Does the Right to Work Part-Time Affect Mothers' Labor Market Outcomes?

Online Appendix

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Contents

A Further Background Information

A.1 Child Care in Germany

In Germany, municipalities and private, non-profit organizations, like churches and non-statutory welfare services are the main providers of child care for children between age zero and six.

For virtually all children aged three to six a subsidized child care slot was available in 1998, both in West and in East Germany. Whereas nearly all of these child care slots in East Germany were full-time slots, full-time child care slots in West Germany were only available for 19 % of children aged three to six (**?**).

As for the children aged zero to two, a child care slot was available for 3% of the children in West Germany and for 37% in East Germany. As pointed out by **?**, in West Germany, the labor force participation rate of mothers with children of age zero to two was much higher than the enrollment rate for paid child care during this time. Thus, a substantial fraction of working mothers with children in this age range relied on some non-paid child care, e.g. provided by grandparents or other family members.

As mentioned in Section 2, the size of the reform effects could potentially be heterogenous across German regions depending on the child care availability. This may determine whether mothers are able to make use of the new right to work part-time. Table 3 shows the results for mothers in West Germany.[1] While there are slight differences in the coefficients for West Germany relative to the baseline effects, these differences are not statistically significant. This could partly be due to the fact that during this time non-paid child care provided e.g. by the family was more prevalent. In addition, the small subsample of East German mothers may impede a more precise analysis of regional heterogeneities.

B Data and Identification

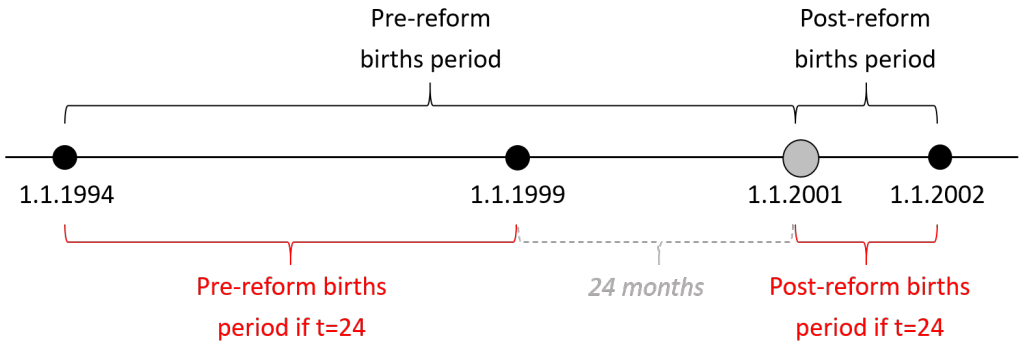
B.1 Timeline

As described in Section 3, I run Equation (1) and Equation (2) separately by time since childbirth. For the differences-in-differences analysis, I define the pre-reform sample in every regression so as to make sure that the time period since childbirth only covers the pre-reform period, i.e. I only include women who gave birth prior to the reform and for whom I also observe the outcome variables (t months after birth) prior to the reform. Appendix Figure B.1 shows an exemplary timeline: if the outcome variable is measured 24 months after childbirth,

[1] As pointed out in Section 4.4, East German mothers constitute only 17% of the sample, which prohibits an analysis based on East German mothers only.

I only include mothers in the pre-reform sample who gave birth on January 1, 1999 or earlier and check their labor market outcome of interest 24 months after childbirth. I use this strategy because eligibility according to the general part-time law was not assigned based on the timing of birth, but depended on the time of observation. Thus, women giving birth before the reform also gained the right to work part-time on January 1, 2001 if all pre-conditions were met.

Figure B.1: Timeline



B.2 Summary Statistics: Establishment

Appendix Table B.1 provides the mean and the median of the establishment size for the full sample, mothers belonging to the treatment group and for the control group.

