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Gender, Incentives and the Division of Labor

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Abstract
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Essay 1: The length of parental leave entitlements is known to affect take-up rates, division of parental leave between parents, and the mother's decision to return to work. So far, however, the importance of the *level* of benefit has received little attention in the literature. Using population wide register data, I exploit the “speed premium” rule in the Swedish parental leave system as a source of random variation in the benefit level. A fuzzy RD strategy is used to estimate the causal effect of a change in the level of benefits per day on the utilization of parental leave among Swedish parents. The results suggest that parents’ take-up of benefits is highly sensitive to the benefit level. A 1% (5 SEK ≈ 0.54 $) increase in the mother's benefits per day is found to increase her length of leave by about 1 % (2.6 days). This translates into an elasticity of take-up duration (length of spell) with respect to the benefit level of 1, a parameter that has not been estimated before. Fathers respond to the increase in mothers’ take-up by reducing their time on leave by an almost equivalent number of days (1.9 days). In other words, the change in benefit level affects not only the individual’s take-up, but the division of parental leave between parents.

Essay II: In this paper, I compare the effect of entering parenthood in lesbian and heterosexual couples using Swedish population-wide register data. Comparing couples with similar pre-childbirth income gaps, a difference-in-differences strategy is used to estimate the impact of the gender composition of the couple on the spousal income gap after childbirth. The results indicate that the gender of the parents' does matter for their division of labor as, five years after childbirth, the income gap is significantly smaller in lesbian than in heterosexual couples, also when comparing couples with the same pre-parenthood income gap. Part of the explanation is a difference in biological restrictions: lesbian partners often give birth to one child each and spend more time at home with the child they carried. Other explanations are the influence of gender norms and differences in preferences between lesbian and heterosexual couples.

Essay III: The skewed division of parental responsibilities during a child's infancy is often assumed to be a natural consequence of the mother being pregnant and wanting to breastfeed. In this paper, I investigate to what extent the tendency to let the mother be the main caregiver of an infant can be explained by the fact that she is the one to be pregnant, not the father. Using the division of parental leave during the child's first two years with the parents as a proxy for the division of parental responsibilities, I compare the behavior of biological parents (where the mother gave birth) to adoptive parents (where she did not) in Swedish population-wide register data. My results show that adoptive parents, both mothers and fathers, spend less time on parental leave than biological parents, but that the mother's share of leave is about the same as among biological parents. There is thus some support for the hypothesis that a biological tie increases parents' initial investment in children, but not that this relationship is stronger for women. Hence, there is no evidence that the mother's birth giving status can explain her share of parental responsibilities. Due to methodological challenges, it is difficult to disentangle the different mechanisms that could explain the results.

Essay IV (with Spencer Bastani and Håkan Selin): No previous quasi-experimental paper has systematically examined the relationship between the extensive margin labor supply response to taxation and the employment level. We model the labor force participation margin and estimate participation responses for married women in Sweden using population-wide administrative data and a solid identification strategy. The participation elasticity is more than twice as large in the lowest-skill sample (with relatively low employment) as compared with the highest-skill sample (with high employment). Our analysis suggests that cross- and within country comparisons of participation elasticities always should be made with reference to the relevant employment level.

Keywords
Parental Leave, Division of Labor, Labor Supply, Same-sex Couples, Transition to Parenthood, Gender Norms, Housing Allowance, In-Work Tax Credits, Take-Up of Transfer Programs, Secondary Earners' Participation Elasticity.

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Dedicated to my grandfather, Edvard Larsson.

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Stockholm, April 2017
Ylva Moberg

Contents

Introduction

This thesis consists of four self-contained papers, three single authored and one coauthored. In essay 1, I study how changes in opportunity costs affect the take-up and division of parental leave among parents in Sweden. In essay 2 and 3, I investigate how gender norms and biology might influence couples' division of parental responsibilities and labor supply choices. In essay 4 my coauthors and I estimate the labor force participation elasticity of married women across different skill groups. These papers are connected in that they all in some way investigate how the decision-making process within households is influenced by economic incentives, gender and social norms. As an introduction to the thesis I briefly discuss how economists have described decision-making within the family, before providing a summary on the content of the essays.

1 Decision-making within the family

The decision-making process within the family was long considered to be outside the realm of economic studies. As economist gradually expanded the subjects which they studied, a process sometimes referred to as economic imperialism (Lazear 2000), questions about the inner workings of family life also got their attention. As economists attempted to describe the intrahousehold decision-making, they did as they are prone to do; once again turning to neoclassical theory combined with statistical models for hypothesis testing. As a result, family life in the economic literature is usually described in strictly mathematical terms, although be it with textual interpretations.[1] One of the leading economists in this process was Gary Becker who, with his influential book *A Treatise on the Family* (first published in 1981), once and for all put family economics into the category of mainstream economics. Becker describes the couple's decision on division of labor as the outcome of an optimization where the couple maximizes their production of household goods and income by devoting each task to the spouse with the highest relative productivity (Becker and Becker, 2009). Thus, in Becker's model, specialization is described purely as a "win-win" type of situation. This view might seem

[1] As a side note, I think that the fact that this approach is so unlike most other social sciences, like sociology and anthropology, has probably been an obstacle to the willingness and ability of family economists to communicate with researchers in other disciplines. This is unfortunate since I think that a lot could be won by learning from each other.

naïve given the complete disregard for the notion that spouses can have different interests and preferences. However, Becker's models were a step towards seeing each individual's contribution to the household. In view of the criticism that the Becker-type models did not describe family life in a realistic way, others have turned to game theory to be able to take into account how different interests and relative strengths in bargaining power affect the allocation of time and tasks between spouses in a household. In these models, the spouses typically make simultaneous choices of their allocation of time in a cooperative process were each partner can threaten to leave the relationship if the outcome of the negotiation is not acceptable to them. The spouses' relative bargaining powers are typically determined by the individual's (financial) situation in case of divorce, as in for example McElroy and Horney (1981).[2] These models have the advantage of being perceived as more nuanced or "realistic", but with the drawback that they are often immensely complicated in a mathematical sense.[3] A special case of the game theory approach is the so-called "male-chauvinist" model, in which couples make decision in a Stackelberg-like way. The husband makes his choice first, deciding for example how many hours to work, after which the wife makes her choice taking her husband's decision as given. As old fashioned as this model may seem, it is not completely unrealistic to describe a situation where the husband is the dominant party in the relationship, for example due to social or cultural norms. It also has at least one other advantage; namely that it is, relative to other models, mathematically more simple.[4] It is easier to model a situation where one person takes another person's already made decision into account, than a situation where two individuals make decisions simultaneously and dependent on each other. As insightful as the simultaneous decision models may be, it can be challenging to formulate them in a way that produces parameters that can easily be estimated in a regression analysis.[5]

The economics of same-sex couples

In the last decades, the economics literature of families has been expanded to the analysis of same-sex couples. Formally, most of the above-mentioned models are formulated in strictly gender-neutral terms. However, it is clear when reading these papers that they were all constructed with a married heterosexual couple in mind. As Lee Badgett, a pioneer in the area, points out, the

[2] However there are alternative views: for example Lundberg and Pollak (1993) consider a situation where the spouses' "threat point" is to stop cooperating with each other within the marriage.

[3] See for example Chiappori (1988) and Chiappori (1992).

[4] Indeed, this is one of the reasons why, in Essay 4, we have this type of model in mind when estimating the effect of changed economic incentives on the wife's labor supply, while taking the husband's income as given.

[5] For a discussion on how to empirically test the so-called collective model see Crespo (2009) or Chiappori et al. (2002).

lives of same-sex couples cannot always be accurately described by economic models that have been developed to analyze heterosexual couples (Badgett, 2003). For example, traditionally the spousal earnings gap is defined as the man's earnings minus the woman's. Thus, to compare the earnings gap of same-sex and heterosexual couples one needs to either fit the same-sex couples into this framework or change the definition of the earnings gap for all couples. Researchers who have been faced with this problem have chosen different paths. Some have chosen to continue to define heterosexual spouses according to gender, while defining the same-sex partners according to age, income or years of schooling. Unavoidably, this leads to an unreasonable way of comparing couples. In Essay 2 in this paper, I get around this methodological problem by defining both types of couples as the partner who gave birth and the other partner.[6]

Power and taxes

Related to how one might think of the decision-making within the household, is how one might think of power. When defending my bachelor thesis, I was asked how I would define power within the neoclassical framework. My answer was that I would define power as "the ability to shape your life according to your preferences". I still think that that is a pretty accurate description of how an economist might think of power. Unlike Robert Dahl's "A makes B do something that B would not otherwise have done", an economist would say something like "A changes the opportunity set of B, in a way that affects B's optimization given her utility function". In other words, B still makes her choice independently, but is forced to take the actions of A into account. As is often assumed in the bargaining models mentioned above, the spouses' earnings (or earnings potentials) play a key role in the distribution of power within families. However, as for example Alesina et al. (2011) proposes, it might be that men as a direct consequence of gender norms/traditions enjoy more bargaining power. We know from the literature on responses to changes in taxes that women's labor supply is generally more elastic than that of men. Therefore Alesina et al. (2011) suggests a higher tax rate for men as a way to promote gender equality (so-called gender based taxation). This way, women would be incentivized to work more hours, and thus increase their earnings, which would tilt the power balance in the couple. For the readers who are enraged by such a proposal let me remind you that, as Andrea Ichino (one of the authors of Alesina et al. 2011) pointed out at an IFAU seminar in the

[6] An economic approach to study LGBT-related questions is still developing. So far, the norm has been to use the labels of gay, for couples consisting of two men, lesbian, for couples consisting of two women, and heterosexual, for couples consisting of one man and one woman. Unfortunately, using these labels hides the existence of bisexuals and also imply a cis-normative view on gender, for example when it's assumed that female partners can get pregnant. I am myself guilty of contributing to this since, in line with the literature, I also use these labels and definitions.

spring of 2017, women's tax rates are in in fact higher than men's in many countries when they are married or cohabiting. Because of joint taxation of couples and transfers based on household income, the de facto tax rate is often higher for secondary earners. As is demonstrated in Essay 4, increasing the gain of entering the labor market for secondary earners can have positive effects on the labor market participation of married women with children. Thus, as a policy recommendation one does not need to go as far as implementing higher tax rates for men; just implementing equal tax rates for men and women regardless of marital status would create a similar effect in many countries.

2 The chapters

In **Essay 1**, I investigate how a change in the parental leave benefit level, and thus the opportunity cost of working, affects the take-up behavior and labor supply choices of couples with children. Today, nearly all OECD-countries have a system of paid parental leave. Yet, we know relatively little about the overall effects of these systems. An aspect that has been particularly underinvestigated is how the *level* of benefits affects utilization of parental leave. Using a regression discontinuity method, I exploit an administrative rule known as the *Speed-premium* in the Swedish parental leave system that gives rise to exogenous variation in the benefit level across a threshold, to estimate the effect on parents' utilization of parental leave and labor supply. To the best of my knowledge, this is the first paper that estimates the causal effect of a change in the parental leave benefit level on the parents' take-up and division of parental leave.

My results indicate that a positive shift of 1% in the benefit level per day of the mother induces her to spend 2.6 more days on paid parental leave. Thus, mothers' take-up is found to be very responsive to changes in the benefit level: the elasticity of take-up duration of parental leave benefits is calculated to be 1. This parameter has not been estimated before using exogenous variation in the parental leave benefit level. Taking advantage of the fact that there is a shift in the benefit level of mothers, but not fathers, I estimate the cross-spousal effect on fathers to be a reduction in take-up of 1.9 parental leave days. In other words, I find that the change in the mother's benefit level primarily induces a shift in the division of parental leave between parents, rather than an increase in the couple's total length of leave. The results indicate that even modest changes in economic incentives can have large effects on couples' division of time spent caring for the child (in terms of time on parental leave). Looking forward, future research should investigate whether these behavioral changes have any long-term consequences for the parents' labor market outcomes or division of labor in the household. However, previous research has found that changes in take-up, within an already generous system, have small or neglectable effects on long-term earnings (Karimi et al., 2012, Lalive and

Zweimüller, 2009). Changes in the division of take-up of parental leave has also typically not been found to have long-term effects on parents' division of labor within the household (Ekberg et al., 2013).

In **Essay 2**, I compare how the income gaps within lesbian and heterosexual couples are affected by becoming parents, in order to investigate whether the gender composition of the parents is a determining factor for the division of labor in couples with children. Earlier studies on heterosexual couples have previously found that the degree of specialization increases after the couple has children. Angelov et al. (2016) for example, find that the earnings trajectories of men and women develop along similar trends before entering parenthood, after which the within couple labor income gap widens and stays permanently larger (the authors estimate the effect for the first 18 years after the first child's birth). Comparing only couples with similar income gaps and levels before becoming parents, I find that couples consisting of one man and one woman specialize more in terms of labor supply after having a child compared to couples consisting of two women. Even though this comparison does not make it possible to estimate a causal "effect" of parental gender, my results indicate that the gender composition of the couple does matter for their choice of division of parental responsibilities and thus labor supply.

The results also show that both types of couples divide time on parental leave in a similar way: namely by letting the partner who gave birth to the child take the vast majority of parental leave days. In **Essay 3**, I investigate further to what extent the birth giving status/biological motherhood matters for the couple's choice of division of parental responsibilities. The empirical strategy is to compare the division of the parental leave among biological parents (where the mother gave birth) to that of adoptive parents (where she did not). My results indicate that the mother's share of the parental leave with the couple's first child is almost the same among (heterosexual) biological and adoptive parents, but that the time spent on leave is shorter for adoptive parents than for biological (both fathers and mothers). Similar results are found when comparing the division of leave for adoptive and biological children within the same family.

In **Essay 4**, which is coauthored with Håkan Selin and Spencer Bastani, we estimate the labor force participation elasticity of married women by exploiting the exogenous variation in incentives to work caused by a reform of the housing allowance system in Sweden. The reform implied moving from a system where the allowance was phased out if the *total household income* exceeded a cap, to a system where it is phased out if the *individual income of either spouse* exceeds a specific level. The reform has similarities with going from a system of joint, to individual taxation. Due to the reform a spouse with low or no income can increase her earnings without the household losing allowance, even if her partner's earnings are above the cap level.

We estimate the participation elasticity of women in low-income households in four different skill-groups with different baseline employment levels and find that the elasticity of relatively low-skilled women is about twice as large as the elasticity of relatively high-skilled women. Our results suggest that one should expect heterogeneous behavioral responses to changes in the tax and transfer system depending on the baseline employment and skill level in a specific group. This result has implications for making correct predictions of the effects of policy changes.

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I. Speedy Responses: Effects of Higher Benefits on Take-up and Division of Parental Leave

The length of parental leave entitlements is known to affect take-up rates, division of parental leave between parents, and the mother's decision to return to work. So far, however, the importance of the level of benefit has received little attention in the literature. Using population wide register data, I exploit the "speed premium" rule in the Swedish parental leave system as a source of random variation in the benefit level. A fuzzy RD strategy is used to estimate the causal effect of a change in the level of benefits per day on the utilization of parental leave among Swedish parents. The results suggest that parents' take-up of benefits is highly sensitive to the benefit level. A 1% (5 SEK ≈ \$0.54) increase in the mother's benefits per day is found to increase her length of leave by about 1 % (2.6 days). This translates into an elasticity of take-up duration (length of spell) with respect to the benefit level of 1, a parameter that has not been estimated before. Fathers respond to the increase in mothers' take-up by reducing their time on leave by an almost equivalent number of days (1.9 days). In other words, the change in benefit level affects not only the individual's take-up, but the division of parental leave between parents.

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1 Introduction

Most countries around the world have some form of family leave policy. Introduction of family leave policies is often motivated by wanting to give parents, in particular mothers, a better chance to balance paid labor and family life. Another policy goal is to promote gender equality through improving job continuity of mothers, in the hopes that this will reduce the long-term gender gap in wage and earnings trajectories. Nearly 50 % of the members of the International Labor Organization (ILO) have at least 14 weeks of paid maternity leave (ILO, 2010). In addition, most countries have some amount of paternity leave, ranging from 1 day (Saudi Arabia) to three months (Iceland, Norway, Sweden and Slovenia). In addition, many OECD countries offer parents a period of parental leave that can be taken by either parent or split between them. There is thus large variation in the precise design of these policies, both when it comes to length of leave and level of benefits when on leave. However, despite the wide implementation of parental leave policies and many studies evaluating parental leave reforms, relatively little is known about their overall effects. There is some evidence that introducing parental leave polices, if they are combined with job-protection, do increase female employment rates and have a long term positive impact on women's wages and earnings (Ruhm, 1998, Waldfogel, 1998). Other results suggest that adding a work-requirement to get full parental leave benefits increases mothers' labor force participation before and in-between births. However, a too high requirement (two years) seems not to have this effect (Stearns, 2016). These results suggest that parental leave systems might have positive effects on women's position in the labor market. However, it has also been suggested that there is a flip-side to the coin. Long periods of absence from the work force could have a negative impact on women's careers through human capital depreciation or signaling effects. Albrecht et al. (2003, 2014) remarks that women's prolonged time on leave due to generous parental leave policies might have created a "system-based" glass ceiling, in particular in Scandinavian countries, by generating negative expectations among employers about women's career commitment after having children. Government-sponsored leave taken mostly by mothers could also cement a traditional division of labor in the home and thereby worsen women's labor market outcomes. Studies from Sweden and Denmark have shown that much of the remaining gender gap in income and wages can be linked to the uneven division of family responsibilities between men and women (Angelov et al., 2016, Kleven et al., 2016). The evidence is particularly scarce when it comes to the role of the benefit level.

In this paper, I investigate the effect of the level of benefits on the utilization of parental leave, taking the Swedish system as my case study. Understanding the role of the benefit level could have wide policy implications. Like in any transfer program, theory predicts that the level of benefits, by changing the relative gain of working and the general appeal of the program, will affect the likelihood and length of take-up of parental leave provisions. Just like in other government-funded transfer systems, the reimbursement level changes the opportunity cost of working and thus is expected to affect participation and take-up rates of the program. Knowing how responsive parents are to the generosity of the parental leave programs has wide practical implications for the design of such policies around the world, and for the theoretical understanding of individual's sensitivity to incentives. Knowledge about the take-up elasticity of parental leave benefits could improve the theoretical models of how individuals respond to economic incentives in the tax and transfer system.

The limited previous literature on the impact of the benefit level indicates a positive relationship between the level of benefits and mothers' take-up, and a negative relationship between the level and mothers' employment. Kluve and Tamm (2013) and Bergemann and Riphahn (2010) both study the effect of a reform in Germany in 2007 where the system changed from a means-tested flat rate when on leave, to a system with wage replacement, which in practice increased the level for most women. The reform is found to have increased mothers' time on parental leave. Lapuerta et al. (2011) exploit variations in generosity in replacement levels across regions in Spain. She finds that larger provisions during parental leave is associated with the mother spending a longer time on leave and later return to work.

Systems with gender neutral leave policies, that are in place in many European countries, give parents an opportunity to have a more equal division of parental responsibilities. Such policies have been motivated as a way to promote gender equality both at home and in the labor market, since they allow men to be more involved in raising their children and women to spend more time on paid work. In a system where paid leave at home can be divided between parents at will, it is reasonable to think that the *relative* benefit levels of parents could affect their choice. If parents make their decision in part as an economic optimization, the parents' benefit levels could be important for their decision since they affect the relative costs of letting each parent stay at home from work. Changes in the benefit level could thus change the length of time that each parent spends at home. A shift in parents' division of time on leave, induced by a shift in relative benefit levels, might also affect parents' long term division of labor if it establishes other patterns of division of household chores and child care. If so, a change in division of parental responsibilities during the first years after a child's birth could have long term consequences for gender equality both in the home and in the labor market as mothers can spend more/less time on their careers.

However, it is hard to find empirical findings that confirm the hypothesis that changes in the parents' length and division of parental leave affect the parents' labor market outcomes in the long run. In fact, most previous research has been unable to find long-term effects on earnings due to changes in parental leave take up. For example, Karimi et al. (2012) investigate the effects of three parental leave reforms in Sweden and find that a 3-month extension increased fathers' and mothers' take-up of PL-days by the same magnitude. The modest effects on the parents' labor earnings correspond directly to loss of income during the extra days on leave, which indicates that there were no long-term effects on wages or career development.[1] Liu and Skans (2010) investigate the same reform and find no significant effect on mothers' earnings seven years after child birth. Two much studied reforms are the Swedish "daddy-month" reforms that earmarked 30 days of leave for each parent for children born in 1995 or later, and another 30 days for children born in 2002 or later. Ekberg et al. (2013) find that the first reform induced a 15 day increase in fathers' take-up of parental leave days and a 25 day decrease in mothers' take-up, but finds no short- or long-term effects on earnings or employment for mothers or fathers. [2] Karimi et al. (2012) find similar results for the parents' take-up and earnings. By contrast Johansson (2010), who use a fixed-effects model, finds that time on parental leave affects the individual's earnings for both mothers and fathers four years after the child was born. In addition, she finds cross-spousal effects in terms of the fathers' time on parental leave having a positive effect on the annual earnings of the mother. This result has not been confirmed in other studies. Most of this empirical evidence seems to suggest that small changes within an already generous parental leave system, such as the one in Sweden, only have minor labor supply effects in the short run and no long-run effects on earnings. However, longer extensions of leave have been found to have only small effects on long-run labor market outcomes. Lalive and Zweimüller (2009), for example, who investigate the effects of a reform in Austria 1990 that extended the parental leave from one to two years, found substantial delays in mothers' return to work, but only small negative effects on their employment and earnings three years after the child's birth, and no significant effects after ten years.

Since most studies find no long-run effects on the earnings of either parent, it seems that small changes in parental leave take-up do not affect the long-term earnings gap within the couple. This suggests that, in the long term, equality is unaffected. A similar result was found by Ekberg et al. (2013) who conclude that the 15-day increase in fathers' take-up of parental leave induced by introducing the first daddy-month, did not have any significant effect on the fathers' share of days spent at home with a sick child (the *temporary parental leave* days). Other studies using exogenous variation in the division of parental leave have not studied effects on the division of child care and household chores during later years. This is an area where future stud-

[1] The authors investigate the effect of a three months extension of the leave in 1989, and the introduction of the two daddy-months in 1995 and 2002

[2] Eriksson (2005) find a similar sized increase in fathers' take-up due to the second"daddy-month" reform. It has also been suggested that "quota-months" could have even larger long term effects through changed social norms. Dahl et al. (2012) show that the introduction of a quota month for fathers in Norway had both a direct effect on eligible fathers and an indirect peer-effect on the father's brothers and co-workers.

ies could contribute with more knowledge. Currently, however, there is little evidence that inducing a more equal division of parental leave results in improved gender equality in the couple later on. The available evidence suggests instead that the exact division of parental leave in term of number of days or weeks does not have any long-run effects on the within-couple earnings gap or division of child care.

The aim of this study is to investigate the impact of the level of parental leave benefits on the take-up and division of paid parental leave among parents. The Swedish parental leave system consists of 13 months of paid leave at 80 % of previous earnings and 3 additional months at a flat rate. Out of the 13 months at the earnings-related level, 2 months are earmarked to each parent and 9 months can be divided between the parents as they wish. Parents enjoy job protection during the first 18 months after childbirth and can thus spend more time on (unpaid) leave than what can be covered full-time by the parental leave benefit entitlements. However, I focus on paid parental leave, leaving unpaid leave outside the analysis. Since the benefit level depends on the parents' earnings before going on leave, it is endogenous to other parental characteristics. This creates an identification problem when wanting to estimate the causal effect of the benefit level on take-up of parental leave benefits. To overcome the endogeneity problem, I use a rule in the Swedish parental leave system known as the "speed premium" as an instrument for the level of benefits. The speed premium rule says that if your second child is born within 30 months (2.5 years) of the first, you can use the earnings level that you had before the first child was born to calculate the benefit level when on leave with the second child. In practice this means that parents can keep the same level of benefits for the second child as they had for the first child, even if they have reduced their earnings in-between births. Since many women reduce their hours of work after having children, this rule can be of great economic significance. Due to the speed premium, parents with less than 30 months of spacing between births have a higher expected benefit level than those with more than 30 months of spacing. Assuming that couples cannot exactly plan the time of conception or birth of their second child, the speed premium creates exogenous variation in parents' benefit levels just around the 30-month threshold. In my empirical analysis, I compare the behavior of parents who had their second child just before and just after the 30-month cutoff to study the effect of the difference in benefit levels using the speed premium as an instrument for the parents' benefit level. A fuzzy regression discontinuity strategy is used to estimate the causal effect of a shift in the benefit level on parents' take-up of parental leave benefits. Since mothers are affected to a much larger degree than fathers, this strategy allows me to study the cross-spousal effect of a change in the mother's benefits and take-up on the father's length of leave, and thus on the division of leave. The empirical estimations are performed on Swedish register data covering the years 1990-2012. The data links parents and children and contains detailed information on take-up of parental leave benefits as well as socio-economic variables.

The results indicate that the decisions of how many parental leave days to use, and how to divide the leave between parents, are very sensitive to the level of benefits. The speed premium is found to affect the benefit level of mothers, but not of fathers. A 5 SEK ($\approx$ \$0.54) increase in the mother's benefit level per day is found to increase her length of leave by about 2.6 days, and decrease the father's time on leave by almost as much; 1.9 days. Since the mothers' average benefit level is 506 SEK per day, and they take on average 268 days of parental leave, this response is equivalent to an elasticity of take-up duration of parental leave benefits of 1 for mothers. This parameter has, to the best of my knowledge, not been estimated before. The fathers' response in take-up, a 4 % decrease, is larger relatively speaking since fathers on average use 49.5 PL-days, but is only slightly smaller in absolute numbers of days. The mirror image response in fathers' take-up to a change in the mother's time on leave suggests that decision on the total time spent at home with the child is not very sensitive to financial incentives, but that the division of leave days between parents is highly sensitive. The fact that a changed benefit level of one parent (in this case the mother) has such a large effect on the division of leave suggests that the parents' *relative* levels of benefits is important for each parents' length of leave.

This paper adds to the small literature on how the benefit level affects take up of paid parental leave entitlements. It is one of few papers estimating the effect of the parental leave benefit level in a causal setting that exploits an exogenous source of variation. Compared to previous work, my paper contributes to the literature in several ways. While previous papers rely on a one-time reform or inter-regional variation, my strategy allows me to aggregate over many years, controlling for the impact of time period in which the child was born. Since the speed premium rule applies to everyone in the same way, I am able to control for variations between regional labor markets and individual characteristics of the parents. In addition, instead of using a sample of parents, my data allows me to study the impact of a change in the benefit level on the whole population of Swedish parents. This paper also contributes to the theoretical literature on how individuals and couples respond to changes in the tax and transfer system, as it provides empirical evidence on a part of this system where it has been lacking. Specifically, this paper presents an estimate of the elasticity of take-up duration of parental leave benefits. This study also contributes to the discussion of what determines the division of parental leave, a topic that has sometimes been intensely debated.

The remaining part of the paper is organized as follows. Section 3 gives a description of the parental leave system in Sweden and the speed premium rule. Section 4 provides the identification strategy and econometric method. In section 5 the data is described and some descriptive statistics are presented. Section 5 goes through some potential threats to identification. Section 6 presents a graphical analysis and results from the regression analysis. Sections 7 and 8 present robustness checks and placebo tests. Finally, section 7 provides a concluding discussion.

2 The Swedish parental leave system

The Swedish parental leave system was first introduced in 1974, replacing an earlier system of maternity leave. Even though, since the start, it has been possible for parents to divide the parental leave evenly, mothers have always taken the vast majority of parental leave days. The fathers' share has increased slowly from 0.5 % in 1974 to around 25 % in 2013. The system has been extended and changed through several reforms. In 1989, the parental leave was extended from 12 to 15 months. In 1995, 30 days were earmarked for each parent, the first so-called "daddy-month" reform. In 2002, another 30 quota days were added, extending the total leave to 16 months. Since then, the paid parental leave consists of 480 days of parental leave (16 months) for each child. The parental leave days can be divided between the parents any way they want, except for the quota days that are reserved for each parent. During 390 of the days (13 months) the reimbursement depends on the individual's earnings prior to leave taking. The reimbursement during these days is equal to nearly 80 % of the individual's *Qualifying Income (QI)*[3]. There is a maximum level of compensation, currently 944 SEK (≈ $106) per day, which is the level if one has a qualifying income that is at or exceeds an inflation-adjusted cap level.[4] However, many employers top up the level to 80, 90 or 100 % of the wage, also if the employees' earnings exceed the cap level, during the whole or parts of the time on leave.[5] The qualifying income is usually equal to the individual's labor earnings during the past 12 months.[6] To have a valid qualifying income the individual must have worked for at least 240 days (8 months) before the child is born. If the individual does not have a valid qualifying income, there is a minimum level of compensation per day, currently

[3]In Swedish: *Sjukpenninggrundande Inkomst (SGI)* (Sickness Benefit Qualifying Income). The compensation level is calculated by taking 80 % of the qualifying income times 0.97 divided by 365

[4]In US $ this is equivalent to $106 in benefits per day. The cap level is at 10 *price base amounts*, which was equal to 444,000 SEK per year in 2014 (≈ $50,000). Before the 1st of July 2006, the threshold was 7.5 price base amounts.

[5]The level of generosity from employers depends on the collective agreement, and has varied over time and sector. In general, the public sector, especially the state, has more generous agreements. The private sector commonly only covers parts of the time on leave, for example the first 30 days. There is currently no compiled information on the regulations in different sectors over time. For this reason, this component of the reimbursements when on parental leave is disregarded in the analysis.

[6]There are some exceptions from this time frame, for example if the individual has been on parental leave during parts of the last year the time frame is prolonged with the same number of days as the leave taken.

225 SEK (≈ \$25).[7] During the remaining 90 parental leave days (3 months), the individual gets a low fixed amount per day on leave, currently 180 SEK (≈ \$20) per day. The parental leave days with wage replacement, during which compensation depends on the individual's qualifying income, are hereafter referred to as *"QI-days"*.The remaining 90 days are referred to as *flat rate days*.

The parental leave system is very flexible from the parents' point of view. For example, the parent does not have to take a full parental leave day per calendar day but can use 0.75, 0.5, 0.25 or 0.12 parental leave days per calendar day. Thus the parental leave days can be smoothed out over a longer time period. Since parents enjoy 18 months of job protection after the birth of a child, this gives them the opportunity to be on parental leave significantly longer than the 16 months that constitute the maximum time if one parental leave day is used per calendar day. The parental leave days can be used at any time until the child turns eight.[8]

During the child's first 12 months, the qualifying income is protected, i.e. does not decrease even if the parent does not work and stays at home without any other type of benefits. After the child turns one, the parents need to take up as much parental leave as they reduce their hours of work in order to keep their qualifying income. For example, if the parent usually works five days a week, the qualifying income level is protected if he/she takes five full parental leave days a week. Since other benefits, such as sick leave benefits, also depend on the qualifying income, parents have great incentives to protect it.

Besides the 480 parental leave days described above, the father can take 10 days of leave that can be used during the first 60 days after the child's birth. These 10 days are often referred to as the "daddy-days", and for convenience I also refer to them in the same way in this paper.[9] Despite the lengthy parental leave utilized by most parents in Sweden, most mothers do go back to work after their parental leave and few stay at home full time to raise the children. One reason for this is that Swedish municipalities offer high quality, heavily subsidized childcare from an early age.[10]

2.1 The speed premium

The speed premium rule stipulates that if a couple has another child within 30 months (2.5 years) of the previous child, the parents are entitled to use the same qualifying income that they had when the first child was born when calculating the level of benefits for the second child. Parents are also eligible in

[7]The minimum level was 60 SEK/day until 2001, 120 in 2002, 150 in 2003, and 180 in 2004-2012.

[8]For children born in 2014 or later, the QI-days can only be used until the child turns three.

[9]The "daddy-days" can also be utilized by a friend or relative of the woman who gave birth.

[10]Parents can enroll their child in childcare from the day the child turns one and the municipalities are required by law to offer a place at a daycare center "without delay", which is generally interpreted as within three months.

cases where the actual birth date occurs later than 30 months but the planned date of birth was within the 30-months time limit. Since many parents (mostly mothers) reduce their earnings after having children, they also reduce their qualifying income. The speed premium rule can thus be of economic significance for these parents who would otherwise be entitled to lower parental leave benefits when on leave with their second child.

There are at least two groups of parents who are in practice not affected by this rule. The first are parents whose qualifying income stays the same or increases in-between births. These are parents who go back to work at least a year before having another child and continue to have an income that is at least as high as before the first child was born. These parents will be entitled to a benefit level with the second child that is at least as high as when they were on leave with the first child. Thus, even if they fulfill the speed premium requirement that their children are born less than 30 months apart, this rule is of no financial significance. Another group that is in practice not affected by the rule are parents who did not have a valid qualifying income, or who had a very low one, before having their first child, and who were therefore only entitled to the minimum level of benefits for the first child. Since these parents could not get a lower benefit level even if they reduced their earnings further, eligibility for the speed premium does not lead to a higher benefit level for them.

The speed premium rule was implemented in the 1970s with a threshold of 12, and later 15, months. Between 1980 and 1985 the threshold was 24 months. Since January 1st, 1986 the 30-month threshold has been in place. The prolonged window from 24 to 30 months in 1986 was part of a larger reform with the intention of improving the financial situation for families with small children (Swedish Government, 1984). Previous studies indicate that the speed premium reduced spacing between the first and second child among Swedish parents (Hoem, 1993). Figure 1, constructed using the Swedish register data used in this paper (described in section 5), shows a clear difference in the density graph for spacing between the first and second child during the early to middle 1980s (dotted line) and the late 1980s (solid line) after the new speed premium rule had been implemented. In the pre-reform period, 1983-1985, only 36 % of couples had their second child within 30 months, and the average number of months between the first and second birth was 39 months. In the years following the reform, 1987-1989, the share of couples who had their second child within 30 months increased to 48 %, and the average number of months between births was 35 months.[11] These numbers and the graphical evidence suggest that when the 24-month rule was in place, many parents did

[11] These numbers refer to couples who had their first and second child together, where the second child was born during the years indicated, who lived together at the time of both births, who did not have twins at the first or second birth and who had both children within 130 months.

not manage to utilize the speed premium. Under the new regime, parents were increasingly able to utilize the premium.

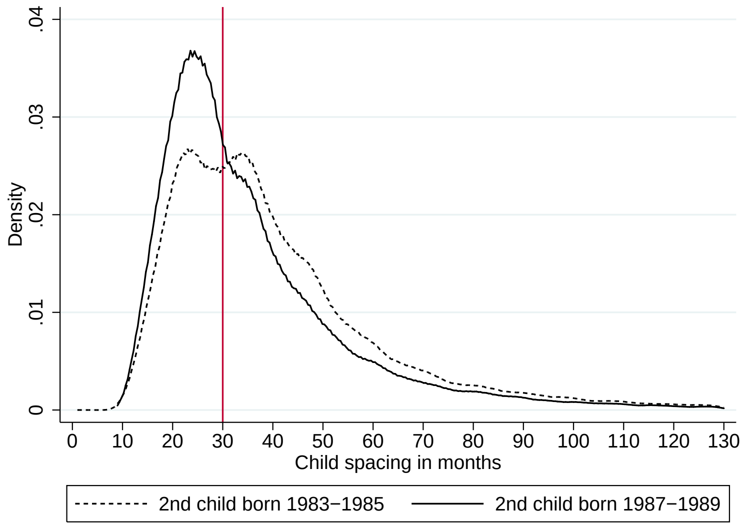

Figure 1. Spacing (in months) between first and second child's birth among families where the second child was born in 1983-1985 and 1987-1989 respectively. The graphs reveal a change in behavior after the speed premium rule was implemented in 1986. Kernel density functions calculated using Swedish register data.

3 Identification strategy

The aim of this study is to identify the causal effect of a shift in the parental leave benefit level on parents' utilization of parental leave. To this end I exploit the variation in benefit level that arises around the 30-month threshold due to the "speed premium" rule. More specifically, I use this rule as an instrument in a fuzzy regression discontinuity setting where spacing between children is used as assignment variable. Identification thus rests on the assumption that couples cannot perfectly manipulate the running variable, i.e. how long after the first child's birth their second child is born.[12] In other words, for the speed premium to be a valid instrument, there must be a random component in the determination of the precise spacing between children in the close neighborhood of the 30-month threshold.

Couples are eligible for the speed premium if the actual or expected birth date is within the 30-month threshold. The expected due date is determined

[12]For an extensive discussion on this and other identifying assumptions in RD design see Lee and Lemieux, 2010.

by the date of conception. There are therefore two sources of variation in the second child's birthdate that I argue create random assignment of couples to either side of the threshold: variation in the date of conception and, given the date of conception, variation in the actual birth date. Both types of variation are relevant and are discussed in turn below.

3.1 Variation in the date of conception

Biologically, a woman in fertile age can get pregnant about one day every month.[13] Information about when this day occurs can be predicted based on the menstrual cycle, but cannot be controlled. Consider a couple that has decided to have a second child and who would like to try to become eligible for the speed premium. As long as more than nine months remain before the 30-month cutoff, they have one chance every month. Any given month, the chance to conceive is between 5 and 40 %, depending on the couple's characteristics.[14] Considering that it can take several months (or years) to successfully get pregnant, most couples who would like to be eligible for the speed premium probably start trying to conceive well in advance of the 30-month threshold, rather than close to nine months before. Since couples with at least one child have an idea from previous experience about how long it is likely to take them to conceive, they can use this as a prediction when planning when to start trying to conceive of the second child. Thus, around nine months before the threshold, many couples who would like to be eligible for the speed premium will already have conceived the second child.

However, some couples who would have liked to use the speed premium will fail to get pregnant in time in order to be eligible. If we make the assumption that such couples continue to try to conceive, whether a couple's expected due date will be in the month just before the threshold or in the month just after, is determined by chance. Thus, under the assumption that a couple's success in conceiving in month x or in month x + 1 is determined by chance, couples that conceive less than 10 months, but no more than 8 months, before the threshold can be considered as randomly assigned to treatment.

Arguably, when considering wider bandwidths, couples with different characteristics could sort into a specific period of spacing according to their preferences. For my identification to be valid, however, I do not need to assume that the time of conception is determined at random along the whole range of potential outcomes. Instead I only have to assume that treatment is randomly

[13] I.e. when she's ovulating: On average, once every 28th day depending on the length of her menstrual cycle

[14] For example, the woman's age, weight, and smoking habits. The probability to conceive falls slowly between age 20 and 30, and then decreases more rapidly. At age 40 the chance of conceiving in a particular month is about 5%. There is some evidence that the man's age can also influence the chance of conceiving. Sources: Laufer et al. (2004), Dunson et al. (2002), Menken et al. (1986), e-mail correspondence with the National Board of Health and Welfare.

Table 1. *Distribution of actual births around the expected due date (percent).*

Week of pregnancy	Percent born
<= 37	4,70
38	11,70
39 + 0	2,60
39 + 1	2,90
39 + 2	3,20
39 + 3	3,60
39 + 4	3,80
39 + 5	4,00
39 + 6	4,30
40 + 0	4,60
40 + 1	4,70
40 + 2	4,70
40 + 3	4,70
40 + 4	4,50
40 + 5	4,40
40 + 6	4,10
41	20,20
>= 42	7,30

Note: Percent of children born in Sweden, 2010–2012, at different gestational ages (pregnancy week and day). The expected due date is at 40 week and zero days (indicated as 40 + 0 in the table). Calculations made by The National Board of Health and Welfare using information from the Medical Birth Register. Information in table was first published by TT News Agency and SVT (SVT, 2014).

assigned among couples within one month of the threshold due to randomness in their date of conception. To test this (weaker) assumption, one can investigate if there is a downward shift in the frequency of births just after the threshold, or if there are any discontinuous jumps in parental characteristics at the threshold. If so, that would indicate that some couples change their mind about wanting to conceive when they realize that they have missed their chance to use the speed premium. In section 5 below I perform these tests and show that there is no evidence of sorting at the threshold.

3.2 Variation in the actual birth day of the second child

The child's expected due date is calculated based on the mother's last menstrual period and measurements of the fetus made during an ultrasound in pregnancy week 17 or 18. The expected length of a pregnancy is 40 weeks (counted from the woman's last period). However, only about 5 % of children are born at exactly 40 weeks + 0 days. Detailed statistics on the distribution of actual births around the expected birth date are presented in Table 1. 25 % of children are born in the week before their expected due date (week 39 of the pregnancy), 32 % are born during week 40, and 28% are born in week 41 or later. About 16% are born before week 39, more than a week before the expected date.[15] These statistics include all births, not just second births for which the pattern could be different. Still, they suggest that the distribution of actual births around the expected due date is wide, with more couples exceeding their expected date than preceding it. This means that a large (about 50 % at the threshold) but decreasing proportion of the couples with an observed spacing between children of more than 30 months, are in fact eligible for the speed premium since their expected due date was before the threshold.[16] Likewise, almost half of the couples with observed spacing of just less than 30 months are eligible due to an early birth (rather than due to their expected due date). Because of the wide dispersion of actual births around an expected due date, the proportion of couples who are eligible for the speed premium due to their expected due date decreases continuously to the right of the threshold. About 27 % of couples whose children are born in the first week after the 30-month threshold, and about 20 % in the week after that, are eligible because of their expected due date. The proportion of eligible couples is thus 100% as long as less than 30 months have passed on the child's actual birthdate, and less than 100%, starting at above 50% and decreasing continuously, among couples whose child was born beyond the threshold.

Using the child's actual birthdate, couples are randomly assigned to treatment (eligibility) under the assumption that parents do not strategically manipulate the child's birthdate. Earlier studies have shown that a strong economic incentive can induce parents to have an early birth or to delay a birth. The empirical evidence presented in Neugart and Ohlsson (2013) suggests that it is possible to delay a birth by about one week. As for inducing an early birth, Borra et al. (2015) show evidence of a shift in birth timing by between one and two weeks in response to the withdrawal of a 2,500 € baby-bonus in Spain.

In Sweden, the proportion of children born with Caesarean section (C-section) increased from 10 to 16 percent between 1994 and 2009. About 50 % of the C-sections are emergency C-sections. A planned C-section can be granted due to medical concerns or psychological reasons such as a strong fear of going through labor. However, a mere wish for the child to be born

[15] These calculations were made by The National Board of Health and Welfare using information from the Medical Birth Register on all births in Sweden during 2010-2012.

[16] For example; for about 55 % of those whose expected due date is the day before the 30-month threshold, the child's actual birth date is beyond the threshold. Among the children born on the first day just beyond the threshold, more than 50 % are eligible due to an earlier expected date, since more people exceed their expected date than those who are born before or on that date.

on a specific date due to practical reasons is not sufficient grounds for the parents to have a planned C-section according to official recommendations for the Swedish health care system (SOC, 2011). The proportion of vaginal births that were medically induced increased from 8 to 12 percent during the same period. The most common reason is that more than 41 pregnancy weeks have passed at which point the birth will be medically induced (SOC, 2009). A likely reason for the increase is that medical and age characteristics of mothers, which affect the risk of a prolonged pregnancy, have changed. A birth is only induced due to medical reasons. In section 5, the risk of strategic timing of births around the threshold is investigated and no evidence of it is found.

3.3 Implications for identification

Assuming that the distribution of actual births around an expected due date is the same on both sides of the threshold, the proportion of couples with an expected birthdate before the threshold decreases *continuously* over the 30-month threshold, when observing actual births over spacing. Likewise, the proportion of couples with an expected birth date to the right of the threshold increases *continuously*. Thus, the variation in expected birth date of the second child does not give rise to a discontinuous jump in the proportion of eligible couples at the threshold. The discontinuous jump in the proportion of eligible couples arises solely from couples whose expected due date was beyond the threshold, but whose child was born before it due to an early birth. Thus, under the assumption that couples cannot manipulate the actual timing of the second child's birth, the variation in actual birth dates of children creates random assignment to treatment status among couples around the threshold.[17] As explained in detail in section 5 below, the data available in this study only contains information about children's year and month of birth, not the exact date. Using this information only a crude measurement of the number of months between children's births can be calculated. However, as discussed above, couples who are further away from the threshold can also be considered randomly assigned to a specific side of the threshold due to randomness in the date of conception. For this reason, a comparison between observations in larger bins on either side of the threshold is a valid method to causally estimate the effect of a randomly assigned eligibility status between otherwise "similar" couples.

When taking the average over time intervals on each side of the threshold, the proportion of individuals who are eligible for the speed premium due to their expected due date will no longer change continuously, but rather in discontinuous steps when moving between bins. If the bin size is large enough, there will be a particularly large shift in the proportion of observations with an

[17]This assumption is discussed more in second 5.2 below where evidence of its reliability is presented.

expected due date before the threshold when moving from the bin to the left of the threshold to the bin on the right. This line of argument is relevant for identification in this study since information on the actual birth date is not in the data, but the child's birth month and year is. The baseline analysis uses a proxy variable for the child's birthdate using detailed information on the dates of parental leave take-up (described in detail in section 4.2). However, the smallest bin size possible is one day.[18] In a complementary analysis, the monthly data is used instead.

3.4 Fuzzy regression discontinuity design

As explained above, couples both before and after the 30-month threshold can be eligible to the speed premium depending on the child's expected due date. Also, eligibility status is only relevant for parents who have a lower qualifying income at the time of the second child's birth than they had when the first child was born. For parents who increase their earnings between births, the speed premium rule does not have any economic significance, since it does not affect their benefit level. Hence, spacing between children only changes the likelihood of eligibility, but does not sharply determine it. Thus, when exploiting the speed premium rule as a source of variation in benefit level, a fuzzy regression discontinuity design is the appropriate strategy.

In fuzzy RD, estimation is made in two steps that can be described as a two-equation system. The first stage (1) and second stage (2) equations can be written as:

$$y_{1i} = \alpha_1 + \lambda 1[s_j < c] + 1[s_j < c]g_1(s_j - c) + 1[s_j \geq c]g_2(s_j - c) + \gamma_1 X_i + \varepsilon_{1i} \quad (1)$$

$$y_{2i} = \alpha_2 + \beta y_{1i} + 1[s_j < c]f_1(s_j - c) + 1[s_j \geq c]f_2(s_j - c) + \gamma_2 X_i + \varepsilon_{2i} \quad (2)$$

where c is the cutoff point at 30 months of spacing and s_j is the spacing between the first and second child of couple j. Further, g_1, g_2, f_1 and f_2 are unknown functional forms that describe the trends in the outcome variables in each equation. X_i is a vector of pre-determined covariates and ε_{1i} and ε_{2i} are error terms. The outcome variables are, in the first stage (1), individual i's level of parental leave benefits, and in the second stage (2), individual i's take up of parental leave days. In the first stage, any discontinuous jump in the parental leave benefit level at the threshold is estimated. In the second stage, the predicted values from the first stage are used to estimate the effect of a one unit change in the benefit level on the utilization of parental leave days.

[18] If very detailed information on time of birth was available, for example hour and minute, an even more thorough analysis could be conducted.

The λ parameter in equation 1 can be interpreted as an estimate of the jump in level of benefits at the threshold. β is the parameter of most interest as it gives an estimate of the causal effect of a one unit (1 SEK) change in the parental leave benefit level on the utilization of parental leave days. X_i is included to control for any sorting on covariates across child spacing that may affect the trends estimated by the functional form of g_1, g_2, f_1 and f_2. For this reason, and to increase precision of the β estimate, X_i is included in the model. Under the assumption of randomization in treatment (eligibility to the speed premium) around the threshold, including the covariate vector X_i should not affect the magnitude of the β estimator directly. However, including the X_i vector could affect the estimated trends on each side of the threshold. Thus, it could also affect the gradients and the end points of the trends and thereby affects the estimate of β.

3.5 Choice of functional forms and bandwidth

In the RD literature there is a discussion on how to choose what functional forms (i.e., g_1, g_2, f_1 and f_2 in equations 1 and 2) and bandwidth to use when implementing the method. One approach is the global parametric method where all data and flexible functional forms such as higher order polynomials are used. Another commonly implemented approach is the local non-parametric method, in which a smaller bandwidth "near" the cutoff is chosen based on some criteria and a less flexible functional form, typically linear, is used. The motivation for choosing linear trends is that it is realistic to assume a linear functional form near the cutoff (Hahn et al., 2001). However, higher order polynomials can also be included, a method sometimes referred to as local polynomial regression (Porter, 2003).

A drawback of the global parametric approach is that it is hard to know which functional form correctly describes the data at all ranges of the running variable. As demonstrated by Gelman and Imbens (2014), RD-estimates tend to be sensitive to the precise form chosen, which introduces a risk of bias. In light of this critique, I use the non-parametric approach, which implies that a bandwidth has to be chosen. Several methods for choosing the bandwidth have been proposed. Two of the most prominent are Imbens and Kalyanaraman (2011) and Calonico et al. (2014), who suggest data-driven methods for the choice of bandwidth (hereafter referred to as the IK and CCT optimal bandwidths). However, both of these methods were developed with a sharp RD design in mind: they derive an optimal bandwidth of the running variable based on *one* outcome variable, not *two* as is the case in a fuzzy RD setting (the first and second stage/reduced form outcome variable). Rather than choosing one of these methods, when in fact there is no consensus in the literature on which one to prefer, I present estimates for a large range of bandwidths, including the

CCT and IK suggestions.[19] To apply equal transparency when it comes to the choice of functional form, I use a linear functional form in the main specification, but include higher order polynomials as a robustness check.[20] To give more weight to observations near the cutoff, triangular weights are used.

4 Data

The data used in this paper consists of population-wide register data covering all residents of Sweden aged 16-65 during the period 1985 - 2010. The dataset contains information on marital status, cohabitation (provided the couple has children together), number of children in the household, and other socioeconomic variables such as labor income and social transfers. The data links parents and children born up until 2009, and there is information on the children's year and month of birth, but not the exact birthdate.

Data on the take-up of parental leave is available for the years 1994-2012. The information is very detailed in the sense that it is possible to observe all periods (exact calendar days) when the individual took out parental benefits, as well as the precise amount of benefits received. The parent can use parental leave benefits "part time", using less than one full parental leave day (i.e. one net days) for each calendar day. However, for each period on leave, the total take-up of net days of leave is also specified.

Using this information, measures of the length of leave of each parent for each child are constructed. The length of leave is measured in three ways: first, in terms of *number of net days with benefits at the "qualifying income" level (QI-days)*; second, as *the total take up of net days also including flat rate days*; and third, in terms of *the number of calendar days with any benefits*. All leave periods that started within the first two years of the child's birth are included in these measures of the length of leave.

Only accounting for paid parental leave is likely to underestimate the actual time spent at home since parents can mix paid leave with unpaid leave. They are especially likely to do so during the child's first year when the qualifying income level is protected. Since information on hours of work per calendar day is not available it is hard to know exactly how long the parents actually stay home from work. Thus, any measurement of take-up of parental leave days should be viewed as a proxy for time spent at home with the child.

The measure of benefits per net day on parental leave is constructed using the individual's first period on leave.

[19]The CCT and IK optimal bandwidths are calculated using the program RD-Robust which has been developed by Calonico et al. (2014) and is described in detail in Calonico et al. (2014).
[20]As suggested by for example Jacob et al. (2012)

4.1 Sample restrictions

From the data, all couples who had their first and second child together during the years 1994 - 2009 are sampled. The sample is restricted to couples that were married or cohabiting at the time of both children's births and where none of the partners had children before. Couples who had multiple births are dropped. Out of these, all couples are kept where both partners had positive earnings the year before the first child was born and whose parental leave benefit levels per net day were above the minimum level when they were on parental leave with the first child. I do this to better capture the group of parents that are more likely to be affected in a significant way financially by the speed premium. Parents who get the minimum level of benefits with the first child cannot benefit from the speed premium rule since they would get the same level of benefit with their next child even if they reduced their earnings between children. Parents who were working the year before their first child was born and whose qualifying income at the time of the first child's birth was high enough for their benefit level to be above the minimum are more likely to be at least somewhat established in the labor market. This implies that they have an actual choice of going *back* to work after staying on leave. The first three rows of table 2 describe how the number of observations in the sample changes when imposing these restrictions.

4.2 Measuring spacing between children

The data used for this study contain information about children's birth year and birth month, but not the exact birthdate. Spacing between the first and second child is therefore calculated using only children's year and month of birth. Depending on the exact day of birth this measurement might indicate (almost) one month too much or one month too little between siblings. Suppose, for example, that the first child is born late in the month, say the 25th, and the second child is born early, say the 1st. Suppose further that the second child is born in the 30th calendar month after the first child. Then the actual spacing between the children is 29 months and 5 or 6 days. However, using only the children's birth year and birth month to calculate spacing gives a measurement that is equal to 30. In fact, in all cases where the second child is born earlier in the month than the first, this measurement of spacing is one unit too large. This will happen in about 50 % of all cases assuming that the birthdays of the first and second child are randomly distributed over the months when they were born. For identification, it is essential to identify all observations as either within the 30-month timeframe or beyond it to be able to estimate a discontinuity at the threshold. Hence such a crude measure of spacing is problematic. Using this measurement of spacing, the jump in benefit level *at the threshold* cannot be estimated.

