

Huebener, Mathias; Kuehnle, Daniel; Spiess, C. Katharina

Article — Accepted Manuscript (Postprint)

Parental leave policies and socio-economic gaps in child development: Evidence from a substantial benefit reform using administrative data

Labour Economics

Provided in Cooperation with:

German Institute for Economic Research (DIW Berlin)

Suggested Citation: Huebener, Mathias; Kuehnle, Daniel; Spiess, C. Katharina (2019) : Parental leave policies and socio-economic gaps in child development: Evidence from a substantial benefit reform using administrative data, Labour Economics, ISSN 0927-5371, Elsevier, Amsterdam, Vol. 61, <https://doi.org/10.1016/j.labeco.2019.101754>

This Version is available at:

<https://hdl.handle.net/10419/243172>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



<https://creativecommons.org/licenses/by-nc-nd/4.0/>

Parental leave policies and socio-economic gaps in child development: Evidence from a substantial benefit reform using administrative data

Mathias Huebener[†]

DIW Berlin, IZA Bonn & Freie Universität Berlin

Daniel Kuehnle

Friedrich-Alexander University of Erlangen-Nuremberg & IZA Bonn

C. Katharina Spiess

DIW Berlin & Freie Universität Berlin

Abstract

This paper examines the effects of substantial changes in paid parental leave on child development and socio-economic development gaps. We exploit a German reform that affected all children born on or after January 1st 2007 and that both expanded paid leave in the first year and removed paid leave in the second year after childbirth. Given that the reform replaced a means-tested with an earnings-related benefit scheme, higher-income households benefited relatively more from the reform than low-income households. We compare children born around the eligibility cut-off and contrast them with children from the preceding, unaffected, cohort within a difference-in-differences approach. The analysis is based on administrative data from mandatory school entrance examinations containing detailed child development assessments at age six. Our precise and robust estimates reveal no effects of the changes in parental leave benefits on child development across various socio-economic groups, and consequently no effects on socio-economic development gaps,

[†]Corresponding author. E-mail: mhuebener@diw.de; Postal address: DIW Berlin, Mohrenstraße 58, 10117 Berlin; Phone: +49 30-89789-373.

Acknowledgements: We thank the editor and two anonymous referees for very helpful comments and suggestions. We further thank Wiji Arulampalam, Stefan Bauernschuster, Pedro Carneiro, Claire Crawford, Kamila Cygan-Rehm, Victor Lavy, Michael Oberfichtner, Regina T. Riphahn, Pia Schober, Felix Weinhardt, Vaishali Zambre, and participants of the European Association of Labour Economists 2017 Conference in St. Gallen, the Society of Labor Economists Meeting 2017, the Royal Economic Society Conference 2017, the “Early Childhood Inequality Workshop” in Nuremberg, the workshop on “Child Development: The Roles of the Family and Public Policies” in Vejle, the 2016 meeting of the “Ausschuss für Sozialpolitik” in Mannheim, and a number of seminar participants for valuable comments and suggestions. Special thanks go to the Ministry of Social Affairs, Health, Family and Equal Opportunities in Schleswig-Holstein, in particular to Sabine Brehm and Prof. Dr. Ute Thyen for providing data access and significant support. Mathias Huebener acknowledges financial support by the German National Academic Foundation. Daniel Kuehnle acknowledges financial support by the German Science Foundation (DFG SPP 1764).

despite substantial reform effects on maternal employment and family income.

Keywords: Parental leave benefits, child development, school readiness, motor skills, language skills, socio-emotional stability

JEL: J13, J18, J22, J24

1. Introduction

Early childhood conditions can have long-lasting effects on children's educational attainment, labour market outcomes, and adult health (e.g. Cunha et al., 2006; Almond and Currie, 2011; Heckman and Mosso, 2014). However, these early conditions differ considerably by children's socio-economic status (SES), contributing to the emergence of SES gaps in child development very early in life: Bradbury et al. (2015), for instance, show that SES gaps in child development are already pronounced at age 5 and increase further throughout the first years of schooling. Consequently, many children from low-SES backgrounds fall behind.¹

One of the most important policy tools across OECD countries to support families around childbirth are parental leave policies. A substantial literature shows that these policies affect the current work force, in particular maternal labour supply (see below). However, less research examines the effects of such policies on the future workforce, namely children – although such policies affect several conditions in early childhood (e.g. Björklund and Salvanes, 2011): Overall, expansions in parental leave policies reduce maternal labour supply after childbirth (e.g. Ondrich et al., 1996; Lalive and Zweimüller, 2009; Schönberg and Ludsteck, 2014) and thus affect the time parents can spend with their children. Parental leave benefits also directly impact household income, which determines the resources parents can invest into the development of their children (e.g. Dahl and Lochner, 2012; Løken et al., 2012). These changes in parental resources early in a child's life may affect children's development in the short-, medium- and long-run.² However, we know little about the effects of such policies on early child development and even less about the effects on SES development gaps.³

A few studies examine introductions of parental leave (see Rossin, 2011; Stearns, 2015; Carneiro

¹Other examples documenting considerable differences in children's skills at school entry include Feinstein (2003), Cunha and Heckman (2007) and Todd and Wolpin (2007).

²The various channels through which parental leave policies may impact child development are carefully described in, e.g., Dustmann and Schönberg (2011) and Danzer and Lavy (2016). Numerous studies show that changes in early childhood conditions and family resources can affect children's short- to long-run outcomes (e.g. Elango et al., 2016).

³The small economic literature is summarised in Appendix Figure A.1. For previous detailed descriptions of the literature on parental leave policies and child development, see, e.g. Danzer and Lavy (2016), Huebener (2016) and Huebener et al. (2017).

et al., 2015), finding some positive effects on infant health, schooling and later labour market outcomes. Studies on parental leave expansions within the first year after childbirth mostly find no effects on children's outcomes (Dustmann and Schönberg, 2011; Würtz Rasmussen, 2010; Dahl et al., 2016; Beuchert et al., 2016; Baker and Milligan, 2008, 2010, 2015). Studies on parental leave expansions in the second year after childbirth show some effects on long-run child development (Liu and Skans, 2010; Dustmann and Schönberg, 2011; Danzer and Lavy, 2016). These mixed findings may suggest that the timing of parental leave policies matters, but an insufficient magnitude of the expansions may also explain some of the results. Furthermore, these studies focus on long-run child outcomes, so it is not clear whether initial reform effects faded out over time. In addition, most of these reforms took effect in the 1970s to 1990s. Since then, many factors related to child development have changed substantially across countries, such as maternal labour force participation, day care availability, and social norms. Only few studies examine reforms taking place after 2000 with a focus on short- and medium-run effects of parental leave reforms (e.g., Baker and Milligan, 2008, 2010, 2015; Beuchert et al., 2016; Lichtman-Sadot and Bell, 2017; Huber, 2019; Pihl and Basso, 2019). Most of these studies rely on outcomes with restrictive information on the cognitive and non-cognitive development of children (e.g. birth weight, infant mortality, premature birth, hospitalisations). Studies examining richer child development measures often use parent-reported information or rely on small sample sizes that require more restrictive assumptions for the identification of causal effects.⁴ Furthermore, previous contributions pay little attention to the mechanisms underlying the effects and whether parental leave policies impact SES development gaps.

Our paper addresses these questions by examining a German reform that completely changed the

⁴The only other studies examining parental leave effects on richer early child development outcomes evaluate a Canadian parental leave expansion from 2001 that increased paid leave from 6 to 12 months. Baker and Milligan (2008, 2010, 2015) mostly find no effects of the reform on health and development outcomes up to age 3, or on measures of children's cognitive and non-cognitive development at ages 4 through 5. While the outcome measures are rich, they estimate causal reform effects through cohort comparisons in an eight-year window around the reform. This approach may be more sensitive to other confounding effects (such as cohort and age-at-test effects) than approaches that compare child outcomes in the close neighbourhood of reform eligibility cut-offs or those that control for underlying trends (e.g., for Canada, see Haeck, 2015).

eligibility criteria and benefit payments: For children born before January 1, 2007, parental leave benefits were means-tested and paid for up to two years after childbirth. After the reform, parental leave benefits were changed to earnings-related benefits and paid for up to 14 months in total per couple. The reform expanded the proportion of mothers eligible for up to 12 months of paid parental leave from 47% to almost 100%. The additional public benefit payments of the programme were fiscally substantial with about 0.1% of GDP in the first year after its implementation.⁵ Gaining eligibility for parental leave benefits correlates positively with parental income and, consequently, with parental education. While the reform increased the average net disposable household income in the first year after childbirth by about 20%, mothers with a university degree received about 40% more than mothers without a university degree (Wrohlich et al., 2012). Although the reform was implemented in a setting with relatively low maternal labour force participation after childbirth, the reform still caused the labour supply of mothers to decrease substantially in the first year after birth (see, e.g., Kluve and Schmitz, 2018, and our own analysis below). Moreover, other studies suggest small effects on fertility (Cygan-Rehm, 2016; Raute, 2019) and breastfeeding duration (Kottwitz et al., 2016).⁶ At the time, critics worried that the reform would widen substantial pre-existing SES gaps in child development (e.g. Henninger et al., 2008), an important concern for early inequalities in the future work force.

Our study makes the following major contributions to the literature. First, we shed new light on whether substantial changes in paid parental leave affect SES development gaps in the medium-run. Our setting is very distinct from the vast majority of the previous literature on parental leave evaluations in terms of the magnitude and directions of policy changes (see Appendix Figure A.1). Whereas the previous literature on child development (apart from Huber, 2019) exclusively studied introductions or expansions of parental leave schemes, the reform we examine both expanded eligibility for paid leave in the first year after childbirth and removed eligibility for paid leave in

⁵Own calculations based on Federal Ministry of Finance (2007), German Federal Statistical Office (2008a), and German Federal Statistical Office (2016).

⁶Huebener et al. (2016) summarise the literature on the 2007 German paid parental leave reform on various outcomes.

the second year after childbirth. Moreover, the reform we analyse changed means-tested benefits to earnings-related benefits such that high-SES households (compared to low-SES households) gained financial and time resources in the first year after childbirth that could be invested in children's development. As the changes in terms of benefits and leave duration correlate strongly with families' SES, the German reform we analyse allows us to study how changes in paid parental leave policies affect SES gaps in child development. For our empirical analysis, we use a difference-in-differences approach and compare children born before and after the 2007 reform cut-off date to children born around the same cut-off date in the previous year as our control group.

Only Huber (2019) has so far studied the effects of this reform on the development of children, in particular newborns and toddlers. Her paper uses survey data with potentially biased parent-reported information on child development measures. This study is based on a small sample size (e.g., 91 treated children born between January and June 2007) and fails to reject even large point estimates. It uses several pre- and post-treatment cohorts for the control group and thus relies on modelling the longer-term trends for this small sample correctly. In contrast, our analysis focuses on pre-schoolers that were assessed by external paediatricians on several important dimensions of human capital. Our much larger sample size allows us to perform differentiated heterogeneity analyses and to rule-out even small effects on child development. In addition, our study focuses much more on the potential mechanisms behind the zero-effect findings.

Second, we contribute novel evidence of parental leave policy effects on medium-run outcomes of children using administrative data from compulsory school entrance examinations at age six. The data covers the full population of children from one German state. The data allows us to examine several important dimensions of child development that are shown to be highly predictive of later educational attainment (e.g. Duncan et al., 2007; Grissmer et al., 2010), later health outcomes and labour market performance in other settings (e.g. Cunha et al., 2006; Blanden et al., 2007; Carneiro et al., 2007). By analysing medium-run outcomes, we provide complementary evidence to previous studies on children's long-run outcomes, which mostly find no or small effects, to address the question whether parental leave policies do not have any effects on children at all, or

whether initial effects may fade out over time.

Our results show that the substantial changes in paid parental leave had no impact on children's language skills, motor skills, socio-emotional stability, and school readiness at age six. The point estimates from our large sample are close to zero and precisely estimated. Since the differential and potentially opposing effects for families who gained or lost eligibility may offset each other, we stratify the sample by parents' likely previous eligibility status for paid leave. We estimate again very small and insignificant treatment effects on child development. The same picture emerges when we stratify the sample by parental education, an important and widely used dimension to assess SES differences in child development (see, e.g. Bradbury et al., 2015). Consequently, we find no evidence for changes in the SES development gaps despite the strong and heterogeneous effects the reform had on maternal employment and family income. Thus, the reform effects on the current work force are not sacrificed by an increase in SES gaps of the future workforce – at least as based on child development measures at age 6. As likely explanations for the zero-effects, we explore several potential factors and show that changes in transfers are transitory, that the share of non-working mothers in the first year after birth was already high before the reform, and that mothers mostly adjusted their employment at the part-time margin – which the previous literature shows to only have a small impact on child development.

2. Institutional details

To set the stage for our analysis, we first provide some information on the institutional background in which the parental leave reform was implemented (based on OECD, 2016a,c,b). In Germany in 2006, the maternal labour force participation rate of women aged 25-54 with at least one child aged 0-14 was 63% (cf. OECD average 66.1%), the fertility rate was 1.33 children per woman (cf. OECD average 1.69), and the day care attendance rate for 0-2 year olds, including centre-based and family day care services, was 13.6% (cf. OECD average 30%). Mothers in Germany are generally

not allowed to work during the six weeks before and the eight weeks after childbirth.⁷ Employed mothers receive a full wage replacement during this mother protection period. Parents who use parental leave are eligible for a maximum job protection period of 36 months during which work positions must be held for the parents on leave.

Parents of children born before January 1, 2007, were eligible for child-rearing benefits. These publicly-funded benefits were means-tested and families were eligible if their yearly net income was below a certain threshold, which varied with the household structure, number of children, and time since giving birth. Once the net income exceeded the threshold, benefits were reduced or withdrawn (see, e.g., Ehlert, 2008, for details). Column 1 of Table 1 shows that 77% of parents were eligible for 300 Euros of monthly benefits (about 11% of average pre-birth net household income) for up to six months after childbirth (based on representative household data from the German Socio-Economic Panel (SOEP), see Wagner et al., 2007). Due to repeated means-testing and lower household income thresholds for eligibility, the share of eligible parents fell to 47% for 7 to 12 months after childbirth and to 40% for benefits 12 to 24 months after childbirth. Part-time work of up to 30 hours per week was permitted during the benefit payment period.⁸

In 2006, the German government reformed the paid parental leave regulations (*Bundeseltern- und Elternzeitgesetz*), seeking to (Bujard, 2013): safeguard family income during the first year after childbirth and increase parental care time during that period; enhance mothers' economic independence by incentivising an earlier return to work after the first year; and expand paternal involvement in child rearing. The reform did not explicitly target child development.

The reform was passed in September 2006 and affected parents of children born on or after January 1, 2007. Instead of being means-tested, the new benefit provides near-universal coverage (German Federal Statistical Office, 2008a). The benefits equal 67% of the parent's average net labour income

⁷Mothers can work in the six weeks prior to the expected childbirth if they provide their explicit consent (see *Mutterschutzgesetz*, *MuSchG*, paragraph 3(1)); mothers can withdraw their consent at any time during this period. After childbirth, there are no exceptions (see *MuSchG*, paragraph 3(2)). Any violations can be enforced legally.

⁸Parents eligible for benefits for up to 24 months could also choose higher benefits (450 Euros) for up to 12 months. For children born in 2005 and 2006, 10% of all parents chose this option (own calculations based on SOEPv30).

earned in the 12 months prior to giving birth, but not more than 1,800 Euros per month. Individuals who did not work prior to giving birth, or those with low earnings, continue to receive 300 Euros per month. On average, mothers receive 634 Euros benefits per month (see Table 1). An additional change was a reduction in the maximum transfer period from 24 to 12 months. Two additional months were granted for single parents or if both partners take parental leave for at least two months.⁹ Alternatively, parents can also choose to receive only half of the monthly benefits for a doubled period of time, but only 8% of parents chose this option (German Federal Statistical Office, 2008a). The additional public expenditures of the programme amount to about 3,500 Euros per child. The reform did not change the 36-months job protection period, the mother protection period, or part-time employment regulations during the benefit payment period.¹⁰

Overall, families were affected differently by the reform, depending on parents' pre-birth earnings and household income: Families that were *previously ineligible* for paid parental leave (or eligible for only 6 months), i.e. higher- income households, gained new eligibility for up to 10 months of paid parental leave (following two months of mandatory mother protection period, see Table 2).¹¹ Families that were *previously eligible* for two years of paid parental leave, i.e. lower-income households, still receive the minimum benefits of 300 Euros per month for the first twelve months. However, in the second year after childbirth, they lose eligibility for benefits (up to 3,600 Euros). Households that were previously eligible for benefits of 300 Euros per month for up to 24 months, but now receive higher benefit payments only during the first year after childbirth, are in between the two groups.

As the pre-reform eligibility status is based on household income, which strongly correlates with

⁹The maximum length of 14 months of paid parental leave could be split flexibly between both parents, with a minimum of two months per parent. Approximately 96% of parents assign the main benefit period (>7 months) to the mother. In our observation period, 13% of fathers take paid parental leave, mostly for 2 months, with average benefits of 1061 Euros (see Table 1).

¹⁰After the reform, parents who work part-time receive a benefit that amounts to 67% of the difference between pre- and post-birth earnings.

¹¹If both parents take paid parental leave, the maximum paid leave period is 14 months. In our observation period close to the introduction of the reform, the share of fathers taking parental leave is still relatively low. Thus, we abstract from this detail to ease the discussion.

parental education and other socio-economic characteristics (see Section 3.2), children from high-SES families benefited more from the reform than low-SES families in terms of eligibility and benefit payments. To illustrate this, we summarise the benefit payments and durations by mothers' school degree (columns 2 and 3 of Table 1).¹² Highly educated mothers (i.e. with upper-secondary school certificates) were less likely than low- and medium-educated mothers (i.e. with lower- and middle-secondary school certificates) to receive parental leave benefits before the reform. While only 40% of the highly educated mothers received parental leave benefits for more than six months, 53% of low- and medium-educated mothers did. For the second year after childbirth, only 33% of highly educated mothers and 45% of low- and medium-educated mothers received benefits. After the reform, highly educated mothers receive, on average, 771 Euros per months, while lower educated mothers receive, on average, 563 Euros per month. 4.8% of highly educated mothers reach the benefit cap of 1,800 Euros, while only 0.5% of low educated and 1% of medium educated mothers do (own calculations based on German Microcensus 2008). In addition, twice as many fathers take (higher-paid) parental leave among the group of highly educated mothers, which further increases the total benefit duration by up to two months.

To account for these heterogeneous changes, our analysis first distinguishes by mothers' highest educational attainment (as a typical measure of SES) to identify the reform effect on SES gaps in child development. Second, to ensure that we do not miss heterogeneous effects, we also distinguish by families' predicted pre-reform eligibility for parental benefits and reach the same conclusions.

¹²Classifying individuals' education level by their school degrees is typical in Germany and will distinguish between groups with very different earnings potentials. Unlike in the US, the German educational system tracks students into separate schools depending on their academic potential. In general, this tracking system only allows individuals who have graduated from high-ability school tracks to study at university. Consequently, graduates from low and medium school tracks have never attended university, while about 50% of graduates from high-ability school tracks have completed university (based on supplemental data from the German Microcensus 2008).

3. Data

3.1. School entrance examinations

We use administrative data from school entrance examinations covering the full population of one German federal state, Schleswig-Holstein.¹³ Before entering primary school at the age of six, all over Germany every child is medically screened by a public health paediatrician. The paediatrician examines children's development in numerous dimensions. Taking into account the results from several tests, the paediatrician ultimately provides an assessment of the child's school readiness.

The administrative records we use in our main analysis cover all children from two cohorts entering school in 2012 or 2013. A school entrance cohort includes children born between July of the previous year and June of the year of school entry. The school entrance examinations are conducted in the six months before school entry. The data includes detailed information about children's health and development, children's year and month of birth¹⁴, and some information about family characteristics, such as parental schooling, migration background, and family structure. This family-related information is reported voluntarily by the accompanying parent (typically the mother). The data does not contain information about parental employment or income.

In our analysis, we focus on four dimensions of child development: children's language skills, motor skills, socio-emotional stability, and an overall assessment of their school readiness. Paediatricians examine children's language development with respect to their ability to use prepositions, build plural words, and repeat pseudo-words. Children receive a score that determines whether or not their language development lags. To assess motor skill development, paediatricians count chil-

¹³Schleswig-Holstein covers 3.6% of the German population. We examine Schleswig-Holstein due to restricted data access in the other federal states. To assess the external validity of our analysis, Appendix Table B.12 compares the demographic and socio-economic characteristics of the population of Schleswig-Holstein to the population in other federal states in West Germany. Schleswig-Holstein is generally close to other West German averages, apart from migration background and the degree of urbanisation. The share of children in day care at age 3-6 is lower than in the rest of West Germany. With a similar level of female labour force participation, this suggests that informal care by relatives may play a larger role.

¹⁴For data protection reasons the data lacks information on the day of birth. Our main analysis is therefore based on children's month of birth.

dren’s jumps on one leg over a line within 10 seconds and measure how long they can stand on one leg. If they do not achieve specific thresholds, they are classified as having motor skill deficiencies. Socio-emotional development is clinically assessed by the paediatrician: children are classified as having socio-emotional problems if they receive medical or psychological treatment, or if the paediatrician diagnoses that further treatment is necessary.¹⁵ In the data, we observe the paediatrician’s assessment of children’s developmental deficiencies in their language skills, motor skills, and socio-emotional stability as binary indicators. We reverse the scales such that higher outcomes are associated with better skills. Some counties also report the specific test results of children on which the paediatricians base their binary assessments. We also exploit this information in our analysis and draw the same conclusions.

Children’s overall school readiness is assessed by the paediatrician taking into account the examination results and other (to the researcher) unobserved factors related to children’s development. It is recorded in the data as a binary variable. A negative school readiness assessment does not defer children’s school entry, but indicates a child’s need for additional support. Delayed school entries are granted only exceptionally based on adverse health conditions of the child.¹⁶ However, a lack of school readiness may prolong primary school for children by one year, which would defer children’s labour market entry and reduce their life-time earnings.¹⁷

¹⁵In some counties, paediatricians base their assessment additionally on information from the Strength and Difficulties Questionnaire (Goodman et al., 1998, SDQ). Our econometric framework accounts for differences between counties regarding the additional usage of the SDQ through county-examination-year fixed effects.

¹⁶For the 2013 school entry cohort, about 1% of children were delayed. We tested whether the reform affected children’s age at examination, an indicator for early or delayed school entry, and found a very small negative (0.068 months, sample mean 72.6 months) and statistically insignificant effect (available upon request).

¹⁷These outcomes have been analysed in other contexts. For instance, Felfe et al. (2019) examine the effect of granting citizenship on the development of immigrant children. They find significant positive effects on language skills (+6%), on socio-emotional maturity (+2.1%), and on school readiness (+2.5%, though not significant). Examining the effect of full- versus part-time child care, Felfe and Zierow (2018) find that increasing the full-day share by 20 percentage points increases the prevalence of socio-emotional problems by around 6%. Finally, Felfe and Lalive (2018) examine the effect of starting child care earlier on children’s language skills, motor skills, and socio-emotional maturity, and report heterogeneous treatment effects.

3.2. Descriptive statistics and sample stratifications

Our sample consists of 28,987 children with complete information on the four domains of child development.¹⁸ Descriptive statistics of the full sample and the subsamples stratified by maternal education levels are provided in Table 4. Panel A describes children’s outcomes in the examinations. Overall, 72.7% of children reach a sufficient level of language competencies, 80.8% are considered stable in their socio-emotional development, 82.6% show a sufficient level of motor skills development, and 84.4% of children are considered ready for school. Stratifying the sample by maternal education reveals considerable and statistically significant SES development gaps between children (columns 2 and 3). Panels B and C of Table 4 summarise information on child and family characteristics. Note that maternal education information is missing for 18% of children; this should not be a problem for our analysis as missing information is not related to the reform (see Section 4).

In Appendix Table A.2, we present OLS estimates from multivariate regressions showing that these child and family characteristics strongly correlate with the child development outcomes. The child’s age, birth weight, time spent in day care, and parental years of schooling all correlate positively with skill development. Across all measures, girls are better developed. Children with more siblings and those who do not live with both parents show lower development levels. Children’s migration background and foreign languages spoken at home correlate negatively with children’s language skills and their school readiness. These observed relationships are common in the literature (for reviews, see, e.g. Bradley and Corwyn, 2002; Maggi et al., 2010) and validate the relevance of the analysed dimensions of child development.

