Table of Contents

[Introduction 1](#_Toc12282211)

[Policy Landscape 1](#_Toc12282212)

[Literature Review 2](#_Toc12282213)

[Theory 3](#_Toc12282214)

[Data 3](#_Toc12282215)

[Methodology 7](#_Toc12282216)

[Results 8](#_Toc12282217)

[Discussion 13](#_Toc12282218)

[Limitations 14](#_Toc12282219)

Table of Figures

[Equation 1 9](#_Toc12541029)

[Table 1 6](#_Toc12541042)

[Table 2 6](#_Toc12541043)

[Table 3 7](#_Toc12541044)

[Table 4 8](#_Toc12541045)

[Table 5 9](#_Toc12541046)

[Table 6 9](#_Toc12541047)

Introduction

What explains the persistence of a gender gap in earnings and labor-force participation in the modern, developed-world labor market? Economists have long attributed the divergent outcomes to differences in human capital stocks between men and women, that is, education and work experience. Economists observed the closing gap between men and women in both respects, and predicted convergence in work and wages. Yet despite gender convergence in educational attainment, parity in work experience for men and women has stubbornly refused to emerge. Women remain the primary care-giver for children, and even the brief career interruptions that this care entails can result in significant and persistent wage penalties.

Whether considered or not in the intent of the law, paid family leave policies have the potential to increase labor-force attachments and reduce career interruptions, contributing to the closing of the gender earnings gap. Tanya Byker (2016) estimates the impact of the California and New Jersey paid family leave policies on these labor-market outcomes. This paper is both reproduction and extension of her findings.

Policy Landscape

Since 1993, eligible US workers have had access to 12 weeks of unpaid leave under the Family and Medical Leave Act (FMLA). However, due to firm size and work history requirements, eligibility is far from universal, and typical use is even smaller. In 2012, the Department of Labor estimated that fewer than 60% of workers nationwide were eligible for FMLA leave and only 16% of those eligible workers actually took FMLA leave (Klerman, Daley, and Pozniak 2012). It is perhaps because of the low usage (or the meager benefits) that previous studies have found no effect of FMLA on mothers’ employment outcomes (Han, Ruhm, and Waldfogel 2009).

Enacted in September 2002, the California Paid Family Leave legislation (CA-PFL) went into effect July 2004. Prior to the enactment of the CA-PFL, California mothers were covered by a Temporary Disability Insurance program (TDI) which typically provides mothers with six weeks of compensated leave to be used during pregnancy or immediately after childbirth. Under the CA-PFL, eligible mothers and fathers are both entitled to 6 weeks of paid leave, providing 55% of base pay constrained by a cap on payment ($1,163 per week in 2014 and $1,252 in 2019)[[1]](#footnote-1), with mothers additionally entitled to the benefits of the TDI. The programs are coordinated such that mothers may take leave under the CA-PFL immediately following leave under the TDI.

Officially entitled the Family Temporary Disability Leave law, the New Jersey Paid Family Leave legislation (NJ-PFL) came into effect in July 2009. The NJ-PFL grants 6 weeks of paid leave to eligible mothers and fathers, providing 2/3 of average weekly pay up to a set maximum that has varied over time ($643 in per week in 2014)[[2]](#footnote-2). Eligible workers are those individuals that have worked at least 20 calendar weeks in New Jersey or that have earned at least $7,150 in the 12 months preceding requested leave.

Under both policies, workers may take leave within 12 months of birth, and leave may be taken concurrently or intermittently. Both policies are funded by a payroll tax on state workers.

Literature Review

In addition to the work of Tanya Byker upon which this paper is primarily based, an insightful literature of other work has helped to inform and contextualize this analysis. Slater, Ruhm, and Waldfogel (2012) estimate the effect of the CA-PFL on leave-taking of mothers following childbirth, as well as their subsequent labor market outcomes. The authors theorize that the policy will increase rates of leave-taking among California mothers, however the predicted effect on employment is ambiguous. If the increase of leave-taking comes primarily from mothers who would otherwise have continued employment, the policy would result in a decrease in work but no change in employment. If the increase of leave-taking comes primarily from mothers who would otherwise have terminated employment, the policy would result in an increase in mothers’ employment in the short-term, with possible positive effects on mothers’ employment in the medium and long term.

