

# **The Impact of Parental Allowance Reform on Maternal Labor Supply: Evidence from Germany**

By:

Deroma, Giuseppe

Leff Yaffe, Daniel

Shupe, Cortnie

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barcelona | **gse**  
graduate school of economics



## 1. Introduction

Across the OECD, low and decreasing fertility rates are driving demographic trends towards ever-aging populations, generating concerns about skills shortages in the workforce and the sustainability of public finances. Against this background, policy-makers in many countries have recently turned their attention toward policies that make both fertility and working more attractive in order to counteract these effects. As studies find female labor supply to be typically more elastic to wage, tax and other policy changes than male labor supply, and because employment rates for women remain lower than those of men, targeting this group could potentially yield large economic benefits (Blundell et al. 1998). In countries with a high share of female part-time work, this potential is particularly great, given the already existing attachment to the labor force. Importantly, however, female labor elasticities tend to vary across subgroups and also depend on within-household interactions (see for example Alesina et al. 2011; Eckstein & Wolpin 1989; Guner et al. 2012; Keane & Wolpin 2010).

The above trend proves especially relevant for Germany, where fertility rates have stagnated and part-time work comprises a large share of overall female employment. The German government responded to this challenge by implementing a fundamental parental allowance reform on January 1st, 2007, which replaced the former means-tested flat rate benefit (*Erziehungsgeld*, child-raising money) of 300 euro for 24 months with one of a shorter duration (12-14 months) and determined by 67 percent of wage earnings from the previous year. The new system, the *Elterngeld* (parental money), thus incentivized more working women to become mothers by partially compensating their opportunity cost of doing so and the shorter duration encouraged a quicker return to the labor force after the benefit expiry period. Given the swift legislative process, with the first announcement in May 2006 and implementation on January 1st, 2007, the reform lends itself to a natural experiment for investigating the impact of these changes on the hours worked of mothers after the benefit expiry period.

Previous studies have analyzed the effect of the parental allowance reform on the hours worked of mothers. Bergemann & Riphahn 2010 provided one of the first positive assessments, but relied on intentions of new mothers to return to the labor force rather than actual behavior. Kluve & Schmitz 2014 then analyzed the effect of the reform on the actual response of mothers in the medium-run, using census data and a regression discontinuity design. They find moderate increases in the probability of working after the benefit expiry date and a relatively large impact for women who are highly educated, those who belong to the upper tercile of the income distribution and first-time mothers. The authors surmise that short to medium-run positive effects of the *Elterngeld* benefit on mothers' employment probability could likely to be offset in married couples with a wide spousal wage gap; an increase in the wife's work hours would significantly reduce the tax splitting advantage due to the high marginal tax rate for secondary earners. Lacking

information on the distinction between marriage and cohabitation for households in their sample, however, the authors are unable to investigate this hypothesis further.

The value added of this project is to use longitudinal data from the German Socioeconomic Panel (SOEP) that contain richer information on household characteristics in order to more thoroughly identify heterogeneous effects among married and cohabiting mothers. Further, we plan to investigate the interaction of the reform with the existing spousal tax splitting advantage for married couples.

## 2. Identification Strategy

### 2.1 Medium-run Effect on Hours Worked for New Mothers

In order to identify the causal effect of the transition from *Erziehungsgeld* to *Elterngeld* on the hours worked of mothers with young children, we rely on a difference in difference (DiD) estimation. We choose a DiD to reliably identify causality, as macroeconomic and demographic trends independent of the reform would likely bias the results upward due to increases in mothers' hours worked over time. The DiD eliminates this bias by constructing a comparison group of mothers with school-aged children<sup>1</sup>, '*Mothers<sup>SA</sup>*', and observing the difference in the change in hours worked by this unaffected group versus the treatment group of mothers with a young child, labelled '*Mothers<sup>New</sup>*', before and after the cut-off. The DiD estimator will yield the following average treatment effect on the treated (ATT):

$$(ATT) = (Mothers_{After}^{New} - Mothers_{Before}^{New}) - (Mothers_{After}^{SA} - Mothers_{Before}^{SA}).$$

