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Child Care Choices and Children's Cognitive Achievement: The Case of Single Mothers

Raquel Bernal
Faculty Fellow, Institute for Policy Research
Assistant Professor of Economics
Northwestern University

Michael P. Keane
Professor of Economics
Yale University

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2040 Sheridan Rd. ◆ Evanston, IL 60208-4100 ◆ Tel: 847-491-3395 Fax: 847-491-9916
www.northwestern.edu/ipr, ◆ ipr@northwestern.edu

Abstract

The authors evaluate the effects of home inputs on children's cognitive development using the sample of single mothers from the National Longitudinal Survey of Youth (NLSY). Important selection problems arise when trying to assess the impact of maternal time and income on children's development. To deal with this, they exploit the (plausibly) exogenous variation in employment and child care use by single mothers generated by differences in welfare regulations across states and over time. In particular, the 1996 welfare reform act along with earlier state policy changes adopted under federal waivers, generated substantial increases in work and child care use. Thus, the authors construct a comprehensive set of welfare policy variables at individual and state levels and use them as instruments to estimate child cognitive ability production functions. They use local demand conditions as instruments as well.

The results indicate that the effect of child care use is negative, significant, and rather sizeable. In particular, an additional year of child care use is associated with a reduction of 2.8 percent (.15 standard deviations) in child test scores. But this general finding masks important differences across types of child care, children's ages, and maternal education. Indeed, only informal care used after the first year leads to significant reductions in child achievement. Formal care (i.e., center-based care and preschool) does not have any adverse effect on cognitive outcomes. In fact, these estimates imply that formal care has large positive effects on cognitive outcomes for children of poorly educated single mothers. Finally, the authors also provide evidence of a strong link between children's test scores at ages 4, 5, and 6 and their completed education.

1. Introduction

The effect of parental time inputs and child care use (as well as child care quality) on children's development has been widely analyzed, especially in the psychology and sociology literatures. Economists have also recognized the importance of this question. For instance, consider the determinants of individuals' labor market performance, in particular, wages. A vast body of research in the human capital literature has concluded that even after controlling for measures of human capital investment like education and work experience, most of the variation of wages across individuals remains unexplained. In other words, wages are largely determined by unobserved (to the researcher) individual characteristics. Furthermore, some recent studies have concluded that the unobserved characteristics that determine wages and other career outcomes are already largely determined by age 16.[2] These unobserved characteristics are often called the individual's "cognitive ability" or "skill endowment." But their determinants remain largely a black box. In this paper, we take a small step towards learning more about development of cognitive ability at young ages.

Extensive research has shown that children's early achievement is a strong predictor of a variety of later life outcomes: high achievers are more likely to have high educational attainment and high earnings; and less likely to have out-of-wedlock births, be on welfare or participate in crime. For this reason, the issue of what determines ability of individuals at early stages of life is critical for the design of public policy aimed at improving labor market outcomes. However, the results from previous literature on determinants of children's cognitive achievement are inconclusive at best.

A major challenge to estimating determinants of achievement is that the available data are often deficient. For example, inherited ability cannot be perfectly measured, creating difficult problems of endogeneity and self-selection. In fact, the main reason for the diversity of results in previous literature may well be the common limitation of failing to control for potential biases that may arise as a result of one or both of the following: (1) Women that work/use child care may be systematically different from women who do not, both in the constraints they face and their tastes; (2) The child's cognitive ability, which is at least partially unobserved by the econometrician, may itself influence the mother's decisions. In general, mothers' decisions whether to work and whether to use child care will depend on unobserved characteristics of both mothers and children.

To make the endogeneity problem clear, we lay out an example in each of two cases. In case (1), a woman with higher skills is more likely to have a child with high cognitive ability and also more likely to work/use child care. Then, a statistical analysis would spuriously attribute the effect

[2] See, e.g., Keane and Wolpin (1997, 2001) and Cameron and Heckman (1998).

of the woman's higher skills to employment (child care use), and the estimated effect of maternal employment (child care) on child cognitive outcomes would be upwardly biased. In case (2), mothers of low ability endowment children may choose to compensate them by spending more time with them, in which case mothers are more likely to work (use child care) if they have high ability children. Again, the estimated effect of maternal employment (child care) on child cognitive outcomes would be upwardly biased. Clearly, these sorts of sample selection issues make evaluation of the effects of women's decisions on child outcomes very difficult.

In this paper, we estimate child cognitive ability production functions for single mothers in the NLSY. We focus on single mothers because recent important changes in welfare rules, generated by welfare waivers and the Temporary Aid to Needy Families (TANF) program, along with increased day care subsidies provided by the Child Care Development Fund (CCDF), have led to dramatic and plausibly exogenous variation in work incentives, child care prices and child care availability for this group. These policy changes have greatly increased employment and childcare use among single mothers, especially those with children aged 0-5. Indeed, the percent of single mothers with young children who work increased from 59% in 1992 to 78% in 2001.

From 1993 to 1996, 43 States were granted federal waivers letting them adopt innovative approaches to welfare reform. Many policies adopted under State waivers were later incorporated in the "Personal Responsibility and Work Opportunity Reconciliation Act" (PRWORA) of 1996. PRWORA changed the welfare system into one requiring work in exchange for time-limited assistance. It created the TANF program, which replaced Aid to Families with Dependent Children (AFDC), and created the CCDF. Under TANF and the CCDF, States operate their own programs, so a great deal of State heterogeneity in work incentives/day care subsidies has emerged.

The main changes in the welfare system under waivers and TANF that are relevant for our exercise can be grouped into four categories: termination and work requirement time limits, earnings disregards, child care assistance and child support enforcement. States differ greatly in the rules they have adopted in each of these dimensions. Thus, we construct an extensive set of State and individual-specific welfare rules variables, and use these as instrumental variables in the estimation of the cognitive ability production function. We then get leverage for identification by (i) comparing outcomes for children born before 1990 vs. those born later, as welfare waivers and TANF only impacted mothers in the later period, and (ii) comparing outcomes across States that adopted different rules in the post-reform period. We also exploit a set of local demand condition instruments that have good explanatory power for behavior of single mothers over the whole sample period.

The study of single mothers extends earlier work by Bernal (2005), who estimated the effects of parental time inputs on children of married women in the NLSY. A key motivation of this work is to see whether her results on the importance of maternal time inputs generalize from married to single mothers. Second, the study of single mothers is of special interest, given the huge welfare policy changes that have substantially altered their work decisions recently.

The main results indicate that the effect of child care use on children's achievement is negative, significant and rather sizeable. Our estimates imply that one additional year of child care use reduces cognitive ability test scores by approximately 2.8%. This corresponds to 0.15 standard deviations, so it is a substantial effect. This result is remarkably robust, in that it differs only slightly across a wide range of production function specifications, instrument sets, and samples.

However, this general finding masks important differences across types of child care, maternal education, and child age ranges. Formal child care (i.e., pre-school, center based care) does not have any adverse effect on cognitive outcomes. In fact, for poorly educated single mothers, we find a strong *positive* effect of formal care. Only informal care (i.e., care by siblings, grandparents or other relatives, or by non-relatives, in non-center based settings) used after the first year leads to significant reductions in children's achievement. We estimate that an additional year of informal care causes a 3.4% reduction in test scores. Our overall negative estimate of the effect of child care obtains because 75% of single mothers use informal care arrangements.

Two other findings are notable: First, we provide evidence of a strong link between test scores at ages 4, 5 and 6 and completed education. Second, the effect of maternal income since birth on child test scores is quantitatively small and insignificant, given controls for maternal education and AFQT, which, in turn, are very important. This is consistent with a view that permanent income is much more important than transitory income in determining investment in children, and hence achievement.[3] But we do not attempt to disentangle the role of (i) genetic transmission of maternal ability from (ii) the impact of maternal permanent income on investment in children.

This paper is organized as follows. In section 2 we review the relevant literature. In Section 3 we describe in detail the welfare policy and local demand condition variables that we use as instruments to identify effects of child care and other endogenous inputs on child outcomes. Section 4 presents a theoretical framework for interpreting the estimates. In Section 5 we discuss the data and the sample used in this paper. Section 6 presents the estimation results and Section 7 concludes.

[3] This finding is reminiscent of the findings by Keane and Wolpin (2001) and Cameron and Heckman (1998) to the effect that transitory fluctuations in parental income have little effect on college attendance decisions by youth.

2. Literature Review

2.1. The Effect of Maternal Employment and Child Care on Children's Cognitive Outcomes

Many prior studies, mostly in the developmental psychology literature, have used the NLSY to assess effects of maternal employment and child care use on child cognitive development. Recent reviews of this literature include Love et al (1996), Blau (1999a), Lamb (1996), Haveman and Wolf (1994), Ruhm (2000) and Blau and Currie (2004). Less than half of these studies provide results that are interpretable in terms of effects of specific inputs.[4] Most present simple correlations between inputs and child outcomes and do not control for family and/or child characteristics. Furthermore, some of these studies use small samples, often nonrandomly selected. In most cases, no control for self-selection of children into child care arrangements (group of working mothers) is implemented.[5]

In Table 1 we summarize recent published papers that use data from the NLSY to assess the effect of maternal employment on children's cognitive outcomes.[6] It is clear that the evidence is quite inconclusive. Approximately a third of the studies report positive effects of maternal employment, a third report negative effects and the remaining report either insignificant effects or effects that vary depending on the group studied or the timing of inputs. A similar picture can be observed in Table 2, where we report a summary of recent papers that evaluate the effects of daycare use (and/or daycare quality) on child outcomes.[7] Again, effects range from positive to negative and are often either insignificant or vary with the specific sample used or the quality of daycare.

Reasons for the diversity in the results may include the wide range of specifications that are estimated, as well as the common limitation of failing to control for potential biases arising from endogeneity of employment and child-care choices. To make our exposition of the literature more clear, it is useful to have a specific framework in mind. Consider the following equation, interpretable as a cognitive ability production function:

$$\ln S_{ijt} = \alpha_1 T_{ijt} + \alpha_2 C_{ijt} + \alpha_3 G_{ijt} + \alpha_4 X_{ijt} + \mu_j + \delta_{ij} + \varepsilon_{ijt} \quad (1)$$

where S_{ijt} is a cognitive outcome (i.e., test score) for child *i* of mother *j* at age *t*. Here, T_{ijt} is a measure of the maternal time input up through age *t*. This might be a scalar, as in a cumulative

[4] Some studies show associations between clusters of child care arrangements and children's development instead of assessing the impact of each input (Howes and Rubenstein (1985), Peterson and Peterson (1986), Studer (1992)). And in some cases, coefficient estimates or signs are not provided by authors (e.g., Howes and Rubenstein (1981)).

[5] See for example, Burchinal et al. (1996) and Parcel and Menaghan (1990).

[6] Todd and Wolpin (2003), Rosenzweig and Wolpin (1994) and Rosenzweig and Schultz (1983) discuss the general topic of the specification/estimation of cognitive ability production functions. We summarize here only studies of parental time and child care inputs.

[7] Since the literature contains fewer studies of day care, Table 2 is not restricted to studies that use NLSY data only.

specification, or a specification where only average or current inputs matter, or a vector, if inputs at different ages are allowed to have different effects. Similarly, C_{ijt} is a measure of nonmaternal time inputs (i.e., child care), and G_{ijt} represents goods and services used in the production of child's ability. Next, X_{ijt} is a set of controls for the child's initial skill endowment. This might include variables such as the mother's age, education, AFQT score, etc. (meant to capture the inherited ability endowment), and/or initial characteristics of the child such as gender, race and birthweight. Turning to the error components, μ_j and δ_{ij} are family and child effects, which capture parts of the *unobserved* skill endowment of the child. Finally, ε_{ijt} is a transitory error term that may be interpreted as the measurement error inherent in the test plus any error in recording the test result.

While this general setup seems to underlie, at least implicitly, most of the papers in the literature, none actually estimate equation (1), and many estimate equations that seem quite far from it. One fundamental problem is that the maternal time input *T* and the goods inputs *G* are not directly observed. Most papers have ignored this problem, simply using maternal employment or child care use in place of maternal time. Also, most papers use one or the other of these variables, and do not examine both. For example, Vandell and Ramanan (1992) estimate the effect of maternal employment on child's cognitive outcomes but do not include child care arrangements as an additional input, while Caughy et al (1994) do the reverse.

Similarly, most papers simply ignore *G,* while a few proxy for it using household income or the NLSY's "HOME" environment index. The latter is problematic because it is based not just on goods inputs (e.g., books in the home) but also on maternal time inputs (e.g., time spent reading to the child). Baydar and Brooks-Gunn (1991) estimate effects of both maternal employment and child care arrangements, but do not include goods/services. Desai et al. (1989) use maternal employment to proxy for *T*, average number of child care arrangements during the first three years after childbirth to proxy for *C* and household income to proxy for *G*. But, as noted by Todd and Wolpin (2003), it is difficult to interpret production function estimates when proxies are used for key inputs. To our knowledge, only James-Burdumy (2005) discusses the relationship between her estimating equation and a child ability production function by pointing out the difficulty interpreting estimates when proxies are used for maternal time and goods inputs. We discuss this issue in detail in Section 4.

Secondly, most papers in the literature have estimated specifications that include only *current* inputs. This is a strong assumption, especially in light of the tradition in the human capital literature of letting cumulative inputs matter. One could think of the effect of inputs cumulating over time or having a more general specification according to which the whole history of inputs since childbirth matters for the child's outcome at time *t*. Most papers do not discuss the implications of their assumptions regarding timing of inputs.[8] We also discuss this issue in Section 4.

Finally, most papers estimate equation (1) by OLS, ignoring potential endogeneity of inputs – i.e., potential correlation of maternal work and day care use decisions, and goods inputs, with the unobserved ability endowments μ_j and δ_{ij}. A few recent studies try to overcome this problem by either: (1) using a very extensive set of variables to proxy for unmeasured endowments, (2) using child or family fixed effects, or "value added" models,[9] and/or (3) using instrumental variables.

Consider first the studies that can be classified as using extensive controls for the child's skill endowment. Among others, Han et al (2001), Baydar and Brooks-Gunn (1991), Parcel and Menaghan (1994), Vandell and Ramanan (1992) and Ruhm (2002), use an extensive set of observable characteristics of the child and the mother, including mother's AFQT score. In spite of this, the results of these papers are inconclusive. For example, Ruhm (2002) finds significant *negative* effects of maternal employment on math scores while Parcel and Menaghan (1994) report small *positive* effects of maternal employment on child's cognitive outcomes. Baydar and Brooks-Gunn (1991) find that maternal employment in the child's first year *negatively* affects cognitive outcomes, while Vandell and Ramanan (1992) find *positive* effects of early maternal employment on math achievement, and of current maternal employment on reading achievement.

Next, consider the studies that use fixed effects. Chase-Lansdale et al. (2003) use child fixed effects models to assess the effect of maternal employment on children's outcomes. They analyzed 2,402 low-income families during the recent era of welfare reform. Their results suggest that mothers' transitions off welfare and into employment are not associated with negative outcomes for preschoolers. They note, however, that this approach does not account for endogeneity of these transitions, and they do not attempt to use the changes in welfare rules as instruments for maternal employment as we do here.

James-Burdumy (2005) estimated household FE models using 498 sibling children in the NLSY. Her results suggest that effects of maternal employment vary depending on the particular

[8] Notable exceptions are Blau (1999a) and Duncan (2003). Some of the papers use maternal employment (and/or child care use) at different years after childbirth but do not discuss the implications of their choice in terms of the assumptions underlying the specification of the production function (e.g., Waldfogel et al. (2002), Vandell and Ramanan (1992), and Baydar and Brooks-Gunn (1991)).

[9] In the value-added approach, the test score in period *t* (S_{ijt}) is a function of the outcome in period *t-1* and the inputs in period *t*, the idea being that the lagged test score proxies for the child's ability at the start of a period.

cognitive ability assessment used and the timing of employment.[10] The use of sibling differences eliminates the mother (or household) fixed effect μ_j from (1) but does not eliminate the child fixed effect δ_{ij}. It is plausible that mothers make time compensations for children depending on their ability type. Then, using a household fixed effect model would not be appropriate, since maternal employment is correlated with the sibling specific part of the cognitive ability endowment. In addition, the FE estimator requires that input choices are unresponsive to prior sibling outcomes. If inputs to child *i'* are responsive to outcomes for child *i*, then ε_{ijt} will be correlated with those inputs.

Blau (1999a) and Duncan and NICHD (2003) both study the effects of child care usage and child care quality on child outcomes. They use very similar methodologies, including both a wide range of proxies for unmeasured child ability endowment (like mother's AFQT and education), controls for many aspects of the home environment, and use of various fixed effects and value added specifications. The main difference in the studies is that Blau (1999a) uses the NLSY while Duncan uses the NICHD Study of Early Child Care. Blau (1999a) concludes that "child care inputs experienced during the first three years of life have little impact on … child outcomes …" while Duncan finds a modest positive effect of improved child care quality.[11]

From our perspective, a key difficulty in interpreting the Blau and Duncan results is that their specifications don't allow one to infer an estimate of the effect of maternal time *per se*. Both studies include the HOME environment index, and the items used to form the index include both goods inputs, like books in the home, and time inputs, like how often the child is read to, eats meals with the parents, or talks with the mother while she does housework. Thus, the coefficients on whether the mother works or uses day care measure effects of those variables holding HOME fixed, which means holding some types of maternal time input fixed. In contrast, we are interested in the total impact of the maternal time input on child outcomes, including how a decline in the time input (from increased work or day care use) affects time spent reading to the child and so on.

