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ABSTRACT

Parental Leave Policies and Socio-Economic Gaps in Child Development: Evidence from a Substantial Benefit Reform Using Administrative Data*

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 from 2007 that both expanded paid leave in the first year and removed paid leave in the second year following childbirth. Higher-income households benefited relatively more from the reform than low-income households. We use administrative data from mandatory school entrance examinations containing detailed child development assessments at age six within a difference-in-differences approach. 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.

JEL Classification: J13, J18, J22, J24

Keywords: parental leave benefit, child development, school readiness, motor skills, language skills, socio-emotional stability

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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). 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]

To what extent then can public policies affect the link between a family's SES and child development? One of the most important policy tools across OECD countries to support families around childbirth are parental leave policies. These policies affect several conditions in early childhood (e.g. Björklund and Salvanes, 2011): 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), thus affecting 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- and long-run.[2]

Despite the substantial impact that parental leave policies have on family resources, we know little about the effects of such policies on early child development and even less on SES development gaps.[3] A few studies examine *introductions* of parental leave (see Rossin, 2011; Stearns, 2015;

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

[2] The 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- and long-run outcomes (e.g. Elango et al., 2016).

[3] The 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).

Carneiro et al., 2015), finding some positive effects on infant health, schooling and 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-run effects of parental leave reforms (e.g., Beuchert et al., 2016; Lichtman-Sadot and Bell, 2017; Baker and Milligan, 2008, 2010, 2015). 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).[4] Furthermore, previous contributions pay little attention as to whether these policies impact SES development gaps.

Our paper addresses these questions by examining a German reform that completely changed the 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. Thereafter, parental leave

[4]The 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). Huber (2015) also analyses the effects of the 2007 German reform on child outcomes at ages 0 through 3. In contrast to our study, the analysis is based on parent-reported measures for child development and a comparably small sample from the German Socio-Economic Panel Study (SOEP). The point estimates are very large compared to previous findings in the literature, unstable across specifications and imprecisely estimated, preventing a clear conclusion.

benefits were earnings-related 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% (Wrohlich et al., 2012), mothers with a university degree received about 40% more than mothers without a university degree. Although the reform was implemented in a setting with a low maternal labour force participation after childbirth, the reform still caused the labour supply of mothers to decrease in the first year after birth (especially for highly educated mothers) and to increase in the second year after birth for less educated mothers (Wrohlich et al., 2012; Kluve and Schmitz, 2017). Other studies suggest small effects on fertility (Cygan-Rehm, 2016; Raute, 2016) 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).

Our study makes the following major contributions to the literature. We shed new light on whether substantial changes in paid parental leave affect SES development gaps: Our setting is very distinct from the previous literature on parental leave evaluations in terms of the magnitude and directions of policy changes (see Appendix Figure A.1). The reform we analyse changed means-tested benefits to earnings-related benefits. Compared to low-SES households, high-SES households gained financial and time resources that could be invested in children's development. Thereby, the German reform provides a rare opportunity to study the relationship between SES gaps in child development and changes in paid parental leave policies. Moreover, examining a reform that expands eligibility but reduces the benefit period adds to the previous literature which exclusively studies introductions

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

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

or expansions of parental leave entitlements. Our contribution is therefore particularly relevant for most OECD countries that already have paid parental leave policies in place and might consider redesigning them.

We provide novel evidence of parental leave policy effects on short-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 and includes detailed information on child development assessed by licensed public health paediatricians. 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). Previous studies analysing short-run effects are either constrained by child outcomes conveying limited information on child development, by potentially biased parent-reported information about the children, or by small sample sizes requiring more restrictive assumptions for the identification of causal effects. By analysing short-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.

In 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 nearby years as our control group. Our results show that this substantial change 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 family resources. Likely explanations for the zero-effects are that the changes in transfers are transitory, that the share of non-working mothers was already high before the reform, and that the affected margins of maternal labour supply only have a small impact on child development.

2. Background

2.1. Heterogeneous changes of the 2007 German paid parental leave reform

Mothers in Germany are generally not allowed to work during the six weeks before and the eight weeks after childbirth. Employed mothers receive 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. Column 1 of Table 1 shows that 77% of parents were eligible for 300 Euros of monthly benefits (about 11% of pre-birth net household income) for up to six months after childbirth (based on representative household data from the 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.[7]

In 2006, the German government reformed the paid parental leave regulations (*Bundeselterngeld- und Elternzeitgesetz*), seeking to (Bujard, 2013): safeguard family income during the first year

[7]Parents 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).

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 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.[8] 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.[9]

Families that were *previously ineligible* for paid parental leave (or 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).[10] 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

[8]The 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).

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

[10]If both parents take paid parental leave, the maximum paid leave period is 14 months. In our observation period, the share of fathers taking parental leave is relatively low, and we abstract from this detail to ease the discussion.

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.

The household income-based pre-reform eligibility strongly correlates with parental education and other socio-economic characteristics (see Section 3.2 for details). Consequently, children from high-SES families benefited more 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).[11] 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. 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.

2.2. Expected reform effects on child development

To investigate how the reform affected child development, we first establish how the differently affected groups responded to the changes in paid parental leave. We focus on the two groups experiencing the most extreme reform changes: *previously ineligible* parents who gain eligibility for up to one year and *previously eligible* parents who lost entitlement for the second year. We focus on two important phases of child development: 0 to 12 months and 13 to 24 months.

[11] Classifying 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).

Previously ineligible 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. After these eight weeks, about 70% of previously ineligible mothers took unpaid leave within the first twelve months after childbirth before the reform (based on Kluve and Schmitz, 2017), i.e. the reform largely *substituted* unpaid leave with paid leave.[12] Effectively, previously ineligible mothers receive a new cash transfer of up to 18,000 Euro (10x1,800 Euro). A substantial literature studies the effects of family income on children's outcomes. For instance, Dahl and Lochner (2012) and Løken et al. (2012) show that permanent increases in family income can improve child outcomes. However, changes in parental leave benefits can be interpreted as transitory household income shocks. Carneiro and Ginja (2016) suggest that parents only adjust their investments in their children in terms of time and goods to changes in permanent income, but not to transitory income shocks.[13] Alternatively, parental leave benefits can be interpreted as unrestricted income transfers, which Heckman and Mosso (2014) and Del Boca et al. (2016) find to be ineffective at affecting child development.

Despite a comparably high share of non-working mothers in the first year after childbirth, previously ineligible mothers responded to the reform by reducing their employment, mostly at the part-time margin (Kluve and Schmitz, 2017, find a reduction of about 39%, with 28% working part-time at baseline). Such employment reductions may increase maternal care time, which can positively affect children's outcomes. The actual effect of maternal time on child development depends on the activities parents perform with the children (e.g. Leibowitz, 1977; Todd and Wolpin, 2007). Maternal employment correlates only weakly with educational activities mothers perform with their children (e.g. Del Bono et al., 2016). Instead, maternal employment may reduce the time mothers spend on activities that are unproductive or even detrimental for child development (e.g. Hsin and

[12]The previous literature documents parental responses to the heterogeneous changes in paid parental leave by focusing on heterogeneities by parental education and household income to approximate pre-reform eligibility. As we show in Section 3.2, parental education correlates highly with income and pre-reform eligibility.

[13]Further evidence that permanent income rather than transitory income fluctuations matter for child development is provided by Cameron and Heckman (1998) and Bernal and Keane (2010). For a literature review of earlier studies, see Currie (2009) and Currie and Almond (2011).

Felfe, 2014). Brooks-Gunn et al. (2010) and Bernal and Keane (2010), for instance, conclude that the effects of early maternal employment are rather small, especially for part-time employment beyond the initial six months following childbirth. Therefore, the reform-related changes in maternal employment may also have a limited impact on child development, especially as mothers in Germany are already spending a comparably high amount of time with their children.

After the first 12 months following childbirth, some evidence shows that previously ineligible mothers were now more likely to work (e.g. Kluve and Schmitz, 2017).[14] The effects of maternal employment beyond the first year after childbirth on children, *inter alia*, depend on the quality of alternative care arrangements. As the availability of publicly-funded day care for children below the age of three was low, informal child care by grandparents or other relatives was the main alternative mode of care when mothers were working (e.g. Hank and Buber, 2009). With a good quality of alternative child care, increases in maternal employment are also unlikely to have a large impact on children.[15]

In summary, *previously ineligible* mothers received a substantial income transfer and reduced their employment in the first year after childbirth, which would allow them to spend more time with their children. While this may, at first glance, suggest an improvement in child development, the temporary transfer, the crowding out of unpaid by paid leave, and the margins of employment reactions may not necessarily translate into positive reform effects on children's development.

Parents who were *previously eligible* for up to two years of paid leave still receive at least the same amount of benefits and did not adjust their labour supply in the first 12 months after childbirth; the share of non-working mothers remained high at 93%. In the second year after childbirth, the share

[14] While the reform did not change the incentives for this group in the second year after childbirth, the observed return-to-work pattern is consistent with the interpretation that the new parental leave benefit generated a new social norm of returning to work once the benefit expired, see also Kluve and Schmitz (2017).

[15] In previously ineligible families, the father was also more likely to use the "daddy leave" months compared to previously eligible families. In our main discussion of reform effects, we abstract from this reform element, as the initial take-up is low (below 15%), and mostly taken jointly with the mother for up to two months. Cools et al. (2015) suggest that paternal leave may improve children's schooling outcomes. This reform element may therefore also favor high-SES households over low-SES households.

of non-working mothers remains high at 87% (e.g. Kluve and Schmitz, 2017) such that these families mainly experience a negative transitory income shock by losing benefits of up to 3,600 Euros (12x300 Euro). They may compensate for this income loss with increased maternal employment (e.g., Wrohlich et al., 2012; Bergemann and Riphahn, 2015, though Kluve and Schmitz, 2017, find no such effects in the second year after childbirth) allowing them to maintain their material investments in children. As discussed, the additional time spent working may not affect activities that the mother and alternative caregiver perform with the children.

