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Abstract

Exploiting unique German administrative data, we estimate the association between an expansion in maternity leave duration from two to six months in 1979 and mothers' post-birth long-term sickness absence over a period of three decades after childbirth. Using a regression discontinuity design, we first show that the leave extension caused mothers to significantly delay their return to work within the first year after childbirth. We then compare the number and length of spells of long-term sickness absence of returned mothers who gave birth before and after the change in leave legislation. Our findings suggest that among those returned, mothers subject to the leave extension exhibit a higher incidence of long-term sickness absence as compared to control mothers. This also holds true after controlling for observable differences in pre-birth illness histories. At the same time, there are no pronounced effects on mothers' medium-run labor market attachment following the short-run delay in return to work, which might rationalize a negative causal health effect. Breaking down the results by mothers' pre-birth health status suggests that the higher incidence of long-term sickness absence among the treated may be explained by the fact that the reform has facilitated re-entry of a negative health selection into the labor market.

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

JEL-Code: J10, J16; J18

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1 Introduction

Recent U.S. evidence suggests that women overall enjoy significant health advantages from being employed (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 are considered an important measure to buffer the stresses associated with childrearing. Most OECD countries offer, at a minimum, relatively brief leave periods covering some weeks before and after childbirth, with the primary aim to protect mothers and their offspring from immediate health impairments around childbirth (see Tanaka 2005, for an overview). In addition to offering such short leave periods, many countries run much more generous policies. One such example is Germany, which over the past three decades experienced several expansions in maternity leave. While the more recent expansions were primarily initiated to enhance children's well-being and the compatibility of childrearing and female employment, Germany's earliest reform explicitly aimed at improving mothers' health (Dustmann and Schönberg 2012). This first reform of maternity leave took place in May 1979 and raised the length of paid, job-protected maternity leave from initially eight weeks of postpartum mothers' protection to six months.

The goal of our study is to evaluate the health consequences of the 1979 expansion in leave coverage in Germany. Given that the policy aimed at supporting the return of a larger fraction of healthier females to the labor market, we look at the health outcomes of mothers participating in the labor market. Health is measured by spells of long-term ($>$ 6 weeks) sickness absence, which Marmot et al. (1995: 124) proposed to "be used as an integrated measure of physical, psychological, and social functioning in studies of working populations." As we will argue below, long-term sickness spells in Germany provide a highly reliable measure of illness, as the misuse of sickness payments is strongly restricted by the health insurance's auditing system. There are several channels through which a longer leave period might affect mothers' post-birth health. Maternal leave policies may, on the one hand, contribute to

reducing potential health insults for mothers by buffering the stresses associated with childrearing, raising the incidence of breastfeeding (e.g., Baker and Milligan 2008a; Boye 2011) or indirectly by enhancing mothers' labor force attachment (e.g., Baker and Milligan 2008b; Schott 2012). On the other hand, extended rights to maternity 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., Frech and Damaske 2012; Smith 2007). Next to causal effects, a further channel through which expansions in leave coverage might alter *returned* mothers' post-birth health outcomes are potential selection effects. The reason is that any reform affecting the length of the mandatory leave period may alter the health composition of those who return to the labor market. Given that leave policies typically aim at improving not only the quantity but also the quality of female labor market participation, both channels are of considerable interest to policy makers.

Although the role of maternity leave regulations in determining labor market outcomes (e.g., Baker and Milligan 2008b; Ondrich et al. 2003; Schönberg and Ludsteck 2014), fertility (Lalive and Zweimüller 2009) as well as children's outcomes (e.g., Baker and Milligan 2010; Dustmann and Schönberg 2012; Rossin 2011) has been widely studied, only very little economic – or other social science – research investigating potential consequences for 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; Case and Paxson 2010), this obviously constitutes a major limitation, which our paper aims to overcome. Exploiting administrative data, we estimate the association between an expansion in maternity leave duration and mothers' long-term sickness absence over a period of up to three decades following the first (West-)German reform of maternity leave in 1979. The reform 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 regression discontinuity

approach to identify the labor market effects of this reform. We then proceed to compare the number and length of spells of long-term sickness absence experienced by gainfully employed mothers who gave birth at most four months before and after the change in maternity leave legislation. In conditioning on labor market participation, our measure is informative about the health outcomes of those mothers who returned to the labor market. Although such a focus may be highly relevant from a policy point of view, it restricts us to recover a joint effect comprising both a causal and a potential unobservable compositional component. While we are not able to separate the latter, we shall attempt to assess the quantitative relevance of *observable* compositional effects. The strategy we pursue here is to examine the relationship between observable pre-birth illness histories and return-to-work patterns across pre- and post-reform mothers. Controlling for such differences in health is not only informative about a health selection upon observables, but may also yield important insights into the likely direction of a potential unobservable composition effect.

The data we use in this study stem from the German Pension Register and the Federal Employment Agency (*BASiD*). This data set provides an ideal basis for analyzing the relationship between an extended leave duration and mothers' long-term sickness absence 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 illness 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 maternity 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 extension of maternity leave on long-term sickness absence, as births around the policy change along with mothers' pre- and post-birth employment biographies can be observed with the data. For the 1979 reform considered in this study, we are able to track women's post-birth employment biographies for almost 30 years. This is a time span that considerably exceeds previous evaluations of maternity 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 is the first one (a) whose database provides information on an important indicator of maternal health outcomes, namely long-term sickness absence from administrative records, which is available for mothers' pre- and post-birth biographies, and (b) which allows differentiating between potential short-run and long-run consequences of maternity leave extensions for long-term illness outcomes.

The remainder of the paper is laid out as follows: Section 2 starts by giving a brief overview of the related literature. Section 3 provides some institutional background information on German maternity leave legislation. Section 4 discusses potential channels through which an expansion in leave coverage might alter employed mothers' health, as measured by the incidence of long-term sickness absence. While Section 5.1. provides a description of the data set, Section 5.2 sets out the empirical strategy for quantifying the association between a longer leave duration and mothers' incidence of long-term sickness absence. Section 6 presents the estimation results and the final Section 7 concludes.

2 Previous empirical findings

In exploring the relationship between maternity leave and sickness absence, our analysis is closely related to the literature on the maternity leave – maternal health nexus. A first strand of related empirical evidence stems from the psychological

and public health literature, whose results are mixed. Some studies suggest that adverse employment conditions – including short maternity leave – bear a negative association with women's postpartum mental health (e.g., Cooklin et al. 2011 for Australia; Hyde et al. 1995 and Tucker et al. 2010 for the U.S). Whitehouse et al. (2013) estimate the optimal leave duration to reduce Australian mothers' psychological distress to be more than 6 months, but not more than 12 months, because longer leave duration would be associated with larger financial economic pressures. Based on a non-representative U.S. survey of women with high work commitment, Killien et al. (2001) find no significant association between the duration of absence from work and mothers' general health status in the first year of their child's life.

A major drawback of the studies cited above is that they are mainly descriptive in nature. Obviously, mothers' timing of their post-birth employment interruption is not independent of their health. To address the resulting endogeneity problem, another strand of literature exploits differences in maternity leave legislation that provide an exogenous variation in individuals' uptake behavior. This approach is, e.g., followed by Baker and Milligan (2008a), who find no effect of an increase in maternity leave entitlements on Canadian mothers self-reported health, depression, or specific postpartum health problems in the first two years following childbirth. Chatterji and Markowitz (2005) exploit regional variation in leave mandates to provide IV estimates from a national sample of employed U.S. mothers. 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. However, with respect to mental health their results suggest 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.