However, as discussed in section 4, the causal effect of a shift in the benefit level can also be estimated by comparing observations within wider bins of spacing, under the condition that random assignment to an exact value of spacing can be assumed over a wider range of spacing. Assuming that there is a random component in the assignment of observations to either spacing equal to 30 or 31 (using the crude spacing-measurement), a comparison between these two bins of observations can be used to estimate the causal effect of a shift in the benefit level. For couples where my measurement of spacing is equal to 30, about 50 % are eligible because of the second child's expected due date, and a smaller proportion are eligible because of an early birth of their second child. For observations where my measurement of spacing is equal to 31, some proportion of the observations are eligible because the expected due date of their second child was within the 30-month threshold, although the actual birth date of the child came later. Hence, a larger proportion of the observations with spacing equal to 30, than among those with spacing equal to 31, are eligible for the speed premium. The speed premium rule thus exogenously creates a discontinuous jump in the expected benefit level when moving between these two bins. This drop in the share of eligible couples can be exploited to estimate the effect of a change in the benefit level.

Proxy variable for spacing between children.
Although causal inference estimating the effect of a higher benefit level on the utilization of parental leave is possible using the crude measurement of spacing described above, more detailed information on the precise spacing between births would improve identification for several reasons. First, information on the exact birth date of children would improve estimation of the trends included on each side of the threshold so that they better reflect the true functional forms in the data. Second, knowing the precise distribution of observations around the threshold would make it possible to exploit the discontinuity in probability of being eligible *at the threshold*. The observations that give rise to this discontinuity are couples with an expected due date beyond the threshold whose second child had an early birth. Thus, the assumption of randomness in date of conception is no longer needed: Identification rests only on the assumption that couples cannot exactly control the actual birth date of their second child. Third, with a measurement of spacing based on the date of birth, estimation can be made comparing observations in a closer proximity to the threshold than what is otherwise possible. The advantage of this is that the assumption of random assignment to a specific birthdate of the second child only needs to hold in a close neighborhood of the threshold, instead of over a wider time span.

To be able to perform estimations *at the threshold*, a proxy variable for spacing between children is created. First, proxy variables for the first and second child's birth days are created, and then the time between these days is calculated. This gives a measure of spacing in days, rather than in months.

The proxy variable for spacing measured in days is created using information about the father's take-up of his first daddy-day, and the mother's take-up of her first parental leave days. The reason for using these variables is that fathers typically use the daddy-days in connection with the child's birth, while using the PL-days later. Mothers, on the other hand, typically go on parental leave in close connection with the child's birth. Fathers can use the ten temporary parental leave days, popularly called "daddy-days", at any time from the child's birthday up until 60 days later. Since fathers cannot use the daddy-days before the child is born, the first daddy-day is the best proxy for the child's birth day.

In cases where the father used his first "daddy-day" in the same month as the child was born, this day is used as a proxy for the child's birthdate. In about 54% of the cases the father used his first daddy-day during the child's birth month for both children. For these observations, only information about the father's take-up of daddy-days is used to create an alternative measurement of spacing. However, this sample might not be representative for all couples since fathers in this sample chose to spend more time with the child early on.

For children where the father did not use his first daddy-day during the child's birth month, the mother's first day on parental leave is instead used as a proxy for the child's birthday, but only if this day occurred in the same month as the child was born. Mothers can use their parental leave days starting 60 days before the child's expected due date. However, by only using this information if the day was taken in the same month as the child was born, the mother's first day on leave is a reasonable guess as to when the child's birthday occurred. By also using information on mother's take-up of PL-days, more observations can be included. This way, the proxy variable for spacing covers about 75 % of the population instead of only 54 %. This improves the external validity of estimations made using the sample. For this reason this proxy for spacing is used in the main specification. The last two rows of table 2 describe the number of observations left in the sample when using this proxy variable and when using the spacing measurement based only on the father's utilization of daddy-days.

Defining treatment when using different measurements for spacing

The two different approaches to estimation described above, using the crude measurement of spacing or the proxy variable, lead to different interpretations of the estimates.

First, using the two different variables has different implications for the external validity of the estimates: The monthly spacing variable covers the entire population of parents, whereas the proxy variable only covers a nonrandom selection of parents who used either the first daddy-day or the mother's first PL-day during the child's birth month.

Second, there is a difference in what "treatment" entails when using the two different measurements of spacing as a running variable. Using the proxy

Table 2. *Table of sample restrictions.*

Sample restriction	No. of obs.
Couples who had their first and second child during the years 1994 - 2009 and were living together at the time of both births. Multiple births excluded.	323,748
Both partners had positive earnings the year before the first child was born.	272,085
Both parents had a parental leave benefit above the minimum, level when on parental leave with the first child.	176,291
Using information about the mother's first PL-day and the father's first "daddy-day" with each child to calculate spacing between children.	133,075
Using only information about the father's first "daddy-day" with each child to calculate spacing between children.	94,554

Note: The table shows the number of observations when applying different sample restrictions. The two last rows show the number of observations included when using the alternative measures for spacing between children: Using only information about the father's utilization of "daddy-days" (last row) or, in addition, also using information about the mother's first parental leave day (second to last row).

variable, estimation is made at the threshold exploiting the variation in actual due date around the threshold among couples whose expected due date was beyond it. Under the assumption that couples cannot manipulate the child's actual birthdate, the observations that might give rise to a jump in benefit level at the threshold (i.e. the compliers) are those who had an expected birthdate that was beyond the threshold but who became eligible because of an early birth of the second child.

The proportion of individuals who are eligible because their expected due date was before the threshold decreases continuously over the threshold. Thus, at least in the (very) close proximity of the threshold, the expected due date of the second child should be balanced across spacing. (However, as soon as any range larger than at most one day on either side of the threshold is considered, this will no longer be true.) This means that, when estimating the discontinuity at the threshold, the eligibility status of the compliers was unexpected. In this case, treatment is thus to become eligible for a higher benefit level than anticipated.

On the other hand, when using the crude monthly measurement of spacing, estimations are made comparing all observations in month 30 to those in month 31. The main difference between these bins of observations is that a significantly larger proportion of those in month 30 had an expected due date within the 30-month time frame: an expected due date that they knew would make them eligible. Thus, when using the monthly variable, most of those who are treated in month 30 have known during the pregnancy that they were going to be eligible for the speed premium. They have thus been able to take this into account when planning for example how to utilize the parental leave with the second child, and their labor supply during the pregnancy.

When using the proxy variable, the eligibility status for the compliers is a surprise to them. Thus it could not have affected their planning on how to use the parental leave with the second child or behavior during the pregnancy. When estimating the response in behavior in this group, it should thus be interpreted as a direct response to an unexpected shift in benefits.

When using the monthly spacing variable, on the other hand, any estimate of a behavioral change should be interpreted as a combination of a response to unexpected eligibility for some couples in month 30 and (predominantly) a response to knowing with certainty that you will be eligible.

4.3 Utilization of parental leave

Figures A1a - A1d show the density of observations over parental leave benefit levels of mothers and fathers with the first and second child. For both mothers and fathers there is bunching of individuals at levels equal to the maximum level of benefits in different years. The maximum level that an individual could be entitled to increased significantly on the 1st of July 2006. This explains why there is a larger density at high levels of benefits for the second child. More fathers than mothers receive the maximum amount of benefits (since they have earnings that exceed the cap level). 12.5% of mothers and 27.5% of fathers receive the highest level of benefit with their first child, and 11.5% of mothers and 27% of fathers receive the highest level with the second child. The average levels of benefit is, for mothers, 462.6 SEK with the first child, and 503.3 SEK with the second, and for fathers, 541.7 SEK with the first child and 600.3 SEK with the second.

Figures A2a - A2d show the distribution of observations over the total number of calendar days with parental leave benefits (both at wage replacement and the flat rate days) for mothers and fathers with their first and second child.[21] The histograms reveal bunching at 330 and 420 days for mothers and at 0 and 30 days for fathers. From 1995 and onwards, 330 has been the maximum number of QI-days that a parent could use. (Before 1995 the maximum number of days was 360). Adding the 90 days at the flat rate gives 420 days. The first quota month was also introduced in 1995, which meant that 30 days are reserved for each parent. The other quota month (30 days) that was introduced in 2002 does not seem to have resulted in bunching at 60 days. The percentage of fathers who did not use any PL-days was 13.8% with the first child and 14.8% with the second child. The average number of days with PL-benefits is, for mothers, 311.4 days with the first child and 303 days with

[21] These measurements do not include the 10 daddy-days.

the second, and for fathers, 58.5 days with the first child and 55.5 days with the second.

5 Potential threats to identification

5.1 Misclassification due to measurement errors

Because the proxy-variable is constructed using information on the parents' use of daddy-days and PL-days, patterns in how the parents utilize these days could induce measurement errors in the proxy-variable for spacing. For example, if parents tend to utilize their days with their first and second child in different ways, systematic measurement errors in spacing are more likely to occur. As a result, observations could be mistakenly classified as lying above (below) the threshold when they should in fact be classified as lying below (above) the threshold. Such misclassification would blur out any discontinuous jumps that are actually present at the threshold, thereby decreasing or erasing an estimate of such jumps.

Since the actual date of birth is not available I cannot investigate the parents' utilization patterns in terms of distance to the child's actual birthday. Figures A3a - A3d show the distribution of when in a month the father used his first daddy-days with the first and second child. Figures A3a and A3c show the distribution for the whole sample. In figure A3b and A3d the sample restriction that the father's first daddy-days should be in the child's birth month is imposed. The histograms for the full sample reveal no obvious pattern other than a uniform distribution of when during a month fathers start to use their daddy-days. The frequency for the 31st is lower because not all months have a 31st day. When imposing the sample restriction, there is a drop in frequency early in the month. This is probably because for children born late in a month, many fathers do not take the first daddy-day until for example the first day in the next calendar month.[22] Most important for identification is that the pattern of when fathers utilize their first daddy-days seems similar for the first and second child. Using the distance between the first daddy-day with the first and second child will thus not systematically over- or underestimate spacing between children, but could still place observations on the wrong side of the threshold due to measurement errors.

Figures A4a - A4d show the distribution the mothers' first day on parental leave with the first and second child over the month. Figures A4a and A4c show the distribution for the whole sample and figure A4b and A4d show the distribution when imposing the restriction that the mother's first PL-day

[22]The histogram indicates that fathers in general tend to use their first daddy-day up to a week after the child is born. The frequency of fathers taking their first daddy-day on the first day of the month is only about one third of the number on days beyond the first week of the month.

should have been taken during the child's birth month. These histograms indicate a clear pattern for when mothers typically go on parental leave, namely the first day in a calendar month. This pattern remains but becomes weaker when imposing the restriction on mother's take up timing: In the restricted sample, about 11 % of mothers go on leave on the first day in the months, compared to around 4 % on any other day. The only other day that stands out is the 21st.[23] As with the daddy-days, patterns in take-up of PL-days only lead to systematic under- or overestimation of spacing if mothers act differently for their first and second child. This does not seem to be the case. However, at least in some cases when the mother goes on leave on the first of the month, this day is a bad proxy for when the child is born. About 25 % of the women who went on leave on the first day of the month with the first child also did so with the second child.[24] For these (relatively few) couples, using this day to calculate spacing between children gives the same measurement as the monthly spacing variable.

69% of fathers used their first daddy-day during the child's birth month with the first child, 65% with the second, and 54% with both children. Among couples where the father used his daddy-days later or not at all, 56% of mothers used their first PL-day during the child's birth month with the first child, and 57% did so with the second child.

Among 71 % of the observations in the sample when using the proxy variable, only information about the father's daddy-days is used to construct the proxy. For about 10%, only information about the mother's PL-days is used, and for the remaining 19 %, a combination of the two is used to calculate spacing. Among these 19 %, the proportion of couples where the first daddy-day is used for the first child and the mother's first PL-day in used for the second child, and vice versa, are about the same. Thus constructing the proxy variable in this way should not systematically over- or underestimate spacing between children. There will however be measurement errors that could in some cases be as large as (almost) two months. This type of measurement-error should create noise, but not bias, in the estimations.

5.2 Strategic timing of births

In section 4 I discussed the identifying assumptions that must hold to be able to assume random assignment of treatment status for observations close to the threshold. In this section I discuss what requirements need to be fulfilled in order for the date of conception and the child's actual birth date to credibly be assumed to be randomly assigned to couples. When it comes to the date

[23]This is possibly because the payments of PL-benefits are made late each month for leave days taken between the 21st of the last month until the 20th of the current month.

[24]Taking into account that some children are actually born the first day of the month, about 2 % of all mothers seem to always go on parental leave on that day regardless of which day the child is born.

of conception (which determines the expected due date) I do not need to assume that this is randomly assigned along the whole range of spacing, but only among couples within one month of the threshold. This assumption requires that couples who have just failed to conceive in time for the expected due date to be within the 30-month time frame do not change their mind about wanting to get pregnant. If this type of couple (or a selected sample of them) decides not to try to conceive in the next month, one would expect a discontinuous shift downwards in the frequency of births at the threshold. An additional indication that a selected sample of couples opt out of having another child because they have missed their chance to be eligible for the speed premium, would be if there were any discontinuous jumps in parental characteristics at the threshold.

When it comes to the actual birthdate, direct manipulation of the birthdate constitutes a potential threat to identification. Some couples in my sample with an expected due date close to but beyond the threshold have a strong economic incentive to try to have an early birth because of the speed premium. If they can somehow schedule a C-section or induce a vaginal birth before the threshold, then the assumption that the child's actual birthdate is randomly assigned is violated. Whether this is the case or not can again be tested by studying the distribution of births just around the threshold. If couples are able to manipulate their child's actual birthdate, one would expect a heap just before the threshold when plotting the distribution of births around it, and a sharp drop in frequency just after. Such manipulation could also create discontinuities in parental characteristics at the threshold if only a selected sample of parents choose to manipulate the birthdate. Thus, a covariate balance test of parental characteristics is also a way to investigate whether there is manipulation in the actual birthdate of the second child.

Graphical investigation

Figure A5a and A5b display the frequency of births over spacing when using the crude monthly measurement based on children's birth year and month.

Figure A6a and A6b show the frequency over the same bandwidths, the first 100 months and between 24 and 36 months of spacing, using the proxy variable for spacing in weekly bins.

The histograms reveal that many couples aim to have their second child in time to become eligible for the speed premium. It seems, though, as if couples who want to become eligible for the speed premium give themselves some margin to succeed: The peak in frequency of birth occurs already around 25 months, rather than just before the threshold. Reassuringly, the histograms show neither apparent heaping in births just before the 30-month threshold, nor a sharp drop in number of births just after.

Figure A6c displays the frequency of births in the two weeks right before and after the cutoff in daily bins of the proxy variable (the smallest bin size possible). The histogram reveals a rather surprising weekly heaping pattern in the proxy variable. It is unclear why this patter occurs. Perhaps it can be explained by parents using their days of leave on specific weekdays. Figures A7a - A7c and figures A8a - A8c replicate the histograms in figure A6a - A6c but when using, first, only information about the fathers use of daddy-days, and second, using only the mother's first PL-day to calculate spacing. The histograms with larger bandwidths and weekly bins show the same type of patters as the histograms described above. The weekly heaping pattern is more pronounced when only using the daddy-days. This pattern could thus have to do with how fathers use their daddy-days. However, this pattern does not constitute evidence that parents are able to manipulate the precise birthdate of the child.

Even though the frequency pattern over wider ranges of spacing suggests that parents plan the timing of their second child's birth in order to become eligible for the speed premium, there is no indication of strategic manipulation just around the threshold. In regression discontinuity analysis, the parameter of interest is evaluated at the threshold. Thus, the overall higher frequency of births on the left side of the threshold is not in itself a threat to identification.

McCrary test

A formal test for whether there is manipulation in the running variable is the McCrary test (McCrary, 2008). It analyzes the number of observations on each side of the threshold and estimates the difference in the log density between the two sides. If there is a heap of observations just before the threshold, or a sharp drop in observations just after, this test will estimate a jump in the density of observations. The test is designed for a continuous running variable, and therefore I use the proxy variable when performing it. Figure A9a shows graphically the result when using a 30-day bandwidth and a bin size of one day (the smallest possible). The estimate of -0.082 (s.d. 0.048) is not significant. However, the estimate is relatively large, which could be caused by the weekly cyclical pattern shown in the histogram of the proxy variable. To avoid bias caused by this pattern, the McCrary test is again estimated using weekly bins and multiple bandwidths between 7 and 182 days (26 weeks). Figure A9b graphically shows the results from this exercise. The estimate is close to zero for the first few months and then becomes significantly negative at about 4 months (125 days), but never larger than -0.085 (at 6 months). It is worth noting that when including observations more than 4 months away from the threshold, one would not expect the assumption of randomization in birthdate to hold. With this bandwidth, also observations from the "heap" in the distribution of birth (with a peak at 25 months of spacing) are included. Thus, it is not surprising or worrisome that the McCrary estimate is significant when including the larger mass of observations in this region. The same exercise is repeated using the "daddy-days"-proxy presented in figure A9c (when only information on daddy-days is used) with similar results.

Covariate balance

As an additional test of sorting, I test whether parental characteristics are balanced around the threshold. To this end, the first stage regression is estimated replacing the benefit level with another covariate: Mother's and father's years of schooling, yearly labor incomes, ages and immigration status, couple's marital status, first and second child's gender, and year and month of birth. Table A1 presents population means for the sample when using the proxy variable for spacing, the mean values for all observations near the threshold/cutoff (less than 30 days from it), and estimates of jumps in mean value at the threshold for the sample near the cutoff, for a number of individual and household covariates. All variables that vary over time indicate the value the year before the first child's birth.

None of the estimated jumps are significant, and most of them are close to zero. In addition, the mean values for the sample near the cutoff are close to those in the whole population. The table shows that, on average, mothers have about additional 6 months of schooling but have lower earnings than fathers the year before the first child was born. Mothers are about two years younger than fathers. About one third of all couples were married the year before they had their first child. About 8.5 % of all mothers and fathers are immigrants, and about 1 percentage point less among those near the cutoff. Couples near the cutoff are also a little more educated and have somewhat higher labor earnings.

5.3 Trends in spacing and parental leave variables over time

Changes in the pattern of spacing or other variables over time could be a problem for estimation. Figures A10 and A11 show the distribution of births across spacing over different birth years of the first and second child. The histograms reveal that the general patterns in spacing differ between years.[25] Figure A12a shows the average birth year of the first and second child for different values of spacing. The average birth year of the first child is about the same regardless of spacing, but the average birth year of the second child increases steadily across the 30-month threshold. The changing pattern in spacing over time means that the second child's birth year is not balanced over the ranges in spacing where the trends in parental leave benefits and take-up of days are estimated.

Figure A12b shows the average benefit level and take-up of PL-days of mothers with the second child over the second child's birth year. Starting in 1998, the average benefit level increased continuously. In part this can be ex-

[25] The distribution of births when the second child is born in 1996 is skewed to the left. This is because only couples who had their first child in 1994 or later are included in the sample. Thus, the only couples who could have 36 months of spacing in-between children in 1996 are those who had their first child in January 1994.

plained by institutional changes in the PL-system.[26] Because the maximum level of benefits is a function of the price base amount, which increases every year, the average level of benefits monotonically increases over time even if income levels do not change. Changes in the labor market is another explanation. During part of the period, there was rapid growth in real wages. The unemployment rate was at its highest level in 1997, after which labor market conditions improved.

At the same time as the average level of benefits grew, mothers' average take-up of PL-days decreased slowly. This reflects a general trend towards a more even division of the parental leave, where the fathers' share of days slowly increased.

Given that the average benefit level increased by about 50 % over the period, the lack of balance in the second child's birth year over spacing is likely to affect the estimated trends in benefit level over spacing. Since the jump in benefit level at the threshold is estimated at the jump between end points of the trends, the estimate of the parameter of interest could be affected. In the following sections I explain how I take this into account when performing the estimations.

6 Results

In this section, graphical evidence of changes in key variables at the 30-month threshold, as well as estimates of those discontinuities, are presented. First, any jumps in the benefit levels of mothers and fathers at the threshold are investigated (i.e. the first stage). If the speed premium affects the benefit level of eligible parents, then the average benefit level among those to the left of the threshold should be higher. If there is a clear jump in the benefit level at the threshold, this jump can be used to investigate how such a change affects the parents' utilization of PL-days. Second, any discontinuities in the parents' take-up of PL-days at the threshold are investigated in a reduced form analysis. Last, second stage estimates of the causal effect of a change in the benefit level of the parents' utilization of parental leave is estimated.

6.1 First stage (mothers)

Figures A13 (a) – (c) show mothers' average level of parental leave benefits over spacing; first in daily averages over the nearest 30 days on each side of the

[26]The replacement rate was partially decreased between 1996 and 1997, but then increased in 1998 from 75 % to 80 % of the individual's wage. Before the 1'st of July 2006 the cap level was reached at a qualifying income of 7.5 price base amounts. After the 1st of July 2006 the cap is at 10 price base amounts.

threshold (a), then over the closest 180 days (b), and last in weekly averages over the closest 180 days around the 30-months cutoff (c). The figures also contain fitted lines on each side of the threshold that are equivalent to including linear trends in the first stage regression (equation 1). There is a negative jump in the benefit level at the threshold—precisely what one would expect if the speed premium rule leads to a higher level of benefits among those eligible (to the left of the threshold). The jump is clear and large (about 10 SEK) when using the smaller bandwidth, but it is smaller and less convincing when using the larger (about 5 SEK). The explanation for the sensitivity to bandwidth becomes apparent when studying the residual plots from the first stage regression which control for the second child's birth year and birth month; figures A14 (a) and (b). As was shown in section 5.3, the frequency of births over spacing changes over time and the average level of benefits increases during the period of study (1994-2009). The increase in average benefit level combined with increase in average birth year of the second child makes any jump in benefit level at the threshold more difficult to detect. However, studying the residuals from the first stage regression when controlling for the second child's birth year and birth month, the jump in benefit level at the threshold is clear both for the smaller and wider bandwidth (figures A14a and A14b). The residual plot when using the 30-day bandwidth looks about the same as the figure for the raw data, which demonstrates that the jump in benefit level cannot be explained by sorting in timing of the child's birth just around the threshold.

Estimates of the jump in the mother's benefit level at the threshold using different bandwidths are presented graphically in figures A15 (a) – (c). In order to measure the effect of being treated (rather than the effect of *not* being treated), the estimates measure the jump when moving from just *above* the threshold to just *below* it.[27] The observations are weighted using a triangular kernel so that observations near the cutoff are given more weight in the regressions. In section 6.4 below, estimates from the most preferred specification are presented in table form.

Figures A15 (a) – (c) display the first stage estimate when using bandwidths between 7 and 365 days (adding 7 days for each estimation). In panel (a), separate linear trends on each side of the cutoff but no control variables are included. In panel (b), controls for the second child's birth year and birth month are added. In panel (c), a vector of additional control variables is also included namely, mother's and father's age, type and level of education, marital status, region of residence, immigration status, and the first child's gender. The figures also indicate the optimal bandwidths for the first stage according to Calonico et al. (2014) (CCT, indicated by the dashed line) and Imbens and Kalyanaraman (2011) (IK, indicated and by the dotted line) at 114 and 149

[27] This is opposed to the conventional way of performing RD-estimates; as the jump when moving across the threshold from left to right.

days respectively. When controlling for the child's birth year and birth month, the estimate is fairly stable at around 6-8 SEK for a large range of bandwidths (with the exception of very narrow bandwidths). The confidence intervals become gradually smaller when including more observations. When including all controls, the confidence intervals shrink further, and the estimate decreases slightly and stabilizes at about 5 SEK. The estimate is about the same and statistically significant for the CCT and IK bandwidths.

In conclusion, the first stage graphs and estimates show that there seems to be a discontinuous shift in the mothers' average level of parental leave benefits. This jump becomes apparent when controlling for overall trends in average benefit level and average spacing over time.

When only including observations just around the cutoff, the estimate is smaller, more unstable, and more sensitive to including control variables. This could be because of large variation in benefit levels between individuals, which would make any estimate based on relatively few observations less precise. Another explanation is that measurement errors in the assignment variable (the constructed proxy variable for spacing) diffuse any real jump in the benefit level because observations are assigned to the wrong side of the threshold. When including observations a little bit further from the threshold, more observations with correct treatment assignment are included that will make the estimations of the linear trends more reflective of the true levels among individuals with each treatment status. This reduces the bias induced by the measurement errors.

When including more and more observations in the estimations, the trends are gradually based on more and more observations that are not very near the cutoff. The implications of this are that: 1) the trends are based on more observations where the measurement errors in assignment to treatment status are probably fewer, and, 2) the trends are estimated to a larger extent on observations where eligibility status was known before the child was born. Since I use linear trends (an inflexible functional form), the end points are influenced by observations further away, i.e. couples who knew their eligibility status during pregnancy.

6.2 First stage (fathers)

Next, let's turn to the variation in the fathers' benefit level around the threshold. Figure A16 (a) and (b) show the daily averages of fathers' benefit level the nearest 30 days on each side of the threshold and in weekly averages over the nearest 6 months. Figure A17 (a) and (b) show the residual plots when controlling for the second child's birth year and birth month. Neither set of figures shows any clear discontinuity at the threshold. Figure A18 graphically displays the results when estimating the jump in the father's benefits using bandwidths between 7 and 365 days and controlling for the timing of the second child's birth. The estimate is close to zero and far from significant for all bandwidths.

In conclusion, there is no indication that fathers' parental leave benefit level is affected by the speed premium. There are at least two likely explanations for this. First, fathers are less likely to reduce their hours of work and/or earnings after having children, which implies that at the time of the second child's birth they are entitled to at least as high benefits as when the first child was born (and hence the speed premium will not be of any economic significance for them).[28] Second, since mothers usually take the first period of leave with the child, the father will have time adjust his labor supply after the child is born in order to qualify for a higher benefit level in the event that the couple is not eligible for the speed premium.

6.3 Reduced form (mothers and fathers)

As shown above, there are clear indications that the speed premium affects the parental leave benefit level of mothers, but not fathers. In this section, the potential effect of the jump in the mother's benefit level on the parents' take-up of parental leave days is investigated in a reduced form analysis. Figures A19 (a) – (f) show the variation in take-up of parental leave days (net QI-days) among couples near the threshold, using bandwidths of 30 days and 6 months, in daily and weekly averages. Figures to the left show the take-up of mothers, figures to the right show that of fathers. If parents' take-up of PL-days is sensitive to the amount of benefits received per day, then we would expect to see a jump in the average number of days taken at the threshold. In all figures, there is a negative jump in the mother's take-up of PL-days, and a positive jump for fathers, when moving across the threshold from left to right. The jump in levels becomes more distinct when including more data (panels c – f) and when averaging over more days (panels e – f).[29]

Reduced form estimates for mothers and fathers estimated at different bandwidths are presented in figures A20 (a) – (f). As before the estimations are performed for all bandwidths between 7 and 365 days, adding 7 days on each side in every regression. The optimal bandwidths according to Calonico et al. (2014) (CCT, indicated by the dashed line) and Imbens and Kalyanaraman (2011) (IK, indicated by the dotted line) when using the mothers or fathers take-up of PL-days as the outcome variable are indicated at 149 (CCT) and

[28] Several studies (for example Angelov et al., 2016) as well as official Swedish statistics (SCB, 2016) have shown that men's earnings are less affected by becoming parents then women's earnings.

[29] The average take-up of PL-days doesn't change as much over time as the average benefit level (see section 5.3). Hence, residual plots when controlling for the child's birth time do not differ radically from graphs of the raw data and are therefore not presented.

268 (IK) days for mothers and at 184 (CCT) and 298 (IK) for fathers.[30] The figures on the left-hand side show the estimates for mothers and the ones to the right for fathers. In figures (a) and (b), no control variables are included; in figures (c) and (d), controls for the second child's birth year and birth month are added; and in figures (e) and (f), the vector of controls for parental characteristics is included.

The estimate for mothers is large when only including observations in the very near proximity of the threshold, but shrinks and stabilizes for bandwidths of about 3 weeks and beyond. As for the first stage estimate, using a wider bandwidth does not change the magnitude of the estimate much but improves the precision. The jump in the mothers' take-up of PL-days at the threshold is estimated to be around 3 for bandwidths of around 140 days and beyond, including at the CCT and IK recommended bandwidths. When including control variables, the estimate decreases slightly, especially at large bandwidths. It is about 2.5 when including the full set of controls at the CCT bandwidth (149 days) and slightly lower, 1.9, at the IK bandwidth (268 days).

For fathers, the estimate of the jump in the take-up of days on parental leave is negative and statistically significant for (almost) all bandwidths displayed. As was concluded in the previous section (section 6.2), the fathers' benefit level is not affected by the speed premium. Thus, the interpretation of the negative reduced form estimate is that an increase in the mothers' take-up of PL-days (induced by a positive jump in her benefit level) results in a shift downward in the father's take-up of days. The estimate of the jump in the father's take-up of (net) days with parental leave benefits is large for narrow bandwidths. When including data between one and three and a half months, the estimate is relatively stable at 3.5 days. In the interval between the CCT and IK bandwidths, the estimate is at around 2 when including the control variables.

What is striking when comparing the estimates for mothers and fathers is that the shapes of the graphs are so similar, in the sense that one is the inverse of the other. Regardless of choice of bandwidth, the magnitudes of the estimates for the parents are relatively close to each other. The reduced form estimates thus indicate that a jump in one parent's benefit level, in this case the mother's, can induce redistribution of days where the reduction in time on leave is almost as large for the father as the increase is for the mother. Thus, in conclusion, changing the benefit level of one parent might not only directly affect that parent's time on parental leave, but can also affect the other parent's time on leave. Rather than affecting the total time the couple spends at home with the child, the change in benefit level seems to induce a change in the division of days between parents.

[30] The CCT and IK optimal bandwidths are derived based on only *one* outcome variable, which is why they are different for the first stage and reduced form for mothers and fathers.

6.4 Second stage: Estimating the effect of the PL-benefit level on take-up of PL-days

Next, a fuzzy regression discontinuity is used to estimate the causal effect of a change in the parental leave benefits level on the take-up of PL-days, using the speed premium rule as an instrument for the mother's level of parental leave benefits. The second stage estimate is numerically equivalent to scaling the reduced form by the first stage estimate. To get accurate confidence intervals, however, the second stage is estimated using two stage least squares.[31] Rather than presenting results for many different model specifications and bandwidths, results are first presented for one baseline specification, and results for alternative specifications are then presented as robustness checks (see section 7).

In my baseline specification, I choose a bandwidth of 180 days (6 months). As was apparent in previous sections, the CCT and IK methods each suggest different choices of optimal bandwidth for the first and second stage outcome variables, and for mothers and fathers. However, in fuzzy RD, the same bandwidth must be chosen in both regressions. In addition, to be able to compare estimates for mothers and fathers, the same bandwidth should be used for both. Thus the CCT and IK suggestions in this case give relatively little guidance.[32] The choice of bandwidth is ultimately a choice between increased risk of bias when estimating the trends based on observations further from the cutoff, and better precision when including more data. As was demonstrated in the previous sections, however, the estimates are relatively stable for bandwidths larger than about four weeks. At the same time, the precision improves drastically when using larger bandwidths, especially for the first stage. To achieve high precision, but at the same time avoid bias induced by including many data points far from the threshold, a bandwidth of 180 days is chosen. This is also the smallest bandwidth suggested for fathers (using the CCT criteria). The baseline specification is thus a local linear model with a bandwidth of 180 days (6 months). The vector of control variables for parental characteristics is also included.

Estimates for mothers

Tables A2 - A5 present OLS, first stage, reduced form and second stage estimates of the direct effect on mothers and fathers, the indirect/cross spousal effect on fathers and the effect on couple level.

[31] As concluded by Angrist and Pischke (2008), the fuzzy RD design is conceptually equivalent to an instrumental variable strategy.

[32] Both Imbens and Kalyanaraman (2011) and Calonico et al. (2014) suggest that when applied to a fuzzy RD setting, one should calculate the optimal bandwidth based on the second stage outcome variable and use that also in the first stage regression. However, the methods were clearly designed for the case of sharp RD.

Turning first to the results for mothers (table A2), the OLS estimates describe the association between the mother's benefit level and her take-up of parental leave days. The take-up of parental leave days is measured in three ways. The take-up of parental leave days is measured in three ways: first, as the take-up of net QI-days, i.e. the same measurement as was used in the graphs and reduced form estimates described in section 6.3; second, the total number of PL-days (both at the QI-level and the flat rate days); and third, as the total number of calendar days with any benefits. The table also contains the mean benefit level and the mean values of the three measurements of PL-days. The OLS estimates show that there is a negative correlation between the mother's level of benefit and her take-up of days. This is probably because mothers with higher earnings, who are entitled to higher benefits, are more career oriented and want to spend less time away from the work place. The second column in table A2, marked as FS, present the first stage estimates for mothers of 4.9 when using the baseline specification described above. The estimate is statistically significant at the 1 % level and has an F-statistic of 8.3.[33] The interpretation of this estimate is that the speed premium raises the benefit level of mothers by 4.9 SEK ($\approx$ \$0.54). Given the mean value of the mothers' benefit level (506 SEK $\approx$ \$56), the first stage estimate suggests that eligibility to the speed premium increases the level by about 1 %.

The reduced form estimates indicate that mothers who are eligible for the speed premium take up about 2.6 more net QI-days of parental leave, an increase of about 1 % given the mothers' average take-up of days. There is no additional increase in take-up when also including the flat-rate days. Mothers spread out the 2.6 additional net days over 3 calendar days. This implies that the actual increase in mothers' time spent at home is probably underestimated if one only considers the increase in net days.

Column four presents the second stage estimates, the causal effect of a 1 SEK increase in the benefit level per day on the take-up of parental leave. The interpretation of the estimate is that a 1 SEK ($\approx$ \$0.1) increase in the benefit level per day induces mothers to take about 0.5 more days of parental leave benefits. The second stage estimates are large but not very precisely estimated (significance levels are 10% for the net QI-days and number of calendar days).

An alternative interpretation is that of a cumulative effect; that an increase in the benefit level of 1 SEK per day during 268 days, adding up to a total of 268 SEK ($\approx$ \$30), induced the increase in take-up of 2.6 QI-days.

The estimates for mothers translate into an elasticity of take-up duration (length of spell) with respect to the benefit level of 1.[34] This elasticity mea-

[33]This is not too far off from the rule of thumb level of 10, suggested by Angrist and Pischke (2008).

[34]The elasticity of duration with respect to the benefit level, using the formula in Gruber (1997), is calculated as:

$$\varepsilon^B = \frac{b}{D}\frac{\partial D}{\partial b} = \frac{506.29}{268.39} \times \frac{2.621}{4.902} = 1.0086 \approx 1.0$$

sures the percentage increase in the number of days on parental leave due to a 1 % increase in the level of benefits per day. This means that, according to my estimates, the mother's take-up duration is very sensitive to changes in the benefit level. An elasticity of 1 is in magnitude a large estimate, but it is not that different from what has been found before for other types of benefits. For example, Meyer (1990) estimates the take-up duration elasticity of unemployment benefits to be 0.9.[35]

Estimates for fathers

Table A3 contains the OLS, first stage, and reduced form estimates for fathers. As concluded in section 6.2, fathers' parental leave benefit levels are not on average affected by the speed premium. The first stage estimate and the F-statistic are close to zero. At the same time, the reduced form estimates indicate that fathers in eligible couples take almost 2 PL-days less than those in non-eligible couples. In magnitude this decrease is about 75% of the increase of mothers, and equivalent to a 4% reduction in the father's total take-up of net QI-days. The reduced form estimates for fathers indicate that a jump in the mother's take-up of PL-days, in this case induced by an increase in her benefit level, can lead to an almost as large jump of the fathers in the opposite direction.

Table A4 contains the cross-spousal estimates of the association between the benefit level of the mother on the father's utilization of parental leave. The OLS estimate indicates that the correlation between the mother's benefit level and the father's take-up of PL-days is positive. This is in line with descriptive statistics that show that couples where the woman's earnings are in the upper part of the distribution divide their parental leave more equally. The first stage estimates indicate the value for mothers (same as in table A2) while the reduced form indicates the estimates for fathers (same as in table A3). Column four contains the second stage estimates of the effect of a change in the *mother's* benefit level on the *father's* take-up of PL-days. The estimate of -0.399 for the take-up of net QI-days is significant at the 10% level and in magnitude about 75% of that of the mother. The significant second stage estimate for fathers confirms that changing the benefit level of one parent, in this case the mother, not only induces a change in that parent's take-up of PL-days, but can also lead to a change in the division of days between parents. For fathers, the reduction in calendar days with benefits seems to be on par with the reduction in net days. The magnitude of father's second stage estimate for the number of calendar days with benefits is about 60% of the corresponding estimate for mothers.

where D is the duration measured as take-up of net days with benefits (net QI-days) and b is the benefit level per day.

[35] As far as I am aware, there are no previously studies that estimate the take-up duration elasticity for take-up of parental leave benefits.

Estimates at couple level

Table A5 contains the estimates of the association between the mother's parental leave benefit level and the couple's total take-up of PL-days. As indicated by the OLS estimates, there is no correlation between the mother's benefit level and the couple's total take-up of PL-days. Thus, the correlations shown between the mother's benefit levels and each parent's take-up of days (discussed above) seem to originate only from difference in the division of days between couples at different ranges of the income distribution of mothers. The second column replicates the first stage estimate for mothers. The reduced form and second stage estimates indicate that an increase in the mother's level of benefits leads to, at most, a small increase in the total take-up of net QI-days. The reduced form estimate is about 2/3 of a day which is equivalent to an increase of 0.2% in the couple's total take-up. The reduced form estimate for the couple's total number of calendar days with benefits is about twice as large at 1.2 which is equivalent to an increase of 0.3 %. These estimates are equivalent to take-up duration elasticities at the couple level of 0.22 for the total take-up of net QI-days with respect to the mother's benefit level. This is much lower than the elasticity of mothers which was calculated to be 1 (see section 6.4). The conclusion is that the mother's take-up of parental leave, and the couple's division of days, seem to be very responsive to changes in the benefit level, but that the couple's total take-up is relatively irresponsive to changes in one parents benefit level.

7 Robustness checks

In section 6.4 above, estimates when using the baseline specification with linear trends, a 180-day bandwidth, a vector of control variables and triangular weights were presented. In this section, the stability of these estimates is tested through re-estimation with alternative specifications. Robustness checks are performed for the direct effect on mothers (table A2) and the cross-spousal effect on fathers (table A4).[36]

Tables A6 and A7 contain estimates on the direct effect for mothers and the cross-spousal effect for fathers for some alternative specifications and sensitivity checks. The top row in each table replicates the baseline estimates for the mother's first stage, and reduced form and second stage estimates for each parent for the three measurements of take-up of PL-days.

First, the sensitivity of the estimates to including different sets of control variables is tested. The second row in each table contains results when all control variables are dropped from the baseline model (the child's birth year

[36]Since there is no first stage effect for fathers there is no further investigation of the direct effect on them. Couple level effects are the sum of the effects on each parent. Thus, it is sufficient to test the stability of the effect on each parent.

and month and the X_i vector). Showing the estimates without any controls is important for transparency. However, because of the trends in the outcome variables over time, shown in section 5.3, not controlling for when the child was born makes the estimates less trustworthy. The third row contains estimates when only including controls for the second child's birth year and birth month.

Rows four and five re-estimate the baseline specification using alternative constructions of the proxy variable for spacing (and thus different samples). The first alternative proxy uses only information on the father's daddy-days to measure spacing. This reduces the sample size by about 1/4. The second uses the baseline proxy, but with the amendment that in the cases where the parents did not use their first PL- or daddy-day during the child's birth month, a randomly selected day in the child's birth month is used as a proxy for the child's birthday. The motivation for doing this is that using information from all data points can improve the estimations of the trends in the outcome variable. The downside is that using a random day as a proxy creates noise in the variable. Since many couples whose child was born close to the cutoff are going to be assigned to the wrong side of it, this method risks to cancel out some of the actual jump in the variable.

Next, the baseline results are re-estimated using the bandwidths recommended by Calonico et al. (2014), marked CCT, and Imbens and Kalyanaraman (2011), marked IK in the tables.[37]

As an additional sensitivity test, a "donut" strategy is used in which all observations in the week before and after the threshold are dropped. The investigation in section 5.2 did not reveal any signs of strategic timing of the second child's birth. However, the donut strategy would ensure that any bias caused by such manipulation — for example, couples who manage to schedule an early C-section — could not bias the estimate.[38]

Last, quadratic and cubic trends are included to allow for more flexible trends on each side of the threshold. As discussed in section 4, the proportion of couples with an expected due date before the threshold decreases continuously across the threshold. Thus, to the left of the threshold the proportion of eligible couples is 100%, out of whom a decreasing proportion are eligible because of their expected due date, and to the right of the threshold the proportion of eligible couples decreases continuously. If eligibility implies a higher benefit level, then it is possible that a linear approximation for the trend in benefit level over spacing is not the correct one just around the threshold. Even if linear trends are a reasonable assumption near the cutoff, it might be

[37] These criteria were created with sharp RD in mind and make separate recommendations for each outcome variable. However, to be able to compare the results for mothers and fathers and perform second stage estimations, all regressions are performed using the recommended bandwidth for mothers' reduced form outcome variable; take-up of net QI-days.

[38] It is unlikely that any health care provider would schedule a C-section more than a week before the expected due date unless there was a strong medical reason to do so.

that a more flexible functional form can better describe the data further away from the cutoff.

Let's turn first to the stability of the first stage estimate (the jump in the mother's benefit level). The magnitude of the estimate is remarkably similar in most specifications (around 5 SEK $\approx$ \$0.6), although it loses precision in some. All the first stage estimates are within one standard error of the baseline. A few specifications change the estimates more than others: Adding the vector of controls for parental characteristics decreases the FS-estimate by 15 % compared to only controlling for timing of the child's birth. When using the "daddy-day" proxy, the first stage estimate is about 80% in magnitude of the baseline. When including all observations by adding a random day as a proxy for the child's birthday for observations that could otherwise not be included, the first stage estimate decreases to about 75% of the baseline. The reduction of the estimate could be explained by the fact that, with this method, some couples are assigned to the wrong side of the cutoff, which to some extent nullifies any jump in levels at the threshold.

The results when using the CCT and IK criteria confirmed what the graphical examination revealed (section 6.1), namely that the first stage estimate is stable over a wide range of bandwidths. When using the donut strategy, the first stage estimate is close to the baseline (slightly larger). This is an indication that the baseline result is not caused by manipulation in the running variable just around the cutoff. Finally, introducing more flexible functional forms in the regression model hardly changes the estimate. This result is reassuring since it implies that using linear trends is not unreasonable.

When it comes to the reduced form estimates, the estimates are consistently lager or close to the baseline estimate. The exception is when using the donut strategy for fathers, in which the magnitude of the estimates is smaller. The reduced form estimates are less stable than the first stage. However, since they are less precisely estimated in the baseline model, perhaps this is not that surprising. Most of the sensitivity checks produce estimates that are within one standard deviation of the baseline estimate. The exceptions are some results for the models with more flexible functional forms: number of calendar days for mothers, and the estimates for all outcome variables for fathers. However, except for the estimates for fathers when adding a third order polynomial, these estimates are within the confidence intervals of the baseline reduced form estimates. It is well known that RD-estimates are sensitive to including higher order polynomials. A drawback of using higher order polynomials is that extreme values close to the intercepts could have a great influence on how the trend curves close to the end points and thus have a great influence on the estimates. In the baseline specification linear trends are used, but weights are imposed to give more importance to observations near the cutoff. Since the first stage estimate when using the flexible functional forms is very close to that in the baseline, this suggests that a linear model is reasonable.

The second stage estimates lose precision in many of the sensitivity tests. However, all but a few estimates are within one standard deviation of the baseline estimates.[39]

To conclude, the baseline estimates seem to be reasonably stable to the exclusion of control variables, using alternative measurements of the running variables, using other bandwidths, and excluding observations very near the cutoff. Crucially, the first stage estimate is particularly stable in magnitude. The reaction to the change in the mother's benefit level is more difficult to estimate precisely.

8 Placebo tests

8.1 Placebo thresholds

To further investigate if the estimates presented above are truly caused by the speed premium (or just by chance), the main specification for the mothers' net QI-days on leave is re-estimated using a number of placebo thresholds. Table A12 contains first stage (FS) and reduced form (RF) estimates when, in addition to the (real) threshold at 30 months of spacing, estimating these parameters at all turns of the month between 25 and 35 months after the first child's birth. The estimates for the (true) 30-month cutoff is the same as in table A2 above.

Reassuringly, all of the placebo estimates are smaller in magnitude than those at the true threshold and many are close to zero. None of the first stage estimates at placebo cutoffs, and only one of the reduced form estimates, is statistically significant (at the 10 % level). The significant estimate is not too worrisome since, when using the 10 % significance level, one would expect two placebo estimates out of twenty to be significant just by chance. The same estimates, with 95 % confidence intervals, are presented again in figures A22 (a) (first stage estimates) and (b) (reduced form estimates).

8.2 Placebo first stage (mothers)

As an additional placebo test, the variation in the mother's benefit level when on parental leave with the first child is investigated. Since the PL-benefit level is based on the parent's previous earnings, more specifically the parent's qualifying income, the level when on leave with the first child should not be affected by spacing between the first and second child (see section 3). Thus, if there is any discontinuity in the benefit level with the first child at 30 months of spacing, that would indicate that parents, with specific levels of qualifying

[39] The exceptions are the estimate for mothers when using the daddy-days proxy, and the models with flexible trends for fathers.

income before the first child was born, sort into being eligible for the speed premium. For example, parents with a high QI at the time of their first child's birth might aim to have their second child within 30 months so that they can base their benefit level with the second child on their old QI level. If so, the jump in level with the second child (the first stage estimate) could be caused by sorting, rather than by randomized eligibility status.

Figures A23 (a) and (b) show the mother's average PL-benefit level with the *first* child over spacing; a "placebo" first stage. Figures A24 (a) and (b) display the residuals when estimating the first stage regression, but now with the benefit level with the *first* child as the outcome variable, and controlling for the child's birth year and birth month. In the raw data, there is a negative jump when using a narrow bandwidth and a positive jump when using the larger. These discontinuous jumps diminish significantly when controlling for the child's time of birth, as demonstrated in the residual plots.

Figure A25 shows the estimates and confidence intervals when estimating the jump in the mother's benefit level with the first child, using different bandwidths and controlling for the child's birth year and birth month. The estimate is close to zero for most bandwidths and never significant. There is thus no indication that couples sort around the threshold based on the mother's benefit level with the first child (or qualifying income at the time of the first child's birth).

9 Results when using monthly data to measure spacing

In the previous sections all estimations were performed using the constructed proxy variable for spacing in which the father's first daddy-day or the mother's first PL-day is used as a proxy for the child's birthday. There are at least two drawbacks to this strategy. One is that not all observations can be included in the sample since in about 25% of the cases the parents did not use any of their daddy-days or PL-days during both children's birth months (see section 4.2). The other is that since this proxy is not always accurate, some observations are assigned to the wrong side of the threshold (as discussed in section 5.1).

As a complement to the analysis using the proxy variable, this section contains graphical evidence and estimation results when instead using only the child's birth month to calculate spacing between children. Since children's birth months are known for all children, behavioral differences when it comes to take-up between different groups of parents cannot systematically influence this measurement. By only using information which is defined in the same way for all observations, the calculation of spacing does not differ systematically between different groups of parents. For example, differences in how fathers with different characteristics use their daddy-days cannot systematically assign these fathers to a specific side of the threshold. Thus, this strategy eliminates at least one potential source of systematic measurement error. Also, by using the crude measurement of spacing, all observations can be included in the sample. This improves the external validity of the estimates.

When using this crude measurement of spacing, the identification strategy is different. Identification here rests on the assumption of randomization in the date of conception, which leads to randomization in the number of months between children. As discussed in section 4.2, the casual effect of a change in the benefit level on the parents' take-up of parental leave can be estimated by exploiting the jump in proportion of eligible couples between those with 30 and 31 months of spacing. Since spacing is measured as the number of months that passes between the child's birth month (month "zero") and the month when the second child is born, this measurement of spacing is sometimes one unit to small, but never larger than the actual number of months that has passed in-between the children's births. Among observations with calculated spacing equal to 30, about 50 % are eligible because the second child's expected due date was within 30 months, a smaller proportion are eligible because of an early birth, and the rest have an expected due date and an actual birth that was beyond the threshold. Thus, among couples with calculated spacing equal to 30, there will be those whose actual spacing was less than 30 months, and those whose actual spacing was more than 30 months. Among those with spacing equal to 31, on the other hand, all couples will have at least 30 months between their children, i.e. none of them will be eligible because of an early birth. However, some proportion of the couples with calculated spacing equal to 31 are eligible because the expected due date of their child was within the 30 month threshold (but the actual birthday came later). This implies that there is a discontinuous jump in the proportion of eligible couples when moving from month 30 to month 31.

Thus, with this strategy, the jump in outcome variables is not estimated *at the threshold*. Comparing observations with spacing equal to 30, to those with spacing equal to 31, the different in "treatment" is mostly that more observations in month 30 knew during their second pregnancy that they were going to be eligible to the speed premium (because of the child's expected due date). Any estimate of a difference in take-up among couples in month 30 to those in month 31 comes mostly from a difference in behavior between couples who knew that they would be eligible and couples who knew that they would probably not be eligible. This is different from estimations performed in previous sections, where the estimates are evaluated at the threshold and thus the jump in proportion of eligible comes from couples who become eligible due to an early birth.[40]

[40] At the threshold, the proportion of couples with an expected due date before the cutoff decreases continuously. The jump in proportion of eligible couples originates from the fact that some couples whose expected due date was beyond the threshold had an early birth, which made them eligible. Treatment, in that case, is to unexpectedly become eligible.

Figures A21 (a) – (d) display the mothers' average benefit level (a), the residuals when controlling for the second child's birth year and birth month (b), and the mothers' and fathers' average take-up of PL-days (c) and (d). The graphs display the values for observations with spacing between 25 and 36 months: six months before and after the month 30 and 31.

In the graph of the raw data, no jump in the mothers' benefit level is present. However, when controlling for the child's birth year and birth month, there is a clear shift in the mothers' level at the threshold.[41] Turning to the graphs for the parents' take-up of days with benefit, there is also a clear shift in the general level, both for mothers and fathers.

Tables A8 - A11 show the results when estimating these discontinuities in a regression analysis similar to the one in section 6.4. As in section 6.4, a bandwidth of six months, linear trends, and triangular weights are used. Table A8 presents OLS, first stage, reduced form, and second stage estimates for mothers. Since all observations can be included, the sample size is larger than when using the proxy variable. Although the discontinuities in the outcome variables are now evaluated by comparing by comparing observations in month 30 and 31 (not at the threshold), the estimates are remarkably similar to those when using the proxy variable. The first stage estimates indicate that the jump in benefit level when moving from month 30 to 31 is about 4.8 SEK, almost the same as when evaluating the jump at the threshold using the proxy variable. The F-statistic is slightly larger at 9.73 (compared to 8.3 when using the proxy). The reduced form and second stage estimates are also very similar in magnitude to those in table A2. The estimates are more precisely estimated which could be a result of using a larger sample size.

Table A9 presents the results when estimating the direct effect on fathers.[42] There is a negative correlation between the father's level of benefits and his take-up of days, but no jump in benefit level when moving from month 30 to 31. The reduced from is somewhat smaller for the number of net QI-days than when using the proxy variable: about 70% in magnitude, -1.388 compared to -1.957. The other estimates are fairly similar. Since there is no first stage jump in the father's benefit level, the second stage is not estimated.

Table A10 presents the cross-spousal effect on fathers by a change in the benefit level of the mother. As in table A4 in section 6.4, the OLS estimates indicate that there is a positive correlation between the mother's benefit level and the father's take-up of parental leave days. Columns two and three in the table reproduce the first stage estimate for mothers and the reduced form estimate for fathers. The second stage estimates the relationship between a change in the benefit level of the *mother* and the *father's* take-up of PL-days;

[41] To make these estimations as comparable as possible to those in section 6, the same specification is used here. Linear trends are included in all regressions and the observations are weighted using triangular weights.

[42] The sample size is slightly larger here because fathers who did not use any PL-days and could therefore not be included in the first stage regression.

the cross-spousal effect. Because of the smaller reduced form estimate, the second stage estimate for the take-up of net QI-days is somewhat smaller than when using the proxy variable.

Finally, table A11 presents estimates at couple level. The OLS estimate indicates the correlation between the mother's benefit level and the couple's total take-up of PL-days. The second column again reproduces the first stage estimate for mothers, and the reduced form indicates the jump in the couple's total take-up of PL-days at the cutoff between month 30 and 31. The second stage estimate indicates the causal effect of a change in the mother's benefit level and the couple's total take-up of days. Because the jump in the father's take-up of days is smaller, the reduced form and second stage estimate for the couple's total take-up of net QI-days is larger than when using the proxy-variable. The estimates for the total number of calendar days with benefits is very similar to the one in table A5, section 6.4.