In summary, *previously eligible* mothers experienced transitory income shocks in the second year after childbirth, which they might compensate by increased part-time employment. Such changes in transitory income and part-time employment, may, however, not translate into changes in productive parental investments in their children.

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.[16] Before entering primary school at the age of six, 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 cover all children from three cohorts entering school between 2012-2014. A school entrance cohort includes children born between July of the previous year

[16]Schleswig-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 A.1 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.

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 birth,[17] 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, pediatricians count children's jumps on one leg over a line within 10 seconds and how 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.[18] 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.

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

[18] In 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.

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.[19] 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.

3.2. Descriptive statistics and sample stratifications

Our sample consists of 44,997 children with complete information on the four domains of child development.[20] Descriptive statistics of the full sample and the subsamples stratified by maternal education levels are provided in Table 3. Panel A describes children's outcomes in the examinations. Overall, 71.5% of children reach a sufficient level of language competencies, 81% are considered stable in their socio-emotional development, 82.5% show a sufficient level of motor skills development, and 84% 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 3 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.3, 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 show higher skill development levels.

[19] For 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 (0.012 months, sample mean 72.6 months) and statistically insignificant effect (see Appendix Table A.2).

[20] Missing 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. Children belonging to Danish minorities living in Schleswig-Holstein are marked in the data and removed from the sample. Our main results remain robust when we include these children in our sample (available upon request).

Children with more siblings and those who are not living with both parents show lower levels of skill development. Children's migration background, missing information on parental education, 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 families. Specifically, Section 2.1 shows that lower-income households were *previously eligible* for paid parental leave and lost eligibility beyond the first year due to the reform. In contrast, higher-income households were *previously 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.[21] 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 probabilites from both data sets (Panel B). Reassuringly, the predicted probabilities in the original SOEP sample and the administrative data set match closely suggesting

[21]The 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 for eligibility increases with the number of children and single motherhood, while it decreases with the education level of the parents (available upon request).

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.4, we show that maternal education is a very good predictor for 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 36 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 intention-to-treat effect (ITT) of the 2007 German paid parental leave reform on children's development, we employ a difference-in-differences approach similar to, e.g., Dustmann and Schönberg (2011) and Danzer and Lavy (2016): We first compare the developmental outcomes of children born before and after the reform cut-off date. Depending on the size of the comparison window, this simple difference may confound the reform effect with seasonal and age-at-examination effects. To eliminate these potential biases, we use children born in the same months but in years not affected by policy changes (both pre- and post-reform) as our control group.

This empirical 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 of reform cohorts and control cohorts in the absence of the reform. We run several checks that support the plausibility of this assumption (see Section 5.3).[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, or in control cohorts. 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 birthyear or related to children's school entry cohort, are taken into account in the analysis by birth cohort fixed effects.

The third assumption is that the reform does not impact the group composition of children born in specific months, e.g. caused by parents strategically manipulating birth dates near the reform cut-off by postponing cesarean sections and labour inductions. Indeed, Neugart and Ohlsson (2013) and Tamm (2013) find that about 8% 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. We address the resulting concern about endogenous sample selection bias by excluding children born in December and January from our main samples.

Another concern regarding the sample composition would be reform effects on fertility patterns. If so, then children born in the year after the treatment cohort may show different child and family background characteristics. In Section 5.3, we show that our conclusions are the same if we exclude children born in the year after the treatment cohort from the sample. 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.2).

[22]The 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.

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

$$\begin{aligned} Y_i = &\beta_1 \, \textit{cohort0607}_i + \beta_2 \, \textit{postJanuary}_i + \beta_{PPL} \left(\textit{cohort0607}_i \cdot \textit{postJanuary}_i \right) \\ &+ \textit{birth month}_i' \delta + \textit{birth cohort}_i' \phi + (\textit{county}_i \cdot \textit{examination year}_i)' \theta + X_i' \gamma + \epsilon_i \end{aligned} \tag{1}$$

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, and July 2007 and June 2008. 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*$_i'$), a vector of fixed effects for each birth cohort (*birth cohort*$_i'$), 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 coverage 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).[23]

[23] Inference 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 1 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 1 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 4, we turn to the 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 -0.0074 (sample mean of 0.715), of socio-emotional stability by -0.0035 (sample mean of 0.810), of motor skills by -0.0067 (sample mean of 0.825), and of being ready for school by 0.0048 (sample mean of 0.840). 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 1.5% for language skills and socio-emotional stability, 1% for motor skills, and 2.2% for school readiness. Similarly, we can rule out negative effects greater than 3.7% for language skills and 1.9% for all other dimensions.

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).[24] 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 3 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 4 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.[25]

Figure 2 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.3). Across all four outcomes, Figure 2 shows that our precise estimates are small in magnitude compared to these development gaps.

One limitation of the binary outcomes is that only children crossing the threshold would identify

[24] We also estimated the reform effects on children with missing information about maternal educatiom levels. The effects are also small and insignificant with coefficients (standard errors) of 0.0095 (0.0200) for language skills, -0.0026 (0.0181) for socio-emotional stability, -0.0067 (0.0178) for motor skills, and 0.0010 (0.0183) for school readiness. The respective graphs are provided in Appendix Figure A.3.

[25] Appendix Table A.5 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 even small treatment effects. We conduct the power analysis at a 5% significance level and present the results graphically in Appendix Figure A.5 for our main sample. The figure shows that we can detect a reduced form treatment effect of 0.04 SD with a probability of almost 90%; with close to 100% probability, we would detect any effects greater than 0.05 SD. Moreover, we are able to detect effect sizes on the order of 0.1 SD at the 5% significance levels with a probability of at least 99% for all subgroups.

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 5 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.6 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 6). 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 sample 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.[26] Unlike Danzer and Lavy (2016), we cannot detect gender-specific treatment effects for children of lower and higher SES households.[27]

As we cannot find any significant treatment effects, it may still be that the reform's effects on different potential channels offset one another: For example, reductions in maternal labour supply in the first year, and increases in the second year after childbirth, may affect child development in isolation, but together they may cancel out. To learn more about the underlying mechanisms, we interact the treatment indicator with regional characteristics that can be relevant for child development and that may moderate reform effects: the availability of day care for children below the age of three (see Table 7, Panel A), the female labour force participation rate (Panel B), and paternal leave-taking (Panel C). Across the three characteristics, the interaction effects are very small and insignificant on average, including for subgroups stratified by maternal education and pre-reform eligibility. For conciseness, we only 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. We conclude that the availability of day care, female labour force participation and paternal leave-taking do not importantly impact the results. We interpret these results as evidence that the reform had no effects on children overall, rather than that substantial effects of different potential channels cancel out.

The zero-reform effects can be rationalised with findings from recent economic studies. The substantial changes in household income due to the changes in paid parental leave can be interpreted

[26] 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 (available on request). 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 (available on request).

[27] Danzer 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).

as changes in unrestricted cash transfers and in transitory income. Carneiro and Ginja (2016) show that temporary household income shocks do not change parents' productive investments and Heckman and Mosso (2014, p. 3) conclude that unrestricted income transfers "are a weak reed" to affect child outcomes. With respect to maternal employment, the reform largely affected maternal labour supply at the part-time margin beyond the first six months after childbirth. Previous studies show that such changes have, at most, a small impact on child development (e.g. Bernal and Keane, 2010; Brooks-Gunn et al., 2010). Moreover, the share of non-working mothers in the first year after childbirth was already high. Therefore, it appears unlikely that the reform substantially changed the maternal care time, especially the time mothers spend on activities that support child development. Some studies that document effects of paid parental leave reforms on children find that the reforms also substantially changed maternal care time, which may moderate their observed effects on children (e.g. Baker and Milligan, 2008; Carneiro et al., 2015).

5.3. Sensitivity checks

Table 8 assesses the sensitivity of our results 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).[28] 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. Including children born between July 2004 and June 2005 in the control group 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

[28] Predetermined variables are balanced across all window sizes, see Appendix Table A.7.

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. We also assess whether any endogenous fertility effects may bias our estimates. Fertility responses might affect children from the control group born one year after the treatment cohort. Excluding children born after June 2007 generates very similar estimates (column 8).

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.

We assess the plausibility of the common trend assumption in Appendix Table A.8. 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.

Finally, we use a basic regression discontinuity design to identify the treatment effects. Given only five birth months on each side of the cut-off, we assume a linear trend in the outcome variables across birth months for children born between July 2006 and June 2007 and identify the treatment effect with an indicator for children born on or after January 1, 2007 (column 6). The results support our main conclusions.[29]

[29] As in the main specification, the underlying sample excludes children born in December and January. The results are similar if we include them in the sample.

We also run all robustness checks separately by maternal education (Appendix Tables A.8 and A.9) and predicted pre-reform eligibility (available upon request), reaching the same conclusions.

6. 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. The heterogeneous changes in parental leave eligibility and benefit payments of the German reform for different socio-economic groups provide a rare opportunity to study the impact of parental leave policies on SES child development gaps. 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 short-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 low-SES families, and, consequently, on SES gaps in child development. Our results are robust to numerous sensitivity checks.

