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Impacts of policies, peers and parenthood on labor market outcomes

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Uppsala, January 2014

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Contents

Introduction

This thesis consists of five self-contained, but related, papers covering the relevance of paid parental leave entitlements (**Policies**), co-workers' behavior (**Peers**), and the number and timing of births (**Parenthood**) for labor market outcomes. The papers are related in terms of all being, in a broad sense, associated to the economics literature on gender disparities in the labor market. Among the most dramatic demographic and labor market changes in developed countries during the last decades include the inflow of women to the labor market, the simultaneous decline in fertility rates, the rising age at first birth, and the overall decline in the male-female wage gap. These variables are all interrelated, a fact that is highlighted by the vast economics literature on the gender wage gap, fertility and female labor supply with varying points of departure. For example, decreased fertility has been studied as a means to explain historical trends in female labor supply. As many countries struggle with below replacement fertility, the negative relationship between fertility and female labor supply has also spurred a large interest in policies that help countries maintain both high fertility and high female labor market participation through reducing barriers to the combination of market work and family.

The correlation between children and women's labor supply is also relevant for the gender wage gap. Traditionally, economists have stressed the difference in human capital accumulation as the main source of the gender gap in earnings. In developed countries, the human capital investments of women - in terms of formal education and labor market experience - have approached, or even surpassed, those of the male population, but women are still observed to work fewer hours and earn less compared to men. There is a large literature attributing the persistent gender wage gap to the difference in family obligations, as it is well established that childbearing is associated with work discontinuities for women, which imply periods of foregone investments in human capital.

Recently, the age at first birth has been explored as a determinant for women's career opportunities and - in extension - the gender wage gap, as the age at first birth has been observed to be correlated to a range of outcomes. Theoretically, postponing motherhood affects the human capital acquisition of women, and thereby affects lifetime earnings and wage growth. At the same time, however, another point of departure in the economics literature includes studies where economic factors, such as rising female wages, are being explored as determinants of the timing and spacing of births. In addition, fluctuations in the timing and spacing of births are, in turn, studied as explanations for fluctuations in aggregate fertility trends.

Thus, the relationships between fertility, female labor supply and wages are complex and interwoven. However, as is clear from the literature, these variables have important implications for societies as a whole, as well as for individuals' opportunities in the labor market. The papers in this thesis focus on the impacts on *individual* outcomes. *Paper 4* directly relates to the literature on the effect of fertility on female labor supply, as we estimate the effect of having one additional child on women's participation and earnings, and how these effects evolve over child age. In *Paper 1* and *Paper 2*, I turn to the questions of whether the timing of first birth and the spacing of births, respectively, affect the labor market careers of women. Paper 1 asks the question of whether the career point at which a woman has her first child affects her lifetime earnings and wage growth, and in Paper 2 I study whether the spacing of births affects women's subsequent participation, earnings and wage rates. In *Paper 5*, we explore how the recent reforms in the Swedish parental leave system have affected mothers' and fathers' parental leave usage, labor supply and wages. We study both a general expansion of paid leave and the introduction of gender quotas in parental leave.

Although the economics literature has traditionally turned to discrimination and human capital accumulation as the main sources of the gender gap in earnings, recent advances propose alternative explanations. Specifically, researchers have turned to the possibility that there are important differences in psychological attributes between men and women. For example, some evidence suggest that women are more averse towards risk and competition compared to men, and that they are more other-regarding and reciprocal.[1] *Paper 3* in this thesis contributes to this literature, by testing for gender differences in social preferences at the workplace, based on a randomized field experiment.

A second central theme throughout this thesis is methodological, namely, the aim to distinguish causation from correlation. When variation in the explanatory variable of interest is not generated by a controlled experiment, but instead is drawn from observational data, determining the causal link between two variables that are correlated is complicated by a number of reasons. Consider, for example, the relationship between the number of children and women's labor supply, which has been shown to be negative in numerous studies. This negative correlation may reflect a causal link running from children to labor supply. However, it could also be driven by a third, unobserved, variable that affects both fertility and labor supply. For instance, the negative correlation could reflect heterogeneity in preferences for children and market work in the population. Alternatively, fertility and career choices may be governed by a joint decision process, giving rise to a simultaneity problem.

In an experiment, the researcher has the ability to control and monitor the environment, and generate variation in the variable of interest while keeping all other determinants of the outcome variable constant. The feature of the experimental methodology that makes the latter possible is randomized assignment; the outcomes of a randomly chosen group which are given a treatment are compared to the outcomes of a control group. Randomized assignment balances all other determinants of the outcome of interest in the treatment and control groups, making treatment received the only thing differing between treated and non-treated individuals. It is not difficult to understand why controlled experiments are often not feasible in the social sciences; the reasons are obvious in the example of children and labor supply. Nevertheless, some problems can be analyzed by conducting social (or field) experiments.[2] Examples of such are field experiments engineered to test the presence of discrimination

[1] See Bertrand (2011) for a review of the literature.

[2] See e.g. List and Rasul (2011) for a review of field experiments in labor economics.

in the labor market, for instance by sending out resumes with randomly assigned male or female names to job advertisements.

Empirical economists must, however, often rely on observational data and methods that are aimed at reproducing the features of an experimental ideal. The most widely used methods involve variation in a variable of interest generated from a natural experiment. A natural experiment is an event that creates exogenous variation in a variable for a subset of the population, creating natural treatment and control groups that are similar in terms of characteristics. Natural experiments can arise due to e.g. policy reforms that change the conditions for a subset of the population, or be the result of randomly occurring events by nature that can be used as a source of exogenous variation in a variable of interest. One example of the latter is twin births, which have been used extensively to study the causal effect of the number of children on women's labor supply.

To address the methodological problems inherent in the research questions addressed in this thesis, I exploit natural experiments and, in one of the papers, data from a large scale randomized field experiment. The first two papers analyze the timing and spacing of births, which are not likely to be orthogonal to unobservable factors that affect the outcomes. Therefore, I make use of the occurrence of miscarriages as a natural experiment, since they are randomly occurring fertility shocks that delay time to birth. In Paper 4, the question addressed is instead the effect of the *number* of children. In that paper, we exploit the fact that parents whose first two children are of the same sex are more likely to move to higher parity compared to parents whose first two children are of mixed sex. Thus parents' preferences for a mixed-sex sibling composition is used as a source of exogenous variation in family size. In Paper 5, we estimate the effect of different parental leave schemes on parents' labor market outcomes, using the fact that eligibility to new rules was based on children's birth date, which is randomly assigned. In Paper 3, we use data from a large scale randomized experiment in the sickness insurance, where the monitoring of absence was decreased for the treated individuals. Exploiting this variation, we test the extent to which male and female workers respond to their co-workers' behavior in their individual shirking decisions.

A necessary requirement for implementation of these empirical methods is the availability of high quality data. The papers in this thesis are all based on Swedish population-wide register data. The data are rather unique in the sense that they allow linking records of individuals' fertility histories, employment, workplaces, earnings, wages, sickness absence, health outcomes, parental leave usage and background characteristics. Moreover, the data are longitudinal and individuals' outcomes are followed over a long time horizon, which creates a possibility to study outcomes over the life cycle.

In the remaining sections of this introductory chapter, I briefly outline the economics literature on the topics related to the contents of this thesis, as well as describe the main results from the individual papers.

1. Parenthood, Labor Supply and Wages

Over the past decades, most of the developed countries have witnessed an increase in the female labor force participation rate and a simultaneous decline in fertility. This has given rise to a large literature that studies the relationship between fertility and labor market outcomes of women. Due to conflicting demands on time from market work and family, some women may drop out of the labor market entirely while others resort to part-time work. The decrease in the labor supply in connection to childbearing influences lifetime earnings, and accounts for a large share of the opportunity costs of children. Consequently, much attention has been devoted to understand the relationship between children and women's labor market involvement. Estimating the effect of children on labor supply has, however, proven to be challenging for a number of reasons. Importantly, labor market and fertility decisions may be jointly determined (Browning 1992, Angrist and Evans 1998, Rosenzweig and Wolpin 1980a,b). Angrist and Evans (1998) noted that this simultaneity was reflected in the research agenda where, on the one hand, studies on labor supply would often treat child-status variables as regressors in labor supply equations, while on the other hand economic demographers and others used models that were aimed at describing the impact of labor supply measures on fertility. The authors further argued that, since fertility variables cannot be endogenous and exogenous at the same time, neither type of regression is likely to have a causal interpretation. The recent literature has been largely focused on establishing a causal relationship between children and labor supply. Angrist and Evans (1998) proposed a new source of exogenous variation in family size, induced by parents' preferences for a mixed-sex sibling composition. Specifically, this instrument exploits the well-known phenomenon that parents whose first two children are of the same sex are more likely to go on to have an additional child compared to parents whose first two children are of mixed sex. Since the sex-mix of children is in essence randomly assigned and not likely to be correlated to labor market outcomes, it can be used as an instrument for the number of children, and the strategy has been adopted in several subsequent studies to estimate the effect of children.[3] Another strategy has been to use the occurrence of twin births as a source of exogenous variation in family size (see e.g. Rosenzweig and Wolpin 1980a,b, Bronars and Grogger 1994).[4] Generally, a common finding in the literature is that when endogeneity of fertility is not taken into account, the effect of children on women's labor supply is overstated. Nevertheless, instrumental variables estimates commonly suggest a non-negligible reduction in the labor supply of women caused by having children (Angrist and Evans 1998, Bronars and Grogger 1994, Vere 2011, Jacobsen et al. 2009).

In addition to affecting lifetime earnings through foregone income during interruptions, the decreased labor market effort around birth is hypothesized to affect women's wage attainment. It is widely observed that women earn lower wages than men on average and that mothers earn less than non-mothers. A large theoretical and empirical literature has been devoted to understand the relevance of career interruptions due to childbearing for the gender- or family wage gap, from which the results have been mixed. This ambiguity of the effect of motherhood on wages may in part be explained by different studies estimating different effects due to there being several possible channels through which motherhood affects wages. A common terminology, for example, is to discuss the motherhood wage penalty as being

[3] For example, Iacovou (2001) uses the sex-mix strategy on data from the United Kingdom, Maurin and Moschion (2009) on data from France, Cruces and Galiani (2007) on data from Argentina and Mexico and Hirvonen (2009) on data from Sweden.

[4] Other strategies to measure the causal effect of childbearing has been to use a dynamic treatment approach to measure the effect of having a first child now versus later (Fitzenberger et al. 2013), or to compare the income- and wage trajectories of women in relation to their male partners before and after parenthood in a difference-in-differences setup (Angelov et al. 2013).

comprised by a direct effect and an indirect effect, where the indirect effect runs through the impact of motherhood on intermediate variables which in turn affect wage attainment. For example, one such indirect effect potentially goes through reduced experience: human capital theory predicts that experience have positive returns because it entails on-the-job training that makes workers more productive (see e.g. Becker 1964, Mincer and Polachek 1974). Thus, time out of work for child rearing is experience foregone, and women earn less than men because they on average have accumulated less experience. Another indirect effect could be that the expectation of future work interruptions may cause women to choose jobs that are more "mother-friendly". As predicted by the theory of compensating differentials, the features of a job that make them easier to combine with family may compensate for the lower wages. For example, Becker (1991) argues that mothers may choose jobs that require less energy or have flexible working hours. These jobs may have a higher starting wage, but flatter wage profiles and less potential for training and advancement.[5]

The direct effect instead, is the potential effect of motherhood on wages that goes over and beyond the effects that goes through reduced experience. Typically, this effect has been tested in the literature by means of wage equations augmented by the inclusion of time-out variables (see e.g. Albrecht et al. 1999). If time out has a negative effect on wages, this is interpreted as women suffering an additional negative effect above and beyond the wage lost due to foregone experience. Such an effect has been interpreted as the result of skill depreciation, i.e., that skills become obsolete or forgotten during time out of the labor market. In addition, there can be demand-side explanations for this result, namely, employer discrimination. Consider, for instance, that mothers are less productive on average than non-mothers. Since individual productivity is difficult for the employer to measure, employers may assign mothers wages or jobs based on the average productivity of mothers, giving rise to a wage gap between mothers and non-mothers that is proportional to their estimated productivity gap.

Thus, depending on whether or what type of experience variables are included in wage regressions, the estimations may produce mixed results on the effect of motherhood on wages. For the United States, several studies find an effect of motherhood on women's wages, both with and without taking experience into account. For instance, Lundberg and Rose (2000) find a penalty of 5 percent for women's first child without controlling for experience. Waldfogel (1997), on the other hand, finds a motherhood penalty net of experience of 6 percent per child. Similarly, Budig and England (2001) find a motherhood wage penalty of 7 percent, which is reduced to 5 percent per child after controlling for experience. For the Nordic countries, Albrecht et al. (1999) find no wage penalty of time out for formal parental leave for women in Sweden, and for Denmark, Gupta and Smith (2002) and Simonsen and Skipper (2006), for example, find no motherhood wage penalty once experience is held constant. Angelov et al. (2013), compare the wage trajectories of women in relation to their male partners before and after parenthood in a difference-in-differences setup, and find that 15 years after the birth of the first child, the male-female wage gap within couples increased by 10 percentage points, not accounting for experience.

[5] It has also been proposed that women with similar human capital differ in their productivity. For example, Becker (1991) argues that mothers may be less productive and exert less effort on the job due to decreased energy from child rearing activities.

In **Paper 4**, (joint with Nikolay Angelov) we contribute to the literature on women's labor supply responses to fertility, as well as to potential consequences of motherhood for wage attainment. Our paper studies how an *additional* child affects women's participation, labor income and wages. We use Swedish administrative data and exploit parents' preferences for a mixed-sex sibling composition as a source of exogenous variation in family size, which was the method originally applied by Angrist and Evans (1998). An important feature of our study is, however, that we employ the sex-mix strategy to uncover the temporal pattern of the fertility effect on mothers' income with respect to time since birth. Understanding the dynamics of individuals' labor supply response to childbearing is crucial to be able to gauge the total effect of children. Also, it provides knowledge about the duration of home time after birth, which may matter for women's subsequent opportunities in the labor market and provide important implications for policy. Several previous studies examine the dynamics of the fertility effect, but the methods have varied. One common method has been to construct synthetic-cohort life cycles by exploiting the fact that women in a cross-sectional sample had their children at different points in time (see e.g. Vere 2011, Jacobsen et al. 1999). These studies generally find that the effects of childbearing are largest in the short-run and tend to dissipate over time as children grow older. Our paper contributes to this existing literature by being able to follow the same mothers over time in a longitudinal data set, and recover the (true) temporal pattern of the effect of children on women's labor market outcomes.

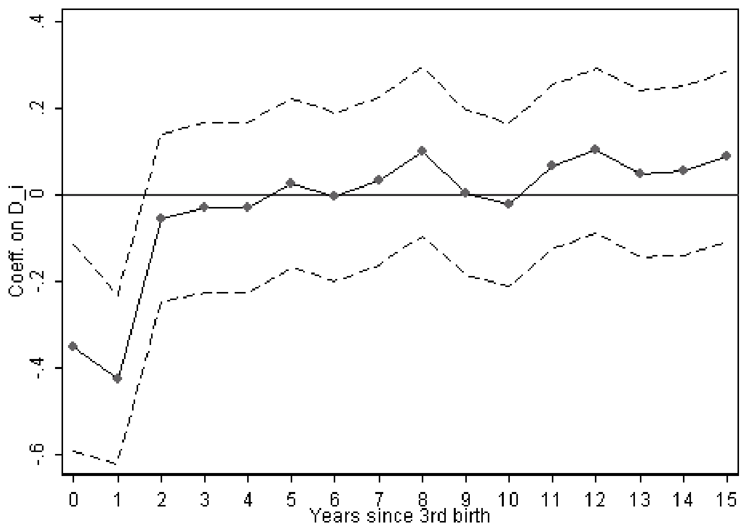

FIGURE 1. Estimated coefficients of the effect of a third child on participation for varying years since third birth, along with the 95 percent confidence intervals.

Figure 1 plots the coefficients from the instrumental variables estimations of the effect of an additional child on labor market participation for the first 15 years after birth. As seen, mothers withdraw from the labor market the first couple of years after birth, an effect which likely reflects formal parental leave, after which participation bounces back. Thus, at least on the margin of moving from two to three children, the negative effect of children on women's labor supply is relatively short-run. The result that the effect of childbearing is relatively short-lived is in line with findings from the United States. However, our results suggest a faster income recovery after childbearing, and a smaller negative effect of children on women's income compared to the United States. The difference in the magnitude of the fertility effect is likely driven by institutional differences. For example, Sweden offers state mandated job-protected leave with wage-replacement, which likely allows less disruptive careers for mothers.

Regarding effects on wage rates, our strategy does not allow testing whether there is an additional wage penalty above and beyond a wage effect that can be attributed to decreased experience. Nevertheless, our methodology provides insights about the extent of wage consequences of further childbearing; while our findings suggest that the labor supply reduction is rather short-lived, the short-run effect is sizeable. Theoretically, an effect on wages in our setting could thus be fully driven by reduced experience, or be attributed to both reduced experience and skill depreciation. However, we do not find any evidence suggesting that an additional child impacts long-run wage rates. This casts some doubt on the importance of skill depreciation and foregone investments in human capital in explaining the motherhood wage penalty.

2. The Timing and Spacing of Births

A demographical change witnessed by most developed countries is the rise in the age at first birth for mothers.[6] In Sweden, for example, the average age at first birth for women increased from 24 in 1970 to 29 in 2012. Also the spacing of births has undergone changes during the same time period. In the United States, the spacing between first and second and second and third births has lengthened over time (Hotz et al. 1997a), while in Sweden the birth spacing seem to have been shortened, in particular during the 1980s. To illustrate, Figure 2 shows the average months between first and second birth for the cohorts of mothers who gave birth to their second child in 1970 to 1995. As shown, the spacing decreased substantially from the early 1980s to the mid 1990s. The shortening of birth intervals has shown to in part be attributed to the introduction of the so called speed-premiums in the Swedish parental leave systems in 1980 and 1986 (indicated by the vertical lines in Figure 2), which allow parents to retain the same level of compensation for parental leave for a subsequent child without having to re-establish eligibility by going back to work after a birth, provided that the birth interval is sufficiently close. This eligibility interval was initially set to a birth spacing shorter than 24 months, but was extended to 30 months in 1986. Hoem (1993) shows that parents reacted to the speed premium by increasing their fertility particularly strongly before the end of the eligibility interval.

In addition to the number of children, research on the determinants of fertility has recognized the importance of the timing of births in explaining aggregate fertility trends (Gustafsson 2001, Hotz et al. 1997a). For example, the baby boom and subsequent bust in the post-war United States has in part been attributed to shifts in the timing of childbearing, with the boom being accounted for by women shifting their childbearing to earlier ages, and the subsequent decline being attributed to the tendency of delayed childbearing (Hotz et al. 1997a). To understand fluctuations in aggregate fertility trends, it is thus important to understand the determinants of the optimal age at motherhood. Indeed, the crucial question of

6 See e.g. Gustafsson (2001) for an overview for European countries.

what factors that explain the trend towards delaying motherhood has received much attention in the economics literature on fertility dynamics. As discussed in the previous section, childbearing entails costs, both in terms of foregone wages and human capital investments and in terms of increased household expenditures. Postponing motherhood may help reduce the costs of children by allowing consumption smoothing and by reducing the time horizon over which the foregone wages and human capital investments are accrued (Happel et al. 1984, Miller 2011). Thus, factors that affect the costs of children - such as education, female wages, child care costs and spousal earnings - will affect the timing decisions of fertility (see e.g. Hotz and Miller 1988, Heckman and Walker 1990, Walker 1995). However, less attention has been devoted to understand the effect of the timing of births on women's labor market outcomes. The latter is important since, although work interruptions are generally associated with adverse effects on women's wage attainment, the negative effects are likely to vary by the timing of the work interruption. Thus, an analysis of the career consequences of first birth timing allows for an analysis of the effect of career interruptions on wages.

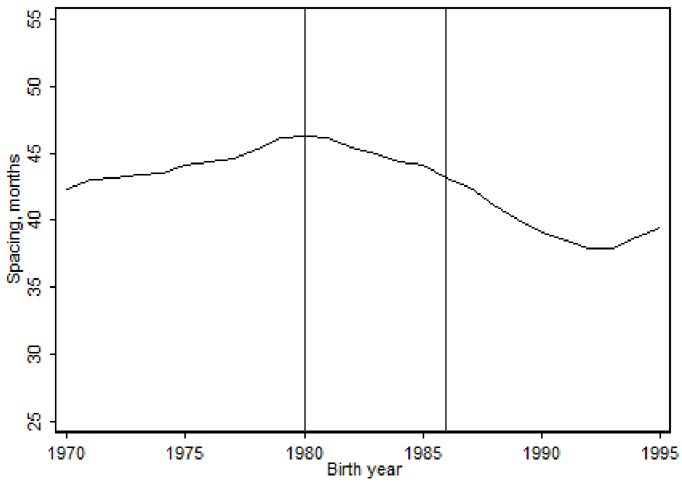

FIGURE 2. Average number of months between the birth of the first and second child by (second) birth cohort. The two vertical lines represent the introduction of the 'speed premium' and the extension of the eligibility interval from 24 to 30 months, respectively.

The theoretical literature on the impacts of postponing motherhood provides a stronger case for delayed motherhood than for early motherhood in terms of benefits to lifetime earnings. However, there are also cases where there are benefits to early motherhood. For example, in a Ben-Porath style model where agents choose between investment in human capital and market work, individuals choose to invest early because the wages are lower when human capital is low. Provided that women are allowed to continue to invest during childbearing periods, this motivates early childbearing since the foregone wages are lower early in the career (Buckles 2005). Moreover, Miller (2011) proposes a case in which wage growth does not depend on experience, or where there are returns to age that are independent of experience. If wages increased at a faster rate than discounting, lifetime earnings would benefit from early childbearing when wages are low. Empirical evidence suggesting that early childbearing leaves mothers better off can be found in Hotz et al. (1997b, 2005), although the focus in these papers are on teenage births.

The bulk of the empirical evidence, however, suggests that delayed childbearing have positive impacts on women's labor market outcomes. A theoretical rationale for benefits to delayed motherhood can be found in models of fertility behavior. In Happel et al. (1984), for instance, women determine the optimal timing of birth by maximizing lifetime earnings in a model where child rearing entails a work interruption of fixed duration, during which human capital depreciates. The amount of experience accumulated before marriage matters in determining to what extent skills decay. When the rate of human capital depreciation and pre-birth human capital levels are high, women will be relatively more likely to delay childbearing. Moreover, Miller (2011) proposes a case where there is a fixed cost of motherhood, either a motherhood penalty or depreciation of human capital during interruptions. In this case, lifetime earnings increase with delay since later mothers work more years on the un-depreciated wage profile. In addition, if wages grow at a slower rate post-birth (a flattening of the wage profile), e.g. due to reduced opportunities for on-the-job training and advancement, mothers who delay childbearing will receive higher earnings (Miller 2011).

In **Paper 1**, I empirically address the effect of first birth timing on labor market outcomes for women in Sweden. The research question posed is whether the career point at which a female worker has her first child affects her income and wages over the life cycle. I estimate the effect of motherhood delay for Swedish women who first finished college, entered the labor market and subsequently became mothers, and study whether the number of years of labor market experience pre-birth matters for labor market outcomes. The underlying challenge in estimating the causal relationship is that fertility timing is not likely to be orthogonal to unobserved variables that determine the outcome. To address this potential endogeneity problem, I follow Hotz et al. (1997b, 2005) and Miller (2011) in exploiting the exogenous variation in birth timing induced by miscarriage before first birth.

My findings suggest that when endogeneity is not taken into account, motherhood delay is positively associated with earnings and wages. However, instrumenting for first birth timing, I find that postponing motherhood has a significantly negative effect on both income and wage rates. This result is in stark contrast to previous studies, where generally a benefit to motherhood delay has been found.

One possible explanation for the negative effects may be found in how motherhood delay affects the timing of subsequent births. Specifically, I find that motherhood delay does not affect the total number of children born to a woman, but instead accelerates the time to next birth. This could imply a longer duration of home time after first birth, potentially during a critical period of career build-up. For instance, as hypothesized by Gustafsson et al. (2002), if a second child is born shortly after the first child, mothers may view the childbearing events as one spell with two births rather than two separate childbearing events, and thus return to work only after the second child is born. Tentative support for this hypothesis is provided by the average participation rates over the life cycles of women with different child spacing, shown in Figure 3. The graph shows that women with short intervals between first and second birth are less likely to participate in the labor market between births, and have a permanently lower participation rate, on average, after second birth. In fact, additional findings in Paper 1 suggest that postponing first birth increases parental leave usage in the immediate years after first birth and hence that the duration of home time after first birth is potentially extended by postponing first birth. A lengthy career interruption, in turn, may have more adverse consequences for one's career than two interruptions spread out over a longer horizon of working life. Thus, important to keep in mind is that the earnings effects estimated in this paper measure the total impact of delay, including effects of delay on experience. It is possible that if the duration of home time could be taken into account, a direct effect of motherhood delay would be zero or even positive.

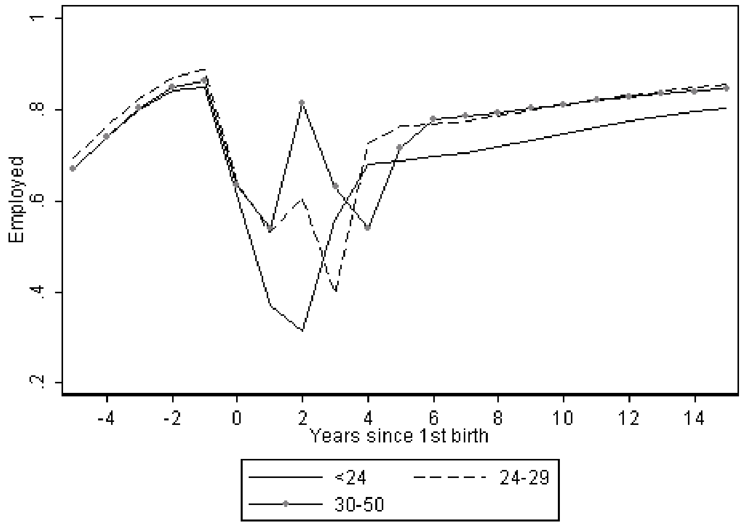

FIGURE 3. Employment status by years since first birth for women with less than 24 months between the first and second child, women with 24-29 months between the first and second child, and with 30-50 months between the first two children, respectively.

As the spacing of births was found to be shortened as a result of postponing first birth, and a shortened birth interval hypothesized to affect the duration of time off after first birth, **Paper 2** in the thesis aims at estimating the causal relationship between spacing births and subsequent income and participation. To address the endogeneity of the timing of births, birth spacing is also here instrumented by the occurrence of miscarriage; having a miscarriage between the first two live births (exogenously) increases the time interval between the first two children. Using this source of variation in birth spacing, my findings suggest that spacing births has a positive effect on the probability to re-enter the labor market between births, and leads to a permanently higher probability to participate in the labor market even 15 years after birth. The impacts on labor income after second birth are also positive and large in magnitude. Furthermore, also long-run wage rates are positively affected, with a more pronounced effect for highly educated mothers. Part of the large effects are driven by a reduced number of children, but completed fertility is not the main driving source of the effects on income, participation and wages. A more likely explanation is that spacing births implies that an otherwise long duration of home time is avoided. In turn, a lengthy interruption could have negative consequences on women's career opportunities.

The findings from both Papers 1 and 2 are policy relevant for at least two reasons. First, fertility behavior - including first birth timing and birth spacing - has been shown to be adjustable to changes in the parental leave system (see e.g. Lalive and Zweimüller 2009, Björklund 2006, Hoem 1993). Some of these reforms were not intended to speed up further childbearing or to increase the age at first birth, which highlights the possibility of unintended effects of policies. Thus, it is important to understand the relevance of such factors for labor market outcomes. Second, the spacing of births has been proposed to affect children's outcomes and can thus be viewed as an input into child quality (Rosenzweig 1986). The medical literature provides evidence of associating both very short and very long birth intervals with adverse consequences for infant health (Buckles and Munnich 2012). If spacing births affect outcomes beyond the health of mothers and infants, e.g. mothers' labor income, that is - the household's financial resources - this could imply additional channels through which spacing births could affect children's outcomes.

3. Family Friendly Policies

In the OECD report "Babies and Bosses", family policies are described as policies that "...facilitate the reconciliation of work and family by ensuring the adequacy of family resources, enhance child development, facilitate parental choice about work and care, and promote gender equality of employment opportunities" (OECD 2007). Thus, the goals of family policies go beyond achieving gender equity. Effective family policies can potentially have beneficial effects on family welfare, fertility, child development, and gender equality in the labor market. Public policies aimed at reducing the barriers to the combination of market work and family have gained increasing salience in the last few decades, and to date, nearly all OECD countries offer governmentally funded paid parental leave policies.

The Nordic countries have for a long time provided generous parental leave systems with job protection and benefits that are conditioned on employment before leave. This has likely contributed to the high female labor force participation rates observed in the Nordic countries (see e.g. Waldfogel 1998, Jaumotte 2003, Baker and Milligan 2008, Han et al. 2009, Ruhm 1998). Women who have access to leave are, all else equal, more likely to return to their previous employer after childbirth and thus to maintain their job-match (Waldfogel 1998).[7] At the same time, however, there is an ongoing debate about whether too generous parental leave systems discourage women's participation in the labor market on the intensive margin (see e.g. Gupta and Smith 2002). Albrecht et al. (2003) hypothesize that the entitlement to generous parental leave durations in Sweden, coupled with the fact that women stand for the majority of take-up, creates room for statistical discrimination against women. Thus, from the literature, it is possible to trace out a difference between introducing parental leave

[7] On the other hand, many studies find limited effects of expanding paid leave on employment and wages. For example, Klerman and Leibowitz (1999) and Baum (2003) find only weak effects on employment and wages in the United States. Schönberg and Ludsteck (2007) study the causal effects of successive changes in parental leave duration on employment and earnings in Germany and find that expansions of leave coverage induced women to delay their return to work. However, the expansions had little effect on women's labor supply in the long run. Similarly, Albrecht et al. (1999) find that time off for formal parental leave is not associated with a wage penalty for women in Sweden.

and expanding already existing systems. This is highlighted in a recent paper by Dahl et al. (2013), where the case for paid maternity leave is evaluated in the context of Norway. The authors stress the importance of a distinction between introducing parental leave and continually expanding entitlements to paid leave. Studying impacts of expanding paid leave in Norway, they find that mothers decrease their labor supply and hence that parental leave does not crowd out unpaid leave. However, they find no effects on children's schooling outcomes, parental earnings or participation in the long run, completed fertility, marriage or divorce.

Thus, a valid question to be raised, related to the effects of expanding an already generous system, is whether policies actually have the ability to affect individuals' labor market behavior. Consider, for instance, the Swedish system where paid leave is granted during 450 days for each child, with job protection exceeding this duration. In a system characterized by a longer duration of job protection than paid leave, it is not obvious that an increase in paid leave would alter individuals' labor supply. For instance, paid leave could crowd out unpaid leave. In terms of policy implications, crowding out of unpaid leave would imply a pure transfer of benefits to families with young children. Thus, when studying the impacts of changes in paid parental leave, it might not be sufficient to study effects on the take-up of parental leave benefits in order to draw inference on the impacts of such changes on the time spent at home with children. In **Paper 5**, (co-authored with Erica Lindahl and Peter Skogman Thoursie) therefore, we study how changes in paid leave entitlement affect parents' labor market behavior, recognizing that parental leave benefit take-up might not fully reflect time off from work. We study a reform that increased entitlement to paid leave by three months, and two reforms that each introduced one month of ear-marked leave to mothers and fathers, respectively. Eligibility to the new rules in all three cases varied discontinuously with children's birth dates, creating natural experiments that allow us to estimate the causal effects of the changes in paid leave. We find that the general expansion of paid leave entitlement by three months increased mothers' take-up of parental leave. Also fathers increased their parental leave days as a response to this reform. We find corresponding decreases in months worked for both mothers and fathers. Hence, paid leave does not seem to have crowded out unpaid leave. However, the additional benefits were spread out over an 8-year horizon, suggesting that the additional paid leave was used to increase job flexibility; the consecutive leave in connection with child birth was unaltered.[8] The introduction of the two "daddy-months" increased fathers' parental leave days, and the first daddy-month reform also decreased mothers' parental leave. However, we do not find any effects on either parents' months worked, earnings or wage rates.

From a policy perspective, our findings have a couple of interesting implications. First, our results suggest that, among parents who are eligible for wage-replaced parental leave, the household's financial constraint may not be binding regarding the amount of leave taken in direct connection with childbirth. Parents seem to use additional benefits to essentially buy job flexibility over a long time horizon. Thus, in a system with job protection that exceeds the duration of paid leave, and with a great portion of flexibility as to how and when to use the parental leave, it is not obvious that the time spent with very young children is affected

[8] The Swedish parental leave system allows parents to use the entitled parental leave days until the child turns eight years old. Hence, parental leave days can be saved and used for occasional days off from work.

by additional paid leave entitlement. Moreover, as we - in line with earlier literature - find no or small effects on long-run outcomes such as income, wages, or fertility, it seems unlikely that the gained flexibility further increased the opportunity to combine work and family.

4. Social Preferences and Peer Effects

One of the most fundamental assumptions in neoclassical economics is that individuals may be expected to behave independently in maximizing their well-being. However, it is reasonable to assume that individuals also consider peers' behavior in their decision making. For example, in laboratory experiments, participants have often been shown to choose actions that do not maximize their own monetary payoffs when those actions affect others' payoffs. Such seemingly non self-interested behavior has been formally modeled as social preferences. These models assume that individuals act out of self-interest, but are also concerned about the payoffs of others, that is, the payoffs of others enter into the individual's own utility function (Croson and Gneezy 2009, Charness and Rabin 2002). Economists have modeled these social preferences in the form of altruism, inequality aversion, or reciprocity (Croson and Gneezy 2009), and a number of studies have empirically established the importance of social preferences in the workplace for worker productivity (see e.g. Bandiera et al. 2005; 2010, Mas and Moretti 2009).

Moreover, recent advances in the economics experimental literature has documented that there are gender differences along various dimensions of social preferences and psychological attributes. For example, empirical evidence suggests that women are, compared to men, more averse to risk and competition, and more other-regarding and reciprocal (see e.g. Bertrand 2011 or Croson and Gneezy 2009, for reviews of the literature). Differences in psychological traits and social mindedness are often hypothesized to explain observed gender differences in consumption and investment behavior, as well as differences in the labor market. However, the empirical evidence on disparities in attributes and social preferences between the genders is most often based on laboratory experiments. It is still largely an open question whether evidence from the lab generalizes to economic behavior in real markets (Bertrand 2011).

In **Paper 3**, (joint with Per Johansson and J Peter Nilsson) we contribute to the laboratory evidence on gender differences in social preferences by studying the extent to which social incentives determine productivity behavior of male and female workers. In the words of List and Rasul (2011), our analysis falls into the category of field experiments that "...take insight from laboratory experiments to show the importance of non-standard preferences or behaviors in real world settings". To this end, we exploit a setting in which peer effects are informative of social preferences to study whether there are differences in social preferences between the genders in determining shirking behavior. Specifically, we study whether the responsiveness to peers in individual shirking behavior differs between male and female workers, and whether individuals are influenced to the same extent by co-workers of their own gender as by those of the opposite sex. The latter analysis is done to test whether women's social preferences are more situationally specific than those of men, as is sometimes suggested in the literature (e.g. Croson and Gneezy 2009). By analyzing whether female workers respond differently to different types of peers, and whether the same pattern of behavior can be found among male workers, we can study whether women's social preferences are more malleable.

We use exogenous variation in co-workers' absence induced by a large scale social experiment that altered the incentives for short term sickness absence for nearly half of all workers in Gothenburg, the second largest city in Sweden. The experiment increased the monitoring-free period of absence from 7 to 14 days for treated workers, which were randomly assigned, whereas the control group faced the usual restriction of 7 days of non-monitored absence.

Our findings suggest that male workers increase their absence almost twice as much as female workers when monitoring decreases. Women's shirking behavior, on the other hand, seems slightly more responsive to peers compared to that of men's shirking. Interestingly, however, we find that men are only affected by their male peers, and women are only affected by their female peers. Decomposing the effect of the fraction treated peers into fractions of male and female treated peers shows that there is no significant difference between the effect of peers on male and female workers' absence. Instead, the entire peer effect among men is driven by the effect of treated male co-workers and vice versa for women. These results hold true even as we control for the fraction of women at the workplace, industry affiliation, as well as dummies taking into account both the field and level of education. Hence, the stronger influence of same-sex co-workers cannot be explained by gender-segregated workplaces. Our results reflect the influence that (fe)male co-workers have on each other conditional on the potential exposure to same-sex colleagues. These findings cast some doubt on the hypothesis that women's social preferences are more malleable: both male and female workers care about their social context when context is defined by worker similarity. Thus, women's decision do not seem to be more situationally specific than men's in our setting.

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PAPER 1

The Effect of Fertility Timing on Career Outcomes - Evidence from Biological Fertility Shocks

Arizo Karimi

ABSTRACT This paper analyzes the causal effect of the timing of first birth on highly educated women's career outcomes. To address the endogeneity of birth timing to labor market outcomes, I instrument the former with the occurrence of pregnancy loss before first birth. Data on miscarriages are provided by hospital registers, which I match with individual level registers on income, wages, parental leave usage and subsequent fertility. The results from OLS estimation suggest that a one-year delay of motherhood is positively associated with income and wages. However, 2SLS estimation instead indicate that a one-year delay has a significantly negative effect on both income and wages. The negative effects might partly be explained by child spacing; motherhood delay induces women to have the second child more closely spaced (but not fewer or more children altogether), and consequently to have a potentially longer consecutive parental leave. The same findings hold true when I employ an individual-fixed effects estimator based on panel data and no instrument, from which the results suggest a larger slope decline in the wage profile post birth for "late" mothers compared to "earlier" mothers.

1. Introduction

There is a vast economics literature that addresses the questions of how fertility is related to women's labor supply, income and wages. It is well established that childbearing reduces women's subsequent labor supply and income (see e.g. Bronars and Grogger 1994, Angrist and Evans 1998, Jacobsen et al. 1999, Vere 2011). Career interruptions due to childbearing also have the potential to affect women's subsequent wage rates, through the foregone investments in human capital and possible skill depreciation while out of the labor market. Moreover, upon returning to work, mothers may experience a flatter wage-profile. Such an effect can have both supply- and demand-side explanations. On the supply-side, mothers

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may exert less effort at work or reduce working hours and, on the demand-side, employers might offer mothers fewer opportunities for on-the-job training and advancement. Thus, work interruptions - and the career costs that they entail - constitute a major component of the opportunity costs of children. Recently, researchers have devoted increasing attention towards the question of whether the timing of parenthood can affect the magnitude of such costs. Partly, this interest has been spurred from the empirical observation that the age at first birth is positively associated with various labor market outcomes (see e.g. Chandler et al. 1994, Hofferth 1984). In addition, many industrialized countries have observed an increase in the age at first birth[1] while simultaneously witnessing improvements in women's labor force participation rates and earnings. Postponing motherhood may reduce the career costs of children as later interruptions imply that the foregone investments are accrued over a shorter time horizon (Miller 2011). In addition, the accumulation of pre-birth work experience may protect mothers from having to start over upon returning to the labor market, as workers with more experience may be better able to protect their human capital from atrophy. However, postponing first birth can potentially affect also other variables which, in turn, act as intermediaries for an effect of birth timing on subsequent labor market outcomes. Such potential mechanisms include the number of children as well as the tempo of subsequent fertility, both of which may be of importance for lifetime earnings and wage growth. Thus, to gauge the total effect of first birth timing on earnings, it is potentially important to take into consideration the effects of birth timing on intermediate variables.

The aim of this paper is to estimate the causal impact of first birth timing on the income and wage rates over the careers of highly educated women - the group most often observed to postpone motherhood. In addition, I estimate the effect of first birth timing on parental leave usage, the total number of children born to a woman as well as on the time interval to the subsequent birth, all of which are likely to be important determinants for long-run labor market outcomes. It is difficult, however, to capture the causal effect of the timing of fertility on female labor market outcomes. The underlying challenge is the endogeneity of the former with respect to the latter. One possible source of endogeneity in this context is that individuals are likely to exhibit unobserved heterogeneity in tastes or motivation that affect both fertility and career choices. In addition, it is not unlikely that fertility timing choices respond to anticipated career outcomes, or that choices about fertility and careers are jointly determined. I exploit a source of exogenous variation in first birth timing resulting from miscarriage before first birth (Miller 2011, Bratti and Cavalli 2013, Hotz et al. 1997, 2005). Pregnancy losses are naturally occurring fertility shocks that delay time to birth and can thus be used as an instrument for first birth timing. As an alternative empirical strategy, I also employ an individual-fixed effects estimator using panel data. The effect of

[1] For instance, the average age at first birth in Sweden increased from 24 in 1970 to 29 in 2007

first birth timing is thus estimated under two different sets of assumptions and the analyses provided allow comparisons of estimates relying on different sources of variation. To this end, I use a combination of different Swedish registers with individual level information on income, wage rates, background characteristics, parental leave usage and sickness absence. Data on miscarriages are provided by hospital registers, which include detailed information about medical diagnoses classified according to the International Classification Standard for Diseases (ICD).

The results obtained from the OLS estimator suggest a positive relationship between the timing of first birth and the net present value of women's earnings over the first 20 years of the career of about 4 percent, and on the average wage rate over the same time period of about 2 percent. However, when instrumenting first birth timing with the occurrence of miscarriage, I find that postponing motherhood has a significantly negative effect on career earnings and average career wage rates of about 15 and 5 percent, respectively. Estimating the effect of postponing first birth on the *yearly* income post birth shows that the earnings drop is apparent in the first four years after birth. Attempting to shed some light on the potential mechanisms driving these effects, I study the effect of a one-year motherhood delay on completed fertility and child spacing. The results from this analysis show that postponing first birth does not affect the total number of children, but instead accelerates the time to the next birth. This could imply being absent from the labor market for a longer portion of a potentially critical period of career build-up. Analyses of the parental-leave usage response indeed suggests that, as first birth is delayed, parental leave usage increases in the immediate years following birth, the time pattern of which is consistent with the time pattern shown on the effects of subsequent fertility. The latter shows an increase in the probability to give birth to a subsequent child two years after first birth, while showing negative effects on the probability to give birth to a subsequent child in later years following first birth. Corroborating the results from the instrumental variables analysis, an individual-fixed effects estimator suggests that the slope decline in wages post birth is larger for 'late' mothers compared to 'earlier' mothers.

My finding that motherhood postponement has negative effects on women's labor market outcomes is in stark contrast to results obtained in existing studies on the topic. For instance, based on panel data, Taniguchi (1999) finds that compared to women without children, women with first births at age 28 or older face no wage penalty, while women with first births at ages 20 to 27 experience a 4 percent wage penalty. Similarly, Amuedo-Dorantes and Kimmel (2005), focusing on college educated women, find that mothers whose first child was born beyond the age of 30 have 13 percent higher wages. Wilde et al. (2010) study the pattern of mothers' wage trajectories before and after first birth and distinguish between low- and high-skilled women. Their findings show that wages diverge after first birth and that early childbearing is more costly for highly skilled women. Troske and Voicu (2012) analyze the effects of the timing and spacing of births using a multinomial probit model for different employment states and for fertility decisions and find that delaying first birth leads to higher pre-natal labor market involvement and reduces the negative effect of the first child on the labor supply of married women. Using a dynamic treatment approach, Fitzenberger et al. (2013) study the effect of having a first child at a certain age against the alternative of delaying childbearing at that age on subsequent employment. They find large and persistent negative effects of first childbirth on employment. However, their results do not lend support to the hypothesis that delaying childbirth reduces the negative employment effects. By using the occurrence of miscarriage and other fertility shocks as exogenous variation in birth timing Miller (2011) finds that a one-year delay of motherhood increases earnings by 10 percent and leads to 2.6 percent higher wages by age 34. In a similar fashion, Bratti and Cavalli (2013) estimate the impact of delaying first birth on Italian mothers' labor market outcomes just around birth using miscarriages and still birth as instruments for fertility timing. They find that postponing motherhood increases the likelihood to participate in the labor market by 1.2 percent.

Related to the research on the effect of the age at first birth on female earnings and wages is the literature on the effects of teenage childbearing. This literature has produced mixed results, however. Studying a panel of Swedish sisters, Holmlund (2005) finds modest effects of teenage childbearing on educational attainment once pre-birth educational performance has been taken into account. Ribar (1994) uses age at menarche and the local abortion rate and ob-gyn availability in the NLSY and find positive effects of early childbearing on educational attainment. In contrast, Klepinger et al. (1999) use menarche combined with county level instruments in the NLSY and find large negative effects of early childbearing. Hotz et al. (1997, 2005) exploit miscarriage as a natural experiment to study the effect of teenage childbearing in the United States and find that women who have births as teens have higher labor market earnings and hours worked compared to what they would have attained if their childbearing had been delayed, and that for most outcomes, the adverse consequences of teenage childbearing are short-lived. Ashcraft et al. (2013) note that estimates of teenage childbearing using miscarriage as an instrument for birth timing are biased towards a benign view. The reason is that teens who choose to abort are positively selected among teens who become pregnant. Moreover, teens who would choose to abort are less likely than others to miscarry. Accounting for this source of bias, they still find only modest adverse effects of teenage motherhood on mothers' adult outcomes.

While using the same strategy employed by Miller (2011), the present paper contributes to the existing findings in several ways. First, most of the previous evidence is based on data from the United States. Sweden is an interesting case to study in the context since the institutional setting differs considerably from that of the US. Family policies in Sweden are universal and generous, and job-protected parental leave with wage replacement is given to all parents. Thus, while postponing fertility may be important in the US to, for instance, gain a suitable job-match that allows a non-disruptive career with childbearing, it is most likely not a strategy needed in Sweden where job-protected leave is the default. Secondly, I draw information on miscarriages from the National Patient Register (NPR) which records the universe of all hospitalizations in Sweden, with information on medical diagnosis associated with each visit. One advantage with using the NPR over relying on survey data - which has been the main type of data source used in previous studies - is that I avoid potential misreporting of abortions as miscarriage, which might be likely considering social stigmas associated with abortions. The data also allows me to more closely investigate the validity of the instrument by estimating pre-natal health differences, including potential risk factors associated with miscarriage, between mothers who miscarry and mothers who don't. Third, recognizing that an impact of first birth timing on career outcomes may be partly mediated through its impacts on completed fertility and spacing of subsequent children, my analysis also provides estimates of the effect of first birth timing on these intermediate fertility variables, as well as on parental leave length. Fourth, the data set on which the analysis is based enables me to follow mothers for up to 20 years after entering the labor market, and thus allows me to estimate long run impacts of motherhood delay on earnings and wages. Lastly, I measure first birth timing as the number of years elapsed between labor market entry and first birth - as opposed to the age at first birth. This implies that fertility timing here can be thought of as a more direct measure of potential (pre-natal) experience, compared to experience being proxied by age. As suggested by Herr (2011), this definition of birth timing might be more appropriate than age at first birth, based on her findings that the latter tends to underestimate the return to motherhood delay for women who remain childless at labor market entry, and obscure the negative return to delay to a first birth after labor market entry for all but college graduates. I thus focus on women who enter the labor market before having children since my timing variable can only measure potential experience for individuals who have some pre-birth labor market experience.

From a policy point of view, the question of whether the timing of parenthood matters for labor market outcomes is particularly interesting because the age at first birth has been observed to be responsive to policy changes. For instance, Björklund (2006) reports that the family policies introduced in Sweden between 1960 and 1980, in which benefits were tied to previous labor earnings, increased women's age at first birth. In an overview on the effects of family policies in industrialized countries, Gauthier (2007) reports that some studies suggest that the effect of policies tend to be on the timing of births rather than on completed fertility.

Thus, family policies may have unintended consequences for the timing of fertility, making it relevant to understand the impacts of fertility timing on labor market outcomes.

