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**Conference Paper**

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Beiträge zur Jahrestagung des Vereins für Socialpolitik 2025: Revival of Industrial Policy

**Provided in Cooperation with:**

Verein für Socialpolitik / German Economic Association

*Suggested Citation:* Riepl, Franziska (2025) : Human Capital and Motherhood: The Extent and Implications of Child Penalties in Training, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2025: Revival of Industrial Policy, ZBW - Leibniz Information Centre for Economics, Kiel, Hamburg

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# Human Capital and Motherhood: The Extent and Implications of Child Penalties in Training

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*Work in Progress*

January 31, 2025

## Abstract

Despite continued policy efforts, gender gaps remain persistent in the labor market, and most of the remaining gaps are linked to motherhood. Human-capital related explanations are frequently advanced, claiming that mothers' human capital depreciates during their absence from the labor market and that they continue to accumulate less human capital even after returning, for example because they only work part-time or in jobs with little potential for career development. However, it is unclear how human capital is acquired and maintained at the workplace and how this process differs across individuals and firms. This paper investigates the dynamics of explicit learning activities, namely job-related training in the form of organized courses, in the context of motherhood. Training is pervasive throughout many jobs, is linked to increases in wages, employment and job satisfaction, and is considered crucial for navigating transformations of the labor market. Using linked educational and labor market data from Germany and a dynamic difference-in-differences strategy, I show that the likelihood of women's training participation decreases by 18 percentage points (over 30% of the pre-birth training rate) after having children. Around one third of the training penalty can be attributed by reduced employment and hours, but a sizeable share remains unexplained. I show that mothers exhibit no substantial shifts into firms with lower training support, highlighting the role of the individual participation choice as well as intra-firm allocation decisions. I connect this finding to the literature on motherhood gaps and returns to training, establishing that earnings fall by 47% (18% conditional on employment and part-time status) as a result of motherhood and that training yields a return of 3% in earnings each year. Thus, changes in training participation after motherhood cannot rationalize earnings losses in the short run, but can potentially help to explain earnings changes as the training losses accumulate over time.

*Keywords:* motherhood, gender gaps, job-related training, human capital, difference-in-differences

*JEL codes:* C23, J16, J24, M53

# 1 Introduction

Gender gaps in the labor market are a well-documented fact in the economic literature (e.g. Cortés and Pan, 2023; Waldfogel, 1998), and recent research associates most of the remaining gaps with motherhood (e.g Cortés and Pan, 2023; Goldin, 2024). One key explanation for motherhood “penalties” in terms of wage and employment is that mothers’ human capital depreciates during their absence from the labor market (Becker, 1964; Mincer and Polacheck, 1974) and that they continue to accumulate less human capital even after returning, for example because they only work part-time or in jobs with little potential for career development (Adda et al., 2017; Blau and Kahn, 2017).

Oftentimes, experience is used as a proxy that is supposed to capture human capital accumulation after secondary education (e.g. as in Costa Dias et al., 2020). This approach yields an outcome that is easily measurable and clearly relatable to labor market outcomes<sup>1</sup>. However, using this approach, it remains unclear how exactly human capital is acquired at the workplace, whether there are heterogeneities across individuals and firms, and how policy interventions may facilitate or hamper human capital acquisition, especially by mothers.

In order to learn more about these processes and to shed light on their implications for broader outcomes, this paper focuses on one particular element of work-related human capital formation, namely job-related training<sup>2</sup>. This concept broadly refers to learning activities that are not part of formal (post-)secondary education, but that occur in a structured setting with the goal of acquiring new skills that are useful for one’s current or future work (BMBF, 2022). These learning activities can happen both privately and at the workplace, though the latter constitutes the clear majority of activities.

Such activities are regarded as pivotal for navigating global challenges like the demographic, green, and technological transitions, which necessitate skill acquisition during working life, both to stay up to date in one’s current area of work and to react to an increasing need for mobility across occupations and sectors (OECD 2021; BAMS & BMBF 2022; ILO 2023). Works from the fields of sociology and education studies have examined gender differences in training participation (Dämmrich et al., 2015; Evertsson, 2004; Massing and Gauly, 2017; Wotschack, 2019) and have documented lower training participation of mothers (Lim and dos Santos, 2024; Zoch, 2023). This paper builds upon this observation by providing (as close as possible to) causal estimates of motherhood penalties in job-related training. I use a staggered difference-in-differences framework to obtain dynamic estimates of reductions in training participation over time. Additionally, using linked training and administrative earnings data, I test explanatory mechanisms such as sorting into different types of firms, and I add novel evidence on the wage and employment consequences of the motherhood training penalty.

I find that first-time mothers reduce their training participation by 18 percentage points (pp), amounting to a reduction in training by ca. 30%, whereas fathers expe-

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<sup>1</sup>Blau and Kahn (2017) estimate that experience variables explain 21.1% of the gender wage gap in 1980 and still 14.1% in 2010. Experience was the single most important explanatory factor in 1980 and still remains the third most important factor, after occupation and industry.

<sup>2</sup>Theoretic textbook approaches as in Blau and Winkler (2018) differentiate between three types of human capital acquisition related to work: job-related formal training, informal training (learning from coworkers) and learning through practice and repetition (learning by doing)

rience no significant drop. Half of the effect for women can be explained by reduced working time, whereas the remaining reduction remains largely unexplained. Training participation is found to have a small positive effect on earnings of 3% annually, but the estimated returns to training together with the partial reduction in training participation are not enough to explain a meaningful share of the motherhood earnings penalty in the short-term. Nevertheless, disparities in training participation may matter in the long run as differences in training participation accumulate.

In the following, I will provide an overview of training measures and existing evidence in Section 2 before introducing the data and estimation strategy in Sections 3 and 4. I will present results on the training penalty in Section 5, including robustness checks, heterogeneity analyses, and an analysis of sorting across firms. The second part of the paper will relate the findings on training participation to motherhood earnings and employment gaps (Sections 6.1-6.3) as well as returns to training (Sections 6.4-6.6) before concluding in Section 7.

## 2 Background

The literature shows that the largest determinant of training participation is the firm environment, in line with the fact that most training is firm-financed. Acemoglu and Pischke (1998) demonstrate theoretically that firms facing less labor market competition offer more training, and Rzepka and Tamm (2016) confirm this empirically for Germany. Anger et al. (2023) and Heß et al. (2023) show that workers who face automation receive less training at their current workplace, and Lammers et al. (2023) show that works councils can increase training.

Despite the dominant role of firms, individual characteristics and demographic patterns play a non-negligible role in determining training participation as well. The education literature documents a clear gradient in age (Schmidt-Hertha and Müller, 2017) and education (Kramer and Tamm, 2018), with further individual characteristics like personality (Laible et al., 2020) playing a minor role. Gender differences are ambiguous and vary across countries, industries and types of training (Dämmrich et al., 2015; Evertsson, 2004; Massing and Gaulty, 2017; Wotschack, 2019), with a tendency for lower participation in employer-financed (but not other types of) training by women. A much clearer pattern emerges when focusing on gender differences in the context of parenthood: Lim and dos Santos (2024) and Zoch (2023) both document a marked decrease in post-birth training participation by mothers but not by fathers.

I build upon this work by applying a staggered difference-in-differences strategy to German panel data to identify the causal effect of motherhood on training participation. I focus on the estimation of dynamic effects, addressing issues of selection and anticipation as well as concerns regarding standard two-way fixed effects (TWFE) estimators in the presence of treatment heterogeneity. Additionally, I leverage linked firm data to examine the role of worker-firm sorting and firms' training provision for the motherhood training penalty. In the following, I will provide a brief theoretical framework for explaining motherhood effects on training participation.

Childbirth may affect human capital formation and labor market outcomes more generally in various ways. Both individuals and firms adjust their behavior to the new

situation, leading to changes within the same job as well as the formation of new worker-firm matches. Accordingly, the potential mechanisms behind changes in training participation can be grouped into three main components: (1) individual behavior, (2) firm behavior, and (3) compositional effects or changes in worker-firm matches.

**Individual behavior** Parents, and in particular mothers, may use existing offers for training less due to two main reasons. The first is the increase in opportunity costs of time and resources, e.g. due to childcare responsibilities. In a stressful environment juggling work and family responsibilities, dedicating efforts to continued learning may take a backseat compared to other activities. The second element is a decrease in career ambitions, both due to shifted priorities and because of anticipating the difficulties that child-related absences from work as well as discriminatory behavior from the side of employers (Jessen et al., 2019; Ishizuka, 2021), pose.

**Firm behavior** Firms are also expected to exhibit different behavior towards individuals with children. They are likely to offer less training to workers which they perceive as less career-oriented (Correll et al., 2007) or which they think will benefit less from the training due to their time constraints and reduced hours. Such behavior may explicitly target parents, but also more generally individuals who work part-time and in non-leadership positions. As women perceive that they are being put on the “mommy-track” (Lommerud et al., 2015; Lucifora et al., 2021), they may subsequently reduce their own ambitions and willingness to participating in training even further, leading to a vicious cycle of non-participation.

**Compositional Effects** Because training provision is associated with a host of firm characteristics, particularly firm size and industry, it appears logical that sorting across firms should also affect training participation. This sorting effect can be an expression both of worker-side (e.g. lower career ambitions and stronger preferences for amenities such as family-friendly work hours) and firm-side (e.g. discriminatory hiring and promotion practices) adjustments. There is an extensive body of research on the role of workplace characteristics for the gendered divergence of careers after childbirth (e.g. Cortés and Pan, 2023; Goldin, 2014). Casarico and Lattanzio (2023), for example, show that in Italy women work in firms with lower productivity, sales, capital and wages. Wilner (2016), however, finds no effect of workplace characteristics on the motherhood earnings gap in France. As this hypothesis remains unanswered, it is useful to examine whether in the context presented here, mothers sort into different types of firms and whether these firms have characteristics that are associated with lower training provision.

