Does maternity leave impact women's health? Evidence from Germany

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Abstract

Exploiting unique administrative data from the German Pension Register and Federal Employment Agency, we estimate the association between gradual increases in maternity leave duration (from initially eight weeks in 1965 to 36 months in 1992) with mothers' health over a period of up to 30 years after their last maternity leave uptake. Adopting a straightforward regression discontinuity approach, we compare the health of gainfully employed mothers who gave birth before and after the respective changes in maternity leave legislations. Health is identified by the number and length of spells of long-term sickness absence, which might be induced by physical and/or mental health problems. Preliminary analyses suggest significant changes in mothers' return-to-work behaviors. The role of early maternity leave extensions in maternal health is less clear, though, and seems to be mediated by changes in women's labor market outcomes.

Keywords: Maternity leave policies, health, administrative data, regression discontinuity design

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1 Introduction

A growing number of studies investigates the relationship between parenthood and various dimensions of individuals' well-being, suggesting that childless individuals do not fare worse than parents in terms of their economic, psychological, or social well-being, for example, and that having children might even be negatively related to well-being (e.g., Hank and Wagner 2013; Stanca 2012). More specifically, studies showed that shocks in fertility increase the likelihood of mothers suffering from high blood pressure and becoming obese (Cáceres-Delpiano and Simonsen 2012) and that the transition to motherhood is associated with higher sickness absence (e.g., Floderus et al. 2012; Mastekaasa 2013); ambiguous findings are reported on the relationship between parenthood and mental health (e.g., Evenson and Simon 2005; Mckenzie and Carter 2013). In addition to short-term effects on health, long-term effects of childbearing on later-life health and mortality have been identified as well (e.g., Grundy and Tomassini 2005; Hank 2010).

One important mechanism driving the frequently observed negative association between fertility and women's health is stress resulting from the 'double burden' of career and family obligations (e.g., Bratberg et al. 2002). Although there is some evidence of a positive effect of employment on health through both economic and non-economic rewards, work-family interference might reduce mothers' well-being (e.g., Boye 2011; Lidwall et al. 2010). Recent U.S. evidence, for example, suggests that women overall enjoy significant health benefits of employment (Frech and Damaske 2012), but that these decline somewhat when paid work is combined with the care of a young child (Schnittker 2007; also see Chatterji et al., 2013).

Leave policies allowing parents to take time off work to care for an infant are widespread in OECD countries (see Tanaka, 2005, for a brief overview). At a minimum, relatively brief periods of leave covering some weeks before and after childbirth are offered, designed to protect mothers and their offspring from immediate health impairments around childbirth. In addition, countries such as Germany run much more generous policies by granting leave durations for up to three years. While being initiated primarily to enhance children's well-being and the compatibility of childbearing and female employment, these

longer leave policies might also have unintended consequences for mothers' subsequent health. Against this background, the aim of this paper is to identify the causal effects of alternative maternity leave durations on mothers' health outcomes, differentiating between potential short- and long-run effects.

Maternal leave policies may, on the one hand, contribute to reducing potential health insults for mothers directly by buffering the stresses associated with childrearing (e.g., Boye 2011; Hank 2010) or indirectly by enhancing mothers' labor force attachment (e.g., Baker and Milligan 2008b; Schott 2012). On the other hand, extended rights to parental leave might also discourage mothers' participation in the labor market, resulting in long-run career and earnings disadvantages and – eventually – in adverse health outcomes (e.g., Economou and Theodossiou 2011; Frech and Damaske 2012). Although the role of maternity leave regulations in determining fertility as well as a variety of labor market outcomes has been widely studied (e.g., Baker and Milligan 2008b; Han et al. 2009; Lalive and Zweimüller 2009), only very little economic – or other social science – research investigating potential effects of leave duration on maternal health has been conducted yet (e.g., Baker and Milligan 2008a; Chatterji and Markowitz 2005; Staehelin et al. 2007).

The coverage of the few studies available today is constrained to a period of at most two to three years after childbirth. Against the background of research suggesting a lasting impact of early health insults on later-life well being (e.g., Case et al. 2005; Grundy and Tomassini 2005), this obviously constitutes a major limitation, which our paper aims to overcome. Exploiting German administrative data, we estimate the net causal effect of different expansions of leave duration on mothers' health over a period of up to three decades. Identification is based on two reforms, increasing the length of (paid, job-protected) parental leave in (West-)Germany from initially eight weeks of postpartum mothers' protection to six months in 1979 and, after several further gradual increases in the 1980s, from 18 to 36 months in 1992. The reforms became effective depending on the birthday of the child, such that the assignment to different policy regimes is close to random. This enables us to adopt a straightforward but powerful regression discontinuity approach. We compare the health of gainfully employed mothers who gave birth at most

four months before and after the respective changes in maternity leave legislations. Health is identified by the number and length of spells of long-term sickness absence (> 6 weeks) – which might be induced by physical and/or mental health problems – nearly up to 30 years after the mother's last maternity leave uptake.

The data we use in this study stem from the German Pension Register and the Federal Employment Agency (*BASiD* data). The data are based on reports in compliance with the notifying procedure for the German social security system. The *BASiD* data provides an ideal basis for analyzing the impact of extended leave durations on mothers' health outcomes for several reasons: First, it is the only German administrative data source that encompasses full employment biographies. In particular, the data contains information on all periods with contributions to the German Pension System (employment, long-term illness, unemployment) as well as periods for which no contributions were paid, but which were nevertheless creditable for the pension insurance. For the wide majority of mothers this allows us to retrieve information on their employment and health histories for the years before they gave birth. Second, the pension insurance also records the year and month of all births as well as periods of child-raising since the latter is a pension-relevant activity. A third advantage of the *BASiD* data is that it covers considerably longer time periods after parental leave periods than comparable administrative data sets. As the *BASiD* data contains full longitudinal information on the pension-relevant biographies of all individuals aged 15 to 67 who have not retired yet, the most recent release allows us to retrieve information on individuals born between 1940 and 1992 up until the end of 2007. This provides an ideal situation for the evaluation of the extensions of parental leave on health outcomes, as the reforms and the subsequent employment biographies can be observed with the data. For the 1992 reform of the length of job protection, we are able to track women's employment and health outcomes for up to 15 years – for the earliest reform in 1979 the data cover a period of almost 30 years. These are time spans that considerably exceed previous evaluations of parental leave legislation.

Our paper thus adds to the existing literature on the maternity leave – maternal health nexus in several important ways. To the best of our knowledge, our study will be

the first one whose database (a) allows differentiating between potential short-run and long-run effects of maternity leave extensions on mothers' health, (b) covers two very distinct maternity leave reforms in 1979 and 1992, allowing identification of potentially changing marginal effects of leave extensions, and (c) provides information on maternal health outcomes from administrative records.

