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PUBLIC POLICIES AND WOMEN'S EMPLOYMENT AFTER CHILDBEARING

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ABSTRACT

This paper examines how the public policy environment in the United States affects work by new mothers following childbirth. We examine four types of policies that vary across states and affect the budget constraint in different ways. The policy environment has important effects, particularly for less advantaged mothers. There is a potential conflict between policies aiming to increase maternal employment and those maximizing the choices available to families with young children. However, this tradeoff is not absolute since some choice-increasing policies (generous child care subsidies and state parental leave laws) foster both choice and higher levels of employment.

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INTRODUCTION

American women begin work quickly after giving birth compared with their counterparts in other developed nations – for example, 41% have worked by three months after the birth compared with only 7% in the UK.[1] The public policy environment facing new mothers in the United States also differs substantially from those in other nations in terms of parental leave rights, provision of universal child care, and cash assistance for families with young children (Waldfogel 2001). A number of studies have explored the association between parental leave rights and longer work absences (Jaumotte 2004; Pettit and Hook 2005; Baker and Milligan 2007; Han, Ruhm and Waldfogel 2009) yet these are not the only public policies that shape the choices available to new parents. In this paper we consider the work incentives generated by leave laws alongside three other policies that affect budget constraints in different and potentially conflicting ways. Specifically, we examine how state parental leave laws, child care subsidies, cash welfare and Food Stamp benefit generosity, and welfare work requirements for mothers of infants affect the employment patterns of mothers of newborns and young children. Fundamental to our analysis is the recognition that the opportunities available vary substantially with socio-economic status and geographic location, and that the policies we examine target different groups and have varying effects on work incentives.

We know surprisingly little about how the policy environment affects the work decisions of American women with infants and young children. This study begins to fill this gap through analysis of the Early Childhood Longitudinal Study-Birth Cohort (ECLS-B), a nationally

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[1] Authors' calculations from the Early Child Longitudinal Birth Cohort study (2001) and the Millennium Cohort Study (2000/1) respectively

representative sample of more than 10,000 U.S. women giving birth in 2001. We exploit both cross-state variation and differential targeting of the policies to identify plausibly causal effects on work participation, as measured by the probability of working at or before 12 weeks and by 9 months post-birth, with limited additional analysis of longer-term effects on employment around the time of the child's fourth birthday. These estimates are used to illustrate potential effects of different policy packages implemented at the national level, paying special attention to the results of implementing a bundle of "work maximization" policies, designed to increase early work participation, and of a "choice promotion" package, intended to provide mothers with the greatest freedom of opportunities. Three of the four focal policies are targeted at relatively disadvantaged women, so our conclusions are particularly relevant for them.

The policy environment matters. Moving from a package of policies with the weakest to the strongest employment incentives is predicted to increase the fraction of mothers working at or before their child's ninth month by around eight percentage points, with much stronger effects for single or less educated mothers. It is noteworthy that some policies increasing the choices available to new mothers – child care subsidies and parental leave laws – are also associated with increased employment, suggesting that the twin goals of providing choice and encouraging work may not be incompatible.

That said, the most work promoting policies include those that decrease nonearned incomes (e.g. by limiting welfare work exemptions and providing low levels of transfer payments) and have the potential to reduce the well-being of mothers. Thus we provide information about but can not determine the "optimal" mix of public policies. Doing so requires evaluation of tradeoffs between the potentially conflicting goals of protecting the employment rights of mothers and incomes of families during the period surrounding a birth versus attempting to maximize the employment of mothers (particularly the disadvantaged), which has been a key policy goal of welfare reform.

STATE POLICIES

This section details our focal policies, discusses their likely effects on early maternal employment, and illustrates the geographical variation in each type of policy.

State parental leave laws

Parental leaves laws increase the choices available to new mothers by extending the time they can take off work without forfeiting their pre-birth jobs. Klerman and Leibowitz (1997) point out that these 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 a longer absence that requires finding a new job (see Ruhm 1998 and Waldfogel 1999 for related discussions). One result is that the effects of such laws may differ in the short-term and medium-term. For instance, work might decrease during the leave period but rise subsequently, because covered women can more often continue in their pre-birth positions rather than having to search for new jobs.

Mothers of the ECLS-B cohort children gave birth in 2001 and so were all covered by the federal 1993 Family and Medical Leave Act (FMLA). Therefore we cannot evaluate the impact of the FMLA but rather focus on state leave laws.[2] Fewer than one-half of private sector workers

[2] Prior research indicates that the FMLA has no overall effect on post-childbirth employment but is associated with more leave-taking and so some initial decreases in work (Han, Ruhm, and Waldfogel, 2009). Ross (1998), Waldfogel (1999) and Han and Waldfogel (2003) provide further evidence on the FMLA, and Klerman and Leibowitz (1997) analyze state leave laws in the period prior FMLA enactment.

meet the qualifying conditions for FMLA (Waldfogel 2001), but 15 states have state parental leave laws more generous than the federal law. Many increase coverage by relaxing the FMLA conditions on firm size, tenure, or work hours requirements (see Appendix Table 1).[3] Some states allow for unpaid leaves in excess of 12 weeks, and five states have temporary disability insurance (TDI) laws that provide a short (typically six weeks) period of paid leave. Our main analysis uses a binary indictor distinguishing states with any leave law, regardless of its terms, from those with just the federal FMLA. We explored the differential effects of the various types of state leave laws, but collinearity and small samples precluded meaningful analysis of this issue.

Figure 1a depicts the geographical variation in state leave laws. Generous leave entitlements tend to be located in the west coast, great lakes, and northeast regions.

Child care subsidies for low income families

Child care subsidies raise the net income a mother receives from given work hours and so are likely to increase rates of employment. Subsidies also lower the cost of paid versus unpaid care, and so make (relatively costly) formal child care more affordable, presumably leading to a substitution of paid and formal care for unpaid and informal care.

Although Blau and Currie (2004) argue that previous research suggests a small overall elasticity of employment with respect to child care prices, Blau and Tekin (2007) emphasize that child care subsidies are typically much larger for low-income families and are targeted towards

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[3] The FMLA provides 12 weeks of unpaid parental leave to those in firms of 50 or more employees who worked at least 1,250 hours in the prior year. We do not code state laws that are 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.

certain population sub-groups, whose responsiveness may be relatively high. Support for this is obtained from studies of local subsidy programs implemented prior to the 1996 welfare reforms (Berger and Black 1992; Anderson and Levine 2000; Meyers et al. 2002), which found substantial positive employment effects for low income or disadvantaged groups.

Under welfare reform, child care funding was consolidated into the 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 percent of state median income (many states set lower thresholds) and where the parents are working or in school. States determine child care prices and parent co-payment rates, and must offer a choice of child care types and providers. They can also transfer funds from their TANF block grants to CCDF and may use TANF funds to directly purchase some child care. A small amount of funding is also available through the federal Social Services Block Grant.

Evidence on the employment effects of these dramatic expansions in child care subsidies is limited. Blau and Tekin (2007), using data for 13 states in 1999, estimated that child care subsidy receipt increased the employment of single mothers by 33 percentage points. Tekin (2007) estimated that a one dollar wage subsidy raised the employment of single mothers by 7 percentage points versus 3.7 points for a corresponding child care subsidy. However per dollar of government funds, the latter is more cost-effective, because it only targets working mothers using paid child care and not all employed mothers. Magnuson, Meyers, and Waldfogel (2007) find that increased child care subsidies explained 14 to 16 percent of the rise in formal child care enrollment occurring between 1992 and 2000, among preschool-age children from low-income families, but they did not examine maternal employment. Lefebvre and Merrigan (2008) show that Quebec's $5-a-day day-care policy had large and statistically significant effects on the employment of Canadian mothers with young children. A general limitation of previous research is that it does not focus on parents of newborns, who might be especially sensitive to child care subsidies because of the relatively high costs of infant care. Conversely, preferences for parental versus non-parental care could be relatively price-inelastic during this period.

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.[4] The timing is not ideal since fiscal year 2000 covers October 1, 1999 to September 30, 2000, whereas the first 9 months of the ECLS-B cohort extend from January 1, 2001 to September 30, 2002 but expenditure data for the relevant period is not available. We experimented with using a weighted average of child care subsidy *allocations* (rather than expenditures) in FY 2001 and 2002. However, CCDF allocations and expenditures vary substantially because some states roll funds over from one fiscal year to the next and the correlation between the two in FY2000 – when both series are available -- is just 0.59. As the correlation between expenditures in FY2000 and FY2003 is 0.97, we believe that the expenditure series for 2000 fairly accurately reflects the subsidies available to ECLS-B parents during the first 9 months after the birth.

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[4] States receive a federal block grant earmarked explicitly for the CCDF and may transfer up to 30 percent of their TANF block grant directly to the CCDF. They can also use remaining TANF funds to subsidize child care directly (largely through vouchers). Our measure includes all three sources of child care subsidies.

Figure 1b shows that the geographical variation in average annual CCDF spending per poor child under 6 follows a fairly strong north-south divide. Spending is divided into low, middle, and high categories in the figure but measured continuously in our multivariate analysis.