Table B.1: Summary Statistics: Establishment Size

Variable	Full Sample	Treated	Control
Median	34	103	5
Mean	195.32	307.93	6.15

Notes: The table reports summary statistics on the number of employees of establishments where mothers worked before giving birth for the full sample, the treatment group and the control group.

B.3 Firm versus Establishment Data

As described in Section 3 and discussed further in Section 4.4, the data set reports the establishment size, while the right to work part-time according to the law is based on the employer size. The numbers of employees will coincide for all single-establishment firms, but not necessarily

for multi-establishment firms. If branches of a firm are located in the same municipality and are active in the same economic area, the data set treats them as being just one establishment. However, in all other cases it is not possible to observe the firm a particular branch belongs to. This means, that employees working for small firms with at most 15 employees and those working in establishments with more than 15 employees will be categorized correctly as being part of the control group in the former case and as being part of the treatment group in the latter. Only if establishments have at most 15 employees while the firm they belong to has more than 15 employees may I erroneously classify women in these establishments as being part of the control group (see Appendix Table B.2). If anything, this will induce a bias of my estimates towards zero. In Section 4.4, I check the robustness of the results and demonstrate that they are not very sensitive to the exclusion of industries where small branches are likely to be most prevalent.

Table B.2: Firm and Establishment Size

	Firm ≤ 15	Firm > 15
Establishment ≤ 15	✓	✗
Establishment > 15	✗	✓

B.4 Identification of Births

I follow Schönberg (2009) and Müller et al. (2017) to identify childbirths. To capture women of childbearing age, I restrict the sample to women between age 16 and age 40, and younger than 38 for their first (observed) childbirth. I only consider employment interruptions of at least 14 weeks which corresponds to the obligatory maternity leave period of 6 weeks before and 8 weeks after childbirth. Finally, I impose the gap between two consecutive births as at least 32 weeks. Following Schönberg and Ludsteck (2014), I compute the birthday of the child to be six weeks after the women went on leave (as the maternity leave in Germany starts 6 weeks before the due date). The results are robust to various alternative selection rules (see Section 4.4).

B.5 Transition between Large and Small Firms

Women may switch the employer in response to the reform to become eligible to work part-time, i.e. they may switch from small firms to firms with more than 15 employees. As discussed in Section 4.4 I address this concern by grouping mothers into the treatment and the control group based on the establishment size on or before the reform date, so that the treatment status is pre-determined. Moreover, I run robustness checks, where I assign the treatment status based

on the establishment size in September 2000, the first time that the bill was discussed. The results are very robust (see Table 5).

I further address the concern by studying transition probabilities between small and large establishment to assess the presence of such strategic firm changes. I compute transition probabilities between small establishments and large establishments between September 12, 2000 (date of the first newspaper article) and the birth date of the child. Appendix Table B.3 shows the likelihood of belonging to either the treatment or the control group at the time of birth ($Treat_i^{birth}$) versus the treatment status in September 2000 ($Treat_i^{sep}$). Only 1.58% of all mothers in the post-sample switched from an establishment with at most 15 employees in September 2000 to a large establishment with more than 15 employees before giving birth. Moreover, the symmetry of movements between small and large establishments is very high. This again alleviates the concern that mothers may have strategically responded to the reform by switching the firm.

Table B.3: Transition Matrix

	$Treat_i^{birth} = 0$	$Treat_i^{birth} = 1$
$Treat_i^{sep} = 0$	25.83	1.58
$Treat_i^{sep} = 1$	1.36	71.23