The Blau (1999a) and Duncan-NICHD (2003) papers contain useful discussions of the limitations of fixed effects and value added specifications. As they point out, neither approach provides a panacea for dealing with the problem of unobserved child ability, as both models rely on

[10] According to James-Burdumy (2005)'s fixed effects (FE) estimates in her Table 5, an increase in maternal work from 0 to 2000 hours in year 1 of the child's life reduces the PIAT math score (measured at ages 3 to 5) by (-.00117) ×2000 = -2.34 points. This is similar to the effect we estimate for one year of full-time work (-3.0%). On the other hand, she finds no significant effect of maternal employment after the first year. Thus, the main difference between her results and ours is that we find maternal time is more important in years 2+ than in year 1.

[11] In particular, a one-standard deviation in child care quality causes a .04 to .08 standard-deviation increment in child cognitive ability. Quality is assessed using the Observational Record of the Caregiver Environment (ORCE).

assumptions that are in some cases stronger than OLS. For example, the household FE estimator requires that input choices are unresponsive to the child specific part of the ability endowment. The value added model runs into the problem that estimates of lagged dependent variable models are inconsistent in the presence of fixed effects like μ_j and δ_{ij}.[12] Neither approach, nor child fixed effects, deals with the endogeneity problem that arises because current inputs may respond to lagged test score realizations. An IV approach is necessary to deal with these endogeneity problems.

To our knowledge, only two prior papers have attempted to use IV in this context. These are Blau and Grossberg (1992) and James-Burdumy (2005).[13] Both look at effects of maternal work on child outcomes, and do not examine effects of day care use *per se*. More importantly, both papers suffer from the problem that the instruments are extremely weak. Thus, we would argue that their attempts to implement IV were not successful.

For instance, Blau and Grossberg (1992) use work experience prior to childbirth as the instrument for maternal employment.[14] It is questionable whether this variable is uncorrelated with the child cognitive ability endowment (as it is likely to be correlated with the mother's cognitive ability). But, setting that problem aside, note that the standard error of the variable for "proportion of weeks worked by the mother in the 2nd and later years of the child's life" increases from 1.864 to 26.831 when this instrument is used in place of running OLS (compare columns 1 and 2 of their Table 2). The latter figure implies that, to attain significance at the 5% level, the coefficient would need to be on the order of -53, implying that a mother shifting from no work to full-time work in the 2nd through 4th years would lower the PPVT test score by 53 points – about 3 standard deviations.

James-Burdumy (2005) uses the percentage of the county labor force employed in services to instrument for maternal employment in her sibling fixed effects specification. Comparing columns FE and IV-FE from her Table 3, we see that the standard error on the variable for "average hours worked per year in the first 3 years of the child's life" increases from .00178 to .01205, a factor of 7. The later standard error implies that, to attain significance at the 5% level, the coefficient on the

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[12] Estimation of a first-differenced version of the value-added specification would eliminate the fixed effects, but Blau (1999a) points out this is difficult or impossible due to limitations of existing data. This would require three outcome observations and two lagged input observations. Even if feasible, this approach would entail a severe efficiency loss.
[13] James-Burdumy (2005) looks at sibling differences in test scores to control for mother specific fixed effects, but she notes that maternal employment may still be endogenous in the differenced equation due to correlation between the time-varying part of the error term in the child outcome equation and the time-varying error term in the maternal employment decision rule. We have stressed another potential source of endogeneity (which we suspect is likely to be more important): correlation between the persistent unobserved component of child ability and the mother's decisions about work and day care (e.g., because mothers compensate relatively low ability children by spending more time with them).
[14] According to their footnote 7, this is the only variable in the prediction equation for maternal employment that does not also appear in the child outcome equation.

maternal employment variable would have to be roughly -.024. This means that increasing average hours from 0 to 2000 over the first three years would lower the PPVT test score by 48 points.

Clearly, in both these papers, the instruments are too weak for the IV estimator to identify plausibly sized effects of maternal employment on child outcomes. The main advantage of our approach is that the welfare policy and local demand instruments we employ are much stronger. Indeed, the first stage marginal R^2 values that we obtain using these instruments (i.e., about .075) are quite large relative to what one typically sees in the IV literature, and, in the second stage, standard errors on maternal employment and day care do not "explode" when these instruments are used.

Bernal (2005) takes a very different approach by estimating a structural model of work and child care choices of married women. She estimates the child cognitive ability production function – which includes mother's cumulative work and child care use (as well as cumulative income since childbirth) as inputs – jointly with the mother's work and child care decision rules. This enables her to implement a selection correction, adjusting for the fact that certain types of children are more likely to be put in child care and/or to have working mothers. Her results suggest rather sizable effects of maternal employment and child care use on children's cognitive ability. In particular, an additional year of maternal work and child care use causes roughly a 2% reduction in cognitive ability test scores of children ages 3 through 7.[15]

It is interesting to extend this work to single mothers for several reasons. First, it is important to see if the results generalize. Second, single mothers are of special policy relevance, as welfare reform led to large increases in their work/day care use. Third, as we have emphasized, as welfare policy rules have large effects on work/day care use by single mothers, their use as instruments provides a stronger basis for identification than is available for married women (i.e., it is difficult to find variables that impact behavior of married women so strongly, yet are so plausibly exogenous).

2.2 Relationship between Test Scores and Subsequent Outcomes (Wages, Education, etc.)

Several studies have examined the relationship between test scores during childhood and subsequent outcomes like educational attainment and wages. While causality is difficult to ascertain, this research has shown that children's cognitive achievements are strong *predictors* of a variety of outcomes later in life. In doing so, this literature has highlighted the importance of the issue of what determines ability of individuals at early stages of life for the design of public policy aimed at improving labor market outcomes. We summarize some of these studies in this section.

[15] Liu et al. (2003) also adopt a structural approach to estimate effects of maternal employment and school inputs on test score outcomes for 5 to 15 year olds in the NLSY. They also find a negative effect of maternal employment on child outcomes. Obviously, the focus in Bernal (2005) and here is rather different, as we are interested in pre-school inputs.

First, consider studies that use U.S. data. Neal and Johnson (1996) use the NLSY to show that scores at ages 14 to 21 on the Armed Forces Qualifying Test (AFQT), an IQ-type measure, are highly significant predictors of wages at ages 26 to 29. Murnane, Willett and Levy (1995) use two longitudinal surveys of high school seniors to document a strong relationship between their math test scores and wages at age 24. Zax and Rees (1998) use the Wisconsin Longitudinal Study (WLS) to document that age 17 IQ is a strong predictor of wages at ages 35 and 53.

Another set of studies use British data to address the same issue. For example, Hutchinson, Prosser and Wedge (1979) use the British National Child Development Study (NCDS) to link test scores at age 7 and test scores at age 16. Similarly, Connolly, Micklewright and Nickell (1992) use the NCDS to show a significant positive relationship between test scores at age 7 and earnings at age 23 in a sample of young men who left school at age 16. More recently, Robertson and Symons (1996) and Harmon and Walker (1998) also find a positive association between age 7 test scores and earnings at age 33. Currie and Thomas (2001) use the NCDS to show that a one standard deviation increase in age 16 math scores is associated with a 14% higher wage rate and a 7% higher employment rate at age 33 (for low or medium-SES individuals). In addition, they provide evidence that age 7 (math) test scores are strong predictors of age-16 math test scores.

From our perspective, a limitation of all these studies is that they look at test scores measured at age 7 or older (14 or older in the U.S. case). Do tests scores measured at earlier ages also predict subsequent achievement? To address this issue, in Appendix 1 we present evidence from the NLSY that the PPVT at age 4 and the PIAT reading and math test scores measured at ages 5 and 6[16] are significantly correlated with educational attainment of young adults who are at least 18 years old. For example, consider a one-point increase in the math test score at age 6 (i.e., roughly a 1% increase, as the mean score is 99.7). Holding parental background variables like mother's education fixed, this is associated with an increase in educational attainment (measured at age 18 or later) of approximately .019 years. Similarly, a one-point (or roughly 1%) increase in the reading test score at age 6 is associated with an increase in highest grade completed of approximately .025 years. These estimated impacts are fairly substantial, since our estimates imply that a year of full-time maternal work combined with informal day care use reduces test scores by roughly 3.4%. This translates into an effect on completed schooling of roughly .065 to .085 years, a large effect.[17]

[16] These are the same test scores used in the estimation of the cognitive ability production function in this paper.

[17] The following back-of-the-envelop calculation helps put these figures in perspective: Say people are of two types, those who finish high school (12 years) and those who finish college (16 years), and that 20% finish college. To increase average completed schooling by .08 years, the percentage finishing college must increase to 22%, a 10% increase.

A striking aspect of the Appendix 1 results is that mother's AFQT score is not a significant predictor of completed education. Thus child test scores, even measured at very young ages, are better predictors of later outcomes than are mother's scores.

3. Construction of Instruments using Welfare Rules and other Policy Variables

To deal with the endogeneity of maternal work/child care (see Section 2.1), we propose using welfare policy rules as instruments to estimate cognitive ability production functions for children of single mothers. Welfare rules are known to have a large impact on their labor supply (see, e.g., Moffitt (1992)). To construct our instruments, we collect detailed information on State welfare policies from many sources. Bernal and Keane (2005) describe these sources, and construction of the instruments, in detail. Here, in the interest of space, we only briefly highlight the key aspects of Section 1115 welfare waivers and the 1996 Welfare Reform that are relevant to this work. Table 3 presents the complete instrument list, including all the policy variables. Each instrument has up to three subscripts: *i* for individual, *s* for State and *t* for period (quarter in our case).

3.1. Benefit Termination Time Limits

Under AFDC, single mothers with children under 18 were *entitled* to receive benefits, as long as they met the income and asset eligibility requirements. But under the Section 1115 Waivers, and under TANF, the States could set time limits on benefit receipt. Indeed, PWRORA forbids States from using federal funds to provide benefits to adults beyond a 60-month lifetime time limit, and it allows states to set shorter time limits. For instance, California imposes a 5-year time limit, and Texas and Florida impose termination time limits in the 2-3 year range.

Time limits can have both direct and indirect effects. The direct effect is straightforward (i.e., when a woman hits the time limit she becomes ineligible). The indirect effect arises if women are forward-looking and try to "bank" months of eligibility for later use. We include a total of eight variables to capture both effects of time limits in the instrument list. These incorporate time limits created under both TANF and AFDC waivers, and are listed in Table 3. We include, for example, a dummy for whether the State of residence of a single mother had imposed a time limit (TLI_{st}) in time *t*, a dummy for whether the time limit could possibly be binding for a particular woman (TL_HIT_{ist}), and her maximum potential remaining time before hitting the time limit ($REMAIN\_TL\_ELIG_{\text{ist}}$).

It is worth emphasizing that we go to a great deal of effort to construct instruments that are person specific. For example, consider TL_HIT_{ist}. Let's suppose a woman resides in a State that had imposed a 5-year time limit 6 years earlier. Then it is possible that she could have hit the limit, provided her oldest child was at least 5. If her oldest child was less than 5, she could not have participated in AFDC/TANF for 5 years, and therefore could not have hit the limit. Thus, using information on age of the oldest child, we can tailor the instrument to individual cases.

Crucially, we do not use a woman's actual welfare participation history to determine if she had hit a time limit (or her remaining time of eligibility), because the actual history is endogenous. Thus, all our individual specific instruments are functions of policy rules and demographics alone.[18]

3.2. Work Requirement Time Limits and Work Requirement Exemptions

Work requirements increase time and utility costs of receiving welfare. Under PRWORA, recipients must participate in "work activities" within two years in order to continue receiving TANF benefits. But many States adopted shorter work requirement time limits. Due to variation in when States implemented TANF, and in the length of their work requirement clocks, there is substantial cross-State variation in how early single mothers could have been subject to binding work requirements. Also, States my exempt single parents with children up to 1 year old from work requirements, and may provide exemptions to other families. Thus, within a State, there is variation across women in whether work requirements can be binding, based on age of the youngest child.

We constructed a total of <u>nine</u> variables, listed in Table 3, meant to capture these various effects. For example, WR_HIT_{ist}, is an indicator for whether the woman could have been subject to work requirements (based on the length of the work requirement, time elapsed since the requirement had been implemented, age of her oldest and youngest child, etc.), and $CHILD\_EXEM_{st}$ is a dummy variable that indicates whether state *s* has an age of youngest child exemption in place at *t*.

3.3. AFDC/TANF Benefit Levels, Earnings Disregards and Benefit Reduction Rates

AFDC/TANF benefits are, roughly speaking, determined by a formula where a State specific grant level, which is increasing in number of children under 18, is reduced by some percentage of the recipient's income. One variable we use to characterize the system is the maximum *potential* real monthly AFDC/TANF benefit (BEN_{ist}), assuming zero earnings, constructed using the State grant level given the mother's family size. We put this variable in real terms using a region-specific CPI.[19]

Under AFDC, benefits were reduced as income increased according to a "benefit reduction rate" (BRR) that changed several times over the history of the program. Under waivers and the TANF program, the BRR was made State specific, and it now varies considerably across States.

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[18] Our assumption is that the welfare policy rules, as well as demographics like ages and numbers of children, are exogenous (conditional on controls for mother characteristics in the main equation).

[19] Since 1980, the BLS computed the CPI for 24 metropolitan areas. The potential benefits of individuals in other areas were deflated using a region-specific (western, south, midwest and northeast) CPI.

In addition, the AFDC program incorporated "earnings disregards" in an effort to encourage work among participants. That is, if a recipient started working, then for a period of time, a fraction of her earnings would not be subject to the BRR. Generally, the disregard consisted of a "flat" component (e.g., the first $30 of monthly earnings) and a "percentage" part (e.g., one-third of earnings beyond the flat part). Both would be eliminated after a certain number of months of work.

Starting in late 1992, many states obtained waivers to increase the income disregard. Under PRWORA, States are not required to adopt any particular earned income disregards, so a great deal of State heterogeneity has emerged. A few States expanded disregards and allowed them to apply indefinitely. We code the BRR and the percentage disregard together in the variable $PERC\_DISREGARD_{st}$. Flat disregards are coded in $FLAT\_DISREGARD_{st}$.

3.4. Child Support Enforcement

Child support is an important source of income for single mothers, despite widespread non-payment by non-custodial fathers.[20] The Child Support Enforcement (CSE) program, enacted in 1975, has implemented programs to locate absent parents and establish paternity. CSE expenditures have significantly increased from $2.9 billion in 1996 to $5.1 billion in 2002 (a 76% increase). These expenditures are an important indication of how likely a single woman is to collect child support. We include a measure of State level CSE activity by taking the State CSE expenditure and dividing it by the State population of single mothers ($ENFORCE_{st}$).

3.5. Child Care Subsidies and the Child Care Development Fund (CCDF)

The CCDF is a block grant to states to provide subsidized child care programs for low-income families, including those who are not current or former cash assistance recipients. Under the CCDF, states have autonomy to design child care assistance programs for low-income families and a great deal of heterogeneity has emerged across States in the design of their programs. As an additional policy instrument, we use the State CCDF expenditure per single mother ($CHILDCARE_{st}$). This variable measures the availability and generosity of child care subsidies in a State.[21]

3.6. Other Instruments: Earned Income Tax Credit (EITC) and Local Demand Conditions

The EITC, enacted in 1975, is a refundable Federal income tax credit that supplements wages for low-income working families. It was originally a minor program, but a major expansion of the EITC occurred in 1994-96, after which it became a sizable wage subsidy to low and moderate-

[20] In 2002, child support accounted for approximately 6.5% of single mother's real incomes (March CPS).

[21] An alternative way to measure generosity of State child care programs would be to use detailed program parameters, such as monthly income eligibility criteria, reimbursement rate ceilings or the co-payment rates, which are State specific and have also varied over time. We opt not to use these measures due to problems associated with rationing.

income families. Thus, EITC may provide an important work incentive.[22] To account for this effect we construct the EITC phase-in rate ($EITC_{ist}$) using Federal and State level EITC rules together with the mother's family composition.

Finally, we use two variables that measure local demand conditions as instruments: the State unemployment rate and the 20th percentile wage rate in the woman's State of residence at time *t*.

4. The Child's Cognitive Ability Production Function

The notion of a child cognitive ability production function is consistent with the idea that child development is a cumulative process, depending on interactions between the child's genetic endowment and parental/child care/school inputs. The human capital production framework (see Ben-Porath (1967)) views human capital acquisition as a process in which current and past inputs interact with an individual's genetic ability endowment to produce a cognitive outcome. Leibowitz (1974) adapted the original human capital framework to examine how investments in children add to preschool stocks of human capital. The process of acquiring preschool human capital is analogous to the acquisition of human capital through schooling or on-the-job training, except that, at preschool ages, the inputs are generated by joint parental/child decisions (i.e., the child's tastes presumably affect the parent's input choices), rather than decisions made by the child alone.

