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

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## Abstract

This paper studies how the statutory right to work part-time affects mothers' post-birth labor market outcomes and higher-order fertility. I use a differences-in-differences design to investigate the introduction of a German law in 2001 that grants the right to work part-time to employees working in firms with more than 15 employees. I find that the reform does not increase the probability to return to work after childbirth significantly. However, mothers who gain the right to work part-time are more likely to work part-time in the short-run after childbirth, indicating that the law is effective in granting access to part-time employment to those mothers who want it. While the probability to return to work after childbirth is unaffected, the law has a positive effect on maternal employment and labor income in the long-run. The results suggest that the increase in the employment rate is due to a lower probability to drop out of the labor market after the temporary return and a lower probability to give birth to an additional child.

*Keywords:* Female Employment, Part-Time Work, Fertility, Family and Work Obligations

*JEL classification:* J13, J18, J22, J83

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# 1 Introduction

Part-time work regulations differ widely across countries (OECD, 2010). In recent decades, many OECD countries have passed laws to facilitate the transition between part-time and full-time employment. In the early 2000s, the Netherlands and Germany, among others, extended the legal rights of workers to demand part-time work and reduced the grounds for refusal of the employer. In countries like Spain (1999), the United Kingdom (2003), Austria (2004), and Australia (2010), these rights were introduced for parents of young children. The perception is that the right to work part-time may help mothers reconcile work and care of children. Given that many developed countries are facing low fertility rates or low maternal labor force participation rates (or both), addressing the family-work-compatibility is a key issue for economic development and the financial sustainability of social security systems. Despite the strong expansion of rights to work part-time, there is little research that investigates the economic consequences. Especially, the impact on maternal employment and fertility is not well understood so far. As the OECD (2010) points out “governments [...] have sought to promote it [part-time work] as a way to mobilise into the labour market groups with traditionally low labour market participation, such as women with young children [...]”. Mothers thus represent an important target group of the policy.

The purpose of this paper is to study how the statutory right to work part-time impacts labor market outcomes and higher-order fertility of women after childbirth, both in the short-run and in the long-run. To study these questions empirically, I focus on a legal change in Germany. On January 1, 2001, a law was implemented that granted employees the right to work part-time (Law on Part-Time Work and Fixed-Term Employment Contracts, §8 TzBfG). The pre-condition to request part-time work according to the law was that the employer usually employed more than 15 employees and that the worker had at least six months of tenure. The employer was only allowed to refuse the request based on business grounds. A reform of the parental leave legislation at the same date made sure that also parents on parental leave were entitled to work part-time during

parental leave under the conditions mentioned above.

A priori, the introduction of the right to work part-time could either increase the post-birth labor supply of mothers (at the extensive margin) or it could decrease it (at the intensive margin). Consider a working mother who finds it optimal to work part-time after childbirth. In the absence of part-time work options, she may either choose to work full-time, which limits the time with her children, or she may choose to stay at home with her children and to drop out of the labor force. Institutions that affect the settings of maternal employment may potentially also spill over to fertility (Lalive and Zweimüller, 2009). This, in turn, may shape long-run female labor market outcomes as subsequent births may have further effects on mothers' labor supply. Finally, the right to work part-time could affect maternal post-birth labor income through various channels: Part-time employment may be associated with fewer opportunities for career advancement (see e.g. Manning and Petrongolo, 2008 and Goldin, 2014). If the law increases the probability to work part-time, this could have a detrimental effect on wages. However, if the entitlement to work part-time leads to an earlier return to work after childbirth, this may affect women's labor market attachment and, thus, wages positively. The law could also have a positive effect on wages, if it makes firm switches or occupational downgrading associated with the transition from full-time to part-time work less likely (Connolly and Gregory, 2008), so that firm-specific and occupation-specific human capital is retained.

Analyzing the causal effect of the right to work part-time on mothers' employment, income, and fertility poses two main empirical challenges. First, the policy change may be endogenous to general trends in social norms. Olivetti and Petrongolo (2017) point out that e.g. a societal change towards more gender equality may both increase female labor force participation and induce the passage of family friendly legislation. It is, thus, important not to attribute the whole increase in female employment to the change in legislation, but to account for the evolution of social norms. Second, one has to account for potential contaminating events that occurred at the same time as the change in part-time regulation.

To deal with these challenges, I use a differences-in-differences approach. I study the effect of the legal claim to work part-time on mothers' return to work, employment, labor income, and higher-order fertility up to six years after childbirth comparing maternal outcomes before the reform to those after the reform. As only mothers working for employers with more than 15 employees became eligible, I use mothers working in establishments with at most 15 employees prior to giving birth as a control group. This allows me to control for general trends in social norms if they affect women working in small and large firms in the same way. It also allows me to control for other potential policy changes taking place in Germany at the same time and applying to all mothers independently of the firm size.

I rely on longitudinal administrative data from the German social security records. This data set includes complete individual employment histories for a large sample of employees and has precise information on employment, earnings, and job-related characteristics like e.g. the industry and establishment size.

The results show that the legal change did not have a significant impact on the probability to return to work of mothers after childbirth. Although I do not find evidence that mothers with the right to work part-time were more likely to return to the labor market after childbirth, they were more likely to work part-time in the short-run. This indicates that the short-term increase in the part-time variable was triggered by mothers who would have returned to full-time work in the absence of the reform rather than by mothers who would have stayed out of the labor market. The findings suggest that the law relaxed a binding constraint, i.e. the law seems to have been effective in granting access to part-time employment to those mothers who wanted it. The part-time status was not affected by the reform in the long-run as the entitled mothers were more likely to upgrade their work-schedule from part-time to full-time work later on.

In the longer-run, the law had a positive effect on the maternal employment probability. Six years after childbirth eligible mothers were about 4 percentage points more likely to be employed (9.5%). The reform increased the time a mother was employed after birth through the first six years

by about 2 months. Where did the positive employment effects come from? Given that I do not find evidence for a significant change in the return to work probability caused by the law, the increase in the employment rate must have been driven by a lower number of entitled mothers dropping out of the labor market again after some time. Specifically, my results suggest that dropping out of the labor market was less common in the group of mothers with the right to work part-time because of a lower probability to give birth to an additional child. The reform decreased the probability to give birth to an additional child during six years by about 4 percentage points (15%) in the group of mothers who returned to work after giving birth. These findings are e.g. compatible with the interpretation that mothers with the right to work part-time have a lower disutility from working and are, therefore, less likely to drop out of the labor market as compared to mothers without the right to work part-time. In line with this interpretation, I find a positive effect on earnings and a higher job continuity after childbirth for mothers with the right to work part-time.

Analyzing the heterogeneity of the effects, I find that the reform effects tend to be stronger in jobs where part-time work is less prevalent traditionally. Namely, the effects on maternal labor market outcomes tend to be stronger for mothers with high pre-birth earnings and mothers working in industries with a low part-time share.

I do not find evidence for a systematic selection into the group of mothers who are entitled to work part-time, nor do I find evidence for the existence of different pre-trends in the treatment and the control group that could drive the results. This is supported by a Placebo check with a pretended Placebo reform during the pre-reform period using pre-reform data only. My results are robust to various sample specifications. Especially, results do not change if I restrict the treatment group to mothers working in either small or medium sized firms only (i.e. dropping those working in large firms) to make them potentially more comparable to mothers in the control group. Results are among others also robust to changes in the definition of the observation period, the exclusion of particular industries or when focusing on West Germany only.

In terms of policy implications, my findings suggest that the reform helped to boost maternal

labor supply in the longer-run. However, there is suggestive evidence that the law might have had a potentially unintended negative impact on higher-order fertility as mothers who returned to work after giving birth either reduced or postponed additional births. This could potentially be of concern given below replacement fertility levels in Germany, just as in many other industrialized countries (see e.g. [Doepke and Kindermann, 2019](#)). It is important to keep in mind, though, that I cannot measure the effect of the reform on completed fertility.

The paper contributes to the literature that assesses the impact on maternal labor outcomes of policies designed to help families reconcile work and family life. Most of these studies analyze the effect of parental leave regulations (see for instance [Ruhm, 1998](#), [Waldfogel, 1998](#), [Albrecht et al., 1999](#), [Baum, 2003](#), [Baker and Milligan, 2008](#), [Lalive and Zweimüller, 2009](#), [Lalive et al., 2013](#), [Schönberg and Ludsteck, 2014](#) and [Ginja et al., 2018](#)). I contribute to this literature by studying the impact of a different family friendly policy: the right to work part-time. In general, parental leave entitlements qualify parents not to work for a certain period after childbirth during which their job is protected. The legal change that I am considering gave parents the right to work part-time both during parental leave and afterwards.

Evidence on the effects of the statutory right to work part-time on labor market outcomes is still scarce. Moreover, the literature so far either studies the effect on the overall group of employees or on women independent of their motherhood status. [Fouarge and Baaijens \(2009\)](#) and [Munz \(2004\)](#) do not find evidence that the legal claim to work part-time affects the probability that employees adjust their working hours without changing the job. Using country-level data from 17 OECD countries, [Blau and Kahn \(2013\)](#) find that the right to work part-time is associated with an increase in female labor force participation and a closing of the gender wage gap. [Fernández-Kranz et al. \(2017\)](#) study the (unintended) consequences of a Spanish reform that gave parents the right to work part-time. The authors show that employers changed their hiring, firing, and promotion of women in childbearing age as compared to men of similar age. I contribute to this literature by specifically focusing on the group of mothers, an important target group of this kind of policy. [Fernández-](#)

Kranz et al. (2017) provide evidence for the indirect effects of such policies by answering the question how employers changed their treatment of women in childbearing age in response to the legal change. I study a different question, namely whether mothers actually made use of the new right and how this affected maternal long-run labor market outcomes. I, thus, provide evidence on the direct labor market effects of the right to work part-time.

Finally, this paper contributes to the small literature studying the relationship between part-time work and fertility. In the context of a structural life-cycle model calibrated to Sweden, Laun and Wallenius (2017) study the impact of different policy experiments on female employment, fertility and the gender wage gap. They show that the complete elimination of the possibility to work part-time leads to a drop in maternal employment and to a small decline in the fertility rate. Del Boca (2002) finds a slight positive relationship between the regional availability of part-time jobs in Italy and the probability of having a child. Ariza et al. (2005) find evidence of a positive relationship between working part-time and fertility only for five of the eleven countries included in their analysis. I add to this literature by applying a tighter identification strategy. By exploiting a natural experiment that entails exogenous variation in the right to work part-time I provide first causal evidence how this right affects fertility. Moreover, in contrast to the existing studies my focus is on the impact on higher-order fertility.

The paper is organized as follows. Section 2 describes the institutional setup and provides some theoretical considerations. Next, Section 3 describes the data and outlines the empirical strategy. Section 4 presents results and Section 5 concludes.



## 2 Institutional Background and Mechanisms

### 2.1 Institutional Background

#### Part-Time Employment

The incidence of part-time work has traditionally been relatively high in Germany, especially among women. In 2000, the year prior to the reform, 18% of all employees worked part-time.<sup>1</sup> In the group of employed women, the share of part-time workers amounted to 39% in 2000. The incidence of part-time work increases with motherhood in Germany. 51% of all employed women with one child worked part-time in Germany in 2000 and 68% of mothers of two or more children. Overall, 58% of all working mothers had a part-time job.<sup>2</sup> The share of part-time working mothers further rose in the subsequent years. It increased from 58% in 2000 to 66% in 2014. Also, female labor force participation has increased steadily over the last decades in Germany. While 68% of all women aged 15 to 64 were part of the labor force in 2000, female labor force participation increased to 79% in 2014.

How many hours does a woman typically work in a full-time and a part-time job in Germany? In 2000, the average number of actual hours worked of female part-time employees in Germany amounted to 23 hours per week with a standard deviation of 8.5. In comparison, female full-time employees worked on average 42 hours per week with a standard deviation of 7.3. Overall, the average number of actual hours worked per employed woman was 35 hours per week with a standard deviation of 12 in 2000. In 2014, women in part-time jobs worked on average 25 hours per week with a standard deviation of 8.6, while the average numbers of hours worked per week of a female full-time worker declined only slightly to 41.

