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PARENTS' EARNINGS AND THE RETURNS TO UNIVERSAL PRE-KINDERGARTEN

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ABSTRACT

This paper asks whether universal pre-kindergarten (UPK) raises parents' earnings and how much these earnings effects matter for evaluating the economic returns to UPK programs. Using a randomized lottery design, we estimate the effects of enrolling in a full-day UPK program in New Haven, Connecticut on parents' labor market outcomes as well as educational expenditures and children's academic performance. During children's pre-kindergarten years, UPK enrollment increases weekly childcare coverage by 11 hours. Enrollment has limited impacts on children's academic outcomes between kindergarten and 8th grade, likely due to a combination of rapid effect fadeout and substitution away from other programs of similar quality but with shorter days. In contrast, parents work more hours, and their earnings increase by 21.7%. Parents' earnings gains persist for at least six years after the end of pre-kindergarten. Excluding impacts on children, each dollar of net government expenditure yields $5.51 in after-tax benefits for families, almost entirely from parents' earnings gains. This return is large compared to other labor market policies. Conversely, excluding earnings gains for parents, each dollar of net government expenditure yields only $0.46 to $1.32 in benefits, lower than many other education and children's health interventions. We conclude that the economic returns to investing in UPK are high, largely because of full-day UPK's effectiveness as an active labor market policy.

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1 Introduction

Over the past thirty years, many US states and cities have implemented free, large-scale, non-means-tested pre-kindergarten programs—i.e., universal pre-kindergarten (UPK).[1] At the federal level, expanded investment in UPK is a topic of ongoing policy debate, with the Biden administration's American Families Plan calling for $200 billion in UPK funding (White House, 2021). The policy logic underlying UPK expansion is that parents may lack access to or underinvest in pre-kindergarten childcare and that the educational benefits for children and expanded labor market opportunities for parents combine to outweigh the costs of public provision (Kabay et al., 2020).

Despite the central role of labor market opportunity for parents in the case for UPK, evidence on how UPK affects parents' earnings is limited. Existing work relies on non-randomized research designs and typically cannot rule out null effects, though, as we discuss below, often with wide confidence bounds (Fitzpatrick, 2010; Cascio and Schanzenbach, 2013; Cascio, 2021). This contrasts with the research on how UPK affects children, which includes several randomized studies documenting UPK's educational effects over the short-, medium-, and long-run (Weiland et al., 2020; Durkin et al., 2022; Gray-Lobe et al., 2023). As a result, the overall cost-benefit proposition posed by UPK programs remains unclear, as does the distribution of benefits across parents and children. Filling in these blanks is crucial for choices about program design, expansion, and evaluation.

A central challenge facing researchers is that it is hard to link the data on UPK admissions lotteries that form the basis for randomized evaluations of children's outcomes to outcomes for parents. In this paper, we address that challenge head-on. We use data from admissions lotteries in a large full-day UPK program in New Haven, Connecticut to provide the first randomized evaluation of the effects of UPK enrollment on parents' labor market outcomes. We combine our estimates of effects for parents with estimates of educational effects for kids and net program costs to conduct cost-benefit analyses that incorporate outcomes for both parents and children.

Our main finding is that UPK is one of the most cost-effective active labor market policies ever evaluated in the US. Parents' earnings rise 21.7% (SE=6.6%) when their children enroll in UPK, with gains persisting for at least six years after children age out. This equates to about $5.50 in after-tax gains per $1 of net government costs, well above estimates of about $1 in gains per $1 in net costs for work-incentive programs like the EITC or recent cuts to top tax rates (Hendren and Sprung-Keyser, 2020). In contrast, estimates of educational gains for children from UPK enrollment max out

[1] Boston, New York, and Washington, DC have each adopted a policy of this type at the municipality level. Friedman-Krauss et al. (2023) review the policy environment and report that six states and Washington, DC have "mostly achieved" universal pre-kindergarten.

at about $1.30 per dollar in net costs, similar to findings from prior evaluations of UPK programs but below the per-dollar gains from other child-focused policies such as increased school spending or expanded college access (Cascio, 2023; Hendren and Sprung-Keyser, 2020). We conclude that the cost-benefit case for UPK is strong and rests primarily on UPK's efficacy as an active labor market policy.

The New Haven Public Schools (NHPS) have offered full-day public pre-kindergarten to three- and four-year-olds since the late 1990s. These programs enroll students from New Haven and the surrounding suburbs and are universal in that they are not means-tested. In practice, however, they are often slot-constrained, with spots allocated through a centralized assignment mechanism that combines coarse priority groups with lottery tiebreakers. The lottery-generated variation in program access is the linchpin of our empirical design.

The NHPS pre-kindergarten programs share key features with programs in other cities. For example, Boston, New York, and Washington, DC each use lottery tiebreakers to allocate slots in universal pre-kindergarten programs. As in other cities, New Haven's universal program operates alongside a wide variety of other pre-kindergarten options, including public means-tested programs such as Head Start and private providers that offer subsidized and unsubsidized slots.

Our analysis relies on a set of linked datasets that together help us capture the effects of enrollment in universal pre-kindergarten on parent, child, and cost outcomes. We start with records of NHPS pre-kindergarten admissions lotteries from 2003 through 2022. We link children in this dataset to state data on school enrollment and achievement. In addition, we link the *parents* listed on the admissions forms to earnings and employment data from state Unemployment Insurance (UI) records. Finally, we survey parents of past applicants. The survey provides information on pre-kindergarten enrollment at institutions not recorded in state data, data on childcare costs, and information on parental labor supply not recorded in UI records, such as hours of work.

We report three broad sets of results. The first set describes the kinds of programs children substitute away from when they enroll in UPK, and how this affects total hours of childcare coverage. We combine administrative and survey data to measure enrollment in both subsidized and non-subsidized programs.

We find that enrolling in UPK affects the intensive margin of childcare usage. UPK enrollment does not raise overall childcare enrollment, as nearly all students substitute away from some other option. High rates of substitution are not unique to New Haven: Weiland et al. (2020) report similar findings for Boston's UPK program. Despite the absence of *enrollment* effects, we find large effects on *hours* of childcare. Children enrolling in UPK get an average of 11.3 more hours of childcare coverage per week. This is because NHPS UPK programs offered a ten-hour day for most of the period we

study, supplementing a 6.5 hour academic day with before- and after-care, similar to extended-day programs in other cities (Boston Public Schools, 2024).

Our second set of results considers how UPK enrollment affects children's educational outcomes. We find little evidence that UPK affects children's academic performance or behavior in the medium run. Effects on test scores, attendance, and grade retention in kindergarten through eighth grade do not statistically differ from zero and are generally small in economic terms. These findings mirror results from past randomized evaluations of UPK programs in which early gains fade out after pre-kindergarten and re-emerge later on (Lipsey et al., 2018; Durkin et al., 2022; Gray-Lobe et al., 2023).

Our third set of results describes the impacts of UPK enrollment on parents' earnings and labor supply. We find that enrolling a child in a UPK program raises parent earnings by 21.7% (SE=6.6%), or $5,461 per year, during the one- or two-year period when the child is of pre-kindergarten age. These gains coincide with increases in hours worked. Using survey data, we show that UPK enrollment causes parents to work 12.8 (SE=4.3) more hours per week in the year following application. In both the administrative and survey data, we find high baseline rates of labor force participation and little evidence that children's UPK enrollment raises these rates further.

Impacts on parents' earnings persist at similar levels for six years after pre-kindergarten ends before fading away. The average earnings effect over this period is 20.9% (SE=7.9%), or $6,469 per year. These gains appear not to be driven by increased labor supply, as hours effects fall to zero after pre-kindergarten.

Our interpretation is that the long-run labor market effects of UPK are likely attributable to returns to experience or job continuity, as emphasized in Goldin (2014) or Bertrand et al. (2010). We find that children's UPK enrollment reduces the likelihood that parents experience short-term career gaps or switch industries during the pre-kindergarten period, while increasing the chances that parents hold a single high-earning job. Qualitative reports lend further support to this idea: survey respondents often described how UPK helped them maintain their career paths, keep normal work hours, or work more effectively.

We bring together our analyses of effects on parents, children, and program costs to construct cost-benefit calculations for the program. We convert children's test score gains into dollars and discount all effects back to the time of application. We then apply the MVPF framework of Hendren and Sprung-Keyser (2020).

The high-level story here is that parents' earnings gains are much larger than projected earnings gains for children or increases in program costs. The present discounted value of the after-tax gain in parent's earnings is equal to $33,456. This value exceeds projected gains in after-tax earnings for children, whether we base these projections on our own estimates of kindergarten score effects ($1,728), Lipsey et al. (2018)'s estimated

pre-kindergarten score gains of 0.4σ from a randomized evaluation of Tennessee's UPK program ($11,312), or Gray-Lobe et al. (2023)'s estimates of the Boston UPK program's effects on college enrollment ($9,886). It also exceeds the increase in education costs associated with UPK enrollment. After subtracting out savings from substitution across pre-kindergarten programs and the additional tax revenues from increases in parent income, the increase in public expenditures from UPK enrollment is $7,020.

We estimate the parent-only MVPF of the UPK program at 5.51. That is, families realize $5.51 (90% CI= $[1.98, 25.40]$) in after-tax benefits per dollar of government expenditure, even if we assume that children do not experience any long-run earnings gains. This value is high relative to other policies that aim to promote adult labor market activity. For example, Hendren and Sprung-Keyser (2020) estimate MVPFs for EITC policies and 2000s-era decreases in top tax rates of around 1.

Our estimates of the child-only MVPF of the UPK program (i.e., excluding gains in parents' earnings from the calculation) range from 0.46 (90% CI= $[0.19, 0.77]$) to 1.32 (90% CI= $[1.07, 1.60]$), depending on which estimate of children's educational gains we use. These values are similar to Cascio (2023), which estimates the MVPF of UPK enrollment at 1.96. All of these estimates are below those reported in Hendren and Sprung-Keyser (2020) for a variety of other policy interventions targeted at children, including increased school spending through school finance reforms, expanded health insurance for children, and expanded college access, each of which have infinite MVPFs.

Combining gains for parents and children yields overall MVPF estimates ranging from 6.13 to 11.92, again depending on how we project earnings gains for children. We conclude that UPK offers a high return on public investment overall, and particularly stands out relative to other active labor market policies. Understanding how UPK affects earnings for adults is crucial for evaluating social benefits overall. The child-only case for UPK is less compelling, both because benefits for children are smaller in dollar terms than those for adults and because the menu of alternate options available to policymakers aiming to improve outcomes for children is more attractive.

Could means-testing improve the targeting of free pre-kindergarten programs while maintaining the efficiency of public spending? To explore this, we split our analysis by terciles of pre-enrollment neighborhood income. Our first finding here is to describe the applicant pool. Because the UPK program (like the broader New Haven school system and many other urban districts) serves a relatively low-income population, the top tercile of the applicant population consists mostly of people with earnings near the population median. Turning to treatment effects, we find that returns on government investment are largest in the top two terciles and close to zero in the bottom tercile. Mechanically, the main reason for this is that earnings gains in dollars are larger when baseline income is higher. Economically, it is consistent with the observation that the

career returns to longer work hours are higher for more skilled workers (Kuhn and Lozano, 2008; Cortés and Pan, 2019).

Another important result from this analysis is that educational gains for children, while approximately zero in the full sample, are large in the middle tercile. UPK enrollment raises kindergarten scores by 0.30σ in this group. These effects fade out in middle grades before reappearing in grade 8. These findings suggest that UPK may help address a "doughnut hole" in quality childcare. Lower-middle-income families have less access to means-tested programs like Head Start than low-income families, and less access to high-quality private programs than higher-income families.

We conclude with a discussion of childcare markets and policy design. Our cost-benefit analysis raises a question: if the benefits of UPK enrollment exceed the costs of provision, why don't private providers offer similar programs? Our finding that childcare investments raise parent earnings over the long run suggests that one plausible answer is borrowing constraints. We show that earnings gains realized during pre-kindergarten are not large enough to pay for the additional costs incurred. Individuals would be happy to pay by borrowing against future earnings at market rates, but banks do not issue loans for childcare (US Department of the Treasury, 2021). Long-run earnings effects thus combine with missing credit markets to raise the value of public childcare programs that allow parents to work full-time.

We contribute to several strands of literature. Most directly, we build on research that seeks to evaluate the labor market effects of other UPK programs (Fitzpatrick, 2010; Cascio and Schanzenbach, 2013). The stylized conclusion from these papers, which use non-randomized, quasi-experimental designs, has been that there is little evidence that UPK affects mothers' labor supply (Cascio, 2021).

Our findings argue for a reassessment of this conclusion. However, they do not conflict with previous work once one accounts for statistical uncertainty. Fitzpatrick (2010), which Cascio (2021) cites as the most convincing available evidence, estimates an intent-to-treat effect of UPK eligibility on mother's earnings of $332 with a standard error of $578. The first stage effect of eligibility on enrollment is 0.072. Scaling by the first stage and converting to 2015 dollars (for consistency with our unit of measure) gives an IV estimate of the effect of pre-kindergarten enrollment on annual earnings of $6,531, with a standard error of $11,371. This point estimate is larger than what we find for the effects of UPK enrollment, and the 95% CIs include large positive and negative values. Similarly, Cascio and Schanzenbach (2013) estimate *extensive* margin labor supply effects. Their finding of a null effect matches our finding that labor market gains operate mostly on the intensive margin. Our interpretation is that the status quo ante was one of substantial uncertainty, which we help resolve.

Our work also builds on papers studying the labor supply effects of means-tested

and demonstration pre-kindergarten programs, as well as public kindergarten programs (Gelbach, 2002; Cascio, 2009; Fitzpatrick, 2012; Sabol and Chase-Lansdale, 2015; Chaparro et al., 2020; Schiman, 2022; Wikle and Wilson, 2023). The contemporary UPK context is fundamentally different for two main reasons. First, means-tested programs target a population that, by definition, has different labor market opportunities and outcomes than those eligible for UPK. Second, evaluations of demonstration and kindergarten programs are based on data from the 1980s through the early 2000s. Childcare costs rose 89% in real terms from 1995 to 2016 (Swenson and Burgess Simms, 2021), dramatically raising the value of childcare access. Our evaluation of a contemporary UPK program links this strand of literature to a pressing policy debate.

We also contribute to the literature on the educational effects and net costs of UPK. Our findings on test scores echo results from randomized evaluations of UPK programs in Boston, Tennessee, and Georgia, pushing this vein of work further towards consensus (Lipsey et al., 2018; Weiland et al., 2020; Gray-Lobe et al., 2023; Woodyard et al., 2023; Bruhn and Emick, 2023). We augment the cost side of the UPK program evaluation by using our survey and administrative data to consider not just the program costs of UPK (Kabay et al., 2020) but costs relative to counterfactual programming. Our analysis of how both achievement and cost effects depend on substitution between UPK and other programs brings insights from Kline and Walters (2016)'s analysis of Head Start to a UPK setting.

2 Institutions

Our study focuses on free, non-means-tested (i.e., untargeted) pre-kindergarten programs in the New Haven Public Schools. New Haven is a low-income, majority-minority school district. As reported in Table 1, 83% of students in NHPS in 2022-23 were Black or Hispanic, compared to 43% in public schools statewide. 66% of NHPS students were eligible for free- or reduced-price lunch, compared to 42% statewide.

New Haven's UPK programs offer two grade levels, PK3 and PK4, serving three- and four-year-olds, respectively. There are nine total sites, each of which is part of an elementary school that runs through eighth grade. As shown in Panel (a) of Figure 1, enrollment in UPK programs grew rapidly during the 2000s before reaching its present size of around 700 students per year in 2014.

Eight of the nine UPK sites are part of NHPS' interdistrict magnet choice program. This program began after a 1996 state court ruling held that requiring students to attend schools in their town of residence contributed to the "racial, economic, and ethnic isolation" of low-income and minority students and therefore violated the state constitution (Office of Legislative Research, 1998). As a result, any student in the

state is eligible to enroll in the NHPS magnet program. In exchange, NHPS receives subsidies for capital and operating costs as long as schools meet or progress toward state-set targets for out-of-district enrollment and racial integration (Bifulco et al., 2009; State of Connecticut, 2017). The ninth site is a local charter school open only to New Haven residents. The charter opened in 2014.

The UPK programs offer high-quality academic programming. Each of the eight magnet-affiliated UPK programs provides 6.5 hours of educational curriculum with a certified teacher during the school day (Bonanno, 2023). Maintaining a high-quality curriculum is one of the requirements of the granting process through which schools are established (State of Connecticut, 1999). The charter school follows a Montessori curriculum, also with a 6.5 hour academic day (Elm City Montessori, 2024).

In addition to their academic curriculum, the UPK programs provide substantial childcare coverage. Prior to the 2021-22 academic year, each magnet school offered before-care starting at 7:30am and after-care until 5:30pm, for a total of 10 hours of free childcare each day.[2] The charter also offers wraparound care starting at 7:30am and ending at 5:30pm. Unlike the magnet programs, the charter charges a modest fee for wraparound care.

NHPS' UPK programs are typically oversubscribed. Panel (b) of Figure 1 reports the ratio of open slots to the number of applicants separately for PK3 and PK4 grades over the years 2003 through 2021. Between 1.5 and 4 slots were available for every 10 PK3 applicants in each application cycle from 2003 through 2020. Relative demand slackened in 2021, when NHPS schools were still in the process of reopening after the Covid-19 pandemic. Oversubscription is even more pronounced in PK4, because most PK4 classroom spots are filled by rising PK3 students admitted the previous year.