Let A_{it} be child *i*'s cognitive ability *t* periods after birth. We write a production function:

$$\ln A_{it} = A(\widetilde{T}_{it}, \widetilde{G}_{it}, \widetilde{C}_{it}, \omega_i) \tag{2}$$

where $\widetilde{T}_{it}$ is a vector of period-by-period maternal time inputs up through period *t*, $\widetilde{G}_{it}$ is a vector of goods inputs, $\widetilde{C}_{it}$ is a vector of day care time inputs, and ω_i is the child's ability endowment. Goods inputs include, e.g., books and toys that enhance cognitive development. Day-care inputs capture contributions of alternative care providers' time to child cognitive development. These may be more or less effective than mother's own time. Also, care in a group setting may contribute to child development by stimulating interaction with other children, learning activities at pre-school, etc..

Several difficult issues arise in estimation of (2). First, estimation of a completely general specification, where inputs may have a different effect at each age *t*, and where the endowment ω_i may differentially affect ability at each age, is infeasible due to proliferation of parameters. For instance, if the effect of just one input (say, maternal time) is allowed to differ between every pair of

[22] For example, in 2003, the phase-in and phase-out rates for a family with one child are 34% and 15.98%, respectively. As of 2003, 17 States had enacted State earned income tax credits that supplement the federal credit.

input and output periods t and t', and we examine outcomes for 20 quarters after birth, we obtain 20·21/2=210 parameters describing effects of that input alone on ability. Thus, to avoid exhausting the degrees of freedom in the data, we obviously need to restrict how inputs enter (2).

One simplification, familiar from the human capital literature, is to assume that: (i) only cumulative inputs matter, rather than their timing, and (ii) the effect of the permanent unobservable is constant over time (e.g., in a Mincer earnings function, only cumulative education and experience affect human capital, and the unobserved skill endowment has a constant effect). We first consider a specialization of (2) that adopts these assumptions, and consider some feasible relaxations later. Letting $\widehat{X}_{it} = \sum_{\tau=1,t} X_{it}$ be the cumulative amount of input X up through time t, and assuming that cumulative inputs affect $ln\ A_{it}$ linearly, we obtain a special case of (2) that takes the form:

$$(3) \qquad \ln A_{it} = \alpha_0 + \alpha_1 \widehat{T}_{it} + \alpha_2 \widehat{C}_{it} + \alpha_3 \ln \widehat{G}_{it} + \omega_i$$

We now consider problems of estimating the production function in the special case in (3).[23]

The second difficult issue is the selection (or endogeneity) problem that arises because the inputs may be correlated with the child's ability endowment ω_i. To clarify this problem, we start by assuming the ability endowment is given by the equation:

$$(4) \qquad \omega_i = \beta_0 + \beta_1 E_i + \hat{\omega}_i,$$

where E_i is a vector of mother characteristics, like education, that are correlated with child ability, and $\hat{\omega}_i$ is the part of the child ability endowment that is mean independent of mother characteristics.

Next, assume that the mother's decision rule for child care time at time t, C_{it}, can be written:

$$(5) \qquad C_{it} = \pi_0 + \pi_1 E_i + \pi_2 \hat{\omega}_i + \pi_3 cc + \pi_4 R_{it} + \varepsilon_{it}^c,$$

where cc is the price per unit of day care time,[24] R_{it} is a set of welfare program rules facing the mother at time t, and ε_{it}^c is a stochastic term subsuming factors like tastes for child care use (both permanent and transitory taste shocks), and shocks to child care availability and the mother's offered wage rate. The presence of $\hat{\omega}_i$ in the decision rule means that $\widehat{C}_{it}$ is endogenous in (3), and we will require instruments that affect C_{it} yet are uncorrelated with $\hat{\omega}_i$ and ε_{it}^c. We discuss this further below, where we argue that the welfare rules R_{it} can plausibly play this role.

[23] Letting cumulative goods enter in log form is analytically convenient, for reasons that will become apparent later.
[24] That the price of child-care *cc* is assumed constant over mothers/time is not an accident. A key problem confronting the literature on child-care is that the geographic variation in *cc* seems too modest to use it as an IV for child-care usage.

The third key issue in estimating (3) is measurement of maternal time and goods inputs. One can imagine a model where mothers decide how much "quality" time to devote to the child while at home (e.g., children's time is divided between day-care, "quality" time with the mother, and time spent watching TV while she does housework). But, we don't observe actual contact time between mothers and children (let alone how much is "quality" time), so we simply side-step the issue by assuming that $T_{it} = T\text{-}C_{it}$, where T is total time in a period. Thus, we distinguish between only two types of time (i.e., time with the mother and time in child-care). Then, we can rewrite (3) as:

$$(6) \qquad \ln A_{it} = \alpha_0 + (\alpha_1 T)\cdot t + (\alpha_2 - \alpha_1)\widehat{C}_{it} + \alpha_3 \ln \widehat{G}_{it} + \omega_i$$

Thus, we can only estimate $\alpha_2 - \alpha_1$, the effect of time in child-care *relative* to that of mother's time.

An issue we abstract from here is that maternal work time may influence how much of $T\text{-}C_{it}$ is "quality time." For example, a mother who uses child care but does not work might devote more of $T\text{-}C_{it}$ to "quality time." Thus, maternal work time might enter the production function directly, independently of how it affects the goods input (through its effect on income) or how it affects C_{it}. However, for single mothers it is very difficult to address this issue, because child care and maternal work time are extremely highly correlated (ρ=.94).[25] Thus, attempts to include both in the model fail due to severe colinearity. But we will make some attempt to address this issue in the results section.

The fourth key issue in estimation of (3) is that goods inputs G_{it} are largely unobserved. For example, the NLSY contains information on books in the home, but not nutrition, health care, tutors, recreation, etc.. To deal with this, consider a decision rule for the cumulative goods input into the child's ability (conditional on work, income and child-care usage decisions) given by:

$$(7) \qquad \ln \widehat{G}_{it} = \gamma_0 + \gamma_1 E_i + \gamma_2 \hat{\omega}_i + \gamma_3 \widehat{C}_{it} + \gamma_4 \ln \widehat{I}_{it}(W,H;R) + \gamma_5 t + \varepsilon_{it}^{g} \, .$$

This is a conditional decision rule, obtained as the second stage of an optimization process, where, in stage one, the mother chooses the child-care time inputs C and hours of market work H. The notation $\widehat{I}_{it}(W,H;R)$ highlights the dependence of income on wages, hours of market work, and the welfare rules R that determine how benefits depend on income. Equation (7) can be thought of as a linear approximation to a more complex decision rule generated by a dynamic model. The key thing captured by (7) is that a mother's decisions about goods inputs into child development may be influenced by (i.e., made jointly with) her decisions about hours of market work and child-care.

[25] Obviously, single mothers must use day care to work, and most cannot afford day care otherwise. In contrast, for married women, use of day care while not working is fairly common (see Bernal (2005)).

Equation (7) also captures the notion that per-period inputs depend on the mother's characteristics E (which determine her human capital), and the child's ability endowment $\hat{\omega}_i$.[26] The time trend in (7) captures the growth of cumulative inputs over time. This stochastic term, ε_{it}^g, captures the mother's idiosyncratic tastes for investment in the form of goods.[27]

Now, substituting (7) into (6) we obtain:

$$
\begin{aligned}
\ln A_{it} &= \alpha_0 + (\alpha_1 T)\cdot t + (\alpha_2 - \alpha_1)\widehat{C}_{it} \\
&\qquad + \alpha_3[\gamma_0 + \gamma_1 E_i + \gamma_2 \hat{\omega}_i + \gamma_3 \widehat{C}_{it} + \gamma_4 \ln \widehat{I}_{it} + \gamma_5 t + \varepsilon_{it}^g] + \omega_i \\
&= (\alpha_0 + \alpha_3\gamma_0) + (\alpha_1 T + \alpha_3\gamma_5)\cdot t + (\alpha_2 - \alpha_1 + \alpha_3\gamma_3)\widehat{C}_{it} \\
&\qquad + \alpha_3\gamma_4 \ln \widehat{I}_{it} + \alpha_3\gamma_1 E_i + (1+\alpha_3\gamma_2)\hat{\omega}_i + \alpha_3\varepsilon_{it}^g \\
&= \varphi_0 + \varphi_1 \cdot t + \varphi_2 \widehat{C}_{it} + \varphi_3 \ln \widehat{I}_{it} + \varphi_4 E_i + \hat{\hat{\omega}}_i + \hat{\varepsilon}_{it}^g
\end{aligned}
\tag{8}
$$

Equation (8) is estimable, because all the independent variables are observable. However, we must be careful about the appropriate estimation method and interpretation of the estimates. As we have already noted, child care utilization may be correlated with the unobserved part of the child's ability endowment $\hat{\omega}_i$. Furthermore, child care use may also be correlated with $\hat{\varepsilon}_{it}^g$, the unobserved taste shifter in equation (7), if tastes for child care usage ε_{it}^c in (5) are correlated with tastes for goods investment in children, as seems plausible.[28] Thus, estimation of (8) using OLS is not appropriate. To our knowledge, it has not been previously noted that consistent estimation of an equation like (8) requires instruments that are not only uncorrelated with the unobserved part of the child's skill endowment, $\hat{\omega}_i$, but also with the mother's tastes for goods investment in the child, ε_{it}^c. In order for the welfare rule parameters R_{it} to be valid instruments for cumulative child care in estimating (8), they must be uncorrelated with these two error components, which seems like a plausible exogeneity assumption.[29] We would make a similar argument for local demand conditions.

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[26] Note that the child's ability endowment may matter for two reasons: Either mother's may choose good inputs based on the child's ability (e.g., they may buy educational toys to compensate a child who is having certain learning problems) or because child ability affects the types of inputs a child demands (e.g., a high ability child may request more books).

[27] This would arise due to heterogeneous preferences for child quality. ε_{it}^g may also be influenced by the child's tastes.

[28] For instance, a mother with a high taste for child quality may both spend more time with the child (i.e., use less day care) and invest more in the child in the form of goods. This would tend to bias estimated effects of day care usage in a negative direction, since not only the maternal time input but also the goods input is lower for children in day care.

[29] In Appendix 5 we report means of child test scores prior to 1990 by State, broken down by whether the State subsequently implemented welfare waivers (i.e., moved towards Welfare reform early), and whether the State implemented strict or lenient welfare rules after 1996. There is no significant difference in average pre-reform test scores between "strict" and "lenient" States. And, in fact, the mean pre-reform test score were higher in States that failed to adopt waivers or set longer time limits. This is opposite to the direction of bias one would worry about in our results.

The cumulative income variable in (8) is also potentially endogenous, for multiple reasons. First, income depends on the jointly made child care use and work decisions. Hence it is potentially correlated with child ability for the same reasons as were operative for child care usage. Second, income depends on the mother's wage rate, which depends on her ability endowment. To the extent that this ability endowment is not perfectly captured by mother's education, and the residual part is correlated with the child ability endowment, this will also generate correlation between the mother's income and $\hat{\omega}_i$. Thus, we need to instrument for mother's income as well. Again, we will argue that welfare rules R_{it} and local demand conditions provide plausibly valid instruments, as they have important effects on work decisions, yet are plausibly uncorrelated with child ability endowments.

Assuming that instrumental variables provides consistent estimates of (8), it is important to recognize that the child care "effect" that is estimated is $\beta_2 = \alpha_2 - \alpha_1 + \alpha_3 \cdot \gamma_3$. This is the effect of child care time (α_2) relative to the effect of mother's time (α_1) plus the effect of any change in goods inputs that the mother may choose as a result of using day care ($\alpha_3 \cdot \gamma_3$). In light of this, it is important to understand the limitations of IV estimates of (8). For instance, such estimates cannot tell us how a policy like child care subsidies would affect child cognitive ability outcomes. Such subsidies would not only alter day care use, but also goods inputs, and in a way not captured by $\alpha_3 \cdot \gamma_3$. The problem arises because, while α_1, α_2, and α_3 are structural parameters of the production technology (3), the parameter γ_3 comes from the decision rule for goods inputs (7), which is not policy invariant.

Thus, when interpreting estimated effects of child care use on child cognitive outcomes, one must be careful to view them as applying only to policy experiments that do not alter the decision rule for goods in investment in children (7). As this decision rule is conditional on work, income and child-care usage decisions, it will be invariant to policies that leave the budget constraint conditional on those decisions unchanged. A work requirement that induces a woman to work and use child care, but that leaves her wage rate and the cost of care unaffected, would fall into this category.

Besides mother's education in (4), we use a rich set of additional controls for the child's cognitive ability endowment. Letting Z_i be a vector of mother/child characteristics that may be correlated with the child's skill endowment (e.g., mother's AFQT score, child gender), we obtain:

(8')
$$\ln A_{it} = \varphi_o + \varphi_1 \cdot t + \varphi_2 \widehat{C}_{it} + \varphi_3 \ln \widehat{I}_{it} + \underline{\varphi}_4 Z_i + \hat{\hat{\omega}}_i + \hat{\varepsilon}_{it}^{g} .$$

A detailed description of the variables included in Z_i can be found in Table 4.

Finally, note that the econometrician does not observe actual cognitive ability of children, but instead has available a set of (age adjusted) cognitive ability test scores from which it is possible to infer the child's cognitive ability. Let S_t be the (age adjusted) test scores observed in period t and let measurement error be specified as:

(9) $$\ln S_{it} = \ln A_{it} + \eta_1 d_{i1t} + \eta_2 d_{i2t} + \varepsilon_{it}$$

where d_{1t} and d_{2t} are cognitive ability test dummies, which capture the mean differences in scores across the three tests we use (PPVT, PIAT-math, reading),[30] and ε_{it} is measurement error.

By substituting (9) into (8') we obtain the equation that is finally estimated:

(10) $$\ln S_{it} = \varphi_o + \varphi_1 \cdot t + \varphi_2 \widehat{C}_{it} + \varphi_3 \ln \widehat{I}_{it} + \underline{\varphi}_4 Z_i + \eta_1 d_{i1t} + \eta_2 d_{i2t} + \upsilon_{it}$$

where $\upsilon_{it} = \hat{\hat{\omega}}_i + \hat{\varepsilon}_{it}^{g} + \varepsilon_{it}$. Equation (10) is the baseline specification that we estimate.

While we have considered particular functional forms in order to clarify estimation issues, in our empirical work we consider many generalizations and alternative formulations of (10). For instance, the effect of income differences may cumulate over time, but we do not know exactly at what rate. We can in principle introduce both cumulative income and current income in the same regression, and let the data reveal which matters more. Similarly, we do not know *a priori* whether it is the cumulative number of periods a child spends in day care, or whether the child is in day care in the current period that matters for current test scores. For this reason, we estimate different specifications and let the data tell us which matters most for children's development.

In addition, we estimate models that allow for heterogeneous treatment effects in the form of interactions between child care use and observed characteristics of the mother (such as education and AFQT score). This captures the notion that the effect of home inputs on child's cognitive ability might vary depending on the type of mother. We also test for differences in the effect of separation from the mother depending on characteristics of the alternative child care provider (i.e., formal vs. informal). And we allow the effect of mother's time to differ by age of the child.

5. Data

5.1. Individual Work and Child Care Histories and Construction of the Sample

We use data from the National Longitudinal Survey of Youth 1979 youth cohort (NLSY79). The data contain 12,686 individuals, approximately half of them women, who were 14-21 years of age as of Jan. 1, 1979. The sample consists of a core random sample and oversamples of blacks, Hispanics, poor whites and the military. Interviews have been conducted annually since 1979. In

[30] In particular, d_{1t}=1 if S_t corresponds to the Peabody Picture Vocabulary Test (PPVT) and d_{2t}=1 if S_t corresponds to the Peabody Individual Achievement Test-Math Section (PIAT-math). The PIAT-reading test is the base case.

1986 a separate survey of all children born to NLSY79 female respondents began. The child survey includes a battery of child cognitive, socio-emotional, and psychological well-being questions that are administered biennially, including the tests that we use in our analysis (see Sect. 5.2).

Using the NLSY79 work history file, we construct a detailed employment history for each mother in the sample for the period surrounding the birth of each child, up to four quarters before birth and 20 quarters after birth (for a period of five years). We use the geocode data to identify the State of residence of each mother in order to construct State specific welfare rule parameters.

For child care, retrospective data were gathered during 1986, 1988, 1992, and 1994-2000 that allow us to construct complete quarterly child care histories for the first three years of a child's life. In addition, data on whether the mother used child care or not during the 4 weeks prior to the interview date are available for the 1982-86, 1988, 1992 and 1994-2000 survey years. This allows us to construct partial histories of child care for the fourth and fifth years after birth.

