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ORIGINAL PAPER

Parental leave benefit and differential fertility responses: evidence from a German reform

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Abstract This paper examines the causal effects of a major change in the German parental leave benefit scheme on fertility. I use the unanticipated reform of 2007 to assess how a move from a means tested to an earnings-related benefit affects higher-order births. By using data from the Mikrozensus, I find that the reform significantly affected the timing of higher-order births in the first 5 years after a last birth. Overall, mothers "just" eligible for the new benefit for the current birth initially reduce subsequent childbearing and extend birth spacing, compared to mothers "just" ineligible. However, by the end of the third year, mothers start to compensate for the earlier losses. The negative effects are largely driven by the low-income mothers, who are now worse-off and do not display any catch-up effects. The differential fertility responses along the income distribution are in line with the heterogeneous structure of the economic incentives.

Keywords Fertility · Family policy · Reform · Parental leave · Elterngeld · Germany

JEL Classification J13 · J18 · J20 · K36

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1 Introduction

Low fertility is a major political issue in many developed countries. However, although fertility rates below the replacement level of 2.1 children per woman are currently common in Europe, birth rates vary considerably across countries. For example, over the past decade, the Nordic countries such as Sweden and Norway, but also France and Ireland, experienced the highest rates of above 1.7. At the same time, Austria, Germany, and most of the southern countries displayed fertility rates between 1.3 and 1.5 (World Bank 2013).

These fertility differences draw attention to a broad range of policies that vary across countries and potentially affect childbearing.[1] Recently, various countries reviewed their family policies with more or less explicit pronatalist intentions. In OECD, between 1990 and 2009, the public spending on family (excluding education) increased from 1.5 to 2.3 % of GDP (OECD 2013).[2] So far, however, research on the impact of modern family policies on fertility is limited, partly because of serious challenges in establishing causality (e.g., Björklund 2007).

This paper extends the literature on fertility responses to economic incentives created by policy changes by providing evidence on parental leave regulations.[3] In 2007, Germany substantially modified the parental benefit scheme with the main intention to "facilitate family formation" by making parenthood more compatible with work (Deutscher Bundestag 2006). Among other specified goals, the reform aimed at shortening mothers' employment interruptions and encouraging fathers' involvement in childcare (Kluve and Tamm 2013). The reform replaced a means-tested system by a new benefit—*Elterngeld*—that substitutes pre-birth earnings and was largely inspired by the "Nordic model" (Spieß and Wrohlich 2008).

The largely unanticipated introduction in January 2007 created a natural experiment that allows for a reliable assessment of the reform in achieving its multiple goals and creating any potential side effects. Previous studies conclude that the new policy succeeded in increasing incentives for mothers to return to work faster and for fathers to get involved in child rearing (e.g., Bergemann and Riphahn (2011a), Bergemann and Riphahn (2011b), Bergemann and Riphahn (2015), and Geisler and Kreyenfeld (2012)). However, except for studies that show a significant shifting of deliveries around the day of implementation (Neugart and Ohlsson 2013; Tamm 2012), so far the evidence on the reform's effects on fertility is missing.

[1]Gauthier (2007) describes family policies as "policies directly targeted at families with children such as direct and indirect cash transfers for families with children, means-tested child welfare benefits, maternity and parental leave benefits, and childcare facilities and related subsidy programs". Aside these measures, many other policy types such as labor market, monetary and fiscal, education, and social security policies may also affect fertility.

[2]For comparison, at the same time, the relation of GDP and public spending on active labor market programs or unemployment remained constant at the level of 0.5 and 1.1 %, respectively (OECD 2013).

[3]While several studies identify fertility effects of taxation schemes (e.g., Milligan (2005), Azmat and González (2010), and Laroque and Salanié (2013)) or direct per-child cash transfers (e.g., Brewer et al. (2011), González (2013), and Cohen et al. (2013)), causal evidence from parental leave reforms is scarce (e.g., Lalive and Zweimüller 2009).

This paper contributes to previous research on the reform by investigating its impact on fertility. Although the recent introduction does not yet facilitate evaluating the effect on completed fertility, I provide first evidence for other important outcomes. Specifically, I study whether and when a mother who has just given birth will have a next child, thereby focusing on higher-order fertility and birth spacing. Generally, 70 % of German women who had a first child go on to have a second, and the progression rate from a second to a third child is 30 % (Goldstein and Kreyenfeld 2011).[4] The new policy incorporates incentives that explicitly address birth spacing, and I examine their effectiveness in the first 5 years after the reform by using data from the Mikrozensus. I acknowledge that this paper provides a partial evaluation of the reform because any fertility responses to date do not necessarily translate to effects on completed fertility. Nevertheless, pure "tempo" effects may have far-reaching consequences themselves because birth spacing seems to affect both children's and mothers' future outcomes (e.g., Pettersson-Lidbom and Skogman Thoursie (2009), Buckles and Munnich (2012), and Karimi (2014)).

I use a combination of a discontinuity design and a difference-in-differences approach that compares mothers "just" eligible for the new benefit after the current birth and mothers "just" ineligible. I find that on average those "just" eligible display significantly lower probability of having a next child within the first 3 years after birth. Consequently, the new benefit initially leads to a postponement of further births. However, except for the lowest-income mothers, the negative effect erodes afterwards and becomes insignificant in the fifth year, thereby suggesting catch-up effects. I demonstrate that the remarkable heterogeneity across income groups is in line with the structure of economic incentives created by the reform. Overall, the results suggest that while a financial loss of roughly 3000 euros significantly lowers higher-order fertility at the lower bound of income distribution, a gain of 4700 euros generates relatively weak and rather temporary effects among the remaining income groups.

The structure of this paper is as follows: Section 2 describes the German parental leave benefit reform and its fertility-related incentives. Section 3 introduces the empirical approach and Section 4 presents the data. Section 5 provides the main results and Section 6 discusses their robustness. Section 7 concludes.

2 Institutional background and economic incentives

2.1 Core institutional changes in January 2007

On January 1, 2007, the parental benefit system in Germany substantially changed, although the duration of protected parental leave remained unchanged and lasts for 3 years. The reform abolished a means-tested system—*Erziehungsgeld*—that paid a maximum of 300 euros monthly for up to 24 months or 450 euros for up to 12 months.

[4] Demographic research largely attributes the low fertility rate in Germany to a high incidence of childlessness, rather than to insufficient progression rates to higher parities (e.g., Sobotka 2011).

Kluve and Tamm (2013) report that the system covered about 66 % of parents with the 300 euros option, 10 % with the 450 euros option, and 24 % were not eligible for any payment. Given rigorous means testing, the old system targeted families at the lower tail of the income distribution. For example, couples received the maximum benefit in the 300 euros option if their annual income did not exceed 30,000 euros in the first 6 post-birth months and 16,500 euros in months 7–24. Each earlier minor child shifted the income thresholds by 3140 euros, so that the eligibility prospects increased with family size.[5]

Parents of children born on January 1, 2007 and later receive a new benefit—*Elterngeld*—that ranges from 300 to 1800 Euro per month. The exact benefit amount depends solely on average net earnings that a parent who takes up leave had in the last 12 months before childbirth. A minimum duration of pre-birth employment is not required. Generally, the new benefit replaces two-thirds of the average monthly net labor income, but if the calculated amount is lower than 300 euros, parents receive 300 euros per month. Moreover, parents with no pre-birth earnings in the relevant 12 months are also eligible for the minimum amount of 300 euros, so that the benefit is not confined to those going on leave from employment (BMFSFJ 2011).

The new system pays for a maximum of 12 months if only one parent applies, and up to 14 months if both parents apply or a single parent takes up leave. However, parents may spread the benefits over a double take-up period, e.g., 24 instead of 12 months, when they receive half of the monthly benefit. Within these time restrictions, parents can flexibly decide about the number of take-up months that they can use consecutively or simultaneously (BMFSFJ 2011).

Two specific features of the new scheme are important for parents who seek further children. First, leave-taking parents who either have one earlier child under age 3 years or at least two earlier children both below 6 years old receive a "sibling premium" of 10 % of their regular benefit (at least 75 Euro). Consequently, and in contrast to the previous means testing, the new system takes larger families into special account only if the spacing between children is relatively tight. Second, each benefit take-up period is excluded from the 12 pre-birth months that are crucial for benefit calculation after future births (BMFSFJ 2011). Section 2.2 discusses in detail how adjustments of birth intervals may affect the future benefit amounts.

Table A.1 in the Appendix[6] provides a few statistical highlights on the benefit take-up in 2010. The numbers reveal that mothers usually exhaust the full eligibility duration. A quarter of fathers take up the benefit, on average for 3 months and usually in addition to the maternal take-up. Almost 12 % of mothers spread the total benefit over a double period. On average, the monthly benefit is more generous than

[5]The age limit for earlier children was 18 years and 27 years for dependents in education. The means-testing was slightly less rigorous for single parents. Generally, the thresholds referred to annual joint family income from the calender year before the childbirth for benefits in months 1–12 and the year of the childbirth for benefits in months 13–24. In practice, often solely the father's income was relevant because the income of the leave-taking parent, i.e., usually of the mother, was omitted as long as she did not work during leave-taking. Although the income definition was not derived from tax law, it was comparable to net annual income (BMFSFJ 2005).

[6]The Appendix is available online as Electronic Supplementary Material.

the maximum of 450 euros in the old scheme. The current distribution of the benefit amount reflects the distribution of pre-birth earnings, with some exceptions. For example, although 39 % of mothers had no pre-birth earnings, less than 29 % actually receive the minimum benefit of 300 euros. The differences emerge essentially from the "sibling premium" that is added to the eligible benefit amount.

