DISCUSSION PAPER SERIES

IZA DP No. 8115

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April 2014

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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IZA Discussion Paper No. 8115
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ABSTRACT

Social Norms and Mothers' Labor Market Attachment: The Medium-Run Effects of Parental Benefits*

Increasing mothers' labor supply is a key policy challenge in many OECD countries. Germany recently introduced a generous parental benefit that allows for strong consumption smoothing after childbirth and, by taking into account opportunity costs of childbearing, incentivizes working women to become mothers and return to the labor force rapidly. Using a sharp regression discontinuity design, we estimate policy impacts for up to 5 years after childbirth and find significant and striking patterns. First, medium-run effects on mothers' employment probability are positive, significant and large, for some subgroups ranging up to 10 per cent. The effects are driven by gains in part-time but not full-time employment. We also find significant increases in working hours. Second, the probability of job continuity rises significantly, i.e. mothers return to their pre-childbirth employer at higher rates. Third, employers reward this return to work by raising job quality significantly and substantially. We argue that the policy generated a profound change in social norms: the new parental benefit defines an "anchor", i.e. a societally preferred point in time at which mothers return to work after childbirth.

JEL Classification: H31, J13, J22

Keywords: parental benefits, female labor supply, regression discontinuity

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* We gratefully acknowledge helpful comments by Felix Arnold, David Card, Sabrina Hahm, Bas van der Klaauw, Jonathan Stöterau, Marcus Tamm, Katrine Vellesen Løken and seminar participants at UC Berkeley, Freie Universität Berlin, Vrije Universiteit Amsterdam, the Berlin Network for Labor Market Research (BeNA), ZEW Mannheim, Leuphana Universität Lüneburg and Universität Hohenheim. Diana Beyer and Claudia Schmiedchen provided excellent research assistance. The usual disclaimer applies.

1 Introduction

Over the last decades countries in the OECD have experienced a steady and frequently pronounced increase in female employment rates. Many factors have contributed to this development, including for instance higher female participation in education and a changing sectoral composition of the skills required in the labor market. A key issue in determining female labor supply is the role of childbearing and childrearing. Despite the fact that in modern families the division of household chores has become more equally distributed between genders, it remains a biological necessity that the mother will dedicate more time to a child than the father, at the very least in the months surrounding the birth of the child. Naturally, both social norms and personal preferences can reinforce - or change - such patterns. This implies a particular challenge for the design of policies aiming at impacting on a mother's return to the labor market and her allocation of time to market work. The importance of such policies, however, is evident from several perspectives: as many OECD countries are facing demographic change towards ageing societies, it is imperative to ensure full labor force participation of the core male and female working-age population, in order to sustain functionality of social security and pension systems. Moreover, further increasing mothers' labor supply over the lifecycle has been identified as a key factor in closing the remaining gender wage gap (Polachek, 2006). Finally, such increases in female lifecycle labor supply could help reduce the risk of old-age poverty, which still remains considerably higher for women than for men (e.g. Bureau of Labor Statistics, 2014).

One type of policy intended to increase maternal labor supply is subsidized childcare, allowing the mother to return to work while the child is being taken care of in publicly financed (or subsidized) nurseries, day-care centers, and kindergarten. The Nordic countries, in particular, have been vanguards of implementing such policies since the 1970s. Empirical research, however, has since found that the effectiveness of these interventions is far from certain: while some studies find the expected significantly positive effects of childcare subsidies on maternal employment rates (e.g. Lefebvre and Merrigan (2008) and Baker et al. (2008) for Quebec), the analysis of a comprehensive expansion of childcare subsidies in Norway in 1975 does not find any such impacts (Havnes and Mogstad, 2011). Also several other studies analyzing the reduction of childcare prices or introduction of childcare subsidies in Sweden and the US do not find effects on mothers' labor supply (Lundin et al. (2008) and Cascio (2009), respectively).

A second type of policy are "cash-for-care" benefits, i.e. a financial transfer paid to mothers (or fathers) who decide to take care of their young child themselves rather than

sending her to public daycare. These benefits are typically introduced against the background of an existing and comprehensive infrastructure of public childcare, and are supposed to give parents freedom of choice. Clearly, this type of intervention potentially affects maternal employment in a different way than the first type of policy described above: by increasing the price of public daycare relative to own care, the policy generates disincentives to work. The evidence on this benefit type indicates that the theoretical prediction seems to be correct: Hardoy and Schøne (2010) for Norway and Gathmann and Sass (2012) for Germany find that "cash-for-care" benefits indeed display significantly negative effects on maternal employment rates.

A third type of policy focuses specifically on the employment situation directly after childbirth and comes in three variants. *Maternity protection* regulations prohibit employment immediately before and after childbirth and typically include wage continuation.[1] *Parental leave* policies consist of job protection for a certain time period, after which parents can return to their previous position. Finally, *parental benefit* policies comprise a financial transfer paid during the post-birth period to allow for consumption smoothing by providing income replacement. In many countries the latter two variants are combinable; thus, some studies refer to paid leave schemes as *parental leave* periods.[2] However, the granted leave and benefit periods do not need to coincide and some countries even allow to combine benefit receipt with reduced labor supply.

The evidence shows that relatively short maternal leave entitlements such as the leave granted by the FMLA (Federal Maternal Legislation Act) in the United States have only little or no impact on the labor market performance of mothers (Waldfogel, 1999; Baum, 2003). For parental leave policies that extend beyond the immediate few weeks after childbirth there is evidence that these policies indeed prolong the time spent at home; however, they may still be beneficial for the mothers' labor market integration, since they facilitate the return to work for those mothers who would have first stayed at home after childbirth anyway. Additionally, Baker and Milligan (2008) show for Canada that job-protected leave increases the propensity of women to return to their pre-birth employer. However, the longer the granted maximum leave period, the more adverse the impact on labor supply; Schönberg and Ludsteck (2014) show that every expansion of the maternity leave period in Germany between 1979 and 1992 indeed led to delayed return to work. But since the marginal effect

[1] In Germany, for instance, maternity protection entitlements cover the time period 6 weeks before the expected delivery date until two months after childbirth.

[2] For the sake of exposition we stick to the dichotomy between *parental leave* = *job-protection* opposed to *parental benefit* = *cash transfer*.

on mothers' behavior becomes smaller with extensions in potential leave, such unpaid leave policies might actually foster mothers' employment as long as they are not too drawn-out.

Parental benefits on the other hand seem to unambiguously delay the return to the labor force without having beneficial labor market effects (Rønsen and Sundström, 2002; Pronzato, 2009). The longer the financial transfer is paid, the longer mothers stay out of the labor force. It is not clear, however, whether these effects result in persistently unfavorable labor market outcomes: Lalive and Zweimüller (2009) find for Austria that changes in the duration of paid parental benefits indeed increase the time spent at home; however, their results do not indicate any persistent differences in the overall medium to long-term employment levels. Also for Germany there is evidence that the maternal benefit schemes in the 1980s and 1990s delayed labor market return at least for some mothers (Ondrich et al. (1996, 2003)).

The most recent parental benefit reform in Germany constitutes a particularly interesting case of public policy. Implemented in 2007, the new parental benefit (called "Elterngeld", literally: "parents' money") combines several key components. First, it is a very generous transfer, replacing 67 per cent of pre-childbirth labor earnings for up to 14 months. The transfer is capped at 1,800 € per month; in case of no labor earnings in the pre-birth period parents receive a minimum monthly transfer of 300 €. The policy thus explicitly intends to incentivize working women to become mothers, by taking into account their opportunity costs of childbearing and allowing for extensive consumption smoothing. Moreover, the policy aims at incentivizing fathers to take paternal leave by granting two additional months if both partners participate.

Our paper contributes to the debate by investigating medium-run effects of parental benefits, i.e. female labor market outcomes up to 5 years after childbirth. Previous studies for Germany and other countries typically looked at short-term effects during benefit receipt and shortly after benefit exhaustion. We estimate the medium-run effects using a Regression Discontinuity (RD) design generated by the coming-into-effect of the regulation. Assignment to treatment is a deterministic function of the date of birth of the child in real time: on the first minute of January 1, 2007, the probability of being eligible for the benefit switches from zero to one, giving rise to a sharp discontinuity. As the RD design hinges on observing units arbitrarily close to the cut-off, we argue that the legislative process generates quasi-random assignment to treatment and control groups within a neighborhood of three months before and after the discontinuity.

The question we try to answer is whether, from a medium- to long-run perspective, the one-time transitory income shock induced by the generous 14-month parental benefit

can effectuate any persistent effects on labor market behavior. The short-run impacts are in line with theoretical expectations given the design of the policy: during benefit receipt, labor force participation and employment are significantly lower in the treatment than in the control group; directly after benefit exhaustion (i.e. during the second year after childbirth) the estimates do not show pronounced differences between the two groups.

Our analysis uses German census data, an annually repeated cross-section representative of the German population. The estimation sample comprises approximately 11,600 mothers, about half of which are in the treatment group (childbirth in-between January 1 and March 31, 2007), and the other half in the control group (childbirth in-between October 1 and December 31, 2006).

The empirical results indicate several significant and striking patterns of the impact of the policy on mothers' labor market outcomes. First, we find that the parental benefit significantly causes mothers to move from non-participation to employment. Second, these positive employment effects are almost exclusively driven by increases in part-time employment. Moreover, the part-time impacts are characterized by significant increases of hours worked close to the upper bound of working hours in part-time jobs (23-32 hours per week). Third, we find significant and large treatment effects on the probability of continuing with the same employer after childbirth: while overall full-time employment is not significantly affected by the policy, treatment group mothers have a 12 per cent higher probability of job continuity than the control group, conditional on having a full-time job. This positive effect on the job match is reinforced by significant and large treatment effects on job quality, i.e. the probability of having an unlimited labor contract.

To investigate robustness of the treatment effect estimates we conduct several sensitivity analyses. First, we implement a difference-in-discontinuities estimator that combines the RD approach with a difference-in-difference design; using the groups of parents with children born in the last quarter of 2005 and the first quarter of 2006, this controls for any time-invariant seasonal differences between the groups surrounding the turn of the year. Moreover, since there is evidence that some parents tried to "defer" childbirth very close to the discontinuity, we re-estimate treatment effects using a restricted sample excluding the January and December births. Our impact estimates are robust to these sensitivity checks.

We argue that the strong impacts on mothers' labor market outcomes provide evidence for an unprecedented change in social norms: the new parental benefit defines an "anchor" at the end of benefit receipt, i.e. a societally preferred point in time at which mothers return to work. This change in social norms is remarkable, since it does not only affect supply-side decisions of mothers. The large and significant positive impacts on job

continuity show that, in addition to influencing mothers' planning horizon, the "anchor" generated by the reform also shapes employers' behavior. Moreover, the fact that mothers return to their previous job at a higher rate is rewarded by employers through improving the job quality with a significant increase in open-ended contracts.

The paper is organized as follows. Section 2 presents details of the German parental benefit reform that came into effect on January 1, 2007, and discusses results from previous studies analyzing the short-run effects of the policy. Section 3 delineates and justifies our identification strategy, along with a presentation of the German census data. In section 4 we present empirical results for a series of outcomes: female employment rates (including part-time and full-time employment, and hours worked); job continuity and job quality; and demographic outcomes (marriage rates, subsequent childbirth). Section 4 also contains the sensitivity analysis. Section 5 discusses our findings and concludes.