To examine potentially heterogeneous effects by parental SES, we stratify the sample in two ways: by parental education and by pre-reform eligibility because the distinction by parental education alone may not entirely capture the different changes the reform had on lower and higher income

¹⁸Missing information is unrelated to the 2007 German parental leave reform. We account for different sample compositions of counties across school entry cohorts with county-examination-year fixed effects.

families. Specifically, lower-income households were previously eligible for paid parental leave and lost eligibility beyond the first year due to the reform (see Section 2). In contrast, higher-income households were previously mostly ineligible and gained access to paid parental leave in the first year after childbirth. While we do not observe pre-reform eligibility in the administrative data, it contains important socio-economic characteristics to predict pre-reform eligibility.

For the prediction, we use a sample of children born in 2005 and 2006 from the SOEP and generate variables on the same family characteristics as we observe them in the administrative data set. Based on these characteristics, we use a logit model to predict the pre-reform eligibility for benefits for 13-24 months to identify the group of *previously eligible* parents. To identify the group of *previously ineligible* parents, we predict the pre-reform eligibility for benefits for 6-12 months.¹⁹ We then take the estimated coefficients from the SOEP to predict pre-reform eligibilities in our administrative data set. Appendix Figure A.2 plots the predicted probabilities for the SOEP (Panel A), and compares the predicted probabilities from both data sets (Panel B). Reassuringly, the predicted probabilities in the original SOEP sample and the administrative data set match closely suggesting that the characteristics are similarly distributed in both samples. In our administrative data, we classify parents as *previously eligible* if their predicted probability for pre-reform benefits for up to 24 months lies above 0.75. Furthermore, we classify parents as *previously ineligible* if their predicted probability for pre-reform benefits for 6-12 months lies below 0.25. While focusing on predicted probabilities above 0.75 and below 0.25 sacrifices about 43% of observations, the model predicts the correct eligibility status for about 80% of observations in the SOEP for these groups.

In Appendix Table A.3, we show that maternal education strongly correlates with pre-reform ineligibility: In a baseline regression of the generated indicator variable for pre-reform ineligibility, i.e. belonging to the group of reform winners, children with highly educated mothers have a 37

¹⁹The regressors in the prediction include dummies for both maternal and paternal education, their interaction, plus a dummy for single parents, the number of children in the family, a dummy for migration background, and an interaction term of mothers' education and the number of children. The sign of the coefficient estimates are consistent with the institutional rules: for instance, the probability of eligibility increases with the number of children and single motherhood, while it decreases with the education level of the parents (Online Appendix Table B.8).

percentage points higher probability of belonging to “winner-families” of the reform (column 1). Reassuringly, this relationship is the same if county-examination-year fixed effects and children’s gender (columns 2-3) are sequentially added in the regression. Including further family and child characteristics (column 4) reveals that belonging to the winners of the reform correlates strongly with paternal education, the migration background, the number of children in the household, and the family structure. As the coefficient on maternal education decreases as we include further control variables, the results confirm that maternal education captures a significant part of children’s SES.

4. Empirical strategy

To estimate the reform effects on children, the reform appears suitable for a regression discontinuity design (RDD): As only parents of children born on or after January 1 are eligible for the new parental leave benefit, we observe a sharp discontinuity in eligibility around January 1st 2007.

The RDD assumes that children (and parents) close to the cut-off are identical in their potential outcomes (*local continuity assumption*, see e.g. Van der Klaauw, 2008), i.e. the observable characteristics of parents and children on both sides of the cut-off should be balanced for a valid RDD. To test whether observable characteristics are balanced, we use children born within six months around the cut-off and estimate discontinuities in their characteristics by fitting linear functions on either side of the cut-off. Overall, we find no strong evidence for imbalances as we reject the null only in the samples with lower educated mothers when testing the joint orthogonality of observable characteristics (see Appendix Table A.1).²⁰

Any sorting around the cut-off related to potential outcomes could violate the local continuity assumption. For example, parents may manipulate children’s birth dates near the reform cut-off by

²⁰We observe a discontinuity in daycare use for the RD sample including January and December births, which may itself be an effect of the parental leave reform. However, we observe a very similar discontinuity in the preceding (placebo) cohort, suggesting that the daycare admission rules likely depend on children’s birth years and affect these cohorts similarly. For the difference-in-differences sample, we do not observe a statistically significant or economically meaningful effect on potentially ‘bad controls’, such as years in day care or age-at-examination.

postponing caesarean sections and labour inductions to benefit from the new regulation. Indeed, Neugart and Ohlsson (2013), Tamm (2013) and Jürges (2017) find that a significant number of births were shifted from the last week of December to the first week of January in a manner consistent with the economic incentives of the reform: Previously ineligible parents are more likely to give birth in January. We perform a density test for manipulations at the threshold in our data as proposed by McCrary (2008) using local polynomial density estimation based on Cattaneo et al. (2018). In Figure 1, we plot the density across children’s birthdays. The distribution of the assignment variable should be smooth around the cut-off. However, we observe a significant discontinuity. In the preceding (placebo) cohort, this discontinuity does not exist. In Table 3, we report the density discontinuity tests for different bandwidths of the local polynomial and find robust evidence for manipulation in line with the previous literature. If parents’ behaviour for birth date manipulations is related with children’s potential outcomes, the RDD is invalidated to evaluate this reform. We address the resulting concerns about sorting in the close neighbourhood of the cut-off by excluding children born in December and January from our main samples.

Our dataset imposes an additional limitation to implement an RDD. We observe children’s birthday only at the month of birth level. We would have to rely on a maximum of six data points (only five if we exclude the critical months of December and January) on each side of the cut-off in the running variable to specify any parametric model. A linear trend specification may be too parsimonious, and higher order polynomials may overfit the model. For these reasons, an RDD is not our preferred identification strategy.²¹

Instead, we employ a difference-in-differences (DiD) approach similar to, e.g. Dustmann and Schönberg (2011) and Danzer and Lavy (2016) to estimate the intention-to-treat effect (ITT) of the 2007 German paid parental leave reform on children’s development. An advantage of a DiD model over an RDD is that identification is now also based on comparing children that are further

²¹Despite these concerns, we provide estimation results for RDD and donut-RDD (excluding December and January) with varying bandwidths (2-6 months on each side of the cut-off), and higher order polynomials, as well as difference-in-discontinuities estimates (with varying bandwidths and donut specifications) in Online Appendix Tables B.9 and B.10.

away from the cut-off, which increases the precision of the estimates. However, comparing children further away from the cut-off also increases the risk of confounding the reform effect with seasonal and age-at-examination effects. We therefore use children born in the same months but in the preceding year, which was not affected by any policy changes, as our control group to eliminate these potential biases.

The DiD framework relies on three main assumptions to produce unbiased estimates of the reform effect. The first assumption requires common trends in seasonal effects and age-at-examination effects between the reform and control cohorts in the absence of the reform. We run several checks that support the plausibility of this assumption (see Section 7).²² The second assumption requires that no transitory shocks or other co-treatments coincide with the eligibility cut-off for the new parental benefits based on children's birthday in the reform cohort.²³ We checked carefully for such potential co-treatments and are not aware of any such coinciding shock. Other shocks that are unrelated to children's birthday, not coinciding with children's birth year or related to children's school entry cohort, are taken into account in the analysis by birth cohort fixed effects. Another concern regarding the sample composition would be reform effects on fertility patterns. As we only include children born up to six months after the introduction of the reform in our sample, it is unlikely that these couples could have reacted to the reform. Overall, observable characteristics of the children and their family background are balanced in our samples, i.e. one sample covering all children, and the subsamples stratified by mothers' levels of education and pre-reform eligibility (see Appendix Table A.1).

To maximise precision, we compare children born up to six months before and after the reform in the reform cohort and control cohort in our main specification. Empirically, we estimate the following regression model:

²²The availability of publicly-funded day care in Schleswig-Holstein for children aged below the age of three experienced a continuous expansion from 7.5% in 2006 to 21.6% in 2011 (German Federal Statistical Office, 2012). Our identification strategy is not affected by this expansion as it relies on the birthday eligibility cut-off of the reform. The day care expansion affects children in reform cohorts and control cohorts born before and after the cut-off similarly.

²³If daycare admissions depend on children's year of birth, this applies to the treatment and control cohort alike and is accounted for in the difference-in-differences approach.

$$\begin{aligned}
Y_i = & \beta_1 \text{cohort0607}_i + \beta_2 \text{postJanuary}_i + \beta_{PPL} (\text{cohort0607}_i \cdot \text{postJanuary}_i) \\
& + \text{birth month}'_i \delta + (\text{county}_i \cdot \text{examination year}_i)' \partial + X_i' \gamma + \epsilon_i
\end{aligned} \tag{1}$$

where Y_i describes the developmental outcome for child i . We define a birth cohort as running from July of one year to June of the subsequent year. Therefore, the variable *cohort0607* takes the value of 1 if child i is born between July 2006 and June 2007, and 0 if born between July 2005 and June 2006. The variable *postJanuary* _{i} is an indicator variable taking the value of 1 if child i is born between February and June, and 0 if born between July and November. The coefficient β_{PPL} on the interaction term identifies the intention-to-treat effect of the 2007 parental leave reform. Take-up of paid parental leave under the new legislation was almost 100% (German Federal Statistical Office, 2008a).

We further include a vector of birth month fixed effects (*birth month*' _{i}) and county-examination-year fixed effects (*county* _{i} · *examination year* _{i}). We exclude two birth month dummies to avoid perfect collinearity with the variable *postJanuary* _{i} . The county-examination-year fixed effects control for unobserved differences between counties that may even vary across years, such as maternal labour force participation rates or child care attendance rates. To increase the precision of the estimates, we sequentially include additional control variables for child and family characteristics in our regressions (X_i , containing a quadratic function of child's age in months at examination, gender, birth weight, indicators for father's and mother's education, and indicators for whether one or both parents have a migration background).²⁴

²⁴Inference is based on Huber-White (heteroskedasticity robust) standard errors. Clustering at the county level or birth month level leads to the same conclusions.

5. Results

5.1. *Treatment effects on child development and SES development gaps*

We now document how the 2007 German parental leave reform affected children's development and first illustrate our results with a series of graphs. Figure 2 plots the average child outcomes by month of birth separately for children of the reform and control cohorts. We fit linear trends separately for children born on either side of the reform cut-off on January 1 and for children of the reform and control cohorts. We also plot average outcomes for children born in December and January, which we drop from our main estimations (see Section 4). Figure 2 shows that the trends in outcomes are fairly smooth around the cut-off for both the treated and control cohorts. Furthermore, we do not observe level shifts in child outcomes after the cut-off compared to the control group.

In Table 5, we turn to the main estimation results based on equation 1. The rows denote the four different dependent variables. In column 1, we only include fixed effects for birth months, birth cohorts, and county-examination-year. In columns 2 and 3, we gradually add control variables for child and family characteristics. While the explanatory power of the model increases substantially with the inclusion of further control variables, the reform estimates remain very similar across the different specifications. The estimation results from column 3 show that the reform affected the probability of being diagnosed with a sufficient level of language skills by -0.0022 (sample mean of 0.727), of socio-emotional stability by -0.0020 (sample mean of 0.808), of motor skills by -0.0070 (sample mean of 0.826), and of being ready for school by 0.0120 (sample mean of 0.844). Given the fiscal size of the reform, these effects are tiny: Using a two-sided t-test with 95% confidence intervals, we can rule out positive effects that are on average larger than 2.5% for language skills and 2.0% for socio-emotional stability, 1.3% for motor skills, and 3.4% for school readiness. Similarly, we can rule out negative effects greater than 3.1% for language skills, 2.9% for socio-emotional stability and motor skills, and 0.5% for school readiness.

To investigate potentially heterogeneous effects by parental SES, we estimate the model separately

by mothers' education in columns 4 and 5. Again, we find that the effects of the paid parental leave reform are very small across the four domains of child development, independent of maternal education (for graphical evidence, see Appendix Figure A.3).²⁵ In column 6, we statistically test whether the parental leave reform consequently affects socio-economic gaps in child development at age six. The effect estimates on the SES gaps are all very small compared to the SES gaps in Table 4 and not statistically significant. The point estimate on socio-emotional stability of children suggests an increase of the gap (0.0238), but it is not statistically different from zero.

Next, we estimate the model separately by mothers' predicted pre-reform eligibility status. Columns 7 and 8 of Table 5 report the estimated reform effects separately for parents who were likely *previously eligible* (i.e. they lost paid leave due to the reform) and parents who were *previously ineligible* (i.e. they gained access to paid parental leave). The estimates again do not reveal any statistically significant effect on children of these two very different groups. Moreover, none of the differences between both groups are significant (for graphical evidence, see Appendix Figure A.4). While the prediction approximates household income and pre-reform eligibility more closely than parental education alone, it ensures that the parental benefit reform indeed had a very limited impact on child development and child development gaps.²⁶

Figure 3 summarises our main findings graphically. It relates the estimated reform effects to the estimated coefficients on the child development gaps in terms of maternal education, gender, and the child's age (based on Table A.2). Across all four outcomes, Figure 3 shows that our precise estimates are small in magnitude compared to these development gaps.

²⁵If we analyse effects on children of low and medium educated mothers separately, we reach the same conclusions, see Online Appendix Table B.11. We also estimated the reform effects on children with missing information about maternal education levels. The effects are also small and insignificant. The respective graphs are provided in Appendix Figure A.3.

²⁶Appendix Table A.4 reports the main reform effect expressed in standard deviations (SD). These results express by how much one should increase the mean of the standard normal distribution to detect the effects measured. A power analysis reveals that our study has enough power to detect meaningful treatment effects. We conduct the power analysis at the 5% and 10% significance levels and present the results graphically in Appendix Figure B.1 for our main sample. The figure shows that we can detect a reduced form treatment effect of 0.075 SD with a probability of around 90%; with close to 100% probability, we would detect any effects greater than 0.09 SD. Moreover, we are able to detect effect sizes on the order of 0.1 SD at the 5% significance level with a probability of at least 80% for all subgroups.

One limitation of the binary outcomes is that only children crossing the threshold would identify the treatment effect. For this reason, we also use detailed information on subtests on which the binary assessments are based to identify effects on other parts of the distribution of child development. Table 6 repeats the main analysis on the continuous measures for children’s language skills, socio-emotional stability, and motor skills. The table reports the mean score in column 1, and the reform effects on the continuous outcomes in column 2. The effects are now even more precisely estimated, but still very small in magnitude. We additionally construct dummy variables that represent children’s position in the specific test score distribution and estimate reform effects for these different margins of child development (columns 3-7). We find no systematic pattern across the distribution of the child development measures. In addition, we also generated a composite index of child development using the mean score of up to three standardised outcome measures from these subtests. The results in Appendix Table A.4 show that we reach the same conclusions. This analysis reassures that our main findings are not constrained by the binary nature of our outcome measures.

5.2. *Further treatment effect heterogeneities*

Ample evidence suggests that boys typically react more sensitively to changes in early childhood conditions (e.g. Waldfogel, 2006), especially in low-SES families (e.g. Autor et al., 2016). Therefore, we now split the samples by children’s gender to consider further heterogeneities in treatment effects of the parental leave reform (see Table 7). The results in column 1 show that the treatment effects are qualitatively very small and not statistically different from zero for both girls and boys. In addition, no statistically significant differences of the treatment effects exist between the groups. When we stratify the samples of girls and boys further by maternal education (columns 2 and 3), the main picture remains the same. Treatment effects are neither statistically different from zero, nor are there statistically significant differences of the treatment effects between girls and boys. The same picture emerges when we stratify the gender-specific samples by paternal education (columns 4 and 5) and by predicted pre-reform eligibility for paid parental leave (columns 6 and 7). Note

that estimates in columns 4-7 are less precise due to the smaller sample sizes as the data lacks more information on paternal education and as predicted pre-reform eligibility is available only for a subsample.²⁷ Unlike Danzer and Lavy (2016), we cannot detect gender-specific treatment effects for children of lower and higher SES households.²⁸

6. Potential mechanisms

This section explores in more detail how the parental leave reform affected family resources that are potentially relevant for child development, such as parental employment (as a proxy for available parental time), labour earnings and household income. We focus on changes during the first six months, 7 to 12 months, and 12 to 24 months after childbirth. The existing studies on the reform's labour market effects do not distinguish between these different phases of child development. However, this distinction between important phases of children's development can be helpful to better understand why we do not find significant effects on child development.

For this part of the analysis, we use administrative data from the Sample of Integrated Labour Market Biographies, a 2% random sample from the population of all individuals who are either employed or unemployed, who receive social assistance or who participate in employment or training measures (for a detailed description, see Antoni et al., 2016). The data provides detailed employment and earnings information so that we can examine how the reform affected mothers'

²⁷Only the effect estimate on boys of high educated fathers turns significant. If we adjust the p-values for multiple hypothesis testing of the four outcomes, the estimate has a p-value of 0.1 (based on Romano-Wolf's stepdown adjusted p-values procedure with 1000 bootstrap replications; see Romano and Wolf, 2005, 2016, for details). We also stratified the sample by highest level of household education and draw the same conclusion (see Huebener et al., 2017). We draw the same conclusions from a more restrictive model in which we only interact the treatment dummy with the heterogeneity dimension (Online Appendix Table B.7). We cannot run the analysis by children's birth order due to data limitations. We checked whether children without siblings are affected differently than children with siblings, and can neither find any significant effects, nor any significant differences between these groups (Online Appendix Table B.6).

²⁸Danzer and Lavy (2016) find that the 1990 paid parental leave expansions in Austria had positive (negative) effects on sons of highly (low-) educated mothers. The differences in findings are likely due to differences in the child development phases, as well as the usage and quality of alternative care arrangements. In our setting, the availability of publicly-funded day care is very low and the common alternative child care is most likely provided by grandparents and other relatives. The quality differences to maternal care are presumably small. Alternative care provided by the universal day care system in Germany is, on average, of relatively high quality (e.g. Spiess, 2008).

employment, work experience, earnings and income up to age six, at which we observe child development.²⁹ We discuss the changes for previously ineligible and previously eligible mothers.³⁰ For descriptive statistics of the sample, see Appendix Table C.1; for the covariate balancing, see Appendix Table C.2.

Figure 4 presents the survival graphs for the return-to-work behaviour of previously ineligible (Panel A) and previously eligible (Panel B) mothers, using births occurring within six months before and six months after the reform. In Table 9, we report the reform effect estimates on maternal employment, labour earnings and approximated household income based on our main difference-in-differences strategy for the different phases after childbirth. As seen in Figure 4, all mothers stayed at home during the first eight weeks after childbirth because of the unchanged universal mother protection period with fully compensated pre-birth earnings.

For previously ineligible mothers (Panel A of Figure 4), the major change occurs in the first year after childbirth as these mothers substantially delay their return to work: While about 50% of mothers took unpaid leave for the entire first twelve months before the reform, the share increases to about 75% after the reform, now with benefits for all mothers on leave. With the expiry of the new benefits after twelve months, mothers return to the labour market and reach employment levels that are similar to pre-reform levels.

The estimated effects for previously ineligible mothers are reported in Panel B of Table 9. The work experience in the first year after childbirth reduces significantly: In the first six months, they work 0.4 months less, in the subsequent six months, they work 1.3 months less. The accumulated gross labour earnings drop by 891.20 Euros during the first six months, and again by an additional

²⁹As couples are not identified in the data, we cannot separately analyse fathers' employment and earnings responses. However, as the share of fathers taking parental leave is comparably small right after the reform, an analysis of maternal employment and earnings responses should approximate the main changes in terms of family financial and time resources fairly well.

³⁰To determine mother's pre-reform eligibility status, we use the distribution of maternal earnings in the year prior to childbirth. We define mothers with earnings above the median as previously ineligible, and mothers below the 25th percentile as previously eligible. Similar to Schönberg and Ludsteck (2014) whose analysis is based on the same administrative data, we have to impute a child's birth date and use the most recently validated imputation method by Müller et al. (2017).

1891 Euros in the period 7-12 months after childbirth, corresponding to an earnings loss in the first year after child birth of around 8.8% based on pre-birth earnings (see Appendix Table C.1). However, these families now receive parental leave benefits that compensate the earnings loss. If we approximate the parental leave benefits based on maternal pre-birth earnings (following the eligibility criteria), net household income actually increases by 3,512 Euros during the first six months, and by 3,980 Euros 7-12 months after childbirth.³¹ The changes occur mainly at the part-time margin (see Appendix Table C.3). At the beginning of the second year, when the new benefits expires, mothers stay at home slightly longer, but the reform effects on accumulated earnings and household income are negligible (column 3). For the third year, we also do not find any effects on employment or earnings (see column 4). Appendix Table C.3 documents no persistent medium-run effects up to six years after the reform.

In summary, previously ineligible mothers reduced their employment especially 7-12 months after childbirth, mainly at the part-time margin, which in principle allows them to spend more time with their children. Brooks-Gunn et al. (2010) and Bernal and Keane (2010), for example, conclude that reductions in maternal employment have rather small effects on child development beyond the initial six months following childbirth, especially when mothers worked part-time instead. Moreover, household income increases temporarily due to the changes in paid parental leave in the first year. This change can be interpreted as an exogenous transitory household income shock. Carneiro and Ginja (2016), for instance, show that parents do not adjust their child investments in terms of time and goods to transitory income shocks. Moreover, the new parental leave benefit may also be interpreted as an unrestricted benefit, which e.g. Heckman and Mosso (2014) and Del Boca et al.

³¹ As the data does not contain information on maternity leave payments, partner's earnings, or parental leave benefit payments, we need to approximate these three components to determine the reform effect on household income. First, we calculate the earnings during the maternity leave period based on the pre-birth earnings. Second, our own calculations based on the German Microcensus (2006 and 2009) show that almost all fathers with children below the age of three work full-time and that this share did not change after the reform. Thus, partner's earnings likely remained constant. Third, for parents of children born before January 1st 2007, we assume that previously eligible mothers would have received 300 Euros for two years and that previously ineligible mothers would have received no parental leave benefits. For parents of children born on or after January 1st 2007, we convert gross earnings to net earnings using a tax rate of 30%, and then calculate the benefit as 66% of net earnings for the observed duration of parental leave.

(2016) suggest to be ineffective at affecting child development. Financial and time investments relevant for child development likely remained unchanged due to the reform. These findings are consistent with Dahl et al. (2016) who analyse a paid parental leave expansion in Norway. While the reform induced women to almost completely substitute paid work with the new paid parental leave within the first year after childbirth, they find no impact on children's long-term outcomes.

We now turn to previously eligible mothers. Before the reform, they received 300 Euros for up to 24 months. Now, they receive benefits only in the first year, but these benefit may now be higher as they depend on pre-birth earnings. Panel B in Figure 4 reveals that previously eligible mothers stayed at home slightly longer during the first year and they returned to the labour market earlier in the second year as benefits expired.

The regressions results confirm that previously eligible mothers work slightly less during the first year (Panel B of Table 9, columns 1 and 2), again mostly by reducing part-time employment (see Appendix Table C.3). Annual labour earnings decrease expectedly by around 8.6%. However, as the new benefits pay 67% of pre-birth earnings instead of 300 Euros lump-sum, the earnings loss is compensated by higher benefits; the household income therefore remains almost constant during the first year. In the second year, when benefits expire, maternal employment increases (though not significantly, see column 3). The increase in employment and labour earnings, however, is not enough to compensate for the reduced benefits such that household income falls by approximately 956 Euros. In the third and subsequent years, employment increases only marginally and leads to minor and insignificant increases in earnings and family income (see column 4, and Appendix Table C.3 for medium-run effects on maternal employment and earnings).

Thus, previously eligible mothers experienced only very small changes in the first twelve months after childbirth, such that they could direct the same investments (time and goods) toward their children during this period. In the second year, these families experience a negative transitory income shock and a small increase in maternal (part-time) employment, i.e. a reduction in the time mothers can in principle spend with their children. Again, the transitory income shock is unlikely

to change parents' productive investments in their children (see above, and Carneiro and Ginja (2016)). Whether changes in parental time affect child development also depends on the activities both the mother and the alternative caregiver perform with the children: Del Bono et al. (2016), for example, note that the educational activities mothers perform with their children correlate only weakly with maternal employment. Second, Hsin and Felfe (2014), for instance, suggest that maternal employment has no impact on maternal activities that positively affect children, while it reduces the time they spend on activities that are unproductive or even detrimental for child development. Third, the quality of alternative care is likely similar to maternal care as it is mostly provided by informal caregivers.

In order to link the potential reform effects on maternal employment, the use of daycare and other potentially related channels to child outcomes, we return to our data on child development and estimate effect heterogeneities. We interact the treatment indicator with regional characteristics on the availability of day care for children below the age of three (see Table 8, Panel A), the female labour force participation rate (Panel B), and paternal leave-taking (Panel C) to check whether changes in these characteristics as a result of the reform were important reform channels.³² We also check for heterogeneous effects for families living in more rural or urban counties, and for families with a migration background (at least one parent born abroad), as the labour supply pattern and access and use of daycare may vary along these dimensions.

