The Motherhood Penalty in Brazil: Wage Differentials and Mobility Dynamics

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Dissertation written under the co-supervision of Professor Joana Silva and Professor Anna Bernard

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Abstract

This dissertation investigates the motherhood penalty in Brazil and examines how job mobility affects wage growth differentials between women with and without children. Using detailed matched employer-employee data from Brazil, we conduct a difference-in-difference event study analysis and find that the arrival of children results in a long-term wage gap of 21% between mothers and their childless counterparts, a penalty which widens across time. Furthermore, our analysis reveals that while mothers and non-mothers exhibit similar rates of between-firm mobility, promotion rates are lower for mothers starting in the early 30s. This pattern, coupled with lower returns to mobility for mothers which decline with age, exacerbates the wage growth gap between mothers and non-mothers, highlighting the compounded impact of motherhood on career advancement and earnings.

Keywords: motherhood penalty, job mobility, wage growth gap

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Resumo

Esta dissertação investiga a penalização da maternidade no Brasil e examina como a mobilidade profissional afeta os diferenciais de crescimento salarial entre mulheres com e sem filhos. Utilizando microdados administrativos ligando trabalhadores e empresas para o Brasil, conduzimos uma análise de estudo de evento de diferença em diferença e descobrimos que a chegada de filhos resulta numa disparidade salarial a longo prazo de 21% entre as mães e mulheres sem filhos, disparidade esta que se expande com o tempo. Além disto, a nossa análise revela que, embora as mães e as mulheres sem filhos exibam taxas semelhantes de mobilidade entre empresas, as taxas de promoção são mais baixas para as mães a partir dos 30s. Este padrão, aliado a retornos de mobilidade mais baixos para as mães que deterioram com a idade, agrava a lacuna de crescimento salarial entre mulheres com e sem filhos, destacando o impacto composto da maternidade no avanço da carreira e nos rendimentos.

Keywords: penalização da maternidade, mobilidade profissional, disparidades de crescimento salarial

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Contents

1 Introduction **1**

2 Institutional Context and Data **4**

2.1 Institutional Context 4

2.2 Data 5

2.3 Descriptive Statistics 7

3 The Motherhood Penalty in Brazil **8**

3.1 Methodology 8

3.1.1 Identification 9

3.1.2 Empirical Strategy 11

3.2 Results 11

3.3 Discussion 15

4 Differentials in Job Mobility and Wage Growth **18**

4.1 Identification 18

4.1.1 Wage Growth 19

4.1.2 Job Mobility Events 19

4.2 Job Mobility Patterns 20

4.3 Returns to Job Mobility 21

4.4 Decomposition Analysis 23

4.5 Discussion 24

5 Robustness Checks and Heterogeneity **26**

5.1 Additional difference-in-difference estimators 26

5.2 Heterogeneity 28

6 Conclusion **29**

Appendix A: Tables **35**

Appendix B: Figures **38**

List of Tables

1 Descriptive Statistics, 1995-2020 Sample 7

2 Descriptive Statistics, Sample with Mothers and Non-Mothers 8

3 Annual probability of any job change, changing employer, and promotions by motherhood status 20

4 The role of job mobility in explaining differences in the wage growth of mothers vis-à-vis women without children: results from the Gelbach decomposition 24
5 Regression coefficients of models estimating the probability of job changes: by age and motherhood status . . . 35
6 Descriptive Statistics of Variables used as Regressors . . . 36
7 Regression coefficients from model estimating returns to job mobility: by age and parenthood status . . . 37

List of Figures

1 Wage Rate Impact of Children in a Difference-in-Differences Event Study Design 12
2 Employment Rate Impact of Children in a Difference-in-Differences Event Study Design . . . 13
3 Probability of Working a Managerial Position: Impact of Children in a Difference-in-Differences Event Study Design . . . 14
4 Annual (adjusted) proportion of women experiencing job mobility events . . . 22
5 Predicted wage gains associated with different types of job mobility. . . . 23
6 Borusyak et al. (2024) Imputation Estimator for Average Treatment Effect on the Treated . . . 38
7 Callaway and Sant'Anna (2021) Estimator for Average Treatment Effect on the Treated . . . 39
9 Wage Rate Impact of Children in a Difference-in-Differences Event Study Design: Large Firms . . . 39
8 Wage Rate Impact of Children in a Difference-in-Differences Event Study Design: Small Firms . . . 40
10 Predicted wage gains associated with different types of job mobility: Small Firms 40
11 Predicted wage gains associated with different types of job mobility: Larger Firms . . . 41

1 Introduction

Gender discrimination in the labor market remains a significant challenge despite progress over recent years. Women continue to face disparities in wages, promotions, and career opportunities compared to their male counterparts (Blau and Kahn (2017)). These inequalities not only hinder individual potential but also result in a loss of talent, expertise, and productivity, ultimately weakening the efficiency and dynamism of the labor market (Goldin (2014); Olivetti and Petrongolo (2016)).

Within this broader context of gender disparities, motherhood stands out as a critical event that profoundly shapes women's career trajectories. Mothers often face additional disadvantages beyond those associated with gender, including reduced productivity, loss of specific human capital, a shift in focus from market to home activities, and direct discrimination (Budig and Hodges (2010); Correll et al. (2007)). Research has highlighted a concerning trend: while the gender pay gap has been narrowing over the past few decades, the gap between mothers and non-mothers has been widening (Kleven et al. (2019)). Yet, despite its importance, there is still a significant lack of understanding regarding the mechanisms driving these disparities (Cukrowska-Torzewska and Matysiak (2020)).

This dissertation fills a critical gap in the literature by not only quantifying the motherhood penalty in Brazil but also by examining the role of job mobility in shaping wage disparities between mothers and their childless counterparts. By shedding light on the extent of the penalty and how career trajectories influence it, this study provides a comprehensive view of how mothers fall behind—and never fully catch up—in labor market outcomes.

We leverage rich, matched employer-employee data from Brazil (RAIS), including detailed records on maternity leave, to create a unique panel dataset covering all formal firms in the country. The Brazilian setting offers a valuable opportunity to study motherhood penalties and job mobility differentials in a context markedly different from developed nations. Brazil, as a developing country, exhibits pronounced inequalities and discrimination in the labor market, providing a stark contrast to the settings of most existing studies. Moreover, Brazil's strong legal framework for maternity rights, established in its Constitution, may significantly influence both the motherhood penalty and job mobility. These rights include up to 120 days of paid maternity leave and job protection during this period, which can impact mobility rates and the returns to job changes.

This dissertation begins by providing empirical evidence on the magnitude of the motherhood penalty in Brazil. To do so, we compare labor market outcomes between mothers and non-mothers, by employing a difference-in-difference event study design centered around the birth of the first child. This event is widely recognized as a pivotal moment in shaping career trajectories, with subsequent children exerting only marginal effects on labor outcomes (Kleven et al. (2019); Lundborg et al. (2017)). The outcomes analyzed include monthly wage rates, employment rates, and the likelihood of holding managerial positions.

Our analysis estimates a long-term wage penalty of approximately 21% for mothers compared to non-mothers. Interestingly, the wage gap does not manifest immediately but grows steadily over time. One year after childbirth, the wage difference is nearly undetectable, but ten years later, the gap reaches 21%. Our findings highlight the cumulative nature of the penalty, showing that mothers never fully recover from the initial setback, and the gap continues to widen throughout their careers.

This gradual divergence contrasts with much of the existing literature, which often reports a sharp wage divergence around childbirth. The specific characteristics of our sample, along with Brazil's institutional context, may explain part of this difference. Casarico and Lattanzio (2021) show that the motherhood penalty varies depending on worker characteristics: shorter maternity leaves, higher wages, and later childbirths are associated with smaller penalties. In our sample, nearly all mothers take maternity leaves of less than six months, with 75% taking leaves shorter than four months. Additionally, 95% of mothers are over 30 at the time of their first childbirth, and our focus on college-educated individuals may also result in lower immediate penalties.

But how can we explain the growing penalty between mothers and non-mothers? Evidence shows that job mobility—both between firms and within firms—plays a key role in wage growth (Costa Dias et al. (2021); Loprest (1992); Manning and Swaffield (2008); Topel and Ward (1992)). Yet, despite motherhood being a life event that can substantially impact both the frequency and quality of job moves, job mobility has received relatively little attention as a factor contributing to the motherhood pay penalty. The added caregiving responsibilities shouldered by mothers often reduce their geographical mobility, temporal flexibility, and job search intensity, leading to fewer career opportunities (Keith and McWilliams (1999); Yankow and Horney (2013)). In many cases, mothers may also be reluctant to leave established relationships with employers, which help facilitate work-care arrangements, or they are tied to job tenure due to benefits like flexible working or family leave (Looze (2017)). Additionally, mothers often have fewer resources to invest in professional networks, which provide access to job opportunities, or they may struggle to leverage these networks effectively (Zhou (2019)).

While mothers often prioritize non-wage characteristics—such as shorter commutes (Eriksson and Lagerström (2012); Le Barbanchon et al. (2020)), the absence of long working hours (Cha and Weeden (2014); Goldin (2014)), and flexible working conditions (Mas and Pallais

(2017))—over financial incentives when changing jobs, these choices tend to limit their wage returns. This affects their ability to ascend the wage ladder, leading to slower wage growth compared to non-mothers (Bielby and Bielby (1992); Fuller (2008); Keith and McWilliams (1999)).

Our findings reveal that while mothers and non-mothers exhibit similar rates of between-firm mobility, mothers are less likely to receive promotions after giving birth. This divergence begins around early motherhood (typically in the early 30s), where promotion rates for mothers drop compared to non-mothers. Nevertheless, by their mid-to-late 40s, mothers show signs of catching up in terms of promotion rates. On top of this, we find evidence that the returns to job mobility—its marginal impact on wage growth—are consistently lower for mothers. Unlike non-mothers, who experience stable wage returns from mobility throughout their careers, mothers see a decline in returns starting around the early 30s, suggesting that mobility may contribute to the widening of the wage growth gap between mothers and women without children.

Finally, a decomposition analysis using Gelbach (2016)'s method confirms that job mobility widens the wage growth gap between mothers and non-mothers. Ultimately, these findings suggest that while mothers eventually catch up in terms of promotions, the long-term impact of job mobility on wage growth remains substantially lower for them, further explaining the persistent motherhood penalty.

This dissertation contributes to two strands of the literature. Firstly, it contributes to the growing body of literature that examines labor market disparities between mothers and women without children, moving beyond the traditional focus on gender comparisons. A substantial number of studies have documented that motherhood is associated with a wage penalty, which persists even when controlling for factors such as education, work experience, and job characteristics. Meta-analyses, like Cukrowska-Torzewska and Matysiak (2020), reveal that mothers earn less across countries, with penalties varying depending on institutional contexts, labor protections, and gender equality levels. The penalty is not solely due to reductions in labor supply or experience loss, but also to employer discrimination and societal expectations that devalue mothers' work. Kleven et al. (2019) highlight that the wage penalty begins with the first childbirth and persists over time, causing a long-term 20% reduction in earnings over a decade. Their study underscores that the penalty is mostly driven by lower hours worked, decreased labor force participation, and wage cuts. Other studies, such as Budig and England (2001), confirm that motherhood wage gaps persist even after adjusting for human capital factors, implying structural barriers and discrimination against mothers in the workplace. Together, these studies indicate that both institutional and cultural factors play key roles in driving wage disparities between mothers and non-mothers.