NHPS resolves excess demand using a centralized assignment mechanism that combines coarse neighborhood, zip code, and sibling priorities with random lottery tiebreakers. Students typically apply in February and learn about their placement outcomes in early spring. Though run simultaneously, the assignment processes for students from New Haven and those from other towns are fully separate, with separate capacities and no competition between students. The details of this process have changed over time (Peak, 2019; Akbarpour et al., 2022), but the basic structure has been in place since the beginning of the interdistrict magnet system in the late 1990s (Davidoff-Gore, 2017, p. 48). Online Appendix C describes the NHPS school assignment mechanism over the period we study. The lottery tiebreakers generate the exogenous variation in school assignments that we use to evaluate the effects of UPK access.

Though many different pre-kindergarten options are available to families in New Haven, the UPK programs we study are the only choice that offers free full-day care

[2]Several schools reduced before- and after-care offerings starting with the 2021-22 school year.

without a means test. Other major options for three- and four-year-olds in the area are as follows. First, several local centers offer Head Start programming. These programs are means-tested and provided mostly part-day slots in 2013 and earlier before switching to mostly full-time slots of at least six hours per day thereafter.[3] NHPS runs some of these centers at school facilities, while others are run by third-party providers. Second, NHPS offers subsidized childcare through a state-funded "School Readiness" grant. School Readiness programs are run by NHPS but are either not full-day, not free, or both (NHPS Office of Early Childhoood, 2024). The School Readiness program also provides subsidized spots at private providers. Third, the state runs a program called "Care 4 Kids" that provides means-tested vouchers for families to use at private pre-kindergartens.[4] Fourth, the state offers a variety of smaller subsidized programs, often payable to daycare centers enrolling lower-income students. Fifth, families may choose to enroll their children in private pre-kindergarten programs without any subsidy. Online Appendix D details the different program types and the data available for each.

Panel (c) of Figure 1 knits together several administrative datasets to describe the market for subsidized pre-kindergarten in New Haven from 2006 through 2021. The data covers students enrolled in New Haven pre-kindergarten programs for grades PK3 and PK4.[5] We report counts of unique student enrollments by program type.

The first point to take away from Panel (c) of Figure 1 is that it is surprisingly hard to count subsidized pre-kindergarten enrollment using administrative records. While we have complete data on enrollment in UPK programs, School Readiness programs, and Care 4 Kids programs from 2006 through 2021, state- and district-level datasets do not include student records from the major New Haven Head Start programs after 2018. We estimate counts for 2019 and later using aggregate data on seat counts from federal Office of Head Start records. Similarly, we observe records of enrollment in non-Care 4 Kids subsidized programs from 2013 to 2019, but not in other years. These programs make up the "other" category reported in the graph. The limited availability of microdata contrasts with the public K-12 setting and highlights the importance of augmenting administrative data with survey reports.

The second point to take away is that NHPS UPK programs account for a large share of pre-kindergarten enrollment for New Haven students. The UPK programs account for 22% of pre-kindergarten enrollment during the 2013-2018 period, the years for which our administrative records are richest. They account for 64% as much enrollment as

[3]Source: authors' calculations from Office of Head Start data.

[4]In addition to income requirements, Care 4 Kids requires the parent to be working or in an approved educational program.

[5]We do not observe program location for Care 4 Kids enrollees, so we classify these students by their town of residence.

Head Start and 79% as much enrollment as Care 4 Kids.

UPK programs in New Haven share key features with large-scale programs in other cities. For example, Boston offers free, full-day, capacity constrained, untargeted public pre-kindergarten programming, with excess demand resolved through a centralized assignment system (Gray-Lobe et al., 2023). At the time of this writing, many of the Boston UPK sites offer before- and after-care that extend childcare coverage from 7:30am to 4:35pm, similar to what we observe in New Haven (Boston Public Schools, 2024). New York City also offers free, untargeted pre-kindergarten programming, but does not guarantee spots at neighborhood schools and resolves excess demand for popular schools using centralized systems (New York City Public Schools, 2023; Shapiro, 2023). Similarly, many states with universal kindergarten policies on the books are in practice slot-constrained and do not treat UPK as an entitlement (Stanford, 2023). For example, New York and Georgia both fit this description (New York State Department of Education, 2024; Childcare Network, 2024).

The market conditions in which New Haven's UPK programs operate are also similar to those in other cities. As in New Haven, UPK programs in New York and Boston share a market with private and means-tested options. The average weekly price for private center-based care in the New Haven area was $255 in 2018, well above the national average of $197, but below an average price of $291 in the Boston area and similar to major cities such as Los Angeles and Chicago. The price of center-based care in New Haven rose steadily over the 2000s, paralleling nationwide trends as well as trends in other cities. See Appendix B for more details.

3 Data

3.1 NHPS admissions and enrollment data

Application and admissions records for the NHPS UPK programs form the center of our analysis. These records span the years 2003 through 2022. For each year in this range, we observe students' full choice applications, the administrative rules and student priorities used to process the applications, and school placement outcomes. Applications also include demographic information such as race/ethnicity, gender, and age. We supplement the admissions records with NHPS data on enrollment in district schools, including the UPK programs. Recall from the previous section that NHPS UPK programs began in the late 1990s, so the program was well-established and running at scale several years before our first cohort of applicants.

The first column of Panel A of Table 2 describes the sample of UPK applicants. We observe 18,795 applications from 16,037 individuals. 41.8% of applicants are Black and 28.5% are Hispanic. 48% apply to grade PK4 and 52% to PK3. On average, applicants

reside in Census block groups where the median household income is $59,708. 25.1% of applicants are assigned to a UPK program, and 26.5% go on to enroll in a program that year. Our UPK assignment variable measures "initial offers" (De Chaisemartin and Behaghel, 2020) so we expect two-sided noncompliance, with some non-offered students attending UPK programs and some offered students declining spots.

3.2 Public school enrollment and achievement

We worked with the Connecticut State Department of Education (CSDE) and NHPS to link UPK applicant records to statewide data on student enrollment and academic achievement using name and date of birth. This link, conducted by state personnel, allows us to observe outcomes for NHPS UPK applicants who enroll in any Connecticut public school. The CSDE data cover the years 2006 through 2022. We observe the oldest UPK applicants in our data through roughly age 23, and observe many applicants through middle school and into high school. We exclude the 2005, 2008, and 2022 UPK application cohorts from our analysis of linked state records because we did not have these data when the merge was conducted. The 2022 application process took place after the merge, and we did not recover historical records of the 2005 and 2008 application processes until after the merge.

In addition to enrollment records, the CSDE data include data on mandated state assessments conducted in kindergarten through grade 8. Online Appendix E describes the achievement measures we use.

In addition to the data linked to NHPS UPK applicants, our state records include each of these fields for a comparison set of students. This set consists of all students enrolled in any public pre-kindergarten program in New Haven County over the 2006-2021 period. While non-applicant students in this sample are not part of our main analysis of the NHPS UPK programs, we do use their scores on achievement tests when creating the standardized score measures that are the focus of our achievement analysis. Online Appendix Table A.1 presents descriptive statistics for this population.

Column 2 of Table 2 describes the sample of applications we match to state data. Conditional on attempting a merge (i.e., excluding the 2005, 2008, and 2022 application years), we match 91% of observations, for a total of 16,485 matched applications and 13,917 matched individuals. Demographics, assignment rates, and enrollment rates for this group are essentially identical to those for the full set of UPK applicants. As reported in Panel C of Table 2, applicants score 0.123 σ higher than the full sample of pre-kindergarten enrollees in New Haven County on their kindergarten entrance assessment; this gap diminishes to 0.069 σ and 0.038 σ for grade 3 and grade 8 scores, respectively.

3.3 Parent earnings data

We worked with NHPS and the Connecticut Department of Labor (DOL) to link parent records reported on the NHPS UPK choice applications to Connecticut Unemployment Insurance records for the years 1999 through 2022. This match was conducted by state personnel using name and address information. Address information on the state side come from the Department of Motor Vehicles. Names are not always unique and addresses change over time, so the match to state data for parents is more challenging than the match for children. Our approach is to keep only unique matches and to conduct extensive checks of the quality and representativeness of matched data. We deflate earnings to 2015 dollars and topcode at the 99th percentile within bins defined by the year of application and the year-by-quarter in which earnings are reported.[6]

Panel D of Table 2 describes the parent data submitted to NHPS. All applicants must list at least one parent or guardian. In 2013 and earlier, the application process was done mainly on paper and applications rarely listed more than one contact. In 2014 and later, the process played out primarily online. Applicants were prompted to list multiple contacts and describe the relationship between the contact and the student (e.g., "father" or "mother"). In the full dataset, 33% of applicants list two parents on their application. After 2013, that share rises to 56%. Where one contact is listed, that contact is usually the mother. In the post-2013 data (for which we observe the student-contact relationship), 69% of listed contacts are mothers. Of the 16,037 UPK applicants, we match 9729, or 61%, to earnings records for at least one parent. As shown in Online Appendix Figure A.1, match rates to earnings are higher for more recent cohorts of applicants, rising from 42% in 2003 to 82% in 2021.

Column 3 of Table 2 reports descriptive statistics for applicants and parents in the parent earnings sample. This sample is similar to the full sample in terms of child age, gender, test scores, and block group income. The main difference between the two samples is that students in the parent earnings sample are somewhat less likely to be Hispanic (21.9% vs. 28.5% in the state sample).

Labor force participation rates around the time of application are high in our sample. 86% of parents report positive income in the baseline pre-application period, which we define as the two academic years prior to the UPK application year. We do not include the year of application in the pre-period because knowledge of future access to pre-kindergarten may shape labor supply choices in advance of enrollment. Mean baseline individual earnings is $25,157.

Two descriptive exercises suggest our merge procedure generates accurate matches. The first explores how earnings evolve for male and female parents around the time of childbirth. Previous research shows that earnings for mothers drop relative to earnings

[6]Omitting the topcoding step does not change our findings.

for fathers following the birth of a child and do not fully recover (Kleven et al., 2019). As reported in panels (a) and (b) of Figure 2, we observe this pattern in our data. Earnings for female parents fall by $6,000 relative to male parents in the academic year the applicant is born, 19% of the mean two years prior to birth. This gap persists for at least five years.

The second exercise compares individual earnings reports measured in the two years prior to application to median household income in the Census block group where the applicant lives. If our merge procedure generates accurate matches, we would expect to see a strong positive relationship since people with higher incomes tend to live in neighborhoods with higher household incomes. This is what we see. As reported in panel (c) of Figure 2, a $1,000 increase in median neighborhood household income is associated with a $360 (SE=$15) increase in individual earnings.

Other than the accuracy of successful matches, the main concern related to the parent merge is that matches may be imbalanced with respect to UPK assignment. We discuss this concern in section 4 and find no evidence that matching to the earnings sample is related to placement in a UPK program.

3.4 Parent surveys

We supplement the administrative data with a survey of parents of past applicants. The survey provides a window into several outcomes that cannot be observed in administrative data. These outcomes come in two types. The first is enrollment and payment information not covered in state sources, including unsubsidized private enrollment, non-CSDE Head Start enrollment, and enrollment-related objects such as out-of-pocket costs. The second type is information on hours worked, which is not recorded in UI data.

We worked with NHPS and NORC to survey the parents of past pre-kindergarten applicants. We fielded the survey between May and November of 2023. Using contact information provided on the application form, we emailed the parents of all past applicants, with phone follow-up for non-completers or those without email addresses. To maximize the statistical power of experimental analyses using survey data subject to our budget constraint, we focused phone follow-up on parents whose children faced an interior probability of admission based on their application and priorities. Online Appendix F describes survey procedures in detail.

Out of all the parents we contacted, 5.7% responded to our survey, yielding 966 survey responses. Excluding applicants to Covid-affected cohorts for whom in-person UPK was not available and late applicants not subject to randomization (see section 4.1 below) leaves us with 840 responses in our analysis sample and a response rate of 5.3%. This overall rate masks a sharp increase over time. As shown in Online Appendix

Figure A.2, response rates rise for recent cohorts, reaching 23% for 2022 applicants. For those applying before 2014, response rates are less than 3%. This is likely because contact information provided on old applications is no longer current or because past applicants become less likely to respond to school-choice-related communications as they move farther from the application date.

As reported in column 4 of Table 2, survey takers tend to come from higher SES backgrounds than the applicant population as a whole. They are less likely to be Black (30.5% vs. 41.8% in the full sample), and more likely to be white (31.4% vs. 21.7% in the full sample). They live in Census block groups where median household income is about 10% higher than in the full sample, and they are more likely to be assigned to a school (41.9% vs. 27.1%) and to enroll (54.6% vs. 26.5%). These differences in SES and admissions rates are consistent with the observation that survey respondents are disproportionately drawn from wealthier suburbs.

Survey respondents are mostly women and report high rates of labor force participation. 90% of respondents are women. 69.6% report working full time in the year following their child's pre-kindergarten application and 16.4% report working part time, for an overall labor force participation rate of 86%—nearly identical to what we observe at baseline in the administrative records. Respondents who work report spending an average of 33.5 hours per week on the job.

Several tests support the idea that survey participants provided accurate responses related to their schooling choices and earnings. First, reported enrollment from the survey is consistent with the enrollment choices that we observe in administrative records. Overall, survey reports of UPK enrollment match administrative data in 89.6% of cases in the pre-2021 application years, for which we can match the survey to administrative records. Of respondents who report that their child did not enroll in a UPK program, we observe UPK enrollment in only 2.4% of cases, for a 98.6% accuracy rate. Of respondents who report their child enrolled in a UPK program in the year following application, we confirm UPK enrollment for 82.4%. We regard this number as high given the many potential sources of confusion for parents about which program their children were enrolled in. For example, of the 17.6% of "false positive" UPK enrollees, 28.3% enrolled in a non-UPK school readiness program physically located in one of the magnet schools or enrolled in UPK in a different year than the one asked about in the survey. Another 34.8% of these students enrolled at some other form of non-UPK pre-kindergarten offered by NHPS. Confusion on the part of parents about the administrative classification of the pre-kindergarten program their children attended seems reasonable. If one re-classifies cases of reasonable confusion as correct, the accuracy rate rises from 89.6% to 94.6%.

Second, the reported pre-kindergarten program type and out-of-pocket costs vary in

intuitive ways across the income distribution. Panel (a) of Figure 3 reports the kinds of pre-kindergarten programs that children not enrolling in UPK programs attend, split by quintile of block group median income. The Head Start share of non-UPK enrollees declines from 35.3% in the bottom quintile to 7.4% at the top, while the private pre-kindergarten share grows from 39.2% to 80.9%. The share of children receiving home care also declines, from 19.6% in the bottom quintile to 3.2% in the top quintile. Panel (b) of Figure 3 shows that out-of-pocket costs grow with block group income, rising from $286 per month in the bottom quintile to $942 per month in the top quintile. As shown in Panel (c), reported childcare costs vary widely by program type, with families whose children enroll in Head Start reporting $208 in costs per month compared to $804 per month for parents whose children enroll in a non-public school, non-Headstart paid program.

Third, and finally, we compare survey reports of household income to median household income in the block groups where respondents lived at the time of their UPK application. As in our comparison of administrative earnings data to block group-level income data, we expect the relationship to be strong and positive, but not one-to-one. As reported in panel (d) of Figure 2, this is what we find. A $1,000 increase in neighborhood median household income is associated with a $560 increase (SE=$54) in reported household income.

As was the case with parent earnings records, a possible concern about the survey data beyond the quality of observed responses is that survey response may be correlated with placement in a UPK program. We discuss this in section 4.

3.5 Early childhood education outside of public schools

In addition to our link to the CSDE public school records, we link our data on NHPS applicants to Connecticut Office of Early Childhood (OEC) records on participation in subsidized private early childhood education programs. The data allow us to observe children who enroll in private childcare programs using state-provided subsidies.

The time span of coverage varies by program type. We observe data on Care 4 Kids voucher use from 2003 through 2022. We observe enrollment data for all major OEC-funded programs from 2016-2018, with more limited data from 2013-15 and 2019. As reported in Panel (c) of Figure 1, these programs together account for about 22% of observed enrollment for lottery applicants over the 2016-18 period. In some of our analyses of outside options we restrict to cohorts who enroll in pre-kindergarten in 2016-18, when our administrative records are most complete.

The administrative early childhood education records have two limitations. The first is that they do not cover students who enroll in private pre-kindergarten programs without state subsidies. The second is that although some Head Start programs are

administered through public schools or other state-level organizations and are therefore included in our public school records, other major New Haven-area Head Start providers are private organizations supported by federal funds. These do not appear in state records of public or subsidized private early childhood enrollment. One of the reasons we conduct a parent survey is to address these issues.

4 Results

4.1 Empirical design

We estimate instrumental variables (IV) specifications of the form

$$\begin{aligned} Y_i &= \beta D_i + \sum_p \alpha_p 1[P_i = p] + X_i'\Gamma + \epsilon_i \\ D_i &= \delta Z_i + \sum_p \rho_p 1[P_i = p] + X_i'\pi + \eta_i. \end{aligned} \tag{1}$$

Here, Y_i is an outcome of interest, such as children's academic achievement. D_i, the endogenous regressor, is an indicator equal to one if a child enrolls in a UPK program. We instrument for D_i using Z_i, an indicator equal to one if an applicant is assigned to a UPK program in the choice process.

