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IZA DP No. 7707

What Is the Case for Paid Maternity Leave?

Gordon B. Dahl
Katrine V. Løken
Magne Mogstad
Kari Vea Salvanes

October 2013

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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Gordon B. Dahl
University of California San Diego and IZA

Katrine V. Løken
University of Bergen and IZA

Magne Mogstad
University College London, Statistics Norway and IZA

Kari Vea Salvanes
University of Oslo

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

What Is the Case for Paid Maternity Leave?

Paid maternity leave has gained greater salience in the past few decades as mothers have increasingly entered the workforce. Indeed, the median number of weeks of paid leave to mothers among OECD countries was 14 in 1980, but had risen to 42 by 2011. We assess the case for paid maternity leave, focusing on parents' responses to a series of policy reforms in Norway which expanded paid leave from 18 to 35 weeks (without changing the length of job protection). Our first empirical result is that none of the reforms seem to crowd out unpaid leave. Each reform increases the amount of time spent at home versus work by roughly the increased number of weeks allowed. Since income replacement was 100% for most women, the reforms caused an increase in mother's time spent at home after birth, without a reduction in family income. Our second set of empirical results reveals the expansions had little effect on a wide variety of outcomes, including children's school outcomes, parental earnings and participation in the labor market in the short or long run, completed fertility, marriage or divorce. Not only is there no evidence that each expansion in isolation had economically significant effects, but this null result holds even if we cumulate our estimates across all expansions from 18 to 35 weeks. Our third finding is that paid maternity leave has negative redistribution properties. The program makes regressive transfers both from ineligibles to eligibles and within the group of eligible mothers. Since there was no crowd out of unpaid leave, the extra leave benefits amounted to a pure leisure transfer, primarily to middle and upper income families. Finally, we investigate the financial costs of the extensions in paid maternity leave. We find these reforms had little impact on parents' future tax payments and benefit receipt. As a result, the large increases in public spending on maternity leave imply a considerable increase in taxes, at a cost to economic efficiency. Taken together, our findings suggest the generous extensions to paid leave were costly, had no measurable effect on outcomes and poor redistribution properties. In a time of harsh budget realities, our findings have important implications for countries that are considering future expansions or contractions in the duration of paid leave.

JEL Classification: J13, J18, H42

Keywords: paid maternity leave, redistribution effects of social programs

Corresponding author:

Gordon B. Dahl
University of California, San Diego
9500 Gilman Drive #0508
La Jolla, CA 92093-0508
USA
E-mail: gdahl@ucsd.edu

1 Introduction

The past 50 years has seen a steady increase in the number of countries offering government funded parental leave.[1] By 2013, all OECD countries except for the United States had federally funded programs, which generally offer high earnings replacement and employment protection to mothers who were working prior to giving birth. But the even more striking trend has been the substantial increase in the duration of paid leave. As illustrated in Figure 1, the median number of weeks of paid leave to mothers among OECD countries was 14 in 1980, but had risen to 42 weeks by 2011. From a policy perspective, it is important to distinguish between the introduction of paid leave (and job protection) versus continual expansions to a program. Government-mandated leave programs involve large transfer payments to a sizable population, and the key question is whether benefits outweigh costs, both on the extensive and intensive margins.

The main argument for paid leave is that there are important social goals or market failures addressed by the program. In the policy arena, proponents claim that paid leave could enhance child development by increasing parental time investment, promote gender equality through higher maternal employment, and encourage fertility and marriage by facilitating family and career compatibility. Paid leave could also serve as a transfer program with desirable redistribution effects. If there are no public benefits, however, paid leave can only be justified if it alleviates market failures. In particular, binding credit constraints could limit the intertemporal substitution of maternal labor supply after birth or prevent families from investing optimally in children.

A comprehensive evaluation of the case for paid parental leave requires answers to at least four key questions. First, does paid leave increase available parental time with children, or does it simply crowd out unpaid leave? Second, what effect does paid leave have on a broad range of child, parent and family outcomes? Third, how do any benefits compare relative to costs? And finally, are there progressive or regressive distributional effects?

We answer these questions in the context of Norway's parental leave program, focusing on parents'

[1] Austria, the Czech Republic, Italy and Poland introduced paid parental leave in the 1960s or earlier; Hungary, Spain, Sweden, Finland, France, and Norway in the 1970s; Iceland, Denmark Portugal, Turkey, Greece, Germany and New Zealand in the 1980s; Canada, the Netherlands, Japan, the Slovak Republic, Australia, Belgium, Ireland, Luxembourg in the 1990s; and the United Kingdom in the 2000s.

responses to the evolution of leave benefits over time. There have been seven major reforms in leave policy which expanded the amount of leave taken by mothers without having a significant effect on fathers.[2] In 1977, Norway introduced 18 weeks of paid parental leave and extended job protection from 12 weeks to one year. In the ensuing years, from 1987 to 1992, there were a series of policy reforms which expanded paid leave from 18 weeks to 35 weeks (without changing the length of job protection). While prior work has documented an impact on childrens' high school graduation rates and future wages from the initial 1977 reform (Carneiro, Løken and Salvanes, 2011), we investigate whether the subsequent generous expansions had any added effect on outcomes which could justify their additional costs. In a time when most countries are facing harsh budget realities, it is important to critically evaluate whether these fiscally expensive parental leave extensions had the desired policy effects.

Each of the reforms specified a birth cutoff date to determine eligibility for benefits. Parents of children born after the cutoff date were eligible for extra weeks of paid leave relative to those parents with children born before the cutoff. We use a regression discontinuity (RD) estimator to obtain causal estimates of the effect of extra weeks of paid leave on a variety of outcomes. The idea is to compare families to the left and right of this cutoff, since they should be similar on average except for the extra allotment of paid leave from the government. By estimating the effect for all expansionary reforms, we can assess whether child, family or social benefits continue to accrue as the generosity of the program increases, or whether additional weeks of paid parental leave simply represent transfer payments without measurable differences in outcomes.

Our first empirical result is that none of the reforms crowd out unpaid leave. Each reform increases the amount of time spent at home versus work by roughly the increased number of weeks allowed. Since income replacement was 100% for most women, the reforms caused an increase in mother's time spent at home after birth, without a reduction in family income. This allows for a clean estimate of the effect of parental time on child and family outcomes, as there are no income effects to worry about. The same is not true in other settings where income is only partially replaced

[2]With the exception of a few weeks specifically reserved for mothers, these leave reforms allowed couples to divide up the weeks of leave as they saw fit. In practice, however, the entire amount of leave was taken by mothers. Throughout this paper, we therefore refer to parental leave and maternity leave interchangeably.

or where there is crowd out of unpaid leave.

Our second set of empirical results reveals the expansions had little effect on a wide variety of outcomes other than mothers' time spent at home after birth. Not only is there no evidence that each expansion in isolation had economically significant effects, but this null result holds even if we cumulate our estimates across all expansions from 18 to 35 weeks. We find no effect of paid leave on children's academic achievement or graduation from high school. We also find no evidence of systematic changes in parental earnings or participation in the labor market in the short or long run, which means the leave expansions did nothing to gender equality on these dimensions. Finally, there is little evidence the expansions to parental leave affected completed fertility, marriage or divorce.

Our third finding is that paid parental leave has negative redistribution properties. The program makes regressive transfers both from ineligibles to eligibles and within the group of eligible mothers. Since there is no crowd out of unpaid leave, the extra leave benefits amount to a pure leisure transfer. Around 74 percent of all mothers were eligible for paid leave during our time period, since a requirement is that the mother works a certain amount prior to the child's birth. Mothers who are eligible and receive parental leave benefits are higher educated, are married to higher educated men, and have substantially higher family income compared to ineligible mothers. The differences in income between ineligibles and eligibles are even larger when comparing eligible mothers to the relatively poor men and women who never have children. Within the group of eligible mothers, the transfer payments were larger to women with higher prior earnings (since the program gave 100% income replacement up to a high threshold); not surprisingly, this type of women also resides in a family with higher total family income.

Finally, we investigate the financial costs of the extensions in paid parental leave. We find these reforms had little, if any, impact on parents' future tax payments and benefit receipt. As a result, the large increases in public spending on parental leave imply a considerable increase in taxes, at a cost to economic efficiency. We estimate a one week increase in paid leave costs taxpayers $687 dollars (in 2010 dollars) on average per birth, based on all the reforms we study.[3] These costs

[3]Leave benefits are taxable at the individual level, but tax revenue is also lost due to the foregone income of mothers on leave. In our setting where there is no crowd out of unpaid leave and 100% income replacement, these two tax effects roughly cancel each other out, so the after tax costs of the program are approximately equal to program

add up: the initial 18 week reform costs an estimated total of $12,354 per eligible birth, while the subsequent expansions from 18 to 35 weeks costs taxpayers $11,668 per eligible birth. Stated differently, payments in 1992 when leave benefits totaled 35 weeks cost a total of over $1 billion (in 2010 dollars), or approximately 0.5% of GDP.

Taken together, our finding suggest the case for extensions to paid maternity leave cannot rely on observable dimensions of public benefits or redistribution. Nor do paid leave extensions seem to alleviate binding credit constraints which would limit intertemporal substitution of labor supply or prevent optimal investment in children: there is no evidence of changes in labor supply in the short or long run; there is no measurable effect on child development; and most transfer payments are received by middle or upper class families, who are less likely to face binding credit constraints. In light of previous studies, one can argue whether a baseline level of paid parental leave or job protection is worth the cost. But the case for extended paid leave periods, which are prevalent in OECD countries, is weak. One would have to argue there are benefits not captured in the relatively rich set of outcome variables we look at. We discuss this possibility in more detail in the conclusion of the paper.

Our findings are related to a growing literature on paid parental leave introductions and extensions. Researchers have examined the effect of paid leave on parental employment and earnings, child outcomes, and fertility and marriage.[4] While these papers make important contributions to our understanding of parental leave, there is no consensus across studies. Consider the research most closely related to our study. In a series of papers, Baker and Milligan (2008a, 2008b, 2010) evaluate the extension of leave from 25 weeks to 50 weeks in Canada, with an earnings replacement rate of 55%. They find that while it increased breastfeeding, it had little effect on child health and development up to age 3 and negative effects on verbal and self-awareness scores at ages 4 and 5. Rasmussen (2010) finds that a single expansion of leave in Denmark did not affect later educational outcomes, while Liu and Skans (2011) find that a single leave expansion in Sweden resulted in in-

expenditures.

[4]See, for example, Baker and Milligan, 2008a, 2008b, 2010; Carneiro, Løken and Salvanes, 2011; Dustmann and Schönberg, 2012; Lalive et al., forthcoming; Lalive and Zweimuller, 2009; Liu et al., 2010; Rasmussen, 2010; Rossin, 2011; Ruhm, 2000; Schönberg and Ludsteck, forthcoming; and Waldfogel et al., 1999.

creased academic test scores for children of highly-educated mothers but otherwise had no effect. Finally, Dustmann and Schönberg (2012) examine two paid leave and job protection expansions (from 2 to 6 months and from 6 to 10 months), as well as an increase in job protection from 18 months to 36 months. These expansions provided a low level of earning replacement (a flat amount equal to about 20 to 30 percent of average pre-earnings, plus a means tested amount after 7 months). They find little effect of the first two expansions, but some negative effects for the increase in job protection without a corresponding increase in payments.

Our paper expands and clarifies this prior literature in several important ways. First, we are able to look at a broad range of outcomes in the same setting over time. Second, we provide direct evidence on the cost side of these policies and their distributional effects. Third, we examine a series of six expansions. Not only does this give us multiple quasi-experiments, it also helps determine whether modest expansions are worthwhile, even if further, larger expansions are not. A comparison between benefits and costs of marginal expansions is key to determine the optimal duration of paid parental leave. Fourth, the reforms we study each extended paid leave without any changes in job protection, which helps isolate the effect of paid leave from employment protection. Fifth, take up is essentially 100%, in contrast to other countries with less generous leave programs. Finally, we are able to hone in on the counterfactual to paid leave because there is no evidence of crowd out of unpaid leave and there was 100% income replacement. This helps us interpret the expansions to paid parental leave as increases in the amount of time spent at home versus work, holding family income constant.[5]

Our paper is also related to a large literature on maternal employment and child outcomes (see Blau and Currie (2006) for a review). That literature is largely inconclusive, with much of the work suffering from endogeneity issues related to which women choose to work (Gregg and Waldfogel (2005), Tanaka (2005), Gregg et al. (2005)). Our setting is ideal to study the effect of maternal employment for two reasons: (i) paid leave did not change the family's income, but only mother's weeks of work and (ii) there are no concerns with sample selection as take up is 100%; our estimates

[5]There was almost no governmentally provided child care coverage for children under the age of 2 during this period (Mogstad and Havnes, 2011). As a result, the counterfactual to parental care is usually informal care, including relatives and nannies.

are internally valid for working women. Our findings suggest that maternal employment between the ages of roughly 4 to 9 months has little effect on a child's later outcomes (as well as no effect on couple's labor market participation, fertility, or family stability), even when family income remains constant.

The remainder of the paper proceeds as follows. The next section provides background on paid parental leave in Norway and our data. Section 3 briefly discusses our regression discontinuity design and threats to identification. In Section 4, we present our main results, followed by a comparison of costs versus benefits and distributional effects in Sections 5 and 6. Section 7 concludes.

2 Background and Data

2.1 Background

Governmental paid parental leave has a long history in Norway.[6] In 1977, parents were granted 18 weeks of paid leave and job protection for paid and unpaid leave up to one year.[7] During the late 1980s and early 1990s, the paid leave period gradually expanded, so that by 1992 there was a maximum of 35 weeks of paid leave, but no increase in job protection. Figure 2 gives an overview of the introduction of paid parental leave in 1977 and the yearly expansions from 1987-1992.

The parental leave mandates provide 100% income replacement up to a generous earnings threshold. The earnings thresholds are non-binding for most mothers, and when they are exceeded, most public and private employers top up benefits so that foregone earnings are fully replaced.[8] The firm is not allowed to dismiss the worker for taking leave, and the parent has the right to return to a comparable job. Apart from a few weeks reserved for the mother, parents could share 35 weeks between them as desired before 1993. Up to that point, the program was de facto a maternity leave program, as very few fathers took any amount of leave.[9]

[6] Our discussion of parental leave in Norway builds on Carneiro, Løken and Salvanes (2011), Dahl, Løken and Mogstad (forthcoming), and Rege and Solli (2010).

[7] Prior to this, parents had 12 weeks of job protection due to legislation passed in 1956, but no paid leave.

[8] In 2010, benefits were capped at earnings of approximately $75,000. Only 7% of mothers earned more than this threshold.

[9] Starting in 1993, father quotas have been added in subsequent reforms, mostly on top of existing parental leave. See Dahl, Løken and Mogstad (forthcoming) for an analysis of the 1993 paternity leave quota.

In our analysis, we focus on the six expansions in paid leave between 1987 and 1992. There are several reasons for this choice. First, this allows us to analyze long term outcomes, such as children's high school graduation rates and parent's long-term labor force attachment. Second, this period gives us a total increase of 17 weeks of paid leave, providing a natural comparison to the initial 18 weeks introduced in 1977. Finally, this is the period before the reforms started to set aside specific quotas for fathers, which would add a new layer of complexity. While virtually all eligible mothers take leave, not all eligible fathers do, even after the introduction of the father quotas.

The various reforms determined benefits based on the birthdate of the child. For the 1987 reform, parents with children born after May 1st 1987 were eligible for 20 weeks of paid parental leave while parents whose children were born before were eligible for just 18 weeks. Subsequent expansions were introduced with the following date cutoffs: July 1st 1988: 2 additional weeks, April 1st 1989: 2 additional weeks, May 1st 1990: 4 additional weeks, July 1st 1991: 4 additional weeks, and April 1st 1992: 3 additional weeks.[10]

In addition to the birth cutoffs, paid parental leave benefits were contingent on the mother working at least 6 of the last 10 months before birth. Earnings in the prior 10 months needed to exceed the "substantial gainful activity" threshold (approximately $12,500 in the year 2010).

The parental leave system is universal, simple, and well-known (including details about eligibility, benefit amounts, and the application process). To apply for parental leave benefits, parents must inform their employers and submit a joint application to a Social Security Administration field office at least six weeks before the pregnancy due date.

2.2 Data

Our analysis employs several data sources that we can link through unique identifiers for each individual. This gives us a rich longitudinal database that covers every resident starting in 1967. For each year, it contains individual demographic information and unique identifiers that allow us to match spouses and parents to their children. The data on parental leave comes from social security

[10]Starting with the 1989 reform, parents were given the option of taking 80% earnings replacement and 20% more weeks of paid leave. Since job protection extends for a year, at the margin it should be the same to take extra weeks at 80 % coverage or 100 % coverage and additional weeks as unpaid leave.

registers that contain complete records for all individuals from 1992 onwards. One advantage of our data is that we can study a wide variety of parents, child and family outcomes.

For mothers and fathers we focus on outcomes related to annual earnings and transfers, as the Norwegian data does not include a measure of hours. Income is measured as total pension-qualifying earnings and transfers reported in the tax registry. This individual-specific measure includes labor earnings and all taxable transfers including sickness benefits, unemployment benefits and parental leave payments. We construct a measure of whether mothers work after birth based on whether they have positive labor earnings. We also look directly at earnings after the reforms. For both mothers and fathers we construct two long term outcomes: total years of employment up to 14 years after the reform and the annuity of income up to 14 years after each reform.

The outcomes we study for children are the written exam taken at the end of junior high school and dropout rates from high school. The written exam is important for determining placement into high school, and must be taken by all students. The exam subject rotates and can be in either math, Norwegian or English. High school dropouts are defined as children not obtaining a three year high school diploma by age 20. Due to age and survey restrictions, we do not have dropout status for the 1992 reform.

For family outcomes we examine combined family income, marriage patterns and fertility. We measure long-term marital stability by observing whether a couple is divorced 14 years after the reform, conditional on being married the year before the reform. We also look at whether a couple is married 14 years later, given that they were not married the year before the reform. Lastly, we look at completed fertility, defined as the cumulative number of children born to a mother 14 years after the reform.

One of the key advantages of our dataset is that we can address the cost side of parental leave, as well as its distributional effects. We can measure the amount of taxes a couple pays in the 14 years after the program to see if the program is self-funding in the long run due to increased female labor force participation or earnings. We can likewise measure the future benefits from other programs a family receives in the long run, including the universal child allowance, the family allowance which

is targeted to single parents, and disability insurance. With these variables, we can compare the annuity of costs to the annuity of benefits.[11]

Since we do not observe hours, we cannot directly observe which parents are eligible for paid leave according to the rules described in the previous section. We therefore predict eligibility using labor earnings the year before birth. We define eligibility for mothers as having earnings above the substantial gainful activity level, described in the previous section, in the year prior to birth. Eligibility increases over time as more women enter the labor market. For example in 1987 about 70% of women are eligible, but by 1992, 76% of women are eligible. We limit our estimation sample to those mothers who are predicted to be eligible.

There is a tradeoff between using too strict of an earnings requirement and excluding parents from our sample who were in fact eligible, and using a less strict earnings requirement and including parents who actually were ineligible. While including ineligible parents may increase the residual variation and thus the standard errors in the RD estimation, excluding eligible parents may affect the external validity of our results.[12] By using a fairly weak earnings requirement in the prediction of eligibility, we assign more weight to the generalizability of our results.

The final variable needed for our analysis is take up of parental leave. The exact amount of paid leave taken by each parent and the amount of the benefit is directly observed from 1992 onwards. For prior years, we assume that paid leave has a take up rate of 100% for mothers (with no take up for fathers). This assumption is supported both by survey evidence (Carneiro, Løken and Salvanes 2011) and the fact that observed take up for the 1992 reform is close to universal for mothers and close to zero for fathers. To calculate the amount of unpaid leave, we follow Carneiro, Løken and Salvanes (2011) and impute days of unpaid leave using information on yearly earnings.[13] Later in the paper, we verify this method by comparing the imputed estimates for 1992 to the actual data

[11] See Blundell, Graber and Mogstad (2012) for a description of the Norwegian tax-transfer system. For all annuities, we use an annual discount rate of 2.3%.

[12] As long as eligibility cannot be manipulated, the internal validity of the RD estimates are unaffected by the exclusion of ineligibles.

[13] Specifically, we impute pre-birth monthly income by dividing earnings the year before the reform by 12. Then we calculate total earnings in the year of the reform and the following three years after, and divide this by pre-reform monthly income. This yields a measure of the number of days of unpaid leave during the first 36 months after birth. For this imputation to work, the assumption is that pre-reform earnings are a good approximation for maternal potential post-birth earnings (the earnings she would have received had she not gone on unpaid leave), adjusted for inflation.

on leave taking.

Appendix Table A1 provides summary statistics for the variables defined in this section, separately for each of the six reform years.

3 Identification

3.1 Regression Discontinuity Design

While we analyze six different expansions in paid leave, in each case we use a similar identification strategy. To make things concrete, we focus the discussion on the 1992 reform. The discontinuity we exploit arises from the reform being contingent on the birthdate of the child. Parents of children born after April 1, 1992 were entitled to 35 weeks of parental leave while those with a child born before this cutoff could only receive 32 weeks. The reduced form model for our RD design can be implemented by the following equation:[14]

$$y = \alpha + 1[t \geq c](g_l(t-c) + \lambda) + 1[t < c]g_r(c-t) + e \tag{1}$$

where y is the relevant outcome variable, t is the birthdate of the child, c is the cut-off date, e is an error term, and g_l, and g_r are unknown functions. The coefficient λ is the "intention to treat" (ITT) effect of the reform on outcomes. To get the average effect of the extra weeks of paid leave, λ needs to be scaled by the jump in the take up of parental leave at around the cutoff from a first-stage RD regression. For 1992, when actual take up is observed, we can estimate this first stage. For earlier years, this is not possible. However, since take up of parental leave benefits for mothers is close to 100% in 1992 and survey data suggests the same holds true from the beginning of the program (Carneiro, Løken and Salvanes, 2011), the ITT estimate should be close to the average effect. For the remainder of the paper, therefore, we focus on the ITT estimates.

