The Impact of Child-Care Subsidies on Child Development: Evidence from Geographic Variation in the Distance to Social Service Agencies

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Abstract

In this paper, we examine the impact of U.S. child-care subsidies on the cognitive and behavioral development of children in low-income female-headed families. We identify the effect of subsidy receipt by exploiting geographic variation in the distance that families must travel from home to reach the nearest social service agency that administers the subsidy application process. Using data from the Kindergarten cohort of the Early Childhood Longitudinal Study, our instrumental variables estimates suggest that children receiving subsidized child care in the year before kindergarten score lower on tests of cognitive ability and reveal more behavior problems throughout kindergarten. An auxiliary analysis of longer-run outcomes shows that these negative effects largely disappear by the time children finish first grade. © *2015 by the Association for Public Policy Analysis and Management.*

INTRODUCTION

In the United States, child-care subsidies have long been considered an important policy instrument for promoting employment and reducing welfare dependency among low-skilled families. Most public expenditures on child-care subsidies are funneled through a means-tested program called the Child Care and Development Fund (CCDF), which was created alongside welfare reform in 1996. Consistent with the overarching goal of welfare reform, the CCDF does at least two things to move disadvantaged mothers from welfare to work. First, eligibility for child-care assistance is conditioned on fulfilling a state-defined work requirement, which typically includes paid employment or participation in a job training or education program. Second, the CCDF invokes the principle of "parental choice," in which subsidized parents are able to purchase child care from virtually any provider, regardless of its level of quality (Blau, 2001; Herbst & Tekin, 2010).

Motivated by the program's employment emphasis, a large number of studies examine the impact of CCDF-funded child-care subsidies on parental employment. Results from this literature provide consistent evidence that mothers receiving a subsidy are more likely to be employed, to be working without receiving welfare, and to be engaged in standard-hour work than their unsubsidized counterparts (Blau & Tekin, 2007; Herbst, 2008a, 2010; Tekin, 2005, 2007). In addition, subsidies enable low-skilled mothers to invest in their own human capital by enrolling in college-level courses and participating in job training programs (Herbst & Tekin, 2011a).

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Despite these favorable results, parental employment is only one among many policy-relevant outcomes that the CCDF might influence. To achieve a full understanding of the impact of child-care subsidy policy, outcomes related to children's health and development must be examined, regardless of whether an explicit goal of the CCDF is to improve such outcomes (Tekin, 2014). Indeed, a large body of evidence indicates that developmental success fortified in early childhood—whether from parental inputs or environmental circumstances—may have lasting effects on schooling and labor market outcomes (Currie, 2011; Currie & Rossin-Slater, 2015). Publicly subsidized early care and education programs are no different regarding their ability to influence a variety of short- and long-run outcomes. Examples of such evidence come from ongoing universal child-care programs in Canada and Europe (Baker, Gruber, & Milligan, 2008; Felfe, Nollenberger, & Rodríguez-Planas, 2015; Havnes & Mogstad, 2010); a short-lived universal child-care program in the United States (Herbst, 2013a); and a variety of highly targeted U.S.-based interventions such as Head Start, Perry Preschool, and Abecedarian (Anderson, 2008; Deming, 2009; Ludwig & Miller, 2007).

Importantly, there is also a sizeable body of scholarship studying the relationship between the CCDF and child well-being. Herbst and Tekin (2010) represent the first effort to examine the effect of subsidy receipt on low-income children's cognitive and behavioral development. The paper finds that subsidy receipt in the year before kindergarten is associated with lower scores on reading and math tests and higher scores on indices of behavior problems at kindergarten entry. In subsequent studies, Herbst and Tekin (2011b, 2012) show that children receiving a subsidy have a higher body mass index and are more likely to be overweight and obese. More recent papers by Hawkinson et al. (2012) and Johnson, Martin, and Brooks-Gunn (2013) find negative or neutral associations between subsidy receipt and school readiness, and an auxiliary line of work shows that subsidies may induce families to use lower-quality child care (Johnson, Ryan, & Brooks-Gunn, 2012). The most recent research by Herbst and Tekin (2014) expands the set of outcomes to include the quality of the parent–child relationship as well as maternal health. Here again the results are largely negative: subsidized mothers are more psychologically and physically aggressive toward their children, and they are in worse mental and physical health than their unsubsidized counterparts.

Although this initial set of studies suggests that the CCDF falls short of meeting children's health and developmental needs, the literature would benefit from gaining clarity on a number of issues. First, the question of the causal effect of subsidy receipt has not been settled. The empirical challenge stems from the fact that recipients and nonrecipients of child-care subsidies are likely to differ in ways that influence child development. If these differences are not accounted for, estimates of the effect of subsidy receipt will be biased. Although all studies in this literature are aware of the omitted-variables problem, very few convincingly remedy it, largely because of the dearth of natural experiments available in U.S. child-care policy. Second, virtually nothing is known about whether the short-run (negative) effects estimated in prior studies persist into the school-age years, or whether they attenuate shortly after children enter school. The issue of persistence is now a common theme in the debate over Head Start and state prekindergarten programs. Likewise, the child-care subsidy literature would be more informative for policymakers if it could also shed light on this issue. Finally, little is known about the potential heterogeneity in the impact of child-care subsidies. It is important from a policy perspective to understand whether the CCDF has differential effects on children from different backgrounds. For example, if the negative effect of subsidy receipt is concentrated on the most economically disadvantaged children—thereby aggravating the developmental disparities of children from different socioeconomic backgrounds—such a finding would be particularly concerning from an equity standpoint.

This paper represents the most thorough attempt to date at estimating the causal effect of CCDF-funded child-care subsidies. As described in more detail below, our primary methodological innovation is the use of a novel instrumental variable (IV) to identify credible estimates of subsidy receipt. In addition, like most of our previous work, we study the effect of subsidy receipt in the year prior to kindergarten on cognitive and behavioral development measured at the beginning and end of kindergarten. However, this paper extends the analysis to longer-run outcomes measured at the end of first, third, and fifth grades. This paper also tests for heterogeneous effects of subsidy receipt across subsets of the analysis sample. Thus, the work discussed here represents the culmination of our efforts over the past few years to evaluate one of the largest early care and education programs in the United States. Our goal is to contribute fresh knowledge to the scholarly literature on child-care subsidies as well as to the ongoing policy debate over the optimal design of the U.S. subsidy system.

To generate the empirical estimates of CCDF subsidies, we leverage plausibly exogenous variation in subsidy receipt through an IV that exploits geographic variation in the distance that low-income families must travel from home in order to reach the nearest social service agency that administers the subsidy application process. To implement this strategy, we collect information on the location of nearly all public social service agencies in the United States, produce geocodes for each agency, and then calculate the distance between the administrative offices and the residential location of families in our data. Thus, the IVs estimates in this paper reflect the difference in the developmental outcomes of otherwise similar children who differ in their propensity to receive a subsidy because their family resides different distances from a social service agency.

Our simple theoretical model suggests that CCDF-funded subsidies do not directly influence child development, but rather operate indirectly through three channels in the child production function. First, child-care subsidies encourage maternal employment, which is shown to have small, negative effects on early and later cognitive abilities (Bernal, 2008; Brooks-Gunn, Han, & Waldfogel, 2002; Herbst, 2014).[1] Second, subsidies increase the use of nonparental child care, especially center- and family-based care, which has conflicting effects on child development (Bernal & Keane, 2011; Herbst, 2013b; National Institute of Child Health and Human Development [NICHD], 2003a, 2003b). However, there is an emerging consensus that high-quality care is beneficial for economically disadvantaged children (e.g., NICHD & Duncan, 2003). Finally, subsidies may influence child development through increases in parental income, which can be spent on goods and services that enhance child ability. Here the existing evidence is also mixed, with some studies documenting small or insignificant effects of family income on child development (e.g., Blau, 1999) and others finding meaningful effects, especially for low-income children (e.g., Dahl & Lochner, 2012). Together, this discussion suggests that child-care subsidies have theoretically ambiguous effects on child development.

Using data from the Kindergarten cohort of the Early Childhood Longitudinal Study (ECLS-K), we apply our IVs strategy to examine the short-run impact of

[1] A few observations about the maternal employment literature are noteworthy. First, the literature has produced mixed results. Bernal and Keane (2011) summarize results from this literature and conclude that about a third of the studies report negative effects and another third report positive effects, with the remainder documenting effects that are either insignificant or that vary by the subgroup analyzed or the timing of maternal employment. It is important to note, however, that several recent papers that address the endogeneity of maternal employment find negative effects (Bernal, 2008; Bernal & Keane, 2011; Herbst, 2014). Second, with the exception of Herbst (2014) and Bernal and Keane (2011), which focus on low-income children, most studies examine economically diverse samples, whose results are less relevant to a study of the subsidy-eligible population.

receiving subsidized child care in the year before kindergarten. Our results point to sizeable negative impacts on cognitive ability tests and teacher-reported behavior measures in the fall and spring of kindergarten. For example, our estimates suggest that subsidized children score 0.4 and 0.3 standard deviations lower on tests of reading and math abilities, respectively, in the fall of kindergarten. We also provide evidence that the adverse effects of subsidy receipt are concentrated on children of relatively high-skilled single mothers. In addition, our results suggest that the impact of subsidy receipt does not persist beyond the kindergarten year.

THE CCDF AND ITS IMPLICATIONS FOR CHILD DEVELOPMENT

As previously stated, the CCDF was created alongside the passage of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA).[2] Welfare reform allocated $21 billion for child-care assistance over the first seven years, 70 percent of which was earmarked to subsidize child-care costs for families receiving welfare or transitioning into work (Greenberg, Lombardi, & Schumacher, 2000). In 1998 (the relevant year in the current analysis), approximately $5.8 billion was spent through the CCDF, providing subsidies to 1.5 million children per month.[3] Given that the CCDF is a close-ended block grant, there is no legal obligation to serve all eligible families. Indeed, it is estimated that the CCDF served only 12 to 15 percent of eligible children in the late-1990s (U.S. Department of Health and Human Services: Administration for Children and Families, 1999). Other estimates suggest 1.5 million of the 9.9 million low- and moderate-income children eligible for subsidies actually received assistance in 1998 (Isaacs, 1999). Eligible families for whom funds are not available are typically placed on a waiting list. As of 2000, 17 states operated a waiting list (Schulman, Blank, & Ewan, 2001).[4]

To qualify for a subsidy, families must have at least one child under age 13, have an income below 85 percent of the state median income, and be employed or participate in a state-defined work activity (e.g., education, job search, or job training). Subsidized child care is available to eligible families largely through vouchers and contracts with providers. In fiscal year 1998, 84 percent of children were in arrangements served by vouchers, and another 10 percent were in arrangements served by contracts. States are given substantial latitude to establish key program parameters, including income eligibility thresholds, benefit reimbursement rates, and copayment rates. Market rate surveys are conducted periodically to ensure that recipients have "equal access" to high-quality providers, defined as reimbursements that cover 75 percent of the local child-care price distribution and copayments that do not exceed 10 percent of family income (Greenberg, Lombardi, & Schumacher, 2000).

