Maternity Leave, Early Maternal Employment, and Child Outcomes in the U.S.

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Abstract

Prior studies in the U.S. have shown that women who have access to parental leave are less likely than other new mothers to return to work within the first 12 weeks after a birth. Additionally, there is evidence that children whose mothers stay home longer in the first year of life tend to have better developmental outcomes. However, selection bias is always a concern in these studies because the women who stay at home longer may differ in important ways from those who return to work earlier. Therefore, causal connections between maternity leave length and child outcomes have not been demonstrated convincingly. 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. In addition to ordinary least squares (OLS) models, instrumental variables (IV) and propensity scores matching methods are utilized to account for selection bias. Considerable associations between early returns to work and children's outcomes are found using OLS models, but IV results are inconclusive. Propensity scores estimates, however, suggest causal relationships between early returns to work and reductions in breastfeeding and immunizations and increases in externalizing behavior problems. Furthermore, these results are generally stronger for mothers who return to work full-time within 12 weeks of giving birth. Children of these mothers also appear to be at elevated risk for reduced well-baby care.

I. Introduction

New mothers in the United States return to work very quickly by international standards. A third of new mothers in the U.S. 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 industrialized nations. In Germany, Sweden, and the United Kingdom, for instance, only about 5 percent of new mothers return to work so quickly (Gustafsson et al., 1996).

The fact that U.S. mothers return to work so quickly is a potential concern if early returns to work result in negative outcomes for their children. It has long been a rule of thumb among physicians that new mothers should remain home for at least 6 weeks after delivery (8 weeks if the delivery was by Caesarean section) to allow them time to recover from birth, to facilitate breastfeeding, and to avoid 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 industrialized countries, maternity leave laws provide for an extended period of job-protected leave, at least a proportion of which is paid. Across the U.S.'s peer nations in the Organization for Economic Cooperation and Development (OECD), maternity leave laws provide for an average of 10 months of job-protected leave and for at least some degree of wage replacement during the leave period (Waldfogel, 2001b).

The situation in the U.S. is very different. In the U.S., 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 U.S. 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. U.S. policy was somewhat formalized in 1993, with the passage of the federal Family and Medical Leave Act (FMLA). Although the FMLA is limited 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, 2001a and b), it nevertheless represents an important expansion of coverage in the U.S. context by providing roughly 50 percent of employed women with a period of job protected leave.[1]

Prior research indicates that maternity leave provisions affect the length of time that women stay home after a birth. In general, the effects of the state laws and the FMLA tend to be fairly weak, reflecting the fact that these laws cover only a portion of the workforce and offer only unpaid leave (Klerman and Leibowitz, 1998a and b; Ross, 1998; Waldfogel, 1999b; Han and Waldfogel, 2003). Employer-provided leave policies tend to have stronger effects (Hofferth, 1996; Joesch, 1997; Glass and Riley, 1998; Waldfogel, 1998). Recent empirical research finds, for instance, that women who have employer-provided leave coverage are less likely to return to work in the first 12 weeks after the birth than other women who worked pre-birth but did not have job-protected leave coverage (Berger and Waldfogel, in press). However, a limitation of the research on employer policies is that such policies may be endogenous; that is, women may choose to work for a particular firm because it offers the length of leave that they prefer to take.

Whether children would benefit if their mothers had the option to stay home for 12 or more weeks, rather than returning in the first 12 weeks, is not clear. In the U.S. 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, Han, and Waldfogel, 2002 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, Waldfogel, and Brooks-Gunn, 2001; Brooks-Gunn, Han, and Waldfogel, 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 a causal connection 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/Oral Polio immunizations (in approximately the first 18 months of life), and cognitive and behavioral outcomes 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

[1] Family leave laws such as the FMLA provide for other types of leave. Here we focus on maternity leave but, in future work, we hope to examine paternity leave as well. We can not do so here due to data limitations (our dataset

bias, we utilize both instrumental variables (IV) and propensity scores matching strategies in addition to ordinary least squares regressions (OLS).

To briefly preview our results, we find considerable associations between early returns to work and many of the outcome measures using OLS models. Unfortunately, our IV models can not confirm our OLS results, as they are imprecisely estimated and inconclusive. Our propensity scores estimates, however, suggest causal relationships between early returns to work and reductions in breastfeeding and immunizations, and increases in externalizing behavior problems, among children whose mothers worked pre-birth. And, these results are generally stronger for mothers who returned full-time. Furthermore, our propensity scores estimates indicate that children whose mothers return 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.

II. Background

Labor force participation among U.S. women with young children has dramatically increased over the last three decades. In 1975, 34 percent of women with children under three were employed. This figure rose to 62 percent by 1998. Women with infants are also working in much greater numbers than they did in the past. In 1998, 58 percent of women with a child under the age of one were participating in the labor market, as compared to only 30 percent in 1975.[2]

Additionally, working women in the U.S. 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,

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does not track how long new fathers remain at home).