We explain the zero-reform effects within the institutional context of low maternal employment

rates and low day care usage by children under the age of three: Before and after the reform, all mothers stayed at home for the first two months after childbirth. Previously ineligible mothers widely substituted unpaid with paid leave and received a new cash transfer that increased the net household income by about 20%. Previously eligible mothers experienced a transitory income shock in the second year after childbirth, which they likely compensated by increased part-time employment. The recent literature has shown that transitory income changes, rather than permanent changes, do not change parents' productive investments and that unrestricted income transfers affect child outcomes ineffectively. Most of the maternal employment reactions occur at the part-time margin beyond the first six months after childbirth, margins that have, at most, a small impact on child development (see Section 2.2).

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, it may also be desirable to disentangle the mechanisms through which the reform results in zero-effects on child development. However, detailled data at the individual child level is not available. 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, 2017; 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. Future research should also 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? Since most OECD countries now have paid parental leave policies in place, governments are mainly interested in re-designing these regulations to better incentivise female labour supply, fertility, or paternal involvement in the child rearing process. The

German reform effectively changed maternal labour supply, family income, and paternal leave-taking. In light of these findings, our study suggests that such policy objectives can be achieved without adverse effects on children's development or SES development gaps.

Compliance with Ethical Standards

The authors declare that they have no conflict of interest.

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Tables

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

		Mothers' education	
	All	Low & medium	High
Pre-birth household annual net income in EUR	31,712.29	27,267.56	37,530.56
Children born before January 1, 2007: Erziehungsgeld			
% recipients for up to 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
Children born on or after January 1, 2007: Elterngeld			
% 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.
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
Previously ineligible				
Before 2007 reform:	Ineligible or eligible for up to 6 months	No change	PPL benefits ↑ PPL duration ↑	No change[a]
After 2007 reform:	Eligible		⇒ gain max. 1,800 Euro × 10 months benefits	
Previously eligible				
Before 2007 reform:	Eligible	No change	No change	PPL benefits ↓
After 2007 reform:	Minimum benefits			PPL duration ↓ ⇒ lose max. 300 Euro × 12 months 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: Descriptive statistics

	(1)	(2)	(3)	(4)	(5)
		Sample stratified by mothers' education			
	All children	Low & medium	High	Difference =(3)-(2)	s.e.
Panel A: Child development outcomes measured at age 6					
Language skills (0/1)	0.715	0.671	0.777	-0.106	(0.005)
Socio-emotional stability (0/1)	0.810	0.776	0.851	-0.075	(0.004)
Motor skills (0/1)	0.825	0.803	0.860	-0.057	(0.004)
School readiness (0/1)	0.840	0.809	0.911	-0.102	(0.004)
Panel B: Child characteristics					
Age at examination in months	72.604	72.718	72.479	0.239	(0.053)
Girl (0/1)	0.488	0.491	0.491	-0.000	(0.005)
Birth weight in grams	3381.661	3359.819	3436.341	-76.522	(6.431)
Birth weight missing (0/1)	0.038	0.026	0.019	0.006	(0.002)
Years in day care (at age 6)	3.417	3.389	3.555	-0.165	(0.011)
Years in kindergarten missing (0/1)	0.242	0.239	0.240	-0.001	(0.005)
Migration background (0/1)	0.212	0.214	0.183	0.030	(0.004)
Migration background missing (0/1)	0.088	0.073	0.059	0.013	(0.003)
Panel C: Family background characteristics					
Mother's years of schooling	10.949	9.649	13.000	-3.351	(0.004)
Mother's education missing (0/1)	0.183	–	–	–	–
Father's years of schooling	11.672	11.039	12.300	-1.262	(0.017)
Father's education missing (0/1)	0.238	0.104	0.034	0.070	(0.003)
Child lives with both parents (0/1)	0.792	0.745	0.882	-0.138	(0.004)
Child lives with one parent (0/1)	0.141	0.175	0.086	0.089	(0.004)
Child with other living arrangements (0/1)	0.066	0.081	0.032	0.048	(0.003)
Child's living arrangement missing (0/1)	0.086	0.006	0.006	0.000	(0.001)
Home language is German (0/1)	0.850	0.854	0.877	-0.023	(0.004)
German is main language (0/1)	0.107	0.102	0.093	0.009	(0.003)
Home language foreign (0/1)	0.043	0.044	0.029	0.014	(0.002)
Home language missing (0/1)	0.039	0.014	0.014	0.000	(0.001)
Number of children in the family	2.195	2.193	2.150	0.043	(0.011)
N	44,997	22,492	14,256	36,748	

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.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

Table 4: 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			Predicted pre-reform eligibility for parental leave benefits	
	Mean (SD)				Low & medium	High	Δ SES gap =(5)-(4)	Previously eligible	Previously ineligible
Language skills	0.715	-0.0079	-0.0065	-0.0074	-0.0181	-0.0092	0.0089	-0.0034	-0.0019
	(0.451)	(0.0094)	(0.0092)	(0.0091)	(0.0134)	(0.0157)	(0.0206)	(0.0169)	(0.0166)
		[0.0734]	[0.1110]	[0.1327]	[0.1286]	[0.1007]		[0.1565]	[0.0868]
Socio-emot. stability	0.810	-0.0035	-0.0029	-0.0035	-0.0127	0.0112	0.0238	-0.0211	0.0076
	(0.392)	(0.0082)	(0.0082)	(0.0081)	(0.0120)	(0.0135)	(0.0181)	(0.0154)	(0.0144)
		[0.0594]	[0.0777]	[0.0951]	[0.1042]	[0.0777]		[0.1017]	[0.0846]
Motor skills	0.825	-0.0067	-0.0062	-0.0067	-0.0051	-0.0127	-0.0076	-0.0139	-0.0056
	(0.380)	(0.0079)	(0.0078)	(0.0077)	(0.0115)	(0.0127)	(0.0172)	(0.0148)	(0.0139)
		[0.0375]	[0.0714]	[0.0802]	[0.0842]	[0.0765]		[0.0764]	[0.0823]
School readiness	0.840	0.0041	0.0054	0.0048	0.0032	0.0079	0.0047	0.0072	0.0074
	(0.366)	(0.0076)	(0.0074)	(0.0073)	(0.0110)	(0.0105)	(0.0152)	(0.0155)	(0.0110)
		[0.0384]	[0.0802]	[0.1110]	[0.1139]	[0.0660]		[0.1144]	[0.0618]
N	44,997	44,997	44,997	44,997	22,492	14,256	36,748	12,836	12,481
Control variables									
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 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 2008.

Table 5: Effects of the parental leave reform on alternative definitions of child development

Panel A: Language skills	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Treatment effect on					
	Language		language score in plural words, pseudo words & prepositions				
	score, mean (SD)	Language score	≤ 12 (5%)	$13-16$ (13%)	$17-19$ (33%)	20 (21%)	21 (28%)
All children ($N = 28,001$)	18.5264 (3.0599)	-0.0980 (0.0714)	0.0065 (0.0056)	0.0129 (0.0086)	-0.0032 (0.0122)	-0.0039 (0.0106)	-0.0124 (0.0110)
Mothers with low/medium education ($N = 14,890$)	18.2052 (3.1551)	-0.1133 (0.1014)	0.0115 (0.0082)	0.0181 (0.0126)	-0.0238 (0.0168)	-0.0086 (0.0141)	0.0029 (0.0140)
Mothers with high education ($N = 8,936$)	19.4417 (2.3083)	-0.0453 (0.0974)	-0.0069 (0.0057)	0.0080 (0.0115)	0.0301 (0.0211)	0.0013 (0.0202)	-0.0324 (0.0220)
Panel B: Socio-emotional stability	SDQ score,	Treatment effect on					
			specific parts of the SDQ score distribution: Score =				
	mean (SD)	SDQ score	0 (9%)	$1-4$ (31%)	$5-8$ (32%)	$9-12$ (17%)	≥ 13 (11%)
All children ($N = 20,603$)	6.4232 (4.8933)	0.1959 (0.1362)	-0.0024 (0.0077)	-0.0014 (0.0131)	-0.0249 (0.0139)	0.0189 (0.0110)	0.0098 (0.0091)
Mothers with low/medium education ($N = 11,371$)	7.2702 (4.9947)	0.0895 (0.1870)	0.0006 (0.0088)	-0.0126 (0.0168)	-0.0039 (0.0187)	0.0200 (0.0159)	-0.0041 (0.0135)
Mothers with high education ($N = 7,042$)	5.0849 (4.1355)	0.3692 (0.2083)	-0.0078 (0.0149)	-0.0033 (0.0249)	-0.0352 (0.0241)	0.0233 (0.0165)	0.0230 (0.0120)
Panel C: Motor skills	Jumps,	Treatment effect on					
			side-jumps within 10 seconds				
	mean (SD)	No. of side-jumps	≤ 7 (13%)	$8-9$ (28%)	10 (31%, mode)	$11-13$ (18%)	≥ 14 (10%)
All children ($N = 20,321$)	10.0705 (3.0472)	-0.1211 (0.0858)	0.0119 (0.0099)	0.0092 (0.0134)	-0.0184 (0.0134)	0.0100 (0.0116)	-0.0127 (0.0088)
Mothers with low/medium education ($N = 11,126$)	9.9229 (3.0253)	-0.0516 (0.1138)	0.0003 (0.0139)	0.0245 (0.0183)	-0.0203 (0.0179)	0.0003 (0.0154)	-0.0048 (0.0114)
Mothers with high education ($N = 6,947$)	10.4814 (3.0984)	-0.2016 (0.1585)	0.0060 (0.0150)	0.0114 (0.0221)	-0.0248 (0.0237)	0.0402 (0.0210)	-0.0328 (0.0172)