Welfare work exemptions and benefit levels

Welfare work exemptions and generous transfer payments reduce the net gain from employment and so are expected to discourage work participation. Consistent with this, previous research finds that more work-oriented welfare rules and lower welfare or Food Stamp benefits raise the employment of single mothers (Blank 2002; Grogger and Karoly 2005). However, few prior studies focus on those with young children or explicitly examine infant work exemptions, which are among the most dramatic welfare reforms. (Before TANF, women were generally exempted from welfare work requirements until their youngest child reached 36 months of age.) The limited available research finds that new mothers work more and breast-feed for shorter durations in states that do not exempt them from work requirements (Jacknowitz and Schoeni 2003; Hill, 2007).

Other potential measures of welfare generosity include time limits on receipt and the stringency of sanctions for families not complying with work requirements. However, these measures are likely to be strongly correlated at the state level, and since we are examining the behavior of families with newborns, the welfare work exemptions seem most relevant. We do not examine the effects of expansions in the EITC program since most policy variation occurred at the federal level.

We measure state welfare work exemptions for parents of infants using a dummy variable equal to one for the 27 states providing an exemption of 12 months or more. Since 23 states had exemptions lasting *exactly* 12 months (see Appendix Table 2), this seems like a sensible threshold. However, our results are robust to the use of other cut-offs.

Transfer payment generosity is proxied by the maximum dollar value of TANF plus Food Stamp (TANF-FS) benefits in the state for a family of three.[5] Again, we treat this as a continuous variable in our multivariate analysis, but define low, middle and high categories for our graphical presentation.

Figures 1c and 1d show that prolonged welfare work exemptions tend to be concentrated in the south, southeast, and midwest , while TANF and Food Stamp benefits are most generous in the northeast, upper midwest, and west, and least so in the south.

Other state policies and characteristics

Figures 1a to 1d showed that each of the four policies tend to be clustered geographically but that the patterns differ markedly across the types of policies. These variations are likely to be associated with differences in other state policies and characteristics, as documented in Appendix Table 3. For instance, states with generous maternity leave tend to have relatively high family incomes, union coverage, abortion rates, and Democratic state legislators, but low rates of infant mortality or uninsured children. Generous leave rights are

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[5] Three is the most representative family size: in FY2001 the average size of TANF families was 2.6, such families averaged two recipient children, and 60 percent had only one adult recipient (2002 TANF Annual Report to Congress, available at http://www.acf.hhs.gov/programs/ofa/data-reports/annualreport5/chap10.pdf).

also correlated with TANF-FS benefits, child care subsidies, stringent child care regulations, high child care worker wages, and more licensed child care centers.

EARLY CHILDHOOD LONGITUDINAL STUDY-BIRTH COHORT

The ECLS-B contains a nationally representative sample of children born in 2001, and oversamples of Asian and Pacific Islanders, American Indian and Alaska Natives, Chinese children, twins, and low birth weight infants.[6] Information is available from the birth certificate, baseline parent interviews, and child assessments when each child was approximately 9 months old. Further survey waves were conducted when the child was 24 months old, and in the years immediately before and after kindergarten entry. The 9-month interview provided rich information on parental employment, demographic characteristics, lifestyles, and behaviors. State of residence at birth, taken from the birth certificate, was used to define the state policies relevant to each mother. We also made use of data on maternal employment status from the pre-kindergarten wave.

Outcomes

At the 9-month interview, mothers were asked if they had worked for pay during the last week and, if not working or on vacation/leave at that time, if they had done so at any time 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 dichotomous work participation variables indicating whether the mother started working less than 12 weeks

[6] Further information on the ECLS-B is available at http://nces.ed.gov/ECLS/index.asp.

post-birth and whether she had worked by 36 weeks (9 months) after it.[7] Longer-term effects were examined by measuring whether the mother was working (or on vacation) in the fall of the year prior to the child's entering kindergarten. Eighty percent of sample children were 4 years old at this interview and, for the sake of brevity, we refer to this below as employment at or around age 4. The sample size for this variable is reduced by attrition (N=8760), an issue we address in our empirical estimation below.

Approximately 28 percent of mothers in our sample worked within 12 weeks of childbirth, and 59 percent within 9 months, with a similar fraction working when their child was around 4 years old (see Appendix Table 4). Figure 2 shows the geographical variation in work participation rates by 9 months and indicates that mothers return to work relatively quickly in the midwest, south, mid-atlantic and mountain states, and slowly in the west and northwest.

Control Variables

We use the rich ECLS-B data (detailed in Appendix Table 4) to control for differences in maternal and family characteristics. These include standard demographic variables (e.g. marital status, race/ethnicity, and education), information on the mother's experiences in childhood and later life, health-related behaviors, attitudes towards motherhood, and her desired number of children.

We also include additional controls to proxy characteristics that may affect the mother's propensity to work but differ systematically between states. However, in doing so, we exclude

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[7] The first (second) variable is coded as one if the answer to the age question was one or two months, or one to 11 weeks (one to 9 months, or one to 36 weeks). Mothers on leave are classified as not working. Observations for whom the respondent is not the biological mother (N = 146) were dropped from the sample, as were those for whom employment timing data was missing (N = 62), leaving an analysis sample of 10,480.

factors potentially affected by post-birth employment (such as breast feeding), since their inclusion might absorb a portion of the true policy effect. Specifically, we control for education of the mother's parents, whether she lived continuously with her biological mother and father until age 16, and whether her family received welfare in childhood. We also include indicators of long-term health behaviors such as body mass index and alcohol use before pregnancy, smoking, drinking/drug problems, criminal behavior, and psychological problems.

METHODS

Our analysis utilizes a difference-in-difference (DD) strategy that identifies "treatment" and "control" groups for each policy. This is possible because the policies are likely to affect the post-birth employment of some groups of mothers but to have little or no effect on others. Operationally, some individuals in our treatment groups may not actually be influenced by the policies, while some in the control groups might be affected by them, with the result that our estimates are likely to understate the true policy effects.

We take two further steps to reduce the effects of potential confounding factors. First, we include state fixed effects, to sweep out influences that are common to all mothers in a given state. Second, we include controls for the exhaustive set of characteristics detailed in Appendix Table 4.

Formally, our estimating equation is:

$$y_{is} = \alpha_s + X'_{is}\beta + \sum_p \left\{ \gamma^p IT_{is}^p + \theta^p \left(ST_s^p \times IT_{is}^p \right) \right\} + \varepsilon_{is}, \qquad (1)$$

where y_{is} is the outcome for individual *i* in state *s*, α_s is a state fixed effect (a vector of state dummy variables), X'_{is} is a vector of individual, child, and family characteristics, IT_{is}^p equals one if the woman is in the treatment group for policy *p* and zero otherwise, and ST_s^p is the state policy variable (e.g. a maternity leave entitlement more generous than the FMLA, a welfare work exemption of a year or more, and levels of CCDF expenditures and TANF-FS allowances). θ^p, the estimated "treatment effect", captures the treatment versus control group difference in states with the specified policy relative to those without it.[8]

We report results of linear probability (LP) models for our dichotomous work participation outcomes because LP coefficients are easier to interpret than corresponding probit or logit estimates, especially when including interaction terms.[9] However, prior to doing so, we compared the results for LP and probit models for specifications that included all covariates except the interactions. The magnitudes and statistical significance of the marginal effects were quite similar, indicating that the linear probability results are informative. 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.[10]

In addition to estimating treatment effects, we simulate overall effects of the policies, either singly or in combination, on the work participation of new mothers. For these estimates, each mother retains her individual characteristics, state fixed effect, and individual treatment

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[8] A standard difference-in-difference model also contains state policy variable "main effects" (not interacted with the individual treatment indicators). These are captured by the vector of state dummy variables in equation (1) and are directly controlled for in the models not including the former.

[9] Ai and Norton (2003) show that probit or logit coefficients may have the opposite sign as the predicted effect of the interaction on the dichotomous dependent variable.

[10] The standard errors tend to be somewhat larger than those obtained by clustering at the state level (Bertrand, Duflo and Mullainathan, 2004), which is less geographically disaggregated than the stratum and primary sampling unit clustering we use.

status. The state treatment indicator for each policy is then "switched on" or "off" to correspond to a particular policy package, and the mother's participation probability is predicted from the estimated coefficients. We average this predicted probability over all mothers (in the specified group) to obtain the simulated employment rate. The difference between this prediction and an alternative is the estimated "effect" of the specified policy change. Because the state policy variable is interacted with the individual treatment indicator, only the treatment groups are assumed to be affected by the change in policies. For example, the participation probability of married mothers is assumed to be unaffected by infant work exemption policies, because the treatment group indicator (single parenthood) is set to zero. Since our model is linear and additive, the effect of the policy on the participation rate is equal to the coefficient on the interaction term multiplied by the proportion of women affected by the policy change. The latter varies across policies and sub-groups.