A common feature of the evidence available today is that much of the literature is limited to considering variations in maternity leave uptake ranging from roughly

6-14 weeks only and monitors mothers' health status for at most 12 months after childbirth (cf. Staehelin et al. 2007). The study by Baker and Milligan (2008a) is an exception in that it considers an increase in mandated maternity leave from roughly 20-30 weeks (depending on province) to 50+ weeks, and observes mothers' health during the first 24 months postpartum.

3 The German institutional background

The German leave legislation allows mothers to take time off work after childbirth by guaranteeing them the right to return to a comparable job at their previous employer. In Germany, the job protected leave period is substantially more generous than in any other country. When paid, job protected maternity leave was introduced in May 1979, its maximum duration was six months, including eight weeks of postpartum mothers' protection (*Mutterschutz*) available since 1968. During the mothers' protection period women continued to receive their average salary over the three months prior to childbirth, whereas benefits in the subsequent four months were fixed to a nominal amount of 750 Deutschmark, regardless of women's prior earnings. However, only mothers who were employed before childbirth were entitled to these benefits.

A subsequent reform in 1986 increased the leave duration from 6 to 10 months (starting in January 1986). Maternity benefits from the third to the sixth month were equal to 600 Deutschmark, whereas from the seventh month onwards maternity benefits were means-tested. The level of income replacement during the mother protection period remained unchanged. Moreover, since 1986 all mothers, irrespective of their prior employment status, became eligible for maternity benefits. Since then the leave duration (along with the benefit payment period) gradually increased to a total of 18 months in 1990. 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 latter reforms were primarily aimed at improving children's well-being and the compatibility of childrearing and female employment, the mother's protection law (*Mutterschutzgesetz*) and the 1979 reform were explicitly designed to protect mothers from health impairments. This and the initially short leave duration makes the 1979 reform particularly suitable for testing potential direct health effects, which - as will be discussed in the next section - may be expected to primarily result from leave expansions within the first year after childbirth.

4 Proposed mechanisms linking maternity leave and health

Our study follows the basic assumption that the relationship between maternity leave and mothers' sickness absence is mediated through the association between sickness absence and maternal health (see Afsa and Givord 2014, for a detailed discussion). Through different pathways, maternity leave duration might impact mothers' health directly and indirectly, and in both positive and negative ways. As to the direct effect, much of the evidence on the labor market effects of maternity leave suggests that expansions in leave duration delay mothers' return to work (e.g., Baker and Milligan 2008b, Lalive and Zweimüller 2009, Schönberg and Ludsteck 2014). By increasing the time spent away from work, the availability of paid and job-protected maternity 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 maternity 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 et al. 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 maternity leave affects their subsequent attachment to the labor market. The direction of this indirect effect – and a possibly resulting impact on maternal health – is theoretically ambiguous, though. On the one hand, 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). However, incentives to postpone one's return to work, set by very generous maternity leave programs, might also result in lower labor market attachment and potential economic disadvantages in the long-term, as time spent out of the labor force may devaluate the individual's human capital and lower her earnings (e.g., Buligescu et al. 2009). Moreover, extensions of leave duration have also been shown to have a variety of unintended consequences. Puhani and Sonderhof (2011), for instance, document that maternity 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 causal studies provide little support for adverse long-run employment consequences (cf. Lalive and Zweimüller 2009 for Austria; Schönberg and Ludsteck 2014 for Germany). 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.

Next to the causal effects discussed above, a further channel through which

expansions in leave coverage might alter post-birth health outcomes of those mothers who have returned to the labor market are potential compositional effects. A key concern is that an expansion in leave coverage might give rise to differences in the dependencies of return-to-work decisions on health status. Note that the direction of this effect 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 potentially disentangle such compositional effects from causal mechanisms, we explicitly explore the short- and long-run association between the expansion in maternity leave and mothers' health. This seems particularly important because the direction and underlying mechanisms of any causal effects are likely to vary over mothers' life-course (e.g., Chatterji et al. 2013). In particular, we argue that the sign of causal health effects should depend on the time horizon as spelled out below:

1. If maternity leave has a direct causal health effect in the short run, we expect to observe a positive effect, where (longer) leave protects mothers from health impairments related to the aftermath of childbirth. In this case, the effect of leave on well-being should be primarily mediated through the effect of the leave extension on mothers' delay in return to work shortly after childbirth.

2. Long-run causal effects of maternity leave extensions on 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. If the latter effect dominates, one might also expect a negative causal effect of a longer leave duration on maternal health in the medium and long run.

5 Data and Empirical Strategy

5.1 Data

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 (see Hochfellner et al. 2011).[1] 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.

The *Pension Register* contains information on all periods for which contributions were paid (such as employment, long-term illness and 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 illness 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

[1] Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequent remote data access.

more detailed description of the individual characteristics provided by the *Pension Register*.

Starting from 1975 (in western 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 Labor Market Biographies and* the *Establishment History Panel*. The *Integrated Labor Market Biographies* provide further time varying individual information on educational status (three categories) and an establishment identifier. The latter allows us to retrieve information on tenure at the current employer. Table A2 in the Appendix provides a more detailed description on the variables gained from the *Employment Statistics Register*.

5.1.1 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 (Kreyenfeld and Mika 2008). Despite this advantage over other administrative data sets, the data offer some disadvantages as well. First, recorded births generally pertain to the child's parent who claims the (pension relevant) period of childrearing. Thus, the data may also include fathers with a recorded birth. However, the pension insurance records the period of childrearing as a default for the child's mother - fathers may claim childrearing periods only upon formal request (Kreyenfeld and Mika 2008). As a result, the fraction of fathers claiming the period of childrearing has been negligible. Second, 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 post-birth employment spell. Thus, while the data allow us to precisely measure post-birth employment interruptions[2], they are not informative about the effective take-up of benefits. However, given

[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.

that we are interested in the *duration* of job protection (as opposed to the take-up of benefits), we argue that this constitutes a minor restriction.

5.1.2 Spells of long-term sickness absence

We retrieve information on spells of long-term sickness absence as a measure of mothers' post-birth health status. 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 (either from employment or unemployment) 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.

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, using spells of long-term illness as an indicator of mothers' health has the clear advantage that one obtains a consistent measure of health over the whole available observation period. 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. Moreover, different

from short-term sickness absence, which has often been suggested to indicate absenteeism rather than ill health (Marmot et al. 1995; also see Johansson and Palme 2002), long-term sickness absence spells may be expected to provide a highly reliable measure of illness. The reason is that the misuse of sickness payments is strongly restricted by the health insurance's auditing system. In particular, the medical service run by the health insurance has the right to audit individuals' sickness absence, if the health insurance expresses profound doubts about its acceptability.[3] Audits may be performed either based on an assessment of the documentation provided by the medical doctor who ascertained the individual's inability to work, or based on a personal assessment of the individual's ability to work by the service's medical staff. Objection against the result of medical service's audit is possible, but must be based on another medical expert's opinion. Overall, this auditing system renders misreporting or misuse of long-term sickness payments very difficult and costly.

