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***ECONOMIC PERSPECTIVES ON THE WELL-BEING OF CHILDREN
AND MOTHERS***

*Three essays in empirical family economics and early childhood educa-
tion*

DISSERTATION

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PREFACE

During my Ph.D. studies in economics, I continuously deepened my research interests in education, innovation, and development and combined them with gender- and country-specific societal conditions. The papers of this dissertation eventuated from these interests. However, finalizing this thesis would not have been possible without all the kind and encouraging backing I received in the last years.

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II

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CONTENT

PREFACE.....	I
CONTENT	III
LIST OF TABLES.....	VI
LIST OF FIGURES	VIII
CHAPTER 1: INTRODUCTION	1
CHAPTER 2: CHILDCARE ARRANGEMENTS	
The impact of family daycare on child development	9
1. <i>Introduction</i>	9
2. <i>Literature review, institutional framework, and hypotheses</i>	11
2.1 Literature review.....	11
2.2 Differences between family daycare and childcare centers in Germany	13
2.3 Hypotheses.....	14
3. <i>Data</i>	15
3.1 Data sets	15
3.2 Variable operationalization.....	16
3.3 Descriptive statistics	18
4. <i>Method</i>	20
4.1 Ordinary least squares approach.....	20
4.2 Regression models with individual fixed effects	21

5. Results.....	23
5.1 Results of the ordinary least squares regressions	23
5.2 Results of the fixed effects estimates.....	25
6. Discussion and conclusion.....	28
7. References.....	29
8. Appendix A: Further information on data and analyses in tabular form	36
9. Appendix B: Additional analysis using an instrumental variable approach	41

CHAPTER 3: PARENTAL LEAVE AND CHILDREN

Maternal employment and long-term subjective well-being of children: Evidence from a parental leave reform in former East Germany	49
1. Introduction	49
2. The historical background and institutional setting	56
3. Empirical approach.....	59
4. Data and descriptive statistics.....	60
4.1 Data source and variable calculation	60
4.2 Descriptive statistics	63
5. Regression results from a difference in differences analysis	68
5.1 Average effects of the reform in 1986.....	68
5.2 Effect heterogeneity by gender	72
5.3 Effects of the reform in 1976 as alternative scenario	76
5.4 Potential channels.....	78
6. Discussion and conclusion.....	85
7. References.....	87
8. Appendix.....	99

CHAPTER 4: PARENTAL LEAVE AND MOTHERS

The long-term impact of paid parental leave on maternal health and subjective well-being	109
<i>1. Introduction</i>	<i>109</i>
<i>2. Theoretical background and related literature.....</i>	<i>114</i>
2.1 Maternal time allocation and well-being of mother and child	114
2.2 Parental leave and maternal health.....	115
2.3 Parental leave and subjective well-being.....	118
<i>3. Family policies in the GDR and the parental leave reform of 1986</i>	<i>119</i>
<i>4. Empirical approach.....</i>	<i>121</i>
<i>5. Data.....</i>	<i>123</i>
<i>6. Descriptive statistics.....</i>	<i>125</i>
<i>7. Results.....</i>	<i>129</i>
<i>8. Discussion and conclusion.....</i>	<i>134</i>
<i>9. References.....</i>	<i>135</i>
<i>10. Appendix.....</i>	<i>144</i>

LIST OF TABLES

CHAPTER 2: CHILDCARE ARRANGEMENTS

Table 1. Comparison of children in different care types.....	20
Table 2. Estimations from ordinary least squares regressions	24
Table 3. Results of fixed-effects estimations for children in family daycare	26
Table 4. Results of fixed-effects estimations for children in a childcare center	27
Table A.1. Overview of the variables used in the analysis.....	36
Table A.2. Proportions of children in types of care and the intensity of care	38
Table A.3. Quality indicators for childcare centers and family daycare	39
Table A.4. OLS regressions for vocabulary with stepwise addition of covariates .	39
Table A.5. Sample for the fixed effects approach and care behavior.....	40
Table B.1. 2SLS estimations for the dependent variables of cognitive skills.....	45
Table B.2. 2SLS estimations for socio-emotional skills.....	46
Table B.3. 2SLS estimations for the dependent variables of parent-child interaction	47

CHAPTER 3: PARENTAL LEAVE AND CHILDREN

Table 5. Maximum of time children spent with their mother after birth.....	57
Table 6. Summary statistics for subgroups of firstborns and laterborn children .	66
Table 7. Estimation results for the parental leave reform of 1986	70
Table 8. Sensitivity analyses with varying time frames around the reform date ..	71
Table 9. Heterogeneity analysis for male individuals.....	74
Table 10. Heterogeneity analysis for female individuals	75
Table 11. Effects of the reform on potential channel variables	84
Table A.6. Studies on short-term effects of parental leave on child outcomes.....	99

Table A.7. Studies on long-term effects of parental leave on child outcomes	101
Table A.8. Big five personality traits and underlying variables.....	103
Table A.9. Summary statistics for laterborn and firstborn children (reform 1976)	104
Table A.10. Results for the parental leave reform of 1976	105
Table A.11. Heterogeneity analysis for male individuals (reform 1976)	106
Table A.12. Heterogeneity analysis for female individuals (reform 1976)	107
CHAPTER 4: PARENTAL LEAVE AND MOTHERS	
Table 12. Descriptive statistics for outcome and control variables.....	126
Table 13. Estimation results for maternal health outcomes	131
Table 14. Estimation results for maternal subjective well-being outcomes.....	133
Table A.13. Studies on parental leave reforms and maternal health	144
Table A.14. Studies on parental leave reforms and subjective well-being.....	146

LIST OF FIGURES

CHAPTER 2: CHILDCARE ARRANGEMENTS

Figure B.1. Share of family daycare places at district level in 2010.....	42
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CHAPTER 3: PARENTAL LEAVE AND CHILDREN

Figure 1. Labor market participation of women in former East and West Germany	56
--	----

Figure 2. Local averages of first- and laterborn individuals' outcomes.....	67
---	----

CHAPTER 4: PARENTAL LEAVE AND MOTHERS

Figure 3. Classification of mothers in the analysis	122
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Figure 4. Health averages of mothers per childbirth cohort	127
--	-----

Figure 5. Subjective well-being averages of mothers pre childbirth cohort	128
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CHAPTER 1: INTRODUCTION

In societies around the globe, succeeding in life depends to a varying extent on the conditions and surroundings during childhood. Our families and, moreover, the political framework determine these conditions to a significant degree. Family background and, in part, genetics lay the foundation for an individual's health conditions and educational career. This, again, can determine labor market success and well-being in general. Surroundings in childhood do not solely drive educational success but also one's values and beliefs. All these aspects are vital for well-being. In recent years, well-being has become a vital component in political discussions on measuring welfare and prosperity in welfare states. Therefore, there is an ongoing need to understand the consequences of the many conditions in childhood that influence well-being in society.

More precisely, the link between childcare arrangements and the development of competencies in early childhood and how they later on affect children is yet to be fully understood. Children's advancement in early childhood is found to be fundamental for their later development of skills, as competencies are built upon one another.¹ However, to which extent this holds, and, furthermore, is persistent until adulthood, depends not solely on one, but various conditions, such as the individual themselves, their gender, their early childhood conditions, their progress through life, as well as their circumstances in adolescence and adulthood.

Not only childcare arrangements but policies in welfare states targeted at adolescents and adults, in general, can also determine the well-being of an individual. In this thesis, I investigate the impact of two welfare state policy tools: public-funded childcare institutions and parental leave policies. Both tools were originally

¹ Heckman, J., Pinto, R. and P. Savalyev (2013). Understanding the Mechanisms through Which an Influential Early Childhood Program Boosted Adult Outcomes. *American Economic Review*, 103(6), pp. 2052–2086. doi:10.1257/aer.103.6.2052

implemented to enable mothers to reconcile family and working life. However, they were found to not solely impact maternal labor market situation during childbirth.

Therefore, I examine if differences in childcare arrangements and parental leave matter for an individual's development in several aspects. Throughout this thesis, I explore means to increase an individual's well-being in the short- and long-term using survey data and settings from contemporary Germany and the former German Democratic Republic.

The three studies aim to establish causal links between the effects of the care form or parental leave policy measure under study on children's development and mothers' and children's well-being. In contrast to solely applying simple or multiple regression analyses based on ordinary least squares (OLS) estimation strategies, the repertoire of causal methods aims to circumvent common threats to statistical analyses. The empirical method in each of the following chapters is carefully selected to fit the respective study focus and requirements to address the underlying specific threats to each scenario with its goal of detecting unbiased effects.

Chapter 2 explores differences between childcare centers and family daycare in Germany and whether these differences matter for a child's development regarding various cognitive and socio-emotional skills and a child's interaction with their parents in the short term. Studies on family daycare, in contrast to care centers, are rare. This is especially the case for causal studies, which are currently nonexistent in the German context. In the context of examining effects of different childcare arrangements, the methodological challenge is based on the non-random assignment of children to a specific care form. As childcare spaces in publicly funded care are limited, the type of care attended by a child partially depends on unobserved parental preferences, child abilities, and municipal availability.

Hanna Brosch, Larissa Zierow, and I explore the causal link between childcare type and child development by applying three different empirical methods. We mitigate the selection problem by including a child's sensorimotor development before they attend public childcare in an OLS estimation. Also, we eliminate bias due to time-constant omitted variables by applying a fixed effects approach. An instrumental variable approach attempts to exploit regional variation in family daycare supply on account of a public childcare expansion act in Germany. We observe children's outcomes up to the age of three and use data of the German *National Educational Panel Study (NEPS)*.

We find for the context of Germany that, in general, children benefit equally from family daycare and childcare centers regarding most developmental measures. However, we find weak evidence for a reduction of information processing speed in children attending family daycare instead of a childcare center. There are no other distinct differences between the impact of the two types of childcare on other cognitive, socio-emotional measures or parent-child interaction. The results contrast previous causal studies conducted in a Danish context, where family daycare was found to be less beneficial for child development than childcare centers. We discuss reasons why differences to other studies occur.

This chapter is the translated and slightly amended version of a German-language paper published in the *Zeitschrift für Erziehungswissenschaft*, Vol. 26 (2023). For the paper, I provided the method descriptions, the result sections of the main part and Appendix B, and the English translation. Hanna Brosch and I provided the tables in the main part of the paper. Hanna Brosch mainly provided the data set construction, analysis conduction, data description, and Appendix A. Larissa Zierow mainly provided the discussion and conclusion chapter. The introduction, literature review, institutional framework and hypotheses, descriptive statistics, and rest of Appendix B were written in cooperation by all of us.

The decision to make use of publicly funded care arrangements depends to a significant extent on maternal labor market attachment. The institution of paid parental leave is a means to support mothers after childbirth without giving up labor market attachment. Therefore, maternal care during parental leave is a counterfactual care mode to publicly funded childcare for children at an early age. Chapter 3 investigates whether mothers extending parental leave by six months to a total of twelve months in the former German Democratic Republic affected various well-being outcomes of children. In contrast to the previous chapter, the focus of this chapter's analysis is on long-term development and well-being.

So far, no study apart from this thesis' Chapter 3 and 4 has examined the effects of a parental leave introduction or extension in the German Democratic Republic. Compared to other studies on parental leave, the setting is favorable as we can circumvent methodological challenges of the self-selection of women and mothers into the labor market and the lack of a distinct counterfactual scenario to maternal care.

Larissa Zierow and I apply a difference in differences analysis as a causal identification strategy and data of the *German Socioeconomic Panel (SOEP)*. The reform targeted first-time mothers, implying that firstborn children were assigned to the reform. Mothers giving birth to their second child or more could already enjoy twelve months of parental leave. The specific reform target and timing allow us to make children with older siblings our control group. By applying survey data of individuals in adulthood between ages 18 to 37 and 18 to 47, we examine the effects of extended parental leave on children's long-term well-being measures, including life satisfaction, personality traits, health, education, and labor market success. Furthermore, we analyze potential channels through which children's outcomes might have been affected.

We find some evidence that extended paid parental leave from six to twelve months improves the life satisfaction of assigned individuals in adulthood and few other outcome measures. We find more pronounced effects for males. We test for bias by a multiple hypotheses test to consider the probability that some effects occur by chance. This test reveals that the estimates are not robust. We conclude that maternal care and care in childcare centers were relatively substitutable in the German Democratic Republic, implying that children did not significantly benefit from their mothers' extension of six to twelve months of paid parental leave in the long term. The results align with many studies on parental leave extensions in other contexts.

The chapter on hand is a considerably revised version of a working paper published 2019 as *CESifo Working Paper No. 7806* and as *SOEPpaper 1059*. In this version, a variety of referee suggestions is included. For the paper, I conducted the data work, the empirical analysis, the literature review, the background section, the data and results chapters, and the Appendix. I also provided the main work on the introduction and conclusion. Larissa Zierow provided the research idea and suggestion for the data set used. She also provided input for the introduction and conclusion.

Paid parental leave might not solely impact the development and well-being of children. It also has the potential to benefit mothers' well-being as the primary targets of parental leave measures. Chapter 4 explores if mothers in the German Democratic Republic benefited from extending parental leave regarding long-term subjective well-being and physical and mental health.

This chapter is closely linked to the preceding chapter, as I use the same reform setting and apply a related difference in differences approach and data of the *SOEP*. However, the assigned and control groups are defined differently. As mothers are

the subject of this study, using pre-reform and post-reform first-time mothers and mothers with a higher-order born child is insufficient to distinguish a clear assigned group from a clear control group. This is because a first-time mother before the reform can be a mother of higher-order-born children after the reform later on. This chapter furthermore explores channels related to mothers through which paid parental leave may impact assigned children's well-being. Finding impacts of the reform on maternal well-being many years after parental leave was utilized can hint at persistent beneficial effects of this well-established family policy means.

I find some evidence for increased satisfaction with household activities, income, and work up to 37 years after the respective birth. As in the previous chapter, regression estimates are not robust throughout sensitivity analyses and multiple hypothesis testing. I conclude that mothers who gave birth to a child in the German Democratic Republic benefitted relatively equally from six or twelve months of paid parental leave in terms of long-term subjective well-being and physical and mental health. My results are in line with other studies on the effects of parental leave extensions on aspects of maternal health. This chapter is a slightly amended version of a single-authored working paper published 2023 as *CESifo Working Paper No 10308*.

The results of this thesis give some overall conclusions beyond the three studies. Firstly, if a child attends a publicly funded childcare center or family daycare or whether they spend the most time together with their mother within the first or three years of life can have similar impacts. In other words, one care form is not necessarily superior to another. This conclusion can be drawn from Chapter 2 for the substitutability of childcare centers and family daycare in Germany within the first three years of a child's life. It can be drawn from Chapter 3 for the substitutability of maternal care and care in a childcare center in the former German Democratic Republic within the first year of a child's life as well.

This thesis provides evidence and input for the ongoing debate on public care quality and the quality of parental care. As Chapter 2 suggests, in contemporary Germany, children in family daycare benefit equally from publicly funded care as children in childcare centers regarding socio-emotional development and parent-child interaction. This conclusion can be drawn for a child's cognitive development to a restricted degree. Finding barely any differential impacts on child development in the German setting can be found in the relatively equal quality of both types of care.

Although staff quality is higher in care centers, group sizes are more favorable in family daycare. Both properties are vital for child development.

Concerning the context of the German Democratic Republic relevant in Chapter 3, care center staff quality was relatively high. However, group sizes were less advantageous for child development. Depending on the mother's educational background, this again suggests that, on average, both childcare centers and maternal care were of comparable quality for long-term child development. Moreover, my thesis encourages further research to understand more profoundly if family daycare and care centers impact children differently.

Secondly, experiences stemming from childhood are, to some extent, vital for child development and later success in life. This is especially true if children from disadvantaged backgrounds are enabled to enter high-quality early childhood interventions.² However, for individuals on average in the (former East) German context, it is a noticeable conclusion that a family's decision over childcare arrangements, as in Chapter 3 for a trade-off between childcare centers and maternal care, might not entirely decide on a child's success in adulthood.

Even though some care arrangements might be of a lower quality, individuals are able to compensate for less beneficial circumstances later on. Thus, deciding between care forms might not necessarily be accompanied by the choice of a better or worse. However, Chapter 2 showed limited evidence for short-term effects of family daycare on a decreased information processing speed compared to childcare centers. Whether short-term differences manifest into long-term differences depends on the child's subsequent development.

Lastly, I can also conclude from Chapters 2 to 4 that the historical and country-specific settings are essential for the outcomes under study. Finding no pronounced differential impacts of various care forms on children or of varying parental leave lengths on children and mothers does not necessarily mean the same conclusion holds in other country-specific settings. The institutional endowment of the same care form or parental leave policy can differ between countries, depending on specific regulations. Therefore, this thesis motivates further research in other country-specific settings.

² Heckman, J. J., Moon, S. H., Pinto, R., Savalyev, P. A. and A. Yavitz (2010). The rate of return to the HighScope Perry Preschool Program. *Journal of Public Economics*, 94(1-2), pp. 114-128. doi:10.1016/j.jpubeco.2009.11.001.

I also encourage further studies to take advantage of policy reform settings as sources of exogenous variation, as this thesis attempts to utilize in Chapters 2 to 4. Studies based on these settings are of high potential compared to non-causal analyses lacking such a source of variation or suffering from omitted variable bias. At the present, making use of a source of exogenous variation is already an established means in family economics literature. However, there is an enormous potential for future causal studies using exogenous variation for estimating different potential effects of family daycare and childcare centers on child development.

CHAPTER 2: CHILDCARE ARRANGEMENTS*

The impact of family daycare on child development

This study examines the short-run impact of family daycare on child development compared to center-based care. International studies suggest family daycare attendance negatively impacts children compared to center-based care. Using the NEPS newborn cohort, we can evaluate whether this also holds in the German context. We use two different methodological approaches to estimate the effect of family daycare. Our results suggest that family daycare does not have statistically significant worse effects on child development than care centers, except for habituation as a measure of information processing speed.

1. Introduction

In recent decades, comprehensive, publicly funded early childhood care and education are not solely used as an effective means to promote maternal labor force participation (e.g., Spieß and Büchel 2003; Baker et al. 2008; Bauernschuster and Schlotter 2015; Huebener et al. 2020). It is also increasingly perceived as an opportunity to positively impact the cognitive or socio-emotional development of children even before they enter school (e.g., Gormley Jr. et al. 2008; Berlinski et al. 2009; Cascio 2009; Datta Gupta and Simonsen 2010; Havnes and Mogstad 2011; Noboa-Hidalgo and Urzúa 2012; Cornelissen et al. 2018; Felfe and Lalive 2018; Bach et al.

* This paper is a translated English and slightly amended version of a German-language paper by Brosch, H., Heisig, K. and L. Zierow titled “Der Einfluss der Tagespflege auf die kindliche Entwicklung” published in the Zeitschrift für Erziehungswissenschaft (2023), vol. 26, pp. 299-318, doi:10.1007/s11618-023-01150-2.

Larissa Zierow received financial support by NORFACE through the project “The impact of childhood circumstances on individual outcomes over the life-course – IMCHILD” and by the DFG through the project “Multidimensional Equality of Opportunity – EOPM”.

2019). Previous studies show that attending a childcare center can help children to start school better prepared. As a result, the family environment may cause less educational inequality between children (e.g., for the U.S.: Schweinhart et al. 2004; international overview: Burger 2010; overview for Germany: Spieß 2022). These studies consider childcare centers as care type. However, especially for children under three years and particularly in Germany, family daycare is expanding.³

Our study focuses on family daycare as a care type in the publicly funded daycare system. We examine the short-term effects of family daycare on child development and compare them with the effects of childcare centers. The study is based on the newborn cohort (starting cohort 1 (SC1)⁴) of the longitudinal data set of the National Education Panel Study (NEPS).

Since the attendance of each care type is unlikely to be random, we use two methodological approaches to reduce the selection problem and estimate the care type effect. In the first approach, we use a multilinear regression model, in which we control for essential characteristics of the child, parent, and the region of living. In our second approach, we use an individual fixed effects (FE) model that exploits the panel dimension of the NEPS data. We calculate the causal effect of attending a particular type of childcare (family daycare or childcare center) using care data on children from three waves (aged approximately seven months, one year, and two years). This method allows us to separate individual fixed characteristics that might influence the choice of care type and the outcome variables.

We use NEPS measures of cognitive and socio-emotional development and parent-child interaction as outcome variables. Our results show that family daycare does not have a statistically significant different impact on child development than childcare centers in most specifications of our analysis. One exception is the competence dimension habituation. We find a negative impact of family daycare on this dimension, implying a lower information processing speed.

The study is organized as follows: Chapter 2 reviews previous literature, describes characteristics of childcare centers and family daycare for children under three years in Germany and presents our hypotheses. Chapter 3 introduces the data used, and

³ Family daycare is increasingly discussed in the public debate and often accompanied by the concern that its care quality is not sufficient (see Deutschlandfunk „Kindertagespflege im Wandel - Billige Kita-Alternative oder individuelle Betreuung?“, <https://www.deutschlandfunk.de/kindertagespflege-im-wandel-billige-kita-alternative-oder-100.html> [last visit 2023/02/15]).

⁴ For a comprehensive overview of this cohort, see Schlesiger et al. (2011).

Chapter 4 describes the empirical approach for estimating the effects of family daycare. Chapter 5 presents the results, and Chapter 6 discusses them and concludes.

2. *Literature review, institutional framework, and hypotheses*

2.1 Literature review

There are already some studies in the social sciences regarding the effects of publicly funded care on children's skills. One strand of this literature examines the causal effects of attending these institutions on children's skills and later educational and labor market outcomes. Studies show that human capital accumulation begins at a very young age. Consequently, providing publicly funded childcare can increase human capital if it positively affects children's development (Blau and Currie 2006; Cunha et al. 2006). Empirical studies examining the causal effects of the expansion of publicly funded childcare on children's skills in different countries vary in their findings.

For example, Havnes and Mogstad (2011) find positive effects for Norway. However, Baker et al. (2008) show adverse effects for Canada, and Fort et al. (2020) for Italy.⁵ In the German context, there is also an increasing number of studies on the effects of publicly funded childcare on children's skills (for two literature reviews, see, e.g., Anders and Roßbach 2020; Spieß 2022). Studies indicate positive (e.g., Cornelissen et al. 2018; Bach et al. 2019), mixed (e.g., Felfe and Lalive 2018), or no effects (e.g., Kuehnle and Oberfichtner 2020).

Most studies focus on childcare centers. So far, family daycare has received less attention, although it is a care option in many countries. The supply of family daycare was expanded in recent years in Germany, and thus, the number of children cared for in family daycare has risen sharply.⁶ In the German context, Kutscher (2018) considers family daycare in addition to childcare centers. She elaborates on the increased focus on children's skill development and families as addressees. One of the few studies examining the effects of childcare centers and family daycare is Datta Gupta and Simonsen (2016) for Denmark. Their results show that childcare centers

⁵ Both Burger (2010) and Anders (2013) provide a substantial overview of international research on the effects of education and care in the early years of a child's life.

⁶ In 2012, 88,000 under-three-year-olds were cared for in public daycare. In 2021, the figure was already 130,000 (Destatis 2022).

compared to family daycare improve children's scores in the Danish language by about 0.2 standard deviations in the last year of compulsory education. This result is explained by the different educational backgrounds of the staff in the two types of care, as family daycare staff is comparable to parents with low education. In contrast, staff in childcare centers is more similar to parents with high education.

An earlier study by Datta Gupta and Simonsen (2010) shows similar results. The authors examine the short-term effects of attending a childcare center compared to family daycare on the socio-emotional competencies of seven-year-old children. In contrast to family daycare, they find a more considerable increase in competencies for children who attended a childcare center. Bernal and Keane (2011) confirm these results. They find for the U.S. that an additional year of informal care, such as family daycare, leads to a 2.6% decrease in performance in cognitive tests of four- to six-year-old children. In contrast, they did not find adverse impacts on cognitive performance if a child was cared for in a childcare center. They suggest that in childcare centers, the staff is better trained than in informal care, leading to more cognitive stimulation for and educational activities of children and inspiring interactions with other children.

For Germany, Zierow (2017) analyzes the impact of family daycare on children's competencies in the school entry examination. Using a reduced-form approach, she shows that family daycare has a slightly positive effect on socio-emotional development compared to childcare centers. Bensel et al. (2017) examine the quality of family daycare in Baden-Württemberg. They show that the strengths of family daycare are language and interaction support, and weaknesses are areas such as diversity, observation and documentation, and safety standards.

With our study, we contribute to the scarce literature in the German context regarding the effects of family daycare on child development. Conclusions from previous international studies are applicable to Germany only to a limited extent, as the effects of publicly funded childcare on child development can be country-specific. Using the newborn cohort of the NEPS panel data set, we can provide evidence on how family daycare affects various dimensions of child development in the short term.

2.2 Differences between family daycare and childcare centers in Germany

On average, about one-fifth of all children under the age of three⁷ in publicly funded childcare in Germany attended family daycare in 2010⁸ (Destatis 2010). Family daycare differs significantly from childcare centers. In childcare centers, children under three years old are cared for in groups of, on average, 40 children, divided into smaller groups of about 13 children each.

Between 2007 and 2011, the childcare ratio was, on average, 1:5 (Zierow 2017, also for the following indicators)⁹. Staff who are active as team leaders are usually required to have two years of theoretical training and at least two years of practice in a childcare center. On average, 64% of caregivers in a childcare center have completed vocational training as educators, and 18% are supplementary pedagogical workers.

Family daycare is usually provided in the private home of the family daycare staff.¹⁰ The average childcare ratio between 2007 and 2011 was 1:3. Unlike pedagogical specialists in childcare centers, family daycare staff are not obliged to have completed training in pedagogy-related fields. In most federal states, they only need to complete a 160-hour short training course (BMFSFJ 2019).

According to calculations by Zierow (2017), 80% of family daycare workers in 2011 had a short training as qualification. Only 19% had completed educator training, and 12% were qualified as supplementary pedagogical workers.¹¹ Before family daycare workers are allowed to care for children, responsible public authorities control their personal suitability and police clearance certificate.

⁷ Family daycare is also sometimes offered to children over the age of three. However, only 0.9% of children aged three to six attend family daycare (Strunz 2011).

⁸ We refer to 2010 since this is relevant for our analysis and shows the situation in the respective municipality before the children's birth in starting cohort 1 of the NEPS.

⁹ Zierow (2017) used data from the Federal Statistical Office (Statistics of child and youth welfare, children and persons working in daycare facilities and publicly funded childcare) for the years 2007-2011 to calculate the average values of the care ratio, group size, and qualifications cited here.

¹⁰ Some federal states also provide large-scale family daycare. It is defined by having two or more care staff caring for more than five children (for more information, see the description of the Bundesverband für Kindertagespflege, available at <https://www.bvktp.de/themen/grosstagespflege/> [last visit 2023/02/15]. We cannot distinguish between regular and large-scale family daycare in the dataset we use.

¹¹ In addition to a short training course, some family daycare workers have also completed an educator training course or a qualification as a supplementary pedagogical worker. Therefore, the values add up to more than 100%.

2.3 Hypotheses

Hypothesis 1: Cognitive skills

Previous studies show that attending family daycare compared to childcare centers negatively affects children's cognitive development (Datta Gupta and Simonson 2010, 2016; Bernal and Keane 2011). Thus, hypothesis 1 is that the effect of family daycare on cognitive skills compared to childcare centers is harmful in the German context as well.

Hypothesis 2: Socio-emotional skills

Houng et al. (2011) showed that the care ratio is relevant for developing short-term socio-emotional competencies. In Germany, the care ratio in family daycare (1:3) is better than in childcare centers (1:5) (see section 2.2). Therefore, the caregiver in the former care type can establish a relatively closer relationship with the child. This relationship can positively affect the child's emotional stability and, thus, their socio-emotional development.

However, smaller group sizes can also imply less social interaction. Especially for children with an immigrant background, studies show that they are emotionally more stable if attending a childcare center before school (Cornelissen et al. 2018; Felfe and Lalive 2018; Bach et al. 2019). However, Houng et al. (2011) find that these effects are not evident in the first three years of life.

Consistent with the fact that the effects of social interactions with peers are essential later in life, Gloger-Tippelt (2018) argues that same-aged peers can positively influence interests and education, especially in middle childhood. Our hypothesis 2 is, therefore, that the effect of family daycare on socio-emotional development in the first three years of a child's life is favorable compared to childcare centers.

Hypothesis 3: Parent-child interaction

Parent-child interactions in early childhood are essential for child development (Tamis-LeMonda et al. 1998, 2001; Newton et al. 2014). Studies showed that childcare center attendance could improve parent-child interaction by raising the quality of stimulation in families (Puma et al. 2010; Jessen et al. 2020).

We hypothesize that family daycare will produce comparable effects, as we can expect similar stimuli here. Therefore, hypothesis 3 is that the effect on the parent-child relationship does not differ between family daycare and childcare centers.

3. Data

3.1 Data sets

Our empirical analysis is based on the Starting Cohort “Newborns” (SC1) of the National Educational Panel Study (NEPS), which was launched in 2012/2013 (Blossfeld et al. 2011; NEPS Network 2022).¹² The SC1 captures the developmental processes of young children. About 3000 parents with newborns born between February and July 2012 participated in the survey.

Because our analysis focuses on the effect of childcare arrangements for children younger than three years on early skill development, we restrict our analysis to waves 1 to 4 (children aged seven to 39 months). We also use INKAR spatial data. This data is aggregated at the county level and then matched to the NEPS data. This data allows us to account for regional factors that could influence the care structure for children under three years (BBSR 2020).

¹² We use the following data sets: CohortProfile and pParent are the baselines. We merge the following to the baseline data set: xDirectMeasure (for competency measures in waves 1 to 3 and parent-child interaction), xTargetCompetencies (for competency measures in wave 4), spChildCare (for care intensity variables), pEducatorChildminder (for variables on the quality of family daycare), and pEducator (for childcare center quality variables).

3.2 Variable operationalization¹³

3.2.1 Outcome variables

Cognitive skills

We use four variables from competence tests conducted in wave 4: *Vocabulary* was measured using the *Peabody picture vocabulary test* (PPVT). This test is a listening comprehension test. Children must select a suitable picture for vocalized words (Lenhard et al. 2015). We use the sum of all solved items. The *maximum digit span* of a child measures the competence dimension of phonological working memory, which is relevant for child development (Gathercole and Baddeley 1993; Baddeley et al. 1998; Weinert 2010; Berendes et al. 2013). In a standardized test, children must reproduce 15 sequences of digits. Again, we use the sum of the solved items.

