



Sick of family responsibilities?

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Abstract

This study estimates the effect of parenthood on the within-couple gender gap in paid sick leave. We find that as a result of parenthood, mothers more than double their sick leave compared with fathers. However, there is no corresponding effect on health measured by hospital stays. We also find that mothers' income trajectory is strongly related to the magnitude of the effect: A less favorable income trajectory is associated with a larger effect of parenthood on the sick leave gap. Since mothers' labor supply is measured 1 year prior to sick leave, this result suggests that the lower labor supply induces an increase in sick leave rather than the other way round.

Keywords Sick leave · Parenthood · Double burden · Health investment · Moral hazard

JEL Classification C23 · D13 · I19 · J22

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1 Introduction

In the OECD countries, expenditures on disability and sickness insurance programs are large: The average spending on such programs amounted to 10% of total public spending in 2005, which is more than twice as much as the spending on unemployment programs (OECD 2009).¹ Moreover, in most EU-countries, the disability and sickness insurances are used to a larger extent by women (EurWORK 2010). This is seen clearly in Fig. 1, showing the gender gap in the absence due to illness (“sick leave”) in eight European countries over the period 1983–2008. A similar picture appears in the USA, where Stewart et al. (2003) show that lost productive time due to self-reported personal illness is 30% higher among females than among males.² Furthermore, the American Time Use Survey shows that among full-time workers (who are parents of children under 18), married fathers worked about one hour more per day than did married mothers (U.S. Bureau of Labor Statistics 2008). This difference partly reflects married mothers’ greater likelihood of being absent from work.

The focus in this paper is on the potential effect of parenthood on the gender gap in sick leave. Given the large amount of public resources spent on sick leave and disability, studying the causes behind the gender gap is important in itself. In addition, the gap in sick leave relates to, and probably can explain part of, the gender gap in pay (as suggested by, e.g., Waldfogel 1998; Skipper and Simonsen 2012). There is also a link to the gender difference in lifetime income and thereby the pension level, since lifetime income is directly affected by less hours worked.

There is a growing literature relating the gender gap in pay to the gender-differential behavior that emerges in connection to parenthood (Angelov et al. 2016; Goldin 2014; Kleven et al. 2015; Bertrand et al. 2010). In this paper, we follow the empirical strategy applied in Angelov et al. (2016) but the focus is here on the use of the sickness insurance. Earlier literature has presented mixed evidence on whether having children at home is associated with more or less absence due to sickness (e.g., Mastekaasa 2000; Markussen et al. 2011). However, few earlier studies explicitly address the casual link between parenthood and sickness absence among mothers. One exception is Skipper and Simonsen (2012) who address the casual link between parenthood and wage development and the authors show that one of the major reasons for lower wages among mothers is their higher absence level, which in many cases is due to sickness.

An important advantage of the present study in contrast to the existing literature is that we have access to two different measures of health, namely the use of sickness benefits, which includes a large part of individual discretion, and a more objective one: spells of in-patient hospital care (“hospital stays”). Both measures stem from rich individual-based register data and cover the same population. Having these two health measures allows us to investigate possible causes for the effect found. The longitudinal aspect of the data allows us to study the process and to estimate both short- and long-run effects on both outcomes. The registers contain information on days with sickness benefits paid by the Swedish Social Insurance Agency, and we

¹ In Sweden and the US, spending was 16 % and 9 % of GDP, respectively. Spending on sickness benefits was about the same number as on disability benefits in both countries.

² Data is from the American Productivity Audit telephone survey, consisting of a random sample of 28,902 US workers designed to quantify the impact of health conditions on work.

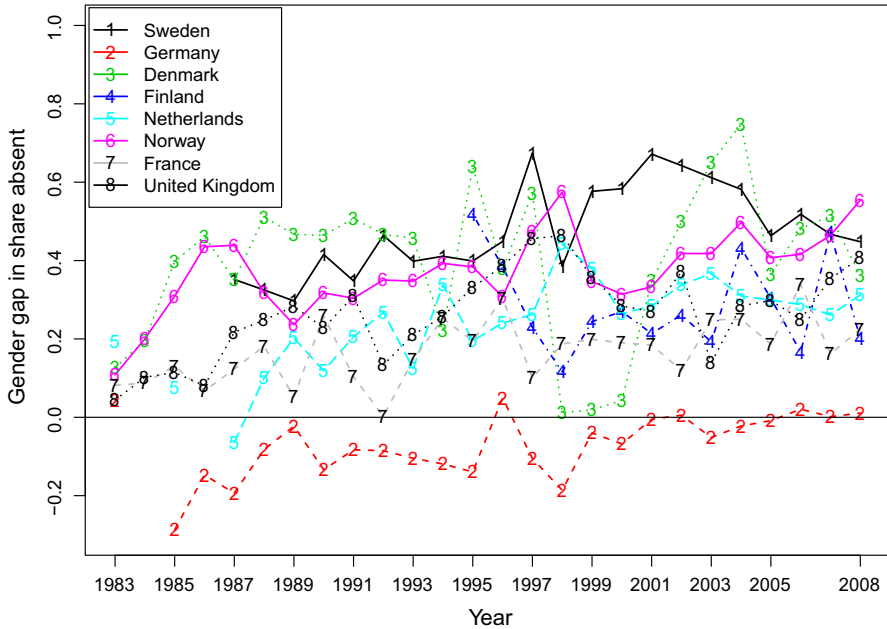


Fig. 1 The percentage female–male gap in sick days during a study-period of 1 week among employed workers in eight European countries. *Source:* Eurostat

have also access to all hospital stays from the National Board of Health and Welfare. Since sickness benefits are paid from the 15th day within a given sick spell, this means that sick leave is measured for spells longer than 14 days.

In the analysis, we compare women's sick leave and hospital stays in relation to their male partners' before and after parenthood. By controlling for the pre-birth within-household gender gap in sick leave and hospital stays, respectively, we control for differences between the genders, which could be due to differences in occupations, economic incentives, preferences, health, etc. Our identification strategy relies on a strict exogeneity assumption of the timing of parenthood, as in a standard difference-in-differences framework. Since we study the effect of parenthood for those who become parents, the identification assumption in our case is that the timing of birth cannot be determined by an expected increase/decrease in the sick leave of the mothers' in contrast to fathers' sick leave or health. Such an assumption can of course always be questioned, and we therefore discuss identification at length later in the paper. It is, however, worth outlining already here why we believe that the identification assumption is met: First, we observe the sick leave and hospital stays on a monthly basis for both men and women within matched couples during a period of 4 years before child birth. As it takes some months of planning and in most cases several attempts before the actual conception can take place, our data allow us to observe the mothers' and fathers' sick leave during the planning period. If the couples' planning is based on within-couple differences in expected future sick leave, this would show up in diverging trends in sick leave before conception. In addition to a visual inspection of

the data, we set up a more formal test taking the form of a unit root test. Furthermore, if expected health changes affect the timing of parenthood for women but not for men (or vice versa), this would be seen in a gradual divergence of women in contrast to the men (or vice versa) in the more objective health measure, hospital stays. Thus, using hospital stays as an alternative outcome variable is also a way to investigate the identification strategy. Finally, we also perform a placebo test on a sample where we know that there should be no parenthood effect.

The starting point for the reason why mothers are more absent due to sickness than fathers is the fact that mothers take more responsibility for child care than fathers do. This fact is confirmed in time-use studies (e.g., OECD 2010a; U.S. Bureau of Labor Statistics 2008) as well as by the fact that more women than men work part-time while having small children. However, in the Swedish context, mothers in general remain in the labor market, implying that they face multiple roles as mothers: labor market work and a new commitment at home. This could, according to the multiple strain theory, be detrimental for an individual's health and may, thus, increase the sick leave.³ Thus, according to this theory, women's health would deteriorate after entering parenthood. Another theory, suggested in Paringer (1983), is that, due to women's dual role, female health is likely to be more important for the household than male health. The reason is that female illness does not only imply lost earnings, but also creates an additional cost in terms of lost home production. In this setting, it may be rational for the household to take more precautions in the case of the woman's illness, by increasing female work absence more than for a similar illness of the spouse (see Avdic and Johansson (2016) for a recent study providing some support for this theory).

A related hypothesis to the Paringer hypothesis investigated in this paper and further discussed in Sect. 2 is that the gender gap in sick leave is driven by parenthood in combination with factors related to the labor market. It is well established that there is room for substantial discretion in the use of sickness insurance programs and that the level of absence depends on the incentives for labor market work (see, e.g., Johansson and Palme 2005). Parenthood implies a new and inevitably time-consuming task at home. A response to this new home commitment could be to reduce labor supply on the intensive margin, either by a reduction in work hours or/and by an increase in the time on sickness benefits. This reduction in labor supply may later affect the incentives for sick leave.

Our results show substantial effects of parenthood on the within-couple gender gap in sick leave for couples who become parents. The within-couple gender gap in sickness increases with between 0.24 days per month (during the child's fifth year) and 1.01 days per month (during year 16). This is a substantial increase when compared to a pre-child gap of 0.16 days per month. Importantly, this difference in response is not likely caused by the child's illness. There are two reasons for this: First, there is a separate (more generous) insurance when caring for a sick child. Secondly, as the sick leave is based on sick-leave spells longer than 14 days, it seems unrealistic that the reason is more exposure to child illnesses as colds or influenzas. Finally, by an extended analysis, we can rule out that the effect stems from later pregnancies.

³ Another theory is that multiple roles actually is beneficial for the health—see Mastekaasa (2000).

Using the same methodological framework on hospital stays, we find no support for a health deterioration among females after entering parenthood. Indeed, we find some evidence for the opposite. Instead, the most convincing explanation we find for the effect of parenthood on the sick leave gap is related to the change in economic incentives among mothers due to parenthood. Mothers' increased commitment at home, induced by parenthood, appears to reduce their incentives for labor market work and thereby lowers their threshold to be on sick leave. To summarize, we find evidence supporting the hypothesis that the effect of parenthood is driven by factors related to the labor market, rather than by health-related factors.

The rest of this paper is organized as follows: Section 2 provides a short literature review on gender differences in sick leave. Section 3 describes the Swedish social insurance system, and in Sect. 4 we discuss identification and empirical strategy. Section 5 describes the data, and Sect. 6 contains the main results. Section 7 presents the analysis of the possible explanations for the effect of parenthood on the gender gap. Finally, Sect. 8 concludes the paper.

2 Gender differences in sick leave and family responsibilities

Women, irrespective of marital status and whether children are present at home, are more absent due to sickness than men (e.g., Mastekaasa 2000; Markussen et al. 2011). Ichino and Moretti (2009) study whether short-term differences in the absence could be a consequence of the menstrual cycle and find some evidence of a monthly pattern for women but not for men. However, the validity of these results was later questioned in Herrman and Rockoff (2012). Another explanation building on, mainly, evidence from experimental studies where women are being seen as more risk averse than men (cf. Eckel and Grossman 2008; Croson and Gneezy 2009; Bertrand 2010) is studied in Avdic and Johansson (2016). They find substantial differences across gender in the take up rate of sick leave after a hospital stay. The gender differences is attributed to differences in preferences for health.