States are able to use CCDF funds in a variety of ways to directly influence child-care quality within the market. For example, states are required to spend a minimum of 4 percent of the annual CCDF allocation to support such initiatives

[2] See Tekin (2007), Blau and Tekin (2007), and Herbst and Tekin (2010) for detailed descriptions of the CCDF.

[3] See http://www.acf.hhs.gov/sites/default/files/occ/charts98.pdf for detailed expenditure and participation data.

[4] The absence of a waiting list does not necessarily indicate the absence of rationing. These states may simply turn away clients for whom funds are not available, without putting them on a waiting list. In 2000, only four states had enough funds to serve all eligible children. Aside from the lack of funds, other reasons for nonreceipt among eligible families include the lack of awareness of benefits as well as difficulties navigating the subsidy system (Blau & Tekin, 2007; Herbst, 2008b).

as teacher training and education, improvements to health and safety conditions, and the establishment of quality rating systems. However, there is substantial variation in quality-related expenditures across the states, with some spending over 10 percent of their allocation on these initiatives (nine states) and others spending just below the 4 percent minimum (13 states). Overall, quality expenditures comprise about 6 percent of CCDF spending, while direct service provision accounts for 82 percent.

In addition, several features of the CCDF may indirectly influence the quality of care purchased by parents (Rigby, Ryan, & Brooks-Gunn, 2007). Perhaps the most important design feature is the principle of "parental choice," in which parents can utilize subsidies to purchase most legally-operating child-care services, including unregulated caregivers. The increased flexibility through parental choice enables parents to quickly transition into employment, but it raises questions over whether subsidized children participate in high-quality arrangements. Without strong quality restrictions, it is plausible that parental decisionmaking is guided instead by personal biases and cultural expectations, word-of-mouth recommendations, and convenience (Pungello & Kurtz-Costes, 1999). Furthermore, previous research finds that parents allocate little time to the child-care search (Forry et al., 2014), consistently overestimate the quality of their children's arrangements (Cryer & Burchinal, 1997), and are unable to distinguish low- from high-quality services (Mocan, 2007). These information asymmetries interact with the parental choice features of the CCDF to discourage low-income parents from purchasing high-quality care, which may ultimately undermine child development.

The CCDF also creates quality challenges on the supply side of the market. For example, by mandating only minimum quality standards, the CCDF reduces the incentive for providers to invest in costly quality improvements that promote child development. In addition, conditioning subsidy eligibility on parental employment further discourages providers from offering high-quality services. Child-care providers that rely heavily on subsidized children as a source of revenue may experience severe fiscal shortfalls when parents lose eligibility because of a job separation. Thus, the volatility associated with serving subsidized children undermines child-care providers' ability to make long-term quality investments.

It is also important to note that frequent changes in eligibility due to changes in parental work status can increase instability for children. Low-skilled mothers are likely to experience frequent job turnover, work irregular hours, and have more difficulty finding care that fits their work schedule, which can lead to the loss of the subsidy (Davis et al., 2014; Ha, 2009). Indeed, Ha (2009) provides evidence that parents cycle on-and-off the child-care subsidy rolls, with 50 percent of first spells ending within six months. If accompanied by frequent changes in child-care arrangements, these interruptions could have adverse effects on child development (Tran & Weinraub, 2006).

Another supply-side constraint is created by states' reimbursement rate policies. It is only a recommendation that subsidy reimbursements be set at the 75th percentile of the local price distribution, leaving states with considerable flexibility to set lower rates. In a 1998 report published by the Office of Inspector General of the Department of Health and Human Services, 29 of 51 states failed to abide by this federal recommendation (U.S. Department of Health and Human Services: Office of Inspector General, 1998). Low reimbursement rates limit parental access to high-quality child care and intensify resource constraints among providers. Indeed, economic theory and anecdotal evidence suggest that low reimbursement rates can depress child-care quality, leaving low-income families with little choice but to purchase low-quality services (Duncan, 2010; Herbst & Tekin, 2010).

DATA DESCRIPTION

The data set used in this paper is the ECLS-K, a nationally representative survey of 21,260 children entering kindergarten in the fall of 1998. Children in the ECLS-K are followed through the eighth grade, with detailed parent and teacher interviews conducted in the fall and spring of kindergarten (1998 and 1999) and the spring of first (2000), third (2002), fifth (2004), and eighth (2007) grades. More than 20 children per school from over 1,200 public and private schools are included in the sample.

Analyses in this study are based primarily on the fall of kindergarten wave of data collection, in which child cognitive and behavioral assessments were conducted and parents were asked about child-care subsidy receipt in the year prior to kindergarten entry. We also exploit the longitudinal design of the ECLS-K and examine child outcomes in the spring of kindergarten as well as the spring of first, third, and fifth grades. Our analysis sample retains children living with an unmarried biological mother or female guardian (related and unrelated) as of the fall of kindergarten.[5] We focus on unmarried mothers because this group constitutes approximately two-thirds of eligible subsidy recipients (Herbst, 2008b). Exclusions from the sample are made if the child is missing information on all outcome variables (1,766) or the entire fall of kindergarten parent interview (740), the questions regarding child-care subsidy receipt (35), and census tract identifiers (2,256). We exclude an additional 12,607 children who do not meet the requirement for residence with an unmarried mother.[6] The resulting analysis sample includes 3,848 children. Table 1 presents summary statistics for children and mothers in the sample, disaggregated by subsidy participation status.

We begin by exploring the impact of subsidy receipt on nine child outcomes measured in the fall of kindergarten.[7] Tests of reading and math abilities comprise the outcomes in the cognitive domain (panel A). As shown in Table 2, the reading test measures language and literacy skills, including print familiarity, letter recognition, beginning and ending sounds, vocabulary, and reading comprehension. The math test evaluates identification of one- and two-digit numerals, recognition of geometric shapes, counting and reading numerals, pattern and sequence recognition, and solving simple word problems. Reading and math outcomes are transformations of the raw scores into *T*-scores, which are population-referenced measures of children's achievement. For ease of interpretation, *T*-scores are scaled to have a mean of 50 and a standard deviation of 10. Effect sizes can be calculated by dividing the coefficient estimates by 10.

[5] In particular, children in our sample live with (1) only a biological mother; (2) a biological mother and a partner "father"; (3) an unmarried adoptive mother who may or may not be living with a partner "father"; and (4) an unrelated, unmarried guardian who may or may not be living with a partner "father."

[6] Additional deletions are made because the mother reported a nonsensical age (6), or information from the parent interview could not be merged with the geographic variables (2). Given that our sample selection criteria led us to omit a large number of children who do not fit the definition of residing with an unmarried mother, we estimate supplementary instrumental variables regressions on two additional samples: the subset of children residing with two biological parents, and the full sample of children, irrespective of household structure/parental marital status. Regarding the first set of analyses, we find that the coefficient on subsidy receipt implies both positive and negative effects, is often implausibly large but rarely statistically significant, and has standard errors that are two to three times the size of those in the sample of unmarried mothers. This is not surprising, given that few two-parent families receive subsidies and that the travel distance instrument is not strongly predictive of subsidy receipt. Regarding the second set of analyses, we find that the results consistently imply negative effects, and several of the subsidy coefficients are statistically significant, but the magnitudes are usually considerably larger than those generated in the sample of unmarried mothers.

[7] A detailed description of all assessments studied here can be found on the website of the National Center for Education Statistics: https://nces.ed.gov/ecls/kinderinstruments.asp.

Table 1. Summary statistics for the ECLS-K sample.

Variable	Full sample	Subsidy recipient	Nonrecipient
Boy (percent)	0.499	0.506	0.498
	(0.500)	(0.501)	(0.500)
Child's age (months, fall of K)	68.396	68.268	68.415
	(4.488)	(4.168)	(4.535)
White (percent)	0.380	0.325	0.389
	(0.486)	(0.469)	(0.488)
Black (percent)	0.345	0.411	0.335
	(0.476)	(0.493)	(0.472)
Hispanic (percent)	0.205	0.174	0.209
	(0.404)	(0.379)	(0.407)
Asian (percent)	0.030	0.018	0.032
	(0.170)	(0.133)	(0.175)
Other race/ethnicity (percent)	0.040	0.072	0.035
	(0.196)	(0.259)	(0.185)
Child's weight (pounds, fall of K)	46.824	47.295	46.753
	(9.402)	(9.644)	(9.364)
Premature birth (percent)	0.184	0.171	0.185
	(0.387)	(0.377)	(0.389)
Low birth weight (percent)	0.077	0.057	0.080
	(0.266)	(0.232)	(0.271)
Child's health is fair/poor (percent)	0.046	0.054	0.045
	(0.210)	(0.226)	(0.207)
First-time kindergartner (percent)	0.944	0.946	0.944
	(0.230)	(0.226)	(0.231)
Mother's age (years, fall of k)	30.448	29.232	30.631
	(6.413)	(5.772)	(6.485)
Less than high school (percent)	0.208	0.161	0.215
	(0.406)	(0.368)	(0.411)
High school (percent)	0.374	0.385	0.372
	(0.484)	(0.487)	(0.483)
Some college (percent)	0.326	0.394	0.316
	(0.469)	(0.489)	(0.465)
B.A.+ (percent)	0.092	0.060	0.097
	(0.289)	(0.237)	(0.296)
Only child (percent)	0.292	0.235	0.300
	(0.455)	(0.425)	(0.458)
One sibling (percent)	0.357	0.345	0.359
	(0.479)	(0.476)	(0.480)
Two or more siblings (percent)	0.351	0.420	0.341
	(0.478)	(0.494)	(0.474)
English primarily spoken at home (percent)	0.899	0.932	0.894
	(0.301)	(0.252)	(0.308)
ln (total family income)	9.664	9.528	9.685
	(1.535)	(1.437)	(1.549)
Urban residence (percent)	0.845	0.815	0.850
	(0.362)	(0.389)	(0.358)

Notes: Standard deviations are in parentheses. Analyses are conducted on children and mothers with non-missing data.

In the behavioral domain (panel B), we explore teacher reports of children's externalizing behavior problems, internalizing behavior problems, approaches to learning, self-control, and interpersonal skills. The externalizing behavior scale inquires about the frequency of acting-out behaviors, including arguing, fighting, anger,

Table 2. Child outcomes by subsidy participation status: Fall of kindergarten.

