Journal of Public Economics 105 (2013) 86–105

Contents lists available at SciVerse ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

The impact of non-parental child care on child development: Evidence from the summer participation "dip"☆

Chris M. Herbst *

School of Public Affairs, Arizona State University, 411 N. Central Ave., Suite 480, Phoenix, AZ 85004-0687, United States and IZA

ARTICLE INFO

Article history:
Received 2 November 2012
Received in revised form 4 June 2013
Accepted 5 June 2013
Available online 21 June 2013

Keywords:
Non-parental child care
Child development
Maternal employment

ABSTRACT

Although a large literature examines the effect of non-parental child care on preschool-aged children's cognitive development, few studies deal convincingly with the potential endogeneity of child care choices. Using a panel of infants and toddlers from the Birth cohort of the Early Childhood Longitudinal Study (ECLS-B), this paper attempts to provide causal estimates by leveraging heretofore unrecognized seasonal variation in child care participation. Child assessments in the ECLS-B were conducted on a rolling basis throughout the year, and I use the participation "dip" among those assessed during the summer as the basis for an instrumental variable. The summer participation dip is likely to be exogenous because ECLS-B administrators strictly controlled the mechanism by which children were assigned to assessment dates. The OLS results show that children utilizing non-parental arrangements score higher on tests of cognitive ability, a finding that holds after accounting for individual fixed effects. However, the instrumental variables estimates point to sizeable negative effects of non-parental care. The adverse effects are driven by participation in formal settings, and, contrary to previous research, I find that disadvantaged children do not benefit from exposure to non-parental care.

1. Introduction

Over the past few decades, there has been a sharp increase in the share of U.S. preschool-aged children participating in non-parental child care arrangements. Currently, two-thirds of young children regularly attend some form of child care, with the average child spending 32 h per week in these settings (Laughlin, 2010). Moreover, the transition into non-parental care occurs rapidly after childbirth. According to the NICHD Study of Early Child Care (1997), the typical child first enters child care at approximately three months. Within the first year of life, 80% experience regular participation in non-parental arrangements, and over one-third have at least three distinct caregivers.

Catalyzed by the finding that early childhood health (e.g., Almond and Currie, 2010; Case et al., 2005, 2002) and educational experiences (e.g., Heckman & Masterov, 2007; Karoly et al., 2005) may have lasting effects on schooling and labor market success, scholars have devoted significant attention to studying the impact of various child care arrangements on child development. Recent work examines outcomes ranging from the incidence of injury and infectious disease to cognitive and social-emotional functioning. Although results from this work are mixed overall, two themes consistently emerge (Bradley and Vandell, 2007; Pianta et al., 2009). First, participation in center-based care has opposing effects on child development, producing small improvements in cognitive ability test scores while increasing behavior problems. Second, higher-quality settings produce more favorable short- and long-run outcomes, especially for economically disadvantaged children.

An important concern with much of this research is the insufficient attention paid to the potential endogeneity of child care choices. Families using non-parental arrangements may differ from those that do not in ways that cannot be fully accounted for even in richly specified child production functions. If these unobserved differences are correlated with measures of child development, a classic case of omitted variable bias arises, in which the estimated effect of non-parental care is confounded. To date, only a small number of U.S. studies attempt to handle these identification issues. One paper makes use of value-added specifications (NICHD ECCRN and Duncan, 2003), another three use fixed effects (Blau, 1999; Currie and Hotz, 2004; Gordon et al., 2007), and one implements an instrumental variables strategy (Bernal and Keane, 2011). The latter paper is noteworthy because it finds that increased child care time during the preschool years reduces cognitive ability test scores, a result at odds with much of the OLS literature. Therefore, considerable uncertainty remains over the developmental implications of non-parental child care utilization.

Using a panel of infants and toddlers from the Birth cohort of the Early Childhood Longitudinal Study (ECLS-B), this paper provides new evidence on the impact of *current* and *cumulative* child care participation on cognitive ability test scores. To estimate the causal effect of non-parental care, I introduce a novel empirical strategy that exploits

☆ I am grateful to participants at the 2012 meetings of the Association for Public Policy Analysis and Management and Southern Economic Association, participants at the 2013 meetings of the American Economic Association and Population Association of America, as well as Joanna Lucio, Erdal Tekin, Bob Bradley, Greg Duncan, Spiro Maroulis, Anna Johnson, Nikki Forry, Melanie Guldi, Lucie Schmidt, Jane Waldfogel, David Frisvold, Liz Ananat, Joseph Doyle, and three anonymous referees for helpful comments and suggestions.

* Tel.: +1 602 496 0458.
E-mail address: chris.herbst@asu.edu.

0047-2727/$ – see front matter © 2013 Elsevier B.V. All rights reserved.
http://dx.doi.org/10.1016/j.jpubeco.2013.06.003

heretofore unrecognized seasonal patterns in child care utilization. During the first two waves of the ECLS-B, children were assessed on a rolling basis throughout the year, and I use the child care participation "dip" experienced by those assessed during the *summer* as the basis for an instrumental variable (IV). In particular, the identifying instrument is a binary indicator for whether a given child was assessed between June and September. The estimated summer-induced drop in utilization is strong in the first-stage equation, and it pervades nearly all demographic groups and child care arrangements observed in the data.

I argue that the timing of ECLS-B assessments generates plausibly exogenous variation in child care utilization because parents were not given the opportunity to choose an assessment date. Instead, since the survey was designed to document age-specific developmental milestones, ECLS-B administrators assigned parents to assessment dates based on the focal child's birthday. Assessments for the first wave were initiated at the child's 9-month birthday, while those for the second wave occurred at 24-months. Nevertheless, the identification strategy must overcome two key threats to the validity of the summer assessment instrument. First, the presence of seasonal patterns in parental health, weather, and families' physical activity and eating patterns may directly affect children's cognitive development. Second, given that the instrument is mechanically related to the child's birthday, there could be unobserved family differences associated with season-of-birth that influence child well-being (Buckles and Hungerman, 2010). I take a number of steps to test for and mitigate the consequences of these confounding factors. I begin by providing evidence that families assessed during the summer are observationally equivalent to their counterparts assessed during other times of the year. Importantly, there is no indication that parents' employment status differs across the summer and non-summer months, nor is there evidence of seasonality in the demand for child care quality, underlying child health, or maternal mental health. I also conduct a series of robustness checks that include many of these labor market and health characteristics as controls in the child production function. Finally, I account for season-of-birth differences in family socioeconomic status by incorporating a variety of quarter- and month-of-birth controls in the production function.

The paper's main findings can be summarized as follows. I first show that children attending non-parental care are more economically advantaged than their peers in parent care. This positive selection suggests that OLS estimates of child care utilization are likely to be biased upward. I then recreate the standard OLS result in the literature that children attending non-parental care score higher on tests of cognitive ability, a result that holds when I account for individual fixed effects. However, the instrumental variables estimates point to sizeable negative effects of non-parental child care utilization. For example, baseline results for the measure of *current* participation suggest that test scores are approximately 0.29 standard deviations lower for children in non-parental settings. The negative effects are driven by participation in formal arrangements and are larger for children in economically advantaged families. Nevertheless, I show that disadvantaged children do not benefit from exposure to non-parental care.

The remainder of the paper proceeds as follows. The next section summarizes previous research examining the relationship between early non-parental care and child development. Section 3 introduces the key features of the ECLS-B analysis sample, and Section 4 develops the identification strategy. The estimation results are presented in Section 5 and Section 6 concludes.

2. Relevant literature

There is a vast literature in developmental psychology and economics examining the association between non-parental child care arrangements and child development.[1] Much of this work focuses on outcomes related to the mother–child relationship, injuries and communicable illnesses, behavior problems, and cognitive ability. This section summarizes previous research on cognitive ability – the most relevant outcome for the current study – with a focus on the short-run effects of infant and toddler child care arrangements.

A large number of studies focus on the implications of early non-parental care for children's cognitive development. Overall, results from this work tend to find beneficial effects of child care exposure (NICHD ECCRN, 2000), although some uncover neutral (Blau, 1999) or negative effects (Bernal and Keane, 2011; NICHD ECCRN, 2004). There is a growing consensus, however, that high-quality center-based settings produce favorable results (Hill et al., 2002; NICHD ECCRN and Duncan, 2003; Peisner-Feinberg et al., 2001), especially when child care teachers engage in cognitively stimulating interactions with children (NICHD ECCRN, 2000). The positive effects of early center-based care tend to be larger for economically disadvantaged children (Loeb et al., 2004), and they are found to persist throughout the school-age years (Belsky et al., 2007).

Despite this extensive literature, most papers do not deal adequately with the potential endogeneity of child care choices. The most common strategy is to estimate the child production function using OLS regression, conditioning on a rich set of observable family characteristics. However, OLS estimates of child care utilization may still be inconsistent because families using non-parental care may differ from those that do not in ways that are difficult to capture. For example, families using child care may have stronger work preferences, face fewer constraints on obtaining work and child care, or place a higher value on socializing children at an early age. In addition, children exposed to non-parental care may have qualities that parents wish to enhance (e.g., high cognitive ability) or ameliorate (e.g., disabilities). In other words, child well-being could itself be a determinant of parental child care decisions. Failure to account for these systematic differences across families will yield inconsistent estimates of the impact of non-parental child care utilization.

To my knowledge, only a few studies attempt to handle these identification problems. One paper makes use of value-added specifications (NICHD ECCRN and Duncan, 2003), another three use fixed effects (Blau, 1999; Currie and Hotz, 2004; Gordon et al., 2007), and one implements an instrumental variables (IV) strategy (Bernal and Keane, 2011).[2] Identification in value-added models is achieved by conditioning on pre-child-care-use (or lagged) cognitive ability, which is assumed to capture the child's ability endowment as well as unobserved historical inputs. However, as Todd and Wolpin (2003) show, endogeneity problems arise when lagged cognitive ability is correlated with the unobserved contemporaneous determinants of ability.[3] The primary advantage of the individual fixed effects model is that it compares a child's cognitive ability in periods of child care exposure with the ability of the same child in periods of non-exposure. Although this within-child estimator accounts for time-invariant unobservables, a concern is that omitted time-varying inputs will still lead to inconsistent estimates.

The paper by Bernal and Keane (2011) represents the only other attempt to use IV methods in the child care–child development literature. The identification strategy uses 78 social policy variables (e.g., welfare reform rules) to instrument for child care time in a sample of single mothers from the NLSY. The paper's main finding is that each year of child care exposure reduces preschool-aged children's cognitive ability test scores by 2.1%, with participation in informal care driving the negative effects. Although the policy instruments are reasonably powerful in the first-stage equation ($F = 14.7$), it is

[1] Comprehensive reviews are found in Bernal and Keane (2011), Bradley and Vandell (2007), and Pianta et al. (2009).

[2] The NICHD ECCRN and Duncan (2003) paper focuses on cognitive ability test scores; Blau (1999) focuses on behavior problems and cognitive ability test scores; and Currie and Hotz (2004) and Gordon et al. (2007) focus on injuries and illnesses.

[3] For example, parents in the current period may engage in optimizing behavior in response to child well-being in previous periods.

possible that many of them directly influence child development, thus potentially invalidating them as instruments.[4] Another concern is that the measure of child care time is derived from mothers' employment history, making it difficult to separately identify the impact of child care exposure and maternal employment.[5] The current paper builds on Bernal and Keane (2011) by introducing a new instrument to produce credible estimates of the impact of non-parental child care arrangements. The strategy developed here is advantageous because it exploits the ECLS-B's birthday-based assessment schedule, a design feature that produces plausibly exogenous seasonal variation in child care participation. In addition, this feature is replicated in other surveys of early childhood (e.g., Fragile Families and Child Well-Being Study); thus future work can exploit this identification strategy to examine other developmental outcomes.

3. Data

Data for this research are drawn from the Birth cohort of the Early Childhood Longitudinal Study (ECLS-B), a nationally representative sample of approximately 11,000 children born in 2001. The survey was designed to track children's early home and educational experiences by conducting detailed parent and child care provider interviews and initiating a battery of child assessments at various points between birth and kindergarten entry. The first wave of data collection occurred when focal children were 9-months-old (2001–2002), with follow-up surveys implemented at 24-months (2003), during the preschool year (2005–2006), and after kindergarten entry (2006–2007).

The analysis sample is a panel of children from the 9- and 24-month waves of data collection. In particular, a home visit was initiated on or near the focal child's 9- and 24-month birthday to administer cognitive and psychomotor assessments and conduct a 60-minute parent interview (which collected information on demographic and labor market characteristics, family well-being, and child care utilization). Exclusions from the sample are made if a child is missing information on the month-of-assessment (1930) or the type of non-parental child care utilized (30).[6] I retain children with at least one non-missing cognitive ability test score from the 9- and 24-month assessments. The result is an unbalanced panel of 10,477 children, providing 19,416 child–wave combinations.[7]

The outcome is a measure of children's early cognitive ability from the Bayley Short Form—Research Edition (BSF-R) test. This instrument was designed specifically for the ECLS-B and includes a subset of items from the full Bayley Scales of Infant Development—Second Edition (BSID-II), a widely used measure of early cognitive and motor development. Although the original BSID-II was designed to be completed in a clinical setting, the BSF-R was developed for ease of administration in a home environment. This study examines only the cognitive component of the BSF-R, containing 31 items during the 9-month survey and 33 items during the 24-month survey. The test assesses several dimensions of early cognitive and language ability, including memory, preverbal communication, expressive and receptive vocabulary, reasoning and problem solving, and concept attainment. Item response theory (IRT) scale scores are used in the analysis.