The majority of studies, however, have focused on differences in family responsibility or parenthood (see e.g., Paringer 1983; Åkerlind et al. 1996; Bratberg et al. 2002; Mastekaasa 2000; Angelov et al. 2011; Rieck et al. 2013) for explaining the gender gap in sick leave. That is also the focus of this paper.

Starting with a broad perspective, sick leave seems to be related to female labor supply. Figure 2 shows that the trend of an increasing gender gap observed in Fig. 1 in sick leave for most countries coincides with the trend of an increasing female labor supply. Furthermore, countries with high labor supply of women have large gender gap in sick leave.

Today, the dual earner family is the most common family form in the OECD countries.⁴ Family responsibilities are, however, not equally shared: Women are active in both the labor market and perform the majority of the household production, while men predominantly specialize in market work (see, e.g., Boye 2008; Booth and Ours

⁴ The median employment rate for partnered mothers in the OECD countries was 66.5% in 2007 (OECD 2010b), and according to the US Bureau of Labor Statistics (2011), the US labor force participation rate of mothers with children under 18 years of age was 71.3% in March 2010.

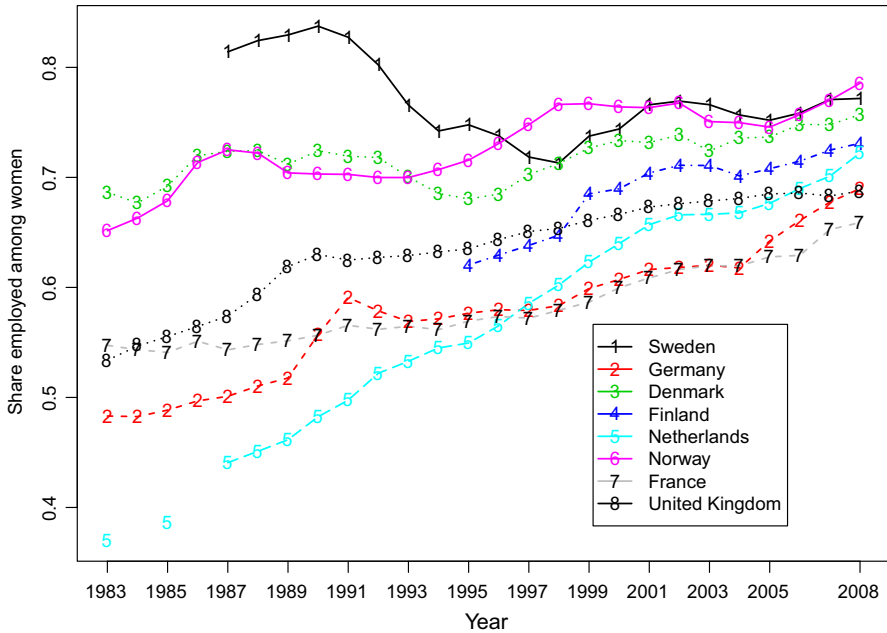


Fig. 2 The percentage of women employed, in eight European countries. Source: Eurostat

2009; Evertsson and Neramo 2007; Tichenor 1999). More effort at home would in general mean less time and effort for labor market work, which is also what we observe: time-use studies in Sweden have consistently shown that labor market work is higher for men, but that total time worked (household and labor market) of men and women is approximately the same (SCB 2009). This result is well in line with time-use studies in the USA, Germany, and the Netherlands (Burda et al. 2008). It has also been empirically established that the unequal gender division of household and market work emerges when couples have their first child (Lippe and Siegers 1994; Sanchez and Thomson 1997; Gauthier and Furstenberg 2002; Gjerdingen and Center 2005; Baxter et al. 2008) and that fertility affects the female labor supply negatively (e.g., Angrist and Evans 1998; Jacobsen et al. 1999), while leaving the labor supply of fathers unchanged, or if anything, increasing it (Kennerberg 2007). The question is if the females' larger family responses can be an underlying cause for their higher sick leave?

Early studies on the effect of family responsibilities compare the sick leave of mothers to non-mothers or that of married to non-married. Åkerlind et al. (1996) estimate gender differences in sick leave at different ages separately for individuals with and without children and find that the gender gap in sick leave is related to custody of small children. More closely related to the present study are Bratberg et al. (2002), Mastekaasa (2000) and Paringer (1983). The theoretical starting point of the two first papers is that the gender gap stems from the psychological pressure of the dual role, or in other words, what they refer to as a double burden for women. In their empirical analysis, Bratberg et al. (2002) use the number of children as a proxy for

family responsibilities and find some weak support for the theory, while Mastekaasa (2000) finds no support. Paringer (1983), on the other hand, argues that women's dual role as both producers in the labor market and at home (in contrast to the more labor market-specialized men) implies that women's health is more important for the household than men's, since a household would suffer more than just lost earnings if the female is ill. In her empirical analysis, Paringer uses marital status as a proxy for household responsibilities and finds that married women are less absent from work for health reasons than unmarried women.

It should be noted that these early studies disregard potential unobserved sorting into marriage or parenthood of the women. This may be problematic as the decision of not having children or to be married could be either voluntary, for instance due to a more career-oriented life style than what is compatible with having children, or nonvoluntary due to health problems that hinder reproduction directly or else make child rearing difficult. Both reasons certainly affect sick leave behavior, which makes the comparison of parents with non-parents problematic.

As far as we know, Angelov et al. (2011) was the first study of the effect of parenthood on the gender gap in sick leave. In their study (a Swedish report), the authors graphically show that the within-couple gender gap in sick leave increases when entering parenthood. In the present study, we formally discuss identification and in addition, we investigate the relevance of different explanations for the potential effect. Rieck et al. (2013) is the study closest to Angelov et al. (2011) and to the present one. Using Norwegian data, Rieck and Telle find similar short-run effects as in Angelov et al. (2011), but they find no effect 3 years after child bearing. However, Rieck and Telle censor women who have a second child, implying that their results are not comparable to ours.

3 The Swedish social insurance system

All residents of Sweden aged 16 and over (employed as well as unemployed) are entitled to sickness benefits in the case of their own illness, as well as to a separate insurance system and earnings replacement in the case of their child's illness. Furthermore, all parents are entitled to paid parental leave. To understand the setting in which we measure the effect of parenthood on the gender gap in sick leave, it might help to consider a typical Swedish family around the time when they have their first child: Most Swedish mothers are on paid parental leave during the larger part of the child's first year. Then, some fathers take a part of the remaining paid parental leave, and most children start attending highly subsidized daycare centers when they are between the ages of one and one and a half. The majority of Swedish mothers return to the labor market, although most work fewer hours than the fathers do. In this section, we briefly explain the above-mentioned parts of the Swedish social insurance system and the entitlements to these benefits.

3.1 General principles

The rules for entitlement have changed over time, but the general idea has always been that both the employed and the unemployed are entitled to a replacement which

is proportional to lost earnings up to a cap. The replacement rate has varied over time between 75 and 90% of lost earnings, up to a cap equal to yearly earnings of about 320,000 SEK (which in 2009 corresponded to earnings in the 70th percentile of the earnings distribution). During the study period, there was no limit to the duration of the benefits of this insurance.⁵

3.2 Sickness benefits

In case of illness, an individual with a positive labor income is qualified for sickness benefits from the first day as employed. However, the first day is not replaced. Thereafter, the employer pays sick-pay for day 2–14. After these 14 days, the Swedish Social Insurance Agency (SIA) disburses sickness benefits. The sickness insurance also covers self-employed, but they can choose their degree of replacement and how many unreplaced days they want within a given sick-spell.

Unemployed persons must be registered as job seekers at a local unemployment office for being eligible for sickness benefits. The benefits are based on the wage before unemployment. Thus, unemployed persons without any employment history do not receive sickness benefits. For unemployed individuals, the SIA starts disbursing sickness benefits from the second day onwards. In this study, we focus on sick leave with sickness benefits, meaning that for employees, we start counting the number of days absent from the first day in the third week within a given illness period. Thus, the type of sick leave we study is not short-term sick leave but a leave due to longer-lasting reduced working capacity (longer than 14 days).

Compensation for illness periods longer than 7 days requires a medical certificate from a physician with information about the expected length of the sick leave. Based on this certificate, the SIA formally decides whether an individual is entitled to compensation or not. When the entitlement period has expired, a renewal certificate is required and the process is repeated.

Although the formal decision about sickness benefits is made by the SIA, the sickness benefit claimant can influence the outcome. According to Arrelöv et al. (2006), the outcome is largely controlled by the insured's motivation. Englund (2001) also finds that doctors believe that they prescribe too long durations of medical-absences, that is, the duration is not motivated by medical considerations only.

Finally, in the context of child bearing and giving birth to children it is also important to note that pregnant women are allowed to use their parental leave benefit two months ahead of delivery. In addition, if the work is too demanding, the woman can apply for a certain pregnancy benefit. Finally, it is against Swedish law for employers to fire, end a temporary employment, or change position due to a woman's pregnancy.

3.3 Insurance coverage in case of a child's illness

To understand why the estimated effects in this study do not stem from children's being ill, but rather from the parents' own sick leave, it is essential to have some knowledge

⁵ A time limit of 2.5 year was introduced in July 2008, thereby coming into effect after the end of our panel data set.

of the Swedish insurance system for cases of child illness. Parents are entitled to so-called *temporary parental benefits* if they have to stay at home to care for an ill child under the age of 12. Parents are jointly eligible for temporary parental benefits for 120 days per child and year. After these 120 days, a further 60 days can be taken, if the need for these extra days has been approved by the SIA.

The work absence due to child illness is financially more beneficial than the work absence due to one's own illness since it is compensated from the first day of the work absence. Until recently, there was no formal monitoring of the absence due to care for an ill child. Engström et al. (2007) show that this disharmony between the two insurances leads to a large excess use of temporary benefits and Persson (2011) finds that this also leads to unintended flows from sickness insurance benefits to temporary parental benefits. The present study focuses on sick leave spells longer than 14 days. The extent of flows from sickness benefits to the temporary parental benefits should be small. However, if anything, days on sickness benefits are an underreported measure of the work absence.

3.4 Paid parental leave

Parents receive parental benefits if they stay at home to take care of their child.⁶ Parental benefits are payable for 450 days for each child. One parent may give up the right to parental benefit to the other parent, with the exception of 60 days. Parents with children under 8 years are also entitled to unpaid job-protected leave with a great portion of flexibility. During the child's first 18 months, both parents can stay at home on a full-time basis with job protection. Thereafter, parents are allowed to reduce their working hours up to 25% until the child turns 8 years old (SFS 1995:584).

4 Identification and empirical strategy

There are several challenges associated with estimating the effect of parenthood on the work absence due to a parent's own sickness. First, it is reasonable to believe that the likelihood of having a child is correlated with health and labor market success. Second, parents within a couple can affect each other's behavior. Third, the timing of parenthood is chosen by the individuals. In this setting, conventional approaches aiming at estimating an average treatment effects using an archetypical framework with a well-defined control and treatment group fails as 'controls' will become treated later on (cf. Fredriksson and Johansson 2008).