Formally, we estimate

$$\hat{y}_{is}^{*} = \hat{\alpha}_s + X'_{is}\hat{\beta} + \sum_{p} \left\{ \hat{\gamma}^{p} IT_{is}^{p} + \hat{\theta}^{p} \left(ST_s^{p*} \times IT_{is}^{p} \right) \right\} \qquad (2)$$

Where ST_s^{p*} is a set of values for the state treatment indicators corresponding to a specific policy scenario, $\hat{y}_{is}^{*}$ is individual *i*'s simulated probability of participation, and the "hats" indicate regression estimates. For example, in one scenario we define ST_s^{p*} such that all states offer leave entitlements beyond the FMLA, have infant work exemptions lasting at least one year, and provide CCDF and TANF-FS allowances at the 90th percentile, a configuration we refer to as "choice-promoting". The effect of switching to this policy scenario from the current one (denoted without a star) for mothers in sub-group *j* is:

$$\frac{1}{N_J}\sum_{i\in J}\left(\hat{y}_{is}^{*}-\hat{y}_{is}\right)=\sum_{p}\hat{\theta}^{p}\frac{1}{N_J}\sum_{i\in J}\left\{\left(ST_{s}^{p*}-ST_{s}^{p}\right)\times IT_{is}^{p}\right\} \quad (3)$$

Treatment and control groups

When evaluating state leave laws, our treatment group is mothers employed at some point in the 12 months prior to the birth; those not so employed are the controls. The rationale is that mothers who do not work during pregnancy are ineligible for leave and so cannot be affected by leave rights.[11] As mentioned, the treatment group contains some women unaffected by the state policies. This occurs because most state laws relax qualifying conditions for FMLA-like benefits (although longer leaves are sometimes provided), implying that women already eligible under the FMLA gain no additional rights. Mothers covered by employer leave policies at least as generous as those provided by state laws will also be unaffected, as will women remaining ineligible under the state laws.

We performed sensitivity analysis using mothers who separated from their pre-birth employers prior to delivery as an alternative control group (dropping from the sample those who did not work in the year before birth). Such women have no access to maternity rights and so cannot be affected by the terms of laws. This specification has the advantage of not being affected by differences in the characteristics of mothers who did and did not work in the year before birth (e.g. if high female employment rates affect the decision by states to adopt leave laws). However, these are not our "preferred" estimates, because leave entitlements might influence the decision of whether or not to quit jobs during pregnancy.

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[11] This assumes that the decision to work during pregnancy is not influenced by state leave policies, which seems reasonable in the U.S., since the leaves are short and mostly unpaid (in contrast to Europe where women often have strong incentives to work prior to birth, so as to qualify for lengthy paid leave).

The treatment group for child care subsidies is families where no parent has a high school diploma. We define our treatment group according to parental education, even though child care subsidies are targeted to low income families, to reduce the endogeneity problem whereby child care costs influence work and therefore earnings. Our choice of educationally-disadvantaged households (the lowest 19% of the population) means that some proportion of the control group will be eligible for child care subsidies. Nevertheless, since the subsidies are awarded on a sliding scale (parental co-payments depend on family income), they are likely to provide far stronger incentives for less than more educated households. In sensitivity analyses we widen the treatment group to include parents with high school diplomas and then those with some college.

The treatment group consists of single mothers when considering infant welfare work exemptions or TANF-FS benefits. Based on evidence that state welfare policies do not substantially affect marital status (Moffitt, 1998), this is an effective way to define welfare eligibility. For instance, 66% of ECLS-B mothers claiming welfare in the first 9 months post-birth were single (at the end of the period), compared with just 16% of those not on welfare. By contrast, defining the treatment group by education yields equivalent figures of 47% and 17% (79% and 37%) if less than 12th grade (high school graduate) is the threshold.[12]

UNADJUSTED DD ESTIMATES

Figures 3a through 3d use our difference-in-difference strategy to provide descriptive evidence of how the four types of work-family policies are related to the post-birth work

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[12] These numbers are only suggestive since welfare participation does not identify all women whose decisions are affected by features of the system.

participation of mothers. For example, the dashed line in Figure 3a displays the extent to which mothers who worked during the year before birth (the treatment group) are more likely to be employed after it than those who did not work in the preceding year (the controls), for women in states with a leave law. The solid line shows corresponding disparities for women in states without leave laws. The difference between these two lines is the unadjusted (for other characteristics) DD estimate, since it shows how the treatment-control gap differs in states with and without the specified policy.

The descriptive patterns suggest substantial and plausible policy effects. State leave laws are associated with lower rates of work beginning 6 weeks after birth, with the largest differences occurring between 8 and 15 weeks; the two groups have virtually identical employment probabilities from 17 to 23 weeks, with some evidence of higher rates of work for the treatment group 6 months or more after birth. Such patterns are consistent with findings from the U.K. (Gregg, Gutierrez-Domenech and Waldfogel, 2007) and make sense, since state leave laws allow some women to take additional leave shortly after pregnancy but may eventually raise work participation by increasing job continuity.

High child care subsidies (>$2500 per poor child under 6) are associated with substantial increases in post-birth work, as shown in Figure 3b, compared with either medium ($1500-$2500) or small (<$1500) amounts. Employment begins to rise within a month after birth and is sustained throughout the first nine months. There is also some suggestion that intermediate subsidy levels elevate work participation in the second and third months, but these differences are small and disappear quickly.

Short welfare work exemptions are associated with higher rates of work beginning 12 weeks after the birth (figure 3c). This is exactly when an effect is expected, since the exemption expires at exactly three months in 16 states. Employment rates increase further for the treatment group at four months, which makes sense since the exemptions expire at that time in four additional states.

The results for TANF-FS benefits (figure 3d) are less clear. Inconsistent patterns are found in the first 8 weeks after birth. The largest relative increases in work thereafter are found in states offering intermediate benefits ($700-$850 per month), with smaller growth predicted for the most generous states (>$850 per month).

ECONOMETRIC ESTIMATES

We next present econometric estimates. The "parsimonious" difference-in-difference specifications in Table 1 control only for the state policies and their interactions with the treatment group indicators. These correspond to the "unadjusted" DD estimates above, except that they assume a linear dose-response effect and consider all the policies simultaneously. The "fully conditioned" models, in Table 2, incorporate state fixed-effects and the wide variety of mother, family, and child covariates. Both tables display coefficients on the interactions between the policies and relevant treatment group indicators. To use leave laws as an example, the parameter estimate on the main effect (not shown) captures differences in the probability of working for *the control group* in states with leave laws relative to those in states without them. Since such mothers are not expected to be affected by the policies, any observed relationship is assumed to result from uncontrolled confounding factors. The interaction coefficient then provides the estimated leave law effect – the differential effect for the treatment group relative to the controls.

The parsimonious estimates in Table 1 largely accord with the descriptive relationships provided previously. The first two rows show that state leave laws are associated with a statistically significant 6.7 percentage point reduction in the work probabilities within 12 weeks of birth but a 4.7 point increase at 9 months. Lengthy welfare work exemptions have no effect at 12 weeks but predict a significant 8.2 percentage point employment reduction at 9 months. An extra thousand dollars of child care subsidies (per poor child) correlates with a 4 point employment increase at both 12 weeks and 9 months. TANF-FS benefits are unrelated to work at 12 weeks but are negatively correlated with it at 9 months.

To assess longer-term effects, we estimated how the policies were related to the probability of the mother working at the age 4 interview. Since attrition shrinks the sample by around 16 percent, we first repeated the <12 week and ≤9 month models on the sample of mothers for whom we have information at the later date. The results, shown in the fourth through sixth columns of Table 1, suggest that most effects observed at 9 months continue through age 4, although the magnitudes and statistical significance are often reduced. For state leave laws, the age 4 effect (4.4 percentage points) is only slightly smaller than at ≤9 months (4.6 points for the same sample), although the standard error increases. Conversely, the work reduction predicted by lengthy welfare work exemptions is just over half as large as at 9 months (4.7 versus 8.3 percentage points) and is not significant. CCDF spending is predicted to have considerably larger effects at four years of age than earlier, but the estimates are imprecise and the coefficients at <12 weeks and ≤9 months are substantially attenuated from those for the full sample. Finally, the work reducing effects of TANF-FS benefits are of similar magnitude at age 4 as at 9 months, but the standard errors are large.

Most estimates are little affected by controlling for state fixed-effects and the extensive set of child, individual, and family characteristics. The results, shown in Table 2, confirm that state leave laws are predicted to lower rates of work prior to 12 weeks but increase them by 9 months post-birth. The welfare work exemption estimates are nearly identical to those previously obtained and indicate reductions in work at ≤9 months and 4 years old. The findings for child care subsidies are also unchanged, continuing to predict positive although not always significant effects in all time periods. Finally, TANF-FS benefits have similar or somewhat more negative predicted effects on work in all periods and the coefficients more often approach or reach statistical significance.

Robustness Checks

The similarity of the results between Tables 1 and 2 increases our confidence in the estimation strategy, since it suggests even the parsimonious difference-in-difference specifications control well for potential confounding factors. The findings are also robust to the use of a variety of alternative specifications, as detailed next.