The *Snijders-Oomen Non-verbal Intelligence Test (SON-R subtest)* comprises picture sorting and measures abstract and inferential thinking (Tellegen et al. 2007). We use a WLE estimator for this measure provided by the NEPS. We also use a measure of *delayed gratification* (deferred gratification) that measures executive control. Self-regulation skills are essential for child development (Wulfert et al. 2002; Baumeister and Vohs 2004; Tangney et al. 2004). In this experiment, children must wait an unspecified amount of time to receive a large gift or cancel the waiting time for a small gift. The dummy variable is one if the child waited for the large gift.

In addition, we use *infant visual habituation* measured in waves 1 and 2 by eye movement behavior and fixation times on pictures. This allows us to measure early processes and abilities of attention and information processing of infants (Colombo and Mitchell 2009). We use the logarithm of the fixation time sum across all pictures. The lower the measured time (corresponding to faster information processing), the better a child's habituation (Hondralis and Kleinert 2021).

¹³ Table A.1 in Appendix A presents all variables used and underlying items.

Socio-emotional skills

Different well-established concepts for personality development are used depending on the child's age. The interviewed parent answers the questions. For each dimension, we calculate the item sum. We code the variable so that positive values correspond to a positive denotation for child development. For waves 1 to 3, the NEPS uses a shortened version of the *Infant Behavior Questionnaire* (IBQ). We use the variable *negative affectivity* (measuring the frequency of emotional acts) since for waves 1 to 3, observations are only available for this variable (Bayer et al. 2015).

We also use *prosocial behavior* and *problem behavior* from the *Strength and Difficulties Questionnaire* (SDQ), which is included from wave 4 onwards (Goodman 1997). In addition, we use the shortened form of the *Children's Behavior Questionnaire* (CBQ), which is also available since wave 4. It contains three items each on the temperament dimensions *negative affectivity* (measures frustration and dejection), *surgency extraversion*, and *effortful control* (Rothbart et al. 2001; Putnam and Rothbart 2006).

Parent-child interaction

In waves 1 to 3, semi-standardized established experiments on parent-child interaction were conducted (Sommer et al. 2016). We use the five standardized items *positive mood*, *negative mood*, *activity level* (measures gross motor skills, its speed and energy input), *non-social sustained attention* (regarding objects and activities of the parent), and the child's *positive engagement with their parent* (Linberg et al. 2019).

3.2.2 Childcare arrangements and covariates

Childcare arrangements

If a child was cared for at least once in family daycare until 27 months old, our variable of interest for family daycare – a dummy variable – is one; otherwise, it is zero. Following the same pattern, we define a dummy variable for childcare centers.¹⁴ When we use the panel dimension of the data, we also define dummy

¹⁴ The care portfolio outside the publicly funded childcare system is very diverse. However, using parental education (as a proxy), we control for differences in the quality of care through parents or relatives. The average duration of care outside the publicly funded childcare system is similar for

variables for care in family daycare or a childcare center. They are one if the child attended the respective type of care in the respective wave and zero if otherwise.

Covariates

As *individual covariates*, we use the child's age and gender, the number of siblings, whether the child has parents with a university degree, the respondent's migrant background, and whether the respondent lives in West or East Germany. The average number of hours per week as well as the number of months spent in each type of care are covariates to control for care intensity.¹⁵

In addition, we use the *weighted likelihood estimates* (WLE) of a child's sensorimotor development at seven months of age as a measure for a child's abilities before entering publicly funded childcare.¹⁶ This allows us to rule out a part of the possible selection into family daycare or a childcare center due to unobserved characteristics – e.g., unobserved characteristics of the child or parents that had an impact on the sensorimotor development up to the age of seven months.

For *regional covariates*, we use dummy variables for GDP per capita, the female labor force participation rate, and conservatism in the respective county in 2010. If the variable value is above the German median, we assign it the value one; otherwise, the variable is zero. We define conservatism by the county's share of CDU/CSU and FDP voters.

3.3 Descriptive statistics

Our sample includes 3481 children. By the age of 27 months, 14% of all children in our sample had attended family daycare for at least one month, and 56% had attended a childcare center for at least one month.¹⁷ We exclude children who attended both family daycare and childcare centers by the age of 27 months (N=100) from the analysis to measure the separate effect of childcare centers and family daycare on competencies. The intensity of care between family daycare and childcare

both groups – family daycare and childcare centers (see Table A.2 in Appendix A). In the following, we therefore speak of care in a childcare center vs. care in family daycare vs. home-based care, under which we understand privately organized care, e.g., by parents, relatives, or a nanny.

¹⁵ This is important if children in family daycare spend less time in the respective facility than children in childcare centers. In this case, the estimated effect of family daycare would show a downward bias by not considering the intensity and duration of care.

¹⁶ Only a tiny fraction of the observed children in our sample are cared for in the publicly funded childcare system before they are seven months old.

¹⁷ See Table A.2 in Appendix A.

centers is mostly the same. Children in family daycare (childcare centers) spend 27 (31) hours per week in this type of care at 27 months. In addition, children attending family daycare or a childcare center at 27 months spent an average of 13 (family daycare) or 12 months (childcare center) in care.¹⁸

Parental decision-making to choose family daycare or a childcare center is not random and may be related to various factors. Table 1 shows that children in family daycare or a childcare center differ in family background from children not cared for in one of these institutions. If we only consider children cared for in a childcare center or family daycare, there are, on average, hardly any differences. However, family daycare seems more widespread in West Germany than in East Germany. In addition, children with more siblings are more likely to attend a childcare center than family daycare.

One possible explanation for why there might be different effects of family daycare or childcare centers on children's development is the quality of care provided by the institution. In our sample, family daycare has, on average, a better care ratio (1:3.8) than childcare centers (1:5.7). However, the qualification level of the educators and caregivers in family daycare is lower: 99% of educators and caregivers in childcare centers have pedagogical training, whereas the same applies to only 41% of caregivers in family daycare.¹⁹

Looking at the transfer of children between the types of care shows that in wave 1, only a few children are already cared for in family daycare (19 children) or a childcare center (54 children). From wave 2 to 3, most children switch from home-based care²⁰ to family daycare (161 children) or a childcare center (904 children). In wave 3, 258 children are cared for in family daycare and 1333 in childcare centers.²¹

¹⁸ See Table A.2 in Appendix A.

¹⁹ See Table A.3 in Appendix A.

²⁰ We use home-based care as a collective term for care provided by parents or other privately organized care (e.g., by siblings, grandparents, other relatives, neighbors, or, relatively rarely in Germany, by a nanny).

²¹ See Table A.4 in Appendix A. In the analysis, we exclude children who attended family daycare or a childcare center but dropped out after wave 1 or 2 (family daycare: eight children, childcare center: five children).

TABLE 1. COMPARISON OF CHILDREN IN DIFFERENT CARE TYPES

	(1)	(2)	(3)	(4)	(5)
	<i>All</i>	<i>Family daycare</i>	<i>Childcare center</i>	<i>Home- based/pri- vately orga- nized care</i>	<i>Family daycare vs. childcare center (t-statistics)</i>
<i>Child gender (1: male)</i>	0.51 (0.50)	0.54 (0.50)	0.51 (0.50)	0.51 (0.50)	-0.04 (1.37)
<i>Age of child</i>	6.98 (0.85)	6.88 (0.82)	6.97 (0.81)	7.02 (0.90)	0.10* (2.21)
<i>Number of siblings</i>	0.82 (1.03)	0.59 (0.84)	0.70 (0.90)	0.99 (1.15)	0.12* (2.35)
<i>Migrant background of respondent</i>	0.24 (0.43)	0.14 (0.35)	0.15 (0.36)	0.35 (0.48)	-0.01 (-0.62)
<i>Tertiary education of parents</i>	0.46 (0.50)	0.60 (0.49)	0.59 (0.49)	0.32 (0.46)	-0.02 (-0.62)
<i>West Germany</i>	0.80 (0.40)	0.79 (0.41)	0.73 (0.44)	0.87 (0.34)	-0.06* (-2.31)
<i>Sensorimotor development</i>	0.15 (0.36)	0.12 (0.33)	0.11 (0.31)	0.21 (0.41)	-0.02 (-0.97)
<i>N</i>	3481	410	1600	1569	1912

Notes: Column (1) to (4) shows the mean values and the standard deviations in parentheses. Column (5) shows the difference between family daycare and childcare centers and the t-statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1. The comparison for our dependent variables is available upon request.

4. Method

4.1 Ordinary least squares approach

In the first step, we use linear regression models to investigate if there is a relationship between the care type and a child's cognitive and socio-emotional abilities and the parent-child interaction in the short term up to the age of three years. The regression models are based on the ordinary least squares (OLS) method. The basic equation is:

$$Y_i = \alpha + \beta_1 CC_i + \beta_2 X_i + u_i \quad (1)$$

Y_i corresponds to each outcome variable considered for child i . CC_i indicates if child i attends family daycare before age three. X_i contains the covariates, and u_i is the robust error term. We control for observable factors at an individual and regional level.

However, there is a risk here of misinterpreting the estimated effects as causal if the selection of children into a particular form of education or care is not random. For example, a parent's decision on enrolling their child in a specific type of publicly funded childcare could relate to other unobserved characteristics of the family that also influence child development. However, we mitigate the selection problem by including the child's sensorimotor development at seven months of age (thus, the approach bears similarities to a value-added approach, see Todd and Wolpin 2003).

4.2 Regression models with individual fixed effects

In a second step, we exploit individual variation in a child's type of care over time and estimate models with individual fixed effects (FE). Here, the identification of the effect is based on a change from home-based care to family daycare (or a childcare center) from one wave to the next.

To compare the effect of family daycare with the effect of childcare centers on children, we first estimate the effect of family daycare (compared to home-based care) and additionally the effect of childcare centers (compared to home-based care) on children's competencies. We separate these two groups because both variables vary over time. However, this variation always affects solely one subsample.²²

In contrast to the OLS analysis, in which we compare children in family daycare with children in childcare centers, we now use the individual variation in a child's type of care over time. Children in home-based or privately organized care form the control group. The equation for both models is as follows:

$$Y_{it} = \alpha + \beta C_{it} + \mu_i + \gamma_t + u_{it} \quad (2)$$

Here, Y_{it} is the expression of the respective outcome variable considered for child i in wave t . C_{it} indicates whether child i is cared for in family daycare or a childcare center in wave t . μ_i is an individual fixed effect that contains all individual time-invariant characteristics, whereas γ_t includes all wave-specific factors. u_{it} is the robust error term.

The advantage of FE models is that individual characteristics of child i (or his family) that are constant over time, and age-specific effects that are constant for all

²² Children switch from home-based or privately organized care to either family daycare or a childcare center. Switching between family daycare and childcare centers is rare. We exclude these cases from our sample (see chapter 3.3).

children in a year, are factored out. This method eliminates bias in the estimated effect on child development due to time-constant omitted variables (e.g., parental IQ). Therefore, the FE model is closer to estimating a causal effect than the OLS model.

However, time-variant factors influencing child development might affect whether a child continues to stay in a specific type of care. For example, we cannot control for a change in parents' preferences for a particular type of care.²³ This issue might affect child development. Thus, an essential assumption for a causal interpretation of the FE estimates is that time-variant unobserved factors influencing child development do not affect shifts from home-based care to family daycare (or childcare centers).

We exclude all time-invariant individual factors (this includes the average duration of care across all waves) through individual fixed effects. We control for the average age-specific care intensity through wave fixed effects.²⁴ However, not all outcome variables of interest are available in panel structure, so we focus on *infant visual habituation*, parent-child interaction variables, and a variable of the socio-emotional competencies (*negative affectivity* of the IBQ).²⁵

²³ Alt et al. (2014) show that there are age-dependent preferences for family daycare or childcare centers, for which we can control. At the same time, we exclude transitions between care types due to changes in preferences or supply shortages by not considering children who switch between family daycare and childcare centers.

²⁴ In our data, there is mainly variation between waves; see Table A.2 in Appendix A.

²⁵ An instrumental variable approach is another possible causal identification strategy. One possible instrument – following the methodology of previous studies in the German context on the expansion of education and childcare (Cornelissen et al. 2018, Felfe and Lalive 2018) – would be the share of family daycare provision relative to the total childcare provision for children under three years at the district level (in 2010, as a percentage). We discuss this approach in Appendix B.

5. Results

5.1 Results of the ordinary least squares regressions

Table 2 shows the results of the estimates for cognitive and socio-emotional skills and parent-child interaction. The first column displays the estimates without covariates and the second column with covariates. For cognitive skills, we find no statistically significant differences between the influence of family daycare and childcare centers on child development. Moreover, the coefficients are close to zero.²⁶

A similar picture emerges for socio-emotional skills. Only for the variable *SDQ: problem behavior*, there is a statistically significant negative effect: children aged 27 or 39 months who are cared for in family daycare show, on average, a more pronounced problem behavior than children in childcare centers. For the parent-child interaction, all estimated coefficients are not statistically significant and close to zero, except for the *activity level*.

²⁶ Table A.5 in Appendix A exemplarily shows regression results for vocabulary with a stepwise addition of covariates.

TABLE 2. ESTIMATIONS FROM ORDINARY LEAST SQUARES REGRESSIONS

	(1)	(2)	(3)	(4)
	<i>Without covariates</i>	<i>With covariates</i>	<i>R² (with covariates)</i>	<i>N</i>
<i>Cognitive skills</i>				
<i>Vocabulary</i>	0.01 (0.08)	-0.05 (0.08)	0.15	1231
<i>SON-R Subtest</i>	-0.09 (0.07)	-0.07 (0.07)	0.04	1456
<i>Digit span</i>	0.00 (0.07)	-0.02 (0.07)	0.05	1262
<i>Delayed gratification</i>	0.01 (0.03)	0.01 (0.03)	0.02	1430
<i>Socio-emotional skills</i>				
<i>SDQ: prosocial behavior</i>	-0.05 (0.07)	-0.02 (0.08)	0.01	1408
<i>SDQ: problem behavior</i>	-0.15** (0.07)	-0.13* (0.07)	0.04	1396
<i>CBQ: negative affectivity</i>	-0.02 (0.06)	-0.02 (0.07)	0.02	1604
<i>CBQ: effortful control</i>	-0.03 (0.07)	-0.01 (0.07)	0.05	1604
<i>CBQ: surgency extraversion</i>	0.02 (0.06)	0.04 (0.06)	0.04	1604
<i>Parent-child interaction</i>				
<i>Positive mood</i>	0.02 (0.08)	-0.01 (0.08)	0.02	1299
<i>Negative mood</i>	-0.03 (0.07)	-0.02 (0.08)	0.02	1299
<i>Activity level</i>	0.10 (0.08)	0.07 (0.08)	0.03	1299
<i>Non-social sustained attention</i>	0.03 (0.08)	0.04 (0.08)	0.01	1299
<i>Positive engagement</i>	-0.02 (0.07)	-0.03 (0.08)	0.03	1299

Notes: Columns (1) and (2) each show the estimated coefficient from an OLS regression (one regression per row and column). Standard errors are in parentheses. All dependent variables are standardized. Parent-child interaction is measured in wave 3. IBQ: affect and infant visual habituation are unavailable in wave 4 and, therefore, not included. Individual covariates are the age and sex of the child, number of siblings, the respondent's migrant background, parents' tertiary education, West Germany, and sensorimotor development. Regional covariates are female labor force participation rate, GDP per capita, and conservatism. Covariates for childcare intensity are hours per week and the number of months. The analysis includes dummy variables for missing values for the number of siblings, sensorimotor development, and care intensity variables. They are 1 if the variable is a missing. The number of observations in column (4) applies equally to the specifics without and with covariates.

*p<0.1, **p<0.05, ***p<0.01.

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.

5.2 Results of the fixed effects estimations

Table 3 displays the results for children who switch from home-based care to family daycare in any wave (excluding children in childcare centers). For the control group, we include children who never switch to publicly funded childcare during the observation period (i.e., who are exclusively in home-based care).²⁷ Estimates show that attending family daycare harms *infant visual habituation* (information processing speed). The effect is statistically significant at the 1% level. The results for all other variables are insignificant.

Comparing these results with the estimates for children switching from home-based care to childcare centers in any wave (Table 4) reveals mixed results. For socio-emotional skills, childcare centers have a statistically significant negative effect at the 1% level on the *IBQ: negative affectivity* variable. Regarding parent-child interaction, there is a positive effect on *negative mood* and *activity level*, respectively, at the 5% and 10% levels. We find no significant results for *positive mood*, *non-social sustained attention*, and *positive engagement with the parent*. This is also true for *infant visual habituation* as a measure of cognitive skills.

Results of the fixed effects model are consistent with the results of the ordinary least squares model: the effect of family daycare on child development does not differ significantly from the effect of childcare centers on child development. When interpreting the results, it is essential to note that the ordinary least squares analysis compares children in family daycare with children in childcare centers. In contrast, in the fixed effects model, we analyze a child's individual variation in switching from home-based or privately organized care to one of the two publicly funded types of care over time.

While family daycare leads to slightly worse *infant visual habituation*, childcare centers lead to slightly worse scores on *negative affectivity* and less robust, *negative mood*. Though, childcare center attendance improves a child's *activity level*. However, no clear picture emerges that one form of childcare outperforms the other in its influence on child development. The following Chapter 6 discusses our results.

²⁷ If we exclude children who never switch to publicly funded childcare during the observation period, results do not change for children in family daycare. The statistically significant effects on *negative mood* and *activity level* for children in childcare centers become insignificant (tables upon request).

TABLE 3. RESULTS OF FIXED-EFFECTS ESTIMATIONS FOR CHILDREN IN FAMILY DAYCARE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<i>Cognitive</i>	<i>Socio-emo- tional</i>	<i>Parent-child interaction</i>				
	<i>Infant visual habituation</i>	<i>IBQ: negative affectivity</i>	<i>Positive mood</i>	<i>Negative mood</i>	<i>Activity level</i>	<i>Non-social sustain- ed attention</i>	<i>Positive engagement</i>
<i>Family daycare</i>	0.75*** (0.23)	-0.03 (0.06)	0.09 (0.10)	-0.05 (0.10)	0.12 (0.10)	0.05 (0.10)	0.11 (0.10)
<i>Number of siblings</i>	0.06 (0.15)	-0.13** (0.06)	0.01 (0.10)	0.10 (0.10)	0.02 (0.10)	0.01 (0.09)	-0.20* (0.11)
<i>Wave 2</i>	0.63*** (0.06)	-0.53*** (0.03)	-0.10 (0.06)	0.50*** (0.06)	0.15** (0.06)	-0.35*** (0.06)	0.08 (0.06)
<i>Wave 3</i>	/	-0.12*** (0.04)	0.19*** (0.06)	0.22*** (0.06)	0.02 (0.06)	-0.58*** (0.06)	0.46*** (0.06)
<i>Constant</i>	-0.31** (0.13)	0.34*** (0.05)	-0.10 (0.09)	-0.28*** (0.09)	-0.11 (0.09)	0.24*** (0.08)	-0.05 (0.09)
<i>R²</i>	0.29	0.13	0.02	0.06	0.01	0.10	0.08
<i>N</i>	1689	4454	2451	2451	2451	2451	2451

Notes: All dependent variables are standardized. For infant visual habituation, only observations in wave 1 and wave 2 are available. A low infant visual habituation score represents good infant visual habituation, corresponding to a lower measured eye fixation time and a faster information processing speed. The number of siblings is the only queried control variable that varies across waves. All other individual control variables are assumed constant and differentiated by the FE estimation. The sample is restricted to children cared for in a childcare center or home-based/privately organized care at least once in waves 1 to 3. Children who attended family daycare are excluded from the sample. Standard errors are clustered at the individual level and indicated in parentheses. *p<0.1, **p<0.05, ***p<0.01

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1

TABLE 4. RESULTS OF FIXED-EFFECTS ESTIMATIONS FOR CHILDREN IN A CHILDCARE CENTER

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<i>Cognitive</i>	<i>Socio-emotional</i>	<i>Parent-child interaction</i>				
	<i>Infant visual habituation</i>	<i>IBQ: negative affectivity</i>	<i>Positive mood</i>	<i>Negative mood</i>	<i>Activity level</i>	<i>Non-social sustained attention</i>	<i>Positive engagement</i>
<i>Childcare center</i>	0.15 (0.12)	-0.13*** (0.03)	0.05 (0.06)	0.14** (0.06)	0.10* (0.06)	-0.01 (0.06)	0.00 (0.06)
<i>Number of siblings</i>	0.24 (0.28)	-0.08 (0.05)	-0.07 (0.08)	0.06 (0.08)	-0.09 (0.08)	0.09 (0.07)	-0.13 (0.08)
<i>Wave 2</i>	0.65*** (0.05)	-0.52*** (0.02)	-0.10** (0.04)	0.49*** (0.05)	0.07 (0.05)	-0.31*** (0.05)	0.12*** (0.05)
<i>Wave 3</i>	/	-0.12*** (0.03)	0.16*** (0.06)	0.21*** (0.05)	0.00 (0.05)	-0.58*** (0.05)	0.47*** (0.05)
<i>Constant</i>	-0.42** (0.21)	0.30*** (0.05)	0.00 (0.06)	-0.28*** (0.07)	0.01 (0.07)	0.20*** (0.06)	-0.09 (0.07)
<i>R</i> ²	0.26	0.12	0.02	0.07	0.00	0.11	0.07
<i>N</i>	2937	7738	4534	4534	4534	4534	4534

Notes: All dependent variables are standardized. For infant visual habituation, only observations in wave 1 and wave 2 are available. A low infant visual habituation score represents good infant visual habituation, corresponding to a lower measured eye fixation time and a faster information processing speed. The number of siblings is the only queried control variable that varies across waves. All other individual covariates are assumed constant and differentiated by the FE estimation. The sample is restricted to children cared for in a childcare center or home-based/private organized care at least once in waves 1 to 3. Children who attended family daycare are excluded from the sample. Standard errors are clustered at the individual level and indicated in parentheses. *p<0.1, **p<0.05, ***p<0.01

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1

6. Discussion and conclusion

Based on the newborn cohort of the NEPS dataset, this study showed that attending family daycare instead of a childcare center has no negative consequences for children under the age of three for most studied dimensions.

We can confirm hypothesis 1 – the effect of family daycare compared to childcare centers on cognitive competencies is negative: the FE-model shows a negative effect of family daycare on *infant visual habituation*. However, attending family daycare does not negatively affect other cognitive skills measures studied. Thus, the result must be interpreted with caution.

We reject hypothesis 2 – the effect of family daycare on socio-emotional skills is positive. For almost all outcome variables of this dimension, the estimated coefficients for family daycare are close to zero and not statistically significant.

We confirm hypothesis 3 – it is irrelevant for parent-child interaction if a child attends a childcare center or family daycare. Concerning the OLS results, it is irrelevant whether a child attends family daycare or a childcare center. Our FE results, however, are inconclusive. Although attending a childcare center increases a child's *negative mood* in parent-child interaction, it also increases their *activity level*. We find no effects of family daycare on parent-child interaction.

In contrast to previous studies, our results do not suggest a pronounced negative impact of family daycare on child development compared to childcare centers. Finding no differences may be due to several reasons. While Datta Gupta and Simonsen (2010, 2016) examine the effects of a well-established early childcare system in Denmark, we look at the effects in a system that is still relatively young. Moreover, our study measures outcome variables of children aged three years or less, whereas introduced studies focus on older children. The varying effects of the care types might manifest at an older age.

The results of this study need to be more precise for a clear policy conclusion and should be complemented with analyses of further research projects. Future research should focus on the effects of the two types of care on children in the German context by applying causal analyses and appropriate data sets. This is especially true for analyses in the medium term and analyses concerning a successful transition of children to the school system.

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8. Appendix A: Further information on data and analyses in tabular form

TABLE A.1. OVERVIEW OF THE VARIABLES USED IN THE ANALYSIS

<i>Variable</i>	<i>Operationalization</i>	<i>Wave</i>
Type of care		
<i>Family daycare (wave 1-3)</i>	1: Child has attended family daycare at least once in waves 1-3, 0: if not	-
<i>Childcare center (wave 1-3)</i>	1: Child has attended a childcare center at least once in waves 1-3, 0: if not	-
<i>Family daycare (current wave)</i>	1: Child has attended family daycare in the current wave, 0: if not	1-4
<i>Childcare center (current wave)</i>	1: Child has attended a childcare center in the current wave, 0: if not	1-4
<i>Total number of months in family daycare</i>	Number of months that the child has spent in total in family daycare	1-4
<i>Total number of months in a childcare center</i>	Number of months that the child has spent in a childcare center	1-4
<i>Average number of hours per week in family daycare</i>	Number of hours a child spent in family daycare on average per week	1-4
<i>Average number of hours per week in a childcare center</i>	Number of hours a child spent in a childcare center on average per week	1-4
Dependent variables		
Cognitive skills		
<i>Vocabulary</i>	Total value of correctly solved items in the vocabulary test	4
<i>SON-R subtest</i>	WLE estimate of the ability of a child calculated by NEPS based on this competency test	4
<i>Digit span</i>	Sum of all correctly repeated digit spans	4
<i>Delayed gratification</i>	1: Child waited to receive the large gift, 0: if not	4
<i>Infant visual habituation</i>	Logarithm of the eye fixation time sum over all images in seconds	1-2
Socio-emotional skills		
<i>IBQ: negative affectivity</i>	Aggregate index of the following three items: When child is tired, how often do they show signs of stress and discomfort?; When child cannot have what they want, how often are they getting angry?; When you are busy with something else and child cannot get your attention, how often are they crying? Scale from 0-6	1-3
<i>SDQ: prosocial behavior</i>	Child is considerate, scale of 1-10.	4

TABLE A.1. (CONTINUED)

<i>SDQ: problem behavior</i>	Kind is a loner, plays mostly alone, scale of 0-8.	4
<i>CBQ: negative affectivity</i>	Aggregate index of the following three items: child is very frustrated when they cannot do what they want; child is depressed when they fail a task; child is very difficult to calm down when they are excited. Scale of 0-6.	4
<i>CBQ: effortful control</i>	Aggregate index of the following three items: child is very focused when drawing or coloring; child can get lost in looking at a picture book, looking at it for a long time; child likes soothing rhythmic activities such as swinging or rocking. Scale of 0-6.	4
<i>CBQ: surgency extraversion</i>	Aggregate index of the following three items: child is full of energy, even in the evening; child is very fond of romp games; child often rushes into new situations. Scale of 0-6.	4
Parent-child-interaction		
<i>Positive mood</i>	This scale captures the extent to which the child is amused and satisfied with the situation as a whole. Scale from 1-5.	1-3
<i>Negative mood</i>	This scale refers to how negative the child's mood is during the observation and if they express their dissatisfaction (crying, frowning, tense body, anger). Scale from 1-5.	1-3
<i>Activity level</i>	This scale refers to how active the child is in terms of gross motor skills during the observation. In addition to the frequency of gross motor activities, special attention should also be paid to their speed and energy. Slow or less energetic movements are rated relatively less strongly. Scale from 1-5.	1-3
<i>Non-social sustained attention</i>	This scale refers to how sustained a child's attention is to objects (and activities of others) – but not social activities. This is about the child's ability to focus on something for a period of time. Scale from 1-5.	1-3
<i>Positive engagement with parent</i>	This scale measures the extent if a child actively participates in interactions with the parent. Interaction is understood as an interplay (more than a mere reaction). In addition to reactions to actions of the parent, initiations of the child with reference to the parent are of interest. Scale from 1-5.	1-3
Individual covariates		
<i>Number of siblings</i>	Number of siblings	1-4
<i>Child gender (male)</i>	1: The child is male, 0: The child is female	1
<i>Age of child</i>	Age of child in months	1-4
<i>Migrant background of respondent</i>	1: Respondent has a direct or indirect migrant background, 0: no migrant background	1
<i>Tertiary education of parents</i>	1: At least one parent has a college or university degree, 0: no parent has a college or university degree	1
<i>West Germany</i>	1: Child lives in West Germany, 0: Child lives in East Germany	1
<i>Sensorimotor development</i>	Estimate for the child's ability (weighted likelihood estimator) which includes domains of cognition, motor skills, and language precursor skills	1

TABLE A.1. (CONTINUED)

Regional covariates		
<i>Female labor force participation rate</i>	1: The female employment rate in the district is above the German median in 2010, 0: if not	-
<i>Conservatism</i>	1: Proportion of people voting for the CDU/CSU or FDP in the district is above the German median in 2010, 0: if not	-
<i>GDP per capita</i>	1: The gross domestic product per capita in the county is above the German median in 2010, 0: if not	-

Notes: All items for socio-emotional skills are recoded so that positive values also have a positive meaning.

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1.

TABLE A.2. PROPORTION OF CHILDREN IN TYPES OF CARE AND THE INTENSITY OF CARE

	(1)	(2)	(3)	(4)	(5)
	<i>Wave 1</i>	<i>Wave 2</i>	<i>Wave 3</i>	<i>Wave 4</i>	<i>Wave 1-3</i>
Proportion of children per type of care					
<i>Family daycare (current wave)</i>	0.01 (0.10)	0.06 (0.24)	0.13 (0.34)	0.09 (0.29)	
<i>Childcare center (current wave)</i>	0.02 (0.14)	0.18 (0.38)	0.58 (0.49)	0.85 (0.35)	
<i>Family daycare (wave 1-3)</i>	0.12 (0.32)	0.13 (0.34)	0.15 (0.36)	0.14 (0.35)	
<i>Childcare center (wave 1-3)</i>	0.46 (0.50)	0.53 (0.50)	0.58 (0.49)	0.56 (0.50)	
<i>Age of child [in months]</i>	6.98 (0.85)	13.51 (1.42)	26.56 (1.28)	38.51 (1.12)	
Intensity of care					
<i>Average number of hours per week in family daycare</i>	18.86 (11.99)	23.67 (10.77)	26.90 (10.43)	27.40 (10.51)	24.97 (10.31)
<i>Average number of hours per week in a childcare center</i>	20.31 (12.68)	28.26 (11.02)	31.17 (9.62)	31.39 (8.84)	29.81 (9.64)
<i>Total number of months in family daycare</i>	/	3.84 (2.30)	13.48 (6.27)	21.30 (11.46)	13.74 (6.55)
<i>Total number of months in a childcare center</i>	/	3.60 (2.32)	12.07 (5.23)	19.67 (9.04)	13.65 (7.38)
<i>N</i>	3481	2849	2609	2478	2064

Notes: Column (1)-(4) show means and standard deviations in parentheses. Family daycare (current wave) and childcare center (current wave) are dummy variables. They are 1 if a child was in the respective type of care in the current wave. Family daycare (wave 1-3) and childcare center (wave 1-3) are dummy variables which are 1 if a child was in the respective form of care at least once in waves 1-3 (values across columns (1)-(4) are not identical in this calculation because the number of observations changes across waves). Column (5) shows the average number of hours per week or the total number of months a child was cared for in a specific type of care in waves 1-3 (only for those children cared for in this specific type of care).

