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ABSTRACT

The Effect of Expansions in Maternity Leave Coverage on Children's Long-Term Outcomes*

This paper evaluates the impact of three major expansions in leave coverage in Germany on the long-run education and labor market outcomes of children. Evaluation of three policy reforms as opposed to a single reform enables us to analyze whether the impact of paid leave differs from that of unpaid leave, and whether an expansion of a relatively short leave period is more beneficial to child development than an expansion of an already long leave period. Our empirical analysis combines two large administrative data sources on wages, unemployment, and school outcomes. We identify the causal impact of the reforms by comparing outcomes of children born shortly before and shortly after a change in maternity leave legislation, and therefore require substantially weaker assumptions for identification than existing studies. We find little support for the hypothesis that an expansion in maternity leave legislation improves children's outcomes. Given the precision of our estimates, we can statistically rule out the hypothesis that the expansion in paid leave from 2 to 6 (unpaid leave from 18 to 36) months raised wages (attendance at high track schools) by more than 0.3 % (0.1 %).

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1 Introduction

Over past decades, many countries have witnessed a large increase in female labor force participation rates, with participation rates of mothers with young children increasing the most. In the US, 20 % of mothers with children under age 6 were working in 1960, but by 1990, this proportion was up to 60 % (e.g. Barrow 1999, Leibowitz and Klerman 1995). Furthermore, more than half of the mothers who were employed during pregnancy go back to work within three months after childbirth (Leibowitz et al. 1992). This number is high by international standards: In the UK, Sweden, and Germany, less than 10 % of previously employed mothers return to work so early (Gustaffson et al. 1996).

In the past, countries have taken different avenues in the way they regulate the re-entry of mothers after child birth, in the form of maternity leave legislation. While women in the United States are entitled to 12 weeks of unpaid leave, women in Germany are eligible for 3 years of job-protected leave. Currently, many of these regulations are under review. For instance, Canada increased paid family leave from 26 to 50 weeks in 2000. In 2003, California approved a policy that entitles women to up to six months of paid leave. Other U.S. states, such as New Jersey, are considering implementing similar policies. Other countries that have recently expanded their leave coverage include the UK (2003, 2007) and Denmark (2002).

An important goal of the recent expansions in leave coverage around the world was the welfare of children, and the expansions were explicitly aimed at increasing the time mothers spend with their infants after childbirth. This is motivated by the agreement among psychologists that the first months and years in a child's life are crucial for its future cognitive and emotional development (e.g. Harris 1983 and Lewis and Brooks-Gunn 1979). Arguments for why a delay in the return to work may benefit children include prolonged breastfeeding, as well as an increase in the quantity and quality of child-related investments and child-parent interactions.

Yet, quantitative evidence on the link between early maternal employment and child development is so far mixed. This is nicely illustrated by Bernal and Keane (2006a), who review studies on the link between maternal employment and child development based on the NLSY 1979. Roughly one third of the papers reviewed report positive effects, a third negative effects, and the remainder insignificant effects or effects that vary depending on the groups studied or the timing of inputs. For Germany, our own calculations based on the German Socio-Economic Panel suggest a strong negative association between the mother's employment after childbirth and the child's track choice at age 10 to 14.[1] We find that working full-time every month during the first three years of the child's life, compared to not working at all, reduces the child's attendance at the high track school by 14.4 percentage points. Mother's part-time work, in contrast, is not associated with lower track choice.[2]

Of course, the association between early maternal employment and children's track choice may not reflect a causal relationship. Establishing a causal relationship requires arguably exogenous variation in weeks worked after childbirth. There are two studies that use such an experimental design. Baker, Gruber, and Milligan (2008) evaluate the impact of a generous child care subsidy in Quebec on child development, using other Canadian provinces as a control group. Bernal and Keane (2006a) exploit the 1996 welfare reform in the United States and use a variety of policy variables, such as termination and work requirements, time limits, earning disregards, child care assistance and child support enforcement, as instruments for female labor supply. Both studies focus on children's short-term outcomes up to age 3 (Baker, Gruber, and Milligan 2008) or age 8 (Bernal and Keane 2006a).

This paper evaluates the impact of three major expansions in leave coverage in Germany on the long-run educational and labor market outcomes of children. Germany seems ideal to study these issues, for several reasons. First, reforms in Germany took place relatively early, which allows us to study child outcomes during adolescence and young adulthood. Second, by evaluating three policy reforms as opposed to a single, specific reform, we are able to analyze a possible nonlinearity in the impact of leave coverage on child outcomes. The first policy reform in Germany took place in 1979 and increased paid leave from 2 to 6 months. The second reform raised paid leave from 6 to 10 months in 1986. The final reform took place in 1992 leave and increased *unpaid* leave from 18 to 36 months–among the longest in the world. The impact of the latest expansion may differ from that of the earlier ones, either because the expansion was unpaid or because maternal employment in the first months after childbirth is more

[1] Track choice is an informative outcome variable because it is highly correlated with standardized test scores (e.g. Ammermüller 2004) and labor market outcomes in adulthood (e.g. Dustmann 2004).

[2] Our regressions control for the months worked full-time and part-time in the first three years after childbirth, mother's education, mother's age and children's sex and age. The sample size is 628. The coefficient on weeks worked full-time is -0.004 with a robust t-value of 2.12. The coefficient on weeks worked part-time is 0.0009 with a robust t-value of 0.52.

harmful for child development than maternal employment in the third year after childbirth. Third, Germany has unusually good data to analyze the impact of the expansions on child outcomes: for the 1979 expansion in leave coverage from 2 to 6 months, our data cover every man and woman born around the reform who ever worked for pay, and our main outcome variables are wages and unemployment; for the 1986 and 1992 expansions in leave coverage from 18 to 36 months, our data include all pupils in three large states (Bavaria, Hesse, and Schleswig-Holstein), and our main outcome variables are grade progression and track choice.

The large sample size allows us to identify the causal impact of the reform by comparing outcomes of children born shortly (i.e. one month) before and after the reform. We therefore require substantially weaker assumptions for identification than existing studies. For instance, for the 1992 policy reform, we compare children born in December 1991 and whose mothers were entitled to 18 months of leave with children born in January 1992 and whose mothers were entitled to 36 months of leave.

To preview our results, we find no evidence that any of the policy reforms improved children's outcomes. This is although each reform had a strong short-term impact on the mother's labor supply and induced women to take more time off from work after childbirth. Given the precision of our estimates, we can statistically rule out the hypotheses that the expansion in paid leave from 2 to 6 months raised wages by more than 0.3 %, or that the expansion in unpaid leave from 18 to 36 months increased selective high school attendance by more than 0.1 percentage points. This contrasts with the large negative correlation between mother's weeks worked and the child's selective high school attendance that we found in the GSOEP. Our results therefore cast doubt on whether maternity leave policies are successful at improving children's long-term outcomes.

The remainder of the paper is organized as follows. First, Section 2 provides the necessary background information for our study, including a detailed description of the major changes in maternity leave legislation in Germany. Section 3 outlines our identification strategy. We describe the data in Section 4. Section 5 first summarizes the impact of expansions in leave coverage on labor market outcomes of mothers, and then turns to their impact on the education and labor market outcomes of children. Section 6 offers a possible interpretation of our findings. Section 7 concludes.

2 Background

2.1 Mechanisms: How Might Expansions in Leave Coverage Affect Child Outcomes?

The primary channel through which an expansion in leave coverage may affect children is a delay in the return to work, and therefore an increase in the time women spend with their child after childbirth.[3] Expansions in leave coverage may also affect child outcomes by influencing mothers' long-run labor market outcomes or fertility decisions.[4]

Why might a delay in the return to work after childbirth benefit children? One channel is breastfeeding. The World Health Organization recommends exclusive breastfeeding for 6 months and breastfeeding complemented with other foods up to the age of 2. Breastfeeding has been associated with protection against for instance diarrhea, asthma, sudden death syndrome, and leukemia as well as with the enhancement of the child's cognitive development. Research further shows that non-working mothers breastfeed longer than working mothers, and that breastfeeding often stops when the mother returns to work (e.g. Berger et al. 2005, Lindberg 1996). Little is known, however, about whether the association between breastfeeding and the child's health is causal, or whether any short-term benefits persist into adolescence and young adulthood. Breast-feeding is one argument why a more modest expansion in leave coverage (e.g. from 2 to 6 months) may be more beneficial to child development than an expansion of an already long leave period (e.g. from 30 to 36 months).

In addition to limiting breastfeeding, early maternal employment may reduce the quantity of child-related investments and child-parent interactions. It may also lower the quality of child-parent interactions if mothers who work long hours experience exhaustion or distress. On the other hand, working mothers may be more confident as a result of their work outside home, which could increase the quality of child-parent interactions.[5] Economists are often agnostic about the exact mechanisms through which

[3] Several studies find that longer leave mandates induce mothers to delay the return to work (e.g. Baker and Milligan 2005, Hanratty and Trzcinski 2005, and Lalive and Zweimüller 2005). We provide evidence on the impact of the expansions in leave coverage on labor market outcomes of mothers in Section 5.1.

[4] Lalive and Zweimüller (2005) provide evidence that the expansion in leave coverage from 1 to 2 years in Austria increased fertility.

[5] Ahnert and Lamb (2003) and Ahnert, Rickert, and Lamb (2000) provide some evidence that working mothers

the quantity and quality of child/mother interactions affect child development, while they have been extensively studied by psychologists. Attachment theory provides one possible channel. This theory posits that an infant's experience of responsive and sensitive care early in life leads to a greater sense of self-efficacy and trust in significant others (Bowlby 1968). The behavior of insecure children tends to be angry, withdrawn, explosive, and disorganized. It is unclear, however, whether employment really prevents mothers from building up a secure and trusting relationship with their infant. Psychologists also agree that once children reach a certain age, they benefit from the exposure to other children and adults. The question of course is: What is the optimal age when children should start day care or kindergarten? This is another argument for why an expansion in leave coverage from, say, 24 to 36 months may be less beneficial to child development than an expansion in leave coverage from 2 to 6 months.

In addition to the positive effects described above, a delay in the return to work might also inhibit child outcomes by (temporarily) reducing family income. While the causal impact of family income on the child's cognitive development is still in dispute, a recent study by Dahl and Lochner (2006) suggests that income has a strong positive impact on children's test scores around age 11. Exploiting changes in the Earned Income Tax Credit as plausibly exogenous variation in income, they find that an additional $1,000 will increase test scores by 0.021 (math) or 0.036 (reading) of a standard deviation.[6] This suggests that the effects of expansions in paid and unpaid leave may differ.

2.2 Previous Studies and Our Contribution

While the literature on the relationships between maternal employment, child care usage, and child development is extensive[7], there are only a few papers that directly evaluate the impact of expansions in leave coverage on child outcomes. Ruhm (2000) and Tanaka (2005) analyze the impact of rights to parental leave on infant and child mortality, as well as on birth weight. They exploit variation in leave coverage across Western-Europe (Ruhm 2000) or OECD countries (Tanaka 2005) and over time. Both

interacted with their children at an increased intensity during the morning and evening hours. As a result, the total amount of attention that children of working and non-working mothers received was roughly the same.

[6] Blau (1999), in contrast, finds only small direct effects of income on child development.

[7] To mention only a few examples, Baum (2003), Bernal (2006), Blau and Grossberg (1992), Belsky (1991), Berger et al. (2006), James-Burdumy (2005), and Ruhm (2004).

conclude that longer leave periods reduce infant and child death, but have only a small impact on low birth weight and the mortality rate of children younger than one month. Tanaka (2005) further finds that paid leave plays a more important role than unpaid leave.

Baker and Milligan (2007, 2008) analyze a specific policy in a single country: the 2000 expansion in leave coverage from 25 to 50 weeks in Canada. Their first paper focuses on the relationship between leave coverage, breastfeeding, and child health, and finds little evidence that the expansion in leave coverage improved self-reported indicators of child health up to age 2, although the reform increased mothers' time away from work after childbirth as well as the time mothers breastfed their child. In their second paper, Baker and Milligan (2008) focus on early child development. Again, they find little evidence that the expansion in leave coverage improved measures of motor-social development and family environment.

This paper extends this literature in several important ways. Our first contribution is methodological: We identify the causal impact of an expansion in leave coverage on child outcomes by comparing children born shortly before and shortly after the reform, thus requiring substantially weaker assumptions for identification than existing studies.[8] Second, we evaluate a series of policy changes within the same country. This is important because–as the previous section highlighted–the impact of an expansion in paid leave from 2 to 6 months on the child's development may be different from that of an expansion in unpaid leave from 18 to 36 months. Third, unlike existing studies, we focus on long-term effects of leave coverage on child outcomes. A further advantage of our study is the large sample size of our data, allowing us to estimate the impact of maternity leave coverage with more precision than previous studies.

