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The impact of state subsidies for family leave on foster care and adoptions

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Abstract

The present analysis explores the impact of changes to state-level benefits in New Jersey (2009), Rhode Island (2013), and California (2004) on the number of children in foster care or adoption systems. Data from the Children's Bureau are combined with Census population estimates to produce yearly per-capita measures of adoption and foster care during 2007-2016. The effects of state-level benefits related with those federally mandated by the Family Medical Leave Act on foster care and adoption are explored, with suggestions made for future policy and research into various kinds of support for individuals providing fostering and adoptive care.

1. Introduction

The United States is one of only three countries—and the only developed nation—to have zero federally-funded maternity or family leave programs in place (Beebe, 2017). With the number of women in the workforce having grown tremendously, and with even *unpaid* medical leave unavailable to many women either due to economic or legal restrictions, the sustainability of the present situation is open to question.[1] The importance of leave and compensation related with childbirth is clearly an issue of utmost concern. The present analysis seeks to examine a currently under-researched area of the literature related to a particular clause in the Family Medical Leave Act in the United States, namely, the effect of state-provided subsidies for family leave on the number of children who are in the foster care or adoption system.

Let me first begin by a consideration of why family leave policies in the United States are not more generous. With concerns regarding both the funding of these and related programs, as well as the fear of program overuse or abuse, being cited as reasons to delay program expansion, it comes as no surprise that federal-level changes have been stalled for the past 25 years (Beebe, 2017). Despite the difficulties with change in Washington DC, it does appear that an increasing number of states have begun to focus their attention on family leave, with some even going so far as to restructure their budgets to allow for more expansive family leave policies to be enacted. As one recent example, in 2016 the state of New York passed legislation funding family medical leave so that it will now pay out a percentage of leave from State funds.[2]

In terms of economic impacts previously considered in the literature, several routes for the effects of the Family Medical Leave Act (FMLA) and state-level changes in paid benefits have generally been considered. Typically, the focus is either on (1) an increase in maternal health and outcomes, such as improved mental health and a reduced incidence of post-partum depression, fewer C-sections, or younger maternal age (pre-35) at first birth, (2) firm pay and productivity changes, such as changes in the rate of return to work after childbirth, firm retraining costs, pay and employment rates, or employment gaps for women, and (3) improved outcomes for children, such as better rates of immunizations, more breastfeeding, higher birth weights, and better behavioral scores (Baker and Milligan, 2007; Berger et al., 2005; Oloomi, 2016; Rossin, 2011).

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[1] The financial restrictions in question concern the inability of low-income women to take FMLA leave, even when it is available to them, because they either cannot afford to go without pay, or else, even in the case of partially-funded leave as part of a more generous firm policy, they still cannot afford to take off without their full paycheck (Rossin, 2011). By law, FMLA is only mandated for individuals in companies with at least 50 employees living within a 75-mile radius of the company headquarters. The employee must also have worked for the company for at least 1,250 hours during 12 months prior to the start of the FMLA leave (Rossin-Slater, 2017). Therefore, the eligible population comprises a very small fraction of the total workforce.

[2] The benefits will phase in over time, with 50% pay initially, and by 2021, 2/3 of employee pay compensated by the state. (NY.GOV/PAIDFAMILYLEAVE)

The preferred method of analysis is typically a differencing structure, employing changes in benefits due to either (1) the federal introduction of (unpaid) FMLA in 1993, (2) the state-level introduction of paid benefits at various times in a straightforward difference-over-time and state structure, or (3) variations in "exposure" by counties to FMLA based on the industrial or workforce composition in the counties (Oloomi, 2016; Rossin, 2011; Tito, 2016). Notice that the most basic structure is to look at the take-up rate of FMLA usage, in order to determine whether the policies actually had an impact on women tending to take off more work after childbirth.

While most of these studies find that the effects tend to be positive, they are not unanimous in this regard. They do generally agree, however, that the benefits of family leave are concentrated in the first year after birth. Importantly, it is also the case that the effects of the 1993 FMLA legislation, and the state changes to funding in conjunction with this act, are often discussed with regard to pregnancy or childbirth, but they are almost never analyzed in relation to the parts of FMLA associated with the care of others in the household. This brings us to the focus of the present analysis.