[14] See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for details on the implementation and assessment of RD designs.

3.2 Threats to Identification

3.2.1 Strategic timing of births

The validity of our RD design requires that individuals cannot manipulate the assignment variable, which is the child's birthdate. If date of birth cannot be timed in response to the reform, the aggregate distribution of the assignment variable should be continuous around the cutoff date.

There is little opportunity to strategically time conception, as the implementation dates for the reforms have been announced less than nine months in advance. The national budgets which propose the parental leave reforms are generally introduced in the fall and passed by parliament in December of the same year. Therefore, mothers giving birth close to the cutoffs, which occurred in the spring and summer, were already pregnant before the announcement of the reform. Furthermore, as discussed in Section 2.1, the month of implementation varied from reform to reform, so the exact timing would be hard to predict.

While strategic timing of conception is unlikely, it is still possible that mothers with due dates close to the cutoff date could postpone induced births and planned cesarean sections. In contrast to current births practices in the U.S., the vast majority of births in Norway during the time of our reforms were spontaneous vaginal deliveries. In 1993, the c-section rate was 12% (with 59% of these being emergency operations) and only 12% of vaginal deliveries were induced (Folkehelseinstituttet, http://mfr-nesstar.uib.no/mfr).[15] Therefore, this type of manipulation is less likely than it would be in the U.S. setting (Dickert-Conlin and Chandra, 1999).

Figure 3 and Appendix Figure A1 graphically confirm there is no systematic effect from the reforms on the distribution of fertility around the cutoffs. While there are some seasonal patterns in the number of births, and a small jump in 1992, the reforms in general show no systematic evidence that births are strategically delayed. Nonetheless, to avoid the possibility that some births in our sample are strategically delayed, we report robustness results which exclude the week immediately before and the week immediately following the cutoff date. As we will show, excluding these observations does not materially affect our findings.

[15] This contrasts with a c-section rate of 33% and an induction rate of 23% in the U.S. in 2011 (National Vital Statistics Reports, Births: Final Data for 2011, Vol. 62, no. 1, June 28, 2013).

3.2.2 Eligibility

Another threat to our identification strategy is that the announcement of the reform could cause a change in eligibility around the cutoff date. If it did, then restricting the sample to eligibles could bias the estimates. As explained above, we predict eligibility based on annual earnings in the year prior to childbirth. It is possible that mothers' earnings could respond to the announcement of the reform. Recall, however, that predicted eligibility of mothers who have a child in the window surrounding the reform is based on annual earnings in the prior year. As the reforms were usually announced in December, it leaves the mother with only one month in which to increase her earnings enough to become eligible by our definition. Given this short time frame, there is limited scope for mothers to manipulate predicted eligibility status.

Figure 4 graphically illustrates there is no measurable change in predicted eligibility of mothers around the cut-off date. While there is some seasonal variation in earnings and thus in predicted eligibility, there is no jump in the fraction of predicted eligible mothers around the discontinuity, a finding which is confirmed with an RD regression in Table 1. Appendix Figure A2 and Table 1 show no significant eligibility changes for the other reform years, except for 1991. But in 1991, the estimate is actually negative, which is the opposite direction from what would be expected if mothers were manipulating their eligibility.

3.2.3 Covariate balance

If families time date of birth or change eligibility status in response to the reform, then we would expect to see changes in the distribution of pre-determined characteristics of the parents around the reform dates. Table 1 shows that there is little evidence to support this, both for the 1992 reform as well as for other reforms. Out of of the 24 estimates, 3 are significant at the 10% level or less, which is not much more than would be expected by chance. More importantly, the point estimates are small in magnitude and our RD estimates barely move when we include the characteristics in the regressions.

4 Results

A key advantage of RD is that results can be presented graphically, which make identification of the estimates more transparent. However, given the large number of outcomes we analyze, we cannot feasibly show graphs for every reform. Therefore, we first present results graphically for the 1992 reform in the paper. Results for the other expansions in parental leave are presented in table form, with graphs relegated to the Online Appendix.

4.1 First stage

The 1992 reform increased the number of paid weeks of parental leave by three, changing the entitlement from 32 weeks to 35 weeks. Figure 5a displays the number of days of paid leave taken by mothers in a window surrounding the reform. The graph includes births three months before and three months after the reform. In the graph, the birth date of the child has been normalized with the cutoff date of April 1, 1992 being labeled as zero. Each point on the graph is the average number of paid leave days taken by mothers over a one week birth interval. There is a sharp jump in the number of days of paid leave at the birth cutoff date.

To obtain a RD estimate, we adopt the following specification for this first stage regression and for subsequent reduced form regressions. We use daily data, include linear trends in birth day on each side of the discontinuity and employ triangular weights. To gain precision, we also include (pre-determined) control variables for mother's and father's years of education, mother's and father's age and age squared, parent's county of residence, marital status prior to birth and gender of the child. The estimated jump in this first stage regression is 22.7 days (s.e. = 2.7), which corresponds closely to the 3 week increase stipulated by the 1992 reform. This estimate confirms survey evidence suggesting 100% take up by mothers of the entire amount of paid leave benefits.

As mentioned previously, we only observe actual days of paid leave starting in 1992. In the results which follow, we therefore focus on reduced form estimates since we lack a first stage for earlier reforms. However, since take up is likely to have been close to 100%, these ITT estimates should be very close to the average treatment effects in magnitude.

4.1.1 Crowd out of unpaid leave

An important question is whether an increase in the number of paid leave days reduces the number of unpaid leave days. If so, then the intention of the reforms to increase the amount of time parents spend with children would only be partially achieved. Moreover, if the unpaid leave decreases as paid leave increases, there would be important income effects associated with the introduction of the reforms, as total income (labor earnings plus leave benefits) would rise after the reform.

Figure 5b graphs the fraction of mothers taking unpaid leave based on the birth date of their child. There is no evidence of any change in unpaid leave as a result of the reform. Table 2 presents RD estimates for the 1992 reform as well as the other expansion. All of the estimates are close to zero and statistically insignificant. Unlike much of the previous research, we are able to hone in on the counterfactual to paid leave because there is no evidence of crowd out of unpaid leave and there was 100% income replacement. This helps us interpret the expansions to paid parental leave as increases in the amount of time spent at home versus work, holding family income constant. Since there was almost no child care coverage for children under the age of 2 during this period (Mogstad and Havnes, 2011), the counterfactual to parental care is usually informal care, including relatives and nannies.

The fact that there is no crowd out of unpaid leave makes our results easier to interpret compared to prior research in other countries. Indeed, one possible explanation for why much of the previous literature either finds no effect or a negative effect of paid leave on child development is that there could be offsetting effects from a mother's increased time at home versus a reduction in family income.

4.1.2 Child achievement and schooling

One of the main arguments made for paid parental leave, and maternity leave in particular, is that it will have a positive impact on child development. The logic is that by reducing the amount of time spent at work, parents will have more time to invest in their children. However, whether this is true is controversial. The existing research on how maternal employment affects children's outcomes

is inconclusive, in part because it is difficult to control for the self-selection of mothers who choose to work and in part because shocks to employment usually coincide with large changes to income which could also affect child development.

The Norwegian parental leave setting is ideal to study how maternal employment in the first year of life affects later child outcomes, as it does not suffer from the same deficiencies as most of the prior studies. First, since take up is essentially 100% and the RD design generates variation which is as good as random around the cutoff, there is no self selection into which eligible mothers take more versus less paid leave after a reform. Of course, the estimates are only internally valid for the group of eligible mothers, but this is an important and sizable group (approximately three-fourths of all mothers). Second, the previous subsection revealed there is no crowd out of unpaid leave, which means the reforms changed the amount of time mothers spent working, but did not affect family resources in the short run. As we show in the next subsection, the reforms did not significantly change family resources (including income from all sources, and not just the mother's labor income) in the long run either. In short, this means that we have an exogenous change to maternal labor supply during the first year after birth, holding family income fixed.

We test for effects on two long-term outcomes related to a child's long-term development: written exams taken at the end of middle school (9th grade) and whether the child graduates from high school. The exam taken at the end of middle school is important for placement into high school. The scores range from 1 to 6, with a standard deviation approximately equal to 1. Figure 6a plots the average exam grade based on a child's birth date in a window surrounding the 1992 reform. Although exam scores are somewhat higher for children born earlier in the year, there is no jump in average scores at the reform cutoff date. Looking at Table 3, there is similarly little evidence that expansions in paid leave affects exam scores for the other reforms. The effect sizes are close to zero and only for the 1990 reform is the estimate significant. But the sign is negative, which if taken literally, suggests that increased parental leave harms a child academically.

Our next child outcome is whether a child drops out of high school. Because children born in 1992 have not yet had a chance to graduate from high school by the end of our data sample, we

cannot show a graph for this year. But we can report results for the other five reforms, which we do in column 2 of Table 3. The RD estimates are close to zero, indicating no effect of paid leave on high school graduation. These last results stand in stark contrast to those of Carneiro, Løken and Salvanes (2011), who find that the 1977 introduction of 18 weeks of paid leave and extension of job protection from 12 weeks to one year decreased high school dropout rates by a statistically significant 2.7 percentage points.

4.1.3 Labor market outcomes

Another primary motivation for government leave programs is the claim that it will ease the transition of mothers back to work after the birth of a child and thereby promote increased attachment to the labor market over the long run. We graphically examine these claims for the 1992 reform in Figure 6, panels (b) - (d). There is no visual evidence that mothers have an increased rate of returning to work two years after the birth of their child. There is also little visual evidence of increased labor market attachment in the long run. Total years of employment and the annuity of income over the next 14 years are unaffected by the 3 week increase in paid leave. The results shown graphically for 1992 are not unique to that reform. Table 4 looks at the same outcomes for all 6 reforms. While a few of the 24 coefficients are significant at the 10% level, the magnitudes are relatively small.

When we look at father's long term labor market attachment, we similarly find little evidence of an effect, either graphically for 1992 in Figure 6, panels (e) and (f), or for any of the other reform years in Table 4. None of the 12 coefficient estimates are statistically significant.

The hope of many proponents of paid leave expansions is that they would promote more within family gender equality in terms of labor force attachment. Panels (g) and (h) in Figure 6 and Table 5 reveal that the ratio of male to female employment and the ratio of male and female income annuities in the long run (14 years after) did not generally change in response to the reforms. One coefficient is significant at the ten percent level, but there is no obvious pattern for the remaining 11 insignificant coefficients. Since these extensions in paid leave increased the amount of time women

spent at home with newborn children, but did not affect men's leave taking, the reforms had the likely effect of widening the male-female childcare gap, at least in the first year of a child's life.

4.1.4 Family structure

We finally turn to outcomes related to family structure. An additional benefit often claimed for parental leave is that it supports and strengthens families. In panels (i) - (k) of Figure 6 we look at a variety of family outcomes for the 1992 reform. The 1992 reform had a small effect on completed fertility over the next 14 years. What about marital outcomes? Many Norwegians are not married at the birth of their child. Therefore, we can examine whether the reform increased entry into marriage or decreased exits out of marriage. We find a small effect for increased entry into marriage, but no effect for divorce in 1992.

Table 5 reveals that the two marginally significant effects (at the 10% significance level) found for the 1992 reform appear to be due to sampling variability. None of the family structure estimates for the other reform years are significant, and the signs of the coefficients do not follow a consistent pattern, with a mixture of positive and negative coefficients. Our reading of the evidence as a whole is that the extensions to paid leave did little to encourage fertility or stabilize marriage.

4.2 Robustness

Before moving on, we first present robustness results. Since there are many reforms, we show robustness for the 1992 reform; the results for other reform years are similarly robust. We begin with a series of specification checks in Appendix Table A2, re-estimating the full set of outcomes (i) omitting control variables, (ii) using separate quadratic trends on each side of the discontinuity instead of linear trends, (iii) and with a 1 week donut around the discontinuity. In each case, the results are remarkably robust, and continue to point towards little effect of the reforms on our long list of outcomes.

We next examine whether our results are sensitive to different window widths. Our baseline estimates use a window of 90 days on either side of the birth cutoff date. In Appendix Table A3 we

try a wider window of 120 days and a narrower window of 60 days. The results are similar regardless of window width, although as expected, the standard errors increase when the window is smaller.

An alternative to using a global estimator for RD is to use local linear regression. This has the advantage of being less influenced by outliers far away from the discontinuity. We report on this final robustness check in Appendix Table A4. Whether we use a bandwidth of 30 or 60 days, the results are similar to our baseline estimates.

5 Costs versus Benefits

To evaluate the argument for paid parental leave, costs and benefits must be compared on a per dollar basis. We begin by discussing the costs of the program, and then translate the benefit per reform estimates from the prior section into benefits per dollar spent.

5.1 Costs

The long-run costs of paid maternity leave depend not only on direct program expenditures, but also on any changes in future tax payments or social benefit receipt. Starting with the 1992 reform as an example, Figure 7 plots the RD graphs associated with expenditures, taxes and benefit payments per participant. As expected due to the strong take up of the extra weeks allowed by the reform, program expenditures jump discontinuously around the reform cutoff in panel (a). Table 6 estimates that expenditures rise by over $2,100 per mother. Since this was a three week reform, expenditures amount to an average transfer of roughly $140 for each workday taken off by mothers.

However, there could be indirect revenue effects from paid leave. For example, if paid leave causes mothers to work more in the future, this increase in revenues could help finance the leave program. Panel (b) in Figure 7 graphs the annuity of taxes paid for 15 years, starting with the year of the birth (year 0). Parental leave benefits are fully taxable at the individual level, so a portion of the leave payments is returned to the government in the form of higher tax payments in the year of the birth. However, there is also a reduction in taxes collected by the government due to the foregone earnings of the mother while on leave. Since there is 100% earnings replacement and empirically no

evidence of crowd out in unpaid leave, these two tax effects largely cancel each other out in the year of the birth. There is also little evidence of a sizable increase in tax receipt over the next 14 years.

Similarly, there could be future indirect costs if paid leave changes participation in other social welfare programs. We find no evidence that transfer payments increase over the 15 year period starting with the year of the birth. This null effect is visually shown in panel (c) and verified numerically in column 3 of Table 7. Hence, the total cost of the program per participant is close to the program expenditure amount, as future taxes and transfers are largely unaffected. Appendix Figures A15 to A17 reveal similar expenditure, tax and transfer patterns for the other reform years, which is perhaps not surprising given the results on family labor supply found in the previous section.

The total cost of the program depends on the number of participating mothers. In 1992, for example, 45,682 eligible women gave birth and participated in the program, at an estimated average cost of roughly $2,100 per mother. If a similar number of mothers continue to take paid leave in the future, this implies the yearly cost of the leave program rose by almost $100 million per year due to the 3 week expansion in paid benefits.

To put the magnitude of the program in perspective, consider the cumulative costs of the initial reform and all of the leave expansions. Based on all the reforms we study, we estimate a one week increase in paid leave costs taxpayers $687 dollars (in 2010 dollars) on average per birth. The initial 1977 reform gave mothers 18 weeks of paid leave and 1 year of job protection. The series of expansions in paid leave we study in this paper expanded paid leave by 17 weeks. Based on the number of births to eligible mothers in 1992, the annual cost of providing 35 weeks of paid leave is over $1 billion per year (in 2010 dollars). This represents almost 0.5% of GDP. Whether the initial 18 weeks is worth the roughly $565 million in annual costs can be debated, since Carneiro, Løken and Salvanes (2011) find some benefits associated with the initial reform. But the expansions nearly doubled this annual cost, without any corresponding changes in a broad range of child, family and gender equality outcomes. These expansionary costs imply a considerable increase in taxes, at a cost to economic efficiency.

5.2 Benefits per $1,000 spent

In the previous section, the potential benefits of paid parental leave were measured per reform. Some of the leave extensions were 2 weeks, while others were 3 or 4 weeks. In this section, we present the results for each reform in term of benefits per $1,000 of program expenditure. These comparisons are presented graphically in Figure 8.

As an example, consider Figure 8a which plots the effect on academic achievement at the end of 9th grade for each reform, standardized per $1,000 of expenditures. The x-axis measures weeks of paid leave, with the vertical red lines indicating each of the 6 extensions in paid leave. The horizontal black lines visually present the standardized estimates, while the dashed lines represent 95% confidence intervals. The graph makes clear that the reforms had little effect on children's academic achievement.

The other graphs in Figure 8 present similar illustrations of effect sizes per $1,000 spent for other outcomes. One advantage of presenting results in this manner is that it is easy to compare the effect of earlier reforms to later reforms. Modest expansions when a child is still young could be worthwhile, even if later, larger expansions are not. For example, one might think the benefits of breastfeeding are more important when an infant is less than 6 months old but that this benefit fades out over time. Similarly, one might think the longer a mother stays out of the labor force, the less will be her future attachment to the labor market. However, even modest expansions, from 18 to 20 weeks or from 20 to 22 weeks, have no significant effect on outcomes. The estimates are all close to zero and generally insignificant.

The first column of Table 7 presents the effect per $1,000 spent, averaged over all six reforms. None of the estimated average effects are large or statistically significant, even though the averaging generally results in smaller standard errors. Even if the average effect is statistically insignificant, one could imagine that the cumulative effect of all the reforms adds up to something which is significant. This is not the case. Column 2 in Table 7 shows that none of the cumulative effects are significant at the 5 percent significance level.[16]

[16] There have been small changes in the composition of which mothers take leave over our sample period. Reweighting the results by observable characteristics to adjust for this does not materially affect our findings.

6 Redistribution

Since there are no measurable benefits associated with extensions to paid leave, but large costs to taxpayers, it is important to consider the distributional effects of the program. Somewhat surprisingly, little attention has been paid in the previous literature to the redistributional effects of this social program.

As discussed previously, not all mothers are eligible for paid leave, since a requirement is that mothers have a minimum level of labor market earnings prior to the child's birth. Table 8 compares eligible versus ineligible mothers on a variety of dimensions. Mothers who are eligible for parental leave benefits are more educated, are married to higher educated men, and are more likely to be natives compared to ineligibles. Eligible mothers also have substantially higher family income compared to ineligible mothers, with disposable income being 60% higher in the year prior to birth. This income gap persists over time, with family income for eligibles being almost 20% higher on average compared to ineligible mothers in 2010.

The distributions of family income for eligible and ineligible mothers are presented in Figure 9. The first graph plots the distributions for family income in the year prior to the child's birth. The graph makes clear that that paid parental leave makes transfers to relatively well-off mothers. Not only is the mean of the eligible distribution farther to the right, but the left tail of the income distribution has a much larger mass for ineligibles. Twenty-five percent of the ineligible distribution has family income below $25,000 compared to only 10% of the eligible distribution. In addition, only 16% of the ineligible distribution has family income above $60,000 compared to 58% of the eligible distribution. These are large distributional disparities.

Even within the group of eligible mothers, the parental leave program is regressive. Leave benefits are tied to prior earnings and women with higher earnings reside in families with higher income. Since the program gave 100% income replacement up to a high threshold, the amount of the transfer increases with family income on average. The black dots in Figure 10 plot the pre-tax value of the leave transfer to mothers versus disposable income, defined as the sum of parent's labor income plus taxes and transfers. Both eligible and ineligible mothers are included in the graph. The pre-

tax transfer is highly regressive, with families earning less than $25,000 receiving $10,560 in paid leave on average compared to families earning greater than $60,000 receiving $21,349 on average. A considerable part of this regressivity is undone by Norway's progressive tax system, as leave benefits are fully taxable at the individual level. But as the grey dots in Figure 10 show, even after taxes, the program continues to favor middle and upper income families, with families earning more than $60,000 receiving roughly 75% more in after-tax leave benefits compared to families earning less than $25,000.

The differences in income between ineligibles and eligibles are even larger when comparing eligible mothers to the relatively poor men and women who never have children. The second panel of Table 8 documents that family income in 2010 is almost two and a half times larger in eligible families compared to childless individuals.

The comparisons in this section make clear that paid parental leave has negative redistribution properties. The program makes regressive transfers both from ineligibles to eligibles and within the group of eligible mothers. Since there is no crowd out of unpaid leave, the extra leave payments amount to a pure leisure transfer to middle and upper income families at the expense of the least well off in society.

7 Conclusion

Paid parental leave has gained greater salience in the past few decades as mothers have increasingly entered the workforce. Indeed, the median number of weeks of paid leave among OECD countries was 14 in 1980, but had risen to 42 by 2011. The usual arguments for paid leave are that there are important social goals or market failures addressed by the program. However, empirical evidence on the benefits and costs of paid leave is scarce and many key questions are still unanswered. First, does paid leave increase available parental time with children, or does it simply crowd out unpaid leave? Second, what effect does paid leave have on a broad range of child, parent and family outcomes? Third, how do any benefits compare relative to costs? And finally, are there progressive or regressive distributional effects?