To estimate the impact of the CA-PFL, Slater et al. utilize difference-in-difference estimation using yearly data from 1999 to 2010 collected in the March Current Population Survey. The authors find that the CA-PFL doubled use of maternity leave from three weeks on average to six weeks on average. Although they find no statistically significant effect, point estimates suggest that the CA-PFL could indeed increase medium-term employment rates of mothers. Slater, Ruhm and Waldfogel do however find a statistically significant 10 to 17 percent increase in the usual weekly work hours of employed mothers of one-to-three year-old children. The authors propose that this increase could be a result of increased job continuity and the longer work hours associated with the accumulation of firm-specific human capital. They acknowledge however that the mechanism for the increase in work hours is not clear from their study.

Baum and Ruhm (2014) use the 1997 cohort of the National Longitudinal Survey of Youth to investigate the effect of the CA-PFL on various labor market outcomes. The authors find that the CA-PFL raised leave-taking on average by one week for fathers and three weeks for mothers. According to their analysis, the authors find that the largest effect of the CA-PFL on mothers’ leave-taking occurs 6 to 14 weeks after birth. The finding is intuitive, as the CA-PFL can be combined with California’s preexisting Temporary Disability Insurance program, which provides six weeks of paid leave to mothers following childbirth. Baum and Ruhm also find that the policy both increased the probability that a mother has returned to work in the year after birth and raised mothers’ weekly hours of work in the second year after birth. They do not, however, find a statistically significant effect upon mothers’ wages. Baum and Ruhm, similarly to Slater et al., hypothesize that the medium-term increases in employment and work hours reflect increased job continuity among mothers.

Das and Polachek (2015) use data from the March Current Population Survey to explore the impact of the CA-PFL on labor force participation and unemployment outcomes. Using a difference-in-difference framework, the authors find that the CA-PFL increased the labor-force participation of young women in California relative to other states. Das and Polachek also investigate unintended negative consequences of the law, and find that the policy increased the rate and average duration of unemployment for young women relative to other states.

Curtis, Hirsch, and Schroeder (2016) use data from the Quarterly Workforce Indicators to estimate the effect of the CA-PFL on labor market outcomes by examining employment flows and wage offers among new hires. The authors find that although the CA-PFL had little effect on earnings for young women in California, the policy did result in increased labor market churn (defined by the authors as separations, hires, and recalls).

Bartel et al. (2018) use data from the 2000 Census and the 2000 to 2013 waves of the American Community Survey to investigate the effect of the California Paid Family Leave law on fathers’ leave-taking. The authors find that the policy raised leave-taking rates of fathers by 46 percent, although fathers still on average only take 1.5 weeks out of the total 6 weeks of leave for which they are eligible under CA-PFL. In contrast, mothers on average take 9 weeks out of the 12 total weeks for which they are eligible under the combined Temporary Disability Insurance policy and the CA-PFL.

Theory

The California and New Jersey paid family leave policies contain aspects of both a payroll tax and employer mandate. The monetary cost of wage replacement is funded by a payroll tax on workers, while the opportunity cost to the firm of employee time spent on leave fits the model of employer mandate. No matter the policy is framed, as Jonathan Gruber puts it, “the general distinction between payroll taxes and mandates is a false one” (2010). The labor-market effects depend upon tax/benefit linkages, not the particular legislative frame of the policy. In both a payroll tax and employer mandate, there is a cost borne and a benefit received, with incidence of each determined by the elasticities of labor supply and demand.