2.2 Affected groups of parents and heterogeneous incentives

Given the universal coverage of the new system and the design of the abolished means-testing, the policy change differently affected various income groups of parents.[7] Figure 1 shows the effective change in the total benefits and eligibility duration as a function of a mother's and father's monthly income. To keep the discussion tractable, I compare the new system with the prevailing 300 euros option of the old one.

Figure 1 highlights the heterogeneous structure of economic incentives along the income distribution. First, compared to the old system, the new scheme disadvantages parents with no or low joint income who would have previously received the maximum amount of 300 euros over 24 months. These lowest-income families experience an effective loss in the total benefits of up to 3600 euros (Fig. 1a, bottom left-hand corner). The change results entirely from a shorter entitlement period that declines by 12 months (Fig. 1b). The loss remains generally uncompensated by other state-provided transfers because the old benefit was laid on the top of potentially eligible social assistance (BMFSFJ 2005). Second, the new system benefits parents with high income who would have failed the means-testing before, thereby being ineligible for any payment. These high-income families experience an effective gain in the total benefits of up to 21,600 euros (Fig. 1a, the black and asterisked lines). The gain emerges because their entitlement period increases by 6 or even 12 months (Fig. 1b, the black and asterisked lines), and the new benefit depends solely on maternal earnings.

Generally, the effective changes in the overall benefits for remaining parents derives from the constellation of earnings within the family. For families who would have qualified for the reduced amount or reduced eligibility period before the reform, higher maternal earnings now increase the probability that a more generous monthly benefit overcompensates for a potential decrease in duration. Büchner et al. (2006) estimate that after the reform, 73 % of couples and roughly 42 % of single parents are better off in the first year of a baby's life.[8] However, their calculations do not consider any changes in the second year, when the pre-reform recipients now experience benefit losses, so that the actual number of "winners" is potentially lower.

In addition to the changes illustrated by Fig. 1, the new system may create specific incentives for different birth spacing among parents who consider having further

[7] See, e.g., Neugart and Ohlsson (2013) and Kluve and Tamm (2013) for a more detailed description of the core legislative changes and their effect on various socioeconomic groups.

[8] Along the distribution of household income, the proportion of "winners" increases from 42 % in the lowest quartile to 88 % in the highest quartile (Büchner et al. 2006).

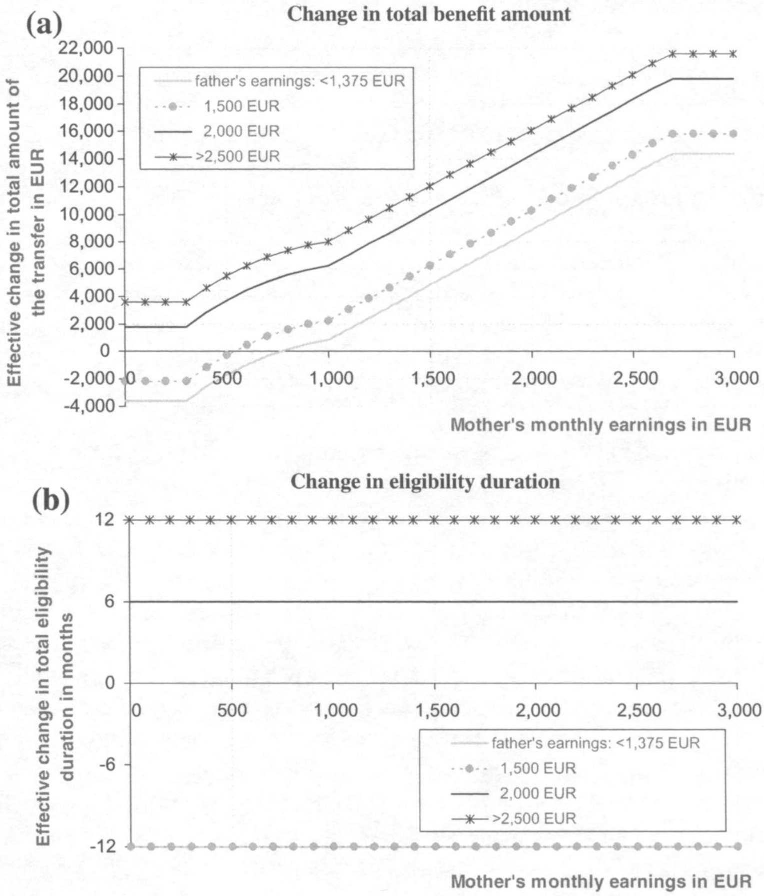

Fig. 1 Effective policy changes by mother's and father's earnings. Note: The plots compare the new benefit (*Elterngeld*) and the 300 option of the previous system (*Erziehungsgeld*) by showing the absolute differences in the total benefit amount and duration. The numbers reflect the situation of a one-child family where the mother takes up the maximum eligibility duration and is not working during the entire benefit take-up period. Source: The corresponding bills are *Bundeserziehungsgeldgesetz* (BErzGG) and *Bundeselterngeld- und Elternzeitgesetz* (BEEG), own calculations

children. In Germany, the average age difference between the first and second child is 4 years, but the prevailing pattern is to have a second child in the third year after the first birth (e.g., Pötzsch 2012). Given that about 70 % of German first-time mothers eventually give birth to a second child (Goldstein and Kreyenfeld 2011), the reform's incentives are of great importance. While the next paragraphs largely refer to the spacing between the first and second child, similar incentives apply to higher-order births.[9]

[9]Nevertheless, in my empirical analysis, I tested whether first-time and higher-order mothers respond differently. I found that there are no substantial and statistically significant birth order differences in the current child effects.

First, the "sibling premium" was intended to support families with short birth intervals (Deutscher Bundestag 2006). The restrictions imposed on age difference between siblings[10] generally imply that the eligibility is essentially independent of the birth order and only depends on a sufficiently close birth spacing. For example, first-time mothers who seek one additional child gain the full potential of the "sibling premium" while taking-up benefit for the second child only if the second child was born in the first 2 years after the first birth. The validity of "sibling premium" extends beyond month 24, but the overall financial gain declines progressively in the third year and expires in month 36 after first birth. Obviously, different parents may differently value and differently respond to a monthly premium of 10 % (at least 75 euros). For example, a "sibling premium" of 75 euros increases the regular low-end benefit of 300 euros by 25 %.

Second, the exclusion of earlier benefit take-ups from the 12 months crucial for benefit calculation may lead to a specific "speed premium"[11] if the spacing between births doesn't exceed 24 months. In contrast to the statutory "sibling premium", the occurrence of the "speed premium" is not straightforward and is confined to mothers who expect that lower (e.g., part-time) labor income between births would reduce their benefits for a next child.[12] Consequently, the "speed premium" does not apply to mothers who received the minimum benefit of 300 euros after a previous birth (i.e., those not-working and lowest-income) and to mothers who hold or even boost their own income level after work return between births.

Generally, a "speed premium" occurs because for mothers who space their further births sufficiently close, the subsequent benefits entirely or partly depend on income they had prior to their previous birth (Neyer and Andersson 2008). For example, a second childbirth in month 13 after a first birth automatically renews the eligibility for further 12 months. Thus, an immediate second birth yields a similar benefit without going back to work in-between, but such a tight spacing is biologically difficult and uncommon. Given that Germany still lacks encompassing day care system for infants,[13] most mothers partly or temporarily withdraw from the labor market between births. A 3-year work protection allows for staying at home on unpaid leave or reducing working hours.[14] However, under the new system, each month of

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[10]The "sibling premium" is granted to leave-taking parents with earlier children as long as at least one earlier child is less than 3 years old or at least two children are both less than 6 years old (BMFSFJ 2011).

[11]I borrow the term "speed premium" from previous literature on a similar feature of the Swedish system (e.g., Neyer and Andersson 2008).

[12]Figure A.1 in the online Appendix exemplifies the rather complex mechanism for a first-time mother with average pre-birth earnings of 1250 euros who desires a second child.

[13]Extensive literature discusses the underdeveloped childcare system in Germany and its adverse consequences for maternal labor force participation (e.g., Wrohlich (2008), Hanel and Riphahn (2012), and Bauernschuster et al. (2014)). The studies document a scarce availability of childcare arrangements for infants, the inflexible opening hours, and predominantly part-time manner. Since August 2013, parents have a legal claim to a subsidized daycare slot for children aged 1 year and above. However, authorities and parents still face considerable excess demand for affordable and high-quality childcare that is particularly pronounced in West German states.

[14]For example, in 2010, about 70 % of mothers whose youngest child was less than 3 years old did not work, 23 % worked part-time, and 7 % full-time (Keller and Haustein 2012).

reduced labor supply may imply lower benefits after the next birth. Thus, speeding-up a further birth may mitigate the progressive benefit losses. The "speed premium" is parallel to the "sibling premium" until the end of the second year after a previous birth and loses its validity afterwards.

Finally, the new system may also create incentives for delaying a further child beyond the second year after a previous birth. Generally, the direct link between benefit amount and pre-birth earnings may lead to a strategic scheduling of births, so that deliveries follow favorable income periods. Thus, mothers expecting increased earnings upon work return face strong incentives to postpone a further birth. Although an immediate income raise is rather unlikely, the mechanism is similar if a mother expects later or progressive raises, e.g., due to a gradual move from part- to full-time work. In contrast to "sibling premium" and "speed premium", the incentives for birth postponement remain indefinitely valid. Moreover, they may generally appeal to all mothers who wish to boost the benefits for further children, and even give incentives to those who initially did not work to enter the labor market before having a next child.