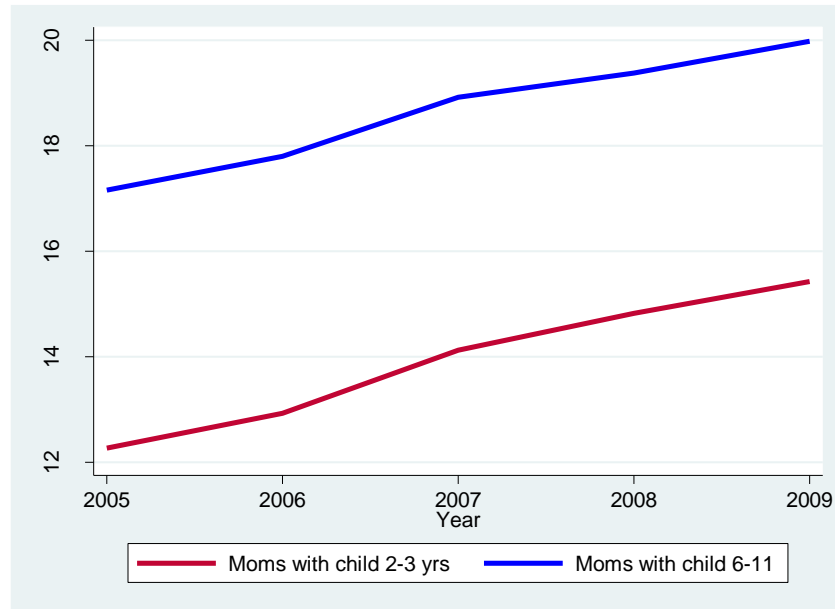
Because this paper focuses on the *medium-run* effects of the reform on hours worked, we observe the hours worked of mothers at least two years after having a child. This cut-off ensures that the benefit expiry period has passed. Otherwise, estimates would pick up the reduced hours worked that can be expected during the time of benefit receipt. For children born before the reform in 2005 or 2006, we observe the hours worked of their mothers in 2008 and 2009, respectively. For children born after the reform in 2007 or 2008, we observe the hours worked of their mothers in 2010 and 2011, respectively. The 'treatment' group in both the before and after periods is defined as mothers whose youngest child is at least two but not older than three years of age.<sup>2</sup> For simplicity, we refer to this treatment group as mothers with a young child, new mothers, or mothers with child 2-3. Figure 1 demonstrates that the identifying assumption of the parallel trend holds for the treatment and comparison groups.

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<sup>1</sup> The choice of the comparison group as mothers with school-aged children follows Schober (2014).

<sup>2</sup> This ambiguity stems from the fact that the data does not provide the month of birth. Therefore, each year of birth necessarily contains a range of possible ages of the child, depending on the month of birth and the month in which the interview is given. See the data section for more details.

**Figure 1: Parallel Trend in Working Hours**



Source: SOEP data, waves 2005-2009

In addition to a simple DiD, we use a Tobit<sup>3</sup> model to estimate the following regression:

$$\text{Hours worked} = \beta_0 + \beta_1 * I_{\text{year} \geq 09} + \beta_2 * I_{\text{Treat}} + \beta_3 (I_{\text{year} \geq 09} * I_{\text{Treat}}) + \beta_4 X + \varepsilon \quad (1)$$

where  $I_{\text{year} \geq 09}$  represents the ‘after’ period and is an indicator dummy that takes the value of 1 when hours worked are observed after 2009;  $I_{\text{Treat}}$  represents the treatment group and is an indicator variable that takes the value of 1 when a mother has a child 2-3 years of age;  $\beta_3$  is the coefficient of interest, which captures the marginal effect of the reform on the treatment group of new mothers after the reform and  $X$  is a vector of covariates.