As discussed, we used an alternative control group for leave laws, consisting of mothers who worked during some part of the year before the birth but were no longer employed at the time of it. These specifications suggest somewhat stronger (11.1 percentage point) reductions in work by 12 weeks and marginally weaker increases at ≤9 months or four years old.

When considering child care subsidies, our main specifications used families where no parent had a high school diploma as the treatment group. Broadening the treatment group to also include those with high school diplomas but no college or, alternatively, some college attendance but no degree, weakened the estimated subsidy effects, as expected since the alternative treatment groups include more families unlikely to receive subsidies. For example, when the treatment group included parents with some college (but no degree), the child care subsidy parameters were small and insignificant.

We also estimated models using funds allocated to child care subsidies in FY 2001 and 2002, rather than child care subsidy expenditures in FY 2000. As discussed, these data correspond more closely to the timing of the period following the births of the ECLS-B cohort but are imperfectly correlated with contemporaneous expenditures. The results are similar to the main specification, but somewhat smaller in magnitude and less precisely estimated. This is expected if there is greater measurement error in the allocations series.

When evaluating welfare work exemptions and TANF-FS benefits, we examined the effect of dropping from the analysis families where a parent had attended college, so that the treatment group includes less educated single mothers and the controls are corresponding married women. These specifications yielded somewhat weaker effects than for the full sample: welfare work exemptions of ≥12 months reduced the predicted probability of working at 9 months by 5.1 percentage points and an extra hundred dollars per month of transfer payments decreased it by 1.0 points. Neither estimate was statistically significant although this may be partly because the sample size was reduced by three-fifths. These estimates compare to statistically significant reductions of 8.2 and 1.6 percentage points in the main model.

Next, we varied the definition or timing of the dependent variables. When we examined work at ≤16 weeks rather than <12 weeks after birth, the effects of state leave laws weakened considerably (falling from 6.7 to an insignificant 2.7 percentage points) whereas longer welfare work exemptions strengthened from virtually no effect to predicting a significant 6.7 percentage point employment. Both results are plausible and reinforce our confidence that we are obtaining causal estimates. Specifically, most state leave laws phase out between 12 and 16 weeks, so that there should be a rapid return to work during this period for the treatment group. Similarly, since the duration of welfare work exemptions is exactly 3 months in 16 of 24 control states, differential rates of work should show up most strongly between 3 and 4 months.

Finally, we ran models where the outcome was a dichotomous variable indicating if the mother was working at nine months post-birth, rather than at any point up to that time. The results were similar but with slightly stronger increases associated with state leave laws or child care subsidies, and marginally weaker reductions predicted for welfare work exemptions. The most significant change was the more than 60 percent decrease (from -0.016 to -0.006) in the coefficient on TANF-FS benefits. This suggests that the early work promoted by low levels of transfer payments is often unstable, so that many mothers beginning jobs have left them by nine months after birth.

SIMULATIONS

We next use results from the fully conditioned models in Table 2 to project how different configurations of work-family policies affect maternal work participation after birth. These simulations assume that the policies have no effect on the control groups and may understate the total effect, to the extent this is not the case. This exercise is limited by the policy variation observed in the United States so that we cannot, for example, estimate the effects of providing the lengthy parental leave entitlements common in Canada and many European countries but nonexistent in the U.S. Our results explore scenarios in which currently existing state policies are implemented nationally. For the dichotomous policy variables – parental leave laws and infant work exemptions – our simulations model turning the policy "on" or "off". For the continuous policy variables – CCDF and TANF-FS spending – we compare benefits at the 10th and 90th percentiles of the actual national distribution, referred to respectively "low" and "high" expenditures.[13]

Table 3 summarizes results for the full sample. Under existing policies, just over one million mothers (roughly 28 percent) were working within 12 weeks of giving birth and 2.3 million (nearly 60 percent) by 9 months. Rows 1-8 show the predicted influence of changing single policies. For instance, abolishing all state leave laws increases predicted employment rates at or before 12 weeks by 1.7 percentage points and reduces the probability of work within 9 months by 1.0 point (row 1). Conversely, instituting leave laws in all states decreases predicted employment rates by 3.1 percentage points before 12 weeks and raises them by 1.8 percentage points by 9 months (row 2). Generous child care subsidies are associated with substantial increases in post-birth work while transfer payments and welfare work exemptions have smaller predicted effects.

The last two panels of Table 3 show results for policy combinations. Row (9) displays the simulated effect of a set of policies that restrict the choices available to new mothers.

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[13] The 10th and 90th percentiles for CCDF annual spending are $974 (South Carolina) and $3,863 (District of Columbia), per poor child under 6. The 10th and 90th percentiles for TANF-FS are $545 per month (Alabama) and $928 per month (Connecticut), where these amounts refer to the maximum allowance for a family of 3.

Specifically, states are assumed to offer no parental leave (beyond the federal legislation), child care subsidies and TANF-FS payments are at the 10th percentile, and welfare work exemptions are shorter than 12 months. Row (10) shows the opposite case, where women have the most choice: all states have leave laws, offer at least a one year welfare work exemption, and child care spending and TANF-FS payments are at the 90th percentile. The last two rows provide information for different counterfactuals, where policies are selected according to the direction of their effect on participation rates by 9 months post-birth. Row (11) refers to policies estimated to promote work participation: all states have a leave law; none have a welfare work exemption lasting a year or more; CCDF and TANF-FS spending are at the 90th and 10th percentiles respectively. The second examines the reverse situation where no state has a leave law, welfare work exemptions last at least one year, and states have low child care subsidies and high transfer payments.[14]

The combined predicted effect of policies promoting choice is relatively modest –work participation falls at <12 weeks by 1.6 percentage points and increases at ≤9 months by 2.0 points. Switching from policies offering the least choice to those supplying the most – calculated by subtracting row (9) from row (10) – is predicted to decrease work before 12 weeks by 2.9 percentage points but increase employment at ≤ 9 months by 2.1 points. This is noteworthy, because it implies that legal and financial supports for new mothers can be structured to allow them to spend a longer period of time at home with infants, without

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[14] Leave laws are predicted to decrease work in the first months but increase it thereafter. We characterize them as an employment-promoting policy, focusing on the 9 month effects, since we think that medium to long term effects are most relevant in terms of policies to encourage work.

reducing employment in the medium-term. The relatively small effect of the "choice package" occurs partly because some components (lengthy work exemptions and generous transfer payments) reduce employment incentives while others (high child care subsidies and leave laws) either promote work or have different effects in the short-term and medium-term.

Public policy nevertheless has potentially powerful effects. A set of policies designed to maximize maternal employment has large effects, as can be seen by taking the difference between rows (12) and (11). Moving from the least to the most work-promoting policies is predicted to reduce work probabilities before 12 weeks by 2.1 percentage points but increase rates at ≤9 months by 7.8 points. The reason that the "work maximization" package reduces employment before 12 weeks is that universal leave laws discourage employment during the first three months but increase it subsequently.

Table 3 provided estimates for all U.S. mothers. Since three of the four focal policies target the disadvantaged, we anticipate stronger effects for such women. Table 4 confirms that this occurs. The simulations suggest that married/cohabiting mothers (results shown in the first two columns) are moderately affected by state leave laws and minimally influenced by child care subsidy policies.[15] By contrast, all four policies have strong predicted effects on single mothers (see columns 3 and 4). We focus our remarks on the packages of policies, summarized in the last four rows of the table, since these show most explicitly the effects and tradeoffs implied by different types of policies.

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[15] When a group represents the control group for a specified policy (e.g. married/cohabiting mothers when considering infant work exemptions or TANF-FS spending), the policy effect is set to zero by assumption.

The effects of a choice promotion package differ substantially across groups. Such a package is associated with increased work participation by ≤9 months for married mothers and in less educated families, with those in the latter group appearing to be particularly constrained by the high costs of child care. Single mothers are the only group where greater choice is associated with a lower work probability by ≤9 months. This reflects the relatively strong employment-reducing effect of welfare work exemptions.

Increased choice generally has a more negative predicted influence on work participation before 12 weeks (than at ≤9 months), suggesting that many mothers are constrained in their ability to stay home with infants and would take advantage of policies that make it easier for them to do so. The least educated families again represent an exception; here the results suggest an unfulfilled demand for early work participation that is constrained by a lack of affordable child care.

The package of work promoting policies is predicted to have a sizable medium-term effect on single mothers, raising their employment at ≤9 months by 12.6 points (relative to the baseline of 61 percent). The difference between this and the set of policies that encourage early work the least is an extremely large 22.3 percentage points. By comparison, the corresponding change for married/cohabiting mothers, moving from the least to most work promoting policies, is 4.2 percentage points.

The estimated effects for women with less than a high school education are, if anything, even bigger than for single mothers. The medium-term effects (by 9 months) are of similar magnitude but larger in relative terms because of their lower (46.5 percent) baseline employment rate, and the short-term (<12 weeks) predicted effects are also greater. As already mentioned, the employment of this group is particularly sensitive to CCDF spending levels, suggesting the importance of child care costs as a constraint on work.