2. Identification Strategy

The objective of this paper is to estimate the effect of first birth timing on women's career outcomes. Setting up the problem in a potential-outcomes framework, let Y denote the labor market outcome of interest and let T denote first birth timing, measured as the number of years elapsed between labor market entry and the birth year of the first child (i.e., pre-birth labor market experience). We are worried that the regressor of interest, T, might be endogenous to labor market outcomes, Y, due to unobserved heterogeneity in tastes or motivation that affects both fertility and work choices. To address the potential endogeneity issues, I make use of the exogenous source of variation in first birth timing induced by the event of miscarriage before first birth, the incidence of which extends time to motherhood (Miller 2011, Hotz et al. 1997, 2005). Let the binary variable Z indicate first pregnancy ending in miscarriage. Then, let T_1 denote the first birth timing for an individual with $Z = 1$ and let T_0 denote the timing for an individual with $Z = 0$. Moreover, we can consider T a treatment with variable treatment intensity, taking on the values $j = 0, 1, 2, ..., J$. Suppose that each individual would earn Y_j if she waited j years between entering the labor market and entering motherhood, for $j = 0, 1, 2, ..., J$. While a full set of Y_j is well defined for each individual, only one is ever observed. The goal is to attain information about the distribution of $Y_j - Y_{j-1}$, which is the causal effect of the first career interruption due to childbearing occurring in the *j*:th year.

For Z to be a valid instrument, the first identifying assumption that needs to hold is that Z is independent of all potential outcomes and potential treatment intensities, i.e., that $T_0, T_1, Y_1, ...Y_J$ are jointly independent of Z. Independence alone is not always sufficient to estimate a meaningful average treatment effect, since it is theoretically possible to have a situation where the treatment effect is positive for everyone, but the sizes of the groups of compliers and defiers are such that the average difference in outcomes is zero or even negative. To get around this problem, a second identifying assumption needed is monotonicity: With probability 1, $T_1 - T_0 \geq 0$ for each person. Given independence, monotonicity and the assumption that $Pr(T_1 \geq j > T_0$ (there exists a First-stage relationship) for at least one J, Angrist & Ibmens (1995) show that

$$LATE = \frac{E(Y|Z = 1) - E(Y|Z = 0)}{E(T|Z = 1) - E(T|Z = 0)}$$

$$= \sum_{j=1}^{J} \omega_j E[Y_j - Y_{j-1}|T_1 \geq j > T_0] \equiv \beta$$

where

$$\omega_j = \frac{P(T_1 \geq j > T_0)}{\sum_{i=1}^{J} Pr(T_1 \geq i > T_0)}$$

with $0 \leq \omega_j \leq 1$ and $\sum_{j=1}^{J} \omega_j = 1$, so that β is a weighted average of a per-unit treatment effect. Angrist and Imbens (1995) refer to β as the average causal response (ACR). This parameter captures a weighted average of causal responses to a unit change in treatment, for those whose treatment status is affected by the instrument. The weight attached to the average of $Y_j - Y_{j-1}$ is proportional to the number of people who, because of the instrument, change their treatment intensity from less than j units to j or more units. This proportion is $Pr(T_1 \geq j > T_0)$; the proportion who, by the event of experiencing a miscarriage, are induced to delay motherhood.

As shown in Angrist and Imbens (1995), for a multi-valued treatment ($J > 1$), the monotonicity assumption has the testable implication that the cumulative distribution function (CDF) of T given $Z = 1$ and the CDF of T given $Z = 0$ should not cross.[2] Although there is no direct reason to be worried that the monotonicity assumption does not hold in the case of miscarriages (there can be no defiers by construction because a miscarriage always delays births), we can plot the empirical CDF:s to gain knowledge about the weighting function of the ACR. The CDF:s for birth timing, by the value of the instruments, are graphed in the upper panel of Figure A1 in the Appendix, along with the best fitted normal model superimposed over the sample CDF. The figure shows that for mothers who experienced a miscarriage, the CDF lies below the CDF for women who did not experience a miscarriage until timing, i.e., T, equals 10. After year 10, the CDF:s cross, and the CDF for women with miscarriages lies above the CDF for women with no miscarriage. This evidence is in support of the monotonicity assumption for those mothers who wait at most 10 years after entering the labor market until they have their first child. One possible explanation is that for some women who wait a long time, miscarriages might be indicative of their trying harder to get pregnant. In the analyses, I will perform the estimations for the sub-samples of mothers with first birth timing less than 11 years.

Furthermore, the weighting function of the ACR for estimates based on comparisons between women who do and do not experience a miscarriage is the difference between the CDF:s normalized to sum to one. This difference is plotted in the lower panel of Figure A1 and shows that the group contributing most to the estimates of the ACR based on the event of miscarriage are those with 2-3 years elapsed between entering the labor market and having a first child. At most 9 percent of the sample was induced by having a miscarriage to have

[2] If $T_1 \geq T_0$ with probability 1, then $Pr(T_1 \geq j) \geq Pr(T_0 \geq j)$ for all j, which implies $Pr(T \geq j|Z = 1) \geq Pr(T \geq j|Z = 0)$ or $F_T(j|Z = 1) \geq F_T(j|Z = 0)$, where F_T is the CDF of T (Angrist & Imbens 1995).

their first child in career year 3, but smaller fractions were induced to have their first child at later career points.

2.1. Threats to Identification. While the existence of a First-stage relationship can be directly addressed, the independence condition cannot be formally tested. One potential concern with instrumenting birth timing with miscarriage is that the health of mothers who miscarry is, on average, worse compared to women who do not. These health limitations in turn would lead women to have lower wages. Another concern is that miscarriages might cause psychological distress and therefore directly affect labor market outcomes, violating the exclusion restriction. This critique against using miscarriage as a source of variation in birth timing is lifted by e.g. Wilde et al. (2010), who in addition also worry that behavioral characteristics differ between women who miscarry and women who do not. For instance, some evidence suggest that miscarriage risk is associated with risky behaviors such as regular or high alcohol consumption, tobacco or drug use during pregnancy (see e.g. Garcia-Enguidanos et al. 2002, Maconochie et al. 2007, for overviews of the medical literature). Garcia-Enguidanos et al. (2002), however, argue that while many risk factors have been suggested in the medical literature, there are only two factors recognized by "all" studies, which are uterine malformations and chromosomal rearrangements. Moreover, miscarriage is a frequently occurring fertility shock; Regan and Rai (2000) review the medical literature and state that sporadic miscarriage is the most common complication of pregnancy, and one in four of all women who become pregnant will experience pregnancy loss. Moreover, the vast majority of pregnancy losses are early, occurring well before 12 weeks of gestation, with sporadic miscarriage after this time complicating no more than 1-2 percent of pregnancies (Regan & Rai 2000).

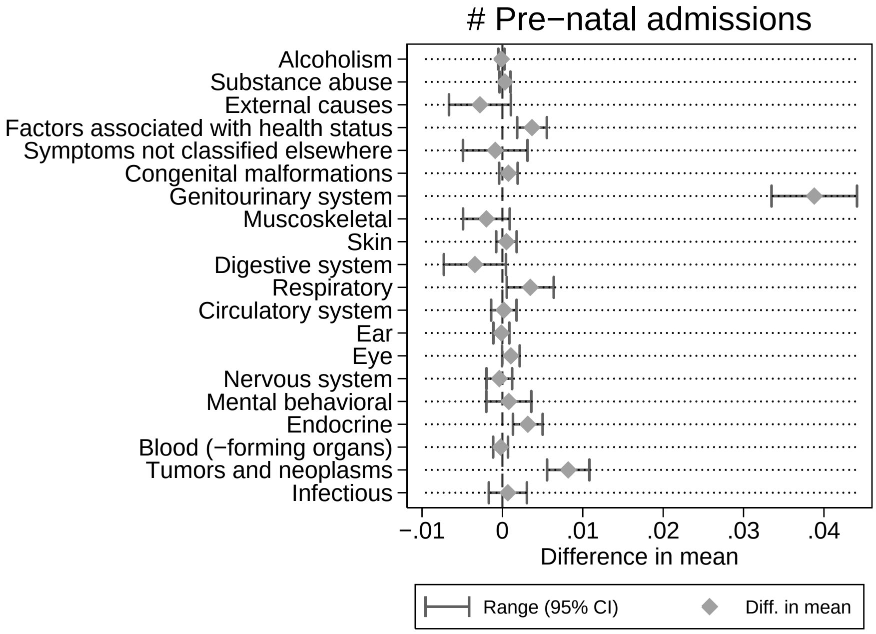
To investigate whether there are health differences between women who miscarry and women who do not in my sample, I make use of detailed individual level data from the National Patient Register (NPR), which covers the universe of all hospitalizations in Sweden between 1987 and 2005. The NPR is an inpatient care record that includes medical information in the form of the diagnosis associated with each hospital visit, classified according to the International Classification Standard for Diseases (ICD).[3] Using these data, I study whether there are any differences in pre-motherhood incidence of hospitalizations between mothers with $Z = 1$ and mothers with $Z = 0$, i.e., between mothers who did and did not experience a miscarriage before first birth. The results from this analysis are presented in Figure 1.1, where the upper graph plots the differences in the average number of hospital visits for different diagnoses during a time period of 4 years before first birth (birth year -4 to birth year -1) with the corresponding 95 percent confidence intervals. A first thing to note

[3] The NPR is also the data source I use for identifying miscarriage events; a more detailed description of the data follows in the Data section.

is that there are, if any, very small differences in the frequency of hospitalizations between women who miscarry and women who don't for any of the diagnoses; in fact, most estimates lie on the vertical zero line. Nevertheless, some diagnoses are shown to be more prevalent among women who miscarried. For instance, women who experienced a miscarriage had somewhat higher frequency of pre-natal hospital visits due tumors and neoplasm diseases and respiratory and endocrine diseases. These differences are, however, very small. The largest difference is found in the average number of hospital visits associated with diseases of the genitourinary system, which are more prevalent among women who later experienced a miscarriage. Importantly, however, there are no indications of differences between the groups concerning hospitalizations associated with risky behaviors such as alcohol or substance abuse.

The lower graph in Figure 1.1 is analogous to the upper graph, but presents differences in average personal characteristics as well as in the first birth timing. As seen, women who experienced a miscarriage had a delayed childbearing by on average 6 months (this is the "raw" first-stage estimate). There are hardly any differences in marital status at the time of labor market entrance, but women who miscarried are somewhat less likely to have been born outside the Nordic countries. The largest difference lies in the age at labor market entry; women who experienced pregnancy loss were, on average, 0.74 years older when they entered the labor market. One possible explanation for this difference is that fecundity is declining with age, and women who enter the labor market at older ages are also somewhat older at the time of first pregnancy attempt. It is important therefore, to control for the age at labor market entrance. In all the analyses I also control for the number of pre-natal hospitalizations, including the number of pre-natal hospital visits associated with each of the diagnoses depicted in Figure 1.1, as well as controls for personal characteristics.

Since hospitalizations reflect the most severe health issues, one might still worry that there are differences in health between the groups that are not captured by differences in hospitalizations. To get a less crude estimate of the differences in average health between the two groups, I therefore also examine the difference in health-related work absence. For this purpose, I use individual level data on sickness absence days from the Social Insurance Agency. Figure 1.2 plots the residuals from an Ordinary Least Squares (OLS) regression of the number of sickness absence days per year on year-fixed effects and age dummies. The two separate lines represent women who experienced pregnancy loss before first birth (solid line) and women who did not (dashed line). The x-axis displays the time since first birth for those who did not experience pregnancy loss, and time since miscarriage for those who did, i.e., the vertical zero-line approximates the year that they would have given birth to their first child, had they not miscarried. Importantly, the trends in sickness absence before first birth and before miscarriage are very similar for the two groups, with a small difference



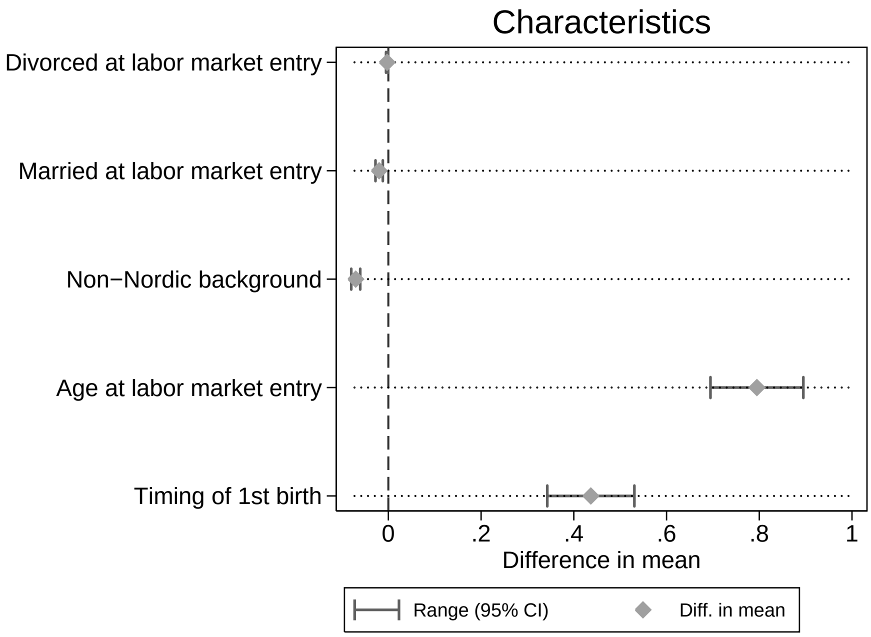


FIGURE 1.1. Differences in average number of (pre-natal) hospital admissions by diagnosis, measured as the total number of hospital visits for each diagnosis category during the five years preceding first birth. The lower graph plots differences in average characteristics. Corresponding 95-percent intervals are also plotted.

emerging a few years before birth/pregnancy loss. Moreover, the peak in sickness absence for those with no pregnancy loss reflect the increased absence associated with pregnancy. The sickness absence then decreases right after birth (which is the time period when they are on formal parental leave) to increase again and then stay rather constant.[4] For women who experienced pregnancy loss, there is an increase in sickness absence days in the year of the pregnancy loss, which likely is directly associated with the miscarriage. The high levels of sickness absence following this peak is most likely connected to childbirth, since this reflects the time period that this group actually have their (delayed) first birth. The reason for this increase seeming to be longer-run compared to women without pregnancy loss could simply be because the women in the former group have their first child at different times after pregnancy loss.

One might be worried here that the sickness absence associated with childbirth is higher for women who miscarried compared to women who did not, since the peak in sickness absence is higher for the former. In this case, the IV estimates would be biased downwards. However, Figure 1.2 also shows that the sickness absence of the two groups converge after 5-6 years after childbirth/miscarriage which tentatively suggests that miscarriages alone do not have any long-run impacts on health. In Figure 1.3 I plot parameter estimates from an OLS regression of sickness absence days on miscarriage, by years since first birth or years since the first birth would have occurred had woman i not miscarried. Included controls are pre-motherhood number of hospitalizations, and hospitalizations by diagnosis type, an indicator for non-Nordic background and the age at labor market entrance as well as the calendar year of labor market entrance. The estimates confirm the findings from Figure 1.2 and suggest that, after an initial period of higher sickness absence, the sickness absence of the two groups of women converge. This is in support of the exclusion restriction, where the concern is that mothers who experience a miscarriage might be adversely affected in that it induces psychological distress. Such a negative effect on health in turn might reduce a woman's hours worked, which would violate the exclusion restriction. Although the results presented in 1.2 do not indicate large long-run differences in sickness absence between the two groups of women, were this the case then 2SLS estimates would again be downward biased.

Lastly, an additional problem potentially inherent in using miscarriages as an instrument for birth timing was raised in a recent paper by Ashcraft et al. (2013) and may be important to keep in mind when interpreting the results in the present paper. They study the effect of

[4] This pattern of women's absenteeism by parenthood status is in line with the pattern recently documented by Angelov et al. (2013). Using Swedish data, the authors show that, before first birth, there are no differences in sickness absence between women and men. After entering parenthood, however, women increase their sickness absence by between 0.5 days per month more than their spouse, a difference which increases to 0.85 days more than the spouse in year 17 after first birth.

teenage childbearing using miscarriages as a source of exogenous variation in birth timing. However, they argue that miscarriages are not socially random in the sense that willingness to abort reduces miscarriage risk. Moreover, teens who have abortions come from less disadvantaged backgrounds than those who do not. Thus, teens who miscarry are not a random sample of pregnant teenagers, but are from a more disadvantaged background. This implies that the IV estimator underestimates the the true costs of teenage childbearing. Thus, the authors conclude that when miscarriage is used as an instrument for birth timing, the estimates are biased towards a benign view of teenage childbearing.

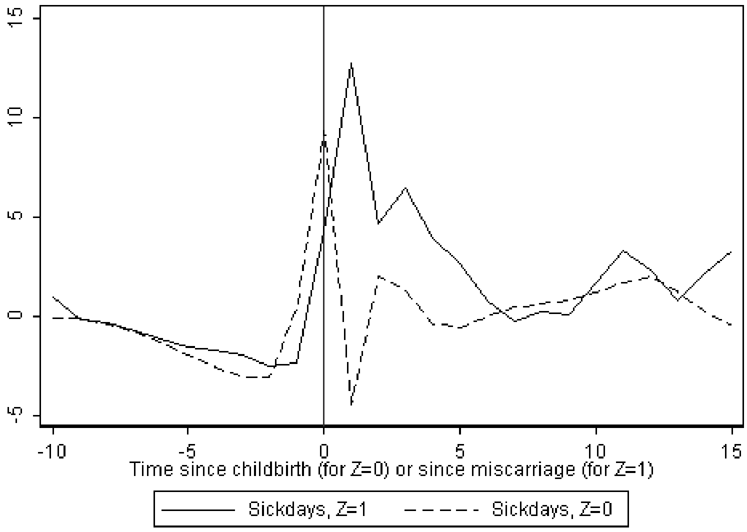


FIGURE 1.2. The residuals from an OLS regression of sickness absence days (per year) on year-fixed effects and age, separately for mothers who experienced a miscarriage before first birth and who did not. The zero-line represents time since miscarriage for women who miscarried, and time since first birth for those who did not, respectively.

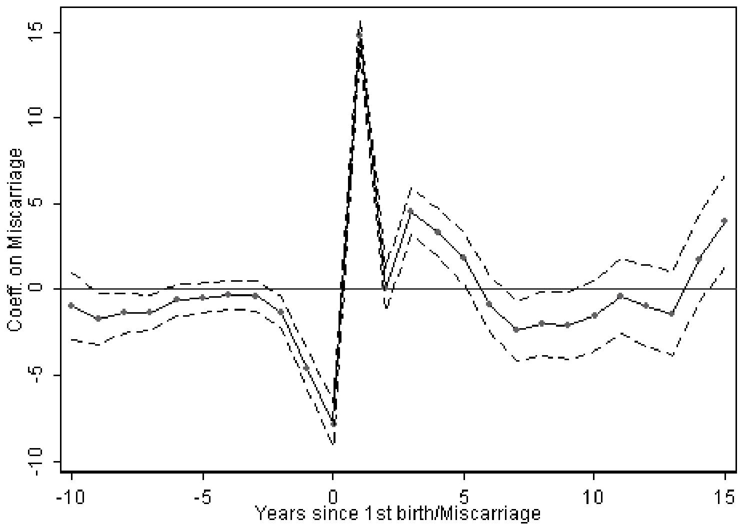


FIGURE 1.3. Parameter estimates from an OLS regression of sickness absence days (per year) on miscarriage before first birth. The x-axis represents time since first birth or time since the first birth would have occurred, had woman *i* not miscarried.

3. Data Description and Analysis Sample

3.1. Data Sources and Definitions. The data used for the analysis is created by combining several Swedish population-wide registers. First, I use the multi-generation register, which links all children to their biological parents and provides information on birth year, birth month and birth order for each of all individuals' children. To these data I match registers containing information on a set of background characteristics such as age, gender, marital status, country of origin, highest attained educational level and graduation year, along with information on annual labor earnings from tax registers. Moreover, I add variables from a linked employer-employee data set providing information on the establishment at which the individual is employed each year, the first and last calendar month in a year that the worker receives income from the specific employer, information on industry affiliation, and the total income earned from the specific employer in that year. These registers cover the entire Swedish population aged 16-64 between 1985 and 2007. I then add individual level data on full-time equivalent monthly wages for each person-year-establishment observation, obtained from the Wage Structure Statistics and available for the entire public sector and about half of the private sector firms, for the time period 1985 through 2007. I also match individual level data on parental leave usage from the Social Insurance Agency for all individuals in my sample.

Finally, individual level data on miscarriages are provided by the National Patient Register (NPR). The NPR covers the universe of all hospitalizations (inpatient care) in Sweden. It includes medical information associated with each hospital visit, classified according to the International Classification Standard for Diseases (ICD). Using the ICD-codes, I can identify all hospital visits associated with miscarriages, for the time period 1987 through 2005.[5] Since the NPR does not record the order of the pregnancy for which the miscarriage occurred, I define the instrument - which indicates whether the *first* pregnancy ended in miscarriage - as being equal to unity if a miscarriage is recorded in the NPR for an individual before the birth year of her first child. Individuals with recurring miscarriages are entirely dropped from the sample.[6]

The inpatient record contains a non-negligible number of reported miscarriages. However, the number of reported cases decreases substantially each year from 1987 to 2005. Figure A2 in the Appendix shows that the trend in reported miscarriages over time does follow the decreasing trend in the number of births in Sweden during the same time period. However, the number of miscarriages when illustrated as the fraction of births shows that the decrease is much larger than would be expected were it proportional to the decrease

[5] The ICD-10 code for miscarriage is O.03.

[6] One might worry that recurring miscarriages induce psychological distress, which in turn might influence labor market outcomes directly, thereby violating the exclusion restriction.

in the fertility rate. This is likely explained by technological change, i.e, changes in the medical treatments following a miscarriage and thereby in what type of medical establishment they are treated; recall that the NPR only includes inpatient care. Over time, it has become more common practice with medicinal treatment, as opposed to surgical treatment, following miscarriage, which do not have to be carried out in a hospital. One potential concern is then that the cases of miscarriages that actually are treated at a hospital are more severe compared to the cases where treatment is acquired at an outpatient establishment. Women experiencing miscarriages with additional medical complications might be induced to reduce their working hours due to both medical and psychological reasons, which would violate the exclusion restriction. To explore this issue, I use study the frequency of reported co-morbidities for all hospital visits due to miscarriage. This is possible because the NPR reports not only main diagnosis for each hospital visit, but when relevant, up to 7 secondary diagnoses for co-morbidities. The frequency table A1 in the Appendix shows that 95 percent of all miscarriages have no reported co-morbidities and 5 percent have 1 co-morbidity. There are very few cases in which more than one co-morbidity is reported. For individuals with a reported co-morbidity, I also tabulate the frequency by the medical causes for the first secondary diagnosis, shown in Table A2. As seen, the majority of the cases concern diseases of the genitourinary system or pregnancy-related diagnoses. However, even if there is not a high frequency of reported co-morbidities to miscarriages, one could still worry that miscarriages that require hospital care are of greater medical severity compared to cases that do not show up in the inpatient records. As an additional analysis, I can also use the fact that the NPR provides a detailed description of the type of miscarriage, which I divide into four categories: complete, with and without complications and incomplete, with and without complications. This information on miscarriage type is, however, only available between 1997 and 2005. Nevertheless, using the available data I can attain an indication of the medical severity of the reported miscarriages. Table A3 in the Appendix tabulates the occurrence of miscarriages divided into the four categories described above, among all miscarriages that are recorded in the inpatient records over the period 1997-2005. The results reported in Table A3 show that the overwhelming majority - about 87 percent - are without complications. Moreover, Figure A3 graphs the proportions of miscarriages with and without complications over the entire time period and shows that the majority of cases are without complications for all years, although the trends converge somewhat. Thus, although technological change has lead to fewer miscarriages being treated at inpatient establishments, there is no strong evidence towards only severe cases being treated at inpatient establishments as more and more cases are treated outside hospitals. Nevertheless, to control for technological change, I include year-fixed effects in all regressions. Furthermore, since I only have access to the inpatient records, an increasing number of “control” individuals will have had a miscarriage, but treated at an outpatient establishment, which implies that I will likely underestimate the effect of miscarriages on first birth timing, that is, the First-stage relationship is likely to be understated.

Since I define first-birth timing in terms of a woman's career - the time elapsed between labor market entry and the birth year of the first child - I need a clear definition of labor market entry. To identify each individual's first job and the associated starting wage, I define the "first" job as the employment that fulfills the following criteria: the first job (i) after completing the highest level of education which (ii) lasts for at least 4 months and (iii) yields annual earnings of at least 3 times the 10th percentile of the full wage distribution.[7] This definition of a first job is drawn upon the definition used in Kramarz and Nordström Skans (2013), although I use a different proxy for the minimum wage and information on graduation year and educational attainment is here obtained from a different data source. Figure A4 in the Appendix shows the time elapsed (cumulative) in order to find a first stable job for women with at most high school education and college education, respectively. For college educated women, about 60 percent find a job already in the same year as college completion and 80 percent find a job within one year of college graduation. For women with at most high school education, it takes significantly longer time to enter a stable employment: roughly 60 percent find a job within one year after high school graduation and about 80 percent find a first job within 3 years after completing high school.[8]

Annual labor income and wages are all expressed in 2008 years prices (deflated using the Consumer Price Index).

3.2. Analysis Sample. I restrict attention to women who gave birth to their first child between 1988 and 2006. This population consists of 901,940 individuals, in total, of which 33,348 were reported to have experienced a miscarriage in the inpatient register some time during 1987 through 2005, with 20,207 of which the miscarriage happened before the birth of the *first* child. Summary statistics for the full sample of mothers is provided in Table A4 in the Appendix. The focus of this paper is on highly educated mothers, which constitute 44 percent of the full sample of mothers. Furthermore, I restrict the sample to college educated women who were aged 21 or older at first birth and had their first child *after* entering the labor market. This restriction is made because, as noted by (Herr 2011), the timing variable can only measure potential experience for women who have some pre-birth labor market experience. Finally, for those who experienced a miscarriage before first birth, I require that the miscarriage occurred after labor market entry, and I exclude those women who wait more than 10 years before having their first child, due to the monotonicity assumption not being

[7] The latter is used to proxy a minimum wage as Sweden does not have a legislated minimum wage.

[8] These results are in line with findings presented in Kramarz and Nordström Skans (2013), from which the definition of a first stable job is drawn.

satisfied for these women. In order to compare the restricted sample of highly educated mothers with the full sample of college educated mothers, Table A4 also provides summary statistics for these two groups. As can be seen from Table A4, the sample restrictions leave me with a positively selected sample of highly educated women; compared to the full sample of college educated women, the women in the study sample are older when they have their first child, they have more years of pre-birth labor market experience (4.3 years compared to 1.4 years), they are younger at labor market entry, find their first stable job sooner after completing college, are less likely to be married at labor market entry and less likely to be born outside the non-Nordic countries, and are more likely to live in a large city. Thus, the studied individuals might have stronger preferences towards market work than college educated in general. In Figure 1.4 I plot the distribution of age at first birth and the distribution

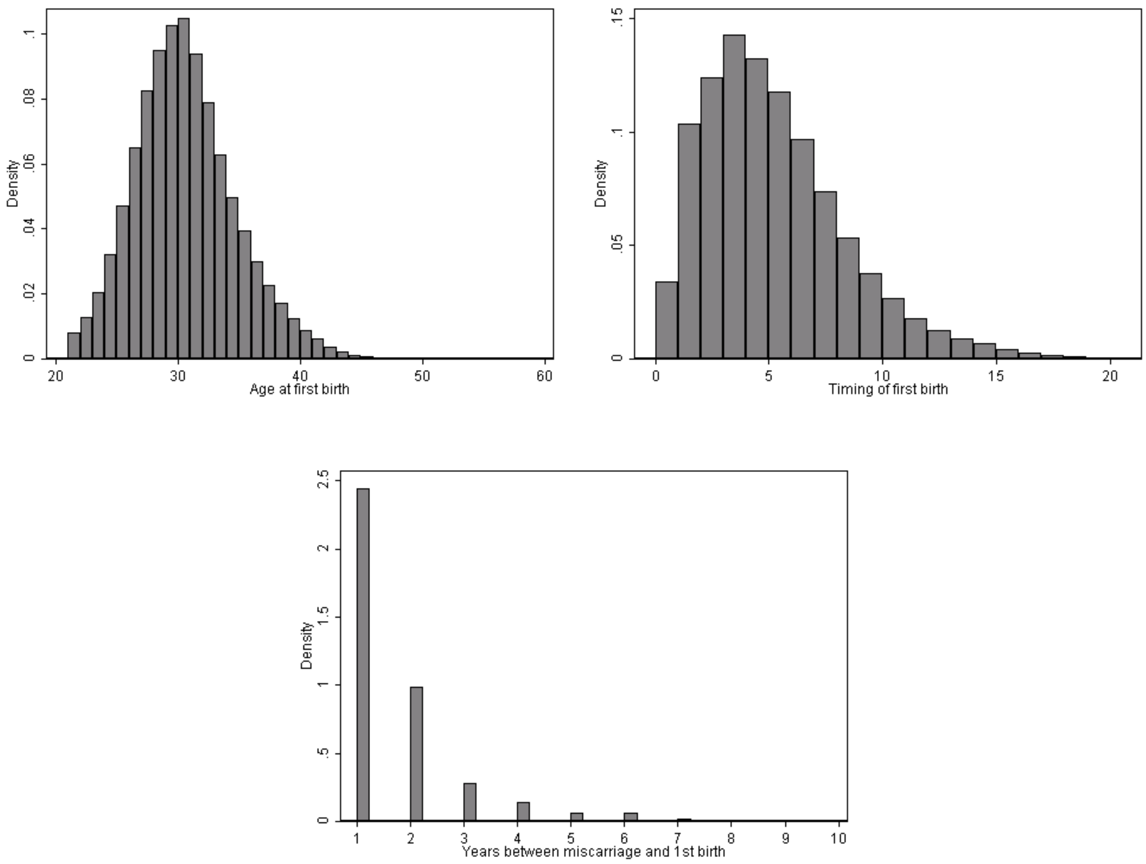


FIGURE 1.4. Distribution of Age at first birth and Timing of first birth with respect to labor market entry, and years elapsed between pregnancy loss and first birth.

of first birth timing in terms of the career for the analysis sample. The lower graph of Figure 1.4 plots the number of years elapsed between miscarriage and birth of the first child, for the women in the analysis sample that experienced a miscarriage. As seen from Figure 1.4, the overwhelming majority of women had their first child within 10 years after entering the labor market, and the overwhelming majority of women who miscarried gave birth to their first child within two years after the miscarriage.

Because I restrict the population of interest to women with at least one child, one question that arises is whether the occurrence of a miscarriage affects the probability to have a child at all, i.e., whether miscarriage affects the extensive margin of fertility. What would then be a cause for concern is whether individuals who miscarry and never become mothers differ from those who miscarry but subsequently give birth to a child. For the population of all Swedish mothers who were aged 45 or older in 2007 and who experienced a miscarriage between 1987 and 2005, Table A5 in the Appendix reports summary statistics for women who had at least one child by the age of 45 and women who remained childless at the age of 45. First, we can note that there are a few statistically significant differences in average characteristics between mothers and childless women. For example, childless women have somewhat lower family incomes in 2007, likely attributed to the lower propensity to have been married. Moreover, childless women have somewhat lower own earnings, albeit not significantly different from mothers, but are weakly significantly more likely to have had a college education, and were on average older in 2007. While it is difficult to draw any clear conclusions regarding the potential selectiveness of the group of women that are excluded from the sample, i.e., women who had a miscarriage but remained childless, they seem to be a slightly negatively selected group in terms of own and family income. This would imply that the Reduced form estimates would be positively biased. However, among women who experienced a miscarriage, very few women - only four percent - remained childless by the age of 45. Hence, there is no immediate concern that conditioning the sample to include only mothers will bias the estimates in a significant way.

4. Results

4.1. The Experience-wage and Experience-income Profiles of Mothers. In this section I illustrate the labor income- and wage-experience profiles graphically for sub-samples of mothers with varying first birth timing. Figure 1.5 plots the residuals from an OLS regression of annual earnings on year-fixed effects and dummies for age at labor market entry over the work history (where year 0 is the entry year in the labor market, defined as outlined in the Data section). Important to note is that labor earnings do not include parental leave benefits (or other transfers) and thus only measure income from market work. The five graphs in Figure 1.5 represent five different groups of women defined by their timing of first birth, that is, by the number of years elapsed between their labor market entry and the birth of their first child: 0-1 years, 2-3, 4-5, 6-7 and 8-9 years. Evident from the figure is that, except for the group of women with first birth timing 0-1 years after entry, labor earnings for all groups of women are more or less identical in the year that they enter the labor market. Also, the income trajectories of the different groups follow each other remarkably closely until the first child is born, after which they diverge. This tentatively suggests that first birth timing may causally affect labor earnings. Secondly, 15 years after entering the labor market and beyond, women who gave birth to their first child as late as 8-9 years after labor market entrance have lower earnings than 'earlier' mothers. Thus, there is a permanently lower income for 'late' mothers after birth compared to earlier mothers. Furthermore, there is a striking downward shift in the income profile after birth for all groups, suggesting an almost permanent shift to part-time work following the birth of the first child. Figure 1.6 graphs the evolution of the full-time equivalent monthly wage over the work history by first-birth timing, with the groups defined analogously to those presented in Figure 1.5. As was the case with labor earnings, also the wage profiles of mothers are strikingly similar until the first child is born, with the exception of mothers with very early childbearing (who have the lowest starting wage). Moreover, all groups seem to experience a slope decline in the wage path after they become mothers, which suggests reduced returns to experience post-birth, either due to reduced effort or due to reduced opportunities for on-the-job training or advancement. Interestingly, women with the lowest starting wages seem to be the ones postponing childbearing the longest; and they also seem to catch up in the long run with women who started at a higher wage level, but gave birth to their first child earlier.

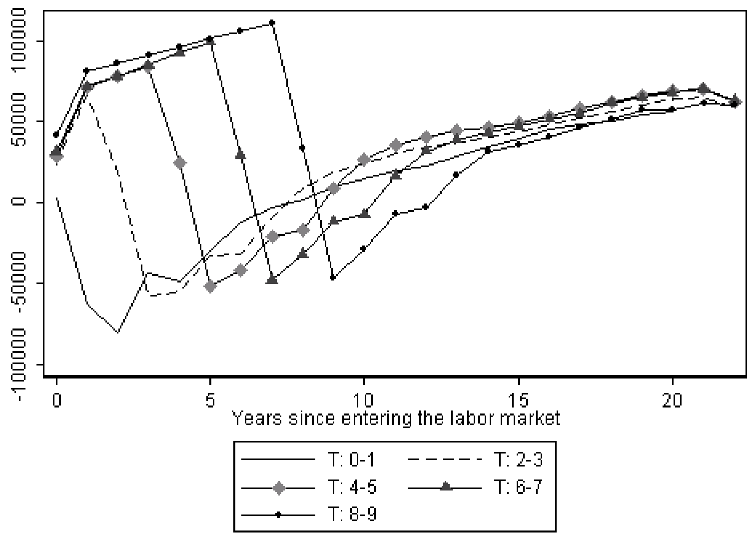


FIGURE 1.5. The residuals from an OLS regression of annual labor income on year-fixed effects and age at labor market entry for five groups of women divided by their first-birth timing.

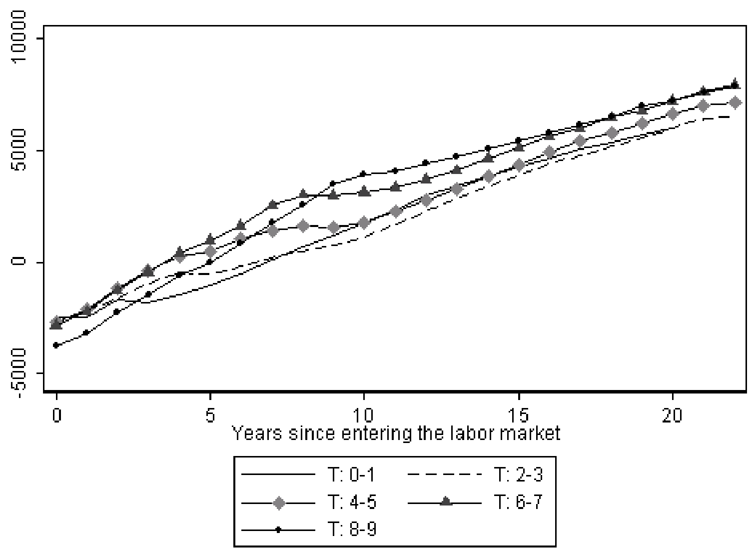


FIGURE 1.6. The residuals from an OLS regression of monthly wages on year-fixed effects and age at labor market entry for five groups of women divided by their first-birth timing.

4.2. The Effect of Motherhood Delay on Earnings and Wages. Consider individual i, who entered the labor market in calendar year l, had her first child in calendar year b, and was a years old when she entered the labor market. The regressor of interest, T, is defined in terms of l as $T = b - l$, such that $T = 0, 1, ..., J$. Furthermore, the main outcome variable measures the natural log of total income earned over the first 20 years of individual i:s career. However, the data does not allow a 20-year follow-up period after labor market entrance for all individuals. Instead, career earnings are defined as the total income earned from the first year on the labor market up to as long as I can follow the individual, but at most up to 20 years. Thus, the estimated regression equation is:

$$ln(\sum_{l=0}^{L} y_{ial}) = \alpha_0 + \beta T_i + \delta_a a_i + \delta_L + \delta_l l_i + \mathbf{x}_i' \delta_x + \epsilon_i \tag{1.1}$$

where a_i are dummies for age at labor market entry; δ_L are dummies for the number of observed career-years for individual *i* in the data, l_i are dummies for the calendar year of labor market entry (which are included to pick up e.g. wage growth in real terms), and finally, $\mathbf{x}_i$ is a vector of individual characteristics, measured pre-motherhood or at labor market entry. Equation (1.1) is estimated using both OLS and Two Stage Least Squares (2SLS) where T_i is instrumented with miscarriage before first birth. The coefficient of interest is β, which measures the average causal effect of a one-year delay of motherhood on the natural log of career earnings, or - in effect - the average impact of one extra year of pre-birth labor market experience. Note that a miscarriage induces a change in the birth timing for women, but also induces a change in the age at first birth. With the specification used here, I cannot identify an effect of postponing birth independent of the age at first birth. Although most predictions in the previous literature about the mechanisms of an effect of the age at first birth concern the level of pre-motherhood labor market involvement, I cannot rule out that also the age at first birth itself matters for outcomes. The effect measured here would then be a combined effect of pre-motherhood experience and the age at first birth.

Before analyzing the effect of first birth timing on income and wages, I first present evidence of the relevance of the instrument - miscarriage before first birth - for the first birth timing. Table 1.1 depicts the OLS estimates of the effect of miscarriage on first birth timing, where the first column reports the results from a regression without covariates, and columns 2 to 4 present results from models where control variables are added stepwise. The results show an estimated delay of first birth timing by around 6 months in the model without control variables. Adding controls for age at labor market entry, non-Nordic background and marital status (column 2) decreases this estimate somewhat; pregnancy loss is then estimated to delay first birth by 5.1 months, on average. However, adding a control variable for the number of pre-natal hospitalizations (column 3) does not alter the estimate much, and neither does adding control variables for the number of pre-natal hospitalizations for different diagnoses (column 4). Column 5 shows the results from a regression where also a full set of dummy variables for the calendar year of labor market entry are included, as well as dummy variables for the number of observed career years. Including the calendar year dummies decreases the magnitude of the first-stage relationship quite considerably; the estimated effect of miscarriage on first birth timing now shows a delay of first birth of 2.4 months (0.2 years) including all relevant control variables. This is likely because an increasing number, over time, of women in the 'control' group are also experiencing a pregnancy loss, but are treated at an outpatient establishment. However, the F-statistic for joint significance in the first-stage (not shown) is 32.53, which is well above the suggested rule of thumb of 10. Thus, there is no concern of a weak instrument. In the following, I always include calendar year dummies in all regressions, as well as included in specification (5) of Table 1.1.

TABLE 1.1. OLS estimates of the effect of miscarriage before first birth on first-birth timing

	(1)	(2)	(3)	(4)	(5)
Miscarriage at first pregnancy	0.508***	0.424***	0.415***	0.393***	0.200***
	(0.037)	(0.036)	(0.036)	(0.036)	(0.035)
Non-Nordic background		-0.144***	-0.146***	-0.150***	0.164***
		(0.023)	(0.023)	(0.023)	(0.022)
Married at labor market entry		-2.274***	-2.277***	-2.279***	-1.764***
		(0.014)	(0.014)	(0.014)	(0.015)
Divorced at labor market entry		-1.180***	-1.193***	-1.205***	-0.475***
		(0.060)	(0.060)	(0.060)	(0.056)
No. of pre-natal hospitalizations			0.122***	-0.017	-0.012
			(0.014)	(0.015)	(0.013)
Additional controls					
Dummies for age at labor market entry		✓	✓	✓	✓
Hospitalizations by diagnosis				✓	✓
Dummies for calendar year of labor market entry					✓
Dummies for the no. of observed career-years					✓
Observations	223412	223412	223412	223412	223412

NOTES.—The outcome variable measures first-birth timing, defined as the number of years elapsed between labor market entry and first birth. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

Table 1.2 depicts the results from the OLS and 2SLS estimations of the effect of first birth timing on the natural log of career earnings and the natural log of the average full-time equivalent monthly wage over the observed career, based on specification 1.1. As mentioned above, career earnings are defined as the total income earned from the first year on the labor market up to as long as I can follow the individuals in my data, at most up to 20 years after labor market entry. This outcome variable can be viewed as the net present value of income over the (observed) career. The average career wage is defined as the average full-time equivalent monthly wage (which is comparable to the hourly wage) over the observed career. Both income and wages are deflated using the Consumer Price Index.

Using OLS estimation, the results suggest that a one-year delay of motherhood is associated with an increase in career earnings with, on average, 3.7 percent. When instrumenting first birth timing with miscarriages, however, the 2SLS estimate suggests a statistically significant negative effect of a one-year delay on career earnings of about 15 percent. The standard errors of the 2SLS estimates are larger compared to the standard errors from the OLS regression, however, the F-statistic for joint significance in the first stage is 32.53, which is well above the suggested rule-of-thumb of 10. Thus, miscarriage before first birth does not seem to be a weak instrument. Moreover, the reduced form suggests a negative effect on earnings by 3 percent, significantly different from zero at the 1 percent level.

Labor earnings reflect both hours worked and hourly wage rates. To get a more complete picture of the career effects of birth timing, therefore, I continue by analyzing the effect of birth timing on the average monthly wage over the career, and the results from this analysis are presented in columns 3 and 4 of Table 1.2. Also here, the OLS estimate suggests a positive effect of delay, estimated to 1.8 percent higher wages, on average. However, instrumenting birth timing with miscarriages, the 2SLS estimate suggests a negative effect of a one-year delay on the average wage by 5.3 percent, significantly different from zero at the ten percent level. These results suggests that OLS exaggerates the positive effect of delay, and once endogeneity is taken into account, the effect of delay even goes in the opposite direction. This is in contrast to earlier studies who find positive effects of motherhood delay on both earnings and wages (see e.g. Miller 2011).

Up to this point the outcome variables measure income or wages over the entire, observed, labor market history for each individual. Hence, income both before and after entry into parenthood are included. As an alternative analysis I also consider the effects of postponing childbearing on the post-motherhood income trajectory, which also allows an examination of some dynamics of the effect of postponing childbearing. To this end, I perform separate yearly 2SLS regressions to estimate the effect of delayed childbearing on post-birth labor market income for varying years since first birth. The estimates from these regressions are plotted in Figure 1.7, which shows a large drop in income for mothers who postpone childbearing in years 0 to 3 after first birth. Income then 'bounces' back somewhat, but remains negative for the entire follow-up horizon (15 years after birth), although the estimates are then not significantly different from zero.

As seen from Figure 1.7, mothers who delay first birth due to the first pregnancy ending in miscarriage have a higher income drop also in the year of childbirth, which might cause some concern that these women would have lower income also before childbirth. However, the results presented in Section 2 did not indicate any major health differences between women who experienced pregnancy loss and those who did not. Nevertheless, as an additional sensitivity analysis, I perform separate regressions of the effect of pregnancy loss on labor income (i.e., the reduced form equation) for varying years since first birth, including pre-motherhood years. The estimates from these regressions are presented in Figure A5 in the Appendix. As seen from this graph, while the effect of pregnancy loss on income is significantly negative for all years *after* birth, there are no large differences in income between women who miscarried and women who did not before motherhood. If anything, there is a tendency of a positive trend in income before motherhood for mothers who later experienced a pregnancy loss. This positive trend is, however, followed by a drop in income in the year before the birth of the first child. This is the year when most of the women in the sample who experienced pregnancy loss actually had the miscarriage, and could therefore be related to e.g. sickness absence associated with the pregnancy loss. It seems unlikely, however, that this income drop by itself drives the *long-run* negative effects of postponing motherhood on labor earnings. Rather, it seems like postponement of first birth causes a longer work interruption after birth. To try shed some light on what may cause the income drop after first birth, I therefore continue by investigating the effects of postponing motherhood on subsequent fertility behavior; the number of children and the spacing of subsequent births, and how these - potentially - intermediate outcomes are affected by postponing the first birth.

TABLE 1.2. OLS and 2SLS estimates of the effect of first-birth timing on the log of total earnings and the average wage rate over the first 20 years of the career

	Log Career earnings		Log Avg. career wages	
	OLS	IV	OLS	IV
Timing	0.037***	-0.147***	0.018***	-0.053*
	(0.000)	(0.050)	(0.000)	(0.029)
Non-Nordic background	-0.197***	-0.166***	-0.159***	-0.147***
	(0.007)	(0.011)	(0.005)	(0.007)
Married at labor market entry	-0.047***	-0.373***	-0.024***	-0.149***
	(0.004)	(0.088)	(0.003)	(0.051)
Divorced at labor market entry	-0.064***	-0.152***	-0.044***	-0.076***
	(0.017)	(0.031)	(0.012)	(0.018)
Pre-natal hospitalizations	-0.020***	-0.022***	-0.013***	-0.014***
	(0.004)	(0.005)	(0.003)	(0.003)
Dummies for calendar year of LM entry	✓	✓	✓	✓
Dummies for age at LM entry	✓	✓	✓	✓
Dummies for the no. of observed career-years	✓	✓	✓	✓
Number of hospital visits by diagnosis	✓	✓	✓	✓
Reduced form		-0.029***		-0.011**
		(0.008)		(0.006)
F-stat		32.5279		33.7912
Observations	223412	223412	222339	222339

NOTES.—The outcome variable measures the log of career earnings (columns 1 and 2), defined as the total income earned from labor market entry up to at most 20 years later, and the log of average career wages over the observed career (at most 20 years). Earnings and wages are deflated by Consumer Price Index. Robust standard errors are reported in parentheses. The control variables include dummies for non-Nordic born; married at labor market entry; divorced at labor market entry; number of hospitalizations pre-birth, number of hospital visits by diagnosis, as well as a full set of dummies for calendar year of labor market entry and the number of observed career-years. *$p<0.1$, **$p<0.05$ ***$p<0.01$.

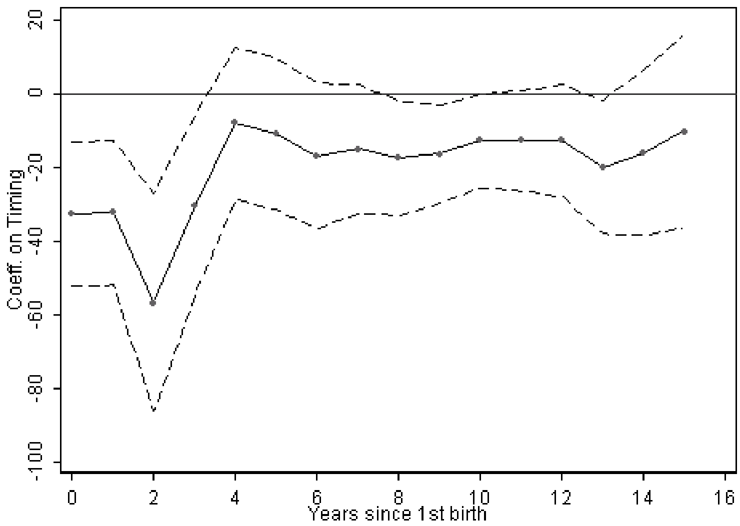


FIGURE 1.7. Parameter estimates from separate 2SLS regressions of the effect of motherhood delay on labor income for varying years since first birth (where year 0 equals the birth year of the first child) and the 95 percent confidence intervals. The outcome variable measures annual labor income in 1000s SEK (expressed in real terms).

