

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 earnings effects matter for evaluating the economic returns to UPK. Using a randomized lottery design, we estimate the effects of enrolling in an extended-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 effect fadeout and substitution away from other programs of similar educational quality. In contrast, UPK enrollment increases parent earnings by 21.7% during pre-kindergarten, and gains persist for at least six years after pre-kindergarten. Gains are largest for middle-income families. Earnings effects for parents have substantial consequences for cost-benefit analysis: tax revenue generated by parents' income gains reduces the net government cost of UPK by 90% compared to what we would have found without data on parent earnings. Under the conservative assumption that families value UPK at the cost of provision, each dollar of government expenditure on UPK yields \$10.04 in benefits. We show that while the benefits of UPK for children per dollar of government expenditure are lower than the benefits of many child-focused policies, the benefits of UPK for adults are high compared to other active labor market policies, and it is gains for adults that generate the high overall returns.

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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.

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 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 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 program design and evaluation.

A central challenge facing researchers is that it is hard to link random variation in children’s access to UPK to outcomes for parents. In this paper, we address that challenge head-on. We use data from admissions lotteries in a large extended-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 key finding is that earnings gains for parents are large, and that because of this the returns to UPK investment are high.

The New Haven Public Schools (NHPS) have offered extended-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. These lotteries provide the random variation in program access necessary for our empirical design.

¹Boston, 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.

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. Also as in other cities, New Haven’s UPK programs offer long hours. New Haven supplements a 6.5 hour academic day with before- and after-care for a total of ten hours of care during most of our study period. This is similar to the nine-hour extended day offered in Boston (BPS, 2024).

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. Due to the long UPK school day, children enrolling in UPK get an average of 11.3 more hours of childcare coverage per week.

Our second set of results considers how UPK enrollment affects children’s educational outcomes. We find little evidence that UPK affects 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 randomized evaluations of UPK programs in which early gains fade out after pre-kindergarten and in some cases 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 evidence of large and sustained earnings gains. 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. Earnings effects persist at similar levels for at least six years after pre-kindergarten ends. The average earnings effect over this period is 20.9% (SE=7.9%), or \$6,469 per year.

The sustained earnings gains appear to arise from a combination of modestly increased labor supply and reduced career disruption while the child is in pre-kindergarten. On the labor supply side, we find that labor force participation during the pre-kindergarten years rises by 5.7 percentage points (SE=2.6), or 7%, on a base of 81.7%. This effect persists at a roughly similar magnitude for the six years following pre-kindergarten.

Turning to job disruption, we find that children’s UPK enrollment reduces the likelihood that parents experience career gaps or switch industries during the pre-kindergarten period, while increasing the chances that parents hold a single high-earning job. These effects are sizeable. For example, UPK enrollment reduces the share of individuals who switch industry during the pre-kindergarten years by 34%. Qualitative reports lend further support to this idea. Survey respondents described how UPK helped them maintain their career paths, keep normal work hours, or work more effectively. Our interpretation is that the long-run labor market effects of UPK are likely due in large part to reduced career disruption (Goldin, 2014; Bertrand et al., 2010).

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 to dollars and discount all effects back to the time of application. We then apply the MVPF framework of Hendren and Sprung-Keyser (2020). The MVPF is the ratio of recipients’ after-tax willingness to pay (WTP) for the program to net program costs.

We find that the overall MVPF of the UPK program is high, and that earnings gains for parents are the crucial reason why. We focus first on the cost side. We show that tax revenues generated by parents’ earnings reduce the net cost of the program by at least 90% compared to what we would have found without earnings data. Concretely, the total upfront cost of the program is \$24,200. Substitution away from other government programs reduces this total cost by \$8,800, and tax revenue from children’s projected future earnings reduces it by a further \$440 under our baseline score estimates. Taxes from parents’ income then bring net government costs down by 90%, to \$1,500. Thus, for any given value of WTP, net cost reductions from parents’ earnings gains multiply the MVPF by 10. Under more optimistic assumptions about gains for children based on estimates from Lipsey et al. (2018) and Gray-Lobe et al. (2023), the net cost of UPK becomes negative, meaning that the program generates revenue for the government.

We then turn to the benefits side. A challenge here is that families’ willingness to pay for the program depends on their ability to purchase substitutes for full-day care in the private market and on how parents weigh benefits for children when making

decisions about labor supply. Our approach is to provide a variety of estimates that bracket the true effect. These estimates yield MVPF values ranging from 4.5 to 39.6. Our benchmark approach, in which families value UPK at the net cost of provision, yields an MVPF of 10.04: each dollar of spending on UPK generates \$10.04 in benefits.

How does the cost-benefit proposition posed by UPK compare to other programs? From the parent side, UPK performs well. Focusing on our benchmark approach, a “no-kid” evaluation of UPK that excludes gains for children yields an MVPF of 7.8. 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 of around 1. In contrast, our benchmark approach yields a no-parent MVPF (i.e., excluding earnings gains for parents from WTP and net cost) of 1.03. Even under more optimistic assumptions, this child-focused MVPF does not exceed 1.32. This estimate is well below estimates reported in [Hendren and Sprung-Keyser \(2020\)](#) for a variety of policy interventions targeted at children, such as increased school spending, expanded health insurance, and expanded college access.

How are the benefits of UPK distributed? To answer this question, we split our analysis by terciles of pre-enrollment neighborhood income. We first show that, because the UPK program 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 parents’ earnings gains are large in the top two terciles and close to zero in the bottom tercile. The result is that the net government costs of UPK enrollment are negative in the top two terciles, which means the policy is revenue-increasing and MVPFs are infinite. This finding 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](#)). We conclude that benefits mostly accrue to middle-income families and that means-testing would have minimal effects at high levels (because high-income people do not apply) but would reduce economic returns at lower levels.

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. Taking into account both earnings and educational gains, lower-middle-class families are arguably the main beneficiaries of the UPK policy.

We contribute to several strands of literature. Most directly, we build on research

that seeks to evaluate the labor market effects of other US 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. For example, Fitzpatrick (2010), which Cascio (2021) cites as the most convincing available evidence, estimates an intent-to-treat effect of UPK eligibility on mother’s annual 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 (to match our unit of measure) yields an IV estimate of \$6,531, with a standard error of roughly \$11,400. This point estimate is larger than what we find, and the 95% CIs include large positive and negative values. 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, public kindergarten programs, and universal pre-kindergarten programs outside the US (Gelbach, 2002; Cascio, 2009; Havnes and Mogstad, 2011a; Fitzpatrick, 2012; Sabol and Chase-Lansdale, 2015; Chaparro et al., 2020; García et al., 2020; Schiman, 2022; Attanasio et al., 2024; Wikle and Wilson, 2023). These settings differ from the contemporary US UPK context. For example, means-tested programs target low-income groups for whom labor market opportunities are, by definition, more limited. Evaluations of demonstration programs, kindergarten programs, and universal programs in developed countries are based on data from the 1970s through the early 2000s, and do not capture the effects of the massive increase in childcare costs in the US that began in the late 1990s and continues through the present (Swenson and Burgess Simms, 2021). In contrast, our evaluation of a contemporary UPK program speaks directly to the ongoing debate over UPK policy in the US.

Finally, we contribute to the literature on the educational effects of UPK. Our findings on test scores echo results from randomized evaluations of programs in Boston, Tennessee, and Georgia (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 program evaluation by using our survey and administrative data to consider both the costs of UPK (Kabay et al., 2020) and the costs of counterfactual programming. Our analysis of how achievement and cost effects depend on substitution between UPK and other programs brings insights from Kline and Walters (2016) 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 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 extended-day 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 extended-day 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. There were between 1.5 and 4 slots available for every

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

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 B 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 extended-day care 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 Childhood, 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 enroll their children in private pre-kindergarten programs without any subsidy. Online Appendix C 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.

³Source: authors’ calculations from Office of Head Start data.

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

⁵We do not observe program location for Care 4 Kids enrollees, so we classify these students by their

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. For comparison, Head Start accounts for 34% of enrollment and Care 4 Kids accounts for 28% of enrollment.

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 (BPS, 2024). New York City also offers free, untargeted pre-kindergarten programming, does not guarantee spots at neighborhood schools, and resolves excess demand for popular schools using centralized systems (NYCPS, 2023; Shapiro, 2023). While New York does not offer extended-day coverage to all students, it offers a minimum day length of 6.5 hours, and provides extended coverage for some students (NYCPS, 2024). Washington, DC provides a 6.5 hour day at minimum, with extended hours at many sites (DCPS, 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 Online Appendix D for more details.

Though the New Haven program has much in common with major *city*-level UPK

town of residence.

programs across the country, it differs in important ways from some *state*-level policies. Foremost among these is the length of the school day. Many states with UPK policies on the books require only a 2- to 3-hour school day (Friedman-Krauss et al., 2023). These states include Oklahoma, the focus (along with Georgia, which requires a 6.5 hour day) of major existing evaluations of UPK’s labor supply effects (Fitzpatrick, 2010; Cascio and Schanzenbach, 2013). While our analysis of the NHPS UPK program may have implications for state-level policymaking, our findings likely apply most directly to the full- or extended-day UPK programs common in major cities.

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 the end of middle 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. The kindergarten assessment is known as the Kindergarten Entrance Inventory (KEI) and includes measures of literacy and numeracy, as well as social skills, physical skills, and creativity. Assessments in grades 3-8 are statewide accountability exams in math and reading. We obtain our main achievement measures by averaging across standardized subscores. 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 includes all students who enrolled in any public pre-kindergarten program in New Haven County over the 2003-2021 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 0.123σ higher than the full sample of pre-kindergarten enrollees in New Haven County on their KEI. This gap diminishes to 0.069σ and 0.038σ 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 comes 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%. This is similar to the 63% of children five and under in New Haven County who are observed living in two-parent households in the 2019 ACS. 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 9,729, 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 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

⁶Omitting the topcoding step does not change our findings.

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 are not in our administrative data. These outcomes come in two types. The first type 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%. These low rates are 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-universal 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 household 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 not in a formal childcare or pre-kindergarten program 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 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. First, they do not cover students who enroll in private pre-kindergarten programs without state subsidies. Second, 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 private providers do not always 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

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

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 in the September following the application 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 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 approach amounts to dropping the indicators for assignment propensities from the control set and instrumenting with $\tilde{Z}_i = Z_i - P_i$.

⁷Online Appendix Table A.2 provides a list of these covariates.

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 included 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 at most 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 who receive 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 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,

⁸Public 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.

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 matching, 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. For the parent earnings sample we additionally test for balance on 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 enrollment in public programs anywhere in the state, as well as the use of public subsidies at private programs. IV specifications using these records show that enrolling in UPK reduces the share of students enrolling in Head Start by 13.9 percentage points, the share enrolling in School Readiness by 7.7 percentage points, the share enrolling in Care 4 Kids by 5 percentage points, and the share enrolling in any other pre-kindergarten program appearing in SDE or OEC records by 17 percentage points. Overall, UPK enrollment raises the rate at which students enroll in any observed program by 39.2 percentage points. 62.4% of compliers would otherwise attend some form of public or subsidized care.¹⁰

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 37.2 percentage points.

