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Parental Leave and Children's Schooling Outcomes: Quasi-Experimental Evidence from a Large Parental Leave Reform

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Abstract

This paper investigates the question whether long-term human capital outcomes are affected by the duration of maternity leave, i.e. by the time mothers spend at home with their newborn before returning to work. Employing RD and difference-in-difference approaches, this paper exploits an unanticipated reform in Austria which extended the maximum duration of paid and job protected parental leave from 12 to 24 months for children born on July 1, 1990 or later. We use test scores from the Austrian PISA test of birth cohorts 1990 and 1987 as measure of human capital. The evidence suggest no significant overall impact of the extended parental leave mandate on standardized test scores at age 15, but that the subgroup of boys of highly educated mothers have benefited from this reform while boys of low educated mothers were harmed by it.

JEL-Code: J130, J240, J220.

Keywords: parental leave reform, maternal employment, human capital, child development, cognitive skills.

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1 Introduction

This paper investigates whether prolonged paid and job protected parental leave has effects on the offspring's cognitive development. The potential channel is reduced maternal employment in the first years of a child's life that has likely increased maternal time for child-rearing. In many industrialized countries, the provision of parental leave is one of the main policy instruments aimed at helping young families reconcile working life with family life. This topic has become increasingly important and prominent in the public debate as female labor force participation rates have been growing over the past decades in many industrialized countries. While proponents of (more generous) parental leave entitlements assert positive consequences for the health and well-being of children and their mothers as well as for the position of women in the workplace, opponents fear that parental leave mandates diminish market flexibility, thereby leading to inefficiencies and lower rates of employment growth (Ruhm 1998, 2000).

The lack of an academic and societal consensus about the potential effects of parental leave on maternal labor market performance and on the development of new-borns is also partly reflected in the large cross-country variation in the generosity of parental leave entitlements in terms of duration of job protection and income replacement level.[1] Since the length of the granted parental leave is relevant for the return-to-work decision, these cross-country differences in legislation help to explain why new mothers in some countries return to work much sooner and spend less time at home with their child compared to mothers in other countries (Ruhm 2000; Tanaka 2005).

Overall, previous empirical studies in psychology and economics have produced mixed evidence regarding the impact of early maternal employment on child outcomes. If anything, the majority of studies seem to support the hypothesis that the labor force participation of mothers during their children's *first* year of life has potentially adverse effects on their subsequent development (Ruhm 2008). In addition, there is some indication for heterogeneous effects across subgroups: children from higher socio-economic backgrounds are potentially more likely to be *negatively* affected by maternal employment, while children from low income families might *benefit* if maternal employment raises family income (Currie 2005). Hence, depending on the specific design of the laws regarding the length of granted leave, the income replacement level during the leave period as well as the medium- to long-

[1] For instance, the US federal Family and Medical Leave Act (FMLA) from 1993 grants a maximum duration of unpaid, job protected leave of 12 weeks (Berger, Hill and Waldfogel 2005). The FMLA regulations apply to only about half of the female workforce; parental leave for the other half is determined in individual employer regulations. In contrast, most European countries provide much longer durations of job protected maternity and parental leave (some of which is even compulsory) with partial or full income compensation (Neyer 2003).

term labor market consequences for the mother, parental leave mandates might affect child outcomes through time effects (more maternal time investments) and potentially through income effects (if household income is reduced due to foregone wage earnings of the mother in the short-run and potentially in the long-run).

The difficulty in estimating the causal effect of early maternal employment on child development is that maternal employment, fertility behaviour and the timing of labor market re-entry after childbirth are choice variables and might be driven by unobserved mother or child characteristics (e.g., ability, fertility and work preferences, role models, regional differences in availability and costs of alternative child care). If particular types of women return to work sooner than others, differences in child outcomes between these groups of mothers might reflect differences in maternal characteristics and intergenerational transmission of ability rather than the causal effect of maternal employment. Reverse causality might be an additional problem, since certain health conditions of a child are likely to impede its mother's return to work.

This study analyses the effects of duration of maternal time at home on long-term cognitive child outcomes in Austria by using an unexpected and unanticipated policy reform that extended the maximum duration of job protected and paid parental leave by twelve months for all eligible mothers giving birth on July 1, 1990 or afterwards. Employed women having a child before this cut-off date were only eligible for job protected and paid parental leave until the child's first birthday while those giving birth afterwards were granted parental leave until the child's second birthday. The reform had a strong impact on the time new mothers stayed at home before returning to work since (a) female labor force participation in Austria in 1990 was already comparatively high, (b) most employed women generally satisfied the eligibility criteria, (c) take-up rates were extremely high and (d) most mothers exhausted the full duration of their leave entitlements (about 80 percent of mothers) (Lalive and Zweimüller 2009; Lalive, Schlosser, Steinhauer and Zweimüller forthcoming). However, although the reform caused mothers to substantially delay their return to work in the short-run, it did not adversely affect medium- or long-run employment and earnings of mothers (Lalive and Zweimüller 2009; Lalive, Schlosser, Steinhauer and Zweimüller forthcoming).

The aim of this paper is to assess the effect of this twelve-months expansion of paid and job-protected parental leave on cognitive skills of affected children, measured at age 15 by test scores from standardized assessments in mathematics, reading and scientific literacy from the international PISA study (Programme for International Student Assessment). The main empirical strategy is based on a Regression Discontinuity (RD) and a related Difference-

in-Differences (DID-RD) which exploit the variation in the duration of parental leave created by the specific cut-off date of the reform.[2] Specifically, we first compare differences in average test scores of children born shortly before and shortly after the reform (born in May/June 1990 versus July/August 1990, respectively). Secondly, we compare these differences with the test score differences in a control year in which there was no reform (children born in May/June 1987 versus July/August 1987). The inclusion of an additional pre-reform control year is motivated by the fact that outcome comparisons across birth months within a given year could be confounded by season of birth or simple age effects (older children being more potentially advantaged at any given test date).

This study contributes to the existing literature in several ways: First, in comparison to the previous quasi-experimental studies, the Austrian reform assessed in this paper is by far the most extensive and generous as it offered 12 additional months of paid and job protected leave and might thus have a stronger impact on child outcomes. Moreover, only a few papers have assessed the effect of maternal care during the child's *second* year of life. Second, in contrast to the study for Germany by Dustmann and Schönberg (2012) which comes closest to the Austrian case in terms of cultural and institutional background, this analysis contains information on parental background and can thus distinguish between heterogeneous effects across subgroups, a distinction to be shown as very important. Third, the analysis sheds light on the causal relationship between maternal employment in the first years of the child's life and the child's cognitive development in a country where most non-parental child care of under three-year-olds is provided informally, for instance, by grandparents, instead of formal day care centres (in contrast to countries like Sweden and Denmark where children participate in formal care-centres already at very young ages). Against this background, the reform most likely caused a replacement of informal care through maternal care, which might have different implications than switching from formal to maternal care.

The overall effect of this large parental leave extension on test scores for the pooled sample of all children is close to zero and statistically insignificant, in line with most of the previous quasi-experimental studies from other countries. However, the subgroup analyses by maternal education and child gender points to significant positive reform effects for children

[2] The majority of previous studies dealt with the endogeneity problem of the maternal return-to-work decision by including as pre-birth controls, by estimating family fixed effects models and comparing sibling differences, by implementing propensity score matching or by employing instrumental variable techniques (Blau and Grossberg 1992; Currie 2005; Almond and Currie 2011; Hill, Waldfogel, Brooks-Gunn and Han 2005). As Currie (2005) notes, each of these studies has severe limitations for causal inference and conclusions drawn from single studies have to be put in specific context and compared to results using other methods. A few recent papers try to tackle the identification problem by exploiting exogenous changes in parental leave mandates (Baker and Milligan (2010, 2011) focus on Canada, Carneiro, Løken and Salvanes (2010) on Norway, Dustmann and Schönberg (2012) on Germany, Liu and Nordstrom Skans (2010) on Sweden, Würtz Rasmussen (2010) on Denmark).

of highly educated mothers, especially for boys. In contrast, schooling outcomes of children from lower educated mothers seem to have been harmed (boys have lower test scores and girls have a higher likelihood of being in a lower grade). Hence, in an institutional setting with no formal child care system for very young children, early maternal employment of highly educated women might have detrimental effects for their offspring. To what extent such potential negative effects can be mitigated or reversed through a high-quality formal day care system is an open question.

The paper is organized as follows: the next section introduces the underlying theoretical framework and highlights and compares the findings and setup of previous quasi-empirical studies of early maternal employment on child outcomes. This is followed by an overview of the institutional background in Austria in Section 3. The section includes details of the Austrian reform as well as a summary of findings from previous studies which evaluated this reform with respect to labor market and fertility outcomes. Section 4 explains the identification strategy and discusses critical assumptions and empirical challenges. Section 5 describes the data and outcome variables. The results of the main specification are presented and discussed in Section 6, while Section 7 contains several robustness checks and sensitivity analyses. Section 8 concludes.

2 Parental Leave, Maternal Employment and Child Development

2.1 The role of maternal employment in the cognitive ability production function

It is helpful to structure the discussion and analysis of potential effects of maternal employment on child outcomes using the following cognitive ability production function (Bernal and Keane 2011; Dustmann and Schönberg 2012).

$$\boldsymbol{Y_{it} = Y_{it}(T_{it}, G_{it}, C_{it}, F_{it}, P_{it}, \omega_i)} \tag{4.1}$$

Cognitive ability of child *i* at age *t* (Y_{it}) is determined by several inputs, namely maternal (parental) time investment, T, up through age *t*, market-purchased goods and services other than non-parental child care, G, which depend on family income (examples are quality of housing, additional educational material, nutrition, health expenditure), time investment through non-parental caregivers, C, (i.e. time in non-parental child care), any direct effect of family composition, F, e.g. number of siblings (interaction between siblings; quantity-quality trade-off), birth order, time intervals between siblings, public investments in children and child development, P, (e.g., early child development programmes, public child care facilities and schools, state child health programmes and health insurance) and an idiosyncratic ability endowment, $\boldsymbol{\omega}$, e.g., through intergenerational transmission of genes. As

the function differentiates between different child ages it allows for varying effects of certain inputs at particular stages of child development.

In this framework, there is a clear trade-off between maternal time investment and maternal earned income which could be used to buy market-based inputs. It is possible that the reduction in maternal time inputs of working mothers can be at least partly compensated by other goods (e.g., health investments, better nutrition) or by higher quality time investments of other caregivers. Generally though, the time-income trade-off can be mitigated to the extent that mothers receive compensating parental leave payments while on leave.

