Early Maternal Employment and Children's Vocabulary and Inductive Reasoning Ability: A Dynamic Approach

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Abstract

This study investigates the relationship between early maternal employment history and children's vocabulary and inductive reasoning ability at age 5, drawing on longitudinal information on 2,200 children from the Growing Up in Scotland data. Prior research rarely addresses dynamics in maternal employment and the methodological ramifications of time-variant confounding. The present study proposes various measures to capture duration, timing, and stability of early maternal employment and uses inverse probability of treatment weighting to control for time-variant confounders that may partially mediate the effect of maternal employment on cognitive scores. The findings suggest only modest differences in the above ability measures between children with similar observed covariate history but who have been exposed to very different patterns of early maternal employment.

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Child development has been conceptualized as a dynamic process whose outcomes are shaped by both individual characteristics and contextual factors that may vary over time (Bronfenbrenner & Morris, 2006; Shonkoff & Phillips, 2000). Importantly, this implies that a child's developmental status at a given time is the cumulative result of a complete context history and not only affected by concurrent experiences (Duncan & Raudenbush, 1999, p. 34). Despite these conceptual and theoretical reflections, empirical studies on the relation between specific contextual factors and developmental outcomes have not consistently heeded the dynamic nature of childhood contexts and its methodological ramifications. One issue is the difficult measurement of potentially complex, multidimensional contextual histories. Moreover, time-variant confounding arising from the causal and temporal interconnectedness of different contextual domains poses a key analytical challenge.

Maternal employment is an important factor in children's early lives whose effect on developmental outcomes is the subject of contentious public and scientific debate. While some researchers highlight its potentially positive impact through an increase in a family's economic resources, others caution that time in the labor market may decrease the quantity and quality of mother-child interactions (for an overview, see Heinrich, 2014). Assessing these conflicting arguments requires measuring long-term maternal employment histories as these mechanisms only unfold after sustained exposure, may vary by timing of exposure, and may have a lasting impact even after subsequent changes. However, prior research, has used either measures of maternal employment at specific time points (e.g. Brooks-Gunn, Han, & Waldfogel, 2002; Vandell & Ramanan, 1992) or employment history measures without appropriately accounting for the processes shaping these histories (e.g. Desai, Chase-Lansdale, & Michael, 1989). The first approach cannot capture the cumulative impact of maternal employment history on developmental

outcomes (without assuming employment status is stable throughout early childhood). The latter approach ignores that mothers may repeatedly change their labor force participation in response to their children's development or other dynamic context factors that are likewise associated with developmental outcomes, such as family structure, maternal health, or residential community.

In this study, we investigated the association between early maternal employment history and children's vocabulary and inductive reasoning ability at age 5. We followed the recent literature on neighborhood effects on child development, which realigns empirical analyses of contextual influences on child development with theoretical and conceptual models by explicitly accounting for the dynamics in childhood conditions (Sharkey & Faber, 2014; Wodtke, Harding, & Elwert, 2011). We aimed to make four distinct contributions to the existing literature. Firstly, we refined the theoretical discussion of the pathways through which maternal employment may influence cognitive development by highlighting the role of time-variant factors that are likely to affect maternal employment decisions and to depend themselves on prior maternal employment. Secondly, we constructed different measures of maternal employment history that capture various significant components such as overall work hours, sequencing of different employment states, as well as diversity and (in)stability. Thirdly, we introduced inverse probability of treatment (IPT) weighting to the literature on maternal employment and child development, a statistical method that was developed in biostatistics to account adequately for time-variant confounders that partially mediate the effect of prior exposure. Lastly, we expand the sparse literature on maternal employment and cognitive ability in European countries (for notable exceptions see Cooksey, Joshi, & Verropoulou, 2009; Gregg, Washbrook, Propper, & Burgess, 2005; Huerta et al., 2011; Verropoulou & Joshi, 2009) by using data from a cohort of children born in Scotland in 2004 and 2005. Following work-family reconciliation policies and increased spending on childcare services,

maternal employment rates have sharply increased in Scotland since the 1980s, particularly among mothers with pre-school children (Fagan & Norman, 2012). Among other initiatives, the Scottish Children & Young People Act 2014 recently increased the entitlement to free early learning and childcare from 475 to 600 hours per year to reduce barriers to maternal employment. Given the focus on increasing mothers' labor market participation in Scotland, and elsewhere, providing robust evidence on the consequences of maternal employment histories for children's early cognitive development is of vital importance to future policy.

Conceptual Framework and Prior Research

The growth of children's vocabulary is generally regarded as a particularly malleable aspect of language acquisition, depending significantly on the quantity and quality of linguistic input directed at children by their parents and other caretakers (Hart & Risley, 1995; Hurtado, Marchman, & Fernald, 2008; Huttenlocher, Haight, Bryk, Seltzer, & Lyons, 1991; Weisleder & Fernald, 2013). The relative importance of contextual factors for developing inductive reasoning abilities (and general cognitive ability) is less clear. However, parent-child interaction and a stimulating learning environment are deemed conducive (Nisbett et al., 2012; Shonkoff & Phillips, 2000). It is reasonable to assume that language input and a stimulating learning environment must be sustained over an extended time period to be effective (Bronfenbrenner & Morris, 2006). In the case of vocabulary development in early childhood, there appears to be no critical period of development—children with previous deficiencies can catch up if given sufficient input later on. For early cognition, there is no robust evidence either for or against timing effects (Shonkoff & Phillips, 2000). Rather, cognitive ability appears more flexible in early childhood, compared to after school entry (Knudsen, Heckman, Cameron, & Shonkoff, 2006).

Pathways from Maternal Employment to Vocabulary and Inductive Reasoning

In the literature, maternal employment is frequently linked to children's learning environment and thereby to the development of vocabulary and inductive reasoning through two pathways: the intensity of employment (i.e., work hours and stress) and income (Heinrich, 2014). Due to regular separation and fatigue, working mothers may be less thoughtful of, and perceptive to, children's requirements, which may decrease cognitive stimulation for children (Belsky, 2001; Chase-Landsdale & Owen, 1987; Vaughn, Gove, & Egeland, 1980). Long working hours, heavy workload, or non-standard work schedules are associated with maternal stress and mental health problems, which have an adverse impact on children's developmental outcomes (Downey & Coyne, 1990). Dual-earner couples also face an increased risk of conflict and relationship dissolution (Frisco & Williams, 2003), both of which have potentially detrimental effects on children's development (McLanahan, Tach, & Schneider, 2013).