Previous research confirms differential responses to the policy change across various socioeconomic groups. For example, low-income mothers respond to the abolished work disincentives by a faster work return and increased labor supply after the benefit expiry (e.g., Bergemann and Riphahn (2011a) and Geyer et al. (2012)). In contrast, high-income mothers reduce labor supply during the take-up (e.g., Kluve and Tamm (2013) and Geyer et al. (2012)). Two studies evaluate the effects of the reform on fathers' behavior. Geisler and Kreyenfeld (2012) find an overall increase in paternal leave usage, mostly driven by highly educated men. However, Kluve and Tamm (2013) do not find that higher take-up rates translate into significant changes in fathers' labor supply or more time spent on childcare during the first year of a baby's life. So far, there is no causal evidence on whether the reform created pronatalist incentives.[15]

2.3 Mechanisms of potential fertility responses

The main presumption in the standard economic approach to fertility is that demand for children depends on a family's budget constraint. A policy change that increases income, reduces the price of the marginal child, or both should therefore raise fertility (e.g., Becker (1960) and Mincer (1962)). Because the new German parental leave regulations aim at mitigating parents' financial loss from employment interruptions (BMFSFJ 2011), the main reason why fertility should respond is that the reform affected the net-of-benefit cost of childbearing. However, a more generous policy may also reduce family size if there is a meaningful trade-off between child quantity and quality (Becker and Lewis 1973). Consequently, theoretical considerations lead to rather ambiguous predictions of fertility responses to the reform.

[15] In a descriptive study for Pomerania (a region in North-East Germany), Thyrian et al. (2010) compare aggregate monthly birth rates up to 23 months before and after January 1, 2007. They do not find any significant differences.

Previous empirical evidence on the link between parental leave schemes and fertility from cross-country and correlation studies is also inconclusive (e.g., D'Addio and D'Ercole (2005), Rønsen (2004), and Gauthier (2007)), and reliable evidence from policy interventions doesn't virtually exist. A notable exception represents research on parental leave reforms in Sweden in the 1980s (e.g., Neyer and Andersson (2008)) and Austria in the 1990s (Lalive and Zweimüller 2009). These studies conclude that mothers adjust their birth spacing in response to changes in leave duration because of strong incentives to have a sequential birth without having to return to work. In both countries, extensions of paid leave led to a tighter birth spacing and higher completed fertility. In contrast, a reduction in leave duration from 24 to 18 months in Austria increased higher-order fertility in the first 22 months after a previous birth, had a negative effect in months 23–28, and the effect disappeared thereafter (Lalive and Zweimüller 2009).

While the empirical evidence on the causal link between parental leave regulations and fertility is scarce, extensive research investigates fertility effects of other financial incentives. Several recent studies found positive effects of child-related tax deductions (e.g., Milligan (2005) for the Canadian province of Quebec, Azmat and González (2010) for Spain, and Laroque and Salanié (2013) for France). With regard to direct per-child cash transfers, Cohen et al. (2013) show positive fertility responses to subsidies for children under the age of 18 years in Israel, González (2013) to a universal benefit for newborns in Spain, and Brewer et al. (2011) to a welfare reform in the UK. My study extends this literature by providing evidence for a large country with permanently low fertility levels and an institutional framework that over recent decades promoted the traditional "male breadwinner" family type (e.g., Hanel and Riphahn 2012). The new German parental leave benefit implies a substantial move towards a "dual-earner" oriented family policy (Spieß and Wrohlich 2008).

Given the complex incentive structure of the German policy change (see Section 2.2), predicted fertility responses differ across socioeconomic groups and over time. For example, for several reasons, we may expect (at least temporary) declines in higher-order fertility among the low-income mothers who would be eligible for the old means-tested benefit after a current birth. First, they now experience overall benefit losses compared to the old system. Second, they now have to give a further birth within 12 (at the latest 14) months to benefit from an automatic renewal, and such a tight spacing of births may be biologically difficult. Moreover, mothers on the lower bound of the benefit amount cannot expect any "speed premium" from having a next child within the first 24 months after current birth. Instead, in the second year of a baby's life, poorer households face strong incentives to speed up a mother's return or entry to the labor market (Bergemann and Riphahn 2011a).[16] Upon work return, the direct link between earnings and future benefits might encourage mothers

[16]Compared to the old system, several mechanisms may drive a faster (re-)entry among the poorer mothers. For example, parents might want to compensate for the sudden benefit drop after the shorter 1-year eligibility. Parents who applied for the optional spreading over 2 years might want to compensate for the less generous benefit because the monthly amount halves. Finally, the reform abolished a work disincentive in the second year, as the old system deducted any labor earnings from the benefit amount (Bergemann and Riphahn 2011a).

to postpone further births by at least 12 months, i.e., beyond the second year after a current birth. However, until the end of the third year, the "sibling premium" might create the contradictory incentive to speed up future births.

As for mothers who are better off after the reform, several incentives might lead to (at least temporary) positive effects on their future childbearing. These mothers generally experience an income effect because they are newly eligible for a benefit over 12 (at most 14) months after a current birth. This generates incentives for working mothers to postpone employment beyond the first year of a newborn's life (Kluve and Tamm 2013). While an automatic renewal due to an immediate next birth is rather unlikely, the option of doubling the take-up period may extend the "economically optimal" interval between births to 28 months. In addition, higher-income mothers potentially benefit from the concurrence of "speed premium" and "sibling premium" if a further birth occurs in the second year. The theoretical expectations for the later periods are unclear for all groups of parents. On the one hand, the "sibling premium" could potentially create incentives for a further birth in the third year, although the gain progressively declines towards zero between months 24 and 36. On the other hand, given that obtaining childcare becomes easier when the newborn gets older, German mothers tend to increase their labor supply with increasing child's age (Hanel and Riphahn 2012). Prospects for increasing labor income may lead to a further birth postponement, so that delivery follows a favorable 12-month income period. Generally, it remains an empirical question which effects predominate.

3 Estimation strategy

The policy change created a natural experiment that allows for a credible assessment of its effects on specific fertility choices. This paper focuses on mothers who have just given birth and studies their higher-order fertility in the following 57 months. To identify causal effects, I compare outcomes of mothers who gave birth shortly before and shortly after the reform's introduction. To eliminate potential seasonal effects, I additionally use mothers who gave birth in previous years as a control group. This strategy combines a discontinuity design with a difference-in-difference approach[17] and estimates a linear model of the form:

$$\mathrm{y}_i = \phi\ \mathrm{reform}_i + \mathrm{year}_i'\gamma + \mathrm{season}_i'\delta + \mathrm{x}_i'\beta + \nu_i \qquad (1)$$

where y_i denotes a future fertility outcome of a woman i. The indicator variable reform_i equals one if a woman gave birth shortly after the reform (i.e., in the first quarter of 2007). The vector year_i includes a set of indicators that are equal to one if this previous birth occurred during a particular turn of the year. I define a turn of year as running from October through the next March and include in the main estimation sample women who gave birth between October and March in the years 2001/2002 through 2006/2007. Consequently, vector year_i comprises five indicators

[17]Dustmann and Schönberg (2012) use a similar strategy to evaluate expansions in maternity leave duration on children long-term outcomes.

for the years 2002/2003 through 2006/2007; the reference is 2001/2002. Seasonal-fixed effects are expressed by season_i, which in the main specification corresponds to an indicator for birth quarter of previous child and in more flexible specifications to indicators for birth month. In the main model, the reform indicator is therefore analogous to an interaction term between the indicators for first quarter of the year and the reform year 2006/2007.

Additionally, x_i captures maternal socio-demographic characteristics at the previous childbirth such as her year of birth, education, employment, and migration status. For sensitivity checks, I also control for similar covariates for the father and several characteristics of the previous child such as indicators for multiple birth, gender, and birth order. In all regressions, x_i includes regional indicators for federal state of residence and aggregate state-level variables such as the unemployment rate, public childcare coverage, and average gross earnings.[18] The terms ϕ, γ, δ, and β represent coefficients to be estimated, and ν_i is a random error term.

The key assumption to identify the coefficient of interest ϕ is that parents could not have influenced the date of a previous childbirth in response to the reform. A major validity threat is that parents would have known about it at the time of conception. However, Kluve and Tamm (2013) provide convincing evidence that the reform was largely unanticipated. The public discussion started in May 2006 when the governing parties agreed on the cornerstones of the new benefit, and Parliament passed it in September 2006. Until then, it was not clear whether the reform would eventually take place. This time line and the simple fact that parents cannot perfectly plan the conception of a child suggest that births in the first quarter of 2007 were still independent of the reform.

The identification strategy would also fail if mothers could have timed births by bringing the exact birth date forward or backward. Indeed, recent studies by Neugart and Ohlsson (2013) and Tamm (2012) show that some women postponed delivery to the New Year to become eligible for the new benefit. However, because less than 8 % of mothers with due dates in the last week of December successfully postponed births, the shifting should not largely affect my results.[19] Nevertheless, in a sensitivity test, I exclude births around the implementation day.