2 The German Parental Benefit Reform of 2007

The parental benefit that came into effect in Germany on January 1, 2007, was designed against two background developments. First, Germany has experienced very low and further decreasing fertility rates over the last decades, and it is among the countries with the lowest fertility worldwide. Second, whereas the female employment rate has been gradually growing in recent years, there is still a delayed return of mothers to the labor market and a relatively high share of part-time employment. This has important repercussions in the social security systems, in particular the pension system: in an ageing society with a contribution-financed pension system, the share of contributors financing those individuals receiving pensions continually decreases, making it imperative to devise policies that ensure that the working-age population actually participates in the labor market. The new parental benefit is one such policy.

The objectives of the policy are manifold. First, a key goal is consumption smoothing for working parents after childbirth, in order to facilitate taking care of their own child during the first year. A complementing objective is the increased integration of fathers into childcare. Moreover, the policy intends to generate fertility incentives for working women by considering their opportunity costs of childbearing.[3] Finally and most importantly, the policy aims at keeping these working mothers close to the labor market and facilitate a timely return to employment.

[3]This is a surprisingly economic element for a social policy or family policy. Not many policies exist in Germany that explicitly take into account opportunity costs of market actors.

When the parental benefit came into force on January 1, 2007, it replaced a previous parental benefit ("Erziehungsgeld", literally: "child-raising allowance") that was much less generous. It came in two options: option 1 implied a transfer of 300 € per month, for up to 24 months after childbirth for either father or mother. Option 2 implied a transfer of 450 € per month, for up to 12 months. The eligibility criteria for the parental benefit specified that no full-time employment[4] was allowed and benefit eligibility was means-tested. In practice, these regulations implied that the coverage was on average as follows: 66 per cent of parents chose Option 1, 10 per cent of households chose Option 2, and 24 per cent of households were not eligible (Kluve and Tamm, 2013).

The new parental benefit replaced this system with a much more generous regulation. In general, the benefit replaces 67 per cent of pre-childbirth net labor earnings. The maximum amount is 1,800 € per month, and a minimum transfer of 300 € per month is paid to parents with no previous labor earnings. The duration of benefit eligibility is up to 12 months after childbirth for either father or mother, and up to 14 months per household if both parents take up the transfer, i.e. each participates for at least two months. The distribution of the 14 benefit months can then be freely chosen by parents. Similar to the old regulation, benefit eligibility is conditional on not being in full-time employment (< 30 h/weeks). However, since eligibility is not means-tested, the coverage is effectively 100 per cent of families.

Whereas the exact changes in the regulations create many detailed, differential incentives and expected impacts in the short-term (see Table 1 in Kluve and Tamm 2013 for a comprehensive description), for our purposes Figure 1 summarizes the key policy changes. The x-axis indicates time since childbirth, separated into three phases. The first phase (up to 14 months) covers the period of receipt of the parental benefit. The second phase (up to 24 months) covers the second year after childbirth, i.e. the short-term perspective. The third phase comprises years three to five after childbirth, i.e. the medium-term perspective.

The y-axis indicates the changes in the monthly benefit level induced by the reform. The gray and blue shaded area displays the "possibility set" of changes in the benefit structure during Phase 1. The upper bound is a delta of +1,800 per month for 14 months, i.e. households in which two high-income parents, who would have not been eligible under the old regime, both take up the transfer. The lower bound is at a delta of zero for 14 months, for those parents who receive the minimum benefit of 300 € per month and would have received the same amount under the previous regulation.[5]

[4] I.e. working time less than 30 h per week can be combined with benefit receipt.

[5] Note that there is a group of households receiving the new minimum transfer of 300 € who would not have received any transfer under the old regulation (indicated by the "+300" on the y-axis). These are essentially households in which the mother - prior to the birth of the current child - was not working but staying at

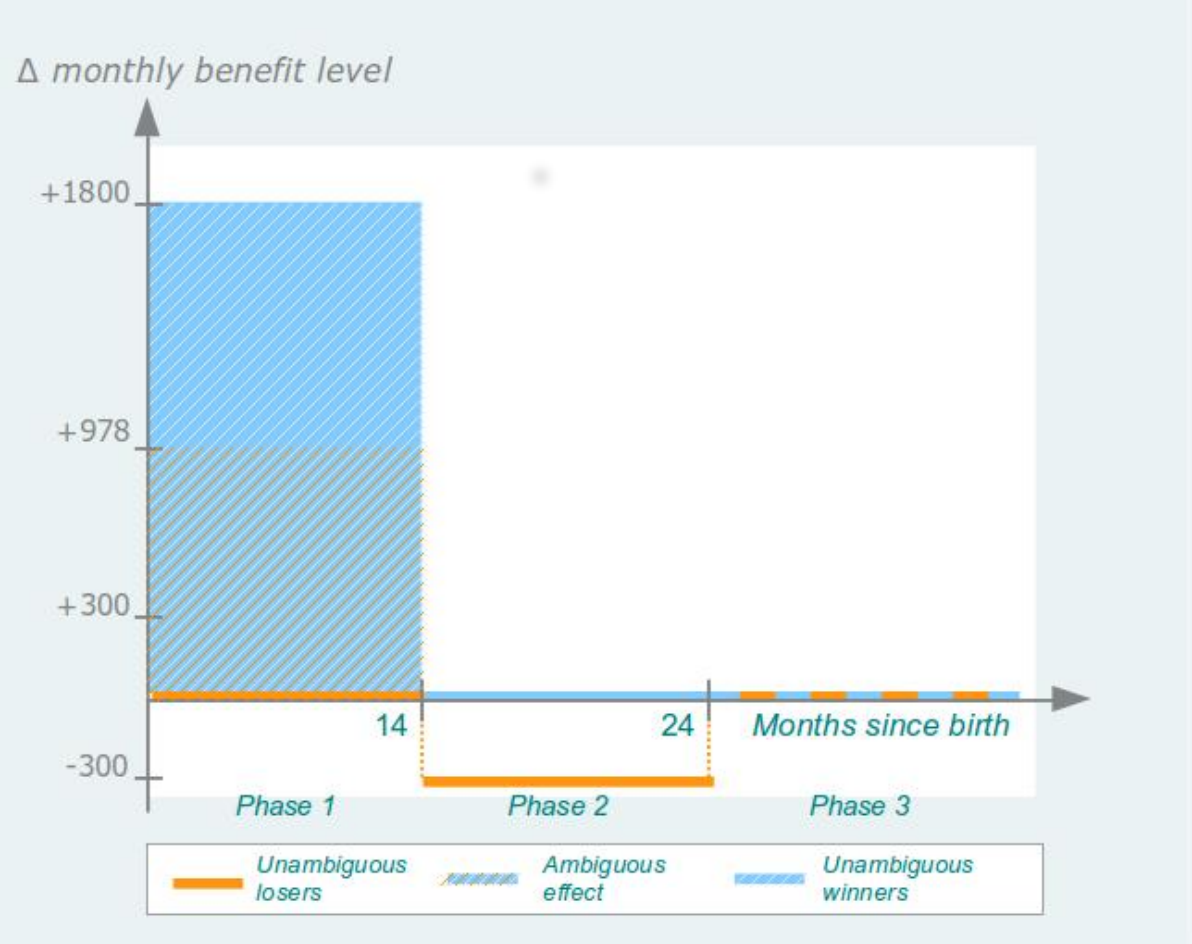

Figure 1: Changes in the Benefit Structure

During Phase 1, the blue shaded area indicates the "unambiguous winners" of the reform, i.e. these are mothers that receive the new and generous benefit but would not have received any benefit under the old, means-tested regulation. This is indicated by the "+978" on the y-axis, which is the benefit payment that corresponds to the maximum income at the cut-off of the means-test. That is, the households below this line (grey shaded area) would have received a transfer also under the old regime. The exact delta for them is ambiguous, however, as it depends on the precise benefit level and duration they would have received previously. Finally, there is a set of households that are "unambiguous losers" of the reform: these are those (low-income) households that after the reform receive the minimum monthly transfer of 300 € for 14 months, but would have received 300 € for 24 months under the

home (e.g. taking care of older siblings) and her partner has a high income, which excluded these households from receiving any benefit before the reform.

previous system. This group is depicted by the yellow line in Phase 1 and Phase 2. For the "unambiguous winners" there is essentially no change in the benefit level during Phase 2.

In sum, the key changes induced by the reform and delineated in Figure 1 are the following: during Phase 1, there is a new, potentially large, and generous change in entitlement for almost the entire population of parents. For some, however, there is no change. During Phase 2, there is no change for the majority, in particular not for those newly entitled. For some part of the population, namely those with no beneficial change during Phase 1, there is a negative change during Phase 2. In Phase 3, from month 24 after childbirth onward, there are no further changes in benefit entitlements, regardless of potential eligibility. It is thus the contribution of this paper to investigate empirically whether and to what extent the large transitory income shock during Phase 1 causes any medium-run impacts three to five years after childbirth.

Existing evidence on the short-term impacts of the reform are in line with the expectations one would derive from Figure 1: the strong disincentive-to-work effect generated by the - on average - large delta in benefit levels during Phase 1 indeed translates into a significant decrease in maternal employment rates during these first 14 months (Kluve et al., 2008; Bergemann and Riphahn, 2011; Wrohlich et al., 2012; Kluve and Tamm, 2013). In Phase 2, on the other hand, there is a small increase in employment rates, which seems to be mostly driven by the "benefit expiry effect" among low-income households, i.e. those households who would have received a low payment for a longer period under the old regulation (i.e. the unambiguous losers).

3 Identification

3.1 Sharp RD design and justification of the identifying assumption

The objective of our analysis is to estimate the *causal* effect of the parental benefit on key outcomes, in particular mothers' labor market behavior. Our treatment group are mothers receiving the benefit, i.e. mothers with children born on or after January 1, 2007.[6] The factual outcomes of the treatment group can be observed in a straightforward way. The key

[6]Technically we could speak of the household instead of the mother as the relevant unit of observation, as 12 per cent of the recipients are male. The focus of our analysis, however, is female labor force behavior, since this is the key outcome that the policy intends to influence (see sections 1 and 2). In line with this objective, the parental benefit is indeed a transfer predominantly taken up by the mothers. Only 1 per cent of mothers do not apply for it, and the number of households in which only the father takes up the transfer is negligible. Among those 12 per cent of households in which both mother and father take up the benefit, more than 2/3 of the fathers opt for only the minimum of two months. (Destatis, 2008, 2012).

challenge lies in identifying the counterfactual, i.e. how would mothers with children born on or after January 1, 2007, have behaved - in terms of the outcomes - if they had not received the parental benefit.

This is a considerable challenge: first, no experimental design to evaluate the policy is conceivable. Second, since the treatment is universal in the sense that every mother is eligible, no possibility exists for a cross-sectional design because there is no potential control group of untreated units, no matter how selective. Third, a longitudinal design is also not feasible, since the policy explicitly aims at behavioral changes and incentivizes different socio-demographic groups in a differential way to become parents (recall section 2), such that the types of women who decide to become mothers are potentially different before and after January 1, 2007.