We use the sample of single mothers in the NLSY to estimate the child's ability production function. We focus on single mothers because their labor supply behavior and child care utilization decisions were greatly affected by the welfare and other policy changes that occurred during our sample period. For instance, the percent of single mothers with children aged 0-5 who work increased from 59% in 1992 to 78% in 2001 (see Fang and Keane (2004)).

Thus, we require that women in our sample be single (or not cohabitating with a male) during five years following the birth of the child, and that we observe at least one test score for the child. There are 1,464 mother/child pairs in the NLSY79 who satisfy these criteria, and they had a total of 3,787 test score observations (an average of 2.59 per child).

In our sample, 251 women had children between 1990 and 2000, so waivers and TANF impacted their labor supply behavior before the children reached school age. Much of our leverage for identification comes from comparing outcomes for these children to those of the 1,213 children born too early for their mothers to be impacted by welfare reform before they reached age 5. However, it is important to note that even in the pre-reform period some of our instruments, like AFDC grant levels and local demand conditions, varied greatly across States and over time, also providing an important source of identification. And, in the post-reform period, we also get leverage for identification by comparing children in States with "strict" vs. "lenient" welfare rules.

In Table 5 we show how mean characteristics of single mothers in our sample compare with those of all single mothers, as well as all mothers, in the NLSY79. A concern is whether our screen that women remain single for 5 years after childbirth leads to very select sample of single mothers.

However, as we see in Table 5, the single mothers in our sample are very similar to the sample of all single mothers in the NLSY79. Of course, the single mothers in our sample differ in significant ways from typical mothers (married or single) in the NLSY. They are younger by 1.7 years, less educated by 0.8 years, and have a low wage rate ($5.08 in 1983$). They are much more likely to be Hispanic or black. They are a bit less likely to work during the first year after giving birth (39% vs. 47%).

Figure 1 displays employment and child care choices for 5 years after birth for women in our sample. During the first quarter after birth, about 73% of single mothers stay at home and do not use child care. The remainder use child care, with 10% working full-time, 5% part-time and 12% staying home. By the end of 16 quarters, only 38% continue to stay at home and not use child care. 29% work full-time, 17% part-time and 26% stay home and use child care.

5.2. Measuring Maternal Time and Other Inputs, and Measuring Child Cognitive Ability

Unfortunately for our purposes, the NLSY does not report the actual number of hours a child spent in child care (rather than maternal care). The child care variable is simply an indicator for whether the mother used child care for at least 10 hours per week during the last month.[31] This information is inadequate to determine whether a child was in child care full or only part-time. However, by combining the child care variable with work history information, we can make a reasonable determination about full vs. part-time care. Specifically, we use the following procedure:

If a woman reported using at least 10 hours per week of child care, she is assumed to have used child care during the quarter. We assume that if she worked full-time (375+ hours in the quarter) then the child care must have been full-time, which seems straightforward. On the other hand, if the mother did not work (<75 hours in the quarter) but still reported using child care, it seems highly likely that the child care usage was only part-time. More difficult is making a reasonable assignment if the mother worked part-time (75-375 hours in the quarter). We decided to assume the child care was part-time in this case. We admit this assignment is not as obvious. However, when we experimented with assigning full-time day care in this case, we found that it had almost no effect on the results. Thus, we define the function:

$$I_t^c = \begin{cases} 1 & \text{if mother works full-time and used child care} \\ 0.5 & \text{if mother works part-time and used child care} \\ 0.5 & \text{if mother did not work and used child care} \\ 0 & \text{otherwise} \end{cases}$$

[31] In 1982, 1983 and 1984, mothers were asked how many hours the youngest child was in daycare. But there is a serious missing data problem (e.g., only 115 of the 1,464 mother-child pairs in our sample have non-missing data in 1982).

and form cumulative child care, $\hat{C}_t$, average child care, $\overline{C}_t$, and current child care, C_t ,as follows:

$$\hat{C}_t = \sum_{\tau=1}^{t} I_{\tau}^{c}, \quad \overline{C}_t = \frac{1}{t}\sum_{\tau=1}^{t} I_{\tau}^{c} \quad \text{and} \quad C_t = I_t^c$$

where t is the age of the child.

As we noted earlier, complete child care histories are only available for the first three years after childbirth. Thus, we impute child care choices in years 4 and 5 after childbirth based on current work and work/child care histories. First, we set I_t^c=1 or 0.5 for mothers who work full or part-time, respectively, in a given period t after the third year. Second, for mothers who do not work in a given period t, we impute the child care choice based on the predicted probability of using child care from a probit model that we estimate using observed work and child care histories. As the probit coefficients in Appendix 2 indicate, day care use by non-working mothers is well predicted by (i) having used day care a lot in the past and (ii) having not worked a lot since child birth. The pseudo R-squared is very large, suggesting these are excellent predictors.

Another input into the child production function (10) is real household income. We measure it by summing income from all sources including wages, public assistance, unemployment benefits, interest or dividends, pension, rentals, alimony, child support and/or transfers from family or relatives. Household income is deflated using a region-specific CPI, just as we did for welfare benefits (see Section 3.3). We will experiment with different specifications of (10) using cumulative income since childbirth, average income since childbirth, and/or current income.

An issue we did not discuss in deriving (10) is that mothers may have multiple children, which may affect resources allocated to any one child. Thus, we include the number of children in Z_i in (10), and also interact the child care time input with number of children (an income interaction was insignificant). This allows day care use to affect child outcomes differently depending on the number of children. Also, the number of children may be endogenous in (10), e.g., if there is a quality/quantity tradeoff, so we instrument for these variables using the instruments in Table 3.

Finally we turn to the child cognitive ability measures in the NLSY79. The measures we use are scores on the Peabody Picture Vocabulary Test (PPVT) at age 3, 4 and 5, and the Peabody Individual Achievement Test (PIAT) at ages 5 and 6. The later consists of reading and math subtests, PIAT-R and PIAT-M, respectively. The PPVT and PIAT are among the most widely used assessments for preschool and early school-aged children. The PPVT is a vocabulary test for standard American English and provides a quick estimate of verbal ability and scholastic aptitude.

The PIAT-M consists of eighty-four multiple-choice items of increasing difficulty. It begins with such early skills as numeral recognition and progresses to measuring advanced concepts in geometry and trigonometry. Finally the PIAT-R measures word recognition and pronunciation ability.

Appendix 3 contains descriptive statistics for test scores in our sample. Note that there is no clear age pattern in the mean scores, as they are age adjusted. Mean scores on the PPVT, PIAT-M and PIAT-R are roughly 80, 95 and 101. Standard deviations seem to vary more by age than by test. For instance, at 5, the one age where we see all three tests, the standard deviations are quite similar: 17.5, 14.3 and 15.3, respectively. Thus, we decided to merge information from the three tests, allowing for mean differences. Interestingly, score differentials between children who are white/non-white and who have high-school graduate vs. high-school drop out parents are already apparent in the PPVT at age 3, and there is no discernable pattern of these differentials growing over time.

5.3. Descriptive Statistics

In Table 6 we present means and standard errors of the variables used in the analysis. For example, the average log test score in the sample is 4.50 with a standard deviation of 0.22. This drops to 0.186 when mean differences across tests are adjusted. 64% of women in the sample worked prior to giving birth at an average hourly rate of $4.39 (1983 dollars). Average work experience was 4.7 years prior to childbirth, and 72% of women had never been married. Average annual real household income is $10.9 thousand (1983) dollars. Finally, during the 20 quarters after childbirth the mothers use child care 35.5% of the time, for a total of 7.1 quarters on average.

6. Estimation Results

6.1. The First Stage Regressions

In the first stage of the two-stage least squares procedure we use the instruments in Table 3, along with all the exogenous variables that appear in (10), to predict the endogenous variables in the model (e.g., cumulative child care since birth, cumulative income since birth, number of children). The procedure is complicated by the fact that the instruments are time varying, and cumulative child care and income from birth of the child up through age t are presumably functions of the values of the instruments for all periods from birth up through time t. Thus, the set of instruments grows with t. We describe this structure in equation (1) of Appendix 4.

Table 7 examines the correlations of the instruments with the endogenous variables. The first column shows the R^2s of regressions that include only the exogenous variables (Z_i) that appear in the main equation (10). The second column shows incremental R^2s from also including the instruments described in Table 3. For cumulative and current child care, R^2 increases by roughly .088 when the instruments are added, and F-tests of their joint significance are 9.7 to 7.3, compared to a 1% critical value of 1.47. This suggests that the welfare policy and local demand instruments that we use are reasonably powerful - first stage marginal R^2s reported in applied microeconomics are often much smaller than what we report here. This is especially true of earlier attempts to use IV to study effects of day care/maternal employment (see Section 2.1).

We do not report the first stage regressions, to conserve on space.[32] It is worth noting that the 77 policy instruments have reasonable coefficients. The strongest predictors for cumulative child care use are: (i) whether a State exempts women with young children from work requirements, which has a strong negative effect (as expected), and a t-stat of about –6, (ii) whether a State had implemented a work requirement, which has a strong positive effect (as expected), and a t-stat of over 3, (iii) the remaining time that a woman can be categorically eligible for welfare (i.e., until her youngest child reaches age 18), which has a negative effect on work/day care use (t-stat of –3.8), presumably because this variable indicates presence of younger children, and (iv) the 20th percentile wage interacted with AFQT, which has a positive effect (t-stat of 2.6). Thus, a stronger labor market increases the probability of employment, and this effect is stronger for more skilled women. Notably, interactions of education with the welfare policy variables are always opposite in sign to the main effects, indicating behavior of more educated mothers is less influenced by welfare rules.

6.2. Baseline Specification of the Child Cognitive Ability Production Function

First, we seek to assess whether it is cumulative inputs or current inputs that matter most for determining children's achievement. In Table 8 we present estimates of various specifications of equation (10), including various combinations of cumulative and current *child care*, together with cumulative income. Similarly, Table 9 presents various specifications of (10) that include combinations of cumulative and current *income*, together with cumulative child care. The tables report only a few key coefficients of interest (Recall the equation includes all variables in Table 4).

In both Tables 8 and 9, OLS estimates of the effect of child care on children's achievement appear to be upwardly biased, regardless of the measure of child care or income that we use. OLS estimates of effects of child care on test scores are either insignificant or *positive* and significant. In contrast, the IV estimates are *negative* both for current and cumulative child care, and for the latter regardless of whether we control for cumulative or current income or both (see Table 9).

[32] These contain 96 variables, of which 19 are exogenous variables also appearing in the main equation, and 77 are excluded instruments. As the main equation contains 3 endogenous variables, there are 74 over-identifying restrictions.

Is current or cumulative child care use more important? When we include each separately, the point estimates are negative for each, but only cumulative child care is significant - see Table 8, columns 2 and 3. When we include both at the same time, only cumulative child care is significant – see Table 8, column 1. Thus, we decide to adopt cumulative day care as the baseline specification.[33]

In the bottom panel of Table 8, we see that the estimated effect of cumulative income since birth of the child is quantitatively small, and statistically insignificant, given controls for mother's education and AFQT score. For instance, the point estimate of .0078 in Table 8 column 2 implies that, at the mean of the data, a doubling of cumulative income would increase test scores by only about .0078 x .69 = 0.5%. In contrast mother's education and AFQT score are highly significant and quantitatively important. This is consistent with a view that permanent income is much more significant than transitory income in determining parental investment in children, and hence the children's achievement.[34] But we make no attempt to disentangle the extent to which the mother's education and AFQT coefficients reflect genetic transmission of maternal ability to the child vs. the impact of maternal permanent income on investment in children.

Finally, in Table 9 we see that the estimated effects of current income are also insignificant and quantitatively very small, regardless of whether it is included by itself or along with cumulative income. We decided to adopt cumulative income as the baseline specification in the remainder of the paper, only because it comes closer to attaining significance and, unlike current income, it has the expected sign in the IV regressions that include cumulative day care.

Thus, our baseline specification is column (2) in Table 8. This model implies that each additional quarter of full-time day care reduces a child's test scores by approximately 0.70%. Thus, a full year of full-time child care, which reduces maternal contact time with the child, is associated with a reduction of about 2.8% in child test scores.[35] This corresponds to approximately .0282/.1861 = 0.15 standard deviations of the score distribution. Viewed another way, in light of our estimates in Appendix 1, a 2.8% test score reduction at age 6 would translate into about a .053 to .070 year reduction in completed schooling. It is worth recalling that this estimate should be interpreted as the

[33] In results not reported, we also found that the average day care variable defined in section 5.2 was not significant. It's coefficient was -.0601, with a standard error of .0516.

[34] This finding is reminiscent of findings by Keane and Wolpin (2001) and Cameron and Heckman (1998) to the effect that transitory fluctuations in parental income have little effect on college attendance decisions by youth. In addition, it is consistent with findings by Blau (1999b) and Carneiro and Heckman (2002) according to which permanent household income is significant in determining investments in children while transitory income is not.

[35] As another point of comparison, for married women, Bernal (2005) estimates that one full year of maternal work and day care use reduces scores by about 2%. And, James-Burdumy (2005) estimates that a full year of maternal work in year 1 of the child's life reduces the PIAT math score (measured at ages 3 to 5) by about 2.3%.

effect of child care time (α_2) relative to the effect of mother's time (α_1) plus the effect of any change in goods inputs that the mother may choose as a result of using day care ($\alpha_3 \cdot \gamma_3$).

In addition to 2SLS, we also estimated the baseline specification using GMM. This produced very similar parameter estimates, and some gain in efficiency. The GMM estimate of the coefficient on cumulative child care is -.0063 with standard error of .0018 (t=-3.5), compared to 2SLS results of -.0070 and .0025 (t=-2.82). Notably, the J-test for validity of the over-identifying restrictions is 89.0. This test statistic is distributed $\chi^2(72)$, so the 5% critical value is 92.8. Thus, we do not reject the over-identifying restrictions at the 5% level (the *p*-value of the test is .085). This result gives us some added confidence in the plausible exogeneity of our instruments.[36]

6.3. Heterogeneity in the Effect of Maternal Time Inputs

To assess how effects of maternal time inputs on children's ability vary with characteristics of the mother/household, we next include interactions between cumulative child care and the mother's education, AFQT score and number of children. Results are presented in Table 10. Note that here, and in all subsequent tables, variables are de-meaned before they are interacted with the cumulative child care input. Thus, the cumulative child care coefficient can be interpreted as the mean affect of cumulative child care for a typical mother/household.

In Table 10 note first that the interaction between mother's education and child care use is *negative*, which is what we expected (maternal time is a less valuable input into child cognitive ability production for less educated mothers). Its t-statistic is –1.85, so it attains significance at the 6.5% level. The point estimate is fairly substantial. It implies, e.g., that if a mother's education is 4 years above the sample average, then the negative day care effect goes from –.58% to –1.12%. The later estimate has a standard error of .32, and hence a t-stat of –3.52.[37] Thus, we have strong evidence that day care has more negative (*positive*) effects for children whose mothers have higher (lower) levels of education. The same pattern holds for AFQT. We return to this issue in Section 6.8.C below, where we look at interactions between maternal characteristics and type of day care.

Also, the interaction between cumulative child care and number of children in the household is very small and insignificant, implying the child care effect does not depend on number of siblings.

[36] We choose to do most of the remaining analysis using 2SLS rather than GMM out of concern for potential instability of the GMM weighting matrix estimates given our large number of instruments. But, given the similarity of the 2SLS and GMM results, this concern may have been exaggerated. We note that ill conditioning of the variance matrix of the moment conditions required GMM to drop 2 moments in order to invert this matrix to form the weighting matrix. Thus, while our GMM estimates are based on 72 over-identifying restrictions, the 2SLS estimates are based on 74. 2SLS results that drop these two extra instruments are nearly identical to our baseline results.

[37] The reason this is so highly significant is due to the negative covariance between the main effect and interaction term.

6.4. Robustness of the Results with Respect to Age of the Mother at Childbirth

Regarding robustness of our results, a concern is that our welfare policy instruments are correlated with mothers' age at childbirth, due to the timing of waivers/welfare reform. Welfare waivers began to be implemented in some States in 1992-3. But few women were substantially impacted by waivers/welfare reform until a few years later. For instance, Fang and Keane (2004, p. 32) note that binding work requirements first began to hit significant numbers of women in 1995-6. Suppose a woman had a child in 1990, reaching age 5 during 1995. Such a woman might well have had her labor supply/child care decisions impacted by waivers when the child was 5 years old, possibly affecting test scores at ages 5 and 6. But, for children born prior to 1990, it is unlikely that waivers could have influenced the mother's labor supply behavior prior to the child reaching age 6. In the NLSY79, women who had children prior to 1990 tend to be younger at the time of childbirth than women who had children later. Indeed, from 1990 onward, all births are to mothers in their 20s and 30s, while, prior to 1990, a significant fraction were to teenage mothers.[38]

To understand the potential bias created by this correlation, consider the following scenario: Since work requirements positively affect maternal work/day care use, and work requirements are positively correlated with age at childbirth, our fitted values for maternal work/day care use from the first stage of the 2SLS procedure are *positively* correlated with maternal age at childbirth. Then, if (i) mother's age at birth has a positive effect on child cognitive ability, and (ii) we fail to adequately control for mother's age in the main equation, this will generate a spurious *positive* effect of maternal work/day care use on child cognitive test scores. Thus, since we actually find a negative effect of maternal work/day care use, the concern is that we *understate* this negative effect.