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1.

TABLE A.3. QUALITY INDICATORS FOR CHILDCARE CENTERS AND FAMILY DAYCARE

	<i>Childcare center</i>					<i>Family daycare</i>				
	<i>Mean</i>	<i>SD</i>	<i>Min</i>	<i>Max</i>	<i>N</i>	<i>Mean</i>	<i>SD</i>	<i>Min</i>	<i>Max</i>	<i>N</i>
<i>Group size</i>	13.45	5.30	4.00	59.00	570	5.17	1.95	1.00	10.00	157
<i>Number of children per caregiver</i>	5.67	2.88	2.00	24.00	136	3.80	1.41	1.19	6.56	20
<i>Share of children with migrant background</i>	0.24	0.20	0.00	1.00	550	0.20	0.27	0.00	1.50	147
<i>Share of qualified caregivers</i>	0.99	0.09	0.00	1.00	589	0.41	0.49	0.00	1.00	134
<i>Number of caregivers with migrant background</i>	0.12	0.32	0.00	1.00	604	0.15	0.36	0.00	1.00	179
<i>Share of female caregivers</i>	0.97	0.16	0.00	1.00	618	0.96	0.19	0.00	1.00	181

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1.

TABLE A.4. SAMPLE FOR THE FIXED-EFFECTS APPROACH AND CARE BEHAVIOR

	(1)	(2)	(3)	(4)	(5)
	<i>Wave 1</i>	<i>Wave 1 to Wave 2</i>	<i>Wave 2</i>	<i>Wave 2 to Wave 3</i>	<i>Wave 3</i>
<i>Family daycare (current wave)</i>	19	107	124	161	258
<i>Childcare center (current wave)</i>	54	449	499	904	1333

Notes: Column (1), (3), and (5) document the total number of children who are in family daycare or a childcare center in the current wave. Column (2) and column (4) report the total number of children who switch from home-based or privately organized care to one of the two publicly funded care types from wave 1 to wave 2 and from wave 2 to wave 3, respectively. Same as in our fixed effects approach, the sample is restricted to children either in family daycare or in a childcare center at least once in the first three waves but who never attended both forms of care or who dropped out of one type of care. The latter excludes eight children in the family daycare sample and five children in the childcare center sample.

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1

TABLE A.5. OLS REGRESSIONS FOR VOCABULARY WITH STEPWISE ADDITION OF COVARIATES

	(1)	(2)	(3)	(4)	(5)
<i>Sum of vocabulary (wave 4)</i>					
<i>Family daycare (wave 1-3)</i>	0.01 (0.08)	-0.01 (0.07)	-0.01 (0.07)	-0.05 (0.08)	-0.05 (0.08)
Individual covariates					
<i>Number of siblings (wave 1)</i>		-0.08** (0.03)	-0.08** (0.03)	-0.08** (0.03)	-0.08** (0.03)
<i>Child gender (male) (wave 1)</i>		0.35*** (0.05)	0.34*** (0.05)	0.35*** (0.05)	0.35*** (0.05)
<i>Age of child (wave 1)</i>		0.08** (0.03)	0.05 (0.03)	0.06 (0.03)	0.05 (0.03)
<i>Migrant background of respondent (wave 1)</i>		-0.49*** (0.08)	-0.50*** (0.08)	-0.48*** (0.08)	-0.49*** (0.09)
<i>Tertiary education of parents (wave 1)</i>		0.44*** (0.05)	0.43*** (0.05)	0.43*** (0.05)	0.42*** (0.05)
<i>West Germany (wave 1)</i>		-0.07 (0.06)	-0.08 (0.06)	-0.12* (0.06)	-0.09 (0.07)
<i>Sensorimotor development (wave 1)</i>			0.08*** (0.02)	0.08*** (0.02)	0.08*** (0.02)
Care intensity					
<i>Hours per week (wave 1-3)</i>				-0.01*** (0.00)	-0.01*** (0.00)
<i>Number of months (wave 1-3)</i>				0.01 (0.01)	0.01 (0.01)
Regional covariates					
<i>Female labor force participation rate</i>					0.05 (0.06)
<i>Conservatism</i>					-0.05 (0.08)
<i>GDP per capita</i>					-0.01 (0.08)
<i>Constant</i>	0.09*** (0.03)	-0.78*** (0.25)	-0.54** (0.25)	-0.37 (0.27)	-0.39 (0.29)
<i>R²</i>	0.00	0.13	0.14	0.15	0.15
<i>N</i>	1231	1231	1231	1231	1231

Notes: Standard errors in parentheses. All dependent variables are standardized. Analysis includes dummy variables for missing values for number of siblings, sensorimotor development, and care intensity variables (hours per week and number of months) and are 1 if the variable is a missing. *p<0.1, **p<0.05, ***p<0.01

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1

9. *Appendix B: Additional analysis using an instrumental variable approach*

In the OLS and FE approaches, there might be an endogenous choice of the type of care by parents.²⁸ To address these possible endogeneity issues, another appropriate causal identification strategy would be an instrumental variable approach (IV).

One possible instrument – following the methodology of previous studies on education and childcare expansions in the German context for over-three-year-old (Cornelissen et al. 2018) and under-three-year-old (Felfe and Lalive 2018) children – would be the share of family daycare provision relative to total publicly funded childcare provision for children under three years (at the district level in 2010 as a percentage), shown in Figure B.1.²⁹

Figure B.1 shows that the shares of family daycare places in public childcare vary largely at the regional level. One reason for this may be the pressure for reform since 2005. In 2005, the *Daycare Expansion Act*³⁰ aimed to expand publicly funded childcare services for children under the age of three. The *Child Promotion Act* (KiföG) in 2008 emphasized this goal when the federal government announced that all children older than one year would have a legal right to a place in publicly funded childcare from August 2013 onwards. It also stipulated that at least 35% of all children under three should have access to early childcare by August 2013.

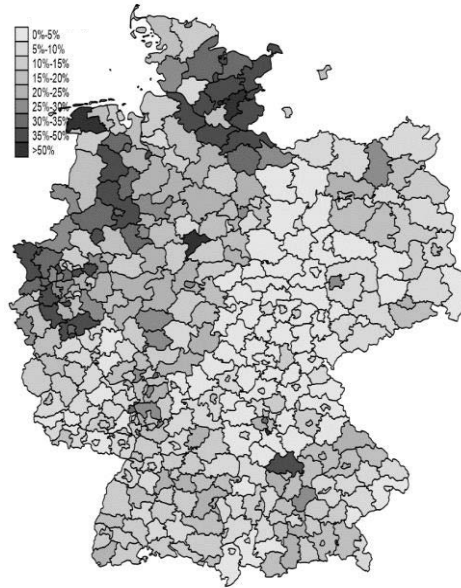
²⁸ In the OLS model, this is likely if time-invariant or time-variant and unobserved factors affect child development and the decision for a particular type of care. In the FE model, endogeneity problems arise (only) if there are time-variant unobserved factors.

²⁹ These data are part of the child and youth welfare statistics provided by the Research Data Center of the German Statistical Offices (Destatis 2010). Using a year before the mother's pregnancy and birth of the child in 2012 in our dataset is necessary to circumvent the problem that pregnant women or parents with newborns in our sample might have influenced these figures. The measure was first summarized in increments of five to comply with NEPS privacy guidelines. However, results do not change if we use all available variation in the NEPS on-site.

³⁰ The *Tagesbetreuungsbaugesetz* (Daycare Expansion Act) is the "Act on the quality-oriented and needs-based expansion of daycare for children (TAG)". It came into force on January 1, 2005. It established several changes in SGB VIII and other laws.

FIGURE B.1. SHARE OF FAMILY DAYCARE PLACES AT DISTRICT LEVEL IN 2010

Share of family daycare on all public care slots for children younger than three, in %



Notes: This figure shows the regional variation in the share of family daycare places on all publicly funded care places at the district level in 2010. Due to data restrictions, except for Rostock and Schwerin, Mecklenburg-Western Pomerania is not depicted.

Source: Destatis (2010).

A change that accompanied this reform was the legal equality of family daycare with childcare centers. Municipalities met the legal entitlement regardless of offering parents a place in a childcare center or family daycare for their children. The specific target was that 33% of all places for children under three in each municipality should be in family daycare. After the reforms in 2005 and 2008, there was a substantial expansion of publicly funded family daycare for children under three years (see Felfe and Lalive 2018). Zierow (2017) uses child and youth welfare statistics to show that, especially in regions where family daycare was little used until 2007, it expanded most substantially (by about 50%). This expansion resulted in a considerable variation between the districts regarding the share of family daycare places in all places of publicly funded childcare for children under three years of age (see Figure B.1).

To use the share of family daycare provision in relation to the total publicly funded childcare provision for children under three years as an instrumental variable, it must be an exogenous and relevant instrument. Thus, parental decision-making must not influence the instrument. We assume this is the case: In Germany, there is high excess demand for places in publicly funded childcare for children under three years. Also, the municipality legally fulfills the entitlement to a childcare place

if a place in family daycare is offered instead of a place in a childcare center. Therefore, we assume that it is not parental preference that decides the offered care form mix but the reform-related expansion and regional factors – for which we can control.

Consequently, if parents want a place in publicly funded childcare for their child, whether they are more likely to be offered a place in family daycare or a childcare center depends on the circumstances of the available care form mix. For a better understanding, one can think of the following thought experiment: There are two identical children whose parents would like to have a childcare place for their child. They only differ in the trait that they live in different districts. Child 1 lives in a district with an ample supply of places in childcare centers relative to family daycare, while child 2 lives in a district with a generous supply of family daycare relative to childcare center places. As a result, child 1 is more likely to get a place in a childcare center than child 2.

Thus, our assumption for the instrument being exogenous is that parents cannot influence the care mix in their district. Another critical assumption for an exogenous instrument is that unobserved characteristics do not determine a district's share of family daycare, which could also affect child development.

However, a problem with the latter assumption is that the NEPS sample includes only one birth cohort of children due to the study design. Papers using similar instrumental variables (Cornelissen et al. 2018, Felfe and Lalive 2018) can access multiple cohorts and exploit the change in expanding childcare provision for children under or over three years over time. In this case, it is possible to improve the instrument's exogeneity by including regional fixed effects in the model. Doing so can exclude the influence of time-invariant unobserved factors that might affect family daycare provision and child development. Unfortunately, this is not possible with the data we use.

Nevertheless, to provide a first input for future analyses with the available data, we conduct an instrumental variable estimation (IV estimation) with a two-stage least squares (2SLS) estimation and estimate the local average treatment effect (LATE). This means that we estimate the effect only for those children who are also affected by our instrument, i.e., children who attend a specific type of care only because of the particular care offered in their county.

In the first stage of the 2SLS-regression, we regress our endogenous variable family daycare (waves 1-3) on our instrument and all individual and regional covariates. We check the relevance condition of the instrument using an F-test for corresponding model specifications (see Tables B.1 to B.3). In the second stage, we replace the values of the endogenous variable with the estimated values obtained in the first stage and estimate the regression equation of interest using the instrument. When we include covariates in the regression analysis, the F-test falls to values below ten, which indicates a weak instrument. Although the estimation results are relatively similar to the OLS and FE estimates, they cannot be interpreted reliably due to the weak instrument problem and possible issues regarding the instrument exogeneity described above.

Another conceivable instrument would be the child's month of birth: Children who are not born in summer or fall might be less likely to be allocated to a place in a childcare center than children who are. The lower probability is linked to their first birthday later in a year. When children are admitted to kindergarten, which usually takes place in August or September, preference is given to already one-year-old children. Thus, children born in winter or spring might then be cared for in family daycare with a higher probability.

However, we cannot use this instrument in our analysis, as the birth month of children in our sample varies only between January and August of 2012. Only 20% of our sample were born between June and August. These children are theoretically affected by a lower probability of being allocated to a place in a childcare center. Children born between September and December are not covered at all in the data. When we assign a dummy variable to the subsample of children born between June and August, a negative correlation manifests between the dummy and the variables for attending family daycare or a childcare center. The negative correlation is sizable when we control for regional factors as well.

We can conclude that younger children are less likely to be in a type of publicly funded care and that the month of birth in our sample does not exogenously influence the choice of a childcare type. If a future study on the effect of family daycare on child development could use a data set that solved the problems described above or find a valid instrument that matches the NEPS data set, it would be of great potential.

TABLE B.1. 2SLS ESTIMATIONS FOR THE DEPENDENT VARIABLES OF COGNITIVE SKILLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Vocabulary</i>		<i>SON-R subtest</i>		<i>Digit span</i>		<i>Delayed gratification</i>	
<i>Family daycare instrumented with the share of family daycare</i>	-1.13* (-2.45)	-0.75 (-1.82)	-0.72 (-1.92)	-0.45 (-1.22)	-0.21 (-0.51)	-0.96* (-2.20)	0.20 (1.26)	0.02 (0.16)
<i>Individual covariates</i>	no	yes	no	yes	no	yes	no	yes
<i>Regional covariates</i>	no	yes	no	yes	no	yes	no	yes
<i>Constant</i>	0.33** (3.21)	-0.35 (-1.16)	0.20* (2.26)	0.50 (1.72)	0.05 (0.54)	-0.34 (-1.09)	0.71*** (19.73)	0.84*** (6.99)
<i>Coefficient 1st stage</i>	0.79*** (7.11)	0.93*** (6.90)	0.79*** (7.66)	0.91*** (7.31)	0.80*** (7.19)	0.91*** (6.79)	0.80*** (7.65)	0.91*** (7.22)
<i>R² 1st stage</i>	0.04	0.06	0.04	0.05	0.04	0.06	0.04	0.05
<i>F-statistics 1st stage</i>	50.56	5.95	58.61	6.71	51.76	6.20	58.55	6.69
<i>N</i>	1308	1308	1540	1540	1341	1341	1511	1511

Notes: Standard errors in parentheses. Individual control variables are the gender and age of child, number of siblings, migrant background (respondent), university degree (parents), West Germany, and sensorimotor development. Regional covariates are the female labor force participation rate, conservatism, and GDP per capita. The analysis includes dummy variables for missing values for number of siblings and sensorimotor development and are 1 if the variable is a missing. *p<0.1, **p<0.05, ***p<0.01

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1

TABLE B.2. 2SLS ESTIMATIONS FOR SOCIO-EMOTIONAL SKILLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	<i>SDQ: prosocial behavior</i>		<i>SDQ: problem behavior</i>		<i>CBQ: negative affectivity</i>		<i>CBQ: effortful control</i>		<i>CBQ: surgency extraversion</i>	
<i>Family daycare instrumented with the family daycare share</i>	-0.03 (-0.09)	0.25 (0.70)	-0.79* (-2.25)	-0.27 (-0.79)	-0.59 (-1.69)	-0.42 (-1.22)	0.34 (1.01)	0.46 (1.38)	-0.15 (-0.45)	0.03 (0.09)
<i>Individual covariates</i>	No	yes	no	yes	no	yes	no	yes	no	yes
<i>Regional covariates</i>	No	yes	no	yes	no	yes	no	yes	no	yes
<i>Constant</i>	-0.09 (-1.13)	-0.21 (-0.76)	0.18* (2.40)	0.03 (0.11)	0.04 (0.51)	-0.11 (-0.41)	-0.11 (-1.39)	-0.23 (-0.84)	0.16* (2.26)	0.23 (0.89)
<i>Coefficient 1st stage</i>	0.78*** (7.49)	0.91*** (7.28)	0.79*** (7.60)	0.91*** (7.23)	0.82*** (8.27)	0.93*** (7.88)	0.82*** (8.27)	0.93*** (7.88)	0.82*** (8.27)	0.93*** (7.88)
<i>R² 1st stage</i>	0.04	0.06	0.04	0.06	0.04	0.06	0.045	0.06	0.045	0.06
<i>F-statistics 1st stage</i>	56.15	6.63	57.74	6.73	68.36	8.34	68.36	8.34	68.36	8.34
<i>N</i>	1492	1492	1481	1481	1698	1698	1698	1698	1698	1698

Notes: Standard errors in parentheses. Individual control variables are the gender and age of child, number of siblings, migrant background (respondent), university degree (parents), West Germany, and sensorimotor development. Regional covariates are the female labor force participation rate, conservatism, and GDP per capita. The analysis includes dummy variables for missing values for number of siblings and sensorimotor development and are 1 if the variable is a missing. *p<0.1, **p<0.05, ***p<0.01

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1

TABLE B.3. 2SLS ESTIMATIONS FOR THE DEPENDENT VARIABLES OF PARENT-CHILD INTERACTION

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	<i>Positive mood</i>		<i>Negative mood</i>		<i>Activity level</i>		<i>Non-social sustained attention</i>		<i>Positive engagement with parent</i>	
<i>Family daycare instrumented with the family daycare share</i>	-0.08 (-0.22)	-0.58 (-1.37)	-0.37 (-0.89)	-0.18 (-0.42)	-0.41 (-1.02)	-0.607 (-1.40)	-0.30 (-0.72)	-0.30 (-0.70)	-0.54 (-1.32)	-0.43 (-1.02)
<i>Individual covariates</i>	No	yes	no	yes	no	yes	no	yes	no	yes
<i>Regional covariates</i>	No	yes	no	yes	no	yes	no	yes	no	yes
<i>Constant</i>	0.04 (0.50)	0.41 (1.29)	0.12 (1.27)	0.31 (0.94)	0.13 (1.41)	0.57 (1.73)	0.05 (0.55)	-0.12 (-0.39)	0.10 (1.03)	0.01 (0.04)
<i>Coefficient 1st stage</i>	0.76*** (6.94)	0.88*** (6.67)	0.76*** (6.94)	0.88*** (6.67)	0.76*** (6.94)	0.88*** (6.67)	0.76*** (6.94)	0.88*** (6.67)	0.76*** (6.94)	0.88*** (6.67)
<i>R² 1st stage</i>	0.04	0.06	0.04	0.06	0.04	0.06	0.04	0.06	0.04	0.06
<i>F-statistics 1st stage</i>	48.19	6.24	48.19	6.24	48.19	6.24	48.19	6.24	48.19	6.24
<i>N</i>	1373	1373	1373	1373	1373	1373	1373	1373	1373	1373

Notes: Standard errors in parentheses. Individual control variables are the gender and age of child, number of siblings, migrant background (respondent), university degree (parents), West Germany, and sensorimotor development. Regional covariates are the female labor force participation rate, conservatism, and GDP per capita. The analysis includes dummy variables for missing values for number of siblings and sensorimotor development and are 1 if the variable is a missing. *p<0.1, **p<0.05, ***p<0.01

Source: SC1 of the NEPS, doi:10.5157/NEPS:SC1:8.0.1

CHAPTER 3: PARENTAL LEAVE AND CHILDREN**

Maternal employment and long-term subjective well-being of children: Evidence from a parental leave reform in former East Germany

This study investigates the impact of extended paid parental leave in former East Germany from six to twelve months on subjective well-being and other outcomes of assigned children in adulthood. The setting featured high maternal labor market participation and almost universal standardized public childcare. By applying a difference-in-differences design and survey data, we find that center-based and maternal care in former East Germany were relatively substitutable. There is weak evidence for positive effects on life satisfaction, especially for males, for the reform of 1986. The effects are sizable for only a brief time frame around the reform and are not robust. There is weak, but not robust, evidence for a post-reform increase in some personality traits and income.

1. Introduction

Every child ought to have the opportunity to grow up in favorable conditions. This is a policy goal many industrialized countries have set as a goal. Especially in industrialized countries, policymakers increasingly aim at establishing policies and laws for child rights and welfare. For example, The United Nations Convention on the

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Rights of the Child has been effective since 1990. It was signed by 140 and ratified by 20 countries. Furthermore, a law is elaborated in Germany to strengthen child rights in the Basic Law for the Federal Republic of Germany. Targeting child rights and welfare is crucial for an individual and society because the early childhood conditions a child grows up in could impact personality, preferences, and competencies, affecting overall development. Also, parental time spent on the child can be crucial. Depending on the quality of childcare arrangements, early maternal employment might harm, and parental leave policies might benefit children. Once maternal care is substituted with other types of care (e.g., center-based care, family daycare, private care, such as nannies, siblings, grandparents, and neighbors), the quality of the respective care type is essential for child development as well.

Under which circumstances childcare conditions benefit or harm children is still discussed. Scholars from different disciplines have been tackling the question of how early childhood conditions affect the short-term development of children and if potential effects persist until adulthood. Child psychology literature is one of the first to suggest that early childhood conditions affect children's development. Spending sufficient time with the primary caregiver within the first year of life positively impacts a child's development, operating through attachment and bonding (Bowlby 1969). Additionally, recent economic studies show connections between early childhood conditions and an individual's development up until adulthood (e.g., Chetty et al. 2011; Currie and Almond 2011; Havnes and Mogstad 2011; Heckman et al. 2013; Gertler et al. 2014; Datta Gupta and Simonsen 2016; Baker et al. 2019).

There is a broad literature on the effects of parental leave and early maternal employment on children. These studies focus on various developmental aspects, such as health, socio-emotional and cognitive competencies. Short-term health studies, most often conducted with U.S. or Canadian data, show increases in children's health subject to parental leave (Rossin 2011; Stearns 2015; Lichtman-Sadot and Bell 2017; Bullinger 2019). Ruhm (2000) finds similar results for various reforms in European countries. The results of studies on parental leave are consistent with studies showing adverse effects of early maternal employment on children's health (Berger et al. 2005; Baker et al. 2008; Ruhm 2008; Morrill 2011). Increased contact with many other children might explain short-term adverse health effects, leading to a higher frequency of illnesses. However, not all studies report substantial effects: Baker and Milligan (2008) and Sayour (2019) find no significant impacts on health for an extension of paid maternity and parental leave in Canada in 2000.

Long-term studies based on different countries confirm that, by tendency, the benefits of children from parental leave are persistent and sizable in adolescence and adulthood (Morrill 2011; Carneiro et al. 2015; Danzer et al. 2020; Fabel 2021). In most of these studies, maternal care replaces or was replaced by informal care (see Table A.6 for short-term and A.7 for long-term parental leave studies). Maternal care replacing informal care might be a reason for sizable health effects, as the quality of informal care is often worse than the quality of maternal care. Especially for advantaged children, maternal labor supply reduces a child's time spent in an enriching home environment.

In terms of socio-emotional competencies, the results are less clear. Short-term parental leave studies for various countries find mostly no effects on children's behavioral, social, and emotional development (Baker and Milligan 2015; Albagli and Rau 2019; Huebener et al. 2019). Baker and Milligan (2010) suggest small but mostly insignificant results on children's social development for a Canadian reform in 2000. Sayour (2019) show improvements in the emotional disorder score but no other effects of the same reform. In contrast, studies on early maternal work in a U.S. or Canadian context reveal, on average, negative results resulting from maternal employment (Berger et al. 2005; Baker et al. 2008). Furthermore, Brooks-Gunn et al. (2010) show adverse effects on the socio-emotional competencies of children for full-time but positive effects for children of part-time working mothers. Hsin and Felfe (2014) find adverse effects for mothers with a high-school education. Lastly, Houmark et al. (2022) suggests that adolescent school-related socio-emotional competencies increase for a Danish parental leave reform.

The mixed results on socio-emotional competencies stem from different counterfactual care scenarios and maternal selection into parental leave or labor market participation. Additionally, Hsin and Felfe (2014) point out that increased maternal labor market participation does not necessarily lead to decreased high-quality time spent with the child. They show that working mothers often trade a high quantity of time for a better quality of time and that effects also depend on paternal time spent on the child.

Results of the comprehensive literature on the effects of early maternal employment on children's cognitive competencies are often mixed and differ between subgroups of children. Again, differences in counterfactual care and maternal selection into parental leave and market work are significant. Baker et al. (2008) report adverse outcomes of early maternal work on children's motor skills. Furthermore, Ruhm

(2008) shows adverse effects only for advantaged adolescents and benefits of limited market work for disadvantaged children. Hsin and Felfe (2014) suggest similar results by showing adverse cognitive outcomes for children of high-school-educated mothers. Waldfogel et al. (2002) report adverse effects for non-Hispanic white children of first-year maternal employment and some positive effects of second- and third-year maternal employment, but no effects for African American or Hispanic children. Brooks-Gunn et al. (2010) show that full-time maternal employment in the first twelve months of a child's life is associated with lower scores on some measures of cognitive competencies. However, part-time work is related to better cognitive outcomes. In the same manner, previous parental leave literature suggests mixed effects as well: Baker and Milligan (2010) and Huebener et al. (2019) report no short-term effects, whereas Baker and Milligan (2015) and Canaan (2022) show adverse effects on children's verbal development. In contrast, Albagli and Rau (2019) report positive effects on children's cognitive, language, and motor skills in Chile. Results are more substantial for children of mothers with lower education.

Long-term studies are similarly inconclusive. Bettinger et al. (2014) and Carneiro et al. (2015) report positive cognitive effects of parental leave reforms in Norway. Studies examining effects of parental leave extensions often find inconsistent or only sub-group effects. Wuertz Rasmussen (2010) and Dahl et al. (2016) find no effects. In contrast, Liu and Nordstrom Skans (2010) point out advantageous effects for children with high-educated mothers, whereas Danzer and Lavy (2017) only find effects for sons of high-educated mothers. Dustmann and Schoenberg (2012) show inconclusive results, as they find mostly no or minimal effects on cognitive development and some adverse effects on school track information. Danzer et al. (2020) report positive effects on human capital only if the reform replaces informal care with maternal care. Ginja et al. (2020) show positive effects on schooling performance and college attendance of the older but not the younger child. They also find children from mothers of a higher socioeconomic background benefit more. However, previous studies on labor market outcomes show positive effects (Dustmann and Schoenberg 2012; Carneiro et al. 2015; Danzer et al. 2020). In these cases, maternal care replaced mostly informal care arrangements.

Thus, the earlier raised question of whether early childhood conditions affect child development is not yet fully answered. Causes for inconsistencies in results are, for example, differences in the generosity and length of parental leave. The counterfactual care mode to parental care, the self-selection of mothers into the labor market, and the selection of specific groups of children into specific alternative care modes

are also important. Studying short- or long-term effects of parental leave or maternal employment is another reason for differing results. Lastly, studies on parental leave introductions often yield more noticeable results than studies on leave extensions.

We contribute to this literature by investigating a historical parental leave reform scenario in former East Germany (*German Democratic Republic, GDR*) and its not-yet-studied long-term effects on affected children's subjective well-being. The reform made mothers eligible for twelve months of paid parental leave instead of six months of maternity leave. The reform was announced unexpectedly and on short notice in mid-April 1986 (Helwig 1988). It offered job-protected paid leave with a wage-replacement rate depending on sickness allowance (70 to 90%) (Kreyenfeld 2004). The new scheme applied to all parents³¹ of children younger than twelve months on May 1st in 1986.

With this setting, we circumvent methodological challenges of the selection of women and mothers into the labor market and the lack of a distinct counterfactual scenario to maternal care. Previous studies often face the issue that only a few women and mothers were employed before parental leave was introduced or extended. Thus, the care situation changed only for a selective group of children. Studies examining countries with a high share of female labor force participation often face the issue that the demand for specific childcare types varies significantly by parental background. In this case, parental leave reforms could imply a change from low-quality informal or formal care to maternal care for some children or from high-quality formal care to maternal care for others.

In contrast, the extension of paid parental leave in former East Germany significantly changed the care situation of almost all children. Before the reform, they shared a similar counterfactual care scenario (center-based care) because former East Germany featured high shares of female labor force participation and almost universal public center-based care. About 90% of working-aged women and mothers were (full-time) employed in the 1980s (Winkler 1990). Standardized full-day care for children from zero to six years was available for most children. It was organized and supervised by the government. Also, there was almost no variation between available childcare institutions and thus no selection of specific children into a particular type of childcare (Helwig 1987; Braun and Klein 1995).

³¹ Fathers were also eligible but barely used parental leave (Groeben 2011).

This paper ties in the literature on the effects of early childhood conditions on a child's development and, more precisely, on persisting effects until well into adulthood. The study contributes to literature focusing on living conditions in a welfare state and builds upon broader discussions about measuring a population's collective happiness and well-being by additional measures besides the gross domestic product (e.g., Diener and Suh 1998; Veenhoven 2001; Kahneman and Krueger 2006; Clark et al. 2008; Diener 2009; Deaton and Stone 2013; Benjamin et al. 2019). As there is a broad political and general interest in well-being research, recent parental leave studies also focus on parental subjective well-being. They find inconsistent and often negligible effects, although there is an indication towards positive effects on parental subjective well-being (Pezzini 2005; D'Addio et al. 2014; Maeder 2014; Korsgren and van Lent 2022; Heisig 2023).

In this vein, subjective well-being effects of parental leave can be a relevant study focus for affected children. To our knowledge, this is the first paper studying parental leave's effects on affected children's life satisfaction and personality traits in adulthood. Our study is closely related to Houmark et al. (2022). They find persistent effects of extended parental leave for adolescents' school-related subjective well-being and socio-emotional competencies in Denmark.³² Our paper differs from Houmark et al. (2022) as our outcome variables for life satisfaction and personality traits are more general.

We apply a difference-in-differences (DiD) approach, where we compare outcomes of the affected and not affected firstborn children by the 1986 reform. We use laterborn children born in the same cohort as a control group to isolate the reform effect. Important is that children are not siblings of the firstborn children under study. Mothers of laterborn children could already use twelve months of parental leave due to a reform of 1976. They were thus unaffected by the reform of 1986.

We causally identify the effects of a parental leave extension without strong assumptions regarding common trends across states or other regions. Still, other concerns might exist about unobserved events with different impacts on firstborn and laterborn children, such as the Chernobyl disaster in April 1986 and the fall of the Berlin

³² Houmark et al. (2022) focus on questionnaires about a child's school life. Questionnaire examples are for well-being: "Are you happy with your school/class?", for emotional stability: "How often do you feel safe at school?" and for Conscientiousness: "Are you able to concentrate in class?"

Wall in 1989. We use the reform of 1976 as an alternative scenario³³ and discuss other challenges. As we use a historical setting of a former communist regime, we are unaware of administrative databases for our analysis. As a data source, we rely on the representative survey data of the German Socio-Economic Panel (SOEP).