Several potential mechanisms through which increased maternal time might positively affect child development are discussed in the literature. One is prolonged breastfeeding which could lead to better health outcomes of children (Baker and Milligan 2008).[3] Increased maternal care time might improve children's cognitive development by improving their health, for example, by better monitoring of their health status and more timely doctor visits (Berger, Hill and Waldfogel 2005), by preparing healthier meals and lowering risk of injuries and infectious disease (Morrill 2011). Early maternal employment, especially when exceeding 10 hours per week, might also negatively influence the attachment of mother and child and might lead to children's behavioural problems (Brooks-Gunn, Han and Waldfogel 2002). It is also possible that the physical burden of market-based work leaves mothers with less energy for stimulating and nurturing their children (Ruhm 2004; Waldfogel, Han and Brooks-Gunn 2002). Conversely, a prolonged absence from work might raise the risk of social detachment and of depressions by mothers who stay at home, which in turn lowers the quality of maternal time and might have adverse effects for children (Baum II 2003).[4]

2.2 Empirical evidence

All recent studies exploit an unexpected extension of the granted parental leave duration which significantly increases the time mothers stay at home before returning to work after childbirth.[5] Most of these studies find either no or only negligible effects of prolonged parental leave on cognitive child outcomes. One exception is Carneiro, Løken and Salvanes (2010) who find for Norway significant positive effects of prolonged maternal time at home during the child's first year on medium-run schooling achievement, IQ measures and height. The effects are stronger for children of mothers with low educational attainment. However,

[3] However, despite a significant impact of prolonged parental leave entitlements on breastfeeding duration in Canada, most of Baker and Milligan's (2008) results do not reveal any positive health effects for children.

[4] In this simplified framework we implicitly hold partner's labour supply and child care input constant. This assumption is less restrictive in countries where the bulk of parental child care is traditionally supplied by the mother and family life follows the 'male bread-winner model' as in Austria in 1990.

[5] See Table A1 in the Online Appendix for a summary of these studies and their findings.

Liu and Nordstrom Skans (2010) report for Sweden a zero average effect based on the whole sample but positive effects for children of better-educated mothers. Interestingly, we in this study also find zero average effect but large and significantly positive effect for children of highly educated mothers. In order to reconcile these seemingly contradictory findings it is important to highlight the essential differences between these studies which complicate straightforward comparisons. The analyses vary predominantly with respect to: (1) *The affected age group of children*: Does the particular extension of parental leave allow women to stay at home longer during the first year of life (e.g., an extension from 8 to 12 months) or does it affect the period when the child is more than two years old? This point is highly relevant if the importance of maternal care varies over the different development stages of the child. (2) *The length of the extension*: The analysed parental leave extensions vary between six weeks and 18 months. The granted length of the extension is likely to influence the additional time that mothers stay home. If there is a positive effect of maternal time on child outcomes and this effect is increasing with time input, then one would expect differential effects depending on leave duration. (3) *The measure of cognitive development and age at its measurement*: While some studies focus on short-run effects measured before the first birthday (parent-reported assessments or psychological tests), others compare medium- or long-term outcomes up to age 29 (e.g., using completed educational attainment). (4) *Different institutional environments*: There are strong differences across countries (and over time) in terms of prevailing non-parental child care arrangements (formal centre-based or informal care by relatives) which determines the type of care likely to be substituted by prolonged maternal care. (5) *The type of reform and any indirect effects on other supposedly relevant determinants of child outcomes like income and fertility*: e.g., does the reform expand the duration of fully, partly and/or unpaid leave? These indirect effects could alter the (opportunity) costs of children and also enhance or change the 'quantity-quality' trade-off. (6) *The precision of the data and the estimations*: Can eligible mothers be identified (only possible in the Norwegian study) or can children be linked to parents? Are the studies based on representative surveys or administrative datasets? (7) *The estimation strategies*: the exact implementation of the DID and RD estimations differs across studies as do the control groups.

To sum up, the institutional background and the details of the reforms vary widely across countries and seem to play an important role for the effect of parental leave on child outcomes. In terms of institutional and cultural set-up, Austria comes closest to Germany, while the type of reform (paid leave extension between the child's first and second birthday) is more similar to the evaluated reform in Sweden where, however, participation rates of one-

to two-year-olds in formal child care are very high. Nevertheless, the reform in Sweden led only to a three months extension of parental leave, while in Austria the extension comprised 12 months. This way the analysis in this paper helps to shed more light on the influence of maternal employment beyond the child's first birthday when formal day care for this age group is generally not available. Furthermore, the Austrian reform is unique in that it involves an exceptionally long extension of paid parental leave.

3 Institutional setting and background

The history of parental leave in Austria dates back to 1957 when working women became entitled to an unpaid, but job protected leave of up to six months on top of the paid mandatory maternity leave of 12 weeks, making Austria the first country in Europe to introduce parental leave (Neyer 2003).[6] 1974 saw an introduction of a monthly flat-rate cash benefit to all mothers on parental leave. In 1990, the amount of the regular flat-rate parental leave payment was about 340 Euros per month corresponding to 31 (40) percent of gross (net) *median* female earnings (Lalive and Zweimüller 2009).

The empirical analysis will exploit a quasi-experiment that was created by the amendment to the parental leave legislation that came in effect on July 1, 1990 (*Karenzurlaubserweiterungsgesetz, June 27, 1990, BGBl. Nr. 408/1990*). The main aspect of this reform was the extension of the maximal duration of the optional paid and job protected leave from the child's first up to the child's second birthday.[7] This extension was only granted to mothers whose children were born on or after the cut-off date of July 1, 1990. There were no 'transition rules' allowing mothers who gave birth before July 1, 1990 to benefit from the new regulations. This increase of 12 months of paid and job protected parental leave is much larger than any of the comparable reforms that took place in other countries and that have been evaluated in terms of child outcomes. The reform was announced and implemented only shortly before it came into effect. Therefore it was not possible for parents to adjust their fertility timing in order to take advantage of the more generous parental leave regime (i.e. there were no anticipatory fertility effects).

The fact that the duration of the leave entitlement before and after the reform in Austria was actually binding and thereby exogenously determining the minimum length of

[6] Fathers became eligible for parental leave only as of January 1, 1990. Their entitlement to parental leave was conditional on the mother meeting all eligibility criteria. The take-up rate of parental leave of fathers remained close to zero during the 1990s (0.2 percent in 1990, 1 percent in 1997; BMUJF 1999).

[7] In Austria, in 1990, working mothers were subject to a compulsory maternity leave of 8 weeks before and after delivery (job protected leave with 100% replacement of the average net earnings of the proceeding 13 weeks). Following this compulsory maternity leave, mothers were entitled to a job protected and paid leave up to the child's first (second) birthday for those born before (after) July 1, 1990.

leave take-up has been demonstrated by Lalive and Zweimüller (2009) and Lalive, Schlosser, Steinhauer and Zweimüller (forthcoming). Overall, the reform has had a significant impact on the time mothers take parental leave after birth. Only about ten percent of mothers return to work immediately after the end of the mandatory maternity leave, i.e. two months after childbirth, and this pattern holds generally true before and after the reform (see Figure 1, Lalive and Zweimüller 2009).[8] The figure reveals also a jump of roughly 20 percentage points at the time when the legal leave entitlement expires (at 12 months before the reform and at 24 months after the reform). The reform reduced the short term (within two years) return to work by approximately 35 percentage points. The average duration of parental leave take-up and receiving benefits (after mandatory maternity leave) increased from 10 to 20 months and most mothers exhaust the full duration of their leaves (Lalive, Schlosser, Steinhauer and Zweimüller forthcoming; Lalive and Zweimüller 2009). A rough estimate of the overall share of mothers who were eligible for parental leave in 1990 can be attained by calculating the share of mothers on mandatory maternity leave among all mothers. For 1990, this figure is about 77 percent (the corresponding figure in 1987 is 73.8 percent).[9] Estimated parental leave take-up rates of eligible mothers range between 93 and 96 percent (Kreimer 2002).

Despite the substantial changes in return-to-work behaviour, there are no medium-term effects on alternative labor market outcomes like average number of months in employment or earnings per month (Lalive and Zweimüller 2009). Even though in the short-run post-reform mothers work significantly fewer months on average and have lower earnings than pre-reform mothers, there are no significant differences in these outcomes after ten years.

The reform had also a positive and significant impact on fertility outcomes in the short (within three years after the previous birth) and medium-run (after ten years) (Lalive and Zweimüller 2009). However, high-wage mothers seem to have reacted to the extended leave period mainly by changing the spacing between births, but not overall fertility (reducing very short birth intervals and longer intervals at the same time), while low-wage mothers have additionally increased the total number of births in the next ten years. These heterogeneous fertility effects play an important role for the interpretation of our empirical results.

3.1 Possible effects of the Austrian reform on child outcomes

There seem to be two potential channels through which the 1990 parental leave extension in Austria could have affected child development: first, a 'quality channel' which works through the potentially superior quality of maternal care as opposed to alternative,

[8] See Figure A 1 in the Online Appendix.
[9] Calculations are based on numbers in the Austrian Family Report 1999 (BMUJF 1999), p. 152-157.

informal forms of child care, and second, the 'fertility channel', which works through changes in the fertility behaviour, i.e. the number of children and the spacing between births.

As noted, the parental leave reform in 1990 significantly raised the time new mothers stayed at home after childbirth between the child's first and second birthday, but it did not affect medium- to long-run income and labor market outcomes of the average mother. Thus, it is unlikely that the reform exerted a negative income effect on child outcomes driven by medium- to long-term income losses; if anything, the leave extension generated a relatively small and only short-term income loss caused by foregone earnings during the additional leave months.[10] At the time of the reform the prevailing form of child care for children under two years of age was almost exclusively informal care provided by grandparents or other persons.[11] Under the hypothesis that maternal care is superior to informal care for very young children, one would expect that the prolonged parental leave period had a positive impact on child outcomes. This could be especially true for better educated mothers if they are able to provide higher quality and more 'productive' maternal care, for instance, through better access to knowledge on how to foster cognitive development of children (Grossman 2006).

However, the reform may also alter the fertility behaviour of parents. If shorter spacing between births reduces the time and material resources that are allocated to each child, this effect could have negative implications for child development and cognitive outcomes. On the contrary, if the relation between spacing and child outcomes is non-linear, a positive effect of the reduction in extremely short birth intervals could outweigh a negative effect from the average reduction in spacing. If the reform increased the total number of births per woman, this could work in the opposite direction and diminish or reverse a potentially positive time effect.

4 Empirical approach

Given the unexpected and strict implementation of the prolonged parental leave period for all children born on July 1, 1990 or later, it is possible to use a Regression Discontinuity methodology and also a Difference-in-Differences analysis based on this RD setup (RD-DID)

[10] However, for those women who would *not* have returned to work after the child's first birthday under the old regime anyways, the prolonged parental leave payments might have implied a *gain* in short term income as these mothers would not have earned *any* income during the second year. Furthermore, it is difficult to assess to what extent this short-term income loss from working mothers translates into a potentially negative income shock for the child: if non-parental child care is costly, a fraction of the mother's earned income will be spent on child care and it is not clear whether the remaining amount of maternal income is actually larger or smaller than the parental leave payments. Only if the reform leads to a significant short-term income loss, there might be negative income effects which could work against any positive time effect.