In conclusion, the results when using the crude measurement of spacing based only on the child's birth month (not day) are very similar to the results when using the proxy-variable for spacing, where I use a proxy for the child's birth day. This is noteworthy since, as discussed in section 7.1, treatment is not defined in quite the same way when using the two different measurements of spacing. When using the proxy variable, any discontinuity in the take-up of days at the threshold originates from behavioral responses among couples who became eligible for the speed premium due to an early birth of their second child. Since these couples had an expected due date that was beyond the threshold, they did not expect to be eligible. Thus their response to the higher benefit level could not have been planned during the second pregnancy. When using the monthly spacing variable, on the other hand, the estimated jump in take-up of PL-days is an estimate partly of a response among those who are unexpectedly eligible, but mostly, a response among those whose expected due date was within the time frame for eligibility and who therefore knew during the second pregnancy that they would benefit from the premium. The response from these couples could potentially be different from the one when unexpectedly receiving a higher benefit level. For example, parents who know that they will be eligible might feel less compelled to continue to work during the second pregnancy. This could in turn affect the parent's opportunities to return to work after going on leave, which could prolong his or her length of leave. However, the estimates of the response, whether estimated at the threshold using the continuous proxy variable, or by comparing couples in month 30 and 31, are remarkably similar. This suggests these two types of "treatment" evoke very similar responses.

The fact that the same results are reached when using an alternative method strengthens the conclusions that were drawn from the baseline estimates, namely the following: The speed premium rule does affect the benefit level of the mother (but not the father) by shifting it upwards. This positive shift induced the mother to spend about half a day more of benefits for every unit of increase (1 SEK). The change in the mother's benefit level not only makes the mother take more days, but induces a redistribution of days between the parents so that the father will end up spending fewer days on leave. On the whole, therefore, the couple's total take-up of days only changes marginally by 0.3 %.

10 Concluding discussion

Policies that enable parents to take paid leave from work to spend time at home with their new baby are common, especially among developed countries. Although the policies are often motivated by wanting to promote women's position in the labor market and work-life balance, relatively little is known about their overall effects. Likewise, the mechanisms behind parents' choice to take the leave, how many days to take and how to divide them between the parents have not yet been fully disentangled, mostly due to lack of exogenous variation in the variables of interest. It is known from studies investigating the effects of policy reforms that changes in the length of entitlements, introduction of quota days and work requirements, and shifting from a flat-rate benefit to a system with wage replacement all affect the parents' take-up patterns (see, for example, Ruhm, 1998 and Waldfogel, 1998 (implementation), Stearns, 2016 (change in work requirements), Karimi et al. (2012) and Lalive and Zweimüller (2009) (extentions), Karimi et al. (2012) and Ekberg et al. (2013) (quota months)). Although there are a few studies that investigate the role of the benefit *level*, none of these do so using an exogenous source of variation in a system where no other changes occurred simultaneously.[43]

This paper investigates the impact of the parental leave benefit level on the take-up and division of parental leave among parents in Sweden. The "speed premium" rule in the Swedish parental leave system is exploited in order to estimate the causal effect of a change in the benefit level on parents' utilization of parental leave. A fuzzy regression discontinuity design is applied, exploiting the fact that the proportion of couples who are eligible for the premium decreases at a threshold of 30 months of spacing between children. Assuming that parents cannot precisely control when their second child is born or conceived, the rule creates exogenous variation in expected benefit level among parents whose child was born just before and just after the threshold.

My results indicate that the speed premium does affect the level of benefits for mothers, but not for fathers, and that a difference in benefit level can induce a strong response in the parents' take-up behavior. Among couples to the left

[43] For example, Kluve and Tamm (2013) and Bergemann and Riphahn (2010) study a reform in Germany that simultaneously changed the reimbursement system from a flat rate to wage replacement, removed the means-testing in the system, and shortened the disbursements from two to one year.

of the threshold (where all couples are eligible), the mother's average level of benefits is about 5 SEK ($\approx$ \$0.54) larger than to the right of it (where a smaller proportion of couples are eligible). This jump is equivalent to a 1% increase in the mother's benefit level.

This shift in levels induced mothers to increase their take-up of parental leave days by 2.6 days. The change in take-up is equivalent to an about 1% increase in the mother's total number of PL-days. These estimates translate into a take-up duration elasticity of 1 for mothers. In other words, a 1% increase in the benefit level leads to a 1% increase in the take-up of net PL-days. This is a large elasticity, and one that has, to the best of my knowledge, not been estimated before. Since the magnitude of this response has previously been unknown, this result has implications for future designs of theoretical models analyzing the tax and transfer system.

Further, my results show that the change in the mother's benefit level does not just affect her, but indirectly induces a response among fathers as well. It turns out that the shift in the mother's take-up of leave days causes an almost as large response in the father's take-up of leave days, but in the opposite direction. A 2.6 day increase in the mother's take-up is found to reduce the father's take-up of PL- days by almost as much: 1.9 days.

This result suggests that the parents' decisions on how many days to spend on parental leave are interdependent, and that if a shift in one parent's time on parental leave is induced, the duration of the other parent's leave will also be affected. Since the parents get to divide the parental leave days for each child between them as they prefer (except for the quota "daddy-months"), this is not surprising. However, the fact that the reduction in the father's number of days so closely follows the increase among mothers is striking. It suggests that couples often aim to use a specific number of leave days (for example, all days) and their decision on the division of days becomes a zero-sum game.

The large effect on father's take-up behavior is worth highlighting, especially since the change in take-up is induced without any direct economic incentive for fathers. A 1.9 day decrease in take-up is equivalent to a 4 % reduction since the average take-up for fathers is 49 days. This is a large effect.

Another finding is that mothers and fathers seem to utilize the parental leave in different ways. Mothers, to a higher degree than fathers, tend to spread out their benefits over more calendar days in order to prolong their time on leave. A 2.6 day increase in mothers' take-up of PL-days leads to a total of 3 calendar days more with benefits. For fathers the change in take-up of *net* days and the change in calendar days with benefit is essentially the same.

The estimates on the effect on take-up of PL-days and calendar days with benefits suggest a negative effect on mothers' labor supply after having their second child. However, earlier studies, including studies on Swedish parents, have been largely unable to find evidence that changes in the precise take-up of PL-days has long term effects on earnings. Thus, long-run effects on labor market outcomes are probably small or non-existent.

My results have wide policy implications as family leave policies are present in many countries and others, like the US, are debating implementation. The results in this study suggest that policymakers who are set to design a parental leave system or reform one should carefully consider the reimbursement level and how it is determined. Furthermore, it is not just the individual's level that is important, but the parents' relative levels also seem to have an impact on their decision of how to divide the time spent at home with the child.

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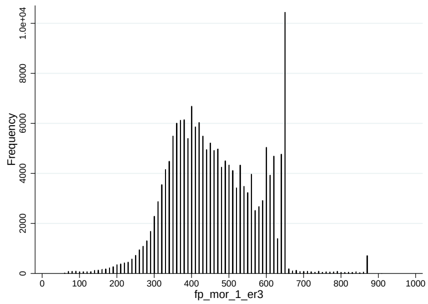
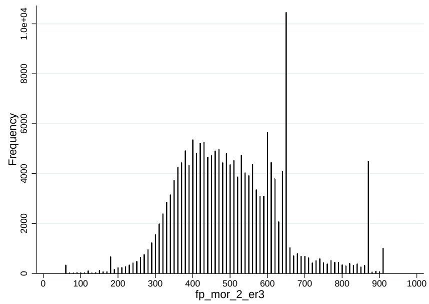
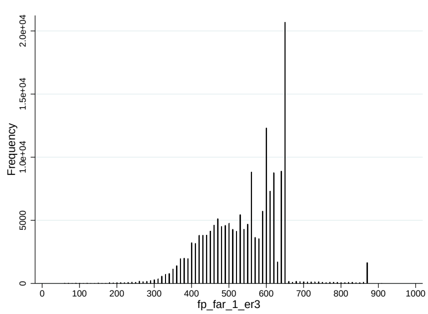
Appendix



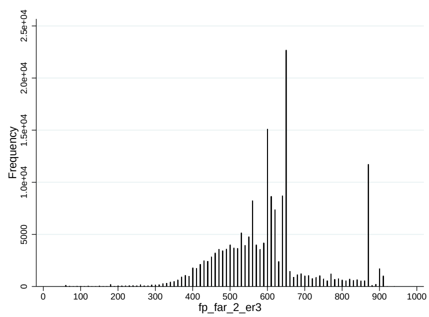
(a) PL-benefit level, 1'st child, mothers.



(b) PL-benefit level, 2'nd child, mothers.

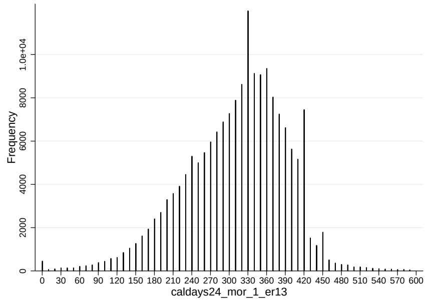


(c) PL-benefit level, 1'st child, fathers..

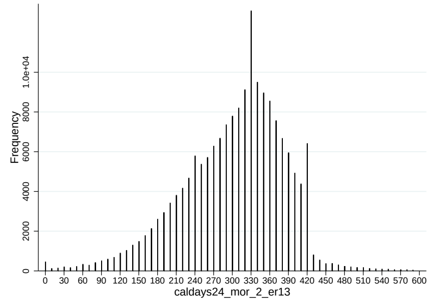


(d) PL-benefit level, 2'nd child, fathers.

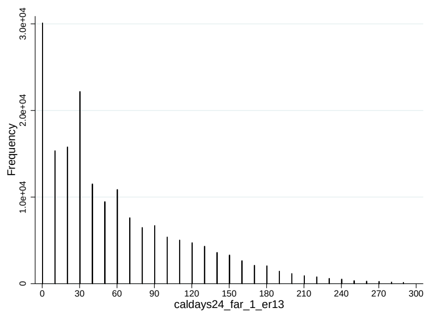
Figure A1. Distribution of the PL-benefit level of mothers and fathers with the first and second child.



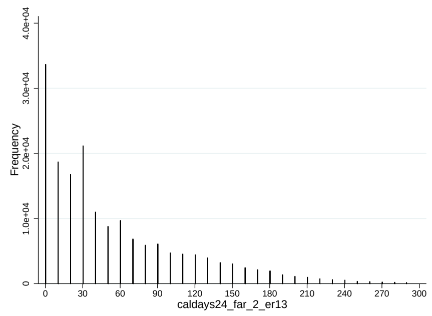
(a) PL-days, 1’st child, mothers.



(b) PL-days, 2’nd child, mothers.

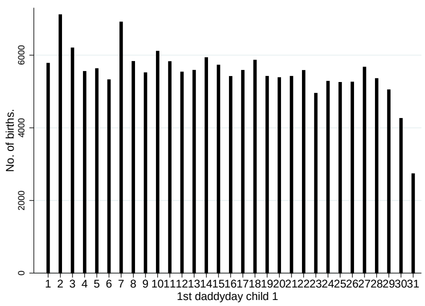


(c) PL-days, 1’st child, fathers.

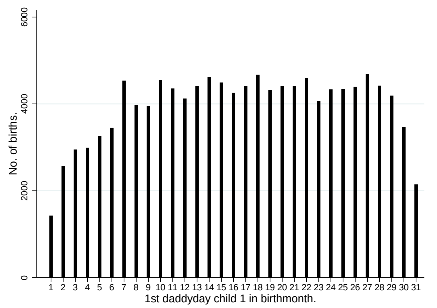


(d) PL-days, 2’nd child, fathers.

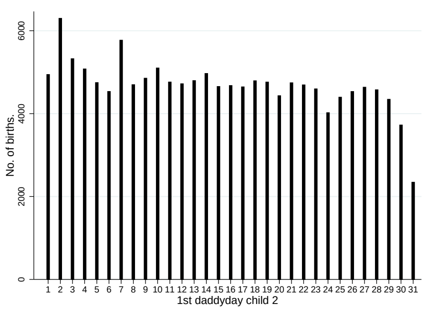
Figure A2. Distribution of the take-up of PL-days of mothers and fathers with the first and second child. Notice the difference in scales on the histograms for mother and fathers.



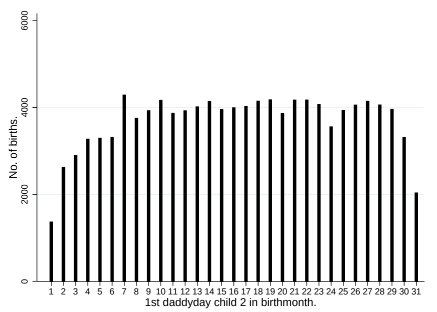
(a) Father's first PL-day with first child.



(b) Father's first PL-day with first child is in child's birth month.

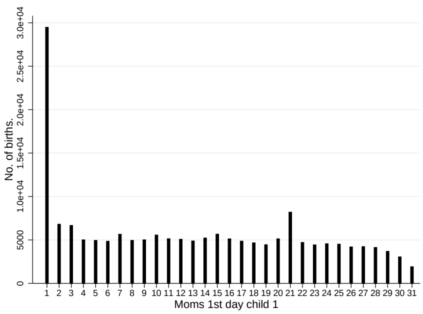


(c) Father's first PL-day with second child.

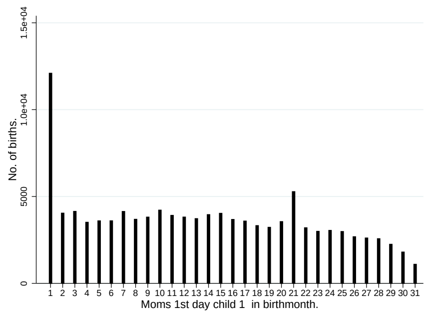


(d) Father's first PL-day with second child is in child's birth month.

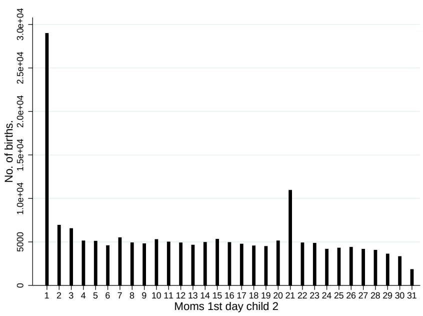
Figure A3. Distribution of father's first "daddy day" with first and second child respectively over calendar days 1-31. All observations and only those where the first daddy day was in the child's birth month.



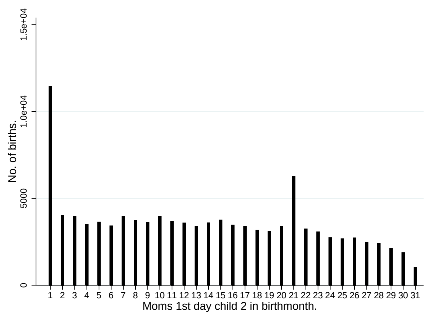
(a) Mother's first PL-day with first child.



(b) Mother's first PL-day with first child is in child's birth month.

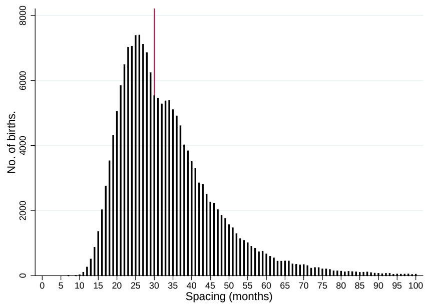


(c) Mother's first PL-day with second child.

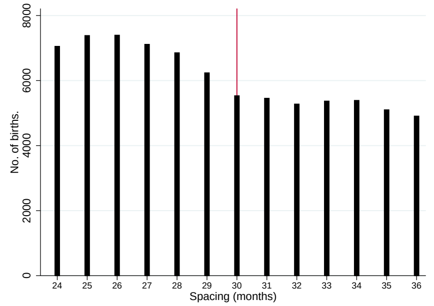


(d) Mother's first PL-day with second child is in child's birth month.

Figure A4. Distribution of mother's first day on PL with first and second child respectively over calendar days 1-31. All observations and only those where the mother's first day was in the child's birth month.

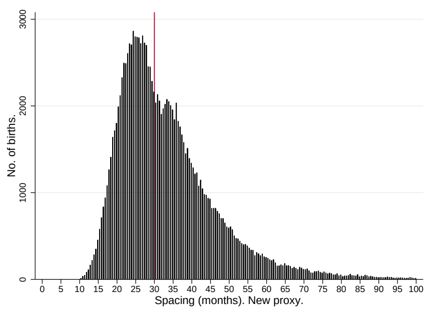


(a) Frequency of births: 0-100 months of spacing.

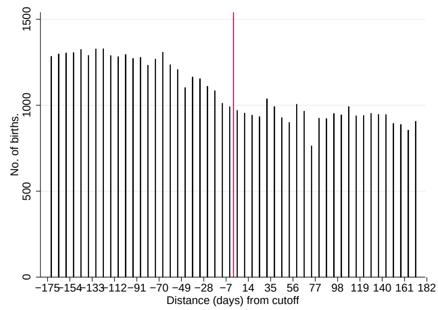


(b) Frequency of births: 24-36 months of spacing.

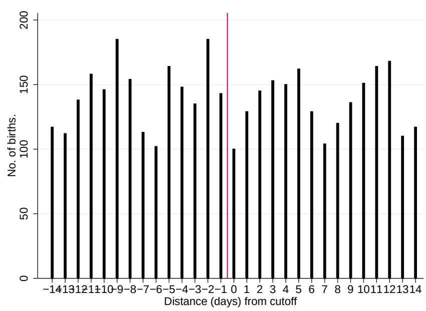
Figure A5. Frequency of births over child spacing (in months) between first and second child's birth calculated using the children's birth year and month. Exact date of birth is not available in the data. There is no apparent discontinuity in the frequency of births around the 30-month threshold.



(a) Frequency of births: 0-100 months of spacing (proxy).

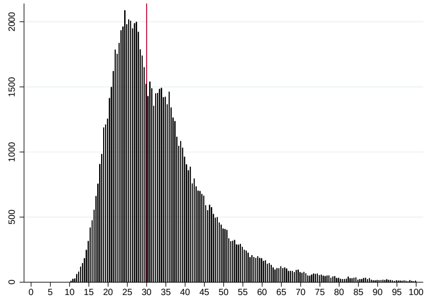


(b) +/- 175 days from 30-month threshold. Bin size=7 days.

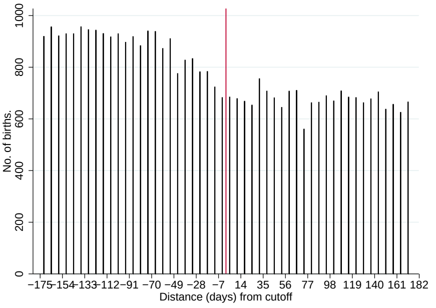


(c) +/- 14 days from 30-month threshold.

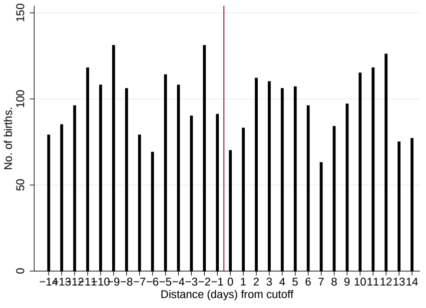
Figure A6. Figures (a) - (c) show the frequency of births over spacing were information on the fathers' first "daddy day" with the first and second child, and the mother's first PL-day with the first and second child, has been used to caluculate spacing between children.



(a) Frequency of births: 0-100 months of spacing (daddy days proxy).

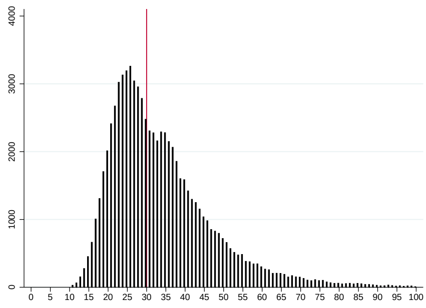


(b) +/- 175 days from 30-month threshold. Bin size=7 days.

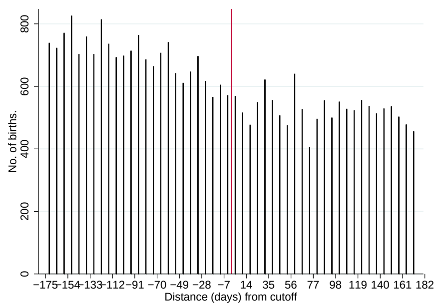


(c) +/- 14 days from 30-month threshold.

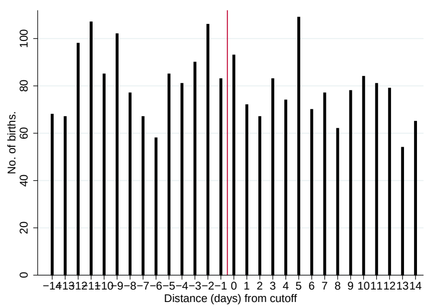
Figure A7. Figures (a) - (c) show the frequency of births over spacing measured as time between the fathers' first "daddy day" with the first and second child.



(a) Frequency of births: 0-100 months of spacing (mother's first PL-day proxy).

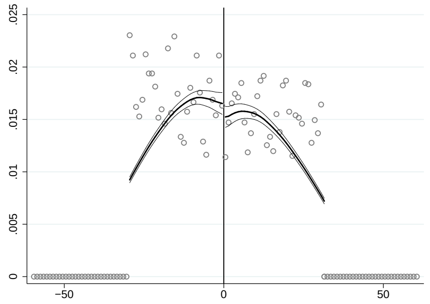


(b) +/- 175 days from 30-month threshold. Bin size=7 days.

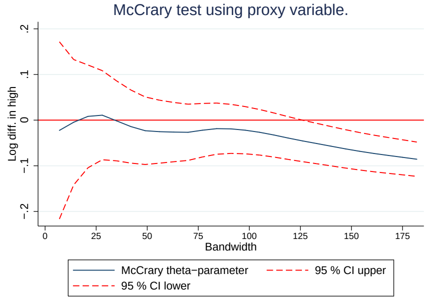


(c) +/- 14 days from 30-month threshold.

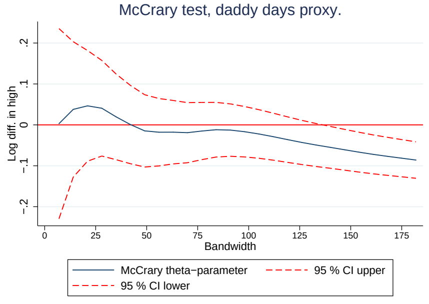
Figure A8. Figures (a) - (c) show the frequency of births over spacing measured as time between the mother's' first PL-day with the first and second child.



(a) McCrary-test with bandwidth=30 days. Bin size=1 day.



(b) McCrary-estimates over different bandwidths. Proxy-variable. Bin size=1 week.



(c) McCrary-estimates over different bandwidths. Daddy-days proxy. Bin size=1 week.

Table A1. *Descriptive statistics and covariate balance test.*

	Population mean	Mean near cutoff	Jump at cutoff
Mothers education	13.8	14.1	-0.1
	(2.5)	(2.5)	(0.1)
Fathers education	13.2	13.5	-0.0
	(2.5)	(2.6)	(0.1)
Mothers income	185,529.3	193,844.9	198.8
	(98,392.2)	(101,067.4)	(5,002.4)
Fathers income	239,288.2	243,629.6	-2,138.8
	(129,421.4)	(131,732.6)	(6,570.3)
Mothers age	27.4	27.5	0.3
	(3.8)	(3.8)	(0.2)
Fathers age	29.4	29.4	0.1
	(4.2)	(4.1)	(0.2)
Percent married	31.4	32.8	2.3
	(46.4)	(46.9)	(2.2)
Percent girls - 1st child	48.7	48.4	-2.3
	(50.0)	(50.0)	(2.4)
Percent girls - 2nd child	48.6	49.1	2.4
	(50.0)	(50.0)	(2.4)
Immigrant mother (percent)	8.6	7.5	-0.1
	(28.1)	(26.3)	(1.3)
Immigrant father (percent)	8.4	7.7	0.2
	(27.8)	(26.7)	(1.2)
1st child birth year	2,000.3	2,000.6	0.1
	(3.9)	(3.9)	(0.2)
1st child birth month	6.2	6.5	0.0
	(3.3)	(3.4)	(0.2)
2nd child birth year	2,003.1	2,003.1	0.1
	(3.9)	(3.9)	(0.2)
2nd child birth month	6.1	6.1	0.0
	(3.3)	(3.3)	(0.2)
N	133,075	8,768	8,768

Note: Average levels of covariates of entire population of couples and for couples near the 30-months threshold (+/- 30 days). Estimates of jumps in covariates at the threshold among those near the threshold. All variables that vary over time indicate the value the year before the first child's birth. The parents' income indicates their yearly labor earnings.

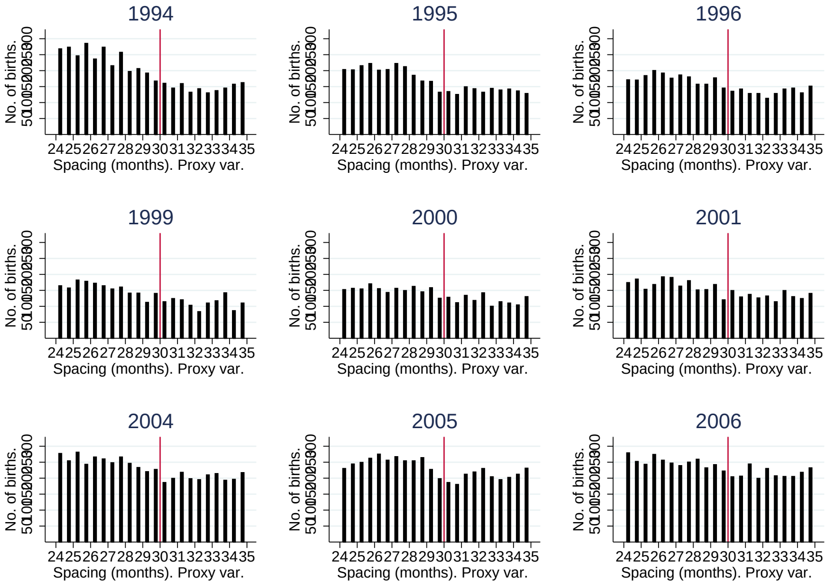


Figure A10. Frequencies of births over spacing over first child's birth year. Proxy variable for spacing.

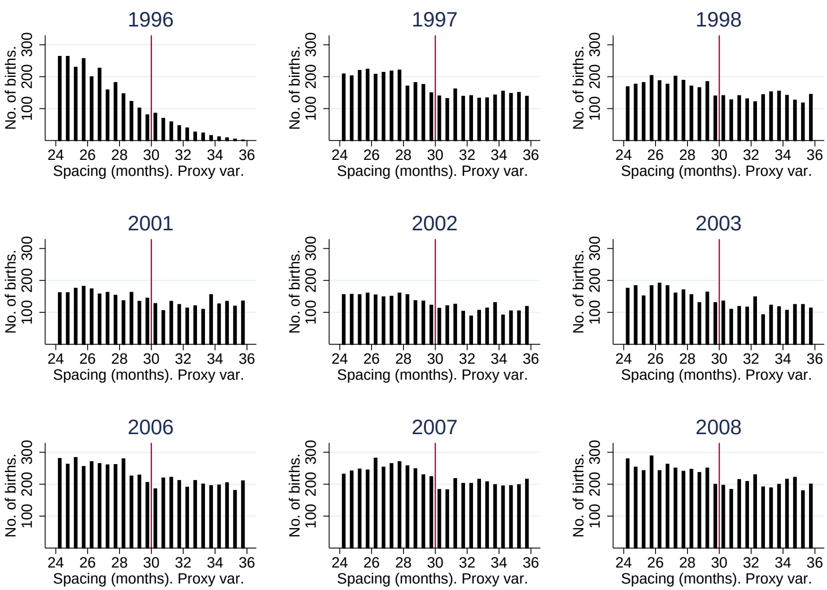
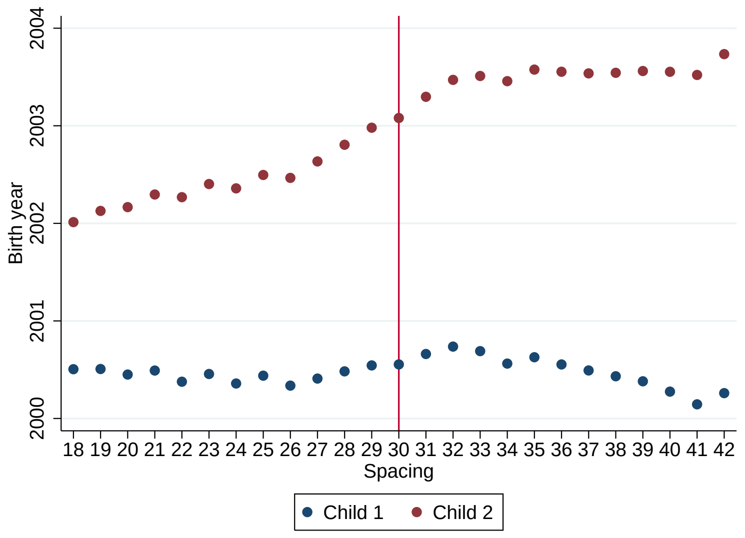
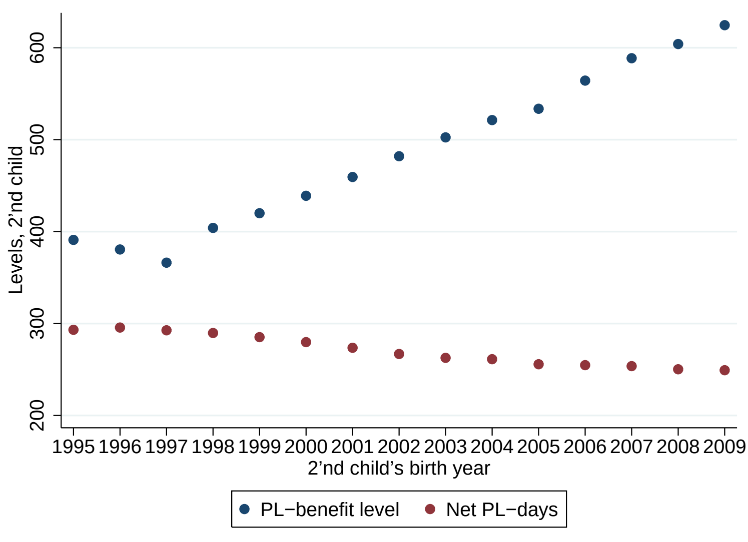


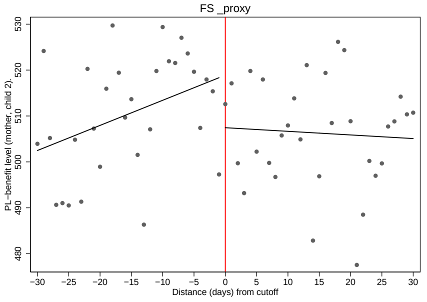
Figure A11. Frequencies of births over spacing over second child's birth year. Proxy variable for spacing.



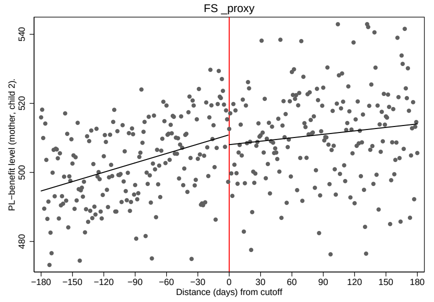
(a) Average birth year of first and second child over spacing. Figure reveals an upward trend in the second child's birth year around the 30-month threshold.



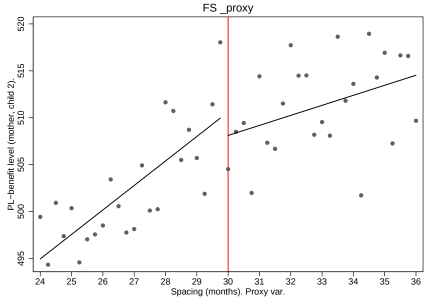
(b) Average levels of mothers' PL-benefits and take-up of net PL-days with the second child over second child's birth year.



(a) +/- 30 days from 30-month threshold. Bin size=1 days.

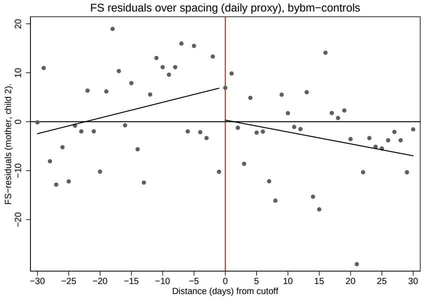


(b) +/- 180 days from 30-month threshold. Bin size=1 days.

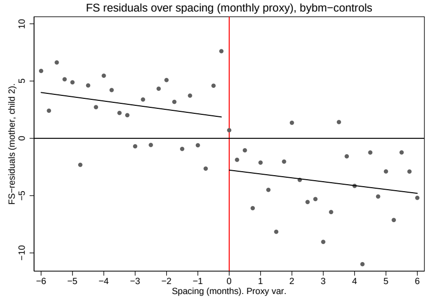


(c) +/- 6 months from 30-month threshold. Bin size=1 week.

Figure A13. First stage graphs (mothers). Figures (a) - (c) show the average PL-benefit level of mothers with the second child over child spacing (proxy variable).

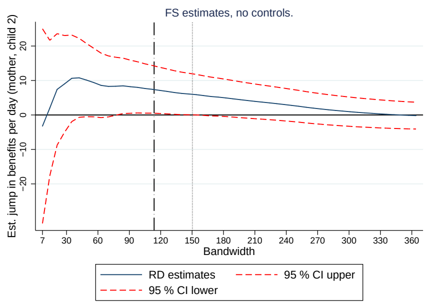


(a) +/- 30 days from 30-month threshold. Bin size=1 days.

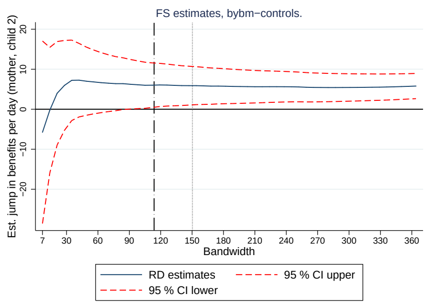


(b) +/- 6 months from 30-month threshold. Bin size=1 week.

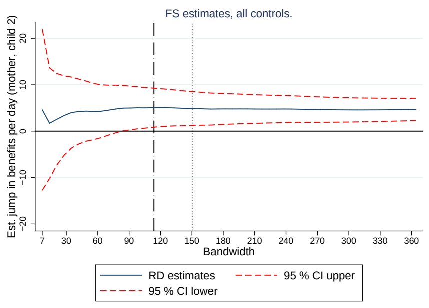
Figure A14. Residual plots from first stage regressions for mothers (mothers' average PL-benefit level with 2'nd child). Residuals when controlling for the child's birth year and birth month.



(a) No control variables. Bin size:1 days.

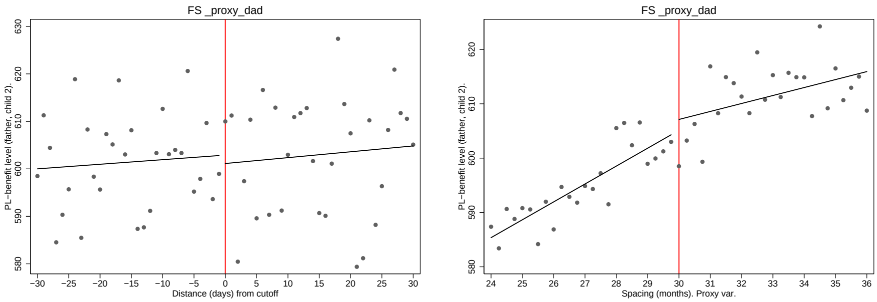


(b) Control variables for 2'nd child's birth year and birth month. Bin size:1 days.



(c) All control variables. Bin size:1 day.

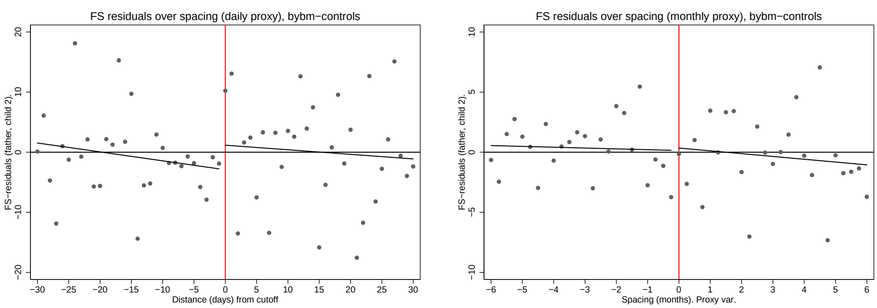
Figure A15. Figures (a) - (c) show the first stage estimate for mothers when using bandwidths between 7 and 365 days. The dashed and dotted lines indicate the bandwidths suggested by the CCT and IK criteria respectively.



(a) +/- 30 days from 30-month threshold. Bin size=1 days.

(b) +/- 6 months from 30-month threshold. Bin size=1 week.

Figure A16. First stage graphs (fathers). Figures (a) - (b) show the average PL-benefit level of fathers with the second child over child spacing (proxy variable).



(a) +/- 30 days from 30-month threshold. Bin size=1 days.

(b) +/- 6 months from 30-month threshold. Bin size=1 week.

Figure A17. Residual plots from first stage regressions for fathers (fathers' average PL-benefit level with 2'nd child). Residuals when controlling for the child's birth year and birth month.

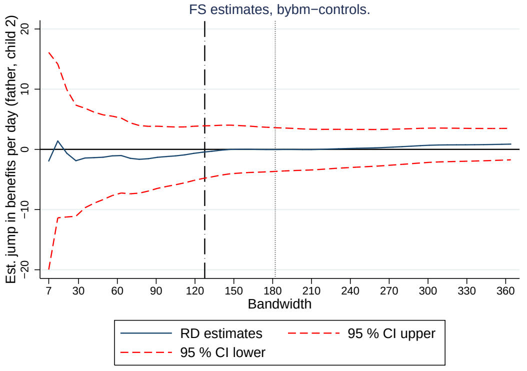
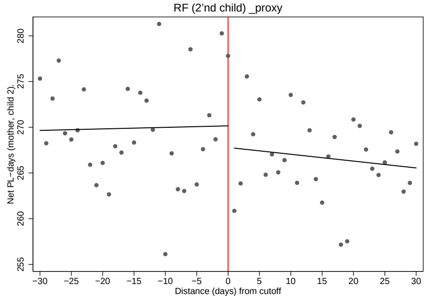
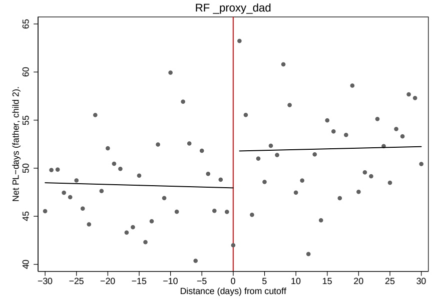


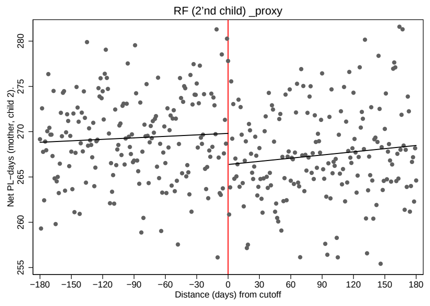
Figure A18. First stage estimate for fathers when using bandwidths between 7 and 365 days. Controls included for the second child's birth month and birth year. The dashed and dotted lines indicate the bandwidths suggested by the CCT and IK criteria respectively.



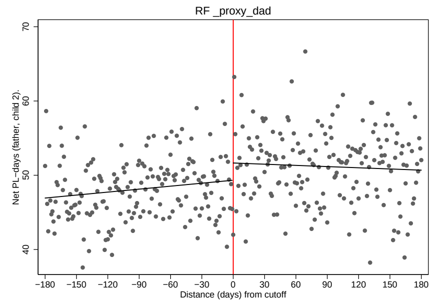
(a) RF graph - mothers, +/- 30 days from 30-month threshold. Bin size=1 days.



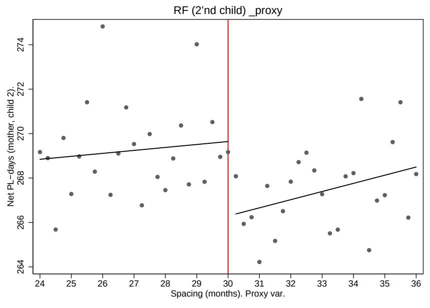
(b) RF graph - fathers, +/- 30 days from 30-month threshold. Bin size=1 days.



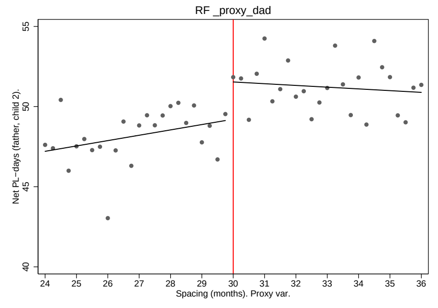
(c) RF graph - mothers, +/- 180 days from 30-month threshold. Bin size=1 days.



(d) RF graph - fathers, +/- 180 days from 30-month threshold. Bin size=1 days.

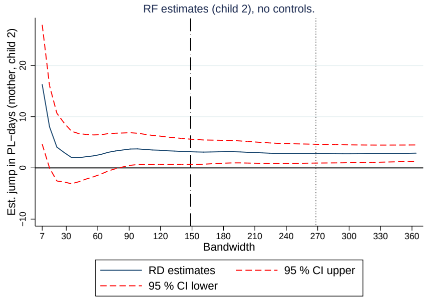


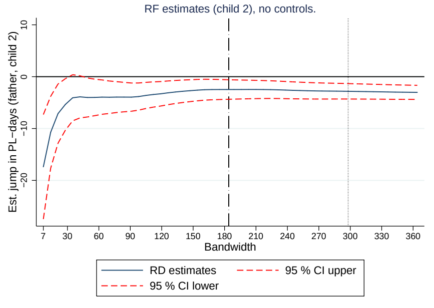
(e) RF graph - mothers, +/- 6 months from 30-month threshold. Bin size=1 week.



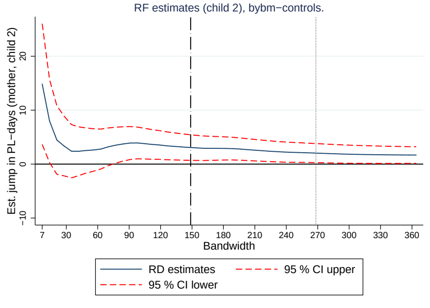
(f) RF graph - fathers, +/- 6 months from 30-month threshold. Bin size=1 week.

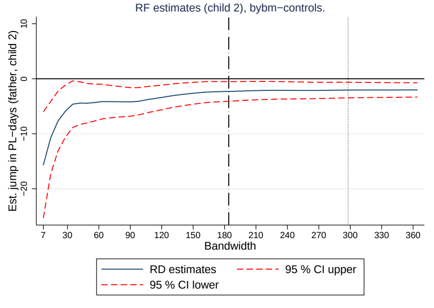
Figure A19. Reduced form graphs - mothers (graphs to the left) and fathers (graphs to the right). Figures show the parents' average take-up of PL-days with the second child over child spacing (proxy variable).





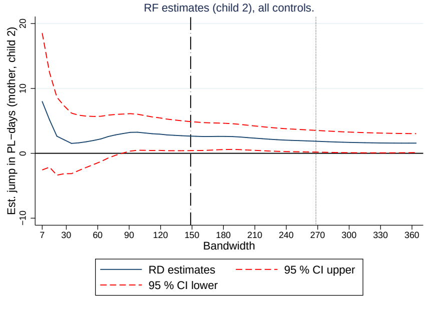
(a) No control variables. Bin size=1 days. *(b)* No control variables. Bin size=1 days.

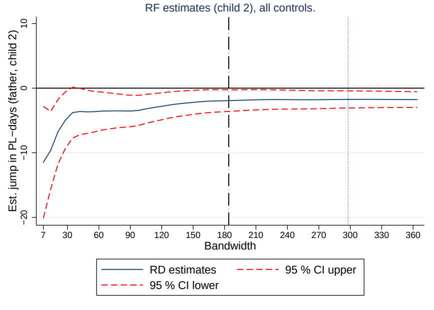




(c) Control variables for 2'nd child's birth year and birth month. Bin size=1 days.

(d) Control variables for 2'nd child's birth year and birth month. Bin size=1 days.





(e) All control variables. Bin size=1 day. *(f)* All control variables. Bin size=1 day.

Figure A20. Figures (a) - (f) show the reduced form estimate for mothers and fathers over increasing bandwidths (7 to 365 days). The dashed and dotted lines indicate the bandwidths suggested by the CCT and IK criteria respectively.

Table A2. *Mothers, direct effects (proxy variable).*

	OLS	FS	RF	SS	Mean	N
Net QI-days	-0.028***	4.902***	2.621**	0.535*	268.39	55,780
	(0.003)	(1.702)	(1.036)	(0.287)		
Total net days	-0.060***	4.902***	2.595**	0.529	295.59	55,780
	(0.004)	(1.702)	(1.292)	(0.333)		
Calendar days	-0.059***	4.902***	3.064**	0.625*	303.88	55,780
w benefits	(0.004)	(1.702)	(1.396)	(0.370)		
Mean benefit level		506.29				
First stage F-stat		8.30				

Table A3. *Fathers, direct effects (proxy variable).*

	OLS	FS	RF	Mean	N
Net QI-days	-0.023***	-0.313	-1.957**	49.52	55,690
	(0.003)	(1.600)	(0.864)		
Total net days	-0.038***	-0.313	-1.753*	54.16	55,690
	(0.003)	(1.600)	(0.955)		
Calendar days w benefits	-0.043***	-0.313	-1.844*	57.07	55,690
	(0.003)	(1.600)	(1.020)		
Mean benefit level		602.06			
First stage F-stat		0.04			

Table A4. *Fathers, cross spousal effects (proxy variable).*

	OLS	FS	RF	SS	Mean	N
Net QI-days	0.022***	4.902***	-1.957**	-0.399*	49.52	55,780
	(0.003)	(1.702)	(0.864)	(0.229)		
Total net days	0.029***	4.902***	-1.753*	-0.358	54.16	55,780
	(0.003)	(1.702)	(0.955)	(0.236)		
Calendar days	0.029***	4.902***	-1.844*	-0.376	57.07	55,780
w benefits	(0.003)	(1.702)	(1.020)	(0.251)		
Mean benefit level		506.29				
First stage F-stat		8.30				

Table A5. *Couple level effects (proxy variable).*

	OLS	FS	RF	SS	Mean	N
Net QI-days	-0.006	4.902***	0.665	0.136	317.91	55,780
	(0.003)	(1.702)	(0.845)	(0.179)		
Total net days	-0.032	4.902***	0.842	0.172	349.75	55,780
	(0.003)	(1.702)	(1.085)	(0.232)		
Calendar days w benefits	-0.030	4.902***	1.221	0.249	360.95	55,780
	(0.004)	(1.702)	(1.241)	(0.271)		
Mean benefit level		506.29				
First stage F-stat		8.30				

Note: The tables contain OLS, first stage, reduced form and second stage estimates of the direct effect on mothers (A2), the direct and cross-spousal effect on fathers (A3 and A4), and couple level effects (A5). Regressions are performed using the proxy variable.

Table A6. *Robustness checks of the direct effects on mothers.*

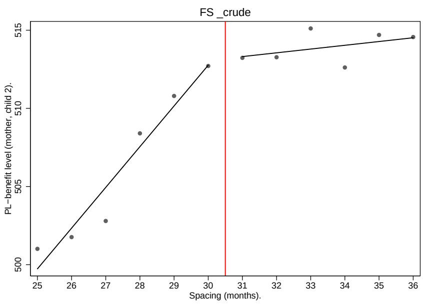
	FS, mothers	RF, net QI-days	SS, net QI-days	RF, total net days	SS, total net days	RF, cal days	SS, cal days	N
Baseline"	4.902***	2.621**	0.535*	2.595**	0.529*	3.064**	0.625*	55,780
	(1.702)	(1.036)	(0.287)	(1.292)	(0.333)	(1.396)	(0.370)	
	[8.298]							
No controls	5.026*	3.154**	0.628	3.314**	0.659	3.845**	0.765	55,780
	(2.773)	(1.141)	(0.470)	(1.456)	(0.543)	(1.528)	(0.596)	
	[3.287]							
BirthYnM	5.760**	2.906**	0.552	2.954**	0.569	3.437**	0.660	55,780
	(2.237)	(1.098)	(0.325)	(1.384)	(0.377)	(1.462)	(0.411)	
	[6.629]							
Daddydays	3.850*	2.980**	0.774	3.711**	0.964	3.733**	0.970	40,086
	(2.042)	(1.249)	(0.537)	(1.547)	(0.679)	(1.676)	(0.701)	
	[3.554]							
Random day	3.631**	2.146*	0.591	2.207*	0.608	2.380*	0.656	73,244
	(1.514)	(0.943)	(0.366)	(1.167)	(0.425)	(1.255)	(0.455)	
	[5.752]							
CCT (BW=149)	4.994***	2.670	0.535	2.207	0.442	2.511	0.503	46,279
	(1.871)	(1.139)	(0.310)	(1.422)	(0.340)	(1.538)	(0.372)	
	[7.128]							
IK (BW=268)	4.730***	1.872**	0.396*	1.979**	0.418*	2.645**	0.559*	80,476
	(1.404)	(0.853)	(0.220)	(1.064)	(0.265)	(1.146)	(0.303)	
	[11.350]							
Quadratic	5.042**	3.090	0.613	1.570	0.311	1.556	0.309	55,780
	(2.503)	(1.531)	(0.438)	(1.911)	(0.420)	(2.078)	(0.449)	
	[4.058]							
Cubic	4.974	3.410	0.686	1.379	0.277	1.297	0.261	55,780
	(3.310)	(2.025)	(0.622)	(2.528)	(0.553)	(2.771)	(0.594)	
	[2.259]							

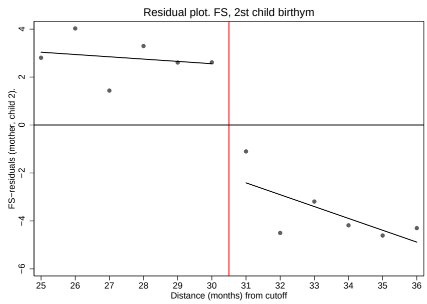
Note: The table contains robustness checks for the first stage, reduced form and second stage estimates of the direct effect on mothers. The first row reproduces the baseline results from table A2. The table shows the results when dropping all control variables, just controlling for the second child's year and month of birth, using the daddy-day proxy, including all observations by using a random day as proxy for the child's birthday, using the bandwidth recommended by Calonico et al. (2014) (CCT) and Imbens and Kalyanaraman (2011) (IK), and when including quadratic and cubic functional forms.

Table A7. *Robustness checks of the cross-spousal effects on fathers.*

	FS, mothers	RF, net QI-days	SS, net QI-days	RF, total net days	SS, total net days	RF, cal days	SS, cal days	N
Baseline"	4.902***	-1.957*	-0.399	-1.753*	-0.358	-1.844*	-0.376	55,780
	(1.702)	(0.864)	(0.229)	(0.955)	(0.236)	(1.020)	(0.251)	
	[8.298]							
No controls	5.026*	-2.474**	-0.492	-2.330**	-0.464	-2.450**	-0.487	55,780
	(2.773)	(0.972)	(0.384)	(1.065)	(0.384)	(1.134)	(0.405)	
	[3.287]							
BirthYnM	5.760**	-2.318**	-0.438	-2.168**	-0.414	-2.277**	-0.439	55,780
	(2.237)	(0.924)	(0.267)	(1.018)	(0.273)	(1.087)	(0.290)	
	[6.629]							
Daddydays	3.850*	-1.423	-0.369	-1.322	-0.343	-1.695	-0.440	40,086
	(2.042)	(1.027)	(0.339)	(1.135)	(0.355)	(1.212)	(0.402)	
	[3.554]							
Random day	3.631**	-1.620*	-0.446	-1.533*	-0.422	-1.637*	-0.451	73,244
	(1.514)	(0.771)	(0.287)	(0.849)	(0.299)	(0.907)	(0.319)	
	[5.752]							
CCT (BW=149)	4.994***	-2.237**	-0.448	-1.962**	-0.393	-2.204**	-0.441	46,279
	(1.871)	(0.951)	(0.259)	(1.052)	(0.263)	(1.122)	(0.285)	
	[7.128]							
IK (BW=268)	4.730***	-1.804**	-0.381*	-1.827**	-0.386*	-1.790**	-0.378*	80,476
	(1.404)	(0.710)	(0.192)	(0.785)	(0.206)	(0.840)	(0.215)	
	[11.350]							
Quadratic	5.042**	-3.360**	-0.666	-2.925**	-0.580	-3.460**	-0.686	55,780
	(2.503)	(1.282)	(0.424)	(1.416)	(0.411)	(1.507)	(0.463)	
	[4.058]							
Cubic	4.974	-4.924**	-0.990	-4.529**	-0.910	-5.007**	-1.007	55,780
	(3.310)	(1.705)	(0.753)	(1.880)	(0.728)	(1.995)	(0.796)	
	[2.259]							

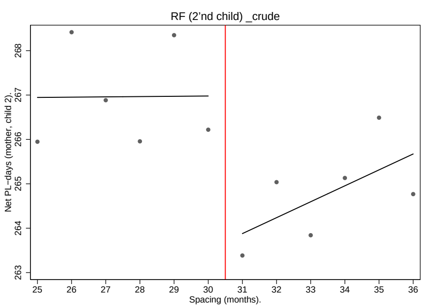
Note: The table contains robustness checks for the first stage, reduced form and second stage estimates of the cross-spousal effect on fathers. The first row reproduces the baseline results from table A3. The table shows the results when dropping all control variables, just controlling for the second child's year and month of birth, using the daddy-day proxy, including all observations by using a random day as proxy for the child's birthday, using the bandwidth recommended by Calonico et al. (2014) (CCT) and Imbens and Kalyanaraman (2011) (IK), and when including quadratic and cubic functional forms.