Prior research shows, however, that while working women spend less time with their children than women staying at home (Fox, Han, Ruhm, & Waldfogel, 2013), reduced time spent with their children was found to have no impact on child cognitive outcomes (Booth, Clarke-Stewart, Vandell, McCartney, & Owen, 2002; Huston & Rosenkrantz Aronson, 2005). In fact, the quality of mother-child interactions appears to be better and more cognitively stimulating when mothers are employed (Hsin & Felfe, 2014). Employed mothers may compensate for spending less time with their children by engaging in activities that are beneficial for children's cognitive ability when they do (Fiorini & Keane, 2014). Employed mothers read to their children more frequently (Zick, Bryant, & Osterbacka, 2001) and are more interactive with their children when it comes to verbal stimulation (Zaslow, Pedersen, Suwalsky, Cain, & Fivel, 1985). Finally, studies show that maternal employment leads to an increase in paternal involvement, which compensates for working mothers' time limitations, in turn, fostering language growth and cognitive stimulation

(Raley, Bianchi, & Wang, 2012; Sandberg & Hofferth, 2001).

Beyond a mere compensation of reduced mothers' time and attention, there may also be tangible benefits of maternal employment for children's cognitive development through an increased family income. Although there are no specific and expensive toys or materials known to increase language and reasoning abilities (Shonkoff & Phillips, 2000), economic resources are essential for providing a beneficial learning environment for children (Linver, Brooks-Gunn, & Kohen, 2002; Yeung, Linver, & Brooks-Gunn, 2002). Financial resources may enable parents to avoid, or leave, housing and residential areas that pose social and physical risks for parental health and well-being, or that may cause direct harm to children's health and development (Harding, Gennetian, Winship, Sonbonmatsu, & Kling, 2011; Shonkoff & Phillips, 2000). Family income is also associated with better child nutrition and lower levels of parental mental health problems (Heinrich, 2014; Shonkoff & Phillips, 2000) that are detrimental for child development. Additional income may also be used for high-quality non-family early education and childcare, giving children the opportunity to interact with adults that are trained to foster their development and well-being, and may provide them with more cognitively challenging equipment and resources than are typically available in their homes (Clarke-Stewart, 1991). Maternal income may also affect child development through family structure, assisting women to leave dysfunctional family contexts (Schoen, Astone, Rothert, Standish, & Kim, 2002). Women's economic resources are also believed to influence fertility decisions, and thereby, the number of siblings (Brewster & Rindfuss, 2000), another predictor of children's cognitive development (Steelman, Powell, Werum, & Carter, 2002).

Empirical studies that aim to identify the causal effect of maternal employment on children's cognitive ability provide mixed evidence (Goldberg, Prause, Lucas-Thompson, &

Himsel, 2008). Many US studies found adverse effects of early maternal employment on children's cognitive scores in early ages, using the 1979 National Longitudinal Study of Youth (NLSY79) (Baum, 2003; Bernal, 2008; Blau & Grossberg, 1992; Cooksey et al., 2009; Desai et al., 1989; James-Burdumy, 2005; Ruhm, 2004; Waldfogel, Han, & Brooks-Gunn, 2002). However, with the same data, Vandell & Ramanan (1992), identified a positive effect of early maternal employment on child cognitive development. Based on the NICHD Study of Early Child Care and Youth Development, Brooks-Gunn et al. (2002, 2010) provided further evidence of adverse effects of early maternal employment on child cognitive scores. Other US studies focusing on subgroups found a positive association between maternal employment and child cognitive development, for instance, among black boys (Greenstein, 1995) or children raised by single mothers (Youngblut et al., 2001). The few existing UK studies found no significant effect of early maternal employment on cognitive ability (Cooksey et al., 2009), a slightly negative effect for low-educated mothers (Verropoulou & Joshi, 2009) or a slightly negative effect for high-educated mothers (Gregg et al., 2005). Apart from using different data, samples and children's age at assessment, the literature commonly explains these contradictory results with differences in analytical approach. Many studies also control for endogenous variables such as family income and non-maternal child care in their analysis and thus control away a mechanism by which maternal employment affects child cognitive ability (e.g. Waldfogel et al., 2002).

Maternal Employment History and Time-Variant Confounding

Because contextual factors such as maternal employment often vary over a child's early life course there are several reasons why measures of context history instead of snapshot measures may be better suited to investigate associations with child development. Firstly, maternal employment in one particular year may not be sufficient for shaping the resilient cognitive

development of children, so that snapshot measures may underestimate the cumulative impact of maternal employment. Secondly, holistic measurement is crucial to assess whether the temporal sequencing of contextual exposures makes a difference for developmental outcomes. For example, mothers who work in the year following birth may be those most likely to work continuously later on. Without holistic measurement, we cannot distinguish whether early or later maternal employment affects child development. Thirdly, independent from the level of exposure, stability in exposure may be decisive, enabling families to develop routines to deal with and compensate for otherwise detrimental factors, whereas frequent transitions may hinder the development of such strategies.

Many studies of maternal employment and children's cognitive development relied on one or more snapshot measures at specific ages of the child (e.g. Brooks-Gunn et al., 2002; James-Burdumy, 2005; Vandell & Ramanan, 1992; Waldfogel et al., 2002). Existing studies measuring maternal employment by average work hours over a specific range of a child's age (e.g. Blau & Grossberg, 1992; Ruhm, 2004; Youngblut et al., 2001) or specific employment patterns over time (e.g. Desai et al., 1989; Greenstein, 1995; Hill, Waldfogel, Brooks-Gunn, & Han, 2005) have not adequately accounted for time-variant confounding.