4.3. Exploring the Mechanisms: Subsequent Fertility Behavior. The results presented in the previous section suggest that a one-year delay of first birth causally induces a reduction of career earnings by 15 percent, on average, and a reduction in the average monthly wage over the career by 5 percent. In light of existing findings that often provide evidence of monetary benefits to postponing childbearing - in particular for highly educated women - these results may seem unexpected and surprising. In this section I aim to explore some of the potential mechanisms through which these effects on labor market outcomes could arise. The mechanisms proposed and analyzed here concern subsequent fertility behavior, both in terms of the total number of children and in terms of the spacing between the first and subsequent children. The former is interesting to analyze since it is possible that costs or benefits to postponing first birth partly capture a higher or lower wage penalty associated with more or fewer children, respectively. Moreover, in the demographics literature and in dynamic models of fertility, not only the timing of first birth is considered, but also the spacing of births, and how the timing of births relate to each other. For instance, in their paper on the timing and spacing of births using Swedish data, Heckman et al. (1985) find that, when controlling for unobserved heterogeneity, a delay in the arrival of one child is compensated for by an acceleration in the arrival of the next child. Furthermore, Troske and Voicu (2012) find that women with higher education have the first birth later in life, have fewer children, and space their subsequent children more closely together. Moreover, their findings suggest that spacing of births in turn affect women's labor market involvement.

To analyze whether completed fertility and child spacing are affected by delaying the first birth, I estimate the effect of motherhood delay on the total number of children born to a woman at the end of the observation period (i.e., in year 2007, which for many of the women in my sample represents completed fertility) and on the time interval between the first and the second child measured in years. The results from this analysis are presented in Table 1.3. The OLS estimation of the effect of motherhood delay on child spacing suggests that a one-year delay of the first birth reduces the spacing to the next child by roughly 2.3 months (0.19 years). Taking endogeneity into account, the 2SLS estimate suggests a reduction in the time interval between the first and the second child by about 8.4 months (0.70 years); an even larger effect compared to the OLS estimate. As the average interval between the first two births is about 2.7 years among women with more than one child in the sample, the reduction in the birth interval potentially implies relatively short birth intervals. However, when studying the effect on the total number of children born to a woman by the end of 2007, 2SLS estimation does not indicate that delay affects the total number of children. Closely related to subsequent childbearing is of course parental leave durations. Troske and

TABLE 1.3. OLS and 2SLS estimates of the effect of first-birth timing on child spacing and number of children

	OLS	2SLS
Dependent variable		
Years between 1st and 2nd child	-0.19***	-0.70***
	(0.00)	(0.16)
Number of children in 2007	-0.10***	0.02
	(0.00)	(0.06)
Controls for personal characteristics	✓	✓
Dummies for calendar year of LM entry	✓	✓
Dummies for age at LM entry	✓	✓
Dummies for the no. of observed career-years	✓	✓
Observations	223412	223412

NOTES.—The outcome variables measure the number of years between the first and second child, and the total number of children to a woman at the end of the observation period (2007), respectively. The control variables include dummies for non-Nordic born; married at labor market entry; divorced at labor market entry; and number of hospitalizations pre-birth, as well as a full set of dummies for calendar year of labor market entry and the number of observed career-years. *p<0.1, **p<0.05 ***p<0.01.

Voicu (2012) conclude that while higher educated women have incentives to postpone second births, they are likely to space births more closely together. They further argue that this may suggest that higher educated women face larger fixed time or money costs of working, which makes them less likely to combine market work and child care. To study whether the first birth timing matters for the parental leave length of mothers, I estimate the effect of postponing first birth on yearly parental leave usage, starting from the birth year of the first child up to 7 years later and on annual fertility for the same years. Annual fertility is here defined as a dummy variable taking the value one when a second or third child is born. The results from this analysis are presented in Table 1.4 and show an interesting pattern. First, we can see that mothers who delay have, on average, 28, 34, 131, and 77 days more parental leave in the birth year of the first child; 1, 2, and 3 years after, respectively. Interestingly, they have 90 days less parental leave days, on average, 4 years after the first child is born. The coefficients for the remaining years are also negative, but not significantly different from zero. Finally, in the lower panel of Table 1.4 I present the result from estimating a 2SLS regression of the effect of motherhood delay on the total number of parental leave days taken during the first eight years after the birth of the first child. This effect is estimated to 55 days, but is not statistically significant. Thus, it seems like postponing first birth leads to a reshuffling of parental leave, rather than maybe a total increase in leave taking. Consistent with this pattern are the results from the estimates of the effect of delay on annual fertility (for subsequent children) which are presented in column 2 of Table 1.4, where we seen an increase in fertility of 14 and 56 percentage points 1 and 2 years after the birth year of the first child, respectively, and then a decrease of 32 and 13 percentage points 3 and 4 years after first birth, respectively, with remaining coefficients also being negative but not significantly different from zero. The time pattern of the effects on subsequent fertility are thus in line with the time pattern of the effect of postponing first birth on labor income shown in Figure 1.7.

The results on the spacing of births and on the total number of children are in line with some findings of the effect of motherhood delay on subsequent childbearing. For example, Bratti and Tatsiramos (2012) study the effect of delaying motherhood on the transition to the second birth for a number of European countries using data from the European Community Household Panel. The effect of delaying motherhood is found to differ across countries. Specifically, women who delay their first birth are less likely to progress to second parity, but a higher availability of family friendly policies raises the probability of having a second birth. For instance, the authors find that delaying age at motherhood from 25 to 30 leads to a positive effect on the likelihood of progressing to higher parity within 5 years from first birth in countries such as Denmark, and a negative effect of 12 percentage points in Southern European countries such as Greece.

Taken together, one possible interpretation of the results presented in this section is that delaying motherhood does not lead to fewer children altogether, but more closely spaced children, and that this, in turn, implies being away from the workplace during a larger part of one section of the working history, perhaps the critical time period of career build-up.

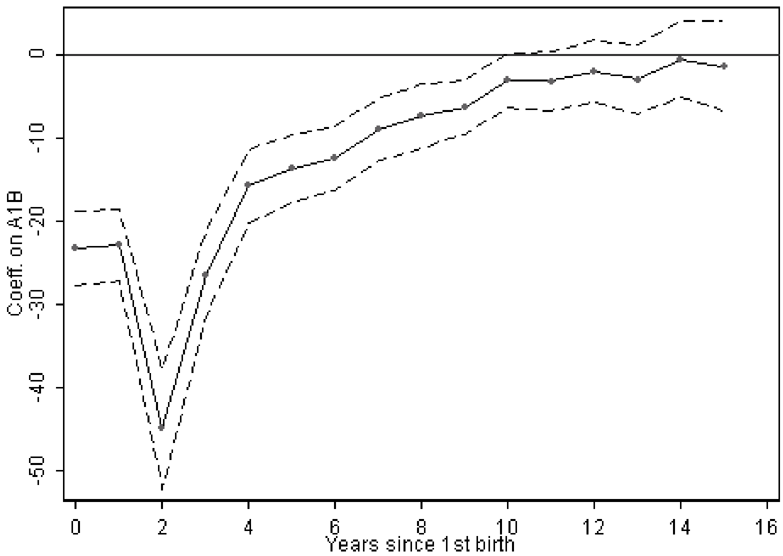
TABLE 1.4. 2SLS estimates of the effects of first birth timing on yearly parental leave usage and yearly probabilities of having another child

Dependent variable	Parental leave days	Additional child
Specification	2SLS	2SLS
Birth year first child	27.884*	0.002
	(16.416)	(0.021)
Birth year first child +1	33.396**	0.142***
	(16.609)	(0.051)
Birth year first child +2	130.939***	0.559***
	(40.989)	(0.165)
Birth year first child +3	77.454**	-0.322***
	(37.520)	(0.112)
Birth year first child +4	-90.274***	-0.126*
	(33.930)	(0.073)
Birth year first child +5	-31.689	-0.019
	(23.099)	(0.051)
Birth year first child +6	-19.399	-0.006
	(17.528)	(0.041)
Birth year first child +7	-2.917	0.009
	(13.688)	(0.029)
Birth year first child +8	77.064	0.013
	(65.890)	(0.051)
Pooled data		
Year 1 to year 8	54.965	
	(58.967)	

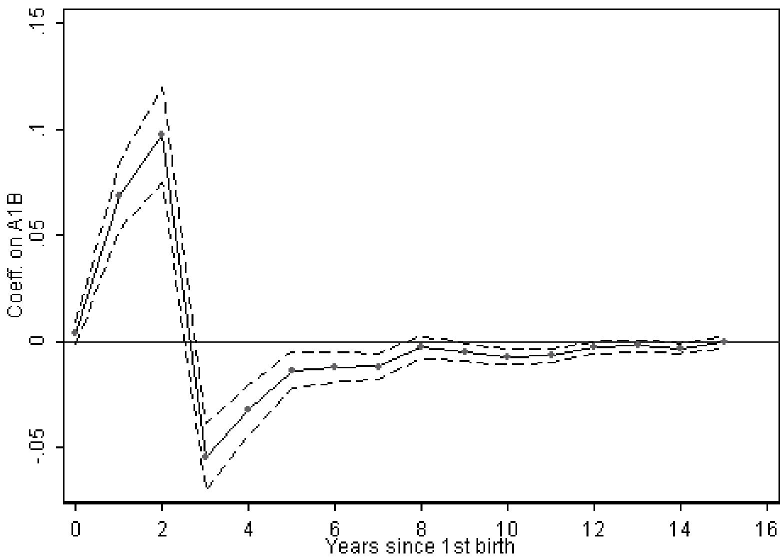
NOTES.— The outcome variables measure the number of parental leave days taken in each year from the birth-year of the first child up to 7 years later (column 1) and the annual probability of having an additional child, respectively. The control variables include dummies for non-Nordic born; married at labor market entry; divorced at labor market entry; and number of hospitalizations pre-birth, as well as a full set of dummies for calendar year of labor market entry and the number of observed career-years. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

4.4. Estimates of the Effect of the Age at First Birth. The estimates of the effect of delayed motherhood on labor market outcomes provided in this article contrasts the results from previous work on the topic. While an analysis of the effect of delayed motherhood on subsequent fertility tentatively suggests that the tempo of subsequent births may partly explain the findings presented in this paper, there still might remain some concerns, for example about whether the results are driven by the somewhat different sampling scheme and the different definition of first birth timing employed. In this section, I therefore present results from analyses where I follow a more traditional sampling scheme. I now restrict attention to mothers whose first child was born between 1988 and 2006, who were aged 21 or older at first birth, but not older than 35 at first birth. Individuals with at most compulsory schooling are excluded from the sample. Thus, in contrast to my main analysis, I do not put any restrictions on the first birth being born before or after labor market entrance. The regressor of interest is here defined as the age at first birth, which is the most commonly used variable to measure first birth timing in existing work. I then estimate the effect of the age at first birth, using miscarriage before first birth to instrument for the former, on yearly labor income over a 15-year horizon after first birth. I also estimate the effect of the age at first birth on the annual probability to give birth to a subsequent child. The control variables include an indicator for non-Nordic background, the number of pre-motherhood hospitalizations, the number of pre-motherhood hospitalizations for each diagnosis type, and an indicator for college education. To conserve space, I present the findings graphically, but the full results are available upon request. As seen from Figure 1.8, the results look very similar to my main population of interest. Specifically, there is a negative effect of postponing motherhood by one year, on average, with a sharp drop in earnings in the second year after first birth. This drop in earnings coincides with a peak in the probability to give birth to a subsequent child. All in all, measuring timing of birth in terms of the career point instead of as the age at first birth does not seem to be what is driving the negative effects of motherhood delay on earnings.

An additional concern regarding the negative effects found on earnings from postponing motherhood, is that the exclusion restriction is not satisfied. While there does not seem to be any long-run effects of miscarriage on sickness absenteeism, an increase in sickness absence appears in the year of the miscarriage event. In the next and final section of the paper, I therefore estimate the effect of first birth timing under a different set of identifying assumptions. Specifically, I estimate the effect of motherhood itself on wages, and allow this effect to vary by birth timing. This is achieved with panel data and an individual-fixed effects estimator.



(A) 2SLS estimates of the effect of age at first birth on labor earnings, 1000s SEK



(B) 2SLS estimates of the effect of age at first birth on the annual probability of giving birth to a subsequent child

FIGURE 1.8. The effect of Age at first birth on labor earnings, and on higher order fertility, mothers with at most high school or college education.

5. Panel Data Estimates of the Effect of Motherhood on Wages

The previous section presented results from estimating the effects of motherhood timing on the net present value of income over the observed career and on the average monthly wage over the career, using cross-sectional variation. These labor market outcomes were both found to be negatively affected by a one-year delay of motherhood. Moreover, I found that motherhood delay also induces a closer spacing between the first and the second born children, whereas no effects were found on the total number of children born by the end of the observation period. This might explain the negative effects on labor market outcomes, if being away from the labor market for a larger share of a critical time period is associated with larger wage penalties. In this section, I aim to explore the effects of motherhood on individual wage growth more closely, and how it varies with first birth timing. To this end, I estimate the effects of motherhood on full-time equivalent monthly wages using panel data specifications in the spirit of Miller (2011). In the first panel specification I include individual-fixed effects, experience (years worked), experience squared, motherhood status (set to equal one in the year of first childbirth onwards) and years since first birth. The motherhood indicator captures a potential downward shift of the wage profile, which Miller (2011) refers to as human capital depreciation or fixed motherhood penalties, whereas a negative coefficient on Year Since First Birth captures a slope decline in the wage profile post-birth, indicating reduced returns to experience. The estimated panel data specification is thus the following:

$$\begin{aligned} ln(w_{it}) &= \beta_0 + \beta_1 Exp_{it} + \beta_2 ExpSq_{it} + \beta_3 Mother_{it} \\ &+ \beta_4 Mother_{it} \times (YearsSinceFirstBirth_{it}) + \alpha_i + \epsilon_{it} \end{aligned} \tag{1.2}$$

where Years Since First Birth is measured as the calendar year minus the calendar year of the first birth. α_i capture unobserved time-invariant individual-fixed effects and thus controls for unobserved individual heterogeneity. In a second panel data specification, the effect of motherhood is allowed to vary with first birth timing, by including interaction terms between years since first birth, motherhood and dummy variables for four groups of women with different birth timing:

$$\begin{aligned} ln(w_{it}) &= \beta_0 + \beta_1 Exp_{it} + \beta_2 ExpSq_{it} + \beta_3 Mother_{it} + \sum_{j=1}^{4} \beta_{4,j} Mother_{it} \\ &\times (YearsSinceFirstBirth_{it}) \times 1(Timing_i = j) + \alpha_i + \epsilon_{it} \end{aligned} \tag{1.3}$$

where $j = 0-2, 3-4, 5-6, 7-10$.

Table 1.5 reports the results from estimating Equations (1.2) and (1.3) in columns 1 and 2, respectively. As seen from Table 1.5, the results from estimating specification (1.2) indicate that wages increase with experience, by on average 5 percent per extra year worked. In addition, there is a fixed motherhood wage penalty of about 3.8 percent (i.e., a shift of the profile, represented by the coefficient on the motherhood indicator). Moreover, the interaction term between motherhood and years since first birth is negative and statistically significant with a coefficient of -0.0107, suggesting that mothers indeed experience a flattening of the wage profile post birth. When the changes in wages for mothers are allowed to vary with first birth timing, we see that the wage penalty is larger for women who delay motherhood for longer times; the coefficients are always negative and almost monotonously more negative for each group of 'delay'. Consistent with the main results presented in the previous section, these findings suggest that the wage penalty is larger for women who have their children later.

TABLE 1.5. Panel estimates of the effect of motherhood on log wages

	(1) FE	(2) FE
$PotentialExperience$	0.0502***	0.0498***
	(0.0002)	(0.0002)
$ExperienceSquared$	-0.0003***	-0.0002***
	(0.0000)	(0.0000)
$Mother$	-0.0376***	-0.0335***
	(0.0005)	(0.0005)
$Mother \times$ (Years Since birth)	-0.0107***	
	(0.0002)	
$Mother \times$ (Years Since birth) $\times 1(T = 0 - 2)$		-0.0108***
		(0.0002)
$Mother \times$ (Years Since birth) $\times 1(T = 3 - 4)$		-0.0130***
		(0.0003)
$Mother \times$ (Years Since birth) $\times 1(T = 5 - 6)$		-0.0124***
		(0.0003)
$Mother \times$ (Years Since birth) $\times 1(T = 7 - 10)$		-0.0141***
		(0.0003)
$Constant$	9.7055***	9.7050***
	(0.0005)	(0.0005)
Observations	1724014	1724014

NOTES.— Standard errors are clustered at the individual level and reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

The analysis based on the panel data set corroborates the negative effects from the analyses based on the cross-sectional variation, and indicate the presence of reduced returns to experience post-birth for mothers, with this slope decline in wages being increasingly larger for "late" mothers. This decreased returns to experience may either be attributed to fewer opportunities for advancement and training (the so called "mommy-track"), or women may exert less effort in the workplace.

Interestingly, postponing first birth seem to induce a tighter interval between the birth of the first two children, while completed fertility does not seem to be affected by the timing of first birth. Although a closer child spacing might imply economies of scale such that hours in market work is not as negatively affected, this fertility effect may be able to partly explain the negative wage effects. Given that the institutional setting in Sweden allows rather lengthy parental leaves, having two (or more) consecutive leave periods closely spaced might be detrimental to college educated women's subsequent career opportunities. The findings summarized in this paper also raises questions about whether the positive effects of motherhood delay found in the US (for example in Miller 2011) are driven by effects on (reduced) total fertility.

6. Concluding Discussion

The negative effects of career interruptions due to childbearing on women's employment and wages are rather well documented. However, the implications of first birth timing on career outcomes are not yet fully understood. In this paper, I aim to estimate the causal effect of postponing first birth on the labor earnings and wages of Swedish college educated women. To isolate the impact of first birth timing on labor market outcomes, I instrument fertility timing with the occurrence of miscarriage before first birth. I focus on highly educated women who first finish college, enter the labor market and subsequently become a parent. This allows me to measure birth timing as potential pre-natal labor market experience, i.e,. the number of years elapsed between labor market entry and the birth of the first child.

In line with previous studies, I find that OLS estimation suggest a positive effect of postponing first birth on the total income earned over the first 20 years of the career, as well as on the average monthly wage over the career. However, exploiting the arguably exogenous variation in birth timing induced by pregnancy loss, I find that postponing first birth by one year on average negatively affects both career earnings and wages. This is in stark contrast to most of the previous studies, who often document monetary benefits to postponing motherhood. I also find that delaying first birth causally reduces the time elapsed to second birth, that is, a decreased spacing between the first two children. However, I find no evidence of an effect of first birth timing on the total number of children. Thus, mothers who delay first birth do not seem to forego further childbearing, but rather to have children more tightly spaced. Closely linked to the findings on subsequent fertility, postponing first birth induces a 'reshuffling' of parental leave usage to be higher in the years closest to the first born child, and lower in the following years. Hence, fertility delay seem to induce mothers to have two (or more) lengthy parental leave periods more closely spaced, which in turn might be more detrimental to subsequent career opportunities compared to taking the same amount of leave but spread out over a longer horizon of working life.

To further study how the individual wage pattern is affected by motherhood and the timing of motherhood, I also estimate the effect of motherhood itself on wage growth for women, using a panel data specification with individual-fixed effects. The panel data results corroborate the negative effects from the cross-sectional data set and suggest a slope decline of the wage profile post-birth, with the slope decline being increasingly larger for late mothers.

The findings provided in this paper are in contrast to studies from the US where fertility delay has generally been found to have positive effects on both income and wages. However, it is important to interpret the results in the present study within the institutional context. In Sweden, all parents are entitled to 480 days of job-protected parental leave for each child and in practice, most women take out a major part of this leave. The benefits are wage-replaced and parents have the right to reduce working hours for up to 25 percent until the child turns eight years old. Hence, Swedish parents do not face the same restrictions to take parental leave as do parents in e.g. the US. In a system that does not offer very generous family policies, establishing a stronger attachment to the labor market before taking leave to care for a child may protect mothers from having to start over when they re-enter the labor market, and also facilitate the possibility of returning to the pre-birth employer. Delaying motherhood in such a system may also partly reflect a lower wage penalty associated with fewer children. In Sweden, however, job protection is the default, and legislated parental leave is rather generous. However, long interruptions, especially if they are closely spaced, might yield stronger penalties in the labor market, at least for college educated women who are the focus of this paper. This of course raises questions about the optimal length of leave, and the potential detrimental effects for mothers' careers of having a too generous parental leave system, in particular if women continue to stand for the majority of parental leave take-up. The findings presented in the present paper and in the emerging literature about so called tempo effects of fertility thus may have important policy implications since, not only policies affecting the number of children, but also tempo policies that affects the age at first birth and spacing of births may have an impact on women's wage trajectories, and should provide interesting avenues for future research.

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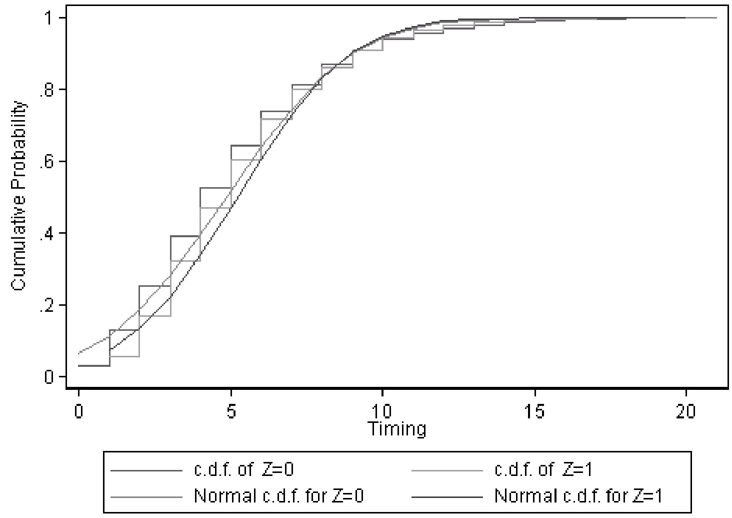
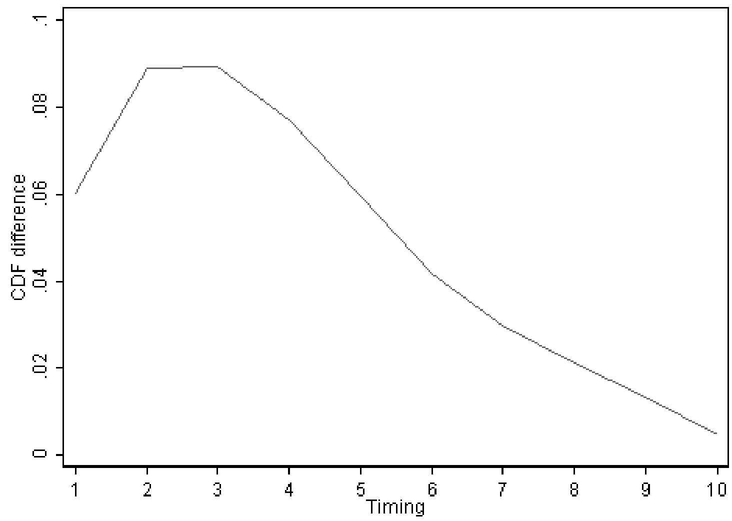
Appendix



(A) Empirical CDF of first birth timing for $Z = 1$ and $Z = 0$ and the best fitting normal model superimposed over the sample CDFs.



(B) Difference between the CDFs graphed in (a)

FIGURE A1. The empirical Cumulative Distribution Functions of birth timing, T, by the occurrence of miscarriage before first birth.

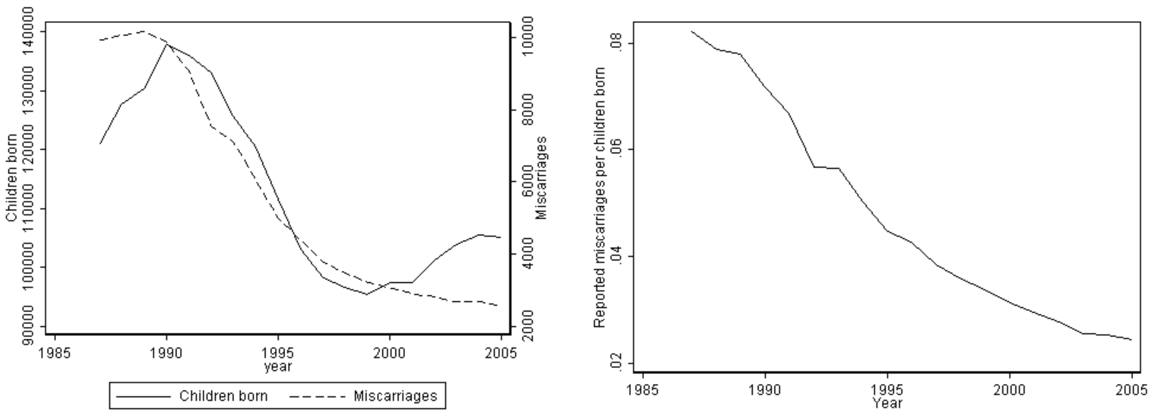


FIGURE A2. Number of reported miscarriages in the NPR, number of children born, and the share of miscarriages of children born, 1987-2005.

TABLE A1. The frequency of co-morbidities for hospital visits associated with miscarriage

	Frequency	Percent
No. of co-morbidities		
0	80,209	95.27
1	3,554	4.22
2	356	0.42
3	63	0.07
4	6	0.01
5	2	0.00
6	1	0.00

NOTES.— The table reports the frequency of co-morbidities to all hospitalizations where the main diagnosis is classified as a miscarriage.

TABLE A2. Diagnosis type for co-morbidities of miscarriage

	Frequency	Percent
Co-morbidity type		
Infectious	123	3.46
Tumors and neoplasms	255	7.18
Blood(-forming organs)	468	13.17
Endocrine	148	4.16
Mental behavioral	36	1.01
Nervous system	29	0.82
Ear	3	0.08
Circulatory system	40	1.13
Respiratory	58	1.63
Digestive system	28	0.79
Skin	15	0.42
Musculoskeletal	34	0.96
Genitourinary system	556	15.64
Pregnancy related	775	21.81
Perinatal	3	0.08
Congenital malformations	59	1.66
Symptoms not classified elsewhere	87	2.45
Factors associated with health status	775	21.81
External causes	62	1.74

NOTES.— The table reports the frequency of the diagnoses of the first co-morbidity (i.e., the first secondary diagnosis to the main diagnosis being miscarriage) to all reported miscarriages with at least one reported co-morbidity.

TABLE A3. Severity of miscarriages, 1997-2005

	Mean
Incomplete with complication	0.108
	(0.311)
Complete with complication	0.0183
	(0.134)
Incomplete without complication	0.676
	(0.468)
Complete without complication	0.197
	(0.398)
Observations	26120

NOTES.— Means and (standard deviations).

Table A3 divides the reported miscarriages into four different types based on severity. The data is based on reported miscarriages that occurred during 1997 to 2005. The table shows that the majority of cases are without any complications (adding both complete and incomplete miscarriages). Moreover, Figure A3 graphs these proportions, now divided only into two categories: with and without complications, by year. We can see that the proportions of reported cases with and without complications seem to converge somewhat over the time period, but the overwhelming majority of miscarriages are reported to have been without any complications throughout the time period.

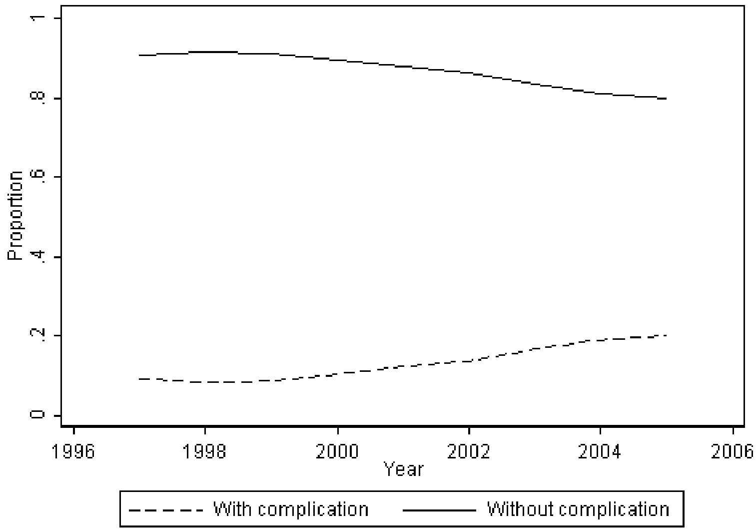


FIGURE A3. Proportion of miscarriages with and without complications, 1997-2005.

DEFINING LABOR MARKET ENTRY

For individuals with more than one employment at the same firm in the same year I calculate the total earnings received from that employer in that year, as well as the total number of months worked and then drop duplicate observations on person-firm-year. Moreover, for individuals with more than one employment in one year, but at different firms, I define that individual's workplace the firm at which she received her main income in that year. This procedure leaves me with unique observations on person-firm-year level.

Since timing of motherhood is here defined as timing with respect to labor market entry, a definition of a first stable employment is needed. To do this, I first back out the year in which the highest attained education level is completed for each individual, where I divide highest attained educational attainment into three categories: compulsory education; high school education; and some college or more. Backing out the graduation year from the panel data gives an average age at completion of compulsory education of about 16.14, which is in line with Swedish compulsory education being 9 years of duration starting at the age of 7. For high school graduates, the corresponding age in the data is about 19.90 and for college educated 27.55 (the high average age for finishing college is partly explained by gap years between high school and college). Second, I define entry to the labor market as the first calendar year after the completion of education that the individual (i) earned at least three times the 10th percentile of the full wage distribution, and (ii) had an employment that lasted at least 4 months. Figure A4 shows the time elapsed between finishing education until a first job is attained for high school educated mothers and college educated mothers, respectively.

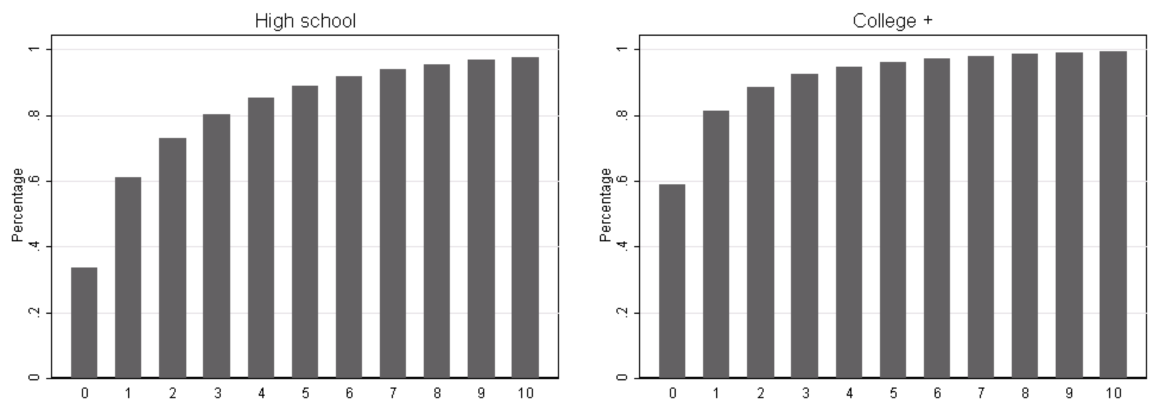


FIGURE A4. Time elapsed from graduation to a first job for high school and college educated mothers.

TABLE A4. Summary statistics for the study-sample, the sample of college educated women, and the full sample of women who had their first child 1988-2006

	Study sample	All college graduates	Full sample
Miscarriage at first pregnancy	0.0185	0.0208	0.0224
	(0.135)	(0.143)	(0.148)
Age at miscarriage	30.73	29.02	27.18
	(4.408)	(5.070)	(5.268)
Age at first birth	30.13	29.19	27.48
	(3.664)	(4.504)	(4.985)
1st Birth Timing	4.291	1.392	3.095
	(2.507)	(6.612)	(6.193)
Age at labor market entry	25.84	28.03	24.74
	(3.363)	(5.483)	(5.727)
Time to labor market entry	0.896	1.072	1.680
	(1.696)	(2.230)	(2.739)
Non-Nordic background	0.0486	0.139	0.164
	(0.215)	(0.346)	(0.371)
Married at labor market entry	0.0933	0.187	0.116
	(0.291)	(0.390)	(0.320)
Live in large city	0.293	0.265	0.209
	(0.455)	(0.441)	(0.406)
Number of children in 2007	1.955	1.957	1.990
	(0.710)	(0.763)	(0.842)
Compulsory schooling			0.0901
			(0.286)
High school			0.472
			(0.499)
College			0.438
			(0.496)
Observations	223412	382439	901940

NOTES.— The table reports summary statistics for the specific sample under study (column 1), the full sample of college educated women, as well as the full sample of women.

TABLE A5. Differences in average characteristics between mothers and childless women, among women who experienced a miscarriage.

	(1) Not childless	(2) Childless	[(1)-(2)] Difference
Age in 2007	50.60	52.08	-1.478***
	(4.478)	(4.721)	(0.117)
Family income in 2007, 1000s SEK	5562.9	4864.6	698.3***
	(4189.9)	(3302.9)	(179.9)
Labor income in 2007, 1000s SEK	2143.2	2108.8	34.45
	(1673.9)	(1779.2)	(43.60)
Compulsory schooling	0.127	0.125	0.00161
	(0.333)	(0.331)	(0.00864)
High school	0.450	0.418	0.0323*
	(0.498)	(0.493)	(0.0129)
College	0.421	0.453	-0.0326*
	(0.494)	(0.498)	(0.0128)
Married	0.590	0.377	0.214***
	(0.492)	(0.485)	(0.0128)
Divorced	0.223	0.182	0.0409***
	(0.416)	(0.386)	(0.0108)
Never married	0.170	0.428	-0.258***
	(0.375)	(0.495)	(0.00990)
Widowed	0.0168	0.0123	0.00455
	(0.129)	(0.110)	(0.00332)
Same-sex partnership	0.0000860	0.00129	-0.00121***
	(0.00927)	(0.0359)	(0.000304)
Observations	34893	1548	

NOTES.— Means, standard deviations and differences in mean characteristics. The sample consists of mothers aged 45 or older in 2007 and who experienced a miscarriage sometime between 1987 and 2005. *p<0.1, **p<0.05 ***p<0.01.

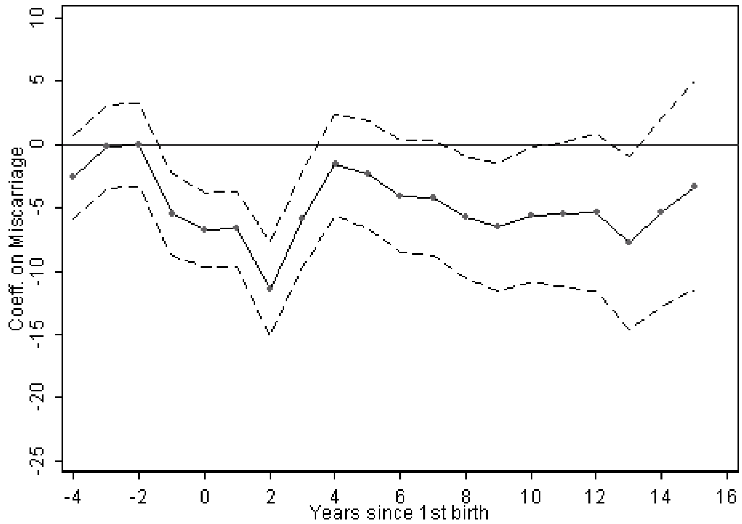


FIGURE A5. Parameter estimates from separate OLS regressions of the effect of pregnancy loss on labor income (the reduced form equations) for varying years since first birth (where year 0 equals the birth year of the first child) and the 95 percent confidence intervals. The outcome variable measures annual labor income in 1000s SEK (expressed in real terms).

PAPER 2

Birth Spacing and Women's Subsequent Earnings - Evidence from a Natural Experiment

Arizo Karimi

ABSTRACT This paper analyzes the relevance of spacing births for women's subsequent earnings and wages. Spacing births in longer intervals may allow women to re-enter the labor market between childbearing events, thereby avoiding expanded work interruptions and, in turn, reduce the negative effects of subsequent children. Based on arguably exogenous variation in birth spacing induced by pregnancy loss between the first two live births, the evidence provided in this paper supports this hypothesis and suggest that delaying second birth by one year, on average, increases the probability of re-entering the labor market between births. Moreover, spacing births are found to increase both labor market participation and labor income over an extended horizon after second birth. Also long-run wages are positively affected, with a more pronounced effect for highly educated mothers.

1. Introduction

As the variance in completed fertility has decreased in developed countries, fluctuations in aggregate fertility trends have become increasingly associated with fluctuations in the timing and spacing of births (Cigno and Ermisch 1989, Hotz et al. 1997a, Gustafsson 2001). Consequently, in addition to completed fertility, tempo fertility has received considerable attention in economic studies.[1] Such work include evaluations of the effects of economic factors on the timing and spacing of births (see e.g. Heckman and Walker 1990, Merrigan and Pierre 1998), but recently an economics literature has emerged where focus lies on evaluating the effects of the timing of births, in particular the timing of first birth, on women's labor market outcomes. Work interruptions in connection with childbearing are associated with foregone wages and foregone investments in human capital. Postponing motherhood may help women reduce the costs of childbearing by shortening the time horizon over which such losses are incurred, and previous empirical work suggest that motherhood delay positively

This paper has benefited greatly from valuable comments and suggestions from Per Johansson, Lena Hensvik, Oskar Nordström Skans, Peter Skogman Thoursie and Hans Grönqvist.

[1] See e.g. Hotz et al. 1997a, Gustafsson 2001 for reviews of the literature.

affects women's earnings (see e.g. Miller 2011, Bratti and Cavalli 2013, Troske and Voicu 2012, Wilde et al. 2010, Amuedo-Dorantes and Kimmel 2005, Taniguchi 1999). However, much less is known about the consequences of spacing births for women's career outcomes.

Spacing births may alleviate adverse consequences of further childbearing through the accumulation of pre-birth labor market experience. In addition, by allowing re-entry into the labor market between births, spacing births could imply work interruptions of shorter durations. For example, Gustafsson et al. (2002) study the impact of motherhood postponement on the duration of time spent at home after birth. For Sweden, they find a considerable decrease in the hazard of leaving full-time home care if a second child is born shortly after the first child. Thus, if a second child is born shortly after the first, the duration of home time after first birth may be extended if mothers view the childbearing events as one spell with two births rather than two separate spells. In turn, an extended work interruption could have important consequences for female workers' subsequent labor market attachment, career opportunities and wage offers.

The aim of this paper is to estimate the effect of spacing births on women's long run labor market earnings and wages. Fertility timing choices and career decisions are, however, likely to be jointly determined. Moreover, women may have differing preferences regarding market work and family that induce some women to invest more effort at work and space their children with longer intervals. A selection-on-observables estimator will then yield an upward biased estimate of the effect of birth spacing on labor market outcomes. On the other hand, if women with higher earnings potential space their children more closely, a selection-on-observables estimator will yield a downward biased estimate of this effect. In addition, re-entering the labor market after first birth may itself affect the timing of subsequent children, giving rise to a reversed causality problem. To address these potential endogeneity issues, I employ an instrumental variables strategy and exploit arguably exogenous variation in birth spacing induced by miscarriages that occur between the first and second live births. Miscarriages are randomly occurring fertility shocks that delay time to birth and thus induce a longer time interval between births.[2]

The analyses provided in this study contribute to the literature on the effects of birth timing on labor market outcomes in several respect. To begin with, the economics literature on the timing of births is still fairly limited. The majority of this work focuses on the effects of teenage childbearing for subsequent outcomes (see e.g. Hotz et al. 1997b, 2005). Although recent studies define birth timing more broadly, the existing evidence on the effect of first birth timing is still scarce. Even less is known about the impact of the timing of subsequent births on women's career outcomes. An exception to the scarcity of evidence on the impacts

[2] Miscarriages have been used in previous studies to instrument for the timing of first birth (see e.g. Ashcraft et al. 2013, Karimi 2013, Miller 2011, Bratti and Cavalli 2013, Herr 2007, Hotz et al. 1997b, 2005).

of birth spacing is provided by Troske and Voicu (2012), who use multinomial probit models to study the effects of the timing and spacing of births on women's labor market involvement and find that both matter for women's labor supply around birth.[3] Secondly, most studies on the consequences of childbearing for labor market outcomes focus on the static effect of children on women's earnings or labor supply, while much less attention has been paid to how childbearing affect long run wage trajectories (Wilde et al. 2010). The unique Swedish register data upon which the analyses in the present paper are based allows an examination of how the impact of spacing birth evolves over time since birth for both labor earnings and participation, but also for women's long run wage growth. Since I can also follow mothers *before* entry into parenthood, the data allows implementation of important falsification tests to assess the validity of the empirical strategy employed. Third, while fertility shocks have been employed in several previous papers to instrument for *first* birth timing, to my knowledge, only one previous paper use miscarriages to instrument for birth spacing; Buckles and Munnich (2012) employ this strategy to study the effect of birth spacing on sibling outcomes. In addition, the individual level data on miscarriages used here are provided by hospital registers. As opposed to survey data - which has been the main type of data source used in previous studies employing the fertility shock instrument - I avoid potential misreporting of abortions as miscarriages which, if there is a social stigma towards abortions, is not unlikely. Also, given the large sample size, the data allows me to analyze heterogenous effects by educational attainment.

The policy relevance of this paper is at least twofold. First, fertility behavior - including birth spacing - has shown to be adjustable to changes in the parental leave system. For example, Lalive and Zweimüller (2009) exploit reforms in the Austrian parental leave system to analyze the effect of paid and job protected leave and find that different changes in these components affect both employment of mothers, the number of children, and the spacing of births. Also, the Swedish parental leave system includes an administrative rule carrying incentives to space children in close intervals. This rule, sometimes referred to as the "speed premium", allows parents to retain the same level of benefits for the subsequent child without having to return to work between births to re-establish eligibility, provided that the birth interval is sufficiently short. Previous evaluations of the introduction of the speed premium show that the policy shortened the birth interval for Swedish parents. For instance, Pettersson-Lidbom and Skogman Thoursie (2009) exploit the 1980 introduction of the speed premium to evaluate the impacts of child spacing on children's educational attainment and found that

[3] Specifically, they find that postponing first births reduce the negative effect of children on labor supply, and that the effect of the second child increases with the spacing of two births. This is because, while the negative effect on participation decreases, the positive effects on the probability of working part-time and the negative effect on the probability of working full-time increases. They conclude that women returning to work after the first birth finance child care time increasingly through reductions in market time.

the spacing of births decreased for mothers with strong labor force attachment compared to mothers with less labor market attachment (and therefore had less strong incentives to adjust their fertility behavior). Also, Hoem (1993) shows that parents reacted by increasing their fertility particularly strongly before the end of the eligibility interval. The author further argues that Swedish couples are willing to adjust the timing of their childbearing after first birth to gain short-term economic advantages, but that this may come at a cost of mothers' long term career advancement. The introduction of the speed premium was not intended to speed up further childbearing; it was a rule concerning the practical implementation of the parental leave system. Nevertheless, it highlights the possibility of unintended effects of policies, and given that family policies affect both the number and spacing of births, it is important to understand the relevance of such factors for labor market outcomes.

Second, the spacing of births has been proposed to affect children's outcomes and can thus be viewed as an input into child quality (see e.g. Rosenzweig 1986). For instance, the medical literature provides a non-negligible amount of evidence associating both very short and very long birth intervals with adverse consequences for infant health (Buckles and Munich 2012). Indeed, policy makers in both industrialized and developing countries have advocated greater spacing between births in order to improve maternal and infant health (Buckles and Munnich 2012). If spacing births affect outcomes beyond the health of mothers and children, e.g. mothers' labor market earnings, that is - the household's financial resources - this could imply the existence of additional channels through which the spacing of births affect child well-being or children's educational attainment.

The results from the analyses carried out in the present paper suggest that increasing the time interval between first and second births largely increases women's subsequent career earnings. The effect of a one-year delay of second births, on average, on labor income is positive and increases in magnitude over a 15-year horizon after second birth. Moreover, spacing births are found to increase the probability of re-entering the labor market between births, and to increase the long-run labor force participation among mothers. Small effects are found on the completed number of children, which implies that the impact of spacing births on long-run earnings cannot be entirely explained by fewer children. Spacing births also have positive consequences for women's long run wages; a one-year delay of second births increases the full-time equivalent monthly wage, an effect that is more pronounced among highly educated mothers.

2. Data

The analysis is based on a panel data set created by combining several Swedish administrative registers. Information on the birth year, birth month, and birth order of each of an individual's children is obtained from the multi-generation register, which links all children to their biological parents. Individual level information on annual labor earnings and background variables are then matched to these data. Moreover, I add information from a linked employer-employee data set containing unique identifiers for the establishment at which the individual is employed each year, the first and last calendar month in each year that the worker received income from the specific employer, and the total income earned from the specific employer each year. These registers cover the entire Swedish population aged 16-64 between 1985 and 2007. From the Wage Structure Statistics, I add individual level data on full-time equivalent monthly wages for each person-year-establishment pair. Data on wages are available for the entire public sector and about half of the private sector firms for the time period 1985-2007.

In addition, I match these records with individual level data on miscarriages, which are provided by the National Patient Register (NPR); the inpatient register administered by the National Board of Health. The NPR covers all hospital visits in Sweden during 1987 to 2005 and includes medical information associated with each hospitalization, classified according to the International Classification Standard for Diseases (ICD). Using the ICD-codes from the patient register, I can recover all hospital visits associated with miscarriages.[4] In ICD10 the definition of a miscarriage is a pregnancy loss occurring between the 6th and 24th week of gestation (the total length of a pregnancy is 40 weeks).[5] The order of the pregnancy is not recorded in the NPR, and neither is the gestational age at which the miscarriage occurred. I define the instrument to equal unity if an individual experienced a pregnancy loss between the first and second live births, where the timing of both are drawn from the multi-generation register.

By combining the registers described, I construct a panel data set consisting of individuals with at least two children, who gave birth to their first child between 1985 and 2006, and were aged 21 or older at first birth. Women with twin (or higher order multiple) births and women who experienced a miscarriage before the birth of the first child are excluded from the sample, as are women with recurring miscarriages. The population net of these sample restrictions consists of 642 464 individuals. The number of women in the study sample that experienced a miscarriage between the first two live births sum to 16 540 women. Figure A1 reports the frequency of these miscarriages by year and shows that the number of miscarriages reported in the NPR for the studied sample decreases over time. This is likely due to miscarriages being increasingly treated at outpatient facilities over time; the NPR only records inpatient care and thus women seeking treatment for pregnancy losses at outpatient medical facilities are not included in the data available here. One cause of concern could thus be that only cases with additional complications are treated as inpatient care

[4] The ICD10 code for miscarriage is O03.

[5] The WHO definition of a miscarriage is a pregnancy loss that occurs before 22 weeks of gestation.

as outpatient care becomes more common. However, as shown in Karimi (2013), there is no indication that the miscarriages that are treated as inpatient care are increasingly associated with medical complications over time. This, and other threats to internal validity are discussed in more detail in the next chapter.

Summary statistics for the study sample are reported in Table A1 in the Appendix and show that 2.6 percent of the sample experienced a miscarriage between the first two live births. Furthermore, the women in the sample were on average aged 27.2 years at first birth, and the average spacing between the first two children - measured as the number of years elapsed between first and second birth - is 3.1 years. In Figure A2 in the Appendix, the distribution of child spacing in months is plotted for the analysis sample and shows that there is considerable variation in the time interval between the first two births and the overwhelming majority of women have their first two children within 50 months from each other. Moreover, as seen in Figure A3, the majority of women who experienced a miscarriage between the first and second live births gave birth to their second child already within one year after the pregnancy loss.

Income data is available from 1985 through 2007, and I follow each individual for at most up to 15 years after second birth; the length of the follow-up period varies with birth cohort due to the time series not being long enough for women who gave birth in later years. The main dependent variable used in the empirical analysis is the annual labor income in 1000s SEK, expressed in 2008 prices (deflated using the Consumer Price Index). Since labor income reflects both earnings and work hours, a second dependent variable analyzed is the full-time equivalent monthly wage in natural logs. Note that since monthly wages reflect full-time equivalents, they are comparable to hourly wage rates. While income observations include zeros during non-working spells,[6] data on full-time equivalent monthly wages are available for the entire public sector, but only for around half of the private sector employees and for individuals present at the workplace in the measuring month. Thus, individuals on e.g. work absence or parental leave are not included in the wage data despite having an employment. For missing observations on wages, in years where individuals have an income, I impute missing wages through linear regression on a set of background characteristics, industry affiliation, labor income and pre-birth wages.[7]

3. Institutional Setting

Along with the other Nordic countries, the Swedish parental leave system is quite generous in international comparison and offers a great deal of flexibility for parents. At the time of the introduction of the system in 1974, Swedish parents were entitled to six months of

[6] Labor income does not include parental leave benefits or other transfers.
[7] Sensitivity analyses are carried out without imputing missing values on wages.

paid leave at a compensation rate of 90 percent of previous earnings. Following the introduction, entitlement to paid leave was extended sequentially and the system now offers 16 months of paid parental leave for each child, of which 13 months are compensated at a rate of 80 percent of previous earnings. The remaining three months entitles a fixed lower rate of compensation. In order to receive wage-replaced benefits, parents must have been employed for at least 240 days before birth. This work requirement has likely contributed to the high female labor force participation rates observed in the Nordic countries (Jaumotte 2004, Baker and Milligan 2008, Han et al. 2009, Waldfogel 1998). In 1995, one month of paid leave was earmarked to each parent as a means to increase fathers' share of parental leave, and an additional month was reserved for each parent in 2002. Parents can be on full-time leave for the child's first 18 months of life, with job protection. Furthermore, paid leave can be used until the child turns eight years old, and parents have the right to reduce working hours with up to 25 percent until the child's eighth birthday.

