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

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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 in 2007 to assess how a move from a means-tested to an earnings-related benefit affects higher-order births. By using the German Mikrozensus 2010, I find that the reform significantly affected the timing of higher-order births in the first three years. Overall, mothers initially reduce childbearing, thereby extending their birth spacing, but eventually fully compensate for the earlier losses. The negative effects are largely driven by lowest-income mothers who also do not display any catch-up effects. I also find a substantial heterogeneity in West and East Germany. Because the reform aimed at parents with strong labor market attachment, the positive effects in the East suggest that the economic incentives essentially perform well, but their impact may be hampered by unfavorable institutional and cultural conditions in the West.

JEL classification: J13, J18, J20, K36

Keywords: fertility, family policy, reform, parental leave, Germany

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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, permanently experienced the highest rates above 1.7. At the same time, Austria, Germany, and most southern countries displayed fertility rates between 1.3 and 1.5 (World Bank 2013).

The cross-country differences in fertility levels draw attention to a broad range of family policies that vary across countries and potentially affect fertility choices.[1] In light of low fertility, various countries reviewed their family policies in recent decades with more or less explicitly formulated pronatalist intentions. Across the OECD, between 1990 and 2009 the public spending on family (excluding education) increased from 1.5 to 2.3 percent of GDP (OECD 2013).[2] So far, however, empirical literature that evaluates the impact of modern family policies on fertility is limited, partly because of serious challenges in estimating causal effects (see, e.g., Björklund 2007, González 2013).

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 specific goals, the reform aimed at shortening mothers' employment interruptions and encouraging fathers' involvement in childrearing tasks (Kluve and Tamm 2012). The reform replaced a means-tested system by a new benefit - *Elterngeld* - that is conditional on pre-birth earnings and was largely inspired by the "Nordic model" (Spieß and Wrohlich 2008).

[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 (Gauthier 2007).

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

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

The largely unanticipated introduction in January 2007 creates a natural experiment that allows for a reliable assessment of the German 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 childrearing (see, e.g., Bergemann and Riphahn 2011a, Geisler and Kreyenfeld 2012). However, save for studies that show a significant shifting of deliveries around the day of implementation (Neugart and Ohlsson 2012, Tamm 2012), so far there is no evidence on the reform's effects on fertility.

This paper contributes to previous research that evaluates the German reform by investigating its impact on fertility. Although the recent introduction does not yet facilitate the investigation of the reform's effect on completed fertility, I provide first evidence for other important fertility outcomes. Specifically, this paper addresses the questions whether and when a mother who has just given birth will have a next child, thereby focusing on higher-order fertility and birth spacing. About 70 percent of German women who had a first child go on to have a second, and the progression ratio from a second to a third child is 30 percent (Goldstein and Kreyenfeld 2011).[4] The reform incorporates incentives that explicitly promote tight birth spacing and by using data from the German Mikrozensus 2010, I examine their effectiveness in the first three years after the policy change. I acknowledge that this paper provides a partial evaluation of the reform because any potential fertility responses to date do not necessary translate to effects on completed fertility. Nevertheless, such pure "tempo" effect may have far-reaching consequences itself because different spacing of births may affect children's future outcomes (see, e.g., Pettersson-Lidbom and Skogman Thoursie 2009, Buckles and Munnich 2012).

By using a combination of a discontinuity design and a difference-in-differences approach, I find that on average the new benefit significantly reduced the probability of having a next child within the first 28 months after last birth. However, save for the lowest-income mothers, the effect erodes afterwards and disappears around month 36, thereby suggesting a full catch-up. I demonstrate that the heterogeneous responses along the income distribution are in line with the structure of economic incentives. I also find remarkable heterogeneity in fertility responses in

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

West and East Germany - two regions that differ in many respects related to childbearing.[5] The effects in the overall population are essentially driven by strong negative responses among West German mothers that offset the positive effects in the East.

The structure of this paper is as follows: Section 2 briefly describes the German parental benefit reform and its fertility-related incentives. Section 3 introduces the empirical approach and Section 4 describes 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 on 1st January 2007

On 1st January 2007 the parental benefit system in Germany substantially changed, though the duration of protected parental leave remained unchanged and lasts for three years. The reform abolished a means-tested system - *Erziehungsgeld* - that paid a maximum of 300 EUR monthly for up to 24 months or 450 EUR for up to 12 months. Kluve and Tamm (2012) report that the system covered about 66 percent of parents with the 300 EUR option, 10 percent with the 450 EUR option, and 24 percent were not eligible for any payment. Given rigorous income limits, the old system targeted families at the lower tail of the income distribution. For example, couples received the maximum benefit in the 300 EUR option if their annual joint income did not exceed 30,000 EUR in the first 6 post-birth months and 16,500 EUR in months 7-24.[6]

Parents of children born on 1st January 2007 and later receive a new benefit - *Elterngeld* - that depends on pre-birth earnings and ranges from 300 to 1,800 Euro. The new benefit replaces two-thirds of the average monthly net income that a parent who interrupts employment earned in the 12 months prior to the birth. The lower threshold of 300 EUR receive also those who earned less than 455 EUR per month and therefore fall below the threshold, including parents

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

[6] Slightly less restrictive income thresholds applied for families with further minor children and single parents. The thresholds referred to annual joint family income from the calender year before the childbirth for benefits in months 1-12 and the calender year of the childbirth for benefits in months 13-24. Although the exact income definition was not derived from tax law, it was comparable to net annual income (BMFSFJ 2005).

with no pre-birth earnings. The new system pays for at most 12 months if only one parent applies, and for at most 14 months if both parents take up leave or a single parent applies.[7] Within these time restrictions parents flexibly decide about the number of take-up months that they use consecutively or simultaneously (BMFSFJ 2011).

Two features of the new scheme promote short intervals between births. First, leave-taking parents who have another child aged under three or at least two further children aged under six receive a "sibling premium" of 10 percent of the benefit amount (at least 75 Euro). Second, the benefit take-up period is excluded from the 12-month basis used for calculation of benefit amount for future births (BMFSFJ 2011). Especially for parents whose regular pre-birth earnings provide a benefit of above 300 EUR, compressed birth intervals mitigate any benefit reductions due to lower labor supply between births. Previous literature terms a similar feature of the Swedish system "speed premium" (see, e.g., Neyer and Andersson 2008).

Table A.1 in the appendix provides a few statistical highlights on the take-up of the new benefit in 2010. The numbers reveal that mothers usually exhaust the full duration of the benefit eligibility. About a quarter of fathers take up the benefit, on average for three months, and usually in addition to a leave taken by the mother. A sharing of the leave taking between both parents provides extra 2 months and results in total leave of 14 months (BMFSFJ 2011). Furthermore, almost 12 percent of mothers spread the total benefit amount over a double period that may lead to a maximum eligibility duration of even 28 months. On average the monthly benefit amount is more generous compared to the maximum 450 EUR in the old scheme. Obviously, the current distribution of the benefit amount reflects the distribution of pre-birth earnings, but there are some exceptions. For example, although 39 percent of mothers had no pre-birth earnings, less than 29 percent of mothers actually receive the minimum benefit of 300 EUR. The differences emerge essentially from the "sibling premium" that is laid on the top of the eligible benefit amount.

[7] However, parents may optionally spread the total amount of the benefit over a double period, e.g., 24 instead of 12 months. During the extended take-up period they receive a half of the eligible monthly benefit (BMFSFJ 2011). In 2010, nearly 10 percent of parents used this option STBA (2012).

2.2 Affected groups of parents

Given the universal coverage of the new system and the design of the abolished means-testing, the policy change differently affected various groups of parents that differ with respect to employment status, earnings, and joint income of both parents.[8] Figure 1 shows the effective change in the total transfer amount 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 EUR option of the old one.[9] The numbers reflect the situation of a one-child family where the mother alone takes up the maximum eligibility duration.