### 3 Data

The sample is constructed from two separate data sources which can be linked via individual-level identifiers. I first describe how each of these datasets is structured and then provide an overview of the final combined sample.

**NEPS** The National Educational Panel Study (NEPS) is a education-focused longitudinal survey spanning the years 2009-2021. I use the Starting Cohort 6, consisting of adults born in 1944–1986, and which focuses on lifelong learning and educational activities after formal education has been completed. First, the NEPS sample is transformed into a yearly panel, and information on training activities is extracted from various types

of spell data, based on the procedure outlined in Grüttgen et al. (2023).<sup>3</sup> I focus attention on training that is job-related, as this type is expected to be more closely linked to labor market outcomes. Training is considered to be job-related if it is either explicitly designated to be so (based on a question that is asked for a subset of the training entries) or if the training was reported in the context of an employment spell.

Additionally, the NEPS contains childbirth histories for all interviewed individuals, which I use to construct my treatment variable. I restrict the sample to biological children and construct dummy and timing variables based on the birth years of the children.<sup>4</sup> Around three quarters of the individuals in the raw sample are parents (comparable with the numbers in official statistics) and 92% of mothers are between 20 and 40 years old at the time of birth, with the most common age at first birth being 27. To increase comparability, I restrict the sample to individuals who experience their first birth between the ages 20 and 40 and who I observe no more than 10 years after childbirth, if applicable.<sup>5</sup>

I restrict the sample to individuals aged 25-50 in the survey years 2009-2021 (birth years 1944-1986) who have been employed for at least one month in the past<sup>6</sup>. I remove partially or fully retired individuals, those who are still in full-time education (vocational training), and individuals who report working in the military. Additionally, individuals with missing information on training participation and those with only one observation in the sample (and therefore not suitable for the estimation of individual fixed effects) are removed.

**ADIAB** The Integrated Employment Biographies (IEB) are provided by the German Federal Employment Agency and are based on mandatory notices for dependent employees for social security purposes. The ADIAB (“Administrative Daten des Instituts für Arbeitsmarkt- und Berufsforschung”) dataset provides the employment spells of all individuals in the NEPS who agreed to have their information matched, and for whom a corresponding entry could be identified based on first and last name, date of birth, gender, and address.<sup>7</sup> The spell data is transformed into a yearly panel with information on daily earnings following the procedure outlined in Stüber et al. (2023).<sup>8</sup> The data also contains establishment characteristics, which can be associated with each spell based on a firm identifier that can be linked to the Establishment History Panel (BHP).

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<sup>3</sup>Trainings that span multiple years are split over the years (at a monthly level), and the training time (hours) is split up accordingly. For courses for which I only know the length of the associated (employment) spell, but not the length of the training spell itself, I randomly allocate the course to a month out of the entire time the training spans, allocate the training start to this date and assign a year accordingly.

<sup>4</sup>I additionally restrict the sample to children with known birth years, which are validated based on the requirement that the individual must have been at least 10 years old at birth to dismiss clearly misreported information.

<sup>5</sup>This is similar to the procedure used by Fitzenberger and Seidlitz (2024), who use ages 21-40 and remove women who already have children at their event time of interest. Similarly, Jessen et al. (2024) generate a panel matched on age and study outcomes up to ten years after birth.

<sup>6</sup>This applies to 99.91% of individuals.

<sup>7</sup>82% of all NEPS respondents agreed to have their data linked – the initial consent rate was even higher at 93%, but some individuals withdrew their consent or stopped participating entirely. 90% of the 14,065 individuals available for linkage could ultimately be linked, see Bachbauer and Wolf (Bachbauer and Wolf).

<sup>8</sup>For each individual, one spell per year is used as the main spell. Priority is given to the longest spell in each year, using the spell with the higher income in case of parity. Additional statistics on *total* working days, earnings and days of unemployment are recorded as well.

**Joint Sample** This comprehensive employment information is then merged to the survey information at the individual-year level, creating a panel with rich information on both individual and work-related characteristics. The sample is further conditioned on the availability of key variables<sup>9</sup>, leveraging the combined information from both sources to reduce attrition. The final sample consists of 21,276 observations from 3,963 individuals (2,149 men and 1,818 women), with 271 first births for women and 301 for men observed within the sample period.

Tables and 9 report summary statistics for the full sample, separately by training participation status.<sup>10</sup> Individuals are on average 37 years old, have almost 15 years of education and more than 90 percent are employed. Slightly more than half of them participate in further education, completing 1.8 courses for a total of 57 hours on average. Individuals who participate in training in a given year are more likely to be employed, tend to work for slightly larger employers and in the public sector, and are less likely to have any children. This difference is particularly strong for women, where 49% of all individuals in the sample have children, but only 44% of those who participate in further education.

Figure 1 further illustrates that having a child matters for training participation, and that it does so particularly for women. The figure compares men and women who eventually have a child before and after becoming parents. Over 60% of women and 55% of men who do not have a child yet participate in training. After childbirth, both men and women reduce their training participation, with levels dropping to less than 50% for both genders. However, the drop for women is much larger, and it is primarily driven by job-related training. This observation motivates a further casual analysis that will follow below.

## 4 Empirical Strategy

The starting point of my analysis is a difference-in-differences framework with staggered treatment, as presented in Equation 1.

$$Y_{it} = \alpha child_{it} + \beta X_{it} + \mu_i + \gamma_t + \varepsilon_{it} \quad (1)$$

This setup compare the training outcomes  $Y_{it}$  for individuals with and without children (dummy  $child_{it}$ ) while controlling for time-invariant individual-level characteristics  $\mu_i$  and year-fixed effects  $\gamma_t$ . All specifications also control for a quadratic in age to capture potential nonlinear dynamics over time. Additional controls can be included in  $X_{it}$ . Different set of time-varying individual- and job-level characteristics are added depending on the specification (all subsumed under  $\beta X_{it}$ ).

Thus, identification of the main coefficient of interest  $\alpha_i$  relies on individuals who have their first child within the sample period<sup>11</sup>. The coefficient  $\alpha_i$  then measures the average within-person change in training for all observed time periods after birth. However, recent

<sup>9</sup>Gender, birth year, education, East vs West German, work experience, dummy for training participation, number of children

<sup>10</sup>Summary statistics for women only are presented in Table 1 in the Appendix.

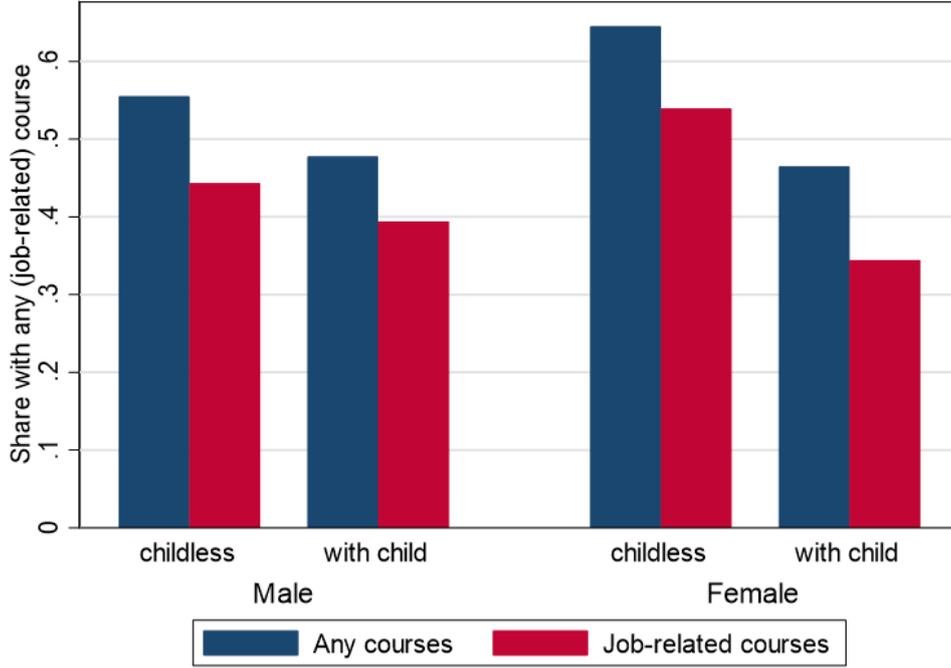
<sup>11</sup>There are 301 women and 271 men who experience a first birth during the sample period (16.6% of all women and 12.6% of men).