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<sup>1</sup>The numbers in this section are computed based on the German Socio-Economic Panel. The part-time status in the German Socio-Economic Panel is self-reported.

<sup>2</sup>The numbers refer to mothers of children younger than 16 that are living in the same household.

## **Legal Change on January 1, 2001**

On January 1, 2001 the Law on Part-Time Work and Fixed-Term Employment Contracts (TzBfG) came into force, which had been passed by the German government on December 21, 2000. With §8 TzBfG, a general entitlement of employees to work part-time was introduced for the first time in Germany. §8 TzBfG granted employees the right to reduce their contractual working hours if certain pre-conditions were met. Namely, the employee had to have at least six months of tenure in a firm that usually employs more than 15 employees (excluding trainees). The request to reduce working hours, including the desired number of working hours, required 3 months' notice and could be made after the waiting time of 6 months was completed. The employer could only refuse the request to work part-time based on business grounds, e.g. if the reduction in working time significantly impaired the organization, operation or safety in the firm or caused unreasonably high costs. The worker could also choose the work-shifts (i.e. how to distribute the working hours over the week), which the employer could only deny based on business grounds. Based on the Law on Part-Time Work and Fixed-Term Employment Contracts, workers did not have the right to return to full-time employment. However, in case of a vacant full-time position, the employer had to give priority to a part-time employee who had notified the employer that he would like to expand his contractual working hours, if candidates had equal qualifications.

A reform of the parental leave legislation on January 1, 2001 made sure that also parents on parental leave were entitled to work part-time (§15 BErzGG) under the pre-conditions that the employer usually employed at least 15 employees, the worker had at least six months of tenure, and the baby was born on or after January 1, 2001. During parental leave, the worker had the right to reduce the working hours to 15 to 30 hours per week for at least three months and the request required 8 weeks notice, which the employer could only deny based on serious business grounds.

How did the parental leave legislation differ from the general right to work part-time? Under which circumstances was it beneficial for parents to make use of one law or the other? First,

parental leave regulation in Germany provided job protection for up to three years after childbirth (of which up to twelve months could be delayed until the child reached the age of eight). However, parental cash benefits were at most paid during the first two years.<sup>3</sup> If parents wanted to make use of the job protection period, but e.g. faced a binding budget constraint after cash benefits had ended, they could make use of the parental leave legislation and choose to work part-time during parental leave.

Second, whether it was optimal to work part-time during parental leave or to make use of the general right to work part-time, depended on whether a mother wanted to work part-time temporarily or permanently. In general, after parental leave had ended, parents had to return to their pre-birth work schedule. This means, in contrast to the Law on Part-Time Work and Fixed-Term Employment Contracts (§8 TzBfG), the parental leave legislation entitled employees to a temporary reduction in working hours (i.e. parents who worked part-time during parental leave returned to full-time work, if they were full-time workers prior to childbirth). However, parents could also combine the two laws and request part-time work based on the Law on Part-Time Work and Fixed-Term Employment Contracts, if they wanted to keep the reduced work-schedule also after parental leave had ended. Third, while part-time requests could be denied based on business grounds in the case of the general part-time legislation, it was harder to refuse part-time work requests during parental leave. Employers only had the right deny requests based on serious business grounds.

While the part-time legislation provided the above mentioned advantages, it was more restrictive in terms of working hours than the Law on Part-Time Work and Fixed-Term Employment

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<sup>3</sup>In Germany, mothers are not allowed to work eight weeks after childbirth. During this period, they receive their full net labor income. Afterwards, parents on parental leave could receive parental cash benefits if their working hours did not exceed a certain threshold (19 hours a week before January 2001, 30 hours afterwards) and the annual net family income was not too high. In 2001, the income limits were 50,000 (37,500) Euros in two-parent households (single-parent households) during the first six months after birth and 16,100 (13,200) Euros afterwards. The threshold increased by 2,400 Euros for each additional child. Before January 1, 2001, parental cash benefits for eligible parents amounted to 300 Euros for 24 months. Starting from January 1, 2001 eligible parents could choose between 450 Euros for 12 months or 300 Euros for 24 months. If the annual net family income exceeded the threshold, the amount of 450 Euros was reduced by  $0.062 * (income - threshold)$  and the amount of 300 Euros was reduced by  $0.042 * (income - threshold)$ .

Contracts. While working hours had to be between 15 and 30 in case of the parental leave legislation, employees were free to pick any number of working hours in case of the general part-time law. Finally, based on §8 TzBfG (Law on Part-Time Work and Fixed-Term Employment Contracts) all employees who fulfilled the requirements mentioned above were eligible to work part-time after January 1, 2001, i.e. only the timing of observation mattered and eligibility was independent of the timing of birth. In contrast, according to §15 BErzGG, mothers were only able to make use of the right to work part-time during parental leave after January 1, 2001, if their child was born on or after January 1, 2001.

## 2.2 Mechanisms and Outcomes

How does the right to work part-time affect labor market behavior and subsequent childbirths of working mothers? A priori, it is not clear whether the introduction of the right to work part-time increases the post-birth labor supply of mothers or decreases it. [Fernández-Kranz et al. \(2017\)](#) argue that it should lead to an increase in maternal employment, because mothers who want to spend more time with their children than possible with a full-time job can choose part-time work instead of becoming inactive. This would mean that mothers who are eligible to work part-time should be observed to return to work earlier than their counterparts and their employment rate should be higher in the short-run. We would thus expect a positive effect on the extensive margin of labor supply. At the same time, the right to work part-time could decrease the labor supply of mothers at the intensive margin, i.e. mothers could be more likely to work part-time rather than full-time. To check these hypotheses, I will study the probability of having returned to the labor market by month  $t$  after childbirth and the probability of working part-time  $t$  months after childbirth as outcome variables.

Among others, [Lalive and Zweimüller \(2009\)](#) show that institutions that change the terms of maternal employment may also significantly affect fertility, which, in turn, will shape long-run female labor market outcomes. Therefore, I also consider the probability of giving birth to an ad-

ditional child by month  $t$  after the previous childbirth as an outcome variable. If women who have returned to work after childbirth drop out of the labor force again after some time, e.g. to give birth to an additional child, this would not be captured by the return to work variable. Therefore, I study the probability of being employed  $t$  months after childbirth. This variable also captures temporary returns to the labor market. The variable “overall number of days worked since childbirth until month  $t$  after childbirth” captures the overall reform effect on maternal employment.

Finally, the right to work part-time could affect maternal post-birth labor income through various channels. On the one hand, part-time work may be detrimental for career advancement. Results by [Manning and Petrongolo \(2008\)](#) suggest that part-time workers are e.g. less likely to get promoted. [Goldin \(2014\)](#) finds that the desire for time flexibility has a negative impact on earnings especially in the corporate, financial, and legal sector. E.g. for lawyers the hourly fees decline when switching from working full-time to part-time. If women are more likely to work part-time after the introduction of the law, this could thus have a negative effect on wages. On the other hand, the entitlement to work part-time could also affect labor earnings positively. If the legal claim to work part-time leads to an earlier return to work after childbirth, this may affect women’s labor market attachment and human capital accumulation positively and could thus have a positive impact on earnings. Moreover, there is empirical evidence that in a setting without the right to work part-time transitions from full-time to part-time employment are often associated with a change in employer ([Fernández-Kranz et al., 2013](#)) and occupational downgrading for women ([Connolly and Gregory, 2008](#)). Women with the statutory right to work part-time who want to reduce working hours can do so without changing occupations, which could affect wages positively as firm-specific and occupation-specific human capital can be retained. In addition, there are channels that have an ambiguous effect on labor income, a priori. First, the reform may affect earnings through a changed selection of mothers into work. Second, a change in fertility is likely to entail changes in earnings. Based on these considerations, I will study the effect on mothers labor earnings  $t$  months after childbirth.

## 3 Empirical Strategy and Data

### 3.1 Data

I use administrative data from the German social security records provided by the Institute for Labor Market Research (IAB). This data set consists of a 2% random sample (1,757,925 individuals) drawn from the social security records of 1975 to 2014. It allows to construct complete individual employment histories for employees who are covered by the social security system, i.e. it excludes civil servants and self-employed. The data set has precise information on employment, earnings, and job-related characteristics like e.g. the establishment size and the industry.

The data set reports whether an employee works full-time or part-time, but does not include information on actual hours worked. The full-time or part-time status of an individual depends on the contractual working hours. Individuals whose contractual working time is lower than the standard working time stipulated in the collective agreement or company agreement are defined as working part-time. As marginal employment is only covered by the data set since 1999, I do not consider it in my analysis<sup>4</sup>.

While the German administrative data has the advantage of allowing me to study the impact of the natural experiment provided by the German part-time law, it entails two sources of measurement error for my analysis. First, the data set reports the establishment size, while the right to work part-time according to the law is based on the employer size. These numbers do not necessarily coincide. In the data, branches located in the same municipality and with the same economic activity are treated as one unit. However, if branches are located in different municipalities or if they are active in different economic areas, it is not possible to recognize that they belong to the same firm. Employees working for small firms with at most 15 employees will be categorized correctly

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<sup>4</sup>In Germany, the term marginal employment refers to a job in which the labor income does not exceed a maximum amount defined by the law (325 Euros per month in 2001) and that is at least partly exempt from social security contributions.

to work for a small firm, and thus, as being part of the control group. Moreover, if branches or establishments have more than 15 employees, they will also be correctly classified as being part of the treatment group as it is clear that they work for a large firm. However, if establishments have at most 15 employees, while the firm they belong to has more than 15 employees I may falsely categorize women working in these kinds of establishments as being part of the control group, while they may actually be part of the treatment group. This will induce a bias of my estimates towards zero. In Section 4.4, I will study the sensitivity of my results by excluding industries where small branches are likely to be most prevalent.

Second, the data set does not provide direct information on children and the timing of birth. However, I observe employment gaps during which the woman receives replacement benefits. These gaps could either be due to giving birth or because of absence from work due to sickness or disability. The data set has frequently been used to analyze questions related to parental leave in Germany (see e.g. [Dustmann and Schönberg, 2012](#), [Schönberg and Ludsteck, 2014](#), and [Welteke and Wrohlich, 2019](#)). Using an earlier version of the data set, [Schönberg \(2009\)](#) demonstrates that it is possible to reliably identify childbirths if the appropriate sample restrictions are imposed.<sup>5</sup> I follow [Müller et al. \(2017\)](#) to identify childbirths. To capture women in their childbearing age, I restrict the sample to women between age 16 and age 40, and younger than 38 for their first (observed) childbirth. I only consider employment interruptions of at least 14 weeks which corresponds to the obligatory maternity leave period of 6 weeks before and 8 weeks after childbirth. Finally, I impose the gap between two consecutive births to be at least 32 weeks. I follow [Schönberg and Ludsteck \(2014\)](#) and compute the birthday of the child to be six weeks after the women went on leave (as the maternity leave in Germany starts 6 weeks before the due date).

I construct the following outcome variables: The return to work-variable is equal to one for

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<sup>5</sup>[Schönberg \(2009\)](#) checks the likelihood of correctly identifying leave spells as maternity leave by linking them to childbirths in the Pension Register. If the sample is restricted to women between age 18 and 40 who are on leave for more than 2 months, 84 % of all leave spells of West Germans are due to childbirths. 12 % of all true maternity leave spells of West Germans are deleted if these restrictions are imposed.

mothers whose employment after childbirth lasts for at least 2 consecutive months, and zero otherwise<sup>6</sup>. As for the employment probability, I construct an indicator variable which is equal to one if the mother has returned to the labor market and is employed  $t$  months after giving birth. I set it to zero otherwise. The variable “Days Worked” adds up the days of all employment spells of the mother since childbirth until month  $t$  after childbirth. The part-time status is equal to one for mothers who returned to the labor market and are working part-time  $t$  months after childbirth. It is equal to zero otherwise. I measure labor income at a daily frequency, and I set it equal to zero if a woman is not working at the respective day to take potential changes in the selection into employment into consideration. I deflate labor income by the consumer price index, where I use 2005 as the base year. To identify higher-order births, I proceed in the same way as for the identification of the main childbirth described above. However, this implies that I can only observe these additional childbirths if a woman has returned to the labor market between the two births. As for the control variables in order to improve the quality of the education information in the data set, I follow the imputation procedure proposed in [Thomsen et al. \(2018\)](#).