We answer these questions in the context of Norway's parental leave program, focusing on parents' responses to a series of policy reforms which expanded paid leave from 18 to 35 weeks (without changing the length of job protection). Our first empirical result is that none of the reforms seem to crowd out unpaid leave. Since income replacement was 100% for most women, the reforms caused an increase in mother's time spent at home after birth, without a reduction in family income. This allows for a clean estimate of the effect of parental time on child and family outcomes, as there are no income effects to worry about. Second, the expansions had little effect on a wide variety of outcomes, including children's school outcomes, parental earnings and participation in the labor market in the short or long run, completed fertility, marriage or divorce. Third, paid parental leave has negative redistribution properties: it makes regressive transfers both from ineligibles to eligibles and within the group of eligible mothers. Since there was no crowd out of unpaid leave, the extra leave benefits amounted to a pure leisure transfer. Finally, the reforms extending paid leave had little impact on parents' future tax payments and benefit receipt. As a result, the large increases in public spending on parental leave imply a considerable increase in taxes, at a cost to economic efficiency.

Taken together, our finding suggest the case for extensions to paid maternity leave must rest on arguments other than improvements in observable dimensions of public benefits, redistribution or market failures. In a time of harsh budget realities, our findings are particularly relevant for countries that are considering future expansions or contractions in the duration of paid leave. One can argue whether a baseline level of paid parental leave or job protection is worth the cost. But the case for extended paid leave periods, which are prevalent in OECD countries, is weak. One would have to argue there are benefits not captured in the relatively rich set of outcome variables we look at. For example, our empirical strategy only identifies the responses of parents with children born around the time of the reforms, which might not be externally valid for parents having children in later periods. In a long-run perspective, when firms might adjust to the extensions of parental leave and women might change their investments in human capital and fertility behavior, the effects of the program could potentially be stronger. Unfortunately, costs and benefits of such long-run adjustments, if

they exist, are difficult to verify. By way of comparison, the opportunity costs to funding parental leave are substantial and verifiable: a growing body of evidence show that government subsidies to early education can yield high returns in the short and long run, particularly for disadvantaged children.[17]

[17] See e.g. the recent review of this literature by Almond and Currie (2011).

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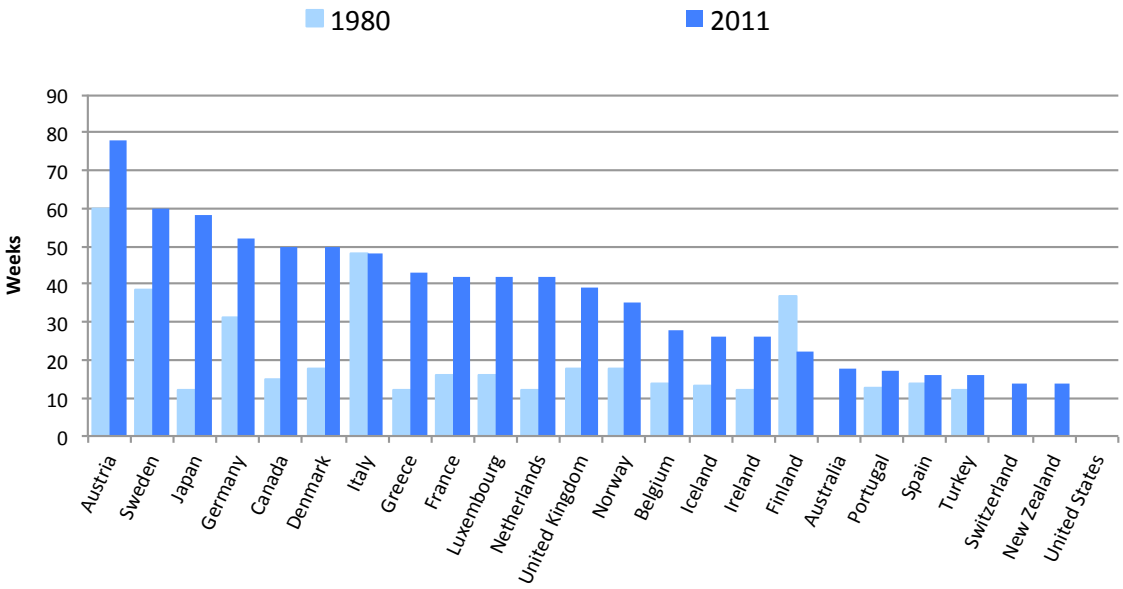
Figure 1: Weeks of paid parental leave across countries



Source: OECD Family database www.oecd.org/els/social/family/database.

Figure 2: Expansions in leave coverage in Norway

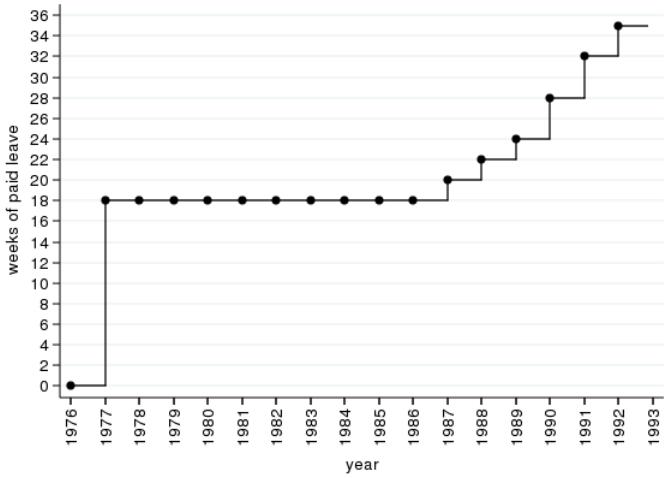
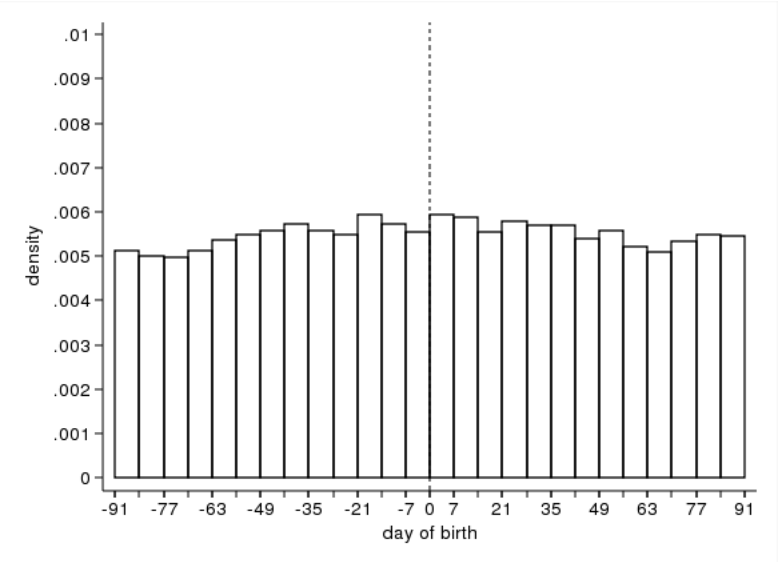
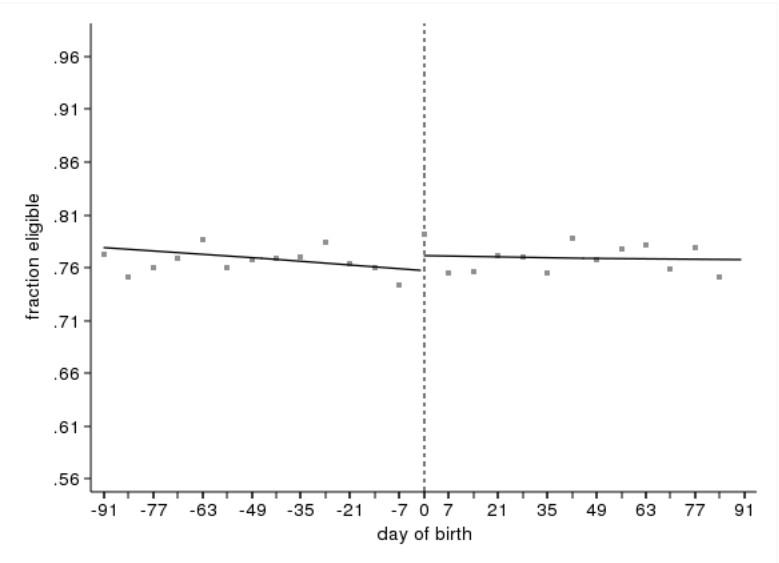


Figure 3: Fertility histogram, 1992 reform



Note: The histogram uses one week bins; the bin starting at 0 includes births in the first week post reform.

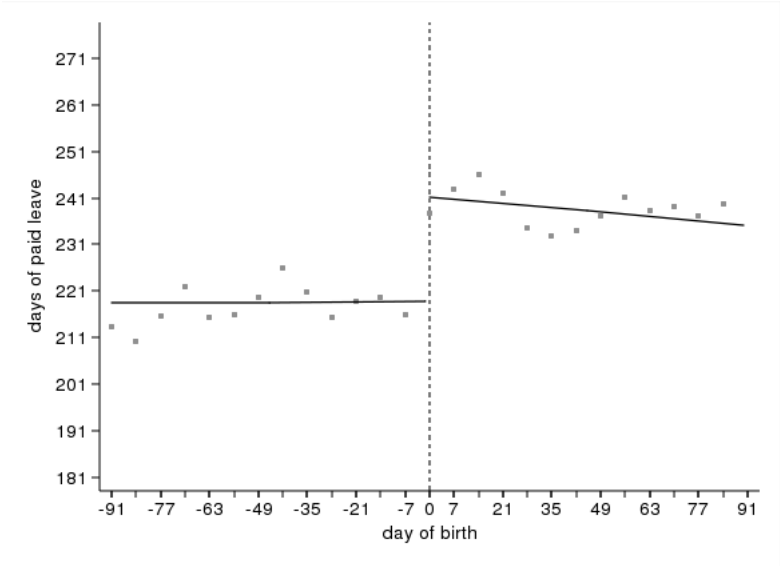
Figure 4: Eligibility, 1992 reform



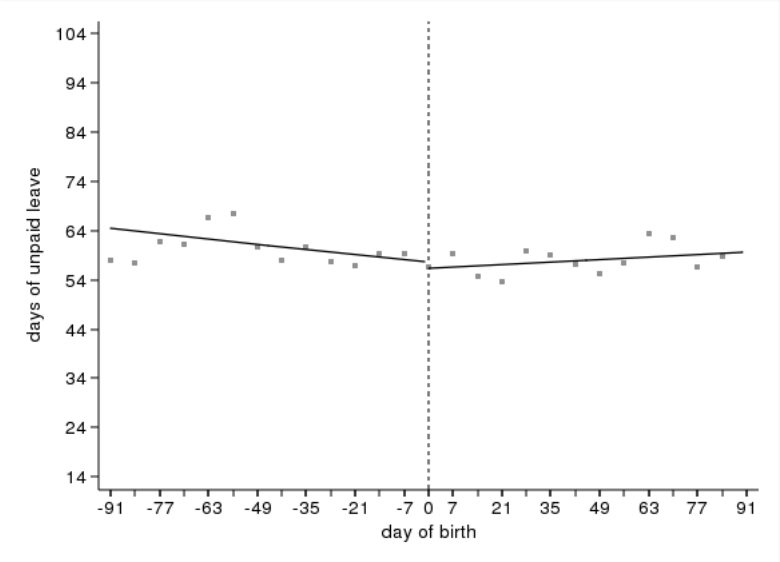
Notes: Each observation is the average fraction of eligible mothers in a one-week bin based on the birth date of the child. The dashed vertical line denotes the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data using triangular weights. The scale of the y-axis is +/-.5 standard deviations of the mean outcome.

Figure 5: Maternity leave

(a) Paid leave, 1992 reform



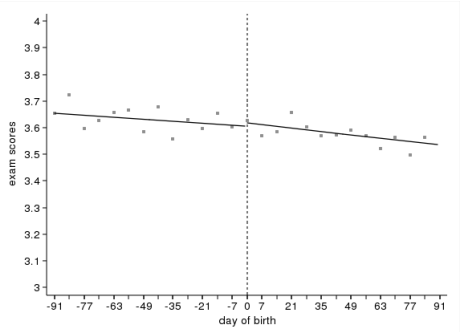
(b) Unpaid leave, 1992 reform



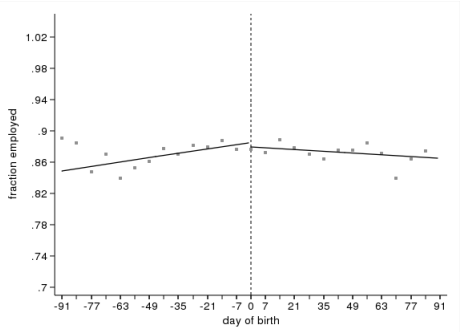
Notes: Each observation is the average days of paid or unpaid leave for mothers in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data using triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure 6: Child, mother, father and family outcomes, 1992 reform

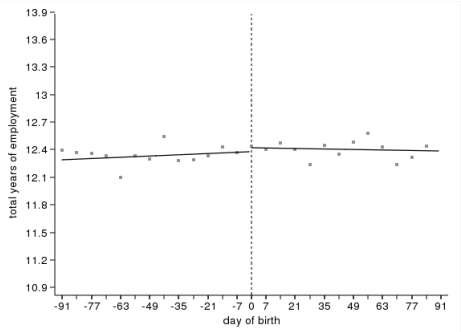
(a) Child: 9th grade exam



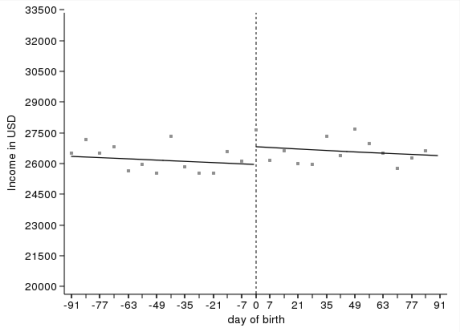
(b) Mother: Returned to work (by year 2)



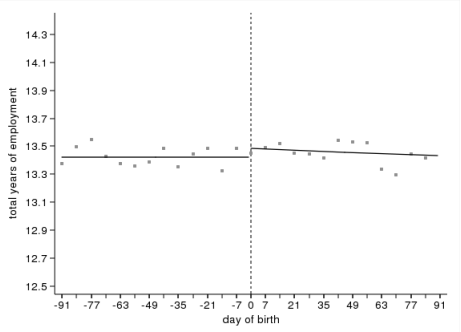
(c) Mother: Years employed (max 14)



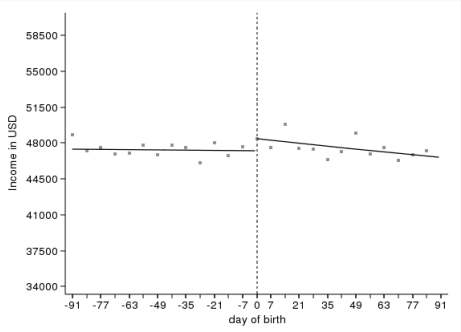
(d) Mother: Annuity of income (years 1-14)



(e) Father: Years employed (max 14)



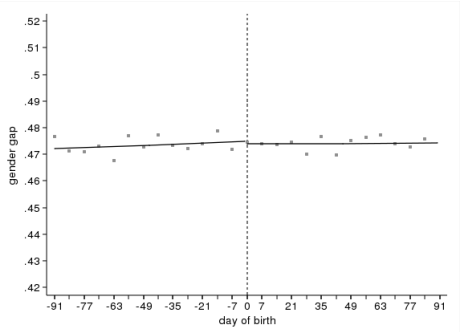
(f) Father: Annuity of income (years 1-14)



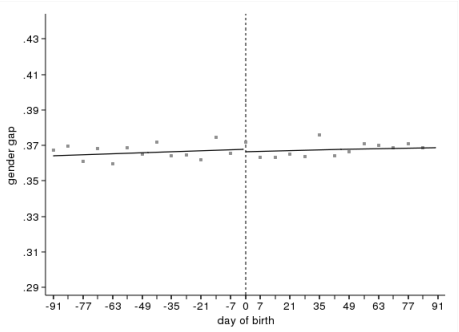
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure 6: Child, mother, father and family outcomes, 1992 reform (continued)

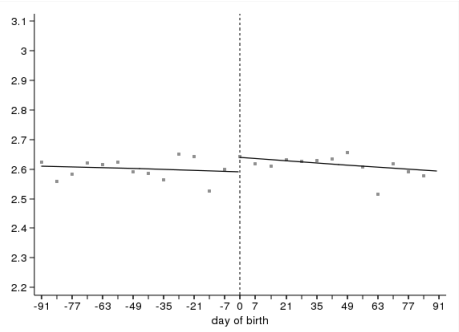
(g) Gender gap: Years employed (max 14)



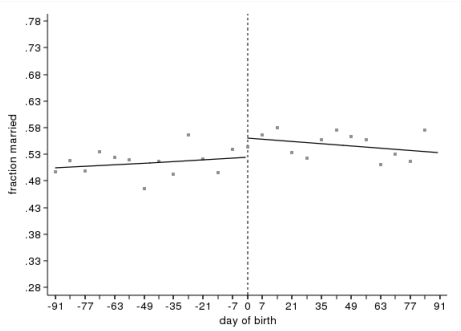
(h) Gender gap: Annuity of income (years 1-14)



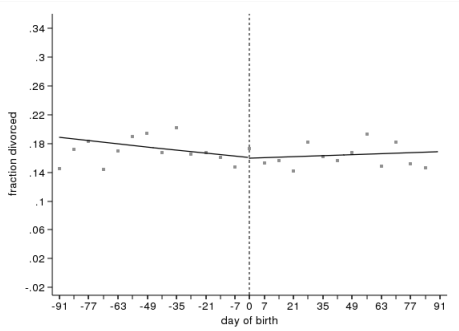
(i) Number of children (14 years after)



(j) Entry into Marriage (14 years after)



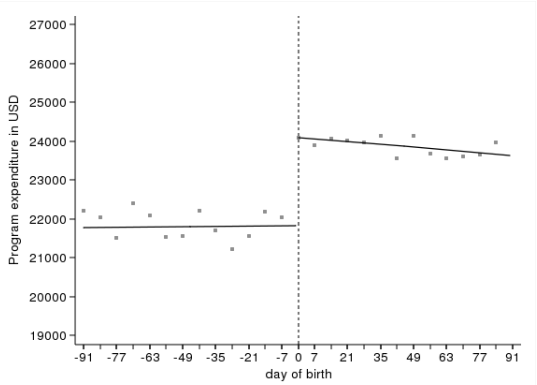
(k) Divorced (14 years after)



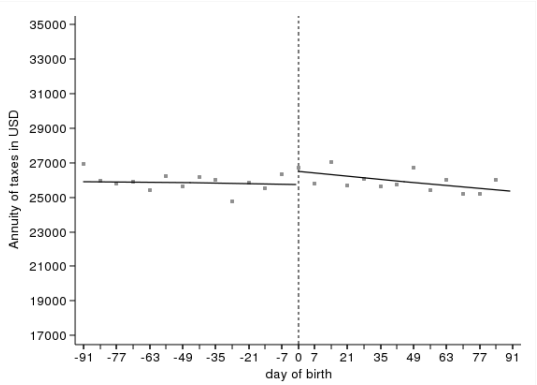
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure 7: Costs, taxes and benefits, 1992 reform

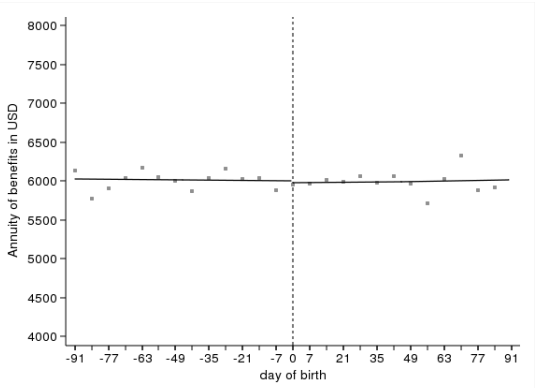
(a) Program expenditures for paid leave



(b) Family: Annuity of taxes paid (0-14 years after birth)



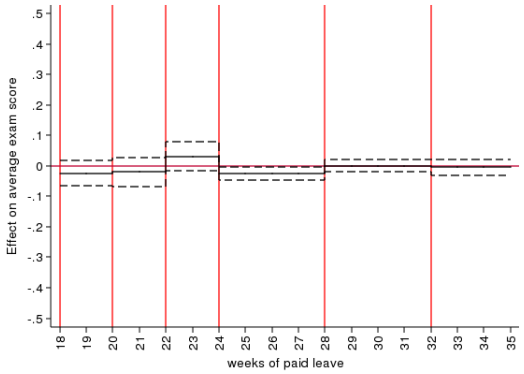
(c) Family: Annuity of benefits (0-14 years after birth)



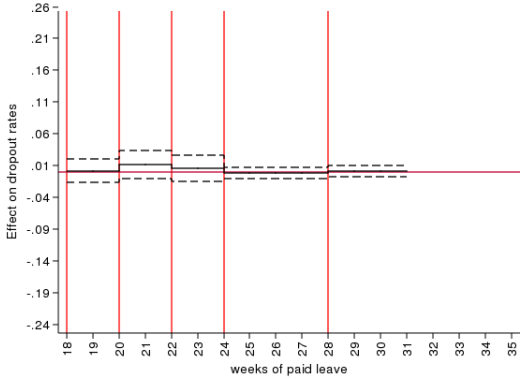
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure 8: Effects per $1,000 Spent