To this end, we restrict the analysis to the estimation of the effect of parenthood on the gender difference in sick leave for those becoming parents [for similar approaches on other outcomes see Angelov et al. (2016) and Kleven et al. (2015)]. By asking how the within-couple gender gap changes when a couple enters parenthood, we control for unobserved individual characteristics that might be correlated with parenthood. In the following, we further explain our identification strategy.⁷

⁶ This holds also for persons without earnings who receive a flat rate of 60 SEK (approx. €5.5) per day.

⁷ A formal derivation is given in "Appendix A."

Both groups (men and women) are affected by the intervention (entering parenthood). By studying the gender gap within couples, the unit of observation in the analysis is couples that have their first child together. Both parents are allowed to be affected by parenthood, and the question we ask is whether mothers in general are more affected than fathers. The latter is important since a child is time demanding and in combination with labor market work this could give rise to a stress that affects health and thereby the sick leave of both the mother and the father. The cost associated with having a child depends on several things: how demanding the child is and the economic and human capital capacity of the parents. By focusing on the within-couple gap, we control for these family-specific consequences of parenthood. It is important to stress that we sample couples at different calendar times and that we observe these couples before and after parenthood. This allows us to estimate the effect year by year after parenthood and to control for potential gender-specific age and calendar time effects.

The identifying assumption is the same as in a traditional difference-in-differences setting, i.e., the intervention must be strictly exogenous. That is, the timing of when to have a child should not be determined by expected trends or shocks to the within-family gender difference in sick leave that the couple would have experienced in absence of entering parenthood. This means that the timing of entering parenthood should not be influenced by, for us, unobservable information about sick leave changes of men in comparison with women or vice versa. This assumption is to some extent possible to validate using data on sick leave before conception, and we perform such a validation graphically in Sect. 5.1 and using a statistical test in Sect. 6.2. In addition, using data on hospital stays, we can assess whether it is likely that the timing of parenthood is induced by an expectation of a negative health shock for the mothers, compared to the fathers (see Sect. 7.3.1). To further investigate our identification strategy, we have also performed a placebo analysis. To this end, we use our base sample but we remove all observations later than the first pregnancy month and randomly assign a first child to each of these (yet) childless couples.

5 Data and descriptive statistics

The data are taken from universal administrative registers from various sources covering all residents in Sweden. First, using the so-called multi-generation register, we define the population as parents who had their first born child between 1992 and 1998. We can link parents to their legal children and have information on birth year and month as well as birth order. For this population, we have also information taken from LOUISE, which is an administrative register covering all residents in Sweden aged between 16 and 65, updated on a yearly basis for 1986–2008. This register provides information about pre-child labor market income and pre-child education.

The observation units are matched couples, i.e., men and women who had their first-born child together. We do not require these couples to be married or cohabiting during any time during the study period.⁸ We have added individual information on

⁸ We do not have register information about cohabiting status or child custody.

the use of sickness benefits (“sick leave”) from SIA for 1986–2008, measuring spells longer than 14 consecutive days. The direct reason for not including shorter spells is that such sick spells are unavailable in the registers since they are paid for by the employer. Although the restriction to longer spells is due to data availability, we think that focusing on longer spells rather than short-run absence due to, e.g., having the flu, is not problematic.

Further, data on in-patient care are retrieved from the National Board of Health and Welfare (for 1987–2005). In-patient care refers to care for a patient who is formally admitted (“hospitalized”) to an institution for treatment and/or care and stays for a minimum of one night in the hospital or other institution providing in-patient care.⁹ This information also stems from national registers covering the whole population. The data on sick leave and hospitalization contain information on both the starting and ending date of a spell on sickness benefits and a hospital stay. This information has been totaled on a monthly basis separately for each parent and outcome variable.

The sampling procedure results in a pooled panel data set of matched couples who had their first child between 1992 and 1998. Since we use monthly data on the two outcome variables (sick leave days and in-patient care days), the complete data set consists of over 50 million observations. To reduce the estimation time, we take 10 percent random samples of each parent cohort (at couple level).

The population of interest consists of individuals who are employed before entering parenthood. Since even a very small labor income is enough for being qualified for sickness benefits, by this restriction, we include only individuals who are in the labor market and thereby are qualified for sickness benefits. Strictly speaking, for a couple to be included in the sample, we require a positive income from labor market work 2 years before entering parenthood for the mother as well as the father. (However, we do not require positive income later on.) This employment requirement reduces the sample of couples by 22% and results in a data set consisting of 5,153,676 couple-month observations. In this context, it is important to know that the female labor force participation rate in Sweden is very high and almost in level with men’s labor supply.¹⁰ Instead many women work part-time: around 50% of all women with preschool children. Thus, the restriction of positive income 2 years ahead of the birth of the first child results in mainly dropping those men and women who do not work on the labor market before entering parenthood. In conditioning on pre-child labor market attachment, we thus make sure that an observation with zero pre-child sick leave implies zero absence due to sickness, and not that the individual lacks eligibility for sickness insurance due to non-participation in the labor force.

Furthermore, we remove couples whose who have twins at first birth (97,272 couple-month observations) and where the education of the mother or father is unknown (39,156 couple-month observations). As a final step, we only keep observations measured 48 months before the birth of the first child or later. This is done because although many of the individuals are in the working age population even before $t = -48$, many

⁹ In the present paper, we use the terms “in-patient care,” “hospital stays,” and “hospitalization” as synonyms.

¹⁰ We provide these numbers for parents in our data set in the online appendix of this paper.

Table 1 Descriptive statistics

	Mothers		Fathers		Mothers–fathers	
	Mean	SD	Mean	SD	Mean	SD
Age	27.4	4.4	29.6	4.9	– 2.2	3.77
Income (SEK)	152,023	80,386	200,066	106,074	– 48,043	108,404
Education (years)	12.2	2.0	12.0	2.1	0.1	2.1
Sick leave (days/months)	0.8	3.7	0.6	3.3	0.2	4.8
Hospitalization (days/months)	0.03	0.52	0.02	0.52	0.01	0.74

The first child is born in month $t = 0$, age is measured in $t = 0$, and income, education, sick leave, and hospitalization in $t = -24$. Income is measured in SEK in 2008 prices. The December 2008 exchange rate was approximately 7.9 SEK/USD. Education is measured in theoretical years of education using the official Swedish SUN classification, which roughly follows the international ISCED 97 standard. Sick leave and hospitalization are measured in days per month

are likely not working and thus not entitled to paid sick leave. This is especially true for the mothers, who are on average 2 years younger than the fathers.¹¹

Table 1 presents the data used in the main analysis. This table shows that the mean age when entering parenthood is 27.4 for women and 29.6 for men. The mean annual labor market income is lower for women than men 2 years before entering parenthood: Women's average income is 76% of men's. This is, in a way, expected due to the age gap before entering parenthood. The education gap is in the opposite direction.

Two years prior to childbirth, mothers are on sick leave on average 0.17 days per month (or about 28%) more than fathers. Finally, the average days of hospital stays are 0.1 days per month (or about 50%) higher for mothers.

5.1 Graphical evidence

In order to get a first look at the data, we present the within-couple gender gap in sick leave before and after the birth of the first child in Fig. 3. The data plotted in Fig. 3 represent raw monthly average days on sick leave for the pooled data set of matched couples starting 48 months prior to parenthood (i.e., having a first child) and ending about 17 years after the birth of the first child. From Fig. 3 we can see that before the conception, women's sick leave is higher than that of their spouse, but what is more important, there is no apparent difference in trends between mothers and fathers. As was discussed in Sect. 2, the pre-parenthood difference in levels could be due to differences in, among other, preferences, occupations and economic incentives.

What is also apparent from Fig. 3 is that the gender difference in sick leave occurring 2 years after the birth of the first child is large and persistent for as long as we can follow the couples. The spike in female sick leave that starts after conception is most likely due to pregnancy-related health problems. The magnitude of the spike is of about 5 days per month on average during the month of delivery. During the period

¹¹ In a previous working paper version of the paper, we kept all observations as long as the individual was in the working age population. Such an analysis is also performed in connection to a placebo test later in the present paper. Removing observations before $t = -48$ does not alter the results.

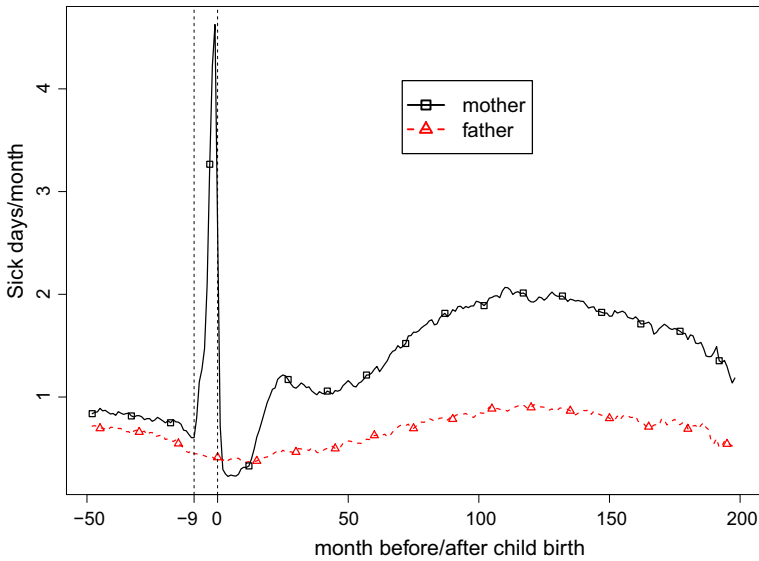


Fig. 3 Average days of sick leave per month before and after the birth of the first child for mothers and fathers who had their first child at $t = 0$. *Note* The analysis is based on all Swedish residents who had their first child between 1992 and 1998, and were active on the labor market 2 years prior to childbirth. Data on sick leave cover the period from 1986 and 2008

directly after childbirth, we observe a dramatic decrease in sick leave for women, and during this period mothers have less sick leave than fathers. The reason for this drop is that most mothers use paid maternity leave during the child's first year and have no reason to be on sick leave unless severely ill.¹²

Figure 3 also shows a decrease in sick leave for both men and women in the end and in the beginning of the time before and after the birth of the first child. The reason for this is the remarkably dramatic cyclical variation in sick leave in Sweden. We control for this variation using regression analysis, as explained later in the paper and in particular in “Appendix A.” Being able to control for potential secular trends by adding year-specific trends is a benefit with our data set, consisting of several cohorts of parents. This way, we ensure that our estimates are not confounded with calendar time variation in the sick leave gender gap.

6 Mean effects of parenthood on the sick leave gap

6.1 Regression model

In the estimation, we use data on couples who are matched by observing in the registers in which year and month a female and a male had their first child. To simplify the notation, we have suppressed the couple index in this section, but it should be noted

¹² Sickness benefits could be paid out during the parental leave if, for example, the illness prohibits the mother from taking care of the child.

that the unit of observation in our empirical specification is a specific couple. We have two time dimensions: time since birth and calendar time. Let $c = 1986, 1987, \dots, 2008$ index the calendar year, and $t = c - c^*$ where c^* is the year when the first child is born. Thus, $t = 1$ during the first child’s first year, $t = 2$ during the first child’s second year, etc. Moreover, time since birth can also be measured in months (the frequency we use for sick leave and in-patient hospitalization). Let $q = m - m^*$ where m^* is the birth month index of the first child and m the calendar month (1 in January 1986). Thus, $q = 0$ during the child’s first month, $q = 1$ during the second month, etc.