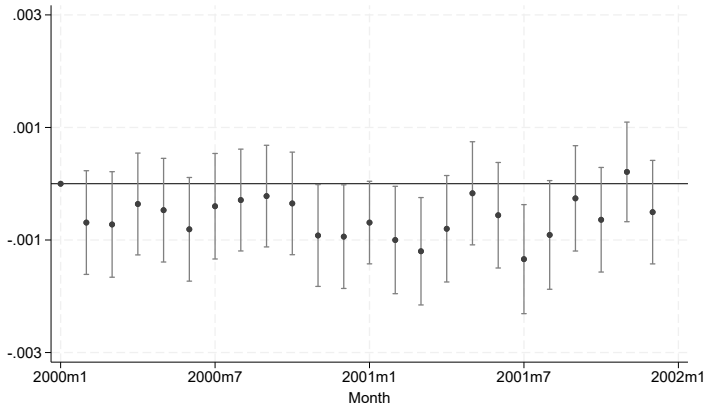
Notes: The table displays the probabilities of belonging to the treatment or the control group before giving birth ($Treat_i^{birth}$) and in September 2000 ($Treat_i^{sep}$), the time when the first articles about the bill were published in newspapers. 1 indicates the treatment group and zero indicates the control group.

B.6 Evolution of Births in Treatment and Control Group

If women altered their plans to have children based on eligibility, this could compromise the validity of the identification strategy. To further assess the importance of this concern, I consider the evolution of the relative number of births in the treatment and control groups (relative to the number of women of childbearing age in large and small establishments) before and after the reform.[2] For this purpose, I construct a sample consisting of all women who are employed in 2000 or 2001 and who are between 16 and 40 years old regardless of motherhood. I construct the data set at the monthly level. The data set consists of about 3,035,000 observations. I estimate the following regression:

[2]See Appendix Figure B.3 for the evolution of the absolute number of births in the treatment and control groups in the years 2000 and 2001. When comparing the absolute number of births in the treatment and control groups over time, I do not find evidence for a spike in the number of births in the treatment group after the reform either.

Figure B.2: Births in 2000 and 2001



Notes: The dots plot the coefficients of the interaction terms $Treat_i * Month_t$ in Equation (B.1). The bars depict 90% confidence intervals. I estimate a linear probability model. The dependent variable is a dummy, which is equal to one if woman i gave birth in month t and zero otherwise. Standard errors are clustered at the individual level.

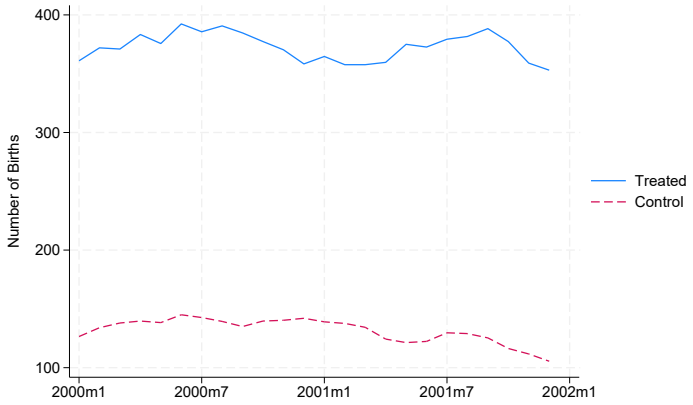
$$Birth_{it} = \alpha_0 + \alpha_1 Treat_i + \sum_{t=2000m2}^{2001m12} \delta_t Month_t + \sum_{t=2000m2}^{2001m12} \gamma_t Treat_i * Month_t + u_{it} \tag{B.1}$$

The variable $Birth_{it}$ is a dummy, which is equal to 1 if individual i gives birth to a child in month t and zero otherwise. $Treat_i$ is equal to one for women employed in establishments with more than 15 employees and zero otherwise. I include month fixed effects and interactions of the treatment status and the monthly dummies in the regression. The standard errors u_{it} are clustered at the individual level. As described before, women may have first learned about the new law in September 2000. If the decision to become a mother was affected by the (expected) legal change, then we would expect the coefficients of the interaction terms of the monthly dummy and the treatment group dummy to be statistically significant starting from June 2001, i.e. about 9 months after the first announcement of the law. Figure B.2 reports the results. The estimation results do not suggest a significantly different evolution of births in the treatment and the control group between July 2001 and December 2001.