Our results show that in former East Germany, extended paid parental leave by six months to a total of twelve months barely affected children's outcomes in the long term. There is weak evidence for positive effects on life satisfaction if we use a limited time frame around the reform. The effect on life satisfaction varies between 27.7% and 44.8% of a standard deviation, corresponding to an increase of 4.6% to 7.6% of the pre-reform mean. There are also small effects on income and adverse effects on neuroticism. However, these effects most likely stem from changes within the control group, not the assigned group. Effect sizes on life satisfaction for males are larger and vary between 40.3% and 63.8% of a standard deviation, corresponding to an increase in life satisfaction of 7.1% and 11.3%. Small effects on conscientiousness, extraversion, and openness and some adverse effects on neuroticism are driven by cohorts born close to the German reunification. We observe no reform effects for females, apart from an increase in income that is not robust. In general, effects are not robust if we correct p-values for multiple hypothesis testing. This suggests overall that center-based care in former East Germany was relatively similar to maternal care, especially regarding health and cognitive development in adulthood. For these aspects, we find no effects. These findings are manifested by a supplementary analysis on the effects of the parental leave reform of 1976.

Finding no effects on most variables corroborates the results of previous studies on extensions of parental leave on children's outcomes in various countries (e.g., Baker and Milligan 2008, 2010 and 2015; Liu and Nordstrom Skans 2010; Wuertz Rasmussen 2010; Dustmann and Schoenberg 2012; Dahl et al. 2016; Huebener et al. 2019). Results on life satisfaction and, for males, conscientiousness are in line with a study by Houmark et al. (2022), who observe positive effects on adolescents' school-related subjective well-being and socio-emotional skills for a parental leave extension in Denmark.

The remainder of this study is as follows. Chapter 2 describes the background setting of the reform. Chapter 3 introduces the empirical approach, and Chapter 4 the

³³ We do not use firstborn and laterborn children in former West Germany as a control group or placebo reform scenario, as there were parental leave reforms in West Germany in our study period as well (see Dustmann and Schoenberg 2012).

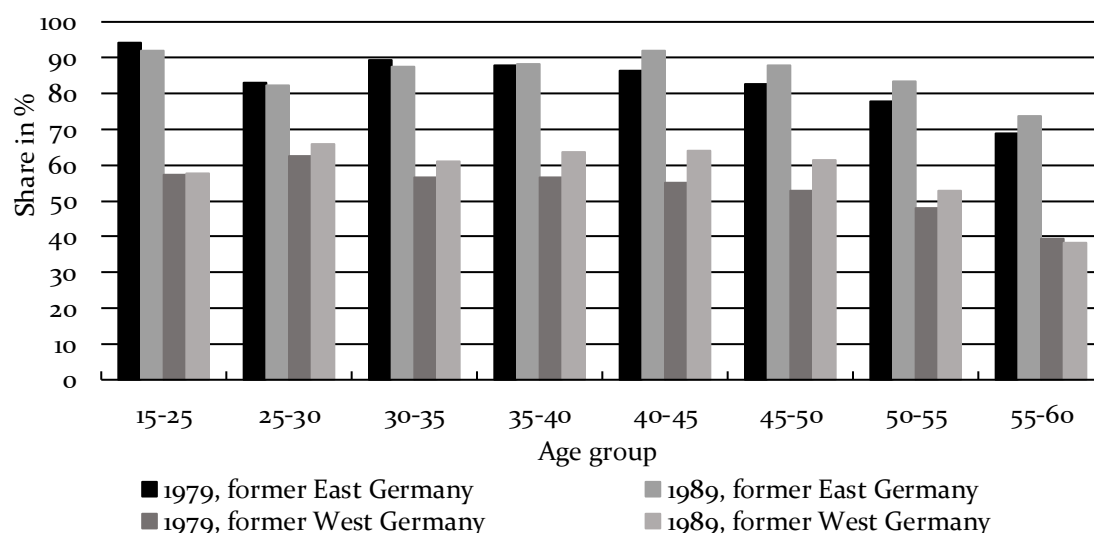
data. Chapter 5 presents results, a heterogeneity analysis by gender, and various sensitivity analyses. We discuss the results and conclude the study in Chapter 6.

2. *The historical background and institutional setting*

Due to economic circumstances and a shortage in labor supply, the government of the German Democratic Republic (GDR) challenged in the 1960s to increase female labor market participation, especially of married women and mothers. Almost half of all manufacturing plants were destroyed in the war. Also, there were high war reparation payments to the USSR and a shrinking working-age population due to internal migration to West Germany before the Berlin Wall was built in 1961 (Oberreis 1986). Subsequently, the GDR introduced policies to promote the participation of females in the labor market and women's rights, such as the law on maternity and child protection of 1950, the family code of 1965, and the law on abortion of 1972.

The average share of female labor force participation was 83.8% in 1979 and 85.8% in 1989, compared to 53.6% in 1979 and 58.0% in 1989 in former West Germany (see Figure 1). Most women in former East Germany were employed full-time (Oberreis 1986; Winkler 1990). Officially, there was full employment. However, many companies were labor hoarding (Akerlof et al. 1991; Dornbusch and Wolf 1994; Hoffmann 2005), resulting in sometimes less intense working life for employees.

FIGURE 1. LABOR MARKET PARTICIPATION OF WOMEN IN FORMER EAST AND WEST GERMANY



Notes: The shares for both the German Democratic Republic (GDR) and former West Germany do not include women in education or training.

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

Fertility decreased in the late 1960s in many developed countries, which also applied to former East Germany. To counteract this development, maternity and parental leave were extended. Many other policies were implemented, such as birth grants and child benefits (for a tabular overview of family policies, see Kreyenfeld 2004). This study focuses on a paid parental leave extension ("*baby year*") implemented on May 1st, 1986. Before the reform, first-time mothers could take six months of paid and job-protected maternity leave (100% wage replacement rate), and after, most of them returned to work. Their children were enrolled in formal center-based child-care, as unpaid parental leave was barely used due to the social desirability of mothers' labor force participation. Importantly, first-time mothers were the only subgroup not yet enjoying one year of paid parental leave.

The new reform allowed first-time mothers for the first time to take a maximum of one year of leave. The wage-replacement rate varied from 70 to 90%, depending on the mother's sickness allowance (Kreyenfeld 2004). The reform was announced on short notice in mid-April. Thus, we can rule out that the timing of birth was affected. The reform applied to all mothers of children younger than one year by May 1st (Helwig 1988). Consequently, from 1986 on, firstborns were also eligible to spend the whole first year of life with their mother instead of only six months (see Table 5).

TABLE 5. MAXIMUM OF TIME CHILDREN SPENT WITH THEIR MOTHER AFTER BIRTH

	1976-1986	Starting in 1986
<i>Firstborn children</i>	6 months	12 months
<i>Laterborn children</i>	12 months	

Notes: The duration of twelve months is divided into six months of maternity leave and six months of parental leave.

However, there was no clear cut-off rule. Most mothers with infants younger than five to six months were still on maternity leave when the reform was announced. Mothers with infants born in January 1986 or later were also likely to take the full twelve months of parental leave. The situation is less clear for mothers with infants born earlier, as they might have already returned to work. For example, mothers with a firstborn child aged ten months on May 1st, 1986, could apply for two additional months of leave. Historical data show that up to 95% of eligible mothers used parental leave 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 six to twelve months from formal care in childcare centers to maternal care. In 1988, only

1% of all children below the age of one year were still cared for in childcare centers (Israel 2008). Notably, the reform did not increase fertility (Buettner and Lutz 1990; Conrad et al. 1996).

One might argue that mothers who could afford 10 to 30% less income self-selected into applying for the *baby year*. 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, proposes no considerable self-selection. Another basis for this assumption is the unmatched equality of income in former East Germany: the Gini coefficient was 0.22 in 1980 and 0.20 in 1987 (West German states: 0.32) (Galbraith et al. 2017). The low Gini coefficient implies a relatively small income inequality and a low probability that specific mothers self-selected into applying for the entire or a shorter duration.

In former East Germany, formal public care in childcare centers supervised by the health ministry was the alternative care mode to parental care within the first year of a child's life. Public childcare availability was almost universal: In 1986, center-based care was available for more than 80% of all infants and toddlers (Statistisches Jahrbuch der DDR 1988). About 90% of all newly born to three-year-old children attending center-based care were enrolled on a full-time basis (Helwig 1987; Braun and Klein 1995). Available were childcare centers (97.7% of all available places in 1989), which opened from 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 run by municipalities; state-owned enterprises ran about 5%. There were no independent and almost no church-operated childcare facilities or other informal care modes (e.g., nannies; childcare on a full-time basis by grandparents or friends was also rare) (Zwiener 1994).

Since childcare centers were required to follow strict regulations, the quality did not vary significantly across the centers, regardless of the cities where they grew up, guaranteeing relatively similar conditions for all children. The focus of the childcare centers' programs was to foster children's health and enhance cognitive development. Children should be able to play, learn to handle everyday situations, and develop positive relations with childcare center staff and other children. Also, childcare centers aimed to stimulate senses, perception, cognitive activity, language acquisition, and moral values of children (Zwiener 1994). Important to note is that socialistic views were already communicated to children in childcare centers, being a constraint for comparability to contemporary childcare centers.

All childcare teachers had a three-year theoretical and practical training at a vocational medical school and specialized in early childhood education, psychology, and pediatrics. This provides evidence that staff quality was relatively high and comparable to standards in modern Germany. However, quality was expectedly lower than in contemporary centralized care in France, Sweden, or the U.S. (Zwiener 1994; OECD 2017). Group sizes varied between five or six children per teacher in childcare centers for children under three years, which is considered large and implies less child orientation. Tietze and Foerster (2005) suggest a minimum of one caretaker per three children below one year of age for a daily care duration of longer than nine hours, which was the most prevalent setting for children in former East Germany (Zwiener 1994).

3. *Empirical approach*

The introduction of the parental leave reform targeted at firstborns provides a quasi-experimental setting. We apply a difference-in-differences approach (DiD) to estimate the effects of the reform on children's outcomes in adulthood. In our main analysis, we compare the differences in the outcomes of firstborn children born before and after the reform in 1986 with those of laterborn individuals born before and after the reform in 1986. Due to missing clear cut-off rules, we treat children born in January 1986 and later as assigned to the reform, as their mothers were still on maternity leave in April 1986 and were most likely to enjoy the same extended leave period as children born in May 1986 or later. As individuals born between June and December 1985 might have been partially treated, we exclude them from our analysis. We re-define this sub-group as assigned to the reform in a sensitivity check.

The DiD approach requires our treatment and control group to have no time-varying differences in the absence of the reform because of the common trends assumption. There are few outliers due to the small sample size resulting from this historical setting, but in almost all cases, there are distinct pre-reform common trends in our outcome variables (see Figure 3). The DiD specification we estimate is the following:

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

y_{ij} is a measure for long-term life outcomes of child i of birth cohort j . The interaction effect between $(Firstborn)_{ij}$ and $(Postreform)_{ij}$ identifies firstborn individuals whose mothers were assigned to the reform and eligible for one year of paid leave. λ

is the coefficient of interest and measures the effect of the leave extension on children's outcomes. π_j measures birth year fixed effects. $(Firstborn)_{ij}$ is a dummy indicator for firstborn individuals, and the coefficient β_1 captures all possible permanent and general differences between individuals born firstborn or laterborn. $(Postreform)_{ij}$ is a dummy indicator for individuals born on or after the first of January 1986. The coefficient β_2 captures the difference of laterborn children between the pre-and post-reform period. Additionally, the vector x_i contains individual and parental covariates. c is the constant, and ϵ_{ij} is the heteroskedasticity-robust error term. We also control for seasonality by including a dummy variable for the child's birth month.

Important to note is the intention-to-treat nature of our estimates since we do not know from the data if and how long mothers took leave and whether mothers worked before the respective birth. However, given the institutional setting of former East Germany, it is very likely that the reform treated a large majority of eligible individuals.³⁴

In this setting, we do not face issues of spillover effects on (not born) older siblings (Bettinger et al. 2014; Ginja et al. 2020).³⁵ However, using laterborn children as a control group, we assume that unobserved events had similar impacts on firstborn and laterborn individuals, e.g., the fall of the Berlin Wall in 1989 and the German reunification in 1990. Furthermore, the Chernobyl disaster occurred in late April of 1986, and firstborn and laterborn individuals might have been affected differently by this adverse event. We discuss both events as possible channels.

4. Data and descriptive statistics

4.1 Data source and variable calculation

Our analysis of this historic reform relies on survey data from the German Socio-Economic Panel (SOEP), a representative sample of the population in Germany (Goebel et al. 2019). We use data from 1991 to 2019, depending on the variables and the analyzed reform. We identify an individual as citizen of the former GDR with

³⁴ We are unaware of data allowing us to analyze this reform's short-term maternal labor supply effects. The SOEP includes East German individuals not before 1990/91. Available data on former East German women's labor supply does not contain information on the age of women's children.

³⁵ The reform of 1976 could indirectly affect older siblings and influence the control group of firstborns. This adverse effect might be a downward bias for DiD estimates.

the question if they ‘lived in the GDR before 1989’. We exclude individuals who lived in the GDR before 1989 but were born in a West German state or another country. Also required for the identification strategy is information on birth year, siblings, and birth order. We rely on survey data of the SOEP since we are unaware of any administrative or other data set with information on birth order. Note that firstborn and laterborn children in our sample are not members of the same household and that we include individuals who moved to West Germany after the German reunification. In the main analysis, we calculate observation averages for individuals between 18 and 37 years old, although the maximum age varies depending on the time frame we use around the reform.³⁶

Assignment to the reform

Firstborn individuals were assigned to the reform, and thus the last group of children who could spend twelve months after birth with their mother instead of only six months. We restrict the sample to birth years 1982 to 1988 to cover a sufficient time frame around the reform. However, we vary the time frames in sensitivity analyses. The reform was implemented on May 1st, 1986. We classify all firstborns born on or after January 1st, 1986, as subject to the reform because their mothers were still on maternity leave and most likely to benefit from all additional six months of leave. Firstborns born between June and December 1985 might have been partially exposed to the reform, depending on their mother’s maternity leave duration and labor market status. Therefore, we exclude individuals born between June and December 1985 from the sample.³⁷ Children born before June 1985 are not subject to the reform. We calculate the DiD variable as the interaction of the variables *firstborn* and *postreform*.

Outcome variables

Spending the whole first year of life with one’s mother instead of only six months might impact general well-being in the long term. We use a self-assessment measure for *life satisfaction* (being satisfied with the current life in general) for individual well-being on a scale between zero and ten, where zero is entirely unsatisfied, and ten is entirely satisfied. We calculate the average using survey years

³⁶ In sensitivity analysis, the maximum age we base average calculations on is 39 years for the birth cohort of 1980.

³⁷ We reclassify firstborns born between June and December 1985 as assigned to the reform in a sensitivity analysis.

for which respondent data is available (1997 to 2019). Although there is criticism of individual well-being measures concerning reliability and variation by age (Baird et al. 2010), this does not apply to the life satisfaction measure in the SOEP, as it maximizes validity and reliability (Kroh 2006).³⁸ Also, the measure varies only slightly in the observed age groups.

Spending the whole first year of life with the mother might affect the long-term personality outcomes of children. We focus on the established measure of the *Big Five personality traits*, which includes *agreeableness*, *conscientiousness*, *extraversion*, *neuroticism*, and *openness* (see Table A.8 for more information). We calculate averages of the underlying variables on a scale of one to seven. We can include five data points for the survey years between 2005 and 2019.

Due to a more extended breastfeeding period and less contact with other children in the first year of a child's life, long-term health might be affected due to an extended parental leave duration. We use a *subjective health* measure based on self-reported overall health on a scale from one to five, where five is the best possible value (after recoding). We calculate the average of all available data points from the survey years 1997-2019.

Depending on the degree of cognitive stimulation in the home environment or childcare center, spending one year instead of six months with the mother might impact a child's long-term educational success. We calculate an individual's average number of *years of education* using all available survey years 1997-2019.

As extended parental leave might impact educational success, it might also affect labor market outcomes. We use data on the (logged) average *gross income* over the last observed ten years (survey years 2008-2018) as a proxy for labor market success.

Covariates

On an individual level, we include cohort fixed effects. The year-of-birth dummies capture age differences, as individuals born later are, on average, younger. We also control for the month of birth to control for seasonality, gender, and if the individual grew up in an urban or rural region. On a family background level,

³⁸ Life satisfaction as a measure of subjective well-being is methodologically advantageous to using happiness scales. Criticism of happiness literature using only a few ordered categories (e.g., "very happy," "pretty happy," and "not too happy") does not apply to the measure of life satisfaction (Bond and Lang 2019).

we include maternal and paternal education, defined as having a high school education. In a sensitivity analysis, we swap it with maternal and parental tertiary education.

Channel variables

We include potential channel variables through which main outcome variables might be affected. Note that channels are introduced more in depth in Chapter 5.3. We use variables on general *trust* and *family satisfaction* for attachment and bonding and family stability. Trust is measured on a scale between one and four. We recode the variable in a way that higher values are associated with higher general trust in other people. The variable is not annually queried and calculated by the average over available data points for waves 2003-2018, covering six waves. Family satisfaction has been queried annually since 2006. We use survey years 2006-2019. It is measured on the same scale as life satisfaction. We also test if there is a reform effect on *democracy satisfaction*. If childcare centers in former East Germany might have facilitated socialist thinking, democracy satisfaction might increase with the parental leave reform. The variable was queried in waves 2005, 2010, and 2015 and is measured on the same scale as life satisfaction. If the reform increased fertility, affected individuals might have been influenced through the channel of the number of siblings. Therefore, we analyze reform effects on the *number of younger siblings*. Being born closer to the German reunification might influence if an individual or a child's family moved to West Germany. We analyze the *current region of living* by using data from the survey year with the most recently available information.

4.2 Descriptive statistics

Table 6 shows summary statistics for firstborn and laterborn individuals born in 1982-1985 and 1986-1988. On average, firstborn individuals score higher in many aspects than laterborn individuals, such as life satisfaction, agreeableness, conscientiousness, openness, and education. In all cases, the differences are more pronounced for the time frame after the reform than before. This indicates that firstborn and laterborn individuals of birth cohorts before 1986 were more similar to each other than of birth cohorts in 1986 and thereafter. There are different explanations for this: Firstly, the reform did make a difference. Secondly, we observe more survey years for early birth cohorts than for later cohorts, as averages are calculated from a higher number of observations. Thirdly, individuals of later cohorts are, on

average, younger, which might lead to more significant deviations from the average, which is an issue we deal with because of the relatively small subsamples.

On an 11-point scale, firstborns score on average between 7.07 and 7.40 points in life satisfaction, laterborn individuals score between 7.02 and 7.13 for the observed pre- and post-reform period. The most considerable difference between averages of pre- and post-reform cohorts in Big Five personality traits of firstborn and laterborn individuals is observed for extraversion. Firstborns born before the reform score on a 7-point scale between 5.37 and 5.35, whereas laterborn individuals score between 5.81 and 5.61. First- and laterborn individuals score lower in agreeableness and conscientiousness for post-reform cohorts than pre-reform cohorts. Neuroticism and openness averages of firstborns of pre- and post-reform cohorts are relatively similar, whereas post-reform laterborn individuals score better in neuroticism and worse in openness than pre-reform cohorts.

In terms of health, pre-reform cohorts of laterborn individuals score slightly higher than firstborns. For post-reform cohorts, there is a reversed pattern. On a 5-point scale, the averages range for firstborns between 3.85 and 3.99 and for laterborn individuals between 3.87 and 3.92. The difference in years of education is substantial: firstborns are, on average, half a year longer in education (12.35 or 12.03 years in education) than laterborn individuals (11.78 or 11.53 years). Regarding the logged gross wage, firstborns of post-reform born cohorts score highest and laterborn individuals lowest. The difference between both groups is not as distinct for pre-reform cohorts with a larger average logged value observed for laterborn individuals.

Concerning covariates, there is a distinct difference in parental education: firstborns have on average higher educated mothers and fathers than laterborn individuals. 21.9% (post-reform) to 25.1% (pre-reform) of firstborns have mothers with high school education; for laterborn individuals, only 19.1% (post-reform) to 21.7% (pre-reform) have the same. Shares for high school-educated fathers are somewhat higher. 68.9% to 69.4% of firstborns grew up in urban regions, and 62.8 to 63.3% of laterborn individuals did. The shares are higher for post-reform cohorts for both subgroups. Note that we also observe relatively more male firstborns than laterborn males.

Regarding the channel variables, firstborns score higher in average trust, family, and democracy satisfaction compared to laterborn individuals. However, there are small differences within the subgroups. Firstborns also have, on average, more than twice

as many younger siblings than laterborn individuals. They also live more frequently in West Germany.

To detect differences between specific birth cohorts, we illustrate local averages of firstborn and laterborn individuals' outcomes in Figure 2. In contrast to Table 6, Figure 2 highlights differences within the subgroups of first- and laterborn individuals by year of birth. It suggests an increase in life satisfaction after the reform for firstborn and relatively stable scores for laterborn individuals, as there is a distinct 'jump' in averages in the reform year of 1986. The increase is present in 1986 and 1987 only. After 1987, life satisfaction of first- and laterborn individuals is, again, relatively equal.

Regarding Big Five personality traits, there is an increase in agreeableness scores in the reform year for firstborns, but it decreases in 1987 and 1988 to a lower level than before the reform. There is a decreasing trend for conscientiousness and a slightly increasing trend for openness, without any noticeable increase in the reform year. The trends in extraversion scores of first- and laterborn individuals follow the same pattern pre- and post-reform, and there is no clear pattern for neuroticism.

For self-reported health, there are higher scores for laterborn individuals compared to firstborns before but lower scores after the reform. After the reform, the difference in self-reported health outcomes between firstborn and laterborn individuals is larger compared to the pre-reform scenario. For both subgroups, however, there is a slightly increasing trend. As Table 6 was able to show, there is a distinct difference between the years of education of first- and laterborn individuals, but we see no reform effect. A slight decrease in income is observable, which is, most likely, a decreasing trend, as individuals born in later cohorts are younger and, on average, have been participating in the labor market for a shorter time.

We also conclude from Figure 2 that regression estimates for conscientiousness, neuroticism, openness, health, and income results should be interpreted cautiously, as there are deviations in the pre-reform trend. We can fix the partially opposing trend by using varying time frames around the reform for openness and health.

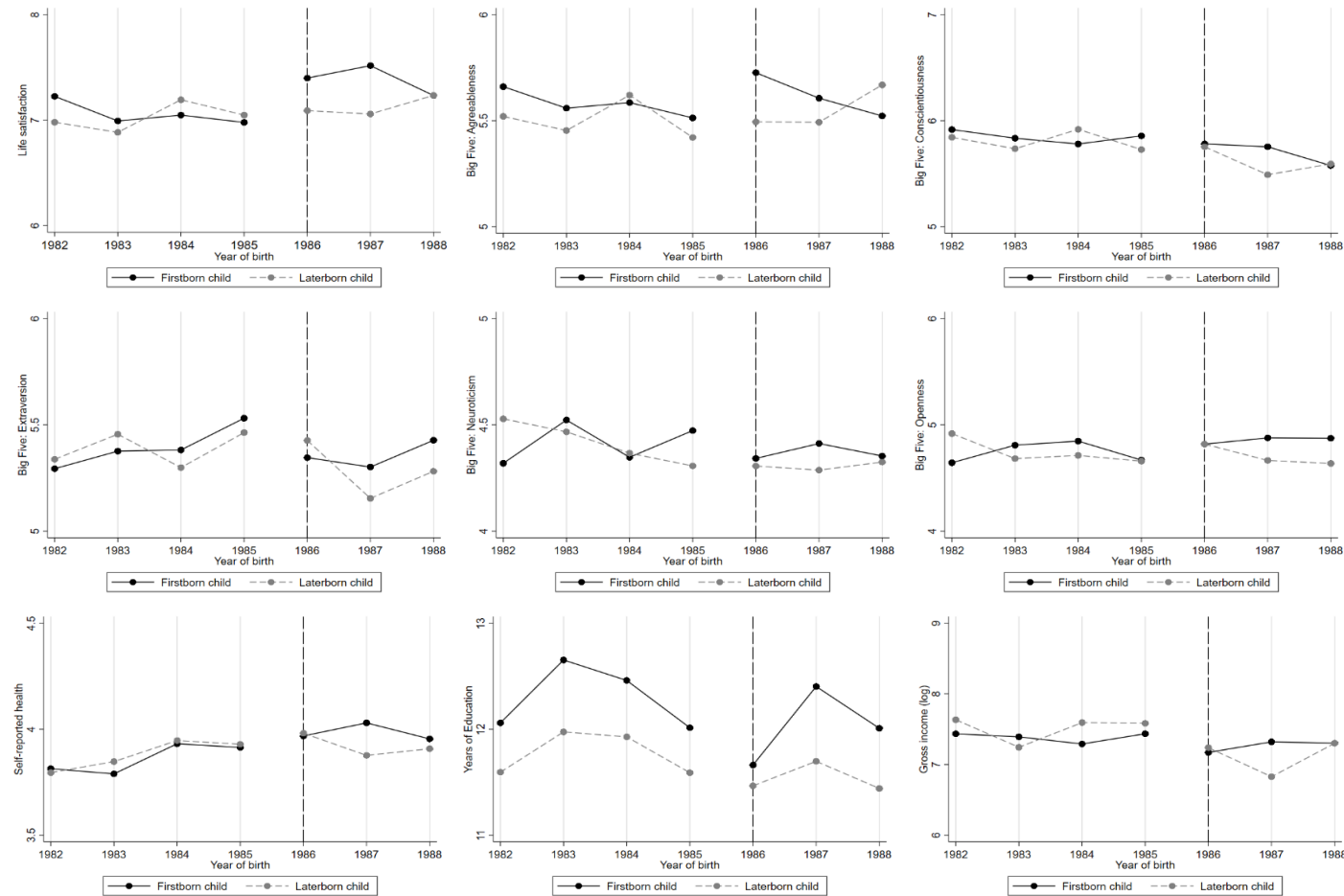
TABLE 6. SUMMARY STATISTICS FOR SUBGROUPS OF FIRSTBORNS AND LATERBORN CHILDREN

	(1)			(2)			(3)			(4)		
	<i>Firstborns (1982-1985)</i>			<i>Firstborns (1986-1988)</i>			<i>Laterborns (1982-1985)</i>			<i>Laterborns (1986-1988)</i>		
	<i>Indiv.</i>	<i>Mean</i>	<i>SD</i>	<i>Indiv.</i>	<i>Mean</i>	<i>SD</i>	<i>Indiv.</i>	<i>Mean</i>	<i>SD</i>	<i>Indiv.</i>	<i>Mean</i>	<i>SD</i>
Panel A: Outcome variables												
<i>Life satisfaction (0-10)</i>	343	7.07	1.18	279	7.40	1.08	229	7.02	1.28	204	7.13	1.17
<i>Big 5: Agreeableness (1-7)</i>	269	5.59	0.70	244	5.63	0.70	171	5.51	0.68	189	5.55	0.72
<i>Big 5: Conscientiousness (1-7)</i>	269	5.84	0.71	244	5.71	0.72	171	5.81	0.74	189	5.61	0.71
<i>Big 5: Extraversion (1-7)</i>	269	5.37	0.81	244	5.35	0.81	171	5.38	0.79	189	5.29	0.81
<i>Big 5: Neuroticism (1-7)</i>	269	4.42	0.82	244	4.37	0.85	171	4.43	0.81	189	4.31	0.77
<i>Big 5: Openness (1-7)</i>	269	4.76	0.94	244	4.86	1.00	171	4.75	1.12	189	4.71	0.90
<i>Self-assessed health (1-5)</i>	343	3.85	0.63	273	3.99	0.60	229	3.87	0.60	202	3.92	0.59
<i>Years of education</i>	295	12.35	2.37	228	12.03	2.27	188	11.78	2.19	154	11.53	1.94
<i>Gross income (log.)</i>	181	5.33	0.65	159	7.27	0.63	162	5.62	2.06	177	5.03	1.97
Panel B: Covariates												
<i>Year of birth</i>	343	1983.20	0.98	279	1986.91	0.80	229	1983.29	1.06	204	1987.00	0.82
<i>Month of birth</i>	315	6.01	3.25	230	6.36	3.38	205	6.00	3.55	149	6.403	3.71
<i>Gender (0/1: male)</i>	343	0.47	0.50	279	0.49	0.50	229	0.52	0.50	204	0.53	0.50
<i>Maternal educ. (0/1: high school)</i>	343	0.25	0.43	279	0.22	0.41	229	0.22	0.42	204	0.19	0.39
<i>Paternal educ. (0/1: high school)</i>	343	0.27	0.44	279	0.22	0.42	229	0.24	0.43	204	0.18	0.39
<i>Region growing up (0/1: urban)</i>	343	0.69	0.46	279	0.69	0.46	229	0.63	0.48	204	0.63	0.49
Panel C: Channel variables												
<i>Trust (1-4)</i>	292	2.62	0.57	218	2.72	0.59	193	2.47	0.62	166	2.64	0.56
<i>Family satisfaction (0-10)</i>	271	7.61	1.39	253	7.90	1.37	163	7.51	1.68	178	7.56	1.47
<i>Democracy satisfaction (0-10)</i>	246	4.71	2.07	212	5.04	2.01	156	4.72	2.17	179	4.55	2.22
<i>Number of younger siblings</i>	333	0.68	0.80	269	0.70	0.98	195	0.32	0.72	198	0.32	0.72
<i>Current residence (0/1: West G.)</i>	343	0.26	0.44	278	0.30	0.46	229	0.25	0.44	204	0.25	0.43

Notes: Assigned to the reform are firstborns born between January 1st 1986 and December 31st 1988 (Column 2). Partially assigned might be firstborns born between June 1985 and December 1985. In this descriptive table, we classify this subgroup as not assigned.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

FIGURE 2. LOCAL AVERAGES OF FIRST- AND LATERBORN INDIVIDUALS' OUTCOMES



Notes: The figure depicts average values for the main outcome variables. Assigned individuals to the reform are firstborns born on January 1st, 1986, or later. Excluded are individuals born between June and December 1985, as the reform might have partially treated firstborns born during this time.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

5. *Regression results from a difference in differences analysis*

5.1 Average effects of the reform in 1986

Table 7 shows average estimation results for the main outcome variables measured in adulthood. Point estimates without covariates are reported in Panel A and with covariates in Panel B. In Panel C, we define parental background covariates differently. Panel D groups two cohorts to provide more explanatory power of the data. Panel E excludes only children to control if this subgroup drives the effects, and in Panel F, we define individuals born between June and December 1985 as being assigned to the reform.

