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MATERNITY LEAVE, EARLY MATERNAL EMPLOYMENT AND CHILD HEALTH AND DEVELOPMENT IN THE US*

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This paper uses data from the National Longitudinal Survey of Youth to explore links between mothers' returns to work within 12 weeks of giving birth and health and developmental outcomes for their children. OLS models and propensity score matching methods are utilised to account for selection bias. Considerable associations between early returns to work and children's outcomes are found suggesting causal relationships between early returns to work and reductions in breastfeeding and immunisations, as well as increases in externalising behaviour problems. These results are generally stronger for mothers who return to work full-time within 12 weeks of giving birth.

New mothers in the US return to work very quickly by international standards. A third of new mothers in the US return to work within 3 months of giving birth (Klerman and Leibowitz, 1990, 1994, 1999; Smith and Bachu, 1999). These figures stand in sharp contrast to figures for other advanced industrialised nations. In Germany, Sweden and the UK, for instance, only about 5% of new mothers return to work so quickly (Gustafsson *et al.*, 1996).

The fact that US mothers return to work so quickly is a potential concern if early returns to work result in negative outcomes for their children. Staying home for at least 6 weeks after delivery (8 weeks if the delivery was by Caesarean section) allows new mothers time to recover from birth, facilitates breastfeeding and may help prevent some adverse health consequences for the mother and child. Indeed, in many countries, maternity leave laws specifically prohibit mothers from working until at least 6 weeks have elapsed. And, in most industrialised countries, maternity leave laws provide for an extended period of job-protected leave, at least a portion of which is paid.

The situation in the US is very different. In the US, the primary source of maternity leave coverage has been employer policies, whether established voluntarily or as part of collective bargaining agreements. Under these employer policies, a new mother can typically stay home for up to 6 weeks, as long as she has the available leave time (e.g., through vacation, sick days, and/or temporary disability coverage) and obtains a doctor's note. In some instances, a longer period of leave is permitted but rarely with pay.

Beginning in the early 1970s, some US states passed maternity leave laws requiring firms to provide job-protected leaves to at least some employees. The most generous of these laws provide 18 weeks of leave; the least generous, only 4 weeks of leave. US policy was somewhat formalised in 1993, with the passage of the federal Family and Medical Leave Act (FMLA). Although the FMLA is limited

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by international standards, in that it does not cover all mothers, provides only 12 weeks of leave and makes no provision for paid leave (Kamerman, 2000; Waldfogel, 2001*a*,*b*), it nevertheless represented an important expansion of coverage in the US context by providing roughly 50% of employed women with a period of job protected leave.

Whether children would benefit if their mothers had the option to stay home for 12 or more weeks after their births, rather than returning in the first 12 weeks, is not clear. In the US context, there is considerable evidence that child developmental outcomes are generally better if mothers do not work, or do not work full-time, in the first year of life (see, most recently, Brooks-Gunn *et al.* (2002) and Ruhm (2004) on early maternal employment and the NICHD Early Child Care Research Network (2003) on early child care). However, the evidence as to timing within the first year of life is much weaker. Relatively few studies have examined the effects of returns within the first 3 months (as distinct from any returns in the first year of life) and the results have not pointed to a consistent pattern of effects for the cognitive outcomes that have been studied (Baydar and Brooks-Gunn, 1991; Han *et al.*, 2001; Brooks-Gunn *et al.*, 2002). Part of the challenge in estimating the effects of returning within the first 12 weeks versus thereafter is that women who return in the first 12 weeks may be a select group. For these reasons, prior research has not established convincingly causal connections between early returns to work and poorer child outcomes.

In this paper, we use data from the National Longitudinal Survey of Youth (NLSY) to investigate links between mothers' returns to work within 12 weeks of giving birth and health and developmental outcomes for their children. Specifically, we examine whether a mother's return to work within 12 weeks of giving birth is associated with adverse effects for the child in terms of regular medical checkups and breastfeeding in the first year of life, the receipt of all DPT (diptheria, pertussis, and tetanus)/Oral Polio immunisations (in approximately the first 18 months of life) and cognitive and behavioural assessment scores at age 3 or 4. We also estimate the extent to which *full-time* returns within the first 12 weeks may be more detrimental for children. To address the problem of selection bias, we utilise propensity score matching strategies in addition to ordinary least squares regressions (OLS).

To preview our results briefly, we find considerable associations between early returns to work and many of the outcome measures using OLS models. Our propensity score estimates are similar, suggesting causal relationships between early returns to work and reductions in breastfeeding and immunisations, and increases in externalising behaviour problems, among children whose mothers worked pre-birth. These results are generally stronger for mothers who returned full-time. Furthermore, our propensity score estimates indicate that children whose mothers returned full-time are also at risk of reduced well-baby care in the first year of life. These results suggest a causal link between early maternal employment and child outcomes. They also imply that longer periods of maternity leave could enhance children's health and development.

1. Background

Labour force participation among US women with young children has increased dramatically over the last three decades. In 1998, 62% of women with children under three were employed, up from 34% in 1975. Women with infants are also working in much greater numbers than in the past. In 1998, 58% of women with a child under the age of one were participating in the labour market, compared to only 30% in 1975.[1]

Women in the US return to work much earlier after giving birth than women in other countries. Although differences in maternity leave usage may reflect differences in preferences, social norms, or opportunities and constraints, family leave policies, which differ so dramatically between the US and other countries, are also likely to play a role. In the following Sections, we briefly review maternity leave provisions in the US, prior literature on relationships between leave coverage and women's leave-taking and lengths of leave, and what is known about whether increased leave coverage, leave-taking and leave lengths are associated with improved outcomes for children.

