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The Impacts of Expanding Access to High-Quality Preschool Education

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The Impacts of Expanding Access to High-Quality Preschool Education[*]

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Abstract

President Obama's "Preschool for All" initiative calls for dramatic increases in the number of 4 year olds enrolled in public preschool programs and in the quality of these programs nationwide. The proposed program shares many characteristics with the universal preschools that have been offered in Georgia and Oklahoma since the 1990s. This study draws together data from multiple sources to estimate the impacts of these "model" state programs on preschool enrollment and a broad set of family and child outcomes. For lower-income children, the state programs increase the likelihood of preschool enrollment, the amount of time spent with mothers on activities such as reading, and test performance as late as eighth grade. For higher-income families, however, the programs shift children from private to public preschools, resulting in less of an impact on overall enrollment but a significant reduction in childcare expenses, and appear to have no effect on children's test scores. There is also little evidence that the state programs increase maternal labor supply, with the exception of a possible temporary increase in employment for mothers in lower-income families.

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In his 2013 State of the Union address, President Obama proposed sweeping reform to preschool education in the United States. His "Preschool for All" initiative calls for dramatic increases in the number of 4 year olds in public preschool programs and in the quality of these programs nationwide. The proposed program would be funded by a $75 billion federal investment over 10 years, to be roughly matched by states, with federal dollars allocated to states based on the share of their 4 year olds from low- and moderate-income families.[1] Local school districts and other providers would be responsible for implementation, but in order to receive federal funding, states would have to adopt certain quality benchmarks, including early learning standards, teacher qualifications, and a plan for assessment.[2] The new preschools would be free for children from low- and moderate-income families and accessible to children from higher-income families at some (unspecified) cost.

A large body of prior research has shown that there is a high rate of return to early childhood education among disadvantaged children. Prior studies of the long-run impacts of preschool programs that have targeted children from low-income families—for example the Perry Preschool Project and the federally funded Head Start program—find that preschool has not only private returns in terms of increased lifetime earnings, but also public returns through reductions in crime and use of public assistance. Because of these externalities to preschool enrollment, economists would tend to agree that there is a role for public policy to expand preschool access.

The Obama plan builds on existing public preschool programs operated by state governments, which have greatly expanded over the past 30 years. Today, several states,

[1] Low- to moderate-income is defined as income at or below 200 percent of the federal poverty line.
[2] The Obama plan also includes expansions of Head Start for low-income 3 year olds and Early Head Start for younger children, incentives for states to adopt full-day kindergarten classes, and extension of voluntary home visiting programs by nurses and social workers.

including Tennessee, North Carolina, New Jersey, Washington, and Kentucky, have public programs that meet many of the quality benchmarks in the Obama plan but serve a small share of preschool-aged students. Other states, including Wisconsin, Texas, and Florida, have programs that score high on access but low on quality. Very few states—most famously, Georgia and Oklahoma—have state-funded preschool programs that score high on both accounts.

The "Preschool for All" proposal can be thought of as pushing all states toward the Georgia and Oklahoma model—an accessible preschool program with high standards. Existing evidence on the impacts of the Georgia and Oklahoma programs is focused on short- to medium-term outcomes, and the findings are mixed. Gormley and Gayer (2005) find evidence that disadvantaged preschool attendees in Tulsa, Oklahoma score higher on tests at the end of the preschool year.[3] Fitzpatrick (2008) finds that the Georgia program increases test scores of disadvantaged children as late as fourth grade, but the effects appear smaller and less widespread than those from the Tulsa study. Fitzpatrick (2010) also finds no evidence that either of these state programs increased the labor supply of mothers of 4 year olds, despite providing a 100 percent price subsidy for childcare on the extensive margin of employment.

To our knowledge, nothing is known to date about the impacts of these programs on child outcomes measured later than fourth grade, or on measures of family well-being or behavior beyond maternal employment. This study addresses this gap in the literature, bringing together data from multiple sources to estimate the impacts of the Georgia and Oklahoma programs on a broad range of family and child outcomes. Our baseline empirical approach compares outcomes in these two "model" states to the rest of the United States, before and after the introduction of their universal preschool programs, in a differences-in-differences (DD) framework. For

[3] Using a similar research design as Gormley and Gayer (2005), described in detail below, Wong, et al. (2008) also find evidence that the Oklahoma preschool program has improved children's end-of-preschool test performance.

outcomes where only more recent data are available, we take an alternative DD approach, comparing families with 5 year olds and families with 4 year olds, in Georgia and Oklahoma versus the rest of the United States. For robustness checks, where possible, we also combine the two strategies into a triple-difference framework, exploiting both the timing and age-targeting of the programs.

We first show the stark difference in the effects of these programs on preschool enrollment patterns by family background. Among students whose mothers have no more than a high school degree, who are much less likely to be enrolled in preschool in the absence of a public option, we find that the Georgia and Oklahoma programs have sharply increased the likelihood of preschool enrollment at age 4; our DD estimates imply that the preschool enrollment rates of children with less-educated mothers in Georgia and Oklahoma are now 18 to 20 percentage points higher than they would have otherwise been. On the other hand, the same empirical approach suggests that 4 or 5 out of every 10 program enrollees whose mothers have at least some college education would otherwise have been in private preschool. As a result, the impact of the programs on their overall preschool attendance has been more muted, at a 12 to 15 percentage-point gain.

We then turn to the impacts of these state "model" programs on the behavior and well-being of families with 4-year-old children, continuing to split the data by maternal education. We hypothesize that the programs could affect how much time or money parents invest in children, which could contribute to their academic achievement independently of preschool attendance itself. Our findings here align with those described above. Higher-education families, for which private preschool participation falls significantly, see a significant reduction in childcare spending. A back-of-the-envelope calculation based on our estimates suggests that this spending

reduction amounts to an income transfer of $3,300 to $5,600 for families that switch out of private programs. By contrast, lower-education families, which gain more from the program in overall preschool participation, show larger declines in overall maternal time spent with children. However, this finding is counterbalanced by an impact of the program on "quality" time between mothers and children in lower-education families, spent reading, playing, doing art projects, and talking.

In addition to its contribution to children's human capital formation, preschool serves as childcare. Indeed, another rationale for preschool expansions is the role that they would play as a childcare subsidy. An economic labor supply model frames a mother's decision to work as a function of her net wage, that is, the difference between her wage and the hourly price of childcare. Reducing the cost of preschool effectively increases a mother's net wage, making it more likely that she will participate in the labor force, and in turn, contribute to family income.[4] While a theoretical possibility, increases in maternal labor supply are hard to detect using our data and research design, consistent with the findings of Fitzpatrick (2010). We find some evidence of an increase in the probability of working among lower-education mothers, but it appears to be confined to the first few years after the program is in place.

Finally, we turn to these programs' reduced-form effects on children's human capital. While the programs are still too young for us to estimate their truly long-term impacts, we are able to explore child outcomes as late as eighth grade with the benefit of more recent data than previously available.[5] For lower-income children, the evidence points to some impact of the programs on eighth-grade math scores. However, despite the fact that some children from higher-income families were more likely to have attended preschool, while others were in

[4] Among mothers already participating in the labor force, the childcare subsidy will change the relative price of leisure and so the net impact on hours worked will depend on whether the income or substitution effect dominates.
[5] The Georgia and Oklahoma preschool programs went universal in fall 1995 and fall 1998, respectively.

families effectively receiving sizable income transfers at age 4, their academic achievement does not appear to have improved on average. While we cannot be completely certain of why this is, one possibility is that programs crowded out enrollment in private preschools that were even higher quality, leading to negative effects on the human capital of "switchers." Because they are lower-scoring on average, disadvantaged children might also have negative effects on higher-income children in universal preschool classrooms.

On one hand, these findings would appear to suggest that an untargeted national preschool program would result in substantial crowd-out, driving up costs and limiting program efficacy. On the other hand, the presence of higher-income children in the universal preschool classrooms in Georgia and Oklahoma—which may help to attract better teachers or have direct human capital spillovers for lower-income children—may be what truly makes these programs "high quality." Specifying the degree of cost-sharing for middle-class families in the Obama plan therefore presents policymakers with an important potential tradeoff.

I. Background

The "Preschool for All" initiative proposes a bold change in the role of the federal government in early childhood education. However, it builds on existing state efforts in preschool education and attempts to garner support from the large and well-identified literature on the long-term impacts of targeted preschool programs. In this section, we describe these state programs, elaborate on the small but growing literature on their impacts, and discuss key findings from the broader literature on preschool education.

I.A. State-funded preschool programs

Policy efforts at the state level have notably increased public preschool enrollment over the last 30 years. In 1980, only four states had subsidized the provision of preschool programs,

and these programs were quite small.[6] But in the five-year period from 1983 and 1987 alone, 11 states started their first preschool programs. Another eight states started programs over the next five-year period, and by 2011, public preschool programs existed in 40 states. Figure 1 (right axis) plots the cumulative fraction of states that had funded preschool programs by year. Increases in the public preschool participation rates of 4 year olds as measured in the October Current Population Survey (CPS) School Enrollment Supplements (left axis) track this state subsidization activity quite well, increasing by almost 25 percentage points between 1980 and 2011. Much of the public preschool enrollment of 4 year olds prior to 1980 in the October CPS may be accounted for by Head Start, the existing federal preschool program that targets children from low-income families. The Head Start enrollment rate of 4 year olds (left axis) has risen little since the early 1990s.

There is significant variation across state preschool programs in who is eligible to attend, both in terms of age and in terms of targeting. Most programs admit only 4 year olds, though approximately 13 percent of enrollment consists of 3 year olds, according to surveys conducted by the National Institute for Early Education Research (NIEER) over the past decade. Most programs also target children from low-income families, though the income threshold for eligibility varies across states, and some state programs target children who have developmental delays or other risk factors regardless of income. Access is universal only in a handful of states, the longest-standing and most studied of which have been Georgia (universal access began in

[6] According to data compiled by the NIEER, these states were California (1965), New York (1966), Maryland (1980), and Oklahoma (1980). The District of Columbia's first program was also in the 1960s. NIEER uses several criteria to identify state preschool programs. For example, the program has to be "funded, controlled, and directed by the state," serve preschool-aged children, and focus on early childhood education in a "group learning" environment and "be distinct from the state's system for subsidized child care" (Barnett et al., 2012; p. 21). As a result, the dates cited here may disagree with those reported elsewhere, such as by the Education Commission of the States.

1995) and Oklahoma (universal access began in 1998)—the states that are also the focus of this study.

The state programs differ not only in terms of access, but also in terms of commonly-used metrics of quality. For the past decade, the NIEER has compiled state standards for preschool programs—related to curriculum, teacher education, class size, and support services—into an index with a maximum value of 10, giving each of 10 quality metrics equal weight; many of these metrics are incentivized by the "Preschool for All" initiative.[7] Figure 2 presents a scatterplot of this index against NIEER's internal estimate of the share of 4 year olds in state-sponsored preschool programs as of the 2011-2012 school year, the most recent with data available; dot sizes represent the state's estimated population of 4 year olds. There appears to be a slight tradeoff between access and quality according to this index, though the slope of the fitted line is not statistically significantly different from zero at conventional levels.[8] There is substantial variation in the index at all levels of access. For example, the programs in Georgia and Oklahoma, as well as West Virginia, are not only high-access, but also high-quality according to the index. However, other states with significant access, such as Florida, Texas, Vermont, and Wisconsin, score low on the index, and there is significant variation in this measure of quality across states where access is quite restricted.

[7] The 10 quality metrics are as follows: program has comprehensive early learning standards; teachers are required to have a BA; teachers are required to have specialized training in preschool education; assistant teachers required to have a Child Development Associates (CDA) Degree (or equivalent); at least 15 hours per year of in-service required; the maximum class size is 20 students; staff to child ratios are 1-to-10 or better; program offers vision, hearing, health, and one support service; program offers at least one meal; program offers site visits.