[2] All figures in this paragraph from Waldfogel, Han, and Brooks-Gunn (2002).

which differ so dramatically between the U.S. and other countries, are also likely to play a role. In the following sections, we provide an overview of maternity leave provisions in the U.S., a review of prior literature on relationships between leave coverage and women's leave-taking and lengths of leave, and, finally, a review of what is known about whether increased leave coverage, leave-taking, and leave lengths are associated with improved outcomes for children.

a. Maternity leave provisions in the U.S.

Until 1993, the U.S. was one of only a few advanced industrialized countries without a national maternity leave law. Although the 1978 Pregnancy Discrimination Act (PDA) requires employers who offer disability coverage to provide the same benefits for maternity as they do for other types of disability, the PDA does not require employers to offer such coverage. Thus, until the passage of the FMLA in 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 private-sector employers to offer job-protected maternity leave. In the fifteen years between 1972 and 1987, a total of 6 states (California, Connecticut, Massachusetts, Minnesota, Rhode Island, and Washington) enacted such laws; in the five years between 1988 and 1993 (when the FMLA was passed), a further 6 states (Maine, New Jersey, Oregon, Tennessee, Vermont, and Wisconsin) and the District of Columbia did so (Waldfogel, 1999a).[3] These laws provided between 4 and 18 weeks of leave to employees who met specific qualifying conditions.

The FMLA, which was passed and signed into law in February 1993, and came into effect in August 1993, for the first time provides the right, under federal law, to a job-protected maternity leave for qualifying employees (the law also provides the right to leave for other family and medical reasons). To qualify for coverage under the FMLA, an employee must work for an employer with 50 or more employees and must have worked at least 1,250 hours for that employer in the prior year.

Evidence suggests that the FMLA increased women's access to maternity leave coverage in the medium-sized and large firms to which it applies. In 1991, only 39 percent of women working in medium-sized and large firms had access to maternity leave coverage. By 1993 over 50 percent of these women had coverage, and by 1997, coverage rates for this group were close to 100 percent.[4]

However, only about 60 percent of private sector employees work in firms that are affected by the FMLA, and only about 45 percent qualify for coverage (a quarter of women working in affected firms have not worked the requisite 1,250 hours in the prior year) (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.

Another limitation of the FMLA is that it does not guarantee paid leave, in contrast to policies in most other industrialized countries. There is evidence that the lack of paid leave in the U.S. leads some workers who are eligible for and in need of a leave to forgo it or to take a shorter leave than they otherwise would have (Cantor et al., 2001; Waldfogel, 2001c).

The FMLA is also limited in the length of leave it provides – only 12 weeks. This stands in sharp contrast to the provisions in other advanced industrialized nations. The 19 advanced industrialized countries that make up the OECD provide, on average, the right to 10 months of maternity leave, with several European countries providing parental or child-rearing leaves that extend into the second or third year after the birth (Kamerman, 2000; Waldfogel, 2001b).

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[3] In addition, 8 states passed laws covering state employees.
[4] All figures in the paragraph from Waldfogel (2001a); see also Waldfogel (1999a).

In short, 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 U.S. have called for extending leave policies to cover all working women, provide paid leave, and provide a longer period of leave (see, for instance, Smolensky and Gootman, eds, 2003).

b. 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, 1998b; Ross, 1998; Waldfogel, 1999b, and 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, 1998b).

A fair amount of empirical evidence from the U.S. 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. Klerman and Leibowitz (1998a) use data from the 1980 and 1990 U.S. Census to study the effect of state maternity leave statutes on leave duration and find that the passage of a state maternity leave statute is associated with about two weeks longer leave for new mothers covered by the laws, although these effects are somewhat sensitive to specification. In later work, Klerman and Leibowitz (1998b) use data from the June Current Population Survey (from various years between 1984 and 1995) to examine the impact of the FMLA and report that the passage of the law led some women to take longer leaves and other women to take leaves rather than quitting their jobs entirely. Additionally, Waldfogel (1999b) uses data from the 1992 to 1995 March Current Population Survey (CPS) and finds that women with infants who gained coverage under the FMLA are more likely to be on leave (this study did not examine leave lengths because the March CPS does not provide such data).

There have also been two studies using the Survey of Income and Program Participation (SIPP) to examine relationships between leave laws and leave length. Ross (1998) uses data from the SIPP from 1990 to 1995 and finds that women who gained coverage under the FMLA took about six weeks more leave post-birth. Han and Waldfogel (2003) use data from the SIPP for 1990 to 1996 and find that women who gained coverage under the FMLA took about three weeks more leave post-birth. However, both studies are limited by the fact that the SIPP tracks only unpaid leaves; thus, if the FMLA had an effect on paid leave-taking, they would not have been able to detect it.