According to the simple model, the payroll tax creates a wedge between labor supply and demand, reducing both wages and employment of workers, and creating deadweight loss. The conferral of benefits to workers, monetary or otherwise, increases the total value of employment for workers and accordingly increases labor supply and reduces the wedge imposed by the tax. Except for the special case in which employees value benefits at exactly the value of lost wages, there will remain a residual wedge between supply and demand, resulting in lower employment than equilibrium in the absence of the tax.

In the case of the California and New Jersey paid family leave policies, the simple model is complicated by the fact that although the tax is equally imposed upon all workers, the benefits gained are dependent upon worker characteristics. As found by Slater et al. (2012), and Baum and Ruhm (2014), women take paid family leave for longer periods of time and in greater proportion than men. It is possible that young women value the benefits granted by the CA-PFL and NJ-PFL in excess of the cost they bear, leading to an increased labor supply of young women and higher employment compared to the prior equilibrium.

In addition to the monetary cost of payroll taxation, it is important to consider the opportunity cost of employee leave. Although the costs may not be significant in many cases, firms may be required to hire temporary, less productive, replacement labor. If the employee’s firm-specific skills depreciate during leave, firms may also have to the cost. Because young women are most likely to take family leave, firms may discriminate against hiring them in favor of men and older women. The combination of increased labor-force participation of young women and decreased demand for their labor could result in increased unemployment of young women (Das and Polachek 2015).

Paid family leave may have additional effects upon labor-market outcomes of mothers in particular. The benefits of the policy may only be claimed if the individual remains attached to her employer during pregnancy and after childbirth, incentivizing job continuity of mothers. As previous studies have concluded, job continuity is an important factor in the later employment and wages of mothers (Waldfogel 1998). The positive effect of paid family leave policies on job continuity of mothers may therefore lead to positive impacts on labor-force participation, employment, and wages of mothers that are independent of the standard labor-market effects of a payroll tax and benefit.

In view of the various possible shifts in supply and demand it is unclear where the post-policy equilibrium of mothers’ labor-force participation and employment will land. To empirically investigate the effects of California and New Jersey’s paid family leave program on mothers’ labor-market outcomes in various periods of time relative to childbirth, I follow the methodology outlined by Byker (2016).

Data

The data in this paper originates from four panels of the Survey of Income and Program Participation. Each panel covers a national stratified sample of the U.S. civilian non-institutionalized population and uses a 4-month recall period, with respondents divided into rotation groups with each group interviewed during one month of the four-month period. Each four-month period constitutes a wave of the survey. As respondents to the survey are recorded over differing lengths of time, the dataset constitutes an unbalanced panel.

Table

|  |  |  |  |
| --- | --- | --- | --- |
| SIPP Panel | Number of Waves | First Month | Last Month |
| 1996 | 12 | Dec 1996 | Feb 2000 |
| 2001 | 8 | Oct 2000 | Dec 2003 |
| 2004 | 12 | Oct 2003 | Dec 2007 |
| 2008 | 16 | May 2008 | Dec 2013 |

Using data from the four panels, Byker constructed a sample of 2,817 unique persons and 103,624 person-month observations containing all women aged 24 to 45 who gave birth during the time coverage of the SIPP panel and lived within one of the treatment or control states. By connecting information on the date of birth of children with information identifying their mothers, Byker was able to generate a variable identifying the month of childbirth for each mother. Using her constructed sample and the full SIPP dataset, I appended additional information on employment to each observation.

