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The relationship between child care subsidies and children's cognitive development☆

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ABSTRACT

Child care subsidies help low-income families pay for child care while parents work or study. Few studies have examined the effects of child care subsidy use on child development, and no studies have done so controlling for prior cognitive skills. We use rich, longitudinal data from the ECLS-B data set to estimate the relationship between child care subsidy use and school readiness, using value-added regression models as well as parametric and non-parametric models with propensity score matching. Compared to a diverse group of subsidy non-recipients in various types of non-parental care as well as parental care only, we find that child care subsidy use during preschool is negatively associated with children's math skills at kindergarten entry. However, sensitivity analysis suggests that these findings could be easily overturned if unobserved factors affect selection into subsidy receipt.

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1. Introduction

Child care subsidies defray the costs of family-selected early care and education for low-income, employed parents. Each year, over 1.1 million children under age six use child care subsidies provided through the Child Care Development Fund (CCDF) program, at a cost of several billion dollars (Committee on Ways and Means, 2008). With so many children participating and so much public investment at stake, it is important to understand how child care subsidies affect recipients.

Child care subsidies offset all or part of the cost of child care and, thereby, increase the short-run returns to employment for parents in families with low socio-economic status (SES). In the long run, increased levels of employment among low-SES parents with subsidies are expected to result in higher family SES. The primary goal of child care subsidy programs is to increase parent workforce participation, and most of the research on child care subsidies has focused on employment-related outcomes (Herbst & Tekin, 2010a; Zaslow et al., 2002). A sizeable body of research suggests that use of child care subsidies has positive effects on parent workforce participation and family economic outcomes (Blau, 2000; Brooks, Risler, Hamilton, & Nackerud, 2002; Ficano, Gennetian, & Morris, 2006; Joo, 2008; Lemke, Witte, Queralt, & Witt, 2000; Schaefer, Kreader, & Collins, 2006; Tekin, 2007).

However, parents are not the only family members affected by the child care subsidy. The decisions parents make about child care after receiving a subsidy also have the potential to affect children's development, positively or negatively. Subsidies can affect parent decisions about whether to use any non-parental care, and can also affect parents' choice of child care arrangements. These choices may have positive or negative implications for children's cognitive development, to the extent that they affect children's early learning experiences in both parental and non-parental care. The purpose of this study is to estimate the relationship between child care subsidy use during preschool and children's cognitive development by the time they enter kindergarten.

Child care subsidies are payments, usually delivered as a voucher, that help cover the cost of child care for the recipients. Child care subsidy programs are operated by states using a combination of federal and state funds, and states set income eligibility limits, provider reimbursement rates, family co-payment rates, and other regulations in accordance with flexible federal guidelines. A notable feature of child care subsidies is that, in contrast to highly regulated publicly funded early childhood programs such as

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Head Start or state pre-kindergarten, the quality and operational standards for participating providers are quite minimal. Although standards for provider participation vary by state, federal CCDF law prohibits states from imposing standards that would significantly restrict family choices, because the goal of the program is to maximize flexibility in order to meet the needs of low-income working parents (Government Accountability Office [GAO], 2005).

Child care subsidies are available to families who are employed or in training and who meet the state's income requirements. However, subsidies are not an entitlement and eligible families may be denied access to child care subsidies altogether or placed on wait lists. Although child care subsidies serve over one million children under age 5 each year, access rates among eligible families are fairly low, with an estimated 16–20% of eligible children using child care subsidies at a given time (Burstein & Layzar, 2007; Committee on Ways and Means, 2008). Low utilization among eligible families is due partly to state funding levels that limit the supply of child care subsidies (Adams, Snyder, & Sandfort, 2002; Crosby, Gennetian, & Huston, 2005; Herbst, 2008; Witte & Queralt, 2003). However, parents' employment decisions also affect use. As a result, eligible families that use child care subsidies may differ from eligible families that do not in ways that matter for studies focused on estimating the impacts of the subsidies on child outcomes.

Child care subsidies are predominantly used by very poor families. In 2005, the median family income among subsidy recipients was just over $15,000 per year, and just 13% of participating families earned over 150% of the federal poverty level (Child Care Bureau, 2005). A number of family characteristics are predictive of subsidy use among low-income families, including being a single parent, being African American, speaking English at home or being native born, having low income, and being a current or prior recipient of welfare and other means-tested benefits (Adams et al., 2002; Blau & Tekin, 2007; Burstein & Layzar, 2007; Committee on Ways and Means, 2008; Danziger, Ananat, & Browning, 2004; Durfee & Meyers, 2006; Herbst, 2008; Johnson, Martin, & Brooks-Gunn, 2011; Schaefer et al., 2006; Shlay, Weinraub, Harmon, & Tran, 2004; Tekin, 2005, 2007; Weinraub, Shlay, Harmon, & Tran, 2005). There is some evidence of regional differences in subsidy utilization, and urbanicity is also related to subsidy use (Burstein & Layzar, 2007; Johnson et al., 2011; Tekin, 2005, 2007). Evidence is mixed on whether parent education level, parent age, and the number and ages of young children in the home are positively or negatively related to subsidy receipt (Blau & Tekin, 2007; Burstein & Layzar, 2007; Danziger et al., 2004; Herbst, 2008; Shlay et al., 2004; Tekin, 2005, 2007; Weinraub et al., 2005).

In estimating the relationship between child care subsidies and child outcomes, the analyses should control for these known predictors of subsidy receipt in order to reduce bias in the estimates. Welfare receipt is a particularly important control variable because states that cannot serve all eligible applicants for child care subsidies usually give first priority for child care subsidies to TANF recipients (Cohen & Lord, 2005; GAO, 2005). Of course, there may be other, less easily measured differences in families that do and do not receive subsidies, such as differences in the extent to which parents value gainful employment or are able to obtain it, or differences in educational values. Differences of this sort are a particular challenge when studying the effects of child care subsidies on child cognitive development, requiring careful consideration of the study design and the use of analysis methods that can reduce the threat of selection bias.

1.1. The present study

This study estimates the net relationship between child care subsidy use during preschool and children's cognitive skills at kindergarten entry, using the Early Childhood Longitudinal Study – Birth cohort (ECLS-B) data set. We compare child care subsidy recipients to a diverse counterfactual group of subsidy non-recipients that includes children in any type of non-parental care that is not paid for with a child care subsidy, as well as children who do not receive any regularly occurring non-parental care. This counterfactual includes all conditions that child care subsidy recipients might experience in absence of the subsidy. Child care subsidies were initially designed to increase access to child care among families that could not otherwise afford it, so it is important to include children who do not attend child care in the counterfactual in order to estimate the true effect of child care subsidies on children. It is also important to include children in all types of child care in the counterfactual, including public preschool programs such as Head Start or public pre-kindergarten in addition to private child care, since all of these types of care are likely alternatives for children who are similar to child care subsidy recipients but do not receive a subsidy. We use several alternative estimation strategies to account for selection into subsidy use, using an extensive set of control variables that includes prior measures of child development and socio-demographic characteristics of children and families. The study addresses two questions:

1. What is the relationship between child care subsidy use in preschool and children's early literacy skills at kindergarten entry?
2. What is the relationship between child care subsidy use in preschool and children's early math skills at kindergarten entry?

Child care subsidies effectively make child care less costly to parents, so policymakers expect the subsidy to increase family economic resources, and possibly also induce parents to purchase child care that is more expensive, and presumably of better quality, than they otherwise would. As a result, one might expect positive effects on child developmental outcomes, either through better quality care or family resources used in other ways to benefit children. In fact, any expected benefits to child development are dependent upon the assumption that the children will receive better quality care than they otherwise would without the subsidy. However, it is not clear that use of child care subsidies leads to improvements in the quality of care children receive.

Several correlational studies on the quality of subsidized care find that child care subsidy recipients tend to receive relatively poor quality care, and the percentage of children using subsidies in a child care program is negatively related to measures of program quality (Adams, Roach, Riley, & Edie, 2001; Antle et al., 2008; Jones-Branch, Torquati, Raikes, & Edwards, 2004; Mocan, 2007; Raikes, Raikes, & Wilcox, 2005). One recent study (Ryan, Johnson, Rigby, & Brooks-Gunn, 2011) tested the relationship between child care subsidy receipt and the quality of care received by individual children, and found that subsidy recipients choose higher quality child care than similar children who did not receive a subsidy when both home-based care and center-based care are included in the same model. However, the authors also found that subsidy recipients are more likely to used center-based care than non-recipients, and that the overall positive association of subsidy use with care quality is driven by more use of center-based care among subsidy recipients. This may be because center-based care tends to be of higher quality than home-based care in preschool, so that quality is higher overall for subsidy recipients because they use more center-based care. In subgroup analyses, Ryan and colleagues found that subsidy recipients in home-based care have higher quality child care than non-recipients in home based care, whereas subsidy recipients who use center based care actually have *worse* quality child care compared to non-recipients in center-based care.

It seems counterintuitive that parents would choose poorer quality care for their children when using a subsidy than otherwise, particularly in center-based arrangements. However, parents who receive subsidies may have insufficient information to judge program quality, and they are limited to the private care options that are available and able to accept subsidies. Parents who use child care subsidies might not have access to high-quality private child care, and alternative low-cost options (such as Head Start or other publicly funded programs) may be of better quality (Ryan et al., 2011). This is consistent with literature suggesting that the quality of private child care available to poor children, and especially the care that will accept subsidies as payment, tends to be of insufficient quality to lead to positive child outcomes (Adams & Rohacek, 2002; Brady-Smith, Brooks-Gunn, Waldfogel, & Fauth, 2001).

