Preschool Attendance and Child Health and Development: Evidence from State Pre-K Programs

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November 1, 2016

JOB MARKET PAPER

Abstract

This paper analyzes the effects of attending state pre-kindergarten programs on child health and development up to eight years after preschool age. Using data from the National Health Interview Survey, the Current Population Survey, state legislature and other sources, I implement reduced-form and two-sample two-stage least squares regression models that exploit the variation in the timing of the implementation of pre-K programs across states. The reduced-form results indicate that boys who live in states that had pre-K programs when they were 4 years old are less likely to receive special education services in the following four years, and their developmental outcomes are improved five to eight years after preschool age. I also find that boys and girls in states with pre-K programs have increased health problems in the first four years after preschool age. In the instrumental variables analysis, my first-stage estimates indicate that the implementation of pre-K programs increases preschool enrollment rates by close to 8 percentage points. When I use this result to estimate the size of treatment-on-the-treated effects of preschool attendance on child outcomes, using pre-K programs as an instrument, the results imply large effects on both developmental and health outcomes.

*Email: marianazerpa@email.arizona.edu. I would like to thank Jessamyn Schaller, Tiemen Woutersen, Gary Solon, Price Fishback and Ron Oaxaca for their comments, advice and support. Funding from the Washington Center for Equitable Growth Dissertation Grant, and from the University of Arizona GPSC Research Grant, is gratefully acknowledged. Part of the research in this paper was conducted at the National Center for Health Statistics Data Center, and I am thankful for the support provided by Pat Barnes of the NCHS. The findings and conclusions expressed are solely those of the author and do not represent the views of Equitable Growth, or the National Center for Health Statistics.

1 Introduction

Enrollment in state-funded pre-kindergarten (pre-K) programs has grown dramatically since the 1990s, making state governments as a group today's largest provider of preschool education for 4-year-olds in the United States. By the 2014-2015 school year, 42 states and the District of Columbia were offering pre-K programs for 4-year-olds, and 29% of 4-year-olds (close to 1.2 million children) attended state-funded pre-K programs, accounting for 43% of total preschool enrollment and over two-thirds of total public enrollment of 4-year-olds.[1] This is more than twice the enrollment of 4-year-olds in the federal Head Start program. The growth in pre-K enrollment was halted by the Great Recession due to limited funding, and has been slowly recovering in the last four years (NIEER, 2013, 2016). Further investments in expanding state pre-K programs are on the agenda of many states and the federal government, underscoring the need for high-quality research on the scope of benefits of preschool education.[2] In order to fully understand the costs and benefits of these proposals, we need more evidence on the scope of effects of large-scale public early education interventions.

In this paper, I study the short- and medium-run effects of pre-K education on child health and development outcomes. In particular, this paper addresses two questions that can contribute to a better understanding of the impacts of attending preschool education, and state pre-K programs in particular. First, do preschool programs have any lasting impacts on child developmental outcomes, other than those measured by test scores? This is still an open question, since studies of both state pre-K and federal Head Start programs have found positive short-run impacts on test scores that fade-out one to three years after preschool, and there has been little work studying impacts on other developmental outcomes. Second, are there any (positive or negative) impacts of attending a preschool program on physical health? The scarce literature that addresses health impacts of preschool attendance has found contradicting evidence, with some studies of Head Start finding evidence of positive effects in the medium run, and studies of child care subsidies finding some large

[1] Based on state-funded pre-K enrollment in 2014 (NIEER, 2016) and total enrollment by age and sector in 2013 (NCES, 2015).

[2] In the past two years, the federal government awarded over $463 million in Preschool Development Grants to support the development and expansion of preschool programs in 18 states (US Department of Education, 2015). President Obama's proposed *Preschool for All Initiative* would provide $75 billion in funding over the next ten years in a federal-state partnership aimed at providing access to preschool education to all 4-year-olds from low- and moderate-income families (White House, 2016).

negative effects on child health status and the incidence of illness in the short run.

There are several potential channels for a causal effect of attending preschool on child health and development. First, preschool programs are designed to prepare children for school, encouraging the development of cognitive and non-cognitive skills, potentially having an impact on child development. Second, preschool may teach children and their parents healthy habits, improve access to health checkups and preventive care, and improve access to health information for parents. Preschool programs often offer parental support services that may positively affect the quality of time that parents spend with children, and the investments that parents make in the child's health. Third, free or reduced-price preschool access may improve labor outcomes for parents (especially mothers), which can increase family income and health investments. However, an increase in the labor market participation of mothers could also have a negative effect on child health if this reduces the quantity and/or quality of time that the mother spends with the child. An earlier enrollment in school may also increase the child's direct exposure to illness through contact with other children. This could potentially have a negative effect in the short-run, though it is not clear how this would affect longer-term health outcomes.

Assessing the effects of the introduction of state-funded pre-K programs for 4-year-olds on health and development outcomes throughout childhood poses two main challenges. First, simply comparing the health outcomes of children who did and did not attend a preschool program is likely to result in biased estimates of the effects of preschool education, because the families of the two groups are likely to differ in attributes that may be related to child health. The second challenge is that very few large and publicly available datasets have information about both child health at different ages and preschool attendance, and those that do have a very limited set of health outcomes. Furthermore, to the best of my knowledge there is currently no available individual-level national data source that includes information on whether a child attends or has attended a state-funded pre-K program.

I overcome these challenges by collecting information on the timing of the introduction of state pre-K programs in 15 states between 1997 and 2005, and combining this information with individual-level data from two different national surveys, to provide evidence on the effects of the introduction of pre-K programs on a wide set of health and development outcomes, up to eight years after preschool age. I exploit the variation in the timing of the introduction of these programs across states as an exogenous source of variation in the access of 4-year-olds to pre-K programs, while controlling for permanent differences in child outcomes across states, changes over time at the national level, individual demographic characteristics, and time-varying characteristics of each state.

I estimate the reduced-form (*intent-to-treat*) effects of the implementation of pre-K programs on child development and health outcomes, using data from the National Health Interview Survey (NHIS) for years 1998 to 2014, on the cohorts of children who were 4 years old between 1997 and 2005. I find that for boys, pre-K programs reduce the likelihood of receiving special education services during the first four years after pre-K, and have a significant negative effect on an index of developmental problems 5 to 8 years after pre-K. This suggests the existence of beneficial effects of pre-K programs on boys' developmental outcomes that persist as late as eight years after preschool age. However, I don't find similar beneficial effects on the developmental outcomes of girls. The reduced-form effects on health outcomes suggest that pre-K programs are associated with worse health, as measured by a summary index of health problems during the first four years after pre-K, also increasing the number of days of school missed for being sick. These results are robust to changes in the specification, control variables and states included in the sample. The effects on reported health are not correlated with changes in access to health care, and there is no evidence of effects on hospitalizations or asthma-related emergency room visits.

Using data from the Current Population Survey on 4-year-olds between 1997 and 2005, I estimate the first-stage effects on enrollment, showing that the implementation of these pre-K programs increases preschool enrollment by close to 8 percentage points, with no statistically significant difference across genders. Because these programs affect not only children who would otherwise not have attended any preschool, but also children that are crowded out from other preschool programs, this estimate likely constitutes a lower bound for the proportion of children affected by these policies. Although I cannot estimate the size of this "crowding-out" effect, I provide evidence that suggests that this effect was not trivial. Finally, I combine the reduced-form effects on child outcomes and the first-stage effects on preschool attendance to provide an estimate of the *treatment-on-the-treated* effect of attending preschool on child health, using a two-sample two-stage least squares (TS-2SLS) strategy with the pre-K policy variable as an instrument for preschool attendance. These estimates imply very large effects on the treated, but because the first-stage provides a lower-bound for the proportion of four-year-olds affected by the policy, the TS-2SLS estimates provide an upper bound for the size of the effects on program participants.

This paper sheds light on the potential of state-funded pre-K programs for improving child developmental outcomes throughout childhood. My findings of improved developmental outcomes for boys up to eight years after preschool age complement previous findings on short-term impacts of pre-K on test scores, as well as research on Head Start and other preschool programs. The results also suggest that increased access to preschool education may increase the incidence of illness in the short-run, similar to what has been found for increased access to child care subsidies. While the estimates imply large effects, they don't seem to reflect serious conditions, as there are no effects on hospitalizations or emergency-room visits related to asthma episodes. Additionally, I find no significant effects on health past four years after preschool.

The paper proceeds as follows. Section 2 discusses related literature and provides background on the state pre-K programs considered in the paper. Section 3 outlines the empirical strategy based on the introduction of pre-K programs in different states and years, and presents the data and outcome variables used. Section 4 presents the estimates of the reduced-form effects of the introduction of pre-K programs, it discusses the robustness of the main results, and presents results of the first stage effects on preschool attendance and instrumental variables estimates of the effects of attending preschool education on health and development outcomes. Section 5 concludes.

2 Background

2.1 Related Literature

This paper contributes to the literature on the effects of attending preschool on child outcomes. A central topic in the discussion about preschool education has been whether impacts fade out over time. This debate was ignited by studies of Head Start that show positive effects on cognitive skills immediately after preschool that fade out during the following years, at least for some groups of children (DHHS, 2010; Currie and Thomas, 1995 and 1999, Deming, 2009). This debate has resurfaced in light of recent evaluations of the effects of some specific state-funded pre-K programs on academic achievement. While evaluations of short-run effects have mostly found positive effects on school readiness test scores (Gormley and Gayer, 2005; Gormley, Phillips and Gayer, 2008;

Wong et al., 2008), the few papers that have evaluated the effects of these programs on test scores some years after preschool age have found mixed results on the persistence of early positive impacts (Lipsey, Farran, and Hofer, 2015; Cascio and Schanzenbach, 2013; Fitzpatrick, 2008; Hill, Gormley and Adelstein, 2015).[3]

Despite the fade-out of the effects on test scores, comparisons of siblings that attended and did not attend Head Start show long-term improvements in educational attainment, earnings, crime, and self-reported health (Garces, Thomas and Currie, 2002; Deming, 2009). Furthermore, randomized controlled trials of early education programs such as the Carolina Abecedarian and Perry Preschool Projects have found that these small, high-quality interventions had long-run positive effects on outcomes such as educational attainment and earnings (Currie, 2001). The contrast between the medium-run effects of preschool programs on test scores and the long-run effects on income and other important outcomes raises the question of whether preschool programs affect other short- and medium-run outcomes that might explain the long-run impacts. In particular, there is very little research evaluating the effects of preschool education on child health, and on development outcomes not measured by test scores, such as special education placement, learning disabilities, and behavioral problems.

Studies of the impact of Head Start have found evidence of some positive health effects, but they have looked at a limited set of health indicators at specific ages. The Head Start Impact Study (DHHS, 2010) found positive impacts on reported health status and health insurance coverage during Kindergarten that fade out by first grade. Ludwig and Miller (2007) show that Head Start reduced childhood mortality during its implementation in the 1960s, which is likely explained by the increased access to immunizations that the program provided (Currie and Thomas, 1995). For more recent cohorts, Carneiro and Ginja (2014) find that participation in Head Start causes improvements in mental health screenings, reductions in obesity prevalence, and some indication

[3] Lipsey, Farran, and Hofer (2015) present results for the first randomized controlled trial of a scaled up, state-funded pre-K program, the Tennessee Voluntary Prekindergarten Program. The study finds positive effects on achievement tests at the end of pre-K, but the differences with the control group fade out by the end of kindergarten. Using difference-in-difference strategies, Cascio and Schanzenbach (2013) find that the introduction universal pre-K programs in Georgia and Oklahoma had positive effects on children's test scores as late as eighth grade, for children with low-education mothers, while Fitzpatrick (2008) finds positive effects of Georgia's universal pre-K program in fourth grade only for disadvantaged children living in rural areas. Hill, Gormley and Adelstein (2015) study two cohorts of Tulsa's pre-K program using propensity score matching, and find evidence of persistence of early gains in test scores through the third-grade of school only for one of the cohorts, in math but not reading, and for boys but not for girls.

of reductions in disability, but they find no significant effects on reported health limitations, health status, or risky behaviors, for boys at ages 12-13 and 16-17. In terms of developmental and cognitive outcomes other than test scores, Deming (2009) finds that participation in Head Start reduces grade retention and learning disability diagnosis for children ages 7 to 14, compared to their siblings that did not attend Head Start.

A related literature has looked at the impacts of child care subsidies and maternal employment, finding some negative short-run effects on child health and behavioral outcomes. For example, Baker, Gruber and Milligan (2008) find that a large-scale child care subsidy in Quebec, Canada had negative impacts on child health and behavioral outcomes. However, it is hard to know whether these findings are applicable to pre-K programs, as child care subsidies differ from preschool programs in that they usually subsidize any form of childcare, affect children since younger ages, and have large impacts on maternal labor supply, while not necessarily promoting access to high-quality early education programs. The literature on the effects of maternal employment has found small effects on child health overall, but larger negative effects for children in high socio-economic status families (Anderson et al., 2003; Gennetian et al., 2010; Ruhm, 2000, 2008; Morrill, 2011).