Unique Individual Counts by State and Year

Table

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
|  | California | Florida | New Jersey | New York | Texas |  |
| Year |  |  |  |  |  | Total |
| 1995 | 62 | 23 | 19 | 43 | 48 | 195 |
| 1996 | 319 | 105 | 76 | 166 | 211 | 873 |
| 1997 | 318 | 107 | 74 | 162 | 216 | 873 |
| 1998 | 303 | 97 | 68 | 151 | 208 | 823 |
| 1999 | 283 | 87 | 64 | 140 | 185 | 758 |
| 2000 | 282 | 95 | 58 | 145 | 207 | 787 |
| 2001 | 207 | 76 | 48 | 106 | 149 | 583 |
| 2002 | 201 | 77 | 44 | 96 | 144 | 562 |
| 2003 | 341 | 139 | 91 | 162 | 234 | 963 |
| 2004 | 204 | 99 | 59 | 103 | 143 | 604 |
| 2005 | 201 | 96 | 56 | 103 | 146 | 597 |
| 2006 | 197 | 91 | 52 | 98 | 140 | 576 |
| 2007 | 127 | 69 | 27 | 71 | 80 | 372 |
| 2008 | 222 | 86 | 74 | 86 | 150 | 618 |
| 2009 | 225 | 85 | 75 | 91 | 160 | 631 |
| 2010 | 220 | 86 | 73 | 90 | 163 | 630 |
| 2011 | 218 | 83 | 69 | 83 | 161 | 612 |
| 2012 | 148 | 51 | 41 | 52 | 103 | 395 |
| Total | 1002 | 389 | 276 | 485 | 707 | 2817 |

(Highlighted values indicate the presence of a paid family leave policy.)

Information on labor-force participation and employment is derived from a categorical SIPP variable encoding the employment status of an individual for a given month. Following Byker’s methodology, I reduced this information into three binary variables describing whether a person is in or out of the labor-force, employed or unemployed, and searching for work or not searching for work[[3]](#footnote-3).

Employment Status for Reference Month

Table

|  |  |
| --- | --- |
| Employment / Labor-force Status | Person-Months |
| With a job entire month, worked all weeks (includes individuals on paid leave) | 57,373 |
| With a job entire month, absent from work without pay 1+ weeks, absence not due to layoff | 2,595 |
| With a job entire month, absent from work without pay 1+ weeks, absence due to layoff | 450 |
| With a job at least 1 but not all weeks, no time on layoff and no time looking for work | 1148 |
| With a job at least 1 but not all weeks, remaining weeks on layoff or looking for work | 523 |
| No job all month, on layoff or looking for work all weeks | 3,323 |
| No job all month, at least one but not all weeks on layoff or looking for work | 440 |
| No job all month, no time on layoff and no time looking for work. | 37,756 |
| Missing data | 16 |

The SIPP allows for information on two possible jobs to be recorded for any given month. In the case of an individual holding multiple jobs during a reference month, the job by which the individual earned the most money over the four-month period will be listed as job 1. The job that provided the next most earnings in the four-month period will be listed as job 2. In order to simplify the analysis, only data on job 1 was used in this paper, and each individual was placed into occupations groups according to job 1.

An important aspect of the data is that the SIPP changed the encoding of occupations after the 2001 panel. The 1996 and 2001 panels of the SIPP encode occupations according to the 1990 Census Bureau occupational classification scheme, while the 2004 and 2008 panels utilize the classification scheme from the 2000 Census. This means that pre-2004 panel occupations cannot be directly matched to 2004 and later panel occupations. However, both types of occupation coding systems could still be placed into general occupation groups (specified by the Standard Occupational Classification system). Individuals were placed into occupation groups according to their occupation most recently recorded before birth.[[4]](#footnote-4) I then codified each occupation group as either ‘white collar’ or ‘blue collar’. The results of this processing are reported in Table 4.