The key control variables here are the P_i, a set of indicators for each value (in bins of size 0.01) of i's assignment propensity, interacted with application year and grade indicators. These blocks identify students facing the same level of assignment risk for the same grade-level treatment in the same year. We obtain assignment propensities through simulation, re-running the assignment algorithm in each grade and year with new random lottery draws. The use of controls for assignment propensity follows Abdulkadiroglu et al. (2017) and Rosenbaum and Rubin (1983), who show that this procedure isolates the random variation in treatment assignment. We also include controls for race/ethnicity, gender, child age at the start of the school year, and measures of neighborhood block group attributes from the ACS, denoted X_i.[7]

We modify this approach across outcome types and datasets to accommodate differences in sample size, data structure, and data availability. When estimating specifications where the outcome is parent earnings, we use panel data identified by parent, application, and academic year. We add interactions between the P_i as defined above and indicators for years elapsed since the time of application. We include controls for baseline income, which we define as the average value of own earnings in the two years prior to the choice process, and use two-way clustered standard errors at the level of

[7]Online appendix Table A.2 provides a list of these covariates.

the application (to account for possible correlations between parents of the same child over time) and at the level of the parent (to account for cases where the same parent shows up in multiple child records). We estimate specifications using data for various time windows following the application year.

When estimating IV specifications in survey data, we use a recentered instrument approach (Borusyak and Hull, 2023). This amounts to dropping the indicators for assignment propensities from the control set and instrumenting with $\tilde{Z}_i = Z_i - P_i$. Like controlling directly for the assignment propensity, recentering isolates random variation in treatment assignment. The benefit of recentering in survey specifications is that we do not need to estimate many fixed effects in the much smaller survey sample.

Several sample selection procedures are important to highlight. First, we drop late applications because they are not considered in the lottery process. Second, we impose application cohort restrictions to limit the effects of Covid-19 school closures on our analysis. NHPS closed its schools in March 2020 and did not reopen elementary schools for hybrid instruction until January 2021. This re-opening provided pre-kindergarten students with up to four days of in-person learning per week (Hays, 2021).[8] District schools resumed their normal schedule in Fall 2021. We therefore exclude all 2020 applicants and 2019 PK3 applicants from our parental labor market specifications.[9]

4.2 Balance and first stage

We begin our empirical analysis by validating the randomized research design. Panel A of Table 3 tests that predetermined student attributes are balanced with respect to lottery assignment. We estimate reduced-form versions of Equation 1, with an indicator for lottery assignment to a UPK program as the independent variable of interest and the covariates listed in table rows as the outcomes. The first two columns report control complier and full control group means for the dependent variables.

A straightforward comparison of assigned and unassigned students reveals substantial demographic differences. Column 3 reports results from a regression *excluding* the controls for propensity scores. We see substantial imbalance in predetermined covariates when these controls are omitted. Students receiving UPK offers are less likely to be female. They are more likely to be white and to live in neighborhoods with higher median household income. Online Appendix Table A.2 reports balance test results for

[8] Public schools reopened more slowly for older students, with hybrid high school instruction not beginning until April 2021 (Zahn, 2021).

[9] The 2020 application cohort submitted their rank lists in February 2020, just prior to the shutdown, and had to make enrollment decisions in Spring 2020 during the early days of the shutdown when re-opening plans were uncertain. Those who enrolled in NHPS UPK then faced either fully remote or part-time instruction during their first UPK year. The 2019 PK3 application cohort was exposed to the shutdown in the second half of the first year, and then to remote or hybrid school for all of their second year.

additional neighborhood characteristics. A joint test rejects the null that the coefficient on UPK assignment is zero in each of these specifications ($p < 0.001$).

As expected, adding the propensity score indicators as controls eliminates these differences. Column 4 reports these results. Differences in race and neighborhood income drop sharply and become statistically insignificant at conventional levels. A modest difference in female share remains, but we fail to reject the joint null that all coefficients are zero ($p = 0.522$). We regard this as strong evidence that, as expected, our approach succeeds in isolating the random component of UPK assignment.

We find no evidence that school assignments affect downstream match rates to other data sources. Panel B of Table 3 reports estimates from reduced-form specifications in which the outcomes are indicators for successful matches between the lottery data and each of these datasets. 89.5% of control compliers match to state student data, 76% match to parent earnings data, and 5.3% match to survey data. As reported in column 4, there is no evidence of differential match for assigned students into the state enrollment or parents' earnings datasets once we control for propensity scores. Placed students are 1.2 percentage points more likely to match to survey data. This effect is marginally statistically significant ($p = 0.055$). We cannot reject the joint null of no selection into any of the state, earnings or survey datasets ($p = 0.133$).

Consistent with our finding of no selection into match, we see no evidence of imbalance on predetermined covariates within our matched samples. Columns 5-7 of Panel A report balance tests within the state, parent earnings, and survey samples, respectively. In each case, we fail to reject the null that coefficients from all specifications are zero. Notably, the balance tests we conduct in our parent earnings sample add a set of measures of the parent's pre-assignment income. We find no evidence of imbalance on income in dollars, log income, or an indicator for any positive income.

The first stage effects of assignment on UPK enrollment are strong. As reported in Panel C of Table 3, assignment raises the share of students enrolling in a UPK program by 0.389 (SE=0.013) in the full sample, conditional on propensity score. The first-stage F-statistic is 842.3, well above rule-of-thumb cutoffs required for conventional statistical inference (Lee et al., 2022). First stage effects are similar in the state and parent match samples. The first stage in the smaller survey sample is 0.409 (SE=0.053), with an F-statistic of 63.0.

UPK assignment increases the number of years students attend a UPK program by 0.576 years in the full sample. This coefficient is 48% higher than the first-stage enrollment effect, consistent with the observation from Table 2 that roughly half of the applications are to PK3 programs that give students the option to stay in UPK for two years once they enroll.

4.3 The effects of UPK on childcare

Our survey and administrative data let us describe how UPK enrollment shifts children across different childcare options and what this means for families' childcare experiences. Our approach is to run instrumental variables regressions of the form shown in Equation 1, taking UPK enrollment as the endogenous regressor and various childcare outcomes as dependent variables. We present these results in Table 4.

We first consider the substitution patterns observed in administrative data sources. Column 3 of Table 4 reports results for all application cohorts matched to state records. These data allow us to observe substitution into public programs anywhere in the state as well as those who use public subsidies at private programs. IV specification using these records indicate that 14.3% of students enrolling in UPK substitute away from Head Start programs, while 7.1% substitute away from school readiness, 5% from Care 4 Kids, and 17% from any other pre-k appearing in SDE or OEC records. Overall, UPK enrollment raises the rate at which students enroll in any observed program by 39.0%, meaning that 62.0% of compliers would otherwise attend some form of public or subsidized care.

Column 4 of Table 4 restricts the sample to the years for which our administrative coverage of subsidized private programs and Head Start is most complete. We observe slightly higher rates of substitution in this sample. UPK enrollment raises the share of students attending any subsidized program by 36.2%.[10]

Administrative data exclude private non-subsidized enrollment and also omit major Head Start programs in some years. Bringing in survey measures allows us to account for this gap and pushes rates of substitution even higher. As reported in column 5 of Table 4, 62.3% of compliers with the UPK treatment offer substitute away from a private or paid pre-kindergarten program. Combining this number with the rates of substitution from Head Start and other non-UPK public programs, we find that UPK does not raise the rate at which students enroll in pre-kindergarten. Essentially all UPK students would otherwise attend another program.

Our finding that nearly all UPK students substitute away from other forms of childcare echoes Weiland et al. (2020)'s finding for UPK in Boston. The implications for cost-benefit analysis are potentially large. For comparison, Kline and Walters (2016) report that about a third of households offered enrollment in Head Start substitute away from other center-based care and argue that accounting for this substitution is crucial to the overall cost-benefit evaluation of the Head Start program.

Though UPK enrollment does not affect *whether* children enroll in childcare, it does increase the hours of childcare coverage that parents can access. To see this, we compile

[10] Children may enroll in multiple pre-kindergarten programs, so the sum of program-specific substitution effects need not equal the rate of substitution away from any program.

center- and year-specific reports of daily childcare schedules, and place measures of available hours at the center a child attends on the left side of our IV specifications. Online Appendix G describes this process in detail.

As reported in Panel B of Table 4, UPK enrollment raises weekly childcare coverage by 11.3 hours. The weekly total equates to 2.26 additional hours per weekday, or roughly 570 hours when aggregated across the school year. The large treatment effects are due to the ten hours of coverage available each day at UPK programs, more than most alternative programs.[11] As reported in Online Appendix G, the finding that UPK enrollment yields a large gain in hours of childcare coverage holds across a wide variety of approaches to constructing childcare schedule data.

In addition to expanding childcare hours, UPK enrollment reduces families' out-of-pocket childcare costs. The last row of the last column of Table 4 reports the effects of UPK enrollment on monthly out-of-pocket (OOP) costs. A caveat in this analysis is that, due to an error in survey logic, we did not ask the parents of students enrolling in magnet programs about their out-of-pocket costs. These costs are likely fairly low, since these students receive ten hours of free childcare each school day. In Table 4, we report results that assign students enrolling in UPK programs to the average value of costs observed among Head Start students. We view this as likely overstating costs to UPK students (and understating treatment effects) given that they receive more hours of free childcare. We find that UPK access reduces parents' OOP childcare costs by \$375 per month (77% of the control complier mean), even as hours of childcare rise.

4.4 Children's educational outcomes

We find little evidence that UPK affects children's academic performance in the medium run. Figure 4 reports results from estimates of Equation 1 that take academic outcomes as the dependent variables of interest. As with our analysis of parent earnings effects, we focus on specifications that use all available data.

Panel (a) of Figure 4 reports the effects of UPK on in-school assessments in kindergarten and grades 3-8. In general, we cannot reject the null that effects are zero. Our point estimate for KEI scores is modestly positive (0.062 σ; SE=0.072). Point estimates in grades 3-8 are negative.

Panels (b) and (c) of Figure 4 report the grade-by-grade effects of UPK enrollment on chronic absenteeism (defined as missing ten percent or more of total school days in a year) and grade retention (defined as a cumulative indicator for ever being retained). Again, we see statistically insignificant effects across the board. Point estimates are economically small, with the exception of the chronic absenteeism effect in grade 8,

[11] Note that the estimated complier control mean of 50.7 has a large standard error (12.7) and is far above the control mean of 28.8.

which is large, positive, and imprecisely measured.

We make three observations about these findings. First, they are consistent with randomized evaluations of other UPK programs, which also report null or negative effects on achievement and behavioral outcomes in the K-8 grade range (Durkin et al., 2022; Gray-Lobe et al., 2023). Second, they do not rule out positive short- or long-run effects of the NHPS UPK programs. The effects of early childhood interventions often fade out after pre-kindergarten before returning in high school or later (Heckman et al., 2013), and previous randomized evaluations of UPK programs have demonstrated both the fade-out of large short-run effects (Lipsey et al., 2018) and the presence of long-run gains following medium-run null effects (Gray-Lobe et al., 2023). Third, even a true long-run null effect would not mean that NHPS' programs offer low-quality educational programming; rather, it would mean that they offer similar quality programming to the public and private programs from which children substitute.

4.5 Parents' labor market outcomes

4.5.1 Earnings and labor supply

Table 5 reports estimates of the IV specification in Equation 1 that take administrative data on parent earnings and survey reports of parent hours as the dependent variables of interest. We report results for several different earnings outcomes: dollars (including zero values), log income (excluding zero values), and an indicator for positive income. To provide a percentage interpretation that includes zero values, we follow the recommendation of Chen and Roth (2024) and report results from a Poisson IV specification, which we compute using the control function approach outlined in Lin and Wooldridge (2019). This is our preferred specification. See Online Appendix H for details. All specifications control for demographics reported at the time of UPK application. Each specification also controls for the baseline value of the dependent variable, measured in the two academic years prior to the year of application. These controls are linear except in the Poisson regression, which instead includes indicators for each decile of the baseline earnings distribution.

Panel A of Table 5 reports results that use all available administrative data on parent earnings. The first row shows our findings for the academic years when children are of pre-kindergarten age. These run from Q4 in the year of application (when the student would enroll for the first time) through Q3 of the following calendar year for PK4 applicants, or Q3 two calendar years later for PK3 applicants.[12]

We find that child UPK enrollment raises parent income by $5,461 (SE=$1,717)

[12]We refer to "academic years" to make clear that they start in the fall. Note, however, that we include summer earnings in all of our analyses.

during the period of potential enrollment. This is 15.9% of the control complier mean of roughly $34,000. The Poisson IV specification indicates a 21.7% (SE=6.6%) earnings gain during this period, while log specifications that drop zeros show earnings gains of 20.9% (SE=6.1%). We find evidence of modest extensive-margin gains, with rates of positive earnings rising 5.7 percentage points (SE=2.6) on a base of 81.7 percent.

To understand how earnings effects evolve as children age out of pre-kindergarten, we compute separate estimates of the effect of UPK on enrollment for each two-year interval following on-schedule pre-kindergarten completion. For children following the standard grade progression, these will correspond to kindergarten and first grade, second and third grade, and so on. We report these findings in the lower rows of Panel A of Table 5. Figure 5 plots estimated effects on dollars of income, pooling years 11 and later after pre-kindergarten. We use all available data for each regression, so sample sizes decline as we push farther from pre-kindergarten completion.

Earnings gains persist after children age out of UPK. As shown in Figure 5, our preferred Poisson IV estimates are stable with earnings effects of 19-23% through six years after kindergarten before fading out. Estimates from other specifications are also stable. A pooled specification combining years 1-6 after pre-kindergarten yields an estimated earnings gain of 20.9% (SE=7.9%) in the Poisson specification with similar values in the log specification.

After six years, effect estimates start to decline, and standard errors grow. Pooling years seven and later in our Poisson specification yields estimated gains of 9.2%, with a standard error of 21.2%. Results for earnings levels and logs are similar. Our interpretation is that there is strong evidence of effects that last for at least six years following pre-kindergarten, with some evidence of fade-out after that. However, we cannot rule out longer-lasting gains.

A possible concern about specifications that use all available data is that they may conflate heterogeneity in treatment effects by time relative to pre-kindergarten with heterogeneity in treatment effects across treatment cohorts. Long lags relative to treatment are only observed for early cohorts. Panel B of Table 5 addresses this issue by reporting results for the pre-kindergarten years and the first four years afterward for a balanced panel of individuals we observe over this whole period. This sample excludes application cohorts after 2017 for PK3 and 2018 for PK4, reducing the sample size by 14% relative to our main specification.

Effects in the balanced panel are slightly smaller than in the full sample. For example, during the pre-kindergarten period, the Poisson effect is 15.3% (SE=6.8%) in the balanced sample, compared to 21.7% in the full sample. However, as in our main specifications, we see no evidence of fade-out in four years following pre-kindergarten. In our Poisson specification, we find an effect of 16.4% (SE=7.3%) in the first two years

after pre-kindergarten, and 18.1% (SE=8.6%) in the third and fourth years.

Earnings gains during UPK enrollment coincide with a large increase in labor supply. Panel C of Table 5 presents results that take survey reports of weekly work hours in the year following child's UPK application (i.e., the first year of UPK for enrolling students) as well as reports for the second year following application for PK3 applicants. Weekly hours rise by 12.80 (SE=4.25) on a control complier base of 27.9. We cannot reject the null hypothesis that this value is equal to the 11.3 hours of additional weekly childcare that we estimate the UPK programs provide, as reported in Table 4. Substitution appears to be largely part-time to full-time work, consistent with the finding of modest extensive margin gains in the administrative data.

Labor supply effects decline after the pre-kindergarten years. The estimated effect of UPK enrollment on post pre-kindergarten hours is 1.48 (SE=4.25). We can reject the null that this effect is equal to the 12.8 hour effects during pre-kindergarten at the p=0.01 level. This decline coincides with an increase in the control complier mean from 27.9 hours during pre-kindergarten to 37.4 hours afterwards. UPK helps parents shift from part-time to full-time work during pre-kindergarten; afterwards, both the control and treatment group work full time.

4.5.2 Career continuity

Our findings thus far raise a question: if labor supply effects largely fade out immediately after pre-kindergarten, why do earnings gains persist? A possible answer is that UPK helps parents avoid career or job disruptions. This would be consistent with evidence from Bertrand et al. (2010) and Goldin (2014) that career continuity and long within-job work hours are important for earnings growth.

We test this hypothesis by evaluating the effects of UPK on career disruptions. Our data do not include employer identifiers, so we cannot identify job spells. However, we do observe job-specific earnings and industry codes in each quarter, so we can measure the consistency of labor force attachment within the academic year, the degree to which individual earnings come from one job as opposed to multiple jobs, and changes in industry. We take these variables as outcomes in equation 1.

Table 6 reports our results. The first column of Table 6 takes an indicator for having a new main industry in an academic year, relative to the previous, as the outcome of interest. This is a simple measure of career disruption. Main industry is defined as the industry in which the individual has the most earnings. We find that UPK enrollment reduces the share of individuals who switch industry during the pre-kindergarten years by 6.1 percentage points (SE=2.8), 25% of the control complier mean of 0.24. Industry switch effects fade to zero immediately following the completion of pre-kindergarten.

Column 2 of Table 6 examines the effects of UPK enrollment on the number of

quarters per year in which individuals hold one "main job," defined as having exactly one job and having that job pay at least $4,000, roughly equivalent to a full-time minimum wage job. This is a simple measure of job attachment. During the pre-kindergarten years, UPK enrollment raises the number of quarters individuals hold a single main job by 0.300 (SE=0.108), 15% of the control complier mean. As with industry-switching, this main job effect goes to zero when pre-kindergarten ends.