2 The German institutional background

Parental leave in Germany is substantially more generous than in any other country. When paid, job protected maternity leave was introduced in Germany in May 1979, its maximum duration was six months, including eight weeks of postpartum Mutterschutz (mothers' protection) available since 1968. While women continued to receive their full salary during the two months following childbirth, maternity benefits in the subsequent four months were fixed to an amount of roughly 375 Euros, regardless of women's prior earnings. However, only mothers who were employed before childbirth were entitled to these benefits. Since then leave duration gradually increased to a total of 18 months in 1990 and all mothers, irrespective of their prior employment status, became eligible for maternity benefits. Eventually, the job protected period of leave was doubled in January 1992, with a maximum duration of now 36 months (followed by an extension of maternity benefit payments to 24 months one year later). While the mother's protection law (Mutterschutzgesetz) was explicitly designed to protect mothers and children from health impairments in the period six weeks before and eight weeks after childbirth, the German parental leave regulations considered here primarily intend to facilitate children's welfare by allowing their parents to care for them without risking losing their jobs.

3 Proposed mechanisms linking maternity leave and health

Through different pathways, parental leave duration might impact mothers' health directly and indirectly, and in both positive and negative ways. First, much of the evidence on the labor market effects of parental leave suggests that expansions in leave duration delay mothers' return-to-work (Baker and Milligan 2008b, Lalive and Zweimüller 2009, Schönberg and Ludsteck 2011). By increasing the time spent away from work, the availability of paid and job-protected parental leave should therefore reduce young mothers' exposure to stress (e.g., Chatterji et al. 2013). It prevents them, for some time, from the double burden of childbearing and gainful employment by allowing to concentrate on childrearing responsibilities while facing only limited – if any – financial strain and job insecurity (e.g., Tucker et al. 2010). This should protect mothers from health impairments in the short run, that is, during the period in which they are eligible to parental leave, but might also have an effect in the longer-run, because later life health has been shown to be closely related to earlier health experiences (e.g., Case and Paxson 2005). Moreover, a later return to work is associated with higher odds of initiating and continuing breastfeeding over a longer period of time (e.g., Baker and Milligan 2008a; Roe et al. 1999), which some studies suggest to bear a positive relationship with mothers' short- and long-term health (cf. Labbok 1999; Lawrence 2000).

Second, mothers' entitlement to parental leave affects their subsequent attachment to the labor market. The direction of this effect – and a possibly resulting impact on maternal health – appears to depend on leave duration, though. On the one hand, modest job-protected leave entitlements seem to facilitate higher levels of labor force participation among women with small children (Schott 2012). They have also been shown to increase job continuity with the pre-birth employer (Baker and Milligan 2008b).

Incentives to postpone one's return to work, set by very generous parental leave programs, might also result in lower labor market attachment and potential economic disadvantage in the long-term, as time spent out of the labor force devaluates the individual's

human capital and lowers her earnings (e.g., Aisenbrey et al. 2009; Buligescu et al. 2009; also see Rippeyoung and Noonan 2012). Moreover, extensions of leave duration have also been shown to have a variety of unintended consequences. Puhani and Sonderhof (2011) showed, for instance, that parental leave extensions in Germany negatively affected job-related training for young women, especially if this is employer-arranged, irrespective of whether they have children. Despite these unintended consequences, the few available studies provide little support for adverse long-run employment consequences (Lalive and Zweimüller 2009 and Schönberg and Ludsteck 2011). However, it needs to be stressed that the coverage of these studies is constrained to a maximum period of ten years after childbirth. A potentially negative career trajectory and lower socio-economic status might bear in it the potential for an indirect 'health penalty' for mothers who took leave from work after childbirth (cf. Frech and Damaske 2012). Moreover, women in particular have been suggested to derive subjective utility from non-economic interpersonal work rewards, such as social support or recognition from others (e.g., Ross and Mirowsky 1996). Women losing their attachment to the labor force might thus not only face economic but also psychosocial disadvantages resulting in adverse health outcomes. Evidence from Finland, for example, suggests that higher levels of 'social capital' at work are associated with sustained good health (Oksanen et al. 2008; also see De Vogli 2010).

4 Previous empirical findings

The net effect of the potentially counteracting mechanisms described above and the optimal length of parental leave (with regard to its proposed impact on mothers' subsequent health) have not been properly determined yet. Almost all empirical evidence available today considers variations in maternity leave uptake ranging from roughly 6-14 weeks, monitoring mothers' health status for at most 12 months after childbirth (cf. Staehelin et al. 2007). The Canadian study by Baker and Milligan (2008a) is an exception in that it considers an increase in mandated parental leave from roughly 20-30 weeks (depending on province) to 50+ weeks, and observes mothers' health during the first 24 months postpartum.

Obviously, mothers' timing of their postbirth employment interruptionis not independent of their health. To address the resulting endogeneity problem, one ideally needs an exogenous variation in individuals' actual leave uptake behavior, which may be generated by reforms of maternity leave legislation. This is the approach followed by Baker and Milligan (2008a) and is also partially accounted for in the study by Chatterji and Markowitz (2005), who provide OLS and IV estimates from a national sample of employed U.S. mothers who had returned to work nine weeks post partum on average. The authors find only weak evidence that returning to work later affects the probability of having three or more outpatient physician or clinic visits in the six months following childbirth. Based on a non-representative sample of women with high work commitment (80% of which had returned to work four months postpartum), Killien et al. (2001) found no significant association between the duration of absence from work and mothers' general health status in the first year of their child's life.

However, other evidence suggests that adverse employment conditions – including short maternity leave – might bear a negative association with women's postpartum mental health (e.g., Cooklin et al. 2011; Hyde et al. 1995; also see Tucker et al. 2010). Chatterji and Markowitz (2005) find that a one-week increase in the length of maternal leave reduces a scale of depressive symptoms by about 7% on average. This result is corroborated by findings reported in Chatterji and Markowitz (2012), which also suggest a positive relationship between longer maternity leave and mothers' overall health. Whitehouse et al. (2013) estimate the optimal leave duration to reduce Australian mothers' psychological distress to be more than 6 months, but not necessarily more than 12 months, because longer leave duration would be associated with larger financial economic pressures. Most recently, Humlum and Vejlin (2012) showed that an increase in the length of maternity leave with full benefit compensation from 32 to 52 weeks did not affect Danish mothers' probability of receiving antidepressants or of being hospitalized with a depression up to three years after childbirth, but found evidence for a negative effect of prolonged maternal leave on the number of hospitalizations. Baker and Milligan (2008a), however, find no effect at all of an increase in parental leave entitlements on Canadian mothers self-reported health, depression, or specific post-partum health problems in the

first two years following childbirth.

5 Hypotheses

Our empirical analysis will focus on the 1979 reform (increasing the period of paid, job protected leave from two to six months) and the 1992 reform (doubling the duration of leave from 18 to 36 months). The primary aim of our research is to identify possible long-run effects of parental leave entitlements on mothers' health and to differentiate between short- and long-run effects. This seems particularly important because the relationship between maternity leave (maternal employment, respectively) and mothers' health may not only vary over individuals' life-course (e.g., Chatterji et al. 2013), but the mechanisms underlying such associations might also be very different ones:

(a) If maternity leave affects maternal well-being in the short run, we are likely to observe a direct causal relationship, where (longer) leave protects mothers from health impairments related to the aftermath of childbirth. We thus expect increases in leave entitlements to bring about an unambiguously positive effect on maternal health outcomes in the short run.