Similar to maternal employment at a given point in time, time-invariant factors such as maternal education at birth or children's birth weight may shape employment histories and drive children's cognitive development and therefore need to be accounted for in the analysis. However, in a dynamic setting, in which the goal is to assess the combined impact of exposure at different points in time, the analyst also needs to address time-variant confounders. Figure 1 depicts these putative causal relations for two exemplary measurements of maternal employment, with the arrows generally signifying a causal effect in the direction of the arrow and the black arrows

summarizing the cumulative effect of maternal employment history on subsequent cognitive ability.

Figure 1 here

Importantly, many of the factors through which maternal employment at one point in time affects cognitive outcomes may confound the effect of maternal employment status at a later date, such as family income, socio-economic status of the residential area, maternal and child health, or family structure. In other words, Figure 1 illustrates that there is good reason to suspect that time-variant confounders mediate some portion of the effect of maternal employment history on cognitive outcomes. This causal structure poses several methodological challenges (Robins & Hernán, 2009). The analyst would be required to control for time-invariant but also any time-variant confounders, to identify the causal effect of maternal employment history on developmental outcomes. However, this control would lead to removing the indirect impact of maternal employment that is mediated by time-variant confounders and thus to underestimate the cumulative effect of maternal employment history (i.e., over-control bias, see Sampson, Morenoff, & Gannon-Rowley, 2002). Moreover, controlling for time-variant confounders may also induce spurious association between maternal employment and cognitive measures through (unmeasured) common causes of time-variant confounders and outcomes (i.e., endogenous selection bias, see Elwert & Winship, 2014). In sum, these problems make it impossible to estimate consistently the cumulative impact of maternal employment history on vocabulary and inductive reasoning with standard analytical tools. This is the case even if all common causes of maternal employment and cognitive abilities were measured and included in the statistical analysis, unless one were willing to stipulate that family structure, family income and other time-variant predictors of maternal employment status are unaffected by prior maternal employment. In the present study, we go

beyond the existing literature by capturing distinct aspects of maternal employment history and by applying statistical methods specifically developed to deal with time-variant confounders that are suspected to mediate partially the effect of interest.

Method

Data and Sample

We used Birth Cohort 1 from the Growing Up in Scotland (GUS) data (ScotCen Social Research, 2013), an annual survey of children born between June 1 2004 and May 31 2005 and living in Scotland at the time of sampling (see Anderson et al., 2007, for detailed information on sampling and data collection). Information on date of birth and residence was obtained from Child Benefit records, including 97% of Scottish residents with children at this time. The original sample consisted of all babies with eligible birth dates from 130 randomly selected geographic areas. If more than one eligible child lived in the same household, one child was chosen at random. The sampled areas were drawn from data zones aggregated to have an average of 57 live births per year over the previous three years. To ensure that areas from all major regions and with diverse socio-economic status were represented, sampling was stratified by the 32 local authorities in Scotland as well as by the Scottish Index of Multiple Deprivation (SIMD), a composite measure of relative deprivation that ranks 6,505 local areas in Scotland from least to most deprived based on indicators of employment, income, health, education, skills and training, geographic access to services, crime and housing.

Sweep 1 of data collection took place when children were around ten months old with fieldwork commencing in April 2005 and continuing through May 2006. From the 8,218 eligible children, 966 cases that were considered sensitive or had already been sampled for research by the Department of Work and Pensions in the three years prior were not contacted. An additional 669

cases had ineligible or incorrect addresses. Altogether, 5,217 children (80% of those 6,583 contacted) were successfully recruited. Models of nonresponse for the first interview suggest that younger parents, those with more than three children, and respondents from urban and more deprived areas may be slightly underrepresented among the initial participants. Sampling weights provided by GUS account for selection into the study (based on observed covariates). Trained social survey interviewers then carried out computer-assisted personal interviews with a respondent from each family at intervals of roughly one year until around the child's fifth birthday in 2009/10. Interviewers were instructed to conduct the survey with the same respondent as in the previous sweep. In 98% of all completed interviews in the first six waves of data collection (N = 25,406), the respondent was the child's biological mother.

Our analytical sample excludes specific socio-demographic groups (e.g. mothers under age 20 age 39) because we believe it likely that among the excluded subgroups the effect of maternal employment on our outcomes is different from the majority. Therefore, the analysis was restricted to single pregnancies (98%) by White (96%), Christian or non-religious mothers (97%) who were born in the United Kingdom (94%), between ages of 20 and 39 at the time of birth (89%), who had worked at some point before the first interview (95%), and who acted as the survey respondent at first interview (99%). We also excluded one child raised by a same-sex couple. Small case numbers prevent us from investigating maternal employment effects on cognitive outcomes for the excluded subgroups.

For the remaining 4,029 children, 3,538 (88%) were observed with complete information on maternal employment and all covariates at the first interview. We followed these children until the fifth wave of data collection or until the child was lost to follow-up, yielding 10,569 (14,107) yearly observations which were used to model selection into maternal employment (and loss to

follow-up). Cases were considered lost to follow-up as soon as they temporarily or permanently dropped out of the survey, changed to a primary respondent other than the mother, or were observed with missing values on either of the covariates or maternal employment. Full information for all five years including cognitive ability measures was available for 2,200 children (62% of those eligible in the first year). We used these cases to estimate the association between different measures of maternal employment history and cognitive ability. Compared to the sample at Sweep 1 (as well as the full GUS sample and the Scottish population at large), this remaining sample, on average, is of higher socio-economic status as indicated most prominently by higher maternal and partner education, higher household income, a higher percentage of homeowners, and lower ranks on the SIMD. We will address this selectivity by using panel attrition weights in our modelling (see the methods section).

Measures

Vocabulary and Inductive Reasoning Ability

At sweep five, around children's fifth birthday, GUS carried out two cognitive assessments, naming vocabulary and picture similarities, which are part of the British Ability Scales Second Edition (Elliott, Smith, & McCulloch, 1996; Elliott, Smith, & McCulloch, 1997), a widely used battery of individually administered tests of cognitive abilities and educational achievements for children between the ages of 2.5 and 17 years. Naming vocabulary consists of identifying different objects in a colored picture booklet and measures expressive language ability and knowledge of nouns. In the "picture similarities" task, an assessment of inductive reasoning ability, children are shown a row of four pictures (e.g., an animal, a person, a car, a house) and are asked to match a fifth picture card (e.g., another animal) to the picture with which it shares a conceptual or elementary link. For both tests, we used the so-called ability score included in GUS, which corrects

for differences in item difficulty and thus provides a standard scale of performance level on each test. The score values, however, are arbitrary. Because extremely low scores on these measures may reflect a reluctance to speak or impulsiveness, four otherwise eligible cases were dropped from our sample.

