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

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Abstract

This paper analyzes the impact of expansions in maternity leave legislation on children's long-term education and labor market outcomes. We identify the causal impact of the reforms by comparing outcomes of children born shortly before and shortly after (i.e. 1 month) a change in maternity leave legislation. Our empirical analysis combines two administrative data sources on wages, unemployment and high school attendance. Overall, 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 leave from 2 to 6 (18 to 36) months raised wages (selective high school attendance) by more than 0.3 % (0.1 %).

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1 Introduction

Over the 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 (BLS report, 2001) 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. 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 US states, such as Massachusetts and New Jersey, are considering implementing similar policies. Other countries that have recently expanded their leave coverage include the UK (2003, 2007) and Denmark (2002). In contrast, countries with extremely generous rules (like Germany) currently consider policies which make earlier re-entry easier for mothers.

An important goal behind the recent expansions in leave coverage around the world was the welfare of the child, 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 breast-feeding, as well as an increase in the quantity and quality of child-related investments and child-parent interactions. There are, however, also arguments for why a delay in the return to work may hurt children. For instance, spending less time with the child may not be detrimental for the child's development if the child attends high quality child care. Moreover, work may increase current as well as future income which may benefit children.

The impact of early maternal employment on child development has received a lot of attention in the media, reflecting the importance of this issue in the public perception. A good example is the recent US study by Brooks-Gunn, Han, and Waldfogel (2002) which linked maternal employment before the child was 9 months old to a lower cognitive ability of the child at age 3[1]. In Germany, the recent proposal by Angela Merkel (Germany's first female chancellor) and Ursula von der Leyen (family minister and mother of 7 children) to substantially increase non-maternal care for children under the age of 3 has stirred an emotional debate on family values and gender roles.

Despite the widespread prevalence of leave policies around the world, there are only few studies that evaluate their impact on child development.[2] The impact of early maternal employment on child development, in contrast, has been widely studied by both psychologists and economists. A major problem of the existing work is to convincingly establish a causal link between early maternal employment and child development. There are only few studies that use experimental designs for analysis. Most studies–and all that use an experimental design–focus on early development outcomes, up to age 8. So far, the literature has not reached an agreement on the quantitative importance of early maternal employment on child development. For instance, Bernal and Keane (2006b) review papers based on the NLSY 79, and report that roughly one third of the academic papers reviewed report positive effects of maternal employment on child development; a third reports negative effects, and the remainder finds insignificant effects or effects that vary depending on the particular empirical specification used.

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 possible nonlinearities in

[1] See e.g. The New York Times, July 17, 2002, "Study Links Working Mothers to Slower Learning"; The New York Times, July 20, 2002, "Help for Working Parents"; The New York Times, July 21, 2002, "A Child Study Is a Peek. It's Not the Whole Picture"; Wall Street Journal, September 5, 2002, "Moving On: Good Mother / Bad Mother – The Debate Over Staying Home With Kids Gets Ugly". For more recent media coverage see for instance: The New York Times Magazine, March 4, 2007, "The Motherhood Experiment"; The Washington Times, May 31, 2006, "Working mothers don't breast-feed enough"; The New York Times, April 2, 2006, "The Time Trap".

[2] We summarize the literature in Section 2.2.

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 - the longest in the world. Schönberg and Ludsteck (2007) show that each reform had a strong short-term impact on a mother's labor supply, and induced women to take more time off from work after childbirth. The impact of the latter expansion may differ from that of the earlier ones for instance because the expansion was unpaid, or because maternal employment in the first months after childbirth is more harmful for child development than maternal employment in the third year after childbirth.

We identify the causal impact of the reform by comparing outcomes of children born shortly (i.e. one months) before and after the reform, and 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. We adopt several strategies to account for the fact that children born before the reform are older on average than children born after the reform.

Our main outcome variables for the 1979 expansion in leave coverage from 2 to 6 months are wages and unemployment. Our data comes from social security records, and covers *every* men and woman born around the policy reform who ever worked for pay. Our main outcome variables for the 1992 expansion in leave coverage from 18 to 36 months are grade progression and track choice. Here, our data covers *all* pupils in three large states (Bavaria, Hesse, and Schleswig-Holstein). Most German states track children into three different types of school (*Hauptschule, Realschule, Gymnasium*) after 4th grade, and research has documented a strong correlation between track choice at age 15 and standardized test scores (e.g. Ammermueller (2004)) and labor market outcomes in young adulthood (e.g. Dustmann (2004)). Our main outcome variables for the 1986 reform is graduation from the highest track choice (*Abitur*). This is an important outcome variable because, contrary to graduation from the other track choices, it provides direct access to university. It is also strongly correlated with earnings in early adulthood.

To preview our results, we find no evidence that any of the policy reforms improved children's outcomes. Given the precision of our estimates, we can statistically rule out the hypothesis that the expansion in leave coverage from 2 to 6 months raised wages by more than 0.3 %, and that the expansion in leave coverage from 18 to 36 months increased selective high school attendance by more than 0.1 %. Hence, any short-term beneficial effects that these reforms may have had do not appear to persist through adolescence and young adulthood. This casts some doubt on whether maternity leave policies are successful at improving children's outcomes.

The remainder of the paper is organized as follows. We first provide the necessary background information for our study, including a detailed description of the major changes in maternity leave legislation in Germany (Section 2). 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 **??** concludes.

2 Background

2.1 Mechanisms: Why Does an 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 affect child outcomes also because they may influence mothers' long-run labor market outcomes or fertility[4].

Why may more time at home with the child after childbirth benefit children? One channel is breast-feeding. The World Health Organization recommends exclusive breast-feeding for 6

[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 4.

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

months and breast-feeding complemented with other foods up to the age of 2. Breast-feeding 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 breast-feed longer than working mothers, and that breast-feeding often stops when the mother returns to work (e.g. Berger et al. 2005, Lindberg, 1996). However, little is known whether the association between breast-feeding and the child's health reflects a causal relationship, or whether any short-term benefits persist into adolescence and young adulthood.

Second, 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. Economists are often agnostic about the exact mechanisms through which the quantity and quality of child/mother interactions affect child development, while they have been extensively studied by psychologists. **attachment theory (how many hours per day are necessary to establish a secure relationship between mother and child?), exposure (from what age?), quality child care**

These arguments highlight that a 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).

Third, unless expansions in leave coverage compensates for the loss through increased maternity benefits, a delay in the return to work leads to loss in (temporary) 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 $1000 will increase test scores by 0.021 (math) or 0.036 (reading) of a standard deviation.[5] This suggest that the impact of an expansion in *unpaid*

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

leave may differ from that of an expansion in *paid* leave.

2.2 Previous Studies and Our Contribution

We are aware of only three papers that 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 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 1 month. Tanaka (2005) further finds that paid leave plays a more important role than unpaid leave.

Baker and Milligan (2006) focus on a specific policy in a single country, the 2000 expansion in leave coverage from 25 to 50 weeks in Canada. They consider a broader set of health and cognitive outcomes of children up to age 3. They find that the expansion had little impact on motor and social skills at the age of 3, although the expansion reduced the age of certain developmental milestones, such as when the child first feeds him/her self or speaks the first word.

While the literature on the direct link between leave coverage and child development is limited, there is a vast literature on the relationship between maternal employment, child care arrangements, and child development. This literature faces two related challenges. The first problem is that of selection: working mothers may differ from stay-at-home mothers in ways that are unobserved by the econometrician. The second problem is reverse causality: the mother's decision when to return to work may be affected by her child's ability. Most papers in the literature attempt to minimize the selection bias by conditioning on a large set of child and household characteristics[6]. A few papers include household or child fixed effect, thus using variation in maternal employment across siblings or over time[7]. We are aware of only two papers that exploit arguably exogenous variation in maternal employment or child care usage. Baker et al. (2005) evaluate the impact of a generous child care subsidy in Quebec on child development, using other Canadian provinces

[6] See for instance Desai et al. (1989), Belsky and Eggebeen (1991), Blau and Grossberg (1992), Vandell and Ramanan (1992), Baum (2003), Ruhm (2004), Berger et al. (2005), and Ruhm (2005).

[7] See for instance James-Burdumy 2005, Blau 1999, Duncan and NICHD 2003, Chase-Lansdale et al. 2003.

as a control group. Bernal and Keane (2006a) exploit the 1996 welfare reform in the US, 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.

The vast majority of papers in the literature, and both papers using an experimental design, focus on children's short-term outcomes up to age 8[8]. We are aware of no paper that analyzes the relationship between maternal employment and children's success in the labor market.

The quantitative evidence on the impact of maternal employment on child development is so far inconclusive. This is well illustrated by Bernal and Keane (2006a) who review studies on the link between maternal employment and child development based on the NSLY 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. There is some evidence that long working hours in the child's first year are particularly harmful[9]. Several studies suggest a substantial heterogeneity, with children at the upper end of the ability distribution being hurt more by their mothers' employment[10].

This paper extends and improves on the existing literature on the impact of maternity leave legislation on child development 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 and thus require substantially weaker assumptions for identification than the existing studies. 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 *long-term* effect 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.

[8] One exception is Ruhm (2005) who focuses on adolescent outcomes.

[9] See for instance Blau and Grossberg (1992), Baum (2003), Berger et al. (2005), Berger et al. (2006).

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

Since the main channel through which maternity leave legislation may affect child development is a delayed return to work, our study also sheds new light on the causal relationship between maternal employment and child development. This paper is one of the very few studies using an experimental design to do so, and the first one that focuses on child outcomes in the long-run.

2.3 Maternity Leave Legislation in Germany

In the US, the Family and Medical Leave Act (FMLA) introduced in 1993 (which was vetoed twice by President George Bush Sr) requires firms with at least 50 employees to provide 12 weeks of *unpaid* leave due to childbirth. Similar coverage has been available to German mothers since 1952. Since 1965, mothers are entitled to paid leave 6 weeks before and 8 weeks after childbirth. During this period firms are 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 last three months prior to childbirth[11].

Starting in the late 70s, 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 2 to 6 months. This reform is comparable 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 (see Gesetzentwurf der Bundesregierung, Drucksache 10/3792. for details).

Payment from 6 weeks before to 8 weeks after childbirth remained unchanged from the mother's income prior to childbirth. Payment from the third month after childbirth onwards was equivalent to sick pay, and up to 375 Euros per month - which corresponds to roughly one third of the mother's average pre-birth salary[12]. Only women who were employed before childbirth were entitled to maternity benefits.

[11] 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 12.50 Euros per calender day, or 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.

[12] It was reduced to 265 Euros per child and month in 1984. Costs between the 3rd and 6th month were borne entirely by the federal government.

The later expansions in leave coverage that took place between the mid-80s and early 90s 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. The reforms were explicitly aimed at encouraging mothers to spend more time with their child after childbirth. The 1986 reform increased the job-protection period from 6 to 10 months and announced a further increase to 12 months starting in January 1988. An important component of this reform was that all mothers -regardless of the employment status before childbirth- became eligible for maternity benefits. A further component of the reform was that fathers became eligible for leave taking. However, the proportion of fathers taking parental leave is very small; in 2001 it was 1.6% (Engstler and Menning (2003)). The German government describes the aim of the reform as follows:

"The introduction of maternity benefits facilitates commitment of one parent to care taking and education in a phase of the child's life that is crucial for later development. More choice will be given to father and mother between family and the job. The policy strengthens the potential of the family to educate the child, and recognizes her contribution to society through child education."
Gesetzentwurf der Bundesregierung, Drucksache 8/2613.

Payment from 6 weeks before and 8 weeks after childbirth remained unchanged from the mother's income prior 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 are equal to 300 Euros, independently of the mother's (and father's) income prior to childbirth. This corresponds to about 20 % of a woman's average pre-birth salary. From the seventh 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 1986, this proportion was 83.6 % (Engstler and Menning (2003), Bundesministerium für Familie, Senioren, Frauen und Jugend (2000)). This reform shares some similarities to the recent reforms in Canada (2000) and Denmark (2002).

In July 1989 and July 1990, job-protected leave period was further raised to 15 and 18 months, respectively. The final policy reform took place in January 1992 when job-protected leave was raised from 18 to 36 months. This reform kept the maximum duration during which women are entitled to maternity benefits at 18 months, but it was announced that this period will be extended to 24 months starting in January 1993. At the same time, 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.

Our empirical analysis focuses on the impact of the three major policy changes in 1979, 1986, and 1992 on child development. Unfortunately, the smaller changes in maternity leave legislation in *July* 1989 and *July* 1990 cannot be used to assess their impact on children's outcomes. This is because in Germany children born in July typically start school a year later than children born in June; we provide more details on the German school system below. This makes it difficult to isolate the effect of age at school entry on educational and labor market outcomes from that of the policy reform.