(b) Long-run associations between parental leave and women's health outcomes, however, are likely to reflect indirect effects. In this case, the effect of leave on well-being should be mediated either through a direct effect of leave duration on mothers' health shortly after childbirth, and/or through a direct effect of maternity leave on mothers' subsequent employment careers. Particularly if the latter effect dominates, one might also expect a negative relationship between leave duration and maternal health in the longer run.

(c) The marginal effect of further leave extensions may change depending on the length of the pre-reform entitlement. The proposed positive direct effect might become zero, for example, once a certain – yet undetermined – threshold is passed. Regarding indirect effects, we suspect that relatively moderate (generous, respectively) entitlements to maternity leave facilitate (impede, respectively) mothers' integration into the labor

market. Assuming further that greater attachment to the labor market tends to bring about health benefits, the extension of leave durations in 1979 may thus have had a positive impact on mothers' subsequent health, whereas the leave extension in 1992 might rather have had a negative health effect.

6 Data and Empirical Strategy

6.1 Data and variable description

The data used in the empirical analysis are taken from German register data (*BASiD*). The data combine information from the *German Pension Register* with various data sources from the German Federal Employment Agency. The *BASiD* data set is a stratified random 1% sample of all birth cohorts from the early 1940s to the early 1990s, who have at least one entry in their social security records. The data provide longitudinal information on individuals' entire pension-relevant biographies up to the year 2007. Individual work histories cover the period from the year individuals were aged fourteen until the age of sixty-seven. In Germany, statutory pension insurance is mandatory for all employees in the private and public sector, thus only excluding civil servants and self-employed individuals. In addition, contributions to the pension insurance are paid by the unemployment insurance or the health insurance during periods of unemployment and prolonged illness. As a consequence, the insurance covers more than 90% of the entire population for whom all past pension-relevant periods have been recorded.

The *Pension Register* contains information on all periods for which contributions were paid (employment, long-term illness, unemployment) as well as periods without contributions, which were still creditable for the pension insurance. The latter refers to activities for which an individual receives pension credits. These are periods of school or university attendance after the age of 15, periods of training and apprenticeship and periods of caring. For the wide majority of mothers this allows us to retrieve information on their entire pre and post-birth employment and health histories. Apart from individual information on employment status and births, the *Pension Register* provides information

on age, gender as well as monthly earnings, which can be calculated by exploiting information on pension credit points gained from social security employment. Table A1 in the Appendix contains a more detailed description of the individual characteristics provided by the *Pension Register*.

Starting from 1975 in western and from 1991 in eastern Germany, employment spells subject to social security contributions from the *Pension Register* can be merged with data from the German Federal Employment Agency, the *Integrated Labour Market Biographies and* the *Establishment History Panel*. The *Integrated Labour Market Biographies* provide further time varying individual information on blue or white-collar status, occupational status, educational status (six categories)[1] and an establishment identifier. The latter allows us to retrieve information on tenure at the current employer. Finally, the *Establishment History Panel* contains information on the establishment's workforce composition, establishment size as well as sector affiliation. Table A2 in the Appendix provides a more detailed description on the variables gained from the *Employment Statistics Register*.

6.2 Measurement of births and maternal leave durations

The pension insurance records the year and month of all births. Compared to other data sets that have been used for the analysis of female employment biographies, the information on children and births in the data can be considered highly reliable. Despite this advantage over other administrative data sets, the data offer some disadvantages as well. First, recorded births in the *Pension Register* not only refer to females giving birth but may also include adoptions. Second, recorded births generally pertain to that part of the child's parent who claims the (pension relevant) period of child raising. Thus, the data may also include fathers with a recorded birth. However, the pension insurance records the period of child raising as a default for the child's mother - fathers may claim child raising periods only upon formal request (Kreyenfeld and Mika 2008). As a result,

[1]The categories are: No degree, vocational training degree, highschool degree (*Abitur*), highschool degree and vocational training, technical college degree and university degree.

the fraction of fathers claiming the period of child raising has been negligible. Third, the pension data does not provide direct information on maternal leave take-up. In our analysis, we will measure leave durations by the number of months that elapse until the first postbirth employment spell. Thus, while the data allow us to quite precisely measure postbirth employment interruptions[2], they are not informative about the effective take-up of benefits. However, given that we are interested in the causal effect of the *duration* of job protection (as opposed to the take-up of benefits), we argue that this constitutes a minor restriction.

6.3 Measurement of health

We measure mothers' health status by using information on spells of long-term sickness absence. The *BASiD* data record (1) all spells of illness which are subject to sickness pay covered by the mandatory health insurance and (2) all spells that cover long-term rehabilitation measures. The first type comes into effect after a period of six weeks of absence and may cover either spells of employment and unemployment. The six-week period corresponds to the mandatory duration of sickness pay to be paid by employers and may also derive from the accumulation of several shorter illness spells within the last twelve months, as long as these are caused by the same disease diagnosis. A potential concern is that these illness spells may - to some limited extent - also cover caring periods for ill infants below the age of 12. However, these periods were capped at a maximum length of five days per year/per child before 1992 and of 10 days thereafter. To address this potentially confounding effect, we will exclude from our illness episodes all those spells up to a length of 5 or 10 days, respectively. The second type of illness spells covers spells with measures aimed at reintegrating long-term disabled individuals into the labor market. Taken together, periods of illness recorded by the *Pension Register Data* generally refer to spells of long-term illness of employees who have been absent due to the same disease diagnosis for more than six weeks.

[2]Note that the data do not allow us to measure the exact day of birth. We therefore set each child's birthday on the 15th of its respective month of birth. As a result, the measured leave duration will be associated with a measurement error of up to +14/-15 days.

Given that the six-week period is strongly linked to the mandatory duration of employer-provided sickness pay, it is important to stress that the latter has remained unchanged since 1970. Thus, measuring health by spells of long-term illness has the clear advantage that one obtains a consistent measure of health across the two reform periods relevant to our analysis. A further advantage over health measures based upon survey data (e.g., Baker and Milligan 2008a) is that our administrative measure does not suffer from attrition bias. A potential concern is that the measure may be somewhat restrictive as shorter illness spells are not captured by the data. However, as the data allows us to measure not only the number but also the length of long-term illness spells, we are confident to obtain sufficient variation with this measure. Even though our proposed measure is not contingent upon employment after childbirth, a disadvantage is that it conditions on the long-term willingness to participate in the labor market. This may impose a selection problem, which we will address below.