Another important assumption is that the potential seasonal patterns are common for the reform year 2006/2007 and for the control ones. Because this assumption is generally not testable, a deliberate selection of control years is important. On the

[18]The monthly state-level unemployment rates are from the Federal Employment Agency and the annual state-level childcare and earnings data from the Federal Statistical Office. I calculate public childcare coverage ratio for children less than 3 years old as the number of slots over the respective population of children. The aggregate earnings indicator corresponds to the average annual gross earnings of an employee measured in 1000 euros.

[19]More precisely, Tamm (2012) estimates that 7.8 % and Neugart and Ohlsson (2013) that 5.4 % of births scheduled for the last week of December 2006 were shifted to the first week of January 2007. While Neugart and Ohlsson (2013) emphasize the biological impossibility to postpone birth by more than a few days, Tamm (2012) argues that some deliveries could have been moved by more than 1 week. There is no evidence for shifts in the opposite direction, i.e., speeding-up birth (e.g., by inducement or elective cesarean). Not surprisingly, birth postponement occurred only among mothers who were more likely to gain from the reform.

one hand, including many cohorts of mothers enlarges the estimation sample and might imply efficiency gains. On the other hand, a small number of control years lowers the risk that the underlying seasonal effects changed over time or that other policy changes may contaminate the control groups. The directly preceding cohorts are natural candidates for this role, and I include four of them. However, the results are robust to alternative choices, e.g., to inclusion of one post-reform year.

Provided that the central assumptions hold, the coefficient ϕ represents what Lalive and Zweimüller (2009) term *current* child effect on higher-order fertility. Paraphrasing their argument, the German reform may affect fertility because it changes the cost of the child that is already born (current child), any child not yet born (future child), or both. Empirically, the current child effect can be isolated by comparing future outcomes of mothers who differ in benefit systems for the current child and would experience identical systems for a future child. In contrast, the future child effect may be estimated by comparing mothers with identical conditions for the current child and different conditions for a future child (Lalive and Zweimüller 2009).

For Austria, Lalive and Zweimüller (2009) find that both the *current* and *future* child effects are quantitatively important and of similar magnitude. They argue that the two effects add up to a *total* impact, i.e., an overall fertility effect generated by a policy change. While Lalive and Zweimüller (2009) quantify both mechanisms by using a regression discontinuity framework,[20] my empirical approach allows for a causal interpretation of the *current* child effect. However, under the assumption that there were no substantial year-specific effects (other than the aggregate variables in x_i), the year-specific coefficients γ should reflect the *future* child effect.[21] Given that both the *current* and *future* child effects work in the same direction, my estimates of the *current* child effect yield a lower bound of the *total* effect that is of main policy interest.

4 Data

I use data from the German Mikrozensus, which annually provides a 1 % sample of the population. The key advantages of the Mikrozensus are the availability of

[20]Specifically, they identify the *current* child effect by comparing mothers who gave birth 1 month before and 1 month after a reform. Obviously, such approach assumes the absence of any month-specific effects. In contrast, my empirical design captures any seasonal effects in δ, though by using comparisons around a cut-off date, I identify the *current* child effect essentially in a similar way. To estimate the *future* child effect, Lalive and Zweimüller (2009) compare mothers who gave birth one month before a policy change and in exactly the same month but three years earlier. The validity of causal inferences rests here on the strong assumption that there are no cohort or year-specific influences that might otherwise explain changes in fertility over a 3-year period.

[21]The indicator for the year 2002/3 may capture the *future* child effect already in my estimation of probability of having a next child within the first 57 months because women who gave birth in October 2002 might have known about the reform in the middle of 2006. If they immediately changed their higher-order fertility plans, these changes would occur by the end of the first quarter of 2007, i.e., around month 54 after a previous birth. Although such exact timing of births is difficult, the probability of potential anticipation effects increases thereafter.

information on an individual's month of birth[22] and relatively large sample sizes. However, my research design explores a rather small subset of the data because I restrict my sample to mothers of children born in Germany from October through March in the years 2001/2002 through 2006/2007. I include only mothers who were 15 to 45 years old when giving birth. Given that West and East Germany differ in many aspects related to childbearing, I focus on mothers who reside in West Germany to obtain a homogenous sample.[23] They represent over 80 % of the respective population. These sample selection criteria yield around 350 births per month in a single survey year, and I pool four Mikrozensus waves from 2009 through 2012 to increase statistical power.

Although the data set is rich in many aspects, except for the 2012 survey year, it does not directly record births. Instead, I can identify an individual's parents if they live in the same household at the time of the interview, so that I observe only children who live in a mother's household and cannot distinguish between her biological and step children. For consistency, I use this definition of a parent-child relationship while coding all included survey years. However, by exploring the Mikrozensus 2012, I found that I most likely observe a complete history of actual births for the vast majority of sampled mothers.[24]

The set of outcome variables determines whether and when a mother gives a next birth. Given the sampling frame of the data, I can fully observe births taking place up to December preceding a particular survey year.[25] For example, the Mikrozensus 2009 fully reports births that occurred not later than in December 2008. Consequently, for the latest cohort of included mothers who gave birth in March 2007, I observe their further births only if they took place not later than in the 21st month after a previous birth. Similarly, in Mikrozensus 2010 and 2011, I can track those mothers until December 2009 and 2010, respectively, i.e., over 33 and 45 consecutive months. Because the latest available Mikrozensus year is 2012, I restrict the time of analysis to 57 months. To shed light on the entire period, I construct a set of 46 indicators that, at a monthly frequency, measure the cumulative probability that a next birth occurs between the 12th and 57th month after a previous birth. I also study the spacing between the last and next child measured in months. The spacing variable is censored after month 57, i.e., it takes the value of 58 for mothers without any further birth within the first 57 months after a previous one.

The key explanatory variable is an indicator for whether the previous birth was after the reform, i.e., between January and March 2007. The main set of conditioning

[22]This information is not available in the scientific use files, thus I use a controlled remote access to the data.

[23]Previous literature emphasizes substantial differences in fertility dynamics, attitudes towards maternal employment, women's labor market attachment, and subsidized childcare infrastructure (e.g., Goldstein and Kreyenfeld (2011), Hanel and Riphahn (2012), and Wrohlich (2008))

[24]The Mikrozensus 2012 reports the actual number of births, i.e., biological children. This number and the number of children living in a mother's household are identical for 96 percent of sampled mothers from the wave 2012. Therefore, a potential measurement error is virtually negligible.

[25]The distribution of interviews is random over the entire year. Therefore, if a mother's interview takes place early in year (e.g., in January), a child born later in the same year is not yet observed in the data.

variables comprises five indicators for the cohorts of mothers (2001/2002 is the reference) and an indicator for whether their last birth occurred from January to March versus the previous October to December.

A significant shortcoming is that the Mikrozensus contains little retrospective information on respondents, but there are some exceptions. For example, I can reconstruct a mother's education as of previous childbirth by using the information on graduation year from the highest degree. From the International Standard Classification of Education (ISCED-1997), I derive three educational groups: low (ISCED 1-2), middle (ISCED 3-4), and high (ISCED 5-6). I also reconstruct a mother's pre-birth employment status by using the information on the start date (year and month) of her current employment for those employed and the termination date for those not employed. I also identify the father and proceed similarly with his pre-birth education and employment.[26] Given the lack of retrospective income information, I cannot precisely identify parents who are worse and better off after the reform. Nevertheless, in an attempt to do so, I deliberately imputed a mother's and a father's pre-birth earnings by using other variables from the Mikrozensus and by drawing on complementary data from the German Socio-Economic Panel (SOEP). The Data Appendix and Table A.5 (available online) provide details on the imputed income measures. I use these variables exclusively for the heterogeneity analysis in Section 5.3.

The final regression sample depends on the period of analysis because the four survey years allow to track the future fertility outcomes of included cohorts of mothers over different times after a previous birth. For example, I may track future fertility of the reform cohort 2006/2007 over 21 months in all survey years, but from month 22 onward, I do not longer observe their outcomes in Mikrozensus 2009. By the same logic, I do not observe outcomes of these mothers in Mikrozensus 2010 starting from month 34 onwards and in Mikrozensus 2011 starting from month 46 onwards. Table A.2 in the online Appendix illustrates the construction and sizes of the samples used in the estimations. The total sample size decreases in four steps over the period of analysis from 52,423 observations for month 12 through 21 to 39,826 observations for month 46 through 57. Section 6 discusses alternative approaches to design an estimation sample.

The sample sizes in Table A.2 represent the number of observations rather than mothers. Although I treat a multiple birth as a single one, some of the sampled women repeatedly gave births between 2001/2002 and 2006/2007 and therefore occur several times in the main analysis.[27] I keep all observations to preserve the representativeness of the sample and its size, and I cluster the standard errors at the individual level

[26] I was unable to link about 15 % of children to their fathers because they don't live in the child's household at the time of interview. I always use a dummy for missing father in regressions that include paternal characteristics.

[27] Specifically, the largest sample used in the analysis for months 12–21 consists of 47,211 mothers who give 52,423 observations. Consequently, I lose 10 % of sample size if I keep only one observation per women. This proportion reduces to 7 % in the smallest sample for months 46–57.

throughout. Nevertheless, Section 6 provides a sensitivity test that drops the duplicate observations.

5 Results

5.1 Descriptive analysis

Figure 2 exemplifies the outcome measures by showing the mean probability of having a higher-order birth within the first 24 and 36 months after a previous childbirth. The horizontal axis shows when the sampled mothers gave their previous birth. The vertical axis depicts the percentage of mothers having a next child within the following 24 and 36 months, respectively.