2.3 Maternity Leave Legislation in Germany

In the United States, the Family and Medical Leave Act (FMLA) introduced in 1993 requires firms with at least 50 employees to provide 12 weeks of unpaid leave after childbirth. In Germany, mothers have been entitled to paid leave 6 weeks before and 8 weeks after childbirth since 1968. During the leave

[8] In a recent paper, Wuertz (2007) adopts a similar strategy to analyze the impact of a single expansion in leave coverage from 14 to 20 weeks in Denmark on children's outcomes. Wuertz (2007) finds no evidence that the expansion improved children's enrollment in high school.

period, the firm is not allowed to dismiss the mother, and the mother has the right to return to a job that is comparable to the job she held before childbirth. Payment during this period is equivalent to her average income over the three months prior to childbirth.[9]

Starting in the late 1970s, Germany experienced a series of expansions in leave coverage. Figure 1 provides a visual overview of the main reforms. The first reform took place in May 1979 when job-protected leave was raised from 8 weeks to 6 months. This reform shares some similarities to the 2003 expansion in leave coverage in California. The primary motivation behind this reform was the health of the mother, although the potential benefits that the expansion may have on the welfare of the child were also recognized (Gesetzentwurf der Bundesregierung, Drucksache 8/2613). From 6 weeks before to 8 weeks after childbirth, mothers were paid their average income over the three months prior to childbirth. From the third month after childbirth onward, payment was 375 Euros per month (about one third of average pre-birth earnings). Only women who were employed before childbirth were entitled to maternity benefits.

The later expansions in leave coverage that took place between the mid-1980s and early 1990s shifted the focus from the health of the mother to the welfare of the child. These reforms were motivated by the agreement among psychologists that the first months and years are the most important in a child's life, and were explicitly aimed at encouraging mothers to spend more time with their child after childbirth (Gesetzentwurf der Bundesregierung, Drucksache 10/3792).

The 1986 reform increased the job-protection period from 6 months to 10 months, and announced a further increase to 12 months starting in January 1988. This reform is comparable to recent reforms in Canada (2000) and Denmark (2002). An important component of this reform was that all mothers, regardless of employment status before childbirth, became eligible for maternity benefits. A further component was that fathers became eligible for paternity leave. However, the proportion of fathers taking leave is very small: in 2001, for example, it was 1.6% (Engstler and Menning 2003). Maternity payment from 6 weeks before and 8 weeks after childbirth remained unchanged from the mother's income prior

[9] Costs are shared between the public health insurance, the federal government, and the employer. The federal government contributes 200 Euros as a one time payment per child. The health insurance pays about 375 Euros per month. The additional costs are borne by the employer. Firms with less than 20 employees are exempt from paying maternity benefits. In this case, the additional costs are borne by the federal government.

to childbirth (or about 300 Euros per month, if the mother was not working before childbirth). From the third to the sixth month after childbirth, maternity benefits were equal to 300 Euros, independent of the mother's (and father's) income prior to childbirth. This corresponds to about 20 % of average pre-birth earnings. From the seventh month onwards, maternity benefits were means-tested, depending on the annual net family income during the two years before childbirth. The majority of women received benefits longer than 6 months: in 1986, for example, this proportion was 83.6 % (Bundesministerium für Familie, Senioren, Frauen und Jugend 2000).

In July 1989 and July 1990, job-protected leave was further lengthened to 15 and 18 months, respectively. The final policy reform took place in January 1992, when job-protected leave was increased from 18 to 36 months. Maternity payments still ended at 18 months, but were to be extended to 24 months one year later. The government promised highly subsidized child care for every child older than 3 years starting in 1996. Hence, the intention of the 1992 reform was to encourage mothers to stay home for three years after childbirth, and then make it easier for mothers to return to work by offering highly subsidized child care.

Three West-German states, Bavaria, Baden-Wuerttemberg, and Rhineland-Palatinate pay maternity benefits in addition to the federal benefits. For all children born after June 30 1989, Bavaria pays 250 Euros per month up until the child's second birthday, starting with the expiration of the federal maternity benefit. A similar rule exists in the other two states.

Our empirical analysis focuses on the impact of the three major policy changes in 1979, 1986, and 1992 on child development. Unfortunately, the impact of the smaller changes in maternity leave legislation in July 1989 and July 1990 on children's outcomes cannot be assessed because in Germany children whose sixth birthday is before the end of June of a given calendar year typically enter school at the beginning of the school year (by the so-called *Hamburg Accord*). This implies that children born in July usually start school a year later than children born in June, making it difficult to isolate the effect of age at school entry on educational and labor market outcomes from that of the policy reform.

3 Identification Strategy

We identify the causal effect of maternity leave legislation on children's outcomes by comparing children who were born just before and just after the law changed. Consider, for instance, the increase in job-protected leave from 18 to 36 months that took place in January 1992. Here, our estimation strategy amounts to comparing children who were born in December 1991 and whose mothers were entitled to 18 months of job-protected leave with children who were born in January 1992 and whose mothers were entitled to 36 months of job-protected leave. A similar strategy has been used by Lalive and Zweimüller (2005) to evaluate the impact of an Austrian policy reform on fertility and by Ekberg et al. (2005) to analyze the impact of Sweden's "daddy month reform" on the labor supply of fathers.

Our identification strategy assumes that whether a child is born in December or January is completely random. One reason why this identification strategy might be violated is that there are inherent differences between births in different months of the year. This problem can be dealt with by comparing children born in the same months, but in a year in which there was no change in maternity leave legislation.

Second, our identification strategy would not be valid if women time the birth of their child as a response to the change in maternity leave legislation. This seems unlikely, since women could not precisely anticipate these reforms. We searched two leading German newspapers[10] for articles about the reform. The first articles typically appear two to three months before the reform was finally implemented. By that time, children who were born around the change in the law had already been conceived. Women may still have some possibilities to time the birth of their child through induced births or cesarean sections.[11] One would expect that this allows women to mostly bring the birth date forward, whereas in our case they would like to postpone childbirth in order to become eligible for the more generous leave policy. However, Gans and Leigh (2006) provide empirical evidence that women also have some opportunities to delay childbirth. They find that the introduction of a $3,000 "Baby Bonus" in July 1, 2004 in Australia lead to a sharp drop in the number of births just before, and to a sharp

[10] The search was conducted using Süddeutsche Zeitung and Frankfurter Allgemeine.

[11] Dickert-Conlin and Chandra (1999) find that tax benefits increase the probability that a child is born in the last week of December rather than the first week of January considerably, partly because of the use of cesarian sections.

increase just after, the policy commenced, although the policy was announced only seven weeks before its introduction. In order to deal with this concern, we have analyzed the evolution of the number of births during the days and weeks surrounding the policy expansion in 1992, using Vital Statistics.[12] We would expect the timing of births to be most important for this reform because throughout the 1970s and mid 1980s, cesarean sections were relatively rare and predominantly occurred for medical reasons. By 1992, in contrast, 15.9% of births were cesarean sections. Results can be found in Appendix A. We find little evidence that the expansion affected the timing of births.

We are still left with one problem: children born in January are on average one month younger than children born in December. In Germany, children who are born in July typically start school a year later than children born in June. Several recent papers show that age is an important determinant of educational outcomes (see e.g. Bedard and Dhuey 2006 for international evidence, Puhani and Weber 2005 for Germany, Fredriksson and Oeckert 2005 for Sweden, and Elder and Lubotsky 2007 for the United States). Our identification strategy thus has to isolate the age effect from the causal impact of the policy reform. We adopt two strategies to do so.

For ease of exposition, consider the 1992 policy reform. Since in Germany the cut-off date for school entry is July, we define a cohort as all pupils born between July and June. Our first identification strategy amounts to a regression discontinuity approach that assumes a functional form for the age effect. Here, we estimate regressions of the following type for respective cohorts affected by maternity leave expansion:

$$y_i = \alpha_1 age_i + \alpha_2 Jan_i + u_i, \tag{1}$$

where y_i denotes pupil i's outcome (e.g. school choice at age 14), age_i measures pupil's age in months, and Jan_i is an indicator variable equal to 1 if the child was born between January and June, i.e. after the policy reform. The coefficient of interest is β_2, the impact of maternity leave legislation on child outcomes. We estimate regression (1) for different samples. First, we restrict the sample to children born two months before or after the policy change. We then include all children born three or four months before or after the policy change. The last two sample restrictions impose stronger assumptions

[12] We cannot analyze the evolution of births sourrounding the other policy reforms because data on the number of births per day is currently available only from 1991 onward.

for identification. However, due to the larger sample size, coefficients are likely to be estimated more precisely.

Our second strategy identifies the causal impact of maternity leave legislation on children's outcomes under the assumption that the age effect for the cohort that is affected by the maternity leave legislation is the same as the age effect for earlier or later cohorts. Here, we estimate regressions of the following type:

$$y_i = \beta_1 Jan_i + \beta_2 cohort_i + \beta_3 Jan_i \cdot cohort_i + v_i, \tag{2}$$

where $cohort_i$ is an indicator variable equal to one if the pupil belongs to the 'treated' cohort that was affected by the reform in maternity leave legislation (i.e. children born between July 1991 and June 1992), and $Jan_i \cdot cohort_i$ is the interaction between this variable and a variable that the pupil was born between January and June, i.e. after the expansion in leave coverage. The coefficient of interest is β_3, the impact of the reform in maternity leave legislation on pupils' outcomes. Note that we compare the treated and the control group at the same age, but in a different school year. We first estimate regression (2) using children born in December or January, in a year in which there was a policy change as well as one year before or after that. To gain precision, we then increase the sample size and include all children born between November to February, October and March, and so forth.. Note that this strategy also eliminates any differences other than age between December and January births, as long as these differences are constant across cohorts.

4 Data

Our empirical analysis combines an administrative data set on wages and unemployment with an administrative data set on school choices. We describe each data set in turn. Since some of the foreign children were not born in Germany and were thus not affected by the expansions in leave coverage, we restrict the sample to pupils who are German citizens.

4.1 Administrative Data on School Choice

Our first data set covers all pupils attending public schools in three German states: Hesse, Bavaria, and Schleswig-Holstein. We use this data set to evaluate the impact of the expansions in leave coverage from 6 to 10 months in 1986 and 18 to 36 months in 1992 on children's outcomes. The data set is available for the academic school years 2002-03 to 2005-06 for the states of Hesse and Schleswig-Holstein, and for the school years 2004-05 to 2005-06 for Bavaria. An important advantage of our data is its large sample size; for each birth month, we observe up to 18,500 children. This is crucial for our estimation strategy that relies on comparing children born shortly before or after the reform. From this data base, we select all German citizens born around the policy change–that is, between July 1984 and June 1987 and between July 1990 and June 1993, respectively. A more detailed data description as well as precise variable definitions can be found in Appendix B.1.

For the 1992 policy reform, our main outcome variable is the type of school attended at age 13-14. Germany tracks children into three main types of schools after 4th grade. The least academic track is called *Hauptschule* (grades 5 to 9), the intermediate track is called *Realschule* (grades 5 to 10), and the most academic track is called *Gymnasium* (grades 5 to 13). In all three states, there exists a fourth comprehensive school type that comprises all three track choices (*Gesamtschule*). We summarize pupils in a *Real*- and *Gesamtschule* into one category. There are also schools for children with special needs (*Sonderschule*). In our sample, 3.6% attend special need schools, 26.73% the low, 32.31% the intermediate, and 35.12% the high track choice. Only graduation from the highest track choice provides direct access to university or polytechnic education. The low and intermediate track choices are designed to prepare students for vocational training within the German apprenticeship system. Special need schools are very diverse, ranging from schools for pupils with down syndrome, to schools for the deaf and blind, to most importantly schools for pupils with learning disabilities and severe behavioral problems. There is no strict rule (such as an entry exam) that determines which type of school children are allowed to attend. While primary school teachers give recommendations, the ultimate decision is up to parents.

To motivate that track choice is an important outcome variable, Figure 2 plots the kernel density of a combined test score (math, German, English), separately by school type. The data comes from LAU (Lernausgangsuntersuchung), a survey that covers all children in Hamburg who attended 7th grade in 1998. We select pupils with German parents, and drop pupils in special need schools from our sample due to the high incidence of missing test scores. We standardize the test score to have a mean of 0 and standard deviation of 1. While there is some overlap in the test score distribution across school types, the figure clearly demonstrates that track choice is strongly associated with test scores.[13]

Table 1 provides more details. In the first column, we regress pupils' combined test scores on indicators of whether the pupil attends the intermediate (*Real-/Gesamtschule*) or high track school, with the low track school (*Hauptschule)* as the omitted category. This alone can explain 53 % of the variation in test scores. Average test scores at the highest school type are almost 2 standard deviations higher than at the lowest school type. These results illustrate that track choice is an informative and important outcome variable.

While we consider track choice as our main outcome variable, we als use two related outcomes variables: grade attendance and grade repetition.[14] As delayed graduation from school as well as grade repetition imply that pupils enter the labor market later and thus have less time left until retirement, both are important contributors to life-time income, and thus useful outcome variables.

For the 1986 policy reform, our main outcome variable is graduation from the highest track choice. This is an important outcome variable, since–as Table 1, column (2), shows–it is highly correlated with wages later in life. Here, we regress individuals' wages on the types of schools the individuals graduated from, using data from the German Socioeconomic Panel (see also Dustmann 2004). The results show that men and women graduating from the high track choice earn 49 % (in log points) higher wages than men and women graduating from the low track choice.

Unfortunately, most children born around the policy reform would attend 11th (Hesse and Schleswig-Holstein) or 13th grade (Bavaria) in the first year we observe them in our data. Hence, since the minimum schooling requirement in Germany is 9 years, some students have left school already and we only observe students attending the highest track choice. To compute graduation rates, we therefore

[13] Ammermüller (2004) provides a similar analysis for Germany, using data from the PISA study.

[14] Information on the previous grade is not available for Schleswig-Holstein; hence our analysis here is based Bavaria and Hesse only.

combine the information in our data on the number of pupils who are still enrolled in high school with information on all recorded births (to parents who are German citizens) in each state. We then proxy graduation from the highest track choice as the ratio between the number of students observed in 13th grade and the number of recorded births.