The solution we propose is based on a sharp regression discontinuity (RD) design (Thistlethwaite and Campbell, 1960; Imbens and Lemieux, 2008; Hahn et al., 2001) generated by the coming-into-effect process of the parental benefit regulation. In a nutshell, the design is based on the fact that close to the discontinuity - January 1, 2007, the day on which the policy came into effect - parents were quasi-randomly assigned to a treatment group receiving the benefit and a control group exposed to the previous regulation. In the following we elaborate on this idea in detail.

In our context, the assignment mechanism D_i is a deterministic function of the "forcing covariate" C denoting the real time calendar day at which a child is born. That is $D_i = I[C_i \geq c]$ where $I = 1$ if the condition is satisfied and $I = 0$ if not. The value c is given by the date of the coming-into-effect of the parental benefit law, January 1st 2007. All observations with $C_i \geq 1/1/2007$ are in the treatment group, all observations with $C_i < 1/1/2007$ are in the control group. The cut-off is sharp, since with the first minute of the first day of the year 2007, the probability of being eligible for the treatment switches from zero to one. The Sharp Regression Discontinuity Design (SRD) focuses on estimating:

$$\Delta_{SRD} = E[Y_1 - Y_0 \mid C = c] = E[Y_1 \mid C = c] - E[Y_0 \mid C = c] \tag{1}$$

where the last term is counterfactual. Under the assumption that the conditional distribution of the potential outcomes is continuous in c, we can estimate Δ_{SRD} by observing units arbitrarily close to the discontinuity.

The two key questions for the design are (i) to justify why assignment close to the discontinuity can be considered as random, and (ii) how precisely to define the neighborhood around the discontinuity in which the assumption holds.

First, the legislative process that led to the reform generates the random assignment of parents into a treatment and a control group close to the discontinuity. This process took place during the year 2006 and proceeded rather rapidly, resulting in the fact that, at the time when those children born shortly before and after January 1, 2007, were *conceived*, none of the parents knew that by the time of birth the reform would be in place.[7]

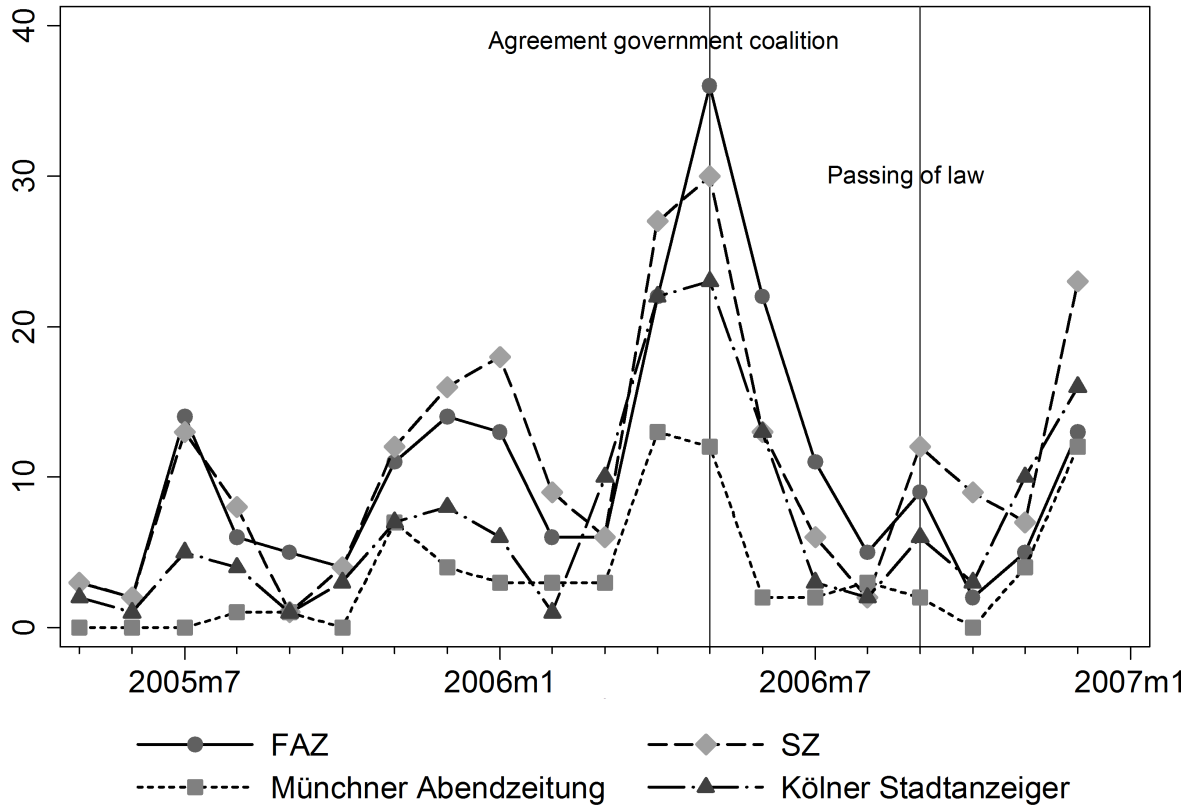

Figure 2: Number of reports on "Elterngeld" per month in major daily newspapers.

Specifically, the government coalition agreed on the reform only in May 2006, and the law passed parliament in September 2006. That is, before September 2006 there was no definitive knowledge that the policy would be implemented. In May 2006, however, those citizens closely following the political debate would be aware that the policy is likely to be put into effect, since an agreement of the government coalition is a strong predictor for legislative outcomes. It is thus theoretically conceivable that any child conceived after May 2006 might have been a conscious and deliberate fertility decision by the parents in order

[7]This "natural experiment" was first pointed out and used in the official evaluation of the policy for the German government (Kluve et al., 2008; Kluve and Tamm, 2013) and subsequently used in other analyses of the short-run impacts (Bergemann and Riphahn, 2011; Wrohlich et al., 2012).

to be eligible for the new parental benefit. For any child conceived before, this is virtually impossible.

Figures 2 and 3 substantiate our identification strategy. Figure 2 shows frequency distributions for media reports on the new parental benefit in major daily newspapers in Germany, during the time period early 2005 until late 2006. The peak in May 2006 coincides with the agreement of the government coalition, the point in time when the legislative process effectively started and mindful observers of the political process could have realized for the first time that this policy would come into effect. The pattern in Figure 2, depicting the supply of information on the new policy, is further reinforced by Figure 3, depicting the demand for information on the new policy: data from the Google Search Volume Index indicate that May 2006 is the first point in time when people in Germany started to look for information on the new policy at a significant rate. Clearly, the second peak in Figure 3 coincides with the point in time when the policy came into effect.

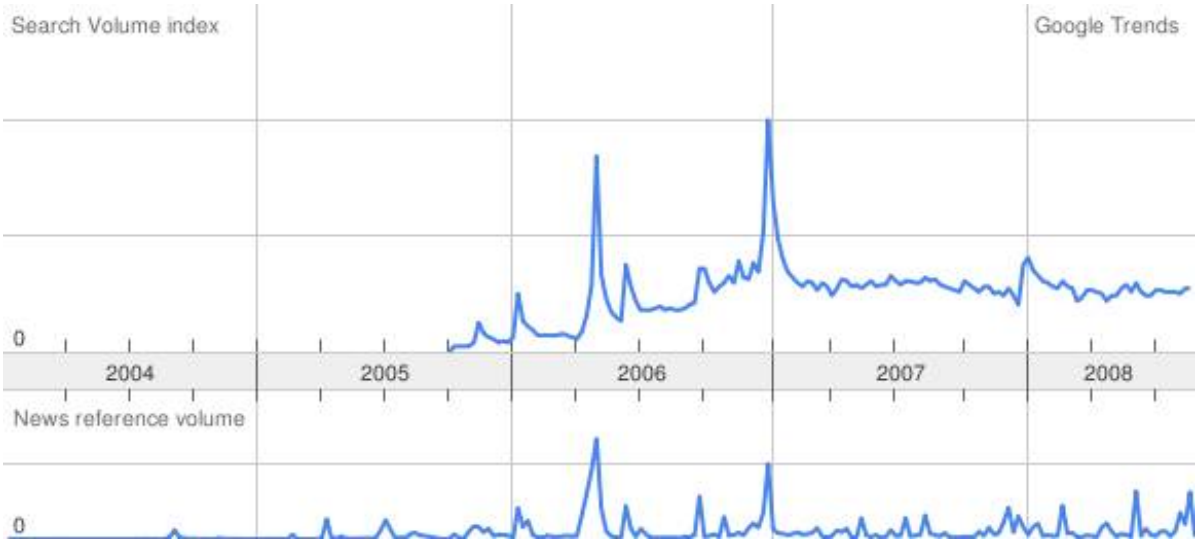

Figure 3: Google Search Volume Index: Number of "Elterngeld" searches relative to all searches (originating in Germany). Source: Kluve and Tamm (2013).

Given these patterns, we therefore believe that in a neighborhood of three months around the discontinuity, i.e. including births until March 2007, no self-selection into the treatment occurred and estimates of the causal reform effects will be unbiased. The quasi-randomly assigned treatment and control groups are therefore given by households with children born in-between January 1 and March 31, 2007 (treatment), and households with children born in-between October 1 and December 31, 2006 (control), respectively.

Several further criteria need to be fulfilled for this design to be valid. One requirement is the absence of any other discontinuities in relevant policies that occur at the same point in time and could potentially affect outcomes. This criterion is given, since no such changes were implemented at the turn of the year 2006/2007. Another criterion is the absence of seasonal effects between distant units within the interval that defines our treatment and control groups, [October 1, 2006; March 31, 2007]. Seasonal differences could not only emerge from seasonal fluctuations in labor demand, but also from other regulations such as the start of the kindergarten year. We believe that, should such differences exist, their influence might affect the short-run estimates assessed in previous research, but will be negligible when looking at outcomes up to five years later. Nonetheless, we additionally implement a sensitivity analysis using a difference-in-discontinuities design with the preceding turn of the year - i.e. the groups with children born [October 1, 2005; December 31, 2005] and [January 1, 2006; March 31, 2006] - to take into account time-invariant seasonal differences between treatment and control groups.

A somewhat more critical issue regards the random sorting very close to the discontinuity. Whereas parents in our treatment and control groups plausibly could not know about the policy at the time of conception, as the pregnancy advances the small subgroup of parents with delivery scheduled just before January 1, 2007, might have tried to influence the date in order to fall under the new regulation. Indeed there is evidence for this behavioral pattern, to the extent that parents can influence the biological process of childbirth. In particular, these intentional deferments seem to be more common for employed mothers than for non-employed mothers (Tamm, 2013; Neugart and Ohlsson, 2013). However, our data indicate no significant differences between treatment and control group with respect to education and income variables, which indicates that potential birth deferments only play a minor and negligible role. Additionally, we address this issue in two ways. First, we can adjust our impact estimates for income groups. As emphasized in section 2, heterogeneous effects by socio-demographic group are to be expected anyway and need to be estimated separately. Second, we implement as a sensitivity check a *restricted sample regression* leaving out observations close to the cut-off date.