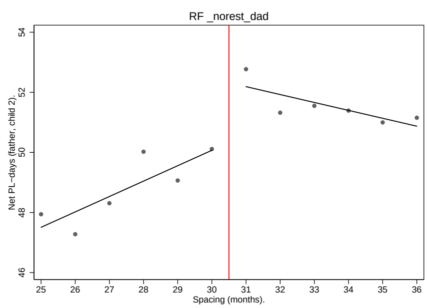




(a) FS graph - mothers, monthly spacing var.

(b) FS residual plot, mothers, monthly spacing var.





(c) RF graph - mothers, monthly spacing var.

(d) RF graph - fathers, monthly spacing var.

Figure A21. Graphical evidence when using the crude monthly spacing variable. All figures display the data for all observations +/- 6 months from 30-month threshold.

Table A8. *Mothers, direct effects (monthly spacing variable).*

	OLS	FS	RF	SS	Mean	N
Net QI-days	-0.027***	4.827***	2.549***	0.528**	266.00	72,000
	(0.003)	(1.548)	(0.960)	(0.267)		
Total net days	-0.058***	4.827***	3.182***	0.659*	292.69	72,000
	(0.004)	(1.548)	(1.185)	(0.337)		
Calendar days	-0.055***	4.827***	3.109**	0.644*	301.27	72,000
w benefits	(0.004)	(1.548)	(1.269)	(0.346)		
Mean benefit level		509.35				
First stage F-stat		9.73				

Table A9. *Fathers, direct effects (monthly spacing variable).*

	OLS	FS	RF	Mean	N
Net QI-days	-0.024***	0.347	-1.388*	49.95	71,854
	(0.003)	(1.435)	(0.784)		
Total net days	-0.038***	0.347	-1.517*	54.49	71,854
	(0.003)	(1.435)	(0.862)		
Calendar days	-0.043***	0.347	-1.914**	57.50	71,854
w benefits	(0.003)	(1.435)	(0.921)		
Mean benefit level		603.08			
First stage F-stat		0.06			

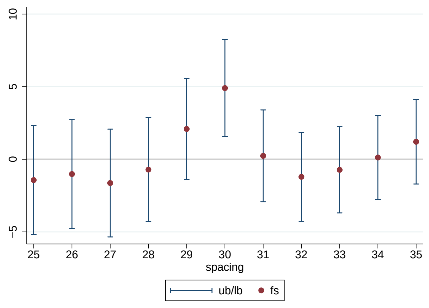
Table A10. *Fathers, cross spousal effects (monthly spacing variable).*

	OLS	FS	RF	SS	Mean	N
Net QI-days	0.018***	4.827***	-1.388*	-0.287	49.95	72,000
	(0.003)	(1.548)	(0.784)	(0.189)		
Total net days	0.025***	4.827***	-1.517*	-0.314	54.49	72,000
	(0.003)	(1.548)	(0.862)	(0.209)		
Calendar days	0.025***	4.827***	-1.914**	-0.397*	57.50	72,000
w benefits	(0.003)	(1.548)	(0.921)	(0.234)		
Mean benefit level		509.35				
First stage F-stat		9.73				

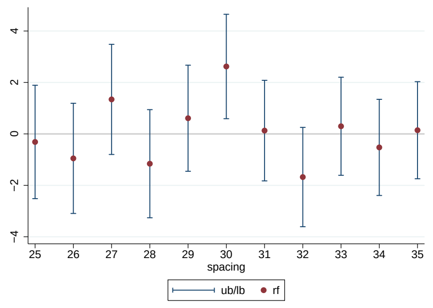
Table A11. *Couple level effects (monthly spacing variable).*

	OLS	FS	RF	SS	Mean	N
Net QI-days	-0.008	4.827***	1.162	0.241	315.95	72,000
	(0.002)	(1.548)	(0.787)	(0.182)		
Total net days	-0.033	4.827***	1.666*	0.345	347.18	72,000
	(0.003)	(1.548)	(0.998)	(0.240)		
Calendar days	-0.030	4.827***	1.195	0.248	358.77	72,000
w benefits	(0.003)	(1.548)	(1.125)	(0.250)		
Mean benefit level		509.35				
First stage F-stat		9.73				

Note: OLS, first stage, reduced form and second stage estimates of the direct effect on mothers (A2), the direct and cross-spousal effect on fathers (A3 and A4), and couple level effects (A5), using the crude monthly measurement of spacing.



(a) First stage estimates estimated at placebo thresholds.



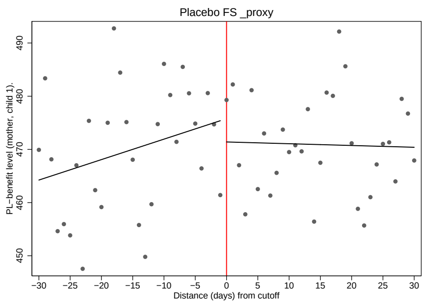
(b) Reduced form estimates estimated at placebo thresholds.

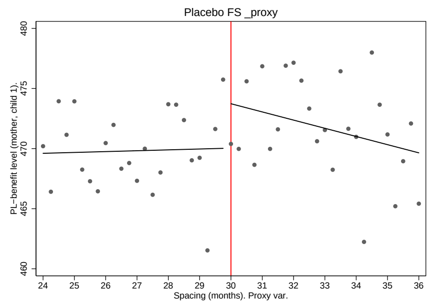
Figure A22. First stage (panel a) and reduced form (panel b) estimates, estimated at placebo thresholds, 25 to 35 months of spacing, and at the real threshold (at 30 months of spacing).

Table A12. *Placebo tests.*

	FS-estimate	RF-estimate
25	-1.433	-0.313
	(1.910)	(1.125)
	[0.562]	
26	-1.016	-0.951
	(1.906)	(1.093)
	[0.284]	
27	-1.635	1.342
	(1.894)	(1.092)
	[0.746]	
28	-0.711	-1.158
	(1.831)	(1.072)
	[0.151]	
29	2.088	0.609
	(1.781)	(1.052)
	[1.374]	
30	4.902***	2.621**
	(1.702)	(1.036)
	[8.298]	
31	0.236	0.129
	(1.614)	(0.997)
	[0.021]	
32	-1.206	-1.677*
	(1.562)	(0.985)
	[0.596]	
33	-0.726	0.297
	(1.513)	(0.973)
	[0.230]	
34	0.123	-0.524
	(1.478)	(0.953)
	[0.007]	
35	1.206	0.144
	(1.484)	(0.963)
	[0.661]	

Note: First stage and reduced form estimated at placebo thresholds, 25 to 35 months of spacing, and at the real threshold (at spacing equal to 30).

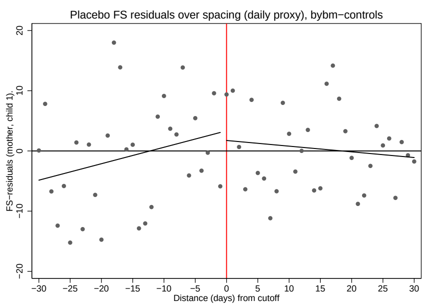


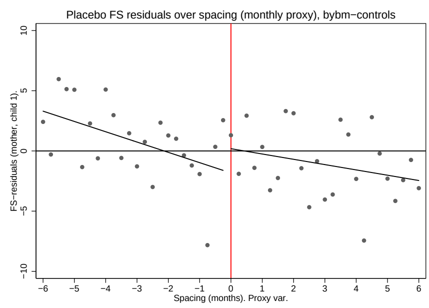


(a) +/- 30 days from 30-month threshold. Bin size=1 days.

(b) +/- 6 months from 30-month threshold. Bin size=1 week.

Figure A23. Placebo first stage (mothers). Figures (a) - (c) show the average PL-benefit level of mothers with the first child over child spacing (proxy variable).

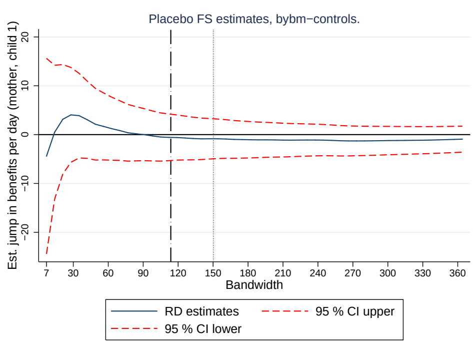




(a) +/- 30 days from 30-month threshold. Bin size=1 days.

(b) +/- 6 months from 30-month threshold. Bin size=1 week.

Figure A24. Residual plots – placebo first stage. Residuals of mothers' average PL-benefit level with 1'st child when controlling for the child's birth year and birth month.



(a) Control variables for 2'nd child's birth year and birth month. Bin size=1 days.

Figure A25. Figures (a) - (b) show the placebo first stage estimate for mothers (PL-benefit level with 1'st child) when using bandwidths between 7 and 365 days.

II. Does the Gender Composition in Couples Matter for the Division of Labor After Childbirth?

In this paper I compare the effect of entering parenthood in lesbian and heterosexual couples using Swedish population-wide register data. Comparing couples with similar pre-childbirth income gaps, a difference-in-differences strategy is used to to estimate the impact of the gender composition of the couple on the spousal income gap after childbirth. The results indicate that the gender of the parents' does matter for their division of labor as, five years after childbirth, the income gap is significantly smaller in lesbian than in heterosexual couples, also when comparing couples with the same pre-parenthood income gap. Part of the explanation is a difference in biological restrictions: lesbian partners often give birth to one child each and spend more time at home with the child they carried. Other explanations are the influence of gender norms and differences in preferences between lesbian and heterosexual couples.

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1 Introduction

Despite the often observed gendered pattern of specialization in the family, most studies in economics do not focus on the impact of spousal gender itself as an explanation for the division of labor. The uneven division of market and household work between spouses is often attributed to the fact that men are relatively more productive in the labor market (i.e. they have higher wages) which makes specialization beneficial for the household. However the prediction that the spouse with the highest earnings potentials will do less of the domestic work regardless of gender cannot be confirmed empirically. Akerlof and Kranton (2000) for example present data from the US that shows that when the wife's income share increases, the husband's share of housework does not increase in a proportional amount. Likewise, Procher et al. (2014) find that when the earnings level of the woman exceeds that of the man, the woman still does more of the housework. Several studies in sociology, for example Tichenor (2005), also find that even when the woman earns substantially more, she still does significantly more of the household chores. These results contradict the gender neutral predictions in most economic theory.

Descriptive statistics show that the gendered division of labor seems to become more pronounced when a couple enters parenthood (SCB, 2012). This conclusion has also been reached in empirical research for example by Dribe and Stanfors (2009) who show that the presence of small children in the household strengthens a traditional division of labor among parents in Sweden. After entering parenthood women increase their hours of work in the home and reduce their hours of work in the labor market. Men on the other hand, do not change their behavior in the same way (see Dribe and Stanfors, 2009, for an overview of this literature). Angelov et al. (2016) who also study Swedish parents, show that the within couple labor income gap widens after childbirth and that this is a long term effect. This pattern is mostly caused by a large decrease in women's market labor supply after becoming mothers.

In this paper I ask the question of how the gender of the spouses influences the division of labor after childbirth. The *influence of gender* refers to all the ways in which biological sex and gender norms can influence spousal behavior. The possibility to distinguish between different aspects of these forms of influence of gender will be discussed throughout the paper. To identify the impact of parental gender I compare the division of labor in lesbian and heterosexual couples before and after having children. I use the within family income gap as a proxy for the division of labor in the couple and compare the development of the spousal income gap in lesbian and heterosexual couples before and after childbirth. In this way I can investigate if the gender composition in couples seems to matter for the division of labor after becoming parents. By studying lesbian couples I can investigate the effect of having children on couples where, at least theoretically, one can make no a priori assumption on which parent "should" take the main responsibility for the care of children according to traditional gender norms. If a couple consisting of two women behaves differently as compared to a couple consisting of one man and one woman, then this could shed some light on whether the parental gender composition matters for the behavior of couples. Earlier studies comparing specialization within same-sex and different-sex couples have in general found that heterosexual couples specialize the most and lesbian couples the least. The results for gay male couples are more mixed (Antecol and Steinberger, 2013, Giddings et al., 2014, Jepsen and Jepsen, 2015, Tebaldi and Elmslie, 2006).

The data used in this study is Swedish population-wide register data that contains information on all lesbian and heterosexual couples who had their first child together at some time during the years 1995-2010. During this period, 502 lesbian and around 500,000 heterosexual couples who had their first child together can be observed in the data. Due to the small number of male same-sex couples with children (only 36 couples over a 15 year period) the analysis will be performed on lesbian and heterosexual couples only. To get comparable samples, lesbian and heterosexual couples are matched based on the birth year of the first child and the spousal income gap before childbirth. With this sampling technique only couples with the same pre-childbirth income gap and birth year of the first child are compared with each other in the empirical analysis. A difference-in-differences strategy is used to estimate whether there is a significant difference in the development of the spousal income gap after childbirth in lesbian and heterosexual couples given their pre-parenthood income gap and birth year of their first child. In this way I can estimate if there is a significant difference in the division of labor after becoming parents in couples with different gender composition. To the best of my knowledge this is the first study to compare the effect of entering parenthood in lesbian and heterosexual couples using population-wide panel data covering a long time period (20 years).

Heterosexual couples show increased specialization, in terms of a vast increase in their labor income gap, after becoming parents. This is a long term effect where the mother's income keeps falling behind, not catching up with the father's. The results suggest that fathers carry less of the financial cost in terms of foregone earnings and lost career opportunities associated with raising a child. In lesbian couples the income gap is in general smaller after childbirth also when comparing lesbian and heterosexual couples with the same pre-parenthood income gap. Initially the birth giving partner in both lesbian and heterosexual couples spends more time on parental leave compared to the non-birth giving partner. Consequently they experience a large decline in their labor market income in the years after childbirth. However, for most lesbian couples this initial increase in the income gap diminishes in the following years. The possibility to choose which partner should be the birth mother gives lesbian couples the opportunity to give birth to one child each. By taking turns giving birth, lesbian couples can share the costs in terms of foregone earnings when taking a longer time off work to stay on parental leave. The couples who do so have the most egalitarian division of income after entering parenthood.

The analysis shows that the parental gender composition matters for the division of labor after childbirth regardless of the pre-parenthood income gap. There are several possible explanations for this behavior. It might be that heterosexual couples use traditional gender norms as a "rule of thumb" when making their decision, while lesbian couples use some other principle such as fairness (Badgett, 2003, Blumstein and Schwartz, 1983). Possibly the spouses get a direct utility from acting according to gender norms and by seeing their partner do so as suggested by Akerlof and Kranton (2000). It might also be that the father in heterosexual couples enjoys a stronger bargaining position as a direct effect of his gender as proposed in for example Alesina et al. (2011). In lesbian couples, spousal gender could not directly influence the relative bargaining powers of the partners. Thus other factors such as relative earnings potentials might actually be more important for the barganing positions in these couples. The fact that in lesbian couples both partners can give birth to the child means that the decision making process when deciding to have children is different in these couples. The partners' preferences for biological motherhood can affect their decision and thereby the partners' income development after childbirth.

The remaining part of the paper is organized as follows; section 2 discusses related literature; section 3 gives a background on the institutional setting for families in Sweden; section 4 provides the identification strategy and econometric method; section 5 presents the data and descriptive statistics; in section 6 results from the main regression analysis are presented; section 7 investigates possible mechanisms behind the results. Finally, section 7 provides summary and concluding remarks.

2 Related literature

Becker (1991) argues that in an efficient family the spouses should allocate their time according to their comparative advantages in order to maximize the total income and household production of the family. Becker assumed that for biological reasons and due to differences in human capital investments between the sexes, women have a higher productivity in household work and men in the labor market. For this reason a gender specific division of labor in the household is the most efficient and beneficial for both spouses. Becker further stresses the point that since women make a larger biological investment in their children (i.e. going through pregnancy, labor and breast feeding) they will be more keen to invest time and money in their children later on in order to get an adequate return to their investment. Thus, according to Becker's theory, the fact that women give birth to children in itself leads to specialization within households after childbirth.

The literature on bargaining in the family takes a somewhat different approach. The division of market and household work is assumed to be determined by differences in bargaining power between the spouses. The bargaining power is determined by whatever determines the outside option in case of divorce for example age, nonlabor income or earnings capacity (see e.g. Apps and Rees, 1997, Chiappori, 1988, 1992, Oreffice, 2011, or Procher et al., 2014). Gender is typically not considered to have a direct effect on bargaining power but at most an indirect effect by for example affecting the earnings potentials of the individual. A common prediction of these models is that the spouse with the highest income or earnings potential will enjoy stronger bargaining power due to a better outside option, and consequently do less of the unpaid domestic work. Since the theory is gender neutral the models predict that if the relative earnings of the spouses are reversed, the proportion of domestic work should consequently also be reversed. However this prediction cannot be confirmed empirically. Akerlof and Kranton (2000) for example present data from the US that shows that when the wife's income share increases, the husband's share of housework does not increase by a proportional amount. Even when only the wife is working in the market the husband's share of housework is only around 35 percent. Procher et al. (2014) find that an increase in the share and/or absolute level of income of a spouse is associated with a decrease in housework for both men and women. However they also find that when the earnings level of the woman exceeds that of the man, the woman's amount of housework again increases. The same result has been found in several studies in sociology. For example Tichenor (2005) finds that when the woman earns substantially more, she still does significantly more of the household chores. The author explains this as a way to reestablish conventional gender norms in the family. Bertrand et al. (2013) study the relationship between gender and the effect of spousal gap in real and potential earnings. They find that if the wife's potential earnings exceed her husband's, the likelihood that she is employed decreases, and that if she does work her real earnings are more likely to be below her potential. These results all contradict the gender neutral predictions in most of the standard family economic theory, but can be explained if the importance of gender is incorporated into the theoretical framework. In a much cited paper, Akerlof and Kranton (2000) consider how a person's identity, for example gender identity, can be added to standard economic models (in the utility function) to better explain economic behavior. The authors propose a model where identities such as "man" or "woman" are prescribed to the individual. Norms regarding the prescribed identity affect both the preferences for and the payoffs of different actions for the individuals who act to confirm their identity as this gives them utility. Acting to confirm your prescribed identity can also lead to externalities as it gives others utility to see you do so. Likewise, violating the norms will result in a feeling of disutility both for the individual him/herself and other people. This framework can explain why individuals sometimes act in a way that seems not to be in their best economic interest. The model proposed by Akerlof and Kranton (2000) predicts an asymmetric division of labor between the spouses. Adding gender identity to a standard model can explain why a woman, even if she is the main financial provider of the family, chooses to do more of the housework to confirm her gender identity. Similarly a man with a lower income than his wife might avoid "feminine" tasks in the household to maintain his self-image as a man.

Hypothetically same-sex couples could "take on" different gender roles and divide the household work in a similar fashion as heterosexual couples. However studies show that same-sex couples who do take on gendered roles (for example "butch" and "femme") do not assign household tasks according to these roles (Badgett, 2003, p. 157, Blumstein and Schwartz, 1983). Studies comparing hours spent on household chores in different types of couples have found that same-sex couples are more likely to share household tasks. Another consistent finding is that heterosexual women spend substantially more time on household chores than their husbands. Blumstein and Schwartz (1983) find, in one of the first studies on same-sex couples, that they more often than heterosexual couples prefer that both partners have an income. They suggest that one reason for this is that same-sex couples value a more fair financial situation. Kurdek (2007) find a more egalitarian division of labor in same-sex couples and suggests that this is because same-sex couples are more committed to "an ethic of equality in their relationships". In a survey of the research on same-sex couples in sociology and psychology in the US, Peplau and Fingerhut (2007) conclude that same-sex couples, in particular lesbian couples, have more egalitarian ideals than heterosexual couples. When studying the actual division of household chores, same-sex couples were indeed more egalitarian than the opposite-sex couple but they did not always live up to their own ideals as well as they thought.

Relevant for this paper are the studies on differences in earnings for individuals of different sexual orientation. Common findings in this literature is that gay men earn less than heterosexual men and that lesbians earn more than heterosexual women also when controlling for many individual and household characteristics (see for example Black et al., 2003, Plug and Berkhout, 2004 and Ahmed et al., 2011). Possible explanations are discrimination, employers' expectation about higher or lower productivity of homosexuals due to stereotypes, or that homo- and heterosexuals have different preferences for work and leisure. Berg and Lien (2002) suggest that expectations about your partner's income can explain differences in the labor supply of hetero- and homosexuals. Given the fact that men have on average higher earnings than women, heterosexual women can expect a higher total household income when cohabiting with a partner than lesbian women can (and the opposite is true for heterosexual men). Once they are cohabiting the income effect caused by the partner's income is on average larger for heterosexual women than for lesbians. This gives lesbian women stronger incentives to work relative to heterosexual women. Since this would affect all lesbian women in the same way this argument does not predict any specialization within lesbian households. Becker (1991) also predicts less specialization in same-sex couples since the partners in same-sex couples have more similar human capital. Same-sex couples, Becker says, cannot enjoy the benefits of specialization based on gender differences in comparative advantages the way heterosexual couples can. Earlier empirical studies comparing specialization within same-sex and different-sex couples have in general found that heterosexual couples specialize the most and lesbian couples the least. The results for gay male couples have been somewhat more varied. Tebaldi and Elmslie (2006) finds that, among individuals who are married or cohabiting, lesbians are more likely to work and to work full time than heterosexual women. Gay men, on the other hand, are less likely to be employed and more likely to work part time if they work than heterosexual men. A consistent finding is that heterosexual men are the most likely to be employed, that heterosexual women are the least likely, and that heterosexual couples are most likely to have only one partner employed (Leppel, 2009, Black et al., 2007). Leppel (2009) further finds that the presence of young children decrease the probability of employment for heterosexual women and gay men but not for lesbians and heterosexual men.

Antecol and Steinberger (2013) stresses the point that when comparing same-sex and different-sex couples it is important to also take into account differences in labor supply between the partners in same-sex couples. They compare the earnings of primary and secondary earners in lesbian couples and compare them with the earnings of men and women in heterosexual couples. They find that the within couple income gap is smaller in lesbian couples but that the lesbian primary earners' labor supply is closer to that of heterosexual men and that lesbian secondary earners' labor supply is closer to that of married heterosexual women. They also find that the presence of children can explain a large part of the unconditional difference in labor supply between secondary earner lesbian partners and married heterosexual women. Giddings et al. (2014) find that same-sex couples are less likely to choose a high degree of specialization, also when controlling for the presence of children. They also find that the "specialization gap" between same-sex and different-sex couples has narrowed substantially across cohorts with smaller differences between same-sex and different-sex couples of younger generations. Jepsen and Jepsen (2015) studies earnings-gaps as a proxy for specialization and find that heterosexual couples have the largest gaps, lesbian couples have the smallest gaps and gay male couples are more similar to heterosexual couples. When it comes to hours worked however male same-sex couples are more similar to lesbian couples who in general have small differences in hours worked.

Most of the studies mentioned above use data from the U.S. There are however a few studies comparing the financial situation for same-sex and different-sex couples in Sweden. Ahmed et al. (2011) compare the individual, household and within couple differences in earnings in gay, lesbian and heterosexual couples in Sweden. They find that lesbian couples have more equal earnings than heterosexual couples, and that gay couples have the largest within couple income gaps. They also show that the total household income of lesbian couples is lower than that of both heterosexual and gay households. Aldén et al. (2015) studies the effects of entering registered partnership or marriage for couples of different gender compositions in Sweden. They find that entering a legal union had similar positive effects on fertility for lesbian and heterosexual couples but that the earnings gap within couples became larger only in heterosexual couples. Andersson et al. (2006) study the demographic characteristics of same-sex couples in registered partnerships in Norway and Sweden. They find that lesbian couples are more similar in terms of age, education and annual earnings than male couples. They also find that the risk of separation is larger in lesbian couples: 30 % separate within five years compared to 20 % of the male couples. The corresponding number for heterosexual couples is 13 %.

The presence of children seems to be particularly important as a cause of increased specialization, at least for heterosexual couples. Angelov et al. (2016) shows that in Sweden the within couple labor income gap is widened by 32 percentage points 15 years after childbirth, as compared to the income gap before the couple had children. This is mostly caused by a large decrease in women's market labor supply when becoming mothers. The wages are affected only in the long run. The gender wage gap has increased by 10 percentage points 15 years after the first child is born. Angelov et al. (2016) also show that the spousal differences in pre-childbirth income and years of schooling matter for the spouses' division of labor post childbirth. They use quantile regressions to study the effect of the size in pre-parenthood income gap on the income gap post childbirth. In households where the woman's income is relatively higher than the man's, the spouses have a more even division of labor, but the woman still bears a larger financial cost of parenthood. In a study on Danish register data, Kleven et al. (2015) show that the female child penalty in Denmark of around 20 percentage points, 10 - 20 years after childbirth, compared to the earnings level before childbirth. There is no evidence of an earnings penalty for men when becoming fathers. Their results indicate that most of the remaining gender gap in earnings in Denmark can be explained by the dynamic effect of having children.

For lesbian couples the link between entering parenthood and an increased specialization is not as clear. Since there is no apparent way to assign child care and housework according to traditional gender norms within a couple consisting of two women, other factors such as economic opportunities or preferences might be more important as determining factors in these couples. Earlier studies have however found that biology also matters in these couples in the way that the biological mother, who gave birth to the child, typically spends more time taking care of the child she gave birth to. Still, the non-biological mother spends more time with the child than heterosexual fathers, and in general lesbian parents divide the child care in a more equal way than heterosexual couples (Badgett, 2003, p. 159). This result has been found in a number of studies for example in Reimann (1997) who studies 25 middle-class lesbian couples with children in the US through in-depth interviews. She concludes that the couples' division of labor is affected mostly by the partners' preferences, economic considerations, and a strong commitment to shared motherhood and equality. Her analysis shows that biological motherhood initially gives the birth mother a closer bond to the child, but that this usually does not result in long term specialization of labor between the biological and non-biological mother. Patterson et al. (2004) compare the division of labor in 33 lesbian and 33 heterosexual parenting couples in the U.S.. They conclude that the lesbian couples, influenced by ideals about equality, were more likely to divide paid and unpaid work evenly, while heterosexual couples were more likely to specialize, mostly because of the better income opportunities of men in the labor market. Tornello et al. (2015) study the division of labor among 335 self-defined gay men in the U.S. with children living in the household. They, like lesbian couples, reported egalitarian ideals about, as well as actual equality in, the division of labor in their relationships. In a resent dissertation in psychology, Malmquist (2015) conducts in-depth interviews with lesbian couples with children in Sweden. In most cases, both partners spend long periods on parental leave, the birth mother typically taking the first period of leave. The couples often motivated their choice with the importance of equality between the partners and giving both parents a chance to form a close relationship with the child. Some couples perceived equality within the relationship when having children as a spontaneous achievement while others described it as the outcome of hard work. A minority of the couples divided the parental leave and parental roles in a way more similar to those in a traditional heterosexual couple, where the birth mother was seen as the child's primary parent.

3 Institutional setting for parents in Sweden

In the following section there will be a short description of the rights and legal conditions for same-sex and opposite-sex couples with children in Sweden.[1]

[1]The information presented below is gathered mostly from the official homepages of the Swedish government, the Swedish parliament (Riksdagen), the Swedish Tax Agency (Skatteverket), The National Board of Health and Welfare (Socialstyrelsen), The Swedish Social Insurance Agency (Försäkringskassan), The Swedish Federation for Lesbian, Gay, Bisexual and Transgender Rights (RFSL) and other LGBT organizations.

3.1 Parental leave and other rights and benefits for Swedish parents

The Swedish parental leave system is one of the most generous in the world and was first introduced in 1974. For each child the parents can take at most 480 days of parental leave. Out of these, 390 days have a replacement rate that is proportional to each parent's income and for the other 90 days the replacement rate is on a basic level. Out of the 390 days at a higher replacement rate 60 are tied to each parent. The rest of the days can be split in any way the couple chooses. In order to utilize the parental leave the individual must be a legal parent of the child or live with the child's legal parent and either 1) already have children together, 2) be married or 3) be registered partners. Thus it is possible to take up parental leave benefits also for a partner who is not (yet) legally recognized as the child's parent. However this opportunity has been open to same-sex partners only since 2003.

Parents have a legal right to stay at home full time with job protection until the child is 18 months old. Thereafter the parents have the right to reduce their working hours up to 25 percent until the child turns 8 years old. If the child is sick and cannot attend school/pre-school the parents or another adult who has a close relation to the child can stay at home from work taking up temporary parental leave for a maximum of 120 days a year. The replacement rate for the temporary parental leave is proportional to each parent's income.

The extensive rights of Swedish parents thus give great opportunities for both parents to combine a career in the labor market with taking an active part in the child care. However women use around 75 percent of the parental leave days, and work part time when having small children to a much greater extent than do men.

3.2 Legal conditions for same-sex couples with children

Nowadays same-sex and opposite-sex couples enjoy practically the same rights when it comes to marriage, inheritance, parental rights and so on. However this has only been the case for the last few years. Same-sex relationships were first legally recognized in Sweden in the Homosexual Cohabitees Act (Lagen om Homosexuella Sambor) in 1988 and in the Registered Partnership Act (Partnerskapslagen) in 1995. The Registered Partnership Act gave same-sex couples who registered as partners some of the rights of married couples but did not enable them to for example both become legal parents of the same child.

In February 2003 the law was changed so that a child could have two legal parents of the same gender. This meant that same-sex couples could adopt children together and that it was possible to adopt your registered partner's biological child. In almost all of the same-sex adoption cases since 2003 the adopting parent has adopted his or her partner's biological child. It has turned out to be very difficult for same-sex couples to adopt children that are not the biological child of either one of the partners, and very few such adoptions have occurred.

In July 2003 the Cohabitees Act (Sambolagen) was changed so that same-sex couples could be legally recognized as cohabitees under the same legislation as heterosexual couples. According to the Cohabitees Act, if a cohabiting couple has a child the birth mother of the child automatically becomes a legal parent. Her partner on the other hand has to register as the child's parent at the Swedish Tax Agency (Skatteverket). Before being able to register, the parenthood is determined by the Social Welfare Committee (Socialnämnden) in the municipality where the child lives. After the registration the birth mother's partner is also recognized as the child's legal parent. The registration can be done even before the child is born.

The process of registering a partner as the child's legal parent sounds pretty straight forward. However for same-sex couples that is not always the case. The female partner of a birth mother can only register as a child's parent if the child has been conceived by insemination or IVF at a Swedish clinic. Insemination for lesbian couples has only been available in Sweden since July 2005[2]. If the child has been conceived through insemination in another country or with the help of a private donor, the non-biological parent has to adopt her partner's child in order to become the child's legal parent. The adoption process can start after the child is born and usually takes between a few months up to a year. However if the parents are registered partners the non-biological mother can also stay on parental leave during the adoption process.

For male same-sex couples it is still rather difficult to become parents. In most cases male same-sex couples have had children through surrogacy abroad (surrogacy is not legal in Sweden). In these cases the biological father can register as the child's legal parent while his spouse has to adopt the child.

In May 2009 same-sex marriage became legal in Sweden. Formally all married couples have the same rights. However when an opposite-sex couple has a child the husband is assumed to be the father and is automatically given all parental rights. Married same-sex couples still have to register as parents or adopt their spouse's biological child to both become legal parents.

[2] The couples must be married, cohabiting or registered partners to get access to the treatment. Formally the law about insemination and IVF does not differ for lesbian and heterosexual couples. However the counties, who are responsible for health care services, have often chosen to treat lesbian and heterosexual couples differently. In some cases lesbian couples have had to pay up to 12,000 SEK per insemination while the fee for opposite-sex couple has been much lower (a few hundred SEK). In some counties lesbian couples have not gotten as many attempts of conceiving as have heterosexual couples. These rules have become more equal in the last few years.

4 Identification and empirical strategy

4.1 Estimating the effect of parenthood on the division of labor in couples

To investigate the effect of entering parenthood on the division of labor in couples, labor market income will be used as a proxy for the labor supply of each spouse. The within couple difference in labor market income will be used as a measure of the difference in spousal labor supply. Labor income can be said to be a measure of both effort (wage) and the amount of work (hours). However when studying changes in labor income over time for the same individual this can be interpreted as changes in the amount of work, rather than changes in effort, especially if there is a large increase or decrease in labor income comparing two consecutive years.

The empirical model presented below, equation 15, (which is much like the one used in Angelov et al. 2016) estimates the effect of entering parenthood, which happens at time period 0, on the income gap between spouses 1 and 2 in couple i:

$$(lny_1 - lny_2)_{it} = c + \gamma(lny_1 - lny_2)_{i-2} + \sum_{k=0}^{T} \alpha_k 1_{t=k} + \beta X_{it} + \varepsilon_{it} \tag{1}$$

where $t \geq$ -1, i denotes the couples, X_{it} is a vector of control variables for couple i at time t and ε_{it} is an error term that measures couple and time specific heterogeneity.

The dependent variable is the difference in log income between spouses *1* and *2* in couple i at time t. By using the log transformation, the spousal income gap can be interpreted as the percentage difference in income. The model controls for the pre-childbirth income gap in time period -2. The estimated α_k can be interpreted as the percentage change in the income gap at time t compared to the gap in the pre-childbirth period. This specification identifies the effect of an event at time 0 (childbirth) on the difference in income between spouses. The assumption of strict exogeneity of the treatment on the outcome variable in this case implies that the time of childbirth needs to be exogenous to the changes in the spousal income gap. Under the assumption that the timing of childbirth is not induced by expectations about changes in the spousal income gap, the α_k parameters for time periods $t = 0$ to T identify the effects of entering parenthood on the within couple income gap for each year after the child is born. By studying the population of couples who have children together, the choice to form a family is left outside the analysis.

4.2 Estimating the impact of gender composition in couples on the division of labor after childbirth

To study the impact of the parents' gender, I compare the behavior of couples with different gender composition. More specifically I compare the division of labor in lesbian and heterosexual couples before and after having children. For the purposes of this paper, identification rests on the assumption that sexual orientation does not determine the individual's ability in the labor market after childbirth. Under this assumption it is possible to estimate the impact of the parental gender composition on the division of labor after childbirth by comparing lesbian and heterosexual couples, similar in observable pre-childbirth characteristics. The following difference-in-differences model captures the impact of the gender composition in couples on the change in the income gap after entering parenthood:

$$\begin{aligned}(lny_1 - lny_2)_{its} = c + \gamma(lny_1 - lny_2)_{i-2s} + \theta_s + \sum_{k=0}^{T} \alpha_k 1_{t=k} \\ + \sum_{k=0}^{T} \lambda_{sk} 1_{t=k} + \beta X_{it} + \varepsilon_{it}\end{aligned} \tag{2}$$

As before the α_k parameters identify the effect of parenthood on the within couple income gap for each year after the child is born. θ_s is a dummy for the gender composition of couple i (where $\theta_s = 1$ if lesbian, $\theta_s = 0$ if heterosexual) and captures the difference in income gap between lesbian and heterosexual couples in the year before childbirth. The λ_{sk} parameters capture the interaction effect of being a lesbian couple in a specific time period compared to being a heterosexual couple in that period. In other words they estimate the difference between lesbian and heterosexual couples in the change in the income gap between time t=-1 and $t = k$. Hence the α_k parameters identify the baseline effect of parenthood for heterosexual couples. In order to identify the impact of the gender composition on the division of labor in lesbian and heterosexual couples after becoming parents, it is important to estimate equation 11 only on lesbian and heterosexual couples who had similar specialization patterns before childbirth. In this setting, this is equivalent to the assumption of parallel trends in the outcome variable before treatment. For the parallel trends assumption to be valid, the income gaps in lesbian and heterosexual couples must develop in the same way before the "treatment" (in this case having children). That is, the income gap cannot be for example growing in heterosexual couples while diminishing in lesbian couples in the years before childbirth. If so the model does not estimate the difference in the effect of entering parenthood between the two types of couples but rather just captures a general difference in trends in the development of the income gaps that started already before childbirth. Even if sexual orientation can be assumed to be "exogenous" in the ways that it is not a choice variable or endogenously determined by life events after birth, the sample of lesbian and heterosexual couples cannot be assumed to be a random draw from the populations of couples in Sweden. There is a risk that lesbian and heterosexual couples might be systematically different in characteristics other than the gender composition that can affect the income gap.

To get comparable samples I match lesbian and heterosexual couples based on pre-parenthood characteristics that may determine the income gap in couples. In this way I can estimate the model on a sample of lesbian and heterosexual couples that are as similar as possible in labor market characteristics before becoming parents. I use a matching method similar to the one in Mörk et al. (2013) and Lundin et al. (2008). All couples are divided into J different household types, one type for each possible combination of values of specific household characteristics. Observations in household types in which there is no common support (both lesbian and heterosexual couples) are dropped from the sample. The following model is estimated on the sample of matched couples:

$$\begin{aligned}(lny_1 - lny_2)_{its} = c + \gamma(lny_1 - lny_2)_{i-2s} + \theta_s + \sum_{k=0}^{T} \alpha_k 1_{t=k} \\ + \sum_{k=0}^{T} \lambda_{sk} 1_{t=k} + \beta X_{it} + FE_j + \varepsilon_{jt}\end{aligned} \tag{3}$$

As before the α_k parameters identify the effect of parenthood on the within couple income gap for each year after the child is born for the baseline heterosexual couples. θ_s is a dummy for the gender composition of the couples that in this equation captures the average difference in income gap between lesbian and heterosexual couples before childbirth within a household type.

By adding household type fixed effects, the λ_{sk} parameters are estimated comparing only lesbian and heterosexual couples within the same household type. In this way it is possible to control for household characteristics that may affect the pre-childbirth income gap and differ systematically between the two types of couples. For the λ_{st} parameters to be unbiased, the parallel trends assumption needs to be fulfilled. In this setting this means that there can be no systematic difference between lesbian and heterosexual couples in the development of the income gap within the same household type before childbirth. If this assumption is fulfilled then the λ_{st} parameters can be said to estimate the impact of the gender composition on the change in the spousal income gap post childbirth comparing couples within the same household type. The error terms are clustered at the household type level and measure household type time specific heterogeneity.

4.3 Defining the income gap within couples

In most studies on heterosexual couples the income gap is defined as the man's income minus the woman's. To compare the income gaps in lesbian and heterosexual couples the gaps in both types of couples, in my opinion, need to be measured in the same way. (However this is actually not the case in many studies comparing same-sex and different-sex couples.) In this study the main question is about the impact of gender. Thus it seems natural to divide the heterosexual couples according to this variable. That is to define the income gap as the man's income minus the woman's. Since in heterosexual couples it is always one partner (the woman) who gives birth to the child, defining the partners in lesbian couples by their birth giving status seems like the obvious choice. Thus, henceforth spouses 1 and 2 in the empirical model are defined as the man (1) and the woman (2) for the heterosexual couples and as the partner (1) and the birth mother (2) in lesbian couples.

4.4 Possible threats to identification

The basic assumption for λ_{sk} to capture the impact of the gender composition in couples on the spousal income gap post childbirth, is that given the same pre-childbirth characteristics in a household type any difference in behavior between lesbian and heterosexual couples is due to the difference in gender composition of the couples. However there are reasons to believe that even when comparing lesbian and heterosexual couples that have been matched on observed pre-childbirth characteristics they may differ in ways that could affect labor supply and the division of labor after having children.

One obvious difference is that the partners in lesbian couples can choose which of the partners is to give birth to the child. Also, for lesbian couples, becoming parents is more often a planned event. This means that the "treatment" of becoming parents is not exactly the same thing for the two types of couples. Lesbian couples may choose the partner who is best suited to give birth based on health reasons, labor market position or the partners' preferences for giving birth. The choice may also be the result of a within couple bargaining. In that case the partner with stronger bargaining power may bargain to give birth or not to do so depending on her preferences. She may also bargain for a certain division of labor after childbirth. Thus the decision process behind the choice of birth mother could be correlated with the division of labor after childbirth.

While heterosexual relationships are seldom questioned, the choice to enter a same-sex relationship may cause some social frictions and discrimination towards the individual. The choice to "come out" may be endogenous to other variables such as family background, financial independence and living area. There is probably a selection among gays and lesbians in the choice to enter such a relationship and have children together. Embracing your sexual orientation may also in itself affect the individual in many ways, for example by inducing more liberal values on family life. There are thus arguments to believe that lesbians and heterosexuals who form families may be different also in unobserved characteristics.

Besides the differences in characteristics between lesbian and heterosexual couples there may also be differences in how external factors affect the couples. For example, labor market opportunities may be affected by discriminatory attitudes among employers towards women and LGBT-persons. Gender discrimination in the labor market would affect both spouses in a lesbian couple, but only one of the spouses of an opposite-sex couple. Discrimination due to sexual orientation only affects lesbian couples. However such discrimination would be present also before childbirth. Thus discrimination due to sexual orientation is only a concern if it leads employers to treat employees differently after they become parents.

There are thus reasons to believe that lesbian and heterosexual couples, even though similar in many observable characteristics, still will differ in ways that affect the division of labor after childbirth. Therefore, estimates achieved using the strategy described above cannot be interpreted as a precise estimate of the causal effect of the gender composition in couples, but can still give an informative measurement on the difference in behavior between lesbian and heterosexual couples.

5 Data, descriptive statistics and sampling method

The empirical analysis is performed on Swedish registry data covering all residents of Sweden aged 16-65, during the period 1990 to 2010. In this data it is possible to see how many children in different age categories that are living in the household. The data contains information on the individual's biological and adopted children and their birth order for children born up until 2009. It is also possible to link spouses, registered partners and cohabitees (provided they have children together). Same-sex couples can thus be identified in the data from 1995 and onwards since the Registered Partnership Act was implemented in that year. Besides family relations the data also contain information on labor market income, wages, educational level, parental leave benefits, municipality of residence, sex, age and other socioeconomic variables. Information on income comes from annual reports from employers to the Swedish Tax Agency. Thus one can only observe total annual income.

From this data I sample all same-sex and opposite-sex couples who were registered partners, cohabitees or married during the year 1995-2010. Out of these I keep all couples who had their first child together during that period. I exclude the couples where either of the spouses already had children from an earlier relationship. Thereafter, I sample information about these individuals for all the years 1990-2010. With this sampling method I can track the labor market income of each individual between five and 20 years before they had their first child and up to 15 years after. Following couples over years allows for an analysis of how the individuals' incomes and the income gap between spouses develop over time. Thus it is possible to capture the short and long term impact of having a child on the spouses' incomes as well as on the gap in labor market income in the household.

A number of 538 same-sex couples who had their first child together during the period can be identified in the data (couples where none of the partners had children from a previous relationship). Almost all of them are lesbian couples; only 36 are male same-sex couples. The main reasons for this are that in male couples none of the partners can give birth, adoption is largely unavailable for same-sex couples and surrogacy is not permitted in Sweden. Figure 1 shows how many of these same-sex couples had their first child in each year. There is a vast increase in same-sex couples having children together over the period reflecting a response to legislative changes and changed attitudes in society. Due to lack of data linking children to their parents in 2010 the number for this year is underestimated and therefore not presented in the figure. During the same time period, 1995-2010, around 500,000 heterosexual couples who had their first child together can be identified.[3]

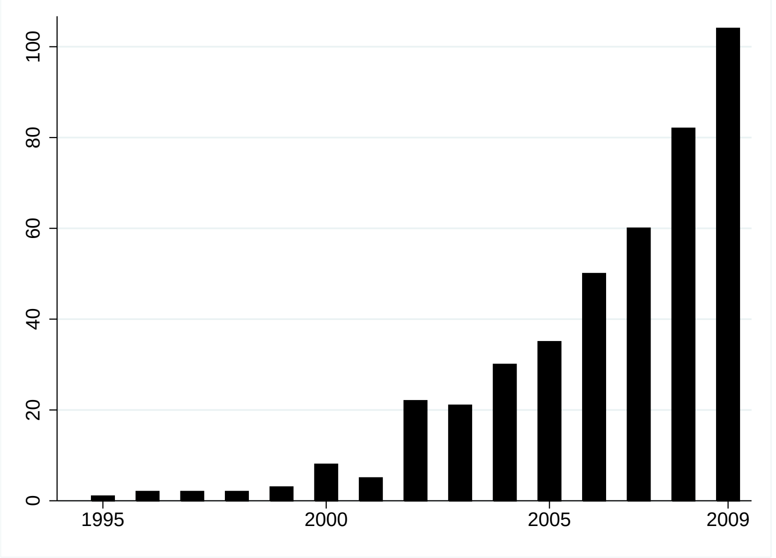

Figure 1. Number of same-sex couples who had their first child together in a certain year during the period 1995-2009 (couples where none of the partners had children since before). No same-sex couples can be identified before 1995. Due to lack of data linking children to their parents in 2010 this year is excluded from the figure.

[3]Please note that because of the differences in legal conditions and attitudes in society these numbers do not reflect the proportion of persons of different sexual orientations in the population.

5.1 Sample restrictions

Due to both ethical concerns and for statistical reasons (lack of observations) male same-sex couples are dropped from the sample. The empirical analysis is thus performed on the sample of lesbian and heterosexual couples only. In 389 out of the 502 lesbian couples it is possible to identify which partner is the biological mother of the child.[4] All of these couples had their first child together in the period 1996-2009. This subsample of 389 lesbian couples will be used in the analysis below. Most of the lesbian couples have children during the latter part of the period (when the legislation had become more favorable). For this reason the statistical analysis for the lesbian couples will be performed only up until 7 years post childbirth.

Out of all opposite-sex couples I keep a sample each year that is proportional to the number of lesbian couples in that year. This is because the number of lesbian couples with children increases vastly during this period. Since the real wages also increased significantly I need to adjust for this or else the income levels of the two groups will not be comparable. After taking a sample of opposite-sex couples proportional to the number of lesbian couples for each year, 73,507 opposite-sex couples remain.

5.2 Descriptive statistics

Table 2 gives some descriptive statistics for the 389 lesbian couples and the proportional sample of 73,507 opposite-sex couples described above. Due to the sampling method the average year for the first child's birth is the same for both groups. The lesbian couples are divided into the partner who gave birth to the couple's first child, who is referred to as the birth mother, and the other partner.

The lesbian birth mothers are on average a few years older than heterosexual women when having their first child. The partners in lesbian couples are more similar in age. In years of schooling heterosexual and lesbian woman are on par, while men have slightly lower levels. Lesbian couples are also more similar in yearly labor market income than heterosexual couples.

Table 2, column 1, shows the coefficients when estimating an OLS regression on the likelihood of being the birth mother in a lesbian couple.[5] The only factor that significantly increases the likelihood of being the birth mother is a higher labor income. For comparison column 2 presents the same estimations performed on the heterosexual couples. Since these couples cannot choose who should give birth the estimates confirm that the heterosexual women are

[4] In 103 couples the child cannot be identified as either the biological or adoptive child of either partner mostly due to lack of data linking children to their parents in 2010.

[5] Couples where at least one of the partners is above 45 years old at the time of the child's birth have been dropped from the sample in order to only include couples who can choose either partner as birth mother.

Table 1. *Descriptive statistics at couple level.*

	Heterosexual couples	Lesbian couples
First child's birthyear	2007	2007
	(3)	(3)
Age at child birth, father/partner	32.2	33.6
	(5.0)	(6.1)
Age at child birth, (birth)mother	29.9	32.9
	(4.4)	(4.4)
Difference in age	2.4	0.7
	(3.9)	(5.5)
Years of schooling, father/partner	13.9	14.6
	(2.8)	(2.5)
Years of schooling, (birth)mother	14.5	14.6
	(2.7)	(2.6)
Difference in years of schooling	-0.6	0.0
	(2.8)	(2.7)
Yearly labor income, father/partner	279	218
	(211)	(147)
Yearly labor income, (birth)mother	207	242
	(148)	(143)
Difference in yearly labor income	72	-24
	(206)	(181)
Number of couples	73507	389

Note: Descriptive statistics (means) for heterosexual and lesbian couples. All statistics are for two years before the first child's birth. Yearly labor income in 1000's SEK, 2008 prices. Standard deviations in parentheses.

Table 2. *Likelihood of being birth mother.*

	Lesbian couples	Heterosexual couples
Yearly labor income	0.0000369***	-0.0000450***
	(0.0000132)	(0.00000173)
Age	-0.00440	-0.0257***
	(0.00413)	(0.000338)
Years of schooling	-0.00627	0.0380***
	(0.00737)	(0.000507)
Constant	0.642***	0.812***
	(0.151)	(0.0104)
Number of individuals	742	144832

Note: OLS regressions on the likelihood of being the birth mother in lesbian and heterosexual couples. Robust standard errors in parentheses.

on average younger, have lower income and more years of schooling than their male partners.

5.3 Graphical analysis

This section presents some graphical evidence on how the raw labor income gap in lesbian and heterosexual couples is affected by having children.

Figure 2 below shows the average annual labor market incomes of the spouses in heterosexual and lesbian couples before and after the birth of their first child (which happens in time period 0). Separate income trajectories are drawn for men and women in heterosexual couples and for the birth mothers and their partners in lesbian couples.

The average spousal income gap is larger in heterosexual couples before childbirth. In the lesbian couples the birth mother of the first child has on average higher income before childbirth as was found in the descriptive tables. In the birth year of the first child the income of both the birth mothers in lesbian couples and heterosexual women drops significantly. The graph suggests that the birth mother in lesbian couples takes more time off work than her partner in the first year of the child's life.

Heterosexual men display a changed trend in their income trajectory in the birth year of the child and a drop in income in the following year. The fathers' incomes recover quickly however and resume a positive trend. The lesbian partners seem largely unaffected in the birth year of the child but experience a drop in their incomes in the next year.

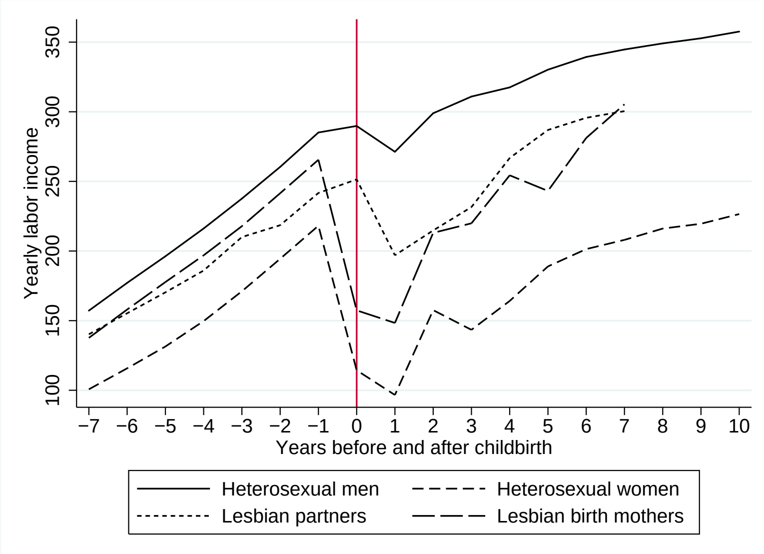

Figure 2. Income trajectories (in 1000's SEK, 2008 prices) of men and women in heterosexual couples and the birth mothers and their partners in lesbian couples before and after childbirth.

Heterosexual mothers keep falling behind, their incomes increasing but not reaching their pre-parenthood level for many years. The lesbian birth mothers' incomes recover a bit faster. Two years after becoming parents and onwards the average income gap in lesbian couples seems to be small. Heterosexual couples, on the other hand, continue to have a large average income gap, vastly larger than before becoming parents.

The graph can also be seen as an informal test of the parallel trends assumption in the difference-in-differences analysis when comparing lesbian and heterosexual couples. Figure 2 shows that the income gaps in lesbian and heterosexual couples cannot be assumed to develop in the same way before childbirth. Since the two types of couples also have vastly different income levels before having children, they are not comparable in their labor market situation in the pre-childbirth period.

