Published in final edited form as:
*B E J Econom Anal Policy*. 2011 July 28; 11(1): . doi:10.2202/1935-1682.2938.

# Public Policies, Women's Employment after Childbearing, and Child Well-Being

**Elizabeth Washbrook**,
Research Associate, Centre for Market and Public Organization, University of Bristol, 2 Priory Road, Bristol BS8 1TX, U.K

**Christopher J. Ruhm**,
Jefferson-Pilot Excellence Professor of Economics at the University of North Carolina–Greensboro, Research Associate at the National Bureau of Economic Research, and Research Fellow at the Institute for the Study of Labor (IZA). Box 26165, UNC Greensboro, Greensboro, NC 27402

**Jane Waldfogel**, and
Professor of Social Work and Public Affairs, Columbia University School of Social Work, 1255 Amsterdam Ave, New York, NY 10027

**Wen-Jui Han**
Associate Professor of Social Work, Columbia University School of Social Work (as above)

Elizabeth Washbrook: liz.washbrook@bristol.ac.uk; Christopher J. Ruhm: chrisruhm@uncg.edu; Jane Waldfogel: jw205@columbia.edu; Wen-Jui Han: wh41@columbia.edu

## 1. Introduction

This paper examines the consequences of three U.S. state policies – job-protected maternity leave, exemptions from welfare work requirements for mothers of infants, and child care subsidies for low income families - designed to affect how parents with newborns manage work and family. The first of these policies affects women employed prior to childbirth, while the other two are targeted to low-income parents. Supported by research showing conditions in infancy have important long-term consequences for health, abilities, and skills (Shonkoff and Phillips 2000; Heckman and Masterov 2007), such policies are justified, in part, by the belief they benefit children, and expenditures on them are substantial.[1]

Yet we know surprisingly little about whether work-family policies, in the form currently existing in the U.S., do yield such improvements. One reason is that we lack exogenous variation in policy 'treatments' – families that choose to make use of a policy are likely to differ from those that do not –making it hard to obtain causal estimates. Second, it is difficult to obtain data allowing children's developmental outcomes to be linked to the policies and conditions they experienced in the first year of life.

This paper exploits two sources of variation in eligibility for these policies – geographical differences in state laws and individual-level differences in demographic characteristics – to identify plausibly causal policy effects. We combine state-level policy data with rich survey data from the Early Childhood Longitudinal Study Birth Cohort (ECLS-B) which provides

Correspondence to: Elizabeth Washbrook, liz.washbrook@bristol.ac.uk.

[1]For example, costs of the 1993 Family and Medical Leave Act, have been estimated to be as high as $21 billion annually (Mulvey 2005, although see Institute of Women's Policy Research 2005), and spending on the Child Care and Development Fund child care subsidies reached $9.5 billion in 2000 (Gish, 2002).


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high-quality assessments of cognitive outcomes as well as reports about children's behavior in the year before school entry for a nationally representative sample of nearly 9,000 children. The effects of work-family policies on children's cognitive and socio-emotional school readiness is of particular interest given the large racial, ethnic, and socio-economic disparities in such outcomes and the evidence that children entering school not yet ready to learn continue to have difficulties later in life (Rouse, Brooks-Gunn and McLanahan 2005).

We begin our analysis by examining effects of the policies on maternal work participation during infancy. These estimates provide a clear test of whether our research design is powerful enough to isolate the expected effects of the policies on the child outcomes of primary interest. Moreover, changes in maternal work behavior in the postnatal period provide key mechanisms through which the policies might be expected to affect children. All three policies are shown to have significant and sizable effects on maternal employment during infancy in ways compatible with the specific incentives provided by each program.

However, changes in maternal employment are not the only channel through which these policies may impact children, and the role of employment may depend crucially on how it affects other aspects of family life. Therefore, we explore a range of other factors hypothesized as possible mechanisms linking work-family policies, maternal employment, and child outcomes.[2] Despite having strong effects on postnatal work behavior, we find that the policies had varying impacts on child care and small or nonexistent consequences for other parental inputs like breast feeding, number of health visits, maternal mental health, sensitivity of mother-child interactions, or subsequent household income.

Finally, we present estimates of the consequences of the focal policies on school readiness. Overall, we find no evidence of policy effects that are either significant or even moderate in size, and our estimates are precise enough to rule out effects that would be economically meaningful. The preceding analyses provide some clues as to why this occurs. Although the policies induce large changes in maternal work behavior in the immediate post-birth period, these do not translate into developmental differences, in part because they induce little or no change in family functioning or parental inputs. This presents something of a puzzle, given the large literature linking early maternal employment to child outcomes. Potential explanations are that parents adapt behaviors to changes in work status such that children's environments remain unaffected and that the U.S. policies are relatively limited in scope, particularly by the standards of many other OECD countries (Waldfogel, 2001, 2006; Ruhm, forthcoming).

## 2. Related literature

Relatively few studies directly investigate the question we focus on here, the relationship between public work-family policies and children's cognitive and socio-emotional school readiness. Closest to our approach is the research of Baker and Milligan (2008, 2010a, 2010b), who analyze the effects of an extension of job-protected leave in Canada from six months to one year in 2000. Although they find that eligible mothers increased their time on leave by 3–3.5 months (from an average of 6 months pre-reform to an average of 9 months subsequently) and increased their time breast-feeding by more than 1 month, they find no evidence of policy effects on children's health and developmental outcomes or family functioning assessed during the first two to five years of life. Also relevant is a study by Baker, Gruber and Milligan (2008), who analyze the effects on a range of child and family

[2]The possibility that policies affect other aspects of family life besides maternal employment status, and that the effects of maternal employment are heterogeneous, argues against using a standard instrumental variables strategy (instrumenting employment with the policies).

outcomes of the introduction of a universal $5-a-day child care program for toddlers and preschool age children in Quebec. This study shows significant negative effects of the subsidy on the socio-emotional and health outcomes of children under the age of five; the authors point to a strong increase in maternal labor supply accompanied by deteriorations in parental health and relationship quality and more hostile, less consistent parenting as an explanation.[3] These results suggest that the effects of work-family policies need not be uniform, as the details of the policies, the children they affect, and the counterfactual arrangements they displace may all differ.

Four recent papers on European parental leave policies examine longer-term outcomes of work-family policies for children. Dustmann and Schönberg (2008), Rasmussen (2010), Liu and Skans (2010), and Carneiro et al. (2010) exploit historical reforms in Germany, Denmark, Sweden, and Norway respectively, linking adolescent and young adult education and labor market outcomes to policies in effect at the time of the individual's birth. The first two studies do not find any evidence of policy effects on children's outcomes; the third study finds no overall effects (although with some indication of benefits for children of well-educated mothers), while the fourth study finds modest positive effects on long-run educational outcomes (with these being largest for disadvantaged children). But, for the most part, the reforms being examined, like the Canadian reform, extend time at home after the first few months of life, when the returns to parental time are likely the greatest.

A larger set of studies explore the effects of work-family policies on outcomes besides school readiness. In terms of child health, Ruhm (2000) and Tanaka (2005) use country-level time series data to document a relationship between increased parental leave generosity and reduced post-neonatal infant mortality. Parental leave policies and infant welfare work exemptions have been causally linked to greater breast feeding (Baker and Milligan 2008; Haider, Jacknowitz and Schoeni 2003). Several studies explore the effects of work-family policies on parental labor market outcomes. As Klerman and Leibowitz (1997) discuss, job-protected leave entitlements are expected to increase leave-taking but have ambiguous effects on work, primarily because some parents may choose a short job-protected leave rather than longer absences that require finding a new job. Most studies find that leave rights are associated with a lower probability that a mother works in the months immediately after a birth but with little if any effect on longer-term employment or earnings (Han et al. 2009; Hanratty and Trzcinski 2009; Lalive and Zweimuller 2009).[4] Conversely, child care subsidies lead to substantial increases in maternal labor supply (Berger and Black 1992; Blau and Tekin 2007; Lefebvre and Merrigan 2008) and welfare reform has been found to increase the employment of single mothers (Blank 2002; Grogger and Karoly 2005). However, few prior welfare studies focus specifically on mothers of infants and toddlers or explicitly examine infant work exemptions, which are among the most dramatic reform provisions.[5] That said, the limited available research indicates that single mothers work more after a birth if they reside in states that do not exempt them from welfare work requirements (Hill, 2007).

Although our main focus is on work-family policies, the large literature on the effects of early maternal employment on child outcomes is clearly relevant. A common finding of this literature is that first-year maternal employment is associated with lower cognitive test scores and higher levels of externalizing behavior problems, particularly for full-time work (Bernal 2006; Brooks-Gunn et al. 2002; Gregg et al. 2005; Hill et al. 2005; Ruhm 2004;

[3]This finding is echoed in recent work on the U.S. child care subsidy program (Herbst and Tekin, 2010; in press).
[4]In a cross-country study Ruhm (1998) finds that parental leave rights increase relative female employment rates, but reduce relative female wages at extended durations.
[5]Before TANF, women were generally exempted from welfare work requirements until their youngest child reached 36 months of age.

Waldfogel et al., 2002). A number of intermediate factors have been identified that point to both positive and negative consequences of maternal employment. Berger et al. (2005) found that children whose mothers worked by 3 months were less likely to be breast-fed and breast-fed for shorter durations, less likely to be up to date on health visits and immunizations in the first year of life, and more likely to have behavior problems at age 3 or 4; these effects were particularly pronounced if mothers worked full-time. Most recently, Brooks-Gunn et al. (2010) showed that maternal employment in the first year (particularly if full-time) has positive effects on some mediators (e.g. maternal earnings) and negative consequences for others (for example, increasing maternal depression), while having mixed effects on the quality of child care; as a result, the authors found that the overall effects of first-year maternal employment on child developmental outcomes were neutral.

## 3. Data

As the only nationally representative birth cohort study in the U.S., the Early Childhood Longitudinal Study Birth Cohort (ECLS-B) provides unique information on the environments of infants and their families. The clustered stratified survey design sampled over 10,000 birth certificates and was designed to be representative of births occurring in 2001. Twins, low birth weight infants, Asian and Pacific Islanders, American Indian and Alaska Natives and Chinese children were oversampled; we use the survey weights in all our estimates to correct for this. State of residence at child’s birth (taken from the birth certificate) was used to define the state policies relevant to each mother.

A baseline interview was conducted when the child was nine months old, with follow-ups at 24 months of age and in the fall of the year before the child entered kindergarten.[6] Each wave collected detailed information on the employment, demographic characteristics, lifestyles, and behaviors of parents, and on the early learning, care, and health experiences of children. A key feature of the ECLS-B is the wealth of information collected directly from children. Many assessments were adapted from well-known psychological scales, with rigorous field testing used to identify the most psychometrically sound items.[7] We use data from the first three survey waves, with most of our analysis drawing on the baseline information and the third (preschool) interview conducted when 80% of the children were four years old. Seventeen percent of the original cohort members were lost between the baseline and preschool waves; this is explored below.

## 4. Policies

### Parental leave

ECLS-B mothers gave birth in 2001 and so all were potentially covered by the 1993 federal Family and Medical Leave Act (FMLA) which provides 12 weeks of unpaid parental leave to employees working at least 1,250 hours in the previous year for firms with 50 or more employees. Our research design relies on cross-sectional geographical variation, so the FMLA (and other federal policies) forms part of the baseline scenario against which the effects of discretionary state policies are evaluated. Although the FMLA is potentially universal in coverage, less than half of private sector workers meet the FMLA qualifying conditions (Waldfogel 2001). As shown in Figure 1, fourteen states and the District of Columbia (covering 36% of sample mothers) had parental leave laws more generous than the FMLA in 2001. This additional generosity occurs through relaxing FMLA requirements for firm size, tenure, or work hours (thus expanding the share of women covered, although still not providing universal coverage), offering longer durations of paid leave, or in five

[6]The final wave of the ECLS-B administered in the fall of the kindergarten year was not available at the time of our analysis.

[7]See http://nces.ed.gov/ecls/birth.asp for additional details.

states, through temporary disability insurance (TDI) that provides a short (typically six week) period of paid leave (see Appendix Table A1 for details).[8]

Our main analysis uses a binary indicator distinguishing states with or without any state leave law. This implies that we are estimating impacts of the “average” state leave law, rather than of possible specific aspects of it. As Appendix Table A1 illustrates, there is too much overlap in the different provisions across states to allow separation of the disaggregated effects but this also implies that the effect of the average bundle may be of considerable relevance.