3.2 Data and Balancing

The analysis is based on German census data ("Mikrozensus"), using the waves 2006 through 2011. The data are a representative stratified sample of the German population and are collected annually in a repeated cross-section (Destatis, 2011). The total number of observations available for our analysis is N = 11,638 mothers with childbirth in the interval [October 1,

2006; March 31, 2007]. The treatment group comprises N = 5,884 mothers, the control group N = 5,754 mothers.

Table 1: **Covariate Balancing**

	Treatment	Control	Difference	Std. error	t-stat
Mother's Age†	29.22	29.19	-.0281	.1005	0.279
Child's age - Phase 1 ‡	10.41	10.41	.0001	.1105	0.000
Child's age - Phase 2 ‡	19.62	19.63	.0093	.1214	0.077
Child's age - Phase 3 ‡	41.68	41.68	.0017	.2274	0.007
First-time mother	.4856	.4910	-.0054	.0088	0.618
Single mothers	.1269	.1336	-.0066	.0059	1.124
Migrant	.2696	.2597	.0098	.0077	1.271
East	.2051	.2023	.0027	.0071	0.385
Low education	.2077	.2058	.0019	.0071	0.262
Medium education	.6257	.6218	.0039	.0085	0.464
High education	.1654	.1709	-.0055	.0066	0.832
Children before	.7807	.7528	.0278	.0175	1.586
Income before childbirth	820.97	830.02	9.06	10.64	0.851
Income before childbirth -T1	.3332	.3267	.0065	.0082	0.789
Income before childbirth -T2	.3285	.3315	-.0030	.0082	0.368
Income before childbirth -T3	.3382	.3417	-.0034	.0082	0.419
N	5884	5754			
N - Phase 1	869	842			
N - Phase 2	1134	1040			
N - Phase 3	3881	3872			

† Age is dated back to 2006 and adjusted for the three months time lag.
‡ Child's age in months
Source: Own calculations based on German census data, waves 2006-2011
FDZ der Statistischen Ämter des Bundes und der Länder, Mikrozensus

Table 1 provides summary statistics of our sample and balancing tests for socio-demographic variables at baseline for the treatment and control groups. The table shows that there are no systematic differences between the two groups, providing evidence for the quasi random assignment generated by the cut-off. Of course, at the time of measuring outcomes treatment group mothers are on average three months older than control group mothers; their average age at the time of giving birth, however, is perfectly balanced at 29.2 years.

The table shows that just under half of all mothers in our sample (49 per cent) are first-time mothers. 13 per cent of mothers are single moms. The share of migrants amounts to 26 per cent, and about a fifth of all mothers in our sample reside in East Germany. On average, mothers in our sample have 0.76 previous children.

The census data contain detailed information on individuals' educational outcomes according to ISCED categories (International Standard Classification of Education). While we use the detailed categories in the empirical analysis, for the purpose of presenting results

we classify the categories into three groups, low, medium and high education, respectively. *Low education* covers ISCED categories 1 and 2, i.e. education up to a general certificate of secondary schooling without an apprenticeship, and corresponds to about 21 per cent of mothers in our sample. The majority of mothers in this category should belong to the "unambiguous losers" of the policy change (A precise mapping of the categories "winners, losers, ambiguous" to educational or income categories does not exist.). *Medium education* (62 per cent) comprises ISCED 3a - 3c, 4, and 5b, and thus entails women with a completed apprenticeship, a high school diploma with no further post-secondary degree, master craftswomen and other lower tertiary degrees such as cooperative education or professional schools. *High education*, i.e. ISCED 5a and 6, contains mothers with degrees from universities and colleges of higher education, as well as PhDs, and corresponds to 17 per cent of the population. Although there might be some mothers in this category eligible for both the old and new benefit (e.g. graduates from the humanities at an early career stage), this category should generally be associated with the "unambiguous winners" of the policy change.

The information on mothers' pre-childbirth income that we use is not directly observable and therefore imputed using the 2006 wave of the census data. Average income amounts to 825 € per month.[8] Our expectations concerning the distribution of households (unambiguous winners, losers and ambiguous effect) in relation to household income corresponds to the one for educational categories. We use both terciles and quintiles as the relevant categories, and for consistency focus on terciles throughout the paper, because for the subgroup analyzes group sizes can become relatively small otherwise. The results for either terciles or quintiles are essentially the same in both quantitative and qualitative terms (and available upon request).

4 Results

In this section we present empirical estimates of the medium-run causal effects of the parental benefit on a set of key outcomes. First and foremost, we discuss the impacts on labor market outcomes, specifically employment rates, part-time and full-time employment, and hours

[8] Income information in the census data is limited, since it merges labor and capital earnings as well as transfers and is only available in form of 24 monthly income categories, the mean of which is taken in order to construct a continuous measure. The imputed income, which should be seen as an ordinal and not as a cardinal proxy for the mothers' earnings before childbirth, is obtained in a three step procedure of estimation, prediction and stratification (Step 1: work or not; step 2: full or part-time, step 3: Mincer type earnings functions), which is performed separately for first-time mothers and mothers who already had a child before. Additionally to the covariates in Table 1 also industry types and education*industry interactions are exploited. All mothers who are predicted not to have worked in 2006 enter with a zero net income.

worked. Second, we look at outcomes capturing the job match, specifically job continuity and job quality, as measured by type of labor contract (fixed-term vs. unlimited). The third subsection focuses on additional outcomes, in particular marriage rates and the probability of subsequent childbirth. Finally, we conduct sensitivity analyses using a difference-in-discontinuities version of the RD design to take into account seasonal effects, and we also present estimates for a restricted sample excluding families with childbirths in January 2007 and December 2006.

Ideally, in order to assess average treatment effects on the treated population, we would estimate an empirical model of the type

$$y_i = \alpha + X_i^{'}\beta + \gamma \textit{ Benefit-Receipt}_i + \epsilon_i \tag{2}$$

where y is the observed outcome, X a vector of observed covariates and γ the causal effect of the treatment; however, since the census data do not contain information on actual benefit receipt, the empirical model that is possible to implement is the following:

$$y_i = \alpha + X_i^{'}\beta + \gamma \textit{ Benefit-Eligibility}_i + \epsilon_i \tag{3}$$

That is, we are estimating an Intention-to-Treat parameter (ITT). This is a typical limitation in the empirical literature on parental benefits and childcare policies that, however, is not always made explicit. In our case, fortunately, the limitation is of little importance, because due to the generosity of the transfer and universal eligibility the take-up rate is close to 100 per cent (Destatis, 2008), such that without loss of generalization it can be assumed that virtually all parents that are intended-to-be-treated actually are treated.

4.1 Effects on Employment Rates

Table 2 presents empirical results for maternal employment rates. This table and the following results tables are structured as follows: We present three groups of columns, one each for Phase 1, Phase 2, and Phase 3, corresponding to the three phases delineated in Figure 1. That is, Phase 1 measures the short-term effect during benefit receipt, Phase 2 measures the short-term effect immediately after benefit exhaustion, and Phase 3 measures the medium-term effect we are interested in, three to five years after childbirth. Within each of these three groups of columns, we first present the base value, i.e. the average outcome in the control group, and then impact estimates from two empirical models. Model 1 merely uses a quadratic adjustment for differences in months since birth between treatment and control

Table 2: **Effects on Employment Rates**

	Phase 1			Phase 2			Phase 3		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.374	-.0327	-.0407*	.414	.0222	.0011	.537	.0132	.0179*
		(.0232)	(.0222)		(.0212)	(.0200)		(.0113)	(.0102)
	Family Types								
First-time mothers	.418	-.0308	-.0472	.473	.0414	.0270	.556	.0419***	.0331**
		(.0346)	(.0335)		(.0306)	(.0291)		(.0161)	(.0146)
More than one child	.336	-.0355	-.0396	.359	-.0020	-.0008	.518	-.0117	.0040
		(.0311)	(.0299)		(.0290)	(.0276)		(.0158)	(.0143)
Single mothers	.213	.0070	-.0271	.344	-.0304	-.0502	.476	.0482	.0512*
		(.0598)	(.0612)		(.0587)	(.0588)		(.0303)	(.0270)
	Educational Attainment								
Low education	.122	.0046	.00145	.190	-.0378	-.0444	.227	.0006	.0004
		(.0354)	(.0360)		(.0359)	(.0358)		(.0211)	(.0202)
Medium education	.417	-.0315	-.0322	.449	.0230	.0144	.592	.0142	.0088
		(.0300)	(.0295)		(.0272)	(.0267)		(.0141)	(.0134)
High education	.546	-.1170**	-.1110*	.571	.0597	.0564	.690	.0727***	.0603**
		(.0589)	(.0584)		(.0503)	(.0489)		(.0250)	(.0240)
	Income								
Lower tercile	.196	-.0251	-.0195	.196	.0062	.0106	.331	-.0118	-.0142
		(.0324)	(.0311)		(.0308)	(.0299)		(.0184)	(.0169)
Middle tercile	.384	-.0287	-.0303	.486	-.0166	-.0285	.595	.0361*	.0325*
		(.0411)	(.0414)		(.0369)	(.0370)		(.0193)	(.0184)
Upper tercile	.551	-.0692*	-.0807*	.547	.0548	.0506	.675	.0420**	.0432**
		(.0417)	(.0416)		(.0359)	(.0351)		(.0182)	(.0170)

Standard errors in parentheses; (*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Phase 1: 7-14 months; Phase 2: 15-24 months; Phase 2: 25-59 months after childbirth
Source: Own calculations based on German census data, waves 2006-2011, see Table 1

group. In addition, Model 2 adjusts for mothers' ISCED educational categories, regional indicators, number of older siblings, mothers' age, and household living arrangements.

In the top row, we present average treatment effects for all mothers. The tables then present heterogeneous treatment effect estimates stratifying by several socio-demographic characteristics. The first panel looks at family types (first-time mothers, mothers with older children; single mothers). The second panel distinguishes three educational groups, and the third panel distinguishes terciles of the imputed income distribution before childbirth.

The results for Phase 1 in Table 2 show the same pattern identified in previous research and intended by the policy: on average, mothers in the treatment group display a four percentage point lower probability (marginally significant) of being employed in the first 14 months after childbirth, a decrease of more than 10 per cent relative to a base probability of 0.374. The effect is mostly driven by the groups of mothers with high education and high

income mothers, i.e. the groups most strongly incentivized by the reform. The coefficient for first-time mothers is also large in size, though not significant at conventional levels.

In Phase 2, there are no significant average impacts, and also no significant impacts for the various socio-demographic groups. The striking pattern arises in Phase 3: Our point estimate for all mothers indicates an average treatment effect on the medium-term employment probability of 1.8 percentage points (marginally significant). This average effect is brought about by significant and sometimes large impacts for several subgroups: in particular for first-time mothers (3.3 percentage points), single mothers (5.1 percentage points), and mothers with high education and/or high income (6.0 and 4.3 percentage points, respectively) the new parental benefit caused a substantial increase in the employment rate up to five years after childbirth.