Secondly, this thesis contributes to the emerging literature on job mobility as a crucial mechanism shaping wage growth differentials between mothers and women without children. Job mobility, both within and between firms, plays a significant role in career progression and wage increases. Yet, mothers face unique barriers to mobility, such as reduced geographical flexibility, limited job search intensity, and discrimination, all of which can hinder their ability to move into higher-paying positions or negotiate better pay. Research by Manning and Swaffield (2008) highlights the impact of early-career wage growth, showing that mobility can drive significant wage increases, but these effects are often muted for mothers. Similarly, Del Bono and Vuri (2011) find that in Italy, job mobility contributes to the gender wage gap, with women—and particularly mothers—experiencing lower returns from job changes compared to men. Avram et al. (2024) further demonstrate that mothers in the United Kingdom are less likely to make wage-enhancing job moves compared to their non-mother counterparts. Their analysis reveals that mothers are more likely to leave jobs for family-related reasons, rather than career advancement, further limiting their wage growth potential.

Our results shed new light on the dynamics of returns to job mobility over time. For mothers, returns on promotions begin to diverge in their late 20s, while returns from employer changes start to decrease in their early 30s. This disparity continues to widen with age, with no evidence of recovery at any stage of their careers. This pattern—characterized by both substantial and diminishing returns— not only underscores a persistent wage penalty for mothers but also provides crucial insight into the widening motherhood penalty over time. By highlighting how job mobility contributes to the growing wage gap between mothers and non-mothers, our findings deepen the understanding of how cumulative career disadvantages shape long-term wage outcomes for mothers in Brazil.

The remainder of this thesis is structured as follows: Section 2 outlines the Brazilian institutional context, describes the dataset, and presents descriptive statistics. Section 3 estimates the motherhood penalty, detailing the empirical methodology, main findings, and a brief discussion. Section 4 examines how job mobility influences wage growth differentials. Section 5 provides robustness checks on the event study estimation using additional difference-in-difference estimators, as well as an heterogeneity analysis that investigates the penalty and returns to job transitions for women depending on firm size. Finally, Section 6 offers concluding remarks.

2 Institutional Context and Data

2.1 Institutional Context

Brazil provides a relatively long paid maternity leave, established by the Constitution since 1988. Women employed for over three months are eligible for 120 days (about four months) of paid leave at their regular salary. This benefit, which exceeds the 80-90 days typical in other Latin American countries, is covered by the employer but can be reimbursed through deductions in social security contributions.

Conditions on the maternity leave state that it may start up to 28 days before the child's birth or on the actual birthdate. Employees on maternity leave are protected from being dismissed without cause for the first five months after the birth.

Starting in 2010, firms who enroll in the *Programa Empresa Cidadã* (PEC) program may offer up to 180 days of paid maternity leave to their female employees (equivalent to roughly 6 months), constituting a 50% increase in leave period. These additional 60 days of paid leave are financed directly by the firm's corporate tax contributions. However, firm eligibility conditions to join the program require that tax returns are calculated based on "*real profit*", rather than "*simple profit*" (a profit modality very attractive for small firms, since it allows for some savings in contributions), which implies the universe of Brazilian firms able to enroll is reduced to roughly 8% (Meireles et al. (2017)) . Furthermore, it is important to note that a firm can take part on the PEC program and still choose not to offer the leave extension.

In contrast, the 1988 Constitution provides for a short period of paid paternity leave, lasting only 5 days, with the full cost covered by the employer. However, companies participating in the PEC program can extend this leave by an additional 15 days, allowing fathers up to 20 days in total following the birth of their child. According to a 2019 report on fatherhood in Brazil, 68% of fathers did not make use of the legal 5-day paternity leave. This lack of uptake is attributed to factors such as insufficient awareness about the benefits of bonding, the perception of leave as a discretionary benefit rather than a right, concerns about workplace discrimination, and career-related apprehensions (Costa Lima and Cunha dos Santos (2019)). Consequently, the limited data on paternity leave in our study leads us to focus exclusively on comparing outcomes of women with and without children.

2.2 Data

This paper uses one of the main administrative datasets from Brazil: the *Relação Anual de Informações Sociais* (RAIS) longitudinal census containing yearly information at the employer-employee-job title level.

RAIS

RAIS is collected by the Ministry of Labor in a compulsory survey to all formal firms and their registered workers. We use these records for the 1995-2020 period. RAIS provides detailed information at the worker level (such as age, gender, schooling, and race), job characteristics (occupation, wage, contracted hours, tenure, hiring and termination dates, reason behind separations, type of contract, among others) and starting in 2007 it reports leaves taken during the year (paid sick leaves, parental leaves, unpaid leaves, worker-related accident leaves, commute-related accident leaves). Several cross-year checks were performed to guarantee the consistency of the data, particularly regarding gender, education, and age. RAIS offers information for each worker-firm, which allows us to perfectly trace individuals across firms during their working lives. All measures of stocks in this dataset refer to December 31^{st} values.

Sample Construction

Our sample consists of full-time college-educated workers aged 15-65 years old, with non-missing occupation and wage. The reasons why we include solely college-educated individuals in this analysis are threefold: first, to alleviate concerns over sample selection or changes in the composition of workers over the life cycle as highlighted by Bronson and Thoursie (2019); second, due to the fact that gender wage differentials are more pronounced among the higher-educated, making this a particularly interesting group to study (De la Rica et al. (2008)); and third, to address the informality prevalence in the Brazilian labor market, by focusing this study on group which has lower incentives to migrate to the informal market. Additionally, we restrict the sample to private sector workers (since layoffs in the public sector have a very different dynamic, which affects mobility and separations). In the case where workers have multiple job spells during a year, we keep in each worker-year cell the highest-paid among all employment spells.

As referred above, information on worker leave-taking only becomes available during the 2007-2020 sub-period. Particularly, information on maternity or paternity leave is what allows us to pinpoint parenthood in this analysis, constituting a pivotal variable.

Starting in 2007, employers can report up to three leaves taken by the worker in a given year. Due to this fact, an important correction was performed in order to guarantee accuracy in the identification of the number of parenthood leaves. If a leave has occurred from one year to the other (eg. started in October of year t and ended in February of $t+1$), it is registered as two leaves: the first starting in October and ending in December 31^{st} of year t and a second leave starting January 1^{st} of year $t+1$ and ending in February, which is problematic since in reality only one leave has been taken.

Therefore, we identify such cases, where a parenthood leave ends by December and starts by January the following year (to cover for possible reporting errors in the actual day the leave is recorded to happen), and account them as only one leave. We also consider parenthood leaves longer than 15 days but shorter than 365 days.

2.3 Descriptive Statistics

Table 1 presents some summary statistics for men and women in the 1995-2020 period.

Table 1: Descriptive Statistics, 1995-2020 Sample

	Women		**Men**		**Overall**
	Mean	S.D.	Mean	S.D.	Difference
Average log annual real wage	10.38	1.03	10.82	1.14	0.44***
Average log hourly wage	2.85	1.05	3.21	1.16	0.36***
Share with MSc degree	0.02	0.13	0.02	0.13	0.00***
Share with PhD degree	0.01	0.07	0.01	0.09	0.00***
Tenure	7.80	8.24	7.73	8.56	-0.07***
Average age	38.88	9.98	39.46	10.34	0.58***
Average potential experience	17.29	9.97	17.86	10.33	0.56***
Weekly contract hours	36.98	7.81	39.38	6.22	2.40***
Share with permanent contract	0.88	0.32	0.92	0.28	0.03***
Share with managerial jobs	0.10	0.30	0.16	0.36	0.05***
Number of worker-years	83,683,232		58,003,255		141,686,487
Number of unique workers	11,585,110		8,665,388		20,250,498
Mean log gender hourly wage gap					0,36
Share male					0,41

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Note: This table presents the average and standard deviation of some variables of interest for female workers, male and both in the period of 1995-2020. Real wages are obtained from annual nominal wages which we deflate using a consumer price index for Brazil. Potential experience is given by age minus years of education minus 6. Managerial roles are identified based on *Classificação Brasileira das Ocupações 2002*, the Brazilian occupation classification - the observations belonging to Great Group 1 of this classification are considered having a managerial role. Employment rates refer for formal employment rates. Mean log gender hourly wage gap in *raw* terms is given simply by the difference between mean log hourly wages of men and mean log hourly wages of women. The difference column conducts t-tests to assess if the difference in outcomes between men and women is statistically significant

Overall, this panel data set spans over 20 years and contains over 140 million individual-year observations. It is important to note that this panel contains more observations for women than for men. This discrepancy is atypical in the context of Brazilian formal labor markets, and is driven by the specific sample employed in this dissertation, which consists exclusively of college-educated individuals. Since women, on average, have higher levels of education than men, it is anticipated that the panel will show a greater representation of women.

Throughout this analysis we will compare labor market outcomes of women with children against women without children in order to estimate the penalty associated with motherhood. Table 2 below presents some summary statistics for both groups, mothers and non-mothers, which are respectively our treatment and control groups, across two distinct moments in time. Considering the year mothers welcome their first child as $t = 0$, the table presents the outcomes of mothers and non-mothers one year prior childbirth and 5 years later (i.e. at $t = -1$ and $t = 5$).

Table 2: Descriptive Statistics, Sample with Mothers and Non-Mothers

	Mothers				**Non-Mothers**				**Difference**
	$t=-1$		$t=5$		$t=-1$		$t=5$		
	Mean	S.D.	Mean	S.D.	Mean	S.D	Mean	S.D	$t=5$
Log annual real wage	10.37	0.94	10.68	0.93	10.32	0.95	10.77	0.89	0.09***
Monthly wage rate	3501.61	3511.92	5362.19	5127.55	3207.05	3193.49	5257.69	4872.92	-104.50***
Age	31.05	3.81	36.81	3.82	32.52	4.38	40.04	3.97	3.24***
Weekly contract hours	38.48	7.35	37.16	7.81	37.96	7.49	36.60	7.85	-0.56***
Employment rate	0.80	0.40	0.64	0.48	0.78	0.41	0.72	0.45	0.08***
Share in managerial job	0.09	0.28	0.11	0.32	0.07	0.26	0.11	0.31	-0.01***
Share female in firm	0.68	0.21	0.67	0.20	0.68	0.20	0.68	0.19	0.01***
Individual-year observations				11,319,009				21,250,710	
Unique individual observations				1,339,415				2,428,454	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: This table presents the average and standard deviation of some variables of interest for the sample of mothers and non-mothers we will focus our analysis on. The details of how we construct the control group of women who never have children are described in Section 3.1.1. We decompose the outcomes further into the year before given birth ($t = -1$) and 5 years after having the first child ($t = 5$), for both mothers and non-mothers. Real wages are obtained from annual nominal wages which we deflate using a consumer price index for Brazil. Managerial roles are identified based on *Classificação Brasileira das Ocupações 2002*, the Brazilian occupation classification - the observations belonging to Great Group 1 of this classification are considered having a managerial role. Employment rates refer for formal employment rates. Share female in firm refers to the average share of college-educated women in the firm where individual works. The difference column conducts t-tests to assess if the difference in outcomes between mothers and non-mothers after giving birth (in this table we consider 5 years after giving birth) is statistically significant.