Maternal Employment

GUS collected mothers' self-reported employment status at each interview as full-time employed, part-time employed, or not working. We used this information to construct measures of mothers' employment history over the first five years after birth. Ideally, we would use an indicator for each possible employment sequence. Such a nonparametric approach, however, imposes prohibitive demands on the data even in the case of only three different employment states and five time points, which yield $3^5 = 243$ possible sequences, 181 of which were populated by the sample. Consequently, we used alternative measurement strategies that sizably reduce the amount of information but nonetheless capture three different key aspects of early maternal employment sequences. The first set of measures captures the number of years a mother worked in full-time and part-time employment, respectively. Two additional measures, the within-sequence turbulence (Elzinga & Liefbroer, 2007) and the sequence complexity index (Gabadinho, Ritschard, Studer, & Müller, 2010), capture the complexity and stability of a given employment sequence. Whereas turbulence accounts for the number of distinct employment episodes in a complete sequence and the variance of the consecutive times spent in these distinct episodes, the complexity index is composed of the number of employment transitions in a sequence and the longitudinal entropy (i.e., a measure of the diversity of the states of a given sequence). Lastly, we constructed a categorical measure for seven typical employment patterns, grouping employment sequences that were similar to each other: (1) never worked (2) always worked full-time (3) always worked part-

time (4) increasing work hours (5) decreasing work hours (6) unstable but mostly not working (7) unstable but mostly working. Obviously, the first three patterns included employment sequences only composed of one and the same employment status. The fourth category was made up of sequences that began with non-employment or part-time employment and later transitioned to part-time or full-time employment. Decreasing work hours summarized the opposite cases, which started out in full-time or part-time employment and then reduced their work hours to part-time employment or non-employment. The two later patterns included all sequences that did not fit any of the aforementioned categories and were divided into those where, in the majority of years, employment was reported and those for which non-employment was reported for most years.

Covariates

Covariate selection was guided by theoretical considerations about which factors may affect mothers' employment status at a given time while at the same time influencing children's cognitive ability at a later point. Some of these covariates were time-invariant, either because they cannot change within individuals or were measured only at the first interview. We included indicators of child's sex, low birth weight (< 2.5 kg), medical treatment during pregnancy, and whether the child attended a special care unit or a neonatal unit. We also accounted for parents' intention regarding pregnancy (i.e., 'planned', 'not really planned', 'not planned at all'), mothers' age at birth ('20-29' vs. '30-39', provided by GUS as categorical variable only), mothers' education at first interview ('no qualification', 'lower secondary education', 'upper secondary education', 'vocational and postsecondary education certificates or diplomas', 'higher education'), whether the mother worked during pregnancy, and whether family and friends provided any childcare in the first months after birth.

In addition to these covariates, mothers' employment status in a given year likely depends on time-variant characteristics, in particular family structure, maternal and child health, and economic resources and opportunities. Family structure was measured by the number of siblings living in the household ('none', 'one', 'two or more'). According to the 2011 census, 26 per cent of the households in Scotland included at least one dependent child. Among those, households with one dependent child and those with two or more were evenly split (National Records of Scotland, 2015). In order to include single mothers and control for partner characteristics at the same time, we combined information on mothers' relationship status with their partners' educational level (i.e., 'no partner', 'cohabitation with a partner with upper secondary education or below, 'cohabitation with a partner with postsecondary qualification or above', 'married with a partner with upper secondary education or below', 'married with a partner with postsecondary qualification or above'). Maternal health and child health were measured by indicators of whether the mother reported that she and the child were not in good health ('fair' and 'poor' vs. 'excellent', 'very good', and 'good'), respectively. We used household income, a dummy for homeownership, the SIMD-quintile in which the current residential area was located, and an indicator on whether the family lived in an urban area (i.e., population $\geq$ 10,000) to cover economic resources and opportunities. To account for possible reverse causality between child development and maternal employment we also included an indicator for whether the mother was concerned about the focal child's development in a given year. Finally, we also controlled for the number of cumulative residential moves during the observation period.

Analytic Approach

In our core analysis, we estimated the association between the two cognitive ability scores at age five and the four different measures of maternal employment history separately. We used

weighted least squares regression to fit eight models of the form

$$Y_5 = \beta x + Z_1, \quad (1)$$

where the respective cognitive ability score, Y_5, is a function of the respective maternal employment history measure, x, and a vector of baseline confounders, Z_1. As shown above in Figure 1, we also hypothesized the existence of time-variant confounders, Z_t, which—if not accounted for—would render β a biased and inconsistent estimator of the causal effect of maternal employment history on cognitive ability.

However, instead of controlling for these characteristics in the outcome models of Equation 1 (e.g., by including their child-specific mean values over the observation period), we used inverse probability of treatment (IPT) weighting to create a pseudo-population in which maternal employment status at a given time point is independent of time-variant confounders (Robins & Hernán, 2009). The resulting causal relations in the pseudo-population are depicted in Figure 2. Compared to those in the original data shown in Figure 1, the causal arrows from time-variant confounders to maternal employment are deleted while the other causal relations, including the indirect effect of maternal employment through time-variant confounders, remain intact. Therefore, controlling for time-variant characteristics is not necessary in the pseudo-population for β in the outcome models of Equation 1 to consistently estimate the causal effect of maternal employment history on cognitive ability scores. At the same time, controlling away the indirect effect (and inducing endogenous selection bias) can be avoided.