Occupation Group Counts and Encodings

Table

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Code | Pre-birth Occupation Group | White Collar / Blue Collar | Count | Proportion | Mean Years of Education |
| 11 | Management | White Collar | 131 |  |  |
| 13 | Business and Finance | White Collar | 86 |  |  |
| 15 | Computer Science and Mathematics | White Collar | 38 |  |  |
| 17 | Architecture and Engineering | White Collar | 24 |  |  |
| 19 | Life, Physical, And Social Science | White Collar | 16 |  |  |
| 21 | Community and Social Service | White Collar | 43 |  |  |
| 23 | Law | White Collar | 20 |  |  |
| 25 | Education, Training, and Library | White Collar | 195 |  |  |
| 27 | Arts, Design, Entertainment, Sports | White Collar | 28 |  |  |
| 29 | Healthcare Practitioners and Technicians | White Collar | 169 |  |  |
| 31 | Healthcare Support | White Collar | 81 |  |  |
| 41 | Sales and Related Occupations | White Collar | 190 |  |  |
| 43 | Office and Administrative Support | White Collar | 424 |  |  |
| 33 | Protective Service | Blue Collar | 78 |  |  |
| 35 | Food Preparation and Serving | Blue Collar | 89 |  |  |
| 37 | Building/Grounds Cleaning and Maintenance | Blue Collar | 54 |  |  |
| 39 | Personal Care and Service | Blue Collar | 78 |  |  |
| 45 | Farming, Fishing, and Forestry | Blue Collar | 25 |  |  |
| 47 | Construction and Extraction | Blue Collar | 6 |  |  |
| 49 | Installation, Maintenance, and Repair | Blue Collar | 5 |  |  |
| 51 | Production | Blue Collar | 80 |  |  |
| 53 | Transportation and Material Moving | Blue Collar | 20 |  |  |
|  | Unclassified | N/A | 39 |  |  |
|  |  | Total Unique Individuals | 1854 |  |  |

Is the segmentation by educational attainment and the segmentation by occupation type capturing the same subpopulations? The cross tabulation below indicates that while there is overlap, the two segmentations indeed divide the sample differently.

Person-Month Frequency Table for Education / Employment Type

Table

|  |  |  |  |
| --- | --- | --- | --- |
|  | White collar | Blue collar | Totals |
| Less than College | 29101 | 12722 | 41823 |
| College Educated | 25484 | 1208 | 26692 |
| Totals | 54585 | 13930 | 68515 |

Person Frequency Table for Education / Employment Type

Table

|  |  |  |  |
| --- | --- | --- | --- |
|  | White collar | Blue collar | Totals |
| Less than College | 807 | 340 | 1147 |
| College Educated | 685 | 37 | 722 |
| Totals | 1445 | 370 | 1815 |

Methodology

*Yits* : Labor-force outcome for woman *i* living in state *s* in period *t*

*λt* : Year Indicators

*θs* : State Indicators

: Months Since Birth Indicators

Equation

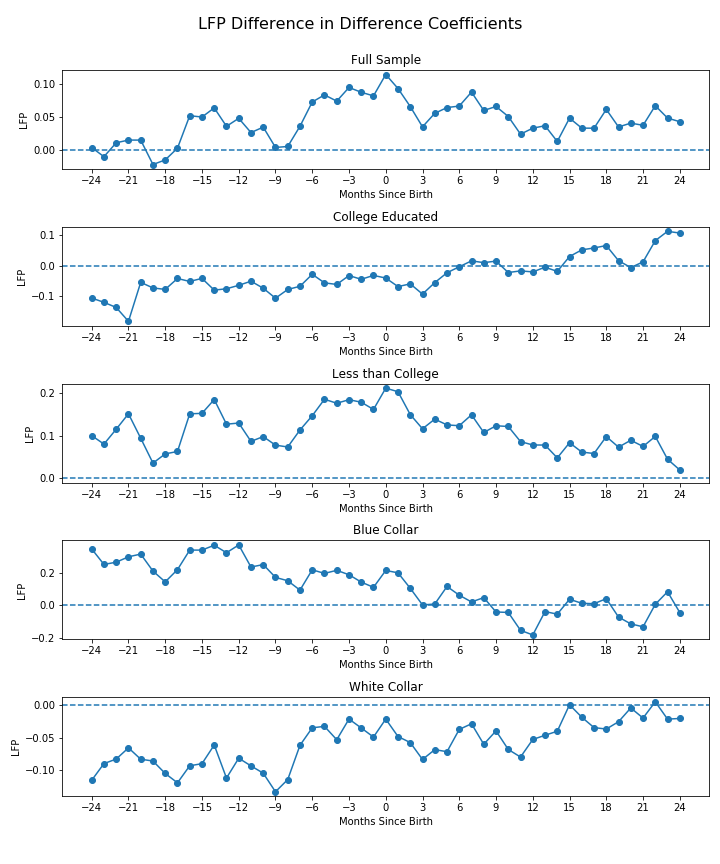
I estimate Equation 1 for each of the five sample groups of interest: full sample, college educated and non-college educated, blue-collar and white-collar, with labor-force participation as the dependent variable. I then repeat the process using the ‘working’ and ‘looking for work’ variables. As the effect of the policy for a given month is unlikely to be significantly different from the effect of the policy for any other given month, I use joint tests for significance over staggered seven month periods.