Columns 3 and 4 of Table 6 consider the effects of UPK enrollment on the count of low-earning quarters per year (column 3) and the total number of low-earning quarters up to and including the current academic year (column 4). We define a low-earning quarter as one where the person earns less than $4,000. The goal here is to measure how career disruptions that workers face accumulate over time.

We find that UPK enrollment reduces the number of low-earning quarters by 0.206 per year (13.8% of the control complier mean) in the pre-kindergarten years, with the cumulative reduction in low-income quarters reaching 0.76 between three and four years after pre-kindergarten before declining back towards zero in years five and six.[13] UPK enrollment causes parents to avoid nine months—one school year—of low earnings during pre-kindergarten and the four following years.

Results from this exercise show that children's UPK enrollment substantially reduces the likelihood that parents will experience career disruptions. We view these findings as helping to rationalize the long-run earnings effects we observe in Table 5. Further, as we discuss in section 8, our findings here are consistent with parents' qualitative reports about how UPK enrollment affected their work lives.

5 Cost-benefit analysis

The previous sections provide evidence that enrolling in UPK programs increases parents' earnings and reduces out-of-pocket costs for families. UPK may also raise earnings for children. However, UPK is costly, with a per-pupil expenditure (PPE) of approximately $15,500 per child per year.[14] Here, we evaluate the costs and benefits of the program using the marginal value of public funds (MVPF) framework from Hendren and Sprung-Keyser (2020). The MVPF is equal to the ratio of the dollar-valued after-tax benefits of the program, i.e., the willingness to pay, divided by the program cost to

[13]Note that because the pre-kindergarten enrollment period can be either one or two years and because the other blocks consist of two-year periods, the cumulative effect coefficients do not equal the sum of the contemporaneous effect coefficients.

[14]This value comes from the average PPE among NHPS elementary schools during the 2018-2019 school year (in 2015 dollars) as we do not have a direct measure of the PPE of the pre-kindergarten program. For comparison, the PPE for Head Start in Connecticut is $10,100 and the average PPE for all public pre-k programs in the state is $9,600 (Friedman-Krauss et al., 2022, 2023).

taxpayers. We compute these objects using estimates from our randomized design. See Online Appendix I for details of the calculations summarized here.

First, we consider the willingness to pay for the benefits accrued to parents and children. We incorporate three channels through which families benefit: changes in out-of-pocket childcare costs, changes in parents' earnings, and changes in kids' future earnings. For changes in out-of-pocket costs, we use the reduction in monthly out-of-pocket costs from Table 4 and assume that this reduction applies for nine months per year and that the average years enrolled in the pre-k program is 1.56.[15] We estimate a $5,200 reduction in out-of-pocket costs. For changes in parents' wage income, we calculate the gains in discounted after-tax income using the impacts on wage income reported in Table 5 and conservatively assume no impact beyond six years after the magnet pre-k program. We estimate an average after-tax gain of $33,456.[16]

For changes in the wage income of children we consider a variety of approaches based on our own findings and those from past work. The goal is to understand how sensitive our conclusions are to different assumptions about children's long-run earnings. Our first approach assumes that the null effects we observe on achievement after pre-kindergarten reflect the long-run effect. Following Kline and Walters (2016) and Cascio (2023), we use the impacts on kindergarten test scores that we measure in our data to predict future income gains using results from Chetty et al. (2011). Specifically, we assume a one standard deviation increase in test scores causes a 10% increase in the present discounted value of lifetime income and assume an average present discounted value of lifetime earnings of $353,507 (Chetty et al., 2011). We estimate these gains at $1,700 after taxes. Our second approach is to take Lipsey et al. (2018)'s estimates of a 0.4σ gain in pre-kindergarten scores from random assignment to Tennessee's UPK program and apply the Chetty et al. (2011) conversion factor to project earnings.[17] This approach yields an estimated after-tax earnings gain of $11,300. Our third approach takes Gray-Lobe et al. (2023)'s estimate of the Boston UPK program's effect on enrolling in 4-year college (which is similar to their estimate for enrolling in any college), and scales it by Zimmerman (2014)'s estimates of attending college. As Zimmerman (2014) only reports impacts on earnings for 14 years, we use estimates from Hendren and Sprung-Keyser (2020), which project impacts over the lifetime and calculate the present discounted value. This approach yields an estimated after-tax gain of $9,900. These latter two approaches assume that New Haven's UPK program raised achievement in

[15] This is the average change in UPK enrollment for the lottery compliers and is estimated using the same framework as our other IV estimates but with years of UPK as the outcome.

[16] We assume an effective tax rate of 0.20 based on Hendren and Sprung-Keyser (2020), who use calculations from Congressional Budget Office (2016) and find that the effective tax rate is close to 0.2 for those at 100 to 400 percent of the poverty line.

[17] This is similar to Cascio (2023)'s approach to computing the MVPF of UPK, but uses score estimates based on Lipsey et al. (2018)'s randomized design.

pre-kindergarten by amounts similar to those observed in other UPK programs, and that early gains will show up in long-run outcomes.

Summing across the cost, parent earnings, and child earnings channels, our estimates of total willingness to pay range from $40,400 to $50,000, depending on which estimate of gains for children we use. Notably, even the largest available estimate of children's earnings effects is equal to roughly one third of the earnings gains for parents.

Next, we consider the net cost of the program to taxpayers. The gross costs of the program are approximately $24,000 per child, which is the yearly PPE times the average of 1.5 years enrolled. However, as shown in Table 4, many applicants substitute away from other publicly funded programs. To account for this substitution, we first estimate the change in years of enrollment in Head Start, School Readiness, Care 4 Kids, and other public state programs for the 2015-2017 application cohorts.[18] We then scale these changes by the per pupil expenditures for each program as reported by Friedman-Krauss et al. (2022) and Friedman-Krauss et al. (2023). As discussed in Kline and Walters (2016), subtracting these cost reductions assumes that the other programs are not also capacity constrained.[19] We estimate that substitution from other publicly funded programs reduces the net cost by $8,800. Gross costs also do not account for changes in current and future tax revenue from parents and children. Using the estimates and tax rates from our willingness-to-pay calculations above, we estimate that the government generates an additional $8,400 dollars in discounted tax revenue from the parents and $430 from the children. Putting these pieces together, we estimate an average net cost of $6,600.

The upper panel of Figure 6 summarizes willingness to pay, net costs, and their inputs, projecting children's earnings gains based on our estimated kindergarten score effects. In these and subsequent MVPF calculations we report 90% bootstrapped confidence intervals based on 500 resamplings. The first bar reports our estimated willingness to pay of $40,400, while the next three bars report its inputs. Family income gains make up more than three quarters of the willingness to pay. The red bars report the average net cost and its inputs. Net costs are equal to 27% of NHPS' program costs.

Dividing the willingness to pay by net costs, we estimate an average MVPF of 6.13 (90% CI=$[2.15, 41.14]$). Gains for adults account for the bulk of the overall MVPF. As reported in the lower panel of Figure 6, our estimated MVPF excluding wage gains for children is 5.51 (90% CI=$[1.98, 25.4]$). In contrast, if we exclude gains for parents, we find an MVPF of 0.46 (90% CI=$[0.19, 0.77]$). The same basic logic holds under

[18] We focus on 2015-2017 as 2015-2018 are the years with the highest coverage in our state administrative data and PK3 applicants can be enrolled for two years.

[19] If the other programs are capacity constrained, then a complete cost-benefit analysis also requires us to know the returns to the marginal enrollee in the other programs, and if that student also substitutes away from another program. We discuss below how this would affect our findings.

more optimistic assumptions about earnings effects for children. If we project children's earnings based on a score gain of 0.4σ, the child-only MVPF rises to 1.32 (90% CI=$[1.07, 1.60]$), but remains below the parent-only MVPF. Similarly, the child-only MVPF estimate based on Gray-Lobe et al. (2023)'s estimated college attendance gains is equal to 1.2. Note that under both of these more optimistic assumptions, which we regard as plausible, the overall MVPF based on gains for both parents and children rises substantially, for example to 11.92 (90% CI=$[3.94, \infty]$) under the 0.4σ assumption. This is because even relatively small reductions in net costs have large effects on the overall MVPF when net costs are small, as they are here.

From a policy perspective, the crucial question is how the gains from UPK compare to other potential uses of the funds. Figure 7 reproduces a key plot from Hendren and Sprung-Keyser (2020), adding points for the child-only and parent-only MVPF estimates of New Haven's UPK program, as well for Cascio (2023)'s child-only UPK evaluation. The horizontal axis is the age of the beneficiary, and the vertical axis is the value of the MVPF. The point that Hendren and Sprung-Keyser (2020) make using their version of this plot is that there are many child-focused programs that yield high and even infinite MVPFs. These include increases in school spending, health insurance for children, and expansions in college access, among others. In contrast, there are relatively few programs targeted at adults that yield high MVPFs. In particular, many labor market programs for adults, including work incentives such as the EITC and recent increases in top tax rates, have MVPFs near one.

Placing the UPK MVPFs on this graph makes two points. The first is that UPK does not offer high MVPFs when only considering children's gains compared to other child-focused policies. This is because the benefits for children from UPK are relatively modest regardless of how they are calculated, and because the other child-focused options offer such high returns. The second is that UPK offers very high returns compared to other policies targeting adults. The parents-only UPK MVPF is higher than the MVPF for all the job training policies, all the unemployment insurance policies, and all the cash transfer policies (the category that includes EITC, Paycheck plus, and Negative Income Tax programs) evaluated in Hendren and Sprung-Keyser (2020).

Online Appendix I considers various alternative approaches to MVPF calculation. Appendix Table I.1 reports the MVPF under various assumptions and 90% confidence intervals. Our estimates are not particularly sensitive to using alternate tax rates, to using survey measures of substitution across programs rather than administrative measures, or to discounting wage gains obtained during pre-kindergarten to account for the possible disutility of increased labor supply during that period, as in Mas and Pallais (2019). We prefer estimates that do not consider the disutility of work because much of the additional time spent working might otherwise have been spent on childcare.

The lowest alternate MVPF estimate we obtain for this full sample is 2.6, under the assumptions that a) capacity constraints on alternative childcare programs bind, so there are no cost savings from program substitution, and b) the benefits to children who gain access to alternative programs due to the new open seat are zero. We regard this as a loose lower bound because it ignores the potentially large gains for children and parents from gaining access to non-UPK childcare programs, as documented in Kline and Walters (2016) and other studies. Online Appendix I reports estimates of two alternate summary measures: the cost-benefit ratio and net social benefit (García et al., 2020; García and Heckman, 2022).

6 Distributional Effects

6.1 Family income

The fundamental difference between UPK and other major pre-kindergarten programs, such as Head Start, is that UPK is not means-tested. Differences in alternative childcare options and career trajectories across the income distribution suggest that effects on children and parents may be heterogeneous along this margin.

To better understand how the costs and benefits of UPK enrollment vary across the income distribution, we split our sample into three groups based on terciles of median household income in the Census block group where the student lived at the time of application. We use this measure rather than individual income because mother's income between childbirth and pre-kindergarten may not be a useful proxy for household income in the long run. We estimate equation 1 within each group for our main childcare, earnings, and educational outcomes. We report our findings in Table 7.

An important preliminary point here is that the higher-income families in our analysis fall in the middle of the population distribution. As reported in Table 7, the mean individual income for control compliers in the second tercile during pre-kindergarten is $34,324 per year. For control compliers in the third tercile it is $43,226. In 2022, median individual income for workers in the US was about $39,000 (in 2015 dollars).

Panel A of Table 7 reports how UPK enrollment affects childcare usage in each income tercile. Focusing first on hours, we find that point estimates for weekly gains are larger in the middle and top terciles (12 hours) than in the bottom tercile (7 hours). Though standard errors in the split survey sample are fairly large, these estimates suggest that gains in access to childcare coverage persist through relatively high income levels.

Turning to substitution across programs, we find that all three income groups substitute away from other forms of outside-the-home childcare when they enroll in UPK.

We find no evidence of an increase in the extensive margin of childcare use for any income tercile. We do see differences in the kinds of programs from which students in different terciles substitute. Bottom-tercile students enrolling in UPK are more likely to substitute away from Head Start according to both survey and administrative records. Middle- and top-tercile students mostly substitute away from other paid options.

Reductions in childcare costs from UPK enrollment increase with income. For bottom-tercile families UPK enrollment does not reduce childcare costs. For middle-tercile families, UPK enrollment reduces costs by $400 per month, or $3,600 per school year. For upper-tercile families, costs fall by $552 per month, or about $5,000 per school year. The broad story is one where low-income families shift from other subsidized options and experience small effects on hours and costs. Top- and middle-tercile families both experience increases in hours and reductions in costs, with particularly large cost reductions for top-tercile families.

Differential substitution patterns translate to differences in educational treatment effects. Panel B of Table 7 reports these results. UPK enrollment raises test scores for middle-tercile students by 0.30 σ. Effects are negative (though noisily estimated) in the bottom tercile and close to zero in the top tercile. As reported in Online Appendix Table A.3, within terciles, effects are similar across the cognitive, social, and physical KEI subscores.

Figure 8 plots test score effects by grade and family income tercile. For the middle-tercile group, we see a classic fade-out/fade-in pattern, with score effects going to zero in elementary grades and then rising again in grades seven and eight. For the upper- and lower-tercile groups, we see no evidence of score gains in any grade. By seventh and eighth grade, point estimates for upper- and lower-tercile students are negative and economically large, though also statistically noisy. As shown in Appendix Figure A.3, we generally cannot rule out null effects on absenteeism and grade retention in any grade.

One story consistent with our findings on substitution patterns and score gains is that there is a "doughnut hole" in access to high-quality pre-kindergarten at middle income levels. Low-income students have access to subsidized care, and higher-income students can access high quality private programming. In the middle of the income distribution, access to subsidized care is more limited and high-quality private options are out of reach.

Parents' earnings effects are also heterogeneous across the income distribution. Panel C of Table 7 reports these findings for our Poisson IV estimates. We find earnings gains in the neighborhood of 25% for middle- and top-tercile families during pre-kindergarten. Earnings gains for bottom-tercile families have a point estimate of 10.6%, and we cannot rule out a null of no effect at conventional levels. After pre-kindergarten,

effects remain high for top- and middle-tercile families for at least six years, although standard errors are in some cases large. Effects for the bottom tercile are again zero. This finding is consistent with the finding above that UPK effects on hours worked are smaller for lower-income families. Another contributing factor may be that gains from expanded work hours are larger for higher-skilled workers (Kuhn and Lozano, 2008; Cortés and Pan, 2019) and that returns to experience may be lower in the jobs that lower-income families hold (Deming, 2023).

6.2 Cost-benefit analysis by income

A natural question to ask about untargeted programs like UPK is whether it might be possible to obtain similar benefits with more desirable distributional properties through a means test. To get a sense of how the value of public funds varies across the distribution of parent income, we repeat our MVPF analysis from section 5, re-estimating the parameters used in the MVPF calculations within neighborhood income tercile. For our main estimates, we project children's earnings based on tercile-specific gains in kindergarten score from Table 7.

The top panel of Figure 9 reports the average MVPF and the MVPF calculated by tercile of neighborhood income. We find an MVPF of 0.46 for the bottom tercile, 26.68 for the middle tercile, and 13.09 for the top tercile.

To help understand where this heterogeneity comes from, the bottom panel of Figure 9 reports the willingness to pay, net costs, and their inputs by tercile. What is happening here is that earnings gains for the middle and top tercile are large and similar to one another. This drives the high MVPF values for terciles 2 and 3 relative to 1. In addition, tercile 2 has fairly high values of children's income gains, due to the large score effects in that group. This drives the net cost down and willingness to pay up relative to tercile 3, increasing the MVPF.

In short, the bulk of the return to UPK expenditure comes from middle- and higher-income families within the applicant pool. This is consistent with the observation that lower-income families have other subsidized options available to them. Note that given the income distribution within our sample, even the upper tercile of the distribution consists mainly of middle-class people who are often the target of work support programs. For example, for a single household head with one child, the maximum AGI value for EITC eligibility in 2022 was just under $40,000 (in 2015 dollars), similar to the mean individual top-tercile income.

6.3 Other demographic categories

Table 8 reports the effects of UPK on parents' earnings by race, relationship to child, and family structure using our preferred Poisson specifications. The columns report results for different samples in the time period listed in the horizontal panel.

The first three columns report results by race and ethnicity. We observe limited evidence of earnings gains for parents of Black students and large effects for parents of White students. Parents of Hispanic students fall in between. These results mirror our finding that earnings effects are larger at higher income levels: complier means are highest for parents of white students, lowest for parents of Black students, and in the middle for parents of Hispanic students.

Splitting by the relationship between the adult and the child (columns 4 and 5), we find evidence of sustained gains for both mothers and fathers. Note that this analysis restricts the sample to post-2013 applicants for which we observe the relationship between the application contact and the applicant; overall effects in this subsample are very similar to those in the main sample. We cannot reject the null that effects for mothers and fathers are equal during ($p = 0.505$) and after pre-kindergarten.

In the final two columns, we split by observed family structure: specifically, whether one or two parents are listed on the application form. We continue to use the post-2013 data, for which family structure information is available. We find positive effects for both groups. We cannot reject the null that effects for the two groups are equal during pre-kindergarten ($p = 0.693$). Gains for one-parent families after pre-kindergarten are larger.