In all other panels, we exclude individuals born between June and December 1985 from the analysis, as firstborns born in this time frame might have been partially treated by the reform. Panel G shows the results of a placebo reform test using 1982 as pseudo reform date.³⁹ We use individuals born between 1978 and 1984 to be not too close to both reforms in 1986 and 1976.

Estimation results from the DiD analysis show that although p-values for life satisfaction estimates vary between $p > 0.10$ and $p < 0.15$, the positive effect is not statistically significant. Finding no significant results might stem from a decrease in life satisfaction of firstborns in 1988, as Figure 2 suggested. We find a statistically significant and robust positive effect on wages with a p-value < 0.05 or $p < 0.10$.

However, it is unclear if this effect occurs because of the parental leave extension: Firstly, Figure 3 showed opposing pre-reform trends two years before the reform, which might bias DiD results. Secondly, there is no distinct reform effect visible in Figure 2; rather, the trend for firstborn individuals is stable, but the trend in income of laterborn individuals is slowly decreasing. The estimation should therefore be interpreted cautiously, as it is most likely not a direct effect of the reform but an effect resulting from a reform-unrelated change in the pattern of the control group.

Figure 2 suggested that potential effects of the reform might only be visible for cohorts born closely after the reform and that later cohorts might be affected to a greater extent by other events, such as the German reunification. To test this, we vary time frames around the reform. Table 8 presents the results. If we use a shorter time frame around the reform (Panels A and B), the effect on the life satisfaction of

³⁹ This year is not too close to either the reform in 1986 or 1976 (reform used in a sensitivity analysis).

assigned individuals becomes statistically significant on a 10-or 5%-level. The effect sizes vary between 27.7% and 44.8% of the standard deviation. Thus, life satisfaction increases by 4.6% to 7.6% compared to the pre-reform mean. The effects are not statistically significant when extending the time frame.

Although Figure 2 suggested a short-term increase in agreeableness as well, the effect is insignificant. An increase in neuroticism is sizable for the most extended time frame of 1980-1989, which, however, most likely stems from a sharp increase in neuroticism for birth cohorts of 1989. We also conduct a multiple hypothesis test and calculate Westfall-Young stepdown adjusted p-values by using the Stata-command *wyoung*.⁴⁰ As a result, statistically significant estimates do not remain robust.

To conclude, we see positive and statistically significant effects on assigned individuals for life satisfaction only if we use a shorter time frame around the reform. Later birth cohorts' life satisfaction might have been affected stronger by the German reunification and preceding tumults, for example, through higher anxiety and stress levels in mothers. The effects on wages occur most likely due to a more pronounced decreasing trend of laterborn individuals' income compared to firstborn individuals' income and a more pronounced variability of averages in 1984 and thereafter. We therefore suggest effects on income do not occur due to the parental leave reform.

Neither the effects on life satisfaction nor on income remain statistically significant when correcting p-values for multiple hypothesis testing. Overall, regarding long-term effects of various well-being measures, maternal care and care in a childcare center in former East Germany were most likely relatively substitutable.

⁴⁰ Refer to McKenzie (2021) for further information on this method.

TABLE 7. ESTIMATION RESULTS FOR THE PARENTAL LEAVE REFORM OF 1986

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Life satisfaction</i>	<i>Agreeableness</i>	<i>Conscientiousness</i>	<i>Extraversion</i>	<i>Neuroticism</i>	<i>Openness</i>	<i>Health</i>	<i>Education</i>	<i>Income</i>
Panel A: Baseline regression	0.21 (0.15)	-0.00 (0.10)	0.07 (0.10)	0.07 (0.11)	0.09 (0.11)	0.15 (0.14)	0.08 (0.08)	-0.05 (0.30)	0.28** (0.12)
<i>N</i>	1055	873	873	873	873	873	1,047	865	554
<i>Covariates</i>	no	no	No	no	no	no	no	no	no
Panel B: Main regression	0.26 (0.16)	-0.06 (0.10)	0.07 (0.10)	0.14 (0.12)	0.13 (0.12)	0.18 (0.14)	0.01 (0.08)	-0.06 (0.32)	0.26** (0.12)
<i>N</i>	899	772	772	772	772	772	891	765	517
<i>Covariates</i>	yes	yes	Yes	yes	yes	yes	yes	yes	yes
Robustness tests (include covariates)									
Panel C: Parent/s is/are academic/s	0.26 (0.17)	-0.06 (0.10)	0.07 (0.11)	0.13 (0.12)	0.15 (0.12)	0.18 (0.14)	0.01 (0.08)	-0.13 (0.31)	0.24** (0.12)
<i>N</i>	899	772	772	772	772	772	891	765	517
Panel D: Grouping two cohorts	0.25 (0.16)	-0.06 (0.10)	0.08 (0.11)	0.13 (0.12)	0.14 (0.11)	0.18 (0.14)	0.00 (0.08)	-0.15 (0.31)	0.25** (0.12)
<i>N</i>	899	772	772	772	772	772	891	765	517
Panel E: Excluding only children	0.15 (0.18)	-0.08 (0.12)	0.01 (0.11)	0.05 (0.13)	0.16 (0.13)	0.22 (0.16)	-0.06 (0.09)	-0.13 (0.36)	0.24* (0.13)
<i>N</i>	689	594	594	594	594	594	682	579	391
Panel F: Jun-Dec 1985 are assigned	0.20 (0.15)	-0.08 (0.10)	0.06 (0.10)	0.07 (0.12)	0.08 (0.11)	0.13 (0.14)	0.01 (0.08)	-0.17 (0.30)	0.21* (0.11)
<i>N</i>	983	841	841	841	841	841	975	831	558
Panel G: Pseudo reform test	-0.01 (0.15)	0.05 (0.08)	-0.13 (0.09)	0.10 (0.10)	0.06 (0.10)	-0.12 (0.12)	0.03 (0.07)	-0.46* (0.25)	0.08 (0.10)
<i>N</i>	1038	859	859	859	859	859	1037	961	602

Notes: This table displays DiD estimates resulting from OLS regressions. Each panel contains results from one regression. Covariates include year of birth, month of birth, gender, maternal and paternal education, and the region of growing up. Individuals are included if life satisfaction measures are reported. The sample consists of birth cohorts 1982 to 1988. Robust standard errors are reported in parentheses. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

TABLE 8. SENSITIVITY ANALYSES WITH VARYING TIME FRAMES AROUND THE REFORM DATE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Life satisfaction</i>	<i>Agreeableness</i>	<i>Conscientiousness</i>	<i>Extraversion</i>	<i>Neuroticism</i>	<i>Openness</i>	<i>Health</i>	<i>Education</i>	<i>Income</i>
Panel A:	0.33*	-0.03	0.11	0.13	0.10	0.10	0.03	-0.11	0.21*
1983-1988	(0.18)	(0.11)	(0.12)	(0.125)	(0.13)	(0.15)	(0.09)	(0.35)	(0.12)
<i>N</i>	758	657	657	657	657	657	750	628	443
<i>Covariates</i>	Yes	yes	Yes	yes	yes	yes	yes	yes	yes
Panel B:	0.53**	0.08	0.18	0.07	0.13	0.04	0.04	-0.14	0.28*
1983-1987	(0.21)	(0.13)	(0.13)	(0.15)	(0.15)	(0.17)	(0.10)	(0.40)	(0.15)
<i>N</i>	616	528	528	528	528	528	615	514	356
<i>Covariates</i>	Yes	yes	Yes	yes	yes	yes	yes	yes	yes
Panel C:	0.24	-0.03	0.05	0.07	0.16	0.12	0.02	-0.02	0.21*
1982-1989	(0.15)	(0.09)	(0.10)	(0.109)	(0.11)	(0.132)	(0.08)	(0.29)	(0.11)
<i>N</i>	1048	893	893	893	893	893	1,040	864	618
<i>Covariates</i>	Yes	yes	Yes	yes	yes	yes	yes	yes	yes
Panel D:	0.21	-0.01	0.02	0.08	0.17*	0.16	0.05	-0.18	0.18*
1980-1989	(0.14)	(0.08)	(0.09)	(0.10)	(0.10)	(0.12)	(0.07)	(0.26)	(0.10)
<i>N</i>	1367	1153	1153	1153	1153	1153	1359	1167	810
<i>Covariates</i>	Yes	yes	Yes	yes	yes	yes	yes	yes	yes

Notes: This table displays DiD estimates resulting from OLS regressions. Each panel contains results from one regression. Covariates include year of birth, month of birth, gender, maternal and paternal education, and the region of growing up. Individuals are included if life satisfaction measures are reported. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, 10.5684/soep.core.v36eu. Own calculations.

5.2 Effect heterogeneity by gender

Regarding gender differences, studies suggest that boys tend to benefit more than girls from maternal attachment in early life: Beeghly et al. (2017) show that boys, but not girls, are vulnerable to early caregiving risks such as maternal depression, which might result in a lower mother-child attachment in toddlerhood. Danzer and Lavy (2017) indicate effects of parental leave on the cognitive skills of sons of highly educated mothers. In the case of former East Germany, childcare centers were less child-oriented than today's childcare centers. Less child orientation might also imply that boys were more strictly raised in childcare centers than a mother would raise a boy.

Table 9 shows results for males. It suggests more pronounced effects on life satisfaction for short time frames around the reform compared to the whole sample. The effect is statistically significant for some specifications, with the most considerable effect occurring in Panel F with a p-value < 0.01 . For a large share of the other specifications, the p-value is < 0.15 and thus, results are statistically insignificant. Effect sizes for the short time frame vary between 40.3% and 63.8% of a standard deviation. Life satisfaction increases between 7.1% and 11.3% of the pre-reform mean for males.

We also observe positive effects on conscientiousness, with a most significant effect again in Panel F with a p-value < 0.01 , and, to a smaller extent, on openness. However, as Figure 2 suggested, the effect is most likely based on a change within the control group rather than from the group assigned to the reform. It is therefore less likely that this effect can be interpreted as a reform effect. Also, there are deviations from the pre-reform trend for this variable. For openness, the increase is based on a larger difference between scores of firstborns and laterborn individuals for cohorts born after 1987. We find no effects on openness when estimating scores with a shorter time frame around the reform.

Estimates suggest a positive impact on health, as p-values are < 0.15 for specifications without covariates; however, the effect is not statistically significant. The effect on extraversion shown in Panel F rather stems from cohorts born in 1988 and thereafter and is thus no effect due to the parental leave extension. Likewise, we find no effects for specifications using a shorter time frame around the reform. Furthermore, we observe an increase in neuroticism in Panel D when using a sample without only children. This suggests that neuroticism increases for males with siblings. However, it is likely that the effect occurs by chance, as it is not robust.

There are no effects of the parental leave reform on females in Table 10, apart from an increase in income ($p < 0.1$) in Panel A. This indicates that the income effect in Tables 2 and 3 are driven by the female subgroup. The effect is likely occurring because of a change in the control group, but not due to an effect on assigned individuals to the parental leave extension, as Figure 2 suggested.

We calculate Westfall-Young stepdown adjusted p-values for both subsamples. Statistically significant effects do not remain robust after controlling for multiple hypothesis testing. Overall, there are hints that males of cohorts born after the reform score higher in life satisfaction and few personality traits. Only the effect on life satisfaction is likely based on the parental leave extension. However, the effect becomes insignificant as well after correcting p-values with a multiple hypotheses test. Maternal care and care in care centers in former East Germany were most likely substitutable for males as well.

We analyze heterogeneity effects only by gender, as heterogeneity by socioeconomic background is less critical in former East Germany. Socioeconomic gaps were less pronounced than in Western societies, and there were more minor income differences across families compared to Western societies. Attending high school or university depended not solely on educational performance but also parental reputation in the Communist society or party. Furthermore, although the SOEP provides data on maternal and paternal education, it does not provide information on parental income.

TABLE 9. HETEROGENEITY ANALYSIS FOR MALE INDIVIDUALS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Life satisfaction</i>	<i>Agreeableness</i>	<i>Conscientiousness</i>	<i>Extraversion</i>	<i>Neuroticism</i>	<i>Openness</i>	<i>Health</i>	<i>Education</i>	<i>Income</i>
Panel A: Baseline regression	0.34 (0.22)	0.03 (0.14)	0.14 (0.15)	0.11 (0.16)	0.21 (0.15)	0.37* (0.20)	0.14 (0.10)	0.37 (0.40)	0.20 (0.16)
<i>N</i>	522	420	420	420	420	420	518	420	281
<i>Covariates</i>	no	No	No	no	no	no	no	no	no
Panel B: Main regression	0.32 (0.24)	-0.01 (0.15)	0.25 (0.15)	0.26 (0.17)	0.18 (0.16)	0.39* (0.21)	0.10 (0.12)	0.20 (0.44)	0.19 (0.16)
<i>N</i>	445	374	374	374	374	374	441	373	256
<i>Covariates</i>	yes	Yes	Yes	yes	yes	yes	yes	yes	yes
Robustness and varying time frames (include covariates)									
Panel C: Grouping two cohorts	0.34 (0.24)	-0.01 (0.15)	0.27* (0.15)	0.25 (0.17)	0.17 (0.16)	0.43** (0.21)	0.09 (0.12)	0.19 (0.41)	0.18 (0.16)
<i>N</i>	445	374	374	374	374	374	441	373	191
Panel D: Without only children	0.09 (0.26)	-0.00 (0.16)	0.04 (0.16)	0.06 (0.19)	0.34* (0.19)	0.41* (0.24)	-0.03 (0.14)	0.01 (0.51)	0.08 (0.19)
<i>N</i>	339	284	284	284	284	284	336	280	191
Panel E: 1983-1988	0.49* (0.26)	0.18 (0.18)	0.37** (0.17)	0.28 (0.19)	0.15 (0.18)	0.18 (0.23)	0.09 (0.13)	0.10 (0.48)	0.16 (0.17)
<i>N</i>	379	321	321	321	321	321	375	313	221
Panel F: 1983-1987	0.79*** (0.30)	0.13 (0.18)	0.55*** (0.19)	0.17 (0.22)	0.13 (0.21)	0.17 (0.26)	0.14 (0.14)	0.09 (0.55)	0.16 (0.18)
<i>N</i>	305	253	253	253	253	253	304	255	173
Panel H: 1980-1989	0.42** (0.19)	0.12 (0.12)	0.15 (0.13)	0.26* (0.14)	0.17 (0.13)	0.35** (0.17)	0.13 (0.10)	-0.04 (0.34)	0.18 (0.14)
<i>N</i>	683	571	571	571	571	571	679	587	410

Notes: Displayed are DiD estimates resulting from OLS regressions. Each panel contains results from one regression. Covariates include year of birth, month of birth, maternal and paternal education, and the region of growing up. Individuals are included if life satisfaction measures are reported. The sample in Panel A-B consists of birth cohorts 1982 to 1988. Robust standard errors are reported in parentheses. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, 10.5684/soep.core.v36eu. Own calculations.

TABLE 10. HETEROGENEITY ANALYSIS FOR FEMALE INDIVIDUALS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Life satisfaction</i>	<i>Agreeableness</i>	<i>Conscientiousness</i>	<i>Extraversion</i>	<i>Neuroticism</i>	<i>Openness</i>	<i>Health</i>	<i>Education</i>	<i>Income</i>
Panel A: Baseline regression	0.09 (0.20)	-0.01 (0.14)	0.01 (0.14)	0.04 (0.15)	-0.02 (0.16)	-0.06 (0.19)	0.02 (0.11)	-0.39 (0.44)	0.36** (0.18)
<i>N</i>	533	453	453	453	453	453	529	445	292
<i>Covariates</i>	no	No	no	no	no	no	no	no	no
Panel B: Main regression	0.17 (0.23)	-0.09 (0.15)	-0.09 (0.15)	0.05 (0.7)	0.07 (0.18)	-0.03 (0.20)	-0.08 (0.12)	-0.49 (0.48)	0.30 (0.18)
<i>N</i>	454	398	398	398	398	398	450	392	274
<i>Covariates</i>	yes	Yes	yes	yes	yes	yes	yes	yes	yes
Robustness and varying time frames (include covariates)									
Panel C: Grouping two cohorts	0.16 (0.23)	-0.08 (0.15)	-0.07 (0.15)	0.03 (0.17)	0.10 (0.18)	-0.07 (0.20)	-0.09 (0.12)	-0.73 (0.50)	0.29 (0.18)
<i>N</i>	454	398	398	398	398	398	450	392	274
Panel D: Without only children	0.10 (0.25)	-0.02 (0.17)	-0.09 (0.17)	-0.01 (0.19)	0.04 (0.20)	-0.06 (0.22)	-0.14 (0.13)	-0.45 (0.55)	0.29 (0.20)
<i>N</i>	350	309	309	309	309	309	346	299	213
Panel E: 1983-1988	0.15 (0.25)	-0.05 (0.16)	-0.12 (0.16)	0.09 (0.17)	-0.01 (0.19)	-0.04 (0.21)	-0.03 (0.13)	-0.50 (0.53)	0.23 (0.19)
<i>N</i>	379	336	336	336	336	336	375	324	235
Panel F: 1983-1987	0.26 (0.31)	-0.05 (0.19)	-0.17 (0.18)	-0.02 (0.20)	0.04 (0.22)	-0.16 (0.23)	-0.03 (0.14)	-0.36 (0.62)	0.38 (0.25)
<i>N</i>	311	275	275	275	275	275	311	268	190
Panel G: 1980-1989	-0.01 (0.19)	-0.10 (0.12)	-0.11 (0.12)	-0.07 (0.14)	0.21 (0.15)	-0.04 (0.16)	-0.04 (0.10)	-0.44 (0.39)	0.19 (0.15)
<i>N</i>	684	582	582	582	582	582	680	595	424

Notes: Displayed are DiD estimates resulting from OLS regressions. Each panel contains results from one regression. Covariates include year of birth, month of birth, maternal and paternal education, and the region of growing up. Individuals are included if life satisfaction measures are reported. The sample in Panel A-B consists of birth cohorts 1982 to 1988. Robust standard errors are reported in parentheses. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, 10.5684/soep.core.v36eu. Own calculations.

5.3 Effects of the reform in 1976 as alternative scenario

We estimate the effects of the parental leave reform in May 1976 in former East Germany as a sensitivity analysis. This reform granted mothers who already gave birth to at least one child one year of paid parental leave with conditions like the reform in 1986. Before the reform, mothers could take twelve (before 1972) or 18 weeks (from 1972 on) of maternity leave. After the reform, they were eligible for one year of paid parental leave (six months of maternity and six months of parental leave). Laterborn children were then enrolled in public childcare at twelve months of age, but firstborn children were still enrolled at six months. In response to this reform, short-term fertility increased temporarily but declined after 1980 (Buettner and Lutz 1990).

If there were similar effects for later- compared to firstborn children, even though analyzing a different decade and children of different birth order, it would mean that the respective reform most likely caused some potential effects. We could mitigate remaining concerns about occurring events (e.g., the Chernobyl disaster and the German reunification). Critical with this approach is the probability of spillover effects on older siblings. Reform effects might be too small to measure if such spillover effects exist. We estimate the following equation:

$$y_{ij} = \lambda(Laterborn)_{ij} \times (Postreform76)_{ij} + \beta_1(Laterborn)_{ij} + \beta_2(Postreform76)_{ij} + \pi_j + \mu X_i + c + \epsilon_{ij}. \quad (4)$$

The interaction of the variables “laterborn” and “postreform76” is the DiD variable. According to the main analysis, we use the same time frame around the reform. Therefore, 1972 and 1978 are cut-off years. The reform implied a change in paid leave from 18 weeks of maternity leave to one year, making the setting slightly different from the setting of the main analysis. We use the same variables and survey years as for the main analysis. Survey data is available for more waves for life satisfaction, health, and education, which is why we additionally use waves from 1991 to 1996 to calculate the average for these three variables. The averages of individuals are measured between 18 and 47 years of age.

Table A.9 shows descriptive statistics of outcome variables and covariates. Life satisfaction of laterborn individuals is both higher after the reform and compared to firstborns. Regarding Big Five personality traits, averages are mixed and there are no substantial differences between pre- and post-reform outcomes of firstborn and laterborn individuals. Regarding health and wage, averages are, again, mixed, depending on pre- and postreform cohorts of laterborn and firstborn individuals. For

education, we see the same pattern as in the main analysis, as firstborns are, on average, for a longer time in education or training. Regarding covariates, parental education is relatively similar, although differences between later- and firstborn individuals are not as pronounced as in our main analysis. This is also true for the region type of growing up.

Table A.10 shows estimation results from a DiD analysis. Results suggest positive effects on life satisfaction, but, as p-values are between 0.10 and 0.15, the effect is statistically insignificant. Table A.11 suggests small, but not robust, effects for males on openness and extraversion. There are also small effects for females in Table A.12 regarding life satisfaction and income, but effects are not robust.

To conclude, there is weak evidence that the reform of 1976 had small positive impacts on similar outcome variables as in the main analysis. However, the effects are not robust and less pronounced. Statistically significant results are not robust if p-values are corrected for multiple hypothesis testing.

Finding less pronounced results compared to the main analysis might be due to the following reasons. Firstly, the reform in 1976 was targeted at laterborn individuals. It could have had positive spillover effects on older siblings. In a scenario where mothers stayed at home for one year with a newborn child, older siblings could have benefitted from the additional time with their mothers (studies that observe spillover effects are, for example, Bettinger et al. 2014 and Ginja et al. 2020). In this case, the estimated effect on laterborn individuals' outcomes could be strongly biased downwards.

Secondly, finding almost no effects might again suggest that maternal care and care in a childcare center in the former GDR were relatively substitutable. Thirdly, as we observe on average older individuals than in our main setting, effects stemming from childhood conditions might be overlaid to a greater extent by other effects occurring in later life.

5.4 Potential channels

Bonding and attachment

There are different potential channels through which reform effects might emerge. Seminal contributions by Bowlby (1969) and Ainsworth (1970) emphasize that early parental attachment influences socio-emotional functioning and relationship patterns measurable throughout life. Also, Schore (2001) points out that the emotion-processing limbic part of the brain is remarkably 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).

Given this literature, it seems plausible that an increase in time with the mother in a child's first year has positive (long-term) effects on the child's outcomes, especially regarding subjective well-being. Berger et al. (2005) point out an increase in problematic behavior in children when mothers return to work soon after birth. Other studies find that parental bonding and minimizing stress factors are essential for children's development (e.g., Kottelenberg and Lehrer 2014; Beeghly et al. 2017; Golding and Fitzgerald 2017; Schore 2017).

A clear attachment figure is essential for very young children, and such a figure might not have been provided in childcare centers of former East Germany (and, which is also not provided in some childcare centers today, see OECD 2017). Therefore, we can expect that being together with the primary caregiver for the first year of life instead of six months is more likely to positively affect a child's attachment than being cared for in a rather large group in a childcare center.

We test if the increase in life satisfaction is driven by bonding and attachment and use trust as an outcome variable for a proxy of this channel.⁴¹ Table 11, Panel A shows no long-term effects of the reform on the trust of individuals, neither on the average nor for subgroups of males and females. Therefore, it is an unlikely channel for the effects on long-term subjective well-being.

⁴¹ Direct mother and child bonding variables are unavailable for the birth cohorts under study. The SOEP introduced the "Mother and Child Questionnaire" for newborns initially in 2003.

Family stability and family satisfaction

Parental marriage or the stability of their relationship might affect a child's subjective well-being. A less stable relationship might decrease affected children's long-term life satisfaction and trust (Amato 2000). Studies show effects of parental leave and maternal labor market participation on family stability for paternity leave (Avdic and Karimi 2018; Olafsson and Steingrimsdottir 2020; Petts et al. 2020) and maternity leave (Brainerd and Malkova 2021).

We have no information on parental relationship satisfaction of the observed children. However, we know that there was no substantial change in divorce rates shortly after the reforms, both in 1986 and 1976 (Boettcher 2006). Heisig (2023) shows a slight increase in satisfaction with household activities of assigned mothers to the reform in 1986, which might be evidence of more considerable family satisfaction. However, the effect occurs only in a single specification and is not robust.

We use family satisfaction of children in adulthood as a proxy for family stability in childhood. Glenn and Kramer (1987) showed that parental divorce negatively affects white females' divorce-proneness in the U.S., based on a lower commitment to and age at marriage. Table 11, Panel B shows increases in family satisfaction, and the average effect seems to be driven by males. The increase in family satisfaction might be a channel for the increase in life satisfaction we observe for males in former East Germany. However, statistically significant effects for family satisfaction are only measurable in a few specifications. In others, p-values are between 0.10 and 0.15. Thus, results should be interpreted cautiously.

Fertility effects and changes in the number of siblings

Previous studies show that the reform of 1986 did not affect fertility rates significantly. There was a slight increase in total fertility rates in 1987, but fertility decreased after that (Buettner and Lutz 1990; Conrad et al. 1996). Having siblings or being an only child might be essential for an individual's outcomes: Children's well-being might increase due to positive experiences of having siblings or decrease due to higher competition in childhood or a worse-off financial situation of parents. In the latter case, having siblings might imply fewer financial resources for parents to dedicate to each child's health or education.

In our analysis, leaving out only children leads to worse (but not statistically significant) health outcomes. The less favorable estimated coefficients align with a

hypothesis that children with siblings might benefit less from the reform than only children. We test if there is a direct and long-lasting effect of the reform on the number of younger siblings of an individual. Panel C of Table 11 suggests overall no reform effects on the number of younger siblings, which is in line with previous findings. However, the effect is positive for males with siblings (i.e., males with a total number of one or more siblings). Having more younger siblings after the reform might be a channel for increased life satisfaction solely for males with siblings. However, results should be interpreted carefully.

Moving to West Germany after the German reunification

The reform of 1986 was chronologically closely introduced to the peaceful revolution and the German reunification in 1989 and 1990. If families with only one child were more likely to move to West Germany after the reunification, this could have affected firstborn children (who are more likely to be only children) differently than laterborn children. For example, it might influence an individual's subjective well-being and other variables, such as health or income. Literature shows that families are more mobile the fewer children they have (e.g., Long 1972; Fischer and Malmberg 2001).

Furthermore, living only one to three years (individuals born between 1986 and 1988) in the GDR compared to five to seven years (individuals born between 1982 and 1985) might impact attachment to the territory of East Germany in terms of migration willingness. Also, if firstborn or laterborn individuals were more or less likely to migrate to West Germany, this might impact their life outcomes. Although there is evidence that firstborn sons in rural India who inherit land migrate less frequently than laterborn sons who inherit land (Fernando 2022), this case is very particular and does not apply to the average population in former East Germany.

Therefore, it is unclear if the probability of moving to West Germany is a possible channel. We test this hypothesis by using the current region of living as an outcome variable. Table 11, Panel D suggests no differences between individuals assigned and not assigned to the reform regarding internal migration behavior. Living in West Germany is therefore an unlikely channel of the increase in life satisfaction.

Adverse effects of socialist rearing in childcare centers

A common worry is that even as early as in childcare centers, children were taught a socialist mindset. Zwiener (1994) states that the development of moral values was one aim of measures in childcare centers, making this assumption plausible. If children were influenced by socialist thoughts early in life, it could impact their personality and ideology. We use democracy satisfaction to investigate if this channel is a likely one. For this to be the case, the democracy satisfaction of assigned individuals should increase after the reform compared to the control group. Table 11, Panel E suggests a weak positive impact on the democracy satisfaction of assigned individuals in adulthood. However, the effect is neither robust nor sizable in the male nor female subgroups and should be cautiously interpreted.

Adverse effects by the Chernobyl disaster in late April of 1986

The Chernobyl disaster happened in late April 1986. Since firstborn children have worse health characteristics than laterborn children at birth (Brenøe and Molitor 2018; Pruckner et al. 2021; Bjoerkegren and Svaleryd 2023), they might have been affected to a more considerable extent by the radioactive contamination. In this case, the Chernobyl disaster adversely affects our study results. However, whether the health differences between firstborn and laterborn children persist in adolescence and adulthood is unclear. Studies find no consistent long-term differences or even a reverse in health patterns (Black et al. 2016; Brenøe and Molitor 2018; Pruckner et al. 2021; Bjoerkegren and Svaleryd 2023).

Important to note is that the most severe health effects of the Chernobyl disaster occurred in the three most contaminated countries Ukraine, Belarus, and Russia, where a sharp increase in thyroid cancer, mental health burdens, and other illnesses emerged (Cardis et al. 1996; Cwikel 1997; Bar Joseph et al. 2004; Yablokov 2006; Sumner 2007; Bromet et al. 2011; Balonov 2019). For other parts of the world, adverse effects of the nuclear disaster on health and cognitive performance are yet unclear (Irl et al. 1995; Sali et al. 1996; Grosche et al. 1997; Koerblein 2006; Bromet et al. 2011).

Although there might be an increased probability of adverse effects in relatively severe contaminated areas, such as (the southern part of) Bavaria (Kuechenhoff et al. 2006; Elsner and Wozny 2018), this is yet to be confirmed by other studies. Most importantly, almost all regions of former East Germany were not strongly

contaminated (Elsner and Wozny 2018). Therefore, our assumption that firstborn and laterborn children were similarly affected by the Chernobyl disaster and no severe adverse health effects distort the results seems plausible.

Learning and health environment at home and in childcare centers

Differences in learning and health environments are another possible channel. Staff in childcare centers was comparably highly educated (Zwiener 1994; OECD 2017), and cognitive development was one focus of former East Germany's childcare centers (Zwiener 1994). However, positive stimulations of cognitive development are often subgroup-specific or not sizable in adulthood (Heckman et al. 2013; Gertler et al. 2014).

As we find no pronounced reform effects on educational success, this channel is unlikely to be relevant for our results. Regarding cognitive development, maternal care and care in childcare centers seem to be substitutable in the first year of a child's life. Also, childcare center quality data is unavailable in our dataset.

Furthermore, it is important to note that there is a higher probability of children being breastfed longer when mothers return to work later after birth (Haider et al. 2003; Baker and Milligan 2008). A longer duration of breastfeeding might impact infants' health positively in the short term. Previous studies find modest effects, and it is unclear whether effects persist into adulthood (Kramer et al. 2003; Chantry et al. 2006; Baker and Milligan 2008).

Another health-related channel is that mothers on more extended parental leave recover better from birth and birth-related adverse health effects, such as postpartum depression. Studies show that fewer depressive symptoms in mothers correlate with better parenting and parent-child-bonding (Clark et al. 1997; Wisner et al. 2002; Binda et al. 2019), which again impact a child's cognitive abilities (Murray and Cooper 1996; Brockington 2004).

For the same reform we use in this study, Heisig (2023) finds no long-term health effects for mothers assigned to the reform. However, this does not imply that short-term effects did not affect children early in life. Although this might be a relevant channel, we cannot test it adequately with our data.