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, enrolling in UPK reduces the share of students who enroll in a private or paid pre-kindergarten program by 62.3 percentage points. 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 or other childcare outside the home. 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 \(2016\)](#) reports 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 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

¹⁰Children 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.

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.

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 (0.062σ ; $SE=0.072$). Point estimates in grades 3-8 are negative. Online Appendix Table A.3 reports results for each KEI subscore; we cannot reject a null of zero for any subject, including personal/social skills.

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, 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

¹¹Note that the estimated complier control mean of 51.24 has a large standard error (13.0) and is far above the control mean of 29.1.

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 annual 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 UPK enrollment raises parent income by \$5,461 (SE=\$1,717) during the child’s pre-kindergarten years. 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.

¹²We 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.

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 percentage effects from the Poisson IV specification, pooling data from 11 or more years 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 through six years after kindergarten, with earnings effects between 19% and 23% in each two-year interval. A pooled specification combining years 1-6 after pre-kindergarten yields an estimated earnings gain of 20.9% (SE=7.9%) over the full period.

After six years, standard errors grow. Pooling years seven and later in our Poisson specification yields estimated earnings gains of 9.2%, with a standard error of 21.2%. We cannot rule out null effects in years seven and later, but we also cannot rule out gains similar to what we observed in the first six years after pre-kindergarten.

Estimates from our other specifications follow similar patterns. Pooling over the first six years after pre-kindergarten, the earnings effect of UPK enrollment is \$6,469 (SE=\$1,838) in dollar terms and 18.7% (SE=5.8%) in logs. Rates of labor force participation rise by a modest 4.8 percentage points (SE=0.028). Similar to our Poisson specifications, standard errors increase substantially when we look beyond six years.

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 the 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.

Panel C of Table 5 presents results that take survey reports of weekly work hours in

the year following the 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. Labor supply effects decline after the pre-kindergarten years, though estimates are noisy and we cannot rule out sustained gains. The estimated effect of UPK enrollment on work hours in the years after pre-kindergarten is 1.48 (SE=4.25). Considering the evidence from both the survey and administrative data, our interpretation is that there is strong evidence of labor supply gains during pre-kindergarten. Results from administrative data suggest that moderate increases may persist after pre-kindergarten.

4.5.2 Reduced career disruption

The large, sustained earnings effects we find are similar to estimates of the effects of career disruptions reported in prior studies, but with the opposite sign. 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. Studying a very different population, [Jacobson et al. \(1993\)](#) find that high-tenure workers displaced from distressed firms see their earnings fall by about 25% over the long run. In contrast, our effect estimates are larger than might be expected based on a pure returns-to-experience model. 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.

We use our data to test the hypothesis that UPK enrollment reduces career disruptions. We do not observe 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 use these variables as outcomes in Equation 1.

Table 6 reports our results. The first column of Table 6 takes an indicator for switching main industry since the prior academic year as the outcome of interest, which we interpret as 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 7.2 percentage points (SE=2.7), 34% of the control complier mean of 0.21. 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 that pays 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 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

Results from this exercise show that children’s UPK enrollment substantially reduces the likelihood that parents will experience career disruptions. These findings help rationalize the long-run earnings effects we observe in Table 5, and suggest that the beneficiaries of urban UPK programs like the one we study face high costs of career disruption. As we discuss in Section 8, our findings here are also 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 to net program costs, i.e., $MVPF = \frac{\Delta W}{\Delta E - \Delta C}$, where ΔW represents beneficiaries’ willingness to pay for the program, ΔE measures upfront program costs, and ΔC measures reductions in government expenditures or increases in government revenue due to the long-run effects of the policy. We compute ΔW , ΔE

¹³Note 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.

¹⁴This 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](#)).

and ΔC using estimates from our randomized design. See Online Appendix I for details of the calculations summarized here.

5.1 Government costs

We begin by considering the denominator of the MVPF, $\Delta E - \Delta C$. The gross cost of the program, ΔE , is approximately \$24,000 per child, which is the yearly PPE times the average of 1.56 years enrolled.

Gross costs to the government are offset by two kinds of cost reductions that result from program enrollment. The first type is substitution away from other subsidized programs. 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.¹⁵ 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.¹⁶ We estimate that substitution from other publicly funded programs reduces the net cost by \$8,800.

The second type of cost reduction arises from changes in current and future tax revenue from parents and children. For changes in parents' wage income, we calculate the gains in discounted after-tax income using the impacts on individual wage income reported in Table 5. We conservatively assume no impact beyond six years after the UPK program ends, and scale individual earnings effects by 1.56, the average number of adults per family in the years for which we systematically observe multiple family members, as reported in Table 2.¹⁷ This approach yields an average change of \$67,100 in cumulative family income. 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 between 100 and 400 percent of the poverty line. The present discounted value of additional tax revenue from parents as a result of the policy is thus $0.2 \times \$67,100 = \$13,400$.

For changes in the wage income of children, our baseline approach combines our findings on the effects of UPK on children's kindergarten test scores with estimates

¹⁵We 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.

¹⁶If 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.

¹⁷Treatment effects on earnings during these years are nearly identical to those in the full sample, as we report in Table 9.

from [Chetty et al. \(2011\)](#) on the relationship between kindergarten scores and adult income. 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](#)). This approach follows [Kline and Walters \(2016\)](#) and [Cascio \(2023\)](#). We obtain estimated gains of \$2,200 before taxes, yielding $0.2 \times \$2,200 = \440 in additional tax revenue.

Putting these numbers together shows that the inclusion of tax revenues from parent earnings profoundly changes government costs of UPK. Panel (a) of Figure 6 illustrates this point. We start with \$24,200 in upfront program costs. Accounting for savings from substitution across programs reduces government costs by \$8,800, to about \$15,400. Accounting for increased revenues due to taxes on children’s increased earnings reduces net costs by a further \$440, to about \$15,000. Accounting for tax revenue from parents’ increased earnings reduces net costs by \$13,400, to \$1,500. Put another way, accounting for the fiscal externality from parents’ additional earnings reduces the net government costs of UPK by 90% relative to estimates that ignore earnings gains for parents. It therefore raises the MVPF by a factor of 10 for any fixed value of willingness to pay.

This finding is even more pronounced under alternate assumptions about children’s earnings gains. Recall from our discussion in Section 4.4 that UPK enrollment might raise children’s earnings in the long run, even if these gains are not observable in kindergarten. With this in mind, we repeat the analysis in Panel (a) of Figure 6 under alternate assumptions about children’s earnings gains. The first of these takes [Lipsey et al. \(2018\)](#)’s estimates of a 0.4σ gain in pre-kindergarten scores from random assignment to Tennessee’s UPK program and applies the [Chetty et al. \(2011\)](#) conversion factor to project earnings.¹⁸ This approach yields an estimated pre-tax earnings gain of \$14,100, leading to \$2,800 in additional tax revenue. The second 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 pre-tax gain of \$12,700, leading to \$2,500 of additional tax revenue. Under either of these alternate approaches, including parent income in the cost calculation leads to negative estimates for net government costs: UPK increases government revenues. As reported in Table 7, the result is an infinite MVPF regardless of willingness to pay.

¹⁸This 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.

5.2 Willingness to pay

We next consider ΔW , the willingness to pay for the benefits that accrue to parents and children. Because UPK is an in-kind transfer, computing willingness to pay presents a conceptual challenge. How much families value the transfer depends on whether parents can purchase substitutes for full-day UPK in the market and on the degree to which parents internalize benefits for children when making choices about childcare and labor supply. Our approach is to describe several different methods for computing WTP, which we believe are likely to bracket the true effect.

Our starting point is to assume families value UPK at the net cost of provision. This assumption parallels procedures for calculating MVPFs of pecuniary transfers ([Hendren and Sprung-Keyser, 2020](#)). An attractive feature of this approach is that the MVPF would be equal to one in the absence of a fiscal externality from increased tax revenue; it therefore helps describe the effects of the parents' and kids' earnings channels that are central to our empirical analysis.

The alternative is to take a hedonic approach that attempts to quantify the value of different elements of the UPK bundle to families. We consider three channels through which families may benefit. The first is changes in out-of-pocket childcare costs. We assume that families value cost reductions dollar-for-dollar. Our view is that cost reductions likely understate the value of the UPK childcare bundle to parents, because UPK offers services (such as extended hours) that other pre-kindergarten options do not fully replace.

The next channel we consider is children's after-tax future earnings. Past attempts to calculate MVPF values for UPK programs have included the PDV of children's after-tax earnings as part of WTP for the program ([Cascio, 2023](#)). This is consistent with [Hendren and Sprung-Keyser \(2020\)](#)'s argument that earnings gains from increases in human capital should be included in WTP calculations. As we show in Online Appendix I, it makes sense to include children's after-tax earnings in WTP calculations if one believes that parents either cannot purchase substitutes for full-day UPK on the private market or can purchase these substitutes but do not incorporate earnings gains for children when making choices about childcare and labor supply. However, if one believes that parents can purchase substitutes for full-day UPK in the private market and fully or partially incorporate gains for children into their decision process, then earnings gains for children should be fully or partially dropped from WTP.

The final channel we consider is parents' after-tax earnings. As with earnings gains for children, whether one should include parents' earnings gains in WTP depends on the underlying economic model. Suppose parents have access to private-market substitutes for full-day UPK and make labor supply choices optimally. In that case, the welfare benefits of earnings gained from access to UPK may be fully offset by the disutility

of work and should not be included in WTP. Alternatively, if parents lack access to full-day care in the private market, they may value additional earnings up to dollar-for-dollar. In this model, access to UPK is best thought of as a scarce opportunity to make a high-return investment in one’s own human capital. A third possibility is that parents make labor supply choices that are statically optimal but do not internalize the dynamic returns to job continuity (Costa-Ramón et al., 2024). In this case, gains in earnings during pre-kindergarten may be welfare neutral, while gains in future earnings may be welfare-enhancing. See Online Appendix I for details.

The data provide some guidance about which approach to choose here, but are not conclusive. In particular, the evidence that parents face constraints on access to full-day care outside the UPK program is strong but not definitive. The two primary points in favor of this proposition are the limited hours of care available at non-UPK childcare centers (see Section 4.3 and Online Appendix G) and parents’ qualitative reports of challenges balancing work and childcare when not enrolled in UPK, documented below in Section 8. These points supplement the observation that childcare may also be hard for parents, and any disutility of market work should likely be measured relative to this benchmark rather than traditional “leisure.” We therefore present estimates that include parents’ after-tax earnings gains in willingness to pay, estimates that exclude parents’ earnings, and estimates that include only earnings realized after pre-kindergarten.

Measurement proceeds as follows. To compute the reduction in out-of-pocket costs, we take our cost results from Table 4 and assume that this reduction applies for nine months per year and that the average years enrolled in the pre-kindergarten program is 1.56.¹⁹ We estimate a \$5,200 reduction in out-of-pocket costs. For parents’ earnings and children’s earnings we use the procedures described above in our discussion of net costs, now focusing on after-tax income. Panel (b) of Figure 6 reports the value of each possible component of willingness to pay discussed here.