Figure 2 here

The pseudo-population was created by re-weighting each child with the stabilized

treatment weight,

$$sew_{5i} = \prod_{t=2}^{5} \frac{P(E_t=e_{ti}|E_{(t-1)i}, E_{1i}, Z_{1i})}{P(E_t=e_{ti}|E_{(t-1)i}, E_{1i}, Z_{(t-1)i}, Z_{1i})}, \quad (2)$$

that is, the ratio of the probability that the child experiences the observed maternal employment status at time *t* conditional on previous maternal employment status, maternal employment status at baseline and covariates at baseline and the same probability conditional also on time-variant covariates at time *t*-1 multiplied over years two to five. Consequently, children with covariate histories overrepresented in the current maternal employment status are given less weight, whereas children with less frequent covariate histories receive a higher weight, so that confounders in the previous year, *t*-1, are equally distributed among all values of maternal employment status at any time *t* after reweighting. Because both probabilities are unknown, we estimated them using multinomial logistic regression (Fewell et al., 2004). The results from these models are shown in Table S1 in the Online Appendix.

In addition to stabilized IPT weights, we estimated stabilized inverse probability of attrition (IPA) weights,

$$saw_{5i} = \prod_{t=2}^{5} \frac{P(C_t=0|C_{t-1}=0, E_{(t-1)i}, E_{ti}, E_{1i}, Z_{1i})}{P(C_t=0|C_{t-1}=0, E_{(t-1)i}, E_{ti}, E_{1i}, Z_{(t-1)i}, Z_{1i})}, \quad (3)$$

to correct for nonrandom loss to follow-up in our data (Robins, Hernan, & Brumback, 2000). Analogous to the procedure described for IPT weighting, reweighting renders loss to follow-up independent of measured time-variant covariates; it creates a pseudo-population in which attrition

occurs at random with respect to these covariates. We estimated the necessary probabilities using logistic regression models (see Table S2 in the Online Appendix) and incorporated the resulting IPA weights by multiplying them with the IPT weights from Equation 2 (Fewell et al., 2004) and the sampling weights provided by GUS.

A causal interpretation of our estimates rests on the strong assumptions of no unmeasured confounding, positivity, and correct parametric specification of the weight models and the outcome models. The assumption of no measured confounding was represented in both Figures 1 and 2 in that none of the unmeasured characteristics directly affected maternal employment status at any time. It cannot be tested empirically and its plausibility depends on whether all (or more realistically, most) theoretically relevant confounders were measured and included in the analyses. Positivity demands that a comparison of "like with like" be feasible by requiring a nonzero probability to experience each possible maternal employment status within any combination of covariate values. Violations of positivity and misspecification of the weight models result in weights with mean values far from one or large standard deviations (Cole & Hernán, 2008). Table S3 in the Online Appendix shows that neither was the case for our weights. Furthermore, Tables S4 and S5 display the distribution of maternal employment status by different combinations of covariates, revealing no evidence of severe violations of positivity.

Compared to conventional regression analyses, however, IPT weighting does not require the assumption that time-variant confounders be unaffected by earlier maternal employment status. We consider this a key advantage for our analysis because, from a theoretical perspective, time-variant characteristics such as family structure, household income and health may both partially mediate the effect of maternal employment and confound the effect of subsequent maternal employment.

Results

Descriptive Statistics

Tables 1 and 2 summarize descriptive statistics for time-invariant and time-variant characteristics pertaining to the sample of children not lost to follow-up until age 5 (n = 2,200). The naming vocabulary score varied between 40 and 161 points (M = 111, SD = 14). For the picture similarities score, variation was smaller, ranging between 43 and 119 points (M = 84, SD = 11). Slightly more than half of the children in the sample were boys. Low birth weight and stays in a special care unit or a neonatal unit were an exception. Medical attention during pregnancy, however, was common. Although most pregnancies were reported as intended, almost one-third were not (entirely) planned. The mothers of most children in our sample were in their thirties at birth and worked during pregnancy, with unqualified mothers being a rare exception. For almost half of the children, mothers reported at the first interview that family or friends provided childcare.

Table 1 here

In the UK, mothers predominantly remain in paid work through part-time employment after maternity leave (Fagan & Norman, 2012). The National Health Service provides universal medical insurance also to mothers who do not work full-time. In our sample, there was a weak trend towards increased work hours over the first five years after birth. Whereas almost half of the children shared their home with no siblings in the year following birth, more than 80% lived with one or more siblings at age 5. Changes in the distribution of other potentially time-variant characteristics appear minor in comparison.

Table 2 here

Maternal Employment Histories

Table 3 provides an overview of maternal employment histories using the four measures described above as well as the number of changes in employment status observed over the first five years after birth. On average, mothers in the sample experienced 1.1 years of full-time employment and 2.3 years of part-time employment. For 41% of the mothers, no changes in employment status were observed. Most of these mothers (17% of the sample) continuously experienced part-time employment. About the same percentage of mothers never worked throughout the observation period. Only 9% of mothers were continuously employed full-time. 19% of the sampled mothers experienced an increase in work hours at least once, whereas only 9% reduced the amount of time spent in the labor market. For a large part of the sample, less stable maternal employment patterns with more than one employment transition were observed, the majority of which consisted of employment in most of the years (25% of the sample). Only 7% of mothers experienced unstable employment patterns in which non-employment was dominant.

Table 3 here

Sequence turbulence and sequence complexity index provide additional measures for the (in)stability of maternal employment independent of specific work hours. They directly capture the diversity of employment states in a given sequence along with the time spent in each distinct episode (turbulence) or the number of transitions (complexity index). Again, both measures indicate that, on average, employment histories were rather stable. However, some mothers experienced highly turbulent and complex employment sequences, potentially associated with little stability in both financial resources and parental routines. In sum, there was a marked diversity in maternal employment histories with no single pattern being dominant.

Regression Results

The estimated associations between maternal employment history measures and cognitive ability scores are displayed in Table 4. We standardized both scores and fitted an IPT weighted model with covariates measured at baseline (as outlined in Equation 1) for each of the four maternal employment measures separately (Models A-D in columns 2 and 5, respectively). Table 4 also includes estimates from separate models for each maternal employment measure that did not account for any covariates (columns 1 and 4) and estimates from models that include time-variant characteristics measured as individual-specific means over the observation period (columns 3 and 6). This way it is possible to evaluate how the IPT weighted estimates compare to both the raw associations and estimates from conventional regression models which inappropriately account for endogenous variables.