3 The Motherhood Penalty in Brazil

This section aims at estimating the impact of children on labour market outcomes of mothers relatively to women without children (non-mothers), by employing a quasi-experimental approach based on event studies around the birth of the first child. This estimated impact, commonly referred across the literature as the ”Motherhood Penalty” (or the ”Child Penalty”), is the percentage by which mothers fall behind non-mothers. We will conduct this analysis solely across parenthood status of women, given the limitations in the identification of fatherhood in our dataset, as described in Section 2.3.

3.1 Methodology

In this section we employ the methodology elaborated by Kleven et al. (2019), to perform a difference-in-difference event study analysis on the impact of motherhood on certain labor market outcomes. This approach allows us to compare the outcomes of mothers to those of non-mothers, whom we consider our control group.

3.1.1 Identification

The fundamental basis of this analysis is the identification of the birth of the first child, which is considered pivotal by the literature, since the impact of having a child for the first time exceeds that of having additional children (Kleven et al., 2019; Lundborg et al., 2017). Since RAIS is a linked employer-employee dataset, it does not provide explicit information on the year of the first child's birth. Consequently, we must make certain assumptions to determine the occurrence of the first child. For our purposes, we define the first child as the first recorded instance of maternity leave within a specified fertility age range. Maternity leaves taken after this age are treated as associated with subsequent children. The results presented throughout this dissertation will consider a fertility age window of 40 years old (inclusive). The reason why we consider such a lengthy period takes into account the fact that in our sample we are working solely with college-educated individuals, who are known to postpone the birth of the first child (Amin and Behrman, 2014; Ní Bhrolcháin and Beaujouan, 2012). Hence, we consider as treatment group for this analysis women who have taken their first maternity leave, i.e. have had their first child, before they turn 40 years old. We designate this period as $t = 0$.

However, simply assigning non-mothers (that is, women who have never been recorded to take a maternity leave) to the control group is not rigorous enough to fit to the event study exercise. This is essentially because those who do not have children in our data could either never have children in their lifetime or just happen to not have had children yet. Ideally we would like to capture in our control group those who belong to the former group. For the individuals whom we observe throughout their full fertility age window, this does not pose an issue. However, for those who have not reached the end of the designated fertility window of 40 years old by 2020, we face a truncation issue in identifying motherhood.

In order to deal with this, we apply Kleven et al. (2019)'s procedure, which allows to allocate individuals from the truncated cohorts (born from 1981 onwards) to the control group.

Firstly, we run a linear probability model of zero lifetime fertility (Equation 1) only for women, as a function of observables, estimated on the non-truncated cohorts. Zero lifetime fertility (z_{it}) is a dummy variable equal to 1 if the individual has never had children (never recorded a maternity leave) and 0 otherwise.

$$P[z_{it} = 0] = X'\beta \tag{1}$$

The explanatory variables included are: quartiles of the income distribution of the individual's cohort, dummies for level of higher education achieved, dummies for state of residence, dummy equal to 1 if the individual works in a managerial position (defined by *Classificação Brasileira das Ocupações 2002* - Great Group 1).

We apply the estimated model to the individuals born in the truncated cohorts to obtain the predicted values of zero lifetime fertility. For each truncated cohort c, we order individuals in descending order of their predicted values of zero lifetime fertility.

Then, we compute the average fraction of women born in the non-truncated cohorts who have zero lifetime fertility $P^w_{c<1980}$, as well as the total number of women born in each truncated cohort c, N^w_c. For each truncated cohort c, we replicate the share of women with no children observed in the non-truncated cohorts. For that, we estimate $n^w_c = N^w_c \times P_{c<1980}$.

Finally, we select the top-n^w_c female observations within cohort c, and assign them to the control group. Since the data is ordered in descending fashion of predicted lifetime fertility, the top observations contain the highest predicted values, indicating those individuals are more likely to never have children (our ideal control group). The rest of cohort c who do not have children are assumed to have children later in life, and therefore not included in the controls.

In sum, the control group is composed by both women without children who belong to the non-truncated cohorts, that is, were born until 1980, and women observed without children in the later cohorts (born from 1981 onwards), derived from the strategy described above.

Next, we assign "placebo births" to these individuals, to facilitate the construction of the event study. This step is essential because, by definition, this group consists of non-mothers, meaning there is no actual $t = 0$—the event of having a first child does not occur for them. By artificially introducing a placebo birth, we create a comparable time-to-event framework for non-mothers, allowing us to align their data with that of mothers and make both groups comparable.

This procedure allows to determine at which age these control group individuals are going to verify their placebo birth, drawing on a log-normal distribution within cells of cohort and education. As such, for each non-truncated cohort ($c < 1980$) and higher education level (e), we estimate a log-normal distribution of age $A_{c,e} \sim \mathcal{LN}(\hat{\mu}_{c,e}, \hat{\sigma}^2_{c,e})$ with mean and variance obtained from the actual distributions within each cohort-education cell (that is, for each cohort c and level of higher education e, we store the mean age at first child and standard deviation to build a log-normal distribution).

We assign a random draw of this age distribution to each individual born in non-truncated cohort years c and with education level e, which is going to be the age at which time-to-event t equals zero. For the truncated cohorts, we draw a random age at first child from $\mathcal{LN}(\tilde{\mu}_{c,e}, \hat{\sigma}^2_{c,e})$, where the mean $\tilde{\mu}_{c,e}$, is the predicted average age at first child obtained by estimating a linear trend on the non-truncated cohorts. This step is crucial to ensure an upward linear drift in the age at first child while keeping the variance constant.

After applying the procedures which allow to identify mothers and non-mothers, we are left with an unbalanced panel of 11,319,009 treated observations and 21,250,710 control observations, which correspond to 1,339,415 mothers and 2,428,454 non-mothers.

3.1.2 Empirical Strategy

We apply Kleven et al. (2019)'s specification separately for mothers and non-mothers, seen below:

$$Y_{ist}^{m} = \sum_{j \neq -1} \alpha_j^m \mathbb{1}[j = t] + \sum_{y} \gamma_y^m \mathbb{1}[y = year] + \sum_{k} \beta_k^m \mathbb{1}[k = age_{is}] + \epsilon_{ist}, \tag{2}$$

where m is motherhood status (mother or non-mother) and Y_{ist}^m are labour market outcomes for individual i, in year s at event time t. In our analysis, we will study as outcomes monthly wage rates, employment and probability of working a managerial role. As explanatory variables we include a full set of time to event dummies ommitted at time $t = -1$ (implying that event time coefficients measure the impact of children relative to the year just before the first childbirth), as well as a full set of year and age dummies. We convert the estimated level effects into percentages by calculating:

$$P_t^m \equiv \frac{\hat{\alpha}_t^m}{E[\tilde{Y}_{ist}^m|t]}, \tag{3}$$

where $\tilde{Y}_{ist}^m$ corresponds to the predicted outcome when omitting the contribution of the event dummies, capturing the year t effect of children as a percentage of the counterfactual outcome absent children.

Finally, the penalty, expressed as the percentage by which women with children fall behind women without children at time t, is given by:

$$P_t \equiv \frac{\hat{\alpha}_t^{nonmother} - \hat{\alpha}_t^{mother}}{E[\tilde{Y}_{ist}^{mother}|t]} \tag{4}$$

Lastly, we also compute long-run penalties, estimated as a difference-in-differences between women with and without children at event time $t = 10$, that is, 10 years after the first child birth.

3.2 Results

This section contains the estimation results of the motherhood penalty between women with children and women without children computed through the difference-in-difference event study design outlined above.

Monthly Wages

Figure 1 plots the impacts of children on monthly wage rates across event time. As defined above, these are outcomes at event time t relative to the year before the first childbirth ($t = -1$), having controlled nonparametrically for time and age trends. The figure includes 95 percent confidence intervals around the event coefficients, although these are not always clearly visible due to the high precision of the data.

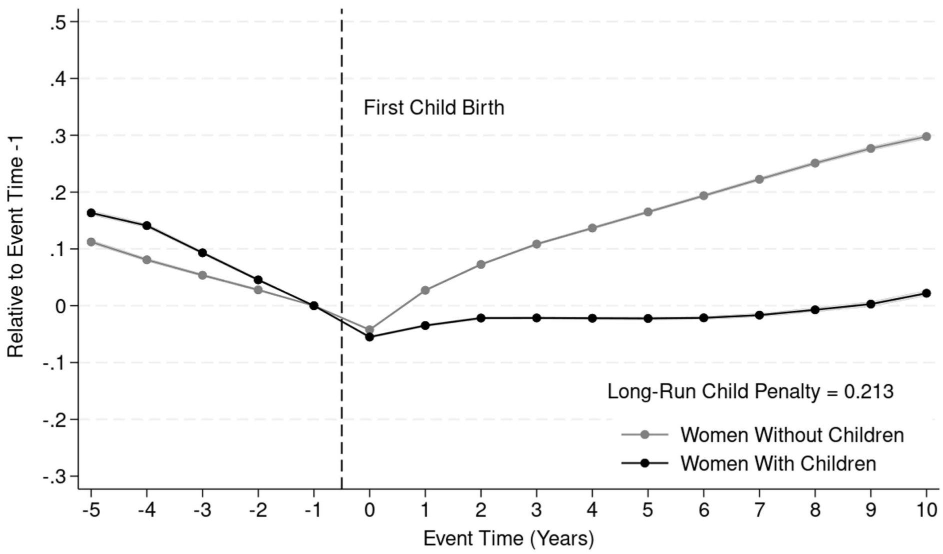

Figure 1: Wage Rate Impact of Children in a Difference-in-Differences Event Study Design

Note: The figure shows the evolution of monthly wage rates relative to the year before the birth of the first child for women with children compared to those who never have children (assigning placebo births based on the observed distribution of age at first child among those who have children). In the figure we control for year and age fixed effects. The details of how we construct the control groups women who never have children are described in Section 3.1.1. The figure reports long-run child penalties for women, estimated as a difference-in-differences between those who have children and those who never have children. The shaded 95% confidence intervals are based on robust standard errors.

The event study analysis presented in the graph reveals a widening wage gap between women with and without children following the first childbirth. In contrast to the findings in the literature (e.g., Casarico and Lattanzio (2021); Kleven et al. (2019); Morchio and Moser (2024)), which typically demonstrate a sharp decline in wages for mothers at the time of birth, no such immediate drop is observed here. Instead, the wage trajectories of mothers and non-mothers begin to diverge gradually post-birth. While non-mothers experience steady wage growth over time, mothers' wage progression remains stagnant, resulting in a long-term child penalty of 21.3%.

Employment

Figure 2 depicts the evolution of the motherhood penalty on employment rates of women with and without children. In order to assess employment as an outcome, we employ a procedure to balance the panel, such that years outside the formal labour market are considered as exits from employment.