7 Discussion

7.1 Credit constraints and the market for childcare

We show that parents' earnings returns from enrolling their children in UPK outweight the costs of childcare provision. That the returns to public provision are so large presents a puzzle: why can't parents randomized out of NHPS UPK purchase a similar product on the private market? In principle, it would seem possible for private providers facing a similar cost structure to NHPS to provide a similar product and charge parents a price somewhere between the cost of provision and the earnings return for the parent.

One plausible answer to this question is that parents face credit constraints. As discussed in Cameron and Taber (2004) in the context of higher education, credit-constrained individuals may forgo human capital investments that are profitable at market interest rates if those investments pay off over time but need to be financed with cash up front. We find that investments in childcare have this structure: most of

the earnings payoff for parents is realized after pre-kindergarten is over. In terms of credit supply, we would expect constraints to bind more tightly in childcare markets than in higher education markets. Banks do not offer loans for childcare expenses (US Department of the Treasury, 2021).

Our findings are quantitatively consistent with an important role for credit constraints. Our estimates show that the present value of the increase in earnings caused by UPK enrollment that is realized while children are in pre-kindergarten is $8,350. The present value of additional childcare costs incurred—all of which are realized while the child is in pre-kindergarten—is $10,170. The cost of the additional childcare inputs parents get from UPK cannot be financed using their additional earnings during pre-kindergarten.

To sum up, our finding that UPK access pays off over the medium run—not just while children are enrolled in pre-kindergarten—helps rationalize the surprising absence of privately-provided substitutes.

7.2 Policy design and the quality-quantity tradeoff

States seeking to expand UPK at a given budget face a tradeoff between raising program quality and raising hours coverage.[20] Our findings indicate that UPK programs that span the work day can have large economic returns. Similarly-priced investments in UPK quality would need to have very large effects on child outcomes to match the gains we see for parents. To illustrate this point, we compute the UPK effects on child test scores that would be required for the child-only MVPF reported in Figure 6 to match the parent-only MVPF of 5.51. We find that UPK would need to raise children's scores by about 1.2 σ. This is equal to about 1.6 times the estimated IQ gains for four and five-year-olds from the Perry Preschool project (Heckman et al., 2013) and double the largest estimates of UPK score effects of which we are aware (Cascio, 2023).

In practice, NHPS UPK programs appear to have been (at minimum) comparable in quality to Head Start or to the private programs chosen by middle-income applicants, and higher quality than the programs chosen by lower-middle-income applicants. As a consequence, the New Haven UPK program raised parents' earnings without compromising outcomes for children. As Cascio (2015, 2021) points out, this is rare in an international context. For example, public childcare in Quebec raised earnings for women but reduced long-run well-being for children (Baker et al., 2008, 2019), while public childcare in Norway improved long-run outcomes for children but did not affect women's labor market outcomes (Havnes and Mogstad, 2011).

[20] Povich (2024) discusses the challenge of expanding hours in UPK programs.

7.3 The costs of career disruption

The large, sustained earnings effects we find are similar to estimates of the effects of career disruptions reported in prior studies. For example, Bertrand et al. (2010) report that MBA graduates with at least six months of non-work at any point between their MBA year and the year in which earnings are reported earn 17-29% less than other observably comparable students, similar to what we find for the medium-run effects of UPK access. The effects we observe are larger than might be expected based on a pure returns-to-experience model estimated in US data. Estimates from Table 6 show that workers lose about half a year of full-time employment experience during pre-kindergarten and the first four years afterward. Early-career estimates of the returns to a year of work experience in the NLSY from Deming (2023) range from 2.5% to about 10%, depending on worker skill. Our interpretation is that UPK may be most attractive to people with high returns to career continuity.

The contemporaneous earnings effects we estimate are similar to those observed for universal childcare programs in other contexts or for children at different ages. For example, Gelbach (2002) studies the expansion of kindergarten programming in the US and finds that kindergarten enrollment raises contemporaneous earnings for mothers by 24%. Lefebvre and Merrigan (2008) and Lefebvre et al. (2009) study the expansion of childcare programming in Quebec and find that childcare access raises mother's earnings by $3,000 to $6,000 annually on a base of $30,000 to $40,000 and that gains persist after children age out of the program.

8 Subjective effects and qualitative reports

The findings from our empirical analysis are consistent with survey respondents' stated priorities when forming preferences over preschool programs and with their beliefs about how access to UPK programming affected their lives. Panel A of Figure 10 describes how respondents valued different program attributes when choosing where to enroll their child in pre-kindergarten. Our survey asked applicants to rate the importance of six different attributes on a 1-5 scale, with one being "not important" and five being "very important." Location and schedule, the two attributes we asked about that are most closely tied to work opportunities, are among parents' top priorities, with mean importance scores ranging from 4.3 to 4.6 across groups. These values are slightly below the average rating parents assign to teachers, where values range from 4.6 to 4.7. However, they are well above mean ratings for class size or school peers, two other potentially important academic inputs, for which reported values range from 3.5 to 3.9. Parents appear to be thinking about their own work lives when they make pre-kindergarten choices for their children.

Panel B of Figure 10 describes parents' beliefs about how access to the NHPS UPK programs affected their lives (if they enrolled in a UPK program) or would have affected their lives (if they did not). 76% of parents whose children enrolled in a UPK program report that enrolling in the program allowed them to work more. Though somewhat below the 91% share who report that enrolling in a UPK program led to a better pre-kindergarten experience for their child and the 85% who report less stress about money, this is still a large share. Findings are similar for survey respondents whose children did not attend UPK: 67% think that attending would have helped them work more, 79% think it would have improved their child's pre-kindergarten experience, and 87% think it would have reduced financial stress. These reports support the findings from our main analysis that UPK enrollment raises parent earnings and student scores while reducing out-of-pocket costs.

We asked parents who said that UPK enrollment did or would have helped them work more what kind of job changes UPK enabled. Online Appendix Figure A.4 tabulates responses to this question. The modal response was that UPK allowed (or would have allowed) the respondent to increase their hours (54% in the untreated group, 33% in the treated group); many respondents also reported that UPK allowed another household member to increase their hours (16% in the untreated group, 15% in the treated group). This is consistent with our findings in administrative and survey data that hours gains largely occur on the intensive margin.

Qualitative responses provide some additional insight into the channels through which UPK access affected parents' labor market outcomes. Parents who did not fit into one of the survey-provided types of labor market gains (e.g., "increased hours," "got a full-time job," "switched part-time to full-time"; see Online Appendix Figure A.4) were prompted to enter a text description of the job change, and many did.

These responses illuminate the diverse channels through which UPK can improve current and future labor market outcomes. A major theme was the ability to maintain existing jobs. One respondent wrote, "I was able to continue working full time. Without the program I would've had to quit my job." Some respondents specifically credited wraparound care for increased hours. One wrote that "[because my child] was in before and after care programs, I was able to stay at work and not have to leave early" while another described how "after school hours were available which enabled us to pick up later." These reports included expressions of regret from those who did not gain enrollment: "I could have worked a different full time schedule—more normal hours. My husband and I ended up doing what felt like shift work [so that] I could pick up [my child] at 3:00 when the private preschool program ended." Others credited the program for improved productivity while they were working, writing, for example, that "[I] was better able to focus on work during the time worked" or that "I was able to

focus more because I work from home." Finally, some respondents reported that UPK enabled them to invest in human capital likely to pay off in the future. One reported that she started an accelerated nursing program. Another described how they were "a full time college student and both parents graduated with bachelor's degrees."

These responses paint a picture in which UPK doesn't shift people in and out of work but rather helps them work in their preferred jobs, work more, work more effectively, or invest more in their careers. We view this as consistent with our finding of sustained labor market gains after children age out of UPK.

9 Integration effects

While primary the purpose of this paper is to understand how UPK affects parents' earnings and what this means for the financial cost-benefit proposition that UPK poses, the legislative purpose of the magnet program that eight of the nine UPK sites we study belong to is to racially integrate schools by bringing white and Asian-American children from nearby suburbs into urban districts. In Online Appendix J, we present results showing that, because many suburban students who enroll in the UPK program stay in NHPS for elementary grades, the program substantially raises the share of white and Asian-American students in the district.

10 Conclusion

This paper uses randomized assignment to a UPK program in New Haven, Connecticut to study how access to UPK affects labor market outcomes for parents. We find that UPK enrollment raises parent earnings by 21.7% during the pre-kindergarten years, and that these gains persist for at least six years following pre-kindergarten completion before fading out. While gains in earnings during pre-kindergarten coincide with increases in labor supply for parents, labor supply gains fade out after pre-kindergarten, suggesting that sustained gains are due to job experience or career continuity.

A cost-benefit calculation incorporating academic gains for children and cost offsets from substitution away from alternate pre-kindergarten programs shows that the MVPF from UPK enrollment is 6.13: the program generates roughly six dollars in benefits for every dollar of costs. An otherwise identical calculation that omitted gains for parents would have estimated an MVPF of 0.46, i.e., that the program generated 46 cents in benefits for each dollar of costs. While alternate approaches to projecting earnings gains for children shift the child-only MVPF calculation up or down somewhat, they do not alter the basic finding that earnings gains for adults make up the bulk of UPK benefits in dollar terms. We conclude that the labor market gains for parents from UPK access are not only large but also crucial to the accurate cost-benefit evaluation of UPK programming. In cost-benefit terms, contemporary UPK programs are perhaps

best thought of as high-return active labor market policies for adults.

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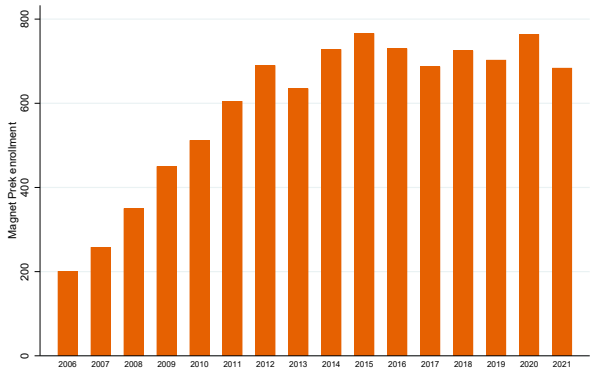
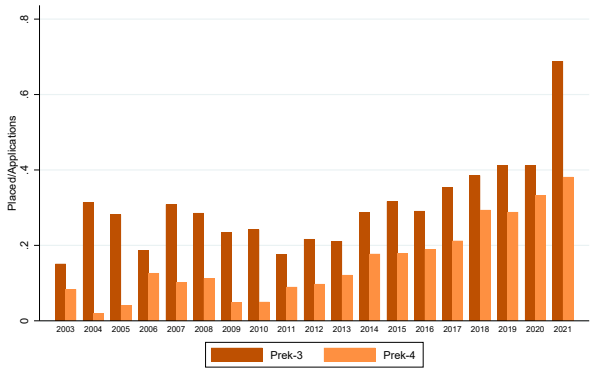
Figures

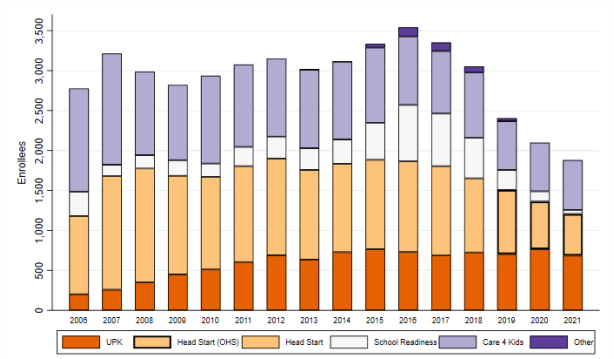
Figure 1: New Haven pre-kindergarten enrollment and applications



(a) UPK enrollment



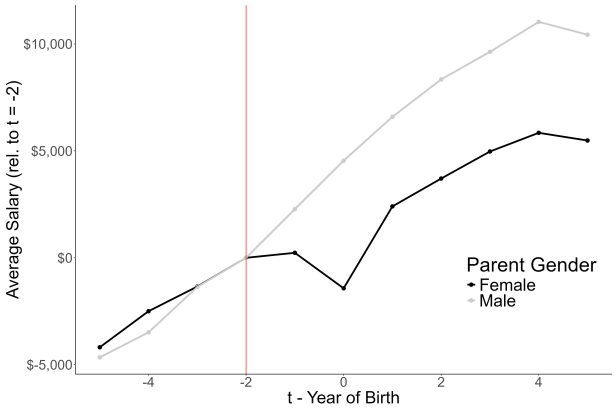
(b) Oversubscription by grade



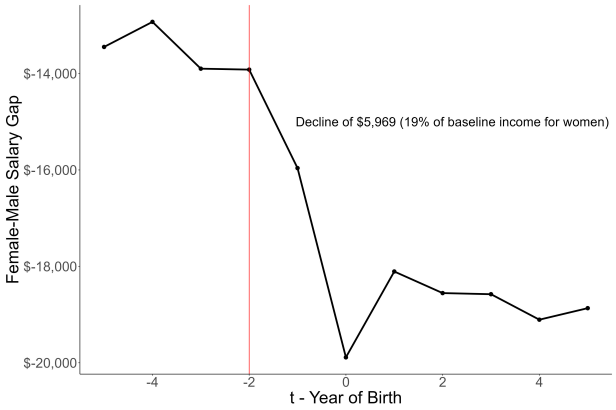
(c) Enrollment in programs with subsidies

Note: Panel (a): total enrollment in NHPS UPK programs by year. Panel (b): the ratio of placements to unique applicants by application grade and lottery year. Panel (c): enrollment in subsidized New Haven pre-kindergarten programs among three- and four-year olds by application year. Head Start enrollment counts are imputed from OHS data in 2019, 2020, and 2021. Care 4 Kids, School Readiness, and Other enrollment also includes imputed unique applicants from spells, using the ratio of spells to unique applicants among UPK applicants. Source: authors' calculations from NHPS data, CT Department of Education data, CT Office of Early Childhood data, and aggregate data from the Office of Head Start. See section 2 for details.

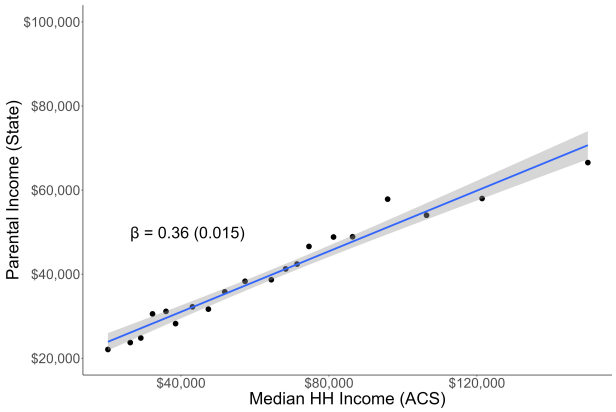
Figure 2: Validating administrative and survey data



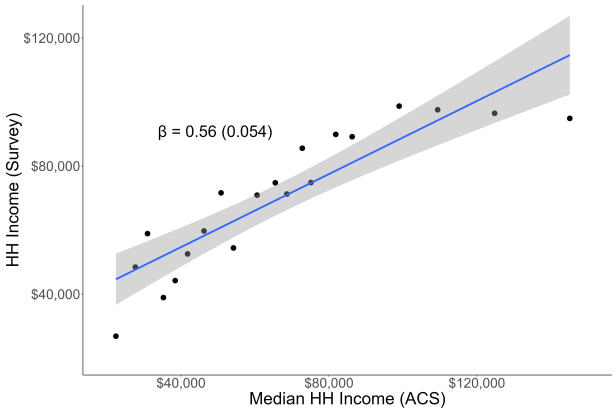
(a) Mothers' and fathers' income around childbirth



(b) Parental income gap around childbirth



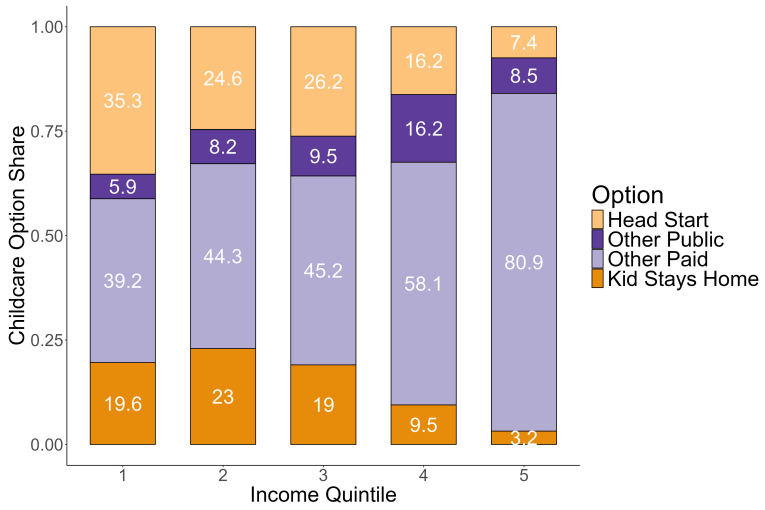
(c) Parental income (state) vs. HH income (ACS)



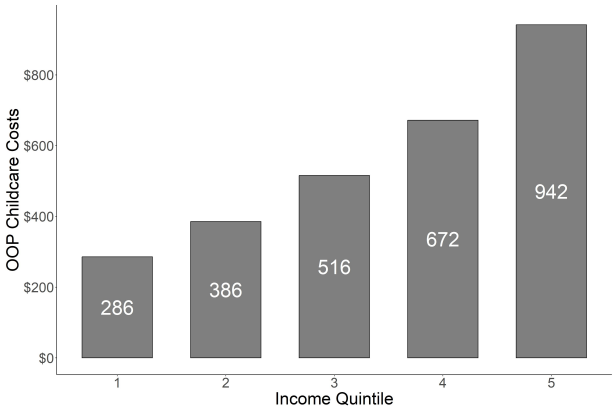
(d) HH income (survey) vs. HH income (ACS)

Note: Panels (a) and (b) show the evolution of mothers' and fathers' incomes around the year of birth for future UPK applicants. Panel (a) normalizes incomes to 0 in period t = -2 and plots mothers' income in red and fathers' income in blue. Panel (b) plots the gap between moms' and dads' incomes over time. The red vertical line indicates two years before a child's birth. Panels (c) and (d) show binscatter plots of median household income at the Census block level from the ACS against parental income taken from administrative records (panel c) and household income reported in the survey (panel d). Panels (c) and (d) also plot the regression line and report the slope. See section 3 for details.