Across the five characteristics, the interaction effects are very small and insignificant on average, and for subgroups stratified by maternal education and pre-reform eligibility. For conciseness, we report the results on the summary measure school readiness; the same picture emerges for children's language and motor skills, and their socio-emotional stability (see Online Appendix Tables B.1 to B.5). We conclude that the availability of day care, female labour force participation and paternal

³²Our reasoning is following: We assume two very similar regions A and B with the same levels of child development and maternal labour supply before the reform; the reform raises maternal labour supply in A, but not in B. Thus, in absence of other changes, finding a difference in child development between these two regions would indicate that maternal labour force supply was an important channel of the effect. Conversely, finding no difference in the effect of the reform between high versus low maternal labour force supply regions would indicate that this was not a major channel. The same logic applies to child care availability and father's paternity leave taking.

leave-taking do not importantly impact the results. We interpret these results as suggestive evidence that the reform had no effects on children, but we cannot entirely rule out that different reform effects on potential channels for child development cancel out the effects on children's development at age 6.

7. Sensitivity checks

Table 10 assesses the sensitivity of our main results for children to varying sample definitions and model specifications. To check whether our estimates are affected by the size of the comparison window, we gradually narrow it down from six to two months on both sides of the cut-off (columns 2 to 5).³³ Our results show that the estimated coefficients are still small, and not statistically different from our main specification. Alternatively, we could include further control cohorts from earlier years. While additional control cohorts may increase the precision of the estimates, these cohorts may also confound the estimated effects, for example, because of different unobserved treatments to the control cohorts. We additionally include children born between July 2004 and June 2005 in the control group as we not aware of any policies relevant for child development that affected this cohort. Including these children increases the sample size by about one third, but the coefficients do not change much (column 6). The gain in the precision of our estimates is small.

Given the evidence of birth shifting that is related to potential reform benefits, we dropped children born in December or January from our main specifications. We include children born in December and January in our main sample (column 7) and draw the same conclusions. Only the effect on socio-emotional stability turns significant in the group of previously eligible parents, but we cannot rule out that this rather small negative point estimate (-0.0378) is confounded by birth shifting.

Since our outcome variables are measured as dummy variables, columns 9 and 10 report the marginal effects on the interaction term of equation 1 from probit and logit models (Puhani, 2012). The estimated effects are very similar to our main results.

³³Predetermined variables are balanced across all window sizes, see Appendix Table A.5.

We assess the plausibility of the common trend assumption in Appendix Table A.6. First, we substitute the birth months fixed effects from our main model with linear (column 2) and quadratic (column 3) cohort-specific time trends. The treatment effect is now identified by differential jumps in the trends on January 1 between reform and control cohorts; reassuringly, we reach the same conclusions. We additionally run two placebo policy reforms at points in time in which no treatment occurred. In the first placebo test, we pretend that the reform was implemented one year earlier (column 4). The second test assumes that the parental leave reform was implemented on April 1, 2007 (column 5). For the second placebo test, we restrict the sample to children born three months before and after the placebo cut-off to avoid overlaps with the real cut-off, and specify the regression model analogously to equation 1. The small and insignificant placebo estimates support the underlying common trend assumption.

We also run all robustness checks separately by maternal education and predicted pre-reform eligibility (Appendix Tables A.6 and A.7), reaching the same conclusions. The Appendix Tables C.4 and C.5 provide the analogous and corroborating robustness checks for the employment outcomes.

8. Conclusion

This paper examines the effects of a substantial paid parental leave reform on child development. The 2007 German reform replaced a means-tested benefit system with an earnings-dependent system, generating near-universal eligibility and causing high-SES households to benefit more from the reform than low-SES households in terms of parental leave benefits. To estimate causal reform effects, we use the eligibility criterion for the new benefits based on children's birth date within a difference-in-differences approach. Our study extends the previous literature along two major lines: First, we examine whether substantial changes in paid parental leave affect SES development gaps. As the changes in terms of benefits and leave duration correlate strongly with families' SES, the German reform we analyse allows us to study how changes in paid parental leave policies affect SES gaps in child development. Whereas the previous literature exclusively studies introductions or

expansions of parental leave schemes, the reform we study both expanded eligibility for paid leave in the first year after childbirth and removed eligibility for paid leave in the second year after childbirth. Second, we provide novel evidence of parental leave policy effects on various medium-run outcomes of children using administrative data from compulsory school entrance examinations at age six. The data covers the full population of one German state and includes detailed information on child development assessed by licensed public health paediatricians.

Our results provide new evidence that even such substantial changes in paid parental leave systems have no impact on various measures of child development at age six. Most point estimates are very close to zero and precisely estimated. We do not find effects on children from high-SES families, on children from lower-SES families, and, consequently, on SES gaps in child development, despite substantial effects on maternal employment and household income. Our results are robust to numerous sensitivity checks.

We explain the zero-reform effects exploring the changes in financial and time resources. We show that the reform affected maternal employment and financial resources mainly in the first two years after child birth, with no persistent medium-run effects. Thus, the changes in financial and time transfers are transitory, which the previous literature has shown to be less effective at affecting child outcomes compared to permanent changes. Similarly, the share of non-working mothers in the first year after birth was already high before the reform and mothers mostly adjusted their employment at the part-time margin – margins that have, at most, a small impact on child development (see Section 6).

As with any other study, our analysis also has some limitations. While we are able to reliably estimate the overall reform effects on various SES groups and to assess the impact on maternal employment and family income, it may also be desirable to disentangle the mechanisms through which the reform results in zero-effects on child development in more detail. However, detailed data on these mechanisms at the individual child level is not available at the individual child level. Moreover, we estimate the reform effects for parents immediately affected by the reform within the

given institutional setting, our empirical strategy cannot capture reform effects within other institutional environments, or effects that unfold gradually over time, such as reform-related changes in social norms about maternal labour supply and paternal leave-taking (e.g. Kluve and Schmitz, 2018; Welteke and Wrohlich, 2016). For example, mothers may give birth at an older age when they are more strongly attached to the labour market, which may itself impact children. Furthermore, the reform may have impacted other child outcomes that are not reflected in the rich set of child development measures that we examine. We also cannot rule out longer-term effects on outcomes, such as future earnings or well-being, that may emerge as potential sleeper effects (e.g., Vandell et al., 2010). Finally, future research should carefully describe the treatment children receive in terms of additional parental time and transfer payments to better learn about the margins of parental leave policies that are relevant for child development.

What do our results mean for public policy? Most OECD countries now have parental leave policies in place, and governments may re-design these regulations, for example, to better incentivise maternal labour supply, fertility, or paternal involvement in the child rearing process. For example, Spain and the UK experience low maternal employment rates, they have only unpaid parental leave in place and observe only low rates of fathers taking parental leave. In order to understand which elements of parental leave policies are effective to reach the various goals associated with parental leave policies, and how this relates to effects on the current and the future work force, it is pivotal to collect recent evidence from different institutional settings. Our analysis of the German reform shows that it effectively changed maternal labour supply, family income, and paternal leave-taking, without adverse effects on the future work force, namely children's development or SES development gaps.

Compliance with Ethical Standards

The authors declare that they have no conflict of interest.

- Almond, D. and J. Currie (2011). Human capital development before age five. In O. Ashenfelter and D. Card (Eds.), *Handbook of Labor Economics*, Volume 4b, Chapter 15, pp. 1315–1486. North Holland.
- Antoni, M., A. Ganzer, P. vom Berge, et al. (2016). Sample of integrated labour market biographies (siab) 1975-2014. *FDZ-Datenreport 4/2016*.
- Autor, D., D. Figlio, K. Karbownik, J. Roth, and M. Wasserman (2016). Family disadvantage and the gender gap in behavioral and educational outcomes. *NBER Working Paper Series 22267*.
- Baker, M. and K. Milligan (2010). Evidence from maternity leave expansions of the impact of maternal care on early child development. *Journal of Human Resources* 45(1), 1–32.
- Baker, M. and K. S. Milligan (2008). Maternal employment, breastfeeding, and health: Evidence from maternity leave mandates. *Journal of Health Economics* 27(4), 871–887.
- Baker, M. and K. S. Milligan (2015). Maternity leave and childrens cognitive and behavioural development. *Journal of Population Economics* 28(2), 373–391.
- Bernal, R. and M. P. Keane (2010). Quasi-structural estimation of a model of childcare choices and child cognitive ability production. *Journal of Econometrics* 156(1), 164–189.
- Beuchert, L. V., M. K. Humlum, and R. Vejlin (2016). The length of maternity leave and family health. *Labour Economics* 43, 55–71.
- Björklund, A. and K. G. Salvanes (2011). Education and family background: Mechanisms and policies. In E. Hanushek, S. Machin, and L. Woessmann (Eds.), *Handbook of Economics of Education*, Volume 3, Chapter 3, pp. 201–247. Amsterdam, London, North Holland: Elsevier.
- Blanden, J., P. Gregg, and L. Macmillan (2007). Accounting for intergenerational income persistence: Noncognitive skills, ability and education. *The Economic Journal* 117(519), C43–C60.
- Bradbury, B., M. Corak, J. Waldfogel, and E. Washbrook (2015). *Too many children left behind: The US achievement gap in comparative perspective*. Russell Sage Foundation.
- Bradley, R. H. and R. F. Corwyn (2002). Socioeconomic Status and Child Development. *Annual Review of Psychology* 53(1), 371–399.
- Brooks-Gunn, J., W.-J. Han, and J. Waldfogel (2010). First-year maternal employment and child development in the first seven years. *Monographs of the Society for Research in Child Development* 75, 1–147.
- Bruhn, M. and D. McKenzie (2009). In Pursuit of Balance: Randomization in Practice in Development Field Experiments. *American Economic Journal: Applied Economics* 1(4), 200–232.
- Bujard, M. (2013). Die fünf Ziele des Elterngelds im Spannungsfeld von Politik, Medien und Wissenschaft. *Zeitschrift für Familienforschung* 25(2), 132–153.
- Carneiro, P., C. Crawford, and A. Goodman (2007). The impact of early cognitive and noncognitive skills on later outcomes. *Centre for the Economics of Education Discussion Paper 92*.

- Carneiro, P. and R. Ginja (2016). Partial insurance and investments in children. *The Economic Journal* 126(596), F66–F95.
- Carneiro, P., K. V. Løken, and K. G. Salvanes (2015). A flying start? Maternity leave benefits and long-run outcomes of children. *Journal of Political Economy* 123(2), 365–412.
- Cattaneo, M. D., M. Jansson, and X. Ma (2018). Manipulation testing based on density discontinuity. *The Stata Journal* 18(1), 234–261.
- Cunha, F. and J. J. Heckman (2007, may). The technology of skill formation. *American Economic Review* 97(2), 31–47.
- Cunha, F., J. J. Heckman, L. Lochner, and D. V. Masterov (2006). Interpreting the evidence on life cycle skill formation. In E. Hanushek and F. Welch (Eds.), *Handbook of Economics of Education*, Volume 1 of *Handbook of the Economics of Education*, Chapter 12, pp. 697–812. Amsterdam: North Holland.
- Cygan-Rehm, K. (2016). Parental leave benefit and differential fertility responses: evidence from a German reform. *Journal of Population Economics* 29(1), 73–103.
- Dahl, G. B. and L. J. Lochner (2012). The impact of family income on child achievement: Evidence from the earned income tax credit. *American Economic Review* 102(5), 1927–1956.
- Dahl, G. B., K. V. Løken, M. Mogstad, and K. V. Salvanes (2016). What is the case for paid maternity leave? *The Review of Economics and Statistics* 98(4), 655–670.
- Danzer, N. and V. Lavy (2016). Paid parental leave and children’s schooling outcomes. *The Economic Journal*, forthcoming.
- Del Boca, D., C. Flinn, and M. Wiswall (2016). Transfers to households with children and child development. *The Economic Journal* 126(596), F136–F183.
- Del Bono, E., M. Francesconi, Y. Kelly, and A. Sacker (2016). Early maternal time investment and early child outcomes. *The Economic Journal* 126(596), F96–F135.
- Duncan, G. J., C. J. Dowsett, A. Claessens, K. Magnuson, A. C. Huston, P. Klebanov, L. S. Pagani, L. Feinstein, M. Engel, J. Brooks-Gunn, H. Sexton, K. Duckworth, and C. Japel (2007). School readiness and later achievement. *Developmental Psychology* 43(6), 1428–1446.
- Dustmann, C. and U. Schönberg (2011). Expansions in maternity leave coverage and children’s long-term outcomes. *American Economic Journal: Applied Economics* 4(3), 190–224.
- Ehlert, N. (2008). Elterngeld als teil nachhaltiger familienpolitik. materialien aus dem kompetenzzentrum für familienbezogene leistungen im bundesministerium für familie, senioren, frauen und jugend.
- Elango, S., J. Garcia, J. Heckman, and A. Hojman (2016). Early Childhood Education. In R. Moffitt (Ed.), *Means-tested Transfer Programs in the United States II*, pp. 235–298. Chicago: University of Chicago Press.

- Federal Ministry of Finance (2007). Haushaltsrechnung und Vermögensrechnung des Bundes für das Haushaltsjahr 2006 (Jahresrechnung 2006). *Bundesministerium der Finanzen*.
- Feinstein, L. (2003). Inequality in the early cognitive development of British children in the 1970 cohort. *Economica* 70(277), 73–97.
- Felfe, C. and R. Lalive (2018). Does early child care affect children’s development? *Journal of Public Economics* 159, 33–53.
- Felfe, C., H. Rainer, and J. Saurer (2019). Why Birthright Citizenship Matters for Immigrant Children: Short- and Long-Run Impacts on Educational Integration. *Journal of Labor Economics*, forthcoming.
- Felfe, C. and L. Zierow (2018). From dawn till dusk: Implications of full-day care for children’s development. *Labour Economics* 55, 259–281.
- German Federal Statistical Office (2008a). Öffentliche Sozialleistungen. Statistik zum Elterngeld. Elterngeld für Geburten 2007 Anträge von Januar 2007 bis Juni 2008. *Statistisches Bundesamt*, Wiesbaden.
- German Federal Statistical Office (2008b). Öffentliche Sozialleistungen. Statistik zum Elterngeld regional: Vergleich aller 439 Kreise in Deutschland, Anträge von Januar bis Dezember 2007. *Statistisches Bundesamt*, Wiesbaden.
- German Federal Statistical Office (2010). Elterngeld für Geburten 2008 nach Kreisen. *Statistisches Bundesamt*, Wiesbaden.
- German Federal Statistical Office (2011). Zensus 2011, Bevölkerung nach Geschlecht, Erwerbsstatus und Altersgruppen - Stichtag 09.05.2011 - regionale Tiefe: Kreise und krfr. Städte. *Statistisches Bundesamt*, Wiesbaden.
- German Federal Statistical Office (2012). Kindertagesbetreuung in Deutschland 2012. *Statistisches Bundesamt*, Wiesbaden.
- German Federal Statistical Office (2016). Volkswirtschaftliche Gesamtrechnung der Länder: Bruttoinlandsprodukt - in jeweiligen Preisen - 1991 bis 2015. *Statistische Ämter des Bundes und der Länder*, Wiesbaden.
- Goodman, R., H. Meltzer, and V. Bailey (1998, oct). The strengths and difficulties questionnaire: A pilot study on the validity of the self-report version. *European Child & Adolescent Psychiatry* 7(3), 125–130.
- Grissmer, D., K. J. Grimm, S. M. Aiyer, W. M. Murrah, and J. S. Steele (2010). Fine motor skills and early comprehension of the world: Two new school readiness indicators. *Developmental Psychology* 46(5), 1008–1017.
- Haeck, C. (2015). Connecting the dots: The early impacts of increased paid maternity leave on child development. *University of Quebec in Montreal’s School of Management, Working Paper 15-01*.

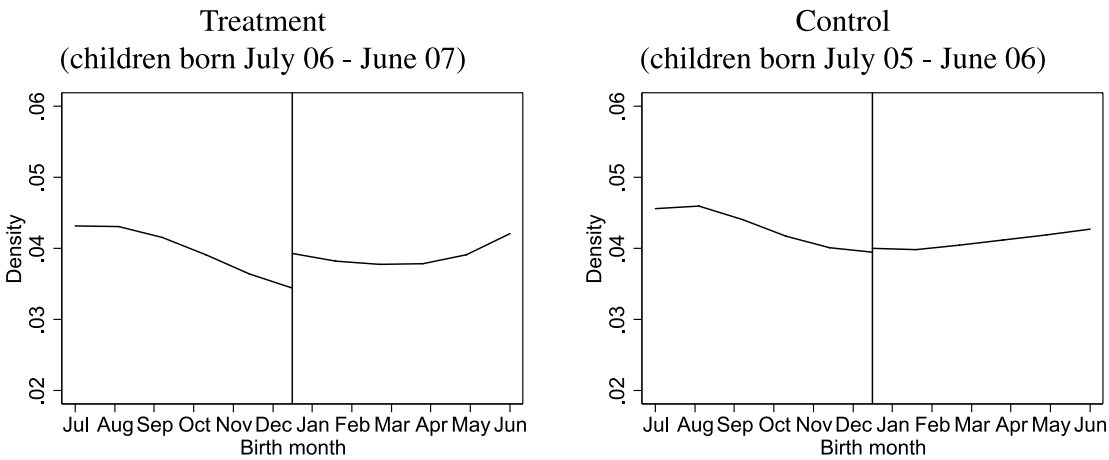
- Heckman, J. J. and S. Mosso (2014). The economics of human development and social mobility. *Annual Review of Economics* 6(1), 689–733.
- Henninger, A., C. Wimbauer, and R. Dombrowski (2008, mar). Geschlechtergleichheit oder "exklusive Emanzipation"? Ungleichheitssoziologische Implikationen der aktuellen familienpolitischen Reformen. *Berliner Journal für Soziologie* 18(1), 99–128.
- Hsin, A. and C. Felfe (2014). When does time matter? Maternal employment, children's time with parents, and child development. *Demography* 51(5), 1867–1894.
- Huber, K. (2019). Changes in parental leave and young children's non-cognitive skills. *Review of Economics of the Household* 17(1), 89–119.
- Huebener, M. (2016). Parental leave policies and child development: A review of empirical findings. *DIW Roundup Politik im Fokus No. 102*.
- Huebener, M., D. Kuehnle, and C. K. Spiess (2017). Paid parental leave and child development: Evidence from the 2007 German parental benefit reform and administrative data. *DIW Discussion Paper Series 1651*, German Institute for Economic Research.
- Huebener, M., K.-U. Müller, C. K. Spiess, and K. Wrohlich (2016). The parental leave benefit: A key family policy measure, one decade later. *DIW Economic Bulletin* 6(49), 571–578.
- Jürges, H. (2017). Financial incentives, timing of births, and infant health: a closer look into the delivery room. *The European Journal of Health Economics* 18(2), 195–208.
- Kluve, J. and S. Schmitz (2018). Back to work: Parental benefits and mothers' labor market outcomes in the medium run. *ILR Review* 71(1), 143–173.
- Kottwitz, A., A. Oppermann, and C. K. Spiess (2016). Parental leave benefits and breastfeeding in Germany: Effects of the 2007 reform. *Review of Economics of the Household* 14(4), 859–890.
- Lalive, R. and J. Zweimüller (2009). How does parental leave affect fertility and return to work? Evidence from two natural experiments. *Quarterly Journal of Economics* 124(3), 1363–1402.
- Lichtman-Sadot, S. and N. P. Bell (2017). Child health in elementary school following california's paid family leave program. *Journal of Policy Analysis and Management* 36(4), 790–827.
- Liu, Q. and O. N. Skans (2010). The duration of paid parental leave and children's scholastic performance. *B.E. Journal of Economic Analysis & Policy Contributions* 10(1), 1–33.
- Løken, K. V., M. Mogstad, and M. Wiswall (2012). What linear estimators miss: The effects of family income on child outcomes. *American Economic Journal: Applied Economics* 4(2), 1–35.
- Maggi, S., L. J. Irwin, A. Siddiqi, and C. Hertzman (2010). The social determinants of early child development: An overview. *Journal of Paediatrics and Child Health* 46(11), 627–635.
- McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of econometrics* 142(2), 698–714.

- Müller, D., K. Strauch, et al. (2017). Identifying mothers in administrative data. Technical report, Institut für Arbeitsmarkt-und Berufsforschung (IAB).
- Neugart, M. and H. Ohlsson (2013). Economic incentives and the timing of births: Evidence from the German parental benefit reform 2007. *Journal of Population Economics* 26(1), 87–108.
- OECD (2016a). OECD data fertility rates (indicator). Retrieved from <https://data.oecd.org/pop/fertility-rates.htm>, downloaded in December, 2016.
- OECD (2016b). OECD Family Database: Enrolment in childcare and pre-school. *OECD - Social Policy Division - Directorate of Employment, Labour and Social Affairs*.
- OECD (2016c). OECD Family database: Maternal employment. *OECD - Social Policy Division - Directorate of Employment, Labour and Social Affairs*.
- Ondrich, J., C. K. Spiess, and Q. Yang (1996). Barefoot and in a German kitchen: Federal parental leave and benefit policy and the return to work after childbirth in Germany. *Journal of Population Economics* 9, 247–266.
- Pei, Z., J.-S. Pischke, and H. Schwandt (2018). Poorly Measured Confounders are More Useful on the Left than on the Right. *Journal of Business & Economic Statistics* *forthcomin*.
- Pihl, A. M. and G. Basso (2019). Did california paid family leave impact infant health? *Journal of Policy Analysis and Management* 38(1), 155–180.
- Puhani, P. A. (2012). The treatment effect, the cross difference, and the interaction term in nonlinear "difference-in-differences" models. *Economics Letters* 115(1), 85–87.
- Raute, A. (2019). Can financial incentives reduce the baby gap? evidence from a reform in maternity leave benefits. *Journal of Public Economics* 169, 203–222.
- Romano, J. P. and M. Wolf (2005). Stepwise multiple testing as formalized data snooping. *Econometrica* 73(4), 1237–1282.
- Romano, J. P. and M. Wolf (2016). Efficient computation of adjusted p-values for resampling-based stepdown multiple testing. *Statistics & Probability Letters* 113, 38–40.
- Rossin, M. (2011). The effects of maternity leave on children's birth and infant health outcomes in the United States. *Journal of Health Economics* 30(2), 221–239.
- Schönberg, U. and J. Ludsteck (2014). Expansions in maternity leave coverage and mothers' labor market outcomes after childbirth. *Journal of Labor Economics* 32(3), 469–505.
- Spiess, C. K. (2008). Early childhood education and care in Germany: The status quo and reform proposals. *Zeitschrift für Betriebswirtschaftslehre* 67, 1–20.
- Stearns, J. (2015). The effects of paid maternity leave: Evidence from Temporary Disability Insurance. *Journal of Health Economics* 43, 85–102.

- Tamm, M. (2013). The impact of a large parental leave benefit reform on the timing of birth around the day of implementation. *Oxford Bulletin of Economics and Statistics* 75(4), 585–601.
- Todd, P. and K. Wolpin (2007). The production of cognitive achievement in children: home, school, and racial test score gaps. *Journal of Human Capital* 1(1), 91–136.
- Van der Klaauw, W. (2008). Regression–discontinuity analysis: a survey of recent developments in economics. *Labour* 22(2), 219–245.
- Vandell, D. L., Belsky, J., Burchinal, M., Steinberg, L., Vandergrift, N., Network, N. E. C. C. R., 2010. Do effects of early child care extend to age 15 years? Results from the NICHD study of early child care and youth development. *Child Development* 81 (3), 737–756.
- Wagner, G. G., J. R. Frick, and J. Schupp (2007). The German Socio-Economic Panel Study (SOEP): Scope, evolution and enhancements. *Schmollers Jahrbuch* 127(1), 139–169.
- Waldfogel, J. (2006). *What children need*. Cambridge, MA: Havard University Press.
- Welteke, C. and K. Wrohlich (2016). Peer effects in parental leave decisions. *DIW Discussion Paper* 1600.
- Wrohlich, K., E. Berger, J. Geyer, P. Haan, D. Sengül, C. K. Spieß, and A. Thiemann (2012). *Elterngeld Monitor*. Deutsches Institut für Wirtschaftsforschung.
- Würtz Rasmussen, A. (2010). Increasing the length of parents’ birth-related leave: The effect on children’s long-term educational outcomes. *Labour Economics* 17(1), 91–100.