[11] Only about 2.5 percent of children aged zero to two were enrolled in formal day care (own calculations of gross enrolment rates based on birth numbers and children in formal child care; official statistics on early child care are recorded since 1995).

to identify the effect of extended maternal care on child outcomes.[12] The RD-DID regression specifications that we estimate are the following:

$$y_i = \alpha + \beta_1 Post\ June + \beta_2 bc1990 + \beta_3 Post\ June * bc1990 + \theta_m birth\ month + \varepsilon_i \quad (4.2)$$

$$y_i = \alpha + \beta_1 Post\ June + \beta_2 bc1990 + \beta_3 Post\ June * bc1990 + \theta_m birth\ month + \mu X + \varepsilon_i \quad (4.3)$$

$\boldsymbol{y_i}$ is the measure for cognitive child outcome (i.e. test scores from standardized tests), *Post June* is a dummy indicator for children whose birthday is on or after July 1 (July – December) and the coefficient $\boldsymbol{\beta_1}$ captures all possible permanent and general differences between children born in the first and the second half of a given year; *bc1990* is a dummy indicator for the birth cohort 1990; the interaction effect between *Post June* and *bc1990* identifies all children whose mothers were affected by the reform and eligible to a longer parental leave – $\boldsymbol{\beta_3}$ is the coefficient of interest and measures the treatment effect; to account for possible season of birth effects as well as age effects the regressions include a set of *birth month* dummy variables. To control for possible differences in sample composition over time, equation (4.3) contains additionally a set of parental and other background characteristics (*X*).

If the assignment into treated (=post reform; 24 months PL) and control group (=pre reform; 12 months PL) is 'as good as random', a simple representation of the estimated treatment effect $\boldsymbol{\beta_3}$ (estimated by OLS) is

$$\beta_3 = \ y_{1990,post} - y_{1990,pre} \ - \ y_{1987,post} - y_{1987,pre} \ . \quad (4.4)$$

This is the difference in average cognitive outcomes (test scores) of children born after versus before the reform (whose mothers were eligible to 24 versus 12 months of paid parental leave respectively) less the difference in outcomes of children born before and after July 1, 1987 who were not subject to the reform. The advantage of this RD-DID approach is that potentially confounding systematic differences between children born before and after July 1 which could otherwise exert a bias are differenced out: First, the test scores used in the analysis stem from tests that took place within a certain month (e.g. April) and children born in January 1990 will be about 12 months older at the time of the test than children born in December 1990. If age in itself has a positive effect on outcomes, any potentially positive effects of the reform will be downward biased, since post-reform children are always younger than pre-reform children. Second, there might be systematic season of birth effects affecting the composition of children and their parents over the year. If certain types of couples are more likely to have babies in particular months of the year this might also impact upon the distribution of test scores across birth months.

[12] Dustmann and Schönberg (2012) used a similar approach to study the German reform.

An important identifying assumption in this approach is that mothers could not self-select into treatment or control group. This basically requires that mothers could not manipulate the date of childbirth around the cut-off date of July 1, 1990. Lalive and Zweimüller (1999) provide several arguments and evidence in support of this assumption: first, an assessment of newspaper reports about a potential reform of parental leave duration revealed that the public discussion did not start before November 11, 1989 and that it was not clear until April 5 whether and when such a reform would be implemented. This timing of policy decision and implementation makes anticipatory adjustments to fertility plans highly unlikely, especially when taking into account that successful conception and date of childbirths cannot be perfectly controlled and planned by parents. Furthermore, as also argued by Würtz Rasmussen (2010), it is biologically infeasible to postpone the expected date of delivery with the exception of planned Caesarean sections. However, an analysis of number of births during the days shortly before and after the reform did not indicate a higher density of births on July 1 or the days after (Lalive and Zweimüller 2009).

Another assumption is the *common trend* assumption which requires seasonal patterns or age effects to be constant across years. The common trend assumption might be problematic if there are changes over time. Certainly, this assumption becomes less restrictive if one limits the sample to children born extremely close to the cut-off date as these children are very similar in age as well as in season of birth. Another advantage of narrowing the window of birth months before and after the reform would be that these children are more likely to face identical kindergarten and schooling regulations and rules, e.g., the Austrian school year typically runs from September to August. Furthermore, their mothers were exposed to similar macroeconomic conditions and labor market developments. However, this strategy would require a very large data set. Unfortunately, the available test scores data base does not meet this criterion. Hence, given the data at hand, there is a *trade-off* between limiting the analysis on children who are as similar as possible (which would also reduce the likelihood of violating the common trend assumption) and having a sufficiently large sample size. Therefore, we estimate each specification several times while successively narrowing down the window of birth months.[13]

[13] Another reason for narrowing the sample window around the cut-off date as much as possible is the proximity of the reform cut-off date to the Austrian school entry cut-off date. All children having their sixth birthday before September 1 are obliged to start school in the same year (typically at the beginning of September). However, until 1999, enforcement of this school entry cut-off date was not strict, conceding some discretionary power to parents regarding the decision whether their child was 'ready' for school or whether it should start one year later. A comparison of the distribution of children in lower than regular grades across birth months for the cohorts 1990 and 1987 (PISA data 2006 and 2003) reveals that this phenomenon is relatively consistent over time which is an important prerequisite for our RD-DID estimation strategy (figures available by the authors upon request).

Since the data neither allow identifying children whose mothers were actually eligible for the more generous parental leave entitlements nor contain information on actual duration of leave taking of mothers, the estimated effect will represent the intention-to-treat effect of the reform, i.e., the reduced form effect of being eligible to 24 instead of 12 months of parental leave. This intention-to-treat effect will be a lower bound estimate of the effect of prolonged parental leave and maternal care on child outcomes, since it is estimated on the full sample including children of mothers who did not change their behaviour because of the reform. This latter group consists of non-working mothers as well as working mothers, who stop working post-birth for much longer than the granted parental leave period irrespective of the actual legislation, and of mothers who return to work very early irrespective of the generosity of the system, either non-eligible working mothers or working mothers would return to work immediately after the compulsory maternity leave period independent of the additional parental leave provision. Furthermore, the intention-to-treat estimate will represent an overall net effect of the reform combining all possible channels through which the parental leave extension might have affected child outcomes.

In the empirical part we will perform subgroup analyses by educational attainment of the mother for two reasons. First, stratifying the sample by mother's education might yield estimates closer to the actual effect of the reform. Mothers with higher (lower) education have on average higher (lower) labor force participation rates and, hence, a higher (lower) likelihood of parental leave eligibility. We will therefore distinguish between mothers having completed post-secondary or tertiary education, 89 percent of whom participate in the labor force (employed or unemployed) and mothers whose highest educational degree is higher secondary school or less, of whom 78 percent are in the labor force.[14] We label these groups as High Educ. Mothers and Low Educ. Mothers, respectively. However, apart from the employment channel, we do not expect maternal education to have any differential effect on the likelihood of eligibility, since – conditional on employment – parental leave eligibility was almost universal (Lalive, Schlosser, Steinhauer, Zweimüller forthcoming). Second, as discussed in Section 3, the parental leave extension had heterogeneous effects on fertility and spacing between births of low- and high-wage mothers which in turn might lead to differential child outcomes (quality-quantity trade-off). Furthermore, an analysis of the Austrian time use survey 1992 shows that, all else equal, highly educated mothers devote on average almost one hour more to child care than mothers with compulsory education (Neuwirth 2004). Hence,

[14] Numbers calculated based on the Austrian Census from May 1991 (see Table A2 in the Online Appendix).

there might be also quantity differences in time investments between low and high educated mothers (and possibly quality differences).

5 Data

We use in this study data from the OECD's Programme for International Student Assessment (PISA) for Austria. The PISA studies from 2006 and 2003 capture the relevant 'reform' birth cohort born between January and December 1990 as well as the 'control' birth cohort from 1987. Several important features make the PISA data especially suitable for our analysis: first, the PISA data provide results from *standardized tests* of cognitive skills in terms of reading, mathematical and scientific literacy. The focus of PISA is less a pure assessment of curriculum based knowledge, but more an evaluation of general skills needed for adult life and of the ability to apply knowledge to real-life problems. Second, the tests are administered to a nationally representative sample of 15-year old students independent of their current grade level in school. In contrast, other international studies like TIMSS and PIRLS assess students in particular grade levels, e.g. 4^{th} and 8^{th} grade, and are thus not representative for a particular birth cohort. Comparisons of outcomes across birth months would be biased if, for example, the propensity of grade retention or early or late school entry differs between children born closer or further away from the school entry cut-off date. Third, the PISA data files contain important student-reported background information on the student (e.g., gender, birth year and month, nationality, attitudes), the student's parents (educational achievement, nationality, occupational information) and the school (e.g., school programme, location, school size, resources).

However, the Austrian PISA data have also several disadvantages: there is no retrospective information on maternal labour market participation at the time of birth which prevents a clear identification of mothers who were truly eligible for parental leave. Moreover, PISA does not contain publicly available information on exact birth dates which prevents any refinement of the analysis beyond the month level.

To increase the cohesiveness of the data for the analysis we first dropped from the sample students whose mothers are highly unlikely to have been eligible for parental leave or affected by the reform. This included children who were not born in Austria and whose mothers were thus unlikely to work in Austria at the time of the reform in 1990. Furthermore, a few observations had missing information on maternal education. A small number of students *in schools for children with special needs* were also excluded from the sample to

increase consistency across students and across years, since these children have a completely different curriculum and were administered special test items.

The variables used in the analysis are described in Table 1. The main outcome variables measuring cognitive skills are test scores in mathematics, reading and science. The test scores in each subject are rescaled by the OECD so that the mean across all participating countries is 500 points and the standard deviation is 100 points.[15] Two other outcome variables are binary variables indicating i) whether a student is in a lower than the regular grade level given his or her birth month and ii) whether the student is enrolled in a school track giving access to university or college education (academic track).[16] Student's level data includes gender, month of birth, an indicator for school location in an urban area, mother's and father's highest completed level of education, and migration status of the family (whether the family speaks German at home and whether mother and father were born in Austria; the base category are non-German speaking families).

The mean values of the outcome variables and variables of the PISA 2006 and 2003 data are presented in Table 1, separately for children born in May and June and for children born in July and August. The evidence in the table show that there are no significant systematic differences between pre- and post-reform children in 2006, which supports the important assumption that the 1990 reform was unexpected and that there was no systematic self-selection of particular types of parents or families across the cut-off date.

