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n° 2021-08 October 2021

WORKING PAPERS

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Michela **BIA**[1]
German **BLANCO**[1,2]
Marie **VALENTOVA**[1]

[1] Luxembourg Institute of Socio-Economic Research (LISER), Luxembourg
[2] Illinois State University, United States of America

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Fonds National de la Recherche Luxembourg

The Causal Impact of Taking Parental Leave on Wages: Evidence from 2005 to 2015*

Michela Bia† German Blanco‡ and Marie Valentova§

Preliminary and Incomplete Draft

September 16, 2021

Abstract

Within the context of Luxembourg, we analyze the causal effect of parental leave take up on post-birth hourly wages of an important subpopulation of parental-leave-eligible first-time mothers employed regardless of having taken parental leave. We use the Social Security administrative data from 2005 to 2015, i.e. data covering the period before the parental leave reform of 2016. In our analyses, we simultaneously address selection of eligible mothers in taking parental leave, and selection of eligible mothers into employment. To deal with the first complication, we assume that conditional on observed pre-intervention covariates there are no unobserved factors associated to both the assignment to parental leave and the potential post-birth hourly wages. To this end, we control for a rich set of pre-intervention characteristics from 2005 to 2010. The second complication arises since the outcome of hourly wages is only defined for the (post-birth) employed subpopulation. We deal with selection into employment by utilizing a Principal Stratification framework and recent non-parametric bounds. We argue that the monotonicity-type assumptions employed for bounding causal parameters are plausible in the context analyzed and potentially weaker than conventional alternatives. Our estimated bounds allow us to undertake inference for a subpopulation of parental-leave-eligible mothers that are always employed regardless of having taken parental leave. This subpopulation accounts for about 80 percent of all eligible mothers in our dataset. Our estimated bounds on average effects of parental leave take-up on hourly wages are consistent with important, albeit statistically insignificant, reductions in all periods analyzed (i.e., 2, 3, 4 and 5 years after birth). Interestingly, we find evidence of heterogeneous impacts of parental leave take-up across the distribution of post-birth wages. Our estimated bounds, in general, show that the quantile treatment effects of parental leave take-up on post-birth wages of always employed mothers are negative and significant in most quantiles above the median.

*We thank seminar participants from LISER and Illinois State University, and conference participants from the 2021 Western Economic Association International. We also thank Timothy Harris and Ann-Zofie Duvander for detailed comments. The authors disclosed receipt of financial support for the research, authorship, and/or publication of this article: FNR CORE 2016 C16/SC/11324101/ PARENT - Evaluation of parental leave in Luxembourg, focus on couples' strategies and the role of workplace characteristics supported by the Luxembourg National Research Fund (FNR).

†Luxembourg Institute of Socio-Economic Research (LISER). Email: michela.bia@liser.lu

‡Illinois State University; GLO and LISER. Email: gblanco@ilstu.edu

§Luxembourg Institute of Socio-Economic Research. Email: marie.valentova@liser.lu

1 Introduction

Job-protected gender-neutral parental leave, where return to a previously held position with the same employer is guaranteed within a predetermined post-birth time window, is a standard feature of labor markets in advanced economies. The main objective of protected parental leave programs is helping parents achieve family-work balance. On the family side of the scale, many studies have documented that parental leave policies improve mothers' health and children's well-being through increased parental time with the infant.[1] On the work side, the intended effects of the leave entitlements and subsequent job protection associated with parental leave policies are to safeguard and enhance the labor market outcomes of parents selecting to participate in the program. Naturally, most studies have focused on analyzing effects of parental leave policy changes (e.g., reform, policy introduction) on the outcomes of employment and earnings for mothers.[2] The task of analyzing changes in employment and earnings, two of the most important labor market outcomes, that are observed after variation in policy is traditional but crucial given parental leave program differences across countries. We, however, depart from traditional analysis in important ways by focusing on estimating the causal effect of parental leave program participation on hourly wages, an understudied but important labor market outcome, for eligible mothers. Our main objective is to shed light on the effects of job-protected parental leave on the human capital, proxied by hourly wages, of eligible mothers that experience a period of labor market intermittency after taking a leave. To this end, we employ high-quality administrative data from Luxembourg to estimate the causal effects of job-protected parental leave take-up on post-birth hourly wages for mothers.

While the economic literature evaluating job-protected parental leave is extensive, important recent studies suggest, explicitly or implicitly, that the economic impact of such policies is not fully understood (e.g., Lalive and Zweimüller, 2009; Schönberg and Ludsteck, 2014; Olivetti and Petrongolo, 2017; Ginja, Jans and Karimi, 2020). To our knowledge, the issue of unintended consequences of mandated parental leave policy on women's careers because of the loss of work experience and depreciation of human capital is an

[1]For example, Hewitt, Strazdins and Martin (2017) find positive impacts of paid parental leave reform in Australia on mothers' health outcomes, and Baker and Milligan (2008) study the impacts of increases in maternity leave entitlements in Canada, finding important effects on breastfeeding duration and smaller impacts on self-reported health for mothers; studies analyzing child welfare effects include Ruhm (2000), who finds positive impacts of parental leave legislation on child's health outcomes using aggregated data for European countries, Ginja, Jans and Karimi (2020), who document positive impacts of parental leave expansions in Sweden on long-term educational outcomes of children, and Pihl and Basso (2019), who find positive infant health impacts of family paid leave in California.

[2]For a comprehensive review of the literature analyzing parental leave policy impacts on labor market and other types of relevant outcomes see Olivetti and Petrongolo (2017), and Nandi et al. (2018).

understudied subject in the literature.[3] The latter claim has well-established theoretical grounds. We note that human capital accumulation has long been recognized as a lifetime process that is not necessarily monotonic, i.e., net investment could be negative (Mincer and Polachek, 1974; Goldin and Polachek, 1987). Beyond the formal education period, human capital accumulation mostly takes place on the job (e.g., on-the-job training). During a period of childbearing and care, however, not only the human capital does not grow but also the prolonged absence from work may cause the human capital previously acquired at school and on the job to depreciate.[4] As a consequence, there is scope for parental leave policies to have a detrimental impact on the human capital of parents that take a longer leave.

The estimation of parental leave effects on total earnings is of extreme importance, however, these estimates could be misleading if one wants to learn about the potential role of human capital changes. Hypothetically, assuming no impact on hourly wages, a positive effect of parental leave participation on total earnings would be attributed to post-birth increases on number of hours worked (the quantity component of earnings).[5] Assessing the human capital effects of labor market intermittency triggered by parental leave take-up requires focusing on the price component of total earnings, i.e., hourly wage.[6] It is common practice in the program evaluation literature to employ the hourly wage rate as a proxy for human capital (e.g., Lee, 2009; Blanco, Flores and Flores-Lagunes, 2013*a*). In line with this literature, we assume that decreases (increases) in human capital decrease (increase) post-birth hourly wages.

Our study focuses on the case of parental leave policy in Luxembourg. We employ high quality administrative data and non-experimental techniques from the program evaluation literature to deal with two main complications in our analysis. Since our focus is on estimating causal effects of parental leave take-up on post-birth hourly wages, a first complication that arises is related to the selection of eligible mothers into taking a leave.

[3]In addition, some authors argue that protected and paid parental leave policies may increase the future cost for employers to hire women of childbearing age, which would also result in eroding women's careers (Ruhm, 1998). While parental leave is gender neutral, note that the focus of the argument above is on women since they traditionally represent the group with high participation rates, and are thus likely more affected by the mandate (Gruber, 1994). The analysis of labor demand response to non-wage cost increases triggered by a parental leave mandate is of interest, however, it is outside the scope of our study.

[4]Mincer and Polachek (1974) incorporate experience profile discontinuities in the human capital earnings function to present a model that allows understanding the effects of labor market intermittency on the wage profiles of women.

[5]Studying parental leave effects on intensive margin is also important and relatively under-researched. For example, Valentova (2019) studies the effect of parental leave policy on the number of post-birth hours worked for a sample of mothers in Luxembourg. Other related studies include Akgunduz and Plantenga (2013), Merz (2004), and Rossin-Slater, Ruhm and Waldfogel (2013).

[6]Alternatively, one could use direct measures of productivity (e.g., Prennushi, Shaw and Ichniowski, 1997; Dearden, Reed and Van Reenen, 2006). Direct productivity measures, however, are not available in the dataset we employ.

To control for this selection we assume that conditional on observed pre-intervention covariates there are no unobserved factors associated to both the assignment to the intervention (parental leave) and the potential outcomes (post-birth hourly wages). This assumption is referred to as unconfounded assignment or selection on observables in the program evaluation literature (e.g., Rosenbaum and Rubin, 1983; Rubin, 1990; ?). While it is widely employed in observational studies, unconfoundedness is a strong assumption whose plausibility relies on the quality of observable predictors for the intervention. Fortunately, we employ social security administrative data from *L'Inspection Générale de la Sécurité Sociale* (IGSS) from 2005 to 2015.[7] This dataset contains detailed information on labor market histories, demographic characteristics, birth dates, parental leave take-up dates, and post-birth working hours, earnings, and hourly wages. Importantly, for the majority of eligible mothers the dataset also contains information on the respective partner or spouse's labor market histories. To our knowledge, the latter are important predictors that are not considered in available studies due to data limitations.

The second complication arises since hourly wages are defined only for individuals who are employed in our post-birth analysis, where employment is simultaneously affected by parental leave take-up. To control for the selection into employment we employ a principal stratification framework (Frangakis and Rubin, 2002) and estimate non-parametric bounds for the effect of parental leave take-up on the post-birth hourly wages of eligible mothers who are observed employed regardless of having taken a leave. Within the program evaluation literature, principal stratification is regarded as a useful framework for analyzing treatment effects when controlling for variables that have been affected by treatment assignment: in our case, the treatment is taking parental leave and an indicator for post-birth employment is the post-treatment variable we need to control for. The principal stratification framework allows computing causal effects by comparing treated and non-treated individuals with the same potential values of the post-treatment variable one wants to control for. This comparison has a causal interpretation since treatment assignment has no effect on membership to these groups, which are known as principal strata. Hence, our focus on eligible mothers who are observed employed regardless of having taken a leave. In addition to assuming an unconfounded treatment assignment mechanism, the estimated bounds assume that treatment (parental leave) has a weak monotonic effect on selection (employment) for every eligible individual. The same type of assumption was employed by Lee (2009) and Blanco, Flores and Flores-Lagunes (2013*a*) to partially identify average wage effects of the US Job Corps, a job training program for disadvantaged youth. To further tighten these bounds, we employ a second assumption, considered by Zhang, Rubin and Mealli (2008) for bounding average treatment effects and by Imai (2008) for bounding quantile treatment effects, that compares

[7] The sample of eligible mothers we analyze gave birth between 2005 to 2010, however, we have access to outcomes measured up to 5 years after birth.

potential outcomes across principal strata.[8] We argue and provide evidence about the plausibility of the assumptions employed in our empirical section.[9]

Our paper contributes to several strands of the literature. First, we complement the literature evaluating parental leave policy effects on labor market outcomes, in particular hourly wages. Studies that have analyzed the wage effects of parental leave are classified into two groups (Olivetti and Petrongolo, 2017). In the first group, studies have exploited cross-country variation to estimate impacts of parental leave reform on aggregated outcomes—this group includes country-level data analyses. For example, Ruhm (1998) studies the impact of parental leave duration on pay, finding larger detrimental wage effects steaming from longer leaves in 9 EU countries, covering the 1969 to 1993 period that brought significant changes in reform. Employing a similar approach, after increasing the number of European countries and time periods analyzed, other studies corroborate the wage reductions effects of parental leave policy (e.g., Thévenon and Solaz, 2013; Akgunduz and Plantenga, 2013; Olivetti and Petrongolo, 2017). Confoundedness and simultaneity issues are well-known caveats for causal interpretation of cross-country and country-level analyses (for a discussion see Ruhm, 1998; Olivetti and Petrongolo, 2017), making the second classification of studies appealing. Our study is best situated within this second group, which is characterized by employing micro-level data and exploiting within-country variation in parental leave policy to identify causal effects of interest. For example, Klerman and Leibowitz (1999) analyze impacts on wage, and other important outcomes, of the US job protected family leave policy, introduced in 1993 under the Family and Medical Leave Act, while Baum and Ruhm (2016) analyze wage effects of the first paid and job-protected US state-level policy introduced in California during 2002. Both studies utilize data from the National Longitudinal Survey of Youth and analyze periods of policy variation. The former study concludes that family leave likely does not impact wages, while the latter finds positive albeit insignificant wage effects one year after birth. Employing social security data from Germany during a period that includes parental leave policy changes, Ejrnæs and Kunze (2013), and Schönberg and Ludsteck (2014) employ difference-in-difference techniques to find negative wage effects of parental leave policy.[10]. In contrast to these studies, the administrative data we employ contains

[8]In the context of an experimental evaluation, Blanco, Flores and Flores-Lagunes (2013*a*,*b*) provide an illustration of trimming bounds, similar to the ones we employ, under the same type of monotonicity assumptions. Other studies within the program evaluation literature employing similar type of assumptions to partially identify parameters of interest include: Imai (2007), Imai (2008), Chen and Flores (2015), and Blanco et al. (2020).