6.4 Empirical Strategy

As set out above, we exploit two extensions of parental leave duration, the first one from two to six months in May 1979 and the second one from 18 to 36 months in January 1992. As the reforms have been implemented depending on the child's birthday, the policy changes create natural experiments which allow us to assess a how changes in parental leave duration affect post-birth return-to-work behavior. In order to identify the ITT (intention-to-treat) effect, we use a regression-discontinuity design (RDD) where the treatments are defined by the extensions of leave protection. The ITT measures the average effect of a given reform, thus also including those who do not respond to the changed legislation by changing the length of their maternity leave (non-compliers). While the ITT remains silent about the impact of actually prolonging maternity leave, it is highly relevant from a policy perspective as it measures the predicted average impact of the changes in legislation. The RDD in both cases exploits the fact that for children born just after the introduction of the respective legislations, mothers could not anticipate the upcoming reform at the time of contraception. At least for births just before and just after

the respective reform starts, mothers should thus be comparable in all relevant observable and unobservable characteristics except for the fact that the latter were exposed to a different institutional setting. We will explore the validity of this identifying assumption by comparing the observable characteristics of the treated and untreated mothers who are included in the estimations. A further important prerequisite for the RDD strategy to work is the assumption that the treated mothers were treated by chance, i.e. their fertility behavior is no response to an anticipated reform. Whether and for which time span this assumption is plausible depends on when the reforms have been seriously debated and announced to the public. With respect to the 1979 and 1992 reform, the validity of the no-anticipation assumption has already been investigated by Schönberg and Ludsteck (2011). Analyzing newspaper articles that appeared prior to both reforms, the authors show that the changes in parental leave legislation were announced to the public only shortly before they came into effect. Finally, a further concern is that the RDD estimates might be confounded by seasonality effects, if, e.g., subsequent health outcomes are affected by the seasonal timing of birth. To address this problem, we will combine the RDD design with a difference-in-difference approach by including as an additional control groups mothers who gave birth shortly before or after the respective threshold months in the year preceding the reforms.

Even though the policy changes allow us to assess the causal impact of changes in parental leave duration on post-birth return-to-work behavior, one needs to be cautious in inferring conlusions about the associated health outcomes. Given that our health measure conditions on labor market participation, there are potential selection effects that might bias our estimates. A key concern is that the reforms might give rise to differences in the dependencies of return-to-work decisions on health status. Note that the direction of a potential bias is not clear a-priori. On the one hand, one may argue that longer leave mandates offer mothers with a bad health status the possibility to recover. This might encourage those who otherwise would have stayed away from the labor market to return to work. On the other hand, more time spent at home causes a depreciation of human capital. The latter is arguably more costly for those with a bad health status and might therefore - compared with the pre-reform setting - worsen the incentives to return to work

for the less healthy ones. To address the selection issue, we will examine the relationship between observable pre-birth health histories and return-to-work patterns across pre- and post-reform mothers. In this regard, it is important to note that the data allow us to retrieve information on the full pre-birth employment and (associated) health histories. Controlling for these observables may thus give us some indication whether a comparison of long-run labor market and health outcomes across pre- and post-reform mothers is confounded by systematic differences in the return behavior across both groups.

7 Empirical Results

7.1 Leave durations

As set out above, the treatment (control) group consists of mothers who gave birth after (before) the respective changes in maternity leave legislations. For both reforms, we choose an observation window of four months before and after the threshold dates. Because the eligibility rules for *job protection* require that mothers be employed prior to childbirth we restrict both groups to those women who were employed for at least three months within the last year prior to giving birth.

Table 1 reports differences in leave durations after childbirth for the different reform scenarios. For the 1979 reform, row (1) indicates that treated mothers, i.e. those giving birth from May to August 1979, spend on average less time away from work than control mothers (?? as compared to ?? months). Row (2) reports the share of mothers whose leave durations are right-censored in the data, i.e. of those who did not return to work until 2007. The figures indicate that the reform does not considerably alter the fraction of mothers never returning to the labor market. As a consequence, the differences in leave durations of conditional on returning are similar to the unconditional figures (35.74 as compared to 40.47 months). Given that we expect an expansion in leave coverage to delay mothers' return-to-work, the differences in leave durations appear to be striking. In the next section, we will explore whether this counter-intuitive result is driven by potentially long-run positive long-run effects that may counteract the short-run negative effects.

Contrary to the 1979 reform, the differences in leave durations for the 1992 reform are in line with what one might expect: Row (4) indicates that treated mothers, i.e. those giving birth from January to April 1992, spend on average more time away from work than control mothers (?? as compared to ?? months). Again average leave durations before and after the reform considerably exceed the mandated maximum maternal leave durations of 18 and 36 months. As for the 1979 reform, the 1992 reform does not substantially lower the fraction of mothers ever returning to work (row (5)).

Table 1: Return-To-Work Behaviour by Treated and Control Group

Variable	Mean	Std.-Dev.	Mean	Std.-Dev.
1979 Reform	Treated		Control	
Postbirth leave duration				
(1) LEAVE DURATION[a)]				
(2) FRACTION RETURNING	92.8	–	93.8	–
(3) LEAVE DURATION (CONDITIONAL ON RETURNING)	35.74	1.93	40.47	2.28
Individuals	1,039		931	
1992 Reform	Treated		Control	
Postbirth leave duration				
(4) LEAVE DURATION[a)]				
(5) FRACTION RETURNING	86.1	–	86.8	–
(6) LEAVE DURATION (CONDITIONAL ON RETURNING)	48,51	1.59	46.27	1.80
Individuals	812		672	

Source: BASID 2007. Note: [a)]Leave duration measures the number of months that elapse until the first postbirth employment spell.

To address whether the observed differences in return-to-work behavior stem from systematic differences across treated and control mothers, Table A4 and A5 in the Appendix provide descriptive evidence on a number of prebirth characteristics for the different reform scenarios. The figures indicate that both groups appear to be quite similar with respect to age and education. For both reform scenarios, treated and control mothers also exhibit similar amounts of previous employment, unemployment and non-employment experience. For the 1979 reform, the same is true for the prebirth illness duration which can be taken as an indicator of mothers' prebirth health status. For the 1992, however, the prebirth

illness duration of treated mothers is lower than that of their controls. This suggests that it is of crucial importance to control for prebirth health status in the multivariate framework.