It is important to stress that our identification strategy exploits the sharp discontinuity in the eligibility for leave coverage -i.e., mothers of children born in December 1991 are entitled to 18 months of job-protected leave, whereas mothers of children born in January 1992 are entitled to 36 months of job-protected leave. This estimation strategy does not allow us to pick up the impact of a (possible) increase in child care attendance on school outcomes.

2.4 The German School System

This section briefly reviews the key features of the German school system. This motivates our choice of children's outcome variables.

In Germany, compulsory schooling starts at the age of 6. More specifically, 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 (so-called *Hamburg Accord*). This implies that children born in July usually start school a year later than children born in June. There are exceptions to this rule, as parents are given a fair amount of choice. This rule implies that children born between July 1997 and June 1998 should start second grade in the fall of 2005. Figure 2, Panel A, plots the share of pupils who are born between July 1996 and June 1999 and attend 2nd grade in 2005, by birth month. The figure is based on administrative data for three German states, Hesse, Bavaria, and

Schleswig-Holstein; we describe this data set in detail in the next section. Panel B, provides a similar analysis for children born between July 1991 and June 1994 and plots the share of children who attend 7th grade in the fall of 2005. As expected, both figures show a clear discontinuity for pupils born in June and July. The figures also illustrate that the rule is not strictly enforced. For instance, 40 % of the pupils born in July 1998 attend 2nd grade in the fall of 2005 although according to the *Hamburg Accord* they should be in first grade only. Notice that the share of pupils attending the 'correct' 7th grade is substantially smaller than the share of pupils attending the 'correct' 2nd grade. This suggests that grade repetition is quite common; in our data, 3.75 % of pupils in grade 1 to 10 repeated a grade in 2005.

In Panel C, we plot the average (approximated) age at school entry for the 1997/98 cohort[13]. The figure shows children born in July are about one third of a year older at school entry than children born in June. Since the cut-off rule is not strictly enforced, children born in August and September are in fact somewhat older at school entry than children born in July; by the same argument, children born in May are about as old at school entry as children born in June.

Unlike in the US where schooling is compulsory up to age 16, in Germany schooling is compulsory up to grade 9. All German states track children into three main types of school after 4th or 6th grade. The least academic track is called *Hauptschule* (grades 5 to 9), the intermediate track is called *Realschule* (grades 5-10), and the most academic track is called *Gymnasium* (grades 5 to 13). In most states, there is a fourth comprehensive school type that (in principle) comprises all three track choices (*Gesamtschule*).[14] Only graduation from the highest track choice provides direct access to university or to a polytechnic education. The other two track choices are designed to prepare students for vocational training within the German apprenticeship system. Table 1 list the share of pupils in each school type in 7th, and 9th grade, for the three states in our data set. The intermediate track choice is the most common, with about x % of pupils attending it in 7th

[13]Our data does not include age at school entry. For the 97/98 cohort, we approximate it as follows. If in the academic school year 2005/06 the child is observed in 1st, 2nd, or 3rd grade, we assume that the child started school in September 2005, 2004, or 2003, respectively. We therefore ignore grade repitition in grade 1 to 3. Less than 0.3 % of children attend grades other than 1, 2 or 3. We ignore these children.

[14]In some states, most notably in Hesse, there s an additional comprehensive school typem In addition to these four main school types, *Foerderschule*. This type of school only exist in grade 5 and 6 and allows pupils to delay the track choice from 4th to 6th grade. There are also schools for children with special needs (*Sonderschule*).

or 9th grade. The table also illustrates that there is considerable downward movement to lower track choices with grade progression: While in 7th grade x % of pupils attended the highest track choice, this share is only x % in 9th grade.

Several studies documented a strong correlation between the type of high school attended and test scores (see for instance Ammermüller (2004)). Figure 3 plots the kernel density of a combined test score (math, German, English), separately by school type; we standardize the test score to have a mean of 0 and standard deviation of 1. The figure summarizes pupils in a Real- and Gesamtschule into one category (intermediate). We do this because the distribution of test scores is very similar for these two school types. The figure is based on all pupils in Hamburg who attended 9th grade in 2000 (LAU); a brief description of this data set can be found in Appendix A.3. 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.

Table 2 provides more details. In the first column, we regress pupils' combined test score on whether the pupil attends the intermediate (*Real-/Gesamtschule*) or high track choice with the lowest track choice (*Hauptschule/Sonderschule)* 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 deviation higher than at the lowest school type. In Column 2, we regress individuals' wages in early adulthood on the type of school attended at age 15, using data from the German Socioeconomic Panel (see also Dustmann, 2004); a brief description of this data set can be found in Appendix A.4. The results show that the type of high school attended as a teenager is a key determinant of wages as young adults. This motivates our use of school choice as our main outcome variable when evaluating the expansion in leave coverage from 18 to 36 months. Finally, in column 3, we regress wages in early adulthood on a dummy variable indicating whether the individual graduated from the highest track choice (*Abitur*), using data from the GSOEP. Only these individuals have direct access to university. Individuals who graduated from high school earn x % higher wages than those who did not. This motivates our use of high school graduation as our main outcome variable when evaluating the expansion in leave coverage from 6 to 10 months.

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, by Ekberg et al. (2005) to analyze the impact of Sweden's 'daddy month reform on the labor supply of fathers, and by Schönberg and Ludsteck (2007) to investigate the impact of the policy reforms in Germany on mothers' labor market outcomes after childbirth.

Our identification strategy assumes that it is random whether a child is born in December or January. One reason for why the 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. We believe that this is unlikely, at least for the three policy reforms we consider[15]. This is because women could not anticipate these reforms. We searched three leading German newspapers[16] 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 were already conceived. One may argue that women still have some possibilities to time the birth of their child through induced births or cesarean cuts[17]. However, this allows women to bring the birth date forward - whereas in our case

[15] The 1988 reform that extended job-protected leave from 10 to 12 months was already decided in 1986. The 1990 reform that extended job-protected leave from 15 to 18 months was already decided in 1989.

[16] The search was conducted for the following newspapers: Süddeutsche Zeitung and Frankfurter Allgemeine.

[17] Dickert-Conlin and Chandra (1999) provide evidence that women time childbirth as a response to financial incentives. They find that the probability that a child is born in the last week of December rather than the first week of January is positively correlated with tax benefits in the US.

women would like to postpone childbirth in order to be eligible for the more generous leave policy. We also would like to point out that throughout the time period of consideration, cesarean cuts were relatively rare and predominantly occurred for medical reasons. Moreover, Schönberg and Ludsteck (2007) show that mothers who give birth shortly before and after the reforms are very similar in terms of observable pre-birth characteristics, such as education, wages, or age.

We are still left with one problem: children born in January are on average one month younger than children born in December. 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, and Fredriksson and Oeckert 2005 for Sweden). Our identification strategy thus has to eliminate 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 children born in July usually start school a year later than children born in June, we define a cohort as all pupils born between July this year and June next year. 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:

$$y_{it} = \beta_{1t} age_{it} + \beta_{2t} Jan_i + u_{it}, \tag{1}$$

where y_{it} denotes pupil i's outcome at time t (e.g. school choice at age 14), age_{it} 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_{2t}, the impact of maternity leave legislation on child outcomes. We estimate regression (1) for different samples. First, we restrict the sample to children born one or two months before or after the policy change. We then include all children born three or four months before or after then policy change. The last two sample restrictions impose stronger assumptions 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:

$$y_{it} = \beta_{1t} Jan_i + \beta_{2t} cohort_i + \beta_{3t} Jan_i \cdot cohort_i + u_{it}, \tag{2}$$

where $cohort_i$ is an indicator variable equal to one of 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_{3g}, the impact of the reform in maternity leave legislation on pupils' outcomes. We first estimate regression (2) using children born in December or January, in a year in which their 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, etc.. 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 many of the foreign pupils were not born in Germany and are thus not affected by the expansions in leave coverage, we restrict the sample to pupils who are German citizens. In Appendix A.3-A.4 we also describe some further data sources that we use for additional evidence.

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. The data for Hesse has been used recently by Puhani and Weber (2005) to analyze the impact of age at school entry on educational outcomes. An important advantage of this data set is its large sample size; each birth months, we observe up to 18500 boys and girls. 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 between July 1984 to June 1987, as well as all German citizens born between July 1990 and June 1993.

For the 1992 policy reform, our main outcome variable is the type of school attended in 7th or 8th grade. We distinguish three school types: low (*Haupt-* and *Sonderschule),* intermediate (*Real-* and *Gesamtschule),* and high *(Gymnasium).* We showed in the previous that school choice in 7th or 8th grade is highly correlated with test scores as well as wages later on. We use two related additional outcomes variables, grade attendance and grade repetition. 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, 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. Here, we combine the information 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. Our analysis here is thus based on data aggregated up to state and birth month level. To get an idea of how reliable this approximation is, we compare for earlier cohorts the number of births in the three states with the number of observations in our data. Results can be found in Figure A.1 in Appendix A.1. While the number of observations in the data always exceeds the number of births, there is no pattern by birth month, suggesting that the approximation provides useful information[18].

[18] Note that there are several reasons for why the number of recorded births may not coincide with the universe of

Table A.1 in Appendix A.1 provides summary statistics of this data set.

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 from 2 to 6 months in 1979. These data share the main advantage of our first data, a large sample size; each birth months, we observe up to 30000 men and women. Moreover, due to its administrative nature, wages and employment are precisely measured, and workers can be followed while in unemployment.

The main 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. Most importantly, some men and women born around 1979 may still be in full-time education in 2004 and have thus 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 disadvantage of this data set is that wages are right-censored at the highest and lowest level at which social security contributions have to be paid. This, however, constitutes only a small problem in our sample as less than 0.5% of the wage observations are right-censored.

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. We only consider 'formal' jobs for which social security contributions have to be paid, and discard so-called marginal jobs, i.e. jobs that last less than 6 weeks or jobs with less than 15 hours per week, from our analysis. We do this because wages earned in marginal jobs are unlikely to reflect the individual's earnings potential; moreover, our wage measure for marginal jobs is left-censored.

Precise variable definitions can be found in Appendix A.2. Table A.2 provides summary statistics of the sample used.

students observed in the administrative data set; the most important one being migration across states. However, as long as an expansion in leave coverage does not affect migration, this will not bias our results.

5 Analysis

5.1 Impact on Labor Market Outcomes of Mothers

Before we analyze the effect of the different reforms on outcomes of children, we first summarize the impact of each policy reform on labor supply and earnings of mothers. More detailed findings as well as a detailed data description can be found in Schönberg and Ludsteck (2007). All results are based on West-German mothers.

We begin with the share of mothers who take maternity leave. Figure 4 plots this share against time; the left y-axis refers to all mothers, while the right y-axis refers to mothers who were employed 9 months prior to childbirth. 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 BHL (a data set which covers *every* woman within the social security system), 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 BLH excludes up to 20 % of the German workforce. Next, we provide an alternative and arguably more reliable estimate of leave taking using data from the German Pension Register. This data set is available only from 1986 onwards. As expected, this data source reveals a higher incidence of leave taking by about 10 percentage points. Figure 4 also shows that leave taking is very common among women who were employed 9 months before childbirth, as about 85 % go on maternity leave.

Table 3 provides an overview of the impact of the expansions in leave coverage on mothers' labor force participation rates and available income after childbirth, by comparing women who give birth shortly before or after a change in maternity leave legislation. In order to account for possible inherent differences between mothers giving birth in different months during the year, we use 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 reveals that each reform induced women to delay their return to work. This effect is strongest for the expansion in job-protected leave from 2 to 6 months in 1979; here, the share of women who are working 2 months after childbirth reduced by 31 percentage points from about 39 % to about 8 %. The effect is weakest for the expansion in job-protected leave from 18 to 36 months in 1992; here, the share of women who are employed 18 months after childbirth declined by about 10 percentage points, from about 37 % to about 27 %. Despite this strong short-term impact on female labor supply, the 1979 and 1986 reform had only a small impact on mothers' participation rates one year after child-birth. The 1992 expansion, in contrast, increased participation rates 3 to 8 years after childbirth by 1 to 2 percentage points.