[9]Alternatively, one can employ Heckman type selection models to point identify the causal effect of parental leave take-up on post-birth hourly wages. Such models rely distributional assumptions that are hard to justify in practice. One can also consider an instrumental variable approach (Imbens and Angrist, 1994; Abadie, Angrist and Imbens, 2002), which requires exclusion restriction variables that determine selection into the sample (post-birth employment) but do not affect the outcome (post-birth hourly wages).

[10]Parental leave policy impacts on earnings, but not on the hourly wage rate, in other European

detailed information on actual parental leave eligibility and take-up after birth, which allows us to focus on estimating causal effects of parental leave actual take-up on post-birth hourly wages for an important group of eligible mothers. Misclassification, which is a potential issue in some of the previous studies (e.g., Baum and Ruhm, 2016), is likely not affecting our analysis. Our identification strategy is unique within this literature and allows to control for selection into taking parental leave and post-birth employment. We argue that the unconfoundedness assumption employed to deal with selection into taking parental leave is plausible given the relatively high-quality information about mothers' demographic characteristics, labor market histories (e.g., employment, earnings) and, importantly, labor market histories for the other parent. Many of the studies above explicitly point out that their analysis of hourly wage effects may be biased since it is conditional on employment. Based on our examination of the literature, our study is distinctive in that we explicitly control for self-selection into employment in our analysis of parental leave effects on post-birth wages.

Second, our analysis and estimated results also contribute to a large literature on gender inequality. In particular, two related strands within this literature include studies that analyze the effects of labor market interruptions due to child birth (e.g., Ruhm, 1998; Albrecht et al., 1999; Anderson, Binder and Krause, 2002; Spivey, 2005) and studies focusing on examining the role of family policies in reducing gender gaps (e.g., Lalive and Zweimüller, 2009; Schönberg and Ludsteck, 2014; Olivetti and Petrongolo, 2017; Kleven et al., 2020). We add to the aforementioned studies by shedding light on the potential wage effects of actual parental leave take-up. Our estimated results suggest that the labor market interruption triggered by parental leave take-up has potentially important implications for the gender gap. To our knowledge, the causal parameter that controls for both selection into parental leave take up and employment has not been considered in both these strands of the literature. From a policy calibration standpoint, our parameter estimates are important for better understanding alternative potential mechanisms of parental leave participation that act counter to the main objective of such family policies.

Third, we add to the growing empirical literature implementing nonparametric bounds in settings where the treatment of interest is not randomly assigned. Most of the literature has focused on estimation of bounds in Instrumental Variables settings, under the assumption of instrument validity (e.g., Huber, Laffers and Mellace, 2017; Chen, Flores and Flores-Lagunes, 2018), and in cases where the exclusion restriction assumption is not valid (e.g., Flores and Flores-Lagunes, 2013; Chen, Flores and Flores-Lagunes, 2016; Wang and Flores-Lagunes, 2020). In the absence of instrumental variables, we review and apply recent partial identification results after assuming unconfounded treatment

countries have been estimated using a similar identification strategy (e.g., Lalive and Zweimüller, 2009; Ginja, Jans and Karimi, 2020).

assignment. Our employed bounds are similar in spirit to the trimming bounds for Intention to Treat Effects in Lee (2009), Zhang, Rubin and Mealli (2008), Imai (2008), and Blanco, Flores and Flores-Lagunes (2013a). Similar to these studies, we deal with sample selection into employment by utilizing principal stratification to draw causal comparisons within a stratum whose members employment is not affected by treatment. In contrast, our focus is on bounds for the Average Treatment Effect on the Treated under Unconfoundedness. We argue that future studies that might face similar econometric challenges could benefit from the detailed illustration we provide herein.

Our findings provide new and important evidence that parental leave take-up negatively affects post-birth wages for some eligible mothers that are employed regardless of having taken parental leave. This group of eligible mothers (the always-employed stratum) is an important one, accounting for about 78 to 83 percent of the sample during the different post-birth time periods analyzed. While the estimated bounds on average treatment effects are consistent with negative parental leave impacts on wages, the respective 95% confidence intervals do not rule out zero effects. For example, the wage reductions measured 2 years post-birth can potentially be as large as 14.7 percent and as low as 3.6 percent, however, the upper bound is not statistically distinguishable from zero. Similar magnitudes are found when analyzing wages measured 3, 4, and 5 years after birth.[11] Importantly, we find evidence of heterogeneous impacts across the distribution of post-birth wages, where estimated bounds, in general, show that the quantile treatment effects of parental leave take-up on post-birth wages of always employed mothers are negative and significant in most quantiles above the median. For example, focusing on wages measured 2 years post-birth, estimated bounds on the 50^{th} quantile indicate that the negative effect is between a 12.9 and 9.9 percent reduction, and estimated bounds on the 75^{th} quantile indicate that the negative effect is between a 15.4 and 12.9 percent reduction—a suggestive trend is indicative of potentially larger reductions in higher quantiles of the wage distribution. On the other hand, estimated bounds in the lower half of the wage distribution do not, in general, identify the sign of the effects of interest.

The rest of the paper is organized as follows. Section 2 summarizes key aspects of parental leave policy in Luxembourg. In Section 3 we describe data sources and present summary statistics. Section 4 describes the causal model and assumptions employed in the identification of causal parental leave take-up effects on post-birth wages. Section 5 presents the main results of our analysis and we provide concluding remarks in Section 6.

[11] The outcomes of interest were measured 21, 33, 45, and 57 months after birth. For simplicity, we refer to these outcomes as being approximately measured 2, 3, 4, and 5 years after birth.

2 Institutional Background

A universal scheme for parental leave was introduced in Luxembourg in 1999, 3 years after the European Union (EU) directive on parental leave (Council Directive 96/34/EC). One of its main objectives is to facilitate the reconciliation of family and professional life for both parents. A special attention, however, was paid to the labor market participation of women (Feyereisen, 1998).

In the late 90's, at the time of the introduction of the parental leave policy, Luxembourg exhibited prevailing pro-familialistic characteristics (pro-male-breadwinner), as defined by Leitner (2003) and Sainsbury et al. (1999). The pro-familialistic nature of the Luxembourg family policy in that period was characterized by a set of specific policies such as a joint taxation, cash-for-care arrangements, lack of subsidized statutory child-care, and the absence of parental leave policies or its equivalents. More concretely, Luxembourg had–and still has–a joint taxation system, in which a married couple is treated as one tax unit and the tax rate is based on the average income of the partners. The larger the discrepancy between the spouses' incomes, the higher the tax relief. This tax arrangement discourages women from participating in the labor market and favours the male-breadwinner model for the household division of labor. Another typical pro-familialistic policy is the so-called child raising allowance (L'allocation d'Education). Introduced in 1989, this cash-for-care type of policy provided a flat-rate benefit to parents spending time at home with a child under the age of two. The benefit was equal to approximately one third of the minimum social salary of a non-qualified worker. Under this policy, returning to a previous job was not guaranteed by law. With respect to statutory formal child-care policies, during 2004, Luxembourg's participation in formal care for children under three years of age was about 14 percent, which represented the fourth lowest enrolment rate in the EU-15 countries (OECD, 2007). The cost of childcare in Luxembourg was one of the highest in all the OECD countries and the highest in the EU. During 2004, the cost of attending early years of care or education services represented 32 percent of the average salary of an average worker in Luxembourg, while the OECD's average was 12.1 percent.

The outlined pro-familialistic welfare legacy was related to the high level of female labor market inactivity due to family responsibilities. Eurostat data shows that in 2004 more than 30 percent of the female labor force still belonged to this category(Hardarson, 2006). OECD (2004) figures show that in 1998 men worked full time and women were not employed in about 49 percent of households, with only a 12 percent of these households preferring this arrangement. This discrepancy played a role in motivating supply side changes. In particular, part-time work played an important role in female labor market engagement and is one important way in which women reconcile work and family life. In the late 1990s and the beginning of 2000, one in four employed women in Luxembourg worked part time (Blond-Hanten, Lejealle and Robert, 2008). More than half of the female part-time workers stated that they limited their labor-market engagement due to the responsibilities of caring for significant others. At the beginning of 2000, part-time arrangement work was, aside from very limited, not providing subsidised childcare services.

Luxembourg's family policy landscape has, however, undergone substantial changes since the early 2000's. The introduction of the parental leave in 1999 was followed by the introduction of the formal childcare statutory subsidies in 2009. These subsidies ware based on the State provided financial support (Le Cheque-Service Accueil) for parents to benefit from childcare provision for children aged 0-12. The support is universal and means tested. Up to 2009, no financial compensation for the high cost of formal childcare was provided to parents. In June 2015, the child raising allowance was abolished. Shortly after, in December 2016, the parental leave policy underwent a major reform, which enlarged the group of eligible parents, increased the flexibility of the leave by offering a wide range of leave types in terms of duration, and made the monthly leave benefit to be income-related with a relatively high ceiling of approximately 3500 Euro. The latter policy reforms coincide with substantial changes in the labor market, mainly in term of female labor market engagement. The beginning of 2000 was characterized by high growth in female employment in Luxembourg. The growth rate in 2000 was 6 percent, compared with an average of 2 percent in the other EU-15 countries. This made Luxembourg the fastest changing country in the EU-15 zone with regard to female engagement in paid labor (Eurostat, 2007).

Parental leave policy parameters

Parental leave policy as it existed in Luxembourg from its introduction in 1999 until a substantive reform in December 2016 was fully job protected, meaning that eligible parents are guaranteed the right to subsequently return to the same or an equivalent work position. The period under analysis in this paper co-insides with the latter period, where there were no changes in parental leave policy. The most significant eligibility requirement for the parental leave policy under analysis is a minimum of one year's employment with the same employer prior to the start of the leave. Leave eligibility also depends on the duration of the contract. The end-date of the requested parental leave cannot exceed the duration of the legal employment contract. Self-employed workers are also eligible if they have been self-employed for at least one year and for at least 20 hours per week. Eligible parents can take either a period of six months full-time leave or twelve months part-time leave. The leave can be taken up to the fifth birthday of the child. The right to take leave is not transferable between parents. For eligible parents who live in couple, there is a requirement that one of the parents has to take the leave immediately after maternity leave or adoption of a child (the so-called first parental leave). The second parent may then take the leave (the so-called second leave) at anytime until the child reaches the age of five. If one parent does not take leave immediately after maternity leave, the right to it is forfeited. Single parents are entitled to only one period of parental leave; however, they do not necessarily have to take it immediately after maternity leave. Parental leave is paid, and the flat-rate benefit up to 2006 was equal to 1,496 euros for the full-time leave. In 2007, the benefit increased to 1,778 euros, which is approximately equivalent to the minimum wage for an unqualified worker.

Regarding the usage of parental leave policy between 1999 and 2015, the official statistics from the Ministry of Family, Integration and the Great Region (2016) show that the total number of users of parental leave has increased markedly over the observed period.[12] For instance, the number of beneficiaries almost tripled among mothers (from 1343 to 3489) between 1999 and 2015. Throughout this period, women were more likely than men to be using the first leave, i.e., immediately after the maternity leave, while fathers were more likely to be using the second leave. Among women, part-time modality of leave has been less used than full-time leave. About 25.5 per cent of female beneficiaries took part-time leave in 1999 and 37.3 in 2015). Among men, in 1999, 34.4 per cent of male beneficiaries took part-time leave, whereas in 2015 it was 65 per cent.

3 Data

We employ high quality administrative data recorded by Luxembourg's General Inspectorate of Social Security (Inspection Genèrale de la Sècuritè Sociale, IGSS). In particular, we have access to information on first-time mothers that meet the eligibility criteria, which legally entitles them to benefit from parental leave policy, and gave birth at any point in time during years 2005 to 2010.[13] Eligibility was measured by a newly created proxy variable that combines key conditions of eligibility stipulated by law (e.g., working full-time for at least one year with the same employer prior birth). Our sample only includes Luxembourg residents. Non-resident workers represent about half of the Luxembourg labor force and are also eligible for parental leave. However, they are not included in our analysis, due to lack of information about their spouses and incomplete records. For every eligible mother in this data set, there is detailed information on labor market his-

[12]The participation rate amongst men has increased by twelve-fold, from 90 in year 1999 to 1106 in 2015. The increased participation of men in parental leave is also evident in the increasing share of male users among total number of leave users, from 6.3 percent at the end of 1999 to 31.1 percent in 2015. For the period we analyze in this paper, births that happened from 2005 to 2010, the relative participation rate for men is low, so the small sample size precludes us from drawing precise inference for men with the tools employed.

[13]We also have access to information on parental leave eligible fathers, however, their sample size is relatively small. Their small sample size would prevent us from employing the same tool for analysing parental leave effects on wages controlling for selection into treatment and employment.

tories dating back to 6 months before birth, demographic characteristics, parental leave take-up date, and labor market outcomes recorded approximately 2, 3, 4, and 5 years after birth—some of these variables have been obtained after matching records from the National Health Fund (Caisse Nationale de Santé).

