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The Fertility Response to Cutting Child Related Welfare Benefits

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Abstract

Despite long-term interest whether welfare benefits motivate fertility, evidence from research has not been consistent. This paper contributes new evidence to this debate by investigating the fertility effect of a German welfare reform. The reform decreased the household income of families on welfare by 18 percent in the first year after the birth of a baby. Using exclusive German social security data on over 460,000 affected women, our analysis finds the reform leads to a fertility reduction of 6.8 percent. This result implies that for mothers on welfare, fertility has an income elasticity of 0.38, which is much smaller than that of general populations reported in the literature. Our findings suggest that welfare recipients' fertility reacts less strongly to financial incentives than the fertility of overall populations.

Zusammenfassung

Bereits seit langer Zeit wird untersucht, ob Sozialleistungen die Fertilität anregen. Dennoch ist die Forschung bisher nicht konsistent. Dieses Papier trägt zu ebendieser Debatte bei, indem es den Fertilitätseffekt einer deutschen Sozialleistungsreform untersucht. Die Reform verringerte das Haushaltseinkommen von Familien mit Sozialhilfebezug um 18 Prozent im ersten Jahr nach Geburt ihres Kindes. Die Analyse exklusiver Sozialversicherungsdaten von über 460.000 betroffenen Frauen ergibt, dass die Reform zu einer Verringerung der Fertilität von 6,8 Prozent führt. Dieses Ergebnis legt nahe, dass für sozialhilfebeziehende Mütter Fertilität eine Einkommenselastizität von 0,38 hat. Das ist wesentlich kleiner als in bisherigen Untersuchungen zur Gesamtbevölkerung angenommen. Unsere Befunden lassen darauf schließen, dass die Fertilität von Sozialhilfeempfängern weniger stark auf finanzielle Anreize reagiert, als die der Gesamtbevölkerung.

JEL classification

J13, I38, C54

Keywords

Fertility, parental leave benefits, welfare benefits

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1 Introduction

Do generous welfare benefits enhance the fertility of families on welfare? For decades, this question has been the object of interdisciplinary research. This issue is of considerable interest to policy makers because a strong fertility reaction to changes in welfare benefits may provide support for individuals who advocate for a reduction in welfare spending. The most prominent example of policy that aligns with this line of thought are Family Caps in the US. These policies reduced welfare payments when children were born to families while the mother received welfare. [1]

The idea that welfare payments enhance fertility aligns with dominant theory on family economics. Becker's (1960) seminal works suggest that fertility is negatively correlated with the price for children. Child-related welfare payments reduce the price for children and should therefore increase fertility. Many studies show a positive correlation between child-related payments and fertility but thus far, only in regard to child benefits paid indiscriminately to entire populations.[2] The fertility reaction of welfare recipients might differ from that of general populations for several reasons.

On the one hand, the fertility reaction of welfare recipients might be stronger because for this group, child-related payments present large amounts relative to the rest of their income. On the other hand, welfare recipients might react less strongly because an additional child may decrease the probability of exiting the welfare system which may a desire of many welfare recipients. Therefore, families on welfare might be less sensitive to short-term financial incentives. Despite the relevance of the question and the theoretical ambiguity of the answer, it is hard to identify empirically the fertility reaction of the specific subpopulation of welfare recipients because fertility research requires large data sets. Data sets on welfare recipients generally lack fertility information or are small. [3]

In this paper, we have exclusive access to an exceptional administrative data set on welfare recipients that contains fertility information about all families on welfare in Germany. The data are precise to one day and contain information on all births in families on welfare for a 12-year period. In addition, the data include information about maternal education, age, nationality, and some additional sociodemographic characteristics.

We use this data to estimate the fertility reaction of families receiving welfare to a very sudden and surprising German reform that effectively reduced the net household income of parents receiving welfare by 18 percent on average in the first year after the birth of a child. The reform, which came into effect in January, 1st 2011, changed the status of parental leave benefits (PLB) for all families receiving welfare payments and with at least one child younger than 14 months. Before the reform, families on welfare and a young child received 300 Euros PLB in addition to their regular welfare payments. After the reform date, the 300 Euros PLB were fully deductible from the regular welfare payments. Therefore, after the reform date the available income of families on welfare decreased

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[1] Since the 1990s, nearly one-half of the US have denied additional cash assistance to low-income mothers who have more children while receiving welfare. In 2018, in 17 of the US, some type of family-cap policy was active. (Urban Institute, 2018)

[2] For an overview of studies, see Table 1.

[3] For example, the U.S. Panel Study of Income Dynamics (PSID) includes only 458 unmarried women with children who received welfare benefits in their fertile age. (Ryan et al., 2006)

by 4,200 Euro (approximately 5,000 US-Dollar) during the first 14 months after the birth of a child in comparison to families who gave birth 14 month earlier.

To identify the reform effect, we use the unexpected introduction of the reform and compare the before and after probability that highly affected mothers receiving welfare payments will give birth.[4] We find a fertility reduction of 6.8 percent as an immediate response to the welfare reduction, indicating an income elasticity of fertility of 0.38[5], which is much smaller than what the literature finds for the reactions of overall populations to changes in general child benefits (cf. Table 1). We also find that the reform had a long-term impact, as the fertility rate remained at a lower level for at least five years after the reform. Furthermore, women who could not defer their fertility, those aged 38 to 45, reacted in a similar order of magnitude as the rest of the sample. In addition, we find that the spacing and timing of births did not increase. Both findings support the conclusion that the reform had an impact on completed fertility. These results are robust to several functional forms of the estimation, sample definitions, and placebo estimations.

This study contributes more robust evidence to the literature on the fertility effect of welfare payments than previous research for two reasons. First, the sudden 18 percent reduction in household income is larger than the monetary impact of reforms investigated in the previous literature.[6] Such a large reduction cannot be compensated by savings, transfers from relatives, or other sources. Therefore, we are certain that the reform reduced the available income by a substantial amount.

Second, we use administrative data obtained from the German Federal Employment Agency to analyze the fertility of welfare recipients. This data source contains detailed information on approximately 463,000 directly affected women and includes a panel of monthly observations over a 12-year period. Previous studies using microlevel data to analyze the nexus of welfare and fertility all used less than 10,000 units of observation. These small samples are problematic since childbirth is a relatively rare event in the life of an individual woman. Only large samples contain enough variation in the dichotomous variable “birth” or “no birth” to robustly identify effects. Therefore, the availability of data on an exceptionally large welfare reduction coupled with the large sample obtained by using administrative data provided an excellent opportunity for this study to reveal the fertility effect of welfare payments.

The rest of the paper is organized as follows: Section 2 summarizes the previous literature. Section 3 describes the relevant institutions. Section 4 describes the data. Section 5 presents the estimation strategy. Section 6 presents the main estimation results. Section 7 presents the robustness checks. Section 8 discusses the results and presents the conclusions.

2 Literature Review

Until the 1990s, the literature on the impact of child-related welfare payments on fertility focused on the US welfare system. In his comprehensive literature review, Moffit (1998) concludes that the

[4] We consider mothers who received welfare before the reform, still received it long after the reform and have at least one previous child.

[5] Elasticity is calculated by dividing the 6.8 percent fertility reduction by the 18 percent average household income reduction.

[6] We discuss the previous literature in detail in Section 2.

evidence points towards a mild positive effect of increased child-related welfare payments on fertility. The range of the point estimates for the effect varies greatly among studies. All results are highly sensitive to the methodology and the estimation sample of the respective studies. Additionally, the results of studies that consider variations among different states in the US and over time are affected by intracluster correlation, which are not adjusted for (cf. Bertrand et al. 2004).

The most recent literature about child-related welfare payments and fertility in the US analyzes the effect of the introduction of family caps in 23 states (e.g., Camasso et al. 1999, Argys et al. 2000, Jaganathan et al. 2004, Kearny 2004, Wallace 2009). These policies reduce or deny welfare payments for additional children who are born while a woman receives welfare payments. Two states, Arkansas and New Jersey, monitored the introduction of their Family Cap policies with randomized controlled trials. A number of studies analyze these trials, most notably Turturro et al. (1997) and Camasso et al. (1999). These studies find a fertility reduction for newly welfare-dependent women. However, as Loury (2000) points out, the results of these studies are difficult to interpret because multiple problems are apparent in the experimental design, such as selective attrition and selective assignment to treatment (cf. Kearny, 2004). The most recent publications on family caps by Kearny (2004) and Wallace (2009) rely on survey data obtained from a small number of potentially affected women. These studies use variations in the introduction of family caps across states and over time to identify the fertility effect of family caps. Neither study finds a significant effect.

Apart from studies on the US, the only studies about the nexus between welfare payments and the fertility of welfare and low-income families come from the UK. In 1999, the UK government enacted the Working Families' Tax Credit, which aimed to encourage low-income families with children to obtain employment, leading to an increase in support for children in low-income families by up to 50 percent. Thus far, two studies have examined the fertility effect of these reforms. Francesconi and van der Klaauw (2007) find no significant effect on the fertility of lone mothers. Brewer et al. (2012) find an increase in fertility of approximately 15 percent for women in couples. However, as these studies investigate a welfare to work program, the effects they identify are confounded by the fertility effect of the work incentives this program provides.

While research on the effect of welfare on fertility is inconclusive, the literature about general child-related public payments and their effect on fertility agrees that these transfers have a positive sizable effect on birth rates. Milligan (2005) examines the effect of a universal child benefit that was introduced in Quebec in 1988. He finds a significant positive effect using a difference-in-differences strategy which employs the rest of Canada as a control group. Cohen et al. (2013) consider variations over time and birth parity in the amount of child benefits for marginal children in Israel. They find a decrease in fertility of 9.6 percent as a reaction to a decrease in child benefits of approximately Dollar 34 a month.[7]

González (2013) and González and Trommlerová (2020) analyze the fertility effect of the introduction and cancellation of a one-time payment of 2.500 Euros to the parents of newborn children in Spain. The articles find an increase in birth rates between 3 percent and 6 percent in reaction to the policy due to a decrease in abortions and an increase in conceptions. The announcement of the cancellation of the policy led to a transitory increase in birth rates of 4 percent just before the cancellation was implemented that was driven by a short-term drop in abortions. The cancellation

[7] This amount adds up to Dollar 7.344 for each child, as the additional 34 Dollar is applied every month of the first 18 years of a child's life.

then led to a 6 percent drop in the birth rate. Azmat and Gonzalez (2010) also find a 5 percent increase in fertility by a reform in the Spanish income tax. In contrast, Tudor (2020) focus on Romania, finding that a substantial increase in maternity leave benefits led to a 4 percent increase in monthly live births. This increase in live births is due to a significant decrease in the probability of abortion, whereas there is no change in the conception rate. However, Tudor (2020) does not quantify the size of the maternity leave benefits increase which does not allow to estimate the income fertility elasticity.

Riphahn and Wiynck (2017) examine the effect of an increase in child benefits in Germany and find no effect on first births and an increase in fertility between 9.6 percent and 22.6 percent for the second births of high-income parents. Cygan-Rehm (2016) and Raute (2018) look at the introduction of PLB in Germany, which increased the cost of children for low-income families and decreased them for high-income families. Cygan-Rehm (2016) finds an effect of consistently lower birth rates for 5 years for low-income women with a previous birth. Raute estimates a fertility effect of 2.1 percent per 1,000 Euro change in the benefit. As Cygan-Rehm (2016) examines the fertility of low-income mothers in Germany, her paper is close to our paper. However, she investigates a reform which just affected previously employed mothers who likely have different income-fertility elasticities than welfare receiving women.

Table 1: Comparison of studies – General populations vs. welfare recipients

Author/Year **Welfare Recipients**	Method	Region	Income Change	Fertility Reaction	Elasticity
Moffit (1998)	Literature review	US	Varies	Varies	Varies
Kearny (2004)	DiD	US	~(-7.9%)	-	-
Wallace (2009)	DiD	US	~(-7.9%)	-	-
Brewer et al. (2012)	DiD	UK	12.0%	15.0%	1.3
General Populations					
Milligan (2005)	DiD	Quebec	4.3%	16.9%	3.9
González (2013)	Pre-post analysis	Spain	8.3%	6.0%	0.7
Cohen et al. (2013)	IV	Israel	-3.3%	-9.6%	2.9

Note: This table gives a very stylized overview. The conditions of the analyzed reforms varied between the studies. Many relevant studies are not included because their findings cannot be condensed into the framework of this table, e.g., because the studies do not state the income of the sample or the findings are too diverse to be displayed in this format. The studies are representative in the respect that studies about general populations find higher income elasticities of fertility than those about welfare recipients.

Source: Own calculations based on the respective studies.

3 Institutions

This study analyzes a reform that affects the interaction between welfare payments and PLB. The following chapter gives an overview of the features of these payments that are relevant for the analysis and of the reform, which altered this interaction.

3.1 Parental Leave Benefits

PLB are state transfers to the parents of young children. They are designed as a substitute for the forgone earnings of parents who take parental leave to care for their child. Parents of children up to the age of 14 months are eligible if they reduce or stop working. Each parent can receive PLB for at most 12 months. The combined number of months for both parents cannot exceed 14 months. The parents can receive PLB at separate times or jointly. Single parents can receive PLB for 14 months. The amount of PLB is calculated as approximately 67 percent of the respective parent's average net labor earnings in the 12 months before the child's birth. There are upper and lower bounds for the amount of PLB parents can receive. Eligible parents who did not work before the child's birth or had net labor earnings of less than 300 Euro received 300 Euro a month. Parents who earned more than 2.769 Euro receive 1.800 Euro a month (BEEG 2010).

3.2 Welfare Payments in Germany

In Germany, basic welfare payments for unemployed and marginally employed individuals are called unemployment benefits II. This program is the only available support for employable adults in Germany and take up is almost complete. In the following, we refer to these payments as welfare. For households without any employable adults, such as disabled people and retired individuals, other rules apply, but the payment amounts are the same.

Eligibility and the amount of welfare are determined at the household level.8 All households are eligible to receive welfare under two conditions. First, the adult members' wealth is below an age-dependent threshold, approximately 10.000 Euros per employable adult (SBG II §12, 2003). Second, the difference obtained by subtracting the deductible income from their welfare entitlement is positive (BMAS 2018; SBG II §7, 2003). This definition of eligibility implies that households that receive welfare and have no other income have the lowest income level possible in Germany. Anyone whose income is lower than this threshold can also apply for welfare.

Three factors (standard rate, rent, and additional needs) determine the amount of welfare entitlement: First, the standard rate differs by household position and is designed by legislature to represent the minimum consumer expenditures for basic necessities such as food, clothes, and transportation required for social existence (SBG II §20, 2003). Table A1 in Appendix A lists the standard rates by household position for 2010 and 2011. The second factor, rent, includes heating costs. However, only rent in the lower range of the local price level is covered. The last factor, additional needs, allows for special circumstances. The most relevant of these circumstances are single parenthood and pregnancy.[9]

The amount of welfare to be paid out to a household is determined by deducting other sources of household income from the entitlement. Capital income, most labor income and most state transfers are fully deducted. The only exempted state transfers are PLB. The 2011 reform ended this exemption.

[8] The legal definition of a household, with respect to welfare, includes people who live together and are either blood related or have a special care relation with each other. This special care relation can exist between spouses or between an adult and the spouse's children.

[9] Welfare payments also include compulsory health insurance.

Table 2 shows two examples of how welfare entitlements are calculated, how other sources of income are deducted, the amount of welfare paid out to families and the total amount that families have at the end of each month. We show these calculations for two different hypothetical families who received PLB in either December 2010 or January 2011. The first family consists of a single mother who does not work and has two children, and the second example is a couple with three children, where the father has net earnings of 1.000 Euro a month. Showing these examples at two different points in time emphasizes that while there were other small changes in the welfare system between December 2010 and January 2011, the deduction of PLB by far had the largest impact on families with young children. The impacts of a 5 Euro increase in the standard rate for adults and a slight decrease in the deduction of labor income are negligible in comparison to the impact of the reduction caused by the reform.

Table 2: Calculation of welfare payments (in Euro)

	Family 1		Family 2	
	Dec. 2010	**Jan. 2011**	**Dec. 2010**	**Jan. 2011**
Standard Rate				
Father	-	-	323	328
Mother	359	364	323	328
Child 1: Age 3 months	215	215	215	215
Child 2: Age 2 years	215	215	215	215
Child 3: Age 4 years	-	-	251	251
Cost of accomodation	400	400	600	600
Additional needs	129	131	-	-
Welfare entitlement	**1.318**	**1.325**	**1.927**	**1.937**
Other Income Sources				
Child benefits	368	368	558	558
Parental leave benefits	300	300	300	300
Labor income	-	-	1.000	1.000
Sum of other income sources	**668**	**668**	**1.858**	**1.858**
Deductions from welfare				
Child benefits	368	368	558	558
Parental leave benefits	-	300	-	300
Part of labor income	-	-	775	720
Total deductions	**368**	**668**	**1.333**	**1.578**
Welfare paid out	**950**	**657**	**594**	**359**
Sum of welfare and other income	**1.618**	**1.325**	**2.452**	**2.217**

Source: Family 1 consists of a single mother who does not work and has two children. Family 2 presents a couple with three children, where the father has net earnings of 1.000 Euro a month. Own calculations based on SBG II (2003) and BMAS (2018).

3.3 The Reform

On June 7, 2010, the German government announced austerity measures as a consequence of the financial and Euro crises. One of the measures changed the status of the 300 Euro minimum PLB provided to welfare recipients (Bundesregierung 2010). The reform took effect on January 1st, 2011. Before this date, 300 Euro was paid out monthly to welfare recipients regardless of whether they worked before childbirth. After January 1st, 2011, these payments were fully deducted from welfare if the parent who received PLB did not work before the birth of the child. This reform led to a cut in benefit receipt of up to 4.200 Euro (14 months·300 Euro) in comparison to welfare families who received PLB for 14 months. Figure 1 gives an overview how the reform affects annual welfare receipt of a single mother with two children depending of the birth month of the second child.

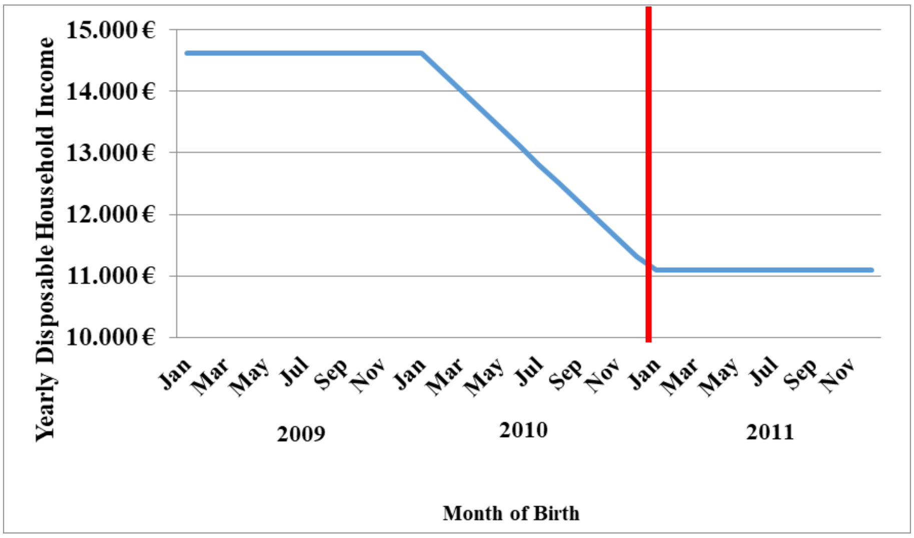
If a parent did work before the child's birth, the PLB is not fully deducted from the welfare payments. The amount of the PLB that corresponds to the average net earnings in the 12 months before the birth is exempt from deduction as long as it does not exceed 300 Euro (BEEG 2010). This new calculation means that if, e.g., a mother earned an average of 200 Euro per month in the year before giving birth, she would receive 300 Euro in PLB and 100 Euro would be deducted from the welfare payments. If a mother earned an average of 400 Euro per month, then she would receive 388 Euro in PLB, and everything exceeding 300 Euro – 88 Euro – would be deducted from her welfare payments.

The government decided to implement the reform during a weekend cabinet retreat that occurred on June 6, 2010, and announced it on June 7 (Bundesregierung 2010). It is highly unlikely that anyone anticipated the reform before this time because it was part of a package of austerity measures introduced in the wake of the economic crisis. The government coalition of Christian Democrats (CDU) and Liberal Democrats (FDP) had a stable majority in parliament; therefore, the fact that the cabinet decision would become legislation was not contested. Accordingly, all media coverage indicated that there was no question about whether the reform would be implemented (cf. Spieker 2010, SZ 2010). The government was so certain about passing this reform that they ordered the unemployment agency to send letters to welfare recipients informing them about the reduction before the reform was even passed in parliament (NTV 2010).

The austerity package that was affirmed on June 6 was the lead topic in all TV news and newspapers on June 7, with many media especially and explicitly discussing the PLB deduction for welfare recipients because it was the most controversial news item (Nitsche 2010). The massive media coverage on the reform that occurred after the cabinet meeting implies that affected families would have learned about this reform in early June 2010. While not everyone might have directly seen the news, it is plausible to assume that welfare recipients, especially those who planned to have a child or were already pregnant, would have heard about the reform.

Before June 2010, there was no indication that the interaction between welfare payments and PLB would be affected by the austerity measures. The government explicitly stated that the reform was implemented due to austerity concerns. No statements made by the government or media that the reform was intended to influence fertility or welfare recipients' work incentives. Therefore, anticipation effects leading to an adjustment in fertility behavior or the labor supply before June 2010 are not plausible.

Figure 1: Yearly disposable household income for a welfare receiving single mother with two children depending on the birth month of the second child



Note: The graph shows the yearly disposable household income for a welfare receiving single mother with two children depending on the birth month of the second child. The red solid vertical line marks January 2011, the first calendar month when the parental leave benefits were deducted from welfare. The graph depicts the case that the mother were eligible to 14 months of PLB after birth with the amount of 300 Euros. The disposable income corresponds to the figures in Table 2 minus cost for accommodation.

Source: Own calculations based on SGB II (2003) and BMAS (2018) and BEEG (2010).

4 Data

This analysis draws from exclusive access to very accurately tracked administrative data from the Federal Employment Agency of Germany.[10] These data provide information about household composition, fertility and the labor market histories of all households in Germany receiving welfare. The data cover specific timespans that are accurate to one day, and a new timespan starts every time the value of any of the stored variables for a person changes. Between January 2005 and April 2017, 7.4 million women in the fertile age range – between 18 and 45 years old – lived in the observed households.

We use a 50 percent random sample to create a panel of monthly observations between January 2005 and December 2016 for every woman in the data set born between 1959 and 1998. The women are present in the data as long as she was between 18 and 45 years old. Therefore, the panel is not balanced as women enter and leave the panel. Overall, the panel contains almost 400 million observations.

To compile our estimation sample, we apply three further sample restrictions to the data. The estimation sample contains only women who, first, received welfare in January 2010 or earlier – one year before the reform –, second, either continued to receive welfare or received it again in December 2016 or later, and third, had at least one child. We impose restriction 1) – women who received

[10] The data sets are Leistungshistorik Grundsicherung (LHG) and Integrierte Erwerbsbio-graphien (IEB). The LHG tracks the benefits received by households receiving welfare and the IEB is a register of all dependent employment in Germany. We use version 13.00.00 of the LHG and version 09.00.00 of the IEB.

welfare for the first time in January 2010 or earlier – because we expect women who received welfare for the first time after the reform was announced to react weaker or not at all to the reform. Since the reform affects women only while they receive welfare, these women's fertility incentives would not abruptly change due to the introduction of the reform but rather at the time they first started receiving welfare, and thus, this change cannot be captured with our estimation approach. We choose January 2010 as the relevant month because it is far enough before the announcement of the reform making it unlikely that the reform itself affects whether or not a family is part of the sample. We show in Section 7 that the results are not affected by the point in time chosen for this condition.

We impose restriction 2) – to include only women who received welfare in December 2016 or later – because the original data track a woman's fertility only until she receives welfare for the last time. As the dependent variable is thus missing after this point in time, we have no observations for women once they no longer receive welfare. If we kept their previous observations, selective attrition, which is correlated to the reform, would be introduced. The suspected reform effect is a reduction in fertility, and having another child is highly correlated to remaining dependent on welfare receipts. The main sample includes women we can observe until December 2016. In the robustness checks describe in Section 7, we show that the month we choose for this restriction does not change the results.

Finally, we impose restriction 3) - women who had at least one child - because the birth of the first child, rather than subsequent births, is often the initial reason for receiving welfare. This renders the strategy of conditioning on welfare receipt before a certain month infeasible for first births and therefore we cannot estimate the precise effect of the reform on first births. Appendix B explains the problem in further detail and also presents suggestive results of the reform on first births.

After applying the restrictions, our sample includes 463,000 women, for whom the data set contains approximately 46 million monthly observations. Between 2005 and 2016, these women gave birth to 285,000 children, which constitutes an average annual birth rate of 7.4 percent. The women in the main estimation sample received welfare approximately 80 percent of the time. Many of the women in the unrestricted sample received welfare for only short periods. On average, the excluded women received welfare for 31 percent of the observed time. Approximately 1,728,000 children were born to women while they received welfare in Germany between 2005 and 2016, and of these, 450,000 are included in our sample.[11] Table 3 gives an overview of additional characteristics: education level[12], federal state, nationality, marital status and cohort. Table A2 summarizes the number of children and the number of births by parity for these women.

[11] In our 50 percent estimation sample (the sample after imposing the restrictions), women receiving welfare payments had 224.789 births; thus, we doubled this number: $224.789 \cdot \left(\frac{100}{50}\right) \approx 450.000$.

[12] Education is measured by two indicators: Highest secondary school degree and a variable indicating whether a woman had a vocational degree, tertiary degree or none. For both variables, we consider the highest degree ever obtained by a woman over the whole time we observe her. We do not use the vocational training/tertiary degree in the estimations because it may be endogenous to the reform.

Table 3: Descriptive statistics: Women's characteristics

	N	%		N	%
Secondary School Degree			**Nationality grouped by Cultural-Geographic Region**		
No Degree	38.733	8.36	Germany	343.933	74.24
Lower Secondary	211.315	45.61	Turkey	38.650	8.34
Middle Secondary	147.559	31.85	East and Southeast Europe	34.251	7.39
Higher Secondary	60.102	12.97	North and West Europe	10.205	2.20
Missing	5.554	1.20	Arabic Countries	13.743	2.97
Total	463.263	100	Non-Arabic Africa	5.583	1.21
Apprenticeship/Tertiary Degree			Latin America and Carribean	1.693	0.37
No Degree	294.319	63.53	US,CAN,AUS,NZL	221	0.05
Apprenticeship	160.831	34.72	Central Asia, Caucasus	7.210	1.56
University Degree	5.772	1.25	Rest of Asia and Oceania	7.705	1.66
Missing	2.341	0.51	Unknown	69	0.01
Total	463.263	100	Missing	0	0.00
Marital Status at last Observation			Total	463.263	100
Unmarried	147.831	31.91	**Federal State of Residence at last Observation**		
Married, together	149.901	32.36	Schleswig-Holstein	17.220	3.72
Married, seperated	37.727	8.14	Hamburg	13.485	2.91
Divorced	61.445	13.26	Lower Saxony	45.644	9.85
Widowed	3.371	0.73	Bremen	6.977	1.51
Missing	62.988	13.60	North Rhine-Westphalia	124.961	26.97
Total	463.263	100	Hesse	30.686	6.62
Cohort			Rhineland-Palatinate	16.682	3.60
1959-1963	45.035	9.72	Baden-Württemberg	30.663	6.62
1964-1968	63.595	13.73	Bavaria	29.239	6.31
1969-1973	70.792	15.28	Saarland	5.852	1.26
1974-1978	75.870	16.38	Berlin	42.006	9.07
1979-1983	84.013	18.14	Brandenburg	19.690	4.25
1984-1988	73.473	15.86	Mecklenburg-Vorpommern	14.826	3.20
1989-1993	40.579	8.76	Saxony	29.040	6.27
1994-1998	9.906	2.14	Saxony-Anhalt	22.288	4.81
Missing	0	0.00	Thuringia	13.949	3.01
Total	463.263	100	Missing	55	0.01
			Total	463.263	100

Note: The numbers refer to the main estimation sample of women who have at least one previous child and received welfare in January 2010 or earlier and in December 2016 or later. The categories secondary school degree and apprenticeship/tertiary degree refer to the highest degree we find in the data for each woman.

Source: Own calculations based on LHG data.

5 Empirical Strategy and Threats to Identification

5.1 Empirical Strategy

The reform that deducts PLB from welfare payments provides a natural quasi-experiment in which the reform's fertility effect can be identified with a linear probability model. We assume that there will be a sudden and sizeable drop in the sample's birth rate because the reform was completely unanticipated before its announcement and implemented only 6 months later and the reduction in relative household income is exceptionally large and directly tied to fertility. To estimate the magnitude of this reduction, we estimate the following model:

$$birth_{it} = \alpha + \beta \cdot post_{it} + \gamma_1 m_{it} + \gamma_2 m_{it}^2 + \gamma_3 m_{it} \cdot post_{it} + \gamma_4 m_{it}^2 \cdot post_{it} + \sum_{c=2}^{12} \mu_c month_{it} + \lambda X_{it} + \varepsilon_{it} \qquad (1)$$

where birth is a dummy variable indicating that woman i gave birth to a child in month *t*. post is a treatment indicator that is set to 0 before the reform could have had an effect on the birth rate - from January 2005 und December 2010 – and 1 afterwards – starting in April 2011. The model looks very similar to a regression discontinuity design (RDD). It is not a classical RDD model though, because the units of observation on both sides of the cut-off are the same. In an RDD model the cut-off divides the sample into treatment and control group.

We exclude observations from January 2011 to March 2011 in the baseline specification, as in these months the welfare reduction can only influence the birth rate via changes in abortions.[13] [14]From April 2011 onwards, we can observe the full effect of the reform, which is impacted not only by more abortions but also by an increased use of contraceptives or higher abstinence. *m* is a trend variable that is included in the baseline specification in both linear and quadratic form and interacted with the post-dummy. *month* is a set of 11 month-of-year dummies that control for seasonality. *X* is a vector of control variables including dummies for the mother's age in four-year steps, her nationality and the federal state of residence. The main coefficient of interest from this regression is β, the coefficient of the postreform dummy.

5.2 Threats to Identification

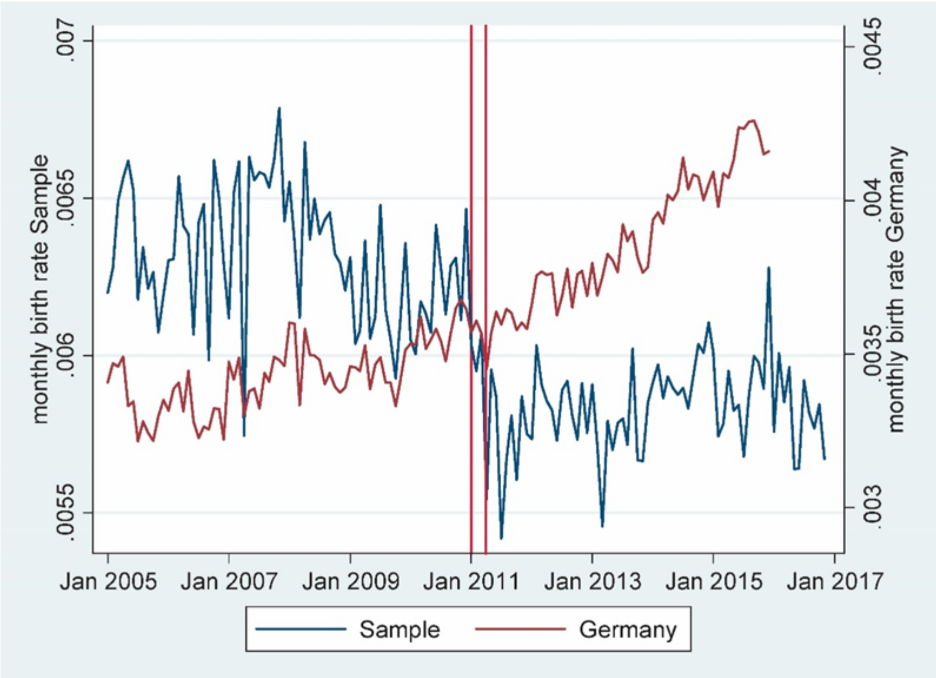
The greatest threat to the identification of the true fertility effect in our analysis is sample selection. The reform reduced welfare payments for the parents of young children. Parents who did not receive welfare when their child was younger than 15 months old were not affected by the reform. Therefore, women and couples who planned to have a child had an incentive to postpone fertility until they entered a period when they did not receive welfare payments; this is not an unreasonable expectation based on the benign labor market conditions at the time of the reform and afterwards. Such fertility postponement would avoid the income reduction caused by the reform. Our data provide fertility information about a woman only until the last month she received welfare. Hence, sample selection may influence our results – women may have deferred fertility as a reaction to the reform. Women who chose not to have another child would stop receiving welfare benefits earlier, and thus, the probability of leaving the sample would increase. In the results section, we test for the influence of sample selection by estimating the main equation using subsamples divided by education and the number of previous children. If sample selection is the driver of the structural break, we should find weaker or no structural breaks for groups at high risk of continuously receiving welfare payments.

[13] We cannot estimate the effect of the reform on abortions because there is no data on abortions that specifies whether the woman received welfare payments. We could use the drop in fertility in the time period from January 2011 to March 2011 as proxy for the effect of the reform on abortion, However, as we have just three months of observation period this analysis may give noisy results.

[14] We present results including the months January 2011 to March 2011 in Table 5, Column 6.

Another potential threat to identification is contemporaneous reforms that might have influenced or caused the effect we identify. None of the law changes in 2010 and 2011 are likely candidates to generate significant bias, as no reforms negatively influenced the German fertility rate at the time in question. Figure 2 compares the seasonality-adjusted monthly birth rate in Germany to that of the main sample.[15] The result shows that before the reform, the birth rate of our sample remains continuously higher without a clear upward or downward trend over time. Then, a break occurs around the time of the reform. The general German birth rate trends upward without any noted changes around 2010 or 2011. Therefore, if any other policy change caused the effect we find, it would have been a change that specifically affected the sample group of welfare recipients. There were no such changes in the relevant period.

Figure 2: Comparison of birth rates: Sample vs. Overall German population



Note: Please note that the monthly birth rate of the sample is age adjusted. Since the sample ages over time, the unadjusted birth rate has a strong negative trend at all times before and after the reform. The German birth rate shown here is not age adjusted because we do not have adequate data for the age adjustment. Overall, the average age of German 18- to 45-year-old women is increasing with time. Thus, an age adjustment would lead to a steeper increase in the birth rate over time. The birth rates of the sample and the German population are seasonally adjusted. The first red vertical line marks January 2011, the first month in which the reform announcement could affect the birth rate due to abortions. The second red vertical line marks April 2011, the first month in which the reform announcement could affect the birth rate due to increased use of contraceptives, higher abstinence, or more abortions.

Source: Own calculations based on LHG and IEB data and the Federal Statistical Office of Germany.

6 Results

This section presents the results of estimating equation 1 for the full main sample followed by estimates for subsamples separated by parity and by the women's level of secondary education.

[15] The German birth rate depicted in the figure also contains first births, because we have no data about parity specific birth rates for Germany as a hole.

Showing the results for different subsamples reveals the degree of heterogeneity in the reform effect. Furthermore, the use of these subsamples allows for conclusions to be drawn about the degree to which selection out of the sample drives the effect because the displayed groups differ greatly in terms of their probability of receiving welfare on a continuous basis. If the groups who receive welfare most persistently react as strongly to the reform as those who are likely to stop receiving welfare payments, then most likely, the effect we identify is driven by an actual reduction in fertility rather than sample selection.

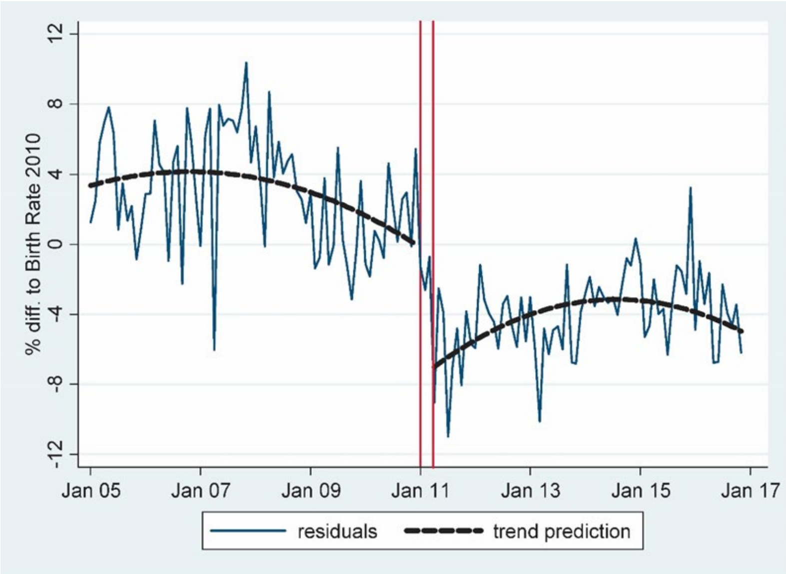
Figure 3 shows a residual plot of the monthly birth rate of the full main sample. The residuals are obtained from a regression that controls for the month of the year and the woman's age, nationality and federal state. The dependent variable is depicted as a residual plot rather than showing raw data because the monthly birth rates have considerable seasonal variations, and the aging of our sample over time introduces a trend, which obscures the reform effect if we do not control for age. Furthermore, the residuals are rescaled to represent a relative deviation from the average birth rate in 2010 because the residuals are miniscule numbers that are not intuitively interpretable.[16]

In 2010, the 349,607 women in the sample at that time gave birth to 25,612 babies, which constitutes an annual birth rate of 7.6 percent. The dashed line shows the quadratic pre- and postreform trends as calculated by equation 1. The vertical red lines mark January 2011, the month that the announcement of the reform could first affect the birth rate via an increased abortion rate, and April 2011, the month increased contraceptive measures could take full effect.

The graph shows that the birth rate dropped sharply in January 2011, followed by a further rapid decline until June 2011. While the birth rate varies substantially from month to month before the reform, the drop in 2011 leads to the lowest birth rate since the beginning of the observation period and remains at a lower level despite an upward trend. This pattern is suggestive evidence that the reform permanently lowered fertility. The residual plot starts dropping in January 2011. This early drop could either be coincidental, as it lies within the usual range of variation seen prior to the reform, or it might be influenced by an increase in abortions starting in June 2010. We split the main sample by birth parity and education. Figures A1–A6 in Appendix A show the residual plots for the subsamples, and all of these plots confirm the pattern displayed in Figure 2.

[16] If, instead of using the 2010 average, we took the average birth rate of the entire prereform period as a reference point, this would be a misrepresentation. β identifies the immediate drop between December 2010 and April 2011 rather than the mean difference between 2005-2010 and 2011-2016. Using the value from December 2010 would be a misrepresentation as well because it would base the scaling on the value of a month of year associated with a low birth rate.

Figure 3: Birth rate residuals relative to the birth rate for 2010



Note: The residuals are obtained from a linear probability model in which the birth dummy variable is regressed on age, federal state, month of year and world region of origin. The sample is the main estimation sample, meaning women who have at least one child and received welfare before January 2010 and after December 2016. The blue line shows the mean of residuals for each month. These means are rescaled to represent how far they deviate from the birth rate for 2010, which was 7.6 percent in yearly terms and 0.63 percent in monthly terms. The first red vertical line marks January 2011, the first month in which the reform announcement could affect the birth rate due to abortions. The second red vertical line marks April 2011, the first month in which the reform announcement could affect the birth rate due to increased use of contraceptives, higher abstinence, or more abortions. The black dashed line shows the quadratic trend prediction as calculated by equation 1.

Source: Own calculations based on LHG and IEB data.

Table 4 shows the results for estimating equation 1 first for the full sample, then by birth parity and education. As in all estimation tables that follow, the dependent variable is rescaled by dividing it by the average monthly birth rate for 2010. This procedure simplifies the interpretation of the reform dummy coefficient, as it is now directly represents the relative percentage change in the birth rate as a reaction to the reform.

Column 1 shows the estimates for the entire sample. These results demonstrate that the birth rate declines by 6.76 percent as a reaction to the reform. Columns 2 and 3 show the results separated by birth parity. The sample in Column 2 contains all mothers that have one child, and the sample in Column 3 contains all mothers with two or more children. The birth rate for mothers with one previous child, thus the birth rate for second children, dropped by 4.9 percent compared to the 2010 level. The birth rate for third- and higher-order children dropped by 8.69 percent. This greater responsiveness of higher-order birth fertility is in line with evidence presented in previous research (cf. Brewer et al. 2012, Laroque and Salanie 2014, Milligan 2005). This increased responsiveness could be due to the preference of women with one child to have at least one more child (cf. Berrington 2004). The desire to have additional children is less quantified and therefore more easily influenced by external determinants, such as money.

Columns 4 to 7 show the results for women with different levels of secondary education. By far the largest and most statistically significant effect is the one for women without a secondary school degree (Column 4). With a reduction in the birth rate of 12.99 percent, the reaction of these women is approximately twice as strong as the reaction for the main sample. This result is the most statistically significant of all subsamples, even though it is based on the fewest observations and the smallest number of individuals. The point estimates for the other educational subgroups are all roughly on the same order of magnitude, with a reduction of 6 percent.

Overall, the reform effect is largest and most statistically significant for women with more than one previous child and low levels of education. The women in these groups receive welfare most persistently. Of the women who first received welfare before January 2010 and had no school degree, 43 percent continued to receive or again received welfare between December 2016 and April 2017. For the women with a lower, middle and higher secondary school degree, this rate is 32 percent, 22 percent and 19 percent, respectively. Figure A7 shows how women with two or more children are consistently more likely to receive welfare payments than women with only one child. Figure A8 shows the same pattern for the subsamples split by education. The lower a woman's secondary school degree is, the more persistently she receives welfare. This result is not surprising, as it is increasingly difficult for women to find a job the more children they have and the worse their education is. Furthermore, households with more children receive higher welfare entitlement amounts, and therefore, a larger expansion of labor supply would be required for these individuals to become independent of welfare payments. The fact that these groups, who are less likely to stop receiving welfare payments, react strongest is evidence that the reform effect we find is mostly driven by an actual reduction in fertility rather than sample selection.

Table 4: Main estimation results

	Full Sample	Second Children	Third and higher Parities	No Sec. School Degree	Lower Sec. School Degree	Middle Sec. School Degree	Higher Sec. School Degree
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reform Dummy	-6.762***	-4.909***	-8.689***	-12.986***	-5.231***	-6.58***	-5.977*
	(1.121)	(1.654)	(1.611)	(3.012)	(1.657)	(2.183)	(3.461)
Observations	45.966.533	18.939.651	27.026.882	4.061.654	21.154.196	14.462.735	5.700.001
Units of Observation	463.263	314.250	293.105	38.733	211.315	147.559	60.102
Birth Rate 2010	7,60%	9,35%	6,32%	11,15%	7,63%	6,52%	6,55%
Birth Rate 2011/12	6,96%	8,76%	5,75%	9,54%	7,17%	5,97%	6,10%
Coefficient	-6,762	-4,909	-8,689	-12,986	-5,231	-6,580	-5,977
S.E.	0,011	0,017	0,016	0,030	0,017	0,022	0,035
p-Value	0,000	0,003	0,000	0	0,002	0,003	0,084
Average Monthly Birthrate 2010	0,00633	0,00779	0,00527	0,00929	0,00636	0,00543	0,00546
Average Yearly BR 2010	0,076	0,094	0,063	0,111	0,076	0,065	0,065
Av. Monthly BR year after Ref.	0,006	0,007	0,005	0,008	0,006	0,005	0,005
Av. Yearly BR year after Ref.	0,070	0,088	0,057	0,095	0,072	0,060	0,061

Note: Robust SE, clustered by woman, in parentheses. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable in all models is birth in t. The two bottom rows list each subsample's yearly birth rate in 2010, the year before the reform, and April 2011 to March 2012, the year after the reform developed its full effect on the birth rate. The coefficients of the control variables are not displayed. They are sets of dummies for mother's age in steps of 4 years, federal state, month of year, world region of origin and linear and quadratic post- and pretrends. All coefficients are rescaled to represent a relative percentage change in comparison to the average yearly birth rate 2010.

Source: Own calculations based on LHG and IEB data.

7 Robustness

Table 5 Panel A shows the results of several robustness tests. Columns 1 to 5 report the results for estimating equation 1 using different functional forms, including different polynomials of the trend variables and excluding the separate post-trend variables. Column 6 includes the months January to March in the estimation as post-reform observations. The coefficient is one percent smaller than the main coefficient indicating that abortions conducted directly after the announcement of the reform present a smaller channel for the fertility reduction. Column 7 shows the results for estimating equation 1 without the control variables, and Column 8 includes individual fixed effects. Most of the changes have only a minute effect on the estimated effect size or its statistical significance. The only exception is adding a quartic trend, which leads to an estimate of 15.8 percent. With four polynomial terms of the post and the pretrend variables, the likely explanation for this deviation is that this model is overfitted.

Column 9 of Table 5 Panel A shows the result for the subsample of all women who never lived in a household with an aggregated labor income of more than 300 Euro a month. We test this specification because it is possible that families with a low labor income who also received welfare cross the threshold for receiving welfare after January 1st, 2011. Crossing the threshold happens because PLB are deducted from welfare payments and treated therefore similar as labor income. These families would no longer qualify for welfare payments, possibly permanently, the month after childbirth. This threshold crossing, if it existed, would remove these families from the analysis sample. If this sample reduction caused our effects on fertility , then we would not expect an effect for women from households who never earned income, since those families would definitely not surpass the income barrier because of the reform. As the reform dummy for this subsample is even more negative and graphical evidence (cf. Figure A9) supports this finding, this mechanic halt in welfare payments cannot explain the reform effect.

Although the graphical evidence is convincing that a persistent drop in the birth rate only occurred after the reform was implemented and did not occur at any other point over the observation period, the high levels of statistical significance of the coefficient for the reform dummy might be due to the extraordinarily large sample size. To test for this issue, we estimate the baseline model with placebo reform dummies, pretending the reform occurred at different points in time.

Table 5: Robustness checks – Functional form and miscellaneous

	Linear Trend	Quadratic Trend	Cubic Trend	Quartic Trend	No Trend Interactions	Include Jan.-Mar. 2011	No Controls	Women F.E.	Exclude if Inc. ever >300€
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Reform Dummy	-6.5*** (0.684)	-6.762*** (1.121)	-6.333*** (1.685)	-15.803*** (2.375)	-6.582*** (0.683)	-5.745*** (1.026)	-7.022*** (1.118)	-6.148*** (1.127)	-10.672*** (2.095)
N	45.966.533	45.966.533	45.966.533	45.966.533	45.966.533	46.982.505	45.966.533	45.966.533	12.760.844
Units of Obs.	463.263	463.263	463.263	463.263	463.263	463.263	463.263	463.263	160.049
Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quadradic Trend	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cubic Trend	No	No	Yes	Yes	No	No	No	No	No
Quartic Trend	No	No	No	Yes	No	No	No	No	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes
Trend Interactions	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Women Fixed Effects	No	No	No	No	No	No	No	Yes	No

Note: Robust SE, clustered by woman, in parentheses. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable in all models is birth in t. The estimation sample in all columns except Column 9 is the full main sample of women with at least one child and on welfare before January 2010 and after December 2016. The coefficients of the control variables are not displayed. They are sets of dummies for mother's age in steps of 4 years, federal state, month of year, world region of origin and linear and quadratic post- and pre trends (except if otherwise indicated – Column 7). All coefficients are rescaled to represent a relative percentage change in comparison to the average yearly birth rate 2010.

Source: Own calculations based on LHG and IEB data.

Table 6 reports the results for placebo reforms at different points in time from two years before the actual reform until two years after it in steps of six months. The coefficient for the actual reform (Column 5) has by far the largest absolute value and is most significant. Most of the coefficients for the placebo reforms are small and have very large standard errors. Only the coefficient for the placebo reform in July 2011 is highly statistically significant, and the point estimate is approximately one-half as large as that of the actual reform. Therefore, this result does not threaten the validity of the main results. This coefficient is statistically significant because it captures part of the actual reform effect.

Table 6: Robustness checks – Placebo test

	Reform Jan. 2009	Reform Jul. 2009	Reform Jan. 2010	Reform Jul. 2010	Reform Jan. 2011	Reform Jul. 2011	Reform Jan. 2012	Reform Jul. 2012	Reform Jan. 2013
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Reform Dummy	-2.349*	0.065	0.836	-1.541	-6.762***	-3.052***	0.716	0.267	1.377
	(1.275)	(1.221)	(1.187)	(1.152)	(1.121)	(1.133)	(1.192)	(1.225)	(1.253)
N	45.995.860	45.985.413	45.975.326	45.969.931	45.966.533	45.965.664	45.964.931	45.965.711	45.965.461
Units of Obs.	463.263	463.263	463.263	463.263	463.263	463.263	463.263	463.263	463.263

Note: Robust SE, clustered by woman, in parentheses. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable and control variables are identical to the baseline model (cf. Table 4). Small deviations in the number of observations come from the shifting of the placebo reforms. In the original model, observations from Jan.-Mar 2011 are dropped. Therefore, for all placebo reforms, we also drop the three following months. The scaling in each month is analogous to the baseline specification, except that for scaling by the birth rate of 2010, we scale by the birth rate in the year before each respective placebo reform.

Source: Own calculations based on LHG and IEB data.

Table 7 Panel A shows the results for estimating equation 1 for samples for which the condition of the first month of receiving welfare payments varies. This approach tests whether the month we select for this condition influences the reform effect. In the baseline specification, women in the sample received welfare in January 2010 or earlier. In Column 1, this condition is shifted to January 2005; in Column 2, it is shifted to January 2006, and so on for each year until 2010 (Column 6, baseline). The point estimate stays extremely stable across these shifts. The standard error becomes smaller as the sample size increases. The increase in sample size occurs due to increasingly loosening the condition by shifting the relevant month forward in time. Furthermore, Figure A.10 in Appendix A shows that the course of the birth rate residuals is very similar for the different samples, indicating that the results are not driven by women who entered the sample at a particular time.

Table 7 Panel B shows the results for shifting the condition of a woman's final observation. This approach tests the influence of the month we choose for this condition on the estimate of the reform effect. In the baseline specification, we choose December 2016. Column 1 shows the results for shifting this condition to December 2011; Column 2 shows the results for shifting to December 2012, and so on. The point estimate is negative and highly significant over all specifications but varies between reductions of 4.47 percent and 9.46 percent with the baseline estimate, and 6.76 percent roughly represents the median. Figure A.11 in Appendix A shows the residual plots for the samples with the different restrictions. The course of the residual plot and therefore the birth rate over time remain very similar across the different samples.

Table 7: Robustness checks – Shifting the conditions of the first and last welfare receipt

Panel A - Shift Condition of First UB II Receipt						
	first obs. 2005	first obs. 2006	first obs. 2007	first obs. 2008	first obs. 2009	first obs. 2010
	(1)	(2)	(3)	(4)	(5)	(6)
Reform Dummy	-6.405***	-6.554***	-6.256***	-7.012***	-6.728***	-6.762***
	(1.825)	(1.359)	(1.258)	(1.194)	(1.153)	(1.121)
N	21.027.040	33.867.457	38.490.311	41.458.319	43.818.061	45.966.533
Units of Obs.	209.312	333.688	381.019	412.472	438.373	463.263
Panel B - Shift Condition of Last UB II Receipt						
	last obs. 2011	last obs. 2012	last obs. 2013	last obs. 2014	last obs. 2015	last obs. 2016
	(1)	(2)	(3)	(4)	(5)	(6)
Reform Dummy	-9.46***	-7.119***	-5.851***	-4.469***	-5.095***	-6.762***
	(2.001)	(1.246)	(1.062)	(1.023)	(1.101)	(1.121)
N	61.821.724	63.911.560	66.515.950	62.055.687	49.887.519	45.966.533
Units of Obs.	976.619	899.329	847.396	723.958	538.411	463.263

Note: Robust SE, clustered by woman, in parentheses. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable and control variables are identical to the baseline model (cf. Table 4). All coefficients are rescaled to represent a relative percentage change in comparison to the yearly birth rate 2010. This birth rate is determined for each sample separately, e.g., the yearly birth rate for women who received welfare for the first time in January 2009 or earlier is 7.5 percent, where it is 7.6 percent for the baseline sample.
Source: Own calculations based on LHG and IEB data.

Table 8 Panel A reports the results of a bandwidth test. Column 1 is the result for a bandwidth of 2 years, 1 year before and 1 year after the reform; Column 2 for a bandwidth of 4 years, 2 years before and 2 years after the reform, and so on until the whole observation period is included in Column 6.[17] All estimates of the reform dummy in the bandwidth tests are statistically highly significant and negative and have a size comparable to that of the baseline.

Table 8 Panel B shows the estimates when the main sample is split by age groups. We bundle the age groups 18 to 21 and 22 to 25 because separately, they are too small for statistically significant estimations, and the residual plots move too erratically. The two groups have few observations because the precondition to be in the sample is having at least one child, which is less common among young women. Similarly, the age groups 38 to 41 and 42 to 45 are grouped together because births are such a rare event for them that outlier months render their separated residual graphs difficult to interpret. Again, all estimates are negative, statistically significant and of a similar order of magnitude. The estimates become less precise with age, though, as births become increasingly rare. The graphical evidence displayed in Figures A12 to A16 in Appendix A supports the finding that the reform effect occurs for all age groups. Additionally, women aged 38 to 45 and therefore unable to postpone fertility reduced this effect. This evidence shows that the reform also affected completed fertility. We further investigate this assumption in Appendix C , which tests whether the reform increased the age at which women had children or the spacing between children. Such an

[17] Apart from changing the bandwidth, the results reported here are estimated with a slightly different functional form. Here, the quadratic pre- and posttrends are excluded because including them together with the month of year fixed effects leads to multicollinearity for bandwidths up to 6 years. The results for the estimation including the quadratic trends are reported in Table A3 in Appendix A.

increase could indicate that the reduction in fertility is caused by postponement rather than a permanent reduction in fertility. Appendix C finds suggestive evidence against postponement and for a reduction in fertility.

Table 8: Robustness checks – Bandwidth sensitivity and age groups

Panel A - Bandwidth Sensitivity						
	2 Years	4 Years	6 Years	8 Years	10 Years	12 Years
	(1)	(2)	(3)	(4)	(5)	(6)
Reform Dummy	-9.375***	-8.885***	-5.557***	-6***	-6.71***	-6.5***
	(3.465)	(1.462)	(1.067)	(0.88)	(0.764)	(0.684)
N	7.096.820	15.143.717	23.083.735	30.881.198	38.517.530	45.966.533
Units of Obs.	362.900	386.405	407.817	428.314	446.985	463.263
Panel B - Age Groups						
	Age 18-25	Age 26-29	Age 30-33	Age 34-37	Age 38-45	-
	(1)	(2)	(3)	(4)	(5)	
Reform Dummy	-6.963***	-8.737***	-5.594**	-5.142*	-7.453*	-
	(2.104)	(2.165)	(2.451)	(3.031)	(4.015)	
N	7.094.110	7.608.332	8.307.799	8.197.989	14.758.303	-
Units of Obs.	171.399	221.152	238.550	236.191	255.292	-
Birth Rate 2010	13,21%	12,12%	9,09%	6,31%	1,95%	-
Birth Rate 2011/12	12,36%	11,13%	8,52%	5,93%	1,77%	-

Note: Robust SE, clustered by woman, in parentheses. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable, control variables and scaling are identical to the baseline model (cf. Table 4). The two bottom lines of Panel B list each of that Panel's subsamples yearly birth rate in 2010, the year before the reform, and April 2011 to March 2012, the year after the reform developed its full effect on the birth rate.

Source: Own calculations based on LHG and IEB data.

8 Discussion and Conclusion

This study investigates the fertility effect of a reform of the German welfare system that deducted PLB from welfare payments. The reform reduced the household income of affected welfare recipients by 18 percent on average. We find that the reform reduced fertility by 6.8 percent for women with at least one child who were on welfare before and after the reform. The availability of large administrative data sets providing detailed information about welfare recipients coupled with a large reduction in relative income directly related to marginal fertility supported an analysis of the effect of welfare on fertility. We obtain robust evidence from regressions and graphical evidence confirming that welfare recipients' fertility decisions are influenced by financial incentives.

The course of the fertility reaction – a sudden drop at almost the first possible moment, with a slight recovery afterwards – suggests that women on welfare were relatively well informed about the reform. Otherwise, the drop would have come later and more gradually.

Graphical evidence suggests that the reform influenced fertility in the short and long run. The birth rate remains at a decreased level for years after the reform. The consistent decrease in the sample's birth rate suggests that the negative effect also influences women's completed fertility. It is plausible to assume that the reform is part of the reason for continuous decrease in fertility in the sample, but we cannot say to what degree because we have no credible way of gauging the birth rate for a scenario in which the reform did not take place.

Separate analyses of birth timing and spacing show that women who had children after the reform were not significantly older and did not wait a longer time between births because of the reform. The sample was separated into age groups, and the result shows that the effect is similar for women of all ages within the fertile age range. These findings are further suggestive evidence of a negative effect on the completed fertility of the affected women.

Generally, this analysis supports those previous contributions to the literature on the nexus of welfare and fertility that find a positive effect (e.g., Brewer et al. 2012, Turturro et al. 1997, Camasso et al. 1999). This study might also contribute to explaining the inconclusiveness of other studies on this topic. While our findings are very statistically significant because of the extraordinarily large sample of directly affected women, the effect size is small compared to those found by studies about general populations – 6.8 percent fewer births as a reaction to household income reduction of 18 percent, with an associated income elasticity of 0.38. For example, Milligan (2005) estimates a 16.9 percent increase in fertility as a response to a 4.3 percent increase in income (elasticity of 3.93), Gonzáles (2013) finds a 6 percent increase in fertility as a response to an 8.3 percent increase in income (elasticity of 0.72) and Cohen et al. (2013) estimate a 9.6 percent decrease in fertility due to a 3.3 percent decrease in income (elasticity of 2.91). Thus, while a fertility response is found among welfare recipients, it seems to be weaker than that of general populations. The smaller an effect is, the more statistical power required to detect it. Accordingly, the scarcity of large data sets focused on welfare recipients might explain the inconclusiveness of former studies.

Our findings provide evidence that welfare recipients react less strongly to financial fertility incentives than general populations. The evidence from this study is important for policy makers because it speaks against the widely held assumption that the fertility patterns of welfare recipients might be excessively motivated by financial concerns. The opposite seems to be the case. This study does not determine the optimal level of child-related welfare benefits; however it does show that concerns about an excessive fertility reaction should not factor into the deliberations of setting such benefit levels.

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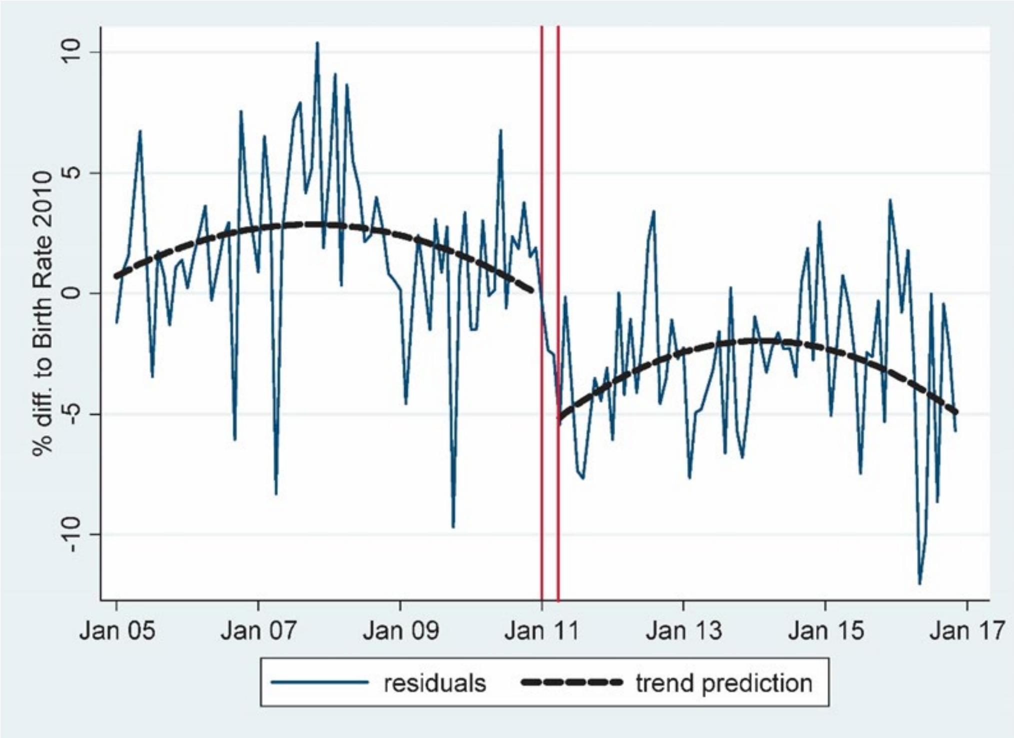
Appendix A

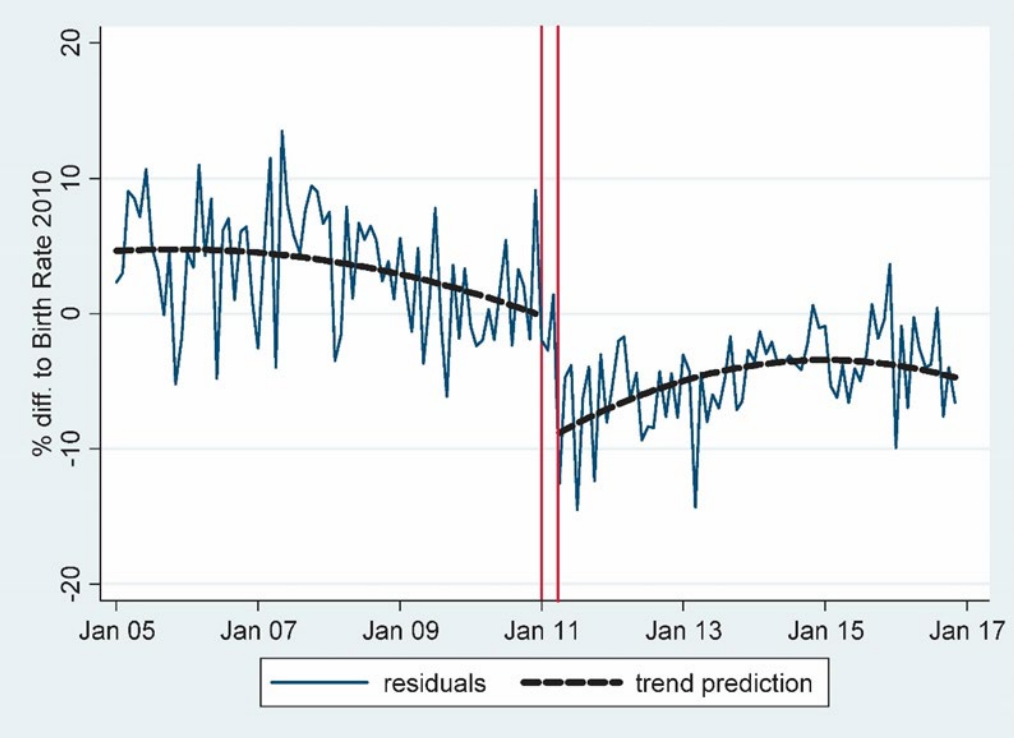
Figure A1: Birth rates residuals relative to birth rate 2010, Sample: Women with one previous child



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

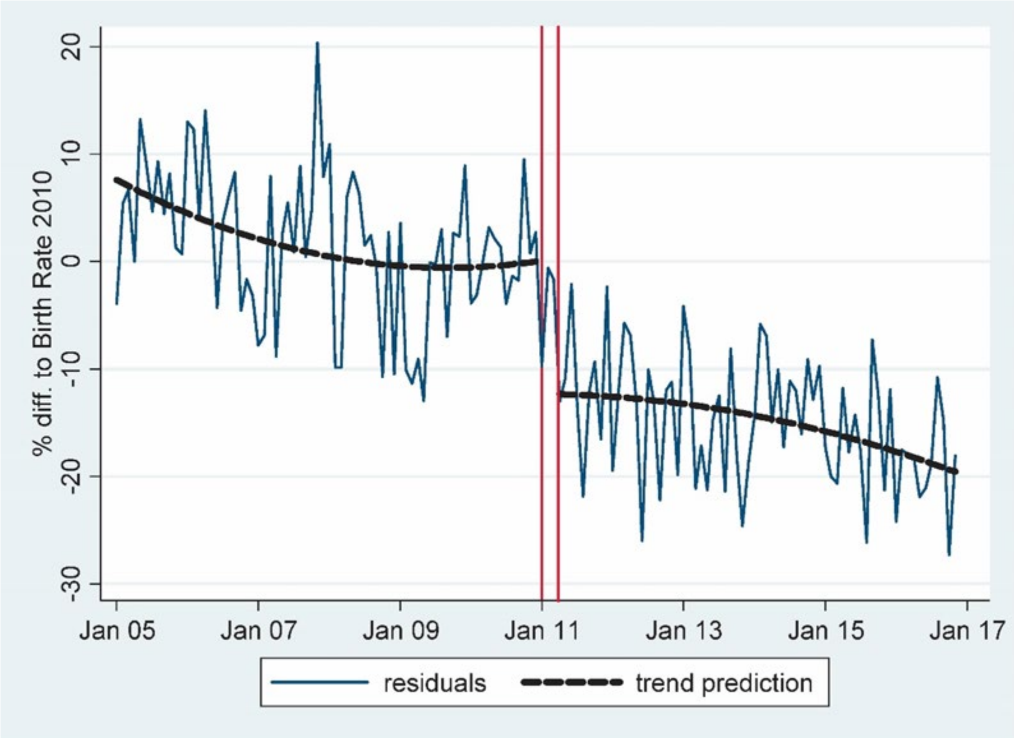
Figure A2: Birth rate residuals relative to birth rate 2010, Sample: Women with two or more previous children



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

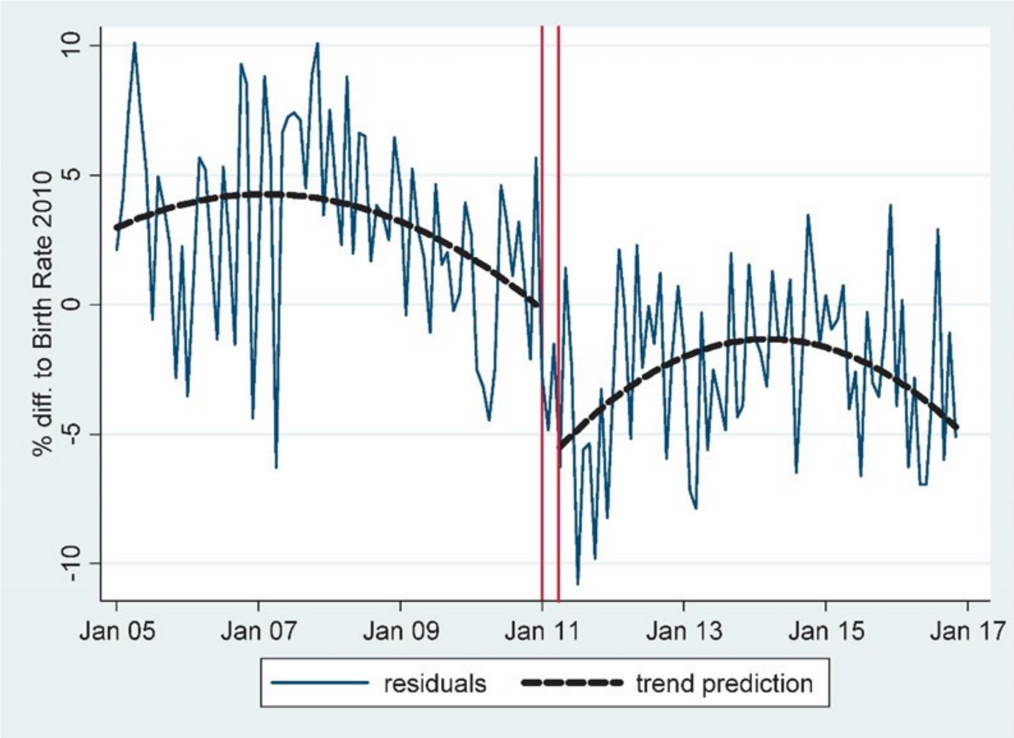
Figure A3: Birth rates residuals relative to birth rate 2010, Sample: Women with no secondary school degree



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

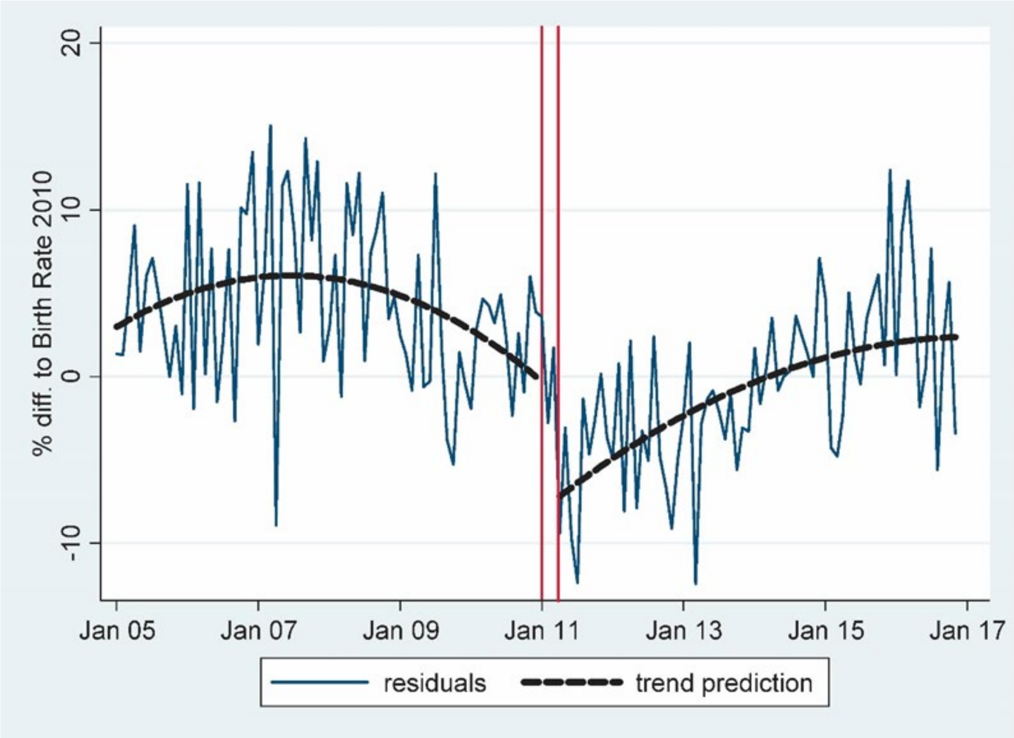
Figure A4: Birth rates residuals relative to birth rate 2010, Sample: Women with a lower secondary school degree



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

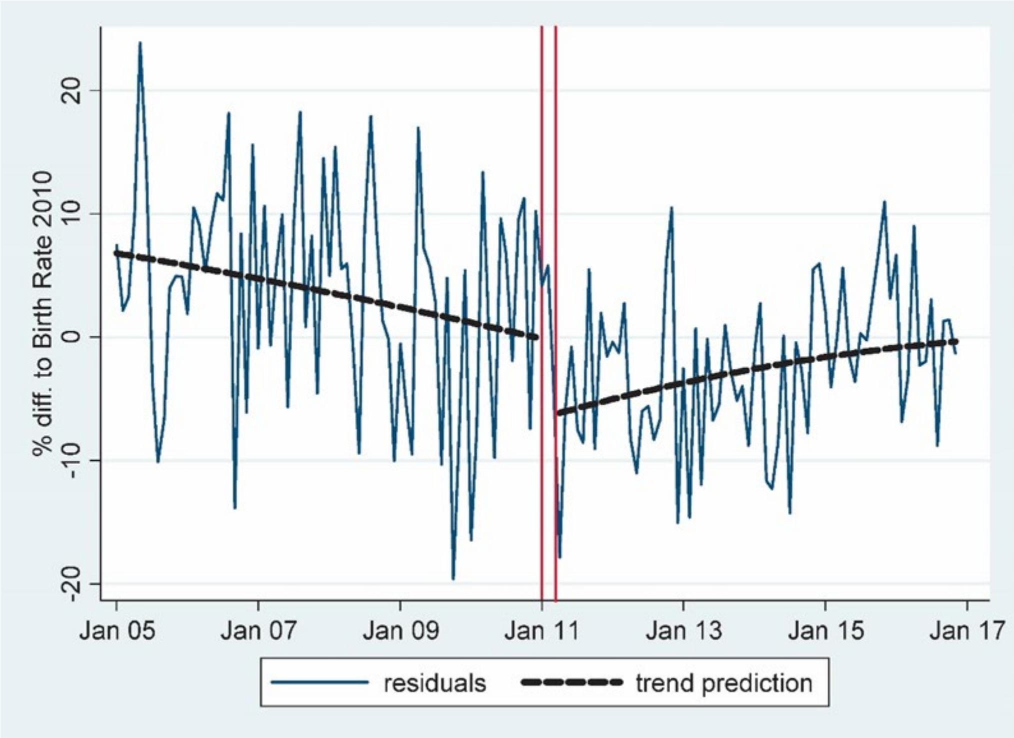
Figure A5: Birth rates residuals relative to birth rate 2010, Sample: Women with a middle secondary school degree



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

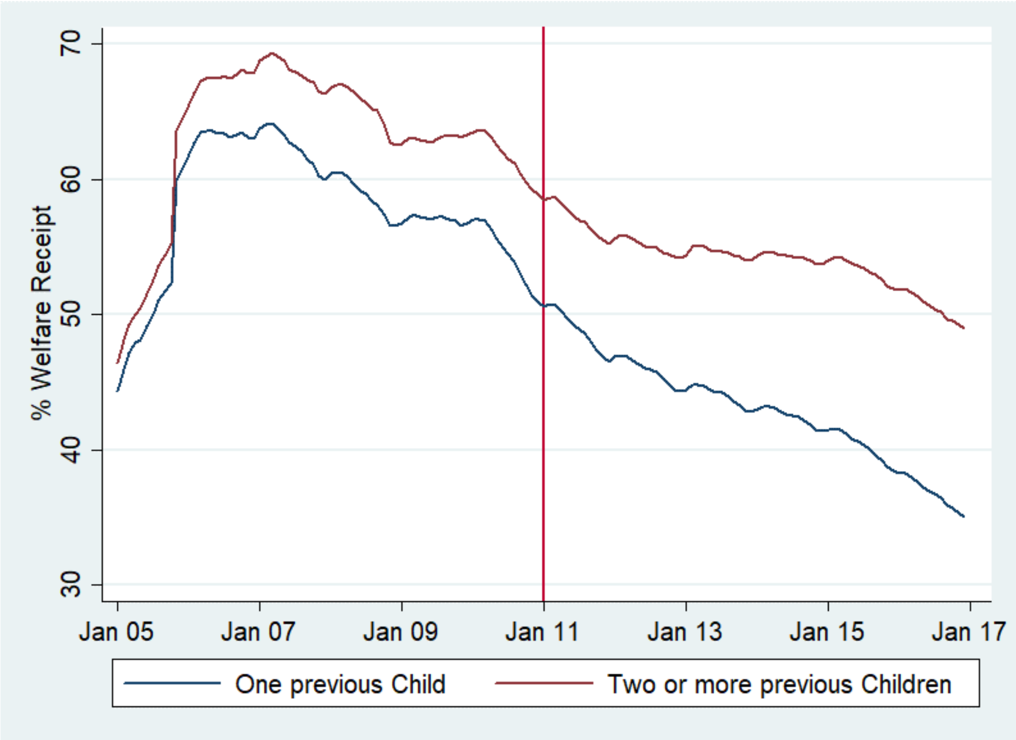
Figure A6: Birth rates residuals relative to birth rate 2010, Sample: Women with a higher secondary school degree



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

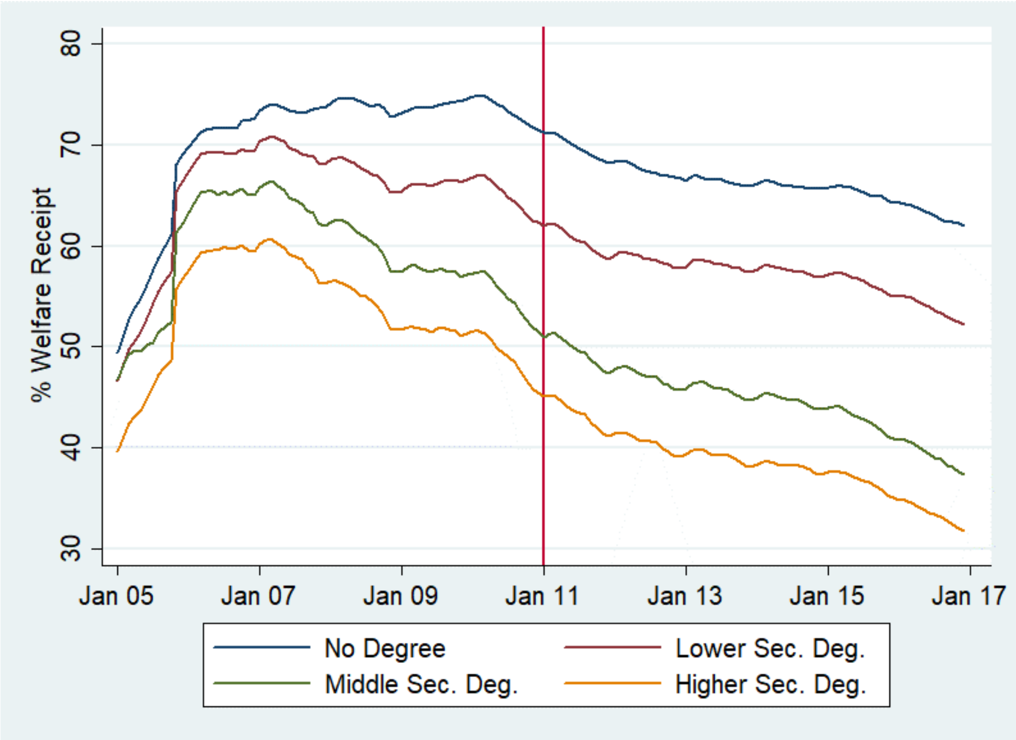
Figure A7: Rate of welfare receipt by number of children



Note: The sample for this figure is the main sample, with the lifted restriction of having to receive welfare in/after December 2016. The restriction is lifted, because the figure serves to show that women are more persistently on welfare the more children they have. (Side note: The upward jump of welfare rates in January 2006 is caused by under-reporting of welfare when the welfare system was reformed in 2005.)

Source: Own calculations based on LHG and IEB data.

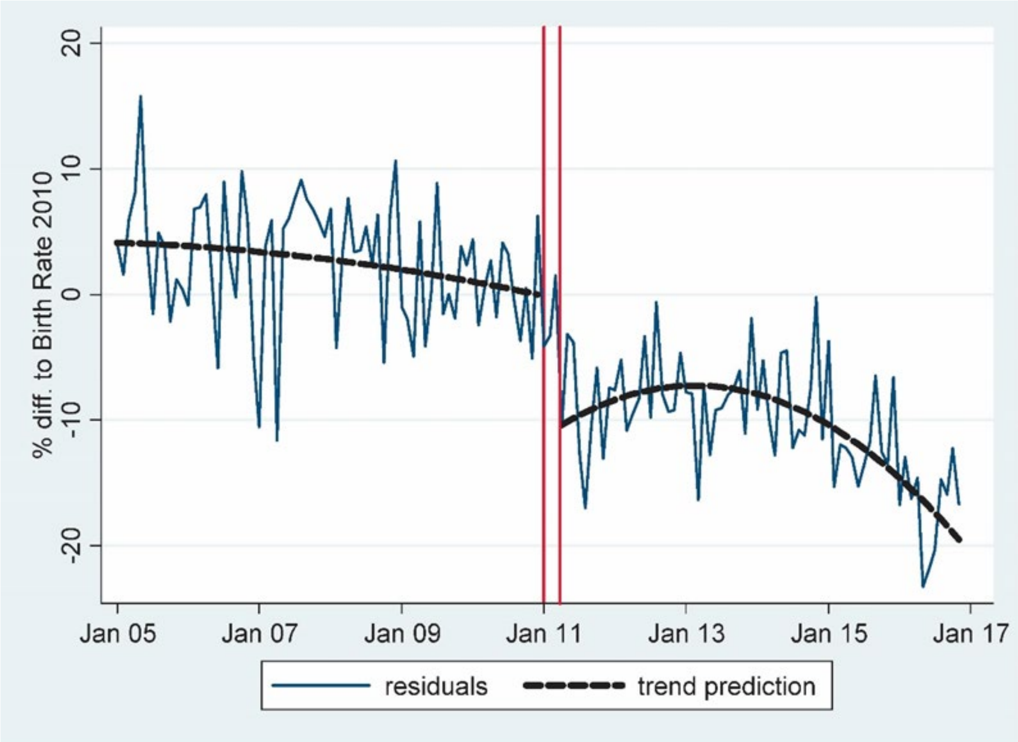
Figure A8: Rate of welfare receipt by secondary school degree



Note: The sample for this figure is the main sample, with the lifted restriction of having to receive welfare in/after December 2016. The restriction is lifted, because the figure serves to show that women are more persistently on welfare the more children they have. (Side note: The upward jump of welfare rates in January 2006 is caused by under-reporting of welfare when the welfare system was reformed in 2005.)

Source: Own calculations based on LHG and IEB data.

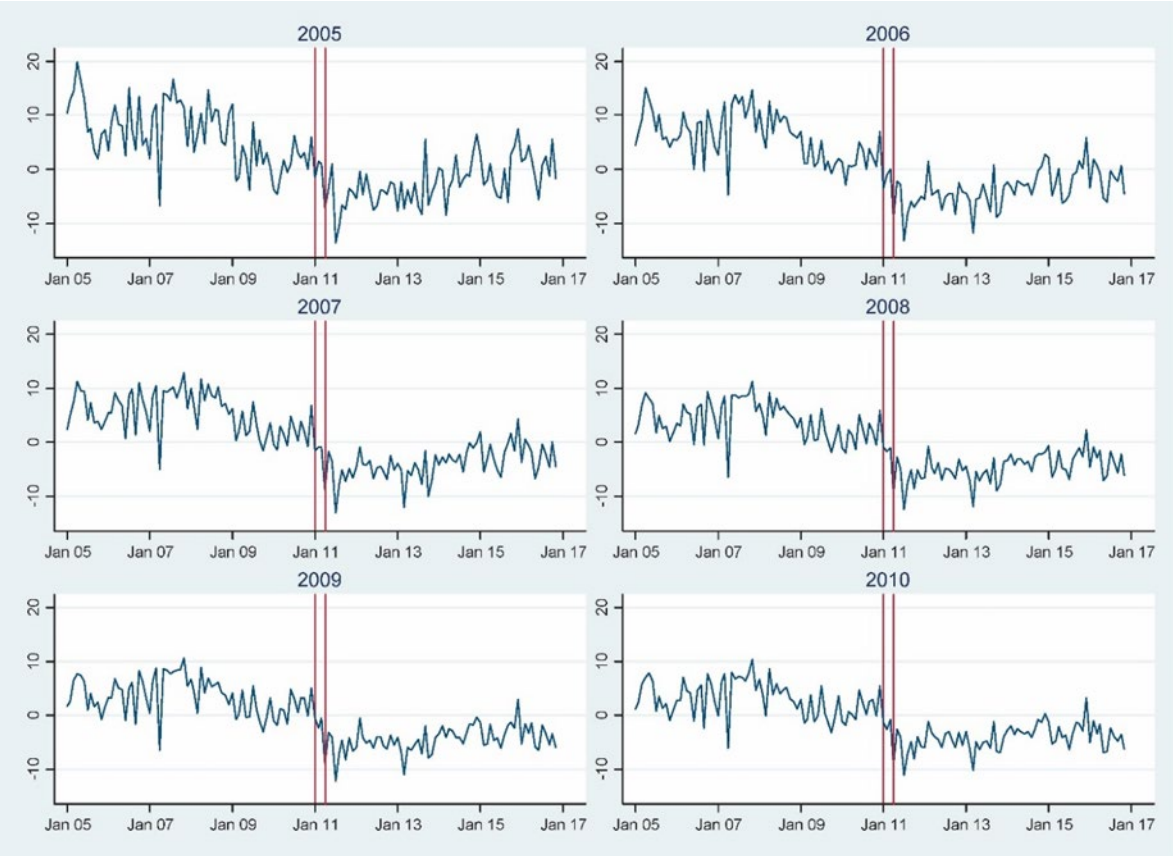
Figure A9: Birth rate residuals relative to birth rate 2010, Sample: Women whose HH-income was permanently less than 300 Euro



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

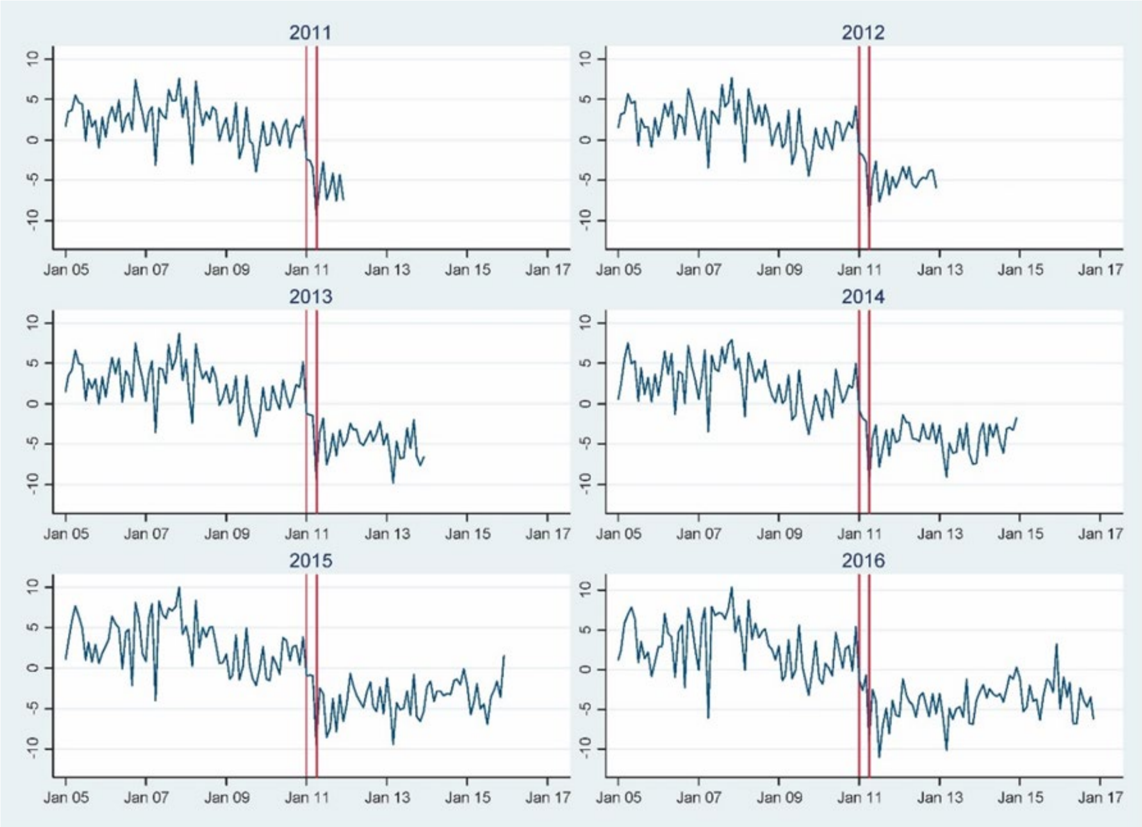
Figure A10: Birth rate residuals relative to birth rate 2010, Sample: Women on welfare after Dec. 2016 and in or before Jan. 2005, Jan. 2006, Jan. 2007



Note: The panels of this figure show the residual plot from Figure 2 for six different samples. The difference between these samples is that the condition of having received welfare for the first time in Jan. 2010 or earlier is shifted. In the top left Panel, this condition is altered from Jan. 2010 to Jan. 2005. In the top right panel, it is Jan. 2006 and so on. The bottom right panel is the residual plot for the base line sample.

Source: Own calculations based on LHG and IEB data.

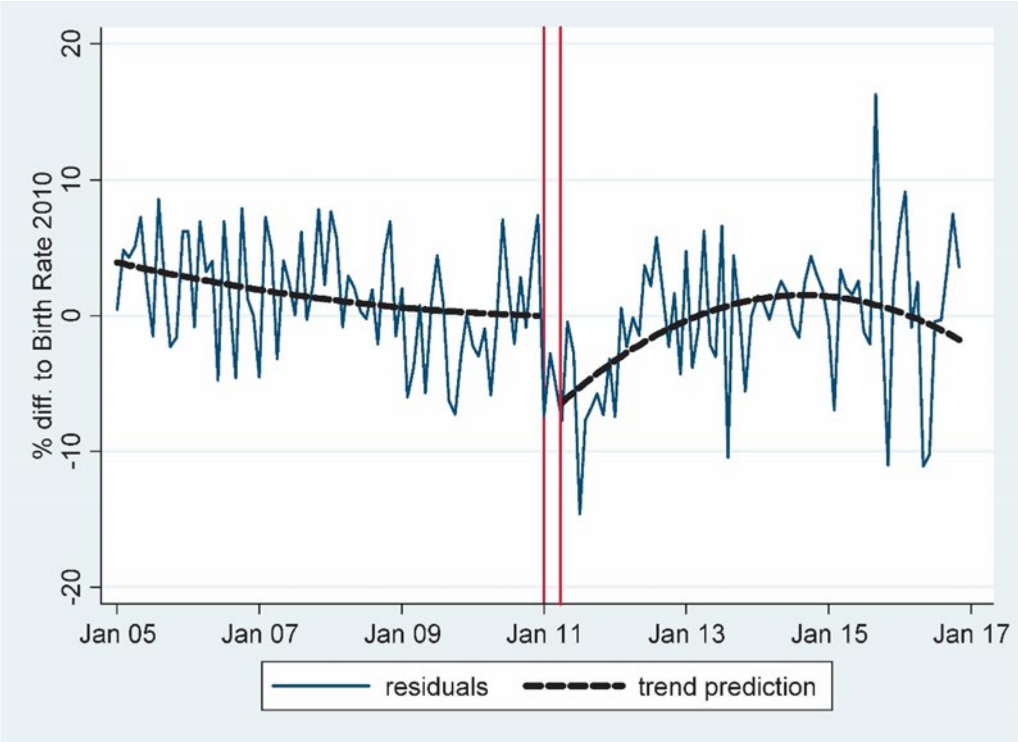
Figure A11: Birth rate residuals relative to birth rate 2010, Sample: Women on welfare before Jan. 2010 and in or after Dec. 2011, Dec. 2012, Dec. 2013



Note: The panels of this figure show the residual plot from Figure 2 for six different samples. The difference between these samples is that the condition of having received welfare for the last time in Dec. 2016 or earlier is shifted. In the top left panel, this condition is altered from Dec. 2016 to Dec. 2011. In the top right panel, it is Dec. 2012 and so on. The residual plots all end in the month after which not all women of that sample received welfare anymore. The bottom right panel is the residual plot for the base line sample.

Source: Own calculations based on LHG and IEB data.

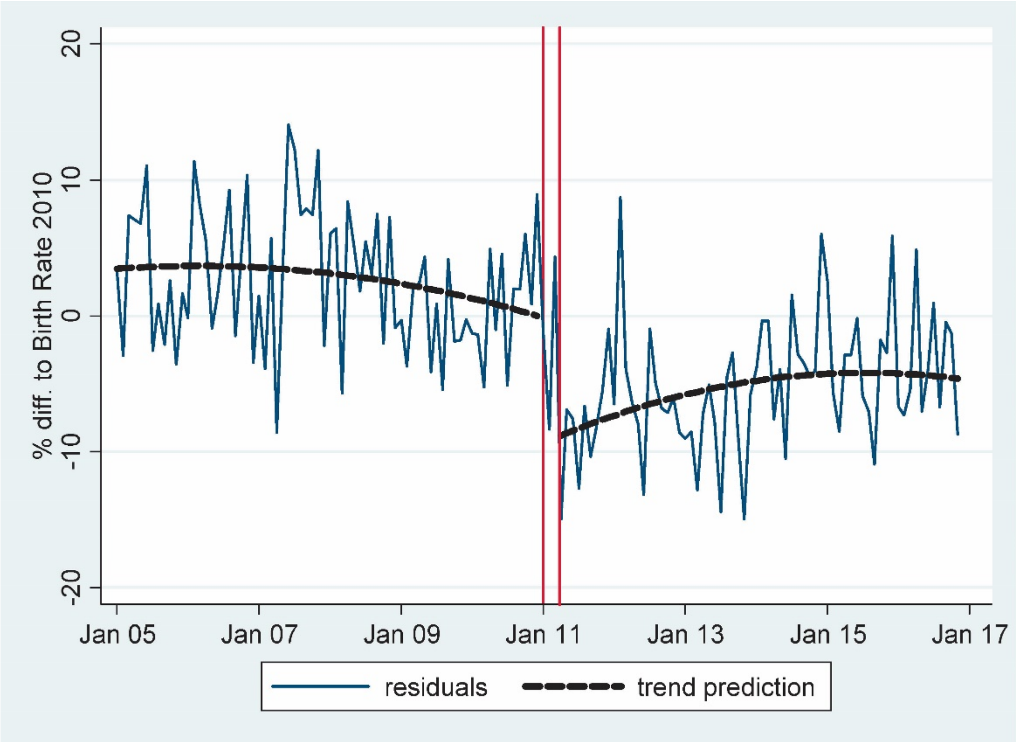
Figure A12: Birth rate residuals relative to birth rate 2010, Sample: Women aged 18-25



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

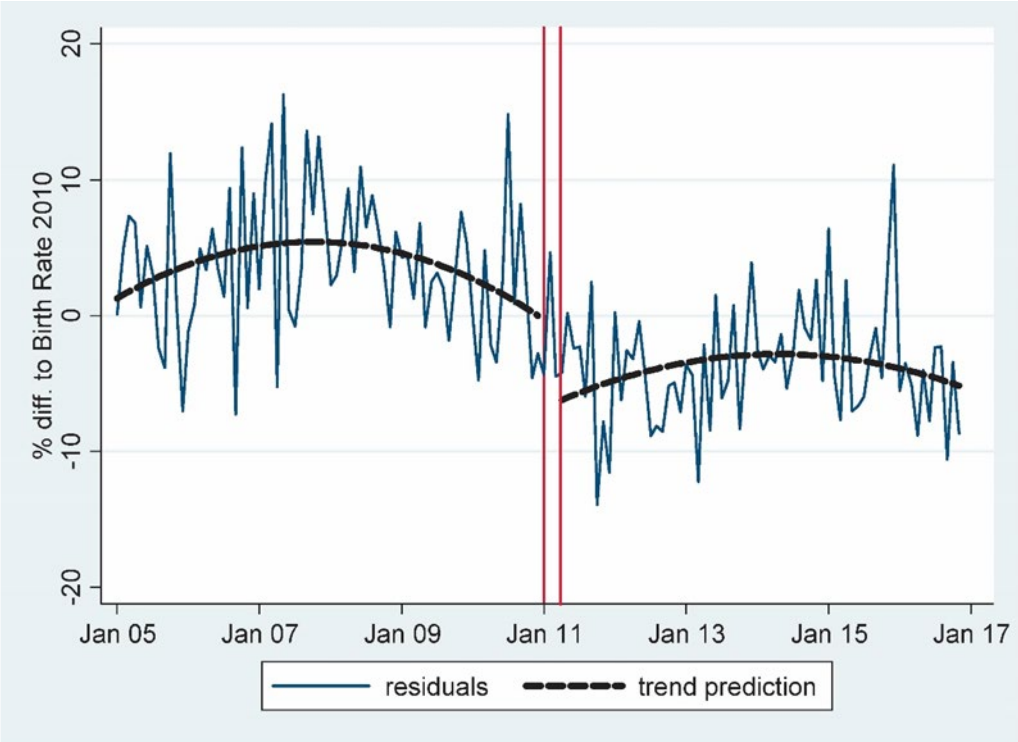
Figure A13: Birth rate residuals relative to birth rate 2010, Sample: Women aged 26-29



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

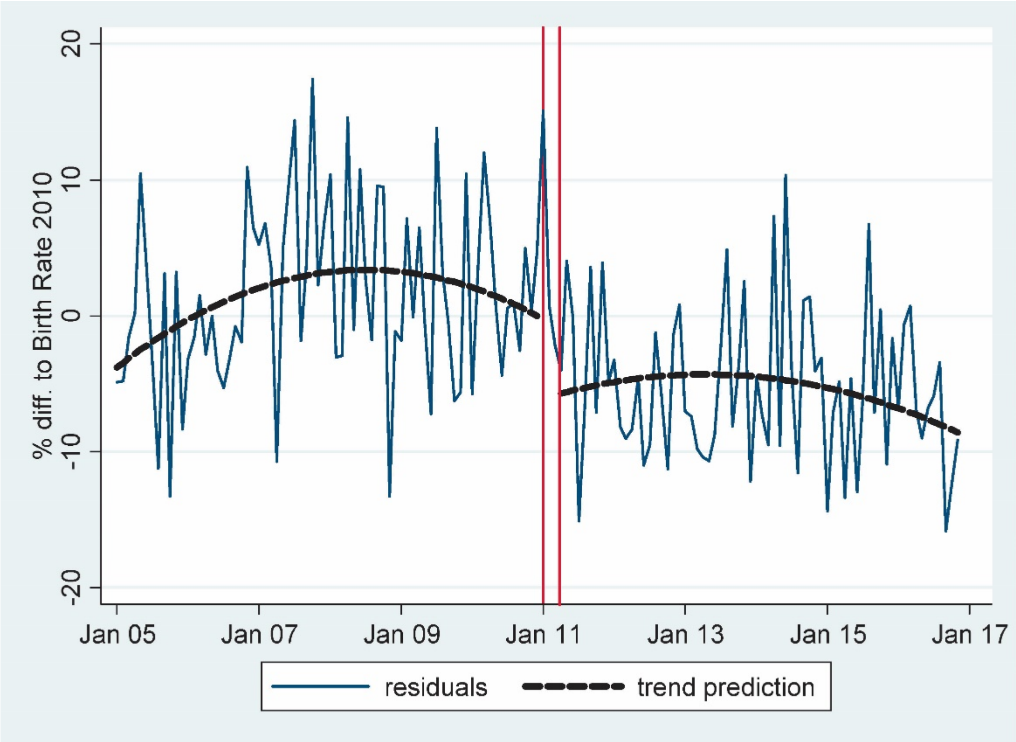
Figure A14: Birth rate residuals relative to birth rate 2010, Sample: Women aged 30-33



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

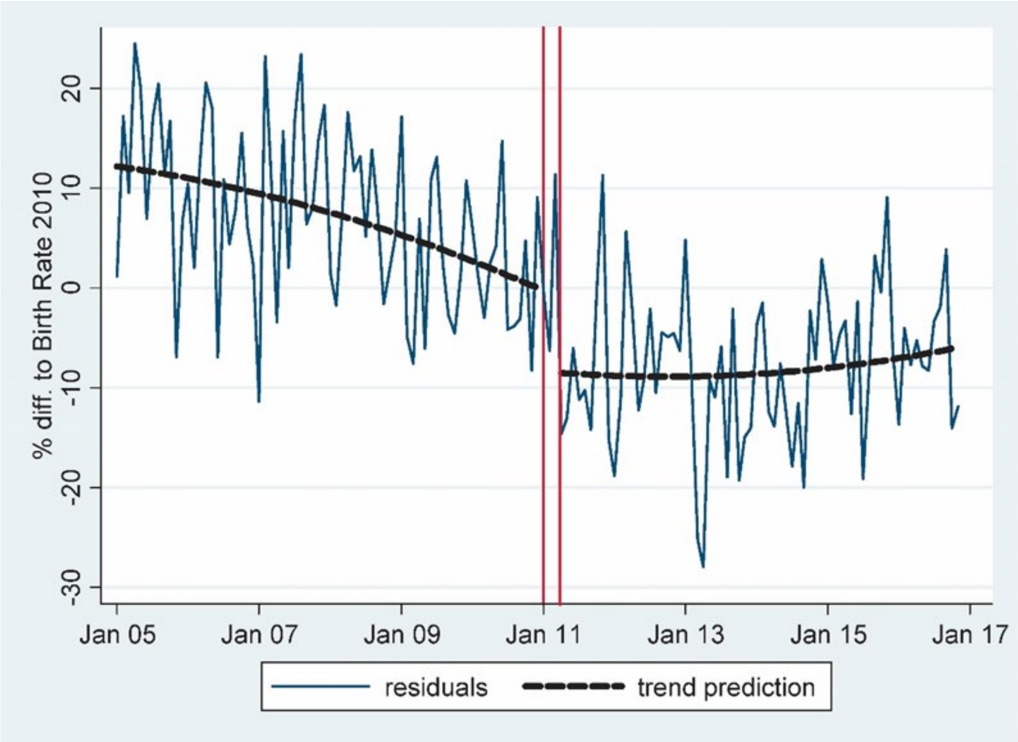
Figure A15: Birth rate residuals relative to birth rate 2010, Sample: Women aged 34-37



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

Figure A16: Birth rate residuals relative to birth rate 2010, Sample: Women aged 38-45



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

Table A1: Standard rate by household position and year

	Monthly Standard Rate	
Household Position	**2010**	**2011**
Single adult	359 €	364 €
Adult in a couple	323 €	328 €
Additional adult	287 €	291 €
Adolescent (15-17)	287 €	287 €
Child (6-14)	251 €	251 €
Child (0-5)	215 €	215 €

Source: SBG II §20 (2010), RBEG §8 (2011).

Table A2: Descriptive statistics: Number children/Number of births

Number of Children	N	%	Birth Parity	N	% of all births
1	170.158	36,73	Second	144.775	50,78
2	152.168	32,85	Third	80.692	28,31
3	82.877	17,89	Fourth	34.904	12,24
4	35.168	7,59	Fifth	14.209	4,98
5 or more	22.892	4,94	Sixth or higher	10.499	3,68
Total	463.263	100	Total	285.079	100
Average	2,15	-			
Maximum	16	-			

Note: The numbers refer to the main estimation sample of women who have at least one previous child and received welfare in January 2010 or earlier and in December 2016 or later. The number of children is taken from each woman's last observation.

Source: Own calculations based on LHG data.

Table A3: Bandwidth tests including quadratic trend variables

	2 Years	4 Years	6 Years	8 Years	10 Years	12 Years
	(1)	(2)	(3)	(4)	(5)	(6)
Reform Dummy	-8.328	-13.375***	-11.369***	-6.424***	-5.341***	-6.762***
	(62112.21)	(2.588)	(1.834)	(1.478)	(1.267)	(1.121)
N	7.096.820	15.143.717	23.083.735	30.881.198	38.517.530	45.966.533
Units of Obs.	362.900	386.405	407.817	428.314	446.985	463.263

Note: See Table 4, Panel C. This table shows the same bandwidth test, with the alteration that the estimation includes quadratic pre- and post trend variables.

Source: Own calculations based on LHG and IEB data.

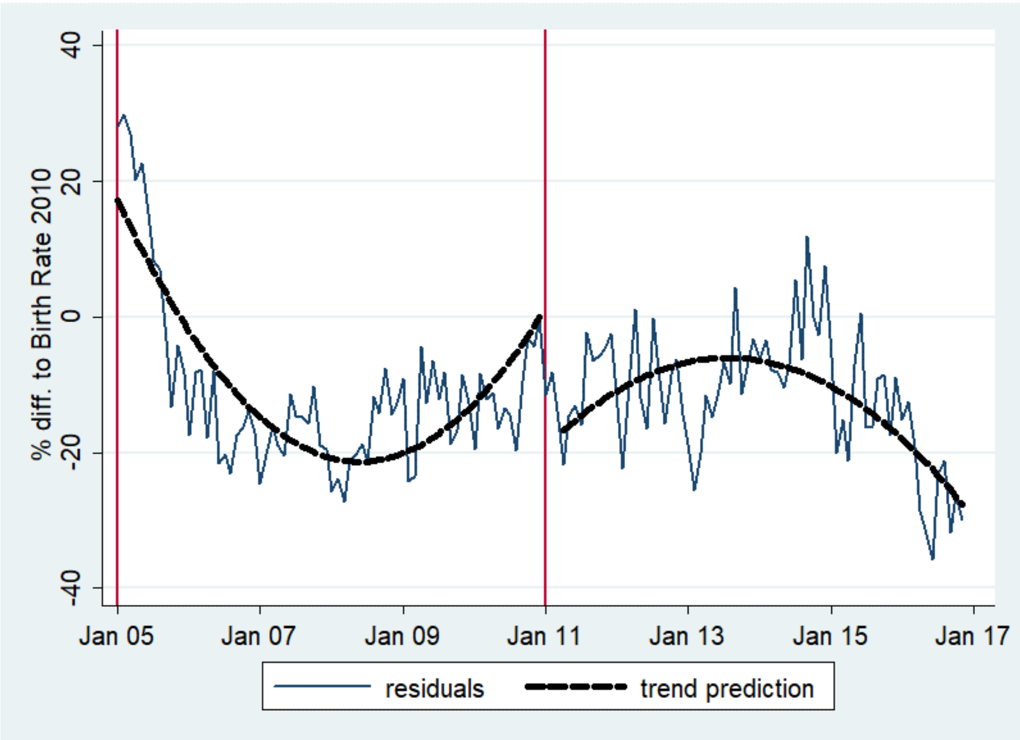
Appendix B

We separate the analysis of first birth fertility from the rest of the investigation, because it requires a different approach than that of higher order fertility in the case at hand. It differs, because the course of aggregate first birth fertility changes when we change the relevant point in time for the first sample restriction (i.e. the year when a woman has to have received welfare to be part of the sample, see section 4). Figures B1 – B6 show residual plots of first birth fertility for which we successively move this restriction towards the reform date. In Figure B1 the sample consists of women who received UBII in January 2005 and in December 2016 or later. In Figure B2 the left side restriction is shifted to having received welfare in January 2006 or earlier and so on until it is at 2010 in Figure B6. The left red line marks the time of the restriction in each figure.

The figures show that the course of the residual plot is highly dependent on the time to which this left side restriction is set. Fertility always peaks shortly before the time of the restriction and declines sharply afterwards. The reason for this pattern is that having a first child is often what leads to welfare receipt. Women who have to care for an infant cannot work and the presence of the child increases the benefit entitlement amount. This pattern makes the baseline specification from the main analysis unfit for the analysis of first birth fertility for several reasons.

The point estimate of the reform dummy is the difference between the end of the pre-reform trend line and the beginning of the post-reform line. Figure B6 illustrates how the quadratic trend captures the fertility drop incorrectly in this case. The estimate is clearly driven by the timing of the restriction. The drop already starts in early 2010. For estimations of the samples displayed in Figures B3 – B6 the estimate of the reform effect would even be positive. Therefore, at least the functional form and probably the bandwidth would have to be adjusted. It would not be clear though, which left side restriction would define the most relevant sample. If the restriction is set to January 2010 or even 2011 the estimated reform effect would be confounded with the effect from the drop that always follows after the point in time to which the restriction is set. A possible solution could be, to set the restriction relatively early in the period of observation. Still, any evidence for the reform effect on first births derived from this would not be very robust. Therefore, we leave the investigation of the reform effect on first birth fertility to future research.

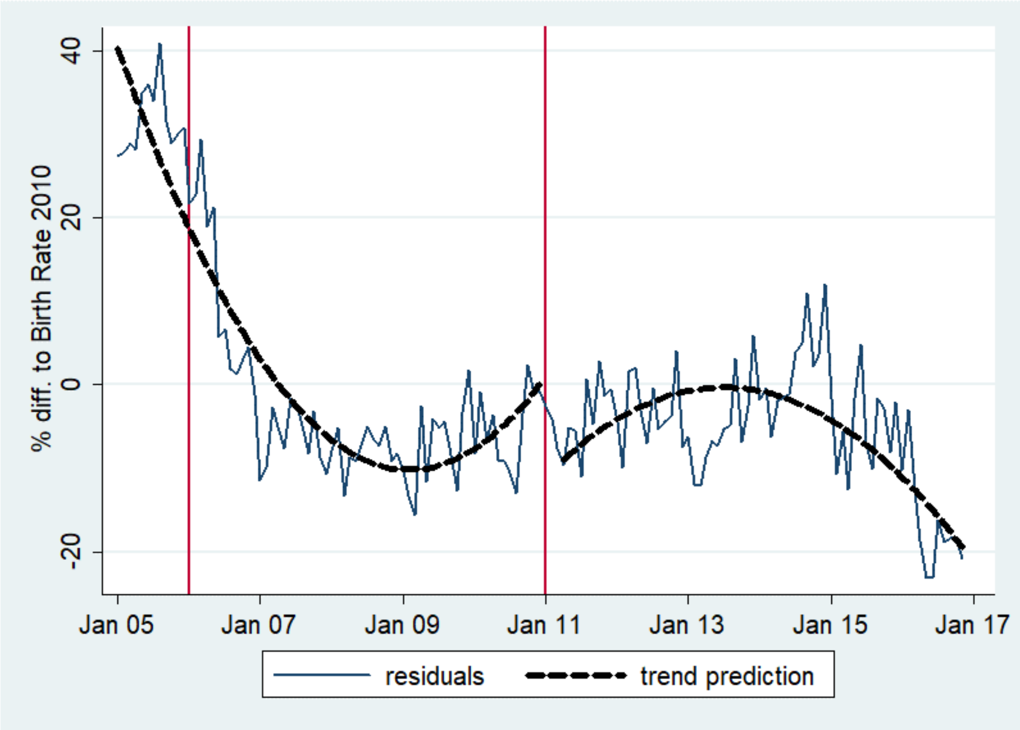
Figure B1: First Birth rate residuals relative to birth rate 2010, Sample: Women on welfare in Jan. 2005 and after or in Dec. 2016



Note: cf. Figure 2. There are two differences between this graph and the one in Figure 2. First, this graph relates to the sample of women without children (these women are not part of the samples in the main analysis). Second, the restriction of when a woman first has to have received welfare is set to January 2005 in this sample. The residuals are rescaled to display the difference to the birth rate 2010 of this sample. In the following graphs of residuals, we will refer to these notes. For them these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

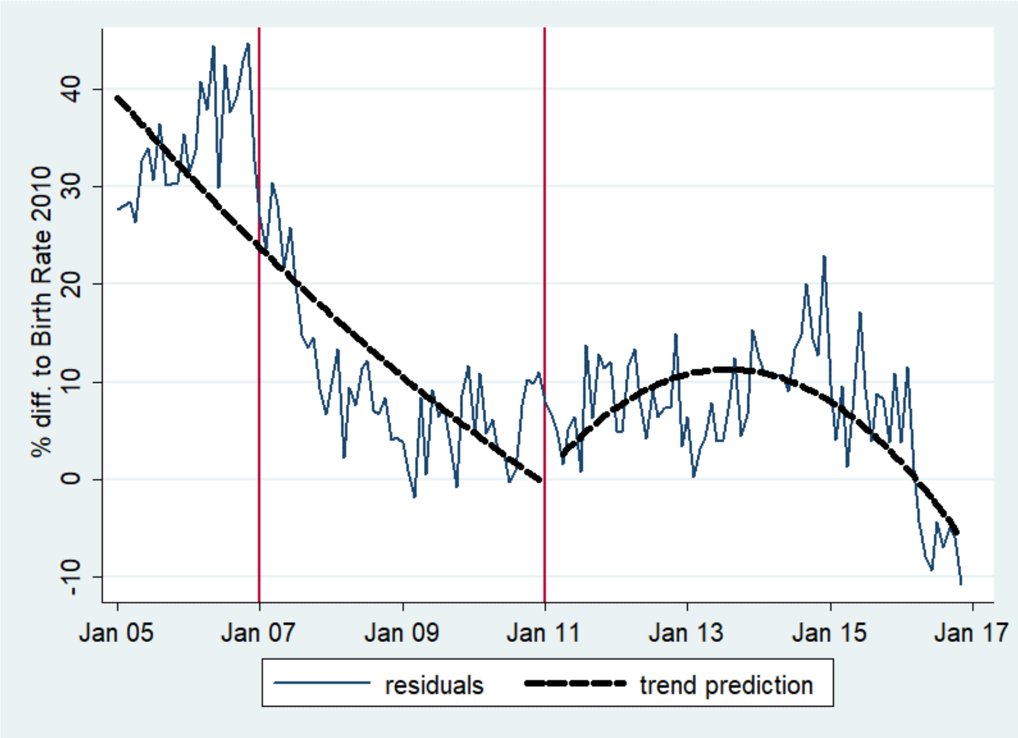
Figure B2: First Birth rate residuals relative to birth rate 2010, Sample: Women on welfare before or in Jan. 2006 and after or in Dec. 2016



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

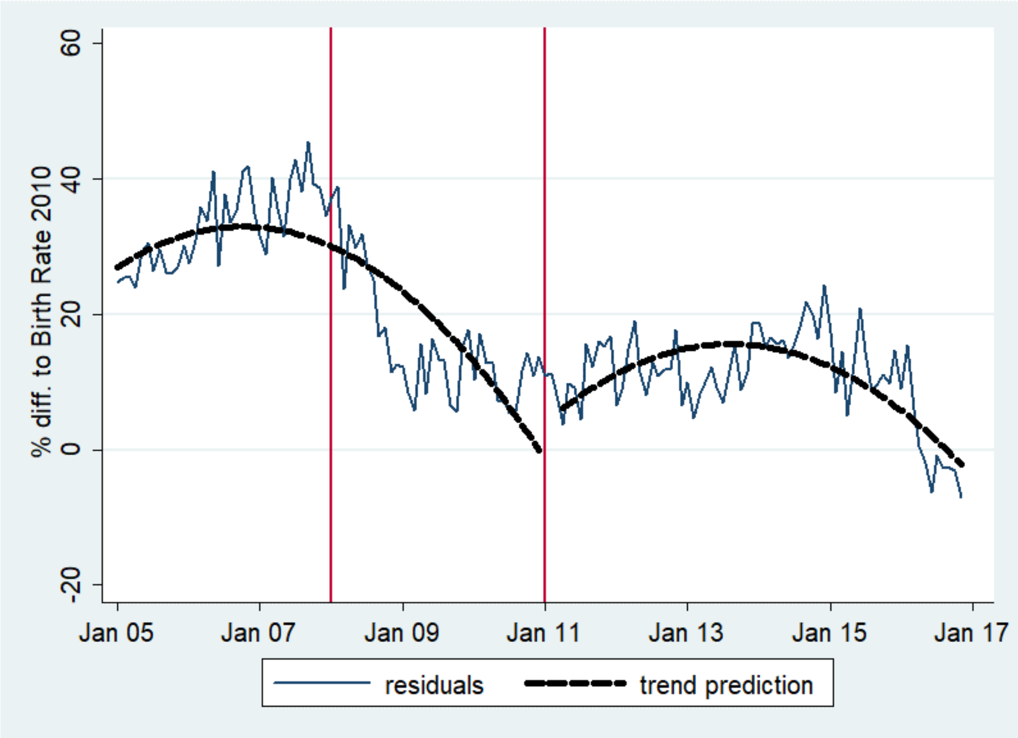
Figure B3: First Birth rate residuals relative to birth rate 2010, Sample: Women on welfare before or in Jan. 2007 and after or in Dec. 2016



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

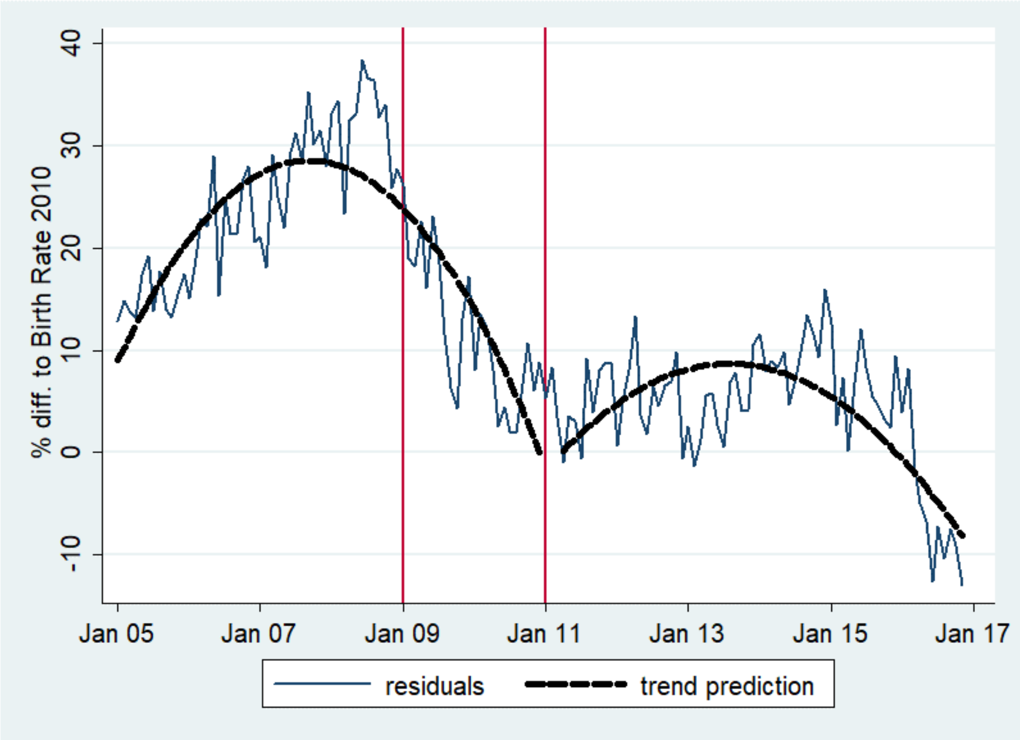
Figure B4: First Birth rate residuals relative to birth rate 2010, Sample: Women on welfare before or in Jan. 2008 and after or in Dec. 2016



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

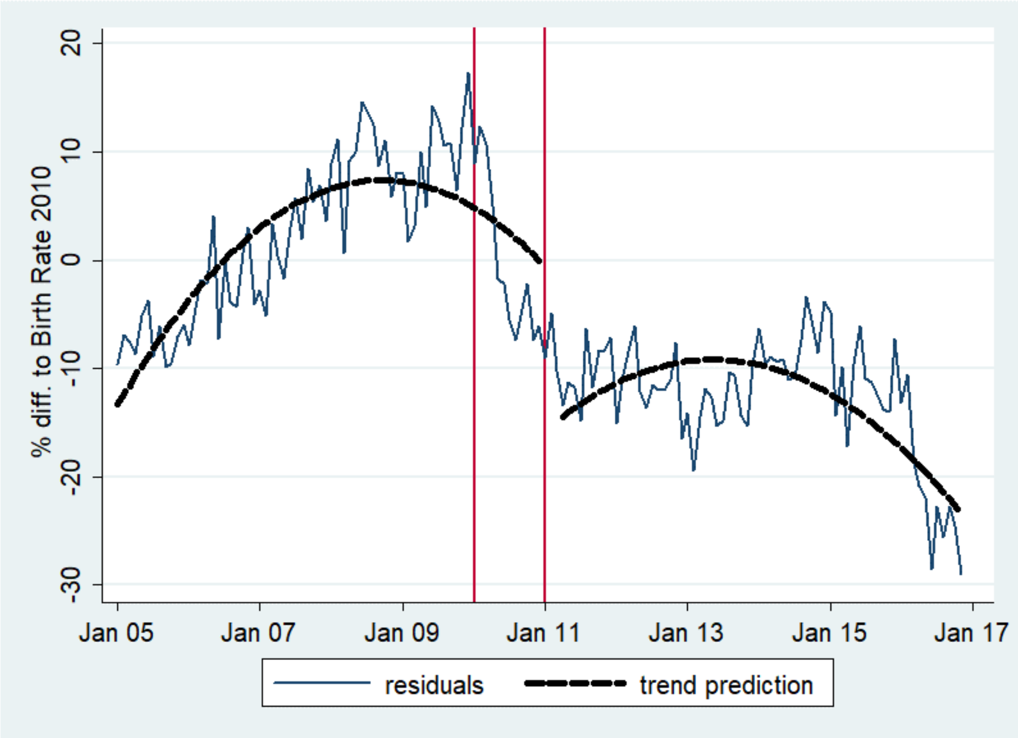
Figure B5: First Birth rate residuals relative to birth rate 2010, Sample: Women on welfare before or in Jan. 2009 and after or in Dec. 2016



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

Source: Own calculations based on LHG and IEB data.

Figure B6: First Birth rate residuals relative to birth rate 2010, Sample: Women on welfare before or in Jan. 2010 and after or in Dec. 2016



Note: cf. Figure 2. The only difference between this graph and the one in Figure 2 is that this graph relates to the subsample of women who have one child (excluding those who already have more than one) and the residuals are rescaled to display the difference to the birth rate 2010 of this subsample. In the following graphs of residuals, we will refer to these notes. For them, these alterations apply for using the respective subsample from the title of the respective figure.

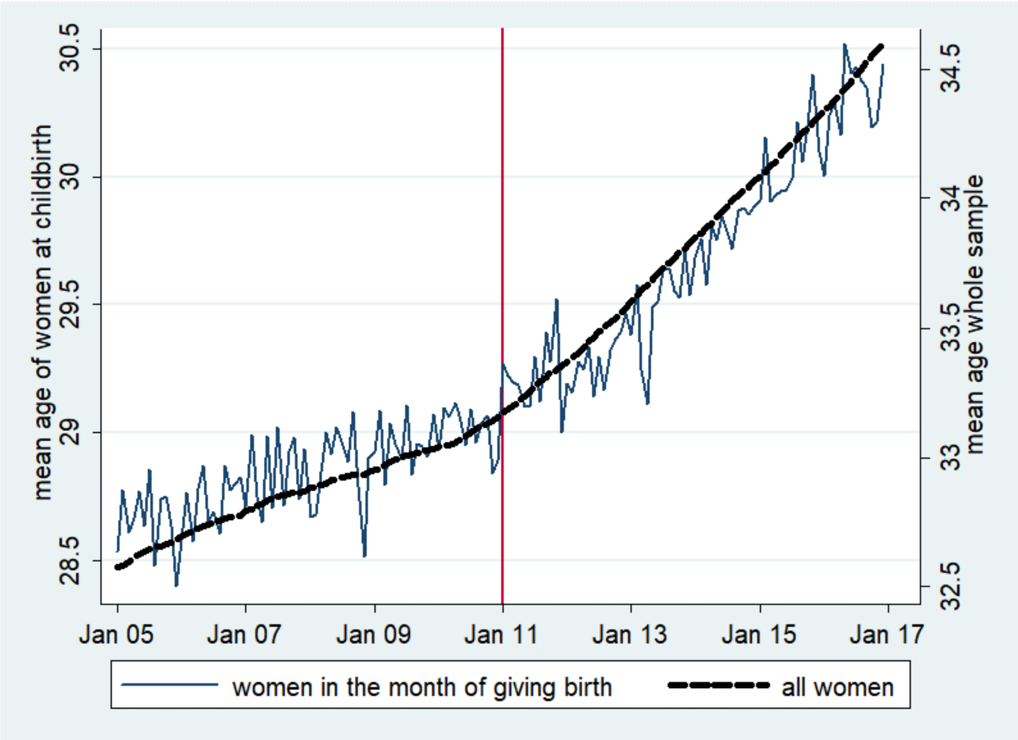
Source: Own calculations based on LHG and IEB data.

Appendix C

This appendix tests whether the reform increased the time between births or the age at which mothers give birth. If this were the case, the fertility decrease we identify in the main section could be the result of a postponement of births rather than a reduction of the final number of children.

To gauge if this is the case, we graph women's average age at birth and the average time between births over time. The blue line in Figure C1 shows the mean age of women in the main estimation sample in the month they give birth over time. The vertical red line marks the time the reform sets in. The dashed black line plots the mean age of all women in the main estimation sample, regardless of giving birth. The graph shows that the age of women at birth increases at the same rate as the age of the overall sample. This is evidence against postponement of births being the explanation for the decreased fertility.

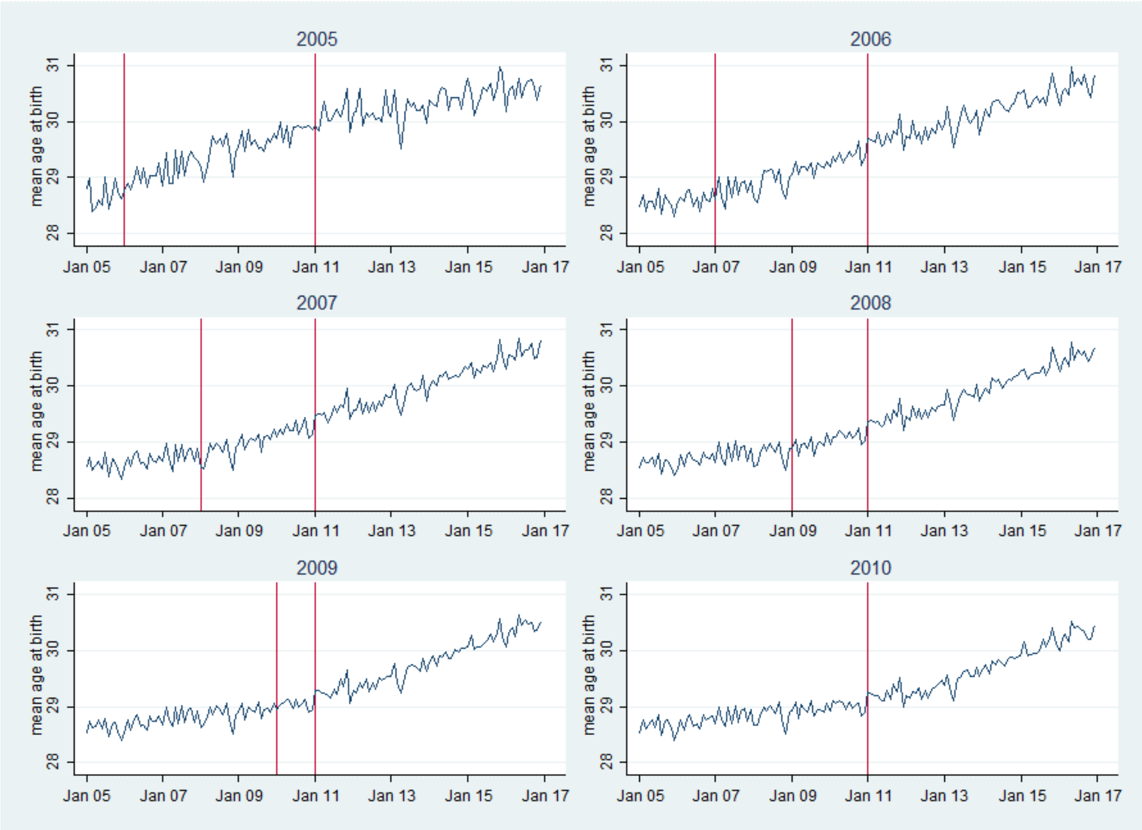
Figure C1: Mean age at birth vs. Mean age sample



Source: Own calculations based on LHG and IEB data

Still, it seems curious that mean age increases with a stronger positive trend after the reform. The reason for the trend increase lies with the restriction of having received welfare for the first time before 2010. This fixes a number of women who can become part of the sample that is displayed by each point in the line. Before the restriction sets in, any young women who receives welfare and has a child joins the pool. After the restriction sets in, the pool of this sample just draws from the same women determined at some point in time in the past. There are relatively few women under e.g. 20 who have their first child in January 2013 and fulfill the condition of having been on welfare already before 2010. So, many of them do not refresh the pool. This leads to the steeper increase in mean age after the time of the restriction, in this case 2010. Figure C2 shows that this trend increase sets in earlier, if the restriction is set to an earlier point in time.

Figure C2: Mean age at childbirth with different time restrictions



Note: The six panels of the figure show the mean age at childbirth over time for women fulfilling different time restrictions. The top left panel contains only women who received welfare as early as January 2005 and still or again in or after December 2016. The top right panel shifts the first restriction to January 2006 and so on. All panels contain two vertical red lines. The left one marks 12 months after the “first welfare receipt” restriction sets in. The right one marks January 2011, the month the reform sets in. In the bottom right panel these two lines overlay each other...

Source: Own calculations based on LHG and IEB data..

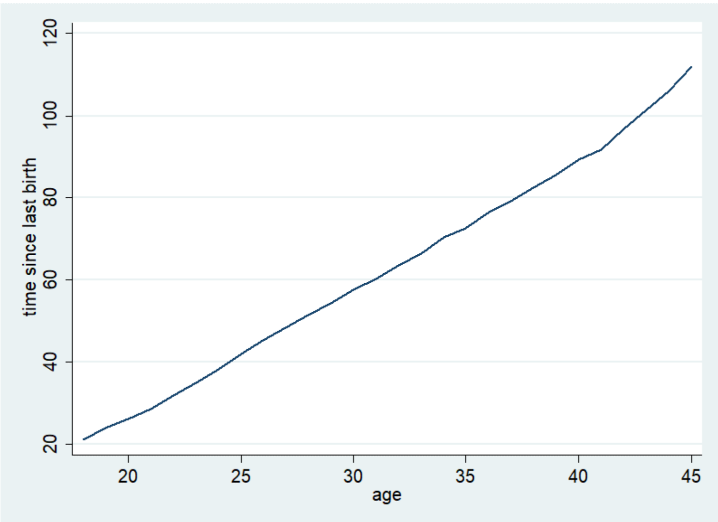
Figure C2 graphs the average age for six different subsamples over time. The top left panel shows the mean age for women in the month of childbirth who received welfare already in January 2005 and are still or again receiving it after December 2016. In the next panel the first restriction is shifted to January 2006 and so on. All panels contain two vertical red lines. The right one marks January 2011, the left one marking one year after the restriction sets in. This means, for the sample of women who received welfare already in January 2005, this first line marks January 2006. The trend in mean age always changes somewhere around the first red line and, except for the main sample, where the two red lines coincide, there is nothing noteworthy happening at the line marking January 2011.

Figure C3 is generated analogously to Figure C2, just with “months since last birth” as the dependent variable. This variable displays the same general pattern of a trend increase roughly a year after the left time restriction sets in. The positive trend with an increase at the left time restriction can be explained by the positive correlation between a women’s age and her birth spacing. Figure C4 shows the mean birth spacing by age at childbirth and is evidence for this positive correlation.

Figure C3: Mean time since last birth with different time restriction

Note: See Figure C.2. This figure was generated analogously, just with "mean months since previous birth" as the dependent variable instead of mean age.
Source: Own calculations based on LHG and IEB data..

Figure C4: Months since last birth by age



Source: Own calculations based on LHG and IEB data.

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