To deal with this concern, in Table 11 we present estimates based on several different specifications of the main equation. The results show that the estimated effect of cumulative child care use is extremely robust to the inclusion/exclusion of various different controls for measures of mother's age – including mother's age, age squared, and dummies for whether the mother is younger than 20 or older than 33. In fact, the estimated child care effect ranges from -0.68% per quarter when we do not control for age at all, to -0.74% when we include all four controls, to -.70% in the baseline specification - column (4) - which includes only the teenager and over 33 indicators.

A striking aspect of the results is that, conditional on measures of the mother's human capital (i.e., education, AFQT), there is absolutely no evidence of a positive association between maternal

[38] Indeed, the youngest women in the NLSY – i.e., those who were 14 in Jan. 1979 – would be 24 by Jan. 1990. Thus, the large majority of mothers who would have been affected by welfare reform would have been at least 24 at childbirth.

age at birth and child cognitive ability outcomes. Indeed, column (2) shows a significant *negative* correlation between maternal age and child cognitive outcomes, and the estimates in column (4) imply that, *ceteris paribus*, children of teenage mothers have 2.7% *higher* cognitive ability (.14 standard deviations). In hindsight, this is unsurprising. For almost all of human history teenage childbearing was the norm rather than the exception, so it would be strange to find a negative effect of teenage childbearing *per se* (controlling for economic resources) on child outcomes.

To further address this issue, we also estimated the baseline specification on subsamples of women based on the age of the mother at birth. The results are in Table 12. In column (1) we present estimates based on the sample of women that were at least 24 years old at childbirth. In column (2) we further restrict the sample to women who were between 24 and 34 years old at childbirth. Using these sub-samples reduces the estimated effect of child care only slightly (to -2.5 or -2.6% per year).

6.5. Robustness of the Results with Respect to Specification of the Main Equation

We now return to Table 11 and consider the sensitivity of our results to two other changes in the specification of the main equation. First, in column (6) we drop the mother's AFQT score. This has essentially no effect on the estimated day care coefficient. But it does produce a large increase in the coefficient on cumulative income, which becomes highly significant. This seems consistent with the view that permanent income is more important than transitory income in determining parental investment in children, and hence the children's achievement. With AFQT omitted, income appears significant, as it proxies for the mother's permanent income/skill endowment. But AFQT is a better proxy, so when it is included the income variable drops out.

Even without AFQT, the implied effect of income remains modest. The point estimate implies that, at the mean of the data, a doubling of cumulative income would increase test scores by about (.0414)·(.69)=2.9%. Recall that column (6) still includes such variables as mother's education and her pre-childbirth wage, which also proxy for her permanent income/skill endowment.

Next, in column (7), we attempt to address the issue of whether maternal work has a separate affect from day care. As noted in Section 4, married women often use day care while not working, so it is possible to estimate the effect of maternal work time holding day care time fixed (and vice-versa). But single mothers rarely use day care when they do not work. Thus, the correlation between cumulative wok and day care time is very high (ρ=.94), making it nearly impossible to separate their effects. Instead, we construct an indicator for maternal employment equal to 1 if the mother works continuously after childbirth, 0.5 if she works part of the time, and 0 if she did not work at all. This variable (which is treated as endogenous) is positive but insignificant in the regression. With its inclusion, the cumulative day care coefficient increases to -.0095 and remains highly significant.

6.6. Robustness of the Results with Respect to State Fixed Effects

Next, we examine robustness of the results to inclusion of State fixed effects. The argument for including State effects is to deal with potential cross-State correlation between the instruments and *unobserved* child skill endowments; e.g., States where children had relatively low unobserved skill endowments may have adopted stricter welfare reform. This would bias the estimated effect of day care negatively. However, we are skeptical of the fixed effects specification for two reasons: (1) Keane and Wolpin (2001) show it can lead to very misleading results if expected future values of policy variables matter for current decisions, and (2) in the child production function context, we are skeptical of the strict exogeneity assumption required for consistency.[39] Hence, we do not adopt fixed effects as the baseline specification, but we report fixed effects estimates in Table 13.

Adding State fixed effects to the main equation has only a small impact on the point estimate for the cumulative day care effect, actually increasing it from -.71% to -.83%. Despite some loss in precision of the estimate, the t-statistic only falls from -2.82 to –2.52. That State fixed effects have only a small impact on the point estimate is not surprising in light of the statistics in Appendix 5. These show there is no significant difference in average child test scores in the pre-reform period between States that subsequently adopted more vs. less strict welfare reform programs.

6.7. Robustness of the Results with Respect to the Instruments

It is well known that IV estimates can be sensitive to the instrument list, and that, given unobserved heterogeneity in treatment effects, what IV identifies depends on the instruments used. Thus, it is important examine the robustness of our results with respect to the specification of the instrument list in the first stage in the 2SLS procedure. Table 14 reports results using the baseline specification of the instrument list in column (1), and six variants on that list in columns (2)-(7).

In column (2) we exclude Child Care Development Fund expenditures. This instrument may be of questionable validity from a theoretical point of view, given the arguments in Section 4, since it may shift a mother's budget constraint conditional on her work, income and child-care usage decisions (by changing the price of child care). Excluding this instrument leads to a slightly stronger estimated day care effect (i.e., -2.8% vs. –2.9% per year) and almost no change in standard errors.

[39] Regarding future values of policy variables, note that a State's expected future level of welfare generosity should affect a woman's current labor supply decisions. Regarding failure of strict exogeneity, note that children's test score realizations at age *t* might affect future inputs into child production, and/or how the welfare policy rules evolve.

In column (3) we use only the main features of TANF as instruments: time limits, work requirements and earnings disregards.[40] This causes our estimate of the impact of day care to increase to –3.6% per year, and the standard error of the estimate increases 16%. In column (4) in contrast, we drop the TANF related instruments (time limits, work requirements and disregards), using only other aspects of the policy and local demand environment to identify the day care effect. This increases the estimated day care effect substantially (-3.9%), and raises the standard error 36%.

In column (5) we drop all instruments that are specific to the welfare reforms of the 90s (i.e., TANF, CCDF, EITC, CSE), using only instruments that would have varied across States and time regardless. These are State welfare grant levels and local demand conditions (i.e., the State unemployment rate and 20th percentile wage rate). This gives a 1st stage marginal R^2 of .058. The resulting estimate of the day care effect is much larger (-4.8%) but the standard error increases 50%.

In our first stage regressions, we interact all the policy and demand condition variables with mother's education and AFQT. This allows changes in policy/demand to have different effects on different types of mothers (e.g., welfare rules are less important for college educated mothers). In column (6) we drop these interactions to see how important they are. This has almost no effect on the point estimate of the day care effect (-2.7%), but the standard error increases 50%, causing the t-statistic to drop to 1.81. Thus, the precision we gain by allowing policy and demand variables to interact with demographics is important for obtaining significant results. In column (7) we make the first stage even more flexible by interacting all variables with mother's age. This has little impact on the estimated day care effect (still -2.8%) and only increases the precision of the estimate slightly.

In summary, our result of a negative day care effect seems remarkably robust to a wide range of alternative instrument sets, with the estimated effect ranging from –2.7% to –4.8% per year. We have experimented with a large number of other instrument sets (not reported) and continue to find results in this general range, provided we choose a set with a reasonably large value for the first stage marginal R^2. These are reported in Table 14b for each specification in Table 14a. Stronger instruments generally give results towards the bottom end of the range (i.e., -2.7% to –2.9%), as is the case here (compare the 1st stage marginal R^2 for columns 1, 2 and 7 with the other columns).

6.8. Differentiating the Effect of Child Care based on Child Age and Quality of Care

The developmental psychology literature argues that the effect of home inputs on children's development depends crucially on the ages at which inputs are applied. It also seems likely that

[40] In other words, we exclude CCDF and CSE expenditures, EITC rates, benefit amounts and local demand conditions. The time limits, work requirements and earnings disregards are still interacted with mother's education and AFQT.

quality of day care would matter. That is, *formal* center-based care provided by trained providers may well have different effects from *informal* care provided by relatives or non-relatives. For this reason, we estimated different specifications of equation (10) in which the effect of day care is allowed to vary by child age and by the type of care.

In Table 15 we present results of the first stage regressions for child care inputs at different ages and/or of different types. Even at this more refined level, the welfare policy/demand condition variables are quite powerful instruments. The marginal R^2 from the excluded instruments in the first stage ranges from .064 to .096, which, again, is quite large relative to values typically reported in the applied micro literature, and all the joint F-tests show the instruments are highly significant.

The coefficient estimates in the first stage regressions (not reported) also appear reasonable. For instance, the equations for formal and informal day care indicate one is more likely to use formal relative to informal care if: (i) the State has a young child exemption from work requirements, (ii) the State has higher child care subsidy (CCDF) spending, (iii) the State has a longer work requirement time limit, and (iv) less time has past since work requirements were implemented. A very interesting result is that work requirements raise the probability of using informal care, but not formal care. Also, if a State has more work requirement exemptions, it reduces the probability of using day care in general, but it reduces that of using informal care much more. Again, the education interactions are always opposite in sign to the main effects of the welfare rules, so the rules have less influence on more educated women. The AFQT interactions indicate those with high AFQT scores are more likely to use formal care after a work requirement is implemented. Also, a high AFQT score reinforces the effect that exemptions for young children increase the likelihood of using formal care, and a higher earnings disregard makes high AFQT women more likely to use formal care.

More briefly, in the equations for child care use in year 1 vs. subsequent years, the main patterns that emerge are: (i) whether a mother uses a child care in year one is strongly influence by whether a State has a young child exemption from work requirements (t= -4), while this is irrelevant in subsequent years, and (ii) labor market conditions (i.e., the unemployment rate and 20th percentile wage rate) influence the likelihood of working/child care use in the expected direction in years 2+. In year 1, these variables are much less important.

6.8.A. Effects of Child Care at Different Child Ages

In Table 16 we differentiate between child care in the first year vs. child care in subsequent years. The OLS results imply positive but insignificant effects of child care at all ages. But the IV results imply that age does matter. Child care during the first year after childbirth does not have any detrimental effect on children's cognitive outcomes. On the other hand, child care at ages 1-5 has a significant negative effect on children's achievement. In particular, the point estimate implies that a year of full-time day care at ages 1-5 lowers the child's cognitive test scores by 3.9%. This result is in agreement with the idea that child-mother interactions are more valuable when the child is ready to engage in more challenging tasks like learning a language and less so during initial stages when the child requires just feeding and very basic care, a point we return to below.[41]

Our results here seem to contradict the conclusions of National Research Council and Institutes of Medicine (2000), who conclude there is compelling evidence that maternal employment in the child's first year can be a negative factor for infant development.[42] Our own reading of the prior evidence is rather different. First, most of the cited studies fail to control for the fact that women who work in the first year after childbirth are systematically different from women that don't. Second, the results of the prior literature are not really so consistent.

For instance, Ruhm (2000) finds that maternal employment in the first year after childbirth reduces PPVT scores at ages 3 and 4. But he also reports that employment in the second and third years after childbirth – and not the first year – are associated with lower PIAT math and reading scores at ages 5 and 6. Of particular note is Waldfogel et al (2001), the only study in this area that uses sibling fixed effects in an attempt to correct for the endogeneity of maternal employment. They follow a sample of children of the NLSY up to ages 7 and 8. Their OLS estimates imply a negative effect of maternal employment during the first year on PPVT and PIAT math and reading scores, but their FE estimates show no negative effects of first year employment. Thus, we do not view or failure to find a negative first year effect as inconsistent with prior literature.

6.8.B. Effects of Different types of Child Care

In principle, one would expect the effect of separation from the mother to differ depending on the type of child care used. Ideally, we would like to evaluate differential effects of child care of different quality. However, it is difficult to measure child care quality in the NLSY. Lacking a direct quality measure, we instead differentiate between formal and informal child care. Formal child care is defined as any care which is center-based, e.g., day care, nursery, pre-school, pre-K. Informal child care refers to care provided by a relative (e.g., siblings, grandparents) or a non-relative.

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[41] We were unsuccessful at differentiating effects of child care more finely by age (i.e., year-by-year) due to imprecision of the estimates of the annual effects.

[42] In particular, they cite Baydar and Brooks-Gunn (1991), Belsky and Eggenbeen (1991), Ruhm (2000), Desai et al. (1989), Vandell and Corasaniti (1988), Han et al. (2000) and Waldfogel et al (2000) as supporting this conclusion.

Given the information in the NLSY, it is hard to show definitively that formal child care is higher quality than informal care. However, we argue that this is plausible given who uses formal care. In Appendix 6 we present a logit for whether a mother uses formal or informal child care (conditional on child care use). The results show that more educated, urban women with fewer children are more likely to use formal care. This strongly suggests that formal care is higher quality, since it is typically used by women who would be better able to afford more expensive care.

The IV results in Table 17 indicate that formal (i.e., center-based) care does not have any adverse effect on cognitive outcomes. Only informal types of care lead to significant reductions in achievement. In particular, an additional year of informal child care causes a 3.4% reduction in test scores. The estimated effect for formal care is actually positive, but insignificant.

Based on prior literature, we speculate that the advantage of formal center-based care over informal care arises for two reasons: (1) informal care providers may give the child less attention, or be less skilled than mothers or formal care providers, (2) center-based care may provide more stimulating interaction with other children, and more educational activity, than informal care. We do not, however, provide direct evidence that either of these mechanisms is in place.[43]

6.8.C. Effects of Child Care for Different types of Mothers

In Table 18 we explore the possibility that the effect of different types of child care might vary depending on the characteristics of the mother. We do this by including interactions of both formal and informal child care use with mother's education. The IV results indicate that informal child care has a significant negative effect on child's achievement that does not vary significantly based on the education of the mother.

However, the interaction between formal care and mother's education is negative and significant at the 10% level, implying formal care has a positive effect for less educated mothers. The point estimate is substantial, implying that, e.g., for a mother with education 2 years below the sample average (i.e., 9th grade), the effect of formal care is .00613+(-2)(-.00323)= .0126. However, this is imprecisely estimated (i.e., a standard error of .0084 and a t-statistic of 1.49).

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[43] McCartney (1984), Melhuish et al. (1992) and NICHD Early Child Care Research Network (2000) show that one of the main differences between high-quality and low-quality care is the amount of language stimulation that children receive. Center-based teachers are more likely to have received training in early development, and to have more education, which is associated with more verbal stimulation, and more supportive, attentive and interactive care (see NICHD Early Child Care Research Network, 2000). Lamb (1998) and Smith (1998) show that children whose caregivers "provide ample verbal and cognitive simulation, who are sensitive and responsible, and who give them generous amounts of attention and support are more advanced in all realms of development compared with children who fail to receive these inputs…" (see National Research Council and Institutes of Medicine (2000, p. 315)).

As this is perhaps the one point in the analysis where a key conclusion seems to hinge on efficiency of the estimates, in column (3) we also report GMM estimates. The parameter estimates hardly move, but the interaction between mother's education and formal care now has a t-statistic of –2.74. For a mother with a 9th grade education, the estimated effect of formal care is .00522+(-2)(-.00329)= .0118, or 4.7% per year, with a standard error of .0058 and a t-statistic of 2.03.

Thus we have evidence that formal center-based care may have large positive effects on cognitive development for children of poorly educated mothers, i.e., a 4.7% per year test score improvement in the case of a mother with a 9th grade education. This evidence is not inconsistent with the conclusions of National Research Council and Institutes of Medicine (2000), whose review of the literature suggests that "… experience in high-quality, center-based care starting in the second year of life may be particularly beneficial for cognitive development…"[44] Similarly, Blau and Currie (2004) summarize results of experimental studies of early childhood interventions for low income children (i.e., the Carolina Abecedarian Project, the Perry Preschool Project and the Early Training Project). They conclude these show positive and long-lasting effects of center-based arrangements that emphasize language development. Clearly, the idea that high quality day care can be a positive intervention for low income children is an important topic that warrants further research.

6.8.D. A Finer Classification of Child Care Types

In this section, we classify child types more finely into (i) informal care provided by relatives, (ii) informal care provided by a non-relative and (iii) formal center-based care. Informal care by relatives is the most used childcare arrangement among the single mothers in our sample, accounting for over 60% of all child care. Informal care by non-relatives accounts for a little less than 20%, and formal center based care accounts for only a bit over 20%.

According to the IV results in Table 19, the effect of informal child care provided by relatives is negative and statistically significant while care provided by nonrelatives and formal care do not have any significant effect on children's achievement. We have already discussed why formal care may be preferable to informal care. The new question that arises here is why informal care by relatives leads to worse outcomes than informal care by non-relatives. We cannot explore this issue directly using the NLSY. But a plausible hypothesis is that non-relatives are more likely paid providers, who would therefore (i) tend to have superior training and (ii) provide the child with more direct attention than would relatives.