The data is aggregated quarterly, so that each set of connected dots compares the future fertility outcomes of mothers who gave birth in the quarter before and after a particular turn of the year. For example, the first dot in the bottom left-hand corner indicates that among mothers of children born in the last quarter of 2001, on average, 10.2 % went on for a next child within the first 24 months. The connected right-hand

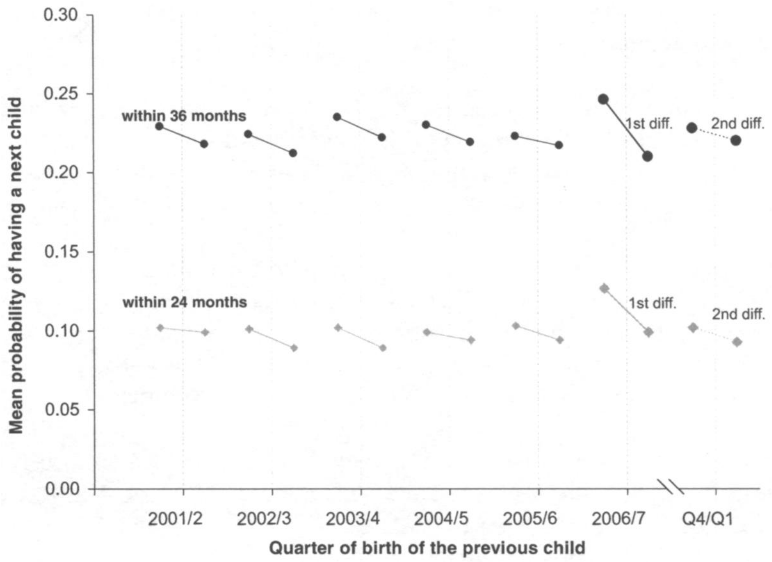

Fig. 2 Mean probabilities of having a next child within 24 and 36 months by quarter of birth of previous child. Note: The plot shows unweighted raw data. Each set of connected dots compares the sample means for mothers of children born one quarter before and after a particular turn of the year. Consequently, the 1st difference comparison is between the means corresponding to the 4th quarter of 2006 and the 1st quarter of 2007. The 2nd difference comparison is between the means for the 4th and 1st quarters aggregated over the pre-reform years 2001/2002–2005/2006. Source: Mikrozensus, pooled survey years 2009–2012, own calculations. Sample restricted to West German mothers of children born in Germany up to 3 months before/after the turn of the years 2001/2002–2006/2007

dot shows that the percentage is 9.9 % among mothers who had their previous birth in the first quarter of 2002.

Figure 2 highlights clear seasonal patterns. The probabilities of having a next child within the first 24 and 36 months are almost always visibly lower for the first-quarter mothers, compared to the fourth-quarter mothers.[28] Thus, a simple comparison of outcomes between mothers who gave previous births shortly before and after the reform (cohort 2006/2007) would be biased due to seasonal effects. To isolate causal effects of the reform, I use the pre-reform cohorts as control groups. The last two sets of connected dots visualize the core of my identification strategy. The first difference comparison is between the means corresponding to the last quarter of 2006 and the first quarter of 2007. The second difference comparison is between the means for the fourth and first quarters aggregated over the years 2001/2002–2005/2006.

Table A.3 in the online Appendix displays detailed comparisons of means for the key dependent and explanatory variables. The last column of Panel A provides first evidence on the difference-in-differences effect of the reform on the probability of having a next child; it is negative throughout but not always statistically significant. The increase in the average spacing between children of less than 0.9 months is also insignificant. Obviously, this preliminary evidence is insufficient for causal inference because other characteristics important for fertility decisions may also vary over time. To remove the influence of these confounding factors, I next estimate Eq. 1.[29]

5.2 Regression analysis

Table 1 reports the key regression results. The outcome measures in columns 1 to 8 are the cumulative probabilities of having a next child within the first 12, 21, 24, 33, 36, 45, 48, and 57 months after a previous birth, respectively.[30] The dependent variable in columns 9 and 10 is the spacing between the last and the next birth in months, which is censored at month 45 or 57, respectively. Each column reports results from a separate linear regression by showing the estimated coefficients and corresponding robust standard errors for selected variables.

The coefficient of the reform indicator for month 12 is insignificant and very close to zero. Given biological difficulties of conceiving soon after a previous birth, one should not expect any major reform effects here. The point estimates for months 21 through 33 increase in magnitude and suggest that the reform significantly reduced the probability of having a next child, e.g., within the first 33 months by 3.5 percentage points. These are all quantitatively large effects compared to the average incidence before the reform. For example, less than 20 % of sampled mothers gave a further birth within the first 33 months.

[28] I find similar seasonal patterns for the remaining outcome measures and for the post-reform years.

[29] For illustration, Panel B describes the explanatory variables in my largest sample for months 12–21. The statistics generally suggest that the treated and control groups are comparable with respect to observable characteristics.

[30] Table 1 reports regression results for selected outcome measures because I am not able to present here estimates for all 46 indicators that I use as dependent variables to investigate the entire period from month 12 through 57. The selection of months is related to the design of my sample, which I describe in Section 4.

Table 1 Effect of the reform on higher-order fertility: baseline results

Outcome	(1) Probability of having a next child within ... months after a previous birth	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9) Spacing in months: censored	(10)
measure:	12	21	24	33	36	45	48	57	at month 45	at month 57
Reform	− 0.003	− 0.012*	− 0.017*	− 0.035***	− 0.029**	− 0.029*	− 0.020	− 0.026	0.698**	0.963
	(0.002)	(0.007)	(0.009)	(0.011)	(0.014)	(0.015)	(0.021)	(0.021)	(0.312)	(0.645)
Month of last birth:										
January-March	0.001	− 0.005	− 0.014***	− 0.009	− 0.009	− 0.002	− 0.003	− 0.007	0.162	0.224
	(0.001)	(0.004)	(0.004)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.135)	(0.220)
October-December	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Year of last birth:										
2006/7	− 0.002	− 0.009	− 0.010	− 0.025	− 0.027	− 0.044**	− 0.071***	− 0.064**	0.330	1.267
	(0.004)	(0.011)	(0.014)	(0.018)	(0.021)	(0.022)	(0.027)	(0.028)	(0.460)	(0.840)
2005/6	0.000	− 0.005	− 0.016	− 0.030*	− 0.025	− 0.037*	− 0.048**	− 0.041*	0.553	1.222*
	(0.003)	(0.010)	(0.012)	(0.016)	(0.018)	(0.019)	(0.022)	(0.022)	(0.388)	(0.668)
2004/5	0.000	− 0.003	− 0.011	− 0.022	− 0.018	− 0.012	− 0.024	− 0.024	0.335	0.777
	(0.003)	(0.009)	(0.011)	(0.014)	(0.016)	(0.017)	(0.020)	(0.020)	(0.348)	(0.597)
2003/4	0.001	0.001	− 0.005	− 0.006	− 0.003	0.001	− 0.009	− 0.007	0.025	0.219
	(0.002)	(0.007)	(0.009)	(0.011)	(0.012)	(0.013)	(0.015)	(0.015)	(0.273)	(0.460)
2002/3	− 0.002	− 0.001	− 0.007	− 0.015*	− 0.010	0.003	− 0.001	− 0.005	0.121	0.155
	(0.002)	(0.005)	(0.006)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.189)	(0.308)
2001/2	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Socio-demographic characteristics at previous childbirth										
Maternal	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	52,423	52,423	50,384	50,384	46,224	46,224	39,826	39,826	46,224	39,826

Source: Mikrozensus, pooled survey years 2009–2012, own calculations. Sample restricted to West German mothers of children born in Germany up to three months before/after the turn of the years 2001/2002-2006/2007

Notes: Each column represents a separate linear regression. All regressions include a constant. Maternal socio-demographic characteristics comprise indicators for year of birth, education, employment, migration status, year of interview, state of residence, and regional unemployment rates, average gross earnings, and childcare coverage. Robust standard errors in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 % level

Interestingly, the reform effect erodes thereafter, because the point estimate for month 36 is smaller in magnitude and becomes insignificant after month 45. The negative effects for the earlier months translate into a slightly larger spacing between births. The point estimate in column 9 is significant at the 5 % level and suggests that the reform led mothers to postpone their next birth on average by 0.7 months within the first 4 years. However, the corresponding estimate in column 10 shows that the reform did not significantly affect the average birth spacing in the entire 57-month period of analysis.[31]

The estimated coefficients on the indicator that a previous birth occurred between January and March capture common seasonality effects and are in line with the graphical inspection in Fig. 2. Some of the point estimates for the year indicators in columns 1 through 8 show significant differences relative to the reference year 2001/2002. While the vast majority of estimates for the year 2003/2004, solely capture year-specific shocks, all shaded coefficients for year-specific indicators may also reflect what Lalive and Zweimüller (2009) term the *future* child effect.

For interpretation, we need to assume that in the second half of 2006 women could have known about the reform and anticipated that they receive the new benefit in case of a next childbirth. If this knowledge caused an immediate change of their further fertility plans, the *future* child effect should occur already 9 months later, i.e., from the second quarter of 2007 onward. Therefore, for mothers with a previous birth in 2002/2003, the *future* child effect may be apparent in the estimates for month 57, for the 2003/2004 mothers in the estimates for months 45 through 57, and so on. By the same logic, the 2006/2007 cohort knew about the reform already at childbirth, so the *future* child effect, if any, should be apparent from month 12 through 57. Looking at the shaded coefficients from a column's bottom to its top, respectively, we observe that although not always significant, the magnitude of the point estimates usually increases across years. Such patterns suggest that they might indeed capture the *future* child effect.