The relationship between maternity leave coverage and leave length may not be linear. Specifically, maternity leave coverage may increase leave length up to a particular threshold, but decrease leave length after that point. This makes sense given that all maternity leave policies have maximum leave allotments. As such, a woman must return by a certain point if she wishes to keep her job. Empirically, two existing studies support such an effect. Joesch (1997) uses data from the 1988 National Survey of Family Growth to estimate the effect of paid leave coverage on women’s leave-taking pre- and post-childbirth and finds that women who have paid leave coverage are more likely to take time off during the first month of life, but return to work more quickly thereafter; she also reports that women who have the opportunity to take more time off work return later.

Most recently, Berger and Waldfogel (in press) use NLSY data from 1988 to 1996 and 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 that the former return more quickly after the 12 week period has elapsed. Their results strongly suggest that women who have leave coverage and who return to work within the first year of giving birth will return within the period of leave to which they are entitled.[5]

c. 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. No research to date has been conducted on the health effects of maternity leave policies in the U.S., but research on maternity leave policies in Europe has found that longer leave periods are associated with improved health outcomes for children (Winegarden and Bracy, 1995; Ruhm, 2000; Tanaka, 2003).

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, Jelliffe, and Jelliffe, 1991).[6] 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, Whittington, Fein, and Teisl, 1996). It is also likely that women who are home longer may be in a better position to

[5] Studies outside the U.S. have also found that maternity leave coverage influences women's leave-taking behavior (see, for instance, studies by Ondrich and colleagues (1996, 1998) for Germany, or Waldfogel (1998) and Burgess et. al (2002) for Britain).

[6] 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 U.S. do not meet these goals (Philipp, Merewood, and O'Brien, 2001). Breast- feeding may also be associated with better cognitive outcomes (see for instance Mortensen et al, 2002) but here the evidence is less conclusive.

monitor their child’s health and to take their child for doctor’s visits (Ruhm, 2000; Tanaka, 2003).

There is also a potential connection between longer leaves and improved developmental outcomes for children. Several U.S. studies have found adverse effects on cognitive development or behavioral 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, Han, and Waldfogel, 2002; for recent evidence on early child care and behavior 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 short, there is considerable recognition of the importance of maternity leave coverage, but no clear consensus as to how long maternity leave should last. It appears that longer leaves are associated with better health outcomes for women and infants, and could potentially lead to better developmental outcomes as well.[7] But convincing empirical evidence regarding causal links between maternity leave, early maternal employment, and child outcomes is lacking.

In this study, we take advantage of variations in women’s maternity leave taking in the U.S. to analyze whether early returns to work have an effect on child health and development outcomes. Our study builds on the prior literature on maternity leave, women’s returns to work,

[7] At the same time, very lengthy periods of maternity leave may have adverse effects on women’s position in the labor market (Ruhm, 1998). And, if such leaves are mandated, employers may be less willing to hire women, or may reduce their wages (Waldfogel, 2001a). These competing effects have led social policy analysts to recommend moderate extensions of the length of the maternity leave entitlement in the U.S. (see, for instance, Waldfogel, 2001b, who calls for extending leave rights to the 10-month OECD average).

and leave-taking behavior, but extends this literature by estimating the effects of leave taking on child outcomes using microdata for the U.S.

III. Theoretical Framework

We posit that leave coverage will impact women's leave usage and, through this impact, will ultimately affect child health and development. Klerman and Leibowitz (1998b) present a very useful model for understanding the likely impact of maternity leave coverage (and extensions) on women's decisions about taking leave and returning to work (see also Klerman and Leibowitz, 1998a, 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. The length of coverage is set by employers (sometimes in response to legislative mandates, sometimes as a result of voluntary company policies or union contracts). Women's preferences are affected by the value they place on staying home after the birth as compared to the value they would receive from working in the labor market. We assume that the value a woman places on staying home versus returning to work is influenced by her perception of the impact her presence in the household may have on her child's health and development.

Klerman and Leibowitz (1998b) emphasize that because the labor market is not a spot market, but rather rewards connections with specific employers, a woman will typically receive a higher wage if she returns to her former employer than if she begins work with a new employer. Thus, in the face of constraints on the length of maternity leave allowed by an employer, a woman who worked prior to the birth must choose among two options post-birth: (1) to take a leave of the length permitted by the employer (which may be shorter than her desired leave length), but to resume work at a wage that is higher than she would receive in a new job; or (2) to quit the job so that she can take a longer leave than that permitted by the employer and, if she chooses to return to work at some later time, to return to the labor market at a lower wage than she would have received had she returned to her old job.