Table 4 here

For the naming vocabulary score, the raw associations suggest substantive and statistically significant advantages for children whose mothers were full-time employed or part-time employed. This is evident for both years of employment and the continuous maternal employment sequences in full-time or part-time when compared to 'never worked'. Similarly, but less pronounced, increasing or decreasing work hours across the child's age is both positively associated with better naming vocabulary scores. However, the degree of stability in maternal employment, as measured by turbulence and complexity index, was not related to the naming vocabulary score. While children whose mothers worked in most of the five years have a significantly higher vocabulary score that is similar in size to the 'always part-time' group, children whose mothers were predominantly not employed do not differ significantly from the reference category. In sum, the largest raw differences emerged between children whose mothers mostly worked and those who mostly did not, irrespective of whether there were repeated changes in

employment status or a higher diversity therein.

Adjusting for differences in covariate history using IPT weighting resulted in much smaller estimates for most of these associations as shown in column 2 of Table 4. Only the positive association between mothers' years of part-time employment and the child's naming vocabulary score remained statistically significant in these models. Therefore, much of the initial differences in the naming vocabulary score between different maternal employment histories appear to be a result of confounding. The remaining associations consistently point to a positive but not statistically significant impact of continued maternal employment on vocabulary, particularly of continued full-time employment. Most of the estimates became even smaller in the models that explicitly included time-variant covariates (as child-specific means over the observation period, column 3 of Table 4), suggesting that some portion of the (undoubtedly small) influence of maternal employment on the naming vocabulary score is indeed mediated by these factors.

Baseline covariates with a statistically significant positive association with vocabulary included being a single child at birth, an older age of the mother, and the presence of a partner with a qualification equal or beyond postsecondary qualification. For, lower maternal education and living in an urban area at birth there was a significantly negative association with the naming vocabulary score (see Table S6 in the Online Appendix). For instance, children with no siblings at birth had a naming vocabulary score that was 0.28 standard deviations higher than the score of children with one sibling. In comparison, the largest association among the maternal employment measures, the difference between 'always in full-time employment' and 'never employed', only is in the range of 0.17 standard deviations.

In the case of the picture similarities score the associations with the four maternal employment history measures were small and for the most part not statistically significant already

in the initial models without covariates (see column 4 of Table 4). The only positive associations that were statistically significant were found for years worked in full-time and the 'always full-time' maternal employment sequence. All associations were reduced by covariate adjustment through IPT weighting (column 5 of Table 4) and further decreased when explicitly including time-variant characteristics (column 6 of Table 4). A statistically significant effect of years of full-time maternal working remained in the IPT-weighted model. Again, the results suggest—if anything—a small positive impact of consistent early maternal employment on inductive reasoning ability.

In sum, the results presented here showed that early maternal employment history is associated with expressive language ability and inductive reasoning ability. At the same time, there was little evidence for a causal effect of early maternal employment history as children with very different exposure histories, but similar covariate histories showed only modest differences in the ability scores under study. Test score differences, for the most part, appear to be driven by characteristics that influence maternal employment decisions in the first five years after birth.

Discussion

The goal of this study was to investigate patterns in early maternal employment history and their association with children's vocabulary and inductive reasoning abilities around age 5. Prior research either ignored the dynamics of maternal employment or was unable to address adequately the methodological challenge of accounting for time-variant confounders when estimating the association between sequences of maternal employment and child developmental outcomes. We argued that snapshot measures of maternal employment in particular ages of the child are unable to capture fully the relation with child cognitive ability at a later age because the causal processes driving the effects of maternal employment may only unfold after sufficient duration of exposure. With our analysis, we extended the prior literature by putting an explicit focus on the dynamic

nature of early maternal employment and by using an analytical approach—IPT weighting—that was able to account for observed time-variant confounders such as family structure, maternal health, or residential community without removing their mediating effect of earlier maternal employment on our cognitive ability outcomes.

We measured maternal employment history in three different ways: number of years in full-time and part-time employment, complexity and stability of maternal employment sequences, and typical maternal employment sequences. The results suggest positive and statistically significant raw associations between maternal employment (either in full-time or part-time) and vocabulary irrespective of complexity and diversity therein. When adjusting for differences in covariate history through IPT weighting, however, the associations between our maternal employment measures and vocabulary were substantially reduced. Maternal employment was also positively associated with inductive reasoning ability in the unadjusted model but to a lesser degree than with vocabulary scores. Children's language development may therefore be more susceptible to change in contextual and family conditions than inductive reasoning ability. Again, after adjusting for covariate history only small associations between maternal employment history in its various forms and inductive reasoning ability remained. In summary, children who were comparable in terms of measured time-invariant and time-variant covariates showed only minor differences in vocabulary and inductive reasoning even if they experienced vastly different maternal employment histories.

Limitations

Our study has several caveats that should be taken into account when interpreting the results. First of all, a causal understanding of our main findings rests on the strong assumptions of positivity, correct parametric model specification, and—particularly—no confounding by

unmeasured factors. Various tests did not provide evidence for (severe) positivity violations or model misspecification (see Tables S3 to S5 in the Online Appendix). Although unmeasured confounders can never be ruled out, the rich set of covariates used in our analyses increases our confidence that the extent of bias is limited.

Second, our measurement of maternal employment history has several limitations. All our measures rely only on yearly information on maternal employment status at the time of the interview. Therefore, we may underestimate the extent of instability in employment histories (see also Adolph, Robinson, Young, & Gill-Alvarez, 2008). Furthermore, there is some uncertainty concerning the meaning of non-employment since we did not differentiate between voluntary and involuntary non-employment. Likewise, it remains unclear whether non-employed mothers engage in other activities like further education. Moreover, measuring maternal employment with exact working hours may reveal a more nuanced picture since the amount of time spent in work is an important causal pathway between maternal employment and child developmental outcomes.

Third, in our study, we only address certain aspects of cognitive development, namely vocabulary and inductive reasoning ability. Differences in maternal employment histories may be more consequential for other cognitive outcomes such as numeric ability or memory ability. Our results also should not be generalized to other dimensions of child development such as behavior problems or child health.