Quality of care is important because researchers find that high-quality care is key in achieving positive developmental outcomes for young children (Barnett, 1995; Gilliam & Zigler, 2000; Love, Schochet, & Meckstroth, 1996; Magnuson, Ruhm, & Waldfogel, 2007; Peisner-Feinberg et al., 2001; Phillips, Voran, Kisker, Howes, & Whitebook, 1994; Shonkoff & Phillips, 2000). A large correlational literature on non-parental care finds that child cognitive and social–emotional outcomes are predicted by program quality, particularly the quality of the educational environment and the closeness and positivity of teacher–child interactions (Belsky et al., 2007; Burchinal et al., 2000; Howes et al., 2008; Love et al., 1996; Mashburn et al., 2008; NICHD ECCRN, 1999, 2002; Peisner-Feinberg et al., 2001). The relatively low quality of care accessed by subsidy recipients in center-based care has worrisome implications for their cognitive outcomes (Adams & Rohacek, 2002). Yet, there is relatively little research on how child care subsidies affect child development (Brooks et al., 2002; Crosby et al., 2005; Herbst & Tekin, 2010a).

1.2. Prior research on child care subsidies and child cognitive development

There is only limited evidence on the relationship between child care subsidy use and children's cognitive outcomes. A large body of research on comprehensive welfare packages, including child care subsidies along with various other work and family supports tied to welfare, suggests that child care subsidies in combination with the other comprehensive family supports can have positive effects on children's cognitive and social–emotional development (Duncan & Chase-Lansdale, 2001; Morris, Huston, Duncan, Crosby, & Bos, 2001; National Forum on Early Childhood Program Evaluation, 2008). These studies all used experimental designs that provide strong causal evidence of the positive impacts of these comprehensive programs on children's development. However, this research does not allow one to disentangle the effects of child care subsidies from the effects of the other comprehensive supports (Blau & Tekin, 2007).

Moreover, although studies described above indicate that welfare receipt increases the likelihood of subsidy receipt and that states prioritize welfare recipients for subsidy receipt (Burstein & Layzar, 2007; Cohen & Lord, 2005; GAO, 2005), recent estimates indicate that only about 18% of families using child care subsidies across the U.S. also receive cash assistance through the TANF program (Committee on Ways and Means, 2008). Thus, the experimental findings from the comprehensive welfare package studies are of limited use in understanding the effects of current child care subsidy policy, which is not directly linked to other welfare package supports.

Very few studies have tested the relationship between the use of stand-alone child care subsidies and children's development. One study comparing cognitive skills between child care subsidy recipients and children on the wait list found no differences in cognitive or social–emotional skills between recipients and non-recipients (Brooks et al., 2002). However, because the data are cross-sectional, the findings do not provide evidence on how subsidies affect children's subsequent development, such as their readiness for school.

An extensive review of the literature identified only one published study (Herbst & Tekin, 2010a) that examines the relationship between standalone child care subsidy use and subsequent cognitive skills, although the same authors also have a working paper available that uses a slightly different methodology to test similar questions (Herbst & Tekin, 2010b). For both papers, the authors use the Early Childhood Longitudinal Study – Kindergarten cohort (ECLS-K) data to study the relationship between subsidy receipt during preschool and school readiness at kindergarten entry across several outcome domains, controlling for a large number of child and family characteristics (but not prior measurements of child skills in the outcome domains of interest). Using instrumental variables methods to control for selection, with different instruments in each paper, Herbst and Tekin find that children who received child care subsidies had lower math and reading skills and worse social–emotional skills in kindergarten than non-recipients. When they use OLS regression models, however, the authors find no difference in cognitive outcomes and fewer significant differences in social–emotional skill areas.

The published paper (Herbst & Tekin, 2010a) uses dummies for the county the child lives in as the instrument, arguing that county-level rationing of child care subsidies predicts who receives a child care subsidy, affecting child outcomes via child care subsidy receipt only. However, there are many ways in which county-level differences in educational resources and social services might affect child cognitive outcomes apart from receiving a child care subsidy. The validity of an instrument relies on the assumption that the variable used as an instrument affects the outcome exclusively through the predictor of interest, also called the exclusion restriction (Reardon, 2011). The authors do include other early childhood policies in the model that might also be related to child outcomes, but the instrument could still be correlated with unobserved determinants of children's skills, such as unmeasured educational resources in the county. If this occurs, the authors' choice of instrument may violate the exclusion restriction.

In the working paper, Herbst and Tekin (2010b) use distance from the county social service agency as the instrument. However, county social service agencies might be located closer to areas with high-risk populations who are at risk of low cognitive skills in kindergarten regardless of subsidy receipt. These concerns are particularly important since the results of the instrumental variables analyses have large effect sizes even while the study design may not adequately account for selection bias. Given these concerns and the conflicting findings using the different analytic strategies in the Herbst and Tekin papers, additional research is needed using a different analytic strategy to account for selection into subsidy receipt. Also, the authors restrict the analyses to single mothers, although this excludes about a third of child care subsidy recipients who have two-parent families. Future research on the effects of child care subsidies should represent the full population of child care subsidy recipients.

This paper tests the relationship between child care subsidy receipt during preschool and children's math and reading outcomes in kindergarten, using analytic methods to address previous gaps in the literature. Our analysis relies upon a completely different set of assumptions to estimate the association between subsidies and child outcomes, and we include child care subsidy recipients from two-parent families as well as single-parent families. We examine the relationship between subsidy receipt and child outcomes using variations of value-added models as well as propensity score matching methods. In addition to using a large number of child and family characteristics as covariates, we also include prior child cognitive scores at multiple time points to control for lagged parental inputs in the cognitive development process – a powerful set of covariates that was not available in the ECLS-K data set used by Herbst and Tekin. Given the conditional nature of causal findings using observational data, we also check the robustness of empirical findings using different methods and data sets.

2. Method

2.1. Participants

To address the research questions presented above, this study used all waves of the Early Childhood Longitudinal Study – Birth cohort (ECLS-B) data. The ECLS-B is a large, longitudinal study of children's early experiences and development that was conducted by the National Center for Education Statistics (NCES). Importantly, given the aims of this study, the ECLS-B contains detailed information on early care experiences and child care subsidy receipt, child cognitive skills before and after preschool, and child and family characteristics and behaviors.

ECLS-B randomly sampled 14,000 children born in 2001 in the United States, producing a nationally representative sample. The sample was drawn from birth certificates using stratification to ensure adequate sample sizes of children from different racial and ethnic backgrounds, and also of twins and children with low birth weights. The sample includes children from diverse socioeconomic backgrounds.

Children were followed over multiple waves of data collection from infancy to kindergarten entry. The ECLS-B includes four rounds of data collection, at roughly 9 months of age (2001–2002), 2 years of age (2003–2004), preschool age (about age 4, 2005–2006), and in kindergarten (at about age 5, in 2006 or in 2007), plus information from the child's birth certificate. In this paper, preschool data refers to the wave of ECLS-B data collected when the children were approximately age 4. In compliance with National Center for Education Statistics policy, all sample sizes reported in this article are rounded to the nearest 50. Of the 14,000 children selected for study participation, approximately 10,700 of the children who participated in the first round of data collection at 9 months constitute the baseline sample for this study. Participation in a study wave is determined by completion of a parent interview in the specified wave, which was required in order to participate in other types of data collection for that wave. Response rates for other data sources and for individual items may differ.

Due to funding constraints, a subsample of approximately 7700 of 9000 children eligible for the kindergarten wave in 2006 was selected for the kindergarten data collection and approximately 7000 completed the kindergarten data collection (Snow et al., 2009). Families were only included in the 2007 kindergarten wave of data collection if the child was repeating kindergarten or if the child was not attending kindergarten during the 2006 wave. Some sample subgroups had slightly higher rates of non-response than others (Snow et al., 2009, Table 141). Kindergarten response rates were positively related to maternal education level, ranging from 88.4% for mothers with less than four years of high school to 94.1% for mothers who completed four or more years of college. American Indian/Alaska Native children and Black Non-Hispanic children also had significantly lower response rates than other racial/ethnic groups, but these differences were small.

2.2. Measures

2.2.1. Cognitive school readiness

The dependent variables used in these analyses are scale scores measuring children's early reading and early mathematics skills at kindergarten entry. The scale scores were constructed by NCES using item response theory (IRT). The early learning domains represented in the early reading scale score for the kindergarten wave include receptive and expressive vocabulary, phonological awareness, knowledge of print conventions, receptive and expressive letter recognition, letter sounds, word recognition, matching words, reading comprehension at several levels (initial understanding, developing interpretation, and demonstrating a critical stance), and English language ability (Najarian, Snow, Lennon, & Kinsey, 2010). The domains represented in the math scale scores include number sense, properties, operations, measurement, geometry and spatial sense, data analysis, statistics, probability, patterns, algebra, and functions (Najarian et al., 2010).

In the fall of their kindergarten year, children in the ECLS-B sample were assessed directly by trained NCES field staff. The cognitive assessment battery comprises a selection of items from a variety of published instruments and previous large-scale early childhood studies. Because IRT adaptive testing was used, children completed different items according to their early performance on the assessment battery in each domain. Instruments from which items were drawn include: the Peabody Picture Vocabulary Test – various forms (PPVT) (Dunn & Dunn, 1981, 1997), Comprehensive Test of Phonological Processing (CTOPP) (Wagner, Torgesen, & Rashotte, 1999), the Pre-Language Assessment Scale 2000 (PreLAS) (Duncan & DeAvila, 1998), and the Test of Early Mathematics Ability-3 (TEMA) (Ginsburg & Baroody, 2003). Items were also drawn from assessments developed for previous large-scale studies, including the Early Childhood Longitudinal Study – Kindergarten class of 1998–1999 (ECLS-K), the Family and Child Experiences Study (FACES), and the Head Start Impact Study (HSIS).

The psychometric report on the ECLS-B kindergarten wave indicates that the cognitive assessment battery had very high internal consistency, with reliability indicated by alpha coefficients ranging from .92 to .93 for the reading assessment IRT-based scores and .92 for the math assessment scores (Najarian et al., 2010). For the kindergarten wave of the ECLS-B, the reading scale score had a mean of 38.60 with a standard deviation of 14.84, and the math scale score had a mean of 40.40 with a standard deviation of 10.56 (Najarian et al., 2010).

2.2.2. Subsidy use

The key independent variable in these analyses is a dichotomous indicator of child care subsidy use during the preschool period. The data were collected during the preschool wave of data collection, as part of the parent survey that was administered by trained ECLS-B assessors using computer-assisted interviewing. The variable is a survey item that asked parents whether they receive help from a social service or welfare agency to pay for the focal child's primary child care arrangement during preschool. Parents were asked this question if they identified any regularly occurring child care arrangement; parents who reported no regular non-parental child care were not asked this question. This measure is similar to those in studies that use the ECLS-K and in the National Survey of America's Families (Blau & Tekin, 2007; Herbst & Tekin, 2010a).