[Insert Figure 1 here]

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 EUR over 24 months. These lowest-income families experience an effective loss in the total benefit amount of up to 3,600 EUR (Figure 1a, bottom left-hand corner). The loss results entirely from a shorter entitlement period that declines by 12 months (Figure 1b, bottom left-hand corner) because their monthly benefit amount of 300 EUR remains roughly unchanged. Second, the new system benefits parents with high joint income who would have failed the means-testing before, thereby losing the eligibility for any payment. These highest-income families experience an effective gain in the total benefit amount between 3,600 and 21,600 EUR (Figure 1a, the black asterisked line and upper right-hand corner). The gain emerges because their entitlement period increases by 12 months (Figure 1b, the black asterisked line and upper right-hand corner) and the new benefit ranges between 300 and 1,800 EUR per month, depending on mother's earnings. Finally, the overall effective changes for remaining parents depend on 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 mother's earnings increase now

[8] See e.g., Neugart and Ohlsson (2012), Kluve and Tamm (2012) for a more detailed description of the core legislative changes and their effect on various socio-economic groups.

[9] A comparison with the 450 EUR option would concentrate only on changes in benefit amount because its entitlement period of maximum 12 months is now similar.

the probability that a more generous monthly benefit overcompensates for a potential decrease in duration.

Previous empirical studies confirm differential responses to the policy change across various income groups. Studies that evaluate the effects on mothers' labor market outcomes find that low-income mothers respond to the abolished work disincentive by a faster work return and increased labor supply after the benefit expires (see, e.g., Bergemann and Riphahn 2011a, Geyer et al. 2012). In contrast, high-income mothers respond to the reform by reduced labor supply during the take-up months (see, e.g., Kluve and Tamm 2012, Geyer et al. 2012). Two studies evaluate the effects of the reform on fathers' behavior. Geisler and Kreyenfeld (2012) find a strong overall increase in fathers' leave usage that is mostly driven by highly educated men and those with fixed-term contracts. However, Kluve and Tamm (2012) 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 baby's life.

2.3 Mechanisms of potential fertility responses

Although previous studies conclude that the German reform succeeded in increasing incentives for mothers' to return to work and for fathers' to get involved in childrearing, so far, there is no causal evidence on whether it actually created pronatalist incentives.[10]

The main presumption in the standard economic approach to fertility is that fertility increases if the price of the marginal child declines (see, e.g., Becker 1960, Mincer 1962). Because the new parental leave regulations aim at mitigating parents' financial loss from employment interruptions (BMFSFJ 2011), the main reason why fertility should response is that the new benefit affects the net-of-benefit cost of childbearing. In addition, a more generous family policy may increase fertility through a positive income effect unless there is a meaningful tradeoff between child quantity and quality (Becker and Lewis 1973). A priori the overall effect of the German policy change on fertility is ambiguous also because it differently affected various

[10] Nevertheless, there is one descriptive study by Thyrian et al. (2010) for Pomerania (a region in North-East Germany). By comparing aggregate monthly birth rates up to 23 months before and after the introduction of the reform, the authors do not find any statistically significant differences.

income groups of parents (see Figure 1). Depending on parents' earnings, the reform not only differently changed the benefit amount but also its entitlement period.

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

While the empirical evidence on the causal link between parental leave regulations and fertility is scarce, a significant literature investigates fertility responses to other financial incentives. Recently, evidence on positive effects of child-related tax deductions provide, e.g., Milligan (2005) for a 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) find positive fertility responses to subsidies for children under the age of 18 in Israel, González (2013) to a universal benefit for newborns in Spain, and Brewer et al. (2011) to a welfare reform in the U.K.. This study extends this literature by providing evidence for a large European country with permanently low fertility levels and institutional framework that promoted over recent decades a traditional "male breadwinner" family type (see, e.g., Hanel and Riphahn 2012). The introduction of an earnings-related parental leave benefit in Germany 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 Figure 1), fertility responses are predictable only for selected groups. For example, fertility should (at least temporary) decline among the lowest-income parents for several reasons. First, they now experience overall benefit losses compared to the old system. Second, they face strong incentives to speed-up a mother's return to the labor market after birth to compensate for the benefit loss. Finally, they now have to have a further child within the next 12 (at the latest 14) months to benefit from an automatic renewal, and such a tight spacing of births may be biologically difficult. In contrast, several incentives should lead to an (at least temporary) increase in fertility among the highest-income parents. First, they experience an income effect because they are newly eligible for a benefit over 12 (at most 14) months. Second, if they would like to reduce their labor supply after childbirth, a sufficiently close spacing of subsequent births provides now a "speed premium". Finally, if a very tight spacing of births is not desirable, the option of spreading the total amount of the benefit over a double eligibility period extends the "economically optimal" interval between births to 28 months.

3 Estimation strategy

The introduction of the reform creates a natural experiment that allows for a credible assessment of how the new family policy affects fertility choices. Because the reform took place very recently, this paper focuses on mothers who have just given birth and studies the role of the reform for giving a sequential birth within the 36 following months. To identify the causal effect of the reform, I compare outcomes of mothers who gave birth shortly before and shortly after the reform came into effect. To isolate possible seasonal effects from those of policy change, I additionally use mothers who gave birth at the turns of previous years, as a control group. This strategy combines a discontinuity design with a difference-in-difference approach[11] and estimates a linear model of the form:

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

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

where y_i denotes future fertility outcome of a woman i. The indicator variable reform_i equals to one if a woman gave her previous birth shortly after the introduction of the reform (i.e., in the first quarter of 2007). The vector year_i includes four indicator variables that are equal to one if a woman gave her previous birth during a particular turn of the year for years between 2003/4 and 2006/7. A turn of the year comprises months from October through the next March and the reference year is 2002/3. Seasonal fixed effects are expressed by season_i, which corresponds to indicators for birth quarter of previous child in the main specification or birth month in the most flexible specification. Additionally, x_i captures maternal socio-demographic characteristics at the previous childbirth such as her year of birth, education, employment, migration status, and residence in East Germany. Some regressions include also similar covariates for the father, and several characteristics of the previous child such as indicators for multiple birth, gender, and birth order. 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 previous childbirth in response to the reform. A major validity threat is that parents would have known about the reform at the time of conception. However, previous research provides convincing evidence that the reform was largely unanticipated. For example, Kluve and Tamm (2012) show that the public discussion started in May 2006 when the governing parties agreed on the cornerstones of the reform. Parliament passed the new benefit in September 2006 and until then it was not clear whether the reform will eventually take place. This timeline and the simple fact that parents cannot perfectly plan a conception of a child suggest that births in the first quarter of 2007 were still largely independent of the reform.

The 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 (2012) and Tamm (2012) show that a significant number of women postponed delivery to the New Year to become eligible for the new benefit. However, the presence of such timing should be of minor importance for my results because less than 8 percent of mothers with due dates in the last December week successfully postponed births (Neugart and Ohlsson 2012). Still, I assess the sensitivity of my results to the exclusion of births from December and January.

Another important assumption is that potential seasonal patterns are common for mothers who gave birth at the turn of year 2006/7 and those who gave birth at the previous ones. Because, save for a graphical inspection, I am not able to test this assumption, a deliberate selection of control years is important. On the one hand, increasing the number of earlier years enlarges the estimation sample and might lead to efficiency gains. On the other hand, reducing the number of control years lowers the risk that the underlying seasonal effects changed over time or that other relevant policy changes contaminate the control groups. The directly preceding turns of years might be natural candidates for this role. To gain precision, I include women who gave birth at four preceding turns of years, but sensitivity tests show that my results are robust to alternative choices.

Provided that the central identification assumptions are satisfied, the coefficient ϕ represents what Lalive and Zweimüller (2009) term *current* child effect on higher-order fertility. Paraphrasing their argument, the German policy change may affect future fertility because it affects the cost of the child that is already born (current child), the child yet not born (future child), or both. While Lalive and Zweimüller (2009) identify both mechanisms by using a regression discontinuity framework, my empirical approach allows for causal interpretation of the *current* child effect of the reform. However, under the strong assumption that there were no substantial year-specific shocks, the coefficients for indicators for the turns of years should reflect the *future* child effect.[12] While I am reluctant to assign a causal interpretation to the magnitude of these estimates, their signs may be informative. Given that both the *current* and *future* child effects work in the same direction, my estimates yield a lower bound of the *total* effect that is of prime policy interest.

[12] Note that the indicator for the turn of year 2004/5 may capture the *future* child effect already in the estimation of probability of having a next child within the first 30 months because women who had their previous birth in October 2004 might have known about the reform in the middle of 2006. If they immediately changed their higher-order fertility plans, these births would occur by the end of the first quarter of 2007 i.e., around month 30 after previous birth. Although such exact timing of births is rather difficult, the probability of potential anticipation effects increases thereafter.