[8] The slope of the fitted line (heteroskedasticity-robust standard error), weighting by 4-year-old population, is -0.033 (0.025). Unweighted, the slope estimate is -0.028 (0.017).

Because our empirical analysis will focus on estimating the impacts of the Georgia and Oklahoma programs, it is useful to describe these programs in more detail.[9] In 2011-2012, the NIEER estimates that enrollment rates of 4 year olds in state-funded preschool in Georgia and Oklahoma were 59 percent and 74 percent, respectively, with both programs serving 4 year olds exclusively.[10] Both states also score high on the NIEER index, most recently receiving scores of 8 and 9, respectively (Barnett et al., 2012). For example, both states require comprehensive learning standards, and that a classroom's lead teacher holds a bachelor's degree, is certified in early childhood education, and participates in annual in-service training.[11] Until recently, both states also mandated that the student-to-teacher ratio in preschool classrooms be no higher than 10-to-1, and that class sizes be no higher than 20.[12]

While these programs are among the highest in access and quality in the nation according to the NIEER, they differ from one another in several respects. Oklahoma's program is based in local school systems, and funding runs from state governments to local school districts that can choose to run half-day programs, full-day programs, or both. Spending per child is almost $7,427, with $3,652 coming from state sources and the remainder from a combination of local and federal contributions. On the other hand, Georgia's state preschool program often runs through private childcare centers, and total spending per child is only $3,490 and is entirely from state sources. Still both state models are possible under the plan, we focus on estimating the average effect across the two programs throughout, giving each program equal weight.

[9] We choose not to include West Virginia, since its program has only recently become high-quality on the NIEER scale. For example, early learning standards were adopted in West Virginia in 2004-2005, but have been in place in Georgia and Oklahoma since 1996-1997 (Barnett et al., 2012).

[10] We do not use the NIEER estimates of enrollment rates in the analysis to follow, because the earliest available data are from the 2001-2002 school year.

[11] The Oklahoma program loses one point on the index because it does not require assistant teachers to have a CDA (or equivalent).

[12] Georgia has recently lost two points on the index on this account, currently mandating a maximum class size of 22, and a maximum teacher-student ratio of 1-to-11. A recent state-commissioned evaluation of the Georgia program characterizes its classroom practices as medium quality (Peisner-Feinberg, Schaaf, and LaFollett, 2013).

I.B. Previous research on the Georgia and Oklahoma programs

Given the policy relevance of the universal preschool programs in Georgia and Oklahoma, it is not surprising that they have already been topics of study. The primary area of interest has been children's academic achievement. In an evaluation of the Oklahoma program using data from the city of Tulsa, Gormley and Gayer (2005) leverage the sharp cutoff in eligibility to participate in the program based on a child's date of birth and find positive impacts of participation on cognitive measures at the end of the preschool year (see also Gormley, Phillips, and Gayer, 2008).[13] Impacts are largest for blacks and Hispanics and low-income children, with little improvement for whites. Wong et al. (2008) use the same research design but different data to estimate the effects of the Oklahoma program, also finding improvements in Peabody Picture Vocabulary Test scores at the end of the preschool year.

Less is known about outcomes in later grades. Fitzpatrick (2008) evaluates the middle-term impacts of Georgia's universal preschool program using fourth grade outcomes measured in the National Assessment for Educational Progress (NAEP). Using a cross-state DD approach, she finds positive effects on fourth-grade NAEP test scores and the probability of being on-grade. The impacts are most consistently positive among disadvantaged, non-urban students, and more mixed among other demographic groups. They also appear smaller than the effects found in the Tulsa study, but this is not surprising given the pattern of "fadeout" in test score effects that pervades the early education evaluation literature. As described below, fadeout does not preclude positive impacts on a child's longer-term economic and social well-being.

These impacts on children's human capital are reduced-form, reflecting not only program participation itself but also potential changes in parental investments in children in response to

[13] Using the same approach, they also find positive impacts for the Head Start center in Tulsa. These are somewhat lower than the preschool program, but notably higher than the impacts measured in the recent randomized evaluation of Head Start.

the program. The literature on this mechanism is by comparison quite thin. Using a regression-discontinuity (RD) design similar to that used in the Tulsa studies, Fitzpatrick (2010) finds no evidence that a child's eligibility for state-funded preschool increases his or her mother's chances of working. Thus, most women appear to be infra-marginal with respect to the price subsidy for childcare implicit in these programs, i.e., many may switch from private preschools and other childcare arrangements to the state program without changing their employment status. As a result, the programs also do not appear to change family income, either by reducing public assistance receipt or increasing maternal earnings, when a child is 4 years old.[14]

We believe we are the first to leverage both the Georgia and Oklahoma expansions in the same DD framework to estimate their impacts on child and family outcomes.[15] There are some advantages to a DD approach over an RD one for the question at hand. One is that the "counterfactual" to the universal program is captured by the experiences of 4 year olds in other states after the initiatives are passed. In an RD approach, by contrast, the counterfactual is approximated by the experiences of children in the same state at a given point in time who will enter preschool the following year. To the extent that the alternatives would be changing over time—e.g., enrollment in private preschools might be increasing in control states—a DD approach using other states as controls might better represent would have occurred if the

[14] A parallel literature exists on the impacts of universal kindergarten programs for 5 year olds. Exploiting variation across states in the timing of first state funding for kindergarten in the 1960s and 1970s, Cascio (2009a) finds little evidence to suggest that kindergarten improved a child's long-term social and economic well-being, while Cascio (2009b) finds an impact on maternal labor supply, but only for single mothers with no children under the age of 5. On the other hand, attempting to leverage age eligibility by using quarter of birth as an instrument for kindergarten attendance in 1980, Gelbach (2002) finds more widespread increases in maternal employment in response to kindergarten. While interesting, these studies are arguably less relevant to the question at hand, given that the kindergarten programs under study were probably lower-quality than the current Georgia and Oklahoma preschool programs, that the counterfactual to kindergarten enrollment in the 1960s and 1970s and even 1980 was different than the counterfactual to preschool enrollment today, and that 5 year olds are at a different point in development than 4 year olds.

[15] Thus, we focus on the demand side. In a recent study, Bassok, Fitzpatrick, and Loeb (2012) use a DD approach to estimate the effects of these particular programs on childcare providers.

program had not been introduced.[16] In addition, using an RD approach, one can only evaluate short-term child outcomes, like end-of-preschool test scores, or contemporaneous outcomes, like maternal employment when a child is 4 years old.

A key contribution of the present study is thus to estimate these programs' impacts on child outcomes beyond the fourth grade test scores considered by Fitzpatrick (2008). Another contribution is to estimate program effects on contemporaneous, family-level outcomes beyond maternal labor supply. Much of what is known about the impacts of preschool programs on outcomes such as these is based on evidence from small-scale experiments in preschool education and the larger-scale federally funded Head Start program.

I.C. Previous research on the impacts of targeted preschool programs

Arguably the most famous experimental preschool program was the Perry Preschool program, a 2-year intervention in the early 1960s involving half-day school attendance and weekly home visits for extremely disadvantaged 3- and 4-year-old African American children living in Ypsilanti, Michigan.[17] Students were randomly assigned to receive treatment or not, and follow-up data on the participants have been collected through age 40, providing excellent evidence on the evolution of the impacts of a high-quality preschool intervention for disadvantaged children. Initial findings from Perry showed increases in IQ scores for the treated group, however, these initial gains did not persist and by age 10 there was no measured difference in IQ scores between the treatment and comparison group (Gramlich, 1986; Schweinhart et al., 2005). Nonetheless, the Perry treatment students performed statistically

[16] Even state programs lower down the ladders of access or quality have been found to have positive effects on children's outcomes, e.g. the low-quality, high-access program in Florida (Figlio and Roth, 2009) and the high-quality, low-access program in North Carolina (Ladd, Muschkin, and Dodge, 2012). The RD study by Wong et al. (2008) also uncovers impacts of the preschool programs in Michigan, New Jersey, South Carolina, and West Virginia on print awareness at the end of the preschool year.

[17] More specifically, the Perry program involved school attendance for 3 hours per day, 5 days a week from October through May, and weekly home visits for 90 minutes by a teacher to discuss a child's progress and instruct parents on how to provide an academically enriching environment at home (reading to children, counting with them, etc.).

significantly better in school: they were absent fewer days and less likely to be assigned to special education, had fewer failing grades and higher high school grade point averages, were more likely to graduate from high school, and generally reported more positive attitudes toward schooling. These improvements persisted into adulthood, when the treatment group was statistically significantly more likely to be employed, and less likely either to have been arrested or have received transfer payments such as cash welfare or food stamps.[18] When the improvements in long-term outcomes are monetized and discounted back to the start of the program, the benefits outweigh the costs by an estimated eight to one ratio (Heckman, 2006).[19]

The high rate of return to Perry probably represents an upper bound on the return to any universal program for disadvantaged children today. While the Perry treatment was high quality in many of the same respects as the Georgia and Oklahoma programs, the alternatives to participating in any program for lower-income children have expanded dramatically over time. Today, not only can lower-income children attend Head Start; they have access to some targeted state funded programs, as described above, and some find themselves in center-based or informal childcare arrangements while their mothers work. The marginal benefit of attending a high-quality program for low-income children today would therefore likely be smaller than it was in the 1960s. The participants in Perry were also extraordinarily disadvantaged, and marginal investments in their human capital might have had high returns.

Based in part on the early successes of Perry, Head Start was started in 1965 as part of the "War on Poverty." It is a large, public preschool program for low-income children that is generally thought to be lower quality than the Perry program, yet higher quality than the

[18] A similar pattern of findings has emerged for other preschool experiments, for example North Carolina's Abecedarian Project (Masse and Barnett, 2002; Anderson, 2008).

[19] Recent reanalysis using more sophisticated methods to account for multiple hypothesis testing by Anderson (2008) generally confirms these findings, but finds that the long-term benefits in Perry are concentrated among female participants.

childcare that is typically available to low-income parents (Currie, 2001). To put relative quality into context, Head Start has been estimated to cost more than many state-funded preschool programs, but less than Perry and other high-quality preschool programs (Currie, 2001; Barnett, 2001). However, Head Start ranks lower on the NIEER scale than many state-funded preschool programs, averaging a score just below 5 (Espinosa, 2002).[20] The program also reaches further up the income distribution than Perry did, and estimates of its longer-term impacts have been drawn not only from the earliest cohorts to attend, for whom the alternatives were relatively limited (Currie and Thomas, 1995; Garces, Thomas, and Currie, 2002), but also from among more recent cohorts with a wider range of alternatives (Deming, 2009).

The first randomized evaluation of Head Start, the Head Start Impact Study (HSIS), was only conducted in 2002, so this literature is quasi-experimental. The predominant research design has been to compare outcomes across siblings who were exposed to different preschool environments (i.e. Head Start, another preschool, or no preschool). Much of the within-family variation appears to be idiosyncratic and may be driven by the availability of slots at local programs, which are often over-subscribed.[21] These quasi-experimental estimates of the long-term effects of Head Start have much the same pattern as found in the experimental literature, but the findings are somewhat muted, as might be expected. Findings from sibling comparisons suggest that Head Start participation, relative to attending either no preschool or a non-Head Start preschool, has a substantial positive effect on vocabulary test scores during elementary school and makes a child less likely to repeat a grade (Currie and Thomas, 1995; Deming, 2009).

[20] For example, Head Start teachers tend to have relatively weak academic credentials. However, the quality of Head Start may have improved recently in response to recent policy initiatives. Currently 56 percent of Head Start teachers hold a bachelor's degree, and another 30 percent hold an associate's degree, up from 30 and 16 percent, respectively, in 1999. Over the same time period, the average child-to-teacher ratio dropped from 19.8 to 17.6, and the average staff turnover rate dropped from 16.8 to 13.2 (Bassok, 2012).

[21] On the other hand, to the extent that a change in a family's economic circumstances renders one sibling eligible and another ineligible, this approach may understate the impact of the program.