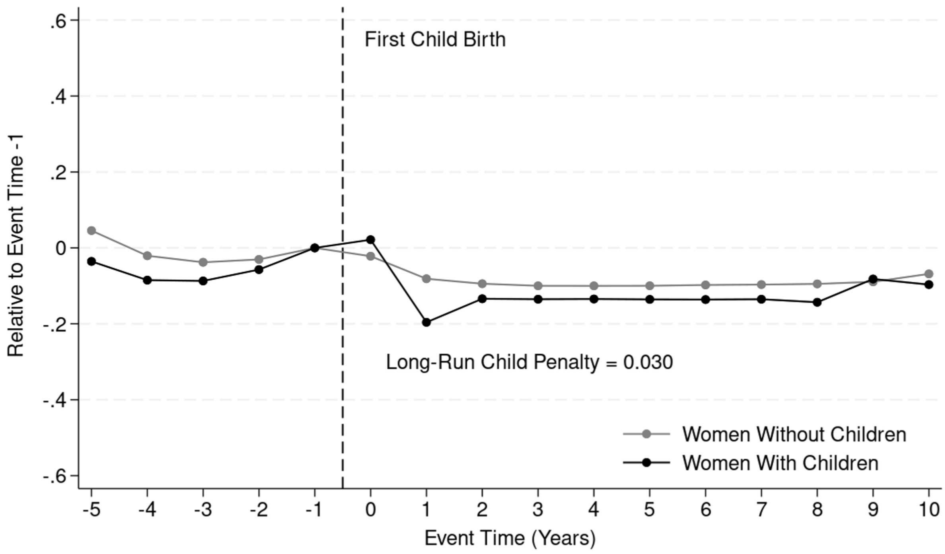

Figure 2: Employment Rate Impact of Children in a Difference-in-Differences Event Study Design

Note: The figure shows the evolution of formal employment rates relative to the year before the birth of the first child for women with children compared to those who never have children (assigning placebo births based on the observed distribution of age at first child among those who have children). In the figure we control for year and age fixed effects. The details of how we construct the control groups women who never have children are described in Section 3.1.1. The figure reports long-run child penalties for women, estimated as a difference-in-differences between those who have children and those who never have children. The shaded 95% confidence intervals are based on robust standard errors.

In terms of employment, we verify that at time $t = 0$ there is no significant difference between women with children and without. Rather, we observe a sharp drop on employment rates on mothers one year after child birth, which is recovered slightly the following year yet remains steady and close to non-mothers trajectory over time, only showing signs of catching up with the path of non-mothers 9 years after childbirth. The estimated penalty in employment status in formal labor markets is relatively small, only 3%.

Two relevant comments should be made about our results. The first one pertains to the timing of the registered penalty. Contrary to the expected pattern of a drop in outcomes at $t = 0$, we observe a decline occurring instead at $t = 1$. This deviation from the anticipated effect can be attributed to the labor and maternity regulations in Brazil, where mothers are entitled to 5 months of job protection following childbirth. Consequently, the potential impact on employment is often delayed until the subsequent year. Secondly, it is important to acknowledge that the RAIS database exclusively covers the formal labor market in Brazil. The informal sector, which represents a significant portion of the economy (Engbom et al. (2022); Firpo and Pieri (2018)), remains unaccounted for in this dataset. Therefore, exiting employment within the confines of our sample does not necessarily imply unemployment, as individuals may still be engaged in informal work. Nevertheless, our focus on college-educated individuals provides some degree of insulation against this influence, as this group typically exhibits a stronger attachment to the formal labor market (Machin and McNally (2010)), which is also suggested by the small penalty in this outcome.

Probability of Working in a Managerial Position

Understanding the impact of children on the probability of working in a managerial position is crucial in comprehending career dynamics and work-life balance of mothers. Figure 3, plots the evolution of this outcome for women with and without children.

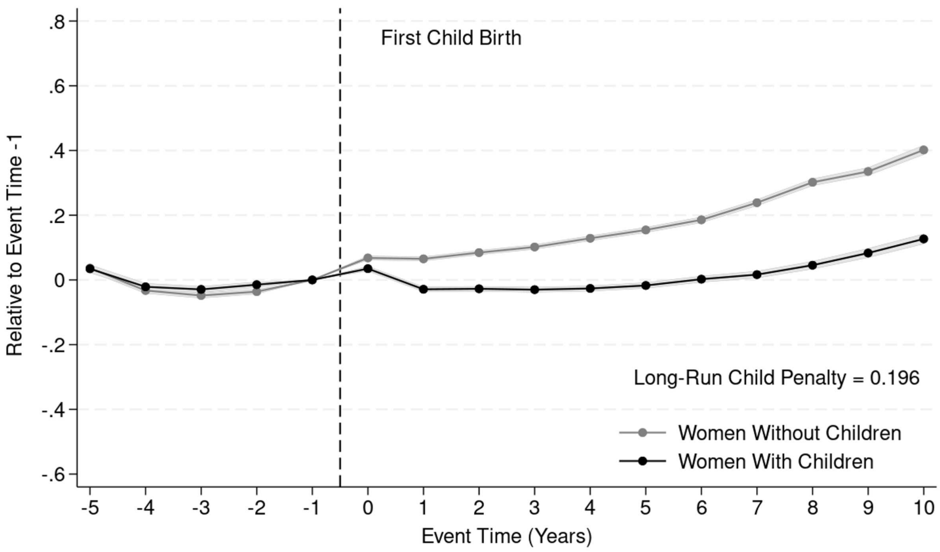

Figure 3: Probability of Working a Managerial Position: Impact of Children in a Difference-in-Differences Event Study Design

Note: The figure shows the evolution of the probability of working in a managerial position relative to the year before the birth of the first child for women with children compared to those who never have children (assigning placebo births based on the observed distribution of age at first child among those who have children). We identify managerial jobs based on *Classificação Brasileira das Ocupações 2002*, the Brazilian occupation classification. The observations belonging to Great Group 1 of this classification are considered having a managerial role. In the figure we control for year and age fixed effects. The details of how we construct the control groups women who never have children are described in Section 3.1.1. The figure reports long-run child penalties for women, estimated as a difference-in-differences between those who have children and those who never have children. The shaded 95% confidence intervals are based on robust standard errors.

The graph highlights a notable trend in the relationship between women's age, childbirth, and the likelihood of working in a managerial position. As women age and their careers progress, a distinct disparity emerges between those without children and mothers. The graph initially starts with a narrow gap precisely at event time, indicating that both groups have a comparable probability of holding managerial positions. Women without children consistently exhibit a substantially higher probability of working in managerial roles compared to mothers, implying faster-paced career advancement, which becomes further exacerbated as time progresses. This observation suggests that the presence of children may introduce additional challenges or constraints that hinder career progression, contributing to the observed disparity in managerial positions between the two groups. In the long-run, we estimate that mothers fare 19.6% behind non-mothers in terms of probability of working in a managerial position.

3.3 Discussion

The present section pretends to discuss the relevance and validity of performing the difference-in-difference event study analysis presented above.
The literature on motherhood penalties on labour market outcomes is extensive (Abendroth et al., 2014; Angelov et al., 2016; Angrist and Evans, 1998; Correll et al., 2007; Fitzenberger et al., 2013; Ní Bhrolcháin and Beaujouan, 2012; Sigle-Rushton and Waldfogel, 2007; Waldfogel, 1998; Wilde et al., 2010) and focuses primarily in assessing outcome trajectories of mothers in comparison with that of fathers.

While event studies based around the birth of the first child allow to derive causal remarks regarding women's career trajectories, it is important to discuss and consider the causal identification underlying the procedure. Event study research designs are employed for treatment effect estimation in a setting where all units in the panel receive treatment but at random times - adequate for the case of childbirth provided that the decision of having a child is exogenous to labour market outcomes. This methodology works by differencing out pre-event levels such that we are able to identify post-event effects, which implies that it does not allow to capture effects happening prior to the event.

At the outset it is useful to briefly discuss the structure of an event study. This simple framework, developed by Kleven et al. (2019) will provide a basis for the discussion of details regarding identification and the relevance of the procedure employed in this dissertation.

The initial task of conducting an event study is to define the event of interest and identify the period over which the intended outcomes will be examined, that is, the event window. We usually choose a range which encompasses some periods before and after the event. Let us consider the outcomes measured Y_{it} are:

$$Y_{it} = F(k_{it}, x_{it}, z_{it}), \tag{5}$$

where k_{it} denotes the number of children that individual i has at time t, x_{it} is a set of determinants chosen based on children and z_{it} a set of determinants not dependent on children. In the context of an event study aiming at identifying child impacts on wage rates, we can consider some measures for x to be hours worked, occupation, sector or firm and some measures for z such as age, preferences or ability.

Event study designs allow to capture short-run post-event impacts by comparing outcomes occurred just before event time (happening at $t-$) and after time zero (happening by $t+$). As such, short-run event study estimates capture:

$$E[Y_{it+} - Y_{it-}] = E[F(1, x_{it+}, z_{it+})] - E[F(0, x_{it-}, z_{it-})] \tag{6}$$

Assuming smoothness of the non-child earnings path, that is, that the expected outcomes of people without children just before and after the event are similar, then the equation above conveys the short-run effect of the first child. Conversely, long-run impacts are estimated based on an event time long after zero, $t++$:

$$E[Y_{it++} - Y_{it-}] = E[F(k_{iT}, x_{it++}, z_{it++})] - E[F(0, x_{it-}, z_{it-})] \tag{7}$$

The main difference between equations 6 and 7, is that long-run impacts do not consider solely the first child, but rather all the subsequent children that may have been born, with k_{iT} denoting lifetime fertility. Moreover, the smoothness assumption established in short-run impact estimates is hardly valid in the long-run, constituting a barrier to identification. In truth, we can have large changes in outcomes of individuals with no children before event time and over a long event window period, causing long-run child penalty estimates to be biased. Additionally, in most studies where the control group consists of men with children, the issue may be accentuated further, given that they are also treated by the event.

Given this, in this dissertation we chose to apply one of the identification checks procedures highlighted as solutions to this problem: using a control group of non-mothers to account for the non-child earnings trend in a difference-in-difference layout. In this exercise we can visualize the parallel pre-trends between women with and without children (different from zero, as expected in a traditional event study), which validates the exercise.

Our results contrast starkly with the ones found in the literature regarding the immediate impact of childbirth on earnings. Casarico and Lattanzio (2021) employ the same methodology with some refinements[1] to perform an analogous analysis in Italy, measuring the impact of motherhood on women with children vis-à-vis women without children. They report a sharp

[1]For instance, they include an individual fixed effect in equation 2 and other controls in the linear probability model of zero lifetime fertility used to assign individuals to the control group, namely: log daily wage distribution, quartiles of the AKM worker fixed effects distribution (Abowd et al. (1999)).

drop in outcomes such as the natural logarithm of annual earnings (about 80 log points), weekly wages (about 15 log points) and full time equivalent weeks worked (about 60 log points) upon the birth of the first child. This impact is largely attributed to changes on the labour supply along the intensive margin: by performing a decomposition of the child penalty, the authors verify that about half of the drop in annual earnings is attributed to the decline in full time-equivalent weeks worked, with the remaining half being associated with the reduction in wages and increase in the share of part-time work.

To address this, it is crucial to keep in mind the Brazilian institutional context as well as the sample we are working with in this dissertation, carefully described in Sections 2.1 and 2.2. Casarico and Lattanzio (2021) provide clear evidence on heterogeneous effects according to worker characteristics: workers who take shorter leaves (less than 6 months) verify a significantly reduced penalty upon child birth in comparison with those with longer leaves (more than half of the penalty is reduced); high-wage workers display a penalty 20 log points lower than low-wage workers; and there's also a slightly lower penalty observed for individuals whose age at first birth is higher than 30 years old.

Legislation in Brazil establishes that women are entitled to a paid maternity leave period of 120 days (equivalent to 4 months), which can be extended in some circumstances to 180 days (approximately 6 months) starting from 2010[2]. Almost 100% of the mothers in our sample take maternity leaves shorter than 6 months, and about 75% take leaves shorter than 4 months, which could explain the absence of a heavy penalty right after childbirth. Furthermore, in our sample, 95% of the women are over 30 years old when they become mothers for the first time, due to the fact that we are considering solely college-educated individuals in the present analysis - a group which has a higher propensity for delaying the birth of the first child. Finally, considering only college-educated individuals also implies we are working with the top of the income distribution, which tend to have lower immediate penalties.