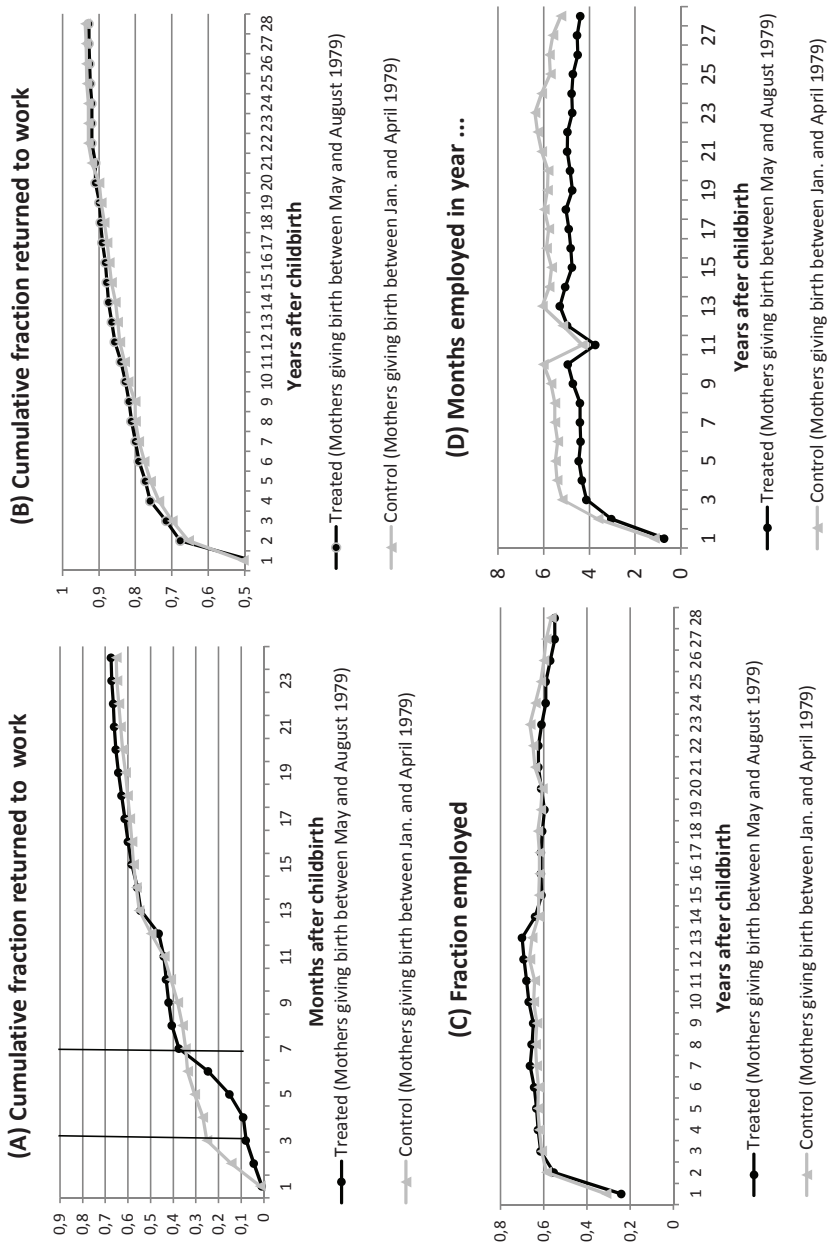
7.2 Return-to-work behavior

7.2.1 Descriptive Results

We next explore to what extent the extensions of the mandated maximum leave durations affect mothers' postbirth return-to-work behavior. To do so, we look at three different indicators. First, we measure the return-to-work probability by constructing an indicator taking on the value of one if a mother has returned at least once to the labor market by month m or year t (*Return-to-work*). To distinguish between short and long-run effects, we construct this indicator for up to 24 months and up to 28 years after childbirth. Second, to address the fact that mothers may only temporarily return to the labor market, we measure whether a mother is employed in year t after childbirth (*Employed*). Third, to capture the intensive dimension of employment, we also look at the months worked per calendar year t after childbirth (*Months worked*). Turning first to the 1979 reform, Figure 1 (A) compares the return-to-work profiles during the 24 months after childbirth for treated and control mothers.

The figure demonstrates that the reform strongly affects the short-run return-to-work behavior. About 25 % of control mothers, but only 7.9 % of treated mothers return to work after two months following childbirth. 75 % of the treated and 85% of the control mothers fully exhaust the maximum mandated leave duration. While 13 % of the treated and 10 % of the control mothers exactly return to work when the mandated leave duration has run out, 62 % of the treated and 75 % of the controls continue to stay away from work at the end of the job protection period. However, the figure also demonstrates that the delay to return to work appears to be of short-run nature since after two years the fraction returned is 2.5 percentage points larger among the treated than for control mothers.

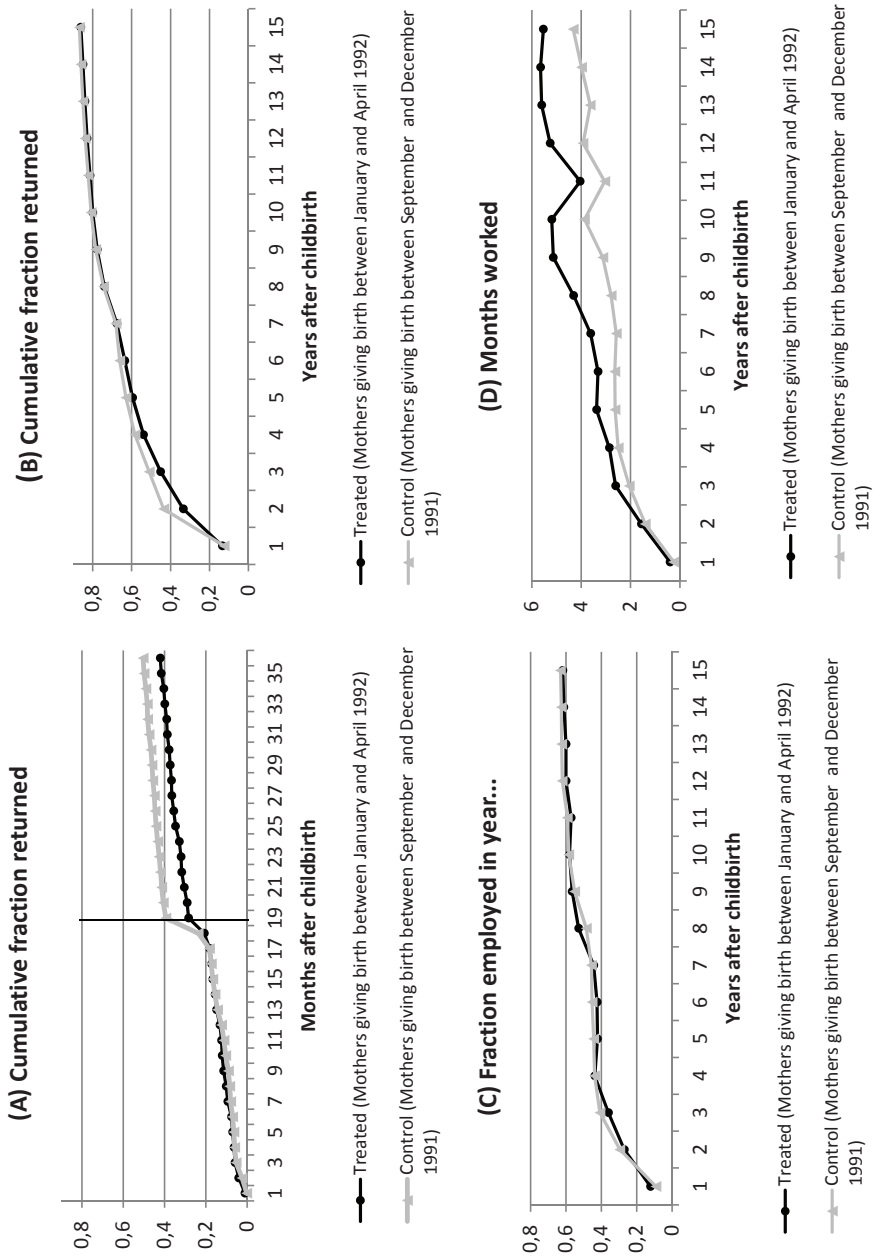
Figure 1: Return to work behavior, 1979 reform - expansion in leave coverage from 2 to 6 months



As set out above, our data offer the great advantage of tracking women's employment histories over a much longer time-span than previously used data sets. To explore whether the long-run return-to-work profiles differ from the short-run profiles, Figure 1 (B) compares the return-to-work profiles during the maximum number of available years after childbirth for treated and control mothers.The figure reveals that return-to-work advantage among the treated tends to continue. However, the difference seems to erode in the long run as it largely disappears about 20 years after childbirth. After 28 years, 93% of treated mothers ever return to the labor market compared with 94% among the controls. Figure 1 (C) and (D) show that while there are no substantial differences in terms of the fraction employed, the control group features a larger number of months worked per year.