## 3.2 Empirical Strategy

To evaluate the effect of the reform on maternal labor market outcomes and fertility, I use a differences-in-differences design. In the main specification, I compare women giving birth prior to the reform (between January 1, 1994 and December 31, 2000) to those giving birth after the reform (between January 1, 2001 and December 31, 2001). I only use the short post-reform period to minimize the likelihood of potential endogenous reactions of women to the law. I use mothers working in establishments with at most 15 employees as a control group, as employees of firms with at most 15 employees did not gain the right to work part-time based on the reform. Mothers working for establishments with more than 15 employees constitute the treatment group.

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<sup>6</sup>This definition has e.g. also been used in [Schönberg and Ludsteck \(2014\)](#).



I estimate the following regression:

$$Y_{it} = \alpha_{0t} + Year_i' \alpha_{1t} + \alpha_{2t} Treat_i + \alpha_{3t} Post_i * Treat_i + X_i' \alpha_{4t} + u_{it} \quad (1)$$

where  $Y_{it}$  is the labor market outcome or higher-order fertility of mothers  $t$  months after childbirth  $i$ .  $Year_i$  are childbirth year-fixed effects.  $Treat_i$  is equal to one if the mother was employed in an establishment with more than 15 employees prior to birth  $i$  and zero otherwise.  $Post_i$  is a dummy variable that is equal to one if child  $i$  was born before the reform (between January 1, 1994 and December 31, 2000), and zero for childbirths between January 1, 2001 and December 31, 2001. For the post-reform sample, I assign the treatment status based on the establishment size on the reform date. If the person was not working on this particular date I use the establishment size in her last job in 2000. Finally,  $X_i'$  is a vector of mothers' characteristics determined prior to childbirth  $i$ , namely age, and age squared, dummies for ISCED-education levels, log pre-birth earnings, pre-birth full-time status, and one-digit industry fixed effects. Again, for the post-sample, the characteristics at the reform date are used rather than those directly before going on maternity leave.  $\alpha_{3t}$  is the parameter of interest.

I run the regression separately by time since childbirth. I define the pre-reform sample in every regression in a way to make sure that the time period since childbirth only covers the pre-reform period, i.e. I only include women that gave birth prior to the reform and that I also observe prior to the reform. E.g. if the outcome variable is return to work 24 months after childbirth, I only include mothers in the pre-reform sample who gave birth on January 1, 1999 or earlier (see Figure A.1) and check whether they have returned to work 24 months after childbirth. I use this strategy because eligibility according to the Law on Part-Time Work and Fixed-Term Employment Contracts (TzBfG) was not assigned based on the timing of birth, but depended on the time of observation. Thus, also women giving birth before the reform gained the right to work part-time on January 1, 2001 if all pre-conditions were met. For the "Additional Births" outcome variable,

Table 1: Summary Statistics: Pre-Birth Characteristics

Variable	Full Sample	Treated	Control
Age (Years)	29.61 (4.61)	29.89 (4.57)	28.94 (4.64)
Full-Time (0/1)	0.78 (0.41)	0.77 (0.42)	0.81 (0.39)
Education (1/5)	3.86 (0.73)	3.85 (0.76)	3.90 (0.64)
Log Daily Earnings (Euros)	4.07 (0.61)	4.16 (0.59)	3.84 (0.59)
Firm Tenure (Years)	4.64 (3.86)	4.92 (3.98)	3.96 (3.46)
Labor Market Experience (Years)	6.91 (4.23)	7.12 (4.27)	6.39 (4.10)
No. of Observations	50,891	36,062	14,829

The table reports the mean and the standard deviation of each pre-birth characteristic for the full sample, the treatment group and the control group. Standard deviations are in parentheses.

this strategy implies that for women who gave birth prior to the reform, I only consider additional children born before January 1, 2001, which means that the right to work part-time neither applied to the first child nor to higher-order births. For treated mothers who gave birth after the reform, the right to work part-time automatically also applied to additional children as the law came into force on January 1, 2001 and stayed effective afterwards. Table 1 shows summary statistics on the pre-birth characteristics of mothers for the full sample, the treatment group, and the control group. Overall, the sample includes 50,891 births.

### Parallel Trends Assumption

The differences-in-differences approach is based on the identifying assumption that in the absence of the policy change trends in the labor market outcomes of mothers in the treatment and the control group would have been the same. To check the plausibility of this assumption, I run event study regressions on the part-time employment status separately by time since childbirth exploiting my data set at the half-annual level. Reformulating equation (1), I interact  $Treat_i$  with half-year dummies, and exclude the last pre-reform period, implying that all effects are relative to this period.

I run the following regressions:

$$PT_{itp} = \alpha_{0t} + \alpha_{1t}Treat_i + \lambda_{itp} + \sum_{p=1}^P \gamma_{tp}Treat_i * Pre_p + \sum_{p=15}^{16} \gamma_{tp}Treat_i * Post_p + X_i' \alpha_{4t} + u_{itp} \quad (2)$$

$PT_{itp}$  is the part-time status  $t$  months after childbirth of mother of birth  $i$ , who has given birth in period  $p$ .  $\lambda_{itp}$  are half-year time fixed-effects. The coefficients of the time period-treatment interaction terms in the pre-reform periods  $\sum_{p=1}^P Treat_i * Pre_p$  indicate whether the treatment and the control group faced parallel pre-trends,<sup>7</sup> whereas the coefficients of the interaction terms with the post-reform-period dummies capture the reform effect. I use the last pre-reform period as excluded baseline category. As depicted in Figure 1, the graphical evidence largely supports the parallel trends assumption for the pre-reform period. The estimates fluctuate around zero and most of them are not significantly different from zero. In contrast, the estimates for the year 2001 are always positive and it seems that the reform effect has picked up especially in the second half of 2001.

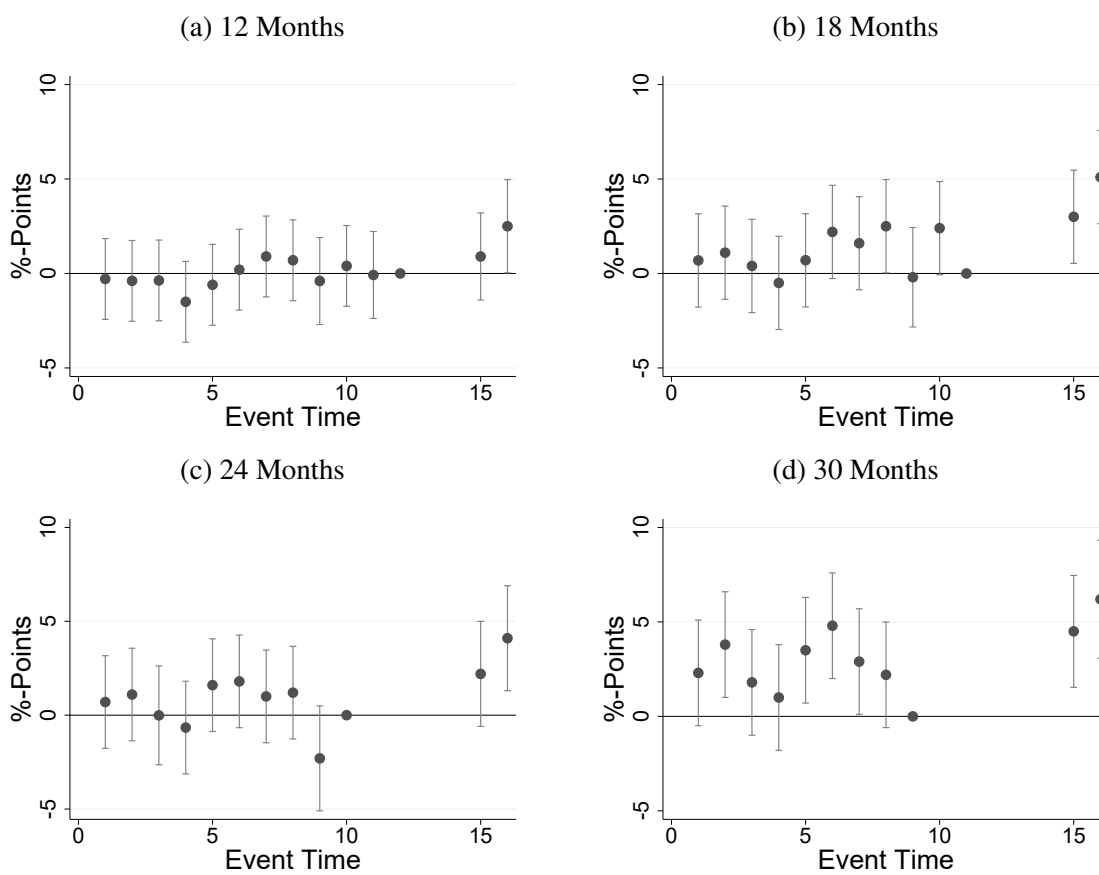
## Selection

There are two potential threats to the validity of the design. First, women may switch the employer in response to the reform to become eligible to work part-time, i.e. they may switch from small firms to firms with more than 15 employees. The concern is partially addressed by the fact that I group mothers into the treatment and the control group based on the establishment size on or before the reform date. Moreover, I only include mothers who give birth in 2001 in my post-reform sample, which makes these switches less likely. Searching and switching to a new job usually takes some time, employment contracts in Germany can usually only be terminated after a period of notice of at least three months, and most of the women who give birth in 2001 are already pregnant at the reform date. While these facts make a self-selection into large firms triggered by the reform in

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<sup>7</sup>The exact pre-reform time periods  $P$  included in the regression will differ by the time since childbirth. It is e.g. equal to 11 in the regression 12 months after childbirth.

Figure 1: Event Study: Part-Time Status by Time Since Childbirth



The graphs plot the coefficients of the time period-treatment interaction terms in equation (2). The bars represent 90% confidence intervals.

Table 2: Transition Matrix

	$Treat_i^{birth} = 0$	$Treat_i^{birth} = 1$
$Treat_i^{sep} = 0$	24.90	1.67
$Treat_i^{sep} = 1$	1.48	71.94

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

my sample less likely, I cannot exclude the possibility. However, it may also be the case that women have anticipated the reform and may have already changed the employer before the actual policy change took place. I check two large German newspapers for articles about the reform.<sup>8</sup> The first articles discussing the bill were published mid-September 2000. Therefore, to assess the presence of such strategic firm changes, I compute transition probabilities between small establishments and large establishments between September 12, 2000 (date of the first newspaper article) and the birth date of the child. Table 2 shows the likelihood of belonging to either the treatment or the control group at the time of birth ( $Treat_i^{birth}$ ) versus the treatment status in September 2000 ( $Treat_i^{sep}$ ). Only 1.7% of all mothers in the post-sample switched from an establishment with at most 15 employees in September 2000 to a large establishment with more than 15 employees before giving birth. Moreover, the symmetry of movements between small and large establishments is very high. This alleviates the concern that mothers may have strategically responded to the reform by switching the firm.