Table 1: Summary Statistics

|                              | No Further Education | Participated   | Total          |
|------------------------------|----------------------|----------------|----------------|
| Age                          | 37.10 (6.20)         | 36.83 (6.32)   | 36.97 (6.27)   |
| Female                       | 0.43 (0.49)          | 0.48 (0.50)    | 0.46 (0.50)    |
| Years of education           | 14.62 (2.31)         | 15.16 (2.21)   | 14.90 (2.27)   |
| East German                  | 0.18 (0.39)          | 0.15 (0.36)    | 0.17 (0.37)    |
| Household size               | 2.65 (1.24)          | 2.54 (1.24)    | 2.60 (1.24)    |
| Any children                 | 0.46 (0.50)          | 0.42 (0.49)    | 0.44 (0.50)    |
| Employed                     | 0.95 (0.22)          | 0.98 (0.16)    | 0.96 (0.19)    |
| Gross income                 | 3,271 (2,378)        | 3,751 (2,526)  | 3,517 (2,466)  |
| Parental leave (months/year) | 0.45 (2.02)          | 0.23 (1.40)    | 0.34 (1.74)    |
| Part-time                    | 0.33 (0.47)          | 0.31 (0.46)    | 0.32 (0.47)    |
| Experience                   | 13.79 (6.87)         | 13.68 (6.80)   | 13.73 (6.84)   |
| Tenure                       | 6.69 (6.04)          | 6.42 (5.83)    | 6.55 (5.93)    |
| Employer size category       | 2.58 (1.22)          | 2.73 (1.20)    | 2.66 (1.21)    |
| Public sector                | 0.19 (0.39)          | 0.28 (0.45)    | 0.23 (0.42)    |
| Fixed-term contract          | 0.27 (0.44)          | 0.26 (0.44)    | 0.27 (0.44)    |
| Self-employed                | 0.11 (0.31)          | 0.10 (0.29)    | 0.10 (0.30)    |
| Leadership position          | 0.27 (0.44)          | 0.31 (0.46)    | 0.29 (0.45)    |
| Any courses                  | 0.00 (0.00)          | 1.00 (0.00)    | 0.51 (0.50)    |
| Any job-related courses      | 0.00 (0.00)          | 0.82 (0.38)    | 0.39 (0.49)    |
| Number of courses            | 0.00 (0.00)          | 1.77 (1.16)    | 0.89 (1.21)    |
| Hours of courses             | 0.00 (0.00)          | 57.53 (139.95) | 28.12 (101.99) |
| Two courses or more          | 0.00 (0.00)          | 0.44 (0.50)    | 0.22 (0.42)    |
| At least 20h of training     | 0.00 (0.00)          | 0.56 (0.50)    | 0.28 (0.45)    |
| One week of training         | 0.00 (0.00)          | 0.33 (0.47)    | 0.16 (0.37)    |

advances in the difference-in-differences literature show that estimating this equation using a simple two-way fixed-effects (TWFE) estimator introduces biases due to heterogeneity in the treatment effect across individuals and time (Callaway and Sant’Anna, 2021; Roth et al., 2023; Gardner et al., 2024). The estimate obtained using TWFE is a weighted average of  $2 \times 2$  comparisons of treated and untreated observations over time. As the weighting scheme is not explicitly known by the researcher, this makes the estimates difficult to interpret, and they may not refer to a policy-relevant measure. Additionally, in the case of staggered treatment, already-treated units may be used as a comparison for later-treated units, a problem often referred to as “forbidden comparisons” (Roth et al., 2023). To address these shortcomings, I use the two-stage difference-in-differences estimator proposed by Gardner et al. (2024). This estimator uses an intuitive procedure similar to the two-stage least-squares method for instrumental variable estimation, whereby individual and time fixed effects are estimated separately from treatment effects in order to rule out that the estimated fixed effects can be “contaminated” by the treatment. The first stage is estimated only observations from non-treated and not-yet treated units, i.e. in this case individuals who are childless. Individual fixed effects  $\mu_i$  and time fixed effects  $\gamma_t$  as well as control variables  $\beta X_{it}$ , if desired, are then regressed on the training outcome  $Y_{it}$ , as presented in Equation 2.

Figure 1: Motivating Statistic – Training Participation



$$Y_{it} = \mu_i + \gamma_t + \beta X_{it} + \epsilon_{it} \quad \forall \text{ observations where } child_{it} = 0 \quad (2)$$

The predicted values for the fixed effects and for  $\beta$  are then included in the second stage, which takes on the usual form of a TWFE estimation (Equation 3).<sup>12</sup>

$$\tilde{Y}_{it} = \alpha child_{it} + \hat{\beta} X_{it} + \epsilon_{it} \quad (3)$$

To show heterogeneity of the treatment effect over time more explicitly, I additionally estimate event-study specifications of the same regression, replacing the dummy  $child_{it}$  with a set of indicators for the year relative to birth  $YPB_t$ , capturing effects for up to five years before and ten years after birth (Equation 4).

$$\tilde{Y}_{it} = \sum_{-5 \geq y \geq 10, y \neq -1} \alpha_y \mathbb{1}\{YPB_{it} = y\} + \hat{\beta} X_{it} + \epsilon_{it} \quad (4)$$

<sup>12</sup>Alternatively, this can also be understood as regressing the dummy  $child_{it}$  on the residualized outcome  $\tilde{Y}_{it} = Y_{it} - \hat{\mu}_i - \hat{\gamma}_t$

## 5 Results - Training Participation

Figure 2 shows the evolution of women’s training participation after childbirth. Participation rates drop by 20 pp in the year of birth and close to 30 pp in the year thereafter – an enormous drop compared to baseline participation rates around 50 percent. The training participation then starts to recover and the difference between mothers and non-mothers becomes insignificant five years after childbirth, but the point estimates remain negative for the entire sample period. The pattern resembles that of employment after motherhood as known from the literature (e.g. Fitzenberger et al., 2013, for Germany), suggesting that absences from the labor market may be a key driver of the training penalty. Indeed, controlling for months of employment and part-time status, as indicated by the red line in the left panel of Figure 3, reduces the gap by five to ten percentage points throughout the entire sample period. Adding additional controls further reduces the gap and even indicates an insignificant “catching up effect” in the long-term, as shown in the right panel of Figure 3.<sup>13</sup> However, a significant drop in the first two years after childbirth remains in all specifications. On the contrary, men do not experience any systematic shifts in their training behavior, as evidenced by Figure 6 in the Appendix.

Repeating the same exercise in a static-setting, i.e. including only a dummy for having a child instead of the event-time dummies, confirms the findings from above and provides a useful reference point for later analyses. The average training penalty for women is 17.6\*\*\* pp without using any controls and 11.2\*\*\* pp when holding time in employment and part-time status constant (see Table 10 in the Appendix for details). Adding labor market history and job characteristics further reduces the size of the average gap to 6.5 and 3.3 pp, respectively. For men, insignificant effects between -1.4 and -3.2 pp are observed.

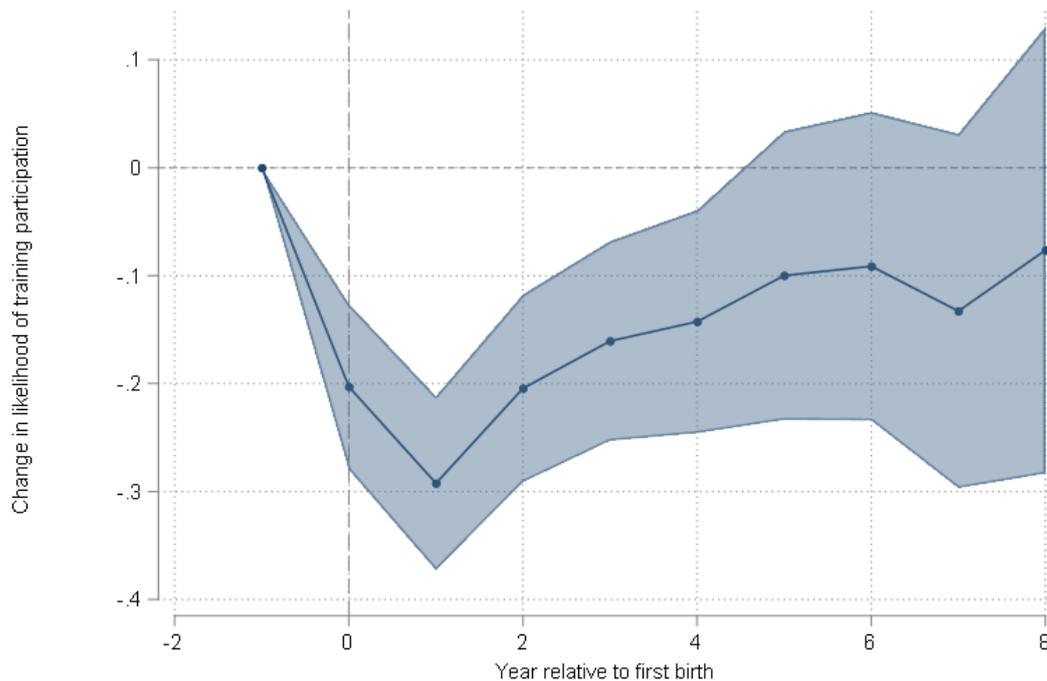
### 5.1 Robustness checks

As a robustness check, I also examine the effect on various margins of training intensity beyond a simple participation dummy. First, I regress the number and hours of training, respectively, on a dummy for having a child and the same set of control variables as before. The results in Table 2 show that the number of trainings decreases, in line with the decrease in overall training participation. Overall hours of training are imprecisely estimated and show no clear trend. The reduction in the number of trainings is driven by an extensive margin response of individuals stopping to train entirely. For those who continue to participate, however, both the number and hours of training tend to go up. One has to caution, however, that especially the hours of training contain a lot of outliers

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<sup>13</sup>Note that the set of control variables used here are currently subject to revisions as they may be over-controlling for characteristics such as being in a leadership position that are supposed to change over time. Additionally, the fact that the coefficients for all control variables have to be estimated from pre-treatment data only further reduces the sample size and makes the estimates noisy. Instead of the extensive set of job characteristics used at the moment, I plan on using indicators of one-digit occupation and sector codes in the future. Preliminary analyses show that these controls induce important changes when added into the full specification, but do not significantly change the results when added on top of employment status and labor market history controls.

