Evaluating a parental leave reform: the effects of ElterngeldPlus on labor supply and domestic work

Maier, Martina

University of Leiden
Faculty of Governance & Global Affairs

Master Thesis

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by

Martina Maier
s4241401
MSc Public Administration
Economics & Governance

Submission Date: June 6, 2025
Supervisor: Dr. Jordy Meekes

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Abstract

This thesis analyzes the causal effects of Germany's 2015 parental leave reform, ElterngeldPlus, which aimed to promote a more gender-equal division of paid and unpaid work. The reform introduced new components to the parental leave system to reduce birth-related employment interruptions for mothers and incentivize fathers' involvement in childbearing. Using longitudinal data from the German Socio-Economic Panel (SOEP), my analysis applies a dynamic difference-in-differences approach to compare labor market and domestic work outcomes of first-time parents with births around the eligibility date. The results provide some evidence that the reform led to a significant increase in maternal employment in the first years after childbirth. However, fathers' employment and working hours remain unaffected. I find no significant effects on either parents' weekly hours worked or time spent on childcare and housework. My findings suggest that although ElterngeldPlus may have supported earlier labor market re-entry for mothers, it did not substantially alter the gendered division of domestic responsibilities. Future research should build on these findings and more clearly investigate the effects of the reform.

Keywords: parental leave, gender division of labor, childcare, housework, difference-in-differences

1. Introduction

Paid parental leave is a central element of family policy in most OECD countries (Ekberg et al., 2013). It enables parents to take time off work to care for their child without having to leave their paid job. As such, parental leave policies aim to help parents balance work and family life, support job continuity and improve overall family well-being (Rossin-Slater, 2017). Moreover, because these policies directly influence how parents allocate their time between paid market work and unpaid family work, they have important implications for gender equality (Haas & Hwang, 2008).

In recent years, many countries have reformed their parental leave systems to promote a more equal division of paid and unpaid work between mothers and fathers (Tamm, 2019). Such reforms include the introduction of "daddy quotas"—leave periods reserved exclusively for fathers—to encourage greater paternal involvement (Thévenon & Solaz, 2013), as well as flexible leave options that allow part-time work while receiving benefits to support an earlier return to work (Schober & Zoch, 2015). These efforts are especially important, as women continue to take the majority of leave in the early months after childbirth, perform more unpaid work than fathers, and, as a result, face long-term disadvantages in the labor market (Huerta et al., 2014; Rossin-Slater, 2017).

In 2015, a major reform of this kind was implemented in Germany. Before the reform, mothers were discouraged from returning to work during the first year after childbirth, and fathers had little incentive to take more than the two months of leave reserved for them (Huebener et al., 2016). The new scheme, *ElterngeldPlus* (parental benefit plus), sought to address these issues and reduce the gender gap in time use among mothers and fathers. It introduced a more generous benefit for part-time work to shorten mothers' employment breaks, and granted extra months of paid leave if both parents reduced their working hours and took leave at the same time (Geyer & Krause, 2016). Despite the reform's ambitious goals, empirical evidence remains limited. So far, only a few studies have examined its effects. First evidence suggests that the reform slightly increased employment for some mothers, at least in the short term (Baertsch & Sandner, 2023; Zimmert & Zimmert, 2024). Another study finds that some fathers took longer leave and reduced their working hours, while mothers tended to shorten their leave (Boll & Nikolka, 2024). Overall, the results are still inconclusive and based on only a small number of studies.[1] Besides, these studies have mostly focused on maternal employment and paid less attention to the reform's other goals, such as the division of unpaid work.

To address this gap, this thesis analyzes the effects of the 2015 parental leave reform on mothers' and fathers' time use after childbirth. Specifically, I ask whether ElterngeldPlus influenced parents' working hours and time devoted to domestic tasks. To this end, I exploit a quasi-experiment created by the introduction of the reform that applied to all births from July 1, 2015

[1] So far, only three studies have examined the effects of ElterngeldPlus. Baertsch and Sandner (2023) and Zimmert and Zimmert (2024) focus exclusively on maternal employment, while Boll and Nikolka (2024) analyze the reform's impact on both mothers' and fathers' leave-taking, labor supply, and childcare arrangements.

onward. Using a dynamic difference-in-differences approach, I compare labor market outcomes as well as time spent on childcare and housework among parents whose first child was born before and after the reform, in each of the first three years after childbirth. The analysis is based on longitudinal data from the German Socio-Economic Panel (SOEP), which provides information on parents' employment status, weekly hours worked, and hours spent on domestic activities. The results suggest that the reform supported earlier labor market re-entry for mothers but had no significant effect on fathers' employment or on working hours for either parent. Moreover, there is no evidence that the reform altered the division of unpaid domestic work.

This thesis makes several contributions to the existing literature. To the best of my knowledge, it is among the first to evaluate the impact of ElterngeldPlus on both mothers' and fathers' labor supply and unpaid care work. It also extends research on Germany's earlier leave reform, Elterngeld (e.g., Bergemann & Riphahn, 2010a; Frodermann et al., 2020; Geisler & Kreyenfeld, 2011; Geyer et al., 2015; Kluve & Tamm, 2013), as well as the broader international literature on parental leave policies.[2] Most of these studies focus on changes in leave duration or benefit generosity, while fewer examine design features such as part-time options (e.g., Joseph et al., 2013). Furthermore, research on the link between parental leave and care outcomes often centers on fathers, typically assessing how paternity quotas affect their involvement in childcare.[3] This thesis adds to this literature by examining both paid and unpaid work outcomes, and by analyzing effects for both mothers and fathers.

Further, evaluating the effects of ElterngeldPlus is highly relevant in the German context, where mothers continue to experience long career interruptions (Huebener et al., 2016), most often return to work through part-time employment (Federal Statistical Office, 2025) and face a relatively large child penalty in earnings (Kleven et al., 2019).[4] Understanding which policy tools can help reduce these inequalities is crucial to promote mothers' long term labor market participation and alleviate the negative economic consequences of an aging population. My findings contribute to a better understanding of how specific features of leave policies shape parents' labor supply and division of care work, and are applicable beyond the German context.

The remainder of the thesis is structured as follows. Chapter 2 presents the institutional background of family policy in Germany, including a brief overview of both the previous and current parental leave schemes. Chapter 3 outlines the theoretical framework and reviews prior research on labor market and unpaid work outcomes of parental leave policies, from which I derive my hypotheses. Chapter 4 presents the empirical strategy and data sources. Chapter 5 describes the main results and robustness checks, while Chapter 6 offers a discussion of the findings and limitations. Finally, Chapter 7 concludes and outlines implications for future research.

[2]For studies on the U.S., see for example Waldfogel (1999), Han et al. (2009) and Rossin-Slater et al. (2013); for Canada, see Baker and Milligan (2008); for Austria, Lalive and Zweimüller (2009) and Lalive et al. (2014); for Norway, see Dahl et al. (2016); and for Japan, Yamaguchi (2019).

[3]See, for example, Ekberg et al. (2013) and Haas and Hwang (2008) for Sweden; or Tamm (2018) and Schober (2014) for Germany.

[4]According to Kleven et al. (2019), German mothers experience a long-run child penalty in earnings—defined as the average earnings reduction between five to ten years after childbirth—of approximately 61%.

2. Institutional background

2.1 Overview of parental leave schemes in Germany

Germany has a range of parental leave policies aimed at helping parents balance work and family responsibilities. Most of these regulations were first introduced in the 1950s and have undergone significant transformations since, gradually moving away a male breadwinner/female caregiver model toward a more Nordic-inspired dual-earner household model (Spiess & Wrohlich, 2008). In brief, Germany's leave system consists of three institutions: maternity leave, unpaid parental leave (*Elternzeit*), and paid parental leave (*Elterngeld*). While this thesis focuses on the paid parental leave scheme and its most recent reform in 2015, it is useful to distinguish this from the two related policies.[5]

Maternity leave applies only to mothers around the time of childbirth. It grants six weeks of leave before and eight weeks after childbirth, during which mothers are not allowed to work, are protected against dismissal and have the right to return to their previous job (Dustmann & Schönberg, 2012; Schönberg & Ludsteck, 2014). During this period, mothers receive *Mutterschaftsgeld*, a benefit that replaces their total net earnings prior to birth and is paid by statutory health insurance. On the other hand, unpaid parental leave, or Elternzeit, is available to both parents and allows them to take up to three years of unpaid, job-protected leave after childbirth (Bergemann & Riphahn, 2022).

The third policy, and the focus of this thesis, is Germany's paid parental leave scheme, which provides income support to parents for a limited period after childbirth. Before the most recent reform in 2015 that introduced ElterngeldPlus, different versions of the scheme were in place. Until 2006, parents could apply for *Erziehungsgeld* (child-rearing benefits), provided they did not engage in full-time work (i.e., worked fewer than 30 hours per week). Erziehungsgeld was a flat-rate means-tested benefit granted to one parent per family, but due to strict income thresholds and the low benefit amount it primarily targeted low-income households (Kluve & Tamm, 2013).[6]

On July 1, 2007, the German government replaced the means-tested child-rearing benefit with Elterngeld (parental benefit) which significantly increased the generosity of paid leave (Spiess & Wrohlich, 2008). Under Elterngeld, parents received between 65% to 100% of their net income instead of a flat-rate benefit. The benefit was calculated on the basis of the average earnings twelve months prior to birth, with a minimum of 300 euros per month paid even if not previously employed. It was granted for up to twelve months. To encourage greater paternal involvement

[5]This is especially relevant, as leave systems differ across countries. In some cases, maternity and parental leave are regulated jointly under a single policy. In Germany, maternity leave is a distinct regulation, separate from both unpaid parental leave and paid parental leave.

[6]Married or cohabiting couples were eligible if their annual net income in the calendar year prior to the birth was below €30,000. It paid a maximum of €300 per month for up to 24 months or, alternatively, €450 per month for twelve months (BMFSFJ, 2006).

in family life, an additional two months of Elterngeld were granted if both father and mother shared parental responsibilities (Huebener et al., 2016). These two months, often referred to as the "paternity quota", were reserved for each parent and would be lost if not used.[7]

The 2007 reform marked a first turning point in German family policy. There is extensive research on the policy's effects on various outcomes.[8] Studies show that it increased employment among mothers, though only in the second year after childbirth when the benefit period ended (Bergemann & Riphahn, 2010b; Geyer et al., 2015; Kluve & Tamm, 2013). In contrast, mothers' labor force participation declined during the first year after childbirth, partly due to policy's implicit disincentive for work (Baertsch & Sandner, 2023). Because benefits were calculated based on lost earnings, any income earned after childbirth directly reduced the benefit amount. As a result, even small earnings led to a small but immediate reduction in benefits.[9]

The reform also increased the share of fathers taking leave, although most used only the two months reserved for them (Pull & Vogt, 2010; Trappe, 2013). Apart from this short break, no significant changes in fathers' labor supply were found in the short and medium term (Kluve & Tamm, 2013). A key reason was that if both parents took Elterngeld at the same time, the benefit duration was halved, which discouraged shared caregiving and lowered incentives for fathers to take longer leave (Huebener et al., 2016). Evidence on the division of unpaid work is mixed. Some studies suggest that fathers' increased leave-taking contributed to a more equal division of care work (e.g., Schober & Zoch, 2015; Tamm, 2018). However, another study found no significant impact on fathers' time spent on childcare (Kluve & Tamm, 2013).