The intention of parental leave laws is to provide parents with a period of time when they can take leave from work after the birth of a newborn. We expect therefore that more generous leave laws will be associated with a greater share of women not employed and at work in the months immediately after the birth. The medium- and long-run effects are harder to predict; if women use leave and return to their pre-birth employers (rather than simply quitting their jobs to stay home with the baby), then long-run employment should be higher, but it is also possible that after a leave, women who might otherwise have stayed employed might develop a taste for being at home and long-run employment rates could be lower. Thus, the effects of leave policies are ambiguous, and depend on which specific groups of women are affected and in which direction.

### Welfare work exemptions

The federal legislation creating the Temporary Assistance for Needy Families (TANF) program, in 1996, provided states with a high degree of flexibility in designing their cash assistance programs for families with children. By 2001 the vast majority of states imposed work requirements of at least 30 hours per week and they were required to impose sanctions for non-compliance.[9]

Our analysis focuses on the age under which the youngest child must be to exempt a mother from work requirements. Specifically, we create a dummy equal to one if the state exempts mothers from working during the child’s first 12 months of life, or longer (27 states; 47% of sample mothers), and zero if the work exemption is less than 12 months. Since 16 states had exemptions lasting exactly 3 months, and 23 had exemptions lasting exactly 12 months, this seems like a sensible threshold. However, our results are robust to the use of cut-offs above 4 or 6 months. Figure 2 shows the geographical variation our binary exemptions indicator (see also Appendix Table A1).

We expect that low-income women who are exempt from welfare work requirements will be less likely to work in the months immediately following the birth. The long-run effects on employment are unclear a priori.

Exemptions are only one aspect of welfare generosity; other dimensions include the amount of cash benefits, time limits on receipt, and the stringency of sanctions for non-compliance. All of these are strongly correlated at the state level, limiting the certainty with which we can attribute any effects specifically to work exemptions. However, robustness checks, detailed below, provide some evidence that it is the duration of exemption from work requirements, rather than welfare policy more generally, that drives our results.

[8]We exclude laws restricted to state government employees, as these cover only a small share of the workforce. Han, Ruhm, and Waldfogel (2009) provide a detailed discussion of state leave laws.

[9]Full details of state policies prevailing in 2001 are available at: http://anfdata.urban.org/wrd/databook.cfm.

### Child care subsidies

Under welfare reform, child care funding for low-income families was consolidated into an expanded Child Care and Development Fund (CCDF) block grant, with total state and federal subsidies increasing from $1.7 billion in 1992 to $9.5 billion in 2000 (Gish 2002). The CCDF allows states to serve families with incomes up to 85% of state median income (many states set lower thresholds) whose parents are working or in school. States determine child care reimbursement rates and parent co-payment rates but must offer a choice of child care types and providers. They can transfer up to 30% of their TANF block grant to the CCDF and may use remaining TANF funds to directly subsidize child care (usually through vouchers). A small amount of child care funding is also available through the federal Social Services Block Grant.

We measure child care subsidies through a continuous variable capturing federal and state Child Care Development Fund (CCDF) expenditures in fiscal year 2000 (in 2001 dollars) per poor child under the age of 6. The timing is not ideal since fiscal year 2000 ends in September (fiscal year 2001 data are not available), whereas the oldest children in the ECLS-B cohort were born in January 2001. However, CCDF spending appears to be stable over this period (the correlation between state expenditures in FY2000 and FY2003 is 0.97) so the slightly lagged timing of the CCDF variable should not induce serious measurement error. Figure 3 gives an idea of the geographical variation in child care subsidy spending, where for ease of presentation we group the continuous expenditures variable into low (< $1500), medium ($1500–$2500) and high (>$2500) categories (see also Appendix Table A1).

The expected effects of more generous child care subsidies on low-income women's employment and child care use are not entirely clear. Because subsidies are conditioned on employment, they should be associated with higher rates of employment and child care use among the target group. However, to the extent that subsidies are being taken up by women who were working already, they might not affect employment and might instead simply affect child care costs (and possibly child care arrangements, if families shift to more formal care in order to receive the subsidy).

### Policy combinations

Our analysis explores the effects of all three policies simultaneously and so allows for the fact that policies may be correlated across states. Table 1 investigates the degree to which this is the case. It shows where states fall in terms of the twelve different potential policy combinations that can be created from our indicators (treating CCDF spending as categorical). The fact that 11 of the 12 combinations are represented by observed state policies shows the policies do not co-vary deterministically. States with leave laws do tend to spend more generously on child care subsidies, but there are leave states in which CCDF spending is relatively low, such as Oregon and California.[10] Welfare work exemptions, in contrast, are largely uncorrelated with the other two policies.

## 5. Empirical strategy

Our goal is to estimate causal effects of the state policies on a range of child and family outcomes. However, these policies are unlikely to be implemented randomly, but instead may arise from a complex interaction of geographical and historical circumstances, reflecting the economic and demographic composition and political preferences of the state population. Our focal policies may therefore be correlated with relevant confounding

[10]With average CCDF spending of $1537, California falls just above the low CCDF boundary.

factors. A classic difference-in-difference (DD) strategy addresses this problem by comparing outcomes of individuals before and after the implementation of a policy, exploiting state variation in the timing and incidence of implementation. Lack of longitudinal variation in our birth cohort sample precludes the use of this strategy, and requires the adaptation of the conventional DD framework for a cross-sectional context. Although the assumptions of the "group" DD estimator we use are arguably stronger than those of the longitudinal version, our analysis has the advantage that the exceptionally rich nature of the ECLS-B data allow us to control for many sources of heterogeneity in a way that is not possible with more conventional longitudinal datasets. In addition, we show our results are robust to controls for a range of potentially confounding state policies and characteristics.

Our estimation strategy relies on the assumption that each policy affects the post-birth outcomes of some mothers and children, while having little or no effect on others. Mothers ineligible for a policy form a 'control' group that is used to adjust for unobserved state-level heterogeneity, and so to infer the counterfactual outcomes of the eligible group in the absence of policy. Our assignment of individuals to eligible and control groups is determined by broad socio-economic characteristics implying that some individuals in our treatment groups will not actually be influenced by the policies, while some in the control groups might be. This approach is necessary because of data limitations on exact program eligibility and to avoid endogeneity whereby fulfillment of the eligibility criteria reflects responses to the policy. The potential consequences of these errors are addressed below, but we note here that such misclassifications make it likely that our estimates understate the true policy effects. We next describe the treatment and control groups for the three focal policies.

When evaluating state leave laws, our eligible group includes mothers employed at some point during the 12 months prior to the birth (71% of sample mothers); those not so employed are the controls. Some women in the treatment group will not be affected by the state policies. This occurs because most state laws relax qualifying conditions for leave benefits (although longer leaves are also sometimes provided), implying that women already eligible under the FMLA gain no additional rights; the same is true for mothers remaining ineligible under the state laws or who are covered by more generous employer leave policies.[11]

The treatment group when considering infant welfare work exemptions consists of women with no resident partner at the nine month interview, henceforth referred to as single mothers (20% of sample mothers). Two-parent families are technically eligible for TANF but rarely meet the income and other qualifying conditions (accounting for less than 4% of TANF families in 2001).[12]

Although eligibility for CCDF payments is explicitly determined by income, we use an education-based treatment group to avoid the endogeneity problem whereby child care costs influence work and therefore earnings. Specifically, our treatment group contains mothers in families where no parent has a high school diploma (i.e. mothers without a high school diploma who are single or reside with a similarly low-educated partner). Some of the control group will likely be eligible for child care subsidies. Nevertheless, since the subsidy amount is determined on a sliding scale (depending on family income), the incentives are likely to

[11]The DD estimates therefore represent the average 'intent to treat' policy effect over all mothers with a pre-birth job. A 'treatment on the treated' research design would scale these estimates by the proportion of that group obtaining new rights under the state laws. We lack the necessary information on tenure, firm size, and employer policies to conduct such calculations with any precision but view the estimated 'intent to treat' effect to be of interest in its own right.

[12]This was calculated using data from: http://www.acf.hhs.gov/programs/ofa/data-reports/caseload/caseload_recent.html [accessed July 16, 2010].

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be strongest for the least educated households. We investigated the consequences of widening the treatment group to include more educated parents, and found that the positive labor supply effects of the subsidies on participation became small or non-existent, consistent with the subsidy-induced work incentives being concentrated among the lowest income families.

A threat to our research design is potential endogeneity of the eligibility conditions with respect to the focal policies. For instance, a common contention is that U.S. welfare rules encourage single headship, although empirical evidence suggests that state welfare policies do not substantially affect marital status (Hoynes 1997; Moffitt 1998); moreover, welfare rules penalizing marriage were abolished in 37 states, and weakened in most others, under TANF (Urban Institute 2002). The assumption that child care subsidies do not strongly influence maternal education decisions seems relatively uncontroversial, although they might permit some young mothers to complete schooling (rather than entering employment). On the other hand, that state leave laws might affect the probability that a mother worked in the year before the birth seems more plausible, particularly for second and later children if leave laws increase long-term job retention and labor market attachment. The last two of these issues are explored further in our robustness checks.

Our main analysis uses a regression version of the DD strategy to model the net effect of each focal policy, holding constant other policies and potential confounding factors. This matters because, as discussed, the three policies tend to be correlated across states. Our estimating equation is

$$y_{is}=\sum_{p=1}^{3}\left\{\gamma^{p} Eligible_{is}^{p}+\theta^{p}\left(Policy_{s}^{p}\times Eligible_{is}^{p}\right)\right\}+X'_{is}\beta+\alpha_{s}+\varepsilon_{is} \quad (1)$$

where $y_{is}$ is the outcome of individual $i$ in state $s$; $Eligible_{is}^{p}$ a binary indicator equal to one if individual $i$ is in the treatment group for policy $p$; $Policy_{s}^{p}$ is the value of policy $p$ in state $s$; $X'_{is}$ is a vector of individual characteristics; $\alpha_s$ is a state-specific intercept; and $\varepsilon_{is}$ is an error term.

The classic pre/post DD estimator for longitudinal data assumes a common time trend in the outcome across policy and non-policy states. In the cross-sectional group DD framework used here, the analogous assumption is that differences in outcomes between eligible and ineligible individuals would be the same across states in the absence of policy.

The inclusion of state-fixed effects in (1) captures the influence of unobserved state-specific factors equally influencing the eligible and controls groups. These fixed effects are a generalization of the basic DD framework that incorporates policy main effects (by substituting $\sum_{p=1}^{3}\lambda^{p} Policy_{s}^{p}$ for $\alpha_s$ in equation 1), and remove the need to control for specific state characteristics such as average wage or unemployment rates. Hence the DD strategy allows *levels* of the outcomes to differ freely across states, in a no-policy world, but assumes that treatment versus control group differences are the same across policy and no-policy states. This is a strong assumption that could be violated if there are systematic differences in the composition of the eligible and control groups between policy and no-policy states. Such heterogeneity could arise, for example, because states differ in their tastes for inequality and as a result target a range of services towards poorer families, not just the focal policies of interest.

To shed light on the extent to which this assumption holds up in our data, we examined differences in mean characteristics between the eligible and control groups in policy and non-policy states for each of our three focal policies. Appendix Table A2 shows these eligible vs. control group differences for states with and without leave laws. For the most part, these differences in means are not significantly different, although some are (for example, mothers employed in the pre-birth period are 18.7 percentage points less likely to be Hispanic than mothers not employed pre-birth in states with leave laws vs. 12.9 percentage points less likely in states without leave laws). Appendix Tables A3 and A4 provide comparable information for our analysis of welfare work exemption policies and child care subsidy policies respectively; again, the majority of differences in means are not significantly different across policy and non-policy states. While these results are reassuring, we cannot rule out the possibility that some differences in outcomes would have occurred between eligible and control groups in policy and non-policy states even in the absence of the policies.