Using these definitions, we estimate the following population regression model:

$$\begin{aligned} \tilde{s}_{\text{qct}} = & \alpha_{\text{pre}} + \alpha_{\text{preg}} \mathbf{1}[-9 \leq q \leq 0] + \sum_{\tau=1}^{17} \alpha_{\tau} \mathbf{1}[t = \tau] + \tilde{\mathbf{x}}' \boldsymbol{\phi} + \theta_c \tag{1} \\ & + \sum_{a=16}^{62} \gamma_a^f \mathbf{1}[a = (c - \text{birthyear}_f)] + \sum_{a=16}^{73} \gamma_a^m \mathbf{1}[a = (c - \text{birthyear}_m)] + \varepsilon_{\text{qct}} \end{aligned}$$

where $\tilde{s}_{\text{qct}} = s_{\text{qct}}^f - s_{\text{qct}}^m$ is the within-couple (f)emale–(m)ale gap in sick leave measured q months (t years) from first birth during calendar year c , $\mathbf{1}[\cdot]$ is the indicator function which takes the value one when the expression within the parenthesis is true and zero otherwise, a is an index for the age of the female/male, $\text{birthyear}_{f/m}$ is the birth year of the female/male, and $\tilde{\mathbf{x}}$ is a covariate vector of pre-pregnancy within-couple differences in education and yearly labor income, measured at the latest during year $t = -2$. For $t < -2$, we set $\tilde{\mathbf{x}} = (\mathbf{x}_c^f - \mathbf{x}_c^m)$, and for $t \geq -2$, we set $\tilde{\mathbf{x}} = (\mathbf{x}_{(c^*-2)}^f - \mathbf{x}_{(c^*-2)}^m)$. Our main parameters of interest are α_{τ} for $\tau = 1, 2, \dots, 17$, which measure the effect of parenthood on the female–male sick leave gap during the child’s τ th year since birth. We use data for $q \geq -48$, i.e., 4 years of pre-birth data are included in the regression.

The intercept parameter α_{pre} controls for pre-pregnancy differences in the sick leave gap and θ_c controls for calendar year effects. The parameters γ_a^m and γ_a^f flexibly control for gender-specific sick leave age trajectories. The pregnancy parameter α_{preg} takes into account the sharp increase in the relative sick leave during pregnancy which can be observed in Fig. 3. As we can observe women and men for a maximum of 203 months after parenthood, we are in a position to estimate 203 ex post-birth parameters. However, as we believe is clear from the analysis provided below, we do not lose any information by keeping the analysis at the yearly level.

6.2 Baseline results

Columns (1)–(3) in Table 2 contain the main estimation results for a varying conditioning set using the specification from Eq. (1). We estimate the effect of pregnancy and delivery as well as the yearly effects of parenthood starting from the year of birth. Column (1) presents the estimates without any controls. In column (2), we control only for calendar years, and in column (3) we also include gender-specific age dummies and pre-child differences in income and years of education. All parameter estimates

that are statistically significant at the 1% level and columns (1)–(3) tell the same story: In the long run, the female–male gender gap in sick leave increases due to parenthood. Before explaining the interpretation of each regression coefficient, we discuss how the different model specifications affect the long-term estimate of the gender gap in sick leave.

Including calendar year controls reduces the magnitude of the estimated effects for years 3 through 13 since the birth of the first child, while the effects for years 14–17 increase somewhat. Adding age and pre-child controls changes the results somewhat, most so for the longer-term effects, which increase.

In the following, we discuss the estimates in the third column. The results confirm what was already seen from the graphical analysis displayed in Fig. 3. Pregnancy drastically increases the gender gap in sick leave: The effect is 1.67 days per month during this period. This increase is most likely due to mothers' pregnancy-related illnesses. But during the first year after birth, the effect is instead negative, i.e., fathers increase their sick leave more than mothers do, leading to an effect of -0.20 days per month. This is due to the fact that the mothers are on parental leave during the first year after giving birth. Parents are eligible for sickness benefit while on parental leave but it is obviously rarely used. During the second year, the gender gap in sick leave increases by 0.28 days per month, which is a substantial increase. This result is to some extent driven by the high frequency of siblings' being born about 2 years after the first childbirth. (We will return to this later.) Finally, our main parameters of interest are the long-term effects from year 3 onwards. The estimates range from 0.24 (year 5) to 1.01 days per month (year 16), which corresponds to the shift in the sick leave gap in Fig. 3, now using a regression approach. There seems to be a gradual increase in the effect of pregnancy approximately between year 5 and 14, and no pronounced change further away from birth.

How should one interpret these results in light of the fact that most women have more than one child? Pregnancy itself and the days around childbirth are associated with a sharp increase in the sick leave gap. Thus, the shift in sick leave after the birth of the first child could potentially be explained by subsequent births and short-term pregnancy-related illnesses.

In order to take this potential problem into account, we estimate a second child effect, which is presented in the fourth column in Table 2. In this analysis, the dummy for the first child's second year captures sick leave differences only as long as the mother is not pregnant with her second child. As soon as the second pregnancy begins (i.e., 9 months before the birth of the second child), the second-child pregnancy dummy captures the sick leave difference. The first-child second year estimates now capture the dynamics of the gender gap in sick leave for (a) the minority of couples that only have one child during this period and (b) the period after the birth of the first child and before the birth of the second child among the majority of couples who do have a second child. In contrast, the variation used to estimate the second-child parameters stems solely from couples that have at least two children. The long-term effects (for year 3 since the birth of the first child and thereafter) are estimated using a dummy variable that has the value one if (a) more than 2 years have passed since the birth of the first child, and (b) for couples that have a second child, either more than 2 years

Table 2 Baseline specification, robustness checks and placebo regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pregnancy (1st child)	1.630*** (0.0335)	1.689*** (0.0360)	1.669*** (0.0361)	1.666*** (0.0357)	1.715*** (0.0406)	1.668*** (0.0373)	-0.0134 (0.0259)
Year 1 (1st child)	-0.238*** (0.0245)	-0.169*** (0.0317)	-0.198*** (0.0324)	-0.218*** (0.0299)	-0.234*** (0.0342)	-0.203*** (0.0331)	-0.0128 (0.0302)
Year 2 (1st child)	0.277*** (0.0297)	0.316*** (0.0394)	0.284*** (0.0404)	-0.108** (0.0385)	-0.0941* (0.0432)	0.277*** (0.0410)	0.0151 (0.0352)
Pregnancy (2nd child)				2.040*** (0.0504)	2.014*** (0.0584)		
Year 1 (2nd child)				-0.422*** (0.0452)	-0.440*** (0.0520)		
Year 2 (2nd child)				-0.135* (0.0533)	-0.174** (0.0614)		
Year 3	0.495*** (0.0346)	0.483*** (0.0460)	0.450*** (0.0475)	0.224* (0.0964)	0.243* (0.0987)	0.444*** (0.0480)	0.0347 (0.0401)
Year 4	0.400*** (0.0351)	0.325*** (0.0508)	0.294*** (0.0528)	0.336*** (0.0889)	0.360*** (0.0967)	0.287*** (0.0533)	0.0367 (0.0453)
Year 5	0.432*** (0.0385)	0.266*** (0.0580)	0.241*** (0.0607)	0.247*** (0.0674)	0.290*** (0.0783)	0.233*** (0.0612)	-0.0166 (0.0516)
Year 6	0.571*** (0.0428)	0.311*** (0.0685)	0.295*** (0.0718)	0.271*** (0.0704)	0.333*** (0.0811)	0.285*** (0.0723)	-0.0382 (0.0611)
Year 7	0.770*** (0.0481)	0.435*** (0.0808)	0.429*** (0.0848)	0.359*** (0.0791)	0.415*** (0.0902)	0.419*** (0.0852)	0.0236 (0.0733)
Year 8	0.865*** (0.0507)	0.482*** (0.0934)	0.490*** (0.0979)	0.409*** (0.0891)	0.528*** (0.102)	0.480*** (0.0983)	-0.0303 (0.0874)

Table 2 continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year 9	0.916*** (0.0530)	0.522*** (0.107)	0.549*** (0.112)	0.455*** (0.102)	0.579*** (0.116)	0.538*** (0.113)	0.0610 (0.111)
Year 10	0.962*** (0.0547)	0.617*** (0.121)	0.664*** (0.126)	0.565*** (0.114)	0.692*** (0.131)	0.652*** (0.127)	0.00950 (0.140)
Year 11	0.908*** (0.0558)	0.630*** (0.131)	0.700*** (0.137)	0.605*** (0.125)	0.728*** (0.142)	0.688*** (0.138)	-0.177 (0.128)
Year 12	0.906*** (0.0592)	0.699*** (0.142)	0.794*** (0.148)	0.697*** (0.136)	0.853*** (0.154)	0.782*** (0.149)	-0.135 (0.180)
Year 13	0.854*** (0.0636)	0.736*** (0.151)	0.853*** (0.159)	0.748*** (0.146)	0.919*** (0.167)	0.841*** (0.159)	
Year 14	0.814*** (0.0681)	0.791*** (0.160)	0.923*** (0.168)	0.821*** (0.156)	0.987*** (0.178)	0.912*** (0.169)	
Year 15	0.746*** (0.0755)	0.825*** (0.171)	0.970*** (0.179)	0.867*** (0.168)	1.062*** (0.192)	0.959*** (0.180)	
Year 16	0.657*** (0.0877)	0.850*** (0.184)	1.012*** (0.193)	0.910*** (0.182)	1.179*** (0.207)	1.001*** (0.193)	
Year 17	0.535*** (0.120)	0.819*** (0.206)	0.997*** (0.216)	0.895*** (0.206)	1.124*** (0.236)	0.985*** (0.216)	
<i>Included controls</i>							
Calendar year dummies	No	Yes	Yes	Yes	Yes	Yes	Yes
Age dummies for each parent	No	No	Yes	Yes	Yes	Yes	Yes
Pre-child differences	No	No	Yes	Yes	Yes	Yes	Yes
N	4,065,717	4,065,717	4,065,717	4,065,717	3,207,650	5,017,248	1,726,833
R ²	0.004	0.005	0.007	0.010	0.010	0.009	0.011

Standard errors within parentheses are clustered at couple level. Significance levels are denoted by * ($p < 0.05$), ** ($p < 0.01$), and *** ($p < 0.001$). The full set of controls consists of calendar year dummies, separate age dummies for each parent, and pre-child controls for differences in income and education.