2.2 The 2015 parental leave reform

ElterngeldPlus marks the second major reform of Germany's parental leave scheme. As stated in the proposed legislation, the reform aimed to reduce gender gaps in time use among mothers and fathers, and more broadly, to better reflect the changing needs and preferences of German families (Bundestag, 2014). The reform added two new components to the existing scheme: *ElterngeldPlus* (parental benefit plus; EG+) and the *PartnerschaftsBonus* (partnership bonus). The original Elterngeld scheme remains in place but is now referred to as Basiselterngeld (basic parental benefit). Together, these elements form the current parental allowance system, which applies to parents with children born on or after July 1, 2015. Eligible parents can choose between the different components. In what follows, the term ElterngeldPlus refers to the 2015 reform as a whole, while EG+ refers specifically to the component of the policy. Table A2 in the Appendix provides a detailed overview of both the former Elterngeld and current ElterngeldPlus policy schemes.

Basiselterngeld remains unchanged from its 2007 design. It replaces lost income at a rate of

[7] Single parents can receive a total of 14 months alone.

[8] See Huebener et al. (2016) and Samtleben et al. (2019) for a detailed overview.

[9] This meant that whether a parent stayed out of the workforce and received the full benefit, or worked part-time and received a reduced amount, their total income remained roughly the same.

between 65% to 100%, depending on the parent's prior earnings. EG+ was introduced to make paid work during parental leave more financially attractive by changing both the duration and calculation of benefits (BMFSFJ, 2020). First, it allows parents to receive benefits for twice as long (up to 24 months instead of twelve). As a result, the payment scheme changes as follows. If post-birth income is less than 50% of pre-birth income, the benefit is independent of actual post-birth earnings and always equals half the full Basiselterngeld amount but is now paid for twice as long (Baertsch & Sandner, 2023). If post-birth income exceeds more than 50% of pre-birth income, the benefit is calculated based on actual income loss, as under Basiselterngeld, but still paid for 24 months. In many cases, this results in a higher total benefit, even if the monthly amount is lower (BMFSFJ, 2020).[10] The full calculation rules are summarized in Table A1 in the Appendix.

To illustrate, the following Table 1 presents three examples of benefit calculations for a parent with a pre-birth income of €2,000. In scenario A, the parent earns no income after childbirth. Both Basiselterngeld and EG+ result in the same total benefit of €15,600. However, while Basiselterngeld pays €1,300 per month over twelve months, EG+ pays half the monthly amount over 24 months. In scenario B, the parent earns €500 after childbirth (below the 50% threshold). Here, EG+ pays the capped monthly maximum of €650 (i.e., half of the full Basiselterngeld benefit from Scenario A), again for 24 months. This results in a higher total benefit of €15,600, compared to €11,700 under Basiselterngeld. In scenario C, the parent earns €1,500 post-birth (above the 50% threshold). Here, the EG+ monthly benefit is calculated based on the actual income loss (€500) and results in a monthly payout of €325 over 24 months. This adds up to €7,800, which still exceeds the Basiselterngeld benefit amount of €3,900.

Table 1. Examples of benefit calculations: Basiselterngeld (Elterngeld 2007) vs. EG+

		A	**B**	**C**
(1) Pre-birth income		€2,000	€2,000	€2,000
(2) Post-birth income		€0	€500	€1,500
(3) Income loss [(1) – (2)]		€2,000	€1,500	€500
(4) Replacement rate		65%	65%	65%
(6) EG+ limit [50% of (7) in A]		€650	€650	€650
(7) Monthly benefit [(3) × (4)]	*Basiselterngeld*	€1,300	€975	€325
	EG+ capped at (6)	€650	€650	€325
(8) Total benefit	*Basiselterngeld* (12 months)	€15,600	€11,700	€3,900
	EG+ (24 months)	€15,600	€15,600	€7,800

*Note. A*dapted from Geyer and Krause (2016). The examples are calculated based on BMFSFJ (2020).

[10]The total EG+ benefit is maximized when post-birth income equals exactly 50% of pre-birth income (Baertsch & Sandner, 2023).

To summarize, EG+ guarantees at least the same benefit amount as Basiselterngeld, and in many cases offers more (BMFSFJ, 2020). Parents who return to work part-time during the leave period generally benefit from higher total payments under the new scheme (Zimmert & Zimmert, 2024). This feature of the reform primarily targeted mothers, as many in Germany still withdraw entirely from the labor force in the first year after childbirth (Drahs et al., 2015).[11]

The last key feature of the reform concerns couples who take leave simultaneously. Unlike the previous scheme, the benefit duration is no longer halved when both take leave and receive benefits at the same time. Instead, to actively promote shared caregiving responsibilities, the final component of the reform, the PartnerschaftsBonus, grants four additional months of EG+ if both parents work part-time simultaneously (between 25 and 30 hours per week).

While the new reform changes incentives for both parents, its actual effects on their time spent in paid and unpaid work remain ambiguous. Consider, for example, the new part-time option targeting mothers' labor supply. On the one hand, combining work and benefits becomes more attractive, as the relative advantage of remaining unemployed declines. On the other hand, since working part-time maximizes total benefit payments, it may reduce incentives to increase working hours. The few studies that evaluate the effects of ElterngeldPlus in a difference-in-differences strategy suggest modest or no effects. Using administrative data, Baertsch and Sandner (2023) and Zimmert and Zimmert (2024), find that the reform increased the likelihood that mothers return to work within the first year after childbirth due to the new part-time option. However, they do not examine other outcomes. Boll and Nikolka (2024) are the first to analyze effects on both mothers and fathers using survey data. They find that fathers tend to take longer leave following the reform, while mothers' leave duration decrease slightly. They also find reduced working hours among highly educated fathers, but find no significant effects beyond that.

Several theoretical mechanisms may help explain how ElterngeldPlus influences parents' labor supply and domestic work arrangements. Building on the existing literature on parental leave, the next chapter draws on neoclassic economic theory, resource-bargaining approaches and constructivist perspectives to discuss the expected effects of ElterngeldPlus on maternal and paternal outcomes.

[11] In 2022, for instance, only one in eight mothers with a child less than one-year old (13%) were employed, compared to 87% of fathers (Federal Statistical Office, 2023).

3. Theory and hypotheses

3.1 Parental leave policies and labor supply

Theoretical explanations of how parental leave effects on labor supply often start from an optimization problem: following childbirth, parents decide whether and how much to work (Zimmert & Zimmert, 2024). Some remain employed without taking leave, while others interrupt or reduce paid leave to receive income-replacing benefits. Among those who take leave, the options include returning to work at reduced or full hours, delaying reentry, or exiting the labor force entirely (Rossin-Slater, 2017).

Labor supply theory views these decisions as a rational trade-off between expected earnings, the availability of alternative income sources, and the value placed on time outside paid work (Blau et al., 2022; Gangl & Ziefle, 2014). Parental leave schemes affect these choices by reducing the opportunity costs of temporarily interrupting employment (Gangl & Ziefle, 2014; Schober & Büchau, 2022). As a result, they affect labor supply both at the extensive margin (i.e., labor force participation choices) and the intensive margin (i.e., choices about hours of work).[12]

While leave policies clearly shape labor supply, the direction and magnitude of their effects are theoretically ambiguous and highly sensitive to policy design (Klerman & Leibowitz, 1999). The literature outlines two mechanisms. First, job-protected paid leave can strengthen labor market attachment by allowing parents maintain ties with their employers during temporary breaks. This helps preserve firm-specific human capital and avoids search costs or wage penalties associated with potential job loss or employer changes (Ondrich et al., 1996; Ruhm, 1998). From this view, paid leave should encourage faster reentry and higher labor force participation over time (Rossin-Slater et al., 2013). Second, however, even when job protection is guaranteed, leave implies time away from work. It may induce some parents that would have otherwise returned sooner, to stay away from work for longer. Human capital theory suggests that career interruptions or reduced working hours can lead to skill deprecation and loss of labor market experience (Becker, 1994; Mincer & Polachek, 1978). Consequently, while shorter leaves may promote stronger labor force attachment, longer leave durations tend to have the opposite effect (Rossin-Slater, 2017).

Empirical evidence finds both positive and negative effects, but generally conclude that the longer the leave, the lower the likelihood of maternal employment. In the United States, for example, short, job-protected leave has been shown to increase maternal employment and job continuity (Glass & Riley, 1998; Waldfogel, 1999).[13] In contrast, studies of more generous schemes that are

[12]While leave policies may also affect wages and career advancement, the empirical evidence discussed here focuses specifically on labor supply outcomes (i.e., participation and hours worked). For a broader overview of studies on parental leave on other employment outcomes, see Rossin-Slater et al. (2013).

[13]There is a large body of literature on U.S. leave schemes their effects on maternal labor supply. It is important to note that the United States differs from most countries, as it offers no nationwide paid leave scheme, and only some states have a paid protection period. The studies cited refer to the unpaid job-protected leave granted through the Federal Maternal Legislation Act (FMLA).

widespread in Canada and European countries find that extended leave tends to delays mothers' return-to-work and weakens their short-term labor force attachment.[14] Overall, most research focuses on employment status, while fewer studies examine working hours. An exception is Schönberg and Ludsteck (2014), who show that leave extensions in Germany between 1979 and 1992 led to a decline in full-time employment among mothers.

Further, most studies deal with the impact on female employment, as women are typically the main users of parental leave and are more responsive to changes in benefit design (Gangl & Ziefle, 2015). Fathers, by contrast, are less frequently studied, as their time use tends to be less affected by such reforms. Where studied, authors find only limited or short-term effects on paternal labor supply, typically in the form of reduced working hours only in the period during which benefits are paid (Cools et al., 2015; Ekberg et al., 2013; Tamm, 2019). Notably, most studies analyze changes in leave duration or generosity, while fewer studies address design features like flexibility in leave use or part-time options. One exception is Joseph et al. (2013), who study a French reform allowing parents to work part-time while receiving benefits. They find that while the policy increased maternal employment, it also reduced long-term earnings by increasing the likelihood that mothers remained in part-time work even after the benefit period ended (Joseph et al., 2013).

3.2 Parental leave policies and domestic work

Research on how parental leave policies affects mothers' and fathers' contributions to unpaid care work draw on two broad approaches: resource-based bargaining models and constructivist approaches on gender. Resource-based approaches build on Becker's (1991) theory of time allocation, which posits that couples seek to maximize joint utility by specializing based on their comparative advantages. Typically, the higher-earning partner specializes in paid work, while the lower-earning partner takes on more domestic responsibilities (Becker, 1991). Over time, the partner who spends more time in paid work accumulates more economic resources than the other. Bargaining theories argue that these resources translate into greater bargaining power within the household (Lundberg & Pollak, 1996). As a result, the higher-earning partner is better positioned to negotiate out of unpaid care responsibilities (Brines, 1994).

A related perspective emphasizes time availability rather than economic resources (Bianchi et al., 2000). Here, domestic tasks are negotiated based on each partner's time constraints: the partner who spends less time in paid work expected to contribute more to unpaid work (Becker, 1991; Blood & Wolfe, 1960; Coverman, 1983). Based on this logic, the partner who takes longer parental leave and spends fewer hours in paid work has less bargaining power is more likely to take on a larger share of domestic tasks during and after the leave period (Schober & Büchau, 2022). Therefore, if leave policies lead to a more equal division of paid work between parents, they may also encourage more equal division of unpaid labor (Schober, 2014).

[14] See, for example, Baker and Milligan (2008) for Canada, Lalive and Zweimüller (2009) and Lalive et al. (2014) for Austria; Dustmann and Schönberg (2012) and Ondrich et al. (1996) for Germany; and Dahl et al. (2016) for Norway.

Generally, these approaches conclude that employed mothers, especially those working full-time, tend to spend less time on childcare and housework (e.g., Craig & Mullan, 2011; Hook, 2010). Although both tasks are often analyzed together, several scholars stress the need of distinguishing between housework and childcare (e.g., Bünning, 2020; Coltrane, 2000; Sundström & Duvander, 2002). For example, housework is often perceived as burdensome, whereas childcare is viewed as more emotionally rewarding (Schober & Zoch, 2015; Sullivan, 2013). As a result, the effects may differ across the two. For instance, as fathers increase their unpaid work at home due to changes in bargaining power, they may still avoid housework but engage more in childcare (Cooke, 2006; Craig & Mullan, 2011).