We restrict our analysis to eligible first-time mothers that give birth only to that first child during the time period analyzed (2005 to 2015). The restriction to eligible first-time mothers is pragmatic. In case of mothers with multiple births, the IGSS data prior 2016 does not allow to link the information on parental leave take-up with a particular child. In addition, the analysis of mothers with multiple births during the time period analyzed would be complicated since parental leave take-up is potentially associated with fertility decisions (e.g., Lalive and Zweimüller, 2009). The latter restriction results in dropping a total of about 32 percent of observations, so the remaining sample has 2933 eligible mothers. We also restrict the sample to eligible first-time mothers that reported being employed either full or part-time. That is, our sample does not include self-employed eligible first-time mothers. In excluding self-employed, we follow conventional practices in the literature (e.g., Mari and Cutuli, 2020). Furthermore, self-employed individuals in the raw data set had missing and/or incomparable information on several of the key variables. The latter restriction resulted in dropping an additional 141 observations. Finally, we restrict our sample by focusing on observations that have labor market histories for the newborn's father, resulting in a final sample of 1818 observations.[14] The latter are considered important predictors of both participation in taking parental leave and labor market outcomes for mothers—to our knowledge, these type of predictors are not considered in studies due to data limitations, and are key for justifying assumptions that are widely employed.

To characterize the population of interest, we present selected summary statistics in Table 1 for the main sample of eligible first-time mothers that toke parental leave, restricted to those that gave birth from 2005 to 2010 and only had that one child during 2005 to 2015, are Luxembourg residents, are not self-employed pre-birth, and have labor market information on the respective spouse/partner.[15] With regards to demographic characteristics, The eligible mothers of interest have on average about 31 years of age, likely living with a partner, close to 60 percent married, with about 60 percent reporting being nationals from Luxembourg, followed by Portugal with 16 percent and France with about 10 percent. Average labor market experiences suggests that the eligible mothers of interest likely work in the private sector (87 percent), have more than 6 years of work

[14]Our main conclusions remain unchanged when including eligible mothers of interest with no information on the respective spouse/partner. These results are available from the authors upon request.

[15]For brevity, the full set of pre-birth variables that we employ in our main analysis are reported in the Appendix, Tables A1 and A2. In addition, in Tables A1 and A2 we contrast information across eligible mothers that took parental leave versus those that did not take parental leave.

experience (more than 75 percent). The eligible mothers of interest likely work at firms with more than 50 employees (about 70 percent), with higher proportions of whites, males and young employees. Pre-birth average labor market outcomes, measured in the last 6 months, for the eligible mothers of interest are consistent with the average levels observed in Luxembourg. On average, the eligible mothers of interest worked continuously for 6 months, for about 167.19 hours in each month (about 40 hours per week), with an hourly wage of 20.92 Euros. Finally, at the bottom of Table 1, we report the pre-birth average labor market outcomes for the spouse/partner of the eligible mothers in our data. We note that the spouse/partner works continuously for the past 6 months, with relatively more hours per month (171.82) and a higher average wage (24.66 Euros). The sample summarized in Table 1 includes eligible mothers that are recorded, prior birth, to be employed in either a full-time or part-time job. In Table 2 we report pre-birth summary statistics for a more homogeneous sample of only full-time employed eligible mothers. As shown in Table 2, values do not differ much relative to those in Table 1. This is not surprising, given that most of the sample is comprised of pre-birth full-time employed eligible mothers.

4 Methodology

4.1 Potential Outcomes and Unconfounded Treatment Assignment

We employ the potential outcomes framework (Rubin, 1974) to define the causal effect of interest. To aid exposition, the framework's standard notation is applied within the context of parental leave take-up and its effect on post-birth hourly wages. Consider a random sample of size n from a large population. Let $T_i = t \in \{0, 1\}$ indicate whether unit i is assigned to the treatment ($T_i = 1$) or to the control group ($T_i = 0$). Let Y_i be the observed outcome of interest for unit i. To introduce and illustrate the first two assumptions we employ in our analysis, let's assume that the outcome Y_i is always defined and observed.[16] Then, let $Y_i(1)$ denote the potential outcome that would have resulted if unit i had been assigned to the treatment ($T_i = 1$), and $Y_i(0)$ denote the potential outcome that would have resulted if unit i had been assigned to the control group that receives no treatment ($T_i = 0$).[17] In our application, for any eligible mother i, T_i denotes whether parental leave is taken ($T_i = 1$) or not ($T_i = 0$), $Y_i(1)$ and $Y_i(0)$ denote the potential outcomes of post-birth hourly wage, when parental leave is taken

[16] In the next subsection, we consider the case in which Y_i may or may not be defined. The latter is relevant in our application since the outcome of post-birth hourly wages is only defined for those that are employed post birth.

[17] Note that by employing this formulation, we are implicitly assuming the Stable Unit Treatment Value Assumption (Rubin, 1980), which rules out interference across units and hidden values of the treatment.

($T_i = 1$) or not ($T_i = 0$), and the observed post-birth hourly wage is represented by Y_i. Then, $Y_i = Y_i(1)T_i + Y_i(0)(1 - T_i)$ depicts the relationship between observed and potential outcomes.

Under the potential outcomes framework, causal effects are based on comparing $Y_i(1)$ and $Y_i(0)$. For example, a causal effect of parental leave take up for unit i is $Y_i(1) - Y_i(0)$. However, since only one of these potential outcomes is observed, as it can be seen from the relationship between observed and potential outcomes, a causal parameter of interest is the average treatment effect on the treated (ATT), defined as: $ATT = E[Y_i(1) - Y_i(0)|T_i = 1]$.[18] Estimation of the ATT parameter is straightforward if treatment, T_i, is randomly assigned. With randomization, the potential outcomes are independent of treatment assignment, and thus, a simple comparison of average outcomes across treatment and control groups would yield an unbiased estimate for the ATT parameter.

In our application, treatment assignment is not random, which creates a first identification issue. To address this problem, we assume that treatment assignment is strongly ignorable (Rosenbaum and Rubin, 1983) for the controls.[19] Formally, we employ the following:

Assumption 1. *Unconfoundedness for Controls:* $Y_i(0) \perp T_i|X_i$.

Assumption 2. *Weak Overlap:* $P(T_i = 1|X_i = x) < 1$, for all x in the support of the treated distribution.

Assumption 1 states that treatment (T_i) is independent of the potential outcomes for controls ($Y_i(0)$) conditional on a set of observed pre-treatment covariates X_i.[20] Assumption 2 states that, after conditioning on X_i, the probability of treatment is bounded away from one. In practice, this assumption requires that there are control group individuals with the same values of X_i as those observed in treatment group.

Unconfoundedness is widely employed in applied work, however, it is strong since it rules out, after conditioning on X_i, unobserved confounders that may be related to the potential outcomes and the probability of receiving the treatment. Using data from the experimental evaluation of the U.S. National Job Training Partnership Act, Heckman, Ichimura and Todd (1997) show that data quality is crucial to any reliable estimation strategy. Specifically, for analyzing non-experimental data Heckman, Ichimura and Todd

[18]An alternative causal parameter would be the average treatment effect $ATE = E[Y_i(1) - Y_i(0)]$. In our particular application, the ATE parameter is less interesting than the ATT parameter, since the latter focuses on the subpopulation of treated units, i.e., eligible mothers that take parental leave, while the ATE focus is on the entire population.

[19]'Strong Ignorability' as introduced by (Rosenbaum and Rubin, 1983) applies to both potential outcomes under treatment and control, and thus it is a stronger assumption than the one we employ here.

[20]The Unconfoundedness assumption will be extended in Sub-section 4.3 to incorporate an additional potential outcome, which will be needed to deal with a second identification issue related to selection into employment.

(1997) recommend that treatment and control individuals reside in the same local labor markets, information comes from the same source, and that data contains a rich set of variables relevant to modeling the program participation. In our application, we strongly believe in the plausibility of unconfoundedness since we have access to rich administrative data, collected by the same government agency in Luxembourg, for the group of eligible mothers that took parental leave and the group of eligible mothers that did not. Importantly, in addition to having access to demographic characteristics and labor market histories of eligible mothers, we employ information on labor market histories of husbands.

In the absence of additional identification issues, under strong ignorability for controls (Assumptions 1 and 2), the average treatment effect on the treated is identified as $ATT = E[Y_i|T_i = 1] - E(E[Y_i|X_i = x, T_i = 0]|T_i = 1)$, with the outer expectation is over the distribution of $X_i|T_i = 1$. Under strong ignorability, it is well-known that the ATT can be estimated if X_i is low dimensional or by employing data preprocessing techniques to achieve some extent of balance in pre-treatment covariates X_i across treatment arms. The most used data preprocessing technique in empirical work is based on the propensity score introduced by Rosenbaum and Rubin (1983). While the propensity score has attractive theoretical features (e.g., Hirano, Imbens and Ridder, 2003), its misspecification can lead to large bias in estimating treatment effects (e.g., Smith and Todd, 2001). To deal with selection into treatment, in our empirical application we employ entropy balancing, introduced by (Hainmueller, 2012), to preprocess the data and achieve balance of observed pre-treatment covariates across treatment arms. Entropy balancing relies on a reweighting procedure that calibrates unit weights, so that the reweighted treatment and control units satisfy a set of balance conditions. This methodology incorporates information about known sample moments. In particular, it adjusts inequalities in the first, second, and potentially higher moments of the pre-treatment covariate distributions. Unlike the propensity score, the application of entropy weights will reduce model dependency for the estimation of treatment effects as well as it will lead to balance improvements on all covariate moments included in the reweighting procedure.

In addition to dealing with the selection into the treatment of parental leave take-up, the analysis of impacts on post-birth hourly wages requires an estimator that controls for sample selection into employment. A detailed discussion of the approach we follow for estimating causal effects of interest is presented below.

4.2 Principal Stratification

Sample selection bias becomes an issue when the outcome of interest is affected by a secondary outcome, with both outcomes potentially impacted by treatment. Consider an application in which Y_i is “truncated” based on values of a secondary outcome. With this

Table 3: Principal Strata within Observed Cells Defined by T_i (Parental Leave) and S_i (Post-Birth Employment).

		T_i	
		0	1
S_i	0	NN & NE	NN & EN
	1	EE & EN	EE & NE

in mind, let $S_i = s \in \{0, 1\}$ be the truncation indicator for unit i, such that Y_i is defined if $S_i = 1$ and not defined if $S_i = 0$. In the context of our application, the post-birth hourly wage Y_i is defined only if eligible mother i is employed ($S_i = 1$), otherwise it will be truncated due to post-birth lack of employment ($S_i = 0$). To complicate matters, both post-birth hourly wages and employment are potentially affected by parental leave take-up. Before formalizing our treatment for the sample selection problem, we first introduce the following potential outcomes. Let the potential employment indicator values for mother i be denoted by $S_i(1)$ when parental leave is taken ($T_i = 1$), and $S_i(0)$ when parental leave is not taken ($T_i = 0$).

To deal with the sample selection problem in our analysis of the post-birth hourly wage effects of parental leave take-up, we employ principal stratification (Frangakis and Rubin, 2002), which is a useful framework for analyzing causal effects when a post-treatment variable(s) needs to be controlled for, as in our context, where we need to control for post-birth employment. The framework's key insight is that outcome comparisons between treated and controls within principal stratum, which is a group whose members have the same potential value of the post-treatment variable(s) one wants to adjust for, have a causal interpretation, implying that membership to a particular principal stratum is not affected by treatment assignment.