Despite the overall advantages over self-reported health measures, our measure has some limitations as well. First, our measure may be somewhat conservative 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 obtain sufficient variation in this measure. Second, even though our proposed measure is not strictly contingent upon employment after childbirth, it clearly conditions on labor market participation. As mentioned earlier, this may imply compositional effects, which we will address below.

5.2 Empirical Strategy

As the reform we consider in this study has been implemented depending on the child's birthday, the policy change creates a natural experiment which allow us to assess how changes in maternity leave duration affect post-birth return-to-work behavior. To identify the ITT (intention-to-treat) effect on mothers' return-to-work

[3]The medical service's auditing rights are laid out in § 275 of Book V of the German Social Code (SGB V). The medical service was originally established in the 1920s as the "*Vertrauensärztlicher Dienst*". Since the late 1980s, it has been referred to as the "*Medizinischer Dienst der Krankenversicherung*" (Medical Service of the Health Insurance).

behavior, we use a regression-discontinuity design (RDD) where the treatment is defined by the extension of leave protection. The ITT measures the average effect of the 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 exploits the fact that for children born just after the introduction of the changed legislation, mothers could not anticipate the upcoming reform at the time of conception. At least for births just before and just after the 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 reform has been seriously debated and announced to the public. For the 1979 reform considered here, the validity of the no-anticipation assumption has already been investigated by Dustmann and Schönberg (2012). Analyzing newspaper articles that appeared prior the reform, the authors show that the change in maternity leave legislation was 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 reform.

Even though the policy change allows us to assess the causal impact of a prolonged maternity leave duration on post-birth return-to-work behavior, one needs

to be cautious in inferring conclusions about the causal effects on health outcomes. Given that our health measure conditions on labor market participation, it restricts us to recover a joint effect comprising both a causal and a potential unobservable compositional component. The reason is that the reform might have given rise to differences in the dependencies of return-to-work decisions on health status. As argued in Section 4, the direction of such a potential effect is not clear a-priori. To address this issue, we will examine the relationship between the observable pre-birth health status 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) illness 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 driven by systematic differences in the return behavior across both groups.

6 Empirical Results

6.1 Leave durations

As set out above, the treatment (control) group consists of mothers who gave birth after (before) the change in maternity leave legislation. We choose an observation window of four months before and after the threshold date. 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. Overall, this results in a sample comprising 977 observations, which corresponds to 42.5 per cent of the total number of births (2294) observed between January and August 1979 in our data. This covers about 60 per cent of a one per cent sample of the official number of births in 1979 in Western Germany.[4]

[4]This number derives from $\approx (580{,}000/12)\cdot 8$. There are several reasons for not observing the full number of births: First, the data typically exclude or underreport employment biographies of civil servants and the self-employed. Second, due to the data's restriction to cohorts born after 1939 we do not observe all relevant cohorts at risk of birth in 1979. Third, until 1967 married women

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

Variable	Mean	Std.-Dev.	Mean	Std.-Dev.
	Treated		Control	
Post-birth leave duration				
(1) LEAVE DURATION[a)]	105.2	5.78	100.88	5.84
(2) FRACTION NEVER RETURNING	0.18	0.017	0.15	0.017
(3) LEAVE DURATION (CONDITIONAL ON RETURNING)	53.35	3.58	56.58	3.91
Observations	496		471	

Source: BASiD 2007. Note: [a)]Leave duration measures the number of months that elapse until the first post-birth employment spell.

Table 2: Descriptive Statistics by Treated and Control Mothers

Variable	Mean	Std.-Dev.	Mean	Std.-Dev.	$t-$Test
	Treated		Control		P-Value
Individual characteristics[a)]					
AGE	26.00	4.87	26.29	4.77	0.37
FIRST BIRTH	0.64	0.48	0.64	0.48	0.95
SECOND BIRTH	0.27	0.27	0.27	0.27	0.90
THIRD OR HIGHER PARITY	0.09	0.29	0.09	0.29	0.76
LOW-SKILLED	0.38	0.49	0.38	0.49	0.83
MEDIUM-SKILLED	0.57	0.50	0.58	0.50	0.83
HIGH-SKILLED	0.03	0.16	0.03	0.18	0.60
SKILL MISSING	0.01	0.11	0.01	0.08	0.35
Individual pre-birth work history[a)]					
TENURE[b)]	28.45	19.01	30.13	16.77	0.15
EMPLOYMENT-DURATION	76.92	49.66	81.40	49.92	0.16
UNMPLOYMENT-DURATION	0.01	0.31	0.02	0.30	0.79
NON-EMPLOYMENT-DUR.	18.14	27.23	18.16	29.20	0.98
ILLNESS-DURATION	0.62	2.36	0.54	1.81	0.56
#UNEMPLOYMENT SPELLS	0.45	0.86	0.37	0.73	0.12
#NON-EMPLOYMENT SPELLS	2.14	1.77	1.97	1.59	0.14
CUM. EARNINGS in 1000 Euro	47.3	31.3	49.7	32.6	0.24
Observations	496		471		

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

Table 1 reports differences in leave durations after childbirth. Row (1) indicates that treated mothers, i.e. those giving birth from May to August 1979, spend on average more time away from work than control mothers (105.2 as compared to 100.88 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 slightly raises the fraction of mothers never returning to the labor market. Conditional on returning, treated mothers feature somewhat lower average leave durations than control mothers. This suggests that the unconditional larger leave duration among the treated can be fully explained by a larger fraction never returning to work. Given that we expect an expansion in leave coverage to delay mothers' return to work, the conditional shorter leave duration among the treated deserves some more attention. In the next section, we will explore whether this result is driven by potentially long or medium-run positive effects that may counteract the short-run negative effects.

To address whether the observed differences in return-to-work behavior stem from systematic differences across treated and control mothers, Table 2 provides descriptive evidence on a number of pre-birth characteristics. The figures indicate that both groups appear to be quite similar with respect to age and education. Treated and control mothers also exhibit similar amounts of previous employment, unemployment and non-employment experience. The same is true for the pre-birth illness duration which can be taken as an indicator of mothers' pre-birth health status.

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had the possibility to apply for an advance payment of their pension entitlements, in which case their pension records were completely deleted. Note, however, that this latter restriction is very unlikely to affect our sample selection, as that these women should have had a weak attachment to the labor market.

6.2 Labor Market Outcomes

6.2.1 Descriptive Results

We next explore to what extent the extension of the mandated maximum leave duration affect mothers' post-birth 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*). A mother is defined to have returned to the labor market if she is employed for at least two consecutive months after childbirth. 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 month m or year t after childbirth (*Employed*). Third, to capture the intensive dimension of employment, we also look at the cumulative number of months worked by year t after childbirth (*Months worked*).

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 38 % of control mothers, but only 6 % of treated mothers return to work after two months following childbirth. 17.9 % of the treated and 21.9 % of the control mothers return to work before the mandated leave duration has run out. This implies that 82.1 % of the treated and 78.1 % of the control mothers fully exhaust the maximum mandated leave duration. While 21.8 % of the treated and 16.3 % of the control mothers exactly return to work when the mandated leave duration has run out, 60.3 % of the treated and 61.8 % of the controls continue to stay away from work at the end of the job protection period. The figure further reveals that the delay in return to work appears to be only of short-run nature since after two years the fraction returned is about 2 percentage points larger among the treated than for control mothers.