Specifications: (1) No controls, (2) Calendar year, (3) Full set of controls (baseline), (4) Second child effects, (5) Couples with at most two children, (6) Baseline including observations for $t < -48$, (7) Placebo

have passed since the second birth, or the mother is not yet pregnant with the second child.¹³

A comparison of the first- and second-child estimates from the fourth column in Table 2 suggests that the positive pregnancy effect is somewhat higher for the second than for the first child (2.04 compared to 1.67 sick-days/month). The negative first-year effect is about twice as large in absolute terms for the second child compared to the first child (−0.42 and −0.22, respectively), but the second-year effects are both positive and have about the same magnitude.

Importantly, the long-term yearly effects of parenthood are of the same magnitude whether we include second-child effects or not. To further push this point, we have estimated the specification with second-child controls for the sub-sample of couples that have at most two children (see column 5). The results are qualitatively unchanged, but the long-term estimates are even somewhat higher for this group. This is an important result, as it implies that the long-term results of parenthood that we estimate are not driven by later pregnancies.

We condition on being eligible for sickness benefits before entering parenthood. Parenthood could, however, cause mothers to leave the labor force to a larger extent than the fathers. If anything, this would attenuate the estimated effect toward zero. However, in order to investigate whether a potential change in the composition of individuals eligible for sickness benefits after entering parenthood may affect the results, we have re-estimated the model using two other samples. In the first, we require a positive income also after the birth of the first child. In the second, we restrict the incomes to be greater than 50,000 SEK (€ 4600) 2 years before childbirth but also after childbirth. The results from these analyses (available in an online appendix) show (1) that the estimated effects are virtually the same as those given in Table 2, and (2) that the long-term effects for the last sample are, as expected, smaller but the effect for year 15 is still as much as 0.62 sick days/month. Thus, qualitatively, the results do not change with the sample restrictions made. The primary reason is that most mothers in Sweden stay in the labor force also after they have entered parenthood. In the online appendix, we also show the share of mothers who leave the labor market after becoming mothers: They are very few.

6.3 The validity of the strict exogeneity assumption and a placebo test

As explained in “Appendix A,” we can test the validity of the strict exogeneity assumption by checking whether the error terms have a unit root. We take the results in column 3 in Table 2 as the base for the test. First, however, we test for a deterministic trend with respect to time to birth (q) by using a second-degree polynomial regression of the residuals $\hat{\varepsilon}_{iq}$ on time to birth for the period before the assumed conception which is nine months before the birth of the first child. The F test with two degrees of freedom

¹³ An example might be useful. Assume that couple A has their first child in June 1996, and no children thereafter. The long-term effect for year 4 is captured by a dummy variable valued one for monthly sick leave observations that occur from June 1999 to May 2000. Assume further that another couple (B) have their first child in June 1996 and a second child in June 1999. Then, no variation from couple B is used in the estimation of the effect for year 4. Instead, the sick leave observations for couple B from June 1999 to May 2000 are used in the estimation of the effect for year 1 (second child).

has a p value of 0.30, and none of the individual parameter estimates is significant. For the formal unit root test, we use the Harris and Tzavalis (1999) panel unit root test (see “Appendix A” for the motivation). We strongly reject the null hypothesis of a unit root.¹⁴

To further investigate our identification strategy, we have also performed a placebo analysis. The general idea is to use our specification [cf. column (3) in Table 2] on a sample where we know that there should be no pregnancy or parenthood effect. To this end, we use our base sample but we remove all observations later than the first pregnancy month, i.e., $t \geq -9$. To apply our empirical specification to this data, we need to randomly assign a first child to each of these (yet) childless couples. There is, however, one practical challenge: For reasons mentioned in Sect. 5, we have removed observations measured earlier than 48 months prior to parenthood in the baseline regressions. If this restriction is kept also in the placebo analysis, we would end up with a sample where each couple is measured over a window of 39 months (from -48 to -10) or slightly above 3 years. This time, window is too short for a meaningful comparison with the estimates based on the base sample, where we measure effects up to 17 years after child birth. To deal with this issue, the data in the placebo analysis contain all available observations, i.e., also prior to $t = -48$ for couples who are in the administrative registers. Define the first observation month for couple c as T_c^0 (e.g., $T_c^0 = -64$). We randomly assign a placebo birth to each couple, where the birth month is the integer value of a number drawn from a uniform distribution over the interval $(T_c^0, -10)$. A final issue has to do with the pre-child controls for income and education within-couple differences. In the placebo analysis, we could in principle measure these controls 2 years prior to the placebo birth date. However, we believe that in many cases, re-measuring the pre-child controls would give misleading values as some of the individuals may be studying at the placebo birth date. Therefore, we keep the original values of the pre-child controls in the placebo analysis. We are now in a position to apply our baseline specification to the placebo data set.

Before showing the placebo results, we demonstrate that the baseline results using our baseline sample are unchanged when we add observations prior to month -48 . These results are shown in column (6) in Table 2, and there are only minor differences compared with the baseline results from column (3). Moving to the results from the placebo regression presented in column (7) in Table 2, we can conclude that all estimated effects of parenthood and pregnancy are statistically insignificant. This shows that all potential gender-differential sick-leave profiles over the life cycle are captured by the gender-specific categorical variables for age included in the regression, and the parenthood effects are not biased by the age effects.¹⁵

¹⁴ The estimated AR-coefficient is 0.53, and the p value is 0.0000. Another available unit root test is for a fixed T -setting, proposed by Im et al. (2003). Im et al. allow the AR(1)-coefficient ρ to vary over individuals. We have tested the null of a unit root using also Im et al. (2003) and get the same results as with the Harris–Tzavalis test (i.e., a p value of 0.0000).

¹⁵ In the baseline regression, the total number of estimated age effects is 103 (from age 16 to 62 for mothers and 16–73 for fathers). The corresponding number for the placebo analysis is of course smaller since we remove observations after a couple has had their first child. For the placebo regression, the total number of estimated age effects is 70 (age 16–45 for mothers and 16–57 for fathers).

7 Family responsibilities and sick leave

In this section, we discuss and investigate possible explanations for the estimated effect of parenthood on the sick-leave gap. We have two ideas. The first focuses on women's dual responsibility associated with parenthood, which may cause a relative deterioration in female health (cf. Bratberg et al. (2002)). The second concerns changes in economic incentives within the household. These two hypothesis provide two very different explanations. In the following, we test the fitness of each of them against data and provide some suggestive evidence of the plausibility of each of them. We first discuss these ideas and then present the empirical results.

7.1 A gender-differential change in health

Bratberg et al. (2002) claim that the gender gap in sick leave stems from the psychological pressure of the dual role of women, the so-called double burden. As the average total time spent on working (including labor market work as well as household work at home) is the same for men and women (SCB 2009), we believe that this hypothesis should not be interpreted as an effect from a higher work load of the women on average, but rather as a potential effect of the psychological strain of switching between roles. The role strain theory argues that having multiple roles is detrimental for an individual's health and may thus increase the sick leave. Thus, according to this hypothesis, women's health would deteriorate after entering parenthood.

However, the dual role could also lead to improved health among women. There is a large literature theorizing about the benefits of multiple roles (the role enhancement theory), as it might make an individual feel that his or her life is more meaningful. This effect would, hence, work in the opposite direction, namely, by improving the individual's health.¹⁶ Yet another theory is suggested by Paringer (1983). The idea is that, due to women's dual role, female health is likely to be more important for the household than male health, since female illness does not only imply lost earnings, but also creates an additional cost in terms of lost home production. In this setting, it may be rational for the household to take more precautions in the case of a negative female health shock, by increasing female work absence more than for a similar male health shock, or in other words: to be more risk averse when it comes to the health of the mother. According to the role enhancement theory and Paringer's hypothesis, we would observe an increased female–male gap in sick leave, but a long-term improvement in female health.

To investigate how well these empirical predictions correspond to the empirical outcomes, we apply the same empirical strategy as in the previous analysis, but instead of sick leave as outcome variable, we directly focus on the effect on health by analyzing in-patient care data.

¹⁶ For more discussion about multiple roles and their implications, see the literature review in, e.g., Mastekaasa (2000).

7.2 Economic incentives

It is well known that insurance coverage may change individual behavior. Due to asymmetric information about employee health, the sickness insurance system (with high replacement rates) can be used by employees as a way of adjusting working time (Allen 1981; Johansson and Palme 1996). Individuals can use sick leave as a way of increasing their leisure time so that their real wage equals their marginal value of leisure.¹⁷ A similar argument applies to the case when there is a need for an increase in home production, as happens when parenthood implies a new and inevitably time-consuming task at home. Thus, a response to this new home commitment could be to reduce the female labor supply, as many women do. However, another way of reducing the labor working time is to increase the time on sickness benefits. We refer to this potential effect as an *ex ante* moral hazard effect.

In comparison with low-income mothers, high-income mothers have most likely better opportunities to deal with the new commitment at home. They have more opportunities to adjust their contracted labor supply, to buy household goods on the market, to employ flexible working hours, and to telecommute. Thus, it is reasonable to assume that low-income mothers have stronger incentives to increase their time on sickness benefits than high-income mothers. An informal test of this *ex ante* moral hazard behavior is thus given by studying whether the magnitude of the effect of parenthood varies with mothers' pre-birth income level. A negative relation between pre-birth income and the effect of parenthood on the sick leave gender gap would support the idea that our main effect is partly driven by *ex ante* moral hazard among mothers.

Economic theory together with empirical evidence tells us that *ex post*-moral hazard is important in the Swedish sickness insurance system (see, e.g., Johansson and Palme 2005). That is, sick leave increases when the cost of being absent drops. Now, as women reduce their working time after parenthood, the cost of being absent may be reduced. For high-income women, there may be a direct effect but there is also a potentially more important indirect effect. The direct effect stems from the fact that there is a cap in the sickness insurance system: For women with incomes above the cap, the real replacement rate is lower than the nominal replacement rate in the insurance. Consequently, a reduction in working hours as a result of parenthood for these women implies an increase in real replacement rates. The indirect effect stems from a change in employers' expectations about a worker's performance due to this reduction in working time. A high level of presence at work is arguably taken as a signal of aspiration and productivity by most employers. Thus, work absence as measured by sick leave and/or a reduction of working hours due to household work might negatively affect future advancement in the workplace. Fewer opportunities and possibilities of advancement will then affect work incentives, which in turn lowers the threshold for using the sickness insurance. Seen from this perspective, the fact that many women reduce their labor supply after entering parenthood means that their cost of being absent falls with their lower labor market attachment.

We investigate the hypothesis of *ex post*-moral hazard behavior due to a change in female labor market attachment after parenthood by studying whether a higher

¹⁷ Real wage = (income + benefits)/(contracted working hours—time on sickness benefits).

income increase between the pre-child level measured in year $j_i = -2$ and year $k = \tau - 1$ is related to a lower effect of parenthood on sick leave during year τ . As an example, we ask whether the effect of parenthood 10 years after childbirth is lower for women with a high income increase between year -2 and year 9. We use yearly income from labor market work (not social transfers), which is a combined result of hours worked and the hourly wage, as a measure of labor supply, since we lack an appropriate measure of labor supply in terms of hours worked. One potential issue with using income instead of hours work is that sickness benefits for sick spells longer than 14 days are not counted as income from work. This implies that there is a mechanic relationship between income and sick leave during a given year. For this reason we use lagged income. The advantage with this approach is that this is a measure of effective working hours (including sick leave) the previous year and not only a measure of contracted working hours. In “Appendix A,” we show that if the timing of parenthood is strictly exogenous with respect to gender differences in sick leave, the average heterogeneous effects for parents are identified.