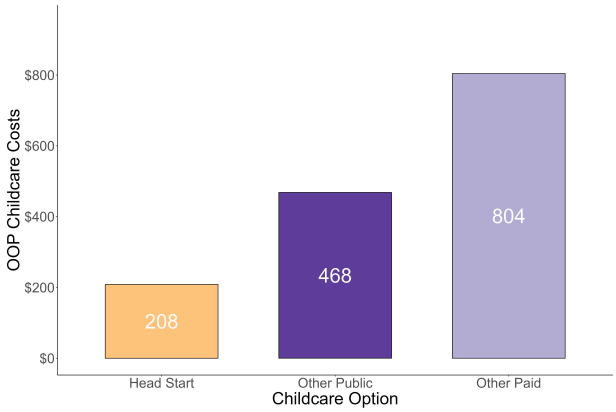
Figure 3: Outside options and out-of-pocket costs in survey data



(a) Childcare Options by Income



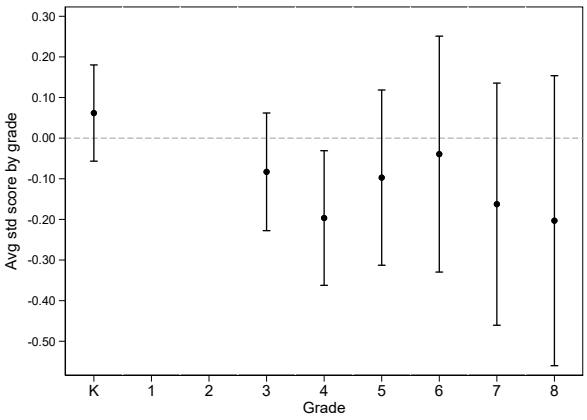
(b) OOP Costs by Income



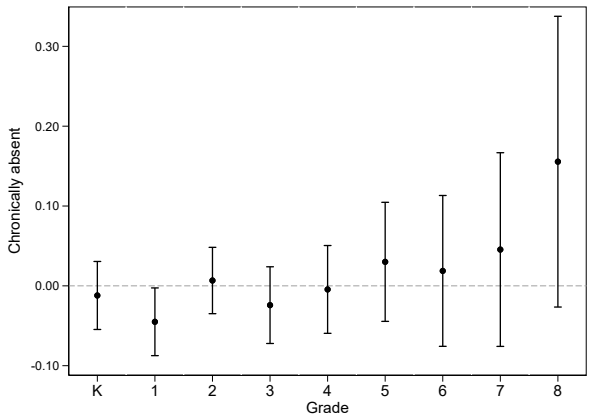
(c) OOP Costs by Childcare Type

Note: This figure shows the composition of outside options for childcare and the out-of-pocket (OOP) costs associated with them based on data from our survey among lottery applicants. Panel (a) shows the composition of non-UPK childcare options for children who applied but did not enroll in UPK. Each bar shows the composition for a specific income quintile based on the median ACS household in the Census block group where the child lived at the time of the UPK application. Panels (b) and (c) show the average monthly OOP costs by ACS income quintiles and by non-UPK pre-kindergarten type. See section 3 for details.

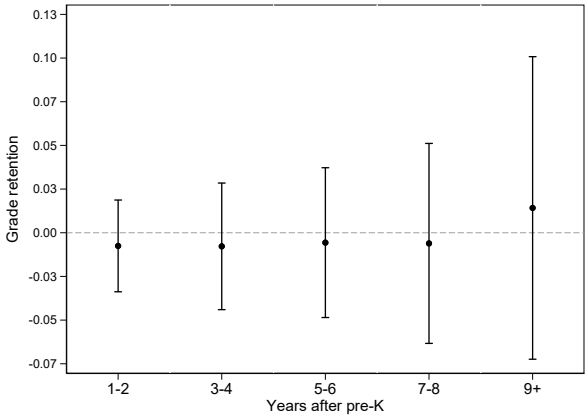
Figure 4: The effects of UPK on children's academic outcomes



(a) Test scores



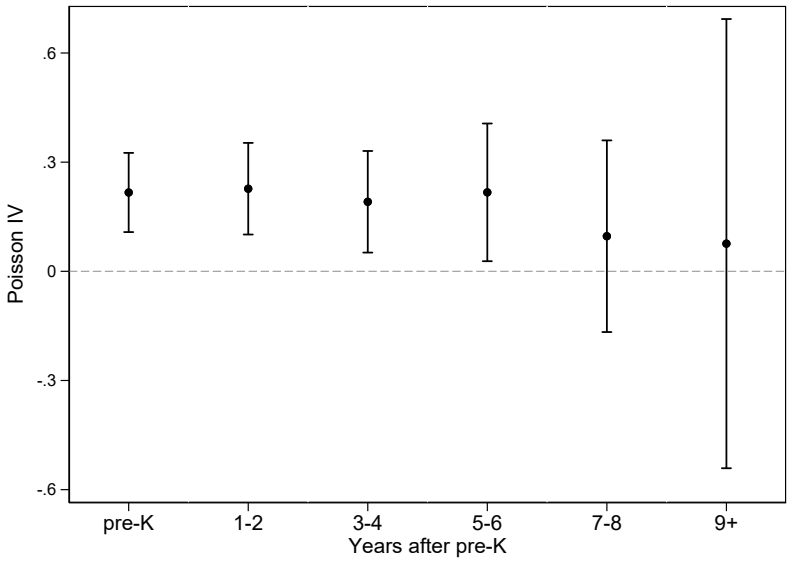
(b) Chronic absenteeism



(c) Grade retention

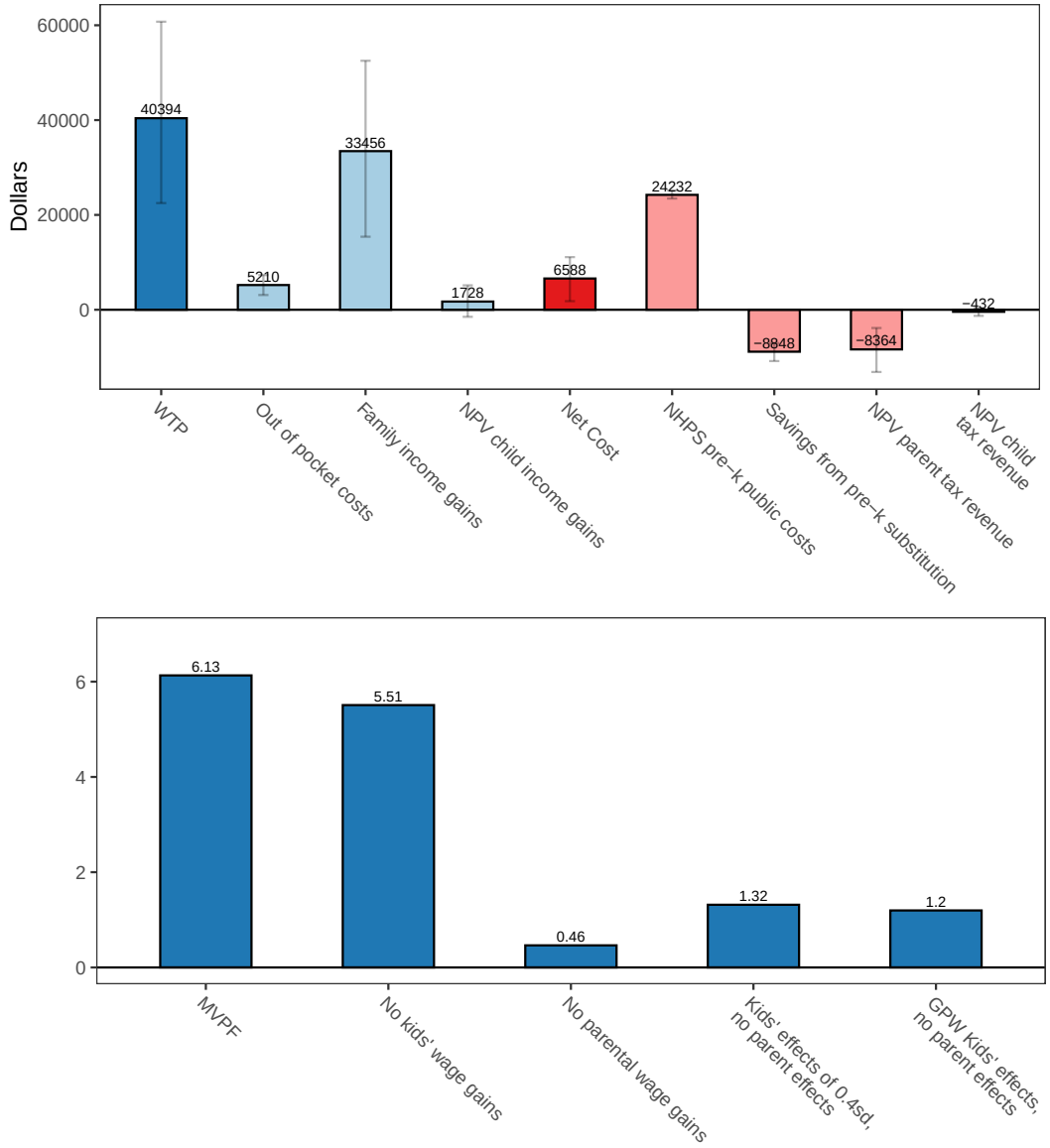
Note: This figure shows IV estimates of the effect of UPK enrollment on average standardized test scores, chronic absenteeism, and grade retention. Panel (a) and (b) show effects by grade, and (c) shows effects by years after childcare grades. Test scores in panel (a) are standardized to have a mean of 0 and variance of one by grade and cohort. Chronic absenteeism in panel (b) is measured using Connecticut's definition: missing 10 percent or more of the total number of days enrolled during the school year. Grade retention for panel (c) is a cumulative indicator for ever being retained up to that point. Black dots correspond to point estimates with the surrounding error bars indicating the 90% confidence interval.

Figure 5: The effects of UPK on parent earnings



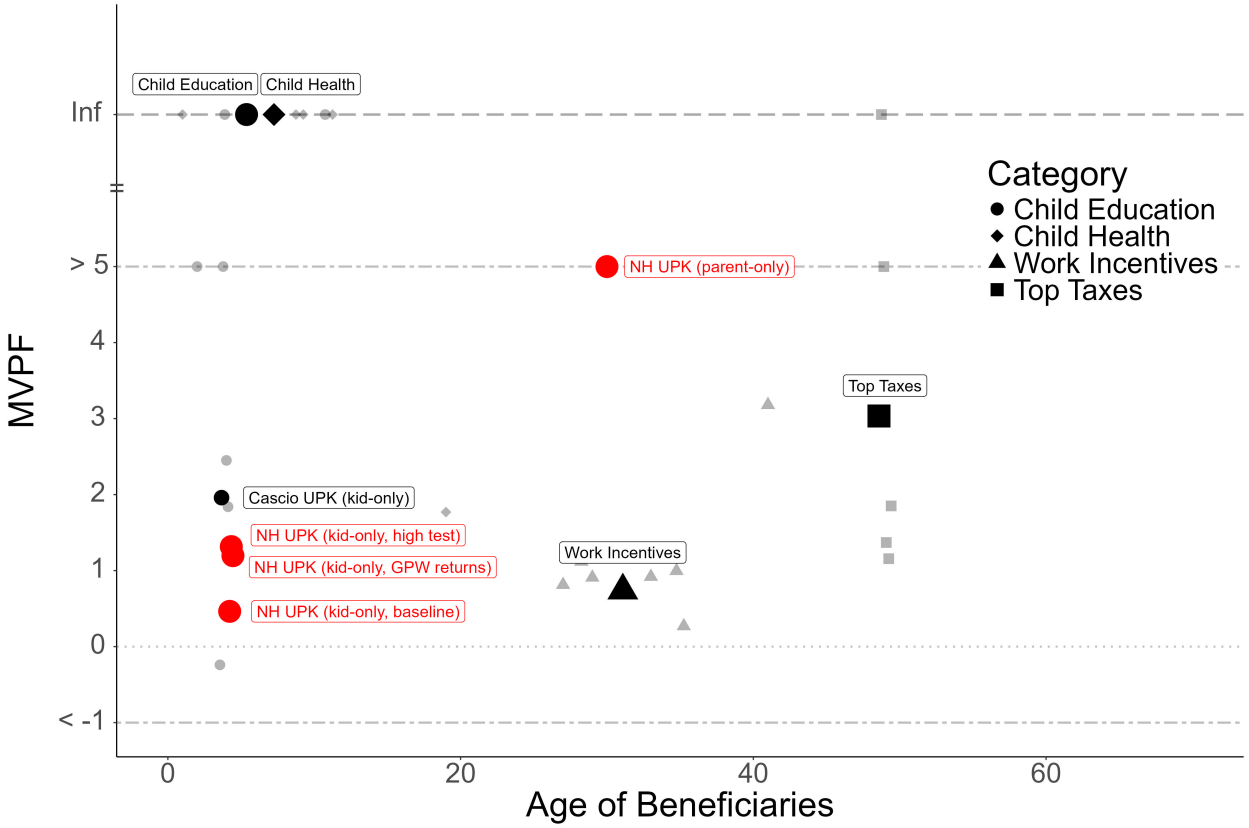
Note: This figure reports Poisson IV estimates where the outcome is parent income in dollars and the endogenous regressor is UPK enrollment; see Appendix H for details on the Poisson specification. Each point is an estimate of the effect (and associated 90% CI) for the time interval reported on the horizontal axis. All estimates include control for demographics and assignment propensities; see section 4.1 for details. The sample consists of all lottery records for which parent earnings information is available over the specified time horizon. See section 4.5 for details.

Figure 6: MVPF and inputs into willingness to pay and net costs



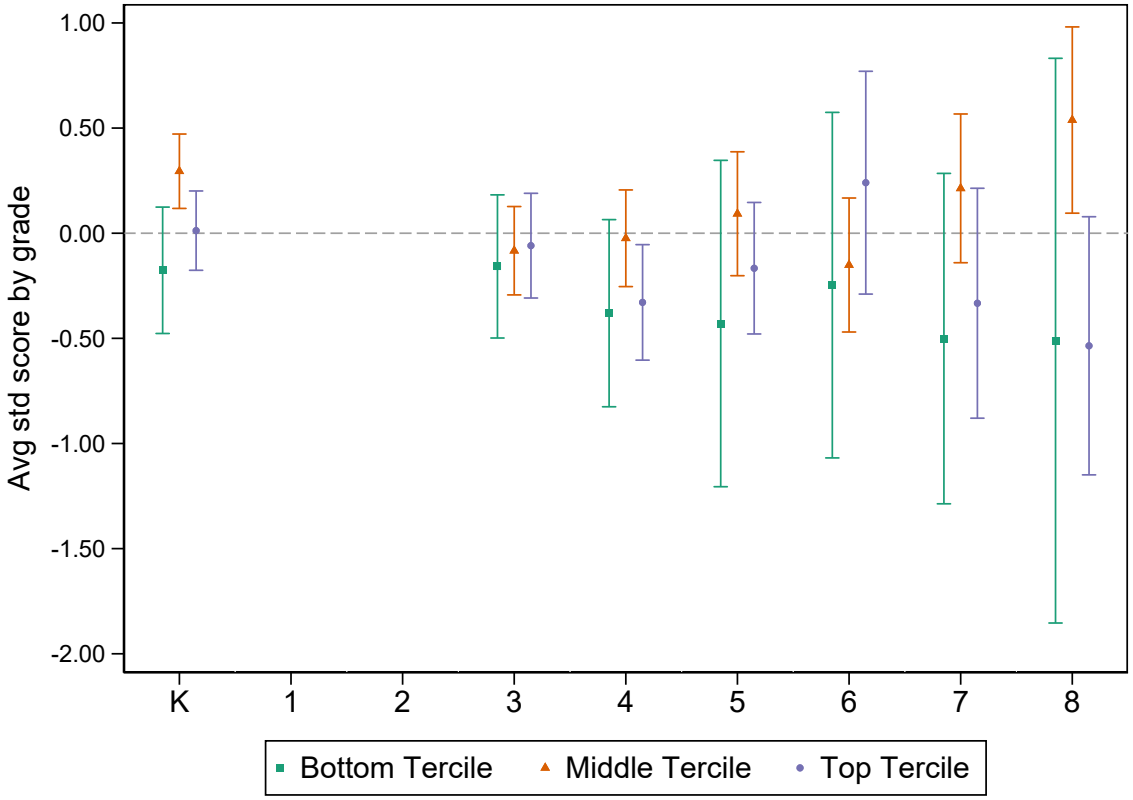
Notes: The top panel reports the numerator and denominator in our MVPF estimate and their inputs. The numerator is the willingness to pay (WTP) and is shown in the first column (dark blue). The next three bars (light blue) show the inputs into WTP: (1) reduction in out-of-pocket costs, (2) parental income gains, and (3) child income gains estimated based on changes in kindergarten test scores. The denominator of the MVPF is the net cost, which is shown by the fifth bar (dark red). The final four bars (light red) show the inputs into net costs: (1) The direct public costs of providing the magnet pre-k slot, (2) the public savings from substitution away from other publicly funded pre-k and childcare programs, (3) changes in discounted tax revenue due to changes in parents' wage income, and (4) changes in discounted tax revenue due to the estimated changes in kids' wage income. We report bootstrapped 90% CIs for each bar. The bottom panel reports the MVPF and alternative MVPF values under different assumptions. The first bar reports the MVPF, which is the ratio of willingness to pay to net costs from the first panel. The second bar reports the MVPF if we exclude estimates on future wage gains of children. The third bar excludes parental wage gains. The fourth bar assumes that UPK increases kindergarten test scores by 0.4σ, and no effect on parent earnings. The final bar calculates benefits to kids based on Gray-Lobe et al. (2023)'s estimated college attendance gains and assumes no effect on parent earnings. See Appendix I for additional details on the MVPF calculations and additional robustness results exploring sensitivity to assumptions.