The sub-clause I consider here is *"…the placement with the employee of a child for <u>adoption or foster care</u> and to bond with the newly placed child within one year of placement."* I focus on state legislation changes for New Jersey (2009) and Rhode Island (2013). While California (2004) is also considered, data restrictions make a full before-and-after comparison more difficult. Each of these states is considered because, in addition to the federal unpaid benefits of FMLA, the state in question uses payroll taxes to finance the programs, in which employees are compensated with between 50% and 67% of their previous pay during the time of their leave.

Although not widely recognized, the foster care system is also a place where economic incentives are clearly at play. Foster parents are paid for each child placed in their care, with studies of state-level variation in foster care payments consistently showing that an increase to the size of financial remuneration to foster care leads to both an increase in the number of willing foster parents, as well as a decrease in the number of children in group homes (Doyle and Peters, 2007; Duncan and Argys, 2007; Hansen, 2007; Hansen and Hansen, 2005).

The current analysis extends this question of foster parent incentivization by examining a slightly less direct route of compensation. Specifically, I determine the impact of state-level changes in benefits related to FMLA on changes in state foster care and adoption statistics. I employ fixed effects and a differencing style methodology in the regression model structure. It is anticipated that increasing financial benefits for foster care and adoption could, in principle, change the number of individuals who are willing to become foster or adoptive parents.

The remainder of the analysis is organized as follows: Section 2 discusses the Materials and Methods employed in the work. Section 3 discusses the summary statistics and regression results, and section 4 presents the conclusions to be gained from this preliminary-level analysis.

2. Materials and Methods

Data on adoption and foster care are culled from the United States Children's Bureau Office of the Administration for Children and Families Adoption and Foster Care Analysis and Reporting System (AFCARS). All population estimates employed to construct the *per capita* measures use Census American Community Survey 1-Year state-level population values.

State-level data changes related to foster care and adoption for the years 2007-2016 employ a time and location differencing structure, as well as a fixed effects model to account for overall state-level changes. Fixed effects are employed to capture the greatest amount of variation between states, rather than relying on specific changes between states in variables that would be subject to omitted variables bias.

The specific model employed for state i in year j is:

$$Outcome_{i,j} = f(BenefitsState_i, BenefitsState_i * Post_j) \qquad (1)$$

Where *Outcome* is, alternatively, (1) the per capita number of children currently being served in the foster and adoption care system--*Served*, (2) the per capita number of children exiting from the adoption and foster care system in a given year--*Exited*, (3) the per capita number of children waiting to be adopted--*Waiting to Adopt*, or (4) the per capita number of completed adoptions for the year--*Adopted*. *BenefitsState* is a Boolean variable for whether the state is one of the maternity-friendly states (Rhode Island, New Jersey, and California), while *BenefitsState*Post* indicates whether it is a maternity-friendly state after the year when the maternity benefits were introduced in the state. The specific regression structure is an ordinary least squares (OLS) model.

In model 1, all states are employed, while in model 2, California is entirely dropped from the analysis in order to parse out the effects of the other two benefit states. Model 3 both drops California and allows for a more flexible functional model by employing a Boolean for whether it is a benefits state rather than the full fixed-effects model. To be precise, the OLS model employed in models 1 and 2 is, for US state i in year j:

$$Outcome_{i,j} = \beta_0 + \beta_1 BenefitsState_i * Post_j + \alpha_i + \varepsilon_{i,j} \qquad (2)$$

Where *Outcome* takes on, alternatively, the values of *Served, Exited, Waiting to Adopt*, or *Adopted* as described previously; α_i represents the state-level, time-invariant fixed effect; and $\varepsilon_{i,j}$ represents the idiosyncratic error term. The variable $BenefitsState_{i,j} * Post_{i,j}$ represents the interaction between the Booleans for whether the observation comes from a state having special benefits, and whether it is also "Post" the institution of said benefits in that state. To reiterate, the only difference between models 1 and 2 is that the former includes data for California and treats it as a benefit state, while the latter drops California entirely from the regression to determine whether results remain the same.

In contrast, model 3 uses the OLS model for US state i in year j as:

$$Outcome_{i,j} = \gamma_0 + \gamma_1 BenefitsState_i * Post_j + \gamma BenefitsState_i + u_{i,j} \qquad (3)$$

Where all variables are as described above, however, this equation now drops the fixed effects of α_i and replaces it with a simple Boolean for whether the observation comes from a state in which special benefits are or are not received during this period ($BenefitsState$). Notice also that β is replaced by γ and ε by u, for intuitive simplicity.

Finally, I note that it is a limitation of the study that a finer level of analysis for the adoption and fostering data is not currently available, such as at the county level, so that the state-level variation could be employed with both fixed effects for state- as well as county-level characteristics.