Conversely, the policies have much smaller effects on more highly educated mothers, as expected, since they constitute the control group for CCDF funding and are relatively frequently married or cohabiting (and thus in the control group for TANF-FS benefits and welfare work exemptions). However, the effects of state leave laws appear to increase with maternal education, consistent with evidence recently presented by Han, Ruhm and Waldfogel (2009). Notice also that a long-term "work maximization" package of policies is predicted to reduce early employment (<12 weeks) among highly educated and married/cohabiting mothers, while increasing that of their less advantaged counterparts.

DISCUSSION

The types of public policies currently enacted in the United States have potentially powerful effects on the employment of new mothers, particularly the disadvantaged. For instance, implementing a set of policies designed to maximize work participation 9 months after the birth is predicted to increase the employment of single or less educated mothers by 30 to 50 percent, compared to corresponding policies providing weak work incentives.

Evidence that disadvantaged mothers are highly sensitive to the net returns of working does not, however, identify the optimal mix of policies, because there may be a tradeoff between policies that maximize the choices available versus those that increase the employment of mothers with young children. On the one hand, evidence on the possible adverse consequences of very early employment for child health and development (e.g., Smolensky and Gootman 2003; Ruhm, 2004; Waldfogel 2006) suggests that policy should seek to discourage early work. On the other hand, proponents of welfare reform view early work to be desirable in its own right and as a route out of poverty, hinting at the opposite conclusion. The timing of employment effects also matters. For instance, we may be less concerned about policies that deter work during the infancy period if they promote it later in the child's life.

However, the tradeoff between choice and employment is not absolute. As mentioned, parental leave policies relax constraints immediately after the birth but are also associated with significantly higher work participation rates by nine months. Child care subsidies targeted to the most disadvantaged mothers also increase choice (particularly if they are offered alongside paid leave policies) and have strongly positive effects on employment, suggesting that the costs of high quality infant care are a real barrier to work among those with lower incomes.[16]

The results should be interpreted in light of several caveats. First, the data are for a single cohort of births and so cannot exploit policy variation occurring over time. One implication is that we cannot investigate federal policies (such as the FMLA) enacted simultaneously throughout the country. Second, our identification strategy involves a series of difference-in-difference estimates where policies are anticipated to affect some groups but not others. Alternative but corresponding policies targeting our current control groups might have different consequences. For instance, child care subsidies for middle- or high-income mothers may have smaller effects than CCDF funding, which assists low-income mothers. Similarly, income support policies to married families (such as those in many European countries) may

[16] Rosenbaum and Ruhm (2007) provide evidence that child care expenditures, as a share of income, are larger for low-income than high-income families, and that the disparity would increase dramatically if not for the lower employment rates and cheaper sources of child care used by the former.

have weaker disemployment effects than TANF-FS benefits targeting single women. Finally, although we analyze a wider constellation of work-family policies than previous research, we neither include all possibly relevant policies nor consider interactions between government and employer policies. In particular, our discussion has assumed a policy environment where disadvantaged mothers face a simple trade-off between poverty and work. Cash assistance policies, such as a system of child allowances, suggest a third alternative – one currently available to more advantaged mothers – in which staying home to care for an infant is an affordable option.

Notwithstanding these qualifications, our results are new and noteworthy. Packages of policies designed to promote or discourage maternal employment are likely to have strong effects. Policies maximizing the choices available to women have smaller but frequently positive predicted employment impacts, because some of them (parental leave entitlements and child care subsidies) promote work in the medium-term. The responsiveness of less educated women to child care subsidies was of particular interest because it was unanticipated when we began our investigation. Future research is needed to verify this result and identify mechanisms for it. More generally, our findings highlight the need for further investigation into the effects of work-family policies, using techniques and approaches that address the limitations in this work and include an even wider array of policies.

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Figure 1a. States leave laws more generous than the federal minimum in 2001

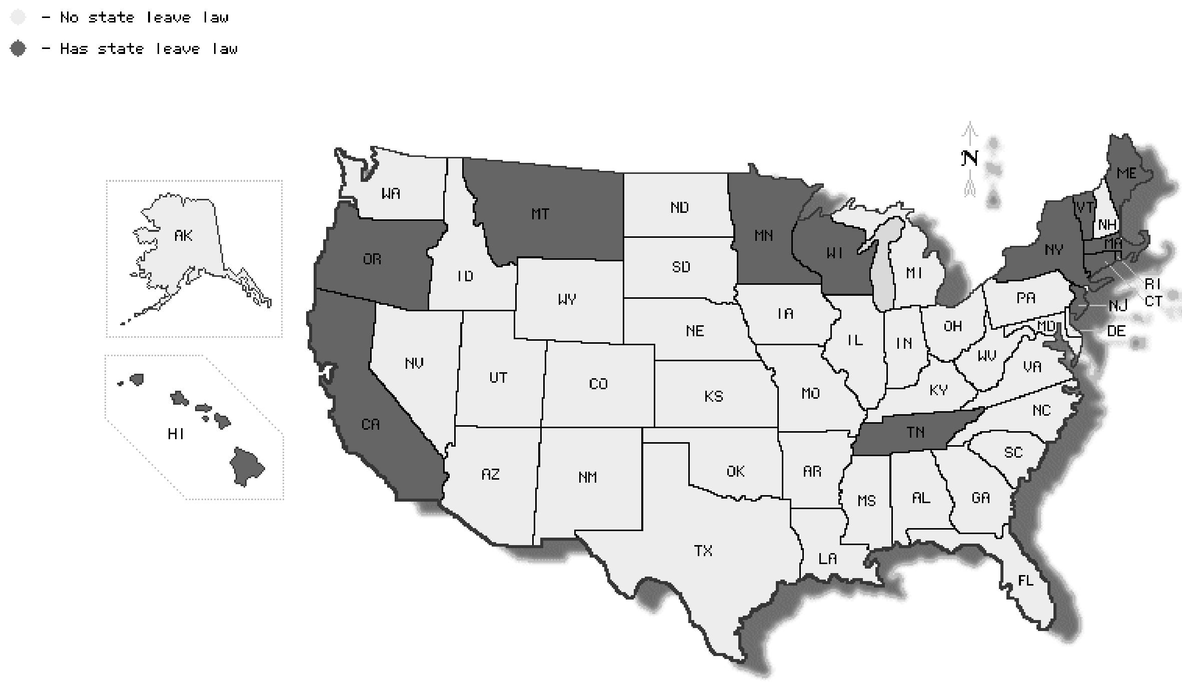


Figure 1b. CCDF spending per poor child under 6 in FY2000 (2001 dollars)

