# Research Statement

Valeria Zurla*

November 2021

I am an applied microeconomist broadly interested in policy-relevant topics. My main research interests lie in the areas of Public Economics, Labor Economics, and Gender Economics, with a focus on social insurance programs and how to use government interventions to reduce inequalities. My work uses administrative data and modern applied econometrics methods to advance our understanding of how to optimally design social insurance programs and analyze their welfare impact, taking into account agents' behavioral responses and interactions between social insurance programs. My current research can be articulated into three main areas. My first line of research studies the effectiveness, welfare effects, and optimal design of social insurance programs. My second line of research focuses on the determinants of gender inequalities and connects to the first line of research by studying specifically the design of government interventions to reduce gender inequality. My third line of research focuses on migration from a public economics point of view, focusing on its effects on countries' government and integration policies.

## 1 Social Insurance Programs, Behavioral Responses and Welfare

This strand of work broadly examines how to optimally design social insurance programs and analyze their welfare impact, taking into account agents' behavioral responses and interactions between social insurance programs. In my Job Market Paper, "*How Should We Design Parental Leave Policies? Evidence from Two Reforms in Italy*", I study the role of different policy instruments in the design of parental leave policies. Taking advantage of Italian administrative data on the universe of working mothers, I implement a difference-in-differences design around two unemployment insurance reforms that increased, respectively, the level of benefits and the duration of benefits without offering job protection. Using a revealed-preference method, I provide novel insights on the trade-offs that mothers face in making leave decisions, the relative value of benefits and job protection, and the

*Brown University, Department of Economics and Population Studies and Training Center. Email: valeria_zurla@brown.edu

incentive costs associated with parental leave policies. By investigating how the relative value of benefits and job protection changes with different reforms, this paper provides one of the first welfare assessments of parental leave policies. I develop a conceptual framework to evaluate the welfare effects of parental leave policies and I compare the welfare effects of increasing the benefit levels and the benefit duration of leave policies within the MVPF framework. I directly consider the possibility that information frictions may make it difficult for mothers to trade-off short-term benefits and long-term costs, and show how welfare calculations change within the MVPF framework when it is not possible rely on the envelope theorem. This paper also shows that substitution between programs (parental leave and unemployment insurance) is an important margin of adjustment in mothers' behavioral responses to policy changes. Accounting for the consequent fiscal externalities is critical in designing optimal social insurance policies. This paper has been awarded the *Early-Career Scholars Grant* by the Policy Impacts Lab at Harvard, which will provide funding and assistance to continue expanding this line of research over the course of the next year.

Intending to expand this line of research, in the ongoing project "*Program Interactions and Welfare Analysis: A Bayesian Adaptive Choice Experiment*", joint with Marshall Drake, Neil Thakral and Lin T. Tô, we use an adaptive survey methodology called the Bayesian Adaptive Choice Experiment (BACE) to elicit individual-level willingness-to-pay for a variety of public policies directly. BACE is a dynamic variation on standard Discrete Choice Experiment (DCE) survey methods that allows for efficiently estimating stated individual preferences while also accounting for heterogeneity. We apply BACE to measure preferences for public policies and shed light on welfare analysis using the MVPF approach. We plan to build on the emerging line of work at the frontier of public economics for analyzing welfare using the MVPF in two critical ways. First, we substantially expand the scope of the MVPF framework by showing how BACE can be used to directly elicit individual-level WTPs for public policies; we contribute new estimates of the MVPF for important redistribution policies for bottom and middle-income groups in five countries. Second, we introduce the consideration of interactions among different social programs, considering the possibility of program substitution or complementarities. Using survey methods, our approach will yield willingness-to-pay estimates for a range of public policies (disability insurance, unemployment insurance, social security, and parental leave) across five countries (Austria, Italy, Sweden, the United Kingdom, and the United States).

In terms of future research, I also plan to expand the line of research on program interactions by carrying out a comprehensive analysis of program substitution patterns across different social programs that support workers as they move in and out of the labor force. Using Italian administrative data, I plan to follow the methodology in Hendren and Sprung-Keyser (2020) and pull together several policy changes in the last 20 years and conduct a comparative analysis across programs. The

final goal of the analysis is to provide extensive evidence on what explains the variation in patterns of program substitution across people and policies and on how changes in the policy space affect the take-up of welfare programs as well as other individual decisions such as labor force participation, retirement, and transitions in and out of self-employment. Theoretically, I plan to explicitly incorporate program interactions in the welfare analysis using the MVPF framework.

Within this strand of research, I also specifically focus on how behavioral responses affect the effectiveness of different government programs. In my paper "*Firm Responses to Earned Income Tax Credit: Evidence from Italy*", I explore behavioral responses to the introduction of a large income EITC program in Italy. Using matched employer-employee administrative data, I study the incidence effects of the program and find that, contrary to the prediction of the standard competitive model, the annual earnings of the recipients of the tax credit do not decrease after the introduction of the program. I find that earnings of eligible workers grow at a slower rate relative to similar non-eligible workers. This finding suggests that firms might respond to the introduction of the program by adjusting earnings growth. This type of response is plausible in a setting where institutional or norm-based wage rigidity prevents firms from adjusting the level of wages directly and highlights the possibility that incidence may be shifted from workers to firms in a dynamic way.

## 2 Gender Inequality: Determinants and Policy Solutions

The second strand of my research studies the determinants and potential policy solutions to gender inequality in the labor market. By disentangling the effects of different policy instruments, my JMP contributes to this research area by expanding our understanding of whether job-protected and paid leave policies have desired consequences for gender inequality in labor-market outcomes.

On the determinants' side, in an ongoing project joint with Martha Bailey and Emily Oster, "*Breastfeeding, 1950 to 2015: Trends, Selection and Labor Force Participation*", we document patterns of breastfeeding behavior in the US over the period from 1950 to 2015. We evaluate how breastfeeding rates evolve with health recommendations and changes in policy and document shifts in the demographic patterns of breastfeeding over this period. Our primary innovation is to bring a comprehensive set of data to this topic, assembling six datasets to provide a time series dataset from the 1950s to the present. We document declining breastfeeding rates from the 1950s through the 1970s and then increases from the 1970s to the present. Breastfeeding initiation rates at present are higher than at any point since 1950. We further document that longer-term breastfeeding (through 6 months) parallels this until the most recent period when initiation rates have continued to rise while continuation has stagnated. Higher socioeconomic status groups adopted breastfeeding more

quickly in the 1970s, with other groups catching up through the 1980s to the present. Notably, these groups have not shown the same catch-up in the continuation of breastfeeding. Breastfeeding through six months remains highly selected towards women with more education, for example. We plan to complement these findings by studying the relationship between breastfeeding, maternity leave, and women's employment in order to relate these selections patterns to inequalities in the labor market.

I also plan to expand this line of research by carrying out a comprehensive analysis of the take-up and impact of family policies and investigate their effectiveness at reducing gender inequality in the labor market by redistributing child penalties across workers of different genders and their firms and changing social norms in the household and in the firm. In a joint project with Sara Spaziani, "*Redistributing Childcare Costs through Family Policies: the Role of Social Norms and Shocks to Family Arrangements*", we make use of the different environments in which policies are implemented to study what are the factors that influence the take-up of family policies intending to isolate the role of three main factors: peer effects, the role of employers and shocks to families' work-life balance and social norms. We will take advantage of the conditions created by Covid-19 pandemic to analyze changes in these policies' capacity to produce effects. The Covid-19 pandemic has generated an unprecedented demand for childcare services, and despite most of the additional burden of childcare falling on the shoulders of mothers, fathers' involvement in childcare responsibilities increased as never before under these exceptional circumstances. The pandemic thus provides a unique setting to study changes in behaviors and social norms with potentially long-lasting effects.

## 3 The Effects of Migration

My third strand of research has focused on the effects of migration from a public economics perspective. In ongoing work with Diego Verdugo, "*The Effect of Physician Migration on Health Outcomes*", we ask whether foreign migration can help address these shortages by exploiting a sudden and arguably exogenous wave of physician migration from Venezuela to Chile, starting in 2015. We build a novel dataset on the universe of physicians working in the public sector in Chile to study the effect of this physician supply shock on health outcomes, health care access and crowd-out of Chilean physicians from the public to the private sector. We present descriptive evidence on how the shock propagated through the health care system. Using an event study design and an instrumental variable strategy we find that, in hospitals and areas most affected, overall mortality decreases right around the time of the inflow of new physicians by around 0.2 percentage points. We perform an heterogeneity analysis looking at mortality from different causes. In future work, we plan to

test different explanations for this result: decreases in waiting times, increases in the availability of specialists and faster diagnoses.

In a separate project joint with Giulia Buccione, *"Welcome to the Neighborhood? Evidence from the Refugees' Reception System in Italy",* we study the massive refugee inflows in Europe between 2014 and 2017. Local reception systems had to adapt, opening new emergency reception centers to host refugees. This phenomenon has prompted an ongoing debate on the impact of forced displacement on host countries' economic and social outcomes. We exploit the unique setting provided by the Italian refugee reception system to study the effect of refugee inflows on housing prices, arguing that they reflect changes in natives' perceptions toward refugees. Using administrative data on the exact location of reception centers and a dynamic event study design, we find that, after the opening of a reception center, areas close to the center experience a relative fall in housing prices of about 1%. The effect is mainly driven by larger cities and is decreasing with the size of the center and the center offering services to facilitate integration. We plan to expand the analysis by testing whether local public spending is affected by refugees inflows, since refugees represent a shock to the homogeneity of the community of reference.

# How Should We Design Parental Leave Policies? Evidence from Two Reforms in Italy

Valeria Zurla*

November 2021

*Job Market Paper*

This paper is updated frequently. Click here for the latest version

**Abstract**

This paper studies the role of different policy instruments in the design of parental leave policies. Using Italian administrative data on the universe of working mothers, I implement a difference-in-differences design around two unemployment insurance reforms that increased, respectively, the level of benefits and the duration of benefits without offering job protection. I provide novel insights on the trade-offs that mothers face in making leave decisions, the relative value of benefits and job protection, and the incentive costs associated with parental leave policies. Both reforms increased separations from the pre-birth employer and delayed mothers' return to work. I estimate the costs of changing the generosity of unprotected benefits in terms of earnings, labor force participation, and benefits from other social programs. Taking up unprotected benefits has an enormous cost in terms of foregone earnings for mothers, suggesting that the insurance value of short-term benefits is much higher than the value of job protection. I explore the role of informational frictions and childcare availability in shaping mothers' leave decisions. I develop a conceptual framework to evaluate the welfare effects of parental leave policies. The analysis demonstrates job protection's key role in reducing the incentive costs of parental leave policies while showing that mothers highly value insurance in the short term. Increasing the duration of benefits while at the same time extending job protection is welfare improving for mothers.