In addition, the system includes an administrative rule sometimes called the 'speed premium'. Before the introduction of the speed premium, women had incentives to postpone subsequent births until eligibility of wage-replaced benefits had been re-established. During the 1970s, however, it became legal practice for parents to keep the level of income compensation paid after one birth during the leave for a subsequent birth, provided the two births were sufficiently close. This interval was initially quite short, but was extended in 1980 to 24 months and extended again to 30 months in 1986. Thus, there are short-term economic incentives for parents to space their children in short intervals, which could result in substantially prolonged work absences.

The average birth spacing in Sweden has decreased over the past decades. In Figure A4 in the Appendix, the average spacing in months between the first two children is graphed for cohorts of mothers who gave birth to their second child in 1970 to 1995. The two horizontal lines represent the introduction of the first and second speed premiums, respectively. There is a sharp decrease in the spacing of births between these two points in time. Moreover, Figure A5 in the Appendix plots kernel density estimates for the likelihood of having the first two children within 30 months for the cohort of women who gave birth to their second child before the extension of the speed premium in 1986 (second child born 1985) and for the cohort of women who gave birth after the extension (second child born in 1987). As seen from Figure A5, there is a clear shift in the distribution of births occurring within a 30 month interval from 1985 to 1987, tentatively suggesting that the rule had an effect on fertility spacing behavior. Previous evaluations of the speed premium suggest that the policy shortened the birth interval for Swedish parents. For example, Pettersson-Lidbom and Skogman Thoursie (2009) exploit the 1980 extension to evaluate the impacts of child spacing on children's educational attainment, and found that the spacing of children decreased for mothers with strong labor force attachment compared to mothers with less labor market attachment (and therefore less strong incentives to adjust their spacing). Also, Hoem (1993) shows that parents reacted by increasing their fertility particularly strongly before the end of the eligibility interval. The author further argues that Swedish couples are willing to adjust the timing of their childbearing after the first birth to gain short-term economic advantages, but that this may come at a cost to mothers' long-term career advancement. Thus, public policies have the potential to adjust individuals' fertility behavior, also in cases where the policies are not aimed at changing fertility behavior. Regarding family policy, Björklund (2006) studies whether the family policies introduced in Sweden from the mid 1960s to 1980 affected fertility by comparing the fertility behavior with neighboring countries where family policies were not extended as much as in Sweden. The results found suggest that the extension of family policies raised the level of fertility, shortened the spacing of births and induced fluctuations in the period fertility rates.

3.1. Graphical Evidence. This section graphically illustrates the employment- and income patterns over the life cycle for women with varying birth spacing to serve as a background to the empirical analysis, and descriptively highlight potential differential patterns of employment by birth spacing. Figure 2.1 shows the employment status, by years since first birth, for women with different spacing between first and second births. Specifically, women are divided into three groups: women with less than 24 months between the first two births; women with 24-29 months between the first two births; and women with 30-50 months between the first two births. Employment is indicated by a dummy variable defined to equal unity if labor income exceeds one basic amount. Figure 2.1 reveals that all three groups participate in the labor market to an equal extent before the birth of the first child. At the time of first birth, all groups of women withdraw from the labor market to some extent. However, there is considerable variation in the length of withdrawal between women with different birth spacing. In particular, women with very short birth intervals appear to return to the labor market only after the birth of the second child, while women with somewhat longer spacing (24-29 months) re-enter the labor market between births to a larger extent. For women with the longest birth spacing, the fraction of mothers re-entering the labor market between births is substantially higher compared to the groups of women with shorter birth spacing. The second drop in employment status for women with birth intervals of 24-29 and 30-50 months, respectively, are most likely associated with second births, but all groups of women re-enter the labor market to a large extent after the second child is born. Interestingly, women with the shortest birth intervals participate in the labor market to a lower extent even 15 years after first birth compared to women with longer birth intervals.

The pattern revealed for employment status is also evident for labor earnings; Figure 2.2 shows the evolution of labor earnings over the years since first birth for the same three groups of women as in the previous figure. Average labor income does not vary across the groups before motherhood. As with employment status, however, there is considerable variation after birth. Women with the longest birth intervals earn higher incomes in the years between first and second birth compared to women with short intervals, and have permanently higher earnings in the long run. The patterns highlighted in Figures 2.1 and 2.2 largely mirror the rules of the Swedish parental leave system described in the previous section. Mothers who have the opportunity to stay on extended leave due to short birth spacing without having to re-establish eligibility are perhaps not unlikely to do so. Although one should be careful to interpret these graphs as causal evidence, the patterns revealed suggest that spacing births may matter for the long-run labor market attachment of women, for example by allowing women to re-enter the labor market between births, gain labor market experience and avoid lengthy interruptions in connection with childbearing. The next section empirically investigates this question by means of OLS estimation as well as 2SLS estimation of the effect of spacing births on women's subsequent labor market outcomes.

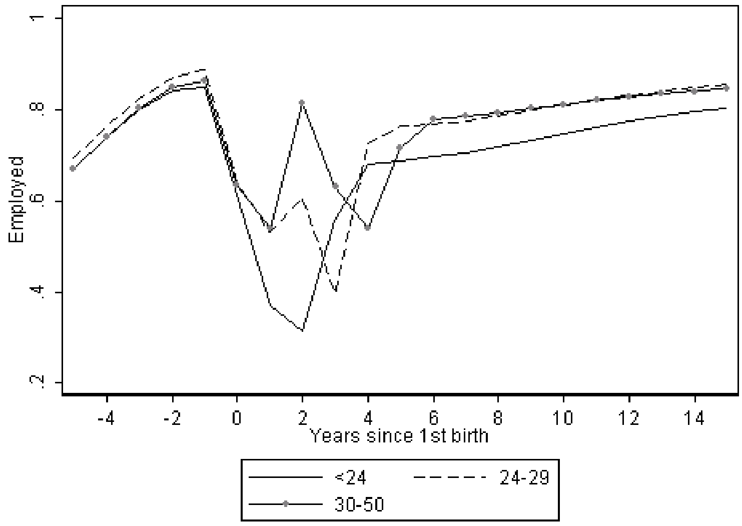


FIGURE 2.1. Employment status by years since first birth for women with varying child spacing.

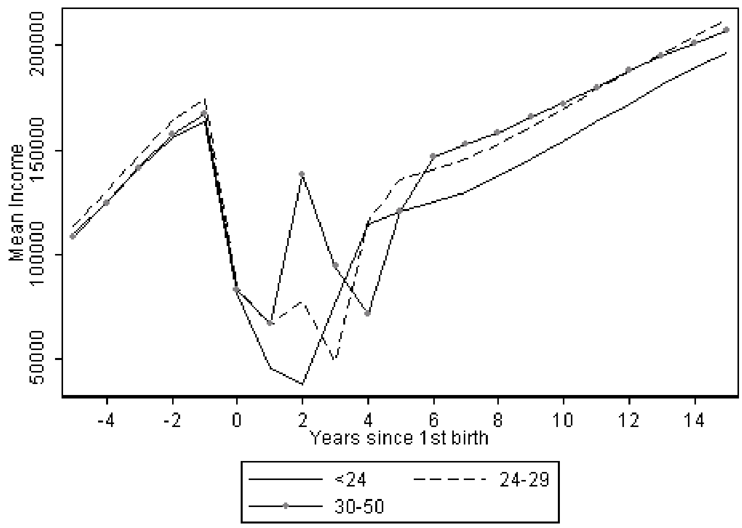


FIGURE 2.2. Labor income by years since first birth for women with varying child spacing.

4. Empirical Strategy

Interest lies in evaluating whether the spacing of births affects the long run labor market outcomes of women. The causal question of interest is summarized by the following equation:

$$y_i = \alpha_0 + \beta S_i + \mathbf{x}_i' \delta_x + \epsilon_i \tag{2.1}$$

where y_i is the labor market outcome of interest and $\mathbf{x}_i'$ is a vector of personal characteristics. The regressor of interest is S_i, and is defined as the time interval, in years, between first and second birth. Ordinary Least Squares (OLS) estimation will yield biased estimates if child spacing is correlated with the error term. For instance, due to heterogeneous preferences for market work and family, some women might invest more effort at work and space their children with longer intervals, or women with higher earnings potential may choose to postpone further childbearing to reduce the costs of childbearing. If mothers with higher earnings potential space their children with longer intervals, the OLS estimator will over-estimate the effect of spacing on labor market outcomes. Alternatively, if women with higher earnings potential are those who have their children within shorter intervals, the OLS estimator will under-estimate the effect of child spacing on career outcomes. In addition, further childbearing could be delayed if women, for some reason or the other, returns to work after first birth, causing a reversed causality problem.

In order to address these potential problems of endogeneity, I make use of the arguably exogenous variation in spacing induced by miscarriages between first and second live births. The First-stage regression equation is thus given by:

$$S_i = \gamma_0 + \gamma_1 M_i + \mathbf{x}_i' \gamma_x + \nu_i \tag{2.2}$$

where $\mathbf{x}_i'$ contains the same control variables as in Equation (2.1), and M_i is a dummy variable taking the value one if individual i experienced a miscarriage between the first and the second birth. 2SLS estimation then yields the effect of birth spacing for women who spaced their children in longer intervals due to experiencing a pregnancy loss after first birth.

4.1. Internal Validity. In order for miscarriages to be a valid instrument for child spacing, miscarriages must affect the time interval between births, i.e., there must exist a First-stage relationship, and the instrument should not be correlated to the error term in Equation (2.1). The first assumption can be tested directly, and evidence of an existing First-stage relationship is shown in the subsequent section. The exclusion restriction, however, cannot be tested and must be argued for on a case-by-case basis.

One potential concern regarding the exclusion restriction is that the health of women who experience pregnancy loss is worse on average compared to women who do not, or that women who miscarry differ in terms of observable characteristics from women that do not experience miscarriage. This critique against the miscarriage instrument is lifted in e.g. Wilde et al. (2012). The clinical definition of a miscarriage is a pregnancy loss occurring within 22 weeks of gestation, and the medical literature reports that miscarriages are common and frequently occurring fertility shocks; one in four of all women who become pregnant is estimated to experience pregnancy loss. Moreover, the vast majority of miscarriages are early, occurring before 12 weeks of gestation (Regan and Rai 2000). Miscarriages have been associated with some extreme behaviors, such as heavy or regular alcohol-, tobacco- or drug use during pregnancy (see e.g. Garcia-Enguidanos et al. 2002, Maconchie et al. 2007, for reviews). However, in their review of the medical literature, Garcia-Enguidanos et al. (2002) argue that the two risk factors recognized by all studies included in their review are chromosomal rearrangements and uterine malformations.

I address these potential issues by examining health differences between women who do and do not experience a miscarriage for my sample. To this end, I make use of the detailed information covered in the National Patient Register, and examine whether there are differences in the average number of hospitalizations (for all medical reasons) during the five year period *before* becoming mothers. I can also break down the hospital visits by diagnosis code, and study if women who later experienced a pregnancy loss visited the hospital for other reasons compared to those who did not experience pregnancy loss. Table 2.1 thus reports correlations between the number of pre-natal hospital visits (and hospital visits due to each diagnosis category, respectively), and the spacing of births (S_i) as well as with miscarriage (M_i). As seen, the spacing of births is positively correlated with the number of pre-natal hospitalizations, whereas there is no correlation between miscarriage and pre-natal hospitalizations. Breaking down the hospital visits by diagnosis category, there are some significant correlations between miscarriage and a few of the diagnoses, albeit small. Nevertheless, in all estimations I will control for the number of pre-natal hospitalizations as well as the number of hospitalizations due to each diagnosis category listed in Table 2.1.

Hospitalizations represent the most severe health issues, and one worry is consequently that there are differences in the average health that is not captured by hospital visits. If women who miscarry have worse health, on average, the 2SLS estimates of the effect of birth spacing would be biased downwards.

In order to further assess the validity of the instrument, Table 2.2 reports correlations between the independent variable of interest, S_i and the instrument M_i , respectively, and a number of individual characteristics. As seen from column 1 of Table 2.2, women who are younger at first birth, with lower educational levels and who have a non-Nordic background tend to space their children at shorter intervals. These fairly strong associations seen between child spacing and background factors are, however, also existent between miscarriage and the same background factors, albeit less strong. Women who are born in one of the

Nordic countries and who are older at labor market entry tend to experience miscarriage to a somewhat larger extent. It thus seems as women who miscarry are not a random selection of the population. However, there is a large chance that these correlations are driven by age at first birth, since female fecundity is known to decline with age. In Table 2.3, therefore, I regress miscarriage onto all of the variables presented in Table 2.2, with and without including dummies for the age at first birth and cohort dummies, presented in columns 1 and 2, respectively. As seen from Table 2.3, including dummies for cohort and age at first birth reduces the magnitude of the coefficients on the background variables considerably, and the statistical significance disappears from all but two variables: the number of pre-natal hospitalizations is now weakly significant, but almost zero. The coefficient on the dummy variable for residing in a large city before parenthood is still statistically significant and estimated to be positively associated with miscarriage by 0.33 percent. In all regressions I will control for the education, birth cohort and age at first birth.

Furthermore, I carry out an additional assessment of the validity of the independence assumption by estimating the reduced form equation on labor income (expressed in 1000s SEK) in years prior to first birth. The results from this falsification test are presented in Table A2 in the Appendix and show that there are no differences in labor earnings before becoming parents between women who later had a miscarriage and women who did not; the estimated coefficients are small in magnitude and not statistically significant.

Another potential issue that is important to raise is that miscarriages reported in the inpatient care record may be of a more severe nature than miscarriages treated at outpatient establishments. For example, the cases referred to hospitals may be cases with medical complications, or pregnancies who have reached a higher gestational age at miscarriage. This potential problem could be increasing with time as more cases are being treated as outpatient care, such that those cases still reported in the NPR at later dates only include the most severe cases (recall that the number of reported miscarriages in the inpatient records declines with time). The NPR includes detailed information about the severity of reported miscarriages for the years 1997-2005. Table A3 in the Appendix reports the type of miscarriage, among all miscarriages reported in the inpatient record during 1997 to 2005 and shows that the vast majority of cases regard miscarriages without additional medical complications. Figure A6 in the Appendix shows the evolution of the type of reported miscarriages over time in the inpatient record, and suggests that, even as cases treated at hospitals become fewer, the vast majority of reported cases are without complications.

Moreover, as the NPR also includes secondary diagnoses for each hospital visit, I can get an additional indication of the severity by examining whether there are any reported comorbidities with the pregnancy loss for those women in my study sample that experienced miscarriage. The results (not shown) suggest that 96.5 percent of the cases did not have any co-morbidities reported. Furthermore, 3.14 percent were reported to have one co-morbidity, of which the most common diagnose was pregnancy-related or related to diseases of the genitourinary system (the results are available upon request). Thus, there is no strong evidence that the miscarriages reported in the NPR are overwhelmingly associated with additional complications. Nevertheless, were this to be the case, the 2SLS estimates will be biased downwards.

A final concern is that miscarriages might affect women's psychological well-being such that labor market outcomes are directly (adversely) affected, violating the exclusion restriction. Regarding the latter issue, as shown in Figure A3, the overwhelming majority of women who miscarry give birth to a child within one year after pregnancy loss. Potential psychological distress resulting from miscarriages is thus not likely to be of great concern, at least not in the long run. However, if miscarriages nevertheless affect mental well-being, the instrumental variables estimate would again be downward biased.

TABLE 2.1. Correlations between pre-natal hospitalizations, child spacing and miscarriage

Dependent variable	Spacing	Miscarriage
Number of pre-natal hospitalizations	0.0262***	-0.0006
	(0.0039)	(0.0004)
By diagnosis		
Infectious	0.0818**	-0.0051**
	(0.0323)	(0.0023)
Tumors and Neoplasms	-0.0712**	-0.0018
	(0.0347)	(0.0029)
Diseases of the blood(-forming) organs	0.1260*	0.0006
	(0.0763)	(0.0068)
Endocrine	0.1037**	-0.0003
	(0.0432)	(0.0034)
Mental behavioral	0.1350***	-0.0023
	(0.0286)	(0.0020)
Nervous system	-0.0145	-0.0003
	(0.0460)	(0.0038)
Eye	-0.0450	0.0058
	(0.0683)	(0.0064)
Ear	-0.0180	-0.0004
	(0.0691)	(0.0055)
Circulatory system	-0.0385	-0.0086**
	(0.0465)	(0.0034)
Respiratory	0.0794***	-0.0041**
	(0.0227)	(0.0017)
Digestive system	-0.0343*	-0.0067***
	(0.0185)	(0.0014)
Skin	0.0707	0.0053
	(0.0499)	(0.0047)
Musculoskeletal	-0.0027	-0.0043**
	(0.0257)	(0.0019)
Genitourinary system	0.1152***	-0.0015
	(0.0152)	(0.0012)
Congenital malformations	0.0761	-0.0027
	(0.0559)	(0.0041)
Symptoms not classified elsewhere	0.0774***	-0.0023*
	(0.0174)	(0.0013)
Factors associated with health status	0.1215***	-0.0048
	(0.0457)	(0.0030)
External causes	0.0091	-0.0058***
	(0.0180)	(0.0013)
Substance or alcohol use	0.6065***	-0.0101
	(0.1278)	(0.0073)
Observations	642464	

NOTES.— Each coefficient reported in the table is obtained from a separate regression of the dependent variable on the control variable listed in each row plus a constant term. Standard errors are presented in parentheses. *p<0.1, **p<0.05 ***p<0.01.

TABLE 2.2. Correlations between background variables, child spacing and miscarriage

	Spacing	Miscarriage	Observations
Age at first birth	-0.0686***	0.0003***	642464
	(0.0006)	(0.0001)	
Non-Nordic background	0.4423***	-0.0072***	642464
	(0.0092)	(0.0005)	
Compulsory schooling	0.2197***	0.0001	642464
	(0.0115)	(0.0008)	
High school	0.1545***	0.0009**	642464
	(0.0049)	(0.0004)	
College	-0.2114***	-0.0009**	642464
	(0.0048)	(0.0004)	
Age at labor market entry	0.0036***	0.0007***	584128
	(0.0004)	(0.0000)	
Live in large city (pre 1st birth)	0.0368***	0.0035***	574447
	(0.0061)	(0.0005)	

NOTES.— Each coefficient reported in the table is obtained from a separate regression of the dependent variable on the control variable listed in each row plus a constant term. Standard errors are presented in parentheses. *p<0.1, **p<0.05 ***p<0.01.

TABLE 2.3. Correlations between background variables and miscarriage

	(1)	(2)
Non-Nordic	-0.0046***	0.0009
	(0.0009)	(0.0009)
High school	-0.0037***	-0.0012
	(0.0009)	(0.0010)
College	-0.0114***	-0.0012
	(0.0010)	(0.0010)
Pre-natal hospitalizations	-0.0009**	0.0006
	(0.0004)	(0.0004)
Live in large city (pre 1st birth)	0.0033***	0.0033***
	(0.0006)	(0.0006)
Age at labor market entry	0.0012***	-0.0000
	(0.0000)	(0.0001)
Birth cohort dummies		✓
Dummies for Age at first birth		✓
Observations	571532	571532

NOTES.— Columns (1) and (2) present results from the regression of miscarriage incidence onto all the control variables listed in the table, with and without including dummies for mothers' birth year and age at first birth, respectively. Standard errors are presented in parentheses. *p<0.1, **p<0.05 ***p<0.01.

5. Results

The empirical analysis includes the estimation of the effect of child spacing on annual labor income and labor market participation after second birth. The baseline model is specified by Equation (2.1), which is estimated with OLS and 2SLS, where the occurrence of miscarriage between the first two live births is used as an instrument for child spacing. The first dependent variable measures annual labor income in 1000s SEK. Separate yearly regressions are performed for each year after the second birth, starting from year one after second birth, up to at most 15 years after second birth.[8] The second outcome variable is defined to capture labor market participation, and is defined to equal unity if labor income exceeds one basic amount.

One proposed channel for a potential effect of birth spacing on subsequent labor market outcomes is through the accumulation of pre-birth labor market experience and human capital. Women who postpone second birth are likely to return to work between births to a greater extent than women with shorter birth intervals. Returning to work between births may also imply avoiding negative signals to the employer about a low work commitment. In turn, this could affect female workers' opportunities for advancement and/or on-the-job training offers. All these factors have the potential to affect the long-run attachment to the labor market, perhaps both on the extensive and the intensive margin. To analyze whether pre-birth labor market experience is affected by increasing the time interval between births, I estimate the impact of spacing births on the probability to return to work between first and second birth, and on the total income earned in the interim between the two first births.

Moreover, birth spacing may affect subsequent fertility. This could imply that a potential effect of birth spacing partly reflects an altered family size. Therefore, I also display results from sensitivity analyses examining whether completed fertility is a main driving channel of any effects found on income and participation. Lastly, I analyze potential consequences of spacing births on women's subsequent wage rates.

5.1. The Effect of Spacing Births on Labor Income. Table 2.4 reports the results from an OLS estimation of the First-stage relationship given by Equation (2.2), adding control variables stepwise. The coefficient on the instrument is, as expected, positive and reasonable in magnitude. Having a miscarriage before second birth delays second birth such that the spacing between the first two children is increased by 11.4 months, on average (0.953 years). Adding dummies for the individuals' birth year and dummies for age at first birth, educational level and a dummy for non-Nordic background reduces this estimate somewhat (depicted in column 2); pregnancy loss is now estimated to yield a delay of second birth by

[8] Due to different lengths of the time series of income for different birth cohorts, I cannot follow all individuals for the entire 15-year horizon, so the sample size will decrease with each yearly regression.

around 10.8 months. This estimate is robust to including the number of pre-natal hospitalizations (column 3) and to including the number of hospital visits for each diagnosis category listed in Table 2.1 (column 4). Thus, the First-stage effect is non-negligible and robust to including control variables. Table 2.5 reports the results from the OLS and 2SLS estimation

TABLE 2.4. The effect of miscarriage on child spacing: OLS estimates of the First-stage relationship

Dependent variable	Child spacing			
	(1)	(2)	(3)	(4)
Miscarriage	0.953***	0.901***	0.900***	0.901***
	(0.017)	(0.017)	(0.017)	(0.017)
High school		0.007	0.011	0.012
		(0.012)	(0.012)	(0.012)
College		-0.047***	-0.041***	-0.039***
		(0.012)	(0.012)	(0.012)
Non-Nordic background		0.421***	0.427***	0.429***
		(0.009)	(0.009)	(0.009)
Pre-natal hospitalizations			0.046***	0.005
			(0.004)	(0.007)
Additional controls				
Cohort dummies		✓	✓	✓
Dummies for Age at first birth		✓	✓	✓
Pre-natal hospitalizations by diagnosis				✓
Observations	642464	642464	642464	642464

NOTES.— The outcome variable measures the number of years elapsed between the births of the first and second child. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

of the effect of child spacing on subsequent labor income. To conserve space, the table only reports estimates for 2, 4, 6, 8 and 10 years after second birth. The full set of yearly 2SLS estimates of the effect of child spacing on labor income is presented in Figure A7 in the Appendix. Table 2.5 shows that postponing second birth by one year, on average, increases labor earnings, both when estimated in OLS and 2SLS. Furthermore, the positive effect is almost monotonously increasing with time since birth. In years 2 and 4 after birth, the OLS estimate is larger in magnitude compared to the 2SLS estimate, however, in the longer run this pattern reverses. Figure A7 graphs the full set of yearly 2SLS estimates of the effect of birth spacing on labor income and shows that there is no effect on income the year after birth, to then become positive and increasingly larger over time.

One feature of the IV strategy employed here is that we can investigate who are driving the estimated effect of birth spacing, by studying the difference in the cumulative distribution functions of birth spacing between women who did and did not experience a miscarriage; as shown by Angrist and Imbens (1995), this difference is the weighting function of the average causal effect in the case of a treatment with variable treatment intensity, normalized to sum to one.[9] The CDF:s of birth timing are depicted in Figure 2.3 and shows that women who miscarry always have longer spacing intervals, but most of the effect seems driven by women who move to a birth spacing of three years. From the graphical analysis provided in Section 3, women with less than two years between first and second births were indicated to have the lowest labor market participation rates and lowest incomes after childbearing.

Figure A8 in the Appendix depicts the yearly reduced form estimates on income, which are large in magnitude, positive and statistically significant for the entire follow-up period with the same pattern as obtained by 2SLS estimation of the second-stage relationship.

TABLE 2.5. The effects of child spacing on subsequent labor income

	OLS	2SLS	Observations
Outcome measured at			
Birth year +2	6.317***	2.552***	556883
	(0.078)	(0.922)	
Birth year +4	8.201***	5.759***	495000
	(0.094)	(1.052)	
Birth year +6	9.662***	10.355***	437274
	(0.117)	(1.206)	
Birth year +8	9.869***	11.432***	384057
	(0.136)	(1.338)	
Birth year +10	9.838***	12.731***	329452
	(0.165)	(1.538)	

NOTES.— The outcome variables measures labor earnings in 1000's SEK at 2, 4, 6 and 8 years after second birth, respectively. Labor earnings are deflated with CPI (2008 prices). Included covariates are the number of pre-natal hospitalizations, a dummy for non-Nordic background, dummies for high school education and college education, a full set of dummies for age at first birth and dummies for cohort. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

The income measure used so far includes women with zero earnings. However, spacing births may also affect the extensive margin of labor supply. In Figure 2.4, coefficients from yearly 2SLS estimates of the effect of birth spacing on subsequent participation are depicted. Participation is defined as earning a labor income exceeding one basic amount. As seen from Figure 2.4, spacing the first two births in a longer interval leads to an increase in the probability to participate in the labor market; aside from an initially negative effect in the years immediately after second birth, a one year delay of second birth causes an increase in the probability to participate by around 2 percentage points, an effect that stays rather constant throughout the follow-up period. The effect of spacing births on labor income is thus found to be positive, sizeable and increasing in magnitude by time since birth. One possible

[9] The treatment here is child spacing, which can take on a range of positive values.

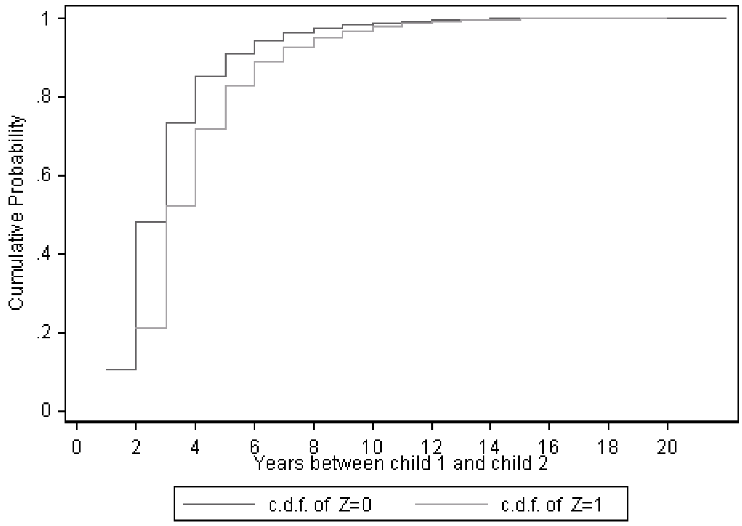


FIGURE 2.3. Cumulative distribution functions (CDF:s) for birth spacing for women who experienced a pregnancy loss and women who did not, respectively.

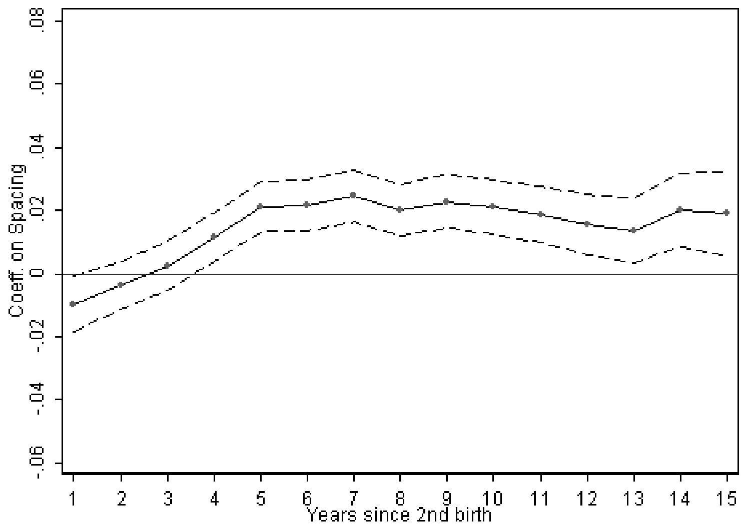


FIGURE 2.4. 2SLS estimates of the effect of spacing births on the probability to participate in the labor market after second birth and corresponding 95-percent confidence intervals.

explanation for this finding is that postponing second birth induces mothers to re-enter the labor market between births to a greater extent, as indicated by the graphical evidence presented above, thereby gaining more labor market experience before the birth of the second child. Thus, spacing births in a longer interval could potentially imply a shorter consecutive absence from work for child care reasons, and a stronger labor market attachment as a result.

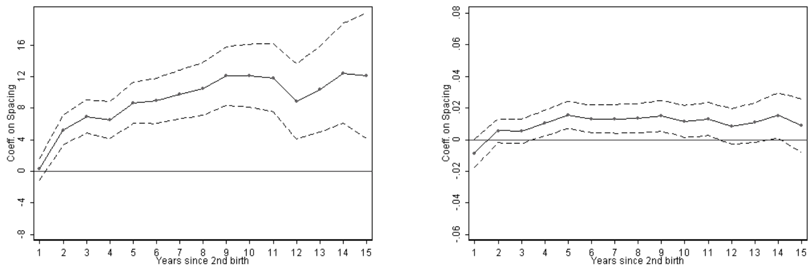
Table 2.6 reports results from 2SLS estimation of the effect of spacing births on the likelihood of returning to work between the first two births and on the total income earned in the years between first and second birth, respectively. The former variable is defined as a dummy variable that equals unity if individual i has at least one year of work between births that yields an income exceeding two basic amounts. The results suggest an increase in the probability to return to work between the first and second child by 18.4 percentage points, and an increase in the total income earned between births by around 130,000 SEK, on average. Since the average earnings of women in the sample was almost 164,000 SEK in the year prior to first birth, the estimate reflects almost one extra year of work in the time period between the first and second births.

TABLE 2.6. The effects of child spacing on the probability to return to work between births and on total income earned between births

Dependent variable	Return to work	Labor income
Specification	OLS	2SLS
Years between child 1 and child 2	0.184***	130.032***
	(0.004)	(2.669)
Control variables		
Personal characteristics	✓	✓
Cohort dummies	✓	✓
Dummies for Age at first birth	✓	✓
Pre-natal hospitalizations	✓	✓
Observations	642464	619686

NOTES.— The outcome variables measures the natural log of the sum of labor income from the year of first birth to the year of second birth. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

5.2. Sensitivity Analysis. The effect of spacing births on subsequent labor income were found to be relatively sizeable. Potentially, they could be mediated by an effect on completed fertility; if postponing second births leads to a lower completed fertility, part of the positive effect found on income could simply reflect more hours worked due to having fewer children. To analyze how the effect of birth spacing affects earnings, without allowing the possibility of this effect being mediated through the number of children, I re-estimate Equation (2.1) for labor income and participation, respectively, in each year following the second birth, up to 15 years later. This time, however, observations are successively dropped for individuals from the year of third birth onwards. Thus, individuals in the sample who subsequently have a third child are dropped from the sample in the year that they give birth to their third child



(A) Labor income (B) Participation

FIGURE 2.5. Coefficients from yearly 2SLS regressions of the effect of birth spacing on labor income (left) and participation (right), and corresponding 95-percent confidence intervals. The sample is censored from the year that individuals have a third child onwards.

and excluded from the estimations thereafter. The estimated coefficients on birth spacing from these estimations are presented in Figure 2.5, and show a strikingly similar pattern as the one observed for the uncensored sample. Nevertheless, both the effect on income and on participation are smaller in magnitude, but the analysis still shows sizeable positive effects on both earnings and participation. Thus, while the number of children seem to mediate some of the effect, completed fertility is clearly not the main driving mechanism of the effect of spacing births on subsequent labor market outcomes.

As an additional sensitivity analysis, Table 2.7 displays estimates from a 2SLS estimation of the effect of birth spacing on the total number of children born to a woman by 2007 (which is the latest for which I can observe childbearing for the sample). For many women in the sample this represents completed fertility. In column 2, however, I restrict the sample to include only mothers who were 45 years of age or older in 2007, such that this sample includes women who most likely have completed their childbearing by 2007. As seen from Table 2.7, spacing births in one year longer intervals, on average, reduces the *number* of children born to a woman by 0.041 and 0.046 in the full and restricted sample, respectively. The average number of children in the full sample is about 2.4, so this effect is relatively modest.

TABLE 2.7. The effects of child spacing on completed fertility

Dependent variable	Number of children in 2007	
	Full sample	Aged$\geq$ 45 in 2007
Specification	2SLS	2SLS
Child spacing	-0.041***	-0.046***
	(0.005)	(0.007)
Control variables		
Personal characteristics	✓	✓
Cohort dummies	✓	✓
Dummies for Age at first birth	✓	✓
Pre-natal hospitalizations	✓	✓
Observations	642464	152982

NOTES.— The outcome variables measures the total number of children born by the end of 2007 (representing completed fertility). Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

5.3. Consequences for Wages. The findings so far suggest that spacing births have sizeable effects on labor income after second birth, and increases the probability to return to work between first and second birth. In this section, the aim is to evaluate whether this increased labor market experience between births affects wages of mothers in the medium- and long run.

In Table 2.8, results are presented from the analysis of the effect of child spacing on the log of full-time equivalent monthly wages 5, 10 and 15 years after second birth. The results show that postponing second births by one year increases wages by around 3 percent 15 years after second birth. Thus, spacing children in longer intervals has a sizeable effect on women's subsequent wage growth. In addition, as seen in Table 2.9, wages are more positively affected for highly educated women (some college or more) compared to lower educated women. This result is in line with previous studies suggesting that highly educated women benefit the most from postponing motherhood (see e.g. Miller 2011). For second births, Troske and Voicu (2012) find that highly educated women have incentives to delay subsequent births as well as first births since women with higher education face larger effects of the second child on their labor supply, with these effects growing more slowly with the spacing of second birth.

TABLE 2.8. The effects of child spacing on subsequent monthly full-time equivalent wage

	OLS	2SLS	Observations
Outcome variable: log wage			
Birth year +5	0.029***	0.034***	374094
	(0.000)	(0.002)	
Birth year +10	0.025***	0.032***	267124
	(0.000)	(0.003)	
Birth year +15	0.023***	0.029***	134668
	(0.001)	(0.005)	

NOTES.— The outcome variables measures the full-time equivalent monthly wage in 1000s SEK, measured at 5, 10 and 15 years after second birth, respectively. Wages are deflated with CPI (2008 prices). Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

TABLE 2.9. Heterogeneous effects on wages by educational level

Sample	Low educated		Highly educated	
Specification	OLS	2SLS	OLS	2SLS
Outcome variable: log wage				
Birth year +15	0.021***	0.025***	0.026***	0.033***
	(0.001)	(0.005)	(0.001)	(0.008)
Control variables				
Personal characteristics	✓	✓	✓	✓
Cohort dummies	✓	✓	✓	✓
Dummies for Age at first birth	✓	✓	✓	✓
Pre-natal hospitalizations	✓	✓	✓	✓
Observations	75714	75714	58954	58954

NOTES.— The outcome variables measures the full-time equivalent monthly wage in 1000s SEK, measured at 15 years after second birth. Wages are deflated with CPI (2008 prices). Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

6. Conclusions

This paper adds to the literature on the timing of births by analyzing whether spacing births affects women's long-run labor earnings, participation and wages. To account for the possible endogeneity of fertility timing decisions and labor market outcomes, I exploit arguably exogenous variation in child spacing resulting from miscarriages between the first two live births. These random fertility shocks delay births and thereby extend the spacing between births. The analyses are based on Swedish individual level register data, which allows me to follow income- and wage trajectories of mothers for up to 15 years following the second birth, as well as a number of years before first birth. Moreover, data on miscarriages are provided by hospital registers, which avoids potential bias associated with misreporting abortions as miscarriages.

I find that spacing births substantially increases mothers' income earned from market work, an effect that becomes increasingly larger in magnitude over the 15-year follow-up horizon after second birth. For labor market participation, I find a positive effect of around 2 percentage points; an effect that remains rather constant throughout the follow-up period. While the total number of children born to a woman is somewhat decreased by spacing births, fewer children is not the main driving mechanism for the large and positive income effects of spacing births. Rather, a more likely explanation is that spacing births allows women to re-enter the labor market between births and thereby to avoid a lower subsequent attachment to the labor market. Finally, spacing births are also found to have positive consequences for long-run wages, with this effect being more pronounced for highly educated mothers.

The results provided in this paper have important policy implications as changes in family policies have previously shown to have unintended consequences for both the number of children as for the spacing of births. In addition, spacing births has also been shown to impact sibling outcomes through a number of different channels. The results provided in this paper thus suggest an additional channel - the household's financial resources - through which spacing births could potentially impact children's outcomes.

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Appendix

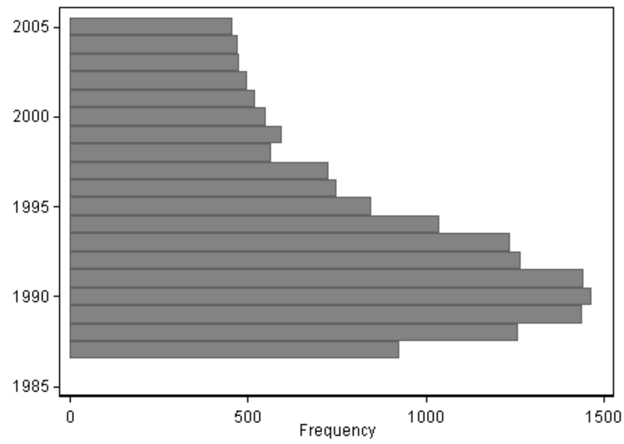

FIGURE A1. Number of reported miscarriages by year in the National Patient Register. The sample consists of women who gave birth to their first child between 1988 and 2006, aged 21 or older at first birth and who had two or more children.

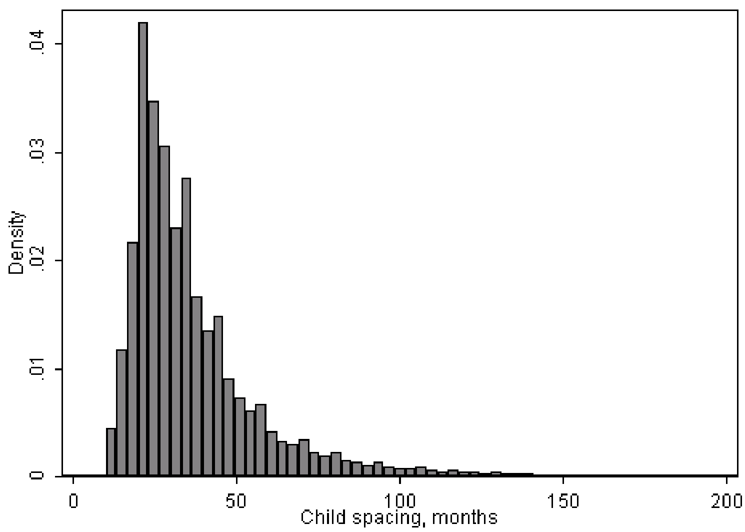

FIGURE A2. Distribution of child spacing in months. Child spacing is defined as the number of months elapsed between the births of the first and second child.

TABLE A1. Summary statistics

	Mean
Miscarriage	0.0257
	(0.158)
Age at first birth	27.20
	(3.973)
Child spacing	3.115
	(1.943)
Number of children in 2007	2.367
	(0.657)
Non-Nordic background	0.123
	(0.328)
Compulsory schooling	0.0675
	(0.251)
High school	0.480
	(0.500)
College	0.453
	(0.498)
Age at labor market entry	25.27
	(5.990)
Live in large city (pre 1st birth)	0.212
	(0.409)
Pre-birth labor income (SEK)	163 731.9
	(99619.8)
Pre-birth monthly wage (SEK)	18 193.6
	(5208.5)
Observations	642464

NOTES.— The table reports means and standard deviations in parentheses. The sample consists of mothers who have at least two children, gave birth to their first child between 1985 and 2006, and for whom income is observed 15 years after the birth of the second child.

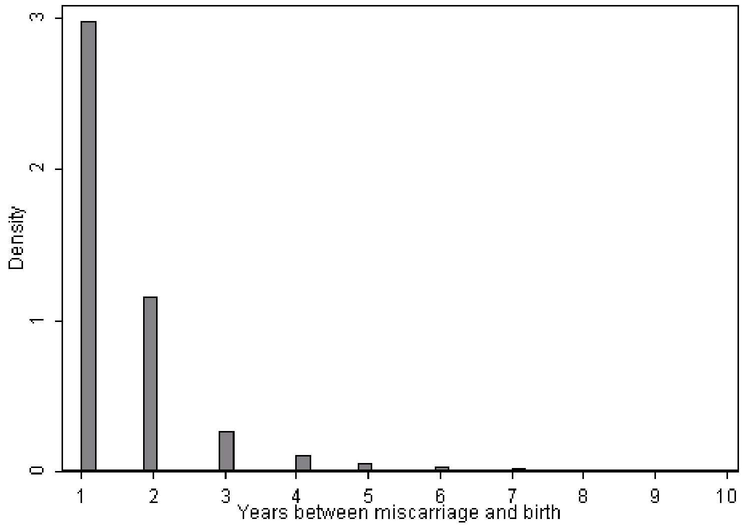

FIGURE A3. Distribution of years elapsed between miscarriage and birth of second child.

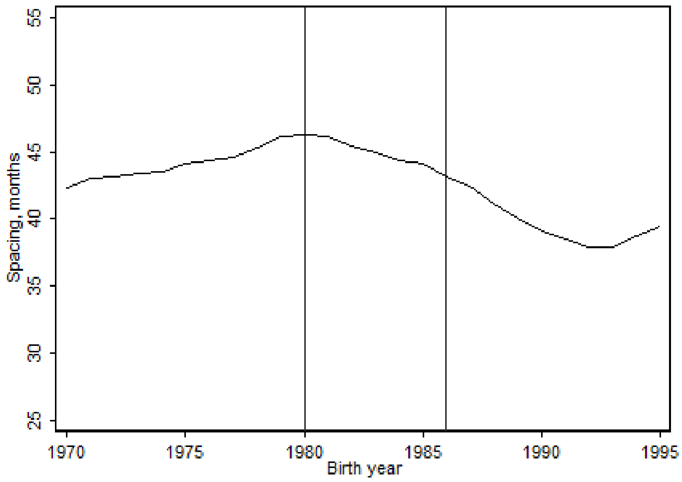

FIGURE A4. Average number of months between the birth of the first and second child by (second) birth cohort. The two vertical lines represent the introduction of the "speed premium" and the extension of the eligibility interval from 24 to 30 months, respectively.

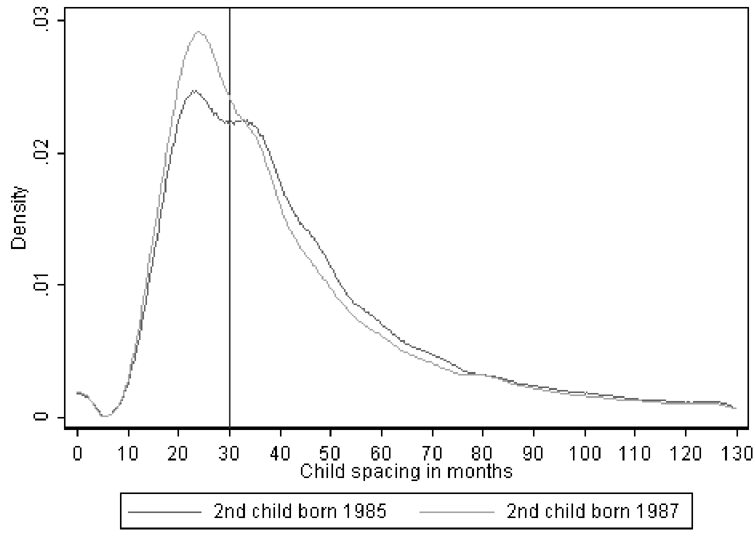

FIGURE A5. Kernel density estimates: the likelihood of giving birth to the first two children within a 30-month interval, by eligibility status for the speed premium. Eligible: second child born 1987.

TABLE A2. Falsification test: Reduced form estimates of the effect of miscarriage on pre (first) birth income

	OLS	Observations
Outcome measured at		
Year of first birth	0.199	610187
	(0.532)	
Year of first birth -1	0.991	574447
	(0.704)	
Year of first birth -2	0.720	532724
	(0.687)	
Year of first birth -3	-0.366	495638
	(0.719)	
Year of first birth -4	-0.351	459376
	(0.714)	
Year of first birth -5	-1.151	423337
	(0.723)	

NOTES.— Included covariates are the number of pre-natal hospitalizations, a dummy for non-nordic background, dummies for high school and college, a full set of dummies for age at first birth and a full set of dummies for cohort. *p<0.1, **p<0.05 ***p<0.01.

TABLE A3. Severity of miscarriages, 1997-2005. Source: Karimi (2013).

	Mean
Incomplete with complication	0.108
	(0.311)
Complete with complication	0.0183
	(0.134)
Incomplete without complication	0.676
	(0.468)
Complete without complication	0.197
	(0.398)
Observations	26120

NOTES.— Means and (standard deviations).

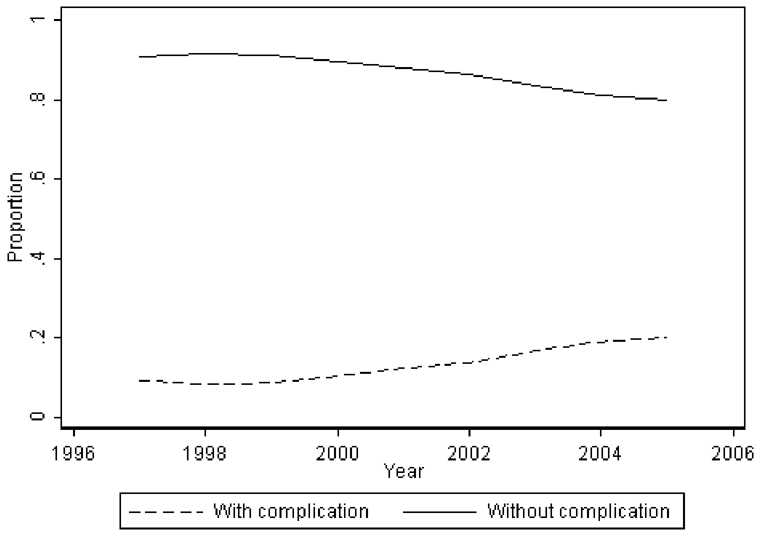

FIGURE A6. Proportion of miscarriages with and without complications, 1997-2005. Source: Karimi (2013).

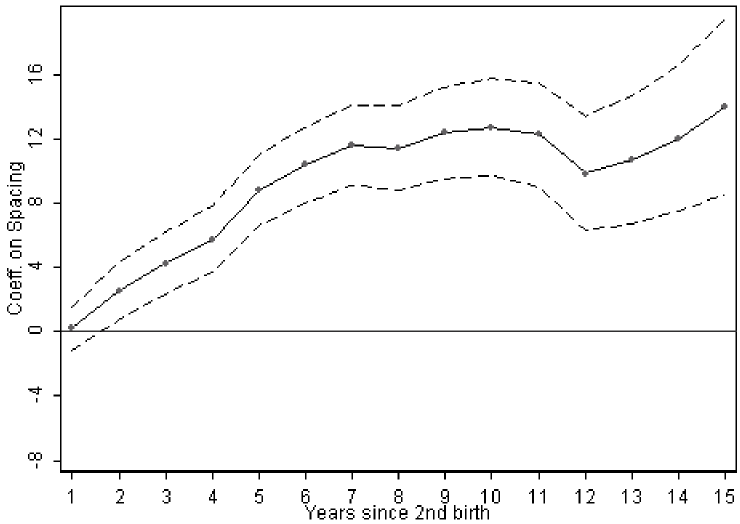

FIGURE A7. Coefficients from the 2SLS estimation of the effect of child spacing on labor income after second birth, and the 95 percent confidence intervals.