Estimates are only about 20 percent smaller than the Perry Preschool impacts. While test score gains fade to a fraction of their initial levels by ages 11 to 14, during their teen years Head Start participants are less likely to have been charged with a crime or be a teenage parent (Deming, 2009), and are more likely to complete high school and attend college as young adults (Garces, Thomas, and Currie, 2002).[22]

The pattern of findings in both Perry and Head Start—strong contemporaneous impacts, followed by smaller medium-term impacts, culminating in strong adult impacts—has been documented repeatedly in the early childhood evaluation literature.[23] For this reason, it would be ideal to measure impacts on longer-term child outcomes besides test scores. Unfortunately, the Georgia and Oklahoma programs were introduced too recently for us to estimate their impacts on adult outcomes. However, finding evidence that these programs improved test scores as late as eighth grade may provide strong indication of their potential for a truly lasting impact.

By comparison to the literature on child outcomes, the literature on the impacts of Perry and Head Start on families is quite thin. Using data and random variation in Head Start participation from the 2002 HSIS, Gelber and Isen (2013) find that Head Start induces parents to increase their involvement in their children's learning, as measured by increases in time spent reading to kids, more visits to cultural events, and more time spent with nonresident fathers. These improvements in the home environment persist even after Head Start ends.

[22] Ludwig and Miller (2007) also find evidence that Head Start participation increases educational attainment, exploiting the sharp difference in special grant-writing assistance afforded to counties with similar poverty rates at the program's inception.

[23] James Heckman and his coauthors (e.g., Heckman, 2006; Knudsen et al., 2006) have put forth a helpful framework to explain this pattern, relying on a distinction between cognitive and non-cognitive skills. While cognitive skills encompass the ability to add, subtract, read, and so on, and are readily measured on traditional standardized tests taken by school-age children, so-called "non-cognitive" skills are less likely to be measured on achievement tests and include social, behavioral, and emotional skills, such as persistence, behavior, and the ability to cooperate. Both types of skills are important inputs to adult outcomes like employment, wages, marriage, and criminal activity. The hypothesized mechanism through which long-term gains are realized is through a permanent improvement in non-cognitive skills that persists after the temporary gains in cognitive skills have faded.

II. The Preschool Experience

For the "Preschool for All" initiative to have an impact on children's human capital, it must first affect their preschool enrollment. The first-order question for our analysis is therefore: How have the state "model" programs in Georgia and Oklahoma affected children's preschool experiences? It stands to reason that these programs have unambiguously increased the chances that disadvantaged children attend preschool and the average quality of preschools attended, given the constraints faced by lower-income families. However, a substantial fraction of these programs' budgets could subsidize higher-income families, where children may already have a relatively high probability of attending preschool, and for whom program quality may already be quite high. For these children, there is not only less scope to increase preschool enrollment, but the private programs "crowded-out" may also be at least as high quality as the state program, opening the possibility of negative impacts on human capital.[24]

Our first analytical challenge is therefore to estimate how these programs have affected preschool experiences, and how this has varied by a child's family background. The best available data for this purpose are from the October CPS School Enrollment Supplements. The October supplements provide annual data back to 1968 (with complete information on state of residence dating to 1977) on the preschool enrollment of 4 year olds, both overall and by school type, public or private.[25] We use the school type question to provide insight into crowd-out, noting that the possible inclination of survey respondents to classify state-funded preschool as

[24] This is of course not to say that choice is welfare reducing, since the reduction in human capital will be offset by the family's lower spending on childcare. We explore the impacts of these programs on consumption of childcare services in the next section.

[25] Ideally, we would have detailed enough information to classify children into "school entry cohorts" based on their birthday and the minimum age of kindergarten entry in their state. While information on school entry regulations is available, we only know a child's age as of October. Fortunately, the vast majority of children who are 4 years old in October would be eligible for preschool rather than kindergarten. In Georgia and Oklahoma, for instance, children must be 4 years old (5 years old) by September 1 to enter preschool (kindergarten).

private enrollment in Georgia, where the program operates in part through private centers, should bias us against finding evidence in favor of it.

Importantly, because entire households are surveyed, the October supplements allow us to match children to other family members, and so obtain information on a family's current socioeconomic status (SES). While the "Preschool for All" initiative conceives of SES as family income, family income itself would be an outcome of the program if it were to change maternal employment—a question that we address below.[26] We therefore think a more desirable sample split is based on a strong predetermined *predictor* of family income. Following a long tradition of measuring family background with parental education, we split the sample into two maternal education categories—children of mothers with no more than a high school degree and children of mothers with at least some college education.[27]

II.A. National Trends in Preschool Enrollment

To set ideas, Figure 3 shows national trends in public preschool enrollment, private preschool enrollment, and overall preschool enrollment (the sum of public and private) from 1968 through 2011, both for the population of 4 year olds overall and for each of the two maternal education categories. As noted in reference to Figure 1, the overall national trend in public preschool enrollment (solid black line in panel a) lines up well with the introduction of state funding for preschool. Given the targeted nature of most of these programs, it is not surprising that the increases in public preschool enrollment have been greater among the children

[26] Likewise, because universal preschool may change a family's disposable income, we hesitate to use a measure family background with an outcome like home ownership.

[27] We have also split the sample into four maternal education categories (high school dropout, high school degree only, some college only, and college degree or more) and found that dividing the sample into two groups was a parsimonious way of capturing the patterns in the data. While the college-educated share of mothers has increased dramatically over the sample period (see, for example, Goldin, Katz, and Kuziemko, 2006), there is no evidence to suggest that maternal education changed in response to the Georgia and Oklahoma programs. Paternal education is arguably another important predetermined predictor of a family income, but we are only able to observe the educational attainment of fathers who are present in the household, leading to a substantial missing data problem.

of women with no more than a high school degree (solid gray line) than among the children of women with some college or more (dashed gray line). That said, by the end of the sample period, the children of more educated women were still more likely be attending any preschool (panel c), continuing their long tradition of being more likely to attend a private program (panel b).

But more striking—and at the heart of the question regarding crowd-out—is the fact private preschool enrollment rates of 4 year olds declined markedly over roughly the same period that state-funded preschool programs expanded. Regardless of maternal education, private preschool enrollment rates in 2011 were not much higher than they were in 1968, having peaked right before the recession in the early 1990s. While not a formal estimate of the extent of crowd-out, Table 1 provides some useful accounting. Between 1985 and 2010, the private preschool enrollment rate of 4 year olds whose mothers have some college education or more fell by 8.7 percentage points; among 4 year olds whose mothers have a high school degree or less, the decline in the private enrollment rate was almost as large, at 7 percentage points. The declines in private enrollment among children in these groups represent significant shares—41 and 25 percent, respectively—of their increases in public preschool enrollment over the same period. As a result, the groups' gains in overall enrollment between 1985 and 2010 have been much more limited than the expansion of public enrollment alone would suggest: gains of 12.5 percentage points and 20.8 percentage points, respectively.

Although suggestive of crowd-out, the declines in private preschool enrollment over the past 25 years might have been caused by other factors, such as the stagnation in maternal labor force participation over the same time period.[28] Indeed, suggesting a link between maternal employment and private enrollment, the increases in private enrollment through 1985 occurred

[28] Increases in the state-mandated age at which children are eligible to enter kindergarten since the mid-1980s and in the practice of "red-shirting" (delaying a child's entrance into school) (Deming and Dynarski, 2008) are other potential contributors to the downward trend in private preschool enrollment of 4 year olds shown in Figure 3.

over a period of rising maternal employment rates (see Table 1).[29] While this too is only a correlation in two national time series, it reminds us that private preschools also provide childcare, the demand for which is driven by factors beyond the availability of public alternatives. We now turn to our formal estimates of crowd-out in response to the Georgia and Oklahoma programs, using an empirical strategy that has the potential to account for those other factors.

II.B. Impacts of the Georgia and Oklahoma Programs on Preschool Enrollment

EMPIRICAL STRATEGY. Our analysis separates out the trends in preschool enrollment in Georgia and Oklahoma from those for states elsewhere in the country, using the October CPS supplements from 1977 forward.[30] With these data in hand, we can compare the changes in preschool enrollment in these "model" states around the introduction of their universal preschool initiatives to changes in preschool enrollment in the rest of the country over the same period. Thus, instead of attempting to measure and control for all of the other factors affecting private preschool enrollment, we assume that the effects of these other factors are accurately embodied in what happened to preschool enrollment in other states.

In its simplest incarnation, this quasi-experimental "differences-in-differences" approach is captured in the following model:

$$(1) \qquad y_{st} = \theta\, post_{st} + \gamma_s + \delta_t + \varepsilon_{st},$$

where y_{st} is a preschool enrollment rate of 4 year olds in state s in October of year t (in percent terms); $post_{st}$ is an indicator variable set to one in Georgia from 1995 forward and in Oklahoma from 1998 forward, zero otherwise; and γ_s and δ_t are vectors of state and year fixed effects,

[29] A full time series of maternal employment rates is provided in Figure 5, panel a.

[30] We estimate the model on data collapsed to state-by-year level means (in this case, enrollment rates), so as not to give more weight to the program in the more populous state of Georgia. However, given that the underlying data are essentially a random sample, and state-by-year-by education category sample sizes can get small, we do weight by the number of observations in the state-by-year cell for efficiency purposes.

respectively. The state fixed effects account for fixed differences in preschool enrollment rates across states, while the year fixed effects account for common shocks to the preschool enrollment rate (e.g., shocks to private preschool demand stemming from the business cycle). In some specifications, we also add a vector of state-by-year-varying controls, including child demographics and the state unemployment rate. ε_{st} is an error term, which represents unobserved determinants of enrollment rates.

Estimation of this simple model will identify the coefficient of interest, θ, only if none of the unobservables is correlated with the adoption of universal preschool. This assumption would be violated if, for example, the private preschool enrollment rates in Georgia and Oklahoma would have been on a steeper downward trajectory than elsewhere even without starting universal preschool programs. In this case, the estimates would imply more crowd-out as a result of universal preschool than has actually taken place. The model is also restrictive in the sense that it assumes that the effects of universal preschool on enrollment rates were both immediate and persistent.

To assess the appropriateness of these assumptions, we actually begin by estimating a less restrictive "event-study" model, which allows us to test whether the "model" states were on different enrollment trajectories prior to introducing their programs, as well as whether the impacts of these programs have remained constant as they have matured. In this model, we replace the $post_{st}$ indicator in equation (1) with a series of indicator variables for year relative to the year that universal preschool was introduced. Instead of creating an indicator for each individual year relative to the initiative, we create dummies for 3-year bins to reduce noise. So that the coefficients are identified, we leave omit the dummy for the three years immediately

prior to the initiative (representing 1992-94 in Georgia and 1995-97 in Oklahoma).[31] This model provides us with transparent graphical evidence on the credibility of model (1)'s identifying assumptions.

FINDINGS. Figure 4 plots the event-study estimates for each of the three preschool enrollment rates. To facilitate the comparison across family background, we present the coefficient estimates for a given variable for children from both maternal education categories in the same graph, offsetting each series to the right and left of the midpoint of the relevant interval for ease of viewing. The capped lines around the coefficient estimates represent 95 percent confidence intervals.[32]

For children whose mothers have a high school degree or less (solid circles), the introduction of the Georgia and Oklahoma programs appears to have increased enrollment in public preschools by 15 to 20 percentage points relative to expectations based on public enrollment trends elsewhere in the country (panel a). The point estimates are somewhat higher among children whose mothers have more education (open circles), centered around a 25 to 30 percentage-point impact. For both groups of children, each of the post-program coefficient estimates is statistically significant, and there is little evidence to suggest that the effects of the program on public enrollment have systematically grown or shrunk over time.[33]

The remainder of the figure then provides evidence of a differential crowd-out response by family background. Children with lower-education mothers did not see faster declines in private preschool enrollment than experienced elsewhere in the country after universal preschool

[31] The first and last indicators represent all prior and subsequent years, respectively.
[32] Standard errors allow for heteroskedasticity and correlation in the error terms within states over time.
[33] This might seem somewhat surprising, given that administrative data suggest that universal preschool enrollment rates in Oklahoma increased somewhat gradually over time, e.g., almost doubling between the 1998-99 and 2005-06 school years (Bassok, Fitzpatrick, and Loeb, 2012). The program was phased in much more rapidly in Georgia, however, and, as suggested by Figure 3, panel a, the counterfactual is one of increasing public preschool enrollment.

programs were introduced (panel b). As a result, their overall enrollment increase tracks almost one-for-one with the increase in public enrollment (panel c). On the other hand, for the higher maternal education group, private preschool enrollment declines between 10 and 20 percentage points more than expected based on national trends (panel b), greatly diminishing the overall enrollment impact (panel c) relative to program take-up (panel a). On net, both groups of children are more likely to be enrolled in preschool after the introduction of the universal preschool, but the overall enrollment effects appear to be smaller for higher-SES children.