Table 3: **Effects on Part-time Employment**

	Phase 1			**Phase 2**			**Phase 3**		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.217	-.0373*	-.0427**	.292	.0093	.0009	.381	.0178	.0202*
		(.0192)	(.0191)		(.0196)	(.0190)		(.0111)	(.0105)
	Family Types								
First-time mothers	.208	-.0418	-.0528**	.312	.0283	.0137	.360	.0376**	.0286*
		(.0270)	(.0274)		(.0287)	(.0279)		(.0159)	(.0152)
More than one child	.226	-.0303	-.0338	.274	-.0131	-.0143	.402	-.0014	.0105
		(.0271)	(.0268)		(.0268)	(.0260)		(.0155)	(.0147)
Single mothers	.056	.0731*	.0658	.180	.0358	.0179	.310	.0462	.0548*
		(.0402)	(.0412)		(.0499)	(.0480)		(.0287)	(.0273)
	Educational Attainment								
Low education	.050	.0293	.0288	.109	.0013	-.0038	.179	.0060	.0304
		(.0261)	(.0273)		(.0298)	(.0298)		(.0193)	(.0186)
Medium education	.243	-.0489*	-.0469*	.317	.0215	.0181	.433	.0122	.0119
		(.0251)	(.0253)		(.0256)	(.0255)		(.0143)	(.0138)
High education	.340	-.1034*	-.1052**	.435	-.0510	-.0642	.427	.0834***	.0672**
		(.0532)	(.0538)		(.0507)	(.0504)		(.0279)	(.0276)
	Income								
Lower tercile	.122	-.0056	-.0021	.148	-.0119	-.0084	.259	-.0036	-.0073
		(.0271)	(.0271)		(.0269)	(.0264)		(.0171)	(.0160)
Middle tercile	.236	-.0115	-.0096	.332	.0279	.0228	.424	.0549***	.0583***
		(.0358)	(.0362)		(.0351)	(.0357)		(.0198)	(.0193)
Upper tercile	.298	-.1031***	-.1090***	.387	-.0013	-.0098	.455	.0175	.0174
		(.0352)	(.0355)		(.0355)	(.0352)		(.0197)	(.0191)

Standard errors in parentheses; (*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Phase 1: 7-14 months; Phase 2: 15-24 months; Phase 2: 25-59 months after childbirth
Source: Own calculations based on German census data, waves 2006-2011, see Table 1

Tables 3 and 4 follow the same structure as Table 2 and investigate whether the employment effects are generated by changes in part-time employment (Table 3) and/or

full-time employment (Table 4). Looking at the base probabilities in both tables, a first general observation is that across all subgroups more than two thirds of mothers are working in part-time positions during the five years after childbirth. This main pattern already present in the control group is exacerbated by the treatment: the estimates in Tables 3 and 4 show that the employment rate results are almost exclusively determined by changes in part-time employment. As Phase 1 in Table 3 indicates, part-time employment during benefit receipt is *reduced* by almost 20 per cent on average (a significant coefficient of 4.3 percentage points on a base probability of 0.217). For some groups - in particular the high income and high education mothers - this reduction is highly statistically significant and amounts to more than 30 per cent.

Phase 3 in Table 3, on the other hand, shows a 5 per cent *increase* in medium-run part-time employment for all mothers (top row, a marginally significant coefficient of .020 on a base probability of 0.381). Again, this average impact is particularly pronounced for some subgroups, especially mothers with high educational attainment (a significant 15 per cent increase implied by the coefficient) and mothers in the middle tercile of the income distribution (14 per cent increase). Also first-time mothers and single mothers benefit more strongly in terms of part-time employment.

Table 4 summarizes the results for full-time employment. Apart from a marginally negative coefficient for single mothers there seems to be no effect in *Phase 1.* This finding might be linked to the difficulties of finding appropriate child-care arrangements. In Phase 2 however, there is a strong positive effect (+10 percentage points) for highly educated mothers. We interpret this finding as evidence for a change in the sequencing of labor market return: under the old regulation, career-oriented mothers tended to resume employment with a part-time job after the expiry of the two month of maternity protection. Now, after the policy change, these mothers take a break for up to 14 months and return to their pre-birth full-time position thereafter. It is remarkable that this short-term positive effect on full-time employment has not been found by previous research, which in Phase 2 only detected the *benefit expiry effect* for mothers with low income.[9]

Concerning the medium-term impact, Table 4 indicates that there are very little changes in full-time employment at all, which suggest that the policy change primarily affected mothers with loose labor market attachment: the overall increase in maternal employment seems to be driven by mothers taking up a part-time job who under the old scheme would not have been working. These findings also suggest that there was little to no substitution

[9]Note that these studies use data collected close to the reform, comprising only information on the *planned* instead of the *actually realized* labor force status of mothers (Kluve et al., 2008; Kluve and Tamm, 2013; Bergemann and Riphahn, 2011).

Table 4: **Effects on Full-time Employment**

	Phase 1			Phase 2			Phase 3		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.157	.0047	.0197	.122	.0129	.0101	.155	-.0046	-.0023
		(.0177)	(.0172)		(.0144)	(.0141)		(.0082)	(.0078)
	Family Types								
First-time mothers	.210	.0110	.0056	.161	.0131	.0133	.194	.0043	.0045
		(.0291)	(.0288)		(.0228)	(.0224)		(.0130)	(.0123)
More than one child	.111	-.0051	-.0058	.085	.0111	.0064	.116	-.0102	-.0065
		(.0206)	(.0204)		(.0174)	(.0172)		(.0100)	(.0095)
Single mothers	.157	-.0660	-.0929*	.164	-.0662*	-.0681*	.166	.0020	-.0035
		(.0485)	(.0483)		(.0418)	(.0436)		(.0229)	(.0222)
	Educational Attainment								
Low education	.072	-.0247	-.0143	.081	-.0390*	-.0406*	.048	.0002	.0008
		(.0255)	(.0260)		(.0228)	(.0231)		(.0107)	(.0105)
Medium education	.174	.0174	.0147	.133	.0015	-.0037	.159	.0020	-.0031
		(.0235)	(.0230)		(.0186)	(.0184)		(.0106)	(.0101)
High education	.206	-.0140	-.0058	.136	.1113***	.1213***	.263	-.0106	.0070
		(.0475)	(.0469)		(.0402)	(.0398)		(.0245)	(.0237)
	Income								
Lower tercile	.073	-.0195	-.0174	.048	.0181	.0190	.072	-.0082	-.0069
		(.0206)	(.0199)		(.0179)	(.0178)		(.0099)	(.0097)
Middle tercile	.148	-.0172	-.0208	.154	-.0445*	-.0513**	.171	-.0188	-.0258*
		(.0294)	(.0300)		(.0249)	(.0246)		(.0147)	(.0141)
Upper tercile	.254	.0341	.0287	.160	.0561**	.0605**	.219	.0244	.0258
		(.0341)	(.0373)		(.0285)	(.0284)		(.0166)	(.0159)

Standard errors in parentheses; (*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Phase 1: 7-14 months; Phase 2: 15-24 months; Phase 2: 25-59 months after childbirth
Source: Own calculations based on German census data, waves 2006-2011, see Table 1

from full-time to part-time jobs in the medium-term, which stresses our interpretation of a brought-forward return to a full-time position.

Table 5 provides further evidence on the changing dynamic of mothers' return to work due to the new parental benefit. The table focuses on medium-run impacts (Phase 3 only) and presents estimates of the treatment effect on hours worked. The five panels distinguish the following categories: (i) not working, (ii) working 1-12 hours per week, (iii) working 13-22 hours per week, (iv) working 23-32 hours per week, and (v) working more than 32 hours per week. The results show a pronounced pattern driven by categories (i) and (iv): the first panel indicates a strong and highly significant reduction in non-participation (2.9 percentage points on average for all mothers; most pronounced for first-time mothers and mothers with high education). The fourth panel shows that the significant positive impacts on part-time employment are driven by the category "23-32 hours per week"; that is, by mothers allocating hours to the labor market at the upper bound of part-time employment.

Table 5: **Medium-run Effects on the Distribution of hours worked**

	Not Working			1-12 hours			13-22 hours			23-32 hours			More than 32 hours		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.473	-.0232**	-.0292***	.118	-.0091	-.0080	.148	.0125	.0148*	.109	.0206***	.0217***	.151	-.0008	.0008
		(.0115)	(.0108)		(.0073)	(.0073)		(.0084)	(.0083)		(.0076)	(.0074)		(.0083)	(.0079)
	Family Types														
First-time mothers	.455	-.0470***	-.0467***	.0832	.0069	.0061	.152	-.0109	-.0112	.120	.0432***	.0421***	.190	.0077	.0097
		(.0164)	(.0155)		(.0093)	(.0093)		(.0117)	(.0115)		(.0116)	(.0115)		(.0131)	(.0125)
More than one child	.492	-.00127	-.0123	.152	-.0257**	-.0203*	.144	.0349***	.0391***	.099	-.0005	-.0005	.114	-.0075	-.0060
		(.0162)	(.0151)		(.0113)	(.0111)		(.0120)	(.0119)		(.0097)	(.0096)		(.0102)	(.0098)
Single mothers	.532	-.0541*	-.0555*	.049	.0212	.0253*	.121	-.0185	-.0197	.129	.0521**	.0561**	.168	-.00067	-.0063
		(.0311)	(.0287)		(.0150)	(.0151)		(.0198)	(.0197)		(.0228)	(.0224)		(.0236)	(.0230)
	Educational Attainment														
Low education	.779	-.0173	-.0200	.0742	.00743	.0070	.069	.0007	-.0010	.030	.0095	.0114	.0485	-.0003	.0026
		(.0217)	(.0214)		(.0139)	(.0138)		(.0131)	(.0129)		(.0095)	(.0096)		(.0111)	(.0108)
Medium education	.421	-.0231	-.0187	.144	-.0237**	-.0188*	.159	.0186*	.0202*	.121	.0225**	.0181*	.155	.0057	-.0009
		(.0144)	(.0143)		(.00994)	(.0098)		(.0110)	(.0110)		(.0100)	(.0099)		(.0107)	(.0103)
High education	.314	-.0694***	-.0697***	.073	.0260	.0227	.202	.0113	.0016	.156	.0392*	.0427**	.256	-.0071	.0027
		(.0252)	(.0249)		(.0159)	(.0161)		(.0229)	(.0228)		(.0215)	(.0216)		(.0246)	(.0240)
	Income														
Lower tercile	.675	.0019	.0009	.109	-.00558	-.0074	.090	.0157	.0147	.051	-.0012	.0005	.075	-.0109	-.0079
		(.0188)	(.0178)		(.0124)	(.0122)		(.0120)	(.0118)		(.0088)	(.0088)		(.0102)	(.0100)
Middle tercile	.420	-.0452**	-.0451**	.153	-.0264*	-.0181	.140	.0536***	.0589***	.121	.0316**	.0253*	.165	-.0136	-.0210
		(.0197)	(.0195)		(.0141)	(.0138)		(.0152)	(.0151)		(.0140)	(.0138)		(.0148)	(.0144)
Upper tercile	.333	-.0421**	-.0464**	.092	.0025	.0024	.212	-.0262	-.0264*	.153	.0370**	.0394***	.211	.0287*	.0310*
		(.0184)	(.0182)		(.0116)	(.0117)		(.0159)	(.0158)		(.0151)	(.0150)		(.0167)	(.0161)

Standard errors in parentheses; (*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Phase 1: 7-14 months; Phase 2: 15-24 months; Phase 2: 25-59 months after childbirth
Source: Own calculations based on German census data, waves 2006-2011, see Table 1

The highly significant average impact estimate of 2.2 percentage points implies a 20 per cent increase in the mothers' probability to work this number of hours. Table 5 therefore further emphasizes the pattern that the new parental benefit in the medium-run causes mothers to move out of non-participation into part-time jobs with a large number of hours. The subgroup results appear evident: first-time mothers (who need to combine part-time work with taking care of one child) display the largest impact in the "23-32 hours" category, while mothers who already have older children display the largest impact in the "13-22 hours" category.