Since some studies have found that working *long hours* in the first year of the child's life are particularly harmful (e.g. Brooks-Gunn, Han, and Waldfogel 2002, Baum 2003), it is useful to analyze the impact of the expansions in leave coverage on hours worked. Unfortunately, our data only includes information on full-time (≥ 35 hours per week) and part-time (< 35 hours per week) work, but not on the exact number of hours worked. There is some evidence that women who return to work before the expiration of leave coverage are more likely to work part-time. For instance, the share of women working full-time 6 months after childbirth is about 77 % shortly before the expansion in leave coverage from 6 to 10 months, but only 72 % afterwards. Similarly, the share of women working full-time 18 months after childbirth is about 64 % shortly before the expansion in leave coverage from 18 to 36 months, but 60 % afterwards. This is an additional channel through which the expansions lowered early maternal employment. There is little evidence that the expansions increased part-time work in the long-run.

Next, consider the impact of maternity leave legislation on income available to the mother. If the mother is working, this variable is equal to the *daily* wage, averaged over the days a woman was working for the employer during the year. If the mother is not working, this variable is equal to the daily maternity benefit (i.e. 10 Euro after 1986) in case the mother is eligible and zero otherwise. This way we are able to assess whether the increase in the duration during which maternity benefits are paid compensates for the temporary earnings loss due to the delay in the return to work. The figure reveals that this is not the case for the expansion in leave coverage from 2 to 6 months in 1979: The mother's daily income between the 3rd and 6th month after childbirth is lower by about 8.50 Euros after the expansion in leave coverage from 2 to 6 months, amounting to a total loss of about 1000 Euros. Turning to the expansion in leave coverage from 6 to 10 months in 1986, available income 6 months after childbirth is also somewhat lower after the expansion (by about 3.20 DM or 1.60 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. It is therefore not surprising that this expansion lowered available income 18 and 30 months after childbirth. What about the long-term impact on available income? The expansion in leave coverage from 2 to 6 months as well as that from 6 to 10 months had no significant effect on income 1 year after childbirth[19]. The expansion in leave coverage from 18 to 36 months, in contrast, raised income by about 30 Euros per month even 8 years after childbirth, primarily due to the increase in labor force participation.

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 of mothers went on maternity leave in 1979 (Figure 2), 10-15 % of the children are affected by this reform. A similar calculation suggests that the 1986 and 1992 reform induced about 13 % (6 %) of mothers to go back to work after 10 rather than 6 months, or 18 rather than 36 months, respectively. The expansions additionally lowered early maternal employment through the increase in part-time work of women who returned to the labor market early prior to the expiration of leave. The rise in maternity benefits associated with the 1979 reform and (to a lesser extent) the 1986 reform did not fully compensate mothers for the temporary income loss. The 1992 reform increased the period of job-protection from 18 to 36 months, but kept the duration during which maternity benefits are paid at 18 months. A back of the envelope calculation suggest that this resulted in an overall average income loss of about 19000 Euros for those affected by the reform.[20]

[19] Schoenberg and Lusdteck (2007) find that the expansion in leave coverage from 2 to 6 months lowered log-wages of *employed* mother even 5 to 8 years after childbirth. Our results here suggest that if we take into account the impact of the expansion on labor force participation rates, the reform did not have a large impact on available income.

[20] This number is computed as follows. The average daily wage between the 18th adn 36th month after childbirth in 1991 for women who returned to work18 months after childbirth is 35 Euros - amounting to an income loss of 1about 19000 Euros (18·30·35).

5.2 Imapct on Long-Run Outcomes of Children

Since outcome variables vary by policy reform, we report our results separately by policy reform. We begin with the latest expansion in leave coverage, the 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 (Panel C - Panel 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). We consider track choice as our most important out come variable as it is strongly correlated not only with test scores, but also with earnings later in life (see Table 2). The vertical line indicates the expansion in leave coverage. The figure also plots the predicted values, obtained from a linear regression that uses data aggregated up to the birth month and 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=6). We also report the coefficient on the discontinuous jump. Results refer to 13-14 year old.

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 that there are many exceptions to this rule (see also Figure 2). In particular, there is 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 in June 1992. There is little evidence that the expansion in leave coverage had an impact on this share. It is true that pupils born one month before the expansion in leave coverage (i.e. in December 1991) are 3 percentage points more likely to attend 8th grade or higher than pupils born one month after the expansion in leave coverage (i.e. January 1992). However, the figure illustrates that this is predominantly due to the age effect, and not due to the expansion in leave coverage; the regression discontinuity approach based on aggregated data gives a point estimate of the expansion is - 0.004. Turning to grade repetition, between 3 and 4 % of pupils have repeated the grade in the last year. There is again little evidence that the expansion in leave coverage from 18 to 36 months affected the share of pupils.

Panel C to E plot the share of students attending the lowest, intermediate, and highest track choice at age 13/14. The figure reveals once more a strong age effect: Children born in May are 8 percentage points less likely to attend the highest track choice than children born in September.[21] Panel E also suggests that the expansion in leave coverage *lowered* the share of pupils attending the highest track choice; in the linear regression based on monthly data the coefficient on the discontinuous jump is -0.008 and statistically significant at a 10 % level. For the lowest and intermediate track choice, the coefficients are positive, but not statistically significant.

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 employ the regression discontinuity approach, and control for the age effect in a linear way (rows (1) to (3)). First, we use all pupils born two months before or after the change in legislation, i.e. all 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' using all pupils born between September 1990 and April 1991, and were thus not affected by the expansion in leave coverage (row (4)). Finally, we report difference-in-difference estimates, including children born 1 to 6 months before or after the expansion in leave coverage. Children born in the same birth month, but in one year before the expansion in leave coverage, serve as the control group (rows (5) to (9)). We do not use the cohort born one year after the expansion as a control group because in January 1993 the duration during which maternity benefits are paid was increased from 18 to 24 months. This may have an independent impact on pupils' outcomes.

Consider first track choice. 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

[21] Children born in August and September are in fact 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. In light of the findings in Figure 5 (in particular Panel C), this is not surprising: Children born around the cut-off age often enter school a year early or a year late. Children born in July and August are therefore somewhat younger at school entry than children born in September, while children born in June are about as old at school entry as children born in May.

lowered the share of pupils attending the highest track choice 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. Yet, given the precision of a typical estimate (e.g. -0.007 (0.005) DinD, November to March), we are able to rule out the hypothesis that the expansion increased attendance of the highest track choice by more than 0.1 percentage points. The point estimates for the lowest and intermediate track choice are positive, smaller in magnitude, and not statistically significant.

The 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 is statistically significant.

We have also run separate regressions for each state. While the impact of the expansion in leave coverage on child outcomes is very similar across states, the impact is because of the smaller sample size less precisely estimated. Results are available from the authors on request.

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

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 choice. This is an important outcome variable since – contrary to graduation from the other track choices – it provides direct access to university. It is also strongly correlated with earnings in early adulthood (see Table 2). Figure 6 plots the share of pupils born between May 1985 and August 1986 graduating from the highest track choice. The solid vertical line indicates the expansion in leave coverage and the dashed vertical lines indicate the discontinuity due to school entry. We measure graduation in two ways. Panel A refers to graduation in the academic year 2004/05, or at age 18/19. For students born between July 1985 and June 1986, this is the age they should graduate if they start school as implied by the *Hamburg Accord* and never repeat a grade. In contrast, students born in May or June 1985 graduated 'late' (they either started school late or repeated a grade), while students born in July or August

1986 graduated 'early' (they either started school early or skipped a grade). Panel B refers to graduation in the academic years 2004/05 or 2005/06 – or at age 18/19 or age 19/20 – and thus includes students who started school a year late or repeated a grade once. The figure also plots the predicted values, obtained from a linear regression that uses data aggregated up to the birth month and 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=6). The coefficient on the discontinuous jump is also reported.

The figure lends little support for the hypothesis that the expansion in leave coverage from 6 to 10 months increased graduation from the highest track choice; according to both measures, the coefficient on the discontinuous jump is -0.004, with a standard error of 0.007 and 0.008, respectively.

We would like to stress that we do not fail to detect a significant impact of leave coverage on graduation because our data is particularly noisy. Figure 4 reveals a strong age pattern: Children born in September are 4 percentage points more likely to graduate from the highest track school by age 18/19, and 2.5 percentage points by age 19/20, than children born in April. While this age effect is still considerable, it is substantially lower than at age 14 when children born in September are 8 percentage points more likely to attend the highest track choice than children born in April (see Figure 5).[22]

Table 5 reports various estimates for the impact of the expansions on high school graduation. The table has a similar structure as Table 4; we begin with regression discontinuity estimates (rows (1) and (2)); we then report the 'placebo test' using children born between September and April one year before or after the expansion. We finally display difference-in-difference estimates where we use students born in the same birth month, but in a year before or after the expansion in leave coverage as a control group. Note that because we only have two years of data, we cannot compute graduation at age 18/19 or age 19/20 for the 1986/87 cohort; similarly, for the 1984/85 cohort our

[22] It may seem surprising that children born in July and August 1985 are less likely to graduate at age 18/19 (or 19/20) than children born in September. This is because students born in July or August are more likely to enter school early than students born in September therefore graduated in the previous academic year. Panel A shows that the share of pupils born in July and August 1986 and graduating early is 7 and 4 percentage points, respectively.

data allows us to only compute graduation *at* 19/20, as opposed to *by* age 19/20 – see Appendix A.1 for more details. We therefore use as a control group the 1986/87 cohort when graduation at age 18/19 is our outcome variable, and the 1984/85 cohort when graduation at 18/19 or 19/20 is our outcome variable. Our results that use the 1984/85 cohort as a control group have to be interpreted with some caution, as graduation is defined differently for the treated and the control cohort. This cohort will serve as a valid control group only if the age effect is the same for the two definitions.

All estimation methods give similar results: The coefficient on the expansion from 6 to 10 months on graduation from the highest track choice is negative, and ranges from -0.002 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 0.48 percentage points.

We have repeated the analysis using data only from the state of Hesse for which we have data since 2003 and can therefore proxy graduation for the 1984/85 cohort more precisely. Our results are similar, but estimated with less precision. Results are available from the authors on request.

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

The Impact on Wages, Education, and Unemployment We begin with a graphical analysis. Figure 7, Panel A, plots the average log-wage for individuals born between May 1978 and August 1979 at age 25/26 (i.e. October 2004). 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 births*.[23] Panel C and D focus on the share of workers with low and medium levels of education. This share by 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.[24] 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, although effect is statistically insignificant. 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). Panel 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 1 percentage point. The latter effect is statistically significant at a 10 percentage level.

These estimates are based on the regression discontinuity approach, and hinge on the assumption that the age effect is linear. Next, we argue that this assumption is likely to be violated, and the estimates just presented are unlikely to reflect the causal impact of the policy reform. Figure 7, Panel A, provides some first evidence that the assumption of a linear age effect is problematic. While the share of pupils attending 8th grade or higher declines roughly linearly with age up until April, it sharply drops sharply from May to June.

Table 6 explores the violation of the linearity assumption in more detail, by comparing results from the regression discontinuity approach with those from the difference-in-difference approach. Results on wages (Column 1) are based on individual data, while results on unemployment and education (Column 2 to 4) refer to data aggregated up to the birth month. Consider first wages. The regression discontinuity approach gives a small negative impact estimate (-0.002) when chil-

[23] Our results are very similar if we use individual data instead and compute the share in unemployment as the number of individuals in registered unemployment in our data divided by the number of all observations in our data.

[24] Again, our results are very similar if we use individual data instead and compute the share as the number of individuals with low and medium levels of education in our data divided by the number of all observations in our data.

dren 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 result in estimates of similar magnitude, confirming that the assumption of a linear age effect is indeed not appropriate. The difference-in-difference estimates all close to zero and statistically insignificant. Given the precision of a typical estimate (e.g. -0.001 (0.002), January-June, DinD, CG 77/78), we are able to rule out 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 low-skilled (Column 3), and in particular for the share of the medium-skilled (Column 4). The regression discontinuity estimates suggest that the expansion may have lowered the share of the medium-skilled by as much as 1.4 percentage points. However, we find negative effects of similar magnitude also for our placebo cohorts. Difference-in-difference estimates are also negative, but small in magnitude. Given the precision of the estimate we can rule out the hypothesis at a 5 percent level that the expansion in leave coverage from 2 to 6 months lowered the share of the low-skilled by more than 0.5 percentage points, and increased the share of the medium-skilled by more than 0.4 percentage points. 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. We would like to point out again that we do not fail detect a significant impact of the policy reform on labor market outcomes because our data are too noisy. In particular, Figure 7, Panel A, reveals a strong age effect. Wages of individuals born in July 1978 are about 4 percent higher than wages of individuals born in June 1979. Our data are also powerful enough to reveal a clear discontinuity at the cut-off date for school entry: Individuals born in June 1978 or 1979 (and were thus among the youngest at school entry) earn about 2 % higher wages than individuals born in July 1978 or 1979 (and were thus among the oldest at school entry). This is despite the fact that older children have better school outcomes in 8th grade (see Figure 5, in particular Panel C to E). Of course, children who are older at school entry enter the labor market later. At age 25/26, the disadvantage due to lower work experience appears to dominate any advantage due to later school entry that may persist into young adulthood.