This study examines two measures of non-parental child care utilization. To capture the short-run effect of child care, I create a measure of *current* participation in any non-parental arrangement, defined as a binary indicator that equals unity if a given child receives – at the time of assessment – regular care from relatives (inside or outside the focal child's home), non-relatives (e.g., friends, neighbors, nannies, or family-based care inside or outside the focal child's home), or center-based services (e.g., nursery or preschools, for-profit centers, or non-profit church organizations).[8] Auxiliary analyses explore more nuanced measures of child care settings by comparing informal and formal child care arrangements as well as relative, non-relative, and center-based settings.[9] I also examine the longer-run effect of child care by constructing a measure of *cumulative* participation in non-parental arrangements, defined as the total number of months of participation at each assessment. I construct this measure by combining information on the age (in months) at which the focal child was first placed in non-parental care with information on the child's participation status at the 9- and 24-month assessment. For example, to determine the number of months of child care exposure among children using care at 9-months, I subtract the age at which the child first began using non-parental care from the age-at-assessment. This figure both approximates cumulative utilization during the first nine months of life, and it is added to the months of exposure that the child accrued between the 9- and 24-month assessments.

Table 1 presents summary statistics for the analysis sample. Children receiving non-parental care at 9-months score one point higher on the BSF-R than their counterparts in parent care, a test score gap that grows to approximately two points by the 24-month assessment. Children in non-parental and parental care are equally likely to be male, classified as low birth weight, or born prematurely. However, there is consistent evidence that children attending non-parental care are more advantaged than their peers in parent care. The former group is more likely to have mothers who are employed and who completed at least a bachelor's degree. In addition, household income is approximately 25% greater among those using non-parental arrangements. Such comparisons are useful because they indicate the potential direction of the OLS bias in the child care estimates. In particular, it appears

[4] For example, state-level child support enforcement expenditures are likely to change the behavior of children's biological fathers in ways that affect child well-being (e.g., through increased time investments). Welfare policies such as child-age-exemptions from work requirements may influence women's fertility decisions, and therefore optimizing behavior regarding quality–quantity trade-offs in maternal investments. Moreover, welfare work requirements and time limits may affect family well-being in ways that are unrelated to child care and work decisions. Finally, local labor market conditions are known to affect health through non-labor-market mechanisms (e.g., through changes in consumption and health-related behaviors).

[5] Indeed, the authors are able to leverage reasonably good explanatory power in the first-stage because the measure of child care time is closely linked to maternal employment, and polices such as welfare reform and the EITC have had large effects on single mothers' employment. This is problematic because early maternal employment itself has implications for child development (Brooks-Gunn et al., 2002; Ruhm, 2004; Morrill, 2011).

[6] Of the 1930 children missing information on the month-of-assessment, 1903 do not have a complete set of assessment data available. Children are without assessment data (which includes the Bayley Short Form—Research Edition test) primarily because a home visit could not be scheduled or parents did not consent to a visit. The remaining 27 children were dropped because survey administrators did not record information on the date of the home visit.

[7] An analysis of the characteristics of children and families excluded from the analysis sample finds some important differences with those retained for the analysis. Children excluded from the analysis are less likely to be white, on average, and are more likely to have been born prematurely. Excluded mothers are less likely to be both married and single (never married). In addition, these women are less likely to have at least a B.A. degree and to be employed. Many of these differences are present only in the 24-month wave (i.e., the child and family characteristics are quite similar at 9-months), suggesting that economically disadvantaged families were more likely to leave the ECLS-B over time. Full results from this analysis are available from the author upon request.

[8] To construct the binary indicator of current child care participation, I draw on a series of questions embedded in the child care module of the parent questionnaire. First, to ascertain whether the child attends relative care: "Is {CHILD/TWIN} now receiving care from a relative other than a parent on a regular basis, for example, from grandparents, brothers or sisters, or any other relatives?" Second, to ascertain whether the child receives non-relative (e.g., family-based) care: "Now I'd like to ask you about any care {CHILD/TWIN} receives from someone not related to {him/her} in your home or someone else's home on a regular basis…Is {CHILD/TWIN} now receiving care in a private home on a regular basis from someone who is not related to {him/her}?" Finally, to ascertain whether the child attends a center: "Now I want to ask you about child care centers {CHILD/TWIN} may attend. Such centers include early learning centers, nursery schools, and preschools. Is {CHILD/TWIN} now attending a child care center on a regular basis?" After each question, parents may respond "yes," "no," or "refused/don't know." There is no hours-of-participation requirement created by ECLS-B administrators to be coded as participating in one of the child care arrangements.

[9] Informal settings include relative care in any home and non-relative care in the focal child's home. Formal settings are defined as non-relative care outside the child's home (i.e., family-based settings) and all forms of center-based care. In all cases, the omitted category includes children in parent care.

Table 1
Summary statistics.

Variable	Full sample	Non-parental child care	Parental child care
Outcome			
Bailey Short Form test score (9-months)	74.84	75.34	74.34***
	(10.08)	(10.05)	(10.07)
Bailey Short Form test score (24-months)	125.53	126.40	124.67***
	(10.99)	(11.05)	(10.87)
Child characteristics			
Male (%)	0.511	0.512	0.510
	(0.500)	(0.500)	(0.500)
Age (months)	17.14	17.15	17.12
	(7.14)	(7.13)	(7.14)
White (%)	0.421	0.409	0.432***
	(0.494)	(0.492)	(0.495)
Low birth weight (%)	0.263	0.262	0.264
	(0.440)	(0.440)	(0.441)
Premature birth (%)	0.115	0.114	0.116
	(0.319)	(0.318)	(0.320)
Weight (kilograms)	10.85	10.92	10.77***
	(2.44)	(2.42)	(2.45)
Family characteristics			
Mother's age (years)	29.17	29.11	29.23
	(6.67)	(6.71)	(6.63)
Mother is single, never married (%)	0.266	0.294	0.238***
	(0.442)	(0.456)	(0.426)
Mother is married (%)	0.668	0.626	0.708***
	(0.471)	(0.484)	(0.455)
Mother is a high school drop-out (%)	0.179	0.129	0.227***
	(0.383)	(0.336)	(0.419)
Mother has a BA + (%)	0.269	0.300	0.238***
	(0.443)	(0.458)	(0.426)
Mother is employed (%)	0.523	0.818	0.233***
	(0.500)	(0.386)	(0.423)
Household income ($/1000)	51.558	57.317	45.871***
	(45.402)	(48.518)	(41.321)
Focal child is an only-child (%)	0.335	0.382	0.287***
	(0.472)	(0.486)	(0.453)
Urban residence (%)	0.847	0.842	0.851
	(0.361)	(0.364)	(0.357)

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard deviations are displayed in parentheses. The means come from the pooled 9- and 24-month sample. ***, **, * indicate that the difference in means between those using non-parental child care and those using parental child care is statistically significant at the 0.01, 0.05, and 0.10 levels, respectively.

that children receiving non-parental care are positively selected, suggesting that the OLS estimates are biased upward.

Table 2 provides information on children's participation in non-parental arrangements over the infant and toddler years. Panel A shows the participation rate and weekly hours of participation for the measure of current child care utilization; Panel B shows data on cumulative participation; and Panel C presents participation rates disaggregated by informal and formal care. Consistent with previous work, children in the ECLS-B experience intensive non-parental care early in life: nearly half spend time in any arrangement as of the 9- and 24-month assessments, and they are engaged in these settings for approximately 33 h per week. Children accumulate about four months of child care exposure by their 9-month birthday, and 11 months of exposure by their 24-month birthday. The use of informal care becomes less common as children age, while formal care becomes more common: approximately 19% of children participate in formal arrangements at 9-months, rising to 27% at 24-months.

4. Empirical framework

4.1. Basic estimation strategy: OLS and individual fixed effects models

The empirical models described below are based on the Becker (1965) and Leibowitz (1974) theoretical framework in which the

Table 2
Child care participation profile at 9- and 24-months.

Variable	Pooled periods	9-month survey	24-month survey
Panel A: Current child care utilization			
Participation rate (%)	0.497	0.499	0.494
	(0.500)	(0.500)	(0.500)
Average weekly hours (no.)	32.68	32.15	33.28
	(17.26)	(18.26)	(16.06)
Panel B: Cumulative child care utilization			
Months of participation (no.)	6.76	3.66	10.92
	(8.09)	(3.96)	(10.09)
Panel C: Informal and formal child care utilization			
Informal participation rate (%)	0.270	0.301	0.227
	(0.444)	(0.462)	(0.419)
Average weekly hours (no.)	28.49	27.41	30.12
	(16.72)	(16.96)	(16.23)
Formal participation rate (%)	0.224	0.186	0.265
	(0.417)	(0.389)	(0.441)
Average weekly hours (no.)	32.20	32.12	32.26
	(13.61)	(13.91)	(13.39)

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard deviations are displayed in parentheses. Informal care includes relative care in any home and non-relative care in the child's home. Formal care includes non-relative care in another's home and center-based care. The figures for average weekly hours in Panel A represent the sum of hours in non-parental child care over all arrangements. The analogous figures in Panel C represent the number of hours in informal or formal care for the arrangement in which the child spent the greatest number of hours. All hours of care calculations are based on the sub-set of children using non-parental child care.

household is assumed to be a productive unit that makes decisions about the allocation of time and material resources. These decisions are aimed at maximizing a household utility function of the form $U(T, A, G; x_1, x_2,\ldots,x_n)$, where T represents maternal time in leisure; A is a latent measure of child quality (or ability); G captures a set of goods and services that enhance family well-being; and x is a series of exogenous preference shifters. This study is concerned with the estimation of the household demand for child quality, A, which, in its most general form, can be specified through the following cognitive ability production function:

$$\ln(A_{it}) = \beta_1 T_{it} + \beta_2 C_{it} + \beta_3 G_{it} + \mathbf{Z}'\beta + \mu_{it}, \quad (1)$$

where T represents maternal time inputs (parental child care) for the ith child in each period, t; C is a measure of time spent outside of maternal care (non-parental child care); $\mathbf{Z}'$ is a matrix of observable child and family characteristics related to the child's ability endowment; and μ captures the unobserved time invariant and time varying determinants of child ability.

As others note, a number of data and conceptual challenges make it infeasible to estimate Eq. (1) in practice (e.g., Bernal and Keane, 2011; Todd and Wolpin, 2007). First, the inputs to child ability are assumed to be measured in each period and to have period-specific effects on the development pathway. To avoid the large number of variables necessary to support such a model, most papers make the simplifying assumption that inputs have contemporaneous or cumulative effects on ability. Another challenge is that maternal and non-maternal time inputs are not measured directly in most survey datasets. Studies typically enter indicator variables for maternal employment and non-parental child care utilization to proxy for time inputs. Such controls, however, do not account for the level of quality in maternal and non-maternal time. Therefore, β_1 and β_2 are the commingled effects of the quantity and quality of maternal and non-maternal time, respectively. Finally, OLS regression will produce inconsistent estimates of the child production function if the unobserved determinants of ability are correlated with the time and goods inputs. Most studies attempt to surmount the omitted variables problem by incorporating a rich set of child and family characteristics in $\mathbf{Z}'$.

Circumventing these data constraints leads to an estimable version of the baseline child production function:

$$\ln(A_{its}) = \alpha_t + \eta_1 NP_{its} + \mathbf{Z}'\eta + \upsilon_s + \mu_{its}, \quad (2)$$

where i indexes children, t indexes time (i.e., survey wave), and s indexes state of residence; $\ln(A)$, the natural logarithm of the child's BSF-R score, is a proxy for latent cognitive ability; α is a binary indicator that captures general time effects or survey design differences between the 9- and 24-month assessment; NP is either the binary indictor of current non-parental child care utilization or the continuous measure of cumulative participation; $\mathbf{Z}'$ is a matrix of observable child and family determinants of cognitive ability, including maternal employment (T) and household income (G); υ is a set of state fixed effects aimed at capturing permanent economic, policy, and cultural differences across jurisdictions that may influence child ability; and μ represents the unobserved time invariant and time varying components of ability.[10] The model also includes interactions between the survey wave indicator, α, and two sets of variables: the family inputs in $\mathbf{Z}'$ and the state fixed effects. The interactions allow for the possibility that contemporaneous family and environmental inputs have different effects on child ability at each assessment point.

The model specified in Eq. (2) is estimated on the panel of ECLS-B children using OLS regression. For the binary measure of NP, the coefficient of interest, η_1, provides an estimate of the average difference in BSF-R scores between infants and toddlers participating in non-parental child care and those using parent care. Given that η_1 is derived from relating BSF-R scores at time t to child care utilization in the same period, the estimate may be interpreted as the short-run effect of participating in non-parental arrangements. It should be noted, however, that children observed using non-parental care at t are likely to have started their spells at different ages, thus leading to different time horizons over which the child care effects can manifest. Nevertheless, to explicitly unpack the longer-run implications of child care utilization, NP is also specified as cumulative participation, in which case η_1 is interpreted as the estimated effect on BSF-R scores of an additional month in non-parental child care settings.

The coefficient η_1 in Eq. (2) is identified through a cross-sectional comparison of BSF-R scores between children utilizing non-parental and parental care. Although this empirical strategy is the most common in the child development literature, estimates derived from this model are likely to be inconsistent because of the omitted variables problem discussed earlier. Thus, I formulate a more convincing identification strategy by exploiting the panel structure of the ECLS-B and including individual fixed effects in the production function. Formally, the fixed effects model is specified as follows:

$$\ln(A_{its}) = \alpha_t + \eta_1 NP_{its} + \mathbf{Z}'\eta + \gamma_i + \mu_{its}, \quad (3)$$

where γ is a set of child-specific effects. The key advantage of the fixed effects is that they account for all unobserved, time-invariant child- and family-level characteristics that are correlated with non-parental child care utilization and child ability. The identification of η_1 does not come from cross-sectional comparisons of different children, but rather from comparisons of the same child over time. This method, however, is not without limitations. Importantly, it does not eliminate sources of time-varying heterogeneity. It is possible, for example, that parental tastes for work and child care evolve over time, or that parental inputs respond to changes in the child's development pathway. If left unobserved such factors could bias η_1 in the fixed effects model.

4.2. Instrumental variables strategy

To deal with the selection problems that arise in Eqs. (2) and (3), an instrumental variables (IV) approach may be appropriate in the absence of a research design that randomly assigns children to parental and non-parental care. The IV method will produce consistent estimates of the impact of non-parental care if at least one variable is found to satisfy two conditions: (i) it is highly correlated with child care participation, and (ii) it is orthogonal to child ability except through its relationship with child care participation. This paper leverages identifying variation through seasonal patterns in child care utilization, in particular, by exploiting the participation dip experienced by focal children assessed during the summer months.