Figure 2 (A) compares the return-to-work profiles during the 24 months after childbirth for treated and control mothers for the 1992 reform. The figure demonstrates that extending the leave duration from 18 to 36 months again causes mothers to stay home longer. The figure shows that 39.4 % of control mothers, but only 28.4 % of the treated return to work after 18 months following childbirth. 58 % of the treated and 76 % of the control mothers fully exhaust the maximum mandated leave duration. While ?? % of the treated and 16 % of the control mothers exactly return to work when the mandated leave duration has run out, ?? % of the treated and 60 % of the controls continue to stay away from work at the end of the job protection period. Figure 2 (B) compares the long-run return-to-work profiles during the maximum number of available years after childbirth. Overall, after 7 years the cumulative fraction returning to work is still slightly lower for treated mothers as compared to their controls. This suggests that the reform's effect on short-run return to work behavior translates into similar long-run effects. However, the figure also demonstrates that this small difference tends to disappear in the long run. Figure 2 (C) and (D) show that while there are no substantial differences in terms of the fraction employed, the reform appears to cause treated mothers to work a larger number of months worked per year.

Figure 2: Return to work behavior, 1992 reform - expansion in leave coverage from 18 to 36 months



7.2.2 Regression Results

Table 2 reports regression results to assess the reforms' impact on subsequent labor market outcomes for the 1979 reform, explaining the outcome variables return-to-work probability, the fraction employed per year and the number of months worked per year by the treatment status as well as a number of controls. For the return to work probability, the estimates are based on a linear probability model. To further assess the importance of different time horizons, the different panels report the regression results measuring the outcomes one, three, ten and 28 (15) years after childbirth, respectively.

Column (1) displays the baseline differences of the outcome variables at different points in time with the figures corresponding to those shown in Figure 1. The baseline estimates in Column (1) show that while treated mothers are less likely to have returned to work one year after childbirth, the long-term effects are estimated to be positive. However, all estimated differences in return-to-work probabilities are insignificant at conventional levels. The difference in the fraction employed is estimated to be significantly negative at least in the short run one year after childbirth. The number of months worked per year is the only outcome that appears to be consistently and significantly lower for treated mothers. Column (2) adds a set of control variables such as information on age, education as well as the prebirth employment and health histories. The figures show that the results are quite robust to including controls especially with regard to the negative difference in the number of months worked. Finally, the last two columns report the results from placebo estimates using data on women giving birth one year prior to the observation window in 1979. Column (4) reports the estimates corresponding to those in Column (2) for mothers giving birth between January and August 1978, whereas Column (5) reports the differences with regard to the estimates in 1979. The placebo estimates suggest that there are some seasonal effects that appear to confound the differences across treated and control mothers. Particularly the number of months worked per year appears to systematically differ across mothers giving birth between January and April and those giving birth between May and August. As a result, the estimated differences in the last column show that the estimated differences largely disappear once these seasonal

Table 2: Regression Results Labor Market Outcomes 1979 Reform

	Base	Controls	Placebo	Difference
1 year after Childbirth				
Return-to-work	-.025	-.014	-.024	0.010
	(0.023)	(0.022)	(0.022)	
Fraction employed	-.068***	-.054***	0.026	-.080***
	(0.020)	(0.019)	(0.019)	
Months worked	-1.117***	-.948***	-.331	-.617
	(0.260)	(0.253)	(0.281)	
3 Years after Childbirth				
Return-to-work	0.017	-.004	-.044***	0.040*
	(0.021)	(0.016)	(0.015)	
Fraction employed	0.005	-.016	-.039***	0.023
	(0.022)	(0.018)	(0.017)	
Months worked	-2.686***	-2.893***	-3.27***	0.379
	(0.533)	(0.450)	(0.439)	
10 Years after Childbirth				
Return-to-work	0.009	-.007	-.003	-.004
	(0.017)	(0.012)	(0.012)	
Fraction employed	0.022	0.005	0.012	-.007
	(0.021)	(0.015)	(0.014)	
Months worked	-2.731***	-3.003***	-2.827***	-.176
	(0.543)	(0.421)	(0.411)	
28 Years after Childbirth				
Return-to-work	-.009	-.005	-.008	0.003
	(0.011)	(0.010)	(0.009)	
Fraction employed	-.0174	0.014	0.023	-.007
	(0.022)	(0.019)	(0.018)	
Months worked	-2.174***	-1.456***	-1.711***	0.255
	(0.576)	(0.492)	(0.482)	

differences are controlled for. Table 3repeats the exercise for the 1992 reform.

Table 3: Regression Results Labor Market Outcomes 1992 Reform

	Base	Controls	Placebo	Difference
1 year after Childbirth				
Return-to-work	0.009	-.001	0.000	-.000
	(0.018)	(0.017)	(0.000)	(0.000)
Fraction employed	0.031*	0.027*	0.000	-.000
	(0.016)	(0.016)	(0.000)	(0.000)
Months worked	0.370*	0.291		
	(0.194)	(0.193)		
3 Years after Childbirth				
Return-to-work	-.060***	-.058***	0.000	-.000
	(0.026)	(0.024)	(0.000)	(0.000)
Fraction employed	-.047*	-.051**	0.000	-.000
	(0.025)	(0.023)	(0.000)	(0.000)
Months worked	1.396***	1.273***		
	(0.538)	(0.501)		
10 Years after Childbirth				
Return-to-work	-.008	0.000	0.000	-.000
	(0.021)	(0.017)	(0.000)	(0.000)
Fraction employed	-.004	0.002	0.000	-.000
	(0.026)	(0.022)	(0.000)	(0.000)
Months worked	3.375***	3.462***		
	(0.662)	(0.583)		
15 Years after Childbirth				
Return-to-work	-.007	-.001	0.000	-.000
	(0.018)	(0.016)	(0.000)	(0.000)
Fraction employed	-.013	-.006	0.000	-.000
	(0.025)	(0.021)	(0.000)	(0.000)
Months worked	3.066***	3.190***		
	(0.676)	(0.580)		

The estimates indicate that mothers who are subject to the expansion in leave duration exhibit significantly lower return-to-work probabilities and a significantly lower fraction being employed three years after childbirth. However, the table also confirms the descriptive evidence from Figure 2 suggesting that the negative impact on labor market

participation erodes in the long run. Column (2) demonstrates that these finding are robust to the inclusion of control variables.