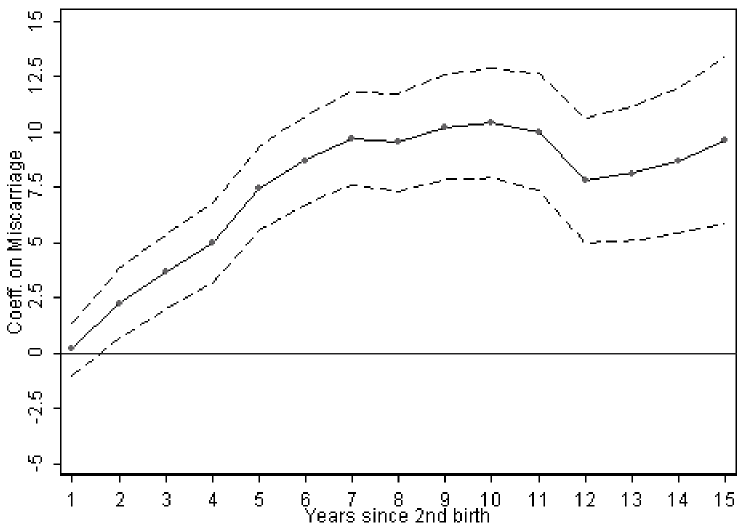

FIGURE A8. Reduced form estimates, labor earnings.

PAPER 3

Gender Differences in Shirking: Monitoring or Social Preferences? Evidence from a Field Experiment

Per Johansson Arizo Karimi J Peter Nilsson

ABSTRACT This paper studies gender differences in the extent to which social preferences affect workers' shirking decisions. Using exogenous variation in work absence induced by a randomized field experiment that increased treated workers' absence, we find that also non-treated workers increased their absence as a response. Furthermore, we find that male workers react more strongly to decreased monitoring. In addition, our results suggest significant heterogeneity in the degree of influence that male and female workers exert on each other: conditional on the potential exposure to same-sex co-workers, men are only affected by their male peers, and women are only affected by their female peers.

1. Introduction

Recent advances in the economics experimental literature has documented gender differences along various dimensions of social preferences and psychological attributes. For example, empirical evidence suggest that women are, compared to men, more averse to risk and competition, and more other-regarding and reciprocal (see e.g. Bertrand 2011 or Croson and Gneezy 2009, for reviews of the literature). Differences in psychological traits and social mindedness are often hypothesized to explain observed gender differences in consumption and investment behavior, as well as differences in the labor market. However, the empirical evidence on disparities in attributes and social preferences between the genders is most often based on laboratory experiments. It is still largely an open question whether evidence from the lab generalizes to economic behavior in real markets (Bertrand 2011).

This paper contributes to the literature on gender differences in social preferences by studying the extent to which social incentives determine productivity behavior of male and female workers. Specifically, we study whether the responsiveness to peers in individual shirking behavior differs between male and female workers, and whether individuals are influenced to the same extent by co-workers of their own gender as by those of the opposite sex.

We thank Hans Grönqvist and Lena Hensvik for useful comments and suggestions.

We use exogenous variation in co-workers' absence induced by a large scale social experiment that altered the incentives for short-term work absence through decreased monitoring for nearly half of all workers in Gothenburg, the second largest city in Sweden.[1] Before the experiment, workers were required to present a doctor's certificate on the 8th day of a sickness absence spell in order to continue receiving temporary benefits for further leave. For individuals assigned to the treatment group, the monitoring-free period was extended to the 15th day of an absence spell. Thus, treated workers could be on leave with benefits at their own discretion for 14 days instead of 7, whereas the control group faced the usual restriction of 7 days of non-monitored absence.

While peer effects can arise due to nonsocial spillovers, such as information sharing and externalities, the experiment provides a setting in which peer effects are informative of the presence of social preferences in the workplace. First, information sharing is an unlikely channel for peer effects in our context; the experiment was preceded by a massive information campaign making both the experimental design and, if not previously known to workers, the rules of the sickness insurance clear. Second, the experiment did not alter the health of workers, and two previous studies, Hesselius et al. (2009, 2013), rule out health spillovers of the experiment. Thus, in the absence of social preferences, workers should not respond to their co-workers' behavior in their decision to be absent from work. Hesselius et al. (2009, 2013) conclude that the positive peer effects found in their respective studies were consistent with preferences for fairness or reciprocity.

The experiment also provides a close to ideal setting in which to identify peer effects. Identifying social interactions has proven to be difficult due to the well known problems of endogenous group membership, and reverse causality. The latter arises because each peer group member is simultaneously affecting every other group member (Manski 1993). Using variation in co-workers' absence induced by the experiment allows us to address these severe identification problems. First, treatment was randomized based on birth date: workers born on an even date were assigned to the treatment group, and workers born on an uneven date were assigned to the control group. The randomized assignment directly addresses the problem of endogenous group membership since it balances all other determinants of work absence. The reverse causality problem can be addressed because, within each workplace, treatment was assigned to only a subset of employees by virtue of the randomization. The

[1] Sickness absence is determined by workers' health status, but solely considering health is not sufficient to explain the large variation in sickness absence within and across firms. Economists have also stressed the importance of economic incentives and several studies document that workers adjust their absence levels to the generosity of the sickness insurance (Johansson and Palme, 2005; Ziebarth and Karlsson, 2013). Recently, some studies have shown that sickness absence is also influenced by co-workers' absence levels (Ichino and Maggi, 2000; Hesselius et al., 2009,2013) and that social interactions thus are an important determinant of worker absenteeism.

experiment thus altered the incentives for the treatment group, leaving the non-treated workers' incentives unchanged. The response among the non-treated, then, provides information about how the reference group affects individual behavior, and not the other way around.[2]

Our analysis provides four main findings. First, consistent with Hartman et al. (2013), we find that the decreased monitoring significantly increased non-monitored absence among the treated workers. Second, in line with Hesselius et al. (2009, 2013), we find significantly positive peer effects in shirking; non-treated workers are estimated to increase their non-monitored absence as a response to being exposed to treated peers.

Third, we find that male workers react more strongly to the decreased monitoring compared to female workers; there is a larger positive effect of being assigned to treatment on non-monitored absence among male workers. Women's shirking behavior, on the other hand, seems slightly more responsive to peers compared to that of men's shirking. This could potentially imply that women are more other-regarding than men: while male workers take the opportunity to increase absence when monitoring decreases, women look more to their surrounding co-workers' behavior when deciding whether to shirk or not. Interestingly, however, we find significant heterogeneity in the degree of influence that male and female workers exert on each other: men are only affected by their male peers, and women are only affected by their female peers. In fact, when we decompose the effect of the fraction treated peers into fractions of male and female treated peers, respectively, there is no significant difference between the effect of peers on male and female workers' absence. Instead, the entire peer effect among men is driven by the effect of male co-workers, and vice versa for women. These results hold true even as we control for the fraction of women at the workplace, industry affiliation, as well as dummies taking into account both the field and level of education. The latter is likely to take into account a large part of the variation in occupations held by men and women. Hence, the stronger influence of same-sex co-workers cannot be explained by gender-segregated workplaces. Rather, our results reflect the influence that (fe)male co-workers exert on each other conditional on the potential exposure to same-sex colleagues.

The paper contributes to two strands of literature. First, we contribute to the literature on gender differences in social preferences by studying if these matter outside the laboratory. The body of work from laboratory experiments has so far provided mixed evidence. Studies on reciprocity and fairness sometimes show that women are more trusting than men and sometimes less. In their review of the experimental literature, Croson and Gneezy (2009) hypothesize that this variance is explained by a differential sensitivity of men and women to the

[2] This "partial population intervention" approach was outlined by Moffitt (2001) and has been used by e.g. Lalive and Cattaneo (2009) to study social interaction effects schooling attendance in Mexico's PROGRESA, and by Dahl et al. (2012) to study peer effects in paternity leave in Norway, exploiting reforms in the parental leave system that altered the price of leave-taking for some fathers but not for others.

social conditions of the experiment. They further argue that small differences in experimental design and implementation can affect these social conditions, leading women to appear more other-regarding in some experiments and less other-regarding in others. They conclude that women are neither more or less socially oriented, but that their social preferences are more malleable. Our results are in line with the result in Croson and Gneezy in that women do not seem to be more other-regarding than men. However, our findings cast some doubt on the hypothesis that women's social preferences are more malleable: both male and female workers care about their social context when this is defined by worker similarity. Thus, women's decision do not seem to be more situationally specific than men's in our setting.

Second, our findings also contribute to the emerging literature on social determinants of worker productivity. Bandiera et al. (2005, 2010) exploit data from a fruit picking farm in the UK and study whether workers have social preferences, both in settings where worker effort imposes an externality on other workers, and in cases where there are no externalities. In the former, they find that the productivity of the average worker is higher under piece rates than under relative incentives, under which worker effort imposes an externality on others' payoffs. They find that this is due to workers partially internalizing the negative externality. In the case without externalities, the authors find that a given worker's productivity is higher when she works alongside friends who are more able than her, and lower when she works with friends who are less able. Mas and Moretti (2009) study peer effects in the workplace and investigate whether, how, and why the productivity of a worker depends on the productivity of co-workers in the same team using data from a large supermarket chain in the US. They find strong evidence of positive productivity spillovers from the introduction of highly productive personnel into a shift. While this body of work examines social preferences as determinants of worker productivity on the intensive margin, the evidence provided in the present paper shows that social incentives also affect worker productivity on the extensive margin.

The rest of the paper is organized as follows. The next section describes the Swedish sickness insurance and the experimental design. Section 3 briefly discusses how to interpret the effect of treatment and peer effects in the experiment, Section 4 presents the data, identifying strategy, and empirical specifications. Section 5 present the results, and Section 6 concludes the paper.

2. The Swedish Sickness Insurance and Experimental Design

2.1. The Sickness Insurance System. The sickness insurance in Sweden is compulsory and covers all workers, unemployed individuals and students. It is financed through a proportional pay-roll tax and replaces individuals' foregone earnings due to temporary illness. In an international context the replacement levels are rather generous. In 1988, the year in which the experiment took place, the benefit level for most workers was set to 90 percent of previous earnings, up to an inflation-adjusted cap. In addition to the public insurance, most Swedish workers are covered by top-up sickness insurance regulated in agreements between the unions and employers' confederations, which generally covers 10 percent of the foregone earnings. The total compensation for work absence due to temporary illness could thus be as high as 100 percent.

The public sickness insurance does not include limits to the duration of sickness benefit payments, or to how often benefits can be claimed.[3] While benefit payments are generous, the monitoring is lax. A sickness absence spell starts when the worker calls the public insurance office and the employer to report sick. On the 8th day of the sickness absence spell, the worker must confirm eligibility status in order to be entitled to *continued* sickness absence by presenting a medical certificate that proves reduced work capacity. The medical certificate is reviewed by the public insurance office, after which further sick leave is either declined or approved. In practice, medical doctors rarely turn down requests for certificates. Of course, some rules make it possible for the public insurance offices to monitor more strictly. When abuse is suspected they could, for instance, visit the claimant's home. Claimants who have been on sickness absence too frequently in the past may be asked to provide a doctor's certificate from day one of the absence spell. Moreover, a new absence spell starting within five working days of the first spell is viewed as a continuation of the first spell, making it impossible to report sick every Monday without ever visiting a doctor. Individuals with chronic illnesses, on the other hand, need not verify their eligibility status each time illness prevents them from going to work.

Given the rather high benefit level and the lax monitoring, ex-post moral hazard in the Swedish sickness insurance system is high (see e.g. Johansson and Palme 1996, 2002, 2005, Henrekson and Persson 2004, for empirical evidence).

2.2. The Experiment. In the second half of 1988, the regional social insurance board in the municipality of Gothenburg, which is the second largest city in Sweden, performed a social experiment that altered the timing of the requirement for a medical certificate.[4] The treatment group, which was randomly assigned, was allowed to be on temporary sickness absence for 14 days before having to present a medical certificate in order to continue their absence spell. The control group faced the usual restriction of 7 days of non-monitored sickness absence. Assignment to treatment was based on individuals' date of birth: individuals born on an even date were assigned to the treatment group, and individuals born on an uneven

[3] Such limits are in place today. However, in this section we describe the rules that applied at the time of the experiment.

[4] The experiment was also conducted in Jämtland, a large and sparsely populated region in the north of Sweden. Here, we only analyze data from the Gothenburg experiment.

date were assigned to the control group. For an individual to be eligible for the experiment, they had to reside in Gothenburg municipality.

The arguments put forth by the insurance agency for running the experiment were based on the belief that extending the monitoring-free period would decrease costs and reduce work absence. The main argument was that, with the 14-day restriction, unnecessary visits to medical doctors could be avoided, which would cut costs not only for the worker, but also for the public health care system. The insurance agency also believed that medical doctors routinely prescribed longer absences than necessary. With an extended certificate-free period, many individuals would have time to return to work before a medical certificate was needed, and thus individual and public costs would be reduced.

The experiment was running during the second half of 1988 and, in addition to the social insurance staff, all employers and medical centres were informed before or during the experiment. Thus, the experiment was non-blind, and a massive information campaign also preceded the experiment including mass-media coverage and distribution of pamphlets and posters at workplaces. Brief information about the experiment was also written on the form which every insured worker reporting sick had to fill in and send to the insurance office to receive sickness benefits.

The existing evaluation of the experiment shows that absence spell durations increased, on average, substantially among the treated compared to the control group. Hartman et al. (2013) estimated that average absence duration in the treatment group increased by 6.6 percent. They also report differential treatment effects between women and men, where men were found to prolong their work absence spells substantially more than women.

3. Decreased Monitoring, Shirking and Social Interactions

The sick-pay that workers receive is paid by the Swedish government, which means that for employers, the only cost of worker absenteeism is the cost of finding and hiring replacement workers and/or foregone productivity. In general, an employer in Sweden cannot fire a worker for shirking. The only valid reason for laying off a worker is if the worker has engaged in illegal activities, such as working during his or her sickness absence. Both these facts imply that the incentives for the employer to monitor employees' sickness absence are low. Given the high level of workers' discretion due to the lax monitoring, we interpret a prolonged absence due to the decreased monitoring as a shirking effect.

To study whether there are peer effects in shirking behavior, we focus on the non-treated workers and interpret a potential increase in the work absence among the non-treated in response to treated peers as evidence of peer effects. The argument behind this interpretation is that, if workers have social preferences, they care about the work absence of their peers in their own decision to be absent from work. Of course, a positive spill-over effect can also be the result of nonsocial spill-overs. For example, if treated workers increase their absence, it is possible that presenteeism decreases, such that the remaining workers are less exposed to ill co-workers. In this case, we would expect to find negative effects on absence among the non-treated. However, if treated workers increase their absence due to shirking, this is not a likely scenario. Another possible scenario is that *negative* externalities arise. If an increased absence among treated shifts the workload to other workers, the latter must increase their work effort. In turn, this might lead to increased stress and thereby illness, which could lead to an increased absence also for the non-treated.

A second possible explanation of a positive peer effect that is not the result of social preferences is joint leisure: co-workers might use the sickness absence to enjoy leisure time together. Evidence provided in Hesselius et al. (2009,2013), who study social interaction effects in the Gothenburg experiment, do not support the joint leisure or health externality hypothesis. Rather, their evidence suggest that the positive spill-over effects found among the non-treated are consistent with fairness or reciprocity concerns being the main channel. If workers care about fairness, the non-treated workers could - as a response to an expected increase in shirking behavior among their peers - increase their own absence in order to get the same amount of leisure as their treated peers. Alternatively, non-treated workers might feel that they are being unfairly treated by the sickness insurance agency and, as a consequence, increase their work absence.

4. Identification Strategy and Data

4.1. Identification Strategy. Identifying social interaction effects has proven to be difficult due to the problems of reflection, correlated unobservables and endogenous group memberhsip (Manski, 1993). The reverse causality problem (reflection) arises because person A's actions affect the actions of person B, and vice versa. As illustrated by Moffitt (2001), suppose we have $g = 1, ..., G$ groups with two individuals $i = A$ and B in each group. Let y_{ig} be the outcome variable of interest for individual i in group g, let x_{ig} be individual socioeconomic characteristics of individual i in group g, and let ϵ_{ig} be an unobservable and assume the structure to be:

$$y_{Ag} = \alpha_g + \theta_1 x_{Ag} + \theta_2 y_{Bg} + \theta_3 x_{Bg} + \epsilon_{Ag} \tag{3.1}$$

$$y_{Bg} = \alpha_g + \theta_1 x_{Bg} + \theta_2 y_{Ag} + \theta_3 x_{Ag} + \epsilon_{Bg} \tag{3.2}$$

The social interaction effects are represented by the parameters θ_2 (endogenous social interaction effect) and θ_1 (the exogenous social interaction effect). Manski (1993) shows that the parameters in (3.1) and (3.2) are not identified. Under the assumptions that ϵ_{Ag} and ϵ_{Bg} are independent to both x_{Ag} and x_{Bg} and of no group sorting (i.e., $E(\alpha_g y_{ig}) = 0$)), it is easy to show the existence of social interactions *in general*. The coefficients on the other individuals' x in the reduced from indicates whether any type of social interaction is present, but endogenous social interactions cannot be distinguished from exogenous social interactions. In addition to the reverse causality problem, however, there is also the potential problem of sorting (unobservables). In the presence of unobservables, even the weak form of identification obtained from the reduced form, i.e., of the existence of *any* social interactions, is lost.

To overcome these identification problems, we study the influence of co-workers by exploiting variation in the incentives for work absence for a subset of employees at workplaces, induced by a randomized social experiment (see Moffitt, 2001). Let D_{ig} denote treatment, where $D_{ig} = 1$ if individual i in group g is eligible for treatment and $D_{ig} = 0$ otherwise. Moreover, treatment is randomly allocated to a subset of each group such that $0 < \overline{D}_g < 1$. In the example above, suppose that individual A is randomly (independently of α_g) assigned to receive treatment, whereas individual B is not. Equation 3.1 now becomes:

$$y_{Ag} = \alpha_g + \theta_1 x_{Ag} + \theta_2 y_{Bg} + \theta_3 x_{Bg} + \theta_4 D_{Ag} + \epsilon_{Ag} \tag{3.3}$$

The absence of D_{Ag} in Equation (3.2) allows all parameters in the model to be identified. Thus, there exists one exogenous variable that affects A directly, but affects the other individual only through the endogenous social interaction. The identifying assumption is that individual B is not directly influenced by D_{Ag}.

The intuition is that if treatment is randomly assigned to a subset in a network, we can explore whether the untreated individuals in the network change their behavior. The response among the non-treated gives us information on how the reference group affects individual outcomes, and not the other way around. In the absence of social interactions, the non-treated should be unaffected by the fraction treated in their peer group.

4.2. Data. The analysis is based on data from a set of administrative registers maintained by Statistics Sweden. In addition to a set of background characteristics, the data contains information on start- and end-dates of all absence spells during 1987 and 1988. We also observe the workplace where the individual is employed.[5] We start by constructing a matched employer-employee data set to obtain information on individual- and workplace characteristics. Since eligibility for the experiment was conditioned on residence in Gothenburg municipality, we restrict attention to individuals who live in Gothenburg in the empirical analysis. Thus, while commuting co-workers are included when calculating workplace average characteristics, commuting workers (who live outside Gothenburg) are not included in the estimation sample. Moreover, we focus on individuals working at workplaces with

[5] A few individuals have multiple workplaces, but for simplicity we assume that the workplace from which the highest yearly earnings are received is also the main arena for co-worker interactions.

10-100 employees, as social interactions are likely to be more prevalent in small- to medium sized workplaces. Our main outcome variables are the number of days spent on sick leave spells that are shorter than 15 or 8 days, which correspond to *non-monitored* absence for treated and non-treated workers, respectively.

Figure 3.1 graphs the distribution of the proportion treated employees for workplaces at which individuals in our analysis sample are employed. There is considerable variation in the fraction of treated workers between workplaces. The average workplace has about 30 percent treated workers. The variation in the fraction treated comes from the random assignment of treatment, but also from the number of commuting workers; recall that eligibility status for the experiment was conditioned on residence in Gothenburg municipality, so the mass point at zero treated workers stems from employees who live outside the experiment region. Similarly, individuals can also commute from Gothenburg to bordering municipalities, which means that some eligible workers have employments at workplaces located in bordering municipalities where the share of treated workers will be low. The commuting patterns

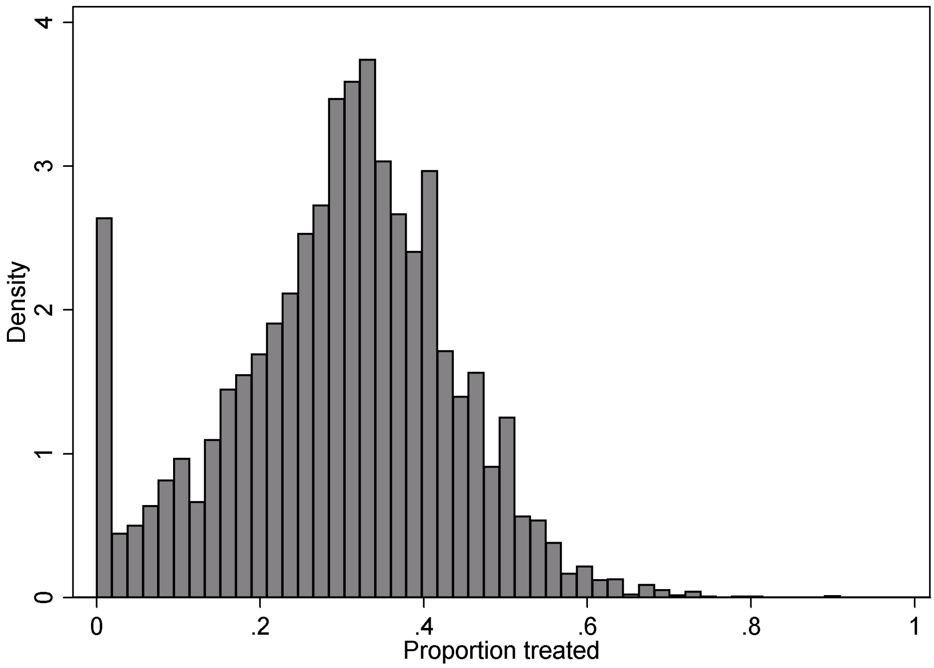

FIGURE 3.1. Distribution of the fraction treated workers at workplaces with 10-100 employees.

can be seen in Figure A1 in the Appendix, where the upper graph shows the proportion of individuals working in Gothenburg as a function of the kilometer distance between the residence neighborhood and Gothenburg city center. 80 percent of workers residing in central Gothenburg work in Gothenburg. This picture is corroborated in the middle graph of Figure

A1, which shows the proportion treated co-workers to the individuals in our study sample, as a function of the kilometer distance between residence neighborhood and Gothenburg city center. The graph shows that individuals living outside Gothenburg municipality (i.e., about 20 kilometers and further away from the city center) have some treated co-workers. The lower graph depicts the proportion assigned to treatment, and shows that workers living outside Gothenburg (further than 20 kilometers away) are never assigned to treatment, whereas about 50 percent of those living in the city center have been assigned to the treatment group.

Table A1 in the Appendix depicts the means and standard deviations of individual- and workplace characteristics by treatment status, for all workers residing in Gothenburg and employed at workplaces with 10-100 employees. The treatment group exhibits, on average, more days on sickness absence during the Fall of 1988 (the experiment period) compared to the control group, with a difference of 0.41 days on average. However, the treatment- and control groups are similar in terms of sickness absence in the time periods preceding the experiment, both in terms of individual- and workplace characteristics, which indicates that the experiment was well conducted.

To measure the presence of peer effects in sickness absence, we make use of the random variation in the share treated co-workers induced by the experiment. One potential threat to the empirical strategy employed is that workplaces with different shares of treated workers differ with respect to sickness absence also in the absence of the experiment. In Table A2 we display the same descriptive statistics depicted in the previous table, but for workers at four different types of workplaces, characterized by the proportion treated workers: those with less than 13 percent treated workers, between 13-28 percent, 28-35 percent and more than 35 percent treated workers, respectively.[6] Indeed, there are some differences between the groups. For instance, one large difference between the groups is commuting workers: 64 percent of the employees at workplaces in group 1 commute, whereas the corresponding number for group 4 is 18 percent. The share of workers with some college education is highest in group 4, but average earnings are the highest in group 1. Furthermore, the share of female employees increases with the share treated (women are less likely to commute).

Importantly, the pre-experimental sickness absence is almost monotonously increasing with the share treated. This is true both in terms of workplace-averages and individual sickness absence. This difference likely arises from the randomization being only on workers living in Gothenburg municipality, and that workplaces with different shares of commuting workers differ in terms of worker characteristics. The analysis includes only workers who were assigned to either the treatment or control group. However, to take workplace heterogeneity into account we control for the share of commuters at the workplace, a number of

[6] The division is defined by the 25th, 50th and 75th percentiles of proportion treated workplaces with 10-100 employees.

other workplace characteristics as well as the workplace average sickness absence. Thus, we make use of the random variation in treatment and the share of treated co-workers induced by the experiment, conditional on the share of non-eligible workers and workplace characteristics. The empirical specifications employed are discussed in further detail in the following section.

4.3. Empirical Specifications. We begin by estimating the effect of being assigned to treatment, and to capture potential peer effects we estimate the effect of the proportion treated co-workers on individual sickness absence. Our baseline model is specified as:

$$y_{ig} = \beta_0 + \beta_1 T_{ig} + \beta_2 \pi_{ig} + x'_{ig}\beta_3 + z'_{(-i)g}\beta_4 + \epsilon_{ig} \tag{3.4}$$

where y_{ig} is the number of days (including zero) on work absence - for spells that are shorter than 15 days or shorter than 8 days (corresponding to non-monitored absence for the treated and non-treated, respectively) in the second half of 1988, for employee i who is employed at workplace g. T_{ig} takes on the value one if individual i at workplace g is treated, and zero otherwise. π_{ig} is the share of treated co-workers at employee *i:s* workplace (excluding employee i). β_1 then measures the main effect of the experiment on work absence, and β_2 the effect of the proportion treated co-workers on individual work absence. x'_{ig} is a vector of individual characteristics and $z'_{(-i)g}$ a vector of workplace characteristics (excluding individual i), such as the number of employees, the average age of workers, share female employees, average income, share of workers with at most high school education or some college education and dummies for industry affiliation. $z'_{(-i)g}$ also includes the workplace average days on sickness absence in Spring 1988, Spring and Fall 1987, as well as dummy variables for different shares of commuting employees at the workplace (10 percent bins). This selection-on-observables estimator allows us to non-parametrically identify peer effects. Compared to a difference-in-differences estimator or to a fixed-effects estimator, this identification strategy has the advantage of providing more precise estimates. An additional advantage is that the strategy employed can be tested using pre-experimental data. Inference is based on standard errors that are clustered at the workplace level, i.e., they are robust to unspecified conditional correlations between individuals at the workplace.

We also estimate a similar specification to Equation (3.4) where we focus separately on treated and non-treated workers, respectively, to estimate the effect of the share treated co-workers on individual work absence:

$$y_{ig} = \beta_0 + \beta_1 \pi_{ig} + x'_{ig}\beta_2 + z'_{(-i)g}\beta_3 + \epsilon_{ig} \tag{3.5}$$

where the vectors x'_{ig} and $z'_{(-i)g}$ are the same as in Specification (3.4).[7]

[7] When focusing on the non-treated individuals, Equation 3.5 can be seen as the reduced form of estimating endogenous social effects in the model specified by Manski (1993).

5. Results

5.1. The Effect of Relaxed Monitoring and the Impact of Peers on Shirking. Before studying gender heterogeneity in the effects of treatment and in peer effects, we analyze the impacts of the experiment for the full sample. Table A3 in the Appendix reports the results from estimating Equation (3.4) and shows that treated workers increased their absence by 0.36 days in the second half of 1988 compared to the control group. Columns (2) and (3) report results from estimating Equation (3.5) on non-monitored absence for treated and non-treated workers separately. There is no significant peer effect among treated workers, but a significantly positive peer effect among non-treated workers of 0.82 days. Table A4 in the Appendix reports results from estimating Equation (3.4) on monthly absence days in 1988. The increased shirking among the treated is instantaneous; while there are no differences in absence between treated and control individuals in January through June (which are essentially placebo tests), treated workers are estimated to have 0.06 days more absence compared to the control group in July, an effect that remains fairly constant throughout the rest of 1988. The peer effect, however, appears already in June, and then gradually wears off. It might be worrying that there is a significant peer effect one month before the start of the experiment. The experiment was preceded by a large information campaign including mass-media coverage. In fact, an article appeared in the largest newspaper in Gothenburg, Göteborgsposten, on June 9th, 1988, with the headline "Sickness absence without medical certificate". It explained that all workers born on an even date would be able to be on sick leave at their own discretion for 14 days. The start-date of the experiment was however not printed in the article. It is thus possible that the newspaper article (and other media) created an expectation among those born on an uneven date that their treated peers would increase their absence, and that this expectation itself triggered an early response to having co-workers that would receive a longer duration of non-monitored absence.

The absence of a significant peer effect among the treated workers suggest that joint leisure is not a driving mechanism for the estimated peer effect, since such a channel would arguably yield similar peer effects for both the treated and non-treated workers. Moreover, since the peer effect is instantaneous, it is unlikely that the response among the non-treated is due to negative externalities on health; if an increased absence among peers would cause an increased workload, and thereby more stress, a more likely pattern would have been a gradual increase in the peer effect over time. Thus, in line with Hesselius et al. (2009, 2013), our findings suggest that the peer effects are not driven by nonsocial spill-overs.

We also estimate placebo regressions based on Specification (3.4) with the outcome variable being sickness absence days in the fall of 1987, i.e., one year before the experiment.

The results are presented in panel B of Table A3 and shows no significant effects of either treatment or of the share treated co-workers.[8]

5.2. Heterogeneous Responses by Gender. Whether women are more other-regarding than men can in our setting be studied by simply analyzing whether the influence of peers differs in magnitude for male and female workers. If women care more about what others do, we expect the peer effect to be of greater importance for women than for men. To study whether women's social preferences are more situationally specific than men's, we can examine whether potential peer effects differ when taking into account who the peers are. Specifically, we study whether men and women are affected to the same extent by same-sex peers as those of the opposite gender.

Table A5 in the Appendix presents summary statistics separately for the male and female workers in our sample. In line with previous empirical findings, female workers have more days on sick leave compared to male workers, in both 1987 and 1988. However, the difference in work absence between the first and second half of 1988 is larger for male workers. Moreover, women earn significantly lower incomes compared to men, and are employed at workplaces with a larger share of female employees, lower average earnings, higher average educational level and a smaller share of commuting co-workers. Thus, the labor market is highly gender segregated, and the absence levels at the average woman's workplace is higher than that of the average male worker's.

Table 3.1 presents the results from OLS regressions, based on Equation (3.4), of the effect of being assigned to treatment and of the fraction of treated peers on the full sample, male and female workers, respectively. The effect of being assigned to treatment is larger for men than for women: being assigned to treatment increases male workers' absence by, on average, 0.46 days in the second half of 1988, whereas the corresponding increase among women is 0.28 days. The table also includes baseline absence days, which correspond to the average number of days spent in spells shorter than 15 days in the second half of 1987, i.e., one year before the experiment. Compared to the baseline absence, the increase in male workers' absence correspond to a 19 percent increase, and for women an increase of about 10 percent. Hence, the effect of decreased monitoring on shirking is almost twice as large for men compared to women.

[8] We have also estimated the effect of treatment and share treated on monthly sickness absence in 1989, which is the first post-experiment year. Results show that there are no significant effects of being assigned to treatment in any month of 1989, and thus sickness absence is higher among the treated only during the experimental period. However, there is a somewhat lingering peer effect. We also tested the sensitivity of our estimates for the inclusion of higher order terms for the number of employees and workers age, as well as including the share of commuters linearly in the model, both with and without higher order terms for the share of commuters. The results are robust to all these variations of the specification and the results are available upon request.

One potential explanation for this result could be that male workers have a lower threshold to shirking compared to female workers. However, a stylized fact in the study of absenteeism is that women, on average, utilize the sickness insurance to a greater extent than men. Under the assumption that the health of women and men is the same, the difference in the effect of monitoring could also stem from men being less inclined to visit a doctor to obtain a certificate. Hence, decreasing the requirement would increase the absence more for male workers than for female workers.

Interestingly, the social interaction coefficient is larger in magnitude for female workers (and not statistically significant for men). In addition, we have also estimated the social interaction effects separately by treatment status and found that the estimated peer effect for women is driven by female non-treated workers, who increase their non-monitored absence.[9] One interpretation of these findings is that women are indeed more socially minded than men: while women take their co-workers' behavior into account to a greater extent when deciding whether to shirk or not, men seem to be more constrained by formal monitoring in the absence decision.

Lastly, Table A6 in the Appendix presents "placebo estimates" where we estimate Equation (3.4) on sickness absence days in the second half of 1987, i.e., one year before the experiment, separately for male and female workers. We find no significant effects of either treatment or of the fraction treated co-workers for any sub-sample.

TABLE 3.1. Parameter estimates from the OLS estimation of the effect of treatment and effect of share treated co-workers on sickness absence days

	All <15 days	Male workers <15 days	Female workers <15 days
Sickness absence days in Fall 1988			
Treatment	0.36***	0.46***	0.28***
	(0.05)	(0.07)	(0.07)
Share treated	0.82**	0.70	1.00**
	(0.33)	(0.48)	(0.44)
Baseline absence days	2.62	2.37	2.86
Observations	61715	29826	31889

NOTES.— The outcome variables are the number of days on sickness absence in spells that are shorter than 15 days in the Fall of 1988. Included covariates are age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, workplace average sickness absence days (excluding individual i) in fall and spring of 1987 and spring 1988. Standard errors are clustered at the workplace level. *p<0.1, **p<0.05 ***p<0.01.

[9] These results are available upon request.

5.3. Differential Responses to Peers by Co-workers' Gender. The results presented in the previous section show that the moral hazard effect is larger for male workers. Regarding the peer effects, the coefficient on the share treated colleagues is slightly larger in magnitude for female workers, and not statistically significant for men. The difference in the social interaction coefficient for men and women is, however, not statistically significant. Thus, we do not find any strong evidence that women are more socially minded than men in their shirking decision. Although women and men may be equally other-regarding on average, there may still be differences in how the social preferences of men and women differ depending on the social context.

Although we cannot change the social conditions in the experiment, we can study whether the social interaction effect among men and women differ when we take into consideration the composition of the reference group. If women's social preferences are more situationally specific we would, for instance, expect to see that the peer effect differs for women depending on who their peers are, whereas the peer effect for men would be the same independently of who their co-workers are. To explore whether this is the case, we consider how the social interaction effect differs with the proportion treated workers that are women or men, respectively. That is, we study whether the similarity of peers matter for the magnitude of the social interaction effect, and whether it matters to a different extent for men and for women.[10] To this end, we decompose the fraction treated co-workers into two variables that measure the fractions of male and female treated workers, respectively. We then estimate Equation (3.4) where the variable *Share treated* is replaced by the two new variables *Share treated men* and *Share treated women*.

The results are presented in Table 3.2, where columns (1) and (2) present the results for men and women, respectively, and include the same covariates as in the previous specifications. Looking at the results for women, the coefficient on the share of treated women is positive and statistically significant, suggesting that increasing the share of treated female co-workers from 0 to 1 increases women's absence by 1.35 days. The coefficient on the share of treated men, however, is small in magnitude and not statistically significant. Turning to the results for male workers in column (1), the pattern is the opposite: the coefficient on the share of treated women is negative, albeit not statistically significant, whereas the coefficient on the share of treated male co-workers is positive and significant, indicating that increasing the share of treated male peers from 0 to 1 increases male workers' absence by 1.08 days, on average. These evidence suggest that both male and female workers are sensitive to the

[10] The tendency of individuals to prefer associating with others that are similar to themselves has been documented as a relatively robust empirical observation (Currarini et al. 2009, Mas and Moretti 2009). For example, Asphjell et al. (2013) study peer effects within the workplace in fertility decisions and find that women's childbearing decisions are indeed affected by the fertility decisions of their co-workers, but the effect is entirely driven by other female peers.

behavior of their peers, but that not all peers have the same influence on individual behavior. Rather, men seem only affected by other men, and women by other women.

As mentioned previously, the Swedish labor market is highly gender segregated. Hence, one might be worried that these results simply reflect the fact that women are more exposed to other female workers and men more exposed to other male workers. The estimates presented in columns (1) and (2) include controls for the fraction of women at the workplace as well as dummy variables for industry affiliation. Nevertheless, also within workplaces there might be gender segregation in the types of occupations held by women and men. For example, female workers are perhaps more likely to hold occupations with administrative tasks, resulting in more frequent interaction with other administrative (female) staff. Ideally, we would like to control for occupations, on which we lack data. However, we can control for the field of education, as well as the combination of educational field and educational level. The latter is likely to take into account a large part of the variation in occupations across the genders. In columns (3) and (4) of Table 3.2 we have included a full set of dummies for educational field (9 categories), and in columns (5) and (6) a full set of dummies for the combination of field and education (47 categories). As seen, the results are robust to the inclusion of both field of education as well as field- *and* level of education. Hence, the stronger influence of same-sex co-workers cannot be explained by gender-segregated workplaces. Rather, our results reflect the influence that (fe)male co-workers have on each other conditional on the potential exposure to same-sex colleagues.

That workers are mainly influenced by same-sex peers might also have interesting policy implications as it shows that social interaction effects are likely to be a function of the similarity of peers. For example, if individuals are more influenced by peers that are similar to themselves, potential spillover effects of policy interventions will arguably be more sizeable in homogenous groups than in groups with a more heterogenous population.

TABLE 3.2. Parameter estimates from the OLS estimation of the effect of treatment and effect of share treated men and share treated women on sickness absence days separately by gender

	(1) Male <15 days	(2) Female <15 days	(3) Male <15 days	(4) Female <15 days	(5) Male <15 days	(6) Female <15 days
Treatment	0.43***	0.28***	0.43***	0.28***	0.45***	0.27***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Share treated women	-0.06	1.35***	-0.06	1.32***	0.01	1.28**
	(0.65)	(0.50)	(0.65)	(0.50)	(0.65)	(0.50)
Share treated men	1.08*	0.10	1.15**	0.18	1.19**	0.25
	(0.56)	(0.68)	(0.56)	(0.68)	(0.55)	(0.68)
Industry dummies	✓	✓	✓	✓	✓	✓
Field of education, 1 level			✓	✓		
Field of education, 2 levels					✓	✓
Observations	29826	31889	29826	31889	29826	31889

NOTES.— The outcome variables are the number of days on sickness absence in spells that are shorter than 15 days in the Fall of 1988. Included covariates are age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, workplace average sickness absence days (excluding individual i) in 1987 and 1988 and a full set of dummies for industry affiliation. Standard errors are clustered at the workplace level. *p<0.1, **p<0.05 ***p<0.01.

6. Concluding Discussion

In this paper, we exploit a setting in which peer effects are informative of social preferences to study whether there are differences in social preferences between the genders in determining shirking behavior. To this end, we use exogenous variation in co-workers' absence induced by a large scale social experiment that altered the incentives for short term sickness absence for nearly half of all workers in Gothenburg. The experiment increased the monitoring-free period of sickness absence from 7 to 14 days for the treated, which were randomly assigned, whereas the control group faced the usual restriction of 7 days of non-monitored absence.

The experiment allows us to address the serious identification issues inherent in estimating peer effects, and to study the presence of social preferences. The latter is made possible due to there being no concern for externalities imposed on other workers from the increased shirking induced by the experiment, and that information sharing is unlikely to be a mechanism for the spillover effects. Thus, in the absence of social preferences, workers should not respond to their co-workers' behavior in their decision to be absent from work.

We find that decreased monitoring significantly increases non-monitored absence among treated workers. Second, we find significantly positive peer effects in shirking; non-treated workers increase their non-monitored absence in response to being exposed to treated peers. Third, we find that male workers increase their absence almost twice as much as female workers when monitoring decreases. Women's shirking behavior, on the other hand, seems slightly more responsive to peers compared to that of men's shirking. Interestingly, however, we find that men are only affected by their male peers, and women are only affected by their female peers. Decomposing the effect of the fraction treated peers into fractions of male and female treated peers shows that there is no significant difference between the effect of peers on male and female workers' absence. Instead, the entire peer effect among men is driven by the effect of treated male co-workers and vice versa for women. These results hold true even as we control for the fraction of women at the workplace, industry affiliation, as well as dummies taking into account both the field and level of education. Hence, the stronger influence of same-sex co-workers cannot be explained by gender-segregated workplaces. Our results reflect the influence that (fe)male co-workers have on each other conditional on the potential exposure to same-sex colleagues.

These findings cast some doubt on the hypothesis that women's social preferences are more malleable: both male and female workers care about their social context when context is defined by worker similarity. Thus, women's decision do not seem to be more situationally specific than men's in our setting.

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Appendix

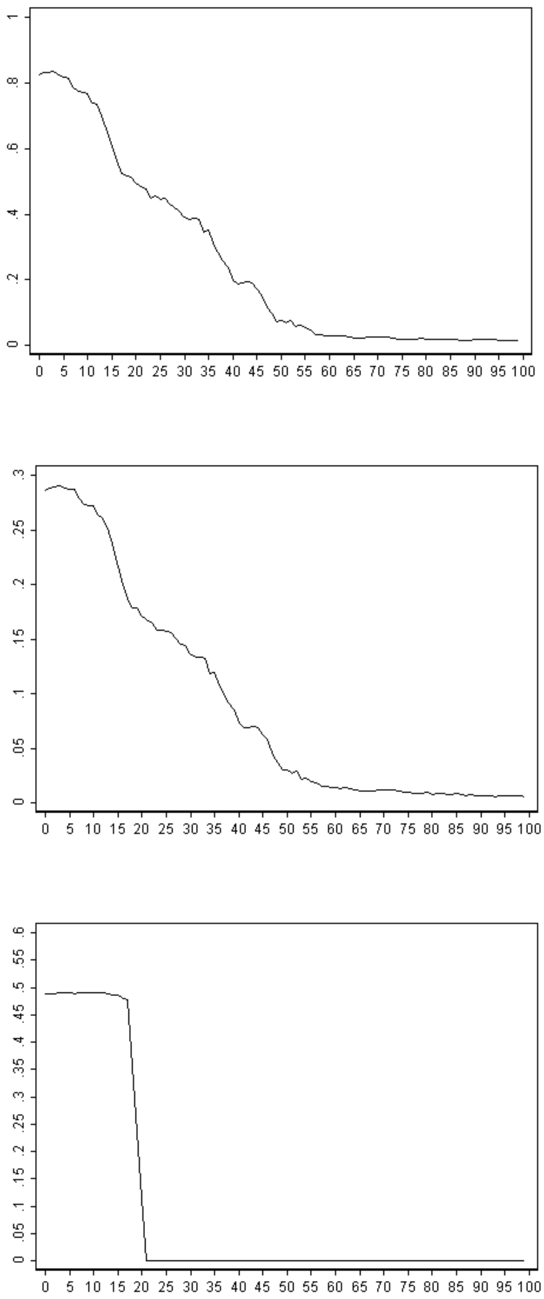

FIGURE A1. The figures graph the proportion working in Gothenburg municipality (upper graph); proportion treated co-workers (middle graph); and the proportion treated (lower graph) against the kilometer distance between residence neighborhood and central Gothenburg.

TABLE A1. Summary statistics by treatment status

	Control	Treated
Individual characteristics		
Absence days < 15 day spells, Fall 1988	3.972	4.380
	(5.848)	(6.637)
Absence days < 15 day spells, Spring 1988	3.444	3.467
	(5.381)	(5.399)
Absence days < 15 day spells, Fall 1987	2.607	2.631
	(4.786)	(4.874)
Absence days < 15 day spells, Spring 1987	2.735	2.687
	(4.940)	(4.892)
Female	0.510	0.508
	(0.500)	(0.500)
Compulsory schooling	0.282	0.282
	(0.450)	(0.450)
High school	0.443	0.446
	(0.497)	(0.497)
College	0.256	0.254
	(0.436)	(0.435)
Earnings in 1988, SEK	98553.3	99189.4
	(68934.1)	(68901.3)
Age	36.35	36.25
	(12.69)	(12.67)
Workplace characteristics		
Share treated	0.293	0.302
	(0.134)	(0.141)
Share commuters	0.377	0.382
	(0.238)	(0.240)
Number of employees	39.39	39.52
	(25.35)	(25.47)
Workplace average age	36.58	36.51
	(5.899)	(5.902)
Workplace average earnings	99562.5	100103.0
	(37256.5)	(37746.9)
Share employees with compulsory education	0.294	0.293
	(0.186)	(0.187)
Share employees with high school education	0.427	0.426
	(0.176)	(0.176)
Share employees with college education	0.233	0.235
	(0.250)	(0.251)
Share female employees	0.507	0.504
	(0.313)	(0.311)
Workplace average sickdays, Fall 1988	3.975	3.976
	(1.922)	(1.941)
Workplace average sickdays, Spring 1988	3.357	3.346
	(1.651)	(1.643)
Workplace average sickdays, Fall 1987	2.557	2.541
	(1.387)	(1.380)
Workplace average sickdays, Spring 1987	2.664	2.647
	(1.350)	(1.350)

NOTES.— The table presents means and standard deviations (in parentheses) of individual and workplace characteristics for individuals assigned to the control and treatment group, respectively. The sample consists of workers living in Gothenburg municipality and working at workplaces with 10-100 employees.

TABLE A2. Summary statistics by share of treated co-workers

	(1) $< 13\%$	(2) $13\% - 28\%$	(3) $28\% - 35\%$	(4) $> 35\%$
Individual characteristics				
Absence days $<$ 15 day spells, Fall 1988	3.714	4.195	4.219	4.651
	(5.887)	(6.247)	(6.276)	(6.607)
Absence days $<$ 15 day spells, Spring 1988	3.061	3.395	3.570	3.872
	(5.017)	(5.362)	(5.478)	(5.715)
Absence days $<$ 15 day spells, Fall 1987	2.293	2.680	2.642	2.926
	(4.475)	(4.952)	(4.836)	(5.078)
Absence days $<$ 15 day spells, Spring 1987	2.373	2.711	2.811	3.018
	(4.534)	(4.880)	(5.002)	(5.268)
Female	0.405	0.447	0.548	0.654
	(0.491)	(0.497)	(0.498)	(0.476)
Compulsory schooling	0.262	0.286	0.289	0.296
	(0.440)	(0.452)	(0.453)	(0.456)
High school	0.461	0.479	0.428	0.408
	(0.498)	(0.500)	(0.495)	(0.491)
College	0.263	0.217	0.263	0.275
	(0.441)	(0.412)	(0.440)	(0.446)
Earnings in 1988, SEK	104915.4	102790.1	98076.4	88491.7
	(74070.6)	(70842.9)	(67552.0)	(60157.2)
Age	35.57	36.17	37.03	36.57
	(12.35)	(12.73)	(12.87)	(12.78)
Workplace characteristics				
Share treated	0.127	0.278	0.351	0.466
	(0.0783)	(0.0237)	(0.0220)	(0.0648)
Share commuters	0.642	0.365	0.281	0.180
	(0.240)	(0.127)	(0.110)	(0.105)
Number of employees	36.55	41.00	44.24	36.60
	(25.14)	(24.41)	(25.45)	(25.82)
Workplace average age	36.40	36.19	37.05	36.57
	(5.907)	(5.840)	(5.882)	(5.938)
Workplace average earnings	106121.6	103926.7	99516.2	88510.3
	(38503.1)	(38019.8)	(37549.3)	(32857.4)
Share employees with compulsory education	0.302	0.288	0.288	0.293
	(0.185)	(0.182)	(0.184)	(0.195)
Share employees with high school education	0.450	0.457	0.410	0.385
	(0.179)	(0.168)	(0.177)	(0.169)
Share employees with college education	0.210	0.210	0.253	0.269
	(0.241)	(0.228)	(0.265)	(0.262)
Share female employees	0.418	0.433	0.541	0.648
	(0.295)	(0.302)	(0.303)	(0.292)
Workplace average sickdays, Fall 1988	3.471	3.952	4.064	4.515
	(1.635)	(1.843)	(1.900)	(2.201)
Workplace average sickdays, Spring 1988	2.975	3.297	3.427	3.782
	(1.472)	(1.623)	(1.577)	(1.817)
Workplace average sickdays, Fall 1987	2.260	2.556	2.577	2.862
	(1.266)	(1.397)	(1.314)	(1.495)
Workplace average sickdays, Spring 1987	2.365	2.624	2.717	2.973
	(1.216)	(1.311)	(1.270)	(1.531)

NOTES.— The table presents means and standard deviations (in parentheses) of individual and workplace characteristics for individuals with different proportions of treated co-workers, where the subgroups are defined by the 25th, 50th, and 75th percentiles. The sample consists of workers living in Gothenburg municipality and working at workplaces with 10-100 employees.