During the first two waves of the ECLS-B, parental questionnaires and child assessments were administered on a rolling basis throughout the year. This interview structure was necessary because ECLS-B administrators sought to assess focal children and inquire about child care participation (among other topics) as close to the 9- and 24-month birthday as possible.[11] Appendix Table 1 provides information on the assessment schedule as well as the number of completed assessments each month.[12] The 9-month survey commenced in October of 2001 and was finished in December of 2002, while the 24-month survey was administered between January and December of 2003. A reasonably consistent number of assessments were completed each month, including the summer months, which, for the purposes of this study, are defined as June, July, August, and September. These months are chosen because they coincide with the summer vacation schedule of most U.S. public school systems (Council of Chief State School Officers, 2002). Fully 29 states contained school districts that began the 2001–2002 school year in September, and the mandated 180-day school-year indicates that June marked the start of summer vacation for most children. Therefore, the instrumental variable is a binary indicator that equals unity if a given 9- or 24-month child assessment was conducted between June and September, and zero otherwise (i.e., non-summer months). In sensitivity tests, I alter the definition of the instrument to include June through August assessments and July through September assessments. The results are robust to these definitions.

4.3. The summer child care participation "dip"

There are several reasons to expect a summer participation dip in non-parental care among preschool-aged children. Families are more likely to rearrange work schedules to accommodate vacations and extended trips, especially those with school-aged children for whom school is no longer in session. Teenage siblings – also no longer in school and potentially available to watch younger children at various times throughout day – could be viewed by parents as a mechanism for temporarily reducing child care expenses. In addition, many child care directors and teachers may use the summer months to take their own vacation time. This may be particularly true among informal providers, including babysitters and relatives, as well as family-based workers. As a result, employment in the child care

[10] The $\mathbf{Z}'$ includes child characteristics such as gender, age (up to a quartic polynomial), race and ethnicity (four dummy variables), low birth weight (one dummy variable), premature birth (one dummy variable), and weight (in kilograms). It also includes family controls such as mother's age (up to a squared polynomial), marital status (four dummy variables), mother's education (three dummy variables), the presence of other siblings in the household (three dummy variables), mother's employment status (one dummy variable), maternal occupation (23 dummy variables), total household income (12 dummy variables), and urban residence (one dummy variable).

[11] In contrast, the preschool and kindergarten waves were fielded largely in the fall of children's entry into preschool and kindergarten in order to measure baseline development at the start of each school year and to enable researchers to examine changes in development in the year prior to kindergarten entry.

[12] Although it might be preferable to have the precise *day*-of-assessment, only the month- and year-of-assessment are available.

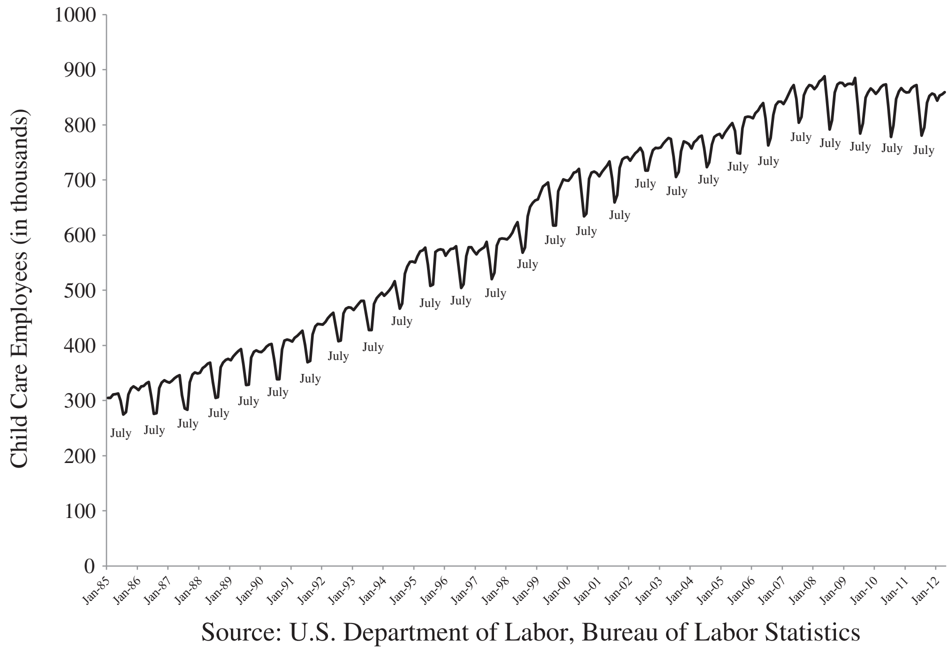

Fig. 1. Seasonal variation in the child care workforce.

sector overall is likely to contract throughout the summer, leaving parents of preschool-aged children with fewer options outside the home.

To my knowledge, only two previous studies compare preschoolers' child care utilization across the summer and non-summer months. Using the National Survey of America's Families, Capizzano et al. (2002) find that participation in center-based programs decreases from 32% during the school year to 23% in the summer and participation in relative care drops from 33% to 27% among preschool-aged children of employed mothers. The observed rise in parent care – from 28% to 35% – almost fully explains these declines. A recent paper by Laughlin (2010), which uses the Survey of Income and Program Participation, confirms this participation dip among working mothers, and provides evidence that children of non-working mothers are also less likely to participate in non-parental care.

Seasonal patterns in the child care market are also evident on the supply-side. Fig. 1 depicts the trend in the monthly number of child care employees between 1985 and 2012. These data are drawn from the Bureau of Labor Statistics' Current Employment Statistics (CES), an establishment-level survey of non-farm employment and earnings. Although the data show a secular rise in the number of child care employees over the past three decades, there are clear seasonal patterns in employment. The child care sector begins to contract in June, reaching a low in July, before returning to pre-summer employment levels in October. The magnitude of this seasonal pattern is fairly large. Since 2000, for example, child care establishments shed approximately 32,000 workers, on average, between May and June and another 44,000 workers between June and July. These losses represent 9.3% of the pre-summer child care workforce.

Seasonality in non-parental child care utilization can be examined formally in the ECLS-B through the following first-stage equation:

$$NP_{its} = \alpha_t + \psi_1 SUMMER_{its} + \mathbf{Z}'\eta + \upsilon_s + \varepsilon_{its}, \tag{4}$$

where *NP* is the measure of current or cumulative participation in non-parental child care arrangements; and *SUMMER* is a binary indicator that equals unity if a given child was assessed (and the corresponding parent was interviewed) during the summer. All other controls are identical to those appearing in Eq. (2). The model is estimated on the pooled set of child–wave combinations from the 9- and 24-month surveys, and the standard errors are adjusted for within-child clustering.

Table 3 presents the first-stage estimates for current (Panel A) and cumulative (Panel B) child care utilization. Column (1) includes the child and family controls, column (2) adds the state fixed effects (with wave interactions), and column (3) adds a control for the child's quarter-of-birth.[13] The state fixed effects account primarily for cross-state policy differences regarding school start and end dates that may be correlated with *SUMMER* and parental child care decisions. The quarter-of-birth control captures unobserved parental preferences for child care arrangements that vary with the focal child's season-of-birth.

It is clear from Table 3 that there are seasonal patterns in non-parental child care utilization. In particular, children assessed during the summer experience a participation dip relative to their counterparts assessed during other times of the year. As shown in Panel A, the coefficient on *SUMMER* in column (3), which is considered the main first-stage equation, indicates that children assessed during the summer are 3.1 percentage points less likely to participate in any non-parental arrangement. With an F-statistic of 24, the *SUMMER* instrument is quite strong and should allow for precise estimates of the endogenous variable, *NP*, in the production function. Turning to Panel B, it is clear that *SUMMER* is also strongly correlated with the cumulative measure of *NP*. Children assessed during the summer participate in about one-half fewer months of non-parental care than their counterparts assessed during other times of the year. The F-statistic on *SUMMER* is nearly 20. In regressions not reported here, I estimate a version of Eq. (4) that replaces the single *SUMMER* instrument with separate month-of-assessment indicators (January is the omitted month). The seasonal pattern in child care utilization is evident in this analysis. All four summer month dummies are negative and statistically significant (magnitudes for current participation range from −0.033 in June to −0.042 in September), while the remaining month-of-assessment dummies are small in magnitude and never statistically significant.

Appendix Table 2 shows that the summer-induced participation drop pervades most demographic sub-groups. Each row reports the

[13] The quarter-of-birth control is a binary indicator that equals unity if a given child was born in the fourth quarter (i.e., October, November, or December) of 2001.

Table 3
First-stage estimates of the relationship between ECLS-B assessment timing and non-parental child care utilization.

Variable	(1)	(2)	(3)
Panel A: Current child care utilization			
Summer assessment	−0.030***	−0.031***	−0.031***
	(0.006)	(0.006)	(0.006)
F-statistic (p-value)	23.49	25.62	23.99
	(0.000)	(0.000)	(0.000)
Panel B: Cumulative child care utilization			
Summer assessment	−0.451***	−0.467***	−0.442***
	(0.096)	(0.096)	(0.010)
F-statistic (p-value)	22.00	23.78	19.63
	(0.000)	(0.000)	(0.000)
Child characteristics	Y	Y	Y
Family characteristics	Y	Y	Y
Family characteristics × wave	Y	Y	Y
State FE with wave interactions	N	Y	Y
Fourth quarter of birth (QOB-4)	N	N	Y

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. The summer assessment variable is a binary indicator for whether a given child was assessed (and parents were interviewed) in the summer (i.e., June, July, August, or September) during the 9- or 24-month survey. The dependent variable in Panel A is a binary indicator for participation in any non-parental child care arrangement. The dependent variable in Panel B is a continuous measure of the number of months of participation in non-parental arrangements. The child characteristics include gender, age (up to a quartic polynomial), race and ethnicity (four dummy variables), low birth weight (one dummy variable), premature birth (one dummy variable), and weight (in kilograms). The family controls include mother's age (up to a squared polynomial), marital status (four dummy variables), mother's education (three dummy variables), the presence of other siblings in the household (three dummy variables), mother's employment status (one dummy variable), maternal occupation (23 dummy variables), total household income (12 dummy variables), and urban residence (one dummy variable). Panel B includes a control for the age children began their first non-parental arrangement. The quarter-of-birth control is a binary indicator for births in the fourth quarter (i.e., October, November, or December). All models include a binary indicator for wave. N = 19,416 in Panel A. N = 17,742 in Panel B. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

coefficient on *SUMMER* from Eq. (4) estimated on different child and parent characteristics. Of particular note is that the magnitude of the summer participation dip is similar across working and non-working mothers and high- and low-SES families. The two exceptions are found at the bottom of the table, in which first-stage regressions are estimated by the number of adults (ages 18 and over) in the household and total household size. The coefficient on *SUMMER* indicates that the participation dip increases monotonically with the focal child's household size. These results seem reasonable in light of the discussion earlier that families with multiple (older) children or adults are more likely to stop purchasing child care in the summer because these individuals may be available to assist parents with the care of young children.

The finding that young children during the summer spend increased time in parental care raises several important questions. First, it is useful to know whether the rise in parental care is offset by a reduction in maternal employment or other changes in work behavior. Second, it is important to determine the types of activities in which children are engaged with parents, and whether they are likely to be high-quality investments. Such information will highlight the potential mechanisms through which the IV estimates operate. The analyses in Appendix Tables 3 through 5 provide detailed evidence on these issues.[14] Briefly, four key findings emerge: (i) mothers interviewed in the summer are equally likely to be employed, to be working full- or part-time, and to be looking for work as their counterparts interviewed during the non-summer months; (ii) the summer-induced reduction in child care use occurs primarily among mothers loosely or not attached to the labor force (i.e., looking for work or outside the labor force) as well as those with flexible work schedules (i.e., working from home); (iii) time investments in the focal child increase substantially during the summer, as evidenced by the increased frequency of several parent–child activities (e.g., sharing meals and visiting the zoo and museums); and (iv) these investments are made disproportionately by the same families experiencing the largest drops in summer child care utilization.

Together, Appendix Tables 2 through 5 provide information on how to interpret the IV estimate of *NP*, or the local average treatment effect (LATE). Generally speaking, LATE reflects the impact of the variable of interest on the compliers, defined as those whose behavior is altered by the instrument. Compliers in this study are families whose child care decisions vary by the timing of the child's assessment. The analyses discussed above indicate that complier families are likely to be larger than non-complier families. The larger family size means that focal children are potentially cared for by multiple individuals, perhaps on an ad hoc basis and for brief time periods. In addition, mothers in complier families are likely to be looking for work or outside the labor force and, if employed, operate in flexible work environments (e.g., self-employed or working from home). Finally, given that *SUMMER* is uncorrelated with parental work behavior, the IV estimates should not reflect the commingled effect of simultaneous changes in work and child care use; rather, the estimates likely operate through increased time investments by mothers with already flexible work-family schedules.[15]

4.4. The validity of the summer assessment instrument

In order for *SUMMER* to serve as an identifying instrument for non-parental child care utilization, it must be validly excluded from the child production function. The main concern is that the timing of ECLS-B assessments might be correlated with unobserved child characteristics or parental inputs that determine cognitive ability. This could have occurred for two reasons. First, ECLS-B administrators assigned families to an assessment month based on specific attributes of the focal child, or families selected into an assessment month for reasons that are related to child ability. For example, parents in some occupations might have found it convenient to choose a summer assessment because work and child care schedules were easier to rearrange after the school year ended.[16] Second, it is conceivable that children assessed during the summer might perform differently on cognitive ability tests even in the absence of changes to child care use. Specifically, summer-driven changes in parental health and family routines, exposure to weather and allergens, and physical activity and eating patterns may alter child health and cognitive development (e.g., Marshall et al., 2000; Merikanto et al., 2012). If families systematically sorted into assessment months or children exhibited seasonal patterns in health and development, then *SUMMER* would be invalidated if these factors are not adequately taken into account.