4.2 Effects on Job Continuity and Job Quality

The census data allow us not only to look at the employment and hours-of-work decisions of mothers, but also at measures of job continuity and job quality. Table 6 presents results on the medium-run impact of the parental benefit on mothers' probability of being employed in the same job that they worked in prior to childbirth. The first panel shows that the average unconditional probability for all mothers increased by 3.1 percentage points (highly significant), i.e. a more than 10 per cent increase over a base probability of 29.7 per cent. The second panel estimates impacts on the probability of job continuity conditional on being employed in a part-time job. The corresponding coefficient is insignificant for the average of all mothers. This result is not surprising, given the findings of the previous section: since the absolute number of mothers in part-time employment has strongly increased in the treatment group relative to the control group, it cannot be expected that, conditional on having a part-time job, the share of continuous jobs increases. In fact, the insignificant coefficient implies a constant share of maintained part-time jobs among all part-time jobs, i.e. effectively the absolute number of mothers who continue in the same part-time job in which they worked prior to childbirth has also increased.

This implicit positive treatment effect on job continuity becomes apparent in the third panel of Table 6. From the previous section we know that the probability of having a full-time job has not increased significantly for mothers in the treatment group. What has increased, however, is the probability of continuing with the same employer, conditional on having a full-time job. On average, the probability of job continuity is 6.9 percentage points higher in the treatment group, relative to a base probability of 59.9 per cent in the control group. The effect is statistically significant.

Taken together, the findings discussed so far in Tables 2 through 6 point to pronounced beneficial medium-run labor market effects of the new parental benefit. First, the policy caused a significant share of mothers in the treatment group to move from non-

Table 6: **Medium-run Effects on the Probability of Job Continuity**

	Unconditional			Part-time Job			Full-time Job		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.297	.0257**	.0307***	.550	-.0013	.0139	.599	.0813***	.0689**
		(.0108)	(.0101)		(.0185)	(.0177)		(.0285)	(.0267)
	Family Types								
First-time mothers	.313	.0375**	.0346**	.570	-.0051	.0173	.582	.0601	.0149
		(.0156)	(.0147)		(.0268)	(.0255)		(.0365)	(.0352)
More than one child	.281	.0152	.0250*	.532	.0012	.0122	.626	.1221***	.1643***
		(.0148)	(.0140)		(.0256)	(.0247)		(.0451)	(.0447)
Single mothers	.222	-.00547	.0021	.486	- .0928*	- .0144	.451	.0171	.0656
		(.0261)	(.0240)		(.0544)	(.0521)		(.0763)	(.0726)
	Educational Attainment								
Low education	.076	-.0075	-.0077	.380	-.132**	-.134**	.206	.2351**	.1652*
		(.0133)	(.0132)		(.0558)	(.0543)		(.107)	(.0934)
Medium education	.334	.0291**	.0249*	.553	.00522	.0176	.641	.0775**	.0674**
		(.0140)	(.0137)		(.0221)	(.0214)		(.0342)	(.0330)
High education	.416	.0824***	.0832***	.616	.0459	.0500	.590	.0567	.0425
		(.0281)	(.0274)		(.0395)	(.0389)		(.0549)	(.0525)
	Income								
Lower tercile	.152	.0012	.0019	.452	.0001	.0093	.517	.0264	.0252
		(.0145)	(.0137)		(.0398)	(.0375)		(.0768)	(.0685)
Middle tercile	.305	.0438**	.0457**	.519	-.0096	.0157	.547	.1582***	.1591***
		(.0191)	(.0185)		(.0305)	(.0295)		(.0479)	(.0470)
Upper tercile	.426	.0458**	.0478**	.627	.0161	.0189	.664	.0391	.00955
		(.0199)	(.0196)		(.0281)	(.0280)		(.0393)	(.0389)

Standard errors in parentheses; (*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Phase 1: 7-14 months; Phase 2: 15-24 months; Phase 2: 25-59 months after childbirth
Source: Own calculations based on German census data, waves 2006-2011, see Table 1

participation into employment. Second, the probability to work in part-time employment has increased significantly for mothers in the treatment group, a finding reinforced by the result that the number of working hours supplied has also increased. Despite the strong increase in part-time employment in the treatment group, the *share* of mothers working part-time who continue post-birth in their pre-birth job has remained constant, i.e. increased in absolute numbers. Third, whereas the treatment effect on the probability of full-time employment is insignificant, the impact estimate on the probability to continue with the same employer is large and significant for full-time employed mothers.

Table 7 allows us to investigate further these pronounced employment effects by also looking at job quality. We measure job quality by contract type, looking at the fraction of mothers employed with an unlimited, i.e. open-ended, contract instead of a fixed-term contract. Unlimited contracts imply a more stable, secure job match and are deemed highly desirable by workers in Germany. As the base probabilities for part-time and full-time jobs

Table 7: **Medium-run Effects on the Probability of Unlimited Contracts**

	Unconditional			Part-time Job			Full-time Job		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.459	.0354***	.0398***	.863	.0229*	.0301**	.791	.0726***	.0692***
		(.0116)	(.0108)		(.0129)	(.0128)		(.0246)	(.0211)
	Family Types								
First-time mothers	.480	.0636***	.0555***	.845	.0430**	.0563***	.757	.0905***	.0623**
		(.0165)	(.0154)		(.0191)	(.0195)		(.0321)	(.0273)
More than one child	.438	.00931	.0218	.879	.00467	.00753	.849	.0468	.0861**
		(.0162)	(.0152)		(.0175)	(.0171)		(.0370)	(.0345)
Single mothers	.339	.0293	.0287	.807	-.0179	-.0223	.696	-.0380	.0108
		(.0300)	(.0277)		(.0464)	(.0467)		(.0778)	(.0604)
	Educational Attainment								
Low education	.161	-.00655	-.00743	.802	-.0584	-.0413	.167	.1552	.1112
		(.0188)	(.0184)		(.0526)	(.0528)		(.1081)	(.0913)
Medium education	.538	.0391***	.0342**	.879	.0309**	.0356**	.847	.0800***	.0801***
		(.0146)	(.0144)		(.0142)	(.0143)		(.0251)	(.0243)
High education	.514	.0999***	.101***	.839	.0361	.0404	.808	.0345	.0406
		(.0280)	(.0277)		(.0317)	(.0306)		(.0483)	(.0463)
	Income								
Lower tercile	.271	-.0078	-.0056	.874	-.0254	-.0306	.600	.0639	.0915
		(.0178)	(.0165)		(.0293)	(.0273)		(.0850)	(.0592)
Middle tercile	.500	.0766***	.0774***	.848	.0542***	.0634***	.798	.0795**	.0574
		(.0202)	(.0199)		(.0205)	(.0214)		(.0404)	(.0380)
Upper tercile	.597	.0527***	.0534***	.871	.0188	.0248	.848	.0548*	.0568*
		(.0194)	(.0191)		(.0201)	(.0196)		(.0301)	(.0291)

Standard errors in parentheses; (*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Phase 1: 7-14 months; Phase 2: 15-24 months; Phase 2: 25-59 months after childbirth
Source: Own calculations based on German census data, waves 2006-2011, see Table 1

in Table 7 indicate - 86.3 per cent and 79.1 per cent, respectively, of working mothers in the control group have an unlimited contract - this contract type also constitutes the most common contractual arrangement. It is thus surprising that the policy achieves a highly significant positive impact on these probabilities: the estimated average increase for all mothers in part-time jobs is 3 percentage points, and for mothers in full-time jobs it is on average 6.9 percentage points. This is an enormous impact considering the already high base probability.

4.3 Effects on Demographic Outcomes

In addition to the detailed labor market results we can also investigate demographic outcomes. Table 8 presents impact estimates of the probability of being married over the three phases. The estimates indicate a significant reduction in the marriage probability that is visible in the short-run (Phase 1) but continues through the medium-run (Phase 3). The

Table 8: **Effects on Marriage Rates**

	Phase 1			Phase 2			Phase 3		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.754	-.0232	-.0398**	.777	-.0273	-.0221	.784	-.0234**	-.0237***
		(.0212)	(.0165)		(.0183)	(.0140)		(.0096)	(.0073)
First-time mothers	.667	-.0481	-.0846***	.676	-.0034	-.0288	.721	-.0376**	-.0393***
		(.0337)	(.0271)		(.0287)	(.0223)		(.0150)	(.0116)
More than one child	.830	.0021	-.0047	.872	-.0415*	-.0169	.845	-.0138	-.0059
		(.0250)	(.0199)		(.0216)	(.0172)		(.0117)	(.0089)
	Educational Attainment								
Low education	.624	.0339	-.0211	.692	.0027	.0090	.708	-.0026	-.0224
		(.0516)	(.0391)		(.0442)	(.0304)		(.0228)	(.0299)
Medium education	.772	-.0362	-.0331	.786	-.0440*	-.0259	.785	-.0307**	-.0272***
		(.0264)	(.0208)		(.0231)	(.0179)		(.0122)	(.0092)
High education	.851	-.0565	-.0691*	.847	-.0201	-.0335	.862	-.0062	-.0101
		(.0452)	(.0384)		(.0382)	(.0342)		(.0195)	(.0174)
	Income								
Lower tercile	.740	.0019	-.0269	.792	.0293	.0043	.806	-.0019	-.0044
		(.0353)	(.0258)		(.0305)	(.0223)		(.0156)	(.0111)
Middle tercile	.708	.0124	.0059	.782	-.0554*	-.0151	.773	-.0320*	-.0325**
		(.0386)	(.0309)		(.0318)	(.0245)		(.0171)	(.0135)
Upper tercile	.787	-.0787**	-.0908***	.758	-.0478	-.0476*	.773	-.409***	-.0329**
		(.0362)	(.0300)		(.0322)	(.0260)		(.0170)	(.0131)

Standard errors in parentheses; (*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Phase 1: 7-14 months; Phase 2: 15-24 months; Phase 2: 25-59 months after childbirth
Source: Own calculations based on German census data, waves 2006-2011, see Table 1

average reductions for all mothers amount to 4 percentage points during benefit receipt (base probability 75.4 per cent) and 2.4 percentage points in the medium-run (base probability 78.4 per cent). These somewhat unexpected effects are mostly driven by first-time mothers and high income mothers.