6 Empirical results

A first graphical impression of the RD-DID setup for all three test subjects is provided in Figure 1.The figure plots average test scores by birth month together with fitted lines of the predicted test scores to the left and to the right of the reform cut-off date (July 1, vertical line) for both, the reform cohort 1990 and the control cohort 1987. Overall, there is a slightly negative trend in average test scores across birth months which could be explained by possible age effects as discussed earlier. Crucial, however, is the fact that there is no jump in test results for children from the 1990 reform cohort born immediately before and after the reform cut-off date. Indeed, for all three test subject, the predicted fits to the left and to the

[15] To ease comparison of results across survey years, the test results were rescaled such that the reading and mathematics reporting scales of 2006 are equal to those in 2003. The test results in science are rescaled such that the mean is 500 and the standard deviation is 100 for the 30 OECD countries participating in PISA 2006 (see OECD 2009, pp. 157-158). However, any general differences in test scores across years and subjects will be accounted for in the analysis by controlling for years and by running separate regressions for each subject.

[16] In Austria, students are allocated to different educational tracks after the fourth grade (i.e. at age 10 or 11) which is relatively early compared to other European countries (Schneeweis and Zweimüller 2009). Starting with grade level nine, a further differentiation into specific tracks takes place.

right of the reform date are continuous at the cut-off, suggesting no effect of the parental leave reform on test scores. This zero finding is substantiated by the virtual overlap of the test scores and fitted lines of the 1990 reform cohort with those of the control cohort from 1987 (except for scientific literacy).

Repeating this graphical analysis separately for male and female students produces the same continuous pattern indicating no reform effect, neither for boys nor for girls.[17] However, these findings of no average effects for the entire group as well as for the gender subgroups mask quite substantial reform effect heterogeneity with respect to child gender in combination with maternal education. Figure 2 and Figure 3 show the RD-DID graphs for boys and girls, respectively, and differentiate between children of mothers with higher and lower educational attainment. In particular, when focusing on the black fitted line for sons of highly educated mothers born in the reform year 1990 (Figure 2, top panel), it appears that there is a clear positive jump in test scores immediately after the reform. The overall downward sloping curve is simply shifted upward for those children born after the cut-off date. In contrast, the grey fitted lines for the corresponding subgroup of children from the control cohort 1987 are continuous around the cut-off indicating no systematic differences in test scores between children born before and after July 1 in the absence of the parental leave reform. Turning to the lower panel of Figure 2, this pattern is different for sons of mothers with lower educational attainment: the predicted fit for this subgroup shows either no reform effect (mathematics) or a negative, but very small jump in reading and science test scores after the reform. The corresponding grey lines for the 1987 control cohort are once again continuous at the cut-off date. Overall, the graphs in Figure 2 suggest a positive reform effect only for boys of higher educated mothers and a zero or slightly negative effect for sons of less educated mothers. These heterogeneous reform effects with respect to maternal education do not seem to hold for girls, though. None of the separate graphs for girls do show any substantial jumps at the cut-off date (Figure 3).

To test whether the test score gap between June and July children is significantly different from zero, we regress test scores from the three different subjects on a set of birth month indicators with June as the excluded base category (controlling for parental background). The estimated 'born in July' coefficients are reported in Table 2 for separate regressions by subject, maternal education and child gender. The first two columns in Table 2 show the results from the full sample of mothers. As the graphs in Figure 1 suggested the average test scores do not differ significantly between pre- and post-reform children in the full

[17] See Figure A2 in the Online Appendix. Separate graphs by maternal education (Figure A3, Online Appendix) reveal a positive jump in test scores at the reform cut-off for children from highly educated mothers.

sample and this applies to all three tested subjects. Furthermore, the estimated coefficients change only marginally after controls for parental background are included in the regression (although standard errors become slightly smaller). Comparing the results by gender (columns 1 and 2, middle and bottom panel) reveals that there might be gender differences as the estimated coefficients for boys tend to be positive (but insignificant), while the estimated coefficients for girls tend to be negative.

Restricting the sample to the subsample of children of higher educated mothers (columns 3 and 4) confirms the graphical impression of a positive effect: the estimated effects become much larger and are significantly different from zero (at the five percent level) for reading and science. The separate analyses by gender reveal that the differences are significantly different from zero only for boys. For completeness, the two columns on the right show the respective results for children of mothers with lower educational attainment. Most of the coefficients have a negative sign implying a decline in test scores after the reform; however, none of these effects is statistically significant.

6.1 Regression Discontinuity-Difference-in-Differences estimations (RD-DID)

As a next step, we refine the analysis by estimating RD-DID models in order to take account of potentially confounding age, seasonality and school-entry effects. The results of the main RD-DID estimates of the effect of the parental leave extension on PISA test scores are presented in Table 3 for the full sample (all mothers) and separately by maternal education. The estimation window covers children born two months before and after the reform (May to August). For each of the three samples the first column shows the results without any control variables (columns 1, 3 and 5), while the second column contains results after controlling for background variables to account for potential changes in sample composition across years (and possibly across months). This more refined estimation approach confirms the findings from the simple graphs shown earlier: the treatment effects of the reform (being born post June in the year 1990) are close to zero in the full sample (columns 1 and 2). Adding background controls changes the coefficients slightly, but given the size of the standard errors these differences are not statistically significant.[18] The average effect on reading test scores is a little bit larger and negative, but still not significantly different from zero.

In contrast to these neutral findings are the results from the regressions based on the subsample of children of mothers with higher education. The average effect for all three test

[18] Standard errors were clustered by school programme, school location and gender to account for the fact that test scores of students in the same programme, location and gender are likely to be correlated and not independent of each other.

subjects is significantly positive and of remarkable size, indicating that the extended parental leave period raised test scores by about 22 percent of a standard deviation (a little bit less in mathematics).[19] It is possible that these differences in results between the full sample and children of highly educated mothers are partly related to the different levels of parental leave eligibility (due to differences in labour force participation rates). More importantly though, the results for this subgroup are more likely to show the pure *time and care* effect of the parental leave extension, since the reform did not affect overall fertility or labour market success of mothers with higher earnings (proxied here by higher educational attainment). In comparison, the effects for children of mothers with lower education are generally negative and even statistically significant for the reading test scores. Although the fraction of affected mothers in this group is smaller, it seems that these children have not benefited from the reform. Furthermore, the statistically significant negative coefficients suggests that the heterogeneous reform effects with respect to maternal education are not driven by differential levels of parental leave eligibility, but by other mechanisms. For instance, the negative effect could be attributed to the increased levels of *fertility* of low-wage mothers and to less available resources (time and market goods) per child (i.e. a possible 'quantity-quality' trade-off), to shorter time intervals between births or maybe to a lower quality of maternal time in the sense of ability to foster cognitive child development.

To shed more light on potential gender differences regarding the effects of maternal employment on the cognitive development of children, we run separate regressions for boys and girls (Table 4). Even though sample sizes become rather small in this subgroup analysis, the estimates seem to indicate that boys react more strongly to the reform than girls, especially when looking at those with higher maternal education (columns 3 and 4). While both, post-reform girls and boys, have higher test scores on average, only the coefficients for the boys are statistically significant from zero and are almost three times as large as that for girls (the effect on reading and science test scores for males corresponds to about 0.3 and 0.4 standard deviations). These results would be in line with potential differences in needs and in development between girls and boys at very young ages causing boys to benefit comparatively more from maternal vis-à-vis informal care between the age of one and two. For instance, Brooks-Gunn, Han and Waldfogel (2002) state that boys seem to be more 'vulnerable to early stressors' and react more adversely to non-maternal child care. Similarly,

[19] As a further refinement, we add a set of dummy variables for the different school programs (which are highly correlated with different average test scores). As expected, their inclusion helps to increase the precision of the estimates. The treatment effects get larger and more significant. However, due to the potential endogeneity of these variables these results are only reported in the Online Appendix, Table A 3.

two US studies on the effect of early maternal employment on children's health outcomes find that boys are more adversely affected than girls (Gennetian, Hill, London and Lopoo 2010; Morrill 2011). As before, the results for the pooled sample are rather small and insignificant (columns 1 and 2) and the results for children of mothers with lower educational attainment (columns 5 and 6) show negative coefficients which are even significantly different from zero for boys in some specifications.

Given the trade-off between raising the sample size and the need to keep the estimation window as narrow as possible to reduce the influence of possibly confounding factors, we re-run the regressions symmetrically while gradually reducing the window of included birth months to the left and to the right of the cut-off date (four/three/two months before and after).[20] The results in Table 5 are reported separately by mother's education. The results for children of mothers with higher education (columns 1 to 3) reveal that increasing the sample size symmetrically to the left and to the right of the cut-off leads to positive, but smaller and insignificant coefficients in the pooled sample on boys and girls (top panel). However, the estimated effects for boys remain highly significant and positive. Those for girls become much smaller and even switch signs. The results for children of lower educated mothers become less negative (or more positive) the more months are included in the analysis. For this subgroup, most of the results remain negative and become generally insignificant; the strongest effects are still found for boys.[21]

To test whether these results are influenced by children born after the school-entry cut-off date (September 1), who are thus in lower grade levels than students born before September, we alternatively added further birth months only to the left of the cut-off date, while holding the number of months to the right constant (July and August).[22] The results change only marginally indicating that the previous results were not confounded by the school-entry cut-off regulation.

[20] We also estimated our main RD-DID specification as well as the robustness checks and placebo tests using a one-month window. However, especially for our subsample analyses, the sample sizes became too small to derive solid and meaningful results.

[21] As a rough check of whether the reform effect was mainly due to replacing informal by maternal care, we re-estimated the regressions from Table 5 excluding students from Vienna, where the proportion of children aged 0-2 enrolled in childcare during the 1990s was much higher than in the rest of Austria. The results in Table A 4 (Online Appendix) seem to support this hypothesis: the estimated effects for children of highly educated mothers are slightly larger, while the negative coefficients for sons of low educated mothers get closer to zero. This is what one could expect if the reform implied more of a trade-off between formal versus maternal child care in Vienna with more severe effects for children (especially sons) of mothers with lower education. Excluding this region from the sample should consequently reduce the pronounced negative effect of the reform.

[22] For results, see Table A 5 in the Online Appendix.

6.2 Difference-in-Difference-in-Differences estimations (RD-DDD)

The RD-DID estimation strategy assumes that age-related differences across birth months (season-of-birth, age-at-test, school-entry effect) are constant over time or at least over the three years that lie between the two birth cohorts in the PISA waves 2006 and 2003. As a further check, we add the German PISA sample as an additional control group participating in both relevant years without being affected by the Austrian parental leave reform in 1990. We chose Germany as control region as it is very similar to its neighbouring country Austria not only in terms of its schooling and tracking system (Schneeweis and Zweimüller 2009). Both countries are also very close in terms of their child care institutions as well as cultural values and attitudes towards the role of families and mothers (Neyer 2003). Furthermore, the PISA test language in both countries is German.