To address this potential heterogeneity, we first use the rich individual-level data in the ECLS-B to control for systematic differences in the relative composition of treatment and control groups across states. Available controls include standard demographic variables, information on the mother's experiences in childhood and later life, health-related behaviors, and attitudes towards motherhood. We also include proxies for potentially relevant attitudes and behaviors although, when doing so, we are careful to exclude factors that are potentially affected by post-birth employment (such as breast feeding), since they might absorb a portion of the true policy effect.[13]

Second, we conduct robustness checks where other state-level policies and characteristics, $Z_s$, are allowed to differentially affect the eligible and control groups. This is done by adding interaction terms $\sum_{p=1}^{3} \delta^p \left(Z_s \times Eligible_{is}^p\right)$ to equation (1). The number of characteristics that can be included in a single regression is limited because there are only 50 states in the U.S., and many of the variables are collinear. Therefore, we only included variables that a priori might be expected to affect disadvantaged groups differentially: the state unemployment rate and percent of workers covered by a union; census region; democratic party representation in state politics; and transfer program provisions – non-refundable or refundable Child and Dependent Care (CADC) tax credits, refundable state Earned Income Tax Credits (EITC), and generosity of Temporary Assistance for Needy Families (TANF) and Food Stamp benefits for a family of three. These are introduced into the econometric models sequentially (in four groups) to examine whether the estimated policy effects survive when outcomes of the treated groups are allowed to vary systematically with these variables between states.

We use linear probability (LP) for our binary work participation outcomes because coefficients on the interaction terms are more straightforward to interpret than probit or logit estimates (Ai and Norton, 2003); however, marginal effects estimated from corresponding probit models were generally quite similar. All estimates are weighted to adjust for disproportionate sampling, survey nonresponse, and noncoverage of the target population. The standard errors account for complex survey design.[14]

[13]The supplementary controls include: marital status/family type (4 variables), race/ethnicity (5 variables), maternal education (4 variables), mother's age at birth, number/age of resident siblings at 9 months (5 variables), mother is foreign born, mother's primary language is non-English, urbanicity (3 variables), father's education (5 variables), maternal welfare receipt during childhood (2 variables), education of mother's parents (4 variables), mother grew up in intact family, the mother's number of risky life events (4 variables), pre-pregnancy BMI (5 variables), maternal smoking and drinking before pregnancy (4 variables), and desired number of children (2 variables).

[14]Standard errors, when clustered at the state level, are generally somewhat *smaller* than those reported in the tables, which account for stratum and primary sampling unit clustering.

## 6. Effects on maternal work participation

We begin the empirical analysis by documenting effects of the policies on maternal work participation in the first nine months after giving birth. These results are both interesting in their own right and provide a useful check on the validity of our research design by allowing us to observe whether the timing of the estimated effects is consistent with the incentives created by the policies. Unfortunately, the second and third sweeps of the ECLS-B contain information only on current employment, and so do not allow us to construct detailed employment histories beyond the first nine months of the child's life. Some indication of longer-term employment effects using the data available, however, is provided in the following section.

At the baseline interview, mothers were asked if they had worked for pay during the last week and, if not working or on vacation/leave, whether they had done so since giving birth. If the answer to either question was yes, they were asked how old the child was when they first went back to work. Using this information, we constructed ten dichotomous work participation variables indicating whether the mother had started work by the time the child was 0 months old, 1 month old, and so on up to 9 months old. [15] Our multivariate analyses focus on three durations: work by child age two, four and nine months. The two-month cut-off is of interest because it is within the job-protected federal leave period mandated by the FMLA and the minimum infant welfare work exemptions in all but four states. Four-months is just after expiration of these provisions; the nine month threshold represents the end of the analysis period.

Figure 4 shows the average work participation rates over the nine months for mothers giving birth in the U.S. in 2001; vertical lines mark the three employment outcomes used subsequently. [16] Fifty-nine percent of mothers had worked by the time their child was nine months old, 47% had worked by four months of age, and 28% by two months, a high proportion by international standards.[17]

Figures 5 to 7 illustrate our DD strategy for the three policies by summarizing average differences in maternal work participation between states and groups at durations up to nine months. Appendix Figures A1 to A3 show the corresponding group profiles from which the difference estimates are calculated. Figure 5 examines state maternity leave laws. The two lines plot differences in the unconditional probability of post-birth employment between treatment and control groups of women in states with and without a leave law. The gap between the lines is the unadjusted DD estimate. Although both groups of mothers are less likely to work after birth in leave than non-leave states, there are clear differences between the treatment and control groups. The DD estimate is largest at two to three months (−6.9 and −6.3 percentage points), precisely when the leave entitlements should have the strongest effects. Treatment group mothers in leave states resume work faster than their counterparts in slightly later months (>3 months after birth), presumably as their leaves expire. Conversely, the gap among women in the control group (who did not work in the year before birth) becomes progressively larger over time, pointing to the role of other factors in leave states that discourage early work. This DD analysis suggests that leave laws increase the probability of working from six months after birth and later, consistent with explanations where job-protected leave increases long-term employment continuity.

[15]When mothers answered in weeks, four weeks is treated as equivalent to one calendar month.

[16]See Han, Ruhm, Waldfogel and Washbrook (2008) for a detailed description of early work participation patterns in the ECLS-B.

[17]For instance, data from the Millennium Cohort Survey indicate that only 7% of UK mothers are at work within two months.

Figure 6 plots the differences in the employment rates of single and married (or cohabiting) mothers in states with and without welfare work exemptions of ≥12 months. The differences are small for single mothers, but married women are more likely to work in states with lengthy exemptions, again suggesting that there are other forces in these states encouraging early work. As a result, the DD estimates – the difference between the two lines –suggest that long welfare work exemptions reduce the employment of single mothers. Emergence of the disincentive effect at three months is notable, since this is when the work requirements take effect in 16 of the 24 states with short (<12 month) exemptions.

Our policy indicator for child care subsidy expenditures is a continuous measure of federal and state CCDF expenditures per poor child under 6. In order to allow a graphical representation we plot the difference between mothers in states spending less than $1500 per year versus those spending $2500 or more. These results, shown in Figure 7, demonstrate that women in low educated families are much more likely to work after birth if they reside in states with relatively generous child care subsidies, while their control group counterparts appear to be slightly less likely to work post-birth. The DD estimate is substantial at all periods greater than one month after birth.

Table 2 presents corresponding multivariate DD estimates, controlling for both individual-level characteristics and state fixed-effects. The first three columns, showing findings for the full sample, indicate that the conclusions drawn from Figures 5 to 7 change little with the addition of controls. For instance, leave laws reduce the predicted probability of work by two months by 6.6 percentage points (24% of the sample mean), after which the gap in work participation between eligible women in leave and non-leave states falls sharply, becoming insignificant by four months post-birth. The association then reverses, such that leave laws increase the expected probability of employment at or before nine months by a marginally significant ($p<.10$) 4.3 points (7% of the sample mean). This pattern points to notably greater initial leave-taking but with no negative effects on employment later in the first year, consistent with prior research on parental leave extensions (e.g. Han et al. 2009; Hanratty and Trzcinski 2009).

The predicted effect of welfare work exemptions lasting a year or more emerges between two and four months post-birth, at precisely the time that short exemptions expire in 19 of the 24 control states. Long exemptions decrease the probability of maternal employment at or before four months by 6.9 percentage points (15% of the sample mean), a reduction sustained intact to nine months after the birth (at which point it repesents 12% of the sample mean); this finding is consistent with the limited prior research on the employment effects of this policy (Hill, 2007). Finally, an extra $1000 of spending on child care subsidies per poor child under age six increases post-birth employment of the treatment group by a modest 3 to 4 percentage points at all three periods of measurement. This result is not directly comparable to those in the prior literature (Blau and Tekin, 2007, for example report that being eligible for a subsidy increases employment by 13% whereas we are estimating the effect of an increase in state subsidy spending).

These results provide strong support for the possibility that we are picking up causal effects of the policies. In particular, if the results were due to confounding factors, we would not expect to see the sharp discontinuities in behavior around the dates that policy entitlements begin or expire that we do observe. It is noticeable that there is no discontinuity in child care subsidy eligibility in this period, as there is for the other two policies, and that we therefore see no substantial difference in the DD estimates of the effect of this policy in the short window between two and four months.

The last three columns of Table 2 checks the effects of restricting the sample to families participating in the preschool (age 4) wave of the survey, when we will observe the school readiness outcomes that are of ultimate interest. Doing so has little effect on the point estimates for leave laws and welfare work exemptions but the predicted magnitude of child care subsidies falls by a third and loses significance, suggesting that attrition is largest for those most affected by the subsidies. It is important to bear this in mind when interpreting our results in the following sections.

Table 3 summarizes the results of a series of simulations examining the effects of combinations of the three focal policies.[18] The estimates are constructed using the regression coefficients from columns 1 and 3 in Table 2, with the policy variables set to the specified values. We focus on participation rates by two months and nine months after birth. Outcomes are predicted for each individual and then averaged over the relevant sample to yield the expected participation rate. As two of the three focal policies are targeted at disadvantaged mothers, we also show results for single mothers (20% of the population) and those with a high school diploma or less (49% of the population).

Column 1 shows the predicted work participation rates under the policies prevailing in 2001 (the "status quo"). Column 2 provides corresponding estimates for a combination of policies designed to maximize work participation by nine months. This involves generous spending on child care subsidies (set to the 90$^{th}$ percentile of the existing state distribution), the abolition of lengthy infant welfare work exemptions, and the implementation of universal state leave laws, a combination similar to that observed in New Jersey, for example. The last of these choices is made because although state leave entitlements are predicted to decrease work immediately after birth, they are expected to increase it in the longer-term. The third combination is the reverse of the second, with child care subsidies set at the 10$^{th}$ percentile, state leave laws abolished, and all states having an infant work exemption of at least 12 months (similar to the policies observed in Texas).

Switching from the most to least employment-promoting policy package is anticipated to reduce the overall proportion of mothers who have worked by the time their child is nine months old by 7 percentage points (from 63 to 56%). Since 4.026 million children were born in the U.S. in 2001 (Martin et al., 2002), this change would increase the number of infants with non-working mothers at nine months by around 280,000 yearly. Much stronger effects are predicted for less advantaged mothers. The estimated nine month work participation rate falls by 13 percentage points for mothers with a high school education or less (affecting roughly 250,000 infants annually), and 15 percentage points for single mothers (affecting 120,000 infants). Notice, however, that the differences are not large for maternal work in the first two months after birth. The reason is that long welfare work exemptions have no impact during this period, while child care subsidies and leave laws have offsetting effects.

## 7. Other influences on child development

Effects of the focal policies on children depend not only on employment status, but also on how families are able to manage the twin demands of the labor market and child-rearing. For this reason we supplement our analysis of work participation with a range of intermediate factors that have been identified in the literature: mode of child care at nine months, the health inputs of breast feeding and well baby visits, maternal mental health, sensitivity of mother-child interactions and other measures of parenting, and longer-run maternal employment and household income. We explore child care mode by distinguishing between

[18]These estimates are calculated under the assumption that the policies have no effect on the control groups and that the treatment effects are uniform across states. We do not imply that these are the only policy combinations of potential interest.

three mutually exclusive and exhaustive categories of regular care at the date of the survey: parental care only (no regular non-parental care of any type); center-based care (day care centers, nursery schools, and preschools); and other non-center-based non-parental care (e.g. provided by relatives, babysitters, or other informal child care providers)[19].

Our breast feeding outcome is the duration in months (right-censored at nine months when the responses were recorded), although we also explored a range of alternatives (whether breast feeding was ever initiated, and whether it lasted beyond two, six, or nine months). An additional health-related behavior analyzed is the number of well-baby visits (for check-ups and vaccinations). Here we focus on the relatively extreme binary outcome of less than 4 visits by nine months (13% of the sample), as we expect the costs of failure to attend these key initial visits to be the highest.

Our measure of maternal mental health is obtained from the Center for Epidemiological Studies' Depression Scale (CES-D), a well-known 12-item self-completed checklist of depressive symptoms. Items are summed and normalized to a z-score (with mean zero and standard deviation one), such that the coefficients of interest can be interpreted as effect sizes.