A second concern to the validity of the identification strategy would be present, if women altered their plans to have children based on eligibility. My analysis is based on the sample of mothers. The estimations could be biased if women self-selected into motherhood based on the

<sup>8</sup>I check the archives of “Süddeutsche Zeitung” and “Frankfurter Allgemeine”.

treatment status, i.e. based on working for a small or a large firm. As I only consider births in the year 2001 for the post-reform period, most of the women were already pregnant at the reform date. Moreover, in section 4.4 I do a robustness check, where I restrict the post reform period to the first 40 weeks in 2001 only (corresponding to average length of gestation) to further increase the likelihood that the women in my post-reform sample were already pregnant at the date of the reform and the results hardly change. To further assess the importance of this concern, I consider the evolution of the relative number of births in the treatment and in the control group (relative to the number of women in childbearing age in large and small establishments) before and after the reform.<sup>9</sup> For this purpose, I construct a sample consisting of all women who are employed in 2000 or 2001 and who are between 16 and 40 years old independent of motherhood. I construct the data set at the monthly level. The data set consists of about 3,035,000 observations. I estimate the following regression:

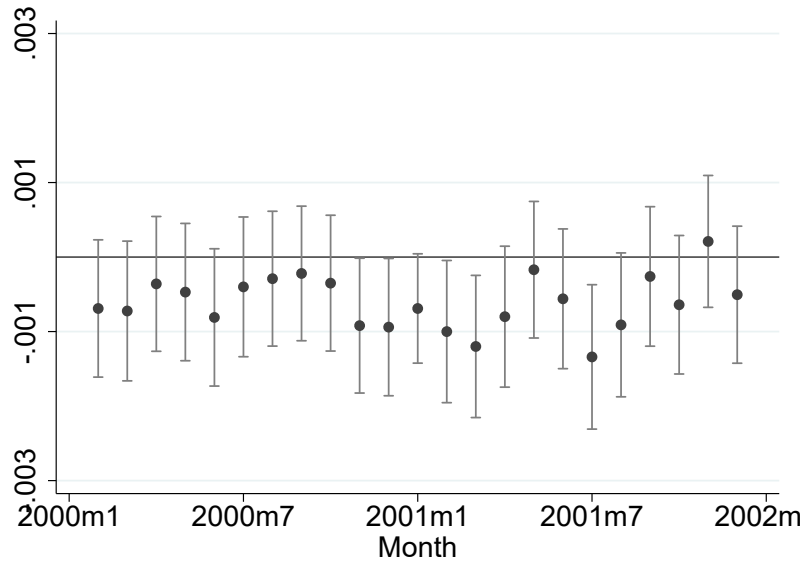
$$Birth_{it} = \alpha_0 + \alpha_1 Treat_i + \sum_{t=1}^{23} \delta_t Month_t + \sum_{t=1}^{23} \gamma_t Treat_i * Month_t + u_{it} \quad (3)$$

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

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<sup>9</sup>See Appendix Figure A.2 for the evolution of the absolute number of births in the treatment and control group in the years 2000 and 2001. Comparing the absolute number of births in the treatment and the control group over time, I do not find evidence for a spike in the number of births in the treatment group after the reform.

Figure 2: Births in 2000 and 2001



The dots plot the coefficients of the interaction terms  $Treat_i * Month_t$  in equation (3). The bars depict 90% confidence intervals. I estimate a linear probability model. The dependent variable is a dummy, which is equal to one if woman  $i$  gave birth in month  $t$  and zero otherwise. Standard errors are clustered at the individual level.

July 2001 and December 2001.

To further address the concerns of systematic sample selection, I compare mothers giving birth before and after the reform, in the treatment and in the control group in terms of their pre-determined observable characteristics. If these pre-determined characteristics are significantly different, this could also suggest some systematic selection into treatment. Table 3 presents the results from estimating differences-in-differences regressions based on equation (1) without any controls, using the different pre-birth characteristics as dependent variable. Table 3 suggests that differences are small and they are only significant at the 5% level in the case of the earnings.

Finally, in Section 4, I report estimation results with and without controls. As the estimated coefficients in specifications with and without controls are very similar, this is also suggestive evidence that sample selection (at least in terms of observable characteristics) does not drive the results, and points to the exogeneity of the reform.

Table 3: Covariate Balance Test

Variable	Coefficient of interaction term	Standard Error
Age	0.146	0.148
Full-Time	-0.016	0.013
Education	-0.026	0.021
Log Daily Earnings	-0.039**	0.018
Firm Tenure	-0.19	0.12
Labor Market Experience	-0.025	0.13
Observations	50,891	

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . The table shows differences-in-differences estimates based on equation (1), but without any controls, using the different pre-birth characteristics as dependent variable. The last column shows robust standard errors.

## 4 Results

In this section, I present differences-in-differences estimation results of the right to work part-time on mothers' labor market outcomes and higher-order fertility up to six years after childbirth. Table 4 reports the main results of estimating equation (1) considering outcome variables 18, 36, and 72 months after childbirth for specifications with and without individual level control variables. Results are similar in the specifications with and without controls, therefore, I will focus on the specification with controls in the following. Figure 3 displays the evolution of the effect of the reform on the different outcome variables from 12 to up to 72 months after childbirth graphically.



## 4.1 Labor Supply

### Return to Work Probability and Part-Time Status

First, I check the hypothesis that the right to work part-time increased the probability that a mother has returned to work by month  $t$  after childbirth. The dependent variable is equal to one if a woman has returned to the labor market for at least two consecutive months by month  $t$  after childbirth, and zero otherwise. The first row of Table 4 and Panel (a) of Figure 3 show the results. While the estimated coefficients are positive (apart from month 24 after childbirth), they are small in size throughout and the effects are not significantly different from zero. So, the findings suggest that the legal change did not increase the probability to return to work of mothers after childbirth significantly.

Did women use the right to work part-time to reduce their working hours to part-time employment? The second row of Table 4 and Panel (b) of Figure 3 show the impact of the reform on the part-time status. The dependent variable is equal to 1 if the woman worked in a part-time job  $t$  months after birth, and zero otherwise. The results show that the law increased the likelihood of mothers to work part-time in the first 30 months after childbirth. Table 4 reports that mothers in the treatment group were 3 percentage points more likely to work part-time 18 months after birth after the reform. The fact that part-time work became more prevalent after the reform indicates that the law relaxed a binding constraint, i.e. the law seems to have been effective in granting access to part-time employment to those mothers who wanted it. Moreover, the joined results on return to work and part-time status suggest that the short-term increase in the part-time variable was triggered by mothers who would have returned to full-time work in the absence of the reform, rather than by mothers who would have stayed out of the labor market.

The reform did not have a significant long-run effect on the part-time status. One possible reason could be that the right to work part-time made mothers not only more likely to work part-time in the short-run, but made them also more likely to upgrade hours to a full-time job later

on. Alternatively, the effect could possibly be due to sample selection, if women in the treatment group who re-entered the labor market in year four to six after childbirth did so with a similar or even higher number of hours than those in the control group. To shed some light on this question I estimate the probability of increasing the working hours from part-time to full-time employment for the subsample of mothers who returned to the labor market with a part-time job. To do so, I use equation (1) and set the outcome variable equal to one if the mother has switched from part-time to full-time employment at least once since birth  $i$  until year six after childbirth.<sup>10</sup> The results suggest that eligible women were more likely to switch from part-time to full-time work (see Appendix Table A.1), which explains at least part of the finding that the part-time work status was unaffected in the long-run.

As described above, the baseline analysis on the part-time employment status includes both employed and non-employed individuals. Appendix Figure A.3 contrasts the baseline findings with the result if I consider employed mothers only. The results are qualitatively in line with the baseline findings, however, the standard errors increase due to the smaller sample size.

### **Employment Probability**

Next, I study the effect of the reform on the probability to be employed  $t$  months after childbirth. As depicted in row three of Table 4 and Panel (c) of Figure 3, the right to work part-time had a positive effect on the employment probability in months 48 to 72 after childbirth. Six years after birth, eligible mothers were 4.4 percentage points more likely to be employed. Evaluated against the corresponding pre-reform employment rate of mothers in the treatment group of 46%, this estimate implies an increase in the employment probability of entitled women of 9.5%. The employment probability was unaffected in the short-run. The variable “Days Worked” summarizes

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<sup>10</sup>In contrast to my general estimation procedure, I consider all mothers giving birth either in the pre- or in the post-period, no matter, whether the calendar day of observation (six years after birth) lies before or after the reform because of sample size issues. Please note, that this makes it only harder to detect a reform effect, as mothers giving birth in the pre-reform period also gained the right to work part-time on January 1, 2001, but increases the power of the estimation.

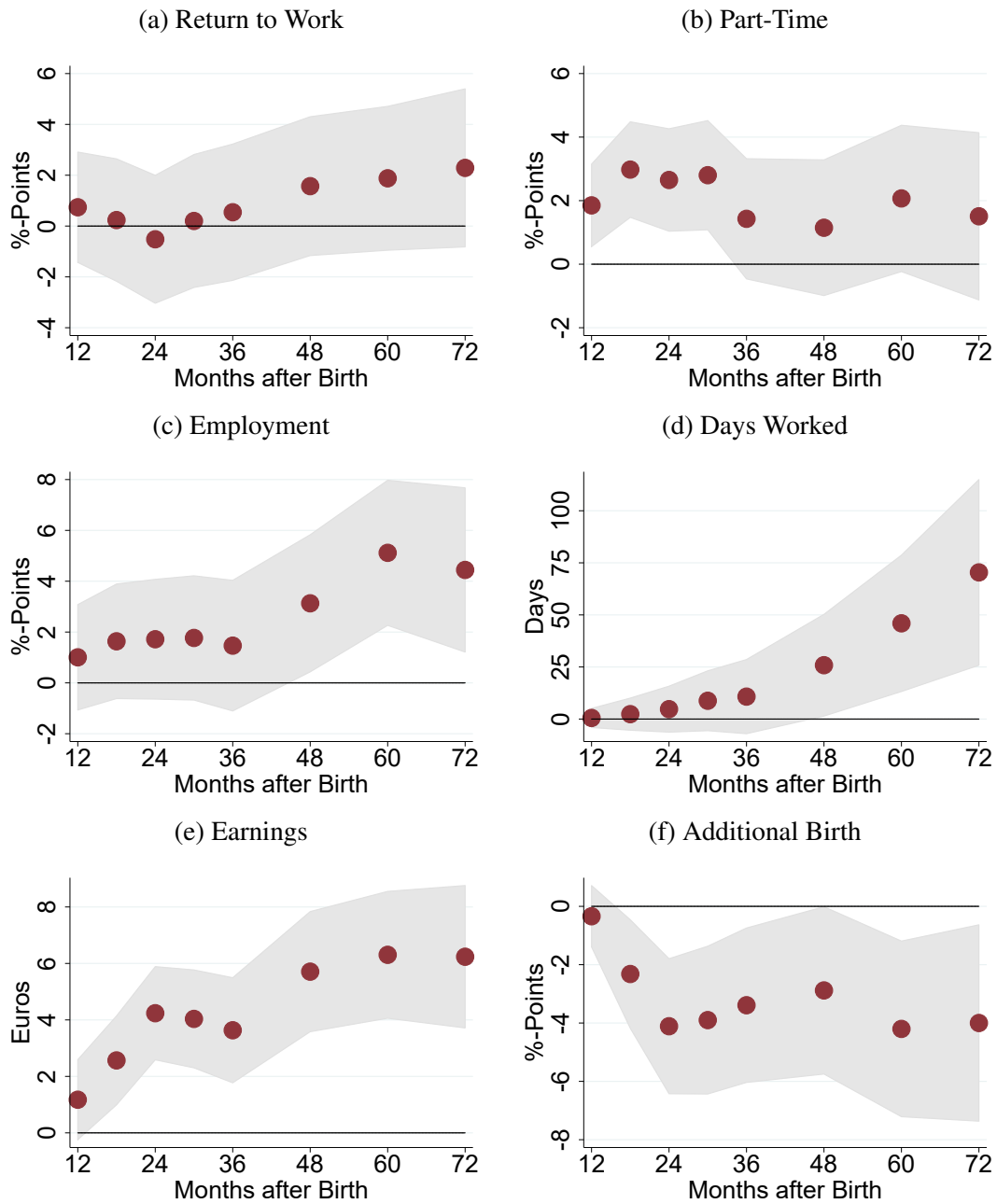
the overall effect of the reform on maternal employment (row 4 of Table 4 and Figure 3, Panel (d)). In line with the results on the employment probability, the effect of the law on the number of days worked is positive and significant in the long-run, but insignificant in the short-run. The reform increased employment through the first six years by about 2.3 months (70 days), which corresponds to an increase of about 10% in terms of the average number of days worked in the first six years after birth of the treatment group in the pre-reform period (698 days).