By including a further control group to the analysis the effect of the parental leave reform on child outcomes will be estimated using a Regression Discontinuity in a triple difference estimation design (RD-DDD). In addition to the previous RD-DID regressions, the regressions contain a further country variable 'Austria', as well as interaction effects between this country variable and 'Post June' births and the birth cohort dummy variable 'bc1990' and the triple interaction term of the 'Post June', 'bc1990' and 'Austria' dummy variables.

$$y_i = \alpha + \beta_1 Post\ June + \beta_2\ bc1990 + \beta_3 Post\ June * bc1990 + \beta_4 Austria + \beta_5 Austria * bc1990 + \beta_6 Austria * Post\ June + \beta_7 Post\ June * bc1990 * Austria + \theta_m birth\ month + \mu X + \varepsilon_i \quad (4.5)$$

The OLS estimate of the treatment effect $\boldsymbol{\beta_7}$ now becomes:[23]

$$\beta_7 = y_{1990,AUT,post} - y_{1990,AUT,pre} - y_{1990,GER,post} - y_{1990,GER,pre} - y_{1987,AUT,post} - y_{1987,AUT,pre} - y_{1987,GER,post} - y_{1987,GER,pre} \quad (4.6)$$

Table 6 displays the estimated treatment effect of the parental leave reform in Austria based on the RD-DDD specifications. Generally, the RD-DDD results correspond to the previous RD-DID estimates. Children of higher educated mothers seem to have benefited from the parental leave extension – the estimated coefficients are positive for all children, but only significant and larger for boys (the estimated coefficients are slightly larger than the RD-DID estimates, corresponding to approximately between 0.4 and 0.7 standard deviations).In contrast, the results for children of lower educated mothers become more negative in the pooled sample of boys and girls and are even statistically significantly for the reading test scores. Splitting the sample into male and female students reveals that this negative effect is

[23] AUT indicates Austria; GER indicates Germany.

mainly driven by boys. In contrast to the previous RD-DID results, the RD-DDD estimates for boys now become much larger and more significant. The estimates for the girls are generally not very large (albeit positive) and insignificant.[24]

7 Further robustness checks and alternative outcome variables

As a robustness check of our estimates we use only data from PISA 2006 and repeat the RD-DID regressions using German students as control group (instead of Austrian students from the pre-reform year 2003). This estimation strategy replaces the assumption of a common month of birth trend across years by the assumption of a common month of birth trend across regions. Again, the results for the children of higher educated mothers are generally consistent with the main previous findings (see Table 7; the estimated effects for boys become even more significant).[25] For children of lower educated mothers the estimated effects in the pooled sample remain insignificant. However, the estimated coefficients for boys become insignificant; those of the girls increase in size and become positive in the extended samples (including three or four pre- and post-reform birth months). When using the two-months window sample, these positive effects are much smaller and only marginally significant for scientific literacy.[26]

Table 8 presents the results of two placebo RD-DID analyses using our original RD-DID estimation strategy based on the Austrian sample and the 1990 and 1987 birth cohorts, but using May 1, 1990, and September 1, 1990, as pseudo reform cut-off dates.[27] In each case, the estimations are based on children born two months before and after the pseudo reform date. None of the estimated placebo effects is significantly different from zero for the sample of boys (irrespective of maternal educational background). Generally, this is also true for the subsample of girls (the exceptions are a significantly negative (positive) coefficient for the reading (science) score of daughters from highly educated mothers, columns 2 and 5). Thus, overall, but especially regarding the male subsample, the placebo tests seem to verify that our

[24] Re-running the RD-DDD regressions holding the number of post-reform months constant, while extending the number of pre-reform months produces very similar results (see Table A 6, Online Appendix).
[25] To reduce the extensiveness of the tables, we only report results from the subgroup regressions by gender.
[26] In contrast to Austria, the general school-entry cut-off date in Germany for the cohort born in 1990 (same for the 1987 cohort) is June 30, which would coincide with the Austrian parental leave reform cut-off date, July 1. This could potentially lead to a violation of the common trend assumption if this causes a drop in average test scores around this threshold in the German sample. While the RD-DDD regressions accounted for this potential drop by differencing also across years, this might be a problem in the RD-DID regression set up (by artificially raising the 'treatment effect').
[27] Due to several data and sample problems we cannot exploit the two other existing PISA waves for Austria from the years 2000 and 2009 for further robustness check and to analyse the common trend assumption (e.g., a biased sampling frame in the Austrian PISA 2000, and a stricter enforcement of the school entry cut-off date that affected the birth cohort tested in 2009 violating the common trend assumption).

main RD-DID results are not confounded by particular patterns in the data and can indeed be interpreted as the causal effect of the parental leave reform.

To better understand potentially underlying mechanisms of our main findings we analyze two alternative schooling outcomes. Using our basic RD-DID setup, we test whether the parental leave extension affects the likelihood that a student is i) in a lower than regular grade level given his or her birth month and ii) enrolled in an academic track school (Table 9).[28] After all, the estimated treatment effect on test scores might partly be driven by these underlying mechanisms. As regards the school track, the estimated reform effects are very small and not significantly different from zero. The same is true for grade retention in the pooled sample and in the subsample for children of higher educated mothers. However, for children of mothers with lower educational attainment the results suggest that the extended leave significantly increased the likelihood of being in a lower than regular grade level. This effect is significantly positive in the pooled sample of boys and girls, but seems to be particularly driven by the reform effect on girls.

8 Conclusions

The objective of this paper is to investigate whether a substantial extension of a paid and job-protected parental leave mandate – from a child's first to its second birthday – has any long-term effects on human capital of children. What makes the Austrian parental leave reform particularly compelling for such a causal analysis is that it was implemented with a strict and unanticipated cut-off date: only those mothers who gave birth to their child on or after July 1, 1990 became eligible for the more generous 24 months parental leave duration. As a consequence of this unexpected cut-off date the allocation of mothers and their children into treatment (24 months parental leave entitlements) and control (12 months parental leave) group was sharp and quasi random. This exogenous discontinuity in parental leave duration helps to control for the problem of the otherwise endogenous return-to-work decision. Another advantage of this particular Austrian reform is that it did not seem to have had any effects on medium or long-term labour market outcomes of mothers and only a small positive effect on fertility of low-wage mothers.

We use mathematics, reading and science test scores from the standardized PISA test at age 15 (using the cohort born in the year of the reform 1990 as well as a control birth cohort born 1987 which was not subject to the parental leave reform). The results of the RD and RD-DID analyses and several robustness checks reveal that there are heterogeneous

[28] As PISA data do not contain information grade repetition or age at school entry we cannot disentangle whether a student is in a lower than should-be grade level due to deferred school entry or grade repetition.

effects of the parental leave reform on PISA test scores across subgroups. When using the *full sample*, the estimates suggest no statistically significant causal effects on cognitive skills. This finding for Austria is in line with the results of most of the studies using changes in parental leave mandates to identify the causal effect of early maternal employment on child outcomes. However, when splitting the sample into two groups of mothers with higher and lower educational attainment, interesting findings emerge: for the children of higher educated mothers, we find significantly positive effects of the parental leave extension on the PISA test scores at age 15. This positive result is especially driven by the large and significant effect on boys. We also find negative reform effects for boys of mothers with lower education. These results are in contrast to findings reported in Carneiro, Løken and Salvanes (2010) for Norway where an extension of maternal leave during the first 12 months since birth had positive effect for children of low educated mothers and not effect on others. Furthermore, Liu and Nordstrom Skans (2010) also find positive effects for their assessed parental leave extension in Sweden when restricting the sample to children of mothers with higher education. However, in contrast to Sweden with a well-established formal child care system for under-two-year olds, the reform in Austria took place at a time when formal childcare for very young children was virtually non-existent. Hence, our empirical results suggest that maternal care of mothers with higher education might be superior to different informal care arrangements for children aged one to two years. The negative effects for children from mothers with lower education might be caused by reduced time and material resources as low wage mothers reacted to the parental leave reform with increased fertility rates and shorter time intervals between births. Our study thus adds unique and policy relevant evidence on heterogeneous effects of a parental leave extension with respect to maternal educational status. The same is true for gender differences: while there exists some evidence of heterogeneous *health* outcomes of girls and boys with respect to early maternal employment in the empirical literature, the previous evaluation studies of parental leave on *cognitive* child outcomes have not tested or not found corresponding gender effects. Against this background, the presumably stronger effect of the Austrian parental leave extension on boys points to an interesting aspect which should be explored in more detail in future research. Our empirical results – in particular, those for children from mothers with higher educational background – are robust to various sensitivity checks – including triple DID estimates using Germany as an additional control region which was not affected by the 1990 parental leave reform and several placebo tests.

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Table 1: Mean comparisons of outcomes and characteristics of students born in May/June versus July/August in the reform year 1990 and the control year 1987

	PISA 2006 (birth cohort 1990)				PISA 2003 (birth cohort 1987)			
	Pre reform	Post reform			Pre reform	Pre reform		
Birth months	**May-June**	**July-Aug.**			**May-June**	**July-Aug.**		
	Mean (1)	Mean (2)	Diff. (2)-(1)	Std. error	Mean (1)	Mean (2)	Diff. (2)-(1)	Std. error
Mathematics score *(Std. deviation: 92.8)*	521.20	519.80	-1.45	5.23	515.90	515.10	-0.74	5.92
Reading score *(Std. deviation: 101.1)*	507.20	500.70	-6.46	5.74	501.10	501.70	0.62	5.73
Science score *(Std. deviation: 90.6)*	526.90	523.70	-3.18	5.09	504.40	502.50	-1.87	5.65
Retained	0.21	0.34	0.13**	0.03	0.25	0.33	0.08**	0.03
Academic track	0.54	0.52	-0.02	0.03	0.46	0.51	0.04	0.03
Male	0.52	0.50	-0.02	0.03	0.50	0.49	-0.01	0.03
Age in years	15.92	15.75	-0.17**	0.00	15.93	15.76	-0.17**	0.00
City	0.41	0.41	-0.01	0.03	0.34	0.35	0.01	0.03
Metropolitan area	0.14	0.13	-0.01	0.02	0.16	0.14	-0.02	0.02
Mother's education:								
Lower secondary	0.09	0.09	0.00	0.02	0.13	0.09	-0.04**	0.02
Upper secondary	0.54	0.56	0.02	0.03	0.59	0.56	-0.03	0.03
Tertiary	0.37	0.35	-0.02	0.03	0.28	0.34	0.07**	0.03
Father's education								
Lower secondary	0.06	0.06	0.00	0.01	0.09	0.07	-0.02	0.02
Upper secondary	0.47	0.46	-0.01	0.03	0.51	0.48	-0.03	0.03
Tertiary	0.45	0.46	0.02	0.03	0.37	0.41	0.05	0.03
Educ. father: miss.	0.02	0.02	0.00	0.01	0.03	0.04	0.01	0.01
Migration family type 1	0.09	0.07	-0.02	0.02	0.06	0.06	0.00	0.01
Migration family type 2	0.85	0.86	0.01	0.02	0.90	0.91	0.01	0.02
School programme:								
Vocational (low track)	0.05	0.07	0.02	0.02	0.05	0.07	0.02	0.02
Apprenticeship	0.26	0.23	-0.04	0.02	0.26	0.24	-0.02	0.03
BMS	0.13	0.16	0.03*	0.02	0.20	0.17	-0.03	0.02
BHS	0.33	0.30	-0.03	0.03	0.31	0.32	0.01	0.03
AHS	0.23	0.24	0.01	0.02	0.18	0.21	0.03	0.02
Number of observations	*716*	*764*			*680*	*680*		

Notes: Estimations weighted by individual inverse probability weights provided in the PISA data set. The standard deviations refer to the 2006 data using the pooled sample from May to August. Significance levels are denoted by: ** p<0.05, * p<0.1. "Migration family type 1" indicates families speaking the test language at home and in which at least one of the parents is foreign-born. "Migration family type 2" indicates families speaking the test language at home and where both parents are native-born. The base category are non-German speaking families.