A unique feature of the ECLS-B is its direct observation and high-quality measures of the sensitivity and responsiveness of maternal parenting behavior obtained through the Nursing Child Assessment Teaching Scale (NCATS), administered at nine months, and the Two Bags assessment, administered at four years. Both assessments are obtained from video-tapes made of the mother and child engaging in a semi-structured task (like playing with various toys) for around five to ten minutes during the survey interview. Mothers' parenting skills, as revealed by the tape, were rated by trained coders on dimensions such as intrusiveness, detachment, and positive regard. As above, we normalize the raw scores on these parenting sub-scales to have a zero mean and a standard deviation of one. We also ran supplementary models using a number of maternal self-reported measures (such as the frequency the child is read to and told stories).

Finally, we examine longer-run financial and labor market consequences of the policies through models on a binary indicator for whether the mother was employed at the date of the age 4 survey and on the log of total gross household income measured at the same wave.

Table 4 presents our results for a range of outcomes measured at the nine month survey, with the samples restricted to children who also participated in the preschool survey wave. The first three columns present findings for binary indicators of the main mode of child care. The estimates in column 1 mirror our findings for work participation. Children of eligible mothers are significantly less likely to be cared for solely by their parents in states with leave laws or high child care subsidies, and more likely to experience parent-only care where there are long welfare work exemptions. Moreover, magnitudes of the point estimates are virtually identical to (but opposite in sign from) the changes in work participation shown in Table 2, indicating that there is virtually a one-to-one offsetting relationship between maternal work and parent-only child care. Interestingly, the mode of non-parental care chosen by mothers induced to work differs strongly across policies. As shown in columns 2 and 3, mothers encouraged to work by state leave laws or welfare work requirements appear overwhelmingly to use informal non-center-based care providers, with no discernible increase in formal center-based care. Conversely, maternal participation increases induced by higher child care subsidies are associated with marginally greater use of center-based but

[19]The variable is defined according to the primary mode of care. When parents use multiple modes of care, the primary type is the one used for the greatest number of hours.

not informal care, which makes sense given that some informal care arrangements are not eligible for subsidy. The increase in center care is small in absolute terms (2.8 percentage points) but represents a 33% increase in enrollment relative to the base enrollment rate (8.5% for children this age).[20] These differing responses may lead to heterogeneous effects if, for example, center-based care tends to be of higher quality than other modes.

We find no evidence that any of the policies affected the duration of breast feeding (see column 4). The associated coeffieients are not statistically significant and, on the basis of the confidence intervals (shown in the Table), we can rule out large positive effects on breast-feeding. For example, we can exclude increases larger than .6 months in the duration of breast-feeding associated with the state parental leave laws. The lack of effects on breast-feeding is surprising given the association usually found between maternal time at home and breast-feeding. For example, Baker and Milligan's (2008) estimates indicated that each one month increase in leave was associated with 1/3 month increase in breast-feeding duration among eligible women. Being able to rule out effects larger than .6 months, in our data, is consistent with the relatively brief additional leave provided to the average woman by state leave laws in the U.S, given that the typical state law covers relatively few additional women and/or extends the federal leave period by just a few weeks.[21]

Column 5 examines the probability that a child participated in fewer than four well-baby visits. Eligible mothers in leave states were a statistically significant 4.2 percentage points less likely to fall below this threshold than equivalent mothers in non-leave states, consistent with parental leave providing parents with more time for important health-related investments in their infants, particularly in the first few months of life when the schedule of visits is most intensive. No corresponding effects are observed for welfare work exemptions or child care subsidies, perhaps because these policies affect employment slightly later in the first year when visits are less intensive.

Column 6 examines normalized scores on the CES-D scale. None of the estimated policy effects on maternal depression are significant, and the confidence intervals allow us to rule out meaningful effects. Even for parental leave laws, where the point estimate is suggestive of an increase in depressive symptoms, we can rule out an effect larger than .215 standard deviations. Column 7 shows results for the NCATS parenting score. Again, the estimated policy effects are statistically insignificant and they are sufficiently precise to exclude economically meaningful effects.[22] These results are similar to those obtained by Baker and Milligan (2008 and 2010a), who found respectively that increases in parental employment associated with leave laws were not accompanied by changes in maternal mental health or parenting shortly after the birth.

Table 5 skips ahead three years to intermediate outcomes at the time of the preschool survey, when children are roughly age four. Column 1 examines maternal employment status. The positive effects of state leave laws on employment at nine months appear sustained in the longer term – eligible mothers are 5.3 percentage points (a 9% increase relative to the sample mean) more likely to be employed at the preschool survey in leave states than non-leave states, consistent with leave laws promoting job continuity in ways that

[20]Previous research (Magnuson, Meyers and Waldfogel 2007) also indicates that child care subsidies increase attendance of low-income children in formal care. That study, focused on 3 and 4 year old children, found that an increase in subsidy spending of $1,000 per child in the state was associated with an 18 percentage point increase in enrollment in center care (a 44% increase relative to the base enrollment rate of 41% in their sample of 3 and 4 year olds).

[21]Our analysis of alternative breast feeding indicators similarly failed to reveal evidence of policy effects. This is consistent with the low correlation (0.083), in our data, between months until work and months of breast feeding (with both outcomes censored at nine months).

[22]Additional analyses of mother-reported parenting variables such as frequency of reading to the child (available on request) revealed no policy effects that were significant or moderate in size.

have lasting effects (consistent with cross-country results obtained by Ruhm, 1998). Lengthy welfare work exemptions and child care subsidies, which respectively reduced and increased employment at 9 months, have smaller and no longer significant effects on employment at 4 years. Together these results suggest that leave policies affecting mothers of newborns have lasting effects on maternal employment, but that effects of early welfare work exemptions and child care subsidies attenuate over time.[23]

The policy effects on child care mode, shown in columns 2 to 4, are also attenuated. In part this reflects age-related changes in care arrangements. By the age of four, 58% of children are in center-based care, compared with only 9% at nine months, while the proportion cared for solely by parents falls from 50% to 20%.

With regard to maternal depression, shown in column 5, again the point estimate for state leave laws, while not statistically significant, is suggestive of an increase in depression, although we can rule out an effect larger than .205 standard deviation. The results in column 6 indicate that child care subsidies are associated with significantly higher parenting quality (and the point estimate for welfare work exemptions is also positive, with a confidence interval that includes an effect of up to .264 standard deviations).

The final column of Table 5 assesses the long-term income consequences of our three focal policies. None has a significant effect on household income four years post-birth, although the confidence interval for parental leave laws includes up to a 17.1% increase in household income. Whether such a change would be large enough to affect child outcomes remains to be seen, although this possibility is worth investigating since some research suggests that positive effects of maternal employment on children occur through the reduced financial stress and greater purchased inputs associated with maternal earnings (Brooks-Gunn et al., 2010).

## 8. Child outcomes

The high quality of the available measures of children's cognitive ability and behavior problems is one advantage of the ECLS-B data. The Language, Literacy and Mathematics assessments were developed specifically for the study. The Language measure assesses verbal ability and spoken vocabulary; the Literacy assessment taps abilities such as letter recognition, letter sounds, recognition of simple words, and phonological awareness; and the Mathematics appraisal captures number sense, geometry, counting, operations, and patterns. Behavior problem scores are derived from maternal responses to 24 statements about the child's behavior (many drawn from the Preschool and Kindergarten Behavioral Scales – Second Edition (PKBS-2)). Items are scored from 0 to 4, with higher scores indicating more problematic behavior; the overall score is the average of the 24 items (alpha = 0.86). The Conduct Problems score averages across a sub-set of five items relating to anti-social and aggressive behaviors, and the Inattention score is the average of five items relating to impulsivity and the ability to concentrate (alphas = 0.76 and 0.70 respectively). The cognitive and behavior scores are standardized to mean zero and standard deviation one, using the survey weights.

Table 6 shows that we find no significant effects of our focal policies on the cognitive or behavioral outcomes. However, several intriguing patterns emerge. First, state leave laws are uniformly associated with lower cognitive scores and more behavioral problems. We can rule out large negative effects on cognitive scores (the lower bound estimates for language,

[23]We also estimated models of maternal employment status at the age two survey wave, and found results that were broadly consistent with findings from the preschool wave.

literacy, and math are .167, .183, and .130 standard deviations respectively) and behavioral problems (the upper bound estimates are .231, .191, and .179 for overall behavior problems, conduct problems, and inattention problems) but nevertheless this pattern of results makes it difficult to justify these laws on the basis of *improvements* in child outcomes (although there certainly could be other benefits to children or adults).We can only speculate as to the reason for this pattern of results, which future research should explore. Under the assumption that children benefit from greater maternal time in the first year, these results raise the possibility that the positive impacts of greater leave-taking in the first months after a birth might be offset by negative consequences of employment increases observed later in the first year. In addition, leave policies have potentially heterogeneous effects on different groups of women and whether they benefit or harm children likely depends on which specific women are affected. Second, lengthy welfare work exemptions have indeterminate effects on the cognitive scores but might be associated with fewer problem behaviors (the confidence intervals indicate that increases in behavior problems are unlikely to be larger than .09 standard deviation and that decreases might be as large as .25 standard deviations), raising possible concern about the consequences for children of reducing the length of work exemptions under welfare reform. Finally, more generous child care subsidies do not have meaningful effects on either cognitive scores or behavior problems (we can rule out an increase of more than .06 standard deviations in behavior problems, and a decrease of more than .075 in cognitive scores). This result is reassuring given prior findings in the U.S. (Herbst and Tekin, 2010, in press) and Canada (Baker, Gruber, and Milligan, 2008) linking child care subsidies with poorer outcomes for children.

## 9. Robustness checks

The estimates of the effects of the policies on early work participation in Table 2 appear plausible as causal effects in terms of their magnitudes, patterns, and direction, and perhaps provide the strongest evidence in favor of our research design. The results of a series of robustness checks, summarized next, allow us to be more confident in our interpretation.

Mothers about to become pregnant for the second (or later) time might be more likely to work in states with a leave law than in those without (because leave entitlements increase job continuity), implying differences in the treatment groups across states with and without leave laws. This was addressed above by including controls for child parity. As a further check, we estimated models on the sub-sample of mothers having first births only, since pre-birth employment of first-time mothers is unlikely to be much affected by the presence of a leave law. The state leave law effects on early work participation have the same pattern as our preferred estimates, with negative coefficients at two months post-birth becoming positive by nine months. In terms of the intermediate outcomes, the leave law coefficients remain insignificant as in the full sample, with the exception that the positive association with well baby visits holds also for first births. The sign of the effects on parenting quality at nine months and at four years reverses to become positive, however, raising the possibility that the negative effects found in the full sample may reflect unobserved heterogeneity among women having a second birth or more. As before, the effects of leave laws on child outcomes are small and insignificant in the first birth sample, with the exception of a single negative coefficient on math scores that becomes marginally significant at the 10% level.

We estimated models for sub-samples of mothers aged 25 or over, with some college or more, and for married mothers.[24] Restricting the sample to those 25 or over removes young low-educated mothers who may use child care subsidies to release time for education rather than work. Consistent with this, child care subsidies have stronger and more significant

[24]Not all policy effects can be estimated for all sub-samples due to a dropping of entire eligible or control groups.

effects on work participation and care mode for the older subsample at all three dates in the nine month post-birth period, and these effects remain significant even at the age 4 survey.

Other research (Han, Ruhm and Waldfogel 2009) shows that advantaged mothers are more likely than their counterparts to be able to use the unpaid maternity leaves provided by most state laws. Our results support this. Compared with the full sample 6.6 percentage point reduction in the probability of work by two months associated with a state leave law, the estimated decreases are 7.8 points for married mothers and 11.3 points among the college educated (both significant at the 1% level). The predicted effects on work by nine months are positive for both groups, and of similar magnitude to the 4.3 percentage point estimate from the full sample, although not significant (because of reduced sample sizes). We find little evidence of larger effects among the advantaged groups for other outcomes although the estimated consequences of leave laws on maternal depression at nine months are significantly negative for married mothers.