However, to draw causal conclusions about the *future* child effect, we need to assume that there are no year-specific influences that might otherwise explain changes in fertility over time. To make this assumption more plausible, all regressions condition on time-variant regional unemployment rates, average earnings, and childcare provision ratios. Nevertheless, their ability to capture any potential year-specific effects that impact fertility is limited, so that I am reluctant to interpret the shaded year-specific estimates causally. Nevertheless, their signs provide observational evidence that the mechanism of *future* child effect works in the same direction as the *current* child effect identified by the reform indicator. Consequently, my causal estimates of the *current* child effect yield a lower bound of the *total* effect that is of prime policy interest.

To shed more light on the entire period between months 12 and 57, Fig. 3 traces the current child effect at a monthly frequency. I plot the coefficients on the reform

[31] Because of censoring, the OLS regressions might underestimate the reform effects on birth spacing in columns 9 and 10. I re-run these estimations by using Tobit models, which indeed yielded slightly larger marginal effects.

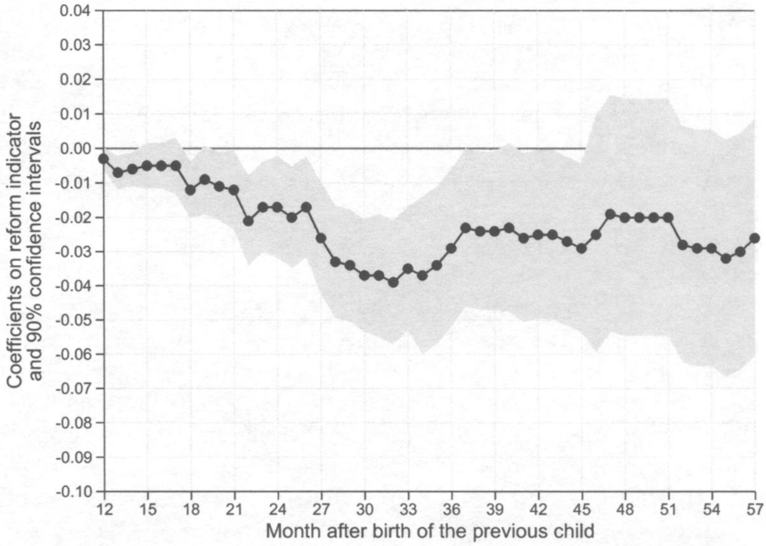

Fig. 3 Effect of the reform on cumulative probability of having a next child within the first 12 through 57 months after a previous birth. Note: Each dot shows a coefficient on reform indicator obtained from a separate linear regression. *Grey areas* represent 90 % confidence intervals around the point estimate. All regressions include a constant, indicators for turns of the years, quarter of birth, and maternal sociodemographic characteristics at previous childbirth such as indicators for year of birth, education, employment, migration status, year of interview, state of residence, and regional unemployment rates, average gross earnings, and childcare coverage. Source: Mikrozensus, pooled survey years 2009–2012, own calculations. Sample restricted to mothers of children born in Germany up to 3 months before/after the turn of the year for years 2001/2002–2006/2007

indicator and 90 percent confidence intervals around these point estimates obtained from 46 separate regressions. The horizontal axis shows the number of months that have passed since a previous birth. The estimates for months 12, 21, 24, 33, 36, 45, 48, and 57 match with those in Table 1.

Figure 3 confirms that the reform had a negligible effect on conceiving a next child almost immediately after a previous birth. However, some of the negative point estimates up to month 21 are significant and increase over time. The most likely explanation for this pattern is that the reform encourages mothers to return to work after the benefit expires (e.g., Bergemann and Riphahn 2015). The lack of a sharp drop after month 12 may illustrate that there is no universal expiration date because parents may claim two additional "daddy months" or double the eligibility duration. The effect is largest around month 32, a few months after the eventual benefit expiry at month 28. The cumulative probability of giving a next birth within the first 32 months drops by 3.9 percentage points. Although afterwards mothers start to compensate for the initial losses, and after month 45, the negative effects are statistically indistinguishable from zero, the point estimates seem to have stalled at a level of around -2.5 percentage point.

To some extent, the results are consistent with the previous evidence for Austria where a reduction of the paid parental leave duration from 24 to 18 months led to temporary effects on timing of higher-order births (Lalive and Zweimüller 2009). However, while Austrian mothers accelerated their childbearing during the benefit receipt, reduced it thereafter, and did not revise their family plans in the long run, fertility responses of German mothers go in the opposite direction. These different fertility responses in both countries may reflect general differences in maternal labor supply and institutional conditions (Dearing et al. 2007).

5.3 Heterogeneity in responses

Because the reform differently affected families across the income distribution, I next assess the heterogeneity in responses across groups with distinctive earnings potential. Table 2 reports the results. Each panel shows estimates for the reform indicator interacted with a particular variable of interest, which splits the sampled mothers into exclusive subgroups.[32] Each column of coefficients and standard errors within a panel is obtained from a separate linear regression.[33]

Panel A first focuses on mother's employment status at a previous childbirth. Official statistics on the benefit take-up after the reform report that mothers with any pre-birth employment receive on average more than twice as much compared to mothers with no pre-birth employment. For example, in 2010, the average monthly benefits for the two groups were 878 and 330 euros, respectively (BMFSFJ 2011). Thus, the proportion of mothers who gained from the reform is potentially higher in the working group because they are now eligible for relatively generous benefits, which exceed any potential payments under the old regime. The first row of panel A evaluates the effects for previously non-working mothers and largely underpins the baseline results in Table 1. However, the coefficients increase in magnitude and the negative effect still persists in month 57. The increase in birth spacing is also more pronounced. The second row of estimates implies that previously employed mothers differ from those not employed in their responses to the reform by the end of the second year. The positive signs of the significant coefficients might suggest that the "speed premium", which does not apply to previously non-working mothers, and/or its coincidence with the "sibling premium" is at work. However, to derive the absolute reform effects for this group, we need to sum up the two reported point estimates in each column of panel A. To facilitate interpretation, I plot these effects in Fig. 4.

Figure 4 shows that in the first 2 years, higher-order fertility of previously working mothers remains mostly unaffected by the reform. In the third year, the negative effects accumulate over time until month 32 and then progressively fade away. The

[32]Save for Panel C, the splitting variables include a separate category for a missing value. Although interacted with the reform indicator and included throughout, the results for these categories are not reported due to serious limitations with their interpretation. Obviously, the missing values could be problematic if their incidence was related to the reform. However, I could exclude such possibility for all regressions reported in Table 2 by regressing the indicators for missing values on the reform indicator (within a framework similar to Eq. 1).

[33]These regressions control for the same set of variables as in Table 1. Given that the reform indicator corresponds to an interaction term between the indicators for first quarter of year and the reform year 2006/2007, each regression in Table 2 additionally includes their interaction terms with the variable defining subgroups.

Table 2 Effect of the reform on higher-order fertility: heterogeneities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome	Probability of having a next child within ... months after a previous birth								Spacing in months: censored	
measure:	12	21	24	33	36	45	48	57	at month 45	at month 57
Panel A: Mother's employment										
Reform (Ref. is non-working)	−0.004	−0.032***	−0.036***	−0.045***	−0.049***	−0.045**	−0.056**	−0.067**	1.218***	2.297**
	(0.003)	(0.009)	(0.012)	(0.015)	(0.019)	(0.020)	(0.028)	(0.029)	(0.439)	(0.899)
Reform*working	0.001	0.036***	0.034**	0.017	0.036	0.029	0.069*	0.080*	−0.909	−2.545**
	(0.004)	(0.013)	(0.017)	(0.021)	(0.027)	(0.029)	(0.041)	(0.042)	(0.601)	(1.262)
Panel B: Eligibility for old means-tested benefits based on a father's imputed net monthly income										
Reform (Ref. is full benefits)	−0.009**	−0.036***	−0.045***	−0.06***	−0.055***	−0.043*	−0.027	−0.008	1.373***	1.462
	(0.004)	(0.011)	(0.014)	(0.017)	(0.021)	(0.022)	(0.031)	(0.031)	(0.473)	(0.965)
Reform*reduced benefits	0.007	0.014	0.012	−0.001	0.008	−0.004	−0.014	−0.049	−0.154	0.650
	(0.005)	(0.015)	(0.02)	(0.026)	(0.033)	(0.035)	(0.051)	(0.051)	(0.719)	(1.555)
Reform*no benefits	0.015***	0.076***	0.122***	0.103***	0.107**	0.151***	0.082	0.002	−3.165***	−3.105
	(0.005)	(0.020)	(0.029)	(0.035)	(0.045)	(0.047)	(0.065)	(0.067)	(0.985)	(2.010)
Panel C: Mother's imputed net monthly earnings (in euro)										
Reform (Ref. is 400 and less)	−0.006*	−0.038***	−0.041***	−0.047***	−0.049***	−0.044**	−0.054**	−0.060**	1.256***	2.167**
	(0.003)	(0.009)	(0.012)	(0.015)	(0.018)	(0.020)	(0.027)	(0.028)	(0.424)	(0.870)
Reform*400−800	0.010**	0.036*	0.035	0.016	0.044	0.073	0.137**	0.166**	−1.768*	−4.508**
	(0.005)	(0.019)	(0.026)	(0.034)	(0.042)	(0.045)	(0.064)	(0.068)	(0.914)	(1.911)
Reform*>800	0.004	0.056***	0.051***	0.024	0.038	0.016	0.048	0.035	−0.873	−1.805
	(0.004)	(0.014)	(0.019)	(0.023)	(0.030)	(0.032)	(0.044)	(0.045)	(0.655)	(1.378)