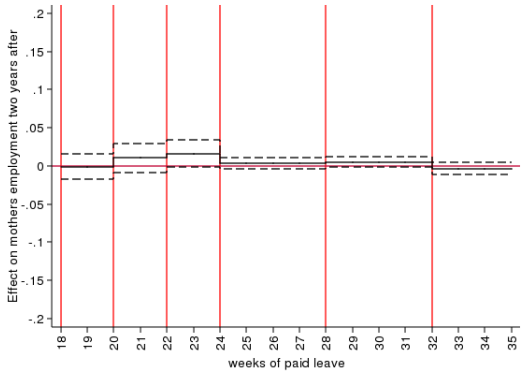
(a) Child: 9th grade exam



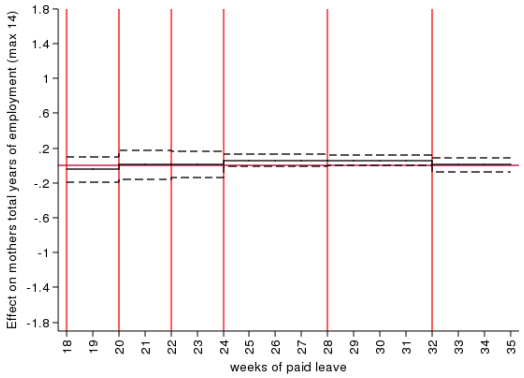
(b) Child: High school dropout



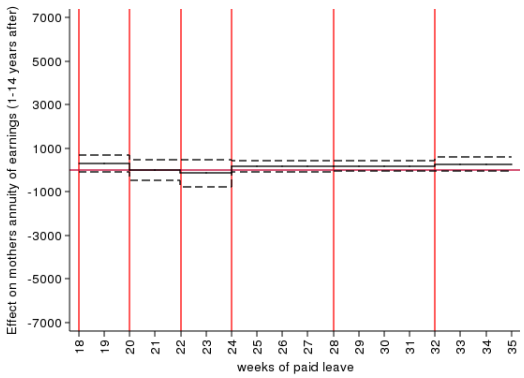
(c) Mother: Returned to work (by year 2)



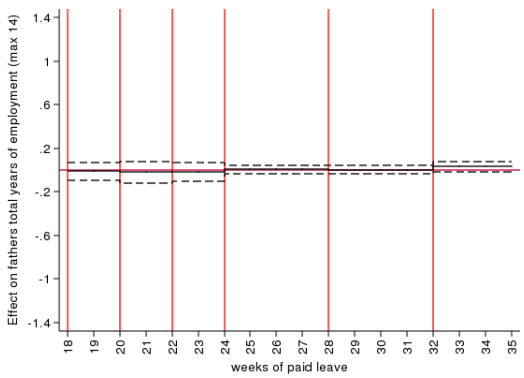
(d) Mother: Years employed (max 14)



(e) Mother: Annuity of income (years 1-14)



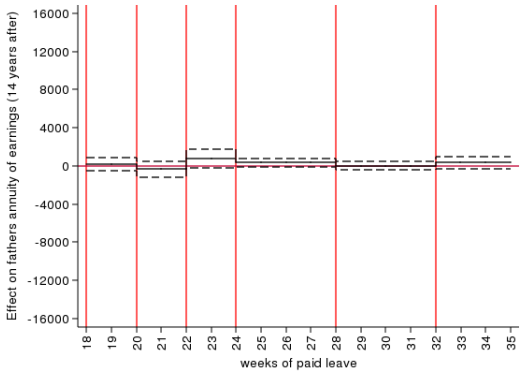
(f) Father: Years employed (max 14)



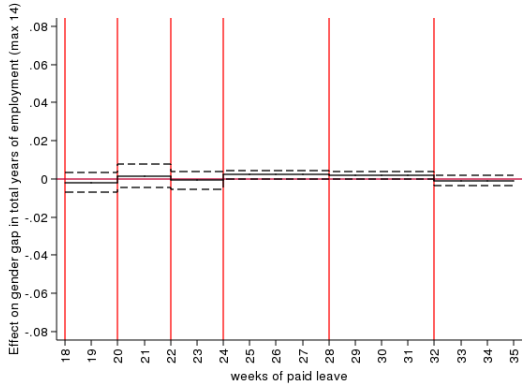
Notes: The estimated effects and standard errors for each of the six different reforms are reported per $1,000 increase in total costs. The vertical lines denote the different reforms in parental leave. The dashed lines represent 95% upper and lower bound confidence intervals. The scales of the y-axes are +/- 0.5 standard deviations of the mean outcome.

Figure 8: Effects per $1,000 Spent (continued)

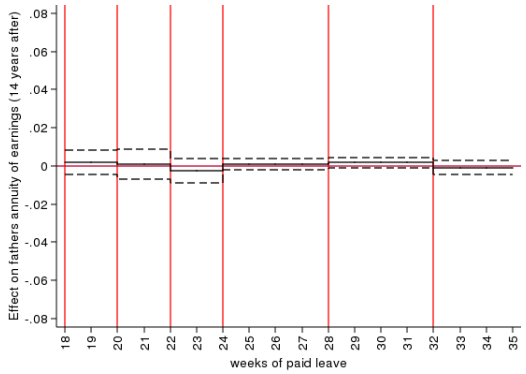
(g) Father: Annuity of income (years 1-14)



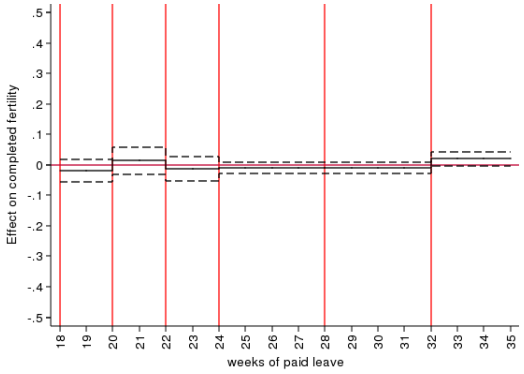
(h) Gender gap: Years employed (max 14)



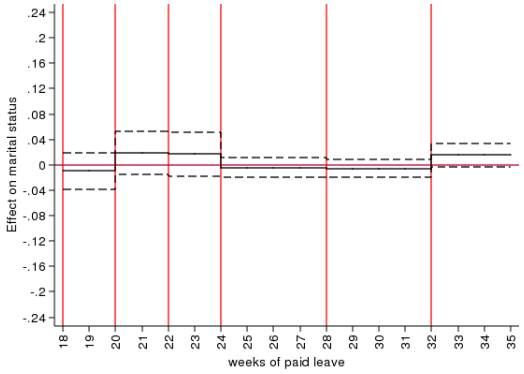
(i) Gender gap: Annuity of income (years 1-14)



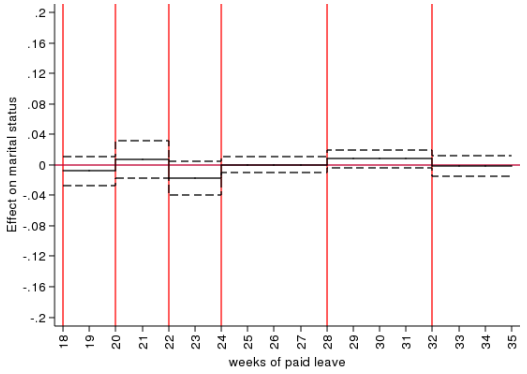
(j) Number of children (14 years after)



(k) Entry into marriage (14 years after)

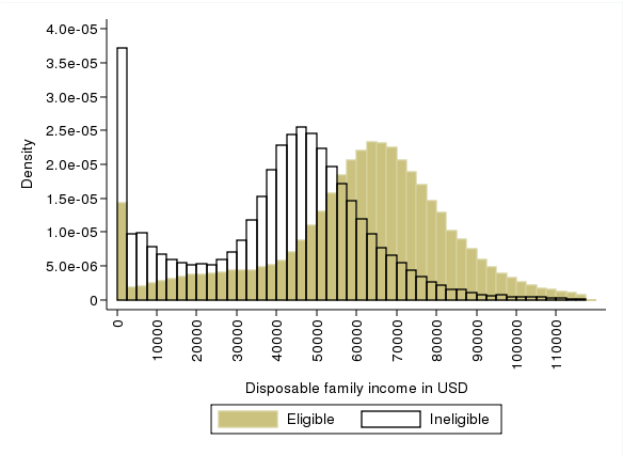


(l) Divorced (14 years after)



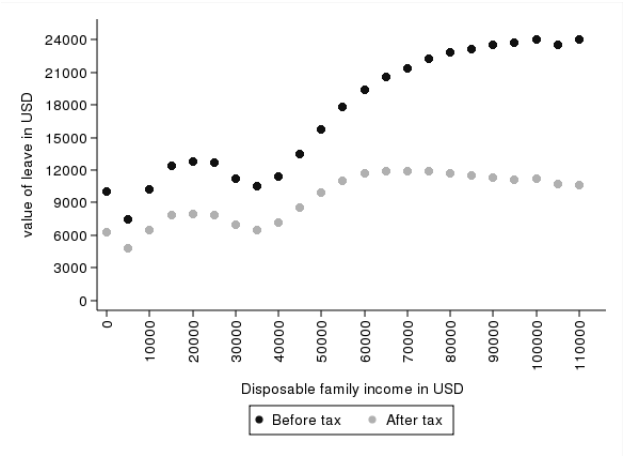
Notes: The estimated effects and standard errors for each of the six different reforms are reported per $1,000 increase in total costs. The vertical lines denote the different reforms in parental leave. The dashed lines represent 95% upper and lower bound confidence intervals. The scales of the y-axes are +/- 0.5 standard deviations of the mean outcome.

Figure 9: Distribution of disposable family income the year prior to birth, for eligible and ineligible mothers, 1987-1992



Notes: Sample of eligible and ineligible mothers aged 25-40 giving birth during the period 1987-1992. Disposable income is calculated as the sum of parents' combined labor income, parental leave benefits, non-taxable transfers, unemployment benefits and sick leave minus taxes paid and is measured the year before birth. Disposable income has been converted into year 2010 dollars. The top one percentile of disposable family income is excluded from the graph. Each bin in the histogram is $2,500 wide.

Figure 10: Average value of paid leave transfer by disposable family income, before and after tax, 1987-1992



Notes: Sample consists of all mothers who gave birth between 1987-1992. Disposable income is calculated as the sum of parents' combined labor income, parental leave benefits, non-taxable transfers, unemployment benefits and sick leave minus taxes paid and is measured the year prior to birth. Disposable income has been converted into year 2010 dollars. The top one percentile of disposable family income is excluded from the graph. Each observation in the graph plots the average value of the leave transfer for a $5,000 interval.

Table 1: Balance tests for parent's background characteristics

	RD estimates					Obs.	
Reform	Mother: Years of education	Father: Years of education	Mother: Age at birth	Father: Age at birth	Mother: Eligible	Educ. and Age	Eligible
1987	-0.135*	0.035	-0.152	-0.077	0.011	17,580	25,067
	(0.082)	(0.087)	(0.151)	(0.173)	(0.012)		
	[11.56]	[11.83]	[28.05]	[30.71]	[0.70]		
1988	-0.039	-0.001	-0.049	0.154	-0.001	19,310	26,225
	(0.077)	(0.081)	(0.147)	(0.170)	(0.012)		
	[11.54]	[11.79]	[27.99]	[30.70]	[0.74]		
1989	0.054	-0.041	-0.144	-0.253	0.015	20,093	26,768
	(0.075)	(0.080)	(0.142)	(0.165)	(0.011)		
	[11.63]	[11.85]	[28.39]	[31.07]	[0.75]		
1990	0.025	0.031	-0.204	-0.187	0.012	21,508	28,713
	(0.073)	(0.076)	(0.138)	(0.158)	(0.011)		
	[11.70]	[11.88]	[28.59]	[31.29]	[0.75]		
1991	-0.158**	-0.029	-0.083	-0.059	-0.024**	21,717	28,631
	(0.075)	(0.078)	(0.140)	(0.163)	(0.011)		
	[11.82]	[11.92]	[28.71]	[31.40]	[0.76]		
1992	0.095	0.066	0.019	0.302*	0.013	21,838	28,441
	(0.073)	(0.076)	(0.135)	(0.156)	(0.011)		
	[11.90]	[12.01]	[29.03]	[31.73]	[0.77]		

Notes: All coefficients are estimated using a linear RD model with triangular weights using children born 3 months before and 3 months after the reform. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1,**p<0.05.

Table 2: Crowd out of unpaid leave, all reforms

	RD estimate	
Reform	Unpaid leave (days)	Obs.
1987	-2.041	17,580
	(4.059)	
	[102.14]	
1988	1.341	19,310
	(3.658)	
	[95.06]	
1989	-3.920	20,093
	(3.473)	
	[80.82]	
1990	0.039	21,508
	(2.991)	
	[61.82]	
1991	-2.726	21,717
	(2.721)	
	[56.49]	
1992	-1.124	21,838
	(2.564)	
	[59.13]	

Notes: All coefficients are estimated using a linear RD model and triangular weights using children born 3 months before and 3 months after the reform. Control variables include father and mother's years of education and age at birth, martial status and county of residence the year prior to birth as well as an indicator of the child's gender. Unpaid leave measured in days. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1,**p<0.05.

Table 3: Child outcomes, all reforms

	RD estimates		Obs.	
Reform	9th grade exams	High school dropout	Exam	Dropout
1987	-0.036	-0.003	16,650	17,580
	(0.033)	(0.014)		
	[3.54]	[0.35]		
1988	-0.025	0.015	18,290	19,310
	(0.031)	(0.014)		
	[3.51]	[0.35]		
1989	0.038	0.007	19,200	20,093
	(0.030)	(0.013)		
	[3.52]	[0.33]		
1990	-0.067**	-0.005	20,056	21,508
	(0.029)	(0.013)		
	[3.52]	[0.33]		
1991	-0.000	0.003	20,459	21,717
	(0.029)	(0.013)		
	[3.52]	[0.33]		
1992	-0.010	.	20,427	.
	(0.029)			
	[3.60]			

Notes: All coefficients are estimated using a linear RD model and triangular weights using children born 3 months before and 3 months after the reform. Control variables include father and mother's years of education and age at birth, martial status and county of residence the year prior to birth as well as an indicator of the child's gender. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1,**p<0.05.

Table 4: Labor market outcomes of mothers and fathers, all reforms

	RD estimates					
Reform	Mother: Returned to work (by year 2)	Mother: Years employed (max 14)	Mother: Annuity of income (years 1-14)	Father: Years employed (max 14)	Father: Annuity of income (years 1-14)	Obs.
1987	-0.002	-0.064	458	-0.016	259	17,580
	(0.013)	(0.114)	(309)	(0.064)	(563)	
	[0.76]	[11.54]	[19,137]	[13.35]	[37,642]	
1988	0.013	0.011	1	-0.026	-447	19,310
	(0.012)	(0.106)	(301)	(0.064)	(535)	
	[0.80]	[11.66]	[20,205]	[13.32]	[39,175]	
1989	0.020*	0.014	-183	-0.021	953	20,093
	(0.011)	(0.095)	(389)	(0.055)	(631)	
	[0.84]	[11.95]	[22,014]	[13.37]	[40,892]	
1990	0.009	0.165*	437	0.017	903	21,508
	(0.010)	(0.090)	(346)	(0.055)	(633)	
	[0.85]	[12.14]	[23,442]	[13.38]	[43,016]	
1991	0.014	0.169*	493	0.003	55	21,717
	(0.010)	(0.088)	(333)	(0.055)	(637)	
	[0.86]	[12.27]	[24,680]	[13.41]	[45,206]	
1992	-0.007	0.016	566	0.062	715	21,838
	(0.009)	(0.082)	(346)	(0.053)	(731)	
	[0.87]	[12.37]	[26,421]	[13.44]	[47,477]	

Notes: All coefficients are estimated using a linear RD model and triangular weights using children born 3 months before and 3 months after the reform. Control variables include father and mother's years of education and age at birth, martial status and county of residence the year prior to birth as well as an indicator of the child's gender. Annuities are measured in year 2010 dollars. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1,**p<0.05.

Table 5: Labor market gender gaps, fertility, marriage and divorce, all reforms

	RD estimates					Obs.		
Reform	Gender gap: Years employed (max 14)	Gender gap: Annuity of income (years 1-14)	Number of Children (14 years after)	Entry into Marriage (14 years after)	Divorced (14 years after)	Gender gaps, number children	Entry into Marriage	Divorced
1987	-0.003	0.003	-0.030	-0.015	0.012	17,580	7,761	9,819
	(0.004)	(0.005)	(0.029)	(0.023)	(0.015)			
	[0.45]	[0.34]	[2.59]	[0.60]	[0.14]			
1988	0.002	0.001	0.017	0.024	-0.009	19,310	9,346	9,964
	(0.004)	(0.005)	(0.028)	(0.022)	(0.016)			
	[0.46]	[0.35]	[2.60]	[0.59]	[0.16]			
1989	-0.001	-0.003	-0.016	0.021	0.022	20,093	9,414	10,679
	(0.003)	(0.004)	(0.026)	(0.022)	(0.014)			
	[0.46]	[0.36]	[2.60]	[0.56]	[0.16]			
1990	0.006*	0.002	-0.026	-0.011	-0.001	21,508	10,227	11,281
	(0.003)	(0.004)	(0.026)	(0.021)	(0.015)			
	[0.47]	[0.36]	[2.62]	[0.55]	[0.17]			
1991	0.005	0.005	-0.024	-0.016	-0.022	21,717	11,171	10,546
	(0.003)	(0.004)	(0.026)	(0.020)	(0.016)			
	[0.47]	[0.36]	[2.60]	[0.55]	[0.17]			
1992	-0.002	-0.002	0.042*	0.034*	0.003	21,838	10,761	11,077
	(0.003)	(0.004)	(0.025)	(0.020)	(0.015)			
	[0.47]	[0.37]	[2.61]	[0.53]	[0.17]			

Notes: All coefficients are estimated using a linear RD model and triangular weights using children born 3 months before and 3 months after the reform. Control variables include father and mother's years of education and age at birth, martial status and county of residence the year prior to birth as well as an indicator of the child's gender. Annuities are measured in year 2010 dollars. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1,**p<0.05.

Table 6: Taxes, benefits and program expenditures, all reforms

	RD estimates			
Reform	Annuity of taxes paid (years 0-14)	Annuity of benefits received (years 0-14)	Program expenditures	Obs.
1987	786*	39	1,532***	17,580
	(457)	(101)	(133)	
	[21,647]	[5,630]	[11,273]	
1988	-53	77	1,257***	19,310
	(360)	(102)	(147)	
	[22,117]	[5,811]	[12,805]	
1989	437	82	1,236***	20,093
	(444)	(93)	(153)	
	[22,924]	[5,869]	[14,601]	
1990	743*	-82	2,714***	21,508
	(427)	(93)	(168)	
	[23,936]	[5,928]	[16,354]	
1991	274	-98	2,797***	21,717
	(401)	(95)	(202)	
	[24,784]	[5,956]	[19,512]	
1992	385	-33	2,132***	21,838
	(518)	(97)	(219)	
	[25,917]	[5,998]	[22,881]	

Notes: All coefficients are estimated using a linear RD model and triangular weights using children born 3 months before and 3 months after the reform. Control variables include father and mother's years of education and age at birth, martial status and county of residence the year prior to birth as well as an indicator of the child's gender. Annuities and expenditures are measured in year 2010 dollars. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1,**p<0.05.

Table 7: Average and cumulative effects, all reforms

Outcomes	Average effect per $1,000 spent	Cumulative effect for all reforms	Comparison mean
Child outcomes:			
9th grade exam	-0.007	-0.042	[3.4]
	(0.019)	(0.045)	
High school dropout	0.003	.015	[0.34]
	(0.009)	(.019)	
Mother's labor market outcomes:			
Returned to work (by year 2)	0.005	0.030*	[0.68]
	(0.007)	(0.017)	
Years employed (max 14)	0.018	0.107	[10.8]
	(0.061)	(0.149)	
Annuity of income (years 1-14)	126	755	[19,765]
	(206)	(504)	
Father's labor market outcomes:			
Years employed (max 14)	-0.002	-0.012	[13.1]
	(0.036)	(0.088)	
Annuity of income (years 1-14)	212	1,272	[41,016]
	(365)	(894)	
Gender gap:			
Years employed (max 14)	0.000	0.002	[0.43]
	(0.002)	(0.005)	
Annuity of income (years 1-14)	0.000	0.002	[0.33]
	(0.003)	(0.007)	
Family outcomes:			
Number of children (14 years after)	-0.003	-0.017	[2.7]
	(0.016)	(0.040)	
Entry into marriage (14 years after)	0.005	0.032	[0.54]
	(0.013)	(0.032)	
Divorced (14 years after)	0.002	0.012	[0.17]
	(0.009)	(0.022)	

Notes: The first column reports the average effect over all the reforms (1987-1992) per $1,000 increase in total cost. The second column reports the cumulative effect over all the reforms (1987-1992). The dropout variable is only available for the 1987-1991 reforms. Costs and annuities are measured in year 2010 dollars. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1.**p<0.05.