As described above, eligible mothers were not significantly more likely to have returned to work by month  $t$  after childbirth next. So, the increase in the employment probability was not driven by an increase in the return to work probability. If the higher employment probability was not due to a higher return to work probability, simple accounting implies that it must have been due to a lower number of women who dropped out of the labor force after their temporary return. These temporary returns are not captured by the return to work variable, but are included in the effect on the employment probability. A possible reason to drop out again after a temporary return to the labor market are additional births. The results on higher-order births discussed below are in line with the conjecture that the right to work part-time reduced the probability to drop out of the labor market after a temporary return, at least partly because of birth to an additional child.

## 4.2 Labor Market Income

Row 5 of Table 4 and Panel (e) of Figure 3 explore the effects of the right to work part-time on earnings. The dependent variable measures the average daily earnings  $t$  months after childbirth. I set them equal to zero if the woman was not employed  $t$  months after childbirth to approach the selection into employment. The statutory right to work part-time had a positive and significant effect on labor earnings of mothers after childbirth both in the short-run and in the long-run. Six years after childbirth, the daily earnings of eligible mothers were on average 6.24 Euros higher. This effect was partly driven by a higher probability to be employed. In Figure 4, Panel (a), I also plot the coefficients of the differences-in-differences estimations on the daily earnings based only

Figure 3: Labor Market Behavior and Fertility over Time



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

Table 4: Labor Market Behavior and Fertility

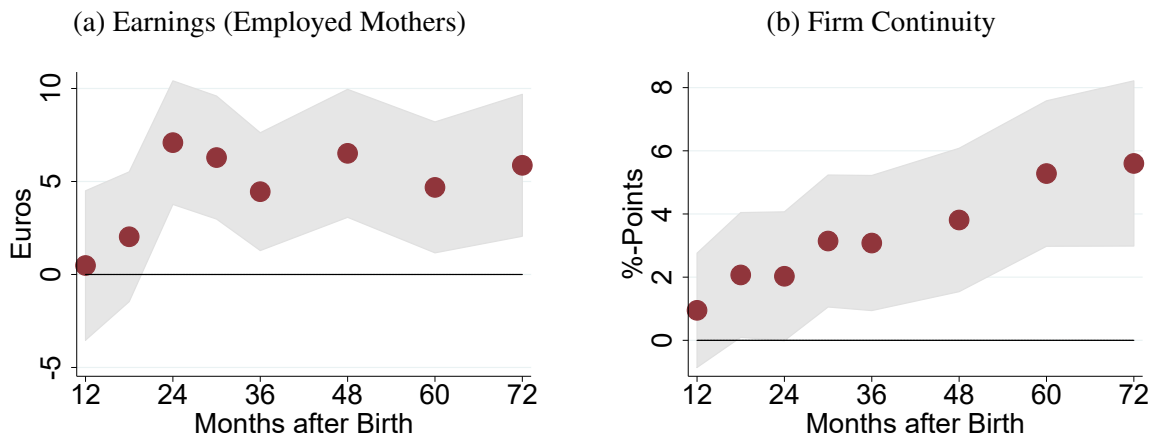
Time since Birth	18 Months		36 Months		72 Months	
	No Controls	Controls	No Controls	Controls	No Controls	Controls
Return to Work	0.008 (0.015)	0.002 (0.015)	0.011 (0.016)	0.005 (0.016)	0.026 (0.019)	0.023 (0.019)
Part-Time	0.032*** (0.009)	0.030*** (0.009)	0.019 (0.012)	0.014 (0.012)	0.020 (0.017)	0.015 (0.016)
Employment	0.020 (0.014)	0.016 (0.014)	0.017 (0.016)	0.015 (0.016)	0.046** (0.020)	0.044** (0.020)
Days Worked	4.752 (4.759)	2.353 (4.689)	15.486 (10.932)	10.768 (10.825)	81.033*** (27.460)	70.405*** (27.101)
Earnings	2.580*** (0.982)	2.566*** (0.957)	3.543*** (1.149)	3.633*** (1.131)	6.207*** (1.542)	6.237*** (1.533)
Additional Birth	-0.022** (0.011)	-0.023** (0.011)	-0.040** (0.016)	-0.034** (0.016)	-0.057** (0.021)	-0.040* (0.020)

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results are based on OLS estimations of equation (1). Robust standard errors are reported in parentheses. The “No Controls”-specification does not include any controls. In the “Controls”-specification, I control for the following characteristics of mothers determined prior to childbirth  $i$ : age, age squared, dummies for ISCED-education levels, log earnings, full-time status, and dummies for one-digit firm industry.

on the sample of women who were employed  $t$  months after childbirth. Even if I consider employed mothers only, the positive effect on labor earnings persists both in the short-run and in the long-run. As argued in Section 2, the reform could have affected labor earnings positively if mothers with the right to work part-time were less likely to experience occupational downgrading (Connolly and Gregory, 2008) or if they were less likely to change the employer (Fernández-Kranz et al., 2013) when switching from full-time to part-time work. In Figure 4, Panel (b) I plot the results of estimating equation (1) using a dummy-variable as outcome variable that is equal to one if the mother was working for the pre-birth employer  $t$  months after childbirth  $i$  and zero otherwise. The results suggest that the introduction of the right to work part-time had a small positive effect on firm continuity. While the effect is partly driven by the higher employment probability of eligible women, the effect on firm continuity is significantly different from zero also in the short-run, in contrast to the effect on the employment probability. Moreover, the coefficients are larger in the case of the firm continuity compared to the coefficients on the employment probability. Three years

after birth, the legal change increased the probability of working for the same firm by 3 percentage points, while the share of women who are employed increased by only 1.5 percentage points. Six years after birth, the reform effect on firm continuity is 5.6 percentage points and 4.4 percentage points in the case of the employment probability. The increase in employer continuity could, thus, have affected labor earnings positively through a positive effect on firm-specific human capital.

Figure 4: Labor Market Earnings (Employed) and Firm Continuity



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

### 4.3 Higher-Order Fertility

Did the right to work part-time also change the timing and the likelihood of having additional children? The data set only allows to identify further births if the woman returned to work between the two births. For this sample of mothers, I study the impact on higher-order fertility exploiting the panel structure of the data set. Results are displayed in the last row of Table 4 and in Figure 3, Panel (f). While the differences-in-differences estimate is close to zero one year after birth, it turns negative and significant from 18 months after birth onward. Six years after birth, eligible mothers were 4 percentage points less likely to have dropped out of the labor market (after their

temporary return) to give birth to an additional child. This corresponds to a 15% decrease relative to the pre-reform mean of the treatment group (26%). While the long-run effect suggests that not only the timing of births, but also the number of births might have been affected, one would have to observe the completed fertility cycle to give a final answer to this question.

The negative effect on additional births is in line with the findings on the labor market supply described above. Eligible women do not seem to have adjusted their return to work behavior. However, I find a positive effect on the likelihood to be employed. This seems to be partly due to the lower probability of dropping out of the labor market to give birth to another child.<sup>11</sup> Why could the reform have induced mothers to change their higher-order fertility? While I do not have a definite answer to this question, the findings are compatible with the interpretation that the right to work part-time increased the job satisfaction of working mothers and, therefore, reduced the likelihood to drop out of the labor market as compared to those mothers without the right to work part-time. Moreover, the fact that eligible mothers were less likely to experience occupational downgrading, indicated by the increase in employer continuity, and earned higher wages, is also an indication for a lower disutility from working of mothers with the right to work part-time. This, in turn, may have decreased the likelihood of dropping out of the labor market again quickly.

## **4.4 Robustness Checks**

### **Alternative Identification of Births**

The data set from the German social security records does not allow to directly distinguish between women who are absent from work due to a childbirth or due to long-term sickness, as described in Section 3. The main sample consists of women younger than 40 who are absent from work for at least 14 weeks (obligatory maternity leave period) in order to capture women who are on

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<sup>11</sup>Please note that it is not possible to directly compare the size of the coefficients in Panel (b) (Employment) and Panel (f) (Additional Births) of Figure 3. The estimates in Panel (b) are based on the whole sample of mothers, whereas estimates in Panel (f) are based on the sample of mothers who have returned to work after childbirth.

maternity leave. Moreover, I impose the gap between two adjacent births to be at least 32 weeks. In this section, I impose even stricter rules to further raise the probability of capturing women on maternity leave only.<sup>12</sup> First, I impose the additional restriction that women must be at most 35 years old at the time of birth to be included in the sample. As described by Meyer et al. (2018), the likelihood of long-term diseases strongly increases with age. Restricting the sample to women younger or equal to 35 years should thus decrease the number of women in the sample who are absent from work due to long-term diseases. This restriction leads to a decline in sample size of about 11%. The results are displayed in Table 5, columns (1), (4), and (7). In columns (2), (5), and (8) of Table 5, I drop mothers working in East Germany, as Schönberg (2009) suggests that the precision of birth identification is higher for West Germany. This restriction leads to decline in sample size of about 17%. Finally, the prevalence of long-term sickness spells varies by industry. Long-term sickness spells are most prevalent in agriculture, forestry and construction (see Meyer et al., 2018). Therefore, I run a further robustness check, where I drop these industries from the sample. As the fraction of women working in these areas is low, this only reduced the sample size by 2%. The results can be found in Table 5, columns (3), (6), and (9). The findings are robust to all three additional sample restrictions. While the size of the coefficients varies slightly, qualitatively, results are unchanged.

### **Difference between Firm and Establishment**

As described in Section 3, the data set only includes information on the establishment, whereas the right to work part-time is assigned based on the employer size. Mothers working in establishments with at most 15 employees, which belong to a firm with more than 15 employees in total, may be assigned to the control group by mistake, while they are actually part of the treatment group. I would expect this to bias the estimates downwards.

The importance of the distinction between firm and establishment is likely to vary across in-

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<sup>12</sup>This comes at the cost of increasing the likelihood of deleting true maternity leave spells from the sample.



Table 5: Robustness: Alternative Identification of Births

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
Time since Birth		18 months			36 months			72 months		
Sample	Age $\leq$ 35	West	Industry	Age $\leq$ 35	West	Industry	Age $\leq$ 35	West	Industry	
Return to Work	-0.002 (0.015)	-0.000 (0.015)	0.004 (0.015)	0.002 (0.017)	0.011 (0.018)	0.007 (0.017)	0.023 (0.020)	0.039* (0.021)	0.024 (0.020)	
Part-Time	0.029*** (0.009)	0.023** (0.010)	0.031*** (0.009)	0.011 (0.012)	0.011 (0.012)	0.015 (0.012)	0.018 (0.010)	0.007 (0.018)	0.017 (0.017)	
Employment	0.016 (0.014)	0.010 (0.014)	0.017 (0.014)	0.013 (0.017)	0.018 (0.017)	0.017 (0.016)	0.053*** (0.021)	0.056*** (0.021)	0.042** (0.020)	
Days Worked	1.121 (4.798)	0.376 (5.282)	2.561 (4.745)	7.747 (11.196)	8.329 (11.819)	11.343 (10.966)	69.687** (28.181)	86.311*** (29.005)	68.533** (27.510)	
Earnings	2.151** (0.980)	1.923* (1.028)	2.532*** (0.975)	2.891** (1.194)	3.120** (1.222)	3.698*** (1.149)	6.459*** (1.592)	6.522*** (1.642)	5.983*** (1.556)	
Additional Birth	-0.027** (0.013)	-0.027** (0.014)	-0.022* (0.011)	-0.039** (0.018)	-0.022 (0.019)	-0.035** (0.016)	-0.045** (0.023)	-0.038* (0.023)	-0.038* (0.021)	