This model implies that maternity leave coverage will lead to increases in women taking leave. However, the effect of maternity leave extensions on leave lengths is theoretically ambiguous. Extensions should lead some women to take longer leaves, as some women who previously took shorter than desired leaves are now able to take longer leaves. Expansions should also lead to more women returning to their pre-birth employers post-birth, as some women who would have previously quit their jobs because the leave period was too short are now able to take a long enough leave to suit their needs. Note, however, that among this latter group, the actual length of time that a mother stays home could be shorter under an extended maternity leave policy (if the policy induces her to return to work sooner than she would have under the pre-extension scenario). Thus, theory would predict that, all else equal, women with maternity leave coverage should be more likely to take leave and that maternity leave extensions should be associated with longer leaves for many women (but, potentially, shorter lengths of time at home for others).[8] And, actual maternity leave lengths should be associated with leave lengths allotted by government or employer policies. The empirical evidence for the U.S., reviewed above, 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.

[8] For a more specific discussion of these effects, see Klerman and Leibowitz, 1998b. Economic theory would also predict that the impact of leave coverage on women's leave utilization would depend on whether the leave provided was paid or unpaid (see Joesch, 1997 for a useful discussion on this point). All else equal, a paid leave would be more likely to be taken up, and to be used for a longer period, than an unpaid leave.

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 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 ("well-baby" visits and immunizations) and breastfeeding. To the extent that shortfalls in leave coverage require early returns, we would expect reductions in these activities. Previous research on early maternal employment also 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 behavior problems and better language and verbal abilities. However, it is unclear whether the 12 weeks provided by the FMLA provide an adequate "window" for improving children's 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.

IV. Data and Measures

a. Sample

We use microdata from the 1987 through 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 biannually thereafter. The NLSY gathers data on the original cohort members and also on children born to the women in the cohort.[9] Variables for this analysis are drawn from the Geographic Micro-Data file, as well as the Children and Young Adults, and Work History files.

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[9] 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 U.S. in these years.

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 accurately identify exact periods of maternity leave.[10] 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. This time period gives us a recent cohort of births, which occurred in the context of considerable changes in state and federal legislation related to maternity leave, including the passage of the FMLA.

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.[11] We identified 1,907 such births in the NLSY between 1988 and 1996.[12]

b. Measure of early return to work

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[10] Although earlier data from the NLSY have been used in prior research (see for instance Leibowitz, Klerman, and Waite (1992), who use data from 1979 to 1986, and Desai and Waite (1991), who use data from 1979 to 1985), the maternity leave data for births prior to 1988 are incomplete. For a detailed discussion of identifying maternity leaves in the NLSY, and the advantages of the 1988 and later data relative to the pre-1988 data, see Klerman, 1993.

[11] 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. A more important concern is that a woman may choose her specific employer at least in part because of the maternity leave policy it offers. For this reason, we do not use data on employer policies to predict leave-taking or child outcomes in this paper.

[12] Many of the child outcomes models are estimated on smaller sample sizes 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 immunizations; 769 for BPI externalizing behavior problems; and, 1,064 for PPVT-R percentile scores. Immunization 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. Therefore, only children born by 1994 are included in these models.

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 labor market.[13]

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 utilize 12 weeks as our threshold because it is policy relevant, as 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.

c. 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;[14] (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)

[13] 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.

[14] According to the American Academy of Pediatrics (2000) guidelines, children should be seen by their doctors at least 8 times in the first year of life for “well-baby” (prevention and screening) visits.

whether the child received all of his or her DPT/oral polio immunizations (in the first 18 months of life); (6) the child's score on the externalizing behavior problems subscale of the Behavioral 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.

Well-baby visits and immunizations 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 behavior, 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 behavior problems reported by the mother, which is administered for children in the NLSY starting at age 4. We focus on externalizing behaviors (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 behavioral and cognitive outcomes in the NLSY. Thus estimating the impact of leave-taking behaviors 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 percent 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 percent 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 percent of sample mothers breastfed for some period of time, but this was true for only 55 percent of mothers who returned to work within 12 weeks, and for 53 percent 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 outcomes for children whose mothers return to work quickly, particularly if they return full-time.

d. Control variables

Clearly, the unadjusted differences in outcomes between mothers with different leave-taking behaviors 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" 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. Model 2 includes these demographic controls, as well as the following state level control variables: the percent of the population in the state and year 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

U.S. (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.

V. Empirical Analyses

a. 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.[16] The results of these models are presented in Panel A of Table 2.

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 immunizations. 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 receive all of their immunizations (marginally significant). They also breastfeed for about 4.5 fewer weeks. The breastfeeding effects are particularly large: given

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[15] This variable was drawn from: U.S. 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] Although several of our outcome variables are dichotomous, we show results from OLS models for all outcomes so that they will be more comparable to the two-stage least squares (IV) estimates presented in the following section. 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.

that, on average, 58.4 percent 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 percent less likely to breastfeed and breastfeed for 41 percent 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 can not rely on OLS estimates to estimate causal relationships between early returns to work and child outcomes. Therefore, we utilize two identification strategies intended to estimate causal effects of early returns on child outcomes in the presence of selection bias: instrumental variables (IV) models and propensity score matching. These results are presented below.

b. Do instrumental variables models suggest causal relationships between early returns to work and children's health and development?