1.1. *Maternity Leave Provisions in the US*

Prior to 1993, whether a woman had maternity leave coverage depended on the employer for which she worked and the state in which she resided. Some employers provided a job-protected maternity leave as a matter of company policy or as the result of a collective bargaining agreement with a union. Additionally, several states enacted laws requiring at least some employers to offer job-protected maternity leaves of between 4 and 18 weeks to employees who met specific qualifying conditions (Waldfogel, 1999*a*).

The FMLA, which came into effect in 1993, for the first time provided the right, under federal law, to a job-protected maternity leave for qualifying employees (i.e. those who work for an employer with 50 or more employees and who have worked at least 1,250 hours for that employer in the prior year). The FMLA increased women's access to maternity leave coverage in the medium-sized and large firms to which it applies (Waldfogel, 1999*a*). However, only about 60% of private sector employees work in firms that are affected by the FMLA and only about 45% qualify for coverage (Commission on Family and Medical Leave, 1996; Cantor *et al.*, 2001). Thus, for roughly half of working women, coverage is still determined by company policy rather than state or federal law. Additionally, the FMLA does not guarantee paid leave and is limited in the length of leave it provides – only 12 weeks. This stands in sharp contrast to the provisions in other advanced industrialised nations which provide an average of 10 months of maternity leave, often followed by a period of parental or child-rearing leave (Waldfogel, 2001*b*).

In summary, the FMLA has increased maternity leave coverage for women but also leaves many women uncovered, provides only unpaid leave and provides a short period of leave. For these reasons, many analysts in the US have called for

[1] See Waldfogel *et al.* (2002).

extending leave policies to cover all working women, provide paid leave and provide a longer period of leave (Smolensky and Gootman, 2003).

1.2. *Maternity Leave Coverage and Leave-taking*

Current research suggests that the FMLA has had less of an impact on leave usage than leave coverage (Klerman and Leibowitz, 1998*b*; Ross, 1998; Waldfogel, 1999*b*; Han and Waldfogel, 2003). This result makes sense, given that its effects on leave usage should depend on whether the leave is paid and also on a worker's preferences, the length of leave provided under the law and the advantages to a worker of returning to her prior job, as compared to starting a new one (Klerman and Leibowitz, 1998*b*).

A fair amount of empirical evidence from the US indicates that employer-provided maternity leave coverage strongly influences women's return to work decisions, particularly in regard to increasing the likelihood that a woman returns to her pre-birth job (Hofferth, 1996; Glass and Riley, 1998; Waldfogel, 1998). There is also evidence that maternity leave legislation is associated with somewhat longer leave-taking (Joesch, 1997; Klerman and Leibowitz, 1998*a,b*; Ross, 1998; Waldfogel, 1999*b*; Han and Waldfogel, 2003).[2] Yet, the relationship between maternity leave coverage and leave length may not be linear (for example, maternity leave coverage may increase leave length up to a particular threshold, but decrease leave length after that point) because maternity leave policies have maximum leave allotments by which a woman must return if she wishes to keep her job. Along these lines, Berger and Waldfogel (2004), using NLSY data from 1988 to 1996, find that women in pre-birth jobs with maternity leave coverage are less likely to return in the first 12 weeks post-birth (the time allotted by the FMLA) than women without coverage but return more quickly after the 12 week period has elapsed.

Taken together, the empirical evidence for the US suggests that the net effect of state and federal family leave statutes should be to increase leave lengths up to the maximum allotted length under the policy. This implies that, given that the FMLA allows only 12 weeks of maternity leave (and most state laws and company policies provide less), women who are covered by the FMLA are likely to return by 12 weeks, unless they work for firms that have more generous policies.

1.3. *Leave Coverage, Maternal Employment and Child Health and Development*

By allowing mothers to stay home and provide care during the first months of a child's life, maternity leave coverage may be expected to result in improved health outcomes for working women and their infants. And, while no research to date has been conducted on the health effects of maternity leave policies within the US, research on maternity leave policies across countries has, indeed, found that longer leave periods are associated with improved health outcomes for children (Winegarden and Bracy, 1995; Ruhm, 2000; Tanaka, 2005).

[2] See also analyses by Ondrich *et al.* (1996, 1998) for Germany and Waldfogel (1998) and Burgess *et al.* (2002) for Britain.

By increasing leave length, one potential effect of expanded leave coverage may be to increase breastfeeding, which is associated with better health outcomes for children (Cunningham *et al.*, 1991).[3] Women who take maternity leave and return to work later are more likely to initiate breastfeeding and to continue breastfeeding for longer periods of time than women returning to work more quickly (Lindberg, 1996; Roe *et al.*, 1996).[4] Additionally, women who are home longer are likely to be in better positions to monitor their children's health and to take their children for doctor's visits (Ruhm, 2000; Tanaka, 2005).

There is also a potential connection between longer periods of maternity leave and improved developmental outcomes for children. Several US studies have found adverse effects on cognitive development or behavioural problems for children whose mothers work in the first year of their lives, and particularly those whose mothers work early and/or long hours in the first year of their lives (for recent evidence on early maternal employment and cognitive outcomes, see Brooks-Gunn *et al.* (2002) and Ruhm (2004); for recent evidence on early child care and behaviour problems, see the NICHD Early Child Care Research Network (2003)). Although the effects of early maternal employment tend to be small and are not found for all children, this literature nevertheless suggests that some children might have better developmental outcomes if their mothers had the chance to stay home, at least part-time, for a longer period of time.

In summary, it appears that longer periods of leave are associated with better health outcomes for women and infants, and could potentially lead to better developmental outcomes as well.[5] But, convincing empirical evidence regarding causal links between maternity leave, early maternal employment, and child outcomes is lacking.