Selection into Work Our outcomes in the previous section refer to individuals at age 25/26. At that age, some workers may still be in full-time education and have thus 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: Did the expansion in leave coverage have a positive impact on children's labor market outcomes, but do we fail to detect this because the expansion changed the selection into work? More specifically, suppose that the reform increased college enrollment, and that the individuals who enter college due to the reform are positively selected compared to those who do not attend college, and negatively selected compared to those who do attend college. This implies that the best workers have not entered yet the labor market after the reform, compared to before the reform. We may therefore find that the expansion had no impact on average wages (conditional on working), even if in fact it raised 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 7, Panel E, reveals that this ratio is about 2 percentage points lower for individuals born in July than for individuals born in June (and thus entered school later). The figure also shows a rather sharp discontinuity when leave coverage is expanded from 2 to 6 months; the coefficient on the discontinuous jump is -0.013 with a standard error of 0.003. However, our findings in the previous section indicate that the identifying assumption behind this approach–a linear age effect–is likely to be violated. Table 6, Column 5, compares estimates obtained from the regression discontinuity approach with those from the difference-in-difference approach. The regression discontinuity approach gives a negative estimate (-0.009) when March to June births are used, and an even more negative, marginally significant estimate when births January to June are used (-0.013). However, when repeating the analysis using individuals born in the same birth month, but in the year before or after the expansion in leave coverage, we get negative estimates of similar magnitude – suggesting that the assumption of the linear age trend is indeed inappropriate. The difference-in-difference estimate give estimates very close to zero, 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. Suppose that the hypothesis that the expansion in leave coverage increased wages, but we fail to detect this because the best workers have not entered the labor market yet, is true. In this case, we would expect the wage density to be shifted to the right after the reform at the lower tail, but not necessarily at the upper tail. Moreover, upper tail inequality should be lower after the reform, and the impact of the expansion in leave coverage on wages, conditional on working, should decline as we move along the wage distribution.

Figure 9, Panel A, compares the density of log-wages for individuals born in March or April with that of individuals born in May or June. In order to take into account of the fact that individuals born after the reform are 2 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). Panel B and C in Figure 10 plot the wage density before and after the reform for workers with low and medium levels of education. The density is very similar before and after the reform also conditional on education, and again we do not reject equality of the two distributions (p-values: 0.652 (low) and 0.587 (medium)).

As a further check whether the impact of the expansion in leave coverage differs along the wage distribution, we estimated quantile regressions. The coefficients on the expansion in leave coverage are close to zero and statistically insignificant for all quantiles. There is thus little evidence that the expansion in leave coverage affected the distribution of log-wages, conditional on labor market entry. 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.

5.2.4 A Note on the Heterogeneity of the Impact of Leave Coverage on Child Outcomes

For each expansion in leave coverage and each outcome variable, we analyzed whether the impact of the expansion on child outcomes varies by gender. This is not the case, and results are therefore not reported. Unfortunately, our data does not permit us to investigate the heterogeneity of the impact of leave coverage in more detail. The next section summarizes our results and offers a possible interpretation of our findings.

6 Discussion and Interpretation

Recently, several countries have expanded leave coverage after childbirth. An important goal behind these reforms was to improve the child'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 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 expansion in leave coverage from 6 to 10 months increased graduation from a selective high school (*Abitur*); here, our point estimates range from -0.003 to -0.005, and we are able to rule out the hypothesis that the expansion raised graduation by more than 0.5 percentage points at a 5 % significance level for most specifications. 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 are able to reject the hypothesis that the expansion raised selective high school attendance by more than 0.1 percentage points at a 5 % level for most specifications.

What do our results imply for the impact of early maternal employment on child development?

Consider first the expansion in leave coverage from 18 to 36 months. Here, our point estimates suggest that the expansion lowered selective high school attendance by about 0.6 percentage points. The *primary* effect of this expansion was the delay in the return to work; our calculations suggest that about 6 % of mothers returned to work after 36 months instead of after 18 months due to the reform. Since the reform only extended job-protected leave, but not the maternity benefit duration, the reform resulted in an overall average income loss of about 4500 Euros, or 20,000 Euros for those affected by the reform (see Section **??**). Ignoring secondary effects of the expansion (such as the increase in part-time work of women who returned to work prior to the expiration of job-protected leave, or the slight long-run increase in labor force participation), 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 ten ($0.06 \cdot 100/6$) percentage points. This is a large effect, and we would like to stress that it has to be interpreted with considerable caution, since the reduced form estimates are–if at all–only marginally statistically significant.

To put this effect in perspective, an additional month in age at school entry increases selective high school attendance by almost 1.5 percentage points (Figure 7, Panel E). In order to better be able to compare this estimate to those in the existing literature, we next transform the changes in track choice into changes in test scores using the estimates from Table 2. This approach assumes that the expansion in leave coverage affected the test score distribution only through track choice. We find that the expansion lowered test scores by about 0.01 of a standard deviation[25], suggesting that returning to work 36 months after childbirth instead of 18 months after childbirth lowers test scores by about 0.17 of a standard deviation - an effect that is similar in magnitude to that of an increase in class size of 8 students (Krueger 1999).

Since this estimate is identified through those women who changed their behavior as a response to the expansion, it is best interpreted as a local average treatment effect (LATE). Which women were affected by the expansion? Schönberg and Ludsteck (2007) find that among women who took maternity leave, women affected by the expansion from 18 to 36 months are slightly negatively selected in terms of education and pre-birth wages. While the estimate of 0.17 of a standard

[25] This number is computed as -1.039*0.002+0.663*0.004-1.979*0.006; see Table 2.

deviation is large for a delay in the return to work from 18 to 36 months, they are not outside those reported in the literature. Several studies conclude that working long hours in the first year of the child's life is particularly harmful, while employment in the child's second year may in fact increase academic performance. For instance, the IV estimate reported by Blau and Grossberg (1992) implies that working 0 weeks instead of 52 weeks in the child's second year reduces test scores of 3- and 4 year-olds by 0.19 of a standard deviation. However, the IV estimate is imprecisely estimated and larger than OLS estimates[26]. Also note that some studies suggest that children from a disadvantaged background benefit from early maternal employment and prekindergarten attendance[27]. However, while women who were affected by the reform are somewhat negatively selected, it seems a stretch to label these women as disadvantaged. Finally, it is important to stress that the 1992 reform increased only the job-protection period, but not the maternity benefit period, resulting in a total income loss of about 19000 Euros on average for those affected by the reform. While the effect of income on child outcomes is still in dispute, recent research by Dahl and Lochner (2006) found a considerable impact of income on the child's academic achievement: an additional $1000 increases test scores by 0.021 (math) or 0.036 (reading) of a standard deviation.

Next, consider the expansions in leave coverage from 2 to 6 months. The primary effect of these reforms that about 10-15 % returned to work 6 months, as opposed to 2 months, after childbirth. Ignoring any secondary effects, a Wald estimate for the impact of delaying the return to work from 2 to 6 month can be obtained by multiplying the reduced form estimates by about 8. Since none of the reduced form estimates obtaines statistical significance, and p-values are often larger than 0.5, we do not consider these estimates to be economically meaningful, and chose not to report them. The same is true for the expansion in leave coverage from 6 to 10 months.

Schönberg and Ludsteck find that among women who took maternity leave, women who were affected by the expansion in leave coverage from 2 to 6 months are somewhat positively selected in terms of education and pre-birth wage – i.e. they are from a group for which some studies have

[26] Gregg et al. (2005) find that working every week between the 18th and 34th month, as opposed to not working at all, rises test scores at age 7 by about 0.06 of a standard deviation. However, this effect is not statistically significant.

[27] See e.g.Bernal and Keane (2006a), Magnuson, Ruhm, and Waldfogel (2005), Gregg et al. (2005).

found the impact of early maternal employment to be the most detrimental[28]. Hence, exploiting truly exogenous variation in early maternal employment, we are unable to confirm a harmful impact of early maternal employment on the child's long-term education and labor market outcomes. Our results therefore suggest that the negative association between early maternal employment and the child's cognitive ability at 3 to 7 found in many studies either do not reflect a causal relationship, or have dissipated by the time the child enters the labor market. 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 by one into an effect on wages. Assume further that the impact of employment is linear during the first year of the child's life. We would then expect 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 -0.003, with a standard error of 0.007. Put differently, the standard deviation of log-wages is about 0.34; we should therefore observe that the expansion increases log-wages by about 3.4 %. Given the precision of our estimates, we are able to rule out such large detrimental effects on wages at a 5 percent level.

It is also worth pointing out that the expansions in leave coverage are unlikely to have affected public child care attendance. While we have no direct information on the impact of the 1979 and 1986 expansion in leave coverage, data from the German Microcensus shows that in the 90s public child care attendance of children below the age of 2 is very small, below 2 %. Turning to the expansion in leave coverage from 18 to 36 months, the share of children in day care at the age of two was 5.2 % for children born in 1991 (i.e. before the reform), and 5.5 % for children born in 1992 (i.e. after the reform). Hence, the expansions are unlikely to have affected children's education and labor market outcomes through increased enrollment in pre-kindergarten[29].

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

[29] The existing research on the impact of child care on child welfare is so far inconclusive. Baker, Gruber, and

7 Conclusion

This paper evaluates 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 (i.e. one months) before and after the reform, and therefore rely on weaker assumptions for identification than existing studies. Despite our large sample sizes, we find no support for the hypothesis that the expansions in *paid* leave coverage from 2 to 6 months or 6 to 10 months improved children's wages, or graduation from a selective high school. There is some tentative evidence that the expansion in *unpaid* leave from 18 to 36 months *lowered* attendance at schools that provide direct access to university, although this effect is not statistically significant for all specifications. It is possible that we find this negative effect because the expansion was unpaid, and resulted in a temporary income loss of about 20000 Euros for those affected by the reform. A complementary explanation is that children older than 18 months benefit from exposure to care givers other than their mother.

Which policy implications can be drawn from our findings? Most importantly, our results cast doubt on whether the expansions in leave coverage recently implemented in several countries will be successful at improving children's long-term education and labor market outcomes. However, the distinction between *paid* and *unpaid* leave may be important here, as there is some tentative evidence that the expansion in leave coverage lowered selective high school attendance. Ruhm (2004) and in particular Tanaka (2006) also report that paid leave reduces infant mortality rates, while other leave has no significant effect.

Our findings also have implications for the design of social assistance and welfare programs. The primary goal of the welfare reforms implemented between 1984 and 1996 in the US was to promote employment among welfare recipients in general and among single mothers in particular. This was motivated by the belief that work is the best way out of poverty and thus a good way to

Milligan (2006) find that the introduction of universal, highly-subsidzed 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. In contrast, Magnuson, Ruhm, and Waldfogel (2005) report that pre-kindergarten is associated with higher reading and mathematics skills at school entry, but also with more behavioral problems. 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 acadamic performance.

help children. However, if maternal employment negatively affects the child's school performance, the reforms may have imposed long-term costs on the society. Our results provide little support for this concern. In line with our findings, recent research by Miller and Zhang (2006) shows that the welfare reforms improved math scores especially of 4th graders who were exposed to the welfare reform at an early age[30].

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A Appendix A: Data Description

A.1 A.1 Administrative Data on School Choice

For the expansion in leave coverage from 18 to 36 months in 1992, our main outcome variable is track choice at age 12/13 and 13/14. We distinguish three track choices. We summarize *Haupt–* and *Sonderschule* into one category ('low'); we also include *Real-* and *Gesamtschule* into one category ('intermediate'). the third track choice is *Gymnasium* ('high'). The small share of students who attend another type of school are included in the analysis.

We consider two additional outcome variables, grade attendance and grade repetition. The variable grade attendance is equal to 1 if the pupil attends 7th grade or higher in 2004, or 8th grade or higher in 2005, and zero otherwise. The variable grade repetition is equal to 1 if the grade attended in the previous school year is equal to the current grade. Information on the previous grade is not available for Schleswig-Holstein; hence our analysis here is based only Bavaria and Hesse. Table A.1 provides an overview of the variables used in the empirical analysis.

