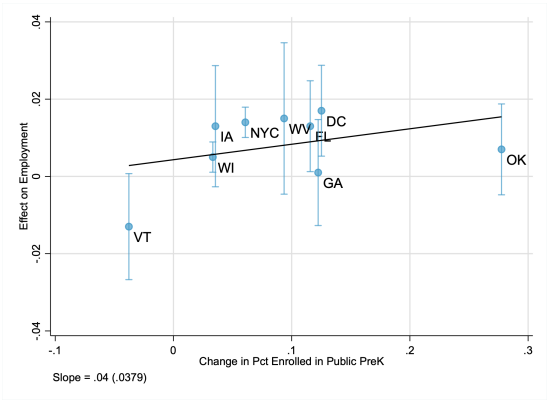
Note: Panel (a) shows point estimates from triple difference specifications that compare the specified treatment group to prime age (18-64) men in treated versus untreated states before and after UPK introduction. Model includes region-by-year fixed effects along with policy controls discussed in Section 2. Effects on hours worked are shown on the right axis of this figure. 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

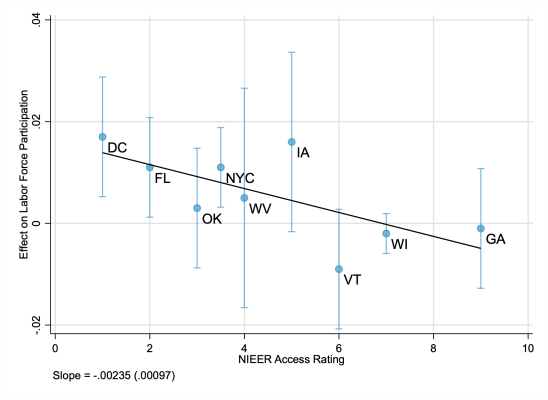
(a) Labor Force Participation Rate



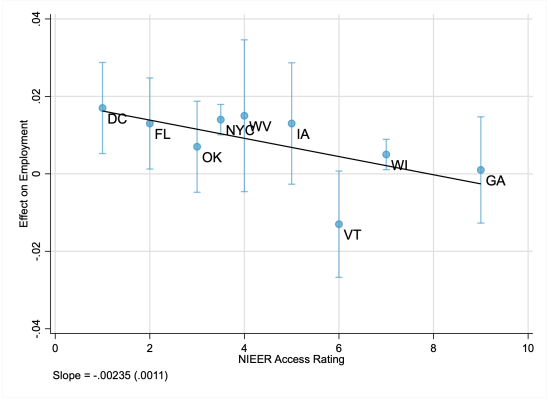
(b) Employment Rate



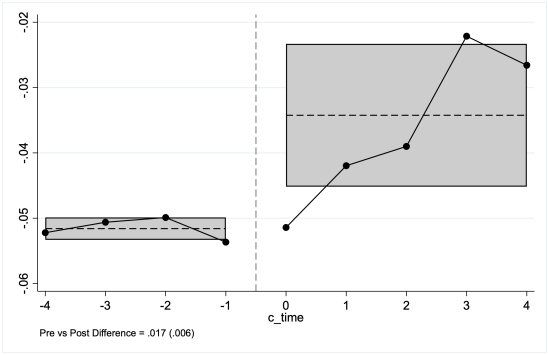
(c) Labor Force Participation Rate (NIEER Ranking)



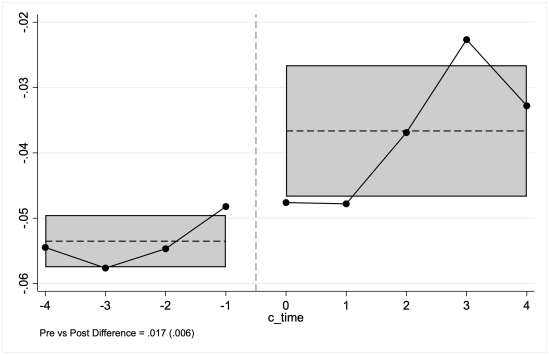
(d) Employment Rate (NIEER Ranking)



(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.