[14] Specifically, Appendix Table 3 explores whether mothers interviewed in the summer and non-summer months exhibit similar work behavior. Appendix Table 4 examines whether the summer participation dip is pronounced for mothers with more flexible work schedules or those who work in different environments. Appendix Table 5 draws on a battery of parent–child activity items to explore whether there are time-use differences across the summer and non-summer months.

[15] It is useful to compare the compliers in this study with those in the IV paper by Bernal and Keane (2011). Because they use welfare reform rules, which are known to be positively associated with single mothers' employment, the IV estimates in Bernal and Keane (2011) are likely to be particularly relevant for children residing with working mothers. Conversely, compliers in this study include mothers either looking for work or not attached to the labor force, making the IV child care estimates potentially less germane to children of working mothers. Nevertheless, the main IV estimates in both papers are quite similar.

[16] Alternatively, others may have requested a non-summer assessment for their child because they were employed in seasonal positions that demanded significant time investments throughout the summer.

Fortunately, parents in the ECLS-B were not given an opportunity to choose an assessment date. Instead, survey administrators linked the timing of assessments to a specific child characteristic, and I observe this assignment "rule" in the data. Specifically, assessments in the first wave were initiated at the focal child's 9-month birthday, while those in the second wave occurred at the 24-month birthday. The survey was structured in this manner to enable the ECLS-B to track age-specific developmental milestones. In principle, as long as parents complied with this assignment rule, the child's age should be the only characteristic related to variation in the timing of assessments. Conditioning on age in the production function would therefore enable *SUMMER* to serve as a valid instrument. On the other hand, if parents failed to comply with this assignment rule, the child's age may not fully explain the observed assessment date, as convenience and other criteria could have been used to schedule the assessment.

I find that parents overwhelmingly complied with the birthday assignment rule. Fully 71% of children during the first wave were assessed in the month immediately prior to, the month of, or the month immediately following the 9-month birthday, and approximately 90% of children during the second wave were assessed in the month immediately prior to, the month of, or the month immediately following the 24-month birthday.[17] Nevertheless, I conduct a number of robustness checks to ensure that children assessed outside of these windows do not drive the results.[18] The IV results are robust to these specification checks.

Consistent with the high compliance rates, I find strong evidence that children and parents interviewed during the summer months are observationally equivalent to their counterparts interviewed during other times of the year. Fig. 2 depicts trends in several child characteristics over the 9- and 24-month survey periods, while Fig. 3 displays a number of family characteristics. The horizontal axis shows each month-of-assessment, and the vertical axis shows the sample proportion with a given characteristic. None of the figures reveal evidence of strong seasonal patterns in the child and family characteristics, and in particular, there are no discontinuous changes in these characteristics at the beginning and end of the summer period.

Table 4 formalizes the raw trends through a series of regressions of each characteristic on separate indicators for the months included in *SUMMER* and the full set of controls in Eq. (2). Panel A displays the regression results for the child characteristics, and Panel B shows the family characteristics. Several observations are noteworthy. First, within a given characteristic, coefficients on the individual summer month dummies are often positively *and* negatively signed, suggesting the absence of clear seasonal patterns. In addition, very few of the individual summer dummies are statistically significant: of the 40 individual summer-month coefficients presented, only one is significantly different from zero. Finally, the F-statistics indicate that the set of summer dummies is never jointly significant for a given child or family characteristic.[19,20]

Seasonality in maternal employment deserves special attention. As previously stated, one concern is that the summer-induced reduction in child care utilization catalyzed a series of changes to maternal work behavior. If this is the case, the IV estimates of *NP* would represent the commingled effect of non-parental care and maternal employment. Appendix Table 3 explores this in detail by estimating regressions of various employment outcomes on the *SUMMER* instrument as well as the full set of controls in Eq. (2). Each outcome represents a different work margin. I find no evidence of seasonality in maternal employment at any work margin. The coefficients on *SUMMER* are small in magnitude and never statistically significant, suggesting that maternal employment rates are consistent across summer and non-summer months. Parallel sets of analyses on fathers as well as mothers in low- and high-income families similarly show that work behavior does not exhibit strong seasonal patterns.[21]

Another concern deals with the possibility of seasonal differences in the level of child care quality to which focal children are exposed. For example, parental preferences regarding provider characteristics might differ across the summer and non-summer months. It is also possible that providers offer bundles of services and activities that change throughout the year. If seasonality in child care quality corresponds to seasonality in utilization, the IV estimates of *NP* will confound a quality effect with a participation effect. Therefore, I examine directly whether the demand for child care quality varies between the summer and non-summer months. During the 24-month survey, the ECLS-B conducted interviews with the focal child's primary non-parental caregiver and observed a subset of center- and home-based settings to produce global ratings of structural and process quality.[22] Table 5 lists the set of global quality measures (Panel A), attributes of the center director (Panel B), and attributes of the child's caregiver (Panel C). Columns (1) and (2) show the coefficient on *SUMMER* from regressions of each child care characteristic on the full set of 24-month controls.[23] Overall, the estimates reveal

[17] In other words, 71% of children were between 8.0 and 10.9 months old in the first wave, and 90% of children were between 23.0 and 25.9 months old in the second wave. These figures are calculated by comparing the focal child's date-of-birth with the month in which the assessment was conducted. Presenting the figures in this three-month window prevents "penalizing" children whose birthdays are at the beginning (or end) of the month, but who were assessed in the last few (or first few) days of the previous (or next) month.

[18] I first add to the production function an explicit control for the amount of "error" in the timing of each focal child's assessment. Assuming that children with a chronological age of exactly 9.0 and 24.0 months in the first and second waves were assessed on their birthday (and hence there is no error in their assessment date), the error variable is constructed by subtracting these numbers from each child's age-at-assessment. Second, I use this variable to restrict the analysis to children assessed within certain bandwidths of assessment error. Specifically, I conduct an analysis that first omits children assessed more than one month earlier or later than the 9- or 24-month birthday, followed by an analysis that omits children assessed more than three months earlier or later than the set of birthdays.

[19] During the 9-month survey, parents completed 12 items from the Center for Epidemiologic Studies Depression (CES-D) scale. I use the CES-D to explore seasonality in maternal mental health. Regressions of the CES-D scale on the summer assessment dummies (as well as the full set of controls) reveal no differences in maternal depressive symptoms by month-of-assessment.

[20] As a check on these results, Tables 5 and 6 of the working paper version estimate unconditional child and family regressions – that is, without controls – and the results continue to show that the summer-month indicators are uncorrelated with the child and family characteristics (Herbst, 2012). In addition, Appendix Tables 5 and 6 of the working paper version reexamine the characteristics separately for families that complied with the birthday assignment rule (i.e., children assessed in the month of the 9- or 24-month birthday) and families that did not comply (i.e., those assessed in a month before or after the 9- or 24-month birthday) (Herbst, 2012). The findings once again suggest that having a summer assessment is unrelated to the observable characteristics of children and families.

[21] I find that low-income mothers (defined as those with family incomes below the sample median of $37,500) are more likely to be employed in the summer, while high-income mothers are less likely to be employed, although the coefficient on *SUMMER* is statistically insignificant in both models. The lower employment rate for high-income mothers is explained almost entirely by the move from part-time work to no work (i.e., a statistically significant 2.5 percentage point decrease in the likelihood of working part-time) during the summer. This result is important because it could account for some of the summer-time test score gains among high-income children.

[22] The observation measures include the Infant/Toddler Environment Rating Scale (ITERS), the Family Day Care Rating Scale (FDCRS), and the Arnett Scale of Caregiver Behavior. The ITERS and FDCRS are classroom-level assessments of global child care quality, based on structural (e.g., child–teacher ratio) and process (e.g., caregiver interaction) features of the environment. The Arnett Scale assesses the nature of interactions between caregiver–child pairs. These observations were conducted with a subsample of providers selected during the 24-month parent interview.

[23] There are cases in which gaps exist between the timing of the parental interviews (when child care participation data are collected) and the timing of the child care interviews and observations. Therefore, all models include a control for the temporal gap between the parent interview and the child care quality data collection.

Fig. 2. Child characteristics by month of ECLS-B assessment.

few differences in the quality measures between the summer and non-summer months. Of the 30 *SUMMER* coefficients presented, only five are significantly different from zero. Thus, the IV estimates of *NP* should not reflect seasonal differences in the level of child care quality to which focal children are exposed.

There is one final concern regarding the validity of *SUMMER*. Given that the assignment rule is based on the focal child's birthday, a mechanical relationship exists between the month-of-assessment and season-of-birth. Table 6 depicts this relationship by showing the fraction of children born in each quarter of 2001 by the month-of-assessment. Not surprisingly, children assessed in each subsequent summer month were born deeper into 2001. For example, children assessed in June for the 9-month survey were overwhelmingly born in the second and third quarters of 2001 (92%), while those assessed in September were largely born in the fourth quarter (72%). One concern is that unobserved family differences associated with season-of-birth might invalidate *SUMMER* as an instrument. Indeed, Buckles and Hungerman (2010) document strong seasonal patterns in the socio-economic characteristics of women giving birth throughout the calendar year. Children born in the first quarter are more likely to have teenage mothers, mothers who are unmarried, and mothers who dropped out of high school. Although much of the variation in birth characteristics is explained by these observable maternal characteristics, several difficult-to-measure factors (e.g., maternal preferences and anticipated conditions at conception/birth) also contribute to season-of-birth patterns.

I take a number of steps to ensure that seasonality in birth characteristics does not confound the IV estimates. The primary strategy is to control explicitly for season-of-birth in the baseline model. I do so by adding a binary indicator for births occurring in the fourth quarter (i.e., October, November, or December) of 2001 (QOB-4). Although it might be preferable to incorporate a full set of QOB indicators, doing so substantially reduces the first-stage power of *SUMMER* because of the strong (and mechanical) correlation between month-of-assessment and season-of-birth, as confirmed in Table 6.[24] For example, including indicators for QOB-1, QOB-2, and QOB-3 decreases the F-statistic on *SUMMER* by 50% in the first-stage current and cumulative child care equations. The loss of power is more problematic in the sub-group analyses. Nevertheless, I present results from a series of specification checks that account for the full set of QOB indicators (three dummy variables); separate December, January, and February indicators (three dummy variables); QOB-by-wave indicators (four dummy variables); and month-of-birth-by-wave indicators (12 dummy variables). The indicators for December, January, and February are chosen because Buckles and Hungerman (2010) show that seasonal differences in birth characteristics are pronounced for children born during the winter and non-winter months. The QOB and month-of-birth interactions with wave are intended to flexibly allow season-of-birth effects to vary with the child's age. Despite the severe loss of power from adding these controls, the main IV estimates are robust to their inclusion.

In results not reported here, I experiment with several alternative methods of accounting for seasonal patterns in birth characteristics. First, I attempt to control for environmental conditions at childbirth by replacing the contemporaneous state-of-residence fixed effects with state-of-residence-at-birth fixed effects. Second, previous work finds that seasonality in birth characteristics is greater in southern states (Lam and Miron, 1991). Therefore, I estimate the IV model with children residing in the South omitted. The IV estimates from these models are similar to the baseline estimates. Finally, I exploit the panel structure of the ECLS-B and estimate IV fixed effects (IV FE) models, which yield within-child estimates of the impact of non-parental child care utilization. Doing so effectively neutralizes

[24] Indeed, in the 9-month wave, the correlations between SUMMER and QOB-1, QOB-2, QOB-3, and QOB-4 are −0.42, −0.23, 0.28, and 0.42, respectively. The corresponding correlations in the 24-month wave are −0.42, 0.10, 0.61, and −0.25.

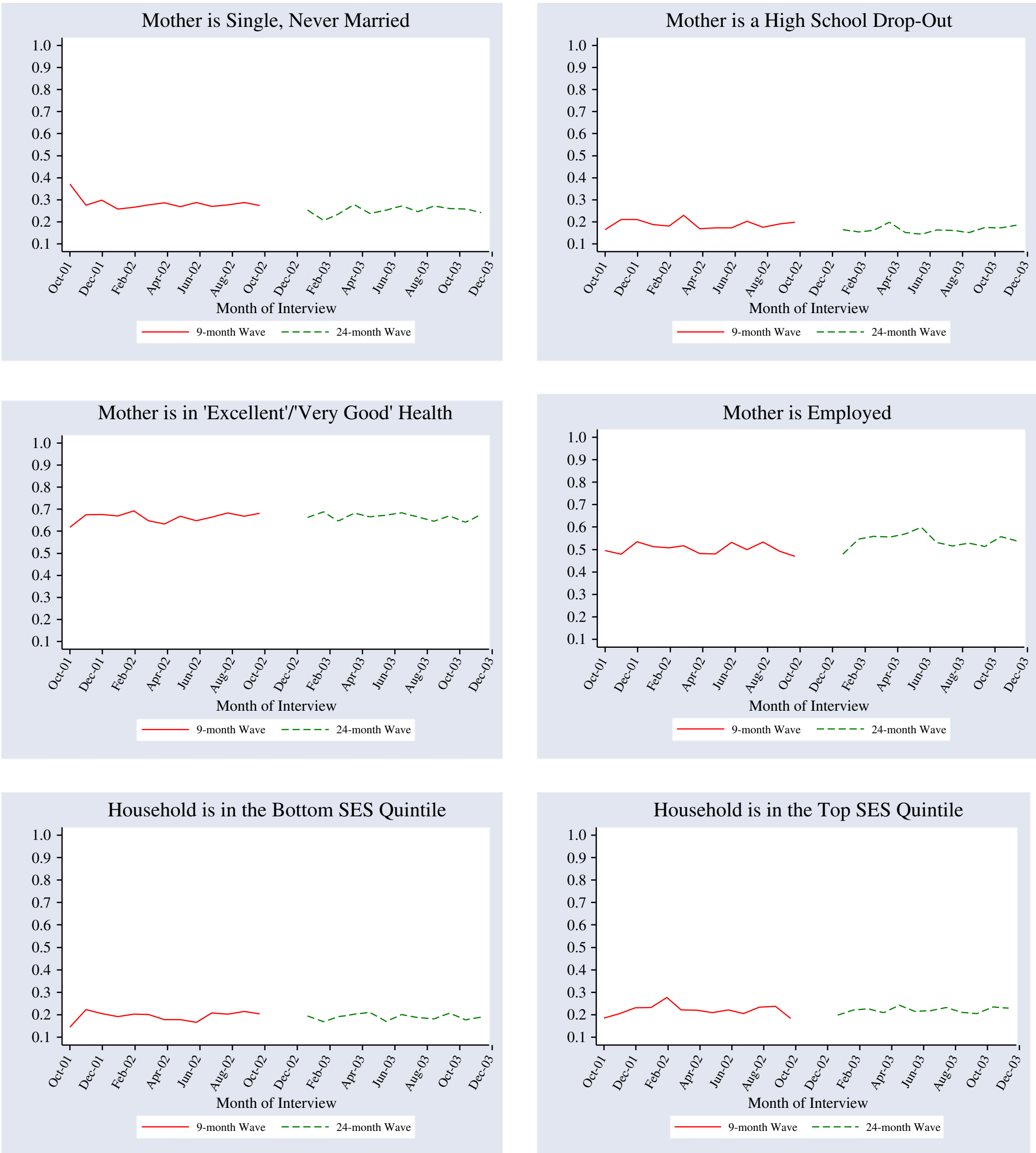

Fig. 3. Family characteristics by month of ECLS-B assessment.

concerns over the unobserved between-child differences in cognitive ability associated with season-of-birth. The baseline IV estimates are once again robust to this specification check.