A possible explanation for this finding is the interaction of the parental benefit with the German system of joint personal income taxation. The benefit payments themselves are indeed tax-free; they are, however, subject to the progressivity proviso, which significantly reduces the income tax splitting advantage for married couples. Due to this fact, apparently, the reform makes it less lucrative for parents to get married. This effect is most pronounced during the immediate phase of benefit receipt, but seems to persist to a significant degree in the medium-run.

As explained in section 2, the parental benefit policy also intends to provide incentives for fertility. While no methodological design exists to identify the causal effect of the policy on overall fertility, we can use our RD design to at least investigate impacts on subsequent childbirths. It is important to emphasize that this analysis cannot say anything about the

overall number of births and any impact of the policy on women's decision to have the first child.

Table 9 depicts the empirical estimates of the policy on mothers' probability of having a subsequent child. The overall impact estimate is small in size (-0.7 percentage points on a base probability of 0.145, i.e. a 5 per cent reduction), but highly significant. The subgroup results show that the overall effect is almost exclusively determined by a strong reduction in the probability of younger mothers (<=29 years of age) to have a subsequent child; with some effect also visible in the low-income group. The channel through which the policy brings about this somewhat anticlimactic and also unexpected finding is difficult to identify; the findings may have to do, however, with the joint decision of returning to work at a higher rate while having the subsequent child (within a 5 year bracket) at a lower rate.

4.4 Sensitivity Analysis

In order to check the validity of our identification strategy we conduct two sensitivity analyses: a difference-in-discontinuities design and a re-estimation using a restricted sample. The results for these sensitivity analyses are presented in the appendix in Tables A1 and A2, respectively. In line with the emphasis of this study we focus on the medium-term estimates only (Phase 3) and report results for the following outcomes - employment rate, part-time and full-time employment, unlimited contracts, marriage behavior and one subsequent childbirth - such that Tables A1 and A2 have six panels each.

The first robustness check combines the Regression Discontinuity approach with a difference-in-differences design. This *difference-in-discontinuities* approach (see for instance Grembi et al., 2012) uses the groups of parents with children born in the last quarter of 2005 and the first quarter of 2006 to control for any *time-invariant seasonal differences* between the groups surrounding the turn of the year. This approach allows us to test whether there are general differences in the labor market chances between mothers with a childbirth in the fourth quarter compared to the first quarter of the year. Such differences could for instance be caused by seasonal fluctuations in labor demand, but also by calender effects such as the start of the kindergarten year. In other words, this approach allows us to test the assumption that the distant observations in our treatment and control window are still comparable.

The difference-in-discontinuities results in Table A1 generally show impact estimates that are slightly more pronounced, but the overall picture shows very similar, robust findings and no qualitative differences. Thus, the results make us confident that seasonality issues do not play a role in our identification strategy. Specifically, there are no statistically significant differences between the treatment effect estimates presented in the previous sections and the

Table 9: **Medium-run Effects on the Probability of Subsequent Childbirth**

	One Subsequent Birth			Multiple Subsequent Births		
	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.145	-.0105**	-.0072***	.023	-.0007**	-.0004***
		(.0046)	.0028		(.0003)	(.0001)
First-time mothers	.210	-.0183**	-.0129**	.026	-.0009**	-.0005**
		(.0083)	(.0056)		(.0004)	(.0002)
More than one child	.082	-.0034	-.0040	.019	-.0003	-.0003
		(.0047)	(.0037)		(.0004)	(.0003)
	Mothers$\leq$ 29 Years at birth					
All mothers	.185	-.0287***	-.0205	.031	-.0018***	-.0010***
		(.0071)	(.0049)		(.0005)	(.0003)
First-time mothers	.234	-.0427***	-.0272***	.030	-.0019***	-.0010***
		(.0107)	(.0076)		(.0005)	(.0003)
More than one child	.112	-.0136*	-.0141	.034	-.0015*	-.0014
		(.0082)	(.0094)		(.0009)	(.0010)
	Mothers > 30 Years at birth					
All mothers	.091	.0134**	.0055	.011	.0008**	.0003
		(.0056)	(.0041)		(.0004)	(.0002)
First-time mothers	.155	.0295**	.0149	.018	.0016**	.0006
		(.0129)	(.0724)		(.0008)	(.0035)
More than one child	.055	.0070	.0019	.007	.0005	.0001
		(.0054)	(.0035)		(.0004)	(.0002)
	Educational Attainment					
Low education	.154	-.0213**	-.0138*	.034	-.0019*	-.0010**
		(.0108)	(.0075)		(.0010)	(.0005)
Medium education	.124	-.0001	-.0027	.019	-.0000	-.0001
		(.0054)	(.0034)		(.0003)	(.0002)
High education	.209	-.0344***	-.0151	.023	-.0017**	-.0005
		(.0132)	(.0355)		(.0007)	(.0012)
	Income					
Lower tercile	.140	-.0194**	-.0122**	.028	-.0015**	-.0008**
		(.0080)	(.0057)		(.0007)	(.0004)
Middle tercile	.110	-.0032	-.0065	.018	-.0002	-.0003
		(.0069)	(.0042)		(.0004)	(.0002)
Upper tercile	.183	-.0067	-.0045	.022	-.0004	-.0002
		(.0089)	(.0063)		(.0005)	(.0003)

Standard errors in parentheses
(*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Marginal effects from an ordered logistic regression
Source: Own calculations based on German census data, waves 2006-2011

results from the difference-in-discontinuities regression. The observation that most of the point estimates are even larger indicates that our main estimates may be rather conservative and might actually represent a lower bound for the treatment effects.

The second sensitivity analysis addresses the concern about non-random sorting discussed in section 3.1. We re-estimate effects using a restricted sample excluding the January and December births. Thus, we reduce the size of our dataset by roughly one third, leaving out those cases in which parents potentially might have tried to shift the childbirth from 2006 to 2007. Ideally, this analysis would use different specifications varying the size of the excluded sample; our data, however, only contain the month of birth, but not the exact day of birth.[10] The advantage of excluding one month on each side of the cut-off is that we can be sure that we eliminated all potential shifters. Given that in our RD set-up the distance from the discontinuity within the 6-month window [Oct 1, 2006; March 31, 2007] does not affect the comparability of treatment and control observations, the only way in which this restriction affects our strategy is by reducing the sample size.

The results in Table A2 indicate that also for this second sensitivity check there are no qualitative or quantitative differences compared to our main results. Clearly, due to the sizeable reduction in sample size the standard errors are larger, resulting in generally lower levels of significance. But since the overall picture of impact estimates remains unchanged, we conclude that our identification strategy is valid also with respect to the random sorting around the cut-off date.

5 Discussion

This paper provides new evidence on a particularly interesting public policy impacting on female labor supply, a new and generous parental benefit regulation implemented in Germany in 2007. Our paper estimates the *medium-run effects* of the policy (up to 5 years after childbirth) using a Regression Discontinuity (RD) design generated by the coming-into-effect of the regulation. Assignment to treatment is a deterministic function of the date of birth of the child in real time: on the first minute of January 1, 2007, the probability of being eligible for the benefit switches from zero to one, giving rise to a sharp discontinuity. As the RD design hinges on observing units arbitrarily close to the cut-off, we argue that the legislative process generates quasi-random assignment to treatment and control groups

[10] Due to this lack of information we cannot use histogram techniques, kernel density estimates, nor formal tests such as the one proposed by McCrary (2008).

within a neighborhood of three months before and after the cut-off date. This allows us to estimate unbiased causal effects of the policy change.

Our estimates indicate several significant and striking patterns. First, we find pronounced positive medium-term effects on mothers' employment rates, in particular for women with high education, women in the upper tercile of the income distribution, and for first-time mothers. For the latter groups, the probability of being employed is up to 6 percentage points higher than in the control group (Average base probability: 54 per cent , i.e. an increase of more than 10 per cent).

Second, we find that the positive medium-run employment impact of the parental benefit is almost exclusively driven by increases in part-time employment. Just over two thirds of all working mothers in the control group are working part-time, so this share is further increased by the policy. Impact estimates of increases in the probability of being in full-time employment are close to zero in size and insignificant. Looking at hours worked we find that the positive employment impact is strongest in the category "23 to 32 hours", i.e. among those women in part-time employment who allocate a large number of hours to the labor market.

Third, the analysis indicates that employers reward this increasingly fast and sizeable return of mothers to work in two important ways: on the one hand, the parental benefit significantly increases the probability of job continuity. That is, mothers in the treatment group return at significantly higher rates to the same employer for whom they worked prior to childbirth. On the other hand, job quality also increases: we find highly significant and relatively large treatment effects on the probability of having an unlimited contract, conditional on being employed. For the average of all mothers, our estimate indicates a statistically significant 3.7 percentage point increase in the treatment group over a base probability of 85.7 per cent in the control group. The point estimates are up to 6.7 percentage points for some subgroups (middle tercile of the income distribution).

Fourth, we also find effects on demographic outcomes. Marriage rates in the treatment group are significantly lower than in the control group. We explain this finding by interactions of the parental benefit with the German system of joint personal income taxation. Whereas benefit payments are tax-free, they are subject to the progressivity proviso, which significantly reduces the income tax splitting advantage for married couples. Thus, the reform made it less attractive to get married. We also find a small but significant negative effect on the probability of having a subsequent child within the 5-year bracket. As this effect is more pronounced for younger mothers we conjecture that it is related to the joint decision of returning to work at a higher rate.

The strong medium-term patterns of the significantly positive impacts on employment rates and hours worked on the one hand and the significantly positive impacts on job continuity and job quality on the other hand can hardly be explained with the pure financial transfer provided in Phase 1: the income shock may be (very) large for some groups, but it is transitory and does neither affect permanent income nor the medium-term budget restriction. It is also virtually impossible to conceive of a plausible story that directly links certain aspects of parental benefit policy to job quality results. And whereas the design of the policy does incentivize fathers to contribute to childcare to some extent, the resulting potential changes in the division of labor cannot be large enough to explain the profound labor market effects we observe.[11]

Instead, we believe that the strong patterns provide evidence for an unprecedented change in social norms. This change in social norms has been brought about in the following way: the new parental benefit defines an "anchor" at the end of benefit receipt, i.e. a societally preferred point in time at which mothers return to work. Both under the old and new regulations mothers who had worked before childbirth have always been entitled to take up to three years of unpaid parental leave. Choosing the exact time period was an individual decision, since there was no "natural" point in time to return to the job. In practice, the chosen durations indeed show substantial variation and most mothers did not exploit the full potential parental leave period of three years (Schönberg and Ludsteck, 2014).

After the policy change, however, the new parental benefit - that explicitly takes into account the opportunity costs of childbearing for working mothers - defines for this group for the first time the point of "natural" return: at the end of benefit receipt. Whereas the gradual extension of the unpaid leave entitlement in the 1980s had led to a delayed return to work of mothers (ibid.), our evidence suggests that the new parental benefit implies a strong message to mothers: do return to work, and do so at the end of benefit entitlement.

This change in social norms is even more remarkable in that it does not only affect supply-side decisions of mothers. We find coherent evidence that in addition to influencing mothers' planning horizon the "anchor" generated by the reform also shapes employers' expectations. This development is substantiated by the large and significant positive impacts on job continuity. That is, caused by the new parental benefit mothers return to their previous position at a higher rate and the job match is maintained. Moreover, this return to the same job is rewarded by employers through improving the job quality with more open-ended contracts.