7.3 Health outcomes

The empirical analysis thus far has documented a delay in return-to-work caused by both expansions in leave coverage. While for the 1979 reform the delay shows up only within the first year after childbirth, it appears to be more long-lasting for the 1992 reform. Do the reform-induced changes in return-to-work behavior after childbirth translate into different health outcomes? To explore this issue, we construct two different health indicators from the information on long-term illness spells in the administrative records. Because these spells are contingent on women's labor market participation, the health outcomes can be measured only conditional on having returned to the labor market (i.e. conditional on being employed or either unemployed). Motivated by the fact that long-term illness spells represent a relatively rare event, we first construct a dummy variable taking on the value of one if a mother has experienced at least a long-term illness spell year t (*Fraction ever become ill*). Second, to capture the intensive dimension of illness, we compute the cumulative length of all long-term illness spells (*Fraction length of illness*) relative to the cumulative length of labor market participation by year t after childbirth (including periods of employment and unemployment).

Turning to the first measure (*Fraction ever become ill*) for the 1979 reform, Figure 3 (A) compares the cumulative proportion of women that have experienced at least one long-term illness spell during the 28 years after childbirth for treated and control mothers. The figure demonstrates that the expansion in leave coverage from two to six months is associated with a slightly larger fraction of women ever having experienced a long-term illness spell at each point in time. Between year six and ten after childbirth the difference amounts to roughly four percentage points. After this time period the health disadvantage among the treated continues to prevail, but it becomes smaller and tends to erode in the long run. Figure 3 (B) compares the cumulative days of illness as a fraction of the duration of labor market participation after childbirth across treated and controls.

The figure illustrates that over the whole observation period treated mothers experience a larger duration of illness relative to the time spent in the labor market than control mothers, with the difference being largest in the first three years after childbirth. Even after 11 year the difference still amounts to one percentage point, suggesting that the differential tends to be long-lasting. The overall result of a health disadvantage among treated mothers is surprising and clearly deserves some further attention. In Section 6.4, we argued that such a finding may be rationalized by a potential negative selection effect. The reason is that the reform might encourage less healthy mothers - who under shorter leave mandates would have stayed away from the labor market - to return to work earlier. We will address this potential negative selection effect in the next section within a regression framework.

Turning to the 1992 reform, Figure 4 (A) shows that the expansion in leave coverage from 18 to 36 months is associated with a lower fraction of women ever having experienced a long-term illness spell at each point in time. Interestingly, even after 15 year the difference still amounts to three percentage points, suggesting that the differential does not erode in the long run. In terms of illness duration, Figure 4 (B) illustrates that over the whole observation period treated mothers experience shorter durations of illness relative to the time spent in the labor market than control mothers, with the durations diverging strongest in the first three years after childbirth. Similar to the first measure (*Fraction ever become ill*), this health advantage does not erode in the long run, as the difference still amounts to 0.5 percentage points 15 years after childbirth.

7.3.1 Regression Results

to be completed

8 Summary and Conclusions

to be completed

Figure 3: Health outcomes, 1979 reform - expansion in leave coverage from 2 to 6 months

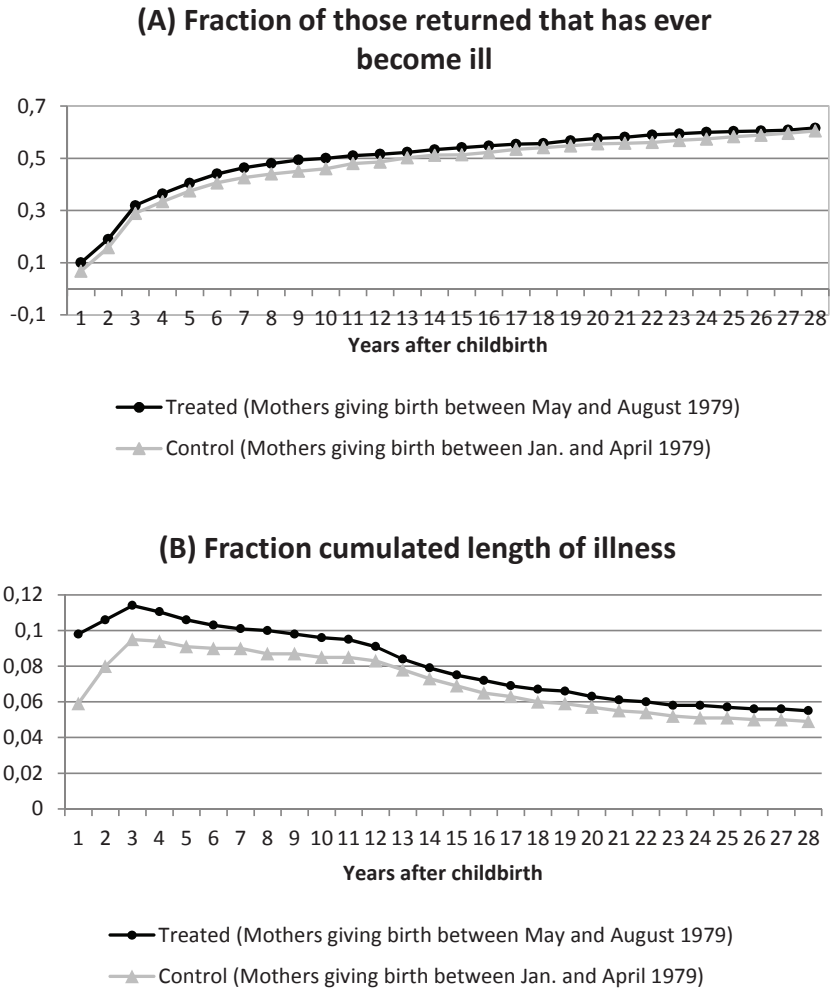
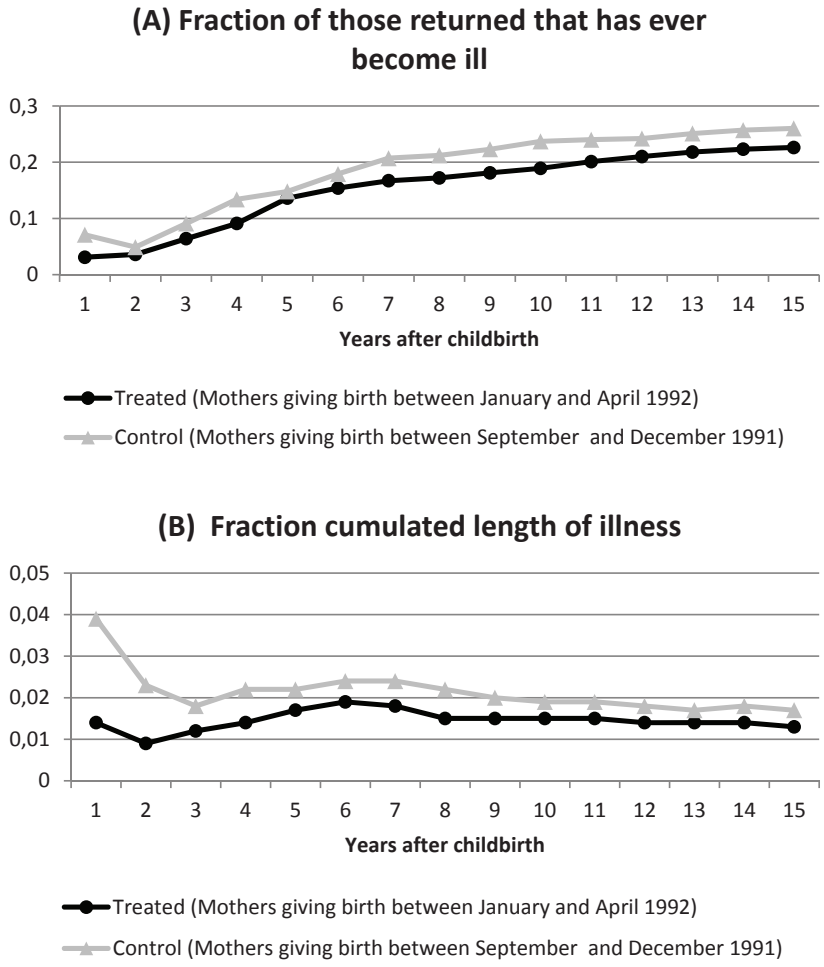