Figure 2: Event-Study Estimates of the Effects of Childbirth on Training Participation – Women



and inconsistencies. Therefore, that measures taken at discrete steps in the data may therefore be more robust.

To address this issue and to allow for the easier inclusion as a control variable later on, in Table 3 I re-estimate the same regressions using various discrete cutoffs as outcome variables. All three measures confirm the existence of sizeable training penalties, with slightly smaller penalties for larger cutoffs, in line with the hypothesis that it is primarily individuals with low training intensity that are already close to the cutoff of non-participation who decide to stop training altogether after having a child.

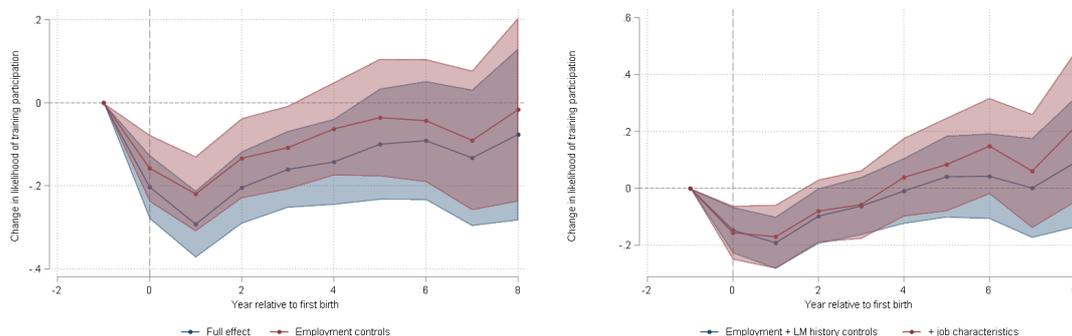
## 5.2 Cumulative results

In this Section, I plan on adding evidence on how the *cumulative* training participation changes over different horizons (e.g. three, five, eight years) after childbirth. This is an important measure with regards to relating training participation to the motherhood earnings gap, as it is the total sum of missed trainings up to a certain point in time that is relevant for explaining wage differentials at that point in time.

## 5.3 Heterogeneities in Content and Financing of Training

Next, I examine differences in the effect of childbirth on different types of training and on different groups of treated individuals. Figure 4 compares the decrease in job-related

Figure 3: Event-Study Estimates of the Effects of Childbirth on Training Participation – Women, With Controls



**Notes:** “Full effect” refers to estimates without control variables. For the other three estimates, control variables are sequentially added while keeping the ones introduced in the previous step. “Employment controls” adds indicators for months of employment in the current year and part-time status in the main spell. For “Employment + LM history controls”, the regression additionally includes controls for overall experience as well as tenure at the current workplace (both as continuous measures). “+ job characteristics” additionally controls for log monthly earnings and a set of dummies related to the current job, namely establishment size (four categories), public sector, self-employed, leadership position, and fixed-term contract.

Table 2: Training Participation - Women: Number and Hours of Courses

|   | Number               |                    | Hours                |                    |
|---|----------------------|--------------------|----------------------|--------------------|
|   | + controls           |                    | + controls           |                    |
| <i>Full sample</i>                          |                      |                    |                      |                    |
| Any child                                   | -0.319***<br>(0.091) | -0.073<br>(0.120)  | 0.095<br>(4.467)     | -5.775<br>(5.916)  |
| N   | 5,876                | 4,469              | 5,117                | 3,918              |
| <i>Only Participants (intensive margin)</i> |                      |                    |                      |                    |
| Any child                                   | 0.163<br>(0.133)     | 0.463**<br>(0.205) | 25.935***<br>(7.440) | 13.985<br>(12.777) |
| N   | 3,168                | 2,464              | 2,445                | 1,941              |

**Notes:** The number and hours of training are calculated by assigning reported trainings to years proportionally based on the time span during which they occur. Both measures are trimmed at the 99<sup>th</sup> percentile to reduce measurement error. Individual and year fixed effects as well as a quadratic in age are included as controls in the first stage. Columns (2) and (4) additionally control for a full set of covariates as described in in the footnotes of Figure 3. Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 3: Training Participation - Women: Discrete Outcome Variables

|           | Two trainings<br>+controls |                  | 20 hours<br>+controls |                   | One week<br>+controls |                   |
|-----------|----------------------------|------------------|-----------------------|-------------------|-----------------------|-------------------|
| Any child | -0.085**<br>(0.035)        | 0.001<br>(0.044) | -0.131***<br>(0.041)  | -0.068<br>(0.051) | -0.047<br>(0.036)     | -0.053<br>(0.046) |
| N         | 5,876                      | 4,469            | 5,527                 | 4,225             | 5,117                 | 3,918             |

**Notes:** The number and hours of training are calculated by assigning reported trainings to years proportionally based on the time span during which they occur. Both measures are trimmed at the 99<sup>th</sup> percentile to reduce measurement error. Standard errors clustered at the individual level are reported in parentheses. Individual and year fixed effects as well as a quadratic in age are included as controls in the first stage. Columns (2), (4) and (6) additionally control for a full set of covariates as described in the footnotes of Figure 3. Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

training (red line), as presented before, with those in general (blue) and firm-financed training. “Any training” is a broader definition of training activities and additionally includes those activities that are taking place away from the workplace and for which individuals do not explicitly report that they are related to their (current) work.<sup>14</sup> This may encompass activities that may be of use in the labor market, such as language courses, as well as entirely unrelated activities, such as a course as a volunteer sports coach (for those not working in sports). Insofar as they carry some (future) value in the labor market, women may increase their participation in these activities if they aim to compensate the reduced amount of training they are receiving in the context of work, either due to absence from the job or because they are being offered less training by their employer. However, if individual time constraints are the driving force behind training participation, then general courses should be at least as affected as job-related ones (or even more if the job-related courses take place during work and do not entail and additional time or energy tradeoff). Alternatively, individuals may *increase* their participation in non-job-related courses relatively to job-related ones because they derive positive utility from them and now have more (non-work) time to complete such courses. Figure 4 suggests that differences are small overall and that, if anything, the first explanation is the most relevant. Participation in general training only decreases marginally in the year of birth and also decreases less than job-related training in the first post-birth period, but afterwards the trends become indistinguishable.

In contrast to this exercise, the variable “firm-financed training” further narrows down the definition of educational activities that are taken into account. These trainings are a subset of job-related trainings that are also paid for by the firm, indicating a stronger support from the side of the firm. The results suggest that participation in this form of training is affected marginally stronger by the birth of the first child, but the sample is not large enough to draw precise conclusions. Figure 5 in the Appendix shows that a more noisy but qualitatively similar pattern prevails when adding control variables, and Table 11 summarizes and confirms these findings in a static estimation. As before, all effects more men are insignificant, as shown by Appendix Figure 7.

<sup>14</sup>Courses reported in the context of an employment spell are always assumed to be job-related, whereas courses that are reported as a response to an additional questions about further education are only assigned to the job-related category if an additional question about their purpose is answered.

Figure 4: Different Types of Training – Women

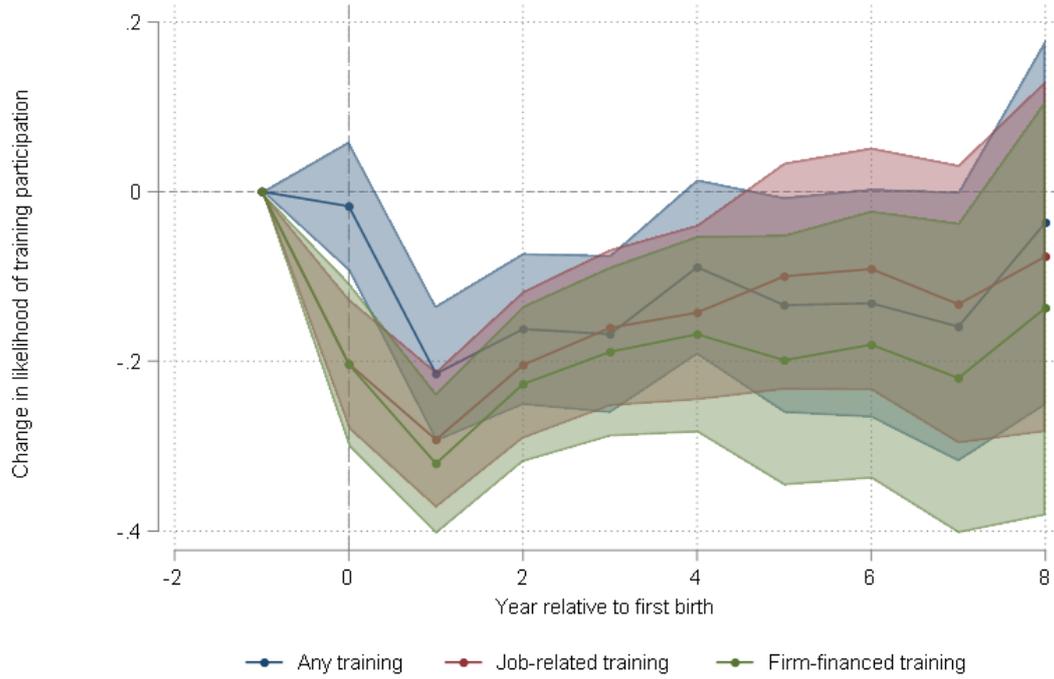
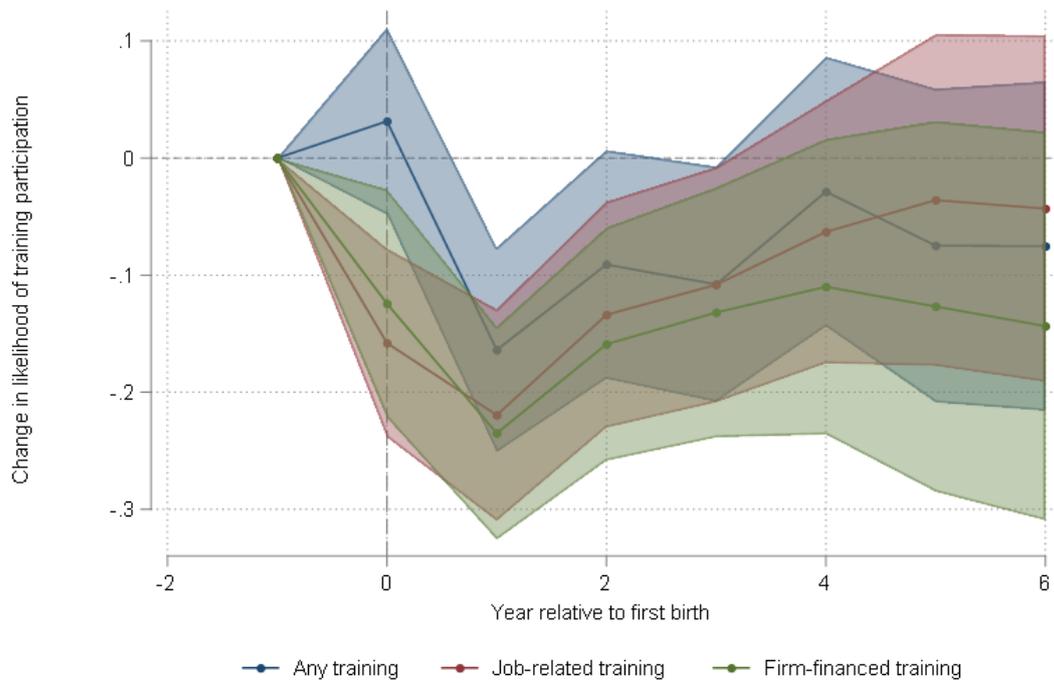