Even though there is evidence of increased infections at the onset of center-based childcare before age 2 (Côté et al., 2010; Miller, Gruber and Milligan, 2008), it is not clear that increasing preschool attendance at age 4 should increase the risk of infections, as children may already be exposed to contagious illnesses at an earlier age from other child-care arrangements and family members. If preschool attendance does increase the prevalence of infectious illnesses, this may be protective of later health. Epidemiological studies have found some support for the *hygiene hypothesis*, which states that increased exposure to certain types of infections early in life might have a protective effect against the development of asthma, allergic diseases, and viral respiratory infections (Ball et al., 2002; Illi et al., 2001; Côté et al., 2010). However, the evidence of a protective effect is mostly based on exposure in the first two to three years of life, and exposure may have negative effects when a disease has already established (Liu and Murphy, 2003).

This paper makes three main contributions to this literature. First, it contributes to a better understanding of the impacts of state-funded pre-K programs on child development and health outcomes, which have not been studied before for this type of programs even though they are currently the largest provider of preschool education in the U.S. Second, by providing evidence on the effects of pre-K programs on developmental outcomes throughout childhood, it can help us build a bridge between the conflicting evidence of fading-out short-run effects of preschool education on test scores, and the positive long-lasting effects on adult outcomes of Head Start and other experimental programs. Third, it contributes to the still scarce literature on the effects of preschool attendance on child health, providing evidence on effects on health status and incidence of illness from 1 to 8 years after preschool age.

2.2 Background on State-Funded Pre-K Programs

The programs studied in this paper are state-funded pre-kindergarten programs for 4-year-olds that were first implemented or scaled up between 1997 and 2005, and that are qualified as state preschool programs by the National Institute for Early Education Research (NIEER). An initiative is considered to be a state preschool program by NIEER if it meets the following criteria: a) the initiative is funded, controlled, and directed by the state; b) it serves 3- and/or 4-year-old children; c) early childhood education is the primary focus of the initiative; d) it offers a group learning experience at least two days per week; e) it is distinct from the state's system for subsidized child care; f) the initiative is not primarily designed to serve children with disabilities, although it may include children with disabilities; and g) state supplements to Head Start are considered to constitute state preschool programs if they substantially expand the number of children served and the state assumed some administrative responsibility for the program.

Figure 1 shows a map of the U.S. with the states that implemented pre-K programs in this period (*treatment states*) in red, and states without programs (*control states*) in yellow. The states that implemented these programs, defined hereafter as **treatment states**, are: Arkansas, Florida, Kansas, Louisiana, Missouri, Nebraska, New Jersey, New Mexico, New York, North Carolina, Oklahoma, Pennsylvania, Tennessee, Vermont, and West Virginia. My control group are children from states that by 2005 had not yet implemented a state-wide pre-K policy, or which only had a very small scale pre-existing program whose impacts are unlikely to be observed at a state level. These states, denominated **control states** hereafter, are: Alabama, Alaska, Arizona, Hawaii, Idaho, Indiana, Iowa, Minnesota, Mississippi, Montana, Nevada, New Hampshire, North Dakota, Rhode Island, South Dakota, Utah, Washington, and Wyoming. Most of these states (12 out of 18) did not have a state preschool program by 2005, and those that had a state preschool program had stable enrollment rates of at most 6% of 4-year-olds by 2005.[4]

The remaining states and the District of Columbia are excluded from the main analysis because they had pre-existing programs with enrollment rates of 10% or more during the period, and/or had a program with significant variations in enrollment during the period (**excluded states** hereafter). The reason for excluding these states from the main analysis is that they can have changes in enrollment and/or funding during the period that would make them an inadequate counterfactual for the changes in outcomes that would have happened in treatment states in the absence of the implementation of a program. These changes in enrollment may or may not be confounding with the implementation of programs in the treatment states, but they would at least introduce noise in the estimation. However, in the robustness analysis I show that my main results are not sensitive to the inclusion or exclusion of any individual state from the sample, nor are they sensitive to including all excluded states in the control group.

Table 1 presents a summary of the characteristics of the programs implemented in the treatment states as of 2005 (NIEER, 2006). There is a combination of full-day (5 to 7.5 hours/day) and half-day (2.5 to 3 hours/day) programs, and some states offer both types of programs depending on the decision of the school district. Full-day programs offer at least lunch and a snack, while half-day programs usually offer a snack. The heterogeneity in hours-per-day served is mirrored by a similar heterogeneity in total spending per student (including all sources of funding): the average spending in states that offer only full-day programs is $6,118, compared to $2,929 in states that only offer half-day programs. The average spending per student across all treatment states is $4,848.

All but two of the states require providers to follow comprehensive early education standards (Kansas and New York adopted comprehensive early education standards after 2005), and over 70% of the states require programs to offer basic health screenings, referral and support services. There is more variation in the quality of the programs in terms of class size and staff-child ratios, and teacher and assistant teacher degree and specialization requirements, which is reflected in the variation in the program scores assigned by NIEER,[5] which range from a minimum of 3 to a

[4]Enrollment rates are available only since 2001, collected by NIEER (2006). Appendix Table A6 shows enrollment rates for each treatment state for years 2001-2005.

[5]This score is the count of the number of benchmarks met by the program, out of a total of 10. The benchmarks are the following: comprehensive early learning standards; teachers have a BA degree; teachers are specialized in pre-K education; assistant teachers have CDA degree or equivalent; teacher in-service at least 15 hours/year; class sizes of 20 or lower; staff-child ratio of 1:10 or better; screening/referral for vision, hearing and health, and at least 1 support service; at least 1 meal a day; and monitoring site visits.

maximum of 10 out of 10, with a median of 7. Within this group of programs, NIEER's score has a correlation coefficient of .59 with spending per student.

Five of the states (Florida, New York, Oklahoma, Vermont, and West Virginia) offer voluntary universal pre-K programs, although not all of them were at the time sufficiently funded to meet demand. Enrollment rates of 4-year-olds in these universal programs range from 29% in New York to 70% in Oklahoma. The rest of the states offer programs targeted towards children from low-income families or who have other risk factors (such as having a disability, being homeless or in foster care, and being an English language learner). Some states use income thresholds to determine eligibility of individual children, while other states determine eligibility of a school or provider by requiring a minimum percentage of children served the school or program to be below the income threshold. The most commonly used income threshold is 185% of the federal poverty line (FPL), which determines eligibility for reduced-price lunch in schools. There are two state programs that use the same income threshold as Head Start (100% of the FPL), while all others use higher cutoffs. Some programs are allowed to serve children that don't meet the eligibility criteria by charging tuition or sliding fees. However, in many cases the eligible population is underserved due to funding limitations, something that happens with Head Start as well. Enrollment rates in targeted pre-K programs range from 4% to 22% of 4-year-olds in the state. The average enrollment rate across all programs in treatment states is 23%.

3 Empirical Strategy and Data

3.1 Reduced-Form Effects of State Pre-K Expansions

I take advantage of large increases in the supply of preschool education caused by the introduction of state pre-K programs to identify its effect on short- and medium-term outcomes of children. Between 1997 and 2005, 15 states implemented or substantially expanded state-funded pre-K education programs for four-year-olds. I evaluate the reduced-form (*intent-to-treat*) effects of these preschool education supply expansions using individual-level data on child health from the National Health Interview Survey (NHIS), supplemented with state-level information on the implementation of pre-K policies and other state characteristics and policy variables. I estimate regressions that take the form of a generalized difference-in-difference specification with state and cohort fixed effects:

$$Y_{isc}^a = \beta_{RF}^a \text{Post\_Pre-K}_{sc} + \gamma^a X_{isc}^a + \delta_c^a + \delta_s^a + \varepsilon_{isc}^a \quad (1)$$

The superscript a indicates that the model is separately estimated for child outcomes evaluated at different number of years after preschool age. In my main estimations I group children in two age groups: 1-4 and 5-8 years after pre-K. The subscript i represents a child, c is the child's pre-K cohort (the *reference year* for attending preschool, i.e. the year when the child was 4 by October), and s is the state where the child lives.[6] Post_Pre-K_{sc} is an indicator variable for whether state s had implemented a pre-K program by year c. X_{isc}^a is a vector of control variables whose components vary across specifications, but in the more general case includes individual time-invariant characteristics of the child and her family (the child's gender, race/ethnicity, and mother's educational attainment), and state policy and economic control variables that vary by state and cohort, including state characteristics when the child was 4 (year c) and current characteristics in the year when the child outcomes are observed.[7] In the main specification, which groups children into two age groups (1-4 and 5-8 years after pre-K), I include dummies for the number of years after pre-K age that the child is observed, to control for differences in the outcomes across specific ages within an age group.[8] State fixed effect, δ_s^a, control for unobserved differences across states (e.g. permanent differences in the quality of health care or education), and cohort fixed effects, δ_c^a, control for any unobserved changes across cohorts that are common to all states (e.g. general changes in female labor supply, parents' valuation of preschool education, national changes in health outcomes).

Identification relies on the assumption that the timing of the implementation of state pre-K programs is not correlated with other factors that may affect child health. In Section 4.2 I present evidence showing that the implementation of state-funded pre-K programs is not correlated with other state-level policies that may affect child health and development. I also discuss the sensitivity

[6] I make the assumption that the state where the child currently lives is the same as the state where the child was living at age 4, because the latter is not observed.

[7] For a complete list of state-level control variables, see Section3.3.

[8] Alternatively, I also estimate separate regressions for outcomes observed in each individual year after pre-K age. Since in this case the regressions are estimated separately for each year after pre-K, the year when an outcome is observed is a linear function of the year when the child was 4 years old. Thus, the cohort fixed effects control for changes not only across birth cohorts but also across the years when outcomes are observed.

of the results to the inclusion of different sets of state-level control variables, adding state-specific linear time trends, and alternative choices of the states included in the control group. The time frame of the study begins in 1997 to avoid the potentially confounding effects of welfare reform.[9]

3.2 Preschool Attendance and *Treatment-on-the-Treated* Effects

Using an analogous identification strategy, I estimate the first-stage average impact of the introduction of this group of pre-K programs on preschool attendance of 4-year-olds. Because the NHIS does not have information on preschool attendance, I use repeated cross-sectional samples of 4-year-olds from Current Population Survey (CPS) October Supplement, also augmented with state-level data. I estimate the following regression:

$$Preschool_{isc} = \beta_{FS}\text{Post\_Pre-K}_{sc} + \pi X_{isc} + \lambda_c + \lambda_s + \nu_{isc} \tag{2}$$

where $Preschool_{isc}$ is a variable that indicates whether the child attended a preschool program at age 4, X^a_{isc} is a vector of individual and state-level control variables, and λ_c and λ_s are cohort and state fixed effects.

I use a Two-Sample Two-Stage Least Squares strategy (TS-2SLS) in order to estimate *treatment-on-the-treated* effects, i.e. the magnitude of the effect of attending pre-K at age 4 on the compliers– the children who attended preschool because there was a pre-K program implemented in their state. In this strategy I use Post_Pre-K_{sc}, the pre-K policy indicator, as an instrument for the endogenous regressor $Preschool_{isc}$. Because the information about preschool attendance is not available in the NHIS, the TS-2SLS strategy consists of using the estimated coefficients from the first-stage regression, along with data from the NHIS on the same variables used as explanatory variables in the first stage, to predict preschool attendance for the children in the NHIS sample, and then regress health outcomes at different ages on predicted preschool attendance at age 4. In this model I only include control variables that are constant over time, as well as cohort and state

[9]Between 1993 and 1996, 43 states received welfare waivers to requirements of the Aid to Families with Dependent Children (AFDC) program, as a first stage of the welfare reform (DHHS, 1997). In 1996 the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) instituted the Temporary Assistance for Needy Families (TANF) program, which replaced AFDC and became effective in July, 1997.

fixed effects, because all control variables must be the same in both stages of the model:

$$Preschool_{isc} = \beta_{FS}\text{Post\_Pre-K}_{sc} + \pi X_{isc} + \lambda_c + \lambda_s + \nu_{isc} \tag{3}$$

$$Y^a_{isc} = \beta^a Preschool_{isc} + \gamma^a X_{isc} + \delta^a_c + \delta^a_s + \varepsilon_{isc} \tag{4}$$

Because the model is exactly identified, the TS-2SLS estimator $\hat{\beta}_{TS2SLS}$ is the ratio of the reduced-form estimator ($\hat{\beta}_{RF}$) and the first-stage estimator ($\hat{\beta}_{FS}$):

$$\hat{\beta}_{TS2SLS} = \frac{\hat{\beta}_{RF}}{\hat{\beta}_{FS}} \tag{5}$$

I compute the first-stage estimates using CPS data and the reduced-form estimates using NHIS data, separately for each age group. Following Dee and Evans (2003), I compute standard errors using the delta method, assuming there is zero covariance between the first-stage and reduced form-estimates. Under this assumption, the delta method implies that the standard errors of the TS-2SLS estimator can be approximated by:

$$\hat{se}_{\hat{\beta}_{TS2SLS}} = \sqrt{(\hat{\beta}^2_{RF}/\hat{\beta}^2_{FS}) * [(\hat{se}_{\hat{\beta}_{FS}}/\hat{\beta}_{FS})^2 + (\hat{se}_{\hat{\beta}_{RF}}/\hat{\beta}_{RF})^2]}$$

Here the identification assumption requires, besides the exogeneity of the instrument, that the expansions of pre-K programs only affect outcomes through an increase in preschool attendance. A potential threat to this assumption is that it could be possible that at least part of the increased state-funded supply crowds-out enrollment in other preschool programs. Kline and Walters (2016) show evidence of this for Head Start, finding that about one third of Head Start participants in the Head Start Impact Study are drawn from other forms of preschool. If there is crowding out from other preschool programs, we would observe an increase in total preschool enrollment that is smaller than the actual increase in state-funded pre-K enrollment. Crowding-out can be a more important concern in states that implement universal programs than in states with targeted programs, because the latter target a population that is underserved by the previously available suppliers. Whether or not this introduces a bias to the TS-2SLS estimates depends on whether switching from other sources of center-based child care to a pre-K program has any effect on outcomes or not. If attending a pre-K program has a similar causal effect on children who would otherwise attend some other type of center-based preschool program as the effect it has on children who would not, then $\hat{\beta}_{TS2SLS}$ will overestimate the magnitude of the causal effect of preschool attendance. With this caveat in mind, I interpret the TS-2SLS estimates as an upper bound for the size of the *treatment-on-the-treated* effects.