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 6: 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
Girls: Language skills	0.0069	-0.0093	0.0071	0.0064	-0.0152	0.0139	0.0065
	(0.0128)	(0.0189)	(0.0216)	(0.0199)	(0.0217)	(0.0238)	(0.0228)
Boys: Language skills	-0.0202	-0.0253	-0.0255	-0.0272	-0.0120	-0.0211	-0.0095
	(0.0130)	(0.0190)	(0.0226)	(0.0201)	(0.0226)	(0.0242)	(0.0239)
Girls: Socio-emo. stability	-0.0142	-0.0306	0.0069	-0.0223	-0.0174	-0.0176	-0.0177
	(0.0109)	(0.0163)	(0.0179)	(0.0170)	(0.0176)	(0.0210)	(0.0185)
Boys: Socio-emo. stability	0.0053	0.0028	0.0132	0.0012	0.0254	-0.0225	0.0271
	(0.0119)	(0.0175)	(0.0200)	(0.0186)	(0.0196)	(0.0224)	(0.0216)
Girls: Motor skills	-0.0038	0.0007	0.0007	0.0029	-0.0068	-0.0174	-0.0130
	(0.0095)	(0.0144)	(0.0146)	(0.0146)	(0.0155)	(0.0190)	(0.0162)
Boys: Motor skills	-0.0103	-0.0115	-0.0271	-0.0195	-0.0144	-0.0089	-0.0014
	(0.0120)	(0.0176)	(0.0202)	(0.0187)	(0.0202)	(0.0226)	(0.0221)
Girls: School readiness	0.0009	-0.0079	0.0145	-0.0038	0.0034	-0.0027	0.0153
	(0.0094)	(0.0143)	(0.0124)	(0.0145)	(0.0130)	(0.0207)	(0.0124)
Boys: School readiness	0.0076	0.0114	0.0013	-0.0003	0.0160	0.0154	0.0008
	(0.0110)	(0.0166)	(0.0166)	(0.0172)	(0.0170)	(0.0228)	(0.0175)
Number of girls	21,981	11,033	6,994	9,875	6,929	6,275	6,156
Number of boys	23,016	11,459	7,262	10,188	7,278	6,561	6,325

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 2008.

Table 7: Effect heterogeneity by day care availability, female labour force participation and fathers' parental leave-taking

	(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
Panel A: Heterogeneity by share of children below age 3 in formal day care (county-year level, min. 8%, max. 27%)					
PPL reform effect	0.0048	0.0033	0.0113	0.0058	0.0093
	(0.0073)	(0.0110)	(0.0106)	(0.0162)	(0.0111)
PPL reform effect · day care share in % (below age 3)	-0.0001	-0.0008	-0.0027	-0.0009	-0.0018
	(0.0014)	(0.0022)	(0.0024)	(0.0028)	(0.0024)
Panel B: Heterogeneity by female labour force participation rate (county level, 2011, min. 70%, max. 79%)					
PPL reform effect	0.0048	0.0032	0.0085	0.0126	0.0047
	(0.0073)	(0.0110)	(0.0108)	(0.0159)	(0.0113)
PPL reform effect · female LFP share in %	0.0006	-0.0013	-0.0011	0.0067	0.0044
	(0.0020)	(0.0029)	(0.0031)	(0.0045)	(0.0032)
Panel C: Heterogeneity by share of fathers taking parental leave (county level, 2008, min. 5.8%, max. 13.3%)					
PPL reform effect	0.0051	0.0050	0.0080	0.0074	0.0073
	(0.0074)	(0.0113)	(0.0105)	(0.0154)	(0.0110)
PPL reform effect · fathers' parental leave share in %	0.0032	0.0079	-0.0009	-0.0041	-0.0046
	(0.0039)	(0.0058)	(0.0055)	(0.0085)	(0.0058)
N	44,997	22,492	14,256	12,836	12,481

Notes: This table reports the estimation results of the parental leave reform effect on school readiness (results on other outcomes available on request). Regressions are based on the main model, including an additional interaction term of the treatment indicator with county (-year) level characteristics, 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 German Federal Statistical Office (2011a) and assigns the respective value when the child was aged two years. The female labour force participation rate for women aged 15-45 is based on German Federal Statistical Office (2011b). Fathers' parental 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. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

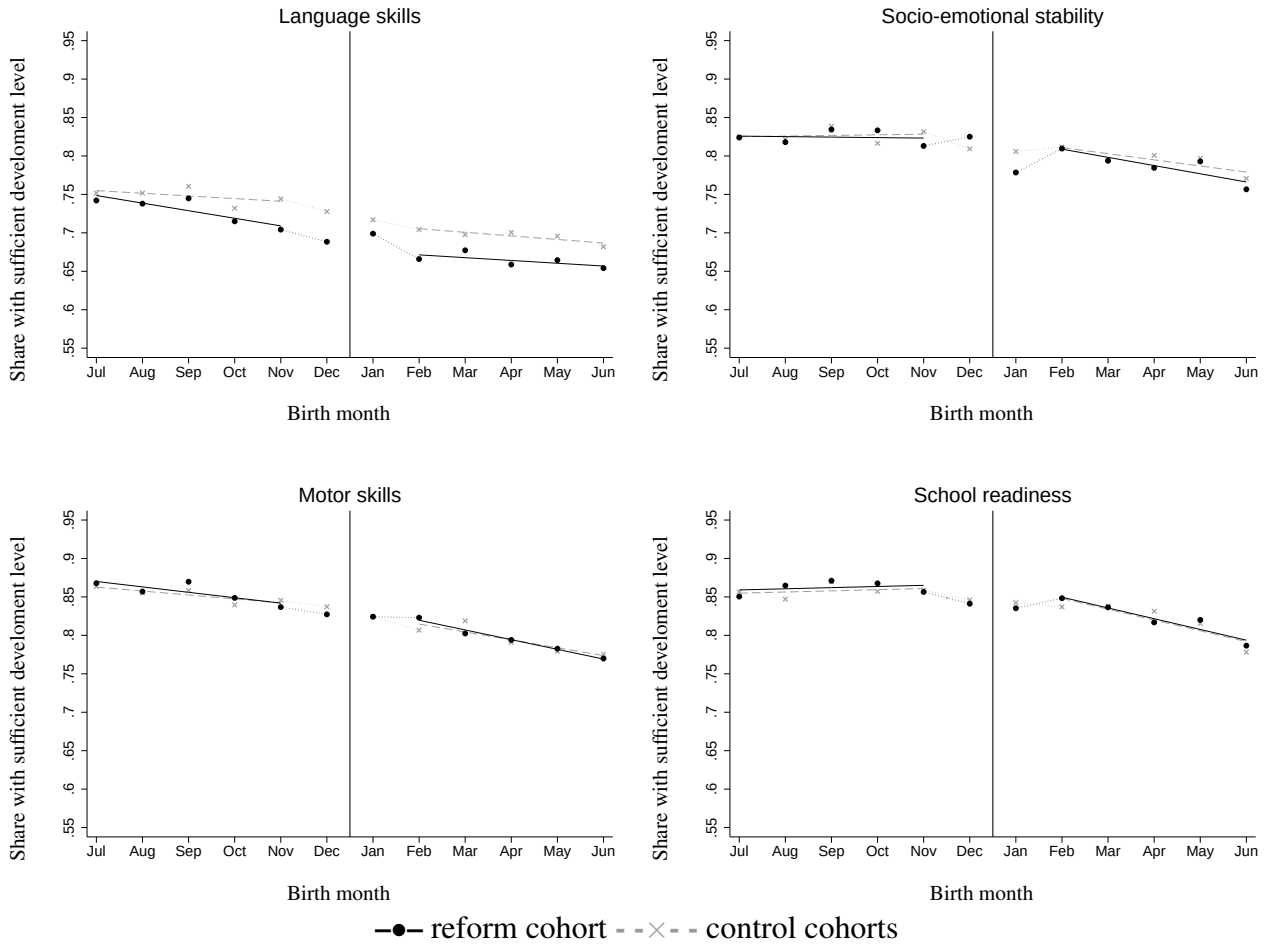
Table 8: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Baseline	Window size without January & December				Including children born		Without children born	Nonlinear models (marginal effects)	
	(Jul-Jun)	Aug-May	Sep-Apr	Oct-Mar	Nov-Feb	7/2004-6/2005	in Jan & Dec	after 6/2007	Probit	Logit
Language skills	-0.0074	-0.0052	-0.0051	0.0064	-0.0035	-0.0121	-0.0037	-0.0022	-0.0055	-0.0050
	(0.0091)	(0.0101)	(0.0117)	(0.0144)	(0.0207)	(0.0086)	(0.0083)	(0.0104)	(0.0087)	(0.0087)
Socio-emot. stability	-0.0035	-0.0005	-0.0038	0.0013	0.0174	-0.0072	-0.0113	-0.0020	-0.0037	-0.0030
	(0.0081)	(0.0089)	(0.0102)	(0.0125)	(0.0179)	(0.0076)	(0.0073)	(0.0094)	(0.0078)	(0.0078)
Motor skills	-0.0067	-0.0033	-0.0058	-0.0011	0.0207	-0.0089	-0.0042	-0.0070	-0.0078	-0.0081
	(0.0077)	(0.0086)	(0.0099)	(0.0122)	(0.0175)	(0.0073)	(0.0070)	(0.0090)	(0.0076)	(0.0077)
School readiness	0.0048	0.0029	-0.0001	0.0026	0.0135	-0.0000	0.0033	0.0120	0.0045	0.0042
	(0.0073)	(0.0080)	(0.0092)	(0.0112)	(0.0161)	(0.0069)	(0.0067)	(0.0084)	(0.0073)	(0.0073)
N	44,997	35,552	26,166	17,157	8,315	60,590	53,627	29,016	44,997	44,997

Notes: This table reports the results from robustness checks of the reform effect of the parental leave reform on child outcomes. 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.