2. Theoretical Framework

In this study, we take advantage of variations in women's maternity leave taking in the US to analyse whether early returns to work have an effect on child health and development outcomes. We posit that leave coverage will impact women's leave usage and, through this impact, will ultimately affect child health and development.[6]

[3] Breastfeeding is associated with better infant health on a number of dimensions. For these reasons, the American Academy of Pediatrics (1997) recommends exclusive breastfeeding for the first six months and breastfeeding alongside other feeding for at least the next six months. However, actual breastfeeding practices in the US do not meet these goals (Philipp *et al.*, 2001). Breastfeeding may also be associated with better cognitive outcomes (see for instance Moretensen *et al.*, 2002) but here the evidence is less conclusive.

[4] And one recent study finds that women in states with tougher work requirements for mothers of infants are less likely to breastfeed (Haider *et al.*, 2003).

[5] At the same time, very lengthy periods of maternity leave may have adverse effects on women's position in the labour market (Ruhm, 1998). And, if such leaves are mandated, employers may be less willing to hire women, or may reduce their wages (Waldfogel, 2001*a*).

[6] Klerman and Leibowitz (1998*b*) present a useful model for understanding the likely impact of maternity leave coverage (and extensions) on women's decisions about taking leave and returning to work (Klerman and Leibowitz, 1998*a*, 1999). Their model makes clear that the impact of maternity leave coverage depends on the length of the coverage provided and also on a woman's preferences about when to return.

To the extent that expanded leave coverage leads women to stay home longer (at least up to the allotted threshold), we would expect expanded leave coverage to translate into improved outcomes for children, as mothers who are not working may have more time to engage in activities that will benefit their children's health and development during the first months of life. Such activities include ensuring the child receives preventative health care (taking the child for 'well-baby' visits and immunisations) and breastfeeding. To the extent that shortfalls in leave coverage require early returns, we would expect reductions in these activities. Indeed, previous research on early maternal employment suggests that children whose mothers spend more time at home in the first months of life may benefit in the longer-run, through having fewer behaviour problems and better language and verbal abilities, because they have the chance to develop more sensitive and responsive relationships with their mothers and/or because the quality of the care they receive at home is better than what they would have received in non-parental child care. However, it is unclear whether the 12 weeks provided by the FMLA provide an adequate 'window' for improving children's health and developmental outcomes. We therefore test whether, all else equal, children have better health and developmental outcomes when their mothers take periods of maternity leave that extend beyond 12 weeks.

3. Data and Measures

3.1. *Sample*

We use microdata from the 1987 to 2000 waves of the NLSY. This large nationally representative dataset has followed a cohort of young women and men since 1979, with interviews annually until 1994 and biennially thereafter. The NLSY gathers data on the original cohort members and also on children born to the women in the cohort.[7] Variables for this analysis are drawn from the Geographic Micro-Data file, as well as the Children and Young Adults, and Work History files.

We use data on births to the women in the sample that occurred between 1988 and 1996. We begin with births in 1988 because, starting in that year, the NLSY Employer Supplements have gathered data on the start and end dates of maternity leave periods. Thus, from 1988 forward we are able to identify exact periods of maternity leave accurately. We end with births occurring in 1996 because those were the latest births for which data were available on child outcomes up to ages 3 or 4. Because our sample excludes early births (births that occurred prior to 1988, to mothers between the ages of 14 and 22) and later births (births after 1996, to mothers age 39 or older), it is not fully representative of children born to the mothers in this cohort and is also not representative of all births in the US in these years.[8] Nevertheless, results for this sample are informative, as they represent a recent set of births to women age 23 to 38, in the context of considerable changes in state and federal legislation related to maternity leave, including the passage of the FMLA.

[7] The children included in the NLSY represent children born to the mothers in the cohort but are not a nationally representative sample of all births in the US in these years.

[8] In 1996, roughly 25% of births were to women younger than age 23; an additional 2% were to women age 39 or older, vital statistics data from US Bureau of the Census (2003, Table 85).

Each child identified in the NLSY Children and Young Adults File was linked to his or her mother's record in both the Geographic Micro-Data and Work History files. We then identified all jobs held by each mother in the year of birth. Each job was linked across the year prior to and the year after birth, in order to determine whether the job started before and ended after birth. We include only those mothers who were in jobs at some point within the three months prior to birth, because this is the group for whom maternity leave policies are relevant.[9] We identified 1,907 such births in the NLSY between 1988 and 1996.[10]

3.2. *Measure of Early Return to Work*

After identifying each job that spanned the date of birth, we constructed maternity leave periods for each mother using four sets of variables: (1) maternity leave variables from the Employer Supplement in the main NLSY file; (2) employment 'gap' data from the Work History file; (3) weeks before and after birth maternity leave started/ended variables from the Child and Young Adult file; and (4) weeks worked in the quarters before and after birth data from the main file. These variables allow us to determine the length of time a mother was away from work based on whether she took maternity leave and/or quit her job, when each leave period began and ended (for women remaining in the same job both prior to and after birth), and when each mother who returned to a different job after birth re-entered the labour market.[11]

We use this leave-length information to construct our measure of post-birth time away from work: whether the mother resumed work within 12 weeks of giving birth. We utilise 12 weeks as our threshold because it is policy relevant, since this is the leave length allowed by the FMLA. We also know from prior work with the NLSY that women who have leave coverage are less likely to return in the first 12 weeks than women who worked pre-birth but lacked leave coverage.[12]

[9] It is possible that whether a woman chooses to work, even prior to becoming pregnant, may to some extent depend upon existing maternity leave policies. However, we assume that this is not a strong determinant of employment for most women.