We examined whether the DD estimates above reflect the influence of correlated state conditions by including four sets of additional controls: the state unemployment rate and the percent of workers covered by a union; census region; democratic party representation in state politics; and transfer program provisions (non-refundable or refundable Child and Dependent Care (CADC) tax credits, refundable state Earned Income Tax Credits (EITC), and generosity of Temporary Assistance for Needy Families (TANF) and Food Stamp benefits (for a family of three). In each case, these variables were interacted with the eligibility indicators for the three focal policies. The results are generally insensitive to these supplementary controls. The biggest change occurred on the estimated effects of state leave laws when other transfer program provisions were allowed to drive differences in outcomes between treatment and control individuals. Although the negative effect of leave laws on work by two months remained, the positive employment effect at nine months or more disappeared and the association of leave laws with child outcomes became uniformly beneficial (but not statistically significant). This raises the possibility that leave laws reduce participation in the very early post-birth period in ways that are beneficial for children but they tend to be confounded with other state policies increasing employment in the longer term and with less positive impacts on children’s development.

## 10. Discussion

This paper considers how three types of public policies that potentially influence the work and child care decisions of mothers with infants – parental leave laws, welfare work exemptions, and child care subsidies – affect the timing of work participation after birth, and a range of intermediate and child outcomes during the subsequent four years. The policies affect early maternal work participation, in ways that would be expected given the incentives they provide and collectively they strongly influence patterns of work by mothers after birth. However, we do not obtain evidence of significant consequences for child well-being.

Our results for parental leave policies are consistent with the findings of recent research for Canada (Baker and Milligan, 2008, 2010a and b), Germany (Dustmann Schönberg, 2008), Denmark (Rasmussen, 2010), and Sweden (Liu and Skans, 2010) that fail to uncover beneficial effects of parental leave expansions for child outcomes, or find only selective effects. In particular, our findings echo those of Baker and Milligan (2010b) who find no significant effects of parental leave extensions on child developmental outcomes at preschool in spite of significant effects of such policies on maternal leave-taking and breast-feeding in the first year of life. This congruence of results is interesting given how markedly Canadian parental leave policies differ from those in the U.S. Prior to the extension of job-protected leave Canadian mothers already remained at home for an average of six months after birth, with the policy reform inducing an increase in this duration to nine months. Such effects might be quite different from those of U.S. state leave laws, which generally extend time at home in the first few months after a birth. To the extent that maternal employment early in the first year is most consequential for development (see e.g. Brooks-Gunn et al., 2010), U.S. laws might be expected to have a larger effect on child outcomes. However, such effects may be muted because U.S. policies mainly provide unpaid leave (in contrast to paid leave in Canada).

Our findings for the welfare work exemptions are more novel, given the limited prior research on them, but are consistent with expectations. Women exempted from welfare work requirements are less likely to work during the period of exemption. But again, developmental benefits are not apparent. Finally, with regard to child care subsidies, we find that greater subsidy spending is associated with higher rates of employment and use of center-based care, consistent with research on families with older children, but with no apparent developmental benefit or harm to the children. This latter result stands in contrast to prior research on child care subsidies which has tended to find negative effects on child outcomes. But prior studies are not readily comparable. For instance, the Quebec preschool program analyzed by Baker et al. (2008) was universal, single mothers were excluded from their analysis and, as they recognize, their study may partly be capturing short-run adjustments to changes in family circumstances. In contrast, the Child Care and Development Fund (CCDF) examined here is a long-running child care subsidy program targeted only to low-income families, many of whom are female-headed. Moreover, the U.S. subsidy program we examined seems to have mostly moved children into center-based care, which in the U.S. context is often of higher quality than the informal care alternatives families might otherwise use.

That said, several caveats should be kept in mind when interpreting our results. First, although the rich data, and large, nationally representative sample available to us, allowed us to be confident in ruling out large consequences of these policies for child well-being, we can be less confident about our ability to detect smaller effects. For example, in subgroup analyses, the point estimates consistently indicate that child care subsidies have positive cognitive and behavioral effects for four-year old children of less educated parents, but with the reduced sample size for that subgroup, none of the parameters approaches statistical significance. Second, it seems possible that the policies implemented in the United States (brief unpaid parental leaves, child care subsidies that are limited to a portion of low-income families, and welfare work exemptions that affect relatively few women) may not be strong enough to induce sizable changes in child well-being, while leaving open the possibility that more expansive policies might have large impacts. Third, we are not able to identify the specific eligibility of individuals in our data for the policies. This may generally lead to an understatement of the treatment effects, with supporting evidence recently provided, in a European context, by Carneiro et al. (2010) who show that the long-term benefits of Norwegian parental leave expansions are underestimated when ineligible children are included in the analysis. Finally, we are not able to examine long-run outcomes (e.g. completed education, labor market outcomes, adult psychological well-being), and it remains possible that the short-run and medium-term measures that we focus upon are less than fully informative in this regard. In addition, the policies could yield other benefits or costs (such as effects on the labor market situation of mothers) that are not focused upon here.

## Acknowledgments

Note: We gratefully acknowledge funding support from NICHD and the Leverhulme Trus

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# Appendix

## Appendix Table A1

Full details of state leave policies

| State | State leave law provisions | Welfare work exemptions (mths) | CCDF spending ($) |
|---|---|---|---|
| Alabama | - | 3 | 1183 |
| Alaska | - | 12 | 2776 |
| Arizona | - | none | 961 |
| Arkansas | - | 3 | 743 |
| California | TDI | 3 | 1537 |
| Colorado | - | 12 | 1513 |
| Connecticut | Max leave 16 weeks, min req hours 20 | 12 | 4938 |
| Delaware | - | 3 | 3895 |
| District of Columbia | Max leave 16 weeks, min req hours 20, no firm size req | 12 | 3864 |
| Florida | - | 3 | 1530 |
| Georgia | - | 12 | 1345 |
| Hawaii | TDI, 6 mths min tenure, no min hours req | 6 | 2355 |
| Idaho | - | none | 1391 |
| Illinois | - | 12 | 2203 |
| Indiana | - | 3 | 1795 |
| Iowa | - | 3 | 2954 |
| Kansas | - | 12 | 1745 |
| Kentucky | - | 12 | 1031 |
| Louisiana | - | 12 | 1188 |
| Maine | No min hours req, min req firm size 15 | 12 | 2005 |
| Maryland | - | 12 | 2452 |
| Massachusetts | No min tenure, no min hours req, no firm size req | 24 | 3300 |
| Michigan | - | 3 | 746 |
| Minnesota | No min tenure, min req hours 20, min req firm size 15 | 12 | 2967 |
| Mississippi | - | 12 | 973 |
| Missouri | - | 12 | 1690 |
| Montana | No min tenure, no min hours req, no firm size req | none | 1888 |
| Nebraska | - | 3 | 3913 |
| Nevada | - | 12 | 874 |
| New Hampshire | - | 24 | 4181 |
| New Jersey | TDI, min req hours 20 | 3 | 3201 |
| New Mexico | - | 12 | 1180 |
| New York | TDI | 3 | 2098 |
| North Carolina | - | 12 | 2178 |
| North Dakota | - | 4 | 1336 |
| Ohio | - | 12 | 2144 |

| State | State leave law provisions | Welfare work exemptions (mths) | CCDF spending ($) |
|---|---|---|---|
| Oklahoma | - | 3 | 1465 |
| Oregon | Max leave 24 weeks, 6 mths min tenure, no min hours req, min req firm size 25 | 3 | 1061 |
| Pennsylvania | - | 12 | 1875 |
| Rhode Island | TDI, max leave 13 weeks | 12 | 4515 |
| South Carolina | - | 12 | 974 |
| South Dakota | - | 3 | 1357 |
| Tennessee | Max leave 17 weeks, no firm size req | 4 | 1719 |
| Texas | - | 12 | 976 |
| Utah | - | none | 1168 |
| Vermont | Min req firm size 10 | 24 | 3392 |
| Virginia | - | 18 | 1862 |
| Washington | - | 4 | 2890 |
| West Virginia | - | 12 | 1336 |
| Wisconsin | Min req hours 20 | 3 | 2646 |
| Wyoming | - | 3 | 1336 |

*Notes.* The Federal FMLA applies in all states and provides for 12 weeks of unpaid leave to employees working at least 25 hours per week in the previous year for firms with 50 or more employees. The entries in the table note whether the state leave law relaxes the maximum leave period, the minimum tenure period, the minimum weekly hours requirement or the minimum firm size requirement for qualification. In addition states with Temporary Disability Insurance (TDI) laws provide some period of paid parental leave.

CCDF spending expressed as thousands of dollars per poor child under 6 in 2001 dollars.

## Appendix Table A2

Eligible-control group differences by state leave laws

| | Mean for eligible group (employed prebirth) – mean for control group (not employed prebirth) | | |
|---|---|---|---|
| | (1) States with a leave law | (2) States without a leave law | (1) and (2) sig diff? |
| White non-Hispanic | 0.170 | 0.122 | |
| Black non-Hispanic | 0.029 | 0.010 | |
| Hispanic | −0.187 | −0.129 | * |
| Asian | −0.013 | −0.010 | |
| Other race/ethnicity | 0.002 | 0.007 | |
| Single | 0.007 | −0.006 | |
| No parent with high school | −0.209 | −0.165 | * |
| Mother: Less than high school | −0.258 | −0.197 | ** |
| Mother: High school | 0.031 | 0.014 | |
| Mother: Some college | 0.097 | 0.117 | |
| Mother: BA degree | 0.051 | 0.047 | |
| Mother: More than BA degree | 0.078 | 0.018 | *** |
| Mother’s age | 1.474 | 1.102 | |
| No resident siblings at 9 months | 0.100 | 0.141 | |
| 1 resident sibling at 9 months | −0.008 | −0.029 | |

| | **Mean for eligible group (employed prebirth) – mean for control group (not employed prebirth)** | | |
|---|---|---|---|
| | **(1) States with a leave law** | **(2) States without a leave law** | **(1) and (2) sig diff?** |
| More than 1 resident sibling at 9 months | −0.091 | −0.112 | |
| Sibling under 3 in household at 9 months | −0.073 | −0.117 | * |
| Sibling age 3 or 4 in household at 9 mths | −0.035 | −0.058 | |
| Mother foreign born | −0.217 | −0.143 | ** |
| Mother's primary language non-English | −0.135 | −0.083 | ** |
| Urban cluster | −0.010 | −0.010 | |
| Rural area | 0.044 | 0.036 | |
| Father: Less than high school | −0.162 | −0.098 | *** |
| Father: High school | 0.004 | 0.022 | |
| Father: Some college | 0.075 | 0.053 | |
| Father: BA degree | 0.048 | 0.046 | |
| Father: More than BA degree | 0.028 | −0.018 | ** |
| Mother received welfare in childhood | −0.003 | −0.002 | |
| Mother's mother some college or more | 0.133 | 0.084 | * |
| Mother's father some college or more | 0.139 | 0.076 | ** |
| Mother's family intact til 16 | −0.005 | −0.005 | |
| 0 risky life events ever happened | −0.015 | −0.011 | |
| 1 risky life event ever happened | 0.002 | −0.001 | |
| 2 to 6 risky life events ever happened | 0.014 | 0.016 | |
| Pre-pregnancy BMI: Under weight | −0.028 | −0.012 | |
| Pre-pregnancy BMI: Normal | −0.001 | 0.010 | |
| Pre-pregnancy BMI: Over weight | 0.041 | 0.014 | |
| Pre-pregnancy BMI: Obese | 0.028 | 0.007 | |
| Ever smoked > 100 cigarettes | 0.072 | 0.086 | |
| Alcohol pre-pregnancy: Never | −0.160 | −0.171 | |
| Alcohol pre-pregnancy: < 4 drinks pwk | 0.110 | 0.131 | |
| Alcohol pre-pregnancy: >=4 drinks pwk | 0.049 | 0.040 | |
| Ideal number of children in whole life | −0.200 | −0.240 | |
| Child is female | −0.023 | 0.007 | |
| Birth weight normal (>2500g) | −0.001 | 0.000 | |
| Birth weight low (1500–2500g) | 0.000 | −0.002 | |
| Birth weight very low (<1500g) | 0.001 | 0.002 | |
| Multiple birth | 0.006 | 0.003 | |

Note.

*** p<.01,

** p<.05,

* p<.1.