Outcome	(1) No. of observations	(2) Full sample	(3) Subsidy recipient	(4) Nonrecipient	(5) Ho:(3)–(4) = 0 *P*-value
Panel A: Cognitive domain					
Reading test score	3,528	47.43	46.27	47.61	0.003
		(9.06)	(8.71)	(9.10)	
Math test score	3,719	47.38	46.63	47.50	0.055
		(9.21)	(8.52)	(9.30)	
Panel B: Behavioral domain					
Externalizing behavior	3,738	1.79	3.79	1.48	0.000
		(10.81)	(11.84)	(10.62)	
Internalizing behavior	3,675	1.24	1.26	1.24	0.968
		(10.77)	(11.24)	(10.70)	
Approaches to learning	3,785	–2.12	–3.05	–1.98	0.027
		(10.14)	(10.15)	(10.13)	
Self-control	3,647	–2.00	–3.70	–1.74	0.000
		(10.22)	(10.51)	(10.16)	
Interpersonal skills	3,596	–1.79	–3.29	–1.56	0.001
		(10.12)	(10.38)	(10.06)	
Panel C: Motor domain					
Fine motor skills	3,705	–2.22	–1.98	–2.26	0.585
		(10.36)	(10.04)	(10.40)	
Gross motor skills	3,683	0.05	–0.74	0.17	0.068
		(10.13)	(10.04)	(10.14)	

Notes: Standard deviations are in parentheses.

and impulsive behavior. The internalizing behavior scale asks about the frequency with which children display anxiety, loneliness, low self-esteem, and sadness. The approaches to learning scale measures behavior reflecting the ease children display in the learning environment, including attentiveness, task persistence, and eagerness to learn. The self-control scale measures the extent to which children are capable of controlling behavior by respecting the property of others, limiting temper, and responding appropriately to peer pressure. Finally, the interpersonal skills scale provides information on children's ability to form and maintain friendships, comfort or help others, and show sensitivity toward one's peers. All of the behavior outcomes are measured on a scale of 1 to 4. Higher scores on the internalizing and externalizing behavior scales indicate more frequent behavior problems, while higher scores on the remaining scales indicate increasingly positive behavior. These measures are scaled to have a mean of 0 and a standard deviation of 10.

The final set of outcomes focuses on children's fine and gross motor skills (panel C). Fine motor skills capture hand–eye coordination, which is evaluated through such tasks as building a gate, drawing a person, and copying simple figures. The test of gross motor skills evaluates children in the areas of balancing, hopping, skipping, and walking backward. Fine motor skills are measured on a scale of 0 to 9, and gross motor skills are measured on a scale of 0 to 8, with higher scores indicating greater ability. As with the behavioral outcomes, these measures are scaled to have a mean of 0 and a standard deviation of 10.

The primary right-hand-side variable is a measure of child-care subsidy receipt, defined as a binary indicator that equals unity if a given child received subsidized, nonparental child care in the year prior to kindergarten. During the fall of the kindergarten interview, parents were asked about nonparental child care arrangements utilized during the previous 12 months. For each arrangement, a set of follow-up questions ascertained whether any help was received in paying for child care. Specifically, parents were asked the following: "Did any of the following people or organizations help to pay for this ... provider to care for (CHILD) the year before (he/she) started kindergarten?" Four possible choices were presented to parents, and we code those answering "a social service agency or welfare office" as receiving a child-care subsidy. Thirteen percent of children in the sample are coded as having received a child-care subsidy at some point in the year before kindergarten entry.[8]

ECONOMETRIC FRAMEWORK

A reduced-form empirical model specifying the impact of child-care subsidy receipt on child development can be expressed in the following form:

$$A_{is} = \delta_0 + \delta_1 SUBSIDY_{is} + \mathbf{H}'_{is}\delta_2 + \mathbf{N}'_{is}\delta_3 + \sum_s \delta_s + \nu_{is}, \quad (1)$$

where A is one of the nine developmental measures (or latent ability) for child i residing in state s, *SUBSIDY* is the binary indicator of child-care subsidy receipt, $\mathbf{H}$ is a vector of exogenous child and maternal determinants of child development, $\mathbf{N}$ represents a vector of neighborhood attributes, $\Sigma_s\delta_s$ is a set of state fixed effects, and ν_{is} captures unobserved skill endowments.[9] It is important to note that equation (1) yields an estimate of the *overall* impact of child-care subsidy receipt, such that it commingles the three channels through which subsidies may influence child development (i.e., child-care quality, maternal time, and consumption). It is also important to note that, given the construction of *SUBSIDY* as a binary indicator that equals unity for all subsidy recipients, we impose the assumption of homogeneous policy treatments and treatment effects across states, child-care providers, and dosages of subsidy receipt. This is clearly a strong assumption, as states differ substantially in the administration of their subsidy systems, and subsidized children use a variety of arrangements, some of which are unregulated. This discussion suggests that the coefficient on subsidy receipt should be interpreted as the average of heterogeneous effects of subsidy receipt across different policy regimes, intensities of exposure, and level of child-care quality.

Estimating equation (1) with ordinary least squares (OLS) will yield an unbiased estimate of the impact of child-care subsidies if the unobserved determinants of child development are uncorrelated with subsidy receipt. This assumption is unlikely to hold because parental decisions regarding work and child care are not the result of random processes.[10] In the absence of a policy setting that randomly assigns child-care subsidies to single mothers, we rely on a novel IV to generate

[8] One concern with the subsidy measure is the potential measurement error in parental reports of subsidy receipt. Studies examining the reliability of such reports are not common. One notable exception is Johnson and Herbst (2013), who conduct a reliability analysis by comparing parental reports of subsidy receipt with those of child-care provider reports using data from the Fragile Families and Child Well-Being Child Care Supplement. They find a significant overlap between parental- and provider-reported subsidy utilization.

[9] Refer to the notes in Table 3 for a complete list of variables in the model.

[10] For example, highly motivated mothers or those with strong work preferences may be more likely to seek child-care assistance. Since labor force participation is one of the channels through which

quasi-experimental variation in child-care subsidy receipt. To produce consistent estimates using IVs, we need at least one variable that is correlated with subsidy utilization but uncorrelated with child development except through its relationship with subsidy receipt. Our proposed instrument is based on the approximate *distance* that families must travel from home to reach the nearest social service agency that administers the subsidy application process. Appendix A provides a detailed discussion of how the instrument is constructed.[11]

The plausibility of this instrument rests in part on the assumption that families living in an area with an agency nearby face lower costs of obtaining a subsidy. It is therefore hypothesized that an inverse relationship exists between the likelihood of subsidy receipt and the distance between home and the closest social service agency. There are several reasons to expect such a relationship. First, it is well documented that low-income families face already substantial work- and child care-related costs because of the limitations of public transportation systems and low car-ownership rates (Allard, 2009). Edin and Lein (1997) estimate that single mothers' work commute sums to an average of 10 hours per week, with another study finding that mothers' daily trip from home to the child-care provider adds 28 percent more time to the total commute (Michelson, 1985). It is therefore not surprising that low-income working mothers stress the importance of finding child-care services close to home or work (Henly & Lyons, 2000).

Further intuition for a negative relationship comes from states' policies regarding the subsidy application and recertification process. Proximity to a social service agency can affect utilization during multiple stages of a family's interaction with the subsidy system (Herbst & Tekin, 2012). In particular, most parents are required to make one or more personal visits to an agency to conduct the initial in-take and eligibility screening.[12] The number of office visits largely depends on state-specific rules governing the stringency of income and employment documentation and the extent to which families require assistance locating suitable child-care providers. In addition, parents in many jurisdictions are required to report in-person all changes to employment status and income, which may significantly increase the number of trips to the local office. Finally, policies regarding eligibility recertification require

subsidies can influence child development, failing to control for maternal motivation and preferences may produce a biased estimate of δ_1. Another selection mechanism deals with the possibility that mothers take children's cognitive ability and temperament into consideration when deciding whether to work and obtain a child-care subsidy. If mothers differentially select work and child care based on unobserved child characteristics, the coefficient on subsidy receipt will be biased. Finally, it is possible that subsidy administrators systematically ration benefits according to specific household characteristics. For example, administrators might target the least employable mothers. Alternatively, case workers may give priority to higher-skilled mothers, thereby generating the greatest "return on investment" and allowing states to meet work participation targets (Blau & Tekin, 2007).

[11] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[12] A growing number of states allow families to apply for subsidies via the mail, online, or telephone. However, as of 1998—the year subsidy information was ascertained in the ECLS-K—these application modes were substantially less common. Only 14 states in our ECLS-K sample allowed families to *request* subsidy applications by mail, telephone, or e-mail. Another five states allowed families to *complete* the subsidy application via mail or telephone. Furthermore, although some parents may not be required to visit an office to receive a subsidy, there are numerous factors that may necessitate in-person visits both initially and later at recertification, such as lack of trust by parents in the system, errors made by parents or case workers, and visits required by case workers to provide additional documentation. Finally, single mothers in our sample may not have the resources to navigate the application process through online or telephone interactions. Therefore, it is plausible that distance continues to be costly for families that are allowed to submit subsidy applications through alternative means. Herbst and Tekin (2012) show that the distance measure is associated with a statistically significant reduction in subsidy utilization irrespective of whether families must make personal visits to the social service agency in the county in which they reside.

parents to make multiple trips to the local social service agency. In particular, the time-limited nature of child-care subsidies—usually lasting three to 12 months—implies that parents need to restart the eligibility process every few months or risk benefit termination.

Finally, in previous work, we use the ECLS-K to provide direct evidence on the relationship between the distance to social service agencies and subsidy utilization (Herbst & Tekin, 2012). Results suggest that increases in the travel distance (in miles) reduce the likelihood that a potentially eligible family receives a child-care subsidy. We estimate an elasticity of subsidy receipt with respect to distance of –0.13. In results not presented in the paper, we formalize this relationship in the context of the current study by estimating regressions of subsidy receipt on single mothers' travel distance. As expected, the results reveal a negative and statistically significant relationship. The coefficient on travel distance implies that a 1 percent increase in the mileage to the nearest social service agency reduces the probability of subsidy receipt by 1.8 percentage points.

It is important to note that the monetary and psychic costs associated with a given travel distance are likely to vary according to where a family resides. For example, there is substantial geographic variation in the availability of local roads and highways, the amount of traffic congestion associated with those roads, and the accessibility of substitute forms of (public) transportation. Such differences within and across states imply that it is not appropriate to constrain the relationship between travel distance and subsidy receipt to be the same across all jurisdictions.