5.4 Construction of household types

In order to get comparable samples of lesbian and heterosexual couples and to attain parallel trends in the development of the pre-childbirth income gap, a matching method is used as described in section 4. All households are divided into J different household types, one type for each possible combination of values of specific household characteristics. All households in a specific household type should be as similar as possible in pre-parenthood characteristics that may affect the spousal income gap. The small sample of lesbian couples makes selecting the number and type of variables to use to define the household types a balance between finding as good and as many matches as possible for all couples. Even with the very large pool of heterosexual couples it is difficult to find matches for all lesbian couples when a too narrow definition is used to define the household types.

A transparent and straight forward choice is to match the couples based on their actual spousal income gap before childbirth. In this way all households within a household type will have similar differences in market labor supply before childbirth. The actual income gap also reflects the spouses' current labor market position and can be assumed to be closely related to earnings potentials in the future. Since income opportunities are strongly affected by the current labor market conditions in a certain year, the birth years of the child is another important factor to take into account.

In the analysis below the household types are defined based on the actual spousal income gap two years before childbirth, in intervals of 1,000 SEK in monthly earnings, and the birth year of the first child. Since information on monthly earnings is not available in the data the income intervals are defined as spans of 12,000 SEK in spousal gap in annual earnings. The spousal income gap in lesbian couples is defined as the partner's income minus the birth mother's. In heterosexual couples it is defined as the father's income minus the mother's. Matching on the spousal income gaps two years before childbirth is equivalent to matching on spans of the term $(lny_{1i-2} - lny_{2i-2})$ in the econometric model. Thus by construction this term is roughly similar for all households of the same household type. The couples are divided into 235 household types. By defining household types in this way it is possible to find at least one match for all of the 389 lesbian couples and many more for most of them. Heterosexual couples who cannot be matched with lesbian couples are dropped from the sample. Table 3 shows descriptive statistics for the matched lesbian and heterosexual couples collapsed at the (household type)×(couple type) level. By construction the difference in yearly labor income and birth year of the first child are the same for the two couple types. The descriptive statistics show that the matching process produces a good match also on the levels of income of the spouses. There are still some differences in average age and years of schooling in lesbian and heterosexual couples within the same household type.

Figure A1 in the appendix shows the average annual labor incomes of the spouses in heterosexual and lesbian couples before and after becoming parents in the sample of matched couples collapsed at the household type level. The graphs confirm that the matching on the spousal income gap two years before childbirth result in a good match also on the levels of income of the spouses. Note that with this matching method the average income of the father is lower than that of the mother in heterosexual couples before childbirth. The income trajectories of the fathers and the lesbian partners show a relatively similar trend in the years before having children. The incomes of the heterosexual

Table 3. *Descriptive statistics. Household type level.*

	Heterosexual couples	Lesbian couples
First child's birthyear	2006 (3)	2006 (3)
Age at first child's birth, father/partner	32.2 (1.6)	34.0 (5.2)
Age at first child's birth, (birth)mother	30.2 (1.7)	33.2 (3.8)
Difference in age	2.0 (0.9)	0.8 (4.7)
Years of schooling, father/partner	14.0 (0.7)	14.7 (2.2)
Years of schooling, (birth)mother	14.7 (0.8)	14.7 (2.2)
Difference in years of schooling	-0.7 (0.9)	0.0 (2.5)
Yearly labor income, father/partner	221 (130)	217 (147)
Yearly labor income, (birth)mother	252 (146)	248 (149)
Difference in yearly labor income	-31 (216)	-31 (216)
Number of household types	235	235

Note: Descriptive statistics for heterosexual and lesbian couples at the household type level. All statistics are for two years before first child's birth. Yearly labor income in 1000's SEK, 2008 prices. Standard deviations in parentheses.

mothers and the lesbian birth mothers are very similar in the three years before childbirth. A few years earlier on the other hand heterosexual women have lower average incomes. This might be a concern if it reflects a difference in productivity or earnings potentials between the two groups. The spousal income gap is by construction similar in lesbian and heterosexual couples two years before childbirth. Also one year and three years before childbirth the income gaps are reasonably similar in the two groups. However for earlier years the income gaps show different trends in the two samples. The parallel trends assumption thus seems to hold for the years right before childbirth, but perhaps not for earlier years. After entering parenthood the matched couples show a pattern that is quite similar to that in the full sample. The income gap widens vastly in heterosexual couples, whilst in lesbian couples it becomes smaller.

Figure A2 in the appendix shows density plots of the monthly spousal income gap (in 1000's SEK) in the lesbian and heterosexual couples for each time period, $t = -2$ to $t = 6$, before and after childbirth (at time period 0). The graphs show the average spousal income gap at the (household type)$\times$(couple type) level. Again, because of the matching method, the spousal income gaps in lesbian (dotted lines) and heterosexual couples (solid lines) are the same at time $t = -2$. It is also quite similar in the year before becoming parents. There is however a clear difference in the development of the income gap in the two types of couples after having children. The income gap in the lesbian couples is centered close to zero (indicated by the vertical lines) for all time periods except the birth year of the child. For heterosexual couples the spousal income gap shifts very clearly to the right indicating that the fathers' income is higher than the mothers' in these time periods.

6 Results

The first step in the regression analysis is to estimate the average effect of entering parenthood on the income gap within lesbian and heterosexual couples separately. This is done primarily for illustrative purposes.[6] All tables can be found in the appendix.

Tables A1 and A2 present the results when estimating equation 15 (described in section 4) on the samples of lesbian and heterosexual couples described in section 5.1. That is, all lesbian couples and a proportional sample of heterosexual couples for each of the years 1996-2009. Equation 15 is estimated three times for each sample: In specification (1) no control variables are added, specification (2) includes controls for calendar years and specification

[6]This part of the analysis is much like the one in Angelov et al. (2016) but with a slightly different model specification and on a sample drawn from a different time period. In Angelov et al. (2016) the analysis was performed on all opposite-sex couples in Sweden who had their first child together during the years 1990-2002.

(3) also controls for the within couple age difference and difference in years of schooling, immigration status of each spouse and type of municipality of residence.[7] The standard errors are clustered at couple level.

As can be seen in table A1 the effect of parenthood on the income gap within lesbian couples is positive and significant for the child's birth year (t=0) and year 1 (the year when the child turns one year old). Note that by using the difference in the logarithm of the spouses' incomes, the gap between spouses can be interpreted as the percentage difference in income for small differences on the log scale. However since the log difference is a good approximation of the percentage gap only for small differences, the estimate of α_t should be interpreted in order of magnitude rather than as a precise estimate of the percentage change in the income gap. For larger differences the log-points can be transformed into percentage points to get more precise estimates.[8]

The estimates of the α_t can be interpreted in the following way for example for year 0: The income gap within lesbian couples changes about 87 log points (138 percentage points) in a positive direction when comparing the gap in the birth year of the child with the pre-birth gap. Since the average income gap in lesbian couples is negative before childbirth, this means moving from a negative to a positive income gap. In other words, entering parenthood changes the situation in lesbian couples from one where, on average, the birth mother has a higher income level (pre-childbirth) to one where the partner has a higher income (in the birth year of the child) since the gap is defined as the partner's income minus the birth mother's.

Table A2 shows the results when estimating equation 15 on the sample of heterosexual couples. The estimates for the α_t parameters are always positive and significant. Compared to the results in Angelov et al. (2016) the point estimates are smaller. Possibly this can be explained by a change in behavior of heterosexual couples over time. Angelov et al. (2016) studied couples who had their first child between 1990 and 2002, while most of the couples in my sample had their first child after 2002.

Table A3 presents the results from the difference-in-differences regressions estimating the impact of the gender composition in couples on the change in the spousal income gap after entering parenthood. The columns represent three different specifications of equation 12 estimated on the sample of matched lesbian and heterosexual couples described in section 5.4. As before, in specification (1) no control variables are added, specification (2) includes

[7]The municipality types are defined according to definitions used by the Swedish Association of Local Authorities and Regions (SKL).

[8]To transform a difference in log-points to percentage points the following transformation is used:

$$pp_k = 100 \times (\exp^{\alpha_k} - 1) \tag{4}$$

where α_k ($k = 0, ..., 7$) are the parameters from equation 15.

controls for calendar years and specification (3) also controls for the within couple age difference and difference in years of schooling, immigration status of each spouse and type of municipality of residence. All specifications include household type fixed effects. Standard errors are clustered at the household type level.

The table shows estimates for the α_t (indicated by "$t = k$"), λ_{st} (referred to as "Lesbian in $t = k$") and θ_s (referred to as "Lesbian") parameters in equation 12. Since the heterosexual couples are used as baseline, the α_t parameters can be interpreted as the effect of entering parenthood on the spousal income gap for the heterosexual couples in the matched sample. The α_t parameters are positive and statistically significant for all time periods. The estimated interaction effects of being a lesbian couple in a specific year are negative and statistically significant for all years. This indicates that being a lesbian couple is associated with a smaller change of the spousal income gap post childbirth compared to the change in heterosexual couples. Reassuringly the estimates do not change much when adding calendar year fixed effects and other control variables.

The estimations presented in the table can be interpreted in the following way: The income gap in the heterosexual couples changes on average 110 log-points (200 percentage points) in a positive direction in the child's birth year compared to the pre-childbirth level in the most preferred specification i column (3). Since the pre-childbirth gap was negative, this indicates a change from a negative gap, where the father had a lower income, to one where he earns more. Being a lesbian couple reduces the change in the gap by on average 19 log-points (21 percentage points). The estimated coefficients associated with being a lesbian couple is larger for later time periods. This indicates that the difference in the development of the income gaps in heterosexual and lesbian couples increases over time. To see the effect of entering parenthood on the spousal income gap in lesbian couples one needs to add the coefficients for α_t and λ_{st} in a specific time period. When doing so it appears that the effect of entering parenthood is much smaller in lesbian couples from time period 1 and onwards. For the last time periods the log-point change might even be negative for the lesbian couples, indicating that the income gap is now even more to the birth mother's advantage compared to the gap before childbirth.

Five years after having children the income gap in heterosexual couples has changed by on average 62 log-points (86 percentage points). For lesbian couples the change is on average 62 log-points smaller. This implies that five years after having children the income gap in heterosexual couples has changed to the fathers' advantage while in lesbian couples the income gap is of the same size as before becoming parents.

The θ_s parameter captures the effect of being a lesbian couple on the income gap in time period t=-1, the year before childbirth. Since the couples are matched on the income gap in $t = -2$, whether this parameter is significant or not can be seen as an informal test of the parallel trends assumption that the income gaps in lesbian and heterosexual couples develop in the same way before parenthood. It is thus reassuring so see that the θ_s coefficient is insignificant in all specifications.

As a further check of the parallel trends assumption, the specifications in table A3 are estimated again adding pre-childbirth periods. For the parallel trends assumption to be credible the interaction terms λ_{st} should be precisely estimated around zero in the time periods before childbirth. If not the assumption of parallel trends in the development of the income gaps before childbirth is not confirmed. The results of this exercise can be found in table A4. The estimates are close to zero in the periods right before childbirth and there is no significant difference in the income gaps between the lesbian and heterosexual couples in the five years before entering parenthood. Based on this test I cannot reject the assumption of parallel trends in the pre-treatment period.

7 Discussion on possible mechanisms

In the following section I will discuss some possible mechanisms that might explain the estimated results presented in section 6, that the gender composition in couples seems to have an effect on the division of labor between parents after childbirth. The regression tables discussed below can be found in the appendix.

One important difference between heterosexual and lesbian couples is that for heterosexual couples a pregnancy may be unplanned, while in a lesbian couple a pregnancy is typically a planned event. If parenthood is more carefully considered in lesbian couples then this may affect the results. Marriage indicates a more stable relationship and therefore increases the likelihood that the pregnancy is planned. To investigate if this difference affects the estimates, column (2) of table A5 reestimates the baseline specification in column (3) in table A3, dropping all heterosexual couples who were not married at least two years before having children. Column (1) replicates the results in column (3) in table A3, the baseline results. This restriction of the sample makes the estimates slightly smaller but does not change the conclusion that there is a difference in behavior between the two types of couples.

Earlier studies have shown that the risk of separation is higher in lesbian partnerships than in heterosexual marriages (Andersson et al., 2006). The same pattern can be found in my sample where 13 percent of the lesbian couples, but only 7 percent of the heterosexual couples separate before the child turns eight years old. Since separation may affect the labor supply decisions of the spouses this may affect the income gap post childbirth. In column (3) in table A5, all couples who separate at some point during the estimation period have been excluded from the sample. This restriction makes the estimates for the interaction terms λ_{st} slightly larger for later time periods, indicating that among the couples who stay together the difference between lesbian and heterosexual couples is larger.

Column (4) in table A5 presents the results when the baseline specification is reestimated dropping all couples who had their first child before 2003. The legal framework changed in 2003 enabling both partners in a same-sex couple to be legally recognized as parents of the same child. This made the choice of entering parenthood less risky for both partners. Before 2003 the birth mother risked ending up having to bear all parental obligations including the financial. Her partner, on the other hand, risked ending up without any right to spend time with the child in case of separation. However estimating the model on couples who had their first child under the new legal framework does not change the results much. The estimates for the interaction term are larger for later time periods, but since with this restriction these estimates are based on very few observations they should be interpreted with great care.

7.1 Different effects of the pre-childbirth income gap

It has been shown that the size of the pre-childbirth income gap matters for the division of labor after entering parenthood (Angelov et al., 2016). Column (2) of table A6 presents the results when estimating the baseline specification only on households where the lesbian birth mother/heterosexual woman had a higher income than her partner two years before childbirth. Column (3) presents the results for couples where the father/partner had the highest income. Column (1) of table A6 replicates the baseline results. Graphs of the income trajectories for the spouses in these samples can be found in the appendix (figures A3 and A4).

The results indicate that the spousal income gap in heterosexual couples changes more to the woman's disadvantage if she has a higher income than her partner before childbirth. This is because regardless of her income level before childbirth, she has a lower income than her partner after becoming a mother. Thus if she had a higher income pre-childbirth her income loss is larger.

As can be seen in figure A3 the income trajectories of lesbian birth mothers and heterosexual women follow each other closely before childbirth. So do the income trajectories of the fathers and lesbian partners. After childbirth the income trajectory of fathers is much closer to the one of the lesbian birth mother. The heterosexual women's incomes develop in a similar way to that of the lesbian partners. The spouses in heterosexual couples thus seem to switch roles in terms of relative incomes after becoming parents.

The estimated effect of being a lesbian couple is quite similar in both samples. Also among couples where the father/partner has a higher pre-parenthood income the lesbian couples have a smaller income gap post childbirth. The relative earnings of the spouses clearly matter for the division of labor after becoming parents. It is also clear that the income distribution before childbirth does not have the same effect for the development of the income gap after childbirth for lesbian and heterosexual couples.

7.2 Differences in the division of parental leave

The division of the parental leave could have an effect on the division of labor later on. The partner who takes more time off work to be on parental leave will accumulate more child rearing abilities while the other partner will accumulate more labor market experience. Economic theory predicts that the couples should utilize this by specializing according to their comparative advantages (Becker, 1991).

Table 4 describes the take up of parental leave and temporary parental leave during the child's first two years of life. Temporary parental leave can be used when the child is sick or by the father/partner to stay at home with the child and birth mother during the first two weeks after the child is born. The table shows that the division of parental leave and temporary parental leave is quite similar in the matched lesbian and heterosexual couples. Since the level of parental leave benefits depends on the individual's income, and the matched couples are very similar in terms of income before childbirth, a similarity in the total amount of benefits can be interpreted as a similarity in the total time on parental leave. Lesbian birth mothers and heterosexual mothers take up more of the parental leave benefits in the child's first and second year of life. This indicates that women who give birth make a larger time investment in their newborn child than their partner, regardless of the partner's gender. The non-birth giving partner in lesbian couples takes up more parental leave in the child's second year of life than heterosexual fathers, but still significantly less than the birth mother. The temporary parental leave is also divided in a quite similar way in both types of couples. One reason that the birth-giving partners take up less temporary parental leave is that you need to be working in order to take up this benefit.

The fact that the partners in both lesbian and heterosexual couples do not divide the parental leave evenly means that the partners will accumulate different kinds of human capital during the child's first years of life. Economic theory predicts that this should lead to an increased specialization between the partners. However the results presented in section 6 indicate that this effect is more pronounced in heterosexual couples. The uneven division of parental leave does not seem to affect the couples in the same way. In conclusion, a difference in time spent on parental leave does not seem to be the reason for the difference in the development of the income gaps later on.

Table 4. *Division of parental leave and temporary parental leave benefits.*

	Heterosexual men	Heterosexual women	Partners in lesbian couples	Birth mothers in lesbian couples
Parental leave benefits at time=0	4	59	4	61
	(12)	(43)	(11)	(46)
Parental leave benefits at time=1	30	74	42	72
	(35)	(44)	(40)	(48)
Temporary parental leave benefits at time=0	7	1	6	1
	(6)	(5)	(8)	(6)
Temporary parental leave benefits at time=1	3	1	2	2
	(6)	(5)	(5)	(7)
Prob. any parental leave at time=0	0.24	0.91	0.23	0.89
	(0.43)	(0.28)	(0.42)	(0.32)
Prob. any parental leave at time=1	0.72	0.96	0.76	0.94
	(0.45)	(0.19)	(0.43)	(0.24)
Prob. any temp. parental leave at time=0	0.75	0.07	0.67	0.07
	(0.44)	(0.25)	(0.47)	(0.26)
Prob. any temp. parental leave at time=1	0.39	0.27	0.40	0.28
	(0.49)	(0.44)	(0.49)	(0.45)
Number observations	78824	78824	389	389

Note: Total amount of parental leave and temporary parental leave benefits in 1000'th SEK, 2008 prices, taken up by the partners in the matched samples of lesbian and heterosexual couples during the child's first two years of life and the probability that they will take up any benefits in a specific year. Standard deviations in parentheses.

Birth induced investments in children?

One may ask why the birth giving parent in both types of couples spends more time on parental leave. The fact that the (birth giving) mother takes a larger responsibility for the care of the child could be a consequence of her being the one carrying, giving birth to and breastfeeding the child. Some may see this as a biologically determined behavior. An economic argument would be that since women for biological reasons are induced to invest in child caring abilities they acquire a comparative advantage in terms of parenting skills. Becker (1991) argued that women, because of their larger biological investment in the child, will be more willing to invest time and money in raising it. Note that these arguments hold also for lesbian birth mothers suggesting that they would be expected to take a larger responsibility for the childcare. However if giving birth induces the woman to make a larger time investment in the child, then one would expect the birth giving partner in both types of couples to continue to make larger time investments in her child also when it gets older. This can be observed for heterosexual women, but not as much for lesbian birth mothers. The empirical analysis thus can confirm the theory about birth induced investments only for the child's first two years.

An alternative explanation is that the difference in time investments in the child between lesbian partners is caused by differences in preferences. Since lesbian couples can choose which partner will give birth it might be that they select the one with stronger preferences for taking care of children. Then the difference in time on parental leave between the lesbian partners reflects a difference in preference rather than a biologically induced behavior. However if lesbian partners select into giving birth because of a preference to devote more time to child rearing, then the birth mother would be expected to continue to act according to this preference in later years. The findings in this study cannot confirm this, since the general pattern in lesbian couples is that of very similar income trajectories a few years after childbirth. Also birth mothers who had a higher pre-childbirth income do not seem to specialize in home production (child care).

Another possible explanation is that there is a social norm that women who give birth to a child should be the primary care giver of that child at least during infancy. Immediately going back to work and letting someone else take the full responsibility for an infant is often frowned upon. A norm that birth giving women should be the primary care giver of her infant would affect all couples in the same way. Hence this could explain the similarity in behavior when it comes to the division of parental leave regardless of the gender composition of the couple.

Table 5. *Statistics on the couples' second child.*

	Heterosexual women	Partners in lesbian couples	Birth mothers in lesbian couples
Prob. gives birth to second child.	0.44	0.23	0.09
	(0.36)	(0.40)	(0.26)
Years between first and second child.	2.08	2.77	2.68
	(0.69)	(1.30)	(1.23)
Number of household types	235	235	235

Note: Descriptive statistics for heterosexual and lesbian couples at the household type level. Standard deviations in parentheses. Due to data restrictions it is only possible to follow many individuals for a few years post the birth of the first child. For this reason the numbers in table 5 underestimate the true fertility of the couples in the sample.

7.3 Differences in biological restrictions

A difference in biological restrictions between the two types of couples is that in lesbian couples typically both partners can give birth. Thus they have the opportunity to "take turns" giving birth thereby splitting the costs of pregnancy and nursing. If this is common it would affect the spouses' income trajectories since being the birth mother seems to induce a longer time spent on parental leave.

Table 5 shows the proportion of lesbian birth mothers (of the first child), lesbian partners and heterosexual mothers who give birth to a second child (for the lesbian partner this would be her first biological child). 44 % of the heterosexual couples but only 32 % of the lesbian couples have a second child during the period that they can be observed in the data. The table shows that it is more than twice as common that the partner of the first child's birth mother gives birth to the couple's second child as that the birth mother herself does so. It is more than four times as common that a heterosexual mother gives birth to a second child than that a lesbian birth mother (of the first child) does so.

If giving birth to a second child leads to a long period away from the labor market then this can explain some of the differences in the income trajectories between heterosexual and lesbian birth mothers. It can also explain why the income trajectories of the lesbian partners are more similar for later years when the other partner may have spent more time on parental leave. The fact that lesbian partners take turns giving birth is perhaps unexpected. Since the birth mother of the first child has accumulated more childrearing specific human capital it would be expected that the same partner is selected again to carry and care for the second child. An explanation might be that only by giving birth can a woman in a lesbian partnership achieve biological motherhood.

It can also be seen as a form of cost-sharing in lesbian couples since a longer time on parental leave can have negative consequence for your career.

To investigate how the opportunity to take turns giving birth impacts the development of the income gap in lesbian couples compared to that in heterosexual couples, the baseline specification is reestimated on three subsamples. Column (2) of table A7 presents the results for couples who only have one child together during the period of analysis. Column (3) presents the results for couples where the partner of the birth mother in lesbian couples and the heterosexual mother gave birth to a second child. Finally column (4) presents the results for couples where the birth mother in lesbian couples and the heterosexual mother gave birth to a second child. Column (1) replicates the baseline results. Graphs for the income trajectories of the spouses in these subsamples can be found in the appendix (figures A5, A6 and A7).

For couples who only have one child together the only significant difference between lesbian and heterosexual couples occurs during the second year of the child's life. The graph indicates that this is caused by the lesbian partners making larger time investments in the child during the first few years compared to heterosexual fathers. In these couples both lesbian birth mothers and heterosexual mothers experience a permanent negative effect of parenthood on their income trajectory. The results in column (3) reveal that letting the other partner give birth to the second child greatly affects the development of the spousal income gap when comparing lesbian couples with heterosexual couples who had another child. The income trajectories within the lesbian couples develop in a very similar way and the income gap is small compared to heterosexual couples who have a second child. In couples where the lesbian birth mother gave birth to a second child it seems that she has a substantially higher income than her partner before childbirth (figure A7). This seems to affect the relative income trajectories of the partners in later time periods since after a few years the birth mother again has a higher earnings level than her partner, something that could not be observed in lesbian couples that only have one child together. Since this subsample consists of only 9 % of the lesbian couples and the parallel trends assumption is not fulfilled one cannot draw any conclusions from the regressions analysis for this sample.

The choice of birth mother of the children is clearly important for the development of the income gap in lesbian couples. Considering these results it seems that the fact that lesbian couples use the possibility to take turns giving birth is an important explanation for the income gap being on average smaller within these couples compared to heterosexual couples.

7.4 Differences in the within couple decision making process

Considering the results found in the empirical analysis, in what follows I discuss how the gender composition in couples can influence the decision making process in couples.

Different "rules of thumb" for the division of labor

It has been suggested that heterosexual couples use traditional gender roles as a rule of thumb when assigning tasks such as child rearing and providing for the family (Badgett, 2003). Since lesbian couples cannot use gender as a rule they may use some other principle when deciding on the division of labor at home and in the market. Previous studies have shown that lesbian couples who take on gendered roles (for example "butch" and "femme") do not assign household tasks according to these roles (Badgett, 2003, 157). Blumstein and Schwartz (1983) find that same-sex couples prefer that both partners have an income since this is perceived as more "fair". They suggest that fairness might be an alternative "rule of thumb" for same-sex couples. The results in this study give some support for this hypothesis. Especially for lesbian couples who take turns giving birth, the theory that lesbian couples use fairness as a rule seems to be accurate. These couples share the costs and benefits of biological motherhood and the division of labor market work after having children.

If heterosexual couples use gender as a rule for the division of labor this would influence the spouses to choose a gendered division of labor regardless of other factors such as relative earnings. When investigating the effect of the pre-childbirth relative incomes one can conclude that the pre-childbirth income gap does matter for the income distribution post childbirth, but that the effect differs between lesbian and heterosexual couples. In the case where the birth giving partner (lesbian or heterosexual) has a higher pre-parenthood income, the heterosexual couples still seem to specialize according to traditional gender roles. The lesbian couples on the other hand seem to be more influenced by their relative earnings potentials, letting the birth mother continue to be the main provider also after having children. In the case where the birth giving partner has a lower income pre-parenthood, both types of couples specialize to a higher degree after childbirth. However the income distribution in heterosexual couples is more unequal than in lesbian couples. It seems as if regardless of the relative earnings potentials of the spouses, the gender composition does influence the couples inducing heterosexual couples to have a more traditional division of labor.

In conclusion it seems as though the theory of gender as a rule of thumb can explain some of the difference in behavior between lesbian and heterosexual couples. For lesbian couples there is some support for fairness as a rule of thumb influencing their behavior.

Differences in the bargaining process in couples

The decision on the division of labor can be seen as an outcome of a bargaining process within the couple. The spouses' relative bargaining power is usually seen as determined by the spouses' outside options in case of divorce. This could be determined by for example age, nonlabor income or earnings capacity. Here I will focus on relative earnings as a proxy for bargaining power and the possible influence of gender norms.

There are reasons to believe that the bargaining processes in lesbian and heterosexual couples may differ. For example Alesina et al. (2011) suggest that gender could have a direct influence on relative bargaining power in heterosexual couples. If gender norms prescribe more bargaining power to the father in heterosexual couples, then this could explain the difference in outcomes between the two types of couples. This would enable fathers to successfully bargain to do less child rearing and invest more time in their careers post childbirth even in the case where they have somewhat lower earnings pre-childbirth. This could explain why the division of labor in heterosexual couples is generally more to the husband's favor in terms of labor income compared to the pattern in lesbian couples.

Since the partners' gender could not have a direct influence on the balance of power in lesbian couples other factors such as relative earnings might actually be more important in these couples. A component that enters the bargaining process in lesbian couples, but not heterosexual, is the choice of which partner should give birth to the children. Since giving birth seems to induce a long period on parental leave this choice will affect the future income trajectories of the partners. If the partner with higher relative earnings is assumed to have a stronger bargaining power her preferences for biological motherhood will be a determining factor for the choice of birth mother and thus for the income distribution in the couples post childbirth. It seems reasonable to think that if the higher income partner bargains not to give birth, she is also less likely to have preferences for child rearing in later time periods. Thus it is likely that she will bargain for an uneven division of labor where she can spend more time on market work than her partner. The results discussed in section 7.1 confirm this as in couples where the higher income partner does not give birth there is an uneven division of labor after childbirth (even though smaller than in heterosexual couples).

In couples where the higher income partner has preferences for giving birth the division of labor is more even. The birth mother's income decreases as a consequence of spending time on parental leave. However after the initial drop in income the birth mother recovers more quickly than comparable heterosexual women (who also had higher income than her partner before childbirth). As can be seen in figure A3 the roles are sort of reversed post childbirth in heterosexual couples where the mother had a higher income before childbirth, a pattern that cannot be observed in lesbian couples. This difference in the division of labor in these couples post childbirth can be the result of a difference in the bargaining processes where in heterosexual couples the father's gender gives him an advantage, while in lesbian couples the birth mother has a stronger bargaining position because of her higher earnings potential.

In couples who only have one child together the birth mother's income is on average higher before parenthood, but is permanently lower than her partner's in the post-childbirth period. If the birth mother's partner does not have any preferences for biological motherhood this could explain why the income distribution is more to the partner's advantage in the post-childbirth period. Perhaps the non-birth giving partner's relative bargaining power is permanently improved after the birth mother's labor income drastically decreases after childbirth.

The most egalitarian among the lesbian couples are those where both partners have preferences for biological motherhood and who take turns giving birth. In these couples the partners' incomes develop in a very similar way due to the fact that they both make a large time investment in their biological child while being on parental leave. Thus both partners' preferences for giving birth affect the outcome of the bargaining process in these couples and are thus important for the division of labor and the development of the income gap in lesbian couples.

The influence of gender norms and identity

According to the gender identity theory proposed by Akerlof and Kranton (2000) individuals act in accordance with the norms associated with one's gender as it gives the individual utility to confirm his/her self-image. The individual also gets utility from seeing others act in accordance with their prescribed gender identity, and consequently it gives people a positive payoff/reaction to act according to these norms. Partners in a couple can thus increase both their own and the partner's utility by mutually confirming their own and their partner's gender identity.

If in heterosexual couples the spouses can gain utility both from acting to confirm their own gender identity and by seeing their spouse do so, then this can explain the gender specific division of labor after childbirth. The mother will do more of the "feminine" work such as household work and child care and the couple will actively choose to let the father be the main provider of the family to confirm his gender identity.

For the lesbian couples there is no obvious scope to increase the partners' utility by dividing the household and labor market work according to gender norms, since both partners have the same gender. An even division of "feminine" tasks in the household could be optimal if both partners get utility from confirming their identities as women. Many associate pregnancies and caring for an infant as something that strengthens a person's identity as a woman. Perhaps this is why it is more common among lesbian couples who have more than one child together to give birth to one child each so that both partners can have this experience. When it comes to labor market opportunities and investing time in one's career, in lesbian couples there is no specific utility gain from prescribing one partner the role as main provider. Thus the partners can disregard gender identity as a factor when deciding on their labor market supply. There are economic arguments for letting both partners invest time in market work since this will make the family more financially secure in case one of the partners loses her job. The gender identity theory can thus explain why heterosexual couples have a more traditional division of labor than lesbian couples given the same pre-childbirth distribution of income.

8 Summary and concluding remarks

When comparing lesbian and heterosexual couples with children in the whole population, lesbian couples have on average smaller income gaps before childbirth and a more egalitarian division of labor after becoming parents. Also when comparing a matched sample of lesbian and heterosexual couples with similar pre-childbirth income gaps, lesbian partners in general have more similar income trajectories a few years after having children. Lesbian and heterosexual couples are matched so that they have similar pre-childbirth income gaps and the birth year of the first child is the same. In this sample the spousal income gap in heterosexual couples on average changed 62 log-points (86 percentage points) more to the father's advantage five years after having children. For lesbian couples the change in the income gap is 62 log-points smaller. In other words, five years after having children the income gap in heterosexual couples has changed to the fathers' advantage, while in lesbian couples the income gap is essentially the same as before becoming parents. Thus it seems clear that, in this sample, the gender composition of the couple has a significant impact on the division of labor and market labor supply of the spouses after childbirth.

The birth giving partner in both lesbian and heterosexual couples spends more time on parental leave in the first two years of the child's life. Consequently they experience a large decline in their labor market income directly after childbirth. In later years the division of labor in lesbian couples depends mainly on which partner gave birth to the child/children and the spouses' relative earnings before childbirth. Lesbian couples where the partners give birth to one child each are the most egalitarian.

The analysis shows that the difference in behavior is not primarily caused by differences in partnership stability, the fact that for lesbian couples parenthood is more planned or changes in legal conditions for lesbian couples. The spouses' relative earnings pre-childbirth affects the spousal division of labor in different ways in lesbian and heterosexual couples. Heterosexual couples show a gendered pattern of specialization after becoming parents regardless of their pre-parenthood income gap. In lesbian couples, if the birth giving partner had a higher pre-childbirth income, she will in general continue to be the main provider of the family also after childbirth.

The fact that lesbian couples can choose which partner should give birth to the child seems to be of great importance. This extra degree of freedom means that lesbian couples can split the costs and benefits of pregnancy and infant child rearing by taking turns giving birth to their children. Among lesbian couples who have a second child it is more common to let the other partner, who did not give birth to the first child, be the birth mother of the second child. The biological mother of each child, at least initially, usually takes the main responsibility for the care of that child. Hence the effect of childbirth on the income gap in lesbian couples depends to a large extent on which partner gave birth to the child/children.

The gender composition of the spouses could influence the decision making process in couples in several ways. It might be that heterosexual couples use traditional gender roles as a "rule of thumb" when making their decision. The spouses might also get a direct utility from acting to confirm their and their partner's gender identity, assigning "feminine" and "masculine" tasks according to gender norms. Lesbian partners might instead divide "feminine" tasks more evenly in order to confirm each partner's identity as a woman. There are some indications that a principle about fairness could influence the behavior of lesbian couples. The decision on the spouses' labor supply can also be seen as the outcome of a bargaining process. It has been suggested that men in heterosexual couples have a stronger bargaining position as a direct effect of their gender. In that case men can more successfully bargain to do less child rearing and invest more time in their careers post childbirth. In lesbian couples gender cannot directly influence the relative bargaining power of the partners the way it can in heterosexual couples. Since in lesbian couples both partners can give birth the choice of who should be the birth mother also enters the decision making process. If the partner giving birth typically spends a longer time on parental leave, the choice of birth mother may lead to a change in the partners' relative earnings and consequently in their relative bargaining power. Thus each partner's preference for biological motherhood is an important factor for their future income trajectories and division of labor.

The results in this study indicate that parental gender does play an important role for the parents' division of labor at home and their labor market supply post childbirth. Since earlier studies on family economics has mostly concentrated on other factors, this paper contributes to the literature by presenting suggestive evidence that spousal gender does affect couple behavior. These results suggest that the impact of spousal gender should not be ignored in any economic analysis of the family.

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Appendix

Table A1. *Effect of parenthood on spousal income gap. Lesbian couples.*

	(1)	(2)	(3)
t=0	0.898***	0.874***	0.867***
	(0.0996)	(0.105)	(0.109)
t=1	0.676***	0.704***	0.685***
	(0.144)	(0.169)	(0.174)
t=2	0.137	0.184	0.122
	(0.177)	(0.205)	(0.214)
t=3	0.242	0.290	0.239
	(0.222)	(0.259)	(0.268)
t=4	0.328	0.381	0.323
	(0.252)	(0.296)	(0.294)
t=5	0.0511	0.0984	-0.0258
	(0.239)	(0.289)	(0.289)
t=6	-0.138	-0.0753	-0.203
	(0.296)	(0.368)	(0.369)
t=7	-0.500	-0.437	-0.480
	(0.351)	(0.426)	(0.429)
Constant	-0.0872	0.796	0.902
	(0.0922)	(0.984)	(1.069)
Calender year FE	No	Yes	Yes
Household controls	No	No	Yes
N	2048	2048	2048
Number of Cluster	389	389	389

Note: Table A1 presents the results when estimating equation 15 on the samples of lesbian couples described in table 2. In column (1) no control variables are added, column (2) includes controls for calendar years and column (3) also controls for the within couple age difference and difference in years of schooling, immigration status of each spouse and type of municipality of residence. The standard errors are clustered at couple level.

Table A2. *Effect of parenthood on spousal income gap. Heterosexual couples.*

	(1)	(2)	(3)
t=0	1.040***	1.059***	1.054***
	(0.00787)	(0.00840)	(0.00846)
t=1	1.420***	1.444***	1.437***
	(0.0120)	(0.0128)	(0.0130)
t=2	0.838***	0.862***	0.853***
	(0.0136)	(0.0149)	(0.0152)
t=3	1.000***	1.028***	1.017***
	(0.0161)	(0.0177)	(0.0181)
t=4	0.732***	0.763***	0.747***
	(0.0182)	(0.0202)	(0.0206)
t=5	0.503***	0.539***	0.520***
	(0.0209)	(0.0233)	(0.0237)
t=6	0.431***	0.474***	0.450***
	(0.0245)	(0.0274)	(0.0278)
t=7	0.369***	0.415***	0.381***
	(0.0302)	(0.0334)	(0.0338)
Constant	0.163***	0.147	0.147
	(0.00730)	(0.0915)	(0.101)
Calender year FE	No	Yes	Yes
Household controls	No	No	Yes
N	345172	345172	345030
Number of Cluster	73507	73507	73507

Note: Table A2 presents the results when estimating equation 15 on the samples of heterosexual couples described in table 2. In column (1) no control variables are added, column (2) includes controls for calendar years and column (3) also controls for the within couple age difference and difference in years of schooling, immigration status of each spouse and type of municipality of residence. The standard errors are clustered at couple level.

Table A3. *Main results table.*

	(1)	(2)	(3)
t=0	1.099***	1.107***	1.105***
	(0.0131)	(0.0254)	(0.0235)
t=1	1.499***	1.513***	1.509***
	(0.0199)	(0.0513)	(0.0470)
t=2	0.917***	0.949***	0.944***
	(0.0217)	(0.0745)	(0.0680)
t=3	1.091***	1.132***	1.127***
	(0.0231)	(0.0995)	(0.0906)
t=4	0.816***	0.867***	0.862***
	(0.0250)	(0.125)	(0.113)
t=5	0.558***	0.621***	0.616***
	(0.0282)	(0.150)	(0.137)
t=6	0.432***	0.507***	0.502***
	(0.0287)	(0.176)	(0.160)
t=7	0.353***	0.433**	0.427**
	(0.0324)	(0.202)	(0.184)
Lesbian in t=0	-0.201**	-0.191*	-0.189*
	(0.102)	(0.102)	(0.103)
Lesbian in t=1	-0.822***	-0.802***	-0.802***
	(0.142)	(0.142)	(0.142)
Lesbian in t=2	-0.829***	-0.810***	-0.826***
	(0.177)	(0.178)	(0.178)
Lesbian in t=3	-0.922***	-0.899***	-0.914***
	(0.229)	(0.229)	(0.230)
Lesbian in t=4	-0.563**	-0.541**	-0.532**
	(0.246)	(0.247)	(0.246)
Lesbian in t=5	-0.609**	-0.592**	-0.624***
	(0.240)	(0.240)	(0.240)
Lesbian in t=6	-0.715**	-0.696**	-0.721**
	(0.300)	(0.301)	(0.301)
Lesbian in t=7	-1.009***	-0.989***	-1.003***
	(0.318)	(0.319)	(0.313)
Lesbian	-0.0243	-0.0388	-0.0588

continued

Table A3. *Main results table.*

	(0.0946)	(0.0950)	(0.0943)
Constant	0.128***	0.0322	0.121
	(0.0151)	(0.162)	(0.149)
Household type FE	Yes	Yes	Yes
Calender year FE	No	Yes	Yes
Household controls	No	No	Yes
N	486829	486829	486464
Number of Cluster	235	235	235

Note: Effect of gender composition in couples on change in spousal income gap post childbirth. Table A3 presents the results from the difference-in-differences regressions estimating the effect of the gender composition in couples on the change in the spousal income gap after entering parenthood The columns represent three different specifications of equation 12 estimated on the sample of matched lesbian and heterosexual couples described in section 5.4: lesbian and heterosexual couples divided into household types with the same income spousal income gap two years before childbirth. In specification (1) no control variables are added, specification (2) includes controls for calendar years and specification (3) also controls for the within couple age difference and difference in years of schooling, immigration status of each spouse and type of municipality of residence. All specifications include household type fixed effects. Standard errors are clustered at the household type level.

Table A4. *Placebo test for time effects on spousal income gap.*

	(1)	(2)	(3)
t=-7	0.347***	0.286***	0.316***
	(0.0261)	(0.0365)	(0.0360)
t=-6	0.293***	0.244***	0.272***
	(0.0233)	(0.0317)	(0.0311)
t=-5	0.232***	0.188***	0.213***
	(0.0187)	(0.0255)	(0.0252)
t=-4	0.135***	0.0996***	0.121***
	(0.0160)	(0.0206)	(0.0200)
t=-3	0.0385**	0.0111	0.0255
	(0.0183)	(0.0194)	(0.0191)
t=-2	-0.0454*	-0.0624**	-0.0620**
	(0.0244)	(0.0241)	(0.0243)
t=0	1.099***	1.127***	1.125***
	(0.0131)	(0.0136)	(0.0137)
t=1	1.498***	1.552***	1.549***
	(0.0199)	(0.0201)	(0.0201)
t=2	0.921***	0.995***	0.992***
	(0.0224)	(0.0250)	(0.0254)
t=3	1.105***	1.193***	1.190***
	(0.0242)	(0.0281)	(0.0282)
t=4	0.827***	0.935***	0.933***
	(0.0272)	(0.0312)	(0.0313)
t=5	0.571***	0.700***	0.698***
	(0.0312)	(0.0351)	(0.0350)
t=6	0.451***	0.606***	0.605***
	(0.0333)	(0.0381)	(0.0383)
t=7	0.393***	0.562***	0.560***
	(0.0392)	(0.0448)	(0.0449)
Lesbian in t=-7	-0.355*	-0.373*	-0.394**
	(0.193)	(0.192)	(0.194)
Lesbian in t=-6	-0.380**	-0.388**	-0.412**
	(0.183)	(0.183)	(0.184)
Lesbian in t=-5	-0.196	-0.199	-0.221
	(0.155)	(0.155)	(0.156)

continued

Table A4. *Placebo test for time effects on spousal income gap.*

Lesbian in t=-4	-0.185	-0.188	-0.207
	(0.154)	(0.154)	(0.155)
Lesbian in t=-3	-0.0643	-0.0679	-0.0823
	(0.130)	(0.130)	(0.130)
Lesbian in t=-2	-0.0184	-0.0251	-0.0246
	(0.107)	(0.107)	(0.107)
Lesbian in t=0	-0.201**	-0.193*	-0.192*
	(0.102)	(0.102)	(0.102)
Lesbian in t=1	-0.822***	-0.808***	-0.806***
	(0.142)	(0.142)	(0.142)
Lesbian in t=2	-0.819***	-0.804***	-0.805***
	(0.177)	(0.177)	(0.178)
Lesbian in t=3	-0.912***	-0.895***	-0.903***
	(0.229)	(0.229)	(0.230)
Lesbian in t=4	-0.553**	-0.539**	-0.550**
	(0.246)	(0.247)	(0.248)
Lesbian in t=5	-0.599**	-0.590**	-0.621**
	(0.239)	(0.240)	(0.241)
Lesbian in t=6	-0.692**	-0.683**	-0.718**
	(0.301)	(0.301)	(0.303)
Lesbian in t=7	-0.984***	-0.973***	-0.990***
	(0.324)	(0.323)	(0.321)
Lesbian	-0.0392	-0.0410	0.0351
	(0.0929)	(0.0928)	(0.0942)
Constant	0.112***	-0.0691**	-0.120***
	(0.0113)	(0.0286)	(0.0311)
Household type FE	Yes	Yes	Yes
Calender year FE	No	Yes	Yes
Household controls	No	No	Yes
N	914993	914993	910177
Number of Cluster	235	235	235

Note: As a further check of the parallel trends assumption the specifications in table A3 are estimated again in table A4 adding pre-childbirth periods.

Table A5. *The effect of a more planned childbirth (2), separation (3) and legislative changes in 2003 (4).*

	(1)	(2)	(3)	(4)
t=0	1.105***	1.117***	1.089***	1.090***
	(0.0235)	(0.0346)	(0.0213)	(0.0145)
t=1	1.509***	1.435***	1.500***	1.465***
	(0.0470)	(0.0683)	(0.0412)	(0.0212)
t=2	0.944***	0.928***	0.946***	0.901***
	(0.0680)	(0.102)	(0.0585)	(0.0245)
t=3	1.127***	1.024***	1.160***	1.071***
	(0.0906)	(0.132)	(0.0783)	(0.0266)
t=4	0.862***	0.789***	0.899***	0.793***
	(0.113)	(0.169)	(0.0968)	(0.0278)
t=5	0.616***	0.550***	0.657***	0.531***
	(0.137)	(0.205)	(0.117)	(0.0306)
t=6	0.502***	0.424*	0.538***	0.411***
	(0.160)	(0.237)	(0.136)	(0.0334)
t=7	0.427**	0.357	0.461***	0.376***
	(0.184)	(0.274)	(0.156)	(0.0502)
Lesbian in t=0	-0.189*	-0.203*	-0.200*	-0.214**
	(0.103)	(0.104)	(0.103)	(0.105)
Lesbian in t=1	-0.802***	-0.722***	-0.883***	-0.811***
	(0.142)	(0.146)	(0.148)	(0.146)
Lesbian in t=2	-0.826***	-0.825***	-0.844***	-0.863***
	(0.178)	(0.182)	(0.186)	(0.196)
Lesbian in t=3	-0.914***	-0.811***	-0.961***	-0.986***
	(0.230)	(0.232)	(0.241)	(0.257)
Lesbian in t=4	-0.532**	-0.435*	-0.735***	-0.579**
	(0.246)	(0.247)	(0.260)	(0.278)
Lesbian in t=5	-0.624***	-0.518**	-0.863***	-0.657**
	(0.240)	(0.240)	(0.291)	(0.305)
Lesbian in t=6	-0.721**	-0.562*	-1.049***	-0.633
	(0.301)	(0.297)	(0.304)	(0.425)
Lesbian in t=7	-1.003***	-0.794**	-1.538***	-1.488**
	(0.313)	(0.306)	(0.392)	(0.634)
Lesbian	-0.0588	-0.0397	-0.0114	-0.0252

continued

Table A5. *The effect of a more planned childbirth (2), separation (3) and legislative changes in 2003 (4).*

	(0.0943)	(0.101)	(0.102)	(0.0995)
Constant	0.121	0.0258	0.0917	1.260***
	(0.149)	(0.209)	(0.124)	(0.0762)
Household type FE	Yes	Yes	Yes	Yes
Calender year FE	Yes	Yes	Yes	No
Household controls	Yes	Yes	Yes	Yes
N	486464	163562	446218	366424
Number of Cluster	235	235	235	207

Note: Column (1) of table A5 replicates the baseline specification; column (3) in table A3. Column (2), (3) and (4) reestimates the baseline specification, dropping all heterosexual couples who were not married at least two years before having children, column (2), dropping all couples who separate at some point during the estimation period, column (3), and last dropping all couples who had their first child before 2003, column (4).

Table A6. *The effect of the pre-childbirth income gap.*

	(1)	(2)	(3)
t=0	1.105***	1.278***	0.927***
	(0.0235)	(0.0179)	(0.0113)
t=1	1.509***	1.733***	1.224***
	(0.0470)	(0.0251)	(0.0165)
t=2	0.944***	1.257***	0.535***
	(0.0680)	(0.0239)	(0.0140)
t=3	1.127***	1.433***	0.654***
	(0.0906)	(0.0279)	(0.0198)
t=4	0.862***	1.202***	0.306***
	(0.113)	(0.0289)	(0.0179)
t=5	0.616***	0.978***	-0.0119
	(0.137)	(0.0260)	(0.0252)
t=6	0.502***	0.889***	-0.191***
	(0.160)	(0.0256)	(0.0226)
t=7	0.427**	0.838***	-0.352***
	(0.184)	(0.0464)	(0.0255)
Lesbian in t=0	-0.189*	-0.121	-0.337***
	(0.103)	(0.160)	(0.119)
Lesbian in t=1	-0.802***	-0.983***	-0.692***
	(0.142)	(0.229)	(0.168)
Lesbian in t=2	-0.826***	-1.043***	-0.727***
	(0.178)	(0.266)	(0.220)
Lesbian in t=3	-0.914***	-0.913***	-1.044***
	(0.230)	(0.341)	(0.299)
Lesbian in t=4	-0.532**	-0.752**	-0.417
	(0.246)	(0.379)	(0.277)
Lesbian in t=5	-0.624***	-0.733*	-0.662***
	(0.240)	(0.389)	(0.242)
Lesbian in t=6	-0.721**	-0.881*	-0.740*
	(0.301)	(0.456)	(0.378)
Lesbian in t=7	-1.003***	-1.167**	-1.018***
	(0.313)	(0.532)	(0.320)
Lesbian	-0.0588	0.0931	-0.0939
	(0.0943)	(0.144)	(0.113)

continued

Table A6. *The effect of the pre-childbirth income gap.*

Constant	0.121	-0.425***	0.798***
	(0.149)	(0.0441)	(0.0342)
Household type FE	Yes	Yes	Yes
Calender year FE	Yes	Yes	Yes
Household controls	Yes	Yes	Yes
N	486464	181651	304813
Number of Cluster	235	130	112

Note: Column (1) of table A6 replicates the baseline specification; column (3) in table A3. Column (2) presents the results when estimating the baseline specification on households where the lesbian birth mother/heterosexual woman had a higher income than her partner two years before childbirth. Column (3) presents the results for couples where the father/partner had the highest income.

Table A7. *The effect of number of children and being birth mother.*

	(1)	(2)	(3)	(4)
t=0	1.105***	1.074***	1.173***	1.173***
	(0.0235)	(0.0232)	(0.0242)	(0.0246)
t=1	1.509***	1.464***	1.573***	1.572***
	(0.0470)	(0.0440)	(0.0474)	(0.0482)
t=2	0.944***	0.621***	1.170***	1.168***
	(0.0680)	(0.0585)	(0.0688)	(0.0701)
t=3	1.127***	0.608***	1.356***	1.354***
	(0.0906)	(0.0776)	(0.0899)	(0.0918)
t=4	0.862***	0.529***	0.993***	0.991***
	(0.113)	(0.0957)	(0.113)	(0.116)
t=5	0.616***	0.385***	0.706***	0.703***
	(0.137)	(0.110)	(0.137)	(0.140)
t=6	0.502***	0.353***	0.566***	0.562***
	(0.160)	(0.123)	(0.162)	(0.166)
t=7	0.427**	0.383**	0.458**	0.453**
	(0.184)	(0.151)	(0.185)	(0.189)
Lesbian in t=0	-0.189*	-0.0599	-0.607***	-0.0356
	(0.103)	(0.125)	(0.164)	(0.291)
Lesbian in t=1	-0.802***	-0.631***	-1.452***	-0.871
	(0.142)	(0.164)	(0.277)	(0.616)
Lesbian in t=2	-0.826***	-0.254	-1.529***	-1.175*
	(0.178)	(0.214)	(0.322)	(0.617)
Lesbian in t=3	-0.914***	-0.153	-1.372***	-1.276**
	(0.230)	(0.320)	(0.344)	(0.542)
Lesbian in t=4	-0.532**	0.129	-0.903***	-1.145*
	(0.246)	(0.423)	(0.237)	(0.591)
Lesbian in t=5	-0.624***	0.175	-0.795***	-1.863***
	(0.240)	(0.385)	(0.192)	(0.653)
Lesbian in t=6	-0.721**	0.345	-1.044***	-2.160***
	(0.301)	(0.495)	(0.195)	(0.823)
Lesbian in t=7	-1.003***	-0.430	-1.025***	-2.469***
	(0.313)	(0.538)	(0.254)	(0.904)
Lesbian	-0.0588	-0.126	0.0370	0.485

continued

Table A7. *The effect of number of children and being birth mother.*

	(0.0943)	(0.112)	(0.159)	(0.382)
Constant	0.121	0.107	0.196	0.199
	(0.149)	(0.0940)	(0.174)	(0.178)
Household type FE	Yes	Yes	Yes	Yes
Calender year FE	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes
N	486464	179431	306906	306576
Number of Cluster	235	234	186	186

Note: Column (1) of table A7 replicates the baseline specification; column (3) in table A3. Column (2) of table A7 presents the results for couples who only have one child together during the period of analysis. Column (3) presents the results for couples where the partner of the birth mother in lesbian couples and the heterosexual mother gave birth to a second child. Finally column (4) presents the results for couples where the birth mother in lesbian couples and the heterosexual mother gave birth to a second child.

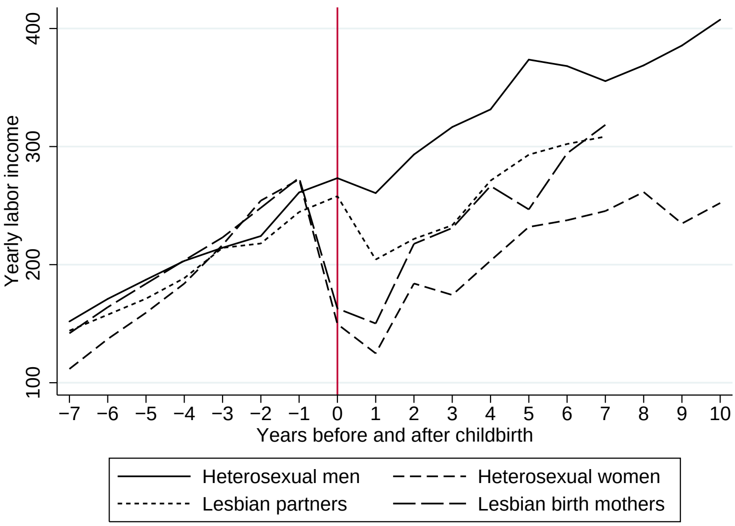

Figure A1. Income trajectories (in 1000's SEK, 2008 prices) of men and women in heterosexual couples and the birth mothers and partners in lesbian couples before and after childbirth in the matched sample.