3.3 Data Sources

National Health Interview Survey (NHIS) The reduced-form analysis and the second stage of the TS-2SLS analysis are conducted using repeated cross-sectional data from the National Health Interview Survey (NHIS) from 1997 to 2014. This part of the empirical analysis was conducted at a National Center for Health Statistics (NCHS) Research Data Center because the state of residence is restricted access information.

The NHIS sample includes children in the "Sample Child" files, supplemented with information for the same sample from the "Person Level" files, for children of ages 5 to 12. More precisely, I impute the year in which each child would have been eligible for pre-K (year when they would have been 4 years old by October) using the information on the month and year of birth, and I keep in my sample the children who are observed between 1 and 8 years after pre-K age. The number of years since they were 4 years old determines the age group a to which they belong. The full sample of children whose age is determined to correspond to 1 to 8 years after pre-K has 38,668 observations. The main estimation sample consists of children who live in treatment and control states, and I also use data from the rest of the states for robustness checks. After dropping the observations in excluded states and children with missing data in the main outcome variables and individual controls, the main sample has 17,941 observations.

I construct various outcome variables that are described in the next section using data from both the "Sample Child" and "Person Level" files. I also use a set of control variables, including the child's gender, race/ethnicity, mother's educational attainment, and mother's marital status. The control variables I use in my estimations are characteristics that do not typically change over time, so the current values can be assumed to be same as they were when the child was 4 years old.[10] Summary statistics for the NHIS data are reported in Table 2.

[10] I only use the mother's marital status in robustness tests and not in the main analysis because it is potentially endogenous, and because the current marital status is not necessarily the same as when the child was age 4.

Current Population Survey (CPS) October Supplement I use individual-level CPS October data from 1997 to 2005 to evaluate the effect of the implementation of pre-K programs on preschool attendance of 4-year-olds. My main CPS sample consists of all children who are four years old in October and live in treatment and control states. I also construct an extended sample that includes four-year-olds from the 50 states and the District of Columbia. The full sample has 15,541 observations, while the main sample (after dropping observations from excluded states) has 8,880 observations.

The CPS October Supplement contains information on attendance to preschool, which comes from two questions from the October questionnaire. First, the CPS asks respondents whether children age 3 and older attend school. Second, it asks which grade they are attending: nursery (preschool or pre-kindergarten), kindergarten, or grades 1 to 12. I code a child as attending preschool if she is reported to be attending school at the nursery level. Table 3 shows summary statistics for the CPS sample. 60.4% of the main sample of 4-year-olds are attending preschool at the time of the survey. Those not attending preschool are either not attending school (33.2%) or are already enrolled in kindergarten (6.4%).[11] I also use CPS data on the same individual-level control variables as in the NHIS.

State-level information I supplement the NHIS and CPS data with state-level data collected from various sources. The primary source of information on the availability and characteristics of state-funded pre-K programs between 1997 and 2005 are NIEER State of Preschool reports.[12] After identifying the existing programs, I establish the school year in which each initiative is effectively established or expanded as the school year in which funding is allocated, based on the corresponding state legislation collected through the Education Commission of the States (ECS) State Policy Database (ECS, 2015) and the states' legislature online databases. This information,

[11] Magnuson, Meyers and Waldfogel (2007) compare the 1999 CPS measure of school attendance of 3- and 4-year-olds to the more detailed data on child care arrangements from other surveys (NHES 1999, the ECLS-K 1998, and NSAF 1999), and their findings indicate that the measure of school enrollment in the October CPS is similar to measures in other studies that include center-based care, Head Start, nursery school, and pre-kindergarten. From this comparison it seems that parents do not identify informal child care and family day care as 'school', even if the latter is a licensed child care provider. Therefore, in this paper the alternative to preschool attendance includes being cared for at home or through informal child care arrangements, attending family day care, or enrolling in kindergarten.

[12] NIEER began collecting and reporting information on state-funded pre-K programs in 2003, with data corresponding to the 2001-2002 school year, but each report includes background information about the programs described, including brief information about the history of program or significant recent changes.

together with the information in the CPS and NHIS about each sample child's state of residence and month and year of birth, is used for the construction of the variable that indicates whether the child was 4 years old after a pre-K policy had been implemented in the state where she lives (*Post Pre-K*).

I use various sources of information to construct control variables at the state level. I obtain the annual average state unemployment rate from the Bureau of Labor Statistics, and the state median household income from the US Census Bureau. I collect the state family-income-to-poverty ratio requirement for eligibility for Medicaid or SCHIP (whichever is lowest) for children ages 1-5 and 6-15, from National Governors Association *MCH Updates* (1997-2011), and Kaiser Commission on Medicaid and the Uninsured *50 State Updates on Eligibility Rules, Enrollment and Renewal Procedures, and Cost-Sharing Practices in Medicaid and SCHIP* (2006-2014).

I compute the federally-funded enrollment of 4-year-olds in Head Start as a percentage of the state's population of 4-year-olds in the following way. First, I compute the number of federally-funded enrollment of 4-year-olds by multiplying the total federally-funded enrollment (number of children) by the percentage of actual enrollment that corresponds to 4-year-olds, both obtained from Head Start Program Information Reports 1997-2005, from the Office of Head Start, US Department of Health and Human Services. I then divide this number by the population of 4-year-olds by state in 2000, obtained from the US Census Bureau.

I also explore the robustness of the main results of the paper to controlling for the estimated percent of 4-year-olds in the state served by the Child Care Development Fund,[13] which I don't include in the main regressions because this information is not available for all years. I use data from the Office of Child Care, U.S. Department of Health and Human Services on the number of children served (monthly average by state) from 1998 to 2005, and the percentage of served children by age from 2002 to 2005. For the years 2002-2005 I compute the percentage of 4-year-olds in each state served by the CCDF, by multiplying the total number of children served by the percentage of children served who were 4-year-olds, and then dividing this number by the population of 4-year-olds in each state in the year 2000 (from the US Census Bureau data). For the years 1998-2001 I follow a similar procedure but, since the percentage of children served by age is not available, I use

[13]CCDF provides child care subsidies to low-income families, and it is implemented by states through a federal block-grant. For more details see Section 4.2.2.

the average percentage by age in the years 2002-2004.

The state-level variables are merged to the CPS dataset by state and year, for the years 1997 to 2005, and they are merged to the NHIS dataset in two different ways. First, I merge the following state-level variables for the years 1997 to 2005 using the year when the child was 4 (*reference year*): annual average state unemployment rate; state median household income; state family income to poverty ratio requirement for eligibility for Medicaid or SCHIP for children between 1 and 5 years old; federally funded enrollment of 4-year-olds in Head Start; and percentage of 4-year-olds served by the CCDF (1998-2005). Second, I merge the following variables for the years 1998 to 2014, using the year when the child health outcomes are observed (i.e. at the current age): annual average state unemployment rate; state median household income; and state family income to poverty ratio requirement for eligibility for Medicaid or SCHIP for children between 6 and 15 years old.

3.4 Outcome Variables

The outcome variables analyzed in this paper can be grouped into two categories: child development and behavioral outcomes, on one hand, and general and physical health outcomes, on the other. Additionally, I complement the analysis of these outcomes by looking at health care utilization and insurance outcomes. Summary statistics of all outcome variables for treatment, control and excluded states are presented in Table 2.

The rich information available in the NHIS allows me to explore the impacts of pre-K on a large set of child outcomes, but this can come at the cost of a multiple inference problem: as the number of outcome variables grows, so does the probability of incorrectly rejecting a true null hypothesis of no causal effects. To reduce the scope of this problem and improve statistical power, I summarize the information of multiple outcome variables into two summary indices, following Anderson (2008).[14] I construct a development problems index and a health problems index. Each index is a weighted sum of z-scores of its component outcome variables. The z-scores are calculated by subtracting each outcome's control group mean, where the control group are children in control states, and dividing by its standard deviation. For all z-scores, a higher value indicates worse outcomes. To compute each index I average the z-scores using the inverse of their variance covariance matrix as weights. Weighting the components this way makes a more efficient use of the information than a simple

[14] Recent applications of this method include Carneiro and Ginja (2014) and Deming (2009).

average, as outcomes that are highly correlated and thus represent similar information are given less weight. Finally, I standardize each index again so that it has mean 0 and standard deviation 1 for the control group for easier interpretation.

I show results for two outcomes related to development and behavioral problems: special education placement and a development problems index. The first one is an indicator for whether the child is receiving special education or early intervention services (7.7% of the full sample). If pre-K education improves developmental outcomes and/or reduces behavioral and mental health problems, it can reduce the need for special education placement or the amount of time a child needs these services. The development problems index uses information on four outcome variables that address specific conditions that may affect learning: learning disability diagnosis, Attention Deficit/Hyperactivity Disorder (ADHD) diagnosis, limitations caused by a speech problem, and limitations caused by a behavioral problem.[15] The indicators for learning disability and ADHD are based on questions that ask the survey respondent whether they have ever been told that the child has a learning disability (7.6% of the full sample) and ADHD (7.5%), respectively. Given that they indicate whether the child has *ever* received a diagnosis, they are both weakly increasing with age. I interpret the results for these outcomes with caution, as they depend on the condition being diagnosed, and thus may be sensitive to health care utilization and may suffer from under- or over-diagnosis for specific groups.[16] The last two outcomes indicate whether the child has any limitation, when the respondent said that this limitation is caused by a speech problem (2.2% of the full sample), and a mental, emotional or behavioral problem (1.5%), respectively. All of these diagnoses and limitations, as well as special education placement, are observed around twice as often for boys as for girls.

I look at three outcome variables related to general and physical health: poor/fair reported health status, an index of health problems, and number of missed school days. The overall health

[15] Within children 3 to 21 years old served under Individuals with Disabilities Education Act (IDEA), the largest category of disability in the 2005-2006 school year was *specific learning disability* (41%), followed by *speech or language impairments* (22%), and *other health impairments*, *mental retardation*, and *emotional disturbance* (each in the order of 7-8%) (Snyder and Dillow, 2010). Even though ADHD is one of the most important mental health problems for children and it can increase the risk of academic difficulties (Currie and Stabile, 2006), it does not have its own specific category of disability. It can be classified under *other health impairments* if the child's educational performance is affected, although children with ADHD may also have be classified under the learning disability category based on another condition (Cuellar, 2015).

[16] In particular, there is large heterogeneity in the diagnosis of ADHD across ethnic and racial groups, although it is not clear whether these differences are due to differential prevalence rates. Additionally, Evans, Morrill, and Parente (2010) show that age relative to peers in class directly affects a child's probability of being diagnosed with ADHD.

status is reported by the survey respondent using a scale from 1–excellent to 5–poor (with a mean score of 1.63 for the full sample across all ages). "Missed school days" indicates the number of days of school missed due to illness or injury in the past 12 months. Children in the full sample missed an average of 3.2 days of school. Preschool education may affect health outcomes primarily through an earlier exposure to infectious illnesses, such as ear or gastrointestinal infections, which in turn could have positive or negative effects on later health, on conditions such as allergies and asthma. Most pre-K programs include health screenings and referrals, which could lead to an earlier detection of health problems, and many provide parental services that could improve access to health information for parents. Finally, if pre-K programs improve school readiness, they could reduce the incidence of conditions related to stress in children, such as frequent headaches. In light of these different potential channels, I construct the health problems index from four specific health conditions: 3+ ear infections, asthma episode, frequent headaches, and frequent diarrhea. "3+ ear infections" is a binary variable that indicates whether the child experienced 3 or more year infections in the past 12 months. This variable has a mean of 4.6% of the full sample, and it is decreasing with age. "Asthma episode" indicates whether the child had an asthma episode in the last 12 months (6.2% of the full sample). "Frequent headaches" indicates whether the child had frequent headaches/migraines in the past 12 months. This variable has a mean of 5.3% of the full sample, and it is increasing with age. "Frequent diarrhea" indicates whether the child had frequent diarrhea/colitis in the past 12 months (1.1% of the full sample).