Our first method for addressing selection is to estimate two-stage-least-squares instrumental variables models to test the robustness of our OLS results and assess their implications regarding causality. The OLS model (where observation subscripts have been omitted for simplicity) discussed in the previous section can be written as

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

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. Our concern with this specification is that *return12* may be (partially) correlated with our error term, u, (that is, E($return12, u|x$)≠0) because women self-select into different leave-taking arrangements. Alternatively, instrumental variables (IV) models set up a two-equation specification, in this case

$$child\ outcomes = \beta_0' + \delta\ return12 + \Sigma\,\beta_k'\,x_k + u \qquad (2)$$
$$return12 = \alpha_0 + \lambda\,Z + \Sigma\,\alpha_k\,x_k + e \qquad (3)$$

(estimated via two stage least squares) in which Z is our instrument (Angrist and Krueger, 1999). Z is assumed to satisfy the following properties:

$$\mathrm{Cov}[Z, return12] \neq 0, \qquad (4)$$
$$\mathrm{E}[Z * e]=0, \text{ and} \qquad (5)$$
$$\mathrm{E}[Z * u]=0. \qquad (6)$$

Therefore we need an instrument that is predictive of women's return to work (equation 4), serves to randomly assign some women's decisions about return to work (equation 5), and is exogenous to the child outcomes in the context of the second equation (equation 6). We can then use this instrument to predict women's returns to work and estimate the effect of these predicted returns to work decisions on child outcomes (δ). In practice the results of such instrumented models are often inconclusive, due to extremely large standard errors in the second stage. Thus, although we attempt such models here we do not hold out much hope that they will provide precise estimates. Rather, we hope that they might provide some information as to the direction of any potential bias.

The instrument we use in our analyses is the share of employed women covered by a union in a state and year (which we calculated from the March Current Population Survey). The percent of unionized working women could impact leave-taking because unionized workplaces tend to provide better employee benefits, on average, than many other types of jobs. Thus, all else equal, a state with a higher unionization rate should have more women who are covered by leave packages and, when covered, provided with more generous leave packages. This in turn should affect leave-taking behavior and we demonstrate later in the paper that the correlation between percent of unionized women and leave-taking behavior is non-trivial. Thus our first assumption appears to be satisfied.

To satisfy our second assumption, we need to believe that the unionization rate for working women creates an exogenous shock to a woman's life. Therefore, of the many variables predicting the share unionized in a state and year, we assume that women's preferences about maternity leave are not likely to play an important role (women didn't self-select into states with high rates of unionization because they anticipated wanting to have a longer maternity leave). Conceptualized in a slightly different and simplistic way, we hope that women who live in states with high rates of unionization are similar to women who live in states with low unionization, conditional on our control variables, except with regard to their subsequent leave-taking decisions and all outcomes subsequent to this.

The third assumption requires that the only way that the percentage of working women unionized in a state for a given year could affect future child outcomes is through its impact on maternal leave-taking. Certainly it is plausible that unionization could also impact (again through better employee benefits) family health insurance coverage. This is the motivation for including the control for the percentage of the population with health insurance coverage in the state and year in Models 2 to 4. Unionization could also be correlated with other state characteristics that impact children's development such as more generous social policies. We hope that our specifications that control for a variety of state characteristics including the party composition of the state legislature (Models 2 to 4) should help to make the exclusion restriction more plausible.

As discussed above, a suitable instrument must affect women's leave-taking behavior but should not affect children's outcomes outside of its impact on leave-taking behavior. For this reason, we are not able to use the policy of the employer for which they worked pre-birth (since as discussed earlier, women may have chosen their employer in part because of the leave coverage offered). It is even questionable whether we can use the leave laws in effects at the time of the birth since those, too, could potentially have been influenced by women's preferences.[17]

(1) First-stage results: are there effects of the share unionized on early returns to work? It is also important for IV estimation that union coverage have a reasonably strong effect on the amount of leave a mother takes. Appendix Table 1 presents the results of OLS models estimating the effect of unionization on returns (i.e., the first stage results of our IV models). In panel A, we see that the share of employed women who are covered by a union in a state and year is negatively related to whether a mother returns to work within 12 weeks in three of the four models. For example, the results for Model 3 suggest that, after controlling for demographic characteristics, state characteristics, and region of residence, a 1 percentage point increase in the proportion of working women who are covered by a union is associated with a 0.5 percentage point decrease in the probability that a mother will return to work within 12 weeks. This result is

[17] In practice, even if we wanted to use the state laws, or the federal law, their effects on women's leave taking are so weak that they would not be acceptable instruments for the analyses we would want to carry out. As discussed earlier, the laws have not covered all workers and have provided only short and unpaid periods of leave. As such, they have had a limited effect on leave-taking. In our data, they significantly increase the likelihood that a new mother stays home more than 6 weeks, but they do not increase the likelihood that she stays home more than 12.

statistically significant in Models 1 and 2, marginally significant ($p < 0.10$) in Model 3, but not significant in Model 4 where we add state fixed effects.[18]

Results when these models are estimated predicting whether mothers return to work full-time within 12 weeks (Panel B) are consistent with these findings, but show slightly larger effects. For this sample, the effect of union coverage is statistically significant in Models 1 through 3 but, again, is not significant in Model 4. Because the effects of union coverage are not statistically significant in Model 4, we estimate only Models 1 to 3 in our IV analyses.