5.3 MVPF results and decomposition

Panel (c) of Figure 6 reports our estimates of the MVPF of UPK under a variety of assumptions about the inputs to willingness to pay. Each group of bars presents a series of MVPF calculations under a different treatment of the WTP inputs, with a description listed on the horizontal axis. Within each group, the different bars decompose the overall MVPF by excluding different elements from the calculation. Table 7 reports MVPFs and 90% CIs for these estimates as well as for additional MVPF calculations.

Our first result here is that the overall MVPF of UPK is high regardless of how we compute willingness to pay. This result is shown in the leftmost dark orange bars in

¹⁹This 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.

each group. When WTP is set to net government cost, we find an MVPF of 10.04 (90% CI=[1.81, ∞]): the policy generates ten dollars in benefits for each dollar of government expenditure. Recall that this MVPF deviates from one only due to the fiscal externality from child and parent earnings.

Moving to the right, our most conservative assumption on WTP does not include parent earnings at all, and values UPK as the sum of the reduction in parent costs and the increase in children’s earnings. Here we find an MVPF of 4.53 (90% CI=[0.60, ∞]). Adding parents’ after-tax earnings back into the WTP raises the MVPF to 39.56 (90% CI=[3.96, ∞]), i.e., almost \$40 in benefits per dollar of government expenditure. Including only the share of parents’ earnings gains realized after pre-kindergarten yields an MVPF of 32.67 (90% CI=[3.29, ∞]).

If one believes that NHPS’ UPK programs had achievement effects similar to those reported in previous studies, these effects rise further. As reported in Panel A of Table 7, we estimate infinite MVPFs for all WTP assumptions when we impute children’s earnings gains on the basis of [Lipsey et al. \(2018\)](#) or [Gray-Lobe et al. \(2023\)](#). This result follows directly from our analysis of net costs above, which showed that government net costs were negative in these two scenarios.

Our second result is that measuring earnings gains for parents is crucial to understanding the return on UPK investments. The dark purple bars second from left in each group report results from MVPF calculations that exclude parent earnings from both the WTP (when applicable) and the net costs. The resulting MVPFs are much lower. Using our cost-based approach to WTP, we find an MVPF of 1.03, just above the minimum possible value of 1. This is because the fiscal externality from children’s additional earnings is small. Hedonic approaches that exclude parent income yield an even lower MVPF value of 0.46.

This finding holds regardless of how we compute earnings gains for children. The light orange and white bars, third and fourth from the left in each group, repeat the no-parents MVPF calculation from the second bar, but replace our baseline estimates of kids’ score effects with alternate earnings projections based on an assumption that children’s scores rise by 0.4σ as in [Lipsey et al. \(2018\)](#) or that UPK raises rates of college-going by an amount similar to [Gray-Lobe et al. \(2023\)](#). MVPFs rise slightly under these alternate assumptions, but remain close to one.

Our third finding is that the gains for parents alone—i.e., excluding benefits for children—yield high MVPF values. The rightmost bar in each group reports MVPF estimates that exclude children’s earnings gains from both WTP and net costs. Using cost-based WTP, the no-kids MVPF is 7.83. Hedonic values range from 2.65 under the conservative OOP-only WTP to 29.98 when all parent income is included in WTP.

5.4 Comparison to other programs

From a policy perspective, the crucial question is how 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 “no-parent” and “no-kid” MVPF estimates for New Haven’s UPK program, as well as for [Cascio \(2023\)](#)’s UPK MVPF estimate, which excludes parents.²⁰ 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, relatively few programs targeted at adults 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 a “no-parent” evaluation of UPK (i.e., one that excludes earnings gains for parents from WTP and net costs) does not offer a high MVPF compared to other child-focused policies. The benefits for children from UPK are relatively modest regardless of how they are calculated, and the other child-focused options offer high returns.

The second is that, even excluding gains for children, UPK offers high returns compared to other policies targeting adults. The “no-kid” UPK MVPF (i.e., excluding gains for children) is higher than the MVPF for all the job training policies, all the unemployment insurance policies, and all the cash transfer policies (the category which includes EITC, Paycheck plus, and Negative Income Tax programs) evaluated in [Hendren and Sprung-Keyser \(2020\)](#).²¹

5.5 Alternate cost-benefit calculations

Online Appendix I considers various alternative approaches to MVPF calculation. Our estimates are qualitatively similar under a variety of different assumptions, such as using alternate tax rates, using survey measures of substitution across programs rather than administrative measures, or an alternative construction of per-pupil expenditure based on [Kabay et al. \(2020\)](#). When WTP is the net subsidy received by families, the lowest alternate MVPF is 2.3. We obtain this value under the assumption that a) capacity constraints on alternative childcare programs bind, so there are no cost

²⁰[Cascio \(2023\)](#) reports MVPFs under different assumptions about program costs. The estimate we plot assumes that UPK program outlays are equal to K-12 spending on a per-pupil basis because that assumption most closely matches our spending analysis for the NHPS program.

²¹[Katz et al. \(2022\)](#) note that there are sector-specific training programs, such as Year Up, which may have MVPFs well above one if observed early earnings gains, not included in [Hendren and Sprung-Keyser \(2020\)](#)’s main analysis, persist.

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, while simultaneously assuming a worst-case scenario regarding the costs of those programs.²² 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 explore 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 8.

An important preliminary point here is that the UPK program draws students mainly from low- and middle-income families. Even the higher-income families in our analysis fall in the middle of the population distribution. As reported in Figure 8, neighborhood median incomes for the middle tercile of the applicant pool fall between about \$40,000 and about \$70,000. \$70,000 is approximately the mean of the population distribution for households with children in New Haven County. Similarly, as reported in Table 8, 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 8 reports how UPK enrollment affects childcare usage in each

²²Results are similarly robust for our other constructions of willingness to pay, except when WTP is conservatively assumed to only include out-of-pocket costs and kids’ future after-tax wage gains, where two of the thirteen estimates are below one. One has to work hard to push the UPK MVPF down to levels commonly observed for active labor market policies.

income tercile. Focusing first on hours, we find that point estimates for weekly gains are larger in the middle and top terciles (11 and 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 mainly 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 8 reports these results. UPK enrollment raises test scores for middle-tercile students by 0.30σ . 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 9 plots test score effects by grade and neighborhood income tercile. In the middle tercile, 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 Online 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 8 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. Our results are consistent with findings that gains from expanded work hours are larger for (relatively) higher-skilled workers, who may be in better position to climb the career ladder (Kuhn and Lozano, 2008; Cortés and Pan, 2019). Taking both educational and labor market effects into account, lower-middle-income families appear to be the biggest beneficiaries of the UPK program.

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. 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 scores from Table 8.

The central result here is that MVPFs are infinite (i.e., net costs of UPK are negative) in the top two terciles because that is where earnings gains are largest. Figure 10 repeats the net government cost analysis from Figure 6 but split by tercile of neighborhood income. Upfront costs are similar across the three groups. The small differences we do see are driven by differences in the number of years enrolling students in each group attend a UPK program. Savings from government costs are largest for the lowest tercile (roughly \$13,000, compared to \$8,000-\$9,000 in the upper terciles) because this group is more likely to substitute away from other subsidized programs. Additional tax revenue from children is fairly small relative to costs across all three groups. The additional tax revenue from parents is much larger in terciles two and three (\$18,000 and \$20,000, respectively) than in the bottom tercile (\$3,000). Together with the program substitution effects, the additional tax revenue from parents is large enough to push net costs negative in the top two terciles.

In short, the bulk of the return to UPK expenditure comes from middle- and higher-income families within the applicant pool. Recall 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 income in our top tercile.

6.3 Other demographic categories

Table 9 reports the effects of UPK on parents' earnings by race, relationship to child, and family structure using our preferred Poisson specifications. 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.

Columns 4-8 of Table 9 restrict the sample to applications after 2013, when the application form encouraged listing multiple adults and recorded the relationship between the adult and the child. Column 4 shows that estimates from the post-2013 sample are very similar to estimates from the full sample. Splitting by the relationship between the adult and the child (columns 5 and 6), we find evidence of sustained gains for both mothers and fathers. We cannot reject the null that effects for mothers and fathers are equal during ($p = 0.505$) and after pre-kindergarten ($p = 0.748$).

In the final two columns, we split by observed family structure: specifically, whether one or two parents are listed on the application form. We cannot reject the null that effects for the two groups are equal during pre-kindergarten ($p = 0.693$). After pre-kindergarten, gains for one-parent families are larger, though again we cannot reject the null that effects are equal ($p = 0.645$).

7 Discussion

7.1 Credit constraints and the market for childcare

We show that parents' earnings returns from enrolling their children in UPK outstrip 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 a role for credit constraints. We estimate that enrolling in UPK increases after-tax family income by \$10,549 during pre-kindergarten, while the additional childcare costs are \$10,174. On average, earnings gains realized during pre-kindergarten are fully offset by childcare costs. Given the similarity in average values, it is likely that childcare costs are larger than contemporaneous earnings gains for many families.

To sum up, our finding that UPK access pays off over the long run—not just while children are enrolled in pre-kindergarten—helps rationalize the surprising absence of privately-provided substitutes. We also note that, if credit constraints do bind, they provide a rationale for transfers to parents of young children that is not captured by our MVPF analysis above—namely, that the marginal value of consumption for this group is high. See Online Appendix I for details.

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.²³ 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 no-parent MVPF reported in Figure 6 to match the no-kid MVPF of 7.83. We find that UPK would need to raise children’s scores by about 1.9σ . This is equal to about 2.5 times the estimated IQ gains for four and five-year-olds from the Perry Preschool project ([Heckman et al., 2013](#)) and three times 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 top-tercile applicants and higher quality than the programs chosen by middle-tercile applicants. As a consequence, the New Haven UPK program raised parents’ earnings without compromising

²³[Povich \(2024\)](#) discusses the challenge of expanding hours in UPK programs.

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, 2011b](#)).

7.3 Comparison to other childcare interventions

The contemporaneous earnings effects we estimate are similar to those observed for universal childcare programs in other contexts or for children of 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. Our results are also consistent with results for increasing the length of the kindergarten school day. [Gibbs et al. \(2024\)](#) studies the impacts on maternal labor supply from increasing kindergarten from part- to full-day, using variation across states and over time from 1992-2022. They find that moving from part- to full-day kindergarten increases maternal employment by 4.5 percentage points, with both increases in part-time and full-time work. Similar to our findings, the gains are broadly realized and not only for disadvantaged mothers.

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 11 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 11 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 while reducing out-of-pocket costs. They also suggest that UPK programs may benefit children in ways that are not captured by test scores.

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). These responses are consistent with our findings of increased labor supply during pre-kindergarten in both the administrative and survey data.

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 and the challenges facing parents trying to purchase similar care on the private market. 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 helps people work in their preferred jobs, work more, work more effectively, and invest more in their careers. This is consistent with our finding of sustained labor market gains after children age out of UPK.

9 Integration effects

While the primary 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 after pre-kindergarten, the program raises the share of White and Asian-American students in the district, especially in early grades.

10 Conclusion

This paper uses randomized assignment to an extended-day 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.