In order to determine whether the health and development changes are associated with changes in health care, I complement the analysis of child development and health outcomes with an analysis of health care utilization and insurance. Pre-k programs may affect access to and sources of health insurance through two main channels. First, they may provide parents with information about public health insurance programs for children that they may be unaware of, potentially increasing access to health insurance and/or substituting public insurance for private insurance. Second, if pre-K programs have a positive effect on maternal labor supply, this may increase access to employer-provided health insurance. I look at three variables related to health insurance: an indicator for whether the child is covered by any health insurance at the time of interview; and two indicators for whether she has public insurance and private insurance. As pre-K programs can also increase families' disposable income, by increasing labor supply or reducing expenditures in child care, they may reduce families' financial constraints for the utilization of health care. To assess this, I create an indicator variable for whether the child had health care access problems due to financial reasons, based on two survey questions that ask whether there was a time in the past 12 months when medical care was delayed or the child did not get medical care because the family could not afford it. Finally, I look at two variables related to health care utilization: an indicator for whether the child had a hospital stay in the past 12 months, and an indicator for any ER visit related to an asthma episode in the past 12 months. Both of these variables indicate utilization of health care for potentially serious health events.

4 Results

4.1 Reduced-Form Effects of Pre-k Programs on Child Outcomes

Table 4 presents reduced-form estimates of the effects of a state pre-K expansion on development and health outcomes, by gender and age-group (1-4 and 5-8 years after pre-K age). For all outcome variables presented here, a positive effect can be interpreted as a detrimental effect. All regressions are estimated separately for each age-group, and include state and cohort fixed effects, individual controls for gender, race/ethnicity and maternal education, and state-level controls for federally-funded Head Start enrollment when the child was 4 years old, and SCHIP/Medicaid eligibility income-to-poverty ratio thresholds, annual unemployment rate and annual state median income, at age 4 and at the current year. I have also estimated separate regressions for each specific year after pre-K, whose point estimates and 95% confidence intervals are ploted in Figure 2.

The results for the pooled sample of both genders (Panel A of Table 4) show no statistically significant effects on the development outcomes, and positive effects on the variables indicating health problems during the first four years after pre-K. Children who live in a state with a Pre-K program at age 4 miss an average of 0.7 days of school more during the following 4 years, and have a health problems index that is 0.12 standard deviations higher. Both effects are significant at a 1% significance level.[17] Consistent with this, respondents are more likely to report that the child's health status is fair or poor, although this effect is only significant at 10% significance level. Despite

[17] To account for the fact that standard errors are clustered at the state level and the number of states is relatively small, all significance tests and confidence intervals are computed using a t-student distribution with $G-1$ degrees of freedom, where G is the number of states in the sample. In my main estimation sample there are 33 states.

these deleterious short-run effects, there are no statistically significant effects on health outcomes 5 to 8 years after pre-K, except for a weakly significant effect on reported health status.

In the separate regressions for each specific year after pre-K (top panel of Figure 2), the samples are too small to obtain robust estimates, but in general the results are in line with what I find for the grouped-age samples: there are no significant effects for developmental outcomes, and there are some statistically significant positive effects on the health problems index and missed days of school in the first to third years after pre-K.

Because developmental and behavioral problems are much more prevalent for boys than girls, it could be that pooled estimates mask differential effects by gender. Panels B and C of Table 4 show estimates of the reduced-form effects 1-4 and 5-8 years after pre-K, for the separate samples of boys and girls, respectively. Similarly, the center and bottom panels of Figure 2 show the point estimates and confidence intervals for the samples of boys and girls, respectively, from separate regressions for each year after pre-K. There are negative effects on special education placement for boys, with a statistically significant decrease of 3.4 percentage points (p.p.) in years 1-4 after pre-K, and a non-significant effect of smaller magnitude for years 5-8. The estimated effects on the development problems index for boys are also negative but only statistically significant for years 5-8 after pre-K, with a decrease of 0.13 standard deviations. The graph for the development index for boys in Figure 2 shows that the point estimates are negative every year, although never statistically significant. In contrast, the results for girls show no significant improvement in any of the development outcomes.

The finding of beneficial effects on developmental outcomes for boys are in line with some of the previous findings in the literature for Head Start. The only paper that estimates separate effects of Head Start for boys and girls is Deming (2009), who finds that Head Start has positive effects on tests scores at different ages during childhood for boys but not for girls. Carneiro and Ginja's (2014) identification strategy only allows them to estimate treatment effects for boys but not for girls; they find beneficial effects of Head Start participation for boys at ages 12-13 on outcomes such as being overweight and a behavior problems index, as well as on an index of symptoms of depression at ages 16-17, but not on cognitive test scores. By contrast, Anderson (2008) re-analyzes the impacts of the Perry Preschool Program and other model programs by gender and finds more consistent evidence of positive impacts for girls than for boys, especially on educational attainment.

When looking at the effects on the individual components of the development problems index (Appendix Table A1), the reduction of the index for boys after 4 years seems to be driven by a reduction in learning disability diagnosis, while the effects on the other components are also negative but not statistically significant. While most of the effects on the components of the index for girls are not statistically significant, there is a significantly positive effect on learning disability diagnosis 5-8 years after pre-K that would suggest a potentially deleterious effect for girls in the medium run.

To explore what specific conditions may be behind the deleterious short-run impacts on child health, Appendix Table A2 presents the effects on each of the components of the health index. The short-run effects seem to be driven by an increase in frequent diarrhea, which is the only statistically significant effect, but the point estimates of the effects have the same sign for the four outcomes. This effect goes away 5 to 8 years after pre-K. When looking at the disaggregated components of the health index by gender, there is a differential pattern for girls: there is a significant increase in the likelihood of having an asthma episode for girls 5-8 years after pre-K. These findings are consistent with an increased exposure to infections during preschool that does not produce a protective effect on the immunological system, or at least not in the short- and medium-run. However, the channel through which pre-K is causing these deleterious health effects is not necessarily the direct effect on increased exposure to illness during pre-K; it could also be, for example, through increased maternal labor force attachment or other changes in behavior.

A usual concern when looking at reported health conditions is that awareness of some health problems may be sensitive to changes in access to health care. The health outcomes that compose the health problems index are acute health problems that can easy be observed by parents, which reduces this concern. Additionally, the information on reported health conditions is supplemented by the information on missed days of school and reported health status. Although both of them are also reported by the survey respondent, it is reassuring that the estimated effects on all of them are consistent.

Changes in access to care may be more of a concern for the reporting of developmental outcomes incorporated in the development index, especially learning disability diagnosis and ADHD diagnosis. If pre-K attendance increases access to health care, either through increased maternal employment or increased access to public programs, this could improve the diagnosis and treatment of certain conditions, biasing the estimates towards finding increases in diagnosis. To explore if there is any evidence to be concerned about this, Table 5 shows the reduced-form effects of pre-K expansions on health care utilization and insurance outcomes. There are no statistically significant changes in the probability of having health insurance, or in the indicator for health care access problems (*Could Not Afford Care*). Not only are the estimates not significantly different from zero for these two variables (for both genders pooled and separately), but the point estimates are also very small and quite precisely estimated. There appears to be, however, a shift from private to public insurance 5-8 years after pre-K.

In estimates of the reduced-form effects on health care utilization and insurance outcomes of pre-K expansions interacted with race/ethnicity groups (presented in Appendix Table A3), I find that this shift towards public health insurance is driven by Hispanic children only, while there are no significant changes for the other race/ethnicity groups. There is an increase in public insurance after pre-K for all age-groups of Hispanic children, which also leads to a significant overall increase in the number of Hispanic children with any insurance 1-4 years after pre-K. However, there is no indication that any of the effects that I find on health outcomes are driven by this group; there are no significantly differential health effects on Hispanics (results presented in Appendix Table A4). There is, however, a significantly differential effect for Hispanics in the development problems index, indicating an increase in the diagnosis of developmental problems for Hispanics that may be related to increased access to health care. In sum, changes is access to health care may be a source of concern only for Hispanic children, but neither the deleterious health effects, nor the beneficial effects on developmental outcomes for boys are driven by this group.[18]

Table 5 also shows the estimated reduced-form effects on hospital stays and asthma-related emergency room visits. There are no significant short- or medium-run effects on any of these two variables, either for the pooled sample of both genders or for each gender separately. This suggest that any deleterious short-term effects are not serious enough to show up in increased hospitalizations, and if there is an increase in the incidence of asthma episodes for girls in the medium-run they do not translate into increased emergency room visits for this reason.

[18] I have also explored the heterogeneity of effects by maternal education (college graduate or not) and by type of program (universal or targeted). I do not find any relevant differential effects, but the estimates are imprecise. The results are available upon request.

4.2 Robustness of the Reduced-Form Estimates

4.2.1 Correlation of pre-K programs with other state characteristics

The main threat to identification of the effects of pre-K expansions on child outcomes is the possible existence of other confounding factors that change at the state level at a similar time as the pre-K expansions are implemented. A possible concern is that states may implement pre-K policies when their economies are strong and they have enough funding. Another concern is that the implementation of pre-K programs may be correlated with the implementation of other social programs. Finally, because the reduced-form models assume that the state of birth is the same as the state where the child currently lives, an additional concern is that the demographic composition of the states may be changing for these cohorts of children in a way that is correlated with the timing of the implementation of pre-K policies. Table 6 explores these possibilities by estimating regression models like equation (2) but where the the individual demographic controls and the state-level economic and policy variables are used as outcome variables instead of controls. The idea is to check whether the implementation of pre-K programs, conditional on state and cohort fixed effects, is predictive of the economic circumstances of states, their demographic composition, or the generosity of other state social programs that may have confounding effects. Although this is not a direct test of the exogeneity of the pre-K policies, it can be informative to test its conditional orthogonality with potentially confounding observed demographic, economic and state policy factors.

Table 6 presents the results of this exercise. The top panel refers to individual demographic characteristics, and each column shows the effects of the pre-K policy variable on different child and family characteristics, as observed for children 1-4 and 5-8 years after pre-K, controlling for state and cohort fixed effects. The outcome variables are dummy variables for female, black, Hispanic, and being born in the U.S., child age in months, dummies for the mother having a college degree and being married, and the mother's age in years. The estimates are not significantly different from zero, so there is no evidence of any changes in the demographic composition of the cohorts affected by pre-K policies.

The bottom panel of Table 6 presents the results for state-level variables. First, I check whether pre-K policies are predictive of the federally-funded Head Start enrollment rate in the state when the child was 4 years old. If states invest in pre-K programs as a substitute for low federal Head Start funding, or if pre-K programs draw students from Head Start programs, this would bias the estimated impacts on child outcomes because the control group would have higher participation in Head Start while the treatment group has higher participation in state pre-K. On the other hand, if states invest more in pre-K at the same time as the federal government invest in Head Start, the estimated effects would confound the effects of the two programs. The estimates in the first column of the bottom panel of the table show that the coefficients for pre-K on Head Start are not significantly different from zero. The point estimates are negative and, if significant, would imply a very small effect: Head Start enrollment is estimated to be 0.3 percentage points lower after a pre-K program is implemented.

Second, I test whether pre-K is predictive of the generosity (income-to-poverty ratio eligibility limits) of the State Child Health Insurance Program and Medicaid (whichever is lower). I look at two variables: the eligibility requirements for children of ages 1-5, measured when the child was 4 years old, and the eligibility requirements for children ages 6-15, measured when the child is observed in the NHIS. Again, none of the estimates are significantly different from zero.

Finally, the last four columns of the bottom panel of Table 6 present the estimates of regressions where the outcome variables are state economic conditions (unemployment rate and median income) when the child was 4 years old and when the child is observed. The estimated coefficients are small and not statistically significant. Similarly, there is no evidence of pre-K policies at age 4 being predictive of the current economic circumstances in the states where the children in the sample live, as measured by current unemployment rate and state median income.

4.2.2 Alternative Specifications and Samples

This section explores the sensitivity of the paper's main results to alternative model specifications, inclusion of control variables, and selection of sample states. I focus on the two main groups of effects that the results show: the beneficial effects on development outcomes of boys, and the deleterious effects on health outcomes of children of both genders in the short-run, with only medium-run effects for girls in the health problems index. Thus, I present robustness checks for the two development outcome variables for boys, the three health outcomes for both genders, and the health problems index for girls.[19]

One of the threats to the identification of treatment effects in a generalized difference-in-difference model like the one used here is if there already existed divergent trends in the outcome variables between the treatment and the control groups. After repeating the main results in Panel A, Table 7 presents the results when state-specific linear time trends are added as controls to the main reduced-form specification. The impacts on health outcomes for both genders are virtually unchanged, with only a slightly larger point estimate on the indicator for fair/poor health status in the short run. Because the samples for each specific state and year are relatively small, the inclusion of state trends reduces the statistical power of the regression models, especially for the disaggregated samples by gender. The estimated effects on special education placement in the short-run, and development problems index in the medium-run for the sample of boys are not significantly different from zero in this specification, but this is mainly a product of larger standard errors, as the point estimates are very slightly changed with respect to the main specification. The results suggest that the estimated effect on the health problems index for girls 5-8 years after pre-K is sensitive to the inclusion of state linear time trends, with a coefficient that is half as large as in the main specification and not statistically significant. Ultimately, the only evidence of long-run health effects from the main results turns out not to be robust to the inclusion of state-specific time trends. Perhaps this is not surprising, given that I do not find significant effects for girls after four years on either missed days of school or reported health status.