All children with an affirmative response to the parent survey question about child care subsidy use during the preschool wave of data collection are considered "child care subsidy recipients." In our study, non-recipients included all children with a negative response to the above question, as well as all children whose parent skipped this item because they reported using no non-parental child care arrangements. As described above, we included children who were not in non-parental care in the comparison group, as well as those who were, because child care subsidy policy is largely motivated by an interest in helping parents engage in the workforce by offsetting the cost of care. This comparison is qualitatively different from comparing children who use subsidies to children in child care who do not get subsidies; we compared children who used subsidies to pay for all or part of the child care with children

Table 1
Percent of ECLS-B sample receiving a subsidy during preschool, by poverty status.

	Full study sample	Under 185% poverty
Subsidy recipients	4.1%	7.6%
Subsidy non-recipients	95.9%	92.4%
N	5650	2450

Source: Early Childhood Longitudinal Study – Birth cohort
Note: In compliance with NCES policy, all sample sizes have been rounded to the nearest 50. ECLS-B survey weights used.

who either spent no time in child care or, if they did spend time in care, did not use subsidies to pay for it.

Table 1 reports the weighted percentage of respondents in the full ECLS-B sample who used a subsidy for preschool, as well as the weighted percentage of those with family income less than 185% of poverty who used a subsidy. About 4.1% of children in the full ECLS-B sample used in our study reported using subsidies, and about 7.6% of children with family income under 185% of poverty reported using subsidies.

2.2.3. Cognitive development prior to preschool-age experiences

An important covariate in our study was children's cognitive skills prior to the preschool period in which subsidy receipt was measured. The inclusion of repeated measures of prior cognitive skills was expected to reduce selection bias by accounting for differences in the cognitive outcome domains that existed between the subsidy user and comparison groups prior to the period in which subsidy use was measured (Campbell & Stanley, 1963; Shadish, Cook, & Campbell, 2002). In the ECLS-B sample, children's cognitive skills were measured at three points in time prior to kindergarten entry, when the children were approximately 9 months, 2 years, and 4 years old.

The age 4 cognitive assessment items in early reading and early math were drawn from the same instruments as the kindergarten wave items (see above). Early reading domains measured at age 4 were the same as those measured in kindergarten, except that reading comprehension was not assessed at age 4. The early math domains measured at age 4 were a subset of those measured in kindergarten: number sense, geometry, counting, operations, and patterns. As in the kindergarten wave of data collection, IRT-based adaptive testing was used, and we used IRT scale scores in this study. Reliability of the preschool IRT scores is .84 for the early reading assessment, and .89 for the math assessment (Najarian et al., 2010). The reading scale score for the preschool sample has a mean of 25.18 and a standard deviation of 10.07, and the math scale score has a mean of 29.31 and a standard deviation of 9.56 (Najarian et al., 2010).

The 9-month and 2-year measures are based on assessments conducted by trained ECLS-B assessors, as close as possible to the ages of 9 months and 2 years. The mental scale scores for both waves comprise items from a single assessment instrument, the Bayley Short Form – Research Edition (BSF-R), which was developed for the ECLS-B using IRT, as a shortened form of the Bayley Scales of Infant Development, 2nd Edition (BSID-II) (Bayley, 1993). The BSID-II is a widely used measure to assess mental ability of children under age four, with sound psychometric properties (Andreassen & Fletcher, 2007). Difficulty in administrating the entire BSID-II to children participating in the ECLS-B study led NCES to develop a short from of the assessment, using IRT to select items with the best psychometric properties for inclusion in the BSF-R. As in the kindergarten and preschool assessments, adaptive testing was used for the BSF-R. High reliability of the BSF-R mental scores is indicated by internal consistency statistics of .81 for the 9 month wave and .88 for the 2 year wave (Andreassen & Fletcher, 2007).

Measures of children's cognitive skills at 9 months and age 2 were also reported as IRT scale scores. However, the measures were formulated as a single score of children's mental ability rather than as scores specific to domains of early cognition, such as reading and mathematics. The mental ability assessment included problem solving ability, language acquisition, ability to vocalize, memory, habituation, and social skills (Andreassen & Fletcher, 2007). The mental IRT scale scores for the BSF-R were expressed in the full BSID-II metric, which ranges from 0 to 178. The score on the 9-month measure for the full sample has a mean of 74.84 and a standard deviation of 10.07, and the score for the 2-year measure has a mean of 127.09 and a standard deviation of 10.65 (Andreassen & Fletcher, 2007).

2.2.4. Family and child characteristics

In addition to the key independent variable and the covariates described above, we included in our analyses several socio-economic and socio-demographic control variables that were likely to influence children's early reading and math outcomes at kindergarten entry. These data are from a survey interview completed by the parent or primary caregiver during the age 2 data collection, unless otherwise noted. NCES field staff conducted the survey interview in the family's home. Family income was measured as a categorical variable with several income ranges. For analysis purposes, we transformed the categorical measure into a continuous variable by converting range values to the mean of the selected income category. This gave the coefficient on income a natural metric of dollars instead of the marginal effect of a change in the categorical value. Mother's education level was measured as a categorical variable, which we converted to approximations of years of education completed. In this way, the coefficient on mother's education can be interpreted as the marginal change in the dependent variable associated with an additional year of mother's education. We included some binary control variables, including whether the family was headed by a single parent and indicators of social service receipt, including WIC and cash assistance or welfare payments. Another control variable is the mother's age when the child was born, which was drawn from the birth certificate data. The data contain several child-level control variables that were also from the birth certificates, including the child's birth weight, race, and gender. We also controlled for the child's age at the time of the kindergarten cognitive assessment and the year in which the child entered kindergarten.

2.3. Analysis plan

We investigated the relationship between child care subsidy use during preschool and children's math and reading skills at kindergarten entry. The families who chose to use child care subsidies are likely to be different from families who did not use child care subsidies in ways that could affect children's cognitive skills. Therefore, it was critical to use appropriate methods to control for possible selection bias when estimating the relationship between child care subsidies and child outcomes. We used two analytic approaches to minimize the threat of selection bias: (1) multiple linear regressions using different control variable specifications and different subsets of the data and (2) propensity score matching with parametric and non-parametric analysis models. (Rosenbaum & Rubin, 1983). We performed several robustness checks of our basic linear specification for the OLS models, to ensure that the findings were not unduly dependent upon the model assumptions. We also conducted sensitivity analyses to test the magnitude of bias that an omitted variable would need to represent in order to overturn our study findings. We presented findings as unstandardized estimates in the tables, and also included standardized effect sizes in the text. For effect size calculation we used Cohen's *d*, dividing the mean difference estimates by the pooled standard deviation for the sample.

For the multiple linear regression estimation, we used a value-added modeling strategy that accounted for selection into child care subsidy use by controlling for children's cognitive skills prior to the preschool year as well as a rich set of child and family covariates. Our basic estimation equation is:

$$Y_{t+1} = \alpha S_t + \gamma Y_t + \beta X_{t-1} + \varepsilon$$

where the outcome variable, Y_{t+1}, is a test score at kindergarten entry. The variable S_t is a binary variable that takes value 1 for subsidy recipients and value 0 for non-recipients. The main parameter of interest is α. We added controls for lagged cognitive skills, Y_t, and lagged family and child characteristics, X_{t-1}.

We estimated the relationship between child care subsidies and the math and reading outcomes in three models with increasing control variables, all using the full sample of children in the ECLS-B. However, a more relevant sample for estimating this relationship would include children in families that were in the target range of income eligibility for child care subsidies. To approximate this comparison, we also estimated the full controls model on a more restricted subsample of children with family incomes under 185% of the poverty line. Income under 185% of the poverty line is very close to the average state cutoff for income eligibility nationwide, and we considered it to be a rough approximation of subsidy eligibility. Still, about 50 out of the 250 subsidy recipients in the full study sample had incomes above this threshold and were excluded, indicating that the results for the under 185% poverty subsample do not generalize to all subsidy recipients. For this reason, we included the results for the full ECLS-B sample as well as the sample of children with family income under 185% poverty.

In our main OLS analyses, we compared children who received child care subsidies to all other children who did not use a child care subsidy. However, this comparison may mask important differences in the educational supports that were available to children in the counterfactual condition. In particular, Head Start and state pre-K programs are highly structured public programs for disadvantaged children, which target similar children to the child care subsidy program but have a stronger focus on enhancing child development. Including children in these child development-focused programs may mask the relationship between subsidy receipt and child cognitive outcomes in the absence of such programs (Ryan et al., 2011). As an additional robustness check, we restricted the sample to children who did not use Head Start or public school pre-K services. This restricted sample was not used as our main analysis sample because child care subsidies may induce families to select a private child care arrangement instead of these publicly funded educational programs, so it was important to include children in alternative publicly funded early childhood programs in the counterfactual for the main analysis.

An additional analytic concern is that value-added models with lagged variables place significant restrictions on the timing of inputs and how lagged variables enter the model (Todd & Wolpin, 2003). As described in the measures section above, prior cognitive skills were measured at 2 years of age and 9 months of age with a test of general cognitive skills, and at the start of preschool with assessments of early reading and early math skills that are vertically aligned with the kindergarten reading and math scores. For these analyses, it was useful to include the vertically aligned math and reading scores at the start of preschool to help control for prior influences on cognitive skills that might otherwise bias the coefficient estimate for subsidy receipt, but may not be captured by the age-2 assessment or by the general mental skills measure used at that age. However, subsidy receipt and preschool cognitive skills were measured contemporaneously, presenting some concern that the preschool cognitive scores already had captured some of the effect of the child care subsidy. Unfortunately, subsidy receipt was measured for a specific point in time, without an indication of duration of use, so we do not know how long families had received the subsidy at the measurement time point.