Table 2 presents the DD estimates, helping us to put some more concrete numbers on these patterns. As anticipated from the graphs, estimates of the simple DD model in equation (1) (column 1) yield larger impacts of universal preschool on the public preschool enrollment of children of more-educated mothers—a 19.6 percentage point increase (panel b)—versus a 16.9 percentage point increase for the children of less-educated mothers (panel a). Controlling for state unemployment rates and racial composition does not change the estimates much (column 2), though the difference across family background is made more stark when we add separate linear time trends for Georgia and Oklahoma in an attempt to account for the relatively strong downward trend in public enrollment among the children of more-educated moms in the "model" states prior to universal preschool availability (column 3).[34]

Regardless of specification, however, the corresponding estimates for private enrollment yield similar implications for private preschool crowd-out. Among children whose mothers have at least some college education, the reductions in private enrollment following the introduction of public preschool imply a crowd-out rate of 39 to 50 percent, depending on the specification.

[34] One concern with a specification such as this, explored most prominently by Wolfers (2006), is that when a policy's effects grow or shrink over time, state-specific trends will pick up these dynamics in addition to pre-existing trends, potentially generating substantial bias in DD coefficients. When we follow his suggestion of including the series of post-initiative indicators from our event-study model in lieu of $post_{st}$ in equation (1), our substantive conclusions are unchanged.

That is, for every 10 children with more-educated mothers who enroll in public preschool, 4 or 5 would otherwise have been enrolled in private school. This figure lines up very well with the back-of-the-envelope calculations based on the national trends in Table 1. Unlike these simple calculations, however, we find no evidence of private preschool crowd-out among children whose mothers have a high school degree or less. In fact, is some evidence of crowd-*in*, which could reflect the operation of state-funded preschools out of private childcare centers in Georgia.[35]

The bottom line is that the universal preschool programs in Georgia and Oklahoma increased the likelihood of enrolling in preschool at age 4 by more among lower-SES children—between 18.5 and 20.2 percentage points—than among higher-SES children—between 11.6 and 14.1 percentage points. As a result of crowd-out, the cost per *new* preschool enrollee in the population at large is between 16 and 19 percent more than the per-pupil cost of the program, e.g., between roughly $8,600 and $8,800 (versus $7,427) in Oklahoma.[36]

III. Family Well-Being and Behavior

While private preschool crowd-out might diminish the potential impact of universal preschool programs on the human capital of higher SES-children, it makes some higher-SES families better off in the short term by reducing their childcare expenses. By increasing a mother's (or primary caregiver's) wage net of childcare costs for at least part of the work day, the childcare price subsidy implicit in universal preschool may also increase labor force participation, and thus a family's income and child investment possibilities, regardless of family background. Further, while these programs should unambiguously reduce the amount of time

[35] And indeed, when we allow for separate effects of the Georgia and Oklahoma programs, we find that Georgia's program leads to significantly less of a reduction in private preschool enrollment.
[36] The 16 to 19 percent figures are the crowd-out ratios from regressions like those shown in Table 2 estimated on the entire population of 4 year olds.

that many 4 year olds will spend at home, they may change the quality of the remaining time that parents spend with their children, depending on whether parents view these investments as substitutes or complements for formal education.

Each of these potential responses to the program—in terms of the allocation of parental time, consumption patterns, and labor supply—might affect a child's human capital accumulation independently of preschool itself. In this sense, any impacts of universal preschool on children's test scores will be reduced-form, reflecting not only participation in the preschool program itself, but also changes in parental investments. In this section, we attempt to understand the magnitude of these changes before turning to our estimates for children's test scores.

III.A. Childcare Spending

By definition, the movement of children from private preschool programs into state-funded ones should put money in a family's pocketbook. But how much do families save in childcare expenses? How large an income transfer does universal preschool provide? For this question, we turn to the Consumer Expenditure Survey (CEX), which has asked comparable questions about expenditures on childcare and daycare services since 1994. Note that this information is on all household childcare expenses, and does not allow us to isolate spending on just the 4- or 5-year-old child. Limited pre-initiative data are available, and the number of households with 4 year olds is much smaller than in the October CPS.

We therefore take an alternative DD approach, comparing the difference in childcare expenses between families with 4 year olds and families with 5 year olds in Georgia and Oklahoma to the same difference elsewhere in the country. We restrict the sample to the period

after the model preschool programs are in place, analyzing monthly data from September 1998 through June 2011.[37] The model of interest is:

$$(2) \qquad ccspend_{as} = \alpha + \theta age4_a treat_s + \delta age4_a + \gamma treat_s + \varepsilon_{as},$$

where $ccspend_{as}$ represents average monthly spending (in inflation-adjusted 2012 dollars) on childcare for families in state s with at least one child aged a; $age4_a$ is an indicator variable set to one if that average corresponds to families with a 4 year old; and $treat_s$ is an indicator variable set to one if the state in question is Georgia or Oklahoma.[38]

The coefficient of interest in (2) is on the interaction between these two indicators, and captures the extent to which universal preschool programs reduce the gap in childcare spending between families with 4 year olds and families with 5 year olds, all of whom have access to at least half-day kindergarten. This coefficient will capture the effect of universal preschool if there is no other reason to expect a smaller gap in the "model" states. For example, the childcare spending gap would be smaller if Georgia and Oklahoma were less likely to offer full-day kindergarten for 5 year olds than other states. In fact, the opposite appears to be the case.[39]

The first column of Table 3 presents estimates of equation (2), again splitting the data into two maternal education groups. Consider first the estimates for families with college-educated mothers (panel b). The coefficient of interest is a statistically significant $66 reduction in monthly childcare expenses (real 2012 dollars). This almost perfectly offsets the additional average childcare spending for families with 4 year olds versus 5 year olds, which is estimated to

[37] We drop spending observations for July and August, when preschool is typically not in session. The CEX suppresses state of residence for confidentiality purposes in some cases. For example, starting in 2006, we can no longer identify Oklahoma in the data because it is suppressed. Because the data are not evenly dispersed over time for all states, we also collapse the data to the year level and include year fixed effects in the models.

[38] As in our estimates based on the October CPS, we weight by the number of observations used to calculate the state-by-age group-by-year averages, for efficiency purposes.

[39] Using the October supplements from 1998 through 2011, we find that 64.5 percent of 5 year olds in Georgia and Oklahoma whose mothers have at most a high school degree attend full-day kindergarten; elsewhere in the country, this figure is 51.7 percent. For the 5-year-old children of women with at least some college education, full-day kindergarten enrollment rates are 54.9 and 54.2 percent for the treatment and comparison states, respectively.

be \$70 per month (the coefficient on age_a). Because the sample sizes are small and we are concerned about outliers, in column 2 we top-code childcare spending at the 99th percentile value (\$1,800 per month), and in column 3 we take the median childcare spending instead of the mean. The results are attenuated somewhat when outliers are restricted, with estimates ranging from a reduction of \$50 to \$56 per month. Scaled up, these coefficients imply on average a \$450 to \$630 reduction in childcare expenses for the 9-month academic year.

While such an effect might seem too small to care much about, recall that this is the reduction in *average* childcare expenses in the population at large, regardless of whether a family takes up universal preschool. Moreover, among those taking up the program, some families—those switching out of private enrollment—may see very large reductions in childcare spending, while other families might not be affected much at all. Although we cannot identify these families in the CEX (and would not want to because of endogeneity concerns), we can do a back-of-the-envelope calculation of the magnitude of the transfer to these families based on our private enrollment estimates from the October CPS. Returning to Table 2, panel b, we see declines in private school enrollment of 8 to 13.5 percentage points among 4 year olds with mothers in the higher education category. Assuming that the effects on childcare expenses in the CEX for this group are explained completely by the movement of 4 year olds out of private preschool, the top-coded estimates from column 2 imply that families that switch from private preschools to the public program save between \$3,333 and \$5,625 per academic year on childcare expenses.[40] By comparison, the maximum EITC in the 2012 tax year for a family with two qualifying children was \$5,236. However, universal preschool provides an income transfer only to relatively high-income families.

[40] These figures are in line with estimates of childcare expenses from the Survey of Income and Program Participation, which range from about \$110 to \$143 per week (in real 2011 dollars) over the CEX sample period (Laughlin, 2013).

Indeed, returning to Table 3, we see that childcare expenses in families where mothers have lower levels of education have not fallen, consistent with no evidence of private preschool crowd-out for this group. In fact, the gap in childcare expenses between families with 4 year olds and families with 5 year olds is higher in Georgia and Oklahoma than it is elsewhere in the country, although this difference disappears when we estimate the differences on median spending (column 3).[41]

III.B. Maternal Labor Supply

The enrollment and childcare spending estimates suggest that some families—relatively high-SES families that arguably place relatively high value on preschool education—receive sizable income transfers from universal preschool programs. However, regardless of income, these programs—and public education more generally—provide a 100 percent price subsidy for childcare on the extensive margin of employment, increasing potential take-home pay and so in theory providing a strong incentive for their primary caregivers (historically, mothers) to enter paid work. Increases in maternal employment can increase family income, expanding a family's consumption possibilities—and child investment possibilities—as well.

How have the Georgia and Oklahoma programs affected maternal labor supply? To address this question, we return to our match of 4 year olds to their mothers in the October CPS and to our original DD estimation strategy. Our outcome of interest is the percent of mothers of 4 year olds who were working in the week prior to the survey.[42] Figure 5 presents national trends in this variable, overall and by maternal education (panel a), alongside the event-study estimates by maternal education category (panel b). The first two columns of Table 4 present the estimates

[41] The positive coefficients might reflect relatively high rate of full-day kindergarten enrollment among 5 year olds with less-educated mothers in Georgia and Oklahoma.

[42] We base our analysis on the employment status recode variable, which has been consistently asked of CPS respondents over our sample frame.

of the $post_{st}$ coefficient from a model analogous to (1), without (column 1) and with (column 2) state-by-year varying controls.

These initial results provide little evidence to suggest that the preschool programs in Georgia and Oklahoma have increased the likelihood that mothers of 4 year olds are at work. In fact, the coefficient estimates are negative, and in the case of more-educated mothers (panel b), statistically different from zero. This is unexpected, and suggests that a simple DD model like (1) might not be identifying the effects of the program. Indeed, the event-study estimates in Figure 5, panel b do not provide compelling evidence of a negative impact. Rather, they suggest that we are finding a negative impact due to unusually high maternal employment rates in Georgia and Oklahoma well before the states introduced universal preschool programs, e.g., the coefficient for 7 to 9 years prior to the initiative (plotted at -8) is positive, large, and statistically significant. Thus, what appear to be negative program impacts may actually reflect convergence of the rest of the country to Georgia's and Oklahoma's higher initial rates of maternal employment.