As discussed in the previous section, another potential threat to identification is the variation of other public policies across states that may be affecting child outcomes at the same time as the implementation of pre-K programs. In the main specification I include controls for the generosity of state public health insurance for children and for the enrollment in Head Start program. Another policy that may be simultaneously affecting the preschool attendance and development and health outcomes of children is the Child Care Development Fund (CCDF). CCDF is a childcare subsidy program targeted towards low-income working families, implemented through a federal block grant. Although the federal government establishes some eligibility requirements, it gives states freedom to decide how to implement the subsidies. States can allocate TANF funds to the program, they can establish family income eligibility limits below the federal maximum, and they are responsible

[19] The results for the other outcomes and samples are available upon request.

for determining eligibility controls, payment rates, and requirements for child care providers. Previous literature has found that receiving the child care subsidy increases maternal supply but has negative effects on children's cognitive and behavioral outcomes, and increases the prevalence of child obesity.[20] If the timing of changes in state regulations or funding of CCDF subsidies is correlated with the implementation of pre-K policies, the regression coefficient for preschool attendance instrumented by the pre-K policies could pick up some of the causal effects of the subsidy. Panel C of Table 7 shows the reduced-form results when the model includes a control variable indicating the number of 4-year-olds served by the CCDF in each state. The estimates are less precise than the main specification because there is no information on the CCDF for the first year of my sample period, and thus these regressions are estimated with a smaller sample. The main results are all in line with the main estimates, with some differences in the sizes of the effects on the development outcomes for boys (of larger magnitude) and on the health index for both genders and for the girls sample (smaller positive effects).

Because the policy variable is at the state level and the number of treatment and control states are not very large (15 and 18 states, respectively), it is possible that the results could be driven by one particular outlier state. To show that this is not the case, I re-estimate the main specification of the reduced-form regressions taking one state out of the sample at a time. The results from this exercise are shown in Figure 3. Each plot shows the point estimates and confidence intervals, for the outcome and sample in the column heading, of the effect of pre-K policies 1-4 and 5-8 years after pre-K (top and bottom rows, respectively). The first thing to note is that the point estimates for all the outcomes presented are fairly unchanged when each state is taken away from the sample. For the short-run effect on special education placement for boys 1-4 years after pre-K, and on the development problems index 5-8 years after pre-K, there are three states whose exclusion from the sample makes the estimates more imprecise, turning them marginally non-significantly different from zero at a 5% significance level. In both cases the three states are part of the treatment group, so it is not unexpected that taking them away from the sample would make the estimated treatment effects weaker. I also explore the robustness of the main results to the inclusion of all *excluded*

[20] Blau and Tekin (2007) show that child care subsidy receipt increases the labor supply of single mothers. Herbst and Tekin (2010) find that receiving the subsidy in the year before kindergarten is associated with lower reading and math test scores and greater behavior problems at kindergarten entry, for children from single mothers. Herbst and Tekin (2012) find that subsidized child care leads to increases in the prevalence of overweight and obesity among low-income children.

states in the sample. Because these are states by the beginning of the sample period already have state pre-K programs, and enrollment rates of 4-year-olds in those states was increasing during the period, their inclusion in the control group should cause a bias toward finding no effects of pre-K programs. The results, presented in panel D of Table 7, show that most of the estimated treatment effects are slightly attenuated, but none of the main conclusions are changed.

I also show that the results of the estimations for the binary outcomes (special education placement and health fair/poor) are not a product of the choice of a linear probability model, by showing the results for the average marginal effects of a Probit model with the same control variables as the main specification (panel E of Table 7). Finally, the last panel of Table 7 presents the main results for the reduced-form specification that is used in the next section to estimate the TS-2SLS models. This specification only includes state and cohort fixed effects and individual demographic controls. It omits all the child age and state-level control variables, which are not used in the first-stage regression of preschool attendance of 4-year-olds. The main estimates are practically unchanged, with very small differences in some point estimates.

In sum, the robustness checks conducted in this section indicate the robustness of the beneficial effects for boy's developmental outcomes, and the deleterious short-run effects on health outcomes for both genders. However, the medium-run impacts on girls' health problems index are not robust to some specifications, so I cannot conclude that there is robust evidence of any lasting impacts on child health five to eight years after preschool age.

4.3 Effects on Preschool Attendance at Age 4 and TS-2SLS Results

The results of the first-stage effect of pre-K expansions on preschool attendance are presented in Table 8. My preferred specification includes state and cohort fixed effects and individual-level controls (dummies for race/ethnic group, gender, and maternal educational attainment). The average estimated effect of a pre-K policy expansion is a 7.7 percentage point increase in the probability of being enrolled in preschool, which implies a 13% increase relative to the control states' average preschool enrollment throughout the period (see summary statistics in Table 3).[21]

[21] Appendix Table A5 presents robustness tests of the first stage regression for the pooled sample of both genders. The estimates change very little when I include state-level control variables and state-specific linear trends in the regression. Including the *excluded states* in the control group reduces the estimated effect, which is expected given that we are including in the control group states that had pre-K programs whose enrollment was growing during the sample period. The estimated average marginal effects are unchanged if a Probit model is estimated instead of the

The second and third columns of Table 8 show the estimates for the separate samples of boys and girls, respectively. The effects are statistically significant for both genders. The point estimate for the effect is larger for girls (9.8 p.p.) than boys (6 p.p.), but I cannot reject the hypothesis that the two coefficients are equal at a 5% significance level.

Given the heterogeneity of the programs studied, I explore whether there are heterogeneous impacts on enrollment across targeted and voluntary universal pre-K programs. In general, targeted programs are smaller and implemented in disadvantaged school districts, while universal programs have a more rapid and broader roll-out. While targeted programs have an average enrollment rate of 12% of their states' four-year-olds by the end of the period, enrollment in universal programs average 39%. Column 4 of Table 8 shows the results of interacting the Post Pre-K expansion variable with an indicator for whether the program is universal. I cannot reject the hypothesis that the coefficient for the interaction of *Post Pre-K* and the indicator for a universal program is zero, meaning that there is no statistically significant difference in the impact of the two types of programs on overall preschool enrollment rates. Given that enrollment in universal programs is higher, this suggests that there is substantial crowding out from other preschool programs in states with voluntary universal pre-K.

Since I do not have data on the specific type of program that each child attends, I cannot directly test the hypothesis of crowding-out. The CPS asks whether the child attends public or private school (or preschool), but it is not clear whether this identifies the source of funding. Head Start programs and many state pre-K programs have public funding but are carried out by private providers, and it is not clear that parents can identify whether a particular preschool center is publicly or privately funded unless it is actually located in a public school. Nevertheless, I present the estimates of the effects of the two types of programs on reported public preschool attendance (Column 5), and private preschool attendance (Column 6). The results of Column 5 suggest that attendance to public preschool is incremented more in states with universal voluntary pre-K programs (10.7 p.p.) than in states with targeted programs (3.8 p.p.). In the case of private preschool attendance, the estimates indicate a 4.4 p.p. increase in attendance in states with targeted programs, and a 4.4 p.p. *decrease* in states with universal programs, suggesting significant crowding-out.[22]

linear probability model (results available upon request).

[22]These results on the crowding-out effects of universal pre-K programs are similar to what Cascio and Schanzenbach (2013) find for the introduction of the universal pre-K programs of Georgia and Oklahoma in 1995 and 1998,

Table 9 presents the Two-Sample Two-Stage Least Squares (TS-2SLS) estimates of the effects of pre-K attendance on development and health outcomes. These results provide an approximation to the magnitude of the *treatment-on-the-treated* effects of pre-K attendance on child outcomes, under the assumption that pre-K expansions only affect children who attend preschool as a consequence of the expansion of these programs and would not attend preschool otherwise. As explained in Section 3.2, these estimates are obtained by re-scaling the reduced-form effects of pre-K expansion by the percentage increase in preschool enrollment that resulted from these pre-K expansions. Because the same control variables must be used in both stages, I use the specification of the reduced-form regressions that only includes state and cohort fixed effects and individual demographic controls (they do not include age dummies and state-level controls). Each panel of the table presents the TS-2SLS for the sub-samples of children observed 1-4 and 5-8 years after pre-K age, followed by the reduced-form and first-stage estimates used to calculate them.

Panel A of Table 9 shows the TS-2SLS estimates for the pooled sample of both genders, where the first-stage is stronger (the F statistic of the instrument is 13.6). The estimated effects on health outcomes are large but less precisely estimated than the reduced-form effects. For example, the estimates for the health problems index suggest that pre-K attendance increases the index during the first four years after pre-K by 1.4 standard deviations of the control group, but it is only statistically different from zero at a 10% level. Similarly, the results in the last column suggest that the number of missed school days are increased by close to 8 days in a year.

The TS-2SLS estimates for the samples of boys and girls are presented in Panels B and C of Table 9, respectively. The results are imprecise because the first stages for the stratified gender samples are weaker due to the smaller sample sizes, with an F-statistic close to 7 for boys and 8 for girls. The most important result from the reduced-form estimates by gender is that there are beneficial effects on development outcomes for boys. The TS-2SLS estimates suggest that pre-K attendance reduces the likelihood of special education placement for boys 1-4 years after pre-K by 60 percentage points, and it reduces the development problems index 5-8 years after pre-K by 2.5 standard deviations of the control group. These estimated effects are extremely large but very imprecisely estimated, and are only statistically different from zero at a 10% significance level.

respectively, also using October CPS data. They also find that crowding-out was larger for children whose mothers had higher educational attainment levels.

Thus, I cannot rule out much smaller treatment effects.

Even when the imprecision of the estimates does not allow me to rule out smaller effect sizes, the generally large magnitude of the point estimates suggest that the relevant “treatment” is not just preschool attendance. There are important reasons to believe that the “treated” children are not only the children whose attendance to preschool is induced by the pre-K expansions. The fact that a child would have attended preschool even in the absence of a state pre-K program does not imply that the child was not affected by the availability of the program. First, children may shift from other, potentially lower-quality, programs to state-funded pre-K. Suggestive evidence of crowding-out of other providers of pre-K education was discussed above. Second, for families that substitute public pre-K for private child care, the public provision of pre-K implies an increase in disposable income, which may in turn positively affect children. Third, children may be simultaneously enrolled in more than one preschool program, as many programs are only part-day. Consequently, pre-K expansions may increase the amount of hours per week that children are exposed to preschool education (intensive margin), rather than just the actual attendance to preschool (extensive margin). The group of children potentially affected by pre-K expansions through the intensive margin includes children who would otherwise be attending some preschool program anyway.

5 Conclusion

In this paper, I have presented new evidence on the short- and medium-run effects of state-funded pre-K programs on child development and health outcomes. I overcome the lack of data sources with information on both attendance to pre-K programs at age 4 and health and developmental outcomes throughout childhood, by collecting information on the implementation of state pre-K programs between 1997 and 2005, and merging this information with individual-level data from the National Health Interview Survey for children observed 1 to 8 years after pre-K age, and with state-level control variables from various sources. I use these data to estimate the reduced-form effects of pre-K programs on a set of child development and health outcomes, including a development problems index and a health problems index that summarize information from multiple outcome variables, in regression models with state and cohort fixed effects.

My results suggest that pre-K programs have beneficial effects on development outcomes for boys both in the short and medium run, but not for girls. This is perhaps not surprising, given that the development and behavioral problems that I look at are much more prevalent for boys. The results also suggest that pre-K programs can have some deleterious health effects during the first four years after preschool age for children of both genders, which is reflected in an increase in an index of health problems and on missed school days. However, there are no significant effects on hospitalizations or asthma-related ER visits, and there is no robust evidence of any medium-run effects on health outcomes.

I also provide estimates of how many 4-year-olds enroll in preschool as a consequence of the implementation of these programs, finding that enrollment rates are increased by 7.7 percentage points. I use this result as the first-stage in two-sample two-stage least squares models where I estimate the effects of preschool attendance on child outcomes, using the implementation of pre-K programs as an instrument for preschool attendance. The results suggest that the magnitudes of the effects on the children who attend preschool are very large, although they are imprecisely estimated so I cannot reject much smaller effect sizes. However, the relatively small effect on overall preschool attendance compared to available information on enrollment rates in pre-K programs suggests that there are more children affected by these programs than what is implied by the first-stage effects. Thus, I interpret these treatment-on-the-treated estimates as an upper bound for the size of the effects.

The results from this analysis have important implications for the literature on the effects of preschool education programs. This paper is the first to estimate health effects of state pre-K programs, finding deleterious short-run effects that are in line with the findings in papers that study the effects of child care subsidies, but against some of the findings of the literature on the federal Head Start program. This raises questions for future research on preschool programs, in terms of the channels that may explain these effects and the role that specific program characteristics can play to prevent these effects. The findings of this paper also show that preschool programs can have impacts on developmental outcomes throughout childhood, as evidenced by the decrease in the development problems index for boys. This result is in line with previous findings of lasting impacts on test scores for boys, and underscores the importance of studying the heterogeneity of impacts of early education programs by gender. Finally, these results also suggest additional channels for explaining the long-term impacts on labor market, educational attainment, and crime found in previous literature on preschool programs.