Whether the inclusion of preschool cognitive skills as a covariate introduces bias into the estimate depends on what the subsidy variable measures. If the subsidy variable captures subsidy receipt from the time of the preschool measurement onwards, then the coefficient on subsidy receipt represents the relationship with the outcome from the point of time at which the preschool cognitive skill is measured. So even though the two variables are measured contemporaneously, the estimated parameter represents the relationship with subsequent subsidy use over the preschool year.

If the subsidy variable measured subsidy use that also occurred prior to preschool, then the subsidy may affect preschool cognitive achievement through the same mechanism through which it affects kindergarten cognitive achievement. In this case, the coefficient on subsidy receipt in a value-added model captured the effect of subsidy use on kindergarten outcomes net of preschool outcomes. But this was essentially the same parameter described in the case that subsidy receipt captures subsequent subsidy use. So, although subsidy use may have occurred prior to preschool, by adding the preschool score to the model we would interpret the effect as the subsequent effect of subsidy use on kindergarten outcomes. In addition, although the subsidy may have influenced cognitive development prior to the preschool year, we would still expect the most important period captured in the subsidy coefficient to occur during the preschool year, after the measurement of subsidy receipt.

For the main OLS analysis, we included the preschool cognitive scores as control variables because the potential bias due to unmeasured confounders that would be captured in the preschool math and reading scores was very important to control for, and accounting for that potential bias outweighed the potential bias in the coefficient due to the contemporaneous measurement. However, we included two alternatives to the main specification as robustness checks to address this issue. First, we removed the "lagged" test scores from the model, which addressed the problem of potential bias in the estimate if previous subsidy use influenced preschool cognitive achievement. Second, we estimated the model on a subset of children that did not receive a subsidy in either of the previous data waves, which meant that the subsidy variable was less likely to capture subsidy use prior to preschool. As shown below, neither of these two robustness checks changed our main conclusions.

The value-added OLS models controlled for a number of important covariates, and should therefore largely account for the issue of selection bias in the estimates. However, the OLS estimates may not be reliable if children receiving subsidies differed greatly from children who were in the comparison group, with little overlap in each group's distribution on covariates. Propensity score matching can address this issue by creating a comparison group that is similar to subsidy recipients across a range of covariates. OLS estimates also rely on linearity and additivity assumptions that are difficult to test with many covariates, whereas analytic models using propensity score matching rely on fewer assumptions about functional form (Hill, Waldfogel, Brooks-Gunn, & Han, 2005; Magnuson et al., 2007). Therefore, we also tested our research questions using propensity score matching methods to create a comparison group that is more closely matched with the subsidy users across predictor variables.

For the analyses with propensity score matching, we matched each child who received a child care subsidy to one child who did not receive a subsidy, but had a similar likelihood of doing so according to the propensity score. To calculate the propensity score, we ran a logistic regression of subsidy receipt on the same set of control variables that were included in the multiple regression analysis, which were important predictors of child care subsidy receipt as indicated in our literature review. For the main propensity score analyses, we did not include the preschool math and reading scores in the propensity score calculation because of particular concerns about matching on contemporaneous measures, although we did include the cognitive skills measures at ages 2 years and 9 months. However, we ran secondary analyses as a robustness check with matched groups that included the powerful preschool cognitive skills covariate in the propensity score calculation.

Because the propensity score matching process matched children on socio-economic as well as other factors, we used the full data set to run the model, rather than the subset of low-income families. Prior to running the propensity score matching algorithm, we compared the overlap in the propensity score distributions in the subsidy recipient and non-recipient groups to ensure that there was common support, so that matching would occur within overlapping ranges. We trimmed the data to ensure that the minimum and maximum propensity scores in both groups were very close in size, and therefore had common support.

States and regions of the country vary in their implementation of child care subsidy policy and also the early care and education landscape more generally. State-level information is available in the ECLS-B data set, so state-level indicators could be conceivably have been added to both the OLS and propensity score models. However, the estimated models would then rely on within-state variation in subsidy use to identify the impact of subsidies on outcomes. The National Center for Education Statistics (NCES) advises that the ECLS-B study was not powered for state-level analyses, so we elected to not use state-level indicators in any of the models to avoid relying on the within-state variation in our analyses. As an alternative, in order to address potential regional variation in early care and education, the propensity score matching procedure matched subsidy recipients and non-recipients within region of the country (divided into four regional groups: the South, the Northeast, the Midwest, and the West).

To create the matched comparison group for the main analysis, we used the global optimal propensity score paired-matching methods (Ming & Rosenbaum, 2001). This matching algorithm paired each subsidy recipient in the data set with one non-recipient within the same region of the country who had not yet been matched, with the requirement that the selected matched pair would result in the smallest overall differences in propensity scores in the data set of children within the region, in comparison to all possible remaining matches. As in the OLS regression models, the non-recipient counterfactual group included children in diverse care arrangements, including parental care only and all types of non-parental care arrangements. The OLS regression models were run with a restricted sample of children with family income under 185% of poverty, to ensure that the comparison group of non-recipients was comparable to subsidy recipients. Such restrictions are not necessary with propensity score matching methods, which are designed to create a highly similar comparison group by selecting non-recipients who are similar to subsidy recipients on family income level and other factors.

To assess the quality of the match, we compared descriptive statistics for the subsidy recipients and non-recipients in the sample of children matched with propensity scores, to ensure that there were no significant differences between groups on these characteristics after matching. We also presented the standardized mean differences between subsidy recipients and non-recipients on each of the covariates, before (Table 2) and after (Table 5) matching. The standardized mean differences were calculated using the Hedges' g statistic (Hedges, 1981), and indicate the difference in group means in standard deviation units. For example, a standardized mean difference of .10 indicates that the mean of the subsidy recipient group is .10 of a standard deviation larger than the mean of the matched non-recipient group. The standardized mean difference for each covariate should be less than .25 after matching (Stuart, 2007), or preferably less than .10.

Using the propensity score matched sample, we estimated the relationship between subsidy receipt and cognitive outcomes using both a parametric method (regression) and a non-parametric method (Hodges–Lehmann estimate based on Wilcoxon signed rank test with covariance adjustment) to allow for non-normal distribution of children's scores on the kindergarten assessments and to account for outlier observations. The regression method used within-pair differences in the same control variables as our regression model. The Hodges–Lehmann estimate is the median of all the possible contrasts between the subsidy recipients and non-recipients using the propensity score matched sample.

Propensity score analysis produces an unbiased estimate if there are no unmeasured confounders with the subsidy receipt measure. However, it relies on the untestable assumption that only observed variables affect selection into the key independent variable of interest, in this case, subsidy receipt (Hill et al., 2005; Zhai, Brooks-Gunn, & Waldfogel, 2011). Although selection on unobservable characteristics cannot be directly tested, sensitivity analysis can indicate about how large hidden biases would need to be in order to substantively alter the findings from an impact analysis using propensity score methods (Rosenbaum, 2002).

The sensitivity parameter Γ represents the odds ratio of receiving a child care subsidy between treatment and comparison subjects who are matched on the set of covariates included in the propensity score matching model. If subsidy receipt is random between the treatment and comparison groups after matching on the observed covariates, the value of Γ is 1 and the study is free from selection bias. If Γ is greater than 1, there is some hidden bias in treatment assignment that was not captured by the matching variables. The actual value of Γ is unknown. Sensitivity analysis indicates the size of Γ that would be required to overturn the findings of the analysis and to indicate no significant difference in kindergarten outcomes between subsidy recipients and non-recipients. The results of this analysis indicate whether the findings of the analysis would be easily overturned by omitted variable bias. We conducted a sensitivity analysis with the Wilcoxon signed rank test, using methods recommended by Rosenbaum (2002).

3. Results

3.1. Descriptive statistics

Table 2 provides means or percentages and standard deviations for the variables used in the analyses for all children and by subsidy receipt status, both for the full ECLS-B sample and for the subsample of children with family incomes under 185% of poverty (referred to as low-income children in this study). The table indicates whether mean differences between subsidy recipients and non-recipients are statistically significant, and also presents the standardized mean differences.

The cognitive scores in Table 2 include the kindergarten math and reading outcome scores, as well as the lagged preschool math and reading scores and children's general cognitive scores (Bayley's Mental score) at age 2 and 9 months. In the full sample of children, child care subsidy recipients had significantly lower math and reading scores in preschool and kindergarten than non-recipients, and significantly lower general cognitive scores at age 2, although there was no difference in cognitive scores at 9 months. For the subsample of low-income children, in contrast, the cognitive skills

Table 2
Summary statistics of the analysis variables by subsidy receipt and poverty status, with standardized mean differences.