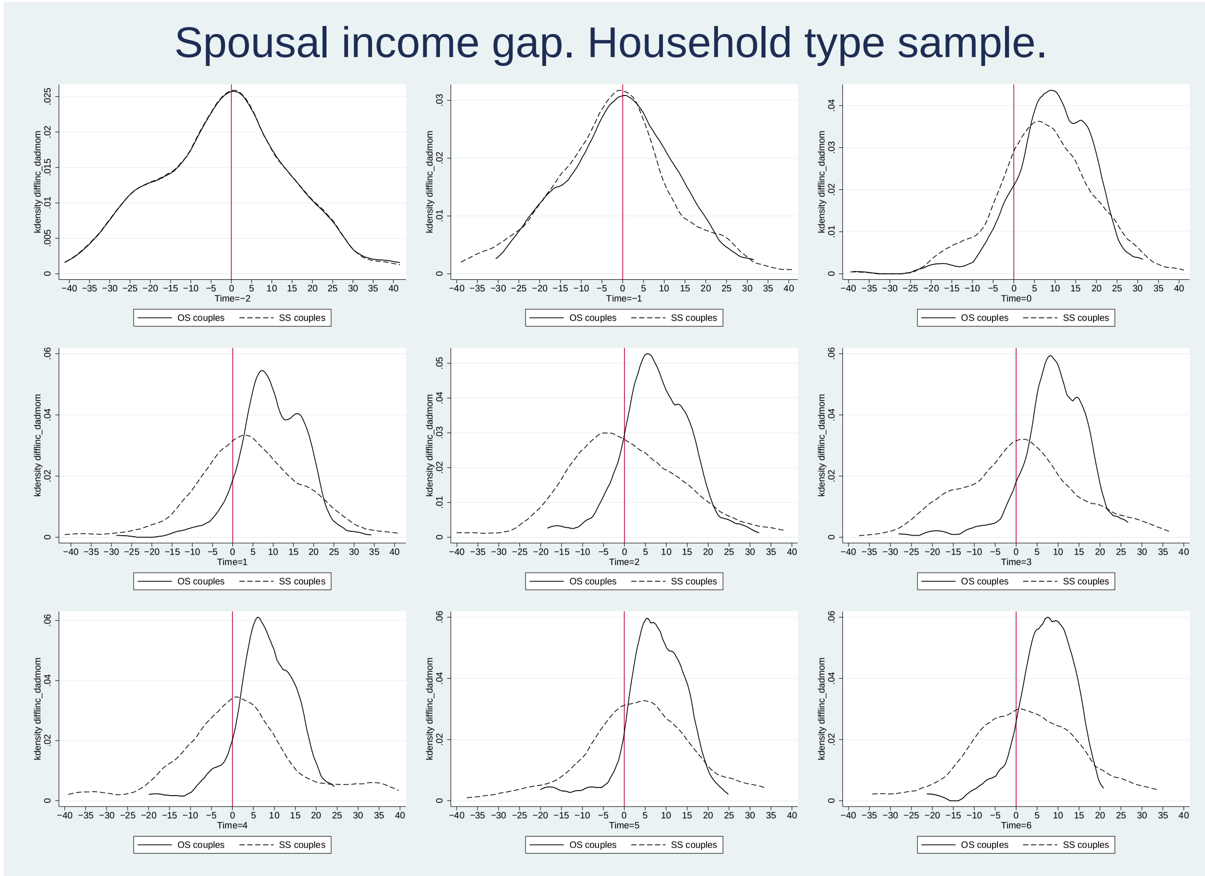

Figure A2. Density of monthly spousal income gap (in 1000's SEK, 2008 prices) in lesbian (dotted lines) and heterosexual (solid lines) couples for each time period, $t = -2$ to $t = 6$, before and after childbirth.

Figure A3. Income trajectories (in 1000's SEK, 2008 prices) of men and women in heterosexual couples and the birth mothers and partners in lesbian couples before and after childbirth for couples where the birth mother/heterosexual woman had the highest income before childbirth.

Figure A4. Income trajectories (in 1000's SEK, 2008 prices) of men and women in heterosexual couples and the birth mothers and partners in lesbian couples before and after childbirth for couples where the father/partner had the highest income before childbirth.

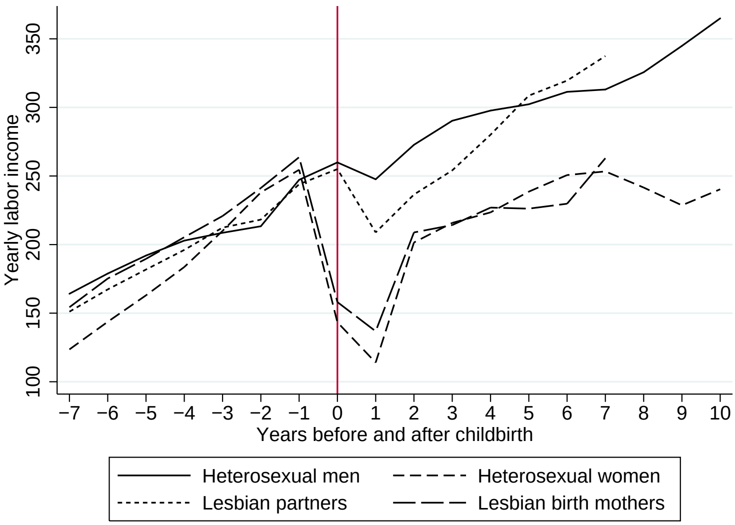

Figure A5. Income trajectories (in 1000's SEK, 2008 prices) of men and women in heterosexual couples and the birth mothers and partners in lesbian couples before and after childbirth in the matched sample who only have one child together.

Figure A6. Income trajectories (in 1000's SEK, 2008 prices) of men and women in heterosexual couples and the birth mothers and partners in lesbian couples before and after childbirth in the matched sample where the birth mother's partner and the heterosexual mother gave birth to a second child.

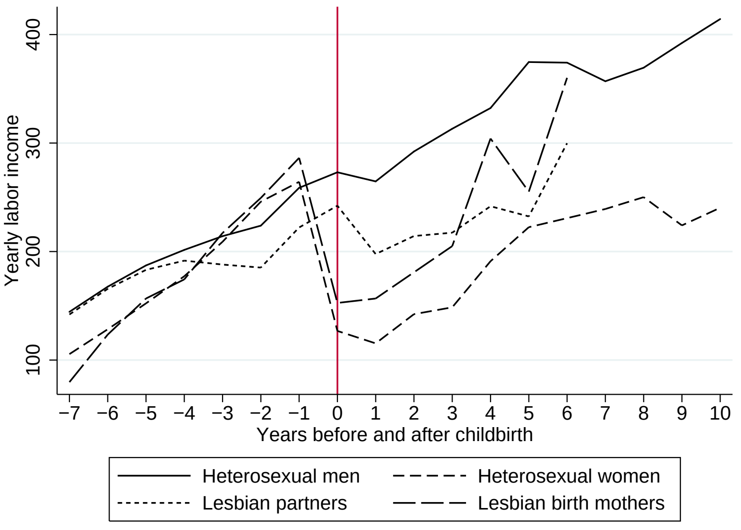

Figure A7. Income trajectories (in 1000's SEK, 2008 prices) of men and women in heterosexual couples and the birth mothers and partners in lesbian couples before and after childbirth in the matched sample where the birth mother and the heterosexual mother gave birth to a second child.

III. Mothers' Birth Giving Status and the Division of Parental Leave – A Comparison of Adoptive and Biological Parents

The skewed division of parental responsibilities during a child's infancy is often assumed to be a natural consequence of the mother being pregnant and wanting to breastfeed. In this paper I investigate to what extent the tendency to let the mother be the main caregiver of an infant can be explained by the fact that she is the one to be pregnant, not the father. Using the division of parental leave during the child's first two years with the parents as a proxy for the division of parental responsibilities, I compare the behavior of biological parents (where the mother gave birth) to adoptive parents (where she did not) in Swedish population-wide register data. My results show that adoptive parents, both mothers and fathers, spend less time on parental leave than biological parents, but that the mother's share of leave is about the same as among biological parents. There is thus some support for the hypothesis that a biological tie increases parents' initial investment in children, but not that this relationship is stronger for women. Hence, there is no evidence that the mother's birth giving status can explain her share of parental responsibilities. Due to methodological challenges, it is difficult to disentangle the different mechanisms that could explain the results.

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1 Introduction

In general parental leave legislation worldwide has a "maternity bias" in the sense that it gives mothers greater opportunities to stay at home with the child. Also in countries where it is formally possible to share the parental leave evenly between the parents, most mothers take a majority of leave days. In Sweden parents enjoy job-protection until the child is 18 months old and 16 months of paid parental leave that can be divided between the parents any way they want, except for three months that are earmarked for each parent. In practice, however, mothers use about 75 % of the parental leave.

Engström et al. (2009) argue that immediately after the birth of a child the mother stays at home for *exogenous biological reasons* such as fatigue after giving birth or a preference to breastfeed and/or because of *tradition*. However, mothers' length of parental leave is often longer than what can be motivated by for example the length of exclusive breastfeeding. The uneven division of total time on leave can, according to Engström et al. (2009), be explained by present-biased preferences: the mother's initial time on leave induces the parents to continue to let the mother stay at home on parental leave putting unproportionately high weight on present costs of paternity leave, for example present income loss. Also Becker (1991) argues that the mother's larger share of parental responsibilities stems from biological gender differences. Since the mother is the one to be pregnant and give birth, he argues, she makes a larger biological investment in her child before it is born. Because of this initial difference in biological investment between parents, mothers will be keener than the father to get a larger return on their investment and hence they will be more willing to invest time and money in their child also later on.

Following the argument of Engström et al. (2009) and Becker (1991), the fact that mothers give birth should be an important explanation as to why mothers tend to take a larger proportion of the parental leave also in countries where it is legally possible to split the leave evenly. In Moberg (2016), I present some empirical evidence that support this hypothesis: in a study that compares lesbian and heterosexual couples, I find that the division of parental leave between the birth mother in a lesbian couple and her partner was very similar to the division between the mother and father in heterosexual couples. Since the child's birth mother in general took up a larger share of the leave this supports the hypothesis that a woman's birth giving status affects the division of parental leave.

The purpose of this study is to investigate to what extent the fact that women give birth can explain that they often spend a longer time on parental leave than men. I investigate this relationship by studying the division and length of parental leave among adoptive parents and compare it to that of biological parents. By studying adoptive parents, it is possible to observe the behavior of couples where the parents are of different gender, but where no one gave birth. In this group of parents, the mothers are not induced to invest more time than the fathers in their child due to pregnancy, breast feeding or social norms associated with biological motherhood. The adoptive mothers will also not experience the hormonal build up during pregnancy that may affect biological mothers' preferences/behavior after childbirth. Given the arguments presented above, mothers who for example adopt would be expected not to invest as much time in their adoptive child as they would have in a biological child. Adoptive parents probably make more equal time and monetary investments while preparing to receive the child, such as going through the adoption process together and traveling to the child's birth country to retrieve him/her. Given the arguments discussed above they would thus be expected to have a more even division of parental leave. A comparison between the division of parental leave between adoptive and biological parents could potentially help us learn more about the mechanisms behind the parents' decision on division of parental responsibilities of a newborn child. Thus, this method is a way to investigate if the fact that women give birth influences the division of parental leave.

Given that parents select into adopting a child, and that adopting a child is not precisely equivalent to having a biological child, a comparison between adoptive and biological parents does not constitute a "natural experiment". However, such a comparison could still be informative if efforts are made to attain samples of couples that are as similar as possible. To achieve this, a matching strategy is used to attain samples of biological and adoptive parents that are as comparable as possible. However, since adoptive and biological parents may still differ in unobserved characteristics even when comparing couples with similar observable characteristics, my approach is mainly descriptive.

Also, when comparing couples in the matched sample the situation for adoptive and biological parents differs in some ways. For example, couples who adopt have gone through a long and often costly process and need to fulfill various requirements to be approved. Adopted children are usually older than newborn when coming into their new family. As an additional strategy, and as a way to control for unobserved differences between couples who adopt and biological parents, I compare the division of parental leave associated with having biological versus adopted children within the same family. By studying the within family variation in the division of parental leave between different children, I control for both observed and unobserved family characteristics that might affect the parents' choice.

My estimates indicate that both adoptive mothers and fathers spend less time on parental leave during the child's first two years in the family when adopting compared to biological parents, but that there is only a small difference in the mother's share of parental leave. Estimates are remarkably similar in the cross-section and within-family estimations.

The paper is organized as follows: Section 2 provides background information about adoptions in Sweden and the institutional setting such as the parental leave system. Section 3 contains a theoretical discussion on the potential connection between the mother's birth giving status and the parents division of child care, results from earlier studies and hypothesis. Section 4 presents the identification and empirical strategy. In section 5 the data sources and sample restrictions are described and some descriptive statistics is provided. The results are presented in section 6. Finally, section 7 provides a concluding discussion.

2 Institutional background

2.1 Adoptions in Sweden

Since the 1940s a few thousand children have been adopted every year in Sweden.[1] From around the mid-1970s a majority of the children are adopted from abroad. The most common countries to adopt from have been South Korea, India, Columbia and China. Among children who were adopted in 2013 the most common countries of origin were China, India, Thailand and Russia. Since almost all adoptions in Sweden during the years studied in this paper are transnational, this section focuses on the conditions for this type of adoptions (DN, 2015).

In the last 15 years, the number of international adoptions have been decreasing from around 1,000 a year per year, to about 300-400 in the last few years. This is mostly due to an increase in internal adoptions in countries like China and South Korea where a growing middle class has resulted in an increased demand to adopt within the country. This is the main reason why children who are adopted internationally are now older and an increasing proportion of them have special needs. In 2003 50 % of all adopted children were below the age of one, and 75 % were under two years old when adopted to Sweden. In 2011, 25 % were under one year old and 50 % were under two. In 2016, only about 10 % were below one year old, 41 % were between one and two, and 22 % were between two and three.[2] Until the mid-2000's, adoption agencies often had a hard time finding homes for orphan children with special needs. Since then the number of children needing adoption has been decreasing and the number of local parents wanting to adopt a child has been increasing. This has led to lengthening waiting times and more and more Swedish couples chose to adopt children with special needs rather than to wait for many years and risk not getting a child at all. In the last couple of years, a majority of adopted children coming to Sweden have been children

[1] The information in this section comes from the homepages of Statistics Sweden, the Swedish Family Law and Parental Support Authority (MFoF), the National Board of Health and Welfare (Socialstyrelsen), the Swedish Social Insurance Agency (Försäkringskassan) and Swedish adoption agencies.

[2] Sources: DN (2012) and Adoptionscentrum (adoptionscentrum.se).

with special needs. However, many adoption agencies in developing countries use a broader definition of special needs then most western countries, that includes conditions that do not entail severe physical or mental disabilities. For example, chronical illnesses, sight and hearing problems, heart disease, cleft palate and other physical malformations, phycological diagnosis, or that the child is older or has been through trauma, are defined as special needs in many countries. Many of these conditions can be treated by the Swedish health care system, which means that many adopted children with special needs are treated and have a full recovery after arriving in Sweden. There are no available statistics on the type of special needs that children adopted by Swedish couples have.

The process of transnational adoptions usually takes several years. To start the process the couple applies to the social services in their municipality. A social worker who is assigned to their case makes an evaluation of whether the couple is suitable to adopt a child. The legal requirements for adoption are that both parents are above 25 years old and that the couple is married. However, when being investigated by the social services the couple must also provide information on their education, profession, family relations, health and religious views. This information is gathered to get a better picture of the applicants and because many adoption countries have their own demands it's important to investigate if the couples meet them. The social worker interviews the couple and their friends or family members and writes a report that is handed over to the politically appointed municipal social welfare committee. The committee can either approve or reject the couple as qualified to adopt. The applicants' financial situation, marital stability and physical and mental health are typically considered important for approval. If the couple receives consent from the municipality to adopt they can then turn to one of the authorized adoption agencies. The chosen agency sends a request for adoption together with the social worker's report to local adoption organizations in one or several countries. After sending a request to a local organization in a country from which the couple wishes to adopt, there is usually some waiting time, sometimes several years.

When there is a child available for adoption the adoption organization contacts the applicants who must again apply to the municipality to get the adoption approved. Typically they must then travel to the child's country to retrieve the child at short notice: planning the timing of when to retrieve the child is usually not possible. The couple might then be expected to stay in the child's country from a week up to a few months. Since the adoption has been approved beforehand by Swedish authorities, the child can usually get a Swedish citizenship immediately after local authorities in the child's home country have approved of the adoption. After the child has been brought to Sweden the parents must again contact Swedish authorities such as the Tax Agency (Skatteverket) and the Swedish Social Insurance Agency (Försäkringskassan) and do a medical checkup of the child.

Besides being a time consuming process, adopting a child from abroad can also be expensive. *Adoptionscentrum*, the largest adoption agency in Sweden, recommends that couples intending to adopt should count on a total cost of approximately 270,000 SEK ($\approx$ 30,000 US \$). To cover the expenses the couple can apply for an adoption allowance from the Swedish Social Insurance Agency of up to 40,000 SEK.[3]

2.2 Parental leave regulations for biological and adoptive parents

The Swedish parental leave consists of 16 months of paid leave, out of which 13 months are reimbursed at 80 % of the parent's previous earnings and 3 months on a flat-rate. There are to date three quota months for both parents, popularly known as the "daddy-months", which were introduced in 1995, 2002, and 2016. All of the other months can be divided between the parents in any way they see fit.

Mothers take about 75 % of all parental leave days. The fathers' share of parental leave days has increased slowly from 0.5 % in the introductory year of 1974, to 5 % in 1995, and then gradually to 25 % in 2013.

The rules for taking parental leave are essentially the same for adoptive and biological parents. The most important difference is in when the parents can go on parental leave. For a biological child the mother can use the parental leave from 60 days before the planned birth date of the child and the father from the child's actual birth date. For adopted children both parents can use the parental leave from the day that the child came into their care i.e. when picking up the child in its birth country.

Until 2014 (which covers the period of study in this paper) biological parents could use the parental leave until the child turned 8 years old. Adoptive parents could use the leave for eight years from the day the child came into their care, but not after the child turned 10 years old.[4]

Parents of a child with special needs get the same number of parental leave days, but in addition get 10 extra days of leave a year known as "contact days".[5]

In addition to the parental leave days mentioned above, someone who is pregnant can get a special pregnancy benefit if they cannot work during their pregnancy for medical reasons. This of course does not apply to adoptive parents.

[3] From 1'st of January 2017 the allowance has been raised to 75,000 SEK.

[4] After 2014 parents must use most of the parental leave before the child has turned 4 years old or has been in the parents' care for 4 years.

[5] The parents get 10 contact days per child each year until the child turns 16, to divide between them as they wish. The contact days are not included in my measurement of take-up of parental leave days.

When having a biological child the mother's partner also gets 10 days of leave to be used at some time between the birth date and until 60 days afterwards. In the case of an adoption, each parent gets 5 days.

3 Theoretical discussion, earlier studies and hypothesis

Following the theory on division of labor within the household formulated by Becker (1991), the opportunity cost for parents when they are on parental leave should matter since it affects the economic optimization of take-up of leave days. Because the father often has a higher wage and expected wage trajectory, letting the mother take most of the parental leave could in many cases seem like the financially rational choice. These considerations are not necessarily affected directly by the circumstance that it is the mother who carries the child, unless the pregnancy in itself affects the parents' expected future productivity (earnings). The pregnancy could affect the mothers' earnings either through lower labor supply during pregnancy due to health reasons, or increased labor supply in order to qualify for a higher benefit level when on parental leave (which is given as a percentage of the wage). This could in turn affect her expected earnings in the labor market. Becker (1991) further argues that women are more willing to invest time and money in their child because of the larger biological investment they must make during pregnancy, giving birth and breastfeeding. They are thus more keen to get an adequate return on their (larger) investment. An initial larger investment in child care by the mother makes her relatively more efficient ("productive") in taking care of the child compared to the father. This further tips the balance in favor of letting the mother do more of the home production/child care if the couple optimizes to maximize their home production and income. Hence Becker's argument can give a theoretical explanation to the skewed division of parental leave among biological parents.

For adoptive parents, the situation in somewhat different. One might expect that waiting to receive an adopted child could increase the parents' labor supply since the adoption process is costly and good finances are a requirement for the couple to be approved as adoptive parents. However, this should hold for both spouses in an adopting couple. One could also argue that in the case of adoption the parents probably make more equivalent investments as the couple goes through the adoption process together. Paper work, interviews by social workers, monetary costs and the journey to the child's birth country to bring the child home are usually shared by the adoptive parents. According to Goldberg (2010), a survey of studies on the transition to adoptive parenthood, both adoptive parents are often highly motivated to parent. Goldberg (2010) describes the adoption process as more egalitarian, were none of the parents are affected by the hormonal changes associated with pregnancy and childbirth. Given these factors, adoptive parents should be expected to share the care of the child relatively equally. Also following Becker's argument, one would thus expect adoptive parents to make more equivalent investments in their child after adoption compared to a biological parental couple after their child's birth.

Engström et al. (2009) argue that right after birth the mother stays at home for "exogenous biological" and "traditional" reasons. This induces parents to continue to let the mother stay at home on parental leave due to present-biased preferences. The parents will put unproportionately high weight on present costs of paternity leave such as immediate income loss and social costs of breaking gender norms if they switch to the father staying at home, and underestimate future costs such as the long term impact on the mother's earnings. Thus the parents will overinvest in the mother's time at home and underinvest in the father's, even if this is not the optimal financial choice for them in the long run. The "exogenous biological reasons" that Engström et al. (2009) refer to could be for example hormonal influence, fatigue after labor and breastfeeding. They thus suggest a direct biological deterministic mechanism behind women's larger parental investment right after birth, as well as influence of "tradition" e.g. social norms.

3.1 How does mothers' birth giving status influence behavior?

In this section I discuss how pregnancy, childbirth and breastfeeding could influence parents' behavior. First of all, the pregnancy and act of giving birth can in itself induce a stronger tie between the child and mother, relative to that between child and father, in at least two ways: as a direct biological/hormonal effect and as a psychological mechanism.

This is in part due to a rise in hormonal levels during pregnancy and right after giving birth, that can been linked to mother-infant attachment; for example the levels of prolactin, that induces the production of breast milk, and cortisol (Storey et al., 2000). Perhaps surprisingly, recent studies show that also among fathers, changes in hormonal levels can be measured right before and after the birth of a child, although these changes are smaller than among mothers (Vreeswijk et al., 2014).[6] In the case of adoption these hormonal influences will not occur in anticipation of receiving the child unless they can be induced by pure psychological mechanisms and, to the best of my knowledge, there is no scientific evidence on this matter.

It has also long been known that biological parents, in particular mothers, can form an emotional bond with their child already during pregnancy. Research in this area has focused mostly on mother's prenatal connection with

[6] The fathers' level of prolactin and cortisol increase and testosterone levels drop which can affect both psychological and physical functions. These changes only occur provided that the father is present during the pregnancy and birth.

the child and the positive consequences for the postnatal mother-child connection (Rydén, 2004). Fathers have been found to feel a mix of emotions such as pressure to provide for his family, a feeling of being left out during pregnancy and finding it difficult to form an emotional bond with the child due to the lack of biological connection to the fetus (Draper, 2003, Rydén, 2004). Studies show that first time fathers develop an increased emotional bond to the fetus that becomes more intense later in the pregnancy, but that prospective mothers on average feel more connected to their unborn child than the fathers to be.[7]

In conclusion, there are both biological and psychological differences in how biological fathers and mothers are affected by pregnancy. Both mothers and fathers are affected by hormonal changes, although mothers are more affected, and the fact that the mother carries the child has psychological consequences on the prospective parents' ability to connect emotionally to the unborn child.

The ability to breastfeed is another biological gender difference that could potentially explain why it is common to let the mother take the first period of leave and why women might want to stay longer on parental leave then fathers. In biological mothers lactation is a natural process that is induced by the increased levels of prolactin when pregnant. Ability to breastfeed can also be induced in non-pregnant women but with more difficulty (Cheales-Siebenaler, 1999). Since, generally speaking, adoptive mothers are not able to breastfeed, this is one difference between adoptive and biological mothers and between biological mothers and fathers.

Among adoptive parents the bond between parents and child cannot form until the child comes into the care of the adoptive parents. Even when adopting very young children, the adoptive parents are usually not present when the child is born or in the first days of the postpartum period. This is especially true for Swedish couples who usually adopt internationally. Thus, the bonding that might occur between a biological mother and child already during pregnancy or right after birth, in part due to breastfeeding, will most likely not occur in the case of an adoption. Hence, the adopting parents have more similar opportunities to adjust and prepare to become parents. Based on the above discussion one could also expect that an adoptive father is less likely to feel "left out" if the woman is not carrying the child.

If the pattern among biological parents to let the mother take the first period of parental leave is mostly induced by biology, then we would expect parental couples where the mother did not give birth to divide the leave more evenly.

[7] Vreeswijk et al. (2014) who study 301 fathers to be in the Netherlands found that 49 % of the fathers were emotionally "disengaged" to the fetus. The same feeling was found in about 17 % of mothers.

Norms and recommendations regarding breastfeeding

There is a social norm that birth mothers breastfeed their children. One example and perhaps reason for this is the World Health Organization's recommendations of exclusive breastfeeding during the infant's first six months.[8] Based on these recommendations breastfeeding is strongly encouraged by health care personnel and public health authorities in Sweden with the aim to promote health among mothers and children. However, this also reinforces a social norm in Sweden that breastfeeding is part of being a good mother. A recent study has shown that the recommendations can make non-breastfeeding mothers and those who fail to fulfill the recommendation experience anxiety and guilt (Fahlquist, 2014). This is an indication that there is a stigma placed on mothers who do not breastfeed.

Of course, to be able to breastfeed the child during the first months, the mother must take the first period of parental leave.[9] Adoptive mothers, on the other hand, are usually not expected to breastfeed since their children are often at least a few months old when adopted, and the adoptive mother is usually not able to breastfeed.

3.2 Empirical evidence

There is some empirical evidence that giving birth does induce greater time investments in the child. These studies typically compare the division of child care among same-sex and different-sex couples who adopt or have biological children.

In a recent paper, I find that in lesbian couples with children in Sweden, the child's birth mother takes a larger part of the parental leave (Moberg, 2016). The division of parental leave between the birth mother and her partner is much like that between the mother and father in heterosexual couples when comparing couples with a similar economic situation before childbirth. The result that the biological mother in lesbian couples spend more time on child care than her partner has also been found in US studies (Reimann, 1997, Goldberg and Perry-Jenkins, 2007). Since in lesbian couples both parents are of the

[8] The WHO recommend that no bottles or pacifiers should be used and no other food or drink should be given to the infant, not even water. This recommendation came into place after many children had died in developing countries when infant formula was mixed with contaminated drinking water. However, although the quality of drinking water differs across countries, the recommendations were made global. Information about the WHO recommendation has been gathered from the WHO official homepage: *who.int/nutrition/topics/exclusive_breastfeeding/en/*

[9] Table A.1 in the appendix describes the rates of breastfeeding, legislated length of parental leave and employment levels among mothers with young children in Sweden, the USA, Canada, Ireland and Taiwan: five countries with different cultures, labor markets and welfare systems. The table reveals that a majority of mothers initially breastfeed their child, but that rates drop continuously in the first few months. A minority of mothers follow the WHO recommendation.

same sex, biological differences between men and women cannot explain this pattern of behavior. Instead it seems that the birth giving status is a determining factor. One possible explanation is that this is a direct biological consequence of pregnancy, for example hormonal build up, fatigue after childbirth and lactation that makes breast feeding possible. In fact, when the couples in Goldberg and Perry-Jenkins (2007) were asked for the reasons for their choice of division of child care, the couples mentioned biological factor such as breastfeeding as the main reason (Goldberg and Perry-Jenkins, 2007). If the birth mother is induced to do more of the initial care work for her child due to biological factors, then this could explain the uneven division of parental leave among lesbian parents as was found in Moberg (2016). In that case, the fact that the heterosexual mothers take more parental leave than fathers could be a consequence of biologically induced behavior caused by the fact that she is the one to give birth, rather than other biological differences between heterosexual parents or gender norms.

Another possible explanation is that lesbian couples are affected by the same social understanding of what being a "good" mother entails as heterosexual couples are: That a woman who gives birth to a child is expected to be the main caregiver, at least in the first period of the child's life. It has been suggested that lesbian mothers incorporate the ideas for heterosexual parenting and motherhood, in which case their behavior has more to do with social norms than biology (Hayden, 1995). If there is a stigma attached to a woman who has given birth leaving her newborn infant in the care of someone else, this could affect all birth mothers, regardless of whether they have a male or female partner. If this is a correct assessment of the social norms present in for example Sweden, then a mother who did not give birth to a child, for example a lesbian partner or an adoptive mother, would not be expected to invest as much time in the child as a birth giving mother. This hypothesis has been confirmed in Holditch-Davis et al. (1999) who conduct an observational study comparing 21 adoptive and 19 biological parents' interaction with their children. They find that both adoptive and biological fathers spent less time taking care of the child than the mother, but that the difference between parents was smaller among adoptive parents. The result that also adoptive couples exhibit some specialization is also found in Goldberg et al. (2012) who compare same-sex and different-sex adoptive parents. They find some degree of specialization in all types of couples, but more so in heterosexual than in lesbian and male gay couples who adopt. As Ciano-Boyce and Shelley-Sireci (2003) show, heterosexual couples' division of child care is also more unequal than that of lesbian couples where one partner gave birth. Thus, it seems like heterosexual couples are the most influenced by traditional gender norms whether there is any biological connection to the child or not. Potentially, couples' adjustment to gender norms and tendency to specialize could be a strategy to achieve acceptance as a legitimate or "normal" family constellation.

Compared to the current paper, the samples used in the above-mentioned studies (except Moberg, 2016) are small and not necessarily representative, and the data is usually collected using self-reporting over few time periods. Also, most of these studies do not take measures to account for the samples of different types of couples being inherently different in characteristics before entering parenthood.

3.3 Hypothesis

The theoretical discussion in the beginning of this section indicated that we should expect the division of child care to be skewed towards mothers among biological heterosexual parents, and to be more even among adoptive parents. One reason for the difference is the larger investment that biological mothers make in the child during pregnancy. Also, when considering biological factors that could influence parental behavior, the discussion points to it being reasonable to expect biological motherhood to influence the time spent with the child. Biological mothers experience greater hormonal changes during and after pregnancy than biological fathers and adoptive parents, and have an easier time forming an attachment to the child already during pregnancy. They are also able to breastfeed the child, something that is usually not possible or expected from adoptive mothers (or fathers). The existing empirical evidence discussed thereafter do to some extent confirm the hypothesis that adoptive parents should have a more even division of child care. At the same time, heterosexual adoptive parents show more of a tendency to specialize than lesbian and gay couples even in the cases when one of the partners in the same-sex couple is a biological parent. Thus, the gender composition of the couple also seems to be important.

In conclusion, the above discussion indicates that biological parenthood, especially mothers' birth giving status, can be expected to induce a skewed division of parental responsibilities and thus parental leave. Based on this discussion, a reasonable hypothesis it that (heterosexual) adoptive parents can be expected to have a more even division of parental leave, than (heterosexual) biological parents. However, as adoptive parents can be influenced by other factors, such as financial considerations, gender norms and possibly other biological gender differences, it is not reasonable to expect them to have a completely egalitarian division of parental responsibilities.

4 Identification and empirical strategy

The aim of this paper is to investigate to what extent the fact that the mother gives birth can explain that she is usually the one to be the main caregiver to a small child. When investigating this question, several methodological challenges arise. As strategies to tackle these challenges I, first, conduct a matched cross-section comparison between couples who adopt a child and couples who have a biological child together. Second, a within couple comparison of the division of parental leave between the same parents but for different children: adopted and biological. Since same-sex couples with children are much fewer and have different biological and legal restrictions I only compare heterosexual couples.

4.1 Comparing adoptive and biological parents

Couples select into adopting children, either because they can't have a biological child or because they rather adopt. Among couples who cannot have a biological child there is selection into adopting a child based on preferences as well as variables associated with the requirements that the couple must meet to get the authorities' permission to adopt such as income, health and marital stability (see section 2.1). They must also be willing to go through this process. Since many adopting couples have first tried to have a biological child, they are usually older and have waited longer to become parents.

The adoptive parents are thus likely to be different from biological parents in both observable and unobservable characteristics. As these characteristics might affect the division of parental tasks, they could bias an OLS-estimator of the effect of having an adopted child in a cross-section comparison. Even if it were possible to control for all observable characteristics the OLS-estimator would still not capture a causal effect of adoption (compared to giving birth) since the compared couples could be different in unobserved ways. Therefore, only a correlation between birth-giving status of the mother and her share of the care work can be captured in this type of comparison (see further discussion on potential threats to identification in section 4.4).

4.2 Between couple comparison

In an attempt to correct for the selection bias described above adoptive and biological parents are matched based on observable characteristics. This does not correct for selection on unobservable characteristics unless they are perfectly correlated with the matching variables, but it makes the comparison between couples more reasonable. I use a matching strategy that is in line with the one in Mörk et al. (2013) and Lundin et al. (2008). All couples are divided into H different household types: each type representing a unique combination of values of specific household characteristics. Only household types where there are both couples with biological and adopted children are kept in the sample. To make the comparison more appropriate, only first time adoptive or biological parents are included. The aim of the matching is to get samples of biological and adoptive parents that are as similar as possible in characteristics that might affect the outcome variable: division of parental leave. The exact method of constructing household types is described in section 5.3.

Equation 15 is estimated on the matched sample of adoptive and biological parents.

$$MomsShare_{ih} = \alpha + \beta X_i + \theta Adopted_i + FE_h + \varepsilon \tag{1}$$

The outcome variable *MomsShare* is the mother's share of the parents' total take-up of parental leave days during the child's first two years for child i in household type h. I choose the mother's share of leave during the child's first two years as outcome variable to best capture how the parents' initial division of parental responsibilities is influenced by the mother's birth giving status. Because of the length of the Swedish parental leave (16 months) I include leave during the first two years as I might otherwise exclude leave taken by fathers.

X_i is a vector of control variables for child i and child $i's$ parents. $Adopted_i$ is a dummy variable indicating whether child i is adopted. ε is an error term.

The term FE_h is a vector of dummy variables (fixed effects): one for each household type. Including the fixed effects-term affects the model by adding or subtracting to the constant α so that each household type gets a unique intercept. Thus, any variation in mothers' share of parental leave days between adoptive and biological parents captured by θ comes from variation *within* household types. The parameter of interest, θ, captures the conditional difference in the mother's share between biological and adoptive parents. As discussed above the model does not control for unobserved couple characteristics. Still, estimates from this model are informative on behavioral differences between adoptive and biological parents after controlling for a large number of household characteristics.

4.3 Within couple estimation

As an additional way to overcome the identification problems caused by couples selecting into having biological and adopting children, the difference in mothers' share of parental leave between adopted and biological children is estimated using only the variation between biological and adopted children within the same families. Equation 11 is estimated on the subsample of parents that have both at least one biological and one adopted child. It is similar to equation 15 but with family specific fixed effects instead of household type dummies. The family specific fixed effects capture unobserved family characteristics that can affect the mother's share of parental leave. The advantage, compared to the between-couple comparison, is that this model controls for these characteristics, something that is not possible in the between-couple estimation. Most adopted children are adopted by childless couples and are thus their parents' first child. In a family with several children the choice of for example division of parental leave for different children cannot be seen as independent of each other. It might be that parents establish a certain pattern of behavior when having their first child which is later used as the default choice when deciding on the division of parental leave for their next child. If for example the couple's first child is a biological child, and their behavior is therefore influenced by biological factors, this influence could also affect their behavior when adopting a child later on. A variable for whether the child is the parents' first child is therefore added. The error term is clustered at family level, j.

$$MomsShare_{ij} = \alpha + \beta X_i + \theta Adopted_i + \delta FirstChild_i + FE_j + \varepsilon_j \quad (2)$$

The θ-parameter here captures the association between adoption status of the child and the mother's share of parental leave days when estimated using the within-family variation between children that were adopted and the family's biological children.

By estimating the effect of birth-giving status on the within family variation the model controls for unobserved family characteristics that might otherwise bias the estimate. Thus, this method increases the internal validity of the estimates but decreases the external validity since couples that have both adopted and biological children are a specific subsample of families that could be different from other couples in many ways.

4.4 Potential threats to identification

There are at least two major challenges to identifying the effect of the mother's birth giving status by comparing the division of parental leave among adoptive and biological parents. One is that couples select into adopting a child in a non-random way. The methods used to overcome this problem are described above: first, the matching strategy in which couples with similar characteristics are compared, and second the within-family estimations were the variation in division of parental leave between children of the same couple is studied, thereby holding family characteristics constant. However, even when comparing couples that are similar in characteristics that can be observed in data, there might still be unobserved differences between them. The other major challenge to identification is that, even in the absence of selection into ways of entering parenthood, the event of adopting a child is different from becoming parent to a biological child in ways that could influence parents' behavior. Below, these challenges are considered in separate sections.

Selection on unobservables

Couples who apply for adoption must meet a number of requirement, some that are not easely observed in data. For example, couples can be rejected as adopting parents based on medical history and health issues that do not hinder people from having biological children. Many adopting countries require that both parents have a BMI that is under some threshold and that they are both in good physical and psychological condition. Thus, adoptive parents might on average be healthier than the biological parents. During the adoption process, a social worker conducts several interviews with the couple and if he/she deems the relationship unstable, this could also lead to rejection of the couple's application. Again, an unstable marriage does not hinder pregnancy (even if it might make is less likely). There may also be differences in values or religious beliefs as some adopting countries require that the adopting parents have a religious faith, usually Christian.

In addition, adoptive parents are probably in general more motivated to become parents. The adoption process requires large investments both in terms of time and money from both parents. The couple must also be willing to parent a child which they have no biological of social connection to and undergo the scrutiny of social workers and adoption agencies both at home and in the adopting country. Finally, since the process of adopting a child could take several years, the adoptive parents have had longer time to plan the child's arrival. Thus, they might be better prepared since they have had more time to adjust their circumstances to entering parenthood.

If couples' division of parental leave is influenced by these factors, they will induce bias in the OLS-estimator as there is no way to control for them in the between-couple comparison. One way to handle this is to use the within-family estimation method. This strategy controls for all the couple's characteristics, both observed and unobserved, through family specific fixed effects. A potentially problematic issue when estimating the difference in mothers' share of parental leave with different children within the same family, is that the couple's choices of division with different children might be dependent on each other. For example, the couple might form a pattern of behavior when deciding on the mother's share with the first child, that they then apply to their decision for their second child whether or not it is adopted or biological. Hence, it is not certain what the division chosen for an adopted child, if it is the family's second child, is a good proxy for what the division *would have been* had the family's first child been adopted, and vice versa. Estimating the difference in division between children within the same family thus risks underestimating the "true" difference in parents' preferred division of leave with adopted and biological children, had they not been influenced by previous patterns of behavior. Even though this concern is not possible to solve methodologically when estimating the difference with-in families, this method is used as a complement to the between-couple comparison as a way to perform estimations while controlling for unobserved characteristics.

Differences between entering parenthood through adoption or pregnancy
The other challenge to identification is that, even when comparing couples with similar characteristics or children within the same family, the event of adopting a child is still different from becoming parent to a biological child in

more ways than just the mother's birth giving status. Thus, when estimating the association between adopting a child and the division of parental leave, the θ-parameter might also capture the influence of those differences.

One such difference is in the age of the child when it enters the family. Adopted children are typically not newborns but rather a few months or years old.[10] An older child has different needs compared to an infant. The child is less dependent on the parents and can participate in more activities. Among biological parents, fathers typically take their parental leave days when the child is older (not an infant). If fathers prefer staying on leave with an older child, then the age of the child when adopted could influence the division of leave among adopted parents. In addition, Swedish municipalities are obligated by law to provide day care to all children within a few months from when they turn 1 years old. Hence, there may be less of a practical need when adopting an older child to stay on parental leave for a long time. When the total length of leave is shorter, parents might choose a different division of leave.

Another difference in circumstances is that an increasing share of adopted children have special needs. The increase has been gradual, picking up speed in the past ten years. Before that most adopted children had no known special needs at the time of the adoption (see section 2.1). As described in section 2.1 however, many conditions that are defined as a special needs in the adopting countries are conditions that can be corrected through surgery and other types of treatments. Thus, it is not certain that a child with special needs at the time of adoption will need more care than other children later. Having a child with special needs could affect parents in many ways. For example, it might induce both parents to invest more time in the child to better meet its needs. Alternately, it might reinforce a more traditional division of labor were the mother reduces her working hours to spend more time taking care of the child and the father takes on the role of breadwinner. Previous research from Sweden and the US indicates that the latter is the more common response among parents of children with special needs.[11] If turning to traditional gender roles is the response also among adoptive parents with more demanding special needs, then we might expect the mother's share of parental leave to be larger for these families.

If these factors; that the child is older, that child care outside the home is more available, and that adopted children more often have special needs, influence adoptive parents' choice of division of parental leave, then that could affect the estimate of the θ-parameter. The estimate would then be biased in the sense that it would not only capture the difference in mothers' share caused by her birth giving status but also the effect of other circumstances associated with adoption.

[10] As described in section 2.1 the age of adopted children has increased over time. In 2003, a majority were under one year old when adopted. Today, it is only about 10 %.

[11] See ISF (2013): a study on Sweden and overview of research in this area.

As a way to handle this issue, in the regression analysis I include control variables for the child's age when adopted in one of my specifications to investigate how this affects the estimate. Since no information is available about children's special needs, it is not possible to judge in what way this affects the estimations. However, during most years in the study period (1994-2009), the proportion of special needs children was smaller. Also, since children with special needs are usually somewhat older when adopted, controlling for age at adoption to some extent also controls for any effect of adopting a special needs child.

5 Data, sample restrictions and descriptive statistics

In the empirical analysis, Swedish population-wide register data is used. The data contains information on socioeconomic variables such as age, sex, earnings, municipality of residence, type and level of education for all residents of Sweden between 16 and 65 years old for the years 1990 to 2010. The multi-generation register, which links parents with their biological and adoptive children, is used to identify couples that adopted or had biological children together up until 2009. The data contains information about the child such as country of origin, year and month of birth. To measure the time spent on parental leave I use detailed information on the take-up of parental leave benefits (PLB) from the Swedish Social Insurance Agency (Försäkringskassan). It contains information on the exact dates on leave, by which parent it was taken, for which child, and the amount of benefits given, during the years 1994-2012. Combining these data sources, I collect information on all couples who had biological or adopted children together between 1994 and 2009.

As measurements of the use of parental leave, I construct a measure of each parents' total take-up of parental leave days during the child's first two years of life or, in the case of adoption, during the first two years that the child was in the adopting parents' care. The motivation for this is both theoretical, as described in section 4, and practical: The parental leave data only includes parental leave spells that have been finished by the end of 2012. Thus, for children born in late 2009 (the last cohort of children) parental leave data for the child's two first years of life (2010 and 2011) are the only years for which I can be sure to capture all leave days taken for those children. Since I want to measure the use of parental leave days in the same way for all children, I restrict the measurement of parental-leave benefits to spells that begun during the first two years that the child spends in the family.

The mother's share of parental leave for child i in family j is calculated as the mother's fraction of the parents' total number of net parental leave days:

$$MomsShare_{ij} = \frac{Mother'sDays_{ij}}{Mother'sDays_{ij} + Father'sDays_{ij}} \quad (3)$$

5.1 Sample restrictions

Turning first to the cross-section estimation, the sample is restricted to couples who had their first child together during the years 1994-2009 and the estimations are performed using parental leave information for their first child only. This is to make the samples of adoptive and biological parents more comparable since most couples who adopt do so because they are unable to have a biological child and thus they become first time parents when adopting. A few other restrictions are imposed due to the legal requirements made on adoptive parents. Since marriage is a legal requirement for adoption, only married couples are included. Another condition is that both adoptive parents are above 25 years old, and hence this restriction is also imposed. Couples where parental leave data is missing for both parents are dropped from the sample.

After implementing these restrictions 150,787 couples who had their first biological child together and 5,527 couples who adopted their first child together are left in the sample.

For the within family estimations, all couples who adopted at least one child and have at least one biological child together are sampled. Again, couples where parental leave data is missing for both parents for at least one child are dropped from the sample. This sample consists of 987 couples. Since this is a limited number of parents, no further restrictions are imposed.

5.2 Descriptive statistics

Table 1 presents descriptive statistics for biological and adoptive parents the year before their first child was born or adopted, except age (measured at the time of entering parenthood) and total number of children (measured in 2009 for all couples). Comparing first time parents (in the cross-section sample) adoptive parents are on average older, slightly more educated, have higher employment rates and labor earnings, and are more often born in Sweden.

Most biological parents have a second child (completed fertility is measured in 2009 for all couples), but only about half of the adoptive parents adopt another child or have a biological child later. Also, sometimes adoptions are of sibling pairs. The adopted children are most commonly from Asia (62 %), Eastern Europe or the former USSR (15 %), or South America (14 %) and are on average 1.44 years (17 months) old when adopted. Only 2 % of the adopted children were born in Sweden.

Biological parents more often live in metropolitan municipalities compared to adoptive parents (indicated as "Metropolitan" in the table). Among adoptive parents it is most common to live in large cities, which is defined as cities with more than 50,000 but less than 300,000 inhabitants. Turning to the parents' take-up of parental leave days (PL-days), both biological mothers and fathers take more parental leave than adoptive parents.

The partners in couples who have both biological and adopted children are in-between the other two groups when it comes to their ages when becoming parents. Their education and employment rates are more similar to that of first time adoptive parents, but their labor earnings are lower. They are also more similar to the adoptive parents when it comes to the parents having Swedish background and their type of municipality of residence. The child's region of origin and number of days on parental leave indicates the values for the couple's first child, which in about 50 % of the cases is an adopted child.

Table 1. *Descriptive statistics - all couples.*

	Biological parents	Adoptive parents	Both adopted and biological child
Year of birth/adoption	2002	2002	1999
	(5)	(4)	(4)
No. of kids, mother	1.9	1.5	1.5
	(0.7)	(1.0)	(0.8)
No. of kids, father	1.9	1.5	1.5
	(0.7)	(1.0)	(0.8)
Mother's age	30.5	35.9	32.9
	(3.5)	(4.5)	(4.4)
Father's age	32.9	37.6	34.6
	(4.6)	(5.1)	(4.9)
Mother's years of schooling	12.4	13.1	13.3
	(2.9)	(2.7)	(2.6)
Father's years of schooling	12.3	13.1	13.5
	(2.9)	(2.5)	(2.7)
Mother employed	0.89	0.97	0.96
	(0.31)	(0.17)	(0.19)
Father employed	0.93	0.98	0.97
	(0.26)	(0.15)	(0.17)
Mother's earnings	237,764	250,238	224,219
	(133,947)	(134,292)	(129,479)
Father's earnings	308,434	334,556	298,069
	(219,631)	(200,000)	(184,893)
Share girls	0.49	0.53	0.49
	(0.50)	(0.50)	(0.50)
Swedish mother	0.77	0.89	0.89
	(0.42)	(0.31)	(0.31)
Swedish father	0.79	0.91	0.92
	(0.41)	(0.28)	(0.27)
Sweden	1.00	0.02	0.53
	(0.00)	(0.14)	(0.50)
Eastern Europe, fmr USSR	0.00	0.15	0.07
	(0.00)	(0.35)	(0.25)
South America	0.00	0.14	0.10
	(0.00)	(0.35)	(0.30)
Sub-Sahara Africa	0.00	0.06	0.03
	(0.00)	(0.23)	(0.17)
MENA region	0.00	0.01	0.01
	(0.00)	(0.11)	(0.07)
Asia	0.00	0.62	0.27
	(0.00)	(0.48)	(0.45)
Age at adoption	.	1.44	1.26
	(.)	(0.99)	(1.03)
Metropolitan	0.30	0.23	0.25
	(0.46)	(0.42)	(0.43)
Suburban	0.19	0.19	0.19
	(0.39)	(0.40)	(0.39)
Large cities	0.30	0.29	0.31

continued

Table 1. *Descriptive statistics - all couples.*

	(0.46)	(0.45)	(0.46)
Densely populated	0.20	0.24	0.22
	(0.40)	(0.43)	(0.42)
Sparsely populated	0.02	0.04	0.03
	(0.14)	(0.19)	(0.16)
Mother's PLB	534.85	526.13	488.21
	(154.03)	(138.11)	(123.40)
Father's PLB	602.03	607.88	554.56
	(148.90)	(138.96)	(111.13)
Mother's PL-days	269.14	176.44	233.75
	(111.61)	(103.78)	(114.19)
Father's PL-days	60.61	35.25	46.98
	(70.51)	(50.95)	(64.45)
Number of couples	150,787	5,527	987

Note: Descriptive statistics (means) for biological and adoptive parents, indicating values the year before the child was born or adopted. Earnings in SEK (1 USD ≈ 8 SEK), 2008 prices. Standard deviations in parentheses.

5.3 Construction of household types

The aim of the matching is to get samples of biological and adoptive parents that are as similar as possible in characteristics that might have an impact on the likelihood of adopting a child and the outcome variable: division of parental leave. Rather than using propensity score matching, where matched couples would have the same propensity score but have different values of specific covariates, exact matching is used.

As couples who wish to adopt must have a stable financial situation for the adoption to be approved and to afford the costs of adoptions, a couple's earnings level affects their likelihood of adopting a child. However, earnings a specific year could reflect a temporary setback in the labor market rather than long term earnings potentials. Also, the earnings of biological mothers could be affected by the pregnancy close to the child's birth. Rather than matching on earnings directly, couples are matched on variables that might determine the spouses' earnings potentials, namely the mother's and father's age, and type and level of education. As a baseline matching strategy, I divide all couples into household types based on the birth/adoption year of the child, the mother's and father's age, and type and level of education before the child's birth/adoption. The parents' educations are indicative of the parents' financial situation and position in the labor market. Another reason to match on the spouses' ages is that most adopt because they cannot have biological children and are thus on average older when becoming parents. Besides being more likely to adopt, older couples have come further in their careers, and might have different priorities or preferences that could influence their willingness to spend time on parental leave. The year of adoption/child's birth is included because the parental leave system was reformed several times over the period, for example by including quota months for fathers, and the level of benefits increased continuously. Thus, the time of birth or adoption could affect the parents' take-up decisions. It is therefore reasonable to make comparisons between couples who had their first biological child or adopted a child in the same year and thus were subject to the same rules. The number of children available for adoption has also changed over time which could change the composition of adopting couples.

All observations are placed into a specific household type based on their values of the matching variables. The age variable is divided into spans of 5 years for each spouse starting at 25 years of age. The variable for type of education has 10 different categories (general, pedagogical, arts, law and administration, science and computing, production, farming, health care, services, and other). The variable for level of education is divided into the categories "less than high school", "high school" and "college". Each birth/adoption year is one category. There is one household type for each possible combination of the values of the matching variables, in total 32,718 combinations. All children in a particular household type were adopted or born in the same year and the mothers and fathers are in the same age categories (in spans of 5 years), and have the same type and level of education (less than high school, high school or college). Only household types where there is at least one adopted and one biological child are kept in the sample. After this restriction 3,787 couples who adopted and 41,569 couples who had their first biological child together remain, out of the 5,527 adoptive and 159,787 biological parents in the original sample, divided into 2,670 household types.

Table 2 describes parental characteristics, collapsed at household level, for the parents of adopted and biological children in the matched sample. The data is collapsed at household level since this is the level at which the difference in the outcome variable between adoptive and biological parents is estimated. The table shows that the matched couples who have been assigned to the same household type are similar also in other observable characteristics than the matching variables. Even though the matching on parents' age is made on age-spans of 5 years, the average parental age is very similar among adoptive and biological parents in the matched sample. Importantly, compared to the statistics for all couples (table 1) biological and adoptive parents are now much more alike when it comes to employment status and earnings. This is reassuring that matching on variables that might affect potential earnings also creates a good match on earnings. The couples are also more similar when it comes to the parents' total number of children and Swedish background, but there are still some differences in the type of municipality in which the families live. The number of variables to match on depends to some extent on the quality of matches attained. Including many variables inevitably results in fewer matches, which makes the estimations less reliable. Matching on the variables described above gives a reasonable balance between attaining many matches and high quality matches, i.e. similarity in parental characteristics. Finally, the parents' level of parental leave benefits (PLB) are now more similar, which is a consequence of the similarity in earnings levels, but the take-up of parental leave days (PL-days) is a lot lower among adoptive parents.