4 Data

I use data from the German Mikrozensus 2010 that provides a one percent sample of the population living in Germany in 2010.[13] The key advantages of the Mikrozensus are the availability of information on an individual's month of birth and the large sample size. I restrict the sample to mothers of children born in Germany at the turns of the years 2002/3 through 2006/7. A turn of the year corresponds to the months between October and the next March in the main specification, but I also tested the sensitivity of my results to larger and smaller time windows. Crucial for my research design is that I observe on average 400 births in each month.

Although the dataset is rich in many aspects, the 2010 survey does not directly record births. Instead, the data allows for identification of an individual's parents if they live in the same household at the time of the interview. Consequently, I observe only children who live in a mother's household and cannot distinguish between her biological and step children. For the same reasons, I also cannot determine the absolute birth order of a child, but I precisely know its birth order relative to other observed children of a mother. Despite these data limitations, I most likely observe a complete history of actual births for the vast majority of sampled mothers.[14]

My set of outcome variables determines whether and when a mother gives a next birth. I focus on mothers who were 15 to 45 years old at previous birth and track each mother up to 36 consecutive months. For example, for mothers of children born in March 2007, I consider the first subsequent birth that occurs not later than in March 2010. To shed light on the entire period, I construct a set of 25 indicators that measure the cumulative probability that a next birth occurs between the 12th and 36th month after the previous birth at a monthly frequency. For completeness, I study the spacing between the last and next child measured in months. Because

[13] Because the information on the month of birth is not available in the scientific use file, I use the data via a controlled remote access.

[14] I explored the scientific use file for the Mikrozensus 2008 that provides information on the actual number of children ever born to female respondents. By using the sample restrictions that I describe here, I found that the number of children actually born and the number of children living in the household are identical for 96 percent of mothers. The method used in this paper underestimated the number of actual births in 3 percent of cases and overestimated it in 1 percent of cases.

of the sampling frame of the data, I censor the spacing variable at the 33rd, instead of at the 36th month although the results are robust to censoring at month 36.[15]

The explanatory variable of interest is an indicator of whether the previous birth took place after the introduction of the reform, i.e., between January and March 2007 in the main specification. The key set of conditioning variables comprises four indicators for the turns of the years (2002/3 is the reference) and an indicator of whether a mother's last birth occurred from January to March versus previous October to December.

An important 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 at the time 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 thresholds: low education combines levels ISCED 1-2, middle ISCED 3-4, and high ISCED 5-6. Similarly, 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 additionally include indicators for a mother's year of birth at previous childbirth, of whether she was born in Germany, and whether she lives in East Germany. Further covariates describe the previous birth and comprise indicators for multiple birth, gender, and birth order of the child. I also identify the father and establish his year of birth, migration status, pre-birth education, and employment status. Note that I observe a father's socio-demographic characteristics only if he is present in the household in 2010.

The final sample consists of 12,079 mothers that give 13,111 observations. Although I treat a multiple birth as a single one, nearly 8 percent of sampled women repeatedly gave births between 2002/3 and 2006/7 and therefore occur several times in the main analysis.[16] However, for sensitivity analyses, I dropped the duplicate observations or run the regressions by parity

[15] The distribution of interviews is random over the entire year 2010. Therefore, if an interview takes place early in year, a child born in 2010 may not yet be observed in the data. Consequently, for mothers of children born in March 2007 the sampling design may slightly overestimate the spacing if the next child was born between January and March 2010 (i.e., after 33 months) and the interview had happened before the child's birth. For this group, the data may also underestimate the probabilities of having a next child within the first 34, 35, and 36 months.

[16] Specifically, out of the sampled women 84.5 percent occur once, 14.9 percent twice, and 0.6 percent three times.

of the previous child, thereby allowing each mother to enter the sample only once. These constraints had no major impact on my results, but they may bias the sample and limit its size.

5 Results

5.1 Descriptive analysis

Figure 2 exemplifies my outcome measures by showing the mean probability of having a higher-order birth within the first 24 and 36 months after previous birth. The horizontal axis shows the month of birth of the previous child. The vertical axis measures the percentage of women who gave birth to a next child within the following 24 and 36 months, respectively.

[Insert Figure 2 here]

Figure 2 highlights that the mean probabilities of having a next child fluctuate within the years, but with no clear trends or visible monthly patterns. Among mothers of children born before January 2007, on average 9 percent had a next child within the first 24 months and 21 percent within the first 36 months. For both presented fertility outcomes Figure 2 indicates falling trends after January 2007.

Figure 3 shows the same two outcome measures as Figure 2, but now aggregated by quarter of year and only for the last and for the first quarter of the analyzed turns of years. 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 between 2002/3 and 2006/7.

[Insert Figure 3 here]

The aggregation uncovers clear seasonal patterns that likely explain the visible changes in trends in Figure 2. The probabilities of having a next child within the first 24 and 36 months are lower for the first-quarter mothers, compared to the fourth-quarter mothers. Although not depicted here, I find similar patterns for the remaining outcome measures. Figure 3 illustrates that a simple comparison between mothers who gave last births shortly before and after the introduction of the reform may be misleading and does not provide a credible causal evidence. To isolate the causal effect of the reform from seasonal effects, I use mothers of children born at the turns of the years 2002/3-2005/6 as a control group. The key assumption here is that the seasonal patterns are common for the turn of year 2006/7 and the control ones. The two last sets of connected dots in Figure 3 visualize the core of my identification strategy by showing the first and second difference comparisons. 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 2002/3-2005/6.

Table A.2 in the 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 fertility. The results suggest that the effect on the probability of having a next child is negative between months 12 and 30 and positive for month 36. The average spacing between children increases by less than 0.4 month. However, this preliminary evidence is insufficient for causal inference because other characteristics important for fertility decisions may also vary over time. Although the statistics in Panel B suggest that the treated and control groups are comparable with respect to observable characteristics, the aggregate numbers in columns 3 and 4 hide some volatility over the four control years. To remove the influence of these confounding factors from the simple differences in mean fertility outcomes, I next estimate equation 1.

5.2 Regression analysis

Table 1 reports my key results on the effect of the reform on higher-order fertility obtained from multivariate regressions. The outcome measures in columns 1 to 5 are the cumulative probabilities of having a next child within the first 12, 18, 24, 30, and 36 months after previous birth, respectively. The dependent variable in column 6 is spacing between the last and the next birth in months. Each column reports results from a separate linear regression by showing the estimated coefficients and corresponding robust standard errors for selected variables. All regressions include a constant and control for a mother's year of birth, pre-birth employment status, education, migration status, and residence in East Germany.

[Insert Table 1 here]

The effects on the probability of having a next child are negative throughout (columns 1-5). The coefficient of the reform indicator for month 12 is insignificant and very close to zero. Given biological difficulties of conceiving soon after a birth, we should not expect any major effects of the reform here. The point estimate for month 18 increases in magnitude, but is also statistically insignificant. Overall the estimates are imprecise. However, given the small number of women affected by the reform in my sample and the low percentage of mothers who chose a very tight spacing of births, large standard errors are not surprising. The coefficients for months 24 and 30 suggest that the reform significantly reduced the probability of having a next child within the first 24 and 30 months by 2.2 and 2.8 percentage points, respectively. These are quantitatively large effects compared to the average incidence before the reform of 11 and 17 percent, respectively.

Interestingly, the reform effect erodes thereafter, because the point estimate for month 36 becomes small and insignificant. The negative effects for the earlier months translate into a slightly larger spacing between births. The point estimate in column 6 is significant at the 10 percent level and shows that the reform lead mothers to postpone their next birth on average by 0.4 month within the first three years.[17]

The estimated coefficients on the indicator for births that occurred between January and March capture seasonality effects that are common across years and are in line with the graphical inspection in Figure 3. Some of the point estimates for the year indicators show significant differences relative to the reference year 2002/3. While the estimates for the year 2003/4 capture solely year-specific shocks, the coefficients for the remaining turns of years may also reflect what Lalive and Zweimüller (2009) term the *future* child effect.