Income effects due to changes in maternal income during parental leave

Furthermore, parental income can be a channel for effects on (long-term) subjective well-being. Previous literature on paid parental leave reforms finds positive income effects (e.g., Baker and Milligan 2008 and 2010; Bettinger et al. 2014; Dustmann and Schoenberg 2012; Carneiro et al. 2015; Fabel 2021; Huebener et al. 2021), especially for (West) Germany or Austria, where many mothers were homemakers prior the reform.

After the reform, they receive financial benefits for child-rearing. For part-time or full-time working mothers, the income effect is less clear and depends on the wage earned prior to childbirth and the income replacement rate of parental leave schemes: There are studies that show adverse income effects, especially for high-earning mothers (e.g., Baker and Milligan 2010; Dustmann and Schoenberg 2012).

In former East Germany, most mothers worked on a full-time basis before childbirth (Winkler 1990). After the reform, mothers received between 70 and 90% of their previous wage, making it likely that income decreased for all mothers. However, because of the circumstances in everyday life in former East Germany, we do not expect significant effects on children. Reasons are, on average, flat rents were as cheap as 0.5 Euros per m² (Henger and Voigtlaender 2015). Also, flats were usually allocated, preferably to (married) couples with children (Hinrichs 1992). Additionally, the government kept basic food prices low (Statistisches Jahrbuch der DDR 1988).

In our dataset, we do not observe parental income of assigned children. Also, the data set does not contain information on the East German population before 1990/91. Heisig (2023) finds no evidence for a (long-lasting) income effect for mothers after the reform. There are minor, but not robust, effects on income satisfaction.

TABLE 11. EFFECTS OF THE REFORM ON POTENTIAL CHANNEL VARIABLES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Full sample</i>			<i>Males</i>			<i>Females</i>		
	1982-1988	1983-1987	Without only children	1982-1988	1983-1987	Without only children	1982-1988	1983-1987	Without only children
Panel A: Trust	-0.05 (0.08)	-0.02 (0.11)	-0.01 (0.10)	0.01 (0.14)	-0.03 (0.17)	0.04 (0.16)	-0.09 (0.13)	0.00 (0.16)	-0.09 (0.14)
<i>N</i>	869	522	589	362	251	281	392	271	308
Panel B: Family satisfaction	0.33 (0.22)	0.49* (0.26)	0.31 (0.24)	0.46 (0.32)	0.82** (0.37)	0.14 (0.37)	0.16 (0.30)	0.15 (0.38)	0.35 (0.32)
<i>N</i>	776	531	584	380	259	283	396	272	301
Panel C: Number of younger siblings	0.02 (0.12)	-0.15 (0.14)	0.16 (0.14)	0.07 (0.17)	-0.20 (0.21)	0.37* (0.20)	-0.05 (0.16)	-0.17 (0.20)	-0.04 (0.19)
<i>N</i>	852	584	650	425	293	324	427	291	326
Panel D: Living in West Germany	0.05 (0.06)	0.04 (0.08)	0.05 (0.07)	0.01 (0.09)	-0.01 (0.11)	-0.01 (0.10)	0.09 (0.09)	0.07 (0.12)	0.13 (0.10)
<i>N</i>	898	615	689	444	304	339	454	311	350
Panel E: Democracy satisfaction	0.47 (0.31)	0.49 (0.40)	0.66* (0.36)	0.40 (0.52)	0.58 (0.64)	0.32 (0.55)	0.30 (0.43)	0.30 (0.50)	0.79 (0.49)
<i>N</i>	793	467	556	331	223	265	363	244	291

Notes: This table displays DiD estimates resulting from OLS regressions. Each panel contains results from one regression. All regressions contain covariates (year of birth, month of birth, maternal and paternal education, and the region of growing up). Individuals are included if life satisfaction measures are reported. The sample in Panel A and B consists of birth cohorts 1982 to 1988. Robust standard errors are reported in parentheses. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, 10.5684/soep.core.v36eu. Own calculations.

6. *Discussion and conclusion*

In this paper, we investigated the effects of a historical parental leave reform in former East Germany in 1986 on children's outcomes in adulthood. The reform extended first-time mothers' parental leave from six months of maternity leave with full wage replacement to an additional six months with a wage replacement rate of 70 to 90% of the pre-birth wage, implying one year of paid parental leave. We examine effects on subjective well-being, personality, health, education, and labor market outcomes and study possible channels.

The institutional setting features a distinct counterfactual care mode, as most women had full-time jobs, and most children between seven and twelve months of age were cared for in public childcare centers. This study does not face major issues of women self-selecting into the labor market and insufficient or heterogeneous childcare supply, contrasting with many other studies on parental leave reforms. Results might be especially of interest for policymakers of countries with less than one year of paid leave (for example, Australia, Belgium, France, Mexico, and the U.S.) and for countries already utilizing a large share of medium- to high-quality public childcare, as this is the counterfactual care scenario in our study.

Results suggest that extended paid parental leave by six months barely affected the studied long-term outcomes of children. We observe weak evidence for positive effects on life satisfaction. For the whole sample, the effect on life satisfaction varies between 27.7% and 44.8% of a standard deviation, corresponding to an increase of 4.6% to 7.6% of the pre-reform mean. There are also small effects on income and neuroticism, but the effects stem, most likely, not from the reform itself but from changes within the control group or cohorts close to the German reunification. For males, effect sizes on life satisfaction are larger and vary between 40.3% and 63.8% of a standard deviation. This implies an increase in life satisfaction of 7.1% and 11.3% of the pre-reform mean.

There are also few positive effects on conscientiousness, extraversion and openness, and adverse effects on neuroticism of males. However, the effects are most likely driven by specific birth cohorts close to the German reunification, and a change in the control group. We observe no reform effects for females, apart from an increase in income, which is not robust and sizable only in a single specification. Effects become insignificant if we correct p-values for multiple hypothesis testing.

Although we find small increases for the named variables, we conclude that center-based care and maternal care were relatively substitutable in this historical setting, especially regarding health and cognitive development in adulthood. For these aspects, we find no effects whatsoever of the reform under study. Furthermore, in an additional setting, by using a parental leave reform in 1976, we find weak support for increased extraversion and openness for males and increased life satisfaction and income driven by females. These effects are, however, also not robust.

We further analyze channels through which effects might be driven. There is weak evidence that family satisfaction, the number of younger siblings, and, to a small extent, an increase in democracy satisfaction might act as channel variables. However, results are not robust, and effects appear in only a few specifications.

Finding almost no effects of the former East German's parental leave reform of 1986, and, in a sensitivity analysis, of 1976, is in line with many studies so far showing negligible effects of parental leave extensions on children's outcomes in various countries (e.g., Baker and Milligan 2008, 2010 and 2015; Liu and Nordstrom Skans 2010; Wuertz Rasmussen 2010; Dustmann and Schoenberg 2012; Dahl et al. 2016; Huebener et al. 2019). Our findings on life satisfaction and, to a smaller extent, some Big Five personality traits are in line with the results of a study by Houmark et al. (2022) for Denmark, who observe improvements in adolescent's school-related subjective well-being and socio-emotional outcomes of a parental leave extension from six to twelve months.

However, several aspects that limit the external validity of our study should be considered when applying the results from the historical setting in a socialist regime to Western societies. Firstly, the economic situation in the 1980s in former East Germany and the political regime must be considered for a cost-benefit analysis of the reform. Several studies have shown labor hoarding in the decade before reunification in former East Germany (e.g., Akerlof et al. 1991; Dornbusch and Wolf 1994; Hoffmann 2005). Therefore, it was no significant trade-off to extending paid parental leave. This starkly contrasts with the shortage of skilled labor in many Western societies facing demographic change, making the trade-off more cost intensive.

Secondly, the former communist regime and ideology touched all aspects of society, including parenting and public childcare centers. Nevertheless, we are convinced that our results apply to Western societies. Family policy in former East Germany was surprisingly advanced and modern in terms of labor market participation of women, childcare, availability of oral contraceptives, options to divorce, and the

acceptance of single mothers. These circumstances are comparable to modern Western societies.

Additionally, the presence of public childcare centers in contrast to other types of childcare is related to conditions in present France or Sweden. However, care centers in Sweden are of higher quality than those in former East Germany. The quality of former East Germany's care centers might be described best as an in-between of present standards in high-quality care centers and lower-quality private care facilities.

However, we see weak evidence for an increase in democracy satisfaction of assigned individuals to the reform, implying that there might have been an impact of childcare attendance on socialist mindsets. Another limitation of our study concerns the small sample size and pooling of several cohorts. This implies we can detect large treatment effects but no effects of smaller sizes. Further studies analyzing short- and long-term effects would be helpful for a more comprehensive insight into parental leave's impact on subjective well-being of children.

7. References

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8. Appendix

TABLE A.6. STUDIES ON SHORT-TERM EFFECTS OF PARENTAL LEAVE ON CHILD OUTCOMES

<i>Study</i>	<i>Country, year</i>	<i>Reform</i>	<i>Counterfactual care</i>	<i>Outcomes</i>	<i>Results</i>
<i>Albagli & Rau (2019)</i>	Chile, 2011	Extension of paid parental leave from 12 to 24 weeks	Either informal or low-quality formal care	Cognitive, language, motor, and socio-emotional competencies between 7 months and 6 years of age	Positive effects for cognitive competencies. Results are stronger for children of mothers with lower education
<i>Baker & Milligan (2008)</i>	Canada, 2000	Extension of paid maternity leave from 25 to 50 weeks. Extension of job-protected parental leave from 18-70 to at least 52 weeks	Mostly informal care (40% of children younger than 2 years). In rare cases formal care (4/6% of children younger than ½ year/s)	Birth outcomes, mother-reported health status of the child, specific ailments, and injuries at 7-24 months	No effects
<i>Baker & Milligan (2010)</i>	Canada, 2000	See Baker & Milligan (2008)	See Baker & Milligan (2008)	Temperament, motor and social development at 7 and 24 months (questionnaire).	Small, mostly insignificant results
<i>Baker & Milligan (2015)</i>	Canada, 2000	See Baker & Milligan (2008)	See Baker & Milligan (2008)	Vocabulary, numbers, parent-reported measures at age 4/5	No effects apart from small negative effects on vocabulary
<i>Bullinger (2019)</i>	U.S., 2004	Introduction of paid, job protected parental leave for 6 weeks, before 12 weeks of unpaid, job protected maternity leave	Mostly informal care	Parent-reported overall child health, asthma, respiratory and food allergies	Positive average effects for parent-reported overall child health. Some effects on reduction in asthma. No effects on respiratory or food allergies
<i>Canaan (2022)</i>	France, 1994	Introduction of 36 months of paid parental leave (with at least 2 children). Before, 6 weeks prenatal and 10 weeks postnatal job-protected maternal leave	1/3 of 2-year-olds in public childcare. Nearly all children 3-6 in public childcare	Phonological awareness, vocabulary development, oral comprehension, spontaneous & overall speech at age 5-6	Negative effects on verbal development

TABLE A.6. (CONTINUED)

<i>Huebener et al. (2019)</i>	Germany (Schleswig-Holstein), 2007	Extension of paid, job protected parental leave in the first year. Removed eligibility in the second year	Mostly informal care. In rare cases formal care	Language skills, motor skills, socio-emotional stability, overall assessment of school readiness at age 6	No effects
<i>Lichtman-Sadot & Bell (2017)</i>	U.S., 2004	Introduction of paid, job protected parental leave for 6 weeks, before 12 weeks of unpaid, job protected maternity leave	Mostly informal care	Overweight, ADHD, hearing-related problems in 5/6-year-olds	Positive average effects in terms of improved health. Driven by children from less advantaged backgrounds
<i>Rossin (2011)</i>	US., 1993	Introduction of 12 weeks of unpaid maternity leave	Mostly informal care	Birth weight, early term birth, infant mortality	Small effects in terms of increase in birth weight, decrease in early term births and decreases in infant mortality for children of college-educated and married mothers
<i>Ruhm (2000)</i>	9 European countries, 1969-1994	Varying	Varying	Birth weight, deaths of infants and young children	Effects for generous leave lengths in terms of fewer deaths of infants and young children
<i>Sayour (2019)</i>	Canada, 2000	See Baker & Milligan (2008)	See Baker & Milligan (2008)	Various non-cognitive and pre-cognitive skills and various health variables	Improvements in the emotional disorder score and differences by child age
<i>Stearns (2015)</i>	U.S. (selected states), 1978	Introduction of paid maternity leave of 30 weeks for or after pregnancy	Mostly informal care	Low birth weight, early term births	Positive average effects in terms of birth weight and early term births. Larger effects for children of unmarried and black mothers

Sources: Named studies, and information drawn from Table 1 in Danzer et al. (2020) and Figure A.1 in Huebener et al. (2019).

TABLE A.7. STUDIES ON LONG-TERM EFFECTS OF PARENTAL LEAVE ON CHILD OUTCOMES

<i>Study</i>	<i>Country, year</i>	<i>Reform</i>	<i>Counterfactual care</i>	<i>Outcomes</i>	<i>Results</i>
<i>Bettinger et al. (2014)</i>	Norway, 1998	Introduction of cash-for-care for children <3 years. Before 42 weeks (100% wage comp.) or 52 weeks (80% comp.) of paid leave	40% of 1-2-year-olds in public formal childcare	GPA in tenth grade of older siblings	Positive small effects on GPA of older siblings
<i>Carneiro et al. (2015)</i>	Norway, 1977	Introduction of paid p. leave for 18 weeks. Income replacement 100%. Extension of unpaid job-protected leave from 12 weeks to 12 months	Mostly informal care. In rare cases formal care (1-2% for children <2 years)	High school dropout, college attendance, earnings (age 30), years of schooling, IQ (males), teenage pregnancy	Positive effects in terms of reduced dropout & teenage pregnancy, increased earnings, college attendance, years of schooling, IQ
<i>Dahl et al. (2016)</i>	Norway, 6 reforms 1987-1992	Extension of paid parental leave varying by 2-4 weeks in the first year of a child's life. Income replacement 100%	Mostly informal care. In rare cases formal care (1-2% of children <2 years)	Compulsory exam at the end of junior high school, high school dropout	No average effects
<i>Danzer & Lavy (2017)</i>	Austria, 1990	Extension of paid, job protected parental leave for children aged 12-24 months	Mostly informal, sometimes formal care (<3% for children <3 years)	Reading, math and science test scores at age 15/16	No average effects. Positive effects for sons of highly educated mothers
<i>Danzer et al. (2020)</i>	Austria, 1990	See Danzer & Lavy (2017)	See Danzer & Lavy (2017)	Reading, math, science test scores at age 15/16, school tracks in grades 5, 8, 9, labor market status at 17-23, physical/mental disability (up to 23), fit for military service (males, up to 23)	Positive effects on health and human capital only if the reform replaces informal care with maternal care

TABLE A.7. (CONTINUED)

<i>Dustmann & Schoenberg (2012)</i>	Germany, 3 reforms (1979, 1986, 1992)	Extension of paid, job protected parental leave from 2 to 6 months (1979), 6 to 10 months (1986). Extension of unpaid, job-protected leave from 18 to 36 months (1992)	Mostly informal care. In rare cases formal care (5% for children <18 months)	Wage, educational attainment at age 28/29 (1979), graduation from tertiary education (1986), school track at age 14 (1992)	Mostly no or very small effects. For school track (1992), some negative effects
<i>Fabel (2021)</i>	Germany, 1979	Extension of paid, job protected leave from 2 to 6 months	Mostly informal, in rare cases formal care (5% of children <18 months)	Hospital admissions and mental and behavioral disorders between ages 16-35	Positive effects in terms of reduced hospital admissions and disorders diagnosed
<i>Ginja et al. (2020)</i>	Sweden, 1986	Expansion of the birth spacing threshold for being eligible to a speed premium (higher parental leave benefits) from 24 to 30 months	Mostly formal care	Schooling performance, likelihood of college attendance by age 24 of the older and younger child	Positive effects on schooling performance & college attendance of the older, not younger child. Children from “advantaged” mothers benefit more
<i>Houmark et al. (2022)</i>	Denmark, 2002	Extension of paid parental leave from 24 to 46 weeks	Mostly formal care	School-related conscientiousness, agreeableness, emotional stability, well-being and absenteeism	Increase in well-being, conscientiousness and emotional stability, reduction in school absenteeism
<i>Liu & Nordstrom Skans (2010)</i>	Sweden, 1988	Extension of paid parental leave from 12 to 15 months gradually by 30 days each month	Mostly formal care	Test scores & grades in the last year of compulsory educ. (16-year-olds)	No average effects. Positive effects for children from mothers with high education
<i>Wuertz Ras-mussen (2010)</i>	Denmark, 1984	Extension of paid parental leave from 14 to 20 weeks	About 50% of children <2 years in formal care	High school enrollment, GPA and reading scores at age 15/16	No significant average effects

Sources: Named studies, and information drawn from Table 1 in Danzer et al. (2020) and Figure A.1 in Huebener et al. (2019).

TABLE A.8. BIG FIVE PERSONALITY TRAITS AND UNDERLYING VARIABLES

<i>Personality trait</i>	<i>Underlying variables</i>
Agreeableness	Has a forgiving nature; is considerate and kind to others; is sometimes somewhat rude to others (reversed)
Conscientiousness	Does a thorough job; does things effectively and efficiently; tends to be lazy (reversed)
Extraversion	Is communicative, talkative; is outgoing and sociable; is reserved (reversed)
Neuroticism	Worries a lot; gets nervous easily; is relaxed and handles stress well (reversed)
Openness	Is original, comes up with new ideas; values artistic, aesthetic experiences; has an active imagination; is eager for knowledge

Notes: In contrast to the short version of the Big Five inventory (Rammstedt and John 2007), in the SOEP, there is no variable for “generally, is a trusting individual”. The third variable for the agreeableness trait in the SOEP is “has a forgiving nature”.

Source: Gerlitz and Schupp (2005); Rammstedt and John (2007).

TABLE A.9. SUMMARY STATISTICS FOR LATERBORN AND FIRSTBORN CHILDREN (REFORM 1976)

	(1)			(2)			(3)			(4)		
	<i>Laterborns (1972-1975)</i>			<i>Laterborns (1976-1978)</i>			<i>Firstborns (1972-1975)</i>			<i>Firstborns (1976-1978)</i>		
	<i>Indiv.</i>	<i>Mean</i>	<i>SD</i>	<i>Indiv.</i>	<i>Mean</i>	<i>SD</i>	<i>Indiv.</i>	<i>Mean</i>	<i>SD</i>	<i>Indiv.</i>	<i>Mean</i>	<i>SD</i>
Panel A: Outcome variables												
<i>Life satisfaction (0-10)</i>	160	6.82	1.34	139	6.98	1.20	376	7.15	1.14	323	7.03	1.26
<i>Big 5: agreeableness (1-7)</i>	102	5.57	0.55	96	5.46	0.69	291	5.62	0.62	245	5.58	0.66
<i>Big 5: conscientiousness (1-7)</i>	102	6.03	0.61	96	5.99	0.57	291	6.04	0.66	245	5.94	0.67
<i>Big 5: extraversion (1-7)</i>	102	5.27	0.77	96	5.33	0.82	291	5.30	0.83	245	5.29	0.77
<i>Big 5: neuroticism (1-7)</i>	102	4.34	0.78	96	4.23	0.69	291	4.32	0.83	245	4.34	0.81
<i>Big 5: openness (1-7)</i>	102	4.73	0.85	96	4.83	0.90	291	4.75	0.90	245	4.81	0.93
<i>Self-assessed health (1-7)</i>	156	3.81	0.61	136	3.77	0.62	371	3.74	0.60	322	3.78	0.56
<i>Years of education</i>	158	12.09	2.25	129	12.37	2.47	369	12.37	2.28	316	12.58	2.37
<i>Gross income (log.)</i>	102	5.96	2.04	95	5.81	1.91	306	6.04	1.99	251	5.62	1.99
Panel B: Covariates												
<i>Year of birth</i>	160	1973.53	1.15	139	1977.13	0.82	376	1973.51	1.12	323	1977.00	0.80
<i>Month of birth</i>	112	6.55	3.37	111	6.15	3.45	318	6.28	3.40	270	5.90	3.45
<i>Gender (0/1: male)</i>	160	0.50	0.50	139	0.55	0.50	376	0.508	0.50	323	0.49	0.50
<i>Maternal education (0/1: high school)</i>	160	0.21	0.41	139	0.24	0.43	376	0.21	0.41	323	0.24	0.43
<i>Paternal educ. (0/1: high school)</i>	160	0.25	0.43	139	0.24	0.43	376	0.25	0.43	323	0.24	0.43
<i>Region growing up (0/1: urban)</i>	160	0.64	0.48	139	0.54	0.50	376	0.68	0.47	323	0.57	0.45

Notes: Assigned to the reform are laterborn children born between January 1st 1976 and December 31st 1978 (column 2).

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

TABLE A.10. RESULTS FOR THE PARENTAL LEAVE REFORM OF 1976

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Life satisfaction</i>	<i>Agreeableness</i>	<i>Conscientiousness</i>	<i>Extraversion</i>	<i>Neuroticism</i>	<i>Openness</i>	<i>Health</i>	<i>Education</i>	<i>Income</i>
Panel A: Baseline regression	0.28 (0.18)	-0.07 (0.11)	0.06 (0.10)	0.08 (0.13)	-0.14 (0.13)	0.06 (0.146)	-0.08 (0.09)	0.09 (0.33)	0.16 (0.12)
<i>N</i>	998	734	734	734	734	734	985	972	601
<i>Covariates</i>	no	no	no	no	no	no	no	no	no
Panel B: Main regression	0.13 (0.19)	-0.04 (0.11)	0.06 (0.10)	0.11 (0.13)	-0.05 (0.12)	0.08 (0.15)	-0.02 (0.09)	-0.12 (0.35)	0.14 (0.11)
<i>N</i>	811	725	725	725	725	725	811	800	594
<i>Covariates</i>	yes	yes	yes	yes	yes	yes	yes	yes	yes
Robustness (including covariates)									
Panel C: Excluding only children	-0.07 (0.20)	-0.03 (0.12)	0.13 (0.11)	0.09 (0.15)	0.12 (0.13)	0.05 (0.16)	-0.10 (0.10)	-0.22 (0.37)	0.10 (0.12)
<i>N</i>	554	499	499	499	499	499	554	548	404
Panel D: 1973-1978	0.04 (0.21)	-0.07 (0.11)	0.10 (0.11)	0.09 (0.14)	-0.07 (0.13)	0.08 (0.17)	-0.06 (0.10)	-0.19 (0.39)	0.145 (0.12)
<i>N</i>	701	630	630	630	630	630	701	691	509
Panel F: 1973-1977	-0.04 (0.24)	-0.02 (0.13)	0.15 (0.12)	0.16 (0.17)	-0.05 (0.15)	0.20 (0.19)	-0.06 (0.11)	-0.35 (0.45)	-0.02 (0.14)
<i>N</i>	569	517	517	517	517	517	569	561	419

Notes: This table displays DiD estimates resulting from OLS regressions. Each panel contains results from one regression. Control variables include year of birth, month of birth, gender, maternal and paternal education, and the region of growing up. Individuals are included if life satisfaction measures are reported. The sample in Panel A and B consists of birth cohorts 1972 to 1978. Robust standard errors are reported in parentheses. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

TABLE A.11. HETEROGENEITY ANALYSIS FOR MALE INDIVIDUALS (REFORM 1976)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Life satisfaction</i>	<i>Agreeableness</i>	<i>Conscientiousness</i>	<i>Extraversion</i>	<i>Neuroticism</i>	<i>Openness</i>	<i>Health</i>	<i>Education</i>	<i>Income</i>
Panel A: Baseline regression	0.16 (0.25)	-0.09 (0.15)	0.01 (0.14)	0.28 (0.20)	-0.08 (0.15)	0.28 (0.21)	-0.03 (0.12)	0.06 (0.46)	0.12 (0.17)
<i>N</i>	505	361	361	361	361	361	498	488	285
<i>Covariates</i>	no	no	no	no	no	no	no	no	no
Panel B: Main regression	-0.15 (0.28)	-0.04 (0.15)	0.04 (0.14)	0.32 (0.20)	-0.02 (0.16)	0.31 (0.21)	0.04 (0.13)	-0.43 (0.53)	0.11 (0.16)
<i>N</i>	403	357	357	357	357	357	403	395	283
<i>Covariates</i>	yes	yes	yes	yes	yes	yes	yes	yes	yes
Robustness (including covariates)									
Panel C: Excluding only children	-0.20 (0.28)	-0.04 (0.17)	0.03 (0.15)	0.32 (0.23)	0.04 (0.17)	0.14 (0.24)	-0.03 (0.14)	-0.46 (0.59)	0.11 (0.16)
<i>N</i>	282	253	253	253	253	253	282	279	198
Panel D: 1973-1978	-0.18 (0.30)	-0.12 (0.16)	0.10 (0.15)	0.36 (0.23)	-0.08 (0.17)	0.31 (0.24)	-0.03 (0.14)	-0.48 (0.62)	0.07 (0.17)
<i>N</i>	350	312	312	312	312	312	350	343	246
Panel F: 1973-1977	-0.47 (0.34)	-0.13 (0.18)	0.11 (0.16)	0.42* (0.25)	0.09 (0.19)	0.51* (0.27)	-0.07 (0.16)	-0.75 (0.73)	-0.20 (0.19)
<i>N</i>	279	251	251	251	251	251	279	274	201

Notes: This table displays DiD estimates resulting from OLS regressions. Each panel contains results from one regression. Covariates include year of birth, month of birth, gender, maternal and paternal education, and the region of growing up. Individuals are included if life satisfaction measures are reported. The sample in Panel A and B consists of birth cohorts 1972 to 1978. Robust standard errors are reported in parentheses. Levels of significance: *** p<0.01, ** p<0.05, * p<0.1.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

TABLE A.12. HETEROGENEITY ANALYSIS FOR FEMALE INDIVIDUALS (REFORM 1976)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Life satisfaction</i>	<i>Agreeableness</i>	<i>Conscientiousness</i>	<i>Extraversion</i>	<i>Neuroticism</i>	<i>Openness</i>	<i>Health</i>	<i>Education</i>	<i>Income</i>
Panel A: Baseline regression	0.42*	0.05	0.17	-0.02	-0.03	-0.08	-0.18	0.21	0.14
	(0.24)	(0.15)	(0.15)	(0.18)	(0.19)	(0.22)	(0.12)	(0.48)	(0.15)
<i>N</i>	493	373	373	373	373	373	487	484	316
<i>Covariates</i>	no	no	no	no	no	no	no	no	no
Panel B: Main regression	0.39	-0.03	0.11	-0.13	-0.10	-0.16	-0.10	0.17	0.26*
	(0.25)	(0.16)	(0.15)	(0.18)	(0.19)	(0.23)	(0.12)	(0.46)	(0.14)
<i>N</i>	408	368	368	368	368	368	408	405	311
<i>Covariates</i>	yes	yes	yes	yes	yes	yes	yes	yes	yes
Robustness (including covariates)									
Panel C: Excluding only children	0.16	-0.04	0.26	-0.19	0.19	-0.11	-0.20	0.04	0.23
	(0.26)	(0.18)	(0.16)	(0.20)	(0.20)	(0.25)	(0.14)	(0.49)	(0.16)
<i>N</i>	272	246	246	246	246	246	272	269	263
Panel D: 1973-1978	0.25	-0.03	0.11	-0.21	-0.09	-0.17	-0.14	0.06	0.29*
	(0.275)	(0.17)	(0.17)	(0.19)	(0.21)	(0.25)	(0.14)	(0.52)	(0.16)
<i>N</i>	351	318	318	318	318	318	351	348	218
Panel F: 1973-1977	0.41	0.06	0.18	-0.13	-0.21	-0.12	-0.07	0.07	0.20
	(0.31)	(0.20)	(0.18)	(0.23)	(0.23)	(0.29)	(0.15)	(0.58)	(0.17)
<i>N</i>	290	266	266	266	266	266	290	287	206

Notes: This table displays DiD estimates resulting from OLS regressions. Each panel contains results from one regression. Covariates include year of birth, month of birth, gender, maternal and paternal education, and the region of growing up. Individuals are included if life satisfaction measures are reported. The sample in Panel A and B consists of birth cohorts 1972 to 1978. Robust standard errors are reported in parentheses. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

CHAPTER 4: PARENTAL LEAVE AND MOTHERS***

The long-term impact of paid parental leave on maternal health and subjective well-being

This paper studies the long-term impact of a paid parental leave reform in former East Germany in 1986 on maternal physical and mental health and subjective well-being. The reform extended paid leave for first-time mothers by six months to a maximum of twelve months. I use representative survey data from the German Socio-Economic Panel and a difference-in-differences design in a quasi-experimental setting. Results show that the effects of the reform were negligible on maternal long-term physical and mental health and subjective well-being. There is weak, but not robust, evidence for increased satisfaction with household activities, income, and work.

1. Introduction

Paid parental leave is a common tool for women giving birth to children and caring for them without fearing losing their job. Furthermore, parental leave enables mothers to find a new job with higher financial security after a break related to childbirth. In the literature, the lack of paid parental leave or a guarantee that mothers can return to previous employers is considered a “family barrier” (Waldfogel 1998, p. 508). Such a barrier prevents women with children from succeeding in their job equally well as women without children or men (Waldfogel 1998). Parental leave is a policy tool contemporarily implemented in almost all OECD countries at the

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national level, except for the U.S. (OECD 2018). However, allowing mothers to manage family and working life is not the sole argument in favor of job-protected paid parental leave policies. Policymakers introduced parental leave to allow mothers to spend more time with their infants without losing financial independence or attachment to the labor market and to promote maternal and child health (International Labour Office 2010).

Previous studies suggest that introducing or extending parental leave might have achieved the desired aims of parental leave policies. For example, studies showed positive and sometimes causal impacts of parental leave on a delay in return-to-work behavior and the time a mother spends with her infant postpartum (e.g., Baker and Milligan 2008b; Dustmann and Schoenberg 2012; Carneiro et al. 2015; Hewitt et al. 2017; Guertzgen and Hank 2018; Albagli and Rau 2019; Canaan 2022).