4.2 Administrative Data on Employment Outcomes (BLH)

Our second data source comes from social security records, and covers all West-German men and women born between July 1977 and June 1980 who by the end of 2004 ever worked for pay. We use this data to analyze the impact of the expansion in leave coverage in 1979 from 2 months to 6 months. As before, the main advantage of these data is a large sample size: each birth months, we observe at least 20,000 men and women. Moreover, due to its administrative nature, wages and employment are precisely measured, and workers can be followed while in unemployment.

One drawback of this data is that it only includes men and women who work for pay. The self-employed, civil servants, and men currently doing their compulsory military service are not included, but constitute 20% of the German work force. More importantly, some men and women born around 1979 may still be in full-time education in 2004, and thus have not entered the (formal) labor market yet. Our data is thus likely to over-sample individuals with no post-secondary education or individuals with an apprenticeship degree. This is especially problematic if the 1979 policy change had an effect on the child's schooling decision. We discuss this in detail in Section 5.2.3.

A further issue with this data set is that wages are right-censored at the highest and lowest levels at which social security contributions have to be paid. This, however, constitutes only a very small problem in our sample, as less than 0.5% of the wage observations are right-censored.[15]

We focus on two outcome variables: the wage earned and unemployment in October of the year of the 24^{th} or 25^{th} birthday. We also use proxies for men's and women's post-secondary education. For the wage analysis, we only consider "formal" jobs for which social security contributions have to be paid, and discard so-called "marginal jobs" that last less than 6 weeks or pay less than 15 hours per week, from our analysis. We do this because wages earned in marginal jobs are unlikely to reflect

[15] We ignore censoring in the reported results. Results based on censored regressions are virtually identical.

the individual's earnings potential; moreover, our wage measure for marginal jobs is left-censored. Our analysis on unemployment includes marginal jobs. Precise variable definitions can be found in Appendix B.2.

5 Analysis

We begin with the impact of the expansions in leave coverage on mothers' labor market outcomes. We then turn to the impact of the expansions on children's outcomes.

5.1 Impact on Labor Market Outcomes of Mothers

For the first expansion from 2 to 6 months, results are based on all West-German mothers. For the later expansions from 6 to 10 months and 18 to 36 months, we restrict the analysis to women who give birth in the three states for which we have data on children (i.e. Bavaria, Hesse, and Schleswig-Holstein). The analysis is based on social security data that covers every mother who takes maternity leave. A detailed data description as well as additional results can be found in Schönberg and Ludsteck (2008) and Schönberg (2008).

We begin with the impact of the expansions in leave coverage on the mother's decision when to return to work after childbirth. Figure 3 plots the share of women who have returned to work t months after childbirth for women who give birth three months before and after the reform.[16] Results refer to women who went on maternity leave. The figure shows that for each policy reform, unusually many mothers return to the labor market at the end of the job-protection or at the end of the maternity leave period, i.e. 2, 6, 10, 18, 24 or 36 months after childbirth.[17] Appearantly, then, each reform induced women to delay the return to work. This effect is considerably stronger for the expansions from 2 to 6 months and from 6 to 10 months than for the expansion from 18 to 36 months.

We provide more details in Table 2. Our dependent variable here is equal to 1 if the mother is

[16] We exclude February births as well as births in the month just before and after the reform. See Schönberg and Ludsteck (2008) and Schönberg (2008) for details.

[17] Recall that since July 1989, mothers in Bavaria are entitled to maternity benefits of at least 250 Euros up until 24 months after childbirth.

working 2, 6, 10, ... months after childbirth and 0 otherwise. In order to account for possible inherent differences between mothers giving birth in different months during the year, we report difference-in-difference estimates using mothers who give birth in the same months, but in a year in which there was no change in maternity leave legislation, as a control group. The table confirms the delay in the return to work. For instance, the 1979 reform reduced the share of women who are working 2 months after childbirth by about 30 percentage points, from about 39 % to about 9 %. The 1992 reform lowered the share of women employed 18 months after childbirth by 8.5 percentage points, and the share of women working 24 months after childbirth by 8.1 percentage points. Despite this strong short-term impact on female labor supply, the reform had only a small impact on mothers' long-term participation rates: once maternity leave expires, labor force participation rates tend to be similar before and after the reform.

Schönberg and Ludsteck (2008) further show that almost all of the delay in the return to work is due to a reduction in full-time work. This is important because some survey-based studies, including our own computations based on the GSOEP, have found that working long hours in the first year of a child's life are particularly harmful (e.g. Brooks-Gunn, Han, and Waldfogel 2002; Baum 2003). There is, however, no evidence that the expansions increased part-time work in the long-run.

Table 2 also reports estimates on the impact of the expansions in leave coverage on income available to the mother. Available income is equal to the daily wage (measured in Euros) if the mother is working, to the daily maternity benefit if the mother is not working but eligible for paid leave, and zero otherwise. Defining income in this way allows us to assess whether maternity benefits compensate for the temporary earnings loss due to the delay in the return to work. The table shows that the 1979 expansion from 2 to 6 months lowered the mother's daily income two months after childbirth by about 1.80 Euros per day. The expansion in leave coverage from 6 to 10 months in 1986 also reduced available income 6 months after childbirth by about 2.30 Euros per day. The 1992 reform increased job-protected leave from 18 to 36 months, but kept the duration during which benefits are paid constant at 18 months in the states Hesse and Schleswig-Holstein, and at 24 months in Bavaria. It is therefore not surprising that this expansion lowered available income 18 and 24 months after childbirth. None of the expansions had a significant effect on income in the long-run, after the expiration of the leave period.

In order to interpret the impact of expansions on leave coverage on children's outcomes, it is important to understand which mothers are affected by the reform. Table 3 reports the difference in the probability of working 2 months (1979 reform), 6 months (1986 reform), and 18 months (1992 reform) after childbirth before and after the reform, separately by the mother's education and her wage prior to childbirth. For the 1979 and 1986 reforms, the delay in the return to work varies little by the mother's education or wage prior to childbirth. For the 1992 reform, in contrast, the drop in the probability of working 18 months after childbirth is somewhat larger for mothers who earned a higher wage prior to childbirth. Recall that this reform increased only the job-protection period, but not the maternity benefit period. Hence, the results suggest that mothers who earn lower wages respond more strongly to maternity benefits, returning to work when maternity benefits expire, as opposed to when the job-protection period ends. Consequently, the mothers who were most affected by the 1992 reform appear to be somewhat positively selected.

The results so far refer to mothers who went on maternity leave. However, not all mothers in Germany take maternity leave. Figure 4 provides an overview about the incidence of leave taking. Vertical lines indicate a change in maternity leave legislation. We first approximate the incidence of leave taking as the number of women on leave observed in the social security data, divided by the number of births in a given year. There appears to be a clear long-run trend in leave taking: The fraction between the number of observations in the BLH and the total number of births increased from 31.74% in 1977 to 44.60% in 1993. These numbers are likely to underestimate leave taking because the social security data excludes up to 20% of the German workforce. Next, we provide a more reliable estimate of leave taking using data from the IABS 75-95 Plus, which is available from 1986 onwards. As expected, this data source reveals a higher incidence of leave taking by about 10 percentage points. More details, including a data description of the IABS 75-95 Plus, can be found in Schönberg (2008).

To summarize, the primary impact of the expansions in leave coverage is the delay in the return to work. The increase in leave from 2 to 6 months in 1979 caused 31% of mothers to return to work after 6 months instead of 2 months. Since at least one third, but probably closer to 45% of mothers went on maternity leave in 1979 (Figure 2), 10% to 15% of the children are affected by this reform. A similar calculation suggests that the 1986 and 1992 reforms induced about 13% and at least 5% of mothers to go back to work after 10 months rather than 6, or after 18 months rather than 36, respectively.

The extension in maternity benefits associated with the 1979 reform and the 1986 reform did not fully compensate mothers for the temporary income loss. The 1992 reform that only increased the job-protection period, but not the maternity leave period, lead to an even larger income loss. A back of the envelope calculation suggest that this resulted in an overall average income loss of about 19,000 Euros for those affected by the reform.[18] However, mothers who stay at home with their children are likely to save child care costs. Average child care costs in Germany are about 250 Euros per month. Hence, the 1992 reform resulted in a net income loss of about 14,500 Euros for those mothers who delayed their return to work due to the expansion in leave coverage.

5.2 Impact on Long-Run Outcomes of Children

We report our results separately by policy reform. We begin with the latest expansion in leave coverage: the 1992 increase in job-protected leave from 18 to 36 months.

5.2.1 The Expansion in Leave from 18 to 36 months (January 1992)

We start with a graphical analysis. Figure 5 compares grade attendance (Panel A), grade repetition (Panel B), as well as track choice (Panels C through E) of children who were born 6 months before or after the expansion in leave coverage from 18 to 36 months (i.e. between July 1991 and June 1992). The vertical line indicates the expansion in leave coverage. The figure also plots the predicted values, obtained from a linear regression that controls for a linear age trend and a dummy variable equal to 1 if the child was born after the policy reform and using children born 3 months before or after the reform (N=101,257). Regressions are weighted by the number of observations in each cell. We also report the coefficient on the discontinuous jump. Results refer to 13-14 year old pupils.

Consider first the share of pupils attending 8th grade or higher (Panel A). According to the Hamburg Accord, all pupils in the figure should have started school in the Fall of 1998 and–unless they repeated a grade–attend 8th grade in the Fall 2005. The figure reveals a strong age effect: Pupils born in July 1991 are more than 35 percentage points more likely to attend at least the 8th grade than pupils born

[18] This number is computed as follows. The average daily wage between the 18th and 36th month after childbirth in 1991 for women who returned to work 18 months after childbirth is 35 Euros - amounting to an income loss of 1about 19000 Euros ($18 \times 30 \times 35$).

in June 1992. There is little evidence that the expansion in leave coverage had an impact on this share; the regression discontinuity approach based on aggregated data gives a point estimate of the impact of the expansion -0.004, with a standard error of 0.007. Turning to grade repetition, there is again little evidence that the expansion in leave coverage from 18 to 36 months affected the share of pupils repeated a grade.

We now turn to our main outcome variable, track choice–which, from Table 1, is strongly correlated with test scores and wages later in life. Panels C to E plot the shares of students attending the low, intermediate, and high track school at ages 13-14. The figure again reveals a strong age effect: children born in May are 8 percentage points less likely to attend the highest track school than children born in September.[19] Panel E also suggests that the expansion in leave coverage lowered the share of pupils attending the highest track school, although the effect is not statistically significant. For the lowest and intermediate track choice, the point estimates are positive, but again not statistically significant.

One may argue that a delay in the mother's return to work does not benefit the average child, but does improve outcomes of children at the extreme. In order to evaluate this hypothesis, Panel F in Figure 5 plots the share of pupils attending special need schools. This includes pupils in schools for the deaf and the blind, but predominantly covers pupils with learning disabilities and pupils with behavioral problems. The figure provides little evidence that the expansion in leave coverage from 18 to 36 months reduced the share of pupils attending these schools.

Table 4 reports various estimates for the impact of the expansion in leave coverage on track choice, grade attendance, and grade repetition. All regressions are based on individual data and condition on gender and state. The first set of estimates (rows (1) to (3)) employ the regression discontinuity approach, and control for the age effect in a linear way. First, we use all pupils born two months before or after the change in legislation (i.e. pupils born between November 1991 and March 1992). We then successively increase the sample, and include all pupils born 3 or 4 months before or after the policy reform. In a second step, we provide a "placebo test" (row (4)) using all pupils who were born between

[19] Notice that children born in August and September are less (more) likely to go to schools of the lowest (highest) type than children born in July; similarly, children born in May are less (more) likely to go to schools of the lowest (highest) type than children born in June. This is because children born in July and August often enter school a year early, and are therefore somewhat younger at school entry than children born in September. Similarly, children born in June sometimes enter school a year late and are therefore slightly younger at school entry than children born in May.

September 1990 and April 1991, and thus unaffected by the expansion in leave coverage. Finally, we report difference-in-difference estimates (rows (5) to (10)), including children born 1 to 6 months before or after the expansion in leave coverage. Children born in the same birth month, but one year before the expansion in leave coverage, serve as the control group. We also report estimates (row (9)) where we exclude children born one months before or after the reform. This is an additional robustness check that our estimates are not affected by the endogenous timing of births as a response to the expansion in leave coverage.[20]

Consider track choice first. In line with Figure 5, both the regression discontinuity and difference-in-difference estimates suggest that the expansion in leave coverage from 18 to 36 months lowered the share of pupils attending high track schools by 0.6 to 1 percentage points. A similar reduction is not observed for the cohort of pupils not affected by the expansion (placebo test–row (4)). However, the coefficient is – if at all – only statistically significant at a 10 percent level. Given the precision of a typical estimate (e.g. -0.007 (0.005) DinD, November to February), we are able to rule out the hypothesis that the expansion increased attendance of the high track school by more than 0.1 percentage point. The point estimates for the low and intermediate track school are positive, smaller in magnitude, and not statistically significant.