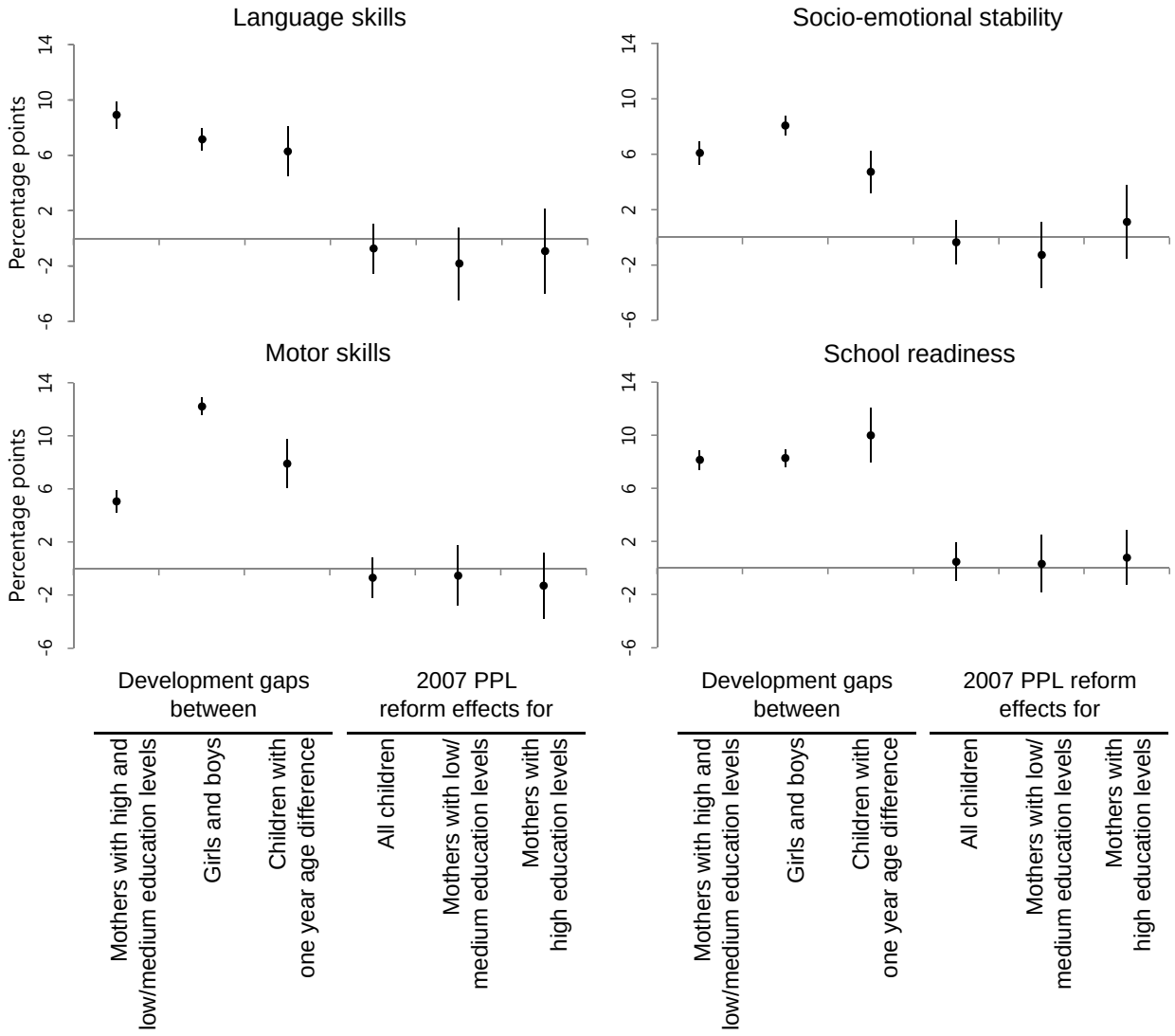
Figures

Figure 1: 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 2008.

Figure 2: Comparing child development gaps by mothers' education, gender and child age to treatment effect sizes of the 2007 German parental leave reform



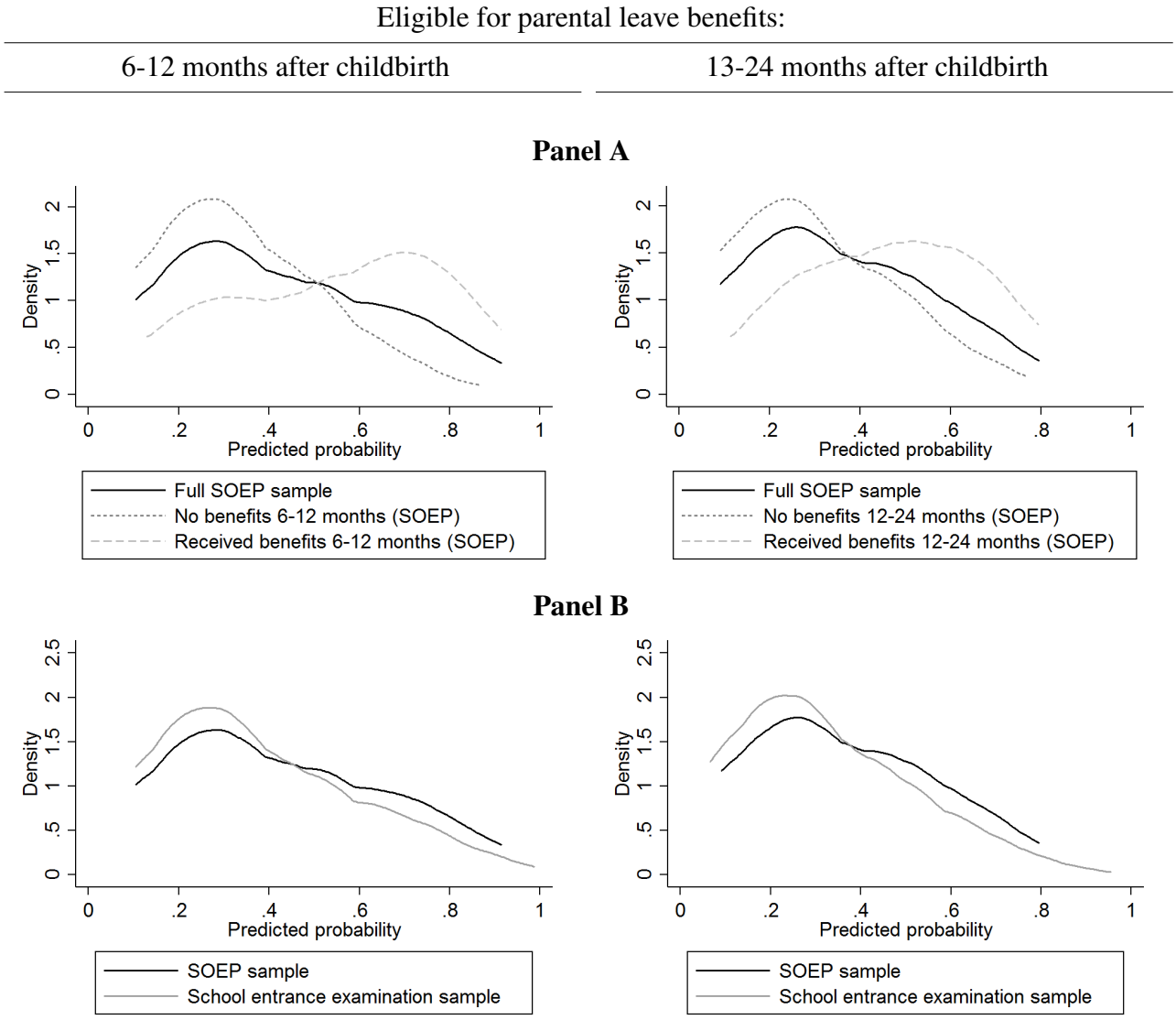
Notes: The figure plots child development gaps at school entrance (coefficient estimates retrieved from Table A.3, coefficient on age is scaled), and estimated treatment effects of the 2007 German paid parental leave (PPL) reform for all children, for children from low/medium educated mothers and from 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 2008.

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).