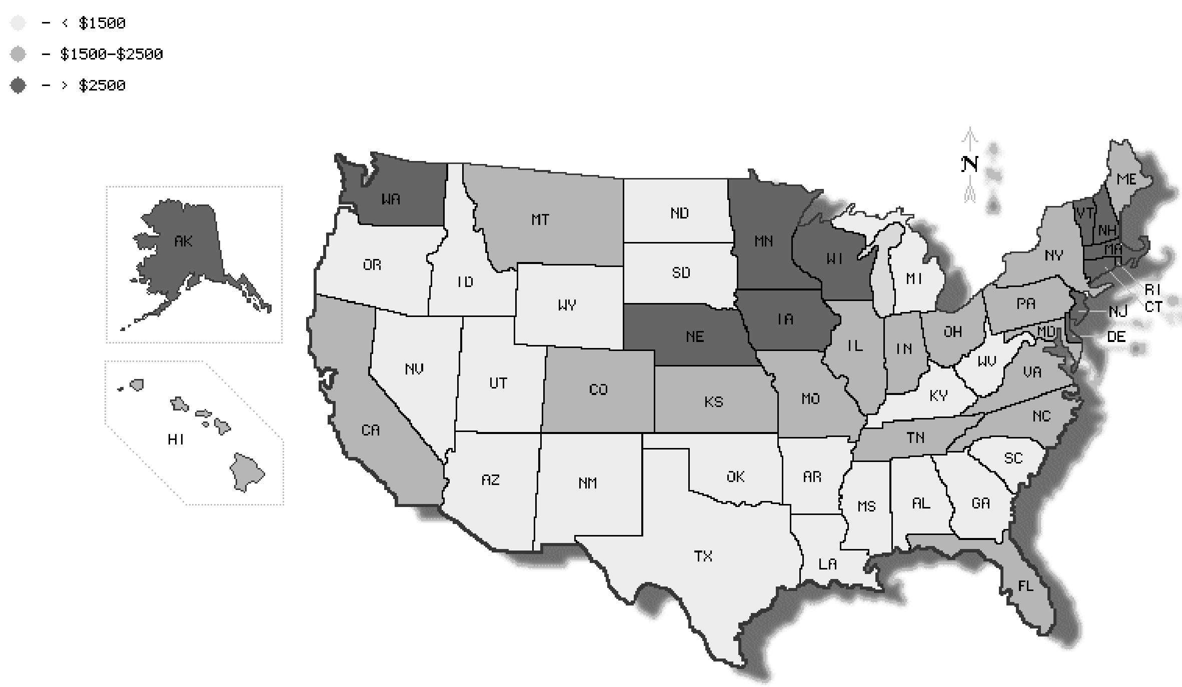


Figure 1c. States infant work exemptions

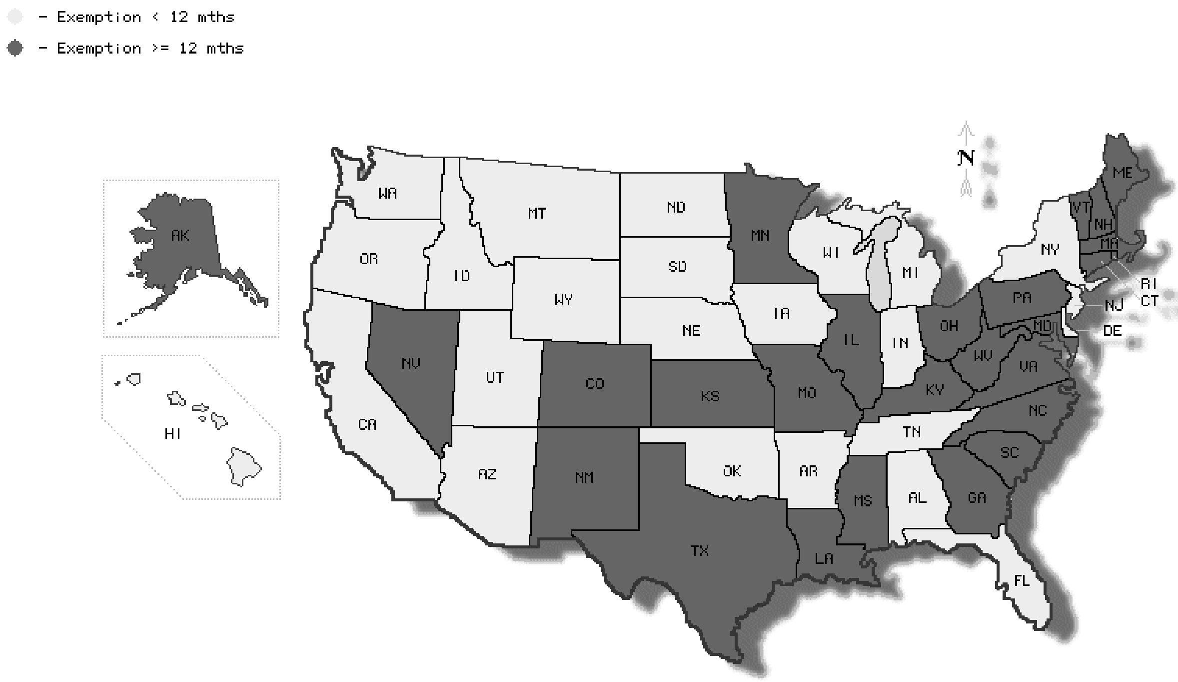


Figure 1d. Monthly TANF plus Food Stamps allowances (2001 dollars)

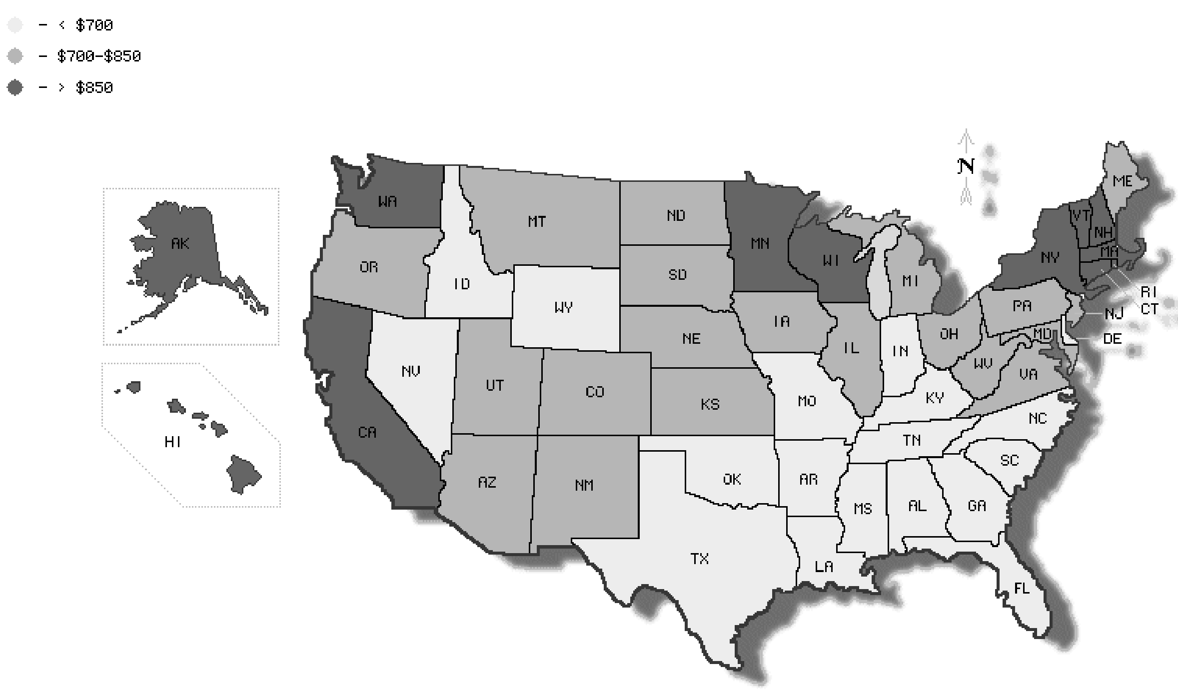
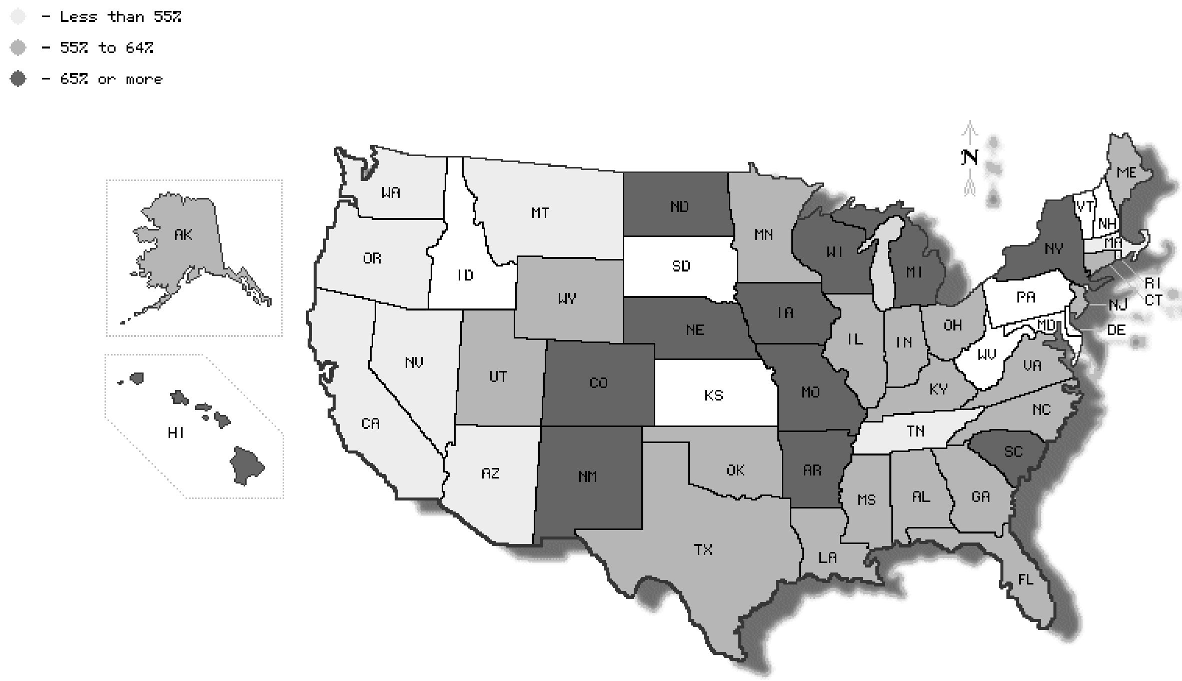


Figure 2. Proportion of mothers participating in work 9 months post-birth



Note: States in white contain fewer than 20 observations.