Table 8: Comparing eligible mothers to ineligibles and those without kids

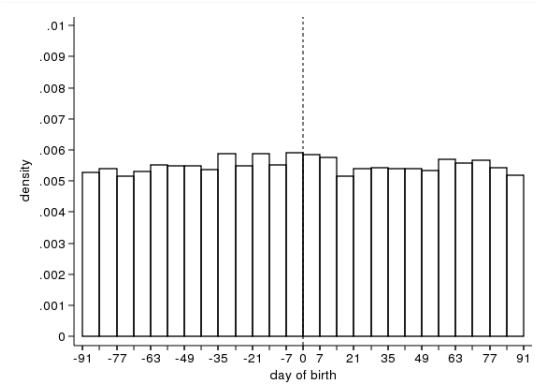
	Sample: with kids	
	Eligible Mothers	Ineligible Mothers
Mother characteristics:		
Age at birth	30.1	30.0
Norwegian citizen	.962	.835
Dropout	.405	.637
High school	.249	.189
College degree	.346	.174
Father characteristics:		
Age at birth	32.5	33.1
Norwegian citizen	.956	.877
Dropout	.377	.492
High school	.303	.271
College degree	.320	.238
Family characteristics:		
Married before birth	.581	.686
Sum of disposable income in 2010	104,114	88,335
Sum of disposable income year prior to birth	61,215	37,935
	Sample: no kids by 2010	
	Men	Women
Age	51.1	51.5
Norwegian citizen	.916	.919
Dropout	.440	.402
High school	.303	.214
College degree	.257	.384
Married	.235	.281
Sum of disposable income in 2010	43,570	42,910

Notes: The top panel uses a sample of mothers age 25-40 giving birth during the period 1987-1993. There are 220,368 eligible mothers and 61,180 ineligible mothers in this sample. The bottom panel uses a sample of men and women age 42-63 who have not had a child by 2010. There are 110,812 men and 69,351 women in this sample. Disposable income is calculated as the sum of parents' labor income, paid parental leave, non-taxable transfers, unemployment benefits and sick leave minus taxes paid and is measured in year 2010 dollars.

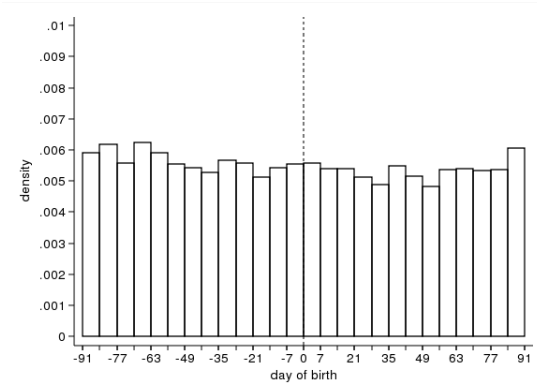
Online Appendix: Additional figures

Figure A1: Fertility

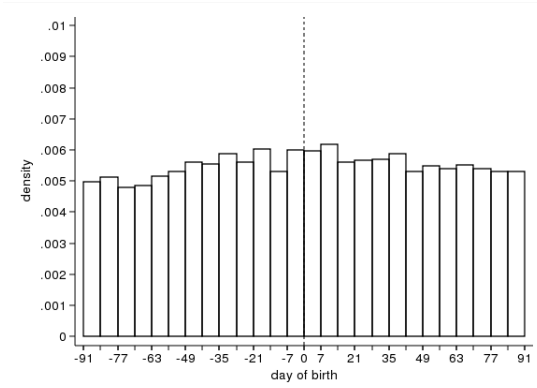
(a) reform May 1st 1987



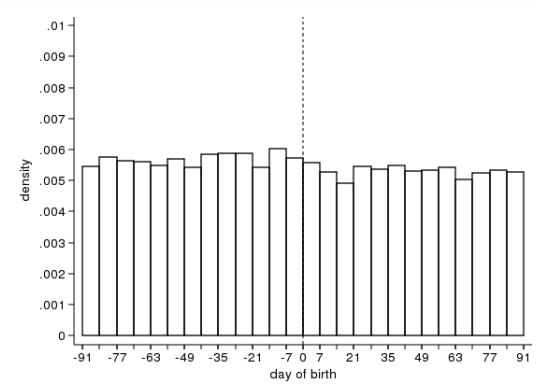
(b) reform July 1st 1988



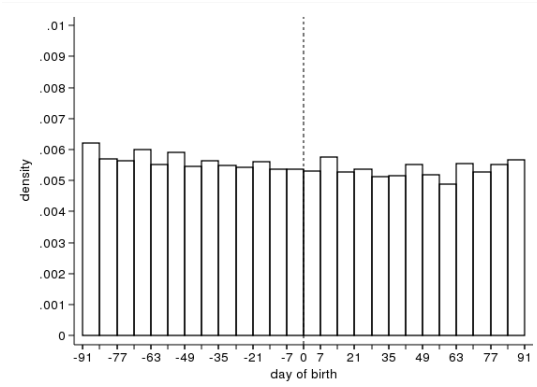
(c) reform April 1st 1989



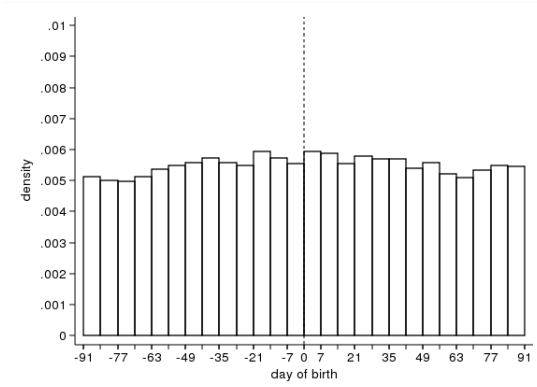
(d) reform May 1st 1990



(e) reform July 1st 1991



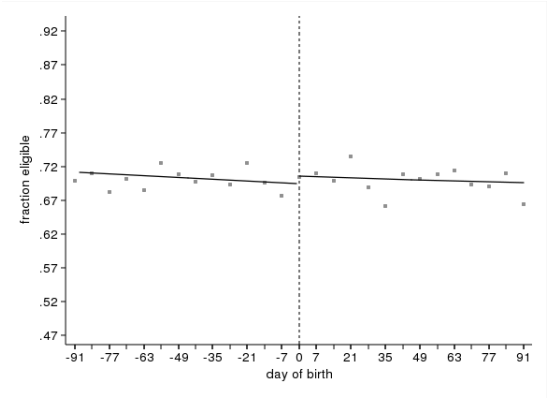
(f) reform April 1st 1992



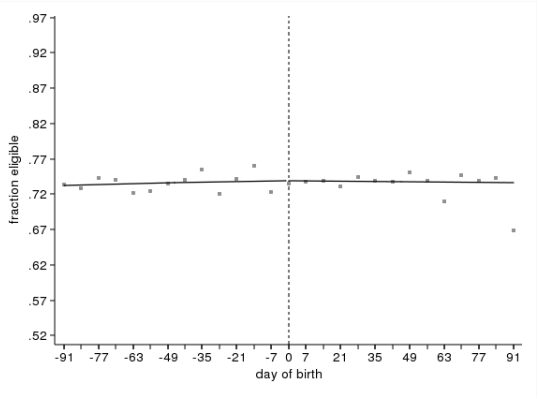
Note: The histogram uses one week bins with the bin starting at 0 includes the births in the first week post reform.

Figure A2: Eligibility

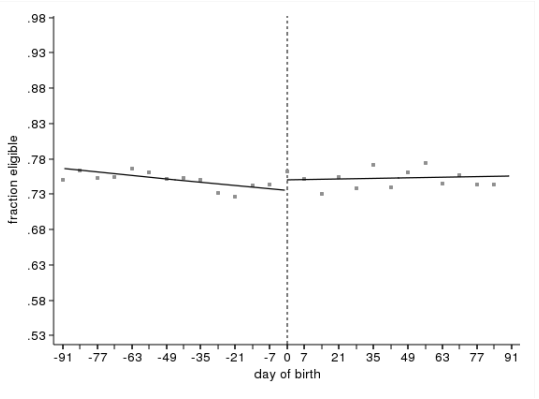
(a) reform May 1st 1987



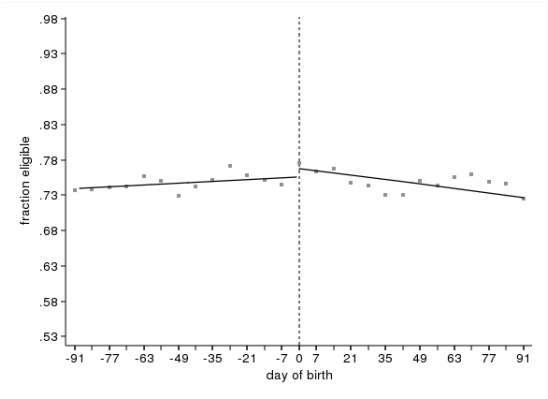
(b) reform July 1st 1988



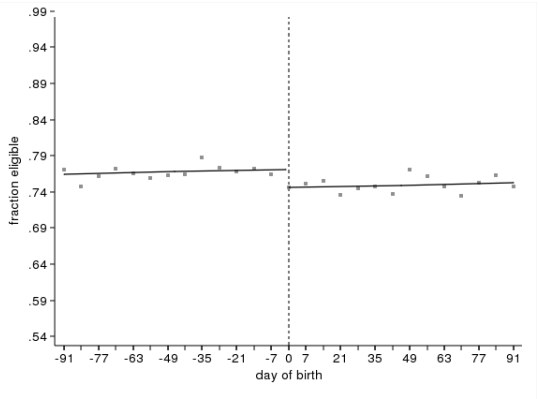
(c) reform April 1st 1989



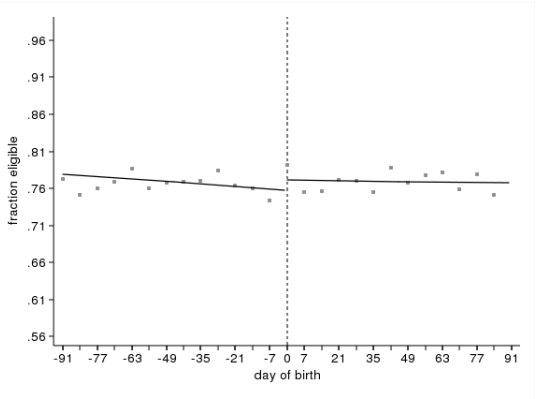
(d) reform May 1st 1990



(e) reform July 1st 1991



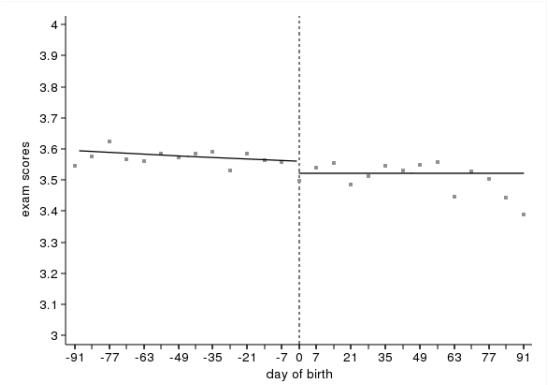
(f) reform April 1st 1992



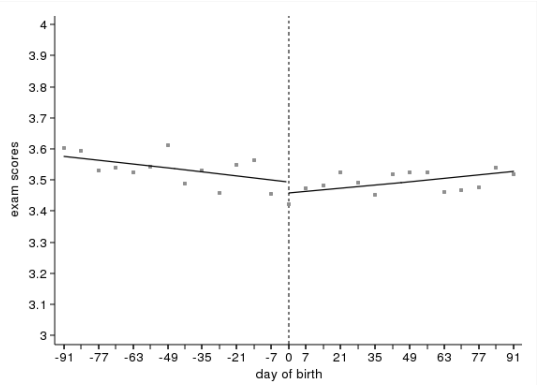
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A3: Child: 9th grade exam

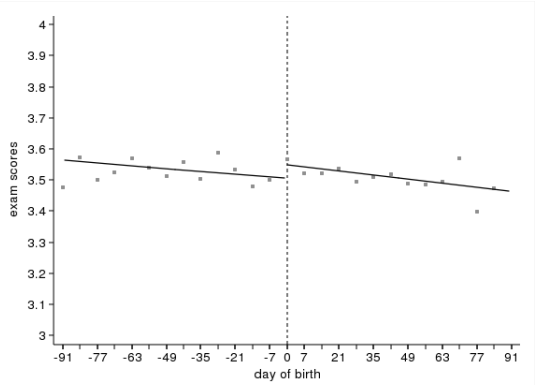
(a) reform May 1st 1987



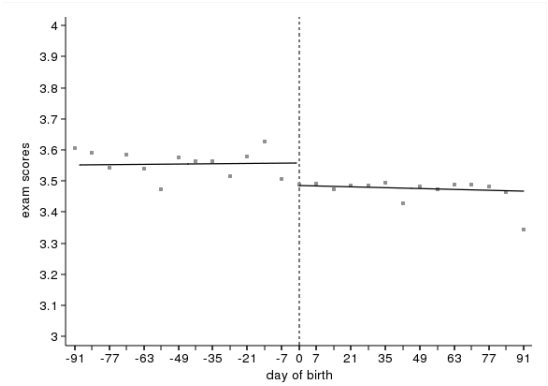
(b) reform July 1st 1988



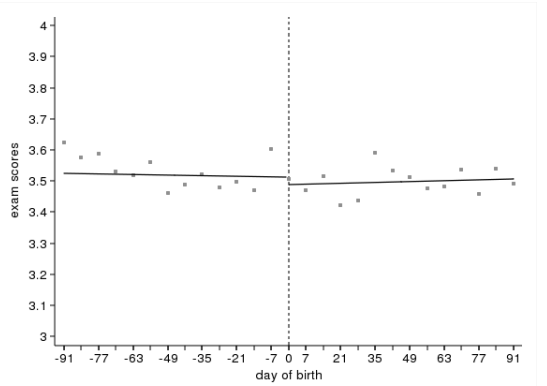
(c) reform April 1st 1989



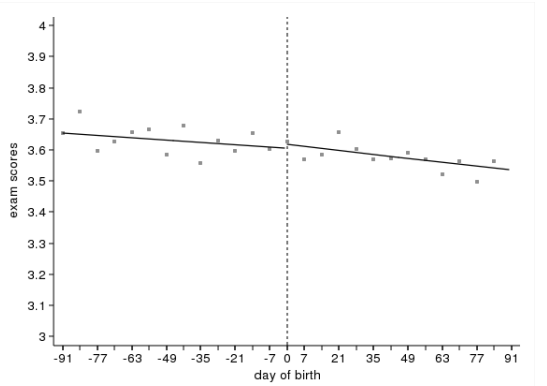
(d) reform May 1st 1990



(e) reform July 1st 1991



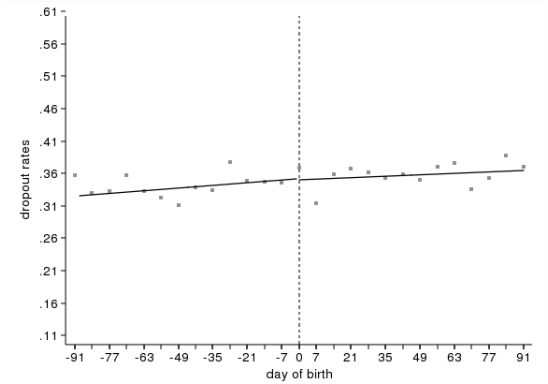
(f) reform April 1st 1992



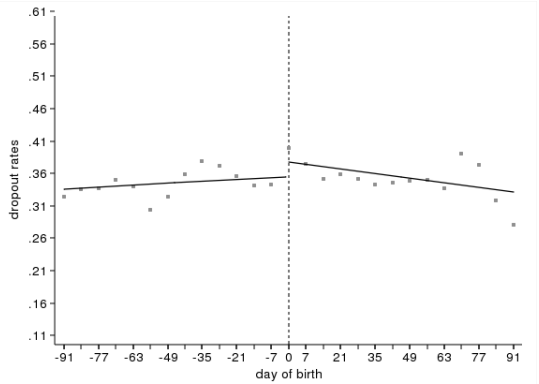
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A4: Child: High school dropout

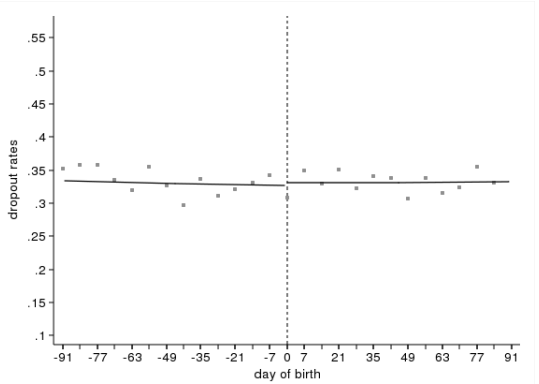
(a) reform May 1st 1987



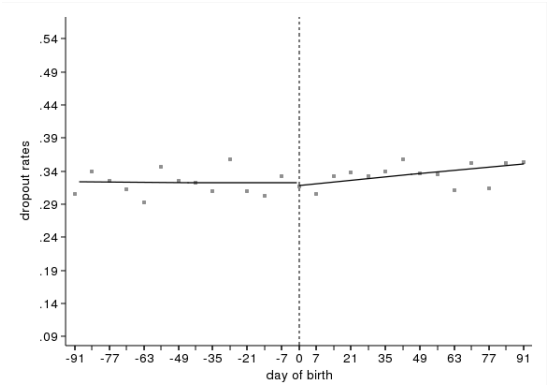
(b) reform July 1st 1988



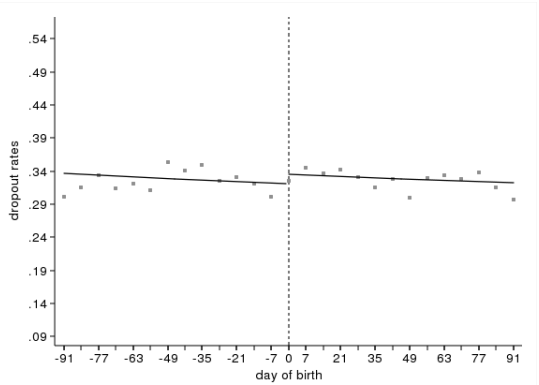
(c) reform April 1st 1989



(d) reform May 1st 1990



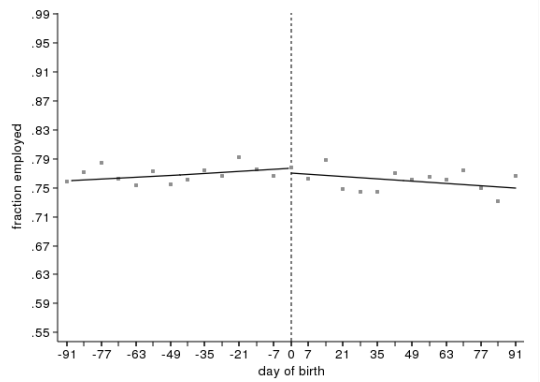
(e) reform July 1st 1991



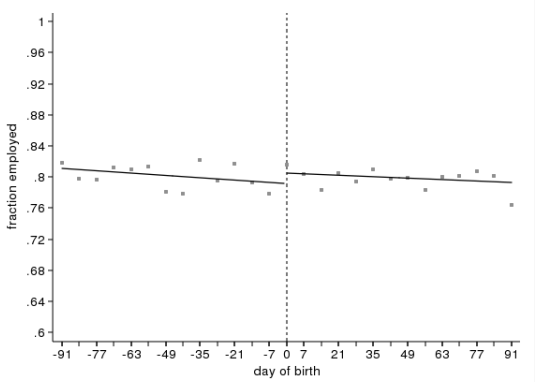
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A5: Mother: Returned to work (by year 2)

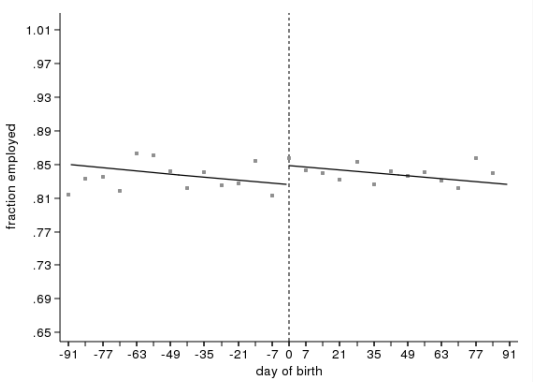
(a) reform May 1st 1987



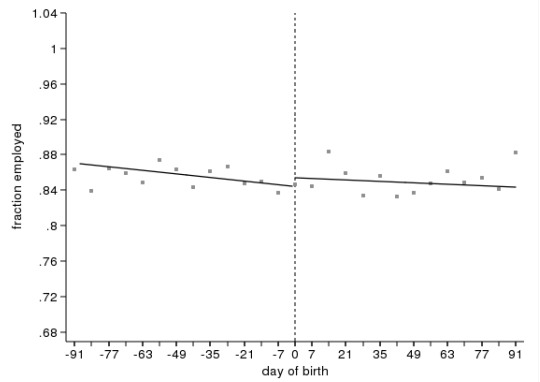
(b) reform July 1st 1988



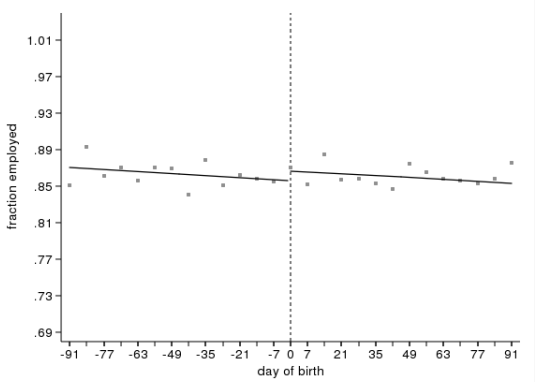
(c) reform April 1st 1989



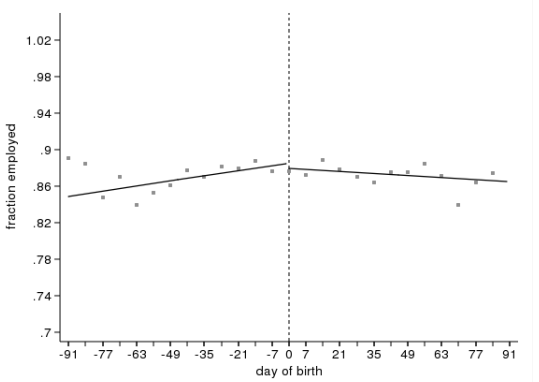
(d) reform May 1st 1990



(e) reform July 1st 1991



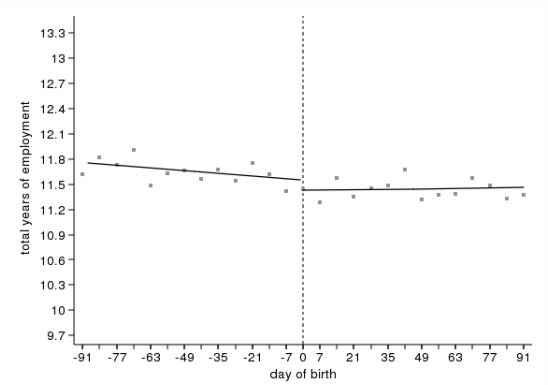
(f) reform April 1st 1992



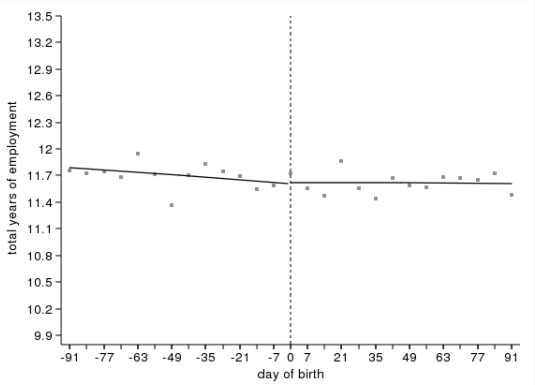
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A6: Mother: Years Employed (max 14)