Figure 5: Different Types of Training – Women, With Controls



## 5.4 Heterogeneities in Treatment Effects by Observable Characteristics

In this section, I conduct a series of classic heterogeneity tests by interacting the treatment dummy with different indicators for subgroups of the treated population. For ease of exposition, I use a static setting and restrict attention to women.<sup>15</sup> First, I examine differences by education levels (columns 1 and 2 in Table 4). Individuals with low education (no professional degree) experience no child penalty in training, whereas those with medium education (vocational degree) experience a large but imprecisely estimated penalty. Individuals with high education (university degree) reduce their training participation the most, by 24 percentage points over eight years after birth. It is possible that these differences are driven by the fact that pre-birth training levels are higher for the higher-educated, and therefore a stronger reduction can be observed. Further work will re-examine this using continuous measures of training participation (number and hours of courses) in order to better capture intensive margin effects as well.

Next, I test the importance of working in the public sector. Those who work in the public sector in the year before having a child experience a substantially larger child penalty, again likely due to higher pre-birth training rates. An additional driver may be that leave times are longer and the prevalence of part-time work is higher in the public sector, and that individuals who plan on prioritizing their family commitments later on may select into the public sector because of the offered family-friendly amenities.

Columns 5 and 6 compare effects for individuals leaving in East vs West Germany, finding that East Germans are insignificantly better off. This is broadly in line with the finding by Zoch (2024) that easily accessible childcare may be able to mitigate the child penalty in training. Lastly, columns 7 and 8 report results separately for individuals with one child and those with two or more children, taking into account that the estimated effect of the first child also captures training reductions due to future childbirths. The interaction effect is small and negative, indicating that second and higher-order children further reduce training outcomes but that the bulk of adjustment is due to the birth of the first child. Table 12 in the Appendix additional tests for differences by age at birth, marriage status, and pre-birth income, finding no significant differences in training penalties in these dimensions. In the future, I want to expand on this by examining more variables relating to the family background, such as the partner's employment status and education.

## 5.5 Firm Sorting as a Driver of Training Participation

After having provided suggestive evidence on the reaction of firms and individuals to the birth of the first child, this section looks at the impact of the worker-firm match on training participation. The literature documents clear patterns of training participation by firm type, indicating that training rates are higher in large firms, in the public sector, etc. Additionally, the literature on gender and child penalties highlights the preference of women for family-friendly amenities at the workplace, such as reliable hours and short commuting times (Goldin, 2014; Le Barbanchon et al., 2020). It therefore seems plausible that women would sort into different firms as they become mothers. Casarico

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<sup>15</sup>Results for men are available upon request.

Table 4: Heterogeneity Analyses

|                             | Education            |                      | Public sector t-1    |                      | East German          |                   | Number of children  |                   |
|-----------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|-------------------|---------------------|-------------------|
|                             |                      | +c                   |                      | +c                   |                      | +c                |                     | +c                |
| Any child                   | 0.011<br>(0.104)     | 0.093<br>(0.078)     | -0.071<br>(0.053)    | 0.085<br>(0.067)     | -0.192***<br>(0.044) | -0.054<br>(0.060) | -0.121**<br>(0.057) | -0.001<br>(0.078) |
| Child ×<br>medium education | -0.160<br>(0.114)    | -0.080<br>(0.097)    |                      |                      |                      |                   |                     |                   |
| Child ×<br>high education   | -0.240***<br>(0.111) | -0.189***<br>(0.092) |                      |                      |                      |                   |                     |                   |
| Child ×<br>public sector    |                      |                      | -0.232***<br>(0.066) | -0.253***<br>(0.080) |                      |                   |                     |                   |
| Child ×<br>East German      |                      |                      |                      |                      | 0.125<br>(0.103)     | 0.151<br>(0.099)  |                     |                   |
| Child ×<br>≥ 2 children     |                      |                      |                      |                      |                      |                   | -0.089<br>(0.067)   | -0.056<br>(0.088) |
| N                           | 5,876                | 4,469                | 5,517                | 4,315                | 5,876                | 4,469             | 5,876               | 4,469             |

**Notes:** Individual fixed effects, year fixed effects and a quadratic in age are included as controls in the first stage. The columns marked with “+c” additionally control for a full set of covariates as described in the footnotes of Figure 3. Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

and Lattanzio (2023) document such an effect for Italy, with mothers sorting into lower-productivity firms – a characteristic that could also be linked to training provision, which is expected to be higher in highly productive firms using frontier technology and knowledge. Wilner (2016), however, finds that worker-firm matching plays no role for the motherhood wage penalty in France. Therefore, to test whether changes in worker-firm matches play a role for training participation, I evaluate changes in various firm characteristics and relate them to their importance for training.

To do so, I estimate the same regression and two-stage procedure as for the standard setting (Equation 1), but with measures of firm characteristics as an outcome variable. The resulting regression is presented in Equation 5.5, where  $Y_{it}$  denotes a firm characteristic, such as establishment size, sector, or average coworker pay.

$$Y_{it} = \alpha \text{child}_{it} + \hat{\beta} X_{it} + \hat{\mu}_i + \hat{\gamma}_t + \varepsilon_{it},$$

where  $\hat{\beta}$ ,  $\hat{\mu}_i$ , and  $\hat{\gamma}_t$  indicate the estimated coefficients from the first stage. Table 5 shows how workplace characteristics change after childbirth. For women, no significant shifts can be observed, with a tendency towards more negative firm quality outcomes (e.g. a lower AKM firm effect and a larger share of coworkers who are only employed on a minijob-basis). For men, the picture is similar, with a marginally significant effect detected for average coworker earnings. However, it has to be cautioned that the current setting may not accurately reflect the firm switching process, as some of the sorting may already happen before birth and post-birth movements may take some time to materialize, resulting in null-estimates in the first few years which move the aggregate estimates towards zero. Further work will expand on this in a dynamic setting and an adjusted estimation strategy. Additionally it has to be noted that despite the descriptive evidence pointing towards large differences in training provision by firm types, these effects largely disappear when using a setting with individual fixed effects and identifying firm effects based on firm movers (results available upon request). Therefore, firm characteristics may ultimately not play as large of a role as previously expected.