However, while the immediate penalty is limited, the long-term gap between mothers and non-mothers continues to widen. Job mobility—both across firms and within firms—could be a potential factor in explaining this dynamic. Literature shows that job mobility is a key driver of wage growth (Costa Dias et al. (2021); Loprest (1992); Topel and Ward (1992)), yet motherhood often constrains these opportunities, which lower mobility rates and wage growth over time compared to non-mothers, for whom upward job mobility remains a critical factor in sustained wage increases.

[2]Firms who choose to enroll in the *Programa Empresa Cidadã* (PEC), can provide an extra 60 days of leave to their female employees, fully subsidized by the Brazilian administration. Other advantages include tax benefits. Nevertheless, a firm can be part of the PEC and still choose not to provide the leave extension. Additionally, it is important to note that eligibility conditions require tax returns to be on real profit, implying that only about 8% of Brazilian firms can enroll in the program (Meireles et al. (2017)).

The next section will investigate further how job mobility influences the gap between mothers and non-mothers in terms of wage growth, focusing specifically on transitions between firms and promotions.

4 Differentials in Job Mobility and Wage Growth

In this section of the dissertation, we aim to explore one of the mechanisms contributing to the wage differentials between women with and without children: job mobility.

Despite its significant impact on wages, job mobility has received relatively little attention as a factor in motherhood pay penalties. This oversight persists even though previous research has demonstrated that job mobility is crucial in shaping wage trajectories during the early stages of young men's and women's careers (Loprest (1992), Topel and Ward (1992))—the same period when substantial wage gaps begin to appear (Costa Dias et al. (2021)). As workers typically change jobs multiple times while establishing their careers, they generally experience considerable wage increases. However, motherhood can disrupt this process, reducing job mobility and hindering wage growth.

Furthermore, previous research on parenthood and job mobility has primarily focused on developed economies. However, patterns of job mobility and associated wage returns may vary in countries like Brazil, which provide guaranteed maternity leave and have a higher prevalence of part-time work (Blau and Kahn (2017), Brady et al. (2019), Mandel and Semyonov (2005), Budig et al. (2016)).

In our analysis, we examine both the role of job moves with the same employer (e.g. promotions) and job moves between employers. Firstly, we will describe these mobility patterns across groups and age, followed by an analysis on the returns to job mobility for mothers and how they vary with age and tenure, following Manning and Swaffield (2008) and Del Bono and Vuri (2011). Finally, we show how differences in rates of, and returns to, job mobility accumulate over the life course to affect mothers' wages using the Gelbach (2016) decomposition, in line with Avram et al. (2024).

4.1 Identification

In this chapter, we aim to elucidate the mechanisms behind wage disparities between mothers and women without children, focusing on job mobility. Our analysis revolves around the evolution of wages from one point in time to another, examining both job movements within firms (promotions) and between firms. Here, we detail the definitions and methodologies used for our key variables: wage growth and job mobility.

4.1.1 Wage Growth

To measure wage growth, we adopt the definition used by Manning and Swaffield (2008). For any year in which an individual is employed, wage growth is defined as the difference in log wages between the next year they are employed and the log wage of the current year. This definition allows for gaps between wage observations, which we will correct for in the regression analysis presented later in this chapter.

4.1.2 Job Mobility Events

We identify a between-firm job movement if an individual changes firms from year $t-1$ to year t. Only job-to-job transitions are considered, meaning the gap between consecutive wage observations must be one year for the individual-year observation to qualify as a between-firm change. This approach ensures we compare exclusively the utility of staying with one firm versus moving to another. This cannot be established if we consider transitions into unemployment and then to other firms.

For within-firm changes, we focus exclusively on promotions, which is highlighted in the literature as being a key driver of wage growth within firms (Blau and Devaro (2007); Card et al. (2016); McCue (1996)). We employ Bronson and Thoursie (2019)'s methodology, which innovatively tracks workers' movements through a firm's wage hierarchy instead of the traditional career ladder. This approach allows to leverage RAIS's worker-firm linkages to observe individuals' wage increases relative to their co-workers.

Despite having occupation codes in RAIS, we choose not to use them for constructing our promotion measure for two main reasons. First, distinguishing between lateral and vertical moves based on occupational codes is challenging. Second, even detailed four-digit codes are not fine-grained enough to capture many career advancements. Since a change in occupation code is neither necessary nor sufficient to indicate an upward move within the firm, we do not use this information in our promotion measure.

Our definitions for within-firm mobility variables are as follows:

Relative Wage Growth This continuous variable compares an individual's wage growth to the mean wage growth of other high-skilled co-workers at the same firm in the same year. For each individual who did not switch firms in the past year, we identify all other college-educated employees at the same firm. We then calculate the mean wage growth for these co-workers by firm and year. Relative wage growth is defined as the difference between an individual's wage growth and this firm-year average. This measure isolates individual wage growth relative to co-workers, accounting for systematic differences in average wage growth across firms in a given year.

Promotion This binary variable identifies years in which individuals experience significant upward wage moves. We transform the relative wage growth variable into a binary one, setting it to one when a worker's wage growth exceeds the average wage growth of their co-workers by n log points. These events are referred to as promotions. In our analysis, we use a conservative threshold of 10 log points to define significant promotions.

These variables allow us to study the impact of job mobility on wage evolution, shedding light on how different job movements influence wage growth and contribute to the wage disparities observed between mothers and women without children.

4.2 Job Mobility Patterns

We begin by presenting descriptive evidence on gender differences in both within-employer and between-employer mobility rates, segmented by motherhood status (Table 3). The data reveals significant mobility, with an average of 20% of women changing jobs annually, of which about two-thirds involve moving between employers. Notably, mothers exhibit higher rates of both internal and external job mobility on average, a trend that contrasts with findings from the United Kingdom (Avram et al. (2024)).

Table 3: Annual probability of any job change, changing employer, and promotions by motherhood status

	Mothers	Non-Mothers
Mobility Annual Probabilities		
All Events	0.241	0.187
Between Firm	0.152	0.109
Promotion	0.089	0.078
Number of observations	1,601,304	7,362,121

To further explore the variations in mobility rates, we calculate adjusted mobility rates using separate logit models for each type of job mobility. These models account for demographic characteristics, prior labor market history, and lagged job attributes. The model specification is as follows:

$$\ln \frac{p_{it}}{1-p_{it}} = \alpha + \sum_k \beta_k M_{it} \times A_{it} + \gamma X_{it} + \delta Z_{it-s} + \phi W_{it-s} + \rho Gap_{it,it-s} + \eta_i + \delta_t + \epsilon_{it} \quad (8)$$

In this equation, p_{it} represents the probability of a promotion or employer change for individual i in year t. The variables include M_{it}, a motherhood indicator; A_{it}, age; X_{it}, a vector of demographic factors like education level and cohort; Z_{it-s}, labor market history variables such as part-time employment, firm size, and employment duration; W_{it-s}, lagged job characteristics like part-time status, managerial role, and firm size; and $Gap_{it,it-s}$, an adjusted potential experience measure as developed by Manning and Swaffield (2008). This measure adjusts for gaps in consecutive wage observations, enhancing the accuracy of our estimates.

The simplest way of getting a gap-adjusted measure of wage growth would be simply to divide the observed wage growth by the gap between wage observations to get an estimate of annualised wage growth. However the validity of this procedure is based on the implicit assumption that wage growth does not vary with experience, something we know to be untrue.

Therefore we employ their correction, which is done by including in the regressors an adjusted potential experience term and its square given by:

$$\sum_{j=0}^{g-1}(e_{it} + j)$$

Here, e_{it} denotes potential experience, and g represents the gap between observations in years.

We also include region and year fixed effects (η_i and δ_t, respectively). Given the longitudinal nature of the data, we cluster standard errors at the individual level.

Figure 4 illustrates the predicted probabilities of different types of job mobility based on motherhood status and age, with detailed model coefficients available in the Appendix. Our findings show that while job mobility generally decreases with age, there are slightly distinct patterns between employer changes and promotions. Throughout the life cycle—whether early in their careers, around the time of childbirth, or later—mothers consistently exhibit slightly higher rates of employer changes compared to non-mothers. This trend is stable, and confidence intervals for the two groups do not overlap. In contrast, promotion rates tell a different story. Early in their careers, mothers and non-mothers have showcase identical promotion rates. However, around the early 30s —coinciding with the average age of first childbirth in our sample (32.5 years) — a divergence emerges, as mothers begin to experience lower promotion rates than their non-mother counterparts. Although, over time, mothers show signs of catching up in their mid-to-late 40s, as their promotion rates gradually approach those of non-mothers.

4.3 Returns to Job Mobility

To assess differences in annual wage growth between job movers and stayers, we estimate a wage growth equation. The specification for wage growth is similar to that of Del Bono and Vuri (2011) and Manning and Swaffield (2008). The model specification is as follows:

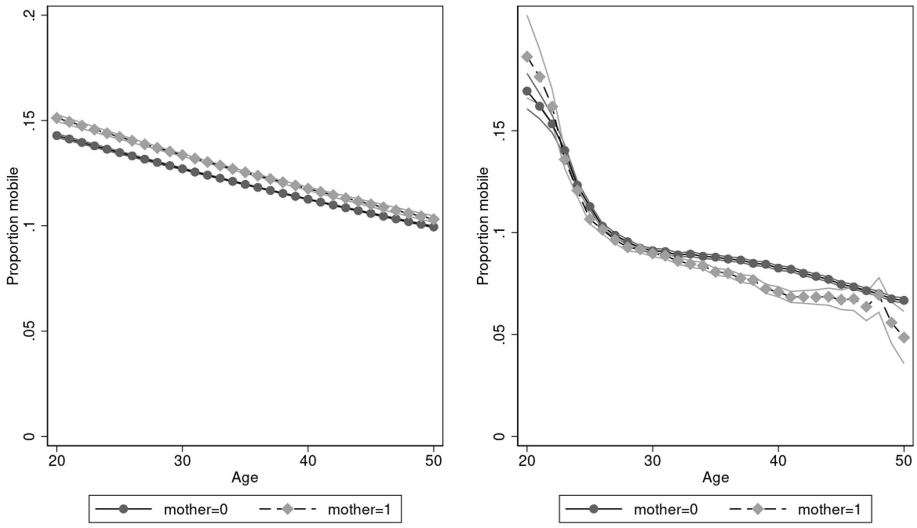

Figure 4: Annual (adjusted) proportion of women experiencing job mobility events

Note: Left-panel shows between employer probability rates and the right-panel within employer probability rates. Coefficients are estimated from logistic regressions for each type of job move after controlling for demographic characteristics, previous labour market history, and lagged job attributes. Details of the covariates included in the specification are given above. Full results from the underlying models are in the Appendix.

$$\Delta W_{it,it-s} = \alpha + \sum_k \beta_k B_{it,it-s} \times M_{it} \times A_{it} + \sum_k \theta_k P_{it,it-s} \times M_{it} \times A_{it} + \gamma X_{it} + \delta Z_{it-s} + \\ + \phi W_{it-s} + \rho Gap_{it,it-s} + \mu_i + \epsilon_{it} \quad (9)$$

In this equation, $\Delta W_{it,it-s}$ the change in log hourly wage for individual i between year t and the most recent previous wage observation $t - s$. The variables include between and within (promotions) job mobility dummies $B_{it,it-s}$ and $P_{it,it-s}$, respectively; M_{it}, a motherhood indicator; A_{it}, age; X_{it}, a vector of demographic factors like education level and cohort; Z_{it-s}, labor market history variables such as part-time employment, firm size, employment duration; W_{it-s}, lagged job characteristics like part-time status, managerial role, and firm size; and $Gap_{it,it-s}$, the adjusted potential experience measure as developed by Manning and Swaffield (2008). This measure adjusts for gaps in consecutive wage observations, enhancing the accuracy of our estimates. Finally, μ_i is the individual fixed effect.