In our context, we employ principal stratification to define subpopulations based on values for the potential employment $\{S_i(0), S_i(1)\}$. Within cells defined by the observed values of the indicators for parental leave take-up (T_i) and post-birth employment (S_i), in Table 3 we show how the population is partitioned into four principal strata, which we label: *always-employed*, $EE = \{i : S_i(0), S_i(1) = (1, 1)\}$; *employed if treated*, $NE = \{i : S_i(0), S_i(1) = (0, 1)\}$; *never-employed*, $NN = \{i : S_i(0), S_i(1) = (0, 0)\}$; and *employed if not treated*, $EN = \{i : S_i(0), S_i(1) = (1, 0)\}$. From Table 3, its clear that the EE stratum of *always-employed* is the only one with defined post-birth hourly wages that are observed under both treatment arms, i.e., taking parental leave and not taking parental leave. To maintain the number and strength of assumptions at a minimum, we then focus on the average treatment effect on the treated for the EE stratum, defined as:

$$ATT_{EE} = E[Y_i(1)|EE, T_i = 1] - E[Y_i(0)|EE, T_i = 1], \quad (1)$$

Stratum specific parameters are commonly employed in the program evaluation literature. The most familiar example is the local average treatment effect as in Imbens and Angrist (1994), where an average treatment effect is point identify for the stratum of individuals that comply with the randomized treatment assignment (i.e., the average treatment effect for compliers). Also within the context of the experimental evaluation for a job training program wage effects, Zhang, Rubin and Mealli (2008), Lee (2009), and Blanco, Flores and Flores-Lagunes (2013*a*, BFF-L hereafter) have partially identify the ATE parameter for the stratum of always employed regardless of randomized treatment assignment (i.e., the ATE_{EE}, borrowing from the terminology employed by BFF-L). We follow an strategy similar in spirit to that of the latter set of papers—note that we are focusing on the stratum of *always-employed* eligible mothers. Our causal parameter of interest, however, is the average treatment effect for the treated (ATT_{EE}), which is a different but more interesting parameter for our context (see footnote 18). Unfortunately, as noted in papers mentioned above, identification of which members belong to a particular stratum is not generally possible, i.e., principal strata are latent subpopulations. Partial identification of the ATT_{EE} in Equation (1), however, is possible under assumptions we consider appropriate for the context we analyze.[21]

4.3 Bounds on Average Treatment Effects

In contrast to well-known point identification techniques, for example, selection models that employ distributional assumptions (Heckman, 1979) or estimators that rely on the availability and validity of exclusion restrictions (Imbens and Angrist, 1994; Abadie, Angrist and Imbens, 2002), we use an alternative approach that constructs bounds on the parameter of interest when sample selection is an issue. Horowitz and Manski (2000) proposed a general framework to construct bounds on treatment effects when data are missing due to a nonrandom process, such as self-selection into employment. These bounds are nonparametric and allow for heterogeneous effects, which departs from the usually employed assumption of constant effects over the population. These bounds only require randomization of treatment assignment and that the outcome has a bounded support. As discussed in Section 4.1, in our application the treatment is not randomly assigned, which is why we rely on the treatment assignment being strongly ignorable (Assumptions 1 and 2). At this point, Assumption 1, as presented in Section 4.1, needs to be extended to incorporate the value of potential employment, i.e., Unconfoundedness: $\{Y_i(0), S_i(0)\} \perp T_i|X_i$. Even under the modified strong ignorability, a caveat of the Horowitz and Manski (2000) bounds is that in practice they are wide and uninformative about the sign of the effects of interest. Nevertheless, we use these bounds as a building

[21] An alternative, which we don't follow, to deal with principal strata membership is to employ an assumption analogous to selection on observables. See Flores and Flores-Lagunes (2009) for an application of an unconfoundedness principal strata assumption.

block and proceed by imposing more structure through principal stratification and the adoption of two additional assumptions.

Assumption 3. *Individual-Level Weak Monotonicity of S in T:* $S_i(1) \geq S_i(0)$ for all i.

This assumption states that treatment assignment weakly affects selection in one direction, effectively ruling out membership in the stratum of *employed if not treated*, $EN = \{i : S_i(0), S_i(1) = (1, 0)\}$.[22] In our application, this assumption implies that the effect of parental leave take-up on post-birth employment is non-negative for every eligible mother. Certainly, the assumption is consistent with the stated objectives of job-protected parental leave policy, which guarantees returning to the same employer one had before childbirth. Different factors, however, may cast a doubt on the plausibility of Assumption 3. For example, the time window analyzed could correspond to a period in which the utility of staying at home with the newborn is relatively high. We believe, however, that the relatively long periods at which post-birth hourly wage are measured lessens the plausibility of this potential threat. While Assumption 3 is fundamentally untestable, a testable implication is that the average effect of parental leave take-up on post-birth employment is non-negative, $E[S(1) - S(0)] \geq 0$, which is point identified under Assumptions 1 and 2. This average effect is positive and statistically significant as shown in the results section.

Assumptions 1 to 3 allow the point identification of the term $E[Y_i(0)|EE, T_i = 1]$ in (1) as $E(E[Y_i|T_i = 0, S_i = 1, X_i = x]|T_i = 1)$, with the outer expectation is over the distribution of $X_i|T_i = 1$. Note that this conditional expectation corresponds to comparable non-treated individuals in the EE stratum that have defined and observed post-birth wages, i.e., eligible mothers that do not take parental leave and are always-employed regardless of parental leave take-up status. This is clearly illustrated in Table 3 (lower left corner) by simply removing the EN stratum from the cell defined by $(T_i, S_i) = (0, 1)$. However, it is not possible to point identify the treatment counterfactual $E[Y_i(1)|EE, T_i = 1]$ in (1), since the observed group in the cell $(T_i, S_i) = (1, 1)$ has a mixture of individuals from two strata, i.e., the *always employed* (EE) of interest and the *employed if treated* (NE). Nevertheless, $E[Y_i(1)|EE, T_i = 1]$ can be bounded. Under Assumptions 1 to 3, the proportion of EE individuals in $(T_i, S_i) = (1, 1)$ can be point identified as $(p_{1|0,x}/p_{1|1,x})$, where $p_{s|t,x} \equiv Pr(S_i = s|T_i = t, X_i = x)$ for $t, s = 0, 1$.[23] Therefore, $E[Y_i(1)|EE, T_i = 1]$ can be bounded from above (below) by the expected value of Y_i for the $(p_{1|0,x}/p_{1|1,x})$ fraction of the largest (smallest) values of Y_i in the observed group $(T_i, S_i) = (1, 1)$. In

[22]Lee (2009), Zhang, Rubin and Mealli (2008), and BFF-L employed this assumption, and similar assumptions are widely used in the instrumental variable (Imbens and Angrist, 1994) and partial identification literature (Manski and Pepper, 2009; Flores, Flores-Lagunes et al., 2010; Blanco et al., 2020).

[23]See BFF-L for details on the calculation of strata proportions, under randomization of treatment assignment and individual level monotonicity of employment in the treatment. The difference here is the conditioning on $X_i = x$.

other words, the upper bound for the treatment counterfactual is obtained under the scenario that the largest $(p_{1|0,x}/p_{1|1,x})$ fraction of values of Y_i in $(T_i, S_i) = (1, 1)$ belongs to individuals from the EE stratum. Analogously, a lower bound for $E[Y_i(1)|EE, T_i = 1]$ is obtained under the scenario where Y_i for individuals in the EE stratum are in the lowest $(p_{1|0,x}/p_{1|1,x})$ portion of the observed distribution in $(T_i, S_i) = (1, 1)$. These bounds on the term $E[Y_i(1)|EE, T = 1]$ and the point identified term $E[Y_i(0)|EE, T_i = 1]$ are used to bound ATT_{EE} in (1). Formally, under Assumptions 1 to 3, the resulting upper (UB_{EE}) and lower (LB_{EE}) bounds for ATT_{EE} are:

$$
\begin{aligned}
UB_{EE} &= E[Y_i|T_i = 1, S_i = 1, Y_i \geq y^{11}_{1-(p_{1|0,x}/p_{1|1,x})}]- \\
&\quad E(E[Y_i|T_i = 0, S_i = 1, X = x]|T = 1) \\
LB_{EE} &= E[Y_i|T_i = 1, S_i = 1, X_i = x, Y_i \leq y^{11}_{(p_{1|0,x}/p_{1|1,x})}]- \\
&\quad E(E[Y_i|T_i = 0, S_i = 1, X = x]|T = 1),
\end{aligned} \tag{2}
$$

where $y^{11}_{1-(p_{1|0,x}/p_{1|1,x})}$ and $y^{11}_{(p_{1|0,x}/p_{1|1,x})}$ respectively denote the $1 - (p_{1|0,x}/p_{1|1,x})$ and the $(p_{1|0,x}/p_{1|1,x})$ quantiles of Y_i conditional on $T_i = 1$, and $S_i = 1$, and the outer expectation is over the distribution of $X_i|T_i = 1$.

In addition, we consider the following assumption.

Assumption 4. *Weak Monotonicity of Mean Potential Outcomes Across the Treated EE and NE Strata:* $E[Y(1)|EE, T_i = 1] \geq E[Y(1)|NE, T_i = 1]$.

Intuitively, this assumption formalizes the notion that the treated EE stratum is likely to be comprised of more "able" individuals than those belonging to the NE stratum. Since "ability" is positively correlated with labor market outcomes, one would expect the post-birth hourly wage of mothers who are always-employed regardless of taking the parental leave treatment (the EE stratum in $T_i = 1$) to weakly dominate on average the post-birth hourly wage of mothers who are employed only if they take the parental leave treatment (the NE stratum in $T_i = 1$). Adding Assumption 4 implies $E[Y_i|T_i = 1, S_i = 1] \leq E[Y_i(1)|EE, T_i = 1]$, that is, the tighter lower bound for the counterfactual $E[Y_i(1)|EE, T_i = 1]$ is $E[Y_i|T_i = 1, S_i = 1]$. Thus, under Assumptions 1 to 4, the upper bound (UB_{EE}) for ATT_{EE} is the same as in (2) and the tighter lower bound becomes: $E[Y_i|T_i = 1, S_i = 1] - E(E[Y_i|T_i = 0, S_i = 1, X_i = x]|T_i = 1)$.

4.4 Bounds on Quantile Treatment Effects

Imai (2008) proposed bounds on quantile treatment effects that are analogous to the bounds on average treatment effects under the assumptions of randomized treatment and individual-level monotonicity of the selection variable on randomized treatment. Following Imai (2008), we employ bounds on quantile treatment effects defined as differences in the quantiles of the marginal distributions of the potential hourly wages for mothers in the EE stratum. More specifically, define the α-quantile effect for the EE stratum as:

$$QTE_{EE}^{\alpha} = F^{-1}_{Y_i(1)|EE,T_i=1}(\alpha) - F^{-1}_{Y_i(0)|EE,T_i=1}(\alpha), \tag{3}$$

where $F^{-1}_{Y_i(t)|EE,T_i=1}(\alpha)$ denotes the α-quantile of the distribution of $Y_i(t)$ for the treated EE stratum.

Similar to the ATT_{EE} case, under Assumptions 1 to 3, the first term in (3) is partially identified by trimming the conditional cumulative distribution function (CDF) of Y_i in cell $(T_i, S_i) = (1, 1)$ based on the proportion $(p_{1|0,x}/p_{1|1,x})$, while the control counterfactual is point identified from the conditional CDF of mothers' post-birth wages in cell $(T_i, S_i) = (0, 1)$. In the case of the control counterfactual, however, we don't employ the quantile function directly since its non-linearity prevents from applying iterated expectations. Instead, we employ the mean an indicator function $1(Y_i \leq \tilde{y})$, and M different values of $\tilde{y}$ spanning the support of the observed outcome, to identify the conditional distribution of Y_i at a given point $\tilde{y}$. In particular, using the different values of $\tilde{y}$ we point identify the conditional CDF $F_{Y_i(0)|EE,T_i=1}(\tilde{y}) = E[E[1(Y_i \leq \tilde{y})|T_i = 0, S_i = 1, X_i = x]|T_i = 1]$, where the outer expectations is over the distribution of $X_i|T_i = 1$. The latter CDF is then inverted in order to identify the α-quantile in the last term of (3). Letting $F^{-1}_{Y_i|T_i=0,S_i=1,X_i=x}(\alpha)$ denote the α-quantile after inverting the identified distribution $F_{Y_i(0)|EE,T_i=1}(\tilde{y})$, under Assumptions 1 to 3, we partially identify QTE_{EE}^{α} as $LB_{EE}^{\alpha} \leq QTE_{EE}^{\alpha} \leq UB_{EE}^{\alpha}$, with

$$\begin{aligned} UB_{EE}^{\alpha} &= F^{-1}_{Y_i|T_i=1,S_i=1,Y_i \geq y^{11}_{1-(p_{1|0,x}/p_{1|1,x})}}(\alpha) - F^{-1}_{Y_i|T_i=0,S_i=1,X_i=x}(\alpha) \\ LB_{EE}^{\alpha} &= F^{-1}_{Y_i|T_i=1,S_i=1,Y_i \leq y^{11}_{(p_{1|0,x}/p_{1|1,x})}}(\alpha) - F^{-1}_{Y_i|T_i=0,S_i=1,X_i=x}(\alpha), \end{aligned} \tag{4}$$

where $F^{-1}_{Y_i|T_i=1,S_i=1,Y_i \geq y^{11}_{1-(p_{1|0,x}/p_{1|1,x})}}(\alpha)$ and $F^{-1}_{Y_i|T_i=1,S_i=1,Y_i \leq y^{11}_{(p_{1|0,x}/p_{1|1,x})}}(\alpha)$ correspond to the α-quantile of Y_i after trimming, respectively, the lower and upper tail of the conditional distribution of Y_i in cell $(T_i, S_i) = (1, 1)$ by $1 - (p_{1|0,x}/p_{1|1,x})$, and thus they provide an upper and lower bound for the counterfactual $F^{-1}_{Y_i(1)|EE,T_i=1}(\alpha)$.

The trimming bounds for QTE_{EE} in (4) can be tightened by employing an assumption analogous to Assumption 4. Let $F_{Y_i(1)|EE,T_i=1}(\cdot)$ and $F_{Y_i(1)|NE,T_i=1}(\cdot)$ denote the CDFs of $Y_i(1)$ for treated individuals who belong to the EE and NE strata, respectively. We employ

Assumption 5. *Stochastic Dominance*: $F_{Y_i(1)|EE,T_i=1}(y) \leq F_{Y_i(1)|NE,T_i=1}(y)$, for all y.