*Brown University, Department of Economics. Email: valeria_zurla@brown.edu. I am grateful to John Friedman, Emily Oster and Neil Thakral for their guidance and advice. This study uses the anonymous data from the Italian Institute of Social Security (INPS). Data access was provided as part of the Visitinps Scholars Initiative. I thank the staff of Direzione Centrale Studi e Ricerche at INPS for their invaluable support with the data and the institutional setting. This project benefited from discussions with Anna Aizer, Nicola Babarcich, Giulia Buccione, Edoardo Di Porto, Maor Milgrom, Pablo Garriga, Marco Stenborg Petterson, Jon Roth, Fabiano Schivardi, Jesse Shapiro, Sara Spaziani, Bryce Steinberg and seminar participants at Brown and INPS. Financial support from PSTC International Fellowship, the VisitINPS fellowship and the Policy Impact Early Career grant is gratefully acknowledged. The findings and conclusions expressed here are solely those of the author and do not represent the views of INPS.

# 1 Introduction

Despite women narrowing the gap with men in labor force participation and surpassing them in educational attainment, gender differences in labor market outcomes persist. Recent research shows that the bulk of labor market gender inequality can be attributed to the unequal impacts of parenthood on men and women (Kleven, Landais, and Søgaard 2019; Kleven et al. 2020). This fact has spurred increasing interest in how government interventions can mitigate the adverse effects of childbirth on women's labor market outcomes. Parental leave programs are among these interventions; they have the double objective of insuring women against income losses when temporarily out of work and keeping them engaged in the labor force.

Evidence on how to *optimally* design leave policies is, however, scarce. Parental leave policies are a bundle of three core features: the duration of benefits, the replacement rate of benefits, and the provision of job protection. While a sizable literature has studied partial elements of the parental leave system (Dahl et al., 2016; Kleven et al., 2020; Lalive et al., 2014; Schönberg and Ludsteck, 2014; Raute, 2019), we still lack a general understanding of how the effectiveness of parental leave policies depends on these different design features. Isolating the effects of these different policy instruments is challenging as most policies change several components at once.

Understanding how different policy instruments address the incentive-insurance trade-off is crucial to evaluating welfare consequences. Providing longer benefits without offering job protection can lead to utility gains for women by insuring them for longer in a period of vulnerability. However, it can also have detrimental effects on labor market attachment, which could exacerbate gender differences and increase the fiscal costs associated with the reform.

In this paper, I study the effects of providing *increased benefits without job protection* on women's decisions to return to work, labor market outcomes, and welfare. I do so by taking advantage of the uniqueness of the Italian social insurance network that grants special unemployment insurance eligibility to women who decide to quit their jobs before their child's first birthday. After childbirth, Italian mothers are compelled to take maternity leave which ends three months after birth. Once that ends, women can choose to take up the standard parental leave program, which replaces pre-birth earnings at a 30% rate for six months, or go back to work and arrange for childcare. The special unemployment insurance eligibility adds an option and effectively acts as *paid parental leave* without the critical element of job protection.

I consider two different policy experiments that exogenously changed the generosity of unemployment insurance benefits keeping job protection constant. The first reform, introduced in 2013,

increased the replacement rate of unemployment insurance from 60 to 75%. The second reform, introduced in 2015, increased the duration of unemployment insurance from 8 months to a maximum of 24 months. Both reforms changed the generosity of the transfers that women could receive after childbirth without offering job protection. The use of variation in unemployment insurance allows isolating the impact of benefits generosity from job protection, overcoming one of the main challenges in the literature, as benefits and job protection are usually tied together in parental leave policies[1].

Studying how these changes affect both women's choices in the short run and their labor force attachment in the long run provides an opportunity to disentangle the effects of transfers from job protection on the incentive-insurance trade-off. Moreover, by exploiting two different reforms that changed, respectively, the level of benefits and the duration of benefits, this setting provides an opportunity to study the differential welfare effect of different policy instruments.

Using novel and rich administrative data on the universe of mothers working in the private sector before childbirth provided by the Italian Social Security Institute (*INPS*), I analyze the effects of the reforms on separations from pre-birth employers, decisions to return to work, take-up of other social programs and long-term labor market outcomes. I implement a difference-in-differences design that compares birth cohorts differentially exposed to the unemployment insurance reforms, before and after childbirth. The treatment and control groups are defined as a function of the end of the compulsory maternity leave date. Identification of the effects comes from comparing the evolution of outcomes after childbirth of cohorts of mothers who gave birth in the year of the reform to cohorts of mothers who gave birth in the year before.

The empirical analysis provides novel insights into trade-offs that mothers face in making parental leave decisions, the insurance value of short-term benefits relative to job protection after childbirth, and the incentive costs associated with parental leave benefits, particularly with forgoing job protection. I find that increasing the generosity of unprotected benefits generates significant behavioral responses: women are more likely to take up unemployment insurance and forgo job protection for enhanced benefits, which results in a significant increase in separations from their pre-birth employer. Increasing the *replacement rate* of unprotected benefits by 10% increases separations by 4%. This increase mostly comes from a crowding-out of parental leave benefits suggesting that the insurance value of increasing the level of benefits is the highest right at the end of maternity leave. Increasing the *duration* of unprotected benefits by 10% increases resignations by almost 2%. While the increase

[1] Baker and Milligan (2008); Dahl et al. (2016); Kleven et al. (2020); Lalive et al. (2014); Schönberg and Ludsteck (2014); Tô (2018); Raute (2019)

in unemployment insurance take-up partially comes from crowding-out of parental leave benefits, most of the behavioral responses are concentrated around the time of parental leave exhaustion. This suggests that the increase in the *duration* of unemployment insurance allows marginal mothers to move closer to their optimal choice of paid leave and extend the subsidized time they spend not working. Overall, both reforms significantly delay women's return to work.

I find evidence that mothers are willing to give up the certainty of returning to their pre-birth employer for more generous benefits in the short run. What are the implications of giving up job protection in terms of labor market outcomes? This question is crucial to evaluate the incentives effects of the reforms in terms of fiscal externalities and the private trade-off that mothers face when making their leave decisions. Both reforms have a negative and significant effect on labor force participation in the medium and long run. Mothers exposed to higher unprotected benefits are 1% less likely to work four years after compulsory maternity leave. Mothers exposed to more extended unprotected benefits are less likely to work in the medium run, but the effect almost disappears after four years. Both reforms have a negative and significant effect on earnings, with child penalties in earnings increasing by around 4% one year after the end of maternity leave and the negative effects persisting up to four years after childbirth. The results suggest that while taking up unprotected benefits brought mothers greater financial security in the short run, it drove long-lasting declines in employment and earnings, most of which occur after the UI benefits expired. The analysis highlights the crucial role of job protection in reducing the incentive costs associated with parental leave reforms and keeping women attached to the labor force.

The quantitative nature of the private trade-off that women face when making parental leave choices is informative of how much they are willing to give up in terms of future earnings for an increase in benefit level or duration in the short run. I find that marginal women forgo an enormous amount of future labor earnings (the cost of losing job protection) to gain additional short-term benefits. Increasing the *replacement rate* of benefits yields an average net benefit increase of 5,040 EUR for mothers who take this up; the cost of this is 31,000 EUR in foregone future earnings. Increasing the *length* of benefits, yields an extra 14,400 EUR per mother; the cost of this is 36,000 EUR in foregone future earnings. These estimates imply that the perceived insurance value of short-term benefits is exceptionally high for marginal mothers relative to the value of job protection.

Are these effects a reasonable estimate of the relative value of job protection and short-term benefits? From a financial perspective, assuming that mothers are forward-looking and have rational expectations, this trade-off requires new mothers to use an extremely high discount rate. However, information frictions may make it difficult for mothers to trade off short-term benefits and future

earnings losses and internalize the earnings losses associated with their leave-taking decisions. An alternative economic explanation is that these women face incredibly high costs of work in the short term, combined with a lack of liquidity to support themselves without working.

I probe the significant effects and valuation of unprotected benefits by performing a heterogeneity analysis to clarify the drivers of behavioral responses and labor market effects. I perform a simple complier analysis and find that compliers to the replacement rate reform are on average younger, less likely to work full-time, more likely to work in "low-quality" establishments, and earn less than both always and never-takers. They are also significantly less likely to work in regions with a high unemployment rate. These findings are overall in line with the idea that individuals induced to take up UI by the replacement rate reform are less likely to value job protection and more likely to associate high insurance value to short-term benefits. Increases in unemployment duration are valued by a broader population of mothers who are less likely to be identified off their pre-birth characteristics in a clear-cut way. The one characteristic that stands out is childcare availability. Increases in UI duration are highly valued by mothers who do not have access to formal childcare options. The high cost of childcare is a plausible explanation for why women are willing to give up future earnings for more extended benefits. In regions with relatively high levels of childcare availability, women are only willing to forgo 13,666 EUR in future earnings, compared with 40,950 EUR in regions with relatively low availability of childcare.

In the last part of the analysis, I evaluate the welfare effects of the two reforms. While the two reforms analyzed here were not targeted to mothers, they are helpful to estimate the welfare effects of directional changes to benefit levels or duration while keeping the other relevant parameters fixed. I develop a conceptual framework to evaluate the welfare effects of changes to parental leave policies in terms of empirically measurable sufficient statistics. I apply a simple model based on Baily (1978); Chetty (2008); Schmieder, Von Wachter, and Bender (2012); Schmieder and Von Wachter (2016) to parental leave policies to study the welfare effects of benefit levels and benefit durations and how different policy instruments address the incentive-insurance trade-off. Using the estimated costs and benefits from the analysis above, I derive the Marginal Value of Public Funds (MVPF, Hendren and Sprung-Keyser, 2020) associated with each of the two changes in unemployment insurance benefits. I find that both reforms are associated with a MVPF lower than 1, reflecting that providing parental leave benefits is very costly per euro spent if it does not come with job protection. Interestingly, the MVPF of the duration reform is higher (0.7) than the MVPF of the replacement rate reform (0.5): the duration reform has a higher insurance value for mothers and, in relative terms, a lower impact on cumulative earnings.

Given the significant losses in long-term earnings that mothers are willing to incur to get higher short-term benefits, it is not evident that mothers are making privately optimal decisions. Mothers might not perfectly forecast the effects of their leave decisions on labor market outcomes or may be myopic. I consider this possibility and calculate the MVPF of the policy changes when mothers are not privately optimizing, and it is impossible to rely on the envelope theorem. Considering the extreme case in which marginal mothers fail entirely to consider the earnings losses from taking up unprotected benefits, I find that the MVPF for the replacement rate reform decreases to -1.14 and 0.06 for the duration reform.