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## Appendix Table A3

Eligible-control group differences by state welfare work exemptions

| | Mean for eligible group (single mothers) – mean for control group (couple mothers) | | (1) and (2) sig diff? |
|---|---|---|---|
| | (1) States with exemption>=12 mths | (1) States with exemption<12 mths | |
| White non-Hispanic | −0.366 | −0.270 | ** |
| Black non-Hispanic | 0.405 | 0.277 | *** |
| Hispanic | −0.023 | 0.008 | |
| Asian | −0.024 | −0.029 | |
| Other race/ethnicity | 0.007 | 0.013 | |
| Employed prebirth | −0.001 | −0.001 | |
| No parent with high school | 0.327 | 0.313 | |
| Mother: Less than high school | 0.240 | 0.203 | |
| Mother: High school | 0.102 | 0.071 | |
| Mother: Some college | −0.046 | −0.056 | |
| Mother: BA degree | −0.183 | −0.136 | ** |
| Mother: More than BA degree | −0.113 | −0.081 | ** |
| Mother's age | −5.026 | −4.573 | |
| No resident siblings at 9 months | 0.099 | 0.176 | ** |
| 1 resident sibling at 9 months | −0.049 | −0.128 | ** |
| More than 1 resident sibling at 9 months | −0.050 | −0.048 | |
| Sibling under 3 in household at 9 months | −0.002 | −0.036 | |
| Sibling age 3 or 4 in household at 9 mths | −0.076 | −0.069 | |
| Mother foreign born | −0.088 | −0.104 | |
| Mother's primary language non-English | −0.068 | −0.052 | |
| Urban cluster | 0.025 | −0.017 | |
| Rural area | −0.009 | −0.012 | |
| Father: Less than high school | −0.212 | −0.218 | |
| Father: High school | −0.224 | −0.235 | |
| Father: Some college | −0.252 | −0.269 | |
| Father: BA degree | −0.181 | −0.162 | |
| Father: More than BA degree | −0.131 | −0.116 | |
| Mother received welfare in childhood | 0.102 | 0.109 | |
| Mother's mother some college or more | −0.112 | −0.093 | |
| Mother's father some college or more | −0.217 | −0.159 | ** |
| Mother's family intact til 16 | −0.258 | −0.219 | |
| 0 risky life events ever happened | −0.259 | −0.216 | |
| 1 risky life event ever happened | 0.139 | 0.142 | |
| 2 to 6 risky life events ever happened | 0.12 | 0.077 | ** |
| Pre-pregnancy BMI: Under weight | 0.021 | 0.036 | |
| Pre-pregnancy BMI: Normal | −0.047 | 0.005 | |
| Pre-pregnancy BMI: Over weight | −0.021 | −0.059 | |

| | Mean for eligible group (single mothers) – mean for control group (couple mothers) | | (1) and (2) sig diff? |
|---|---|---|---|
| | (1) States with exemption>=12 mths | (1) States with exemption<12 mths | |
| Pre-pregnancy BMI: Obese | 0.046 | 0.021 | |
| Ever smoked > 100 cigarettes | 0.052 | 0.059 | |
| Alcohol pre-pregnancy: Never | 0.062 | 0.043 | |
| Alcohol pre-pregnancy: < 4 drinks pwk | −0.075 | −0.062 | |
| Alcohol pre-pregnancy: >=4 drinks pwk | 0.013 | 0.017 | |
| Ideal number of children in whole life | −0.3 | −0.223 | |
| Child is female | 0.047 | −0.005 | |
| Birth weight normal (>2500g) | −0.029 | −0.026 | |
| Birth weight low (1500–2500g) | 0.024 | 0.022 | |
| Birth weight very low (<1500g) | 0.005 | 0.005 | |
| Multiple birth | −0.009 | −0.007 | |

Note.

*** p<.01,

** p<.05,

* p<.1.

## Appendix Table A4

Eligible-control group differences by state CCDF spending

| | Mean for eligible group (no parent with HS) – mean for control group (parent with HS or more) | | | Tests for sig diff | | |
|---|---|---|---|---|---|---|
| | State CCDF spending: | | | | | |
| | (1) > $2500 | (2) $1500–2500 | (3) < $1500 | (1) vs (2) | (2) vs (3) | (1) vs (3) |
| White non-Hispanic | −0.426 | −0.373 | −0.353 | | | |
| Black non-Hispanic | 0.095 | 0.070 | 0.096 | | | |
| Hispanic | 0.314 | 0.337 | 0.278 | | | |
| Asian | −0.004 | −0.033 | −0.014 | *** | *** | |
| Other race/ethnicity | 0.021 | −0.001 | −0.008 | | | ** |
| Single | 0.410 | 0.305 | 0.316 | * | | * |
| Employed prebirth | −0.235 | −0.223 | −0.252 | | | |
| Mother: Less than high school | 0.914 | 0.891 | 0.906 | | | |
| Mother: High school | −0.254 | −0.256 | −0.298 | | * | |
| Mother: Some college | −0.283 | −0.323 | −0.362 | | ** | *** |
| Mother: BA degree | −0.236 | −0.188 | −0.164 | * | | ** |
| Mother: More than BA degree | −0.141 | −0.123 | −0.082 | | *** | ** |
| Mother’s age | −5.410 | −4.604 | −4.358 | | | |
| No resident siblings at 9 months | 0.061 | 0.029 | 0.041 | | | |
| 1 resident sibling at 9 months | −0.074 | −0.113 | −0.075 | | | |
| More than 1 resident sibling at 9 months | 0.013 | 0.084 | 0.034 | | | |

| | Mean for eligible group (no parent with HS) – mean for control group (parent with HS or more) | | | Tests for sig diff | | |
|---|---|---|---|---|---|---|
| | State CCDF spending: | | | | | |
| | (1) > $2500 | (2) $1500–2500 | (3) < $1500 | (1) vs (2) | (2) vs (3) | (1) vs (3) |
| Sibling under 3 in household at 9 months | 0.027 | 0.015 | −0.012 | | | |
| Sibling age 3 or 4 in household at 9 mths | −0.089 | −0.044 | −0.005 | | | ** |
| Mother foreign born | 0.248 | 0.244 | 0.230 | | | |
| Mother’s primary language non-English | 0.201 | 0.149 | 0.094 | | | ** |
| Urban cluster | −0.018 | 0.019 | −0.070 | | ** | |
| Rural area | −0.012 | −0.008 | −0.009 | | | |
| Father: Less than high school | 0.407 | 0.479 | 0.443 | | | |
| Father: High school | −0.236 | −0.212 | −0.247 | | * | |
| Father: Some college | −0.235 | −0.261 | −0.276 | | | ** |
| Father: BA degree | −0.197 | −0.179 | −0.141 | | ** | ** |
| Father: More than BA degree | −0.148 | −0.132 | −0.094 | | *** | ** |
| Mother received welfare in childhood | 0.102 | 0.075 | 0.066 | | | |
| Mother’s mother some college or more | −0.322 | −0.304 | −0.285 | | | |
| Mother’s father some college or more | −0.327 | −0.323 | −0.316 | | | |
| Mother’s family intact til 16 | −0.178 | −0.161 | −0.103 | | | |
| 0 risky life events ever happened | −0.143 | −0.074 | −0.188 | | *** | |
| 1 risky life event ever happened | 0.069 | 0.023 | 0.106 | | ** | |
| 2 to 6 risky life events ever happened | 0.050 | 0.046 | 0.072 | | | |
| Pre-pregnancy BMI: Under weight | 0.051 | 0.015 | 0.000 | | | ** |
| Pre-pregnancy BMI: Normal | −0.043 | −0.081 | −0.018 | | | |
| Pre-pregnancy BMI: Over weight | −0.016 | 0.001 | 0.006 | | | |
| Pre-pregnancy BMI: Obese | −0.010 | 0.001 | −0.006 | | | |
| Ever smoked > 100 cigarettes | −0.006 | −0.013 | 0.013 | | | |
| Alcohol pre-pregnancy: Never | 0.267 | 0.196 | 0.147 | | | ** |
| Alcohol pre-pregnancy: < 4 drinks pwk | −0.237 | −0.181 | −0.126 | | * | ** |
| Alcohol pre-pregnancy: >=4 drinks pwk | −0.031 | −0.017 | −0.021 | | | |
| Ideal number of children in whole life | −0.225 | 0.059 | −0.119 | *** | ** | |
| Child is female | 0.016 | −0.001 | 0.024 | | | |
| Birth weight normal (>2500g) | −0.014 | −0.006 | −0.020 | | | |
| Birth weight low (1500–2500g) | 0.003 | 0.005 | 0.017 | | | |

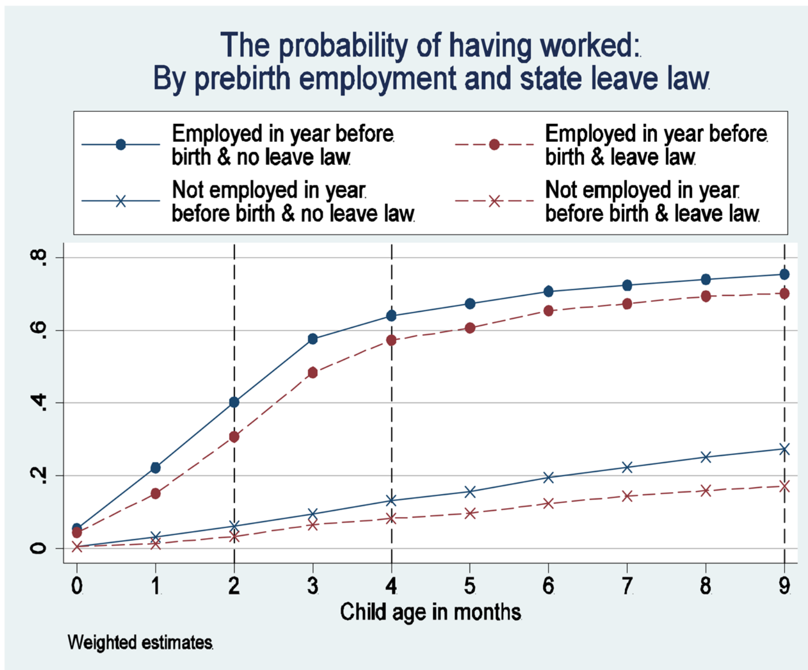
| | Mean for eligible group (no parent with HS) – mean for control group (parent with HS or more) | | | Tests for sig diff | | |
|---|---|---|---|---|---|---|
| | State CCDF spending: | | | | | |
| | (1) > $2500 | (2) $1500–2500 | (3) < $1500 | (1) vs (2) | (2) vs (3) | (1) vs (3) |
| Birth weight very low (<1500g) | 0.011 | 0.001 | 0.003 | *** | | * |
| Multiple birth | −0.024 | −0.013 | −0.004 | ** | | *** |

Note.

*** p<.01,

** p<.05,

* p<.1.