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 high. Incorporating the fiscal externality from parents’ income gains reduces the net government costs UPK by 90% relative to what we would have obtained without access to data on parent earnings. Our benchmark estimates, which we believe are fairly conservative, value UPK at net cost and yield an MVPF of 10: the program yields \$10 in benefits for each dollar of government costs. An otherwise identical calculation that omitted gains for parents would have estimated an MVPF of 1.03. We conclude that the labor market gains for parents are crucial to the accurate cost-benefit evaluation of UPK programming. In cost-benefit terms, contemporary extended-day UPK programs are perhaps best thought of as high-return active labor

market policies for adults.

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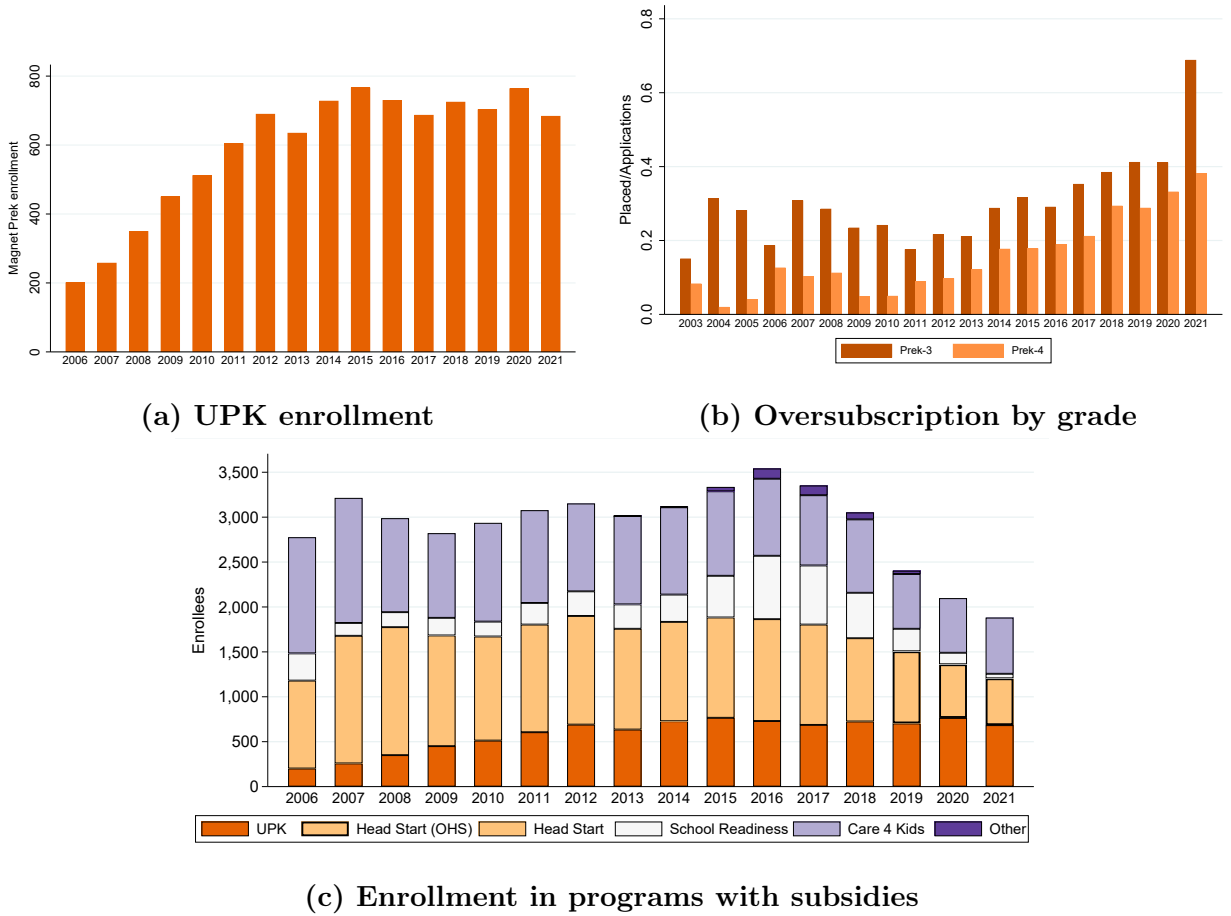
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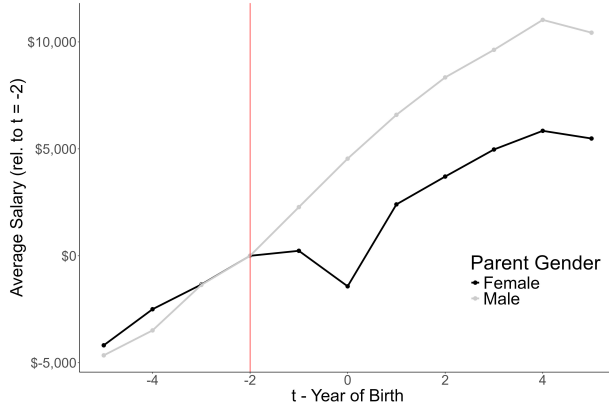
Figures

Figure 1: New Haven pre-kindergarten enrollment and applications

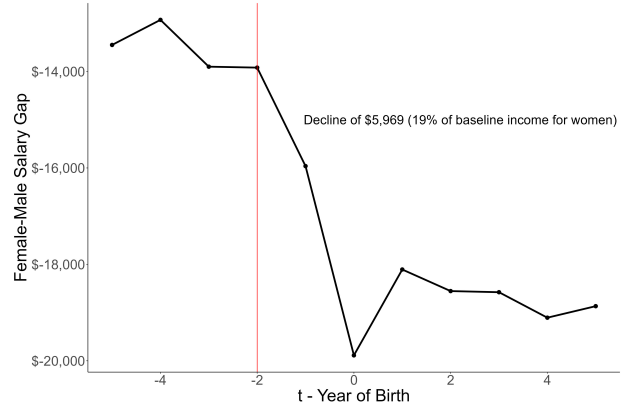


Notes: Panel (a): total enrollment in NHPS UPK programs by year. Panel (b): the ratio of placement counts to unique applicants by application grade and lottery year. Panel (c): enrollment in subsidized New Haven pre-kindergarten programs among three- and four-year olds by application year. Head Start enrollment counts are imputed from 2019, 2020, and 2021 OHS data. Care 4 Kids, School Readiness, and Other enrollment bars impute unique enrollment counts from data on enrollment spells, using the ratio of spells to unique students among applicants to the UPK program. 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.

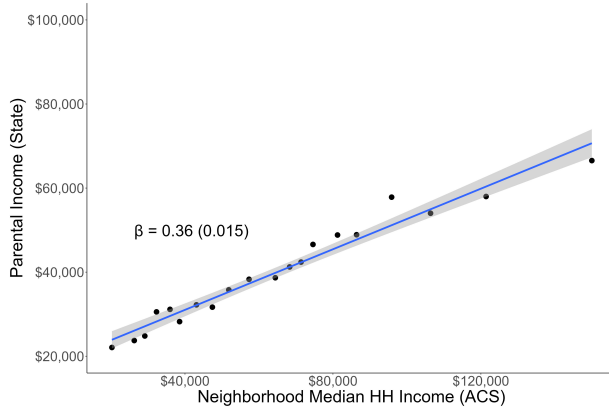
Figure 2: Validating administrative and survey data



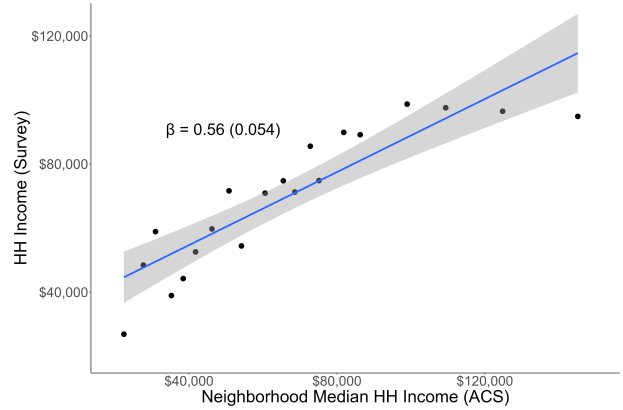
(a) Mothers' and fathers' income around childbirth



(b) Parental income gap around childbirth



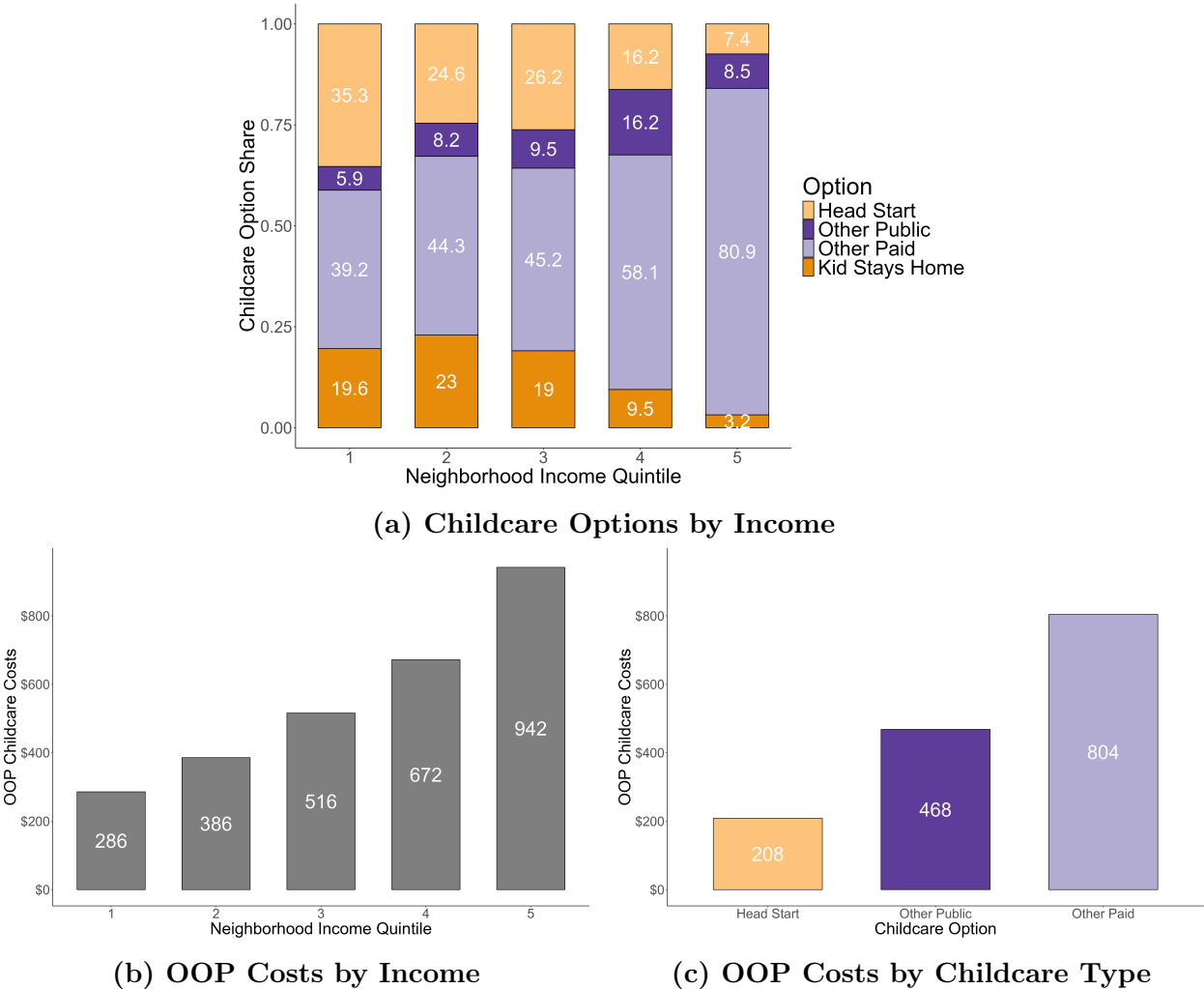
(c) Parental income (state) vs. HH income (ACS)