To investigate this issue, we produce aggregated county- and state-specific correlations between the travel distance measure and subsidy receipt. As expected, both sets of correlations are negative, on average, but the amount of variation is substantially greater across counties than states, as evidenced by a comparison of the standard deviations (0.305 for the county-specific correlations and 0.172 for the state-specific correlations). Additional evidence of between-county variation in the distance–subsidy relationship is provided by comparing correlations across urban and rural counties. Not surprisingly, the average correlation in rural counties is nearly three times larger than that in urban counties, but the spread of correlations around the mean is also greater (SD rural: 0.397 vs. SD urban: 0.277). Our identification strategy therefore exploits this county-level variation in the travel distance–subsidy relationship by allowing the impact of distance to differ by mothers' county of residence. This is formally implemented in the first-stage subsidy receipt equation by interacting travel distance with county-specific binary indicator variables. With an *F*-statistic of 23.5, the set of IVs—that is, the set of distance-by-county interactions—is highly statistically significant in the first-stage equation. Next, the predicted values for subsidy receipt derived from the first-stage equation are included in the second-stage equation for the impact of subsidy receipt on child development. The second-stage equation is identical to that outlined in equation (1).

In order to serve as an appropriate instrument for subsidy receipt, travel distance must be validly excluded from the child production function specified in equation (1). One concern is that travel distance could be determined in part by the joint location preferences of families and social service agencies. For example, given the low car-ownership rates among low-income families, such individuals may prefer to live near critical support services or employment centers. In addition, administrative offices might locate in low-income neighborhoods to be accessible to potentially eligible clients. If these unobserved family and agency location preferences influence travel distance in ways that affect child development, the coefficient on subsidy receipt will be biased. Another concern with using travel distance is that it may proxy neighborhood and individual characteristics that influence child development. In other words, it is possible that the neighborhood environment directly affects or is correlated with individual characteristics that influence child development. In

Appendix B, we provide a detailed discussion of these concerns, and we outline a series of strategies to mitigate their influence on the IV estimates.[13]

ESTIMATION RESULTS

Main Results

Table 2 provides the means and standard deviations for the child outcomes measured in the fall of kindergarten. We present these summary statistics for the full ECLS-K sample and for the subsets of subsidized and unsubsidized children. The table shows that subsidized children score lower on tests of cognitive ability and display more behavior problems than their unsubsidized counterparts. In fact, subsidy recipients perform worse on eight of the nine outcome measures, and the raw differences are statistically significant for seven of the outcomes. Such differences should be interpreted with caution, however, as they do not account for the possibility that subsidy receipt is correlated with other factors that determine child development.

We begin to address the presence of confounding variables in Table 3, which presents OLS estimates of the relationship between child-care subsidy receipt and child development. Each cell in the table presents the coefficient on subsidy receipt and its standard error (adjusted for clustering at the county level). Column (1) shows the estimates for subsidy receipt without controls. Columns (2) and (3) add the child and family covariates, respectively, and column (4) incorporates the neighborhood-level controls. Finally, column (5) adds the state fixed effects.

Overall, the results point to poorer outcomes for subsidized children. Looking at the fullest OLS specification in column (5), we find that subsidy receipt is related to lower reading and math test scores and increased behavior problems, although only the behavior measures show statistically significant results. Children receiving subsidized care, for example, score 2.4 points higher on the measure of externalizing behavior problems and about two points lower on the measures of self-control and interpersonal skills. Findings for the psychomotor outcomes are mixed, with the subsidy coefficient indicating statistically significant increases in fine motor skills and decreases in gross motor skills. In sum, the OLS results are indicative of a negative relationship between child-care subsidies and child development. These estimates, however, should not be interpreted as causal because the models do not fully account for the possibility that families nonrandomly select into subsidy receipt.

The paper's main results are depicted in Table 4. Specifically, it presents various IV estimates of the impact of child-care subsidy receipt. As shown in column (1), the baseline results—which use the full set of distance-by-county interactions as instruments—consistently point to a negative effect of subsidy receipt on child development. The estimates are generally larger in magnitude compared to the OLS estimates and are statistically significant for all outcomes except fine and gross motor skills. To conserve space, only the baseline results are discussed below. However, as shown in columns (2) through (6), we estimate a number of auxiliary IV models to highlight the robustness of the baseline estimates. A full discussion of

[13] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 3. Ordinary least squares estimates of the relationship between child-care subsidy receipt and child development—Fall of kindergarten.

Outcome	(1) No controls	(2) + Child controls	(3) + Family controls	(4) + Local controls	(5) + State FE
Reading test score	–1.340***	–1.084**	–0.670	–0.582	–0.376
	(0.484)	(0.484)	(0.466)	(0.465)	(0.449)
Math test score	–0.861*	–0.669	–0.427	–0.402	–0.210
	(0.470)	(0.444)	(0.415)	(0.418)	(0.411)
Externalizing behavior	2.310***	2.199***	2.274***	2.264***	2.400***
	(0.637)	(0.614)	(0.621)	(0.624)	(0.625)
Internalizing behavior	0.020	0.159	0.179	0.091	0.169
	(0.604)	(0.596)	(0.597)	(0.581)	(0.596)
Approaches to learning	–1.076*	–0.897*	–0.899*	–0.964*	–0.971*
	(0.582)	(0.537)	(0.535)	(0.552)	(0.552)
Self-control	–1.956***	–1.748***	–1.834***	–1.920***	–2.105***
	(0.547)	(0.511)	(0.506)	(0.508)	(0.526)
Interpersonal skills	–1.728***	–1.534***	–1.544***	–1.576***	–1.674***
	(0.557)	(0.524)	(0.530)	(0.521)	(0.513)
Fine motor skills	0.275	0.536	0.667	0.813	0.877*
	(0.530)	(0.535)	(0.549)	(0.542)	(0.529)
Gross motor skills	–0.905	–1.093*	–1.140**	–1.079*	–0.929*
	(0.557)	(0.561)	(0.563)	(0.552)	(0.535)

Notes: Each cell presents the coefficient on child-care subsidy receipt in the year before kindergarten entry and the standard error (in parentheses) that is adjusted for clustering at the county level. Column (2) adds controls for child's gender, child's age, child's age squared, race/ethnicity, child's weight in the fall of kindergarten, premature birth, low birth weight, fair/poor health status, and first-time kindergartner. Column (3) adds controls mother's age, mother's educational attainment, number of other children in the family, English as the primary home language, log of family income, and urban residence. Column (3) adds controls for log of median household income; log of population density; percentage non-Hispanic white; percentage foreign-born; percentage age 65 and over; percentage female; percentage of children ages 0 to 2, 3 to 5, 6 to 11, 12 to 13, 14, and 15 to 17 living in female-headed households (all at the census tract-level); percentage of children in the school eligible for free/reduced price lunch, an indicator for whether a majority of children in the school are minorities, and an indicator for whether the school receives Title I funding. Column (5) adds state fixed effects. *, **, *** indicate that the subsidy coefficient is statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

these estimations can be found in Appendix B.[14] Suffice it to say that estimates from these alternative specifications are consistent in sign and magnitude with those in the baseline model.

Looking first at the cognitive outcomes, we find that children receiving subsidized care score approximately five points lower on the reading test and four points lower on the math test. These point estimates correspond to effect sizes of about 0.49 and 0.37 standard deviations, respectively, and would move the median child to the 36th percentile of the reading score distribution and the 29th percentile of the math score distribution. In the behavioral domain, subsidy receipt has a deleterious effect on all five outcomes. The coefficient on subsidy receipt is statistically significant in a majority of the behavior models, and the magnitude usually implies an effect size larger than that in the cognitive domain. The IV coefficient implies a

[14] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 4. Instrumental variables estimates of the impact of child-care subsidy receipt on child development—Fall of kindergarten.

	Baseline model:	Alternate models: Reduced instrument set			Alternate models: Additional controls	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Full instrument set	Top 75 percent	Top 50 percent	Top 40 percent	Agency neigh-borhood	County group FE
Reading test score	–4.891**	–4.365*	–5.980**	–4.782*	–3.899*	–3.631*
	(2.045)	(2.662)	(2.738)	(2.732)	(2.162)	(1.934)
Math test score	–3.713**	–3.803*	–5.685***	–4.935**	–3.051*	–2.938*
	(1.675)	(2.036)	(1.952)	(2.017)	(1.731)	(1.725)
Externalizing behavior	7.172***	9.207***	9.078***	8.310***	6.691***	7.279***
	(2.123)	(2.475)	(2.661)	(2.556)	(2.053)	(2.061)
Internalizing behavior	2.926*	4.981**	4.196*	4.237*	3.516**	0.971
	(1.722)	(2.199)	(2.434)	(2.462)	(1.729)	(1.815)
Approaches to learning	–5.084**	–3.918*	–3.501	–2.343	–4.173**	–1.604
	(2.167)	(2.109)	(2.269)	(2.371)	(2.078)	(2.099)
Self-control	–5.839**	–4.942**	–5.019*	–3.380	–6.080***	–4.882**
	(2.326)	(2.412)	(2.594)	(2.624)	(2.247)	(2.222)
Interpersonal skills	–4.468*	–1.956	–2.504	–1.224	–3.800*	–2.268
	(2.390)	(2.589)	(2.780)	(2.786)	(2.291)	(2.333)
Fine motor skills	–0.446	–0.621	–1.498	–1.710	–0.045	–0.372
	(1.583)	(1.903)	(2.096)	(2.215)	(1.607)	(1.640)
Gross motor skills	–2.790	–1.624	–0.728	–1.519	–1.747	–0.738
	(1.768)	(2.128)	(2.308)	(2.406)	(1.784)	(2.338)

Notes: Each cell presents the coefficient on child-care subsidy receipt in the year before kindergarten entry and the standard error (in parentheses). Standard errors are adjusted for clustering at the county level. Column (1) includes the full set of distance-by-county interactions as the instrument set. Columns (2) through (4) use the reduced instrument sets: top 75 percent of the subsidy–county correlation distribution, top 50 percent of the subsidy–county correlation distribution, and top 40 percent of the subsidy–county correlation distribution, respectively. Column (5) adds controls for the neighborhood characteristics in which social service agencies are located. *, **, *** indicate that the subsidy coefficient is statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

seven-point increase in the externalizing behavior problems scale and a three-point increase in the internalizing behavior problems scale. These translate to effect sizes of 0.71 and 0.29 standard deviations, respectively. The estimates also suggest that subsidy receipt reduces a number of positive social behaviors, including self-control and interpersonal skills. Coefficients imply reductions of approximately half a standard deviation in the various measures of positive social behaviors. Turning to the psychomotor domain, the coefficient on subsidy receipt points to a negative effect on fine and gross motor skills, but the estimates are statistically insignificant and the effect sizes are smaller than those in the other domains.