To illustrate the difference between the *current* and the *future* child effect, 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 after their next birth. Given that they immediately changed their further fertility plans in view of the reform, the *future* child effect should occur already nine months later, i.e., from the second quarter of 2007 onwards. Therefore, for mothers with previous birth at the turn of 2004/5 the *future* child effect may be apparent in the estimates for

[17] Because the dependent variable in column 6 is censored, I alternatively estimated a Tobit model. This estimation yielded comparable results.

months 30 and 36, for the 2005/6 mothers for months 18 through 36, and for the 2006/7 mothers just after last birth, i.e., at months 12 through 36. Although I am reluctant to interpret the point estimates for the year indicators causally, their signs provide observational evidence that the mechanism of *future* child effect works in the same direction as the *current* child effect that is captured by the reform indicator. Therefore, 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 36, Figure 4 traces the *current* child effect at a monthly frequency. I plot the coefficients on the reform indicator and 90 percent confidence intervals around these point estimates obtained from 25 separate regressions. The horizontal axis shows the number of months that have passed since birth of the previous child. The estimates for months 12, 18, 24, 30, and 36 match with those in Table 1.

[Insert Figure 4 here]

Figure 4 confirms that the reform had a negligible effect on conceiving the next child almost immediately after the last birth. However, although the effects up to month 22 are statistically indistinguishable from zero, the point estimates decline over time. The most likely explanation for this pattern is that the reform encourages mothers to return to work after the transfer expires (see, e.g., Bergemann and Riphahn 2011b, Kluve and Tamm 2012). 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 optionally spread the total amount of the benefit over a double period. The effect is largest exactly at month 28 when the benefit eligibility eventually expires. The cumulative probability of giving a next birth within the first 28 months drops by 3.4 percentage points. However, afterwards mothers start to compensate for the initial losses and the negative effect disappears around month 36. Because of this catch-up effect, the spacing between births measured at month 36 hardly increases, on average by less than half a month. Overall, my results are consistent with previous evidence for Austria where a reduction of the paid parental leave duration from 24 to 18 months lead to temporary effects on timing of higher-order births, but did not induce parents to revise their plans in a longer run (Lalive and Zweimüller 2009). However, while Austrian mothers accelerated their childbearing during the benefit receipt and reduced thereafter, fertility responses of German mothers go in the opposite directions. The heterogeneity in fertility responses to parental leave reductions 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 fertility responses by income groups. Given the lack of retrospective information on income in the Mikrozensus, I cannot precisely determine which families would have been eligible for the means-tested parental benefit under the old regime. Instead, I focus on parents' employment status and education at previous childbirth to proxy for their joint income, thereby distinguishing between families who were likely eligible and ineligible for benefit before the reform. Recall that after the reform, the eligibility is virtually universal, but the benefit amount depends on the pre-birth earnings of the parent who interrupts employment, i.e., usually the mother.

Table 2 reports the reform effect for several subgroups of parents. Each cell shows the coefficient of the reform indicator obtained from a separate linear regression and its standard error. These regressions include the same set of conditioning variables as regressions in Table 1. For comparison, Panel A repeats the results of Table 1. Note that smaller sample sizes in Panels B and C may implicate larger standard errors.

[Insert Table 2 here]

Panel B shows the results by a mother's pre-birth employment status. The first row evaluates the effects of the reform for previously not employed mothers and largely underpins the baseline results in Panel A. The point coefficients even increase slightly. Interestingly, the negative effect still persists in month 36 and contributes to a more pronounced increase in the spacing between births of almost 0.6 month. The corresponding results for previously employed mothers suggest that the reform left their higher-order fertility less affected, but the negative effect for month 30 and an increase afterwards are still visible.

Panel C repeats the analysis by parents' educational attainment. The results demonstrate that the low educated parents drive the overall effects in Panel A. Families where both parents have a middle education follow a similar pattern, but their fertility is slightly less affected and they appear to fully catch up in month 36. The highest educated parents seem to exhibit increased probability of having their next child immediately after last childbirth and lower probability after the second year. However, their responses (if any) are essentially weak because the point estimates are small in magnitude and imprecise.

Although mostly statistically insignificant, the coefficients in Panel B and 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 childrearing increased, thereby significantly reducing fertility. Among remaining families, which more likely exceeded the income thresholds in the means-testing under the old regime, fertility responses are less pronounced and vary with potential earnings. 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 decrease in the benefit duration. Therefore, we may observe a temporary reduction and a subsequent catch-up in fertility around month 36. In contrast, the high-income families who failed the means-testing before are now eligible for a benefit. This income effect and the "speed premium" may lead mothers with a strong desire to have a further child to give a next birth during the benefit receipt. After the eligibility eventually expires, their fertility starts to decline. However, as opposed to lower-income groups, fertility of high-income mothers hardly responses to the new benefit within the analyzed period. Importantly, these differential responses along the income distribution are robust to using other plausible proxies for pre-birth income such as parents' current earnings, which are available in the dataset as of 2010.

Finally, Panel D of Table 2 repeats the analysis by region of residence. Although since the German unification in 1990, East and West Germany share a common legal framework, the two regions still differ in many respects. Previous literature emphasizes striking differences in childbearing behavior (see, e.g., Goldstein and Kreyenfeld 2011), labor market conditions (see, e.g., Orlowski and Riphahn 2009), public childcare infrastructure (see, e.g., Wrohlich 2008), attitudes with regard to maternal labor supply, and women's labor market attachment (see, e.g., Hanel and Riphahn 2012). Therefore, splitting the sample by region of residence creates internally more homogeneous groups.

Panel D of Table 2 uncovers interesting differences in responses to the parental benefit reform in the two German regions.[18] Although at month 36 (column 6) in both West and East Germany the reform effect is statistically indistinguishable from zero, the effects in previous months differ substantially. While in West Germany the reform yielded strong negative effects on the probability of having a next child (columns 1-4) and increased the spacing between births by almost 0.6 month (column 6), fertility responses in East Germany go exactly in the opposite direction throughout. Although statistically insignificant, the coefficient in column 6 shows that among East German women the reform compressed birth spacing by more than 0.6 month.

Figure 5 illustrates the development of those differences at a monthly frequency. The plot for West Germany follows the average pattern from Figure 4, save for larger magnitude of the effects. For example, the effect is strongest for the probability of having a next child within the first 28 months, but the decrease in West Germany is by 4.5 percentage points, compared to 3.4 percentage points in the total population. In contrast, in East Germany the point estimates are positive from month 12 onwards and increase in magnitude up to month 21. Therefore, among East German women the reform significantly increased the probability of bearing a next child within the first 21 months by 7 percentage points, but the effect also declines afterwards.

[Insert Figure 5 here]

Several important differences between East and West German mothers may potentially explain their differential responses to the reform. In general, East German mothers demonstrate a considerably higher labor force supply, face stronger social attitudes in favor of maternal employment, and given the better availability of childcare infrastructure in the East, experience more encouraging conditions to combine family and work (see, e.g., Hanel and Riphahn 2012). As far as the reform intended to facilitate childbearing among parents with strong labor market attachment, the positive effects in the East illustrate that the created incentives essentially success in achieving their goals. In contrast, for West German mothers the policy change meant

[18] I cannot distinguish between former East and West Berlin and assign both to West Germany, but the results are robust to excluding Berlin from the sample.

a more radical move from a traditional "male breadwinner" to a "dual-earner" family model (Spieß and Wrohlich 2008). Different childbearing behavior itself may also play a role. For example, East German women generally space their children further apart (Pötzsch 2012), so that they had, from a biological perspective, more scope for fertility shifts due to the reform than West German mothers.

6 Sensitivity analysis

Table 3 shows the results of several sensitivity tests that I perform by changing the sample criteria and using alternative specifications of equation 1. Each cell shows the coefficient of the reform indicator obtained from a separate linear regression and the corresponding standard error. All regressions condition on maternal characteristics and include a constant. For comparison, Panel A repeats the main results from Table 1.

[Insert Table 3 here]

First, I test whether the main findings in Panel A, which control for a mother's characteristics, are robust to the inclusion of further covariates. Results in Panel B demonstrate that controlling for characteristics of the previous child leaves the magnitude of the point estimates and standard errors virtually unchanged. I also find that adding a child's exact month of birth yields identical results (not reported). Estimations in Panel C show that conditioning on father's characteristics slightly increases the point estimates and their significance. However, because the availability of a father's variables depends on his presence in the household in 2010, which may be a post-treatment outcome, I omit them in the main analysis.