	Full sample				Under 185% poverty			
	(1) All children	(2) Subsidy recipients	(3) Subsidy non-recipients	(4) Standardized mean difference by subsidy receipt	(5) All children	(6) Subsidy recipients	(7) Subsidy non-recipients	(8) Standardized mean difference by subsidy receipt
Dependent variables								
Kindergarten math score	44.74	40.53	44.92**	−0.31	41.00	39.35	41.14	−0.14
	(14.05)	(13.28)	(14.02)		(12.96)	(12.56)	(12.97)	
Kindergarten reading score	44.76	39.53	44.98**	−0.27	40.07	38.21	40.22	−0.11
	(19.96)	(18.74)	(19.94)		(18.19)	(17.17)	(18.23)	
Pre-test measures of cognitive skills								
Preschool math score	29.53	26.71	29.65**	−0.22	25.79	25.35	25.83	−0.04
	(13.58)	(12.71)	(13.59)		(12.18)	(10.87)	(12.26)	
Preschool reading score	25.40	22.10	25.54**	−0.24	21.59	20.54	21.67	−0.10
	(14.27)	(11.87)	(14.31)		(11.58)	(8.97)	(11.72)	
2 years mental score	127.95	126.94	127.99**	−0.07	125.21	126.16	125.14	0.07
	(14.95)	(12.84)	(15.03)		(14.06)	(11.98)	(14.19)	
9 months mental score	77.03	77.14	77.02	0.01	76.54	76.52	76.54	0.00
	(14.93)	(13.86)	(14.97)		(13.64)	(11.75)	(13.76)	
Child characteristics								
Birth weight[a]	3.33	3.17	3.33**	−0.22	(3.26	3.16	3.27*	−0.15
	(.73)	(.83)	(.73)		(.72)	(.80)	(.71)	
Percent male	50	50	50	0.00	51	50	51	−0.01
	(73)	(72)	(73)		(70)	(71)	(70)	
Percent black[b]	14	37	13**	0.57	23	41	21**	0.38
	(43)	(66)	(41)		(52)	(67)	(51)	
Percent Hispanic[b]	15	15	15	0.00	20	17	21	−0.07
	(51)	(52)	(51)		(58)	(56)	(58)	
Percent other race[b]	7	9	7	0.07	6	11	6	0.18
	(29)	(41)	(28)		(27)	(46)	(25)	
Percent started K in 2007	26	32	26	−0.38	25	35	24*	0.17
	(67)	(70)	(67)		(64)	(70)	(64)	
Age (months) of K assessment	68.15	68.27	68.15	0.02	68.01	68.52	67.97	0.09
	(6.55)	(6.58)	(6.55)		(6.38)	(6.24)	(6.38)	
Family characteristics at age 2								
Family income[c]	5.49	2.68	5.61**	−0.44	2.54	2.03	2.58**	−0.19
	(6.69)	(4.07)	(6.71)		(2.88)	(2.18)	(2.91)	
Percent welfare recipients	7	27	6**	0.59	14	33	13**	0.42
	(36)	(63)	(34)		(48)	(67)	(46)	
Percent WIC recipients	39	67	38**	0.41	69	74	69	0.08
	(70)	(71)	(70)		(66)	(64)	(66)	
Percent single parents	20	58	19**	0.70	35	63	32**	0.48
	(57)	(71)	(55)		(66)	(68)	(65)	
Mother's years of education	13.42	12.54	13.46**	−0.26	11.86	12.33	11.82**	0.20
	(3.61)	(2.18)	(3.64)		(2.57)	(2.08)	(2.59)	
Mother's age at child's birth	27.38	23.94	27.53**	−0.40	24.96	23.93	25.04*	−0.13
	(8.93)	(7.68)	(8.92)		(8.27)	(7.23)	(8.33)	
Percent speak English at home	85	91	84**	0.14	78	91	76**	0.26
	(50)	(40)	(51)		(58)	(41)	(59)	
N	5650	250	5400	–	2450	200	2250	–

Note: Columns 4 and 8 report standardized mean differences between subsidy recipients and non recipients, and statistical significance of the mean differences is indicated in columns 3 and 7.

[a] Birthweight is measured in 1000 g units.
[b] Referent group is white.
[c] Income is measured in $10,000 units.
* $p < 0.05$.
** $p < 0.01$.

of subsidy recipients appeared lower than those of non-recipients in preschool and kindergarten, but the differences were not statistically significant.

Table 2 also presents summary statistics for child demographic characteristics and family socio-economic characteristics by subsidy receipt status for the full sample and the under 185% of poverty sub-sample. In the full sample, subsidy recipients were very disadvantaged in comparison to non-recipients, with much lower family incomes, much higher rates of welfare and WIC receipt, and somewhat lower maternal education levels. Subsidy recipients were younger than non-recipients and much more likely to be single parents. Children who received subsidies tended to have lower birth weights and were much more likely to be black, non-Hispanic. They were more likely to speak English at home than non-recipients, possibly because subsidies were difficult for parents to obtain if they were born outside of the U.S.

In the sample of children under 185% of poverty, most of these differences between subsidy recipients and non-recipients persisted but were mostly smaller in magnitude. The difference in average family income was smaller than in the full sample, but was still sizeable in the under 185% of poverty subsample. While maternal education levels were lower for subsidy recipients than non-recipients in the full ECLS-B sample, the subsidy recipients actually had higher education levels than non-recipients in the low-income sample. This may be because of the work requirement for the subsidy, which would be easier for mothers to meet if they have completed more years of schooling. Also, the gap in English-speaking households was larger between subsidy recipients and non-recipients in the low-income sample. The many differences by subsidy receipt status in these covariates suggests that differing family backgrounds may account for some of the gap in outcome scores. Therefore, it was important to control for these variables in the prediction models.

3.2. Regression results

Table 3 presents the results of four OLS regression models that tested the relationship between child care subsidy use in preschool and children's math and reading skills in kindergarten, with each successive model including additional control variables or sample restrictions. We first ran the unadjusted differences between subsidy recipients and non-recipients and gradually added more controls for the full sample of children. The difference in the estimated relationship between subsidy use and child outcomes from one model to the next provides an indication of the extent of selection bias that was controlled for by the addition of control variables. The first three models in Table 3 include all students in the study sample and the fourth is restricted to the low-income sample of children under 185% of poverty.

Model 1 estimated the average differences in kindergarten math and reading scores that were predicted by preschool subsidy receipt alone, and found a large and significant negative coefficient on subsidy receipt for both reading and math outcomes. Without controlling for any other possible explanatory factors, children who used child care subsidies during preschool had significantly lower math ($d = .31$) and reading skills ($d = .27$) at kindergarten entry than children who did not use subsidies.

Model 2 presents a simple value-added estimate of the average difference in math and reading scores associated with preschool subsidy receipt, controlling for preschool reading and math skills but no other covariates. The results of Model 2 indicate that adding lagged test scores to the outcome equation reduced the associations between subsidy receipt and cognitive outcomes by 44% for math (from −4.39 to −2.44 scale score points) and 52% for reading (from −5.46 to −2.61 scale score points). The lagged test scores were a powerful covariate, and it was important to include those scores in models estimating the association between child care subsidies and children's later math and reading skills.

Model 3 added the full set of child and family covariates to the value-added estimate, for the full ECLS-B sample. Adding the covariates further reduced the size of the subsidy coefficient, suggesting that these child and family characteristics (measured prior to preschool) accounted for important selection effects not captured by the lagged test scores. After controlling for these myriad child and family characteristics, there was still a statistically significant negative mean difference between the test scores of children who received subsidies and those who did not (−1.62 scale score points in math, $d = .12$, and −1.87 scale score points in reading, $d = .09$).

The first three models in Table 3 compared children receiving subsidies to children who did not receive subsidies in the full ECLS-B sample. Model 4 in Table 3 re-estimated the value-added model with full controls, in a subset of the data restricted to children whose families had incomes less than 185% of the poverty level. As described previously, this low-income subsample closely approximates the universe of families who were statutorily eligible for subsidies. The results of Model 4 indicate that child care subsidy receipt was still significantly negatively related to math and reading outcomes among children with family income under 185% of poverty, and the magnitude of the estimated relationship was slightly larger than in the full sample. For this restricted sample, subsidy receipt was associated with 1.96 scale score points (.15 standard deviations) lower average scores on the pre-kindergarten math assessment and 2.09 scale score points (.11 standard deviations) lower average scores on the pre-kindergarten reading assessment. As a point of comparison, the black–white school readiness gap in the ECLS-B is approximately .4 standard deviations for both math and reading in kindergarten, so the effect size on subsidy receipt is approximately 25% of the unadjusted black–white school readiness gap.

In Table 4, we compared the results of the base OLS model findings presented in Table 3 with the results from three additional robustness checks. The top row in Table 4 provides the subsidy receipt coefficients from each of the base models represented in Table 3, and the subsequent rows present the findings from alternative model specifications for each of the base models. The findings had similar magnitudes to the main results in Table 3, including relatively similar coefficient estimates and effect sizes.

The estimated coefficient on subsidy receipt reported in the second row of Table 4 was based on the same four model specifications used for the findings reported in Table 3, but we restricted the sample by excluding children who received Head Start or public school pre-K in preschool. Without control variables, the difference between subsidy recipients and non-recipients was much larger in this subsample. However, after adding our full set of control variables, the estimated coefficients again became fairly similar to the estimates for the main analysis model. The effect sizes were slightly larger for math ($d = .15$ in the full sample and .21 in the low-income sample) and slightly smaller for reading ($d = .07$ in the full sample and .10 in the low-income sample) than in the main analysis model. The coefficient on subsidy receipt was still highly significant for math outcomes in the subsample of children who did not receive Head Start or public school pre-K, but was not statistically significant for reading outcomes.

The third row of Table 4 presents the model with the sample restricted to those children whose parents reported not having received a child care subsidy in either of the first two waves of data collection (administered at 9 months and at 2 years). All of the estimated coefficients were negative and statistically significant, and they were larger than the corresponding estimates in the base model that included children who received subsidies in previous waves in the analysis. The effect sizes were also slightly larger for the analysis model with full controls, both for math ($d = .13$ in the full sample and .16 in the low-income sample) and for reading ($d = .13$ in the full sample and .15 in the low-income sample). As noted above, this restriction of the study sample mitigates the concern that the subsidy variable might have captured a combined effect of receiving a subsidy for multiple periods.

The fourth row presents estimates for the main analysis models with the full set of control variables excluding preschool reading and math scores. Preschool reading and math scores were highly predictive of kindergarten scores in these domains, so we expected to find larger coefficients and effect sizes for subsidy receipt after excluding the preschool scores. In fact, we found little difference in the magnitude of the estimates in the full analysis sample ($d = .11$ for math and .10 for reading), although the reading estimate was no longer statistically significant at the .05 alpha level without the preschool covariates in the model. However, in the low-income sample, the negative relationship between subsidy receipt and cognitive outcomes was a bit stronger (.16 for math and .14 for reading) than in the main analysis model.

Based on the results of the robustness checks, the conclusions of our OLS results did not change importantly in any of our alternative estimations. The negative association between subsidy receipt and children's math skills in kindergarten was highly robust to changes in the model specifications and estimation samples. The negative association between subsidy receipt and reading outcomes was

Table 3
Results of OLS regressions of subsidy receipt on kindergarten math and reading scale scores, main results with covariates.