The difference-in-difference specification used in this study implies a number of assumptions about the data. First and foremost is the assumption of parallel trends. The parallel trends assumption is violated if there exists unobserved time-varying confounding. Essentially, for the estimates to be unbiased, the assumption must hold that the treatment group would have followed a parallel time trend as the control group if the treatment had not taken place. As we cannot observe the counterfactual, we have no empirical method to confirm this assumption.

Another important assumption is the exogeneity of treatment. The estimation is unbiased only if the assignment of treatment is not caused in any way by the outcome variable. In this case, it is likely that the assumption holds, as is unlikely that policy-makers enacted paid leave policies as a result of any particular pattern of mothers’ labor force participation.

Unbiased difference-in-difference estimation requires the assumption that pre-treatment outcomes are not affected by treatment. That is, we assume that mothers do not anticipate the implementation of a paid leave policy and adjust their labor force participation accordingly. It is quite possible that this assumption is violated in this analysis. Mothers expecting shortly before the implementation of a paid leave policy, who would otherwise have dropped out of the labor-force, may have stayed in the labor-force anticipating eligibility for paid leave once the policy came into effect. However, if the proportion of mothers who were pregnant in the 9 months before the implementation of a paid leave policy is small compared to the total sample, the effect on the DiD coefficients is likely to also be small.

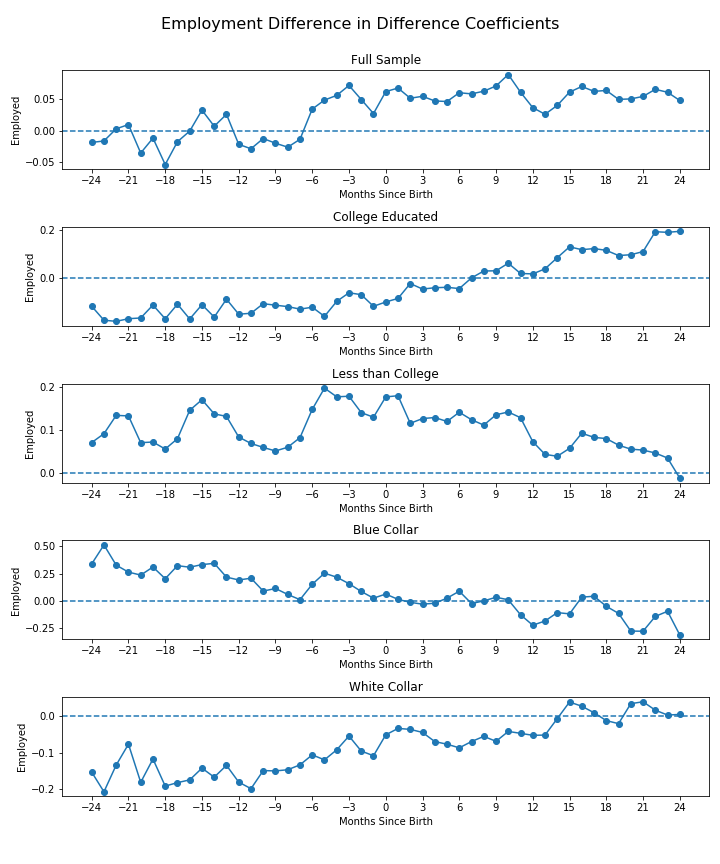
Results



Dependent Variable: LFP

P-Values of Joint Tests for Significance: Months Since Birth \* Policy Coefficients