Second, I assess whether the shifts of births from the last weeks of December 2006 into the New Year (Neugart and Ohlsson 2012, Tamm 2012) damage the credibility of my identification strategy. A higher proportion of mothers with pre-birth employment and a lower proportion of those not employed in column 7 of Table A.2 are in line with such sorting effects. Because I don't observe the exact week of delivery, to exclude the impact of potential shifting of births, I need to omit all births that occur in December and January in any year from my sample. This restriction reduces my sample by one third. Results in Panel D show that this test yields a lower point estimate for month 18, but this small change does not imply any consequences to my interpretation. Given the considerably smaller sample size, the standard errors increase slightly throughout and the coefficients for months 24 and 30 are no longer significant. However, their magnitude remains very similar to the original ones, thereby supporting a causal interpretation.

Third, I test whether the main findings in Panel A are driven by my selection of the control turns of years. I repeat the estimations after omitting single years, after excluding pairs of years, and after including only single control years. Each of these exercises leads to nearly identical magnitudes of the effects, though not always significant (not reported). Results in Panel E exemplify these tests by showing results from regressions that exclude the years 2004/5 and 2005/6. Recall that their inclusion may raise concerns in light of the discussion on current versus future child effects (see Section 3). However, the estimates obtained from this restricted sample largely underpin my main results.

Finally, I examine whether my main findings hold if I keep only one observation for the small number of mothers who gave more than one birth in the considered period. Panel F shows the results obtained from this adjusted sample. Some of the coefficients even slightly increase, thereby strengthening the credibility of my inferences.

7 Conclusions

This paper studies the impact of a recent change in the German parental benefit scheme on fertility. Although on average the new system is more generous, it pays for a much shorter period of time (Bergemann and Riphahn 2011a). Moreover, given the universal coverage of the new scheme and the design of the abolished means-testing, the policy change differently affected the cost of childbearing across various groups of parents. The largely unanticipated introduction of the reform in January 2007 allows for identification of its causal effects on fertility dynamics. Given the recent introduction, this paper evaluates short-run fertility responses and focuses on higher-order births.

I find that the reform significantly affected the timing of higher-order births within the three-year period of analysis. Consistently with evidence from a reduction in parental leave duration in Austria (Lalive and Zweimüller 2009), fertility responses are strongest around the ultimate expiry of the benefit and vanish thereafter. However, in contrast to Austrian mothers, German mothers initially significantly reduce childbearing. The negative effect on probability of having a further child is mainly driven by the lowest-income mothers whose fertility reduction extends beyond the duration of the benefit. Their negative fertility responses are consistent with the increased cost of childrearing (Becker 1960). Among remaining mothers, fertility responses to a more generous benefit are less pronounced and vary with potential earnings.

I also find substantially different effects of the reform in West and East Germany. The initial effects in the overall population are essentially driven by strong negative effects in West Germany that offset positive effects in the East. Given that the reform targeted at parents with strong labor market attachment and mothers in the East generally display a considerably higher labor supply than West German mothers (see, e.g., Hanel and Riphahn 2012), these heterogeneous effects are not surprising. However, the East-West differences in the reform effect may be also a result of substantially different institutional and cultural conditions. For example, while the new benefit scheme promotes a tight spacing of births, the fairly strong social attitudes against maternal employment and low coverage of subsidized childcare for infants in West Germany (see, e.g., Hanel and Riphahn 2012) make the compliance to such incentives less feasible than in the East. However, these explanations are largely speculative and need to be tested with more comprehensive data to allow for far-reaching conclusions.

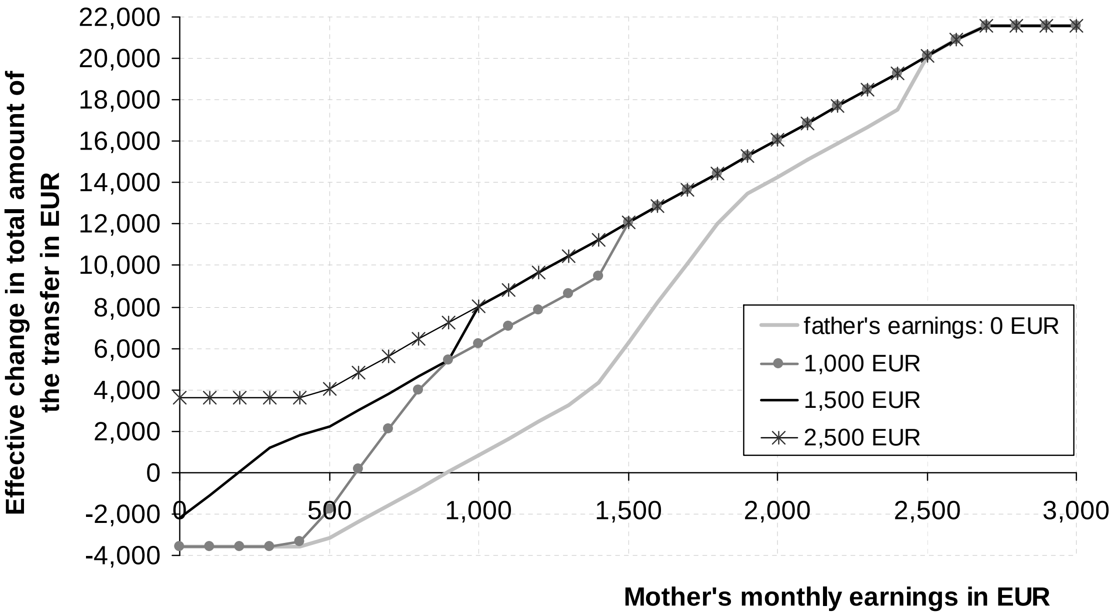
Previous studies conclude that the German reform succeeded in encouraging mothers' post-birth labor supply (see, e.g., Bergemann and Riphahn 2011a, Geyer et al. 2012). However, my results show that these labor supply effects may spill over to fertility and have adverse effects among particular groups of mothers, especially among those with relatively weak labor market attachment. While the reform design and heterogeneities of the German population allow for a wide range of plausible comparisons with other countries, extrapolating my results to other context should take place with caution. A unique feature of the German setting is the inconsistency and ambivalence of various 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. 2013), a number policy measures still continues to promote the traditional "male breadwinner" family type (Hanel and Riphahn 2012, Spieß 2012).

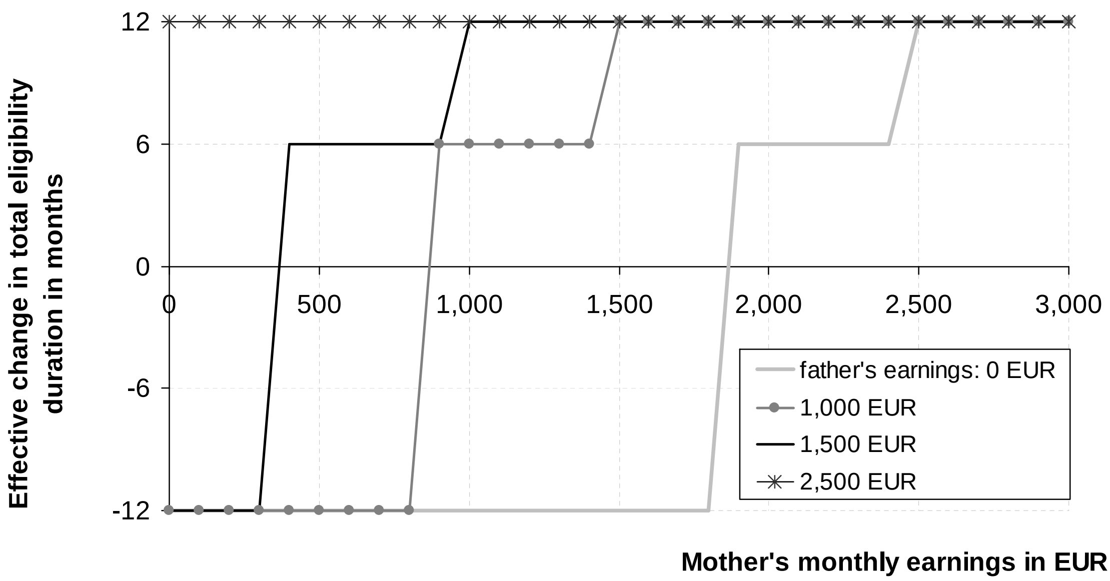
In light of permanently low fertility and increasing postponement of first births, the issue of shortening the birth spacing has recently grown in importance, mainly because of the conjecture that a more compressed childbearing eventually increases completed fertility (Pötzsch 2012). However, although it is commonly held that modern family policies affect the timing of births, their effects on completed family size are highly controversial and yet not fully explored (see, e.g., Gauthier 2007, Milligan 2005, Cohen et al. 2013, Laroque and Salanié 2013). While this paper shows that the German reform affected the timing of higher-order births in the first three years, this conclusion generates at least two further questions for future research. First, whether the transitory fertility shifts within various groups of parents will eventually affect their completed family size? Second, whether the different spacing of births itself will have consequences for the children's future outcomes?