Fourth, although we used data from a nationally representative cohort, our analyses were limited to a subsample that excluded ethnic and religious minorities, multiple births, children born to mothers below 20 and above 39 years of age, and in which children's socio-economic background was somewhat above the national average. Therefore, the results can be generalized only to a substantial subset of the Scottish population. Moreover, we only considered average

effects of maternal employment history on cognitive outcomes in this subpopulation. There may be considerable effect heterogeneity between subgroups (e.g. by child's gender, parents' socio-economic status, or family structure) which is obscured by looking at the population average.

Lastly, the association between maternal employment history and child cognitive development may also vary between countries depending on national features such as differences in childcare provision, support for families, family structure or cultural attitudes. Cross-national comparisons (e.g. Huerta et al. 2011) on contextual conditions and child developmental outcomes may therefore be a stimulating approach for future research.

Conclusions and Implications

Under the premise to (provisionally) accept the causal assumptions outlined above, our study provides only little support for an influence of early maternal employment history on vocabulary and inductive reasoning. If anything, prolonged maternal employment appears to be slightly beneficial for the developmental outcomes at age 5. For the most part, however, assessment score differences (apparent in the larger raw associations), seem to be driven by characteristics influencing maternal employment decisions in the first five years after birth, such as maternal education and family structure. While the benefits of maternal employment for children may be limited at least when it comes to the cognitive measures under study here, we also found no evidence that mothers' participation in the labor market is harmful to child development, an important implication given universal attempts to increase the share of working mothers. While this is the case for the population level, future research may be concerned with the effect of early maternal employment histories on developmental outcomes among different subgroups.

Given that our estimated associations were even smaller when explicitly including time-variant covariates, e.g. household income or family structure, in our outcome models, they seem

to partially account for the undoubtedly low impact of maternal employment history on cognitive scores. This finding lends some support to the proposed causal pathways for a positive impact of maternal employment on child cognitive ability. It further illustrates that the conclusions of an analysis in some cases are sensitive to the inclusion of time-variant confounders and that studies using a conventional regression approach may produce biased and inconsistent causal estimates as they control away the effects of observed mediating characteristics.

Although the dynamics of contextual conditions and their influence on children's developmental outcomes are an integral part of general conceptual and theoretical frameworks, empirical analyses rarely address the methodological ramifications, particularly time-variant confounding. Our study, together with an emerging literature on neighborhood effects on child development, suggests that IPT weighting may be a useful tool for realigning empirical investigations with the existing theoretical and conceptual frameworks of child development in context. Therefore, it may be worthwhile to expand this analytical approach to other outcomes (e.g. socio-emotional development, health) and the potential influence of other contextual factors that vary over time, for instance, family structure or parental involvement.

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Tables

Table 1

Descriptive Statistics for Time-Invariant Characteristics

	M/%	SD	Min	Max
Cognitive abilities at age 5				
Naming vocabulary score	111.04	13.72	40.00	161.00
Picture similarities score	83.63	11.10	43.00	119.00
Child is male (vs. female)	0.51			
Child in special care unit or neo-natal unit	0.09			
Low birth weight (< 2.5 kg)	0.04			
Medical attention during pregnancy	0.38			
Pregnancy planned or unplanned				
Planned	0.70			
Not really planned	0.13			
Not planned at all	0.16			
Mother aged 30–39 at birth (vs. 20–29)	0.61			
Mother's education level at birth				
No qualification	0.04			
Lower secondary education	0.24			
Upper secondary education	0.22			
Vocational or postsecondary certificates or diploma	0.15			
Higher education	0.34			
Mother worked during pregnancy	0.81			
Childcare from family and friends at first interview	0.49			

Note. Statistics pertain to children who have been continuously observed until age five (n = 2,200).

Table 2

Time-Variant Characteristics as Age-Specific Sample Mean or Percentage of the Sample

	Age 1	Age 2	Age 3	Age 4	Age 5
Maternal employment status					
Full-time	0.18	0.21	0.20	0.21	0.26
Part-time	0.44	0.46	0.46	0.46	0.44
Not working	0.38	0.33	0.34	0.33	0.30
Child not in good health	0.04	0.06	0.05	0.06	0.04
Mother concerned about development	0.06	0.11	0.10	0.10	0.09
Mother not in good health	0.12	0.10	0.11	0.12	0.10
No. of siblings in household					
None	0.47	0.42	0.31	0.23	0.19
One	0.35	0.39	0.47	0.53	0.55
Two or more	0.18	0.19	0.22	0.24	0.26
Partner characteristics					
No partner	0.11	0.11	0.12	0.11	0.12
Cohabitation and lower educated	0.17	0.15	0.14	0.14	0.12
Married and lower educated	0.33	0.34	0.35	0.36	0.36
Cohabitation and higher educated	0.07	0.06	0.05	0.05	0.04
Married and higher educated	0.33	0.33	0.34	0.35	0.36
Household income (in 1,000 GBP)	23.30	24.74	25.50	26.02	26.11
Homeowner	0.76	0.76	0.76	0.76	0.77
Urban area	0.64	0.64	0.65	0.66	0.65
SIMD quintile	2.77	2.76	2.73	2.72	2.72
Cumulative no. of residential moves	0.10	0.24	0.37	0.46	0.54

Note. Statistics pertain to children who have been continuously observed until age five (n = 2,200). GBP = Pound Sterling. SIMD = Scottish Index of Multiple Deprivation.

Table 3

Maternal Employment Sequences from Ages 1 to 5

	M/%	SD	Min	Max
No. of years in full-time and part-time employment				
Full-time	1.06	1.68	0.00	5.00
Part-time	2.25	1.89	0.00	5.00
No. of changes in maternal employment status				
None	0.41			
One	0.26			
Two	0.22			
Three or more	0.11			
Sequence pattern				
Never worked	0.16			
Always worked full-time	0.09			
Always worked part-time	0.17			
Increasing work hours	0.19			
Decreasing work hours	0.09			
Unstable: mostly not working	0.07			
Unstable: mostly working	0.25			
Sequence turbulence	2.16	1.11	1.00	4.75
Sequence complexity index	0.29	0.28	0.00	0.98

Note. Statistics pertain to children who have been continuously observed until age five (n = 2,200).