Turning to the share of pupils attending special needs schools, the regression discontinuity and difference-in-difference approach yield somewhat conflicting results. However, when the sample is restricted to pupils born within two months of the policy reform (i.e. children born between November and February), neither approach suggests that the expansion in leave coverage from 18 to 36 months lowered attendance of special need schools. Estimates for the impact of the expansion in leave coverage on grade attendance vary from -0.007 (row (3)) to +0.005 (row (9)), while those on grade repetition range from -0.002 (row (1)) to +0.003 (row (3)). None of the estimates obtain statistical significance.

Overall, these results provide little if any support for the hypothesis that the expansion in leave coverage from 18 to 36 months improved children's schooling outcomes.

[20] Recall that we find little evidence for this in the number of births during the days and weeks surrounding the reform (Appendix A).

5.2.2 The Expansion in Leave from 6 to 10 months (January 1986)

Next, we turn to the expansion in leave coverage from 6 to 10 months. Our outcome variable here is *graduation* from the highest track school. This is an important outcome variable since, contrary to graduation from the other track choices, it provides direct access to university education. It is also strongly correlated with earnings in early adulthood (see Table 1, column (2)). We measure graduation in two ways: first, graduation at age 18-19 and second, graduation at age 18-19 or 19-20. The latter variable includes students who started school a year late or repeated a grade once.

Table 5 reports various estimates for the impact of the expansions on high school graduation. The table has a similar structure to Table 3; we begin with regression discontinuity estimates (rows (1) and (2)); we then report estimates from the "placebo test", based on children born between September and April one year after the expansion. Finally, we display difference-in-difference estimates where we use children born in the same birth month, but one year after the expansion in leave coverage as a control group. Results are based on data aggregated to the birth month, and weighted by the number of observations in each cell. Note that because we only have two years of data, we can compute only graduation at age 18-19, but not at ages 18-19 or 19-20 for the 1986-87 cohort. Similarly, for the 1984-85 cohort our data only allows us to compute graduation at 19-20. For this reason, we report the difference-in-difference (and placebo) estimates only for graduation by age 18-19, using the 1986/87 as the control group.

All estimation methods give similar results: the coefficient on the expansion from 6 to 10 months is negative, and ranges from -0.003 to -0.006. Given the precision of a typical estimate (e.g. -0.005 (0.006), DinD, October to March), we can rule out the hypothesis that the expansion raised graduation by more than 0.8 percentage points. Again, these results are not supportive of the hypothesis that expansions in leave coverage improve children's educational outcomes.

5.2.3 The Expansion in Leave from 2 to 6 months (May 1979)

Next, we turn to the expansion in leave coverage in 1979 from 2 to 6 months. Our main outcome variable here is the log-wage at age 25-26; as additional outcome variables we use unemployment and the share of workers who are low- and medium-skilled. Not all individuals may have entered the labor market by that age, most importantly because they are still full-time students. This is especially problematic if the expansion affected enrollment at universities. We first analyze the impact of the expansion on wages, conditional on labor market entry. We then discuss how selection into work may affect our results.

The Impact on Wages, Education, and Unemployment We begin with a graphical analysis. Figure 6, Panel A, plots the average log-wage at age 25-26 (i.e. October 2004) for individuals born between May 1978 and August 1979. Panel B provides a similar analysis for unemployment, measured as the number of individuals in registered unemployment in our data, divided by the number of observations in our data. Panels C and D focus on the share of workers with low and medium levels of education. We proxy this share as the number of individuals in our data with low (i.e. no post-secondary education) or medium (i.e. apprenticeship completion) level of education, divided by the number of births.[21] In each figure, the solid vertical line indicates the expansion in leave coverage from 2 to 6 months, while the dashed vertical lines indicate the discontinuity due to school entry.

The figure suggests that the expansion in leave coverage lowered wages; the coefficient on the discontinuous jump is -0.005, with a standard error of 0.003 (Panel A). There is little evidence that the expansion in leave coverage affected unemployment. The coefficient on the discontinuous jump is 0.001, with a standard error of 0.003 (Panel B). Panels C and D further suggest that the expansion increased the share of the low-skilled by about 0.5 percentage points, and lowered the share of the medium-skilled by more than 1 percentage point. The latter effect is statistically significant at a 10 percent level.

However, these regression-discontinuity estimates hinge on the assumption that the age effect is linear. This assumption may to be violated because the reform affected children born close to the July cut-off date for school entry (i.e. children born in April and May). Since the later expansions in leave coverage from 6 to 10 months and 18 to 36 months affected children born farther from the cut-off date (i.e. children born in December and January), the assumption of a linear age trend is less problematic here. Figure 5, Panel A, provides some evidence for this. While the share of pupils attending 8th grade

[21] The estimate for the impact of the expansion in leave coverage is very similar if we divide by the number of all observations in our data instead. We consider our computation a better proxy for the true share of the low- and medium-skilled because university graduates may not yet have entered the labor market and are therefore not included in our data.

or higher declines roughly linearly with age up until April, it drops sharply from May to June.

Table 6 compares results from the regression discontinuity approach with those from the difference-in-difference approach. Results on wages and unemployment are based on the individual data and control for state and gender. Results on education are based on data aggregated at the birth month and birth year level and are weighted by the average number of observations in each cell. Consider first wages: the regression discontinuity approach gives a small negative estimate (-0.001) when children born between March to June are used. The estimate becomes more negative and marginally significant when the sample is extended to children born between January and June. However, our placebo tests produce estimates of similar magnitude, confirming that the assumption of a linear age effect is indeed not appropriate. Difference-in-difference estimates are all close to zero, and statistically insignificant. Given the precision of a typical estimate (e.g. -0.001 (0.003), January-June, DinD, CG 77-78), we reject the hypothesis that the expansion increased wages by more than 0.3% at a 5% level.

A similar picture emerges for the share of the workforce that are low-skilled, and in particular for the share that are medium-skilled. Here, the preferred difference-in-difference estimates are small in magnitude and statistically insignificant. Given the precision of a typical estimate we can rule out the hypothesis that the expansion in leave coverage from 2 months to 6 lowered the share of the low-skilled by more than 0.5 percentage points, or increased the share of the medium-skilled by more than 0.4 percentage points, at a 5% level. Turning to unemployment, both our regression discontinuity and difference-and-difference estimates consistently suggest that the expansion had little impact on unemployment.

We thus conclude that there is little evidence that the expansion in leave coverage from 2 to 6 months improved labor market or education outcomes.

Selection into Work Our outcomes in the previous section refer to individuals at ages 25-26. At that age, some workers may still be full-time students and thus have not entered the labor market yet. This may bias our results if the expansion in leave coverage had an impact on college enrollment. The question we focus on in this section is: is it possible that the expansion in leave coverage did have a positive impact on children's labor market outcomes, but that we fail to detect this effect because the expansion also changed people's selection into work? For example, suppose that the reform increased college enrollment and that the individuals who entered college due to the reform were positively selected compared to those who did not attend college, and negatively selected compared to those who would have attended college even without the reform. This would imply that compared to before the reform, the best workers have not yet entered the labor market after the reform. We may therefore find that the expansion had no impact on average wages (conditional on working), even if it did in fact raise wages.

In order to assess this concern, we first compare the ratio between the number of observations (with a valid wage) in our data and the number of total births, before and after the reform. Figure 6, Panel E, plots the share for children born around the reform, and Table 6 reports estimates from the regression discontinuity and difference-in-difference approach. The preferred difference-in-difference estimates are small in magnitude and statistically insignificant, regardless of the sample our results are based on. We interpret this as a first piece of evidence that the selection into work does not severely bias our estimates.

Second, we compare the entire distribution of log-wages before and after the reform. The idea here is that if the expansion in leave coverage did affect selection into work, the density of log-wages should look different for children born before and after the reform. If, in contrast, the entire distribution of wages is similar before and after the reform, then this is evidence that the impact of the expansion in leave coverage on wages is indeed zero. Figure 8 plots the kernel density of log-wages for individuals born in March or April with that of individuals born in May or June. In order account for the fact that individuals born after the reform are two months younger on average, we add to each May/June wage observation the average wage difference between individuals born in March/April or May/June in the year before the expansion. The figure shows that the wage density is very similar before and after the reform, and the Kolmogorov-Smirnov test does not reject equality of the two distributions (p-value: 0.213). We interpret this as additional evidence that the zero mean impact of the expansion in leave coverage is not due to the changed selection into work.

6 Discussion and Interpretation

The Impact of Expansions in Leave Coverage on Child Outcomes

Recently, several countries have expanded leave coverage after childbirth. An important goal behind these reforms was to improve children's welfare, through a reduction in early maternal employment. Evaluating three major expansions in leave coverage in Germany, we find little support for the hypothesis that expansions in leave coverage boost children's long-term education and labor market outcomes. The 1979 expansion in leave coverage from 2 to 6 months had little impact on wages and unemployment at age 25, or on the share of individuals with low and medium levels of education. Given the precision of our estimates, we can rule out the hypothesis that the expansion increased wages by more than 0.3%, and lowered unemployment by more than 0.3 percentage points.

There is also little evidence that the 1986 expansion in leave coverage from 6 months to 10 increased graduation from a selective high school (*Abitur*); here, our point estimates range from -0.003 to -0.006, and we are able to rule out the hypothesis that the expansion raised graduation by more than 0.8 percentage points at the 5% significance level for a typical specification.

Turning to the expansion in leave coverage from 18 to 36 months, our point estimates consistently suggest that the expansion lowered selective high school attendance at age 14 by about 0.6 percentage points. The coefficient is (marginally) significant for some, but not all specifications. We can reject the hypothesis that the expansion raised selective high school attendance by more than 0.1 percentage points at a 5% level for a typical specification. How does the impact of the expansion on track choice translate into an impact on educational attainment? Pupils leave the low track schools after 9th grade, the intermediate track schools after 10th grade, and the high track schools after 13th grade. Hence, under the assumption that pupils graduate from the track choice which they attend at age 14, a typical estimate of the effect of the expansion of leave coverage on track choice (e.g. September to April, DinD in Table 4) implies a reduction in 0.02 years (or about one week) of education.[22] Given the precision of our estimates, we are able to reject the hypothesis that the expansion increased years of schooling by more than 0.09 years (or about 4.5 weeks). This calculation ignores that track choice may have an

[22] This is computed as $0.002 \cdot 9 + 0.004 \cdot 10 - 0.006 \cdot 13$; see Table 4, DinD, September to April.

impact on post-secondary education, and may therefore understate the negative impact of the reform on years of schooling. What about wages? From Table 1, column (2), graduation from the high track school is associated with a wage increase of 0.494 log-points relative to graduation from the low track school, and 0.260 log-points relative to graduation from the intermediate track school. Hence, under the assumption that these numbers are causal, a typical estimate of the expansion in leave coverage from 18 to 36 months on track choice implies a reduction in wages by 0.34%.[23]

We have also used the estimates in Table 1, column (1), to transform the impact of the expansion in leave coverage on track choice into an impact on test scores. This approach assumes that the test score of a child who, due to the reform, no longer attends the highest track choice is reduced by the *mean* difference in test scores of children in the highest and intermediate track choices. However, in reality it is probably children who are at the margin of attending the intermediate and highest track choices who are affected by the reform. This leads us to overestimate the impact of the expansion on average test scores. According to this computation, the expansion lowered test scores by about 0.014 of a standard deviation.[24]

In order to interpret these estimates of the impact of expansions of leave coverage on children's outcomes, it is important to understand which mothers are affected by the reforms. Recall from Table 3 that for the 1979 and 1986 expansions, mothers with low and high levels of education, as well as mothers with low and high pre-birth wages, reacted similarly to the reform. For the 1992 expansion in leave coverage from 18 to 36 months, in contrast, women who earned a higher wage prior to childbirth showed a somewhat larger reduction in the probability of working 18 months after childbirth. Hence, mothers who are most affected by this reform are somewhat positively selected. Some studies have found early maternal employment to be more detrimental on child development for better educated and high-wage women.[25] This reinforces our finding of no positive (or at least no large positive) effects on child outcomes for this reform.

[23] For this calculation, we again used difference-estimates for the September-April sample in Table 4. The number is computed as $0.238 \cdot (0.004–0.002) + 0.498 \cdot (-0.006\text{-}0.002)$; see Table 1, Column 2.

[24] For this calculation, we again used difference-estimates for the September-April sample in Table 4. The number is computed as $0.663 \cdot (0.004–0.002) + 1.979 \cdot (-0.006\text{-}0.002)$; see Table 1, Column 2.

[25] See for instance Ruhm (2005), Gregg et al. (2005), Bernal (2006), Bernal and Keane (2006a, 2006b).

The Impact of Early Maternal Employment on Child Outcomes What do our findings have to say about the impact of early maternal employment on child development? We would like to stress that the following estimates have to be interpreted with considerable caution. First, the computation assumes that the only impact of the expansion in leave coverage on mothers is the delay in the return to work. While this is clearly the primary effect, there may be other secondary effects, such as effects on fertility (see Lalive and Zweimüller 2005). Second, the reduced form estimates of the expansions on child outcomes are, if at all, only marginally significant.

Consider first the expansion in unpaid leave from 18 to 36 months in 1992. From Section 5.1, this reform caused at least 5% of mothers to return to work after 36 months rather than after 18 months. Hence, a simple Wald estimate suggests that returning to work after 3 years rather than 18 months lowers attendance at the highest track choice by about 12 ($0.06 \cdot 100/5$) percentage points (see Table 4). This effect is similar in magnitude to delaying school entry by one year. Hence, using arguably exogenous variation in early maternal employment, we are unable to confirm the large negative impact of mothers' weeks worked after childbirth on children's selective high school attendance that we found in the GSOEP.