Table 2: Simple OLS regressions on test score differences between children born in June versus July 1990

	Full sample		High Educ. Mothers		Low Educ. Mothers	
	(1)	(2)	(4)	(5)	(7)	(8)
			BOYS + GIRLS			
Mathematics	3.0	3.3	15.7	15.3	-3.4	-2.9
	(7.0)	(6.2)	(11.0)	(11.1)	(8.4)	(7.5)
Reading	3.3	4.0	28.9**	28.6**	-10.1	-8.9
	(6.9)	(6.0)	(13.5)	(13.7)	(8.7)	(7.4)
Science	3.9	3.8	23.7**	22.1**	-6.4	-5.8
	(6.3)	(5.4)	(10.4)	(10.2)	(8.1)	(6.9)
Observations	1,480	1,480	523	523	957	957
			BOYS			
Mathematics	11.7	8.6	27.9	21.9	2.9	1.9
	(9.0)	(8.0)	(17.3)	(14.8)	(8.8)	(9.1)
Reading	12.5	10.5	50.3**	44.3*	-7.8	-5.0
	(9.2)	(7.9)	(22.4)	(22.0)	(10.8)	(9.8)
Science	7.7	4.7	40.8**	34.6**	-9.9	-10.5
	(8.6)	(7.3)	(15.5)	(14.4)	(8.7)	(7.9)
Observations	752	752	265	265	487	487
			GIRLS			
Mathematics	-5.8	-2.3	3.5	5.8	-9.6	-4.5
	(10.0)	(9.8)	(14.7)	(15.5)	(14.0)	(13.1)
Reading	-6.1	-3.0	8.2	11.8	-12.2	-8.2
	(9.8)	(8.7)	(15.3)	(15.4)	(13.7)	(11.1)
Science	-0.1	2.8	7.1	7.3	-2.8	2.2
	(9.3)	(8.6)	(14.3)	(14.5)	(14.1)	(12.6)
Observations	728	728	258	258	470	470
Controls for parental background	-	✓	-	✓	-	✓

Notes: The presented numbers are the estimated effects of 'being born in July' as opposed to 'being born in June' using the 2006 PISA data. The results in the top panel stem from the pooled sample of males and females, the middle panel from the male and the bottom panel from the female sample. All regressions include children born between May 1 and August 31 and control for birth months (dummy variables) and gender (top panel). The control variables on parental background include dummy variables for mother's and father's educational attainment, school location, and migration background. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table 3: RD-DID estimation results (boys and girls)

	Full sample		High Educ. Mothers		Low Educ. Mothers	
	(1)	(2)	(3)	(4)	(5)	(6)
Mathematics						
Treatment effect	-0.4	2.0	17.2*	16.1*	-7.1	-5.0
	(6.9)	(6.7)	(9.7)	(9.2)	(9.6)	(8.8)
Post June	0.2	-2.5	-21.7*	-19.5*	8.3	6.0
	(7.0)	(6.8)	(10.9)	(9.9)	(8.6)	(8.0)
Born May	-2.3	-4.2	-11.7	-14.1	1.0	0.2
	(5.9)	(5.6)	(8.9)	(8.6)	(6.0)	(5.7)
Born July	-3.8	-2.9	8.4	5.1	-10.0	-8.4
	(5.2)	(4.8)	(8.8)	(8.6)	(6.7)	(5.2)
BC 1990	5.0	3.9	-6.7	-6.2	8.1	8.3
	(6.4)	(5.8)	(10.4)	(8.9)	(6.8)	(6.4)
Constant	503.8***	418.5***	528.2***	457.1***	494.8***	407.6***
	(12.4)	(19.0)	(14.2)	(23.3)	(12.4)	(20.8)
Reading						
Treatment effect	-7.4	-4.1	22.7*	21.2*	-19.8*	-17.1*
	(8.6)	(8.1)	(12.4)	(11.7)	(10.4)	(9.5)
Post June	4.6	1.3	-24.6*	-22.4**	15.4*	13.5*
	(7.6)	(7.3)	(12.4)	(11.1)	(7.9)	(7.5)
Born May	6.9	4.7	0.6	-2.7	8.4	8.2
	(4.9)	(4.7)	(10.1)	(8.9)	(5.7)	(5.8)
Born July	-1.8	-0.9	17.0	13.3	-11.3	-9.9
	(5.7)	(5.3)	(10.2)	(10.0)	(7.9)	(6.0)
BC1990	6.5	4.8	-13.5	-12.2	13.0*	12.8*
	(7.0)	(6.4)	(11.8)	(11.2)	(7.5)	(7.1)
Constant	515.9***	408.7***	545.7***	447.5***	505.3***	399.3***
	(13.0)	(22.2)	(15.7)	(27.5)	(13.6)	(24.8)
Science						
Treatment effect	-1.0	2.1	23.7**	23.0**	-11.1	-8.5
	(7.6)	(7.4)	(10.3)	(10.0)	(9.9)	(9.0)
Post June	0.7	-2.7	-22.0**	-20.7**	9.0	6.4
	(7.0)	(6.8)	(10.6)	(9.7)	(8.6)	(8.0)
Born May	3.9	1.6	0.5	-2.7	4.2	3.4
	(5.9)	(5.7)	(7.8)	(6.9)	(6.9)	(6.8)
Born July	-1.1	-0.1	13.1	9.6	-8.2	-6.6
	(5.3)	(5.0)	(8.7)	(8.7)	(7.5)	(5.8)
BC1990	22.1***	20.5***	5.2	5.4	27.6***	27.2***
	(6.8)	(6.2)	(10.2)	(8.8)	(7.3)	(6.9)
Constant	495.0***	388.4***	520.5***	419.7***	485.9***	380.3***
	(12.7)	(18.1)	(14.6)	(24.0)	(12.9)	(18.7)
Observations	2,840	2,840	943	943	1,897	1,897
Background controls	-	✓	-	✓	-	✓

Notes: All regressions control for gender. The control variables on parental background include dummy variables for mother's and father's educational attainment, school location, and migration background. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table 4: RD-DID estimation results by gender.

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		**High Educ. Mothers**		**Low Educ. Mothers**	
			BOYS			
Mathematics	0.1	0.3	17.9	15.8	-8.4	-9.0
	(7.9)	(8.1)	(12.4)	(12.3)	(13.2)	(11.8)
Reading	-7.3	-6.8	34.0**	33.1**	-27.7*	-26.6**
	(9.8)	(9.7)	(15.2)	(15.0)	(14.5)	(12.9)
Science	-2.1	-1.2	41.0***	40.4***	-23.7	-23.3*
	(9.2)	(9.2)	(10.9)	(11.4)	(15.4)	(13.4)
Observations	1,426	1,426	482	482	944	944
			GIRLS			
Mathematics	-0.5	4.1	18.2	16.0	-5.6	-2.0
	(11.1)	(10.7)	(15.5)	(15.2)	(14.3)	(13.3)
Reading	-7.4	-1.9	13.4	13.9	-12.8	-8.9
	(13.7)	(12.6)	(19.7)	(19.1)	(15.5)	(14.0)
Science	0.6	5.8	7.8	6.3	1.7	5.8
	(12.1)	(11.9)	(16.7)	(15.8)	(14.1)	(13.1)
Observations	1,414	1,414	461	461	953	953
Background controls	-	✓	-	✓	-	✓

Notes: Each cell reports the estimated treatment effect from a separate regression. The upper panel includes only male, the lower panel only female students. All regressions include dummy variable controls for survey year, birth months and for all children born post June. The control variables on parental background include dummy variables for mother's and father's educational attainment, school location, and migration background. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table 5: RD-DID estimates based on symmetrically extended estimation samples (up to four pre- and post-reform birth months)

	High Educ. Mothers			Low Educ. Mothers		
	(1) Mar-Oct	(2) Apr-Sep	(3) May-Aug	(4) Mar-Oct	(5) Apr-Sep	(6) May-Aug
	BOYS + GIRLS			**BOYS + GIRLS**		
Mathematics	5.7	9.9	16.4*	2.2	0.5	-5.6
	(7.7)	(7.3)	(9.3)	(6.5)	(7.3)	(9.1)
Reading	9.2	13.1	20.6*	-8.1	-9.7	-16.3*
	(9.5)	(9.8)	(11.7)	(6.8)	(7.7)	(9.2)
Science	11.7	15.9*	23.1**	-3.1	-5.1	-8.8
	(8.7)	(8.9)	(10.1)	(7.1)	(7.7)	(9.1)
Observations	1,887	1,425	943	3,772	2,840	1,897
	BOYS			**BOYS**		
Mathematics	13.3	13.5	15.8	-3.0	-3.6	-9.0
	(11.2)	(9.7)	(12.3)	(8.9)	(10.3)	(11.8)
Reading	27.2**	28.9**	33.1**	-16.8	-20.9*	-26.6**
	(11.6)	(12.0)	(15.0)	(10.1)	(12.2)	(12.9)
Science	32.6***	34.4***	40.4***	-16.9*	-19.0	-23.3*
	(10.4)	(9.9)	(11.4)	(9.7)	(11.1)	(13.4)
Observations	953	716	482	1,866	1,397	944
	GIRLS			**GIRLS**		
Mathematics	-0.2	6.0	16.0	7.3	4.9	-2.0
	(10.6)	(11.0)	(15.2)	(9.0)	(10.0)	(13.3)
Reading	-9.7	-1.3	13.9	4.0	2.3	-8.9
	(14.5)	(14.6)	(19.1)	(9.1)	(9.9)	(14.0)
Science	-8.2	-2.6	6.3	11.6	9.3	5.8
	(11.6)	(12.6)	(15.8)	(10.1)	(11.1)	(13.1)
Observations	934	709	461	1,906	1,443	953