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

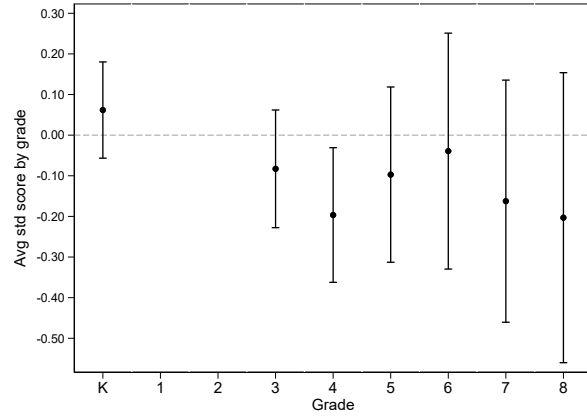
Notes: Panels (a) and (b) show the evolution of mothers' and fathers' incomes around the year of birth for future UPK applicants. Panel (a) normalizes incomes to 0 in period $t = -2$ and plots mothers' income in black and fathers' income in gray. Panel (b) plots the gap between moms' and dads' incomes 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

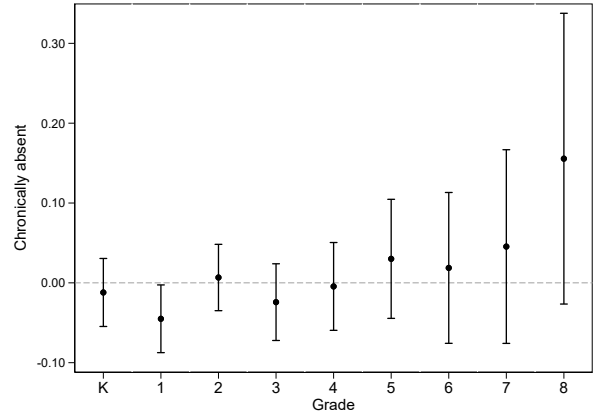


Notes: This figure shows the composition of outside options for childcare and the out-of-pocket (OOP) costs associated with them based on data from our survey of lottery applicants. Panel (a) shows the composition of non-UPK childcare options for children who applied but did not enroll in UPK. Each bar shows the composition for a specific neighborhood 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.

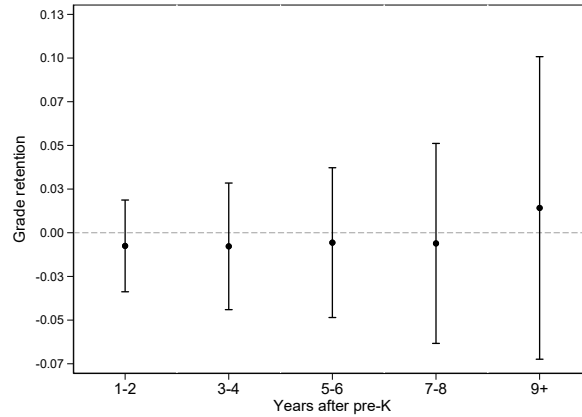
Figure 4: The effects of UPK on children's academic outcomes



(a) Test scores



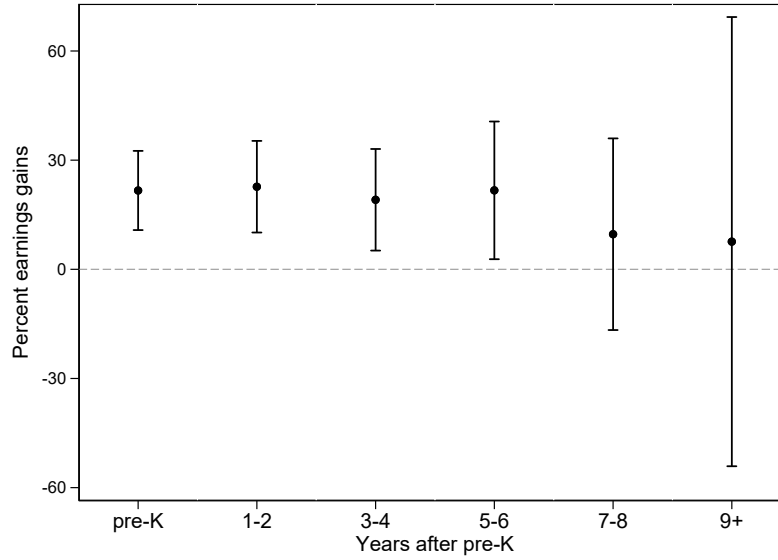
(b) Chronic absenteeism



(c) Grade retention

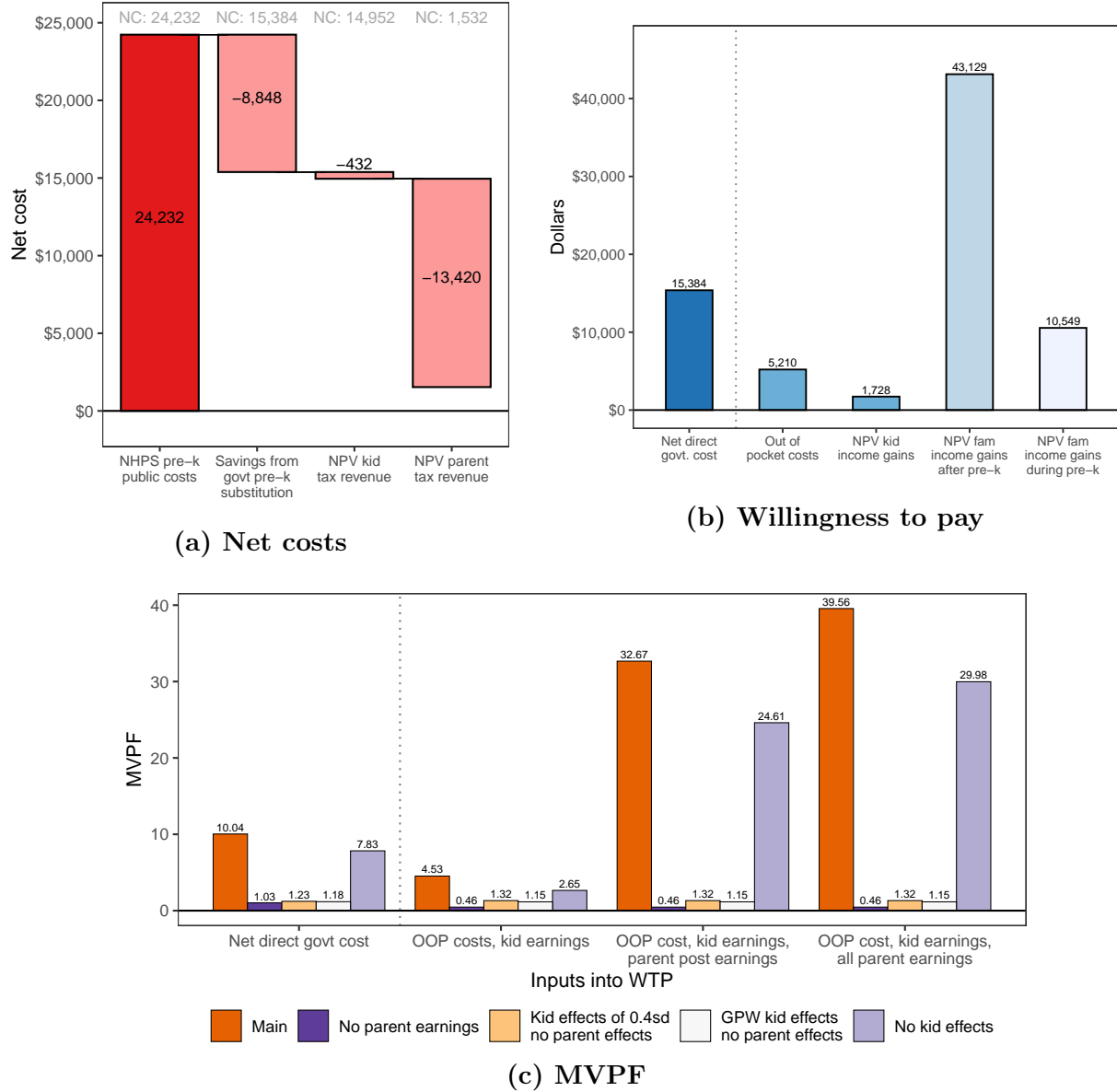
Notes: This figure shows IV estimates of the effect of UPK enrollment on average standardized test scores, chronic absenteeism, and grade retention. Panels (a) and (b) show effects by grade, and (c) shows effects by years after pre-kindergarten. Test scores in Panel (a) are standardized to have a mean 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. Each point estimate uses all available observations in a given grade for students subject to random assignment. 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



Notes: This figure reports Poisson IV estimates where the outcome is parent income in dollars and the endogenous regressor is UPK enrollment. We report $\exp(\hat{\beta}) - 1$, which is an estimate of the proportional change $(E[Y(1) - Y(0)]/E[Y(0)])$. Each point is an effect estimate (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 discussion and Online Appendix H for details on the Poisson specification.