One approach to this problem would be to re-estimate the simple DD models dropping or dummying up the earliest years of data (i.e., data 7 or more years before the initiatives), so that they do not contribute to identification. Another approach, which we find more compelling, is to attempt to account for any such convergence using the mothers of other young children—like mothers of 5 year olds—as an additional comparison group. The idea is these mothers should have experienced the same labor market shocks as mothers of 4 year olds, but their children are not eligible for universal preschool. To the extent that their employment trends of these two groups of moms diverge after the initiatives were passed in Georgia and Oklahoma, relative to the rest of the United States, it would suggest an employment impact of the program.

We thus combine the two empirical approaches that we have used thus far into a "triple-difference" (DDD) model that uses variation across states, over time, and across women with children of different ages. The model is given by

$$(3) \qquad y_{ast} = \theta\, post_{st} \times age4_a + \lambda\, post_{st} + \gamma_{as} + \delta_{at} + \varepsilon_{ast},$$

where y_{ast} is the percent of mothers of children aged a in state s who reported working in October of year t, and $post_{st}$ and $age4_a$ are as earlier defined. The vectors γ_{as} and δ_{at} represent age-by-state and age-by-year fixed effects, which control for fixed differences across states in employment rates of mothers with children of different ages, and national shocks to employment rates of mothers of children of the same age. The $post_{st}$ direct effect, λ, is the "effect" of universal preschool on women with age-ineligible children. The interaction coefficient, θ, is then the effect of interest, capturing the *differential* impact of universal preschool on the employment of women with 4 year olds.[43]

The remaining columns of Table 4 show estimates of θ in model (3) using three alternative comparison groups: mothers of 3 year olds (column 3), mothers of 5 year olds (column 4), and mothers of 3 year olds or 5 year olds (column 5). The estimates are consistent across comparison groups, and quite different than the findings from the basic DD specification. Regardless of maternal education, the estimates are much less negative than those in the first two columns. This suggests that the use of a comparison group helps to account for that convergence suggested in Figure 5, panel b, and indeed, when we examine the triple-difference event-study

[43] Cascio (2009b) uses a similar DDD approach to estimate the effect of state funding for kindergartens (and kindergarten enrollment) on employment of women with 5 year olds. Most of the kindergarten funding initiatives had passed by the time that the data used here begin (1977).

that uses mothers of 5 year olds as a comparison group, in panel c of Figure 5, the pre-initiative coefficients fluctuate around zero.[44]

Turning to the DDD estimates themselves, we consistently see no impact of universal preschool on the employment rates of women with at least some college education (panel b). The estimates are negative, small, and generally precisely estimated enough to allow us to rule out even small positive impacts. Importantly, this is not because the DDD specification fails to yield an impact of universal preschool on school enrollment: as shown lower down in the panel, the introduction of universal preschool is associated with between 16.6 and 21.7 percentage-point increases in 4 year olds' public school enrollment rates—figures that, along with the crowd-out rates implied by the corresponding estimates for private school enrollment, line up well with the simple DD estimates in Table 2.[45]

The estimates for less-educated mothers, by contrast, are consistently positive, and in the specification using mothers of 5 year olds as a comparison group, statistically different from zero; for their children, there is also clear evidence of an impact of the programs on school enrollment. While these findings are promising, the corresponding event-study estimates (Figure 5, panel c) are less compelling. As shown in panel c of Figure 5, the positive coefficient appears to be driven by an impact in the first three years of the program (the solid coefficient plotted above 1). All remaining post-initiative event-study coefficients are statistically insignificant, and some are negative.

[44] To arrive at these event-study estimates, we replace $post_{st}$ in model (3) with a full set of indicators for year relative to the initiative (in 3-year-bins), and we replace $post_{st}$ x age_a with interactions between these indicators and age_a. Figure 5, panel c plots the estimated coefficients on these interaction terms, separately by maternal education.

[45] We look at public and private *school* enrollment here, rather than public and private *preschool* enrollment, because most 5 year olds would be expected to be in kindergarten. From the mother's perspective, however, what matters for maternal employment should be whether a child is in (public) school, not the child's grade of enrollment. To the extent that these numbers deviate in any meaningful way from those in Table 2, it suggests an impact of universal preschool programs on the grade in which a child is enrolled. For example, some children may have attended private kindergarten in the absence of public preschool.

On net, these findings suggest that the Georgia and Oklahoma programs may have increased maternal employment, but only among less-educated moms, and only temporarily. These findings are consistent with those in Fitzpatrick (2010), who approaches the question using a different research design and finds no impact, as well as with the fact that maternal employment rates appear to have "topped out" since the early 1990s (Figure 5, panel a).

III.C. Maternal Time with Children

By definition the time parents spend on childcare should decline essentially hour-for-hour with the time that children are newly in preschool. Accordingly, given the relatively low degree of private preschool crowd-out among lower-SES families, we would expect relatively large reductions in the time mothers spend with children in these families. However, universal programs may change optimal time investments in children. For example, preschool exposure may encourage parents to spend more or less time engaging in educational activities with their child, such as reading to children, doing art projects, and so on.

To investigate this question, we use data from the American Time Use Surveys (ATUS) spanning January 2003 through December 2012. The ATUS provides nationally representative estimates of how, where, and with whom respondents spend their time, and can be linked to information from the CPS. Because the ATUS contains no pre-initiative data, we take the same general approach as in the CEX, comparing differences across mothers with 4 year olds and mothers with 5 year olds on daily time spent on childcare, in the two "model" states versus the rest of the country.[46] Note that, analogous to the CEX, we cannot isolate time spent on an individual child when there are other children in the household. Time use is measured in minutes.

[46] We limit the dataset to women co-residing with a 4- or 5-year-old child, and exclude time use observations collected on weekends, holidays, or during July or August. To increase statistical power, we average within state-by-education cell across all years pooled together.

The first column of Table 5 shows the impact on the overall amount of time a mother spends in the presence of her child.[47] Regardless of education, mothers spend an average of almost 8 hours per day with their children. In the absence of universal preschool, mothers also spend more time with 4 year olds than with 5 year olds, though this difference is only statistically significant for lower-education moms (panel a). With universal preschool, however, this gap is eliminated: the DD coefficient indicates that less-educated mothers in Georgia and Oklahoma on average spend 46 fewer minutes per weekday in the presence of their 4 year olds than their counterparts elsewhere in the country—an effect amounting to a little over 4 hours a day when rescaled by the impacts of universal preschool on overall school enrollment.[48] There is not a significant reduction in the time that more-educated mothers spend in the presence of 4 year olds, but the DD estimate is noisy (panel b).

In column 2, we then turn to the impact on time spent caring for and helping the household's children, and includes time spent with children reading, playing, doing art projects, and talking. The program increases maternal time spent in these activities by 25 minutes per day, which is consistent with experimental evidence from Head Start (Gelber and Isen, 2013). Among women with high levels of education, time spent caring for and helping children does not appear to be affected.

IV. Standardized Test Scores

The human capital benefits to high-quality universal preschool are likely to accrue in large part from the preschool experience relative to the alternative use of a child's time. Because of the findings for enrollment and crowd-out, we expect to see clear benefits for lower-SES

[47] Activities for which the ATUS does not collect information on who else was present, such as sleeping, are omitted from this measure.

[48] This is more than would be expected if the preschool programs were only half-day. However, in auxiliary regressions, we find that much of the impact of the Georgia and Oklahoma programs on preschool enrollment, regardless of maternal education, can be accounted for by full-day programs.

children: the marginal public preschool enrollee would otherwise not have attended preschool, the programs themselves meet high-quality benchmarks, and exposure to higher-income peers in a universal preschool classroom may have additional positive impacts. Parental investments also appear to rise in lower-income families. For higher-SES children, the potential effects are less clear: the marginal attendee has a high probability of attending a private preschool, that private preschool may be of equal or better quality than the state-funded one, and their academic achievement might be diminished by negative peer effects in the classroom. Higher-SES children may also see lesser gain in academic performance if there are diminishing returns to human capital investment.

To measure whether test scores improve in response to the model state programs, we assembled state-by-year aggregate data from the NAEP, which is the only standardized test that is comparable across the nation. Students are tested in mathematics and reading in grades 4 and 8 in selected years. Information is not available on maternal educational attainment, so we separate the sample instead by whether a student's family earns less than 185 percent of the poverty line and is therefore eligible for free or reduced-price lunch. While this sample split could be endogenous, our earlier analysis of maternal employment provides little evidence to suggest that the Georgia and Oklahoma programs have had a persistent impact on family income.

We focus on state-by-year mean scale scores on the NAEP math and reading tests. Figure 6 presents trends in mean national scale scores for math (panel a) and reading (panel b), plotting fourth and eighth grade scores on the same graph against the preschool cohort to which they pertain. For example, the earliest available data are from spring 1996; these data correspond to children who would have been preschool age in fall 1990 in the case of fourth grade scores, and

in fall 1986 in the case of eighth grade scores.[49] Thus, preschool cohort is given on the horizontal axis, and the year of the test is given above each data point. Unsurprisingly, eighth graders score higher on this vertically-scaled test, and lower-income students perform relatively poorly in each grade. Test scores have improved over time, albeit with little reduction in the test score gap by family income, with the most notable improvements coming in fourth-grade scores for cohorts turning age 4 around the time that the Georgia and Oklahoma programs went into effect.

Recall that our preferred empirical approach, used to estimate impacts on preschool enrollment and maternal employment, has been to compare trends in Georgia and Oklahoma relative to the rest of the country, before and after the introduction of their initiatives, and that simple DD estimates rely on the assumption that these trends are similar in the treatment and comparison states in the pre-initiative period. Figure 7, which plots the event-study estimates, provides strong evidence that this assumption is violated for fourth grade NAEP scores.[50] For example, fourth-grade math scores are significantly lower than would be expected given trends elsewhere in the country in the years immediately before the initiative (panel a); fourth-grade reading scores show a similar pattern, though the differences across pre-initiative years are not statistically significant (panel b). This appearance of an "Ashenfelter dip" (Ashenfelter, 1978) suggests that the positive and statistically significant coefficients on the post-initiative indicators for lower-income students may simply be mean reversion, rather than a true treatment effect.

When we look at eighth-grade test scores (panels c and d), on the other hand, there is less evidence of an "Ashenfelter dip," and more evidence that longer-term downward trends in test performance in Georgia and Oklahoma relative to the rest of the country were reversed once

[49] This calculation is made under the assumption of normal grade progression. Data are available by state and family income starting in spring 2000 (math) and spring 1998 (reading).

[50] Here, we use two-year bins. As above, however, the first and last indicators correspond to all prior and subsequent years, and we omit the identifier for the year(s) immediately prior to the initiative to identify the model.

these states implemented their universal preschool programs. For example, lower-income children in Georgia and Oklahoma who were 4 years old seven or more years prior to the introduction of universal preschool scored higher in eighth-grade math and reading than their counterparts elsewhere in the country. However, this advantage no longer existed among later cohorts that were 4 years old prior to the initiative, and among cohorts eligible for universal preschool, the advantage to be being in Georgia or Oklahoma returned, suggesting an impact of the program. A somewhat similar pre-initiative pattern is seen for higher-income children, but there is no real evidence of post-initiative test-score gains.

Table 6 presents DD estimates for eighth grade NAEP scores, separately for lower- and higher-income children; we focus exclusively on eighth-grade scores since a lack of sufficient pre-initiative data makes it impossible for us to rule out mean reversion in the case of fourth-grade scores. None of basic DD estimates is statistically significant (column 1). However, when we attempt to account for the early pre-initiative differences in test scores by adding a dummy for cohorts 7 or more years prior to the initiative (column 2), the estimates become more positive for lower-income students and marginally statistically different from zero in the case of math scores (panel a). An alternative approach, which controls for state-specific trends (column 3), also yields a positive, statistically significant impact on the eighth-grade math scores of lower-income students, but also a negative, statistically significant impact on the math scores of higher-income eighth graders. Because this model does not seem as a good characterization of the patterns shown in Figure 7, we return to the specification in column 2 in the remaining columns of the table, adding controls for student demographics (column 4) and years that a state has had

"consequential" school accountability (column 5).[51] Neither of these sets of controls appreciably changes the point estimates.