## 2.2 Heterogeneous Effects: Interaction with Spousal Tax Splitting

As suggested by Kluve & Schmitz (2014), mothers may react differently to the reform, depending on how the incentives of the parental allowance interact with the tax system. The marginal tax rate for secondary earners in Germany is quite high if they choose to split their taxes. Thus, for a woman whose husband earns a substantially higher wage, the tax splitting advantage might offset any positive incentive to re-enter into the labor force after having a child because any wage she earns will be subject to the progressive taxation system at a high marginal rate. Cohabiting couples are not privy to the tax-splitting advantage and an advantage only exists for married couples with high wage differentials.

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<sup>3</sup> We use the Tobit because we have censored observations for women working zero hours. We assume they work zero hours because the offered wage in the labor market is below their reservation wage, maximizing household utility. We assume normality and homoscedasticity.

The first step in identifying the effect of the tax-splitting regime is to show that heterogeneous impacts of the reform on married and cohabiting couples in fact exist. If the tax-splitting offsets the positive labor supply incentive of the reform, married mothers with a high wage differential between her and her partner would show a smaller reaction to the parental allowance reform than similar cohabiting mothers with high wage differential. To rule out endogeneity through selection into a married or cohabiting living arrangement according to preferences for market or household work, we would also need to demonstrate that cohabiting and married households are not fundamentally different in the first place. Given that no tax splitting advantage exists for married or cohabiting couples with small wage differentials, we will investigate the presence of selection by observing whether mothers of these two types of households respond the same to the reform; if selection is driving different work choices, we would expect to find a difference also between the groups with a low wage differential.

### 3. Data

The data for our study stem from the German Socioeconomic Panel (SOEP), which is a representative longitudinal study of 11,000 private households and 20,000 people every year. It allows us to explore household composition in greater detail than other data sources because it includes information on both partners in a household, such as wage, education and training, the type of partnership and other covariates of interest to our identification strategy. In particular, for the analysis of heterogeneous effects by household type, the SOEP data distinguish married couples from cohabiting couples.

In order to construct the DiD, we restrict our sample to our observations of interest, depicted in Table 1. Our sample includes in total 3,835 observations.

**Table 1: Sample of Interest**

	<b>Hours worked by mothers in</b>	<b>Treatment Group Youngest child born in</b>	<b>Control Group Youngest child born</b>
<b>Before</b>	<b>2008</b>	2005	1997 - 2002
	<b>2009</b>	2006	1998 - 2003
<b>After</b>	<b>2010</b>	2007	1999 - 2004
	<b>2011</b>	2008	2000 - 2005

### 4. Results

Table 2 below shows the simple first differences in means of hours worked by mothers in the treatment and comparison group before and after the cut-off date at the end of 2009. The difference in means in weekly hours allows us to compute the DiD estimator for hours worked, which is simply given by:

$$\text{DiD} = (15.53 - 15.05) - (21.46 - 19.68) = -1.3$$

This result suggests a negative impact of the reform on hours worked by mothers. Nevertheless, this estimate is statistically not different than 0 (the 95% confidence interval, computed with robust standard

errors, is between -4.004 and 1.393). Moreover, we know hours worked are censored for those women working zero hours which means this result is likely to be biased. Thus we proceed by estimating the Tobit model to obtain unbiased estimates.

**Table 2: Means and Difference in Means of Key Variables**

	Treatment Group			Control Group		
	Mothers with Young Child			Mothers with School-Aged Child		
	2010-11	2008-9	Difference	2010-11	2008-9	Difference
Weekly hours	15.53 (0.96)	15.05 (0.81)	0.48 (1.25)	21.46 (0.41)	19.68 (0.38)	1.78*** (0.56)
Age of mother	34.33 (0.31)	33.41 (0.32)	0.92** (0.45)	39.99 (0.14)	39.57 (0.13)	0.41** (0.19)
Number of children	1.89 (0.05)	1.72 (0.05)	0.18** (0.07)	1.76 (0.02)	1.79 (0.02)	-0.03 (0.03)
Education of mother	12.92 (0.16)	12.78 (0.15)	0.13 (0.22)	12.63 (0.07)	12.38 (0.06)	0.25*** (0.09)
Lives in East Germany	0.25 (0.02)	0.26 (0.02)	-0.01 (0.03)	0.23 (0.01)	0.19 (0.01)	0.04*** (0.01)
Cohabiting	0.15 (0.02)	0.17 (0.02)	-0.02 (0.03)	0.09 (0.01)	0.08 (0.01)	0.01 (0.01)
Married	0.77 (0.02)	0.72 (0.02)	-0.06* (0.03)	0.74 (0.01)	0.74 (0.01)	0.00 (0.02)
Childcare by relative	0.34 (0.03)	0.40 (0.03)	-0.06 (0.04)	0.28 (0.01)	0.25 (0.01)	0.04** (0.02)
Gross wage father	3085.94 (139.80)	3049.58 (156.19)	36.36 (214.44)	3581.69 (81.59)	3399.79 (66.26)	181.90* (103.99)
Wage differential	2078.68 (163.83)	2159.36 (162.14)	-80.69 (232.86)	2333.76 (88.37)	2333.96 (73.69)	-0.21 (114.14)