[44] They cite Broberg et al. (1997), Hartmann (1995), and NICHD Early Child Care Research Network (2000) to support this conclusion.

The notion that care by non-relatives is superior is consistent with the evidence on who uses it. In Appendix 6 we present a logit for whether a mother uses non-relatives or relatives (conditional on using informal, i.e., non-center based, care). The results show that more educated, urban women with fewer children are more likely to use non-relatives. This strongly suggests that non-relatives provide higher quality care, since it is typically used by women who are able to afford better care.

6.8.E. Child Care Effects by Age and Type of Care

Lastly, we ask whether different types of child care have different effects at different ages. The results in Table 20 indicate that formal child care (i.e., pre-school, formal center based care) does not have an adverse effect on cognitive outcomes at any age. Nor does informal care in the first year after the birth of the child. However, we estimate that informal child care after the first year has a significant adverse effect on cognitive outcomes. In particular, an additional full year of informal child care used after the first year is predicted to reduce a child's cognitive test scores by 5.3% (.0534/.1861=.29 standard deviations). This is much larger than our baseline estimate of –2.8% obtained when we do not differentiate by type of child care or child age.

7. Conclusions

This paper evaluates the effects of home inputs on children's cognitive development using the sample of single mothers in the National Longitudinal Survey of Youth (NLSY). In particular, we assess the effects of child care use and household income on children's cognitive ability test scores at ages 3, 4, 5 and 6. Of course, an important selection problem arises when trying to assess the impact of maternal time and income on child development. To deal with this, we take advantage of (plausibly) exogenous variation in employment and child care choices of single mothers generated by the differences in welfare regulations across states and over time. This approach is motivated by the fact that the Welfare Reform of 1996, as well as earlier State level changes adopted under Section 1115 Welfare Waivers, generated substantial new incentives for single mothers to work and use child care. Thus, we construct a comprehensive set of welfare policy variables at the individual and State level, and use these as instrumental variables to estimate child cognitive ability production functions. We also use local demand conditions as additional instruments.

Our main results indicate that the effect of child care use on children's achievement is negative, significant and rather sizeable. In particular, one additional year of full time child care use is associated with a reduction of approximately 2.8% in cognitive ability test scores. This corresponds to 0.15 standard deviations, so it is a substantial effect.

However, this general finding masks important differences across types of mothers, types of child care, and child age. What drives the negative estimate of the effect of day care is that most (i.e., about 75%) of the day care used by the single mothers in the sample is informal (i.e., care by siblings, grandparents or other relatives, or by non-relatives in non-center based settings). Our estimates imply that a year of informal day care reduces child test scores by 3.4%.

In contrast, we find that formal center based care (e.g., center based day care, nursery, pre-K programs, etc.), has no adverse effect on child outcomes. Indeed, a particularly intriguing finding is that, for poorly educated mothers, formal care may have large *positive* effects on child outcomes. For example, for a mother with a 9th grade education, our GMM point estimates imply that an additional year of center based care raises the child's cognitive ability test scores by 4.7%. This is a substantial effect (i.e., .25 standard deviations of the score distribution). However, the effect is somewhat imprecisely estimated (i.e., standard error of 2.3, t-statistic of 2.03). Clearly, more work is needed on this important issue.

We also provide estimates of how test scores at young ages are related to completed schooling. These imply, for example, that a 1% increase in PIAT math test scores at age 6, holding parental background variables like mother's education fixed, is associated with an increase in educational attainment (measured at age 18 or later) of approximately .019 years. For reading scores the figure is .025 years. Thus, for example, the 4.7% increase in test scores induced when a mother with 9th grade education places her child in formal care for a full year subsequently translates into roughly .09 to .12 additional years of completed schooling for the child. As we've noted, this corresponds to about a 10 to 12% increase in the college graduation rate.

We also find that the age of the child matters for the effects of child care. Informal care does not have an adverse effect during the child's first year. It's adverse effect on cognitive outcomes only emerges from the 2nd year onward. We speculate that this may be because maternal or other care provider's time inputs are more critical when the child is developing language skills.

Finally, we find that the estimated effect of household income since the birth of the child is quantitatively small, and statistically insignificant, given controls for mother's education and AFQT. In contrast, mother's education and AFQT are both highly significant in the child's cognitive ability production function. This is consistent with a view that permanent income is significant in determining parental investment in children, and hence the children's achievement, while transitory income is not. We do no attempt to disentangle the extent to which this reflects genetic transmission of maternal ability to the child vs. the impact of maternal permanent income on child investment.

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Table 1
THE EFFECT OF MATERNAL EMPLOYMENT ON CHILDREN'S COGNITIVE ABILITY
(Studies that use NLSY data)

Author, year	Sample	Method	Effect of mother's employment
Mott, 1991	2387 1-4 yr olds	OLS	Negative effects
Harvey, 1999	3-12 yr olds	OLS	Negative effects
Ruhm, 2000	3-6 yr olds	OLS	Negative effects
Han et al., 2001	462 birth-8 yrs	OLS	Negative effects
Bernal, 2005	529 3 to 7 yr olds	Structural model	Negative effects
Liu, Mroz & Van der Klaauw, 2003	5 to 15 yr olds	Structural model	Negative effects
Vandel & Ramanan, 1992	1889 2nd graders	OLS	Positive effects
Parcel & Menaghan, 1994	768 3-6 yr olds	OLS	Positive effects
Greenstein, 1995	2040 4-6 yr olds	OLS	Insignificant effects
Moore & Driscoll, 1997	1154 5-14 yr olds	OLS	Insignificant effects
James-Burdumy, 2005	498 3-4 yr olds	FE and IV-FE[1]	Differing depending on test used
Waldfogel, et al., 2002	1872 birth-8 yrs	OLS and FE	Differing depending on group
Desai, et al., 1989	503 4 yr olds	OLS	Differing depending on group
Baydar & Brooks-Gunn, 1991	572 4 yr olds	OLS	Differing depending on timing
Blau & Grossberg, 1992	8784 3-4 yr olds	OLS and IV[2]	Differing depending on timing
Todd and Wolpin, 2004	6-13 yr olds	IV Child FE	Not reported

[1] Household FE, and instruments are local market conditions, e.g., county unemployment rate and percentage of the labor force in the services sector

[2] Work experience prior to childbirth is the instrument for maternal employment.

Table 2

THE EFFECT OF CHILD CARE ON CHILDREN'S COGNITIVE ABILITY

Author, year	Sample	Method	Effect of child care use
Baydar and Brooks-Gunn, 1991	572 4 yr olds	OLS	Negative effects (vary with timing)
Desai, et al., 1989	503 4 yr olds	OLS	Negative effects (only for boys)
Vandell & Corasaniti, 1990	236 8-year olds	OLS	Negative effects
Thornburg et al., 1990	835 kindergarten children	OLS	Insignificant effects
Ackerman-Ross and Khanna, 1989	3-yr olds, whites	OLS	Insignificant effects
Parcel and Menaghan, 1990	697 3-6 yr olds	OLS	Insignificant effects
Studer, 1992	95 children	OLS	Insignificant effects
Burchinal et al., 1995	6-12 yr olds	OLS	Insignificant effects
Blau, 1999	2000+ 3-5 yr olds	OLS and FE[1]	Differing depending on quality
Caughy, et al., 1994	867 5-6 year olds	OLS	Differing depending on background
Dunn, 1993	4-yr olds, middle-class	OLS	Differing depending on quality of daycare
Clarke-Stewart et al., 1994	2-4 yr olds, middle class	OLS	Differing depending on quality of daycare
Ruopp, et al., 1979	1600 preschool children	Experiment[2]	Differing depending on measure of quality
Peisner-Feinberg et al., 2001	773 4 - 8 yr olds	OLS	Positive effects (of high quality daycare)
NICHD Early Child Care Research Network, 2000	595 0-3 yr olds	OLS	Positive effects of center-based arrangements
Duncan-NICHD, 2003	1162 24-54 months old	OLS and FE[3]	Positive effects (of high quality daycare)

[1] Household fixed effects.

[2] The National Day Care Study randomly assigned children to classrooms with different staff-child ratios and to teachers with different levels of training. However, the 64 day care centers were not randomly selected.

[3] Child fixed effects.

Table 3

List of Instruments

Variable	Description
Time Limits	
TLI_{st}	Dummy for whether state s has time limit in place in period t.
TL_LENGTH_{st}	Length of time limit in state s in period t.
$ELAPSED\_TL_{st}$	Time (in months) elapsed since the implementation of time limit in state s.
TL_HIT_{ist}	Dummy variable indicating whether a woman would have hit time limit
$ELAPSED\_TL\_HIT_{ist}$	Time elapsed since woman i may potentially be subject to time limit
$REMAIN\_TL\_ELIG_{ist}$	Maximum potential remaining length of a woman's time limit, constructed: TL_LENGTH_{st}-min{$AGE\_OLDEST\_CHILD_{ist}$, $ELAPSED\_TL_{st}$}
$REMAIN\_CAT\_ELIG_{ist}$	Remaining length of time to be categorically eligible for welfare benefits: 18-$AGE\_YOUNGEST\_CHILD_{ist}$
$DCHILDBEN_{st}$	Dummy variable indicating whether the child portion of the welfare benefit continues after time limit exhaustion
Work Requirements	
DWR_{st}	Dummy for whether state s has work requirement in place in period t.
WR_LENGTH_{st}	Length (in months) of work requirement limit in state s in period t.
$ELAPSED\_WR_{st}$	Time (in months) elapsed since the implementation of work requirement in state s.
WR_HIT_{ist}	Indicator for whether a woman could be subject to a work requirement: =1 if[$WR\_LENGTH_{st} \leq$ min{$AGE\_OLDEST\_CHILD_{ist}$, $ELAPSED\_WR_{st}$} & $AGE\_YOUNGEST\_CHILD_{ist} \geq AGE\_CHILD\_EXEM_{st}$]
$ELAPSED\_WR\_HIT_{ist}$	Time elapsed since woman i may be potentially subject to work requirement
$CHILD\_EXEM_{st}$	Dummy for whether state s has age of youngest child exemption in place at t
AGE_EXEM_{st}	Age of youngest child below which the mother will be exempted from work requirement in state s at time t.
$WR\_ULT\_SANC_{st}$	Dummy for whether state s has a full sanction for non-compliance of work requirement in state s at time t.
$EXEMP_{st}$	Number of work requirement exemptions in state s
Earnings Disregards	
$FLAT\_DISREGARD_{st}$	Flat amount of earnings disregarded in calculating the benefit amount.
$PERC\_DISREGARD_{st}$	Benefit reduction rate (Does not include phase-out)
Other Policy Variables	
BEN_{ist}	Real AFDC/TANF maximum benefits, calculated using the state level benefit rule and the mother's family composition.
$EITC_{ist}$	EITC phase in rate constructed from both the federal and state level
$CHILDCARE_{st}$	CCDF expenditure per single mother in state s at time t.
$ENFORCE_{st}$	Child support enforcement expenditure in state s at year t per single mother.
Local Demand Conditions	
UE_{st}	Unemployment rate in State s in period t
$SWAGE_{st}$	Hourly wage rate at the 20th percentile of the wage distribution in State s in period t.

The instruments used in our baseline specification also include these policy variables and local demand conditions interacted with mother's education and AFQT score. In addition, workbef, EXPBEF, urban and age of mother (see definitions in Table 4) are interacted with child's age.

Table 4
Control Variables in the Cognitive Ability Production Function

Variable	Description
Baseline Specification	
$I[AGE_{i}<20]$	Dummy for whether mother is younger than 20 years old
$I[AGE_{i}>=33]$	Dummy for whether mother is older than 33 years old
$EDUC_{i}$	Mother's educational attainment at childbirth
$AFQT_{i}$	Mother's AFQT score
$I[WORK\_BEF]_{i}$	Dummy for whether mother worked prior to childbirth
$I[WORK\_BEF]_{i}$ x $SKILL_{i}$	Work dummy interacted with mother's skill*
$EXPBEF_{i}$	Mother's total work experience (in number of years) prior to childbirth
$EXPBEF_{i}*age_{i}$	EXPBEF interacted with mother's age
$MARAFT_{i}$	Mother's marital status at time of child's test
$URBAN_{i}$	Urban/Rural residence at time of child's test
$NUMCHILD_{i}$	Number of children
$RACE_{i}$	Child's race (1 if black/hispanic, 0 otherwise)
$GENDER_{i}$	Child's gender (1 if male, 0 if female)
BW_{i}	Child's birthweight
$AGECHILD_{i}$	Child's age at assessment date
$dPPVT_{i}$	Dummy for whether the corresponding test is PPVT
$dMATH_{i}$	Dummy for whether the corresponding test is PIAT-MATH
Alternative specifications also include	
AGE_{i}	Age of the mother at childbirth
AGE_{i}^{2}	Age of the mother at childbirth squared
$C_{ti}*EDUC_{i}$	Cumulative child care use interacted with mother's education
$C_{ti}*AFQT_{i}$	Cumulative child care use interacted with mother's AFQT score
$C_{ti}*NUMCHILD_{i}$	Cumulative child care use interacted with number of children

*The variable "skill" is defined as the residual from a regression of mother's initial wage on age, age squared, education and race.

Table 5
Mean Characteristics of Mothers in the Sample

Description	All mothers in NLSY	Single mothers at childbirth only	Single mothers for 5 yrs after childbirth	Our Sample
Mother's age at childbirth	24.8 (5.56)	23.56 (5.07)	23.80 (5.15)	23.13 (4.59)
Mother's education at childbirth (in years)	12.0 (2.475)	11.3 (1.920)	11.3 (1.917)	11.2 (1.909)
Mother's AFQT score	37.9 (27.23)	21.7 (20.09)	19.9 (19.11)	19.3 (18.30)
Hispanic or Black	0.47 (0.499)	0.73 (0.445)	0.79 (0.404)	0.83 (0.379)
Hourly wage before childbirth (first child)	6.32 (7.71)	4.74 (8.23)	4.90 (9.85)	4.39 (2.01)
Total number of children of mother	2.9 (1.37)	3.1 (1.57)	3.1 (1.61)	3.1 (1.53)
Father present at birth	0.55 (0.004)	-	-	-
Observations	4,814	2,528	1,820	1,464
Cases with wages at childbirth observed	2,622	1,208	753	670

Our sample screens are (1) The mother does not have a husband/partner for 5 years after childbirth and (2) The child has at least one test score observation.

Table 6
Summary of Variables used in the Empirical Analysis

Variable	Mean (standard error)
log(Test Score)	4.49855 (0.1861)*
Mother's education	11.208 (1.8972)
Mother's age	23.136 (4.5820)
Boys (Children of single mothers)	0.4976 (0.5001)
Hispanic or Black	0.8262 (0.3790)
Birthweight	111.97 (21.976)
Mother worked before giving birth	0.6431 (0.4792)
Wage rate prior to giving birth	4.3938 (2.0075)
Accumulated work experience prior to giving birth (number of years)	4.7202 (6.0088)
Never married after childbirth	0.7215 (0.4483)
Separated after childbirth	0.1540 (0.3611)
Divorced after childbirth	0.1158 (0.3201)
Urban	0.8189 (0.3851)
Average Yearly Income (Thousands)	10.9274 (13.568)
Cumulative Income (Thousands)	51.1787 (67.415)
Average Child Care Use (% of periods)	0.3546 (0.3064)
Cumulative Child Care Use (Quarters)	7.0923 (6.1273)

*Standard error of log(test score) calculated after taking out the test-specific means of the three tests, i.e., the standard error of the residuals from a regression of log(test score) on test dummies PPVT and PIAT Math.

Table 7
R-squared Values for First Stage Regressions of Inputs on Instruments Set

Input	R-squared with variables in main eqtn as instruments	Incremental R-squared after including instruments	F-test for joint significance of instruments#
Cumulative Child Care Use	0.4755	0.0881	9.6745
Current Child Care Use	0.3291	0.0889	7.3201
Cumulative Income	0.2158	0.0928	6.4321
Current Income	0.1128	0.0744	4.3866
Number of Children	0.2468	0.3964	53.241

Instruments are: variables in the main equation (see Table 4) plus mother's age and age squared, all policy variables (see Table 3), policy variables interacted with mother's education and mother's AFQT, unemployment rate in State of residence and average hourly wage at 20th percentile of wage distribution in State of residence and both interacted with mother's educations and AFQT and child's age interacted with workbef, EXPBEF, urban and age of mother (see definitions in Table 4).
Cumulative child care and income are predicted using lags and current values of the instruments listed above.
Current child care and income are predicted using current values of the instruments listed above.
Critical value at 1% is 1.47 (77 d.f. in the numerator and 3690 in the denominator)

Table 8
Do maternal time inputs matter for children's achievement?