The take-away message from Table 6 is that lower-income children who were likely eligible for universal preschool in Georgia and Oklahoma scored on average about 2.2 points higher on the NAEP math test in eighth grade than would have been expected based on national trends, after accounting for several other state-by-year varying factors. Depending on the estimate, this amounts to a gain of 0.058 to 0.067 student-level standard deviations. Rescaled by the programs impacts' on preschool enrollment rates of lower-SES children, these findings suggest that preschool attendance increases eighth-grade math scores by almost a third of a standard deviation. Given our findings for maternal time use and labor supply, however, we think it unlikely that the impacts are working through preschool attendance alone.

V. Conclusions

President Obama's $75 billion "Preschool for All" initiative calls for dramatic increases in the number of 4 year olds in public preschool programs and in the quality of these programs across the nation. The proposed programs share many characteristics with the state-funded preschool programs Georgia and Oklahoma, which both meet high-quality benchmarks and are accessible to all children. We estimate the impacts of these "model" programs on a variety of outcomes using difference-in-difference frameworks. Our findings suggest sharply different impacts for children from across the income distribution, which is not surprising when one

[51] The school accountability programs put in place after the No Child Left Behind (NCLB) Act of 2001, as well as the "consequential" state accountability programs that preceded NCLB, may be an alternative explanation for the patterns we see in the data. Oklahoma implemented a consequential accountability in 1996 and Georgia in 2000 (Dee and Jacob, 2011). Comparing states that implemented consequential accountability as a result of NCLB to those that had it prior to NCLB, Dee and Jacob (2010, 2011) find evidence that NCLB resulted in higher NAEP scores, particularly in fourth grade.

recognizes that the impact of attending a high-quality public preschool depends crucially on what the child would have been doing in the absence of the program.

For lower-SES children, we find modest, sustained increases in math scores by eighth grade. The increases may be working through multiple channels. First, children are likely to switch from not attending preschool to attending a high-quality public preschool when a universal program is introduced. Moreover, there is evidence that although they spend less time overall in the presence of their mothers, they spend more time actively engaging in activities such as playing and reading with them. We also find suggestive evidence that there is a short-run increase in the likelihood that their mothers become employed. Among higher-SES children, we find no positive impacts on student achievement. These children are much less likely to be moved on the extensive margin of preschool enrollment, and instead are more likely to switch from private to public preschool in response to the program. We also find that some families are able to substantially reduce their spending on private preschool and childcare in response to the program, freeing these resources up for other purposes.

This pattern of results raises the question of whether the proposal design could be altered to obtain the same positive impacts without inducing as much crowd-out. Could a targeted program meet these goals more efficiently? For example, an alternative proposal could be to expand and improve the quality of the existing Head Start preschool program for lower-SES children, while leaving higher-SES children to continue to attend (and pay for) private preschool. One important caveat is that the test score gains observed in the universal public state programs might not be matched by a targeted program if preschool peer effects are an important mechanism. Indeed, these programs might be “high quality” not because they meet specific quality benchmarks, but rather because of improvements in the classroom environment from the

presence of higher-SES children. We cannot rule out this possibility, and we think it is an important question for future research. Alternatively, if the central policy goal is to ease the burden of childcare costs, there may be more targeted and equitable approaches to do this, e.g. through the tax code.

That said, while the Georgia and Oklahoma programs share a number of features with the preschool programs proposed under the Obama plan, there are several reasons to be cautious in generalizing our findings. First, and most importantly, the Obama plan calls for middle-class families to pay something to access the public programs. While the proposal is short on specifics, this aspect of it could make crowd-out less than what we estimate here. Second, while it has been the focus of our analysis, universal preschool is not all that there is to the "Preschool for All" initiative. For example, it also calls for increases in Head Start enrollment among 3 year olds, which affect the success of universal preschool in ways that we have not been able to measure. On the other hand, our suggestion that the programs in Georgia and Oklahoma have had some lasting impacts on the human capital of lower-SES children might have nothing to do with the quality benchmarks emphasized in the Obama plan. We have estimated the reduced-form impacts of these programs, and the mechanisms at work are not completely clear. The available data have also limited our analysis in important ways that might be rectified in future research.

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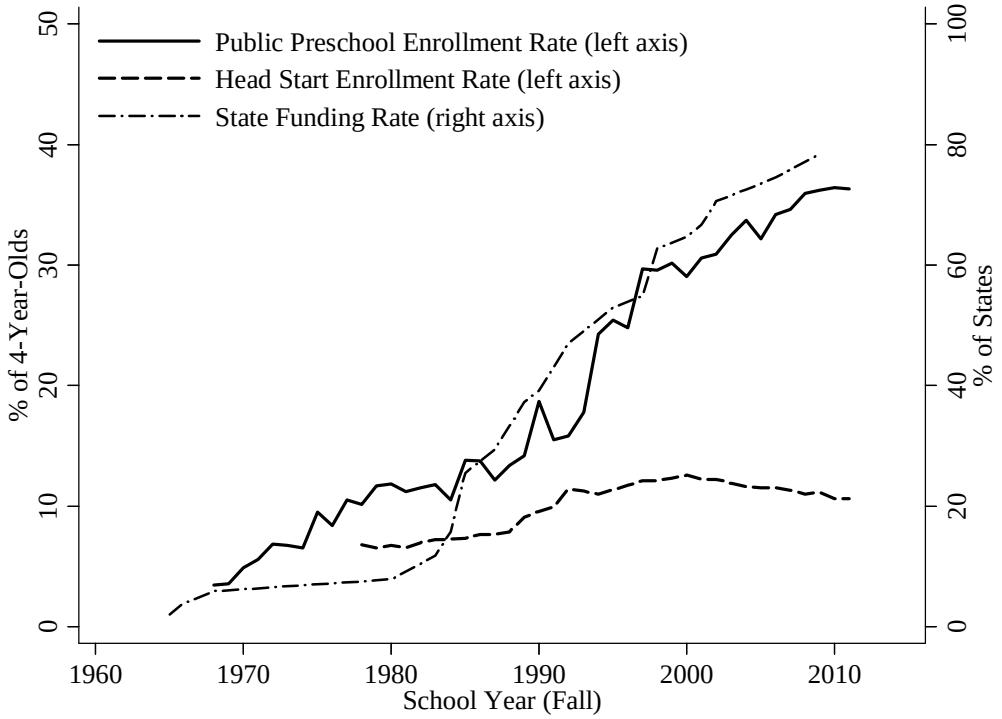
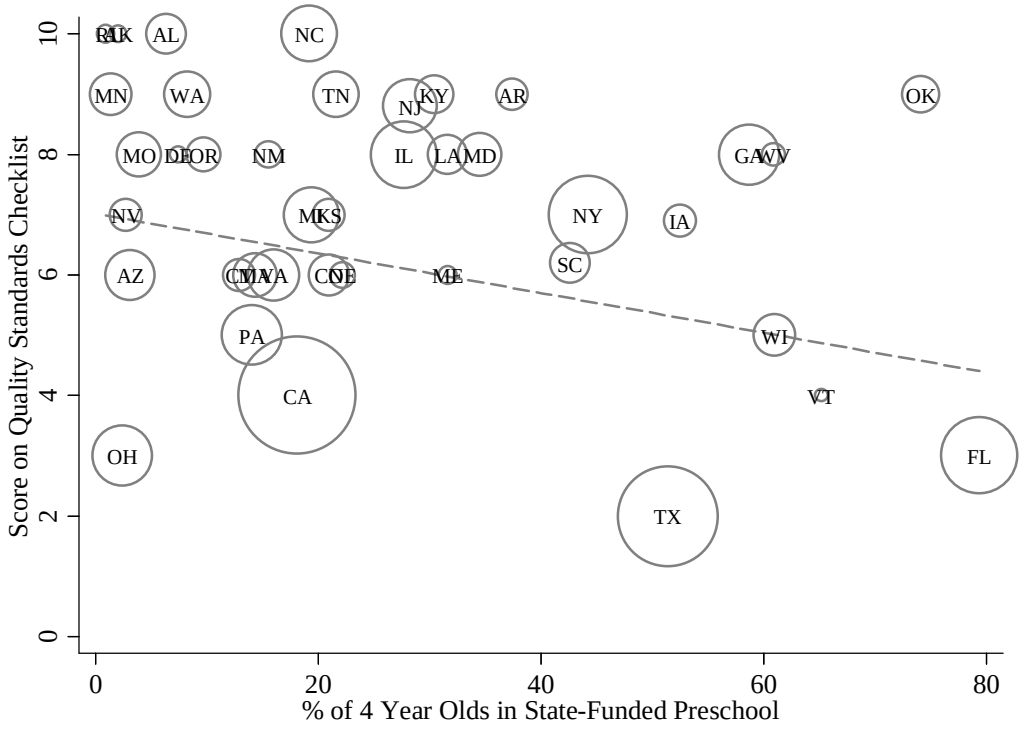
Figure 1. Percent of 4 Year Olds Enrolled in Public Preschool Programs and Percent of States Funding Preschool Programs, 1965-2011



Sources: October CPS (public preschool enrollment rate), Head Start Bureau (Head Start enrollment rate), National Institute for Early Education Research (state funding dates).

Figure 2. Relationship between Quality and Access in State-Funded Preschool Programs, 2011-2012 School Year [a]

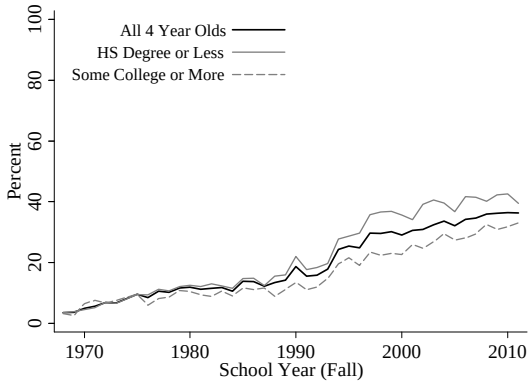


Sources: National Institute for Early Education Research (2012) and Vital Statistics.

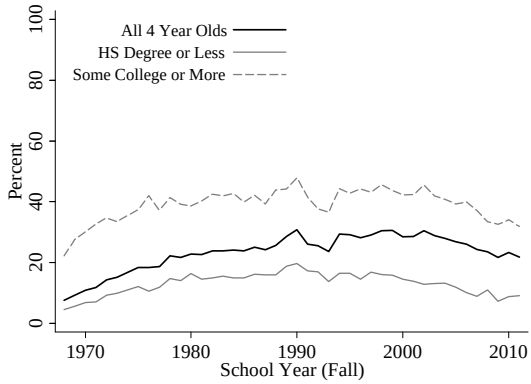
[a] Dot sizes represent the number of children born in the state four years prior. The dashed line represents the regression fit, weighting by this figure; the unweighted fit is substantively similar. The quality standards checklist gives equal weight to each of 10 factors: program has comprehensive early learning standards; teachers are required to have a BA; teachers are required to have specialized training in preschool; assistant teachers required to have a Child Development Associates (CDA) Degree (or equivalent); at least 15 hours per year of in-service required; the maximum class size is 20 students; staff to child ratios are 1-to-10 or better; program offers vision, hearing, health, and one support service; program offers at least one meal; program offers site visits.