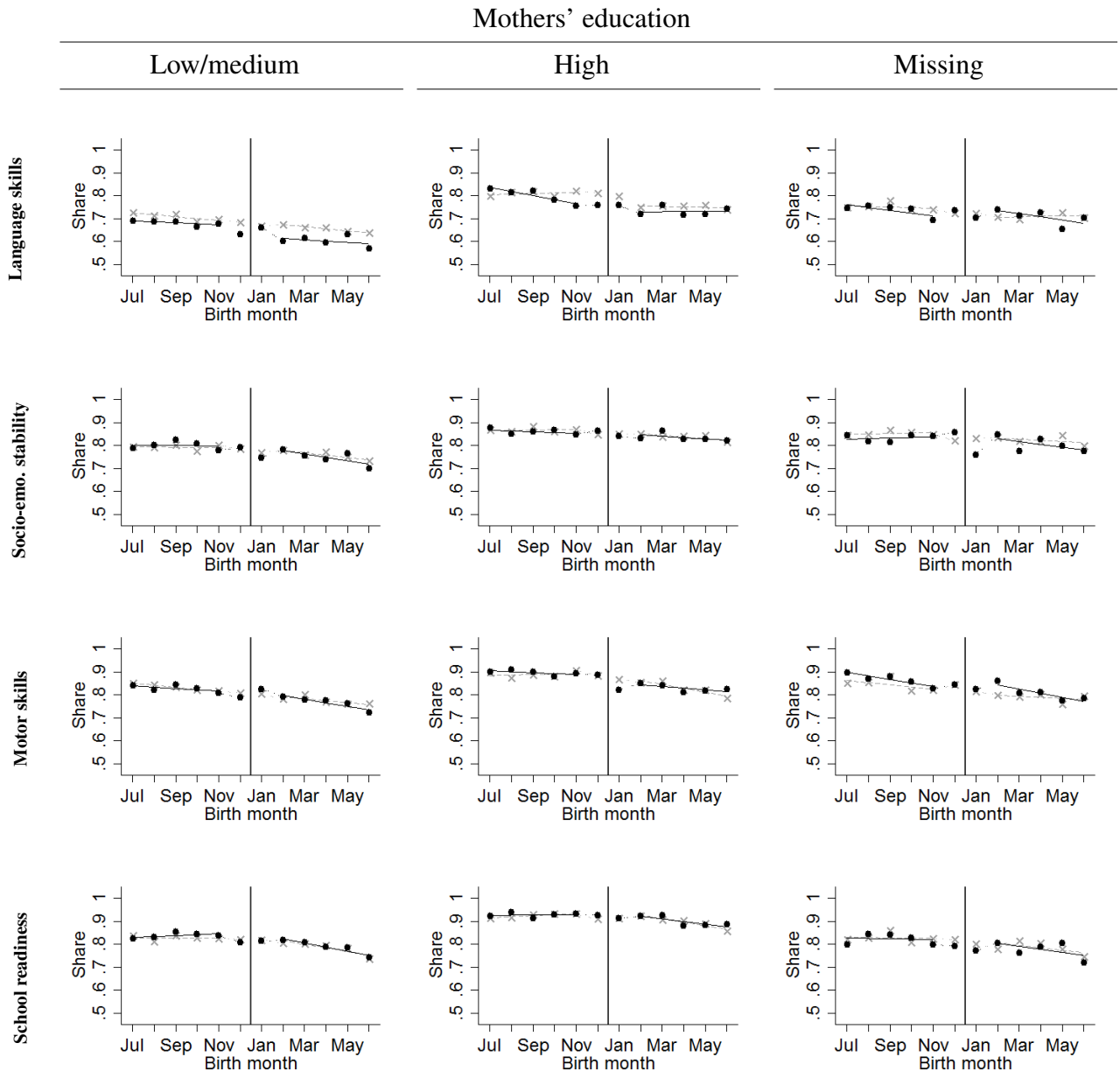
Figure A.2: Predicted eligibility for parental leave benefits under the policy rules applying before the 2007 reform



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 2008.

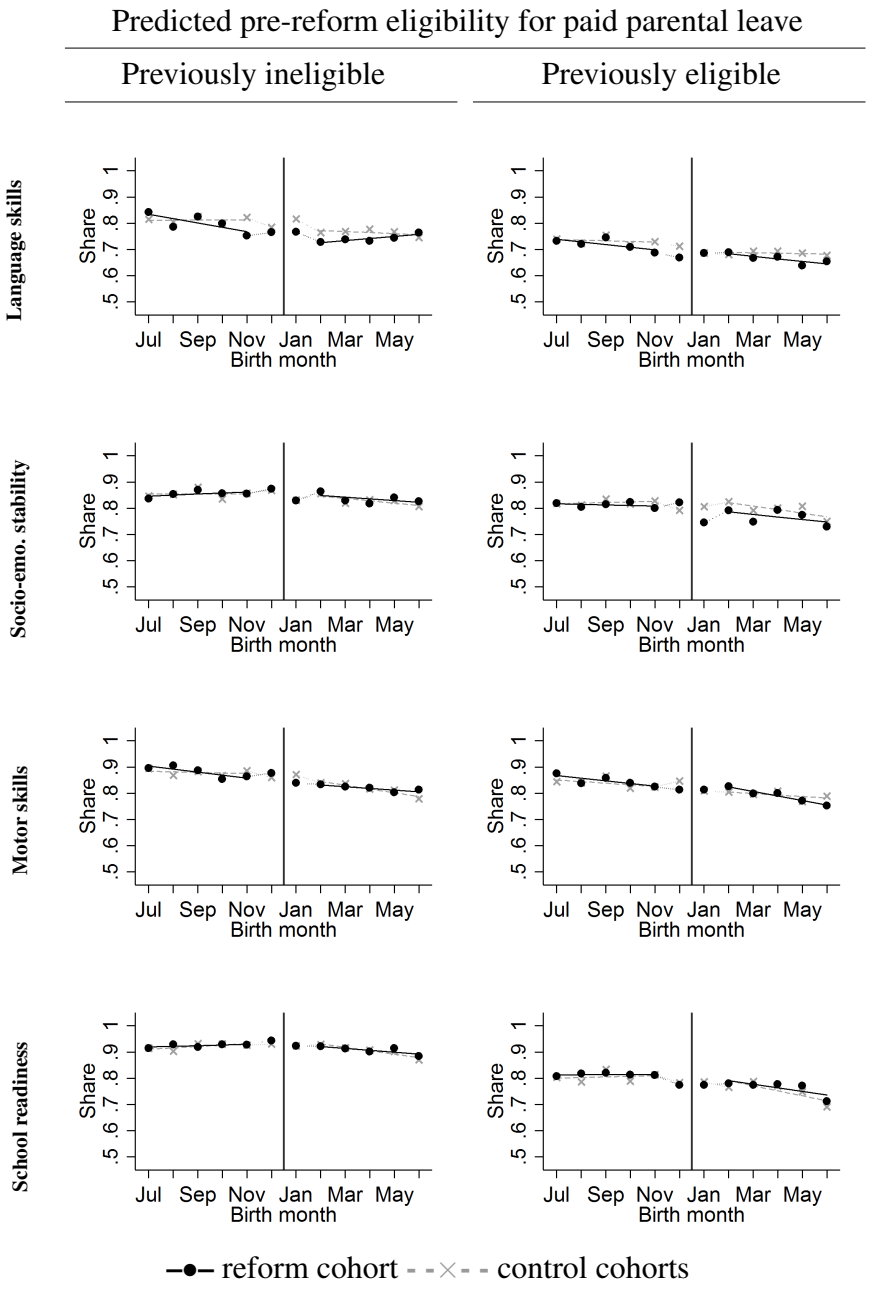
Figure A.3: The impact of the 2007 German parental leave reform on child development for subgroups



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 2008.

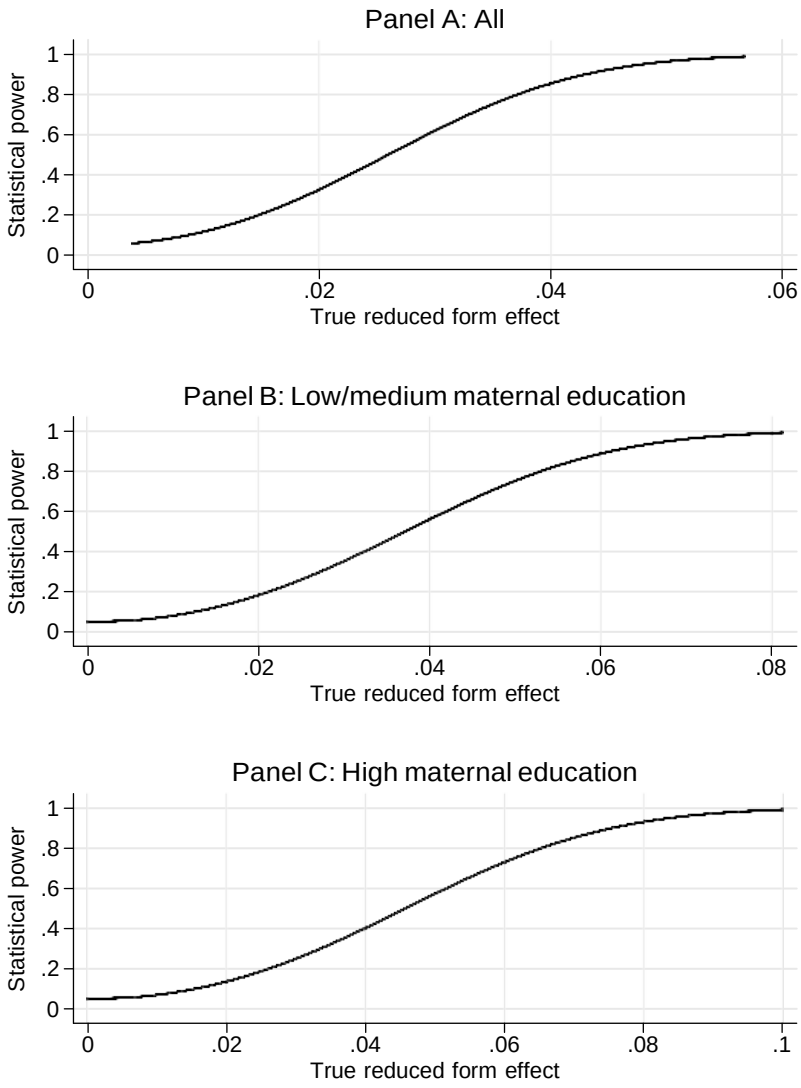
Figure A.4: The impact of the 2007 German parental leave reform on child development for subgroups



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 our main analyses.

Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

Figure A.5: Power analysis



Notes: The figure plots the statistical power for a t-test for equality of standard-normalised group means assuming equal variance for the treatment and control groups. Testing with type I error probability of 5%.
Source: Own calculations based on STATA -power- command.

Table A.1: 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
Highest level of education			
$\leq$ 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
$\leq$ ISCED7	0.18	0.20	0.20
Personal monthly net income			
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
Household monthly net income			
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
Municipality size			
$<$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.