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 school year 2004/05, divided by the number of births to German parents, by birth month; these are pupils who started school as implied by the Hamburger Abkommen 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, by birth month. This variable inlcudes 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 -unless they entered school early- only in 13th grade in the latest year the data is available. Similarly, high school graduation cannot be computed accordingly for earlier cohorts since some pupils have graduated already by the first year the data is available. In Table 5, we nevertheless report for robustness results for high school graduation by age 20, using the 1984/85 cohort as a control group. For this cohort, this variable refers to high school graduation *at* (as opposed to by) age 20 and excludes pupils who graduate earlier; the cohort will serve as a valid control goup as long as the age effect is the same for the two cohorts and the two variables. For Schleswig-Hostein, data on the number of births is not available separately for German and foreign parents. Hence, our analysis here is based on Hesse and Bavaria only.

The reliability of this approximation depends on whether our data really covers the universe of pupils in the three states. This can be checked for earlier cohorts. Figure A.1 plots the ratio of the number of observations in our data and the number of births, for pupils born between July 1990 and June 1993. Results refer to German citizens. For each birth month, the number of observations in our data always exceed the number of births. This may be because of net migration into the three states, or because of a different definition of German citizenship in the two data sources. Importantly, there is no clearly visible pattern by birth month, suggesting that our approximation of high school graduation provides useful information.

A.2 A.2 Administrative Data on Employment Outcomes

We restrict the analysis to German citizens, and delete all workers with at least one spell as a foreigner from our sample. We also discard all workers with at least one spell in East Germany.

Our wage analysis considers only formal jobs for which social security contributions have to be paid. Marginal jobs, apprenticeship spells as well as observations with a wage less then 20 Euro per day are discarded.

Our unemployment variable is based on registered unemployment. Here, our analysis is based on two samples. Our first sample 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. As a robustness check, we compute the share of unemployed workers as the ratio of the number of unemployed in our data and the number of births, by birth month. Here, our analysis is based on data aggregated up to the birth month.

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 or graduated from high school, are considered as low-skilled. The low-skilled include workers with unknown education. We impute the education for unemployed workers using the future or previous wage spell. We then proxy the share of the low- and medium-skilled as the ratio between the number of low- and medium-skilled workers in our data divided by the number of births, by birth month. Here, we count the unemployed, but do not count workers on marginal jobs. We do this because the majority of workers on marginal jobs are likely to be college students.

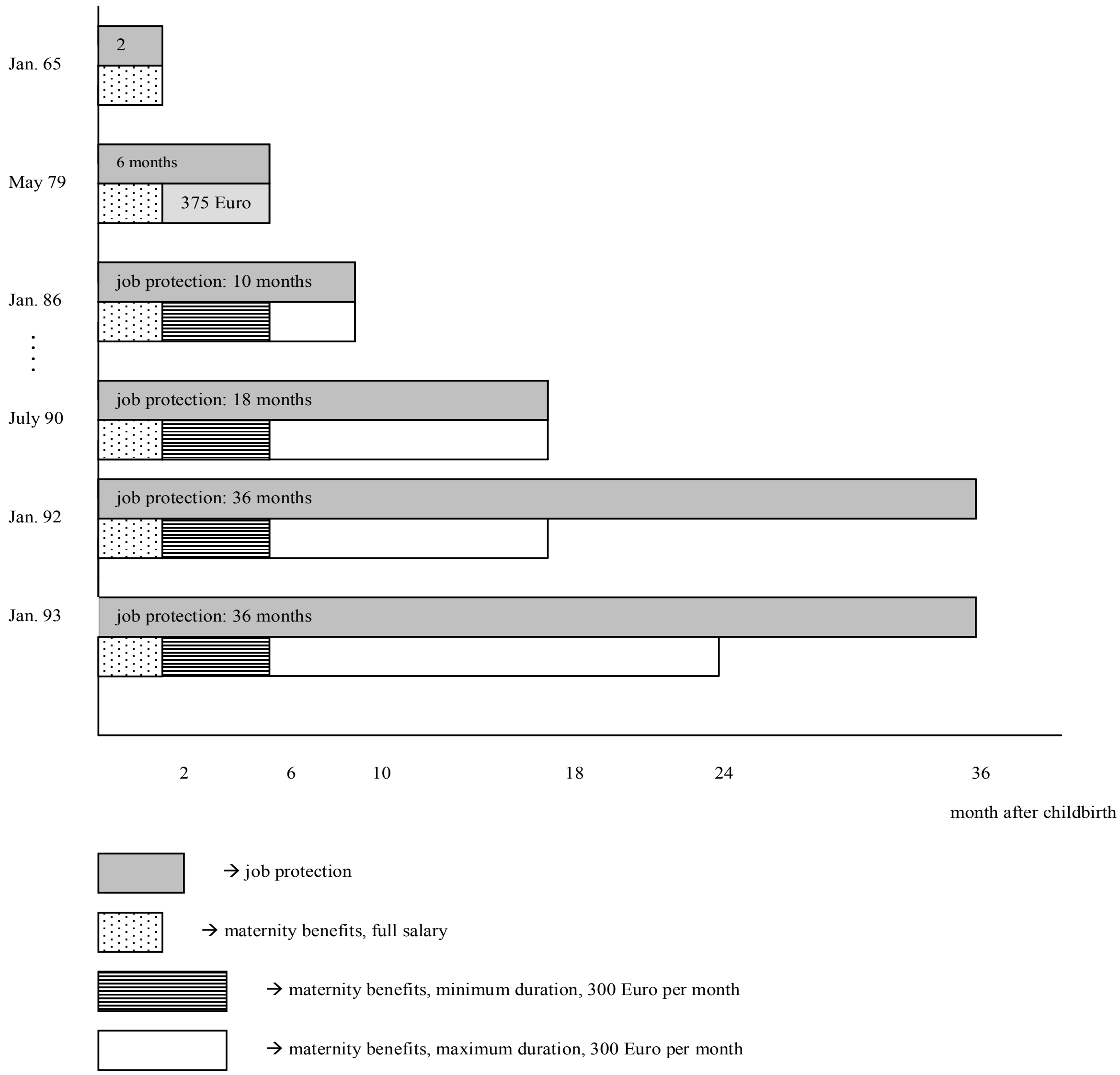
Table A.2 provides an overview of the variables used in the empirical analysis.

A.3 A.3 LAU

LAU (Aspekte der Learnauslage und Lernentwicklung von Hamburger Schuelerinnen und Schuelern) is a survey data set covering all children in the city of Hamburg who attended 5th grade in 1996. Two and four years later, kids in 7th adn 9th grade are surveyed so that pupils can be followed if they do not repeat a grade. The data contains detailed information on children's school enrollment (e.g. type of school, grade repetition) and school performance (e.g. reading, writing and mathematics skills). In Figure 6 and Table 1, we select all pupils in 7th grade with German parents and non-missing combined test score.

A.4 A.4 GSOEP

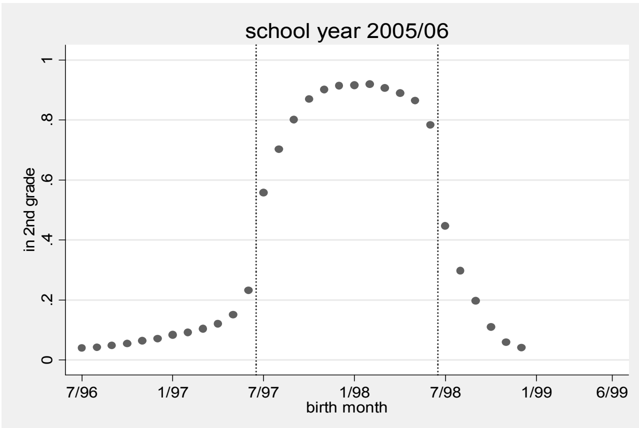
Figure 1: Maternity Leave Legislation in Germany (Selected Reforms)



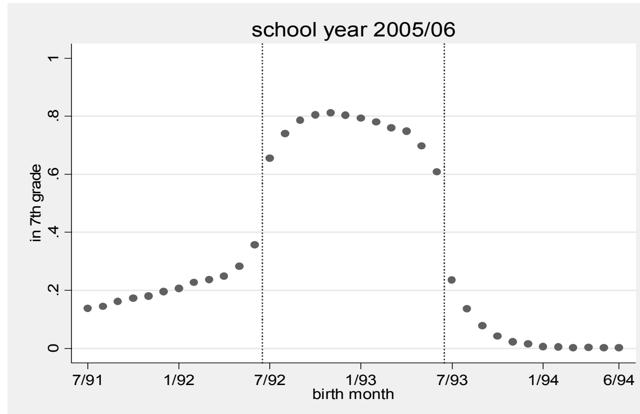
Note: Since 1986, all women –employed before childbirth or not- are entitled to a maternity benefit of 300 Euro per month for a minimum of 6 months. From the 7th month onwards, maternity benefits are means-tested, and depend on the annual net family income two years before childbirth. The majority of 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: Grade Attendance by Month of Birth

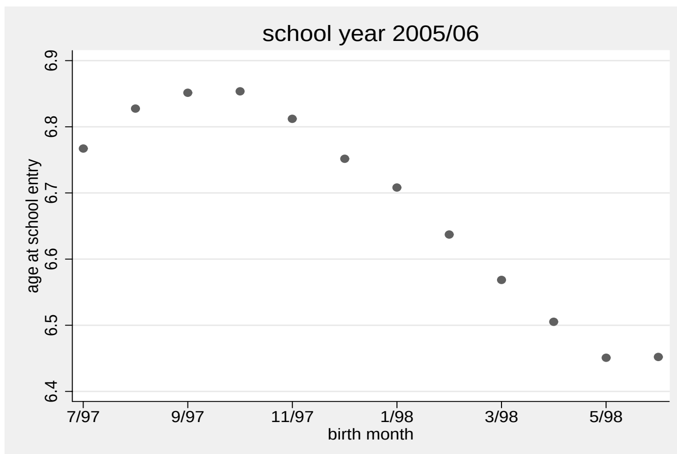
Panel A: 2nd grade



Panel B: 7th grade



Panel C: Average Age at School Entry



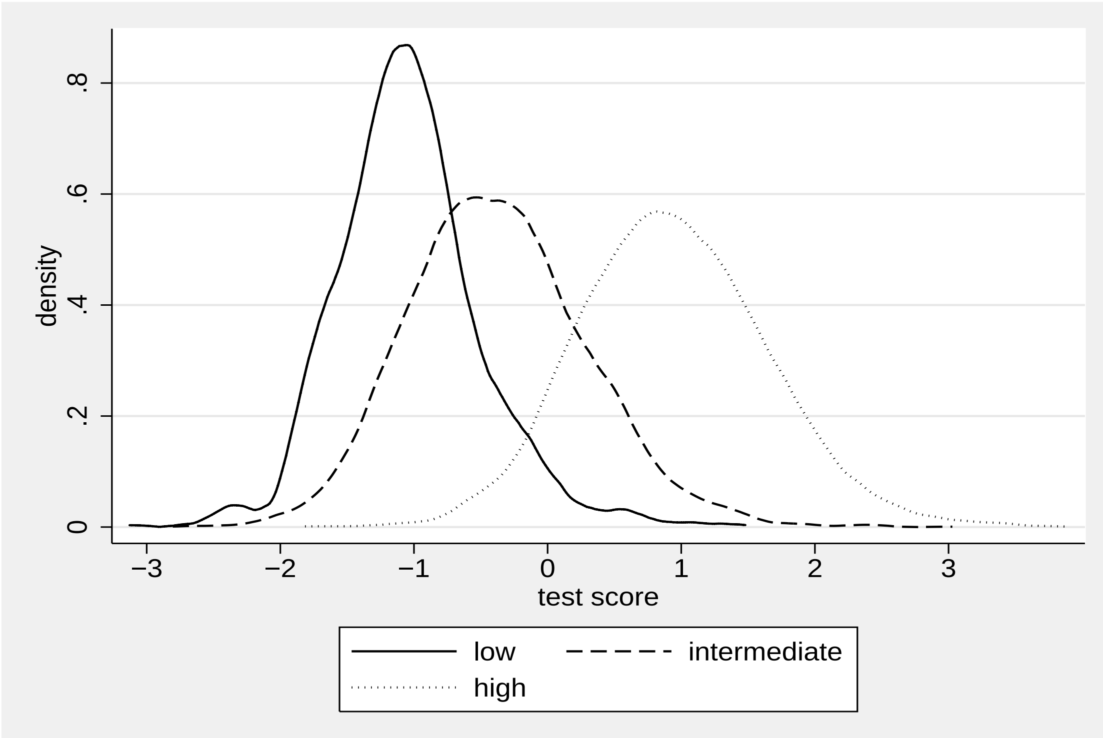
Note: Panel A plots the share of pupils born between July 1996 and June 1999 who attend 2nd grade in the fall of 2005. Panel B plots the share of pupils born between July 1991 and June 1994 who attend 7th grade in the fall of 2005. Panel C plots the average age at school entry for pupils born between July 1997 and June 1998. Data source: Administrative Data for the states of Hesse, Bavaria, and Schleswig-Holstein.