**Appendix Figure A1.**

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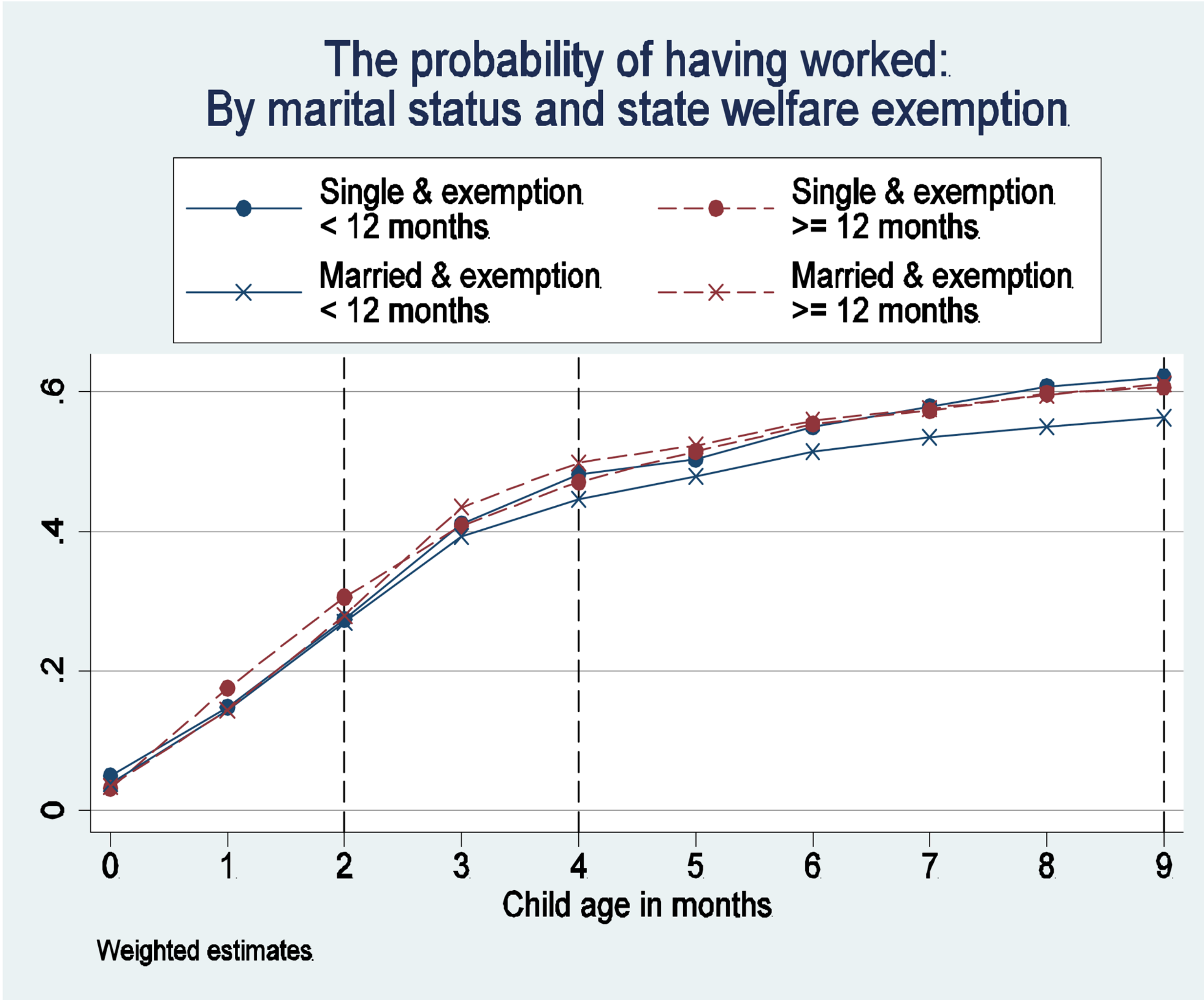
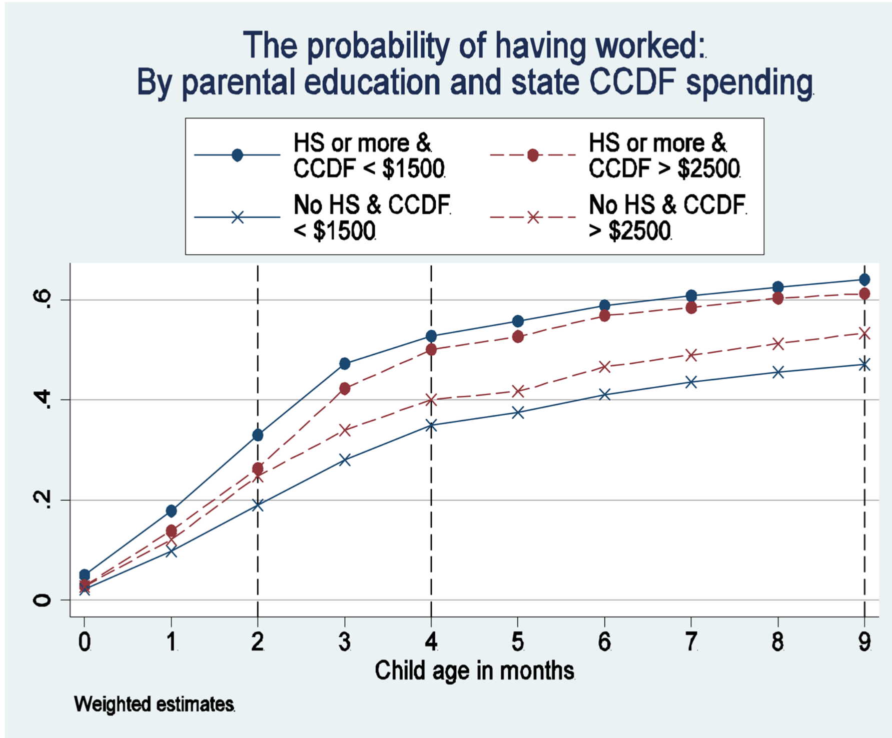



**Appendix Figure A2.**




**Appendix Figure A3.**

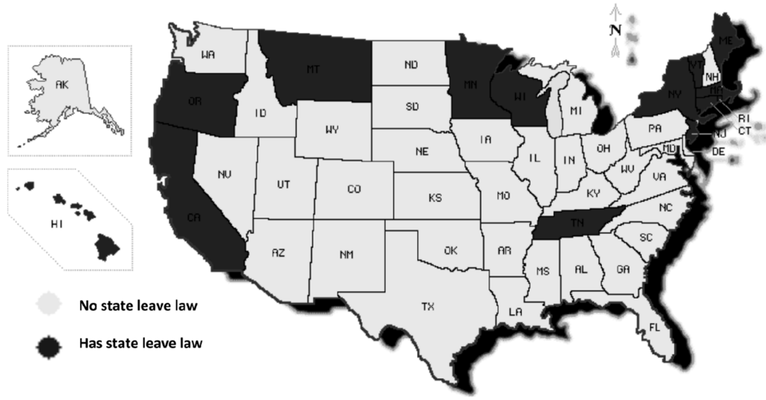



**Figure 1.**
State leave laws more generous than the federal minimum in 2001

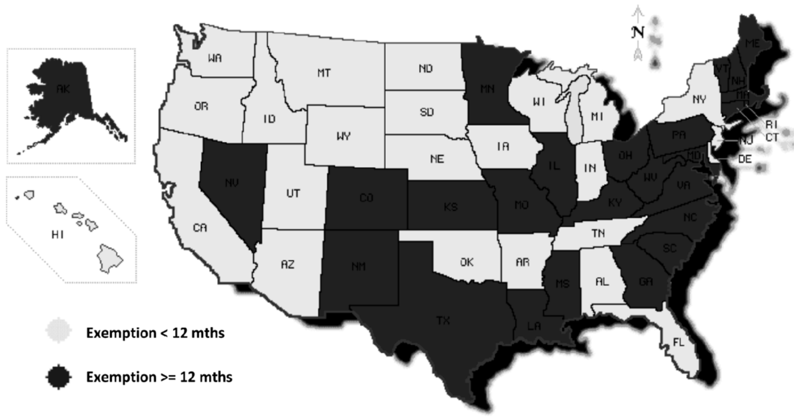



**Figure 2.**
State infant work exemptions

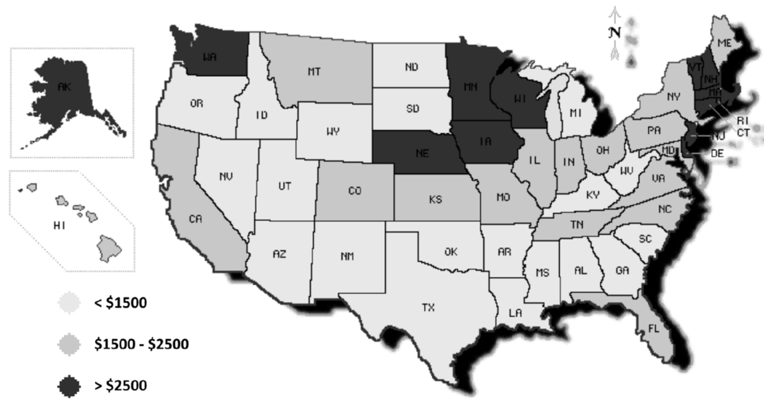



**Figure 3.**
CCDF spending per poor child under 6 in FY2000 (2001 dollars)

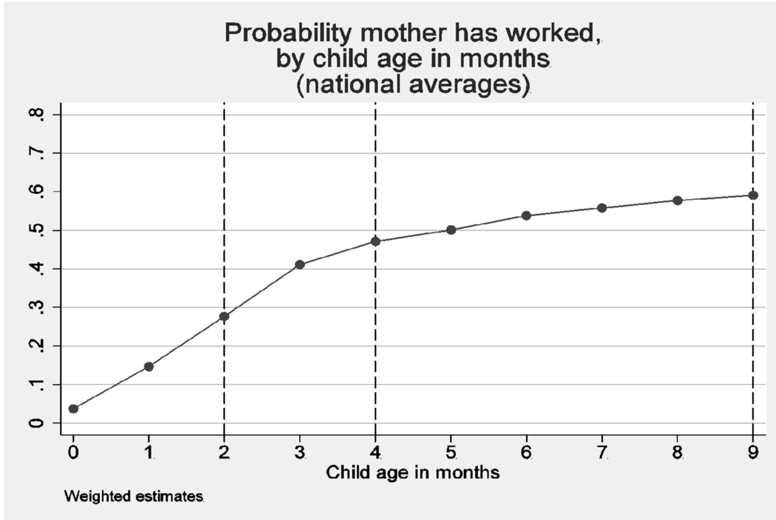



**Figure 4.**
Post-birth work participation rates in the ECLS-B cohort of mothers

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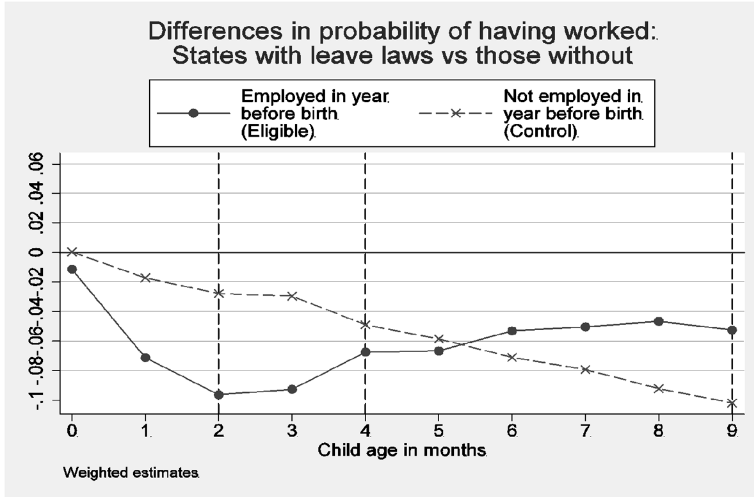


**Figure 5.**
Unconditional effects of state leave laws on the eligible and control groups

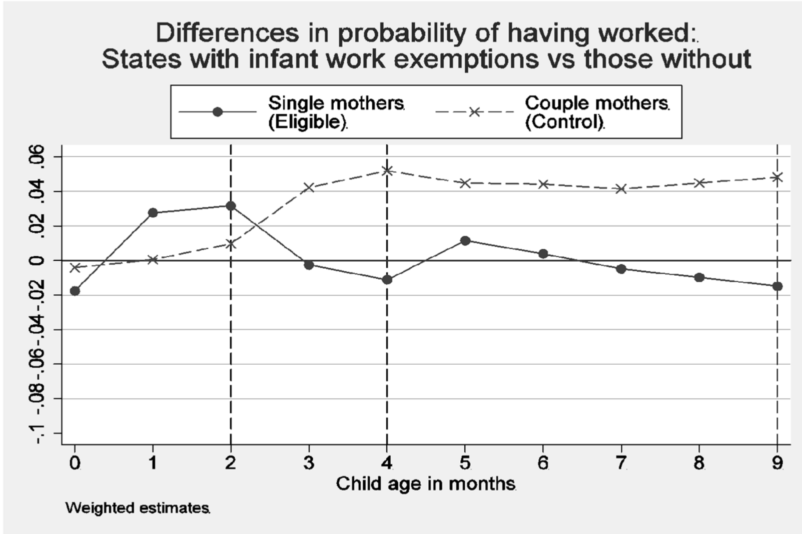


**Figure 6.**

Unconditional effects of infant welfare work exemptions on the eligible and control groups

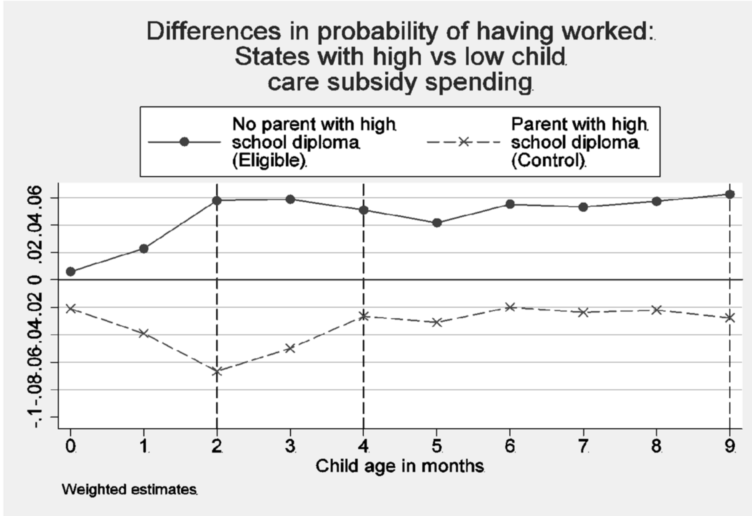


**Figure 7.**
Unconditional effects of child care subsidy expenditures on the eligible and control groups

## Table 1

Summary of state policies

| | | Welfare work exemption >= 12 months | |
|---|---|---|---|
| **State leave law** | **CCDF spending** | **Yes** | **No** |
| Yes | <$1500 | (1) - | (2) OR |
| | $1500–$2500 | (3) ME | (4) CA, HI, MT, NY, TN |
| | >$2500 | (5) CT, DC, MA, MT, RI, VT | (6) NJ, WI |
| No | <$1500 | (7) GA, KY, LA, MS, NM, NV, SC, TX, WV | (8) AL, AR, AZ, ID, MI, ND, OK, SD, UT, WY |
| | $1500–$2500 | (9) CO, IL, KS, MD, MO, NC, OH, PA, VA | (10) FL, IN |
| | >$2500 | (11) AK, NH | (12) DE, IA, NE, WA |

*Note.* CCDF spending expressed as thousands of dollars per poor child under 6 in 2001 dollars.