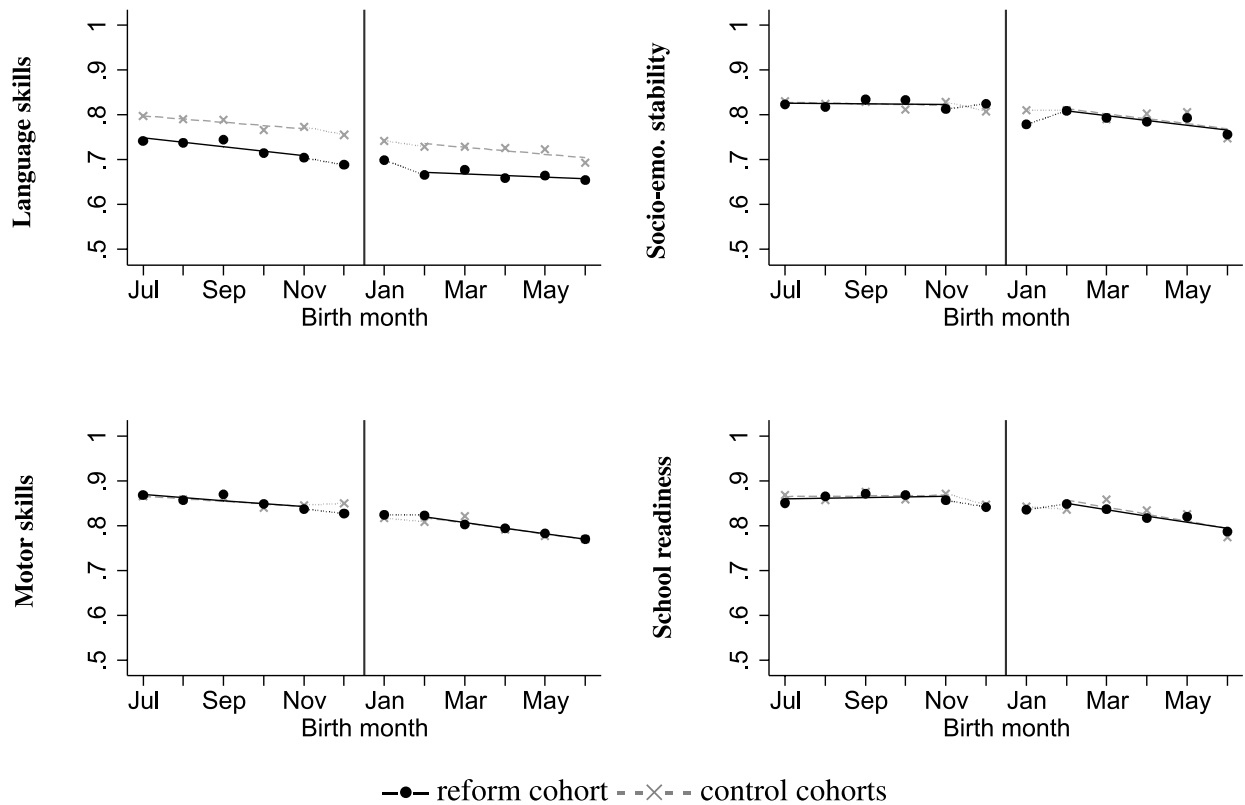
Figures

Figure 1: Cut-off manipulation test



Notes: The figure plots manipulation tests at the threshold from the rddensity command in STATA based on ? with a bandwidth choice of $h(3)$. Formal testings of different bandwidths are reported in Table 3.

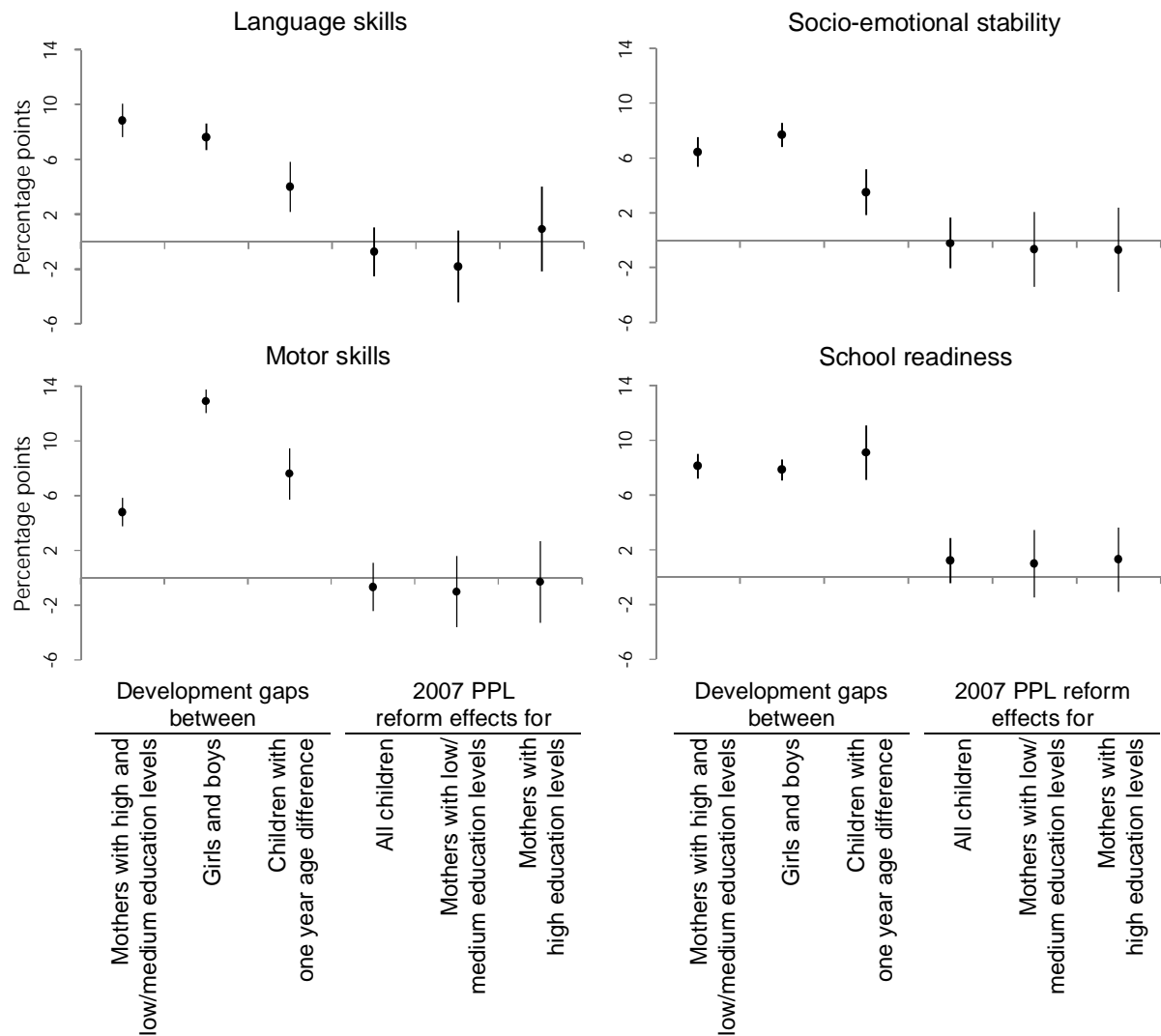
Figure 2: The impact of the 2007 German parental leave reform on child development



Notes: The figure plots the share of children diagnosed with a sufficient level of the respective skill for children born 6 months before and 6 months after the new parental leave legislation in Germany (reform cohort), and for children born in the same months in the year before and the year after (control cohorts). The vertical bar between December and January indicates the introduction of the reform on January 1, 2007. The solid and dashed lines represent linear fits for children in our main sample. The dotted lines refer to children in months that are likely to be affected by birth date manipulations. They are exempted from our main analyses.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

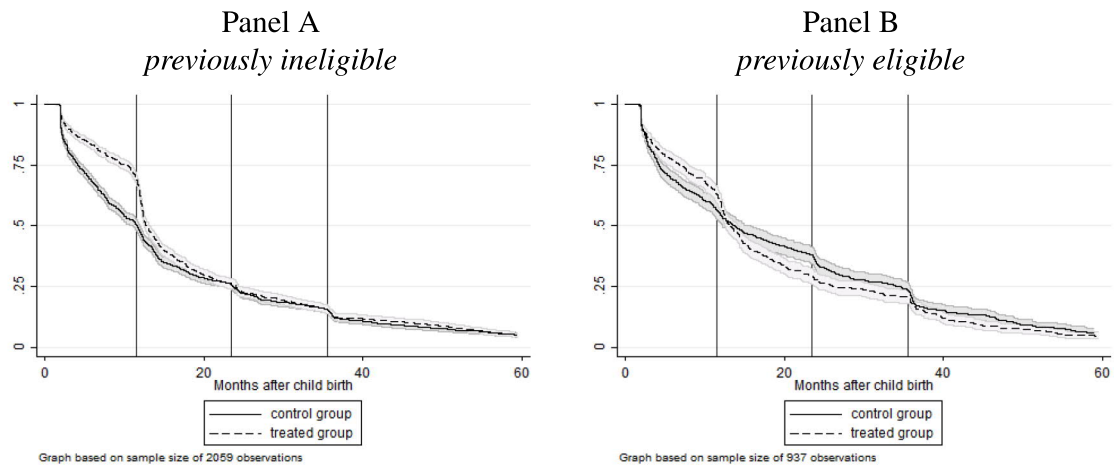
Figure 3: Comparing child development gaps by mothers' education, gender and child age to treatment effect sizes of the 2007 German parental leave reform



Notes: The figure plots child development gaps at school entrance (coefficient estimates retrieved from Table A.2, coefficient on age is scaled), and estimated treatment effects of the 2007 German paid parental leave (PPL) reform for all children, for children from low/medium educated mothers and from highly educated mothers. Bars indicate the 95% confidence interval of the estimated coefficients.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Figure 4: Mother's return to the labour market - survival analysis



Notes: The figures plot the share of mothers who have not returned to work by month t after childbirth, separately for mothers who give birth 6 months before (control group) or 6 months after (treated group) January 1st 2007. We exclude mothers who give birth within 1 month before and after January 1st 2007 from the sample because of measurement error in children's month of birth. The vertical lines refer to one, two and three years after childbirth.

Source: Own calculations based on SIAB-V7514.

Tables

Table 1: Parental leave benefits for parents of children born before and on or after January 1, 2007

	All	Mothers' education	
		Low & medium	High
Pre-birth household annual net income in EUR	31,712.29	27,267.56	37,530.56
<i>Children born before January 1, 2007: Erziehungsgeld</i>			
% recipients for 1-6 months	77.25	84.13	71.07
% recipients for 6-12 months	47.11	52.98	39.80
% recipients for > 12 months	39.91	45.34	33.02
N	311	173	138
<i>Children born on or after January 1, 2007: Elterngeld</i>			
% recipients	nearly 100%	nearly 100%	nearly 100%
Monthly benefits of the mothers in EUR	634.28	562.72	771.12
% fathers taking parental leave	12.81	9.32	20.85
Monthly benefits of the fathers in EUR	1,060.52	864.11	1,190.43
N	197	124	73

Notes: Descriptive statistics on parental leave benefits for parents of children born two years before and two years after the 2007 German paid parental leave reform (2005 through 2008). Statistics exclude civil servants and self-employed mothers, and consider household weights in the year of birth of the child. Survey information is cleaned based on plausibility checks on duration, amount and eligibility criteria under consideration of the provided net household income information.

Source: Own calculations based on SOEPv30 for children born in 2005 through 2008.

Table 2: Changes in parental leave benefits in the first two years after childbirth after the 2007 German paid parental leave reform

		Changes in paid parental leave		
		Months after child birth		
		0-2	3-12	13-24
<i>Previously ineligible</i>				
Before 2007 reform:	Ineligible or eligible for up to 6 months	No change	PPL benefits ↑ PPL duration ↑ ⇒ gain max. 1,800 Euro × 10 months benefits	No change ^a
After 2007 reform:	Eligible			
<i>Previously eligible</i>				
Before 2007 reform:	Eligible	No change	No change	PPL benefits ↓ PPL duration ↓ ⇒ lose max. 300 Euro × 12 months benefits
After 2007 reform:	Minimum benefits			

Notes: This table describes the effects on paid parental leave (PPL) eligibility and benefit payments depending on the pre-reform eligibility for paid parental leave and the amount of benefit payments after the reform. Households that were previously eligible for benefits of 300 Euros per month for up to 24 months, but now receive higher benefit payments only during the first year after childbirth, are in between the two groups. ^a A small share of parents (13%) receives up to 14 months of paid parental leave if the partner also takes parental leave for at least two months.

Source: Own compilation.

Table 3: RD manipulation test using local polynomial density estimation

Bandwidth	Control cohort		Treatment cohort	
	T	$P > T $	T	$P > T $
h(3)	0.207	0.836	2.000	0.045
h(4)	0.234	0.815	2.693	0.007
h(5)	0.775	0.438	2.949	0.003
Data-driven bandwidth selection	0.357	0.721	-20.702	0.000

Notes: $h()$ denotes the bandwidth used to construct the density estimators on the two sides of the cutoff.

Table 4: Descriptive statistics

	(1)	(2)	(3)	(4)	(5)
	All	Sample stratified by mothers' education			
	children	Low & medium	High	Difference	s.e.
<i>Panel A: Child development outcomes measured at age 6</i>					
Language skills (0/1)	0.727	0.682	0.790	-0.109	(0.006)
Socio-emotional stability (0/1)	0.808	0.774	0.854	-0.080	(0.005)
Motor skills (0/1)	0.826	0.804	0.855	-0.051	(0.005)
School readiness (0/1)	0.844	0.815	0.914	-0.099	(0.005)
<i>Panel B: Child characteristics</i>					
Age at examination in months	72.568	72.720	72.312	0.409	(0.065)
Girl (0/1)	0.487	0.490	0.488	0.002	(0.007)
Birth weight in grams	3385.7	3365.4	3437.7	-72.280	(8.076)
Birth weight missing (0/1)	0.039	0.026	0.019	0.008	(0.002)
Years in day care (at age 6)	3.345	3.336	3.466	-0.129	(0.012)
Years in day care missing (0/1)	0.196	0.189	0.185	0.004	(0.005)
Migration background (0/1)	0.215	0.218	0.188	0.030	(0.006)
Migration background missing (0/1)	0.110	0.095	0.084	0.011	(0.004)
<i>Panel C: Family background characteristics</i>					
Mother's years of schooling	10.932	9.648	13.000	-3.352	(0.005)
Mother's education missing (0/1)	0.183	0.000	0.000	0.000	(0.000)
Father's years of schooling	11.661	11.029	12.299	-1.270	(0.021)
Father's education missing (0/1)	0.238	0.104	0.034	0.070	(0.004)
Child lives with both parents (0/1)	0.797	0.751	0.885	-0.134	(0.005)
Child lives with one parent (0/1)	0.139	0.171	0.084	0.087	(0.005)
Child with other living arrangement (0/1)	0.064	0.078	0.031	0.047	(0.003)
Child's living arrangement missing (0/1)	0.087	0.006	0.006	0.000	(0.001)
Home language is German (0/1)	0.852	0.853	0.879	-0.026	(0.005)
German is main language (0/1)	0.106	0.103	0.093	0.010	(0.004)
Home language foreign (0/1)	0.042	0.044	0.028	0.016	(0.003)
Home language missing (0/1)	0.023	0.014	0.014	0.000	(0.002)
Number of children in the family	2.203	2.194	2.167	0.027	(0.013)
N	28,987	14,610	9,072	23,682	

Notes: This table reports descriptive statistics for our main samples. "Low & medium" education refers to lower and medium-secondary school certificates. "High" education refers to upper-secondary school certificates (*Abitur*). The means have been calculated based on non-missing information. The column "Difference" reports the difference in characteristics between children from high and low/medium educated mothers.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table 5: Main results — Estimated effects of the parental leave reform on child development and SES development gaps

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All children		Mothers' education		Δ SES gap =(5)-(4)		Predicted pre-reform eligibility for parental leave benefits	
	Mean (SD)			Low & medium	High		Previously eligible	Previously ineligible
Language skills	0.727 (0.446)	-0.0054 (0.0107) [0.0714]	-0.0027 (0.0105) [0.1090]	-0.0219 (0.0152) [0.1321]	0.0051 (0.0179) [0.0920]	0.0270 (0.0235) [0.1587]	0.0085 (0.0191) [0.1587]	0.0032 (0.0190) [0.0813]
Socio-emot. stability	0.808 (0.394)	-0.0038 (0.0096) [0.0517]	-0.0027 (0.0095) [0.0684]	-0.0065 (0.0139) [0.0978]	0.0069 (0.0157) [0.0713]	0.0134 (0.0209) [0.0976]	-0.0217 (0.0178) [0.0976]	-0.0006 (0.0170) [0.0782]
Motor skills	0.826 (0.379)	-0.0089 (0.0092) [0.0398]	-0.0074 (0.0090) [0.0760]	-0.0103 (0.0132) [0.0879]	-0.0032 (0.0151) [0.0877]	0.0071 (0.0201) [0.0883]	-0.0124 (0.0168) [0.0883]	0.0018 (0.0166) [0.0916]
School readiness	0.844 (0.362)	0.0087 (0.0088) [0.0425]	0.0109 (0.0086) [0.0830]	0.0097 (0.0126) [0.1177]	0.0129 (0.0120) [0.0655]	0.0032 (0.0174) [0.1201]	0.0096 (0.0177) [0.1201]	0.0201 (0.0125) [0.0592]
N	28,987	28,987	28,987	14,610	9,072	23,682	8,458	7,849
<i>Control variables</i>								
Child characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family characteristics	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the coefficient estimates of the parental leave reform effect (β_{ppl}) on child outcomes and on development gaps between children from low/medium and high educated mothers. All regressions are based on equation 1 and include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects and dummies for missing variables. The stratification in columns 7 and 8 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. R^2 are reported in brackets. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table 6: Effects of the parental leave reform on alternative definitions of child development

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Language skills</i>							
	Language		Treatment effect on				
	score, mean	Language	language score in plural words, pseudo words & prepositions				
	(SD)	score	≤ 12	13 – 16	17 – 19	20	21
			(6%)	(14%)	(34%)	(21%)	(26%)
Pooled sample (N=13,047)	18.3898 (3.1216)	-0.0298 (0.0990)	0.0048 (0.0076)	0.0203 (0.0115)	-0.0182 (0.0169)	-0.0039 (0.0147)	-0.0030 (0.0150)
Mothers with low/medium education (N=7,189)	18.0396 (3.2534)	-0.0773 (0.1432)	0.0108 (0.0112)	0.0270 (0.0167)	-0.0247 (0.0231)	-0.0179 (0.0194)	0.0048 (0.0189)
Mothers with high education (N=4,442)	19.3001 (2.3849)	0.1483 (0.1369)	-0.0089 (0.0077)	-0.0080 (0.0155)	0.0058 (0.0280)	0.0101 (0.0271)	0.0010 (0.0290)
<i>Panel B: Socio-emotional stability</i>							
	SDQ score,		Treatment effect on				
	mean	SDQ	specific parts of the SDQ score distribution: Score =				
	(SD)	score	0	1 – 4	5 – 8	9 – 12	≥ 13
			(8%)	(32%)	(33%)	(16%)	(11%)
Pooled sample (N=12,587)	6.3924 (4.8181)	0.0534 (0.1699)	-0.0009 (0.0095)	0.0062 (0.0167)	-0.0276 (0.0174)	0.0079 (0.0138)	0.0144 (0.0115)
Mothers with low/medium education (N=6,582)	7.2876 (4.9430)	-0.0894 (0.2413)	-0.0017 (0.0105)	-0.0105 (0.0219)	0.0058 (0.0242)	0.0115 (0.0205)	-0.0052 (0.0176)
Mothers with high education (N=4,674)	5.1574 (4.0960)	0.1705 (0.2490)	-0.0062 (0.0175)	0.0229 (0.0300)	-0.0517 (0.0290)	0.0043 (0.0200)	0.0308* (0.0144)
<i>Panel C: Motor skills</i>							
	Jumps,		Treatment effect on				
	mean	No. of	side-jumps within 10 seconds				
	(SD)	side-jumps	≤ 7	8 – 9	10	11 – 13	≥ 14
			(13%)	(27%)	(31%, mode)	(18%)	(11%)
Pooled sample (N=12,855)	10.1165 (3.0851)	-0.1037 (0.1058)	0.0022 (0.0118)	0.0199 (0.0158)	-0.0134 (0.0154)	-0.0113 (0.0136)	-0.0074 (0.0108)
Mothers with low/medium education (N=7,130)	9.9771 (3.0253)	-0.0996 (0.1402)	-0.0049 (0.0165)	0.0235 (0.0215)	-0.0077 (0.0206)	-0.0300 (0.0180)	-0.0008 (0.0140)
Mothers with high education (N=4,286)	10.5422 (3.2031)	-0.1229 (0.1951)	-0.0066 (0.0178)	0.0251 (0.0266)	-0.0214 (0.0269)	0.0271 (0.0251)	-0.0242 (0.0211)

Notes: This table reports estimated reform effects on subdimensions tested in school entrance examinations, i.e. on SOPESS language test scores (plurals, pseudo words and prepositions, see Panel A), on the sum of SDQ subscales, ranging from 0 to 40 (Panel B), and on the number of side-jumps (Panel C). This information is only available for a subset of counties. The treatment effects are reported for the pooled sample, and for subsamples stratified by mothers' education. All regressions are based on equation 1, and include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. The sample is restricted to counties that delivered the raw scores to the data-compiling Ministry of Social Affairs, Health, Family and Equal Opportunities in Schleswig-Holstein. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

Table 7: Heterogeneity analysis by gender, parental education and pre-reform eligibility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Mothers' education		Fathers' education		Predicted eligibility for parental leave benefits	
	All	Low/med.	High	Low/med.	High	Previously eligible	Previously ineligible
Language skills							
Girls	0.0122 (0.0145)	-0.0062 (0.0214)	0.0137 (0.0246)	0.0193 (0.0225)	-0.0325 (0.0248)	0.0214 (0.0268)	0.0000 (0.0261)
Boys	-0.0151 (0.0149)	-0.0360 (0.0217)	-0.0066 (0.0259)	-0.0309 (0.0230)	-0.0083 (0.0258)	-0.0050 (0.0273)	0.0069 (0.0275)
Socio-emo. stability							
Girls	-0.0215 (0.0127)	-0.0338 (0.0188)	-0.0073 (0.0208)	-0.0369 (0.0196)	-0.0328 (0.0206)	-0.0186 (0.0243)	-0.0415 (0.0220)
Boys	0.0153 (0.0138)	0.0194 (0.0203)	0.0143 (0.0232)	0.0041 (0.0215)	0.0505* (0.0228)	-0.0230 (0.0260)	0.0323 (0.0255)
Motor skills							
Girls	-0.0090 (0.0110)	0.0031 (0.0165)	-0.0140 (0.0177)	-0.0004 (0.0168)	-0.0133 (0.0184)	-0.0231 (0.0212)	-0.0234 (0.0194)
Boys	-0.0069 (0.0139)	-0.0257 (0.0203)	-0.0020 (0.0238)	-0.0202 (0.0215)	-0.0110 (0.0238)	0.0003 (0.0259)	0.0209 (0.0264)
School readiness							
Girls	0.0006 (0.0110)	-0.0072 (0.0164)	0.0123 (0.0147)	-0.0113 (0.0169)	0.0048 (0.0153)	-0.0052 (0.0239)	0.0117 (0.0150)
Boys	0.0203 (0.0126)	0.0219 (0.0188)	0.0087 (0.0186)	0.0121 (0.0196)	0.0309 (0.0191)	0.0208 (0.0259)	0.0262 (0.0195)
Number of girls	14,126	7,160	4,426	6,439	4,373	4,126	3,833
Number of boys	14,861	7,450	4,646	6,588	4,696	4,332	4,016

Notes: This table reports the estimation results of the parental leave reform on child outcomes on samples stratified by gender, different definitions of parental education, and pre-reform eligibility for parental leave benefits. Each coefficient comes from a separate regression. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. The stratification in columns 6 and 7 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table 8: Further effect heterogeneity