Figure 3. Preschool Enrollment Rates of 4 Year Olds, Overall and by Maternal Education, 1968-2011

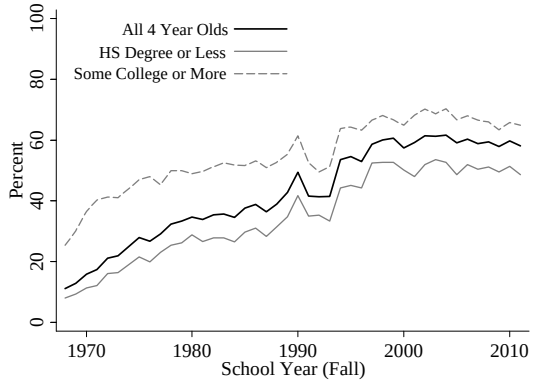
a. Public Preschool Enrollment



b. Private Preschool Enrollment



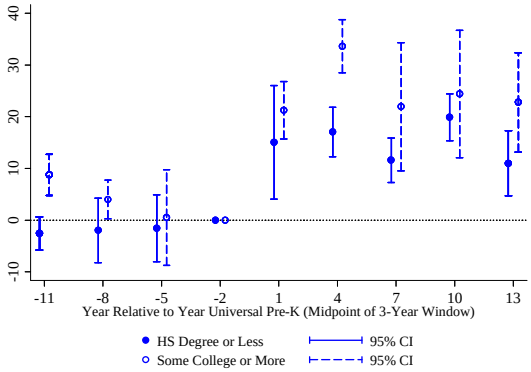
c. Any Preschool Enrollment



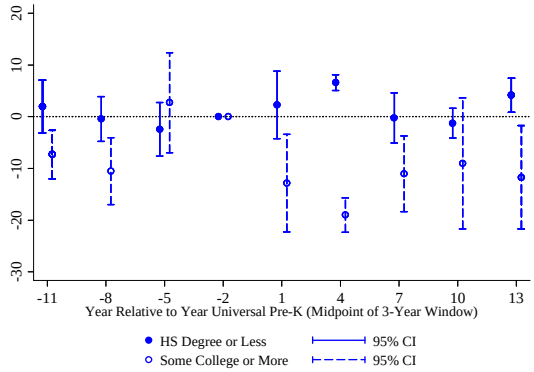
Source: October CPS school enrollment supplements, 1968-2011.

Figure 4. Event-Study Estimates of the Effect of Introducing High-Quality Universal Preschool on Preschool Enrollment Rates of 4 Year Olds, by Maternal Education[a]

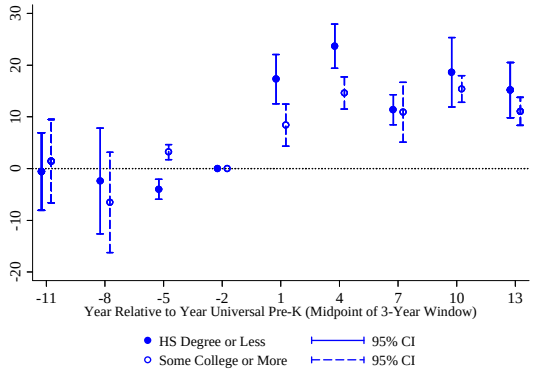
a. Public Preschool Enrollment



b. Private Preschool Enrollment



c. Any Preschool Enrollment

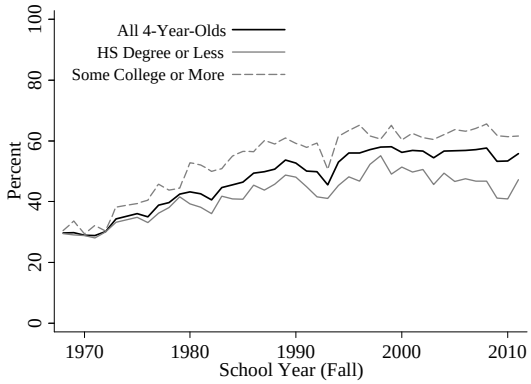


Source: October CPS school enrollment supplements, 1977-2011.

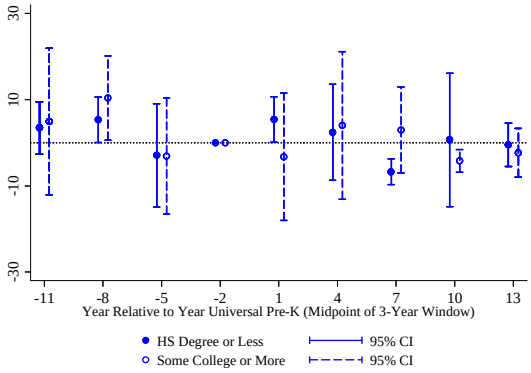
[a] States with high-quality preschool programs are Georgia (introduced in fall 1995) and Oklahoma (introduced in fall 1998). All regressions include state fixed effects and year fixed effects in addition to dummies for 3-year groupings of year relative to the year that high-quality preschool was introduced. The coefficients plotted at -11 represented 10 years or more prior to introduction, while the coefficients plotted at 13 represent 12 or more years after; the dummy at -2, representing the three years immediately prior to introduction, is omitted to identify the model. All regressions were weighted by cell size, and standard errors were clustered on state.

Figure 5. Trends in Employment Rates of Mothers of 4 Year Olds and Event-Study Estimates of the Effect of Introducing High-Quality Universal Preschool on Maternal Employment, by Maternal Education[a]

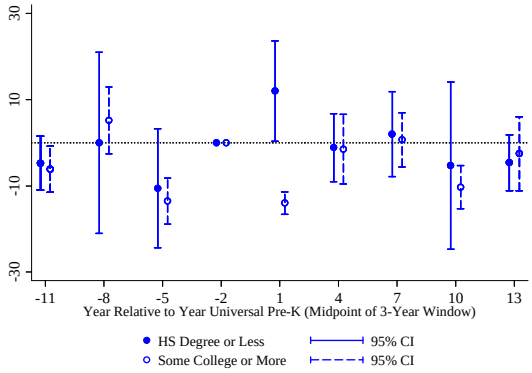
a. National Trends



b. Event-Study Estimates
(No Additional Comparison Group)



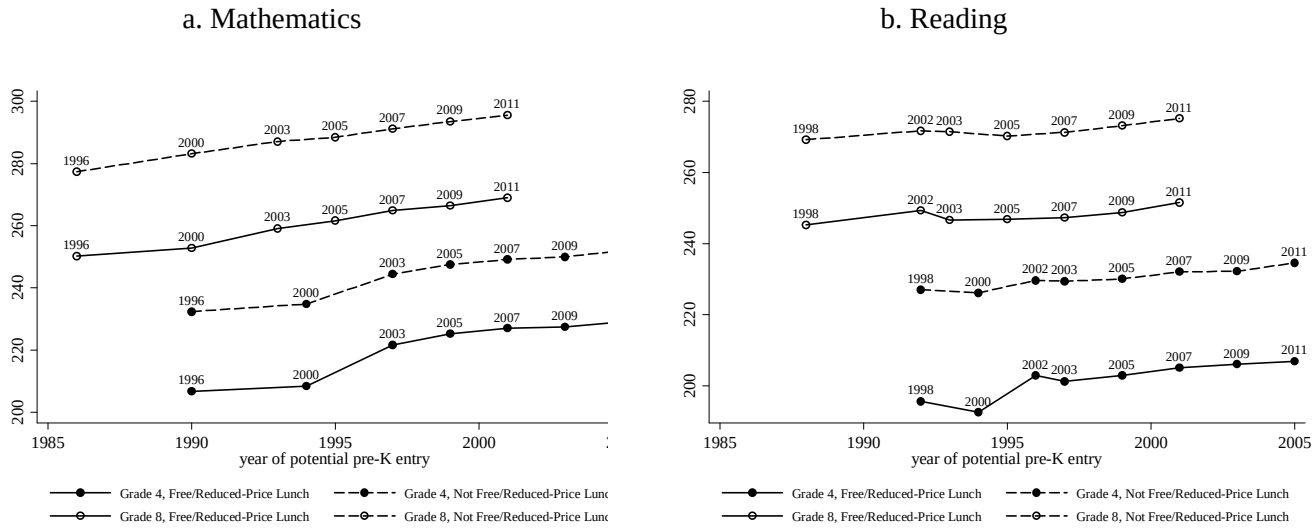
c. Event-Study Estimates
(Mothers of 5 Years Olds as Additional Comparison Group)



Source: October CPS school enrollment supplements, 1977-2011.

[a] States with high-quality preschool programs are Georgia (introduced in fall 1995) and Oklahoma (introduced in fall 1998). Regressions underlying the coefficients in panel b include state fixed effects and year fixed effects in addition to dummies for 3-year groupings of year relative to the year that high-quality universal preschool was introduced. The coefficients plotted at -11 represented 10 years or more prior to introduction, while the coefficients plotted at 13 represent 12 or more years after; the dummy at -2, representing the three years immediately prior to introduction, is omitted to identify the model. Regressions underlying the coefficients in panel c include all of the same controls, entered both directly and interacted with an indicator for whether the mother has a 4 year old. The coefficients plotted are the coefficients on the interactions with the indicators for year relative to the initiative. All regressions were weighted by cell size, and standard errors were clustered on state.

Figure 6. National Trends in Mean NAEP Scale Scores, by Grade, Subject, and Family Income[a]

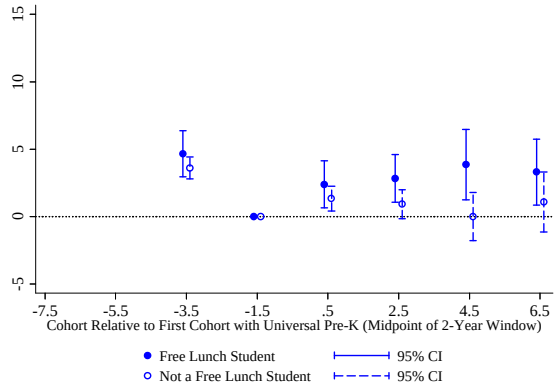


Source: Main NAEP Data Explorer (NDE).

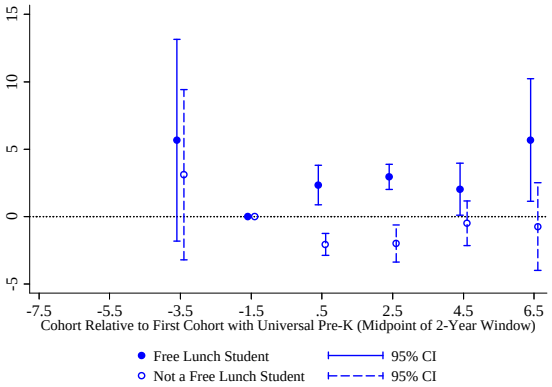
[a] Test years (spring) are given above the data points.

Figure 7. Event-Study Estimates of the Effect of Introducing High-Quality Universal Preschool on Fourth and Eighth Grade State Mean NAEP Scores, by Family Income[a]

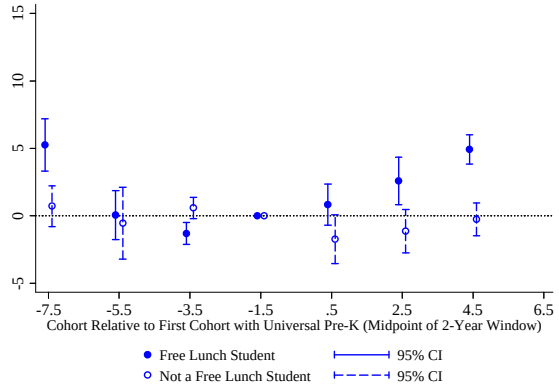
a. Grade 4 Mathematics



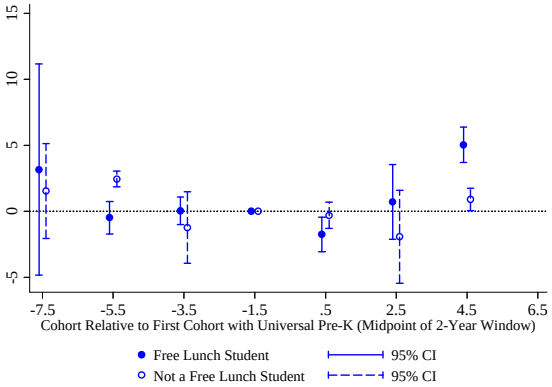
b. Grade 4 Reading



c. Grade 8 Mathematics



d. Grade 8 Reading



Source: Main NAEP Data Explorer (NDE). State mean scores by free/reduced-price lunch status in math are available for spring 2000, 2003, 2005, 2007, 2009, and 2011. Mean scores by free/reduced-price lunch status in reading are available for spring 1998, 2002, 2003, 2005, 2007, 2009, and 2011. Grade 4 (8) scores pertain to cohorts of age to enter preschool the fall 6 (10) years prior, assuming normal grade progression.