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 the return-to-work advantage among the treated disappears about 4 years after childbirth. After 20 years the pattern tends to reverse as control mothers exhibit slightly larger return-to-work probabilities. After 28 years, 82% of treated mothers ever return to the labor market compared with 85% among the controls. Figure 2 (A) and (B) show that the control group features a larger fraction employed as well as a larger cumulative number of months worked by year t especially during the last years of our observation period.

6.2.2 Regression Results

Table 3 reports regression results to assess the leave extension's 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 three months as well as three, ten and 28 years after childbirth, respectively.

Column (1) displays the baseline differences in the outcomes at different points in time with the figures corresponding to those shown in Figure 1. The estimates in the first panel show that treated mothers are significantly less likely to have returned to work three months after childbirth, with the difference amounting to 32 percentage points. The difference in the fraction employed is also estimated to be significantly negative in the short run (i.e. three months after childbirth). A similar

Figure 1: Return-to-work behavior

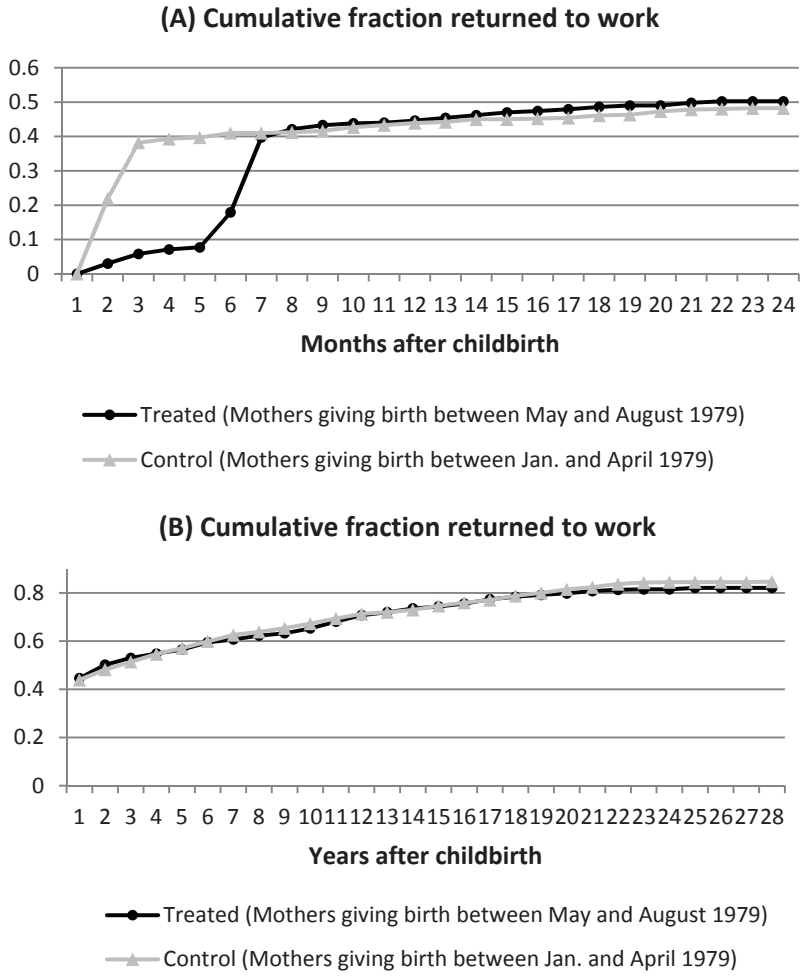


Figure 2: Fraction employed and cumulative months worked

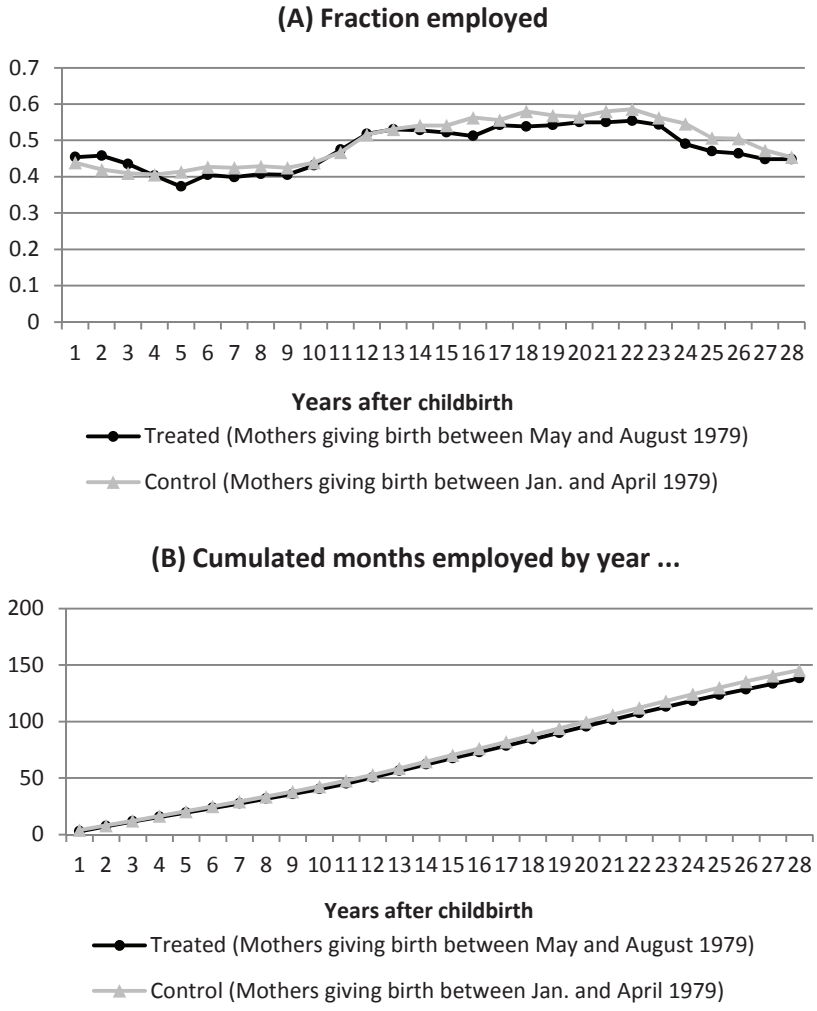


Table 3: Regression Results Labor Market Outcomes

	Base	Controls	Placebo	Difference
	(1)	(2)	(3)	(4)
3 Months after Childbirth				
Return-to-work	-.324***	-.325***	-.038	-.287***
	(0.025)	(0.024)	(0.027)	(0.036)
Employed	-.324***	-.325***	-.038	-.287***
	(0.025)	(0.024)	(0.027)	(0.036)
Cum. months worked	-.406***	-.040***	-.017	-.384***
	(0.035)	(0.033)	(0.033)	(0.047)
3 Years after Childbirth				
Return-to-work	0.016	0.017	-.029	0.046
	(0.032)	(0.028)	(0.027)	(0.038)
Employed	0.026	0.031	-.036	0.068*
	(0.032)	(0.026)	(0.026)	(0.037)
Cum. months worked	-.598	-.255	-1.161*	0.907
	(0.883)	(0.704)	(0.694)	(0.985)
10 Years after Childbirth				
Return-to-work	-.020	-.023	-.001	-.021
	(0.030)	(0.025)	(0.024)	(0.034)
Employed	-.008	0.005	0.030	-.026
	(0.032)	(0.025)	(0.024)	(0.034)
Cum. months worked	-2.519	-.579	-.870	0.291
	(2.855)	(1.672)	(1.570)	(2.228)
28 Years after Childbirth				
Return-to-work	-.027	0.000	-.006	0.007
	(0.024)	(0.020)	(0.018)	(0.027)
Employed	-.007	0.014	0.025	-.011
	(0.032)	(0.026)	(0.025)	(0.036)
Cum. months worked	-7.336	3.596	2.758	0.837
	(7.322)	(2.433)	(2.221)	(3.272)