Overall, the analysis demonstrates the critical role of job protection in reducing the fiscal externalities of parental leave benefits. It also shows that mothers assign a very high value to insurance in the short term. In terms of policy implications, I find that extending the duration or level of benefits while at the same time extending job protection is welfare-improving for mothers. Alternatively, addressing the market failure behind the low availability of childcare, provided that costs are not too high, is also likely to be welfare-improving by helping new mothers return to the labor force sooner. Second, extending the duration of benefits is preferable in terms of welfare than increasing the level of benefits. Third, leave policies that push for more extended benefits should consider the high costs in terms of earnings losses that they potentially generate. Finally, the evidence shows that mothers do not internalize the career costs of their leave choices when making decisions, and policy-makers should consider this when designing leave policies.

This paper contributes to different strands of literature. First, it contributes to the rich literature on the effects of leave policies on employment and earnings (Dahl et al., 2016; Olivetti and Petrongolo, 2017; Kleven, Landais, and Søgaard, 2019; Kleven et al., 2020). By disentangling the effects of different policy instruments, this paper contributes to our understanding of whether job protected and paid leave entitlements have desired consequences for gender inequality in labor market outcomes. This is especially important given recent research showing persistent gender differences in the labor market, mainly attributable to differential impacts of childbirth. Most of the papers analyzing parental leave policies find no effects on labor market outcomes in the long run (Baker and Milligan, 2008; Dahl et al., 2016; Kleven et al., 2020), suggesting that these policies have no impact on gender inequality. The results of this paper offer a different assessment and find that offering benefits without job protection around childbirth significantly increases child penalties in the labor market[2].

[2]This is consistent with the results in Schönberg and Ludsteck (2014) that find that one reform that extended cash benefits beyond job protection lowered maternal employment through the first six years after childbirth.

Secondly, this paper contributes to the literature on *how* to design parental leave policies (Lalive et al., 2014; Schönberg and Ludsteck, 2014; Jørgensen and Søgaard, 2021; Ginja, Jans, and Karimi, 2020). It is closest in spirit to Lalive et al. (2014) and Schönberg and Ludsteck (2014), which study the relative importance of job protection and duration of cash benefits for parental leave policies in Austria and Germany. The first contribution of this paper to this literature is to analyze the importance of the level of benefits in addition to the duration of benefits. Secondly, using a revealed preferences approach, it provides novel insights into the trade-offs that mothers face in making their parental leave decisions, the value that mothers assign to different elements of parental leave policies, and the incentive costs associated with parental leave benefits. By investigating how the relative value of benefits and job protection changes with different reforms, this paper provides one of the first welfare assessments of parental leave policies[3]. Using the MVPF framework, I can compare the welfare effects of increasing the benefit levels and the benefit duration of leave policies, including the possibility that information frictions may make it difficult for mothers to trade-off between short-term benefits and future earnings losses and internalize the earnings losses associated with their leave-taking decisions. I directly consider the case in which women may not be forward-looking and may not make optimal decisions.

Finally, this paper contributes to the recent and growing literature on program interactions and their implications for individual outcomes and welfare. While there is increasing evidence on the importance of the interaction between unemployment and disability insurance programs (Inderbitzin, Staubli, and Zweimüller, 2016; Borghans, Gielen, and Luttmer, 2014; Lawson, 2017; Leung and O'Leary, 2019), evidence in other public policy contexts has been scarce. I provide novel evidence of the interaction between parental leave and unemployment insurance and highlight the importance of accounting for program substitution when thinking about how to optimally design parental leave policies. This paper clearly shows that substitution between programs is an important margin of adjustment in mothers' behavioral responses to policy changes and that accounting for the consequent fiscal externalities is critical in designing optimal social insurance policies. More generally, one should ideally analyze social insurance and tax policies in a unified framework rather than optimizing each program separately (Chetty and Finkelstein, 2013).

The paper is structured as follows. Section 2 describes the institutional background and the conceptual framework. Section 3 describes the data used in the empirical analysis and presents the empirical strategy. Estimates of the behavioral responses to the changes in generosity of unprotected benefits are presented in Section 4. Section 5 estimates the costs of the changes in generosity of

[3] Jørgensen and Søgaard (2021) study the behavioral effects and efficiency cost of earmarked parental leave policies.

unprotected benefits in terms of labor market outcomes. Section 6 documents heterogeneity in responses and explores potential mechanisms. Section 7 performs welfare analysis and explores the policy implications of the findings. Section 8 concludes.

# 2 Institutional Background and Conceptual Framework

## 2.1 Institutional Background

In Italy, social insurance for working mothers is divided into a period of compulsory maternity leave and a period of parental leave. Compulsory maternity leave (ML) starts two months before the estimated date of birth of the child and lasts for five months[4]. During the compulsory maternity leave period, mothers cannot work and get a government transfer that replaces 80%[5] of the average daily wage in the last month before the start of the compulsory maternity leave.

After the compulsory maternity leave, mothers are entitled to optional parental leave (PL). Each household is entitled to 10 months of optional parental leave, which can be used up to the child's 8th birthday. The first six months are paid at a 30% replacement rate if used before the child's 6th birthday, while the remaining months are unpaid. Each parent can take up at most six months of parental leave. If the father uses at least three months of PL, the household is entitled to an additional month (for a total of 11 months). While this clause was introduced to increase the take-up of parental leave by fathers, parental leave is used almost exclusively by mothers, representing over 90% of the beneficiaries of paid parental leave benefits. Moreover, paid parental leave benefits are usually exhausted in the first six months after birth.

Between 300 days before the expected birth date and the child's first birthday, mothers are entitled to *job protection*, irrespective of whether they take up parental leave or not. During that period, they cannot be fired and have the right to maintain the same pre-birth working conditions in terms of role, tasks and, salary.

In addition to the maternity leave and parental leave benefits, the Italian social insurance system grants special unemployment insurance eligibility to women who *voluntarily* resign before their child's first birthday. This is a crucial exception to the unemployment insurance (UI) system, as in ordinary circumstances, workers who quit their jobs are not entitled to UI benefits. This provision implies

[4]The start of ML can be anticipated if the mothers' occupation is deemed risky for the pregnancy or if the pregnancy is considered at risk. In these cases, the compulsory ML lasts more than five months. Also, women are entitled to some degree of flexibility regarding the start date of the compulsory maternity leave: they can also choose to use one month of leave before the estimated date of birth and the remaining four months afterwards.

[5]Some CBAs dictate that the employer is responsible for integrating the difference between the transfer from the social security institute and the pre-birth wage so that the replacement rate is effectively 100% in many cases.

that the unemployment insurance program can effectively work as *paid unprotected leave*: if a woman decides to resign before her child's first birthday, she loses the right to job protection, but she receives cash transfers from the government in the form of unemployment insurance. In recent years, this option has become popular and has been advertised as an alternative (to use as a substitute or complement) to standard parental leave policies (Figure A1).

I exploit this peculiar feature of the Italian system to study the effects of increasing the level and duration of benefits without offering job protection. I take advantage of two unemployment insurance reforms that changed the generosity of benefits and thus changed the policy choices available to women after compulsory maternity leave. Figure 1 shows the timeline of women's decisions. I focus on mothers' choices between $t = 3$ and $t = 12$, the "choice period", where mothers can choose between returning to work, taking up PL, taking up UI, or taking up PL and UI in combination. The analysis investigates how providing different incentives to mothers' in the time window right after childbirth impacts the incentive-insurance trade-off of parental leave policies.

**The Unemployment Insurance Reforms of 2013 and 2015** Until 2012, the main unemployment insurance subsidy available to Italian workers was the Ordinary Unemployment with Normal Requirement (*Disoccupazione Ordinaria a Requisiti Normali, OUNR*). This UI subsidy covered all employees in the non-agricultural sector. Age at layoff entirely determined the potential benefit duration with a threshold mechanism: workers fired before turning 50 were eligible for 8 months of unemployment benefits, while workers fired after turning 50 received up to 12 months of subsidy. The amount of subsidy was proportional to the average wage in the three months preceding the layoff. Workers received 60% of their average wage for the first six months of the subsidy, 50% for the following two months, and 40% for the remaining four months, if still eligible. The amount of subsidy was capped by law, and the threshold changed every year. To be eligible, workers needed to meet two main requirements: the worker should have contributed for the first time to social security at least two years before the start of the unemployment benefit spell; the worker should have worked for at least 52 weeks in the last two calendar years. The relative strictness of these requirements implied that not all workers were eligible for UI after a separation.

In 2013, the unemployment insurance system was completely reformed with the primary goal of increasing coverage. The reform affected all terminations after January 1st, 2013, and it brought about two main changes. First, the *Aspi* subsidy substituted the *OUNR*: while the eligibility requirements were constant, the initial replacement rate increased from 60 to 75%. The potential benefit duration of the subsidy was increased for older workers but remained constant to 8 months

for our sample of interest (mothers who just gave birth, who are presumably younger than 50 years old). Secondly, the replacement rate reform introduced an additional UI subsidy, the *MiniAspi,* with less strict eligibility criteria: to be eligible, workers needed only 13 weeks of contribution in the year before the start of the unemployment spell. The potential benefit duration of the *Miniaspi* was equal to half of the weeks of contribution in the last year, and the replacement rate was 75%. Figure 3 shows the changes in UI generosity after the replacement rate reform. Panel A reports the average daily UI benefit by month-of-end-of-termination bin for women less than 50 years old with a permanent contract around the introduction of the replacement rate reform. It shows that, after the reform, the daily benefit increased by around 4 EUR. Panel B reports the average potential unemployment insurance duration before and after the replacement rate reform and confirms that the reform did not change the duration of the UI subsidy. Figure A2 shows that the introduction of the *Miniaspi* resulted in around 300,000 extra claims, an increase of 50% relative to the baseline before the reform.

Not long after the replacement rate reform, the unemployment insurance system was reformed again. In 2015, the government combined the *Aspi* and *MiniAspi* subsidies into one insurance subsidy, the so-called *Naspi*. The reform kept the initial replacement rate unchanged while modifying the rules determining the potential benefit duration, making it dependent on the number of weeks of contribution in the four years before the start of the UI spell. The potential benefit duration was equal to half of the weeks of contribution in the four years before the start of the UI spell, with a maximum of 2 years. This implied an average increase in potential benefit duration, particularly for workers with relatively long and stable contribution histories. Panel C of Figure 3 shows that the average daily UI benefit by month-of-end-of-termination bin did not significantly change after the duration reform. Panel D clearly shows that the reform had a significant impact on duration. The average potential unemployment insurance duration for women younger than 50 increased by around 250 days, jumping to around 500 days. This corresponds to a 100% increase in average potential duration. Figure A8 shows the distribution of potential benefit duration for mothers who decided to use unemployment insurance after the duration reform: on average, the reform increased the potential benefit duration from 8 months to around 16 months.