Table 1: Track Choice in 7th and 9th grade

	7th grade	9th grade
low (*Haupt- and Sonderschule*)		
medium (*Real- and Gesamtschule*)		
high (*Gymnasium*)		

Note: The table reports the share of pupils in different school tracks in 5th and 7th grade. Data source: Administrative data on all pupils in Hesse, Schleswig-Holstein, and Bavaria.

Figure 3: Test Score Distribution by Track Choice, 7th Grade



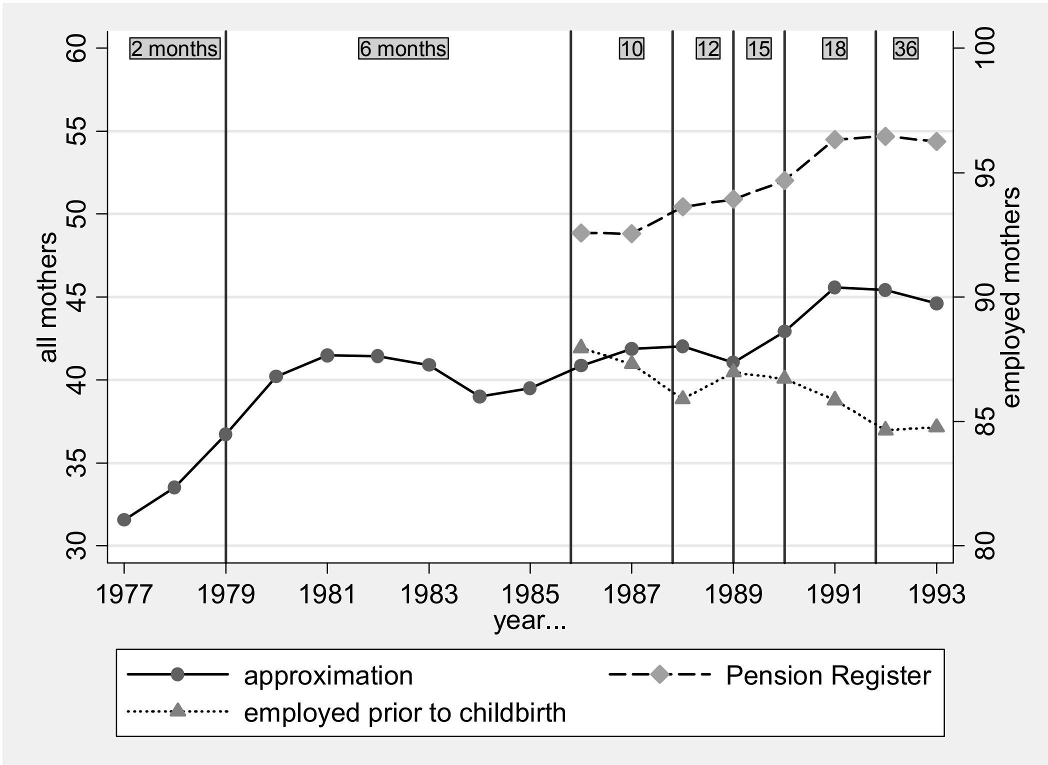
Note: The figure plots the distribution of the combined performance index (math, German, English) by school type in 7th grade. Results are based on all pupils in Hamburg who attended 7th grade in 1998 (Lau).

Table 2: Track Choice and the Test Score Distribution

	1	2	3
	test score	log-wage	log-wage
constant	-1.039		
(*low*)	(0.022)***		
intermediate	0.663		
	(0.024)***		
high	1.979		
	(0.024)***		
high school graduation (*Abitur*)			
adj. R squared	0.539		
N	9878		

Note: The dependent variable in Column 1 is the combined (math, German, English) performance index. The lowest track coice (Hauptschule) is the the omitted category. Data source: Lau.

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 German Pension Register. Triangles refer to mothers who were employed 9 months prior to birth. Results are based on the German Pension Register. Vertical lines indicate an expansion in leave coverage.

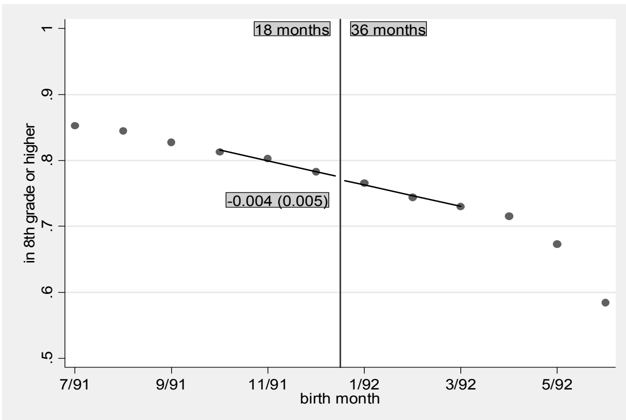
Table 3: The Impact of Leave Coverage on Labor Supply and Wages after Childbirth

	Panel A: 2 versus 6 months (May 1979)				
	working t months after childbirth				
	2	12	36	60	96
fraction working, 3/79	*38.9%*	*41.3%*	*37.9%*	*37.1%*	*37.6%*
March 79 vs June/July 79, DinD	**-0.313**	0.005	0.012	0.008	0.006
(control group: 1978)	(0.004)	(0.007)	(0.007)*	(0.007)	(0.007)
	working full-time t months after childbirth				
March 79 vs June/July 79, DinD	**-0.022**	0.002	-0.005	-0.009	-0.003
(control group: 1978)	(0.011)**	(0.010)	(0.011)	(0.011)	(0.011)
	Available income t months after childbirth				
	2	12	36	60	96
daily income, 3/79	*47.24*	*33.01*	*29.51*	*29.41*	*32.23*
March 79 vs June/July 79, DinD	**-19.216**	0.771	0.557	0.337	0.547
(control group: 1978)	(0.534)***	(0.646)	(0.624)	(0.627)	(0.681)
	Panel B: 6 versus 10 months (January 1986)				
	working t months after childbirth				
	6	10	30	66	90
fraction working, 11/85	*44.4%*	*46.1%*	*42.5%*	*43.9%*	*43.9%*
Oct./Nov. 85 vs March 86, DinD	**-0.283**	-0.019	-0.003	-0.001	-0.004
(control group: 1985/86)	(0.006)***	(0.006)**	(0.006)	0.006	(0.006)
	working full-time t months after childbirth				
Oct./Nov. 85 vs March 86, DinD	**-0.053**	-0.005	-0.009	-0.009	-0.005
(control group: 1985/86)	(0.012)***	(0.007)	(0.009)	(0.009)	(0.009)
	available income t months after childbirth				
	6	18	30	66	90
daily income, 11/85	*35.47*	*37.83*	*35.92*	*39.04*	*39.16*
Oct./Nov. 85 vs March 86, DinD	**-3.216**	-1.264	-0.564	-0.594	-0.733
(control group: 1986/87)	(0.483)***	(0.643)*	(0.637)	(0.676)	(0.677)
	Panel C: 18 versus 36 months (January 1992)				
	working t months after childbirth				
	6	18	36	66	90
fraction working, 11/91	*15.8%*	*37.2%*	*37.7%*	40.4%	*39.9%*
Oct./Nov. 91 vs March 92, DinD	-0.001	**-0.107**	0.016	0.011	0.011
(control group: 90/91)	(0.004)	**(0.005)*****	(0.005)***	(0.004)***	(0.005)**
	working full-time t months after childbirth				
Oct./Nov. 91 vs March 92, DinD	0.015	**-0.041**	-0.001	-0.009	-0.016
(control group: 90/91)	(0.013)	(0.009)***	(0.009)	(0.009)	(0.009)*
	available income t months after childbirth				
	18	30	42	66	90
daily income, 11/91	*32.56*	*32.76*	*33.56*	*35.99*	*38.82*
Oct./Nov. 91 vs March 92, DinD	**-9.925**	**-4.607**	1.267	1.042	0.777
(control group: 90/91)	(0.584)***	(0.571)***	(0.582)**	(0.407)**	(0.618)

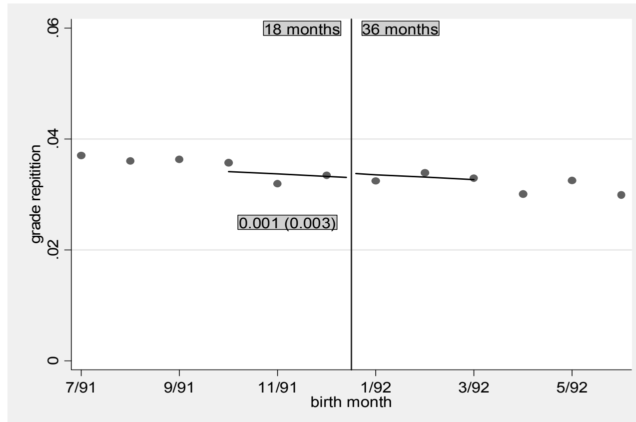
Note: The table reports the impact of an expansion in leave coverage on labor force participation rates, full-time work (conditional on working), and available income t months after childbirth. Income is equal to the woman's daily wage if she is working; if the mother is not working, the income is equal to the daily maternity benefit in case the mother is eligible, and zero otherwise. Results are based on administrative data drawn from social security records, covering *every* woman within the social security system. Additional results can be found in Schönberg and Ludsteck (2007).

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

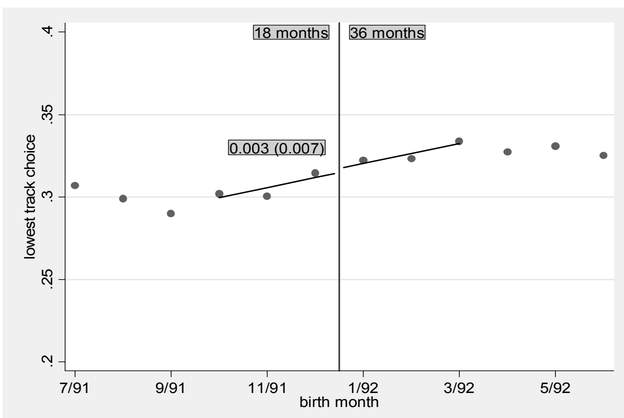
Panel A: 8th grade or higher



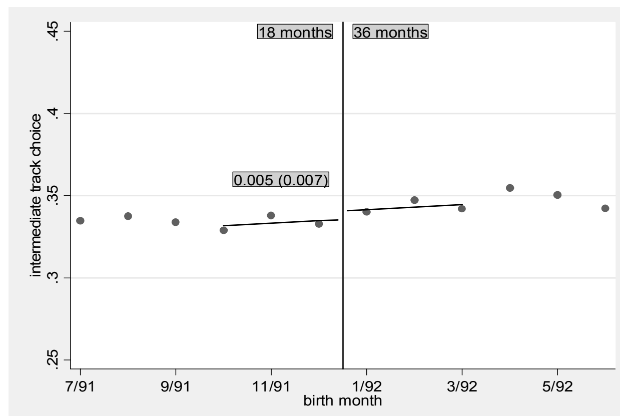
Panel B: Grade Repitition



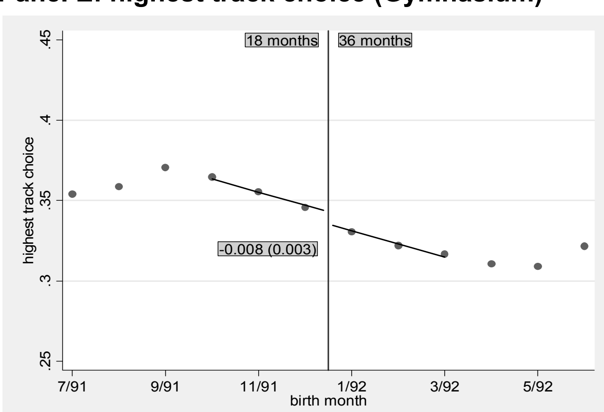
Panel C: lowest track choice (Hauptschule)



Panel D: intermediate track choice (Realschule)



Panel E: highest track choice (Gymnasium)



Note: Results refer to pupils 13/14 years old (i.e. school year 2005/06). The vertical line indicates the expansion in leave coverage from 18 to 36 months. The figures plot the share of pupils in 8th grade or higher (Panel A), the share of pupils who repeated a grade last year (Panel B), as well as the share of pupils in the three track choices (Panel C to E), for children born 6 months before or after the expansion in leave coverage. The figure also plots predicted share, 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=6). The figure in the box is the cofficient on this discontinuous jump.