	(1)	(2)	(3)	(4)	(5)
	Dep. variable: School readiness				
		Mothers' education		Predicted eligibility for parental leave benefits	
	All	Low/med.	High	Previously eligible	Previously ineligible
<i>Panel A: Heterogeneity by share of children below age 3 in formal day care (county-year level, min. 8%, max. 27%)</i>					
PPL reform effect	0.0117 (0.0084)	0.0097 (0.0125)	0.0157 (0.0120)	0.0067 (0.0184)	0.0205 (0.0125)
PPL reform effect · chilcare share in % (below age 3)	0.0002 (0.0014)	-0.0004 (0.0022)	-0.0023 (0.0023)	-0.0024 (0.0029)	-0.0009 (0.0024)
<i>Panel B: Heterogeneity by female labour force participation rate (county level, 2011, min. 70%, max. 79%)</i>					
PPL reform effect	0.0120 (0.0084)	0.0097 (0.0126)	0.0132 (0.0123)	0.0164 (0.0181)	0.0176 (0.0128)
PPL reform effect · female LFP share in %	0.0012 (0.0020)	-0.0006 (0.0029)	-0.0005 (0.0031)	0.0082 (0.0047)	0.0043 (0.0032)
<i>Panel C: Heterogeneity by share of fathers taking parental leave (county level, 2008, min. 5.8%, max. 13.3%)</i>					
PPL reform effect	0.0123 (0.0085)	0.0116 (0.0128)	0.0130 (0.0120)	0.0097 (0.0176)	0.0198 (0.0126)
PPL reform effect · paternal leave share in %	0.0031 (0.0040)	0.0075 (0.0059)	-0.0041 (0.0056)	-0.0043 (0.0089)	-0.0061 (0.0058)
<i>Panel D: Heterogeneity by population density (county level, 2006, rural if ≤ 125 inhabitants/km²)</i>					
PPL reform effect	0.0088 (0.0131)	0.0085 (0.0198)	0.0316 (0.0217)	0.0108 (0.0256)	0.0276 (0.0234)
PPL reform effect · rural	0.0095 (0.0093)	0.0073 (0.0140)	0.0077 (0.0128)	0.0055 (0.0212)	0.0153 (0.0132)
<i>Panel E: Heterogeneity by migration background (sample mean: 21%)</i>					
PPL reform effect · Migration background	-0.0052 (0.0142)	-0.0107 (0.0206)	-0.0282 (0.0228)	-0.0065 (0.0259)	0.0173 (0.0291)
PPL reform effect · No migration background	0.0121 (0.0088)	0.0108 (0.0132)	0.0206 (0.0120)	0.0246 (0.0126)	-0.0113 (0.0204)

Notes: This table reports the estimation results of the parental leave reform effect on school readiness (results on other outcomes available in Online Appendix Tables B.1-B.5). Regressions are based on the main model, including an additional interaction term of the treatment indicator with county level characteristics in 2006, and the baseline variable of the interacted category. Effects are evaluated at the mean of the interacted variable. The childcare share at the county-year level is based on ? and assigns counties their the pre-reform values from 2006. The female labour force participation rate for women aged 15-45 is based on German Federal Statistical Office (2011). FPaternal leave share at the county level is based on German Federal Statistical Office (2008b). The stratification in columns 4 and 5 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table 9: Mechanisms - estimated effects of the parental leave reform on employment outcomes

when observed after child birth	0-6 months	7-12 months	13-24 months	25-36 months
Panel A: All (N=8,817)				
Work experience	-.423** (.06878)	-.9895** (.1193)	-.2323 (.24)	-.02273 (.2467)
Labour earnings	-546.4** (120.1)	-1173** (211.8)	-69.36 (493.4)	244.3 (522.2)
Household income	1748** (161.1)	1968** (153.1)	-330.8 (347.7)	271.6 (378.8)
Panel B: Previously ineligible (N=4,345)				
Work experience	-.4409** (.09938)	-1.275** (.1684)	-.6187* (.3344)	-.0705 (.3421)
Labour earnings	-891.2** (222)	-1891** (385.8)	-276.6 (845.4)	622 (880.1)
Household income	3512** (200.5)	3980** (261.1)	-193.6 (591.8)	435.4 (616.1)
Panel C: Previously eligible (N=2,073)				
Work experience	-.2899** (.1425)	-.5649** (.2485)	.5409 (.5043)	.2717 (.5153)
Labour earnings	-174.5* (101.4)	-424.8** (171.6)	487.9 (442.6)	245 (547)
Household income	118.8 (99.35)	56.33 (125.2)	-1004** (357.7)	220.5 (492.3)

Notes: This table reports the coefficient estimates of the parental leave reform effect (β_{PPL}) on maternal employment outcomes accumulated during specific phases after child birth. The sample includes mothers who give birth between July 2005 and June 2007, but excludes mothers who give birth in December or January because of measurement error in children's month of birth. All regressions are based on equation 1 and include state fixed effects, birth months fixed effects, birth cohort fixed effects, and controls for maternal age at child birth (squared), German citizenship, first child indicator, and work experience before child birth. The stratification in Panels B and C is based on pre-reform wages and separates parents into those who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth and who were likely not *previously ineligible* for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on SIAB-V7514.

Table 10: Robustness checks

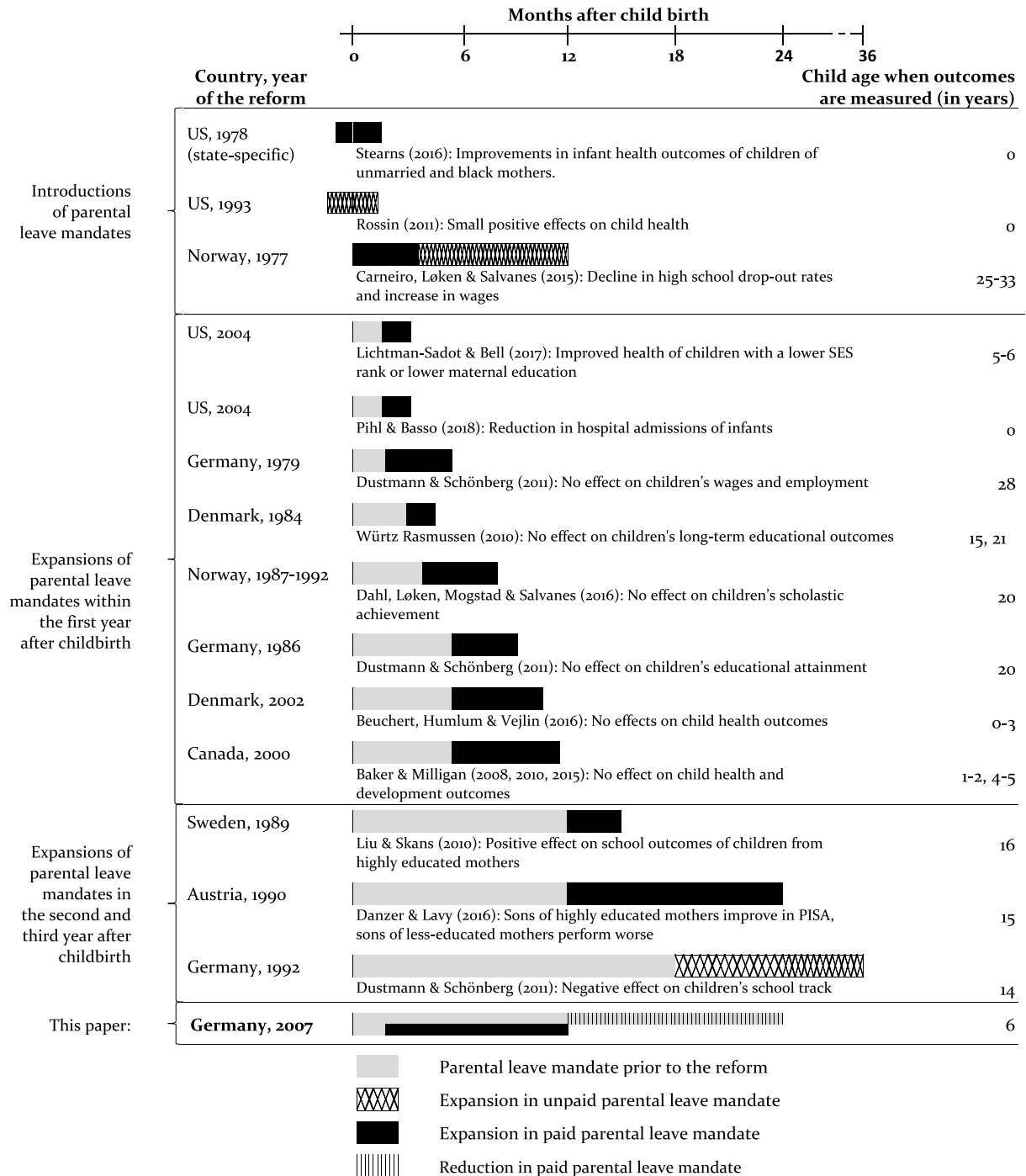
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	Window size without January & December				Including children born	Including children born	Nonlinear models (marginal effects)	
	(Jul-Jun)	Aug-May	Sep-Apr	Oct-Mar	Nov-Feb	7/2004-6/2005	Jan & Dec	Probit	Logit
<i>Mothers' education: All</i>									
Language skills	-0.0022 (0.0104)	0.0000 (0.0115)	0.0003 (0.0133)	0.0140 (0.0163)	0.0058 (0.0235)	-0.0078 (0.0089)	0.0003 (0.0094)	0.0006 (0.0101)	0.0025 (0.0101)
Socio-emot. stability	-0.0020 (0.0094)	-0.0028 (0.0104)	-0.0050 (0.0119)	0.0040 (0.0145)	0.0159 (0.0207)	-0.0013 (0.0081)	-0.0117 (0.0085)	-0.0040 (0.0091)	-0.0031 (0.0092)
Motor skills	-0.0070 (0.0090)	-0.0031 (0.0100)	-0.0075 (0.0114)	-0.0034 (0.0140)	0.0148 (0.0202)	-0.0044 (0.0077)	-0.0014 (0.0082)	-0.0087 (0.0088)	-0.0090 (0.0089)
School readiness	0.0120 (0.0084)	0.0079 (0.0092)	0.0057 (0.0105)	0.0049 (0.0129)	0.0282 (0.0185)	0.0056 (0.0072)	0.0088 (0.0077)	0.0122 (0.0083)	0.0120 (0.0084)
N	28987	22825	16787	11011	5322	44576	34560	28968	28968

Notes: This table reports the results from robustness checks of the reform effect of the parental leave reform on child outcomes stratified by mothers' education. The window size around the reform cut-off and definitions of the control group are varied. Further, the robustness to non-linear model specifications is tested. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

Appendix

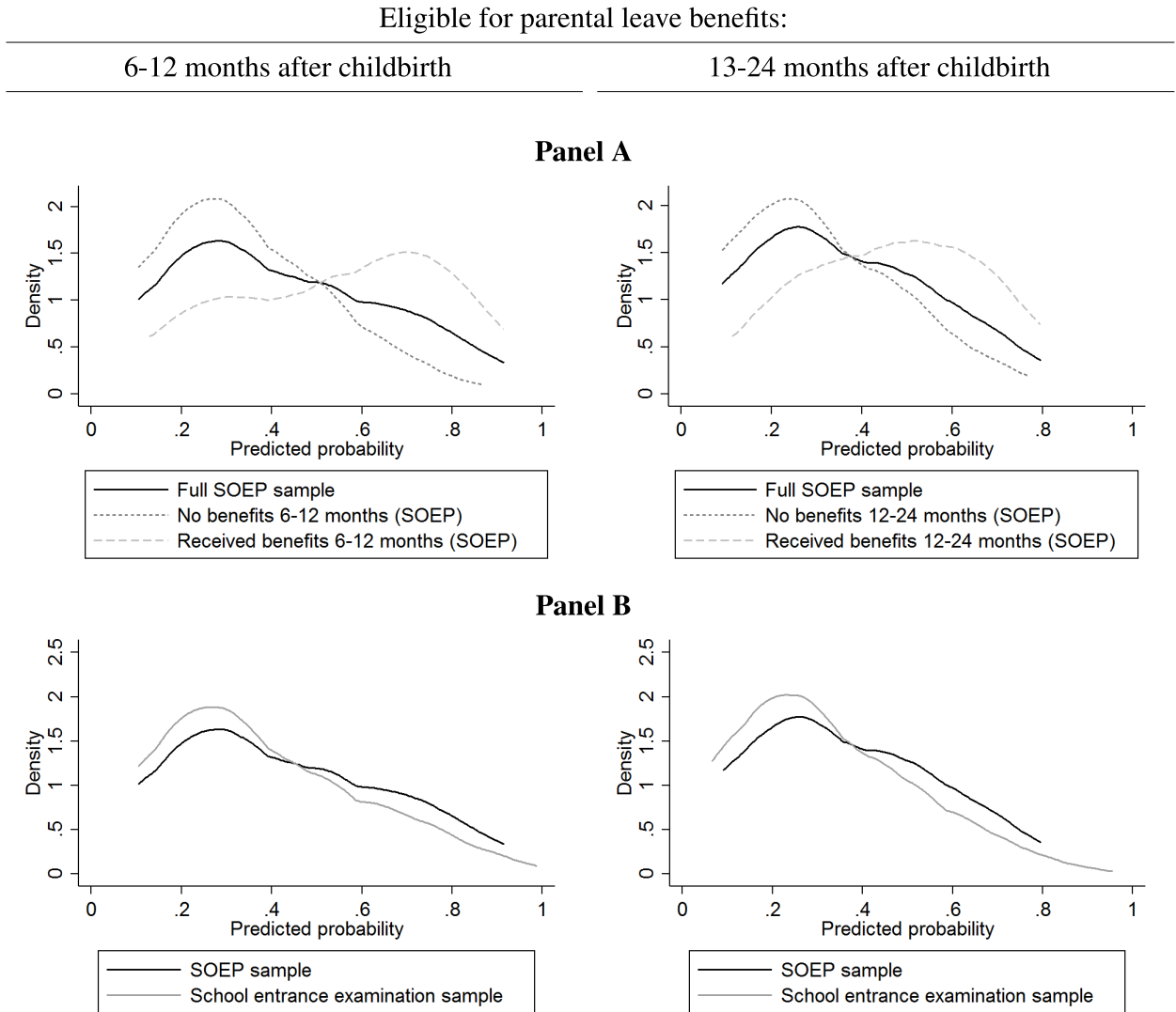
Figure A.1: Evaluated parental leave reforms and their impact on child outcomes



Notes: This figure provides an overview of peer-reviewed economic studies evaluating parental leave reforms and their impact on child outcomes in individual level data.

Source: Illustration based on Huebener (2016).

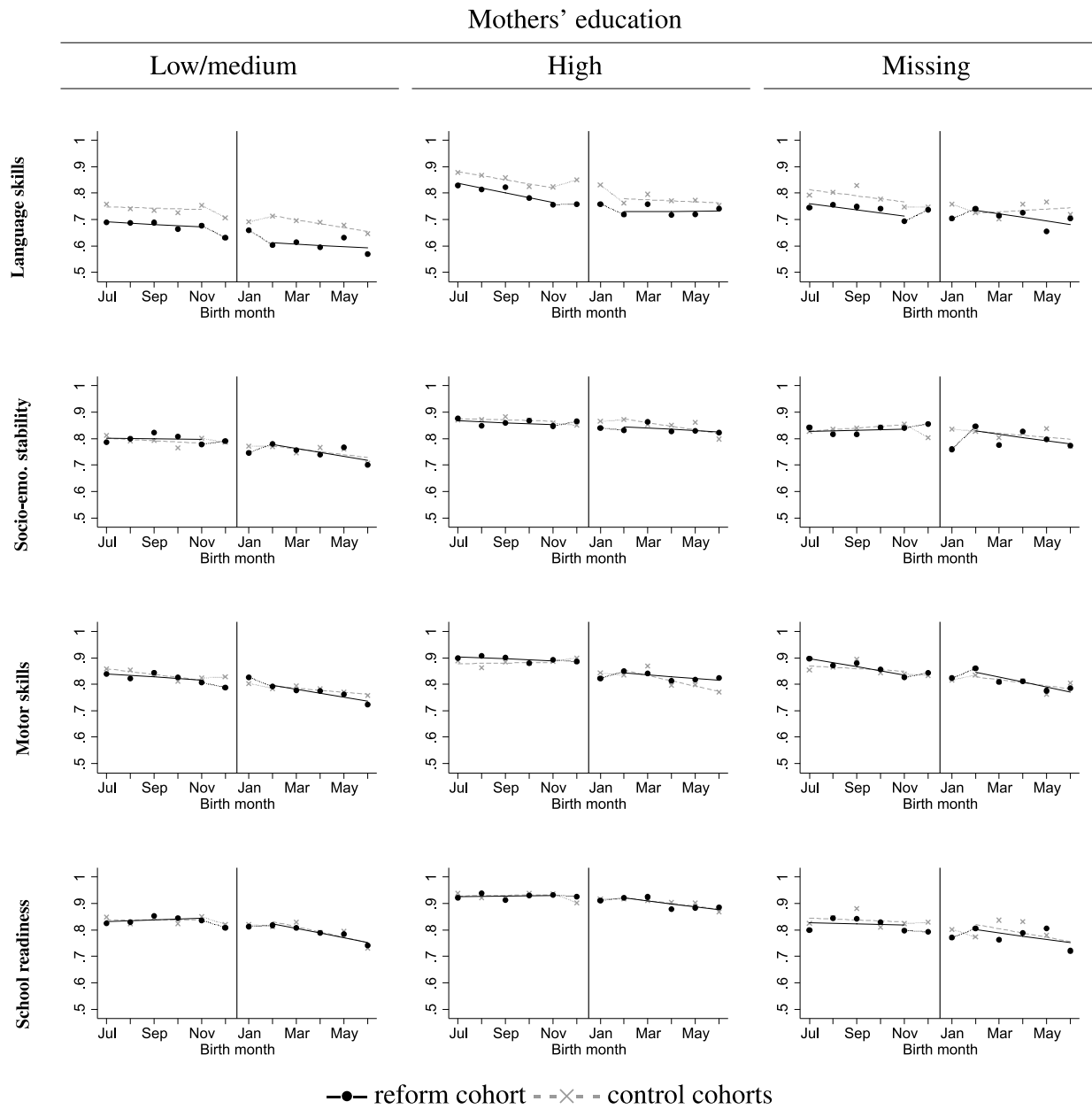
Figure A.2: Predicted eligibility for parental leave benefits under the policy rules applying before the 2007 reform



Notes: The figures in Panel A show kernel density plots (Epanechnikov) of the predicted probabilities of families to receive paid parental leave for 6-12 months and for 13-24 months after childbirth under the policy rules applying before the 2007 parental leave reform. The prediction uses SOEP data on real take-up and is based on a logit model including dummies for mothers' education and fathers' education, their interaction, a dummy for single parents, the number of children in the family, a dummy for migration background, and an interaction terms of mothers' education and the number of children. All variables are measured at age 6 if available (earlier otherwise). The figures in Panel B show the predictions based on the original SOEP information, and the out-of-sample predictions in the school entrance examinations data.

Source: Own calculations based on SOEPv32 for children born in 2005 through 2006, and school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

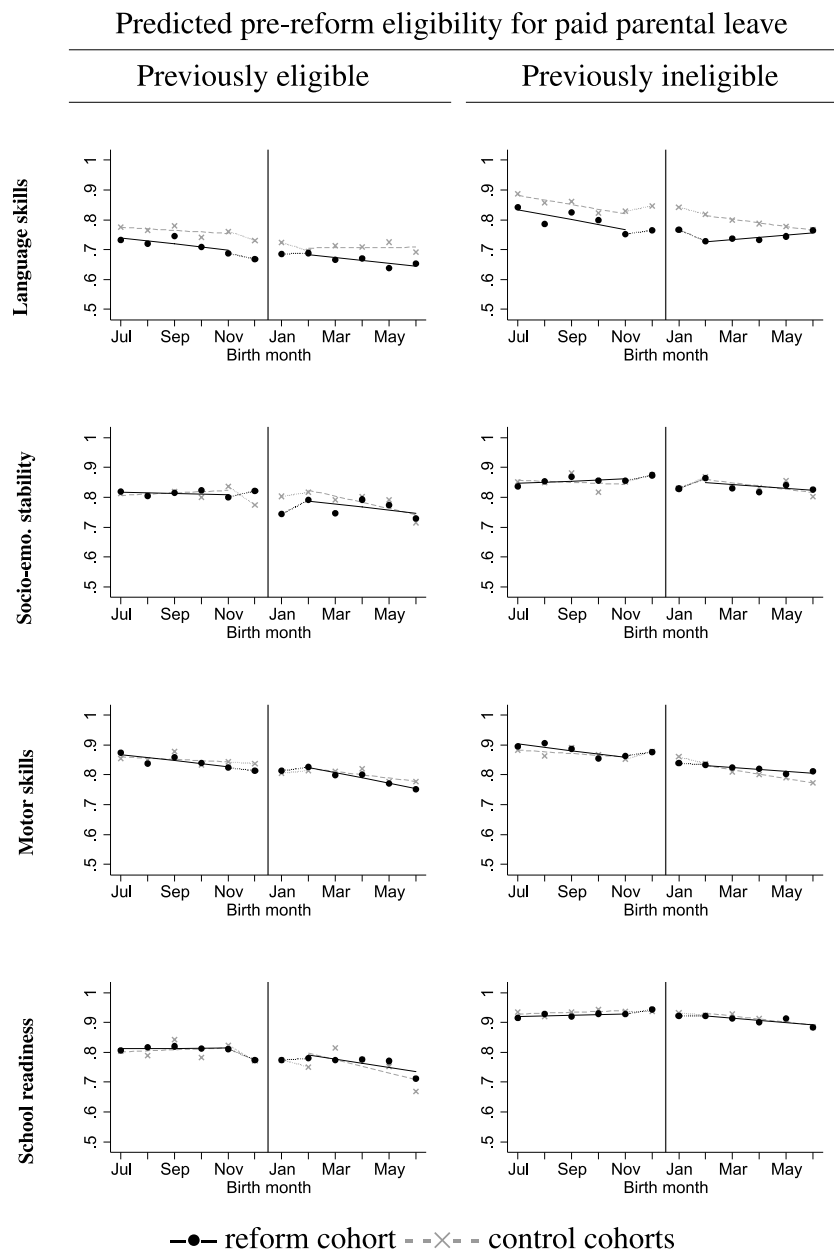
Figure A.3: The impact of the 2007 German parental leave reform on child development for subgroups



Notes: The figure plots the share of children diagnosed with a sufficient level of the respective skill for children born 6 months before and 6 months after the new parental leave legislation in Germany (reform cohort), and for children born in the same months in the year before and the year after (control cohorts) separately by mothers' education. The vertical bar between December and January indicates the introduction of the reform on January 1, 2007. The solid and dashed lines represent linear fits for children in our main sample. The dotted lines refer to children in months that are likely to be affected by birth date manipulations. They are exempted from our main analyses.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Figure A.4: The impact of the 2007 German parental leave reform on child development for subgroups



Notes: The figure plots the share of children diagnosed with a sufficient level of the respective skill for children born 6 months before and 6 months after the new parental leave legislation in Germany (reform cohort), and for children born in the same months in the year before and the year after (control cohorts) separately by parents' predicted eligibility for paid parental leave before the 2007 reform. The vertical bar between December and January indicates the introduction of the reform on January 1, 2007. The solid and dashed lines represent linear fits for children in our main sample. The dotted lines refer to children in months that are likely to be affected by birth date manipulations. They are exempted from the main analyses.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table A.1: Balancing of covariates in RD and DiD settings