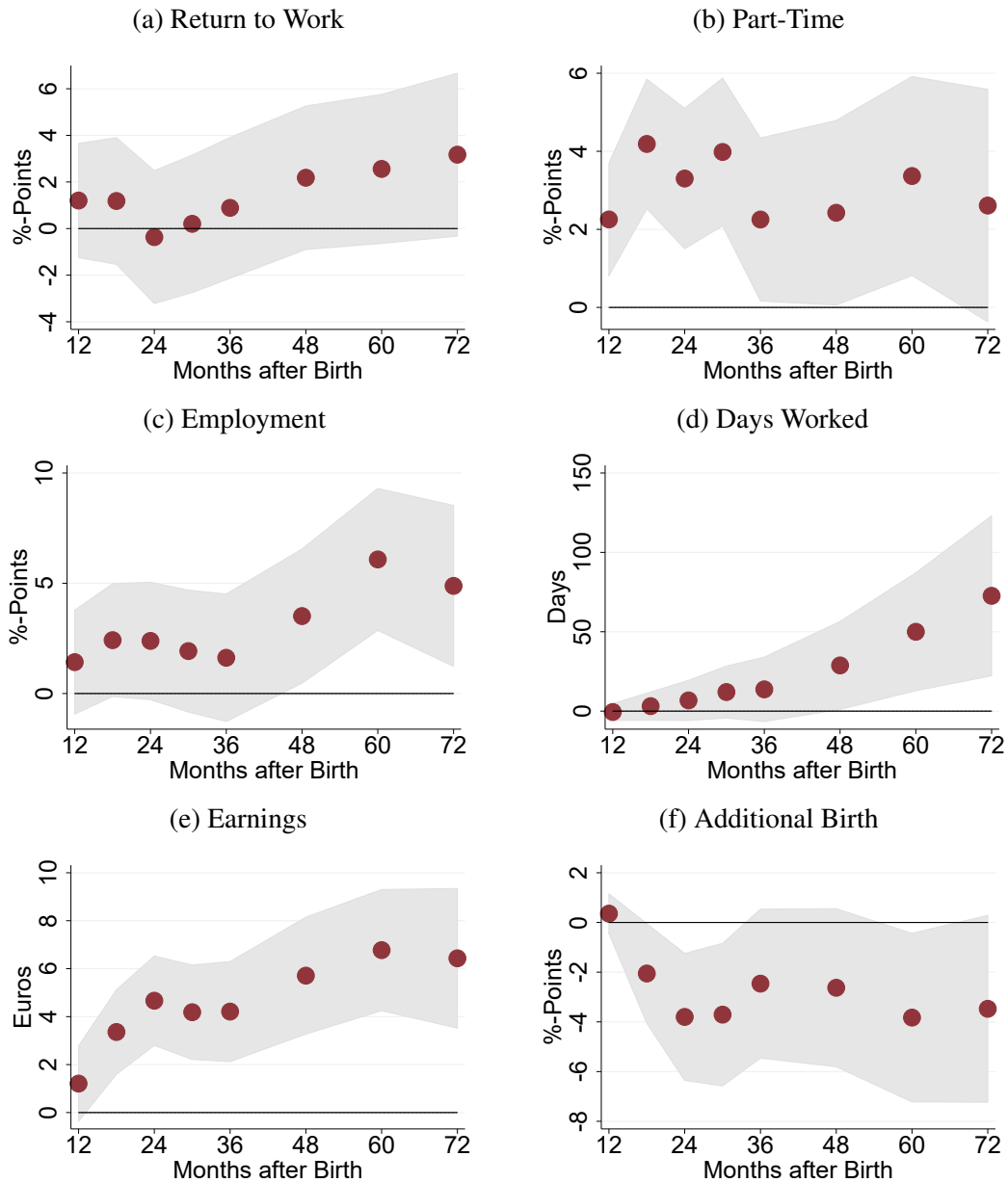
Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results are based on OLS estimations. Robust standard errors are reported in parentheses. Individual controls are age, age squared, dummies for ISCED-education levels, log earnings, full-time status, and dummies for one-digit firm industry of mothers determined prior to childbirth  $i$ . Columns “Age  $<$  35” refer to the specification where individuals older than 35 are dropped from the sample. Columns “West” refer to the specification where individuals who work in East Germany are dropped from the sample. Columns “Industry” refer to the specification where I drop industries with the highest prevalence of long-term sickness spells, namely agriculture, forestry and construction.

dustries. E.g. [Kaas and Kimasa \(2018\)](#) show that in the sectors of manufacturing, mining, and quarrying, among the firms with more than 20 employees, more than three fourths are single-unit firms. In contrast, small branches are likely to be relatively prevalent in other sectors, like e.g. the retail sector, which means that the difference between establishment size and employer size could be of special importance in this sector. To explore this further, I run a sensitivity check, where I exclude women who are working in the retail sector before giving birth. [Figure 5](#) reports the results. The results are robust to this specification. Coefficients tend to be slightly larger in absolute values, in line with the argument of a downward bias of the baseline results introduced by the difference between firm and establishment definition.

### Alternative Observation Period

In this section, I analyze the sensitivity of the results to the definition of the post-reform period. As discussed in [Section 3](#), women could potentially self-select into the treatment group by basing

Figure 5: Robustness: Without Retail Sector



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

their decision to have a child on the law. Although I do not find evidence for this concern in the checks presented in Section 3, I investigate this further here. I restrict the post-reform period to the first 40 weeks in 2001 (corresponding to the usual length of gestation) to further increase the likelihood that women are already pregnant at the reform date. Figure 6 shows that the results hardly change. Given the lower number of observations in the post reform period, standard errors tend to increase slightly. Also, the coefficients tend to be slightly smaller, which could either be evidence for some minor selection effects, but which is also consistent with the idea that it may take some time until mothers adjust their behavior in response to the reform. Still, the results are qualitatively unchanged.

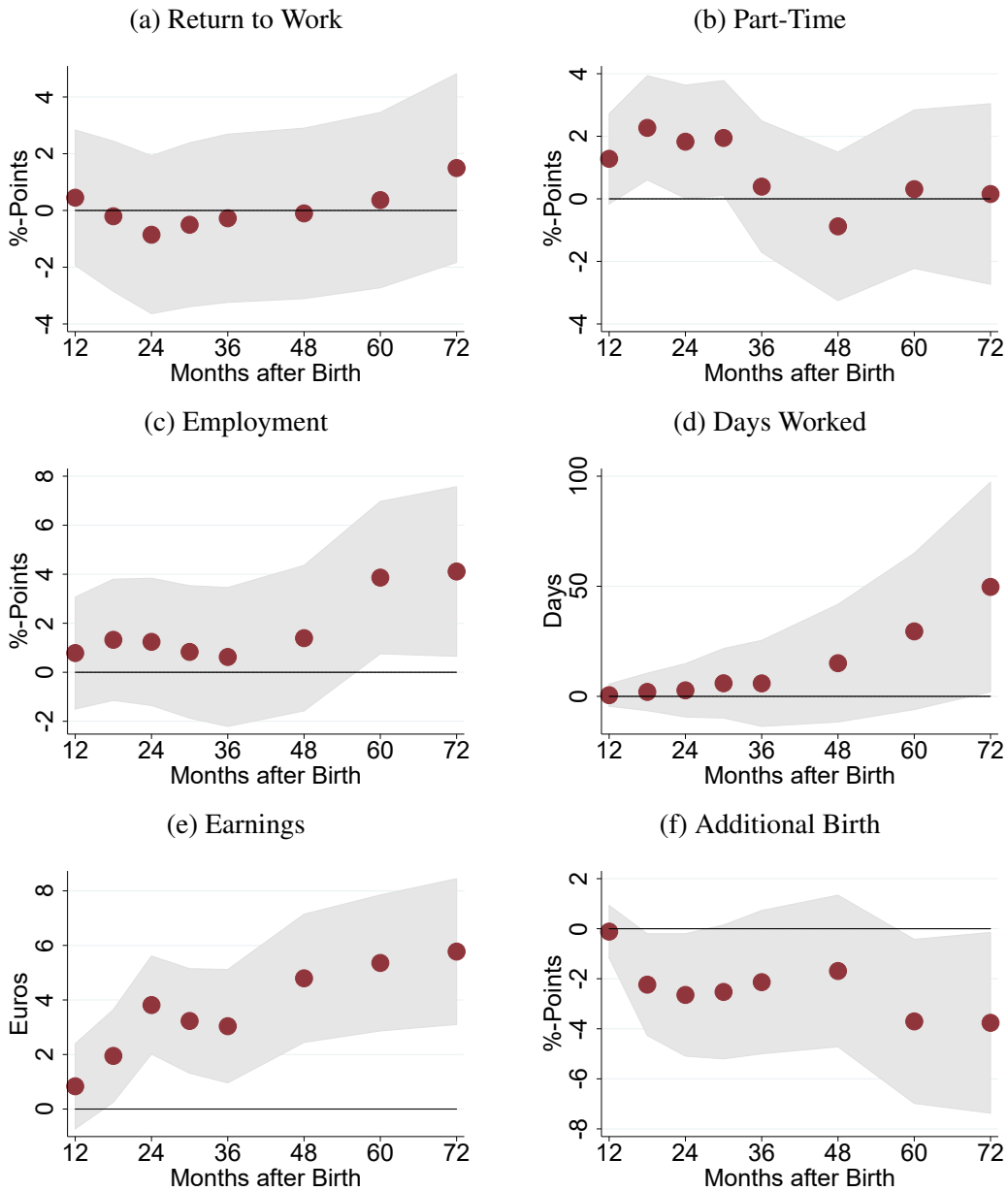
### **Alternative Classification of the Treatment Group**

Next, I check the robustness to the treatment definition. One may be worried that women working in very large companies are systematically different from women working in small companies. If these differences are persistent over time, they do not violate the validity of the differences-in-differences estimates. It would only be problematic if these differences changed over time. The balance test in Table 3 does not detect major changes in terms of observable characteristics. However, to tackle this concern further, I also do a robustness check, in which I only keep women working in small and medium sized establishments. I follow the definition of the IfM Bonn (Institut für Mittelstandsforschung) and drop mothers from the sample who are working in establishments with more than 500 employees. As shown in Figure 7 standard errors increase slightly, due to the drop in the number of observations. However, all main results hold true.

### **Placebo**

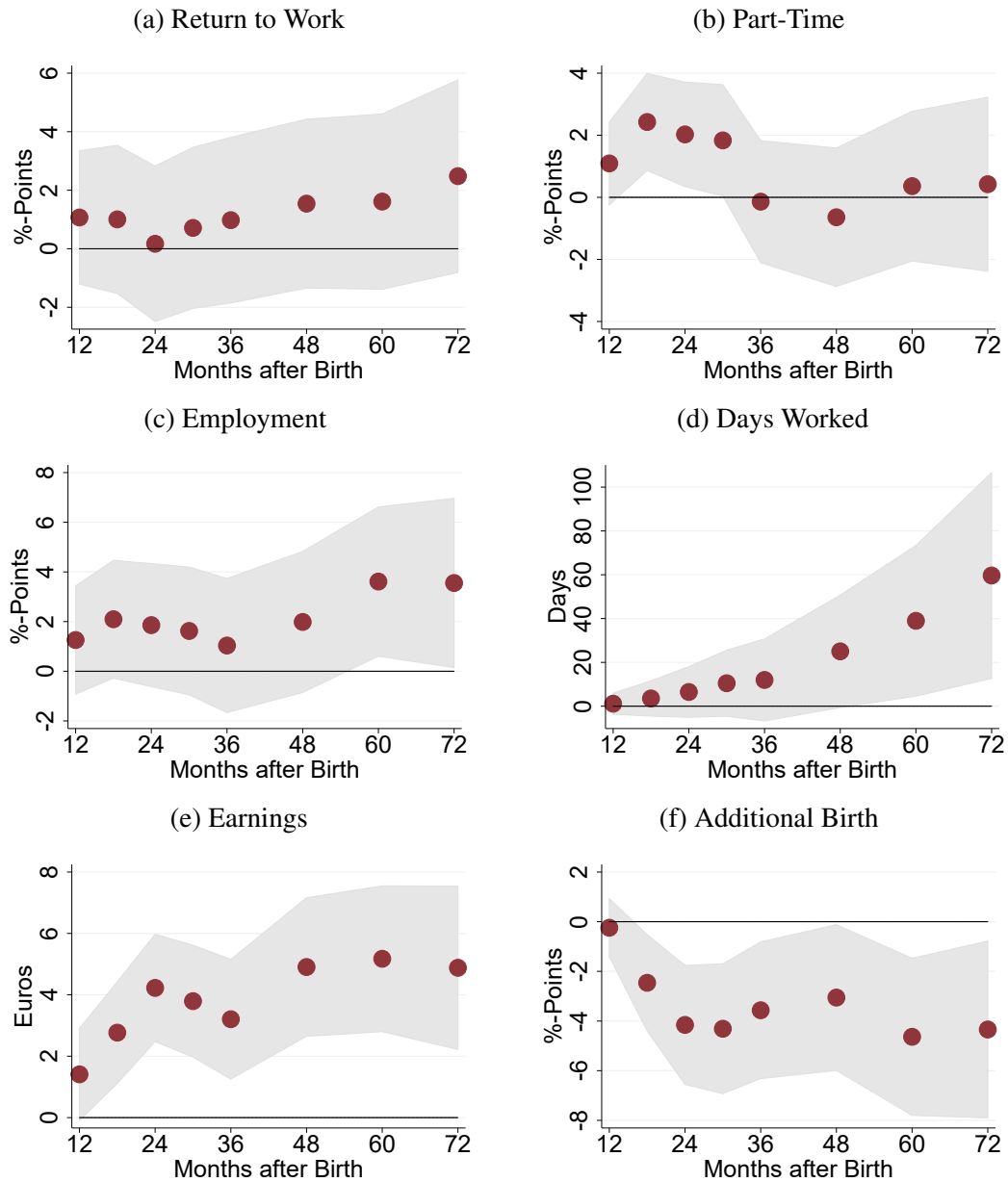
The identifying assumption of the differences-in-differences approach is that the labor market outcomes and fertility of treatment and control group would have evolved in a parallel way if the reform had not taken place. In order to further assess the validity of the common trend assumption,