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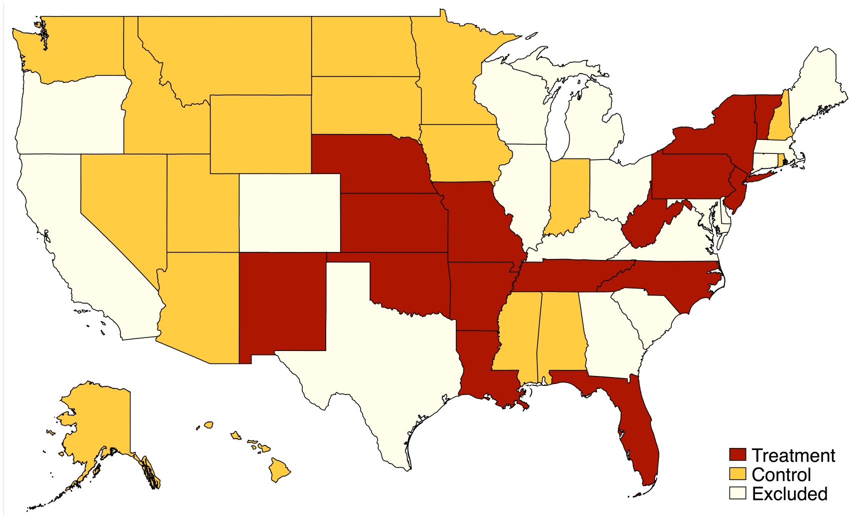
Main Figures

Figure 1: Map of Treatment, Control, and Excluded States

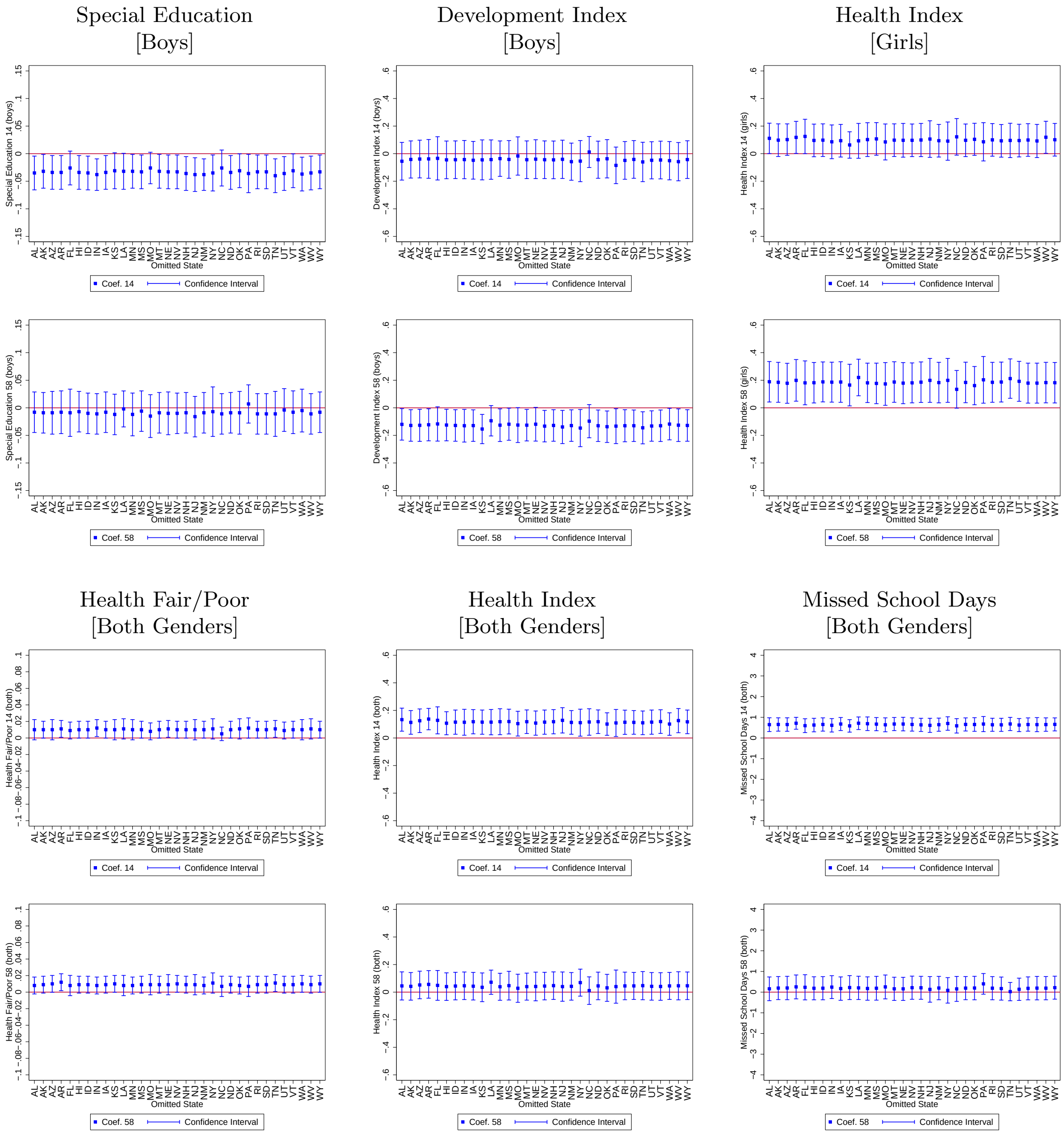


Notes: *Treatment* indicates that a state implemented or significantly expanded pre-K between 1998 and 2005. *Control* indicates that a state had not yet implemented or had only a very small state pre-K program by 2005. *Excluded* indicates that a state is excluded from the main sample because it already had a pre-K program by 1997. For more details on sample states definitions see the Background Section.

Figure 2: Reduced-Form Effects for Each Year after Pre-K, by Gender

Notes: Figure plots the reduced-form estimate of the effects of a pre-K expansion on developmental and health outcomes in each year after pre-K age, by gender. Shaded region is 95% confidence interval. Each point estimate is obtained in a separate regression, with the sample corresponding to both genders (top panel), boys (middle panel) or girls (bottom panel) observed in the NHIS *sample child* sample the number of years after pre-K age indicated in the x-axis. Standard errors clustered at state level.

Figure 3: Sensitivity of Reduced-Form Results to Each State in Sample



Notes: Figure plots the reduced-form estimate of the effects of a pre-K expansion on developmental and health outcomes when excluding one state at a time from the main sample. Each point estimate is obtained in a separate regression, with the sample corresponding to boys or both gender, as indicated in the column headings, observed 1-4 years after pre-K (top row of each panel) and 5-8 years after pre-K (bottom row of each panel).

Main Tables

Table 1: Pre-K Program Characteristics in Treatment States in 2005

State (Year Implemented)	Classroom Hrs/week	Income Targeted	Health Components	CELS	Spending/ Student	NIEER Score	4-Year-Old Enrollment
Arkansas (2004)	38	Yes	Yes	Yes	7,769	9	0.18
Florida (2005)	15	No	Yes	Yes	2,163	4	0.47
Kansas (2002)	11	Yes	Yes	No	2,554	3	0.15
Louisiana (2002)	40	Yes	Yes	Yes	5,012	8	0.22
Missouri (1999)	DL	Yes	No	Yes	2,632	6	0.04
Nebraska (2001)	DL	Yes	DL	Yes	7,418	8	0.04
New Jersey (1999)	30	Yes	Yes	Yes	9,854	9	0.18
New Mexico (2005)	14	Yes	Yes	Yes	2,269	5	0.07
New York (1998)	DL	No	Yes	No	3,512	5	0.29
North Carolina (2002)	30	Yes	Yes	Yes	3,892	10	0.12
Oklahoma (1998)	DL	No	DL	Yes	6,167	9	0.70
Pennsylvania (2004)	20	Yes	DL	Yes	4,730	4	0.06
Tennessee (2005)	28	Yes	Yes	Yes	4,061	9	0.11
Vermont (2003)	10	No	Yes	Yes	2,930	7	0.47
West Virginia (2002)	12	No	Yes	Yes	7,758	7	0.40
Mean (or % Yes)	16	0.67	0.73	0.87	4,848	6.87	0.23
Median					4,061	7	0.18

Notes: Source: NIEER (2006). Year Implemented is the year program was established or expanded. Classroom Hrs/week is the number of hours in classroom that all providers must offer per week. Income Targeted indicates there are income limits for eligibility of students, or a minimum percentage of low-income for eligibility of a school district, school or program. Health Components indicates providers required to offer physical, vision and hearing screenings and referrals (some also include dental and/or developmental screenings and referrals). CELS indicates providers are required to follow comprehensive early learning standards. Spending/Student is the total spending per enrollee in 2005-2006 (including state funding, local matching funds, and federal grants administered by the state. NIEER Score is the number of NIEER quality standards met by the program in 2005-2006 (out of 10). 4-Year-Old Enrollment is the percentage of 4-year-olds in the state that were enrolled in state funded pre-K in school year 2005-2006. DL stands for determined locally.

Table 2: NHIS Sample Characteristics in Treatment, Control and Excluded States

	Treatment States		Control States		Excluded States	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Individual Characteristics						
Female	0.49	0.50	0.49	0.50	0.49	0.50
Black	0.18	0.38	0.10	0.29	0.15	0.35
Hispanic	0.15	0.36	0.13	0.34	0.26	0.44
Other Race/Ethnicity	0.04	0.21	0.07	0.26	0.06	0.23
Age (years after pre-K age)	4.49	2.30	4.55	2.29	4.55	2.29
Mom High-School Graduate	0.27	0.44	0.24	0.43	0.23	0.42
Mom Some College	0.19	0.39	0.21	0.41	0.20	0.40
Mom College Graduate	0.40	0.49	0.42	0.49	0.39	0.49
State Characteristics at Pre-K Age						
Federal Head Start Enrollment (%)	12.01	3.31	12.39	6.52	11.77	2.79
SCHIP/Medicaid Income-to-Pov Ratio (Age 1-5)	2.18	0.60	2.05	0.44	2.03	0.39
Annual Unemployment Rate	4.88	0.85	4.59	1.12	5.12	1.11
Median Income ($1,000s)	52.97	7.25	56.50	7.87	58.01	5.94
State Characteristics at Current Age						
SCHIP/Medicaid Income-to-Pov (Age 6-15)	2.44	0.66	2.24	0.41	2.21	0.35
Annual Unemployment Rate	5.88	1.84	5.69	2.07	6.46	2.19
Median Income ($1,000s)	52.63	7.61	55.81	8.00	57.31	6.66
Outcome Variables						
Special Education	0.08	0.28	0.07	0.26	0.08	0.26
Development Index	0.03	1.06	0.00	1.00	0.00	0.99
Health Index	0.03	1.07	0.00	1.00	0.01	1.03
Health Status Fair/Poor	0.02	0.14	0.02	0.12	0.02	0.14
School Days Missed	3.33	5.30	3.21	4.49	3.20	5.81
Learning Disability Diagnosis	0.08	0.27	0.07	0.26	0.08	0.27
ADHD Diagnosis	0.08	0.27	0.08	0.27	0.07	0.26
Limitations Caused by Speech Problems	0.03	0.16	0.02	0.14	0.02	0.14
Limitations Caused by Behavior Problems	0.02	0.12	0.02	0.13	0.01	0.12
3+ Ear Infections	0.05	0.21	0.05	0.22	0.05	0.21
Asthma Episode	0.07	0.25	0.06	0.23	0.06	0.24
Frequent Headaches/Migraine	0.06	0.23	0.05	0.21	0.05	0.22
Frequent Diarrhea	0.01	0.11	0.01	0.10	0.01	0.11
Any Hospitalization	0.02	0.13	0.01	0.11	0.02	0.13
Asthma ER Visit	0.02	0.15	0.01	0.12	0.02	0.14
Could Not Afford Care	0.04	0.20	0.05	0.22	0.05	0.21
Any Insurance	0.91	0.28	0.90	0.30	0.90	0.30
Public Insurance	0.30	0.46	0.26	0.44	0.29	0.45
Private Insurance	0.61	0.49	0.65	0.48	0.61	0.49
Observations	12,060		6,016		20,866	

Notes: Summary statistics for the NHIS samples of children living in treatment, control, and excluded states, observed 1 to 8 years after pre-K age, for the pre-K cohorts of 1997 to 2005, observed between 1998 and 2014 (weighted using sample weights). State characteristics at pre-K age are imputed according to the state where the child currently lives and the estimated year when the child would have been 4 years old (based on month/year of birth and month/year of interview). State characteristics at current age correspond to the state of residence and the interview year. All state characteristics are merged to the NHIS samples from other data sources (see Data section for more details).