Figure 1: Effective policy changes by mother's and father's earnings

(a) Change in total benefit amount

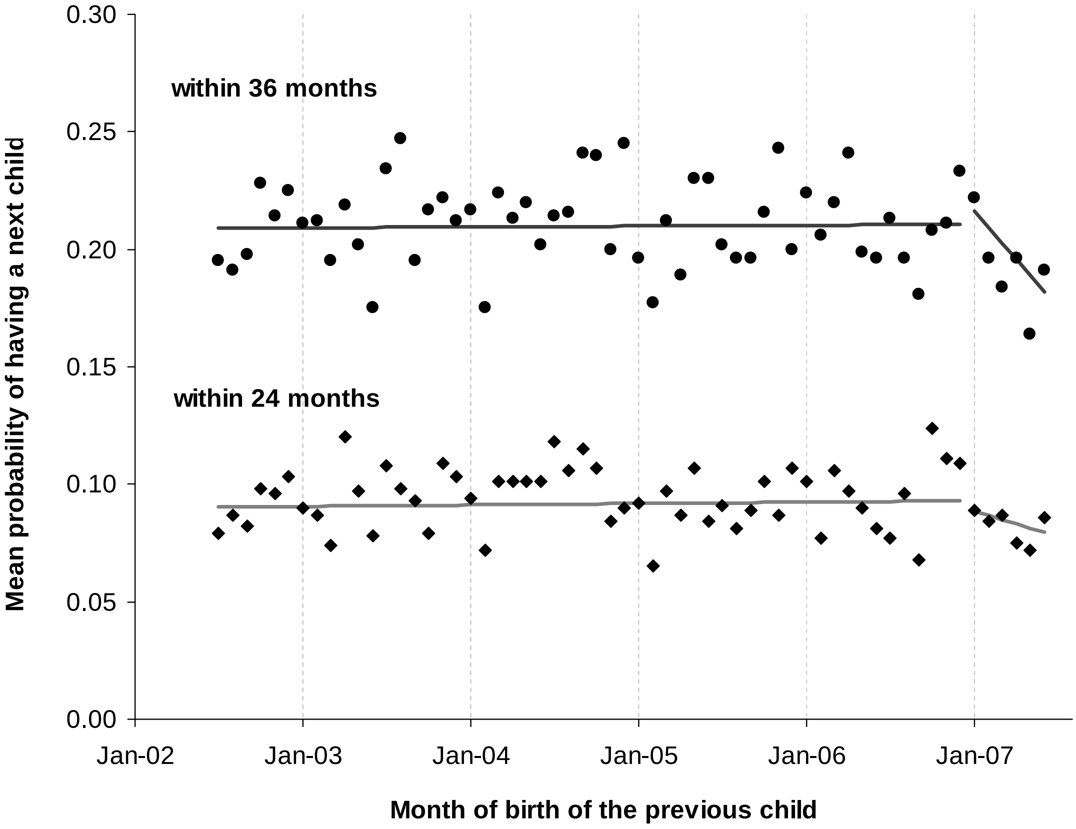


(b) Change in eligibility duration



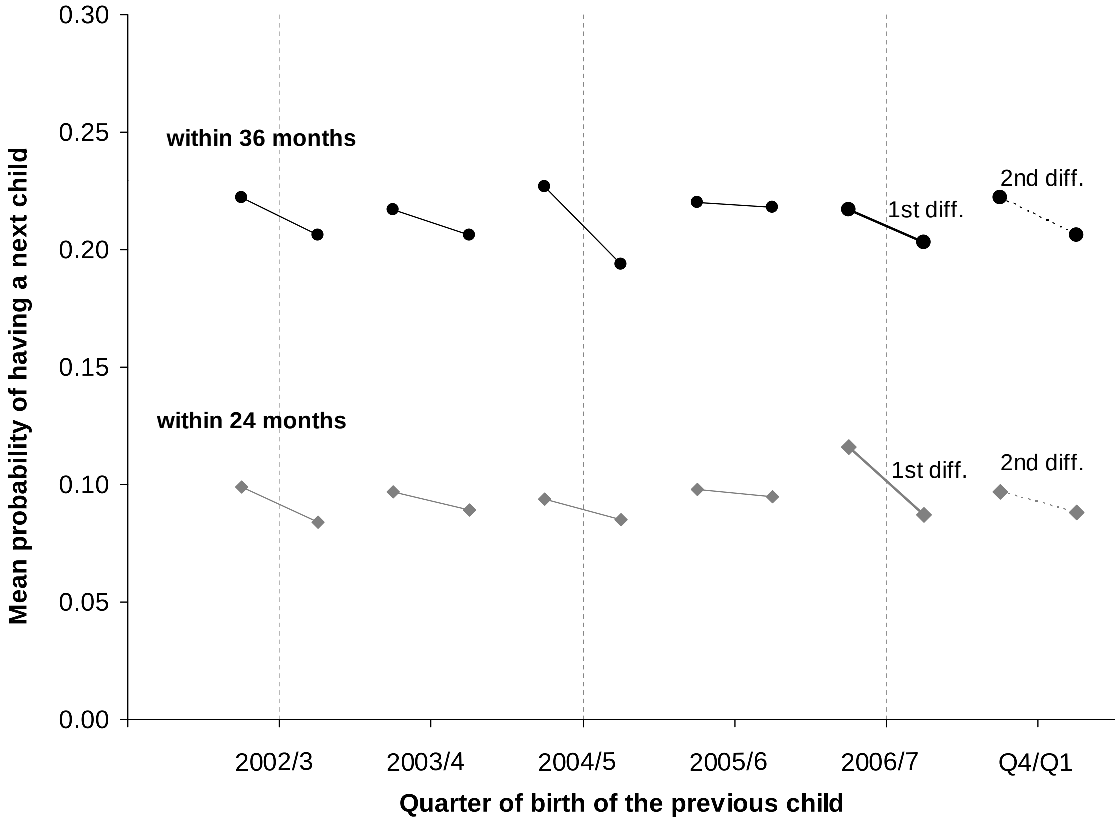
Note: The plots compare the new benefit (*Elterngeld*) and the 300 EUR option of the previous system (*Erziehungsgeld*) by showing the absolute differences in the total benefit amount and duration. The numbers reflect the situation of a one-child family where the mother takes up the maximum eligibility duration.
Source: The corresponding bills are *Bundeserziehungsgeldgesetz* (BErzGG) and *Bundeselterngeld- und Elternzeitgesetz* (BEEG), own calculations.