Figure 6: Robustness: Alternative Observation Period



The dots plot the coefficients of the interaction term  $Post_i * Treat_i$  in equation (1) for the different outcome variables specified at the top of the sub-figures  $t$  months after birth ( $x$ -axis). Results are based on OLS estimations. Individual controls are age, age squared, dummies for ISCED-education levels, log earnings, full-time status, and dummies for one-digit firm industry of mothers determined prior to childbirth  $i$ . The gray areas represent 90% confidence intervals. The post-reform sample is restricted to mothers giving birth in the first 40 weeks in 2001.

Figure 7: Robustness: Alternative Classification of the Treatment Group



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

I run a placebo regression on the pre-reform period data only and pretend that a reform took place during this period. More precisely, I introduce a fake reform on January 1, 1998. I choose this particular date, because it allows me to observe mothers up to 30 months after birth both for the pre and the post placebo-reform period and still ensures that the observation date is before January 1, 2001 - the true reform date. The results of estimating equation (1) using this setup are displayed in Figure 8. The estimates do not show any systematic pattern that could drive the findings and the estimated coefficients are small, in general. This is supportive evidence for a common trend of the treatment and the control group in the pre-reform period. Only in the case of the part-time variable, the placebo test results are significant for two subsequent periods (24 and 30 months after birth). However, if anything this would suggest a negative pre-reform evolution of the part-time share in the treatment group as compared to the control group, while I find a positive and significant effect on part-time employment in months 24 and 30 after birth in my baseline results.

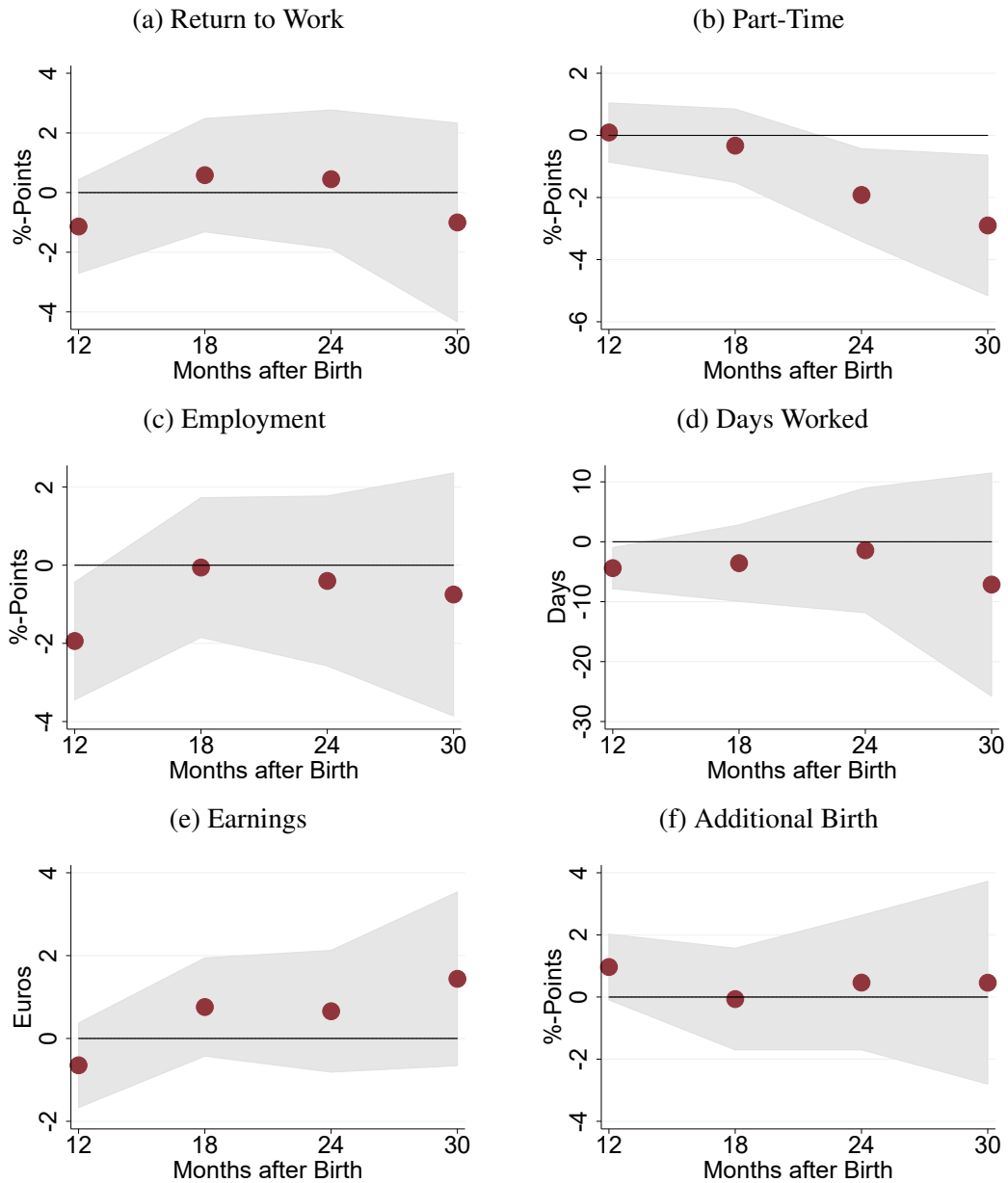
#### **4.5 Heterogeneity by Income and Full-Time Share**

Before the reform, mothers were able to reduce their post-birth work-schedule to part-time work if the employer agreed to it. After the reform, it became much easier to switch from full-time to part-time employment as the employer was only able to deny the request based on business grounds. Therefore, I would expect that the reform had a stronger impact on mothers working in jobs where part-time work was less prevalent, traditionally. In these jobs, it is more likely that the reform relaxed a binding constraint.

Part-time work is especially concentrated in the service sector (restaurants, education and health- and social work) in Germany ([Bundesagentur für Arbeit, 2018](#)) and in low-income jobs. In this section, I check the heterogeneities of the reform effects based on the pre-birth income of women and the full-time share in the industry.

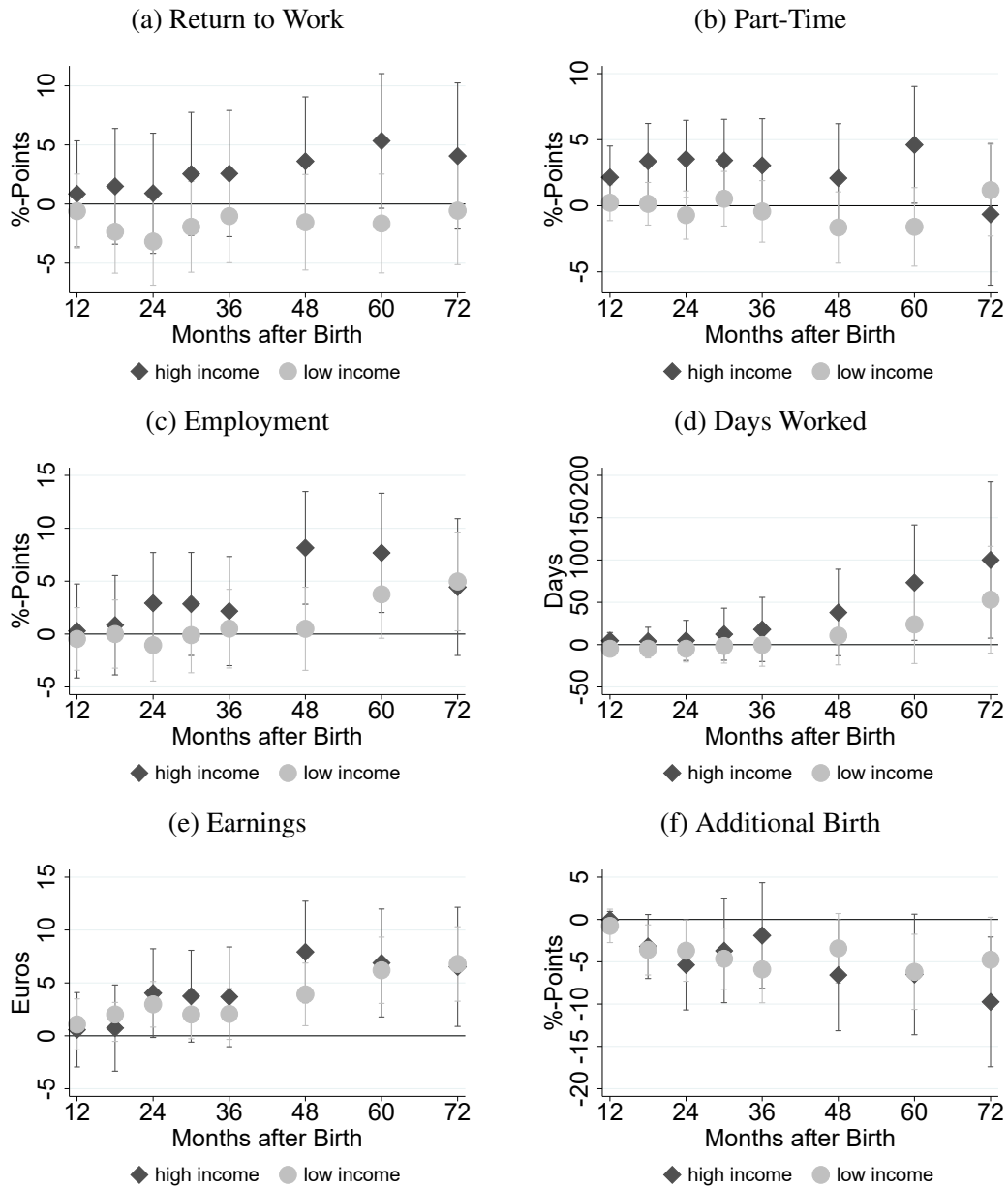
Figure 9 displays the results of estimating equation (1) separately for high-income and low-income earners. The sample is split at the median pre-birth income of mothers who worked full-

Figure 8: Robustness: Placebo Reform in 1998



The dots plot the coefficients of the interaction term  $Post_i * Treat_i$  in equation (1) for the different outcome variables specified at the top of the sub-figures  $t$  months after birth ( $x$ -axis). Results are based on OLS estimations. Individual controls are age, age squared, dummies for ISCED-education levels, log earnings, full-time status, and dummies for one-digit firm industry of mothers determined prior to childbirth  $i$ . The gray areas represent 90% confidence intervals. The pretended reform date is January 1, 1998.