Table 2. *Descriptive statistics - matched couples.*

	Biological parents	Adoptive parents
Year of birth/adoption	2001	2001
	(4)	(4)
No. of kids, mother	1.7	1.5
	(0.5)	(1.0)
No. of kids, father	1.7	1.5
	(0.5)	(1.1)
Mother's age	34.3	34.7
	(3.9)	(4.0)
Father's age	36.5	36.6
	(4.8)	(4.7)
Mother's years of schooling	13.0	13.0
	(2.4)	(2.5)
Father's years of schooling	13.1	13.1
	(2.3)	(2.3)
Mother employed	0.95	0.98
	(0.16)	(0.13)
Father employed	0.97	0.99
	(0.13)	(0.11)
Mother's earnings	243,759	241,308
	(111599)	(121988)
Father's earnings	326,981	325,367
	(160907)	(177262)
Share girls	0.50	0.51
	(0.30)	(0.46)
Swedish mother	0.86	0.91
	(0.25)	(0.27)
Swedish father	0.88	0.93
	(0.23)	(0.23)
Sweden	1.00	0.02
	(0.00)	(0.14)
Eastern Europe, fmr USSR	0.00	0.13
	(0.00)	(0.31)
South America	0.00	0.16
	(0.00)	(0.34)
Sub-Sahara Africa	0.00	0.06
	(0.00)	(0.22)
MENA region	0.00	0.01
	(0.00)	(0.09)
Asia	0.00	0.62
	(0.00)	(0.45)
Age at adoption	.	1.37

continued

Table 2. *Descriptive statistics - matched couples.*

	(.)	(0.90)
Metropolitan	0.30	0.20
	(0.29)	(0.37)
Suburban	0.21	0.19
	(0.25)	(0.36)
Large cities	0.27	0.31
	(0.27)	(0.43)
Densely populated	0.19	0.26
	(0.25)	(0.40)
Sparsely populated	0.02	0.04
	(0.10)	(0.18)
Mother's PLB	527.45	521.32
	(118.71)	(130.93)
Father's PLB	603.20	605.27
	(119.13)	(133.23)
Mother's PL-days	285.00	177.69
	(71.27)	(97.31)
Father's PL-days	58.89	35.39
	(44.50)	(46.65)
Number of couples	41569	3784
No. of hh-types	2670	2670

Note: Descriptive statistics (means) for biological and adoptive parents in the sample of matched couples used in the regression analysis. The statistics indicate values the year before the child was born or adopted. Earnings in SEK (1 USD $\approx$ 8 SEK), 2008 prices. Standard deviations in parentheses.

6 Results

In this section presents estimates of the association between the child being adopted and the mother's share of days on parental leave. Results are presented for the cross-section sample of matched couples, and the sample of couples who have at least one biological and one adopted child. As an extension equation 15 and 11 are also estimated on the mother's and father's take-up of parental leave days separately. This is to further investigate the relationship between biological parenthood and the parents' choice of time spent at home with the child.

6.1 Between couples estimations

Table 3 presents the results when estimating equation 15 on the sample of matched couples described in table 2. The outcome variable in table 3, mother's share of parental leave days, has been scaled up by 100 so that all coefficients can be interpreted as percentage points.

Model (1) is the most basic model in which no control variables are included except household type fixed effect. Since the household types are constructed using child's birth or adoption year, parents' ages, and types and levels of education, including the household type fixed effects is a way of controlling for these variables, as only couples within the same household type are compared with each other when estimation the coefficient for *Adopted*. Column (1) reveals that there is a small positive correlation between having an adopted child and the mother's share of the couple's take-up of parental leave days. The estimated θ-parameter of 0.899 can be interpreted in the following way: in couples who adopted a child the mother's share of the parental leave is almost one percentage point larger than in couples within the same household type who have a biological child. The constant indicates that the baseline level of the mothers share of the parental leave days is 81 percent. Thus, adopting a child rather than giving birth is associated with an increase of about one percent in the mothers share of leave.

In model (2), a number of control variables for child and household characteristics are added, namely dummy variables indicating if the parents were born in Sweden and type of municipality of residence. The parents' ages when the child was born are also included as fixed effects for each year between 25 and 45 years (or older) rather than in spans of 5 years (as was used when constructing the household types). Adding these control variables decreases the estimate slightly, but does not improve efficiency.

In model (3), the mother's share of the household's total income before having children is added. If the couples choose their division of parental leave in a way that takes their financial situation into consideration then the mother's share of the couple's income could be an important factor in their choice of her share of the total time on parental leave. However, adding this variable to the model only changes the estimate slightly. This is probably not because this variable is not important but because couples within the same household types have sufficiently similar financial situations and thus this mechanism is already controlled for by adding the household fixed effects. When comparing the division of parental leave within household types while controlling for a rich set of child and parental characteristics, and thus indirectly parents' earnings levels, as in model (3), there is a small positive association between the child being adopted and the mother's share of parental leave. In this specification, the mother's share is estimated to be about 0.8 percentage points higher.

In the last model, model (4), controls are added for the adopted child's age when adopted. As described in section 4.4, if the fact that adopted children are older than newborn when adopted influences the parents' division of parental leave, than the θ-parameter would not just capture the influence of the mother giving birth but also capture this effect. Including the child's age at the time of adoption as a control variable is thus a way to correct for this potential bias. As was also discussed in section 4.4, including this variable could to some extent also control for adopting a child with special needs since these children are typically adopted at a somewhat older age. The baseline in the model is to adopt a child that is less than 6 months old. Since all biological children are zero months old when they are born, all variation in this variable comes from adopted children. Two indicator variables are included; one indicates that the child is between 6 and 18 months when adopted, the other that the child is more than 18 months old when adopted. Controlling for the child's age when adopted decreases the estimate only slightly. However, the coefficients for adopting a child that is older than six months, or 1.5 years, are relatively large and significant. The interpretation is that among couples that adopt a child who is at least 6 months old, the mother's share of parental leave is on average 3 or 4 percentage points larger. This is contradictory to what one might expect given that biological fathers tend to take more parental leave when the child is older. A potential explanation is that the variables may capture the effect of adopting a child with special needs that induce a more traditional division of labor among parents. Among couples who adopt a child that is less than 6 months, the mother's share is about 0.7 percentage points larger than among biological parents. This estimate is close in magnitude to the other models, although now it is insignificant. Thus, the difference between the mother's share of leave for biological and adoptive children is small among parents with a young child.

6.2 Within family estimations

Turning to the within-family estimations, table 4 presents the results when estimating equation 11 on the subsample of families who have both at least one adopted and one biological child (described in column 3 in table 1).

Model (1) is the same as in the cross-sample estimations, but now including family fixed effects instead of household type fixed effects. Hence, the estimations are made using only the variation in the mother's share of parental leave between different children in the same family. Including the family fixed effects is a way of controlling for time invariant (observed and unobserved) family characteristics. However, this model does not control for characteristics that change over time that might influence the parents to make different choices for different children. A specific challenge when comparing parents' behavior concerning different children is that the parents' behavior with one child might influence their behavior with their next child. The choices of parental leave division with two different children can thus not be seen as independent. This might be one reason why the positive correlation between the child being adopted and the mother's share of parental leave is smaller in model (1) in the within-family estimation than in the between-family estimation. The constant, which can be interpreted as the mother's average share, is however close to the level in the cross-section sample.

Model (2) includes controls for whether the child is the family's first child and whether the child is female, and parents' ages when the child was born or adopted (included as fixed effects for each year between 25 and 45 years or older). When adding the control variables for child and parental characteristics, the estimate increases to a magnitude that is larger than the cross-section estimates. Being a first-time parent is associated with a somewhat more even division of leave, although this parameter in not significant.[12]

In the last model, model (3), controls are again added for the adopted child's age when adopted to investigate whether any influence of the fact that adopted children are usually older is captured in the θ-estimate. This does not change the estimate of θ much. The indicator variables for the adopted child's age are again larger, but for this sample negative and not significant. This means that when comparing the division of parental leave between children in the same family, the parents have an, on average, more even division for the adopted children if it was more than 6 months old when adopted.

The estimate in the preferred model for the within-family sample, model (2), of 0.936 is on par with the estimate in the preferred model for the cross-section sample, model (3), of 0.818. Both estimates are small: they indicate that having an adopted child instead of a biological child is associated with the mother's share of parental leave being nearly one percentage point larger, although the estimate in the within-family sample is insignificant. In conclusion, there doesn't seem to be any large difference in the mother's share of parental leave with an adopted and a biological child. If anything, the mother's share is somewhat larger for adopted children.

6.3 Estimates on each parent's take-up of leave days

To further investigate the relationship between biological parenthood and the parents' time spent at home with the child, model 15 and 11 are estimated with each parent's take-up of parental leave days as dependent variables.

Tables 5 and 6 present the results for the cross-section sample when the mother's and father's take-up of parental leave days (PLD) during the child's first two years in the family is used as the dependent variables. Both adoptive mothers and fathers spend less time on parental leave. The results presented in table 5 reveal that first-time adoptive mothers spend around 108.4 days (3.5 months) less on parental leave compared to first time biological mothers within the same household type. For fathers, the results in table 6 indicate that adoptive fathers take, on average, 24.4 days (0.8 months) less with parental leave benefits then biological fathers take. The estimates are relatively consistent across specifications. The difference between biological and adoptive parents is large. Further, including the indicator variables for the child's age at

[12]The only way that adding control variables can affect the within-family estimates is through variation in these variables within the same family between different children. Other control variables are not included since they either do not change between the birth or adoption of different children or because changes in the variables could be endogenous to the parents' division of parental leave with the first child. For this reason, the mother's share of household income is not added.

adoption reveals that among couples who adopt a child that is over 6 months old, the mother spends more time on leave and the father less. The increase in mothers' days is not completely offset by the decrease of fathers, which means that the couples total time on leave is longer for older adopted children.

Tables 7 and 8 present the results when performing the same exercise on the within-family sample. When comparing the take-up of parental leave days with adopted and biological children within the same family, the difference is larger for mothers, and about the same for fathers, compared to the between-family comparison. Mothers take 116.6 days less with benefits and fathers take 22.8 days less. The estimate for mothers becomes smaller in magnitude, and closer to the cross-section estimate, when controlling for child's age at adoption; 107.7 days. For fathers, the estimate becomes slightly larger when adding control variables; 24.2 days. Also among these parents, adopting an older child seems to induce the parents to spend a longer time on leave in total. The tables also reveal that fathers in general spend more time on leave with their first child, and mothers less, resulting in a slightly more even division of leave.

7 Concluding remarks

The aim of this paper is to investigate to what extent the fact that the mother is the one to be pregnant and give birth can explain the uneven division of family responsibilities between mothers and fathers. The division of parental leave is used as a proxy for division of time spent at home caring for the child. Hence the focus of the analysis is on understanding the mechanisms behind parents' division of parental leave.

The division of parental leave among biological parents (where the mother gave birth) is compared to that of parents who adopt a child (where the mother did not give birth) in order to study the importance of the mother's birth giving status. The association between having an adopted child and the mother's share of the couple's total take-up of parental leave is estimated, first, among first time parents of either an adopted or a biological child, and second, within families who have both at least one biological and one adopted child.

When comparing the mother's share between couples in the cross-section sample, couples are matched to each other based on the child's birth year or year of adoption, parents' ages, and type and level of education: variables that could determine the likelihood of adopting a child and the division of parental leave. The estimations are performed comparing couples who were matched to each other and thus the model controls for the influence of these variables. The results indicate that adoptive mother's share of the couples' total take-up of parental leave is about one percentage point larger than the share of biological mothers. Further, the estimates show that the adoptive parents (both

Table 3. *Mother's share of PL. Cross-section estimation.*

	(1) MomsShare	(2) MomsShare	(3) MomsShare	(4) MomsShare
Adopted	0.899**	0.708*	0.818**	0.786
	(0.393)	(0.398)	(0.398)	(0.535)
Female child		0.198	0.196	0.194
		(0.181)	(0.180)	(0.180)
Adop. age 0.5 - 1.5 years				4.317***
				(1.328)
Adop. age >= 1.5 years				3.247**
				(1.302)
Constant	80.71***	78.84***	73.51***	70.23***
	(0.0916)	(0.665)	(0.764)	(1.505)
Household controls	No	Yes	Yes	Yes
Mother's earnings share	No	No	Yes	Yes
N	45269	45269	45269	45269
r2	0.19	0.20	0.21	0.21

Table 4. *Mother's share of PL. Within family estimation.*

	(1) MomsShare	(2) MomsShare	(3) MomsShare
Adopted	0.390	0.934	0.964
	(1.099)	(1.195)	(1.538)
Female child		0.0788	0.0858
		(1.444)	(1.449)
First child		-2.530	-2.595
		(2.137)	(2.171)
Adop. age 0.5 - 1.5 years			-2.217
			(3.805)
Adop. age >= 1.5 years			-1.641
			(3.540)
Constant	82.53***	92.18***	93.84***
	(0.536)	(3.557)	(4.869)
Household controls	No	Yes	Yes
N	2251	2251	2251
r2	0.57	0.59	0.59
Number of Cluster	998	998	998

Table 5. *Mother's PLD. Cross-section estimation.*

	(1) Mom's PLD	(2) Mom's PLD	(3) Mom's PLD	(4) Mom's PLD
Adopted	-108.4***	-109.9***	-109.3***	-105.6***
	(1.776)	(1.792)	(1.792)	(2.231)
Female child		0.429	0.410	0.316
		(0.823)	(0.817)	(0.817)
Adop. age 0.5 - 1.5 years				23.64***
				(7.135)
Adop. age >= 1.5 years				27.73***
				(6.802)
Constant	283.1***	252.6***	226.9***	199.2***
	(0.418)	(3.113)	(3.458)	(7.610)
Household controls	No	Yes	Yes	Yes
Mother's earnings share	No	No	Yes	Yes
N	45353	45353	45353	45353
r2	0.28	0.29	0.30	0.30

Table 6. *Father's PLD. Cross-section estimation.*

	(1) Dad's PLD	(2) Dad's PLD	(3) Dad's PLD	(4) Dad's PLD
Adopted	-24.37***	-23.97***	-24.12***	-26.31***
	(0.913)	(0.926)	(0.927)	(1.187)
Female child		-0.919*	-0.914	-0.861
		(0.558)	(0.557)	(0.558)
Adop. age 0.5 - 1.5 years				-8.862***
				(3.241)
Adop. age >= 1.5 years				-12.38***
				(3.105)
Constant	63.65***	59.90***	67.11***	79.46***
	(0.286)	(1.817)	(2.069)	(3.695)
Household controls	No	Yes	Yes	Yes
Mother's earnings share	No	No	Yes	Yes
N	45353	45353	45353	45353
r2	0.19	0.20	0.20	0.20

Table 7. *Mother's PLD. Within family estimation.*

	(1) Mom's PLD	(2) Mom's PLD	(3) Mom's PLD
Adopted	-116.6***	-114.2***	-107.6***
	(5.091)	(5.391)	(6.689)
Female child		4.454	3.840
		(6.715)	(6.739)
First child		-14.09	-11.26
		(9.444)	(9.544)
Adop. age 0.5 - 1.5 years			-0.430
			(20.11)
Adop. age >= 1.5 years			17.79
			(19.36)
Constant	286.4***	330.0***	312.2***
	(2.483)	(17.04)	(25.77)
Household controls	No	Yes	Yes
N	2251	2251	2251
r2	0.67	0.69	0.69
Number of Cluster	998	998	998

Table 8. *Father's PLD. Within family estimation.*

	(1) Dad's PLD	(2) Dad's PLD	(3) Dad's PLD
Adopted	-22.79***	-24.38***	-24.18***
	(3.084)	(3.280)	(4.155)
Female child		1.692	1.695
		(3.642)	(3.658)
First child		8.999*	8.912
		(5.451)	(5.515)
Adop. age 0.5 - 1.5 years			-4.886
			(9.418)
Adop. age >= 1.5 years			-3.270
			(8.926)
Constant	56.84***	20.14**	23.45*
	(1.504)	(9.243)	(12.36)
Household controls	No	Yes	Yes
N	2251	2251	2251
r2	0.60	0.63	0.63
Number of Cluster	998	998	998

mothers and fathers) spend less time on leave than biological parents. First-time adoptive mothers take about 108 parental leave days less than biological mothers, and adoptive fathers use about 24 days less.

A methodological drawback of comparing take-up of parental leave between families is that couples select into adopting a child and it is not possible to control for unobserved differences in characteristics between couples when matching couples on observable characteristics. When estimating the difference in the mother's share of parental leave between adopted and biological children within the same family, all family specific characteristics that are time-invariant between different children are held constant. Turning to the result for the within-family estimations, these are close in magnitude to the results for the cross-section sample. The mother's share of the parental leave is about one percentage point larger when the couple adopts a child than when they have a biological child, though the estimate is not statistically significant. The difference in the number of parental leave days taken by mothers and fathers with an adopted child compared to a biological child is also close to that in the cross-section sample: 116 days less for the mother and 23 days less for the father.

Thus, to summarize the results, both mothers and fathers spend less time on parental leave when adopting a child, but they divide the parental leave in about the same way as biological parents. There is no evidence that adoptive parents would choose a more gender equal division of family responsibilities.

There are theoretical, as well as biological reasons to expect adoptive parents to have a more even division of parental leave, and thus time spent taking care of the child. First, adoptive parents make more similar investments in the child before adoption (both financial and in term of time) than biological parents where the mother must make a larger biological investment during pregnancy. Previous studies have shown that both adoptive parents are usually highly motivated to parent. Second, among biological parents, studies have shown that because the mother is the one to be pregnant, she finds it easier to form an emotional bond to the child already before it is born, something that the father can find it hard to do. There is also a difference in hormonal influence and build up during the time of pregnancy between the biological parents. These differences are not present in the same way among adoptive parents. Third, breastfeeding as a motivation for letting the mother be the one to stay at home with the child is not usually a consideration among adoptive parents. Forth, the expectation from society, or social norm, that a woman who has given birth to a child should be the one to take care of it at least during the first period after birth does not affect adoptive parents in the same way. Since adoptive children are typically not newborns but on average 1,5 years old when adopted, this probably decreases the stigma for adoptive parents to let the fathers take a larger share of the parental leave.

My results, however, do not indicate any difference in the division of parental responsibilities between biological and adoptive children. The results when controlling for adopted children's age when adopted point in somewhat different directions for the two different samples, but indicate that mothers may spend more time on leave when adopting an older child, and fathers less. The results do not indicate that couples who adopt a very young child have a more equal division of parental leave than biological parents.

One possible interpretation is that the fact that the mother is the one to give birth is not an important factor for the parents' division of parental responsibilities. Alternatively, mother's birth giving status is an important factor, but due to methodological challenges it is not possible to identify this effect. The results do show that adoptive mothers and fathers both spend significantly less time on parental leave. This suggests that a biological tie between parent and child does matter and can induce the parents to make larger time investments in the child, but not that this effect is stronger for mothers than for fathers.

If it is the case that mother's birth giving status affects the division among biological parents, then this could establish a social norm that also affects adoptive parents. For example, it could be that adoptive parents want to mimic the behavior of biological parents as a way to gain acceptance as a legitimate family constellation. In that case, the mother's birth giving status is an important factor in forming social norms that also affect adoptive parents indirectly. Thus, even if adoptive parents' decisions are guided by social norms, these norms could still have their roots in biological gender differences among heterosexual biological parents.

It could also be that the mother's birth giving status does affect the parents' division of leave, but that I am not able to identify this effect with the strategies applied in this paper. Despite attempts made to compare the division of parental leave between as comparable couples as possible, through matching and between children within the same family, there are still differences between adopting and having a biological child that may affect the parents' decision. For example, adoptive children are usually older and more often have special needs.

This paper adds to the literature as the first to estimate the difference in parents' division of parental responsibility, in terms of time on parental leave, between biological and adoptive parents using nation-wide panel data that covers many consecutive years. Compared to results found in previous studies, this paper can confirm the finding that heterosexual adoptive parents, just like biological, have in general a traditional division of parental responsibilities of their children, where the mother takes a larger share. Contrary to earlier findings however, my results do not indicate that adoptive parents would be less influenced by gender norms than biological parents.

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Appendix

Table A1. *Descriptive statistics on breastfeeding and parental leave.*

Breastfeeding		Sweden	USA	Canada	Ireland	Taiwan
At 1 week	total	95,5%	80,3%	88,5%	55,7%	83,7%
	exclusively	79,1%	62,6%			
At 1 month	total		76,6%	77,0%		67,9%
	exclusively		56,5%			
At 2 months	total	84,9%	71,5%			
	exclusively	63,9%	50,1%			
At 3 months	total		66,7%	70,0%		39,4%
	exclusively		44,4%	59,5%		
At 4 months	total	74,2%	59,3%	62,6%		
	exclusively	51,8%	35,7%	49,0%		
At 6 months	total	63,0%	51,8%	55,0%		25,4%
	exclusively	15,4%	22,3%	25,8%		
At 12 months	total	19,1%	30,7%	17,5%		12,7%
	exclusively	0,0%				
Parental leave (weeks)						
Maternity leave paid		12		15	26	8
Maternity leave unpaid			12		16	
Paternity leave paid		12				1
Paternity leave unpaid			12			
Parental leave paid		40		35		
Parental leave unpaid					36	104
Employment rate						
Mothers with young children		78,0%	61,8%	69,5%	59,5%	59,0%

Note: Descriptive statistics on breastfeeding, parental leave and mothers' employment. The employment level indicates the level of mothers with children below 7 years old in Sweden, below 4 in Ireland, and below 3 years old in USA, Canada, and Taiwan.

Table A1 describes the rates of breastfeeding, legislated length of parental leave and employment levels among mothers with young children in Sweden, the USA, Canada, Ireland and Taiwan: five countries with different cultures, labor markets and welfare systems. Most mothers initially breastfeed, but rates drop continuously in the first few months and few mothers follow the WHO recommendation.[13] The recommendation to breastfeed exclusively for

[13]There can be several reasons why women stop breastfeeding (or never start). A survey on more than 20,000 mothers in Taiwan, Chuang et al. (2010) show that the main reasons for

six months to some extent creates a conflict in policy goals in policy makers also want to promote gender equality since following the recommendation could make it difficult for the father to get involved in caring for the child if no bottle feeding is to occur. However, the cross-country comparison shows no obvious correlation between the prevalence of breastfeeding and the length of parental leave, or between breastfeeding and the employment level of mothers with young children (below 7 years old in Sweden, below 4 in Ireland, and below 3 years old in the USA, Canada, and Taiwan). In fact, many mothers breastfeed for a shorter time than the total length of parental leave would permit. At the same time many mothers in the US seem to breastfeed for a longer time than the total length of parental leave guarantee, many of them presumably leaving the work force to do so. Although there is thus some empirical evidence that prolonged parental leave also increases the time of breastfeeding, it seems there is no strong deterministic relationship between the length of leave and the number of months breastfeeding.[14] Although the need for biological mothers to stay on maternity leave in order to keep breastfeeding constitutes a difference in circumstances between biological and adoptive parents, it is probably not what determines any differences in behavior between the two groups.

not initiating breastfeeding directly after birth was insufficient milk (57% of cases) and that the baby refused to suckle (13 %). Only 13 % of mothers stated that they did not start to breastfeed because they needed to return to work quickly. The most common reason for weaning the child was insufficient milk production (43 %) followed by an inability to combine breastfeeding with returning to work (20.5 %).

[14]Baker and Milligan (2008) study the relationship between prolonged parental leave and the prevalence of breastfeeding in Canada. They find that the reform that increased the parental leave from six months to a year, increased the average number of months that the child was breastfed from 5.34 to 6.8 months.

IV. The Anatomy of the Extensive Margin Labor Supply Response

Co-authored with Spencer Bastani and Håkan Selin.

No previous quasi-experimental paper has systematically examined the relationship between the extensive margin labor supply response to taxation and the employment level. We model the labor force participation margin and estimate participation responses for married women in Sweden using population-wide administrative data and a solid identification strategy. The participation elasticity is more than twice as large in the lowest-skill sample (with relatively low employment) as compared with the highest-skill sample (with high employment). Our analysis suggests that cross- and within country comparisons of participation elasticities always should be made with reference to the relevant employment level.

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1 Introduction

In recent decades there has been a large expansion of in-work tax credit programs. Examples are the Earned Income Tax Credit (EITC) in the United States and the Working Tax Credit (WTC) in the United Kingdom. The primary goal of these programs is to support low income families and encourage labor force participation. The consensus view in the literature is that these policies increased labor supply at the extensive margin for single mothers (Eissa and Liebman 1996, Meyer and Rosenbaum 2001) but at the same time discouraged work for a large number of secondary earners in couples (Eissa and Hoynes 2004, Francesconi et al. 2009). The reason is that the tax credits are phased out as a function of family income rather than individual income. This implies that if the primary earner's income is sufficiently large, the family will experience a reduction in the tax credit if the secondary earner choses to work, thereby lowering the incentives for the secondary earner to enter the labor force.[1] To assess the optimality of the tax system, a key issue is therefore to understand the sensitivity of the secondary earners' participation decision to work incentives. This can be achieved by quantifying the *participation elasticity of secondary earners*, i.e. the percentage change in secondary earner labor force participation in response to a percentage change in the financial reward of working. This elasticity determines the efficiency gains from reducing participation tax rates applying to secondary earners (Immervoll et al. 2011).

Despite their central importance, there are very few quasi-experimental estimates of participation elasticities. This is evident from the meta-analysis by Chetty (2012).[2] Moreover, as far as we know, there is currently no quasi-experimental evidence on how these elasticities differ across different skill groups of the population. Understanding such heterogeneity is important because the labor supply response to taxation along the extensive margin depends on the distribution of fixed costs/reservation wages or, equivalently, as we show in the paper, the employment level at the economy's current equilibrium. It is therefore problematic to infer, for example, the extensive margin labor responsiveness for economies with high employment on the basis of estimates obtained for economies where the employment level is much lower.

In this paper we systematically estimate participation elasticities of secondary earners by exploiting high-quality administrative data on the full pop-

[1] According to Kearney and Turner (2013), under the current U.S. federal tax and transfer system, a family with standard child care costs and a primary earner making $25,000 a year will take home less than 30 percent of a spouse's earnings.

[2] The enormous literature on in-work tax credit policies focuses on singles. Eissa and Hoynes 2004, Francesconi et al. (2009),Bosch and van der Klaauw (2012) and Ellwood (2000) are notable exceptions. To our knowledge, the only previous studies explicitly reporting the secondary earner's participation elasticity are Selin (2014) and Kosonen (2014). Related papers using quasi-experimental methods to estimate the effect of childcare prices on female labor supply are Lundin et al. (2008) for Sweden and Havnes and Mogstad (2011) for Norway. None of them found an effect of child-care prices.

ulation of Swedish taxpayers. We make two primary contributions. First, we present a transparent estimate of 0.13 of the average participation elasticity in a population of women where the average labor force participation already is high. Second, we partition the sample and systematically investigate the participation responses for different subgroups of individuals with different baseline employment rates. We divide the sample into four quartiles based on the wife's skill (predicted income) and, interestingly, find elasticities that are monotonically falling in the skill level of the wife (ranging from 0.24 to 0.09).

For identification we use a reform in the Swedish system for housing allowances for couples with children in 1997. Before 1997 the housing allowance was means-tested based on family income - a family received maximal housing allowance if the joint income of the household did not exceed SEK 117,000 (appr. USD 15,000). After the reform the system was individualized so that the housing allowance was phased out if the individual labor income of either spouse in the household exceeded SEK 58,500. Both before and after 1997 the phase-out rate was 20%. The reform substantially lowered participation tax rates of secondary earners married to low- and middle income spouses, mainly by making not working less attractive.[3] We carefully calculate the participation tax rates, which reflect the financial gain from working, in the treatment- and control groups before and after the reform.

Following earlier work on secondary earners' labor supply on survey data (e.g. Eissa and Hoynes 2004, Francesconi et al. 2009) we compare eligible households (with children) with ineligible households (without children) before and after the 1997 reform. Since we have access to several pre-reform years of data we can examine the parallel trends assumption. We focus on wives married to husbands with an income below the median and document that female employment increases in households with children relative to households without children in the post-reform period.

A caveat of the transfer program that we analyze is that it is subject to voluntary take-up. A final contribution of the paper is that we set up a simple model where the household decides not only about the secondary earner's working status, but also about transfer program take-up and show how the elasticity estimated using variation in the transfer system relates to the concept of participation elasticity in the public finance literature.

The paper is organized as follows. In the next section we describe the 1997 reform in the Swedish housing allowance system. In section 3 we describe our data sources, section 4 develops a model for interpreting the evidence and section 5 presents the empirical strategy. A graphical analysis is provided in

[3]From a different angle the same reform has earlier been analyzed by Enström Öst (2012). Using data from the Swedish Social Insurance Agency she compares earnings growth in households with different income compositions in 1996. She estimates significant earnings responses for women. In an experimental study on U.S. data Jacob and Ludwig (2012) estimated a negative effect of housing assistance on labor supply.

section 6, whereas the regression results and implied elasticities are reported in section 7. Finally, section 8 offers concluding remarks.

2 The reform

We begin by describing the reform in 1997 that we exploit to identify extensive margin labor supply responses.

2.1 General description of the transfer program

The housing allowance system can be characterized as an *out-of-work program* as there is no work-requirement for eligibility and the associated transfer is reduced as a function of the income of the members of the household (means-testing). The program is administered by the Social Insurance Agency ("Försäkringskassan") and payments are given on a monthly basis. To receive the transfer (which is a cash transfer), the household has to apply for it by the end of each year. In 1996, 180,000 Swedish couples received housing allowance and the transfer made up an important budget share of many low income households. The particular program that we analyze in this paper applies to low income families with children.[4] We will motivate our choice of control group in section 5.1.

2.2 Incentive effects

To ease the description of the incentive effects of the housing allowance we introduce some notation. The housing allowance can be written as a function $B(\tilde{z}^p, \tilde{z})$ where $\tilde{z}^p$ and $\tilde{z}$ are, respectively, the two spouses' *qualifying income* or "bidragsgrundande inkomst", which is the income concept used to assess eligibility for welfare programs in Sweden.[5] Without loss of generality we assume $\tilde{z}^p > \tilde{z}$ making one spouse the "primary earner" and the other spouse the "secondary earner". The function B is weakly decreasing in both its arguments which reflects that the housing allowance is a means-tested program. The maximal level of the housing allowance is obtained when neither spouse has any qualifying income and is equal to $B(0,0)$ which we denote B^{00}. The value of B^{00} depends on a number of non-income characteristics such as the number of children in the household, housing costs and the living space (sq.m.) of the household.[6]

[4] There is also a separate and different housing allowance system applying to young families without children that was not subject to reform and that we do not analyze in this paper.

[5] Qualifying income does not only include earnings, but also capital income and a fraction of wealth.

[6] In appendix A we describe in more detail how the value of B^{00} is determined.

Before the reform in 1997 the transfer was reduced as a function of *the sum* of the two spouses qualifying incomes, i.e. the housing allowance pre-reform could be written $B(\tilde{z}^p, \tilde{z}) = B^{pre}(\tilde{z}^p + \tilde{z})$ and took the following form:

$$B^{pre}(\tilde{z}^p + \tilde{z}) = \begin{cases} B^{00} & \text{if } \tilde{z}^p + \tilde{z} \leq 117,000 \\ \max\left\{B^{00} - h^{pre}(\tilde{z}^p + \tilde{z}), 0\right\} & \text{if } \tilde{z}^p + \tilde{z} > 117,000. \end{cases}$$

where $h^{pre}(x) = 0.2 \times (x - 117,000)$. Thus, a family received the maximum transfer if the joint income of the household did not exceed SEK 117,000 SEK. If the joint income exceeded this exemption level, the transfer was reduced at a phase-out rate of 20 percent. Hence, if say, family income was 118,000 SEK, the transfer was reduced by 200 SEK $[= 0.2 \times (118,000 - 117,000)]$.

After the 1997 reform, the system was individualized so that the household received the maximum transfer only if the income of *neither* spouse exceeded SEK 58,500. The phase-out rate was kept at 20 %.[7] Thus the post-1997 housing allowance can be written as $B(\tilde{z}^p, \tilde{z}) = B^{post}(\tilde{z}^p, \tilde{z})$ defined as:

$$B^{post}(\tilde{z}^p, \tilde{z}) =$$
$$= \begin{cases} B^{00} & \text{if } \tilde{z}^p \leq 58,500 \quad \text{and} \quad \tilde{z} \leq 58,500 \\ \max\left\{B^{00} - h^{post}(\tilde{z}_p), 0\right\} & \text{if } \tilde{z}^p > 58,500 \quad \text{and} \quad \tilde{z} \leq 58,500. \\ \max\left\{B^{00} - h^{post}(\tilde{z}_p) - h^{post}(\tilde{z}), 0\right\} & \text{if } \tilde{z}^p > 58,500 \quad \text{and} \quad \tilde{z} > 58,500. \end{cases}$$

where $h^{post}(x) = 0.2 \times (x - 58,500)$.

How did the 1997 reform affect work incentives? To answer this question we need to make an assumption about how economic decisions within the family are organized. Even though there is individual taxation in Sweden, the transfer system depends on the income of both spouses, hence the total tax/transfer relevant for the labor force participation decision of one member of the family depends on the economic decision of his/her spouse. We analyze the incentive changes from the point of view of a sequential model, where the secondary earner decides whether to work or not conditional on the labor supply choice of the primary earner. For the moment we abstract from the take-up issue, and simply assume that the household always takes up the transfer when eligible.

In figure 1 we have illustrated the pre- and post-reform transfers $B^{pre}(\tilde{z}^p + \tilde{z})$ and $B^{post}(\tilde{z}^p, \tilde{z})$ for a family with two children as a function of the secondary earner's income $\tilde{z}$ while fixing $\tilde{z}^p$ to $170,000$ (a typical value of the primary earner's qualifying income in our estimation sample). We assume that if neither spouse would work, the household would be entitled to the maximum

[7]The reform implied no change to the income thresholds, the level of the housing allowance or the phase-out rates for single parents. Therefore, singles with children could *a priori* be considered to serve as a control group to married with children in the empirical analysis. However, owing to differential employment trends and levels we have not chosen this strategy.

level of housing allowance for households with two children, $B^{00} = 38,100$. Given these assumptions, in the pre-reform scenario, the household is eligible for a transfer amounting to $38,100 - 0.2 \times (170,000 - 117,000) = 27,500$ when the secondary earner has zero earnings. According to the pre-reform rules, as soon as the secondary earners supplies any amount of positive earnings, the housing allowance is reduced. More specifically, it is reduced by 0.2 SEK for every SEK of secondary earnings up until the point where the total amount of 27,500 SEK is phased out (which happens at 137,500 SEK) . In the post-reform scenario, on the other hand, the transfer at zero earnings of the secondary earner is significantly smaller: $38,100 - 0.2 \times (170,000 - 58,500) = 15,800$ but the phase-out does not kick in until the secondary earner exceeds the income level of $58,500$. At this point the pre- and post-reform transfers are equal and the functions B^{pre} and B^{post} coincide for secondary earnings exceeding 58,500.

The important lesson from figure 1 is that if the potential earnings of the secondary earner is SEK 58,500 or more, the difference between the household's disposable income in the state of work and non-work, respectively, will entirely be driven by the difference in the transfer in the state of non-work. Since most married women earn annual incomes above SEK 58,500 when working we therefore conclude that the variation used to recover participation elasticities in this paper is a variation in the housing allowance at zero earnings of the secondary earner. In summary, the reform makes not working much less attractive for the secondary earner. Accordingly, even though households may not be perfectly aware of the income splitting rules, one-earner households will certainly recognize that the size of the transfer will be reduced after the reform.

2.3 Time line and anticipation issues

The main objective of the 1997 reform was to cut government expenditures related to the housing allowance program. The size of the program more than doubled between 1990 and 1995 (Boverket 2006). In April 1995, when the annual expenditures were projected to amount to more than SEK 9 billion, the Social Democratic government appointed a government committee (Kommittédirektiv 1995:65). The mandate of the committee was straightforward: The committee was supposed to propose expenditure reductions, e.g. by changing the rules for means-testing. The committee issued their report in December, 1995. The committee's proposal was similar to the reform that was to be implented on January 1, 1997. The Social Democratic government presented a government bill in March 1996 and the bill was passed in parliament on May 8, 1996.[8]

[8]The Social Democratic party was in minority in the parliament, but was supported by the Centre (agrarian) party ("Centerpartiet").

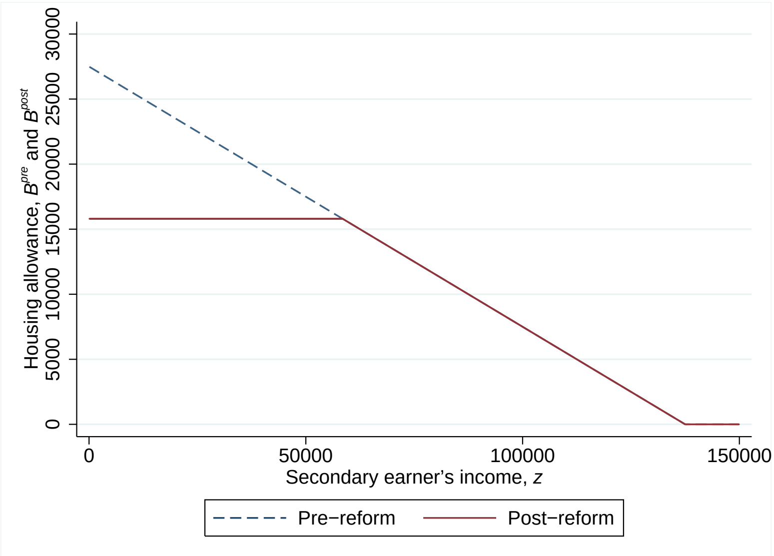

Figure 1. Housing allowance before and after the reform according to the functions $B^{pre}(\tilde{z}^p + \tilde{z})$ and $B^{post}(\tilde{z}^p, \tilde{z})$ as a function of secondary income $\tilde{z}$ for a family with two children. The primary earner's income is fixed at $\tilde{z}^p = 170,000$.

Did households anticipate the 1997 reform? This is a key issue when interpreting the estimated elasticities (Blundell et al. 2011). In principle, well-informed households could have adjusted their behavior already in December 1995 when the committee's report became publicly known.[9]. However, we think that large-scale pre-reform anticipatory responses are unlikely. As far as we can tell, there was no public discussion about the income limits when the committee's report was presented. [10] According to Enström Öst (2012) the Social Insurance Agency ("Försäkringskassan") informed beneficiaries about the reform by sending out letters in June and October 1996. Accordingly, it is likely that the vast majority became aware of the new earnings limits close to the implementation of the reform on January 1, 1997.

[9] As discussed by Blundell et al. it is not *a priori* clear in which direction such anticipatory responses would go. If intertemporal substitution is the dominating mechanism, we would observe people working less in anticipation of the reform. If, on the other hand, labor market frictions is the key mechanism we would expect people to start searching for new jobs already in the pre-reform period.

[10] A search on "bostadsbidrag" in the media archive "Newsline" suggests that the main media focus was on actions against fraud in the system for housing allowances, rather than work incentives when the committee presented their report. The media coverage was larger when the reform was legislated on May 8, 1996, but the focus was not on the earnings limits.

3 Data

3.1 Administrative data

This study primarily exploits large population-wide administrative data sets provided by Statistics Sweden. We have access to all key variables from 1991 and onwards. These include earned income (which we define as the sum of wage income and self-employment income), education level, geographical indicators, the number of children in the household and region of origin. Our graphical analysis of section 6 will cover the years 1991-2010 whereas, as we motivate in section 5.1 below, we focus on the years 1994-2001 in the regression analysis.

Since the variables that we use are collected from administrative registers, the overall quality is very good. A caveat is that the data quality on variables for non-natives might be slightly lower in some cases. In particular, in the 1990's data on education level for many non-natives (who obtained their education degrees from other countries) was missing. We have been able to correct the missing values by using leads of the education variable. The Swedish authorities later on actively sent questionnaires to immigrants where they were asked to report their education level.[11]

In the Swedish register data non-married cohabiting couples without common children are observed as singles in the administrative data. Therefore, even though the housing allowance system applies both to married and cohabiting couples, we limit the sample to formally married couples. We simply do not observe cohabiting couples *without* children.

3.2 Supplementary survey data and micro-simulation model

The housing allowance interacts with other parts of the transfer system, most notably social assistance. Therefore, it is important to take into account the entire tax-and transfer system when constructing households' budget sets. To achieve this, we use the microsimulation model FASIT developed by the Swedish Ministry of Finance and Statistics Sweden.

As FASIT relies on a larger set of variables than is available in our population data, we use as input to FASIT, the smaller supplementary data set HEK ('Hushållens ekonomi') that is based on both surveys and administrative registers. After having imposed the same sample restrictions on HEK as on the administrative data, the size of the HEK sample varies between 1000 and 2000 observations across years. Since HEK both includes the full set of variables that determine eligibility for the housing allowance program and the size of the beneift actually received (from registers), we also use HEK to compute the take-up of the housing allowance.

[11] Unless the individual died or migrated between year t and year 2000 we use education information as of 2000 when constructing the variable for education level.

3.3 Participation tax rates

Let us now formally define participation tax rates (PTR) and describe in more detail how they are computed. We let $T^{total}(z^p, z)$ refer to all taxes paid and benefits received by a household with primary earnings z^p and earnings of the secondary earner equal to z, assuming the household takes up all transfers.[12] The PTR for the secondary earner is defined in the following way:

$$\tau(z^p, z) = \frac{T^{total}(z^p, z) - T^{total}(z^p, 0)}{z}. \quad (1)$$

This is the key independent variable that appears in our estimation equations (11) and (12) below. Importantly, we compute PTR:s for all households assuming that households eligible for housing allowance and social assistance take up the transfers. As mentioned already, when calculating PTR:s we leverage on the micro-simulation model FASIT and the HEK data set which are tailor-made to measure the impact of taxes and transfers on households' disposable incomes.

The PTR concept implies that the household chooses between two hypothetical disposable incomes; the disposable incomes when the secondary earner is working and non-working, respectively. To be able to estimate the impact of PTR:s on employment we need to compute PTR:s for all individuals, both labor force participants (with positive earnings) and labor force non-participants (with zero earnings) in our population-wide register data. Two issues arise. First, earnings in the state work are observed for those who are working only. Second, some of the variables needed to compute PTR:s (e.g. housing costs and dwelling space) are present in HEK, but not in the population wide data. Hence, we need to impute PTR:s.

We proceed in the following way. We start by calculating the PTR:s for all secondary earners with *positive* earnings in the HEK data. This is achieved by computing the disposable income for each household while setting the secondary earner's earnings to zero in the HEK data. We then subtract the household's disposable income at zero earnings from the household's actual disposable income (in the state of work) to obtain the household's financial gain from secondary earner employment. Finally, we divide the financial gain by the secondary earner's earnings to obtain the PTR according to equation (1).[13]

Next, pooling the HEK data for the years 1994-2001, we regress PTR:s on four dummies based on the actual qualifying income of the husband (year-specific quartiles), four dummies based on the number of children in the house-

[12] The function T^{total} corresponds to $T + B$ below in section 4.

[13] We acknowledge that earnings in the state of work may differ for employed and unemployed women, even conditional on observable characteristics, which may induce a selection bias. We have however not been able to find any valid instruments that enable us to use a selection correction term. In this respect, our approach bares some similarities with Gelber and Mitchell (2011) and Meyer and Rosenbaum (2001).

hold and eight year dummies as well as the full set of interactions between the income, children and year dummies. The estimated coefficients from these regressions are then used to impute PTR:s for *all* secondary earners in the *population wide register data*, both participants (with positive earnings) and non-participants (with zero earnings). Since the imputation model is fully interacted, the predictions can be interpreted as group means for women who are working.

While the HEK sample is too small to be used in the labor supply analysis described in section 5, it is still very useful for the purpose of estimating PTR:s. Remember that the households' budget sets are given *deterministically* by the micro-simulation model and the variables in the HEK data. Of course, this does not mean that the sample size of HEK is unimportant, because the precision of the estimated group means become more precise the larger is the number of households represented in the HEK sample.

As already mentioned, the FASIT model is very detailed and should, in principle, be able to account for the entire tax- and transfer system. Since the main purpose of FASIT has been to assess revenue effects of changes in the tax- and transfer system we had to rewrite the code carefully so that it served our purposes. Most importantly, there were no modules computing social assistance benefits for the years 1994-1995. Hence, for these years, we wrote the code ourselves based on national guidelines for social assistance.[14]

4 A model to interpret the evidence

4.1 The model

To support the interpretation of our empirical evidence we sketch a simple model that will allow us to (i) clarify conditions under which there is a very simple relationship between elasticities describing the responsiveness to transfers with imperfect take-up and elasticities with respect to changes in taxes (which by assumption have perfect take-up) (see section 4.2 below) and (ii) highlight how estimated participation elasticities depend on the skill-specific employment level (see section 4.3 below).

We consider a model with a discrete set of household types $\mathscr{H}$ indexed by $h \in \mathscr{H}$. There are π_h number of households of each household type. Each household consists of two agents with earnings capacaties z_h^p and z_h, where $z_h^p > z_h$, making one household member the "primary earner" and the other household member the "secondary earner". In a given household type all households are identical with respect to their potential earnings z_h^p and z_h. We

[14]Rules for social assistance differ across municipalities. For some, but not all, years we can compute social assistance both as a function of municipality-specific parameters and national guidelines. For coherency, we have chosen to use national guidelines for all years. We have verified that the two methods produce similar results for the years that both methods are available to us.

focus on the optimal decision-making of the secondary earner from the perspective of the household, treating the primary earner as a passive agent with fixed income z_h^p. Thus, in line with earlier literature (see e.g. Eissa 1995; Eissa and Hoynes 2004) we treat the primary earner as exogenous.[15]

The household decides whether the secondary earner should enter the labor force or not and whether the household should take up the transfer or not. There is no intensive margin hours choice in the theoretical model. As the reform changed marginal work incentives at very low earnings levels of the secondary earner we *a priori* consider the extensive margin to be the important one.[16] Within a given household type households differ along two dimensions, 'fixed costs of working', q_h, and 'take-up costs', χ_h. Each household i of household type h makes a draw from the joint distribution of q_h and χ_h with the associated bi-variate probability density function $f_h(q_h, \chi_h)$. In the tradition of Cogan (1981) and Hausman (1980) the fixed cost of working, q_h, can be interpreted broadly to accomodate the utility costs (stemming from foregone leisure or the psychological costs associated with leaving a child under the supervision of a non-parent) or monetary costs (such as commuting or child care costs) associated with secondary earner labor market entry. The take-up cost, χ_h, can be interpreted as a cost from gathering information about the transfer program, a time-cost associated with filling out the paperwork, a complexity cost (understanding, and gathering the correct information about how to fill out the paperwork) or simply the social stigma associated with accepting transfers from the government.[17]

The two binary decisions at the household level implies that each household selects between four different states: (i) working without transfers, (ii) working with transfers, (iii) not-working and not taking up transfers, and, finally, (iv) not working and taking up transfers. We denote the decision of the household by $(M, L) \in \{0,1\} \times \{0,1\}$ where M is the take-up decision and L is the labor force participation decision of the secondary earner. Let c_{ih} de-

[15] Several remarks are in order. The model of household behavior is closely related to Immervoll et al. (2011) and their case without income effects on labor supply. In line with these authors, we assume Pareto efficiency and a sharing rule (dictating how resources are divided in the family) that is unaffected by taxes. In contrast to these authors, to simplify the interpretation of our empirical results, we assume the extensive margin of the primary to be inelastic. This does not seem unreasonable ex-ante given the high participation rate of primary earners in Sweden. Moreover, the non-responsiveness of primary earners along the extensive margin is supported by our empirical results in table D in the appendix. The omission of income effects is not without loss of generality, but simplifies the analysis considerable and has become a standard practice in the literature (see Brewer et al. 2010))

[16] We have also conducted a reduced form analysis which strongly points in this direction, see section 7.1 and table A1.

[17] Using a large-scale policy experiment, conducted in collaboration with the Internal Revenue Service (IRS) in the US, Bhargava and Manoli (forthcoming) find that incomplete take-up among low-income earners can at least partially be attributed to lack of program awareness and understanding combined with an aversion to program complexity.

note household consumption of household i in household type h. The utility function for each household is:

$$u_{ih}(c_{ih}, M_{ih}, L_{ih}) = c_{ih} - q_{ih}L_{ih} - \chi_{ih}M_{ih}, \tag{2}$$

and the budget constraint of the household is given by:

$$c_{ih} \leq z_h^p + z_h L_{ih} - T(z_h^p, z_h L_{ih}) + B(z_h^p, z_h L_{ih}) M_{ih} \tag{3}$$

where $T(z_h^p, z_h L_{ih})$ is the total tax liability (possibly negative) and $B(z_h^p, z_h L_{ih})$ is a non-negative transfer received from the government. It is a standard practice in the public finance literature to treat the nonlinear income tax T as representing the complete tax system (including transfers). In this paper we follow this approach with the exception that we leave out the *particular components* of the transfer system that are associated with costly take-up and designate these to the B-function.

Each household of type h chooses, based on its realized characteristics $(q_{ih}, \chi_{ih}) \in \mathbb{R}_+^2$, one out of the four different alternative states to maximize their utility (2) subject to the budget constraint (3). The mass of individuals choosing each state (M, L) correspond to different regions in the (q, χ)-space. We denote the share of households of household type h in each state with e_h^{ML}, $M = 0,1; L = 0,1$. Employment in household h is defined as $e_h = e_h^{11} + e_h^{01}$.

4.2 Participation elasticites with imperfect take-up

We now introduce the following simplified notation based on the T and B functions introduced in the budget constraint (3): $T_h^1 = T_h(z_h^p, z_h)$, $T_h^0 = T(z_h^p, 0)$, $T_h = T_h^1 - T_h^0$ and $B_h^1 = B_h(z_h^p, z_h)$, $B_h^0 = B_h(z_h^p, 0)$. We assume $B_h^0 > B_h^1$ and $T_h^1 > T_h^0$, which is the relevant case that applies when transfers are means-tested and participation taxes are less than 100%. In terms of the variables above, the participation tax introduced in (1) can be decomposed as:

$$\tau_h = \frac{T_h(z_h^p, z_h) - T(z_h^p, 0) + \left[B_h(z_h^p, 0) - B_h(z_h^p, z_h)\right]}{z_h} = \frac{T_h + B_h^0 - B_h^1}{z_h}. \tag{4}$$

This is the relevant participation tax rate for an individual who takes up both the work-related transfer and the non-work transfer and allows us to distinguish, for theoretical purposes, between three possible sources of variation in the incentives to participate in the labor force. These are, (i) a variation in T_h (the difference in taxes between the work and non-work state), (ii) a variation in transfer in the state of non-employment B^0, and, (iii) a variation in the transfer in the state of employment B^1.[18]

[18] The difference between T^{total} entering equation (1) and T entering (4) is that T excludes those components of the transfer system that are associated with costly take-up which we instead capture with the B-function. In our empirical analysis the variation in T^{total} stems mainly from variation in B^0.

We define $\varepsilon_h = -\frac{de_h}{dT_h}\frac{z_h - T_h - B_h^0 + B_h^1}{e_h}$ as the *participation elasticity* which yields the percentage increase in employment following a one percent increase in the financial reward from working $z_h - T_h - B_h^0 + B_h^1$ due to a change in T_h. Moreover, we define $\varepsilon_h^{B^0} = -\frac{de_h}{dB_h^0}\frac{z_h - T_h - B_h^0 + B_h^1}{e_h}$ and $\varepsilon_h^{B^1} = \frac{de_h}{dB_h^1}\frac{z_h - T_h - B_h^0 + B_h^1}{e_h}$ as the *transfer elasticities*, i.e. the elasticities obtained when using variation in the transfer system (which are subject to take-up costs).[19] We can then derive the following proposition which is very useful:

Proposition 1. *Suppose that at the household-type level, namely, for each $h \in \mathscr{H}$, (i) the random variables q_h and χ_h are independent, and, (ii) q_h is locally uniform on the open interval $(z_h - T_h - B_h^0, z - T_h) \subset \mathbb{R}^+$ and unrestricted elsewhere. Then, letting G_h denote the CDF of χ_h,*

$$\varepsilon_h = \frac{\varepsilon_h^{B^0}}{G_h(B_h^0)} = -\frac{\varepsilon_h^{B^1}}{G_h(B_h^1)},$$

where $G_h(B_h)$ is the take-up rate in household type h when the level of transfers is B_h, or, equivalently, the fraction of type-h workers with take-up costs less than B_h.

Proof. See appendix C. □

The above proposition specifies sufficient conditions under which reforms in transfers (that are subject to take-up decisions) can readily be used to assess the sensitivity of employment to taxes. The only necessary adjustment in this case is to scale the transfer-elasticities with the inverse of the take-up rate. Notice that the distributional assumptions in proposition 1 are not very restrictive since they apply at the *household-type* level. Even though we in this paper study an out-of-work program (a variation in B^0), Proposition 1 can also be fruitfully applied when studying in-work tax credits (variations in B^1).