This assumption directly imposes restrictions on the distribution of potential outcomes under treatment for individuals in the EE stratum, which results in a tighter lower bound. Adding Assumption 5 results in sharp bounds (Imai, 2008), where the lower bound is now the untrimmed difference: $F^{-1}_{Y_i|T_i=1,S_i=1}(\alpha) - F^{-1}_{Y_i|T_i=0,S_i=1,X_i=x}(\alpha)$.

4.5 Estimation

Estimation of the bounds on the ATT_{EE} and QTE_{EE} rely on simple sample analogs. That is, conditional proportions are used to determine the extent of trimming to calculate conditional means. The outer expectations for the control counterfactuals are calculated by employing inverse probability weighting based on the entropy balancing weights (see Hainmueller, 2012). As previously explained, in the case of estimating QTE_{EE} we rely on indicator functions rather than the quantile function in order to be able to employ iterated expectations to average out $X|T_i = 1$ via entropy weights. Finally, we perform statistical inference using the Imbens and Manski (2004, IM hereafter) confidence intervals. These confidence intervals include the true value of the parameter of interest with a given probability. We employ bootstrapped standard errors in order to construct 95% IM confidence intervals.

5 Results

5.1 Estimated Bounds for the Average Treatment Effects of Parental Leave Take Up on Wages

The main estimates for analyzing average effects of parental leave are presented in Table 4. The focus is on eligible first-time mothers that are always employed, regardless of having taken parental leave, during 2, 3, 4, and 5 years post-birth. In addition to the main sample we analyze, which considers eligible first-time mothers employed at baseline in either full or part-time jobs, we analyze a more homogeneous sample of baseline full-time employed eligible first-time mothers. We first focus on discussing results for the main sample. As shown in the top three rows of the Table 4, the *always-employed* account for about 78 to 83 percent of the entire population of mothers. The other estimated principal strata proportions suggest that the *never-employed* are the second largest group accounting for about 13 to 15 percent, followed by those who are *employed if treated* with a small but significant proportion that ranges from about 5 to 7 percent. In the last six rows of Table 4 we report the estimated bounds within brackets and their respective 95% and 90% Imbens and Manski (2004; IM hereafter) confidence intervals, which are reported within parentheses. Focusing on outcomes measured 2 years post-birth, under Assumptions 1 to 3, the estimated lower bound is indicative of a wage reduction of 14.7 percent due to parental leave take up. While the estimated upper bound is also consistent with a wage reduction of about 3.6 percent, the respective 95 and 90% IM confidence intervals do not rule out zero effects of parental leave take up on wages. A similar qualitative conclusion is reached when analyzing outcomes measured 3, 4 and 5 years post-birth, that is, while there is potential for economically significant reductions on wages due to parental leave participation, one cannot rule out insignificant impacts based on confidence intervals.

While individual-level weak monotonicity of the effect of parental leave take up on post-birth employment (Assumption 3) is not testable, the literature employing similar assumptions suggests employing the following testable implication (e.g., Imai (2008)). Under Assumptions 1 and 2, Assumption 3 implies that $E(S_i|T_i = 1, X_i = x) - E(S_i|T_i = 0, X_i = x) \geq 0$. Note that $E(S_i|T_i = 1, X_i = x) - E(S_i|T_i = 0, X_i = x) = \pi_{NE}$, which is the proportion of *employed if treated* reported in the third row of Table 4. The estimated proportion, π_{NE}, is positive and statistically significant at conventional levels in all post-birth years analyzed.

Under Assumptions 1 to 3, the lower bound places all *always-employed* mothers (i.e., EE stratum) at the bottom of the observed conditional wage distribution in $(T_i, S_i) = (1, 1)$. Therefore, the lower bound implies a perfect negative correlation between post-birth employment and wages, which is implausible from the standpoint of standard models of labor supply. Adding Assumption 4 formalizes this theoretical notion to tighten the lower bound. Estimated lower bounds under Assumptions 1 to 4 are presented in the bottom panel of Table 4. Focusing on the analysis of outcomes measured 2 years post-birth, adding Assumption 4 reduces the estimated lower bound by about 40 percent, suggesting a wage reduction of 8.7 percent due to parental leave participation. Estimated lower bounds when analyzing outcomes measured at the other post-birth time horizons considered are similar in magnitudes. As discussed in the previous section, the upper bound remains the same after introducing Assumption 4. While the qualitative conclusions based on the estimated upper bounds' do not change after adding Assumption 4, the estimated lower bounds and their respective confidence intervals now rule out the potential for negative wage effects that are larger in magnitude than a 10 percent.

Assumption 4 implies that the *always-employed*, who conform the EE stratum, possess traits that result in better labor market outcomes relative to the *employed if treated* that conform the NE stratum. While Assumption 4 is not directly testable, we exploit the latter implication and compare pre-treatment covariates (measured pre-birth) correlated with post-birth wages across the EE and NE strata. To implement it, note that under Assumptions 1 to 3 average pre-birth labor market characteristics (X) for the EE stratum are point identified as: $E[E[X_i|T_i = 0, S_i = 1]|T_i = 1]$. While the NE stratum characteristics are not point identified, one can point identified average pre-birth labor market characteristics for the $\{EE, NE\}$ strata by: $E[X_i|T_i = 1, S_i = 1]$. With the implied ranking of Assumption 4, one would expect to find evidence suggesting that the average pre-birth labor market characteristics for the EE stratum are more favorable than those combining the $\{EE, NE\}$ strata, i.e., $E[X_i|EE] \geq E[X_i|EE, NE]$. We selected the pre-birth labor market variables: age, proportion working in the public sector, proportion working in the Financial sector, hourly wages, and hours worked. With the exception of the variable age, the estimated differences between the average pre-birth labor market variables employed for the EE and the $\{EE, NE\}$ strata were all positive, suggesting that Assumption 4 is plausible in our context.[24]

We now turn our attention to the analysis of eligible mothers in the EE stratum that reported to be full-time employed at baseline. The estimated results are presented in Table 5. The top portion of the table shows estimated EE stratum proportions over time that are similar in magnitude to the estimates for the main sample, which includes both full and part-time workers at baseline. Relative to the main sample, the estimated NN stratum proportions are slightly smaller, while the estimated NE stratum proportions are slightly higher. The latter comparison is consistent with the composition of the sample analyzed in Table 5, i.e., full-time employed at baseline eligible mothers. In the middle panel of Table 5, we note that the estimated lower bounds under Assumptions 1 to 3 tend to penalize eligible mothers in the EE stratum that were working full-time at baseline slightly more than the estimated lower bounds for the main sample. This result is not surprising, as reported in the summary statistics for the full-time at baseline sample, these eligible mothers tend to have better labor market outcomes. Focusing on the estimates under the preferred set of Assumptions (1 to 4), which are reported in the bottom panel of Table 5, we find that the estimated lower bounds rule out effects larger than about 8 to 9 percent when analyzing post-birth wages measured 2, 3 and 5 years after birth, while the lower bound estimate for the outcome at 4 years post-birth rules out effects larger than about 5 percent. The latter estimates are quite similar in magnitudes than those reported for the main sample. Estimated upper bounds are consistent with relatively smaller reductions when wages are measured 2 and 3 years post-birth, however, the respective 95 confidence intervals do not rule out zero effects. In contrast to the estimates for the main sample, estimated upper bounds are positive, albeit quite small, when analyzing outcomes measured 4 and 5 years after birth.

5.2 Estimated Bounds for the Quantile Treatment Effects of Parental Leave Take Up on Wages

An advantage of estimating bounds for quantile treatment effects is that it allows for the analysis of effects beyond average impacts. First, we focus on presenting estimates for the main sample (full or part-time employed at baseline), then we discuss results for the full-time at baseline sample in this section's last paragraph. Under Assumptions 1 to 3, we present in Table B2 the estimated bounds for quantile treatment effects on several quantiles of the post-birth wage distribution for *always-employed* mothers. As

[24]See Appendix Table B1 for detailed estimates related to the assessment of Assumption 4.

before, estimated bounds are reported in brackets and the respective 95% IM confidence intervals are reported in parentheses.[25] Focusing on outcomes measured 2 years post-birth, estimated lower bounds at the bottom of the wage distribution are smaller in magnitude, and positive in the case of the 10^{th} and 20^{th} quantiles. Estimated lower bounds beyond the 25^{th} quantile, which indicates that wages can potentially be reduced by 1.4%, are all negative and increase in magnitude, suggesting that negative impacts are potentially larger in the upper part of the wage distribution. For example, the estimated lower bound at the 90^{th} quantile indicates a potential wage reduction of 24.4% due to parental leave participation. Similar qualitative conclusions are reached when analyzing estimates for outcomes measured beyond the 2 year post-birth horizon.

Analogous to the bounds on average impacts after employing mean monotonicity across strata, adding Assumption 5 results in tighter lower bounds for the quantile treatment effects of interest, while the upper bounds remain unchanged. In Table 7 we present estimates for the quantile treatment effects after adding Assumption 5. Focusing on the first column, for the outcome measured 2 years post-birth, we find that all estimated lower bounds below the 30^{th} quantile are positive and relatively larger in magnitude, however, the respective 95% IM confidence intervals do not rule out that these effects are different from zero. While all other estimated lower bounds on the different quantiles of the wage distribution remain negative, it is quite evident that after adding Assumption 5 magnitudes are significantly reduced. For example, the quantile treatment effect lower bound at the median goes from a -0.159 to -0.129, and at the 75^{th} quantile it goes from -0.244 to -0.154. Interestingly, a similar trend based on the estimated lower bounds that is suggestive of potentially larger impacts on higher quantiles is still evident after adding Assumption 5. In longer time horizons we observe similar important reductions in estimated lower bounds after adding Assumption 5. For instance, when the outcome is measured 5 years post-birth, the quantile treatment effect lower bound at the median goes from a -0.187 to -0.142, and at the 75^{th} quantile it goes from -0.377 to -0.272 when Assumption 5 is added.

Focusing on the upper bound estimates for the quantile treatment effects reported in Table 7, for the outcome measured 2 years post-birth we find positive effects from the 30^{th} quantile and below. For these lower portion of the distribution, estimated bounds and their respective 95% IM confidence intervals do not allow one to rule out negative or positive effects of parental leave on wages. A similar conclusion about effects in lower quantiles of the wage distribution also applies for the analysis of outcomes measured 3, 4 and 5 years after birth. In general, the estimated bounds do not identify the sign of the effect for quantiles strictly below the median. While the estimated upper bounds for the effects at the median are negative for outcomes measured during the different time

[25]To avoid cluttering, we report the same estimates with a 90% IM confidence interval in the appendix.

horizons considered, only in the case of outcomes measured 2 years post-birth the effect at the median is significantly different from zero based on the 95% IM confidence intervals. The latter suggests that parental leave take-up significantly reduces the median wages of *always-employed* mothers, and this effect is bounded between a 9.9% to a 12.9% reduction. For outcomes measured 3, 4 and 5 years post-birth, the estimated upper bounds for quantile treatment effects beyond the median are always negative and, in most cases, statistically different from zero based on their 95% IM confidence intervals. For example, the statistically significant upper bound at the 75^{th} quantile of the wage distribution indicates that parental leave effects can potentially reduce wages by at least 12.9% two years after birth, where this effect does not tamper off by the fifth year after birth. In fact, the estimated magnitudes for upper bounds at higher quantiles are suggestive of an effect that becomes larger as time passes, which stands in contrast with the conclusions drawn from the estimated bounds for the average effects of interest. It is important to highlight that there is a large extent of overlap based on estimated confidence intervals, and thus, the latter evidence is suggestive.

We report estimated bounds on quantile treatment effects of interest for the sample of eligible mothers in the EE stratum that reported to be full-time employed at baseline in Table 8. For brevity, these estimated results we report were obtained under the preferred set of assumptions (i.e., Assumptions 1 to 3 and 5). Relative to the main sample, which includes both full and part-time employed at baseline eligible mothers, we draw similar qualitative conclusions from analysing the sample restricted to full-time employed at baseline eligible mothers. For example, for most of the time horizons analyzed, the estimated bounds, and their respective confidence intervals, at the 40^{th} quantile and below are not indicative of statistically significant effects of parental leave take-up on wages for eligible mothers in the EE stratum, while most estimated upper bounds are negative and statistically different from zero at quantiles above the 60^{th}. In these higher quantiles, where we find detrimental wage effects due to parental leave take up, the estimated magnitudes are somewhat smaller when analyzing the outcome that excludes eligible mothers that reported to be working part-time at birth. For instance, two years post-birth, estimated bounds for the quantile treatment effect at the 75^{th} quantile is bounded between -0.139 and -0.114, which are smaller in magnitude when compared to the estimates -0.154 and -0.129 for the main sample. Similar to the trend observed in the main sample analysis, for the sample of full-time employed at baseline mothers the estimated magnitudes for upper bounds at higher quantiles are suggestive of an effect that becomes larger over time.