	Sample stratified by maternal education						Sample stratified by predicted pre-reform eligibility for parental leave benefits			
	All		Low & medium		High		Previously eligible		Previously ineligible	
	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.
RD including December and January										
Girl	0.0168	(0.0161)	0.0058	(0.0227)	0.0125	(0.0291)	0.0531	(0.0304)	-0.0323	(0.0307)
Birth weight in grams	-6.4663	(18.8581)	11.5029	(27.1924)	-46.0231	(33.3644)	30.5303	(35.3611)	-51.7187	(35.3815)
Birth weight missing	0.0093	(0.0063)	0.0109	(0.0069)	0.0120	(0.0085)	-0.0021	(0.0170)	0.0076	(0.0085)
Mother's years of schooling	-0.0479	(0.0474)	-0.0068	(0.0216)	—	—	0.0574	(0.0528)	-0.0942	(0.0856)
Mother's education missing	-0.0024	(0.0107)	—	—	—	—	0.0025	(0.0256)	—	—
Father's years of schooling	-0.0069	(0.0353)	-0.0142	(0.0498)	0.0303	(0.0698)	-0.0238	(0.0214)	-0.0206	(0.0793)
Father's education missing	-0.0145	(0.0123)	-0.0301*	(0.0139)	0.0065	(0.0105)	-0.0372	(0.0192)	—	—
Single parent	0.0082	(0.0109)	-0.0144	(0.0176)	0.0303	(0.0166)	-0.0023	(0.0236)	—	—
Other living arrangement	-0.0051	(0.0076)	-0.0188	(0.0125)	0.0118	(0.0100)	-0.0164	(0.0166)	0.0057	(0.0120)
Living arrangement missing	0.0051	(0.0055)	0.0053	(0.0035)	0.0058	(0.0048)	0.0107	(0.0168)	—	—
One parent with mig. back.	0.0052	(0.0093)	0.0107	(0.0135)	-0.0036	(0.0175)	0.0080	(0.0176)	0.0043	(0.0173)
Two parents with mig. back.	0.0006	(0.0105)	0.0152	(0.0150)	-0.0312	(0.0169)	0.0060	(0.0214)	-0.0210	(0.0147)
Migration background missing	0.0060	(0.0069)	0.0113	(0.0073)	-0.0027	(0.0062)	0.0235	(0.0219)	—	—
Home language German	0.0055	(0.0103)	0.0134	(0.0141)	-0.0190	(0.0180)	0.0138	(0.0211)	-0.0152	(0.0165)
Home language foreign	0.0080	(0.0063)	0.0195*	(0.0089)	-0.0137	(0.0102)	0.0161	(0.0130)	-0.0123	(0.0092)
Home language missing	0.0031	(0.0055)	0.0107	(0.0063)	-0.0007	(0.0085)	-0.0118	(0.0142)	0.0080	(0.0090)
Number of siblings	0.0393	(0.0334)	0.0876	(0.0473)	0.0096	(0.0529)	0.0386	(0.0802)	-0.0169	(0.0294)
Number of siblings missing	-0.0032	(0.0020)	-0.0046	(0.0025)	0.0024	(0.0031)	-0.0082	(0.0068)	—	—
	<i>F</i> -stat	<i>p</i> -value	<i>F</i> -stat	<i>p</i> -value	<i>F</i> -stat	<i>p</i> -value	<i>F</i> -stat	<i>p</i> -value	<i>F</i> -stat	<i>p</i> -value
Testing for joint orthogonality	0.83	0.670	1.91	0.013	1.12	0.329	1.05	0.398	0.86	0.593
DiD excluding December and January										
Girl	-0.0060	(0.0123)	-0.0073	(0.0172)	-0.0117	(0.0225)	-0.0149	(0.0226)	0.0072	(0.0240)
Birth weight in grams	3.0190	(14.6230)	-3.2538	(20.5677)	19.8829	(26.5321)	-10.0258	(26.6979)	0.5279	(28.0543)
Birth weight missing, dummy	-0.0042	(0.0048)	-0.0052	(0.0054)	-0.0019	(0.0061)	-0.0043	(0.0125)	-0.0047	(0.0058)
Mother's years of schooling	0.0017	(0.0363)	0.0016	(0.0163)	—	—	-0.0353	(0.0428)	-0.0180	(0.0677)
Mother's education missing	0.0098	(0.0080)	—	—	—	—	0.0146	(0.0192)	—	—
Father's years of schooling	0.0342	(0.0271)	0.0763*	(0.0379)	-0.0106	(0.0548)	-0.0375	(0.0207)	0.0634	(0.0622)
Father's education missing	0.0131	(0.0093)	0.0090	(0.0106)	0.0045	(0.0082)	0.0186	(0.0154)	—	—
Single parent, dummy	-0.0003	(0.0082)	-0.0122	(0.0130)	0.0038	(0.0126)	0.0185	(0.0175)	—	—
Other living arrangement, dummy	-0.0037	(0.0058)	0.0084	(0.0092)	-0.0098	(0.0078)	-0.0093	(0.0126)	-0.0048	(0.0095)
Living arrangement missing	0.0032	(0.0039)	0.0049	(0.0026)	-0.0008	(0.0034)	0.0053	(0.0117)	—	—
One parent with mig. back., dummy	0.0049	(0.0068)	0.0051	(0.0094)	0.0022	(0.0135)	0.0140	(0.0124)	0.0119	(0.0129)
Two parents with mig. back., dummy	0.0050	(0.0076)	0.0119	(0.0110)	-0.0077	(0.0118)	0.0065	(0.0143)	0.0010	(0.0112)
Migration background missing, dummy	0.0043	(0.0050)	-0.0036	(0.0055)	-0.0022	(0.0050)	0.0057	(0.0150)	—	—
Home language German	0.0112	(0.0075)	0.0056	(0.0103)	0.0125	(0.0130)	0.0181	(0.0146)	0.0101	(0.0123)
Home language foreign	-0.0048	(0.0049)	0.0001	(0.0070)	-0.0179*	(0.0076)	0.0026	(0.0095)	-0.0119	(0.0071)
Home language missing	0.0012	(0.0037)	0.0072	(0.0040)	-0.0014	(0.0054)	-0.0028	(0.0095)	-0.0040	(0.0055)
Number of siblings	0.0059	(0.0255)	0.0266	(0.0358)	-0.0400	(0.0395)	0.0120	(0.0604)	-0.0192	(0.0227)
Number of siblings missing	-0.0002	(0.0022)	-0.0010	(0.0023)	0.0027	(0.0035)	0.0011	(0.0065)	—	—
	<i>F</i> -stat	<i>p</i> -value	<i>F</i> -stat	<i>p</i> -value	<i>F</i> -stat	<i>p</i> -value	<i>F</i> -stat	<i>p</i> -value	<i>F</i> -stat	<i>p</i> -value
Testing for joint orthogonality	0.51	0.921	0.92	0.529	0.75	0.706	0.55	0.892	0.41	0.962

Notes: This table reports coefficient estimates of β_{PPL} of regression models outlined in equation 1 (without X) to check the balance of child and family characteristics. The dependent variables are listed in the rows. The results are reported for the sample including all children, and subsamples stratified by mothers' education and predicted pre-reform eligibility. "Low & medium" education refers to lower and medium-secondary school certificates. "High" education refers to upper-secondary school certificates (*Abitur*). *Previously eligible* families likely received benefits 13-24 months after childbirth. *Previously ineligible* families were likely not eligible for parental leave benefits 6-12 months after childbirth (see Section 3.2). Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$. The test for joint orthogonality of the child- and family characteristics is based on the RD and DiD specification. The treatment indicator is moved to the left-hand side. An F-test tests for the joint significance of the socio-economic characteristics (*right-hand-side balancing test*, as described in, e.g., Pei et al., 2018; Bruhn and McKenzie, 2009).

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table A.2: The relation between child development outcomes and child and family characteristics

(1)	(2)	(3)	(4)
Dependent variable:			
Language skills	Soc. emot. stability	Motor skills	School ready

Notes: This table reports multivariate OLS regression results of the child development outcome (column) on the variables listed in the rows. These regressions include the following control variables: examination year-by-county fixed effects, birth months fixed effects and birth cohort fixed effects and dummies for missing variables. Missing values are imputed (zero-category for dummy variables and sample means for continuous variables). Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table A.3: Relationship between gaining eligibility for parental leave benefits and socio-economic characteristics of the families and children

<i>Indep. variable</i>	<i>Dep. variable: Previously ineligible for paid parental leave</i>			
	(1)	(2)	(3)	(4)
High maternal education	0.370*** (0.008)	0.346*** (0.008)	0.346*** (0.008)	0.121*** (0.005)
Girl			-0.004 (0.005)	-0.002 (0.003)
High paternal education				0.150*** (0.006)
No migration background				0.054*** (0.004)
Number of children in the family				-0.068*** (0.002)
Child lives with both parents				0.189*** (0.008)
Child age in months				0.002*** (0.000)
Birth weight in grams $\times 10^{-4}$				0.011 (0.029)
R ²	0.539	0.578	0.578	0.812
N	16,307	16,307	16,307	16,307

Notes: This table reports OLS regression results of the winner-status of the reform on maternal education, and family characteristics related to children's socio-economic background. The outcome variable takes the value one if families were previously ineligible for benefits between 6 and 12 months after childbirth, and zero if families were previously eligible for up to 24 months of benefits (predictions are based on SOEP, details are described in the main part of the paper). The regressions in columns 2-4 include examination year-by-county fixed effects, birth months fixed effects. Robust standard errors are reported in parentheses. All regressions include dummies for missing variables. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on SOEP v32 and school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table A.4: Estimation results for standardised outcomes and composite development index

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All children			Maternal education		Predicted eligibility for parental leave benefits	
				Low/ medium	High	Previously eligible	Previously ineligible
Language skills (z-score)	-0.0121 (0.0241) [0.0714]	-0.0062 (0.0236) [0.1090]	-0.0048 (0.0233) [0.1325]	-0.0492 (0.0341) [0.1321]	0.0115 (0.0402) [0.0920]	0.0191 (0.0428) [0.1587]	0.0071 (0.0427) [0.0813]
Socio-emo. stability (z-score)	-0.0097 (0.0243) [0.0517]	-0.0069 (0.0241) [0.0684]	-0.0050 (0.0239) [0.0872]	-0.0166 (0.0352) [0.0978]	0.0175 (0.0398) [0.0713]	-0.0550 (0.0451) [0.0976]	-0.0015 (0.0431) [0.0782]
Motor skills (z-score)	-0.0236 (0.0243) [0.0398]	-0.0196 (0.0238) [0.0760]	-0.0185 (0.0237) [0.0846]	-0.0273 (0.0348) [0.0879]	-0.0084 (0.0399) [0.0877]	-0.0326 (0.0443) [0.0883]	0.0046 (0.0439) [0.0916]
School readiness (z-score)	0.0241 (0.0242) [0.0425]	0.0302 (0.0237) [0.0830]	0.0330 (0.0233) [0.1124]	0.0268 (0.0347) [0.1177]	0.0356 (0.0332) [0.0655]	0.0265 (0.0487) [0.1201]	0.0555 (0.0345) [0.0592]
Composite development index (z-score)	-0.0053 (0.0160) [0.0691]	-0.0006 (0.0155) [0.1252]	0.0012 (0.0151) [0.1684]	-0.0165 (0.0225) [0.1708]	0.0140 (0.0238) [0.1294]	-0.0105 (0.0294) [0.1794]	0.0164 (0.0257) [0.1275]
N	28,987	28,987	28,987	14,610	9,072	8,458	7,849
<i>Control variables</i>							
Child characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes
Family characteristics	No	No	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the coefficient estimates of the parental leave reform effect (β_{PPL}) on standardised child outcomes. We calculate the composite index of child development as the mean of all z-scores of the four main outcome variables. All regressions are based on the main equation and include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects and dummies for missing variables, as well as the interaction category. The stratification in columns 6 and 7 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. R^2 are reported in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table A.5: Balancing of covariates for varying window sizes around the reform cut-off

	(1)	(2)	(3)	(4)
	Window size without January & December			
	Aug-May	Sep-Apr	Oct-Mar	Nov-Feb
Age at examination in months	-0.0701 (0.0741)	-0.0311 (0.0886)	-0.0702 (0.1116)	-0.0134 (0.1306)
Girl	0.0011 (0.0136)	0.0075 (0.0157)	-0.0030 (0.0193)	0.0260 (0.0277)
Birth weight in grams	0.0005 (0.0016)	0.0012 (0.0019)	0.0011 (0.0023)	-0.0026 (0.0032)
Years child spent in day care	-0.0050 (0.0192)	-0.0028 (0.0222)	0.0004 (0.0274)	0.0309 (0.0393)
Maternal years of schooling	-0.0086 (0.0403)	-0.0317 (0.0467)	0.0098 (0.0572)	0.0832 (0.0813)
Paternal years of schooling	0.0329 (0.0302)	0.0469 (0.0350)	0.0351 (0.0428)	0.0108 (0.0612)
Child lives with both parents	-0.0061 (0.0111)	-0.0001 (0.0127)	-0.0127 (0.0156)	0.0090 (0.0225)
Child lives with one parent	0.0047 (0.0091)	-0.0026 (0.0105)	0.0155 (0.0128)	0.0024 (0.0183)
Child lives in other living arrangements	-0.0022 (0.0064)	-0.0005 (0.0074)	-0.0023 (0.0091)	-0.0068 (0.0134)
At least one parent with mig. back.	0.0157 (0.0117)	0.0171 (0.0135)	0.0212 (0.0167)	0.0453 (0.0235)
German is main language	-0.0122 (0.0100)	-0.0192 (0.0116)	-0.0171 (0.0144)	-0.0334 (0.0205)
Home language foreign	-0.0063 (0.0054)	-0.0033 (0.0062)	0.0022 (0.0077)	0.0128 (0.0112)
Number of children in the family	-0.0002 (0.0284)	0.0092 (0.0327)	0.0396 (0.0403)	0.0647 (0.0588)
N	22,824	16,787	11,011	5,322

Notes: This table reports results of difference-in-differences regressions as outlined in equation 1 on the covariates listed in the rows with varying window sizes around the reform cut-off. The regressions include the following control variables: examination year-by-county fixed effects, birth months fixed effects and birth cohort fixed effects. The regressions exclude the X -vector. Each coefficient estimate stems from a separate regression. Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

Table A.6: Common trend checks

	(1)	(2)	(3)	(4)	(5)
	Baseline	Cohort-specific time trends		Placebo reforms	
		Linear	Quadratic	Placebo cohort	Mar/Apr 2007
<i>All children</i>					
Language skills	-0.0022 (0.0104)	0.0137 (0.0270)	0.0132 (0.0270)	0.0096 (0.0099)	-0.0099 (0.0116)
Socio-emo. stability	-0.0020 (0.0094)	0.0008 (0.0243)	0.0002 (0.0243)	0.0005 (0.0089)	-0.0041 (0.0105)
Motor skills	-0.0070 (0.0090)	0.0086 (0.0234)	0.0081 (0.0234)	-0.0032 (0.0087)	-0.0024 (0.0103)
School readiness	0.0120 (0.0084)	-0.0079 (0.0219)	-0.0085 (0.0219)	0.0058 (0.0082)	-0.0003 (0.0096)
N	28,987	28,987	28,987	31,031	26,036
<i>Mothers' education: Low/medium</i>					
Language skills	-0.0219 (0.0152)	-0.0201 (0.0398)	-0.0215 (0.0398)	-0.0097 (0.0145)	-0.0032 (0.0172)
Socio-emo. stability	-0.0065 (0.0139)	0.0072 (0.0364)	0.0074 (0.0365)	0.0098 (0.0132)	-0.0083 (0.0158)
Motor skills	-0.0103 (0.0132)	0.0080 (0.0349)	0.0085 (0.0350)	-0.0116 (0.0127)	-0.0136 (0.0153)
School readiness	0.0097 (0.0126)	-0.0159 (0.0330)	-0.0157 (0.0331)	0.0063 (0.0123)	-0.0050 (0.0145)
N	14,610	14,610	14,610	15,636	12,948
<i>Mothers' education: High</i>					
Language skills	0.0051 (0.0179)	0.0078 (0.0462)	0.0092 (0.0461)	0.0231 (0.0168)	-0.0027 (0.0199)
Socio-emo. stability	0.0069 (0.0157)	-0.0200 (0.0397)	-0.0211 (0.0397)	-0.0115 (0.0147)	-0.0081 (0.0170)
Motor skills	-0.0032 (0.0151)	-0.0132 (0.0385)	-0.0129 (0.0384)	0.0110 (0.0144)	0.0323 (0.0172)
School readiness	0.0129 (0.0120)	0.0028 (0.0307)	0.0015 (0.0306)	0.0012 (0.0115)	-0.0027 (0.0139)
N	9,072	9,072	9,072	9,839	8,250
<i>Previously eligible</i>					
Language skills	0.0085 (0.0191)	0.0812 (0.0496)	0.0816 (0.0496)	0.0255 (0.0185)	-0.0418 (0.0216)
Socio-emo. stability	-0.0217 (0.0178)	-0.0252 (0.0453)	-0.0278 (0.0453)	-0.0002 (0.0167)	0.0189 (0.0201)
Motor skills	-0.0124 (0.0168)	0.0344 (0.0435)	0.0339 (0.0436)	0.0031 (0.0165)	-0.0308 (0.0197)
School readiness	0.0096 (0.0177)	0.0078 (0.0462)	0.0060 (0.0462)	-0.0001 (0.0172)	0.0091 (0.0202)
N	8,458	8,458	8,458	8,951	7,491
<i>Previously ineligible</i>					
Language skills	0.0032 (0.0190)	-0.0593 (0.0489)	-0.0590 (0.0489)	0.0105 (0.0179)	0.0272 (0.0211)
Socio-emo. stability	-0.0006 (0.0170)	-0.0464 (0.0433)	-0.0468 (0.0432)	-0.0171 (0.0162)	-0.0034 (0.0183)
Motor skills	0.0018 (0.0166)	0.0189 (0.0433)	0.0189 (0.0433)	0.0059 (0.0158)	0.0246 (0.0187)
School readiness	0.0201 (0.0125)	-0.0030 (0.0325)	-0.0033 (0.0326)	0.0151 (0.0120)	0.0116 (0.0142)
N	7,849	7,849	7,849	8,582	7,206

Notes: This table reports the results of sensitivity checks to alternative model specifications for the common trend assumption for the full sample, and subsamples stratified by mothers' education. It also reports the results from placebo regressions. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

Table A.7: Robustness checks separately by subgroups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	Window size without January & December				Including children born	Including children born	Nonlinear models (marginal effects)	
	(Jul-Jun)	Aug-May	Sep-Apr	Oct-Mar	Nov-Feb	7/2004-6/2005	Jan & Dec	Probit	Logit
<i>Mothers' education: Low/medium</i>									
Language skills	-0.0219 (0.0152)	-0.0149 (0.0169)	-0.0231 (0.0194)	-0.0083 (0.0239)	-0.0311 (0.0341)	-0.0236 (0.0132)	-0.0123 (0.0138)	-0.0198 (0.0148)	-0.0172 (0.0148)
Socio-emot. stability	-0.0065 (0.0139)	-0.0051 (0.0153)	-0.0085 (0.0177)	0.0150 (0.0219)	0.0337 (0.0312)	-0.0059 (0.0120)	-0.0125 (0.0126)	-0.0083 (0.0135)	-0.0083 (0.0135)
Motor skills	-0.0103 (0.0132)	-0.0021 (0.0146)	-0.0109 (0.0169)	-0.0035 (0.0209)	0.0015 (0.0304)	-0.0096 (0.0114)	0.0016 (0.0119)	-0.0084 (0.0129)	-0.0096 (0.0130)
School readiness	0.0097 (0.0126)	0.0064 (0.0138)	0.0040 (0.0158)	0.0042 (0.0193)	0.0230 (0.0278)	-0.0002 (0.0108)	0.0087 (0.0114)	0.0075 (0.0125)	0.0080 (0.0126)
N	14610	11475	8415	5527	2662	22310	17488	14599	14599
<i>Mothers' education: High</i>									
Language skills	0.0051 (0.0179)	0.0021 (0.0198)	-0.0012 (0.0228)	0.0187 (0.0283)	0.0256 (0.0416)	-0.0075 (0.0153)	0.0071 (0.0162)	0.0093 (0.0174)	0.0134 (0.0175)
Socio-emot. stability	0.0069 (0.0157)	0.0012 (0.0171)	-0.0017 (0.0195)	-0.0018 (0.0235)	-0.0085 (0.0348)	0.0136 (0.0135)	-0.0024 (0.0142)	0.0062 (0.0151)	0.0064 (0.0153)
Motor skills	-0.0032 (0.0151)	-0.0150 (0.0167)	-0.0119 (0.0190)	-0.0126 (0.0229)	0.0056 (0.0330)	-0.0088 (0.0127)	-0.0043 (0.0137)	-0.0108 (0.0154)	-0.0126 (0.0157)
School readiness	0.0129 (0.0120)	0.0036 (0.0132)	0.0090 (0.0149)	0.0139 (0.0178)	0.0099 (0.0254)	0.0101 (0.0103)	0.0061 (0.0110)	0.0162 (0.0124)	0.0156 (0.0128)
N	9072	7161	5298	3430	1625	13985	10710	8961	8961
<i>Previously eligible</i>									
Language skills	0.0085 (0.0191)	0.0132 (0.0212)	0.0286 (0.0245)	0.0466 (0.0298)	0.0743 (0.0430)	0.0016 (0.0164)	0.0106 (0.0173)	0.0144 (0.0186)	0.0132 (0.0186)
Socio-emot. stability	-0.0217 (0.0178)	-0.0244 (0.0195)	-0.0244 (0.0224)	-0.0339 (0.0272)	0.0034 (0.0380)	-0.0226 (0.0151)	-0.0378* (0.0161)	-0.0227 (0.0172)	-0.0224 (0.0173)
Motor skills	-0.0124 (0.0168)	0.0028 (0.0187)	0.0016 (0.0214)	0.0047 (0.0263)	0.0242 (0.0378)	-0.0034 (0.0144)	-0.0052 (0.0152)	-0.0132 (0.0163)	-0.0119 (0.0165)
School readiness	0.0096 (0.0177)	0.0068 (0.0194)	0.0050 (0.0223)	-0.0016 (0.0275)	0.0565 (0.0397)	0.0008 (0.0150)	0.0044 (0.0161)	0.0096 (0.0174)	0.0088 (0.0176)
N	8458	6675	4905	3261	1622	13991	10141	8409	8409
<i>Previously ineligible</i>									
Language skills	0.0032 (0.0190)	-0.0083 (0.0212)	-0.0263 (0.0243)	-0.0194 (0.0303)	-0.0075 (0.0436)	-0.0033 (0.0163)	0.0008 (0.0172)	0.0086 (0.0184)	0.0112 (0.0186)
Socio-emot. stability	-0.0006 (0.0170)	-0.0122 (0.0185)	-0.0177 (0.0214)	-0.0216 (0.0261)	0.0125 (0.0362)	0.0104 (0.0146)	-0.0029 (0.0153)	-0.0022 (0.0167)	-0.0009 (0.0168)
Motor skills	0.0018 (0.0166)	-0.0060 (0.0185)	0.0036 (0.0212)	0.0024 (0.0262)	-0.0138 (0.0377)	-0.0068 (0.0141)	-0.0026 (0.0150)	-0.0025 (0.0168)	-0.0055 (0.0171)
School readiness	0.0201 (0.0125)	0.0127 (0.0136)	0.0158 (0.0154)	0.0058 (0.0189)	0.0172 (0.0266)	0.0154 (0.0108)	0.0127 (0.0112)	0.0242 (0.0129)	0.0250 (0.0134)
N	7849	6189	4541	2956	1430	11717	9303	7830	7830

Notes: This table reports the results from robustness checks of the reform effect of the parental leave reform on child outcomes stratified by mothers' education. The window size around the reform cut-off and definitions of the control group are varied. Further, the robustness to non-linear model specifications is tested. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. Robust standard errors are reported in parentheses.

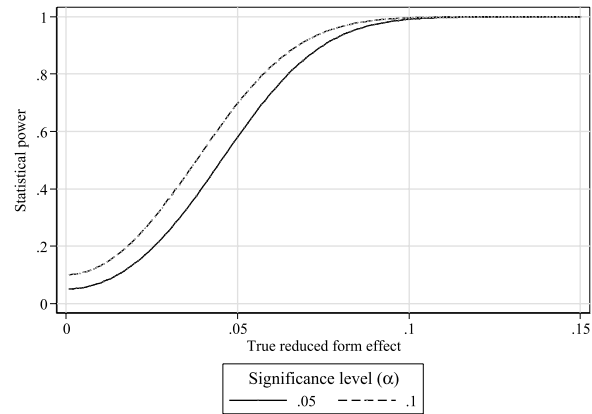
** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

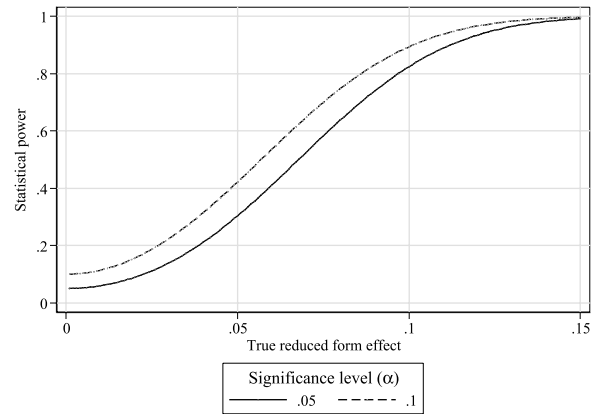
Online Appendix

Figure B.1: Power analysis

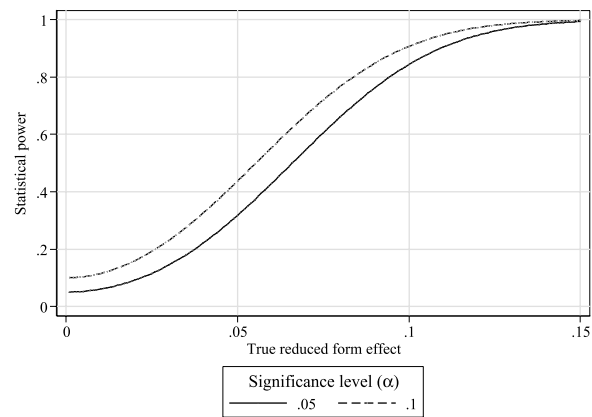
Panel A: All



Panel B: Low/medium maternal education



Panel C: High maternal education



Notes: The figure plots the statistical power for school readiness with a type I error probabilities of 5% and 10%. For the calculations, we specify (separately for each group) the number of observations, the conditional standard deviation of the treatment effect dummy and the conditional standard deviation of the standardised school readiness score.

Source: Own calculations using STATA - power oneslope - command.