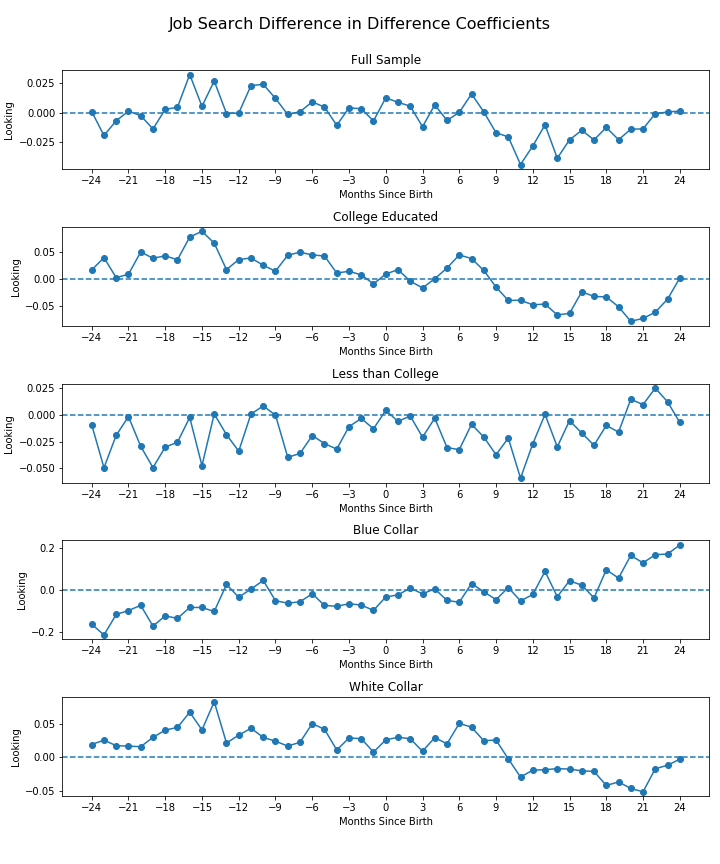
|  |  |  |  |
| --- | --- | --- | --- |
| Months Since Birth | -3 to +3 | 0 to + 6 | +3 to +9 |
| Full Sample | 0.12 | 0.33 | 0.47 |
| College Educated | 0.78 | 0.36 | 0.52 |
| Less Than College | 0.02\*\* | 0.045\*\* | 0.48 |
| Blue Collar | 0.17 | 0.17 | 0.56 |
| White Collar | 0.53 | 0.62 | 0.54 |



Dependent Variable: Working

P-Values of Joint Tests for Significance: Months Since Birth \* Policy Coefficients

|  |  |  |  |
| --- | --- | --- | --- |
| Months Since Birth | -3 to +3 | 0 to + 6 | +3 to +9 |
| Full Sample | 0.89 | 0.98 | 0.99 |
| College Educated | 0.76 | 0.90 | 0.87 |
| Less Than College | 0.09\* | 0.24 | 0.67 |
| Blue Collar | 0.83 | 0.94 | 0.60 |
| White Collar | 0.65 | 0.95 | 0.94 |



Dependent Variable: Looking

P-Values of Joint Tests for Significance: Months Since Birth \* Policy Coefficients

|  |  |  |  |
| --- | --- | --- | --- |
| Months Since Birth | -3 to +3 | 0 to + 6 | +3 to +9 |
| Full Sample | 0.38 | 0.53 | 0.42 |
| College Educated | 0.38 | 0.38 | 0.37 |
| Less Than College | 0.77 | 0.49 | 0.44 |
| Blue Collar | 0.60 | 0.90 | 0.40 |
| White Collar | 0.47 | 0.51 | 0.65 |