Figure 3a: State Leave Laws – Treatment vs. Control Group Differences in Work Propensities

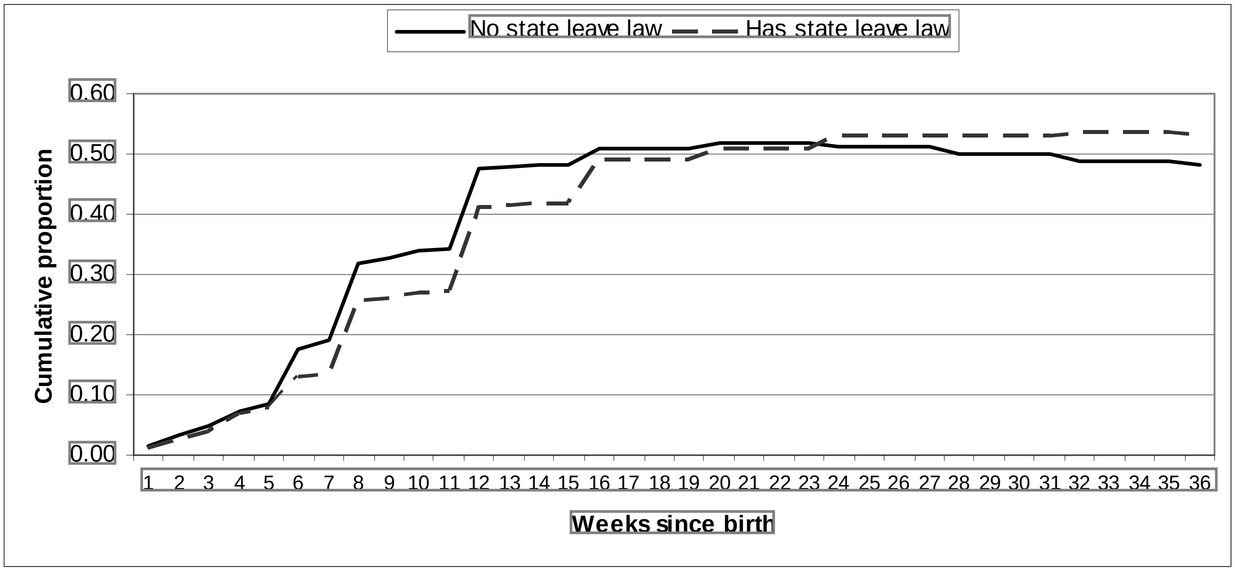


Fig. 3b: Child Care Subsidies – Treatment vs. Control Group Differences in Work Propensities

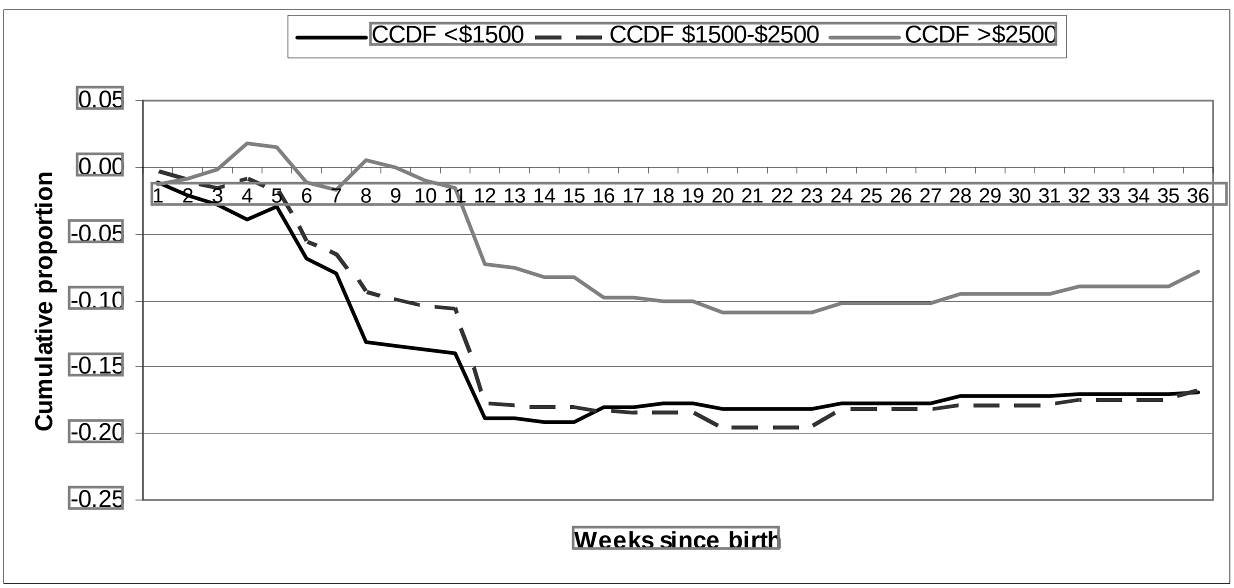


Fig. 3c: Welfare Work Exemption – Treatment vs. Control Group Differences in Work Propensities

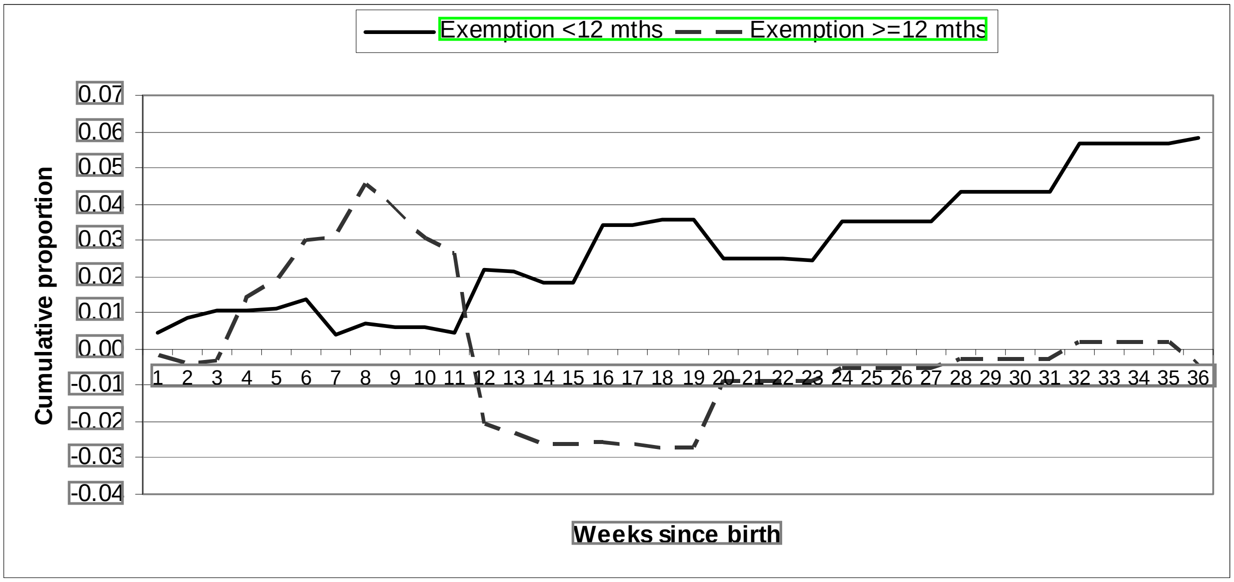


Fig. 3d: Transfer Payments – Treatment vs. Control Group Differences in Work Propensities

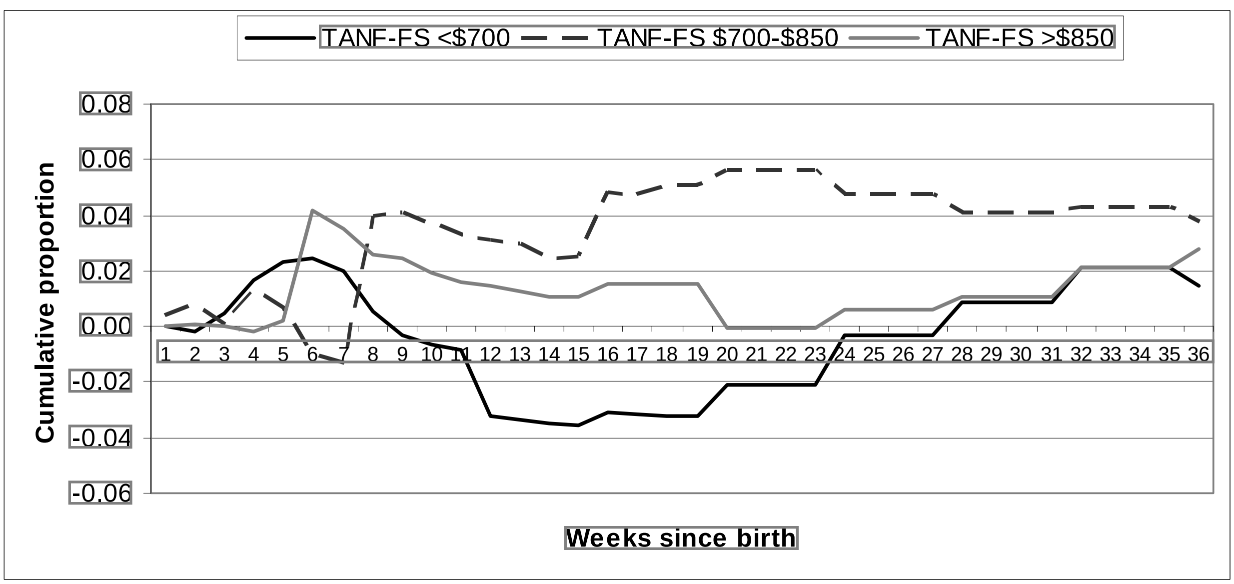


Table 1. Parsimonious Difference-in-Difference Estimates

	Dummy Variable = 1 if mother at work in:				
Treatment effects (interaction terms)	< 12 wks	≤ 9 mths	< 12 wks	≤ 9 mths	At approx. age 4
	Full sample		Restricted sample		
State leave law x	-0.067***	0.047*	-0.079***	0.046*	0.044
employed in year before birth	(0.019)	(0.025)	(0.022)	(0.027)	(0.029)
CCDF spending x	0.041**	0.038*	0.025	0.020	0.035
No parental HS diploma	(0.018)	(0.020)	(0.022)	(0.019)	(0.022)
Exemption≥12 mths x	0.014	-0.082***	-0.014	-0.083***	-0.047
single mother	(0.031)	(0.023)	(0.035)	(0.028)	(0.033)
TANF-FS x single mother	-0.003	-0.016*	-0.003	-0.018	-0.013
	(0.010)	(0.010)	(0.011)	(0.011)	(0.013)
Observations	10480	10480	8760	8760	8760
R-squared	0.12	0.22	0.11	0.22	0.1