Table B.1: Effect heterogeneity on further child outcomes by daycare availability

	(1)	(2)	(3)	(4)	(5)
	Dep. variable: School readiness				
	Mothers' education		Predicted eligibility for parental leave benefits		
	All	Low/med.	High	Previously eligible	Previously ineligible
<i>Heterogeneity by share of children below age 3 in formal day care (county-year level, min. 8%, max. 27%)</i>					
Language skills					
PPL reform effect	-0.0012 (0.0104)	-0.0213 (0.0152)	0.0114 (0.0186)	0.0039 (0.0198)	0.0086 (0.0195)
PPL reform effect · childcare share in %	-0.0038 (0.0020)	-0.0024 (0.0031)	-0.0048 (0.0041)	-0.0047 (0.0033)	-0.0043 (0.0043)
Socio-emotional stability					
PPL reform effect	-0.0020 (0.0094)	-0.0072 (0.0139)	0.0114 (0.0159)	-0.0249 (0.0185)	-0.0053 (0.0170)
PPL reform effect · childcare share in %	0.0002 (0.0017)	0.0033 (0.0026)	-0.0035 (0.0032)	-0.0029 (0.0031)	0.0041 (0.0033)
Motor skills					
PPL reform effect	-0.0069 (0.0090)	-0.0109 (0.0132)	-0.0003 (0.0153)	-0.0166 (0.0173)	0.0011 (0.0169)
PPL reform effect · childcare share in %	-0.0004 (0.0016)	0.0031 (0.0025)	-0.0023 (0.0030)	-0.0044 (0.0029)	0.0005 (0.0035)

Notes: This table reports the estimation results of the parental leave reform effect on language skills, socio-emotional stability and motor skills. Regressions are based on the main model, including an additional interaction term of the treatment indicator with the heterogeneity category, and the baseline variable of the interacted category. The stratification in columns 4 and 5 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.2: Effect heterogeneity on further child outcomes by female labour force participation

	(1)	(2)	(3)	(4)	(5)
	Dep. variable: School readiness				
		Mothers' education		Predicted eligibility for parental leave benefits	
	All	Low/med.	High	Previously eligible	Previously ineligible
<i>Heterogeneity by female labour force participation rate (county level, 2011, min. 70%, max. 79%)</i>					
Language skills					
PPL reform effect	-0.0024 (0.0104)	-0.0221 (0.0152)	0.0064 (0.0180)	0.0142 (0.0197)	0.0056 (0.0191)
PPL reform effect · female LFP share in %	-0.0033 (0.0026)	-0.0050 (0.0037)	-0.0025 (0.0045)	0.0069 (0.0053)	-0.0041 (0.0048)
Socio-emotional stability					
PPL reform effect	-0.0018 (0.0094)	-0.0065 (0.0139)	0.0062 (0.0155)	-0.0160 (0.0185)	-0.0029 (0.0168)
PPL reform effect · female LFP share in %	0.0020 (0.0023)	0.0008 (0.0032)	0.0014 (0.0038)	0.0069 (0.0049)	0.0039 (0.0040)
Motor skills					
PPL reform effect	-0.0070 (0.0090)	-0.0103 (0.0132)	-0.0014 (0.0152)	-0.0094 (0.0173)	0.0001 (0.0168)
PPL reform effect · female LFP share in %	-0.0001 (0.0022)	0.0002 (0.0032)	-0.0036 (0.0036)	0.0036 (0.0047)	0.0029 (0.0041)

Notes: This table reports the estimation results of the parental leave reform effect on language skills, socio-emotional stability and motor skills. Regressions are based on the main model, including an additional interaction term of the treatment indicator with the heterogeneity category, and the baseline variable of the interacted category. The stratification in columns 4 and 5 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.3: Effect heterogeneity on further child outcomes by paternal leave share

	(1)	(2)	(3)	(4)	(5)
	Dep. variable: School readiness				
		Mothers' education		Predicted eligibility for parental leave benefits	
	All	Low/med.	High	Previously eligible	Previously ineligible
<i>Heterogeneity by share of fathers taking parental leave (county level, 2008, min. 5.8%, max. 13.3%)</i>					
Language skills					
PPL reform effect	-0.0026 (0.0104)	-0.0218 (0.0153)	0.0052 (0.0179)	0.0088 (0.0191)	0.0026 (0.0190)
PPL reform effect · paternal leave share in %	-0.0040 (0.0046)	0.0003 (0.0067)	-0.0062 (0.0071)	-0.0142 (0.0092)	-0.0100 (0.0079)
Socio-emotional stability					
PPL reform effect	-0.0020 (0.0094)	-0.0055 (0.0140)	0.0069 (0.0157)	-0.0214 (0.0178)	-0.0005 (0.0170)
PPL reform effect · paternal leave share in %	-0.0005 (0.0039)	0.0039 (0.0056)	0.0008 (0.0064)	-0.0133 (0.0083)	0.0024 (0.0070)
Motor skills					
PPL reform effect	-0.0070 (0.0090)	-0.0094 (0.0132)	-0.0032 (0.0151)	-0.0121 (0.0168)	0.0016 (0.0166)
PPL reform effect · paternal leave share in %	0.0001 (0.0039)	0.0035 (0.0058)	0.0030 (0.0061)	-0.0118 (0.0081)	-0.0030 (0.0071)

Notes: This table reports the estimation results of the parental leave reform effect on language skills, socio-emotional stability and motor skills. Regressions are based on the main model, including an additional interaction term of the treatment indicator with the heterogeneity category, and the baseline variable of the interacted category. The stratification in columns 4 and 5 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.4: Effect heterogeneity on further child outcomes by population density in the area

	(1)	(2)	(3)	(4)	(5)
	Dep. variable: School readiness				
		Mothers' education		Predicted eligibility for parental leave benefits	
	All	Low/med.	High	Previously eligible	Previously ineligible
<i>Heterogeneity by population density (county level, 2006, rural if ≤ 125 inhabitants/km²)</i>					
Language skills					
PPL reform effect · rural	0.0406 (0.0371)	0.0348 (0.0256)	0.0549 (0.0361)	0.0311 (0.0290)	0.0687 (0.0373)
PPL reform effect · urban	-0.0134 (0.0114)	-0.0317 (0.0167)	-0.0039 (0.0188)	-0.0033 (0.0228)	-0.0087 (0.0200)
Socio-emotional stability					
PPL reform effect · rural	-0.0007 (0.0151)	-0.0158 (0.0228)	0.0213 (0.0297)	0.0274 (0.0267)	0.0034 (0.0308)
PPL reform effect · urban	-0.0018 (0.0103)	-0.0021 (0.0151)	0.0034 (0.0165)	-0.0321 (0.0210)	-0.0012 (0.0179)
Motor skills					
PPL reform effect · rural	-0.0063 (0.0144)	-0.0321 (0.0215)	0.0352 (0.0282)	0.0224 (0.0255)	-0.0306 (0.0307)
PPL reform effect · urban	-0.0053 (0.0098)	-0.0013 (0.0145)	-0.0090 (0.0158)	-0.0209 (0.0198)	0.0070 (0.0174)

Notes: This table reports the estimation results of the parental leave reform effect on language skills, socio-emotional stability and motor skills. Regressions are based on the main model, including an additional interaction term of the treatment indicator with the heterogeneity category, and the baseline variable of the interacted category. The stratification in columns 4 and 5 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.5: Effect heterogeneity on further child outcomes by migration background

	(1)	(2)	(3)	(4)	(5)
	Dep. variable: School readiness				
		Mothers' education		Predicted eligibility for parental leave benefits	
	All	Low/med.	High	Previously eligible	Previously ineligible
<i>Heterogeneity by migration background (sample mean: 21%)</i>					
Language skills					
PPL reform effect · Migration background	-0.0179 (0.0165)	-0.0471 (0.0333)	-0.0023 (0.0299)	-0.0085 (0.0352)	0.0007 (0.0315)
PPL reform effect · No migration background	-0.0044 (0.0113)	-0.0177 (0.0164)	0.0055 (0.0188)	0.0051 (0.0194)	-0.0112 (0.0232)
Socio-emotional stability					
PPL reform effect · Migration background	-0.0246 (0.0145)	-0.0258 (0.0208)	-0.0527 (0.0362)	-0.0488 (0.0301)	-0.0236 (0.0279)
PPL reform effect · No migration background	0.0075 (0.0102)	0.0075 (0.0151)	0.0159 (0.0164)	0.0075 (0.0174)	-0.0124 (0.0220)
Motor skills					
PPL reform effect · Migration background	-0.0114 (0.0142)	-0.0029 (0.0204)	-0.0322 (0.0256)	-0.0268 (0.0298)	-0.0143 (0.0274)
PPL reform effect · No migration background	-0.0053 (0.0098)	-0.0054 (0.0144)	-0.0028 (0.0160)	0.0066 (0.0171)	-0.0104 (0.0211)

Notes: This table reports the estimation results of the parental leave reform effect on language skills, socio-emotional stability and motor skills. Regressions are based on the main model, including an additional interaction term of the treatment indicator with the heterogeneity category, and the baseline variable of the interacted category. The stratification in columns 4 and 5 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.6: Effect heterogeneity on further child outcomes by number of children

	(1)	(2)	(3)	(4)	(5)
	Dep. variable: School readiness				
		Mothers' education		Predicted eligibility for parental leave benefits	
	All	Low/med.	High	Previously eligible	Previously ineligible
<i>Heterogeneity by number of children</i>					
Language skills					
PPL reform effect · Single child	0.0044 (0.0146)	0.0041 (0.0214)	-0.0031 (0.0263)	0.0106 (0.0252)	0.0014 (0.0243)
PPL reform effect · More children	-0.0037 (0.0110)	-0.0289 (0.0161)	0.0077 (0.0188)	0.0057 (0.0205)	0.0039 (0.0207)
Socio-emo. stability					
PPL reform effect · Single child	0.0017 (0.0135)	0.0077 (0.0198)	0.0128 (0.0226)	-0.0275 (0.0241)	-0.0101 (0.0216)
PPL reform effect · More children	-0.0044 (0.0099)	-0.0098 (0.0147)	0.0028 (0.0164)	-0.0261 (0.0189)	0.0043 (0.0183)
Motor skills					
PPL reform effect · Single child	-0.0065 (0.0131)	-0.0033 (0.0192)	0.0141 (0.0228)	-0.0219 (0.0230)	0.0138 (0.0215)
PPL reform effect · More children	-0.0089 (0.0094)	-0.0152 (0.0139)	-0.0056 (0.0158)	-0.0161 (0.0180)	-0.0044 (0.0180)
School readiness					
PPL reform effect · Single child	0.0215 (0.0120)	0.0341 (0.0274)	0.0229 (0.0181)	-0.0032 (0.0236)	0.0331 (0.0263)
PPL reform effect · More children	0.0089 (0.0089)	0.0014 (0.0134)	0.0094 (0.0126)	0.0127 (0.0188)	0.0132 (0.0136)

Notes: This table reports the estimation results of the parental leave reform effect on language skills, socio-emotional stability and motor skills. Regressions are based on the main model, including an additional interaction term of the treatment indicator with the heterogeneity category, and the baseline variable of the interacted category. The stratification in columns 4 and 5 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.7: Heterogeneity analysis by gender, parental education and pre-reform eligibility - interacted models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Mothers' education		Fathers' education		Predicted eligibility for parental leave benefits	
	All	Low/med.	High	Low/med.	High	Previously eligible	Previously ineligible
Language skills							
Reform · Girls	0.0096 (0.0119)	-0.0181 (0.0176)	0.0229 (0.0201)	0.0028 (0.0186)	-0.0023 (0.0202)	0.0181 (0.0218)	0.0200 (0.0213)
Reform · Boys	-0.0133 (0.0121)	-0.0255 (0.0177)	-0.0116 (0.0211)	-0.0168 (0.0187)	-0.0303 (0.0212)	-0.0009 (0.0224)	-0.0125 (0.0225)
Socio-emo. stability							
Reform · Girls	-0.0029 (0.0105)	-0.0100 (0.0157)	0.0144 (0.0171)	-0.0115 (0.0165)	0.0097 (0.0169)	-0.0302 (0.0201)	0.0013 (0.0184)
Reform · Boys	-0.0012 (0.0112)	-0.0033 (0.0165)	-0.0002 (0.0187)	-0.0182 (0.0174)	0.0148 (0.0185)	-0.0136 (0.0210)	-0.0023 (0.0203)
Motor skills							
Reform · Girls	0.0013 (0.0097)	-0.0000 (0.0145)	0.0171 (0.0157)	0.0107 (0.0149)	-0.0039 (0.0163)	-0.0161 (0.0184)	0.0087 (0.0176)
Reform · Boys	-0.0150 (0.0111)	-0.0201 (0.0162)	-0.0223 (0.0189)	-0.0293 (0.0171)	-0.0161 (0.0189)	-0.0090 (0.0207)	-0.0047 (0.0206)
School readiness							
Reform · Girls	0.0242 (0.0292)	0.0222 (0.0239)	0.0271 (0.0227)	0.0231 (0.0243)	0.0273 (0.0231)	0.0128 (0.0297)	0.0409 (0.0327)
Reform · Boys	0.0003 (0.0202)	-0.0021 (0.0253)	-0.0005 (0.0250)	-0.0168 (0.0259)	0.0135 (0.0254)	0.0064 (0.0211)	0.0007 (0.0259)
N	28,987	14,610	9,072	13,027	9,069	8,458	7,849

Notes: This table reports the estimation results of the parental leave reform on child outcomes on samples stratified by different definitions of parental education, and pre-reform eligibility for parental leave benefits. The reform indicator is interacted with dummies indicating girls and boys. All regressions include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables and control variables for child and family characteristics. The stratification in columns 6 and 7 is based on pre-reform eligibility predictions for parents who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth. *Previously ineligible* parents were likely not eligible for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.8: Logit estimation on pre-reform eligibility for paid parental leave 13-24 months

<i>Independent variable</i>	(1) Coefficient	(2) s.e.
Single mother	2.161***	(0.753)
Number of children	0.417**	(0.186)
Number of children \times Single mother	-0.650*	(0.352)
Maternal educ. middle	-0.396	(0.740)
Maternal educ. high	-0.473	(0.793)
Paternal educ. middle	-0.204	(0.876)
Paternal educ. high	-1.115	(1.460)
Migration background	-0.006	(0.449)
N	263	

Notes: This table reports logit estimation results of pre-reform eligibility (13-24 months) for paid parental leave before the reform in 2007. The sign of the coefficient estimates are consistent with the institutional rules: for instance, the probability for eligibility increases with the number of children and single motherhood, while it decreases with the education level of the parents. Robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: Own calculations based on SOEP v30.

Table B.9: Regression-discontinuity based specifications

Specification:	Sample stratified by maternal education						Sample stratified by predicted pre-reform eligibility for parental leave benefits			
	All		Low & medium		High		Previously eligible		Previously ineligible	
	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.
Dep. variables: Language skills										
RD analysis										
Linear, 6 Months-window	-0.0037	(0.0144)	-0.0020	(0.0212)	-0.0036	(0.0243)	0.0146	(0.0266)	-0.0108	(0.0255)
Linear, 4 Months-window	0.0207	(0.0181)	0.0252	(0.0267)	0.0207	(0.0307)	0.0444	(0.0337)	0.0278	(0.0320)
Linear, 2 Months-window	0.0196	(0.0304)	0.0690	(0.0445)	-0.0123	(0.0520)	0.0486	(0.0567)	-0.0191	(0.0540)
Second order polynomial	-0.0018	(0.0142)	0.0019	(0.0210)	-0.0073	(0.0241)	0.0177	(0.0263)	-0.0164	(0.0252)
Third order polynomial	0.0207	(0.0194)	0.0219	(0.0285)	0.0199	(0.0329)	0.0427	(0.0360)	0.0137	(0.0343)
Controlling for X-characteristics	0.0005	(0.0139)	0.0071	(0.0205)	-0.0100	(0.0241)	0.0174	(0.0257)	-0.0102	(0.0253)
Donut-RD analysis (excluding January and February)										
Linear, 6 Months-window	-0.0267	(0.0209)	-0.0533	(0.0310)	-0.0123	(0.0352)	-0.0060	(0.0386)	-0.0309	(0.0373)
Linear, 4 Months-window	0.0143	(0.0332)	-0.0362	(0.0493)	0.0405	(0.0559)	0.0467	(0.0618)	0.0566	(0.0594)
Linear, 2 Months-window	-0.0407*	(0.0181)	-0.0713**	(0.0268)	-0.0336	(0.0315)	-0.0194	(0.0330)	-0.0186	(0.0335)
Second order polynomial	-0.0255	(0.0209)	-0.0503	(0.0309)	-0.0190	(0.0351)	-0.0012	(0.0383)	-0.0399	(0.0372)
Third order polynomial	0.0013	(0.0399)	-0.0913	(0.0591)	0.0444	(0.0678)	0.0437	(0.0733)	0.0170	(0.0721)
Controlling for X-characteristics	-0.0082	(0.0203)	-0.0311	(0.0303)	-0.0104	(0.0349)	0.0121	(0.0376)	-0.0266	(0.0369)
Difference-in-discontinuities										
Linear, 6 Months-window	0.0205	(0.0194)	0.0162	(0.0283)	0.0214	(0.0324)	0.0495	(0.0354)	-0.0260	(0.0340)
Linear, 4 Months-window	0.0118	(0.0219)	0.0127	(0.0318)	0.0339	(0.0362)	0.0368	(0.0401)	-0.0088	(0.0381)
Linear, 2 Months-window	-0.0002	(0.0308)	-0.0298	(0.0445)	0.0763	(0.0501)	-0.0012	(0.0568)	0.0200	(0.0533)
Linear, 6 Months-window w/o Dec/Jan	0.0127	(0.0280)	-0.0273	(0.0410)	0.0240	(0.0469)	0.0552	(0.0509)	-0.0516	(0.0495)
Linear, 4 Months-window w/o Dec/Jan	-0.0035	(0.0364)	-0.0223	(0.0533)	0.0142	(0.0610)	0.0485	(0.0658)	-0.0429	(0.0651)
Linear, 2 Months-window w/o Dec/Jan	-0.0066	(0.0212)	-0.0360	(0.0308)	-0.0020	(0.0362)	0.0385	(0.0381)	-0.0280	(0.0386)
Dep. variables: Socio-emot. stability										
RD analysis										
Linear, 6 Months-window	-0.0308*	(0.0124)	-0.0257	(0.0185)	-0.0205	(0.0204)	-0.0484*	(0.0238)	-0.0417	(0.0214)
Linear, 4 Months-window	-0.0252	(0.0155)	-0.0086	(0.0230)	-0.0235	(0.0256)	-0.0602*	(0.0298)	-0.0239	(0.0263)
Linear, 2 Months-window	-0.0593*	(0.0255)	-0.0619	(0.0379)	-0.0443	(0.0430)	-0.0792	(0.0491)	-0.0658	(0.0432)
Second order polynomial	-0.0262*	(0.0123)	-0.0206	(0.0185)	-0.0163	(0.0202)	-0.0451	(0.0238)	-0.0363	(0.0212)
Third order polynomial	-0.0370*	(0.0168)	-0.0234	(0.0250)	-0.0277	(0.0276)	-0.0771*	(0.0326)	-0.0281	(0.0287)
Controlling for X-characteristics	-0.0293*	(0.0121)	-0.0291	(0.0180)	-0.0182	(0.0203)	-0.0531*	(0.0234)	-0.0357	(0.0213)
Donut-RD analysis (excluding January and February)										
Linear, 6 Months-window	-0.0095	(0.0179)	-0.0028	(0.0271)	-0.0090	(0.0297)	-0.0192	(0.0343)	-0.0215	(0.0315)
Linear, 4 Months-window	0.0250	(0.0283)	0.0707	(0.0427)	-0.0033	(0.0477)	-0.0382	(0.0539)	0.0530	(0.0486)
Linear, 2 Months-window	0.0019	(0.0153)	0.0139	(0.0233)	-0.0116	(0.0263)	-0.0165	(0.0287)	0.0114	(0.0267)
Second order polynomial	-0.0012	(0.0179)	0.0073	(0.0270)	-0.0039	(0.0295)	-0.0125	(0.0344)	-0.0135	(0.0312)
Third order polynomial	0.0142	(0.0341)	0.0578	(0.0514)	-0.0147	(0.0573)	-0.0589	(0.0645)	0.0650	(0.0587)
Controlling for X-characteristics	-0.0035	(0.0176)	-0.0044	(0.0265)	-0.0002	(0.0296)	-0.0195	(0.0338)	-0.0111	(0.0313)
Difference-in-discontinuities										
Linear, 6 Months-window	-0.0278	(0.0172)	-0.0145	(0.0255)	-0.0361	(0.0282)	-0.0637*	(0.0322)	-0.0337	(0.0304)
Linear, 4 Months-window	-0.0446*	(0.0195)	-0.0219	(0.0288)	-0.0595	(0.0322)	-0.0901*	(0.0367)	-0.0382	(0.0341)
Linear, 2 Months-window	-0.0843**	(0.0281)	-0.0560	(0.0411)	-0.0735	(0.0469)	-0.1884**	(0.0526)	-0.0163	(0.0481)
Linear, 6 Months-window w/o Dec/Jan	0.0041	(0.0248)	0.0089	(0.0371)	-0.0157	(0.0403)	-0.0165	(0.0460)	-0.0471	(0.0440)
Linear, 4 Months-window w/o Dec/Jan	0.0018	(0.0323)	0.0254	(0.0482)	-0.0497	(0.0531)	-0.0230	(0.0594)	-0.0523	(0.0570)
Linear, 2 Months-window w/o Dec/Jan	0.0080	(0.0185)	0.0196	(0.0276)	-0.0280	(0.0308)	-0.0055	(0.0341)	0.0002	(0.0323)

Notes: This table reports coefficient estimates of β_{PPL} of different regression models. The dependent variables is school readiness. The results are reported for the sample including all children, and subsamples stratified by mothers' education and predicted pre-reform eligibility. "Low & medium" education refers to lower and medium-secondary school certificates. "High" education refers to upper-secondary school certificates (*Abitur*). *Previously eligible* families likely received benefits 13-24 months after childbirth. *Previously ineligible* families were likely not eligible for parental leave benefits 6-12 months after childbirth (see Section 3.2). The regressions include county-by-examination year fixed effects. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.10: Regression-discontinuity based specifications