Source: BASiD 2007.
Notes: Robust standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, 10%-level. Column (1) contains the baseline estimates, Columns (2) adds control variables. Column (3) reports Placebo estimates corresponding to those in Column (2) for mothers giving birth between January and August 1978. Column (4) reports the difference with respect to Column (2).

pattern emerges for the intensive dimension of employment as treated mothers work approximately 2 weeks (0.406 months) less during the first three months after childbirth. For the return-to-work probability and the fraction employed, the pattern appears to reverse three years after childbirth, even though the positive differences of 1.6 and 2.6 percentage points are estimated fairly imprecisely. The long-term differences in all outcome variables, in contrast, are estimated to be negative and insignificant at conventional levels. How do these results compare to those that have been obtained earlier in the literature for Germany? Schönberg and Ludsteck (2014) estimate differences in the outcome variables that are of the same order of magnitude as our estimates for month three after childbirth.[5] While their medium-run responses differ somewhat from our findings[6], their estimated effects 72 months after childbirth are broadly consistent with our results pointing to small negative return-to-work effects in the long-run.

Column (2) adds a set of control variables such as information on age, education as well as the pre-birth employment and illness histories. The figures show that in the short to medium run the results are quite robust to including controls. 28 years after childbirth the estimated coefficients reverse their sign, but remain still insignificant. 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 (3) reports the estimates corresponding to those in Column (2) for mothers giving birth between January and August 1978, whereas Column (4) reports the differences with regard to the estimates in 1979. The placebo estimates suggest that for the number of months worked per year there are some seasonal effects that appear to confound the differences across treated and control mothers. In year three after childbirth, mothers giving birth between May and August appear to supply

[5]More precisely, the authors' estimates of the differences in the return-to-work probability, the fraction employed and the number of months worked are -30.5, -28.4 and -.406, respectively (see Table 1, Column (1) in Schönberg and Ludsteck 2014).

[6]While Schönberg and Ludsteck (2014) obtain a 1 percentage point lower return-to-work probability 28 months after childbirth, our estimates three years after childbirth point to a positive, albeit insignificant effect. Note, however, that their results are not directly comparable to ours. The reason is that their analysis includes a different set of control variables and is based on a data set that does not allow for a precise identification of career interruptions due to childbirth.

less labor in intensive terms compared with those giving birth between January and April. This difference is in absolute terms considerably larger than the difference in the reform year, such that the estimated net effect in column (4) becomes even positive, albeit insignificantly so. Apart from that, there are no further significant seasonal differences between three and 28 years after childbirth even though the difference in the fraction employed gives rise to a significant positive net effect three years after childbirth. For the other outcomes, in contrast, there are no significant long-run effects three, ten or 28 years after childbirth, respectively.