5. Estimation results

5.1. OLS and fixed effects estimates for non-parental child care utilization

This section discusses results from the OLS and fixed effects models examining the relationship between non-parental child care utilization and cognitive ability test scores. As shown in Table 7, the OLS results are presented in columns (1) through (4), while the fixed effects results are presented in columns (5) and (6). Differences across the columns are related to the types of controls added to the production function. Columns (4) and (6) represent the richest OLS and fixed effects specifications, respectively. Each cell displays the coefficient and standard error on the measure of current (Panel A) or cumulative (Panel B) non-parental child care utilization.

Looking at the OLS results, the evidence consistently points to a positive association between child care utilization and children's cognitive ability test scores, a result that is consistent with much of the prior OLS literature. However, it appears that the estimate declines substantially as controls are added to the production function. For example, in a model that controls only for survey wave, current use of any non-parental arrangement is associated with an increase in the BSF-R score of 1.4% [Panel A, column (1)]. This corresponds to an effect size of 0.05 standard deviations (*SDs*). This effect is reduced to a 0.3% increase in the BSF-R (0.01 *SDs*) in the richest OLS specification [Panel A, column (4)]. Overall, the magnitude of the child care effect declines about five-fold moving from the sparsest to the fullest model. A consistent story emerges for the measure of cumulative child care use: the magnitude of the coefficient decreases about seven-fold as of the fullest model, suggesting that an additional month of child care exposure is associated with a 0.03% increase in the BSF-R.

The estimates in columns (5) and (6) account for child fixed effects, an empirical strategy used by only a small number of previous

Table 4
Child and family characteristics by month of ECLS-B assessment.

Month of assessment	(1)	(2)	(3)	(4)	(5)
Panel A: Child characteristics	Male	White	LBW	Premature	Weight
June	−0.003	−0.002	−0.002	0.007	−0.057
	(0.014)	(0.012)	(0.010)	(0.007)	(0.044)
July	0.003	−0.013	0.005	0.002	−0.075*
	(0.013)	(0.011)	(0.009)	(0.007)	(0.044)
August	0.004	0.000	0.010	0.011	−0.032
	(0.013)	(0.011)	(0.009)	(0.007)	(0.044)
September	−0.012	−0.002	0.011	−0.001	−0.012
	(0.014)	(0.012)	(0.010)	(0.007)	(0.046)
F-statistic (p-value)	0.28	0.36	0.67	0.74	1.05
	(0.892)	(0.838)	(0.610)	(0.567)	(0.382)
Panel B: Family characteristics	Never married	BA or higher	Excell/VG health	Bottom SES quintile	Top SES quintile
June	0.005	−0.013	−0.008	−0.010	0.002
	(0.009)	(0.009)	(0.012)	(0.006)	(0.008)
July	−0.002	0.009	0.007	0.000	−0.001
	(0.009)	(0.009)	(0.012)	(0.006)	(0.007)
August	−0.003	−0.007	0.007	0.007	0.007
	(0.009)	(0.009)	(0.012)	(0.006)	(0.006)
September	0.003	−0.006	−0.011	−0.003	0.003
	(0.010)	(0.009)	(0.012)	(0.006)	(0.007)
F-statistic (p-value)	0.16	1.09	0.58	1.15	0.38
	(0.961)	(0.359)	(0.680)	(0.331)	(0.822)

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. LBW is low birth weight. The outcomes in Panel B, columns (1) through (3) relate to the focal child's mother. The outcome in Panel B, column (3) is a binary indicator for whether a given mother self-reports being in "excellent" or "very good" health. See column (3) of Table 3 for a list of the controls included in the models in Panel B. All models include a binary indicator for wave. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

child care studies. Column (5) omits the time-varying controls, while column (6) adds them. In both cases, the estimated effect of current non-parental care continues to be positive, statistically significant, and of a magnitude similar to the OLS results. Interestingly, inclusion of the time-varying controls reduces the magnitude of the current child care effect by about half, to a 0.4% increase in the BSF-R. As shown in Panel B, adding these controls to the model of cumulative child care utilization renders the coefficient statistically insignificant. This pattern underscores the importance of the time-varying determinants of cognitive ability, and raises the concern that the fixed effects estimator could still be inconsistent if any such factors remain unobserved.

5.2. Instrumental variables estimates for non-parental child care utilization

Table 8 reports the reduced form and IV estimates of the impact of current (Panel A) and cumulative (Panel B) child care utilization. Columns (1) and (2) depict the first-stage estimates on *SUMMER*. Columns (3) and (4) report the reduced form results, in which BSF-R scores are regressed on *SUMMER*. Columns (5) and (6) report the IV estimates for *NP*. Each set of models is estimated with and without the control for QOB-4.

The first-stage estimates differ slightly from those presented in Table 3 because of the sample construction. The initial estimates come from the full sample of children, including those with missing test score data. The estimates in Table 8 are derived from the sub-set of children with non-missing test score data. Nevertheless, with F-statistics of 20.7 [Panel A, column (2)] and 17.6 [Panel B, column (2)], *SUMMER* remains highly correlated with *NP*. The bottom rows of Table 8 confirm the strength of *SUMMER* through a series of Cragg and Donald (1993) tests of weak instruments. In a just-identified

Table 5
Comparison of non-parental child care environments across the summer and non-summer months, 24-month wave.

			Coefficient on summer assessment	
Dependent variable	N	Mean	(1)	(2)
Panel A: Global child care quality				
ITERS for center-based settings	596	4.163	0.003	−0.095
(range: 1–7)			(0.086)	(0.088)
FDCRS for home-based settings	776	3.403	0.004	−0.055
(range: 1–7)			(0.079)	(0.072)
Arnett scale of caregiver behavior	1359	60.85	−0.086	−0.138
			(0.613)	(0.601)
Panel B: Characteristics of the child care center director				
B.A. degree or more (%)	568	0.528	−0.025	0.001
			(0.044)	(0.047)
Child development associate credential (%)	561	0.301	−0.031	−0.053
			(0.040)	(0.045)
Degree in early childhood education (%)	568	0.489	−0.010	−0.024
			(0.044)	(0.046)
Experience in child care/education (years)	570	14.79	−2.104***	−1.619*
			(0.708)	(0.843)
Panel C: characteristics of the caregiver				
Female (%)	3066	0.967	−0.001	0.002
			(0.007)	(0.007)
Born in the U.S. (%)	3058	0.810	0.004	−0.006
			(0.015)	(0.013)
Black (%)	3032	0.211	0.030*	−0.011
			(0.016)	(0.011)
High school degree or more (%)	3057	0.826	−0.010	−0.011
			(0.015)	(0.015)
Child Development Associate credential (%)	2517	0.167	0.045***	0.027*
			(0.016)	(0.016)
Experience in child care field (years)	3052	9.742	−0.261	−0.220
			(0.361)	(0.367)
Reported health is excellent/very good (%)	3058	0.712	0.004	0.006
			(0.018)	(0.018)
Smokes cigarettes (%)	3059	0.133	−0.004	−0.014
			(0.013)	(0.013)
Time difference: child/provider assessment			Y	Y
Child characteristics			N	Y
Family characteristics			N	Y
State FE			N	Y

Source: Author's analysis of the 24-month wave of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are robust to arbitrary forms of heteroskedasticity. See column (3) of Table 3 for a list of the controls included in column (2). ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

model, the Stock and Yogo (2004) critical value is 16.4 under the null hypothesis that the 2SLS bias exceeds that from OLS by 10%. In all cases, the test statistic exceeds the critical value, suggesting that *SUMMER* passes the weak instruments test.

The reduced form estimates are consistently positive, suggesting that children assessed in the summer perform better on the BSF-R. For example, the coefficient in Panel A, column (4), implies that children assessed during the summer score 0.2% higher on the BSF-R than children assessed during other times of the year. This estimate is interesting because it provides insight into the direct relationship between assignment to a summer assessment and early cognitive ability. The discussion of Appendix Tables 3 through 5 indicates that the mechanism for this reduced form effect is not likely to be through changes in maternal employment. Rather, it appears that mothers with flexible work–family schedules are less likely to use non-parental arrangements in the summer, and are more likely to make high-quality time investments in their children.

Given the negative coefficient on *SUMMER* in the first-stage equation and its positive coefficient in the reduced form equation, it is not surprising that the IV estimates on *NP* are negative. Looking first at Panel A, the coefficient in the baseline model [column (6)] indicates

Table 6
The relationship between assessment and birth timing in the ECLS-B.

	Quarter-of-birth (2001, %)			
Month-of-assessment	QOB-1	QOB-2	QOB-3	QOB-4
Panel A: 9-month survey (2002)				
June	0.048	0.214	0.708	0.029
	(0.213)	(0.411)	(0.455)	(0.169)
July	0.042	0.123	0.468	0.368
	(0.200)	(0.328)	(0.499)	(0.482)
August	0.026	0.062	0.244	0.669
	(0.158)	(0.241)	(0.430)	(0.471)
September	0.028	0.058	0.191	0.723
	(0.166)	(0.234)	(0.393)	(0.448)
Panel B: 24-month survey (2003)				
June	0.107	0.815	0.078	0.000
	(0.309)	(0.387)	(0.269)	(0.000)
July	0.024	0.313	0.663	0.000
	(0.154)	(0.464)	(0.473)	(0.000)
August	0.018	0.108	0.865	0.008
	(0.133)	(0.311)	(0.342)	(0.091)
September	0.016	0.058	0.689	0.237
	(0.127)	(0.234)	(0.463)	(0.425)

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard deviations are displayed in parentheses. Each cell depicts the fraction of children born in each calendar quarter (quarter-of-birth) by the summer month in which children are assessed (month-of-interview) during the 9- and 24-month surveys. Each row sums to one (100%).

that infants and toddlers attending non-parental arrangements score 8.4% lower on the BSF-R than children in parental care. This corresponds to an effect size of approximately 0.29 *SD*s. The IV coefficient on cumulative child care is similarly negative, implying that an additional month of child care exposure reduces cognitive ability test scores by 0.6%. It is useful to compare the effect size above with those generated by other early care and education programs. Bernal and Keane (2011) estimate that an additional year of non-parental care reduces test scores by 0.11 *SD*s. Studies of child care subsidies in the U.S. show reading test score reductions of 0.36 to 0.49 *SD*s and math test score reductions of 0.29 to 0.37 *SD*s (Herbst and Tekin, 2010a), while Baker et al.'s (2008) analysis of Quebec's subsidy program shows a reduction in social development of 0.17 *SD*s. A recent study of five states' pre-kindergarten programs shows one-year gains of 0.26 and 0.28 *SD*s for early reading and math ability, respectively (Barnett et al., 2005). Finally, one-year impact estimates for Chicago's Child-Parent Centers show test score gains of 0.20 to 0.65 *SD*s (Reynolds, 2000).

Although the IV estimates in Table 8 are not sensitive to the QOB-4 control, I provide a more rigorous check on the potential implications of seasonality in birth characteristics. In results not reported here, I estimate two versions of an IV FE model that replace the state fixed effects with individual fixed effects. I first use as the instrument the *SUMMER* variable from the previous IV regressions. With a point estimate of −0.057, it is reassuring that the current child care effect is similar to that from the baseline IV model. The key difference resides with the standard error, which becomes nearly four times larger in the IV FE regression. Because a non-trivial number of children are assessed in the summer of both the 9- and 24-month surveys, the fixed effects substantially reduce the amount of (within-child) variation in *SUMMER*, decreasing the efficiency of the IV FE estimate. One way to improve efficiency is to increase the number of identifying instruments, in this case by replacing *SUMMER* with a set of eight indicators for each summer month in which focal children were assessed. Doing so increases the amount of within-child variation in the IVs because those with summer assessments in both waves likely had the assessment completed in different months. Results from this model imply an 8.2% reduction in BSF-R scores – similar in magnitude to the baseline IV estimate – and the standard error decreases by more than half relative to the first IV FE specification ($t = 1.61$). Overall, these results suggest that unobserved seasonal patterns in birth characteristics are not likely to be problematic in this analysis.[25]

5.3. Robustness checks

Table 9 presents results from a battery of specification tests intended to check the robustness of the main IV estimates. The first three rows control for various dimensions of child health that may have a seasonal component and share a correlation with early cognitive ability. First, if there are underlying seasonal differences in child well-being, one might assume that they would be partially reflected in parents' self-reports of overall child health. Row (1) includes such a control in the model. Second, the controls in rows (2) and (3) account for a number of specific dimensions of child health that potentially exhibit seasonal patterns and that could influence cognitive ability. In particular, row (2) adds indicators for recent asthma and ear infection diagnoses, while row (3) controls for recent injuries. Inclusion of these additional child health controls does not alter the estimated effect of current or cumulative child care.