**Child Care Availability** When thinking about mothers' return to work choices around childbirth, it is not possible to abstract from the role played by institutional child care. Formal child care in Italy is divided into nursery care (*Asili Nido*), covering children from 0 to 2 years old, and pre-school care (*Scuola dell'Infanzia*) for 3 to 5 years old children. Pre-school care is prevalently public

and regulated by the Ministry of Education. Pre-schools are considered the first level of the school system: they are almost free (except for a relatively low lunch fee) and broadly available. Indeed, they are used by over 95% of 3-5 years old children. While pre-school availability and attendance are almost universal, the situation is significantly different for nursery schools in short supply and considerably more expensive than pre-schools. As reported in Carta and Rizzica (2018), in Italy, a family composed of two working adults and one child with a yearly household income of 44,200 EUR pays on average 311 EUR per month for a full-time seat in a *public* nursery (CittadinanzaAttiva, 2015). *Private* nurseries, which in some regions outnumber public ones, cost around 487 EUR per month (IstitutoDegliInnocenti, 2011). Together, public and private nurseries accommodate only 24% of children from 0 to 2 years old, with public nurseries accounting for less than half of this share. There is pronounced heterogeneity at the regional level, with some regions able to accommodate less than 10% of children in nursery care.

### 2.2 Conceptual Framework

A parental leave benefit system consists of the following policy parameters: *(i)* the benefit level identified by a replacement rate as a share of pre-birth earnings $b = \alpha w_0$, *(ii)* the duration of benefits, i.e. the number of months cash benefits last $B$, *(iii)* the duration of job protection $P$, i.e. the number of months mothers can return to their pre-birth job at wage $w_0$.

As illustrated in Section 2.1, in the Italian social insurance system, unemployment insurance is effectively a paid parental leave without job protection since it provides cash benefits $b$, for a period of time $B$. This feature allows to consider the unemployment insurance reforms as policy changes to one design feature of parental leave policies ($b$ or $B$), keeping constant the other parameters.

I now consider the expected effects of changes in $db$ and $dB$ and highlight the trade-off mothers face between different options. Since mothers can choose between unprotected benefits, protected benefits, and returning to work, the setting provides an opportunity to study mothers' preferences for different policy instruments.

The two reforms affect mothers' set of choices in the "choice period", as illustrated in Figure 1. At the end of their compulsory maternity leave at $t = 3$, mothers can:

1. Go back to work at $t = 3$, earn their pre-birth wage $w_0$ and arrange for childcare formally or informally, with a cost $c_t$, decreasing over time

2. Take up parental leave, which pays a monthly benefit equal to $b_{PL} = \alpha_{PL} w_0$, where $\alpha_{PL}$ is the replacement rate, equal to 30%. Parental leave lasts for a number of period $B_{PL} = 6$.

Mothers are entitled to job protection and can go back to their pre-birth job after exhausting PL benefits

3. Quit their job and take up unemployment insurance. Unemployment insurance pays a monthly benefit $b_{UI} = \alpha_{UI} w_0$, where $\alpha_{UI} > \alpha_{PL}$ and lasts for $B_{UI}$ months but is associated with a cost $e$, which can be interpreted as the search cost to find a new job when UI benefits are exhausted due to the absence of job protection

Note that mothers can also take up parental leave and then take up UI at $t = 9$, receiving benefits for $B_{PL} + B_{UI}$ months. To simplify, I assume that mothers who decide to take up UI either do so at $t = 3$ or at $t = 9$, so either completely substitute between UI and PL or complement by exhausting their PL benefits first. This assumption is aligned with the patterns observed empirically (Figure A3). Figure 2 describe how the reforms changed the choice set of women after the end of their compulsory maternity leave.

Both reforms increased the generosity of UI making it more attractive for women to separate from their pre-birth employers either at $t = 3$ or at $t = 9$. Assume that each mother has utility $u_{it}(c, z)$ where $c \in \{w, pl, ui\}$ indexes the choice at the end of the maternity leave and $z \in \{0, 1\}$ indexes eligibility status for more generous UI. Mother $i$ chooses the $c$ that maximizes her utility, before the reform ($z = 0$):

$$C_{it}(0) = argmax_{c \in \{w,pl,ui\}} u_{it}(c, 0)$$

For the set of mothers who switch to UI because of the reform ($C_{it}(0) \neq ui$ and $C_{it}(1) = ui$, i.e. the compliers), I know, by revealed preferences, that $u_{it}(ui, 1) > u_{it}(C_{it}(0), 1)$. It must be that the utility of choosing UI after the reform is higher than the utility of the pre-reform option.

As an example, consider mothers who, because of the reform, decide to take up UI at $t = 3$ and, in the absence of the reform, would have taken up PL. By revealed preferences, $u_{it}(ui, 1) > u_{it}(pl, 1)$. This implies that:

$$\begin{aligned} B_{UI}\alpha_{UI} w_0 - e &> B_{PL}\alpha_{PL} w_0 + JP \\ (B_{UI}\alpha_{UI} - B_{PL}\alpha_{PL})\, w_0 - JP &> e \end{aligned} \tag{1}$$

where $e$ is the cost of taking up UI, that can take a very general form and be interpreted, for example, as the losses in future earnings due to the take up of UI. $JP$ is the value of having job protection, or more generally having the possibility of going back to their pre-birth employer in terms

for example of future earnings gains. The additional insurance value of unemployment benefits must be greater than the value of job protection as long as $e > 0$. In this case, women's *private trade-off* is between higher short-term benefits and job protection. If we assume that mothers are forward-looking and have rational expectations, the cost of taking up UI $e$ can be used to infer the relative value that women assign to benefits and job protection

Similarly, by revealed preferences, mothers who, because of the reform, decide to take up UI at $t = 9$ instead of going back to work must value more the increase in short-term benefits than the possibility of going back to their pre-birth employer, taking into account the cost associated with unprotected benefits.

In the next sections, I use the behavioral responses to the reforms to estimate the relative value that women assign to benefits and job protection.

# 3 Data and Empirical Strategy

## 3.1 Data

I use novel, confidential administrative data from the Italian Social Security Institute (*INPS*) on the universe of private-sector employees. My primary data source is matched employer-employee records at the monthly level for the period 2009-2019. For each worker-firm record, the following information is available: beginning and end date of the contract, alongside the underlying motivation for termination (e.g., layoff, resignation); type of contract (permanent vs. temporary, full-time vs. part-time); broad occupation group (blue-collar, white-collar or manager); earnings, wage and number of days worked, and a unique firm and worker identifier. I link these records to workers' and firms' registers containing baseline information, such as gender and age of employees and opening date, sector, and location of businesses.

Importantly, these records also contain information on any event that might lead to the entitlement to a benefit for a given worker at the monthly level. These events include, among others, maternity leave, parental leave, and sick leave. I identify maternity and parental leave events, supplementing the information on events from the employer-employee records with information from the registers on maternity leave and parental leave applications. These records contain more detailed information about maternity and parental leaves, such as the exact beginning and end date of each leave period, the date of application, the expected birth date and the actual birth date of the child, the type of leave (e.g., standard or anticipated for medical reasons in the case of maternity leave applications), a child identifier and spouse identifier. This level of detail allows me to pinpoint the

exact leave periods and the exact beginning and end of job protection[6].

I link these records to information on unemployment insurance claims from the *Sistema Percettori* database (*SIP*), which collects information on the universe of income support measures administered by INPS as a consequence of job separations. I observe the scheme type (e.g., *Aspi* vs *Naspi*), its start date, potential duration, actual duration, and total amount paid for every claim.

I build up a panel of mothers' working and benefit histories at monthly frequency by combining the archives described above. The final dataset is a balanced[7] panel containing all mothers who gave birth to their *first* child (parity one) between 2012 and 2016. The panel spans from 2 years before the end of their compulsory maternity leave to 4 years after and comprises approximately 640,000 mothers employed in the private sector before giving birth. I focus on mothers giving birth to their *first* child for a number of reasons. First, the labour market history of first-time mothers before birth is more informative about their skills and earnings capacity than for higher-parity mothers. Second, the decision of whether to take up unemployment insurance or parental leave and in general the return-to-work decision is likely to be influenced by total fertility as there might be selection of higher-parity mothers into participating in the labor market. Lastly, unemployment insurance eligibility is likely to be higher for parity one mothers since they are more likely to have worked continuously prior to giving birth. Parity one mothers are also less likely than higher-parity mothers to have experienced significant periods of interruptions from work before childbirth. My primary sample also excludes mothers whose temporary contract expires around the end of their maternity leave. This is because mothers with temporary contracts are not entitled to job protection and therefore face a different set of incentives than other mothers[8].

Table A3 reports the mean and standard deviation of a set of individual characteristics for the main sample. Mothers working in the private sector and giving birth to their first child in the sample period are on average 33 years old and have consistently worked for their pre-birth employer in the two years before giving birth. The vast majority of them are employed with a permanent contract (93%), 65% are employed full-time, 60% are employed in white-collar occupations while 30% are employed in blue-collar occupations. Their contracted monthly wage is 1,417 EUR, and their monthly earnings are slightly higher at 1,521 EUR. Monthly earnings will be the preferred earnings measure for the rest of the analysis[9]. In terms of characteristics of firms in which they work, 40%

[6]See Appendix A for more details.

[7]Details on the construction of the dataset are reported in Appendix Section A.

[8]Details on the construction of the dataset and on the selection of the sample are reported in Appendix Section A. Results are robust to fully restricting to women with permanent contracts, as discussed in Appendix Section A.

[9]The contracted monthly wage refers to the wage that the worker would get in the absence of events that might lead to the entitlement of benefits. It does not include overtime and bonuses. The monthly earnings refer to the

of them are employed in small firms (with less than 15 employees), and 65% are employed in "high quality" firms. "High quality" firms are measured by whether the firm is above the median AKM firm fixed effects estimated from AKM two-way fixed effects regressions (Abowd, Kramarz, and Margolis, 1999)[10].