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



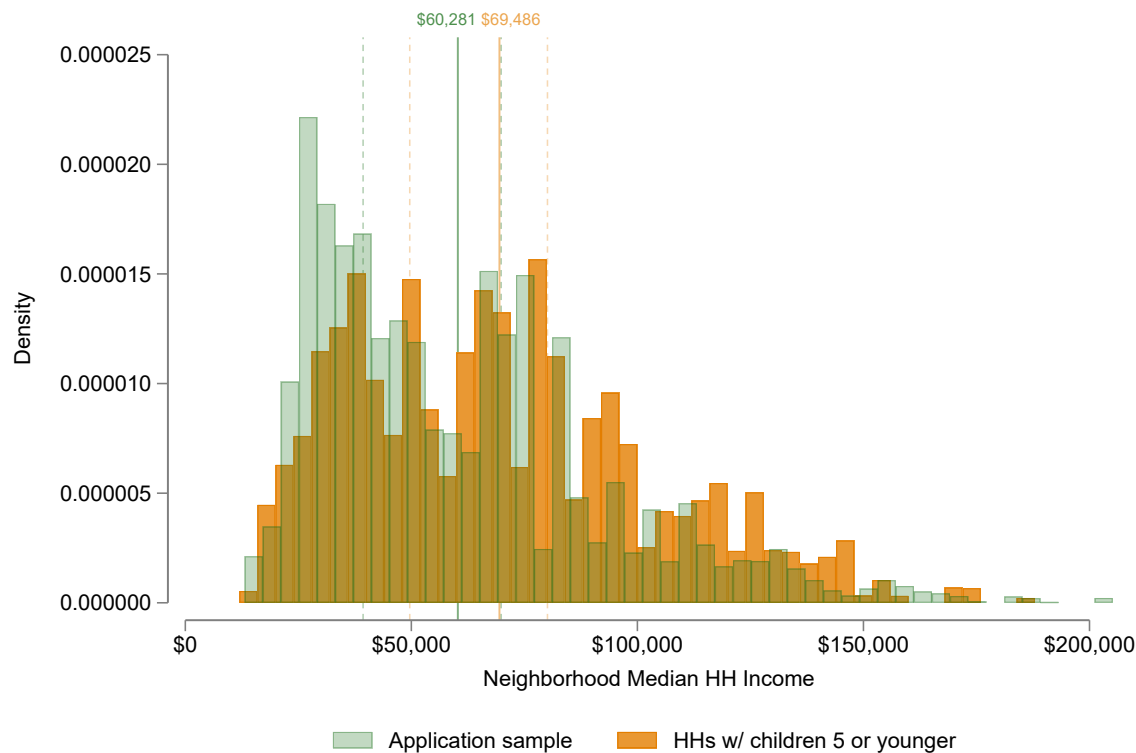
Notes: Panel (a): Net government costs of UPK provision are constructed by summing four different inputs. We add inputs sequentially, moving from left to right on the graph. (1) The direct public costs of providing the magnet pre-k slot (dark red bar), (2) the public savings from substitution away from other publicly funded pre-k and childcare programs, (3) changes in discounted tax revenue due to the estimated changes in kids' earnings, and (4) changes in discounted tax revenue due to changes in parents' earnings. Panel (b): we consider five possible inputs to willingness to pay (WTP) for UPK. (1) Net direct government cost (dark blue bar), (2) reduction in out-of-pocket costs, (3) child income gains estimated using changes in kindergarten test scores, (4) parental earnings gains after pre-k, (5) parental earnings gains during pre-k. Panel (c): we construct MVPF estimates using four different approaches to computing WTP (listed under each group of bars) and under five different inclusion/exclusion criteria for the overall MVPF calculation (bars within groups). The "Net direct govt cost" bars compute WTP based on cost of provision. The other three sets of bars use hedonic WTP estimates that include the inputs listed in the row labels. See Section 5.2 for details. Within each group, the "Main" bars include all available inputs to the MVPF calculation, and compute earnings gains for children based on our estimates of score effects. The "no parent earnings" bars remove parent earnings from both costs and WTP (as applicable). "Kids' effects of 0.4sd" assumes test score gains of 0.40 standard deviations and "GPW Kids' effects" assumes kids benefit through increased college enrollment as reported in [Gray-Lobe et al. \(2023\)](#), in both cases continuing to ignore parent earnings effects. "No kid effects" puts parents' earnings back in and removes increases in kids' earnings from both the net costs and WTP. See Section 5 for details.

Figure 7: MVPF estimates across programs and program categories



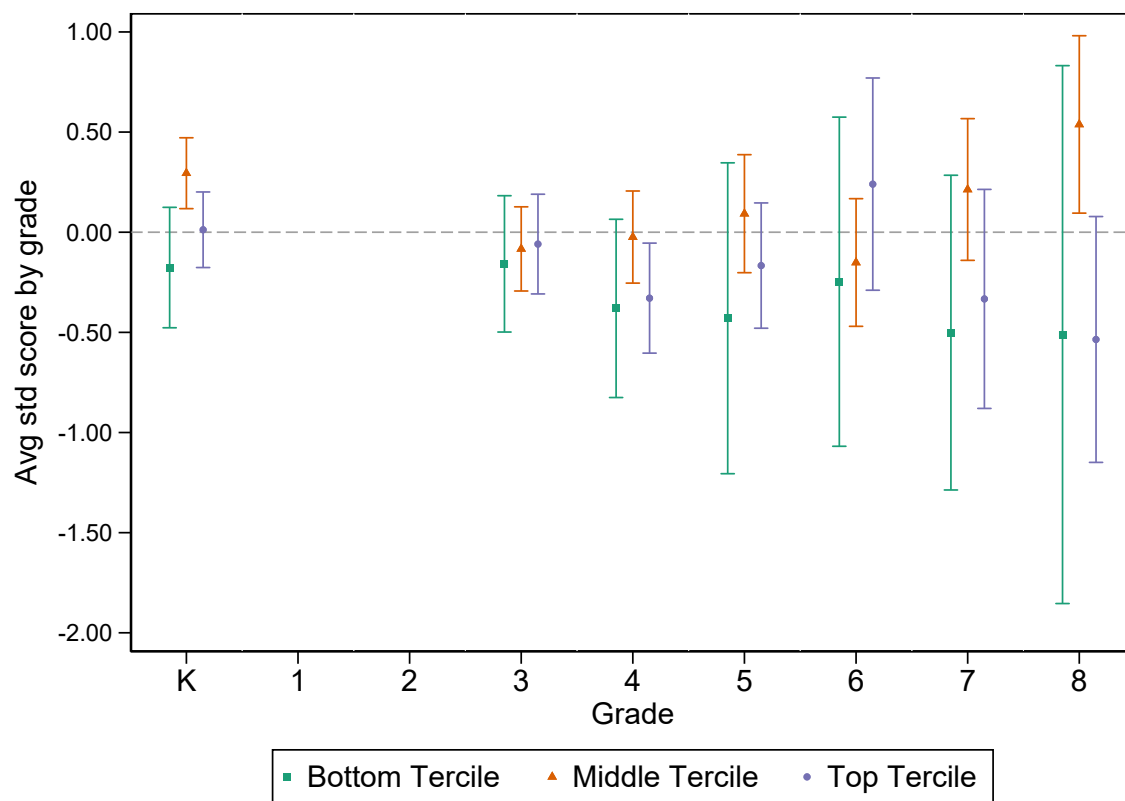
Notes: This figure combines our own calculations of MVPFs for New Haven’s UPK program with [Cascio \(2023\)](#)’s estimated MVPF for UPK and with estimates of other MVPF values for other program types from [Hendren and Sprung-Keyser \(2020\)](#). The horizontal axis is the age of program beneficiaries. The vertical axis is the MVPF value, pooling all values from $[5, \infty)$. The points labeled “NH UPK (no par, ...)” report MVPFs that exclude earnings gains for parents from the calculation, 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 as implied by the [Gray-Lobe et al. \(2023\)](#)’s estimates of four-year college attendance, projecting earnings gains using [Zimmerman \(2014\)](#). “NH UPK (no kid...)” report MVPFs that exclude earnings gains for children from the calculation. “incl par earn”, “govt. cost”, “no par earn”, and “incl par post earn” represent the four different assumptions we make about inputs into willingness to pay, which we believe likely bracket the true value. See Section 5.2. Lighter gray points are other policy evaluations reported in [Hendren and Sprung-Keyser \(2020\)](#). See Section 5 for details.

Figure 8: Neighborhood income distribution for applicants and households with children under age six



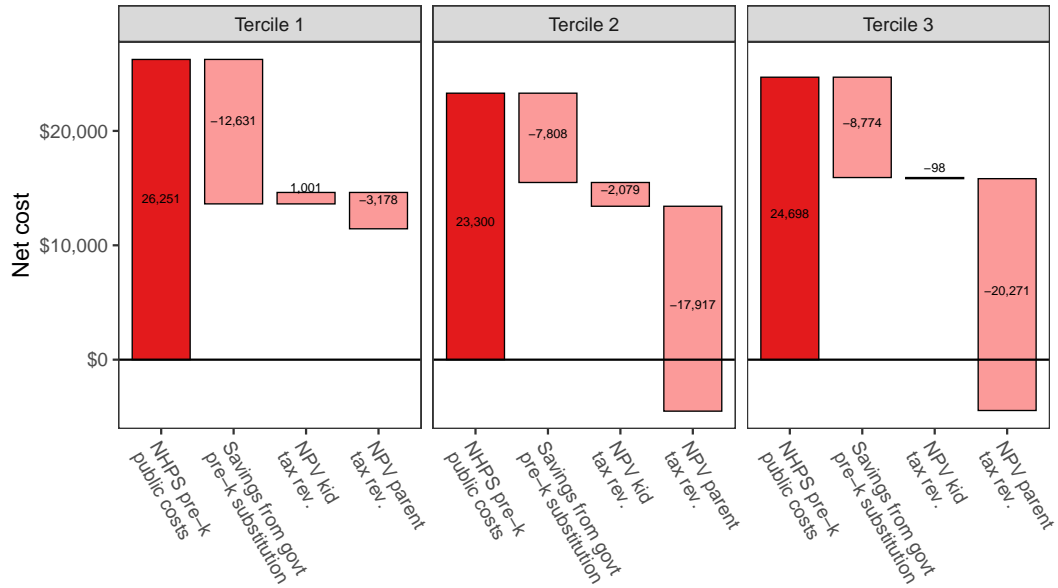
Notes: Distribution of neighborhood median household income in the sample of UPK applicants (light green bars) and all households with children under age six in New Haven County (orange bars). Source: block-group data from the 2019 ACS 5-year sample. Solid vertical lines represent each sample's mean, with the value shown on top of the lines. Dashed lines to the left and right represent each sample's 33.3rd and 66.7th percentiles, respectively. The 33.3rd and 66.7th percentiles in the applicant distribution are \$39,323 and \$69,868 respectively. For NH county, these values are \$49,645 and \$80,106. See Section 6 for details.