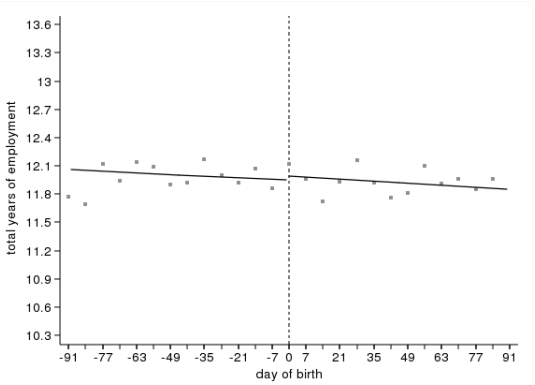
(a) reform May 1st 1987



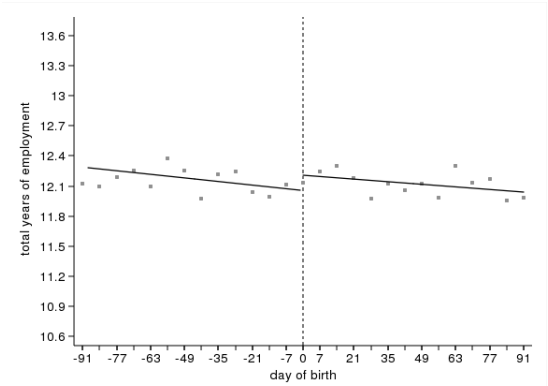
(b) reform July 1st 1988



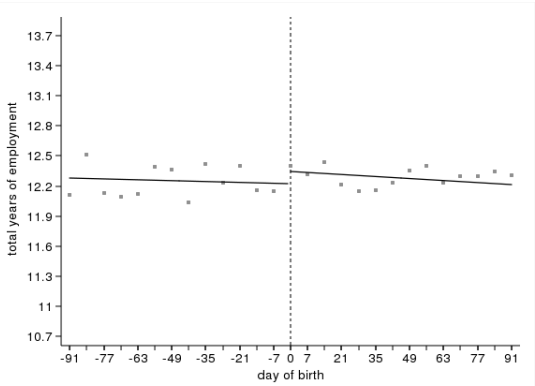
(c) reform April 1st 1989



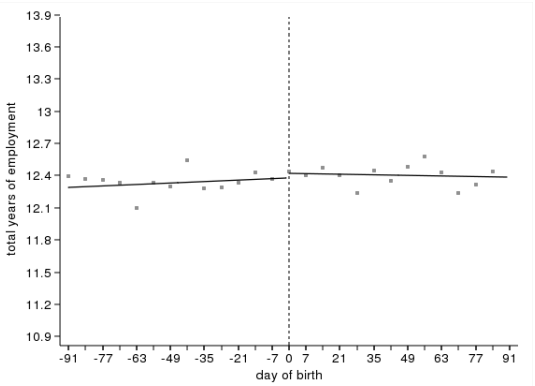
(d) reform May 1st 1990



(e) reform July 1st 1991



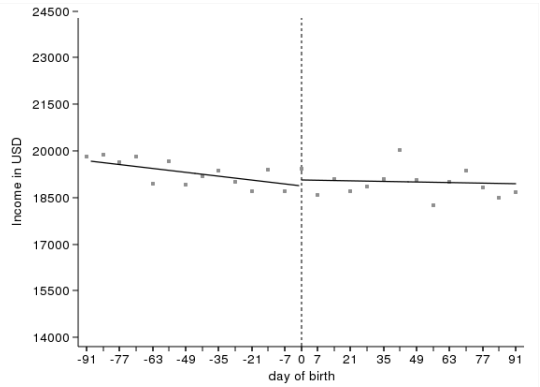
(f) reform April 1st 1992



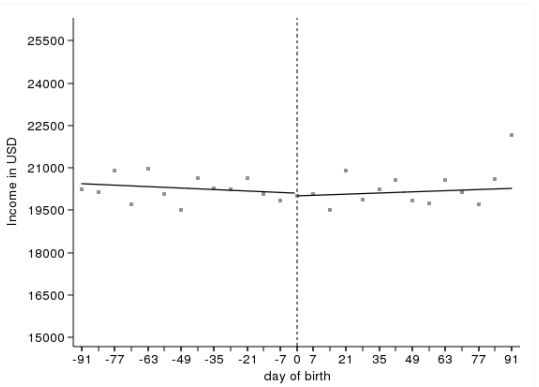
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A7: Mother: Annuity of income (years 1-14)

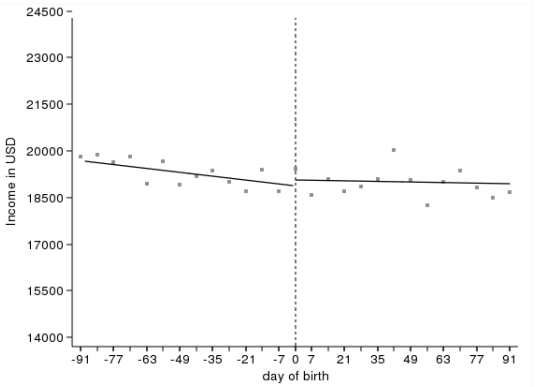
(a) reform May 1st 1987



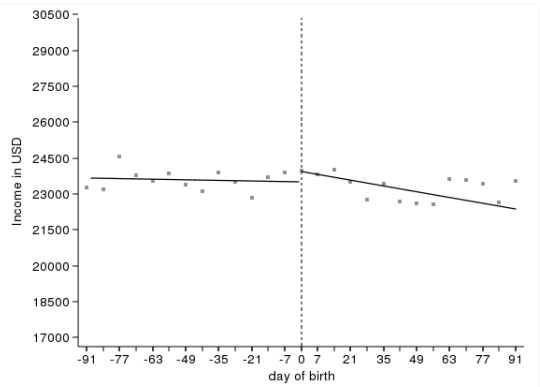
(b) reform July 1st 1988



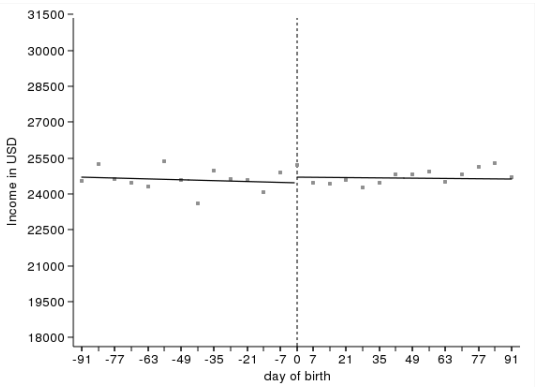
(c) reform April 1st 1989



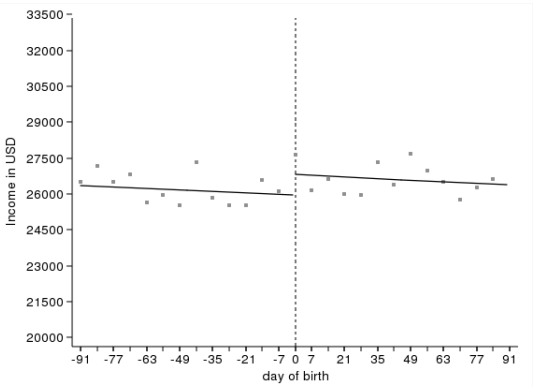
(d) reform May 1st 1990



(e) reform July 1st 1991



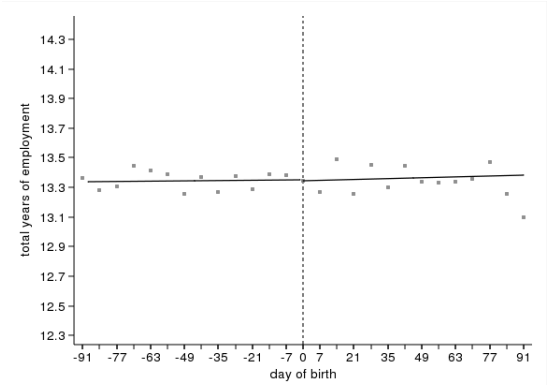
(f) reform April 1st 1992



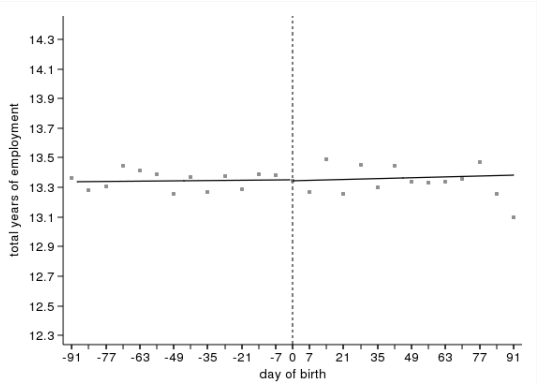
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A8: Father: Years employed (max 14)

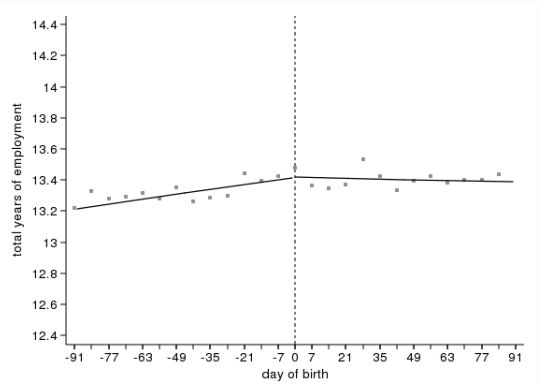
(a) reform May 1st 1987



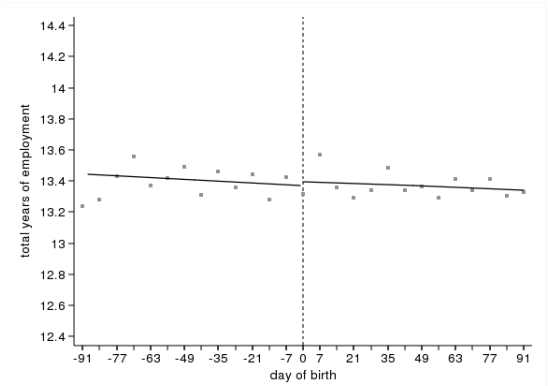
(b) reform July 1st 1988



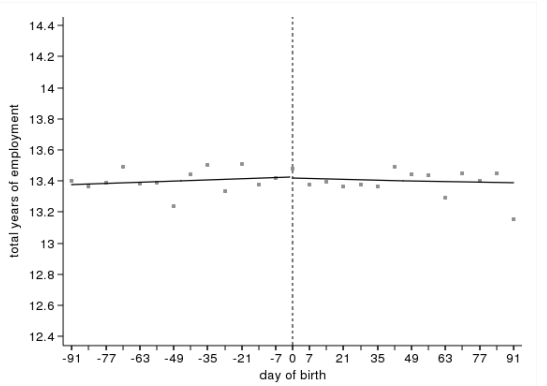
(c) reform April 1st 1989



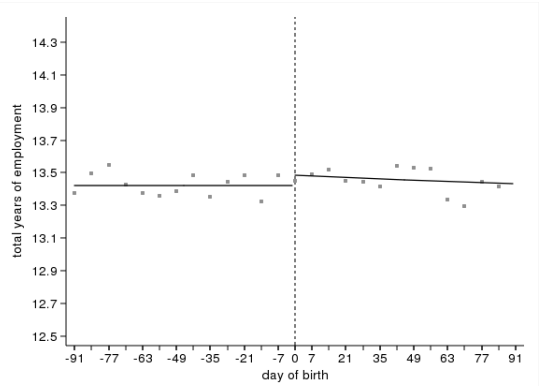
(d) reform May 1st 1990



(e) reform July 1st 1991



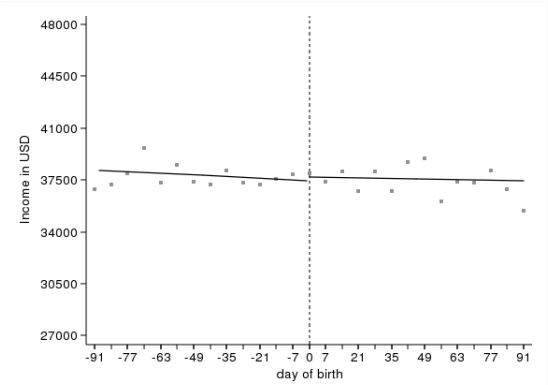
(f) reform April 1st 1992



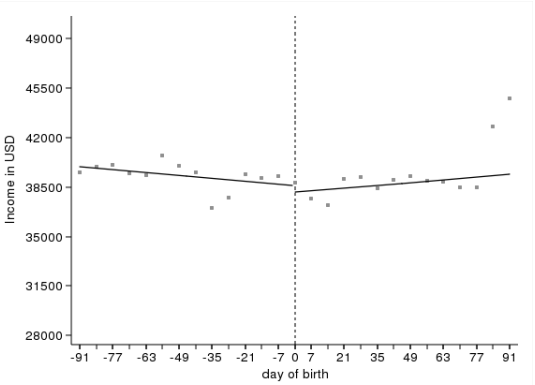
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A9: Father: Annuity of income (years 1-14)

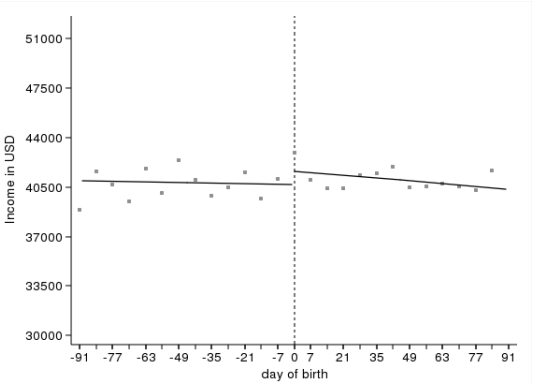
(a) reform May 1st 1987



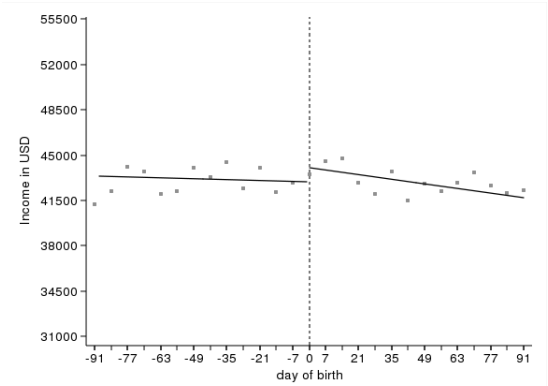
(b) reform July 1st 1988



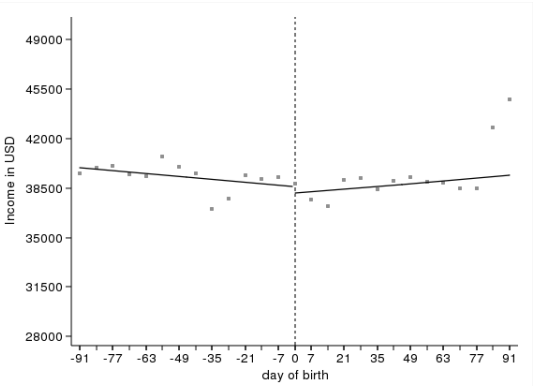
(c) reform April 1st 1989



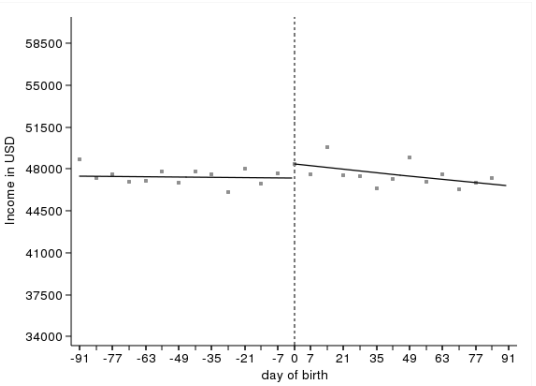
(d) reform May 1st 1990



(e) reform July 1st 1991



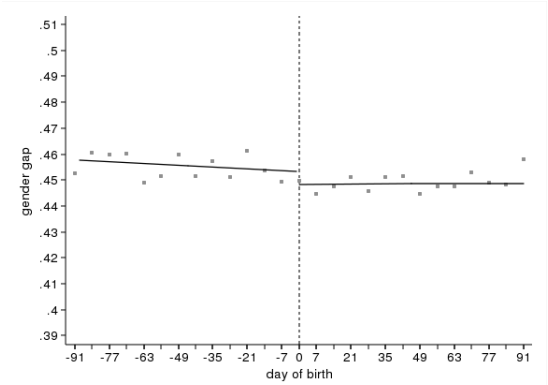
(f) reform April 1st 1992



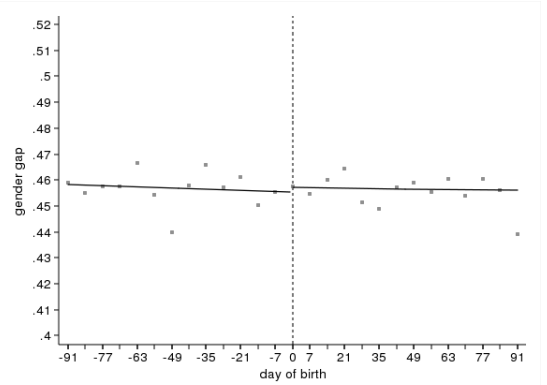
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A10: Gender gap: Years employed (max 14)