It is also possible that elements of maternal health exhibit seasonal patterns. Recall the evidence discussed earlier that maternal depressive symptoms do not vary across the summer and non-summer months of the 9-month survey. However, given that the CES-D is not available in the 24-month survey, it cannot be included in the model. Therefore, row (4) proxies for underlying maternal physical and mental health by adding a measure of self-reported overall health status. Inclusion of self-reported health does not alter the estimated child care effects.

The next five rows [rows (5) through (9)] experiment with alternative controls for the focal child's season-of-birth. To this point, potential seasonal patterns in child development have been handled by adding a QOB-4 indicator to the IV models. It is important to investigate whether the estimates are sensitive to richer sets of season-of-birth controls. Row (5) adds a full set of QOB indicators (three dummy variables); row (6) adds separate December, January, and February indicators (three dummy variables); row (7) includes QOB-by-wave indicators (four dummy variables); and row (8) includes month-of-birth-by-wave indicators (12 dummy variables). Row (9) takes a different approach to seasonality by restricting the analysis to children assessed between March and October of the 9- or 24-month survey. Doing so necessarily produces a sample of children born in a smaller number of more homogenous months. As shown in Table 9, the main IV results are robust to these controls and sample refinements.

Next, I examine the sensitivity of the IV estimates to the exclusion of clusters of control variables. This exercise constitutes a useful test of *SUMMER*'s validity as an instrument: if *SUMMER* does in fact generate exogenous variation in child care choices, then the point estimates on current and cumulative care should not be highly sensitive to the exclusion of certain observable controls. Such results would also bolster confidence that *SUMMER* is capable of purging the unobserved determinants of BSF-R scores. Rows (10) through (14) experiment with omitting various combinations of controls, including the full set of child characteristics and several key household determinants of child care choices. The point estimates on current and cumulative child care are consistently similar to the baseline IV estimates, although the standard errors are often substantially larger, suggesting that the control variables are important primarily for increasing

[25] In results not reported here, I test an alternative instrument based on the cumulative number of summer months to which children were exposed. In the first-stage equation for cumulative child care, the coefficient (and standard error) on the instrument is −0.141 (0.031). The second-stage estimate (and standard error) of the impact of cumulative child care is −0.006 (0.003), which is similar to that based on the single *SUMMER* instrument reported in the paper. I thank an anonymous referee for making this suggestion.

Table 7
OLS and fixed effects estimates of the impact of non-parental child care utilization.

	OLS	OLS	OLS	OLS	Child FE	Child FE
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Current child care utilization						
Non-parental child care	0.014***	0.003**	0.003**	0.003**	0.008***	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Panel B: Cumulative child care utilization						
Non-parental child care	0.002***	0.0003**	0.0003**	0.0003***	0.002***	0.0001
	(0.000)	(0.0001)	(0.0001)	(0.0001)	(0.000)	(0.0002)
Child characteristics	N	Y	Y	Y	N	Y
Family characteristics	N	Y	Y	Y	N	Y
Family characteristics × wave	N	Y	Y	Y	N	Y
State FE with wave interactions	N	N	Y	Y	N	N
Fourth quarter of birth (QOB-4)	N	N	N	Y	N	N

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. The outcome in all models is the natural log of the Bailey Short Form-Research Edition test score. See column (3) of Table 3 for a list of the controls included in the models. All models include a binary indicator for wave. N = 19,071 in Panel A. N = 17,546 in Panel B. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

efficiency. Together, these results provide additional support for the validity of *SUMMER*.

In robustness checks not reported here, I conduct another test of *SUMMER*'s validity. Recall that the reduced form results suggest that children assessed in the summer score higher on the BSF-R than children assessed during other times of the year. If this occurs because children are less likely to use non-parental care in the summer, then reduced form models estimated on those who *never* or *always* used child care should not yield seasonal differences in test scores. To conduct the analysis, I first identified the subset of children (at 9- and 24-months) who were either never in non-parental arrangements or participating for the maximum number of months. I then estimated separate reduced form equations (including various controls for QOB) by survey wave. In no case is the coefficient on *SUMMER* statistically significant, providing additional evidence that *SUMMER*'s effect on test scores operates through changes in non-parental care.

In the final set of rows [rows (15) and (16)], I experiment with alternative definitions of *SUMMER*. Row (15) classifies June as a non-summer month instead of a summer month, and row (16) conducts the analogous exercise with September. In three of the four models, the coefficient on *NP* remains negative and statistically significant, while the only insignificant coefficient – cumulative child care in row (15) – has a t-statistic of 1.62. It is interesting to note that reclassifying June as a non-summer month leads to smaller (negative) IV estimates relative to the baseline results. There is suggestive evidence that families interviewed in June are somewhat more economically advantaged than their peers interviewed during other months of the year. In a forthcoming section, I will present evidence that the negative child care effects are pronounced for these advantaged families. Therefore, it is possible that by reclassifying June as a non-summer month, the remaining months in *SUMMER* include comparatively *dis*advantaged families – whose children are not as adversely affected by non-parental arrangements – which might explain why the IV estimates in row (15) imply smaller negative child care effects.

To this point, identification of *NP* has come from a single instrument, *SUMMER*. However, there are ways to alter the instrument set, for example, by over-identifying the model. Increasing the number of instruments can allow one to examine several things. First, it is useful to determine whether the IV estimates are sensitive to changes in the instrument set. Second, with multiple instruments, one can test the exogeneity of the overidentifying restrictions. Finally, IV estimates from an alternative estimator, limited information maximum likelihood (LIML), can be compared to those from the 2SLS estimator. It has been shown that 2SLS estimates tend to be biased toward the OLS estimates as the number of instruments increases

Table 8
Instrumental variables estimates of the impact of non-parental child care utilization.

Variable	First stage (1)	First stage (2)	Reduced form (3)	Reduced form (4)	IV 2SLS (5)	IV 2SLS (6)
Panel A: Current child care utilization						
Non-parental child care/summer assessment	−0.030***	−0.029***	0.003**	0.002*	−0.092**	−0.084**
	(0.006)	(0.006)	(0.001)	(0.001)	(0.038)	(0.038)
Panel B: Cumulative child care utilization						
Non-parental child care/summer assessment	−0.445***	−0.420***	0.003**	0.003**	−0.007**	−0.006*
	(0.096)	(0.100)	(0.001)	(0.001)	(0.003)	(0.003)
Child characteristics	Y	Y	Y	Y	Y	Y
Family characteristics	Y	Y	Y	Y	Y	Y
Family characteristics × wave	Y	Y	Y	Y	Y	Y
State FE with wave interactions	Y	Y	Y	Y	Y	Y
Fourth quarter of birth (QOB-4)	N	Y	N	Y	N	Y
Weak instruments test: Panel A	n/a	n/a	n/a	n/a	24.13	22.27
Weak instruments test: Panel B	n/a	n/a	n/a	n/a	23.50	20.42

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. The outcome columns (1) and (2) is the measure of non-parental child care utilization (current or cumulative). The outcome in columns (3) through (6) is the log of the Bailey Short Form Research-Edition test score. See column (3) of Table 3 for a list of the controls included in the models. All models include a binary indicator for wave. N = 19,071 in Panel A. N = 17,546 in Panel B. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 9
Robustness checks.

Specification	Current utilization (1)	Cumulative utilization (2)
(1) Control for parent reports of child's health	−0.075**	−0.006
	(0.038)	(0.003)
(2) Control for child asthma and ear infections	−0.089**	−0.007**
	(0.039)	(0.004)
(3) Control for child injuries	−0.084**	−0.006*
	(0.038)	(0.003)
(4) Control for self-reported maternal health	−0.083**	−0.006*
	(0.038)	(0.003)
(5) Control for full set of QOB indicators	−0.076*	−0.007*
	(0.040)	(0.004)
(6) Control for Dec, Jan, and Feb indicators	−0.077**	−0.006*
	(0.039)	(0.003)
(7) Control for QOB-by-wave indicators	−0.101***	−0.008**
	(0.038)	(0.004)
(8) Control for MOB-by-wave indicators	−0.086**	−0.007*
	(0.038)	(0.004)
(9) Restrict to March-October assessments	−0.071**	−0.008*
	(0.035)	(0.004)
(10) Omit child controls	−0.096	−0.008
	(0.066)	(0.006)
(11) Omit employment/income controls	−0.095***	−0.008***
	(0.031)	(0.003)
(12) Omit child/family × wave controls	−0.088	−0.007
	(0.065)	(0.006)
(13) Omit state FE with wave interactions	−0.101**	−0.008**
	(0.052)	(0.004)
(14) Omit child/family × wave/state FE with wave interactions	−0.112	−0.009
	(0.073)	(0.007)
(15) Classify June as a non-summer month	−0.069*	−0.005
	(0.039)	(0.003)
(16) Classify Sept as a non-summer month	−0.112***	−0.013***
	(0.042)	(0.005)

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. The outcome in all models is the log of the Bailey Short Form Research-Edition test score. The model in row (7) includes interactions between the four quarter-of-birth indicators and the wave indicator (four controls). The model in row (8) includes interactions between the 12 month-of-birth indicators and the wave indicator (12 controls). See column (3) of Table 3 for a list of the controls included in the models. All models include a binary indicator for wave. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

(e.g., Bound et al., 1995; Hansen et al., 2008). LIML corrects for this many-instruments bias, thereby providing a way to assess the 2SLS estimates.

Appendix Table 6 examines these issues in the context of three alternative definitions of the instrument set. Columns (1A) and (1B) construct two instruments: one binary indicator for summer assessments at the 9-month survey and another for summer assessments at 24-months. Columns (2A) and (2B) contain four instruments: one binary indicator for each summer month across the 9- and 24-month waves. Finally, columns (3A) and (3B) examine eight instruments: one set of four summer-month indicators for the 9-month wave and another set of four indicators for the 24-month wave. Restricting the analyses to the measure of current child care utilization, several noteworthy findings emerge. First, increasing the number of instruments does not substantially change the 2SLS estimates. As shown in columns (1A), (2A), and (3A), the estimates of *NP* reside within a narrow range, and are similar to the baseline IV estimate. In addition, the instruments consistently pass the exogeneity test. With p-values of 0.59, 0.17, and 0.19, Wooldridge Score tests of the null hypothesis that the overidentifying instruments are valid are never rejected. Finally, although the 2SLS and LIML estimates tend to be similar, the latter are consistently larger, and the difference grows with the number of instruments. This suggests that the 2SLS estimates represent a lower-bound estimate of the (negative) impact of non-parental child care utilization.

Table 10
OLS, fixed effects, and instrumental variables estimates for alternative measures of non-parental child care utilization.

Variable	OLS full (1)	Child FE full (2)	IV 2SLS (3)
Panel A: Informal and formal child care measures			
Informal child care (vs. parental care)	0.003	0.006**	−0.070
	(0.002)	(0.003)	(0.053)
Formal child care (vs. parental care)	0.005***	0.002	−0.111***
	(0.002)	(0.003)	(0.043)
Panel B: Relative, non-relative, and center-based child care measures			
Relative child care (vs. parental care)	0.003*	0.008***	−0.080
	(0.002)	(0.003)	(0.056)
Non-relative child care (vs. parental care)	0.001	−0.002	−0.140***
	(0.002)	(0.003)	(0.049)
Center-based child care (vs. parental care)	0.007***	0.004	−0.118**
	(0.002)	(0.003)	(0.054)
Child characteristics	Y	Y	Y
Family characteristics	Y	Y	Y
Family characteristics × wave	Y	Y	Y
State FE with wave interactions	Y	N	Y
Fourth quarter of birth (QOB-4)	Y	N	Y

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. In columns (1) and (2), a single regression of test scores is estimated on separate binary indicators of informal and formal child care utilization, and a single regression is estimated on the binary indicators of relative, non-relative, and center-based care. In column (3), each cell represents a different regression of test scores on the corresponding child care arrangement. See column (3) of Table 3 for a list of the controls included in the models. All models include a binary indicator for wave. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

5.4. Informal versus formal non-parental arrangements

To this point, the analysis treats all non-parental arrangements with a single variable. This masks potentially important heterogeneity in the impact of different child care settings. For example, it is conceivable that formal environments (e.g., center-based care), in which teachers must meet state-specific education and training requirements, have more favorable effects on the cognitive development of infants and toddlers. On the other hand, the comparatively large group sizes and higher child–teacher ratios found in centers may weaken the ability of teachers to engage in stimulating interactions with children. Most previous research finds that children attending formal care perform better on tests of early cognitive ability than their counterparts in informal settings.

Table 10 investigates possible heterogeneity using the measure of current child care utilization.[26] Panel A explores differences across informal and formal providers, while Panel B compares relative, non-relative, and center-based care. Columns (1) and (2) estimate single OLS and fixed effects regressions, respectively, while column (3) estimates separate IV models by arrangement-type.[27] Consistent with previous

[26] Data exigencies in the ECLS-B preclude the construction of arrangement-specific measures of cumulative child care use.

[27] Specifically, in columns (1) and (2), I use the full analysis sample to estimate a single regression of test scores on separate binary indicators for each non-parental child care arrangement. In column (3), I estimate separate test score regressions on subsets of the full sample that combine children in a given non-parental arrangement with those in parent care. Given that the IV results in column (3) condition the sample on a potentially endogenous variable (child care utilization), a form of sample selection bias could be introduced. To investigate this possibility, I experiment with single IV models of test scores on separate indicators for informal and formal arrangements, using the expanded sets of instruments described in Appendix Table 6. Consistent with the IV estimates in Table 10, the results show larger negative effects on children in formal care. For example, using the instruments outlined in columns (1a) and (1b) of Appendix Table 6, the IV estimate on informal care is −0.032 and that on formal care is −0.091. The standard errors are too large, however, for the effects to be statistically significant. Although the pattern of results is quite similar across both strategies, it may still be prudent to view the IV estimates in Table 10 as suggestive.

OLS studies, children attending formal non-parental arrangements score statistically significantly higher on the BSF-R than their counterparts in parent care, while children in informal arrangements perform no differently. The finer categorizations in Panel B reveal that center-based care is the most beneficial setting for infants and toddlers, increasing BSF-R scores by 0.7%. Thus, it appears that the positive OLS effect of any non-parental care discussed earlier is driven almost exclusively by those in formal, center-based child care settings.