Notes: \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10% levels, respectively. The first row reports the mean of the variable or difference with standard errors in parentheses.

We first regress the number of weekly hours worked of mothers with a young child on the reform variable of interest, namely the interaction term of the treatment group and after period dummies. We label this model the ‘raw effect’. In a second model, we add covariates of interest, including the number of children present in the household, the age of the mother as well as her age squared, the number of years in education or training, a dummy variable taking the value of 1 if she lives in the former East Germany, the gross wage of the father and two dummies for whether the mother is married and whether relatives assist with childcare.

As depicted in Table 3, neither of these models finds significance for the impact of the reform on the hours worked by mothers with a young child. As expected, having a small child is strongly negatively associated with the number of hours worked by the mother. Compared to the comparison group, mothers with a young child work on average more than 7 hours fewer per week. Notably, living in the former East Germany and having a relative who assumes some childcare duties positively and significantly explains the number of

hours worked. Finally, being married is associated with a mother working almost 5 hours fewer than mothers who are single, cohabiting, divorced or separated. Consequently, we investigate whether the propensity of married mothers to work less on average also translates to heterogeneous effects of the reform on married and cohabiting mothers with a young child. Running the above models for the hours worked by married and cohabiting mothers separately, however, does not yield any indication of an effect of the reform on either group on the intensive margin. The small sample size of cohabiting mothers (295) or increased part-time work may be responsible for the lack of significance on hours worked for this group.

Given the high and increasing part-time work in Germany, we investigate whether the reform affected married and cohabiting mothers differently on the extensive margin. Table 4 shows this is in fact the case: the reform increased the probability of cohabiting women to hold a full-time job by .23 while there is no effect on the married mothers with a young child. It appears that while the reform induced more cohabiting mothers to re-enter the labor force full-time after the benefit expiry period, it had no effect on the propensity of married mothers to do so. Married mothers appear to be unresponsive to the reform on both the intensive and extensive margins.

**Table 3: Marginal Effects on Hours Worked of Mothers with a Young Child**

	Raw Effect		With Controls	
After period dummy	1.813***	(0.569)	0.657	(0.581)
Treatment group dummy	-5.785***	(1.057)	-7.461***	(1.191)
Interaction (after*treat)	-1.627	(1.606)	0.26	(1.702)
Observations	3835		2770	

Notes: \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10% levels, respectively. Standard errors (in parentheses) are calculated using the delta method. Controls include number of children, age of mother, age of mother squared, education of mom, dummy for East Germany region, gross wage of father, married dummy and relatives dummy.

The distinct reaction of the two groups of mothers corresponds to our expectations and could be a sign of different preferences for market or household work. Alternatively, the difference in response might be driven by the incentives of the spousal tax splitting system, which discourages married mothers who would be secondary earners in a household with a high wage differential from working more. In the following, we make a first attempt to understand why cohabiting and married mothers reacted differently to the reform. Our small sample size restricts a more robust identification of this channel, but nevertheless offers insights and a starting point for further analysis.