Table 5: Changes in Firm Characteristics as a Result of Childbirth

|              | Public<br>sector  | Large<br>firm<br>( $\geq 50$ ) | Training<br>support<br>index | AKM<br>FE         | Coworker<br>earnings<br>+ control |                   | Share of<br>minijobbers<br>+ control |                  |
|--------------|-------------------|--------------------------------|------------------------------|-------------------|-----------------------------------|-------------------|--------------------------------------|------------------|
| <i>Women</i> |                   |                                |                              |                   |                                   |                   |                                      |                  |
| Any child    | -0.011<br>(0.032) | 0.027<br>(0.041)               | -0.049<br>(0.083)            | -0.021<br>(0.016) | -0.010<br>(0.018)                 | 0.025<br>(0.019)  | 0.027<br>(0.020)                     | 0.013<br>(0.021) |
| N            | 5,438             | 5,587                          | 4,185                        | 4,622             | 3,779                             | 3,762             | 4,885                                | 4,885            |
| <i>Men</i>   |                   |                                |                              |                   |                                   |                   |                                      |                  |
| Any child    | 0.001<br>(0.022)  | 0.001<br>(0.031)               | 0.022<br>(0.053)             | -0.019<br>(0.012) | -0.027*<br>(0.015)                | -0.023<br>(0.014) | 0.004<br>(0.013)                     | 0.001<br>(0.013) |
| N            | 7,552             | 7,524                          | 5,721                        | 6,567             | 5,436                             | 5,431             | 6,777                                | 6,777            |

**Notes:** “Public sector” is a dummy for whether the individual currently works in the public sector, and “large firm” is a dummy for working in a firm with at least 50 employees. The training support index ranges from zero to one and is constructed as the normalized first component of a principal component analysis of six items on firm-level training support (see Table 6 for the underlying items). The “AKM FE” variable indicates average AKM fixed effect of the firm between 2007 and 2021 (see Lochner et al. (2023) for details). “Coworker earnings” are constructed using the information average earnings within the firm from the establishment (BHP) files and transformed into a yearly percentile rank. The share of minijobbers is also taken from the establishment (BHP) files. Individual and year fixed effects as well as a quadratic in age are included in the first stage of each regression. Columns (6) and (8) additionally control for own income (log monthly earnings) and the part-time status of the individual (as I don’t have an individual-level minijob variable as of now), respectively, in order to construct something akin to a leave-one-out measure of coworker quality. Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

However, the analysis of the role of firms does not have to rely on observable characteristics but can, thanks to the detailed information available in the NEPS, be conducted using an even more direct measure of firm training support. The survey provides a set of six questions regarding firm support for different aspects of training, such as whether there is an establishment-level agreement (“Betriebsvereinbarung”) on training or whether the firm finances training measures. The summary indicator presented in Table ?? is insignificant, but examining each item separately yields an interesting pattern. While all coefficients remain imprecisely estimated (see Table 6), there is a tendency towards a lower provision of resource-related (work time, financing, pay) support – the type of support that is arguably more decisive for training participation, especially when family responsibilities restrain financial and time budgets. This pattern warrants further examination to understand the firm-level response in training provision.

In sum, so far we have learned that women’s training participation decreases strongly immediately after birth of the first child and remains low in the medium term. Decreases in working time can explain around one third of this effect, but a sizeable gap remains. There is no clear pattern in sorting across firms that could rationalize these results, but suggestive evidence that women benefit less from firm support in the form of training financing. Explanations regarding the opportunity costs of time of others and their lower career ambitions – both as realized in their own behavior, and as anticipated by firms – need to be further explored in order to provide more evidence on the origin of the training gap.

Table 6: Changes in Training Support at the Firm Level

|              | “Does your firm have support for training in the form of...?” |                  |                   |                   |                   |                   |
|--------------|---|------------------|-------------------|-------------------|-------------------|-------------------|
|              | Agreement   | Planning         | Person            | Worktime          | Financing         | Paid              |
| <i>Women</i> |   |                  |                   |                   |                   |                   |
| Any child    | -0.017<br>(0.038)   | 0.065<br>(0.049) | 0.021<br>(0.047)  | -0.027<br>(0.050) | -0.042<br>(0.036) | -0.069<br>(0.046) |
| N            | 4,505   | 4,745            | 4,786             | 4,800             | 4,818             | 4,779             |
| <i>Men</i>   |   |                  |                   |                   |                   |                   |
| Any child    | 0.027<br>(0.032)  | 0.023<br>(0.032) | -0.018<br>(0.028) | 0.024<br>(0.034)  | -0.005<br>(0.028) | 0.008<br>(0.034)  |
| N            | 6,104   | 6,388            | 6,385             | 6,463             | 6,495             | 6,423             |

**Notes:** The baseline share of firms providing this type of support is between 52 (further education agreement) and 78 percent (financing). Individual fixed effects, year fixed effects and a quadratic in age are included as controls in the first stage. Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 6 Connection to Earnings and Employment Outcomes

After having ascertained a drop in training participation after childbirth, I now relate this change in training to conventional measures of motherhood earnings and employment gaps. The goal of this section is to gauge how much of the drops in female labor market participation and earnings, especially in the medium to long term, can be explained by reduced human capital as a result of missed job-related training. I first provide evidence on the size of the motherhood gap in earnings and employment, both from the literature and estimated using the sample from above. Next, I determine the size of the returns to training, again from the literature and using the NEPS-ADIAB data with a slightly different estimation strategy. I conclude by providing a back-of-the-envelope calculation combining these three elements.

### 6.1 Literature – Motherhood Earnings Gaps

The literature has produced a broad set of estimates on child penalties, ranging from small and only present in the short term to reductions in long-term earnings of up to 60% (Kleven et al., 2019). While different methods and outcome measures obstruct a systematic comparisons, these estimates can provide guidance regarding the expected effects in the setting used here. In an overview paper, Kleven et al. (2019) estimate child penalties in earnings ranging from 21% in Denmark to 61% in Germany. Recently, this effort was expanded the form of a “child penalty atlas” (Kleven et al., 2024), which features child penalties in employment of 29% for Europe and 41% for Germany specifically. Other estimates range around 39% for annual labor earnings in the United States (Cortés and Pan, 2023), 32 pp in income and 10pp in wages for Sweden (Angelov et al., 2016, , 15 years after childbirth) and a small but persistent long term-effect on earnings of around 3% using IVF births in Denmark (Lundborg et al., 2017). For Germany, most studies look at the effect of parent leave introductions, only estimating child penalties

in passing. Adda et al. (2017) use a model-based approach and data from Germany and find a decrease of 35 percent in the net present value of income over one’s career, primarily driven by a decrease in employment. Fitzenberger et al. (2013) estimate employment penalties and found a tremendous decrease of 81 percentage points in the first year after birth – driven by statutory parental leave – but which reduces substantially to around 20 pp after five years.

## 6.2 Empirical Strategy – Motherhood Earnings Gaps

To obtain comparable estimates using the same sample as for the penalties in training, I estimate earnings and employment gaps using the same empirical strategy as before (Equations 2 and 3). The outcome variables are now log monthly earnings<sup>16</sup> and months of employment per year.

## 6.3 Results – Motherhood Earnings Gaps

The results of this exercise are presented in Table 7.<sup>17</sup> Women experience a drop in log monthly earnings by 47 log points (-37.7%). When accounting for part-time status and months of employment per year, the gap shrinks considerably to 18 log points (-16.8%). This indicates that the extensive margin of earnings, i.e. hours worked per month, are the main driver of the decrease in earnings, but lower hourly wages play a role as well. Adding the full set of controls turns the coefficient insignificant<sup>18</sup> but the point estimate continues to indicate a large gap in earnings. For men, on the contrary, only very small and insignificant changes can be observed. Results for employment can be found in Appendix Table 13, with a decrease for women of ca. 3 months across all specifications, and a small but significant decrease for men of 0.3 months.

## 6.4 Literature – Returns to Training

After having ascertained the existence of a sizeable motherhood earnings gap in this particular institutional setting and sample, I shift attention to the returns to training. Again, a large literature exists, with estimates varying widely across countries, types of training, and empirical strategies. A key problem is the presence of selection into training and the lack of (quasi-) experimental variation in training assignment, making it difficult to separate the effects of innate characteristics of training participants from the effects of training itself. In the context of Germany and using the NEPS data, Ehlert (2017) finds an increase in wages of 1.2% for each week of employer-financed mandatory training, driven by individuals in large organizations and occupations without strong licensing and qualification requirements. Dauth (2020) uses differences in policy styles of employment agencies to provide quasi-experimental evidence on the effect of a large-scale training program (with a median of 75 days of training). This type of training

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<sup>16</sup>Earnings are taken from the longest and highest-paid spell reported in that year and extrapolated from daily wages to monthly earnings assuming a full month of work. If the SIAB spell is missing (e.g. for the self-employed), monthly earnings from the NEPS are used.

<sup>17</sup>Event study estimates will be added soon.

<sup>18</sup>As noted before, this is likely because the set of controls currently used is too big, and will be adjusted soon.

Table 7: Effect of Parenthood on Log Monthly Net Income

|              | no<br>controls       | training<br>+ LM status | + occupation<br>+ sector | all controls      |
|--------------|----------------------|-------------------------|--------------------------|-------------------|
| <i>Women</i> |                      |                         |                          |                   |
| Any child    | -0.473***<br>(0.078) | -0.184**<br>(0.077)     | -0.437***<br>(0.077)     | -0.139<br>(0.086) |
| N            | 5,511                | 5,273                   | 5,501                    | 4,663             |
| <i>Men</i>   |                      |                         |                          |                   |
| Any child    | -0.036<br>(0.041)    | -0.007<br>(0.036)       | -0.007<br>(0.040)        | -0.007<br>(0.034) |
| N            | 7,697                | 7,561                   | 7,644                    | 6,440             |

**Notes:** Individual fixed effects, year fixed effects and a quadratic in age are included as controls estimated in all regressions in the first stage. The second column adds part-time status and months as employment as controls. The third column alternatively controls for one-digit occupation (10 categories) and sector (15 categories) dummies. The last column controls for part-time status and months of employment (as in column 2) as well as experience, tenure and a set of job-related controls (establishment size, public sector, self-employed, leader position, fixed-term contract, log monthly income). Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

yields large returns of 14 days of employment and 7.2% in wages, however, the size of the interventions is much larger than the trainings identified in this paper. Lastly, in recent work Denzler et al. (2024) identify returns to training Switzerland using an event study setting together with entropy balancing to address selections. They find an increase in 0.034 log points in yearly earnings and a decrease in the employment risk of 2.1% over the course of four years after participating in training.