### 3.2 Empirical Strategy

The main goal of the empirical analysis is to estimate the effects of having access to more generous unemployment insurance subsidies after the end of compulsory maternity leave on different outcomes of interest. I define *treatment* as having the possibility to quit and be eligible for the more generous unemployment insurance regime between the end of their compulsory maternity leave and when the child turns one year old (between $t = 3$ and $t = 12$ in Figure 1). The reforms naturally define a treatment and a control group as a function of the end of compulsory maternity leave date: mothers who based on their date of maternity leave *had the option* to resign with the more generous unemployment insurance benefits, and mothers who based on their end of maternity leave did not.

The possibility of unemployment take-up within the "choice period" needs to be taken into account when estimating the effects of the reforms. Taking the replacement rate reform as an example, while the reform was effective from January 2013 onwards, women who ended their maternity leave *before* January 2013 were eligible for the more generous UI benefit for a portion of their "choice period". This is exemplified in Table A1, which shows the eligibility status of mothers for the more generous UI regime by their end of maternity leave date and by the timing of their resignations. The table highlights that women who ended their maternity leave from January 2013 onwards were eligible for the new UI regime regardless of the timing of their resignations. On the other hand, women who ended their maternity in July 2012 were eligible for the old regime if they decided to resign at $t = 3$ but for the new UI regime if they resigned at $t = 9$. Table A2 reports the same information for the duration reform. I refer to the subsample of mothers eligible for the more generous UI regime only for a portion of their "choice period" as *partially treated* mothers. Figure A4 illustrates the three groups mothers can be divided into based on their end of maternity leave date.

While the reforms would create a natural setting for a regression discontinuity design, the pres-

actual gross monthly earnings inclusive of overtime, bonuses and adjusted for the events that happened during the month. This implies that this wage measure is not contractual but is tightly linked to how many hours and days are effectively worked during the month.

[10]When estimating AKM regressions I restrict the sample to the largest connected set of workers and firms that contains all workers that have ever been employed at one of the firms in the group and all the firms that have ever hired one of the workers in the group. Specifically, I estimate the following regression: $log(w_{it}) = \alpha_i + \psi_{J(i,t)} + \varepsilon_{it}$, where $log(w_{it})$ is the log weekly wage of worker $i$ in year $t$, $\alpha_i$ and $\psi_{J(i,t)}$ are worker and firm fixed effects. Although the analysis focuses on mothers, I consider both men and women workers in my sample period for the estimation.

ence of partially treated mothers complicates the use of this empirical strategy. As my main empirical strategy, I use a difference-in-differences approach similar to Kleven et al. (2020), that compares cohorts of mothers over time. I compare the outcomes of women fully eligible for the new UI regime to women fully ineligible based on their date of end of compulsory maternity leave (Figure A4). I estimate the following equation:

$$Y_{it} = \alpha + \sum_{k=-L}^{R} \delta_k D_{it}^k + \sum_{k=-L}^{R} \beta_k D_{it}^k \cdot T_i + \gamma T_i + \lambda_i^m + \mathbf{X_i'}\eta + \varepsilon_{it} \tag{2}$$

where $t$ denotes event time ($t = 0$ corresponds to the month of end of compulsory maternity leave)[11], $D_{it}^k$ is an event study indicator for each month relative to the month before the end of compulsory maternity leave, $\lambda_i^m$ are month of end of maternity leave fixed effects, to control for seasonality of births and $\mathbf{X_i'}$ contains a set of pre-birth individual-level controls such as age fixed effects, average earnings in the two years before the end of maternity leave and occupation. $T_i$ is an indicator for whether the mother is in the treatment group, namely for whether she has the option to resign with the more generous unemployment benefits. For the replacement rate reform, $T_i = 1$ if a mother ended her compulsory maternity leave between January and May 2013 and $T_i = 0$ if a mother ended her compulsory maternity leave between January and May 2012. For the duration reform, $T_i = 1$ if a mother ended her compulsory maternity leave between May and September 2015 and $T_i = 0$ if a mother ended her compulsory maternity leave between May and September 2014[12]. Standard errors are clustered at the individual level. Depending on the outcome considered, I estimate the specification above on different relative time windows (up to 12 or 48 months from the end of ML). I omit the event time indicator at $t = -1$ so that the coefficients $\beta_k$ measure the differential impact of the reforms in the treatment group relative to the control group with respect to the month before the end of compulsory maternity leave.

The causal interpretation of the results relies on the assumption that there is no confounding trend in outcomes by birth cohort. To validate this assumption, I consider several identification checks. First, I show that both reforms' treatment and control groups are very similar in terms of pre-birth characteristics and pre-birth labor market outcomes. Table 1 shows that the treatment and control group samples defined by the replacement rate reform are balanced across a variety of characteristics, including pre-birth age, type of contract (permanent vs. temporary, part-time vs.

[11] Figure 1 and 2 defined $t = 0$ as the month of birth of the child. Since I am interested in estimating the effects of the unemployment insurance reforms on women's choices and since the choice period for mothers starts from the end of maternity leave, from now onwards, I will define $t = 0$ as the month of end of maternity leave.

[12] All results are robust to both narrower and broader definitions of both reforms' treatment and control groups.

full-time), type of occupation, gross monthly earnings, tenure in the last two years and days worked. Table 2 reports the same balance table for the treatment and control group samples defined by the duration reform. While the characteristics are in general balanced, there are two instances in which the treatment and control groups have slightly significantly different means. Control group mothers are somewhat more likely to be full-time workers and slightly more likely to have a permanent contract. As a robustness check, I estimate specification 2 using the predicted values of my outcomes of interest calculated from a regression on observable characteristics as dependent variable.

Second, I show placebo results from specification 2 using sample of mothers who gave birth in 2009 vs. 2010, 2010 vs. 2011 and so on. The placebo results, reported below in more detail, support the identifying assumption by showing no significant differences in take-up of unemployment insurance benefits across birth cohorts.

Note that using the preceding year to a reform as the control group is common in the literature (Schönberg and Ludsteck 2014; Kleven et al. 2020). However, it is important to recognize that the identified causal effect is the effect of having been potentially exposed to the more generous UI system. For all outcomes measured at 12 months or more after birth, the control group could also become eligible for the more generous UI regime if they have another child. As a robustness check, I investigate the role of subsequent fertility by estimating the dynamic effects of the UI reforms on the restricted sample of mothers with only one child (with completed fertility of one). I also consider the effects of the reform on average completed fertility. As an additional robustness check, I also use the regression discontinuity design described in Appendix Section B as alternative empirical strategy with the primary goal of checking the robustness of the results to different specifications and to the inclusion of partially treated mothers.

Recent econometric literature has raised concerns about the validity of difference-in-differences designs rely on staggered timing for identification (De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2020; Callaway and Sant'Anna, 2020; Goodman-Bacon, 2021). My empirical strategy is unlikely to be subject to these concerns for different reasons. Firstly, my design is not staggered in calendar time, implying I do not use already-treated units as controls. Second, my design relies on a balanced panel in relative event time, where the treatment is centered at the same "relative treatment date" which allows me to estimate a conventional TWFE model controlling for group and event time fixed effects.

# 4 Behavioral Responses to Changes in Unprotected Benefits

This section analyzes mothers' behavioral responses to changes in the generosity of unprotected benefits. I document how the take-up of unemployment insurance changes when the level and duration of benefits change. Then, I take advantage of the fact that I observe how women's choices change after the reform to relate the estimated behavioral responses to incentives and investigate the value of unprotected benefits for women after childbirth.

## 4.1 What Is the Impact of Changes in Unprotected Benefits on Separations?

I start the analysis by plotting the share of women taking up unemployment insurance during their choice period by month-of-end-of-maternity-leave bin. Figure A5, Panel A reports the results for the replacement rate reform while Panel B reports the results for the duration reform. The red vertical lines indicate the month of end of maternity leave from which women become partially exposed to the more generous unemployment insurance regime (Table A1), grey points represent partially treated mothers. After both reforms, there is a clear increase in take-up of unemployment insurance, corresponding to an increase of 20% relative to the baseline mean for the replacement rate reform and to a 10% increase for the duration reform.

To understand how much of the increase in UI take-up is driven by *active choices* of women, I estimate specification 2 using as dependent variable the probability of voluntary separation[13] from pre-birth employers at relative time $t$[14]. A potential concern is that separations recorded as voluntary might reflect pressure from employers to resign rather than actual choices. I believe this is unlikely for two reasons. First, women who decide to resign around childbirth have to follow a specific process that involves an interview with the governmental office of labor where they have to declare that they are voluntarily quitting their job. Second, survey evidence suggests that the vast majority of mothers who resigned between 2010 and 2016 decided to do so voluntarily.

Figure 4 Panel A shows the dynamics of separations in the treatment group relative to the control group after the replacement rate reform. There is a significant increase in separations at $t = 0$ of around 0.25 percentage points and an additional increase of 0.18 percentage points at $t = 8$. Figure 4 Panel B reports the results for the duration reform. In this case, there is an even sharper increase in $t = 0$ of around 1 percentage point followed by an additional increase in separations between

[13] I consider a voluntary separation, resignations and all other types of separations that do not involve firing for just cause, firm closures and mass-layoffs. For robustness, Figure A10 shows the effect separately for resignations and other separations.

[14] I expect these two types of separations to reflect active choices of mothers to leave their pre-birth employer, unlike for example, separations due to firm closures or mass layoffs.

$t = 5$ and $t = 7$ of around 0.6 percentage points.

Table A4 reports the reduced-form estimates (pooled over the period $t \in [0, 12]$) scaled by the amount by which the generosity of unprotected benefits increases due to the reforms, in terms of replacement rate and duration, respectively. A one percentage point increase in the replacement rate of unprotected benefits increases separations by 0.03 percentage points. A one-month increase in benefit duration increases separations by 0.2 percentage points. To compare the effects of the two reforms, I also report the elasticity of separations with respect to benefit level $\varepsilon_b = \frac{\delta y}{\delta b}\frac{b_0}{\bar{y_0}}$ and duration $\varepsilon_B = \frac{\delta y}{\delta B}\frac{B_0}{\bar{y_0}}$, where $y$ refers to the share of separations, $b_0$ refers to the pre-reform UI benefits, $B_0$ refers to the pre-reform UI duration and $\bar{y_0}$ indicates the baseline level of separations before the reforms. According to the evidence, a 1% increase in the level of unprotected benefits increases separations by 0.4% while a 1% increase in the duration of unprotected benefits increases separations by 0.16%.

These findings indicate that women are willing to give up the certainty of returning to their pre-birth employer for more generous benefits. The level and the duration of benefits play an essential role in the trade-off between insurance and labor force attachment. The comparison between $\varepsilon_b$ and $\varepsilon_B$ suggests that increasing the generosity of benefits by increasing the duration has a smaller effect on separations than increasing the level of benefits.