[10] Many of the child outcomes models are estimated on smaller samples as some of the outcomes were not measured in each year, had a large number of missing values, or only apply to a subsample of children. Sample sizes for these outcomes are: 1,678 for well baby care; 1,674 for breastfeeding; 1,620 for weeks of breast feeding; 737 for DPT/oral polio immunisations; 769 for BPI externalising behaviour problems; and, 1,064 for PPVT-R percentile scores. Immunisation items were included in the NLSY only prior to 1990, so this measure only includes births prior to that time. PPVT-R measures are included only for children age 3 and older, and the BPI is included only for children age 4 and older.

[11] Short periods of paid vacation or sick leave that are not official 'maternity leave' may be coded as time at work in the NLSY. Therefore, if a woman was coded as at work immediately after the birth but then had a period of maternity leave within the first 13 weeks after birth, we treated that period of leave as having started at the time of the birth.

[12] In additional analyses, not reported here, we also examined other return period partitions, categorising women who did not return by 12 weeks by whether they returned in 13 to 18 weeks, 19 to 25 weeks, 26 to 52 weeks, or had not returned by 52 weeks. These analyses revealed some differences between these groups (for instance, weeks of breast-feeding increase with weeks of time away from work) but did not change the overall pattern of results reported here. We also examined whether the effects of returning in 0 to 12 weeks differed from the effects of returning in 13 to 18 weeks and found that in several instances (well-baby care, number of well-baby visits, breast-feeding, duration of breast-feeding and immunisation), the effects of returning in 13 to 18 weeks were significantly or marginally significantly better.

3.3. *Child Health and Development Measures*

We constructed 7 outcome variables related to child health and development:

(1) whether a child had any 'well-baby' visits in the first year of life;
(2) the number of months (0–12) in which the child had a well-baby visit in the first year of life;[13]
(3) whether the child was ever breastfed;
(4) the number of weeks (0–52) the child was breastfed during the first year of life;
(5) whether the child received all of his or her DPT/oral polio immunisations (in the first 18 months of life);
(6) the child's score on the externalising behaviour problems subscale of the Behavioural Problems Index (BPI) at age four (0–36 potential points); and
(7) the child's total percentile score on the Peabody Picture Vocabulary Test-Revised (PPVT-R) at age three or four.

Each of these variables measures either an aspect of children's health and development or activities related to health and development. Well-baby visits and immunisations are important indicators of whether a child received preventative health care as recommended in the first 12 to 18 months of life. Variables related to breastfeeding capture another important health-related behaviour, and one that has been shown in prior research to be very sensitive to whether and when the mother is working. The BPI is a measure of behaviour problems reported by the mother, which is administered for children in the NLSY starting at age 4. We focus on externalising behaviours (such as aggressiveness, impulsivity, and defiance) because these are the ones that theory and prior research suggest would be most strongly affected by early maternal employment (see recent review by Belsky (2001)). The child's score on the PPVT-R is a widely used measure of the child's language and cognitive ability; this measure is administered for children in the NLSY starting at age 3. Both the BPI and the PPVT-R have been very commonly used in studies examining the impact of early maternal employment on behavioural and cognitive outcomes in the NLSY. Thus estimating the impact of leave-taking behaviours on these outcomes is of particular interest.

Descriptive statistics regarding early returns to work and child outcomes are displayed in Table 1. In the top row, we see that nearly 63% of the mothers in this sample of women who worked prior to the birth returned to work within 12 weeks after giving birth, and that more than half of this group (about 37% of mothers) returned full-time within 12 weeks. We also see that child outcomes tend to be slightly lower for children whose mothers returned to work within 12 weeks, and particularly when mothers returned full-time. For example, approximately 58% of sample mothers breastfed for some period of time, but this was true for only 55% of mothers who returned to work within 12 weeks and for 53% who returned full-time within this time period. This pattern is relatively consistent across all of the outcomes and suggests that, in the raw data, there are some small differences in

[13] According to the American Academy of Pediatrics (2000) guidelines, children should be seen by their doctors at least eight times in the first year of life for 'well-baby' (prevention and screening) visits.

Table 1

Returns to Work and Child Outcomes

	All mothers working pre-birth	Mothers returning in 0–12 weeks	Mothers returning full-time in 0–12 weeks
	Obs. (%)	Obs. (%)	Obs. (%)
All mothers working pre-birth (Obs. = 1,907)	1,907 (100.0)	1,191 (62.5)	707 (37.1)
	Mean (S.D.)	Mean (S.D.)	Mean (S.D.)
Any well care (Obs. = 1,678)	0.954 (0.211)	0.944 (0.231)	0.939 (0.240)
No. Months well care (0–12) (Obs. = 1,678)	3.412 (2.436)	3.316 (2.412)	3.303 (2.338)
Any breast feeding (Obs. = 1,674)	0.584 (0.493)	0.549 (0.498)	0.525 (0.500)
No. Weeks breast fed (0–52) (Obs. = 1,620)	11.088 (15.696)	9.338 (14.464)	8.128 (13.083)
All DPT/Oral polio immunisations. (Obs. = 737)	0.927 (0.261)	0.906 (0.293)	0.899 (0.302)
Externalising behaviour problems Age 4 (Obs = 769)	5.151 (3.563)	5.229 (3.580)	5.427 (3.554)
PPVT Percentile score Age 3–4 (Obs. = 1,064)	42.675 (32.898)	41.402 (32.557)	39.490 (32.501)

Note: These figures are based on data from the 1988 to 2000 Waves of the NLSY. The full sample includes 1,907 mothers who worked within 3-months of giving birth and who gave birth between 1988 and 1996.

outcomes for children whose mothers return to work quickly, particularly if they return full-time.

3.4. *Control Variables*

Clearly, the unadjusted differences in outcomes between mothers with different leave-taking behaviours are unlikely to represent valid estimates of the causal effects of early maternal employment on these outcomes because the groups of women differ on so many other characteristics as well. Thus, the analyses we perform include progressively more detailed measures of these 'confounding covariates' (i.e. background variables that predict both maternal employment and subsequent outcomes) to help identify causal effects (note that the analyses use different identification strategies so these variables play slightly different roles in each).