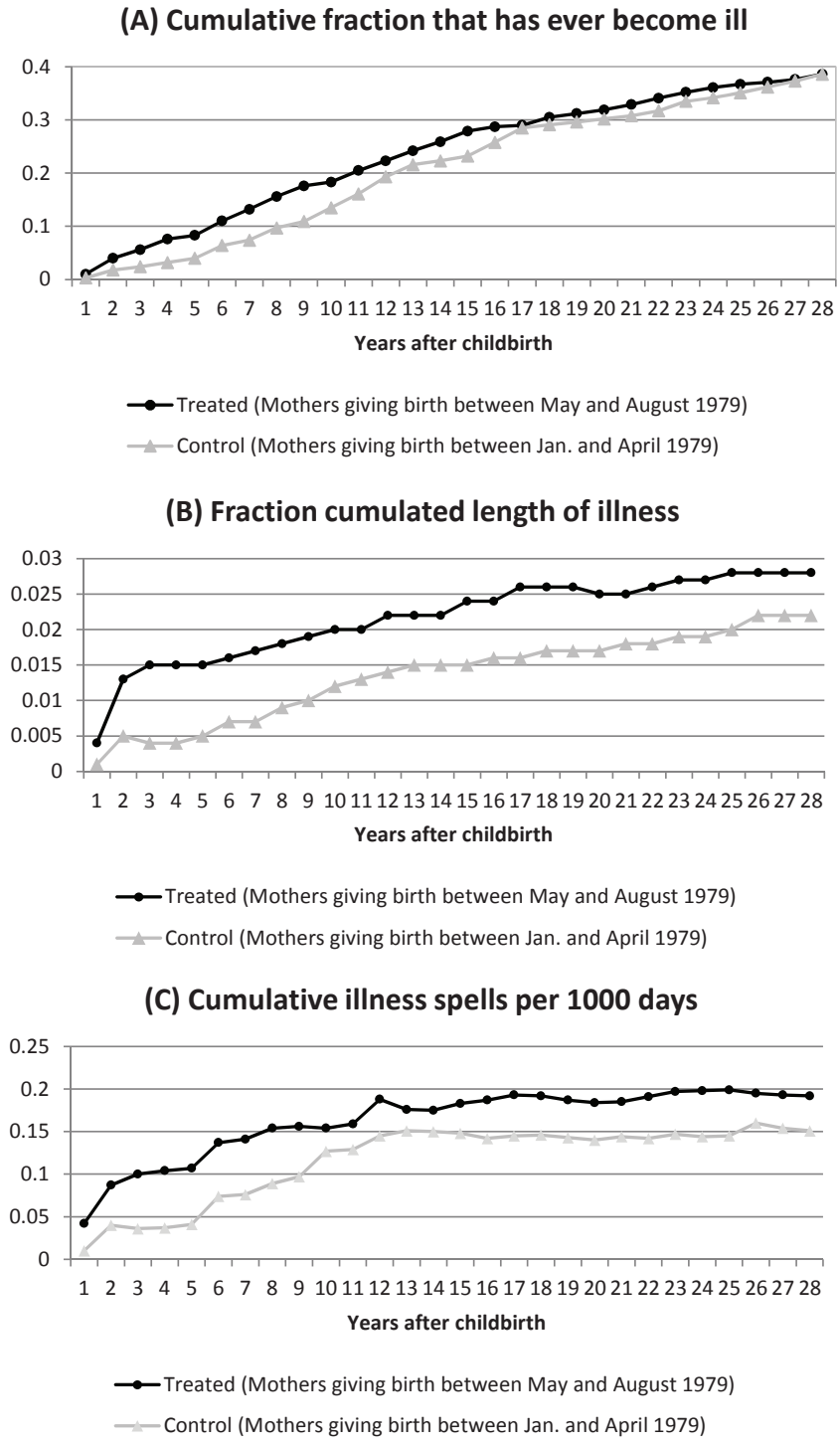
6.3 Health outcomes

6.3.1 Descriptive Results

The empirical analysis thus far has documented a delay in return-to-work caused by the expansion in leave coverage. Moreover, the delay is most pronounced and significant only within the first year after childbirth. Does the reform-induced change in return-to-work behavior after childbirth translate into different health outcomes? To explore this issue, we construct three different health indicators from the information on long-term sickness absence 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 being employed or either unemployed. Motivated by the fact that long-term sickness absence spells represent a relatively infrequent event, we first construct a dummy variable taking on the value of one if a mother has experienced at least one such a long-term illness spell by year t (*Ever become ill*). Second, we also look at the number of such long-term illness spells (*# Illness spells/per 1000 days*) relative to the cumulative time spent in the labor market by year t after childbirth. Finally, to capture the intensive dimension of illness, we compute the cumulative length of all long-term sickness absence spells (*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 (*Ever become ill*), 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 larger fraction of women ever having experienced a long-term illness spell at each point in time. Between year three and eleven after childbirth the difference amounts up to 6.8 percentage points. After this time period the health disadvantage among the treated becomes somewhat smaller and almost disappears starting from year 17 after childbirth. Figure 3 (B) compares the number of illness spells per 1000 days in the labor market after childbirth across treated and controls. The figure shows that over the whole observation period treated mothers experience more illness spells than control mothers, with the difference being largest between year three and nine after childbirth. Evaluated at the mean time spent in the labor market, this implies that after 28 years treated (control) mothers who have returned to the labor market have experienced on average 1.03 (0.88) illness spells. Figure 3 (C) 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 longer duration of illness relative to the time spent in the labor market than control mothers. Even after about 20 years the difference still amounts to about one percentage point, suggesting that the differential tends to be long-lasting. Evaluated at the mean time spent in the labor market the figures imply that after 28 years treated (control) mothers have experienced on average 141 (131) days of long-term illness. The overall result of a health disadvantage among treated mothers is surprising and clearly deserves some further attention. In Section 5.2, we argued that such a finding may be rationalized by a potential negative compositional 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. We will address this potential compositional effect in the next section within a regression framework.

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



6.3.2 Regression Results

Table 4 reports regression results to assess the reform's impact on subsequent health outcomes, explaining the outcome variables the indicator variable "Ever become ill", the number of illness spells and the fraction length of illness by the treatment status as well as a number of controls. For the indicator variable "Ever become ill", the estimates are based on a linear probability model. To further assess the importance of different time horizons, the different panels again report the regression results measuring the outcomes one as well as three, ten and 28 years after childbirth, respectively.

Column (1) displays the baseline differences in the outcome variables at different points in time with the figures corresponding to those shown in Figure 3. The baseline estimates in Column (1) show that the fraction of women ever having experienced a long-term illness spell is 3.2 percentage points larger among the treated as compared with controls three years after childbirth. The difference in the fraction length of illness is also estimated to be significantly positive in the medium run, amounting to 1.1 percentage points three years after childbirth. The same is true for the number of illness spells, with the difference being 0.064 per 1000 days (i.e. per approximately three years) spent in the labor market. Column (2) adds a set of control variables such as information on age, education as well as the pre-birth employment histories. The figures show that three years after childbirth the results are robust to including controls, whereas adding controls leads to a decline in the estimated coefficients after ten and 28 years, respectively. This suggests that among the treated those who return between year three and ten are particularly negatively selected with respect to their observable characteristics. To explore whether the health disadvantage of treated mothers can be explained by a negative health selection among this group, Column (3) shows the results after additionally controlling for pre-birth illness histories.

Table 4: Regression Results Health Outcomes

	Base	Controls w/o illness	Controls incl. illness	Placebo	Difference
	(1)	(2)	(3)	(4)	(5)
1 Year after Childbirth					
Ever become ill	0.007	0.009	0.008	0.007	0.001
	(0.006)	(0.007)	(0.006)	(0.007)	(0.009)
Length of illness	0.003	0.004	0.004	0.002	0.002
	(0.002)	(0.003)	(0.002)	(0.004)	(0.004)
# Illness spells per 1000 days	0.032	0.040	0.037	0.021	0.016
	(0.026)	(0.028)	(0.026)	(0.020)	(0.031)
3 Years after Childbirth					
Ever become ill	0.032**	0.033**	0.021*	-.010	0.031*
	(0.015)	(0.015)	(0.011)	(0.014)	(0.017)
Length of illness	0.011**	0.011**	0.007***	-.001	0.008**
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
# Illness spells per 1000 days	0.064**	0.068**	0.041**	-.011	0.052*
	(0.029)	(0.029)	(0.020)	(0.024)	(0.031)
10 Years after Childbirth					
Ever become ill	0.049*	0.032	0.007	-.020	0.027
	(0.026)	(0.025)	(0.018)	(0.018)	(0.026)
Length of illness	0.007*	0.005	0.001	0.002	-.001
	(0.004)	(0.004)	(0.003)	(0.002)	(0.004)
# Illness spells per 1000 days	0.027	0.003	-.027	0.019	-.046
	(0.037)	(0.038)	(0.033)	(0.020)	(0.039)
28 Years after Childbirth					
Ever become ill	-.000	0.003	-.015	-.035	0.020
	(0.033)	(0.030)	(0.025)	(0.023)	(0.034)
Length of illness	0.006	0.005	0.005*	0.001	0.004
	(0.004)	(0.004)	(0.003)	(0.002)	(0.003)
# Illness spells per 1000 days	0.040*	0.032	0.016	0.003	0.013
	(0.024)	(0.022)	(0.015)	(0.012)	(0.019)

Source: BASiD 2007.
Notes: Robust standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, 10%-level. Column (1) contains the baseline estimates, Column (2) and (3) add control variables excluding and including pre-birth illness differences. Column (4) reports Placebo estimates corresponding to those in Column (3) for mothers giving birth between January and August 1978. Column (5) reports the difference with respect to Column (3).

The positive coefficients for all outcome variables become slightly smaller but are still significant three years after childbirth. Even though the differences between the estimated coefficients from Column (2) to (3) are not significant, the observed declines may be taken as some (weak) evidence of a negative observable health selection among those returned after the reform. 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. Contrary to the labor supply outcomes from Table 3, all placebo estimates turn out to be insignificant, suggesting that there are no distinct seasonal effects confounding the results. In terms of illness duration, the estimates indicates that treated mothers exhibit a 0.8 (0.4) percentage point larger fraction length of illness three (28) years after childbirth. Evaluated at the mean time spent in the labor market, this implies that treated mothers experience about five (13) more (long-term) illness days as compared to control mothers three (28) years after childbirth, respectively.

Overall, the estimates indicate that even after controlling for observable health differences, mothers subject to the leave extension still exhibit unfavorable health outcomes as compared to control mothers. While being particularly pronounced after three years, the health disadvantage is already visible (albeit small and imprecisely estimated) within the first year after childbirth. Note that this finding strongly argues against a dominating substitution effect within the first year after childbirth, inducing those who are subject to the shorter leave duration to substitute maternity leave by an illness episode. At the same time, the results in Section 6.2.2 have shown that there are no pronounced effects on return-to-work behavior within the first three years after childbirth which might rationalize a negative causal health effect for the treated. Overall, these findings suggest that the positive coefficient reflects a further negative health selection upon unobservables.