B.7 Covariate Balance Test

As described in Section 3, to further check for concerns of systematic sample selection, I compare mothers giving birth before and after the reform, in the treatment and control groups in terms of their pre-determined observable characteristics. Appendix Table B.4 presents the results from estimating differences-in-differences regressions based on Equation (1), using the different control variables from the baseline specification as the dependent variable. Appendix

Figure B.3: Evolution of Number of Births



Notes: The Figure plots three month moving averages of the number of births in the treatment and control groups in 2000 and 2001.

Table B.4 suggests that differences are small and statistically insignificant.

Table B.4: Covariate Balance Test

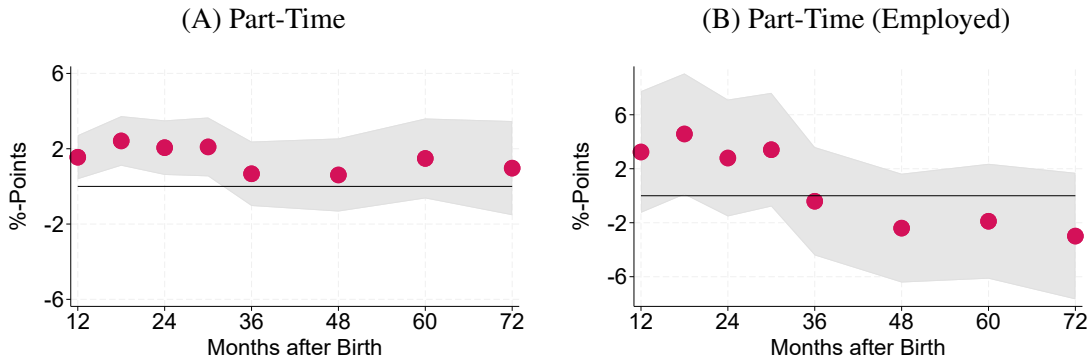
Variable	Coefficient of interaction term	Standard Error
Age	0.207	0.131
Full-Time	-0.014	0.011
Education	-0.006	0.018
Log Daily Earnings	0.012	0.016
Observations	51,512	

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. The table shows differences-in-differences estimates based on Equation (1), using the different pre-birth characteristics as the dependent variable. The last column shows robust standard errors.