(2) Second stage results: are there effects of (instrumented) early returns to work on children's health and development? Appendix Table 1 provided evidence that our instrument, share unionized, is related to early returns to work. Panel B of Table 2 shows the results of IV models in which we estimate the effects of early returns on child health and development for those whose leave-taking behavior was influenced by unionization rates. In general, we see that these models are imprecisely estimated and that the effects of early returns to work are (predominantly) not statistically significant. While many, but by no means all, of the effects function in expected directions, it is difficult to draw any firm conclusions from these estimates – they shed little light on whether there may be causal links between early returns and child outcomes. It is important to keep in mind, however, that, as is often the case in IV estimations, these estimates are quite "noisy." As stated above, we do not provide IV estimates for Model 4 because our instrument (percent union) was not statistically significant in the first stage equation.

c. Do propensity scores models suggest causal relationships between early returns to work and children's health and development?

[18] The fact that our first-stage estimates are not significant when we control for state fixed effects may be due to noise, or to other state factors that are correlated both with unionization and leave-taking. In any case, as we discuss below, because these estimates are not significant, we do not estimate Model 4 using IV.

A second method for estimating causal effects in the presence of selection bias is propensity score matching (Rosenbaum and Rubin, 1983, 1984, 1985). These models account for selection on observables by "matching" mothers who returned to work early with those who did not, effectively allowing us to compare "like" mothers who return to work in different periods. The identification strategy for propensity score matching is similar to that of linear regression in that both require that all confounding covariates have been measured. The advantage of propensity score matching over linear regression is that it does not require specification of a functional form for the relationship between the outcome and the "treatment" variable (leave-taking) which ends up, in most cases, forcing the researcher to rely on extrapolations over parts of the covariate space where we have no data (that is, it forces the researcher to rely strongly on out-of-sample predictions). The goal instead is to find a group among our comparison population (those who stayed home at least 12 weeks) that look as similar as possible to those in our treatment group (those who went back to work within 12 weeks). To achieve this goal we match observations from each of these groups based on the estimated propensity score,

$$\Pr(\, return12{=}1 \mid x_1, \ldots, x_k),$$

which is the conditional probability of being in the treatment group given our measured covariates. This can be estimated using standard logistic or probit regression models. Fortunately, treatment effect estimates are relatively robust to misspecification of this equation (Drake, 1993).

In theory, then, 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 help to control for any bias not fully controlled for by the matching (and 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). Of course, as with linear regression, if we have failed to control for an important confounding covariate our estimates can be biased.

The first step in propensity score analyses is to match treatment and comparison groups based on their propensity scores. When we do so in our sample, we find that the groups are very well-matched, with no significant differences in measured background characteristics (confounding covariates) between the two groups. This close balance gives us confidence that when we run our regression models for these samples we will be comparing like to like and will not have to rely on model extrapolations.

In Panel C of Table 2, we provide regression results obtained through propensity score matching methods.[19] 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 externalizing behavioral 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 immunizations. Children whose mothers return to work within 12 weeks are 7.9 percentage points less likely to be breastfed, are breastfed for about 4.5 fewer weeks, are 4.1 percentage points less likely to receive all DPT/Oral Polio immunizations, and have 0.8 more externalizing behavior problems. In contrast to our inconclusive IV models, these results provide support for a

[19] Sample sizes for these regressions are smaller than in the OLS or IV 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 immunizations to a high of 1485 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.

causal link between leave length and child outcomes such as breastfeeding, immunizations, and externalizing behavior problems.

d. 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 returning full-time within 12 weeks to those not returning within 12 weeks. 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 externalizing behavioral problems. There is also some indication that they are less likely to receive all of their DPT/Oral Polio immunizations. These differences are particularly large in terms of breastfeeding and externalizing behavior problems. The earlier OLS models indicated that women returning within 12 weeks are 13 percent less likely to breastfeed and breastfeed for about 41 percent fewer weeks, but that their children are not significantly more likely to have behavioral problems. By comparison, the OLS estimates for women returning full-time within 12 weeks indicate that they are about 18 percent less likely to breastfeed and breastfeed for about 54 percent fewer weeks.

Additionally, their children have about 12 percent more behavioral problems. Thus, we see substantially larger negative effects of early returns to work when these returns are full-time. Again, however, these results may be subject to selection bias. We therefore present our IV and propensity scores results next.