Notes: Each cell reports the estimated treatment effect from a separate regression. All regressions include dummy variables for month of birth, a year dummy for 2006, a dummy variable for all children born after June. Estimations from columns 1, 2, 4, and 5 include a further dummy variable for all children born between September and December to account for the school entry cut-off date in Austria and an interaction effect of this dummy variable with the year 2006 variable to account for potential general trends in school entry or repetition norms. The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table 6: RD-DDD estimations including German students as further control group

	High Educ. Mothers			Low Educ. Mothers		
	(1) Mar-Oct	(2) Apr-Sep	(3) May-Aug	(4) Mar-Oct	(5) Apr-Sep	(6) May-Aug
	BOYS + GIRLS			**BOYS + GIRLS**		
Mathematics	19.2*	22.2*	22.8	-6.2	-9.9	-17.1
	(9.9)	(11.3)	(14.8)	(10.0)	(11.7)	(14.1)
Reading	18.6	20.6	25.1	-23.2*	-26.5*	-31.0*
	(13.5)	(15.2)	(17.3)	(12.3)	(13.4)	(15.5)
Science	29.1**	33.5**	37.8**	-14.9	-17.0	-20.9
	(11.8)	(13.3)	(15.3)	(11.3)	(13.1)	(14.8)
Observations	4,228	3,212	2,158	6,519	4,968	3,325
	BOYS			**BOYS**		
Mathematics	28.5**	32.7**	38.1**	-33.1**	-34.1*	-37.2**
	(13.0)	(13.4)	(18.2)	(15.8)	(17.2)	(16.7)
Reading	32.2*	36.1*	40.0*	-55.2***	-56.8**	-57.1**
	(16.7)	(18.3)	(22.6)	(19.3)	(21.2)	(22.1)
Science	48.6***	56.6***	66.9***	-51.5***	-53.2***	-53.1***
	(10.6)	(12.1)	(15.8)	(16.2)	(18.6)	(18.6)
Observations	2,157	1,634	1,113	3,209	2,438	1,636
	GIRLS			**GIRLS**		
Mathematics	11.1	11.6	9.4	16.2	10.1	2.6
	(16.4)	(19.0)	(23.6)	(11.3)	(15.0)	(21.7)
Reading	0.9	3.2	13.6	7.9	1.6	-8.5
	(18.4)	(21.2)	(21.2)	(12.6)	(16.2)	(23.9)
Science	9.3	9.5	10.4	16.9	14.0	9.9
	(18.9)	(21.5)	(23.3)	(13.3)	(17.3)	(22.1)
Observations	2,071	1,578	1,045	3,310	2,530	1,689

Notes: Each cell reports the estimated treatment effect from a separate regression. All regressions include dummy variables for month of birth, year and country fixed effects, a dummy variable for all children born after June, interaction effects between year and the 'post June' dummy, year and country, country and 'post June'. Estimations from columns 1, 2, 4, and 5 include a further dummy variable for all children born between September and December to account for the school entry cut-off date in Austria (and a year interaction). The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by country, school track (more/less academic), school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table 7: Robustness check. RD-DID estimations using only PISA 2006 data and Germany as a control group.

	High Educ. Mothers			Low Educ. Mothers		
	(1) Mar-Oct	(2) Apr-Sep	(3) May-Aug	(4) Mar-Oct	(5) Apr-Sep	(6) May-Aug
	BOYS + GIRLS			**BOYS + GIRLS**		
Mathematics	18.7*	23.9**	27.2**	14.2**	10.6	-0.1
	(10.0)	(10.2)	(11.1)	(6.5)	(8.0)	(9.7)
Reading	19.6*	22.9*	27.5*	5.8	1.1	-4.1
	(11.4)	(12.0)	(14.5)	(8.3)	(9.0)	(9.9)
Science	21.5**	26.7**	31.9***	8.1	4.6	-1.2
	(10.2)	(10.2)	(11.5)	(6.3)	(7.2)	(8.3)
Observations	2,353	1,799	1,214	3,296	2,487	1,669
	BOYS			**BOYS**		
Mathematics	22.9*	31.9**	34.0**	6.2	1.7	-10.3
	(11.0)	(11.2)	(14.8)	(13.3)	(15.4)	(15.7)
Reading	31.7*	40.4**	44.2*	-2.7	-7.6	-15.9
	(16.6)	(17.2)	(22.6)	(16.1)	(17.2)	(17.5)
Science	34.8**	44.8***	52.0***	-4.4	-11.0	-21.2
	(13.1)	(12.2)	(16.2)	(11.3)	(12.5)	(12.5)
Observations	1,202	911	618	1,621	1,226	826
	GIRLS			**GIRLS**		
Mathematics	15.4	15.9	21.9	22.3***	18.0**	11.5
	(16.7)	(17.8)	(18.4)	(5.8)	(8.2)	(11.3)
Reading	5.4	5.0	12.3	14.5**	9.9	8.1
	(14.2)	(15.3)	(15.1)	(6.9)	(9.4)	(12.2)
Science	8.7	9.2	13.5	20.1**	18.5*	19.1*
	(14.3)	(15.3)	(15.2)	(8.0)	(9.6)	(11.0)
Observations	1,151	888	596	1,675	1,261	843

Notes: Each cell reports the estimated treatment effect from a separate regression. All regressions include dummy variables for month of birth, a dummy variable for all children born after June. Estimations from columns (1)–(4) include a further dummy variable for all children born between September and December to account for the school entry cut-off date in Austria. The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by country, school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table 8: RD-DID placebo tests

	(A) Pseudo cut-off is *May 1, 1990* RD-DID 2006+2003			(B) Pseudo cut-off is *Sep 1, 1990* RD-DID 2006+2003		
	Full sample	**High Educ. Mothers**	**Low Educ. mothers**	**Full sample**	**High Educ. Mothers**	**Low Educ. mothers**
	(1) Mar-Jun	(2) Mar-Jun	(3) Mar-Jun	(4) Jul-Oct	(5) Jul-Oct	(6) Jul-Oct
		BOYS			**BOYS**	
Mathematics	4.007	-5.903	10.418	-3.8	9.5	-10.5
	(10.081)	(14.688)	(12.885)	(8.8)	(15.2)	(10.9)
Reading	7.702	-14.833	19.133	-6.6	-0.8	-8.9
	(11.491)	(17.469)	(14.598)	(10.8)	(14.7)	(14.4)
Science	1.185	-15.328	10.248	-3.7	-2.8	-3.1
	(11.928)	(13.059)	(15.292)	(9.1)	(15.6)	(11.6)
Observations	1,432	462	970	1,387	491	896
		GIRLS			**GIRLS**	
Mathematics	1.437	-26.332	18.209	-5.6	6.7	-10.4
	(11.353)	(15.491)	(16.229)	(12.0)	(13.0)	(16.1)
Reading	2.017	-39.974**	25.166	-4.2	10.3	-9.9
	(13.365)	(15.263)	(19.199)	(11.0)	(14.3)	(12.3)
Science	-1.225	-23.605	12.143	1.4	23.8*	-8.6
	(9.896)	(13.953)	(12.958)	(11.3)	(13.6)	(13.4)
Observations	1,361	450	911	1,479	484	995

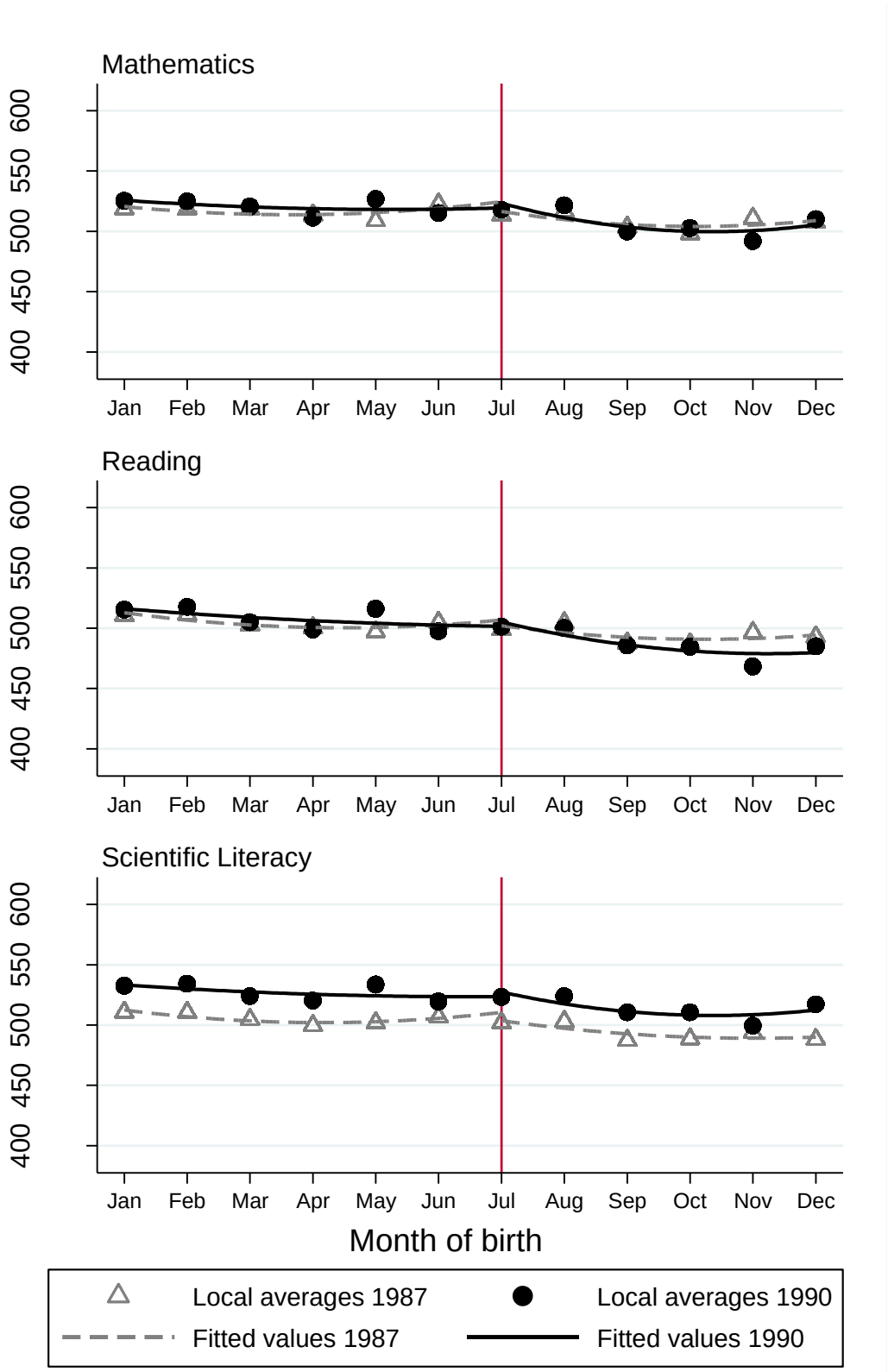
Notes: Each cell reports the estimated pseudo treatment effect from a separate regression. The left (right) three results are based on the sample of children born between April 1 and July 31 (July 1 and October 31) of 1987 and 1990. All regressions include a year dummy (bc1990) and control for mother's and father's educational attainment, school location, and migration background of the family. Furthermore the regressions include controls for birth months, a dummy variable for all children born after the pseudo cut-off dates May 1 (May and June) or September 1 (September and October) as well as year-pseudo reform interaction effect. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table 9: Probability of being in lower than regular grade level (grade retention) or being enrolled in the academic track (linear probability models)