Figure 2: Mean probabilities of having a next child within 24 and 36 months by month of birth of previous child



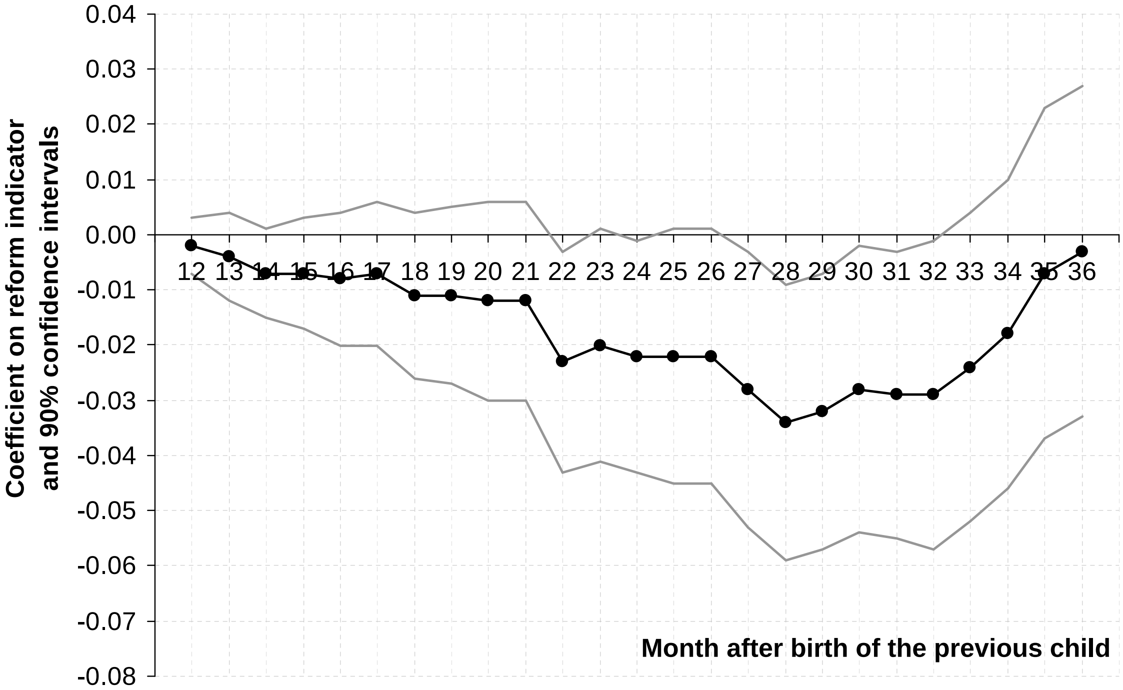
Note: The plot shows unweighted raw data.
Source: German Mikrozensus 2010, own calculations. Sample restricted to mothers of children born in Germany between July 2002 and July 2007.

Figure 3: 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 years 2002/3-2005/6. Source: Mikrozensus 2010, own calculations. Sample restricted to mothers of children born in Germany up to three months before/after the turn of the year for years 2002/3-2006/7.

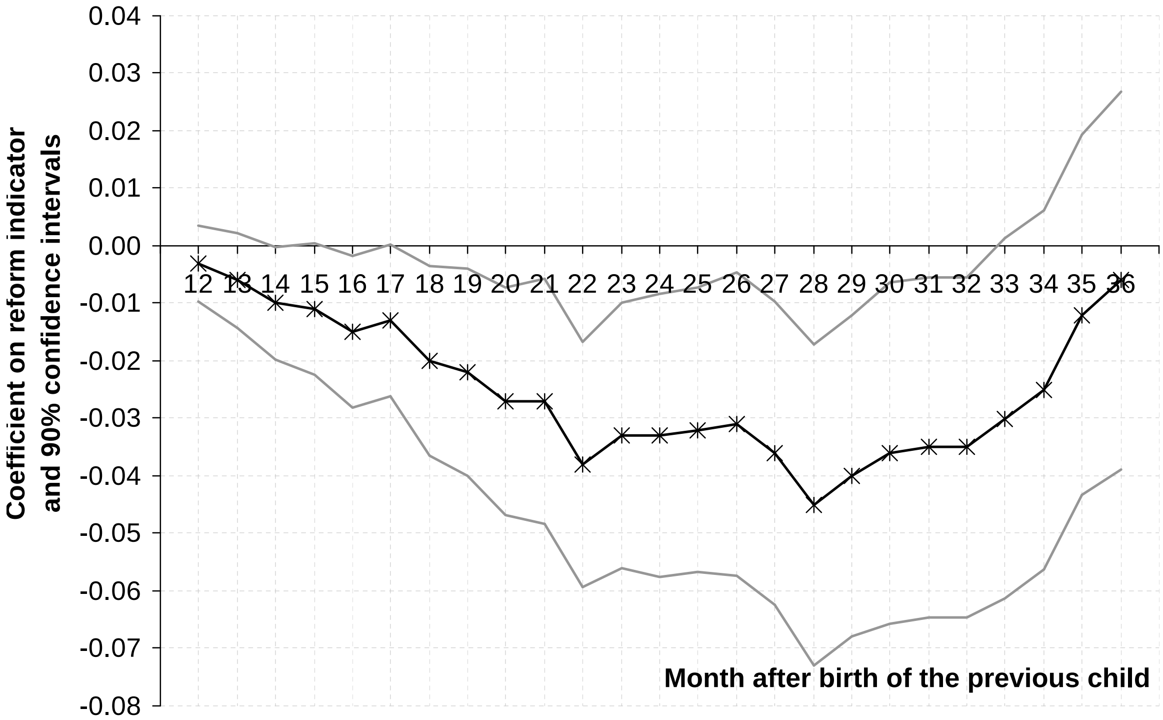
Figure 4: Effect of the reform on cumulative probability of having a next child within the first 12 through 36 months after previous birth



Note: Each dot shows a coefficient on reform indicator obtained from a separate linear regression. Grey lines are 90% confidence intervals around the point estimate. All regressions include a constant, indicators for turns of the years, quarter of birth, and maternal socio-demographic characteristics at previous childbirth such as indicators for year of birth, education, employment, migration status, and residence in East Germany.
Source: Mikrozensus 2010, own calculations. Sample restricted to mothers of children born in Germany up to three months before/after the turn of the year for years 2002/3-2006/7.

Figure 5: Effect of the reform on cumulative probability of having a next child within the first 12 through 36 months for West and East Germany

(a) West Germany



(b) East Germany

Note: Berlin included in West Germany. Each asterisk/square shows a coefficient on reform indicator obtained from a separate linear regression. Grey lines are 90% confidence intervals around the point estimate. All regressions include a constant, indicators for turns of the years, quarter of birth, and maternal socio-demographic characteristics at previous childbirth such as indicators for year of birth, education, employment, and migration status.

Source: Mikrozensus 2010, own calculations. Sample restricted to mothers of children born in Germany up to three months before/after the turn of the year for years 2002/3-2006/7.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome measure:	Probability of having a next child within ... months 12	18	24	30	36	Spacing in months
Reform	-0.002	-0.011	-0.022 *	-0.028 *	-0.003	0.406 *
	(0.003)	(0.009)	(0.013)	(0.016)	(0.018)	(0.212)
Month of previous birth:						
January-March	-0.001	-0.001	-0.010 *	-0.009	-0.019 **	0.126
	(0.002)	(0.004)	(0.006)	(0.007)	(0.008)	(0.091)
October-December	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Year of previous birth:						
2006/7	0.002	-0.002	-0.004	-0.025 *	-0.057 ***	0.151
	(0.003)	(0.007)	(0.011)	(0.013)	(0.015)	(0.178)
2005/6	0.002	-0.002	-0.013	-0.021 **	-0.035 ***	0.213 *
	(0.002)	(0.005)	(0.008)	(0.010)	(0.011)	(0.129)
2004/5	0.003	-0.007	-0.012	-0.020 **	-0.029 ***	0.215 *
	(0.002)	(0.005)	(0.008)	(0.010)	(0.011)	(0.125)
2003/4	0.003	0.002	-0.002	-0.004	-0.011	-0.008
	(0.002)	(0.005)	(0.008)	(0.010)	(0.011)	(0.128)
2002/3	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Socio-demographic characteristics at previous childbirth						
Maternal	yes	yes	yes	yes	yes	yes
Observations			13,111			