Table 4

Estimated Association Between Maternal Employment Sequences and Standardized Ability Scores (with Robust Standard Errors in Parentheses)

	Naming vocabulary			Picture similarities		
	No controls	IPTW & Baseline	Baseline & Time-variant	No controls	IPTW & Baseline	Baseline & Time-variant
A: Years worked						
Full-time	.079***	.030	.004	.041**	.035*	.014
	(.015)	(.017)	(.017)	(.014)	(.018)	(.017)
Part-time	.077***	.037*	.016	.022	.019	.005
	(.014)	(.016)	(.015)	(.013)	(.017)	(.016)
B: Sequence turbulence	-.005	-.036	-.020	-.003	-.004	-.005
	(.019)	(.019)	(.018)	(.019)	(.021)	(.019)
C: Sequence complexity index	.024	-.118	-.055	.035	.018	.014
	(.077)	(.078)	(.074)	(.077)	(.083)	(.078)
D: Sequence pattern (Ref. never worked)						
Always full-time	.434***	.174	.076	.176*	.141	.047
	(.092)	(.101)	(.098)	(.087)	(.106)	(.102)
Always part-time	.293***	.089	.006	.075	.076	.008
	(.082)	(.089)	(.087)	(.076)	(.096)	(.090)
Increasing work hours	.206**	.018	-.046	.103	.088	.017
	(.079)	(.080)	(.078)	(.077)	(.088)	(.083)
Decreasing work hours	.255**	.010	.015	.021	.031	-.022
	(.097)	(.103)	(.098)	(.092)	(.108)	(.102)
Unstable						
mostly not working	-.101	-.187	-.179	-.062	-.018	-.055
	(.098)	(.101)	(.095)	(.106)	(.117)	(.106)
mostly working	.294***	.030	.004	.109	.087	.019
	(.076)	(.084)	(.080)	(.072)	(.092)	(.087)

Note. Statistics pertain to children who have been continuously observed until age five (n = 2,200). All models weighted by sampling weight and stabilized attrition weight to correct for nonrandom loss to follow-up. Models A-D refer to different models testing the effect of alternative measures of maternal employment on naming vocabulary and picture similarities. IPTW = inverse probability of treatment weighting.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figures

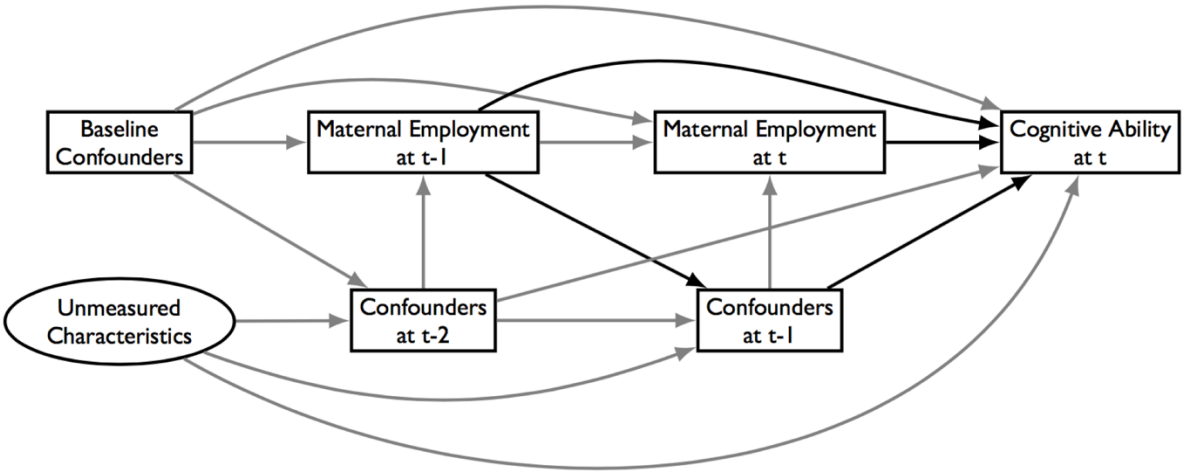

Figure 1. Hypothesized causal relations in the population between cognitive ability, maternal employment, and covariates for maternal employment at two exemplary time points.

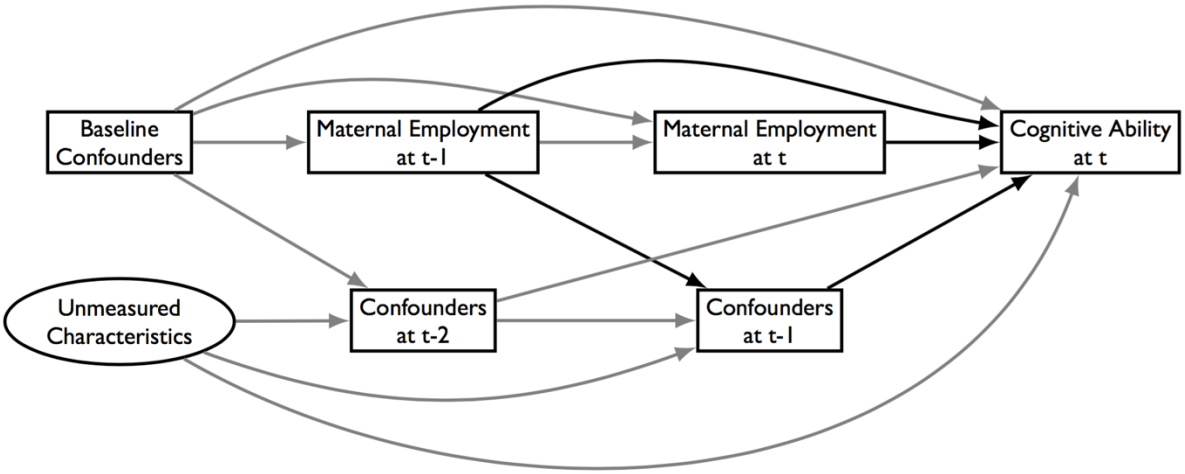

Figure 2. Removal of time-variant confounding in the weighted pseudo-population.