3. Results

3.1 Summary Statistics

Figure 1 displays 2007-2016 averaged state-level estimates for the *per capita* values of the various outcome measures as a "rate" per 100,000 individuals in the state. I have highlighted both the national values as well as the specific values for the benefits states examined here in detail in order to show where the benefits states fall in comparison to the other states in the US. Also displayed are the percentage served in the adoption system in each state. Notice that, while these histograms are presented for the sake of brevity, I next refer in the text to the specific values as well as the totals—population unadjusted—in the more extended summary statistics (available upon request).

FIGURE 1: Per capita Adoption and Fostering Histograms for US States
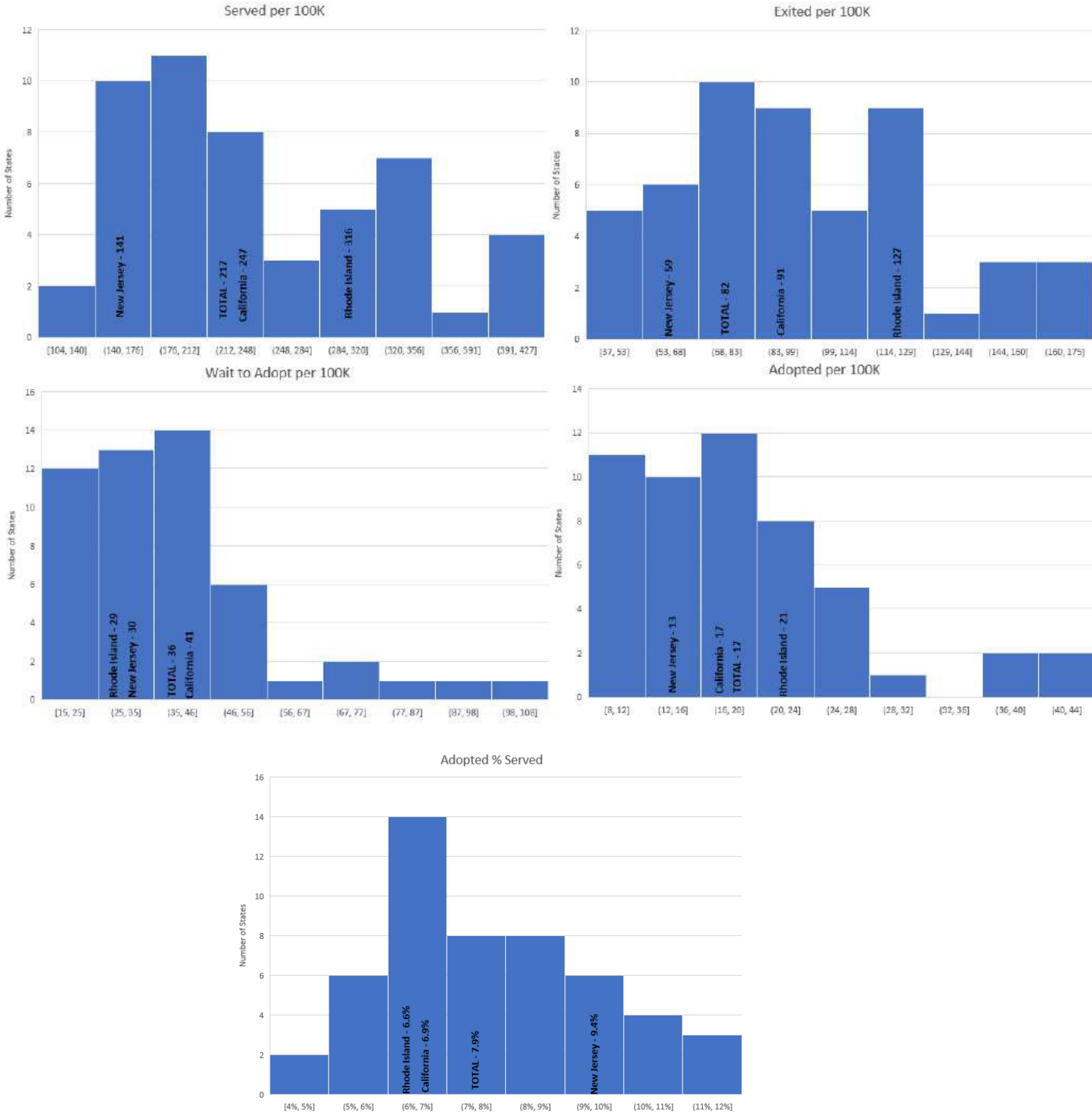
Turning first to adoption statistics, likely due to their larger populations, California, Texas, and Florida dominate the number of yearly adoptions. Leaving California out of the regression structure is therefore likely to have a noticeable impact on the results. While New Jersey is in the upper third of adoptions (14th highest), Rhode Island is closer to the bottom of the list (43rd). Any effect of state maternity benefits from Rhode Island and New Jersey will thus be operating not from the largest states in the adoption system, and will actually give us a stronger sense of what we can expect from changes in maternity benefits, compared with focusing solely on the largest states in the system. The other element to notice is that the adoptions per year is much smaller than most people realize. With two thirds of states having fewer than 1,000 completed adoptions per year (unscaled by population size), this statistic is much less of an issue than might be assumed.