	(1) Full sample	(2) High Educ. Mothers	(3) Low Educ. Mothers
		BOYS + GIRLS	
Retained	0.055	-0.022	0.089**
	(0.035)	(0.059)	(0.041)
Academic track	-0.042	0.004	-0.067
	(0.034)	(0.059)	(0.043)
Observations	2,840	943	1,897
		BOYS	
Retained	0.019	-0.031	0.042
	(0.051)	(0.090)	(0.049)
Academic track	-0.044	-0.005	-0.064
	(0.036)	(0.062)	(0.047)
Observations	1,426	482	944
		GIRLS	
Retained	0.089**	0.002	0.137**
	(0.040)	(0.077)	(0.062)
Academic track	-0.047	0.019	-0.065
	(0.057)	(0.091)	(0.073)
Observations	1,414	461	953

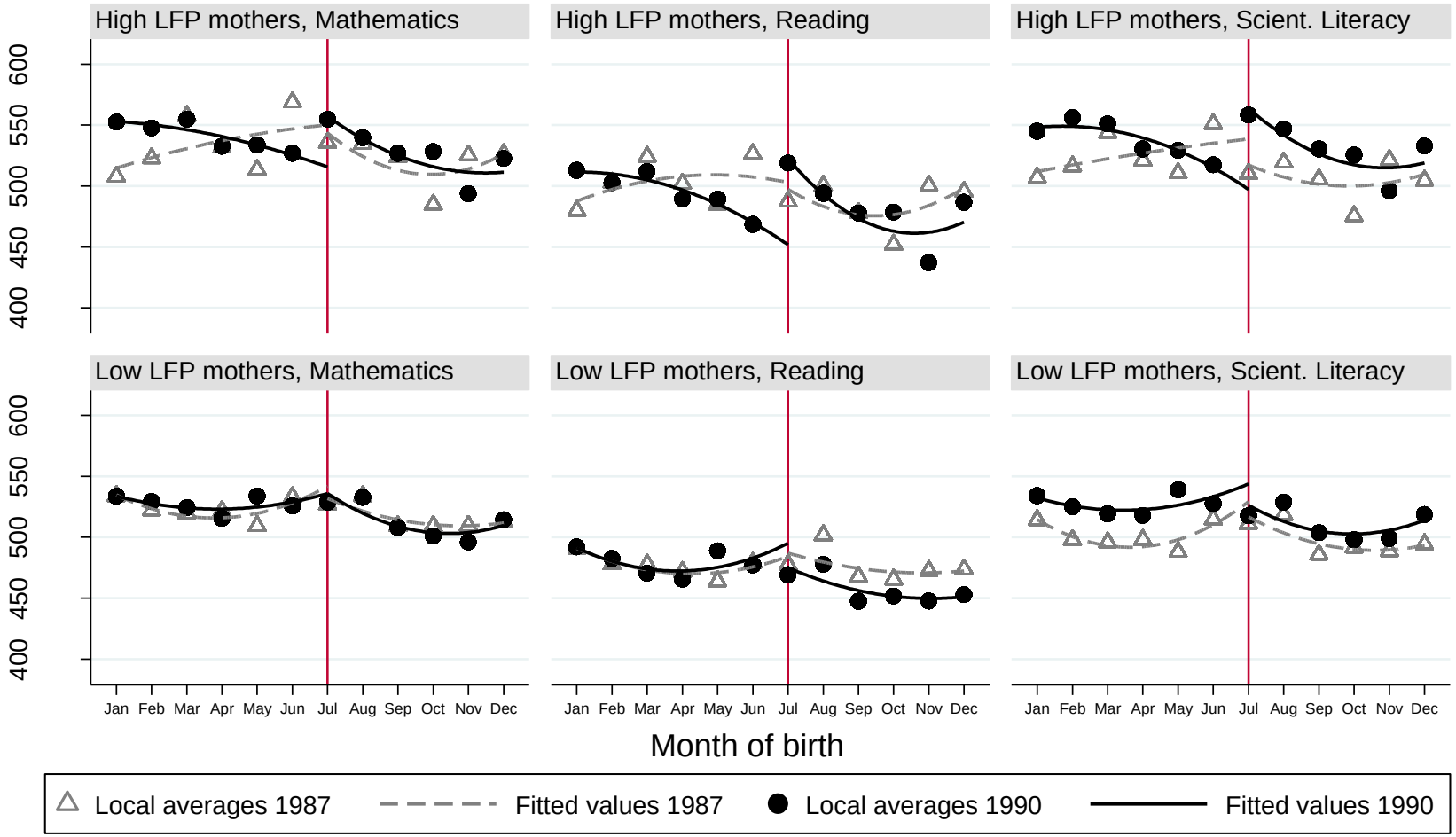
Notes: Each cell reports the estimated treatment effects from separate DID regressions controlling for the standard set of background variables as in the original DID regressions in the main specification (e.g., Tables 3 and 4). The samples consists of children born between May 1 and August 31 of 1987 or 1990. The dependent variables are two dummy variables indicating whether a student is enrolled in a lower than regular grade level given his birth date and whether the student is enrolled in the academic track. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Figure 1: Local averages by subject and parametric fit (RD-DID graphs)



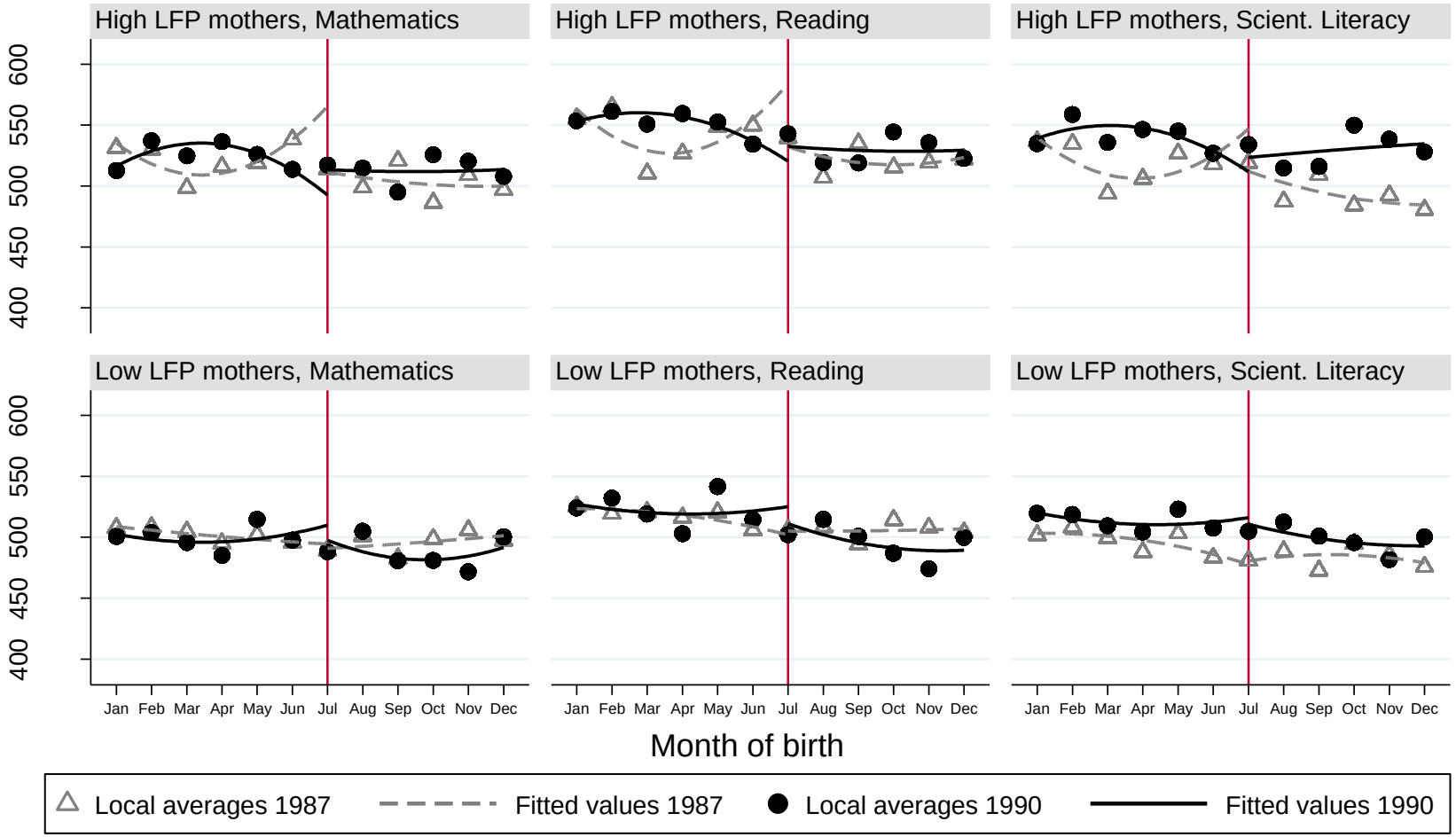
Notes: Average test scores by birth month plotted for the reform cohort 1990 and the control cohort 1987 (using official individual weights). The vertical line indicates the month of the parental leave reform (July 1, 1990). The curved line shows the predicted fit from two regressions of test scores on birth month and its square based on (1) all observations born between January and June and (2) born between July and December.

Figure 2: Local averages and parametric fit for boys, by maternal LFP group (RD-DID graphs)



Notes: Sample restricted to male students. Average test scores by birth month and mother's LFP group plotted for the reform cohort 1990 and the control cohort 1987 (using official individual PISA weights). The vertical line indicates the month of the parental leave reform (July 1, 1990). The curved line shows the predicted fit from two regressions of test scores on birth month and its square based on (1) all observations born between January and June and (2) born between July and December.

Figure 3: Local averages and parametric fit for girls, by maternal LFP group (RD-DID graphs)



Notes: Sample restricted to female students. Average test scores by birth month and mother's LFP group plotted for the reform cohort 1990 and the control cohort 1987 (using official individual PISA weights). The vertical line indicates the month of the parental leave reform (July 1, 1990). The curved line shows the predicted fit from two regressions of test scores on birth month and its square based on (1) all observations born between January and June and (2) born between July and December.