5.3 Discussion of Results

Estimated bounds for the average effects of actual parental leave participation on wages of eligible first-time mothers that are always employed regardless of leave take-up are, in general, consistent with the existing empirical evidence on the wage consequence of parental leave policies. For example, Ruhm (1998) claims that leave of 9 months represents an hourly wage penalty of 3 percent, whereas leave of three months appears not to have any negative impact on hourly wage, and Datta Gupta and Smith (2002) estimate that in Denmark a career break of one year represents a 7 percent decrease in wage at age of 40. Similar wage reductions are reported throughout this literature (e.g., Thévenon and Solaz, 2013; Ejrnæs and Kunze, 2013; Schönberg and Ludsteck, 2014; Olivetti and Petrongolo, 2017), where most of the conditional-on-employment point estimates reported are contained within our estimated bounds that do control for selection. Qualitatively, our estimates on quantile treatment effects are also consistent with findings suggesting that negative wage effects of parental leave policy are larger for high-skilled mothers (e.g., Akgunduz and Plantenga, 2013). We highlight the importance on the analysis beyond average impacts, since our quantile treatment effects estimates that control for selection do suggest that potential wage penalties, in the upper part of the wage distribution, for the stratum of eligible mothers analyzed could be quite large (e.g., in the order of 20 percent) relative to average impacts, which could be less than 10 percent in the worst-case scenario. Understanding the impacts of actual parental leave participation of eligible mothers is also of importance to the gender wage gap literature. Our estimates are consistent with empirical findings in this literature. For example, studies have found that parental leave take-up explains about 17 percent of the gender pay gap in Denmark (Meilland, 2001) and 27 percent in France (Meurs and Ponthieux, 2000).

We now discuss about the potential mechanisms that could explain the estimated wage penalties for the eligible mothers that took parental leave and are always employed regardless of leave take-up. The first possible explanation relates to human capital depreciation and lack of accumulation resulting from the labor market intermittency after having taken parental leave. This claim has well-established theoretical grounds and several studies have presented empirical evidence suggesting economically significant effects of labor market intermittency on wages for females (e.g., Mincer and Polachek, 1974; Mincer and Ofek, 1982; Kim and Polachek, 1994; Beblo and Wolf, 2000). We argue that the career break experienced by the eligible mothers of interest that took parental leave may have significantly impacted their human capital. While our analysis of average effects on wages was consistent with relative reductions in human capital stock that persisted over the time horizon analyzed (from 2 to 5 years after birth), the estimated bounds' confidence intervals do not rule out zero effects. Interestingly, in our analysis of effects on the distribution of wages we do find important and significant reductions in higher quantiles, that is, parental leave negative human capital effects are found for eligible mothers of interest that have a higher earnings potential and may be working in higher-qualified jobs, where regular upgrade of skills is important (e.g., Anderson, Binder and Krause, 2002).

While we believe that the argument for a human capital mechanism is strong in our application, at least three other potential mechanisms can be confounded in the total estimated effects we found. A second possible mechanism is related to the possibility that parental leave take-up might have triggered a labor supply change. In particular, parental leave take-up might have resulted in some of the eligible mothers of interest to switch from full-time to part-time work. A somewhat extensive literature, theoretical and empirical, documents the existence of a part-time penalty with a focus on women (e.g., Ermisch and Wright, 1993; Hotchkiss, 1991; Aaronson and French, 2004), with findings ranging from relatively small to significant effects. To explore the importance of this potential underlying mechanism in our application, we conduct two (informal) exercises. First, we report that the extent of eligible mothers that worked full-time before birth and switch to part-time employment after birth is quite small—the estimated proportions indicate that less than 0.28 percent of eligible mothers switch from full-time (at birth) to part-time employment measured at the different time periods under analysis. Second, we employ a continuous measure of labor supply as an outcome to estimate parental leave take-up effects, under Assumptions 1 and 2. These estimates are reported in Table A2. In the top panel, for the main sample that includes eligible mothers that reported working full- or part-time before birth, we find negative effects of parental leave on hours worked measured at 2, 3, 4 and 5 years post-birth that range from 3.19 to 5.4 percent reductions, where the only estimate significant at a 5 percent level is for the outcome measured 2 years after birth, while the other estimates are significant at a 10 percent level. In the bottom panel, we remove eligible mothers that reported to be working part-time before birth, and find estimated magnitudes that are relatively small and not statistically significant, raging from 2.79 to 4.2 percent.[26] The latter results consider all eligible mothers that took parental leave, however, we consider them as evidence that for eligible mothers of interest, i.e., those that took parental leave and are always employed regardless of taking the leave, labor supply related penalties may play a lesser role in explaining our estimated results.

Another possible explanation focuses on the difference in the promotion prospects between eligible mothers who experienced a career break due to parental leave and those who did not. Due to absence from the work place and impossibility to invest in or

[26]These results stand in contrast with those reported by Valentova (2019), who found positive impacts of parental leave policy on labor supply for eligible mothers. We argue that the differences are attributed to the fact that Valentova (2019) analyzed the time period around the policy introduction (1999), and exploited a difference-in-difference strategy with non-mother females as controls.

to negotiate career development, leave-takers might have experienced a slower pace of career progression and promotion in their job (Blau and Kahn, 1992). Women taking parental leave may be also less likely to benefit from new job offers on the labour market than mothers with uninterrupted career and, consequently, they loose opportunities by increasing their wages by changing a job. It is reasonable to assume that the promotion penalty translated into the wage penalty might be higher for eligible mothers in higher quantiles of the wage distribution. The latter claim can be indirectly supported by the evidence on gender differences in wage growth and promotion in Luxembourg that suggests that promotion rates are positively associated with the level of wage, where the gender difference in promotion are larger among low-pay workers and they decrease in the upper half of the wage distribution (Philippe Van Kerm, 2006). A last possible mechanism we discuss is related to the fact that eligible mothers that take a leave can be perceive by employers as less motivated and committed to work. As a result, there is the potential for employers to systematically discriminate against this group, resulting in lower promotion prospects or allocation of less strategic tasks in easier replaceable positions. The latter may also lead to lower wage increases overtime compared to counterparts that do not take or are not likely to take parental leave (in line with Datta Gupta, Verner and Smith (2006)). We consider that exploring the above possible mechanisms, which could contribute to the wage penalties found, are interesting topics for further research.

6 Conclusions

We employ high-quality social security records from Luxembourg to analyze the effect of parental leave take-up on post-birth wages of eligible first-time mothers, who are post-birth always-employed regardless of having taken parental leave. The data available to us was recorded from 2005 to 2015, and allow us to shed light on the evolution of effects for outcomes measured 2, 3, 4 and 5 years after child birth. We estimate bounds on causal effects of interest after simultaneously controlling for selection into parental leave take-up and post-birth employment since wages are only observed for the employed. To control for the former issue, we employ selection on observables, which we argue is plausible in our context given the richness of the data. To control for selection into employment we implement Principal Stratification and assume positive monotonicity of parental leave take-up on employment and monotonicity of mean potential outcomes across two strata, i.e., the always-employed of interest, which account for 80 percent of all eligible mothers in our data, and eligible mothers that are employed only if they take parental leave. To our knowledge, our estimated bounds for the causal parameters analyzed, average and quantile treatment effects, are unique within the literature evaluating parental leave policy. As such, our contribution to the literature is both substantive and methodological, given that we provide a detailed discussion on the implementation of the econometric strategy employed. We find new evidence that is consistent with a detrimental effect of parental leave take-up on post-birth wages for the eligible mothers of interest. Our analysis of average impacts suggest that the potential effect can be economically significant (e.g., up to a 8 percent reduction for wages measured 2 years after birth), with worst case estimates not diminishing over time. Our best case scenarios, based on the estimated lower bounds and their respective confidence intervals, do not rule out the potential for having zero average impact on wages. Our analysis of quantile treatment effects allow us to observe heterogeneous impacts of leave uptake across the distribution of post-birth wages for the eligible mothers of interest. These estimated bounds, in general, show that parental leave take up has an important and statistically significant detrimental effect at most quatiles above the median of the post-birth wage distribution for the eligible mothers of interest. Furthermore, we report that the latter wage penalty for higher-paid eligible first-time mothers of interest does not diminish over time. We argue that the estimated wage penalties, for the eligible first-time mothers that took parental leave and are always-employed regardless of leave take-up, are mostly explained by human capital depreciation and lack of accumulation resulting from the labor market intermittency experienced after having taken parental leave. Other plausible mechanisms are discussed, e.g., changes in labor supply: from full to part-time jobs. While we cannot rule out the existence of alternative mechanisms, we find suggestive evidence that is indicative of their lesser role in explaining our results.

The literature evaluating parental leave policy recognizes leave duration as an important variable whose length helps determine the extent of impacts on outcomes of interest. In the case of hourly wages, longer leave duration is often associated with larger negative impacts (Ruhm, 1998; Datta Gupta, Verner and Smith, 2006). Then, one could argue that policy improvement can be achieved by way of introducing financial incentives that promote the usage of shorter parental leave (or part-time forms), reducing the extent of human capital deterioration and partly compensating for what is actually lost. Given that it has also been extensively documented that parental leave policy has positive effects in other important dimensions (e.g., mother and newborn's health, employment), policy changes that affect the length of the leave should consider the potential impact on these other dimensions. The wage penalty could also be reduced by extending advanced tools that promote equal pay between men and women, specially in private sector companies within Luxembourg. The availability of delegates for gender equality has been a common practice within Luxembourg's public sector. Additionally, stimulating fathers' parental leave participation could potentially lower the wage penalty through a reduction in the length of leave for mothers—parental leave participation for eligible men in Luxembourg is relatively low, where eligible mothers are 60 percent more likely to take a leave. A potential increase in parental leave participation of men would also have a positive effect on gender equality.

Finally, we note that the present paper explores the effects of parental leave take up on wages of eligible first-time mothers of interest during the 2005-2010 period. During the time period analyzed, parental leave policy parameters did not significantly change, basically remaining unchanged since the policy introduction in 1999. A major parental leave policy reform in Luxembourg was introduced on December of 2016. Unfortunately, the data covering a long enough period after the reform is still not available. Of the many significant changes that were introduced by the 2016 reform, a significant one is related to time flexibility in parental leave, which allows parents to better reconcile family and work, coordinate take-up strategies, and incorporate the needs of employers. Another element of the reform was the switch from the flat-rate parental leave benefit (approximately equal to a minimum social salary) to an income-related benefit with a relatively high upper threshold. This parameter of the reform further incentivized take up rates for high earners, specially for fathers. The latter is argued to have contributed to the improvement and balance of the parental labor division within the household. We suggest that further research is needed to comprehensively analyze the effect the reform on the post-birth wage and other important outcomes for eligible parents.

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Table 1: Descriptive Statistics for Selected Baseline Characteristics of Eligible Mothers that Took Parental Leave, Full and Part-Time Jobs Sample.)

Demographic characteristics	Mean	Std. Dev.
Age	31.18	4.680
Single	0.385	0.486
Married	0.568	0.495
Divorced	0.045	0.207
Not living with partner	0.095	0.293
Living with partner	0.899	0.300
Partner not existing	0.000	0.000
Nationality		
Luxembourg	0.564	0.496
French	0.098	0.298
Belgian	0.031	0.175
German	0.024	0.155
Portuguese	0.163	0.370
Other	0.116	0.320
Labor Market experiences		
Private Sector employee	0.868	0.338
Civil Servant employee	0.131	0.338
Low work experience (0-5 years)	0.217	0.412
Medium work experience (6-10 years)	0.372	0.483
High work experience (>10 years)	0.410	0.492
Firm characteristics		
1-49 employees	0.301	0.458
50-250 employees	0.230	0.421
>250 employees	0.468	0.499
Proportion of White	*0.737*	0.440
Proportion of Women	*0.397*	0.489
Proportion of Young	*0.950*	0.216
Sector Activity		
Commerce	0.121	0.326
Finance	0.182	0.3865
Real Estate	0.124	0.330
Health	0.188	0.3901
Teaching/Research	0.164	0.370
Other	0.219	0.414
Labor Market Outcomes (6 months pre-birth)		
Hours worked	*167.189*	20.472
Months worked	*5.999*	0.026
Hourly wage	*20.921*	10.491
Spouse Labor Market Outcomes (6 months pre-birth)		
Hours worked	*171.816*	18.522
Months worked	*5.891*	0.580
Hourly wages	*24.658*	14.053

Table 2: Descriptive Statistics for Selected Baseline Characteristics of Eligible Mothers that Took Parental Leave, Full-Time Jobs Sample.)