Figure 5 presents the wage returns associated with different types of job mobility for women with and without children, based on the model outlined above (full set of coefficients are available in the Appendix). The results indicate significant differences in wage returns by motherhood status. Non-mothers consistently achieve higher returns from job mobility compared to mothers. While wage returns for non-mothers remain stable across their careers, mothers experience a notable decline. For mothers, returns on promotions begin to diverge in their late 20s, and for employer changes, the divergence starts in their early 30s. This gap in returns continues to widen with age, and there is no evidence of recovery for mothers at any stage of their careers.

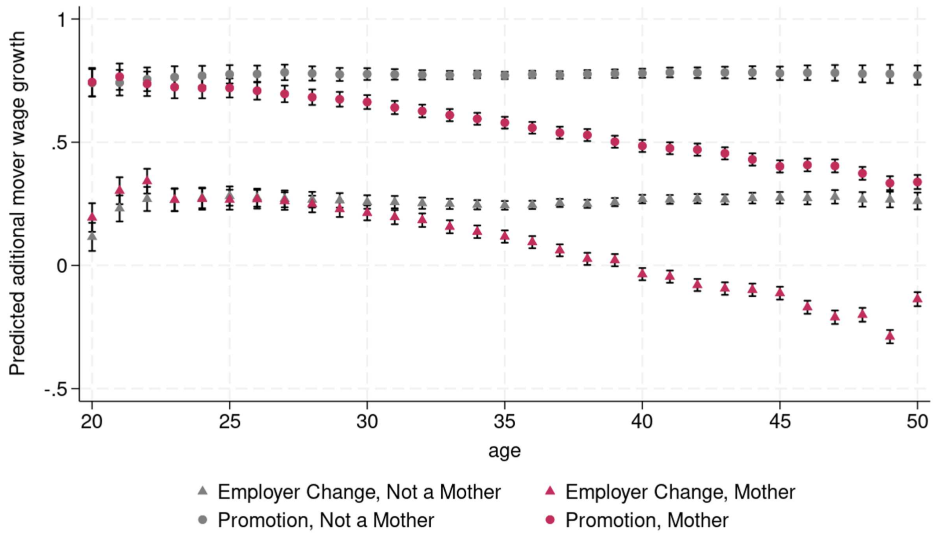

Figure 5: Predicted wage gains associated with different types of job mobility.

Note: Estimates come from regressions of annual changes in log wages on our measures of mobility after controlling for demographics characteristics, previous labour market history, and lagged job attributes. Details of the covariates included in the models are given above. Full results from the underlying models are in the Appendix.

4.4 Decomposition Analysis

In the final part of our analysis, we assess how job mobility contributes to differences in wage growth rates between mothers and women without children using the decomposition method developed by Gelbach (2016). Traditionally, the impact of covariates on wage gaps is measured by comparing a "base" model, which lacks the covariate of interest, to a "full" model that includes it. This method, however, can be affected by the sequence in which covariates are introduced.

Gelbach (2016) addresses this issue by proposing a decomposition method based on the omitted variable bias formula. This method, like traditional approaches, involves comparing a "base" model to a "full" model and estimating how much of the wage gap in the "base" model can be attributed to each covariate added in the "full" model. The key advantage is that Gelbach's method is not influenced by the order of covariates in the "full" model.

For our analysis, the "base" model includes a motherhood indicator, age (and its square), year and region fixed effects, the number of gap years, and an interaction term between motherhood and age. The "full" model incorporates all controls from our main specification (Equation 4.3), including job mobility indicators. The decomposition results reveal how much of the lower wage growth observed for mothers, relative to childless women, can be explained by differences in job mobility. Results can be seen in Table 4 below. The first two columns show wage growth differences in the "Base" and "Full" models, respectively. The difference between these two columns represents the differences in wage growth explained by all covariates, including demographics, labour market history, prior job characteristic and job mobility. The last column shows differences in wage growth explained by job mobility only.

Table 4: The role of job mobility in explaining differences in the wage growth of mothers vis-à-vis women without children: results from the Gelbach decomposition

Difference in wage growth ('Base' model)	Difference in wage growth ('Full' model)	Difference explained by job mobility
0.06	-0.07	-0.02

Notes: The "base" model contains controls for motherhood, age (and its square), year and region fixed effects, the gap and interaction term between motherhood and age. The "full" model adds controls for education, labour market history, prior job characteristics and a job mobility dummy (equal to 1 if the woman has verified either a between or within firm job movement). The part of the wage growth gap explained by job mobility is given in the 3rd column.

Our findings suggest that, before including any covariates, the gap in wage growth between mothers and non-mothers is positive and favours mothers, with higher wage growth for this former group. However, the full model which includes additional controls and mobility flags reveals instead a gap in wage growth which penalizes mothers. In this gap decomposition, we find that differences in job mobility rates account for a reduction of the growth gap between mothers and non-mothers of -0.02, which signals job mobility as a factor that hinders the wage growth of mothers.

4.5 Discussion

In many high-income countries, the wage penalties associated with motherhood now account for nearly the entire remaining gender pay gap (Cortés and Pan (2023); Kleven et al. (2019)). However, in developing nations, the motherhood penalty is often even more severe due to a combination of weaker labor protections, limited access to affordable childcare, and deeply rooted gender norms. According to the International Labour Organization (ILO), the gender pay gap is often wider in these regions, with motherhood contributing to a substantial portion of the disparity. The scarcity of formal employment opportunities and the absence of robust social safety nets further exacerbate this issue, leading to significant long-term economic disadvantages for mothers (Jayachandran (2020)). As time since childbirth increases, the associated economic disadvantage tends to accumulate (DiPrete and Eirich (2006)).

The lower earnings observed among mothers have been attributed to several factors, including reduced labor market experience due to time spent out of the workforce or working part-time (Blundell et al. (2016)), loss of job-specific human capital when women do not return to the same job after maternity leave (Waldfogel (1998)), and occupational choices that favor jobs with family-friendly working conditions (England (2005); Fuller (2017); Fuller and Hirsh (2019)). Dynamic perspectives have emphasized how reduced training and promotion opportunities often shift women onto the "mommy track" after childbirth, limiting their career progression (Wilde et al. (2010)).

In this section, we explored an additional and crucial mechanism behind the motherhood wage penalty: the impact of motherhood on job mobility and its associated wage gains. We began by examining overall rates of job mobility and found that while mothers and non-mothers exhibit similar patterns of mobility, key differences emerge around the early 30s. Specifically, mothers are slightly more likely to change employers but less likely to receive promotions during this period. This latter finding may suggest the presence of direct or indirect workplace discrimination, with employers potentially overlooking mothers for promotion. Furthermore, the negotiation processes that often accompany internal promotions, which are frequently based on external job offers, may disproportionately disadvantage women with children.

We also analyzed whether mothers experience lower rates of wage growth when changing jobs or employers compared to other workers. Previous studies of North America have found that returns to job mobility are generally lower for women than for men (Fuller (2008); Keith and McWilliams (1999); Loprest (1992); Pearlman (2018)), a pattern also observed in Germany (Wieschke (2020)), Spain (Hospido (2009)), and Italy (Del Bono and Vuri (2011)). Extending this analysis to mothers, our findings reveal that, on average, returns to mobility are systematically lower for mothers compared to women without children, across both types of mobility events. While both groups experience similar returns in the early stages of their careers, a divergence emerges in the late 20s, with mothers increasingly receiving lower wage gains, which continue to decline as they age.

In developing countries, research on the returns to job mobility for mothers versus women without children is less extensive but reveals significant disparities. Studies indicate that motherhood often constrains job mobility in these regions due to cultural and societal norms that prioritize family responsibilities over career advancement. For instance, a World Bank study highlighted that the lack of affordable and accessible childcare in many low- and middle-income countries significantly hinders mothers' ability to engage in the labor market or pursue better job opportunities. This barrier limits their earning potential and career growth compared to women without children, who generally experience greater mobility and better economic outcomes from job changes (Halim et al. (2022)).

Furthermore, the motherhood penalty in developing countries is often exacerbated by lower levels of social mobility and fewer opportunities for women to transition into higher-paying jobs or industries. Analyses in these regions frequently overlook the specific challenges faced by women, focusing predominantly on male-dominated sectors or assuming uniform mobility trends across genders (Iversen et al. (2021)). Our findings underscore that, while job mobility is typically associated with wage increases and career progression for women without children, mothers in developing countries frequently encounter systemic barriers that limit these benefits. This dynamic further exacerbates existing gender wage gaps and constrains their economic advancement (Choi et al. (2023)).

5 Robustness Checks and Heterogeneity

In this section we will be addressing some shortcomings of our event study analysis through the performance of robustness checks. We will also assess if our results on the motherhood penalty and returns to mobility differ depending on firm size in an heterogeneity analysis.

5.1 Additional difference-in-difference estimators

There are three main contributing factors in this work that may induce some bias into the average treatment effect estimations performed in the previous sections, related to data limitations and sample-specific characteristics.

The first one is a direct consequence of working with an unbalanced panel of individuals, which can lead to biased estimates of treatment effects. An unbalanced panel consists of individuals with varying observation patterns, including those with fewer observations or observations only during specific time periods. This diversity in observation patterns may result in an incomplete representation of the true heterogeneity of treatment effects across individuals. As a consequence, our estimates may not fully capture the actual impact of the treatment. It is important to acknowledge that this shortcoming introduces a potential source of bias in our estimates, which could affect the magnitude of the estimated treatment effects.

Additionally, group composition (mothers-to-be vs. never-mothers) introduces another dimension of unbalancedness related to periods away from the formal labor market. This affects mothers and non-mothers differently due to anticipation effects: for instance, anticipating motherhood can influence women's decision-making and lead to specific choices in education and career investment. In light of the forthcoming responsibilities associated with having children, women may opt to invest less in education or career advancement, perceiving potential challenges and reduced earnings in the future. This could also mean that the periods away from work may be longer or more frequent for women who wish to become mothers, resulting in a greater disruption to their career progression, potentially influencing their labor market outcomes and introducing bias in our estimates.

Finally, another limitation is that our data only allows us to observe maternity leave taking and not the birth of the first child directly. We assume that the first leave taken by an individual within the observed period and fertility window corresponds to the birth of the first child. However, this assumption overlooks the possibility that individuals may have had children prior to entering the formal labor market, such as teenage pregnancies or children born during periods of informal employment. This limitation biases our estimates downward because it fails to account for potential pre-existing motherhood effects that could impact labor market outcomes.

To address the potential sources of bias resulting from the aforementioned limitations, this dissertation employs two different estimators to check and enhance the robustness of the findings.