The story changes substantially once the fixed effects are introduced. As shown in Panel A, the estimate on formal care becomes smaller and imprecisely estimated, while that on informal care doubles and becomes statistically significant. This pattern is also evident in Panel B: the coefficient on center-based care decreases by about half and is rendered imprecisely estimated, while the coefficient on relative care more than doubles in magnitude. Such large changes deserve careful attention. One potential explanation is that families using formal care are positively selected – imparting an upward bias on the corresponding OLS estimate – and families using informal care are negatively selected—imparting a downward bias on the corresponding OLS estimate. Once the time-invariant unobservables are accounted for, the impact of formal care becomes smaller and that of informal care becomes larger.

The IV estimates magnify the pattern established by the fixed effects. The sign on informal and formal care flips from positive to negative, but the magnitude of the negative effect is substantially larger for the latter. The IV estimates imply that children currently attending formal care score a statistically significant 11% lower on the BSF-R than their peers in parent care, while those in informal care score a statistically insignificant 7% lower. The IV results in Panel B reveal that children utilizing non-relative and center-based services appear to be driving these negative effects. Indeed, such children are about equally worse off compared to their counterparts in parent care. Children in relative settings also perform worse than those in parent care, but the difference is smaller in magnitude and not precisely estimated. To my knowledge, no prior study has found that infants and toddlers attending non-relative (e.g., family-based) and center-based services perform *worse* on tests of cognitive ability than those in virtually every other environment. It is also noteworthy that they stand in contrast with Bernal and Keane (2011), who find that the negative impact of overall child care time is driven by time spent in informal arrangements.

5.5. Heterogeneous effects of non-parental child care utilization

The results presented so far assume that non-parental arrangements have homogeneous effects on early cognitive ability. To explore possible heterogeneity, I estimate the OLS, fixed effects, and IV models on a variety of sub-groups. Such analyses are potentially important, given the finding in previous work that economically advantaged children are more adversely affected by early maternal work (Anderson et al., 2003) and child care policy reforms (Herbst and Tekin, 2010a, 2010b, 2012). A related stream of research finds that economically disadvantaged children benefit substantially more from early exposure to non-parental care (Loeb et al., 2004).

Table 11 reports results from the sub-group analyses. Columns (1) and (2) show the OLS and fixed effects results for the measure of current child care. Columns (3) and (4) present the IV estimates for current and cumulative care. Results from the OLS and fixed effects regressions are consistent with previous studies showing that economically disadvantaged children benefit from exposure to non-parental arrangements, and that the benefits exceed those accruing to advantaged children. In particular, non-white children, children residing with unmarried mothers, and those in lower-income households score higher on the BSF-R if they participate in non-parental care. Furthermore, the magnitude of the positive child care effect is larger than that for the comparable set of economically advantaged children.

The IV estimates reveal a different story in two respects. First, in no case do I find that disadvantaged children attending non-parental care

Table 11
Heterogeneous effects of non-parental child care utilization.

Demographic sub-group	OLS full (1)	Child FE full (2)	IV 2SLS current (3)	IV 2SLS cumulative (4)
Focal child is male	0.004*	0.002	−0.105	−0.007
	(0.002)	(0.003)	(0.084)	(0.006)
Focal child is female	0.002	0.006*	−0.061	−0.007
	(0.002)	(0.004)	(0.057)	(0.005)
Focal child is white	0.003	0.003	−0.219*	−0.003
	(0.002)	(0.004)	(0.117)	(0.002)
Focal child is non-white	0.003*	0.004	0.015	−0.001
	(0.002)	(0.003)	(0.041)	(0.003)
Mother is married	0.004*	0.003	−0.140**	−0.010*
	(0.002)	(0.003)	(0.070)	(0.006)
Mother is unmarried	0.003	0.007*	−0.006	−0.003
	(0.003)	(0.004)	(0.073)	(0.005)
Mother is employed	0.001	0.005	−0.135***	−0.011*
	(0.002)	(0.004)	(0.041)	(0.006)
Mother is not employed	0.007***	0.004	−0.006	−0.002
	(0.002)	(0.005)	(0.069)	(0.003)
HH income is below median	0.006***	0.008**	−0.015	−0.003
	(0.002)	(0.004)	(0.055)	(0.004)
HH income is at/above median	0.001	0.004	−0.162***	−0.003
	(0.002)	(0.004)	(0.042)	(0.002)
Child characteristics	Y	Y	Y	Y
Family characteristics	Y	Y	Y	Y
Family characteristics × wave	Y	Y	Y	Y
State FE with wave interactions	Y	N	Y	Y
Fourth quarter of birth (QOB-4)	Y	N	Y	Y

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. The outcome in all models is the log of the Bailey Short Form Research-Edition score. See column (3) of Table 3 for a list of the controls included in the models. All models include a binary indicator for wave. Median household income in the analysis sample is $37,500. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

score higher on the BSF-R than those in parent care: the coefficients on current and cumulative care are consistently negatively signed, although their magnitudes are small and imprecisely estimated. Therefore, whereas the OLS and fixed effects models imply that non-parental arrangements have a positive association with child development, the IV results imply neutral effects. To explore further heterogeneity, I estimate separate models for informal and formal arrangements on the sub-sets of disadvantaged children. The coefficient on formal care is consistently negatively signed, although it is always small in magnitude and usually imprecisely estimated, thus confirming the neutral effects of non-parental care for disadvantaged children. Second, exposure to non-parental arrangements among advantaged children generates large and statistically significant negative effects on BSF-R scores. In particular, white children, children of married and working mothers, and those in higher-income households are adversely affected by non-parental care. In sum, the IV estimates indicate that disadvantaged children neither benefit from nor are harmed by non-parental arrangements, while advantaged children perform significantly worse than their peers in parent care.

6. Conclusion

Using a panel of infants and toddlers from the ECLS-B, this paper estimates the causal effect of current and cumulative non-parental child care utilization by leveraging plausibly exogenous seasonal variation in child care participation. The OLS and fixed effects models find that children attending non-parental care score higher on the BSF-R. However, the IV results suggest that ability test scores are 0.29 *SDs* lower for children currently attending non-parental arrangements, and that an additional month in non-parental settings reduces ability test scores by 0.6%. The negative effects are pronounced for children participating in formal care and those from economically advantaged backgrounds. Contrary to previous work, I find that disadvantaged children do not benefit from non-parental care.

These results are important from a public policy perspective. In response to the growing reliance on non-parental caregivers, policies to support families and providers have grown in scope, magnitude, and delivery mechanism. Indeed, contemporary early care and education policy is administered through a complex web of direct price subsidies to support parental employment (Child Care and Development Fund, CCDF), tax credits to offset child care expenses (Child and Dependent Care Tax Credit), and provider reimbursements for meals served in family- and center-based environments (Child and Adult Care Food Program). In addition, federal and state governments fund an array of education-based services through Head Start and pre-kindergarten.

The potential impact of these policies and programs on child development has been the subject of growing scholarly interest. Herbst and Tekin (2010a, 2010b, 2012) find that CCDF child care subsidies have negative effects on preschool-aged children, lowering cognitive ability test scores and increasing a variety of behavior problems. On the other hand, studies of Head Start (e.g., Ludwig and Miller, 2007; U.S. Department of Health and Human Services, 2005, 2010) and pre-kindergarten (e.g., Gormley and Gayer, 2005; Hustedt et al., 2007, 2008; Wong et al., 2008) produce more favorable cognitive and social-emotional outcomes.

Drawing definitive conclusions from this policy research is complicated because many programs rely on the existing child care market to deliver its services. As such, children are exposed to extremely diverse child care environments with variable health and safety regulations, teacher quality, and resource availability. Therefore, it is crucial to produce credible evidence on the first-order question of the impact of these diverse non-parental arrangements on child development. The primary advantage of such evidence is that it will increase policymakers' understanding of the mechanisms through which early care and education policies operate. In particular, the research may help to explain why some policies are found to have seemingly counterintuitive effects on children.

Results from previous studies of the CCDF are illustrative of this point. Given that child care subsidies are largely used to purchase center-based services, and, as discussed in the literature review, most prior studies find that center-based care is particularly beneficial for low-income children, many assume that child care subsidies should have positive developmental effects. However, this assumption is not borne out by existing research on the CCDF. Two results in this paper may lend some clarity to the negative subsidy results. First, I find that children using formal, center-based care – where most subsidized children are placed – experience the largest reductions in cognitive ability test scores. Second, the group targeted by the CCDF – low-income children – does not perform better when they participate in non-parental care.

Another policy implication focuses on whether early care and education policies should be structured as universal or targeted interventions. Indeed, this debate was reignited recently by President Obama's ambitious plan to extend high-quality, full-day pre-kindergarten programs to all low- and moderate-income families. The well-known finding that early education interventions – including high-quality center-based care, state administered pre-kindergarten, and intensive boutique services (e.g., Perry Preschool and the Infant Health and Development Program) – produce larger positive impacts for disadvantaged children has led some to advocate that the Obama plan should be targeted even more narrowly. The finding in this study that the negative effect of non-parental care applies disproportionately to higher-income children seems consistent with a policy design that focuses its services on the most disadvantaged children.

Appendix A

Appendix Table 1
ECLS-B assessment schedule.

Month and year of assessment	Number of assessments
Wave 1: 9-month survey	
Oct-01	97
Nov-01	934
Dec-01	685
Jan-02	954
Feb-02	911
Mar-02	551
Apr-02	900
May-02	826
Jun-02	816
July-02	865
Aug-02	935
Sep-02	813
Oct-02	538
Nov-02	289
Dec-02	66
Wave 2: 24-month survey	
Jan-03	237
Feb-03	994
Mar-03	1047
Apr-03	925
May-03	837
Jun-03	740
Jul-03	822
Aug-03	831
Sep-03	795
Oct-03	822
Nov-03	704
Dec-03	466
Apr-04	16

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.

Appendix Table 2
The relationship between ECLS-B assessment timing and non-parental child care utilization by demographic sub-group.

Demographic sub-group	Current utilization (1)	Cumulative utilization (2)
Focal child is male	−0.026***	−0.432***
	(0.009)	(0.135)
Focal child is female	−0.036***	−0.491***
	(0.009)	(0.136)
Focal child is white	−0.028***	−0.305**
	(0.010)	(0.148)
Focal child is non-white	−0.035***	−0.619***
	(0.008)	(0.125)
Mother is married	−0.030***	−0.403***
	(0.008)	(0.118)
Mother is unmarried	−0.032***	−0.623***
	(0.011)	(0.165)
Mother has a high school degree or less	−0.027***	−0.679***
	(0.009)	(0.139)
Mother has more than a high school degree	−0.035***	−0.294**
	(0.009)	(0.131)
Mother is employed	−0.030***	−0.400***
	(0.009)	(0.132)
Mother is not employed	−0.033***	−0.307***
	(0.009)	(0.116)
Household SES is in bottom two quintiles	−0.027***	−0.547***
	(0.010)	(0.153)
Household SES is in top three quintiles	−0.035***	−0.391***
	(0.008)	(0.122)
Household has one/two adults ages 18+	−0.028***	−0.416***
	(0.007)	(0.104)
Household has three adults ages 18+	−0.048**	−0.211
	(0.021)	(0.328)
Household has four or more adults ages 18+	−0.056**	−1.176***
	(0.023)	(0.352)
Total household size: 2–4 members	−0.023***	−0.329***
	(0.008)	(0.121)
Total household size: 5–7 members	−0.035***	−0.536***
	(0.010)	(0.162)
Total household size: 8+ members	−0.080**	−1.315**
	(0.037)	(0.559)

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Reported here is the coefficient and standard error (in parentheses) on the *SUMMER* instrument. The dependent in (1) is a binary indicator for current participation in any non-parental child care arrangement, while that in (2) is a continuous measure of the cumulative number of months in non-parental care. See column (3) of Table 3 for a list of the controls included in the models. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Appendix Table 3
Summer maternal work activity.

	Work vs. no work	Part- vs. full-time work	Full-Time vs. no work	Part-Time vs. no work	Looking vs. no work
Variable	(1)	(2)	(3)	(4)	(5)
Summer assessment	0.000	−0.015	0.001	−0.008	−0.011
	(0.008)	(0.010)	(0.009)	(0.009)	(0.008)
Child characteristics	Y	Y	Y	Y	Y
Family characteristics	Y	Y	Y	Y	Y
Family characteristics × wave	Y	Y	Y	Y	Y
State FE with wave interactions	Y	Y	Y	Y	Y
Number of observations	19,312	10,098	14,320	11,278	9214

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. The summer assessment variable is a binary indicator for whether a given family was interviewed and the child was assessed in the summer (i.e., June, July, August, or September) during the 9- or 24-month wave. The dependent variable in column (1) equals unity for employed mothers and zero for mothers not in the labor force. The dependent variable in column (2) equals unity for full-time working mothers (employed 35+ hours per week) and zero for part-time working mothers. The dependent variables in columns (3) and (4) equal unity for full-time and part-time working mothers, respectively, and zero for mothers not in the labor force. The dependent variable in column (5) equals unity for mothers looking for work and zero for mothers not in the labor force. See column (3) of Table 3 for a list of the controls included in the models (the models presented here exclude the maternal employment and occupation controls). All models include a binary indicator for wave. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Appendix Table 4
Summer non-parental child care utilization by maternal employment status and place of employment.

Variable	Regression results (1)	Child Care utilization rate (2)	Implied change in utilization (3)
Panel A: By employment status			
Summer assessment			
× Works full-time	−0.023**	0.838	−2.7%
	(0.010)		
× Works part-time	−0.023	0.659	−3.5%
	(0.017)		
× Looking for work	−0.061**	0.339	−18.0%
	(0.025)		
× Not in labor force	−0.027***	0.162	−16.7%
	(0.009)		
Panel B: By place of employment			
Summer assessment			
× Works outside home	−0.010	0.801	−1.2%
	(0.010)		
× Works from home	−0.062***	0.725	−8.5%
	(0.022)		
× Self-employed	−0.021	0.484	−4.2%
	(0.056)		
× Unemployed	−0.035***	0.192	−18.1%
	(0.009)		
Child characteristics	Y		
Family characteristics	Y		
Family characteristics × wave	Y		
State FE with wave interactions	Y		
Number of observations	19,312/19,410		

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. The summer interview variable is a binary indicator for whether a given family was interviewed and the child was assessed in the summer (i.e., June, July, August, or September) during the 9- or 24-month wave. The dependent variable in all models is a binary indicator for participation in any non-parental child care arrangement. See column (3) of Table 3 for a list of the controls included in the models (the models presented here exclude the occupation controls). The model in Panel B replaces the controls for maternal employment status with separate dummy variables indicating the place of employment. All models include a binary indicator for wave. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Appendix Table 5
Summer activities with the focal child by maternal employment status and place of employment.