Because tax splitting should only offset the incentives of the reform for married mothers from a high wage differential household, we further divide our sample according to the wage differential between mother and father. Table 5 provides three important insights. Firstly, considering cohabiting mothers with a low wage differential, their reaction to the reform becomes significant at the 10 percent level and amounts to an

increase of over 13 hours weekly. When we add controls, the magnitude of the effect decreases to 10 hours weekly, but becomes slightly more significant, at the 5 percent level. Secondly, there is a high concentration of cohabiting women in the low wage differential group, which could indicate that these mothers were living in more homogeneous households in terms of work preferences in the first place. Statistical inference beyond the first quartile is unreliable due to lack of observations. Thirdly, despite the much larger sample size of married mothers with a young child in comparison to their cohabiting counterparts, they remain inelastic regardless of wage differential.

**Table 4: Marginal Effect on Employment Probability of Mothers with a Young Child (Married & Cohabiting)**

	With Controls			
	Employed Full-Time		Employed Part-Time	
	Married	Cohabiting	Married	Cohabiting
After period dummy	0.009 (0.015)	-0.055 (0.065)	-0.000 (0.022)	0.092 (0.070)
Treatment group dummy	-0.036 (0.027)	-0.373*** (0.105)	-0.163*** (0.041)	0.229** (0.102)
Interaction (after*treat)	0.000 (0.037)	0.232* (0.141)	-0.018 (0.057)	-0.186 (0.135)
Observations	2464	295	2464	295

Notes: \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10% levels, respectively. Standard errors (in parentheses) are calculated using the delta method. Controls include number of children, age of mother, age of mother squared, education of mom, dummy for East Germany region, gross wage of father, married dummy and relatives dummy.

**Table 5: Marginal Effect on Hours Worked of Mothers with a Young Child by Wage Differential Quartiles (Married vs. Cohabiting)**

By wage differential	Raw Effect							
	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Married	Cohabiting	Married	Cohabiting	Married	Cohabiting	Married	Cohabiting
After period dummy	4.258*** (1.484)	-4.498 (2.958)	0.607 (1.114)	0.013 (3.713)	-0.122 (0.954)	10.786 (6.700)	0.590 (0.964)	13.276** (5.361)
Treatment group dummy	-3.869 (2.935)	-22.881*** (5.240)	-6.750*** (2.089)	-8.566* (5.170)	-8.057*** (1.855)	1.044 (6.973)	-2.432 (1.827)	-8.715** (4.203)
Interaction (after*treat)	1.922 (4.439)	13.395* (6.939)	-2.354 (3.408)	-3.964 (9.055)	3.849 (2.577)	-7.189 (13.647)	-4.376 (2.915)	4.317 (8.921)
Observations	573	144	608	86	660	45	675	27

## 5. Conclusions

On the intensive margin, we find no medium-run causal effect of parental money on the labor supply of mothers with a young child on aggregate. However, when we estimate the models for the intensive and extensive margins, we find a significant and positive effect on the hours worked and probability of cohabiting women to work full-time. In contrast, no significant effect could be detected for married women. These results confirm heterogeneous effects of the parental allowance reform on married and cohabiting mothers that previous work has suggested but not been able to prove.



Further disaggregating the married and cohabiting groups, we only find significance in the group with a low wage differential between the mother and father. Had tax splitting counteracted the reform impact, we should have found that reform had a much higher effect on the cohabiting group in the high wage differential quartile than for married in the same quartile, but we did not. This could have several explanations. Firstly, cohabiting households appear on average in our sample to have a low wage differential and thus our number of observations in higher wage differential quartiles is inadequate for analysis. Alternatively, this result could indicate endogeneity and self-selection into married or cohabiting living arrangements depending on unobserved preferences.

Finally, several reasons could explain failure to find more significance. Firstly, a longer medium-run timeframe might be necessary to detect a larger effect, for instance that stemming from changes in fertility behavior of working women. Secondly, while female labor elasticity is higher than male elasticity on average, women with a strong attachment to the labor force, who comprised the target group for the reform, likely have a lower elasticity than women with lower earnings or education. Therefore, the specific target group of educated, working women might be less elastic than policy-makers hoped. Thirdly, lack of childcare may prevent new mothers from reacting to the financial incentive of the reform. Going forward, further research should address these possible explanations.

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