Borusyak et al. (2024) identify several problems that can arise in difference-in-difference settings. One issue is that the OLS estimator can conflate situations where neither unit is treated at a given time or both units have the same treatment status, which can lead to misleading comparisons. Another problem is that if causal effects exhibit dynamics, the difference between post-treatment outcomes of treated and untreated units may reflect the wedge between long- and short-run effects, since the treated units were treated earlier. This difference can lead to an overestimation of short-run effects and an underestimation of long-run ones. Finally, underidentification and negative weighting can emerge when the control group is allowed to be on a different time trend. The authors develop their own imputation method for estimating causal effects and testing for pre-trends, which we will apply to our estimation on the average treatment effect on the treated. Results can be found in Figure 6, in the Appendix.

To further strengthen the robustness of our findings, we also employ the Callaway and Sant'Anna (2021) estimator, which is particularly well-suited for settings with staggered treatment adoption and heterogeneous treatment effects across units and time periods. Their approach introduces a flexible framework for estimating group-time average treatment effects, which are causal parameters defined for each group of units (based on when they first received treatment) at each time period. This allows for a more granular analysis of treatment effects that can vary across different groups and over time, unlike traditional difference-in-difference methods that often assume homogeneity in treatment effects. Results are depicted in Figure 7, in the Appendix.

The Borusyak et al. (2024) estimator reveals consistently negative treatment effects, showing signs of improvement only around 7 years after the first birth, yet never reaching pre-treatment values. This finding highlights that the immediate penalty captured by OLS may indeed underestimate the long-term impact of motherhood on wages. This method better accounts for the dynamic nature of treatment effects and pre-trends, isolating the causal impact of motherhood over time and avoiding potential distortions due to short- and long-term wage effects being conflated.

Similarly, the Callaway and Sant'Anna (2021) estimator corroborates the previous findings, with treatment effects remaining negative over time, however, without the gradual improvement identified in the previous estimation. This approach allows for more granular analysis by accounting for heterogeneity in treatment effects across different groups and time periods, providing a clearer understanding of how motherhood impacts labor market outcomes for different individuals. The consistent negative treatment effects further underscore that motherhood leads to a substantial and prolonged wage penalty.

In sum, both estimators demonstrate that the wage penalty mothers experience is larger and negatively more persistent than suggested by OLS. These findings suggest that motherhood imposes a more severe long-term economic penalty, one that the OLS estimator does not capture as strongly. Nevertheless, the use of these more sophisticated estimators strengthens the validity of the results and provides a clearer understanding of the enduring nature of the impact of motherhood in wage rates.

5.2 Heterogeneity

This section complements the previously presented findings by conducting an heterogeneity analysis that allow us to understand the drivers of the observed effects. Using the same framework introduced previously, we explore how the estimated child penalty and returns to mobility events vary across different firm characteristics, particularly firm size.

Figures 8 and 9 show that the motherhood penalty in wage outcomes is noticeably smaller in for women working in small firms ($n \leq 10$) compared to larger firms ($n > 10$), with an estimated penalty of 20% in the former group versus 24% in the latter. Additionally, we find that returns to mobility differ by firm size as well: promotions in small firms yield smaller wage increases compared to promotions in large firms. Results can be found in Figures 10 and 11. However, the difference in returns between mothers and non-mothers is narrower in small firms, suggesting less disparity in how wage progression is distributed between the two groups in those settings.

These results can be understood through the different structural dynamics of small versus large firms. Smaller firms typically offer fewer opportunities for mobility, which may serve as a protective mechanism for mothers. In these environments, the limitations in mobility likely lead to fewer instances where mothers experience wage reductions due to interruptions or reduced flexibility after childbirth. This relative stability in small firms means that while overall wage growth is slower, the penalty mothers face is less severe than in larger firms, where the stakes and competition for mobility are higher.

On the other hand, large firms offer more frequent opportunities for mobility, promotions, and career advancement, but these opportunities come at a cost for mothers, who may be perceived as less flexible or available. This contributes to a larger motherhood penalty, as mothers face steeper wage losses when attempting to navigate career disruptions post-childbirth. In small firms, where mobility and competition are more constrained, the wage returns to promotions are lower for all employees, but the environment offers less potential for exacerbating the wage gap between mothers and non-mothers. Thus, it appears that the workplace context plays a crucial role in shaping the severity of the motherhood wage penalty, with smaller firms providing a comparatively more protective but limited environment for mothers.

6 Conclusion

This dissertation provides an in-depth analysis of the motherhood penalty in Brazil, shedding light on the profound and persistent impact of motherhood on labor market outcomes. One of the most striking findings is that the child penalty between mothers and non-mothers not only persists but widens over time, making it increasingly difficult for mothers to catch up. This widening differential, which reaches a long-term penalty of 21%, underscores the cumulative disadvantages mothers face throughout their careers.

But what drives this widening wage gap over time? Job mobility emerges as a contributing factor. This dissertation reveals that mothers face lower promotion rates than non-mothers until their mid-to-late 40s, and even when promotions do occur, the wage gains they realize from these career advancements diminish with age. This growing disparity in the returns to job mobility compounds the wage growth gap, which in turn helps to explain the widening of the motherhood penalty. This highlights the role of job mobility and demonstrates how the labor market systematically disadvantages mothers cumulatively over their entire careers.

An important avenue for future research lies in exploring how these wage returns vary not just with age, but also with worker tenure. Tenure is a critical factor in wage progression, as it often correlates with higher wages, promotions, and job stability. By examining how wage returns differ across tenure, future studies could uncover whether mothers face disadvantages even within long-term employment relationships. Such research could reveal whether mothers are less likely to benefit from tenure-based wage increases or promotions compared to non-mothers, shedding light on potential biases or structural barriers within firms.

Investigating returns across tenure could also clarify the impact of maternity leave on wage trajectories, particularly if time off for childbearing disrupts the accumulation of tenure-related benefits. This focus would provide a more nuanced understanding of the motherhood penalty, emphasizing the need to consider both mobility and tenure when analyzing the career outcomes of mothers.

In conclusion, this dissertation contributes to the broader understanding of gender disparities in the labor market by focusing on the underexplored area of motherhood and job mobility in a developing country context. It underscores the importance of considering both immediate and long-term effects of motherhood on career trajectories and earnings, paving the way for future research that could further investigate the nuances of these dynamics across different settings and populations, providing a more comprehensive view of the challenges mothers face in the labor market.

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Appendix A: Tables

Table 5: Regression coefficients of models estimating the probability of job changes: by age and motherhood status

	Probability of job mobility event	
	Between Firm	Within Firm
Mother	0.106***	-0.098***
	(0.019)	(0.021)
Age	-0.016***	-0.008***
	(0.000)	(0.000)
Mother x Age	-0.001	0.003***
	(0.001)	(0.001)
MSc	0.253***	-0.136***
	(0.009)	(0.012)
PhD	0.002	-.110***
	(0.017)	(0.021)
Firm size (lagged)	-0.039***	2.13e-06***
	(0.000)	(3.34e-08)
Part-time (lagged)	0.385***	0.140***
	(0.004)	(0.004)
Months worked (lagged)	0.401***	0.102***
	(0.002)	(0.002)
Months worked sq. (lagged)	-0.049***	-0.024***
	(0.000)	(0.000)
Manager (lagged)	-0.034***	0.085***
	(0.004)	(0.005)
Cohort 1930-1954	-0.019***	0.107***
	(0.010)	(0.016)
Cohort 1955-1980	-0.244+	0.087***
	(0.004)	(0.005)
Adjusted Gap	0.158***	-0.133***
	(0.001)	(0.001)
Adjusted Gap Squared	-0.006***	0.005***
	(0.000)	(0.000)
Constant	0.237***	-0.017
	(0.026)	(0.026)
N	9,675,695	9,675,695

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$
Note: Standard errors in parentheses. Estimates after controlling for individual fixed effects.

Table 6: Descriptive Statistics of Variables used as Regressors

	Mothers	Non-Mothers
Mobility Annual Probabilities		
All Events	0.241	0.187
Between Firm	0.152	0.109
Promotion	0.089	0.078
Average Wages		
Log Hourly Wage	2.973	3.078
Log Hourly Wage Growth	.072	0.052
Average Wages x Mobility		
Log Wage Growth Between Firm	0.413	0.306
Log Wage Growth Within Firm	0.824	0.773
Covariates		
Education		
Share MSc degree	0.020	0.019
Share PhD degree	0.006	0.006
Age	33.0	41.5
Months in Employment	10.7	11.0
Share Part-time	0.128	0.159
Firm Size		
Share Managerial Position	0.090	0.094
Cohort		
Share born 1930-1954	0.000	0.069
Share born 1955-1980	0.385	0.695
Motherhood Specific Variables		
Average Leave Duration (days)	106.58	(-)
Average Age at First Child	32.5	(-)
Number of observations	1,601,304	7,362,121

Note: This table presents the average of the variables used as regressors in our job mobility analysis, pertaining the sample of mothers and non-mothers. Details on the identification of between firm moves and promotions can be found in section 4.1.2. To facilitate the computation of high dimension fixed-effects regressions, we work with a random sample of mothers and non-mothers, corresponding to roughly 30% of the original sample.

Table 7: Regression coefficients from model estimating returns to job mobility: by age and parenthood status

	Log hourly wage growth
Age	0.009***
	(0.002)
Mother x Age	-0.006***
	(0.001)
Promotion	1.004***
	(0.006)
Promotion x Mother	0.114***
	(0.018)
Promotion x Mother x Age	-0.009***
	(0.001)
Employer Change	1.218***
	(0.007)
Employer Change x Mother	0.139***
	(0.019)
Employer Change x Mother x Age	-0.020***
	(0.001)
MSc	0.004*
	(0.003)
PhD	0.003
	(0.004)
Adjusted Gap	-0.018***
	(0.002)
Adjusted Gap Squared	0.202***
	(0.002)
Months worked	0.436***
	(0.001)
Months worked (squared)	-0.017***
	(0.000)
Part-time employment	0.202***
	(0.002)
Manager position (lagged)	-0.034***
	(0.001)
Firm size (lagged)	-0.033***
	(0.001)
Number of promotions	-0.038***
	(0.002)
Days of maternity leave	-0.000***
	(0.000)
Constant	-2.945***
	(0.075)
N	9,675,695

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$
Note: Standard errors in parentheses. Estimates after controlling for individual fixed effects.

Appendix B: Figures

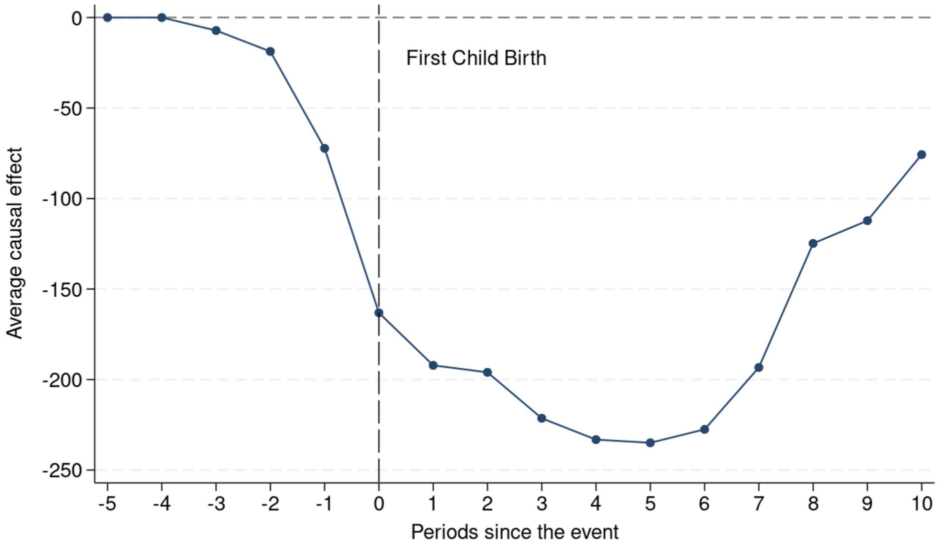

Figure 6: Borusyak et al. (2024) Imputation Estimator for Average Treatment Effect on the Treated

Note: These figures present event study estimates on wage rates around the year of the first child's arrival for women, using the Borusyak et al. (2024) imputation method. The sample comprises the panel of mothers described in Section 3.1.1. The imputation estimator addresses potential biases arising from dynamic treatment effects and heterogeneous timing of treatment, ensuring robustness in capturing causal impacts.