## 4.2 What Is the Impact of Changes in Unprotected Benefits on Substitution Patterns?

The increase in take-up of unemployment insurance after the end of compulsory maternity leave must correspond to a decrease in one of two margins: the probability of returning to work or the probability of taking up standard parental leave. To investigate the dynamics behind the behavioral responses, I estimate specification 2 on three different dependent variables: an indicator equal to 1 if a mother is taking up unemployment insurance in relative month $t$ ($UI$), an indicator equal to 1 if a mother is on parental leave ($PL$) in relative month $t$ and an indicator equal to 1 is working in month $t$ ($Work$). I define a mother as working in month $t$ if she has positive earnings and is not on leave or unemployed.

Figure 5 shows the dynamic evolution of these three outcomes. Panel A shows the results of the replacement rate reform. The figure shows an apparent pattern: in the first six months after the end of compulsory maternity leave, the increase in UI take-up induced by the reform is compensated by a decrease in take-up of standard parental leave of similar magnitude. This evidence suggests that women who take up unemployment insurance after the reform would have taken up parental leave

in the absence of the reform. A consequence of the switch from protected benefits to unprotected benefits is that from $t = 6$ onwards, women in the treatment group are less likely to return to work. The decrease in the share of women returning to work is of the same magnitude as the decrease in take-up of PL benefits between $t \in [0, 6]$ and is partially mechanical: UI benefits have a duration of 8 months while PL benefits have a duration of 6 months. However, even after the exhaustion of UI benefits (at $t = 9$), the share of women returning to work is still significantly lower in the treatment group relative to the control group. This result highlights the importance of job protection in incentivizing women to go back to work earlier.

Figure 5 Panel B shows the same results for the duration reform. When the duration of unprotected benefits increases, the increase in UI take-up is partially compensated by a decrease in take-up of standard parental leave in $t \in [0, 6]$. However, most of the behavioral responses to the increase in unprotected benefits duration are concentrated around $t = 6$. The figure shows that the increase in duration induces a significant share of mothers in the treatment group to not return to work at $t = 6$ but to take up unemployment insurance instead. This is consistent with the evidence on separations shown in Section 4 and suggests that mothers respond to the increase in UI duration by taking up PL benefits and UI benefits in combination to maximize the total subsidized time women can spend out of the labor force.

To better understand the magnitude of the effects, Table 4 reports the coefficients $\beta_k$ from model 2 for $k = 0$, $k = 6$ and $k = 12$ on the share of women taking up UI, the share of women taking up PL, and the share of women working.

### 4.3 Interpretation and the Value of Benefits

Looking at the timing and dynamics of the estimates in Figure 4 and Figure 5 is helpful to interpret the results above and compare them across reforms. The increase in replacement rate induces women to separate from their pre-birth employers right after compulsory maternity leave and switch from parental leave benefits to unemployment insurance benefits. Following the discussion in Section 2.2, by revealed preferences, this implies that for marginal mothers, the value of the additional benefits at $t = 0$ is larger than the value of job protection. At $t = 6$, increasing the *level* of benefits without offering job protection does not impact separation rates. This suggests that increasing the *level* of benefits without offering job protection is more valuable right after compulsory maternity leave.

On the other hand, mothers respond to the increase in duration by separating at $t = 0$ and at $t = 5, 6, 7$, when parental leave benefits are exhausted. By revealed preferences, it must be that the value of additional benefit duration for marginal mothers is higher than the value of returning to their

pre-birth employer. This suggests that the increase in duration of unemployment insurance allows marginal mothers to move closer to their optimal choice of paid leave and extend the subsidized time they spend not working.

Finally, Figure 6 illustrates how the interactions between social programs affect the total subsidized time women spend not working. This is an integral part of the insurance-incentive trade-off and translates into fiscal externalities for the government. Panel A and B show that the replacement rate reform does not significantly affect the total subsidized time mothers spent out of work but only on the type of leave taken up by mothers. On the other hand, the duration reform effectively results in an increase in the total subsidized non-work time, as shown in Figure 6 Panel C and D. Despite these differences, Appendix Figure A14 shows that both reforms have a positive and significant effect on the total non-participation time after childbirth[15]. I investigate this more in detail in Section 5.

### 4.4 Robustness

As mentioned in Section 3.2, the key identification assumption for the difference-in-differences model in 2 is that there are no confounding trends in outcomes by birth cohort. In a standard difference-in-differences model, this assumption could usually be validated by looking at the evolution of pre-trends. In my specific case, it is not possible to look at pre-trends when considering UI take-up as an outcome since mothers are, by construction, employed before birth. I, therefore, take advantage of a different set of overidentifying restrictions. I perform placebo tests estimating equation 2 comparing mothers who ended their maternity leave in 2010 ($T_i = 1$) to mothers who ended their maternity leave in 2009 ($T_i = 0$), mothers who ended their maternity leave in 2011 ($T_i = 1$) to mothers who ended their maternity leave in 2010 ($T_i = 0$), and so on. Results are reported in Figure A9 and support the identifying assumption by showing no significant trend in UI take-up across birth cohorts.

Another potential concern is that the increase in resignations and separations is driven by factors other than the increase in generosity of unemployment insurance (differences in macroeconomic conditions, differences in unemployment rates, or differences in firms' behaviors). If this is the case, we would expect to see differences in separations that are not impacted by workers' decisions (layoffs, firm closures, separations for just cause[16]). Figure A12 shows that this is not the case and provides suggestive evidence that the increase in UI take-up is driven by women's choices.

[15] This is defined as the total time spent not working and not on parental leave. Using as alternative measure, the total time spent not working (including months spent on parental leave) leads to similar qualitative results.

[16] Note that these types of separations are exempt from the job protection rule: mothers can be fired even during the protected period for one of these reasons.

One important thing to notice is that the magnitude of the effect of the replacement rate reform on separations is significantly smaller than the magnitude of the effect on UI take-up, particularly around $t = 6$. Table 3 reports the coefficients $\beta_k$ (for $k = 0$, $k = 6$, and $k = 12$) estimated from equation 2 using as dependent variable the probability of taking up UI at time $k$ and the probability of separations at time $k$ and confirms this finding. This pattern is driven by the fact that the replacement rate reform also expanded the pool of eligible mothers, as shown in Figure A13, which decomposes the effect of the reform on UI take-up into the effect driven by the increase in replacement rate and the effect driven by the expansion in eligibility. At least a portion of newly eligible mothers are inframarginal: they would have exited the labor force even in the absence of the reform but are now eligible to take up UI.

# 5 The Labor Market Costs of Increasing Unprotected Benefits

Section 4 provides evidence that women are more likely to separate from their pre-birth employer when the generosity of unprotected benefits increases. The findings indicate that women are willing to give up the certainty of returning to their pre-birth employer for more generous benefits, both in terms of *level* and in terms of *duration*. In this section, I investigate the long-term effects of this change in choices on women's labor market outcomes. I then relate the estimates to the private trade-off that mothers face when making their leave decisions.

## 5.1 The Labor Market Costs of Giving Up Job Protection

I start the analysis by estimating the effects of the reform on long-term labor force participation patterns. I estimate specification 2 using as dependent variable the probability that a mother is working at relative time $t$[17]. Figure 7 shows the results. The solid red line represents the time of exhaustion of parental leave benefits, while "End of UI" indicates the time window of exhaustion of UI benefits if women resign in $t \in [0, 6]$[18]. For the replacement rate reform (Panel A), there are no differences in participation between the treatment and control group in the first six months after the end of compulsory maternity leave, consistent with the fact that women in the treatment group who take up UI would have used PL in the absence of the reform (Figure 5). The coefficients become

[17] I consider a woman working at a given point in time if: *(i)* I observe her matched with an employer in the data at time $t$, *(ii)* she has positive earnings at time $t$, *(iii)* she is not on leave at time $t$.

[18] While the replacement rate reform did not affect the UI duration, which was constant at 8 months, after the duration reform the potential benefit duration of unemployment benefits for each mother is equal to half of the weeks of contribution in the 4 years before the start of the UI spell, with a maximum of 24 months. The time window reported in the figure assumes a potential benefit duration of 24 months. Appendix Figure A8 shows the distribution of potential benefit duration for mothers who take up UI after the duration reform.

negative and significant from $t = 6$ onwards. While the gap in participation temporarily narrows around the exhaustion of UI benefits, the difference is overall negative and persistent: four years after the end of compulsory maternity leave, the share of mothers working in the treatment group is still 1% lower than in the control group. This suggests that giving up job protection has persistent adverse effects on labor force participation.

Figure 7 Panel B shows the dynamic of participation for the duration reform. There are no significant differences between $t = 0$ and $t = 6$. From $t = 6$ onwards, the coefficients $\beta_k$ from specification 2 become negative and significant, with the gap between the treatment and control group reaching almost 2% by $t = 8$. The gap in participation starts to narrow around the time of UI benefits exhaustion, suggesting that some women can go back to work when benefits end. While the negative effect of the reform is relatively persistent, the gap between the treatment and control group almost completely closes by $t = 48$.

Although both reforms increased the share of people giving up job protection, increasing the *level* of benefits has worse long-term effects in terms of participation than increasing the duration of benefits. This is potentially explained by the fact that longer unemployment benefits allow mothers to search for better jobs while at the same time keeping some attachment to the labor force.

How do these patterns translate into earnings losses? I estimate specification 2 using monthly gross labor earnings as dependent variable, specified in levels in order to keep the zeros from non-participation. Figure 7 Panel C and D report the evolution of the $\beta_k$ coefficients normalized by the predicted earnings in the treatment group in the absence of children: $\frac{\hat{\beta}_k}{E\left[\tilde{Y_{ik}}|k,T=1\right]}$, where $\tilde{Y_{ik}}$ is the predicted outcome when omitting the contribution of the event time dummies (Kleven, Landais, and Søgaard 2019, Kleven et al. 2020). This normalization allows to interpret the earnings results in terms of impact on child penalties for women eligible to more generous unprotected benefits[19]. Panel C reports the results for the replacement rate reform, while Panel D reports the results for the duration reform. The patterns are roughly similar to those in participation. The replacement rate reform increased child penalties by almost 4% in $t = 9$. The gap between the treatment and control group subsequently narrows but remains stable at around 2% even four years after the end of compulsory maternity leave. The duration reform had an even larger impact on child penalties in the short run, with the gap between the two groups peaking around $t = 24$ reaching 4%, then progressively closing as UI benefits start to expire. Table 5 summarizes the results on labor market outcomes at $t = 12$, $t = 24$ and $t = 48$.