Our IV estimates (Table 3, Panel B) are once again imprecisely estimated and do not shed much light on the extent to which our OLS estimates are biased, nor whether there may be causal relationships between early full-time returns and children's outcomes. But, once again, our propensity scores models (Table 3, Panel C) suggest causal links between these 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 externalizing behavior 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 7.9 percentage points less likely to breastfeed, while these results suggest that women who return full-time are 14.1 percentage points less likely to breastfeed. 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.

VI. Conclusions

New mothers in the U.S. are less likely than mothers in other countries to have the right to a job-protected maternity leave and, when they do, the leave they are entitled to tends to be shorter and is more likely to be unpaid. Perhaps as a result, new mothers in the U.S. 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. U.S. 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 do return more quickly thereafter (Berger and Waldfogel, in press). In our sample, nearly 63 percent of women who are working pre-birth return to work within 12 weeks of giving birth, and half of that group, 37 percent of women working pre-birth, return full-time.

Do the short periods of maternity leave among U.S. 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 outcome 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 immunizations (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 externalizing behavior problems at age 4.

However, OLS regressions may be inappropriate for estimating causal effects. We therefore attempt to address selection bias through both IV and propensity scores methods. Unfortunately, our IV estimates are imprecise and inconclusive. But, our propensity scores estimates are suggestive of causal relationships between early returns and several of the outcomes, particularly breastfeeding, immunizations, and externalizing behavior problems.

Additionally, the propensity score results are generally stronger for mothers returning full-time within 12 weeks. And, children of these mothers may also be at risk of reduced well-baby care.

Although further research on this topic is needed, the evidence provided here raises a “red flag” concerning policies that unnecessarily hasten women’s returns to work after giving birth. Given this evidence, we concur with the recent National Academy of Sciences panel (Smolensky and Gootman, eds, 2003) that U.S. policy makers should reconsider the wisdom of TANF policies that require women to return to work within 3 months after giving birth, and should explore options to extend parental leave coverage to cover more new parents, provide some mechanism for paid leave, and grant a longer period of leave.

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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.=1907)	1907 (100.0)	1191 (62.5)	707 37.1
	Mean (S.D.)	**Mean (S.D.)**	**Mean (S.D.)**
Any Well Care (Obs.=1678)	0.954 (0.211)	0.944 (0.231)	0.939 (0.240)
# Months Well Care (0-12) (Obs.=1678)	3.412 (2.436)	3.316 (2.412)	3.303 (2.338)
Any Breast Feeding (Obs.=1674)	0.584 (0.493)	0.549 (0.498)	0.525 (0.500)
# Weeks Breast Fed (0-52) (Obs.=1620)	11.088 (15.696)	9.338 (14.464)	8.128 (13.083)
All DPT/Oral Polio Immzs. (Obs.=737)	0.927 (0.261)	0.906 (0.293)	0.899 (0.302)
Externalizing Behavior Problems Age 4 (Obs =769)	5.151 (3.563)	5.229 (3.580)	5.427 (3.554)
PPVT Percentile Score Age 3-4 (Obs.=1064)	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.

Table 2: Effects of returning to work by 12 weeks on child outcomes (All Mothers Working Pre-Birth)

	Panel A: OLS Models				Panel B: IV Models				Panel C: Propensity Scores Models			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**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*	-0.114	0.029	-0.018		-0.025*	-0.030*	-0.017	-0.019
(Obs.=1678)	(0.010)	(0.010)	(0.010)	(0.010)	(0.100)	(0.173)	(0.171)		(0.012)	(0.012)	(0.013)	(0.013)
# Months Well Care	-0.203+	-0.126	-0.115	-0.054	-5.831**	-2.566	-3.470		-0.225	-0.091	-0.217	-0.135
(Obs.=1678)	(0.121)	(0.120)	(0.120)	(0.122)	(1.746)	(1.988)	(2.397)		(0.150)	(0.145)	(0.147)	(0.145)
Any Breast Feeding	-0.076**	-0.078**	-0.078**	-0.075**	-0.124	-0.211	0.434		-0.107**	-0.047	-0.092**	-0.079**
(Obs.=1674)	(0.024)	(0.024)	(0.023)	(0.024)	(0.226)	(0.373)	(0.457)		(0.030)	(0.029)	(0.028)	(0.028)
# Weeks Breast Fed	-4.677**	-4.683**	-4.645**	-4.482**	-6.563	-12.075	2.102	NOT	-3.771**	-4.383**	-5.185**	-5.224**
(Obs.=1620)	(0.798)	(0.797)	(0.783)	(0.801)	(6.884)	(11.650)	(12.327)	ESTIMATED	(0.982)	(1.008)	(1.077)	(1.088)
All DPT/Oral Polio Immzs.	-0.046**	-0.042*	-0.040*	-0.034+	-0.298	-0.109	0.338		-0.047+	-0.052*	-0.046+	-0.041+
(Obs.=737)	(0.018)	(0.018)	(0.018)	(0.018)	(0.227)	(0.367)	(1.083)		(0.024)	(0.021)	(0.025)	(0.024)
Externalizing BPI Age 4	0.317	0.303	0.319	0.322	-0.127	-4.661	-7.980		0.338	0.655+	0.709*	0.773*
(Obs.=769)	(0.271)	(0.275)	(0.276)	(0.285)	(2.943)	(10.122)	(12.954)		(0.319)	(0.344)	(0.337)	(0.328)
PPVT Pctile. Score Age 3-4	-1.637	-1.374	-1.279	-1.007	-18.253	-24.213	3.266		-2.046	-1.918	-0.611	-0.674
(Obs.=1064)	(1.595)	(1.609)	(1.612)	(1.656)	(18.191)	(76.402)	(155.129)		(2.330)	(2.039)	(1.958)	(1.971)
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
State controls		yes	yes	yes		yes	yes	yes		yes	yes	yes
Region controls			yes				yes				yes	
State Fixed Effects				yes				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%; *** significant at 0.1%.