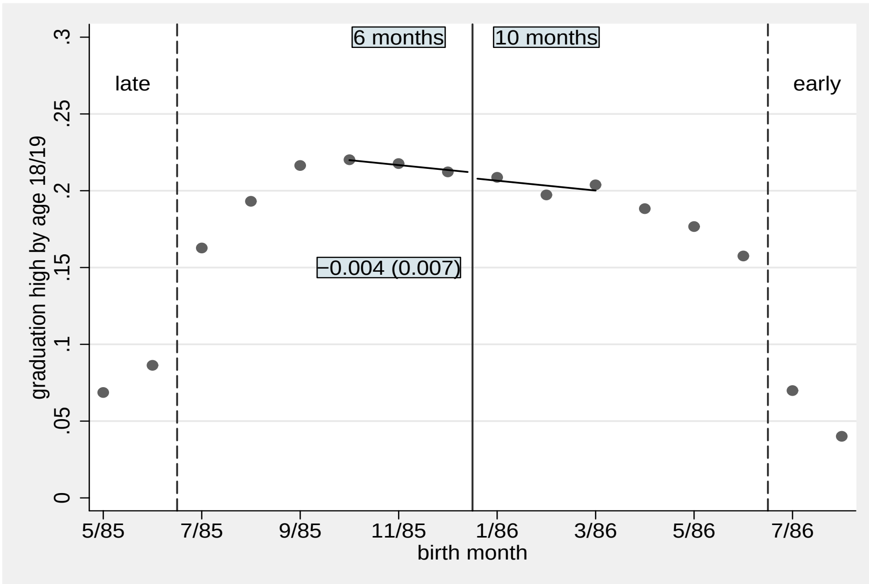
Table 4: The Impact of the Expansion in Leave Coverage from 18 to 36 Months on Children's Outcomes, Alternative Specifications

		track choice			grade attendance	
		low	*medium*	*high*	*in 8th grade or higher*	*repeated grade*
				regression discontunuity		
(1)	November - February	0.001	0.006	**-0.007**	0.002	-0.002
	linear age trend	(0.008)	(0.008)	(0.008)	(0.007)	(0.003)
	N	67086	67086	67086	67086	57762
(2)	October - March	0.003	0.006	**-0.008**	-0.005	0.001
	linear age trend	(0.006)	(0.006)	(0.006)	(0.005)	(0.003)
	N	101257	101257	101257	101257	86650
(3)	September - April	0.005	0.005	**-0.010**	-0.007	0.002
	linear age trend	(0.005)	(0.005)	(0.005)**	(0.005)	(0.002)
	N	136366	136366	136366	136366	117360
				placebo test: cohort 90/91		
(4)	September - April	-0.004	0.005	0.000	-0.002	-0.004
	linear age trend	(0.005)	(0.005)	(0.005)	(0.005)	(0.002)
	N	142712	142712	142712	142712	122161
				difference-in-difference (control group: 90/91)		
(5)	January - December	0.008	0.001	**-0.011**	-0.002	0.000
		(0.007)	(0.007)	(0.007)	(0.006)	(0.003)
	N	69393	69393	69393	69393	59346
(6)	November - February	0.007	-0.001	**-0.007**	-0.001	0.003
		(0.005)	(0.005)	(0.005)	(0.005)	(0.002)
	N	136411	136411	136411	136411	116717
(7)	October - March	0.003	0.003	**-0.007**	-0.001	0.003
		(0.004)	(0.004)	(0.004)*	(0.004)	(0.002)
	N	207095	207095	207095	207095	177152
(8)	September - April	0.002	0.004	**-0.006**	0.002	0.000
		(0.003)	(0.004)	(0.004)*	(0.003)	(0.002)
	N	279078	279078	279078	279078	238893
(9)	July-June,	0.001	0.005	**-0.006**	0.005	0.001
		(0.003)	(0.003)	(0.003)**	(0.003)*	(0.001)
	N	426106	426106	426106	426106	364597

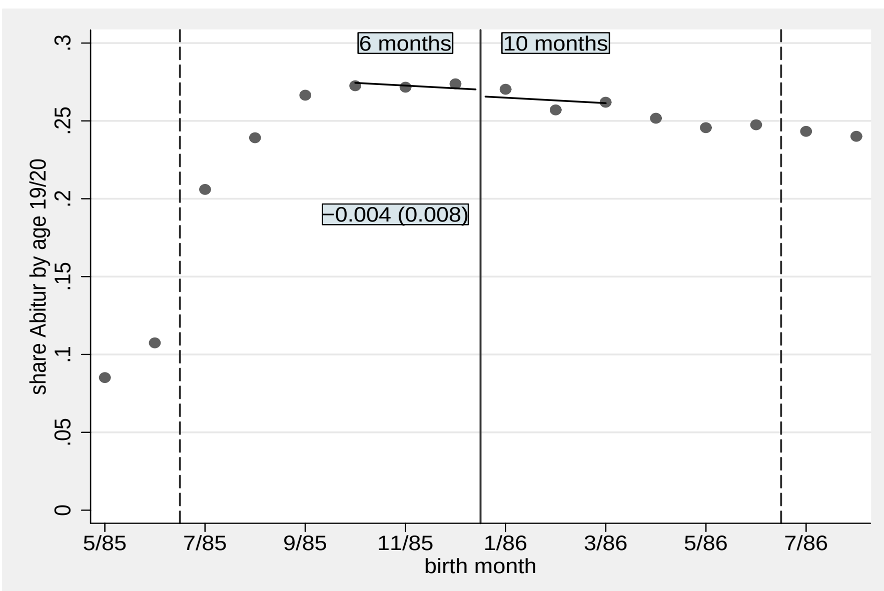
Note: The table reports various estimates of the impact of the expansion in leave coverage from 18 to 36 months on track choice and grade attendance. Regression discontinuity estimates regress the outcome variable on a linear age trend and a dummy variable equal to 1 if the child was born after the policy 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. Results using the 90/91 cohort as a control group refer to pupils 13/14 years olds, results using the 92/93 cohort as a control group refer to pupils 12/13 years old. Robust standard errors in parentheses. *Statistically significant at 0.10 level, ** at 0.05 level, *** at 0.01 level.

Figure 6: The Impact of the Expansion in Leave Coverage from 6 to 10 months on Graduation from Highest School Track (Abitur)

Panel A: at age 18/19



Panel B: at age 18/19 or 19/20



Note: The figure plots the share of pupils who graduate from the highest track choice (Abitur) and thus qualify for university by age 19 (Panel A), and by age 20 (Panel B). The solid vertical line indicates the expansion in leave coverage from 6 to 10 months. The dashed lines indicate the discontinuity due to school entry. It also plots the predicted share, obtained from a regression that controls for a linear age trend as well as a dummy variable indicating that the pupil was born after the policy reform. The figure in the box is the coefficient on this discontinuous jump.

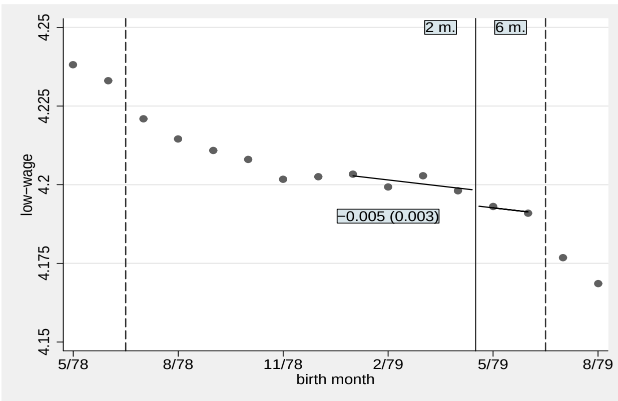
Table 5: The Impact of the Expansion in Leave Coverage from 6 to 10 Months on Graduation from Highest Track Choice, Alternative Specifications

		graduation by age 19	graduation by age 20
		regression discontinuity	
(1)	October - March, linear age trend	-0.004 (0.007)	-0.004 (0.009)
(2)	September - April, linear age trend	-0.003 (0.008)	-0.005 (0.009)
		placebo test: cohort 86/87 (19); cohort 84/85 (20)	
(3)	September - April linear age trend	0.001 (0.007)	-0.004 (0.010)
		difference-in-difference (control group: 85/86)	
(4)	November - February	-0.006 (0.010)	
(5)	October - March	-0.005 (0.006)	
(6)	September - April	-0.005 (0.007)	
		difference-in-difference (control group: 84/85)	
(7)	November - February		-0.001 (0.010)
(8)	October - March		-0.002 (0.006)
(9)	September - April		-0.006 (0.006)

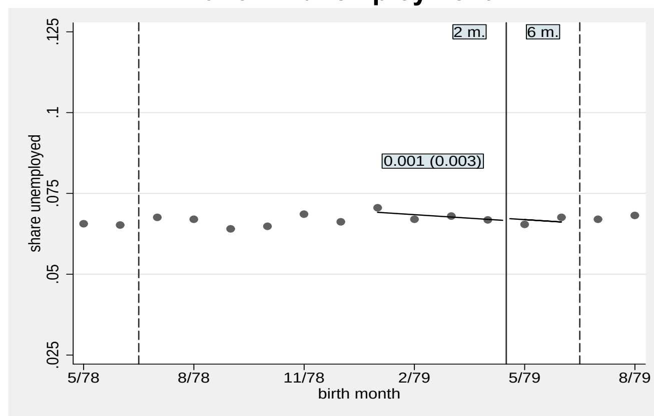
Note: The table reports various estimates of the impact of the expansion in leave coverage from 6 to 10 months onhigh school graduation (Abitur). Regression discontinuity estimates regress the outcome variable on a linear age trend and a dummy variable equal to 1 if the child was born after the policy reform. The placebo test repeats the analysis for 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. We use two control groups, children born between July 1986 and June 1987 (main control group), and children born between July 1984 and June 1985.

Figure 7: The Impact of the Expansion in Leave Coverage on Children's Labor Market and Education Outcomes

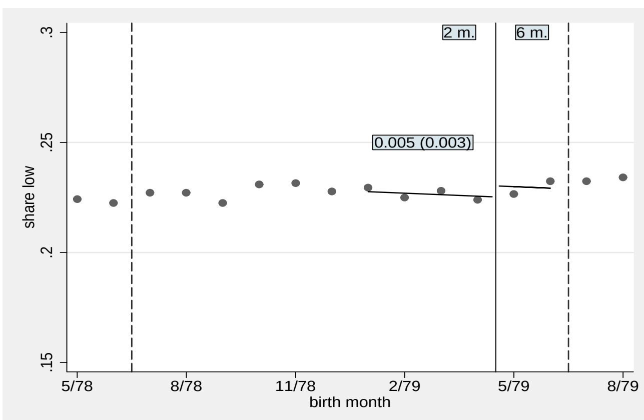
Pabel A: log wages



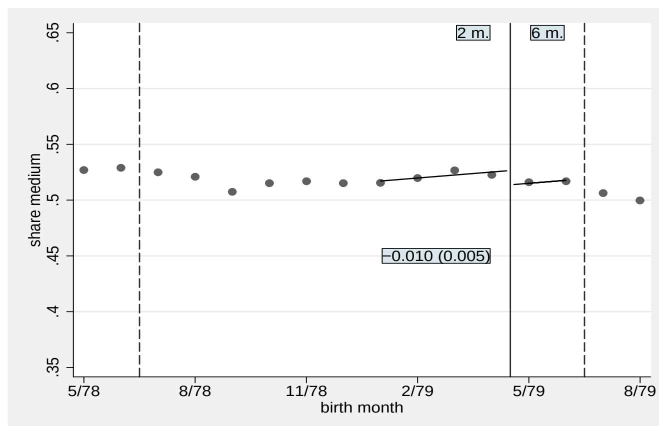
Panel B: unemployment



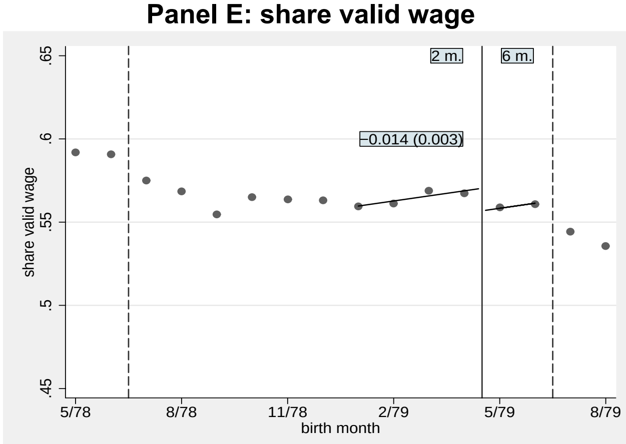
Panel C: low-skilled



Panel D: medium-skilled



Panel E: share valid wage



Note: Results refer to young adults 25/26 years old (i.e. October 2004). 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. The figures plot the mean log wage (Panel A), the share of unemployed individuals (Panel B), as well as the share of low- and medium skilled individuals (Panel and D), and the share of individuals with a valid wage (Panel E), for individuals born around the expansion in leave coverage. The figure also plots predicted share, 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 4 months before or 2 months after the reform (N=6). The figure in the box is the cofficient on this discontinuous jump.