Demographic characteristics	Mean	Std. Dev.
Age	31.225	4.567
Single	0.383	0.486
Married	0.569	0.495
Divorced	0.048	0.214
Not living with partner	0.098	0.297
Living with partner	0.897	0.304
Nationality		
Luxembourg	0.58	0.494
French	0.101	0.301
Belgian	0.034	0.182
German	0.026	0.161
Portuguese	0.142	0.349
Other	0.116	0.321
Labor Market experiences		
Private Sector employee	0.857	0.351
Civil Servant employee	0.143	0.351
Low work experience (0-5 years)	0.198	0.399
Medium work experience (6-10 years)	0.389	0.488
High work experience (>10 years)	0.413	0.493
Firm characteristics		
1-49 employees	0.302	0.459
50-250 employees	0.235	0.424
>250 employees	0.461	0.498
Proportion of White	0.764	0.425
Proportion of Women	0.363	0.481
Proportion of Young	0.954	0.21
Sector Activity		
Commerce	0.128	0.334
Finance	0.199	0.399
Real Estate	0.106	0.308
Health	0.195	0.397
Teaching/Research	0.15	0.358
Other	0.221	0.415
Labor Market Outcomes (6 months pre-birth)		
Hours worked	172.615	11.218
Months worked	6	0
Hourly wage	21.184	10.25
Spouse Labor Market Outcomes (6 months pre-birth)		
Hours worked	171.225	15.973
Months worked	5.891	0.588
Hourly wages	24.961	13.81

Table 4: Estimated Bounds on the Average Treatment Effect of Parental Leave Take up on Hourly Wages for Always Employed Mothers, Full and Part-Time Jobs Sample.

	2 years	3 years	4 years	5 years
Principal Strata				
Always employed, π_{EE}	0.824	0.829	0.820	0.775
Never employed, π_{NN}	0.125	0.121	0.132	0.152
Employed if treated, π_{NE}	0.051	0.049	0.047	0.072
Bounds under Assumptions 1, 2 and 3				
$[LB_{EE}, UB_{EE}]$	$[-0.147, -0.036]$	$[-0.135, -0.026]$	$[-0.108, -0.001]$	$[-0.182, -0.022]$
(95% IM confidence intervals)	$(-0.232, 0.048)$	$(-0.216, 0.059)$	$(-0.197, 0.091)$	$(-0.273, 0.075)$
(90% IM confidence intervals)	$(-0.214, 0.029)$	$(-0.198, 0.040)$	$(-0.177, 0.070)$	$(-0.253, 0.054)$
Bounds adding Assumption 4				
$[LB_{EE}, UB_{EE}]$	$[-0.087, -0.036]$	$[-0.077, -0.026]$	$[-0.050, -0.001]$	$[-0.100, -0.022]$
(95% IM confidence intervals)	$(-0.156, 0.048)$	$(-0.146, 0.059)$	$(-0.128, 0.091)$	$(-0.179, 0.075)$
(90% IM confidence intervals)	$(-0.141, 0.029)$	$(-0.131, 0.040)$	$(-0.111, 0.070)$	$(-0.162, 0.054)$

Note: IM c.i. refers to the Imbens and Manski (2004) confidence intervals. These confidence intervals were computed using bootstrap standard errors from 2,000 replications.

Table 5: Estimated Bounds on the Average Treatment Effect of Parental Leave Take up on Hourly Wages for Always Employed Mothers. Full-Time Jobs Sample.

	2 years	3 years	4 years	5 years
Principal Strata				
Always employed, π_{EE}	0.815	0.831	0.824	0.773
Never employed, π_{NN}	0.118	0.110	0.125	0.143
Employed if treated, π_{NE}	0.067	0.059	0.052	0.084
Bounds under Assumptions 1, 2 and 3				
$[LB_{EE}, UB_{EE}]$	$[-0.156, -0.017]$	$[-0.144, -0.019]$	$[-0.110, 0.006]$	$[-0.185, 0.000]$
(95% IM confidence intervals)	$(-0.254, 0.081)$	$(-0.235, 0.071)$	$(-0.208, 0.104)$	$(-0.291, 0.111)$
(90% IM confidence intervals)	$(-0.232, 0.059)$	$(-0.215, 0.051)$	$(-0.186, 0.082)$	$(-0.267, 0.086)$
Bounds adding Assumption 4				
$[LB_{EE}, UB_{EE}]$	$[-0.082, -0.017]$	$[-0.078, -0.019]$	$[-0.048, 0.006]$	$[-0.089, 0.000]$
(95% IM confidence intervals)	$(-0.161, 0.081)$	$(-0.151, 0.071)$	$(-0.132, 0.104)$	$(-0.178, 0.111)$
(90% IM confidence intervals)	$(-0.143, 0.059)$	$(-0.135, 0.051)$	$(-0.113, 0.082)$	$(-0.158, 0.086)$

Note: IM c.i. refers to the Imbens and Manski (2004) confidence intervals. These confidence intervals were computed using bootstrap standard errors from 2,000 replications.

Table 6: Estimated Bounds on the Quantile Treatment Effect of Parental Leave Take Up on Hourly Wages for Always Employed Mothers, Under Assumptions 1 to 3. Full and Part-Time Jobs Sample.

Alpha-quantile	2 years	3 years	4 years	5 years
0.10	[0.026 , 0.106]	[0.004 , 0.104]	[0.068 , 0.158]	[0.043 , 0.203]
	(-0.038 , 0.218)	(-0.068 , 0.226)	(-0.012 , 0.310)	(-0.045 , 0.367)
0.20	[0.016 , 0.136]	[0.014 , 0.154]	[0.088 , 0.193]	[0.008 , 0.238]
	(-0.098 , 0.291)	(-0.091 , 0.292)	(-0.011 , 0.331)	(-0.129 , 0.411)
0.25	[-0.004 , 0.131]	[0.029 , 0.174]	[0.103 , 0.213]	[0.003 , 0.183]
	(-0.171 , 0.313)	(-0.150 , 0.371)	(-0.037 , 0.373)	(-0.173 , 0.369)
0.30	[-0.049 , 0.061]	[-0.101 , -0.001]	[0.068 , 0.143]	[-0.067 , 0.063]
	(-0.245 , 0.265)	(-0.309 , 0.209)	(-0.142 , 0.357)	(-0.246 , 0.247)
0.40	[-0.129 , -0.079]	[-0.081 , -0.021]	[-0.042 , 0.018]	[-0.137 , -0.027]
	(-0.227 , 0.026)	(-0.163 , 0.073)	(-0.151 , 0.127)	(-0.249 , 0.091)
0.50	[-0.159 , -0.099]	[-0.121 , -0.071]	[-0.092 , -0.032]	[-0.187 , -0.087]
	(-0.244 , -0.011)	(-0.221 , 0.035)	(-0.194 , 0.071)	(-0.312 , 0.039)
0.60	[-0.199 , -0.129]	[-0.206 , -0.146]	[-0.157 , -0.102]	[-0.292 , -0.202]
	(-0.314 , -0.018)	(-0.327 , -0.025)	(-0.294 , 0.038)	(-0.418 , -0.066)
0.70	[-0.239 , -0.149]	[-0.256 , -0.166]	[-0.292 , -0.232]	[-0.322 , -0.192]
	(-0.337 , -0.054)	(-0.366 , -0.062)	(-0.415 , -0.114)	(-0.441 , -0.077)
0.75	[-0.244 , -0.129]	[-0.271 , -0.166]	[-0.272 , -0.182]	[-0.377 , -0.227]
	(-0.348 , -0.045)	(-0.383 , -0.057)	(-0.374 , -0.085)	(-0.489 , -0.110)
0.80	[-0.194 , -0.094]	[-0.261 , -0.131]	[-0.272 , -0.152]	[-0.342 , -0.122]
	(-0.331 , 0.023)	(-0.377 , -0.045)	(-0.398 , -0.056)	(-0.458 , -0.028)
0.90	[-0.244 , -0.114]	[-0.216 , -0.106]	[-0.232 , -0.107]	[-0.282 , -0.122]
	(-0.383 , -0.020)	(-0.336 , -0.032)	(-0.385 , 0.001)	(-0.469 , 0.014)

Note: Estimated bounds are reported in brackets. 95% Imbens and Manski (2004) confidence intervals reported in parentheses. Bootstrapped standard errors to compute confidence intervals are based on 2000 replications.

Table 7: Estimated Bounds on the Quantile Treatment Effect of Parental Leave Take Up on Hourly Wages for Always Employed Mothers, Under Assumptions 1, 2, 3 and 5. Full and Part-Time Jobs Sample.

Alpha-quantile	2 years	3 years	4 years	5 years
0.10	[0.051 , 0.106]	[0.019 , 0.104]	[0.078 , 0.158]	[0.053 , 0.203]
	(-0.012 , 0.218)	(-0.052 , 0.226)	(-0.002 , 0.310)	(-0.033 , 0.367)
0.20	[0.031 , 0.136]	[0.054 , 0.154]	[0.113 , 0.193]	[0.058 , 0.238]
	(-0.081 , 0.291)	(-0.048 , 0.292)	(0.019 , 0.331)	(-0.078 , 0.411)
0.25	[0.036 , 0.131]	[0.069 , 0.174]	[0.128 , 0.213]	[0.068 , 0.183]
	(-0.127 , 0.313)	(-0.109 , 0.371)	(-0.012 , 0.373)	(-0.105 , 0.369)
0.30	[-0.014 , 0.061]	[-0.056 , -0.001]	[0.093 , 0.143]	[-0.022 , 0.063]
	(-0.209 , 0.265)	(-0.260 , 0.209)	(-0.114 , 0.357)	(-0.195 , 0.247)
0.40	[-0.114 , -0.079]	[-0.061 , -0.021]	[-0.017 , 0.018]	[-0.092 , -0.027]
	(-0.208 , 0.026)	(-0.141 , 0.073)	(-0.122 , 0.127)	(-0.203 , 0.091)
0.50	[-0.129 , -0.099]	[-0.101 , -0.071]	[-0.057 , -0.032]	[-0.142 , -0.087]
	(-0.208 , -0.011)	(-0.199 , 0.035)	(-0.158 , 0.071)	(-0.265 , 0.039)
0.60	[-0.159 , -0.129]	[-0.166 , -0.146]	[-0.127 , -0.102]	[-0.242 , -0.202]
	(-0.268 , -0.018)	(-0.282 , -0.025)	(-0.262 , 0.038)	(-0.367 , -0.066)
0.70	[-0.179 , -0.149]	[-0.191 , -0.166]	[-0.257 , -0.232]	[-0.237 , -0.192]
	(-0.266 , -0.054)	(-0.290 , -0.062)	(-0.370 , -0.114)	(-0.343 , -0.077)
0.75	[-0.154 , -0.129]	[-0.201 , -0.166]	[-0.197 , -0.182]	[-0.272 , -0.227]
	(-0.237 , -0.045)	(-0.300 , -0.057)	(-0.285 , -0.085)	(-0.375 , -0.110)
0.80	[-0.109 , -0.094]	[-0.151 , -0.131]	[-0.167 , -0.152]	[-0.167 , -0.122]
	(-0.223 , 0.023)	(-0.234 , -0.045)	(-0.260 , -0.056)	(-0.264 , -0.028)
0.90	[-0.134 , -0.114]	[-0.116 , -0.106]	[-0.122 , -0.107]	[-0.127 , -0.122]
	(-0.226 , -0.020)	(-0.187 , -0.032)	(-0.231 , 0.001)	(-0.264 , 0.014)

Note: Estimated bounds are reported in brackets. 95% Imbens and Manski (2004) confidence intervals reported in parentheses. Bootstrapped standard errors to compute confidence intervals are based on 2000 replications.

Table 8: Estimated Bounds on the Quantile Treatment Effect of Parental Leave Take Up on Hourly Wages for Always Employed Mothers, Under Assumptions 1, 2, 3 and 5. Full-Time Jobs Sample.

Alpha-quantile	2 years	3 years	4 years	5 years
0.10	[0.036 , 0.146]	[-0.006 , 0.099]	[0.069 , 0.169]	[0.032 , 0.232]
	(-0.054 , 0.307)	(-0.084 , 0.252)	(-0.044 , 0.358)	(-0.070 , 0.440)
0.20	[0.006 , 0.151]	[0.059 , 0.164]	[0.074 , 0.169]	[0.027 , 0.227]
	(-0.148 , 0.348)	(-0.089 , 0.349)	(-0.062 , 0.344)	(-0.131 , 0.414)
0.25	[-0.014 , 0.111]	[-0.011 , 0.094]	[0.114 , 0.209]	[0.052 , 0.187]
	(-0.207 , 0.323)	(-0.251 , 0.346)	(-0.097 , 0.429)	(-0.149 , 0.397)
0.30	[-0.054 , 0.031]	[-0.116 , -0.041]	[-0.021 , 0.019]	[-0.048 , 0.037]
	(-0.237 , 0.221)	(-0.267 , 0.120)	(-0.212 , 0.220)	(-0.219 , 0.219)
0.40	[-0.124 , -0.064]	[-0.066 , -0.001]	[-0.001 , 0.034]	[-0.068 , -0.003]
	(-0.233 , 0.055)	(-0.153 , 0.095)	(-0.109 , 0.145)	(-0.179 , 0.116)
0.50	[-0.114 , -0.069]	[-0.086 , -0.051]	[-0.046 , -0.026]	[-0.088 , -0.043]
	(-0.204 , 0.027)	(-0.199 , 0.070)	(-0.158 , 0.089)	(-0.236 , 0.107)
0.60	[-0.154 , -0.119]	[-0.166 , -0.131]	[-0.121 , -0.086]	[-0.243 , -0.173]
	(-0.275 , 0.007)	(-0.291 , 0.003)	(-0.269 , 0.068)	(-0.375 , -0.030)
0.70	[-0.164 , -0.129]	[-0.161 , -0.146]	[-0.221 , -0.191]	[-0.208 , -0.163]
	(-0.254 , -0.031)	(-0.263 , -0.036)	(-0.342 , -0.063)	(-0.318 , -0.043)
0.75	[-0.139 , -0.114]	[-0.166 , -0.121]	[-0.181 , -0.156]	[-0.248 , -0.193]
	(-0.226 , -0.023)	(-0.281 , 0.003)	(-0.281 , -0.047)	(-0.363 , -0.061)
0.80	[-0.094 , -0.064]	[-0.146 , -0.131]	[-0.131 , -0.101]	[-0.148 , -0.098]
	(-0.213 , 0.059)	(-0.241 , -0.031)	(-0.236 , 0.007)	(-0.252 , 0.001)
0.90	[-0.114 , -0.089]	[-0.116 , -0.096]	[-0.146 , -0.131]	[-0.128 , -0.118]
	(-0.222 , 0.021)	(-0.211 , 0.003)	(-0.270 , -0.007)	(-0.274 , 0.028)

Note: Estimated bounds are reported in brackets. 95% Imbens and Manski (2004) confidence intervals reported in parentheses. Bootstrapped standard errors to compute confidence intervals are based on 2000 replications.