Figure 7: MVPF estimates across programs and program categories



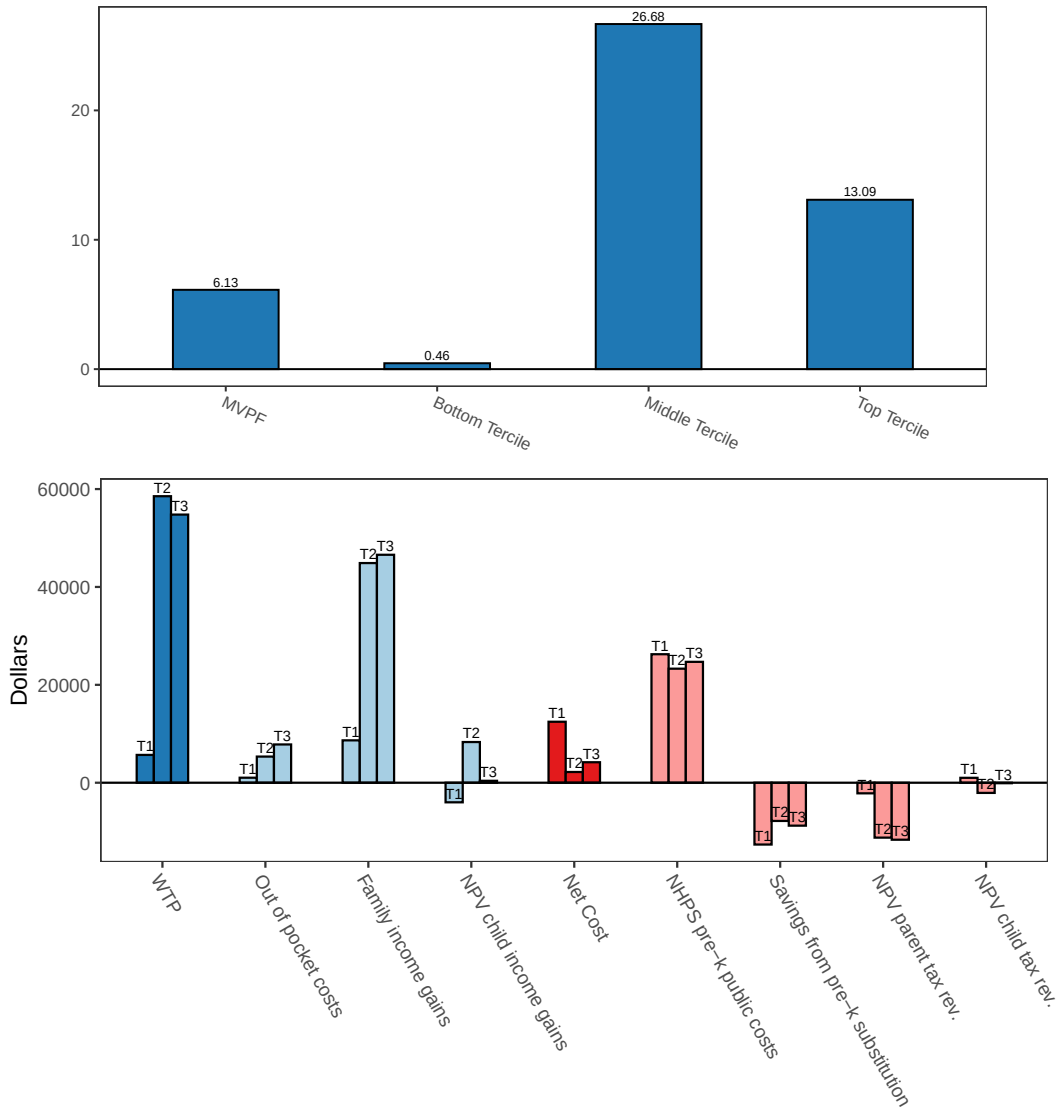
Note: This figure combines our own calculations of MVPFs for New Haven's UPK program with Cascio (2023)'s estimated MVPF for UPK and with estimates of other MVPF values for other program types from Hendren and Sprung-Keyser (2020). Horizontal axis is age of program beneficiaries. Vertical axis is the MVPF value, pooling all values from $[5, \infty)$. The point labeled "NH UPK (kid only, ...)" displays our estimated MVPF values based on gains for children only, under different assumptions about what children's gains are. "Baseline" is the estimated score gains in NHPS data. "High test" assumes a pre-kindergarten score gain of 0.4 as in Lipsey et al. (2018), with earnings gains projected using Chetty et al. (2011). "GPW returns" assumes children's gains from the NHPS UPK program are the same as implied by the Gray-Lobe et al. (2023)'s estimates of four-year college attendance, with earnings gains projected using Zimmerman (2014). "NH UPK (parent-only)" shows the MVPF that excludes earnings gains for children from the calculation. Lighter gray points are other policy evaluations reported in Hendren and Sprung-Keyser (2020). See section 5 for details.

Figure 8: The effects of UPK on test scores by family income tercile



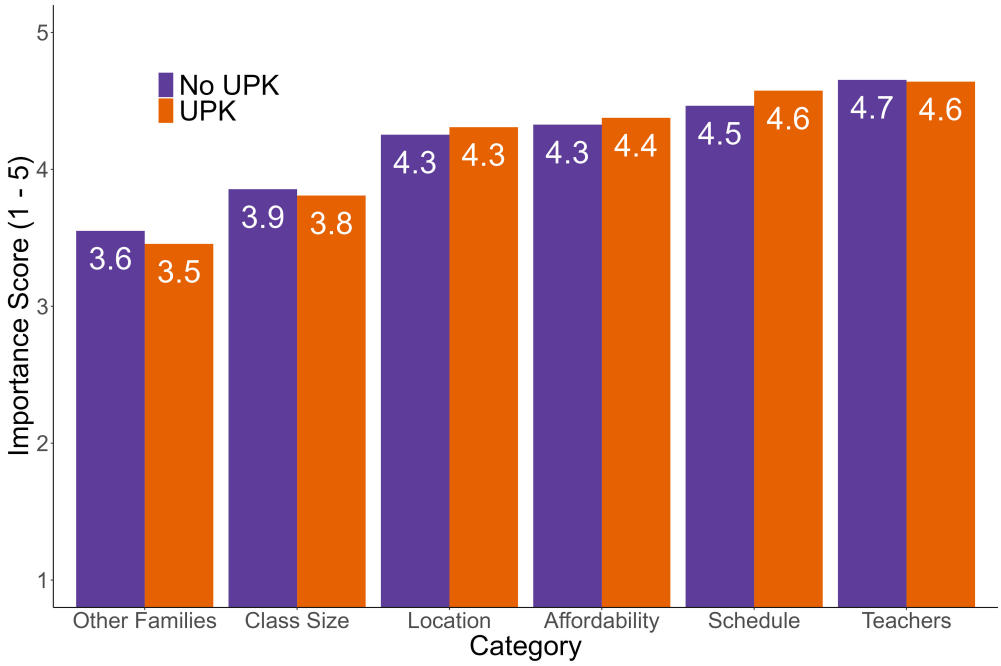
Note: This figure shows IV estimates for the effect of magnet childcare enrollment on average standardized test scores by income tercile analogous to those presented in figure 4. Dots correspond to point estimates with the surrounding error bars indicating the 90% confidence interval. See Section 6 for details.

Figure 9: MVPF and inputs into willingness to pay and net costs by neighborhood income tercile

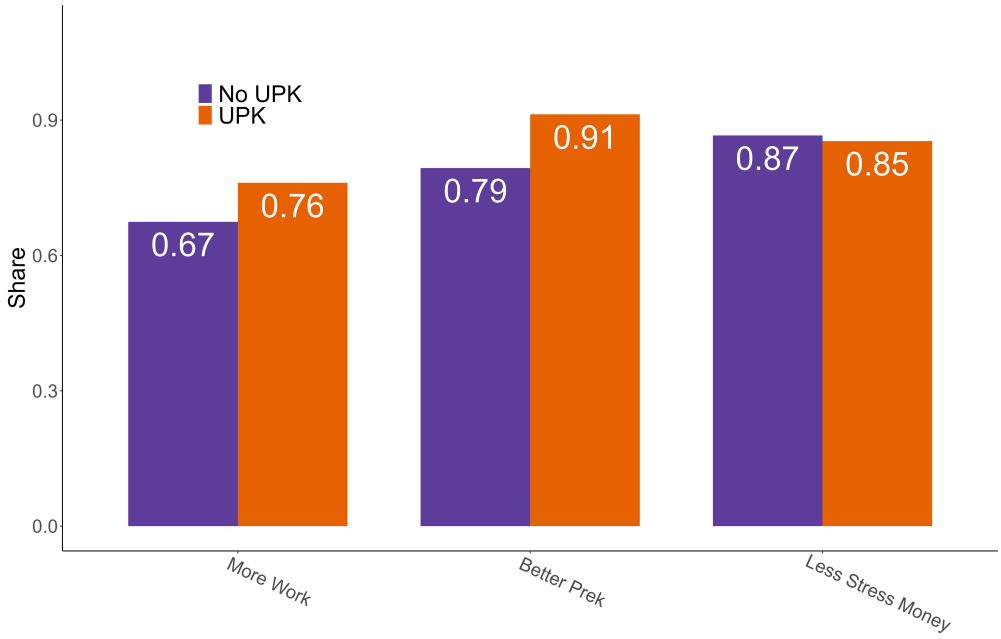


Notes: The top panel reports the marginal value of public funds (MVPF) for the UPK program we study. The first bar reports the MVPF based on the full sample, while the second through fourth bars report the MVPF calculated by terciles of neighborhood median household income. Median household income is calculated from the ACS and corresponds to the block group where the family lived when applying to the pre-k program. The bottom panel reports willingness to pay (WTP) and its inputs by neighborhood income terciles (blue and light blue bars) and the net cost and its inputs by neighborhood income tercile (red and light red bars). "T1" is the lowest tercile while "T3" is the highest. See Section 6 for details. See Appendix I for additional discussion of the MVPF calculations and additional robustness results exploring sensitivity to assumptions as well as for CIs.

Figure 10: Reported priorities and subjective treatment effects



(a) Survey reports of attribute importance



(b) Survey reports of subjective treatment effects of UPK

Note: Panel (a) describes survey respondents' stated priorities over different program attributes when choosing a preschool. Ratings are on a 1-5 scale with one being "not important" and five being "very important." Panel (b) describes survey reports of subjective treatment effects. Applicants were asked whether they believed receiving a UPK slot did (or would have) resulted in "you or other adults in your household being able to work more," "less stress about money," and "better pre-k education for [the child]." Bars report the share responding yes to each question. Purple "No UPK" bars in each panel report data for the group that did not receive UPK and orange "UPK" bars for the group that did. See section 8 for details.

Tables

Table 1: Demographics in New Haven and Connecticut public schools

	NHPS	CT public schools
Asian	3.2%	5.2%
Black or African American	34.4%	12.5%
Hispanic	48.5%	30.0%
White	10.5%	47.5%
English Learners	20.5 %	9.7%
Free or reduced price meals	65.9%	42.4%
Students with disabilities	15.6%	17.1%

Source: This table reports population characteristics (expressed as percentages) for New Haven Public Schools (column 1) and all public schools in Connecticut (column 2). Calculations from the State Department of Education for the 2022-23 school year (Connecticut State Department of Education, 2023).

Table 2: Sample characteristics

	Lottery Sample	State Sample	Parent Earnings Sample	Survey Sample
Panel A: Lottery demographics				
Black	0.418	0.421	0.455	0.305
White	0.217	0.217	0.233	0.314
Hispanic	0.285	0.290	0.219	0.280
Female	0.506	0.505	0.507	0.519
Age at application	3.68	3.68	3.69	3.43
Pre-K 4 applications	0.479	0.485	0.495	0.258
New Haven applicant	0.617	0.617	0.586	0.576
ACS median HH income	59,708	59,729	61,591	65,905
Matched to state data	0.911	1.000	0.912	0.936
Matched to earnings	0.609	0.634	1.000	0.594
Survey respondent	0.043			1.000
Panel B: Lottery outcomes				
UPK assignment	0.251	0.246	0.244	0.419
UPK enrollment	0.265	0.282	0.276	0.546
Panel C: Test scores				
Avg std score K		0.123	0.160	
Avg std grade 3 test score		0.069	0.097	
Avg std grade 8 test score		0.038	0.041	
Panel D: Parent demographics				
Share with two parents	0.329	0.335	0.398	
Share of two parents post 2013	0.562	0.566	0.612	
Share of moms post 2013	0.690	0.691	0.676	
Any positive baseline income			0.863	
Baseline income (with zeros)			25,157	
Baseline log income (no zeros)			9.86	
Panel E: Survey data				
Employed full-time				0.696
Employed part-time				0.164
Hours worked per week				33.51
Respondent mom				0.897
N individuals	16037	13917	9162	840
N applications	18795	16485	10866	

Notes: This table shows the means of variables listed in the rows within samples defined by the columns. The "Lottery Sample" column describes the full set of UPK applicants who applied through the choice process. The "State Sample" column describes applicants matched to enrollment data. The "Parent Earnings Sample" column describes applicants whose parents we identify in the earnings data. The "Survey Sample" column describes applicants whose parents took the survey. Match rates reported in the lower part of Panel A are computed relative to attempted matches. Test scores in Panel C are standardized to have a mean zero and a standard deviation of one in the population of pre-kindergarten students in New Haven County. See section 3 for details.

Table 3: Lottery design validation and first stage

	Comp Cont. Mean	Control Mean	NHPS sample	NHPS sample	State sample	Earnings sample	Survey sample
Panel A: Balance							
Black	0.347	0.430	-0.027	-0.006	-0.005	-0.011	-0.034
	(0.034)		(0.009)	(0.013)	(0.014)	(0.017)	(0.053)
White	0.246	0.199	0.034	0.009	0.008	0.014	0.060
	(0.028)		(0.007)	(0.011)	(0.012)	(0.016)	(0.050)
Female	0.565	0.509	-0.033	-0.028	-0.033	-0.019	-0.073
	(0.034)		(0.009)	(0.014)	(0.015)	(0.019)	(0.058)
Age at application	3.490	3.727	-0.010	0.002	0.008	-0.004	0.039
	(0.034)		(0.005)	(0.008)	(0.009)	(0.011)	(0.032)
ACS median HH income	63,609	58,651	1,510	-500	-272	-1,351	-3,664
	(2,176)		(575)	(777)	(828)	(1,066)	(3,983)
Pre-period income (dollars)	27,443	23,769				232	
	(2,286)					(949)	
Pre-period log income	10.022	9.775				0.027	
	(0.131)					(0.050)	
Any pre-period income	0.921	0.856				-0.006	
	(0.030)					(0.012)	
Earnings-weighted index			823	-41	25	-207	-526
			(197)	(260)	(277)	(357)	(1,317)
Joint test			0.000	0.522	0.384	0.840	0.384
Panel B: Match							
Matched to state	0.895	0.862	0.026	0.011			
	(0.027)		(0.005)	(0.009)			
Matched to earnings	0.759	0.616	-0.005	-0.006			
	(0.033)		(0.008)	(0.013)			
Matched to survey	0.053	0.028	0.007	0.012			
	(0.013)		(0.004)	(0.006)			
Panel C: First Stage							
Enrolled NHPS UPK	0.000	0.000	0.581	0.389	0.402	0.416	0.409
			(0.008)	(0.013)	(0.014)	(0.018)	(0.053)
Years NHPS UPK	-0.044	0.076	0.833	0.576	0.634	0.625	
	(0.030)		(0.013)	(0.022)	(0.023)	(0.029)	
Year and Grade FEs			✓	✓	✓	✓	✓
Admit prob. indicators				✓	✓	✓	✓
First stage partial F-stat			5,481.1	842.3	836.7	569.4	63.0
N individuals			16037	15931	13847	9078	829
N applications			18795	18669	16389	10753	

Notes: Panels A and B of this table report results from reduced-form versions of equation 1, taking either predetermined student and parent covariates (Panel A) or indicators for match to the listed data source (Panel B) as the dependent variable of interest. The joint test in Panel A considers the hypothesis that all coefficients shown in a given column, except for coefficient on the earnings-weighted index, as well as the coefficients for an additional set of ACS tract-level controls, shown in Table A.2, are zero. Panel C reports first-stage estimates of equation 1 where the outcome is either following-year enrollment in an NHPS UPK program or years of enrollment in an NHPS UPK program. Columns 1 and 2 report the control complier and control group means of the dependent variable listed in the row. Columns three through seven report regression results from a specification where the dependent variable is as listed in the table row and the controls and samples vary across columns. Each cell reports results from a separate regression. The reported estimates are coefficients on an indicator for being offered a UPK spot, with standard errors in parentheses. Column 3 uses all available application data and includes only grade-by-year fixed effects. Column 4 uses all available application data and adds controls for the P_i, as described in section 4.1. Column 5 has the same controls as column 4, but restricts to application data that is successfully matched to state records. Column 6 has the same controls as column 4 but restricts to application data merged to parent earnings records. Column 7 restricts to the survey sample. Standard errors are clustered at the level of the application (columns 3-5 and 7), or two ways at the level of the application and the level of the parent (column 6). See section 4.2 for details.