In addition to these rationalist explanations, research on parental leave has also drawn on constructivist approaches (Bittman et al., 2003; Boll & Nikolka, 2024; Reich, 2010; Schober, 2014; Schober & Zoch, 2015; Trappe, 2013). These emphasize that behavior is also shaped by gender norms and social expectations (Geisler & Kreyenfeld, 2011). From this perspective, individuals "do gender" in everyday activities by performing tasks in line with socially expected roles for mothers and fathers (West & Zimmermann, 1987). In addition to setting economic incentives, parental leave policies may influence individual behavior by signaling cultural ideals about appropriate caregiving roles (Geisler & Kreyenfeld, 2011). Depending on how they are framed and implemented, these policies can either reinforce or challenge traditional divisions of labor (Pfau-Effinger, 2017; Schober & Büchau, 2022).

Evidence on the effects of parental leave reforms on parents' time spent in unpaid care and domestic work is limited and mixed. Most studies focus on reforms that introduced reserved leave months for fathers and then primarily compare changes in fathers' hours in care work. To measure domestic work, these studies typically rely on survey data on reported childcare and housework hours (e.g., Schober, 2014; Schober & Zoch, 2015; Tamm, 2018) or time diaries (e.g., Bittman et al., 2003; Hook, 2006, 2010). Several studies find that reserving leave for fathers increases their involvement in childcare and reduces mothers' time in housework (Hook, 2006, 2010; Kotsadam & Finseraas, 2011; Nepomnyaschy & Waldfogel, 2007). In Germany, Schober (2014) shows that fathers reported spending more time on childcare but not on housework following the introduction of the "daddy months" in the 2007 Elterngeld reform. However, the reform did not change mothers' childcare or household work hours. Similarly, evidence from Sweden suggests no change in household tasks such as caring for sick children after the introduction of a similar reform (Ekberg et al., 2013).

3.3 Hypotheses

Based on the theoretical mechanisms and empirical findings discussed above, I formulate the following hypotheses on how the ElterngeldPlus reform is expected to influence mothers' and fathers' labor supply, as well as their time spent in unpaid care and household work.

Labor supply

First, in line with previous research, I expect the reform to have stronger effects on mothers' labor supply than on fathers'. Unlike the previous scheme, which implicitly encouraged a full withdrawal from the labor market force during the benefit period, ElterngeldPlus incentivizes parents to instead return earlier at reduced hours by offering part-time working parents higher total benefits paid over a longer period. Instead of exiting the labor force entirely, the reform may encourage mothers to return to work shortly after childbirth, and as a result, strengthen their labor market attachment both in the short and long run. Consistent with the theoretical argument above, shorter interruptions help maintain labor market ties, preserve human capital and support sustained employment. Therefore, I expect that mothers under the new scheme are more likely to remain employed both shortly after and in the following years compared to mothers under the previous scheme (Hypothesis 1). For fathers, I expect no meaningful change in their likelihood of employment (Hypothesis 2).

Even if the reform increases maternal employment at the extensive margin, its effects on working hours are less clear. For mothers who would otherwise have exited the labor force, the new reform may encourage part-time work, and thus, increase their total hours worked. For those who would have returned to full-time employment, the more favorable treatment of part-time work may instead create an incentive to reduce hours and remain part-time for a longer period. Overall, I expect the net effect to be an increase in average maternal working hours, at least in the first two years after childbirth when benefits are paid (Hypothesis 3). However, whether this lasts beyond the two first years, after the leave period, is uncertain. On the one hand, early reentry may help prevent skill depreciation and strengthen long-term labor force attachment. On the other hand, the extension of benefit duration from twelve to 24 months and the financial attractiveness of part-time work may also lead some mothers to continue working part-time longer, creating a "part-time trap" (Zimmert & Zimmert, 2024).

Although I expect no change in paternal employment, the PartnershipBonus, which grants four additional months of benefits if both parents work part-time simultaneously, may incentivize some fathers to reduce their working hours temporarily in the first years following childbirth, however I expect this effect to be weak (Hypothesis 4).

In sum, my hypothesis on parents' labor supply outcomes are:

Hypothesis 1. *Mothers under the new ElterngeldPlus scheme are more likely to be employed after childbirth than mothers under the previous scheme.*

Hypothesis 2. *Fathers' employment will not be affected under the new ElterngeldPlus scheme.*

Hypothesis 3. *Mothers under the new ElterngeldPlus scheme will work more hours per week in the first two years after childbirth than mothers under the previous scheme.*

Hypothesis 4. *Fathers under the ElterngeldPlus scheme will work slightly fewer hours per week in the first two years after childbirth than fathers under the previous scheme.*

Domestic work

If, as hypothesized, maternal employment and working hours increase, mothers may gain greater relative bargaining power—both in terms of economic resources and time availability. Based on bargaining theory, these shifts should lead to a reduction in their hours spent on unpaid care work and an increase in fathers' involvement in domestic tasks, at least temporarily. Further, beyond these economic mechanisms, the 2015 reform may have also influenced caregiving behavior by changing cultural expectations. The reform marked an important turning point in German family policy and was publicly framed as a measure to improve the compatibility of work and family life and promote more gender-equal caregiving (Geyer & Krause, 2016). Constructivist approaches suggest that policy changes can also affect individual behavior by shaping ideals of parenthood and gendered assumptions about appropriate caregiving roles (Geisler & Kreyenfeld, 2011).

Based on these mechanisms, I expect a small reduction in mothers' time spent on domestic responsibilities in the first two years after childbirth (Hypothesis 5), alongside a slight increase in fathers' involvement (Hypothesis 6). However, as discussed earlier, the effects of leave reform likely differ across types of unpaid work. Even if mothers reduce their overall time spent on care and fathers take on a greater share, these changes may mainly affect childcare rather than housework. Therefore, I expect more pronounced effects on childcare hours, and little to no change in housework hours for both parents (Hypothesis 7).

My hypotheses on parents' domestic work outcomes are as follows:

Hypothesis 5. *Mothers under the new ElterngeldPlus scheme will spend slightly less time on unpaid domestic work in the first two years after childbirth than mothers under the previous scheme.*

Hypothesis 6. *Fathers under the new ElterngeldPlus scheme will spend slightly more time on unpaid domestic work in the first two years after childbirth than fathers under the previous scheme.*

Hypothesis 7. *ElterngeldPlus will primarily affect time spent on childcare, while having little to no effect on time spent on housework.*

4. Data and methodology

4.1 Data source

The analysis is based on survey data from the German Socio-Economic Panel (SOEP), a representative panel study of private households (Goebel et al., 2019).[15] Starting in 1984 in West Germany and expanded to East German households in 1990, the SOEP currently surveys around 15,000 households and 30,000 individuals annually (Goebel et al., 2019). Since my focus is on the effects of the 2015 parental leave reform, I use a subsample of first-time parents whose first child was born between 2014 and 2015. My main analysis uses SOEP waves 2011 through 2018.[16] The SOEP offers several advantages for this analysis. First, because the same individuals are interviewed annually, the data allow me to track respondents over time. Annual attrition rates in the SOEP are moderate, averaging around 9% (Kroh et al., 2018). Second, it provides detailed information on fertility histories of adult respondents, which allows me to identify first-time parents eligible for the new policy, as described in the following section. Third, the SOEP covers a wide range of demographic, socio-economic and behavioral measures. Crucially, the survey collects detailed data on employment measures and time spent on childcare and housework, making it particularly suitable for analyzing changes in both paid and unpaid work for parents. The next section outlines the sample selection process in more detail and introduces the variables used in the analysis.

4.1.1 Sample selection

My main analysis is based on an unbalanced panel of first-time parents whose first child was born between 2014 and 2015 (i.e., January 2014 through December 2015). First-time parents are identified using the BIOBIRTH dataset, which documents the birth biographies of SOEP respondents, including the sum, year of birth, and sex of biological children of adult respondents.[17] Identified individuals are then merged with other SOEP datasets using their personal identifiers. I illustrate the construction process of the final dataset in Figure A2.

Several additional sample selection criteria as well as limitations are worth nothing. First, following the approach of prior research on the effects of leave policies on parental outcomes (e.g., Baertsch & Sandner, 2023; Bergemann & Riphahn, 2010b; Cygan-Rehm et al., 2018; Frodermann et al., 2020), I focus exclusively on first-time parents. The birth of a first child is seen as a critical life event that brings about major shifts in employment and care responsibilities (Baxter et al., 2015). These shifts are especially pronounced for mothers, who tend to experience the most significant and persistent labor market effects compared to fathers after the birth of the

[15] I use SOEP-Core, version 39, which contains data for years 1984–2022. The SOEP data is supplied by the German Institute for Economic Research (DIW Berlin). For more detailed information, see Wagner et al. (2007).

[16] For the pre-trends analysis and robustness tests, I additionally use waves 2008–2010.

[17] BIOBIRTH is one of the generated datasets prepared by the SOEP team. These datasets are subject to additional plausibility and quality checks, making them both user-friendly and highly reliable.

first child (Tamm, 2019). First-time parents are expected to respond more strongly to parental leave reforms, whereas those with higher-order births may already have adjusted their labor supply and are less likely to change behavior (Huebener et al., 2024).

Second, to ensure a sufficient sample size while mitigating confounding, I restrict the main analysis to parents whose first child was born within six months before and after the reform's cut-off. A narrower window (e.g., three months before and after the reform) could reduce potential confounding but would sharply reduce the number of observations. The number of new parents with births immediately before and after the reform is already limited in the SOEP, and information on key variables is often missing. For this reason, the main analysis is based on a six-month window around the reform. I further exclude parents who do not have at least one observation in the relevant outcome variables both in the year before and the year after the birth. This ensures that all individuals in the main analysis are observed across a minimally balanced time frame around childbirth. Due to missing data in the SOEP, it is not possible to conduct the analysis with a strongly balanced panel.[18] Finally, to avoid later policy exposure in the control group, it would be ideal to exclude parents who had a higher-order birth after their first birth and may have become eligible for the new policy at a later point. However, further restricting the sample would again result in a severe loss of observations.[19] For this reason, I keep all first-time parents in the sample and acknowledge this is a potential source of bias. While this decision increases statistical power, it may bias the results in my analysis. I address these limitations in the discussion of the results.

Table 2 presents descriptive statistics for mothers and fathers in the four comparison groups, measured in the survey year prior to birth. Columns (1)–(5) report means for the full sample and each group. Standard deviations are omitted here for space reasons but are reported in Table A3 in the Appendix. Overall, I observe 102 first-time parents who gave birth between January and June 2015, and 128 parents between July and December 2015. In the year prior to the reform, I observe 110 individuals who gave birth between January and June 2014, and 120 individuals in the second half of 2014. This results in a total of 332 parents under the old regime and 128 parents under the new regime. Mothers in the sample were on average 28.5 years old, 42% had a migration background, and 16% lived in East Germany. About 70% reported having a partner, 40% reported having a high level of education, and roughly 83% were employed in the year before birth. Fathers were on average 32 years old, 34% had a migration background, and 14% resided in East Germany. Around 85% of fathers had a partner, 92% were employed, and 41% reported having a high level of education.[20]

[18] Balancing the panel so that each individual has data on all key variables in every year of the observation window reduced the sample size to fewer than 100 parents (including both mothers and fathers).

[19] I tested excluding individuals who had second or higher-order births within three years following the birth of their first child and found that this substantially reduced the already limited sample size.