[11] Kluve and Tamm (2013) do not find any evidence that the new policy increased the fathers' time devoted to childcare. Analogously, Ekberg et al. (2013) analyze the impact of a similar "daddy month" reform in Sweden and also do not find any learning-by doing or specialization effects later in life.

Our estimates corroborate the hypothesis that parental benefit policies can have profound impacts on female labor supply behavior, positively affecting an entire set of labor market outcomes. These findings are of even broader significance, given that the evidence shows that policies increasing female labor force participation over the lifecycle are exactly the policies needed to further bring down the remaining gender wage gap (Polachek, 2006). The channel to reduce the gender wage gap may not only be labor force participation, but also the increase in hours supplied. As Goldin (2014) points out, differences in hours worked contribute significantly to the gender wage gap. Hence, the hours-increasing aspect of the new parental benefit is likely to have a narrowing effect on gender pay differences in Germany.

Taking into account also the effects on demographic outcomes, we find that the reform does not imply solely positive results. The small but significant decrease in the probability of having a subsequent child certainly contradicts the intentions of the policy. As emphasized before, however, we cannot identify effects on overall fertility, above all not for the probability among all women to become mothers at all. Since the effect that we measure is most strongly pronounced for younger women (below 30 years of age), it might still be the case that due to the policy these women return to the labor market now and have the subsequent child later, outside the 5-year bracket.

With respect to the second demographic outcome, the normative implications of the slightly but significantly lower marriage rate in the treatment group are not at all obvious; from a policy perspective this might only be worrying if a decrease in marriage probability came with a decrease in the stability of the parental relationship. This is not necessarily the case. Also, the diminishing effect size over time might indicate that the channel through which the effect comes about, tax savings, is relevant mostly during the period of benefit receipt, and that in the long-run marriage rates of treatment and control groups may converge.

All in all, it is remarkable to see that this type of public policy can cause profound changes in individual and societal outcomes. From the perspective of an ageing society with low female employment rates and a persistent gender wage gap, the impact patterns we observe prove that the policy changes many variables in a desirable way. This change comes at a cost, though: the average annual expenditure on the policy has been in the order of 5 € billion ($7 billion). At roughly 0.15 per cent of GDP and in light of the sizeable impacts, however, this does not appear exceptionally costly compared to e.g. the expenditure on labor market policies, which considerably exceeds this amount.

Our findings allow us to draw policy conclusions that are applicable also beyond the German context. Previous research on parental leave and benefit policies seemed to indicate

that parental benefit payments generally had an adverse effect on female labor participation, opposed to the potentially beneficial effect of the job-protecting element of unpaid parental leave regulations. Our positive impact estimates for the newly eligible mothers show that also parental benefit schemes can enhance female labor participation substantially in the medium-run by institutionalizing a specific point in time for the return to the job. Moreover, benefit entitlements that are short and generous increase the continuity of full-time employment relationships, since they appropriately bridge the time period during which mothers cannot work full-time given the childcare duties immediately after childbirth. These seem to be key design features for effective policies aiming to increase mothers' labor supply.

Appendix - Sensitivity Analysis

Table A1: **Difference-in-Discontinuities Estimation: Medium-run Effects**

	Employment Rates			Part-time Employment			Full-time Employment		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.527	.0265	.0303**	.375	.0397**	.0402***	.152	-.0132	-.0099
		(.0164)	(.0154)		(.0160)	(.0155)		(.0114)	(.0109)
First-time mothers	.545	.0637***	.0570***	.356	.0643***	.0568**	.190	-.0006	.0002
		(.0233)	(.0221)		(.0229)	(.0223)		(.0181)	(.0174)
More than one child	.508	-.0061	.0041	.395	.0167	.0217	.114	-.0227	-.0176
		(.0229)	(.0214)		(.0224)	(.0216)		(.0139)	(.0134)
	Educational Attainment								
Low education	.221	.0251	.0120	.173	.0103	-.0014	.0486	.0148	.0134
		(.0303)	(.0301)		(.0273)	(.0271)		(.0165)	(.0162)
Medium education	.579	.0190	.0144	.424	.0275	.0251	.155	-.0085	-.0107
		(.0204)	(.0202)		(.0206)	(.0205)		(.0145)	(.0140)
High education	.686	.1009***	.1049***	.430	.1465***	.1383***	.256	-.0456	-.0333
		(.0367)	(.0364)		(.0402)	(.0399)		(.0346)	(.0335)
	Income								
Lower tercile	.325	-.0080	-.0110	.250	.0018	-.0023	.075	-.0098	-.0087
		(0269)	(.0252)		(.0252)	(.0238)		(.0144)	(.0141)
Middle tercile	.580	.0472*	.0475*	.414	.0856***	.0864***	.165	-.0384*	-.0389*
		(.0286)	(.0281)		(.0288)	(.0282)		(.0207)	(.0200)
Upper tercile	.667	.0210	.0378	.457	.0173	.0197	.211	.0037	.0181
		(.0260)	(.0255)		(.0277)	.0275		(.0223)	(.0216)
	Unlimited Contracts			**Marriage Behavior**			**Childbirth(1)**		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.858	.0297**	.0380***	.787	-.0299**	-.0301***	.178	-.0215***	-.0111***
		(.0145)	(.0137)		(.0136)	(.0103)		(.0063)	(.0042)
First-time mothers	.836	.0346*	.0389**	.724	-.0422**	-.0358**	.250	-.0432***	-.0232***
		(.0205)	(.0190)		(.0214)	(.0162)		(.0114)	(.0085)
More than one child	.884	.0255	.0349*	.850	-.0224	-.0204	.109	-.0041	-.0044
		(.0201)	(.0195)		(.0166)	(.0129)		(.0066)	(.0046)
	Educational Attainment								
Low education	.698	-.0805	-.0219	.709	-.0232	-.0460**	.203	-.0389***	-.0203**
		(.0719)	(.0646)		(.0329)	(.0235)		(.0136)	(.0102)
Medium education	.886	.0336**	.0397***	.790	-.0289*	-.0213	.149	-.0047	-.0040
		(.0152)	(.0149)		(.0173)	(.0131)		(.0081)	(.0051)
High education	.831	.0682**	.0528	.868	-.0250	-.0469*	.253	-.0549***	-.0249**
		(.0341)	(.0324)		(.0275)	(.0240)		(.0167)	(.0111)
	Income								
Lower tercile	.835	-.0075	-.0042	.804	-.0168	-.0216	.178	-.0263**	-.0142**
		(.0351)	(.0317)		(.0224)	(.0164)		(.0099)	(.0073)
Middle tercile	.850	.0296	.0511**	.777	-.0450*	-.0437**	.135	-.0112	-.0102
		(.0258)	(.0251)		(.0253)	(.0192)		(.0106)	(.0064)
Upper tercile	.875	.0431**	.0439**	.780	-.0275	-.0216	.221	-.0237*	-.0075
		(.0197)	(.0189)		(.0231)	(.0178)		(.0123)	(.0081)

Standard errors in parentheses; (*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Source: Own calculations based on German census data, waves 2005-2011, see Table 1

Table A2: **Restricted sample excluding mothers with childbirth in 12/2006 and 1/2007: Medium-run Effects**

	Employment Rates			Part-time Employment			Full-time Employment		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.520	.0283	.0464**	.370	.0377*	.0457**	.151	-.0094	.0008
		(.0200)	(.0187)		(.0196)	(.0190)		(.0142)	(.0139)
First-time mothers	.534	.0427	.0456*	.359	.0368	.0357	.174	.0059	.0099
		(.0278)	.0264		(.0272)	0.0264		(.0214)	(.0207)
More than one child	.506	.0145	.0431	.381	.0392	.0571**	.125	-.0247	-.0140
		(.0288)	(.0266)		(.0283)	(.0273)		(.0182)	(.0182)
	Educational Attainment								
Low education	.200	.0037	.0075	.158	.0116	-.0021	.042	-.0079	.0095
		(.0354)	(.0374)		(.0327)	(.0339)		(.0167)	(.0195)
Medium education	.578	.0425*	.0489**	.416	.0549**	.0624**	.162	-.0124	-.0135
		(.0250)	(.0248)		(.0254)	(.0252)		(.0186)	(.0184)
High education	.679	.0767*	.0661	.446	.0465	.0385	.233	.0302	.0276
		(.0434)	(.0434)		(.0486)	(.0490)		(.0426)	(.0416)
	Income								
Lower tercile	.324	-.0261	-.0007	.238	-.0040	.0155	.087	-.0222	-.0162
		(.0317)	(.0303)		(.0292)	(.0281)		(.0180)	(.0183)
Middle tercile	.561	.0509	.0711**	.402	.0785**	.0943***	.160	-.0277	-.0232
		(.0352)	(.0349)		(.0357)	(.0355)		(.0253)	(.0258)
Upper tercile	.676	.0707**	.0650**	.471	.0457	.0379	.205	.0251	.0270
		(.0310)	(.0311)		(.0344)	(.0344)		.0285	(.0278)
	Unlimited Contracts			**Marriage Behavior**			**Childbirth**		
	Base	Model 1	Model 2	Base	Model 1	Model 2	Base	Model 1	Model 2
All mothers	.851	.0402**	.0367**	.763	.0132	-.0122	.178	-.0107	-.0116
		(.0183)	(.0180)		(.0169)	(.0133)		(.0082)	(.0050)
First-time mothers	.812	.0778***	.0603**	.706	-.0113	-.0328*	.237	-.0201	-.0183**
		(.0261)	(.0254)		(.0257)	(.0197)		(.0140)	(.0092)
More than one child	.897	-.0046	-.0023	.825	.0379	.0170	.115	-.0017	-.0052
		(.0253)	(.0257)		(.0259)	(.0173)		(.0089)	(.0060)
	Educational Attainment								
Low education	.674	.0004	-.0923	.637	.0122	-.0011	.189	-.0202	-.0173
		(.0928)	(.0934)		(.0396)	(.0316)		(.0189)	(.0140)
Medium education	.885	.0458**	.0426	.782	-.0139	-.0096	.163	-.0072	-.0118**
		(.0184)	(.0193)		(.0214)	(.0167)		(.0097)	(.0057)
High education	.804	.0221	0.0080	.834	-.0029	-.0251	.219	-.0038	-.0036
		(.0487)	0.0502		(.0368)	(.0347)		(.0234)	(.0187)
	Income								
Lower tercile	.881	-.0329	-.0447	.787	.0338	-.0061	.189	-.0204	-.0163
		(.0420)	(.0313)		(.0272)	(.0210)		(.0145)	(.0102)
Middle tercile	.824	.0757**	.0716**	.727	.0283	.0010	.121	.0094	-.0034
		(.0315)	(.0340)		(.0314)	(.0246)		(.0122)	(.0064)
Upper tercile	.855	.0459*	.0508*	.771	-.0231	-.0238	.2195	-.0169	-.0108
		(.0268)	(.0273)		(.0295)	(.0248)		(.0156)	(.0307)

Standard errors in parentheses; (*/**/***) significant at (10 per cent/ 5 per cent/ 1 per cent)
Source: Own calculations based on German census data, waves 2006-2011, see Table 1

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