Heterogeneous Effects of Child-Care Subsidy Receipt

The IV results discussed above suggest that subsidy receipt leads to reductions in tests of reading and math abilities and increases in a variety of behavior problems. We now assess the impact of subsidy receipt on several demographic subgroups. In particular, we examine differential effects of subsidy receipt across children's gender, mothers' educational attainment, and families' socioeconomic status (SES). Estimates from the subgroup analyses are presented in Table 5. Appendix C provides

Table 5. Heterogeneous effects of child-care subsidy receipt by child and maternal characteristics.

Outcome	(1) Boys	(2) Girls	(3) Low skilled	(4) High skilled	(5) Low SES	(6) High SES
Reading test score	–1.816	–2.714*	–3.422*	–3.211**	0.548	–5.586***
	(1.927)	(1.594)	(1.847)	(1.472)	(1.881)	(1.712)
Math test score	–3.838**	–1.475	–4.181**	–1.811	–1.086	–2.057
	(1.730)	(1.316)	(1.867)	(1.493)	(1.947)	(1.675)
Externalizing behavior	5.307*	3.498**	0.668	8.184***	0.192	10.206***
	(2.772)	(1.759)	(2.051)	(1.982)	(2.281)	(2.182)
Internalizing behavior	5.305**	–0.256	–0.399	0.638	–0.337	1.112
	(2.265)	(1.700)	(2.410)	(1.902)	(2.191)	(2.015)
Approaches to learning	–5.404**	–1.331	–0.766	–4.756***	2.057	–7.025***
	(2.343)	(1.889)	(2.207)	(1.850)	(2.011)	(2.219)
Self-control	–4.885**	–2.118	–0.349	–5.340**	2.166	–8.455***
	(2.302)	(2.082)	(1.826)	(2.119)	(2.192)	(2.342)
Interpersonal skills	–3.696	–0.848	0.896	–4.783**	4.556**	–6.780***
	(2.311)	(2.147)	(2.301)	(2.009)	(2.095)	(2.319)
Fine motor skills	–0.644	2.388	–0.017	–0.600	1.548	–1.479
	(1.842)	(1.688)	(2.055)	(1.540)	(1.955)	(1.573)
Gross motor skills	–2.369	–2.282	–2.734	–0.462	–1.248	–0.470
	(2.471)	(1.862)	(2.260)	(2.015)	(2.493)	(1.853)

Notes: Each cell presents the coefficient on child-care subsidy receipt in the year before kindergarten entry and the standard error (in parentheses). Standard errors are adjusted for clustering at the county level. Low skilled is defined as mothers with a high school degree and less, and high skilled is defined as mothers with some college education and above. Low SES is defined as families in the bottom SES quintile, and high SES is defined as families in the top SES quintile. *, **, *** indicate that the subsidy coefficient is statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

a deeper discussion of the interpretation of these subgroup estimates in the context of an IV analysis.[15]

Columns (1) and (2) present the subsidy estimates separately for boys and girls. In the cognitive domain, boys' math scores are more negatively affected by subsidy receipt. In fact, the coefficient on subsidy receipt for boys is more than double that for girls and is highly statistically significant. Conversely, the impact of subsidy receipt on reading test scores is larger for girls by nearly 0.10 standard deviations. In addition, subsidy receipt has large negative effects on the behavior outcomes for boys, while the effects for girls are small in magnitude and usually imprecisely estimated. The finding of a pronounced effect of subsidy receipt on boys' behavior is consistent with several studies that find that boys tend to be more adversely affected by early maternal employment (e.g., Brooks-Gunn, Han, & Waldfogel, 2002).

Several studies find that the negative effect of child-care participation and maternal employment is concentrated on economically advantaged children (Bernal & Keane, 2011; Herbst, 2013b). To examine such patterns for subsidies, we first estimate the IV model separately on low- and high-education mothers. Low-education mothers are those with a high school degree or less, and high-education mothers are those with at least some college education. As shown in columns (3) and (4), our results are largely consistent with previous work. Child-care subsidies have

[15] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 6. The long-term impact of child-care subsidy receipt.

Outcome	(1) Spring kindergarten	(2) Spring first grade	(3) Spring third grade	(4) Spring fifth grade
Reading test score	–3.739**	–2.681	–0.757	–0.512
	(1.913)	(1.927)	(1.541)	(1.871)
Math test score	–3.356*	–0.239	–0.289	–3.573
	(1.730)	(1.850)	(1.765)	(2.266)
Externalizing behavior	8.612***	2.899	2.782	–2.056
	(2.522)	(2.305)	(2.060)	(2.165)
Internalizing behavior	2.662	0.552	1.461	–0.445
	(2.152)	(2.113)	(1.995)	(2.214)
Approaches to learning	–6.530***	–3.451	–4.088**	1.037
	(2.237)	(2.169)	(2.022)	(2.016)
Self-control	–7.284***	–4.868**	–2.280	–0.118
	(2.072)	(2.345)	(2.137)	(2.118)
Interpersonal skills	–4.283**	–4.706**	–2.585	1.507
	(2.151)	(2.347)	(2.135)	(2.134)

Notes: Each cell presents the coefficient on child-care subsidy receipt in the year before kindergarten entry and the standard error (in parentheses) that is adjusted for clustering at the county level. *, **, *** indicate that the subsidy coefficient is statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

similar-sized effects on the cognitive ability test scores for children with low- and high-skilled mothers. In the behavioral domain, however, the negative impact of subsidy receipt is much larger for children with higher-skilled mothers.

It is important to note that splitting the sample by maternal education may not be the most appropriate way to test for differential subsidy effects across different family environments. It is possible, for example, that education alone does not capture the most important family inputs to child development. Therefore, we also consider models that stratify the sample according to the bottom and top quintiles of the SES distribution. As shown in columns (5) and (6), the results now strongly indicate that subsidy receipt among relatively advantaged (i.e., high-SES) children adversely affects cognitive and behavioral development, compared to neutral or even positive effects on their relatively disadvantaged (i.e., low-SES) peers.

Longer-Run Effects of Child-Care Subsidy Receipt

The results discussed so far indicate that child-care subsidies lower cognitive ability test scores and increase behavior problems in the short run. An important question is whether these negative effects persist throughout the school-age years. To address this question, we estimate the baseline IV model using the available outcomes measured in the spring of kindergarten and the spring of first, third, and fifth grades. Note that the assessments for fine and gross motor skills are not administered beyond the fall of kindergarten, but all other outcomes are available through the fifth-grade wave of data collection.

As shown in Table 6, the impact of child-care subsidy receipt persists throughout kindergarten. As of the end of kindergarten, subsidized children continue to experience lower reading and math test scores and increased behavior problems. The effects remain sizeable and statistically significant at conventional levels. However, one year later, that is, at the end of first grade, these negative effects largely disappear. The remaining negative and statistically significant effects are found for the

measures of self-control and interpersonal skills. The impact of subsidy receipt continues to attenuate in magnitude and statistical significance throughout the third and fifth grades. In fact, by the end of fifth grade, the effect sizes are substantially smaller than was the case in the fall of kindergarten, and none of the subsidy coefficients are precisely estimated.

Robustness Checks and Falsification Tests

We subject our baseline results to several additional specification and falsification tests to ensure robustness. Results from the robustness analyses are presented in Appendix Table C1.[16] To this point, the IV estimates are based on mothers' travel distance to the *nearest* social service agency. Some mothers have multiple agencies from which to choose in the county of residence. For these mothers, we have assumed that the relevant agency is the one closest to the residential location. We believe this is a plausible assumption. However, to make more explicit use of the presence of multiple administrative offices, we estimate the IV model using the sum of the inverse distances as an alternative instrument. An advantage of this measure is that it gives more weight to distances closer to mothers' residential location. As shown in column (1), results using this measure are quite similar to the main findings.

To the extent that our instruments are orthogonal to child development, the estimation strategy should produce consistent estimates of the impact of subsidy receipt. To strengthen our confidence in this assumption, we incorporate in column (2) controls for parental participation in two means-tested programs (TANF and SNAP). In addition to controlling for unobserved work and welfare preferences that may influence child development, inclusion of these variables should account for the possibility that parents need to travel to the same social service agency to apply for these benefits.[17] If TANF and SNAP participation are correlated with both the travel distance instrument and various dimensions of child development, then omitting them from the production function would invalidate the instruments. Fortunately, our main results do not change when controls for TANF and SNAP participation are included in the model.

Next, we explore the implications of omitting from the analysis children who participated in Head Start in the year before kindergarten. Such children make up about 10 percent of the sample. This exercise is important because, while the presence of Head Start might be relevant to the decision to apply for child-care assistance, it is considerably less important to mothers' work and child-care decisions after obtaining a subsidy. As shown in column (3), the results are similar when Head Start children are dropped. Note that we retain Head Start children in the baseline sample and model because dropping them may create a form of sample selection bias.

We also estimate models in which children living in nonurban areas are excluded from the analysis. We do so because families living in urban neighborhoods are presumably more homogeneous with respect to family and neighborhood attributes that influence both the distance to social service agencies and measures of child development. In particular, it is plausible that urban families face relatively similar psychic and monetary costs associated with accessing major roadways and public

[16] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[17] In addition to TANF and SNAP, families may apply for WIC benefits in the same office. Therefore, we acknowledge the possibility that controlling for TANF and SNAP in the IV model may not sufficiently account for the confounding that might be caused by the receipt of other benefits and services.

transportation. Therefore, an urban sample is less likely to suffer from endogenous location choices and unobserved neighborhood characteristics that affect child development. The estimates are robust to this change in sample definition, as shown in column (4).

As previously discussed, the baseline IV model includes a control for family income. Unfortunately, the ECLS-K does not allow researchers to identify the individual sources of parental income, including earnings and various sources of nonwage income. Ideally, we prefer to omit sources of income that change as a result of utilizing a child-care subsidy. For example, the employment effects of subsidy receipt may cause earnings to increase while reducing the amount of income drawn from traditional cash-assistance programs. To the extent that subsidized families experience an increase in disposable income, these additional resources can be spent on goods and services that might positively impact child development. Indeed, this is one of the anticipated channels through which subsidy receipt can influence child outcomes. However, the permanent component of family income is likely to be correlated with subsidy receipt and child development. Failure to account for such resources may therefore bias the IV estimates. To assess whether the results are robust to the exclusion of family income, we estimate the IV models without this control. As reported in column (5), the exclusion of income does not significantly alter the results.