Table 4: UPK effects on childcare outcomes

	Comp Cont. Mean	Control Mean	State sample	State 2015-17	Survey sample
Panel A: Substitution					
Enrolled Head Start (admin)	0.211	0.209	-0.143	-0.199	
	(0.030)		(0.019)	(0.027)	
Enrolled School Readiness	0.063	0.087	-0.071	-0.064	
	(0.021)		(0.014)	(0.017)	
Care4Kids	0.186	0.197	-0.050	-0.017	
	(0.028)		(0.026)	(0.034)	
Any other SDE/OEC pre-k	0.456	0.350	-0.170	-0.157	
	(0.036)		(0.032)	(0.043)	
Any pre-k or childcare	0.626	0.547	0.392	0.370	
	(0.035)		(0.025)	(0.029)	
Enrolled Head Start (survey)	0.162	0.221			-0.192
	(0.151)				(0.068)
Other paid option (survey)	0.720	0.548			-0.623
	(0.183)				(0.082)
Another public option (survey)	0.144	0.097			-0.117
	(0.111)				(0.051)
Any pre-k or childcare (survey)	1.096	0.894			0.022
	(0.122)				(0.044)
Panel B: Usage intensity					
Weekly childcare hours	51.235	29.067			11.319
	(13.022)				(3.200)
Monthly OOP costs	487	620			-375
	(216)				(90)
First stage partial F-stat			837.7	592.4	63.0
N individuals			13842	3599	829
N applications			16331	4051	

Notes: This table describes how compliers substitute away from other programs when they enroll in UPK. Results are from IV estimates of equation 1 where the outcome is as listed in the rows. All specifications include controls for assignment propensity and demographics as described in section 4.1. The first two columns report the complier control mean and the control mean. The third through sixth columns report the IV estimates in different datasets. "NHPS sample" is the set of all applications, with outcomes observed in NHPS records. "State sample" is the set of applications matched to state records, with outcomes observed in state data. "State 2015-17" restricts the state sample to the 2015-17 application period, the years with the best coverage of other programs in the administrative data. "Survey sample" is the set of survey respondents, with outcomes observed in survey data. Outcome variables, listed in the rows are from administrative sources, except where "(survey)" is specified. Panel A reports results from specifications where the outcomes are enrollment indicators for other pre-kindergarten programs. For the rows, "Enrolled in Head Start", "Enrolled in School Readiness", "Care 4 Kids", and "Any other SDE/OEC pre-k" are indicators equal to one if a child enrolls in a program of the listed type in the administrative data. "Any pre-k or childcare" is an indicator equal to one if the child enrolls in any subsidized program, including UPK. The final four rows of Panel A report similar outcomes based on survey data. Panel B reports IV regressions where outcomes are measures of childcare usage based on survey data. "Weekly hours" reports the change in weekly hours of childcare or pre-k provided. "Monthly OOP costs" is the self-reported monthly cost of childcare. Standard errors are in parentheses and are clustered at the applicant level. See section 4.3 for details.

Table 5: Labor market effects

	Income Cntrl Mn	Work Indicator	Income (Incl. 0s)	Poisson IV	Log Income	N individuals
Panel A: Earnings from admin data						
Pre-K years	34,363	0.057	5,461	0.217	0.209	10619; 10619; 10619; 8528
	(2,484)	(0.026)	(1,717)	(0.066)	(0.061)	
Years after pre-K 1-2	36,567	0.037	6,482	0.227	0.215	10282; 10282; 10282; 8025
	(2,781)	(0.028)	(2,216)	(0.077)	(0.068)	
Years after pre-K 3-4	35,001	0.050	6,413	0.191	0.187	9989; 9989; 9989; 7434
	(3,293)	(0.031)	(2,444)	(0.085)	(0.066)	
Years after pre-K 5-6	36,208	0.062	6,849	0.217	0.142	8221; 8221; 8221; 5860
	(3,607)	(0.040)	(3,197)	(0.115)	(0.090)	
Pooled post pre-k						
Years after pre-K 1-6	35,957	0.048	6,469	0.209	0.187	10282; 10282; 10282; 8224
	(2,818)	(0.028)	(2,258)	(0.079)	(0.058)	
Years after pre-K 7+	33,716	0.054	3,539	0.092	0.193	6128; 6128; 6128; 4235
	(6,662)	(0.076)	(5,824)	(0.212)	(0.178)	
Panel B: Earnings from admin data (Balanced)						
Pre-K years	36,574	0.044	4,377	0.153	0.187	9139; 9139; 9139; 7244
	(2,569)	(0.027)	(1,838)	(0.068)	(0.066)	
Years after pre-K 1-2	38,802	0.035	5,231	0.164	0.162	9141; 9141; 9141; 7084
	(3,080)	(0.029)	(2,279)	(0.073)	(0.068)	
Years after pre-K 3-4	36,547	0.042	6,248	0.181	0.196	9141; 9141; 9141; 6784
	(3,238)	(0.032)	(2,560)	(0.086)	(0.069)	
Pooled post pre-k						
Years after pre-K 1-4	37,674	0.039	5,740	0.172	0.180	9141; 9141; 9141; 7229
	(3,073)	(0.028)	(2,300)	(0.075)	(0.061)	
	Weekly Hrs Cntrl Mn	Employed FT	Employed PT	Hours/ week		
Panel C: Survey data						
During pre-k	27.87	0.221	-0.133	12.80		726; 726; 721
	(5.89)	(0.138)	(0.119)	(4.25)		
After pre-k	37.44	0.00	0.08	1.48		497; 497; 487
	(5.46)	(0.11)	(0.09)	(3.91)		

Notes: This table reports IV estimate of equation 1 where outcomes are measures of parent earnings and labor supply. All specifications include controls for assignment propensity and demographics, as described in section 4.1. Rows are samples defined by time relative to pre-kindergarten enrollment. Panels A and B report results obtained using administrative earnings records. Column 1 is the control complier mean of base-period income, in dollars. The remaining columns report regression results for different outcome variables or specifications. "Work indicator" takes a dummy for annual earnings (computed as the sum of earnings over 4 quarters) being greater than 0 as the outcome. "Income (incl 0s)" takes dollar income as the outcome, including zero income data points. "Poisson IV" takes dollar income as the outcome but estimates a Poisson IV rather than a linear IV (see Online Appendix H). "Log income" takes the natural log of income as the outcome, restricting to positive income values. Panel A uses all available data in each specification. Panel B restricts the sample to a balanced panel of individuals in cohorts that we can follow through four years after pre-kindergarten. Panel C uses survey records to estimate the labor supply effects of UPK enrollment. Columns 2-4 are different survey measures of labor supply taken as outcomes in linear IV specifications; estimates are for the time period listed in the row. Sample sizes for each specification in a row are listed in the last column. Standard errors (in parentheses) are clustered at the applicant by parent level (Panel A and B, except for the Poisson specification) or the respondent level (Panel C). For the Poisson specifications in Panels A and B, we estimate standard errors using a bootstrap clustered at the application level with 500 bootstrap draws. See section 4.5 for details.

Table 6: Career continuity

	New main industry	One job over $ 4,000	Quarters earn. ≤ $ 4,000 (Incl. 0s)	Total qts earn. ≤ $ 4,000 since PK	N individuals
Disaggregated					
Pre-K years	-0.061	0.300	-0.206	-0.316	8743; 10621; 10727; 10727
	(0.028)	(0.108)	(0.097)	(0.143)	
Yrs after PK 1-2	0.001	0.220	-0.152	-0.659	8422; 10285; 10391; 10391
	(0.024)	(0.117)	(0.096)	(0.267)	
Yrs after PK 3-4	-0.019	0.132	-0.094	-0.761	7877; 9990; 10096; 10096
	(0.026)	(0.117)	(0.094)	(0.392)	
Yrs after PK 5-6	-0.003	0.043	0.020	-0.318	6306; 8221; 8327; 8327
	(0.032)	(0.143)	(0.112)	(0.562)	
Pooled post pre-k					
Yrs after PK 1-6	-0.007	0.139	-0.087	-0.648	9024; 10285; 10391; 10391
	(0.018)	(0.104)	(0.078)	(0.360)	
Yrs after PK 7+	-0.020	0.015	0.064	-0.984	4706; 6128; 6234; 6234
	(0.044)	(0.267)	(0.167)	(1.117)	

Notes: This table reports IV estimate of equation 1 where outcomes are measures of career continuity. All specifications include controls for assignment propensity and demographics, as described in section 4.1. Rows are samples defined by time relative to pre-kindergarten enrollment. Columns are outcomes. "New main industry" is an indicator equal to one if the industry in which an applicant earns the most money in the current academic year is different than the industry where they earned the most in the previous academic year. New main industry is only defined for individuals earning positive income in the current academic year. "One job over $4,000" counts the number of quarters in an academic year an individual has exactly one job and that job pays at least $4,000. "Quarters earn ≤ 4,000" counts quarters with earnings of less than $4,000 in an academic year. "Total qts earn ≤ 4,000 since PK" counts the total number of quarters, including those in the present year, in which an individual has earned less than $4,000 since the start of the pre-K years. Complier means for all specifications reported in Table A.4. Standard errors (in parentheses) are clustered at the applicant by parent level. See section 4.5.2 for details.

Table 7: UPK effects by family income tercile

	ACS 1st tercile		ACS 2nd tercile		ACS 3rd tercile	
	CCM	IV	CCM	IV	CCM	IV
Panel A: Substitution						
Weekly childcare hours (survey)	54	7	43	11	42	12
	(40)	(10)	(33)	(6)	(12)	(5)
Any pre-k or childcare (survey)	1.334	-0.036	0.944	0.063	1.081	-0.025
	(0.705)	(0.187)	(0.295)	(0.103)	(0.109)	(0.034)
Enrolled Head Start (admin)	0.318	-0.338	0.295	-0.229	0.055	-0.048
	(0.090)	(0.060)	(0.096)	(0.041)	(0.060)	(0.038)
Enrolled Head Start (survey)	0.417	-0.389	0.187	-0.190	0.219	-0.156
	(0.547)	(0.163)	(0.375)	(0.160)	(0.170)	(0.078)
Another public option (survey)	0.044	-0.052	0.111	-0.110	0.169	-0.151
	(0.233)	(0.061)	(0.218)	(0.100)	(0.168)	(0.084)
Other paid option (survey)	0.816	-0.565	0.597	-0.589	0.604	-0.660
	(0.754)	(0.192)	(0.416)	(0.168)	(0.247)	(0.113)
Monthly OOP costs (survey)	57	-68	596	-400	637	-552
	(727)	(132)	(468)	(188)	(312)	(145)
N individuals (survey)	175	175	250	250	273	273
N individuals (admin)	1402	1402	1281	1281	1259	1259
N applications (admin)	1558	1558	1397	1397	1400	1400
Panel B: Test scores						
Avg std score K	0.126	-0.176	0.168	0.295	0.217	0.012
	(0.138)	(0.183)	(0.156)	(0.108)	(0.095)	(0.115)
N individuals	2996	2996	2909	2909	2969	2969
N applications	3548	3548	3397	3397	3479	3479
Panel C: Parent Earnings						
Pre-K years	18,173	0.106	34,324	0.247	43,226	0.268
	(2,202)	(0.135)	(3,566)	(0.120)	(4,843)	(0.108)
Years after pre-K 1-2	20,064	0.034	36,826	0.277	42,390	0.293
	(2,348)	(0.155)	(4,326)	(0.159)	(5,588)	(0.138)
Years after pre-K 3-4	19,344	-0.063	36,776	0.351	41,100	0.176
	(2,462)	(0.140)	(4,529)	(0.150)	(6,178)	(0.138)
Years after pre-K 5-6	15,907	-0.015	36,629	0.336	43,238	0.192
	(5,086)	(0.282)	(5,077)	(0.232)	(7,532)	(0.157)
N applicants	3336	3336	3296	3296	3781	3781

Notes: This table reports estimates on substitution patterns, test score impacts, and parental wage income impacts by tercile of ACS median block-group household income, based on the residential address of those who applied for the UPK program. Terciles are based on lottery applicants with the 2nd tercile starting at $39,323, and the 3rd tercile starting at $69,619. All specifications are IV estimates of equation 1 except for parents' earnings which uses the Poisson IV specification (see Appendix H). All regressions include controls for demographics and assignment propensity, as described in section 4.1. Panel A reports substitution patterns from other pre-k programs, weekly hours of pre-k, and monthly out-of-pocket costs. The Head Start program from administrative data sources only considers the application years between 2015 and 2017, when the administrative data on Head Start is most complete. Implausible values of the complier control mean for "Monthly OOP costs" and "Another public option" for the first tercile are due to small samples. Panel B reports impacts on students' average kindergarten entrance inventory (KEI) scores. This is the Z-scored average of the various sub-tests (see Appendix E for details). Panel C reports estimates on parental wage income gains. The first two columns report the control-group complier mean and the Poisson IV estimate for the lowest ACS median household income tercile, while the remaining columns report the same estimates for the 2nd and 3rd terciles. Standard errors are clustered at the child level in Panels A and B, and at the applicant level for Panel C. See section 6 for details.

Table 8: UPK labor market effects by demographic group

	Black	Hispanic	White	Moms	Dads	1-Parent	2-Parents
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pre-K years	0.141	0.174	0.385	0.199	0.281	0.181	0.244
	(0.100)	(0.144)	(0.162)	(0.081)	(0.116)	(0.089)	(0.112)
	[26,029]	[27,999]	[43,027]	[28,252]	[39,750]	[27,179]	[36,778]
	<3,757>	<3,220>	<7,517>	<2,717>	<5,257>	<1,966>	<6,817>
	{4770}	{2236}	{2434}	{4996}	{2619}	{3944}	{1885}
Years after pre-K 1-2	0.043	0.129	0.513	0.187	0.292	0.152	0.245
	(0.110)	(0.171)	(0.205)	(0.103)	(0.125)	(0.103)	(0.133)
	[28,278]	[31,738]	[44,016]	[30,584]	[44,152]	[29,193]	[41,670]
	<4,373>	<4,376>	<6,381>	<2,924>	<5,020>	<2,726>	<6,537>
	{4629}	{2147}	{2354}	{4718}	{2449}	{3769}	{1738}
Years after pre-K 3-4	0.043	0.237	0.424	0.218	0.123	0.224	0.114
	(0.117)	(0.207)	(0.214)	(0.118)	(0.133)	(0.123)	(0.147)
	[25,490]	[28,091]	[38,786]	[28,455]	[41,588]	[27,296]	[39,395]
	<4,520>	<4,328>	<7,891>	<3,061>	<4,936>	<2,938>	<5,961>
	{4524}	{2075}	{2308}	{4489}	{2337}	{3593}	{1639}
Years after pre-K 5-6	-0.014	0.208	0.396	0.183	0.353	0.413	0.086
	(0.165)	(0.386)	(0.238)	(0.144)	(0.233)	(0.203)	(0.192)
	[25,458]	[29,882]	[36,410]	[27,378]	[31,891]	[22,135]	[37,348]
	<5,982>	<8,454>	<8,985>	<4,106>	<7,420>	<5,134>	<7,935>
	{3779}	{1629}	{1992}	{3016}	{1532}	{2470}	{1048}

Notes: This table replicates the Poisson specifications from Panel (A) of Table 5 for different subsets of applicants. Columns (1)-(3) restricts the sample to parents listed on applications from Black, Hispanic, and white students. Columns (4)-(7) restrict the sample to applications in 2014 or later, as starting in 2014 information on guardians was collected systematically on applications. Columns (4)-(5) restrict the sample to only mothers and only fathers listed on UPK applications. Finally, columns (6)-(7) restrict the sample to parents listed on applications that had one or two parents listed. Labor market outcomes are reported for the period the child was in pre-k as well as 1-2 years, 3-4 years, and 5-6 years after pre-k. Estimates are derived from Poisson IV regression run on annual income (including 0s) as described in Appendix H. Standard errors, in parentheses, are estimated via bootstrap using 500 bootstrap samples and are clustered at the applicant level. Complier control means are shown in square brackets. Their standard error, shown between the <> signs, is estimated via bootstrap using 500 bootstrap samples and are clustered at the applicant level. The number of observations is shown in curly brackets. All regressions include controls for applicant race, gender, age, and neighborhood characteristics, as described in section 4.1.