[20] Education is coded as follows: low (less than high school), average (high school, i.e., upper secondary school, apprenticeship, specialized vocational school), and high (more than high school). Migration background is a binary variable equal to 1 if the respondent has either a direct or indirect migration background, and 0 otherwise. Partner status is coded as 1 if the respondent reports having a partner, spouse, or registered partner, and 0 if no partner is reported.

Table 2. Descriptive statistics

		Placebo cohort with births between		Reform cohort with births between		
	All	Jan–Jun 14	Jul–Dec 14	Jan–Jun 15	Jul–Dec 15	DID
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Pre-birth characteristics: mothers						
Age in years	28.525	29.328	28.111	28.320	28.377	1.273
						(1.450)
East Germany	0.160	0.190	0.108	0.196	0.159	0.045
						(0.097)
Migration background	0.420	0.379	0.508	0.333	0.435	-0.027
						(0.127)
Partner	0.696	0.704	0.726	0.780	0.596	-0.206
						(0.126)
Low-educated	0.126	0.109	0.125	0.024	0.218	0.177*
						(0.089)
Average-educated	0.475	0.543	0.536	0.415	0.400	-0.007
						(0.143)
High-educated	0.399	0.348	0.339	0.561	0.382	-0.171
						(0.225)
Employed	0.834	0.896	0.817	0.844	0.793	0.028
						(0.104)
Number of individuals	243	58	65	51	69	
Panel B. Pre-birth characteristics: fathers						
Age in years	32.210	31.962	32.037	31.900	32.862	0.887
						(1.759)
East Germany	0.144	0.216	0.073	0.118	0.169	0.195**
						(0.096)
Migration background	0.336	0.365	0.327	0.255	0.390	0.173
						(0.129)
Partner	0.854	0.930	0.824	0.846	0.822	0.083
						(0.106)
Low-educated	0.127	0.047	0.104	0.154	0.209	-0.002
						(0.102)
Average-educated	0.468	0.605	0.417	0.385	0.465	0.268*
						(0.152)
High-educated	0.405	0.349	0.479	0.462	0.326	-0.266*
						(0.150)
Employed	0.916	0.932	0.900	0.875	0.956	0.112
						(0.084)
Number of individuals	217	52	55	51	59	

Note. This table reports characteristics of mothers and fathers in the survey year before the birth of their first child. Columns (1)–(5) show means for the total sample and by subgroup. Columns (2)–(3) refer to the Placebo cohort (births in 2014), Columns (4)–(5) to the Reform cohort (births in 2015). Column (6) reports estimates from a difference-in-differences specification in (2). Robust standard errors are reported in parentheses. Due to missing values, categorical variables such as education do not always sum to one. Standard deviations are omitted here for space reasons and reported in full in Table A3. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4.1.2 Measures

My main outcomes of interest are parent's allocation of time across the three domains paid work, housework, and childcare. These dependent variables are drawn from the raw individual questionnaire of the SOEP. To analyze the reforms' effects on parent's labor supply, I use self-reported actual weekly working hours as well as employment status as my primary measures of paid work. The latter is a binary measure which equals one if the parent is employed.[21] For unpaid work, I rely on the SOEP's time-use question that asks respondents how many hours they typically spend on childcare and housework on a weekday. The question reads: "How many hours do you spend on the following activities on a typical weekday?", with listed categories including "housework (washing, cooking, cleaning)" and "childcare" (no specific activities are defined) (SOEP Group, 2024). To ensure comparability with weekly working hours, I recode the childcare and housework variables to a weekly basis by multiplying reported daily hours by five, assuming a five-day workweek. Hours worked and hours spent on childcare and housework are measured and reported in levels. Although a logarithmic transformation would lead to more interpretable results, the high prevalence of zeros in my outcome variables makes this rather unsuitable.

Due to sample size limitations and missing data, my baseline models are estimated without additional control variables. In robustness tests, I include several time-varying demographic covariates shown to be associated with differences in time use. Following previous research (e.g., Bünning, 2015; Schober, 2014; Wanger & Zapf, 2022), I control for education level (coded as 1 = low, 2 = medium, 3 = high) and marital status (1 = partnered, 0 = otherwise). I also include a binary indicator for the parent's region of residence, distinguishing between East- and West-Germany in each survey year. This is particularly important given the historical differences in family policy. Before the reunification in 1990, West German policy typically supported the traditional male breadwinner model, while East German policy encouraged maternal employment and provided extensive public childcare (Schober & Spiess, 2015). These differences are still visible today. For example, even today, East German mothers tend to work substantially more hours than those in the West (Boll & Lagemann, 2019).

4.2 Empirical strategy

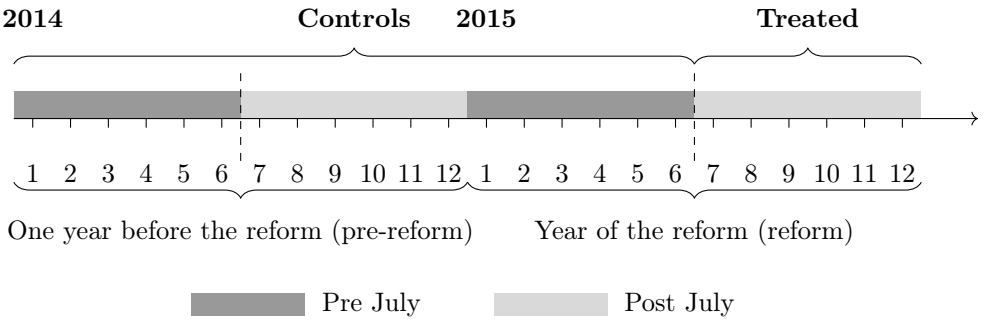
To study the causal effect of ElterngeldPlus on mothers' and fathers' paid and unpaid work outcomes, I exploit the quasi-experimental setting of the largely unanticipated policy reform. Under the new ElterngeldPlus scheme, parents qualified for the new benefit if their child was born after July 2015. Following the approach of Huebener et al. (2024), I apply a dynamic difference-in-differences (DID) design that compares outcomes of first-time parents who gave birth before July 2015 (control group) to those who gave birth after July 2015 (treatment group) in several

[21] According to SOEP documentation, individuals categorized as "employed" include not only full- and part-time workers, but also the self-employed, those in short-time work, vocational training, and marginal employment.

periods after birth.[22] To account for general time trends and potential seasonal effects, I add a second difference by including parents who gave birth in the same calendar months of the previous year, that is in 2014 where no reform took place. In addition, I follow an event-study setup and examine how treatment effects evolve over time, relative to the year before childbirth.

Figure 1 visualizes the definition of treatment and control group. Specifically, I compare outcomes between (i) parents with first births from July to December and January to June, (ii) parents with births occurring from January 2014 to December 2014 and January 2015 to December 2015, and (iii) at multiple points in time before and after childbirth.

Figure 1. Depiction of treatment and control groups



Note. This figure illustrates the definition of treatment and control group used in the main analysis. Figure design inspired by Frodermann et al. (2020). Own illustration.

Thus, my identification strategy combines a DID framework with a discontinuity design, where treatment is determined by a strict eligibility threshold. Because actual leave take-up is not observed in my data, I estimate the reform's intention-to-treat (ITT) effect. This approach follows previous studies on parental leave reforms. These similarly exploit the fact that some parents are not eligible for the benefit and estimate causal effects using a combination DID and discontinuity designs.[23] However, in contrast to many earlier studies that rely primarily on cross-sectional data and on the canonical DID setting with two time periods, I take advantage of the panel structure of the SOEP and implement a dynamic DID (event-study) design.

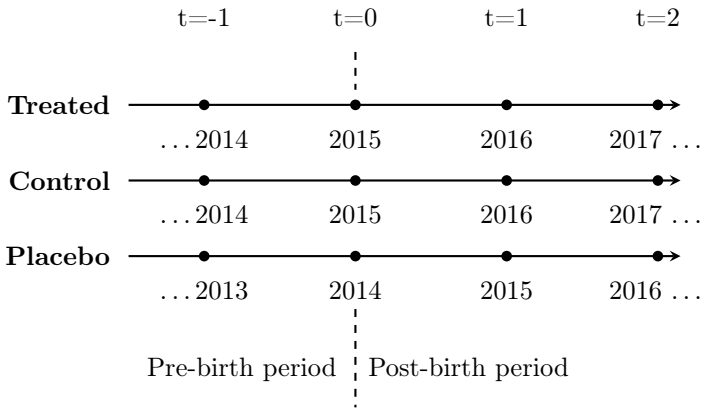
4.2.1 Dynamic difference-in-differences

Alongside being able to estimate dynamic treatment effects, another key advantage to this design is that it allows for variation in first birth timing across individuals. For each parent

[22] In their paper, Huebener et al. (2024) analyze the effect of the 2007 parental leave reform on firms' employment, wages and likelihood of shutdown.

[23] See also Lalive and Zweimüller (2009) on Austria's parental leave reform, as well as related work on Sweden's "Daddy Months" (e.g., Ekberg et al., 2013), and analyses of the prior Elterngeld reform by Cygan-Rehm et al. (2018), Dustmann and Schönberg (2012), and Frodermann et al. (2020) as well as the analysis of Baertsch and Sandner (2023) on the same reform.

Figure 2. Timeline of treatment, control, and placebo cohorts



Note. Own illustration.

in the sample, the year of the first child's birth is defined as the event year ($t = 0$), and all other years are indexed relative to this year (see Figure 2 above). Thus, treatment is staggered, as individuals enter treatment at different points in time, depending on when the first child was born (Sun & Abraham, 2021). One limitation, however, arises from the fact that SOEP interviews are conducted annually. As a result, some parents are interviewed in a month before their child is actually born. To address this and avoid misclassification of such cases, I adjust the event year variable accordingly: if the interview in the birth occurs before the month of childbirth, the survey year is assigned to $t = -1$ instead of $t = 0$.[24] This ensures consistency in how pre- and post-birth periods are defined across individuals.

In my main analysis, parents are observed annually from three years before to three years after the birth of their first child, so event time t runs from –3 to +3. Formally, I estimate the effects of the parental leave reform on yearly outcomes using the following individual fixed effects (FE) model, estimated separately for mothers and fathers:

$$\begin{aligned} Y_{it} = & \sum_{\substack{t=-3 \\ t\neq -1}}^{3} \lambda_t (T_t \times \mathit{reform}_i \times \mathit{postjuly}_i) + \sum_{\substack{t=-3 \\ t\neq -1}}^{3} \rho_t (T_t \times \mathit{reform}_i) \\ & + \sum_{\substack{t=-3 \\ t\neq -1}}^{3} \tau_t (T_t \times \mathit{postjuly}_i) + \sum_{\substack{t=-3 \\ t\neq -1}}^{3} \delta_t T_t + \gamma_t + \beta_{it} + \alpha_i + \varepsilon_{it}, \end{aligned} \tag{1}$$

where Y_{it} is the outcome of interest (e.g., weekly hours spent on paid work, childcare, or housework) for parent i in survey year t. The variable reform_i is a binary indicator equal to 1 if a child was born in 2015 (the year of the policy reform), and $\mathit{postjuly}_i$ equals 1 if a child was born after July. I omit the event-time dummy at $t = -1$, so that the year before childbirth

[24] This adjustment avoids bias in outcome. For example, a mother who gave birth in June 2015 but was interviewed earlier in March would report zero hours of childcare, whereas another mother interviewed in August after a June birth would already report increased childcare time. Without this correction, such cases would bias the analysis of birth-related effects.

serves as the reference category. Under my identifying assumptions, the coefficient λ_t of the three-way interaction term $\left(T_t \times \textit{reform}_i \times \textit{postjuly}_i\right)$ estimates the ITT effect of the reform in each year t, relative to the baseline year before childbirth.[25] This triple difference captures the reform's impact by comparing changes in outcomes for parents giving birth shortly before versus after the cutoff (July 2015), against the same difference in a year without the reform (2014). I include survey year and interview month FE, denoted by γ_t and β_{it}, respectively, to account for time trends and within-year seasonality. Since the FE model compares individuals over time, it isolates within-person changes. Thus, individual FE, denoted by α_i, absorb all time-invariant observed and unobserved characteristics. ε_{it} denotes the error term. Standard errors are clustered at the individual level.