Independent Variable -> Log(Test Score)

	(1)		(2)		(3)		(4)		(5)		(6)	
Cumulative Child Care	-0.00987		-0.00705				-0.00031		0.00080			
	(0.0029)	**	(0.0025)	**			(0.0011)		(0.0008)			
Current Child Care	0.01959				-0.00517		0.00518				0.00440	
	(0.0132)				(0.0113)		(0.0038)	**			(0.0027)	*
Log(Cumulative Income)	0.00212		0.00784		-0.00422		-0.00313		-0.00301		-0.00338	
	(0.0164)		(0.0165)		(0.0166)		(0.0057)		(0.0057)		(0.0056)	
Mother's education	0.01296		0.01388		0.01171		0.01045		0.01058		0.01040	
	(0.0030)	**	(0.0030)	**	(0.0029)	**	(0.0027)	**	(0.0027)	**	(0.0026)	**
Mother's AFQT	0.00141		0.00142		0.00139		0.00134		0.00134		0.00134	
	(0.0003)	**	(0.0003)	**	(0.0003)	**	(0.0002)	**	(0.0002)	**	(0.0002)	**
Estimation Method	IV		IV		IV		OLS		OLS		OLS	
Number of Observations	3,787		3,787		3,787		3,787		3,787		3,787	
R-squared	0.3765		0.3759		0.3739		0.3786		0.3782		0.3786	

Instruments are: variables in the main equation (see Table 4) plus mother´s age and age squared, all policy variables (see Table 3), policy variables interacted with mother's education and mother's AFQT, unemployment rate in State of residence and average hourly wage at 20th percentile of wage distribution in State of residence, and both interacted with mother's educations and AFQT and AFQT and child's age interacted with workbef, EXPBEF, urban and age of mother (see definitions in Table 4).

Cumulative child care is predicted using lags and current values of the instruments listed above. Current child care is predicted using current values of the instruments listed above.

Robust standard errors (Huber-White) by child clusters.

** Significant at 5%; * Significant at 10%

Table 9
Does Income Matter for Children's Achievement?

Independent Variable -> Log(Test Score)

	(1)	(2)	(3)	(4)	(5)	(6)
Cumulative Child Care	-0.00703	-0.00705	-0.00652	0.00075	0.00080	0.00056
	(0.0025) **	(0.0025) **	(0.0025) **	(0.0008)	(0.0008)	(0.0008)
Log(Cumulative Income)	0.01830	0.00784		-0.00897	-0.00301	
	(0.0222)	(0.0165)		(0.0070)	(0.0057)	
Log(Current Income)	-0.01406		-0.00489	0.00717		0.00309
	(0.0195)		(0.0145)	(0.0051)		(0.0042)
Mother's education	0.01413	0.01388	0.01420	0.01049	0.01058	0.01045
	(0.0030) **	(0.0030) **	(0.0030) **	(0.0027) **	(0.0027) **	(0.0027) **
Mother's AFQT score	0.00144	0.00142	0.00149	0.00133	0.00134	0.00130
	(0.0003) **	(0.0003) **	(0.0002) **	(0.0002) **	(0.0002) **	(0.0002) **
Method of Estimation	IV	IV	IV	OLS	OLS	OLS
Number of Observations	3,787	3,787	3,787	3,787	3,787	3,787
R-squared	0.3760	0.3759	0.3758	0.3787	0.3782	0.3783

Instruments are: variables in the main equation (see Table 4) plus mother's age and age squared, all policy variables (see Table 3), policy variables interacted with mother's education and mother's AFQT, unemployment rate in State of residence and average hourly wage at 20th percentile of wage distribution in State of residence, and both interacted with mother's educations and AFQT and child's age interacted with workbef, EXPBEF, urban and age of mother (see definitions in Table 4).

Cumulative income is predicted using lags and current values of the instruments listed above. Current income is predicted using current values of the instruments listed above.

Robust standard errors (Huber-White) by child clusters.

** Significant at 5%; * Significant at 10%

Table 10
Heterogeneity in Effect of Maternal Time Inputs

Independent Variable -> Log(Score)

	(1)	(2)	(3)	(4)
Cumulative Child Care	-0.00581	-0.00619	-0.00707	-0.00540
	(0.0027) **	(0.0026) **	(0.0026) **	(0.0026) *
$\widetilde{\text{Education}}$*(Cum. Child Care)	-0.001344			-0.001364
	(0.000728) *			(0.000795) *
$\widetilde{\text{AFQT}}$*(Cum. Child Care)		-0.000144		-0.000119
		(0.000078) *		(0.000080)
$\widetilde{\text{(Number of Children)}}$*(Cum. Child Care)			-0.00007	-0.00163
			0.0015	(0.0016)
Log(Cumulative Income)	0.01032	0.00847	0.00771	0.00764
	(0.0169)	(0.0169)	(0.0173)	(0.0178)
Mother's Education	0.02270	0.01309	0.01387	0.02192
	(0.0060) **	(0.0031) **	(0.0030) **	(0.0065) **
Mother's AFQT score	0.00141	0.00275	0.00142	0.00252
	(0.0003) **	(0.0007) **	(0.0003) **	(0.0008) **
No. of observations	3,787	3,787	3,787	3,787
Estimation Method	IV	IV	IV	IV
R-squared	0.3769	0.3769	0.3759	0.3778

$\widetilde{\text{Education}}$=Education-$\overline{\text{Education}}$, where $\overline{\text{Education}}$ is the mean (same for number of children and mother's AFQT score).
Instruments are: variables in the main equation (see Table 4) plus mother's age and age squared, all policy variables (see Table 3), policy variables interacted with mother's education and AFQT, unemployment rate in State of residence and average hourly wage at 20th percentile of wage distribution in State of residence, and both of these interacted with mother's educations and AFQT and child's age interacted with workbef, EXPBEF, urban and age of mother (see definitions in Table 4).
Robust standard errors (Huber-White) by child clusters.
** Significant at 5%; * Significant at 10%

Table 11

Robustness with respect to the Specification of the Main Equation

Independent Variable -> Log(Test Score)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cumulative Child Care	-0.00677	-0.00733	-0.00740	-0.00705	-0.00737	-0.00720	-0.00952
	(0.0025) **	(0.0025) **	(0.0025) **	(0.0025) **	(0.0025) **	(0.0026) **	(0.0031) **
Log(Cumulative Income)	0.01413	0.01675	0.00848	0.00784	0.00782	0.04140	0.01207
	(0.0159)	(0.0160)	(0.0169)	(0.0165)	(0.0170)	(0.0164) **	(0.0170)
Mother's education	0.01180	0.01367	0.01486	0.01388	0.01492	0.01660	0.01458
	(0.0028) **	(0.0029) **	(0.0031) **	(0.0030) **	(0.0031) **	(0.0032) **	(0.0032) **
Mother's AFQT score	0.00143	0.00136	0.00140	0.00142	0.00140		0.00130
	(0.0003) **	(0.0003) **	(0.0003) **	(0.0003) **	(0.0003) **		(0.0003) **
Child's age	0.04038	0.03968	0.04176	0.04179	0.04185	0.03181	0.04168
	(0.0102) **	(0.0103) **	(0.0104) **	(0.0102) **	(0.0104) **	(0.0104) **	(0.0104) **
Mother's age		-0.00307	-0.01927		-0.01435	-0.00760	
		(0.0015) **	(0.0110) *		(0.0153)	(0.0159)	
Mother's age squared			0.00034		0.00025	0.00009	
			(0.0002)		(0.0003)	(0.0003)	
I[age of $mother_i$<20]				0.02651	0.01058	0.01198	0.02521
				(0.0108) **	(0.0153)	(0.0154)	(0.0108) **
I[age of $mother_i$>=33]				0.00564	-0.00231	0.00489	-0.00334
				(0.0254)	(0.0322)	(0.0333)	(0.0270)
Maternal employment[#]							0.11444
							(0.0985)
Estimation Method	IV	IV	IV	IV	IV	IV	IV
Number of Observations	3787	3787	3787	3787	3787	3787	3787
R-squared	0.3744	0.3757	0.3762	0.3759	0.3764	0.3688	0.3756

Instruments are: variables in the main equation (see Table 4) plus mother´s age and age squared, all policy variables (see Table 3), policy variables interacte with mother's education and mother's AFQT, unemployment rate in State of residence and average hourly wage at 20th percentile of wage distribution in State of residence, and both interacted with mother's educations and AFQT and child's age interacted with workbef, EXPBEF, urban and age of mother

[#] Equals 1 if mother always worked since childbirth, 0.5 if she worked at least one quarter and 0 if she did not work at all (also instrumented for).

Robust standard errors (Huber-White) by child clusters.

** Significant at 5%; * Significant at 10%

Table 12
Robustness with respect to mother's age

Independent Variable -> Log(Test Score)

	(1)		(2)	
Cumulative Child Care	-0.00651		-0.00614	
	(0.0033)	**	(0.0032)	*
Log(Cumulative Income)	0.00337		0.00445	
	(0.0178)		(0.0174)	
Mother's education	0.01253		0.01200	
	(0.0045)	**	(0.0045)	**
Mother's AFQT	0.00150		0.00152	
	(0.0004)	**	(0.0004)	**
Estimation Method	IV		IV	
Number of Observations	2,176		2,304	
R-squared	0.3673		0.3677	

Instruments are: see footnote in Table 11
(1) Mothers 24 + years old at childbirth
(2) Mothers 24 to 34 years old at childbirth
Robust standard errors (Huber-White) by child clusters.
** Significant at 5%, * Significant at 10%

Table 13
Robustness with respect to State Fixed Effects

Independent Variable -> Log(Test Score)

	No State Fixed Effects		Includes State F.E.	
Cumulative Child Care	-0.00705		-0.00831	
	(0.0025)	**	(0.0033)	**
Log(Cumulative Income)	0.00784		0.00943	
	(0.0165)		(0.0190)	
Mother's education	0.01388		0.01528	
	(0.0030)	**	(0.0033)	**
Mother's AFQT	0.00142		0.00144	
	(0.0003)	**	(0.0003)	**
Estimation Method	IV		IV	
Number of Observations	3,787		3,787	
R-squared	0.3759		0.3860	

Instruments are: see footnote in Table 11.
Robust standard errors (Huber-White) by child clusters.
** Significant at 5%; * Significant at 10%

Table 14a
Robustness with respect to the Instrument List

Independent Variable -> Log(Test Score)

	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
Cumulative Child Care	-0.00705		-0.00738		-0.00890		-0.00979		-0.01198		-0.00670		-0.00691	
	(0.0025)	**	(0.0025)	**	(0.0029)	**	(0.0034)	**	(0.0037)	**	(0.0037)	*	(0.0024)	**
Log(Cumulative Income)	0.00784		0.00923		0.01189		0.02511		0.04614		0.00994		-0.01499	
	(0.0165)		(0.0168)		(0.0203)		(0.0314)		(0.0413)		(0.0208)		(0.0169)	
Mother's education	0.01388		0.01396		0.01409		0.01368		0.01287		0.01336		0.01487	
	(0.0030)	**	(0.0030)	**	(0.0031)	**	(0.0033)	**	(0.0036)	**	(0.0031)	**	(0.0030)	**
Mother's AFQT	0.00142		0.00141		0.00143		0.00136		0.00126		0.00140		0.00157	
	(0.0003)	**	(0.0003)	**	(0.0003)	**	(0.0003)	**	(0.0004)	**	(0.0003)	**	(0.0003)	**
Estimation Method	IV		IV		IV		IV		IV		IV		IV	
Number of Observations	3787		3787		3787		3787		3787		3787		3787	
R-squared	0.3759		0.3761		0.3753		0.3737		0.3733		0.3750		0.3765	

(1) All policy variables and local demand conditions in Table 3 (+ policy variables and local demand conditions interacted with mother's education and AFQT and and child's age interacted with workbef, EXPBEF, urban and age of mother). Unless otherwise noted, the specifications below still include these interaction terms.
(2) Excluding childcare expenditures (CCDF)
(3) Only work requirements, time limits and earnings disregards (excludes CCDF expenditures, CSE expenditures, EITC, local demand conditions and benefit amounts).
(4) Excluding time limits, work requirements and earnings disregards.
(5) Only benefit amounts and local demand conditions (unemployment rate and 20th percentile wage distribution).
(6) Instruments in (1) without all interactions.
(7) Instruments in (1) + policy variables interacted with child's age in addition to interactions with mother's education and AFQT
Robust standard errors (Huber-White) by child clusters.
** Significant at 5%; * Significant at 10%

Table 14b
B. R-squared Values for First Stage Regressions of
Child Care on Instruments Sets in Table 14a

Dep var-> Cum. Childcare

Instrument list corresponds to footnotes in Table 14a	R-squared*	Incremental R-squared&	F-statistic	Critical value (1%)
(1)	0.5636	0.0881	9.674	1.415
(2)	0.5614	0.0859	9.774	1.420
(3)	0.5469	0.0714	9.736	1.470
(4)	0.5367	0.0612	20.601	1.790
(5)	0.5335	0.0580	31.099	2.040
(6)	0.5412	0.0657	20.604	1.750
(7)	0.5650	0.0895	7.623	1.360

* With variables in main equation and list of instruments listed in corresponding footnote in Table 14a.
& Difference between first column and the R-squared of the first stage regression with only the variables in the main equation as instruments (which is 0.4755).

Table 15
Marginal R-squared Values for First Stage Regressions

Input	R-squared with variables in main eqtn as instruments	Incremental R-squared after including instruments	F-test for joint significance of instruments#
Cumulative Formal Child Care	0.0898	0.0832	4.8212
Cumulative Non-Formal Child Care	0.3381	0.0888	7.4254
Cumulative Child Care by Nonrelatives	0.0884	0.0858	4.9791
Cumulative Child Care by Relatives	0.2254	0.0932	6.5546
Cumulative Child Care in 1st year	0.3613	0.0625	5.1981
Cumulative Child Care after 1st Year	0.4550	0.0958	10.220
Cumulative Formal Child Care in 1st Year	0.0497	0.0822	4.5377
Cumulative Formal Child Care after 1st Year	0.0885	0.0791	4.5539
Cumulative Informal Child Care in 1st Year	0.2890	0.0637	4.7160
Cumulative Informal Child Care after 1st Year	0.3142	0.0931	7.5275

Instruments are: variables in the main equation (see Table 4) plus mother´s age and age squared, all policy variables (see Table 3), policy variables interacted with mother's education and mother's AFQT, unemployment rate in State of residence and average hourly wage at 20th percentile of wage distribution in State of residence and both interacted with mother's educations and AFQT and child's age interacted with workbef, EXPBEF, urban and age of mother (see definitions in Table 4).

Critical value at 1% is 1.47 (77 d.f. in the numerator and 3690 in the denominator)

Table 16
Child Care Effects by Age

Independent Variable -> Log(Score)

	Mean (sd error)	(1)	(2)
Cumulative Child Care 1st year	1.0817	0.00388	0.00784
	(1.3332)	(0.0037)	(0.0109)
Cumulative Child Care after 1st year	6.0110	0.00021	-0.00965
	(5.1614)	(0.0011)	(0.0028) **
Log(Cumulative Income)	3.6332	-0.00312	0.00979
	(0.7304)	(0.0057)	(0.0139)
No. of observations		3787	3787
Method of Estimation		OLS	IV
R-squared		0.3784	0.3762

Table 17
Child Care Effects by Type of Care

Independent Variable -> Log(Score)

	Mean (sd error)	(1)	(2)
Cumulative Informal child care	5.85331	0.00049	-0.00852
	(5.8728)	(0.0008)	(0.0026) **
Cumulative Formal child care	1.2229	0.00283	0.00079
	(3.0551)	(0.0011) **	(0.0042)
Log(Cumulative Income)	3.6332	-0.00316	0.00587
	(0.7304)	(0.0057)	(0.0164)
No. of observations		3787	3787
Method of Estimation		OLS	IV
R-squared		0.3791	0.3772

Instruments are: variables in the main equation (see Table 4) plus mother's age and age squared, all policy variables (see Table 3), policy variables interacted with mother's education and mother's AFQT, unemployme rate in State of residence and average hourly wage at 20th percentile of wage distribution in State of residenc and both interacted with mother's educations and AFQT and child's age interacted with workbef, EXPBEF, urban and age of mother (see definitions in Table 4).
Robust standard errors (Huber-White) by child clusters.
** Significant at 5%; * Significant at 10%

Table 18
Child Care Types and Interactions with Maternal Education

Independent Variable -> Log(Score)	(1)	(2)	(3)
Cumulative Informal child care	0.00050	-0.00818	-0.00778
	(0.0008)	(0.0029) **	(0.0021) **
Cumulative Formal child care	0.00382	0.00613	0.00522
	(0.0011) **	(0.0056)	(0.0039)
Mother's $\widetilde{\text{education}}$ * Informal	-0.00048	-0.00091	-0.00061
	(0.0003)	(0.0009)	(0.0006)
Mother's $\widetilde{\text{education}}$ * Formal	-0.00131	-0.00323	-0.00329
	(0.0006) **	(0.0019) *	(0.0012) **
Log(Cumulative Income)	-0.00210	0.01007	0.01663
	(0.0057)	(0.0166)	(0.0116)
No. of observations	3787	3787	3787
Method of Estimation	OLS	IV	GMM
R-squared	0.3803	0.3787	
J-statistic			86.234
P-value			0.0784