There are also studies showing that parental leave impacts maternal health in the short term (Chatterji and Markowitz 2005; Beuchert et al. 2012 and 2016; Hewitt et al. 2017; Guertzgen and Hank 2018; Albagli and Rau 2019; Lee et al. 2020; Chuard 2023) and, less frequent, in the long term (Avendano et al. 2015; Guertzgen and Hank 2018; Buetikofer et al. 2021; Chuard 2023). There is also a broad literature concluding that parental leave has positive effects on a child's health both in the short term (Ruhm 2000; Berger et al. 2005; Rossin 2011; Stearns 2015; Lichtman-Sadot and Bell 2017; Bullinger 2019) and long term (Carneiro et al. 2015; Danzer et al. 2020; Fabel 2021).⁴²

However, there are many debates on parental leave policies. Often, opponents are concerned about the maternal labor market position, financial situation, and gender equality aspects. Parental leave might worsen female labor market positions and wages, as (a long) time away from work decreases labor market attachment and wages due to human capital depreciation (Albrecht et al. 1999; Anderson et al. 2002; Spivey 2005; Schoenberg and Ludsteck 2014; Rossin-Slater 2018). Longer paid leave schemes are exceptionally costly for employers who might hesitate to employ women of child-bearing age or offer them lower wages than men (Gruber 1994; Ruhm 1998; Schoenberg and Ludsteck 2014). As policymakers primarily target these policies at mothers, this might also impact gender roles via a further reinforcement

⁴² Another large strand of the literature finds significant effects of parental leave reforms on non-cognitive and cognitive skills as well as labor market outcomes of children (Dustmann and Schoenberg 2012; Carneiro et al. 2015; Danzer and Lavy 2017; Albagli and Rau 2018; Danzer et al. 2020; Canaan 2022).

of the social norm that it is mainly the task of women to raise children.⁴³ It might also increase the workload of fathers, who then have even less time for their children (Canaan 2022).

In a country comparison, the duration, degree of job protection, and wage replacement rates vary significantly: Most OECD countries grant around 52 weeks of paid leave. Generous in terms of length are Estonia, Hungary, Slovakia, and Finland, who grant more than 150 weeks of total leave to mothers. Eight OECD countries grant the leave-taking parent a full-pay equivalent in the first year of parental leave. In contrast, there is no federally funded paid leave for mothers in the U.S.: On the federal level, there are twelve weeks of unpaid leave for mothers at the most (OECD 2018).⁴⁴

As leave schemes around the globe are diverse, more research is needed to fully understand how different schemes of parental leave impact mothers, children, and families. Research focusing on the impacts of parental leave reforms on affected mothers is exceptionally scarce in two fields: subjective well-being effects and long-term health effects of parental leave reforms with medium-long durations (i.e., approximately one year of leave). Furthermore, it is not yet fully answered if there are positive consequences of paid parental leave in societies with a well-established childcare system for very young children, i.e., a system in which mothers are encouraged to participate in the labor market in full-time jobs.

Therefore, the research question focuses if an extension of paid parental leave from six to twelve months affects long-term maternal health and subjective well-being. For this purpose, I use a parental leave reform of former East Germany from May 1st in 1986.⁴⁵ The reform introduced one year of leave for all first-time mothers. It was announced unexpectedly and offered job-protected leave with a wage-replacement rate between 70 and 90% (Kreyenfeld 2004). In the historical setting of former East Germany, there was almost no maternal self-selection into the labor market and into

⁴³ Paternal leave policies have been in place in many countries recently. Nevertheless, the share of fathers taking leave and their leave durations are much smaller than those of mothers (see Korsgren and van Lent 2022).

⁴⁴ In the U.S., roughly half the working force cannot afford unpaid leave (Lee 2020). Although no federal program exists, some states provide paid leave benefits (OECD 2018).

⁴⁵ Heisig and Zierow (2019) also studied this reform but looked at the long-term outcomes of affected children. The estimation strategy in this paper is closely related to their study.

taking paid leave: Female labor market participation rate was as large as 86% in 1989, excluding trainees and university students (Obertreis 1986; Winkler 1990). Full-time employment was as high as 75% for women of working age and 85% for mothers (Israel 2008). Furthermore, in the late 1980s, up to 95% of mothers used paid parental leave (Hoeckner 1995). Therefore, the setting in this study is favorable, as many prior papers struggle with severe maternal self-selection and lack a precise counterfactual scenario. No precise counterfactual scenario might bias the universal validity of results. Previous studies face the problem that only a few women and mothers were employed before the government implemented any (paid) parental leave. Thus, after giving birth to a child, the situation changed only for a small selective group of mothers. Results are then only valid for this specific group.

The main contribution of this study is to add to the very sparse literature on the effects of paid parental leave on subjective well-being, as this is the first study to examine the long-term effects of maternal parental leave on her subjective well-being.⁴⁶ Long-term studies are needed because subjective well-being is increasingly seen as essential for measuring living conditions in a welfare state (Kahneman and Krueger 2006; Clark et al. 2008; Diener 2009; Deaton and Stone 2013; Benjamin et al. 2019; Frijters et al. 2020). Previous studies hint that parental leave might have long-term effects on affected children's subjective well-being (Heisig and Zierow 2019; Houmark et al. 2022). Also, paternal leave might have long-term effects on fathers' and mothers' subjective well-being (Korsgren and van Lent 2022). However, there are no studies so far on the long-term effects of maternal leave on affected mothers. If paid leave promotes subjective well-being of a significant share of the population in the long run, this could be of importance for policymakers.

This study is the first paper that causally analyzes paid leave's long-term effects on maternal health with a generous pre-reform leave length. It contributes to the scarce literature studying the effects of paid parental leave with a medium-long duration (i.e., one year) on long-term maternal physical and mental health.⁴⁷ Long-term studies so far look at shorter (Buetikofer 2021) or longer (Guertzgen and Hank 2018; Chuard 2023) leave lengths, but not medium-long⁴⁸ lengths of about one year. As

⁴⁶ So far, three studies have analyzed short-term leave effects (Pezzini 2005; D'Addio et al. 2014; Maeder 2014).

⁴⁷ This paper is closely related to Baker and Milligan (2008a) and Beuchert et al. (2016), who look at leave extensions from six to twelve months, but on short-term outcomes.

⁴⁸ Avendano et al. (2015) examine many European reforms – including medium-long leave. However, they do not differ between the reform's leave lengths and use an indicator of leave length*wage replacement rate.

medium-long leave is most common in recent economies, research should focus on it more frequently. Also, previous literature suggests the most beneficial health and well-being effects for not too short and not too long paid leave periods. However, this study does not focus on maternal labor market outcomes, such as labor market participation or wages. Firstly, maternal labor market attachment was already very high in the former GDR before the reform. Thus, no sizable average changes might appear, especially in the long term. Secondly, the data set used does not include individuals before 1990/91. Instead of objective variables, this study focusses on the subjective variables work satisfaction and income satisfaction.

This study isolates the causal effect by applying a difference-in-differences approach. It exploits that the reform solely targeted first-time mothers.⁴⁹ I compare outcomes of mothers with an only child born between 1982 to 1989 before and after the reform. The control group contains mothers with at least two children and a second child born in the same observation period. I apply representative survey data from the German Socio-Economic Panel (SOEP) of mothers who gave birth to a child in the GDR. This study uses all available survey waves to circumvent bias due to short-term changes in observed well-being and health (e.g., death of relatives, severe illness). Outcomes are measured up to 37 years after the respective birth.

Results suggest negligible effects of extending paid parental leave from six to twelve months. In the context of (former) East Germany, extending already generous paid parental leave yields no further substantial increase in health benefits for affected mothers. Thus, the results corroborate previous studies on extending generous paid leave and its (short-term) effects on health (Baker and Milligan 2008a; Beuchert et al. 2016). In terms of subjective well-being, this study finds weak evidence for increased satisfaction with household activities, income, and work of affected mothers. Throughout a range of robustness checks, the effects remain negligible, suggesting a solid attachment to the labor market of mothers in the former GDR.

The remainder of this study is as follows: Chapter 2 describes the theoretical background and previous literature. Chapter 3 informs about the social and economic background of former East Germany in the 1970s and 1980s and the reform used. Chapter 4 describes the empirical approach, Chapter 5 the data set, and Chapter 6

⁴⁹ Mothers with more than one child have been eligible since 1976. Thus, regarding the eligibility to take paid parental leave, everything stayed the same for them in 1986.

shows descriptive statistics. In Chapter 7, the results of the analysis are introduced. Chapter 8 discusses the results and concludes.

2. *Theoretical background and related literature*

2.1 Maternal time allocation and well-being of mother and child

From a theoretic perspective, the effect of paid parental leave on maternal (and child) health and subjective well-being is unclear. According to the household production model of Becker (1965) and the production of health model of Grossman (1972), working mothers maximize their utility function, which includes their and their child's health and well-being. Market goods and time both produce health and subjective well-being. As mothers have a limited budget of time, the optimal quantities of her own and her child's health demanded by the mother depend on the marginal utilities and shadow price (Chatterji and Markowitz 2005).

A working mother's time is divided between work at the labor market, work at home, and time spent to maximize her and her child's mental and physical health and well-being. If she re-enters the labor market postpartum, the opportunity costs of time for childcare and leisure time increase. Thus, the shadow prices of her health and well-being increase, suggesting a decrease in her health and well-being (Chatterji and Markowitz 2005). However, if labor market work enhances maternal (mental) health as well (Grzywacz and Bass 2003; Brook et al. 2008; Zagefka et al. 2021), there might be a "cut-off" length of leave that still yields positive effects. Thus, as health and well-being improve with time and market goods, the effect of paid parental leave on health and well-being is unclear. The sole income effect through paid work on the labor market suggests an increase in (the demand for) health and well-being. The combined effects, however, depend on the relative time intensity of the health and well-being production functions and the wage replacement rate of leave (Chatterji and Markowitz 2005).

Previous empirical studies support the theoretic assumptions regarding time allocation and maternal and child health. Mothers who re-enter the labor market early face more physical and mental health restrictions than mothers who recover longer (Gjerdingen et al. 1993; McGovern et al. 1997; Chatterji et al. 2013; Dagher et al. 2014). After giving birth, roughly 13-25% of women face mental health issues such as postpartum blues/depression or other psychiatric disorders, including depression and anxiety. Posttraumatic stress disorders and many physical health problems are

common (O'Hara and Swain 1996; Wisner et al. 2002; Brockington 2004; Cheng and Li 2008; Vesga-López et al. 2008; Pearlstein et al. 2009). Having a single episode of depression increases the risk of a major depression or other disorder later in life (O'Hara and Swain 1996; Wisner et al. 2002; Vesga-López et al. 2008; Pearlstein et al. 2009). Mental health restrictions are an enormous challenge for the global health system: Depression is the second largest cause of years of disability (Ferrari et al. 2013). It affects up to 44% of the world's population (WHO 2017), and its costs are a significant economic burden in developed economies such as the U.S. (Greenberg et al. 2003) and the European Union (Andlin-Sobocki et al. 2005; Olesen et al. 2012).

Apart from mental and physical health issues, women face other challenges after giving birth. For example, identifying with the new role as a mother might be challenging (Mercer 1985; McGovern et al. 1997). Mothers who sooner re-enter the labor market face more severe issues with adapting to the role (McGovern et al. 1997; Chatterji et al. 2013). Furthermore, the interaction style and quality of mother-infant interaction were found to correlate with antenatal and postnatal depressive symptoms of mothers (Clark et al. 1997; Wisner et al. 2002; Binda et al. 2019) and maternity leave length (Clark et al. 1997).

The mother-infant relationship quality impacts the infant regarding attachment, early interactions, and cognitive functioning (Murray and Cooper 1996; Brockington 2004). Disorders in the mother-infant relationship concern 10 to 25% of mothers in psychotherapy (Brockington 1996). However, if there are no role conflicts, well-being is improved by boosting identification sources and self-esteem (Grzywacz and Bass 2003; Brook et al. 2008; Zagefka et al. 2021). Thus, previous literature shows positive effects of a later re-entry into the labor market on maternal mental and physical health and well-being, which can positively influence the infant's health (Ruhm 2000; Rossin 2011; Stearns 2015; Bullinger 2019).

2.2 Parental leave and maternal health

Studies on parental leave reforms confirm that a later re-entry into the labor market positively affects maternal mental and physical health. However, often they show a turning point, where positive effects diminish or reverse into adverse effects.⁵⁰ For

⁵⁰ For a summary of studies that analyze parental leave reform effects, see Table A.13.

short-term effects, Chatterji and Markowitz (2005 and 2012) analyze the introduction of twelve weeks of unpaid maternity leave in the U.S. As maternal leave durations vary in their sample⁵¹, they show that maternity leave affects health dependent on leave lengths. They apply an instrumental variables (IV)-approach by using state-level labor market conditions and state-level maternal leave policies as instruments. The likelihood of clinical depression and outpatient visits during the first six months after childbirth is almost unaffected. Mothers absent for more than eight weeks reported better overall health, and mothers absent for more than twelve weeks reported fewer depressive symptoms after a few months postpartum.

Baker and Milligan (2008a) analyze an increase in paid maternity leave from 25 weeks to 50 weeks in Canada with a wage replacement rate of up to 55%. The authors find a significant increase in mothers' time absent from work but no impact on self-reported health, depression, and other postpartum health issues up to two years after giving birth.

Dagher et al. (2014) apply an IV approach on data of eligible mothers in Minnesota for a maximum of 52 weeks of paid leave with partial wage replacement since 1997. They find a u-shaped relationship between leave duration and short-term postpartum depression with a minimum probability at 26 weeks and a small positive association with physical health. Beuchert et al. (2016) study a reform in Denmark in 2002. Before, mothers were entitled to 24 weeks with complete income replacement and 52 weeks with a replacement rate of 60%. After the reform, mothers became eligible for 46 weeks of paid leave with full income replacement. The reform increased maternal health up to five years after childbirth in terms of fewer inpatient and outpatient hospital admissions, but other outcomes (emergency department visits, antidepressant prescriptions) were unaffected.

Beneficial effects are more pronounced for low- than for high-resource families. Hewitt et al. (2017) analyze the effects of a reform in 2011 in Australia that introduced 18 weeks of paid leave at a minimum wage rate. Before, mothers were eligible for twelve months of unpaid leave. They find minor positive effects on maternal mental and physical health one year postpartum. Mandal (2018) shows negative effects of a return to work within twelve weeks after giving birth on short-term mental health for mothers in the U.S. However, this effect was alleviated when mothers took paid leave. Women who returned to work within 36 weeks after giving birth had better

⁵¹ For example, they differ between returning to work six to eight, eight to twelve, or later than twelve weeks postpartum.

mental health scores than mothers who returned within twelve weeks. Albagli and Rau (2019) study a paid maternity leave increase from twelve to 24 weeks in 2011 in Chile with full income replacement. They find a significant reduction in stress up to one year later. Lee et al. (2020) study California's paid parental leave reform of 2004, which introduced six weeks of paid leave with a wage replacement rate of 55% (no job protection). Before the reform, twelve weeks of unpaid leave after birth was granted. They show increases in self-rated health and decreases in psychological distress, the likelihood of being overweight, and alcohol consumption.

Apart from papers on the short term, few studies concentrate on long-term health effects. In a country comparison across Europe, Avendano et al. (2015) find small and positive long-term effects of more generous leave (indicator: length*wage replacement rate) on mental health in mothers roughly 25 years after giving birth to their first child. Guertzgen and Hank (2018) investigate the effects of a paid leave expansion from eight to 24 months with a wage replacement rate of roughly 33% in former West Germany in 1979. Up to 30 years after childbirth, mothers were more frequently absent from work for more than six weeks. They argue mothers with worse pre-reform health re-enter the labor market due to the reform. They find no effects on maternal health.

Buetikofer et al. (2021) estimate the long-term health effects of a reform in 1977 in Norway and six subsequent expansions between 1987 and 1992. Before, mothers were eligible for twelve weeks of unpaid leave. After that, they could apply for four months of paid – with full income replacement – and twelve months of unpaid leave. The reform improved maternal body mass index (BMI), blood pressure, pain, mental health, and health-promoting behaviors (exercising, no smoking). They find diminishing returns to leave lengths: extending paid leave by two or more weeks had less robust effects.

Chuard (2023) analyzes the long-term health effects of three subsequent reforms in Austria from 1990 to 2000 on various physical and mental health measures (among them outpatient costs, medication, days of hospitalization, mental disorders, and circulatory system diseases). Results suggest a hump-shaped relationship between leave length and health effects, and adverse health effects for exceptionally long parental leave lengths (up to 2.5 years). However, long leave spells might benefit low-

SES mothers, mothers of girls, and mothers with unhealthy babies. She also suggests that health effects accumulate over time.

The following conclusions from the health literature can be drawn: a) Health benefits are most considerable for short (but not too short) and medium-long leave lengths (this might be applied to subjective well-being as well). b) If leave lengths are too long, health effects might turn negative. c) Health effects might accumulate over time. d) Expanding leave from an already generous level yields more negligible or no effects than introducing parental leave. There is not enough literature on subjective well-being effects to conclude apparent effects. For this study on long-term effects on subjective well-being and health, arguments a) and c) suggest that positive effects on maternal mental and physical health can be expected, possibly on subjective well-being as well. However, argument d) proposes only minor or no results, as the reform under study extends parental leave from an already generous leave length.

2.3 Parental leave and subjective well-being

Literature on the effects of parental leave reforms on subjective well-being is scarce.⁵² Pezzini (2005) finds negligible short-term effects of reforms between 1975 and 1998 in twelve European countries. D'Addio et al. (2014) analyze various reforms from (West) Germany between 1984 and 2008 and Great Britain between 1973 and 2007. By applying a DiD analysis and an instrumental variables estimation, they find positive short-term effects on life satisfaction in Germany up to six months after delivery. They show a decrease in life satisfaction for leave lengths longer than 16 months, suggesting that effects on life satisfaction might follow a hump-shaped pattern. Effects for Great Britain are relatively similar but somewhat smaller in size.

Maeder (2014) studies a change in leave benefits in Germany in 2007. Prior to the reform, mothers received 300€ for 24 months or 450€ for 12 months per child. After the reform, they were able to take paid parental leave of up to 14 months with a 67% wage replacement rate. There are no statistically significant overall effects up to 5.5 years after giving birth for various satisfaction variables (life, school, training/job, friends/social network, family). However, she finds an increase in life, school/job, and family satisfaction for mothers having a partner with high- or medium-level education, and an increase in school or job and family satisfaction for West German mothers. For East German mothers, results suggest decreased satisfaction with the

⁵² See Table A.14 for a summary of previous studies.

social network. These differences might stem from varying preferences of West and East German women about working or staying at home as a mother, originating in different historical backgrounds.

To conclude, literature of parental leave effects on well-being is yet too scarce for clear findings. It is likely that negative effects on well-being arise, as Maeder (2014) suggested for East Germany. In contrast, other studies suggest no or slightly positive effects of parental leave on maternal life satisfaction.

3. *Family policies in the GDR and the parental leave reform of 1986*

The government of the GDR tried to increase female labor market participation due to a significant decrease in population size in the 1960s. Many policies – e.g., the law on maternity and child protection (1950), the family code (1965), and the law on abortion (1972) – were introduced to promote the independence of women and mothers. As a result, both married women and mothers increasingly entered higher education and the labor market. Center-based care was expanded vastly (Obertreis 1986).⁵³ As a result, female labor market participation increased to 83.8% in 1979 and 85.8% in 1989 (Winkler 1990). Up to 75% of women and 85% of mothers were employed on a full-time basis (Höckner 1995; Israel 2008).

However, domestic work was not divided between men and women, doubling the workload of the latter (labeled as “the second shift”), which was an enormous physical and mental burden for women (Helwig 1988). In the late 1960s, fertility plunged. The government tried to counteract with various policies to increase fertility in the 1970s and 1980s.⁵⁴ One of the measures was one year of job-protected, paid parental leave with a wage replacement rate between 70 and 90% – depending on the mother’s sickness allowance – for mothers with at least two children on the 27th of

⁵³ More than 80% of all infants and toddlers attended formal center-based care in 1986 (Statistisches Jahrbuch der DDR 1988), and up to 90% of all newborn- to three-year-old children attended full-day center-based care in the late 1980s (Helwig 1987; Zwiener 1994; Braun and Klein 1995).

⁵⁴ For example, child benefits, birth grants, and interest-free loans for marriage were introduced and extended to higher age limits and sums. Mothers became eligible for reduced working hours, extra holidays, and paid child sick leave. Also, policymakers extended paid maternity leave (100% wage replacement rate) to 18 weeks in 1972 and 26 weeks in 1976. Single first-time mothers were eligible for paid parental leave if no childcare place was available. Mothers with three or more children became eligible for 1.5 years of paid leave in 1984 (Kreyenfeld 2004).

May 1976 (Kreyenfeld 2004). After its introduction, short-term fertility increased temporarily (Buettner and Lutz 1990; Conrad et al. 1996).

Family policies were expanded to counteract the further decreasing fertility in the early 1980s (Buettner and Lutz 1990; Conrad et al. 1996). The reform under study in this paper is the reform of 1986. In 1986, paid parental leave was expanded to all mothers starting with the 1st of May. The reform was announced in mid-April on short notice (Helwig 1988). Therefore, mothers could not adjust their child's birth date to be born after the reform. Also, there was no fertility response after the reform (Buettner and Lutz 1990; Conrad et al. 1996).

Before the reform, first-time mothers were the only group not eligible for one year of paid leave (Braun and Klein 1995). The most common pre-reform scenario for them was full-time, or, less frequently, part-time employment and a maximum of six months of fully paid maternity leave (Winkler 1990).⁵⁵ In contrast, studies exploiting reforms in other countries lack a precise pre-reform scenario of mothers. Some mothers were homemakers, many were employed part-time, and others were employed full-time. Since only a particular share of mothers would have been in the labor market without the reform, the post-reform framework changed only for a selective group of mothers. In this case, effects are measured only for this self-selected subgroup of mothers.

Considering the eligibility for paid leave, all mothers with children younger than one year by the 1st of May 1986 were eligible. The eligibility criterium implies that first-time mothers with a child born in the second half of 1985 were partly eligible. For example, if a child aged ten months on the 1st of May, their mother could apply for two months of paid leave. However, it is unlikely that mothers who already returned to their workplace after the end of maternity leave (i.e., mothers with children born before November 1985) used the additional paid parental leave.

Thus, assigned are, most likely, mothers who gave birth in November 1985 or later. However, mothers with children born in 1985 might have already had a place in childcare and decided to return to work nonetheless, without extending parental leave. This situation might apply to a smaller extent to mothers with children born in the first few months of 1986 as well.

⁵⁵ They were eligible for one year of unpaid leave. However, mothers rarely used unpaid leave, as most mothers could not afford it, and the socialist regime expected women to participate in the labor market (Kreyenfeld 2004).

Regarding take-up rates, there was little to no self-selection: In the late 1980s, up to 95% of mothers used paid leave (Hoeckner 1995). Data also shows that in 1988, only 1% of children under one year attended childcare – another hint that most mothers used the entire parental leave duration (Israel 2008).

4. *Empirical approach*

This study uses a difference-in-differences approach to investigate whether the reform of 1986 had long-term impacts on maternal health and subjective well-being. The eligibility criterium for being a first-time mother is exploited in a quasi-experimental setting to causally identify the effect of the shift in leave duration. An essential condition is that a pre-reform common trend exists in the assigned and control groups' outcomes. Common trends are expected, as other family policies targeted at mothers were absent in the observed time frame. Because the data does not contain if and for how long mothers made use of paid parental leave, this study estimates an intention-to-treat effect.

The assigned group consists of first-time mothers who gave birth in former East Germany to their only child in January 1986 or later. This restriction ensures that she was, firstly, eligible for the entire duration and, secondly, still on maternity leave. The constraint to mothers with an only child is essential, as mothers in the assigned group are required to be fully distinguishable over the whole observation period from mothers in the control group. The control group consists of mothers with two (or more) children, where the second child was born between 1982 and 1989, and mothers with an only child born between 1982 and May 1985 (Figure 3).

In other settings, it might be feasible to use other control groups. However, in the underlying setting, it is problematic: Firstly, I cannot use mothers in West Germany as a control group as there were several changes in family policies in the observed period. For example, parental leave was extended from six to ten months in 1986 and to twelve months in 1988 (Kreyenfeld 2004).

Secondly, using fathers as a control group might be prone to errors as well, as they were also eligible to take paid parental leave. However, they rarely used it (Helwig 1988), but they might have been indirectly affected by the reform by their wives or partners: If mothers were healthier and happier after the reform, this might indirectly impact her partner.

FIGURE 3. CLASSIFICATION OF MOTHERS IN THE ANALYSIS

	<i>Birth of child in 1982-1985</i>	<i>Birth of child in 1986-1989</i>
<i>Mother with two or more children</i>	Not subject to reform N = 420	Not subject to reform N = 437
<i>Mother of an only child</i>	Not subject to reform N = 159	Subject to reform N = 197

Notes: The Figure shows the assigned and control groups of the analysis, as well as the sample size for the subgroups.

Source: Own illustration.

The underlying equation of the regression model can be written as follows:

$$y_{ij} = \lambda(Mother_{onlychild})_{ij} \times (Postreform)_{ij} + \beta_1(Mother_{onlychild})_{ij} + \beta_2(Postreform)_{ij} + \pi_j + \mu X_i + c + \epsilon_{ij} . \quad (5)$$

The term y_{ij} indicates estimated health and subjective well-being outcomes of mother i who gave birth to her first/only or second child in year j . The dummy variable $(Mother_{onlychild})_{ij}$ designates if the respective mother i gave birth to her first/only (the dummy is 1) or second child (the dummy is 0) in year j of the study period. The coefficient β_1 captures general differences between mothers with an only child or more children. The dummy variable $(Postreform)_{ij}$ indicates if a mother gave birth to a child in the reform year of 1986 or later (the dummy is 1) or before 1986 (the dummy is 0). β_2 captures general differences between mothers giving birth before or after the reform. The interaction term of the dummy variables $(Mother_{onlychild})_{ij} \times (Postreform)_{ij}$ shows eligibility (if the interaction term is 1). λ measures the reform effect on health and subjective well-being outcomes. π_j contains cohort fixed effects, X_i covers covariates, c is the constant, and ϵ_{ij} is the heteroskedasticity-robust error term.

I use alternative eligibility criteria and additional covariates to test the robustness of estimates. Also, as this paper studies various outcomes, I verify whether regression estimates hold after adjusting p-values for multiple hypothesis testing by calculating Westfall-Young stepdown adjusted p-values by using the Stata-command `wyyoung`.⁵⁶

⁵⁶ For more information on this method, see McKenzie (2021).

5. Data

This study uses data from the German Socio-Economic Panel (SOEP), which is a representative sample of the population in Germany. From 1990/91 on, it also covers individuals in East Germany (Goebel et al. 2019). Only women who gave birth in former East Germany are subject to this study. Therefore, I exclude women who lived in West Germany in 1989 and women with missing information about their location in 1989. I use all possible SOEP waves to calculate the outcome variable average to circumvent bias in the data due to short-term changes in the observed well-being and health variables. Short-term decreases or increases might occur due to unobserved events such as the death of a relative, friend, or child, a phase of severe illness, or other challenging life phases (e.g., a divorce).

Treatment variable

The treatment variable (intention-to-treat) is unity for mothers who gave birth to their first child in January 1986 or later. It takes the value zero for all other groups of mothers. Cut-off dates are January 1st, 1982, and December 31st, 1989, to exclude directly affected individuals by the reform of 1976 and mothers with children born after the German unification, as parental leave schemes changed afterward.

Outcome variables

The SOEP contains self-reported items on an individual's *physical and mental health*. This study includes five measures for overall health and two for mental health. I use a measure for *self-assessed health*, where respondents rate their overall health. It ranges from one (best) to five (worst). Included in the analysis is a recoded version of this variable where five is associated with the best possible health assessment. Data is available for 27 waves, covering 1992 to 2019. I also use the *BMI index* calculated using data from 2002 to 2019 on the weight and height of a person. The sample covers nine waves, as the SOEP queries the variables bi-annually.⁵⁷ A measure for the *number of outpatient visits* in the previous year is calculated using the average of 28 waves, covering 1991 to 2019. Furthermore, I include the number of *absent days from work due to illness*. It is calculated as an

⁵⁷ A value between 18.5 and 24.9 represents a healthy weight. However, values considered healthy are increasing with age.

average from 17 waves, covering 2002 to 2019. Furthermore, this study uses two variables on mental health. Firstly, the variable *rarely depressed* measures how often the mother felt depressed within the last four weeks. The question is queried biannually. One is the worst (“all the time”), and five is the best (“never”) value. I calculate the average mental health state using values of eleven waves, covering 1998 to 2019. Secondly, I use a dummy variable for *clinical depression*, where one indicates a clinical case of depression or other mental illness and zero if otherwise. Data is available for six waves; this variable has been included since 2008 and is covered biannually.

The SOEP covers dimensions of subjective well-being as satisfaction with different life spheres. This study includes dimensions that can be affected by parental leave: overall *life satisfaction*, *work satisfaction*, personal *income satisfaction*, and *satisfaction with household activity*. All variables are measured on an 11-point scale of zero (lowest) to ten (highest). Questions on life and work satisfaction are covered from 1990 to 2019 and asked in 29 waves. The SOEP queries satisfaction with personal income since 2003. Thus, the average of this variable is calculated using 16 waves. Satisfaction with household activity is included from 2006 onwards, covering 14 waves.

Covariates

Covariates are *year of birth*, *age at birth*, *German nationality*, and a dummy for the *current region of living* (whether the respondent contemporarily lives in East or West Germany). I also include an *education dummy* defined as having a high school diploma (“Abitur” or vocational baccalaureate diploma) and a dummy indicating whether the mother of the respondent has some university education or a university degree (*academic mother*). A dummy for the *region of growing up* (urban or rural) indicates whether the respondent grew up in a small, medium, or big city or a rural area.⁵⁸

In robustness checks, I further include dummies for the *child’s gender* and *birth month* and the *relationship status* of the respondent (married, partnership, single) in the year before the birth of the respective child. Relationship status is only available for a subsample of observations. I focus on covariates most likely unaffected by the reform itself.

⁵⁸ I code missing data for the variables *academic mother*, *region of growing up*, and the *child’s birth month* as zero and include dummies that indicate missing information. Table 12 excludes missing dummies.

6. *Descriptive statistics*

Assigned individuals are 197 of the 1213 mothers in the sample (see Figure 3). Table 12 displays summary statistics for outcome and control variables. Observed mothers were born between 1942 and 1971 and between 17 and 43 years old when they gave birth to the respective child. Most mothers are of German nationality (94%), temporarily live in East Germany (81%), have no high school diploma (84%), and have a mother without tertiary education (93%).

Also, most of the women were married before giving birth. Remarkably, a large share of mothers was not living with their husbands or partners in the year before the respective birth (23%). This condition might be due to a severe apartment shortage and apartment allocation in the former GDR, leading to the circumstance that many (also) married couples still lived at their parent's place when their first child was born.