Note: Each column represents a separate linear regression. All regressions include a constant. Maternal socio-demographic characteristics include indicators for year of birth, education, employment, migration status, and residence in East Germany. Robust standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.
Source: Mikrozensus 2010, own calculations. Sample restricted to mothers of children born in Germany up to three months before/after the turn of the year for years 2002/3-2006/7.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Probability of having a next child within ... months					Spacing
measure:	12	18	24	30	36	in months
Panel A: Baseline						
Reform	-0.002	-0.011	-0.022 *	-0.028 *	-0.003	0.406 *
(n=13,111)	(0.003)	(0.009)	(0.013)	(0.016)	(0.018)	(0.212)
Panel B: Mother's employment						
Not employed	-0.006	-0.017	-0.029	-0.036	-0.036	0.567 *
(n=6,425)	(0.004)	(0.013)	(0.021)	(0.025)	(0.028)	(0.310)
Employed	0.003	0.001	-0.013	-0.025	0.019	0.228
(n=5,059)	(0.004)	(0.006)	(0.007)	(0.008)	(0.091)	(0.318)
Panel C: Parents' education						
Both low	-0.002	-0.046	-0.063	-0.061	-0.063	1.132
(n=1,031)	(0.023)	(0.042)	(0.054)	(0.065)	(0.068)	(0.955)
Both middle	-0.007	-0.020	-0.046 **	-0.055 **	0.011	0.715 **
(n=4,128)	(0.006)	(0.014)	(0.022)	(0.026)	(0.030)	(0.353)
Both high	0.009	0.011	0.018	-0.009	-0.042	-0.070
(n=1,768)	(0.005)	(0.020)	(0.038)	(0.045)	(0.051)	(0.545)
Panel D: Region of residence						
West Germany	-0.003	-0.020 **	-0.033 **	-0.036 **	-0.006	0.593 **
(n=11,209)	(0.004)	(0.010)	(0.015)	(0.018)	(0.020)	(0.236)
East Germany	0.003	0.037 **	0.037	0.014	0.012	-0.619
(n=1,902)	(0.003)	(0.018)	(0.029)	(0.036)	(0.040)	(0.463)
Socio-demographic characteristics at previous childbirth						
Maternal	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 socio-demographic characteristics include indicators for year of birth, education, employment, migration status, and residence in East Germany. Robust standard errors in parentheses.
***, ** and * indicate statistical significance at the 1%, 5% and 10% level.
Source: Mikrozensus 2010, own calculations. Sample restricted to mothers of children born in Germany up to three months before/after the turn of the year for years 2002/3-2006/7.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome measure:	Probability of having a next child within ... months 12	18	24	30	36	Spacing in months
Panel A: Baseline						
Reform	-0.002	-0.011	-0.022 *	-0.028 *	-0.003	0.406 *
(n=13,111)	(0.003)	(0.009)	(0.013)	(0.016)	(0.018)	(0.212)
Panel B: Control for previous child's characteristics						
Reform	-0.002	-0.010	-0.021	-0.027 *	-0.002	0.396 *
(n=13,111)	(0.003)	(0.009)	(0.013)	(0.016)	(0.017)	(0.210)
Panel C: Control for previous child's and father's characteristics						
Reform	-0.003	-0.011	-0.022 *	-0.029 *	-0.004	0.412 **
(n=13,111)	(0.003)	(0.009)	(0.013)	(0.016)	(0.017)	(0.209)
Panel D: Exclude January and December births						
Reform	-0.002	-0.001	-0.024	-0.031	-0.001	0.338
(n=8,710)	(0.004)	(0.010)	(0.016)	(0.019)	(0.021)	(0.255)
Panel E: Exclude 2004/5 and 2005/6 births						
Reform	-0.002	-0.011	-0.019	-0.028	-0.004	0.363
(n=7,881)	(0.004)	(0.009)	(0.014)	(0.017)	(0.020)	(0.230)
Panel F: Drop duplicate observations						
Reform	-0.001	-0.026 ***	-0.022 *	-0.055 ***	-0.013	0.658 ***
(n=12,079)	(0.003)	(0.008)	(0.013)	(0.015)	(0.017)	(0.203)
Socio-demographic characteristics at previous childbirth						
Maternal	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 socio-demographic characteristics include indicators for year of birth, education, employment, migration status, and residence in East Germany. Robust standard errors in parentheses.
***, ** and * indicate statistical significance at the 1%, 5% and 10% level.
Source: Mikrozensus 2010, own calculations. Sample restricted to mothers of children born in Germany up to three months before/after the turn of the year for years 2002/3-2006/7.

Appendix

Table A.1: ***Elterngeld*** **for children born in 2010**

	Benefit-taking parents	
	Mothers	Fathers
Children covered (in %)	96.2	25.3
Take-up period		
Average duration (in months)	11.7	3.3
Maximal duration: 12 - 14 months (in %)	91.3	6.5
Shared with partner (in %)	24.2	92.7
Spread over a double period (in %)	11.6	1.3
Monthly benefit amount		
Average amount (in EUR)	664	1,076
Benefit of exactly 300 EUR (in %)	28.5	15.0
Benefit from 300 to 500 EUR (in %)	21.0	6.5
Benefit from 500 to 1,800 EUR (in %)	47.2	62.8
Benefit of 1,800 EUR and more (in %)	3.3	15.7
Sibling premium (in %)	22.3	17.4
Pre-birth net monthly earnings		
No earnings (in %)	39.1	14.3
Earnings below 500 (in %)	9.8	2.8
Earnings from 500 to 1,000 (in %)	15.5	6.5
Earnings from 1,000 to 1,500 (in %)	16.3	16.2
Earnings from 1,500 to 2,000 (in %)	10.3	22.3
Earnings from 2,000 to 2,700 (in %)	5.8	19.9
Earnings of 2,700 and more (in %)	3.2	18.1

Source: STBA (2012).

Table A.2: Sample means

	(1)	(2)	(3)	(4)	(5) (2)-(1)	(6) (4)-(3)	(7) (5)-(6)
Turn of year	2006/7		2002/3 - 2005/6		Differences		
Quarter	Q4	Q1	Q4	Q1	1st	2nd	Diff-in-Diff
Panel A: Fertility outcomes							
Probability of having a next child within ... months							
12	0.008	0.004	0.007	0.006	-0.004	-0.001	-0.003
18	0.045	0.034	0.039	0.038	-0.011	-0.001	-0.010
24	0.116	0.087	0.097	0.088	-0.029	-0.009	-0.020
30	0.169	0.138	0.160	0.154	-0.031	-0.006	-0.025
36	0.217	0.203	0.222	0.206	-0.014	-0.016	0.002
Spacing between the previous and next child in months							
	31.83	32.30	32.08	32.17	0.464	0.096	0.368
Panel B: Socio-demographic characteristics (at previous childbirth)							
Mother's characteristics							
German-born	0.758	0.763	0.772	0.771	0.005	-0.001	0.006
East	0.159	0.157	0.144	0.140	-0.002	-0.004	0.002
Age	29.95	29.65	29.74	29.76	-0.299	0.023	-0.322
Education low	0.191	0.211	0.189	0.193	0.020	0.004	0.016
Education mid	0.564	0.553	0.579	0.574	-0.011	-0.005	-0.006
Education high	0.245	0.236	0.232	0.232	-0.009	0.000	-0.009
Not employed	0.470	0.445	0.498	0.498	-0.025	0.000	-0.025
Employed	0.417	0.454	0.372	0.376	0.037	0.004	0.033
Empl. missing	0.113	0.101	0.130	0.127	-0.012	-0.003	-0.009
Father's characteristics							
Living in HH (2010)	0.844	0.856	0.837	0.830	0.012	-0.007	0.019
German-born	0.639	0.640	0.645	0.640	0.001	-0.005	0.006
Age	33.67	33.65	33.38	33.46	-0.013	0.085	-0.098
Education low	0.107	0.133	0.110	0.109	0.026	-0.001	0.027
Education mid	0.449	0.442	0.436	0.431	-0.007	-0.005	-0.002
Education high	0.288	0.281	0.291	0.290	-0.007	-0.001	-0.006
Not employed	0.266	0.266	0.308	0.284	0.000	-0.024	0.024
Employed	0.520	0.529	0.470	0.481	0.009	0.011	-0.002
Empl. missing	0.058	0.061	0.059	0.065	0.003	0.006	-0.003
Previous child's characteristics							
Boy	0.496	0.530	0.513	0.507	0.034	-0.006	0.040
Twin	0.015	0.014	0.019	0.018	-0.001	-0.001	0.000
Parity 1	0.496	0.487	0.511	0.503	-0.009	-0.008	-0.001
Parity 2	0.348	0.347	0.350	0.349	-0.001	-0.001	0.000
Parity ≥ 3	0.156	0.165	0.139	0.148	0.009	0.009	0.000
Observations	1,186	1,348	5,243	5,334			

Source: Mikrozensus 2010, own calculations. Sample restricted to mothers of children born in Germany up to three months before/after the turns of the years 2002/3-2006/7. The 1st difference comparison in column 5 is between the means corresponding to the 4th quarter of 2006 and the 1st quarter of 2007. The 2nd difference comparison in column 6 is between the means for the 4th and 1st quarters aggregated over the years 2002/3-2005/6.

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