As discussed above, I estimate my main models without time-varying covariates. In robustness tests, I rerun my models including time-varying controls.

4.2.2 Identifying assumptions

The validity of the DID approach relies on several assumptions. First, it assumes that the treatment had no causal effect before to its implementation, i.e., that parents did not anticipate the reform. In my DID setup, parents "as if" randomly assigned to a treatment or control based on a sharp eligibility cut-off. A potential concern is that parents close to the cut-off date may have delayed childbirth to qualify for the new policy. However, this seems highly unlikely. The law was passed in November 2014 (Bundestag, 2014)—a period shorter than the typical nine-month gestation period, and also shorter than the three to six months average conception period even for fertile couples (Baertsch & Sandner, 2023; González, 2013). To further validate my identification strategy, I check whether control and treatment groups differ in their pre-birth characteristics within my DID framework. Specifically, I estimate the following regression model:

$$x_i = \beta_0 + \beta_1 \textit{reform}_i + \beta_2 \textit{postjuly}_i + \beta_3 \textit{reform}_i \times \textit{postjuly}_i + \varepsilon_{it}, \tag{2}$$

where x_i represents pre-birth characteristics of mother or father i. The variable $\textit{reform}_i$ is a binary indicator equal to one if a birth occurs between January and December 2015, and $\textit{postjuly}_i$ is a binary indicator equal to one if a birth occurs between July and December. The coefficient on the interaction term captures any covariate imbalances for parents in the treatment group under the new leave reform.

The results are reported in column (9) of Table 2. The groups appear overall balanced. Among mothers, the share of low-educated individuals is significantly higher in the treatment group. Among fathers, there are some statistically significant yet small differences: the share residing in East Germany and the share with average education are both higher in the treatment group, while the share of low-educated fathers is lower in the treatment group. Given the small sample

[25] Since $\textit{reform}_i$ and $\textit{postjuly}_i$ do not vary over time, their main effects as well as the two-way interaction $\left(T_t \times \text{reform}_i\right)$ are fully absorbed by the individual FE (α_i). Similarly, the event time dummies $\left(T_t\right)$ are absorbed by the year FE γ_t. Thus, the model identifies the effect of the triple interaction term $\left(T_t \times \textit{reform}_i \times \textit{postjuly}_i\right)$.

size and missing data, such imbalances are not unexpected and likely reflect random occurrences. Still, this emphasizes caution when interpreting results. In robustness tests, I run alternative specifications and include time-varying covariates.

The second critical assumption is the parallel trends assumption, which states that, in absence of the reform, potential outcomes for treatment and control groups would have followed similar trends (Angrist & Pischke, 2009). To assess this, I compare outcome trends between treatment and control groups in years prior to the birth of the first child. Figure 3 shows trends in weekly hours worked, as well as time spent on childcare and housework. Due to space constraints, pre-trends for employment are reported in the Appendix (see Figure A1). Among mothers, pre-trends appear largely parallel across all four outcomes. For fathers, the parallel trends are less consistent, especially regarding paid work and housework. These patterns are likely influenced by a small number of outliers and suggest that estimates should be interpreted with additional caution. This also reflects one of the main limitations of the analysis, which is the small sample size and the presence of missing data. I address this when discussing limitations of my analysis.

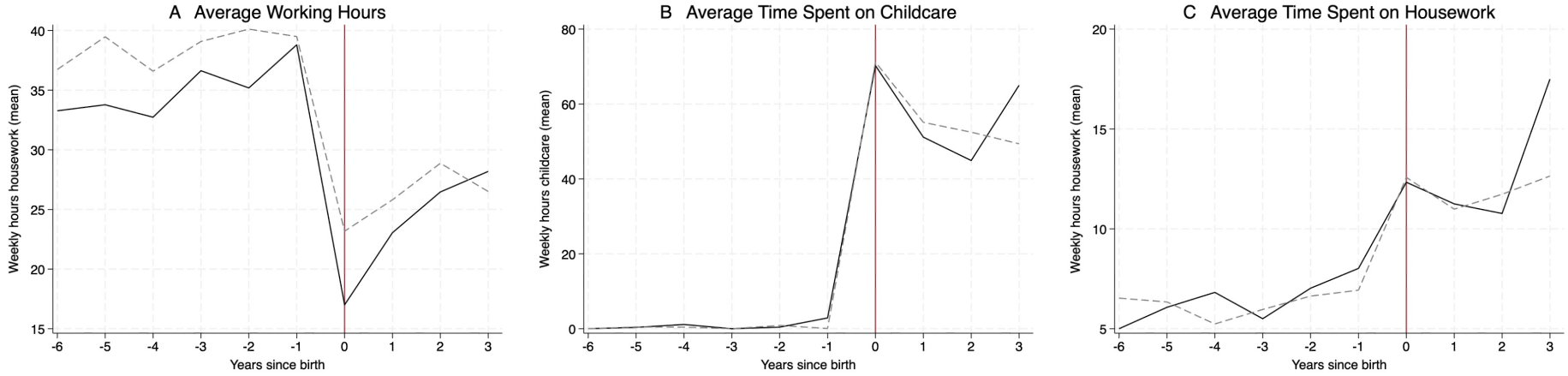
Overall, despite some minor imbalances, the DID approach remains credible in this setting. Given both its methodological advantages and the data limitations discussed above, I use the DID design as my preferred method for estimating treatment effects. While the sharp eligibility cut-off could, in principle, motivate an RD design, the small sample size and high rates of missing data for key covariates pose serious limitations. For example, narrowing the sample to observations closer to the cut-off would substantially reduce statistical power, while using a wider bandwidth would require conditioning on covariates that are not consistently observed in the data.[26] By contrast, the DID methodology allows me to take advantage of the full panel structure of the SOEP and retain a larger sample of observations, including individuals further away from the threshold. It also controls for both fixed observed and unobserved differences between treatment and control group, which is a major advantage over RD. Moreover, the DID design allows me to isolate seasonal effects by comparing parents with births in the same months of the preceding year.[27]

[26]When controlling for time-constant covariates in a RD specification, I observed a substantial loss of observations due to listwise deletion. This further reduced statistical power, which is already limited by the small sample of first-time parents in my dataset.

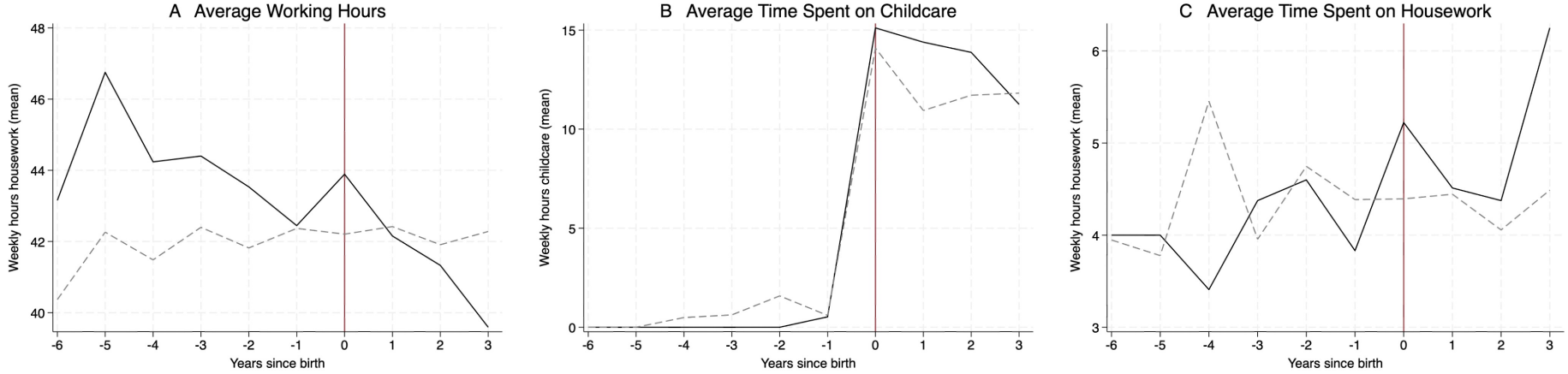
[27]When running a DID comparing only January-June 2015 vs. July-December 2015 births, results were substantially stronger, showing that seasonality is indeed a problem.

Figure 3. Pre-trends in time use

Panel A. Mothers



Panel B. Fathers



Note. This figure shows average weekly hours allocated to paid work, childcare, and housework from six years before to three years after the birth of the first child. *Panel A* presents outcomes for mothers; *Panel B* for fathers. The solid line represents the treated group (birth between July–December 2015), and the dashed line represents the control group (placebo and reform cohorts, i.e., birth between January 2014–June 2015). Time 0 denotes the year of birth. Each data point represents the mean across individuals observed in that time window.

5. Results

5.1 Employment and working hours

I first examine the effects of the reform on mothers' and fathers' labor supply. Table 3 presents the results from estimating equation (1), using the baseline specification which does not include time-varying sociodemographic controls. It reports separate results for mothers (columns 1–2) and fathers (columns 3–4). Columns (1) and (3) present estimates for the binary employment indicator, while columns (2) and (4) show estimates for weekly hours worked. Each coefficient corresponds to a specific event time t, with $t = -1$ (the year before birth) serving as the reference period. Consequently, each coefficient reflects the change in outcomes in between treatment and control groups in event year t, relative to this baseline. Given that parents' observed characteristics are largely balanced, and that they followed similar pre-trends, I interpret the post-treatment coefficients as the causal effects of the reform. This is further supported by the fact that almost all pre-treatment coefficients are small and statistically insignificant, indicating that mothers and fathers who gave birth just before or just after July 1, 2015, showed similar labor supply patterns in years before birth. One exception is a significant negative coefficient for fathers' employment status at $t = -3$ and $t = -2$. Although this difference is likely driven by random variation, I interpret all subsequent estimates with caution.

First, examining maternal employment in column (1), my results indicate that mothers who became eligible for ElterngeldPlus are significantly more likely to be employed in the second and third years after childbirth. The point estimate at $t = 2$ is 0.28 ($p < 0.10$) and at $t = 3$ is 0.53 ($p < 0.10$). Despite only being marginally statistically significant, these differences of 28 and 53 percentage points seem unusually high. Consequently, these estimates should be interpreted with caution, as they may be driven by outliers or sample noise. Coefficients in earlier periods, at $t = 0$ and $t = 1$, are also positive but statistically insignificant. Overall, the results somewhat support the hypothesis that ElterngeldPlus strengthened mothers' labor market attachment in the years following childbirth but need to be treated with caution. Turning to fathers' employment in column (3), no coefficient for any post-birth year is statistically different from zero. Across all years, the estimates are small and statistically insignificant, implying that the reform did not affect fathers' probability of employment. In other words, I find no evidence that the extension and part-time incentives of ElterngeldPlus induced fathers to exit the labor force at higher rates than the control group.

The results for mothers' weekly hours worked are reported in column (2). Here, the point estimates for $t = 0$ and $t = 1$ are slightly positive but are not statistically significant. Interestingly, the coefficient in the third year after childbirth turns negative, suggesting an (insignificant) reduction in hours once benefit eligibility ends. Because none of these coefficients are statistically significant, I cannot confirm any systemic effect of ElterngeldPlus on mothers' hours worked. Similarly, fathers' weekly working hours in column (4) decrease from the second year onward, but again without statistical significance. The results therefore do not support a robust decrease

in fathers' weekly hours following the introduction of ElterngeldPlus.