Specifically, we estimate four sets of models for each of the outcomes. Model 1 includes only demographic control variables: mother's age, mother's age squared, mother's education, mother's race/ethnicity, mother's marital status, mother's age-adjusted Armed Forces Qualification Test (AFQT) score (a measure of cognitive ability), other family income (not including the mother's earnings) in the previous year, parity (i.e. whether this is a first birth), low birth weight birth (i.e. whether the child weighed 5.5 pounds or less at birth), child disabled and child female.[14] Model 2 includes these demographic controls, as well as the following state level control variables: the percentage of the population in the state and year

[14] Although one might worry that low birthweight or child disability might be endogenous if mothers take leave pre-birth and that in turn affects birth outcomes, this is less of a concern in the US context where leave periods are short and relatively little leave is taken pre-birth. In our sample, the average leave length pre-birth was 2 weeks, and half the sample did not take any leave pre-birth. Thus, pre-birth leave is unlikely to have strong effects on child birth outcomes in this sample.

with health insurance coverage,[15] the mean per capita income, whether both houses of the state legislature are majority Democrat and the local unemployment rate. Model 3 adds controls for the 4 major regions of US (North East, North Central, South, and West). And, Model 4 adds state fixed effects in place of the region variables. All four models also control for the year of the birth, through year fixed effects, and include dummy variables to control for missing values in income, education, marital status, and birth weight.[16]

4. Empirical Analyses

4.1. *Do Linear Regression Estimates Suggest that Returns to Work Within 12 Weeks are Associated with Poorer Child Health and Development?*

We begin by using linear regressions estimated via ordinary least squares (OLS) to examine the effects of returning to work within 12 weeks on each of the 7 outcome variables.[17] The linear regression model (where observation subscripts have been omitted for simplicity) can be written as:

$$child\ outcomes = \beta_0 + \tau return12 + \Sigma\beta_k x_k + u, \quad (1)$$

where *return12* is a dummy variable for whether the mother returned to work in 12 weeks and the x_k ($k = 1,\ldots K$) stand for our measured covariates. The coefficient on *return12* can be interpreted causally only if several assumptions are true. First, the 'selection on observables' (Heckman, 1979) assumption must be satisfied. This requires that we have included in our model all variables that predict both leave-taking and children's outcomes. These variables are often called 'confounding covariates' because if they are not appropriately controlled for, their effect on the outcome is confounded with the effect of the 'causing variable' of interest (Angrist and Krueger, 1999),[18] in this case, leave-taking. This is an untestable assumption.

The second assumption is that the model is 'true' (or acts as a good enough approximation to the truth). This is problematic because, while assumptions like linearity and additivity are relatively straightforward to check if we have only one or two predictors, they become nearly impossible to verify when we have ten or twenty predictors.[19] Moreover, when there is lack of overlap in covariate distributions across leave-taking groups, regression models are forced to extrapolate models over portions of the overlap where there is no support. There is no way of directly checking whether such extrapolations are appropriate.

[15] This variable was drawn from: US Census Bureau. Historical Health Insurance Tables. Table HI-4. Health Insurance Coverage Status and Type of Coverage by State All People: 1987 to 2001. Available at: http://www.census.gov/hhes/hlthins/historic/hihistt4.html.

[16] As a robustness check, we re-estimated the models dropping the cases with missing values. The results were not substantially different from those reported here.

[17] We show results from OLS models for all outcomes. To check the validity of our results, we also estimated probit models for the dichotomous outcomes and the results (not shown) were consistent with those presented here.

[18] We borrow the term 'causing variable' from Angrist and Krueger (1999) who use it to refer to the variable whose effect is of primary interest. In medical contexts, this would be referred to as the 'treatment variable'.

[19] Note, for instance, that checking pair-wise linearity between the outcome and each predictor is not sufficient for judging linearity in this context.

Finally, it is worth noting that in the absence of explicitly including interaction terms between the causing variable (leave-taking) and the predictors, regression models implicitly assume constant (or additive) treatment effects across all the different types of people in the sample. This assumption is often inappropriate.

If the assumptions for linear regression are met, however, it can be a simple and very efficient means of estimating causal effects. The results of these models are presented in panel (*a*) of Table 2. Here, we find several associations between early returns to work and child outcomes. In particular, early returns to work are associated with decreased probabilities of a child receiving well-baby care, being breastfed, and receiving all DPT/Oral Polio immunisations. Returning to work within 12 weeks is also associated with shorter periods of breastfeeding. These results are robust to the various model specifications. In terms of the magnitude of the effects, we see that, after controlling for demographic and state characteristics, as well as state fixed effects (Model 4), children whose mothers returned to work within 12 weeks are 2.4 percentage points less likely to receive well-baby care, 7.5 percentage points less likely to be breastfed and 3.4 percentage points less likely to

Table 2

Effects of Returning to Work by 12 Weeks on Child Outcomes (All Mothers Working Pre-Birth)