TABLE A3. Parameter estimates from the OLS estimation of the effect of treatment and effect of share treated co-workers on sickness absence days

	All <15 days	Treated <15 days	Non-treated <8 days
A. *Sickness absence days in Fall 1988*			
Treatment	0.36***		
	(0.05)		
Proportion treated	0.82**	0.53	0.92***
	(0.33)	(0.47)	(0.32)
B. *Sickness absence days in Fall 1987 (Placebo)*			
Treatment	0.03		
	(0.04)		
Proportion treated	-0.09	-0.40	-0.06
	(0.22)	(0.31)	(0.22)
Observations	61715	30339	31376

NOTES.— The outcome variables are the number of days on non-monitored absence in the Fall of 1988 and the Fall of 1987 (placebo year). Included covariates are gender, age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, dummies for industry affiliation, workplace average sickness absence days (excluding individual i) in fall and spring of 1987 and spring 1988. The samples consists of individuals living in Gothenburg municipality and employed at workplaces with 10-100 employees. Standard errors are clustered at the workplace level. *p<0.1, **p<0.05 ***p<0.01.

TABLE A4. Parameter estimates from the OLS estimation of the effect of treatment and effect of share treated co-workers on monthly sickness absence days in 1988

Coefficient on	Treatment	Proportion treated
January	-0.01	-0.06
	(0.01)	(0.08)
February	0.00	-0.08
	(0.01)	(0.09)
March	0.01	0.08
	(0.01)	(0.09)
April	0.02*	-0.10
	(0.01)	(0.09)
May	-0.01	0.11
	(0.01)	(0.08)
June	0.01	0.30***
	(0.01)	(0.08)
July	0.06***	0.19**
	(0.01)	(0.09)
August	0.06***	0.15
	(0.01)	(0.10)
September	0.05***	0.14
	(0.02)	(0.10)
October	0.06***	0.25**
	(0.02)	(0.10)
November	0.06***	0.06
	(0.02)	(0.10)
December	0.08***	0.04
	(0.02)	(0.13)

NOTES.— The outcome variables are the number of days on sickness absence in spells that are shorter than 15-days in each month of 1988. Included covariates are gender, age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, dummies for industry affiliation, workplace average sickness absence days (excluding individual i) in fall and spring of 1987 and spring 1988. The samples consists of individuals living in Gothenburg municipality and employed at workplaces with 10-100 employees. Standard errors are clustered at the workplace level. *p<0.1, **p<0.05 ***p<0.01.

TABLE A5. Summary statistics by gender

	Male	Female
Individual characteristics		
Absence days < 15 day spells, Fall 1988	3.900	4.436
	(6.307)	(6.187)
Absence days < 15 day spells, Spring 1988	3.136	3.764
	(5.319)	(5.440)
Absence days < 15 day spells, Fall 1987	2.367	2.863
	(4.720)	(4.920)
Absence days < 15 day spells, Spring 1987	2.447	2.967
	(4.722)	(5.084)
Compulsory schooling	0.274	0.290
	(0.446)	(0.454)
High school	0.471	0.421
	(0.499)	(0.494)
College	0.233	0.276
	(0.423)	(0.447)
Earnings in 1988, SEK	117900.1	80476.1
	(81158.3)	(47821.3)
Age	35.87	36.71
	(12.53)	(12.81)
Workplace characteristics		
Share treated	0.272	0.322
	(0.132)	(0.137)
Share commuters	0.433	0.328
	(0.232)	(0.235)
Number of employees	39.97	38.95
	(25.29)	(25.52)
Workplace average age	36.07	37.00
	(5.682)	(6.070)
Workplace average earnings	106964.6	92933.2
	(38012.9)	(35664.6)
Share employees with compulsory education	0.310	0.277
	(0.179)	(0.192)
Share employees with high school education	0.456	0.399
	(0.167)	(0.179
Share employees with college education	0.180	0.285
	(0.222)	(0.264)
Share female employees	0.312	0.693
	(0.242)	(0.251)
Workplace average sickdays, Fall 1988	3.899	4.049
	(1.894)	(1.964)
Workplace average sickdays, Spring 1988	3.243	3.456
	(1.556)	(1.723)
Workplace average sickdays, Fall 1987	2.485	2.612
	(1.333)	(1.428)
Workplace average sickdays, Spring 1987	2.561	2.747
	(1.248)	(1.437)

NOTES.— The table presents means and standard deviations (in parentheses) of individual and workplace characteristics for male and female workers separately. The sample consists of workers living in Gothenburg municipality and working at workplaces with 10-100 employees.

TABLE A6. Placebo estimates from the OLS estimation of the effect of treatment and effect of share treated co-workers on non-monitored absence in 1987

	All <15 days	Treated <15 days	Non-treated <8 days
A. *Fall 1987, Male workers*			
Treatment	0.03		
	(0.05)		
Proportion treated	0.08	-0.26	-0.02
	(0.32)	(0.46)	(0.33)
N	29826	14710	15116
B. *Fall 1987, Female workers*			
Treatment	0.02		
	(0.05)		
Proportion treated	-0.18	-0.40	-0.10
	(0.32)	(0.45)	(0.31)
N	31889	15629	16260

NOTES.— The outcome variables are the number of days on non-monitored absence in the fall of 1987. Included covariates are gender, age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, dummies for industry affiliation, workplace average sickness absence days (excluding individual i) in fall and spring of 1987 and spring 1988. The samples consists of individuals living in Gothenburg municipality and employed at workplaces with 10-100 employees. Standard errors are clustered at the workplace level. *p<0.1, **p<0.05 ***p<0.01.

PAPER 4

Mothers' Income Recovery after Childbearing

Nikolay Angelov Arizo Karimi

ABSTRACT We examine the temporal pattern of the causal effect of fertility on female labor income using panel data based on Swedish registers, and instrumenting family size with parents' preferences for a mixed-sex sibling composition. The effect of a third child over the life cycle is evaluated against the alternative of stopping at two children. Our findings indicate a sizeable income reduction in the immediate years after birth, followed by a catching-up effect in income. The short-lived reduction likely corresponds to formal parental leave. Gauging the magnitude of the effect, we find that income decreases by roughly 11 percent over a 10-year horizon after birth. No effects are found on long-run wage rates or on contracted hours of work.

1. Introduction

Despite the dramatic increase in women's labor force participation observed in the last decades, women continue to take the main responsibility for the family. Due to conflicting demands on time from market and non-market work, some women may drop out of the labor market entirely, while others resort to part-time work. An extensive literature has focused on the relationship between fertility and female labor market outcomes, and it is well established that childbearing reduces women's subsequent labor supply. Many countries have increased efforts to reduce the negative effect of childbearing on female labor supply, and to date nearly all OECD countries offer government funded programs with wage replaced parental leave. Along with the other Nordic countries, Sweden has long been at the forefront regarding policy initiatives aimed at helping parents to reconcile market work and family, and the parental leave system is generous in an international comparison. As an assessment of the effectiveness of family programs, the impact of childbearing on women's labor market behavior in a family friendly policy context provides a lower bound to the causal effect of fertility.

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We use Swedish administrative data and exploit parents' preferences for a mixed-sex sibling composition as a source of exogenous variation in family size to measure the causal effect of children on female income. The method was originally applied by Angrist and Evans (1998) and has been used in several subsequent studies.[1] An important feature of the present study is, however, that we employ the sex-mix strategy in a novel way to uncover the temporal pattern of the fertility effect on mothers' labor income with respect to time since birth. Understanding the dynamics of individuals' labor supply response to childbearing is crucial to gauge the total effect of children. To this end, we use longitudinal population-wide data and follow individuals over a 15-year horizon after birth to assess how the the causal effect of an increase in family size evolves over child age. In addition to studying labor supply responses, we assess whether an increase in family size affects women's wage rates and contracted hours of work in the long-run. Furthermore, we evaluate how the temporal pattern of the fertility effect compare across different methodological approaches. This analysis provides insights into how strategies based on cross-sectional data perform in reconstructing life cycle patterns obtained using longitudinal data.

Several previous studies examine the causal effect of children on earnings and labor supply over the life cycle using instrumental variables. While most of the existing work exploits twin birth as an instrument for higher order fertility, the data and methods used to uncover the *dynamics* of the fertility effect have varied. One strategy, for example, has been to follow a cohort of mothers at different points in time using successive cross-sections (Bronars and Grogger, 1994), and another to construct a synthetic-cohort life cycle by exploiting the fact that women in a cross-sectional sample had their (twin) births at different points in time (see e.g. Vere, 2011 and Jacobsen et al., 1999). Angrist and Evans (1998) instead take advantage of having two instruments for family size - twin birth and same-sex siblings - coupled with the fact that third children born as a consequence of twinning are always older than third children born for other reasons (twin third children are always the exact same age as their sibling). Assuming that the age difference between third children is the only reason why the same-sex and twins-instruments generate different estimates of the effect of family size on labor supply, the authors provide estimates of the (child) age at which the effect of children dissipates. Using Swedish panel data, Hirvonen (2011) exploits the same-sex instrument and measures long-run effects by measuring the effect of having given birth to a third child before or during the year in which the individuals' labor market status is observed, for consecutive years over a long time horizon.[2] The findings from this body of work suggest that

[1] For example, Iacovou (2001) uses the sex-mix strategy on data from the United Kingdom, Maurin and Moschion (2009) on data from France, Cruces and Galiani (2007) on data from Argentina and Mexico and Hirvonen (2009) on data from Sweden.

[2] A few studies also estimate then life cycle effect of first childbirth. Fitzenberger et al. (2013) use data from Germany and a dynamic treatment approach to measure the effect on employment of having a first child now versus later. They find a large and persistent negative causal effect of first childbirth on subsequent

the impacts of childbearing on female labor supply and earnings are largest in the short-run and thus gradually catch up with time since birth.

The present paper contributes to this existing literature in a number of ways. Most importantly, by being able to follow the *same* mothers in a longitudinal data set over a 15-year horizon after birth, we can recover the true temporal pattern of the labor supply response to childbearing. In our main analysis, we sample mothers with two children, of which the oldest child was born 1981-1989. We define treatment to equal unity if an individual gave birth to a third child in 1990 and zero otherwise. We thus fix the year of treatment to occur in one specific calendar year and estimate the effect of having a third child on yearly labor market outcomes over a 15-year horizon after birth.[3] To study the temporal pattern of the fertility effect, we wish to compare the impact of a third child against the alternative of staying in the state of having only two children. To this end, control group individuals are dropped from the estimation sample from the year that they give birth to a third child onwards, under the assumption that this "censoring" of observations is ignorable conditional on observable characteristics. Thus, we estimate life cycle effects of childbearing under less restrictive assumptions than in e.g. Angrist and Evans (1998). In addition, we need not worry about potential age- and cohort effects which could potentially confound synthetic-cohort life cycle effects. Moreover, in contrast to studies where successive cross-sections are used to follow a cohort at two time periods - which lacks information on events between those points - we are able to evaluate the yearly evolution of the fertility effect over an extended time period.

Our contribution relative to Hirvonen (2009) lies in the different estimation approach taken in our study and, consequently, in the parameter recovered. While Hirvonen (2009) successively moves untreated individuals to the treatment group as they have (more) children, we successively drop these individuals. The former estimates a weighted average of the impact of having a third child now and having given birth to a third child some while ago. Instead, our approach measures the effect of having a third child over the life cycle against the alternative of having stopped at two children.

Finally, we can compare estimates obtained with our panel data with estimates obtained from data extracted to mimic a cross-sectional data set for one year. This allows a comparison between, on the one hand, estimates of the effect of children on Swedish women's earnings and estimates for women in the US and, on the other hand, the temporal pattern of the effect of childbearing generated by following the same individuals over time with that obtained

employment. Moreover, the overall treatment effect does not level off to zero, suggesting that employment rates of the treatment group do not catch up completely to the control group. For Sweden, Angelov et al. (2013) compare the income- and wage trajectories of women in relation to their male partners before and after parenthood in a difference-in-differences setup. Focusing on the within-couple gap, they find that the effect of parenthood on the gender gap in income and wages 15 years after the birth of the first child increased with 35 and 10 percentage points, respectively.

[3] Sensitivity analyses with multiple third birth cohorts pooled together are also provided.

from a synthetic-cohort life cycle. Finally, we compare long-run estimates from our panel data with those generated by exploiting variation in family size from the two instruments - twin births and same-sex siblings - as in Angrist and Evans (1998).

Using the OLS estimator and following the same mothers over time, we find that having a third child is associated with a statistically and economically significant decrease in mothers' earnings that largely takes place during the first couple of years after giving birth, followed by a gradual catching-up effect over the life cycle and nearly full recovery of earnings 15 years after birth. Our 2SLS estimates support this finding, but suggest a faster recovery of earnings compared to the OLS estimator. The same qualitative pattern holds true also for labor market participation. The magnitude of the effect on earnings is estimated to amount to roughly 11 percent lower income over a 10-year horizon after birth. Sensitivity analyses where data is pooled across multiple third-birth cohorts provide the same results as obtained from the main analysis. Thus, the results are not specific to mothers who gave birth to a third child in 1990. No effects are found on long run wage rates or on contracted working hours.

Estimates generated by using the occurrence of twin births as an instrument for third births are smaller in magnitude than estimates generated by the same-sex instrument. While some of this difference might be driven by age differences between twin third children and other third children (see Angrist and Evans 1998), our findings tentatively suggest that there is a direct effect of having a twin birth, such that estimates generated with the two instruments are not directly comparable. Finally, we construct a synthetic-cohort life cycle by exploiting that twin second births occur at different time periods. The findings from this analysis are in line with the results obtained using the panel data set and suggest that the impact of childbearing is largest in the first couple of years after birth, and then wears off. Thus, constructing a synthetic-cohort life cycle to estimate long-run effects seems to work well in reproducing the true life cycle pattern.

To a large extent, our result that childbearing produces short-lived labor supply reductions for women are in line with findings from the United States. For example, Jacobsen et al. (1999) study the effect of fertility on married women's labor supply and earnings and find that the overall effects of childbearing are small, but that there are significant impacts in the years following birth. Similarly, Vere (2011) finds that the effect of fertility on female labor supply are greatest when the child is born and then rapidly decline. Bronars and Grogger (1994) focus on the effects of unwed motherhood and find large short run effects of childbearing on labor force participation, with most of the adverse effects dissipating over time for whites, but a more persistent negative effect for black unwed mothers.[4] However,

[4] For Italy, Rondinelli and Zizza (2011) use exogenous variation in family size resulting from infertility shocks and find that childbearing does not have a persistent effect on Italian women's labor market outcomes. Since they estimate the effect for women aged 35 or older, they interpret the estimated effects as long run effects.

our findings suggest a faster earnings recovery after childbearing, and a smaller negative effect of children on women's earnings compared to the United States. The difference in the magnitude of the fertility effect is likely driven by institutional differences. For example, Sweden offers state mandated job-protected parental leave, with wage replacement, which likely allows less disruptive careers for mothers. While the labor supply reduction is rather short-lived in Sweden, the short-run effects are sizeable. Nevertheless, we do not find any evidence that an additional child impacts long-run wage rates. The latter finding casts some doubt on the importance of human capital depreciation and foregone investments in human capital in explaining the motherhood wage penalty, at least at the margin of moving from two to three children.

The next section of the paper briefly sketches the institutional setting and the Swedish parental leave system, the subsequent section outlines the data sources and the sample used. Section 4 describes the empirical strategy and Section 5 presents the results from our main analysis. Section 6 presents the results from analyses based on a stock sample of mothers in one year and compares these estimates with those obtained using panel data. Section 7 concludes the paper.

2. Institutional Setting

From the mid 1960s to the end of the 1980s, Sweden extended its family policies extensively, with provisions of financial support for families with young children, where the policies were designed to facilitate labor force participation for mothers. Since 1974, Sweden offers both mothers and fathers the right to paid parental leave and the duration of paid leave has been successively extended, in particular throughout the 1980s. To date, the system offers 16 months of paid leave, of which 13 months are reimbursed at a rate of 80 percent of previous earnings, and three months at a lower fixed rate. In addition, job-protection exceeds the 16 months of paid leave: parents are entitled to full-time leave, with or without wage replacement, during the child's first 18 months of life. Parents are also entitled to reduce their working hours with up to 25 percent until the child turns eight years old. An important characteristic of the parental leave system is that benefits are conditioned on employment before birth. Thus, the system exhibits strong incentives to be attached to the labor market before birth. This has likely contributed to the high labor force participation rate observed in the Nordic countries (Jaumotte, 2003; Baker and Milligan, 2008; Han et al., 2009; Waldfogel, 1998).

In combination with high labor force participation rates, Sweden has long had high total fertility rates. This relatively unique combination has in several studies been attributed to family policies. For example, Stafford and Sundström (1996) find that family policies stimulated both fertility and women's paid work by reducing the cost of having children while requiring parents to be employed to collect full benefits. Also, Björklund (2006) studies the evolution of completed fertility patterns for Swedish women born in 1925 to 1958 and makes comparisons with neighboring countries in which policies were not extended to the same degree as in Sweden. He finds that the extensions of family policies raised the level of fertility, shortened the spacing of births and induced fluctuations in the period fertility rates.

Since mothers stand for the majority of parental leave take-up, it is not difficult to expect a nearly full withdrawal from the labor market in the short run. Furthermore, Swedish parents are entitled to reduce their working hours with up to 25 percent until the child turns 8 years old, so a longer-run reduction of working hours might also be expected. However, with job-protection for 18 months after birth, an increase in family size is perhaps less likely to result in labor supply responses on the extensive margin in the medium- to long run. Within this institutional setting, we study the impact of additional childbearing on both earnings and participation, as well as on long run wage rates and contracted hours of work.

3. Data

The analysis is based on Swedish population-wide administrative registers. We make use of the multi-generational register which links all children to their biological parents and contains information on birth year, birth order and gender of each of the individuals' children. To these data we add individual level background characteristics as well as annual labor income from the LOUISE register. Annual labor income does not include parental leave- or other benefits, and thus measure income from market work. The information provided in LOUISE covers the time period 1985 through 2007, and in the multi-generational register we observe the number of children born to all women by the end of 2007.

Labor income reflects both hours worked and hourly wage rates, and we cannot distinguish the effect on hours and wage rates when estimating the effect of children on earnings. However, in the short run, it is unlikely that hourly wage rates are affected, so that any short-run effects on earnings should most likely be interpreted as labor supply responses. To analyze whether also hourly wages are affected by childbearing, we add individual level data on full-time equivalent monthly wages (thus comparable to hourly wage rates), obtained from the Wage Structure Statistics and covering the entire public sector and about half of the private sector from 1985 through 2007. While we lack information on hours worked, the latter register includes information on *contracted* work hours, expressed as percent of full-time.[5]

Our identification strategy, which is presented in closer detail in the next section, relies on exploiting the sex-mix of the first two siblings, so the population of interest is mothers

[5] Important to note is that wages and work hours are only available for individuals present at the workplace in the survey month for each year. Thus, individuals who are e.g. on sick leave, parental leave or other absence are not included in the wage data.

with at least two children. In our main analysis we restrict attention to mothers with two children, of which the oldest child was born between 1981 and 1989. Furthermore, we make the additional restriction that mothers are at most 45 years old in 1989. This leaves us with, in total, 212,994 individuals. Table A1 in the Appendix reports summary statistics for our sample and shows that the individuals had given birth to almost 2.4 children, on average, by the end of 2007. About 51 percent of the first born children were boys and about 51 percent of the second born children were boys. Moreover, 26 percent of the sample had two first born boys whereas 23 percent had two first born girls. The age at first birth for the women in our sample is on average 25. More than half of the sample, 52 percent, had attained no more than high school education by the end of 1989, 27 percent had some college education or more and around 3.4 percent was born in a non-Nordic country. Lastly, their average earnings in 1989 amounted to around 107,000 SEK.

4. Empirical Strategy

Fertility decisions are likely made taking into account one's earnings potential. Moreover, unobserved individual heterogeneity in preferences might affect both labor market and fertility decisions (see e.g. Browning, 1992, for an overview of the endogeneity problems inherent in estimating the effect of fertility on labor market outcomes). Failing to account for the endogeneity of fertility means that an estimated relationship between children and their parents' labor market behavior will not have a causal interpretation. To address these issues, we follow Angrist and Evans (1998) and exploit parents' preferences for a mixed-sex sibling composition as a source of exogenous variation in family size. Specifically, we exploit the fact that parents whose first two children are of the same sex are more likely to move to higher parity compared to parents whose first two children are of mixed sex. While the sex-mix of children, which is in essence randomly assigned, has an impact on the number of children, it is not likely to have a direct impact on parental labor market behavior. Therefore, a dummy variable indicating whether an individual's first two children are of the same sex can be used as an instrumental variable for higher order fertility among individuals with at least two children.

Interest then lies in the difference in labor market outcomes of a mother with and without further childbearing. In particular, the focus of this paper is to investigate the temporal pattern of this difference over child age. Thus, we are interested in how a potential impact of a third child on women's labor market outcomes evolves as the child grows older. To this end, we restrict attention to the population of mothers with two children, whose oldest child was born between 1981 and 1989. We then fix the year of treatment, where treatment is defined as giving birth to a third child, to occur in one specific year, namely 1990. Let D_i be an indicator for treatment where D_i equals unity if individual i gave birth to a third child in 1990 and zero otherwise. The labor market response, y_i is related to the treatment, D_i according to the following equation:

$$y_i = \beta_0 + \beta_1 D_i + \mathbf{x}_i{}'\beta_2 + \epsilon_i \tag{4.1}$$

where $\mathbf{x}_i$ is a vector of personal characteristics including mother *i:s* age at first birth, a full set of dummy variables indicating mothers' birth year and dummy variables indicating first- and second born boys. The latter two variables are included to control for potential additive effects of child gender which could arise if, for instance, parents behave differently towards boys and girls (Angrist & Evans 1998). Equation (4.1) is estimated using both OLS estimation and 2SLS estimation, with the latter using an indicator variable for the first two children being of the same sex as an instrumental variable for the treatment variable D_i. The first-stage relationship is given by the following equation:

$$D_i = \gamma_0 + \gamma_1 Same_i + \mathbf{x}_i{}'\gamma_2 + \nu_i \tag{4.2}$$

where $Same_i$ is a dummy variable that takes the value one if individual *i:s* first two children are of the same sex and zero otherwise and $\mathbf{x}_i$ contains the same covariates as in specification (4.1).

Since our treatment variable is defined as giving birth to a third child in 1990 and our data allows us to follow all individuals until 2007, we can investigate how the effect of treatment evolves over child age for the *same individuals*. Specifically, we estimate separate yearly regressions of the effect of a third child on mothers' labor market outcomes at child ages 0 (1990) to 15 (2005) using both OLS and 2SLS estimation.

Because we are interested in how the effect of a third birth evolves over child age, we always want to compare the impact of a third child against the alternative of staying at two children. For this reason, when estimating the regression equation (4.1), non-treated individuals (i.e., individuals with $D_i = 0$) are only included in the estimation sample until they potentially have a third child and are censored starting from the year that they get a third child onwards. The same censoring is applied to the treated individuals (i.e., individuals with $D_i = 1$) from the year that they have a fourth child onwards.

The key conditions needed for consistency of the IV estimator is that there exists a first stage relationship and that the instrument is not correlated with the error term in Equation (4.1). The first assumption can be tested directly and evidence of an existing first stage relationship is shown in the next section. To evaluate the validity of the second assumption, we can study whether mothers to mixed- and same-sex siblings differ with respect to personal characteristics. Table 4.1 reports average personal characteristics among mothers to same- and mixed-sex siblings, respectively, and the estimated differences in these characteristics between the two groups of mothers. As seen from Table 4.1, mothers of same-sex children are, on average, 0.145 percentage points more likely to have given birth to a third child in 1990 and had more children by the end of 2007. These findings are in support of an existing, positive, first stage relationship. Moreover, mothers whose first two children are of the same sex are more likely to have two boys than two girls. They are also slightly younger at first birth compared to women with mixed-sex children. Importantly, however, there are no differences between the groups concerning educational attainment, country of origin, or the spacing between the first two children. Thus, the results presented in Table 4.1 supports the notion of the instrument being 'as good as randomly assigned'. In contrast, there seem to be some differences with respect to personal characteristics between mothers who were and were not treated, i.e., between mothers who gave birth to a third child in 1990 and mothers who did not, as indicated by the results shown in Table 4.2. Specifically, treated mothers are more likely to have finished at most compulsory education, are more likely to have been born outside the Nordic countries and have, on average, shorter interval between their first two children.

Another potential threat to our empirical strategy could be inherent in our censoring approach. Specifically, although the instrument is as good as random with respect to all other determinants of labor market outcomes in the full sample of mothers with two children, one might be worried that this will not be the case as we sequentially exclude individuals who move to higher parities. An identifying assumption here is thus that the censoring is ignorable conditional on observable characteristics. We can again study differences in observable characteristics, this time between the full sample and the censored sample. To this end, we estimate differences in average characteristics between mothers with same- and mixed-sex siblings 10 years after third birth, where observations for those women who have given birth to a third child during 1991 and 2000 have been censored, as have observations for individuals who gave birth to a fourth child during the same time frame. The findings from this analysis are presented in Figure A1, where the leftmost graph plots the differences in average characteristics between mothers of same- and mixed-sex siblings for the full sample (as presented in Table 4.1) and the rightmost graph presents the same differences in year 10 after third birth. Observations for individuals in the control group who gave birth to a third child between 1991 and 2000 have then been censored, and likewise for treated individuals who gave birth to a fourth child during the same time period. As seen from Figure A1, the differences in characteristics are strikingly similar in the full and censored samples. Thus, there is no evidence of the censoring being non-random with respect to observable characteristics; rather, the instrument is still as good as randomly assigned in the censored sample. In Figure A2 we also plot the proportion of mothers with same-sex children in the full sample and in the censored samples, respectively. The proportion of mothers with same-sex children decreases with the number of women dropped from the sample (who are dropped due to giving birth to a third child if they belong to the control group or due to giving birth to a fourth child if they belong to the treatment group). However, the difference in the proportion of mothers with same-sex children between the full sample and the fully censored sample (year 15 after third birth) amounts to only 0.01. To summarize, we do not find any evidence that the censoring approach leaves us with an increasingly selected sample.

Estimates of the long-run effect of third births was also provided by Angrist and Evans (1998), using a different approach. Based on data from Census Public Use Micro Samples (PUMS), the authors exploit fact that they have two instruments for higher order parity: same-sex of first two children and the occurrence of twin births in the second birth. The latter is referred to as the Twins-2 instrument. They note that third children born as a consequence of twinning are always older than third children born for other reasons, since third children born as twins are always of the exact same age as second children, while other third children are at least one year older than their younger sibling. The authors find smaller effects when using the Twins-2 instrument compared to the same-sex instrument and explain that this may be due to the third children born as a consequence of same-sex siblings being younger than twin third children, if the effect of childbearing wears off with child age. Further, relying on the assumption that child age is the only reason for the difference between the Twins-2 and same-sex estimates, they use an IV procedure to estimate the long-run effects of childbearing and conclude that the effect of fertility wears off by the time the third child is 13 years old. In our setting, we thus rely on a less restrictive assumption of ignorable censoring conditional on covariates compared to the assumptions made in Angrist and Evans (1998).

Finally, another potential threat to the identifying strategy employed here is that the instrument fails the restriction of not having a direct effect on labor market outcomes. For instance, Rosenzweig and Wolpin (2000) study expenditures per children in rural India and find that same-sex siblings are associated with significantly lower levels of expenditures due to hand-me-down savings for e.g. clothing. It is likely, however, that such concerns are more valid in developing countries where spending on clothing is likely to make up a larger fraction of household expenditures. For Sweden, the Household Budget Survey reports that the percentage share of total consumption per household on clothes and shoes in 2007-2009 ranges between 5.0 and 6.3 depending on the number of children and whether the household is a single- or two-parent household (Statistics Sweden, 2010). These figures are relatively low compared to the figures from India reported by Rosenzweig and Wolpin (2000), which were found to be roughly 11 percent of household income. In addition, Rosenzweig and Wolpin (2000) estimated the hand-me-down savings for child goods to amount to 1.3 percent of average earnings. Thus, if these savings are existent also in Sweden, they would likely be too small to be able to explain a major part of a negative relationship between same-sex children and parents' labor earnings.

Another potential concern, evident in some developing countries, is a potential son preference among parents, which could affect the sex-composition of children through e.g. selective abortions. However, as shown in Table A1, the summary statistics for our sample rules this out as the fraction of first-born boys is close to 50 percent. Thus, a preference towards boys is not likely to invalidate our estimates and it does not seem likely that the sex-mix of children would affect expenditures to a practically significant degree in Sweden.

TABLE 4.1. Summary statistics for mothers with same- and mixed-sex siblings

	(1) Same	(2) Mixed	(1)-(2) Difference
Number of kids in 2007	2.401	2.349	0.0519*** (0.00292)
3rd child born 1990 (treated)	0.0797	0.0652	0.0145*** (0.00112)
Boy 1st	0.528	0.500	0.0284*** (0.00217)
Boy 2nd	0.528	0.500	0.0279*** (0.00217)
Two boys	0.528	0	0.528*** (0.00151)
Two girls	0.472	0	0.472*** (0.00151)
Age at 1st birth	25.11	25.05	0.0558*** (0.0168)
Compulsory schooling	0.208	0.210	-0.00190 (0.00176)
High school	0.524	0.523	0.000751 (0.00217)
College	0.268	0.267	0.00115 (0.00192)
Non-Nordic background	0.0354	0.0354	0.0000591 (0.000801)
Years btw 1st and 2nd birth	3.851	3.839	0.0115 (0.0114)
Observations	103839	109155	

NOTES.— The table depicts summary statistics for mothers with same- and mixed-sex siblings, respectively, and the differences in characteristics between the two groups. Standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

TABLE 4.2. Summary statistics by treatment status

	(1) Treated	(2) Control	(1)-(2) Difference
Number of kids in 2007	3.381	2.296	1.084*** (0.00513)
Boy 1st	0.519	0.513	0.00631 (0.00418)
Boy 2nd	0.524	0.513	0.0110** (0.00418)
Two boys	0.291	0.255	0.0356*** (0.00366)
Two girls	0.247	0.229	0.0183*** (0.00352)
Age at 1st birth	24.01	25.16	-1.150*** (0.0324)
Compulsory schooling	0.226	0.208	0.0186*** (0.00340)
High school	0.512	0.524	-0.0126** (0.00418)
College	0.262	0.268	-0.00603 (0.00370)
Non-Nordic background	0.0387	0.0352	0.00350* (0.00155)
Years btw 1st and 2nd birth	3.316	3.886	-0.570*** (0.0220)
Observations	15388	197606	

NOTES.— The table depicts summary statistics for mothers by their treatment status, and differences in characteristics between treated and non-treated. Treatment is defined to equal unity if an individual gave birth to a third child in 1990, and zero otherwise. Standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

5. Results

5.1. Sex Composition and Fertility. Before exploring the effects of fertility on mothers' labor earnings, we study the presence of mixed-sex preferences among Swedish mothers in general, and among mothers in our sample in more detail. Parental preferences for a mixed-sex sibling composition have been documented in several industrialized countries. For the Nordic countries, Andersson et al. (2006) find a distinct preference for at least one child of each sex among parents of two children. However, they do not find an effect of the sex of the firstborn child on second-birth probabilities. Furthermore, for Denmark, Norway and Sweden they find that parents develop a preference for daughters in third births. To analyze the impact of child gender on higher order birth probabilities, we focus first on the sample of mothers who were at most aged 45 and who had given birth to at least one child by the end of 1989. Table 4.3 shows the fraction of mothers with more than one child (among women with at least one child) conditional on the sex of the first child (panel A) and the fraction of mothers with more than two children (among women with at least two children) conditional on the sex-mix of the first two children. As seen in Panel A, there is hardly any effect of the sex of the first child on second-birth probabilities; the fraction of mothers who have more than one child among those whose first child was a boy does not differ from the fraction of mothers who have more than one child among those whose first child was a girl. Thus, consistent with Andersson et al. (2006), there does not seem to be an effect of the sex of the firstborn child on second-birth probabilities.

In contrast, as shown in Panel B of Table 4.3, there is a distinct difference in the probability of moving to higher parity among mothers with at least two children, depending on the sex-mix of the first two children. Specifically, having two children of the same sex is associated with roughly 5 percentage points higher likelihood of moving to a third birth compared to having two children of mixed sex. Moreover, there seems to be a small preference for girls in third births as the fraction of mothers moving to higher parity is somewhat larger among mothers with two boys compared to mothers with two girls.

To study the relevance of same-sex sibship as an instrument for our treatment indicator, D_i, which indicates whether a third child was born to woman i in 1990, we perform yearly regressions of the impact of the same-sex indicator on D_i, starting from the birth year of the third child until the third child is 10 years old. Coefficients from the OLS regressions of the first-stage relationship are depicted in Table 4.4, for each year starting from the year that the third child is born until the year that the child turns 10 years old, separately. The different sample sizes in the columns of Table 4.4 is due to the censoring described in the previous chapter. The first line depicts results for the full sample, i.e., before any individuals are excluded from the sample, and suggests that having two children of the same sex increases the likelihood of having a third child in 1990 by 0.14 percentage points, on average. This estimate differs significantly from the first-stage estimate provided in e.g. Angrist and Evans (1998) who find the effect of same-sex on third birth probabilities to range between 6 and 7 percentage points. However, the difference between our first-stage estimate and that of Angrist and Evans (1998) and other papers that use the sex-mix strategy is expected; earlier studies estimate the effect of same-sex children on the likelihood of haven given birth to a third child, whereas we estimate the effect of same-sex siblings on the likelihood of giving birth to a third child in a *particular* year, namely in 1990. So our estimate should be lower.[6][7]

Moreover, the censoring that we apply to the sample in our estimations does not lead to dramatic changes of the first-stage estimate. In year 10, the estimated effect of having two children of the same sex on the likelihood of having progressed to higher parity in 1990 is 0.17 percentage points, only slightly higher than in year 0. The F-statistic for testing the relevance of the instrument conditional on covariates ranges between 163 and 195, which is well above the rule of thumb of 10 that is sometimes suggested. Thus, the same-sex instrument does not appear to be a weak one.

[6] When using a cross-sectional data set from 1990 and the same-sex strategy, we get a first-stage estimate of about 5 percentage points increase in the likelihood to move to higher parity among women with at least two children. This is further discussed in a subsequent section of the paper.

[7] The first-stage estimate is slightly lower for mothers with two girls compared to mothers with two boys, which confirms that, for third births, Swedish parents have a slight preference for daughters as found in e.g. Andersson et al. (2006). The results from this analysis are available upon request.

TABLE 4.3. Higher order parity by child gender

	Proportion of sample	Prop. that had another child
A. Families with one or more children		
(i.) First born boy	0.514	0.844
(ii.) First born girl	0.486	0.845
Difference (ii.)-(i.)		0.001 (0.001)
B. Families with two or more children		
(i.) One boy, one girl	0.500	0.419
(ii.) Two boys	0.265	0.479
(iii.) Two girls	0.236	0.473
(iv.) Both same sex	0.500	0.476
Difference (iv.)-(i.)		0.057*** (0.001)

NOTES.— The sample consists of mothers who had at least one child in 1989 and who were at most 45 years of age in 1945. Standard errors are reported in parentheses. *$p<0.1$, **$p<0.05$ ***$p<0.01$.

TABLE 4.4. Parameter estimates from the OLS regression of the effect of same-sex sibship on the likelihood of having a third child born in 1990

	OLS	Obs	F-stat.
Child Age			
Birth year third child	0.014*** (0.001)	212994	163.245
Birth year third child +1	0.016*** (0.001)	197844	191.956
Birth year third child +2	0.017*** (0.001)	186088	192.157
Birth year third child +3	0.017*** (0.001)	178256	195.591
Birth year third child +4	0.017*** (0.001)	172926	193.292
Birth year third child +5	0.017*** (0.001)	169389	189.563
Birth year third child +6	0.017*** (0.001)	167210	189.983
Birth year third child +7	0.017*** (0.001)	165733	191.253
Birth year third child +8	0.017*** (0.001)	164613	189.301
Birth year third child +9	0.017*** (0.001)	163766	190.749
Birth year third child +10	0.017*** (0.001)	163092	190.65

NOTES.— The table reports yearly estimates of the first-stage relationship. The covariates included are dummies for mothers' birth year, mothers' age at first birth, indicators for first- and second born boys. Standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

5.2. The Time Profile of the Fertility Effect on Earnings. Since young children require 24 hour supervision, having an additional child increases the value of home time versus time in market work. As a response to the increased value of home time, mothers may reduce the number of hours worked, or fully withdraw from the labor market. Furthermore, since the amount of time required for child care differs depending on the age of the child, it is reasonable to assume that the labor supply response to an additional child varies with time since birth. At the time of the study period covered in this article, Swedish parents were entitled to 15 months of paid parental leave, with job-protected full-time leave entitlements for the first 18 months after birth. Since mothers stand for the majority of parental leave take-up, we expect to find a nearly full withdrawal from the labor market in the short-run. Furthermore, Swedish parents are entitled to reduce their working hours with up to 25 percent until the child turns 8 years old, so a longer-run reduction of working hours might also be expected.

To study the temporal pattern of labor supply responses to fertility, therefore, we estimate Equation (4.1) by using both OLS and 2SLS estimation on annual labor earnings and participation over a 15-year horizon after birth and labor earnings responses are estimated by means of separate regressions for each year after child birth, starting from the birth year and up to the year the child is 15 years old. The results are depicted in Table 4.5. To conserve space, we present estimates for child ages 0 through 10 in the table, but Figures A3 and A4 in the Appendix graph the estimates through child age 15. The OLS estimation reveals that having a third child is associated with a large reduction in labor earnings in the immediate years following birth, with the earnings reduction gradually wearing off as the child ages. The 2SLS estimates confirm the OLS results and suggest that the effect of childbearing is largest in the years immediately following birth. However, the 2SLS estimates suggest that not taking endogeneity into account exaggerates not only the magnitude of the fertility effect, but also the degree of persistence. Using sex-mix as a source of exogenous variation in family size thus suggests a faster recovery of earnings as the earnings estimates seize to be statistically significantly different from zero after years 0 and 1. The same pattern holds true also for participation, where participation is defined as earning an income exceeding 50,000 SEK.[8] To gauge the magnitude of the effect of childbearing over the 10-year horizon after third birth depicted in Table 4.5, we perform a simple calculation where we first calculate the yearly percentage effect with average annual earnings as the baseline, and then average the yearly effects over the 10-year follow-up period. This simple calculation suggests that the total effect of a third child on labor earnings amounts to a decrease of roughly 11 percent over the first 10 years after birth.

To summarize, our findings indicate that there are larger short-run effects of childbearing at higher parity, which is in line with previous studies that find that fertility effects are larger

[8] Approximately 4,500 EUR in 2008 prices.

for women with young children (see e.g. Jacobsen et al., 1999; Bronars and Grogger, 1994; Rondinelli and Zizza, 2011; Vere, 2011).

However, it should be noted that the IV strategy used in our paper only allows us to investigate the margin of moving from two to three children. It is possible that the impact of childbearing is non-linear in the number of children, such that the allocation of time devoted to family and market work is decided upon in connection with the first birth, and simply maintained at higher parity. In that case, we would not find any impacts of third births over and above the time corresponding to formal parental leave, i.e., during the child's first two years of life. It should also be noted that our 2SLS estimates estimates are not very precisely estimated. To increase precision, in the following section, we pool data from several 'birth cohorts', meaning that we pool data for women who gave birth to a third child in 1990-1997 and estimate the effect of a third birth on this pooled sample of mothers.

TABLE 4.5. Parameter estimates from the OLS and 2SLS estimations of the effect of a third child on labor income and participation over child age

	Earnings		Participation		
	OLS	2SLS	OLS	2SLS	N
Child Age					
Birth year third child	-59.451***	-52.648**	-0.337***	-0.354***	212994
	(0.446)	(22.232)	(0.004)	(0.122)	
Birth year third child +1	-80.516***	-58.670***	-0.486***	-0.427***	197844
	(0.461)	(20.055)	(0.004)	(0.099)	
Birth year third child +2	-35.380***	-18.038	-0.135***	-0.054	186088
	(0.645)	(22.326)	(0.004)	(0.099)	
Birth year third child +3	-21.414***	-18.870	-0.079***	-0.030	178256
	(0.706)	(22.771)	(0.004)	(0.100)	
Birth year third child +4	-19.826***	-11.294	-0.069***	-0.030	172926
	(0.779)	(25.370)	(0.004)	(0.101)	
Birth year third child +5	-19.309***	3.938	-0.060***	0.026	169389
	(0.793)	(25.524)	(0.004)	(0.100)	
Birth year third child +6	-19.128***	-5.803	-0.054***	-0.006	167210
	(0.865)	(27.494)	(0.004)	(0.099)	
Birth year third child +7	-19.199***	-11.418	-0.047***	0.032	165733
	(0.933)	(29.259)	(0.004)	(0.100)	
Birth year third child +8	-18.690***	5.300	-0.037***	0.100	164613
	(1.001)	(31.454)	(0.004)	(0.100)	
Birth year third child +9	-16.760***	-7.339	-0.028***	0.005	163766
	(1.074)	(33.115)	(0.004)	(0.097)	
Birth year third child +10	-14.760***	-20.570	-0.021***	-0.024	163092
	(1.192)	(35.309)	(0.004)	(0.096)	

NOTES.— The outcome variable measures income from market work in 1000s SEK (expressed in 2008 prices) and labor market participation, defined as earning an income above 50,000 SEK, respectively. The covariates included are dummies for mothers' birth year, mothers' age at first birth, indicators for first- and second born boys. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

5.3. Sensitivity Analysis: Additional Birth Cohorts. In the previous section we studied the effect of a third birth for women who gave birth to a third child in 1990. The choice of 1990 as the start year was made primarily to enable a long follow-up period of labor market outcomes, but is arguably somewhat arbitrary. To tackle the arbitrary choice of 1990, and at the same time attempt to gain precision, we provide estimates from a pooled sample of women who gave birth to a third child in 1990, 1991, 1992 and so on, until 1997. Thus, with different start dates of third birth, we normalize the time of third birth to zero and follow mothers until their third child is 10 years old. However, this means that the same mothers will be considered treated and, at the same time, act as a control individual to a different birth cohort in the same estimation. Therefore, we cluster the standard errors at the individual level. The strategy of pooling together data from multiple birth cohorts also allows us to control for potential macroeconomic effects, such as real wage increases, by including calendar year dummies. The results from this analysis are presented in Table 4.6 and are very similar to the results obtained for the 1990-third-birth cohort. While the OLS estimator shows large and persistent negative effects on labor earnings, the 2SLS estimates suggest a faster income recovery, but with large reductions in earnings in the first two years after childbirth. Pooling data for multiple third-birth cohorts gains us some precision (the standard errors would have been lower had we not clustered them at the individual level) and the point estimates are smaller in the medium- to long-run compared to when using solely the 1990-third-birth cohort, as they center around zero.

Turning to the estimates on participation, also these show a similar pattern as in the analysis of a single third-birth cohort, with the largest effect taking place immediately after birth. However, the reduction in participation is now revealed to be somewhat more persistent and the point estimates, although not significantly different from zero, are somewhat larger in magnitude compared to when only analyzing the 1990-third-birth cohorts. One possible explanation is that the effect of a third birth on labor earnings reflects both an effect on participation, hours worked and potentially on wage rates. Thus, while some women exit the labor force, those who remain in work might increase working hours, resulting in a zero average effect on earnings in the long-run. The next section studies some effects on wage rates and contracted working time.

TABLE 4.6. Parameter estimates from the OLS and 2SLS estimations of the effect of a third child on labor income and participation over child age

	Earnings		Participation		
	OLS	2SLS	OLS	2SLS	N
Child Age					
Birth year third child	-57.97***	-74.78***	-0.31***	-0.47***	1953614
	(0.19)	(17.85)	(0.00)	(0.08)	
Birth year third child +1	-80.26***	-70.62***	-0.43***	-0.48***	1855256
	(0.20)	(18.20)	(0.00)	(0.08)	
Birth year third child +2	-35.94***	-22.28	-0.14***	-0.17**	1780918
	(0.28)	(19.44)	(0.00)	(0.08)	
Birth year third child +3	-25.70***	-8.84	-0.09***	-0.11	1728838
	(0.31)	(20.50)	(0.00)	(0.07)	
Birth year third child +4	-23.48***	-4.65	-0.07***	-0.10	1692259
	(0.34)	(21.75)	(0.00)	(0.07)	
Birth year third child +5	-23.03***	-3.50	-0.06***	-0.09	1662814
	(0.37)	(23.24)	(0.00)	(0.07)	
Birth year third child +6	-23.11***	-1.65	-0.06***	-0.07	1641653
	(0.39)	(24.67)	(0.00)	(0.07)	
Birth year third child +7	-22.49***	1.06	-0.05***	-0.07	1626644
	(0.42)	(26.25)	(0.00)	(0.07)	
Birth year third child +8	-22.10***	2.60	-0.04***	-0.06	1615857
	(0.45)	(27.95)	(0.00)	(0.07)	
Birth year third child +9	-21.00***	-2.65	-0.04***	-0.09	1608750
	(0.47)	(29.35)	(0.00)	(0.07)	
Birth year third child +10	-20.19***	-2.61	-0.04***	-0.08	1604222
	(0.49)	(30.85)	(0.00)	(0.07)	

NOTES.— The outcome variable measures income from market work in 1000s SEK (expressed in 2008 prices) and labor market participation, defined as earning an income above 50,000 SEK, respectively. The sample consists of mothers who gave birth to a third child in 1990-1997. The covariates included are dummies for mothers' birth year, mothers' age at first birth, indicators for first- and second born boys and calendar year dummies. Standard errors are clustered at the individual level and reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

5.4. Consequences for Monthly Wages and Contracted Work Hours. Thus far we have studied the effects of childbearing on labor earnings and participation. Labor earnings reflect both hours worked and hourly wage rates. In the short run, a decrease in earnings should most likely be interpreted as the result of decreased hours worked, as individuals are entitled to paid parental leave and wages are not likely to be altered in the short run. In the medium- to long run, however, wages may be affected by career interruptions. For instance, mothers may be subject to a flatter wage path post-birth due to either decreased effort at work or decreased opportunities for on-the-job training and advancement upon returning to work. To study whether the work interruption associated with a third child affects long-run wage growth, we estimate the effect of childbearing on the full-time equivalent monthly wage rate 5, 10 and 15 years after third birth. The data on wages originates from the wage structure statistics, which covers the entire public sector and about half of the private sector workers (with the sampling done at the firm level). Unfortunately, the wage structure statistics does not include working hours, but for all workers included in the data it reports the contracted working hours, measured as percent of full-time, so we can at least study whether additional childbearing leads to changes in the contracted working time.

The results for monthly wage rates are presented in panel (i.) of Table 4.7 and show that OLS estimation indicates a negative association between long run wages and childbearing, with a gradually diminishing impact over time, suggesting an initial negative effect but with a catching-up effect in the long run. In contrast, 2SLS estimation does not yield any statistically significant estimates of the effect of a third birth on wage rates. Similarly, panel (ii.) presents OLS and 2SLS estimates of the effect of a third birth on the log of contracted working hours (percent of full-time) 5, 10 and 15 years after birth. As for wage rates, OLS estimation yields a negative association between working hours and childbearing, an association which diminishes in magnitude over time since birth. Using the sex-mix of the first two children as a source of exogenous variation in family size, however, does not yield any significant effects. If anything, the point estimates suggest an increased effort in terms of contracted working hours among those who return to work after childbearing, although these findings should be interpreted with great caution. Also important to note is that the average working time for the full sample of mothers is about 77 percent of full-time. Many women resort to part-time work in connection with the first birth, which could perhaps explain the result that no effects are found of *further* childbearing on working hours.