## Table 2

Linear probability models of work participation after childbearing

| | A. Unrestricted sample | | | B. Sample remaining at age 4 wave | | |
|---|---|---|---|---|---|---|
| | Mother worked by child age: | | | Mother worked by child age: | | |
| | (1) 2 months | (2) 4 months | (3) 9 months | (4) 2 months | (5) 4 months | (6) 9 months |
| State leave law × Worked before birth | −0.066*** (0.021) [−0.108, −0.024] | −0.026 (0.022) [−0.069, 0.017] | 0.043* (0.023) [−0.003, 0.089] | −0.078*** (0.024) [−0.125, −0.031] | −0.037 (0.025) [−0.087, 0.013] | 0.044 (0.027) [−0.009, 0.097] |
| Work exemption × Single mother | 0.011 (0.025) [−0.039, 0.061] | −0.069*** (0.024) [−0.116, −0.022] | −0.068*** (0.021) [−0.110, −0.026] | −0.021 (0.029) [−0.078, 0.036] | −0.088*** (0.028) [−0.145, −0.032] | −0.071** (0.028) [−0.126, −0.015] |
| CCDF spending × No parent with HS | 0.040** (0.017) [0.007, 0.073] | 0.031* (0.016) [0.000, 0.062] | 0.035** (0.017) [0.000, 0.069] | 0.025 (0.022) [−0.019, 0.068] | 0.014 (0.016) [−0.017, 0.045] | 0.020 (0.017) [−0.013, 0.054] |
| R-squared | 0.150 | 0.246 | 0.252 | 0.149 | 0.251 | 0.253 |
| Mean of dep. var. | 0.277 | 0.471 | 0.591 | 0.283 | 0.478 | 0.599 |

*Notes.* Weighted estimates. Standard errors clustered for complex sampling design in parenthesis. 95% confidence intervals in square brackets.

*** p<.01,

** p<.05,

* p<.1. Sample sizes are 10,500 for the unrestricted sample and 8,800 for the subsample remaining at the age 4 wave. Regressions include state dummies, main effects of eligibility criteria (worked before birth, single mother and no parent with a high school diploma) and controls for: race/ethnicity; education of mother and partner at 9 months; mother's age at birth and its square; parents cohabiting; other family type; number of previous children; other child age 2 or under in household; other child age 3 or 4 in household; mother foreign born; mother's primary language not English; urban/rural; mother's family received welfare in childhood; education of mother's parents; mother lived with biological parents until 16; pre-pregnancy BMI and alcohol consumption; desired number of children; number of challenging life events; child sex; multiple birth status; normal/low/verylow birth weight.

**Table 3**

Predicted work participation rates at national level under alternative policy packages

| | Policy combinations | | |
|---|---|---|---|
| | **(1) Status quo** | **(2) Package to maximize maternal work by 9 months** | **(3) Package to minimize maternal time by 9 months** |
| Policies | Policies in effect in 2001 | Leave laws in all states; No welfare work exemptions ≥12 months; CCDF spending at 90th percentile ($3,865) in all states) | No state leave laws; Welfare work exemptions of 12 months or more in all states; CCDF spending at 10th percentile ($975) in all states) |

| | Predicted work participation rates by child age (in months): | | | | | |
|---|---|---|---|---|---|---|
| **Sub-group** | **2** | **9** | **2** | **9** | **2** | **9** |
| All mothers | 0.25 | 0.59 | 0.26 | 0.63 | 0.25 | 0.56 |
| High school or less | 0.28 | 0.52 | 0.25 | 0.58 | 0.29 | 0.45 |
| Single mothers | 0.29 | 0.61 | 0.29 | 0.70 | 0.25 | 0.55 |

*Notes.* Table shows average predicted probability of having worked for the relevant sample of mothers. Predicted probabilities are calculated using the regression coefficients shown in Table 2, columns 1 and 3. Policy variables are set to the specified values and the outcome is predicted for each individual before averaging. Calculations assume that the policies have no effect of the work probabilities of the control groups.

## Table 4

Regression models of intermediate factors nine months after birth

| | Mode of child care at 9 months | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) Parent only | (2) Center-based | (3) Non-center based | (4) Duration of breast feeding (months) | (5) < 4 well baby visits (binary) | (6) CES-D Maternal depression score (z-score) | (7) NCATS Maternal parenting score (z-score) |
| State leave law × Worked before birth | −0.045* (0.025) [−0.096, 0.005] | −0.000 (0.017) [−0.033, 0.033] | 0.046* (0.027) [−0.008, 0.100] | 0.183 (0.211) [−0.236, 0.601] | −0.042** (0.019) [−0.080, −0.004] | 0.102 (0.062) [−0.022, 0.215] | −0.057 (0.080) [−0.216, 0.102] |
| Work exemption × Single mother | 0.075** (0.033) [0.010, 0.140] | 0.008 (0.024) [−0.039, 0.056] | −0.084*** (0.027) [−0.138, −0.029] | −0.305 (0.253) [−0.807, 0.198] | 0.006 (0.024) [−0.042, 0.054] | −0.018 (0.103) [−0.223, 0.187] | −0.098 (0.076) [−0.249, 0.053] |
| CCDF spending × No parent with HS | −0.030* (0.015) [−0.061, 0.001] | 0.028* (0.016) [−0.003, 0.059] | 0.002 (0.019) [−0.035, 0.040] | 0.117 (0.172) [−0.225, 0.459] | −0.008 (0.018) [−0.044, 0.027] | 0.010 (0.071) [−0.131, 0.152] | 0.059 (0.044) [−0.029, 0.147] |
| R-squared | 0.160 | 0.059 | 0.120 | 0.259 | 0.052 | 0.110 | 0.153 |
| Mean [SD] of dep var | 0.498 | 0.085 | 0.418 | 3.603 [3.593] | 0.130 | −0.005 [1.003] | 0.023 [0.999] |

Notes: Weighted estimates. Standard errors clustered for complex sampling design in parentheses. 95% confidence intervals in square brackets.

*** p<.01,

** p<.05,

* p<.1. All regressions include state dummies, main effects of eligibility criteria (worked before birth, single mother and no parent with a high school diploma), plus the controls listed in the note of Table 2. Samples are limited to those children participating in the preschool (age 4) survey wave. Sample size is 8900 in columns (1) through (3) and 8800, 8850, 7900 and 7300 in columns (4) through (7).

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## Table 5

Regression models of intermediate factors approx. four after birth

| | Mode of child care at 4 years | | | | | | |
|---|---|---|---|---|---|---|---|
| | **(1) Mother employed at survey date** | **(2) Parent only** | **(3) Center-based** | **(4) Non-center based** | **(5) CES-D maternal depression score (z-score)** | **(6) Two Bags maternal parenting score (z-score)** | **(7) Log household income at survey** |
| State leave law × Worked before birth | 0.053* (0.029) [−0.005, 0.111] | 0.008 (0.030) [−0.051, 0.067] | −0.018 (0.034) [−0.085, 0.048] | 0.010 (0.025) [−0.040, 0.060] | 0.084 (0.061) [−0.036, 0.205] | −0.048 (0.074) [−0.195, 0.100] | 0.066 (0.052) [−0.038, 0.171] |
| Work exemption × Single mother | −0.032 (0.031) [−0.094, 0.029] | 0.026 (0.028) [−0.030, 0.082] | −0.010 (0.038) [−0.085, 0.066] | −0.016 (0.032) [−0.080, 0.047] | −0.034 (0.084) [−0.201, 0.133] | 0.104 (0.081) [−0.057, 0.264] | 0.019 (0.053) [−0.087, 0.125] |
| CCDF spending × No parent with HS | 0.031 (0.021) [−0.011, 0.072] | −0.021 (0.020) [−0.060, 0.019] | 0.021 (0.023) [−0.026, 0.068] | −0.000 (0.028) [−0.057, 0.056] | 0.006 (0.057) [−0.107, 0.118] | 0.096** (0.045) [0.006, 0.186] | −0.055 (0.048) [−0.151, 0.041] |
| Observations | 8850 | 8850 | 8850 | 8850 | 8900 | 7600 | 8900 |
| R-squared | 0.137 | 0.089 | 0.085 | 0.054 | 0.494 | 0.169 | 0.494 |
| Mean [SD] of dep var | 0.601 | 0.195 | 0.580 | 0.225 | 0.000 [1.001] | −0.001 [1.001] | 3.728 [1.030] |

See notes to Table 4.

## Table 6

Regression models of child outcomes at preschool survey

| | (1) Language z-score | (2) Literacy z-score | (3) Math z-score | (4) Overall behavior problems z-score | (5) Conduct behavior problems z-score | (6) Inattention behavior problems z-score |
|---|---|---|---|---|---|---|
| State leave law × Worked before birth | −0.042 (0.063) [−0.167, 0.083] | −0.060 (0.062) [−0.183, 0.063] | −0.019 (0.056) [−0.130, 0.093] | 0.086 (0.073) [−0.059, 0.231] | 0.054 (0.069) [−0.082, 0.191] | 0.068 (0.056) [−0.043, 0.179] |
| Work exemption × Single mother | −0.039 (0.067) [−0.173, 0.094] | −0.001 (0.058) [−0.116, 0.113] | 0.024 (0.064) [−0.102, 0.151] | −0.059 (0.077) [−0.212, 0.093] | −0.035 (0.065) [−0.164, 0.094] | −0.100 (0.077) [−0.253, 0.052] |
| CCDF spending × No parent with HS | 0.011 (0.043) [−0.075, 0.096] | 0.024 (0.044) [−0.063, 0.111] | 0.055 (0.053) [−0.050, 0.160] | −0.071 (0.063) [−0.196, 0.054] | −0.064 (0.061) [−0.185, 0.057] | −0.052 (0.055) [−0.161, 0.057] |
| R-squared | 0.302 | 0.249 | 0.234 | 0.107 | 0.092 | 0.107 |
| Mean [SD] of dep var | 0.001 [1.001] | 0.002 [1.001] | 0.003 [0.999] | −0.003 [0.996] | −0.003 [0.997] | −0.003 [0.999] |

*Notes.* Weighted estimates. Standard errors clustered for complex sampling design in parentheses. 95% confidence intervals in square brackets.

*** p<.01,

** p<.05,

* p<.1. All regressions include state dummies, main effects of eligibility criteria (worked before birth, single mother and no parent with a high school diploma), plus the controls listed in the note to Table 2. Sample size is 8400, 8200, 8250, 8900, 8850 and 8850 in columns (1) through (6).