	(*a*) OLS Models				(*b*) Propensity Score Models			
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Any well care	−0.026*	−0.024*	−0.024*	−0.024*	−0.025*	−0.030*	−0.017	−0.019
(Obs. = 1,678)	(0.010)	(0.010)	(0.010)	(0.010)	(0.012)	(0.012)	(0.013)	(0.013)
No. Months well care	−0.203^{+}	−0.126	−0.115	−0.054	−0.225	−0.091	−0.217	−0.135
(Obs. = 1,678)	(0.121)	(0.120)	(0.120)	(0.122)	(0.150)	(0.145)	(0.147)	(0.145)
Any breast feeding	−0.076**	−0.078**	−0.078**	−0.075**	−0.107**	−0.047	−0.092**	−0.079**
(Obs. = 1,674)	(0.024)	(0.024)	(0.023)	(0.024)	(0.030)	(0.029)	(0.028)	(0.028)
No. Weeks breast fed	−4.677**	−4.683**	−4.645**	−4.482**	−3.771**	−4.383**	−5.185**	−5.224**
(Obs. = 1,620)	(0.798)	(0.797)	(0.783)	(0.801)	(0.982)	(1.008)	(1.077)	(1.088)
All DPT/Oral polio	−0.046**	−0.042*	−0.040*	−0.034^{+}	−0.047^{+}	−0.052*	−0.046^{+}	−0.041^{+}
immss. (Obs. = 737)	(0.018)	(0.018)	(0.018)	(0.018)	(0.024)	(0.021)	(0.025)	(0.024)
Externalising BPI Age 4	0.317	0.303	0.319	0.322	0.338	0.655^{+}	0.709*	0.773*
(Obs. = 769)	(0.271)	(0.275)	(0.276)	(0.285)	(0.319)	(0.344)	(0.337)	(0.328)
PPVT Pctile. score	−1.637	−1.374	−1.279	−1.007	−2.046	−1.918	−0.611	−0.674
Age 3-4 (Obs. = 1,064)	(1.595)	(1.609)	(1.612)	(1.656)	(2.330)	(2.039)	(1.958)	(1.971)
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes
State controls		yes	yes	yes		yes	yes	yes
Region controls			yes				yes	
State Fixed Effects				yes				yes

Note: Demographic controls include: mother's age, mother's age squared, mother's education, mother's race/ethnicity, mother's marital status, mother's AFQT score, other family income in the previous year, parity, low birth weight birth, child disabled and child female. State controls include: percent with health coverage, mean per capita income, whether both houses of the state legislature are majority Democrat, and the local unemployment rate. All models also control for year and include dummy variables to control for missing values in income, education, marital status, and birth weight. Coefficients presented. Robust standard errors in parentheses. Standard errors are corrected for intra-cluster correlation in the error terms due to multiple observations from the same individual. +marginally significant at 10%; *significant at 5%; **significant at 1%.

receive all of their immunisations (marginally significant). They also breastfeed for about 4.5 fewer weeks. The breastfeeding effects are particularly noteworthy: given that, on average, 58.4% of women in this sample breastfeed, and that they breastfeed for about 11.1 weeks, these results suggest that women who return within 12 weeks are about 13% less likely to breastfeed and breastfeed for 41% fewer weeks.

As previously stated, however, we are concerned that selection may bias these estimates. Children of women who return to work within the first 12 weeks may have poorer outcomes for reasons other than their mothers' early returns to work, as women who return to work early may be different from other mothers in ways that are correlated both with their return to work decisions and their children's outcomes. The models that we have presented thus far address these selection issues only through the inclusion of a progressively more detailed set of confounding covariates. However, we are only able to control for those factors observed in our dataset and, if women who work early differ from others in ways that are not observed, these estimates could still be biased. Moreover, OLS estimates can be heavily dependent on functional form assumptions. For both of these reasons, we do not want to rely exclusively on OLS estimates to estimate causal relationships between early returns to work and child outcomes. Therefore, we consider an identification strategy intended to estimate causal effects of early returns on child outcomes in the presence of selection bias: propensity score matching. These results are presented below.[20]

4.2. *Do Propensity Score Models Suggest Causal Relationships Between Early Returns to Work and Children's Health and Development?*

One method for estimating causal effects in the presence of selection bias is propensity score matching (Rosenbaum and Rubin, 1983, 1984, 1985). These models rely on the same structural assumption for causal inference as linear regression models do, namely the selection on observables assumption. There are several advantages, however, to using propensity score matching rather than linear regression. We outline these advantages, below, after first describing the propensity score matching procedure.

The goal of propensity score matching is to find a group among our comparison population (those who stayed home at least 12 weeks, including those we do not observe returning to work at all) that looks as similar as possible to our treatment group (those who went back to work within 12 weeks). To achieve this, we must first estimate, for each individual, a quantity called the propensity score, which is

[20] In other analyses not reported here, we also tried two other strategies for estimating causal effects in the presence of selection bias: instrumental variables (IV) models, and family fixed effects (sibling) models. However, the results from both were inconclusive. In the case of the IV models, although we had a relatively strong instrument (percentage of female workforce unionised by state and year), as is often the case with such models the standard errors in the second stage were too large to allow us to draw any firm conclusions. In the case of the family fixed effects models, because these models are limited to families with siblings and (effectively) to cases where the mother returned in a different timeframe for different children, the analyses were hampered by small sample sizes and large standard errors.

the conditional probability of being in the treatment group (going back to work within 12 weeks) given our measured covariates:

$$\Pr(return12 = 1|x_1, \ldots, x_k),$$

and which can be estimated using standard logistic or probit regression models. We then use the estimated propensity scores to create a matched control group for our treatment group from among our comparison population. So, for each treatment group member we find the comparison member with the closest propensity score. We use matching with replacement (i.e. comparison units can be used as matches multiple times if necessary) because it has been shown to reduce bias relative to matching without replacement (Dehejia and Wahba, 2002). Comparison units that are not matched are discarded from subsequent analyses. Our analyses were performed using a shareware program available for download for Stata8 called 'psmatch2' (Leuven and Sianesi, http://fmwww.bc.edu/RePec/bocode/p).