TABLE 4.7. Parameter estimates from the OLS and 2SLS estimations of the effect of a third child on monthly wage rates and contracted hours of work by years since birth

Years after third birth	5	10	15
(i.) Full-time eq. Monthly wage rates			
A. OLS			
Third child	-0.022***	-0.018***	-0.005**
	(0.002)	(0.002)	(0.003)
B. 2SLS			
Third child	0.061	-0.036	0.087
	(0.064)	(0.071)	(0.077)
(ii.) Contracted work hours, percent of full-time			
C. OLS			
Third child	-0.050***	-0.046***	-0.026***
	(0.003)	(0.003)	(0.003)
D. IV			
Third child	0.020	0.020	0.054
	(0.081)	(0.079)	(0.080)
Observations	159510	154196	150569

NOTES.— The outcome variable measures the log of full-time equivalent monthly wages and the log of contracted work hours (percent of full-time), respectively. The sample consists of mothers with two children whose oldest child was born 1981-1989. The covariates included are dummies for mothers' birth year, mothers' age at first birth and indicators for first- and second born boys. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

6. The Effect of Fertility in a Cross-sectional Sample of Mothers

Our main findings so far suggest that there is a significant time heterogeneity in the causal impact of childbearing with respect to child age. Specifically, our results indicate that there is a relatively short-lived effect of fertility at higher parity on women's labor earnings and participation. However, since we sample mothers somewhat differently compared to previous studies using the sex-mix instrument, as well as compared to studies using the occurrence of twin births as an instrument for family size, it is useful to explore whether we can reproduce our findings using strategies employed in existing work. In this section we present results for the stock sample of women with at least two children in 1990 and whose oldest child was born before or in 1990, and where labor market outcomes for these individuals are observed in 1990 (thus reflecting a one-period cross-sectional data set). We then let D_i indicate whether individual i has more than two children in this sample of mothers, and estimate specification (4.1) using both OLS and 2SLS estimation on this sample.

The results are presented in Table 4.8 and suggest that having two firstborn children of the same sex increases the likelihood of having a third child by almost 6 percentage points. This estimate of the first-stage relationship is, in terms of magnitude, in line with previous studies using the sex-mix instrument. Moreover, the OLS estimate suggests that a third child is associated with a reduction in earnings of about 31,000 SEK on average. 2SLS estimation yields a smaller estimate of this effect, a reduction of about 20,000 SEK, but the estimate is still large and both economically and statistically significant. To gauge the magnitude of these estimates, we can compare them to the average earnings in the sample in 1990, which is about 127,400 SEK. Thus, the earning reductions correspond to 24 and 16 percent estimated with OLS and 2SLS, respectively.

In comparison, estimates provided by Angrist and Evans (1998) suggested that having a third child causes a 20-30 percent reduction in women's labor supply and earnings. The effect of a third birth thus seems to generate smaller negative effects on women's labor market outcomes in Sweden compared to the US, which is perhaps not surprising given the extensive family policies in place, including job-protection. Compared to the analysis where we follow the same mothers over time, the 2SLS estimate generated in this section suggests a somewhat larger percentage effect on earnings as the panel data analysis indicated an 11 percent reduction in earnings over a ten-year horizon.

TABLE 4.8. First-stage, OLS and 2SLS estimates of the effect of a third child on labor income

	(1) OLS	(2) 2SLS
A. First stage		
Same-sex	0.059***	
	(0.001)	
B. Earnings estimate		
Third child	-31.143***	-20.258***
	(0.213)	(3.427)
Covariates		
Boy 1st	Yes	Yes
Boy 2nd	Yes	Yes
Cohort dummies	Yes	Yes
Age at 1st birth	Yes	Yes
Observations	680931	680931

NOTES.— The outcome variable measures labor income in 1000s SEK, expressed in 2008 years' prices. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

6.1. Comparisons between Twins-2 and Same-sex Instrumental Variables Estimates. As noted by Angrist and Evans (1998) (hereon labelled AE), the most widely used source of exogenous variation in family size is the occurrence of twin births (see e.g. Rosenzweig & Wolpin, 1980a,b). Similar to the sex-mix of the first two children, the occurrence of twins at second birth can be used as an instrument to estimate the causal effect of moving from two to three children. AE compared estimates of the effect of a third child using twin second births, referred to as the twins-2 instrument, and the same-sex instrument and found that the effect of a third birth was smaller in magnitude when obtained using the twins-2 instrument. 2SLS estimates of the effect of a third birth on labor earnings using the same-sex and twins-2 instruments, respectively, are reported in Panel A of Table 4.9. The estimated models include the same covariates as in previous specifications, as well the ages of the first and second child. In line with AE, the labor earnings estimate generated with the twins-2 instrument is smaller in magnitude compared to the estimate generated with the same-sex instrument. As the average labor income among women in the sample amounts to 127,400 SEK, the estimate generated using the twins-2 instrument corresponds to a 7 percent reduction of earnings. This is considerably smaller than the 16-percent reduction generated using the same-sex instrument. AE hypothesized that the difference in estimates generated by the two instruments is due to the age difference between third children that are twins and third children that are not twins. For instance, the gap in age between third children who are born as twins and other third children in our data is 5.55 years on average. As explained by AE, this age gap has implications for labor market outcomes estimates if the effect of children varies with child age, as is suggested by our main analysis above. Then, the following model can be used to check whether differences in estimates generated by same-sex and twins-2 can be explained by the ages of third children. The equation of interest is:

$$y_i = \mathbf{x}'\alpha_0 + \alpha_1 s_{1i} + \alpha_2 s_{2i} + \alpha_3 a_{1i} + \alpha_4 a_{2i} + \beta_i D_i + \epsilon_i \tag{4.3}$$

where a_{1i} and a_{2i} are the ages of the first and second child, respectively. The coefficient β_i is now an individually varying effect that depends on the age of the third child with the following assumed structure:

$$\beta_i = \beta_0 + \beta_1 a_{3i} \tag{4.4}$$

where a_{3i} is the age of the third child for women who have a third child and zero otherwise. Combining Equations (4.3) and (4.4) yields the following equation to be estimated:

$$y_i = \mathbf{x}'\alpha_0 + \alpha_1 s_{1i} + \alpha_2 s_{2i} + \alpha_3 a_{1i} + \alpha_4 a_{2i} + \beta_0 D_i + \beta_1 (a_{3i} D_i) + \epsilon_i \tag{4.5}$$

Assuming that differences in a_{3i} are the only reason why the same-sex and twins-2 instruments yield different estimates, AE state that one can use both instruments to estimate the coefficients on the two endogenous regressors in Equation (4.5), D_i and $a_{3i}D_i$. 2SLS estimates of both β_0 and β_1 are reported in panel B of Table 4.9. The estimate for β_0 is negative and the estimate for β_1 is positive, suggesting that the negative effect of children declines as the child ages. The table also reports the estimate of the value of a_{3i} at which $\beta_i = 0$, which is given by $a^* = -\beta_0/\beta_1$. The estimate of a^* is about 12.3 years. That is, the effect of a third child on women's earnings goes to zero at child age 12.3 years. This estimate is surprisingly similar to estimate of a^* for the earnings effect in AE, which was 12.8 years of age. However, it is very different from the results obtained using only one instrument (same-sex) and following the same individual mothers over time, where 2SLS estimation suggest that the effect of children on earnings dissipate already two years after birth. This could imply that there is an effect of multiple births specific to having one additional child through twinning that goes over and beyond the effect that goes through differences in age between twin-third children and other third born children.

One way to investigate this issue is to use the twins-2 instrument and the fact that women in this cross-sectional sample had their births at different points in time. This means that we can create a synthetic-cohort life cycle and explore how the effect of a third child evolves over child age when the occurrence of twins at second birth is used as an instrument for higher order parity among women with at least two children. To this end, we estimate the following equation:

$$y_i = \lambda_0 + \mathbf{x}_i{}'\lambda_1 + \lambda_2 D_i + \sum_{j=0}^{15} \delta_j \times D_i \times 1(T = j) + \nu_i \tag{4.6}$$

where $j = 0, 1, ..., 15$ is years since third birth for individuals with three children and zero otherwise. Thus, we allow the effect of a third birth to vary with years since third birth.[9] Equation (4.6) is estimated with 2SLS using Twins-2 as an instrument for third birth and the age-specific effects of third births. The coefficients on the interaction terms are plotted in Figure 4.1 and show the same qualitative pattern as that obtained using the longitudinal data set; namely that the effect of a third child on women's earnings is large and significantly negative in the years immediately following birth to then catch up rather quickly. However, the effect seems somewhat more persistent compared to when using the same-sex instrument and longitudinal data. In the latter the effect dissipates after year 1, whereas the former shows an effect that persists year 0 to year 2 after birth. Moreover, the point estimates are positive in later years. Nevertheless, comparing to the results based on the assumption of age differences being the only thing generating differences between same-sex and twins-2

[9] This analysis is similar to the specification estimated in Vere (2011) to obtain life cycle effects of childbearing.

estimates, these findings suggest that there are other factors beyond the age of the third child that generate such differences in estimates.

TABLE 4.9. 2SLS estimates of the two-parameter labor supply model

	2SLS	2SLS
A. Instruments *Same sex* and *Twins-2* used separately		
	<u>*Same sex*</u>	<u>*Twins-2*</u>
β	-21.208***	-9.396***
	(3.505)	(1.512)
B. Instruments: *Same sex* and *Twins-2*		
β_0	-56.509***	
	(14.897)	
β_1	4.604***	
	(1.509)	
a^*	12.274	
Observations	648342	

NOTES.— The outcome variable measures labor income in 1000s SEK, expressed in 2008 years' prices. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

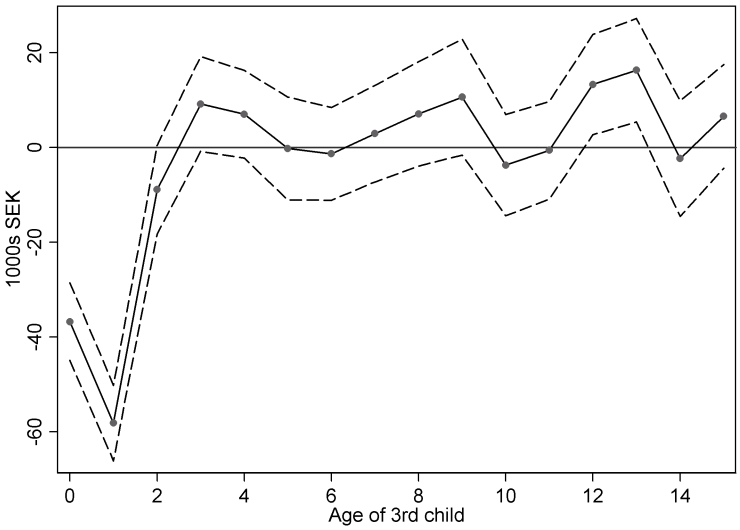

FIGURE 4.1. The figure plots estimated coefficients of the effect of a third child on labor income for varying years since third birth, along with the 95 percent confidence intervals. The effects are estimated using the occurrence of twins at second birth as an instrument for having more than two children.

7. Concluding Discussion

The main purpose of this study is to explore the temporal pattern of the effect of childbearing on female labor income. Understanding the dynamics of the fertility effect on women's labor market behavior is crucial to gauge the total effect of childbearing. To address the potential endogeneity of fertility to labor market outcomes we exploit parental preferences for a mixed-sex sibling composition as a source of exogenous variation in family size. Our analyses are based on longitudinal Swedish registry data which allows us to follow the same mothers over a 15-year horizon after birth. We find that a third child has a negative effect on female labor income in the immediate years following birth, after which earnings gradually catch up. 2SLS estimates suggest a faster recovery of income compared to OLS estimates. Thus, not taking endogeneity into account exaggerates the persistence of the negative effect of further childbearing on women's earnings. The same qualitative pattern holds true for labor force participation.

We also evaluate how the temporal pattern of the fertility effect compares across different empirical approaches in order to provide insights into how strategies based on cross-sectional data perform in reconstructing (true) life cycle patterns. Exploiting the occurrence of twins at second birth as an instrument for third births, we find that a synthetic-cohort life cycle analysis with cross-sectional data works well in reconstructing the temporal pattern obtained when following the same individuals over time. Estimates generated using twins at second birth as an instrument for third births are smaller in magnitude than estimates generated using the same-sex indicator. While some of this difference might be driven by the age differences between twin third children and other third children (twin third children are always older than other third children), our findings tentatively suggest that there is a direct effect of having twins that goes over and beyond this age difference. Thus, estimates generated with the two instruments are not likely to be directly comparable.

Compared to studies on data from the United States, the effect of third births on Swedish mothers' earnings are less persistent, and smaller in magnitude. These findings are perhaps not surprising given the Swedish parental leave system which provides 18 months of job-protected full-time leave after birth, of which the absolute majority constitutes paid leave. The system thus exhibits strong incentives for women to be attached to the labor market before childbearing and entitles the right to return to the pre-birth employer after parental leave. The short-run decrease in earnings is thus likely to be entirely or mostly driven by labor market withdrawals during paid parental leave.

However, the empirical strategy used in this paper only allows analyzing the effect of moving from two to three children. We cannot rule out that the effect of childbearing is non-linear in the number of children. It might well be the case that the largest impact takes place in connection with the first birth, and that higher parity childbearing does not additionally reduce women's working hours over and beyond an initial effect while on parental leave. Part-time work is common among mothers with children, but we do not find any evidence suggesting that contracted working hours decrease as a result of having a third child compared to having only two children.

Lastly, while the labor supply effects of a third child are largest in the short run, the short run effects are sizeable. Nevertheless, we do not find any evidence that an additional child impacts long run wage rates. The latter finding casts some doubt on the importance of human capital depreciation and foregone investments in human capital in explaining the motherhood wage penalty. Alternative explanations might be employers' expectations, or occupational choices resulting in a wage profile where the costs of children are being accounted for already at the start of women's careers.

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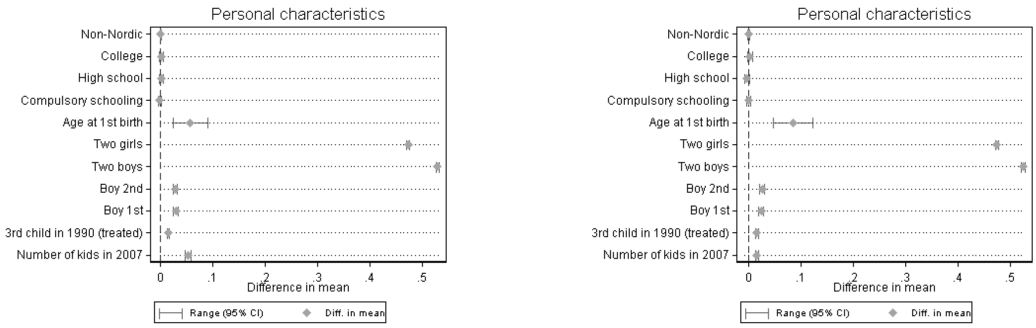
Appendix



(A) Full sample (B) Censored sample

FIGURE A1. The figure graph estimated differences in average personal characteristics between mothers with same- and mixed-sex siblings, along with the 95-percent confidence bands, for the full and censored sample, respectively.

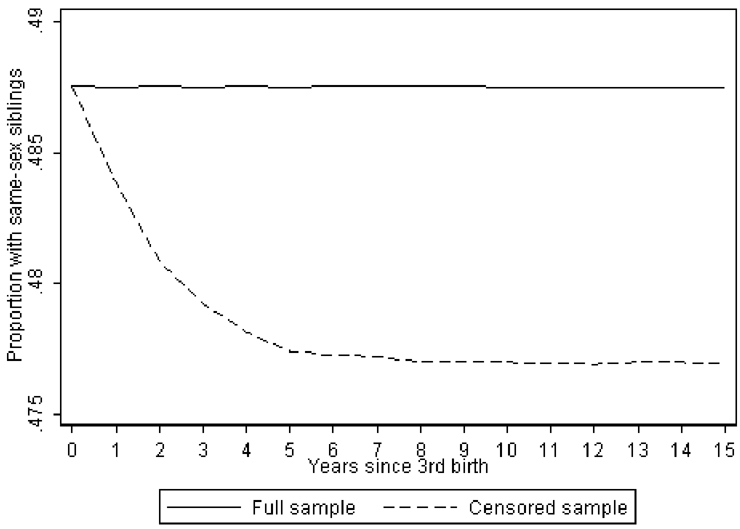
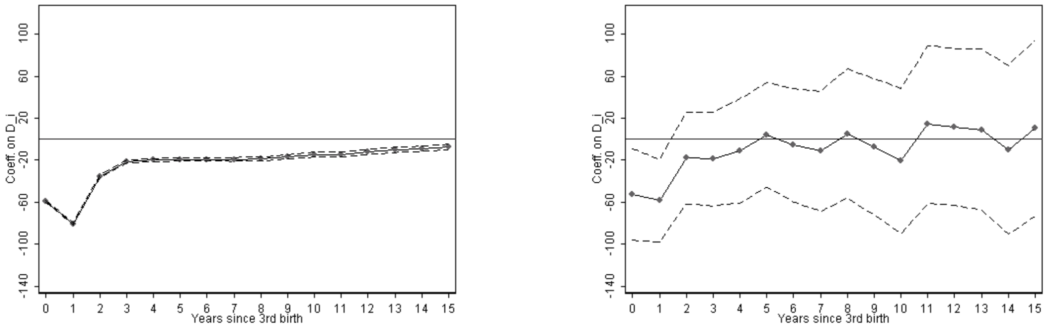
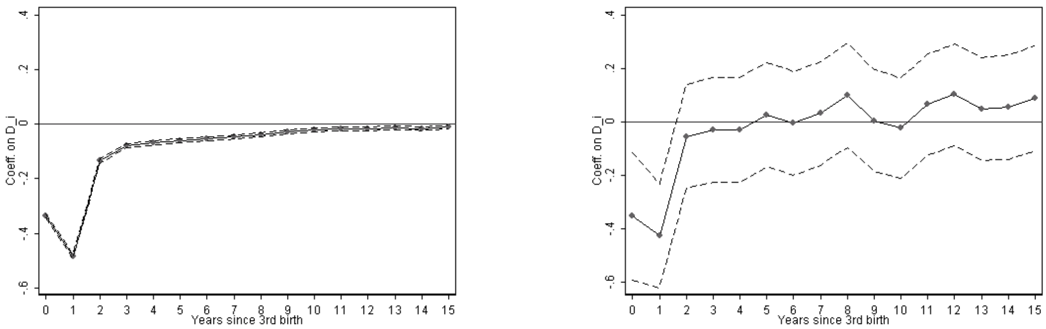


FIGURE A2. The figure plots the proportion of mothers with same-sex children for the full and censored samples, respectively.



(A) OLS estimates (B) 2SLS estimates

FIGURE A3. The figure plots estimated coefficients of the effect of a third child on labor income for varying years since third birth, along with the 95 percent confidence intervals.



(A) OLS estimates (B) 2SLS estimates

FIGURE A4. The figure plots estimated coefficients of the effect of a third child on participation for varying years since third birth, along with the 95 percent confidence intervals.

TABLE A1. Summary statistics for mothers with two children

	Mean
Number of kids in 2007	2.375
	(0.674)
3rd child born 1990	0.0722
	(0.259)
Boy 1st	0.514
	(0.500)
Boy 2nd	0.514
	(0.500)
Two boys	0.257
	(0.437)
Two girls	0.230
	(0.421)
Age at 1st birth	25.08
	(3.882)
Compulsory schooling	0.209
	(0.407)
High school	0.524
	(0.499)
College	0.267
	(0.443)
Non-Nordic background	0.0354
	(0.185)
Income in 1988, 1000s SEK	100.5
	(71.36)
Monthly wage in 1988, SEK	16326.6
	(3253.8)
Contracted work hours in 1988, percent of full-time	77.26
	(20.69)
Observations	212994

NOTES.— The table depicts summary statistics for the full sample of mothers who had given birth to two children by the end of 1989. Standard deviations are presented in parentheses.

PAPER 5

Labor Supply Responses to Paid Parental Leave

Arizo Karimi   Erica Lindahl   Peter Skogman Thoursie

ABSTRACT This paper re-examines the labor supply responses to changes in the Swedish parental leave system, recognizing that take-up of parental leave benefits might not fully reflect time off from work in a system where job protection exceeds paid leave. We study three reforms, of which the first expanded the entitlement to paid leave by three months, and the two other reforms introduced gender quotas in paid leave. We find that both mothers and fathers decreased their labor supply when entitlement to paid leave was increased. However, the additional benefits were spread out over a long horizon and thus seem to have been used by parents to increase job flexibility. In addition, we find no evidence suggesting that the introduced gender quotas in paid leave altered parents' labor supply.

1. Introduction

Public policies aimed at reducing barriers to the combination of market work and family life have reached increasing salience in the past few decades and to date, nearly all OECD countries offer governmentally paid parental leave benefits. Effective family policies can potentially have beneficial effects on family welfare, fertility, child development, and gender equality in the labor market by promoting a continual attachment to the labor market for mothers. The Nordic countries have for a long time provided generous parental leave systems with job protection and benefits that are conditioned on employment before leave. This has likely contributed to the high female labor force participation rates observed in the Nordic countries (see e.g. Waldfogel 1998, Jaumotte 2003, Baker and Milligan 2008, Han et al. 2009) At the same time, however, there is an ongoing debate about whether too generous parental leave durations are detrimental to women's labor market opportunities. It is, for example, argued that extensive parental leave systems discourage women's participation in the labor market on the intensive margin (Gupta and Smith 2002). Similarly, Albrecht et al. (2003) hypothesize that the entitlement to generous parental leave durations in Sweden,

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coupled with the fact that women stand for the majority of the take-up, creates room for statistical discrimination against women.

The unequal division of leave has been addressed directly by policy initiatives in the Nordic countries in the form of gender quotas in paid leave. Studies from both Norway and Sweden show that such programs increase fathers' parental leave take-up (Ekberg et al. 2013, Duvander and Johansson 2012, Dahl et al. 2012). For Sweden, Figure 5.1 shows that fathers' share of parental leave has increased during the last decades. However, it seems somewhat puzzling that the gender earnings- and gender wage gaps have not undergone the same relative change, but instead remained constant during the same time period. The latter raises the question of whether changes in paid leave, in an already generous system, have the potential to affect the labor market behavior of parents. For instance, paid leave could potentially crowd out unpaid leave, in particular if the duration of job protection exceeds the entitled duration of paid leave.[1] Thus, when studying the impacts of changes in paid parental leave, it might not be sufficient to study effects on the take-up of parental leave benefits in order to draw inference on the impacts of such changes on the time spent at home with children.

This paper re-evaluates the three of the latest reforms in the Swedish parental leave system. We study whether changes in paid leave entitlement affect parents' labor market behavior, recognizing that parental leave benefit take-up might not fully reflect actual time off from work. First, we estimate the effect of a reform implemented in 1989, which expanded paid parental leave to eligible parents from 12 to 15 months. Second, we study parents' labor supply responses to the introduction of the so called "daddy-month" in 1995 which implied that, for eligible parents, one month of paid leave was reserved to each parent. Third, we study the impacts of the second "daddy-month" reform introduced in 2002, which reserved an additional month of paid leave to each parent, at the same time as entitlement to paid leave was extended from 15 to 16 months. In all three reforms, eligibility varied discontinuously with child birth date, creating natural experiments that allow us to estimate the causal effect of changes in paid parental leave. The analyses are based on longitudinal Swedish register data that provides individual level information on fertility, annual labor income, wage rates, parental leave and demographic characteristics.

We find that the expansion of paid leave in 1989 increased both mothers' and fathers' take-up of parental leave benefits. The positive effect on fathers' take-up is perhaps surprising given that there were no gender restrictions in benefit take-up at the time of this reform. Moreover, both mothers and fathers decreased their months worked as a response to the reform, implying that it did not crowd out unpaid leave. However, the additional paid leave

[1] Among women in Sweden, survey evidence shows that women are on leave longer than the duration of parental leave benefits (Berglund 2004).

was spread out over an 8-year horizon, such that additional leave was used for occasional days off from work over a long time period. Thus, parents use additional days to increase job flexibility; the consecutive leave in direct connection with childbirth was more or less unchanged, for both mothers and fathers.

Secondly, we find that the introduction of the first daddy-month in paid leave increased fathers' parental leave benefit take-up, and decreased mothers' benefit take-up. The reason for the latter result is that, although parents are given half of the entitled paid days each, they are free to transfer days between each other. In practice, this means that fathers transfer most of their days to mothers, such that an earmarked month implied re-allocating paid leave from mothers to fathers. Also the second daddy-month reform increased fathers benefit take-up. For mothers, on the other hand, we find no significant effect of the 2002-reform on parental leave. This may be because the reform also increased entitlement to paid leave by one month, which is likely to have been used mostly by mothers. None of the two daddy-month reforms, however, affected parents' labor supply. In addition, we find no effects on short- or medium run labor income, nor on wage rates. Thus, these reforms seem to have had limited effects on the actual labor market behavior of parents.

Our paper contributes to the strand of literature on how family policies affect mothers' and fathers' leave durations and labor market outcomes, which has grown substantially during the last years. The literature often finds that introducing paid parental leave positively affects women's labor force participation. For instance, Baker and Milligan (2008) for Canada and Ruhm (1998) for European countries, find that parental leave provisions affect the labor market attachment of mothers. On the other hand, many studies find limited effects of expanding paid leave on employment and wages. For example, Klerman and Leibowitz (1999) and Baum (2003) find only weak effects on employment and wages in the United States. Schönberg and Ludsteck (2007) study the causal effects of successive changes in parental leave duration on employment and earnings in Germany and find that expansions of leave coverage induced women to delay their return to work. However, the expansions had little effect on women's labor supply in the long run. Also, Albrecht et al. (1999) find that time off for formal parental leave is not associated with a wage penalty for women in Sweden. A few papers also examine the relative importance of job protection and cash benefits for mothers' return to work behavior and long run labor market outcomes. Specifically, Lalive and Zweimüller (2009) and Lalive et al. (2011) study reforms in the Austrian system and find that extending parental leave benefits *and* job protection delays mothers' return to work; while reducing cash benefits but keeping the duration of job protection constant (and thus longer than paid leave) speeds up return to work. However, the authors do not find long run effects on earnings or employment.

Liu and Skans (2010) study how parental leave affects children's scholastic performance by using variation in entitled parental leave durations induced by the 1989-reform in Sweden, which is one of the reforms studied in this paper. Although the focus of Liu and Skans is on children's outcomes, they also study the impact of the reform on mothers' earnings and find no significant effects. Our analysis complements their results by examining the effects on an outcome variable constructed to measure months worked, and thus in measuring whether increased entitlement to paid leave affected the duration of time off from work. In addition, we contribute by studying the effects of the 1989-reform on fathers' outcomes. Our paper also complements the studies by Ekberg et al. (2013) and Eriksson (2005) who evaluate the first and second daddy-month reforms, respectively, on fathers' parental leave. Both studies find a positive effect of the gender quota reforms on fathers' parental leave. Similarly, Duvander and Johansson (2012) study the impacts of both the first and second daddy-month reforms in Sweden, as well as the introduction of an equality bonus which granted tax credits to parents who shared the leave equally. They find strong effects on parental leave use resulting from the first daddy-month reform, more modest effects of the second, and no effects of the equality bonus. Johansson (2010) looks at the effect of own and spousal parental leave on earnings in Sweden and find that mothers' earnings are positively affected by spousal parental leave.[2] In a recent paper by Dahl et al. (2013), the case for paid parental leave is evaluated in the context of Norway. The authors highlight the importance of a distinction between introducing parental leave and continually expanding entitlements to paid leave. Studying the impacts of expanding paid leave in Norway, they find that mothers decrease their labor supply and hence that paid leave does not crowd out unpaid leave. However, they find no effects on children's schooling outcomes, parental earnings or participation in the long run, completed fertility, marriage or divorce.

Our paper adds to this literature by re-examining the impacts of changes in paid parental leave entitlements on *both* benefit take-up and on months worked, and thus by examining whether paid leave crowds out unpaid leave. Second, we estimate the impacts of both a general expansion of paid leave entitlements as well as of introducing gender quotas in paid parental leave. Thus, we estimate the effects of three different reforms on the parental leave take-up and months worked for both mothers and fathers, in the short and medium run. Third, we also analyze the impacts of the changes in paid leave entitlements on parents' wage rates.

The rest of the paper is organized as follows. The next section describes the institutional setting and the three reforms in the Swedish parental leave system that we evaluate. Section 3 outlines the empirical strategies employed to measure the causal effects of the reforms on parents' outcomes. Section 4 describes the data sources, variable definitions and presents

[2] Other papers studying impacts of parental leave policies include Han et al. (2009), Ejrnaes and Kunze (2006), Rege and Solli (2010), Cools et al. (2011), Hashimoto et al. (2004).

analyses of the validity of the identifying assumptions. In section 5 we present the results from estimating the effects of the reforms on parental leave and labor market outcomes, Section 6 presents the results from sensitivity checks and Section 7 concludes the paper.

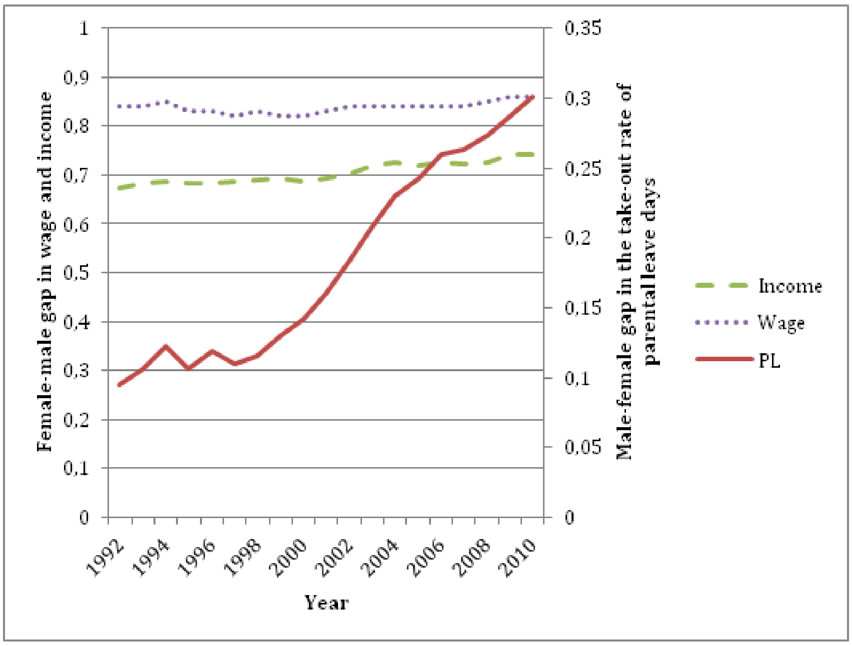


FIGURE 5.1. Gender gaps in income, wage rates and parental leave in Sweden, 1992-2010. Sources: Wage gap: National Mediation Office. Income cap: Statistics Sweden. PL days: Social Insurance Board.

2. The Swedish Parental Leave System

The Swedish parental leave system was introduced in 1974, and replaced the preceding maternity leave. The parental leave system is comprised by several parts, of which governmentally paid benefits for leave to care for young children, and temporary leave to care for sick children are the most important. Parental leave benefits, in turn, are divided into three components. First, ten days of leave are given to the fathers, which can be used during the first 60 days after the birth of the child. The benefits are based on previous earnings, up to an inflation-adjusted cap. Since 1978, part of the parental leave is replaced at a lower flat rate of 60-180 SEK per day during the time period studied. To date, these "base-level" benefits are received for a maximum of 90 days for each child. In addition, parents receive 390 days of leave (per child) in which benefits replaces wages at a rate of 75 to 90 percent during the period studied. Wage-replaced benefits are conditioned on at least 240 days of employment before the birth of the child. For individuals that do not meet the work requirement, all parental leave days are compensated at a lower fixed rate.

The leave is job protected, and the system offers a great portion of flexibility. During the child's first 18 months, both parents are entitled to full-time job protected leave, with or without collecting benefits. Thereafter, parents are allowed to reduce their working hours with up to 25 percent until the child turns eight years old.[3] The governmentally paid parental leave benefits do not have to be taken in one sequence, and can instead be saved and used until the child turns eight years old. Workers must notify their leave to their employers at least two months in advance, but employers cannot deny an employee to use their parental leave days given that this condition is fulfilled. Thus, parents may have incentives to save paid days for future use to increase their flexibility to take a day off for child care reasons (or e.g. to prolong vacations). In comparison to other countries, Sweden has a relatively generous system with respect to job protected and unpaid leave. However, Ray et al. (2010) show that in most countries the unpaid job protected leave is more than twice as long as the job protected paid leave. Figure 5.2 sums up these components of the parental leave system for 21 OECD countries.

Since the introduction in 1974, the parental leave system in Sweden has been subject to several extensions. In this section, we mainly describe the three reforms that are studied in this paper, but Table A1 in the Appendix provides a detailed description of the changes in the parental leave system from 1988 through 2010.

Between 1980 and 1989, parents were entitled to 12 months of paid leave, of which three months were compensated at the lower fixed rate of 60 SEK per day. In 1989, entitlement to paid leave was extended from 12 to 15 months. The three additional months of paid leave concerned the wage-replaced component of benefits. The aim of the reform was to increase parents' possibilities to take care of their newborn children. The reform was implemented on July 1st, 1989, but retroactively covered parents to children born in October 1988. Transition rules following the implementation implied that also parents to children born in August and September 1988 received one and two additional months of paid leave, respectively. The extension of paid leave in the 1989-reform was not implemented with any gender quotas; parents were free to allocate the additional benefits between each other as they wished.

The next policy change that we explore is the introduction of the first so called "daddy-month" reform, which was implemented in 1995. The reform implied that one month of the wage-replaced leave was earmarked to each parent, and could not be transferred to the other parent. Eligibility of the 1995-reform varied with child birth month, with parents to children born on January 1st, 1995 and onwards became eligible. Parents to children born before 1995, on the other hand, were given an equal share of the paid leave, but were free to transfer leave days between each other. In practice, this meant that fathers transferred most of their

[3] The right to reduce working hours was introduced in 1989, but retroactively covered all parents to children born in 1986 onwards.

days to mothers. For eligible parents, however, the 1995-reform implied that one month of parental leave benefits would be lost if the father did not take any leave.

In order to further promote fathers' parental leave usage, the government introduced a second daddy-month in 2002. For parents to children born on January 1st, 2002, and onwards, one additional month of wage-replaced leave was earmarked to each parent. At the same time, the number of (wage-replaced) leave months were increased from 15 to 16 months.

Since eligibility for all three reforms explored in this article, the 1989-, 1995-, and 2002-reform, varied with children's birth date, we can rely on variation in child birth date to estimate the causal effect of, on the one hand, a general expansion of entitlement to paid leave and, on the other hand, introducing gender quotas in paid leave. The next section describes the empirical strategies employed to capture the causal effects of these reforms.

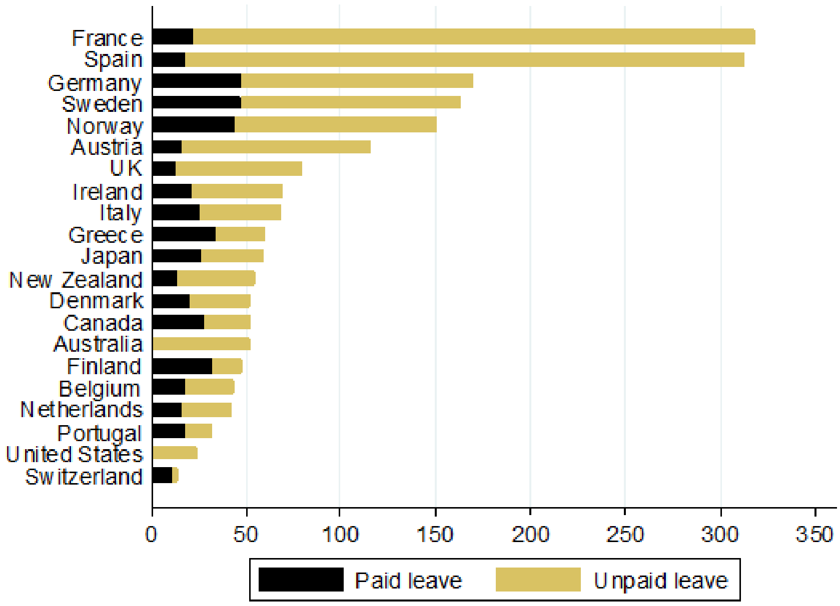


FIGURE 5.2. Paid and unpaid job protected, full-time equivalent parental leave in weeks for two-parent families in the OECD. Source: Ray et al. (2009).

3. Empirical Strategy

The 1989 reform increased the entitlement to wage-replaced parental leave benefits with three months, from 12 to 15 months. The reform was implemented on July 1st 1989, but retroactively covered parents to children born in October 1988 onwards. Moreover, transition rules following the implementation of the system implied that also parents to children born in August (September) 1988 received 1 (2) additional months of paid leave. Due to the transition rules we cannot apply a meaningful regression discontinuity approach. Instead, we rely on variation in entitlement among parents whose child was born in 1988 and compare differences in outcomes between parents whose children were born in August-December 1988 with the outcomes of parents to children born in January-July 1988. To control for potential seasonal effects, we let parents to children born in 1987 (pre-reform cohort) and 1989 (post-reform cohort) serve as additional controls in a difference-in-differences setup. Specifically, we estimate the following regression equation:

$$y_{icm} = \beta_0 + \beta_1 E_i + \sum_{c=1988}^{1989} \beta_c D_{i,c} + \sum_{m=1}^{11} \beta_m D_{i,m} + \mathbf{x'}_i \beta_x + \epsilon_{icm} \tag{5.1}$$

where y_{icm} is the outcome variable of interest for parent i, who had their first child in calendar year c (1987,1988 or 1989), and calendar month m (1, 2,...,11). $D_{i,c}$ are dummy variables indicating the birth year of individual *i*:s child (1987 being the reference cohort), and the $D_{i,m}$ are eleven dummy variables indicating the birth month of individual *i*:s child. The regressor of interest, E_i, is set to equal unity if individual *i*:s child is born in August 1988, equals 2 if the child is born in September 1988 and 3 if the child is born in October-December 1988. Thus, β_1 measures the linear effect of being entitled to one additional month of wage-replaced parental leave benefits. Thus, we compare the difference in outcomes between parents who had their first child born between August and December 1988 with parents who had their first child in January to July 1988. To control for potential seasonal effects, we take into account potential differences in outcomes due to child birth month for parents to children born in 1987 and 1989. In order for β_1 to identify the causal effect of increased entitlement to paid parental leave, any birth month effects must be the same across birth cohorts.

With respect to the two "daddy-month" reforms, the first reform reserved one month of (the existing) wage-replaced leave to each parent, for parents to children born January 1st, 1995 onwards. The second reform reserved one additional month of paid leave to each parent, for parents to children born on January 1st, 2002 onwards (at the same time, the 2002-reform increased total paid leave with one month from 15 to 16 months). Thus, to evaluate the reforms, we can compare differences in outcomes of parents to children born around the reform cutoff dates.

To evaluate the 1995 reform, we compare differences in average outcomes between parents to children born in December 1994 (ineligible) to parents of children born in January 1995 (eligible). To account for potential seasonal effects, we estimate a difference-in-differences model, where parents to children born in December/January the subsequent turn of the year are included as controls. We estimate the following regression equation:

$$y_i = \gamma_0 + \gamma_1 D_{i,1995/94} + \gamma_2 D_{i,Jan} + \gamma_3 (D_{i,1995/94} \times D_{i,Jan}) + \mu_i \tag{5.2}$$

where $D_{i,1995/94}$ is a dummy variable that equals unity if parent *i*:s child was born in December/January 1994/1995 and zero if the child was born in December/January 1995/1996. $D_{i,Jan}$ is a dummy variable that equals unity if the child was born in January and zero if the child was born in December. The regressor of interest is $(D_{i,1995/94} \times D_{i,Jan})$, the coefficient of which, γ_3, measures the difference in outcomes between parents whose child was born in January 1995 and those whose child was born in December 1994, in comparison to the corresponding difference between parents of children born in January 1996 and December 1995.

The 2002-reform was implemented in a similar fashion, and we apply the same empirical strategy using data on parents to children born in January/December 2001/2002 and parents to children born at the year-end preceding the reform; January/December 2000/2001. The identifying assumption is again that potential birth month effects are similar across years.

4. Data

The analyses are based on population-wide Swedish registers. We use the multi-generational register to link all children to their biological parents and to obtain information on children's birth month and birth year. To this data we match individual level data from the LOUISE register on annual labor income, educational attainment and a number of demographic variables such as age and country of origin.

For a sub-sample of individuals, we have information on full-time equivalent wage rates from the Wage Structure Statistics. The wage data covers the entire public sector, and about half of the private sector workers (stratified at the firm level). The measuring period for the wage data is a single month (November for the municipality and county council employees, and September for workers in the private and governmental sectors). Thus, we only have information on wages for individuals that are present at the workplace during the measuring period. Hence, individuals that are, e.g. on parental leave or sickness absence, during the measuring month are not included in the wage data. Nevertheless, for individuals employed and working we have information on full-time equivalent wage rates. These data cover the time period 1985 through 2007.

The data that we use do not contain information on hours worked. We construct a measure of units worked using the annual labor income variable together with full-time equivalent monthly wage rates. Annual labor income comes from tax registers, is updated on a calendar year basis, and measures income from market work. They do not include parental leave benefits or other transfers. For unemployed individuals, labor income is reported as zero income. Thus, the income measure reflects both hours worked and hourly wage rates.

To obtain a measure of individual labor supply, we divide labor income with the full-time equivalent monthly wage rate earned one year prior to childbirth.[4] This gives us a measure of labor supply indicating months worked per calendar year. For example, an individual with an income of SEK 200,000 in year X, and a monthly wage rate of SEK 20,000 prior to childbirth (year X-1) will be reported to have worked for 10 months in year X. In the short run, we do not expect wages to be affected by parental leave, which means that any changes to the annual income measure is likely to be interpreted as labor supply responses. However, if wages rates are negatively affected by increased parental leave in the medium run or long run, this implies that we will underestimate the number of months worked in later years. In turn, we would then overestimate the effect of the reforms on labor supply. However, as a sensitivity analysis, we will estimate the reform effects separately on monthly wage rates.

Data on parental leave take-up comes from the National Social Insurance Agency and covers all parental leave spells taken between 1988 and 2008. However, the parental leave data differs in quality before and after 1994. For all spells taken in the years 1988 through 1993, we have information on start- and end-dates of each parental leave spell. However, we cannot link these days to a particular child, or know whether the leave was taken in full-time or part-time. Moreover, we do not know how many days have been taken at the wage-replaced level and how many days were compensated at the lower flat rate. This implies that the number of leave days will be overstated for all individuals when studying the 1989 reform. However, this is the case for both the treated and control individuals.

From 1994 onwards, the parental leave data contains information about for which particular child the leave was taken (indicated by an individual child identifier for each spell), how many days of leave were compensated with wage-replaced benefits, and how many days were replaced on the lower flat rate.

Due to the restrictions of the older parental leave data, when studying the impacts of the 1989-reform, we focus on individuals who gave birth to their *first* child in 1987, 1988 and 1989 to avoid counting parental leave days that were used for younger siblings. We then calculate the number of leave days taken between a start date, which is set to the 15th of the child's birth month (since we do not know which date of the month the child is born), and 1095 or 1460 days after that start date. That is, we measure the number of parental leave days taken within the first child's first 3 and 4 years of life, respectively. In the medium- to long run, these parental leave days will arguably include days taken for both the first and subsequent children.

[4] The reason for using the wage rate in the year prior to childbirth instead of updating wage rates each calendar year is that wages are only reported for employees that are present at the workplace in the measuring month (September or November). In the years following childbirth, many individuals will be on parental leave and thus not be included in the wage data. Those who do work are expected to be a selected sample group of individuals, in particular among mothers.

When studying the effects of the 1995- and 2002-reforms, the two "daddy-month reforms", we instead focus on all individuals who gave birth to a child in 1994-2004, irrespective of parity. We calculate the number of parental leave days that were remunerated with *wage-replaced* benefits, for each individual child. We count the number of days taken (for each individual child) during exactly three years (1095 days) and eight years (2922 days), respectively, where we again count from a start date set to the 15th of each child's birth month. The reason for the 8-year follow-up period is that parents are entitled to save and use paid parental leave until the child turns eight years old. For the cohorts of parents to children born around the 2002-reform cutoff, we only study the impact on parental leave (and labor market outcomes) until the child turns three years old, since we do not observe outcomes past 2007.

4.1. Summary Statistics, Parallel Trends and Covariate Balance. The empirical strategies employed to evaluate the causal effects of the changes in the parental leave system involve exploiting variation in entitlement to the reforms, given by the birth date of children. To evaluate the 1989-reform, we exploit variation in birth month among parents to children born in 1988, and take into account the corresponding difference in outcomes between parents to children born in 1987 and 1989, which are the first pre- and post-reform cohorts, respectively. The identifying assumption of this strategy is that any birth month effects should be similar across the cohorts 1987, 1988 and 1989. To evaluate this identifying assumption, we estimate a model similar to Equation (5.1), but where each birth month dummy is interacted with the cohort dummy variables:

$$\begin{aligned} y_{icm} = \delta_0 &+ \sum_{m=1}^{11} \delta_{1988,m}(D_{i,m} \times D_{i,1988}) + \sum_{m=1}^{11} \delta_{1989,m}(D_{i,m} \times D_{i,1989}) \\ &+ \sum_{m=1}^{11} \delta_m D_{i,m} + \sum_{c=1988}^{1989} \delta_c D_{i,c} + \mathbf{x'}_i \delta_x + \nu_{icm} \end{aligned} \tag{5.3}$$

where July is used as the reference birth month, and 1987 as the reference birth cohort. If our identifying assumption hold, there should be no differences in outcomes between parents to children born in January to July 1988 and other years. However, differences in outcomes for parents to children born in August to December 1988 and other years would indicate an effect of the reform.

Figure 5.3 graphs mothers' average months worked during the first child's first three years of life (cumulated labor supply during the three calendar years after birth) by child birth month, for mothers to children born in 1987, 1988 and 1989, respectively. The lower graph of Figure 5.3 show the estimated month-effects based on Specification (5.3). As seen in Panel B of Figure 5.3, there are no differences in months worked between mothers to children born in January to July in 1988 and mothers to children born in January to July

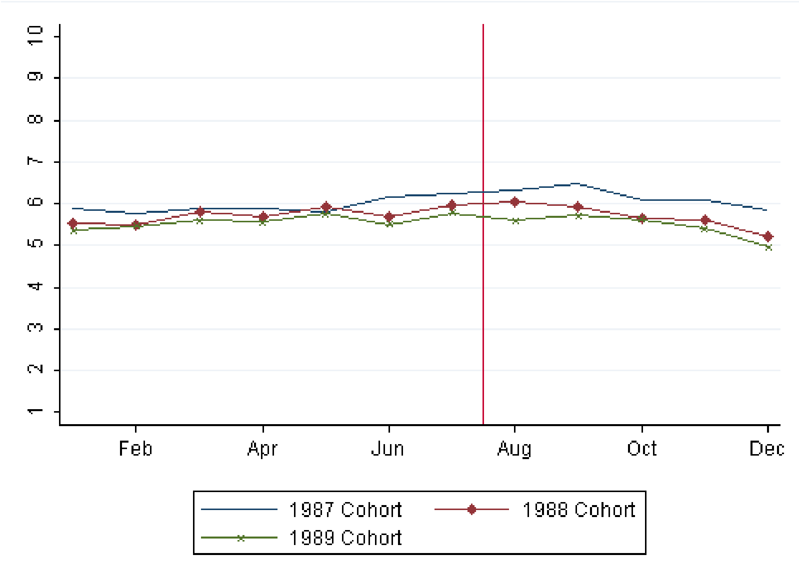
1987 and 1988. However, mothers to children born in August-December in 1988 have a lower cumulated labor supply compared to parents born in August-December 1987 and 1989. The effect is linear by birth month, starting from August, and suggests that the reform indeed had a negative effect on the labor supply of mothers. Importantly, the birth-month effects are more or less parallel across the three cohorts for mothers to children born in January through July.

The corresponding graph is shown for fathers in Figure 5.4. Surprisingly, also fathers' labor supply seem to have been negatively affected by the increased entitlement to paid leave. While there is somewhat more heterogeneity in the birth month effects for fathers, there is a downward trend for fathers to children born in August 1988 and onwards consistent with the eligibility rules.