In summary, the baseline estimates provide some evidence that mothers under ElterngeldPlus become more likely to be employed in two and three years after childbirth, whereas fathers' employment is unaffected. At the same time, neither mothers' or fathers' weekly hours worked show significant changes in any post-birth year. Section 5.3 presents robustness checks using alternative specifications to assess whether these findings hold under different models.

Table 3. Effects of ElterngeldPlus on employment and working hours

	Mothers		**Fathers**	
	Employment	Working hours	Employment	Working hours
	(1)	(2)	(3)	(4)
$t = -3$	-0.079	1.024	-0.229**	-3.616
	(0.133)	(3.796)	(0.114)	(3.462)
$t = -2$	0.020	2.729	-0.194*	-1.628
	(0.123)	(2.976)	(0.109)	(2.969)
$t = 0$	-0.201	-0.563	0.0278	-1.733
	(0.146)	(10.387)	(0.0967)	(2.841)
$t = 1$	0.063	2.401	0.00531	2.804
	(0.157)	(4.907)	(0.0833)	(2.538)
$t = 2$	0.283*	0.0117	-0.0235	-2.012
	(0.169)	(5.214)	(0.0731)	(2.287)
$t = 3$	0.533*	-2.12	-0.0122	-1.423
	(0.316)	(5.996)	(0.114)	(3.628)
Intercept	0.859***	41.70***	0.591***	36.56***
	(0.264)	(5.511)	(0.169)	(5.895)
Observations	1,323	771	1,071	983
R^2	0.385	0.453	0.062	0.038
# of individuals	234	207	193	191

Note. Table reports parameter estimates of the three-way interaction $T_t \times postjuly_i \times treat_i$ from estimating Equation (1). $t = -1$ is the omitted reference category. Year dummies and interview month dummies are included. Robust standard errors, clustered at the individual level, are shown in parentheses. Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

5.2 Childcare and housework hours

Next, I look at the effects of the reform on parents' weekly time spent on childcare and housework for mothers and fathers, testing Hypotheses 5–7. Table 4 mirrors the structure of Table 3, but

now reports the event-study estimates for childcare hours (Columns 1 and 3) and housework hours (Columns 2 and 4), separately for mothers and fathers. Each coefficient again corresponds to an event year t (from -3 to $+3$), with $t = -1$ as the omitted reference period. All pre-treatment coefficients are statistically insignificant.

Table 4. Effects of ElterngeldPlus on childcare and housework hours

	Mothers		**Fathers**	
	Childcare hours (1)	Housework hours (2)	Childcare hours (3)	Housework hours (4)
$t = -3$	4.287	0.612	-1.225	0.506
	(3.808)	(1.774)	(1.912)	(1.199)
$t = -2$	-3.625	1.572	0.640	-0.486
	(4.157)	(1.724)	(2.955)	(1.285)
$t = 0$	-6.378	2.444	3.978	0.819
	(10.51)	(2.229)	(4.445)	(1.288)
$t = 1$	-12.44	0.356	0.121	-0.0490
	(11.37)	(1.805)	(3.141)	(1.395)
$t = 2$	-24.27**	-1.364	3.118	0.487
	(12.11)	(2.168)	(3.956)	(1.410)
$t = 3$	19.96	0.249	-8.861	0.704
	(36.54)	(2.396)	(8.562)	(2.060)
Intercept	15.88	10.84***	-9.900**	-0.173
	(11.3)	(3.301)	(4.394)	(2.917)
Observations	1,249	1,265	1,009	1,016
R^2	0.653	0.221	0.413	0.050
# of individuals	233	234	192	192

Note. Table reports parameter estimates of the three-way interaction $T_t \times postjuly_i \times treat_i$ from estimating equation (1). $t = -1$ is the omitted reference category. Year dummies and interview month dummies are included. Robust standard errors, clustered at the individual level, are shown in parentheses. Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Focusing first on childcare hours for mothers (column 1), most coefficients in the periods following childbirth are negative and in line with Hypothesis 5, which predicts that ElterngeldPlus would reduce their time spent on childcare. However, these estimates generally fail to reach statistical significance. The single most striking observation is in the second year after childbirth ($t = 2$), where I find a significant and unusually large drop of 24 hours per week. This single significant coefficient may reflect measurement error, potentially driven by a small number of mothers underreporting or overreporting their time use, because childcare hours increase sharply again in the following year ($t = 3$). An alternative interpretation is that treated mothers temporarily reduce childcare, and that care hours increase again once the ElterngeldPlus benefit period ends

after around two years. Nonetheless, because only one coefficient is significant and its magnitude is extreme, I interpret this result with caution. Turning to fathers' childcare hours (column 3), the point estimates for $t = 0$ and $t = 1$ are positive, which might suggest a temporary increase in paternal care, in line with Hypothesis 6. By the second and third year ($t = 2$ and $t = 3$), however, the coefficients become small or even negative. None of these estimates are statistically significant, implying that my results do not provide robust evidence of a sustained increase in fathers' childcare time under ElterngeldPlus.

Examining housework hours in column 2 (mothers) and column 4 (fathers), all post-birth coefficients are very close to zero and statistically significant. This finding contradicts any claim that ElterngeldPlus affected time spent on housework, leading me to conclude that parents did not reallocate their domestic responsibilities in response to the reform. If at all, any change in domestic labor appears to be confined to childcare rather than housework.

In summary, the domestic work results do not support Hypotheses 5 or 6: mothers' childcare and housework hours do not show a consistent and statistically significant decline, and fathers' outcomes do not show any significant increase. The stronger effects on childcare and the absence of any effect on housework provide some suggestive evidence for Hypothesis 7, which anticipated larger effects on childcare than on housework. Yet this remains weakly suggestive and needs to be further examined in the robustness tests that follow.

5.3 Robustness checks

Although only a few estimates in my main specifications are statistically significant, I perform several robustness checks to assess the sensitivity of my findings. The full results are reported in Tables A1-A6 in the Appendix.

First, as discussed earlier, I conduct a second placebo test by simulating a reform in 2014—a year in which no actual policy change occurred and thus, no effect should be observed. Specifically, I re-estimate equation (1) treating 2014 as the reform year and using the 2013 cohort as the placebo control group to account for seasonality. As shown in Table A4 in the Appendix, this placebo analysis produces no significant effects, with one minor exception: the coefficient for mother's housework hours in the third year after childbirth is statistically significant. However, because this effect is only observed once, I conclude that this finding is likely a random chance occurrence due to sampling. In addition, most placebo coefficients are substantially smaller than those in the baseline results. Overall, this lends credibility to the identifying assumptions and the robustness of my main findings.

Second, to account for potential confounders, I re-estimate my main models by including the time-varying control variables as described in Section 4.1.2. Reassuringly, Table A5 shows that most coefficients remain largely unchanged in both magnitude and statistical significance. This suggests that the main results do not depend on controlling for individual characteristics of mothers and fathers. Importantly, I continue to find strong and statistically significant effects on mothers' employment in the second and third years after childbirth. This is in line with my

previous finding that the reform positively affected maternal employment, albeit only from the second year onward. Likewise, the reduction in mothers' childcare hours in the second year after childbirth holds and is statistically significant. Still, its extreme magnitude implies that this needs to be interpreted with caution. Again, there is no is no corresponding increase in fathers' childcare hours, leaving the underlying mechanism for the drop I observe in mothers' care time unclear.

Finally, I estimate a static DID to complement the event study results. This specification mirrors Equation (1) but replaces the full set of event-time indicators with a single post-birth dummy, $post_{it}$, which takes the value one for all observations in the post-birth period ($t \geq 1$) and zero otherwise. By averaging over years before and after childbirth, one advantage of this specification is that it can improve precision by smoothing out the influence of extreme observations. This is especially important, given the relatively small sample size and missing data issues. Table A6 reports the static DID estimates, which broadly align with the event-study results. Specifically, mothers' employment continues to show positive and significant effects, although its magnitude is somewhat smaller than in the year-by-year analysis. On average, mothers in the treated cohort are about 18.8 percentage points more likely to be employed than mothers in the control cohort. While this difference still seems relatively large, it is not implausible but should be still be viewed cautiously. Contrary to my earlier findings, the previous observed drop in mothers' childcare hours at $t = 2$ is no longer significant under the static specification. This suggests that the effect observed in $t = 2$ may have been driven by a small number of individuals, rather than a systematic shift in time allocation.

In sum, the placebo test, the inclusion of time-varying controls, and the static DID specification all reaffirm my main finding which suggests that ElterngeldPlus increases maternal employment in the years following childbirth. At the same time, the evidence for an effect on mothers' childcare hours is less robust. I again find no evidence for changes in any of fathers' outcomes. While the robustness tests support my overall conclusions, they also highlight the need to treat my results with caution due to several limitations, which I discuss below.

6. Discussion and limitations

Having performed these analyses, I now discuss the main results in relation to the theoretical framework and hypotheses, after which I address key limitations and possible explanations.

Overall, my results provide some evidence for Hypothesis 2: ElterngeldPlus supports mothers' employment after childbirth. Moreover, as I observe no change in fathers' employment, this is in line with Hypothesis 2. However, I find no evidence to support Hypotheses 3 or 4, which anticipated changes in weekly hours worked. Similarly, there is no indication of shifts in childcare and housework time for either parent (Hypotheses 5–7). Although the negative estimates for mothers' childcare hours hint at a possible reduction, only the 24-hour drop in the second year after childbirth remains statistically significant and likely reflects outliers rather than a true causal effect. Because no other childcare and housework coefficients are significant for either parent, I cannot accept or reject Hypotheses 5 and 6. Although childcare estimates are generally larger compared to those for housework, suggesting that effects are more pronounced for childcare, their lack of statistical significance means I cannot confirm Hypothesis 7.

Turning to the theoretical framework, these findings align only partially with the mechanisms described above. The positive employment effect for mothers suggests that stretching parental benefits and rewarding part-time work did strengthen mothers' labor market attachment, at least at the extensive margin. Under ElterngeldPlus's part-time options and financial incentives, some mothers appear to have returned to work earlier. Yet because I see no significant change in mothers' weekly hours, it seems likely that many may have chosen fewer hours by opting for part-time instead of full-time work. A comparable shift occurred in France, where Joseph et al. (2013) found that mothers reduced their working hours after a part-time parental leave option was introduced. However, since my estimates on hours worked are neither significantly positive nor negative, I cannot confirm whether the same holds here or there are other mechanisms at play. My results for fathers also align with the theoretical framework presented and prior studies on parental leave reforms, which often find minimal labor supply responses from men. Although I expected at least a modest decrease in fathers' weekly hours worked, given the new financial incentives under ElterngeldPlus to qualify for extended benefits, I observe no statistically significant change. This finding could indicate that paternal labor supply decisions are less sensitive to such incentives. At the same time, the lack of significant results may also simply reflect that any adjustments are too small for my analysis to detect.

Further, time-availability and resource-bargaining models predict that if mothers return to work sooner and earn income, they will reduce their unpaid care work, whereas fathers will assume a greater share. Yet my analyses provide only weak evidence of a temporary reduction in mothers' childcare hours, and, somewhat disappointingly, no corresponding increase in either childcare or housework for fathers. On the one hand, this suggests that any increase in mothers' bargaining power did not translate into a reallocation of domestic tasks. On the other hand, given the lack of significant changes in parents' labor supply to begin with, perhaps neither time availability nor bargaining power changed enough to trigger a measurable redistribution of

unpaid work. This makes it difficult to assess whether the underlying theoretical assumptions hold. From a constructivist perspective, I had hoped that ElterngeldPlus would help signal new norms by explicitly incentivizing part-time and dual caregiving. Instead, the reform itself may have been too modest, or my analysis may simply have been too limited to detect any effects. Despite finding no conclusive evidence on parents' domestic work outcomes, the larger effects on childcare hint that, in line with theory, childcare may generally be more susceptible to change than routine tasks such as housework.