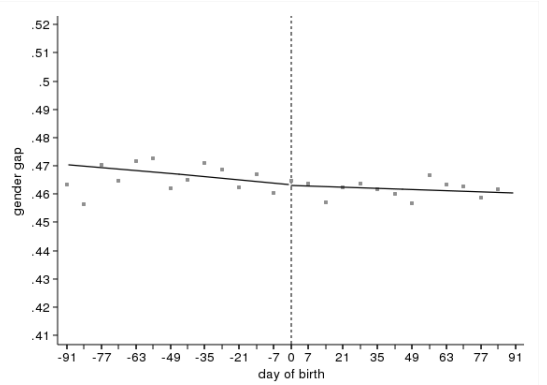
(a) reform May 1st 1987



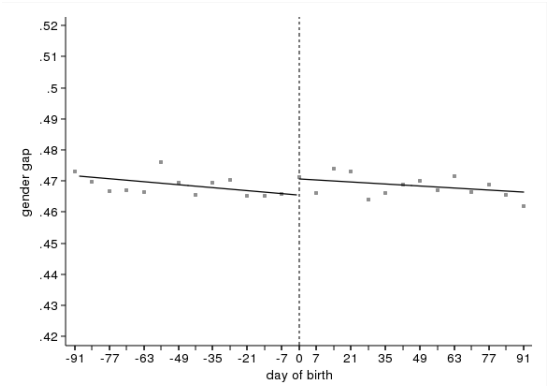
(b) reform July 1st 1988



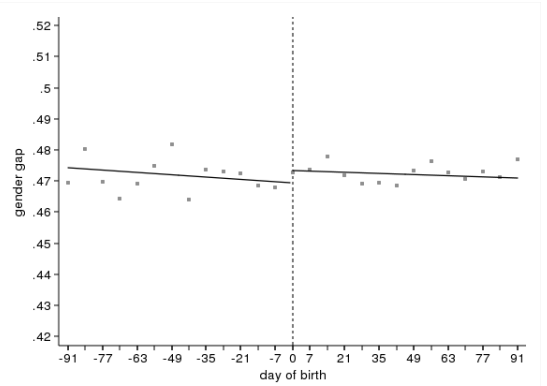
(c) reform April 1st 1989



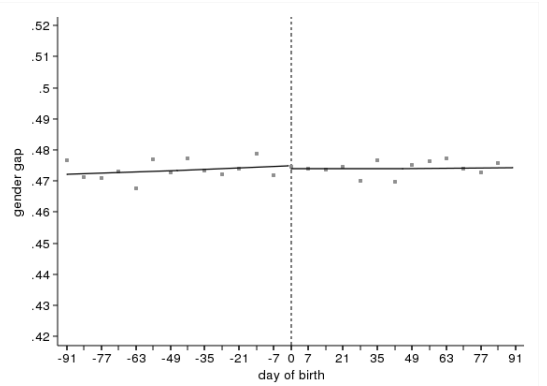
(d) reform May 1st 1990



(e) reform July 1st 1991



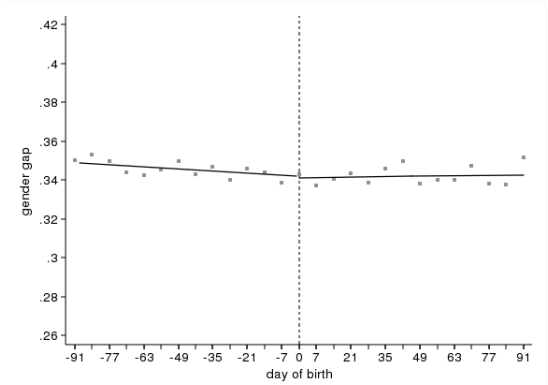
(f) reform April 1st 1992



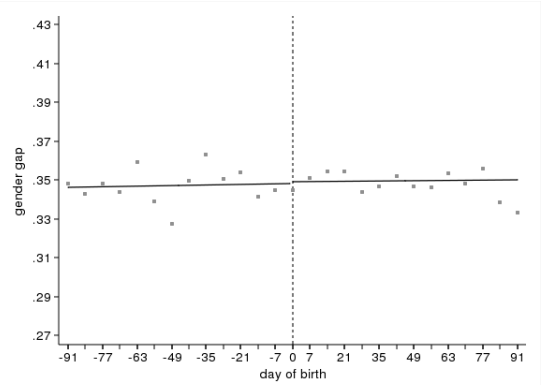
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A11: Gender gap: Annuity of income (years 1-14)

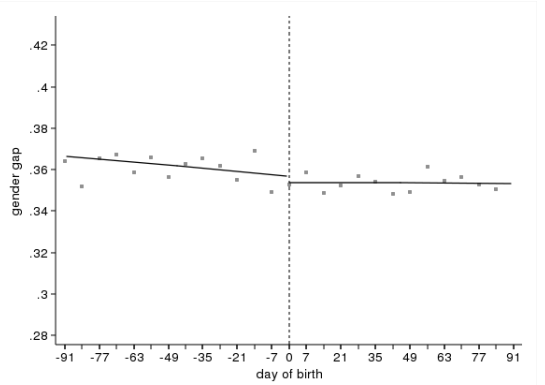
(a) reform May 1st 1987



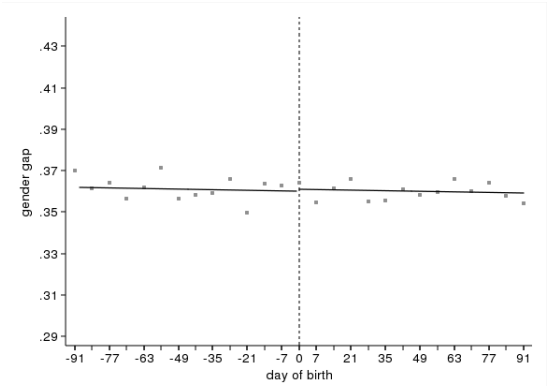
(b) reform July 1st 1988



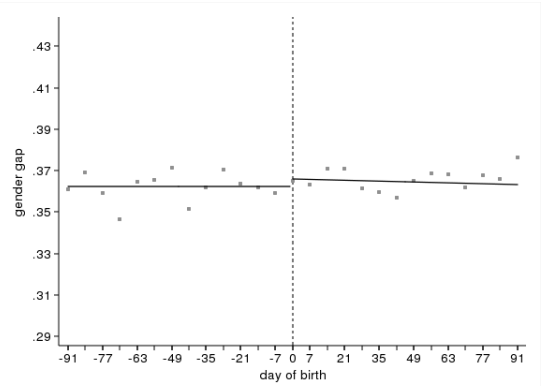
(c) reform April 1st 1989



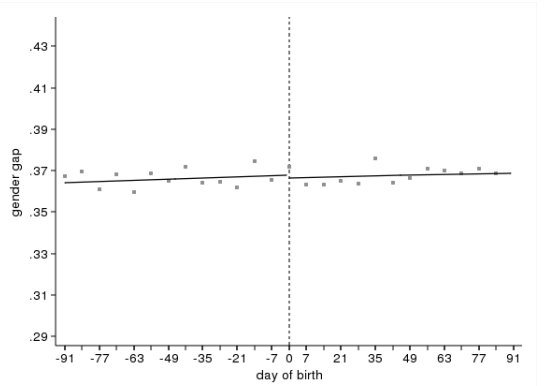
(d) reform May 1st 1990



(e) reform July 1st 1991



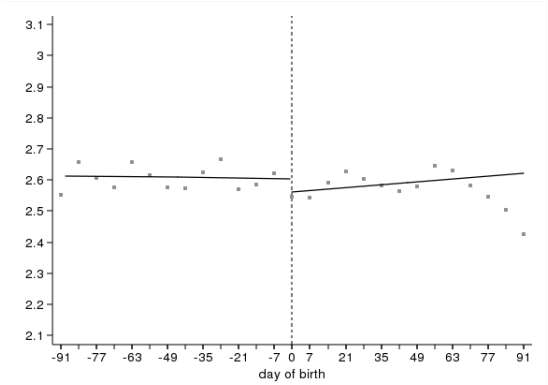
(f) reform April 1st 1992



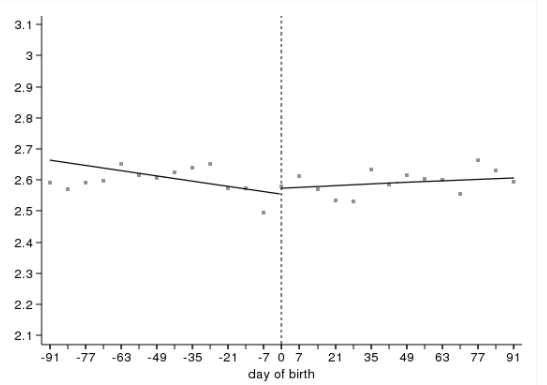
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A12: Number of children (14 years after)

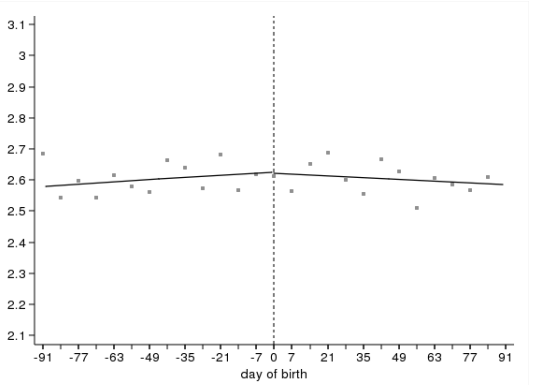
(a) reform May 1st 1987



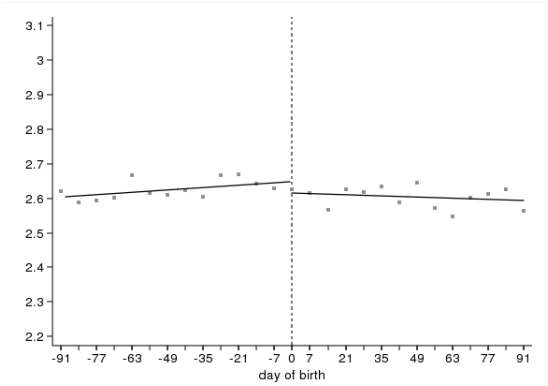
(b) reform July 1st 1988



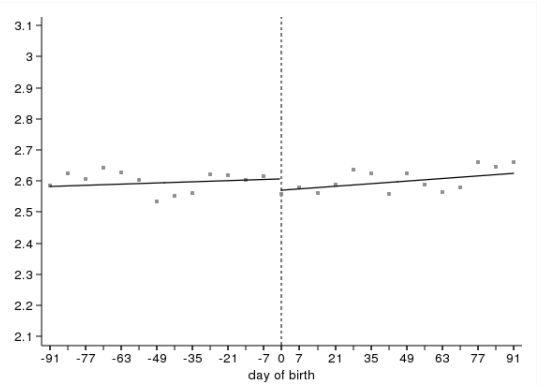
(c) reform April 1st 1989



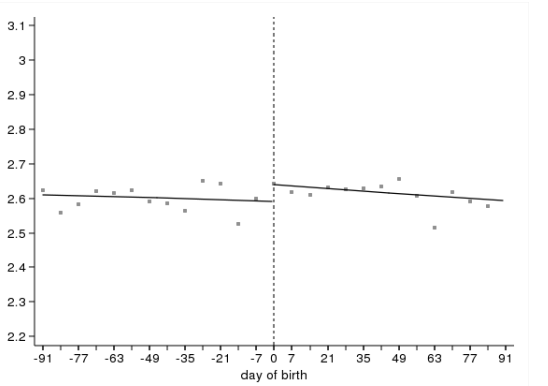
(d) reform May 1st 1990



(e) reform July 1st 1991



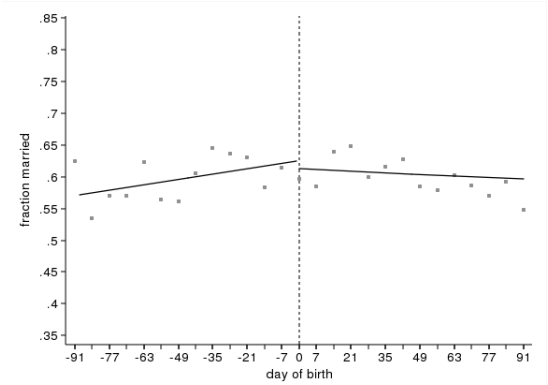
(f) reform April 1st 1992



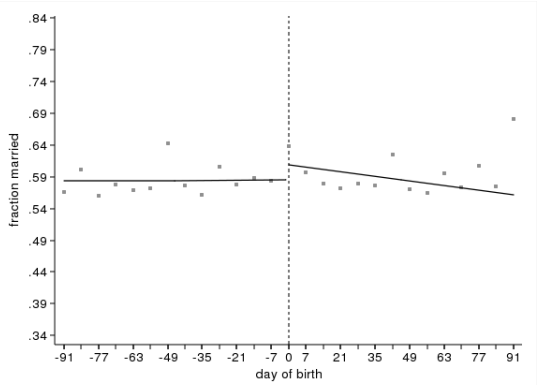
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A13: Entry into marriage (14 years after)

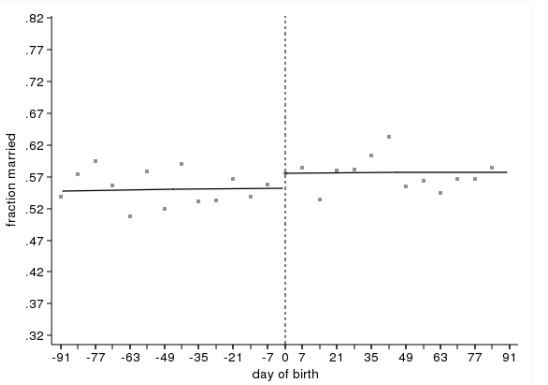
(a) reform May 1st 1987



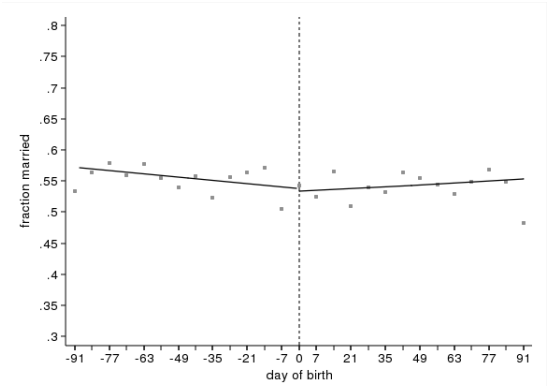
(b) reform July 1st 1988



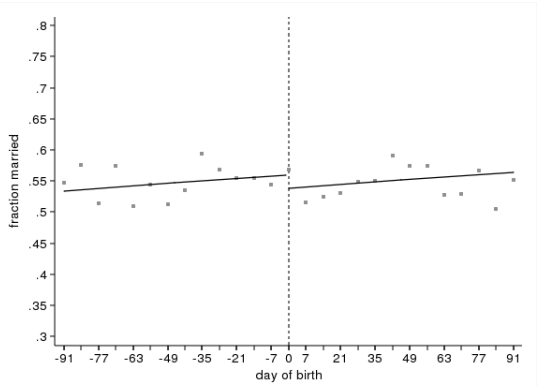
(c) reform April 1st 1989



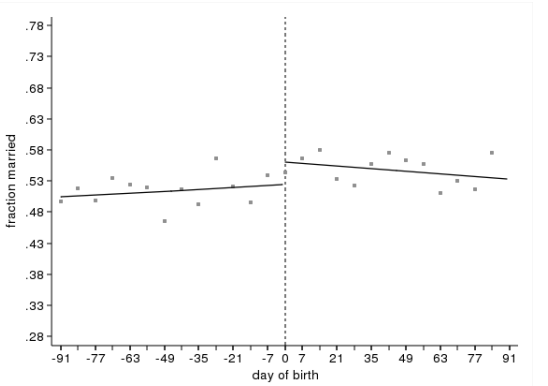
(d) reform May 1st 1990



(e) reform July 1st 1991



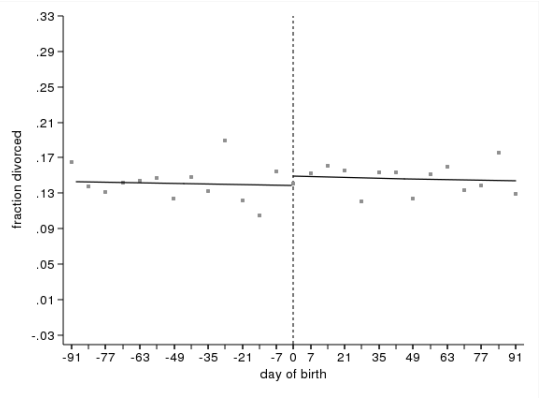
(f) reform April 1st 1992



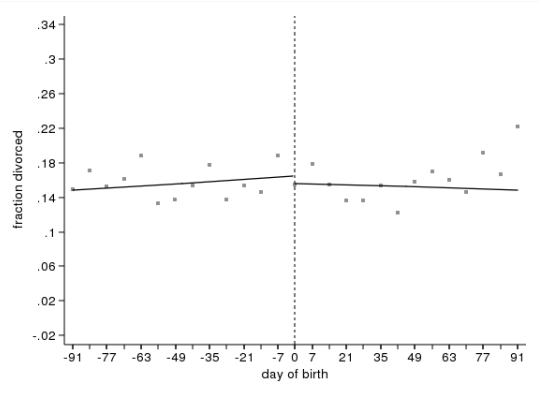
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A14: Divorced (14 years after)

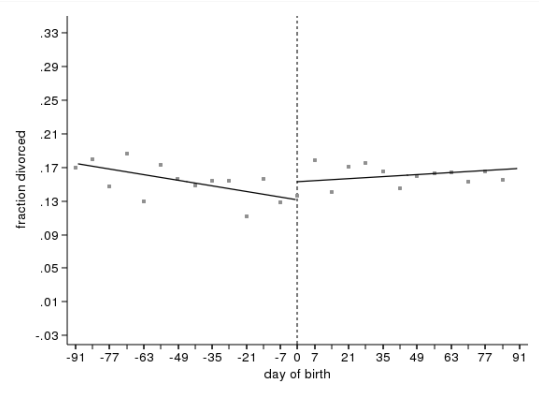
(a) reform May 1st 1987



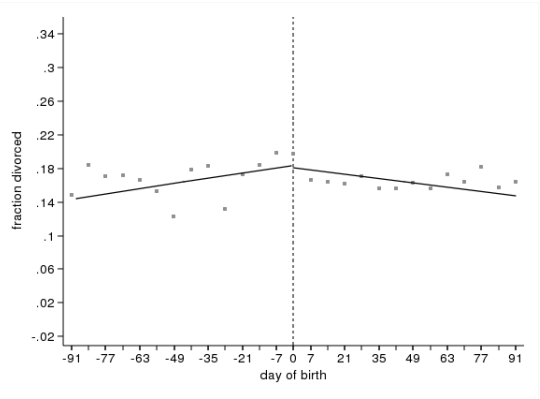
(b) reform July 1st 1988



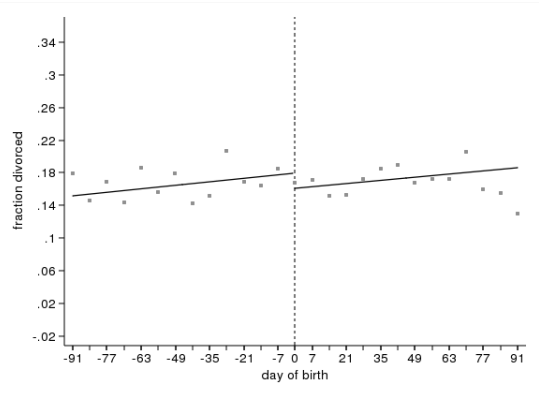
(c) reform April 1st 1989



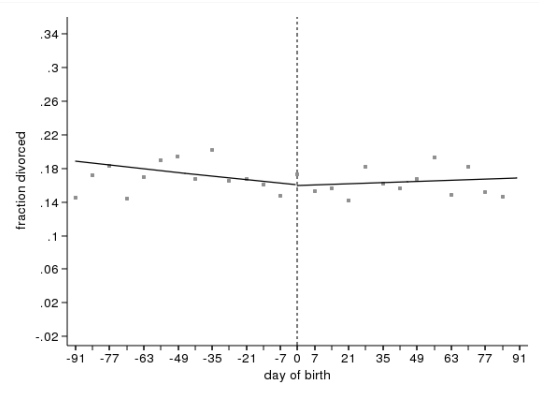
(d) reform May 1st 1990



(e) reform July 1st 1991



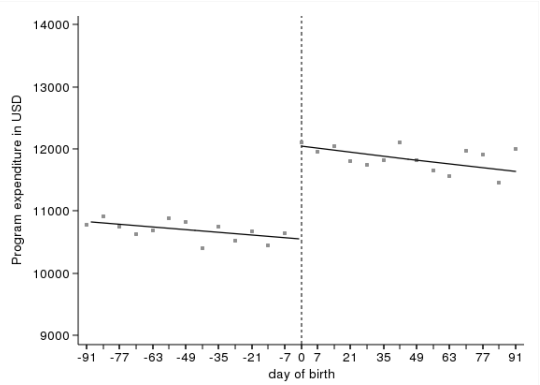
(f) reform April 1st 1992



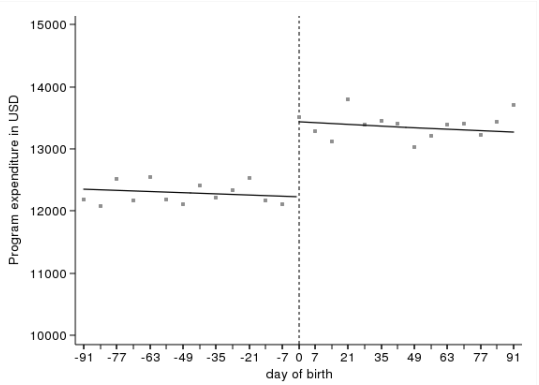
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A15: Program expenditure