Figure 9: The effects of UPK on test scores by grade and family income tercile



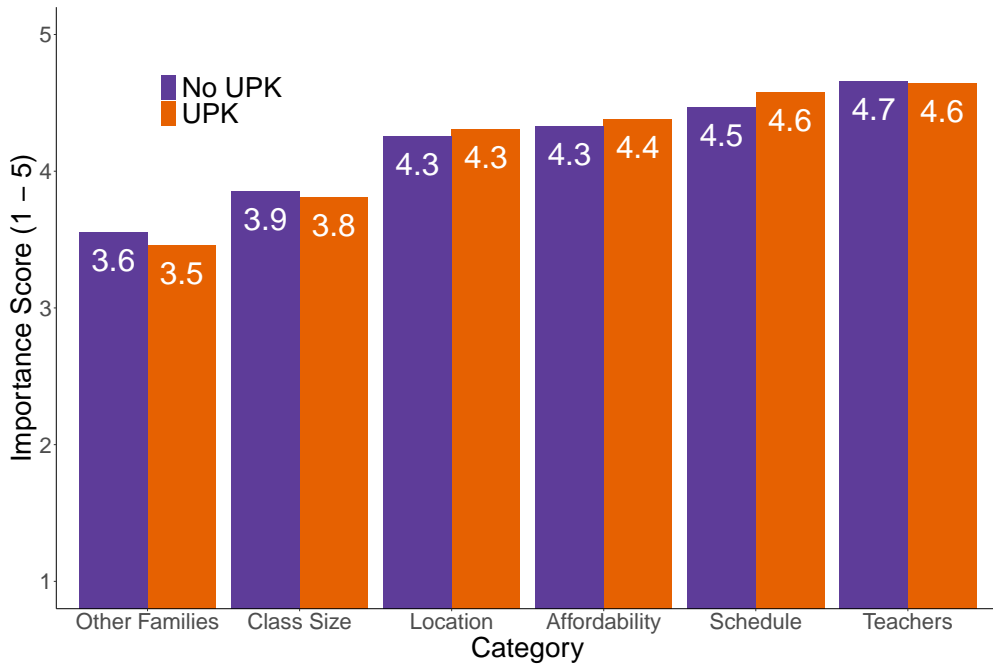
Notes: This figure shows IV estimates of the effect of UPK enrollment on standardized test scores by grade and tercile of ACS median block-group household income. Dots are point estimates. The surrounding error bars show 90% confidence intervals. See Section 6 for details.

Figure 10: Inputs into net costs by neighborhood income tercile

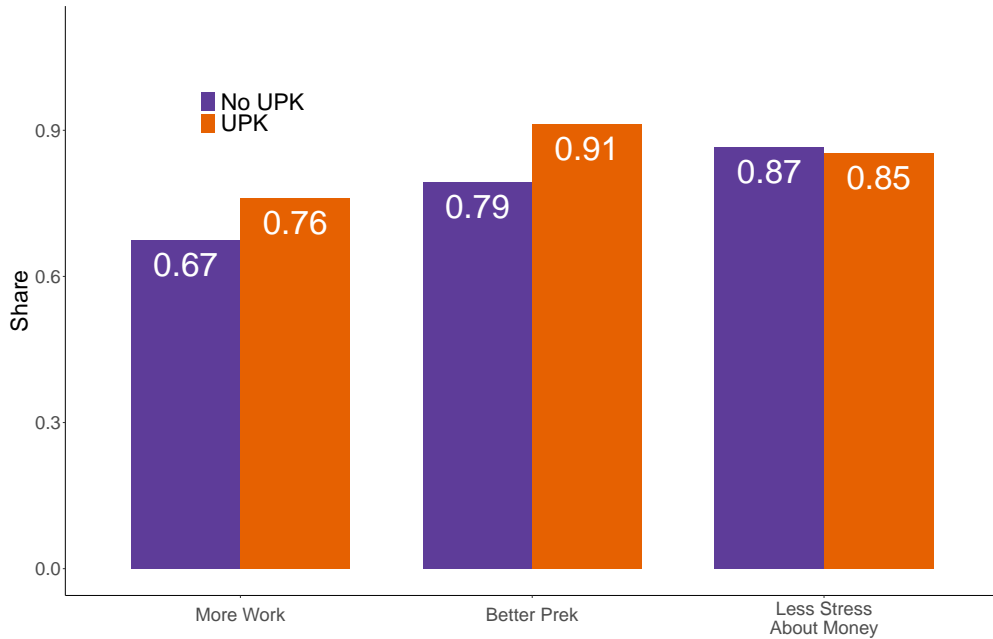


Notes: This figure reports the inputs into UPK program net cost by tercile of ACS median block-group household income. Inputs to net cost are added sequentially from left to right. The dark red bar on the left of each panel shows the direct public costs of providing the UPK slot. The second bar from the left shows the public savings from substitution away from other publicly funded pre-k and childcare programs. The third bar from the left shows the changes in discounted tax revenue due to the estimated changes in kids' wage income. The fourth bar shows the changes in discounted tax revenue due to changes in parents' wage income. See Section 6 for details.

Figure 11: Reported priorities and subjective treatment effects



(a) Survey reports of attribute importance



(b) Survey reports of subjective treatment effects of UPK

Notes: 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 (if enrolled) or would have (if not enrolled) 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
<i>Panel A: Lottery demographics</i>				
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
<i>Panel B: Lottery outcomes</i>				
UPK assignment	0.251	0.246	0.244	0.419
UPK enrollment	0.265	0.282	0.276	0.546
<i>Panel C: Test scores</i>				
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	
<i>Panel D: Parent demographics</i>				
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	
<i>Panel E: Survey data</i>				
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
<i>Panel A: Balance</i>							
Black	0.347 (0.034)	0.430	-0.027 (0.009)	-0.006 (0.013)	-0.005 (0.014)	-0.011 (0.017)	-0.034 (0.053)
White	0.246 (0.028)	0.199	0.034 (0.007)	0.009 (0.011)	0.008 (0.012)	0.014 (0.016)	0.060 (0.050)
Female	0.565 (0.034)	0.509	-0.033 (0.009)	-0.028 (0.014)	-0.033 (0.015)	-0.019 (0.019)	-0.073 (0.058)
Age at application	3.490 (0.034)	3.727	-0.010 (0.005)	0.002 (0.008)	0.008 (0.009)	-0.004 (0.011)	0.039 (0.032)
ACS median HH income	63,609 (2,176)	58,651	1,510 (575)	-500 (777)	-272 (828)	-1,351 (1,066)	-3,664 (3,983)
Pre-period income (dollars)	27,443 (2,286)	23,769				232 (949)	
Pre-period log income	10.022 (0.131)	9.775				0.027 (0.050)	
Any pre-period income	0.921 (0.030)	0.856				-0.006 (0.012)	
Earnings-weighted index			823 (197)	-41 (260)	25 (277)	-207 (357)	-526 (1,317)
Joint test			0.000	0.522	0.384	0.840	0.384
<i>Panel B: Match</i>							
Matched to state	0.895 (0.027)	0.862	0.026 (0.005)	0.011 (0.009)			
Matched to earnings	0.759 (0.033)	0.616	-0.005 (0.008)	-0.006 (0.013)			
Matched to survey	0.053 (0.013)	0.028	0.007 (0.004)	0.012 (0.006)			
<i>Panel C: First Stage</i>							
Enrolled NHPS UPK	0.000	0.000	0.581 (0.008)	0.389 (0.013)	0.402 (0.014)	0.416 (0.018)	0.409 (0.053)
Years NHPS UPK	-0.044 (0.030)	0.076	0.833 (0.013)	0.576 (0.022)	0.634 (0.023)	0.625 (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 evaluates the hypothesis that all coefficients shown in a given column (except for the 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 3-7 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 linked to parent earnings records. Column 7 restricts to the survey sample and uses the recentered instrument. Standard errors are clustered at the application level (columns 3-5 and 7), or two ways at the application and parent level (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
<i>Panel A: Substitution</i>					
Enrolled Head Start (admin)	0.203 (0.029)	0.202	-0.139 (0.019)	-0.194 (0.027)	
Enrolled School Readiness	0.066 (0.021)	0.086	-0.077 (0.013)	-0.064 (0.017)	
Care4Kids	0.186 (0.028)	0.197	-0.050 (0.026)	-0.017 (0.034)	
Any other SDE/OEC pre-k	0.456 (0.036)	0.350	-0.170 (0.032)	-0.157 (0.043)	
Any pre-k or childcare (admin)	0.624 (0.034)	0.540	0.392 (0.025)	0.372 (0.029)	
Enrolled Head Start (survey)	0.162 (0.151)	0.221			-0.192 (0.068)
Other paid option (survey)	0.720 (0.183)	0.548			-0.623 (0.082)
Another public option (survey)	0.144 (0.111)	0.097			-0.117 (0.051)
Any pre-k or childcare (survey)	1.096 (0.122)	0.894			0.022 (0.044)
<i>Panel B: Usage intensity</i>					
Weekly childcare hours	51.235 (13.022)	29.067			11.319 (3.200)
Monthly OOP costs	487 (216)	620			-375 (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. Specifications using survey outcomes use re-centered instruments. The first two columns report complier control means and control group means. The third through fifth columns report the IV estimates for our different datasets. “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. Panel A reports results from specifications where outcomes enrollment indicators for other pre-kindergarten programs. Outcome variables listed in the rows of Panel A are from administrative sources, except where “(survey)” is specified. 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
<i>Panel A: Earnings from admin data</i>						
Pre-K years	34,363 (2,484)	0.057 (0.026)	5,461 (1,717)	0.217 (0.066)	0.209 (0.061)	10619; 10619; 10619; 8528
Years after pre-K 1-2	36,567 (2,781)	0.037 (0.028)	6,482 (2,216)	0.227 (0.077)	0.215 (0.068)	10282; 10282; 10282; 8025
Years after pre-K 3-4	35,001 (3,293)	0.050 (0.031)	6,413 (2,444)	0.191 (0.085)	0.187 (0.066)	9989; 9989; 9989; 7434
Years after pre-K 5-6	36,208 (3,607)	0.062 (0.040)	6,849 (3,197)	0.217 (0.115)	0.142 (0.090)	8221; 8221; 8221; 5860
<i>Pooled post pre-k</i>						
Years after pre-K 1-6	35,957 (2,818)	0.048 (0.028)	6,469 (2,258)	0.209 (0.079)	0.187 (0.058)	10282; 10282; 10282; 8224
Years after pre-K 7+	33,716 (6,662)	0.054 (0.076)	3,539 (5,824)	0.092 (0.212)	0.193 (0.178)	6128; 6128; 6128; 4235
<i>Panel B: Earnings from admin data (Balanced)</i>						
Pre-K years	36,574 (2,569)	0.044 (0.027)	4,377 (1,838)	0.153 (0.068)	0.187 (0.066)	9139; 9139; 9139; 7244
Years after pre-K 1-2	38,802 (3,080)	0.035 (0.029)	5,231 (2,279)	0.164 (0.073)	0.162 (0.068)	9141; 9141; 9141; 7084
Years after pre-K 3-4	36,547 (3,238)	0.042 (0.032)	6,248 (2,560)	0.181 (0.086)	0.196 (0.069)	9141; 9141; 9141; 6784
<i>Pooled post pre-k</i>						
Years after pre-K 1-4	37,674 (3,073)	0.039 (0.028)	5,740 (2,300)	0.172 (0.075)	0.180 (0.061)	9141; 9141; 9141; 7229
	Weekly Hrs Cntrl Mn	Employed FT	Employed PT	Hours/ week		
<i>Panel C: Survey data</i>						
During pre-k	27.87 (5.89)	0.221 (0.138)	-0.133 (0.119)	12.80 (4.25)	726; 726; 721	
After pre-k	37.44 (5.46)	0.00 (0.11)	0.08 (0.09)	1.48 (3.91)	497; 497; 487	

Notes: This table reports IV estimates 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 and estimates a Poisson regression using a control function approach (see Online Appendix H). For the Poisson regression, we report $\exp(\hat{\beta}) - 1$, which is an estimate of the proportional change $(E[Y(1) - Y(0)]/E[Y(0)])$. “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. In Panel C, column 1 reports the control complier mean hours worked. Columns 2-4 report IV estimates for different labor supply outcomes from the survey. “Employed FT” is an indicator for if the respondent reported working full time. “Employed PT” is an indicator for if the respondent reported working part time. “Hours/week” is the hours per week the respondent reported working. Sample sizes for each specification are included in the last column. In Panels A and B, standard errors are two-way clustered at the applicant and parent level, except for the Poisson specification, where standard errors are estimated via bootstrap clustered at the at the application level with 500 bootstrap draws. In Panel C standard errors are clustered at the respondent level. See Section 4.5 for details.