[a] States with high-quality preschool programs are Georgia (introduced in fall 1995) and Oklahoma (introduced in fall 1998). All regressions include state fixed effects and cohort fixed effects in addition to dummies for 2-year groupings of cohorts relative to the first cohort exposed to high-quality universal preschool. The first and last coefficients in each graph are on dummies set to one for all prior and later cohorts, respectively; the dummy at -1.5, representing the two cohorts immediately prior to introduction, is omitted to identify the model. Regression standard errors were clustered on state.

Table 1. Preschool Enrollment of 4 Year-Olds and Employment of Mothers of 4-Year-Olds, by Maternal Education: 1970, 1985, and 2010

	Year:			*Change:*	
	1970	*1985*	*2010*	*1970-1985*	*1985-2010*
		a. Mother Has High School Degree or Less			
Enrolled in Public Preschool	4.4	13.7	41.4	9.2	27.7
Enrolled in Private Preschool	6.4	15.4	8.4	8.9	-7.0
Enrolled in Any Preschool	10.9	29.0	49.8	18.1	20.8
Mother Employed Last Week	28.6	42.4	43.2	13.7	0.8
		b. Mother Has Some College or More			
Enrolled in Public Preschool	5.5	10.6	31.9	5.1	21.3
Enrolled in Private Preschool	30.1	41.6	32.8	11.4	-8.7
Enrolled in Any Preschool	35.6	52.2	64.7	16.6	12.5
Mother Employed Last Week	31.7	56.1	61.6	24.3	5.5

Source: Authors' calculations from the October CPS, 1969-71 (for 1970), 1984-86 (for 1985), and 2009-11 (for 2010).

Table 2. Differences-in-Differences Estimates of the Impact of High-Quality Universal Preschool on Preschool Enrollment Rates of 4-Year-Olds, by Maternal Education[a]

	Effect Sizes:		
Dependent variable (%)	*Baseline DD 2-1*	*Add Demographics 2-2*	*Add State Linear Trends 2-3*
	a. Mother Has High School Degree or Less (N=1785)		
Public Preschool Enrollment Rate	16.90***	16.49***	16.63***
	(1.391)	(1.316)	(1.031)
Private Preschool Enrollment Rate	1.584	2.171	3.555***
	(2.576)	(2.144)	(1.013)
Crowd-out ratio	*-0.09*	*-0.13*	*-0.21*
Overall Preschool Enrollment Rate	18.48***	18.66***	20.18***
	(2.385)	(2.268)	(1.381)
	b. Mother Has Some College or More (N=1784)		
Public Preschool Enrollment Rate	19.63***	19.75***	28.45***
	(3.124)	(3.264)	(2.632)
Private Preschool Enrollment Rate	-8.003	-7.783	-14.36***
	(5.175)	(5.914)	(2.751)
Crowd-out ratio	*0.41*	*0.39*	*0.50*
Overall Preschool Enrollment Rate	11.62***	11.97***	14.09***
	(2.298)	(2.849)	(0.971)
Controls:			
State Fixed Effects	Y	Y	Y
Year Fixed Effects	Y	Y	Y
Nonwhite (%)	N	Y	Y
State Unemployment Rate	N	Y	Y
Linear Trends for GA and OK	N	N	Y

Source: Authors' regressions based on state-by-year aggregates of microdata from the October CPS School Enrollment Supplements, 1977-2011.

[a] Each reported coefficient is a separate estimate of θ from model (1) in the text. See the text for details. Regressions are weighted by the number of children used to calculate the enrollment rate. Standard errors clustered on state are in parentheses. Asterisks indicate statistical significance at the ***1 percent level.

Table 3. Differences-in-Differences Estimates of the Impact of High-Quality Universal Preschool on Monthly Spending on Child Care, by Maternal Education[a]

Coefficient on:	*Baseline DD 3-1*	*Topcode Spending 3-2*	*Use Median Spending 3-3*
	a. Mother Has High School Degree or Less (N=920)		
Age4 x GA or OK	35.48***	34.94***	-1.723
	(9.428)	(9.415)	(4.145)
Age 4	7.747	8.414	0.0475
	(5.716)	(5.703)	(3.582)
GA or OK	-31.34***	-30.36***	-9.438
	(5.595)	(5.445)	(6.495)
	b. Mother Has Some College or More (N=1008)		
Age4 x GA or OK	-65.86***	-49.69**	-56.17***
	(21.66)	(19.45)	(14.12)
Age 4	70.37***	63.29***	54.39***
	(14.25)	(12.40)	(11.57)
GA or OK	-32.30**	-31.47***	4.046
	(12.87)	(10.94)	(13.90)
Additional Controls:			
Year Fixed Effects	Y	Y	Y

Source: Authors' regressions based on state-by-year-by presence of 4-year-old aggregates of microdata from the Consumer Expenditure Survey, September 1998-June 2011.

[a] Each panel and column represents a separate estimate of model (2) in the text. Monthly childcare spending is in real 2012 dollars. In column 3-2, we topcode spending at the 99th percentile before collapsing the data to state-by-year-by-age means. In column 3-3, we collapse the data to cell medians instead of cell means. Regressions are weighted by the number of children used to calculate the dependent variable. Standard errors clustered on state are in parentheses. Asterisks indicate statistical significance at the ***1 percent, **5 percent, or *10 percent level.

Table 4. Differences-in-Differences and Triple-Difference Estimates of the Impact of High-Quality Universal Preschool on Employment Rates of Mothers of 4-Year-Olds, by Maternal Education[a]

	Coefficient on post *(Model 1):*		*Coefficient on* post x age4 *(Model 3) Comparison Group is (Mothers of):*		
	Baseline	*Add Controls*	*3-Year-Olds*	*5-Year-Olds*	*3- or 5-Year-Olds*
Dependent Variable	*4-1*	*4-2*	*4-3*	*4-4*	*4-5*
		a. Mother Has High School Degree or Less			
Maternal Employment:					
% Mothers at Work Prior Week	-2.110	-1.390	2.283	4.593**	3.314
	(2.513)	(2.680)	(2.741)	(2.102)	(2.360)
Child's School Enrollment:					
Public School Enrollment Rate (%)	19.35***	18.48***	16.61***	13.90***	13.30***
	(1.550)	(1.406)	(3.356)	(3.058)	(1.412)
Private School Enrollment Rate (%)	-0.940	-0.260	-2.818***	1.461	-0.616
	(1.344)	(0.944)	(0.794)	(2.251)	(0.887)
Crowd-out ratio	*0.05*	*0.01*	*0.17*	*-0.11*	*0.05*
N (state-by-year-by-age cells)	1,785	1,785	3,570	3,570	5,355
		b. Mother Has Some College or More			
Maternal Employment:					
% Mothers at Work Prior Week	-4.138***	-3.997***	-0.419	-0.345	-0.598
	(1.394)	(1.297)	(1.449)	(2.771)	(0.913)
Child's School Enrollment:					
Public School Enrollment Rate (%)	23.38***	23.46***	21.65***	16.59*	18.05**
	(5.441)	(5.781)	(6.557)	(8.325)	(7.507)
Private School Enrollment Rate (%)	-15.06***	-14.82***	-10.68**	-6.766***	-8.633**
	(1.617)	(0.989)	(4.861)	(1.625)	(3.241)
Crowd-out ratio	*0.64*	*0.63*	*0.49*	*0.41*	*0.48*
N (state-by-year-by-age cells)	1,784	1,784	3,569	3,569	5,354
Controls:					
State Fixed Effects	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y
Nonwhite (%)	N	Y	Y	Y	Y
State Unemployment Rate	N	Y	Y	Y	Y
All controls x *age4* indicator	N	N	Y	Y	Y

Source: Authors' regressions based on state-by-year aggregates of microdata from the October Current Population Survey, 1977-2011.

[a] See the text for further details on model (1) and model (3). Regressions are weighted by the number of children used to calculate the enrollment rate. Standard errors clustered on state are in parentheses. Asterisks indicate statistical significance at the ***1 percent, **5 percent, or *10 percent level.

Table 5. Differences-in-Differences Estimates of the Impact of High-Quality Universal Preschool on Mothers' Time Use, by Maternal Education[a]

Coefficient on:	*Time spent with child present 5-1*	*Time spent caring for and/or helping child 5-2*
	a. Mother Has High School Degree or Less (N=96)	
Age4 x GA or OK	-46.18*	24.96**
	(24.65)	(11.59)
Age 4	32.74***	16.54**
	(11.80)	(6.693)
GA or OK	27.95	-15.65
	(41.25)	(10.95)
Constant	477.3***	101.6***
	(12.12)	(5.787)
	b. Mother Has Some College or More (N=102)	
Age4 x GA or OK	2.702	-7.703
	(44.41)	(7.352)
Age 4	13.29	15.51***
	(10.50)	(4.867)
GA or OK	-1.274	-7.849
	(34.52)	(6.122)
Constant	461.8***	121.5***
	(7.781)	(3.408)

Source: Authors' regressions based on state-by-maternal education-by presence of 4-year-old aggregates of microdata from the American Time Use Survey, January 2003-December 2012.

[a] Each panel and column represents a separate regression estimate. Time is measured in minutes, and the sample is limited to women in households with a 4 or 5 year old child, and to non-holiday weekdays from September through June. Regressions are weighted by the number of observations used to calculate the dependent variable. Standard errors clustered on state are in parentheses. Asterisks indicate statistical significance at the ***1 percent, **5 percent, or *10 percent level.

Table 6. Differences-in-Differences Estimates of the Impact of High-Quality Universal Preschool on Eighth Grade NAEP Mathematics and Reading Scores, by Family Income[a]

	Effect Sizes:				
Dependent variable (%)	*Baseline DD 2-1*	*Baseline + Indicator for 7+ Cohorts Pre-initiative 2-2*	*Baseline + Add State Linear Trends 2-3*	*Specification 2 + Student Demographics 2-4*	*Specification 4 + Other State Education Policies 2-5*
		a. Child Eligible for Free or Reduced-Price Lunch			
Math Scale Score	2.015	2.724*	2.455**	2.254*	2.152*
	(1.938)	(1.434)	(1.215)	(1.192)	(1.116)
N (state-years)	295	295	295	294	294
Reading Scale Score	0.193	1.014	0.155	0.897	0.818
	(2.729)	(1.912)	(1.393)	(1.867)	(1.869)
N (state-years)	334	334	334	333	333
		b. Child Not Eligible for Free or Reduced-Price Lunch			
Math Scale Score	-1.115	-1.009	-1.756***	-1.260	-1.293
	(1.033)	(0.983)	(0.360)	(0.850)	(0.834)
N (state-years)	295	295	295	294	294
Reading Scale Score	-1.078	-0.763	0.111	-0.769	-0.813
	(2.387)	(2.304)	(1.061)	(2.295)	(2.298)
N (state-years)	334	334	334	333	333
Controls:					
State Fixed Effects	Y	Y	Y	Y	Y
Cohort Fixed Effects	Y	Y	Y	Y	Y
7+ Cohorts Prior to First Affected Cohort (=1)	N	Y	N	Y	Y
Linear Trends for GA and OK	N	N	Y	N	N
Student Demographics	N	N	N	Y	Y
Years with School Accountability	N	N	N	N	Y

Source: Authors' regressions based on state-by-year data from NAEP Data Explorer.

[a] Each reported coefficient is a separate estimate of θ from model (1) in the text. See the text for details. Standard errors clustered on state are in parentheses. Asterisks indicate statistical significance at the ***1 percent, **5 percent, or *10 percent level.