	Full sample						Under 185% poverty	
	(1) No control variables		(2) Only preschool cognitive controls		(3) Full set of control variables		(4) Full set of control variables	
	Math score	Reading score	Math score	Reading score	Math score	Reading score	Math score	Reading score
Preschool subsidy receipt	−4.39** (0.86)	−5.46** (1.21)	−2.44** (0.72)	−2.61** (1.01)	−1.62** (0.58)	−1.87* (0.87)	−1.96** (0.66)	−2.09* (0.93)
Pre-test measures of cognitive skills								
Preschool math score	–	–	0.45** (0.026)	0.36** (0.035)	0.46** (0.023)	0.39** (0.033)	0.46** (0.035)	0.41** (0.052)
Preschool reading score	–	–	0.18** (0.024)	0.52** (0.035)	0.13** (0.021)	0.47** (0.033)	0.15** (0.033)	0.43** (0.057)
2 years mental score	–	–	–	–	0.090** (0.015)	0.048* (0.022)	0.098** (0.025)	0.050 (0.032)
9 months mental score	–	–	–	–	0.0020 (0.014)	−0.010 (0.021)	0.0066 (0.020)	0.0021 (0.030)
Child characteristics								
Birth weight[a]	–	–	–	–	0.54** (0.18)	0.069 (0.28)	0.62* (0.27)	0.083 (0.42)
Percent male	–	–	–	–	0.57** (0.26)	−0.63 (0.39)	−0.029 (0.40)	−0.91 (0.59)
Percent black[b]	–	–	–	–	−0.73* (0.35)	1.40* (0.55)	−0.42 (0.49)	1.35 (0.74)
Percent Hispanic[b]	–	–	–	–	−0.23 (0.39)	0.39 (0.64)	−0.16 (0.57)	0.25 (0.84)
Percent other race[b]	–	–	–	–	0.20 (0.42)	1.19 (0.69)	0.10 (0.57)	0.71 (0.98)
Percent started K in 2007	–	–	–	–	5.23** (0.49)	5.83** (0.73)	4.55** (0.75)	4.85** (1.11)
Age (months) of K assessment	–	–	–	–	0.46** (0.049)	0.67** (0.072)	0.53** (0.077)	0.72** (0.11)
Family characteristics at age 2								
Family income[c]	–	–	–	–	0.039 (0.036)	0.041 (0.059)	−0.0024 (0.094)	0.057 (0.16)
Percent welfare recipients	–	–	–	–	−0.65 (0.55)	−2.24** (0.78)	−0.67 (0.59)	−1.83* (0.82)
Percent WIC recipients	–	–	–	–	−0.45 (0.32)	−0.14 (0.51)	0.045 (0.44)	0.41 (0.69)
Percent single parents	–	–	–	–	−0.64 (0.35)	−1.14* (0.57)	−0.52 (0.45)	−0.88 (0.69)
Mother's years of education	–	–	–	–	0.37** (0.067)	0.52** (0.11)	0.49** (0.12)	0.98** (0.18)
Mother's age at child's birth	–	–	–	–	0.0052 (0.024)	−0.024 (0.039)	0.021 (0.035)	0.012 (0.055)
Percent speak English at home	–	–	–	–	−0.19 (0.39)	−1.44* (0.63)	−0.53 (0.56)	−2.14* (0.88)
Constant	44.9** (0.19)	45.0** (0.27)	27.0** (0.54)	21.1** (0.72)	−23.4** (3.76)	−36.4** (5.69)	−31.6** (5.98)	−46.6** (8.45)
N	5650	5650	5650	5650	5650	5650	2450	2450
r^2	0.008	0.006	0.348	0.339	0.556	0.493	0.466	0.405
Cohen's *d*	0.31	0.27	0.17	0.13	0.12	0.09	0.15	0.11

Note: Coefficients are unstandardized and include standard errors in parentheses.
[a] Birthweight is measured in 1000 g units;
[b] referent group is white;
[c] income is measured in $10,000 units
* $p < 0.05$.
** $p < 0.01$.

somewhat more variable across models, with a loss of statistical significance in some models. However, the size of the estimate on the reading outcome did not change that much, and was fairly small in most models.

3.3. Propensity score matching results

The results of analyses using a comparison sample matched with propensity scores were generally consistent with the OLS regression results presented above. In all propensity score analysis models, we found a significant negative relationship between subsidy use in preschool and children's math skills at kindergarten entry. We also found negative associations between subsidy use and reading outcomes in all models, but the relationship was not statistically significant in the models that included the preschool cognitive scores from the model, while it was highly significant when the preschool covariates were excluded. The results of the main analysis models, excluding the preschool covariates, are presented in Tables 5, 7 and 9. The results of the robustness check models, which included the contemporaneous preschool cognitive covariates in the propensity score calculation, are presented in Tables 6, 8 and 10.

Table 4
Results of OLS regressions of subsidy receipt on kindergarten math and reading scale scores, with alternative specifications and estimation samples.

	Full sample						Under 185% poverty	
	(1) No control variables		(2) Only preschool cognitive controls		(3) Full set of control variables		(4) Full set of control variables	
	Math score	Reading score	Math score	Reading score	Math score	Reading score	Math score	Reading score
1. Main results[a]	−4.39** (0.86)	−5.46** (1.21)	−2.44** (0.72)	−2.61** (1.01)	−1.62** (0.58)	−1.87* (0.87)	−1.96** (0.66)	−2.09* (0.93)
2. Sample excludes children who attend Head Start or public preschool	−7.19** (1.11)	−8.23** (1.58)	−3.90** (0.93)	−2.99* (1.34)	−2.16** (0.79)	−1.50 (1.20)	−2.73** (0.99)	−1.84 (1.31)
3. Sample excludes children who received a subsidy prior to preschool	−4.56** (1.17)	−5.89** (1.51)	−3.07** (0.96)	−3.58** (1.23)	−1.86* (0.73)	−2.54* (1.04)	−2.13* (0.85)	−2.69* (1.07)
4. Models exclude preschool math and reading scores	–	–	–	–	−1.54* (0.70)	−1.97 (1.03)	−2.10** (0.77)	−2.50* (1.04)

Note: This table only shows the coefficients for the child care subsidy variable for each alternative specification or estimation sample, although some models include other covariates as indicated in the column headings for each type of regression model. The full list of covariates for each regression model are listed in Table 3. Coefficients are unstandardized and include standard errors in parentheses.

[a] Main results, also reported in Table 3.
* $p < 0.05$.
** $p < 0.01$.

The matching process successfully created highly similar matched groups of children who did and did not receive child care subsidies, both in the main analysis model without preschool cognitive skills and in the robustness check model that included the preschool scores. All child care subsidy recipients were included in the propensity score matched sample, indicating that highly similar comparison cases were found for each subsidy recipient in the full sample. Table 5 presents descriptive statistics for the groups of subsidy recipients and non-recipients after propensity score matching was completed for the main analysis model, and indicates that we achieved the desired outcome of no significant differences on any of the covariates after propensity score matching. The comparison group of subsidy recipients was more closely matched to subsidy recipients than the under 185% of poverty sub-sample used in the OLS regression models, even on the family income variable, without losing any of the subsidy recipients from the sample (in contrast, the 185% poverty subsample lost approximately 20% of the subsidy recipients from the sample, as described above). This was also the case in Table 6, for the robustness check model. For the main analysis, Table 5 also shows that all of the standardized mean differences between subsidy recipients and non-recipients after matching were smaller than .10, except for the percentage of males, with a difference of .14. For the robustness check model, in Table 6, all of the standardized mean differences were below .10, indicating a very good match. Comparing these small standardized mean differences to those before matching in Table 2, we see that the global optimal matching procedure was successful at finding a comparison group that was much more similar to subsidy recipients in terms of socio-economic disadvantage and child demographic characteristics than the low-income sample in the full ECLS-B.

Tables 7 and 8 display parametric and non-parametric estimates of the relationship between preschool subsidy receipt and kindergarten math and reading scale scores in the propensity score matched comparison groups, for the main analysis model and the robustness check model that included preschool math and reading scores. For math, the results were consistent across parametric and non-parametric analyses, and across the main analysis without the preschool cognitive covariates and the robustness check analysis that includes them. In the main analysis for math, shown in Table 7, subsidy recipients scored −1.99 scale score points lower for the parametric regression analysis ($d = .21$) and −2.16 points lower for the Hodges–Lehmann non-parametric estimate ($d = .23$). The coefficients for subsidy receipt were smaller in the robustness check analysis for the math outcome, shown in Table 8, with effect sizes of −.15 for the parametric analysis and −.16 for the non-parametric analysis.

For the reading outcomes, the propensity score matching analyses resulted in statistically significant coefficients for subsidy receipt in the main analysis that excluded preschool cognitive covariates, but in the robustness check analysis the coefficients were not statistically significant at the .05 alpha level. In the main analysis, shown in Table 7, the estimate for reading was −2.65 for the parametric regression ($d = .20$), and −2.74 for the Hodges–Lehmann ($d = .20$). However, in the robustness check analysis that included preschool cognitive scores, the effect sizes were much smaller, −.11 for the parametric and −.09 for the non-parametric analysis, and were not statistically significant. The greater variability of findings for the relationship between subsidy receipt and the reading outcome is similar to the results of the OLS analyses.

Finally, Tables 9 and 10 present the sensitivity intervals around the estimated average impacts of the association between subsidy receipt and math and reading scale scores, for the main analysis model and the robustness check model. The tables include sensitivity intervals for potential values of Γ (the sensitivity parameter representing the odds ratio of receiving a child care subsidy between treatment and comparison subjects who are matched on the set of covariates included in the matching model). For the main analyses that excluded the preschool cognitive covariate, the sensitivity intervals indicate that there was a significant difference in math and reading outcomes at kindergarten entry if Γ had a value of 1 in our research, indicating no hidden bias in treatment assignment. However, if the sensitivity parameter Γ was equal to or bigger than 1.12 for reading or 1.15 for math (i.e. if an unobserved confounder led to an odds ratio of 1.12 or 1.15 for subsidy receipt

Table 5
Summary statistics of the analysis variables by subsidy receipt status after propensity score matching with standardized mean differences, without preschool cognitive covariates.