Table 6: Career disruption

	Switch main industry	One job over \$ 4,000	Quarters earn. \leq \$ 4,000 (Incl. 0s)	Total qts earn. \leq \$ 4,000 since PK	N individuals
<i>Disaggregated</i>					
Pre-K years	-0.072 (0.027)	0.300 (0.108)	-0.206 (0.097)	-0.316 (0.143)	9205; 10621; 10727; 10727
Yrs after PK 1-2	-0.022 (0.024)	0.220 (0.117)	-0.152 (0.096)	-0.659 (0.267)	9010; 10285; 10391; 10391
Yrs after PK 3-4	-0.040 (0.024)	0.132 (0.117)	-0.094 (0.094)	-0.761 (0.392)	8385; 9990; 10096; 10096
Yrs after PK 5-6	-0.017 (0.030)	0.043 (0.143)	0.020 (0.112)	-0.318 (0.562)	6633; 8221; 8327; 8327
<i>Pooled post pre-k</i>					
Yrs after PK 1-6	-0.026 (0.017)	0.139 (0.104)	-0.087 (0.078)	-0.648 (0.360)	9476; 10285; 10391; 10391
Yrs after PK 7+	-0.030 (0.043)	0.015 (0.267)	0.064 (0.167)	-0.984 (1.117)	4964; 6128; 6234; 6234

Notes: This table reports IV estimates of Equation 1 where outcomes are measures of career disruption. 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. “Switch main industry” is a binary variable equal to one if the applicant’s top-earning industry in the current academic year differs from the prior year, conditional on being employed in the prior year, with unemployment in the current year also counted as a switch. “One job over \$4,000” counts the number of quarters in an academic year that an individual has exactly one job and that job pays at least \$4,000. “Quarters earn \leq 4,000” counts quarters with earnings of less than \$4,000 in an academic year. “Total qts earn \leq 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 two-way clustered at the application and parent levels. See Section 4.5.2 for details.

Table 7: MVPF estimates using varying willingness to pay constructions

Specification	WTP is net direct govt cost	WTP is OOP costs, kid earnings	WTP is OOP costs, kid earnings, parent post earnings	WTP is OOP costs, kid earnings, all parent earnings
<i>Varying Assumptions on Kids Effects</i>				
Main (estimated kids' effects)	10.04	4.53	32.67	39.56
	[1.81, Inf]	[0.60, Inf]	[3.29, Inf]	[3.96, Inf]
Kids' effects of 0.4sd	Inf	Inf	Inf	Inf
	[2.60, Inf]	[2.74, Inf]	[5.61, Inf]	[7.19, Inf]
GPW kids' effects	Inf	Inf	Inf	Inf
	[2.37, Inf]	[2.30, Inf]	[4.90, Inf]	[6.30, Inf]
<i>Excluding Parents or Kids</i>				
Main, no kids' earnings gains	7.83	2.65	24.61	29.98
	[1.71, Inf]	[0.51, Inf]	[2.54, Inf]	[3.37, Inf]
Main, no parent earnings gains	1.03	0.46	0.46	0.46
	[0.98, 1.09]	[0.20, 0.77]	[0.20, 0.77]	[0.20, 0.77]
Kid effects of 0.4sd, no parent earnings gains	1.23	1.32	1.32	1.32
	[1.19, 1.27]	[1.07, 1.58]	[1.07, 1.58]	[1.07, 1.58]
GPW kids' effects, no parent earnings gains	1.18	1.15	1.15	1.15
	[1.15, 1.21]	[0.94, 1.39]	[0.94, 1.39]	[0.94, 1.39]

Notes: This table reports estimates of the MVPF of UPK under different assumptions about earnings effects for children (upper panel) and inclusion/exclusion criteria for parent and child earnings gains (lower panel), using four different constructions of willingness to pay (one in each column). The first column assumes parents value the program at its net direct government cost. The next three columns report results from different hedonic approaches to WTP calculation. The second excludes parental earnings from WTP and considers only change in out of pocket expenditures and kids future earnings. The third adds post-pre-k parental earnings, and the fourth considers all parental earnings. The first three rows show results under different assumptions about the effect of UPK on students. “Main” assumes the kindergarten score gains found in our analysis. “Kids’ effects of 0.4sd” assumes test score gains of 0.40 standard deviations. “GPW Kids’ effects” assumes kids benefit through increased college enrollment as reported in [Gray-Lobe et al. \(2023\)](#). The bottom four rows report results when excluding parents’ earnings gains (“no parent earnings gains”) or kids’ earnings gains (“No kids’ earnings gains”) from the calculation. See Section 5 and Online Appendix I for details.

Table 8: UPK effects by family income tercile

	ACS 1st tercile		ACS 2nd tercile		ACS 3rd tercile	
	CCM	IV	CCM	IV	CCM	IV
<i>Panel A: Substitution</i>						
Weekly childcare hours (survey)	53.9 (40.3)	7.0 (10.2)	43.3 (32.8)	10.9 (5.9)	41.8 (12.2)	11.8 (5.1)
Any pre-k or childcare (survey)	1.334 (0.705)	-0.036 (0.187)	0.944 (0.295)	0.063 (0.103)	1.081 (0.109)	-0.025 (0.034)
Enrolled Head Start (admin)	0.308 (0.088)	-0.332 (0.061)	0.215 (0.086)	-0.190 (0.037)	0.065 (0.061)	-0.060 (0.038)
Enrolled Head Start (survey)	0.417 (0.547)	-0.389 (0.163)	0.187 (0.375)	-0.190 (0.160)	0.219 (0.170)	-0.156 (0.078)
Another public option (survey)	0.044 (0.233)	-0.052 (0.061)	0.111 (0.218)	-0.110 (0.100)	0.169 (0.168)	-0.151 (0.084)
Other paid option (survey)	0.816 (0.754)	-0.565 (0.192)	0.597 (0.416)	-0.589 (0.168)	0.604 (0.247)	-0.660 (0.113)
Monthly OOP costs (survey)	57 (727)	-68 (132)	596 (468)	-400 (188)	637 (312)	-552 (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
<i>Panel B: Test scores</i>						
Avg std score K	0.126 (0.138)	-0.176 (0.183)	0.168 (0.156)	0.295 (0.108)	0.217 (0.095)	0.012 (0.115)
N individuals	2996	2996	2909	2909	2969	2969
N applications	3548	3548	3397	3397	3479	3479
<i>Panel C: Parent Earnings</i>						
Pre-K years	18,173 (2,202)	0.106 (0.135)	34,324 (3,566)	0.247 (0.120)	43,226 (4,843)	0.268 (0.108)
Years after pre-K 1-2	20,064 (2,348)	0.034 (0.155)	36,826 (4,326)	0.277 (0.159)	42,390 (5,588)	0.293 (0.138)
Years after pre-K 3-4	19,344 (2,462)	-0.063 (0.140)	36,776 (4,529)	0.351 (0.150)	41,100 (6,178)	0.176 (0.138)
Years after pre-K 5-6	15,907 (5,086)	-0.015 (0.282)	36,629 (5,077)	0.336 (0.232)	43,238 (7,532)	0.192 (0.157)
N applicants	3336	3336	3296	3296	3781	3781

Notes: This table reports IV estimates of substitution patterns, test score impacts, and parental wage income impacts by tercile of ACS median block-group household income. Terciles are computed based on the distribution of neighborhood median household income among 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 specification and reports $\exp(\hat{\beta}) - 1$ which is an estimate of the proportional change ($E[Y(1) - Y(0)]/E[Y(0)]$). See Online Appendix H for details. 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 "Enrolled Head Start (admin)" row uses data from application years 2015-2017, when the administrative data on Head Start is most complete. Implausible values of the complier control mean for "Enrolled Head Start (admin)", "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. See Online 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 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 application level in Panel C. See Section 6 for details.

Table 9: UPK labor market effects by demographic group

	Black (1)	Hispanic (2)	White (3)	Post 2013 (4)	Moms (5)	Dads (6)	1-Parent (7)	2-Parents (8)
Pre-K years	0.141 (0.100) [26,029] <3,757> {4770}	0.174 (0.144) [27,999] <3,220> {2236}	0.385 (0.162) [43,027] <7,517> {2434}	0.221 (0.068) [31,335] <2,467> {6147}	0.199 (0.081) [28,252] <2,717> {4996}	0.281 (0.116) [39,750] <5,257> {2619}	0.181 (0.089) [27,179] <1,966> {3944}	0.244 (0.112) [36,778] <6,817> {1885}
Years after pre-K 1-2	0.043 (0.110) [28,278] <4,373> {4629}	0.129 (0.171) [31,738] <4,376> {2147}	0.513 (0.205) [44,016] <6,381> {2354}	0.233 (0.085) [33,864] <2,828> {5808}	0.187 (0.103) [30,584] <2,924> {4718}	0.292 (0.125) [44,152] <5,020> {2449}	0.152 (0.103) [29,193] <2,726> {3769}	0.245 (0.133) [41,670] <6,537> {1738}
Years after pre-K 3-4	0.043 (0.117) [25,490] <4,520> {4524}	0.237 (0.207) [28,091] <4,328> {2075}	0.424 (0.214) [38,786] <7,891> {2308}	0.183 (0.087) [31,940] <3,160> {5515}	0.218 (0.118) [28,455] <3,061> {4489}	0.123 (0.133) [41,588] <4,936> {2337}	0.224 (0.123) [27,296] <2,938> {3593}	0.114 (0.147) [39,395] <5,961> {1639}
Years after pre-K 5-6	-0.014 (0.165) [25,458] <5,982> {3779}	0.208 (0.386) [29,882] <8,454> {1629}	0.396 (0.238) [36,410] <8,985> {1992}	0.231 (0.123) [27,328] <4,197> {3747}	0.183 (0.144) [27,378] <4,106> {3016}	0.353 (0.233) [31,891] <7,420> {1532}	0.413 (0.203) [22,135] <5,134> {2470}	0.086 (0.192) [37,348] <7,935> {1048}

Notes: This table replicates the Poisson specifications from Panel A of Table 5 for different subsets of applicants. Columns 1-3 restrict 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. Column 4 restricts the sample to all applications in 2014 or later. Columns 5-6 restrict the sample to only mothers or only fathers listed on UPK applications. Columns 7-8 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 regression run on annual income (including 0s) as described in Online Appendix H. The table reports $\exp(\hat{\beta}) - 1$, which is an estimate of the proportional change $(E[Y(1) - Y(0)]/E[Y(0)])$. 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 errors, shown between the <> signs, are 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.