Finally, we implement two falsification tests. Recall that the key IV identifying assumption would be violated if there are unobserved family and agency location preferences that jointly determine travel distance and child development. If this assumption is valid, then variables predicting child-care subsidy receipt for single mothers—a group likely to be eligible for assistance—should not influence the well-being of children residing in families unlikely to be eligible. Since all families in the ECLS-K have children, we focus the first falsification test on two-parent families in the top-two quintiles of the SES distribution. We first estimate the first-stage subsidy receipt equation on the sample of single mothers in order to calculate a predicted probability of subsidy receipt for the subset of two-parent families in the top SES quintiles. We then include this variable in the production function. As shown in column (1) of Appendix Table C2, this test provides evidence in favor of our identification strategy: in only one case (internalizing behavior) do we find that predicted subsidy receipt negatively affects the well-being of children in high-SES two-parent families.[18] In a second test, we focus on married nonworking mothers, a group whose employment status makes them ineligible to receive a subsidy. As shown in column (2) of Appendix Table C2, we confirm that subsidy receipt does not influence children who are unlikely to receive child-care assistance.[19]

The Employment Effect of Child-Care Subsidy Receipt

Given that maternal employment is one of the primary channels through which child-care subsidies may influence child development, it is useful to examine the impact of subsidy receipt on mothers' employment decisions. Indeed, this question has received considerable attention in previous research (e.g., Blau & Tekin, 2007; Herbst, 2008a, 2010; Tekin, 2005, 2007). We estimate OLS and IV regressions of a binary indicator for mothers' employment status on the indicator for subsidy

[18] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[19] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

receipt and the full set of controls. As shown in Appendix Table D1, the results indicate that subsidy receipt has a positive short-run effect on maternal employment.[20] The IV estimate in column (1) indicates that the employment rate for subsidized mothers is about 11 percentage points higher, on average, than their unsubsidized counterparts. This finding is consistent with the employment mandate attached to CCDF-funded subsidies. However, the results indicate that these positive employment effects dissipate by the spring of first grade. Indeed, the IV estimates are small in magnitude and statistically insignificant starting with the spring of first-grade interview.

CONCLUSIONS

Parental decisions regarding employment and child care can have important implications for children's development. Given that child-care subsidies affect both decisions, they have the ability to shape the developmental trajectory of economically disadvantaged children. Using rich longitudinal data from the ECLS-K, this paper provides a comprehensive analysis of the impact of subsidy receipt on child development. Our results suggest that subsidy receipt in the year before kindergarten entry reduces reading and math test scores and increases a variety of behavior problems at the start of kindergarten. Results from the subgroup analyses indicate that the negative effect of subsidies tends to be larger for boys and for children living in relatively advantaged families. Although the negative impact of subsidy receipt persists throughout kindergarten, our results show that it largely disappears by the end of first grade.

To put these findings into perspective, we compare the effect sizes in Table 4 (column [1]) with those reported elsewhere in the early care and education literature. Herbst (2013) finds that infant and toddler participation in nonparental child care reduces cognitive ability test scores by 0.29 standard deviations. Bernal and Keane (2011) document that an additional year of nonparental child care is associated with a 0.11 standard deviation decrease in disadvantaged children's test scores. In a study of the Quebec's universal child-care program, Baker, Gruber, and Milligan (2008) find that subsidized children experience a reduction in social development of 0.17 standard deviations. In an evaluation of Tulsa, Oklahoma's prekindergarten program, Gormley and Gayer (2005) find effect sizes of 0.39 (cognitive ability), 0.38 (language ability), and 0.24 (motor skills). Thus, the (absolute value of the) estimates presented here are broadly consistent with those found elsewhere.

Our research on the CCDF over the past few years generally leads to a singular conclusion, that is, although U.S. child-care subsidy policy is effective at increasing the work effort of low-skilled mothers, it is less successful at enhancing the health and well-being of children and families. The important question for scholars and policymakers is why. Unfortunately, our research permits only tentative answers to this question, but we believe there are at least three plausible explanations (Herbst, 2013c). First, the CCDF is essentially a labor market program. It was created to solve the "problem" of low employment rates among single mothers, and it accomplishes this by conditioning eligibility for child-care assistance on fulfilling a work requirement. However, a child-care subsidy is at best an indirect tool for inculcating a work ethic and increasing human capital among low-skilled families. Indeed, labor market policies, such as the EITC, minimum wage, and job training programs, are more direct and effective interventions. At its worst, an employment-conditioned subsidy

[20] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

attempts to remedy a distortion in the labor market, but instead creates distortions in the child care market that did not initially exist. For example, subsidies encourage a shift in child care demand from unpaid to paid services. In addition, they may exacerbate quality constraints in the larger child-care market by discouraging child-care providers from making costly quality enhancements.

Second, the CCDF's commitment to parental choice means that low-skilled parents can move quickly into the labor force. However, the primary disadvantage of parental choice is that parents often have insufficient information to make optimal purchasing decisions. When parents cannot make informed decisions, child-care providers have little incentive to invest in costly quality enhancements. This ultimately forces high-quality providers out of the market, leaving those willing to supply low-quality services. Parents receiving CCDF-funded subsidies are both negatively affected by and exacerbate the information problems in the child-care market—that is, their choices are constrained to low- and mediocre-quality providers, and the inability to make informed decisions further reduces the quality of care rendered in the market.

The third explanation focuses on the subsidy reimbursement rate, or the maximum amount a state or local agency pays child-care providers to serve subsidized children. As previously stated, the CCDF assists low-income families with accessing high-quality care by recommending that reimbursement rates be set at the 75th percentile of the local price distribution. As this is only a recommendation, the CCDF gives states the legal authority to establish lower rates. In 2014, one state set its reimbursement rate at the 75th percentile, and only two-fifths of states updated their rate structure within the last two years (Schulman & Blank, 2014). Low reimbursement rates are a problem, of course, because they prevent families from purchasing high-quality care, in addition to reducing the resources available to providers to invest in quality.

Based on this diagnostic assessment, our policy recommendations are twofold. First, the CCDF should provide parents with strong incentives to purchase high-quality child care. This can be accomplished through a means-tested voucher whose value is an increasing function of the quality of child care purchased. A number of states already have a so-called quality-tiered reimbursement structure, but very few are generous enough to meet or exceed the 75th percentile recommendation. Second, states and the federal government should inform parents about the potential benefits of high-quality child care. As consumers, parents play a decisive role in establishing the level of quality rendered in the market. However, given that they are often unable to discern levels of child-care quality or are unwilling to pay more for high-quality services, governments at all levels should engage in an aggressive public information campaign to inform parents about the importance of child-care quality. The campaign could be funded by the CCDF, but should target families inside and outside the subsidy system so that its effect on demand is sufficiently powerful to compel providers to invest in quality.

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APPENDIX A: CONSTRUCTION OF THE DATABASE ON U.S. SOCIAL SERVICE AGENCIES

The process for creating the distance measure began by collecting data on the precise location of every social service agency in the United States. In most cases, address data were available on the Web site of the state agency responsible for administering the child-care subsidy system. For example, the Department of Economic Security administers the subsidy program in Arizona, and the office locations can be found at https://www.azdes.gov/main.aspx?menu=128&id=2724. In Maryland, the subsidy program is managed by the Office of Child Care in the Department of Education, and information on agency locations can be found at http://www.dhr.state.md.us/county.php. For some states, we were not able to readily find the office locations on states' websites, so we relied on administrator contact lists provided by the National Child Care Information Center (NCCIC), found at http://nccic.acf.hhs.gov/statedata/dirs/display.cfm?title=ccdf#az), and the *Child Care and Development Fund Report of State Plans* (various years) for this information. We were careful to verify that each agency is involved in eligibility and benefit determination for child-care subsidies.

For each social service agency, we collected information on the state name, state FIPS code, county name, and county FIPS code in which each office is located; the address (including building or suite number), city, and zip code; telephone and fax numbers; and the name of the agency that administers the subsidy program. Most states organize social service provision at the county level, with one agency located in each county. However, in some urban counties and many cities, there are multiple agencies located in the jurisdiction. For example, La Paz County, located in Western Arizona, is a rural jurisdiction, and its residents have access to a single social service agency. Maricopa County, in contrast, is an urban area (containing the city of Phoenix), and its residents have access to eight offices. As for Maryland, every county contains one social service agency, except for Baltimore City, which has nine offices. In a small number of cases, a locale does not include a social service agency, so that its residents must travel to adjacent counties to apply for child-care assistance. For example, Pend Orielle County in Washington State does not have a social service agency. Therefore, as stipulated by the Department of Social and Health Services, residents in this county must travel to a branch office in Spokane County (located south of Pend Orielle) to apply for assistance. Generally speaking, these agencies serve residents from multiple counties.

Our database attempts to account for these complications. Agencies located in multiple agency jurisdictions are each treated as separate entries in the database. Agencies that serve residents from multiple jurisdictions (because their county of residence does not have one) are repeated in the database, with each entry denoting the relevant county served by the office. In all, we collected data on approximately 3,600 unique social service agencies.

One concern is that our agency database captures the current address of each agency, while our child-care subsidy data come from surveys that were conducted in the late 1990s and early 2000s. To the extent that some of these agencies moved to their current address after these years, our distance measure contains measurement error. However, as previously stated, we recorded the telephone number of each agency in the database, and we asked two research assistants to make phone calls to more than 10 percent of (randomly chosen) offices to inquire about their location history since 1998. Fortunately, an overwhelming majority of these agencies have been at the same location during this period, and we were able to identify the previous address in most cases for the small number of movers. Of the 405 phone calls made to social service agencies, we were able to speak to a representative in 228 cases. Of these cases, only 35 reported that they had moved at some point since 1997. The rest stated that they were either in the same location for sure or that they

had "probably" been in the same location. Despite this evidence, it is inevitable that some of our distance calculation would contain measurement error.

The next step in the process involved geocoding the location of social service offices by assigning a latitude and longitude coordinate to each. We worked in collaboration with Geocoder (www.geocoder.us) to generate the coordinates. Geocoder was able to provide these coordinates using its own application programming interface (API) as well as that from Google, now considered the gold standard for producing geocodes. Based on our discussions with Geocoder analysts, we concluded that the Google-based geocodes were of higher quality, so we use these as the basis for making the distance calculations. Of the 3,659 social service agencies (unique or repeated) in our database, 2,887 (approximately 80 percent) were able to be geocoded to its exact location (i.e., typically to 30 feet or less). Another 543 agencies (15 percent) were geocoded to roughly block- or street-level accuracy. For 229 agencies (6 percent), only the city or zip code was available to be geocoded, decreasing locational precision to a few miles. In sum, approximately 95 percent of social service agencies were geocoded with a level of precision at the block level or better.