C Further Results

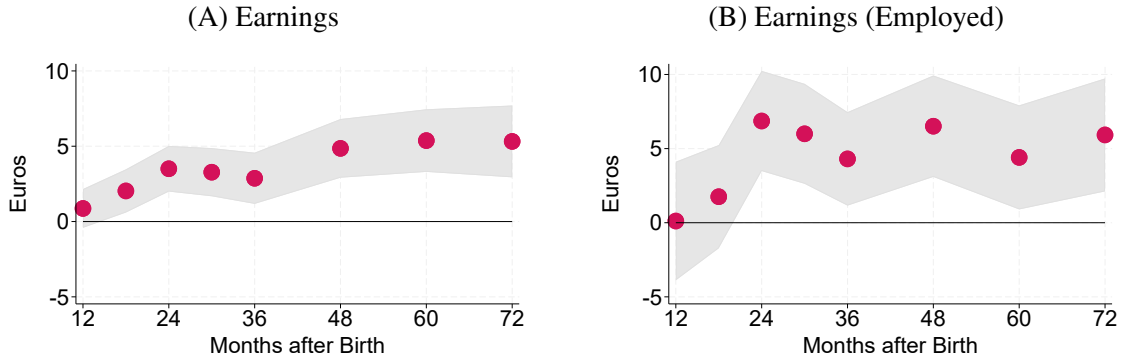
C.1 Additional Figures

Figure C.1: Part-Time Status



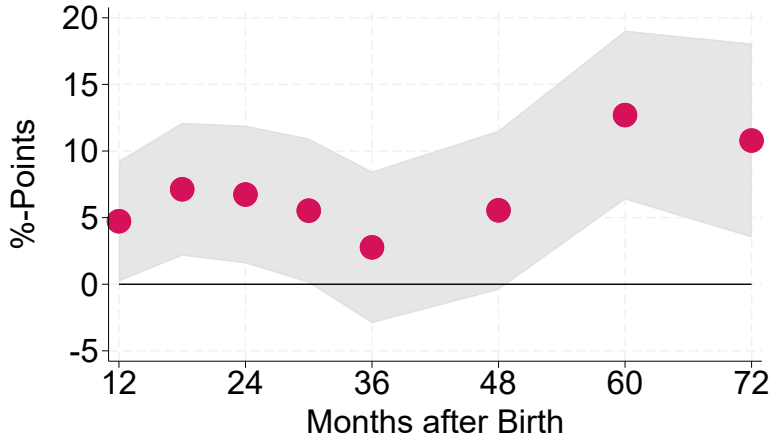
Notes: The dots plot the coefficients of the interaction term $Post_i * Treat_i$ in Equation (1) for the different outcome variables specified at the top of the sub-figures t months after birth (x-axis). The results are based on OLS estimations. Individual controls are age, age squared, dummies for ISCED education levels, log earnings, full-time status, and dummies for the one-digit firm industry of mothers determined prior to childbirth i. The gray areas represent 90% confidence intervals.

Figure C.2: Labor Market Earnings



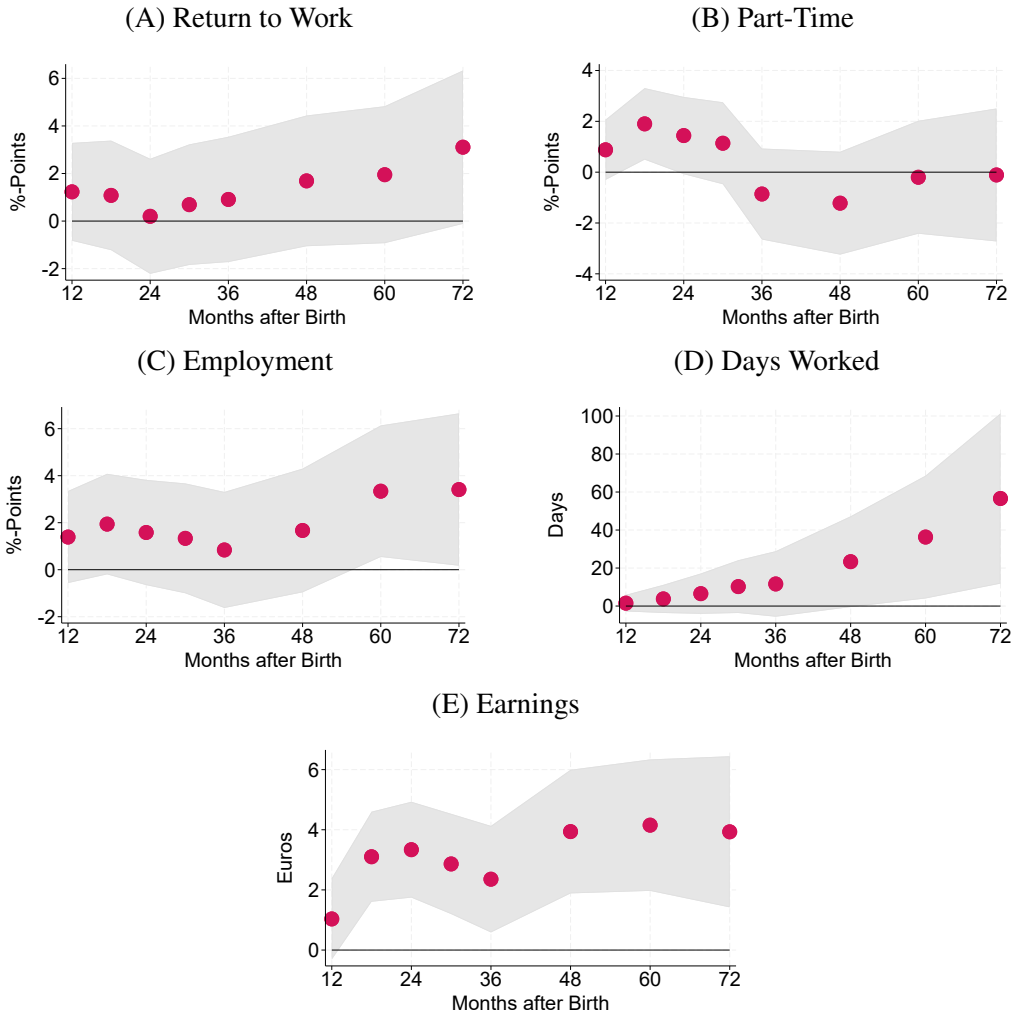
Notes: The dots plot the coefficients of the interaction term $Post_i * Treat_i$ in Equation (1) for the different outcome variables specified at the top of the sub-figures t months after birth (x-axis). The results are based on OLS estimations. Individual controls are age, age squared, dummies for ISCED education levels, log earnings, full-time status, and dummies for the one-digit firm industry of mothers determined prior to childbirth i. The gray areas represent 90% confidence intervals.