Table 2 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome	Probability of having a next child within ... months after a previous birth								Spacing in months: censored	
measure:	12	21	24	33	36	45	48	57	at month 45	at month 57
Panel D: Status after the reform due to change in the eligible benefit amount										
Reform (Ref. is "loser")	−0.009**	−0.058***	−0.056***	−0.059***	−0.059***	−0.051**	−0.064*	−0.050	1.609***	2.535**
	(0.004)	(0.012)	(0.015)	(0.018)	(0.023)	(0.024)	(0.034)	(0.035)	(0.521)	(1.076)
Reform*"winner"	0.009**	0.068***	0.061***	0.035	0.045	0.042	0.075*	0.046	−1.366**	−2.408*
	(0.005)	(0.014)	(0.019)	(0.023)	(0.029)	(0.031)	(0.044)	(0.045)	(0.654)	(1.372)
Observations	52,423	52,423	50,384	50,384	46,224	46,224	39,826	39,826	46,224	39,826

Source: Mikrozensus, pooled survey years 2009–2012, own calculations. Sample restricted to West German mothers of children born in Germany up to 3 months before/after the turn of the years 2001/2002–2006/2007

Notes: Each panel shows coefficients and standard errors for the reform indicator interacted with a grouping variable that splits the sample into exclusive subgroups. "Ref." labels the reference category. Within a panel, each column is obtained from a separate linear regression. All regressions include a constant, indicators for turns of the years, first quarter of birth, maternal sociodemographic characteristics (see notes below Table 1), and interaction terms between the variable defining subgroups and the indicators for first quarter of year and the reform year 2006/2007. Save for panel C, the missing category of the grouping variable and its respective interaction terms are also included. Robust standard errors in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 % level

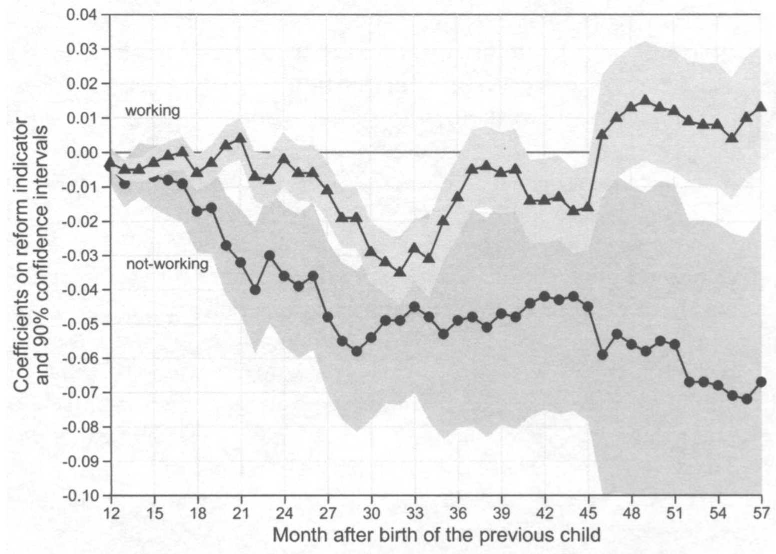

Fig. 4 Effect of the reform on cumulative probability of having a next child by a mother's employment status at previous childbirth. Note: A set of dot and triangle for each month is obtained from a separate linear regression that interacts reform indicator with a mother's pre-birth employment status. The *dots* shows coefficients on reform indicator for not-working mothers, which are the reference category. The *triangles* represent the reform effects for working mothers, which is the sum of coefficients for the reference category and the interaction term. *Grey areas* represent 90 % confidence intervals around the point estimates. All regressions include a constant, indicators for turns of the years, quarter of birth, its interaction with maternal employment status, and an interaction term of employment status with the indicator for reform year 2006/2007. In addition, all regressions condition on maternal sociodemographic characteristics at previous childbirth such as indicators for year of birth, education, employment, migration status, year of interview, state of residence, and regional unemployment rates, average gross earnings, and childcare coverage. Source: Mikrozensus, pooled survey years 2009–2012, own calculations. Sample restricted to mothers of children born in Germany up to 3 months before/after the turn of the year for years 2001/2002–2006/2007

U-shaped pattern implies that working mothers postpone their further births beyond the second year and fully catch up for the initial losses by the end of the fourth year. Such pure timing effects are in line with the new incentives to return to the labor market for at least 1 year prior to having a next child. In contrast, previously non-working mothers who are now potentially worse-off do not catch up. Although the fertility responses are statistically indistinguishable across the groups over several months, the effects created by non-working mothers are visually more pronounced throughout, remain relatively stable over time, and are still detectable by the end of the fifth year.

Panel B evaluates the effects by the potential eligibility for the old means-tested benefits, which usually depended on the father's income. Here, I use the imputed paternal income that allows me to distinguish between families who would have been previously eligible for the full benefit of 300 euros over 2 years, those eligible for reduced benefits (in terms of amount and/or duration), and those not eligible at all. The estimates in panel B demonstrate that the previously eligible mothers drive the negative effects in Table 1. I do not find any significant differences between families eligible for the full and reduced benefits. In contrast, families with the highest father's income, which are now newly eligible for benefits, exhibit increased probability of having a next child throughout, though the estimates after month 45 are insignificant.

Panel C reports the effects by a mother's imputed earnings. For two reasons, I distinguish between monthly earnings of 400 euros and less, those between 400 and 800 euros, and those of more than 800 euros. First, mothers who earn around 800 euros (and more) are better-off after the reform regardless of whether or not they would have been eligible for the old benefit. Second, the statutory income thresholds for subsidized employment potentially stack maternal earnings at 400 and 800 euros.[34] Panel C confirms that the reform substantially reduces the probability of having a next child among the lowest-income mothers, and extends their birth spacing. The middle-income group clearly follows a U-shaped pattern, with no effects in the first 2 years, negative effects in the third year, and subsequent catch-up; which even overcompensates the earlier losses and translates into a significantly tighter birth spacing.[35] In contrast, the highest-income mothers yield initially positive responses, which fade over time, and are statistically indistinguishable from the negative effects in the lowest-income group after the second year.

The findings in panels A through C are clearly in line with the structure of economic incentives. For the low-income families who after the reform receive a similar monthly benefit amount, but for a shorter period, the cost of childbearing increases, thereby significantly reducing fertility. The reform may encourage mothers in the middle of the income distribution to return to work faster after birth, but their cost of childbearing did not increase because the more generous benefit potentially overcompensates for the shorter eligibility. Therefore, we may observe a temporary fertility reduction and a subsequent catch-up. In contrast, the high-income families who failed the old means-testing are now eligible for benefits. This income effect and the "speed premium" may lead mothers with a strong desire for an additional child to bear it during the benefit receipt. After the eligibility eventually expires, their fertility declines. However, their fertility responses are generally weak.

Finally, panel D summarizes the findings on the heterogeneous fertility responses by reporting the effects for "losers" and "winners" of the reform. I label as "winners" those mothers for whom the absolute difference between the total predicted benefits under the new and the old regime is positive and as "losers" those with reduced or unchanged benefits. The estimates in panel D are generally in line with those for the earlier sample splits. While the "losers" exhibit strong negative fertility responses, the mostly mirror-inverted point estimates for the interaction terms translate into virtually no effects for the "winners". Relating these effects to the absolute

[34] In Germany, employment with net monthly earnings up to 800 euros is labeled as a "midi job" and qualifies for some wage subsidies. However, more widespread and generously subsidized are "mini jobs" if monthly earnings do not exceed 400 euros. In 2013, the thresholds increased to 850 and 450 euros, respectively.

[35] Again, the effects are a sum of the respective coefficients for the interaction terms to the effects in the first row.

benefit changes suggests that a financial loss of roughly 3000 euros lowers higher-order fertility at the lower bound of the income distribution, and this fertility decline seems to be rather persistent. In contrast, a gain of 4700 euros doesn't incentivize any remarkable fertility responses among the remaining income groups, and the slight birth postponement seems to be temporary.

6 Sensitivity analysis

Table 3 reports the results of several sensitivity tests that I perform by changing the sample criteria and using alternative specifications of Eq. 1. For transparency, I focus here on the months between 12 and 45.[36] Each cell shows the coefficient of the reform indicator obtained from a separate linear regression and the corresponding standard error.

First, I test whether my main results from Table 1 (repeated in Panel A) are robust to changes in the original set of control variables. Panel B shows that omitting the maternal covariates does not affect the results. These regressions condition solely on the year-specific indicators and the indicator for the first quarter of the year. Thus, the estimates are nearly identical with the simple comparison of means (reported in column 7 of Table A.2). Panel C demonstrates that the main results also remain unchanged if I include further covariates. These describe the previous child and comprise indicators for multiple birth, gender, birth order, and exact month of birth. I also include the father's characteristics such as age, migration status, education, employment status, and an indicator for a missing father. The robustness of my main results to changing the original sets of covariates strongly supports the "as good as random" assignment of the treatment.