Specification:	Sample stratified by maternal education						Sample stratified by predicted pre-reform eligibility for parental leave benefits			
	All		Low & medium		High		Previously eligible		Previously ineligible	
	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.	β_{PPL}	s.e.
Dep. variables: Motor skills										
RD analysis										
Linear, 6 Months-window	0.0046	(0.0120)	0.0307	(0.0179)	-0.0431*	(0.0195)	0.0240	(0.0229)	-0.0095	(0.0212)
Linear, 4 Months-window	0.0168	(0.0150)	0.0496*	(0.0223)	-0.0439	(0.0243)	0.0263	(0.0290)	-0.0203	(0.0262)
Linear, 2 Months-window	0.0053	(0.0252)	0.0546	(0.0378)	-0.0580	(0.0406)	0.0137	(0.0490)	-0.0490	(0.0433)
Second order polynomial	0.0067	(0.0119)	0.0356*	(0.0178)	-0.0450*	(0.0196)	0.0262	(0.0228)	-0.0124	(0.0212)
Third order polynomial	0.0124	(0.0162)	0.0449	(0.0239)	-0.0490	(0.0271)	0.0150	(0.0312)	-0.0226	(0.0286)
Controlling for X-characteristics	0.0051	(0.0117)	0.0300	(0.0175)	-0.0420*	(0.0193)	0.0201	(0.0226)	-0.0011	(0.0208)
Donut-RD analysis (excluding January and February)										
Linear, 6 Months-window	0.0048	(0.0173)	0.0107	(0.0264)	-0.0215	(0.0279)	0.0326	(0.0328)	0.0201	(0.0314)
Linear, 4 Months-window	0.0349	(0.0273)	0.0354	(0.0417)	-0.0028	(0.0431)	0.0393	(0.0522)	0.0286	(0.0490)
Linear, 2 Months-window	-0.0138	(0.0151)	-0.0137	(0.0233)	-0.0452	(0.0240)	-0.0040	(0.0284)	-0.0278	(0.0275)
Second order polynomial	0.0082	(0.0174)	0.0163	(0.0265)	-0.0213	(0.0281)	0.0371	(0.0330)	0.0153	(0.0316)
Third order polynomial	0.0297	(0.0332)	0.0031	(0.0506)	0.0121	(0.0531)	0.0222	(0.0630)	0.0372	(0.0598)
Controlling for X-characteristics	0.0098	(0.0170)	0.0131	(0.0259)	-0.0197	(0.0274)	0.0287	(0.0323)	0.0364	(0.0307)
Difference-in-discontinuities										
Linear, 6 Months-window	0.0260	(0.0165)	0.0413	(0.0243)	-0.0024	(0.0272)	0.0458	(0.0308)	0.0019	(0.0301)
Linear, 4 Months-window	0.0230	(0.0186)	0.0455	(0.0272)	-0.0028	(0.0306)	0.0259	(0.0346)	-0.0085	(0.0339)
Linear, 2 Months-window	0.0281	(0.0263)	0.0618	(0.0382)	-0.0146	(0.0428)	0.0299	(0.0488)	0.0349	(0.0487)
Linear, 6 Months-window w/o Dec/Jan	0.0158	(0.0239)	0.0119	(0.0357)	0.0050	(0.0389)	0.0374	(0.0441)	0.0286	(0.0441)
Linear, 4 Months-window w/o Dec/Jan	0.0063	(0.0311)	-0.0013	(0.0463)	0.0187	(0.0510)	-0.0016	(0.0569)	-0.0109	(0.0578)
Linear, 2 Months-window w/o Dec/Jan	0.0057	(0.0181)	0.0005	(0.0270)	-0.0027	(0.0297)	0.0127	(0.0332)	-0.0024	(0.0342)
Dep. variables: School readiness										
RD analysis										
Linear, 6 Months-window	-0.0077	(0.0115)	-0.0016	(0.0174)	-0.0061	(0.0156)	-0.0083	(0.0245)	-0.0158	(0.0159)
Linear, 4 Months-window	0.0061	(0.0144)	0.0191	(0.0217)	-0.0171	(0.0195)	0.0036	(0.0309)	-0.0251	(0.0192)
Linear, 2 Months-window	0.0066	(0.0242)	0.0227	(0.0364)	-0.0126	(0.0324)	0.0344	(0.0519)	-0.0401	(0.0308)
Second order polynomial	-0.0026	(0.0114)	0.0050	(0.0172)	-0.0018	(0.0154)	-0.0045	(0.0243)	-0.0105	(0.0157)
Third order polynomial	0.0004	(0.0156)	0.0107	(0.0234)	-0.0099	(0.0211)	-0.0157	(0.0335)	-0.0230	(0.0208)
Controlling for X-characteristics	-0.0024	(0.0110)	0.0035	(0.0166)	-0.0052	(0.0152)	-0.0052	(0.0237)	-0.0117	(0.0156)
Donut-RD analysis (excluding January and February)										
Linear, 6 Months-window	-0.0091	(0.0165)	-0.0140	(0.0250)	0.0072	(0.0227)	-0.0186	(0.0347)	0.0002	(0.0245)
Linear, 4 Months-window	0.0161	(0.0257)	0.0182	(0.0391)	-0.0097	(0.0356)	-0.0253	(0.0544)	-0.0102	(0.0373)
Linear, 2 Months-window	-0.0092	(0.0140)	-0.0168	(0.0214)	-0.0125	(0.0187)	-0.0390	(0.0300)	-0.0122	(0.0202)
Second order polynomial	0.0004	(0.0165)	-0.0021	(0.0251)	0.0138	(0.0226)	-0.0096	(0.0348)	0.0049	(0.0244)
Third order polynomial	0.0154	(0.0313)	0.0025	(0.0477)	0.0212	(0.0431)	-0.0651	(0.0662)	-0.0087	(0.0455)
Controlling for X-characteristics	0.0049	(0.0159)	-0.0001	(0.0241)	0.0129	(0.0224)	-0.0090	(0.0336)	0.0071	(0.0242)
Difference-in-discontinuities										
Linear, 6 Months-window	-0.0076	(0.0159)	-0.0075	(0.0236)	-0.0180	(0.0221)	-0.0005	(0.0332)	-0.0175	(0.0222)
Linear, 4 Months-window	-0.0232	(0.0179)	-0.0168	(0.0266)	-0.0218	(0.0256)	-0.0394	(0.0375)	-0.0199	(0.0249)
Linear, 2 Months-window	-0.0475	(0.0256)	-0.0434	(0.0377)	-0.0654	(0.0375)	-0.0885	(0.0530)	-0.0212	(0.0354)
Linear, 6 Months-window w/o Dec/Jan	-0.0104	(0.0227)	-0.0242	(0.0341)	0.0024	(0.0310)	-0.0004	(0.0473)	-0.0070	(0.0331)
Linear, 4 Months-window w/o Dec/Jan	-0.0243	(0.0293)	-0.0326	(0.0440)	0.0267	(0.0402)	-0.0593	(0.0611)	-0.0102	(0.0423)
Linear, 2 Months-window w/o Dec/Jan	0.0036	(0.0168)	-0.0004	(0.0252)	0.0112	(0.0228)	-0.0075	(0.0354)	0.0100	(0.0242)

Notes: This table reports coefficient estimates of β_{PPL} of different regression models. The dependent variables is school readiness. The results are reported for the sample including all children, and subsamples stratified by mothers' education and predicted pre-reform eligibility. "Low & medium" education refers to lower and medium-secondary school certificates. "High" education refers to upper-secondary school certificates (*Abitur*). *Previously eligible* families likely received benefits 13-24 months after childbirth. *Previously ineligible* families were likely not eligible for parental leave benefits 6-12 months after childbirth (see Section 3.2). The regressions include county-by-examination year fixed effects. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.11: Estimation results for children with low and medium educated mothers separately

	(1)	(2)	(3)
	Maternal education		
	Low/med.	Low	Medium
Language skills	-0.0219 (0.0152) [0.1321]	-0.0310 (0.0265) [0.1437]	-0.0161 (0.0185) [0.1044]
Socio-emotional stability	-0.0065 (0.0139) [0.0978]	0.0015 (0.0248) [0.1080]	-0.0123 (0.0167) [0.0872]
Motor skills	-0.0103 (0.0132) [0.0879]	-0.0181 (0.0238) [0.0846]	-0.0076 (0.0157) [0.0865]
School readiness	0.0097 (0.0126) [0.1177]	-0.0089 (0.0242) [0.1196]	0.0194 (0.0142) [0.0864]
N	14,610	5,146	9,464
<i>Control variables</i>			
Child characteristics	Yes	Yes	Yes
Family characteristics	Yes	Yes	Yes

Notes: This table reports the coefficient estimates of the parental leave reform effect (β_{PPL}) on child outcomes. All regressions are based on the main equation and include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects and dummies for missing variables. Robust standard errors are reported in parentheses. R^2 are reported in brackets. * $p < 0.05$, ** $p < 0.01$.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2007.

Table B.12: Comparison of socio-economic characteristics of Schleswig-Holstein to the rest of West Germany

	Schleswig-Holstein	West	West*
Age	44.07	43.21	43.22
Female	0.52	0.52	0.52
Unmarried	0.38	0.39	0.39
Married	0.47	0.47	0.48
Divorced	0.07	0.06	0.06
Household size	2.67	2.75	2.76
Children in household	0.92	0.97	0.98
Born in Germany	0.89	0.85	0.85
Working	0.45	0.47	0.47
Unemployed	0.03	0.03	0.03
Out of the labour force	0.51	0.5	0.5
Female labour force participation rate	0.80	0.80	0.80
Share of children below age 3 in day care ^a	0.12	0.12	0.12
Share of children aged 3-6 in day care ^a	0.84	0.91	0.91
Share of fathers taking parental leave ^b	0.18	0.20	0.20
<i>Highest level of education</i>			
≤ ISCED3	0.25	0.28	0.28
ISCED4	0.04	0.05	0.05
ISCED5	0.47	0.42	0.42
ISCED6	0.06	0.06	0.06
≤ ISCED7	0.18	0.20	0.20
<i>Personal monthly net income</i>			
0 - 1,100	0.52	0.53	0.53
1,100-2,300	0.29	0.3	0.3
2,300-3,600	0.07	0.06	0.06
3,600-5,000	0.03	0.03	0.03
5,000-18,000	0.01	0.01	0.01
<i>Household monthly net income</i>			
0 - 1,100	0.1	0.1	0.1
1,100-2,300	0.29	0.31	0.31
2,300-3,600	0.19	0.2	0.2
3,600-5,000	0.14	0.15	0.15
5,000-18,000	0.09	0.09	0.09
<i>Municipality size</i>			
<2,000	0.19	0.05	0.05
2,000-5,000	0.11	0.09	0.09
5,000-10,000	0.12	0.12	0.12
10,000-50,000	0.33	0.35	0.36
50,000-100,000	0.08	0.1	0.1
>100,000	0.16	0.31	0.27
N	25,249	533,229	513,241

Notes: This table reports socio-economic and socio-demographic characteristics of the population in Schleswig-Holstein and West Germany. “West” includes only West German federal states, without Schleswig-Holstein. “West*” further excludes the city-states of Hamburg and Bremen. ^a Based on information in 2008. ^b Based on German Federal Statistical Office (2010).

Source: Own calculations based on German Mikrozensus 2009.

Online Appendix on employment outcomes using the SIAB

Table C.1: Descriptive statistics

	(1)	(2)	(3)	(4)	(5)
	All	Sample stratified by Mothers' pre-birth wages			
	mothers	low	high	difference	s.e.
If employed 6 months before child birth	.958	.957	.989	-.032	.003
If employed 18 months before child birth	.889	.805	.960	-.155	.007
If employed 30 months before child birth	.837	.673	.931	-.257	.009
Labour earnings 6-18 months before child birth	21289.10	6933.158	31594.87	-24661.71	245.116
Daily wage 6 months before child birth	60.215	20.942	88.403	-67.461	.663
Daily wage 18 months before child birth	54.964	16.187	83.013	-66.826	.746
Daily wage 30 months before child birth	51.145	16.758	76.301	-59.543	.847
German citizenship	.924	.900	.939	-.038	.006
Age at child birth	30.801	29.820	31.610	-1.790	.121
East German	.206	.241	.168	.072	.010
Mother's first child	.681	.498	.800	-.302	.011
Work experience (years)	9.970	8.526	10.911	-2.385	.119
Schleswig-Holstein	.032	.035	.030	.005	.004
Hamburg	.022	.018	.027	-.009	.004
Lower Saxony	.092	.095	.083	.011	.007
Bremen	.006	.009	.006	.003	.002
North Rhine-Westphalia	.201	.187	.216	-.029	.01
Hesse	.076	.065	.086	-.021	.007
Rhineland-Palatinate	.044	.043	.042	.001	.005
Baden-Wuerttemberg	.128	.130	.136	-.005	.009
Bavaria	.172	.160	.186	-.025	.010
Saarland	.010	.009	.009	-.000	.002

Notes: This table reports descriptive statistics for our main SIAB samples. "Low" pre-birth wages refers to mothers below the 25th percentile of the pre-birth earnings distribution. "High" pre-birth wages refers to mothers above the median of the pre-birth earnings distribution. The means have been calculated based on non-missing information. The column "Difference" reports the difference in characteristics between children from high- and low-wage mothers.

Source: Own calculations based on SIAB-V7514.

Table C.2: Balancing SIAB

	All	Sample stratified by mothers' education	
		Low	High
If employed 6 months before child birth	.001935 (.008522)	-.01121 (.01764)	.006913 (.006407)
If employed 18 months before child birth	.002588 (.01333)	.003812 (.03489)	-.001543 (.0118)
If employed 30 months before child birth	.01452 (.01571)	-.01492 (.04102)	.001673 (.01542)
Labour earnings 6-18 months before child birth	10.96 (576)	630.5* (361.1)	-182.4 (655.9)
Daily wage 6 months before child birth	.145 (1.567)	.5221 (.974)	.04681 (1.78)
Daily wage 18 months before child birth	-.1007 (1.648)	1.62 (1.139)	-1.128 (1.993)
Daily wage 30 months before child birth	.3458 (1.683)	1.594 (1.776)	-.4514 (2.187)
German citizenship	-.003767 (.01129)	-.03587 (.02625)	.0109 (.01455)
Mother's first child	.02468 (.01986)	.05611 (.04397)	.00857 (.02431)
Age at child birth	-.3714* (.201)	-.7731 (.4793)	-.2831 (.2455)
East German	-.02233 (.01725)	-.01299 (.0376)	-.02688 (.02273)
Work experience (in years)	-.2269 (.195)	-.5837 (.4268)	-.1577 (.2618)
Schleswig-Holstein	.01024 (.007572)	-.007061 (.01628)	.01576 (.0104)
Hamburg	-.0002663 (.006357)	-.001129 (.01173)	-.001399 (.009913)
Lower Saxony	.01096 (.01234)	.04334* (.02582)	.005448 (.01681)
Bremen	.002892 (.003535)	.01482* (.008549)	.002436 (.004803)
North Rhine-Westphalia	.005859 (.0171)	-.0009813 (.0343)	.02155 (.02503)
Hesse	-.001585 (.01136)	.00206 (.02168)	-.0204 (.01708)
Rhineland-Palatinate	-.004465 (.00874)	-.0244 (.01807)	.00181 (.01217)
Baden-Wuerttemberg	.006912 (.01425)	-.04596 (.02966)	.02797 (.02087)
Bavaria	-.006796 (.01611)	.04247 (.03229)	-.02531 (.02369)
Saarland	.005496 (.004396)	-.008088 (.008692)	.008714 (.005914)
N	8817	2073	4345

Notes: This table reports coefficient estimates of β_{PPL} of regression models outlined in equation 1 (without X) to check the balance of mother's pre-birth characteristics. The dependent variables are listed in the rows. The results are reported for the sample including all children, and subsamples stratified by mothers' pre-birth wages. "Low" pre-birth wages refers to mothers below the 25th percentile of the pre-birth earnings distribution. "High" pre-birth wages refers to mothers above the median of the pre-birth earnings distribution. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on SIAB-V7514.

Table C.3: Mechanisms - medium-run effects of parental leave reform on employment outcomes

when observed after child birth	1 year	2 years	3 years	4 years	5 years	6 years
Panel A: All (N=8,817)						
Employment	-.1276** (.02112)	-.01771 (.02066)	.03148 (.01985)	-.008564 (.01952)	-.002307 (.01909)	-.00635 (.01826)
Part-time employment	-.09534** (.0183)	-.02671 (.02036)	-.0007221 (.02061)	.02692 (.02077)	-.0423** (.0209)	-.02917 (.02083)
Full-time employment	-.03631** (.01712)	.009513 (.01823)	.0341* (.01923)	-.04359** (.01842)	.04192** (.01784)	.01798 (.01707)
Cumulated work experience	-1.412** (.171)	-1.645** (.3588)	-1.668** (.5291)	-1.545** (.6746)	-1.483* (.8239)	-1.42 (.969)
Cumulated earnings	-1719** (312.1)	-1789** (741)	-1544 (1183)	-983.2 (1615)	-408.7 (2073)	115.9 (2550)
Panel B: Previously ineligible (N=4,345)						
Employment	-.1836** (.03014)	-.04606 (.02904)	.02675 (.02833)	-.002944 (.0281)	.018 (.02716)	.01434 (.02551)
Part-time employment	-.1456** (.02547)	-.04857* (.02904)	-.01125 (.02931)	.05253* (.02981)	-.02075 (.03018)	-.005177 (.02996)
Full-time employment	-.04725* (.02611)	.00449 (.02741)	.0359 (.02822)	-.06727** (.02722)	.03889 (.02632)	.01344 (.02515)
Cumulated work experience	-1.716** (.2423)	-2.334** (.4991)	-2.405** (.7369)	-2.379** (.941)	-1.97* (1.151)	-1.646 (1.349)
Cumulated earnings	-2783** (570.1)	-3059** (1296)	-2437 (2028)	-1409 (2730)	56.32 (3469)	1319 (4233)
Panel C: Previously eligible (N=2,073)						
Employment	-.04732 (.04349)	.03129 (.04353)	.03527 (.04117)	.008945 (.04008)	.0112 (.03924)	.0002752 (.03902)
Part-time employment	-.001583 (.03856)	.0158 (.04191)	.03524 (.04272)	.03125 (.04251)	-.01378 (.04239)	.0006207 (.04233)
Full-time employment	-.04529 (.03176)	.01738 (.03387)	.016 (.03758)	-.01489 (.03511)	.03583 (.03396)	.002648 (.03235)
Cumulated work experience	-.8548** (.3567)	-.314 (.7601)	-.04229 (1.107)	.669 (1.422)	.7627 (1.746)	1.107 (2.068)
Cumulated earnings	-599.3** (248.8)	-111.4 (624.2)	133.6 (1057)	969.7 (1537)	1050 (2081)	1360 (2679)

Notes: This table reports the coefficient estimates of the parental leave reform effect (β_{PPL}) on maternal employment outcomes observed at different years after child birth. The sample includes mothers who give birth between July 2005 and June 2007, but excludes mothers who give birth in December or January because of measurement error in children's month of birth. All regressions are based on equation 1 and include state fixed effects, birth months fixed effects, birth cohort fixed effects, and controls for maternal age at child birth (squared), German citizenship, first child indicator, and work experience before child birth. The stratification in Panels B and C is based on pre-reform wages and separates parents into those who were likely *previously eligible* for parental leave benefits 13-24 months after childbirth and who were likely not *previously ineligible* for parental leave benefits 6-12 months after childbirth. Robust standard errors are reported in parentheses. **

* $p < 0.1$, ** $p < 0.05$

Table C.4: Robustness checks - employment outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	no controls	Window size without January & December				Including children born	Including children born
	(Jul-Jun)		Aug-May	Sep-Apr	Oct-Mar	Nov-Feb	7/2004-6/2005	Jan & Dec
Panel A: All (N=8,817)								
Employment after 6 months	-.1316** (.01904)	-.1333** (.01908)	-.1311** (.02157)	-.1126** (.02528)	-.1021** (.03152)	-.1085** (.04584)	-.1155** (.01617)	-.139** (.01813)
Part-time employment after 6 months	-.09546** (.01488)	-.09563** (.01494)	-.1025** (.01688)	-.09455** (.01963)	-.09685** (.02443)	-.1287** (.03563)	-.08156** (.01264)	-.0995** (.0142)
Full-time employment after 6 months	-.03881** (.01499)	-.04014** (.01501)	-.03205* (.017)	-.01781 (.02017)	-.001628 (.02509)	.02478 (.03609)	-.03768** (.01267)	-.04153** (.01429)
Cumulated work experience after 6 months	-.423** (.06878)	-.4342** (.0692)	-.4043** (.0782)	-.3525** (.093)	-.375** (.1194)	-.4024** (.1775)	-.0394 (.05969)	-.4293** (.06623)
Cumulated earnings after 6 months	-546.4** (120.1)	-577.3** (121.7)	-562.4** (136)	-614.9** (157.4)	-631.2** (194.5)	-424.3 (286.4)	-498.2** (101.6)	-591.5** (114.8)
N	8817	8817	6929	5117	3305	1613	13451	9725
Panel B: Previously ineligible (N=4,345)								
Employment after 6 months	-.1398** (.02726)	-.1436** (.0274)	-.1459** (.03078)	-.1381** (.03597)	-.1053** (.04521)	-.1485** (.06505)	-.1331** (.02294)	-.1481** (.02596)
Part-time employment after 6 months	-.09803** (.01971)	-.09665** (.01969)	-.1097** (.02223)	-.1122** (.02558)	-.09863** (.03238)	-.1502** (.04847)	-.08446** (.01642)	-.09973** (.01877)
Full-time employment after 6 months	-.04749** (.02313)	-.05215** (.02326)	-.04551* (.02619)	-.03393 (.03091)	-.00686 (.03857)	-.0001061 (.05401)	-.05534** (.01951)	-.05284** (.02204)
Cumulated work experience after 6 months	-.4409** (.09938)	-.459** (.1005)	-.4675** (.1123)	-.4654** (.1333)	-.3645** (.1733)	-.4109 (.2513)	-.02734 (.08642)	-.4348** (.09536)
Cumulated earnings after 6 months	-891.2** (222)	-938.4** (226)	-1015** (250.6)	-1107** (288.3)	-1221** (362.3)	-1037** (521.3)	-855.1** (185.4)	-997.7** (211.7)
N	4345	4345	3432	2548	1651	823	6684	4797
Panel C: Previously eligible (N=2,073)								
Employment after 6 months	-.1054** (.0401)	-.1071** (.04003)	-.08513* (.04572)	-.05734 (.05385)	-.06007 (.06675)	-.01889 (.09925)	-.08288** (.03462)	-.1063** (.03814)
Part-time employment after 6 months	-.07205** (.03398)	-.07364** (.03449)	-.06222 (.03861)	-.04934 (.04534)	-.05637 (.0559)	-.09594 (.07855)	-.07066** (.02982)	-.08016** (.03242)
Full-time employment after 6 months	-.03144 (.02749)	-.03228 (.02761)	-.01841 (.0315)	-.003399 (.03767)	-.01134 (.04653)	.07705 (.07027)	-.01287 (.02313)	-.02156 (.0261)
Cumulated work experience after 6 months	-.2899** (.1425)	-.3224** (.1434)	-.1782 (.1608)	-.01328 (.1904)	-.04727 (.2462)	-.05441 (.3721)	-.02077 (.1246)	-.2632* (.1371)
Cumulated earnings after 6 months	-174.5* (101.4)	-193.4* (99.4)	-107.8 (115.5)	-21.15 (145)	67.68 (133.2)	141.2 (208.4)	-113.4 (89.68)	-174.3* (96.69)
N	2073	2073	1615	1184	763	373	3137	2283

Notes: This table reports the results from robustness checks of the reform effect of the parental leave reform on maternal employment outcomes stratified by mothers' pre-birth wages. The window size around the reform cut-off and definitions of the control group are varied. All regressions include state fixed effects, birth months fixed effects, birth cohort fixed effects, and controls for maternal age at child birth (squared), German citizenship, first child indicator, and work experience before child birth. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on SIAB-V7514.

Table C.5: Common trend checks - employment outcomes

	(1)	(2)	(3)	(4)	(5)
	Baseline	Cohort-specific time trends		Placebo reforms	
		Linear	Quadratic	Placebo cohort	Mar/Apr 2007
<i>Panel A: All</i>					
Employment after 6 months	-.1316** (.01904)	-.1317** (.01903)	-.1316** (.01904)	.03092 (.01937)	.00052 (.02672)
Part-time employment after 6 months	-.09546** (.01488)	-.09558** (.01488)	-.09565** (.01488)	.02618* (.01517)	.005829 (.02064)
Full-time employment after 6 months	-.03881** (.01499)	-.0387** (.015)	-.03862** (.015)	.002658 (.01531)	-.01274 (.02039)
Cumulated work experience after 6 months	-.423** (.06878)	-.4235** (.06879)	-.4229** (.06884)	.7488** (.07189)	-.03948 (.1011)
Cumulated earnings after 6 months	-546.4** (120.1)	-544.8** (120)	-545.6** (120)	88.83 (119)	145.5 (169.4)
N	8817	8817	8817	9024	4302
<i>Panel B: Previously ineligible</i>					
Employment after 6 months	-.1398** (.02726)	-.1402** (.02723)	-.1398** (.02722)	.01484 (.02794)	-.03118 (.03902)
Part-time employment after 6 months	-.09803** (.01971)	-.0971** (.01968)	-.09735** (.01968)	.02642 (.02063)	-.005342 (.0275)
Full-time employment after 6 months	-.04749** (.02313)	-.04919** (.02312)	-.04839** (.0231)	-.01306 (.02367)	-.03196 (.03231)
Cumulated work experience after 6 months	-.4409** (.09938)	-.4444** (.09932)	-.4421** (.09927)	.7801** (.104)	-.08119 (.1466)
Cumulated earnings after 6 months	-891.2** (222)	-886.5** (221.9)	-883** (221.8)	45.83 (220.2)	392.2 (320.5)
N	4345	4345	4345	4476	2068
<i>Panel C: Previously eligible</i>					
Employment after 6 months	-.1054** (.0401)	-.1057** (.04015)	-.1084** (.04014)	.05154 (.04023)	-.01223 (.05704)
Part-time employment after 6 months	-.07205** (.03398)	-.07268** (.03401)	-.07167** (.03404)	.00901 (.03403)	-.03554 (.04741)
Full-time employment after 6 months	-.03144 (.02749)	-.03107 (.0275)	-.03503 (.02753)	.03738 (.0278)	.01723 (.03783)
Cumulated work experience after 6 months	-.2899** (.1425)	-.2986** (.1431)	-.3069** (.1429)	.5836** (.15)	-.2672 (.2204)
Cumulated earnings after 6 months	-174.5* (101.4)	-181.2* (101.5)	-186* (101.4)	131 (84.07)	-180.3 (131.8)
N	2073	2073	2073	2130	1028

Notes: This table reports the results of sensitivity checks to alternative model specifications for the common trend assumption for the full sample, and subsamples stratified by mothers' pre-birth wages. It also reports the results from placebo regressions. All regressions include state fixed effects, birth months fixed effects, birth cohort fixed effects, and controls for maternal age at child birth (squared), German citizenship, first child indicator, and work experience before child birth. Robust standard errors are reported in parentheses. ** $p < 0.01$, * $p < 0.05$.

Source: Own calculations based on SIAB-V7514.