Transforming this effect into an effect on test scores, returning to work after 36 rather than 18 months lowers test scores by about 0.28 of a standard deviation–an effect that is similar in magnitude to that of an increase in class size of 8 students (Krueger 1999). While this estimate has to be interpreted with considerable caution, one possible explanation for this negative impact of delaying the return to work is the loss in income. The reform lead to a total income loss–net of the savings in child care–of about 14,500 Euros for mothers who were affected by the reform. While the causal effect of income on child outcomes is still in dispute, a recent study by Dahl and Lochner (2006) exploits variation in the Earned Income Tax Credit (EITC) as a plausibly exogenous variation in income, and finds a considerable positive impact of income on the child's academic achievement: an additional $1,000 increases test scores by 0.021 (math) or 0.036 (reading) of a standard deviation. This suggests that the loss in income can go a long way in explaining part of the negative impact of the expansion on track choice and test scores.[26]

[26] Ruhm (2000) and especially Tanaka (2005) also report that paid leave reduces infant mortality rates, while other leave has no such effect.

An alternative explanation for the negative impact of delaying the return to work on children's school outcomes is that children older than 18 months benefit from exposure to care givers other than their mothers. While there is still no agreement on what is the impact of day care on child achievement, some studies have found that pre-kindergarten programs may raise children's test scores.[27]

Next, consider the expansions in leave coverage in 1979 from 2 to 6 months and from 6 to 10 months in 1986. The primary effect of these reforms is that about 10-15% returned to work 6 months after childbirth rather than 2, or 10 months rather than 6. Hence, a Wald estimate for the impact of delaying the return to work from 2 to 6 months, or 6 to 10 months, can be obtained by multiplying the reduced form estimates by about 8. Since none of the reduced form estimates obtains statistical significance, and p-values are often larger than 0.5, we do not consider these estimates to be economically meaningful. We would like to point out, however, that our estimates for the 1979 reform are precise enough to rule out some of the large negative findings reported in the literature. Consider for instance the estimate by Baum (2003) who finds that working every week during the child's first year reduces the child's test scores (such as the Peabody Individual Achievement Test of Mathematics and Readings Recognition) around the age of 5 by 0.23 of a standard deviation. Others, such as Bernal (2006) and Bernal and Keane (2006a, 2006b) have found effects of similar magnitude. Suppose the effect on test scores translates one to one into an effect on wages, and that the impact of employment is linear during the first year of the child's life. Under these assumptions, Baum's estimates imply that delaying the return to work from 2 to 6 months should increase wages by about 0.077 of a standard deviation, and the expansion in leave coverage from 2 to 6 months should have increased wages by about 0.01 of a standard deviation. If we use the standardized log-wage as our dependent variable, a typical point estimate is -0.004, with a standard error of 0.008. Put differently, the standard deviation of log-wages is about 0.34; we should

[27] For instance, Magnuson, Ruhm, and Waldfogel (2005) report that pre-kindergarten care is associated with higher reading and mathematics skills at school entry–although it also increases behavioral problems. In contrast, Baker, Gruber, and Milligan (2008) find that the introduction of universal, highly-subsidized child care in Quebec left children (up to age 5) worse off in a variety of dimensions, ranging from aggression to motor skills to illness. Bernal and Keane (2006a) find that informal child care (i.e. care by siblings or other relatives, grandparents, or other relatives, or by non-relatives, in non-center-based settings) is particularly harmful, while formal child care (i.e. center-based care, pre-school) is not associated with lower academic performance. In other related research, Currie and Thomas (1995) and Garces et al. (2002) provide evidence that Head Start improves test scores and educational attainment, especially among whites. However, these findings may not be directly relevant in our context, since Head Start was explicitly aimed at improving the learning and social skills of poor children, aged between 3 and 5.

therefore observe that the expansion increases log-wages by about 0.34 %. Given the precision of our estimates, we are (just) able to rule out such large detrimental effects on wages at a 5 percent level.

7 Conclusion

In this paper, we evaluate the impact of three major expansions in leave coverage in Germany on children's long-term educational and labor market outcomes. We identify the causal impact of the reform by comparing outcomes of children born shortly before and after the reform, and therefore rely on weaker assumptions for identification than existing studies. We find no evidence that the expansions improved children's outcomes, although they had a strong impact on mother's labor supply after childbirth. Given the precision of our estimates, we are able to rule out that the 1979 expansion in paid leave from 2 to 6 months increased children's wages by more than 0.3%, and that the 1992 expansion in unpaid leave from 18 to 36 months increased selective high school attendance by more than 0.1 percentage points. The latter finding contrasts with the large negative correlation between mother's weeks worked during the child's first three years and selective high school attendance that we found in the GSOEP.

Our results therefore do not support the hypothesis that the expansions in maternity leave coverage implemented in Germany since the late 1970s have improved children's long-term education and labor market outcomes. This, however, was an important goal of the reforms. This casts doubt on whether the expansions recently implemented in other countries, which were likewise motivated by the child's well-being, will be successful at achieving this goal.

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A Timing of Births and the 1992 Policy Reform

In this section, we investigate whether the expansion in unpaid leave coverage from 18 to 36 months affected the timing of births. In particular, we check whether there is an increase in the number of births in days just after, and a decrease in the number of births just before, the reform commenced. Gans and Leigh (2006) find some evidence of such behavior with the introduction of a $3000 "Baby Bonus" on July 1, 2004 in Australia. In Figure A.1, Panel A, we plot the number of live births per day in December 1991 and in January 1992, i.e. one month before and after the reform came into effect. For comparison, Panel B shows the number of live births per day in December 1993 and January 1994, when there was no change in maternity leave legislation.[28] While fewer children are born during the weekend and during the holidays, there is little evidence for a drop just before, or an increase just after, the reform commenced. In Panel C, we plot the regression-adjusted differences between the numbers of births per week from August 1993 to May 1994 and from August 1991 to May 1992. The regression controls for day of the week, bank holidays (Easter, Ascension Day, Pentecost, Day of the German Unification, All Saints Day, Christmas, and New Year's Day). There is no clear pattern visible. The difference between the numbers of births in the first week of January 1992 and December 1991, relative to January 1994 and December 1993, is 25.6, with a standard error of 17.4.

To conclude, there is little evidence that the expansion in leave coverage from 18 to 36 months in 1992 affected the timing of births.

B Data Appendix

B.1 Administrative Data on School Choice

For the expansion in leave coverage from 6 to 10 months in 1986, our outcome variable is graduation from the highest track choice (*Abitur*). We define two variables: graduation by age 18-19, and graduation by age 19-20. For pupils born between July 1985 and June 1986, graduation by age 19 is defined as the number of German pupils in 13th grade in the 2004-05 school year, divided by the number of births to

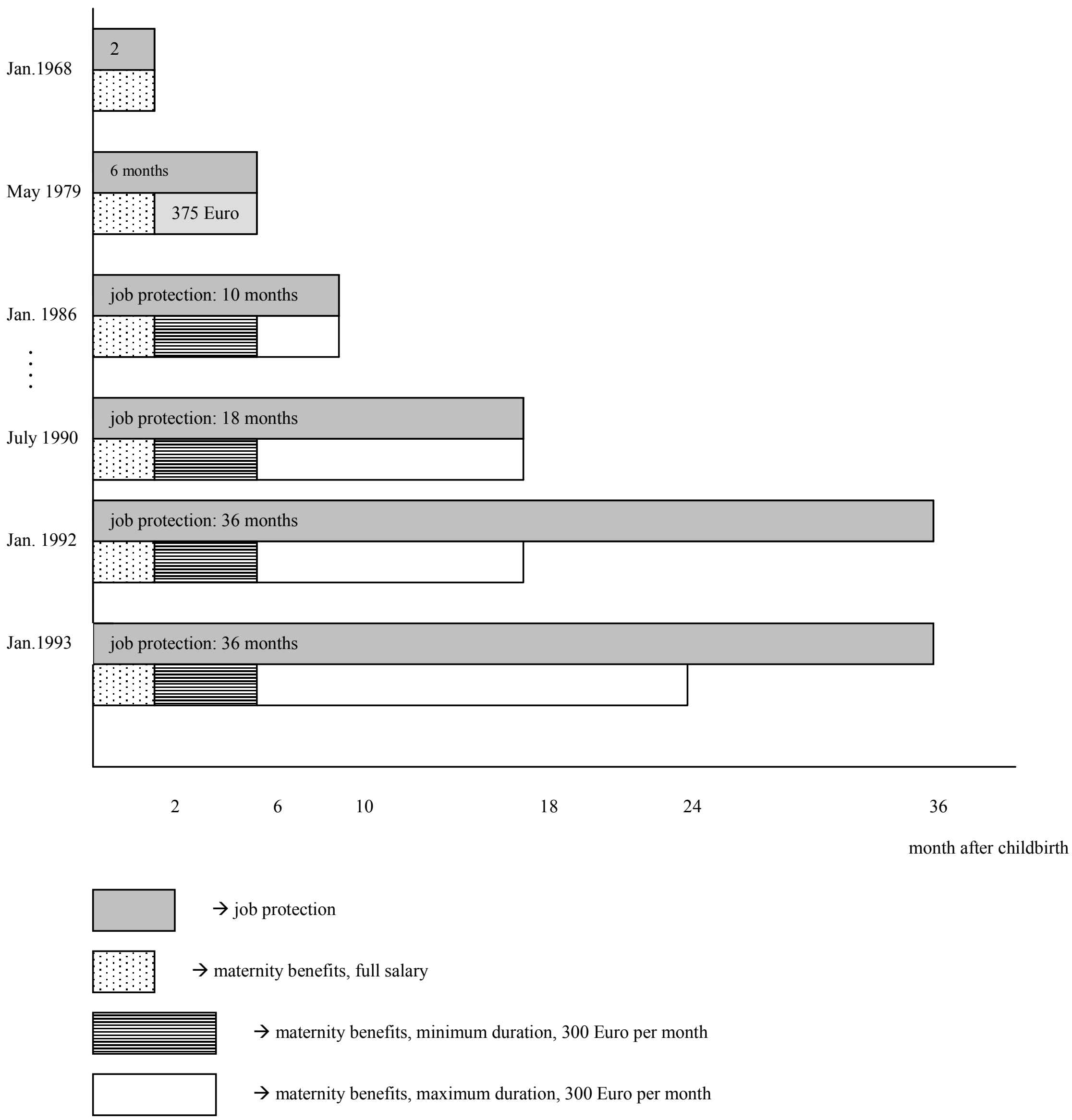
[28] We do not use December 1992 and January 1993 as a comparison group because in January 1993 the period during which maternity benefits are paid was extended from 18 to 24 months.

German parents; these are pupils who started school as implied by the Hamburger Accord and never repeated a grade. For the same cohort, graduation by age 19/20 is defined as the number of German pupils in 13th grade in school year 2004/05 plus the number of German pupils in 13th grade in school year 2005/06, divided by the number of births to German parents. This variable includes pupils who either started school one year late or repeated a grade once. For pupils born between July 1986 and June 1987 high school graduation is defined accordingly. Note that for this cohort, high school graduation by age 19/20 cannot be computed, as these pupils are only in 13th grade in the latest year the data is available, unless they entered school early or skipped a grade. Similarly, high school graduation cannot be computed for earlier cohorts since some pupils have already graduated by the first year the data is available.

B.2 Administrative Data on Employment Outcomes

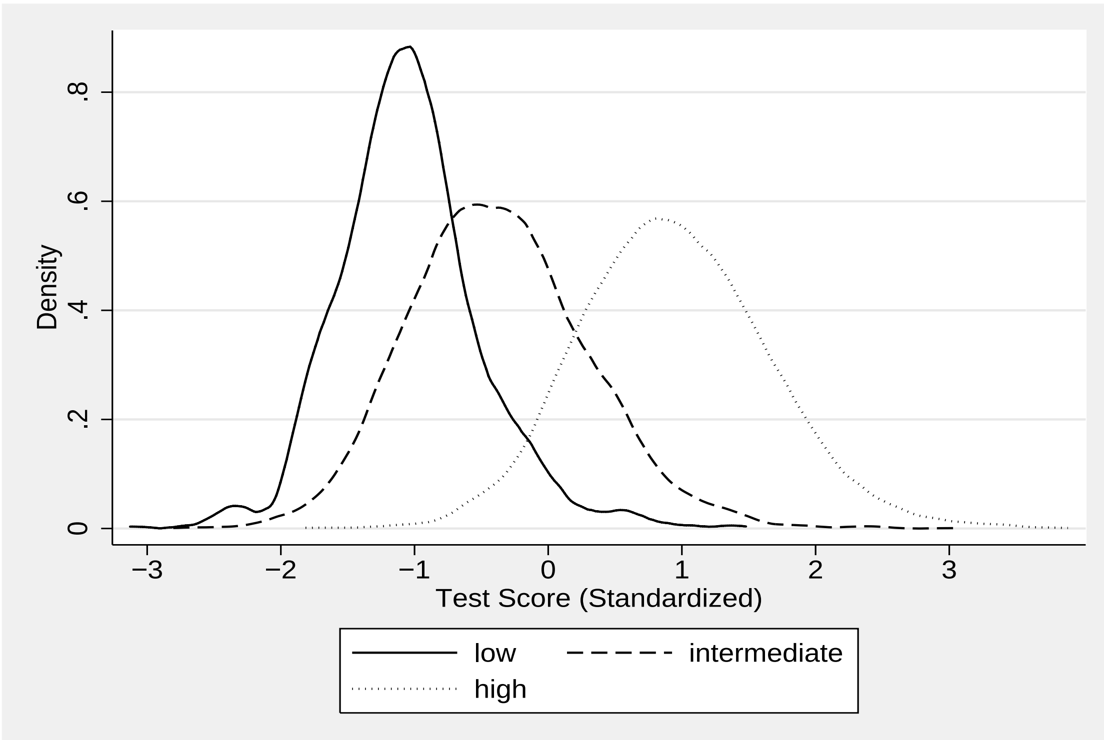
We restrict the analysis to West Germans, and delete all workers whose first spell classifies the worker as a foreigner or from East Germany. Our wage analysis considers only formal jobs for which social security contributions have to be paid. Marginal jobs, apprenticeship spells, and observations with a wage less then 20 Euros per day are discarded. Our unemployment variable is based on registered unemployment. Here, our analysis uses all workers in the data (including those on marginal jobs), and we define a dummy variable that is equal to 1 if the worker is registered as unemployed, and zero otherwise. We define the medium-skilled as individuals who have completed apprenticeship training or have graduated from the highest track choice (*Abitur*). Workers who are not currently in apprenticeship training and have neither completed apprenticeship training nor graduated from high school are considered as low-skilled. The low-skilled include workers with unknown education. In the original data, the education variable is missing if the worker is unemployed. We therefore impute the education for unemployed workers using the future and previous wage spells.