Figure 4: Health outcomes, 1992 reform - expansion in leave coverage from 18 to 36 months



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9 Appendix

Variable	Definition
Employment Status	
EMPLOYMENT	Employment spells include periods of employment subject to social security contributions and (after 1998) marginal employment.
UNEMPLOYMENT	Unemployment spells include periods of unemployment with and without transfer receipt.[1)]
NON-EMPLOYMENT	Non-employment spells include periods of child raising, care giving as well as periods with missing information on the employment status.
ILLNESS	Illness spells includes periods of long-term illness ($>$ 6 weeks) and periods with long-term rehabilitation measures.
TRAINING	Training spells includes periods of school or university attendance after the age of 15 and periods of training and apprenticeship.
# UN(NON)EMPLOY-MENT_SPELLS	Number of un- or non-employment spells. An unemployment spell is counted as a new spell if the gap between a preceding unemployment spell exceeds four weeks.

Table A2: Description of individual employment history variables gained from the *Pension Register*

[1)] A spell of unemployment in the *Pension Register* requires individuals to be registered as unemployed *and* to obtain public transfers. The latter include benefits such as unemployment insurance, and - prior to 2005 - the means-tested social assistance and unemployment assistance benefits. After 2004, unemployment and social assistance were merged into one unified benefit, also known as 'unemployment benefit II' (ALG II). As the latter targets only employable individuals, a spell involving the receipt of ALG II automatically fulfills the requirements to be recorded as unemployed in the *Pension Register*. Prior to 2005, spells with social assistance benefits fulfill the above requirements only if individuals were registered as unemployed. Otherwise they are recorded as non-employment spells. As a consequence, the *Pension Register* does not permit a consistent definition of un and non-employment prior to and after 2005. To distinguish further between voluntary and involuntary unemployment, gaps between periods of employment and unemployment are treated as involuntary unemployment as long as the gap does not exceed six weeks, otherwise the gap is treated as non-employment.

Variable/Categories	Definition
Educational Status	
LOW-SKILLED	No degree or highschool degree (Reference category)
MEDIUM-SKILLED	Completed vocational training
HIGH-SKILLED	Technical college degree or university degree
Age	Age in years
Occupational Type	
WHITE-COLLAR	White-collar worker (Reference: blue-collar)
Seniority	
TENURE	Number of previous months at current employer. Employment interruptions a the same employer may not exceed 6 months - otherwise tenure is reset to zero after the employment interruption.
Earnings	
EARNINGS	Gross monthly earnings are retrieved from credit points to the German Pension Insurance. One credit point corresponds to the average of yearly earnings of all gainfully employed workers in Germany. Monthly earnings are thus obtained by multiplying monthly credit points with the average of earnings as documented in the Appendix 10 to the German Social Act (*SGB VI*). Credit points are reported upthe contribution limit of the German social security system.

Table A2: Description of individual characteristics
gained from the *Pension and Employment Statistics Register*

Variable	Definition/Categories:
Establishment size	Size $\leq$ *20* (Reference category)
	20 $\leq$ Size $<$ *50*
	50 $\leq$ Size $<$ *200*
	200 $\leq$ Size $<$ *1000*
	Size $\geq$ *1000*
Workforce composition	Share of employees younger than 30 years
	Share of employees older than 50 years
	Share of low-skilled employees
	Share of female employees
Sector affiliation	Agriculture/Forestry (Reference category)
	Mining and manufacturing
	Energy/Water supplies
	Construction
	Wholesale and retail trade
	Transport and communication
	Financial intermediation
	Other service activities
	Public administration

Table A3: Definition of establishment characteristics gained from the *Employment Statistics Register*

Variable	Mean	Std.-Dev.	Mean	Std.-Dev.	Variable	Mean	Std.-Dev.	Mean	Std.-Dev.
1979 Reform	Treated		Control		1991/92 Reform	Treated		Control	
Individual characteristics[a)]					**Individual characteristics**[a)]				
AGE	25.49	4.62	25.79	4.46	AGE	27.64	4.75	26.98***	4.61
WHITECOLL					WHITECOLL				
LOW-SKILLED	0.28	0.15	0.30	0.15	LOW-SKILLED	0.26	0.44	0.23	0.43
MEDIUM-SKILLED	0.60	0.15	0.60	0.15	MEDIUM-SKILLED	0.67	0.47	0.69	0.45
HIGH-SKILLED	0.03	0.17	0.03	0.15	HIGH-SKILLED	0.05	0.21	0.04	0.21
SKILL MISSING	0.09	0.30	0.07	0.27	SKILL MISSING	0.02	0.15	0.04	0.19
Individual prebirth work history[b)]					**Individual prebirth work history**[b)]				
TENURE (in months)					TENURE (in months)				
EMPLOYMENT-DURATION	70.84	47.81	73.75	46.82	EMPLOYMENT-DURATION	77.37	52,59	76.88	51.98
UNMPLOYMENT-DURATION	0.01	0.21	0.01	0.21	UNMPLOYMENT-DURATION	0.07	0.84	0.04	0.40
NON-EMPLOYMENT-DUR.	13.35	23.22	14.06	24.26	NON-EMPLOYMENT-DUR.	16.05	25.60	16.45	26.92
ILLNESS-DURATION	1.57	3.08	1.41	3.04	ILLNESS-DURATION	1.64	4.18	2.11***	5.03
#UNEMPLOYMENT SPELLS	0.22	0.64	0.19	0.55	#UNEMPLOYMENT SPELLS	0.73	1.34	0.58***	1.10
#NON-EMPLOYMENT SPELLS	1.82	1.67	1.76	1.61	#NON-EMPLOYMENT SPELLS	1.98	1.58	2.12	1.69
Individuals	1,039		931		Individuals	812		672	

Source: BASID 2007. Note: [a)]All individual characteristics are measured on the last day prior to childbirth.
[b)]All durations are measured in months.

Table A4: Descriptive Statistics by Treated and Control Group for 1979 and 1991/92 Reform