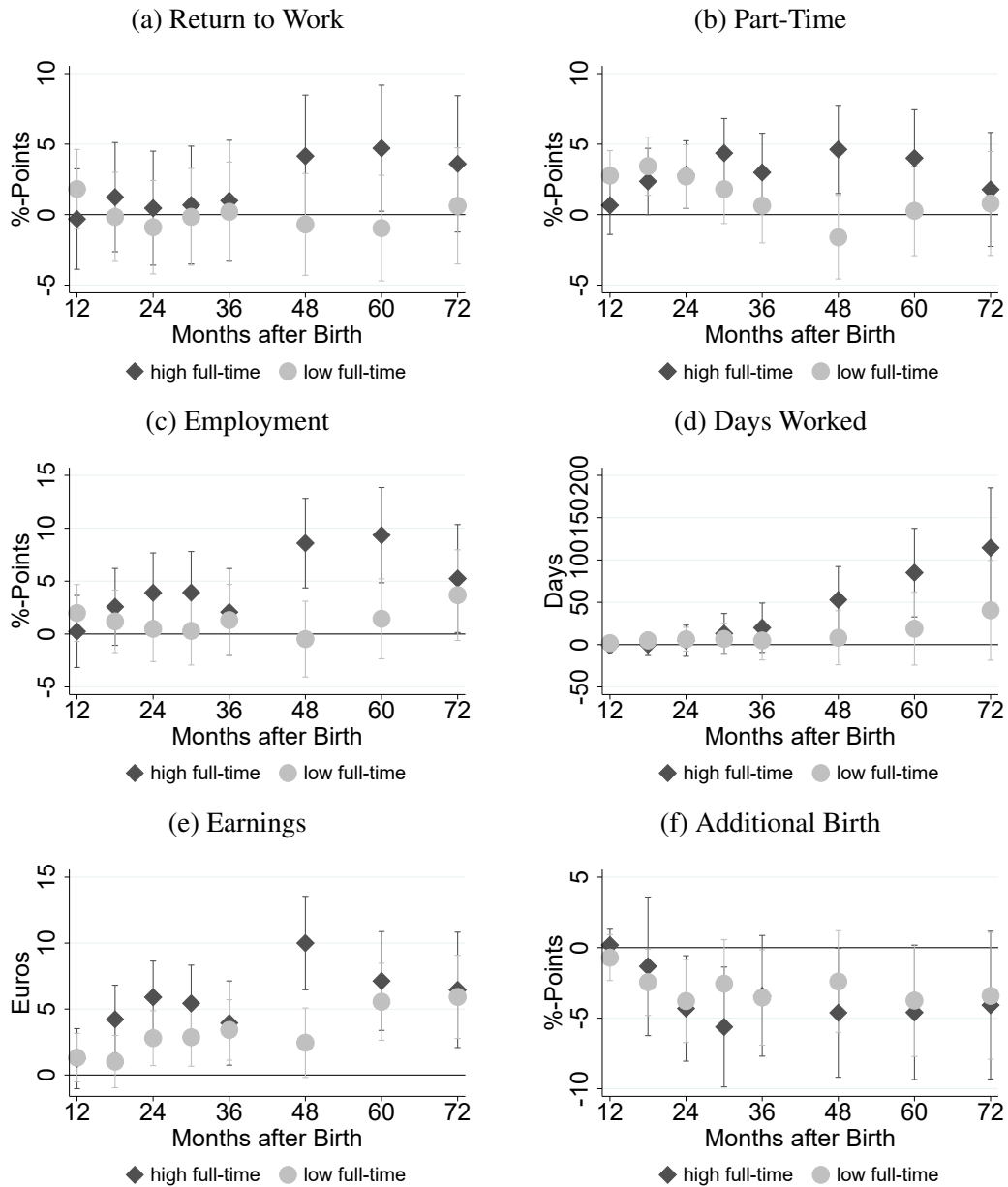
Figure 9: Heterogeneity by Income



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



Figure 10: Heterogeneity by Full-Time Share



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

time before giving birth. Only mothers who worked full-time before giving birth are considered in the analysis. Figure 10 shows the heterogeneities by the share of full-time workers per three-digit industry. I consider the industry in which the mother was working before giving birth. The sample is split at the median full-time share.

In line with my hypothesis, the right to work part-time tends to have a stronger impact on labor-market outcomes of mothers with jobs where part-time work has been less common traditionally. However, results become less stable and are less precisely estimated due to the lower number of observations in the splitted samples. The evidence suggests that mothers with below median pre-birth income and those who worked in industries where part-time work was relatively prevalent were hardly affected by the reform. The lion's share of the gray dots in Figures 9 and 10, which represent the reform effects for these women, are close to zero and statistically insignificant. In contrast, the reform effect on the probability to work part-time tends to be stronger for mothers with higher income and those working in industries with a high full-time share. There is also some evidence that these women were more likely to be employed in the long-run and earned higher wages in response to the legal change. The impact on additional births is less clear-cut.

## 5 Conclusion

This paper studies the impact of a German law that gave the statutory right to work part-time to employees of firms with more than 15 employees on maternal labor market outcomes and fertility after giving birth. I find evidence that the law was effective in granting access to part-time work to those who wanted it: part-time employment increased in the short run. In the long-run, the law had a positive effect on maternal employment and labor earnings. However, the law led to a reduction or a postponement of higher-order births for women who returned to work after giving birth.

In terms of policy implications, this suggests that the legal change helped to increase maternal labor supply in the long-run. However, it might have had a potentially unintended negative impact

on (higher-order) fertility<sup>13</sup>, which may be of concern given below replacement fertility levels in Germany. A large number of OECD countries adjusted their part-time work regulations during the last decades and introduced laws which grant employees the right to reduce their working hours. These results may, thus, also be of interest in a non-German context. The findings show that the right to work part-time can have a strong impact on maternal labor market outcomes and fertility. Moreover, reform effects could potentially be expected to be even larger in countries, where part-time work was less prevalent before the introduction of the right to work part-time than in Germany.

In my analysis I only consider a relatively short post-reform period of one year. If the introduction of the law is associated with information frictions or implementation frictions, the effects of the reform are likely to grow further over time. Moreover, on January 1, 2019, the German government introduced a law that further extends the rights of employees granted by the Law on Part-Time Work and Fixed-Term Employment Contracts (TzBfG). The new law allows temporary part-time work with a right of return to the previous working time. So, employees do not only have the legal right to downgrade hours, but also the right to upgrade hours later on. This extension of rights could possibly lead to an even higher take-up rate of part-time work and could thus further strengthen the effects on labor outcomes and fertility.

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<sup>13</sup>As previously emphasized, this is only suggestive evidence as I only observe higher-order fertility of women who returned to the labor market after their previous birth. Moreover, my results refer to higher-order births up to six years after the previous birth. This means, that it is theoretically possible that women only postponed further births and the total number of children born is unaffected.

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# A Appendix

Figure A.1: Timeline

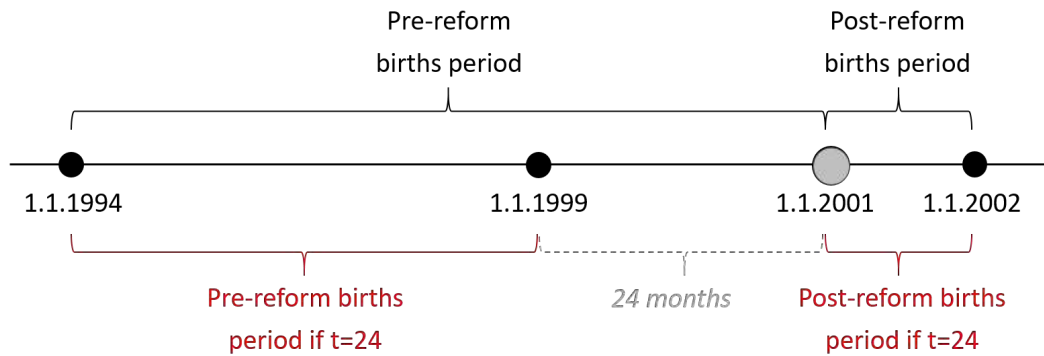
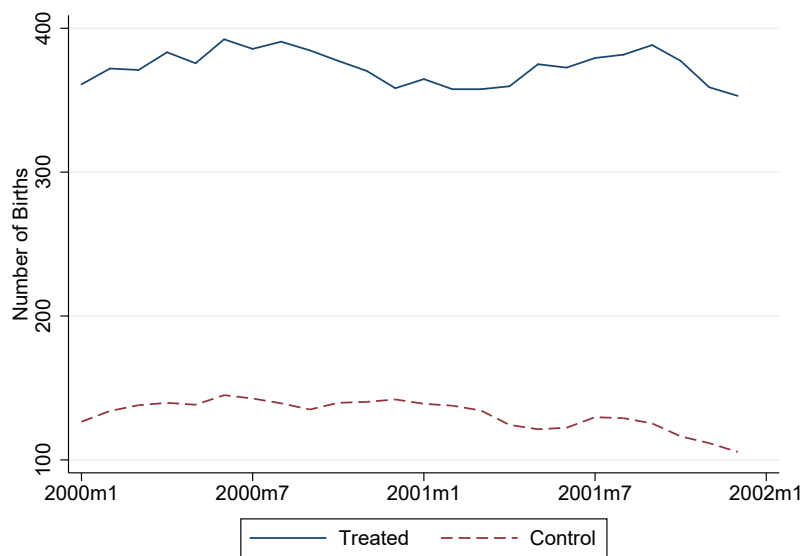


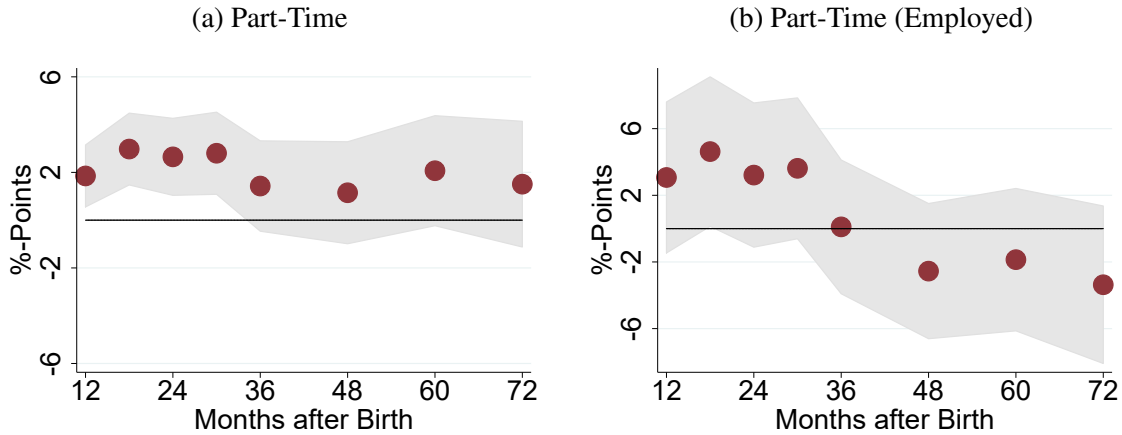
Figure A.2: Evolution of Number of Births



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



Figure A.3: Part-Time Status



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

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

Part-Time to Full-Time	
Coefficient	0.027*
	(0.015)
Observations	22,404

Significance levels: \*\*\*  
 p<0.01, \*\* p<0.05, \* p<0.1.  
 Results are based on OLS estimations of equation (1). Robust standard errors are reported in parentheses. Individual controls are age, age squared, dummies for ISCED-education levels, log earnings, full-time status, and dummies for one-digit firm industry of mothers determined prior to childbirth  $i$ .