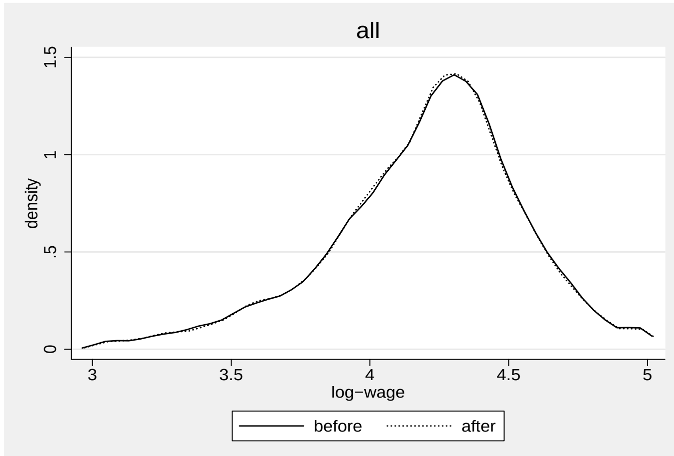
Table 6: The Impact of the Expansion in Leave Coverage from 2 to 6 Months on Wages, Alternative Specifications

	wages	unemp.	low	medium	valid wage
	regression discontinuity				
March - June	-0.002	-0.002	0.002	-0.005	-0.009
linear age trend	(0.005)	(0.002)	(0.011)	(0.006)	(0.004)
N	95716	4	4	4	4
January - June,	-0.006	0.001	0.005	-0.013	-0.014
linear age trend	(0.003)*	(0.003)	(0.005)	(0.005)*	(0.003)*
N	140777	6	6	6	6
	placebo test (cohort 79/80)				
January - June,	-0.007	0.000	0.006	-0.009	-0.006
linear age trend	(0.003)**	(0.003)	(0.007)	(0.005)*	(0.002)*
N	151164	6	6	6	6
	placebo test (cohort 77/78)				
January - June,	-0.004	0.000	0.002	-0.009	-0.008
linear age trend	(0.003)	(0.003)	(0.002)	(0.005)	0.012
N	140541	6	6	6	6
	difference-in-difference (control group: 77/78)				
April - May	0.000				
	(0.004)				
N	99994				
March - June	-0.001	0.000	0.002	-0.003	-0.004
	(0.003)	(0.002)	(0.005)	(0.005)	(0.003)
N	198480	8	8	8	8
January - June,	-0.001	0.000	0.003	-0.004	-0.002
	(0.003)	(0.002)	(0.005)	(0.005)	(0.004)
N	291898	12			12
July-June	0.000	-0.001	0.003	-0.001	0.001
	(0.002)	(0.001)	(0.003)	(0.004)	(0.004)
N	578629	24	24	24	24
	difference-in-difference (control group: 79/80)				
April - May	-0.002				
	(0.004)				
N	96566				
January - June,	-0.002	0.002	0.001	-0.002	-0.004
	(0.003)	(0.002)	(0.002)	(0.006)	(0.005)
N	281753	6	6	6	6

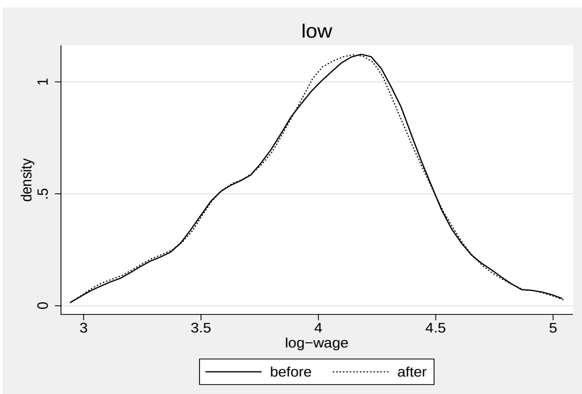
Note: The table reports various estimates of the impact of the expansion in leave coverage from 2 to 6 months on log-wages, education, and unemployment. Regression discontinuity estimates regress the outcome variable on a linear age trend and a dummy variable equal to 1 if the child was born after the 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. Robust standard errors in parentheses. Results using the 77/78 cohort as a control group refer to individuals 25/26 years olds, results using the 79/80 cohort as a control group refer to individuals 24/25 years old. Results on education and unemployment are based on data aggegrated up to the birth month. *Statistically significant at 0.10 level, ** at 0.05 level, *** at 0.01 level.

Figure 8: The Wage Density Before and After the Reform

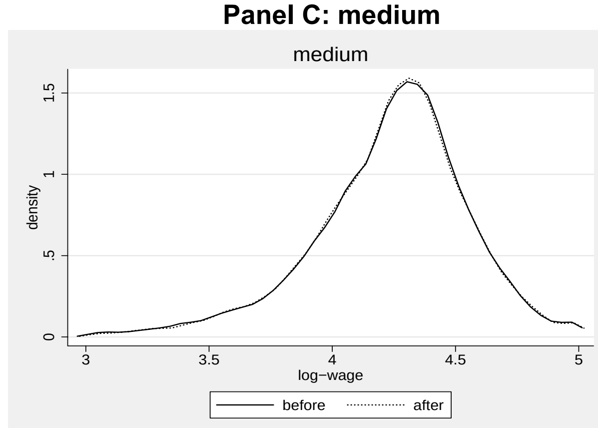
Panel A: all



Panel B: low



Panel C: medium



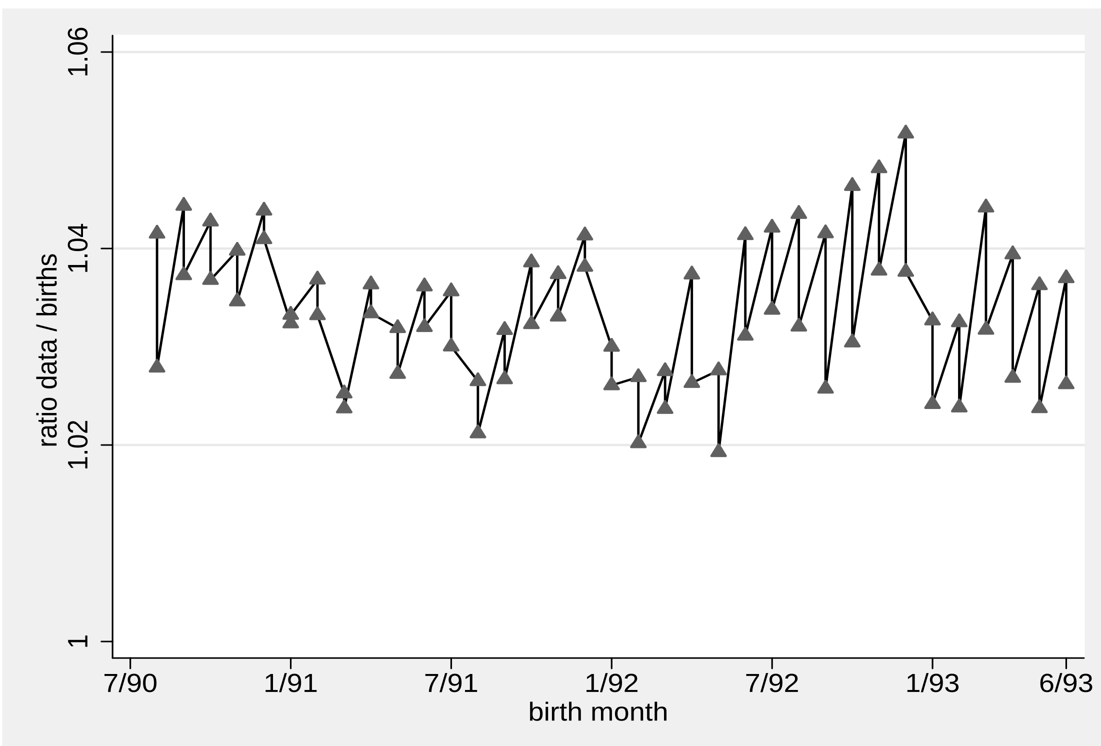
Note: The figure compares the wage density for individuals born in March/April (before) and May/June (after). We eliminate the age effect using individuals born one year before the expansion as a control group.

Table A.1: Descriptive Statistics, Adiministrative School Data, Expansion from 18 to 36 months

	school year 2004/05			school year 2005/06		
	7/90 - 6/91	**7/91 - 6/92**	**6/92-7/93**	**6/90 - 7/91**	**6/91 - 7/92**	**6/92-7/93**
predicted grade	**8**	**7**	**6**	**9**	**8**	**7**
# obs.	217,918	209,209	206,947	216,707	208,188	204,987
mean # obs. per month	18,159	17,434	17,245	18,058	17,349	17,028
Hesse	25.51	25.51	24.97	25.65	25.67	25.3
Schleswig-Holstein	14.44	14.33	14.16	14.54	14.43	14.32
Bavaria	60.04	60.15	60.87	59.81	59.9	60.38
girls	48.58	48.93	48.82	48.58	48.97	48.79
track choice:						
low	32.83	30.35	27.73	33.72	31.45	28.8
medium	33.63	32.31	29.39	34.1	34.02	33.37
high	32.81	35.12	35.24	31.71	33.84	35.72
in predicted grade or higher	75.1	79.23	81.26	71.32	76.23	79.39
repeated grade	4.25	2.56	6.02	4.4	3.36	2.53

Note: The table reports the means of the outcome variables used to evaluate the expansion in leave coverage from 18 to 36 months, by school year and cohort. Data Source: Administrative Data on School Choice.

Figure A.1: Ratio of Number of Observations in Data and Number of Births, July 1990-June 1993



Note: The figure plots the ratio between the number of observations in the data and the number of births, for the birth months July 1990 to June 1993. Results refer to German citizents only.

Table A.2: Descriptive Statistics, Social Security Data, Expansion from 2 to 6 Months

		October 2003			October 2004		
		7/77-6/78	7/78-6/79	7/79-6/80	7/77-6/78	7/78-6/79	7/79-6/80
	age	25-26	24-25	23-24	26-27	24-25	24-25
1	# obs.	422,152	411,165	425,826	418,852	406,454	419,173
2	# valid wage	300,187	280,860	273,619	300,142	280,126	276,734
3	# unemployed	35,153	33,435	34,870	32,569	33,267	34,632
4	share men	50.86	49.52	48.27	52.05	50.62	49.4
5	share low	20.26	21.09	21.65	20.37	21.04	21.85
6	share medium	74.4	75.68	76.24	71.07	73.33	74.76
7	log daily wage	4.21 (0.35)	4.17 (0.34)	4.12 (0.33)	4.24 (0.37)	4.20 (0.35)	4.16 (0.34)
8	censored	0.74%	0.41%	0.22%	1.34%	0.75%	0.40%

Note: The first row (# observations) refers to all men and women observed in our data set as of October 2003 or October 2004, including the unemployed, workers in appretniceship training, and workers on marginal jobs. The second row (# valid wage) lists the number of valid wage observations, and excludes the unemployed, workers on marginal jobs, and workers in apprenticship training. The third row lists the number of unemployed. The remaining variables are based on workers with a valid wage. Data source: Social Security Data.