Discussion

Of all sub-populations identified in the analysis, the effect of a paid leave policy on labor-force participation was only significant for women without college degrees. This effect was jointly significant for the seven-month period surrounding birth (*p* = 0.02). The effect was additionally jointly significant for month of birth to six months following birth (*p* = 0.045). However, there was no joint significance for months 3 to 9. These results suggest that, at least for women without college degrees, paid family leave does indeed strengthen labor-force attachments for the months surrounding pregnancy. However, there is no evidence for long-term effects of the policy on labor force participation of mothers. This evidence supports the theory that the main impact of the CA-PFL and NJ-PFL is to increase the probability of women continuing with their employer through pregnancy instead of temporarily dropping out of the labor-force. The fact that the effect is no longer significant six months after pregnancy suggests that the proportion of women remaining in the labor-force normalizes to pre-policy levels after a short interval.

The analysis of employment effects of paid leave policies yields joint statistical significance only for women without a college degree in the period of three months before birth to three months after birth (*p* = 0.09). This result corroborates the findings in the analysis of labor-force participation. Because the SIPP classifies women on paid family leave as working, this finding is likely a result of the same mechanism proposed in the labor-force analysis. That is, women are more likely to stay with their employer in the months of late pregnancy and early months post-birth if they have access to paid family leave.

Analysis of the impact of the CA-PFL and NJ-PFL on the probability of looking for work yield no statistically significance effect for any sub-population group at any time period. There are multiple possible explanations for this result. There could indeed be no effect of paid family leave policies on the probability of looking for work in any of the time periods analyzed, even with a positive effect on labor-force participation. Although the proposed mechanism of increasing ties to current employers would predict a negative effect of paid family leave on probability of searching for employment, a general increase in demand for employment by women as a result of the paid leave policy would yield a counteracting positive effect on the probability of searching for employment. However, I believe a more likely explanation is that the analysis simply lacks the statistical power to identify a causal effect. Out of 103,624 total person-month observations recorded in the sample, only 4,286 are encoded as looking for work. Under such circumstances, a small effect is unlikely to be identified under the specification used.

A further difficulty in interpreting the effect of paid family leave on employment outcomes is due to the nature of the specification. While a binary encoding is intuitive for labor-force participation, using a binary encoding of ‘working’ and ‘looking for work’ does not allow for a nuanced interpretation of effects. A change in the probability of working or looking for work reflects both changes in labor-force participation and changes in employment. Under the specification used for example, it is unclear whether the increase in probability of working for women without a college degree is due to women remaining employed when they would have otherwise dropped out of the labor-force, or women remaining employed when they would have otherwise become unemployed. If I were to redo this study from the beginning, I would use a multinomial logistic regression model to identify the relative changes of probability of each labor-market outcome compared to each other possible outcome.

Limitations

Empirical analysis of employer mandates is often difficult to examine with rigor due to the limitations of available data. Segmenting individual level data like the Survey of Income and Program Participation by geographical location and time period leads to small sample sizes and low statistical power. Aggregate data, on the other hand, lacks the granularity necessary to estimate the effect of the mandate on the subpopulations that are likely to be affect most by the mandate.

**Conclusion**

On the basis of the evidence gathered, a few qualified conclusions may be made. The California and New Jersey paid family leave policies do indeed seem to increase labor-force participation and employment for mothers without college degrees in the short-term period surrounding birth. The study reveals no significant effects for any of the other subgroups, or for any subgroup in the long-term.

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1. In terms of 2019 dollars [↑](#footnote-ref-1)
2. In terms of 2019 dollars [↑](#footnote-ref-2)
3. If an individual is away on paid leave, she is encoded by the SIPP as employed and working all weeks. [↑](#footnote-ref-3)
4. As some individuals were classified as unemployed or out of the labor-force immediately prior to birth or had missing data on occupation, the month relative to birth used to extract this information differs from individual to individual. [↑](#footnote-ref-4)