Despite some indications of a positive effect on maternal employment, the mixed and inconclusive results highlight several limitations of this thesis. First, a major constraint of my analysis is the small sample size, which results from both limited data availability in the SOEP and the need to restrict the sample to a small subset of parents. This not only reduced the statistical power of my analyses but also limited the ability to conduct additional robustness tests. For example, it was not possible to narrow the time window of births further (e.g., to three months before and after the reform) to improve comparability and reduce potential confounding. It was also not feasible to exclude births very close to the eligibility cut-off to account for potential birth-shifting, or, as previously discussed, to drop parents with higher-order births who may have become eligible during the observation period.[28] Moreover, my analysis is based on an unbalanced panel, since not all parents appear in every survey wave. Naturally, this raises concerns about potential bias if attrition or non-response is systematically related to the outcomes studied. Although I test the validity of my identifying assumptions to address these concerns, there are instances of possible violations, such as slightly diverging pre-trends in fathers' employment outcomes, and one or two significant results in the pre-treatment periods and placebo test. Crucially, another limitation arises from the fact that outcome measures for weekly hours worked, childcare and housework hours are based on self-reported data collected through annual surveys. This can introduce bias, especially since respondents in the SOEP were asked to retrospectively estimate how much time they typically spend on these activities. The use of time diaries would likely produce more accurate estimates than survey questions.

In addition, while the limitations of this analysis may partly explain the absence of significant results and the theoretical mechanisms, my findings could also indicate that the reform itself had only a minimal impact. The financial incentives may have been too weak, or take-up of the new components too low for the reform to produce measurable changes. Because I estimate ITT effects without observing actual take-up, I cannot distinguish between families who used ElterngeldPlus and those who remained under the old scheme. Sample and data constraints also prevented me from testing whether treatment effects varied among different subgroups. Yet as Geyer and Krause (2016) point out in their early discussion of the reform's potential impact, the financial incentives created by ElterngeldPlus vary depending on the distribution of both paid working hours and income between both parents prior to childbirth. Further, prior research on the 2007 Elterngeld reform already found that reported effects were stronger among groups with higher income and education (e.g., Cygan-Rehm, 2016; Frodermann et al., 2020; Geisler

[28] Prior studies often exclude parents with births 14 days before and after the eligibility cut-off. Since my data only records the month of birth rather than the exact date, I would have had to drop the entire month around the threshold. Dropping two full months, however, would have further reduced an already limited sample size.

& Kreyenfeld, 2011). Similarly, first evidence on the 2015 reform suggests that high-educated fathers were more likely to take up leave and reduce working hours (Boll & Nikolka, 2024).

Although not a limitation of my empirical design, I could not fully account for Germany's broader family policy landscape, where policies have traditionally followed a conservative, male-breadwinner model (Spiess & Wrohlich, 2008). A central, ongoing debate concerns Germany's joint income taxation, which continues to discourage married women from participating fully in the labor market participation (Steiner & Wrohlich, 2004).[29] Numerous scholars argue that existing institutions in Germany reinforce traditional gender roles and create structural barriers to a more equal division of both paid and unpaid work (e.g., Beblo, 2012; Beblo & Boll, 2014; Geyer & Krause, 2016; Huebener et al., 2016). In their forecast for ElterngeldPlus, Huebener et al. (2016) warn that unless complementary reforms are implemented, such as changes to joint taxation or expansions in public childcare, existing policies "will thwart the majority of the family goals" (p. 578). In other words, even a well-intended parental leave reform may be insufficient if these broader institutional barriers remain in place.

Taken together, these discussions imply that my findings should be interpreted with caution and underscore the need for future research to assess the reform's impact more precisely. Several avenues for future research will be discussed in the concluding chapter.

[29]In Germany, married couples are subject to a joint income tax system with full income splitting. The couple's total income is split in half, and tax is calculated on this average; the resulting amount is then doubled to determine the final tax burden. This method effectively benefits households with large income differences and discourages secondary earners—typically women—from increasing paid work (Steiner & Wrohlich, 2004).

7. Conclusion

In this thesis, I analyzed the effects of the 2015 paid parental leave reform in Germany, which aimed to promote a more gender-equal division of both paid and unpaid work. The reform increased incentives for mothers to return to work sooner after childbirth and encouraged fathers to reduce their working hours to become more involved in unpaid care work. Exploiting the quasi-experimental setting of this policy change, I applied a dynamic difference-in-differences approach using panel data from the German Socio-Economic Panel (SOEP) to analyze its effects on parents' labor market outcomes and domestic work patterns. By doing so, this study is among the first to evaluate the impact of ElterngeldPlus across a range of outcomes for both mothers and fathers. While my findings are somewhat ambiguous, they provide partial support for the reform's intended objectives. My analysis indicates that the reform may have encouraged mothers to re-enter the labor market sooner after childbirth. By extending parental benefits and making part-time work more financially attractive, ElterngeldPlus likely strengthened mothers' labor-market attachment on the extensive margin. However, despite the reform's intentions, I find no evidence that it led to significant changes in working hours for either parent. Similarly, I observe no robust shift in the division of unpaid domestic work.

Several methodological limitations suggest that these findings should be interpreted with caution. Notably, the absence of significant effects, both positive and negative, could imply that any true effect of the reform is either too small or heterogenous to detect with the available SOEP data. Since subgroup analyses were not feasible with the data at hand, future research should investigate whether the ElterngeldPlus affects parents differently, by income, educational attainment, or household type (e.g., single-earner versus dual-earner households). Access to richer (administrative) data on earnings, employment histories, and actual take-up of parental leave, could provide clearer insights into potential subgroup effects. Moreover, as the observation window in this thesis was limited to the first three years after childbirth, it remains unclear how the reform affects outcomes in the longer term. Understanding the longer-term outcomes is especially important with regard to maternal employment. If the reform encourages mothers to return to work sooner but mainly into part-time jobs, it may ultimately hinder rather than promote progress toward gender equality. Given the high rate of missing observations on outcome variables in later survey years, I was unable to investigate effects beyond the three-year period. Future studies should therefore examine longer-term impacts and consider additional employment outcomes, such as earnings, career progression, and job satisfaction to better assess the reform's effects on parents' labor market positions beyond mere employment status or hours worked.

The mixed evidence regarding domestic labor, especially mothers' childcare hours, also warrant further investigation. Time-diary data instead of retrospective survey questions, for example, could improve reliability. If the observed drop in mothers' childcare hours was true, it would be valuable to explore whether this effect persisted and what mechanisms drove it. In addition, since previous studies have generally found little impact of parental leave reforms on domestic

work, it might be worthwhile to revisit existing theoretical explanations to clearly understand how parents allocate time to unpaid care work.

Finally, future research should examine broader implications of the reform. For example, previous work on the 2007 Elterngeld reform examined fertility responses (e.g., Cygan-Rehm, 2016), or parents' well-being (e.g., Maeder, 2014). The greater flexibility offered by ElterngeldPlus may have improved family well-being by allowing more time with children and reducing stress during the leave period by extending the benefit period. It would also be interesting to explore potential effects on children's outcomes, such as early childhood development and school performance (e.g., Dahl et al., 2016; Danzer & Lavy, 2018; Dustmann & Schönberg, 2012). Taking into account both these considerations and the limitations of this analysis, this thesis highlights the need for future research to build on its findings and further investigate the effects of ElterngeldPlus. With larger samples, access to richer data, and additional resources, future work could better understand the mechanisms through which the reform operates and thus, guide policy decisions. In light of the current demographic, economic, and social challenges, designing effective policies to support balancing family life and employment, and crucially, promote women's labor force participation, is more urgent than ever.

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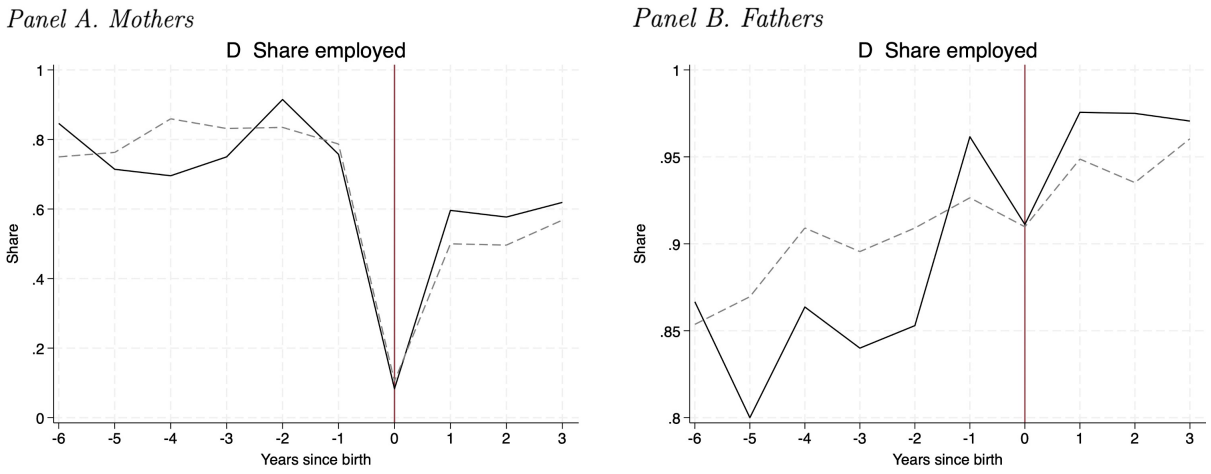
Appendix

Table A1. Comparison of benefit calculation

	Basiselterngeld (Elterngeld)	**ElterngeldPlus**
Benefit calculation	$(inc^{pre} - inc^{post}) \cdot r$	$inc^{pre} \cdot \frac{1}{2} \cdot r$ if $inc^{post} \leq \frac{1}{2} \cdot inc^{pre}$
		$(inc^{pre} - inc^{post}) \cdot r$ if $inc^{post} \geq \frac{1}{2} \cdot inc^{pre}$

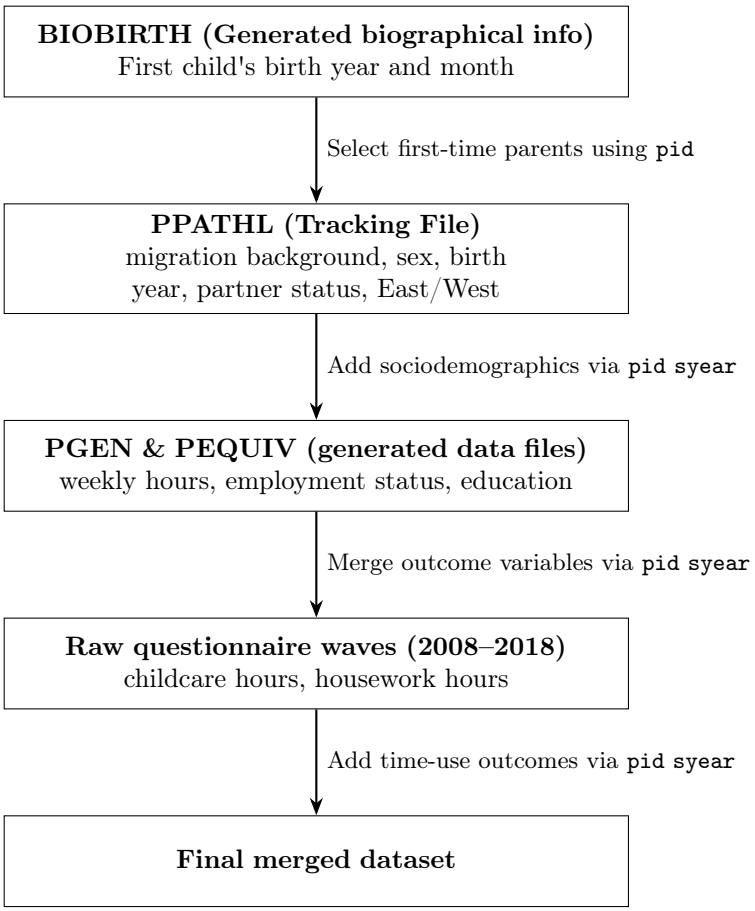
Note. This table compares the benefit formulas under Basiselterngeld and ElterngeldPlus. Under Basiselterngeld, benefits are calculated based on lost income. ElterngeldPlus modifies this formula by capping benefits at half the amount of Basiselterngeld if post-birth income exceeds 50% of pre-birth income. Adapted from Baertsch and Sandner (2023).