Table A.2: Balancing of covariates

	All		Sample stratified by mothers' education				Sample stratified by predicted pre-reform eligibility for parental leave benefits			
			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.
Age at examination in months	0.0120	(0.0611)	-0.0217	(0.0801)	0.0335	(0.1314)	0.0601	(0.0997)	-0.0026	(0.1233)
Girl	-0.0022	(0.0106)	-0.0030	(0.0149)	-0.0071	(0.0192)	-0.0091	(0.0197)	0.0070	(0.0203)
Birth weight in grams	-7.9751	(12.6692)	-16.9056	(17.9797)	4.0378	(22.9523)	-23.4121	(23.4887)	-1.0926	(24.2484)
Birth weight missing	-0.0027	(0.0041)	-0.0047	(0.0047)	0.0014	(0.0052)	-0.0013	(0.0111)	0.0012	(0.0051)
Years child spent in day care	-0.0145	(0.0152)	-0.0398	(0.0208)	-0.0087	(0.0276)	0.0258	(0.0293)	-0.0288	(0.0290)
Years in day care missing	0.0006	(0.0046)	0.0031	(0.0060)	0.0074	(0.0070)	-0.0161	(0.0106)	0.0080	(0.0072)
Mother's years of schooling	0.0411	(0.0313)	-0.0037	(0.0142)	–		-0.0038	(0.0356)	0.0527	(0.0570)
Mother's education missing	0.0052	(0.0070)	–		–		0.0024	(0.0167)	–	
Father's years of schooling	0.0327	(0.0234)	0.0403	(0.0330)	0.0199	(0.0470)	-0.0256	(0.0153)	0.0535	(0.0529)
Father's education missing	0.0052	(0.0081)	0.0031	(0.0092)	0.0060	(0.0070)	-0.0045	(0.0127)	–	
Child lives with one parent	-0.0029	(0.0071)	-0.0172	(0.0113)	0.0125	(0.0109)	-0.0012	(0.0153)	–	
Child in other living arrangements	-0.0008	(0.0050)	0.0069	(0.0082)	-0.0091	(0.0069)	0.0045	(0.0111)	-0.0032	(0.0084)
Living arrangement missing	0.0037	(0.0036)	0.0045	(0.0023)	0.0026	(0.0032)	0.0046	(0.0108)	–	
One parent born abroad	0.0026	(0.0060)	0.0016	(0.0084)	0.0018	(0.0116)	0.0091	(0.0113)	0.0094	(0.0109)
Two parents born abroad	0.0033	(0.0067)	0.0139	(0.0098)	-0.0079	(0.0104)	0.0002	(0.0129)	0.0054	(0.0096)
Migration background missing	0.0067	(0.0045)	0.0008	(0.0049)	-0.0009	(0.0043)	0.0129	(0.0139)	–	
German is main language	0.0090	(0.0065)	0.0080	(0.0091)	0.0032	(0.0111)	0.0176	(0.0129)	0.0090	(0.0104)
Home language foreign	-0.0022	(0.0042)	0.0029	(0.0061)	-0.0106	(0.0067)	0.0019	(0.0085)	-0.0080	(0.0060)
Home language missing	0.0026	(0.0034)	0.0069	(0.0039)	0.0012	(0.0051)	-0.0021	(0.0089)	-0.0031	(0.0053)
Number of children in the family	0.0333	(0.0221)	0.0478	(0.0312)	-0.0052	(0.0336)	0.0613	(0.0533)	-0.0051	(0.0193)
Number of children missing	-0.0011	(0.0015)	-0.0004	(0.0018)	0.0003	(0.0024)	-0.0006	(0.0049)	–	

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). The regressions include the following control variables: county-by-examination year fixed effects, birth months fixed effects and birth cohort 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 2008.

Table A.3: 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
Age at examination in months	0.0052**	0.0039**	0.0066**	0.0083**
	(0.0008)	(0.0006)	(0.0008)	(0.0009)
Girl	0.0717**	0.0807**	0.1223**	0.0829**
	(0.0040)	(0.0035)	(0.0035)	(0.0033)
Birth weight in grams $*10^{-4}$	2.2406**	2.8822**	3.4318**	5.1665**
	(0.3523)	(0.3184)	(0.3266)	(0.3130)
Years child spent in day care	0.0135**	0.0123**	0.0124**	0.0175**
	(0.0029)	(0.0026)	(0.0026)	(0.0023)
Mother's years of schooling	0.0318**	0.0215**	0.0177**	0.0293**
	(0.0015)	(0.0013)	(0.0013)	(0.0012)
Father's years of schooling	0.0064**	0.0062**	0.0030	0.0007
	(0.0019)	(0.0017)	(0.0016)	(0.0014)
Number of children in the family	-0.0281**	-0.0073**	-0.0048**	-0.0240**
	(0.0020)	(0.0018)	(0.0018)	(0.0018)
Family structure (reference: child lives with both parents)				
Child lives with one parent	-0.0452**	-0.0771**	-0.0272**	-0.0535**
	(0.0068)	(0.0063)	(0.0059)	(0.0058)
Child lives in other living arrangements	-0.0438**	-0.1154**	-0.0574**	-0.0802**
	(0.0092)	(0.0090)	(0.0084)	(0.0084)
Migration background (reference: no migration background)				
One parent born abroad	-0.0265**	0.0151*	0.0095	0.0018
	(0.0081)	(0.0071)	(0.0070)	(0.0066)
Both parents born abroad	-0.1361**	0.0393**	0.0112	-0.0383**
	(0.0103)	(0.0084)	(0.0084)	(0.0088)
Language spoken at home (reference: Home language is German)				
German is main language	-0.0742**	0.0178*	0.0131	-0.0356**
	(0.0098)	(0.0080)	(0.0079)	(0.0083)
Home language foreign	-0.1351**	-0.0079	0.0081	-0.1219**
	(0.0135)	(0.0114)	(0.0114)	(0.0126)
Missing information				
Mother's education missing	-0.0147	-0.0046	-0.0163	-0.0310**
	(0.0101)	(0.0094)	(0.0089)	(0.0092)
Father's education missing	-0.0215*	-0.0203*	-0.0044	-0.0200*
	(0.0090)	(0.0085)	(0.0080)	(0.0081)
Birth weight missing	-0.0141	0.0219*	0.0044	-0.0408**
	(0.0107)	(0.0089)	(0.0090)	(0.0104)
Years in day care missing	-0.0419**	-0.0275**	-0.0246**	-0.0654**
	(0.0097)	(0.0081)	(0.0079)	(0.0092)
Living arrangement missing	0.0035	-0.0120	-0.0076	0.0127
	(0.0152)	(0.0132)	(0.0131)	(0.0139)
Migration background missing	-0.0088	-0.0154	0.0045	-0.0420**
	(0.0116)	(0.0106)	(0.0101)	(0.0110)
Home language missing	0.0145	0.0274*	0.0022	-0.0066
	(0.0150)	(0.0125)	(0.0128)	(0.0137)
Number of children missing	0.0395	0.0548**	0.0274	0.0616*
	(0.0251)	(0.0208)	(0.0220)	(0.0248)
Sample mean	0.715	0.810	0.825	0.840
N	44,997	44,997	44,997	44,997

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. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

Table A.4: Relationship between gaining eligibility for parental leave benefits and socio-ecnomic characteristics of the families and children

	Dep. variable: Previously ineligible for paid parental leave			
Indep. variable	(1)	(2)	(3)	(4)
High maternal education	0.368**	0.349**	0.349**	0.117**
	(0.006)	(0.006)	(0.006)	(0.004)
Girl			-0.005	-0.004
			(0.004)	(0.003)
High paternal education				0.149**
				(0.005)
No migration background				0.057**
				(0.003)
Number of children in the family				-0.066**
				(0.002)
Child lives with both parents				0.184**
				(0.006)
Child age in months				0.001**
				(0.000)
Birth weight in grams $\times$ 10^{-4}				0.013
				(0.023)
R^2	0.561	0.588	0.588	0.827
N	25,317	25,317	25,317	25,317

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

Source: Own calculations based on SOEP v32 and school entrance examinations for Schleswig-Holstein for children born between July 2005 and June 2008.

Table A.5: Estimation results for standardised outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All children			Mothers' education		Predicted eligibility for parental leave benefits	
				Low/ medium	High	Previously eligible	Previously ineligible
Language skills (z-score)	-0.0175	-0.0145	-0.0163	-0.0401	-0.0203	-0.0075	-0.0041
	(0.0209)	(0.0205)	(0.0202)	(0.0297)	(0.0347)	(0.0375)	(0.0367)
	[0.0734]	[0.1110]	[0.1327]	[0.1286]	[0.1007]	[0.1565]	[0.0868]
Socio-emo. stability (z-score)	-0.0089	-0.0075	-0.0090	-0.0323	0.0285	-0.0537	0.0193
	(0.0210)	(0.0208)	(0.0206)	(0.0306)	(0.0344)	(0.0393)	(0.0367)
	[0.0594]	[0.0777]	[0.0951]	[0.1042]	[0.0777]	[0.1017]	[0.0846]
Motor skills (z-score)	-0.0175	-0.0162	-0.0175	-0.0134	-0.0335	-0.0366	-0.0148
	(0.0209)	(0.0205)	(0.0204)	(0.0303)	(0.0335)	(0.0390)	(0.0367)
	[0.0375]	[0.0714]	[0.0802]	[0.0842]	[0.0765]	[0.0764]	[0.0823]
School readiness (z-score)	0.0113	0.0147	0.0132	0.0087	0.0216	0.0197	0.0202
	(0.0208)	(0.0203)	(0.0200)	(0.0301)	(0.0287)	(0.0422)	(0.0299)
	[0.0384]	[0.0802]	[0.1110]	[0.1139]	[0.0660]	[0.1144]	[0.0618]
N	44,997	44,997	44,997	22,492	14,256	12,836	12,481
Control variables							
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. 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 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 2008.