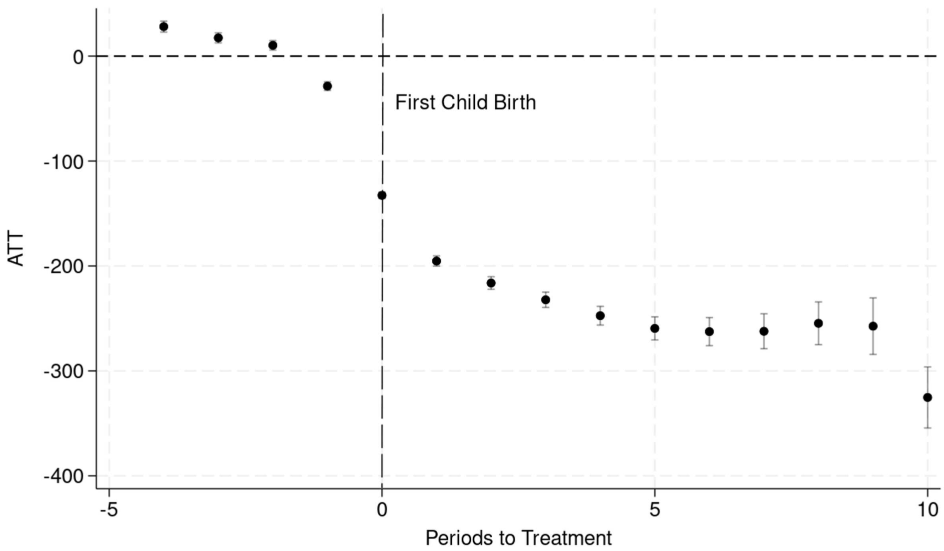

Figure 7: Callaway and Sant'Anna (2021) Estimator for Average Treatment Effect on the Treated

Note: These figures present event study estimates on wage rates around the year of the first child's arrival for women, using the estimator developed by Callaway and Sant'Anna (2021). The coefficients are aggregated by relative event time, reflecting the impact of the first childbirth on wage outcomes. The sample is composed of mothers as described in Section 3.1.1, and the estimator flexibly accounts for treatment effect heterogeneity.

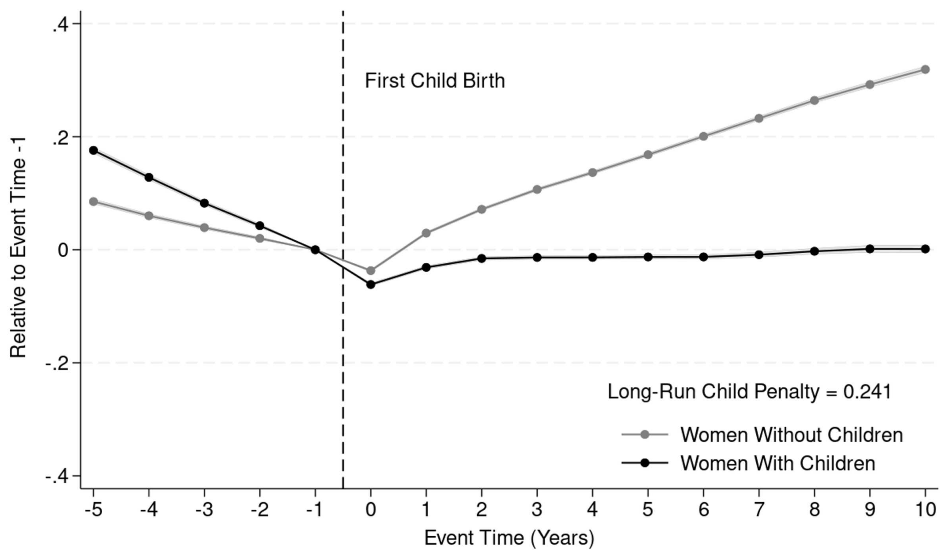

Figure 9: Wage Rate Impact of Children in a Difference-in-Differences Event Study Design: Large Firms

Note: The figure shows the evolution of monthly wage rates relative to the year before the birth of the first child for women working in larger firms ($n > 10$) with children compared to those who never have children (assigning placebo births based on the observed distribution of age at first child among those who have children). In the figure we control for year and age fixed effects. The details of how we construct the control groups women who never have children are described in Section 3.1.1. The figure reports long-run child penalties for women, estimated as a difference-in-differences between those who have children and those who never have children. The shaded 95% confidence intervals are based on robust standard errors.

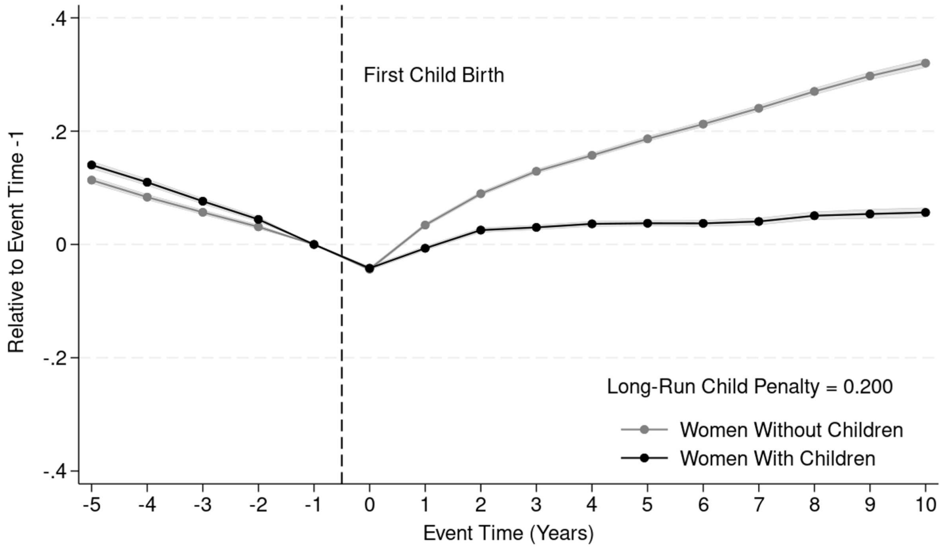

Figure 8: Wage Rate Impact of Children in a Difference-in-Differences Event Study Design: Small Firms

Note: The figure shows the evolution of monthly wage rates relative to the year before the birth of the first child for women working in small firms ($n \leq 10$) with children compared to those who never have children (assigning placebo births based on the observed distribution of age at first child among those who have children). In the figure we control for year and age fixed effects. The details of how we construct the control groups women who never have children are described in Section 3.1.1. The figure reports long-run child penalties for women, estimated as a difference-in-differences between those who have children and those who never have children. The shaded 95% confidence intervals are based on robust standard errors.

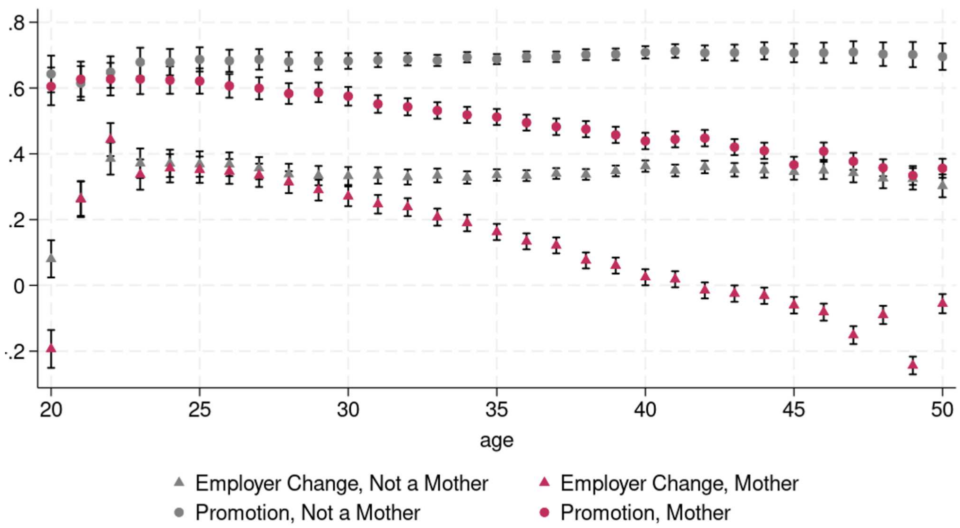

Figure 10: Predicted wage gains associated with different types of job mobility: Small Firms

Note: Estimates come from regressions of annual changes in log wages on our measures of mobility for women working in small firms ($n \leq 10$) after controlling for demographics characteristics, previous labour market history, and lagged job attributes. Details of the covariates included in the models are given in Section 4.3.

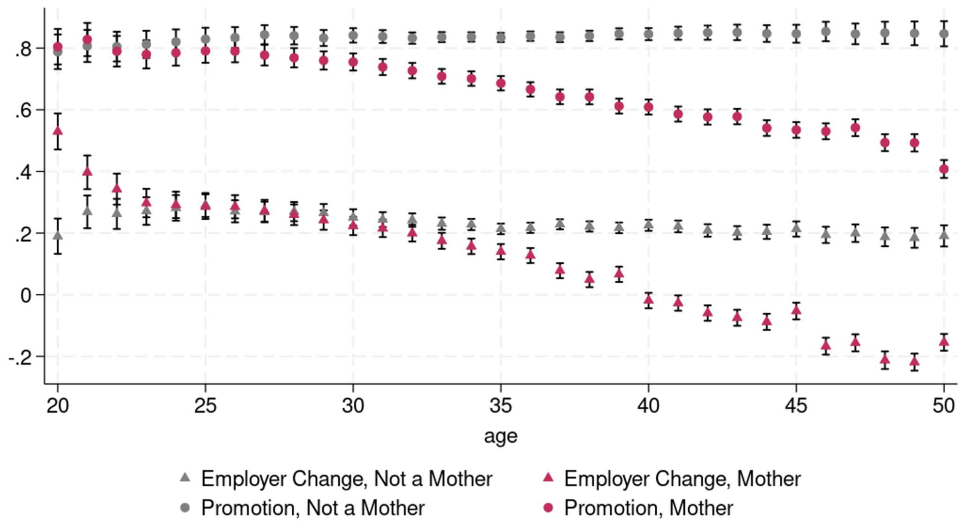

Figure 11: Predicted wage gains associated with different types of job mobility: Larger Firms

Note: Estimates come from regressions of annual changes in log wages on our measures of mobility for women working in larger firms ($n > 10$) after controlling for demographics characteristics, previous labour market history, and lagged job attributes. Details of the covariates included in the models are given in Section 4.3.