Figure 1: Maternity Leave Legislation in Germany (Selected Reforms)



Note: Since 1986, all women -- employed before childbirth or not -- are entitled to a maternity benefit of 300 Euros per month for a minimum of 6 months. From the 7th month onwards, maternity benefits are means-tested, and depend on the annual net family income two years before childbirth. The majority of women receive benefits longer than 6 months. In January 1988, maternity leave was extended from 10 to 12 months. Two further changes occurred in July 1989 and July 1990, when maternity leave was increased to 15 and 18 months, respectively.

Figure 2: Test Score Distribution by Track Choice, 7th Grade



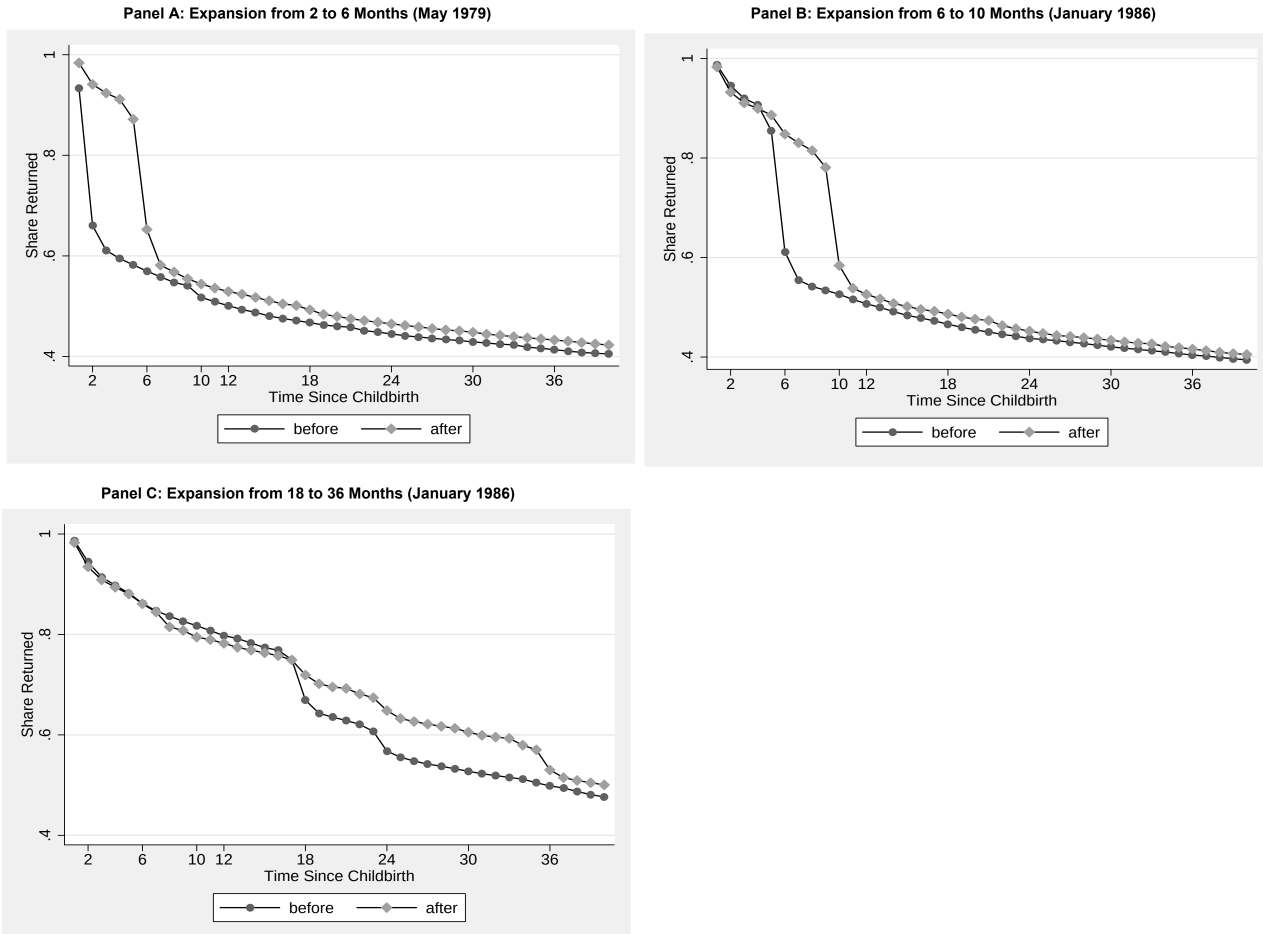
Note: The figure plots the distribution of the combined performance index (math, German, English) by school type in 7th grade.
Data Source: LAU. Sample of pupils in Hamburg who attended 7th grade in 1998.

Table 1: Track Choice and Test Scores

	1	2
	Test score	Log-wage
intermediate	0.664	0.238
	(0.024)***	(0.005)***
high	1.979	0.494
	(0.025)***	(0.005)***
adj. R squared	0.538	0.347
N	9,836	31,175

Note: The dependent variable in Column 1 is the combined (math, German, English) performance index in 7th grade. The lowest track coice is the omitted category. The dependent variable in column 2 is the log real hourly wage. The sample is based on the years 1984 to 1998, and includes employed men and women between ages 21 and 65. Regressions condition on age (polynomial of order 3), gender, and year dummies.
Data sources: LAU, Sample of pupils in Hamburg who attended 7th grade in 1998 (column 1). GSOEP for men and women between 21 and 65 years of age, 1984-1998 (column 2).

Figure 3: The Impact of Leave Coverage on Mother's Decision When to Return to the Labor Market



Note : The figures compare the shares of women who have returned to the labor market t months after childbirth, for women who give birth shortly before or shortly after the expansion in leave coverage.
Source : BLH Social Security Records. Panel A: West German women who give birth in March, June or July 1979. Panel B: German women who give birth in Bavaria, Hesse, or Schleswig-Holstein in October and November 1985 or March 1986. Panel C: German women who give birth in Bavaria, Hesse, or Schleswig-Holstein in October and November 1991 or March 1992.

Table 2: The Impact of Leave Coverage on Labor Supply and Wages after Childbirth

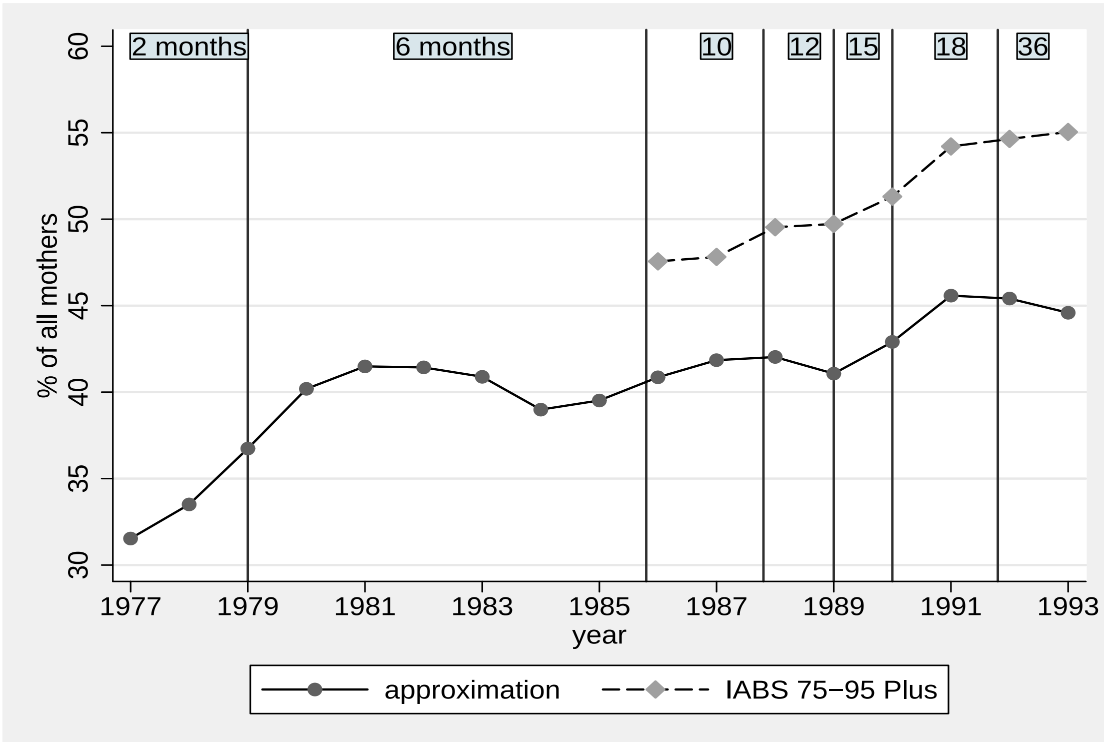
	Panel A: 2 versus 6 months (May 1979)				
	Working t months after childbirth				
	2	6	24	36	60
Fraction working, 3/79	*0.389*	*0.395*	*0.396*	*0.379*	*0.371*
March 79 vs June/July 79, DinD	**-0.299**	0.024	0.009	0.012	0.008
(control group: 1978)	(0.006)***	(0.007)**	(0.007)	(0.007)*	(0.007)
	Available income t months after childbirth				
	2	12	24	36	60
Daily income, 3/79	*16.35*	*16.88*	*15.97*	*15.09*	*15.03*
March 79 vs June/July 79, DinD	**-1.817**	0.404	0.204	0.285	0.178
(control group: 1978)	(0.326)***	(0.319)	(0.315)	(0.311)	(0.313)
	Panel B: 6 versus 10 months (January 1986)				
	Working t months after childbirth				
	6	10	30	42	66
Fraction working, 11/85	*0.442*	*0.459*	*0.438*	*0.424*	*0.448*
Oct./Nov. 85 vs March 86, DinD	**-0.294**	-0.023	0.000	0.004	0.003
(control group: 1984/85)	(0.010)***	(0.011)**	(0.011)	(0.011)	(0.011)
	available income t months after childbirth				
	6	18	30	42	66
Daily income, 11/85	*17.48*	*19.19*	*18.67*	*18.13*	*19.98*
Oct./Nov. 85 vs March 86, DinD	**-4.913**	-0.194	-0.190	0.104	-0.158
(control group: 1984/85)	(0.516)***	(0.538)	(0.546)	(0.552)	(0.580)
	Panel C: 18 versus 36 months (January 1992)				
	Working t months after childbirth				
	6	18	24	36	66
Fraction working, 11/91	*0.153*	*0.328*	*0.384*	0.376	*0.409*
Oct./Nov. 91 vs March 92, DinD	-0.014	**-0.085**	**-0.083**	0.018	-0.006
(control group: 90/91)	(0.007)**	(0.009)***	(0.009)***	(0.009)*	(0.010)
	available income t months after childbirth				
	18	24	42	66	90
Daily income, 11/91	*18.53*	*16.76*	*16.73*	*18.30*	*19.79*
Oct./Nov. 91 vs March 92, DinD	**-3.981**	**-3.388**	0.517	-0.165	0.078
(control group: 90/91)	(0.436)***	(0.480)***	(0.490)	(0.502)	(0.520)

Note: The table reports the impact of an expansion in leave coverage on labor force participation rates and available income t months after childbirth. Income is defined as the woman's daily wage if she is working; if the mother is not working, the income is equal to the daily maternity benefit in case the mother is eligible and zero otherwise. We report difference-in-difference estimates, using mothers who give birth in the same birth months, but one year before the reform, as a control group. Robust standard errors in parentheses.

*Statistically significant at 0.10 level, ** at 0.05 level, *** at 0.01 level.

Data Source: BLH Social Security Records for women who give birth in around te policy change. Results for the expansion in leave coverage from 6 to 10 months and 18 to 36 months refer to the three states only for which data on children are available.