Overall, it is important to note that both reforms lead to long-lasting declines in employment

[19]Figure A15 reports the $\beta_k$ from Figure 7 without the normalization.

and earnings, most of which occur *after* the UI benefits expired. This result highlights the crucial role of job protection in reducing the incentive costs associated with parental leave reforms and keeping women attached to the labor force.

### 5.2 Implications of Empirical Estimates on Mothers' Private Trade-Off

The results presented show that taking up unprotected benefits has large negative effects on mothers' future earnings. As explained in Section 2.2, these estimates can inform the private trade-off that mothers face when making these choices. If we assume that mothers are forward-looking and have rational expectations, the effects of the reforms on labor market outcomes indicate how much women are willing to give up in terms of future earnings for a short-term increase in the level or duration of unprotected benefits.

Table 6 reports the difference-in-difference reduced form estimates of the effects of the reforms on mothers' cumulative earnings through month 48 after the end of maternity leave. Mothers exposed to higher *levels* of unprotected benefits lose, on average, 493 EUR in cumulative earnings over the four years after the end of maternity leave. Mothers exposed to *longer* unprotected benefits lose, on average, 662 EUR in cumulative earnings. Scaling the coefficients by the increase in generosity of unprotected benefits, I find that one percentage point increase in replacement rate of unprotected benefits decreases cumulative earnings by 33 EUR. On the other hand, one month increase in duration decreases cumulative earnings by 82.75 EUR. To compare the effects of the two reforms on cumulative earnings, Table 6 also reports the elasticity of earnings with respect to benefit levels $\varepsilon_b$ and with respect to duration $\varepsilon_B$. A 10% increase in benefit level decreases cumulative earnings by 0.3%, while a 10% increase in duration decreases earnings by 0.1%.

Table 6 also reports the IV-scaled coefficients for cumulative earnings: $\hat{\gamma}_{IV} = \frac{\hat{\beta}}{\hat{\delta}}$ where $\hat{\beta}$ is the reduced-form effect on cumulative earnings and $\hat{\delta}$ is the first-stage effect of the reforms on UI take-up. The IV-scaled coefficient is the relevant estimate to evaluate the private insurance trade-off for compliers, i.e., for mothers who take up UI because of the reform. The estimates imply that taking up unprotected benefits, and therefore giving up *job protection* has a considerable cost in terms of future income for mothers. If we consider these estimates as proxies of $e$ in equation 1, the reduced-form effects provide insights on the perceived value of job protection relative to the insurance value of short-term benefits for mothers. In the replacement rate reform context, increasing the replacement rate of benefits yields an average net benefit increase of 5,040[20] EUR for marginal

[20]This is calculated considering the total amount of benefits that marginal mothers have after switching to UI compared to what they would have had in the counterfactual case, using the results from Section 4. For example, for

mothers. The cost of this is 31,000 EUR in foregone future earnings. On the other hand, increasing the length of benefits yields an extra 14,400 EUR per mother; the cost of this is 36,000 EUR in foregone future earnings. These estimates imply that the perceived insurance value of short-term benefits is exceptionally high for marginal mothers relative to the value of job protection. Taken at face-value, the estimates suggest that, for this subsample of women, parental leave policies should increase benefits rather than be providing job protection. From a financial perspective, assuming that mothers are forward-looking and have rational expectations, this trade-off requires new mothers to use an extremely high discount rate.

Are these effects a reasonable estimate of the relative value of job protection and short-term benefits? The assumption that women can perfectly predict the effects of their leave choices on labor market outcomes is a strong one. For example, Kuziemko et al. (2018) shows that women significantly underestimate the employment costs of motherhood and cannot correctly predict the effects of childbirth on labor force participation. An alternative economic explanation is that these women face incredibly high costs of work in the short term (combined with a lack of liquidity to support themselves without working). I probe the large valuation of marginal mothers by looking at the underlying drivers of the responses in Section 6.

### 5.3 Other Outcomes and Robustness

The effect on gross labor earnings can be decomposed along three margins: participation, intensive margin, and wage rate. After examining the effect on the participation margin, I check whether the reforms impacted the intensive margin of labor supply and the wage rate. Since the administrative data do not provide information on hours worked, I use days worked conditional on employment to measure of the intensive margin of employment. The wage rate is defined as earnings per day worked conditional on employment. The results are reported in Table 5 which reports the $\beta_k$ coefficients from equation 2 at $k = 12$, $k = 24$, and $k = 48$ for different outcomes. Panel A reports the results for the replacement rate reform. The effects on days worked, albeit small, are positive and significant at $t = 12$ and $t = 24$. This can be explained by selection into working: in the short run, working mothers in the treatment group are likely to be positively selected. There are no significant differences in days worked four years after the end of compulsory maternity leave. The table also reports the effects on the daily wage rate. While there is a slight negative effect at $t = 12$, there is no significant differences at $t = 24$ and $t = 48$. Panel B of Table 5 reports the results for the

the replacement rate reform, this is calculated as the difference between the more generous UI benefits for marginal mothers (calculated using the average earnings of compliers, see Section C) and the parental leave benefits they would have taken in the absence of the reform.

duration reform. Again, we observe a positive and significant effect on days worked at $t = 12$ but zero effects at $t = 24$ and $t = 48$. There are no significant differences in wage rate four years after the end of compulsory maternity leave.

As mentioned in Section 3.2, subsequent fertility might impact the evolution of child penalties, particularly in the long run. Appendix Figure A16 reports the dynamic effects of the UI reforms on child penalties, restricting the sample to mothers with only one child (with completed fertility of one). The results for the replacement rate reform remain almost unchanged. On the other hand, the results for the duration reform show that the gap in child penalties between the treatment and the control group closes earlier in the sample of mothers with only one child. This suggests that the duration reform, by increasing the subsidized time mothers can spend without working, might positively impact fertility. Moreover, it confirms that, at least for this subsample, the duration reform has almost no effects on women's long-run outcomes, while the replacement rate reform has a persistent negative effect on child penalties even four years after the end of compulsory maternity leave.

# 6 Heterogeneity and Mechanisms

The results above show that women are willing to give up a large amount of future earnings for more generous benefits in the short-term. In this section, I probe the large effects and valuation of unprotected benefits by performing a heterogeneity analysis to shed light on the drivers of behavioral responses and effects on labor market outcomes.

## 6.1 Heterogeneity in Behavioral Responses

The results in Section 5 suggest the insurance value of short-term benefits is higher for women than the value of job protection. As a first step to shed light on the drivers behind the differential behavioral responses after the two reforms, I conduct a simple complier analysis. The goal is to trace out some persistent observable attributes that characterize mothers who respond to the two reforms and distinguish them from mothers who do not. Investigating the characteristics of compliers, always-takers (women who would have taken up UI both under the less generous and more generous UI system) and never-takers (women who would not take up UI under either the more generous or the less generous system) for the different reforms can provide policy-relevant insights on who responds to different changes in incentives.

Because compliers are defined based on a counterfactual (I never simultaneously observe the

outcome for any given mother under both the more generous and the less generous UI system), I infer their characteristics of compliers by comparing the demographic characteristics of the cohort of mothers ineligible for the more generous UI regime (control group, to the left of the cutoff in Figure A5) to the characteristics of the cohort of mothers eligible for the more generous UI regime (treatment group). The comparison is based on a simple intuition: ineligible mothers who take up UI are, by definition, always-takers. Eligible mothers who take up UI are a mix of always-takers and compliers. Assuming monotonicity – i.e., mothers who would take up UI under the less generous system would certainly do so under the more generous system - we expect the demographic characteristics of always-takers to be the same in expectation before and after the policy change. This fact allows us to back out the demographic characteristics of compliers, as described in detail in Appendix Section C.

Results of the complier analysis are reported in Table 7, which provides an overview of the pre-birth characteristics of the three groups for the replacement rate reform. A clear pattern emerges: women that are induced to take up UI in response to an increase in replacement rate are on average younger, less likely to be working full-time, more likely to work in "low-quality" establishments (measured by whether the firm is below or above the median AKM firm effect, low- and high-wage firms, respectively) and earn less than both always-takers and never-takers. Compliers are also less likely to hold permanent jobs and to live in regions with significantly less childcare availability than never-takers. Finally, compliers are significantly less likely to work in regions with a high unemployment rate than always-takers. Job protection is valuable only in a labor market with search frictions, therefore, is likely to be more valuable when the unemployment rate is higher. These findings are overall in line with the idea that individuals induced to take up UI by the replacement rate reform are less likely to value job protection and more likely to associate high insurance value with short-term benefits.

Table 8 reports the results for the duration reform. Women that are induced to take up UI after an increase in UI duration are less likely to be identified off their pre-birth characteristics in a clear-cut way. Compliers are more likely to have permanent jobs, work in blue-collar occupations, and work part-time than both always- and never-takers. On the other hand, compliers earn significantly more than always-takers but still less than never-takers. Overall, this suggests that the duration reform affected a broader population of mothers. Interestingly, compliers are significantly less likely to live in regions with high childcare availability. This suggests that increases in UI duration are particularly appealing to mothers who do not have access to formal childcare options. The high cost of childcare is a plausible explanation for why women are willing to give up future earnings

for longer benefit duration. The cost or unavailability of childcare is likely to enter the trade-off between benefits and job protection by decreasing the relative value of going back to work.

I complement the analysis by presenting estimates of treatment effect heterogeneity of the UI reforms on UI take-up. For each covariate, I separately estimate equation 2, splitting the sample according to the pre-birth value of the characteristic. Results are reported in Figure 8. Panel A reports the results for the replacement rate reform, while Panel B reports the results for the duration reform. Overall, the results primarily reflect the conclusions of the complier analysis.

### 6.2 Heterogeneity in Labor Market Costs of Increasing Unprotected Benefits

Mean effects on labor market outcomes mask substantial heterogeneity across subgroups. In this section, I conduct a heterogeneity analysis to understand the possible mechanisms behind the effects of the reforms on labor market outcomes. I focus on three main dimensions of heterogeneity: occupation, regional unemployment rate, and childcare availability.

I start by investigating whether the effects of the reforms are different for blue-collar workers and white-collar workers. This distinction can be informative on the role of *human capital depreciation*, as experience effects have been estimated to be smaller for blue-collar occupations than for white-collar ones. Figure A17 Panel A and B report the results of the estimation of specification 2, splitting the sample by occupation, on the probability of working at time $t$. There are no significant differences in the evolution of participation after the reforms by occupation category. The results look very similar when focusing on earnings instead of participation (Appendix Figure A18). This seems to suggest that human capital depreciation does not play a significant role in explaining the long-term effects of increasing UI generosity on labor market outcomes. However, there is a critical caveat to this analysis: the occupation categories reported in the data are vast and do not provide much details on the type of occupations other than the distinction between blue and white-collar. Experience effects are likely to be small also in many white-collar occupations, and this might explain the lack of significant differences in labor market outcomes by occupation.