Variable	Read stories sing songs (1)	Run errands (2)	Walk/play yard/park (3)	Visit zoo (4)	Visit museum (5)	Breakfast together (6)	Dinner together (7)
Panel A: By employment status							
Summer assessment							
× Works full-time	0.017*	0.043***	0.184***	0.114***	0.036***	0.027***	0.013*
	(0.010)	(0.013)	(0.013)	(0.017)	(0.013)	(0.009)	(0.007)
× Works part-time	0.022	0.039**	0.228***	0.170***	0.020	0.014	0.024**
	(0.014)	(0.017)	(0.017)	(0.024)	(0.019)	(0.012)	(0.010)
× Looking for work	−0.022	0.060**	0.152***	0.100***	0.026	0.016	0.032**
	(0.018)	(0.026)	(0.027)	(0.038)	(0.027)	(0.018)	(0.013)
× Not in labor force	−0.003	0.030**	0.195***	0.109***	0.056***	0.012	0.001
	(0.009)	(0.012)	(0.012)	(0.016)	(0.012)	(0.008)	(0.006)
Panel B: By place of employment							
Summer assessment							
× Works outside home	0.011	0.040***	0.190***	0.118***	0.033***	0.024***	0.012*
	(0.009)	(0.012)	(0.012)	(0.016)	(0.011)	(0.008)	(0.007)
× Works from home	0.047**	0.047**	0.235***	0.187***	0.031	0.025	0.032***
	(0.020)	(0.023)	(0.023)	(0.033)	(0.028)	(0.016)	(0.012)
× Self-employed	−0.038	0.017	0.186***	0.079	−0.012	−0.056	0.034
	(0.044)	(0.055)	(0.056)	(0.073)	(0.052)	(0.038)	(0.029)
× Unemployed	−0.004	0.036***	0.189***	0.106***	0.050***	0.013*	0.007
	(0.008)	(0.011)	(0.011)	(0.015)	(0.011)	(0.007)	(0.005)
Child characteristics	Y	Y	Y	Y	Y	Y	Y
Family characteristics	Y	Y	Y	Y	Y	Y	Y
Family characteristics × wave	Y	Y	Y	N	N	Y	Y
State FE with wave interactions	Y	Y	Y	Y	Y	Y	Y
Outcome availability	9-/24-months	9-/24-months	9-/24-months	24-months	24-months	9-/24-months	9-/24-months

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. The summer interview variable is a binary indicator for whether a given family was interviewed and the child was assessed in the summer (i.e., June, July, August, or September) during the 9- or 24-month wave. All outcomes are defined as binary indicators of whether the respondent engaged in a given activity with the focal child. The outcomes in columns (1) through (3) inquire about activities engaged in *everyday*, columns (4) and (5) inquire about activities engaged in within the previous *month*; and columns (6) and (7) inquire about activities engaged in *5 + days per week*. See column (3) of Table 3 for a list of the controls included in the models (the models presented here exclude the occupation controls). The model in Panel B replaces the controls for maternal employment status with separate dummy variables indicating the place of employment. The models in columns (4) and (5) include only the state fixed effects (not the wave interactions). ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Appendix Table 6
Alternative instrument sets and IV estimator.

Variable	2SLS (1A)	LIML (1B)	2SLS (2A)	LIML (2B)	2SLS (3A)	LIML (3B)
Current child care utilization	−0.078**	−0.086**	−0.080**	−0.107**	−0.075**	−0.115**
	(0.037)	(0.041)	(0.038)	(0.051)	(0.037)	(0.056)
Instrument set (number)	Dummies for summer assessment at 9- and 24-months (2)		Dummies for June, July, Aug, Sept assessment (4)		Dummies for June, July, Aug, Sept assessment at 9- and 24-months (8)	
Test of overidentifying restrictions: p-value	0.585		0.170		0.188	
Child characteristics	Y	Y	Y	Y	Y	Y
Family characteristics	Y	Y	Y	Y	Y	Y
Family characteristics × wave	Y	Y	Y	Y	Y	Y
State FE with wave interactions	Y	Y	Y	Y	Y	Y
Fourth quarter of birth (QOB-4)	Y	Y	Y	Y	Y	Y

Source: Author's analysis of the 9- and 24-month waves of the ECLS-B.
Notes: Standard errors are displayed in parentheses and are adjusted for within-child clustering. The outcome in all models the log of the Bailey Short Form Research-Edition score. The instruments in columns (1A) and (1B) are separate binary indicators for a summer assessment in the 9- and 12-month waves. The instruments in columns (2A) and (2B) are separate binary indicators for each summer assessment month (i.e., June through September) irrespective of wave. The instruments in columns (3A) and (3B) are separate binary indicators for each summer assessment month in both the 9- and 12-month waves. The test of overidentifying restrictions is the Wooldridge Score test. See column (3) of Table 3 for a list of the controls included in the models. All models include a binary indicator for wave. N = 19,071. ***, **, * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

References

Anderson, P., Butcher, K., Levine, 2003. Maternal Employment and Overweight Children. Journal of Health Economics 22, 477–504.
Almond, D., Currie, J., 2010. Human capital development before age five. NBER Working Paper No. 15827.National Bureau of Economic Research, Cambridge, MA.
Baker, M., Gruber, J., Milligan, K., 2008. Universal child care, maternal labor supply, and family well-being. Journal of Political Economy 116, 709–745.
Barnett, W.S., Lamy, C., Jung, K., 2005. The effects of state prekindergarten programs on young children's school readiness in five states. Working Paper. Rutgers University: The National Institute for Early Education Research.
Becker, G., 1965. A theory of the allocation of time. The Economic Journal 40, 493–508.
Belsky, J., Vandell, D.L., Burchinal, M., Clarke-Stewart, K.A., McCartney, K., Owen, M.T., et al., 2007. Are there long-term effects of early child care? Child Development 78, 681–701.
Bernal, R., Keane, M., 2011. Child care choices and children's cognitive achievement: the case of single mothers. Journal of Labor Economics 29, 459–512.
Blau, D., 1999. The effects of child care characteristics on child development. Journal of Human Resources 34, 786–822.
Bound, J., Jaeger, D., Baker, R., 1995. Problems with instrumental variables estimation when the correlation between the instruments and the endogenous explanatory variable is weak. Journal of the American Statistical Association 90, 443–450.
Bradley, R., Vandell, D.L., 2007. Child care and the well-being of children. Archives of Pediatrics & Adolescent Medicine 161, 669–676.
Brooks-Gunn, J., Han, W.-J., Waldfogel, J., 2002. Maternal employment and child cognitive outcomes in the first three years of life: the NICHD study of early child care. Child Development 73, 1052–1072.
Buckles, K., Hungerman, D., 2010. Season of birth and later outcomes: old questions, new answers. NBER Working Paper No. 14573. National Bureau of Economic Research, Cambridge, MA.
Capizzano, J., Adelman, S., Stagner, M., 2002. What happens when the school year is over? The use and costs of child care for school-age children during the summer months. Occasional Paper Number 58. The Urban Institute, Washington, DC.
Case, A., Lubotsky, D., Paxson, C., 2002. Economic status and health in childhood: the origins of the gradient. American Economic Review 92, 1308–1334.
Case, A., Fertig, A., Paxson, C., 2005. The lasting impact of childhood health and circumstance. Journal of Health Economics 24, 365–389.
Council of Chief State School Officers, 2002. Key state education policies on PK-12 Education: 2002. Council of Chief State School Officers, Washington, DC.
Cragg, J.G., Donald, S., 1993. Testing identifiability and specification in instrumental variables models. Econometric Theory 9, 222–240.
Currie, J., Hotz, V.J., 2004. Accidents will happen? Unintentional childhood injuries and the effects of child care regulations. Journal of Health Economics 23, 25–59.
Gordon, R., Kaestner, R., Korenman, S., 2007. The effects of maternal employment on child injuries and infectious disease. Demography 44, 307–333.
Gormley, W., Gayer, T., 2005. Promoting school readiness in Oklahoma: an evaluation of Tulsa's pre-k program. Journal of Human Resources 40, 533–558.
Hansen, C., Hausman, J., Newey, W., 2008. Estimation with many instrumental variables. Journal of Business and Economic Statistics 26, 398–422.
Heckman, J., Masterov, D., 2007. The productivity argument for investing in young children. NBER Working Paper No. 13016. National Bureau of Economic Research, Cambridge, MA.
Herbst, C.M., 2012. The Impact of non-parental child care on child development: evidence from the summer participation “dip.” Discussion Paper No. 7039. Institute for the Study of Labor (IZA), Bonn, Germany.
Herbst, C.M., Tekin, E., 2010a. The impact of child care subsidies on child well-being: evidence from geographic variation in the distance to social service agencies. NBER Working Paper No. 16250.
Herbst, C., Tekin, E., 2010b. Child care subsidies and child development. Economics of Education Review 29, 618–638.
Herbst, C.M., Tekin, E., 2012. The geographic accessibility of child care subsidies and evidence on the impact of subsidy receipt on childhood obesity. Journal of Urban Economics 1, 37–52.
Hill, J., Waldfogel, J., Brooks-Gunn, J., 2002. Differential effects of high quality child care. Journal of Policy Analysis and Management 21, 601–627.
Hustedt, J.T., Barnett, W.S., Jung, K., Thomas, J., 2007. The Effects of the Arkansas Better Chance Program on Young Children's School Readiness. Rutgers, The State University of New Jersey, National Institute for Early Education Research, New Brunswick.
Hustedt, J.T., Barnett, W.S., Jung, K., Figueras, A., 2008. Impacts of New Mexico PreK on Children's School Readiness at Kindergarten Entry: Results from the Second Year of a Growing Initiative. Rutgers, The State University of New Jersey, National Institute for Early Education Research, New Brunswick.
Karoly, L., Kilburn, R., Cannon, J., 2005. Early Childhood Interventions: Proven Results, Future Promise. RAND Corporation, Santa Monica, CA.
Lam, D., Miron, J.A., 1991. Seasonality of births in human populations. Social Biology 38, 51–78.
Laughlin, L., 2010. Who's minding the kids? Child care Arrangements: Spring 2005/Summer 2006. Current Population Reports P70-121.U.S. Census Bureau, Washington, DC.
Leibowitz, A., 1974. Home investments in children. Journal of Political Economy 82 (2), S111–S131.
Loeb, S., Fuller, B., Kagan, S., Carrol, B., 2004. Child care in poor communities: early learning effects of type, quality and stability. Child Development 75, 47–65.
Ludwig, J., Miller, D., 2007. Does head start improve children's life chances? Evidence from a regression discontinuity design. Quarterly Journal of Economics 122, 159–208.
Marshall, P., O'Hara, C., Steinberg, P., 2000. Effects of seasonal allergic rhinitis on selected cognitive abilities. Annals of Allergy, Asthma & Immunology 84, 403–410.
Merikanto, I., Lahti, Tuuli, Castaneda, Anu E., Tuulio-Henriksson, Annamari, Aalto-Setälä, Terhi, Suvisaari, Jaana, Partonen, Timo, 2012. Influence of seasonal variation in mood and behavior on cognitive test performance among young adults. Nordic Journal of Psychiatry 66, 303–310.
Morrill, M., 2011. The effects of maternal employment on the health of school-age children. Journal of Health Economics 30, 240–257.
NICHD ECCRN, 1997. Child care in the first year of life. Merrill-Palmer Quarterly 43, 340–360.
NICHD ECCRN, 2000. The relation of child care to cognitive and language development. Child Development 71, 960–980.
NICHD ECCRN, 2004. Type of child care and children's development at 54-months. Early Childhood Research Quarterly 19, 203–230.
NICHD ECCRN, Duncan, G., 2003. Modeling the impacts of child care quality on children's preschool cognitive development. Child Development 74, 1454–1475.
Peisner-Feinberg, E., Burchinal, M., Clifford, R., Culkin, M., Howes, C., Kagan, S., Yazejian, N., 2001. The relation of preschool child-care quality to children's cognitive and social development trajectories through second grade. Child Development 72, 1534–1553.
Pianta, R., Steven Barnett, W., Burchinal, Margaret, Thornburg, Kathy R., 2009. The effects of preschool education: what we know, how public policy is or is not aligned with the evidence base, and what we need to know. Psychological Science in the Public Interest 10, 49–88.
Reynolds, A.J., 2000. Success in Early Intervention: The Chicago Child-Parent Centers. University of Nebraska Press, Lincoln.
Ruhm, C., 2004. Parental employment and child cognitive development. Journal of Human 39, 155–192.
Stock, J., Yogo, M., 2004. Testing for weak instruments in linear IV regression. Working paper. Department of Economics, Harvard University.
Todd, P., Wolpin, K., 2003. On the specification and estimation of the production function for cognitive achievement. The Economic Journal 113, F3–F33.

Todd, P., Wolpin, K., 2007. The production of cognitive achievement in children: Home, school, and racial test score gaps. Journal of Human Capital 1, 91–136.
U.S. Department of Health and Human Services, Administration for Children and Families, 2005. Head Start Impact Study: First Year Findings. U.S. Department of Health and Human Services, Washington, DC.
U.S. Department of Health and Human Services, Administration for Children and Families, 2010. Head Start Impact Study: Final Report. U.S. Department of Health and Human Services, Washington, DC.
Wong, V.C., Cook, T.D., Barnett, W.S., Jung, K., 2008. An effectiveness-based evaluation of five state pre-kindergarten programs. Journal of Policy Analysis and Management 27, 122–154.