* p < .05, ** p < .01, *** p < .001

Note: Models include state- and individual-level treatment indicators (listed in Appendix Table 4), but not state fixed effects or individual-level control variables. Standard errors, corrected for complex survey design, are in brackets. CCDF spending and TANF level variables are represented as deviations from means. CCDF spending is expressed in thousands of 2001 dollars per poor child under 6. TANF-FS allowance is monthly value in hundreds of 2001 dollars for family of 3.

Table 2. Estimated Difference-in-Difference Estimates from Fully Conditioned Model

	Dummy Variable = 1 if mother at work:				
Treatment effects (interaction terms)	< 12 wks	≤ 9 mths	< 12 wks	≤ 9 mths	At approx. age 4
	Full sample		Restricted sample		
State leave law x	-0.067***	0.040*	-0.078***	0.04	0.047
employed in year before birth	(0.021)	(0.023)	(0.024)	(0.027)	(0.030)
CCDF spending x	0.041**	0.038**	0.026	0.021	0.034
No parental HS diploma	(0.017)	(0.018)	(0.022)	(0.018)	(0.021)
Exemption≥12 mths x	0.005	-0.082***	-0.027	-0.084***	-0.045
single mother	(0.025)	(0.020)	(0.029)	(0.027)	(0.031)
TANF-FS x single mother	-0.007	-0.016*	-0.008	-0.018*	-0.015
	(0.009)	(0.009)	(0.010)	(0.010)	(0.012)
Observations	10480	10480	8760	8760	8760
R-squared	0.15	0.25	0.15	0.25	0.14

* p < .05, ** p < .01, *** p < .001

Note: Models include individual-level treatment effects from Table 1, as well as the individual-level demographic and additional controls detailed in Appendix Table 4, and a full set of state dummy variables. Standard errors, corrected for complex survey design, are in brackets.

Table 3. Simulated Effect of Different State Policies on Maternal Work Participation After Birth

	Work participation			
	< 12 weeks		≤ 9 months	
	Percentage	Number	Percentage	Number
Predicted work participation under existing policies	27.7	1,089,679	59.1	2,325,350
	Changes due to policy reform:			
Single policy reforms (holding other policies constant):	% points	Numbers	% points	Numbers
State leave laws				
(1) Abolished	+1.7	+65,876	-1.0	-38,896
(2) Universal	-3.1	-122,080	+1.8	+72,082
CCDF spending equalized at:				
(3) 10th percentile	-0.6	-23,398	-0.5	-21,535
(4) 90th percentile	+1.7	+67,809	+1.6	+62,413
Infant work exemptions 12 months or more:				
(5) Abolished	-0.1	-2,056	+0.8	+31,835
(6) Universal	+0.1	+2,054	-0.8	-31,828
TANF spending equalized at:				
(7) 10th percentile	+0.3	+10,295	+0.6	+22,938
(8) 90th percentile	-0.3	-11,583	-0.7	-25,806
Combined policies to promote choice				
(9) Least - (1), (3), (5), (7)	+1.3	+50,718	-0.1	-5,659
(10) Most - (2), (4), (6), (8)	-1.6	-63,798	+2.0	+76,861
Combined policies to maximize early work				
(11) Most - (2), (4), (5), (7)	-1.2	-46,032	+4.8	+189,268
(12) Least – (1), (3), (6), (8)	+0.9	+32,949	-3.0	-118,065

Simulations are conducted using the full sample estimates from Table 2 and are evaluated holding individual characteristics and state fixed effects constant. Sample weights are used to adjust these predicted individual probabilities to provide nationally representative estimates. All changes are relative to existing policies.

Appendix Table 1. The content of state leave law policies

	TDI state	Min. firm size < 50	Max. weeks leave > 12	Min. tenure < 1 yr	Min. hours < 25	Obs in state
California	X					1,581
Connecticut			X		X	130
District of Columbia		X	X		X	3
Hawaii	X			X	X	104
Maine		X			X	100
Massachusetts		X		X	X	246
Minnesota		X		X	X	223
Montana		X		X	X	21
New Jersey	X				X	690
New York	X					390
Oregon		X	X	X	X	105
Rhode Island	X		X			70
Tennessee		X	X			171
Vermont		X				1
Wisconsin					X	74
Obs affected	2,835	870	479	699	1696	3,909

Appendix Table 2. Distribution of lengths of infant work exemptions

# months of exemption	# of states	States
0	4	AZ, ID, MT, UT
3	16	AL, AR, CA, DE, FL, IA, IN, MI, NE, NJ, NY, OK, OR, SD, WI, WY
4	3	ND, TN, WA
6	1	HI
12	23	AK, CO, CT, DC, GA, IL, KS, KY, LA, MD, ME, MN, MO, MS, NC, NM, NV, OH, PA, RI, SC, TX, WV
18	1	VA
24	3	MA, NH, VT

Appendix Table 4. Descriptive Statistics for Variables Used in Main Analysis

Variable	**Mean (SD)**
Dependent variables	
Mother at work before 12 weeks	0.28
Mother at work at or before 9 months	0.59
Mother employed at pre-school wave (approx. age 4; N = 8760)	0.60
Demographic control variables	
Married to resident biological father	0.65
Cohabiting with resident biological father	0.14
Single mother (no resident father)	0.20
Other family type (e.g. step-father)	0.01
White non-Hispanic	0.57
Black non-Hispanic	0.14
Hispanic	0.23
Asian	0.03
Other race/ethnicity	0.03
Mother: Less than high school	0.27
Mother: High school	0.22
Mother: Some college	0.36
Mother: BA degree	0.15
Mother: More than BA degree	0.09
Mother's age at birth	28.18 (6.18)
No resident siblings at 9 months	0.41
1 resident sibling at 9 months	0.34
More than 1 resident sibling at 9 months	0.26
Sibling under 3 in household at 9 months	0.16
Sibling age 3 or 4 in household at 9 months	0.21
Mother foreign born	0.22
Mother's primary language non-English	0.13
Urban area	0.74
Urban cluster	0.12
Rural area	0.14
Father: Less than high school	0.17
Father: High school	0.18
Father: Some college	0.21
Father: BA degree	0.14
Father: More than BA degree	0.10
Father: No resident father	0.20
Additional control variables	
Mother received welfare in childhood	0.11
Childhood welfare missing	0.01
Mother's mother some college or more	0.32
Mother's mother's education missing	0.04
Mother's father some college or more	0.33
Mother's father's education missing	0.11

Mother's family intact til 16	0.58
0 risky life events ever happened	0.74
1 risky life event ever happened	0.16
2 to 6 risky life events ever happened	0.08
Risky life events missing	0.02
Pre-pregnancy BMI: Under weight	0.05
Pre-pregnancy BMI: Normal	0.54
Pre-pregnancy BMI: Over weight	0.23
Pre-pregnancy BMI: Obese	0.14
Pre-pregnancy BMI: Missing	0.03
Ever smoked > 100 cigarettes	0.33
Alcohol pre-pregnancy: Never	0.62
Alcohol pre-pregnancy: < 4 drinks pwk	0.31
Alcohol pre-pregnancy: ≥4 drinks pwk	0.07
Ideal number of children in whole life	2.85 (1.09)
Ideal number of children missing	0.10
State-level treatment variables	
State leave law	0.36
Annual CCDF spending per poor child under 6 (thousands 2001 dollars, FY2000)	1.85 (0.87)
Infant work exemption 12 mths or more	0.47
Max. monthly TANF+FS family of 3 (hundreds 2001 dollars)	7.44 (1.49)
Individual-level treatment variables	
Mother employed in year before birth	0.71
No parent with high school diploma	0.19
Single mother (no resident father)	0.20

Note: Standard deviation of continuous variables shown in brackets. All other variables are dichotomous. N = 10480 unless otherwise indicated. All individual-level control variables taken from baseline survey 9 months post-birth. Statistics are weighted to correct for complex survey design.