Next we compare covariate distributions across our matched groups to ensure that adequate balance has been obtained. When we do so in our samples, we find that our treatment and comparison groups, though initially quite different, look quite similar after matching (and discarding the unmatched), with no significant differences in measured background characteristics (confounding covariates) remaining between the two groups.[21]

In theory, if adequate balance on all covariates is obtained, differences in mean outcomes between the treatment group and matched comparison group can be used as treatment effect estimates. In practice, running regressions in these matched groups can increase efficiency and help to control for any bias not fully controlled for by the matching (Rubin, 1973, 1979; Rubin and Thomas, 2000).[22] Of course, as with standard (unmatched) linear regression, if we have failed to control for an important confounding covariate our estimates can be biased.

The main advantage of propensity score matching over regression is that it restricts inferences to samples for whom we have overlap in covariate distributions across 'treatment groups' (levels of the causing variable, leave-taking), thereby avoiding unwarranted model extrapolations. Although models are used to estimate propensity scores, since these scores are just a means to an end for creating greater balance, and this balance can be checked directly using diagnostics, and since these scores are used just for matching (not put directly into a model or used as weights) inferences tend to be fairly robust to misspecification of the model used for estimating propensity scores (Drake, 1993). Moreover, since the model selection process takes place in the absence of outcome variables, it can be considered a more 'honest' process because models (propensity score models) can be

[21] Note that many of the models had to be adjusted several times in order to achieve what we considered to be very good balance. This iterative process of model fitting and diagnoses was performed in the absence of examining treatment effect estimates.

[22] We do not have to worry about model extrapolations in this case because, if the matching has succeeded, the distributions of our covariates are the same across groups. Moreover the harmful effects of model misspecification on treatment effect estimation are greatly reduced in situations with sufficient overlap.

selected based on the balance they create without any glimpses of what the corresponding treatment effects estimate is.

An additional advantage of propensity score matching is that it targets inferences on a specific subpopulation defined by levels of the causing variable. For instance, in our analyses, because we leave the group of mothers returning to work early intact and then find matches for them from among the comparison group, we are estimating treatment effects that apply specifically to this group of early-returning mothers (and their children). This is one group that might plausibly be affected by a policy change. To the extent that mothers (and their children) who return later have different characteristics and might experience effects of changes in leave-taking behaviour, this effect (sometimes called the effect of the treatment on the treated) may differ from effects estimated for different subpopulations, such as effects for the those who return later, or effects overall. Given that we are considering specifically policies that might help mothers currently going back to work before 12 weeks to be able to extend their leave longer, propensity score matching lets us focus on average effects of leave-taking specifically for this subpopulation of women.

In panel (*b*) of Table 2, we provide results obtained through regression performed on propensity score matched samples.[23] We see that all of the effects of early returns function in expected directions and that many are statistically significant. For example, in Model 4, which includes demographic and state controls, as well as state fixed effects, we find statistically significant effects of early returns to work on whether a child is breastfed, the length of time the child is breastfed and the number of externalising behavioural problems a child has at age 4, as well as a marginally significant decrease in the likelihood that a child has received all DPT/Oral Polio immunisations. Children whose mothers return to work within 12 weeks are 7.9 percentage points less likely to be breastfed, are breastfed for about 5.2 fewer weeks, are 4.1 percentage points less likely to receive all DPT/Oral Polio immunisations and have 0.8 more externalising behaviour problems. These results provide support for a causal link between leave length and child outcomes such as breastfeeding, immunisations, and externalising behaviour problems.

4.3. *Are Full-time Early Returns to Work Associated with Poorer Child Outcomes?*

To the extent that early returns to work may result in negative health and development outcomes for children, we may expect that the negative effects will be larger when mothers spend greater amounts of time at work. Therefore, we next present estimates of the effects of full-time returns to work within 12 weeks. For these analyses, we drop women who returned part-time within 12 weeks (as well as those who returned within 12 weeks but for whom we do not have data regarding whether they returned full or part-time). Thus, we are comparing women

[23] Sample sizes for these regressions are smaller than in the OLS models, because not all women in the comparison group are used, only those who most closely match the women in the treatment group. Sample sizes range from a low of 611 for immunisations to a high of 1,485 for well-baby visits in the analyses for all mothers and from a low of 360 to a high of 955 in the analyses for mothers returning full-time.

returning full-time within 12 weeks to those not returning within 12 weeks.[24] These results are presented in Table 3.

Again, we begin with OLS estimates. Looking at panel (*a*) of Table 3, we see that the effects of returning full-time within 12 weeks are consistent with and, generally, somewhat stronger than the effects of any return to work within 12 weeks (shown in panel (*a*) of Table 2). In these OLS models, children whose mothers return full-time within 12 weeks have poorer outcomes in terms of any well care, breastfeeding and externalising behavioural problems. There is also some indication that they are less likely to receive all of their DPT/Oral Polio immunisations. These differences are particularly striking for breastfeeding and externalising behaviour problems. The earlier OLS models indicated that women returning within 12 weeks are approximately 8 percentage points (13%) less likely to breastfeed and breastfeed for 4 to 5 fewer weeks but that their children are not significantly more likely to have behavioural problems. By comparison, the OLS estimates for women returning full-time within 12 weeks indicate that they are about 10 percentage points (17%) less likely to breastfeed and breastfeed for about 6 fewer weeks. Additionally, their children have 0.6 more behavioural problems. Thus, we see larger negative effects of early returns

Table 3

Effects of Returning to Work by 12 Weeks on Child Outcomes (Mothers Returning Full-Time)