Regarding the health outcome variables, the mean of maternal self-assessed health amounts to an average value of 3.43. The mean BMI is 26.48, indicating minor overweight. However, as birth cohorts as early as 1942 are included, a slightly higher BMI is still considered healthy. The average number of outpatient visits is, with a mean of 2.36 in the last three months, relatively low. However, there are severe outliers with a maximum of 34 outpatient visits. This indicates that there are few women with relatively severe health conditions in the data. The number of absent days from work due to illness amounts to an average of 15.73 days. The data includes women with long-term illness, reporting up to 365 days of absence.

Regarding mental health, on average, there is a tendency for being depressed rarely than being depressed frequently (mean of 3.4). Also, most mothers were not diagnosed with depression or another mental illness (mean of 0.17).

When examining descriptive statistics of the observed satisfaction measures, data shows that the mean values for life and work satisfaction as well as the satisfaction with household activities vary between 6.69 and 6.77, indicating medium to high satisfaction levels and low variation between the different measures. In contrast, income satisfaction is, on average, considerably lower and amounts to an average of 5.51. Also, the standard deviation of income satisfaction is considerably larger. This difference stems most likely from income disparities between mothers.

TABLE 12. DESCRIPTIVE STATISTICS FOR OUTCOME AND CONTROL VARIABLES

	<i>N</i>	<i>Mean</i>	<i>SD</i>	<i>Min</i>	<i>Max</i>
Health					
Overall/physical health					
<i>Self-assessed health</i>	1211	3.43	0.71	1	5
<i>BMI</i>	941	26.48	5.14	16.77	61.03
<i>Outpatient visits in last 3 months</i>	1213	2.36	2.38	0	34
<i>Absent days from work due to illness</i>	921	15.73	33.10	0	365
Mental health					
<i>Rarely depressed</i>	947	3.40	0.79	1	5
<i>Clinical depression</i>	834	0.17	0.38	0	1
Subjective well-being					
<i>Life satisfaction</i>	1203	6.69	1.47	0.91	10
<i>Work satisfaction</i>	1100	6.75	1.69	0	10
<i>Income satisfaction</i>	986	5.51	2.34	0	10
<i>Satisfaction with household activity</i>	900	6.77	1.50	0	10
Covariates					
<i>Year of birth</i>	1213	1960.16	4.38	1942	1971
<i>Age at birth</i>	1213	25.45	3.81	17	43
<i>German nationality</i>	1213	0.94	0.23	0	1
<i>Current region (1: West Germany)</i>	1213	0.19	0.39	0	1
<i>Education (1: high school)</i>	1213	0.16	0.36	0	1
<i>Region of growing up (1: urban)</i>	1213	0.64	0.48	0	1
<i>Academic mother</i>	1213	0.07	0.25	0	1
<i>Child gender (1: male)</i>	1213	0.50	0.50	0	1
<i>Birth month of child</i>	1213	5.36	3.91	0	12
<i>Relationship status (pre-birth)</i>	375	1.55	0.68	0	2
<i>Partner lives in household (pre-birth)</i>	374	0.77	0.42	0	1

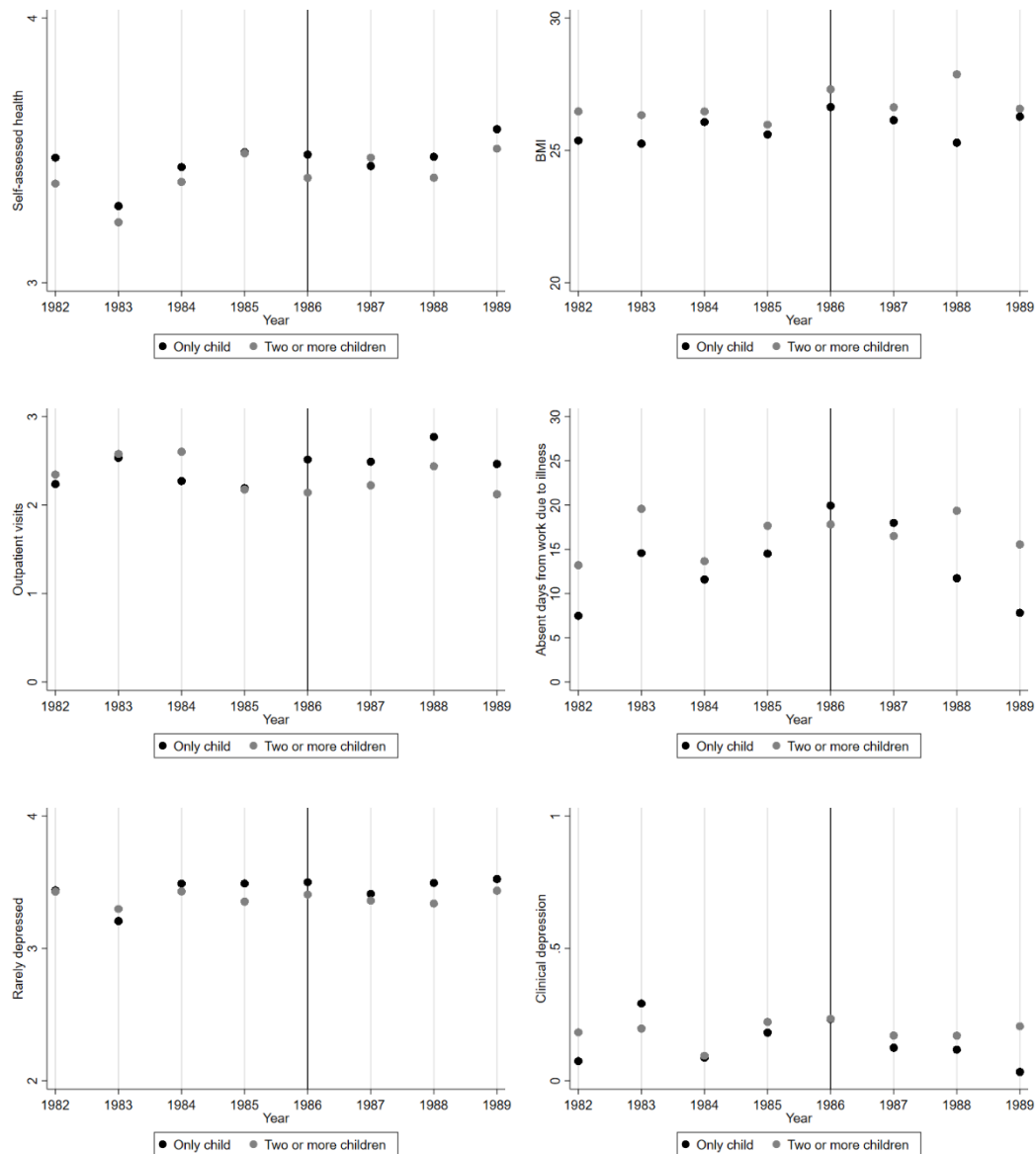
Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

Figure 4 shows the mean health outcomes of mothers of an only child and mothers with at least two children by the child's birth year. The upper left panel indicates a higher self-assessed health of mothers with an only child than that of mothers with two or more children. There is no visible long-term effect of the reform. The BMI of the assigned group is healthier than that of the control group, as depicted in the upper right panel. We also see no discontinuity for this variable in the year of the reform or thereafter.

Shown in the centered left panel, mothers with two or more children have more outpatient visits before the reform than mothers with an only child. After the

reform, the pattern reverses. However, there is no distinct jump in outpatient visits for mothers with an only child after the reform as well. For all other variables – the absent days from work, subjective feeling of depression and clinical depression – no differences in the means before and after the reform emerge. Overall, Figure 4 suggests no effects of the reform in 1986 on the long-term physical or mental health of mothers.

FIGURE 4. HEALTH AVERAGES OF MOTHERS PER CHILDBIRTH COHORT



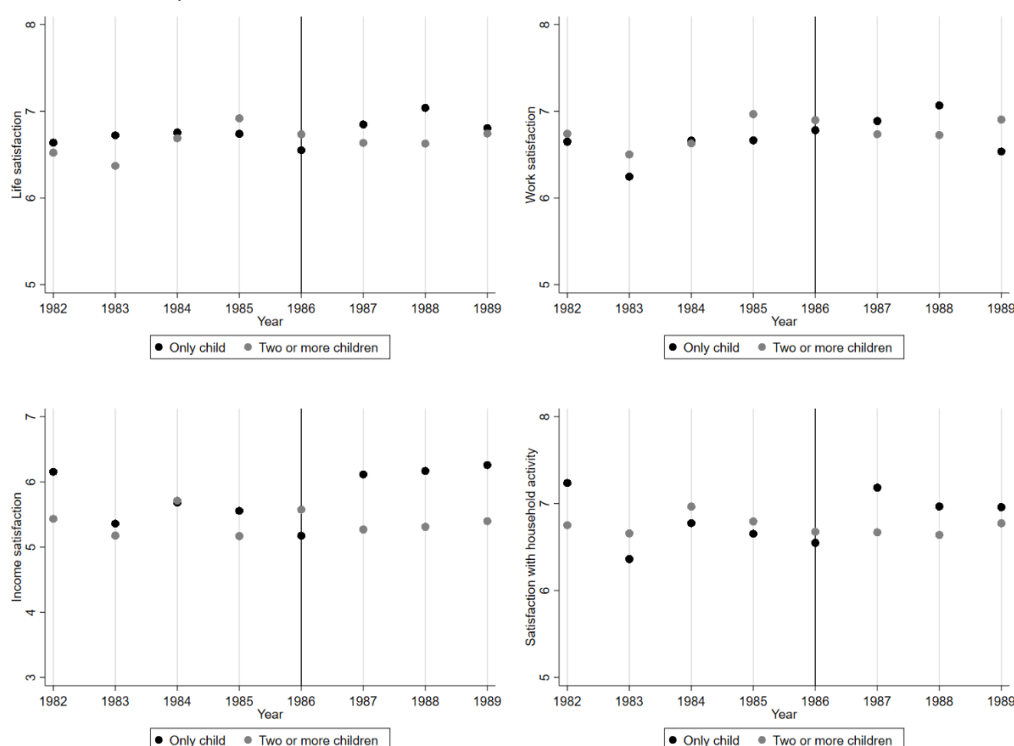
Notes: This figure depicts average values for health outcome variables. Assigned mothers to the reform are mothers with an only child born on January 1st, 1986, or later.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

Figure 5 displays maternal subjective well-being averages of mothers of an only child and mothers with at least two children by the child's birth year. For life satisfaction in the upper left and work satisfaction in the upper right panel, the means do not vary to a significant extent across groups and over time. Thus, the reform appears to have no impact. In the lower-left panel, pattern changes in income satisfaction of mothers with an only child after the reform, starting with 1987, are depicted. From then on, the income satisfaction of mothers with an only child is higher than the income satisfaction of mothers with two or more children.

For mothers with children born prior to the reform, there are no distinct differences, except for the gap in income satisfaction in 1982. A similar conclusion can be drawn from the lower right panel depicting household activity satisfaction averages. From 1987 on, household activity satisfaction means of mothers with an only child increased and exceeds the household activity satisfaction of mothers with two or more children. There is no change in the household activity satisfaction pattern of mothers with two or more children after the reform.

FIGURE 5. SUBJECTIVE WELL-BEING AVERAGES OF MOTHERS PER CHILDBIRTH COHORT



Notes: This figure depicts average values for outcome variables on subjective well-being. Assigned mothers to the reform are mothers with an only child born on January 1st, 1986, or later.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

7. Results

In a regression analysis based on a DiD approach, I test whether these differences are statistically significant. Table 13 shows estimation results for long-term maternal health, and Table 14 for subjective well-being outcomes. Panel A displays results without covariates. In Panel B, results of the main specification are shown. The model in this specification contains most covariates not affected by the reform (birth year, age at birth, German nationality, education, whether she currently lives in West Germany, grew up in an urban area, or her mother has tertiary education).

Panel C additionally includes child gender and Panel D the child's birth month as a new control variable, as they might impact the probability of taking paid leave or differences in maternal outcomes. In Panel E, I use a shorter time frame around the reform by applying 1983 and 1988 as cut-off years.⁵⁹ Doing so is important to rule out that other unobserved events, such as the fall of the Berlin Wall and the German reunification in 1989, affected pattern changes in maternal outcomes. The regression model is otherwise similar to Panel B.

Furthermore, I redefine treated mothers in Panels F and G. Firstly, in Panel F, mothers with children born in May 1986 and later are assigned to the reform. I conduct this robustness test because mothers with children born in early 1986 might already have had a place in childcare for their child after maternal leave ended. Many mothers might have decided not to extend parental leave or withdraw from a childcare place. Additionally, their employer might already expect her to return to work on an already decided date.

Secondly, in Panel G, mothers with children born in November 1985 or later are defined as assigned to the reform. This subgroup with children born between November and December of 1985 was likely still on maternity leave when the reform was announced.⁶⁰

⁵⁹ A shorter time frame is problematic due to a more significant decrease in sample size.

⁶⁰ Not shown in Tables 13 and 14 is a robustness test classifying mothers with children born in June 1985 or later as treated. However, those mothers were partially eligible for extended paid leave. Furthermore, mothers are unlikely to drop out of the labor market again since maternity leave already ended when policymakers announced the reform. Results do not change significantly, apart from a change in the algebraic sign for life satisfaction estimates.

In another specification, I consider that single mothers without a place in childcare for their child were eligible for one year of paid parental leave since 1972. Therefore, I exclude mothers who were single when the respective child was born. However, this subgroup is expectedly very small, as public childcare was comprehensive. Results are shown in Panel H. Lastly, I include relationship status variables in Panel I. In this specification, sample size decreases sharply. Thus, sample comparability is exacerbated, and estimation results are expected to be less reliable.

The estimation results imply the following. Firstly, Columns 1 to 4 of Table 13 display overall and physical health estimates. The estimated coefficients are, as Figure 4 already suggested, not statistically significant. For self-assessed health, estimated coefficients are very close to zero. Thus, no distinct effects of extended parental leave and overall or physical health outcomes are sizable in the setting of the former GDR. Column 2 suggests a by tendency decreased BMI of assigned mothers. However, results are statistically insignificant. Furthermore, estimates in Column 3 propose an increase in outpatient visits and estimates in Column 4 in absent days from work. None of the coefficients are statistically significant.

Lastly, Columns 5 and 6 show the effects of extended parental leave on long-term maternal mental health. Results hint on fewer subjective depressive symptoms of assigned mothers. Furthermore, cases of clinical depression or other mental illnesses decrease. However, the estimates are statistically insignificant.

Overall, the results from a DiD analysis imply that the effects of the reform extending paid parental leave from six to twelve months in former East Germany on assigned mothers' physical and mental health are statistically insignificant. There is no clear tendency for either positive or negative effects. However, most estimates on overall or physical health suggest a somewhat negative impact of the reform on the one hand. On the other hand, estimates suggest a positive effect on mental health. These findings corroborate descriptive statistics from Figure 4.

When Westfall-Young stepdown adjusted p-values are calculated, the p-values increase more considerably. These results align with previous studies analyzing effects of parental leave extensions from already generous pre-reform leave lengths on maternal outcomes (Baker and Milligan 2008a; Beuchert et al. 2016).

TABLE 13. ESTIMATION RESULTS FOR MATERNAL HEALTH OUTCOMES

	Overall/physical health			Mental health		
	(1)	(2)	(3)	(4)	(5)	(6)
	Health	BMI	Outpatient visits	Work ab- sent days	Rarely de- pressed	Clinical depression
A) Baseline	-0.00	-0.28	0.44	1.32	0.06	-0.05
<i>No covariates</i>	(0.09)	(0.69)	(0.29)	(4.28)	(0.11)	(0.06)
<i>N</i>	1211	941	1213	921	947	834
B) Main panel	-0.00	-0.26	0.38	0.56	0.04	-0.02
<i>With covariates</i>	(0.09)	(0.76)	(0.32)	(4.57)	(0.12)	(0.06)
<i>N</i>	1211	941	1213	921	947	834
C) With child gender	-0.00	-0.25	0.38	0.65	0.04	-0.02
	(0.09)	(0.76)	(0.32)	(4.58)	(0.12)	(0.06)
<i>N</i>	1211	941	1213	921	947	834
D) With month of childbirth	0.01	-0.27	0.40	0.61	0.04	-0.02
	(0.09)	(0.76)	(0.32)	(4.59)	(0.12)	(0.06)
<i>N</i>	1211	941	1213	921	947	834
E) Short time frame	0.05	-0.59	0.36	0.69	0.16	-0.03
	(0.11)	(0.96)	(0.39)	(5.82)	(0.15)	(0.08)
<i>N</i>	914	708	914	689	711	627
F) Treated: May 1986	-0.01	-0.28	0.31	0.29	-0.05	-0.01
	(0.09)	(0.76)	(0.32)	(4.54)	(0.12)	(0.06)
<i>N</i>	1211	941	1213	921	947	834
G) Treated: Nov. 1985	0.01	-0.23	0.24	-0.09	0.03	-0.04
	(0.09)	(0.76)	(0.34)	(4.64)	(0.12)	(0.06)
<i>N</i>	1177	939	1179	917	945	832
H) Without single mothers	-0.03	-0.40	0.28	0.61	-0.02	0.02
	(0.09)	(0.78)	(0.28)	(4.74)	(0.12)	(0.06)
<i>N</i>	1171	912	1173	889	918	801
I) With relati- onship status	-0.04	-0.74	0.41	-2.72	0.02	0.02
	(0.20)	(1.91)	(0.80)	(11.39)	(0.29)	(0.10)
<i>N</i>	374	282	374	315	285	346

Notes: This table displays DiD estimates from OLS regressions with maternal health outcome variables. Covariates include, if not stated otherwise: year of birth, age at birth, German nationality, education before giving birth, whether she lives in West Germany, whether she grew up in an urban area and whether her own mother is an academic. The sample consists of mothers giving birth between 1982 to 1989, if not stated otherwise. Panel C to I contain covariates of Panel B. Robust standard errors are reported in parentheses. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

Table 14 displays DiD regression results for measures of maternal subjective well-being. For all measures of satisfaction on varying dimensions, results of the estimations hint on an overall positive effect of the reform. However, the coefficients are mostly statistically insignificant. This was previously suggested as well in Figure 5.

Regarding work satisfaction in Column 2, the estimate for the specification with a shorter time frame around the reform is statistically significant on a 10%-level. However, the effect is not robust. Figure 5 proposes the effect can be ascribed to an increase in work satisfaction of the assigned group of mothers in 1988. Thus, it is rather unlikely an effect occurring by the reform.

Regarding income satisfaction shown in Column 3 and satisfaction with household activities in Column 4, a positive effect occurs for the specifications in Panel F (mothers with children born in May 1986 or later are assigned). The effect is statistically significant on a 10%-level.⁶¹ By conducting a multiple hypothesis test, adjusted p-values increase for all variables and statistically significant estimates become insignificant.

To conclude, estimation results suggest that extended paid parental leave from six to twelve months in former East Germany had negligible effects on long-term maternal subjective well-being. By tendency, results suggest somewhat positive effects, particularly regarding satisfaction with household activities, income satisfaction, and work satisfaction to a smaller extent. Estimated effects are statistically significant only in exceptional cases and not robust.

⁶¹ If I use this classification for all other variables and repeat robustness tests from Panel C to I (but without Panel F and G), results do not change for all other considered variables. Solely for satisfaction with household activities, the coefficient becomes statistically significant on a 5%-level in the specification that uses a shorter time frame around the reform.

TABLE 14. ESTIMATION RESULTS FOR MATERNAL SUBJECTIVE WELL-BEING OUTCOMES

	(1)	(2)	(3)	(4)
	<i>Life satisfac- tion</i>	<i>Work satisfac- tion</i>	<i>Income satis- faction</i>	<i>Satisfaction with household activity</i>
A) Baseline	0.06	0.17	0.20	0.25
<i>No covariates</i>	(0.19)	(0.23)	(0.32)	(0.22)
<i>N</i>	1203	1100	986	900
B) Main panel	0.16	0.25	0.29	0.24
<i>With covariates</i>	(0.19)	(0.24)	(0.33)	(0.23)
<i>N</i>	1203	1100	986	900
C) With child gender	0.16	0.25	0.29	0.25
	(0.19)	(0.24)	(0.33)	(0.23)
<i>N</i>	1203	1100	986	900
D) With month of child-birth	0.15	0.24	0.29	0.24
	(0.19)	(0.24)	(0.34)	(0.23)
<i>N</i>	1203	1100	986	900
E) Short time frame	0.31	0.49*	0.22	0.42
	(0.23)	(0.29)	(0.40)	(0.28)
<i>N</i>	907	829	738	670
F) Treated: May 1986	0.20	0.38	0.56*	0.43*
	(0.20)	(0.24)	(0.33)	(0.23)
<i>N</i>	1203	1200	986	900
G) Treated: Nov. 1985	0.06	0.20	0.28	0.27
	(0.19)	(0.25)	(0.34)	(0.24)
<i>N</i>	1169	1071	983	898
H) Without single mothers	0.15	0.21	0.24	0.21
	(0.19)	(0.25)	(0.34)	(0.24)
<i>N</i>	1164	1070	947	861
I) With relationship status	0.16	0.58	0.18	0.08
	(0.35)	(0.47)	(0.51)	(0.45)
<i>N</i>	364	296	362	343

Notes: This table displays DiD estimates from OLS regressions with maternal long-term subjective well-being after birth. Controls include, if not stated otherwise: year of birth, age at birth, German nationality, education before giving birth, whether she lives in West Germany, whether she grew up in an urban area and whether her own mother is an academic. The sample consists of mothers giving birth between 1982 to 1989, if not stated otherwise. Panel C to I contain control variables of Panel B. Robust standard errors are reported in parentheses. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: Socio-Economic Panel (SOEP), version 36, SOEP, 2020, doi:10.5684/soep.core.v36eu. Own calculations.

8. *Discussion and conclusion*

This study analyzed the long-term maternal health and subjective well-being effects of a paid parental leave reform in former East Germany. The reform was implemented in 1986 in the GDR and made first-time mothers eligible for a maximum of one year of paid parental leave with a 70 to 90%-wage replacement rate. The pre-reform scenario of first-time mothers was a maximum of six months of paid maternity leave with a 100% wage replacement rate. I compare first-time mothers with an only child to mothers of two or more children. Mothers of two or more children have been eligible for one year of paid parental leave since 1976.

By making use of this very precise reform setting, I apply a difference in differences approach as a causal estimation strategy. As a result, I am able to identify the causal effect of extending six months of paid parental leave to twelve months of paid parental leave on maternal outcomes. This study exploits all available waves of the SOEP panel survey to calculate various physical and mental health variables and subjective well-being measures. I make use of all available data points and calculate averages in maternal outcomes from two years post-birth up to 37 years post-birth, depending on the respective child's birth cohort.

Empirical results suggest negligible long-term effects of the reform for considered health and subjective well-being variables of mothers. Finding no physical or mental health effects corroborates previous studies examining the effects of paid parental leave extensions from already generous pre-reform leave lengths to an even more generous scheme (Baker and Milligan 2008a; Beuchert et al. 2016). On average, six months of maternity leave and twelve months of parental leave yielded similar benefits for mothers in the former GDR regarding health.

For observed subjective well-being variables, there are minor hints on positive effects in terms of satisfaction with household activity, income satisfaction, and work satisfaction. However, estimates are statistically significant solely in exceptional cases. Finding barely any sizable effects suggests that mothers in the former GDR were firmly attached to the labor market and their pre-birth jobs with paid work as an important source of well-being.

I confirm the results of Maeder (2014) regarding well-being outcomes in an East German context. She analyzes the effects of a German reform providing higher financial incentives to stay home for a more extended period after childbirth. On the one hand, she finds an increase in short-term school or job and family satisfaction

of West German mothers for the reform. On the other hand, for East German mothers, satisfaction with their social network decreased.

This paper adds to the literature by showing that extending parental leave from already generous leave lengths yields no additional benefit for long-term maternal health and subjective well-being – at least for the average mother. Finding no average benefit might, firstly, stem from the former GDR's specific situation – a high labor market attachment of women – or, secondly, the small sample size on which the study is built on.

However, Buetikofer et al. (2021) and Chuard (2023) conclude that low-resource mothers and mothers of girls or unhealthy babies might benefit from a more extended leave period regarding their health, even in terms of relatively generous leave lengths. This indicates that, although an average mother might not benefit from twelve instead of six months of paid leave, there are subgroups of mothers who might indeed benefit from generous parental leave.

9. References

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10. Appendix

TABLE A.13. STUDIES ON PARENTAL LEAVE REFORMS AND MATERNAL HEALTH

<i>Study</i>	<i>Country, year</i>	<i>Reform</i>	<i>Outcomes</i>	<i>Results</i>
<i>Physical and mental health</i>				
<i>Short term effects</i>				
<i>Albagli & Rau (2019)</i>	Chile, 2011	Extension of paid parental leave (PL) from 12 to 24 weeks.	Stress, probability of being employed, wages 7 months to 6 years after birth.	Mothers extended leave by 4 weeks. Decrease in stress, increase in the probability of being employed; no effect on wages.
<i>Baker & Milligan (2008a)</i>	Canada, 2000	Extension of paid maternity leave (ML) from 25 to 50 weeks. Extension of job-protected PL from 18-70 to 52 weeks.	Self-reported health, depression index, post-partum depression, other post-partum problems up to 2 years after giving birth.	No statistically significant effects.
<i>Beuchert et al. (2016)</i>	Denmark, 2002	Prior, 24 weeks of leave with a 100% compensation rate and 52 weeks with 60% wage replacement. After, 46 weeks with 100% wage replacement.	Hospital admissions, non-birth related hospital admissions, being hospitalized with depression, receiving antidepressants 1-5 years after delivery.	On average, mothers extended leave by 32 days. Weak evidence for fewer hospital admissions in the very short run. Larger effects for low-resource mothers.
<i>Chatterji & Markowitz (2005)</i>	U.S., 1993	Introduction of 12 weeks of unpaid ML.	CES-D depression scale 6-24 months after childbirth, number of outpatient visits in first 6 months after childbirth.	Decrease in the frequency of depressive symptoms, no increase in cases of clinical depression. Little evidence for effects on outpatient visits.
<i>Chatterji & Markowitz (2012)</i>	U.S., 1993	See Chatterji & Markowitz (2005).	CES-D depression symptoms, severe depression 6-24 months after childbirth, self-reported health, drinking and smoking behavior.	Decrease in depression symptoms and severe depression. Increase in self-reported health. Increase in depression symptoms if spouse did not take leave. Mixed results on drinking and smoking.
<i>Hewitt et al. (2017)</i>	Australia, 2011	Prior, 12 months unpaid PL. After, 18 weeks of paid PL (wage replacement at minimum wage rate).	Physical and mental health 12 months after delivery.	Small positive effects on mental and physical health.

TABLE A.13. (CONTINUED)

<i>Lee et al. (2020)</i>	California, 2004	Prior to the reform, 12 weeks of unpaid PL. After, up to 6 weeks of paid PL (55% of weekly earnings, no job protection).	Self-reported health, mental health, overweight, obesity, alcohol consumption.	Increase in self-rated health. Decrease in psychological distress, overweight and alcohol consumption.
(Short and) long term effects				
<i>Avendano et al. (2015)</i>	8 EU countries, 1960-94	Many different reforms were observed.	Depression symptoms (12-item version of the Euro-D scale) in 50-year-old (or older) mothers.	Decrease in depression symptoms.
<i>Buetikofer et al. (2021)</i>	Norway, 1977, 6 other reforms 1987-92	Before the reform, 12 weeks of unpaid ML. After the reform, 4 months of paid PL and 12 months of unpaid PL.	Various mental and physical health dimensions of around 40-year-old mothers.	Increase in various dimensions of metabolic health, pain, self-reported mental and overall health, increase in exercise and decrease in smoking. Larger effects for first-time and low-resource mothers. Diminishing returns to leave lengths.
<i>Chuard (2023)</i>	Austria, 1990, 1996, 2000	Extension of parental leave from 1 to 2 years (1990), partial decrease by six months to 1.5 years (1996), and an increase to 2.5 years (2000).	Outpatient costs, medication, days of hospitalization, mental disorders, depression, antidepressant, nervous system drugs and analgetics prescriptions, diseases of circulatory system, cardiovascular drugs up to 17 years postpartum.	Hump-shaped relationship between maternal health and parental leave duration: Increase in health for short leave lengths. Decrease in mental health for very long leave lengths. Longer leave spells are more beneficial for low-SES mothers, mothers of girls, and mothers with unhealthy babies.
<i>Guertzen and Hank (2018)</i>	Germany, 1979	Extension of job protected paid PL from 2 to 6 months.	Return-to-work behavior, sickness absence from work and length of illness (up to 30 years after delivery).	Delay in return-to-work behavior in the first year after delivery. Longer sickness absence from work 3-10 years after delivery.

Sources: Chuard (2023), Buetikofer et al. (2021), Lee et al. (2020), Albagli and Rau (2019), Guertzen and Hank (2018), Hewitt et al. (2017), Beuchert et al. (2016), Avendano et al. (2015), Baker and Milligan (2008a), Chatterji and Markowitz (2005, 2012).

TABLE A.14. STUDIES ON PARENTAL LEAVE REFORMS AND SUBJECTIVE WELL-BEING

<i>Study</i>	<i>Country, year</i>	<i>Reform</i>	<i>Outcomes</i>	<i>Results</i>
<i>D'Addio et al. (2014)</i>	Germany (8 reforms 1984-2008) and GB (6 reforms, 1973-2007)	Many different reforms were observed.	Life satisfaction and subjective well-being shortly before and up to 3 years after giving birth.	Increase in life satisfaction up to 6 months after delivery. Decrease in life satisfaction for leave lengths longer than 16 months. Smaller effects for Great Britain.
<i>Maeder (2014)</i>	Germany, 2007	Prior to the reform, 300€ for 24 months or 450€ for 12 months per child. After the reform, paid PL of 12 or 14 months (67% wage replacement rate).	Overall life satisfaction, satisfaction with school, training, job, satisfaction with friends and social network, satisfaction with family.	No overall effects. Increase in life, school/job and family satisfaction if partner's education is medium or high. Increase in school/job and family satisfaction for West German mothers. Decrease in satisfaction with social network for East German mothers.
<i>Pezzini (2005)</i>	12 EU countries, 1975-98	Many different reforms were observed.	Life satisfaction of mothers.	Negligible effects.

Sources: D'Addio et al. (2014), Maeder (2014), Pezzini (2005).