4.3 Heterogeneous responses and aggregate elasticities

It is well-known that the responsiveness along the extensive margin is not captured by a single structural parameter but instead by the number of workers who are, at the margin, indifferent between working and not working. To illustrate this in the simplest possible way, consider our model while assuming *identical* fixed cost distributions for all $h \in \mathscr{H}$, with pdf $f(q)$ and cdf $F(q)$. In this simple example we abstract from the take-up decision. Hence, employment in household type h can be written $e_h = \int_0^{z_h - T_h} f(q)dq = F(z_h - T_h)$. Notice that when the fixed cost functions are identical across h, the employment

[19] Notice that we have chosen to evaluate all elasticities at the point $z_h - T_h - B_h^0 + B_h^1$ which is the financial reward from work for a person who takes up transfers both in the state of work and non-work.

level will solely depend on disposable income in the state of work, $z_h - T_h$, and employment will be larger in household types with larger potential earnings. We have that $z_h - T_h = F^{-1}(e_h)$ where $F^{-1}(e_h)$ is the generalized inverse distribution function defined as $F^{-1}(e_h) = \inf\{x \in \mathbb{R} \mid F(x) \geq e_h\}$. Moreover,

$$\frac{de_h}{dT_h} = -F'(z_h - T_h) = -F'(F^{-1}(e_h)). \tag{5}$$

This shows that the employment effect depends on the mass (density) of the fixed cost distribution at the *quantile* $F^{-1}(e_h)$. Specifically, $\frac{de_h}{dT_h}$ will depend on e_h, unless F is uniform. A related observation is made by Chetty et al. 2012 who notes that the size of the extensive margin responses depend on the density of the distribution of reservation wages around the economy's equilibrium and that these elasticities vary with the wage rate unless the density of the reservation wage distribution happens to be uniform.[20]

In the empirical analysis we will recover participation elasticities for different subgroups by using variation in the secondary earner's PTR. Recall that the PTR conditional on taking up the transfer is $\tau_h = \frac{T_h + B_h^0 - B_h^1}{z_h}$. As explained in section 2, the variation in τ_h mainly originates from changes in transfers received in the state of non-work, B^0. We now assume that there are Θ subsets of $\mathscr{H}$ and denote each subset by $\mathscr{H}_\theta$. One possibility, that we consider in the empirical analysis below, is to group household types into four groups (quartiles) $\{\mathscr{H}_\theta\}_{\theta=1}^4$ based on the secondary earners' predicted income. The average employment in each set $\mathscr{H}_\theta$ is $\bar{e}_\theta = \sum_{h \in \mathscr{H}_\theta} \frac{\pi_h e_h}{\sum_{h \in \mathscr{H}_\theta} \pi_h}$. Consider now how this quantity responds to a marginal increase in the PTRs $\{\tau_h\}_{h \in \mathscr{H}_\theta}$ induced by marginal increases in B_h^0, $h \in \mathscr{H}_\theta$. The marginal effect on $\bar{e}_\theta$ of such a change can, invoking the assumptions in proposition 1, be written as:

$$\nabla_{\mathbf{v}} \bar{e}_\theta = -\sum_{h \in \mathscr{H}_\theta} \frac{\pi_h}{\sum_{h \in \mathscr{H}_\theta} \pi_h} \frac{de_h}{dB_h^0} z_h \tag{6}$$

$$= -\sum_{h \in \mathscr{H}_\theta} \frac{\pi_h}{\sum_{h \in \mathscr{H}_\theta} \pi_h} \gamma_h z_h G_h(B_h^0) \tag{7}$$

$$= \beta_\theta, \tag{8}$$

where $\nabla_{\mathbf{v}} \bar{e}_\theta$ is the directional derivative of the average employment in group $\mathscr{H}_\theta$ along the direction $\mathbf{v}$ specified by the change in the PTRs $\{\tau_h\}_{h \in \mathscr{H}_\theta}$ (which operate through changes in $\{B_h^0\}_{h \in \mathscr{H}_\theta}$). G_h is the CDF of the take-up cost distribution and γ_h is the density of the fixed cost of work distribution (see appendix C for details). The parameter of interest that we will estimate is β_θ. It

[20]The model analyzed by Chetty et al. (2012) is isomorphic to ours. The reservation wage corresponds to the fixed-cost threshold for labor force participation that appear in the derivation of proposition 1 in section C. Moreover, in a perfectly competitive labor market equilibrium, there is a one-to-one relationship between the wage rate and the employment level.

is, however, more in line with previous literature to transform marginal effects into elasticities. We define the average participation elasticity in subpopulation $\mathscr{H}_\theta$ as:

$$\bar{\varepsilon}^T_\theta = -\sum_{h\in\mathscr{H}_\theta} \frac{\pi_h}{\sum_{h\in\mathscr{H}_\theta}\pi_h} \frac{de_h}{dT_h} \frac{z_h - T_h - B^0_h + B^1_h}{e_h} = -\sum_{h\in\mathscr{H}_\theta} \frac{\pi_h}{\sum_{h\in\mathscr{H}_\theta}\pi_h} \frac{de_h}{dT_h} z_h \frac{(1-\tau_h)}{e_h}.$$

Using equations (6)-(8), we can approximate the average participation elasticity in subgroup $\mathscr{H}_\theta$ as

$$\bar{\varepsilon}^T_\theta \approx \beta_\theta \frac{(1-\bar{\tau}_\theta)}{\bar{e}_\theta \overline{G}_\theta(B^0)}, \tag{9}$$

where for a variable x, $\bar{x}_\theta$ denotes an average over the subset $\mathscr{H}_\theta$. Finally, note that we could use the same reasoning as that behind (9) to aggregate over the entire treated population.

5 Empirical labor supply analysis

5.1 Econometric method

Our aim is to estimate the following relationship on secondary earners in (formally) married couples where both spouses are aged 30-55

$$e_{ihkt} = \alpha + \beta\tau_{ihkt} + \eta_{ihkt} \tag{10}$$

where β can be given the interpretation in equations (6)-(8). The time period of study is 1994 to 2001. The dependent variable e_{ihkt} is a dummy which takes on the value of 1 if individual i with k children in household type h in year t is employed and is zero otherwise. In our baseline specification we define employment as having positive earnings. Moreover, k will be binary in the analysis and equal to 1 if there is at least one child aged below 20 in the household and 0 otherwise. The independent variable τ_{ihkt} is individual i's PTR which is calculated assuming that eligible households take up the housing allowance. Finally, η_{ihkt} is an error term.

We define household types, h, based on the two spouses' age (five groups) and education (four groups). This leaves us with $4^2 \times 5^2 = 400$ household types. In the empirical analysis, the household types primarily function as fully saturated controls for age and education. We will estimate the model on broad aggregates of household types (discussed in section 4.3).

As already described in section 3.3, we estimate τ_{ihkt} on a smaller survey data set that contains all variables necessary to compute the household's taxes and transfers accurately. Let W denote a vector of variables that are contained both in the main (population wide) data set and in the smaller survey data set (W is a subset of the variables needed to compute the PTR). We refer to the coefficient vector in the regression of τ_{ihkt} on W_{it} on the smaller data set as ρ and focus on the following regression model for the population wide data set:

$$e_{ihkt} = \alpha + \beta \hat{\tau}_{ihkt} + \eta_{ihkt}, \tag{11}$$

where $\hat{\tau}_{ihkt} = \hat{\rho} W_{it}$. To account for the fact that $\hat{\rho}$ is estimated with uncertainty we have checked that the standard errors are robust to the corrections suggested by Murphy and Topel (1985), see section 7.2 below.

If we were to estimate (11) in a cross section without any control variables one would fear $\widehat{\beta}$ being biased. The reason is of course that $\widehat{\beta}$ also would capture direct effects of W on e. If, on the other hand, one would include controls for W in a flexible way, identification would be lost. The leading idea of our paper is to exploit the 1997 housing allowance (HA) reform to address the potential endogeneity of $\hat{\tau}_{ihkt}$ in equation (11). The HA reform substantially reduced PTRs for households with children in certain income intervals, but left households without children unaffected. Hence, if there are no direct effects on the outcome variable of the interactions between the children dummy, λ_k, and the time dummies, λ_t, (conditional on λ_k and λ_t) the HA reform can be used as an instrument for τ.

The richness of the data enables us to control for covariates and time trends in a very flexible way. We let λ_{kt} be the vector of excluded instruments. λ_{kt} is the full set of interactions between the child and time dummies. Ultimately, we wish to estimate the equation

$$e_{ihkt} = \alpha + \beta \hat{\tau}_{ihkt} + \lambda_t + \lambda_k + \lambda_h + \lambda_{hk} + \lambda_{ht} + \gamma X_{ihkt} + \eta_{ihkt}, \tag{12}$$

where X_{ihkt} is a rich set of pre-determined control variables not used to construct the household types. In the X vector we include seven dummies for region of origin as it is well-known that foreign-born on average exhibit lower employment rates than natives.[21] In addition, we include 21 dummies for county of residence to account for regional employment differences. Moreover, we interact the dummies for region of origin and the county dummies with the children and the time dummies. Finally, we also include detailed age dummies (one dummy per age), which we interact with the children dummmy. Technically, due to the very large number of dummy variables included, we estimate (12) by the control function method, which under linearity produces identical point estimates as 2SLS. [22]

[21] These regions are (i) Sweden , (ii) Western Europe, North America and Oceania, (iii) Eastern Europe and former Soviet Union , (iv) South America, (v) Sub-Saharan Africa, (vi) Northern Africa and Middle East and (vii) Asia.

[22] We plug in the residuals from the first stage regression into equation (12). We use the Stata areg command while demeaning the data with respect to time-specific household fixed effects. A potential issue is that standard errors will be biased. Fortunately, for specifications with a smaller set of covariates we can compare the standard errors obtained from standard 2SLS regressions with the standard errors obtained from the control function method. We find that

Notice that, at the *individual* level, the imputed participation tax rate $\hat{\tau}$ in equation (11) will often be measured with error. The reason is that the imputations are made at the group level (see section 3.3). However, since we instrument $\hat{\tau}$ with λ_{tk}, the requirement for consistent estimation of β in equation (12) is that the year-specific group averages are correct.

Why do we compare low income households with and without children? An alternative would be to focus only on households with children and define treatment status according to the income of the husband. That is, wifes with low income husbands would be assigned to the treatment group and wives married to high income husbands would be assigned to the control group. Remember, however, that for the structural interpretation of β to hold we need to impose the assumption that the marginal effect of τ on e is the same in the treatment and control groups. In practice, this means that we will not only have to consider common trends for households with and without children, but we also need to check that the employment *levels* are reasonably similar between the groups. As emphasized in section 4.3, we expect the employment response to depend on the employment level. It will be apparent from figure 3 below that this is indeed the case for couples with and without children. In contrast, female employment is systematically higher in high income households than in low income households. Therefore, as explained below in section 5.3 we instead exploit untreated high-income households for making placebo tests. Reduced form results are, however, quite similar if we keep 'low income households with children' as the treatment group, but instead use 'high income households with children' as the control group.

Throughout the results section we will report standard errors that are clustered at the individual level rather than the household type level. The logic is the following. In our analysis we compare labor supply behavior in similar household types with and without children. This is conceptually different from using within-individual variation to identify the response. [23] However, recall that we are using individual level data on the entire population. Hence, over time, individuals will change household type (as they grow older). The reported standard errors are robust to non-independence of the error terms for the same individual.

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the confidence intervals are quite similar. In a specification with time, children and household dummies only, the point estimate for the PTR is -0.102. The 95 percent confidence interval ranges from -0.121 to -0.084 with 2SLS and from -0.125 to -0.079 with the control function method. Hence, we do not believe that a correction substantially would change the interpretation of the results. We have therefore chosen not to make such a correction, which is computational burdensome with a very large number of control variables.

[23] The fundamental problem of exploiting within-individual variation in this context is that aging parents' and aging non-parents' labor supply are likely to evolve differentially also in the absence of a housing allowance reform. When using household types we compare parents of the *same age* both before and after the reform. This approach also circumvents issues related to child births.

5.2 Sample restrictions

In line with previous literature (e.g. Eissa and Hoynes 2004) we assume that the wife is the secondary earner and that the husband is the primary earner.[24] We make the following sample restrictions. First, we restrict the sample on that the husband has positive earnings in order to guarantee that the secondary earner's PTR is well-defined.[25] Second, we estimate equation (12) on the subsample of household types substantially affected by the differential drop in PTRs. This is achieved by restricting the sample as a function of the husband's actual qualifying income.[26] More specifically, a household is included in the main estimation sample if the actual qualifying income falls below the median level of qualifying income. The cut-off at the median income was chosen because it corresponds to an income level of around 230,000 SEK in 1996, and households with levels of qualifying income exceeding this threshold were not eligible to any sizable housing allowances prior to the reform.[27] As described below in section (5.3) we will also run placebo regressions on a separate sample of high-income couples, which is identical to the main sample in all other respects. Finally, we drop households where any of the two spouses are aged below 30 or above 55. As described in section 2, households with two spouses aged below 30 were subject to different housing allowance rules both before and after the reform. The upper age limit is imposed as we are interested in the labor supply behavior of prime-aged individuals and not in retirement behavior.

As already mentioned, equations (12) and (13) are estimated on the time period 1994 to 2001 while the graphical analysis of section 6 covers the years 1991-2010. The reason for focusing on the time period 1994-2001 in the regression analysis is that reliable estimates from the micro-simulation FASIT are available from 1994 and onwards. There was also a severe macroeconomic crisis in the beginning of the 1990's in Sweden. The reason for not using years after 2001 is that a large childcare fee reform was implemented in 2002 (see Lundin et al. 2008).

[24] In our data, the vast majority of secondary earners are women.

[25] If the husband has zero earnings the wife's PTR will be the PTR of the primary earner.

[26] In the register data, we compute qualifying income based on information on earnings and capital income and imputing financial assets from information on capital income.

[27] The upper limits of qualifying income (i.e. the income level where the entire housing allowance was phased out) differed depending on the number of children below 20 in the household. In 1997, the upper limit was SEK 267,000 for 1 child, SEK 307,500 for 2 children and SEK 351,000 for 3 or more children. Since we pool all households in the main analysis, we cannot use separate income cut-offs.

5.3 Reduced form and placebo regressions

We also estimate reduced form regressions. To be more specific, we will estimate

$$e_{ihkt} = \mu_{kt} + \mu_t + \mu_k + \mu_h + \mu_{hk} + \mu_{ht} + \delta X_{ihkt} + \upsilon_{ihkt} \quad (13)$$

where μ_{kt} is a shorthand for the interactions between the children dummy and the time dummies.

Since the housing allowance reform occurred in 1997, the estimation sample contains three pre-reform years and five post-reform years. We chose 1996 as the reference year. Due to the length of the estimation sample we are able to account for both pre-reform trends in the estimation as well as estimate how responses evolve across post-reform years. The dynamic dimension is crucial: In the presence of adjustment costs we expect the long-run response to be larger than the short-run response.

The identifying assumption in the difference-in-difference specification is that labor supply behavior of secondary earners with and without children would have evolved similarly in the absence of the reform. The fact that we have access to several years of pre-reform data allows us to test this 'parallel-trends' assumption for the years before the reform. For obvious reasons, we cannot verify if this assumption holds in our low income sample for the years after the reform. However, given that the housing allowance reform only affected low income households we can run 'placebo'-regressions on the sample of rich households. If the labor force participation of secondary earners in high income households with and without children (which were essentially all untreated) evolved similarly after the reform, this provides some evidence on the likelihood that the post-reform trends for the low income sample would be similar as well and thereby serve as an important robustness test. More specifically, we have constructed a placebo-test by estimating equation (13) on females married to husbands with qualifying incomes above the 75th percentile which in 1996 corresponded to an income level of around 310,000 SEK.[28] If there is a 'response' of high-income households in the post-reform period there is a concern that the estimated effect in the low-income sample reflects some underlying employment trend of women with children rather than a causal effect of the reform.

6 Graphical analysis

In Figure 2 we plot the evolution of the average PTR for the treatment and control groups (households with and without children) over the time period 1994-2001 which is the focus of our regression analysis. The PTR:s have

[28] In fact, some households with 3 or more children could be eligible for housing allowance up to 351,100 SEK.

been calculated on HEK-data using the micro-simulation model FASIT (which takes the entire Swedish tax- and transfer system into account). As can be seen from the Figure, the reform in 1997 implied a sharp drop in the average PTR for the treatment group. This drop was caused by the housing allowance reform and demonstrates the strength in the first stage of our IV strategy. Before the housing allowance reform of 1997 the gap in the average PTR:s for couples with and without children respectively exceeded 10 percentage points and was substantially smaller in the post-reform period.

In Figure 3 we show how the employment of married women (defined as having positive earnings) evolved in couples with and without children between 1991 and 2010. A nice feature of figure 3 is that it illustrates the evolution of employment outside the more narrow time period of our regression analysis. [29] We make the following observations. In the beginning of the 1990's, there was a sharp decline in employment due to a deep economic recession. Figure 3 suggests that female employment decreased slightly more among households with kids 1991-1993. However, between 1993 and 1996 the two lines moved in parallel. Note also that the employment *levels* are strikingly similar. After the 1997 reform, employment continued to evolve similarly until 1998. Then there was a relative employment increase of women with children, which continued in the post-reform period.

7 Results

In the following sections we present and discuss our empirical findings.

7.1 Reduced form effects

We start off by presenting results from the simplest and most transparent specification, equation (13), where we are interested in the interactions between the indicator variables for having children and the year dummies. The coefficients on these interactions for the post-reform years capture the dynamics of the reform effect and the coefficients on the interactions for the pre-reform years allow us to test that the pre-reform trends were parallel for households with and without children.

Our complete set of results for the reduced form effects analysis are presented in table 1. Columns 1-4 show the coefficients for the main 'low income' sample where most households with children were eligible for housing allowances (at zero earnings of the wife). The first column reports the results of a difference-in-difference specification without any control variables. In

[29] In both figures 2 and 3 we maintain the same sample restrictions as in the regression analysis, i.e. we focus on households where the husband's qualifying income falls below the 50th percentile and where the husband reports positive earnings.

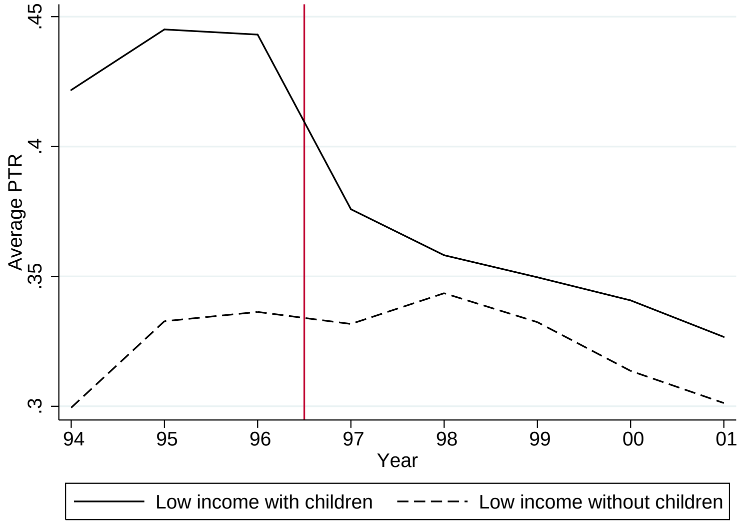

Figure 2. (*Graphical first-stage*) Average participation tax rates (PTR) by child status on HEK data. PTR:s are calculated in FASIT. The sample is restricted to households where the husband's qualifying income falls below the 50th percentile and where the husband reports positive earnings.

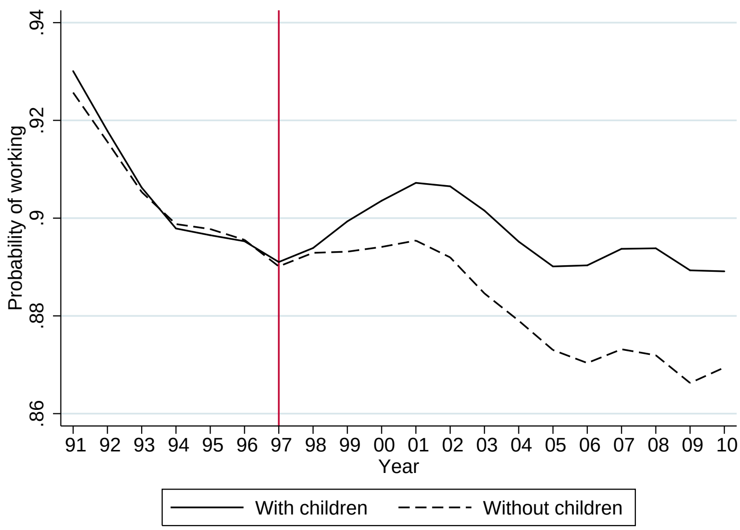

Figure 3. (*Graphical reduced form evidence and long term trends*). Female participation (share with positive earnings) in low income households where the husband participates in the labor force.

this column, the first thing to notice is that the coefficients for the pre-reform years, 1994 and 1995, are statistically insignificant, confirming the visual evidence of figure 3 that the pre-reform trends were very similar for the treatment and control group. In fact, the coefficients for the pre-reform years remain insignificant for all the specifications that we have considered as evident by columns 1-4. Moreover, also consistent with figure 3, we see that there is a statistically significant response to the reform in 1999 and that the response grows monotonically across the post-reform years. For 2001 the estimated effect amounts to 1.2 percentage points.

In column 2 we have added household type controls and the estimated effects become somewhat larger. In column 3 we control for trends in a flexible way including the full set of interactions between the time dummies and the household type dummies as well as the interactions between the household type dummmies and the dummy for having children. Interestingly, in this specification, the reduced form effect estimates are also significant for the two post-treatment years 1996 and 1997 (at the 5 percent level). Finally, when the full set of controls are included in column 4, the overall pattern of coefficients is similar to column 3, but the reform effect estimate for 2001 is more in line with that obtained in the specification without controls in column 1. Our preferred estimate of the reform effect is the coefficient for 2001 in our most ambitious specification of column 4 and amounts to a 1.12 percentage point increase in the probability of married women to participate in the labor force.[30]

We see that the response in general is increasing in each post-reform year. This suggests that adjustment costs, e.g. the search cost of finding a new job, are important. As discussed above in section 2.3 information about the reform became publicly available close before its implementation, and it probably takes some time for households to adjust.

In column 5 we report the results from a 'placebo-regression' with the full set of controls, where we have estimated equation (13) on a sample consisting of women married to husbands with qualifying income over the 75th percentile (which were essentially all untreated by the reform). In all other respects, the selection criteria are identical to the main low-income sample. It is striking that all estimated coefficients are insignificant at the 5 percent level. One interaction, the interaction for 2001, is significant at the 10 percent level, but the coefficient estimate is considerably smaller than the corresponding point estimate in the low income sample. The results of this placebo regression, considered in conjunction with the results in column 1-4 showing that the trends before the reform were parallel, and the visual evidence in 3, allow us to be reasonably confident that the identifying assumption in our difference-in-difference setup is satisfied.

[30]These results are robust to excluding cells (defined based on year×children×household type) that contain less than 100 observations.

In order to examine the validity of 'primary-secondary earner' assumption we have estimated equation (13) on a sample of males. Our idea has been to construct the male sample as a *mirror image* of the female low income sample by conditioning the male sample on the wife's qualifying income falling below the 50th percentile. The results are presented in column 1 of table A1 of Appendix D where it can be inferred that the estimated coefficients for this male sample are very different from the female sample. For 1994-2000 none of the interaction terms are statistically significantly different from zero. For 2001 we estimate a *negative* effect on male employment equal to -0.36 percentage points which is significant at the 5 percent level. To dig deeper into the potential mechanisms at play we have also examined the males' potential *earnings* responses (intensive margin response). We found no clear evidence of a response in log earnings after including the full set of controls, see columns 2 and 3 of of table A1.

Finally, we have also estimated equation (13) on the main *female* sample with log earnings instead of employment on the left hand side. We first transformed earnings into log earnings in the standard way, thereby *excluding* women with zero earnings. The estimation of this pure 'intensive margin' response resulted in small positive coefficients for the post-reform years (see column 4 of table A1). However, we then used log of (earnings+1) as dependent variable, thereby *including* females with zero earnings in the regression and found that the estimated coefficients were significant in all post-reform years and also substantially larger (see column 5 of table A1). The results from these two exercises lead us to conclude that women primarily reacted to the reform along the extensive margin, i.e. they went from zero earnings to a positive amount of earnings.

7.2 Elasticities

We now turn to our participation elasticity estimates. Before discussing the results we briefly comment on how we construct the participation elasticities based on the regression coefficients (marginal effects). The elasticities are calculated according to equation (9) where we have multiplied the estimate $\hat{\beta}$ of β of equation (12) with the ratio $\frac{\overline{1-\tau}}{\bar{e}\bar{G}(B^0)}$. In this expression, $\overline{1-\tau}$ and $\bar{e}$ are the averages of $1-\tau$ and e (the employment rate) over the years 1994-2001 in the low income sample and $\bar{G}(B^0)$ is the average take-up rate of one-earner households in the pre-reform period, which is observed to be around 0.6 in the HEK sample.[31] Moreover, if the conditions specified in proposition 1 in section 4.2 are satisfied, the participation elasticities that we construct based on the marginal effect in regression (12), can be given a structural interpretation.

[31] We obtained this figure by pooling the pre-reform years, 1994-1996. Due to the fact that the sample is restricted to only include households where the wife does not work, the sample size is too small to provide a more disaggregated estimate of the take-up rate.

Table 1. *Reduced form effects (in percentage points)*

	Low income	Low income	Low income	Low income	High income
	(1)	(2)	(3)	(4)	(5)
Year 1994 × children	-0.060	-0.152	0.000	-0.097	-0.264
	(0.130)	(0.129)	(0.163)	(0.159)	(0.171)
Year 1995 × children	-0.097	-0.121	-0.095	-0.140	0.016
	(0.111)	(0.110)	(0.140)	(0.137)	(0.149)
Year 1997 × children	0.120	0.154	0.348**	0.404***	-0.117
	(0.114)	(0.113)	(0.144)	(0.141)	(0.153)
Year 1998 × children	0.129	0.245*	0.331**	0.392**	0.000
	(0.134)	(0.132)	(0.169)	(0.164)	(0.178)
Year 1999 × children	0.652***	0.833***	0.681***	0.813***	0.189
	(0.145)	(0.144)	(0.181)	(0.177)	(0.192)
Year 2000 × children	0.976***	1.24***	0.790***	0.992***	0.245
	(0.154)	(0.152)	(0.189)	(0.185)	(0.202)
Year 2001 × children	1.214***	1.485***	0.863***	1.120***	0.385*
	(0.160)	(0.159)	(0.196)	(0.193)	(0.211)
Household type dummies	No	Yes	Yes	Yes	Yes
Household type × children	No	No	Yes	Yes	Yes
Household type × year dummies	No	No	Yes	Yes	Yes
Additional controls	No	No	No	Yes	Yes
No. of observations	2,770,100	2,770,100	2,770,100	2,770,100	1,385,071

Note: Dependent variable: probability of having positive earnings. 'Low income' sample consists of wives married to husbands with a positive qualifying income, which falls below the 50th percentile. 'High income' sample consists of wives married to husbands with a positive qualifying income that falls above the 75th percentile. All specifications contain a dummy for having children and a full set of year dummies. 400 household types are defined based on 5 age dummies for each spouse and 4 education level dummies for each spouse. The additional control variables are specified in section 5.1. Standard errors reported below the estimates are robust to heteroscedasticity and clustered at the household level. * indicates significance at 10%. level, ** 5% level and *** at 1% level.

Table 2. *Participation elasticity estimates*

	(1)	(2)	(3)	(4)
Participation elasticity	0.088***	0.117***	0.098***	0.127***
	(0.013)	(0.013)	(0.020)	(0.020)
Household type dummies	No	Yes	Yes	Yes
Household type × children	No	No	Yes	Yes
Household type × year dummies	No	No	Yes	Yes
Additional controls	No	No	No	Yes
No. of observations	2,770,100	2,770,100	2,770,100	2,770,100

Note: Elasticities are evaluated at the mean values of employment (0.897) and (1-PTR) (0.659) over the years 1994-2001 in the total 'low income sample'. 2SLS regressions are run on 'low income sample', which consists of wives married to husbands with a qualifying income below the 50th percentile. The average take-up rate is set to 0.6. The interactions between the year dummies and the dummy for having children are the excluded instruments. All specifications contain a dummy for having children and a full set of year dummies. 400 household types are defined based on 5 age dummies for each spouse and 4 education level dummies for each spouse. The additional control variables are specified in section 5.1. Standard errors reported below the estimates are robust to heteroscedasticity and clustered at the household level. * indicates significance at 10%. level, ** 5% level and *** at 1% level. Standard errors for elasticities are obtained by the delta method.

The results are presented in table 2. Columns 1-4 show estimates using different sets of control variables. The instruments are strongly correlated with the PTR. In the 2SLS regression presented in column 1 the first-stage F-statistic of the excluded instruments is as large as 66,834. In each case we obtain precise estimates of the participation elasticity. Our preferred estimate is obtained for our most ambitious set of controls (column 4) in which case the elasticity estimate amounts to 0.13. The exact magnitude of the elasticity estimate varies somewhat depending on the set of control variables used in the regressions. This is perhaps not too surprising in light of the results for the reduced form effects in table 1.

Before closing this section we would like to point out that we are aware of the fact that since the PTR:s have been estimated in a separate step, our standard errors might be slightly biased due to presence of a generated regressor in equation (12). Since performing a proper correction of the covariance matrix for the full specification, which contains a huge amount of dummy variables, would be computationally very burdensome we have instead made a correction á la Murphy and Topel (1985) for the specification without control variables reported in column 1. More specifically, we have computed the covariance matrix given by equation (15') of Murphy and Topel (1985) and verified that the correction did not, at least in this case have any profound impact on the

Table 3. *Heterogenous response*

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Participation elasticity	0.235***	0.117*	0.109**	0.090***
	(0.058)	(0.047)	(0.038)	(0.027)
Mean employment level	0.808	0.903	0.923	0.955
Household type dummies	Yes	Yes	Yes	Yes
Household type × children	Yes	Yes	Yes	Yes
Household type × year dummies	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
No. of observations	692,559	692,542	692,476	692,523

Note: Elasticities are evaluated at the mean values of each subsample. 2SLS regressions are run on 'low income sample', which consists of wives married to husbands with a qualifying income below the 50th percentile. Quartiles are created based on tbe wife's predicted income. The average take-up rate is set to 0.6. The interactions between the year dummies and the dummy for having children are the excluded instruments. All specifications contain a dummy for having children and a full set of year dummies. 400 household types are defined based on 5 age dummies for each spouse and 4 education level dummies for each spouse. The additional control variables are specified in section 5.1. Standard errors reported below the estimates are robust to heteroscedasticity and clustered at the household level. * indicates significance at 10%. level, ** 5% level and *** at 1% level. Standard errors for elasticities are obtained by the delta method.

standard errors. The implied standard error increased only slightly from 0.013 to 0.014. We therefore conclude that the generated regressor bias is likely to be small and of little practical importance.

Table 3 reports the subsample analysis with the full set of control variables. As we move across the four quartiles we see that the elasticities are falling monotonically in the wife's skill level mirrored by a corresponding monotonic increase in the employment level. In line with our expectations, the elasticity is the largest in the first quartile, where the employment level is substantially smaller than in the other three quartiles. The elasticity estimate for the first quartile (0.235) and the fourth quartile (0.09) are statistically different at a level of 95 percent.[32]

[32]Following e.g. Clogg et al. (1995), p.1276, we test this using the fact that differences between the coefficients from a regression run on two independent large samples x and y can be assessed by the statistic $Z = \left(\hat{\beta}_x - \hat{\beta}_y\right) \Big/ \sqrt{se_x^2 + se_y^2}$, which follows a standard unit normal distribution. $\hat{\beta}_j$ and se_j are the coefficient and the standard error of sample $j = x, y$. Since we are interested in testing for differences in elasticities, we have made the proper adjustments by multiplying the coefficients and standard errors by different constants. Using the values

7.3 Heterogenous response

As emphasized in section 4 above we anticipate the elasticity to differ across subpopulations with different baseline employment rates. In the past, extensive margin responses to taxes have been estimated on relatively small data sets. Since we have access to population wide registers we are able to examine how the elasticity differs across subpopulations in a systematic way.

We divide the low income sample into four *quartiles* based on imputed log earnings. In the imputation regressions, which are run separately for each year, we control for household type (as defined above). In addition we include dummies for 7 regions of origin, dummies for municipality of residence and a full set of age dummies. After partitioning the sample into four quartiles, we rerun equation (12) on each quartile. Following the procedure suggested by equation (9) we evaluate the elasticity at the *subsample-specific* mean values of employment and $(1-\tau)$.

8 Concluding remarks

In this paper we have estimated participation elasticities of secondary earners using a credible and transparent identification strategy. Our central estimate of the participation elasticity is 0.13, arguably a lower value than many earlier estimates obtained in the literature. Crucially, we have also presented quasi-experimental estimates of participation elasticities for subgroups of the population with different employment levels. This exercise was made possible by virtue of our large sample size. Dividing up the population into four quartiles based on the wife's skill level we find participation elasticities ranging from 0.24 at the bottom to 0.09 at the top. The point estimates of the elasticities fall monotonically in skill level, and the elasticity differences between the bottom and the top are statistically significant.

Intuitively, the higher the employment level, the smaller the pool of unemployed that can be incentivized to enter the labor force. Following e.g. Chetty et al. (2012) we have emphasized that the participation elasticity is determined by the number of individuals who are indifferent between working and not working, which in the context of our simple model, depends on the local shape of the distribution of fixed costs of work. In line with the public finance literature, we have assumed that employment is voluntary and focused on the decision to enter the labor force. If involuntary unemployment is more common among the low-skilled we potentially underestimate the participation elasticity in this group.

Our central estimate of 0.13 is well below the participation elasticities of married Swedish women estimated by Selin (2014). Selin exploited the 1971

for the elasticities and standard errors in column 1 and 4 of table 2 we obtain a Z-ratio of 2.266, which is larger than the critical value 1.96.

Swedish tax reform which implied a switch from joint to individual taxation and found estimates in the range 0.5-1.0. The estimates that we have reported in the present study are consistent with Selin's estimates. Selin (2014) reports that the pre-reform share of married women with positive earnings was 67% (Table 8) whereas the corresponding share in the present study is 90%. This adds another important data point and corroborating evidence in support of the important relationship between the participation elasticity and employment level that we have emphasized in this paper.

The key insight from this paper is that the participation elasticity is fundamentally different in nature from the intensive margin labor elasticity. When 'plugging in' the participation elasticity into simulation models it is indeed important to consider the employment level in the subpopulation of interest.[33] This point has been made before, see e.g. Chetty et al. (2012); our contribution is to examine this feature of the participation response using administrative data and transparent identification.

[33] Our quasi-experimental estimates provide a useful contrast against estimates obtained using microsimulation models. Immervoll et al. (2007) analyze welfare reforms in 15 European countries including Sweden, and calibrate the average participation elasticity for the whole economy to 0.2, but decreasing across deciles. In a related exercise, which is more focused on participation responses, Immervoll et al. (2011) assume participation elasticities for secondary earners in the range 0.3-0.7. In light of this paper these elasticities appear to be too large, at least for a country like Sweden.

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Appendix

A Formula for calculating the HA

Both for 1996 and 1997 the maximum monthly housing allowance (MMHA) can be written

$$MMHA = 600 + \max\{0, (\min(QHE, 3000) - 2000) \times 0.75\} + \max(0, (QHE - 3000) \times 0.50) \quad (14)$$

where HA = household housing allowance [SEK/month], QHE = qualifying household housing expenses [SEK/month], and I = household income before tax [SEK/month]. However, the qualifying housing expenses changed between 1996 and 1997.

In 1996 QHE was simply the rent paid by the tenant. There was also a minimum guaranteed housing expense level (which was a function of the number of children).

For 1997 the QHE can be written

$$QHE = \max\left(MHE, HE \times \frac{\min(SC, AS)}{AS}\right), \quad (15)$$

where MHE =minimum guaranteed housing expense level , HE = actual housing expense (rent), SC = space constraint and AS = actual space constraint. The space constraint depends on the number of kids in the houshold.[34] [35]

[34] 1 child: 80 sqm, 2 children: 100 sqm, 3 children: 120 sqm, 4 children: 140 , 5 or more: 160 sqm.

[35] The yearly rent per square meter was approximately SEK 700, 1996-97. Rent statistics: http://www.boverket.se/Global/Webbokhandel/Dokument/2011/Hyror-i-Sverige-1975-2009.pdf, figure 2.1.

B Other components of the reform

In the discussion of section (2.2) we only considered the individualization of the exemption level, which is the main focus of our paper. However, two other potentially important components of the reform deserve to be mentioned as well; the new space restriction and the *ex post* adjustment of the allowance.

Although the upper cap on the transfer before phase-out, B^{00}, did not change, many households nevertheless experienced a decrease in B^{00}. In the 1997 reform package the government introduced an upper limit to the qualifying housing space, i.e. the number of square-meters of dwelling space the household could be compensated for. We take this space restriction into account when calculating the participation tax rates. It lowered the transfers, especially for couples who tend to live in larger apartments than singles.

Both before and after 1997 the beneficiary had to repay the benefit if the household's qualifying income substantially increased and the household did not report this increase in income.[36] However, before 1997 the household never had to repay an allowance it was eligible for at the month of the monthly benefit payment. From 1997 and onwards, the monthly allowance receipt was labeled as 'preliminary'. In the new system, the beneficiary applies in December year t for housing allowance in year $t+1$. In year $t+1$ the beneficiary each month receives the housing allowance based on the qualifying income reported in the application in December year t. In year $t+2$ the two spouses file their tax returns. By the end of $t+2$ the Social Insurance Agency receive information from the Tax Agency on the household's *ex post* qualifying incomes in $t+1$. Finally, in the spring of year $t+3$ the Social Insurance Agency charge/reimburse households where the incomes reported in year t deviate from the realized income in year $t+1$.

From the point of view of fiscal sustainability, the reform was a great success, to say the least. As can be seen from figure A1, the government's expenditures on the program fell dramatically in the years following 1997 (marked with a vertical line). Moreover, we see that there was a huge decrease in the number of couples receiving the transfer between 1996 and 1997. The decrease among singles was arguably more modest. Single households were affected both by the space restriction (but to a smaller degree than couples as their dwellings typically were smaller) and by the new rules for *ex post* repayments/reimbursements. However, the income limits of singles were unchanged. Why did the size of the program decrease in the years following 1997? In the post-reform period the benefit levels and the income limits were kept at their nominal levels of 1996; they were not indexed. Accordingly, with inflation and real wage growth, a growing fraction of couples and singles became eligible only for small amounts, or became ineligible.

[36] See Boverket (2006) (in Swedish) for a description of these pretty complex rules.

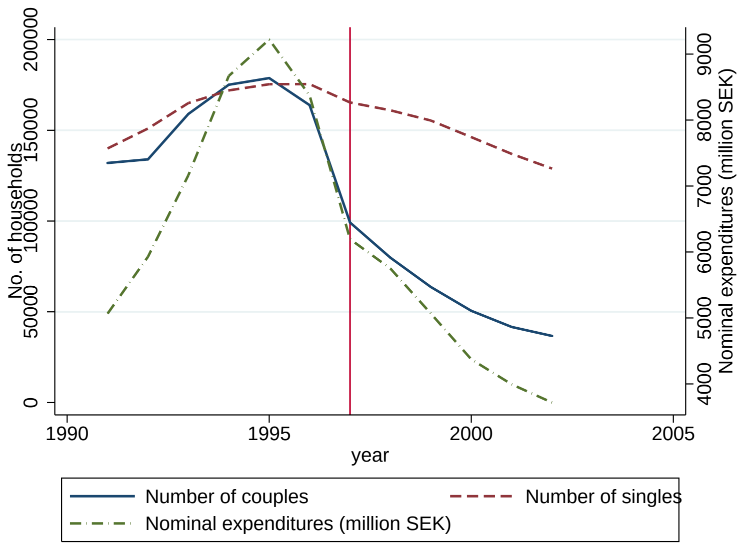

Figure A1. Number of couples and singles receiving housing allowances, as well as nominal expenditures on housing allowances in million SEK. Source Boverket 2006, Table A.

C Proof of proposition 1

First we characterize the fractions of the population in each of the four household states emphasized on page 192 (i.e. $e^{ij}, i, j = 0, 1$) without making any distributional assumptions. Thereafter we impose the assumptions in proposition 1 to derive the relevant derivatives of e with respect to T, B^0, and, B^1, that can be used to establish the relationship between the two key elasticities given in the proposition. To simplify the exposition in this appendix we omit the h index. All calculations are valid at the household-type level.

A general characterization

We describe the decision-making of the household by considering the labor-market entry conditions for the secondary earner depending for different values of the take-up cost χ.

If $0 \leq \chi \leq B^1$ the household always takes up the transfer (both when working and not working) and therefore participates in the labor force when the following condition is met:

$$z - (T^1 - T^0) - (B^0 - B^1) \geq q \quad \text{(low)} \tag{16}$$

If $\chi > B^0$ the household does not take up the transfer in the state of work nor in the state of non-work, and the participation equation becomes:

$$z - (T^1 - T^0) \geq q \quad \text{(high)} \tag{17}$$

If $B^1 < \chi \leq B^0$ the household takes up the transfer when unemployed, but not when working, which implies that the participation equation becomes:

$$z - (T^1 - T^0) - B^0 \geq q - \chi \quad \text{(intermediate)} \tag{18}$$

Note that this last condition depends on χ. That is, the incentive to enter the labor force depends on the size of the take-up cost. It appears in (18) because households with $B^1 < \chi \leq B^0$ only experience the take-up cost when they are outside the labor force.

As the above conditions only depend on the difference between T^1 and T^0 we set $T = T^1 - T^0 \geq 0$ without loss of generality. We denote the threshold values of q which cause inequalities (16),(17), and (18) to bind by q^L, q^H, and q^I, respectively. We have that $q^L \leq q^I \leq q^H$ by virtue of the fact that $B^0 > B^1$ (and the fact that q^I only applies for values of χ satisfying $B^1 < \chi \leq B^0$). Notice that q^L and q^H are fixed and can be expressed in terms of observable quantities as $q^L \equiv q^L(z, T, B^1, B^0)$ and $q^H \equiv q^H(z, T)$ [specifically, $q^L = z - (T^1 - T^0) - (B^0 - B^1)$ and $q^H = z - (T^1 - T^0)$] whereas q^I depends on the take-up cost χ and takes on the value $q^I = q^L$ when $\chi = B^1$ and $q^I = q^H$ when $\chi = B^0$.[37]

In the following we assume q and χ are jointly distributed according to the probability density function $f(q, \chi)$.

Based on conditions (16)-(18) we can write down the number of workers in each state e^{ML}, $M = 0, 1; L = 0, 1$. Note that the division of agents into the four categories above based on their innate characteristics (q, χ) completely characterizes the optimal behavior of agents.

The number of households who work and take-up transfers are:

$$e^{11} = \int_0^{q^L} \int_0^{B^1} f(q, \chi) d\chi dq$$

The number of households who work and do not take up transfers are:

$$e^{10} = \int_0^{q^L} \int_{B^1}^{\infty} f(q, \chi) d\chi dq + \int_{q^L}^{q^H} \int_{q - q^I}^{\infty} f(q, \chi) d\chi dq$$

The number of households who do not work and take up transfers are:

$$e^{01} = \int_{q^H}^{\infty} \int_0^{B^0} f(q, \chi) d\chi dq + \int_{q^L}^{q^H} \int_0^{q - q^I} f(q, \chi) d\chi dq$$

Finally, the number of households who neither work nor take up transfers are:

$$e^{00} = \int_{q^H}^{\infty} \int_{B^0}^{\infty} f(q, \chi) d\chi dq.$$

It follows by construction that the total number of workers is $e^1 = e^{11} + e^{10}$ and that the total number of unemployed agents is $e^0 = e^{00} + e^{01}$ with $e^0 + e^1 = 1$.

[37] Notice that q^I will be a line in the (χ, q)-space.

Derivation of marginal effects of tax/transfer instruments

Assuming q and χ are independent we can write the number of individuals in each group as follows:

$$
\begin{aligned}
e^{11} &= F(q^L)G(B^1) \\
e^{10} &= F(q^L)[1-G(B^1)] + \int_{q^L}^{q^H} f(q)\left[\int_{q-q^I}^{\infty} g(\chi)d\chi\right] dq \\
&= F(q^L)[1-G(B^1)] + \int_{q^L}^{q^H} f(q)[1-G(q-q^I)]dq \\
e^{01} &= [1-F(q^H)]G(B^0) + \int_{q^L}^{q^H} f(q)\left[\int_{0}^{q-q^I} g(\chi)d\chi\right] dq \\
&= [1-F(q^H)]G(B^0) + \int_{q^L}^{q^H} f(q)G(q-q^I)dq \\
e^{00} &= [1-F(q^H)][1-G(B^0)]
\end{aligned}
$$

To establish proposition 1 we need to compute the derivatives of $e = e^{11} + e^{10}$ with respect to the tax/transfer instruments T, B^0 and B^1. That is, we are interested in computing:

$$
\begin{aligned}
\frac{de}{dB^0} &= \frac{de^{11}}{dB^0} + \frac{de^{10}}{dB^0} \\
\frac{de}{dB^1} &= \frac{de^{11}}{dB^1} + \frac{de^{10}}{dB^1} \\
\frac{de}{dT^1} &= \frac{de^{11}}{dT^1} + \frac{de^{10}}{dT^1}.
\end{aligned}
$$

To make progress we impose the additional assumption that $F(q)$ is *locally* uniform on the open interval $(z-T-B^0, z-T)$ in the sense that it has constant pdf with density γ on this interval and is unrestricted elsewhere. In the derivations below, recall that $q^L = z-(T^1-T^0)-(B^0-B^1)$ and $q^H = z-(T^1-T^0)$.

Then, we first notice that:

$$
\begin{aligned}
\frac{de^{11}}{dB^0} &= -\gamma G(B^1) \\
\frac{de^{11}}{dB^1} &= \gamma G(B^1) + G'(B^1)F(q^L) \\
\frac{de^{11}}{dT^1} &= -\gamma G(B^1).
\end{aligned}
$$

For example, the first condition above states that as B^0 is marginally increased, there will be an outflow from the group of workers who take-up transfers according to their number $G(B^1)$ times the marginal density of the fixed-cost distribution γ (which simply reflects the number of individuals who are indifferent between working and not working).[38] In the second condition, the first term states that as B^1 is increased, the fraction of workers who take up the transfer when working will be incentivized to join the labor force, according to the marginal density γ. In addition, there will be an increase in take-up represented by the second term.

Applying slightly more effort we can apply Leibniz integral rule and derive:

$$\frac{de^{10}}{dB^0} = \frac{d}{dB^0}\Big(F(q^L)[1-G(B^1)]\Big) +$$
$$\gamma\int_{q^L}^{q^H}\frac{d}{dB^0}[1-G(q-q^I)]dq + \gamma\frac{dq^H}{dB^0}[1-G(q^H-q^I)] - \gamma\frac{dq^L}{dB^0}[1-G(q^L-q^I)] =$$
$$= -\gamma[1-G(B^1)] + \gamma[-G(q-q^I)]_{q^L}^{q^H} + \gamma[1-G(B^1)] =$$
$$= -\gamma[G(B^0)-G(B^1)]$$

This condition gives the change in the group who works and does not take up transfers in response to an increase in the out-of-work transfer B^0. An increase in B^0 increases non-participation proportionally to $[G(B^0)-G(B^1)]$ which is the fraction of workers with intermediate take-up costs in the sense that they only take-up transfers when unemployed.

Similarly, we can derive:

$$\frac{de^{10}}{dB^1} = \frac{d}{dB^1}\Big(F(q^L)[1-G(B^1)]\Big) +$$
$$\gamma\int_{q^L}^{q^H}\frac{d}{dB^1}[1-G(q-q^I)]dq + \gamma\frac{dq^H}{dB^1}[1-G(q^H-q^I)] - \gamma\frac{dq^L}{dB^1}[1-G(q^L-q^I)] =$$
$$= -F(q^L)G'(B^1).$$

This expression states that as B^1 increases, there will be a dynamic take-up response. Some who previously worked without transfers will now work and take up transfers.

Finally, we derive:

$$\frac{de^{10}}{dT^1} = \frac{d}{dT^1}(\gamma q^L+\rho)[1-G(B^1)] +$$
$$\gamma\int_{q^L}^{q^H}\frac{d}{dT^1}[1-G(q-q^I)]dq + \gamma\frac{dq^H}{dT^1}[1-G(q^H-q^I)] - \gamma\frac{dq^L}{dT^1}[1-G(q^L-q^I)] =$$
$$= -\gamma[1-G(B^1)]$$

To understand this effect note that e^{10} is the fraction of workers who do not take up the transfer while working represented by the fraction $[1-G(B^1)]$ of

[38]Notice that any worker who belongs to the group e^{11} will by assumption also take up the transfer when not-working since $B^1 \leq B^0$.

the population. A number of these individuals will drop out of the labor force in response to the tax increase according to the marginal density γ.

Putting things together we get:

$$\frac{de}{dB^0} = \frac{de^{11}}{dB^0} + \frac{de^{10}}{dB^0} = -\gamma G(B^1) - \gamma[G(B^0) - G(B^1)] = -\gamma G(B^0)$$
$$\frac{de}{dB^1} = \frac{de^{11}}{dB^1} + \frac{de^{10}}{dB^1} = \gamma G(B^1) + G'(B^1)F(q^L) - F(q^L)G'(B^1) = \gamma G(B^1)$$
$$\frac{de}{dT^1} = \frac{de^{11}}{dT^1} + \frac{de^{10}}{dT^1} = -\gamma G(B^1) - \gamma[1 - G(B^1)] = -\gamma.$$

This establishes proposition 1.

D Alternative empirical specifications

Regression results from alternative specifications are reported in table A1.

E Summary statistics

Summary statistics are reported in table A2.

Table A1. *Reduced form effects in alternative specifications)*

	Male employment (1)	Male log(earnings) (2)	Male log(earnings+1) (3)	Female log(earnings) (4)	Female log(earnings+1) (5)
Year 1994 × children	0.045	-0.000	0.002	-0.008	-0.015
	(0.159)	(0.006)	(0.012)	(0.005)	(0.011)
Year 1995 × children	-0.061	0.005	-0.001	-0.004	-0.013
	(0.139)	(0.005)	(0.011)	(0.005)	(0.010)
Year 1997 × children	0.049	-0.000	0.005	0.002	0.031***
	(0.141)	(0.005)	(0.011)	(0.005)	(0.010)
Year 1998 × children	-0.036	0.0039	0.003	0.000	0.029**
	(0.161)	(0.006)	(0.013)	(0.006)	(0.012)
Year 1999 × children	-0.081	0.007	0.004	0.012*	0.068***
	(0.170)	(0.006)	(0.013)	(0.006)	(0.013)
Year 2000 × children	-0.096	0.011*	0.009	0.029***	0.095***
	(0.177)	(0.006)	(0.014)	(0.006)	(0.014)
Year 2001 × children	-0.364**	0.009	-0.012	0.038***	0.112***
	(0.182)	(0.007)	(0.015)	(0.007)	(0.015)
Household type dummies	Yes	Yes	Yes	Yes	Yes
Household type × children	Yes	Yes	Yes	Yes	Yes
Household type × year dummies	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes
No. of observations	2,658,815	2,521,767	2,658,815	2,485,259	2,770,100

Note: Dependent variable: probability of having positive earnings. 'Male sample' consists of husbands married to wives with a positive qualifying income, which falls below the 50th percentile.'Female sample' sample consists of wives married to husbands with a positive qualifying income, which falls below the 50th percentile. All specifications contain a dummy for having children and a full set of year dummies. 400 household types are defined based on 5 age dummies for each spouse and 4 education level dummies for each spouse. The additional control variables are specified in section 5.1. Standard errors reported below the estimates are robust to heteroscedasticity and clustered at the household level. * indicates significance at 10%. level, ** 5% level and *** at 1% level.

Table A2. *Summary statistics*

	With children		Without children	
Labor force participation	0.898	(0.303)	0.895	(0.307)
Net of tax rate $1-\tau$	0.603	(0.067)	0.663	(0.033)
Age of secondary earner	39.720	(5.962)	47.649	(5.765)
Age of primary earner	42.201	(6.246)	49.286	(5.560)
Earnings	1245.602	(841.517)	1395.388	(868.631)
Qualifying income of primary earner	1891.367	(1121.539)	1895.504	(758.296)
Education				
At most 9 years of education	0.156	(0.363)	0.282	(0.450)
At most high school education	0.573	(0.495)	0.536	(0.499)
College education	0.265	(0.441)	0.172	(0.377)
Country of origin				
Sweden	0.920	(0.271)	0.949	(0.220)
Western Europe, North America and Oceania	0.058	(0.233)	0.039	(0.194)
Eastern Europe and former Soviet Union	0.015	(0.121)	0.010	(0.100)
South America	0.001	(0.030)	0.000	(0.016)
Sub-Saharian Africa	0.000	(0.016)	0.000	(0.010)
Northern Africa and Middle East	0.005	(0.069)	0.001	(0.025)
Asia	0.001	(0.033)	0.000	(0.019)
Number of observations	2,069,793		700,307	

Note: Standard deviations reported in parenthesis. Incomes are expressed in 100 SEK. Summary statistics refer to the period 1994-2001.