7.3 Empirical results

7.3.1 Health

In order to investigate whether there is an effect of parenthood on the gender gap in health, we use the same empirical specification as previously (see Eq. 1), but now using hospitalization data as outcome. Before presenting our empirical approach, we discuss hospitalization as a measure of health.

The concept of health has no precise definition. As such, health is measured in many ways in the literature, ranging from the overall self-assessments of the patients to a wide range of clinical indicators and mortality. In this paper, we use hospitalization care as a less subjective measure of health than the sick leave. In the context of an economic model, we believe that in-patient care provides a measure that is not affected by moral hazard while sick leave is. Admittedly, in-patient care is not a perfect measure. The advantage with the measure is that we can follow the progress of individuals over time. If there are differences in health not showing up in a hospital stay between two sub-groups in year 1, than these health differences are likely to show up in a follow-up horizon of 14 years.

Where we previously used the within-couple gap in days per month on sick leave, we now use the corresponding gap in days per month of hospital stays. As we have hospitalization data for a shorter period of time (1987–2005 instead of 1986–2008 as is the case for sick leave), we re-estimate, for the sake of the comparison, the effect on sick leave for this shorter period. In order to keep the analysis simple, we use couples with at most two children. This simplifies the analysis because we do not need to control for subsequent child births (which by definition imply a hospital stay) among those with more than two children (a minority of couples).

The separate results on hospitalization and sick leave are presented in Fig. 4 (see Table 3 “Appendix B” for the complete results). First, it is clear that the results on sick leave are very similar to the ones presented previously for the longer time period (cf.

column 5 in Table 2 and Fig. 4). Furthermore, as expected, there is a substantial increase in hospitalization for women both during the first and the second pregnancy (0.61 and 0.42 in hospitalization days/month, respectively). However, there is no evidence of a long-term increase in the female–male gap in hospitalization. In fact, if anything, there is some evidence for the opposite: After the birth of the first child, the average monthly number of hospital stays among mothers seems to decrease somewhat relative to fathers' hospital stays. This result provides some support for the role enhancement theory and/or for the theory proposed by Paringer (1983), namely that women, who are the main household producers, use work absence as a means of investment in their health.

In addition, the result that the gender gap in hospital stays decreases after parenthood provides some support for our identifying assumption for the effect on sick leave. In particular, if the gender gap in hospital decreases after parenthood, it seems unlikely that the timing of parenthood is based in couples' expectations of the opposite (a negative relative health shock for the mothers).

7.3.2 Economic incentives

In the following, we present heterogeneous effects depending on mothers' pre-birth income and the income trajectory after the birth of the first child.¹⁸ The complete results are presented in detail in Table 4 in "Appendix C," while here, we present the essence of the results graphically. To keep the discussion simple, we focus on how the effect during the 10th year after the birth of the first child varies with the mother's pre-birth income as well as income trajectory. As explained in "Appendix C," the signs of the parameter estimates are the same also for other years, and thus by focusing on the effect during year 10 we gain simplicity without losing generality.

Panel (a) in Fig. 5 depicts how the effect of parenthood 10 years after the birth of the first child varies with the mother's pre-child income. Figure 5 reveals a negative relation between the mother's pre-child income and the effect of parenthood 10 years after the first child is born. We have chosen the range of the horizontal axis to represent the range of the empirical distribution of mothers' pre-child incomes, with almost all the mass between 50,000 and 400,000 SEK measured in 2008 prices (approximately between € 4600 and € 36,800). As the figure shows, although the relation is negative as expected, the slope is not particularly steep. In the top of the earnings distribution, there appears to be no effect of parenthood on the gender gap in sick leave.

Next, in panel (b) in Fig. 5, we present how the magnitude of the effect during year 10 varies with the mothers' income trajectories. The income trajectories are the changes in income between 2 years before giving birth and each year after giving birth. In order to remove the mechanical link between sick leave and income, we measure the income the year before we measure the sick leave.

The range of the horizontal axis has been chosen so that it covers most of the empirical distribution of women's income changes for the period starting 2 years before

¹⁸ As the marriage market is characterized by assortative mating in terms of labor market productivity (see, e.g., Boschini et al. 2011) also fathers' income may be important. For this reason, we have also estimated how the effect varies with the pre-birth income level of the household, with fathers' pre-birth income, with fathers' income growth, and the within-couple relative income growth. All these analyses provide the same conclusion on the relative importance of ex post and ex ante moral hazard as the results discussed below.

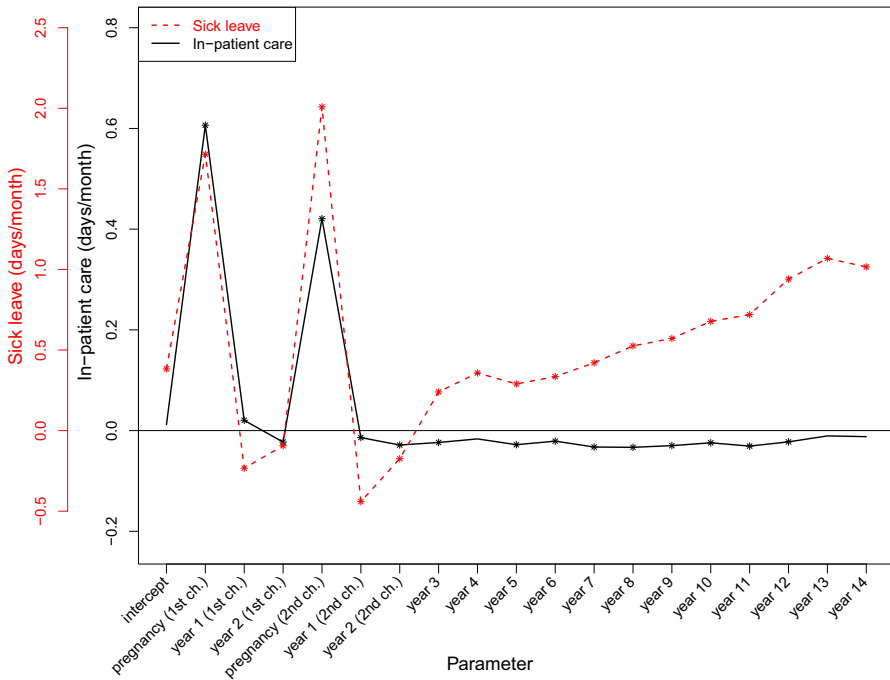


Fig. 4 The effect of parenthood on the gender gaps in sick leave and in-patient care, respectively. *Note* Significance at the 5% level for a particular parameter estimate is annotated by asterisk. The effect estimates, along with their standard errors, are presented in Table 3 in “Appendix B.” The analysis is based on a 10% representative sample of Swedish residents who had their first child between 1992 and 1998, and were active on the labor market 2 years prior to childbirth. The specification contains calendar year dummies, separate age dummies for each parents, and pre-child controls for differences in income and education. The data cover the period 1987–2005, since this is the period of coverage for the in-patient care data. Estimated for couples with at most two children

the birth of the first child and ending nine years after. As seen from the figure, the effect of parenthood varies significantly with the mother’s income trajectory. For mothers with the highest income trajectories, the effect of parenthood is even negative. In other words, for mothers that have the best labor market attachment, having a child even decreases the female–male gap in sick leave.

To summarize, a mother’s labor market attachment seems to be important for explaining the effect of parenthood on the gender gap in sick leave. The effect variation with respect to women’ income trajectory is significant, both in statistical and economic terms. We also find statistical evidence for the importance of mothers’ pre-child incomes, but not to such a large extent as the income increase.

8 Conclusion

Entering parenthood increases women’s sick leave rate in comparison with the corresponding rate for men. The effect is long lasting and persists for as long as the data allow us to follow the couples: up to 16 years after the birth of the first child. Moreover, we show that the estimated effects are not confounded by later pregnancies. We fur-

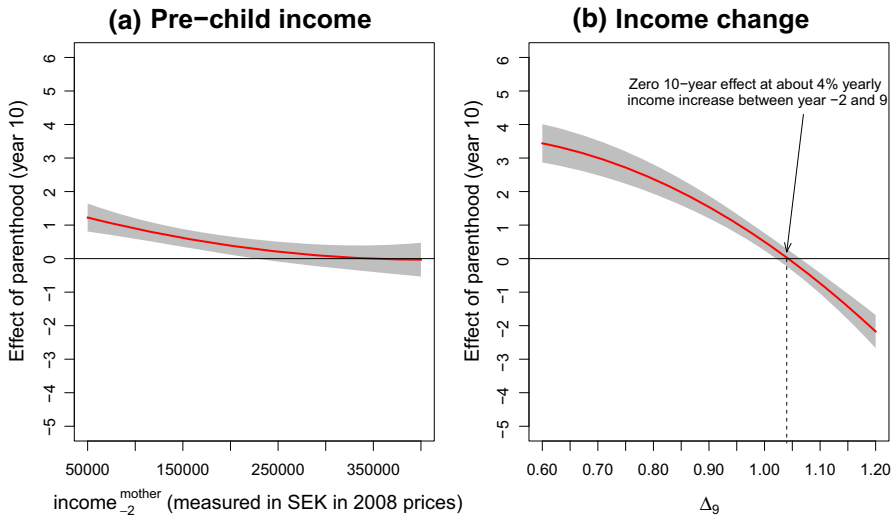


Fig. 5 Heterogeneity of the effect 10 years after child birth with regards to mothers' pre-birth income (left panel) and income change (right panel). *Note* The figure illustrates of how the year 10 effect of parenthood varies with the mother's income 2 years prior to child birth, y_{-2}^{mother} , and with $\Delta_9 = (y_9^{\text{mother}}/y_{-2}^{\text{mother}})^{\frac{1}{11}}$, where y_9^{mother} is the mother's yearly income 9 years after birth. In other words, the yearly %-rate of income change between year -2 and 9 is given by $\Delta_9 - 1$. See "Appendix C" for estimation details. Estimates for panel a) and b) come from the third and fifth columns in Table 4 in "Appendix C." The shaded areas represent 95% confidence intervals

ther investigate whether this increase in sickness leave among mothers after the arrival of the first child seems to be driven by a health deterioration or rather by economic incentives, namely by the fact that the time at home has been relatively more valuable for mothers who do most of the household work.

We find no support for a deterioration of health among women after entering parenthood. Furthermore, the estimated effect of parenthood on the gender gap in sick leave does not decline over time suggesting that the increase is not mainly driven by a health deterioration associated with having small children. We also perform a more formal test using in-patient data, and find that mothers' hospitalization rate after parenthood in comparison with before and in comparison with the corresponding rate among fathers, in fact decreases somewhat.