	(1) Subsidy recipients	(2) Subsidy non-recipients	(3) Standardized mean difference by subsidy receipt status
Pre-test measures of cognitive skills			
2 years mental score	124.12	124.79	−0.07
	(10.03)	(10.20)	
9 months mental score	74.60	75.20	−0.06
	(10.05)	(9.51)	
Child characteristics			
Birth weight[a]	2.76	2.79	−0.03
	(0.88)	(0.83)	
Percent male	48	55	0.14
	(50)	(50)	
Percent black[b]	39	36	0.07
	(49)	(48)	
Percent Hispanic[b]	20	23	−0.09
	(40)	(42)	
Percent other race[b]	15	15	0.01
	(36)	(36)	
Percent started K in 2007	26	24	0.04
	(44)	(43)	
Age (months) of K assessment	68.20	68.28	−0.02
	(4.28)	(4.26)	
Family characteristics at age 2			
Family income[c]	2.93	3.11	−0.06
	(3.29)	(3.34)	
Percent welfare recipients	25	29	−0.08
	(44)	(45)	
Percent WIC recipients	71	71	0.00
	(46)	(46)	
Percent single parents	53	54	−0.03
	(50)	(50)	
Mother's years of education	12.85	12.81	0.01
	(1.97)	(2.27)	
Mother's age at child's birth	24.58	24.41	0.03
	(6.33)	(6.09)	
Percent speak English at home	93	94	0.03
	(26)	(24)	
N	250	250	–

Note: The estimates in the first two columns report either the mean or percentage by subsidy receipt status with standard deviations, after propensity score matching without the preschool cognitive covariates. The estimates are not weighted because the trimmed sample implies that the weights are no longer correct, and the sample is a subset of the full ECLS-B data reported in Table 2. Standardized mean differences between subsidy recipients and non-recipients are reported in column 3, and statistical significance of the mean differences is indicated in column 2 (note that there are no significant mean differences on any of the covariates).

[a] Birthweight is measured in 1000 g units.

[b] Referent group is white.

[c] Income is measured in $10,000 units.

* $p < 0.05$.

** $p < 0.01$.

among children matched on propensity scores from our model), then the estimated impact of subsidy receipt would not be statistically significant for either math or reading outcomes. For the robustness check analysis that included preschool reading scores, the 95% sensitivity interval covered zero even if there was no hidden bias ($\Gamma = 1$). Therefore, in Table 10, we only present a 95% sensitivity interval for a sensitivity parameter of 1 for the reading outcome. This finding is consistent with the estimate reported in Table 8 that the reading coefficient was not significant at alpha of 0.05 level. For the math outcome in the robustness check, the sensitivity analysis indicates that an unobserved confounder could overturn the study findings with a Γ value of 1.14 or greater. The results of the sensitivity analysis do not indicate that there actually was omitted variable bias with certainty, but they did find a fairly low threshold for non-significant results if bias was present.

4. Discussion

Researchers have stressed that cognitive skills at early ages are related to both educational and labor market outcomes (Currie & Thomas, 1999). To the extent that this relationship is causal, it is especially important that public policies, such as child care subsidies, do not have adverse effects on the cognitive development of children. Low-income children who are eligible for child care subsidies are among the most vulnerable in the U.S. and, as a group, enter kindergarten significantly less ready for school than their more affluent peers. Our results indicate that children who receive subsidies in preschool have lower average scores in math at kindergarten entry than similar children who do not receive subsidies, even after accounting for preschool cognitive scores and a wide range of student and family characteristics. We also find a negative association between child care subsidy use during preschool and children's reading scores in kindergarten, but these results have high standard errors and are not significant in a few of the robustness check models, so the negative association with reading scores should be interpreted with caution. In our main non-parametric analysis with the propensity score matched groups, we estimate a negative effect of .23 standard deviations on children's kindergarten math scores and .20 standard deviations on children's kindergarten reading scores. The effect sizes from the propensity score matching model are somewhat larger in magnitude than those from the value-added OLS models, but are in the same negative direction.

Table 6
Summary statistics of the analysis variables by subsidy receipt status after propensity score matching with standardized mean differences, with preschool cognitive covariates.

	(1) Subsidy recipients	(2) Subsidy non-recipients	(3) Standardized mean difference by subsidy receipt status
Pre-test measures of cognitive skills			
Preschool math score	26.90	26.09	0.09
	(9.20)	(9.30)	
Preschool reading score	22.50	21.74	0.09
	(8.48)	(8.73)	
2 years mental score	124.12	124.49	−0.04
	(10.03)	(8.93)	
9 months mental score	74.60	73.77	0.09
	(10.05)	(9.00)	
Child characteristics			
Birth weight[a]	2.76	2.70	0.07
	(0.88)	(0.90)	
Percent male	48	49	0.02
	(50)	(50)	
Percent black[b]	39	36	0.07
	(49)	(48)	
Percent Hispanic[b]	20	19	0.01
	(40)	(40)	
Percent other race[b]	15	14	0.03
	(36)	(35)	
Percent started K in 2007	26	26	0.00
	(44)	(44)	
Age (months) of K assessment	68.20	68.34	−0.03
	(4.28)	(4.29)	
Family characteristics at age 2			
Family income[c]	2.93	2.95	−0.01
	(3.29)	(3.21)	
Percent welfare recipients	25	25	0.01
	(44)	(43)	
Percent WIC recipients	71	74	−0.07
	(46)	(44)	
Percent single parents	53	55	−0.04
	(50)	(50)	
Mother's years of education	12.85	12.65	0.09
	(1.97)	(2.37)	
Mother's age at child's birth	24.58	24.17	0.06
	(6.33)	(6.22)	
Percent speak English at home	93	93	0.02
	(26)	(25)	
N	250	250	–

Note: The estimates in the first two columns report either the mean or percentage by subsidy receipt status with standard deviations, after propensity score matching with the preschool cognitive covariates. The estimates are not weighted because the trimmed sample implies that the weights are no longer correct, and the sample is a subset of the full ECLS-B data reported in Table 2. Standardized mean differences between subsidy recipients and non-recipients are reported in column 3, and statistical significance of the mean differences is indicated in column 2 (note that there are no significant mean differences on any of the covariates).

[a] Birthweight is measured in 1000 g units
[b] Referent group is white
[c] Income is measured in $10,000 units
*$p < 0.05$.
**$p < 0.01$.

Our findings are broadly consistent with those from Herbst and Tekin (2010a), although Herbst and Tekin find larger differences than we report here. Our robustness checks suggest that our main findings are not a result of the functional form assumptions, the estimation sample we used, or our conditioning variables. Caution is warranted because some robustness check analyses result in non-significant findings, although the direction and rough size of the estimates remains consistent. Another source of caution is that the sensitivity analysis for our propensity score model suggests that even small hidden bias due to an unobserved variable that determines subsidy receipt or non-receipt could explain the currently estimated negative association between child care subsidy receipt and the math and reading outcomes. However, this does not indicate that there is an unobserved confounder with certainty – only that the results could be easily overturned if one did exist.

The observed negative relationship between child care subsidy use and child developmental outcomes, particularly math, may seem surprising. Although child care subsidy policy is not primarily intended as a way to improve child developmental outcomes among at-risk children, intuition suggests that providing families additional money to pay for child care would lead them to obtain higher quality care for their children. In theory, one would expect parents to increase their child care expenditures once they get the subsidy, purchasing more expensive and presumably better care than they otherwise would have without the subsidy. However, in practice, parents may not necessarily end up purchasing higher quality care after receiving a child care subsidy.

Child care subsidies are designed both to encourage and support parent employment and, by extension, placement of children in non-parental care. Delivering subsidies to families (rather than directly subsidizing providers) prioritizes family choice over public influence on the type and quality of care parents select for their children. There are no provisions within child care subsidy policy to ensure that subsidies are used for high quality of care, and federal requirements prevent states from imposing quality standards that are restrictive enough to limit parental choice. Because providers

Table 7
Parametric and non-parametric estimates of the relationship between subsidy receipt and math and reading scale scores in the propensity score matched sample, without preschool cognitive covariates.

	Kindergarten math score	Kindergarten reading score
Parametric (regression)	−1.99** (0.72)	−2.65** (1.01)
Non-parametric (Hodges–Lehmann estimate based on Wilcoxon signed rank test)	−2.16** (0.76)	−2.74** (1.03)
N	500	500

Note: The parametric and non-parametric estimates in this table use the roughly 250 matched pairs (500 observations) from the propensity score matching in Table 5. The parametric regression model includes within pair differences in the variables listed in Table 2 as controls. The non-parametric Hodges–Lehmann estimate is the median of the 250 × 250 possible comparisons using the propensity score matched sample in Table 5.
* $p < 0.05$.
** $p < 0.01$.

Table 8
Parametric and non-parametric estimates of the relationship between subsidy receipt and math and reading scale scores in the propensity score matched sample, with preschool cognitive covariates.

	Kindergarten math score	Kindergarten reading score
Parametric (regression)	−1.56** (0.59)	−1.47 (0.86)
Non-parametric (Hodges–Lehmann estimate based on Wilcoxon signed rank test)	−1.64** (0.60)	−1.20 (0.86)
N	500	500

Note: The parametric and non-parametric estimates in this table use the roughly 250 matched pairs (500 observations) from the propensity score matching in Table 6. The parametric regression model includes within pair differences in the variables listed in Table 2 as controls. The non-parametric Hodges–Lehmann estimate is the median of the 250 × 250 possible comparisons using the propensity score matched sample in Table 6.
* $p < 0.05$.
** $p < 0.01$.

Table 9
Sensitivity intervals for the estimates of the relationship between preschool subsidy receipt and kindergarten math and reading scores in the propensity score matched sample, without preschool cognitive covariates.

	Sensitivity parameter (Γ)	95% Sensitivity interval
Kindergarten math score	1.00	(−3.56, −0.66)
	1.15	(−4.26, 0.05)
Kindergarten reading score	1.00	(−4.72, −0.73)
	1.12	(−5.52, 0.05)

Note: Sensitivity intervals are based on the Wilcoxon signed rank test. The sample for this sensitivity analysis is the propensity score matched sample from Table 5, with preschool cognitive covariates excluded from the matching procedure. The sensitivity parameter Gamma is the odds ratio of an unobserved variable causing subsidy receipt within the matched pairs of children. When Gamma equals 1 there is no differential probability of receiving a subsidy and the 95% sensitivity interval corresponds to the confidence interval for the propensity score matched estimate using the Hodges–Lehmann estimate. When the sensitivity parameter equals 1.15 for math and 1.12 for reading, the corresponding 95% sensitivity interval is such that we fail to reject the hypothesis of no impact of subsidy receipt.