6.4 Heterogeneous Effects

In this section, we provide further support for the idea that the reform may have facilitated re-entry of a negative (unobservable) health selection of mothers into the labor market. Recall from Section 4 that a potential negative selection effect might stem from longer leave mandates offering mothers with a bad health status the possibility to recover. This suggests that a potential negative health selection upon unobservables is likely to be particularly pronounced for mothers with more unfavorable observable pre-birth illness histories.

To investigate this hypothesis, Figure 4 first illustrates the return-to-work profiles by mothers' pre-birth health status. To do so, the sample is broken down by "Good health" (A) and "Bad health" (B) mothers. A mother is considered "Bad health" if her pre-birth long-term illness duration per year in the labor market exceeds the median of mothers' pre-birth illness durations (which equals zero in our sample) and "Good health" otherwise. According to this definition 124 mothers are classified as being of bad (pre-birth) health and 843 of good (pre-birth) health. Comparing (A) and (B), the figure clearly documents that the return-to-work behavior of both groups is affected differently by the reform. In particular, bad health treated mothers show much larger positive differences in return-to-work probabilities after the extended leave duration has expired as compared to good health treated mothers.

To demonstrate that this difference also holds after controlling for observables and seasonal effects, Table 5 repeats the baseline result from Table 3 and 4 for both groups. For the sake of expositional brevity, the results are reported for the labor market outcomes (*Return-to-work*) and (*Employed*) as well as for the health outcomes (*Length of illness*) and (*# Illness spells*).

Turning to the labor market outcomes in Column (1) and (2) of Table 5, the estimates indicate that the reform causes particularly those with a bad pre-birth health status to increase their short and medium-run labor supply. Moreover, the differences in the labor market effects across bad and good health mothers are significant

Figure 4: Return-to-work behavior by pre-birth health status

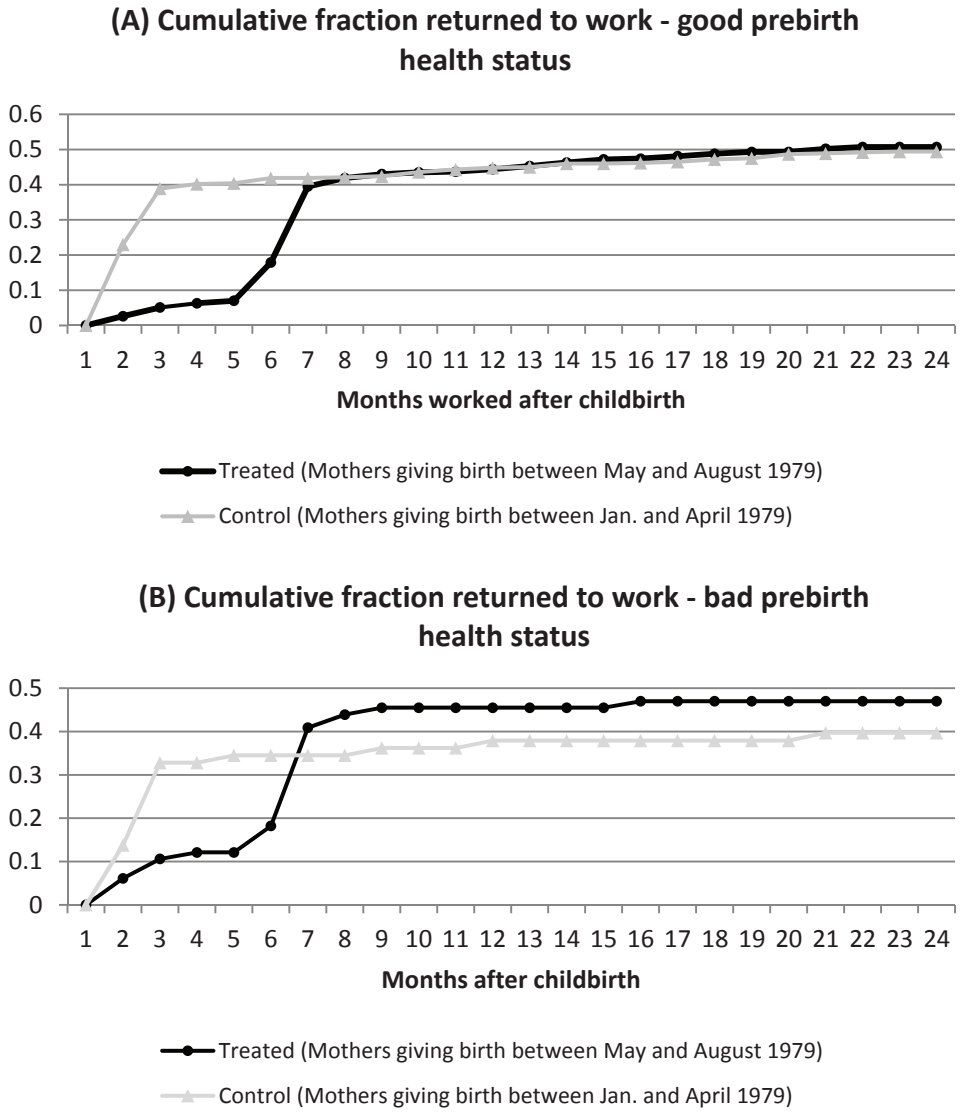


Table 5: Heterogeneous Effects

Labor Market Outcomes	Bad Health	Good Health	Health Outcomes	Bad Health	Good Health
	(1)	(2)		(3)	(4)
			1 Year after Childbirth		
Return-to-work	0.261***	0.041	Length of illness	-.017	0.001
	(0.115)	(0.042)		(0.028)	(0.001)
Employed	0.261***	0.043	# Illness spells	-.027	0.006
	(0.115)	(0.043)		(0.153)	(0.004)
			3 Years after Childbirth		
Return-to-work	0.154	0.024	Length of illness	0.057**	0.001
	(0.108)	(0.040)		(0.023)	(0.002)
Employed	0.160	0.054	# Illness spells	0.481***	0.006
	(0.111)	(0.040)		(0.164)	(0.012)
			10 Years after Childbirth		
Return-to-work	0.088	-.042	Length of illness	0.022	-.004
	(0.098)	(0.036)		(0.019)	(0.003)
Employed	-.000	-.034	# Illness spells	0.043	-.054*
	(0.010)	(0.036)		(0.217)	(0.029)
			28 Years after Childbirth		
Return-to-work	-.053	0.015	Length of illness	0.031*	0.001
	(0.086)	(0.029)		(0.017)	(0.003)
Employed	-.080	-.001	# Illness spells	0.198**	-.012
	(0.104)	(0.038)		(0.101)	(0.016)

Source: BASiD 2007.

Notes: Robust standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, 10%-level. A mother is considered "Bad health" if her pre-birth illness duration per year in the labor market is larger than zero and "Good health" otherwise. The estimates correspond to the Placebo corrected estimates in Column (4) of Table 3 and in Column (5) of Table 4, respectively.

at conventional levels in the first year after childbirth. Note that this difference in return-to-work behaviour was already visible by comparison of Column (2) and (3) of Table 4.