Table 3: Effects of returning to work by 12 weeks on child outcomes (Mothers Returning Full-Time)

	Panel A: OLS Models				Panel B: IV Models				Panel C: Propensity Scores Models			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**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*	-0.117+	-0.043	-0.027		-0.043**	-0.025	-0.027+	-0.027+
(Obs.=1253)	(0.012)	(0.012)	(0.012)	(0.012)	(0.070)	(0.124)	(0.141)		(0.013)	(0.016)	(0.014)	(0.015)
# Months Well Care	-0.237+	-0.121	-0.107	-0.052	-3.270**	-0.983	-0.806		-0.298+	0.113	-0.213	-0.188
(Obs.=1253)	(0.133)	(0.132)	(0.132)	(0.134)	(0.962)	(1.320)	(1.542)		(0.167)	(0.164)	(0.161)	(0.158)
Any Breast Feeding	-0.105**	-0.106**	-0.108**	-0.104**	-0.169	-0.255	0.178		-0.122**	-0.088**	-0.135**	-0.141**
(Obs.=1250)	(0.027)	(0.028)	(0.027)	(0.028)	(0.157)	(0.278)	(0.337)		(0.032)	(0.033)	(0.033)	(0.033)
# Weeks Breast Fed	-6.123**	-6.165**	-6.234**	-6.010**	-5.909	-8.200	0.176	NOT	-4.995**	-6.720**	-5.404**	-5.283**
(Obs.=1215)	(0.865)	(0.866)	(0.855)	(0.882)	(4.845)	(8.839)	(10.330)	ESTIMATED	(1.048)	(1.291)	(1.127)	(1.147)
All DPT/Oral Polio Immzs.	-0.047*	-0.042+	-0.038+	-0.035	-0.180	-0.045	0.077		-0.020	-0.025	-0.030	-0.038
(Obs.=539)	(0.022)	(0.022)	(0.022)	(0.023)	(0.125)	(0.188)	(0.335)		(0.028)	(0.030)	(0.029)	(0.028)
Externalizing BPI Age 4	0.617*	0.616*	0.643*	0.576+	-1.319	-9.322	-26.795		0.807*	0.168	0.696*	0.478
(Obs.=574)	(0.304)	(0.313)	(0.315)	(0.333)	(2.192)	(14.571)	(89.118)		(0.396)	(0.398)	(0.343)	(0.370)
PPVT Pctile. Score Age 3-4	-2.631	-2.162	-2.148	-2.409	-10.588	1.588	46.533		-2.633	-2.335	-0.070	0.580
(Obs.=809)	(1.780)	(1.811)	(1.822)	(1.882)	(12.162)	(42.943)	(118.936)		(2.149)	(2.189)	(2.248)	(2.221)
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
State controls			yes	yes			yes	yes			yes	yes
Region controls			yes				yes				yes	
State Fixed Effects				yes				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%; *** significant at 0.1%.

Appendix Table 1: Effect of percent union (instrument) on returning to work in 0 to 12 weeks								
	Panel A: All Mothers Working Pre-Birth				**Panel B: Mothers Returning Full-Time**			
	(1)	**(2)**	**(3)**	**(4)**	**(1)**	**(2)**	**(3)**	**(4)**
Percent union	-0.008**	-0.005*	-0.005+	-0.006	-0.012**	-0.008**	-0.007*	-0.003
	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)	(0.004)
Observations	1907	1907	1907	1907	1423	1423	1423	1423
R-Squared	0.04	0.04	0.05	0.07	0.07	0.07	0.08	0.11
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: Percent union is the percentage of working women who are unionized in the state and year of the child's birth. 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%; *** significant at 0.1%.