Figure C.3: Skill Level: Categorical Measure



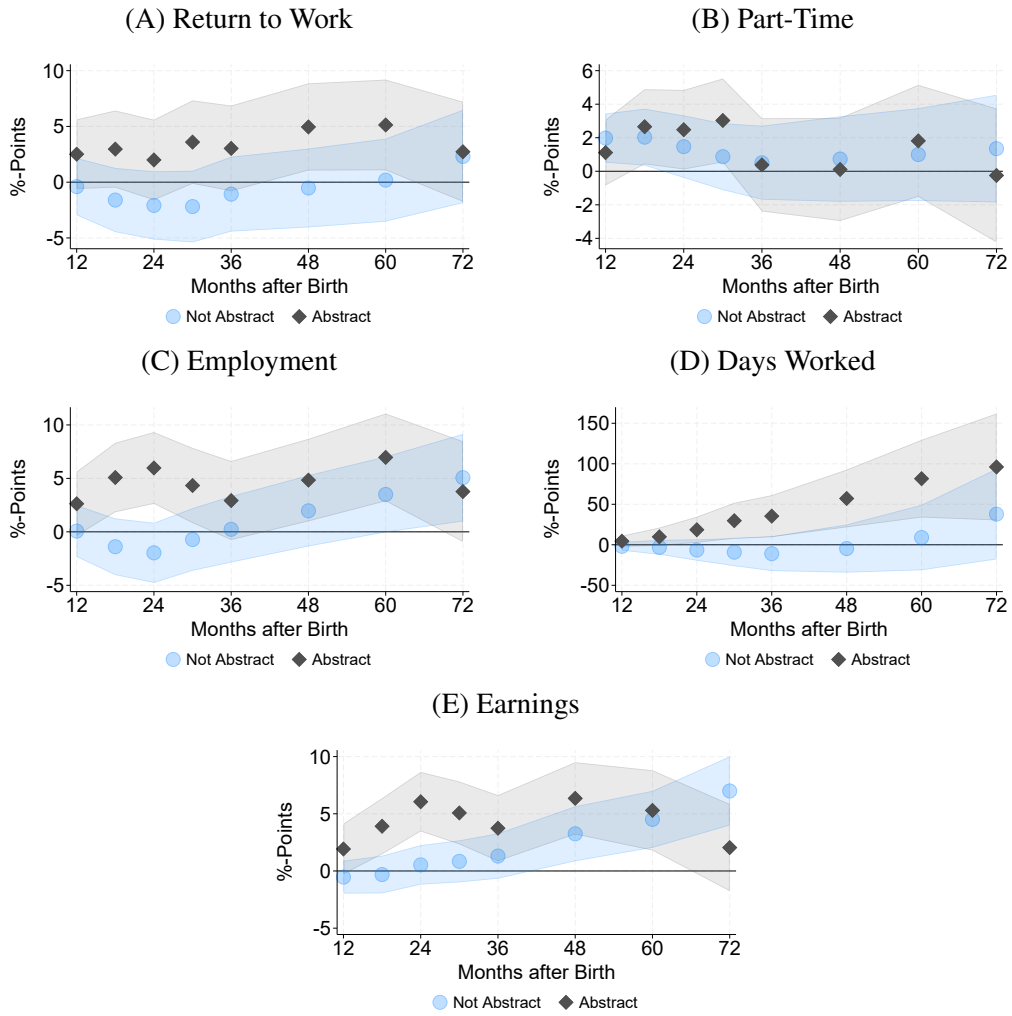
Notes: The dots plot the coefficients of the interaction term $Post_i * Treat_i$ in Equation (1). The dependent variable is a categorical measure of the skill requirements in the job of the mother t months after birth (x-axis). The coding of the variable is as follows: 0-mother is not employed, 1-unskilled or semi-skilled occupation, 2-skilled occupation, 3-complex occupation, 4-highly complex occupation. The results are based on OLS estimations. Individual controls are age, age squared, dummies for ISCED education levels, log earnings, full-time status, and dummies for the one-digit firm industry of mothers determined prior to childbirth i. The gray areas represent 90% confidence intervals.