Figure A1. Pre-reform trends in employment



Note. This figure shows the share of parents employed from six years before to three years after the birth of the first child. Panel A presents outcomes for mothers; Panel B for fathers. The solid line represents the treated group (birth between July–December 2015), and the dashed line represents the control group (placebo and reform cohorts, i.e., birth between January 2014–June 2015). Time 0 denotes the year of birth. Each data point represents the mean across individuals observed in that time window.

Figure A2. Construction of analytical sample using SOEP files



Note. `pid` refers to the personal identifier and `syear` to the survey year in the SOEP. Own illustration.

Table A2. Key features of Elterngeld (2007) and ElterngeldPlus (2015)

	≥01/2007: Elterngeld (2007)	**≥07/2015:** ElterngeldPlus (current system)		
		Basiselterngeld (formerly *Elterngeld*)	**ElterngeldPlus**	**Partnerschaftsbonus**
Eligibility	Parent must work no more than **30 hours per week** during the benefit period	Parent must work no more than **32 hours per week** during the benefit period	Parent must work no more than **32 hours per week** during the benefit period	Both parents must work between **24 and 32 hours per week** at the same time for four consecutive months
Replacement rate	**67% to 100% of net income** (based on the 12 months before childbirth); minimum payment of €300 per month, maximum payment of €1,800 per month Rate increases by 0.1 percentage points for every €2 below €1,000, up to 100% replacement; decreases by 0.1 percentage points for every €2 above €1,200, down to a minimum of 65% replacement If working part-time after childbirth, *Elterngeld* is based on 67% (or adjusted rate) of lost income (prenatal minus postnatal earnings)	Same as *Elterngeld* (2007)	(see Table A1)	Same amount as *ElterngeldPlus*
Entitlement period	**Maximum of 12 months total**, plus 2 additional reserved months, see *Partnermonate* (partner months) below If both parents take *Elterngeld* at the same time, the total duration is reduced to 7 months (*Doppelter Anspruchsverbrauch*) *Partnermonate*: 2 additional months granted if the other parent works no more than 30 hours per week	Same as *Elterngeld* (2007)	Twice as long as *Basiselterngeld*, **maximum of 24 months total**, plus 4 additional months per parent through the *Partnerschaftsbonus*	**Maximum of 8 months total** (4 months per parent)

Note. This table summarizes the key features of the *Elterngeld* system introduced in 2007 and the revised *ElterngeldPlus* scheme introduced in 2015 according to the *Bundeselterngeld- und Elternzeitgesetz* (BEEG).

Table A3. Descriptive statistics

	All		Placebo cohort with births between Jan–Jun 14		Placebo cohort with births between Jul–Dec 14		Reform cohort with births between Jan–Jun 15		Reform cohort with births between Jul–Dec 15	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Pre-birth characteristics: mothers										
Age in years	28.525	5.636	29.328	5.437	28.111	5.023	28.320	5.393	28.377	6.492
East Germany	0.160	0.368	0.190	0.395	0.108	0.312	0.196	0.401	0.159	0.369
Migration background	0.420	0.495	0.379	0.489	0.508	0.504	0.333	0.476	0.435	0.499
Partner	0.696	0.461	0.704	0.461	0.726	0.450	0.780	0.419	0.596	0.495
Low-educated	0.126	0.333	0.109	0.315	0.125	0.334	0.024	0.156	0.218	0.417
Average-educated	0.475	0.501	0.543	0.504	0.536	0.503	0.415	0.499	0.400	0.494
High-educated	0.399	0.491	0.348	0.482	0.339	0.478	0.561	0.502	0.382	0.490
Employed	0.834	0.373	0.896	0.309	0.817	0.390	0.844	0.367	0.793	0.409
Number of individuals	243	—	58	—	65	—	51	—	69	—
Panel B. Pre-birth characteristics: fathers										
Age in years	32.210	6.431	31.962	4.806	32.037	5.687	31.900	7.234	32.862	7.631
East Germany	0.144	0.351	0.216	0.415	0.073	0.262	0.118	0.325	0.169	0.378
Migration background	0.336	0.473	0.365	0.484	0.327	0.470	0.255	0.437	0.390	0.493
Partner	0.854	0.355	0.930	0.259	0.824	0.385	0.846	0.363	0.822	0.389
Low-educated	0.127	0.333	0.047	0.213	0.104	0.306	0.154	0.365	0.209	0.409
Average-educated	0.468	0.500	0.605	0.492	0.417	0.498	0.385	0.490	0.465	0.505
High-educated	0.405	0.492	0.349	0.481	0.479	0.503	0.462	0.500	0.326	0.470
Employed	0.916	0.277	0.932	0.255	0.900	0.308	0.875	0.332	0.956	0.209
Number of individuals	217	—	52	—	55	—	51	—	59	—

Note. This table reports characteristics of mothers and fathers in the survey year before the birth of their first child. Columns (1)–(2) show the overall means and standard deviations. Columns (3)–(6) refer to the Placebo cohort; columns (7)–(10) to the Reform cohort. Odd-numbered columns show means; even-numbered columns show standard deviations. Due to missing values, categorical variables such as education do not necessarily sum to one.

Table A4. Placebo test for hypothetical reform in 2014

	Mothers				Fathers			
	Working hours	Employment	Childcare hours	Housework hours	Working hours	Employment	Childcare hours	Housework hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$t = -3$	2.970	0.198	-2.212	0.389	2.076	-0.00331	-1.398	-1.081
	(4.321)	(0.139)	(4.354)	(1.875)	(3.522)	(0.136)	(2.105)	(1.514)
$t = -2$	-1.198	0.163	-0.465	-0.382	2.600	0.118	-1.221	-0.451
	(3.343)	(0.129)	(2.951)	(1.677)	(3.661)	(0.101)	(2.803)	(1.242)
$t = 0$	-0.620	-0.0794	12.89	0.397	1.904	-0.0982	0.849	-0.399
	(10.93)	(0.146)	(11.24)	(2.233)	(3.138)	(0.102)	(4.329)	(1.434)
$t = 1$	-1.935	0.102	-5.148	0.874	-2.232	0.0625	2.241	-1.357
	(5.268)	(0.162)	(11.28)	(2.062)	(2.963)	(0.0799)	(4.148)	(1.472)
$t = 2$	-0.413	0.123	-9.817	-0.0471	1.760	0.00707	1.215	-1.814
	(5.329)	(0.174)	(12.24)	(2.351)	(2.916)	(0.0959)	(4.411)	(1.488)
$t = 3$	2.469	-0.241	5.182	5.229**	5.560	0.0622	-1.453	-2.093
	(5.973)	(0.187)	(12.80)	(2.119)	(3.491)	(0.106)	(4.472)	(1.428)
Intercept	28.47***	1.130***	7.885	3.708	42.67***	0.880***	-11.04**	7.032**
	(8.723)	(0.255)	(13.05)	(2.88)	(5.48)	(0.203)	(4.735)	(2.707)
Observations	772	1,298	1,230	1,246	926	1,015	957	962
R^2	0.411	0.384	0.637	0.207	0.040	0.068	0.374	0.066
# of individuals	199	219	218	219	171	175	174	174

Note. Table reports parameter estimates of the three-way interaction $T_t \times postjuly_i \times treat_i$ from estimating equation (1), using 2014 as the hypothetical reform year (placebo test). $t = -1$ is the omitted reference category. Year dummies and interview month dummies are included. Robust standard errors, clustered at the individual level, are shown in parentheses. Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A5. Baseline model including covariates

	Mothers				Fathers			
	Working hours	Employment	Childcare hours	Housework hours	Working hours	Employment	Childcare hours	Housework hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$t = -3$	0.0116	1.574	6.788	-0.253	-0.189*	-4.578	-1.050	0.579
	(0.127)	(3.924)	(4.284)	(1.704)	(0.0987)	(3.227)	(1.948)	(1.204)
$t = -2$	0.0411	2.823	-1.316	1.157	-0.148	-1.447	0.863	-0.263
	(0.129)	(2.883)	(4.406)	(1.744)	(0.101)	(2.798)	(3.066)	(1.361)
$t = 0$	-0.122	0.466	-4.943	1.670	0.0522	-0.920	4.772	0.492
	(0.141)	(10.69)	(10.68)	(2.274)	(0.0972)	(2.857)	(4.561)	(1.318)
$t = 1$	0.0749	3.069	-12.61	-0.359	0.00136	2.982	0.729	-0.286
	(0.162)	(4.930)	(11.67)	(1.798)	(0.0776)	(2.580)	(3.267)	(1.424)
$t = 2$	0.318*	-0.329	-23.57*	-2.504	-0.0209	-2.335	3.232	0.248
	(0.171)	(5.201)	(12.39)	(2.133)	(0.0767)	(2.323)	(4.070)	(1.433)
$t = 3$	0.556*	-2.695	25.39	-0.438	0.0451	-0.408	-9.372	0.549
	(0.323)	(5.999)	(39.18)	(2.382)	(0.107)	(3.706)	(8.619)	(2.108)
Intercept	0.893***	40.74***	12.32	9.266***	0.552***	30.05***	-6.535	-0.827
	(0.302)	(6.270)	(13.24)	(3.532)	(0.176)	(6.206)	(4.920)	(3.112)
Observations	1,265	755	1,193	1,210	1,041	961	980	987
R^2	0.405	0.481	0.647	0.236	0.114	0.061	0.412	0.050
# of individuals	223	201	222	223	188	186	187	187

Note. Table reports parameter estimates of the three-way interaction $T_t \times postjuly_i \times treat_i$ from estimating equation (1). $t = -1$ is the omitted reference category. Year dummies and interview month dummies are included. Time-varying covariates include indicators for East Germany, having a partner, and education level. Robust standard errors, clustered at the individual level, are shown in parentheses. Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A6. Static DID

	Mothers				Fathers			
	Employment (1)	Working hours (2)	Childcare hours (3)	Housework hours (4)	Employment (5)	Working hours (6)	Childcare hours (7)	Housework hours (8)
Reform × *Post*	0.188*	-2.772	-8.804	-1.724	0.0222	1.244	3.561	0.379
	(0.101)	(4.188)	(8.772)	(1.341)	(0.057)	(1.979)	(2.957)	(0.896)
Intercept	0.718***	39.62***	12.10	6.701***	0.824***	43.72***	-5.526**	0.808
	(0.171)	(2.636)	(9.966)	(1.901)	(0.0496)	(1.583)	(2.797)	(2.135)
Observations	1,323	771	1,249	1,265	1,071	983	1,009	1,016
R^2	0.283	0.393	0.493	0.178	0.043	0.020	0.310	0.037
# of individuals	234	207	233	234	193	191	192	192

Note. Table reports parameter estimates of a static DID with a single post-birth dummy *postjuly*. Year dummies and interview month dummies are included. Robust standard errors, clustered at the individual level, are shown in parentheses. Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.