Second, I assess whether potential shifts of births around the reform's day (Neugart and Ohlsson 2013; Tamm 2012) may damage the credibility of my identification strategy. Obviously, such short-run distortions may threaten empirical designs that rely on a sharp discontinuity framework and a short period around the cut-off date. However, I consider 3 months before and 3 months after January 1, 2007, so that the percentage of women who intentionally postponed delivery by a few days is negligible in my estimation sample. The shifting effects are therefore rather too small to have important consequences for my results.[37] Nevertheless, excluding 1 week before and 1 week after, the New Year's Eve from the estimation sample could provide a useful test for this argument. Given the lack of weekly information in my data, I need to omit all births from December and January, which reduces my sample by one third. Results in panel D show that this test yields somehow lower point estimates. Given the considerably smaller sample size, the standard errors increase throughout and the coefficient for month 33 remains significant. Nevertheless, qualitatively the results lead to similar conclusions.

[36]The overall reform effects for later months (columns 7, 8, and 10 of Table 1) are already very imprecise and the result remains unchanged in virtually all performed sensitivity tests.

[37]Nevertheless, given that delaying birth is biologically difficult, I acknowledge that generally, we cannot ignore the shifting and its potentially adverse health consequences for mothers and children.

Table 3 Effect of the reform on higher-order fertility: sensitivity tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome measure:	Probability of having a next child within ... months after a previous birth 12	21	24	33	36	45	Spacing in months
Panel A: Baseline							
Reform	−0.003	−0.012*	−0.017*	−0.035***	−0.029**	−0.029*	0.698**
	(0.002)	(0.007)	(0.009)	(0.011)	(0.014)	(0.015)	(0.312)
Obs.	52,423	52,423	50,384	50,384	46,224	46,224	46,224
Panel B: Maternal characteristics not included							
Reform	−0.002	−0.011*	−0.019**	−0.035***	−0.027**	−0.025*	0.667**
	(0.002)	(0.006)	(0.009)	(0.011)	(0.014)	(0.015)	(0.308)
Panel C: Control for maternal, previous child's, and father's characteristics							
Reform	−0.003*	−0.013**	−0.019**	−0.037***	−0.030**	−0.029**	0.679**
	(0.002)	(0.007)	(0.009)	(0.011)	(0.014)	(0.015)	(0.307)
Panel D: Exclude January and December births							
Reform	−0.001	−0.008	−0.017	−0.030**	−0.023	−0.018	0.398
	(0.003)	(0.008)	(0.011)	(0.014)	(0.018)	(0.019)	(0.387)
Obs.	34,856	34,856	33,512	33,512	30,750	30,750	30,750
Panel E: Exclude 2003/2004–2005/2006 births							
Reform	−0.003	−0.017**	−0.024**	−0.035***	−0.037**	−0.041**	0.915***
	(0.002)	(0.008)	(0.010)	(0.013)	(0.016)	(0.017)	(0.350)
Obs.	26,514	26,514	24,475	24,475	22,422	22,422	22,422
Panel F: Include 2007/2008 births							
Reform	−0.003	−0.010	−0.018**	−0.033***	−0.029**	−0.029*	0.717**
	(0.002)	(0.007)	(0.009)	(0.011)	(0.014)	(0.015)	(0.310)
Obs.	58,765	58,765	54,567	54,567	48,291	48,291	48,291
Panel G: Drop duplicate observations							
Reform	−0.001	−0.011	−0.029***	−0.043***	−0.035**	−0.041**	0.928***
	(0.002)	(0.008)	(0.010)	(0.013)	(0.016)	(0.017)	(0.359)
Obs.	47,211	47,211	45,606	45,606	42,261	42,261	42,261
Panel H: Alternative pooling of the Mikrozensus survey years 2009–2012							
Reform	−0.003	−0.012*	−0.019**	−0.037***	−0.030**	−0.028*	0.678**
	(0.002)	(0.007)	(0.009)	(0.012)	(0.015)	(0.016)	(0.333)
Obs.	52,423	52,423	39,203	39,203	25,909	25,909	25,909

Table 3 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Probability of having a next child within ... months after a previous birth						Spacing
measure:	12	21	24	33	36	45	in months
Panel I: Include only biological children from Mikrozensus 2012							
Reform	−0.009*	−0.011	−0.014	−0.052**	−0.042*	−0.039	0.791*
	(0.004)	(0.015)	(0.017)	(0.021)	(0.022)	(0.024)	(0.472)
Obs.	11,486	11,486	11,486	11,486	11,486	11,486	11,486
Maternal socio-demographic characteristics at previous childbirth							
save for Panel B	yes	yes	yes	yes	yes	yes	yes

Note: Each coefficient represents the coefficient of the reform indicator obtained from a separate linear regression. All regressions include a constant, indicators for turns of the years, and quarter of birth. Maternal sociodemographic characteristics include indicators for year of birth, education, employment, migration status, year of interview, state of residence, and regional unemployment rates, average gross earnings, and childcare coverage. Robust standard errors in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 % level

Source: Mikrozensus, pooled survey years 2009–2012, own calculations. In panel H, columns 3 and 4 are based on pooled Mikrozensus years 2010–2012 and columns 5–7 on the pooled years 2011–2012. Panel I uses only Mikrozensus 2012. Samples restricted to West German mothers of children born in Germany up to 3 months before/after the turn of the years 2001/2002–2006/2007. Panel F additionally includes childbirths in 2007/2008

Third, I test whether my selection of the control cohorts of mothers might drive the results. I repeated the estimations after omitting single and several cohorts, and after including only single ones. Each of these exercises led to nearly identical point estimates, though the dramatically reduced sample sizes affected statistical power in some specifications (not reported). Results in panel E exemplify these tests by excluding the cohorts 2003/2004-2005/2006. Their inclusion as control groups may raise concerns in light of the discussion on current versus future child effects (see Section 3) but the test leads to even slightly higher point estimates. In addition to all included pre-reform cohorts, panel F additionally considers the post-reform cohort 2007/2008 as a control group. The estimates also largely fortify my main results.

Finally, I examine whether the specific design of my estimation sample affects the findings. In panel G, I keep only one observation for mothers who repeatedly gave birth between 2001/2002 and 2006/2007. Some coefficients even slightly increase. In panel H, I modify the way of pooling the Mikrozensus surveys 2009–2012. Specifically, while I keep using all four waves for the analysis until month 21, I pool only the waves 2010–2012 for months 22–33, and only the waves 2011 and 2012 for months 34–45. Such a design reduces the later samples but might be more transparent than the original one (in Table A.2). This alternative approach yields nearly identical effects. In panel I, I perform all regressions on the single Mikrozensus 2012, which provides information on actual births. I can therefore exclude mothers for whom the number of children in the household and biological children differs. This conservative approach dramatically reduces the sample size but leaves the overall conclusions virtually unchanged.

7 Conclusions

This paper studies the impact of a recent change in the German parental leave benefit scheme on higher-order fertility. Although on average the new universal system is more generous, it pays for a shorter period than the abolished means-tested one. The reform differently affected the cost of childbearing across various groups of parents, and its unanticipated introduction in January 2007 allows for identification of causal effects on fertility dynamics. I compare mothers who were "just" eligible for the new benefit after birth with mothers "just" ineligible, and investigate how their later childbearing behavior differs. Based on data from Mikrozensus 2009–2012, this paper evaluates fertility responses in the first 5 years after the policy change.

I find that the reform significantly affected the timing of higher-order births. Consistent with evidence from a reduction in parental leave duration in Austria (Lalive and Zweimüller 2009), the effects are strongest around the ultimate benefit expiry and vanish thereafter. However, in contrast to Austrian mothers, German mothers who were "just" eligible for the new benefit postpone further births by an initial reduction of childbearing and a subsequent catch-up. The negative effects on the probability of having a further child are driven by the lowest-income mothers who do not catch up; their fertility reduction still persists in the fifth year. Such adverse effects are consistent with the increased cost of childbearing (Becker 1960) that stemmed from the reform for this group. In contrast, among mothers who are now better-off, the more generous benefits create relatively weak and rather temporary effects on higher-order births.

Previous studies conclude that the reform succeeded in encouraging mothers' post-birth labor supply (e.g., Bergemann and Riphahn (2011a) and Geyer et al. (2012)). However, my results suggest that these effects may spill over to fertility and have adverse consequences especially for mothers with relatively weak attachment to the labor market. While the reform's design allows for a wide range of plausible comparisons with other countries, extrapolating my results to other contexts should take place with caution. A unique feature of the German setting is the inconsistency and ambivalence of various social policies in their goals (Geisler and Kreyenfeld 2012). While the new parental leave regulations and recent expansions in public childcare provision imply a substantial paradigm shift towards a "dual-earner" oriented family policy (Spieß and Wrohlich 2008; Bauernschuster et al. 2014), a number policy measures still continues to promote the traditional "male breadwinner" family type (Hanel and Riphahn 2012; Spieß 2012).

In light of permanently low fertility and increasing postponement of first births, the issue of shortening the birth spacing has recently grown in importance, mainly because of the conjecture that compressed childbearing eventually increases completed fertility (Pötzsch 2012). However, although it is commonly held that modern family policies affect the timing of births, their effects on completed fertility are highly controversial and not yet fully explored (e.g., Gauthier (2007) and Laroque and Salanié (2013)). While this paper shows that the German reform affected the timing of higher-order births in the first 5 years, this conclusion generates at least two further questions for future research. First, will the transitory fertility shifts within various groups of parents eventually affect their completed family sizes? Second, will the different spacing of births itself have consequences for the children and mothers' future outcomes? Furthermore, given the high incidence of childlessness in Germany (e.g., Sobotka 2011), it would be extremely valuable to investigate the reform's impact on first-time motherhood in future work.

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