Table 9: Estimated Average Treatment Effect of Parental Leave Take Up on Hours Worked and Earnings, Under Assumptions 1 and 2.

	Full + Part Time Sample Hours Worked	% Change
ATT at 2 years post-birth	-6.916 **	-5.40 %
	(3.169)	
Constant	128.066	
ATT at 3 years post-birth	-5.930 *	-4.68 %
	(3.155)	
Constant	126.752	
ATT at 4 years post-birth	-6.113 *	-4.88 %
	(3.201)	
Constant	125.360	
ATT at 5 years post-birth	-3.832	-3.19 %
	(3.342)	
Constant	120.045	

	Full Time Sample Hours Worked	% Change
ATT at 2 years post-birth	-5.382	
	(3.432)	
Constant	129.733	
ATT at 3 years post-birth	-4.054	-3.15 %
	(3.378)	
Constant	128.582	
ATT at 4 years post-birth	-5.364	-4.20 %
	(3.426)	
Constant	127.559	
ATT at 5 years post-birth	-3.419	-2.79 %
	(3.606)	
Constant	122.676	

Note: *, **, and *** denote statistical significance at a 90, 95 and 99 percent confidence level. Computations use entropy weights.

Table A1: Full Set of Baseline Characteristics Employed in Entropy Balancing, Separated by Treatment Status for the Full and Part-Time Jobs Sample.

	Controls (Did not take PL)		Treated (PL Takers)	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
2005	0.146	0.354	0.15	0.357
2006	0.174	0.379	0.152	0.359
2007	0.184	0.388	0.162	0.368
2008	0.201	0.401	0.18	0.384
2009	0.124	0.33	0.177	0.382
2010	0.169	0.375	0.177	0.38
Private Sector	0.902	0.296	0.868	0.338
Civil Servant	0.097	0.296	0.131	0.338
Not living with partner	0.087	0.282	0.095	0.293
Living with partner	0.91	0.285	0.899	0.3
Info NA	0.002	0.049	0.004	0.07
Age	30.619	5.616	31.18	4.68
Luxembourg	0.363	0.481	0.564	0.496
French	0.104	0.306	0.098	0.298
Belgian	0.047	0.212	0.031	0.175
German	0.019	0.139	0.024	0.155
Portuguese	0.36	0.48	0.163	0.37
Other nationality	0.104	0.306	0.116	0.32
Single	0.3	0.459	0.385	0.486
Married	0.659	0.474	0.568	0.495
Divorced	0.039	0.195	0.045	0.207
Low work experience	0.345	0.476	0.217	0.412
Medium work experience	0.33	0.471	0.372	0.483
High work experience	0.323	0.468	0.41	0.492
Agriculture	0.046	0.211	0.042	0.201
Construction	0.023	0.151	0.018	0.134
Commerce	0.149	0.357	0.121	0.326
Transport, Communication	0.058	0.235	0.061	0.24
Horeca	0.099	0.3	0.06	0.238
Finance	0.152	0.36	0.182	0.386
Real Estate	0.178	0.383	0.124	0.33
Health	0.14	0.348	0.188	0.39
Teaching, Research support	0.111	0.315	0.164	0.37
Collective Services	0.038	0.191	0.037	0.189
Firm size: 1-9	0.161	0.368	0.11	0.313
Firm size: 10-19	0.059	0.237	0.076	0.266
Firm size: 20-49	0.107	0.31	0.114	0.318
Firm size: 50-99	0.082	0.275	0.094	0.292
Firm size: 100-249	0.113	0.317	0.136	0.343
Firm size: ¿ 250	0.474	0.5	0.468	0.499
Hours worked past 6 mo.	162.116	26.726	167.189	20.472
Wage past 6 mo.	22.278	14.521	20.921	10.491
Months worked past 6 mo.	5.985	0.157	5.999	0.026
Spouse: Hours worked past 6 mo.	169.589	20.002	171.816	18.522
Spouse: Wage past 6 mo.	21.473	12.868	24.658	14.053
Spouse: Months worked past 6 mo.	5.815	0.761	5.891	0.58
White	0.599	0.49	0.737	0.44
Young	0.934	0.247	0.95	0.216

Note: PL stands for Parental Leave, Std. Dev. stands for standard deviation. For brevity, we don't report the customary difference in means across treated and controls, but note that large imbalances in pre-treatment covariates exist. Similarly, we don't report balancing after employing Entropy Weights, since it is achieved for all pre-treatment covariates employed.

Table A2: Full Set of Baseline Characteristics Employed in Entropy Balancing, Separated by Treatment Status for the Full-Time Jobs Sample.

	Controls (Did not take PL)		Treated (PL Takers)	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
2005	0.144	0.352	0.154	0.361
2006	0.185	0.389	0.157	0.364
2007	0.195	0.396	0.168	0.374
2008	0.188	0.391	0.169	0.375
2009	0.125	0.332	0.179	0.384
2010	0.878	0.328	0.172	0.383
Private Sector	0.878	0.328	0.857	0.351
Civil Servant	0.122	0.328	0.143	0.351
Not living with partner	0.085	0.279	0.098	0.297
Living with partner	0.912	0.283	0.897	0.304
Info NA	0.003	0.056	0.005	0.069
Age	30.84	5.41	31.225	4.567
Luxembourg	0.395	0.49	0.58	0.494
French	0.113	0.317	0.101	0.301
Belgian	0.056	0.231	0.034	0.182
German	0.019	0.136	0.026	0.161
Portuguese	0.317	0.466	0.142	0.349
Other nationality	0.1	0.301	0.116	0.321
Single	0.31	0.463	0.383	0.486
Married	0.649	0.478	0.569	0.495
Divorced	0.04	0.198	0.048	0.214
Low work experience	0.288	0.454	0.198	0.399
Medium work experience	0.366	0.482	0.389	0.488
High work experience	0.344	0.476	0.413	0.493
Agriculture	0.046	0.21	0.044	0.205
Construction	0.025	0.156	0.02	0.139
Commerce	0.156	0.364	0.128	0.334
Transport, Communication	0.06	0.238	0.066	0.248
Horeca	0.092	0.29	0.053	0.224
Finance	0.164	0.37	0.199	0.399
Real Estate	0.138	0.346	0.106	0.308
Health	0.163	0.37	0.195	0.397
Teaching, Research support	0.11	0.313	0.15	0.358
Collective Services	0.042	0.202	0.039	0.193
Firm size: 1-9	0.14	0.348	0.11	0.313
Firm size: 10-19	0.072	0.258	0.077	0.267
Firm size: 20-49	0.11	0.313	0.115	0.32
Firm size: 50-99	0.082	0.275	0.096	0.294
Firm size: 100-249	0.082	0.275	0.139	0.347
Firm size: ¿ 250	0.113	0.317	0.461	0.498
Hours worked past 6 mo.	172.424	8.201	172.615	11.218
Wage past 6 mo.	21.596	12.848	21.184	10.25
Months worked past 6 mo.	5.996	0.056	6	0
Spouse: Hours worked past 6 mo.	170.692	19.163	171.225	15.973
Spouse: Wage past 6 mo.	21.94	12.833	24.961	13.81
Spouse: Months worked past 6 mo.	5.812	0.802	5.891	0.588
White	0.654	0.476	0.764	0.425
Young	0.935	0.247	0.954	0.21

Note: PL stands for Parental Leave, Std. Dev. stands for standard deviation. For brevity, we don't report the customary difference in means across treated and controls, but note that large imbalances in pre-treatment covariates exist. Similarly, we don't report balancing after employing Entropy Weights, since it is achieved for all pre-treatment covariates employed.

Table B1: Indirect Assessment of Assumption 4, Main Sample.

	$E[X\|EE]$	$E[X\|EE,NE]$	Difference	Std. Error
Age	31.196	31.383	-0.187	0.28
Works in Public Sector	0.170	0.154	0.016	0.020
Works in Finance	0.194	0.185	0.009	0.021
Hourly wage	22.723	22.034	0.689	0.60
Hours worked	168.83	167.750	1.079	0.983

Note: *, **, and *** denote statistical significance difference in means, Diff., at a 90, 95 and 99 percent confidence level. Bootstrapped standard errors for the difference in means were computed with 1000 replications. Computations use design weights.

Table B2: Estimated Bounds on the Quantile Treatment Effect of Parental Leave Take Up on Hourly Wages for Always Employed Mothers, Under Assumptions 1 to 3. Full-Time Jobs Sample.

	2 years	3 years	4 years	5 years
Alpha-quantile				
0.10	[0.026 , 0.146]	[-0.021 , 0.099]	[0.059 , 0.169]	[0.022 , 0.232]
	(-0.065 , 0.307)	(-0.099 , 0.252)	(-0.055 , 0.358)	(-0.081 , 0.440)
0.20	[-0.039 , 0.151]	[0.024 , 0.164]	[0.049 , 0.169]	[-0.023 , 0.227]
	(-0.195 , 0.348)	(-0.131 , 0.349)	(-0.090 , 0.344)	(-0.186 , 0.414)
0.25	[-0.074 , 0.111]	[-0.046 , 0.094]	[0.084 , 0.209]	[-0.008 , 0.187]
	(-0.270 , 0.323)	(-0.287 , 0.346)	(-0.129 , 0.429)	(-0.212 , 0.397)
0.30	[-0.099 , 0.031]	[-0.151 , -0.041]	[-0.041 , 0.019]	[-0.083 , 0.037]
	(-0.288 , 0.221)	(-0.308 , 0.120)	(-0.238 , 0.220)	(-0.262 , 0.219)
0.40	[-0.154 , -0.064]	[-0.091 , -0.001]	[-0.036 , 0.034]	[-0.123 , -0.003]
	(-0.267 , 0.055)	(-0.179 , 0.095)	(-0.149 , 0.145)	(-0.238 , 0.116)
0.50	[-0.154 , -0.069]	[-0.111 , -0.051]	[-0.086 , -0.026]	[-0.143 , -0.043]
	(-0.253 , 0.027)	(-0.225 , 0.070)	(-0.202 , 0.089)	(-0.295 , 0.107)
0.60	[-0.204 , -0.119]	[-0.201 , -0.131]	[-0.156 , -0.086]	[-0.288 , -0.173]
	(-0.331 , 0.007)	(-0.330 , 0.003)	(-0.306 , 0.068)	(-0.424 , -0.030)
0.70	[-0.249 , -0.129]	[-0.236 , -0.146]	[-0.271 , -0.191]	[-0.298 , -0.163]
	(-0.356 , -0.031)	(-0.355 , -0.036)	(-0.405 , -0.063)	(-0.429 , -0.043)
0.75	[-0.244 , -0.114]	[-0.241 , -0.121]	[-0.261 , -0.156]	[-0.368 , -0.193]
	(-0.361 , -0.023)	(-0.370 , 0.003)	(-0.378 , -0.047)	(-0.502 , -0.061)
0.80	[-0.194 , -0.064]	[-0.261 , -0.131]	[-0.241 , -0.101]	[-0.333 , -0.098]
	(-0.344 , 0.059)	(-0.390 , -0.031)	(-0.382 , 0.007)	(-0.471 , 0.001)
0.90	[-0.249 , -0.089]	[-0.236 , -0.096]	[-0.251 , -0.131]	[-0.288 , -0.118]
	(-0.414 , 0.021)	(-0.378 , 0.003)	(-0.427 , -0.007)	(-0.496 , 0.028)

Note: Estimated bounds are reported in brackets. 95% Imbens and Manski (2004) confidence intervals reported in parentheses. Bootstrapped standard errors to compute confidence intervals are based on 2000 replications.

LUXEMBOURG INSTITUTE OF
SOCIO-ECONOMIC RESEARCH

11, Porte des Sciences
Campus Belval
L-4366 Esch-sur-Alzette

T +352 58 58 55-1
F +352 58 58 55-700

www.liser.lu