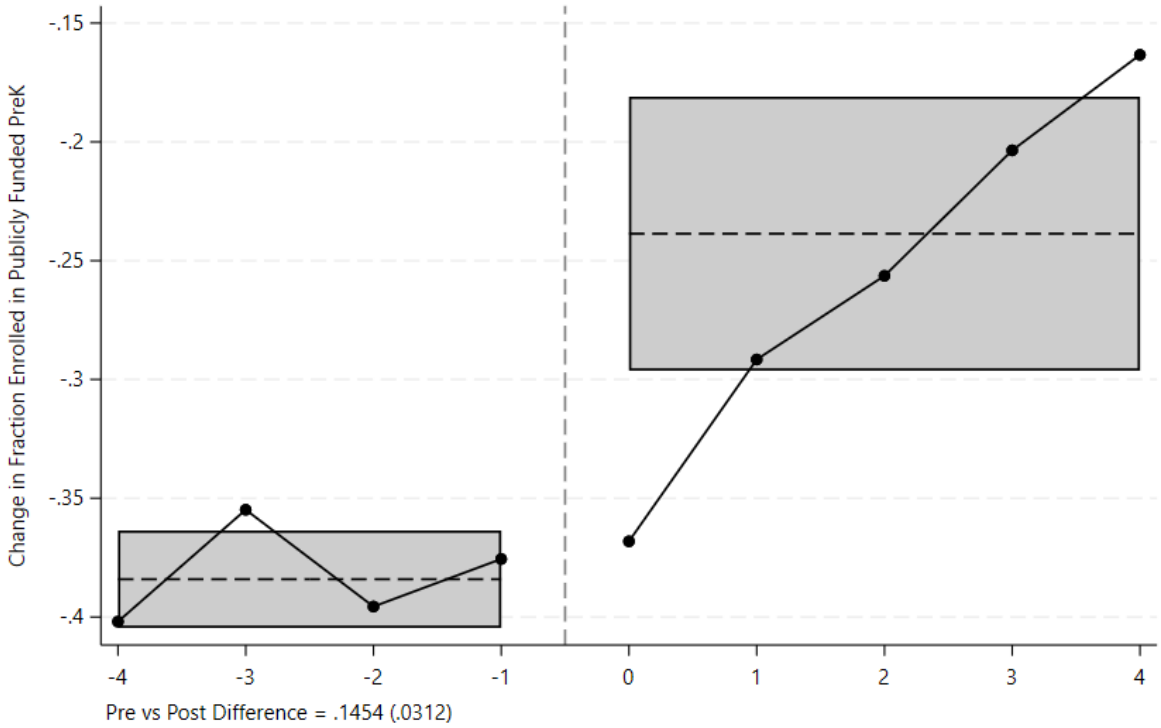
A Appendix Tables and Figures

Table A.1: Summary Statistics Before and After 2008 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. 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)



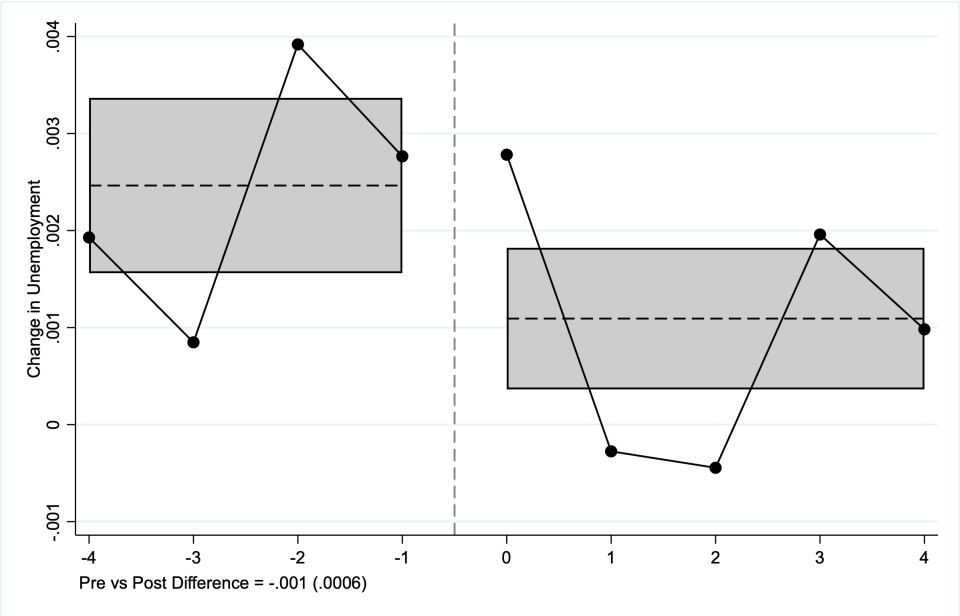
Note: Event study shows the effect of UPK introduction on publicly funded Pre-K enrollment using NIEER data. Since NIEER data starts in 2002, UPK states include IA, WI, DC, VT, and NYC. Specification is identical to panels a and b in Figure 2. Specification includes region-year controls as well as policy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