A potential concern with the geocoding process is that the agency addresses would not match those found in the Geocoder's database. For example, slight errors in spelling or formatting in a set of agency addresses could cause a different set of addresses to be geocoded. Fortunately, Geocoder provided us with a measure called the Levenshtein–Damerau, which calculates the "edit distance" (or level of textual discrepancy) between the addresses provided and the addresses actually assigned geocodes. We used this measure to double check the accuracy of agency addresses that were assigned low scores, and we corrected any errors that were discovered. Generally speaking, we found this measure to be quite sensitive to small inconsistencies between the provided and the geocoded addresses. Therefore, our data checks were extensive.

In the final step, we calculated the distance between the location of social service agencies and the residential location of each family in our analysis samples. Given that we plan to use this distance measure with a number of data sets (e.g., ECLS-K cohort and Fragile Families and Child Development Study), we utilized the following approach. Users of the ECLS-K and FFCW contract data are able to observe families' residential locations at the census tract level. Since child-care subsidies are distributed by agencies organized at the county level, we use the county as the geographic boundary for calculating the distances. As a result, we calculated the Euclidean (or as-the-crow-flies) distance (in miles) between the location of social service agencies and *every* census tract centroid in the county in which each agency resides. For example, La Paz County in Arizona has one social service agency and six census tracts. Therefore, our database contains six sets of distances associated with this agency: one for each census tract. In Maryland, Montgomery County also has one social serve agency but 176 census tracts. Our database contains the distance from this agency to each census tract in the county. Jurisdictions with multiple agencies have a set of distance calculations for each agency. For example, Baltimore City has nine agencies and nearly 200 census tracts, leading to approximately 1,800 separate agency-tract calculations. In addition to calculating the distance, we produced the census tract identification number associated with each agency-tract combination. We use the census tract code to merge the distance measure with families in our analysis samples. Although this process was extremely time intensive, the results provide us with the flexibility to append the distance measure to virtually any data set with census tract codes.

A potential concern with using the census tract centroid to create the travel distance is that it introduces a form of aggregation error (Hewko et al., 2002). Such measurement error plagues spatial accessibility indicators aggregated to a unit of

analysis (the census tract in this study) that varies substantially in size. Given that the land area of census tracts differs substantially across children in our ECLS-K sample, a distance measure based on its geographic center introduces a form of nonrandom measurement error, such that the amount of error is likely to increase with the size of the census tract (Apparicio et al., 2008). Large census tracts are more common in suburban and rural areas, indicating that measurement error in travel distance is likely to be more problematic for families residing in these neighborhoods. In results not reported in the paper, we attempt to deal with aggregation error by estimating IV models that control explicitly for census tract land area (defined as squares miles) and a quadratic in land area. The inclusion of these controls does not change the estimates on or the statistical significance of subsidy receipt.

APPENDIX B: EXOGENEITY OF TRAVEL DISTANCE TO SOCIAL SERVICE AGENCIES

As stated in the paper, there are at least two concerns regarding the exogeneity of travel distance to social service agencies. The first is that the distance measure could be determined in part by the joint location preferences of families and social service agencies. For example, given the low car ownership rates among low-income families, such individuals may prefer to live near critical support services or employment centers. In addition, administrative offices might locate in low-income neighborhoods to be accessible to large numbers of potentially eligible clients. If these unobserved family and agency preferences influence the distance measure in ways that affect child development, the coefficient on subsidy receipt will be biased.

Although endogenous location choices are plausible for entitlement programs or services with open-ended funding streams, it is unlikely that low-income parents move to a given neighborhood to be close to an agency administering child care subsidies. These benefits are heavily rationed by local agencies, and consequently it is common for parents to experience frozen intake or long waiting lists (Herbst, 2008). Therefore, it is risky to choose a residential location based on the location of these agencies. Endogenous preferences among social service agencies are also unlikely to be a problem in our analysis. According to Allard (2009), agency location choices are severely constrained, limiting the ability to adjust to changes in the spatial distribution of low-income families. These constraints may explain why one-fifth of the social service agencies in Allard's (2009) three-city study had been operating in the same location for six to 10 years, and over half were in the same location for over 10 years.

Nevertheless, we take a number of steps to mitigate the influence of endogenous family and agency location choices. First, we control extensively for the neighborhood environment in which ECLS-K families live. Specifically, we include 15 census-tract- and school-level variables in the child production function[21] These variables capture several dimensions of neighborhoods' wealth and resources, urbanicity, racial and ethnic composition, and family structure that either directly affect child development or are correlated with unobserved residential preferences related to travel distance. Second, we incorporate a comparable set of eight controls for the neighborhood environment in which social service agencies are

[21] The census tract (neighborhood) variables include the log of median household income; log of population density; percentage non-Hispanic white; percentage foreign born; percentage age 65 and over; percentage female; and percentage ages 0 to 2, 3 to 5, 6 to 11, 12 to 13, 14, and 15 to 17 in female-headed households. The school variables include the percentage of children in school who are eligible for free lunches, a dummy variable indicating whether a majority of children in school are racial/ethnic minorities, and a dummy variable indicating whether the school receives Title I funding.

located.[22] Finally, we include a vector of state fixed effects to account for state-level policy, economic, and demographic unobservables that may influence child development.[23]

The second concern with using travel distance as an instrument is that it may be a proxy for neighborhood and individual characteristics that influence child development. For example, it is possible that mothers facing shorter distances to an agency do so in part because they reside in high-population-density (urban) and low-income neighborhoods. Conversely, those with longer distances may reside in largely rural and racially homogenous areas. To the extent that the neighborhood environment directly affects or is correlated with individual characteristics that influence child development, we might be worried that the travel distance is systematically related to child development. If these environmental factors are not properly accounted for, the distance measure would not constitute a valid instrument.

In Appendix Table B1, we explore the extent to which child and family characteristics are random with respect to travel distance. Specifically, we present means for several key characteristics at each quartile of the distance distribution, controlling for the log of median household income and population density at the census tract level. We condition on these neighborhood attributes because they are likely to be important environmental factors that lead to the differential sorting of families in space. The last two columns display the F-statistic and p-value associated with a test of the null hypothesis of equivalent child and family characteristics over the distance distribution. We find strong evidence that, conditional on the neighborhood environment, child and family characteristics are not systematically related to the proposed instrument. In fact, in no case is the F-statistic large enough to reject the null hypothesis. Such results suggest that as long as the neighborhood controls are included in the production function, our IVs strategy is valid.[24]

Despite the evidence in support of our identification strategy, we test several variants of the baseline model to further bolster confidence in the validity and robustness of the distance instrument. Together, these add up to a total of six distinct versions of the IVs model. Appendix Table B2 provides summary information on the first-stage equation for each strategy. We show the total number of instruments in column (1) and the percentage change in the number of instruments from the

[22] These variables include log of median household income, log of population density, percent non-Hispanic white, percentage foreign born, percentage age 65 and over, percentage female, percentage of households receiving welfare, and percentage of employed females age 16 and over. These variables represent the county average of the census tract characteristics in which social service agencies are located.

[23] In results not presented in the paper, we take advantage of two items in the ECLS-K to further probe the role of endogenous family location decisions. The survey asks parents whether the current home location was chosen based on the attributes of local schools. Assuming that the demand for certain school characteristics is correlated with parental preferences for other public services and benefits, including those for child care, including this variable should help to purge the IV estimates of biases stemming from unobserved family location decisions. We also add a control for whether the family moved residences since the focal child's birth. This variable should in principle account for the opportunity to choose a home location based on the availability of public services among families that are more likely to move. The IV estimates on subsidy receipt are robust to the inclusion of these additional controls.

[24] In results not reported in the paper, we conduct a more stringent test of the randomness of travel distance with respect families' observable characteristics. In particular, we estimate separate regressions of child and family characteristics on the full set of distance-by-county instruments along with the child, family, and neighborhood controls and the state fixed effects. In these regressions, we find that travel distance continues to be largely uncorrelated with the observable determinants of child development. Importantly, the distance-by-county interactions are not systematically related to maternal educational attainment and household SES. The exceptions include the indicators for white, premature birth, and low birth weight, all of which are included in the baseline model.

Table B1. Child and family characteristics by distance quartile.

		Distance quartile				Test of equivalence	
Variable	Full sample	1st	2nd	3rd	4th	*F*-statistic	*p*-Value
Distance to social service agency (miles)	6.87	1.56	3.75	7.24	19.92		
Child characteristics							
Boy (percent)	0.499	0.490	0.503	0.501	0.481	0.35	0.788
White (percent)	0.380	0.468	0.462	0.487	0.477	0.50	0.685
Premature birth (percent)	0.184	0.179	0.183	0.174	0.175	0.09	0.965
Low birth weight (percent)	0.077	0.059	0.065	0.074	0.060	0.57	0.636
Fair/poor health (percent, fall of k)	0.046	0.039	0.036	0.028	0.043	0.72	0.543
Family characteristics							
Mother: age (years, fall of k)	30.45	30.56	30.89	30.89	31.24	1.48	0.217
Two or more other children (percent)	0.351	0.301	0.312	0.318	0.334	0.63	0.594
Mother: Less than high school (percent)	0.208	0.180	0.149	0.166	0.155	1.20	0.307
In bottom SES quintile (percent)	0.334	0.280	0.248	0.254	0.265	1.04	0.375
ln(family income) ($, fall of k)	9.664	9.783	9.874	9.930	9.842	1.65	0.175

Notes: Analyses are conducted on children and mothers with nonmissing data. Means are derived from an OLS regression of each child/family characteristic on four distance quartile dummy variables (with the constant omitted), the demeaned log of census tract median household income, and the demeaned log of census tract population density. The *F*-statistic (and *p*-value) is from a test of the null hypothesis of the equivalence of child/family characteristics over the quartiles of the distance distribution.

baseline model in column (2). Columns (3) and (4) show the overall R^2 and the incremental R^2 for each first-stage equation. Finally, the *F*-statistic and *p*-value on each set of exclusion restrictions are displayed in columns (5) and (6), respectively. For comparison purposes, the first row presents summary information for the baseline model, which contains 239 distance-by-county instruments. The incremental R^2 is 0.07, and the joint *F*-statistic 23.5.

The model in the second row replaces the vector of state fixed effects with county-group fixed effects. Although the ideal model would control for individual county dummies, we are precluded from doing so because of the strong correlation between the county dummies and the distance-by-county interactions. Instead, we create a set of county-group dummies by merging small numbers of adjacent counties into a single county cluster. Doing so reduces the number of geographic fixed effects from 239 counties to 90 county groups, and substantially lessens the multicollinearity problem. This approach still enables us to account for within-state differences in characteristics associated with travel distance and child development. With an *F*-statistic of 24, the distance-by-county interactions continue to be powerful predictors of subsidy receipt.