$\widetilde{\text{Education}}=\text{Education} - \overline{\text{Education}}$
Mean education in the sample is 11.208 years.
Instruments are: see footnote in Table 17.
Robust standard errors (Huber-White) by child clusters.
** Significant at 5%; * Significant at 10%

Table 19
Effect of Different Types of Child Care on Child's Achievement

Independent Variable -> Log(Score)	Mean (sd error)	(1)	(2)
Cumulative Informal Child Care			
Relatives	5.00766	0.00021	-0.00973
	(5.7360)	(0.0008)	(0.0027) **
Nonrelatives	1.14537	0.00142	0.00058
	(3.3549)	(0.0011)	(0.0042)
Cumulative Formal Child Care	1.2229	0.00286	0.00102
(Daycare, Nursery, Pre-K, Other)	(3.0551)	(0.0011) **	(0.0042)
Log(Cumulative Income)	3.6332	-0.00309	0.00523
	(0.7304)	(0.0057)	(0.0165)
No. of observations		3787	3787
Method of Estimation		OLS	IV
R-squared		0.3794	0.3790

Instruments: variables in the main equation (see Table 4) plus mother's age and age squared, all policy variables (see Table 3), policy variables interacted with mother's education and mother's AFQT, unemployment rate in State and average hourly wage at 20th percentile of wage distribution in State of residence, and both interacted with mother's educations and AFQT and child's age interacted with workbef, EXPBEF, urban and age of mother.
Robust standard errors (Huber-White) by child clusters.
** Significant at 5%; * Significant at 10%

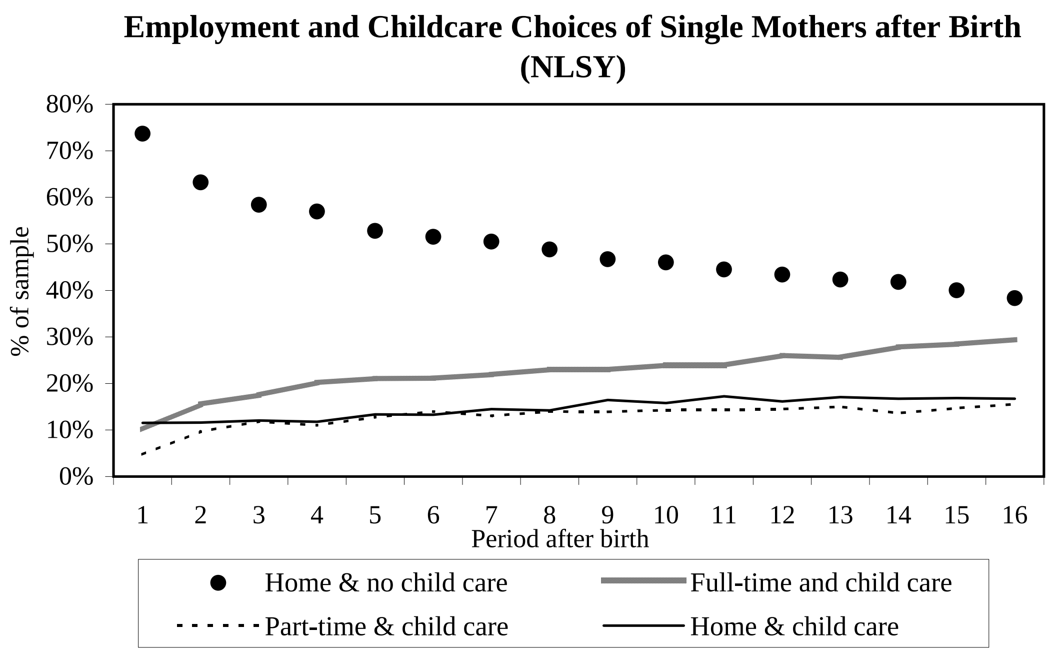
Table 20
Child Care Effects by Age and Type of Care

Independent Variable -> Log(Score)

	(1)		(2)	
Cumulative Informal child care in 1st year	0.00493		0.01843	
	(0.0038)		(0.0159)	
Cumulative Formal child care in 1st year	-0.00171		-0.00842	
	(0.0071)		(0.0266)	
Cumulative Informal after 1st year	-0.00041		-0.01334	
	(0.0011)		(0.0038)	**
Cumulative Formal after 1st year	0.00332		0.00345	
	(0.0016)	**	(0.0071)	
Mother's Education	0.01045		0.01270	
	(0.0027)	**	(0.0031)	**
Mother's AFQT	0.00133		0.00133	
	(0.0002)	**	(0.0003)	**
Log(Cumulative Income)	-0.00317		0.00904	
	(0.0057)		(0.0169)	
No. of observations	3787		3787	
Method of Estimation	OLS		IV	
R-squared	0.3797		0.3781	

Instruments are: variables in the main equation (see Table 4) plus mother's age and age squared, all policy variables (see Table 3), policy variables interacted with mother's education and mother's AFQT, unemployment rate in State of residence and average hourly wage at 20th percentile of wage distribution in State of residence, and both interacted with mother's educations and AFQT and child's age interacted with workbef, EXPBEF, urban and age of mother (see Table 4).
Robust standard errors (Huber-White) by child clusters.
** Significant at 5%; * Significant at 10%

Figure 1



Note: The first 12 periods correspond to quarters (3-months) and the last four correspond to semesters.

Appendix 1
Effect of early cognitive ability test scores on highest grade completed by 2000 (sample=young adults 18 years or older)

Independent Variable -> Highest grade completed by 2000

	PPVT at age 4		PIAT Math at age 5		PIAT Reading at age 5		PIAT Math at age 6		PIAT Reading at age 6	
Test score	0.00819	0.01574	0.00633	0.01627	0.00960	0.02092	0.01908	0.03166	0.02493	0.03744
(Test and age in column heading)	(0.0041) **	(0.0035) **	(0.0046)	(0.0044) **	(0.0048) **	(0.0045) **	(0.0049) **	(0.0045) **	(0.0056) **	(0.0055) **
Age of completed education measure#	0.16449	0.06336	0.69629	0.68394	0.69097	0.66007	0.45305	0.41675	0.45629	0.40288
	(0.1563)	(0.1575)	(0.0752) **	(0.0717) **	(0.0758) **	(0.0723) **	(0.0438) **	(0.0409) **	(0.0439) **	(0.0411) **
Highest grade completed by mother	0.09231		0.05216		0.04901		0.09646		0.10179	
	(0.0403) **		(0.0348) *		(0.0343)		(0.0270) **		(0.0268) **	
Highest grade completed by father	0.02147		0.02069		0.01948		0.00833		0.01065	
	(0.0083) **		(0.0076) **		(0.0075) **		(0.0064)		(0.0064) *	
Number of siblings	-0.14160		-0.14066		-0.12912		-0.08883		-0.08942	
	(0.0586) **		(0.0543) *		(0.0535) **		(0.0428) *		(0.0424) **	
Birthorder	-0.11146		-0.13111		-0.09435		-0.11223		-0.07853	
	(0.0979)		(0.0957)		(0.0946)		(0.0754)		(0.0751)	
Race (1=Non-white)	0.06958		0.08739		0.06939		-0.06182		-0.21639	
	(0.1751)		(0.1523)		(0.1496)		(0.1258)		(0.1243) *	
Gender (1=Male)	-0.36024		-0.42114		-0.42716		-0.39478		-0.37505	
	(0.1380) **		(0.1236) **		(0.1228) **		(0.1011) **		(0.1008) **	
Mother's age at child's birth	-0.03878		-0.01219		-0.02523		0.02586		0.03390	
	(0.0387)		(0.0336)		(0.0331)		(0.0282)		(0.0280)	
Mother's AFQT score	0.00389		0.00378		0.00450		0.00128		-0.00030	
	(0.0038)		(0.0033)		(0.0033)		(0.0029)		(0.0028)	
Constant	7.2531	8.3078	-2.8778	-3.8171	-2.9770	-4.0097	-0.6925	-0.1622	-1.5869	-0.6049
	(3.1866) **	(2.9599) **	(1.8248)	(1.4088) **	(1.7977)	(1.3892) **	(1.2501)	-(0.1622) **	(1.2644)	(0.9295) **
No. of observations	363	363	451	451	446	446	747	747	739	739
Pseudo R-squared	0.1578	0.0537	0.2791	0.2014	0.2912	0.2209	0.2365	0.1761	0.2457	0.1760

All estimated by OLS. ** indicates significance at 5% and * at 10%.
The age of the young adult by 2000 if she is older than 18 years old. The average age is 21.8.
PPVT: Peabody Picture Vocabulary Test; PIAT: Peabody Individual Achievement Test

Appendix 2

Probit to predict child care choices of non-working women in years 4 and 5 after childbirth

Dependent Variable-> Pr(using child care in *t*)		
Whether worked before giving birth	0.5920	
	(0.208)	**
(Whether worked before) x (Avg. wage before)	-0.0642	
	(0.040)	*
Total work experience (prior to giving birth)	-0.0060	
	(0.019)	
Child's race	-0.0874	
	(0.170)	
Child's gender	0.0497	
	(0.120)	
Mother's education	0.0821	
	(0.038)	**
Total work experience since child birth	-0.3983	
	(0.070)	**
Total child care use since child birth	0.2226	
	(0.053)	**
Whether used child care or not in *t-1*	1.7801	
	(0.164)	**
Estimation	Probit	
Number of observations	867	
Pseudo-R^2	0.4585	

*Additional controls: Marital status at child birth (never married, separated,divorced, widowed),urban/rural residence and mother's age at birth.

**For women who reported working full-time in a given period after the third year, we imputed a child care value equal to 1; if the mother reported working part-time, we imputed a child care value equal to 0.5. Finally, if the mother does not work in a given period, we imputed a child care value of 0.5 if thepredicted probability of child care use based on this model exceeds 0.65.We choose this threshold to obtain a smooth trend of child care use since childbirth and until the end of the fifth year.

Appendix 3
Cognitive Ability Tests in our NLSY sample
Descriptive Statistics

	PPVT			PIAT - Math		PIAT-Reading	
Child's Age	3	4	5	5	6	5	6
Log(test score) in our sample	4.367	4.2689	4.402	4.539	4.543	4.633	4.606
	(0.191)	(0.295)	(0.239)	(0.152)	(0.128)	(0.152)	(0.095)
Test Scores in our sample	80.263	74.334	83.767	94.719	94.802	104.089	100.585
	(14.952)	(19.512)	(17.504)	(14.329)	(11.727)	(15.319)	(9.462)
Non-whites	78.007	70.836	82.135	93.836	94.247	103.358	100.482
	(14.169)	(17.958)	(16.889)	(14.289)	(11.685)	(15.454)	(9.269)
Whites	92.167	89.299	93.852	99.576	97.657	108.100	101.112
	(13.348)	(18.885)	(18.001)	(13.634)	(11.578)	(13.970)	(10.422)
Maternal education (12 yrs+)	82.820	78.748	88.743	97.084	96.823	106.755	102.265
	(14.369)	(18.917)	(17.648)	(14.178)	(11.663)	(15.131)	(9.425)
Maternal education (<12 yrs)	76.301	68.748	79.508	91.767	92.751	100.697	98.847
	(15.025)	(18.847)	(16.245)	(13.991)	(11.449)	(14.909)	(9.197)
Male	79.753	72.242	83.035	93.726	93.710	102.557	99.232
	(14.664)	(20.048)	(18.143)	(14.307)	(12.292)	(15.563)	(9.404)
Female	80.707	76.299	84.569	95.739	95.827	105.685	101.838
	(15.225)	(18.820)	(16.783)	(14.305)	(11.091)	(14.922)	(9.357)
No. of observations	339	512	438	598	663	584	653

PPVT: Peabody Picture Vocabulary Test

PIAT: Peabody Individual Achievement Test

Standard errors in parenthesis.

Appendix 4

Let S_3, S_4, S_5 and S_6 be the child's test scores at ages 3 though 6, respectively.[1] For example, S_3 can be the PPVT score at age 3.[2] In addition, let Y_3, Y_4 and Y_5 represent the endogenous variables that appear in the test score equation (10) in year 3, 4 and 5 after childbirth respectively. For example, Y_5 would include cumulative child care use up through age 5. Finally, let $R_1, R_2, \ldots, R_5$ represent vectors of instruments that are relevant for the mother's decisions in years 1 through 5 after the birth of the child. Thus, for example, R_5 would include welfare policy rules operative in the state of residence of the mother in year 5,[3] and Y_5 is potentially influenced by R_1 through R_5. The first stage regressions in the 2SLS procedure will thus look like:

$$\begin{aligned}
Y_{3i} &= \alpha_0 + \alpha_1 R_{1i} + \alpha_2 R_{2i} + \alpha_3 R_{3i} + \alpha_4 \cdot 0 \;\; + \alpha_5 \cdot 0 \;\; + \underline{\alpha_6} X_{3i} + \varepsilon_i \\
Y_{4i} &= \alpha_0 + \alpha_1 R_{1i} + \alpha_2 R_{2i} + \alpha_3 R_{3i} + \alpha_4 . R_{4i} + \alpha_5 \cdot 0 \;\; + \underline{\alpha_7} X_{4i} + \varepsilon_i \\
Y_{5i} &= \alpha_0 + \alpha_1 R_{1i} + \alpha_2 R_{2i} + \alpha_3 R_{3i} + \alpha_4 . R_{4i} + \alpha_5 R_{5i} + \underline{\alpha_8} X_{5i} + \varepsilon_i
\end{aligned} \qquad \text{(A4.1)}$$

where X_{ti} is a vector of exogenous characteristics of mothers and children that include all variables described in Table 4, and $\underline{\alpha_6}$ is an associated parameter vector. Notice that R_1 through R_t all enter the equation for Y_t. From A4.1, we obtain the fitted values $\hat{Y}_{ti}$ for t=3, 4, 5.

Finally, the second stage regressions in the 2SLS procedure would look like:

$$\begin{aligned}
S_{3i} &= \beta_0 + \beta_1 \hat{Y}_{3i} + \underline{\beta_2} X_{3i} + \xi_i \\
S_{4i} &= \beta_0 + \beta_1 \hat{Y}_{4i} + \underline{\beta_2} X_{4i} + \xi_i \\
S_{5i} &= \beta_0 + \beta_1 \hat{Y}_{5i} + \underline{\beta_2} X_{5i} + \xi_i \\
S_{6i} &= \beta_0 + \beta_1 \hat{Y}_{5i} + \underline{\beta_2} X_{6i} + \xi_i
\end{aligned} \qquad \text{(A4.2)}$$

where β_1 is the parameter of interest. Notice that the test score at age 6 is only influenced by the endogenous variable dated at t=5 (i.e., cumulative day care use up through age 5), since at age 6 the child is of school age so day care is no longer necessary.

In the baseline specification, in order to avoid proliferation of parameters, we estimate a constrained version of the 1st stage regressions A4.1 where we assume the effects of the instruments on the endogenous variable Y_{ti} are the same in every year after birth, i.e., $\alpha_1 = \alpha_2 = \ldots = \alpha_5$.[4] Also, we constrain $\underline{\alpha_6}$, $\underline{\alpha_7}$ and $\underline{\alpha_8}$ to differ only in that a subset of the elements of X are interacted with child age.[5]

[1] Recall that cognitive ability test scores are available as early as age 3 in the NLSY.
[2] Since we have quarterly data, a test score at 3 literally means a test score in the 12th quarter after the birth of the child.
[3] As well as interactions of the policy rules with mother's education and AFQT.
[4] In one robustness test (Table 14 column 7) we interact all the instruments with child age, but it makes little difference.
[5] These are workbef, EXPBEF, urban and age of mother. Other interactions were insignificant.

Appendix 5
Average Test Scores for Children born prior to 1990

	Average	St. Dev	ttest
States that implemented TL waivers	93.34	(1.82)	-0.46
States that did not implement TL waivers	92.42	(1.08)	
States that implemented WR waivers	89.77	(1.35)	1.56
States that did not implement WR waivers	93.45	(1.09)	
States with TL lower than 3 years	90.2	(2.46)	0.87
States with TL higher than 3 years	93.02	(1.00)	
States with immediate WRs	93.48	(1.81)	-0.66
States with WRs of at least 1 month	92.20	(0.95)	
States with Age of Youngest child exemption < 6 mont	93.40	(2.20)	-0.51
States with Age of Youngest child exemption > 6 mont	92.38	(0.84)	

Source: NLSY, sample of single mothers

Appendix 6
Who is using formal child care and care provided by non-relatives?

Independent Variable ->	1 if formal childcare used (0 if informal)	1 if care provided by non-relative (0 if relative)
Mother's education	0.12126	0.11247
	(0.0149) **	(0.0167) **
Mother's age at birth	-0.01140	0.02024
	(0.0056) *	(0.0061) **
Number of children	-0.08925	-0.04318
	(0.0191) **	(0.0213) **
Urban/rural	0.17590	0.63539
	(0.0637) **	(0.0798) **
No. of observations	12,167	9,471
Method of Estimation	Logit	Logit
Pseudo R-squared	0.0116	0.0209