Figure 4: The Incidence of Leave Taking



Note: Dots approximate the share of mothers taking maternity leave as the number of women on leave in the BLH, divided by the number of births in that year. This measure is best interpreted as a lower bound for the true share, as the BLH covers only 80% of the German workforce. Diamonds provide a more reliable estimate of the share of mothers taking maternity leave based on the IABS 75-95 Plus. Vertical lines indicate an expansion in leave coverage.

Data Source: BLH Social Security Records and IABS 75-95 Plus, 1977-1993.

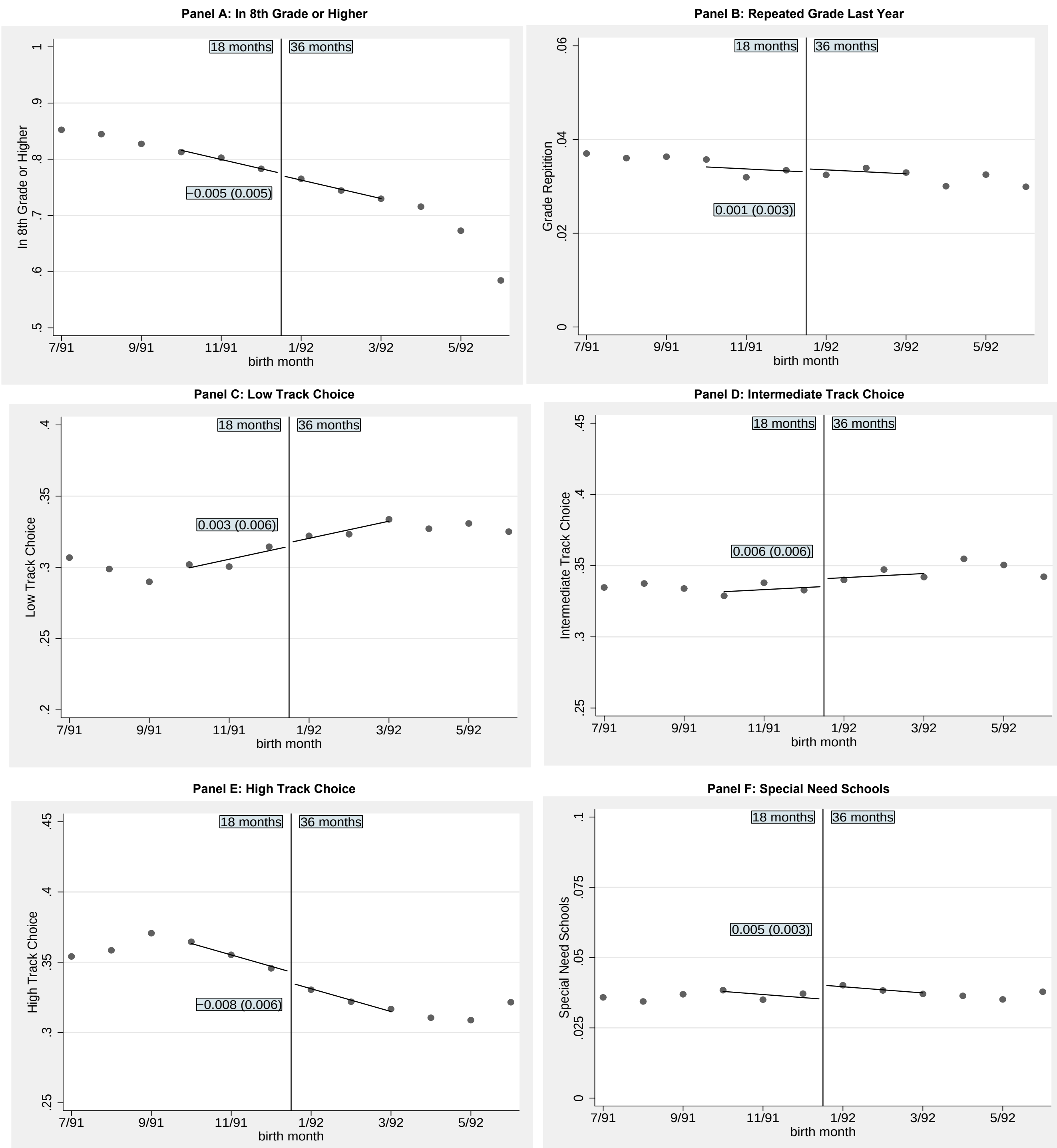
Table 3: Which Women Are Affected by the Expansion in Leave Coverage?

Panel A: Expansion 2 to 6 months (May 1979)					
			Education		
	low	medium	high		p-value
Delays return to work	-0.295	-0.310	-0.344		0.325
	(0.011)***	(0.005)***	(0.035)***		
			Pre-birth wage		
	1st quartile	2nd quartile	3rd quartile	4th quartile	p-value
Delays return to work	-0.290	-0.281	-0.323	-0.303	0.113
	(0.013)***	(0.013)***	(0.013)***	(0.013)***	
Panel B: Expansion 6 to 10 months (January 1986)					
			Education		
	low	medium	high		p-value
Delays return to work	-0.313	-0.289	-0.313		0.640
	(0.025)***	(0.012)***	(0.060)***		
			Pre-birth wage		
	1st quartile	2nd quartile	3rd quartile	4th quartile	p-value
Delays return to work	-0.300	-0.296	-0.273	-0.306	0.686
	(0.020)***	(0.020)***	(0.021)***	(0.021)***	
Panel C: Expansion 18 to 36 months (January 1992)					
			Education		
	low	medium	high		p-value
Delays return to work	-0.076	-0.088	-0.045		0.585
	(0.023)***	(0.010)***	(0.047)		
			Pre-birth wage		
	1st quartile	2nd quartile	3rd quartile	4th quartile	p-value
Delays return to work	-0.076	-0.067	-0.078	-0.123	0.062
	(0.017)***	(0.017)***	(0.018)***	(0.018)***	

Note: The table reports how the delay in the return to work due to the expansion in leave coverage differs by education and pre-birth wage (4 equally sized groups). The dependent variable in Panel A is equal to 1 if the mother is working 2 months after chldbirth, and 0 otherwise. Results are based on women who give birth in March, June, and July, and use women who give birth in the same months, but one year after the expansion, as a control group. The dependent variable in Panel B is equal to 1 if the mother is working 6 months after childbirth, and zero otherwise. The dependent variable in Panel C is equal to 1 if the mother is working 18 months after childbirth, and zero otherwise. Results in Panels B and C are based on women who give birth in October, November, and March, and use women who give birth in the same months, but one year before the expansion as a control group. The last column reports the p-value for the hypothesis that the impact of the reform does not differ by education and the pre-birth wage. Robust standard errors in parentheses.

Source: BLH Social Security Records. Panel A: West German women who give birth in March, June or July 1979. Panel B: German women who give birth in Bavaria, Hesse, or Schleswig-Holstein in October and November 1985 or March 1986. Panel C: German women who give birth in Bavaria, Hesse, or Schleswig-Holstein in October and November 1991 or March 1992.

Figure 5: The Impact of the Expansion in Leave Coverage from 18 to 36 Months on Children's Outcomes



Note: The figures plot the shares of pupils in 8th grade or higher (Panel A), the shares of pupils who repeated a grade last year (Panel B), the shares of pupils in the low, intermediate, or high track choice (Panel C to E), as well as the shares of pupils in special need schools (Panel F), for children born 6 months before or after the expansion in leave coverage. The figures also plot predicted shares, obtained from a regression that controls for a linear age trend and a dummy variable equal to 1 if the child was born after the policy reform and using children born 3 months before or after the reform (N=101,257). The figure in the box is the coefficient on this discontinuous jump. The vertical line indicates the expansion in leave coverage from 18 to 36 months.
Data Source: Administrative School Data for Bavaria, Hesse, and Schleswig-Holstein for pupils born between July 1991 and June 1992, 2005-06.

Table 4: The Impact of the Expansion in Leave Coverage from 18 to 36 Months on Children's Outcomes, Alternative Specifications

		track choice				grade attendance	
		Special need	*Low*	*Intermediate*	*High*	*In 8th grade*	*Repeated grade*
		Regression discontunuity					
(1)	November - February	0.003	0.001	0.006	-0.007	0.002	-0.002
	linear age trend	(0.003)	(0.008)	(0.008)	(0.008)	(0.007)	(0.003)
	N	67,086	67,086	67,086	67,086	67,086	57,507
(2)	October - March	0.005	0.003	0.006	-0.008	-0.005	0.001
	linear age trend	(0.003)*	(0.006)	(0.006)	(0.006)	(0.005)	(0.003)
	N	101,257	101,257	101,257	101,257	101,257	86,650
(3)	September - April	0.004	0.005	0.005	-0.010	-0.007	0.002
	linear age trend	(0.002)*	(0.005)	(0.005)	(0.005)*	(0.005)	(0.002)
	N	136,366	136,366	136,366	136,366	136,366	116,732
		Placebo test: cohort 1990-91					
(4)	September - April	0.000	-0.005	0.005	0.000	-0.002	-0.004
	linear age trend	(0.002)	(0.005)	(0.005)	(0.005)	(0.005)	(0.002)
	N	142,712	142,712	142,712	142,712	142,712	122,161
		Difference-in-difference (control group: 1990-91)					
(5)	January - December	0.002	0.008	0.002	-0.011	-0.002	0.000
		(0.003)	(0.007)	(0.007)	(0.007)	(0.006)	(0.003)
	N	69,393	69,393	69,393	69,393	69,393	59,346
(6)	November - February	0.001	0.007	-0.001	-0.007	-0.001	0.003
		(0.002)	(0.005)	(0.005)	(0.005)	(0.005)	(0.002)
	N	136,411	136,411	136,411	136,411	136,411	116,717
(7)	October - March	-0.001	0.003	0.003	-0.007	-0.001	0.003
		(0.002)	(0.004)	(0.004)	(0.004)*	(0.004)	(0.002)
	N	207,095	207,095	207,095	207,095	207,095	177,152
(8)	Oct. - Nov., Feb. - March	-0.002	0.001	0.004	-0.005	0.000	0.003
		(0.002)	(0.005)	(0.005)	(0.004)	(0.005)	(0.002)
	N	137,702	137,702	137,702	137,702	137,702	117,778
(9)	September - April	-0.003	0.002	0.004	-0.006	0.002	0.000
		(0.001)*	(0.003)	(0.004)	(0.004)*	(0.003)	(0.002)
	N	279,078	279,078	279,078	279,078	279,078	238,893
(10)	July-June,	-0.003	0.001	0.005	-0.006	0.005	0.001
		(0.001)**	(0.003)	(0.003)	(0.003)**	(0.003)*	(0.001)
	N	426,106	426,106	426,106	426,106	426,106	364,597

Note: The table reports various estimates of the impact of the expansion in leave coverage from 18 to 36 months on track choice and grade attendance. Regression discontinuity estimates regress the outcome variable on a linear age trend and a dummy variable equal to 1 if the child was born after the policy reform. The placebo test repeats the analysis for a cohort that was not affected by the expansion in leave coverage. Difference-in-difference estimates use children born in the same birth months, but in a year in which there was no change in leave coverage, as a control group. All regressions control for gender and state. Robust standard errors in parentheses. Robust standard errors in parentheses.

*Statistically significant at 0.10 level, ** at 0.05 level, *** at 0.01 level.

Data Source: Administrative School Data for Bavaria, Hesse, and Schleswig-Holstein for pupils born between July 1990 and June 1992, 2005-06.

Table 5: The Impact of the Expansion in Leave Coverage from 6 to 10 Months on Graduation from Highest Track Choice, Alternative Specifications