## 6.5 Empirical Strategy – Returns to Training

In the following, I apply the strategy by Denzler et al. (2024) to the NEPS-ADIAB dataset to create a reference point for the size of returns to training that can be expected in this setting. I use the same sample as before, but focus on one year of training data only, akin to the cross-sectional survey data available to Denzler et al. (2024).<sup>19</sup> This allows me to create a clear intervention date (as opposed to the staggered treatment setting used in the context of motherhood), and balancing methods can be applied based on the pre-intervention observations to limit selection bias. I use entropy balancing, a non-parametric reweighting technique similar to matching or inverse probability weighting but with the advantage that it relies on few assumptions and can be applied not only to the first but also the second moments of selected covariates (i.e. both the mean and the variance of the distribution). The procedure then chooses a set of weights

<sup>19</sup>I use the year 2013 because it has the most observations and a sufficiently long pre- & post-training period observed in the data. In the future, I will repeat this exercise with different reference years to test the robustness of my findings.

$w(i, j)$  so that

$$\sum_{i \in Treated} X_i = \sum_{j \in Control} w(i, j) X_j, \quad (5)$$

i.e. the mean of all covariates  $X$  in the treatment group equals the (reweighted) mean in the control group. All control variables are either time-fixed or observed before treatment. Following Denzler et al. (2024), I use monthly earnings and months employed for  $t-1$  and  $t-2$ , part-time, occupation, age, gender, married, any children, German, state, education (all  $t=0$ )

Contrary to other methods such as nearest-neighbor-matching, the balancing is successful by design and does not need to be tested. The regression of interest, as presented in Equation 6, can directly be applied to the weighted data.

$$Y_{it} = \alpha T_i \times Post_t + \mu_i + \gamma_t + \varepsilon_{it} \quad (6)$$

$Y_{it}$  represents the outcomes of interest, such as log monthly net income,  $T_i$  is an indicator for the treatment group (i.e. participated in training in 2013), and  $Post_t$  is an indicator for the period 2013-2021. For the event study setting, this indicator is replaced by a set of indicators of time relative to treatment.

## 6.6 Results – Returns to Training

Table 8 presents the results for the static setting. Log income increases by almost 2.9% relative to individuals who did not participate in training, and the share of individuals working part-time decreases by 1.9%. Employment only insignificantly increase by 0.053 months per year (translating into a reduction in employment risk of 0.4%), and no shifts can be observed in career, firm, or occupation switching. Figure 8 in the Appendix displays results for the event-study setting, showing that most gains are realized in the two years after the training and then remain flat, with noisy estimation due to the decreasing sample size. Additionally, preliminary analyses show that these returns are primarily driven by women, who are also more likely to work part-time a priori and can more easily increase their monthly earnings by increasing their hours. Further, more analyses are necessary to examine whether there are differential returns for childless individuals and parents.

## 7 Conclusion

Taken together these findings reveal that (1) also in this particular context, there are sizeable child penalties in earnings and employment for mothers of 18-47% and 2.5-3.3 months per year, respectively, (2) returns to training are estimated to be around 3% of monthly earnings but insignificant for employment, and (3) mothers decrease their training participation by 11-18 percentage points on average over the eight years after the birth of their first child.

When calculated on a yearly basis, reduced training can thus explain a decrease in mothers' earnings of ca.  $-15pp \times 3\% = -0.45\%$ , or 1% of the unconditional motherhood earnings gap. Assuming that the returns to training accumulate over time, however,

Table 8: Returns to Training

|                        | Log<br>income      | Months<br>employed | Part-time           | Upward career<br>switch | Firm<br>switch    | Occ<br>switch     |
|------------------------|--------------------|--------------------|---------------------|-------------------------|-------------------|-------------------|
| Post $\times$ Training | 0.029**<br>(0.014) | 0.053<br>(0.058)   | -0.019**<br>(0.011) | 0.005<br>(0.011)        | -0.006<br>(0.009) | -0.003<br>(0.008) |
| N                      | 33,526             | 33,993             | 33,371              | 29,535                  | 26,746            | 29,770            |

**Notes:** Log income is measured as monthly log net income, extrapolated to a full month of work but regardless of the number of hours worked. “Upward career switch” takes on the value one if an individual increases their income by more than 10% between two observations. Firm switches are recorded based on establishment identifiers from the ADIAB/BHP data, and occupation switches are defined as changes in the one-digit KldB2010 classification of occupations. Individual fixed effects are included in all regressions, and results are weighted based on entropy balancing on an extensive set of covariates (see text). Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

this would result in a gap of  $(1 - 0.045)^8 - 1 = -0.31$ , i.e. 31% of earnings in the long run. Of course, this logic is exaggerated, but it highlights the potential impact that reduced human capital in the form of missed training opportunities can have on mothers’ long-term careers.

In sum, this project shows that participation in on-the-job training falls for mothers relative to non-mothers by around 15% in the medium run, whereas no such effect exists for fathers. The reduction is stronger for the highly educated and those working in the public sector and can partially be rationalized by reduced labor supply in terms of hours and employment rates. Firm-level factors do not seem to play a large role, but the training offerings for mothers within comparable firms, as well as the individual decision to participate, need to be investigated further. While missed on-the-job training bears not immediate impact on wages or employment in the short, it can accumulate over time and potentially help in explaining why gaps in labor market success exist in the long run, even after children have grown up (see for example Goldin et al., 2022).

In further work, I plan on making this idea of accumulated training losses more explicit and incorporate more directly into a framework of estimating motherhood wage gaps in a dynamic setting. I also plan on formalizing the role of different covariates for accounting for the drop in training participation, using the decomposition method by Gelbach (2016). Lastly, I will exploit further survey questions on family status and individual characteristics to help understand the structures driving individuals’ training participation decision in this context.

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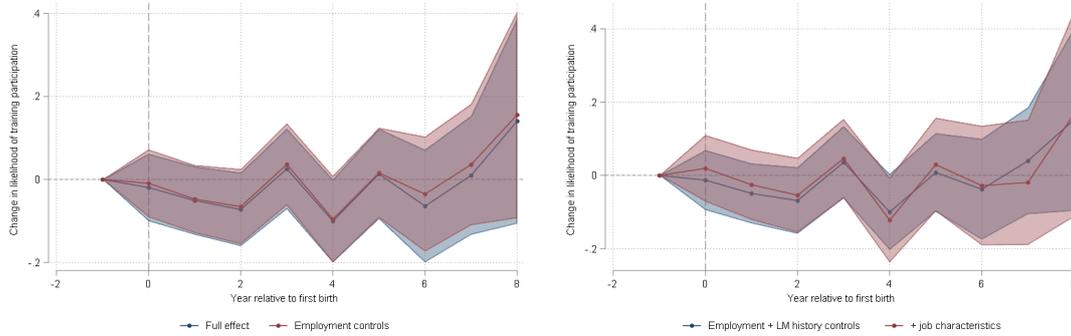
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## A Additional Results

Figure 6: Event-Study Estimates of the Effects of Childbirth on Training Participation – Men



**Notes:** The controls used are the same as described in Figure 3.

Figure 7: Different Types of Training – Men

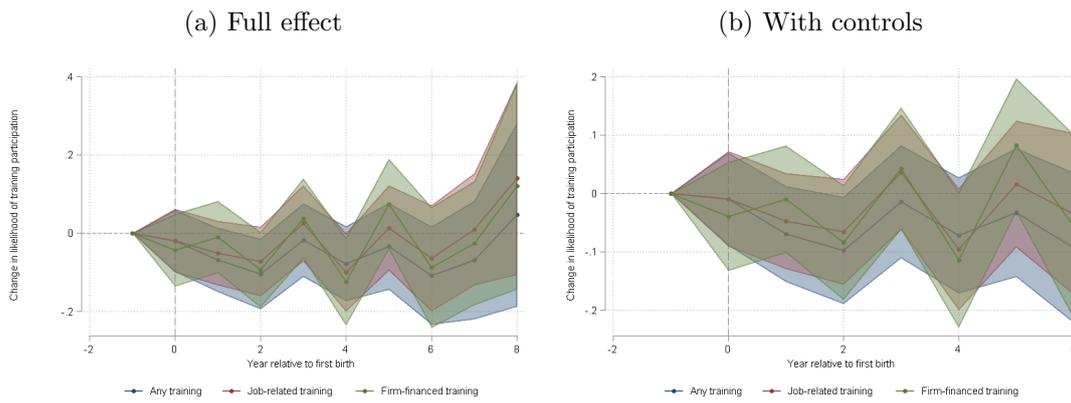
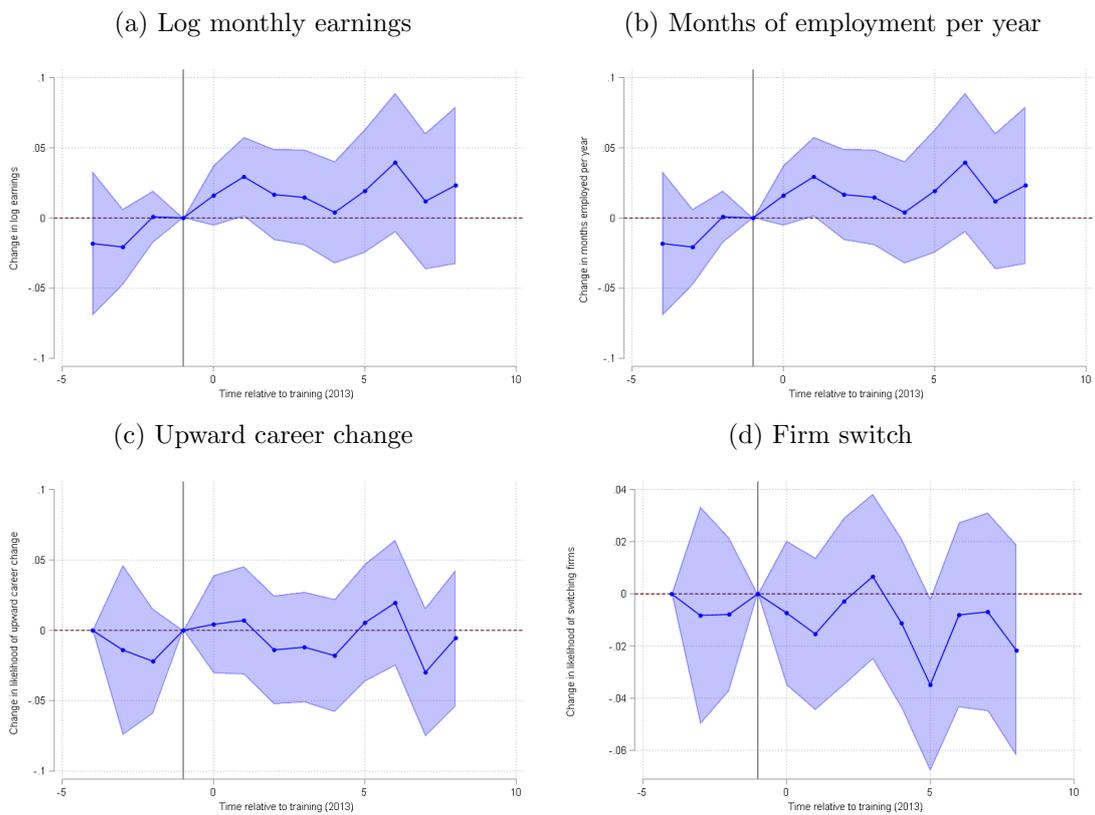