Turning next to the number of children in the foster or adoption care systems, we now see much larger numbers. While essentially the same states still dominate (California, Texas, Florida, and now also New York), there are about 93,500 children in the system in California, as compared with the 6,500 who were actually adopted in a given year. This means that the annual rate of adoptions corresponds to less than 7% of the total number of children in the system in California, which is similar to the national average of 7.9%. In terms of the ranks of Rhode Island and New Jersey, we see similar positions within the state hierarchy. It is notable that, compared to California's 93,500, Rhode Island and New Jersey only have a combined 16,000 children in the system.

Finally, the number of children who exited the system—as well as the number who are still waiting to be adopted—tends to lie somewhere between these two extreme values. In California, there are, on average, 34,500 yearly exits, and 15,500 children who are waiting to be adopted, while there are 6,500 completed adoptions and 93,500 children in the system. Taken together, it is clear that the number of children who are actually adopted is dramatically different from the number who are still waiting in the system at some point in the fostering and adoption process. It is also clear that Rhode Island and New Jersey represent just a small fraction of the total number of children in the system. Their adoption to total in the system rates are, respectively, 6.6% and 9.4%.

3.2 *Regression Results*

Table I displays results from the OLS regressions on the effect of being in a maternity benefit state, particularly after the adoption of those benefits, on the various outcomes of interest. Each of the four columns represents any of the four outcomes—*Served, Exited, Waiting to be Adopted, and Adopted*—while each outcome is run in three different models. The first model employs fixed effects for the state, and includes California. The second model is identical but drops California from the regression. In the third model, fixed effects are substituted for a simple Boolean for whether it is a maternity benefits state. Notice that the District of Columbia is also run as a separate state in the regression analysis.

Table I: Effects of State Benefits on Foster/Adoptions

	Served	Exited	Wait Adopt	Adopted
MODEL 1				
Benefits State * Post	-55.207	-24.334	-6.57	-2.122
	[2.76]**	[3.12]**	[1.69]+	[1.16]
N	510	510	510	510
MODEL 2				
Benefits State * Post	-55.207	-24.334	-6.57	-2.122
	[2.75]**	[3.12]**	[1.69]+	[1.15]
N	500	500	500	500
MODEL 3				
Benefits State * Post	-118.956	-48.61	-4.968	-4.908
	[2.82]**	[2.85]**	[0.53]	[1.2]
Benefits State	55.068	26.792	-7.108	0.94
	[1.67]	[2.01]*	[0.98]	[0.29]
N	500	500	500	500

Note: + indicates significance at the 10% level, * indicates significance at the 5% level, and ** indicates significance at the 1% level.

What we can see from these regressions is that there does not appear to have been an effect of being in a maternity benefits state (Rhode Island, New Jersey, and perhaps California) on the number of adoptions that occurred in that state. This is true in all versions of the regression structure. Notice also that, while one could argue that the state-level analysis is quite crude, it is telling that the fixed-effects model did little in these regressions to change the effects from the basic Boolean for whether it was a maternity benefit state. It does appear, however, that the number of individuals who were served in the foster care or adoption systems is related to whether they were in a benefits state after the implementation of the benefits. Specifically, there were actually *fewer* individuals who were either exiting, being served, or waiting to be adopted (in most instantiations), in these states. The result of having fewer individuals served or waiting to be adopted in maternity benefit-states post-benefits is consistent with having more individuals being fostered or adopted. The fact that there is an insignificant effect on adoption and the wrong sign on exited—there was expected to be a positive relationship between exited and benefit states—implies that there could be a more complicated mechanism at play.