Table 3: CPS Sample Characteristics in Treatment, Control and Excluded States

	Treatment States		Control States		Excluded States	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Individual Characteristics						
Female	0.48	0.50	0.48	0.50	0.50	0.50
Black	0.17	0.37	0.10	0.30	0.14	0.35
Hispanic	0.14	0.34	0.12	0.33	0.24	0.43
Other Race/Ethnicity	0.05	0.22	0.07	0.26	0.06	0.24
Mom High-School Graduate	0.31	0.46	0.29	0.45	0.28	0.45
Mom Some College	0.18	0.38	0.20	0.40	0.19	0.39
Mom College Graduate	0.38	0.49	0.39	0.49	0.35	0.48
State Characteristics						
Federal Head Start Enrollment (%)	11.91	3.24	12.72	7.15	11.81	2.68
SCHIP/Medicaid Income-to-Pov Ratio (Age 1-5)	2.19	0.59	2.03	0.43	2.03	0.40
Annual Unemployment Rate	4.90	0.85	4.64	1.14	5.17	1.10
Median Income ($1,000s)	53.11	7.14	56.12	7.86	58.04	5.73
Outcome Variables						
Preschool	0.61	0.49	0.58	0.49	0.61	0.49
Public Preschoo	0.29	0.45	0.27	0.44	0.32	0.47
Private Preschool	0.32	0.47	0.31	0.46	0.28	0.45
Any School	0.68	0.46	0.64	0.48	0.68	0.47
Kindergarten	0.07	0.26	0.05	0.22	0.08	0.27
Observations	4,765		4,115		6,661	

Notes: Summary statistics for the CPS samples of children living in treatment, control, and excluded states, observed at 4 years of age, 1997 to 2005 (weighted using sample weights). All state characteristics are merged to the CPS from other data sources (see Data section for more details).

Table 4: Reduced-Form Effects on Development and Health Outcomes

	Special Education	Development Index	Health Fair/Poor	Health Index	Missed School Days
		Panel A: Both Genders			
1-4 Years After Pre-K					
Post Pre-K	-0.016	-0.050	0.010*	0.116***	0.651***
	(0.010)	(0.056)	(0.005)	(0.042)	(0.156)
N	9069	9069	9069	9069	8825
N Treatment States	6171	6171	6171	6171	5993
5-8 Years After Pre-K					
Post Pre-K	0.014	-0.050	0.009*	0.044	-0.006
	(0.010)	(0.043)	(0.005)	(0.049)	(0.023)
N	9007	9007	9007	9007	8921
N Treatment States	5889	5889	5889	5889	5821
		Panel B: Boys			
1-4 Years After Pre-K					
Post Pre-K	-0.034**	-0.044	0.006	0.138**	0.587**
	(0.015)	(0.067)	(0.009)	(0.060)	(0.270)
N	4643	4643	4643	4642	4510
N Treatment States	3154	3154	3154	3153	3061
5-8 Years After Pre-K					
Post Pre-K	-0.009	-0.127**	0.006	-0.067	0.226
	(0.018)	(0.056)	(0.008)	(0.066)	(0.432)
N	4576	4576	4576	4576	4537
N Treatment States	3015	3015	3015	3015	2983
		Panel C: Girls			
1 to 4 Years After Pre-K					
Post Pre-K	0.002	-0.053	0.017*	0.099*	0.750**
	(0.010)	(0.057)	(0.010)	(0.058)	(0.337)
N	4426	4426	4426	4425	4315
N Treatment States	3017	3017	3017	3017	2932
Girls, 5 to 8 Years After Pre-K					
Post Pre-K	0.041*	0.069	0.014	0.185**	0.207
	(0.021)	(0.085)	(0.008)	(0.071)	(0.287)
N	4431	4431	4431	4431	4384
N Treatment States	2874	2874	2874	2874	2838

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample (gender and age–number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, individual-level control variables for maternal education and race/ethnicity (and gender in the Panel A), age dummies, and state-level control variables. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Reduced-Form Effects on Health Care Utilization and Insurance

	Hospital Stay	Asthma ER Visit	Could Not Afford Care	Any Insurance	Public Insurance	Private Insurance
			Panel A: Both Genders			
1-4 Years After Pre-K						
Post Pre-K	0.007	0.005	-0.007	-0.008	-0.003	-0.008
	(0.005)	(0.008)	(0.006)	(0.008)	(0.022)	(0.020)
N	9065	8376	9069	8858	8841	9044
N Treatment States	6168	5673	6171	6025	6017	6153
5-8 Years After Pre-K						
Post Pre-K	0.005	-0.004	-0.011	0.013	0.053***	-0.040**
	(0.006)	(0.007)	(0.012)	(0.011)	(0.019)	(0.017)
N	9003	8497	9007	8793	8763	8988
N Treatment States	5887	5519	5889	5764	5755	5875
			Panel B: Boys			
1-4 Years After Pre-K						
Post Pre-K	0.008	0.006	-0.017	-0.019	0.016	-0.051**
	(0.007)	(0.016)	(0.010)	(0.014)	(0.027)	(0.025)
N	4640	4222	4643	4536	4528	4631
N Treatment States	3152	2850	3154	3071	3068	3145
5-8 Years After Pre-K						
Post Pre-K	0.009	-0.009	-0.017	0.014	0.066**	-0.061**
	(0.007)	(0.009)	(0.012)	(0.019)	(0.030)	(0.024)
N	4574	4265	4576	4476	4467	4564
N Treatment States	3014	2784	3015	2951	2948	3005
			Panel C: Girls			
1-4 Years After Pre-K						
Post Pre-K	0.008	0.006	0.004	0.002	-0.023	0.035
	(0.008)	(0.007)	(0.012)	(0.017)	(0.041)	(0.032)
N	4425	4154	4426	4322	4313	4413
N Treatment States	3016	2823	3017	2954	2949	3008
5-8 Years After Pre-K						
Post Pre-K	0.002	0.002	-0.004	0.009	0.040*	-0.023
	(0.008)	(0.006)	(0.018)	(0.015)	(0.022)	(0.025)
N	4429	4232	4431	4317	4296	4424
N Treatment States	2873	2735	2874	2813	2807	2870

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample (gender and age–number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, individual-level control variables for maternal education and race/ethnicity (and gender in the first panel), age dummies, and state-level control variables. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 6: Prediction of Demographic Characteristics and State-Level Controls

	Female	Black	Hispanic	Born in U.S.	Age (Months)	Mom College	Mom Married	Mom's Age (Years)
1-4 Years After Pre-K								
Post Pre-K	0.031	0.016	0.013	-0.004	-0.019	0.015	-0.000	-0.196
	(0.019)	(0.028)	(0.014)	(0.008)	(0.177)	(0.031)	(0.018)	(0.286)
N	9069	9069	9069	9062	8381	9069	9069	9023
5-8 Years After Pre-K								
Post Pre-K	0.002	-0.011	-0.014	0.008	-0.331	0.006	0.002	0.199
	(0.016)	(0.023)	(0.014)	(0.010)	(0.205)	(0.033)	(0.024)	(0.377)
N	9007	9007	9007	9002	8290	9007	9007	6361

	Fed. Head Start Enroll. at 4	SCHIP/ Medicaid (1-5) at 4	SCHIP/ Medicaid (6-15)	Unemploy-ment Rate at 4	Unemploy-ment Rate	Median Income at 4	Median Income
1-4 Years After Pre-K							
Post Pre-K	-0.286	0.222	0.026	-0.137	0.030	-0.097	0.108
	(0.217)	(0.135)	(0.077)	(0.173)	(0.185)	(0.673)	(0.600)
N	9069	9069	9069	9069	9069	9069	9069
5-8 Years After Pre-K							
Post Pre-K	-0.291	0.209	-0.048	-0.132	-0.144	-0.080	0.182
	(0.219)	(0.132)	(0.071)	(0.158)	(0.244)	(0.636)	(0.333)
N	9007	9007	9007	9007	9007	9007	9007

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample of both genders for the age (number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, and age dummies. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 7: Alternative Specifications and Samples of Main Reduced-Form Effects

	(1) Main	(2) Trends	(3) CCDF	(4) All States	(5) Probit	(6) FE+Ind.
Special Education [Boys]						
Post Pre-K *(1-4 Years After Pre-K)*	-0.034**	-0.028	-0.044***	-0.027**	-0.036*	-0.036**
	(0.015)	(0.020)	(0.016)	(0.013)	(0.015)	(0.015)
Post Pre-K *(5-8 Years After Pre-K)*	-0.009	-0.004	-0.012	-0.009	-0.008	-0.013
	(0.018)	(0.021)	(0.024)	(0.020)	(0.018)	(0.018)
Development Index [Boys]						
Post Pre-K *(1-4 Years After Pre-K)*	-0.044	-0.004	-0.014	-0.020		-0.057
	(0.067)	(0.067)	(0.078)	(0.068)		(0.066)
Post Pre-K *(5-8 Years After Pre-K)*	-0.127**	-0.110	-0.194***	-0.105*		-0.149**
	(0.056)	(0.093)	(0.067)	(0.061)		(0.063)
Health Fair/Poor [Both]						
Post Pre-K *(1-4 Years After Pre-K)*	0.010*	0.017**	0.013*	0.010**	0.013*	0.010**
	(0.005)	(0.007)	(0.008)	(0.004)	(0.006)	(0.005)
Post Pre-K *(5-8 Years After Pre-K)*	0.009*	0.012	0.013**	0.009*	0.009	0.008
	(0.005)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)
Health Index [Both]						
Post Pre-K *(1-4 Years After Pre-K)*	0.116***	0.123**	0.081*	0.082**		0.104**
	(0.042)	(0.053)	(0.045)	(0.036)		(0.044)
Post Pre-K *(5-8 Years After Pre-K)*	0.044	0.003	0.014	0.019		0.040
	(0.049)	(0.051)	(0.055)	(0.049)		(0.055)
Health Index [Girls]						
Post Pre-K *(1-4 Years After Pre-K)*	0.099*	0.108*	0.097	0.112*		0.085
	(0.058)	(0.062)	(0.082)	(0.062)		(0.061)
Post Pre-K *(5-8 Years After Pre-K)*	0.185**	0.079	0.139*	0.098		0.158**
	(0.071)	(0.067)	(0.075)	(0.075)		(0.077)
Missed School Days [Both]						
Post Pre-K *(1-4 Years After Pre-K)*	0.651***	0.612***	0.620***	0.561***		0.595***
	(0.156)	(0.157)	(0.187)	(0.121)		(0.159)
Post Pre-K *(5-8 Years After Pre-K)*	-0.006	0.014	0.069	0.217		0.105
	(0.023)	(0.366)	(0.301)	(0.235)		(0.245)

Notes: Each cell shows results for separate regressions, for the outcome variable and gender indicated in the panel heading, and for the sample of children observed the number of years after pre-K age indicated in the row headings. Column (1) repeats the main results presented in Table 4 (specification includes controls for state and cohort fixed effects, age dummies, individual demographic controls and state-level controls). The specification in Column (2) includes all the controls of the main specification and state-specific linear time trends. The specification in Column (3) includes all the controls of the main specification and the number of 4-year-olds served by the CCDF in each state. Column (4) shows results of the main specification when *excluded* states are included in the sample. Column (5) shows results of marginal effects from Probit models for the binary outcomes, with the same controls as the main specification. Column (6) presents results for regressions that only include state and cohort fixed effects and individual demographic controls. Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 8: First Stage Results

	Preschool [Both]	Preschool [Boys]	Preschool [Girls]	Preschool [Both]	Public [Both]	Private [Both]
Post Pre-K	0.077***	0.060**	0.098***	0.082***	0.038	0.044
	(0.021)	(0.023)	(0.034)	(0.025)	(0.023)	(0.029)
Post Pre-K * Universal				-0.018	0.069**	-0.088***
				(0.025)	(0.031)	(0.024)
Observations	8880	4610	4270	8880	8880	8880
F(Post Pre-K)	13.6	6.8	8.4			

Notes: Each column shows regression estimates of the effect of Pre-K expansions on preschool attendance, except for the last two columns, where the outcome variable is public preschool and private preschool attendance, respectively. Each regression is estimated on the sample of children age 4 in the October CPS 1997-2005 in Treatment and Control States; the genders included in the sample are indicated in the column header between brackets. All regressions include state and cohort fixed effects, and individual controls (indicator variables for race, maternal education, and gender when both genders are included in the sample). Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 9: TS-2SLS Effects on Development and Health Outcomes