We find some weak evidence that the effect of parenthood on sick leave varies across women with different pre-birth incomes. This result supports the idea that, depending on their income, women face different opportunities to reconcile their commitments to home and to work on the labor market. This in turn affects their incentives for using the sickness insurance.

However, we find a much more significant factor for the magnitude of the effect, namely mothers' income trajectories since childbirth. Many mothers change their intensive margin labor supply due to parenthood. This is particularly so in Sweden, where a lower labor supply from parents is indirectly encouraged by the flexible and generous parental leave system. We find that a mother's income trajectory since giving birth is strongly related to the magnitude of the effect: the less favorable the income

trajectory, the higher the effect of parenthood on the sick leave gap. Mothers' labor supply is measured 1 year prior to sick leave, and thus, this result suggests that the lower labor supply induces an increase in sick leave rather than the other way around. Most women reduce their labor supply after parenthood and also increase their sick leave, but among those who do not, the effect of parenthood on sick leave is modest or even negative. While mothers may change occupation after parenthood as a response to the new demanding commitment at home, we believe that the most reasonable explanation for a negative income trajectory is a reduction in labor supply within the same occupation. Further investigating the potential role of changes in occupation is an interesting research question but beyond the scope of this paper.

Our interpretation of the variation of effect size with respect to the income trajectory is that the lower labor supply induces a lower threshold for using the sickness insurance, i.e., ex post-moral hazard behavior. However, a less favorable income trajectory might also be associated with occupations with low flexibility when it comes to the challenge of combining duties at home with labor market work. Thus, we cannot exclude that women with a bad income trajectory (e.g., cashiers) in comparison with women with a better income trajectory (e.g., university lecturers) to a higher extent have to report sick when they experience health problems. However, since the magnitude of the effect varies more with the income trajectory than with the pre-birth level of income, we think that the most important explanation for the majority of women is the moral hazard story.

To summarize, we have shown that parenthood increases mothers' sick leave relative to fathers' and that the effect is particularly large among mothers who reduce their labor supply. This is an important result since the absence from work arguably implies a lower wage in the long run. The gender gap in pay among parents due to a gender-differential effect of parenthood is shown in Angelov et al. (2016). In this paper, we thus provide an additional source for explaining the gender gap in pay: mothers' higher sick leave rates compared to fathers'.

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Appendix A: Identification strategy

Let $S_j(p)$, $j = f, m$, be the potential sick leave for an individual if being a parent ($p = 1$) and if not being a parent ($p = 0$), where f denotes the father and m the mother. Let $\tilde{S}(p) = (S_f(p) - S_m(p))$ and define the estimand

$$\alpha = E \{ \tilde{S}(1) - \tilde{S}(0) | p = 1 \}.$$

Furthermore, let $\tilde{S}_i(0) = \theta + \omega_i$ where i denotes couple i . In the analysis we use data on parents only, and we rely on *changes over time*. We observe couples' sick leave before ($D = 0$) and after ($D = 1$) becoming parents. Under the assumption that the gender difference in sick leave in the absence of a child is independent of D , α can be

estimated using ordinary least squares (OLS) on

$$\tilde{s}_{iD} = \theta + D\alpha + \omega_{iD}, \quad i = 1, \dots, n, \quad D = 0, 1.$$

The identifying assumption is that $E(\omega_{iD}) = 0$ for $D = 1, 0$. That is, there cannot be a trend in the sick leave gender difference in the absence of parenthood. This assumption is a direct analog to the parallel trend assumption in difference in difference (DD) frameworks.

As we observe sick leave at many different time periods we have the possibility to identify, not only the average effect up to a given time α , but also effects at different sub intervals, $t = 1, \dots, T$, where $t = 0$ is the time of child birth. Thus, we can also identify

$$\alpha_t = E \{ \tilde{S}_{it}(1) - \tilde{S}_{it}(0) | p = 1 \}, \quad t > 0.$$

Under the assumption of constant gender differences in the absence of a child, stated as

$$\tilde{S}_{it}(0) = \theta + \omega_{it}, \quad t > 0, \tag{2}$$

estimation of α_t can be performed using OLS on

$$\tilde{s}_{it} = \theta + 1(t = \tau)\alpha_\tau + \omega_{it}, \quad \tau = 1, \dots, T. \tag{3}$$

One potential reason why θ could vary with t is gender differences in health deterioration, changes in preferences or the threshold for using the sickness insurance, etc., over the age. However, as we observe the age of the parents we can control for these changes working through age by adding age to Eq. (3).

In a traditional DD framework, a potential problem is that there are might be other shocks than the specific treatment during the given year or month of treatment that affects treated and controls differently. In our case, both groups (women and men) are treated, but the analogy to the DD case is still clear. However, as we observe couples becoming parents at different calendar times $c = 1992, \dots, 1998$ we can control for these potential specific calendar time effects. To this end, let \tilde{s}_{itc} be the observed sick leave t periods before or after child birth and let c denote calendar time. We then estimate

$$\tilde{s}_{itc} = \theta + \sum_{\tau=1}^T \alpha_\tau 1[t = \tau] + \theta_c + \tilde{\mathbf{x}}' \boldsymbol{\phi} + \omega_{itc}, \quad c = 1988, \dots, 2008, t = t^*, \dots, T$$

where $\tilde{\mathbf{x}}$ is a covariate vector of pre-pregnancy within-couple differences in education, yearly labor income, and age. The estimation is performed for $t^* = -4$ and $T = 17$.

A further advantage with our sampling is that we have no sample selection problem which could be a problem in a tradition DD framework using repeated cross-sectional data. If the change in population is a consequence or correlated with the reform or

policy under investigation the DD estimator using repeated cross sections will be flawed. This problem would most likely increase with time since the given reform or policy change and hence increase the problem of estimating long run effects.

The threat to the identification of the effect α_t is that ω_{itc} could be a function of t , or in other words, that assumption (2) does not hold despite adding control variables. This would happen if there was a trend in ω_{itc} , if using the DD language, that the trends in sick leave of women and men in the absence of parenthood diverge after parenthood. From Fig. 3, and from the test for a deterministic time trend provided in the main text, we find no support for differences in trends for males and females before conception. This hence provides suggestive evidence that the identifying assumption holds. However, as we observe the same individuals before and after child birth, we can also set up a formal test by testing for a unit root in the error terms.

To this end, we use the Harris and Tzavalis (1999) (H–T) panel unit root test with the following specification:

$$\widehat{\omega}_{iq} = \mu_i + \phi_i q + \rho \widehat{\omega}_{iq-1} + \psi_{iq} \text{ for } q = -48, -47, \dots, -10$$

where q is month before birth, $\widehat{\omega}_{iq}$ for $q = -48, -47, \dots, -10$ are fitted residuals during a period of four years before child birth to 10 months before (i.e., the period ends just before conception). This specification is a novel way of testing for a unit root since the test is performed with respect to time to birth (q) instead of calendar time. The index for calendar time is instead suppressed in the notation above, because the fitted residuals come from a model with calendar time dummies. This particular panel unit root test uses cross-sectional asymptotics for a fixed T , which makes it suitable for our application. Under the null hypothesis of a unit root, $\rho = 1$ and the series are non-stationary. If we reject the null hypothesis of a unit root, and also do not find a deterministic trend in time to birth, we find support for our identification assumption.

Effect heterogeneity

In the following, we discuss the identification of the effect heterogeneity with respect to the income trajectory. Define the yearly income growth rate (“the income growth trajectory”) between the pre-conception income 2 years before birth (y_{-2}) and $t - 1$ (y_{t-1}) as

$$\Delta_{t-1} = (y_{t-1}^{\text{mother}} / y_{-2}^{\text{mother}})^{\frac{1}{t-1+2}}.$$

for a mother. We estimate the following model using OLS

$$\widetilde{s}_{itc} = \theta + \sum_{\tau=1}^{17} \alpha_{\tau} \mathbf{1}[t = \tau] + \sum_{\tau=3}^{17} \delta_{\tau} \Delta_{\tau-1} \mathbf{1}[t = \tau] + \widetilde{\mathbf{x}}' \boldsymbol{\phi} + \theta_c + \omega_{itc}.$$

Note that we have excluded the interactions with Δ_{t-1} during the first 2 years after delivery (i.e., for $t = 1, 2$). The reason for this is that Δ_{t-1} for $t = 1$ and 2 does not have a clear interpretation since most Swedish mothers are at home during the child’s

first years with low or zero income, but with income replacement from the parental benefits system.

If ω_{itc} is uncorrelated with Δ_{t-1} , then the OLS estimator of $\alpha_t + \delta_t \Delta_{t-1}$ is a consistent estimator of the average relative effect t years from birth for a mother with income growth Δ_{t-1} . Since sick leave benefits are only received when the person cannot work (and thus has no income), ω_{itc} has a direct impact on mothers' income at t . However, if there is no deterministic or stochastic trend in $\{\omega_{ijc}\}_{j=1}^t$, ω_{itc} will be uncorrelated with Δ_{t-1} .

Appendix B: Estimation results for in-patient care

See Table 3.

Table 3 In-patient care and sick leave

	In-patient care	Sick leave
Pregnancy (1st child)	0.606*** (0.00438)	1.715*** (0.0406)
Year 1 (1st child)	0.0204*** (0.00397)	-0.233*** (0.0343)
Year 2 (1st child)	-0.0228*** (0.00325)	-0.0938* (0.0432)
Pregnancy (2nd child)	0.421*** (0.00450)	2.009*** (0.0584)
Year 1 (1st child)	-0.0139** (0.00474)	-0.441*** (0.0518)
Year 2 (2nd child)	-0.0286*** (0.00382)	-0.175** (0.0615)
Year 3	-0.0236* (0.00964)	0.240* (0.0987)
Year 4	-0.0165 (0.00934)	0.358*** (0.0967)
Year 5	-0.0280*** (0.00596)	0.289*** (0.0783)
Year 6	-0.0209*** (0.00609)	0.335*** (0.0812)
Year 7	-0.0326*** (0.00682)	0.421*** (0.0906)
Year 8	-0.0332*** (0.00671)	0.525*** (0.103)
Year 9	-0.0299*** (0.00723)	0.572*** (0.118)

Table 3 continued

	In-patient care	Sick leave
Year 10	−0.0243** (0.00813)	0.678*** (0.136)
Year 11	−0.0308** (0.00940)	0.719*** (0.151)
Year 12	−0.0224** (0.00837)	0.940*** (0.170)
Year 13	−0.0106 (0.00955)	1.069*** (0.198)
Year 14	−0.0120 (0.0103)	1.016*** (0.244)
<i>N</i>	2,674,802	2,674,802
<i>R</i> ²	0.029	0.011

Standard errors within parentheses are clustered at couple level. Significance levels are denoted by * ($p < 0.05$), ** ($p < 0.01$), and *** ($p < 0.001$). Both specifications contain calendar year dummies, age difference, and pre-child controls for differences in income and education. The data cover the period 1987–2005, since this is the period of coverage for the in-patient care data. Estimated for couples with at most two children

Appendix C: Results on the role of economic incentives

In the following, we present the heterogeneous effects depending on a mother's pre-birth income and her income trajectory after the birth of the first child, based on the discussion in Sect. 7.2. The results are presented in Table 4. In order to have a reasonable measure of a mother's income trajectory, the estimates in Table 4 are based on a sample where the mother's pre-birth income at $t = -2$ is higher than 50,000 SEK (approximately €4600), measured in 2008 prices. The first column presents estimates from the baseline specification for this sample with the full set of controls. The estimates are close to the case with the full sample: for instance, the year 10 effect of parenthood is estimated to be 0.654 sick days/month and the corresponding number for the full sample is 0.664 (see Table 2).