In fact, while initially counter-intuitive, these results may actually reflect the pregnancy and childbirth mechanism of FMLA. Specifically, I would surmise from these initial regression results that, if maternity benefits in Rhode Island and New Jersey (and California) were

enhanced, then the mothers could better care for their children, and will have an a reduced likelihood of intervention by state child services, and children would be less likely to then enter the system in the first place. This would also explain the negative coefficient for the served and waiting to be adopted regressions. The fact that there is a negative sign on exit could then be because the remaining children are more difficult to place each year than the marginal ones who were prevented from entering.

In order to test this last hypothesis, in a separate regression (not shown), I examined the outcome of the per capita termination of parental rights—also constructed from the AFCARS data. The hypothesis above would imply that, if the channel for a decrease in exits due to maternity benefits is through the increased ability for parents to care for their own children after birth, then there will be a decrease in the termination of parental rights after benefits are increased to birth parents in Rhode Island and New Jersey. This is indeed borne out by the data, with the regression showing a coefficient of -24.3 (t=-3.1**), implying that a decrease in parental rights termination after the enactment of benefits could be a portion of the channel in question.

Finally, it is possible that there is a difference in the effects on stocks and flows of individuals in the adoption and foster care system. In order to examine this possible different channel of effects in additional regressions (not shown), each of the previous outcomes was run for model 2 for both the Rhode Island and the New Jersey benefits changes separately. The one change to model 2 was that only the benefits state being examined was retained of the set of Rhode Island, California and New Jersey. Specifically, in one set of regressions, I examined whether there is an immediate effect of being in Rhode Island one year after the change in benefits as opposed to one year prior to the change taking effect (2012 versus 2013-2014). In those regressions, both New Jersey and California are dropped from the analysis. I repeated this examination for New Jersey just before and just after the change (2008 versus 2009-2010), dropping both California and Rhode Island from the regressions. I found that there was no statistically significant effect of being in a benefits state on "any" of the regressions outcomes of interest. While it is possible that the reduced number of observations (147 rather than 500) was at least partially responsible for this decrease in significance, I would also conclude from this information that the immediate effect of these flows was not apparent here and that the channel was more of a longer term stock changing one. This is in keeping with the previous discussion regarding changes in incentives which are also likely to occur over a longer period of time and are more likely to show up in the longer ten-year time frame but may not in the immediately prior and post type of analysis.

Overall, however, it is clear that there is a non-negligible effect of state-sponsored enhancements to FMLA benefits on the lives of children in the foster care and adoption system. Given that the data point more to a longer term stock-related structure, I would posit that there are likely also other complex interrelationships between adoption, pregnancy, and childbirth benefits that need to be further explored.

4. Conclusions

This analysis has endeavored to understand the effect on the foster care and adoption systems from state-level benefits instituted in addition to the Federal FMLA mandate. This is an important question to consider, since it affects children who are already at risk, and whose lives are particularly fragile. The recent consideration of changes to FMLA benefits at the Federal level, as well as changes being considered in a number of state legislatures, makes an analysis of the effects of benefits changes on the lives of children particularly timely and relevant. In this sense, the present work can be considered a first step in an important process. It is also the case that this work highlighted the intricate relationship between childbirth and the foster care and adoption systems. A lack of immediate effects of benefits changes makes it likely that immediate flows over time do not constitute the channel at play. It is also clear that additional analyses are needed to carefully disentangle the effects of pregnancy and childbirth from the effects on children who are already waiting to be adopted and placed in foster care. This initial stock and flow structure is the first step in that direction. More detailed information would be necessary to complete those steps in the process of analysis. Unfortunately, it is very difficult to tease out the number of children by age ranges who are in foster or adoptive care at any point in time.

In closing, I would note that the financial incentives for foster care parents are typically supplemental to their other financial compensation, so that the current FMLA-based structure of state-level benefits as "replacement pay" at a certain (less than full) percentage of pay is different than the subsidies received for each child fostered. In this sense, the present analysis endeavors to answer a slightly different question, that is, for individuals who may have already wanted to become foster or adoptive parents, would increasing their ability to do so without adversely affecting their work situations actually have a measurable effect? This is a different question than considering an add-on subsidy that would specifically encourage them to foster a child. It is not surprising explicit evidence is lacking for changes in state-level subsidies via FMLA encouraging individuals to foster or adopt and presumably seeing a non-negligible impact of childbirth and pregnancy in this equation.

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