	(*a*) OLS Models				(*b*) Propensity Score Models			
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Any well care	−0.030*	−0.026*	−0.025*	−0.027*	−0.043**	−0.025	−0.027^{+}	−0.027^{+}
(Obs. = 1,253)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.016)	(0.014)	(0.015)
No. Months well care	−0.237^{+}	−0.121	−0.107	−0.052	−0.298^{+}	0.113	−0.213	−0.188
(Obs. = 1,253)	(0.133)	(0.132)	(0.132)	(0.134)	(0.167)	(0.164)	(0.161)	(0.158)
Any breast feeding	−0.105**	−0.106**	−0.108**	−0.104**	−0.122**	−0.088**	−0.135**	−0.141**
(Obs. = 1,250)	(0.027)	(0.028)	(0.027)	(0.028)	(0.032)	(0.033)	(0.033)	(0.033)
No. Weeks breast fed	−6.123**	−6.165**	−6.234**	−6.010**	−4.995**	−6.720**	−5.404**	−5.283**
(Obs. = 1,215)	(0.865)	(0.866)	(0.855)	(0.882)	(1.048)	(1.291)	(1.127)	(1.147)
All DPT/Oral polio	−0.047*	−0.042^{+}	−0.038^{+}	−0.035	−0.020	−0.025	−0.030	−0.038
immunisations.	(0.022)	(0.022)	(0.022)	(0.023)	(0.028)	(0.030)	(0.029)	(0.028)
(Obs. = 539)								
Externalising BPI	0.617*	0.616*	0.643*	0.576^{+}	0.807*	0.168	0.696*	0.478
Age 4 (Obs. = 574)	(0.304)	(0.313)	(0.315)	(0.333)	(0.396)	(0.398)	(0.343)	(0.370)
PPVT Pctile. score	−2.631	−2.162	−2.148	−2.409	−2.633	−2.335	−0.070	0.580
Age 3-4 (Obs. = 809)	(1.780)	(1.811)	(1.822)	(1.882)	(2.149)	(2.189)	(2.248)	(2.221)
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes
State controls			yes	yes			yes	yes
Region controls			yes				yes	
State Fixed Effects				yes				yes

Note. See Table 2.

[24] In additional analyses, we also compared outcomes for children whose mothers worked full-time by 12 weeks with those whose mothers worked part-time by 12 weeks and found, in results not shown but available upon request, that children whose mothers worked full-time by 12 weeks had significantly lower levels of breast-feeding, significantly fewer weeks breast-fed and marginally significantly more externalising behaviour problems ($p < 0.10$).

to work when these returns are full-time. Again, however, these results may be biased by model misspecification. We therefore present our propensity score results next.

Once again, our propensity score models (Table 3, panel (*b*) suggest causal links between early returns to work and children's health and development variables. In particular, we see consistent evidence that early full-time returns are linked to reductions in breastfeeding. There is also some, although more limited, evidence that early full-time returns are related to reductions in well-baby care and increases in children's externalising behaviour problems. As with our OLS models, these results are consistent with and, often, somewhat stronger among women returning full-time than among all women returning within 12 weeks. For example, our propensity score results for all women returning within 12 weeks suggested that they are 8 percentage points less likely to breastfeed (Table 2, Model 4), while these results suggest that women who return full-time are 14 percentage points less likely to breastfeed (Table 3, Model 4). While the significant effects on externalising behaviour problems disappear for our full-time model once all covariates are included, the sample size for these analyses has also decreased considerably (to only 574 observations). Thus, the drop in magnitude may simply reflect sampling variability. In general, these results point to a causal link between early returns to work and some poorer health and developmental outcomes for children, particularly when mothers return to work full-time within 12 weeks. However, the extent to which there is an effect of early full-time returns on externalising behaviour problems is less clear.

5. Conclusions

New mothers in the US are less likely than mothers in other countries to have the right to a job-protected maternity leave and, when they do, the leave tends to be shorter and is more likely to be unpaid. Perhaps as a result, new mothers in the US return to work very quickly. Previous research indicates that maternity leave coverage is related to the length of time women spend at home after birth. US women whose jobs provide leave coverage are less likely to return to work in the first 12 weeks after giving birth – the length of federally provided leave under the FMLA – but then return more quickly thereafter (Berger and Waldfogel, 2004). In our sample, nearly 63% of women who are working pre-birth return to work within 12 weeks of giving birth, and half of that group, 37% of women working pre-birth, return full-time.

Do the short periods of maternity leave among US mothers affect child health and development? The evidence in this paper suggests that the answer may well be yes. In OLS models estimating the effects of returning to work within 12 weeks on various measures of child health and development, we find consistent evidence of associations between early returns to work and many of the measures. Children whose mothers return to work early are less likely to receive regular medical checkups and breastfeeding in the first year of life, as well as to have all of their DPT/Oral Polio immunisations (in approximately the first 18 months of life). These impacts are stronger when mothers return to work full-time within the first

12 weeks. Furthermore, children whose mothers return full-time within 12 weeks are more likely to have externalising behaviour problems at age 4.

However, OLS regressions may be inappropriate for estimating causal effects given that mothers who take leave of a certain length may be a select group. We therefore attempt to address this selection bias through propensity score methods. Our propensity score estimates are generally consistent with the OLS results and are suggestive of causal relationships between early returns and several of the health and development outcomes, particularly breastfeeding, immunisations, and externalising behaviour problems. Additionally, the propensity score results are generally stronger for mothers returning full-time within 12 weeks. And, these mothers' children may also be at risk of reduced well-baby care.

We do not know the long-run impact of the effects we have estimated here. It would also be useful, as data become available, to repeat these analyses for a more contemporary, and representative, cohort of children. However, although further research on this topic is needed, the evidence provided here is sufficient to raise a 'red flag' concerning policies that unnecessarily hasten women's returns to work after giving birth. Thus, given this evidence and prior evidence (Smolensky and Gootman, 2003), it may be prudent for US policy makers to reconsider TANF policies that require women to return to work within three months after giving birth and to explore options to extend parental leave coverage (e.g., to cover more new parents, provide some mechanism for paid leave, and grant a longer period of leave).

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