	Special Education	Development Index	Health Fair/Poor	Health Index	Missed School Days
			Panel A: Both Genders		
TS-2SLS					
Post Pre-K *(1-4 Years After Pre-K)*	-0.235	-0.718	0.131*	1.357*	7.766*
	(0.145)	(0.706)	(0.074)	(0.682)	(2.956)
Post Pre-K *(5-8 Years After Pre-K)*	0.144	-0.848	0.104	0.522	1.371
	(0.136)	(0.594)	(0.071)	(0.732)	(3.219)
Reduced Form					
Post Pre-K *(1-4 Years After Pre-K)*	-0.018*	-0.055	0.010**	0.104**	0.595***
	(0.010)	(0.052)	(0.005)	(0.044)	(0.159)
Post Pre-K *(5-8 Years After Pre-K)*	0.011	-0.065	0.008	0.040	0.105
	(0.010)	(0.042)	(0.005)	(0.055)	(0.245)
First Stage (Preschool at 4)					
Post Pre-K	0.077***	0.077***	0.077***	0.077***	0.077***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
F(Post Pre-K)	13.6	13.6	13.6	13.6	13.6
			Panel B: Boys		
TS-2SLS					
Post Pre-K *(1-4 Years After Pre-K)*	-0.604*	-0.957	0.117	2.165	9.247
	(0.343)	(1.167)	(0.158)	(1.294)	(5.629)
Post Pre-K *(5-8 Years After Pre-K)*	-0.218	-2.501*	0.101	-0.772	3.591
	(0.314)	(1.429)	(0.140)	(1.147)	(6.722)
Reduced Form					
Post Pre-K *(1-4 Years After Pre-K)*	-0.036**	-0.057	0.007	0.129**	0.551**
	(0.015)	(0.066)	(0.009)	(0.059)	(0.260)
Post Pre-K *(5-8 Years After Pre-K)*	-0.013	-0.149**	0.006	-0.046	0.214
	(0.018)	(0.063)	(0.008)	(0.066)	(0.392)
First Stage (Preschool at 4)					
Post Pre-K	0.060**	0.060**	0.060**	0.060**	0.060**
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
F(Post Pre-K)	6.8	6.8	6.8	6.8	6.8
			Panel C: Girls		
TS-2SLS					
Post Pre-K *(1-4 Years After Pre-K)*	0.000	-0.518	0.163	0.864	6.706
	(0.091)	(0.606)	(0.116)	(0.688)	(4.086)
Post Pre-K *(5-8 Years After Pre-K)*	0.386	0.589	0.112	1.605	0.660
	(0.235)	(0.848)	(0.099)	(0.960)	(2.773)
Reduced Form					
Post Pre-K *(1-4 Years After Pre-K)*	0.000	-0.051	0.016	0.085	0.660*
	(0.009)	(0.057)	(0.010)	(0.061)	(0.331)
Post Pre-K *(5-8 Years After Pre-K)*	0.038*	0.058	0.011	0.158**	0.065
	(0.019)	(0.081)	(0.009)	(0.077)	(0.272)
First Stage (Preschool at 4)					
Post Pre-K	0.098***	0.098***	0.098***	0.098***	0.098***
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)
F(Post Pre-K)	8.4	8.4	8.4	8.4	8.4

Notes: All reduced-form and first-stage regressions include state and cohort fixed effects, and individual-level control variables for maternal education and race/ethnicity (and gender in Panel A). Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Appendix A: Additional Tables

Table A1: Reduced-Form Effects on Development Index Components

	Learning Disability Diag.	ADHD Diagnosis	Limitation Speech	Limitation Behavior
Both Genders, 1-4 Years After Pre-K				
Post Pre-K	-0.021	-0.003	0.001	-0.007*
	(0.014)	(0.013)	(0.008)	(0.004)
N	9069	9069	9069	9069
N Treatment States	6171	6171	6171	6171
Both Genders, 5-8 Years After Pre-K				
Post Pre-K	-0.004	-0.008	-0.008	-0.002
	(0.006)	(0.016)	(0.005)	(0.005)
N	9007	9007	9007	9007
N Treatment States	5889	5889	5889	5889
Boys, 1-4 Years After Pre-K				
Post Pre-K	-0.019	0.007	-0.002	-0.011*
	(0.019)	(0.018)	(0.013)	(0.006)
N	4643	4643	4643	4643
N Treatment States	3154	3154	3154	3154
Boys, 5-8 Years After Pre-K				
Post Pre-K	-0.049***	-0.026	-0.006	-0.008
	(0.016)	(0.025)	(0.006)	(0.007)
N	4576	4576	4576	4576
N Treatment States	3015	3015	3015	3015
Girls, 1-4 Years After Pre-K				
Post Pre-K	-0.020	-0.014	0.004	-0.004
	(0.015)	(0.015)	(0.008)	(0.005)
N	4426	4426	4426	4426
N Treatment States	3017	3017	3017	3017
Girls, 5-8 Years After Pre-K				
Post Pre-K	0.047**	0.014	-0.009	0.006
	(0.018)	(0.019)	(0.007)	(0.010)
N	4431	4431	4431	4431
N Treatment States	2874	2874	2874	2874

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample (gender and age–number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, individual-level control variables for maternal education and race/ethnicity (and gender in the first panel), age dummies, and state-level control variables. Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A2: Reduced-Form Effects on Health Index Components

	3+ Ear Infections	Asthma Episode	Frequent Headaches	Frequent Diarrhea
Both Genders, 1-4 Years After Pre-K				
Post Pre-K	0.002	0.016	0.007	0.016***
	(0.013)	(0.011)	(0.011)	(0.005)
N	9069	9069	9069	9067
N Treatment States	6171	6171	6171	6170
Both Genders, 5-8 Years After Pre-K				
Post Pre-K	-0.004	0.019*	0.007	0.000
	(0.007)	(0.011)	(0.015)	(0.005)
N	9007	9007	9007	9007
N Treatment States	5889	5889	5889	5889
Boys, 1-4 Years After Pre-K				
Post Pre-K	0.015	0.022	0.015	0.009
	(0.016)	(0.015)	(0.016)	(0.007)
N	4643	4643	4643	4642
N Treatment States	3154	3154	3154	3153
Boys, 5-8 Years After Pre-K				
Post Pre-K	-0.023***	0.006	-0.008	-0.001
	(0.007)	(0.016)	(0.020)	(0.008)
N	4576	4576	4576	4576
N Treatment States	3015	3015	3015	3015
Girls, 1-4 Years After Pre-K				
Post Pre-K	-0.014	0.009	-0.002	0.023***
	(0.019)	(0.015)	(0.013)	(0.007)
N	4426	4426	4426	4425
N Treatment States	3017	3017	3017	3017
Girls, 5-8 Years After Pre-K				
Post Pre-K	0.017*	0.038**	0.027	0.001
	(0.009)	(0.015)	(0.020)	(0.007)
N	4431	4431	4431	4431
N Treatment States	2874	2874	2874	2874

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample (gender and age–number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, individual-level control variables for maternal education and race/ethnicity (and gender in the first panel), age dummies, and state-level control variables. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A3: Heterogeneity of Effects on Health Care Utilization and Insurance by Race/Ethnicity (Both Genders)

	Hospital Stay	Asthma ER Visit	Could Not Afford Care	Any Insurance	Public Insurance	Private Insurance
1-4 Years After Pre-K						
Post Pre-K	0.007	0.001	-0.002	-0.013	-0.020	0.004
	(0.006)	(0.008)	(0.006)	(0.009)	(0.025)	(0.023)
Post Pre-K * Black	0.002	0.010	-0.003	-0.004	0.025	-0.029
	(0.007)	(0.014)	(0.012)	(0.020)	(0.026)	(0.034)
Post Pre-K * Hispanic	0.001	0.018	-0.038**	0.052**	0.099**	-0.054*
	(0.009)	(0.018)	(0.014)	(0.020)	(0.041)	(0.031)
5-8 Years After Pre-K						
Post Pre-K	0.004	-0.004	-0.007	0.004	0.027	-0.024
	(0.006)	(0.006)	(0.013)	(0.014)	(0.020)	(0.021)
Post Pre-K * Black	-0.002	0.003	-0.007	0.013	0.057*	-0.037
	(0.015)	(0.016)	(0.012)	(0.013)	(0.034)	(0.041)
Post Pre-K * Hispanic	0.008	-0.006	-0.016	0.041	0.109***	-0.063
	(0.006)	(0.013)	(0.012)	(0.035)	(0.035)	(0.044)

Notes: Each panel shows results for separate regressions, for the outcome variable indicated in the column heading, and the age group (number of years after pre-K age) indicated in each panel heading. The regressors are the indicator for Post Pre-K, and its interaction with dummies for black and Hispanic. All regressions include state and cohort fixed effects, individual-level control variables for maternal education, gender, and race/ethnicity, age dummies, and state-level control variables. Sample sizes are shown in Panel A of Table 5. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A4: Heterogeneity of Effects on Development and Health Outcomes by Race/Ethnicity (Both Genders)

	Special Education	Development Index	Health Fair/Poor	Health Index	Missed School Days
1-4 Years After Pre-K					
Post Pre-K Expansion	-0.015	-0.063	0.006	0.122**	0.647***
	(0.012)	(0.064)	(0.007)	(0.049)	(0.167)
Post Pre-K Exp. * Black	-0.011	-0.002	0.008	-0.078	-0.237
	(0.018)	(0.069)	(0.008)	(0.089)	(0.358)
Post Pre-K Exp. * Hispanic	0.014	0.112**	0.018	0.072	0.425
	(0.013)	(0.044)	(0.020)	(0.082)	(0.347)
5-8 Years After Pre-K					
Post Pre-K Expansion	0.019	-0.076	0.012*	0.055	0.114
	(0.017)	(0.053)	(0.006)	(0.047)	(0.284)
Post Pre-K Exp. * Black	-0.018	0.036	-0.011	-0.012	-0.088
	(0.029)	(0.094)	(0.011)	(0.068)	(0.303)
Post Pre-K Exp. * Hispanic	-0.011	0.139	-0.009	-0.064	0.662
	(0.027)	(0.084)	(0.008)	(0.072)	(0.508)

Notes: Each panel shows results for separate regressions, for the outcome variable indicated in the column heading, and the age group (number of years after pre-K age) indicated in each panel heading. The regressors are the indicator for Post Pre-K, and its interaction with dummies for black and Hispanic. All regressions include state and cohort fixed effects, individual-level control variables for maternal education, gender, and race/ethnicity, age dummies, and state-level control variables. Sample sizes are shown in Panel A of Table 4. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A5: Alternative Specifications First Stage Results

	(1) Preschool [Both]	(2) Preschool [Both]	(3) Preschool [Both]	(4) Preschool [Both]
Post Pre-K	0.079***	0.073***	0.067**	0.043**
	(0.023)	(0.020)	(0.025)	(0.021)
State FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes
State controls	No	Yes	Yes	Yes
State trends	No	No	Yes	No
Observations	8880	8880	8880	15541
F(Post Pre-K)	11.46	13.40	7.06	4.34

Notes: All columns show estimates of the first-stage effect of a pre-K expansion on preschool attendance of 4-year-olds. The sample includes children of both genders of age 4 in the October CPS 1997-2005. The samples used for the first three columns include only Treatment and Control States, while the sample in column (4) includes the Excluded States in the control group. Individual controls include indicator variables for race, maternal education, and indicators for female and Hispanic female. State controls include Head Start enrollment, SCHIP/Medicaid eligibility, and economic conditions. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A6: State Pre-K Enrollment in Treatment States 2001-2005

State	State Pre-K Enrollment					Year	Post Pre-K	Pre Pre-K	Enrollment	4-Year-Old
	2001	2002	2003	2004	2005	Implemented	Average	Average	Increase	Population
Arkansas	6	6	6	12	18	2004	12	6	6	36,909
Florida	0	0	0	0	47	2005	47	0	47	194,475
Kansas	6	15	15	15	15	2002	15	6	9	38,444
Louisiana	12	21	22	20	22	2002	22	12	10	64,196
Missouri	5	4	4	4	4	1999	4	0	4	75,416
Nebraska	2	3	4	3	4	2001	3	1^a	2	23,881
New Jersey	20	24	26	26	25	1999	23	9^b	14	114,766
New Mexico	1	1	1	1	7	2005	7	1	6	26,461
New York	25	30	30	29	29	1998	28	4^c	24	256,184
North Carolina	1	6	9	10	12	2002	8	1	7	107,107
Oklahoma	56	59	64	68	70	1998	60	32^d	28	47,075
Pennsylvania	2	2	2	5	6	2004	5	2	3	152,001
Tennessee	2	3	3	3	11	2005	11	3	9	74,575
Vermont	9	10	36	45	47	2003	41	9	32	7,421
West Virginia	24	29	33	35	40	2002	31	24	7	21,141
All Treatment States										1,240,052

Notes: Enrollment rates correspond to years 2001-2005 for each treatment state, from NIEER (2006). The population of 4-year-olds by state corresponds to the information from the 2000 Census (US Census Bureau). *Post Pre-K Average* enrollment is computed as the simple average of the enrollment rates for the years after a state pre-K policy was implemented or expanded, if implemented in or after 2001, or the simple average of years 2001-2005, if implemented before 2001. *Pre Pre-K Average* enrollment is computed as the simple average of the enrollment rates for the years before a state pre-K policy was implemented or expanded with available information, if implemented after 2001. For states with pre-K programs implemented in 2001 or before, the pre-pre-K enrollment rates were computed using information from other sources, as indicated in the note corresponding to each figure.

[a] Information not available.

[b] Pre-K expansion corresponds to program implemented for Abbot Districts, so pre-expansion enrollment was computed by adding Non-Abbot districts' enrollment in 2001 and the estimated Abbot districts enrollment of 4-year-olds in 1998, from...

[c] EPK enrollment in 2001.

[d] From CPS, public preschool: OK 0.23 (93-97) 0.51 (98-02) 0.53 (03-07), so increase of 28 pp from 93-97 to 98-02 [mainly operates in pub sch, so assumption that increase is captured in survey is reasonable].