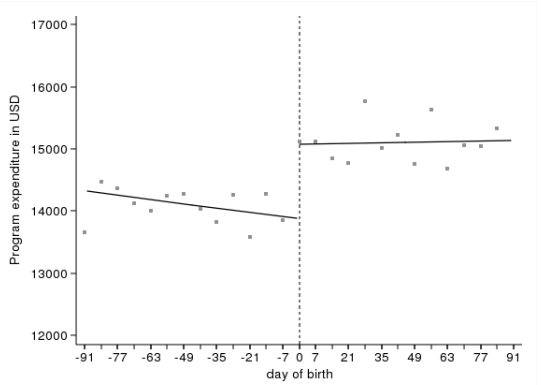
(a) reform May 1st 1987



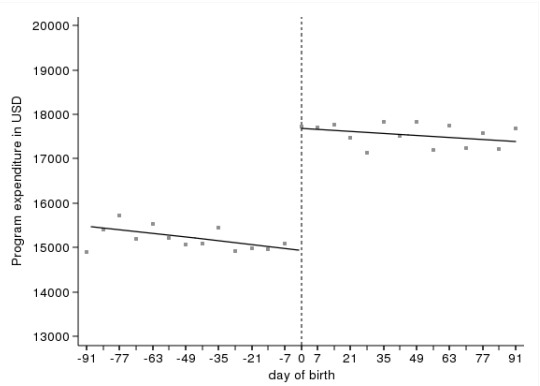
(b) reform July 1st 1988



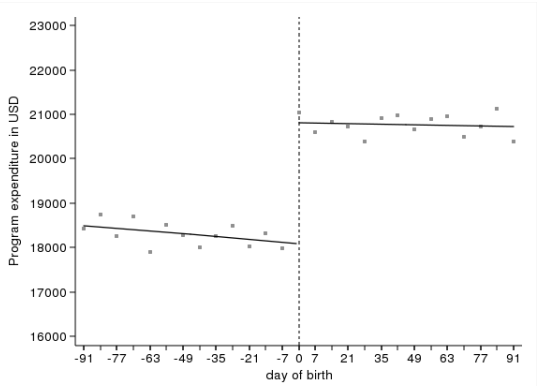
(c) reform April 1st 1989



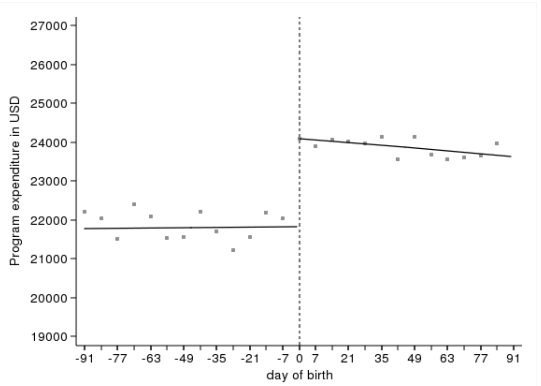
(d) reform May 1st 1990



(e) reform July 1st 1991



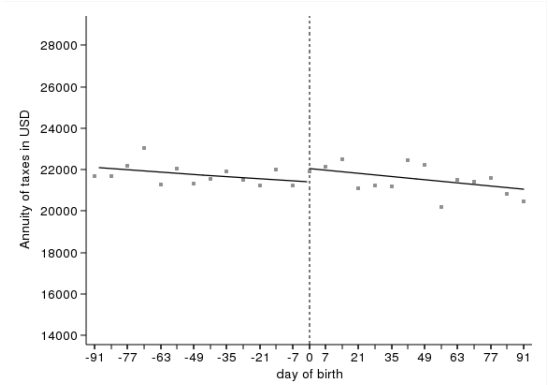
(f) reform April 1st 1992



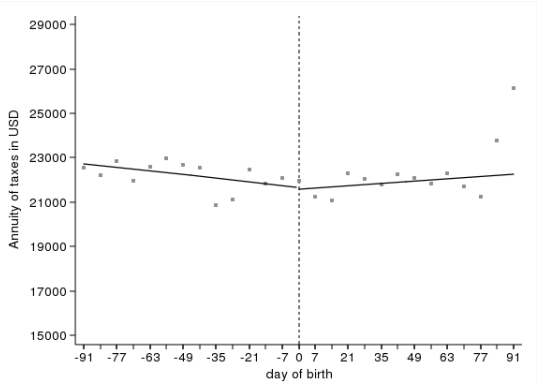
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A16: Family: Annuity of taxes paid (0-14 years after birth)

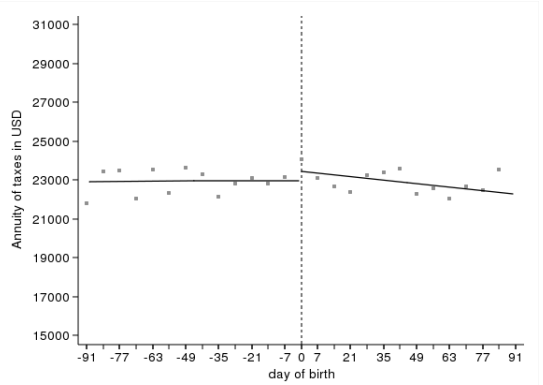
(a) reform May 1st 1987



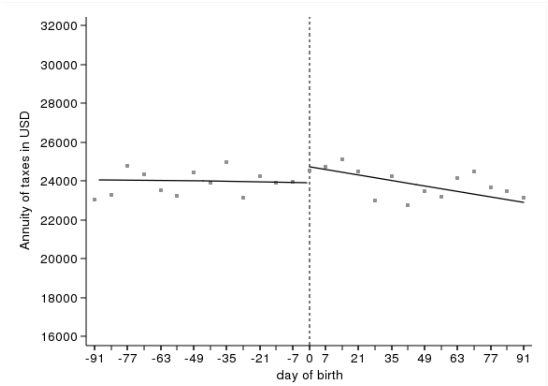
(b) reform July 1st 1988



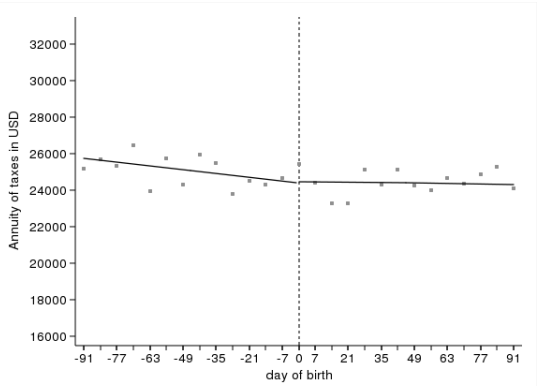
(c) reform April 1st 1989



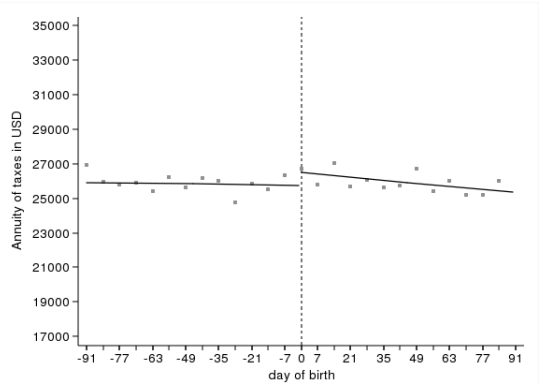
(d) reform May 1st 1990



(e) reform July 1st 1991



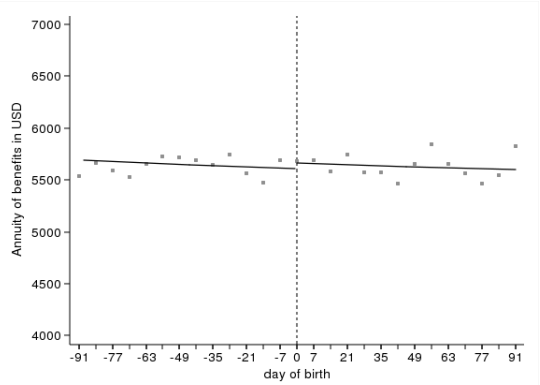
(f) reform April 1st 1992



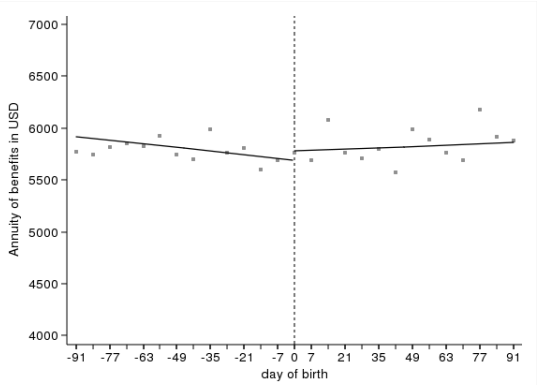
Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Figure A17: Family: Annuity of benefits received (0-14 years after birth)

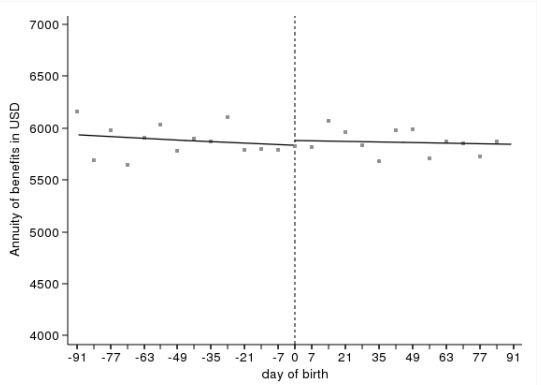
(a) reform May 1st 1987



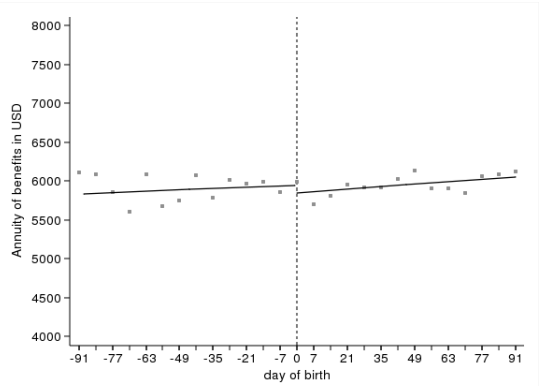
(b) reform July 1st 1988



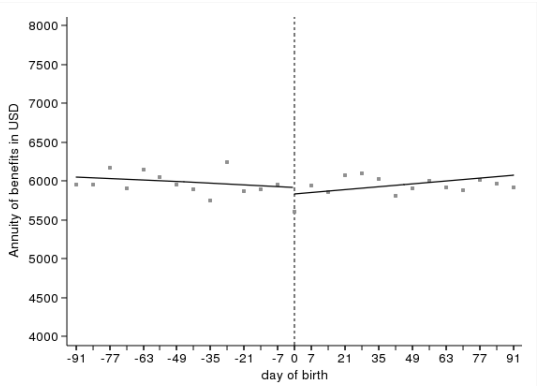
(c) reform April 1st 1989



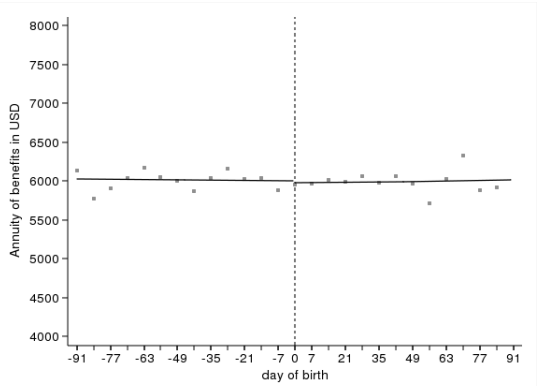
(d) reform May 1st 1990



(e) reform July 1st 1991



(f) reform April 1st 1992



Notes: Each observation is the average of the relevant outcome in a one week bin based on the birth date of the child. The dashed vertical lines denote the reform cutoff date, which has been normalized to zero. The solid lines are estimated using a linear regression based on daily individual level data with triangular weights. The scales of the y-axes are +/- .5 standard deviations of the mean outcome.

Online Appendix: Additional tables

Table A1: Summary statistics by reform year

	Reform					
	1987	1988	1989	1990	1991	1992
Mother: Age at birth	28.05	27.99	28.39	28.59	28.71	29.03
	(4.65)	(4.76)	(4.73)	(4.67)	(4.71)	(4.62)
Father: Age at birth	30.71	30.70	31.07	31.29	31.40	31.73
	(5.27)	(5.42)	(5.45)	(5.39)	(5.46)	(5.40)
Mother: Years of education	11.56	11.54	11.63	11.70	11.82	11.90
	(2.53)	(2.49)	(2.50)	(2.50)	(2.51)	(2.50)
Father: Years of education	11.83	11.79	11.85	11.88	11.92	12.01
	(2.65)	(2.63)	(2.63)	(2.61)	(2.61)	(2.61)
Married	0.56	0.52	0.53	0.52	0.49	0.51
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Child is a boy	0.51	0.51	0.52	0.50	0.52	0.52
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Eligible	0.70	0.74	0.75	0.75	0.76	0.77
	(0.46)	(0.44)	(0.43)	(0.43)	(0.43)	(0.42)
Unpaid leave (days)	102.14	95.06	80.82	61.82	56.49	59.13
	(124.26)	(123.18)	(112.37)	(102.68)	(90.04)	(89.48)
Child: 9th grade exam	3.54	3.51	3.52	3.52	3.52	3.60
	(1.07)	(1.04)	(1.06)	(1.02)	(1.03)	(1.04)
Child: High school dropout	0.35	0.35	0.33	0.33	0.33	.
	(0.48)	(0.48)	(0.47)	(0.47)	(0.47)	
Mother: Returned to work (by year 2)	0.76	0.80	0.84	0.85	0.86	0.87
	(0.42)	(0.40)	(0.37)	(0.35)	(0.35)	(0.33)
Mother: Years employed (max 14)	11.54	11.66	11.95	12.14	12.27	12.37
	(3.67)	(3.57)	(3.34)	(3.17)	(3.05)	(2.94)
Mother: Annuity of income (years 1-14)	19,137	20,205	22,014	23,442	24,680	26,421
	(10,645)	(11,023)	(12,121)	(12,613)	(12,683)	(13,822)
Father: Years employed (max 14)	13.35	13.32	13.37	13.38	13.41	13.44
	(2.02)	(2.07)	(1.97)	(1.97)	(1.94)	(1.87)
Father: Annuity of income (years 1-14)	37,642	39,175	40,891	43,015	45,206	47,477
	(21,142)	(21,740)	(22,317)	(23,876)	(27,789)	(25,975)
Gender gap: Years employed (max 14)	0.45	0.46	0.46	0.47	0.47	0.47
	(0.12)	(0.12)	(0.11)	(0.11)	(0.10)	(0.10)
Gender gap: Annuity of income (yrs 1-14)	0.34	0.35	0.36	0.36	0.36	0.37
	(0.16)	(0.16)	(0.15)	(0.15)	(0.15)	(0.15)
Number of children (14 years after)	2.59	2.60	2.60	2.62	2.60	2.61
	(0.92)	(0.91)	(0.91)	(0.91)	(0.90)	(0.90)
Entry into marriage (14 years after)	0.60	0.59	0.56	0.55	0.55	0.53
	(0.49)	(0.49)	(0.50)	(0.50)	(0.50)	(0.50)
Divorced (14 years after)	0.14	0.16	0.16	0.17	0.17	0.17
	(0.35)	(0.36)	(0.36)	(0.37)	(0.37)	(0.37)
Annuity of taxes paid (years 0-14)	21,647	22,117	22,924	23,936	24,784	25,915
	(14,322)	(14,029)	(15,925)	(16,273)	(16,833)	(18,543)
Annuity of benefits received (years 0-14)	5,630	5,811	5,869	5,928	5,956	5,998
	(3,101)	(3,317)	(3,245)	(3,307)	(3,453)	(3,405)
Program expenditure	11,272	12,805	14,600	16,354	19,512	22,881
	(4,635)	(5,274)	(5,763)	(6,539)	(7,626)	(8,631)
Obs.						
All	17, 580	19,310	20,093	21,508	21,717	21,838
Eligible	25,067	26,225	26,768	28,713	28,631	28,441
Exam score	16,650	18,290	19,200	20,056	20,459	20,427
Marital stability	9,819	9,964	10,679	11,281	10,546	11,077
Marriage	7,761	9,346	9,414	10,227	11,171	10,761

Table A2: Specification checks, 1992 reform

Outcomes	Baseline	No Controls	Quadratic trends	Baseline with 1 week donut
Unpaid leave (days)	-1.124	-1.218	-0.221	-0.817
	(2.564)	(2.594)	(3.834)	(2.936)
9th grade exam	-0.010	0.012	-0.026	-0.015
	(0.029)	(0.031)	(0.042)	(0.035)
Mother's labor market outcomes:				
Returned to work (by year 2)	-0.007	-0.006	-0.013	-0.008
	(0.009)	(0.010)	(0.014)	(0.011)
Years employed (max 14)	0.016	0.042	0.011	0.005
	(0.082)	(0.085)	(0.122)	(0.097)
Annuity of income (years 1-14)	566	860**	428	247
	(346)	(397)	(510)	(407)
Father's labor market outcomes:				
Years employed (max 14)	0.062	0.061	0.016	0.125**
	(0.053)	(0.054)	(0.077)	(0.064)
Annuity of income (years 1-14)	715	1,143	667	1,263
	(731)	(804)	(1028)	(904)
Gender gap:				
Years employed (max 14)	-0.002	-0.001	-0.001	-0.004
	(0.003)	(0.003)	(0.004)	(0.003)
Annuity of income (years 1-14)	-0.002	-0.001	-0.002	-0.007
	(0.004)	(0.004)	(0.006)	(0.005)
Family outcomes:				
Divorced (14 years after)	0.003	-0.001	0.018	-0.012
	(0.015)	(0.015)	(0.022)	(0.018)
Entry into marriage (14 years after)	0.034*	0.037*	0.026	0.058**
	(0.020)	(0.021)	(0.030)	(0.024)
Number of children (14 years after)	0.042*	0.048*	0.045	0.039
	(0.025)	(0.026)	(0.036)	(0.030)
Annuity of taxes paid (years 0-14)	385	761	242	827
	(518)	(579)	(743)	(588)
Annuity of benefits received (yrs 0-14)	-33	-25	-23	-95
	(97)	(98)	(140)	(118)
Program expenditure	2,132***	2,274***	1,793***	2,387***
	(219)	(248)	(320)	(259)

Notes: All coefficients are estimated using a linear RD model and triangular weights using children born 3 months before and 3 months after the reform. Control variables include father and mother's years of education and age at birth, martial status and county of residence the year prior to birth as well as an indicator of the child's gender. The number of observations for the full sample, exam score, divorce and marriages are respectively 21,838, 20,427, 11,077 and 10,761. For the one week donut the number of observations are 20,076, 18,777, 10,169 and 9,907. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1,**p<0.05.

Table A3: Window robustness checks, 1992 reform

Outcomes	90 days (baseline)	120 days	60 days
Unpaid leave (days)	-1.124	-2.687	-0.759
	(2.564)	(2.231)	(3.206)
9th grade exam	-0.010	-0.032	-0.021
	(0.029)	(0.025)	(0.036)
Mother's labor market outcomes:			
Returned to work (by year 2)	-0.007	-0.003	-0.009
	(0.009)	(0.008)	(0.012)
Years employed (max 14)	0.016	0.043	0.031
	(0.082)	(0.072)	(0.102)
Annuity of income (years 1-14)	566	553*	493
	(346)	(302)	(427)
Father's labor market outcomes:			
Years employed (max 14)	0.062	0.060	0.037
	(0.053)	(0.046)	(0.065)
Annuity of income (years 1-14)	715	459	747
	(731)	(641)	(881)
Gender gap:			
Years employed (max 14)	-0.002	-0.001	-0.001
	(0.003)	(0.002)	(0.004)
Annuity of income (year 1-14)	-0.002	-0.001	-0.002
	(0.004)	(0.004)	(0.005)
Family outcomes:			
Divorced (14 years after)	0.003	-0.001	0.014
	(0.015)	(0.013)	(0.018)
Entry into marriage (14 years after)	0.034*	0.043**	0.029
	(0.020)	(0.018)	(0.025)
Number of children (14 years after)	0.042*	0.041*	0.039
	(0.025)	(0.022)	(0.030)
Annuity of taxes paid (years 0-14)	385	344	325
	(518)	(452)	(635)
Annuity of benefits received (years 0-14)	-33	-38	-48
	(97)	(85)	(118)
Program expenditure	2,132***	2,082***	1,898***
	(219)	(192)	(270)

Notes: All coefficients are estimated using a linear RD model and triangular weights using children born 3 months before and 3 months after the reform. Control variables include father and mother's years of education and age at birth, martial status and county of residence the year prior to birth as well as an indicator of the child's gender. The number of observations for the full sample, exam score, divorced and marriages are respectively 21,838, 20,427, 11,077 and 10,761 for baseline, 28,346, 26,481, 14,075 and 14,271 for the 120 day window and 14,856, 13,902, 7,591 and 7,265 for the 60 day window. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1,**p<0.05.

Table A4: Local linear regression robustness checks, 1992 reform

	Bandwidth	
Outcomes	30 days	60 days
Unpaid leave (days)	-2.765	1.510
	(4.115)	(2.982)
9th grade exam	-0.038	-0.001
	(0.050)	(0.035)
Mother's labor market outcomes:		
Returned to work (by year 2)	-0.002	-0.012
	(0.015)	(0.011)
Years employed (max 14)	0.113	-0.006
	(0.135)	(0.095)
Annuity of income (years 1-14)	1206*	448
	(634)	(4478)
Father's labor market outcomes:		
Years employed (max 14)	0.037	-0.006
	(0.088)	(0.061)
Annuity of income (years 1-14)	430	1,238
	(1215)	(906)
Gender gap:		
Years employed (max 14)	0.002	-0.000
	(0.005)	(0.003)
Annuity of income (years 1-14)	0.004	-0.002
	(0.007)	(0.005)
Family outcomes:		
Divorce (14 years after)	0.017	0.006
	(0.024)	(0.017)
Entry into marriage (14 years after)	0.049	0.026
	(0.033)	(0.024)
Number of children (14 years after)	0.048	0.040
	(0.041)	(0.030)
Program expenditure	1,922***	2,188***
	(399)	(278)
Annuity of taxes paid (years 0-14)	290	753
	(903)	(650)
Annuity of benefits received (yrs 0-14)	14	35
	(155)	(109)

Notes: All coefficients are estimated using a local linear RD model. Control variables include father and mother's years of education and age at birth, martial status and county of residence the year prior to birth as well as an indicator of the child's gender. The number of observations for the full sample, exam score, divorce and marriages are respectively 21,838, 20,427, 11,077 and 10,761. Standard errors are reported in parentheses and comparison means in brackets. *p<0.1,**p<0.05.