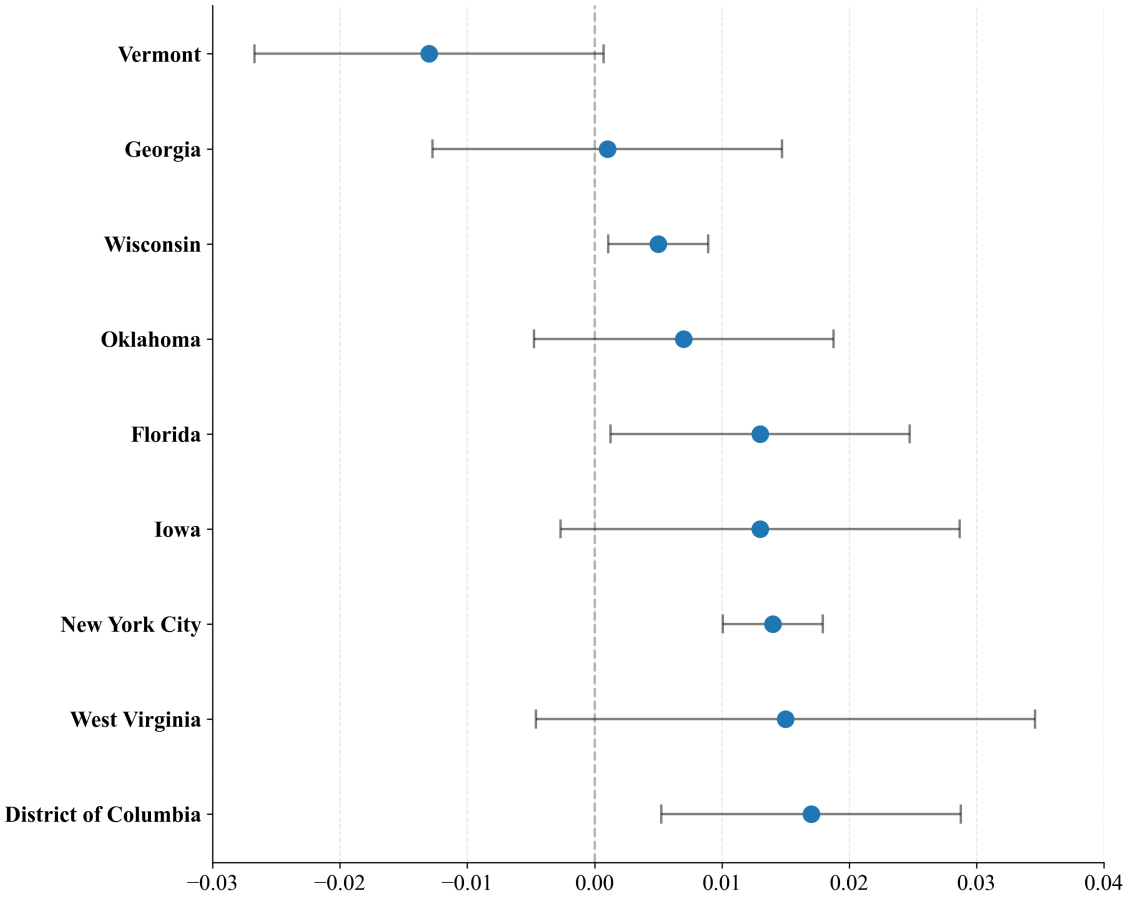
Notes: Summary statistics presented above use full samples (18 and over) from both ACS and CPS. Average of each variable are computed using perwt in ACS and asecwt in CPS (ASEC sample used).

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 over the age of 18. 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 region by year controls as well as the 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 include region by year controls as well as the policy specific controls discussed in Section 2.

Table A.3: 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, weighted by population size. Specifications that include controls include region-year fixed effects as well as policy specific controls discussed in Section 2.

Table A.4: 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. 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, weighted by population size. Where indicated, region by year as well as Bartik-style control using a shift-share approach