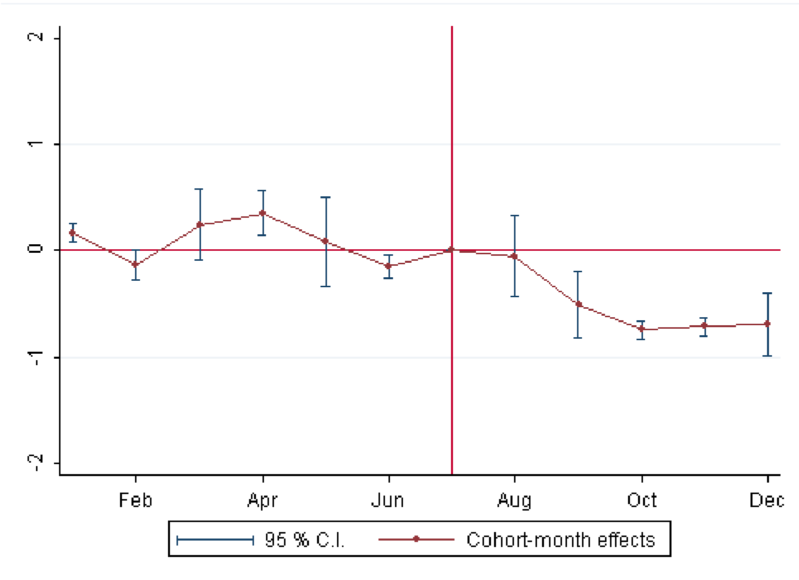
Table A2 in the Appendix presents covariate balance tests between parents to children born in the second and first half of 1987, 1988 and 1989, respectively. Columns (3) and (4) also show difference-in-differences estimates between parents to children born in 1987 and 1988, or 1988 and 1989, respectively. Aside from a small significant difference in the number of children born to a woman by 2007, there are no significant differences in average characteristics between the groups. This suggests that individuals to children born in different months of adjacent years comprise a valid comparison group to parents to children born in 1988.

When studying the effects of the 1995- and 2002-reforms, which both introduced gender quotas in parental leave benefits, we compare outcomes of parents to children born in December 1994 or 2001 (ineligible) to outcomes of parents to children born in January 1995 or 2002 (eligible). To account for potential seasonal effects, we also estimate specifications where we take into account the difference in outcomes between parents to children born over the turn of the year in adjacent years. In Table 5.1, we report average characteristics of parents to children born in December/Janauary 1994/1995 and December/January 1995/1996, respectively. Column (5) presents the difference-in-difference estimates, and shows there is a significant difference in mothers' age at first birth across the two year-ends, and for fathers, there is a significant difference in the labor income earned in the year prior to birth. There are no other significant differences between the groups.

The corresponding figures for the 2002-reform is given in Table 5.2, which shows that there are significant differences in the labor income earned in the year prior to birth, as well as the monthly wage rate, for both mothers and fathers. There are no other significant differences between the groups, however. In all estimations we therefore control for the labor income and wage rate in the year prior to birth.

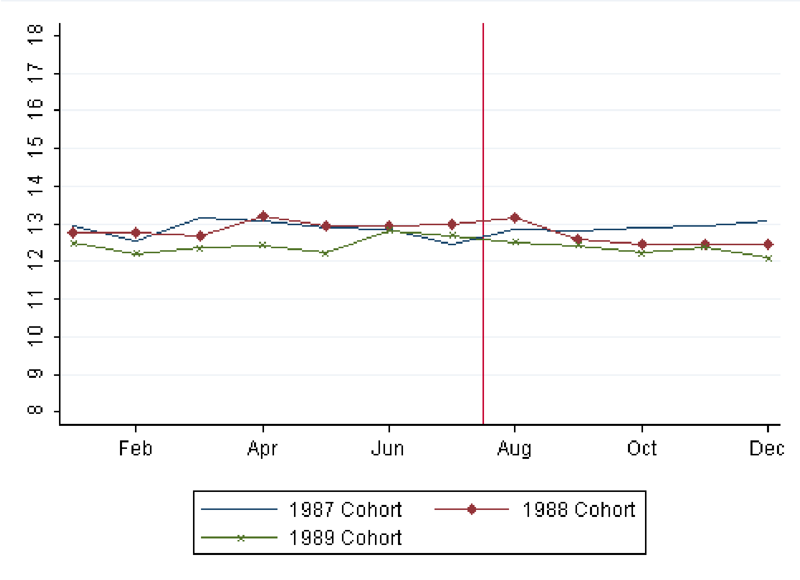


(A) Average labor supply by birth month

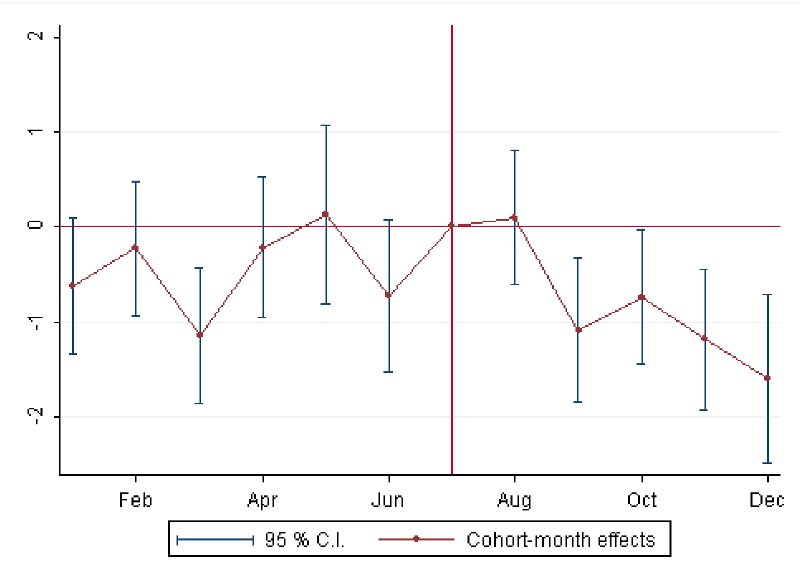


(B) Estimated differences in labor supply by birth month

FIGURE 5.3. Mothers' average months worked by child birth month (panel A), and estimated differences in months worked between mothers to children born in different months in 1988 and those to children born in 1987 and 1989 (panel B).



(A) Average labor supply by birth month



(B) Estimated differences in labor supply by birth month

FIGURE 5.4. Fathers' average months worked by child birth month (panel A), and estimated differences in months worked between fathers to children born in different months in 1988 and those to children born in 1987 and 1989 (panel B).

TABLE 5.1. Covariate balance tests, 1995-reform cohorts

	(1) Dec 94	(2) Jan 95	(3) Dec 95	(4) Jan 96	[(1)-(2)]-[(3)-(4)] DiD
A. Mothers					
Age at 1st birth	28.77	27.28	28.79	27.90	0.616*
	(4.626)	(4.570)	(4.681)	(4.746)	(0.346)
Number of kids in 2007	1.998	2.023	1.980	1.943	-0.063
	(0.747)	(0.747)	(0.765)	(0.783)	(0.057)
Immigrant	0.0866	0.0934	0.0720	0.0955	0.017
	(0.281)	(0.291)	(0.259)	(0.294)	(0.014)
Live in large city	0.132	0.145	0.142	0.139	-0.016
	(0.338)	(0.352)	(0.349)	(0.346)	(0.018)
Income prior to birth	121860.2	121722.3	122546.6	124242.5	1949.258
	(58143.3)	(54558.3)	(60451.0)	(55934.0)	(2915.539)
Wage prior to birth	13922.2	13817.2	14303.0	14174.9	-27.664
	(2517.1)	(2494.3)	(3135.7)	(2901.7)	(139.537)
Observations	1820	1802	1299	1388	
B. Fathers					
Age at 1st birth	30.97	30.36	30.66	30.10	0.054
	(5.260)	(5.639)	(5.472)	(5.195)	(0.503)
Number of kids in 2007	1.934	1.889	1.935	1.937	0.048
	(0.775)	(0.786)	(0.755)	(0.753)	(0.072)
Immigrant	0.130	0.143	0.0938	0.131	0.023
	(0.336)	(0.351)	(0.292)	(0.338)	(0.020)
Live in large city	0.137	0.147	0.147	0.140	-0.017
	(0.344)	(0.354)	(0.354)	(0.348)	(0.021)
Income prior to birth	184513.0	194290.2	190473.6	192482.2	-8012.401*
	(74507.3)	(78360.6)	(77359.7)	(71658.6)	(4526.636)
Wage prior to birth	16079.4	16387.2	16389.5	16390.4	-312.805
	(4522.5)	(4712.3)	(4866.9)	(4598.5)	(279.566)
Observations	1116	1521	962	1002	

NOTES.— The table reports mean characteristics between parents to children born over the turn of the years in 1995 and 1996, and the DiD estimates of the difference in average characteristics. *p<0.1, **p<0.05 ***p<0.01.

TABLE 5.2. Covariate balance tests, 2002-reform cohorts

	(1) Dec 01	(2) Jan 02	(3) Dec 00	(4) Jan 01	[(1)-(2)]-[(3)-(4)] DiD
A. Mothers					
Age at 1st birth	29.54	28.45	29.92	28.18	-0.632**
	(4.446)	(4.469)	(4.383)	(4.531)	(0.268)
Number of kids in 2007	1.897	1.890	1.965	1.957	-0.002
	(0.612)	(0.612)	(0.629)	(0.640)	(0.037)
Immigrant	0.0919	0.105	0.100	0.118	0.004
	(0.289)	(0.307)	(0.301)	(0.323)	(0.017)
Live in large city	0.165	0.196	0.187	0.194	-0.024
	(0.372)	(0.397)	(0.390)	(0.395)	(0.022)
Income prior to birth	183565.1	182846.1	190256.6	173367.4	-16071.5***
	(85690.0)	(95823.7)	(78485.8)	(75313.2)	(4702.472)
Wage prior to birth	17688.2	17747.8	17713.8	16933.7	-834.858***
	(5053.6)	(6510.2)	(5175.9)	(4359.3)	(296.312)
Observations	1053	1567	1140	1465	
B. Fathers					
Age at 1st birth	31.56	30.97	32.14	31.02	-0.554*
	(5.085)	(5.170)	(5.033)	(5.258)	(0.305)
Number of kids in 2007	1.858	1.903	1.916	1.960	0.002
	(0.625)	(0.665)	(0.652)	(0.693)	(0.039)
Immigrant	0.122	0.141	0.119	0.145	0.007
	(0.327)	(0.348)	(0.324)	(0.352)	(0.018)
Live in large city	0.171	0.204	0.184	0.195	-0.022
	(0.377)	(0.403)	(0.388)	(0.396)	(0.021)
Income prior to birth	262539.2	254793.3	249303.3	254557.0	13223.3*
	(138103.0)	(127300.8)	(119481.2)	(125728.7)	(6936.464)
Wage prior to birth	21832.0	21732.6	20783.2	21450.8	826.832*
	(9535.4)	(8500.6)	(7653.7)	(8259.7)	(462.590)
Observations	1262	1415	1336	1328	

NOTES.— The table reports mean characteristics between parents to children born over the turn of the years in 2001 and 2002, and the DiD estimates of the difference in average characteristics. *p<0.1, **p<0.05 ***p<0.01.

5. Results

5.1. Parental Leave. Before studying the effects of the three reforms in the parental leave system on parents' labor supply, we outline the results from estimating the effects of the reforms on parental leave take-up of both mothers and fathers.

As the data on parental leave usage is only available from 1988 onwards, we cannot study how parental leave take-up compares with the pre-reform cohort. However, we can study how the leave taking of parents to children born in 1988 compare to the leave taken by parents to children born in 1989, where all parents were eligible for the additional benefits. Figure 5.5 graphs the average parental leave days taken for mothers to children born in 1988, the first reform cohort, and to children born in 1989, the post-reform cohort. The level is higher for mothers to children born in 1989 since they are all eligible for the three additional months of parental leave. However, the trends in parental leave take-up by child birth month are more or less parallel for the birth months January to July. However, for mothers to children born in 1988, there is an almost linear increase in parental leave take-up for those individuals whose children were born in August onwards, that is consistent with the eligibility rules. Hence, unsurprisingly, the 1989-reform seemed to have had a positive effect of parental leave usage by mothers.

The corresponding graph for fathers is given in Figure 5.6. Interestingly, also fathers seem to have made use of the additional parental leave days. This is perhaps unexpected given that the increased entitlement to paid leave resulting from this reform did not earmark any paid leave for either parent. Thus, no gender quotas were in place or implemented for these cohorts. One possible explanation is that, making the leave more generous in a system where mothers can already stay at home with their children for one year increases the possibilities for fathers to use the additional days. In fact, this was explicitly expressed in the government proposition for the 1989 extension of leave entitlement. In the proposition it was argued that due to biological reasons (breastfeeding) it was natural that mothers claimed the majority of the 12 months of paid leave that were in place before the extension. It was further argued that if entitlement to paid leave was extended, it would therefore increase fathers' possibility to take a larger share of the total paid leave, which would be beneficial for both mothers and fathers, and thus for gender equality.

In Table 5.3 we report estimates of the effects of the 1989-reform on the parental leave days of mothers and fathers based on Equation (5.1). However, only parents to children born in 1988 and 1989 are included in the estimations since we lack data on parental leave days before 1988. Only individuals eligible to wage-replaced leave are included in the estimations, where we determine eligibility by the existence of a wage observation in the Wage Structure Statistics in the year prior to birth. The outcome variables are measured as the cumulated number of days taken within 2, 3, and 4 years after the birth of the first child, respectively.

Within two years after first birth, mothers' increased their parental leave days by roughly 17 days, on average, and by year four mothers had increased their leave by around 23 days. For fathers, the parental leave increased by roughly 2 days within the first 4 years after the birth of the first child.

The 1989-reform hence had a positive effect on the take-up of parental leave benefits. Also fathers increased their take-up of parental leave benefits as a result of the extension.

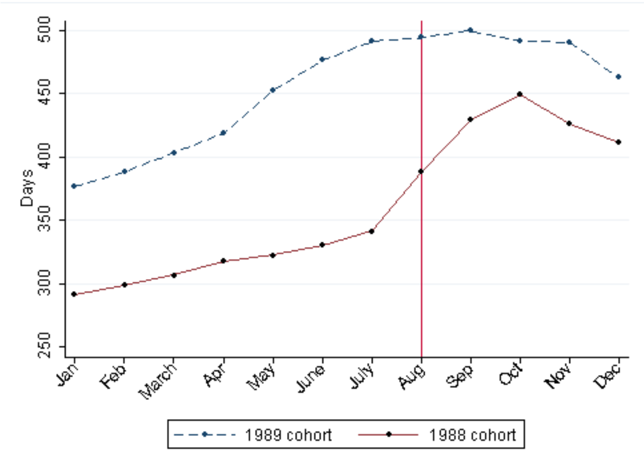


FIGURE 5.5. Parental leave days for mothers to children born in 1988 (reform cohort) and 1989 (post-reform cohort).

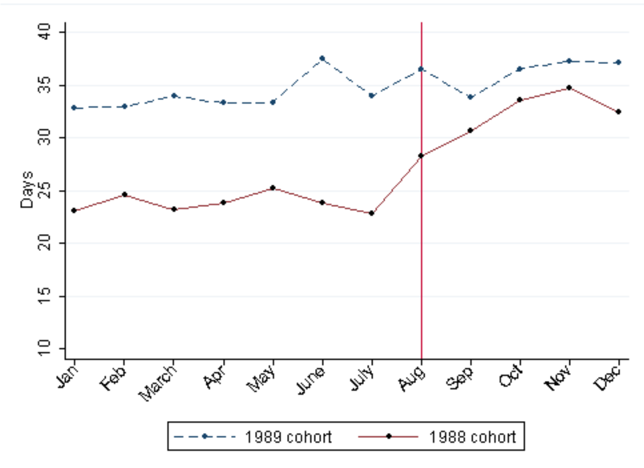


FIGURE 5.6. Parental leave days for fathers to children born in 1988 (reform cohort) and 1989 (post-reform cohort).

TABLE 5.3. The effect of the 1989-reform on parental leave take-up

Child age	2	3	4
A. Mothers			
Treatment	17.121***	27.038***	23.207***
	(3.019)	(3.170)	(2.609)
N=21824			
B. Fathers			
Treatment	1.632***	1.581**	2.142**
	(0.524)	(0.713)	(0.799)
N=10317			

NOTES.— The outcome variable measures the total number of parental leave days taken during the child's first two, first three and first four years, respectively. The sample consists of parents to children born in 1988 and 1989, and who were included in the Wage Structure Statistics one year prior to childbirth. Included covariates are dummies for individuals' birth year, labor income in the year prior to birth and a dummy for immigrants. Differences in the number of observations between (1) and (2) and between (3) and (4), respectively, are due to missing observations on covariates. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

Figure 5.7 plots the average number of parental leave days taken by fathers by children's birth month, for fathers of children born January 1994 to December 2004. There are sharp discontinuities around the two "daddy-month" reform cutoffs, indicating that the reforms increased fathers' parental leave take-up. The corresponding graph for mothers is given by Figure 5.8 and shows that mothers decreased their parental leave take-up in connection with the first daddy-month implementation. There is no evidence of a sharp discontinuity around the 2002-reform cutoff. However, the 2002 reform also increased the entitlement to paid leave by one month. This additional month is likely to have been used to a larger extent by mothers.

Table 5.4 presents the results from comparing averages between parents to children born in December 1994 and January 1995, columns (1) and (2), as well as estimates based on Specification (3), where the corresponding difference between parents to children born in December/January 1995/1996 have been taken into account to control for potential seasonal effects. Both models are estimated with and without control variables. Panel A presents the results for mothers. The simple comparison of averages shows that eligible mothers had, on average, 18 fewer days of parental leave during the child's first three years of life, compared to noneligible mothers. Including control variables hardly changes this estimate. Employing the difference-in-differences strategy, eligible mothers are estimated to have taken out around 15 fewer days of parental leave, a somewhat smaller effect compared to the simple differences in means between December/January mothers in 1994/1995. Thus, some of the difference between January and December parents are attributed to seasonal effects.

For fathers, the results suggest that eligible fathers had about 6-7 days more parental leave take-up compared to noneligible fathers. These findings are in line with previous studies which have shown that the first daddy-month reform increased fathers' leave (see e.g. Ekberg et al. 2013, Johasson 2010, Duvander and Johansson 2012, Eriksson 2005). Hence, both so called "daddy-month" reforms indeed increased fathers' parental leave usage.

The results from the analysis of the effect of the 2002-reform on parental leave days are presented in Table 5.5. For mothers, the difference in average parental leave days between mothers to children born in January 2002 (eligible) and mothers to children born in December 2001 (ineligible) suggests that there was no effect of the second daddy-month reform on mothers' parental leave days. However, the difference-in-differences estimates suggest that mothers' parental leave increased by roughly 6 days, on average. Recall that the 2002-reform also expanded benefits from 15 to 16 months. It is not surprising, therefore, that mothers' increased their take-up as a result of the reform.

For fathers, the difference analysis suggests that fathers increased their parental leave with around 12 days, on average. The difference-in-differences estimates, however, suggests a smaller effect of around 7 days. Thus, there seem to be some differences between fathers to children born in January and December that are correlated with parental leave take-up. However, taking these differences into account suggests that the second daddy-month reform had about the same average effect on fathers' parental leave days as the first daddy-month reform.

In summary, the expansion of parental leave benefits increased mothers' leave, as well as fathers' parental leave. The first and second daddy-month reforms increased fathers take-up of parental leave benefits, and for mothers, the first daddy-month reform implied a decrease of leave days. In the next section, we evaluate the effect of all three reforms on parents' labor supply, earnings and wage rates.

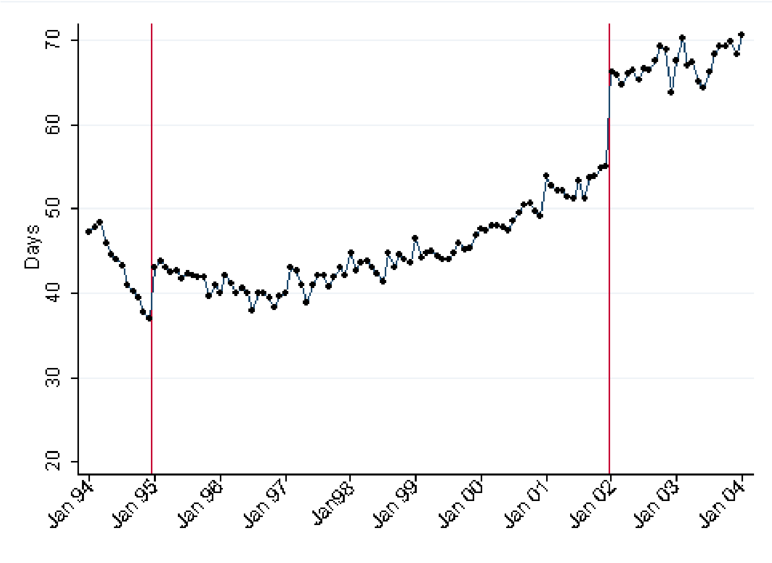


FIGURE 5.7. Fathers' total parental leave days during years 1, 2 and 3 after childbirth, by children's birth month.

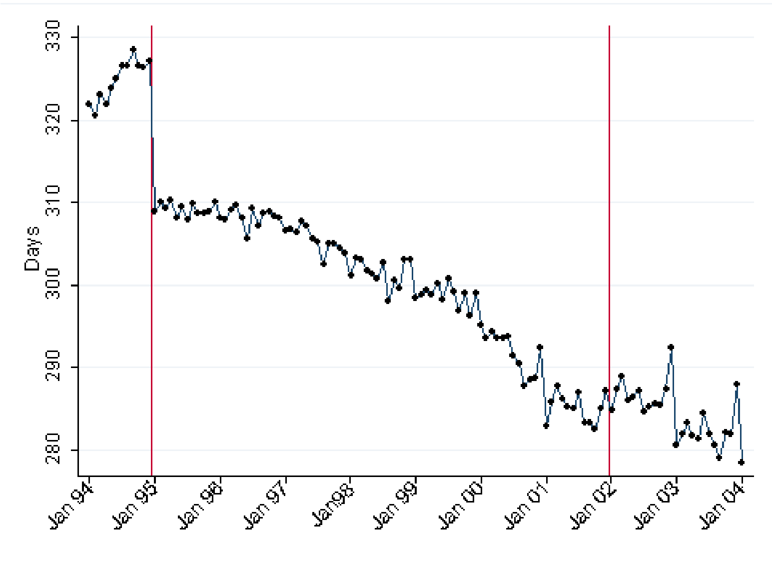


FIGURE 5.8. Mothers' total parental leave days during years 1, 2 and 3 after childbirth, by children's birth month.

TABLE 5.4. The effect of the 1995-reform on parental leave take-up

	(1) Diff	(2) Diff	(3) DiD	(4) DiD
A. Mothers				
January	-18.246***	-17.751***		
	(1.500)	(1.496)		
Jan x After			-14.875***	-15.239***
			(2.154)	(2.168)
After			16.572***	17.571***
			(1.590)	(1.600)
January			-3.021**	-2.254
			(1.534)	(1.551)
N	9706	9521	14931	14680
B. Fathers				
January	6.049***	6.314***		
	(1.378)	(1.422)		
Jan x After			6.712***	6.625***
			(1.923)	(1.999)
After			-3.682**	-4.272***
			(1.447)	(1.501)
January			-0.565	-0.341
			(1.345)	(1.415)
N	10441	9774	18860	17578

NOTES.— The outcome variable measures the total number of parental leave days with wage-replaced benefits taken during the child's first three years. Covariates are included in models (2) and (4) and include dummies for individuals' birth year, indicators for three levels of educational attainment (compulsory, high school and college) and a dummy for immigrants. Differences in the number of observations between (1) and (2) and between (3) and (4), respectively, are due to missing observations on covariates. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

TABLE 5.5. The effect of the 2002-reform on parental leave take-up

	(1) Diff	(2) Diff	(3) DiD	(4) DiD
A. Mothers				
January	-2.346	-1.380		
	(2.010)	(1.989)		
Jan x After			6.533**	6.256**
			(2.655)	(2.694)
After			-4.687**	-4.719**
			(1.967)	(2.001)
January			-8.779***	-7.572***
			(1.894)	(1.933)
N	5033	4766	9419	9003
B. Fathers				
January	11.281***	12.411***		
	(1.742)	(1.836)		
Jan x After			6.478***	7.152***
			(2.351)	(2.446)
After			6.122***	7.932***
			(1.704)	(1.784)
January			4.602***	5.026***
			(1.567)	(1.627)
N	6736	6134	13203	12125

NOTES.— The outcome variable measures the total number of parental leave days with wage-replaced benefits taken during the child's first three years. Covariates are included in models (2) and (4) and include dummies for individuals' birth year, indicators for three levels of educational attainment (compulsory, high school and college) and a dummy for immigrants. Differences in the number of observations between (1) and (2) and between (3) and (4), respectively, are due to missing observations on covariates. Robust standard errors are reported in parentheses. *$p<0.1$, **$p<0.05$ ***$p<0.01$.

5.2. Labor Supply. Based on the findings on parental leave take-up presented in the previous section, both mothers' and fathers' labor supply are expected to decrease as a result of the expansion of paid leave in 1989, if paid leave does not crowd out unpaid leave. Moreover, we expect mothers' labor supply to increase as a result of the first daddy-month reform introduced in 1995, and fathers' labor supply to decrease as a response. The 2002-reform increased total benefits by one month, at the same time as one additional month of paid leave was earmarked to each parent. As such, we expect to see a decrease in fathers' labor supply.

Table 5.6 shows the results from estimating the effect of the expansion of paid leave in 1989 based on Equation (5.1). The effect of the reform is evaluated up to different child ages, and each estimate corresponds to the effect of the expansion on the cumulated months worked from the birth year of the child up to the year that the child turns eight years old. Panel A reports the results for mothers, and panel B for fathers. For mothers, there is no effect of the expansion of paid leave on the labor supply in the year of childbirth. However, the cumulated months worked during child ages 1 and 2 are estimated to decrease by 0.11 months, on average. During the whole follow-up period of eight years, mothers' labor supply is estimated to decrease by 0.32 months, on average. For fathers, months worked are estimated to decrease by 0.63 on average, over the first eight years after childbirth.

Translated into the total reform effect, the estimate for mothers over the entire follow-up period amounts to an increase of about 10 days, and for fathers about 19 days, on average. The sum of the decrease for mothers and fathers thus amounts to 30 days, which corresponds to the total increased entitlement of paid leave. Hence, adding mothers' and fathers' labor supply responses together implies a full effect of the reform on labor supply. However, the labor supply responses seem to be spread out over a time horizon as long as eight years. The individual *consecutive* leave in connection to childbirth, i.e., within two years after birth, increased by less than a week (0.11 months for mothers, and an estimated decrease of 0.03 months, albeit not significantly different from zero, for fathers). Thus, a large part of the additional days of benefits was used for occasional days off from work for child care reasons over a relatively long time period.

Turning to the introduction of the first daddy-month reform, the results presented in Table 5.7 suggest that there is no effect on mothers' labor supply. The difference-in-differences estimates are positive, but small in magnitude and not significantly different from zero. For fathers, no clear-cut conclusions can be drawn. The difference in average labor supply between fathers to children born in December 1994 and January 1995 show statistically insignificant effects of 0.002 and 0.066 days in the models with and without control variables, respectively. The estimates from the difference-in-differences analysis suggest an increase in months worked, estimated to 0.072 months in the model without control variables. This estimate is, however, not significantly different from zero. In the model with covariates, there is a weakly significant effect suggesting that fathers' months worked increased by 0.13 months, on average.

The results from the analysis of the 2002-reform on parents' labor supply are presented in Table 5.8 and show no significant effects of the reform on either mothers' or fathers' months worked.

Taken together, the results presented suggest zero to small effects of the gender quotas in paid leave on parents' labor supply. Surprisingly, however, the expansion of paid leave in 1989 decreased fathers' labor supply more than for mothers, despite there being no gender quotas in place at the time of this reform. However, the additional days seem to have been spread out over the eight-year follow-up period, perhaps to increase job flexibility. The consecutive leave in connection with childbirth seems unaffected.

TABLE 5.6. The effect of the 1989-reform on parents' cumulated labor supply

Child age	1	2	3	4	5	6	7	8
A. Mothers								
Treatment	0.000	-0.110***	-0.218***	-0.257***	-0.228***	-0.196***	-0.196***	-0.324***
	(0.003)	(0.016)	(0.017)	(0.029)	(0.049)	(0.066)	(0.066)	(0.098)
N = 41830								
Baseline months: 10.302								
B. Fathers								
Treatment	-0.001	-0.034	-0.092*	-0.233***	-0.363***	-0.477***	-0.477***	-0.631***
	(0.007)	(0.022)	(0.046)	(0.074)	(0.099)	(0.141)	(0.141)	(0.199)
N = 22582								
Baseline months: 11.965								

NOTES.— The outcome variable measures the accumulated labor supply (in months) during the child's 1st year, first 2 years, first 3 years and so on, until the child turns eight years old. The covariates included are dummies for individuals' birth year, indicators for three levels of educational attainment (compulsory, high school and college), labor income the year prior to birth and a dummy for immigrants. The reason for the fewer observations on fathers is that we condition on wage observations in the year prior to birth, which exist for the entire public sector and half of the private sectors. As male workers are predominantly found in the private sector, males are under-sampled. Inference is based on clustered standard errors at the birth month/year level and reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

TABLE 5.7. The effect of the 1995-reform on parents' cumulated labor supply

	(1) Diff	(2) Diff	(3) DiD	(4) DiD
A. Mothers				
January	0.010	-0.003		
	(0.030)	(0.029)		
Jan x After			0.033	0.016
			(0.047)	(0.045)
After			0.051	0.047
			(0.034)	(0.032)
January			-0.023	-0.016
			(0.036)	(0.034)
Observations	3320	3320	5747	5747
B. Fathers				
January	0.002	0.066		
	(0.049)	(0.043)		
Jan x After			0.072	0.126*
			(0.075)	(0.066)
After			-0.206***	-0.197***
			(0.053)	(0.047)
January			-0.071	-0.057
			(0.057)	(0.050)
Observations	2579	2579	4491	4491

NOTES.— The outcome variable measures the accumulated labor supply (in months) during the child's first three years of life. The covariates included are dummies for individuals' birth year, indicators for three levels of educational attainment (compulsory, high school and college), labor income the year prior to birth and a dummy for immigrants. Differences in the number of observations between (1) and (2) and between (3) and (4), respectively, are due to missing observations on covariates. Inference is based on clustered standard errors at the birth month/year level and reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

TABLE 5.8. The effect of the 2002-reform on parents' cumulated labor supply

	(1) Diff	(2) Diff	(3) DiD	(4) DiD
A. Mothers				
January	0.002	-0.003		
	(0.037)	(0.036)		
Jan x After			-0.061	-0.021
			(0.051)	(0.049)
After			-0.057	-0.063*
			(0.039)	(0.038)
January			0.063*	0.017
			(0.035)	(0.034)
Observations	2512	2512	4983	4983
B. Fathers				
January	0.043	-0.004		
	(0.047)	(0.043)		
Jan x After			0.033	-0.000
			(0.070)	(0.064)
After			-0.268***	-0.276***
			(0.049)	(0.045)
January			0.010	-0.010
			(0.051)	(0.047)
Observations	2607	2607	5212	5212

NOTES.— The outcome variable measures the accumulated labor supply (in months) during the child's first three years of life. The covariates included are dummies for individuals' birth year, indicators for three levels of educational attainment (compulsory, high school and college), labor income the year prior to birth and a dummy for immigrants. Differences in the number of observations between (1) and (2) and between (3) and (4), respectively, are due to missing observations on covariates. Inference is based on clustered standard errors at the birth month/year level and reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

6. Sensitivity Analysis

6.1. Effects of the Reforms on Monthly Wage Rates. Our main outcome variable measures months worked in a calendar year, and is constructed by dividing the annual labor income with the wage rate in the year prior to birth. One potential concern with this definition of months worked is that wage rates themselves are affected by extended parental leave periods. While this is likely to be of lesser concern in the short run when most parents - at least mothers - are on formal parental leave, this could potentially bias our estimates on labor supply in the long run. To investigate this issue, we estimate the effect of each of the reforms on individuals' log monthly wage rates, by years since birth, starting from year five after birth, up to eight years after birth. The reason for studying medium- to long run wage effects is that most parents are back at the workplace in the medium run, and we only observe wages for individuals that are present at the workplace during the measuring month (November or September). Thus, individuals with wage observations in the very short run are likely to be a selected group, as most parents, in particularly mothers, stay at home to take care of their children during the first one to two years after birth.

The results for the 1989-reform are presented in Table 5.9 and show that there are no effects of expanded parental leave benefits on either mothers' or fathers' wages. The estimates are all either zero or close to zero and not significantly different from zero, aside from a weakly significant effect for fathers in year 7 after the birth of the first child. Thus, we find no evidence that the extension of paid leave introduced in 1989 affected parents' wages. It is likely therefore, that the effects found on our labor supply measure are driven by hours worked, and not by changes in the wage rate.

The corresponding figures for the introduction of the first daddy-month in 1995 are shown in Table 5.10. Also here, the estimates for mothers are zero and not statistically significant. For fathers, there is a weakly significant positive effect on wages in year 7 after the birth of the first child. All in all, however, there is no evidence suggesting that either reforms affected individuals' wages. For parents eligible for the second daddy-month reform introduced in 2002, we cannot estimate medium- to long-run effects on wages since the data only allows us to follow individuals until 2007.

As an additional sensitivity check, we also run regressions where we estimate the effect of the 1995- and 2002-reforms on the annual labor income, separately by child age. Note that the same analysis cannot be done for the 1989-reform. The reason for this is that the effect of the reform will show up either the first or the second full year after childbirth depending on the children's birth month. As pointed out in Liu and Skans (2010), this creates a mechanical interaction between the reform and birth month when studying outcomes using data on an annual basis. In our main analysis, we therefore rely on aggregated data, where we cumulate our labor supply measure over an increasingly wider window. However, when studying the effect of the 1995- and 2002-reforms we study differences in outcomes between parents who had children within one month before and one month after the eligibility cutoff, which makes a mechanical interaction between eligibility and outcomes less likely when relying on annual data. Thus, we can study the effects of the two daddy-month reforms on annual labor income by child age. The results from this analysis are presented in Table A3 and shows that all estimates are small in magnitude, and most are statistically insignificant. There is, however, a small positive effect of around 350 SEK for mothers in the year of childbirth, resulting from the 1995-reform, and a positive effect also for fathers of about 520 SEK in the same year. There is also a weakly significant positive effect on mothers' income in year 7 after birth. For the 2002-reform, there are no significant effects for fathers, and two positive effects for mothers of around 800 SEK in years 2 and 4 after birth, respectively. All estimates are, however, small in magnitude, and it is difficult to clear stark conclusions from this analysis. Nevertheless, we do not find any strong evidence suggesting that mothers' earnings are positively affected by any of the reforms, or that fathers' earnings decreases as they take more parental leave, which is in line with the findings from our previous analyses.

TABLE 5.9. The effects of the 1989-reform on the log of full-time equivalent monthly wage rates 5-8 years after childbirth

Years since birth	5	6	7	8
A. Mothers				
Treatment	-0.000	-0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	28265	30890	34277	35445
B. Fathers				
Treatment	0.003	-0.001	-0.002*	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Observations	15327	18534	22879	24569

NOTES.— The outcome variable measures the natural log of full-time equivalent monthly wages in different years since birth. The sample consists of individuals whose first child was born during 1987-1989, and who are observed to have at least one wage observation during the follow-up period. Standard errors are clustered at the child birth month/year level and reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

TABLE 5.10. The effects of the 1995-reform on the log of full-time equivalent monthly wage rates 5-8 years after childbirth

Years since birth	5	6	7	8
A. Mothers				
Jan x After	0.006	-0.006	0.007	0.002
	(0.011)	(0.011)	(0.011)	(0.011)
Observations	3930	3747	3944	3965
B. Fathers				
Jan x After	0.014	0.021	0.032*	0.023
	(0.014)	(0.016)	(0.017)	(0.017)
Observations	3130	3041	3098	3068

NOTES.— The outcome variable measures the natural log of full-time equivalent monthly wages in different years since birth. The sample consists of individuals whose first child was born during January-December 1994,1995 and 1996, and who are observed to have at least one wage observation during the follow-up period. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.

6.2. Subsequent Fertility and Strategic Timing of Birth. The extension of paid leave from 12 to 15 months was implemented in July 1st, 1989, but retroactively covered parents to children born in 1988. It is therefore unlikely that parents could have anticipated the reform and manipulated the timing of their children's birth in order to benefit from the reform. Moreover, the total effect of the 1989-reform on both mothers' and fathers' labor supply sum up to a full reform effect, suggesting that parental leave days for additional children are not likely to complicate our analysis. Nevertheless, we estimate the effect of the 1989-reform on the annual probability of a (subsequent) birth by years since first birth. The results from this analysis are presented in Figure 5.9. The estimates lie on the zero-line for all years except years 5 and 6, where there are significantly negative and positive effects, respectively, on the probability of birth. This pattern suggest a shift in the spacing of births. The estimates are, however, small in magnitude and not economically significant.

Another potential concern, regarding the 1995- and 2002-reforms is that parents strategically time the births of their children to benefit (or not benefit) from the reforms. Table 5.11 reports the total number of children born in the years 1994-2004, and the total number of children born in January and December, respectively, in the same years. As seen, the share of January- and December-born children is constant over the studied time period. Thus, there is no evidence that parents strategically manipulated the timing of their children's birth in order to benefit from the two reforms.

Taken together, the sensitivity checks provided in this section suggest that there were no impacts on either of the reforms on monthly wage rates of parents, no economically significant effects on subsequent fertility, and no strategic manipulation of the timing of births. The non-existent impacts on wages support the notion that our labor supply measure indeed measures months worked, and that it is not biased by potential effects of the reforms on wage rates.

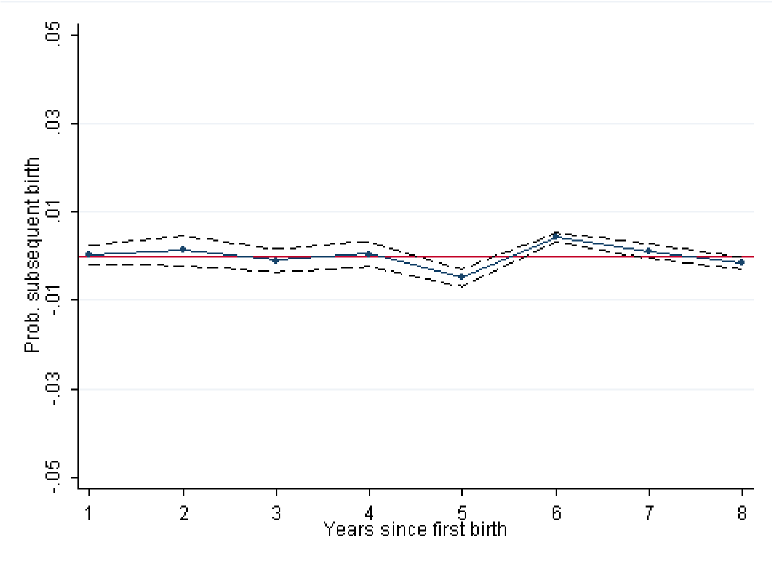


FIGURE 5.9. Estimates of the effect of increased entitlement to paid leave (the 1989-reform) on the yearly probability of giving birth to subsequent children, by time since first birth.

TABLE 5.11. The share of January-born children among all children born during 1994-2004

	(1) # kids born	(2) # kids born in January	(3) Share January-born	(4) # kids born in December	(5) Share December-born
1994	120504	10074	0.084	8780	0.073
1995	111443	9554	0.086	7576	0.070
1996	103026	8732	0.085	7420	0.072
1997	98123	8502	0.087	6914	0.071
1998	96666	8017	0.083	6844	0.071
1999	95377	7852	0.082	6985	0.073
2000	97372	7981	0.082	6991	0.072
2001	97418	8263	0.085	6835	0.072
2002	101270	8298	0.082	7356	0.073
2003	103894	8468	0.082	7643	0.074
2004	105377	8785	0.082	7673	0.073

NOTES.— Frequency of births and births in January 1994-2004.

7. Concluding Remarks

This paper analyzes labor supply responses to three of the most recent reforms in the Swedish parental leave system. The first reform that we evaluate expanded entitlement to governmentally funded, wage-replaced parental leave benefits from 12 to 15 months. The second reform that we study earmarked one month of wage-replaced parental leave benefits to each parent among eligible households, and thus introduced a so called "daddy-month" in parental leave. The last reform reserved an additional month of paid leave to each parent, and at the same time extended benefit entitlement with one month; from 15 to 16 months. In all reforms, eligibility varied with child birth date, creating natural experiments that allow us to estimate the causal effects of the reforms on parental labor market behavior.

We find that the general expansion of paid leave entitlement by three months increased mothers' take-up of parental leave. Also fathers increased their parental leave days as a response to this reform. We find corresponding decreases in months worked for both mothers and fathers. Hence, paid leave does not seem to have crowded out unpaid leave. However, the additional benefits were spread out over an 8-year long time horizon, which is the time period after the birth of the child during which the benefits must be used. The latter finding suggests that parents used the additional paid leave to increase job flexibility; the consecutive leave in connection with child birth was unaltered.

The introduction of the two "daddy-months" increased fathers' parental leave days, and the first daddy-month reform also decreased mothers' parental leave. However, we do not find any effects on either parents' months worked, earnings or wage rates. One possible explanation for the nonexistent labor supply responses among mothers - who should arguably return to work sooner as part of the paid leave is re-allocated from mothers to fathers - is that mothers instead resort to unpaid, but job protected, leave. For fathers, it seems unlikely that paid leave crowds out unpaid leave. However, a possible explanation could be that additional days are used for occasional days off or to prolong vacations.

From a policy perspective, our findings have a couple of interesting implications. First, our results suggest that, among parents who are eligible for wage-replaced parental leave, the household's financial constraint may not be binding regarding the amount of leave taken in direct connection with childbirth. Parents seem to use additional benefits to essentially buy job flexibility over a long time horizon. Thus, in a system with job protection that exceeds the duration of paid leave, and with a great portion of flexibility as to how and when to use the parental leave, it is not obvious that that the time spent with very young children is affected by additional paid leave entitlement. Moreover, as we - in line with earlier literature - find no or small effects on long-run outcomes such as income, wages, or fertility, it seems unlikely that the gained flexibility increased the opportunity to combine work and family, or that it came at a cost - or gain - to long run labor market attachment.

In a recent paper, Dahl et al. (2013) point out that the parental leave system in Norway is costly and has regressive redistributive properties; a pure leisure transfer, primarily to middle and upper income families. In comparison to the Norwegian system, the Swedish system is also costly, but the redistributive component is less salient, mostly due to institutional differences between the countries; the replacement rate in Sweden is less than 100 percent and the income cap is at a lower level (36 percent of all women and 54 percent of all men have incomes above the income cap in Sweden). Nevertheless, the further expansion of paid leave in Sweden seem to have worked as a transfer of funds and increased work flexibility to families with children.

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Appendix

TABLE A1. The Parental leave system 1988-2009

Year	Wage-replaced days	Replacement rate,%	SEK/day if SGI= 0	Flat rate days	SEK/day, flat rate
1988	270	90	60	90	60
1989	360	90	60	90	60
1990	360	90	60	90	60
1991	360	90	60	90	60
1992	360	90	60	90	60
1993	360	90	60	90	60
1994[a]	360	90	64	90/0	60/0
1995[b]	360	80	60	90	60
1996[c]	360	75	60	90	60
1997	360	75	60	90	60
1998	360	80	60	90	60
1999	360	80	60	90	60
2000	360	80	60	90	60
2001	360	80	60	90	60
2002[d]	390	80	120	90	60
2003	390	80	150	90	60
2004	390	80	180	90	60
2005	390	80	180	90	60
2006[e]	390	80	180	90	60/180
2007	390	80	180	90	180
2008	390	80	180	90	180
2009	390	80	180	90	180

NOTES.— a) During the second half of 1994, the flat-rate days were temporarily abolished for children older than one year. b) The first "daddy month" was introduced for parents to children born January 1, 1995 or later. For the 30 days of reserved leave, the replacement rate remained at 90 percent of previous earnings. c) For the 30 days of reserved leave, the replacement rate remained at 80 percent of previous earnings. d) The flat rate was set to 180 SEK from July 1, 2006 onwards. e) The second "daddy month" was introduced for parents to children born January 1, 2002 or later.

TABLE A2. Covariate balance tests between parents to children born in the reform cohort, pre- and post-reform cohorts (1989-reform)

	(1) Fall-Spring 1987	(2) Fall-Spring 1988	(3) Fall-Spring 1989	(4) DiD 87/88	(5) DiD 89/88
A. Mothers					
Age at 1st birth	-0.387***	-0.265*	-0.278***	-0.123	-0.013
	(0.0789)	(0.105)	(0.0745)	(0.132)	(0.127)
Number of children in 2007	-0.00477	-0.0154	-0.0679***	0.011	-0.052**
	(0.0152)	(0.0149)	(0.0143)	(0.021)	(0.021)
Immigrant	-0.000633	0.00115	0.00974*	-0.002	0.009
	(0.00478)	(0.00467)	(0.00454)	(0.007)	(0.007)
Live in large city	0.00168	0.0142*	0.0204**	-0.013	0.006
	(0.00734)	(0.00713)	(0.00681)	(0.010)	(0.010)
Income prior to birth	742.1	524.6	1663.7*	217.511	1139.077
	(774.4)	(765.9)	(739.7)	(1089.6)	(1064.5)
Wage prior to birth	-113.2**	-71.54	-1.210	-41.710	70.328
	(37.91)	(38.03)	(41.39)	(53.73)	(56.37)
B. Fathers					
Age at 1st birth	0.511***	0.631***	0.324	-0.121	-0.308
	(0.131)	(0.132)	(0.188)	(0.187)	(0.231)
Number of children in 2007	0.0205	-0.00888	-0.0195	0.029	-0.011
	(0.0208)	(0.0205)	(0.0200)	(0.029)	(0.029)
Immigrant	0.0101	0.0152*	0.0164*	-0.005	0.001
	(0.00732)	(0.00721)	(0.00721)	(0.010)	(0.010)
Live in large city	0.0110	0.0209*	0.0142	-0.010	-0.007
	(0.00960)	(0.00944)	(0.00920)	(0.013)	(0.013)
Income prior to birth	1650.0	603.1	-2263.7	1046.941	-2866.780
	(1417.9)	(1471.4)	(1451.6)	(2044.9)	(2067.0)
Wage prior to birth	73.45	98.89	8.075	-25.431	-90.810
	(74.48)	(77.73)	(76.03)	(107.74)	(108.72)

NOTES.— The table reports differences in mean characteristics between parents to children born in the Fall and Spring, within 1987, 1988 and 1989, respectively (column 1-3), and the DiD estimates of these differences between 1987/1988 and 1989/1988 (column 4 and 5, respectively). *p<0.1, **p<0.05 ***p<0.01.

TABLE A3. The effects of the 1995- and 2002-reforms on parents' yearly labor income, 1000s SEK

Child age	1	2	3	4	5	6	7	8
				1995-reform				
A. Mothers								
Jan x After	0.355***	0.260	0.073	0.159	0.177	0.353	0.639*	0.439
	(0.091)	(0.188)	(0.239)	(0.263)	(0.276)	(0.302)	(0.332)	(0.335)
N	15147	15080	15039	15002	14981	14955	14952	14944
B. Fathers								
Jan x After	0.521**	0.326	0.425	0.069	-0.391	0.253	0.663	0.719
	(0.216)	(0.270)	(0.293)	(0.342)	(0.505)	(0.511)	(0.451)	(0.439)
N	20655	20541	20444	20372	20313	20258	20242	20196
				2002-reform				
A. Mothers								
Jan x After	0.076	0.790***	0.009	0.788**				
	(0.159)	(0.300)	(0.370)	(0.378)				
N	9338	9319	9310	9290				
B. Fathers								
Jan x After	0.134	0.328	0.619	0.299				
	(0.353)	(0.385)	(0.438)	(0.486)				
N	14101	14059	14036	14000				

NOTES.— The outcome variable measures the yearly labor income, in 1000s SEK, in different years after childbirth. Covariates are included are dummies for individuals' birth year, indicators for three levels of educational attainment (compulsory, high school and college) and a dummy for immigrants. Robust reported in parentheses. *p<0.1, **p<0.05 ***p<0.01.