Central to our argument is the question of whether this negative health selection also accounts for the unfavorable health outcomes among treated mothers. To explore this issue, Column (3) and (4) report separate health outcome estimates for both groups. The results indicate that the established health disadvantage among the treated three years after childbirth is strongly driven by bad health mothers. Turning to the long run effects, the health disadvantage is still visible even 28 years after childbirth. Good health mothers, in contrast, do not exhibit major positive and statistically significant differences in health outcomes between treated and controls. Taken together, these results therefore strongly support the view that the reform appears to have induced particularly those with a bad pre-birth health status to re-enter the labor market and that the unfavorable health outcomes among treated mothers are driven by this negatively selected group.

6.5 Robustness Checks

In this section, we conduct several robustness checks. As a first restriction, we chose a relatively large observation window of four months before and after the reform. To check whether our results are robust to the adopted time window around the reform's threshold date, we re-estimated the model by reducing the time window to two (instead of four) months before and after the reform. The results for the labor market and health outcome variables corresponding to the placebo-corrected estimates in the last columns of Table 3 and 4 are shown in Column (1) and (2) of Table A3 in the Appendix. As to the labor market effects, the results in Column (1) indicate that especially the magnitude of the short-run effects seems quite robust and varies only slightly over the particular time window one adopts. Even though the estimates in the medium and long-run differ somewhat from those in Table 3, the pattern of insignificant medium and long-run results is very similar to that in Table 3. As to the health outcomes, the estimates in Column (2) also corroborate

the pattern of results that has been found earlier. An exception is the estimated difference in the fraction ever become ill after 28 years, which is now considerably larger than in Table 4.

A second concern is that we excluded from our illness episodes short-term spells up to a length of 5 days (before 1992) or 10 days (after 1992), respectively. This affects 172 out of the total number of 1,002 post-birth illness spells we observe in our data. We did so to address the fact that these shorter spells may also cover caring periods for ill infants below the age of 12. If the distribution of shorter and longer spells systematically varies across treated and control mothers, this exclusion may potentially bias our estimates. To deal with this issue, we re-estimated our regressions after including these shorter spells in our health outcomes. The results corresponding to those in Column (5) of Table 4 are reported in Column (3) of Table A3. The estimates show that the magnitude of the effects again seems to be quite robust to including these shorter spells. This is particularly true three years after childbirth, where the estimated differences between treated and control mothers turned out to be most pronounced. Here, the estimated differences are very similar to those in Table 4. The difference in the number of illness spells is estimated somewhat more imprecisely, but borders the 10-% significance level.

7 Summary and Conclusions

The purpose of this study was to explore the relationship between the 1979 expansion in maternity leave coverage in Germany and long-term sickness absence among mothers who returned to the labor market after childbirth. By increasing the leave duration from two to six months, this reform explicitly aimed at improving working mothers' health by alleviating the "double burden" of childrearing and paid employment immediately following childbirth. Exploiting unique administrative data from the German Pension Register and Federal Employment Agency, we estimate the association between the reform induced expansion in leave duration with mothers' long-term sickness absence over a period of up to 30 years after childbirth. Adopting

a regression discontinuity approach, we exploit the fact that the reform caused an exogenous variation in mothers' actual leave uptake behavior. Consistent with what has been found earlier in the literature, our results suggest that the leave extension caused mothers to significantly delay their return to work within the first year after childbirth. To explore whether the reform-induced change in return-to-work behavior after childbirth translates into different health outcomes, we compare the number and length of long-term sickness spells of gainfully employed mothers who gave birth before and after the change in leave legislation. In conditioning on labor market participation, our analysis is informative about the health outcomes of those mothers who returned to the labor market.

Our findings suggest that mothers subject to the leave extension exhibit a higher incidence of long-term sickness absence (in terms of the intensive and extensive dimension) as compared to control mothers three years after childbirth. This result also holds after controlling for observable pre-birth illness differences. Because there are no pronounced effects on mothers' labor market participation following the short-run delay in return to work which might rationalize a negative causal effect, the less favourable health outcomes among the treated might reflect the lower bound of a negative unobservable health selection. To provide further support for this idea, we break down the estimates by mothers' observable pre-birth health status. Two major findings emerge from this analysis: First, the leave expansion induced particularly those with a bad pre-birth health status to re-enter the labor market. Second, the unfavorable health outcomes among treated mothers are mainly driven by this negatively selected group. This lends support to hypothesis that the 1979 reform has indeed facilitated re-entry of a negative health selection of mothers into the labor market.

Taken together, our findings lead us to conclude that the reform failed to improve the "quality" of female labor market participation by supporting the return of a larger fraction of healthier female workers to the labor market. The overall benefits of expansions in maternity leave might therefore be very small (or even negative), if

such reforms are not complemented by further measures directed at maintaining or improving working mothers' health following their return to the labor market.

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8 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 childrearing, care giving as well as periods with missing information on the employment status.
ILLNESS	Illness spells includes periods of long-term sickness absence ($>$ 6 weeks) and periods with long-term rehabilitation measures.
# UN(NON)EMPLOYMENT_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 A1: 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
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 up to the contribution limit of the German social security system.

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

Labor Market Outcomes	Half-window[a)]	Health Outcomes	Half-window[a)]	Short-term spells[b)]
	(1)		(2)	(3)
		3 Months/ 1 Year after Childbirth		
Return-to-work	-.315***	Ever become ill	0.004	0.010
	(0.049)		(0.008)	(0.010)
Employed	-.315***	Length of illness	0.003	0.004
	(0.049)		(0.003)	(0.004)
Cum. months worked	-.415***	# Illness spells	0.023	0.067
	(0.063)		(0.035)	(0.045)
		3 Years after Childbirth		
Return-to-work	0.001	Ever become ill	0.042*	0.031*
	(0.005)		(0.024)	(0.018)
Employed	-.007	Length of illness	0.008	0.009**
	(0.056)		(0.005)	(0.004)
Cum. months worked	0.907	# Illness spells	0.072*	0.055
	(0.985)		(0.040)	(0.035)
		10 Years after Childbirth		
Return-to-work	-.032	Ever become ill	0.025	0.016
	(0.047)		(0.034)	(0.027)
Employed	0.018	Length of illness	0.004	-.001
	(0.047)		(0.006)	(0.004)
Cum. months worked	0.291	# Illness spells	-.047	-.054
	(0.228)		(0.068)	(0.042)
		28 Years after Childbirth		
Return-to-work	-.021	Ever become ill	0.076*	0.011
	(0.038)		(0.046)	(0.036)
Employed	0.065	Length of illness	0.003	0.004
	(0.050)		(0.004)	(0.003)
Cum. months worked	0.837	# Illness spells	0.005	0.013
	(0.327)		(0.023)	(0.023)

Table A3: Robustness Checks

Source: BASiD 2007.

Notes: Robust standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, 10%-level. Half-window defines treated (control) mothers as those giving birth in May and June (March and April) 1979. Short-term spells also include illness spells up to 5 (10) days before (after) 1992, respectively. The estimates correspond to the Placebo corrected estimates in Column (4) of Table 3 and in Column (5) of Table 4, respectively.