Figure C.4: Robustness: Excluding Large Establishments



Notes: The dots plot the coefficients of the interaction term $Post_i * Treat_i$ in Equation (1) for the different outcome variables specified at the top of the sub-figures t months after birth (x-axis). The results are based on OLS estimations. Individual controls are age, age squared, dummies for ISCED education levels, log earnings, full-time status, and dummies for the one-digit firm industry of mothers determined prior to childbirth i. The gray areas represent 90% confidence intervals. The treatment group is restricted to mothers working in small and medium sized establishments, i.e. establishments with up to 500 employees.

Figure C.5: Heterogeneity by Occupation



Notes: The diamonds and the dots plot the coefficients of the interaction term $Post_i * Treat_i$ in Equation (1) for the different outcome variables specified at the top of the sub-figures t months after birth (x-axis), for the sub-samples with non-abstract occupations and abstract occupations, respectively. The results are based on OLS estimations. Individual controls are age, age squared, dummies for ISCED education levels, log earnings, full-time status, and dummies for the one-digit firm industry of mothers determined prior to childbirth i. The bars represent 90% confidence intervals.

C.2 Additional Tables

Table C.1: Transitions from Part-Time to Full-Time

	Part-Time to Full-Time
Coefficient	0.026*
	(0.015)
Observations	22,415

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. The results are based on OLS estimations of Equation (1). Robust standard errors are reported in parentheses. Individual controls are age, age squared, dummies for ISCED education levels, log earnings, full-time status, and dummies for the one-digit firm industry of mothers determined prior to childbirth *i*. The dependent variable is equal to one if the mother has switched from part-time to full-time employment at least once between birth *i* and year six after childbirth

Table C.2: Donut Hole-Specification

Time since Birth	18 Months	36 Months	72 Months
Return to Work	0.009	0.010	0.038
	(0.014)	(0.016)	(0.019)
Part-Time	0.023***	0.004	0.008
	(0.009)	(0.011)	(0.016)
Employment	0.020	0.016	0.048**
	(0.013)	(0.015)	(0.020)
Days Worked	4.412	12.685	77.291***
	(4.457)	(10.392)	(27.017)
Earnings	2.322***	2.802***	5.338***
	(0.900)	(1.071)	(1.479)

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. The results are based on OLS estimations of Equation (1). Robust standard errors are reported in parentheses. Establishments around 15 employee threshold with 13 to 18 employees are excluded from the regression. I control for the following characteristics of mothers determined prior to childbirth *i*: age, age squared, dummies for ISCED education levels, log earnings, full-time status, and dummies for the one-digit firm industry.