In the third row, we enrich the set of control variables by adding characteristics of the neighborhoods in which social service agencies are located. Adding these variables allow us to purge the estimates of confounding location preferences on both sides of the child care subsidy market.

In the final three rows of Appendix Table B2, we reduce the number of IVs. Although our expanded instrument set can increase the efficiency of the estimates,

Table B2. Summary information on the first-stage child-care subsidy receipt equation.

Instrument set	Number of instruments	Percentage change from baseline	Overall R^2	Incremental R^2	F-statistic on the excluded instruments	p-Value
Baseline model						
Full instrument set	239	–	0.140	0.070	23.52	0.000
Alternate models						
Add county-group fixed effects	239	0.000	0.151	0.067	24.02	0.000
Add controls for the neighborhood in which social service agencies are located	239	0.000	0.143	0.064	20.62	0.000
Reduce instrument set to top 75 percent of the subsidy-county correlation distribution	203	−0.151	0.159	0.089	25.09	0.000
Reduce instrument set to top 50 percent of the subsidy-county correlation distribution	177	−0.259	0.179	0.109	22.78	0.000
Reduce instrument set to top 40 percent of the subsidy-county correlation distribution	167	−0.301	0.201	0.131	31.45	0.000

Notes: All first-stage equations include the following child and family controls: child's gender, child's age, child's age squared, race/ethnicity, child's weight in the fall of kindergarten, premature birth, low birth weight, fair/poor health status, first-time kindergartner, mother's age, mother's educational attainment, number of other children in the family, English as the primary home language, log of family income, and urban residence. With the exception of the last model, all first-stage equations include the following neighborhood and state controls: log of median household income; log of population density; percentage non-Hispanic white; percentage foreign born; percentage age 65 and over; percentage female; percentage of children ages 0 to 2, 3 to 5, 6 to11, 12 to13, 14, and 15 to 17 living in female-headed households (all at the census tract level); percentage of children in the school eligible for free/reduced price lunch; an indicator for whether a majority of children in the school are minorities; an indicator for whether the school receives Title I funding; and state fixed effects. The social service agency neighborhood controls include the following: log of median household income, log of population density, percentage non-Hispanic white, percentage foreign born, percentage age 65 and over, percentage female, percentage of households receiving welfare, and percentage of employed females age 16 and over.

it can also lead 2SLS to perform poorly.[25] Our approach consists of first calculating the correlation between subsidy receipt and distance for families in each county, and then arraying all of the correlations in descending order of magnitude. Next, we limit the distance-by-county instruments to those falling within the top 75 percent, 50 percent, and 40 percent of the correlation-strength distribution. As shown in Appendix Table B2, this exercise reduces the instrument set by 15 percent, 26 percent, and 30 percent, respectively, from the baseline model. In addition, it results in a considerable increase in the incremental R^2 and first-stage F-statistic. Nevertheless, despite the evidence in support of our identification strategy, it is

[25] In particular, the "many-and-weak-instruments" problem is shown to produce bias in the 2SLS estimates that converge to the OLS estimates as the number of identifying instruments increases (Bound et al., 1995).

important to acknowledge that, because our strategy does not randomly assign subsidy benefits to low-income families, concerns remain over the presence of unobserved confounders.

APPENDIX C: HETEROGENEOUS EFFECTS OF CHILD-CARE SUBSIDY RECEIPT

Our IV estimates capture the effect of subsidy receipt on those whose treatment status is manipulated by the distance to the social service agency. This is referred to as the Local Average Treatment Effect (LATE; Angrist & Imbens, 1994). A key assumption here is the idea that distance to a social service agency has a causal impact on the probability of a mother taking up a child-care subsidy. We also need the assumptions of conditional independence (i.e., the distance is as good as randomly assigned among parents conditional on covariates) and monotonicity (i.e., distance may have no effect on some parents because they are "always takers" or "never takers"). Since the treatment consists of both "always takers" and compliers, the LATE obtained from the IV strategy is not equal to the average causal effect on the treated. In other words, our IV estimate provides the average treatment impact on the group whose child-care subsidy receipt is influenced by the distance measure. Accordingly, the subsidy decisions of "always takers" are not influenced by the instrument and thus do not contribute to the IV estimate. (Note that defiers, i.e., those for whom the likelihood of subsidy receipt is positively correlated with distance, are ruled out by the monotonicity assumption.) On the one hand, this is a limitation of the LATE since we cannot get the average treatment effect on all those who are treated. To the extent that compliers and "always takers" are different from each other, our IV estimates cannot serve as a guide for the behavior

Table C1. Robustness checks.

Outcome	(1) Inverse distance	(2) + TANF and SNAP controls	(3) Omit head start children	(4) Urban children	(5) Omit family income
Reading test score	−4.308**	−4.078*	−4.493**	−4.558*	−5.034**
	(2.023)	(2.101)	(1.969)	(2.510)	(2.046)
Math test score	−3.878**	−3.120*	−3.888**	−3.086	−3.937**
	(1.660)	(1.691)	(1.663)	(1.979)	(1.673)
Externalizing behavior	7.887***	7.226***	6.906***	5.808**	7.221***
	(2.238)	(2.239)	(2.071)	(2.552)	(2.109)
Internalizing behavior	3.120*	3.043	2.211	3.847*	3.082*
	(1.735)	(1.903)	(1.694)	(2.277)	(1.708)
Approaches to learning	−4.165*	−4.907**	−5.984***	−6.879***	−5.191**
	(2.343)	(2.288)	(2.138)	(2.416)	(2.155)
Self-control	−5.439**	−5.890**	−6.835***	−5.142*	−5.966***
	(2.423)	(2.417)	(2.284)	(2.659)	(2.298)
Interpersonal skills	−4.750*	−4.250*	−5.290**	−5.019*	−4.588*
	(2.492)	(2.462)	(2.411)	(2.816)	(2.374)
Fine motor skills	−0.229	−0.014	−0.844	0.926	−0.633
	(1.483)	(1.629)	(1.629)	(1.892)	(1.572)
Gross motor skills	−2.145	−2.451	−3.293*	−2.089	−3.033*
	(1.767)	(1.857)	(1.725)	(2.159)	(1.756)

Notes: Each cell presents the coefficient on child-care subsidy receipt in the year before kindergarten entry and the standard error (in parentheses) that is adjusted for clustering at the county level.
The subsidy coefficient is statistically significant at the *0.10, **0.05, and ***0.01 levels, respectively.

Table C2. Falsification tests of the baseline instrumental variables estimates.

Outcome	Two-parent families, top two SES quintiles (1)	Two-parent families, nonworking mothers (2)
Reading test score	0.275	−1.730
	(1.746)	(2.201)
Math test score	1.741	0.248
	(1.742)	(2.214)
Externalizing behavior	−0.319	0.864
	(1.623)	(0.872)
Internalizing behavior	4.245*	1.531
	(2.362)	(2.097)
Approaches to learning	−0.533	0.588
	(2.152)	(1.614)
Self-control	2.357	1.926
	(1.797)	(2.375)
Interpersonal skills	0.577	0.292
	(2.003)	(1.845)
Fine motor skills	0.522	−1.824
	(2.272)	(1.352)
Gross motor skills	−2.507	−3.157***
	(2.704)	(1.145)

Notes: Each cell presents the coefficient on predicted child-care subsidy receipt in the year before kindergarten entry and the standard error (in parentheses). Standard errors are adjusted for clustering at the county level. The estimates in column (1) are derived from a sample of children living in two-parent households in the top two SES quintiles. The estimates in column (2) are derived from a sample of children living in two-parent households in which the mother was never employed since the child's birth and was not enrolled in education or job training in the fall of kindergarten. The model includes controls for child's gender; child's age; child's age squared; race/ethnicity; child's weight in the fall of kindergarten; premature birth; low birth weight; fair/poor health status; first-time kindergartner; mother's age; mother's educational attainment; number of other children in the family; English as the primary home language; log of family income; urban residence; log of median household income; log of population density; percentage non-Hispanic white; percentage foreign born; percentage age 65 and over; percentage female; percentage of children ages 0 to 2, 3 to 5, 6 to 11, 12 to 13, 14, and 15 to 17 living in female-headed households (all at the census tract level); percentage of children in the school eligible for free/reduced price lunch; an indicator for whether a majority of children in the school are minorities; an indicator for whether the school receives Title I funding; and state fixed effects. The subsidy coefficient is statistically significant at the *0.10, **0.05, and ***0.01 levels, respectively.

of "always takers." For example, it may be the case that compliers are poorer than "always takers" in terms of income and assets (e.g., car ownership). Then, failing to control for income and assets would be a reason why our IV estimates cannot be generalized to "always takers." On the other hand, it is still very useful and policy-relevant especially since it provides insights into the behavior of individuals who are amenable to policy change.

APPENDIX D

Table D1. OLS and IV estimates of the impact of child-care subsidy receipt on maternal employment—Fall of kindergarten through spring of fifth grade.

Variable	(1) Fall kindergarten	(2) Spring first grade	(3) Spring third grade	(4) Spring fifth grade
OLS results				
Child-care subsidy receipt	0.075***	0.041*	0.032	0.024
	(0.019)	(0.024)	(0.026)	(0.033)
2SLS results				
Child-care subsidy receipt	0.114*	0.004	0.083	−0.021
	(0.067)	(0.083)	(0.093)	(0.089)
Number of observations	3,804	2,509	1,976	1,495
Percentage employed	0.731	0.773	0.785	0.755

Notes: Each cell presents the coefficient on child-care subsidy receipt in the year before kindergarten entry and the standard error (in parentheses) that is adjusted for clustering at the county level. All models include controls for mother's age and age-squared; mother's educational attainment; number of other children in the household; total household size; mother's race and ethnicity; mother's health status; log of total family income; child's gender; child's low birth weight status; whether the child is a first-time kindergartner; log of median household income; log of population density; percentage non-Hispanic white; percentage foreign born; percentage age 65 and over; percentage female; percentage of children ages 0 to 2, 3 to 5, 6 to 11, 12 to 13, 14, and 15 to 17 living in female-headed households (all at the census tract level); percentage of children in the school eligible for free/reduced price lunch; an indicator for whether a majority of children in the school are minorities; and an indicator for whether the school receives Title I funding. All models include state fixed effects. The instruments in the 2SLS models are the full set of distance-by-county interactions.
The subsidy coefficient is statistically significant at the *0.10, **0.05, and ***0.01 levels, respectively.