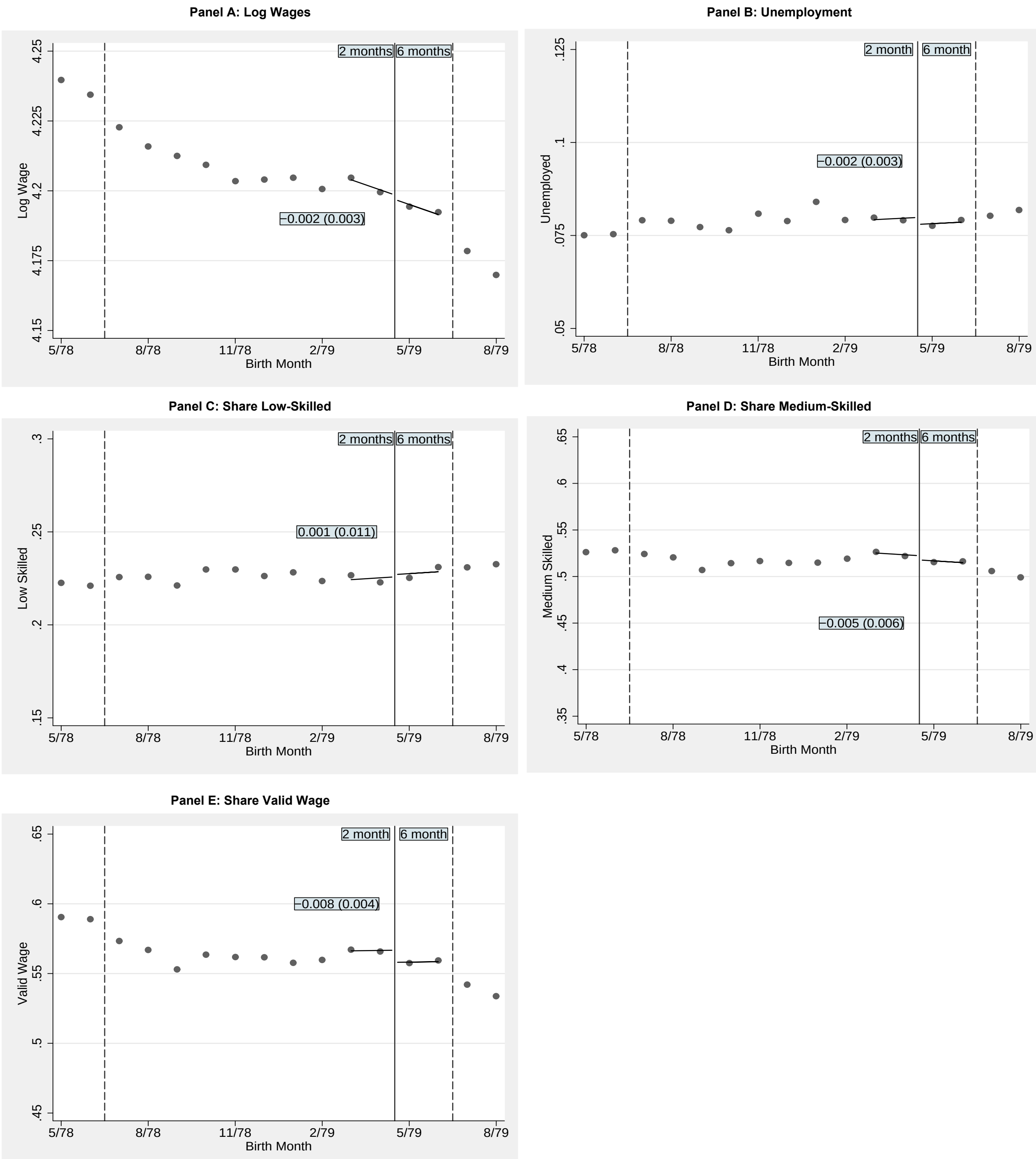
		graduation at age 18-19	*graduation at age 18-20*
		Regression discontinuity	
(1)	October - March, linear age trend	-0.004	-0.004
		(0.005)	(0.010)
		N=6	N=6
(2)	September - April, linear age trend	-0.003	-0.004
		(0.006)	(0.011)
		N=8	N=8
		Placebo test: cohort 1986-87	
(3)	September - April linear age trend	0.001	
		(0.011)	
		N=8	
		Difference-in-difference (control group: 1986-87)	
(4)	November - February	-0.006	
		(0.010)	
		N=8	
(5)	October - March	-0.005	
		(0.006)	
		N=12	
(6)	September - April	-0.005	
		(0.007)	
		N=16	
(7)	Sept.-Nov.; Feb.-April	-0.006	
		(0.006)	
		N=12	

Note: The table reports various estimates of the impact of the expansion in leave coverage from 6 to 10 months on high school graduation (*Abitur*). Regression discontinuity estimates regress the outcome variable on a linear age trend and a dummy variable equal to 1 if the child was born after the policy reform. The placebo test repeats the analysis for the 1886/87 cohort that was not affected by the expansion in leave coverage. Difference-in-difference estimates use children born in the same birth months, but one year after the change in leave coverage as a control group. Regressions are based on data aggregated up to the birth month. They are weighted by the number of observations in each cell. Robust standard errors in parentheses.

*Statistically significant at 0.10 level, ** at 0.05 level, *** at 0.01 level.

Data Source: Administrative school data for Bavaria, Hesse, and Schleswig-Holstein for pupils born between July 1985 and June 1987, school years 2003/2004 to 2005/2006.

Figure 6: The Impact of the Expansion in Leave Coverage on Children's Labor Market and Education Outcomes



Note: The figures plot mean log wages (Panel A), the shares of unemployed individuals (Panel B), the shares of low- and medium skilled individuals (Panel and D), and the shares of individuals with a valid wage (Panel E), for individuals born around the expansion in leave coverage. The figure also plots predicted shares, obtained from a regression that controls for a linear age trend and a dummy variable equal to 1 if the child was born after the policy reform. Results are based on children born 2 months before or 2 months after the reform. The figure in the box is the coefficient on this discontinuous jump. The solid vertical line indicates the expansion in leave coverage from 2 to 6 months; the dashed vertical lines indicate the discontinuity due to school entry.
Source: BLH social security records (October 2004) for men and women born between March 1978 and August 1979.

Table 6: The Impact of the Expansion in Leave Coverage from 2 to 6 Months on Wages, Unemployment, and Education; Alternative Specifications

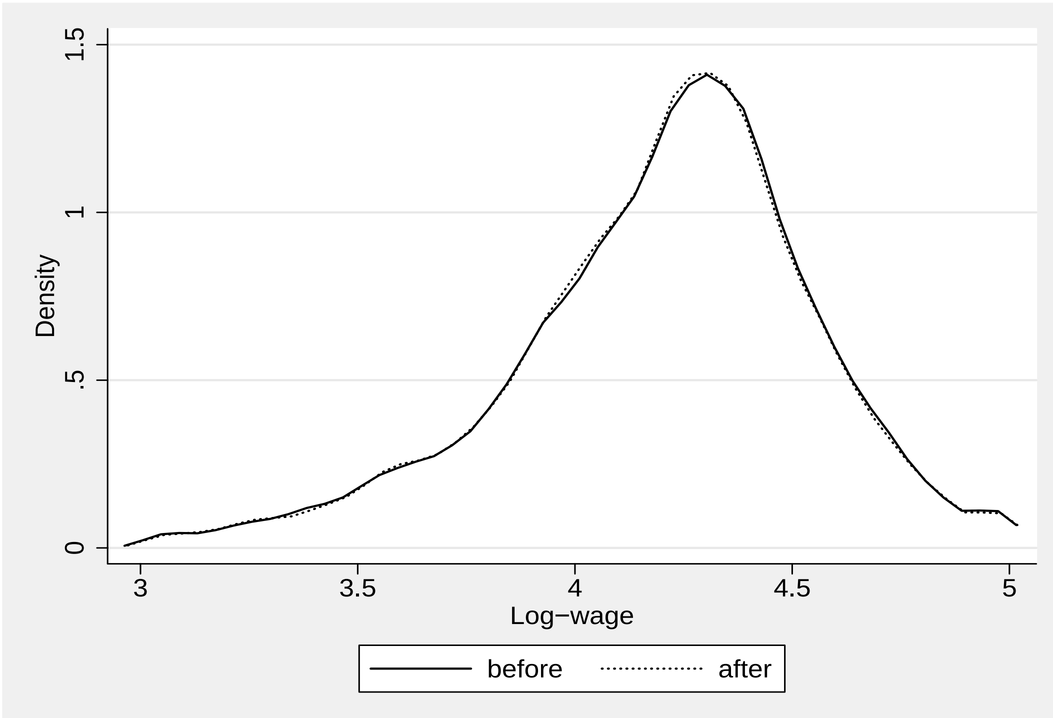
	Wages	*Unemployment*	*Low Ed.*	*Medium Ed.*	*Valid wage*
	Regression discontinuity, treated cohort				
March - June	-0.001	-0.002	0.001	-0.005	-0.009
linear age trend	(0.005)	(0.003)	(0.011)	(0.005)	(0.004)
N	95,714	140,387	4	4	4
January - June,	-0.005	0.000	0.005	-0.013	-0.013
linear age trend	(0.003)	(0.002)	(0.005)	(0.005)*	(0.003)**
N	140,774	206,594	6	6	6
	Regression discontinuity, placebo test (1978)				
January - June,	-0.006	0.000	0.005	-0.009	-0.004
linear age trend	(0.003)*	(0.002)	(0.006)	(0.005)	0.005
N	151,162	213,482	6	6	6
	Regression discontinuity, placebo test (1980)				
January - June,	-0.004	-0.003	0.002	-0.016	-0.008
linear age trend	(0.003)	(0.002)	(0.002)	(0.010)	0.011
N	140,541	215,251	6	6	6
	Difference-in-difference (control group: 1977-78)				
April - May	0.001	0.002			
	(0.004)	(0.003)			
N	99,993	143,086			
March - June	-0.001	0.000	0.003	-0.006	-0.004
	(0.003)	(0.002)	(0.005)	(0.005)	(0.003)
N	198,478	284,433	8	8	8
January - June,	-0.001	0.000	0.003	-0.004	-0.002
	(0.003)	(0.002)	(0.004)	(0.004)	(0.004)
N	291,895	418,499	12	12	12
July-June	-0.001	0.001	0.000	-0.004	-0.003
	(0.002)	(0.002)	(0.003)	(0.002)	0.003
N	578,624	829,425	24	24	24

Note: The table reports various estimates of the impact of the expansion in leave coverage from 2 to 6 months on log-wages, unemployment, education (low and medium), and the shares of workers with a valid wage spell in our data. Regression discontinuity estimates regress the outcome variable on a linear age trend and a dummy variable equal to 1 if the child was born after the policy reform. The placebo test repeats the analysis for a cohort that was not affected by the expansion in leave coverage. Difference-in-difference estimates use children born in the same birth months, but in a year in which there was no change in leave coverage as a control group. Results in the last column are based on data aggregated up to the birth year and birth month, and are weighted by the number of observations in each cell. Robust standard errors in parentheses.

*Statistically significant at 0.10 level, ** at 0.05 level, *** at 0.01 level.

Data Source: BLH Social Security Records (October 2003 and 2004) for men and women born between January and June 1979 (1978 or 1980 for control group).

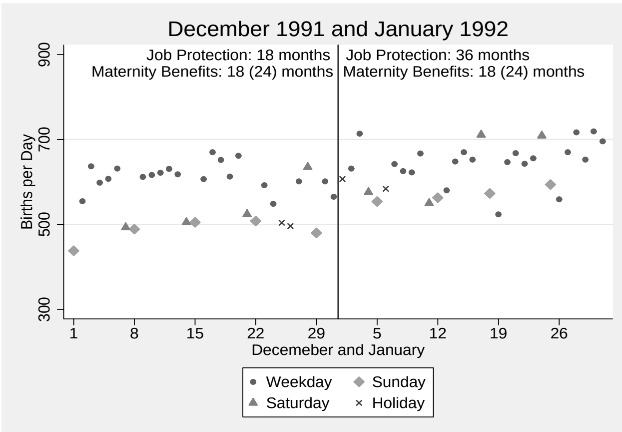
Figure 7: The Wage Density Before and After the Expansion from 2 to 6 Months



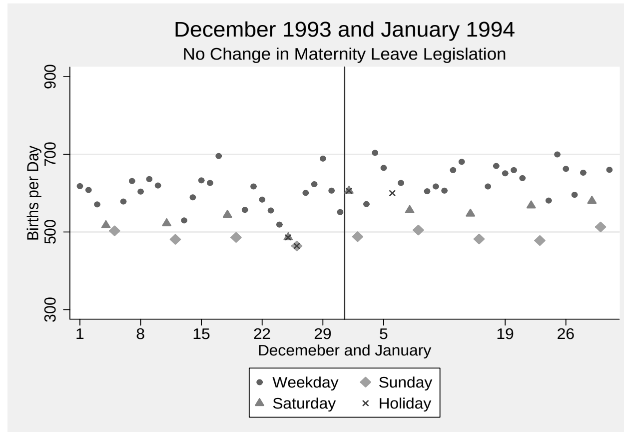
Note: The figure compares the wage density for individuals born in March/April (before) and May/June (after). We eliminate the age effect using individuals born one year before the expansion as a control group.
Source: BLH Social Security Records (October 2004) for men and women born between March and June 1979.

Figure A.1: Did The Expansion in Leave Coverage in 1992 Affect the Timing of Births?

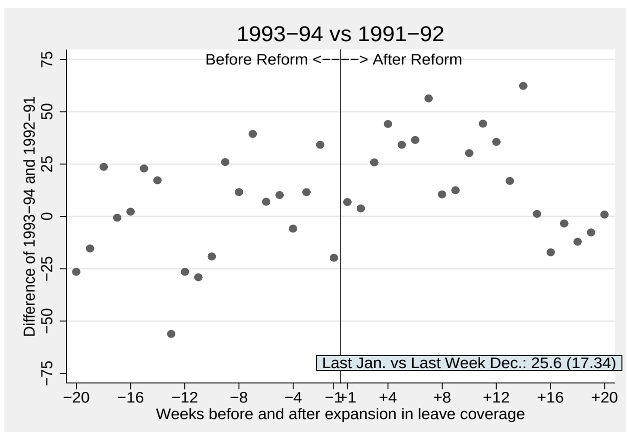
Panel A: Number of Births per Day Sourrounding the 1992 Reform



Panel B: Number of Births per Day Two Years After the Reform



Panel C: Regression-Adjusted Difference in the Weekly Number of Births, August 1993 to May 1994 versus August 1991 to May 1992



Note: Panel A plots the number of life births per day surrounding the 1992 expansion in leave coverage, i.e. December 1991 and January 1992. For comparison, Panel B plots the number of life births per day in December 1993 and January 1994, when there was no change in maternity leave legislation. Panel C plots the regression-adjusted difference between the number of births per week in August 1993 to May 1994 and August 1991 to May 1992. The regression controls for week day, bank holidays, Christmas, and New Year's Day. The difference between the (regression-adjusted) number of births in the first week of January 1992 and the last week of December 1991, relative to the first week of January 1994 and the last week of January 1993, is 25.6, with a standard error of 17.4.
Source: Vital Statistics for Bavaria, Hesse, and Schleswig-Holstein.