Table 10
Sensitivity intervals for the estimates of the relationship between preschool subsidy receipt and kindergarten math and reading scores in the propensity score matched sample, with preschool cognitive covariates.

	Sensitivity parameter (Γ)	95% Sensitivity interval
Kindergarten math score	1.00	(−2.76, −0.47)
	1.15	(−3.27, 0.08)
Kindergarten reading score	1.00	(−2.92, 0.50)

Note: Sensitivity intervals are based on the Wilcoxon signed rank test. The sample for this sensitivity analysis is the propensity score matched sample from Table 6, with preschool cognitive covariates included in the matching procedure. The sensitivity parameter Gamma is the odds ratio of an unobserved variable causing subsidy receipt within the matched pairs of children. When Gamma equals 1 there is no differential probability of receiving a subsidy and the 95% sensitivity interval corresponds to the confidence interval for the propensity score matched estimate using the Hodges–Lehmann estimate. When the sensitivity parameter equals 1.14 for math, the corresponding 95% sensitivity interval is such that we fail to reject the hypothesis of no impact of subsidy receipt.

can receive child care subsidy payments regardless of the quality of care they offer, the subsidies themselves do not provide incentive for providers improve the quality of care they offer.

As a result, parents may not succeed in obtaining high quality care for several reasons. First, parents could have difficulty obtaining high quality care if there is limited availability of such care in the areas where subsidy recipients live and work (Adams & Rohacek, 2002; Brady-Smith et al., 2001). Second, provider reimbursement rates may be sufficiently low in some areas that they prevent parents from obtaining high-quality care even if it is available (Cohen & Lord, 2005; Government Accountability Office, 2002). Some states pay higher reimbursement rates for programs that meet certain quality indicators, but this does not guarantee that the higher rates will suffice to pay the full costs of high quality care. Third, parents have difficulty identifying high-quality care or even distinguishing high from low quality care (Mocan, 2007; Morris & Helburn, 2000). Finally, parent priorities for child care choices may rank other needs above quality (Barbarin et al., 2006).

Even parents actively seeking high quality child care experiences for their children may have difficulty obtaining the kind of child care they want. Such supply inadequacies might occur in the child care market in terms of service quality because consumers (parents) have trouble evaluating service quality and, therefore, do not place demand pressure for high-quality care on providers (Mocan, 2007; Morris & Helburn, 2000). Parents seem to be better able to identify care that is safe and adequate more easily than they are able to identify high-quality care, in part because they have limited opportunity to observe the child care settings over large parts of the day.

Furthermore, empirical evidence suggests that parent priorities for child care may not always emphasize quality of care. One mixed-methods study finds that parents conceptualize desirable child care differently from the way educators and researchers do, placing less emphasis on quality of educational and social interactions and more emphasis on physical attributes, convenience, and price (Barbarin et al., 2006). Furthermore, parents tend to rate their children's preschool quality much higher than trained outside observers do, with some evidence of larger rating disparities for low-income parents and parents without college education (Cryer & Burchinal, 1997; Cryer, Tietze, &Wessels, 2002; Mocan, 2007). When low-income, disadvantaged parents are given child care subsidies, their resulting child care choices could possibly place children in low to mediocre quality care that may have negative consequences for their cognitive development. Child care subsidy policy does not include incentives for parents to select high-quality care beyond higher reimbursement rates for higher quality care in some states, and therefore may not increase parent access to high-quality care even as they increase parent purchasing power (Holloway & Fuller, 1992).

In the case of children who did not receive child care prior to the subsidy, it may be that the child care choices are of worse quality than the parental care the child would otherwise have had. In the case of children who had child care prior to subsidy receipt, it may be that new child care choices after the subsidy lead to worse quality care. For example, child care subsidies tend to increase family utilization of center-based child care at the expense of informal care options such as home-based providers or relatives (Brooks et al., 2002; Burstein & Layzar, 2007; Crosby et al., 2005; Meyers & van Leuwen, 1992; Weinraub et al., 2005). Although center-based care generally is linked to better cognitive outcomes than is informal care (Loeb, Fuller, Kagan, & Carroll, 2004; Magnuson et al., 2007), this association may not hold for the care available in low-income areas (Adams & Rohacek, 2002; Brady-Smith et al., 2001; Dowsett, Huston, Imes, & Gennetian, 2008), where child care subsidy recipients are likely to enroll their children. Furthermore, receiving a child care subsidy could reduce the likelihood that a child will attend more child development-focused public programs such as Head Start or public pre-K, possibly leading to worse care quality than children might receive otherwise.

Indeed, empirical studies find that parents with child care subsidies tend to utilize relatively poor quality care (Antle et al., 2008; Jones-Branch et al., 2004; Mocan, 2007; Raikes et al., 2005). Furthermore, there is some evidence that, on average, families in states with more generous subsidies do not purchase higher-quality care than families in states with less generous subsidies (Rigby, Ryan, & Brooks-Gunn, 2007). These findings suggest that giving parents additional money to pay for child care, without establishing minimum standards for the type of care purchased, may not cause them to choose better-quality care than they otherwise would. The research presented here does not test whether child care choices and quality mediate the observed negative relationship between child care subsidy use and child development. This important topic will be the focus of a subsequent study.

This study contributes to an emerging literature examining the consequences of child care subsidies for children's developmental outcomes. Our findings, that child care subsidy use in preschool is associated with lower math scores at kindergarten entry, and possibly lower reading scores as well, are consistent with findings in other research on this topic (Herbst & Tekin, 2010a). Our research shares similar limitations to the work by Herbst and Tekin, the most notable being that both involve secondary analysis of observational data, are subject to risks of selection bias, and rely on untestable assumptions to minimize this bias. Still, the consistency of negative findings in both studies given rigorous methods intended to control for selection suggests that further research is warranted to investigate the causal validity of these findings.

4.1. Study limitations

The most critical limitation of our study is that there is likely non-random selection into subsidy receipt that is impossible to fully control for in the analysis. For example, caseworkers may give preference for subsidies to families that need more help. If the children of these families are doing poorly in ways not captured by the control variables used in the analysis, the coefficient on subsidy receipt would be biased downwards. That is, subsidy receipt would appear to have more negative consequences than it actually has. Although we use prior cognitive skills and controls for child and family characteristics to mitigate selection bias, there may yet be other differences between families that do and do not receive subsidies not controlled for in the regression analyses. For example, maternal ability could also potentially confound our results if the mothers of subsidy recipients are systematically less skilled than the mothers of non-recipients, either by directly affecting child outcomes or by reducing long-term employment options and opportunities that could affect child cognitive skills in kindergarten. Our sensitivity test suggests that even small unmeasured differences between the subsidy users and non-users could bias the main results of our study sufficiently to yield misleading estimates of the impacts of subsidy receipt.

Another limitation of our study and others examining the relationship between subsidy receipt and child outcomes is that the subsidy is typically treated as a "black box." Even assuming we have estimated the true causal effect of subsidies on cognitive skills, we still do not know what aspects of the subsidies cause families to make decisions that negatively affect child development. For example, subsidies might allow mothers to begin working because child care becomes more affordable, and it could be that spending less time with the mother negatively affects cognitive skills. Or, parents might make poor decisions about the type of child care to purchase, and low quality child care could affect cognitive skills. Unlike the ECLS-K, which does not have measures of the preschool quality environment or other characteristics of the child's experiences during preschool, the ECLS-B does have these measures and this is an important avenue for future research to understand better *how* child care subsidies might harm cognitive skills.

5. Conclusions

As with any study using observational data, the causal validity of our results is dependent on strong and untestable assumptions. We recommend a randomized control trial, the "gold standard" of causal research, to determine the impact of child care subsidies on children's development. Moreover, many states' subsidy programs are oversubscribed, so an experimental evaluation could be done without affecting the number of program recipients. This research is not experimental and therefore does not support causal inferences about the effects of subsidies on child cognitive outcomes, but it still is an important contribution to the literature on the relationship between child care subsidies and child outcomes. We used two approaches – propensity score matching and value-added modeling – to minimize selection bias, the most serious threat to causal inference in this research. We find a negative relationship between subsidy receipt and math at kindergarten entry, and possibly also between subsidy receipt and reading at kindergarten entry. The finding of a negative association with cognitive outcomes is consistent with other recent research on child care subsidies and child development.

Policymakers should carefully consider how different policies foster the cognitive skills of children, versus labor market participation of poor parents. Our research and that of Herbst and Tekin (2010a) find negative associations between subsidy use and child outcomes. However, a sizeable body of research finds that use of child care subsidies has positive effects on parent workforce participation and on family economic outcomes, both by increasing income and by offsetting the cost of child care (Blau, 2000; Brooks et al., 2002; Crawford, 2006; Ficano et al., 2006; Forry, 2009; Joo, 2008; Lemke et al., 2000; Schaefer et al., 2006; Tekin, 2007). If the negative relationship between child care subsidy use and children's cognitive skills is indeed causal in nature, then policymakers must consider this information in light of the positive effects on family economic outcomes. Furthermore, parents entering the workforce due to PRWORA requirements tend to have low earning power, making the cost of child care prohibitive (Hill & Morris, 2008), so child care subsidies are an important resource for enforcing work rules for welfare recipients. Child care subsidies provide a critical support to low-income families for whom the cost of child care would otherwise be prohibitive.

Blau (2000) posits that early childhood policies confront a "trade-off" between child development goals and workforce participation goals, arguing that a policy designed to meet one goal will not be effective at achieving the other. It certainly would be logistically challenging to hold child care providers that accept subsidies to high quality standards, without also reducing the flexibility in provider choice that facilitates parent workforce participation. Still, the disadvantaged children in families eligible for subsidies need child care that fosters developmental gains and is also flexible to the needs of low-income working families (Adams & Rohacek, 2002). Child care subsidy policy should attempt to meet both goals. A critical first step is identifying which features of child care subsidy policy might mediate the negative relationship between subsidy use and child outcomes.

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