The rest of Table 4 presents a heterogeneity analyses with respect to mothers' pre-child incomes (column 2), mothers' pre-child incomes in level and squared (column 3), mothers' income trajectories since before giving birth in levels (column 4), and in levels and, in order to take the functional form assumption into account, squared (column 5). Below, we discuss the results in columns 3 and 5, which both contain levels as well as squares of the interaction variables.

First, consider column 3 in Table 4, where we empirically investigate whether the magnitude of the effect varies with a second-degree polynomial in the mother's pre-birth income level. Generally, the point estimates for the interaction term between the effect of parenthood and the mother's pre-child income have the expected negative sign, and some are statistically significant. For a particular year t after childbirth, these are the effects denoted by year $t \times y_{-2}^{\text{mother}}$, where y_{-2}^{mother} is the income of the mother

2 years before childbirth, in Table 4. The estimates are very small (see also the graphical representation in Sect. 7.3.2). The point estimates for the interaction between the effect of parenthood and the mother's pre-child income squared are statistically insignificant. These parameters are denoted by year $t \times (y_{-2}^{\text{mother}})^2$ in Table 4. Panel a) in Fig. 5 in the main text is based on column 3 in Table 4: the baseline year 10 effect estimate of 1.605, the estimate of the interaction between pre-child income level and the year 10 effect of -0.00000808 , and the estimate of the interaction between pre-child income squared and the year 10 effect of $9.99e-12$.

Looking at our second hypothesis, namely, whether the effect of parenthood is larger for mothers with a low income trajectory, we check whether the magnitude of the effect of parenthood t years after childbirth varies with a mother's income trajectory between year -2 (i.e., 2 years before giving birth) and year $t - 1$ (i.e., the year before we measure the sick leave). The results from this analysis are presented in column 5, where we have included the level as well as the square of the interaction variable defined in terms of an income ratio. The interaction terms with the level year $t \times (\Delta_{t-1})$, where $\Delta_{t-1} = y_{t-1}^{\text{mother}}/y_{-2}^{\text{mother}}$, and square year $t \times (\Delta_{t-1})^2$ of the interaction variable are both statistically and economically significant, for all years. Panel b) in Fig. 5 in the main text is based on column 5 in Table 4: The main effect estimate for year 10 is 1.799, the estimate of the interaction between Δ_9 and the year 10 effect of 8.776, and the estimate of the interaction between pre-child income squared and the year 10 effect of -10.07 . Note that the empirical distribution of Δ_9 also contains a mass at 0, i.e., women that have withdrawn from the labor force in year 9. Those observations are used in the estimation but not shown in the figure in the main text. For instance, for $\Delta_9 = 0$, the value of the effect in year 10 is estimated to be 1.799 (i.e., the main effect estimate).

Table 4 Heterogeneity analysis with yearly effects for the sample of couples where $y_{-2}^{mother} > 50,000$ SEK in order for Δ_{t-1} to be meaningful ($\Delta_{t-1} = (y_{t-1}^{mother} / y_{-2}^{mother})^{1/T}$ with t being time in years since birth)

	With interaction terms				
	Baseline	y_{-2}^{mother}	$(y_{-2}^{mother})^2$	Δ_{t-1}	Δ_{t-1}^2
Pregnancy	1.754***	2.383***	2.426***	1.755***	1.752***
Year 1	-0.136***	-0.205*	-0.187	-0.135***	-0.137***
Year 2	0.333***	0.336**	0.540**	0.335***	0.334***
Year 3	0.514***	0.655***	0.756***	0.316***	0.332**
Year 4	0.334***	0.556***	0.833***	0.279**	0.328**
Year 5	0.286***	0.608***	1.064***	0.515***	0.517***
Year 6	0.305***	0.650***	1.129***	1.046***	0.899***
Year 7	0.411***	0.757***	1.063***	1.413***	1.097***
Year 8	0.463***	0.926***	1.068***	1.541***	0.995***
Year 9	0.530***	1.145***	1.430***	1.993***	1.290***
Year 10	0.654***	1.241***	1.605***	2.577***	1.799***
Year 11	0.721***	1.437***	1.600***	2.922	2.035***
Year 12	0.801***	1.697***	1.674***	3.110***	2.213***
Year 13	0.822***	1.427***	1.546***	2.925***	2.140***
Year 14	0.886***	1.521***	1.677***	2.544***	1.796***
Year 15	0.969***	1.517***	1.646**	2.676***	1.887***
Year 16	0.999***	1.112**	1.220*	2.525***	1.925***
Year 17	1.019***	0.939*	1.404	2.299***	1.805**

Table 4 continued

	Baseline	With interaction terms y_{t-2}^{mother}	$(y_{t-2}^{mother})^2$	Δ_{t-1}	Δ_{t-1}^2
	Pregnancy $\times y_{t-2}^{mother}$	-0.00000361***	-0.00000405*		
	Year 1 $\times y_{t-2}^{mother}$	0.000000358	0.000000194		
	Year 2 $\times y_{t-2}^{mother}$	-9.10e-08	-0.00000235		
	Year 3 $\times y_{t-2}^{mother}$	-0.000000922	-0.00000204		
	Year 4 $\times y_{t-2}^{mother}$	-0.00000144*	-0.00000453*		
	Year 5 $\times y_{t-2}^{mother}$	-0.00000207**	-0.00000720***		
	Year 6 $\times y_{t-2}^{mother}$	-0.00000227**	-0.00000764***		
	Year 7 $\times y_{t-2}^{mother}$	-0.00000236**	-0.00000577*		
	Year 8 $\times y_{t-2}^{mother}$	-0.00000312***	-0.00000469*		
	Year 9 $\times y_{t-2}^{mother}$	-0.00000410***	-0.00000725**		
	Year 10 $\times y_{t-2}^{mother}$	-0.00000405***	-0.00000808***		
	Year 11 $\times y_{t-2}^{mother}$	-0.00000492***	-0.00000669**		
	Year 12 $\times y_{t-2}^{mother}$	-0.00000610***	-0.00000576		
	Year 13 $\times y_{t-2}^{mother}$	-0.00000454***	-0.00000581*		
	Year 14 $\times y_{t-2}^{mother}$	-0.00000483***	-0.00000653		
	Year 15 $\times y_{t-2}^{mother}$	-0.00000444**	-0.00000582		
	Year 16 $\times y_{t-2}^{mother}$	-0.00000200	-0.00000314		
	Year 17 $\times y_{t-2}^{mother}$	-0.000000951	-0.00000622		

Table 4 continued

	With interaction terms		
	Baseline	y_{-2}^{mother}	Δ_{t-1}
Pregnancy $\times (y_{-2}^{\text{mother}})^2$		$(y_{-2}^{\text{mother}})^2$	Δ_{t-1}^2
Year 1 $\times (y_{-2}^{\text{mother}})^2$		9.59e-13	
Year 2 $\times (y_{-2}^{\text{mother}})^2$		2.74e-13	
Year 3 $\times (y_{-2}^{\text{mother}})^2$		5.44e-12	
Year 4 $\times (y_{-2}^{\text{mother}})^2$		2.62e-12	
Year 5 $\times (y_{-2}^{\text{mother}})^2$		7.55e-12	
Year 6 $\times (y_{-2}^{\text{mother}})^2$		1.26e-11*	
Year 7 $\times (y_{-2}^{\text{mother}})^2$		1.33e-11*	
Year 8 $\times (y_{-2}^{\text{mother}})^2$		8.40e-12	
Year 9 $\times (y_{-2}^{\text{mother}})^2$		3.85e-12	
Year 10 $\times (y_{-2}^{\text{mother}})^2$		7.80e-12	
Year 11 $\times (y_{-2}^{\text{mother}})^2$		9.99e-12*	
Year 12 $\times (y_{-2}^{\text{mother}})^2$		4.41e-12	
Year 13 $\times (y_{-2}^{\text{mother}})^2$		-7.96e-13	
Year 14 $\times (y_{-2}^{\text{mother}})^2$		3.30e-12	
Year 15 $\times (y_{-2}^{\text{mother}})^2$		4.46e-12	
Year 16 $\times (y_{-2}^{\text{mother}})^2$		3.67e-12	
Year 17 $\times (y_{-2}^{\text{mother}})^2$		3.03e-12	
		1.38e-11	

Table 4 continued

	With interaction terms		
	Baseline	y_{-2}^{mother}	$(y_{-2}^{mother})^2$
Year 3 × Δ ₂		0.270*	Δ _{t-1} ²
Year 4 × Δ ₂		0.0681	0.152
Year 5 × Δ ₂		-0.287*	-0.292
Year 6 × Δ ₂		-0.865***	-0.293
Year 7 × Δ ₂		-1.136***	0.471
Year 8 × Δ ₂		-1.209***	2.240**
Year 9 × Δ ₂		-1.621***	5.067***
Year 10 × Δ ₂		-2.115***	7.158***
Year 11 × Δ ₂		-2.415***	8.776***
Year 12 × Δ ₂		-2.532***	11.78***
Year 13 × Δ ₂		-2.319***	13.66***
Year 14 × Δ ₂		-1.857***	13.72***
Year 15 × Δ ₂		-1.920***	16.12***
Year 16 × Δ ₂		-1.744***	19.25***
Year 17 × Δ ₂		-1.488*	14.03***
Year 3 × Δ ₂ ²			9.839*
Year 4 × Δ ₂ ²			0.111
Year 5 × Δ ₂ ²			0.326
Year 6 × Δ ₂ ²			0.00372
Year 7 × Δ ₂ ²			-1.216**
Year 8 × Δ ₂ ²			-3.083***
Year 9 × Δ ₂ ²			-5.738***
			-8.064***

Table 4 continued

	With interaction terms		
	Baseline	y_{-2}^{mother}	$(y_{-2}^{\text{mother}})^2$
		Δ_{t-1}	Δ_{t-1}^2
Year 10 $\times \Delta_2^2$			-10.07***
Year 11 $\times \Delta_2^2$			-13.22***
Year 12 $\times \Delta_2^2$			-15.17***
Year 13 $\times \Delta_2^2$			-15.13***
Year 14 $\times \Delta_2^2$			-17.07***
Year 15 $\times \Delta_2^2$			-20.17***
Year 16 $\times \Delta_2^2$			-15.07***
Year 17 $\times \Delta_2^2$			-10.84**
N	3,474,942	3,474,942	3,474,942

Standard errors are clustered at couple level but omitted for conciseness. Significance levels are denoted by * ($p < 0.05$), ** ($p < 0.01$), and *** ($p < 0.001$). The regressions include calendar year dummies, separate age dummies for each parent, and pre-child controls for differences in income and education

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