Figure 8: Returns to Training – Event-Study Setting



**Notes:** For a description of the underlying variables, see Table 8.

Table 9: Summary Statistics - Women

|                              | No Further Education | Participated   | Total          |
|------------------------------|----------------------|----------------|----------------|
| Age                          | 36.75 (5.98)         | 36.65 (6.31)   | 36.69 (6.16)   |
| Female                       | 1.00 (0.00)          | 1.00 (0.00)    | 1.00 (0.00)    |
| Years of education           | 14.80 (2.29)         | 15.36 (2.16)   | 15.10 (2.24)   |
| East German                  | 0.17 (0.38)          | 0.14 (0.35)    | 0.16 (0.36)    |
| Household size               | 2.81 (1.20)          | 2.57 (1.20)    | 2.70 (1.21)    |
| Any children                 | 0.54 (0.50)          | 0.44 (0.50)    | 0.49 (0.50)    |
| Employed                     | 0.92 (0.28)          | 0.97 (0.18)    | 0.94 (0.23)    |
| Gross income                 | 2,504 (1,916)        | 3,043 (2,052)  | 2,801 (2,009)  |
| Parental leave (months/year) | 1.03 (2.96)          | 0.47 (1.98)    | 0.73 (2.50)    |
| Part-time                    | 0.56 (0.50)          | 0.50 (0.50)    | 0.52 (0.50)    |
| Experience                   | 13.35 (6.76)         | 13.52 (6.81)   | 13.44 (6.79)   |
| Tenure                       | 6.09 (5.82)          | 6.22 (5.82)    | 6.16 (5.82)    |
| Employer size category       | 2.46 (1.22)          | 2.63 (1.21)    | 2.55 (1.22)    |
| Public sector                | 0.23 (0.42)          | 0.36 (0.48)    | 0.30 (0.46)    |
| Fixed-term contract          | 0.28 (0.45)          | 0.29 (0.45)    | 0.28 (0.45)    |
| Self-employed                | 0.09 (0.28)          | 0.08 (0.27)    | 0.08 (0.28)    |
| Leadership position          | 0.18 (0.38)          | 0.23 (0.42)    | 0.20 (0.40)    |
| Any courses                  | 0.00 (0.00)          | 1.00 (0.00)    | 0.54 (0.50)    |
| Any job-related courses      | 0.00 (0.00)          | 0.79 (0.41)    | 0.39 (0.49)    |
| Number of courses            | 0.00 (0.00)          | 1.84 (1.21)    | 0.98 (1.27)    |
| Hours of courses             | 0.00 (0.00)          | 55.71 (143.50) | 29.26 (107.66) |
| Two courses or more          | 0.00 (0.00)          | 0.47 (0.50)    | 0.25 (0.43)    |
| At least 20h of training     | 0.00 (0.00)          | 0.54 (0.50)    | 0.28 (0.45)    |
| One week of training         | 0.00 (0.00)          | 0.30 (0.46)    | 0.16 (0.37)    |

Table 10: Effect of Parenthood on Training Participation

|              | No<br>controls       | + LM<br>status      | + LM<br>history   | + Job/Firm<br>characteristics |
|--------------|----------------------|---------------------|-------------------|-------------------------------|
| <i>Women</i> |                      |                     |                   |                               |
| Any child    | -0.176***<br>(0.042) | -0.112**<br>(0.046) | -0.065<br>(0.051) | -0.033<br>(0.056)             |
| N            | 5,876                | 5,585               | 5,585             | 4,469                         |
| <i>Men</i>   |                      |                     |                   |                               |
| Any child    | -0.032<br>(0.037)    | -0.022<br>(0.037)   | -0.024<br>(0.037) | -0.014<br>(0.042)             |
| N            | 8,175                | 7,877               | 7,877             | 6,327                         |

**Notes:** Individual fixed effects, year fixed effects and a quadratic in age are included as controls estimated in all regressions in the first stage. The second column adds part-time status and months as employment as controls, the third column additionally adds experience and tenure and the fourth column adds a set of job-related controls (establishment size, public sector, self-employed, leader position, fixed-term contract, log monthly income). Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 11: Different Types of Training – Static Results

|              | Any courses          |                   | Firm-sponsored       |                      |
|--------------|----------------------|-------------------|----------------------|----------------------|
|              | + controls           |                   | + controls           |                      |
| <i>Women</i> |                      |                   |                      |                      |
| Any child    | -0.125***<br>(0.037) | -0.066<br>(0.042) | -0.218***<br>(0.042) | -0.150***<br>(0.048) |
| N            | 6,134                | 5,830             | 4,710                | 4,511                |
| <i>Men</i>   |                      |                   |                      |                      |
| Any child    | -0.057<br>(0.035)    | -0.050<br>(0.036) | -0.031<br>(0.039)    | -0.021<br>(0.039)    |
| N            | 8,331                | 8,026             | 6,924                | 6,699                |

**Notes:** Individual fixed effects, year fixed effects and a quadratic in age are included as controls in the first stage. Regressions marked with +c additionally control for part-time status and months of employment. Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 12: Additional Heterogeneity Analyses

|                           | Age > 31             |                   | East German          |                   | Not married          |                   | High income       |                   |
|---------------------------|----------------------|-------------------|----------------------|-------------------|----------------------|-------------------|-------------------|-------------------|
|                           | +c                   |                   | +c                   |                   | +c                   |                   | +c                |                   |
| Any child                 | -0.160***<br>(0.052) | 0.002<br>(0.069)  | -0.192***<br>(0.044) | -0.054<br>(0.060) | -0.179***<br>(0.047) | -0.046<br>(0.064) | -0.184<br>(0.116) | 0.030<br>(0.267)  |
| Child × Age > 31 at birth | -0.043<br>(0.067)    | -0.094<br>(0.081) |                      |                   |                      |                   |                   |                   |
| Child × East German       |                      |                   | 0.125<br>(0.103)     | 0.151<br>(0.099)  |                      |                   |                   |                   |
| Child × Not married       |                      |                   |                      |                   | -0.025<br>(0.069)    | -0.020<br>(0.096) |                   |                   |
| Child × High income       |                      |                   |                      |                   |                      |                   | -0.006<br>(0.072) | -0.138<br>(0.164) |
| N                         | 5,876                | 4,469             | 5,876                | 4,469             | 5,010                | 3,756             | 1,761             | 1,263             |

**Notes:** Individual fixed effects, year fixed effects and a quadratic in age are included as controls in the first stage. The columns marked with “+c” additionally control for a full set of covariates as described in the footnotes of Figure 3. Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 13: Effect of Parenthood on Employment Time (in Months per year)

|              | no<br>controls       | + training<br>+      | training<br>+experience |
|--------------|----------------------|----------------------|-------------------------|
| <i>Women</i> |                      |                      |                         |
| Any child    | -3.277***<br>(0.293) | -3.309***<br>(0.293) | -2.548***<br>(0.432)    |
| Training     |                      | 0.217***<br>(0.040)  | 0.195***<br>(0.040)     |
| N            | 6,134                | 5,876                | 5,876                   |
| <i>Men</i>   |                      |                      |                         |
| Any child    | -0.330**<br>(0.157)  | -0.356**<br>(0.157)  | -0.436***<br>(0.154)    |
| Training     |                      | 0.027<br>(0.028)     | 0.020<br>(0.028)        |
| N            | 9,331                | 8,181                | 8,181                   |

**Notes:** Individual fixed effects, year fixed effects and a quadratic in age are included as controls estimated in all regressions in the first stage. The second column adds a control for current training participation, and the third column additionally controls for past labor market experience. Standard errors clustered at the individual level are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .