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*Katharina Heisig, Larissa Zierow*

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Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email [office@cesifo.de](mailto:office@cesifo.de)

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# Parental Leave and Long-Term Life Satisfaction of Children - Quasi-Experimental Evidence from Former East Germany

## Abstract

This study investigates the impact of an increase in paid parental leave — twelve instead of five months — on children’s long-term life satisfaction. Our setting, former East Germany, features high labor market participation of mothers and universal supply of standardized childcare. It thus mitigates identification issues such as selection into the labor market and provides a clear counterfactual to maternal care. Applying a difference-in-differences design we exploit the specific timing of parental leave reforms in 1976 and 1986. We find a significant and robust positive effect on now-adult children’s life satisfaction, and show that long-run subjective well-being can already be impacted by policies during the first year of an individual’s life.

JEL-Codes: J130, J220, I310.

Keywords: parental leave, well-being, childcare, child development.

*Katharina Heisig*  
*ifo Institute – Leibniz Institute for Economic*  
*Research – Dresden Branch*  
*Einsteinstr. 3*  
*Germany – 01069 Dresden*  
*heisig@ifo.de*

*Larissa Zierow*  
*ifo Institute – Leibniz Institute for Economic*  
*Research at the University of Munich*  
*Poschingerstrasse 5*  
*Germany – 81679 Munich*  
*zierow@ifo.de*

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## 1 Introduction

Increasing the well-being of citizens is a major goal of welfare states. An important tool of welfare state expansions during the last decades are paid parental leave policies (Henderson and White, 2004). It has been found that enabling young families to spend time with their newborns without giving up financial stability increases fertility (e.g., Lalive and Zweimüller, 2009). At the same time, child psychology literature suggests that spending more time with the primary caregiver — most often the mother — within the first year of life has a positive impact on a child’s development (Bowlby, 1969).

Recent economic studies evaluate the impact of parental leave on children’s development by looking at educational, health, or socio-emotional dimensions (e.g., Ruhm, 2000; Berger et al., 2005; Rasmussen, 2010; Baker and Milligan, 2008; Carneiro et al., 2011; Dustmann and Schönberg, 2011; Danzer and Lavy, 2017; Huebener et al., 2018; Bullinger, 2019; Albagli and Rau, 2019; Ginja et al., 2020; Fabel 2021). However, to the best of our knowledge, there is no study looking at short- or long-term impacts of parental leave reforms on children’s well-being to measure potential welfare gains. Yet, subjective well-being of citizens is a frequently used indicator to measure living conditions in a welfare state (e.g., Veenhoven, 2001; Diener and Suh, 1998; Diener, 2009; Kahneman and Krueger, 2006) and can therefore be considered an important outcome of parental leave reforms.

For that reason, we study the effects of a substantial increase of parental leave on long-term subjective well-being<sup>1</sup> of children. We use the context of former East Germany to circumvent methodological challenges of previous parental leave studies. Former East Germany serves as a pioneer in terms of high shares of female labor force participation and extensive public childcare: first, the vast majority of all working-aged women and mothers were employed. Female labor market participation as an aspect of gender equality was an important pillar of socialism and was about 90% in the 1980s.<sup>2</sup> If women and mothers wanted to be socially accepted, they had no alternative but to be (full-time) employed. Second, childcare — standardized full-day care for children from zero to six years — was available for most children and organized and supervised by the government. There was almost no variation between available childcare facilities and thus no selection of specific groups of children into particular forms of childcare.

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<sup>1</sup> We measure subjective well-being by overall life satisfaction, which is methodologically advantageous to using happiness scales (see, e.g., Bond and Lang, 2019).

<sup>2</sup> In 1989, 91.2% were employed or in training (Winkler, 1990).

We analyze a sequence of parental leave reforms introduced on short notice in 1976 and 1986 in former East Germany: the so-called *baby year* reforms. Thus, these reforms were unanticipated by the public.<sup>3</sup> The specific eligibility criteria and timing of the reforms allow for a causal analysis. Before the reforms, mothers were granted five months of paid maternal leave. After this period, most mothers returned to work and their children were enrolled in childcare centers.<sup>4</sup> The 1976 *baby year* reform extended the leave period from five to twelve months for mothers with newborns, but only for mothers who already had at least one further child. Thus, firstborn children still attended childcare centers from the age of five months onwards and mothers of firstborns returned to work. This changed in 1986, when paid parental leave of twelve months was also granted to mothers with a firstborn child. We exploit this quasi-experimental variation in paid parental leave induced by the reforms for our identification strategy.

In our main analysis we apply a difference-in-differences (DiD) approach focusing on the 1986 *baby year* reform. We compare outcomes of firstborn children born before (*not assigned*) and after (*assigned*) the 1986 reform.<sup>5</sup> To isolate the causal reform effect, we use laterborn children (who already have one or more older siblings) born in the same cohort as control group. This implies that being a firstborn or laterborn is the second difference dimension in our DiD design. Laterborns were already granted twelve months with their mother as a result of the *baby year* reform of 1976 and were thus unaffected by the reform of 1986. The most important identification assumption in this approach is the common trends assumption of firstborns and laterborns in our sample. We provide evidence that this assumption is plausible.

Still, there might be concerns about any unobserved events which had differential impacts on firstborns and laterborns, such as the Chernobyl disaster in 1986. In order to mitigate this concern, we additionally analyze the long-term effect of the 1976 *baby year* reform on life satisfaction of laterborn children. Inversely to our main approach, we compare long-term overall life satisfaction of laterborn children born before (*not assigned*) and after (*assigned*) the 1976 reform. In this approach, firstborns serve as control group, as their mothers were not eligible for the *baby year* granted by the reform of 1976.

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<sup>3</sup> Eligible parents were mothers and fathers. However, since almost no fathers made use of the *baby year* (Groeben, 2011), we most likely estimate the effects of a *mother's* extended parental leave on their child's long-term life satisfaction.

<sup>4</sup> Taking up unpaid parental leave was rather unusual due to the social desirability of mother's labor force participation, but not impossible in former East Germany.

<sup>5</sup> Mothers with children younger than twelve months were eligible for the reforms. See Section 3 for further details on how assigned and not assigned individuals are defined.

To estimate the introduced DiD framework, we use the German Socio-Economic Panel (SOEP), which is a representative sample of people living in Germany. To analyze the reform in 1986 (1976), we draw upon the SOEP waves of 2008 to 2018 (1991 to 2018) and observe individuals who were born between 1980 and 1991 (1966 and 1984) and raised in former East Germany. The SOEP contains necessary information on siblings for our identification strategy, so that we are able to identify whether an individual is a firstborn or laterborn, and, thereby determine the *baby year* eligibility status of an individual's mother. Note that the SOEP data does not contain information on the take-up rate of the *baby year*. Given evidence from other sources, we know that 95% of all mothers took a *baby year* in the late 1980s (Hoeckner, 1995). This indicates that almost every eligible mother made use of the *baby year* — meaning that almost every eligible child was affected by the reform. Strictly speaking, however, we estimate the intention-to-treat effect of the reforms.

Estimation results of the 1986 *baby year* reform effect show that extending parental leave by seven months has a significant positive effect on long-term life satisfaction of children. The reform targeted at firstborn children increases firstborn's life satisfaction scores in adult age by 41.3% of a standard deviation, corresponding to an increase by 9.3% of the pre-reform mean. In relation to life event impacts on subjective well-being, the long-term increase in life satisfaction we find is larger compared to the short-term increase in subjective well-being if one's first child is born, which amounts to 4.3% for Germany (Myrskylä and Margolis, 2014). This finding is reinforced by estimation results of the 1976 *baby year* reform effect which was targeted at laterborn children, where we also find positive effects for laterborns. In this case, the effect amounts to 25.6% of a standard deviation and 6.2% of the pre-reform mean. Given this additional evidence it seems very unlikely that birth order-specific heterogeneous reactions to unobserved shocks are driving the effect on life satisfaction.

Our paper contributes to the literature in at least three different ways. First and foremost, to the best of our knowledge, this is the first study analyzing long-term effects of parental leave reforms on subjective well-being, although recent economic literature increasingly focuses on these measures (e.g., Kahneman and Krueger, 2006; Clark et al., 2008; Deaton and Stone, 2013; Benjamin et al., 2019). Specifically, we measure overall life satisfaction when individuals are 19 to 37 years old, that is, we investigate whether extending parental leave in the first year of life has a long-term impact.

Second, we evaluate a historical setting in which the extension of paid parental leave indeed significantly changed the care situation of very young children, who shared a very similar counterfactual center-based care situation since most women and mothers were in (full-

time) employment. Common methodological challenges of previous parental leave studies are both the potential selection of women and mothers into the labor market as well as the lack of a distinct counterfactual scenario to maternal care. For example, when studying parental leave reforms in West Germany and Austria (e.g., Dustmann and Schönberg, 2011; Danzer and Lavy, 2017; Huebener et al., 2018; Fabel 2021), only a quite limited number of women and mothers were employed before any parental leave reform was implemented. This implies that the care situation changed only for a small selective group of children. In countries with a high share of female labor force participation, e.g., in Norway and the US, the demand for different childcare arrangements varies a lot by parental background (e.g., Carneiro et al., 2011; Bullinger, 2019). Here, parental leave reforms could imply a change from low-quality informal or formal care to maternal care for some children — as well as a change from high-quality formal care to maternal care for others.

Third, we implement a DiD-approach making use of the different parental leave eligibility criteria of firstborn in contrast to laterborn children as a quasi-experimental setting. The common trend assumption only requires a common trend in outcomes of firstborn versus laterborn children in absence of the reform, a scenario which seems plausible, as we show in section 4. We are thus able to causally identify the parental leave effect without strong assumptions regarding common trends across states or other regions. Concerns about different trends between firstborns and laterborns are furthermore mitigated by finding large and positive effects of the parental leave extension independent of firstborns (1986) or laterborns (1976) being targeted by the reform.

The remainder of this study is structured as follows. Section 2 describes the background and institutional setting of former East Germany, in which the *baby year* reforms took place. Sections 3 and 4 introduce the empirical approach and our data, respectively. Section 5 presents our empirical results for the effect of the 1986 reform on long-term life satisfaction, sensitivity and effect heterogeneity analyses. Section 6 shows estimation results for the effect of the 1976 reform on long-term life satisfaction. We discuss our findings in section 7. Section 8 concludes our study.

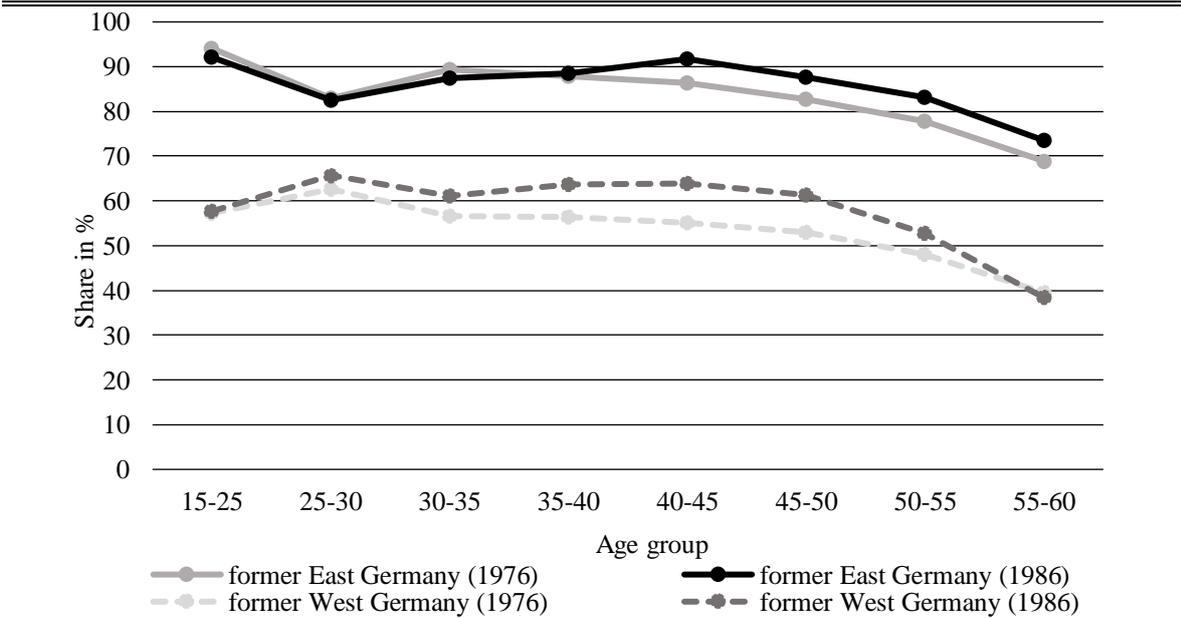
## **2 Background and institutional setting**

### **2.1 The pre-reform scenario of the baby year reforms in 1976 and 1986**

In socialist countries such as former East Germany (the German Democratic Republic), gender equality was said to be acquirable only through women's participation in the labor

market. Women were therefore encouraged by the government to participate in higher education and in the labor market. However, until the 1950s mostly single women were employed in former East Germany. From the 1960s on, the government needed to increase female labor market participation due to economic circumstances<sup>6</sup>, and increasingly targeted married women and mothers as well. Various policies (e.g., law on maternity and child protection of 1950, family code of 1965, law on abortion of 1972) were introduced to promote women’s rights and the ‘working women’, which led, compared to other countries, to high female labor force participation rates (Obertreis, 1986): excluding trainees and university students, the average share of female labor force participation was as large as 83.8% in 1979 and 85.8% in 1989 (Winkler, 1990), compared to 53.6% in 1979 and 58.0% in 1989 in former West Germany (OECD, 2019).

Figure 1:  
*Female employment rates in % per age group in former East and former West Germany, 1979 and 1989*



Notes: Employment rates in % per age group of working-aged women — both full- and part-time employed — in former East and West Germany for the years 1979 and 1989. The shares do not include women in education or training.

Source: Winkler (1990), OECD (2019).

Figure 1 shows the high female labor force participation rates in former East Germany compared to former West Germany throughout all age cohorts. The highest share is reached for

<sup>6</sup> 45% of all manufacturing plants in former East Germany were destroyed in the Second World War, compared to 20% in former West Germany. Also, former East Germany had to pay roughly 25bn German Mark as war reparations to the USSR. The working aged population decreased from roughly 12m individuals in 1949 to 10.5m individuals in 1960 due to a large number of people who moved to former West Germany before the building of the Berlin Wall in 1961 (Obertreis, 1986).

women aged 15 to 24 with 94.1% in 1979. Notably, the average age of women when the first child was born was 22.4 years. About 75% of female employees were employed full-time. Full-time employment among mothers was even higher, namely up to 85%. Most mothers went back to work after roughly 20 weeks of giving birth, when maternal leave ended (Israel and Kerz-Rühling, 2008).

Just like female labor force participation, public childcare availability in former East Germany, especially for very young children, was large compared to other countries. In 1986, childcare was available for more than 80% of all infants and toddlers (Statistisches Jahrbuch der Deutschen Demokratischen Republik, 1988).<sup>7</sup> Almost 90% of all newborn to three-year-old children attending childcare centers were enrolled on a full-time basis, as childcare on a half-day basis was uncommon in former East Germany (Helwig, 1987; Braun and Klein, 1995). In contrast to former West Germany, informal care by nannies was extremely rare and most grandparents, neighbors or friends of mothers were working full-time as well, so they were not able to regularly help mothers caring for the children (Zwiener, 1994).

## **2.2 The *baby year* reforms in 1976 and 1986**

As in many industrialized countries, fertility in former East Germany started decreasing sharply in the late 1960s. To counteract this development, former East Germany introduced the *baby year* reform in 1976. This reform granted mothers with already at least one child one year of job-protected paid parental leave (see Figure 2). It allowed mothers to stay at home with their second (or third, ...) child for twelve months instead of five months after birth while being paid 80% of their pre-birth wage. After the 1976 reform, laterborn children were not enrolled in public childcare with five months of age, but with twelve months of age. In contrast, firstborn children were still enrolled at an age of five months.

As a response to the 1976 *baby year* reform, short-term fertility increased temporarily, but further declined after 1980. As a reaction, the *baby year* was extended to all mothers independent of a mother's number of children. The new reform was announced in mid-April and implemented on the first of May, 1986 (see Figure 2). It applied to all mothers of children who were younger than one year of age by the 1<sup>st</sup> of May, 1986.<sup>8</sup> As a result, parents of

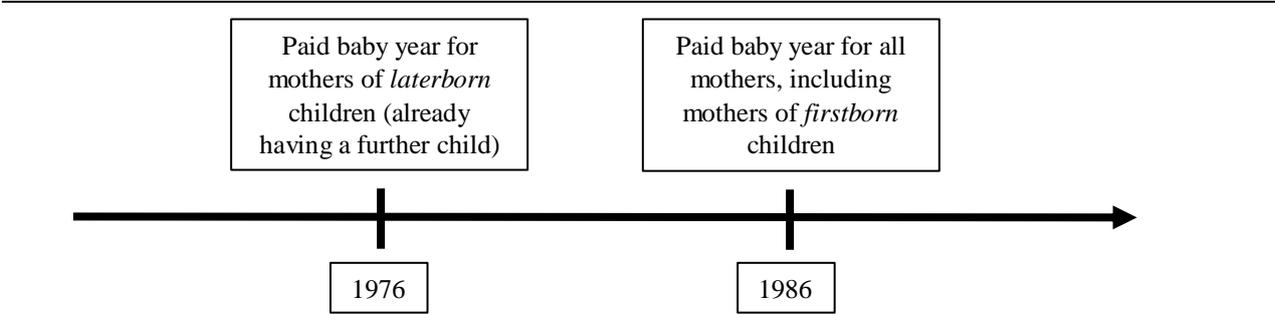
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<sup>7</sup> Data on enrolment rates of newborn to one-year-olds is unfortunately not available for former East Germany. However, childcare attendance in 1986 was supposedly as large as childcare attendance in Western countries nowadays (e.g., Denmark had the highest enrolment rates in 2016 in early childhood education and care services of newborn to two-year-olds with 62% and Belgium as well as Iceland with 60% (OECD, 2018a).

<sup>8</sup> For example, mothers with a firstborn child aged ten months on the 1<sup>st</sup> of May, 1986, could apply for two months of leave. However, we define all individuals who were born on or after the 1<sup>st</sup> of January, 1986, as being assigned to the reform due to the following reasons. First, when mothers returned to work after maternal leave, it should be

firstborns became eligible for one year of job-protected paid parental leave as well (Braun and Klein, 1995). Consequently, from 1986 on, firstborns were also eligible to spend the entire first year of life with their mother instead of only the first five months.<sup>9</sup>

Figure 2:  
Former East Germany’s paid baby year reforms in 1976 and 1986



Source: Own illustration.

Data show that up to 95% of eligible mothers made use of the *baby year* in the late 1980s (Hoeckner, 1995). Thus, the introduction of the *baby year* in 1986 changed the most common care mode for firstborn children aged five to twelve months from childcare centers to maternal care. As a result, in 1988, only 1% of all children below the age of one year were still cared for in childcare centers (Israel and Kerz-Rühling, 2008).<sup>10</sup> Interestingly though, in terms of fertility responses, the reform in 1986 was found not to be effective (Buettner and Lutz, 1990).

**2.3 Counterfactual care mode**

The alternative care mode to parental care within the first year in a child’s life in former East Germany was public formal care. Centers were supervised by the health ministry. Available were daycare centers (97.7% of all available places in 1989), which were open from

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less likely that she dropped out of the job again. Second, we make sure that all assigned individuals faced a parental leave period as long as roughly seven months. In a sensitivity analysis we define assigned individuals as children born after the 1<sup>st</sup> of June, 1985, and results stay similar.

<sup>9</sup> Strictly speaking, there was no formal paid parental leave so far. Mothers were only entitled to 20 weeks (i.e., five months) of paid maternal leave. However, due to reasons of simplicity, we refer to the reform setting as an extension of paid parental leave from five to twelve months, although it was a change from five months of paid maternal leave to twelve months of paid parental leave.

<sup>10</sup> It might be argued that mothers self-selected into applying for the *baby year* (mothers who were able to afford 20% less income). However, the framework of high female labor force participation rates before and after the reform on the one hand and high childcare attendance before and low attendance after the reform on the other hand makes us confident that there was no self-selection. Another foundation for this assumption is the unmatched equality of income in former East Germany: the Gini coefficient was as small as 0.22 in 1980 and 0.20 in 1987 (West German states: 0.32) (Galbraith et al., 2017). This implies a comparatively small income inequality and thus a low probability that a specific group of mothers selected themselves into applying for the *baby year*.

six a.m. to six p.m. on weekdays; week-care centers, which opened 24 hours on weekdays (1.3% of all available places), and children's homes (1% of all available places). Most childcare centers were public and operated by municipalities, about 5% were operated by state-owned enterprises. There were no independent and almost no church-operated childcare facilities, nannies or other informal care modes (Zwiener, 1994).

Since childcare centers were required to adhere to strict regulations, quality did not vary significantly across the centers, guaranteeing similar conditions for all children. The focus of the childcare centers' programs supervised by pediatricians was to foster children's health and to enhance cognitive development. Children were meant to have the opportunity to play, learn to handle everyday situations and develop positive relations with teachers and other children. The aim of these measures was to encourage the development of senses, perception, cognitive activity and language acquisition, and moral values (Zwiener, 1994). All childcare teachers underwent a three-year theoretical and practical training at a medical vocational school and specialized in subjects such as early childhood education, psychology, and pediatrics. This provides evidence that childcare centers' staff quality was rather high and comparable to standards as they are nowadays in Germany. However, staff quality was lower than it is today in centralized care in France, Sweden or the USA (see Table A.1). Also, group sizes varied between five or six children per teacher in daycare centers for children under three years, which is considered rather large, suggesting smaller child orientation than it is common in daycare centers nowadays. Tietze and Foerster (2005) suggest a minimum standard of one teacher per three children below one year of age when the daily care duration is more than nine hours, as it was mostly the case in former East Germany (Zwiener, 1994).

## **2.4 Mechanism**

With the introduction of the *baby year*, public childcare was substituted by maternal care for children aged five to twelve months. Given the literature on related settings and outcomes it seems very plausible that more time with the mother in the first year of life has an impact on children's life satisfaction. For example, Berger et al. (2005) find an increase in problematic behavior in children when mothers return to work within twelve weeks after giving birth. Further empirical studies find that parental bonding and minimized stress factors is important for children's development (e.g., Kottelenberg and Lehrer, 2014; Beeghly et al., 2017; Golding and Fitzgerald, 2017; Schore, 2017).

As we stated in the introduction, economics literature has been neglecting potential effects of parental leave reforms on long-term life satisfaction so far. However, our paper is

related to an array of literature in the field of developmental psychology that investigates the link between early parent-infant bonding and socio-emotional well-being in adulthood. Building on the seminal contributions by Bowlby (1969) and Ainsworth (1970), this strand of literature emphasizes that early parental attachment influences socio-emotional functioning and relationship patterns measurable throughout the life span. Schore (2001) finds that the emotion-processing limbic part of the brain is particularly malleable during early infancy by emotionally charged attachment experiences. Consequently, dimensions of emotional behavior related to the limbic system, such as emotional self-regulation, stress coping, resilience and adaptivity, are likely to be affected by experiences of early parent-infant bonding (Malekpour 2007).

Building upon this literature we expect a positive impact of the *baby year* reforms on long-term life satisfaction. Being together with the primary caregiver for the first twelve instead of five months in life is likely to lead to more positive attachment experiences than being cared for in rather large groups in childcare centers from the age of five months on.

### **3 Empirical approach**

The setting under study facilitates identifying the reform effect on long-term life satisfaction for the following reasons. First, in former East Germany the female employment rate was high, childcare was standardized full-day care for most children aged up to six years, and there was almost no quality variation across available childcare facilities and thus no selection of specific groups of children into particular forms of childcare. Second, the *baby year* reforms in 1976 and in 1986 guaranteed all mothers one year of paid parental leave, and we know that almost all mothers made use of it. Third, the sequential introduction of the reforms provides a quasi-experimental setting which we evaluate with a DiD-approach.

In our main analysis we focus on the reform in 1986 which granted the *baby year* also to firstborn children (while laterborn children were granted the *baby year* already from 1976 on). We compare the difference in the outcomes of firstborns born before and after the reform in 1986 with the difference in the outcomes of laterborns born before and after the reform in 1986. In our secondary analysis, we focus on the *baby year* reform in 1976 and inversely estimate the *baby year* effect on laterborn children who were targeted by the reform, while firstborns were not (see Figure 3).

Importantly, the implementation of the reforms in 1976 and 1986 was unexpected. Therefore, we can rule out that the timing of birth was affected by the reform. For the analysis of the 1986 reform effect, we restrict the sample to individuals born between 1980 until 1991 to be far enough from the 1976 reform and close enough to the 1986 reform. We also do

robustness checks reducing the number of birth cohorts under study, which, however, does not lead to major changes in the results. For the analysis of the 1976 reform effect, in turn, we restrict the sample to individuals born between 1966 and 1984.

Figure 3:  
*The former East Germany's baby year reforms: treatment and control groups*

Main analysis (baby year reform of 1986)		
	Treatment group (intention-to-treat)	Control group
Firstborn children	✓	✗
Laterborn children	✗	✓

Secondary analysis (baby year reform of 1976)		
	Treatment group (intention-to-treat)	Control group
Firstborn children	✗	✓
Laterborn children	✓	✗

Source: Own illustration.

A further important issue is the intention-to-treat-nature of our estimates, since we do not know from the survey data if mothers made use of the paid parental leave and whether mothers were employed before birth. However, given the institutional setting of former East Germany (high female labor force participation, standardized full-day childcare, take-up rate of the *baby year* of up to 95%), it is very likely that a large majority of eligible children were treated by the reform.<sup>11</sup>

The 1986 *baby year* reform is the preferred setting for our analysis, since firstborn children were granted the *baby year*. This implies that we do not face problems of spillovers on older siblings (see Bettinger et al. 2014, who look at medium-term educational outcomes of school-aged children whose younger siblings are subject to a parental leave reform in Norway). The 1976 *baby year* reform, targeted at laterborns, could have indirect effects on older siblings and by that influence the control group of firstborns, which would downward-bias the DiD estimates.

<sup>11</sup> Unfortunately, there is no data available which would allow us to analyze maternal labor supply effects of the reform. Available data on former East German women's labor supply does not contain information on the age of women's children.

Yet, when evaluating the 1986 *baby year* reform, comparing first- and laterborn children might be also challenging, since we assume that any unobserved events had the same impact on firstborns and laterborns. For example, we assume that the Chernobyl disaster had the same impact on firstborn and laterborn children’s health. However, it might be the case that parents have had more resources to compensate for potential disadvantages that might occur due to Chernobyl when having only one child, or that they give more resources to their firstborn child to compensate for potential disadvantages. Also, the 1986 *baby year* reform is very close to the date of the fall of the Berlin Wall in 1989 and the German reunification in 1990. There was turmoil in former East Germany even before these events took place, which might have influenced child investment decisions — e.g., regarding education — of parents. We assume that already-born children were affected equally by these developments regardless of their birth order and their number of siblings.

We can mitigate any concerns about these assumptions by analyzing effects of the 1976 *baby year* reform on children’s long-term life satisfaction. If we find similar reform effects for second-born children in this setting — even though analyzing a different decade and children of a different birth order — this would reassure us that potential effects are caused by the respective *baby year* reform.

For applying a DiD-approach in our analysis, we must be sure that the trend of life satisfaction of firstborn and laterborn children would have been the same in absence of the reform. Graphical evidence for the common trend is shown in Figure 3. Furthermore, we are unaware of any other family policy or labor market reform during the time under study. In any case, potential reforms would have occurred at the state level and would thus be captured by birth cohort fixed effects. However, as explained above, potential other reforms might have had different effects on firstborn and laterborn children. Therefore, we also estimate the 1976 reform effect.

In our main analysis, we estimate the following DiD-specification:

$$(1) \ y_{ij} = \lambda(Firstborn)_{ij} \times (Postreform)_{ij} + \beta_1(Firstborn)_{ij} + \beta_2(Postreform)_{ij} + \pi_j + \mu X_i + c + \epsilon_{ij} .$$

$y_{ij}$  is the measure for long-term life satisfaction in later adult life of child  $i$  belonging to birth cohort  $j$ . The interaction effect between  $(Firstborn)_{ij}$  and  $(Postreform)_{ij}$  identifies all firstborn children whose mothers were affected by the *baby year* reform and thus eligible to one year of paid parental leave with their firstborn child.  $\lambda$  is the coefficient of interest and

measures the intention-to-treat effect.  $\pi_j$  measures birth year fixed effects.  $(Firstborn)_{ij}$  is a dummy indicator for children who are firstborns and the coefficient  $\beta_1$  captures all possible permanent and general differences between children who were born as firstborns or as laterborns.  $(Postreform)_{ij}$  is a dummy indicator for children who were born on or after the 1<sup>st</sup> of January 1986, and the coefficient  $\beta_2$  captures the difference of laterborn children between the pre- and post-reform period. Additionally, the vector  $X_i$  contains individual and parental control variables.  $c$  is the constant, and  $\epsilon_{ijm}$  is the heteroskedasticity-robust error term. We estimate the differences in outcome changes before and after the reform between firstborns and laterborns:

$$(2) \hat{\lambda} = (\bar{y}_{Firstborn,post} - \bar{y}_{Firstborn,pre}) - (\bar{y}_{Later-born,post} - \bar{y}_{Later-born,pre}).$$

Equation (2) displays the difference in average long-term life satisfaction of children born after the reform versus before, whose mothers were eligible for the reform (firstborns) less the difference in outcomes of children born before and after the reform who were not subject to the reform (laterborns).

When we evaluate the *baby year* reform in 1976, equation (1) changes to its mirror image. Since laterborns were the targeted group in 1976, the equation is the following:

$$(3) y_{ij} = \lambda(Laterborn)_{ij} \times (Postreform)_{ij} + \beta_1(Laterborn)_{ij} + \beta_2(Postreform)_{ij} + \pi_j + \mu X_i + c + \epsilon_{ij}.$$

When analyzing the effect of the 1976 *baby year* reform, the coefficient of interest is the following:

$$(4) \hat{\lambda} = (\bar{y}_{Laterborn,post} - \bar{y}_{Laterborn,pre}) - (\bar{y}_{Firstborn,post} - \bar{y}_{Firstborn,pre}).$$

## 4 Data and descriptive statistics

### 4.1 Data

Our analysis is based on the German Socio-Economic Panel (SOEP), which is a representative sample of the population in Germany (Goebel et al., 2019). The SOEP includes all necessary information to estimate causal effects of the prolonged parental leave on children's life satisfaction. To measure long-term outcomes, we restrict our main sample to adults around

the age of 28 years. Individuals included in the analysis are those for whom we have information on birth place, birth year, and birth-order.<sup>12</sup> Note that firstborn and laterborn children in our sample are not members of the same family. In the main analysis, we draw upon the SOEP waves of 2008 to 2018 and observe individuals who were born in former East Germany between 1980 and 1991 to cover a sufficient timeframe around the reform of 1986. For the secondary analysis, we draw upon the SOEP waves 1991 to 2018 and observe individuals who were born in former East Germany between 1966 and 1984.

*Treatment:* Our intention-to-treat variable is unity for all firstborn children who were born on or after the 1<sup>st</sup> of January 1986. Before the *baby year* reform in 1986, firstborn children were the sole group of individuals who could only spend their first 20 weeks with their mother instead of the entire first year of life. However, this changed with the 1<sup>st</sup> of May 1986. The intention-to-treat variable takes the value zero for all other groups of individuals. Before the *baby year* reform in 1976, firstborn and laterborn children could spend their first 20 weeks with their mother. This changed for laterborn children in May 1976, when the first *baby year* reform was introduced. Thus, in our secondary analysis, our intention-to-treat variable is unity for all laterborn children born between 1976 and 1984.<sup>13</sup> The intention-to-treat variable takes the value zero for all other groups of individuals.

*Life satisfaction:* This variable is a self-assessment variable in the SOEP and varies between zero and ten (11-point scale), where zero is fully unsatisfied and ten is fully satisfied.<sup>14</sup> Criticism on using individual well-being measures<sup>15</sup> is twofold. It concerns first reliability of life satisfaction measures and second the significant variation in life satisfaction with age (see, for example, Baird et al., 2010). However, Kroh (2006) shows that using the 11-point scale in a computer assisted personal interview (CAPI), as it is done in the SOEP, maximizes data quality in terms of validity and reliability. Furthermore, we check marginal effects of an ordered probit regression and results remain significantly different from zero, although the coefficient decreases in size (see Table 3). To tackle the second concern, Figure A.2 in the appendix displays life satisfaction scores of all individuals in the SOEP for the age groups 19 to 37. Life

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<sup>12</sup> To include a large number of individuals in our sample, we select information on siblings from all available variables. Information on birth order is available for more individuals than information on the number of siblings. This results in a smaller sample when controlling for number of siblings or being an only child. Note that information on siblings' birth order is crucial for our identification strategy. This is one reason why we cannot use administrative datasets, which lack this information.

<sup>13</sup> We restrict the sample in the secondary analysis to individuals born before the *baby year* reform in 1986 was implemented. Therefore, we use 1984 as a cut-off year.

<sup>14</sup> The question taken from the SOEP is shown in Figure A.1.

<sup>15</sup> Criticism of Bond and Lang (2019) regarding recent happiness literature using few ordered categories (e.g., "very happy", "pretty happy" and "not too happy") does not apply to our study, since these categories differ rigorously from the 11-point scale of measuring life satisfaction in the SOEP.

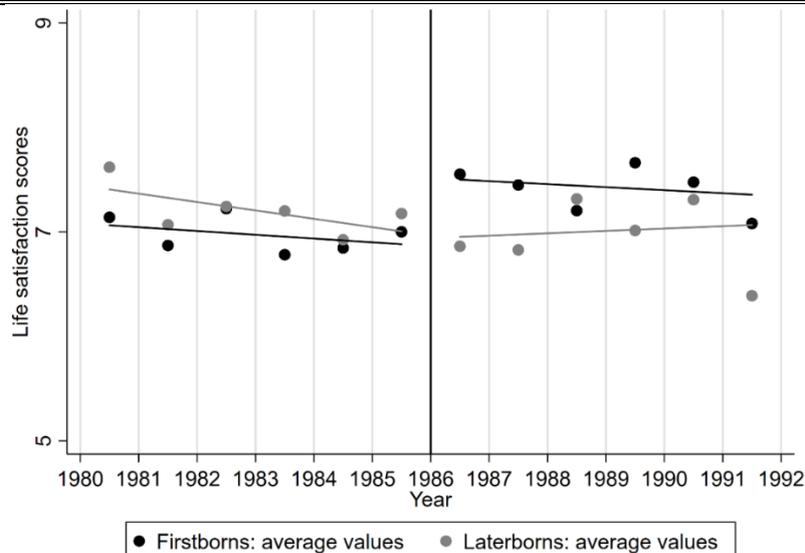
satisfaction varies only slightly with a minimum of 7.2 at age 28 and a maximum of 7.5 at age 33 in the age groups we observe. Many variables are observed more than once for each individual. This applies also to life satisfaction, yet, we include each individual just once in our analysis. To make sure that the observed individuals are as close in age as possible, and to measure long-term effects, we choose age 28 as the starting age to select observations. For individuals who we do not have information for when they were aged 28, we check observation availability in both directions and fill the variable with the nearest available information (following with age 29, 27, 30, 26, etc.). Using this technique, most observed individuals are 28 years old or very close to this age. However, results are robust if we calculate the average of life satisfaction over all available data points, although the coefficient decreases in size (see Table 3).

*Control variables:* We consider the following controls on the individual level: year of birth (cohort fixed effects), a dummy for male gender and a dummy whether an individual grew up in an urban region. In sensitivity analyses, we additionally use controls on the individual level for the current region of living, number of siblings and whether the observed individual is an only child. As control variables on family background, we consider mother's and father's highest education (defined by unity if having achieved the German high-track degree or a university degree, and zero if otherwise). In sensitivity analyses, we modify these variables with information on whether the mother and the father are academics. As another robustness test, we use these controls at the household level.

## **4.2 Summary statistics**

A descriptive graphical illustration for life satisfaction scores in our sample is provided in Figure 3. The figure plots the raw variable means by year of birth to the left and to the right of the reform cut-off date (1986, vertical line) for both firstborn and laterborn children before and after the reform. Before the reform, laterborn individuals are more satisfied with life in adulthood than firstborns. We also see a common trend in life satisfaction scores before the reform. In the year of the reform, there is a jump in life satisfaction scores for firstborn individuals, but not for laterborns. In the years after, life satisfaction of firstborn individuals is higher than life satisfaction of laterborn individuals.

Figure 3:  
Local averages for life satisfaction before and after the baby year reform of 1986



Notes: The figure shows average values of life satisfaction plotted for firstborn (black) and laterborn (grey) individuals. In this descriptive analysis, we do not control for other variables. The vertical line indicates the time of the *baby year* reform on the 1<sup>st</sup> of May 1986. Assigned individuals are individuals born on January 1<sup>st</sup> 1986 or later.

Source: Socio-Economic Panel (SOEP), data for years 1984-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

Table 1 shows summary statistics for the sample used in our main analysis. The total number of individuals we observe is 1,298. 25,9% of individuals are assigned to the *baby year* reform (336 individuals). In the mean, firstborns in our sample score almost the same in life satisfaction as laterborns. They are slightly older and more often an only child/have less siblings than laterborns. They also grew up more often in urban areas and have higher educated parents. Individuals assigned to the *baby year* reform score, in the mean, higher in life satisfaction, are younger, and are more often an only child/have less siblings. They also grew up more often in urban areas and have less educated parents than not assigned individuals.

Table 1:  
*Summary statistics for firstborns and laterborns, assigned and not assigned individuals*

	Firstborn child			Laterborn child			Assigned			Not assigned		
	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.
<b>Panel A: Interaction variables</b>												
Firstborn child*born in 1986 or later	761	0.442	0.497	537	0	0	336	1	0	962	0	0
Firstborn child	761	1	0	537	0	0	336	1	0	962	0.442	0.497
Born in 1986 or later	761	0.442	0.497	537	0.533	0.499	336	1	0	962	0.297	0.457
<b>Panel B: Outcome variable</b>												
Overall life satisfaction	761	7.164	1.608	537	7.112	1.648	336	7.432	0.141	962	7.042	1.682
<b>Panel C: Control variables</b>												
Year of birth	761	1984,883	3.233	537	1985,6	3.216	336	1988,027	1.546	962	1984,182	3.088
Male	761	0.491	0.500	537	0.497	0.500	336	0.515	0.501	962	0.486	0.500
Survey year	761	2012,247	3.018	537	2012,367	2.889	336	2013,979	2.982	962	2011,709	2.726
Number of siblings	706	1,061	1.258	493	1,696	1.052	310	0,987	1.196	889	1,439	1.205
Only child	750	0.409	0.492	535	0.034	0.180	332	0.464	0.499	953	0.179	0.384
Grew up in urban area	761	0.707	0.455	537	0.656	0.476	336	0.723	0.448	962	0.673	0.470
Currently living in West Germany	761	0.276	0.447	537	0.279	0.449	336	0.274	0.447	962	0.279	0.449
Born before 1989	761	0.830	0.375	537	0.756	0.430	336	0.616	0.487	962	0.864	0.343
Mother has Abitur or higher education	761	0.238	0.426	537	0.225	0.418	336	0.208	0.407	962	0.241	0.428
Father has Abitur or higher education	761	0.262	0.440	537	0.240	0.428	336	0.214	0.411	962	0.266	0.442
<b>Panel C: Altered variable definitions</b>												
Overall life satisfaction (rule of selection: mean of all observations)	761	7.170	1.230	537	7.108	1.317	336	7.325	1.139	962	7.081	1.303
At least one parent has Abitur or higher education	761	0.292	0.455	537	0.274	0.446	336	0.253	0.435	962	0.295	0.456
Mother is an academic	761	0.154	0.361	537	0.1490	0.356	336	0.110	0.314	962	0.166	0.373
Father is an academic	761	0.147	0.355	537	0.136	0.343	336	0.116	0.321	962	0.152	0.359
At least one parent is an academic	761	0.233	0.423	537	0.212	0.409	336	0.188	0.391	962	0.237	0.425

Notes: Observations, means and standard deviations are displayed for firstborns, laterborns, assigned and not assigned individuals. The sample consists of birth cohorts 1980 to 1991 taken from SOEP waves 2008 to 2018. Assigned individuals are firstborns born in (former) East Germany on January 1<sup>st</sup> 1986 or later. The total number of individual persons we observe is 1,298. Source: Socio-Economic Panel (SOEP), data for years 1984-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

## 5 Empirical results – *baby year* reform of 1986

### 5.1 Effects of the reform on life satisfaction

Table 2 shows the results of our DiD design when applied to the 1986 *baby year* reform. The estimated coefficient for the baseline specification is reported in Table 2, column (1). It differs significantly from zero and has the size of 41.3% of the standard deviation.<sup>16</sup> Compared to the pre-reform level, life satisfaction increases by 9.3% of the pre-reform mean.<sup>17</sup>

Table 2:  
*Effects of the baby year reform 1986 on long-term life satisfaction of children*

	Baseline	Additional control variables			Effect heterogeneity	
	(1)	(2)	(3)	(4)	(5)	(6)
	Main specification	Currently living in West Germany	Number of siblings	Only child	Gender	Maternal education
Firstborn child*born in 1986 or later (interaction)	0.671*** (0.183)	0.663*** (0.183)	0.607*** (0.192)	0.709*** (0.186)	0.654*** (0.207)	0.532** (0.264)
Firstborn child	-0.246* (0.136)	-0.242* (0.136)	-0.197 (0.145)	-0.205 (0.144)	-0.247* (0.136)	-0.247* (0.136)
Born in 1986 or later	-1.123*** (0.371)	-1.111*** (0.369)	-1.060*** (0.390)	-1.169*** (0.368)	-1.123*** (0.372)	-1.122*** (0.372)
Interaction term*male					0.0329 (0.191)	
Interaction term*mother's education: less than Abitur						0.176 (0.246)
Constant	7.420*** (0.183)	7.354*** (0.187)	7.514*** (0.205)	7.410*** (0.186)	7.424*** (0.186)	7.415*** (0.183)
R-squared	0.057	0.061	0.063	0.061	0.057	0.058
Observations	1,298	1,298	1,199	1,287	1,298	1,298
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the DiD estimates resulting from an OLS regression with life satisfaction as outcome variable. Regressions include birth cohort fixed effects, individual controls such as gender, age and the survey year, as well as background controls such as maternal and paternal education and whether the individual grew up in an urban area. We also include dummies to control for missing observations for following variables: grew up in urban area, mother's and father's education. The number of missing observations is, however, small (between 1% for grew up in an urban area and 7% for father's education). The sample consists of birth cohorts 1980 to 1991 taken from SOEP waves 2008 to 2018. 336 of 1,298 individuals are assigned to the reform, 962 are not. Robust standard errors are reported in parentheses. Levels of significance: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Source: Socio-Economic Panel (SOEP), data for years 1984-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

<sup>16</sup> The calculation is as follows: The coefficient shown in Table 2 column (1), first row, divided by the standard deviation of life satisfaction in Table A.2, Panel B, gives the named result (0.671/1.624=41.3%).

<sup>17</sup> The pre-reform life satisfaction mean is 7.241. The point estimate of life satisfaction is 0.671, which thus gives the named result (0.671/7.241=9.3%).

In relation to life event impacts on subjective well-being, the long-term increase in life satisfaction we find is larger compared to the short-term increase in subjective well-being if one's first child is born, which amounts to 4.3% for Germany (Myrskylä and Margolis, 2014).

Our baseline specification does not include measures that could change in reaction to being exposed to prolonged maternal care due to the *baby year* reform. We additionally include these variables in columns (2) to (4) of Table 2. The estimated coefficient decreases only marginally after controlling for the current region of living (column 2). When controlling for the number of siblings (column 3), the estimated coefficient decreases in size, but the standard error increases slightly. The results remain significantly different from zero. Controlling for being an only child (column 4) increases the estimated coefficient.

We also enhance the model of our baseline specification by interacting the treatment variable with the individual's gender (column 5) and maternal education (column 6). There are no significant differences between the effect by gender, nor by maternal education.

## 5.2 Robustness tests

In Table 3, results of robustness tests are displayed. First, we restrict our sample (columns (1)-(2)). In column (1), we exclude individuals who were born in 1986. We do this since there might be a slight probability that mothers postpone birth by a few days or weeks to be eligible to the reform. Also, there might have been difficulties in the first months of implementing the reform or applying for parental leave afterwards. Lastly, mothers of children born in the second half of 1986 might already have had a place in a childcare center, and did not want to withdraw due to social or work-related reasons. In this test, results remain fairly similar, although the coefficient decreases slightly in size. Column (2) shows results of a robustness test where our sample includes individuals born before 1989 only, since due to the closeness of their birthday to the German reunification there might be potential differences between them and other children. Results remain significantly different from zero, but the coefficient decreases in size and the standard error increases.

In column (3), we estimate the coefficient by using a pseudo reform date (May 1982).<sup>18</sup> For this estimation, we include individuals born between 1978 and 1984, to be not too close to both reforms in 1976 and 1986. Reassuringly, the estimated coefficient is not significantly different from zero.

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<sup>18</sup> This date was chosen, so that the placebo date is relatively far away from the reforms in 1986 and 1976.

Column (4) shows results when we use another rule to select observations for life satisfaction. In this specification, we calculated life satisfaction scores by using the average of all available observations in the dataset for each individual. Results imply a roughly half as large reform effect compared to our main specification.

In columns (5) to (7), we substitute family background controls by using different variables (either education on household level, or using a dummy whether parents are academics). All results remain fairly similar when comparing them to our baseline scenario.

In column (8), we define assigned individuals as being born in June 1985 or later. We conduct this robustness test since all mothers were eligible for the *baby year* if they had a firstborn child still younger than one year by the 1st of May, 1986. Results remain significantly different from zero, but the coefficient decreases slightly in size. Column (9) shows results of an ordered probit regression. Although the estimated coefficient decreases in size, results remain significantly different from zero.

Table 3:  
Robustness checks for the 1986 baby year reform

	Sample restriction		Placebo test	Observation selection	Altered variable definitions			Assigned individuals	Regression type
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Without individuals born in 1986	Only individuals born before 1989	Fake reform date 1982	Average of all existing observations	At least one parent is a high-track graduate	Mother and father are academics	At least one parent is an academic	Assigned from June 1985	Ordered probit regression
Firstborn child*born in 1986 or later (interaction)	0.613*** (0.193)	0.581*** (0.208)	-0.151 (0.261)	0.392*** (0.145)	0.670*** (0.183)	0.661*** (0.182)	0.655*** (0.182)	0.647*** (0.187)	0.444*** (0.119)
Firstborn child	-0.253* (0.136)	-0.249* (0.136)	-0.0998 (0.166)	-0.118 (0.105)	-0.249* (0.136)	-0.235* (0.136)	-0.236* (0.136)	-0.259* (0.143)	-0.162* (0.085)
Born in 1986 or later	-1.099*** (0.380)	-0.497* (0.285)	-0.165 (0.345)	-0.452** (0.230)	-1.108*** (0.372)	-1.051*** (0.376)	-1.054*** (0.372)	-0.333 (0.349)	-0.753*** (0.230)
Constant	7.456*** (0.185)	7.465*** (0.187)	6.939*** (0.236)	7.353*** (0.137)	7.411*** (0.183)	7.398*** (0.185)	7.397*** (0.184)	7.437*** (0.185)	.
R-squared	0.055	0.060	0.067	0.026	0.056	0.057	0.059	0.057	0.018
Observations	1,180	1,038	765	1,303	1,298	1,298	1,298	1,295	1,298
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the DiD estimates resulting from an OLS regression with life satisfaction as outcome. Regressions include birth cohort fixed effects, individual controls such as gender, age and the survey year, as well as background controls such as maternal and paternal education and whether the individual grew up in an urban area. The sample consists of birth cohorts 1980 to 1991 taken from SOEP waves 2008 to 2018. Robust standard errors are reported in parentheses. Pseudo R-squared is reported for the ordered probit regression. Levels of significance: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Source: Socio-Economic Panel (SOEP), data for years 1984-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

## 6 Empirical results – *baby year* reform of 1976

To check whether our results are robust in a broader sense, we conduct a second analysis with the *baby year* reform of 1976 as scenario. We – inversely to the approach in Section 5 – estimate the *baby year* effect on laterborn children who were targeted by the reform, while firstborns were not (summary statistics are shown in Table A.3). Table 4 shows regression results for this analysis.

Table 4:  
*Long-term effects of the 1976 baby year reform on long-term life satisfaction of children*

	Baseline	Additional control variables	Sample restriction	Heterogeneity		
	(1)	(2)	(3)	(4)	(5)	(6)
	Main specification	Currently living in West Germany	Number of siblings	Leave out individuals born in 1976	Gender	Mother's education
Laterborn child*born in 1976 or later (interaction)	0.429*** (0.155)	0.425*** (0.155)	0.328* (0.176)	0.453*** (0.157)	0.511*** (0.170)	0.362* (0.219)
Laterborn child	-0.409*** (0.122)	-0.402*** (0.122)	-0.257* (0.141)	-0.407*** (0.122)	-0.408*** (0.122)	-0.408*** (0.122)
Born in 1976 or later	-0.865** (0.361)	-0.846** (0.359)	-1.044** (0.468)	-0.878** (0.365)	-0.865** (0.361)	-0.872** (0.363)
Interaction*male					-0.168 (0.171)	
Interaction*low-educated mother						0.0882 (0.201)
Constant	6.842*** (0.619)	6.819*** (0.621)	6.938*** (0.317)	6.881*** (0.620)	6.840*** (0.618)	6.823*** (0.616)
R-squared	0.047	0.048	0.051	0.045	0.047	0.047
Observations	2,333	2,333	1,893	2,209	2,333	2,333
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays DiD estimates of an analysis of the 1976's baby year reform. Results stem from an OLS regression with life satisfaction as outcome variable. Regressions include birth cohort fixed effects, individual controls such as gender, age and the survey year, as well as background controls such as maternal and paternal education and whether the individual grew up in an urban area. We also include dummies to control for missing observations for following variables: grew up in urban area, mother's and father's education. The number of missing observations is, however, small (between 3.8% for grew up in an urban area and 6.3% for father's education). 497 of 2,333 individuals are assigned to the reform, 1,836 are not. Robust standard errors are reported in parentheses. Levels of significance: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Source: Socio-Economic Panel (SOEP), data for years 1991-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

The estimated coefficient for life satisfaction differs significantly from zero and has the size of 25.6% of the standard deviation. Compared to the pre-reform level, life satisfaction increases by 6.2% of the pre-reform mean. Results are robust, although the coefficient decreases

in size when we include other control variables in columns (2) and (3). Comparing these results to our estimates of the reform of 1986, the effect size is nearly halved, but still significant in size. When we leave out individuals born in 1976 to account for mothers' potential delay in births, effects do not change in comparison to the main specification in column (1). Columns (5) and (6) show heterogeneity analyses. We can rule out significant heterogeneous effects of the *baby year* by gender and maternal education. All children seem to benefit similarly from spending their first twelve months together with their mother instead of only five months.

## 7 Discussion

Both of our analyses – the main analysis of the 1986 *baby year* reform targeted at firstborns, as well as the secondary analysis of the 1976 *baby year* reform targeted at laterborns – show that extending parental leave by seven months has a significant positive effect on long-term life satisfaction. Finding significant positive effects of the parental leave extensions in both settings suggests that it is indeed the extended parental leave which makes the difference — and not birth order-specific heterogeneous reactions to unobserved shocks driving the results.

The effect size of the 1986 *baby year* reform targeted at firstborns (41.3% of a standard deviation) is, however, larger than the effect size of the 1976 *baby year* reform targeted at laterborns (25.6% of a standard deviation). This could be due to the following reasons: First, the reform in 1976 targeted at laterborn children could have had positive spillovers on older siblings. In a scenario in which mothers stayed at home 12 months with a newborn child, older siblings could have benefitted from the additional time with their mothers (see, e.g., Bettinger et al., 2014, for a reform in Norway). Thus, the estimated effect of the 1976 reform on laterborn children's life satisfaction could be downward-biased. Importantly, the issue of positive spillovers on siblings does not apply to the setting of the 1986 *baby year* reform targeted at firstborn children. Being twelve months in parental leave with a firstborn child cannot cause spillovers on (unborn) siblings during the firstborn's first year of life.

A second potential reason lies in migration decisions. If families were more likely to move to West German states after the reunification when having only one child, this could have affected (firstborn) children positively. Literature shows that families are more mobile the less children they have (e.g., Fischer and Malmberg, 2001; Long, 1972). The 1986 *baby year* reform was very close to the German reunification date, and if families with only one child were more likely to move to West German states after the reunification, there might be a higher probability that the relocation to West German states influenced an individual's overall life satisfaction.

We therefore use information on an individual's current location (either West or East German state) as an outcome variable in an additional analysis. The estimation results in Table A.4 in the appendix show that there is no significant effect of the 1986 reform on an individual's current location. Thus, this channel is unlikely to drive our estimates of the 1986 *baby year* reform effect on life satisfaction.

A third reason might be an effect of the reform on having siblings. If families decided more or less often to have another child due to the reform, assigned firstborns might be more or less likely to grow up with siblings.<sup>19</sup> This might influence life satisfaction in two directions: on the one hand, children could be more satisfied with life due to positive experiences of having one or more siblings. On the other hand, they might be less satisfied with life by having more siblings due to higher competition in childhood with other children, a worse financial situation of parents, etc. A descriptive figure and estimations of a regression with the number of siblings as outcome variable can be found in Figure A.3 and Table A.4 in Appendix A. We find a negative effect of the reform on the number of siblings of children, and the effect is statistically different from zero (individuals who were subject to the *baby year* reform have 0.25 siblings less than individuals not subject to the reform). Still, if having less or no siblings increases life satisfaction, this might upward-bias the estimated effect. However, Figure A.3 suggests that the smaller number of siblings after 1986 seems to be more of an ongoing trend and less an effect of the 1986 *baby year* reform itself.

Finally, to relate our study to previous literature of parental leave effects on short- and medium-term personality, health and educational outcomes, we also look at these dimensions using our dataset (Table B.1 in Appendix B shows descriptive statistics of the variables used). Table B.2 in Appendix B shows that there are no pronounced long-term effects of the 1986 *baby year* reform on personality development, health, or schooling and labor-market related outcomes. Not finding any significant long-run effects on these dimensions suggests that the sizeable and positive effect of parental leave on life satisfaction is not driven by these channels. In contrast, it seems to be a direct long-run effect of the extended parental leave on individuals' subjective well-being.

## 8 Conclusion

In this paper, we study the effect of the former East Germany's 1976 and 1986 parental leave reforms, so-called *baby year* reforms, on long-term subjective well-being, measured by

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<sup>19</sup> Conrad et al. (1995) shows that there was just a very small, not sizable increase in total fertility rates in 1987. Fertility continued to decrease thereafter.

overall life satisfaction, of now-adult children. Each of the reforms extended paid parental leave by seven months for a certain group of mothers. As a result, mothers could stay at home with their baby for up to twelve instead of five months after birth. Because of the high labor force participation rate of women in former East Germany and standardized full-day care for most children, our evaluation of former East Germany's parental leave reforms does not face major problems of selection of women into the labor market and insufficient or heterogeneous childcare supply, which contrasts with many other studies on parental leave reforms.

To the best of our knowledge, subjective well-being has not been studied in parental leave literature as a potential outcome dimension so far. Yet, there are various reasons why subjective well-being of affected children is an important outcome when evaluating parental leave reforms. First, child psychology literature suggests that spending more time with the mother within the first year of life has a positive impact on a child's personality development (Bowlby 1969). Second, well-being has become an important outcome variable in recent economic literature (e.g., Kahneman and Krueger, 2006; Clark et al., 2008; Deaton and Stone, 2013; Benjamin et al., 2019). Third, literature shows that subjective well-being is an important and more direct way to measure welfare. Responses to questions about subjective well-being can also predict future behavior (Kahneman and Krueger, 2006).

We find that an extension of parental leave by seven months has positive effects on subjective well-being measured by life satisfaction. Overall life satisfaction scores of firstborns increase for the 1986 *baby year* reform by 41.3% of a standard deviation, which corresponds to an increase of the pre-reform mean score by 9.3%. Results are robust throughout a variety of sensitivity analyses. This finding is confirmed when we estimate the effect of the 1976 *baby year* reform on life satisfaction of laterborn children, although the effect size is smaller.

Our results are highly policy-relevant, as they add a new aspect to finding the right policy mix regarding childcare and parental leave policies, and might be especially of interest for countries with less than one year of paid leave.<sup>20</sup>

Importantly, though, the economic situation of former East Germany in the 1980s has to be taken into consideration when considering a cost-benefit analysis of the reforms. Several studies have shown the existence of labor hoarding in the decade before reunification (e.g., Akerlof et al., 1991; Dornbusch and Wolf, 1994). Therefore, it was probably no large trade-off

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<sup>20</sup> These include the following countries (state of 2018): Australia, New Zealand, Cyprus and Malta (18 weeks each), Belgium (32.3 weeks), Chile (30 weeks), France (42 weeks), Greece (43 weeks), Iceland and Ireland (26 weeks each), Israel (15 weeks), Italy (47.7 weeks), Luxembourg (37.3 weeks), Mexico (12 weeks), Netherlands, Spain and Turkey (16 weeks each), Portugal (30.1 weeks), Switzerland (14 weeks), United Kingdom (39 weeks), United States (0 weeks), and Costa Rica (17.3 weeks) (cf. OECD, 2018b).

for policy makers in former East Germany to have mothers back in the labor market seven months later than before. In today's industries, the trade-off between decreasing the labor force and increasing children's time with their parents is likely to be more cost-intensive than in former East Germany.

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## Appendix A

Table A.1:  
*Education of childcare teachers in selected countries*

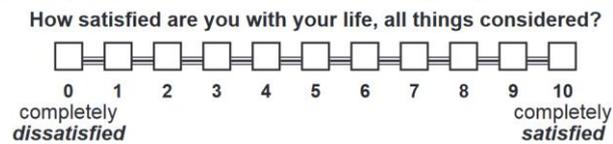
Country	Education of teachers
France	Master's degree
Former East Germany	Three years of post-secondary vocational training
Germany	Three years of post-secondary vocational training
Sweden	Bachelor's degree
USA	Bachelor's degree

Sources: Zwiener (1994) and OECD (2017).

Figure A.1:  
*Question asked in the SOEP, version 35, on life satisfaction (english version)*

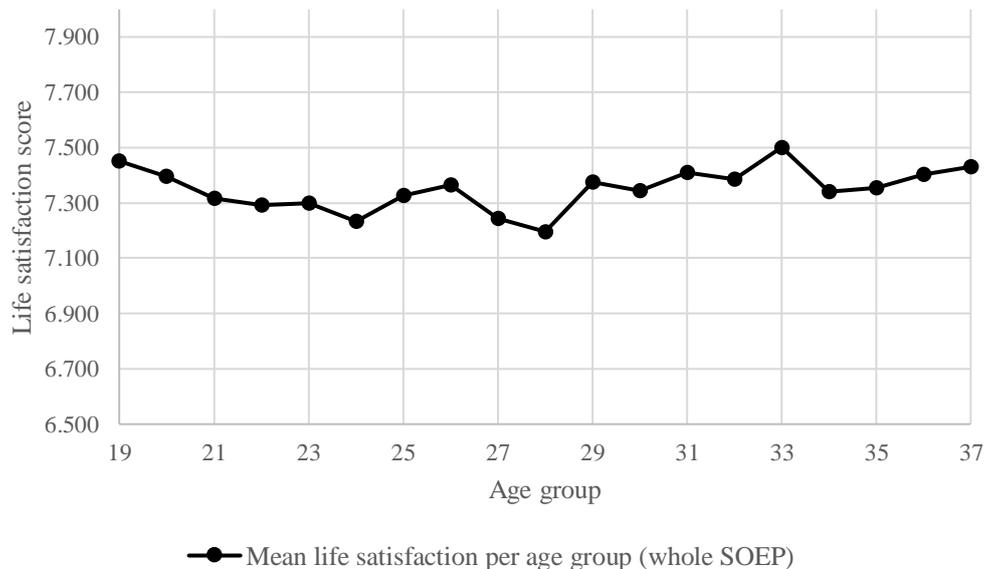
202. In conclusion, we would like to ask you about your satisfaction with your life in general.

 Please answer on a scale from 0 to 10, where 0 means completely dissatisfied and 10 means completely satisfied.



Source: Kantar Public (2018).

Figure A.2:  
*Mean life satisfaction scores in the SOEP for age groups 19 to 37*



Notes: The figure shows mean scores for life satisfaction for age groups 19 to 37. We calculated the scores based on all individuals in the SOEP. For selected age groups, the total number of observations is 43,093.

Source: Socio-Economic Panel (SOEP), data for survey years 1984-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

Table A.2:  
*Descriptive statistics of variables used in the analysis of the 1986 baby year reform*

	Observations	Mean	Std. dev.	Min.	Max.
<b>Panel A: Interaction variables</b>					
Firstborn child*born in 1986 or later	1,298	0.259	0.439	0	1
Firstborn child	1,298	0.586	0.493	0	1
Born in 1986 or later	1,298	0.479	0.500	0	1
<b>Panel B: Outcome variable</b>					
Overall life satisfaction	1,298	7.143	1.624	0	10
<b>Panel C: Control variables</b>					
Year of birth	1,298	1985,177	3.244	1980	1991
Male	1,298	0.494	0.500	0	1
Survey year	1,298	2012,297	2.965	2008	2018
Number of siblings	1,199	1,322	1.218	0	11
Only child	1,285	0.253	0.435	0	1
Grew up in urban area	1,298	0.686	0.464	0	1
Currently living in West Germany	1,298	0.277	0.448	0	1
Born before 1989	1,298	0.800	0.400	0	1
Mother has Abitur or higher education	1,298	0.233	0.423	0	1
Father has Abitur or higher education	1,298	0.253	0.435	0	1
<b>Panel D: Altered variable definitions</b>					
Overall life satisfaction (rule of selection: mean of all observations)	1,303	7.145	1.265	0	10
At least one parent has Abitur or higher education	1,298	0.284	0.451	0	1
Mother is an academic	1,298	0.152	0.359	0	1
Father is an academic	1,298	0.143	0.350	0	1
At least one parent is an academic	1,298	0.224	0.417	0	1

Notes: This table displays descriptives for the analysis of the baby year reform of 1986 based on the birth cohorts 1980 to 1991. Descriptive statistics are provided for variables used for the interaction term (Panel A), the outcome variable life satisfaction (Panel B), control variables (Panel C) and variables used for altered definitions (Panel D).

Source: Socio-Economic Panel (SOEP), data for survey years 1984-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

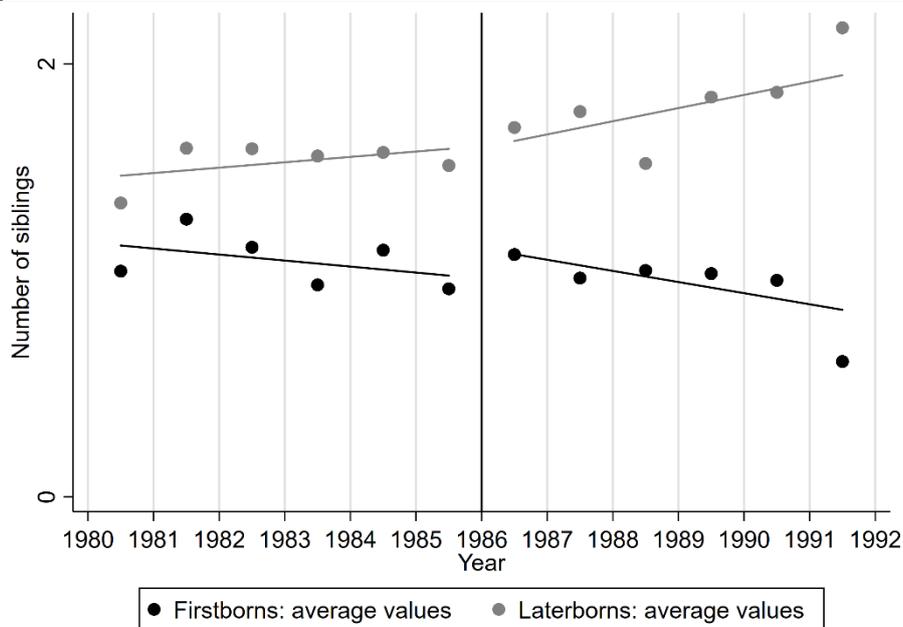
Table A.3:  
*Descriptive statistics of variables used in the analysis of the 1976 baby year reform*

	Observations	Mean	Std. dev.	Min.	Max.
<b>Panel A: Interaction variables</b>					
Laterborn child*born in 1976 or later (Interaction)	2,333	0.213	0.410	0	1
Laterborn child	2,333	0.338	0.473	0	1
Born in 1976 or later	2,333	0.603	0.489	0	1
<b>Panel B: Outcome variable</b>					
Overall life satisfaction	2,333	6.959	1.676	0	10
<b>Panel C: Control variables</b>					
Year of birth	2,333	1976,490	5.160	1966	1984
Male	2,333	0.495	0.500	0	1
Number of siblings	1,893	1.243	1.175	0	11
Grew up in urban area	2,333	0.648	0.478	0	1
Currently living in West Germany	2,333	0.229	0.420	0	1
Mother has Abitur or higher education	2,333	0.233	0.423	0	1
Father has Abitur or higher education	2,333	0.252	0.434	0	1
Survey year	2,333	2004,4116	5.704	1991	2018

Notes: This table displays descriptives for an analysis of the 1976's baby year reform, based on birth cohorts 1966 to 1984. Panel A provides descriptive statistics on interaction variables, Panel B on the outcome variable life satisfaction, and Panel C on control variables.

Source: Socio-Economic Panel (SOEP), data for survey years 1991-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

Figure A.3:  
*Local averages for the number of siblings before and after the 1986 baby year reform*



Notes: The figure shows average values of the number of siblings plotted for firstborn (black) and laterborn (grey) individuals. The vertical line indicates the time of the *baby year* reform on the 1<sup>st</sup> of May 1986.

Source: Socio-Economic Panel (SOEP), data for survey years 2008-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

Table A.4:  
*Long-term effects of the baby year reform 1986 on alternate channel variables*

	Number of siblings	Current region of living (East or West Germany)
Firstborn child*born in 1986 or later (interaction)	-0.341*** (0.131)	0.0375 (0.0512)
Firstborn child	-0.425*** (0.0890)	-0.0231 (0.0367)
Born in 1986 or later	0.103 (0.262)	-0.0560 (0.0936)
Constant	1.600*** (0.112)	0.310*** (0.0567)
R-squared	0.111	0.042
Observations	1,199	1,298
Cohort fixed effects	Yes	Yes
Individual controls	Yes	Yes
Background controls	Yes	Yes

Notes: This table displays DiD estimates for OLS regressions with outcome variables for number of siblings and current region of living — either East or West German states — (measured between ages 19 and 37), respectively, dependent on the mother’s eligibility status for the baby year paid parental leave reform. Regressions include birth cohort fixed effects, individual controls such as gender, age and the survey year, as well as background controls such as maternal and paternal education and whether the individual grew up in an urban area, or not. We also include dummies to control for missing observations for following variables: grew up in urban area, mother’s and father’s education. The number of missing observations is, however, small (between 1% for grew up in an urban area and 7% for father’s education). Only individuals are included for whom life satisfaction scores are available. Robust standard errors are reported in parentheses. Levels of significance: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Source: Socio-Economic Panel (SOEP), data for survey years 1984-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35.

## Appendix B

Table B.1:  
*Descriptive statistics for long-term effects of the 1986 baby year reform on dimensions studied in previous literature on parental leave reforms*

	Observations	Mean	Std. dev.	Min.	Max.
<b>Panel A: Personality</b>					
Trust	1,179	2.615	0.647	1	4
Agreeableness	1,191	5.277	0.954	1.333	7
Conscientiousness	1,191	5.547	1.122	1	7
Extraversion	1,191	4.676	1.227	1	7
Neuroticism	1,191	3.585	1.266	1	7
Openness	1,191	4.476	1.304	1	7
<b>Panel B: Health</b>					
Current overall health	1,298	3.829	0.812	1	5
Obesity	1,228	0.112	0.315	0	1
Chronic illnesses	1,114	0.212	0.409	0	1
<b>Panel C: Schooling and labor market</b>					
Education: Abitur or higher	1,184	0.398	0.490	0	1
Logarithmized gross income	1,050	7.175	0.767	4.318	9.210

Notes: This table displays descriptives for an additional analyses of the baby year reform of 1986 for outcomes used in previous papers based on the birth cohorts 1980 to 1991 taken from SOEP waves 2008 to 2018. Descriptive statistics are provided for personality (Panel A), health (Panel B), and schooling and labor market (Panel C). Control variables are similar to those shown in Table 1. Observations for all variables are adjusted to the sample used in the main analysis.

Source: Socio-Economic Panel (SOEP), data for survey years 1984-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.

Table B.2:

*Long-term effects of the 1986 baby year reform on dimensions studied in previous literature on parental leave reforms*

	Personality						Health			Education and labor market	
	(1) Trust	(2) Agreeable- ness	(3) Conscientious- ness	(4) Extra- version	(5) Neuroti- cism	(6) Open- ness	(7) Obesity	(8) Overall health	(9) Chronic illnesses	(10) Education: at least Abitur	(11) Gross income (log)
Firstborn child*born in 1986 or later (Interaction)	0.00931 (0.0766)	-0.0431 (0.112)	-0.0688 (0.134)	-0.173 (0.146)	0.258* (0.147)	0.0379 (0.151)	-0.007 (0.036)	-0.004 (0.095)	0.081 (0.050)	0.063 (0.054)	0.069 (0.090)
Firstborn child	0.0304 (0.0540)	0.163** (0.0801)	0.144 (0.0938)	-0.0197 (0.102)	-0.0926 (0.105)	-0.123 (0.108)	-0.018 (0.026)	0.015 (0.065)	-0.039 (0.036)	0.017 (0.038)	-0.047 (0.059)
Born in 1986 or later	0.181 (0.154)	-0.118 (0.210)	-1.035*** (0.225)	0.475* (0.257)	0.0819 (0.241)	0.333 (0.295)	-0.019 (0.066)	0.013 (0.195)	-0.063 (0.089)	-0.084 (0.100)	-1.081*** (0.161)
Constant	2.495*** (0.280)	5.369*** (0.197)	6.107*** (0.216)	4.383*** (0.231)	3.982*** (0.243)	4.078*** (0.299)	0.097** (0.038)	4.920*** (0.174)	0.204*** (0.052)	0.036 (0.212)	7.264*** (0.082)
R-squared	0.039	0.038	0.078	0.025	0.052	0.061	0.046	0.026	0.026	0.170	0.181
Observations	1,179	1,191	1,191	1,191	1,191	1,191	1,228	1,298	1,114	1,184	1,050
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the DiD estimates resulting from an OLS regression with various possible channel variables as outcomes (measured between ages 19 and 37), respectively, dependent on the mother's eligibility status for the baby year paid parental leave reform. Regressions include birth cohort fixed effects, individual controls such as gender, age and the survey year, as well as background controls such as maternal and paternal education and whether the individual grew up in an urban area. We also include dummies to control for missing observations for following variables: grew up in urban area, mother's and father's education. The number of missing observations is, however, small (between 1% for grew up in an urban area and 7% for father's education). Robust standard errors are reported in parentheses. Levels of significance: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Source: Socio-Economic Panel (SOEP), data for survey years 2008-2018, version 35, SOEP, 2019, 10.5684/soep-core.v35. Own calculations.