Table A.6: Reform effect on composite child development index

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All children			Mothers' education		Predicted eligibility for parental leave benefits	
				Low/ medium	High	Previously eligible	Previously ineligible
Child development	0.0078	0.0054	0.0044	0.0047	0.0205	-0.0037	0.0192
	(0.0170)	(0.0167)	(0.0167)	(0.0224)	(0.0265)	(0.0398)	(0.0277)
	[0.0664]	[0.1031]	[0.1112]	[0.0970]	[0.1319]	[0.1071]	[0.1124]
N	30,198	30,198	30,198	15,964	9,540	7,676	8,462
Control variables							
Child characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes
Family characteristics	No	No	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimation results of the parental leave reform on a composite index of child development. We calculate the dependent variable as the mean of all available z-scores of the following three domains: Language skills (prepositions, plural, pseudo words), motor skills (jumps) and socio-emotional stability (SDQ). All regressions are based on equation 1. They include examination year-by-county fixed effects, birth months fixed effects, birth cohort fixed effects, dummies for missing variables. 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 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 2008.

Table A.7: Balancing of covariates for varying window sizes around the reform cut-off

	(1)	(2)	(3)	(4)
	Window size without January & December			
	Nov-Feb	Oct-Mar	Sep-Apr	Aug-May
Age at examination in months	0.0062	-0.0140	0.0105	-0.0013
	(0.1280)	(0.1062)	(0.0817)	(0.0693)
Girl	0.0062	-0.0042	0.0105	0.0030
	(0.0240)	(0.0166)	(0.0136)	(0.0117)
Birth weight in grams	-35.0317	0.8321	-8.8259	-12.8237
	(28.3773)	(19.8000)	(16.1827)	(14.1342)
Years child spent in day care	0.0145	-0.0076	-0.0149	-0.0179
	(0.0344)	(0.0240)	(0.0194)	(0.0167)
Mother's years of schooling	0.0752	0.0379	0.0077	0.0216
	(0.0704)	(0.0493)	(0.0403)	(0.0348)
Father's years of schooling	-0.0076	0.0181	0.0307	0.0174
	(0.0529)	(0.0369)	(0.0301)	(0.0261)
Child lives with both parents	0.0127	-0.0127	-0.0013	-0.0059
	(0.0195)	(0.0136)	(0.0111)	(0.0096)
Child lives with one parent	-0.0004	0.0120	-0.0001	0.0046
	(0.0160)	(0.0110)	(0.0091)	(0.0079)
Child lives in other living arrangements	-0.0086	-0.0015	-0.0009	-0.0014
	(0.0114)	(0.0080)	(0.0064)	(0.0056)
At least one parent with mig. back.	0.0199	0.0076	0.0110	0.0125
	(0.0199)	(0.0140)	(0.0115)	(0.0099)
German is main language	-0.0219	-0.0123	-0.0178	-0.0141
	(0.0176)	(0.0124)	(0.0101)	(0.0088)
Home language foreign	0.0085	0.0035	0.0004	-0.0029
	(0.0095)	(0.0067)	(0.0054)	(0.0047)
Number of children in the family	0.1110*	0.0724*	0.0515	0.0328
	(0.0508)	(0.0348)	(0.0282)	(0.0245)
N	8,315	17,157	26,166	35,552

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. ** $p < 0.01$, * $p < 0.05$.
Source: Own calculations based on school entrance examinations for Schleswig-Holstein.

Table A.8: Common trend checks

	(1)	(2)	(3)	(4)	(5)	(6)
		Cohort-specific time trends		Placebo reforms		Regression
	Baseline	Linear	Quadratic	1 year earlier	Mar/Apr 2007	discontinuity
All children						
Language skills	-0.0074	0.0092	0.0090	-0.0095	-0.0069	-0.0145
	(0.0091)	(0.0239)	(0.0239)	(0.0099)	(0.0131)	(0.0202)
Socio-emot. stability	-0.0035	0.0107	0.0102	-0.0010	-0.0138	0.0005
	(0.0081)	(0.0210)	(0.0210)	(0.0089)	(0.0117)	(0.0176)
Motor skills	-0.0067	0.0135	0.0132	0.0027	-0.0030	0.0101
	(0.0077)	(0.0204)	(0.0204)	(0.0087)	(0.0116)	(0.0170)
School readiness	0.0048	-0.0033	-0.0038	-0.0087	-0.0054	0.0070
	(0.0073)	(0.0191)	(0.0191)	(0.0082)	(0.0107)	(0.0159)
N	44,997	44,997	44,997	44,997	21,540	13,998
Mothers' education: Low/medium						
Language skills	-0.0181	-0.0431	-0.0436	0.0093	0.0168	-0.0379
	(0.0134)	(0.0355)	(0.0355)	(0.0145)	(0.0195)	(0.0301)
Socio-emot. stability	-0.0127	0.0122	0.0126	-0.0105	-0.0165	0.0046
	(0.0120)	(0.0316)	(0.0316)	(0.0132)	(0.0176)	(0.0265)
Motor skills	-0.0051	0.0194	0.0199	0.0114	-0.0044	0.0150
	(0.0115)	(0.0307)	(0.0308)	(0.0126)	(0.0173)	(0.0259)
School readiness	0.0032	-0.0076	-0.0070	-0.0088	-0.0100	0.0016
	(0.0110)	(0.0291)	(0.0291)	(0.0123)	(0.0163)	(0.0241)
N	22,492	22,492	22,492	22,492	10,706	6,877
Mothers' education: High						
Language skills	-0.0092	0.0325	0.0338	-0.0254	-0.0122	-0.0245
	(0.0157)	(0.0406)	(0.0405)	(0.0168)	(0.0225)	(0.0345)
Socio-emot. stability	0.0112	0.0031	0.0017	0.0108	-0.0134	-0.0030
	(0.0135)	(0.0346)	(0.0345)	(0.0147)	(0.0190)	(0.0292)
Motor skills	-0.0127	-0.0201	-0.0200	-0.0129	0.0216	-0.0202
	(0.0127)	(0.0329)	(0.0328)	(0.0144)	(0.0191)	(0.0276)
School readiness	0.0079	-0.0048	-0.0064	-0.0048	-0.0055	0.0150
	(0.0105)	(0.0267)	(0.0266)	(0.0116)	(0.0153)	(0.0223)
N	14,256	14,256	14,256	14,256	6,910	4,426

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.9: Robustness checks separately by mothers' education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Baseline	Window size without January & December				Including children born		Without children born	Nonlinear models (marginal effects)	
	(Jul-Jun)	Aug-May	Sep-Apr	Oct-Mar	Nov-Feb	7/2004-6/2005	in Jan & Dec	after 6/2007	Probit	Logit
Mothers' education: Low/medium										
Language skills	-0.0181	-0.0136	-0.0270	-0.0248	-0.0530	-0.0239	-0.0074	-0.0219	-0.0161	-0.0150
	(0.0134)	(0.0149)	(0.0172)	(0.0213)	(0.0305)	(0.0127)	(0.0122)	(0.0152)	(0.0129)	(0.0128)
Socio-emot. stability	-0.0127	-0.0070	-0.0150	0.0050	0.0348	-0.0154	-0.0166	-0.0065	-0.0130	-0.0121
	(0.0120)	(0.0133)	(0.0153)	(0.0188)	(0.0270)	(0.0114)	(0.0109)	(0.0139)	(0.0116)	(0.0117)
Motor skills	-0.0051	0.0040	-0.0037	-0.0031	0.0156	-0.0098	0.0031	-0.0103	-0.0039	-0.0043
	(0.0115)	(0.0128)	(0.0147)	(0.0182)	(0.0267)	(0.0109)	(0.0104)	(0.0132)	(0.0112)	(0.0113)
School readiness	0.0032	0.0014	-0.0024	0.0048	0.0099	-0.0041	0.0039	0.0097	0.0000	0.0004
	(0.0110)	(0.0121)	(0.0139)	(0.0170)	(0.0247)	(0.0104)	(0.0100)	(0.0126)	(0.0110)	(0.0111)
N	22,492	17,716	12,998	8,533	4,115	30,195	26,876	14,629	22,492	22,492
Mothers' education: High										
Language skills	-0.0092	-0.0102	-0.0056	0.0249	0.0272	-0.0150	-0.0068	0.0051	-0.0069	-0.0057
	(0.0157)	(0.0174)	(0.0200)	(0.0247)	(0.0360)	(0.0147)	(0.0142)	(0.0179)	(0.0149)	(0.0149)
Socio-emot. stability	0.0112	0.0086	0.0076	0.0070	0.0080	0.0094	0.0030	0.0069	0.0113	0.0113
	(0.0135)	(0.0149)	(0.0169)	(0.0205)	(0.0302)	(0.0128)	(0.0122)	(0.0157)	(0.0130)	(0.0131)
Motor skills	-0.0127	-0.0246	-0.0235	-0.0130	0.0038	-0.0165	-0.0186	-0.0032	-0.0149	-0.0165
	(0.0127)	(0.0141)	(0.0162)	(0.0197)	(0.0278)	(0.0120)	(0.0116)	(0.0151)	(0.0130)	(0.0133)
School readiness	0.0079	0.0002	0.0015	0.0046	-0.0057	0.0044	0.0028	0.0129	0.0081	0.0079
	(0.0105)	(0.0115)	(0.0130)	(0.0154)	(0.0218)	(0.0099)	(0.0095)	(0.0120)	(0.0107)	(0.0110)
N	14,256	11,274	8,340	5,426	2,591	19,169	16,826	9,081	14,161	14,161

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.