Another useful dimension of heterogeneity is the unemployment rate in the region of residence. I test whether the effects on participation are significantly different by different levels of the regional unemployment rate (above the median or below the median). This distinction can be informative on the importance of search frictions and on the role of job protection in influencing labor market outcomes. Results are reported in Figure A17 Panel C and D. As expected, the reforms' long-term effects on participation are significantly smaller in regions with lower unemployment rates. After the duration reform, women in low unemployment regions can return to work as soon as they exhaust

their unemployment benefits (around $t = 24$). This is not the case for women in high unemployment regions. For the replacement rate reform, we observe no significant effects in the long run in regions with low unemployment. This suggests that job protection is critical in the presence of search frictions.

Finally, I test whether the effects on labor market outcomes are different by childcare availability. The results confirm that the lack of formal childcare is one of the main drivers of the take-up of unemployment insurance. They also suggest that the difficulty of finding childcare is a clear explanation for women's willingness to forgo an enormous amount of future labor earnings (the cost of losing job protection) in order to gain additional short-term benefits. In regions with relatively high levels of childcare availability, women are only willing to forgo 13,666 EUR in future earnings, compared with 40,950 EUR in regions with relatively low availability of childcare. Moreover, note that, after the duration reform, the gap in participation between the treatment and control group starts to close at around $t = 30$, which roughly corresponds to the age at which the child can start formal and free childcare.

Figure 9 summarizes these results and reports additional results on the heterogeneity of the effects of the reforms on participation along other dimensions. Panel A shows the results for the replacement rate reform. Panel B reports the results for the duration reform. It is interesting to notice how women living in regions with high childcare availability experience less negative long-run effects on participation, while women living in regions with high unemployment experience more negative long-run effects on participation, mirroring the results presented above.

# 7 Welfare Effects and Policy Implications

## 7.1 Welfare

In this Section, I take advantage of the empirical results described above to derive normative implications on how to optimally design parental leave policies. While estimating the welfare effects of these specific unemployment insurance reforms might have low external validity, it can be helpful to estimate the welfare effects of directional changes to different policy parameters while keeping the other relevant parameters fixed.

I start by adapting the standard Baily-Chetty formula to the parental leave context, following Schmieder and Von Wachter (2016). I present a very simplified version of the model that allows thinking about the different roles of benefit level and benefit duration without offering job protection.

**Setup** The model centers on a worker who has a child at time $t = 0$. The model is set in continuous time and the horizon is assumed to last until time $T$. Each mother chooses how much time to spend on leave. While on leave she has a probability to returning to work of $s_t$ at each point in time. The survival probability of remaining on leave at time $t$ is given by $S_t = exp\left(-\int_0^t s_t dt\right)$. Returning to work implies a search cost $\psi_t(s_t)$. This cost can be interpreted in the standard way if we assume no job protection but also more broadly to include the cost of childcare, the value of home production or human capital depreciation.

While on leave, the worker receives benefits $b_t$ and consumes $c_{l,t} = b_t + y_l$ where $y_l$ may be income from other sources such as home production. The corresponding flow utility is given by $u(c_{l,t})$. If she returns to work, the worker receives a fixed wage $w$ and pays taxes $\tau$, resulting in consumption $c_e = w - t$ and utility $v(c_e)$. Different utility functions when employed and when on leave capture the possible effort cost of working or the valuation of staying at home with the child.

The lifetime expected utility of a mother is given by:

$$W = \int_0^T \left\{S_t u(c_{l,t}) + [1 - S_t]\, v(c_e) - S_t \psi_t(s_t)\right\} dt \tag{3}$$

Note that this equation captures the basic trade-off in the mother's decision problem. Returning to work sooner results in higher utility if $v(c_e) > u(c_{l,t})$ but it comes at a higher cost $\psi_t(s_t)$.

**Government Problem** The government sets the parental leave benefit path to maximize social welfare while taking into account that the mother will adjust her decisions in response to the path of parental leave benefits (by returning to work later for example) and the tax level $\tau$ to finance PL benefits. I restrict the government choice set to benefit paths with constant benefit levels up to a finite horizon $B$ so that $b_t = b$ for $t \leq B$ and $b_t = 0$ for $t > B$. Consumption during parental leave is then $c_{l,t} = b + y_l$ for $t \leq B$ and $c_{l,t} = y_l$ for $t > B$. In this case, equation 3 becomes:

$$W = \int_0^B S_t u(c_{l,t\leq B}) dt + \int_B^T S_t u(c_{l,t>B}) dt + \int_0^T [1 - S_t]\, v(c_e) dt - \int_0^T S_t \psi_t(s_t) dt \tag{4}$$

The government has the following budget constraint:

$$(T - D)\tau = Bb + E \tag{5}$$

where $E$ is some level of exogenous per capita spending, $L = \int_0^B S_t dt$ is the expected duration of receiving parental leave benefits and $D = \int_0^T S_t dt$ is the expected duration of non-participation.

Appendix D shows that the optimal level of parental leave benefits is determined by

$$\frac{dW}{db}\frac{1}{Lv^{'}(c_e)} = \frac{u^{'}(c_{l,t\leq B}) - v^{'}(c_e)}{v^{'}(c_e)} - \left(\eta_{L,b} + \eta_{D,b}\frac{D}{L}\frac{\tau}{b}\right) \tag{6}$$

where $\eta_{L,b} = \frac{dL}{db}\frac{b}{L}$ is the elasticity of the duration of receiving PL benefits with respect to the monthly benefit level and $\eta_{D,b} = \frac{dD}{db}\frac{b}{L}$ is the elasticity of non-participation with respect to the monthly benefit level. The first term on the right hand side represents the value of increasing the transfer by 1 EUR which depends on the gap between the marginal utility of leave recipients relative to the marginal utility of the employed. The second term represents the behavioral cost of increasing the transfer by 1 EUR to the government budget.

The marginal effect of increasing transfers by 1EUR through a duration extension on welfare can be written as:

$$\frac{dW}{dB}\frac{1}{S_B v^{'}(c_e)} = \frac{\tilde{u}^{'}(c_{l,t\leq B}) - v^{'}(c_e)}{v^{'}(c_e)} - \frac{1}{S_B}\left(\int_0^B \frac{dS_t}{dB}dt + \frac{dD}{dP}\frac{\tau}{b}\right) \tag{7}$$

where $\tilde{u}^{'}(c_{l,t\leq B}) = \frac{1}{b}\int_{y_l}^{y_l+b} u^{'}(c)dc$ is the average marginal utility for an individual with consumption between $y_l$ and $y_l + b$. Again, the first term on the right hand side represents the value of increasing the transfer by 1 EUR, while the second term represents the behavioral cost of increasing benefit duration to the government.

Both formulas show that the welfare trade-off for parental leave policies is conceptually the same as unemployment insurance: it compares individuals' willingness to pay for the policy change to the net cost of additional benefits, inclusive of the fiscal externalities on government revenue generated by behavioral responses to the policy change.

### 7.2 The Welfare Effects of Increasing Unprotected Benefits

Using the framework derived above, I now follow Hendren and Sprung-Keyser (2020) and analyze the welfare effects of the two policy changes by estimating the Marginal Value of Public Funds (MVPF). The MVPF is defined as:

$$MVPF = \frac{WTP}{Net\,Costs}$$

The denominator of the MVPF is relatively straightforward to calculate as I can directly estimate the reduced-form estimates of the net costs of increasing unprotected benefits, which includes the mechanical cost and the behavioral cost (described as the second term on the right-hand side of equations 6 and 7). Estimating the WTP of mothers for different policy changes is more complicated.

It entails estimating the first term on the right-hand side of equations 6 and 7. Note that the calculation of the WTP in the MVPF (as the derivation of equations 6 and 7) relies heavily on the envelope theorem, which implies that individuals who change their behavior in response to the policy change are making privately optimal decisions and are therefore indifferent to changing their behavior. Because of the envelope theorem, behavioral responses to policies have first-order effects on policy costs because of fiscal externalities, but only second-order effects on welfare. The two critical assumptions for the envelope theorem are that the policy change is small and that individuals are making privately optimal decisions. I first calculate the MVPF assuming that mothers are forward-looking and have rational expectations. I then consider the case in which information frictions may make it difficult for mothers to trade-off between short-term benefits and future earnings losses or internalize the earnings losses associated with their leave-taking decisions.

**Costs** I start by assessing the effects of the reforms on the net costs for the government. I estimate the effects of the reforms on three components of government costs. The first component of the costs is the increase in unemployment insurance spending, which is partially mechanical (driven by the increased generosity of benefits for mothers who do not change behavior in response to the policy change) and partially behavioral (driven by the fiscal externalities on government revenue of behavioral responses to the policy change). The second component of the costs is parental leave benefits. In Section 4, I document significant program interactions between parental leave and unemployment insurance that need to be considered when calculating the net costs of the policy change. For example, the fact that more generous unprotected benefits lead to crowding out of parental leave benefits is a saving for the government that needs to be taken into account. The last component of the costs is the effect of the policy changes on labor force participation and earnings and, therefore, on tax revenues. Evidence from Section 5 shows that the behavioral responses to the increased generosity of unprotected benefits have negative and persistent income on labor force participation and earnings that need to be taken into account in calculating the net costs. Figure 10 shows the results of the estimation of equation 2 on individual-level monthly unemployment insurance benefits, individual-level monthly parental leave benefits, and monthly gross earnings.

Table 9 reports the net cost for the government of the reforms up to 48 months from the end of compulsory maternity leave. For the replacement rate reform, the net cost for the government of increasing the level of benefits on UI benefits corresponds is 228 EUR, which is the cumulative increase in monthly UI spending between $t = 0$ and $t = 48$. This cost is comprehensive of the policy change's mechanical cost and policy change's behavioral cost. Because of program substitution,

increasing unprotected benefits generate savings for the government in the form of lower parental leave spending, equal to 67 EUR. The reform generates long-term losses in earnings of 497 EUR, assuming a tax rate of 27%. This translates in a loss in tax revenue of 134 EUR. Taken together, the total net costs of changing the level of benefits is equal to 295 EUR.

I estimate the costs of the duration reform similarly. The increase in UI spending driven by the increase in benefit duration is equal to 898 EUR, a much higher effect than the replacement rate reform given the higher generosity of the duration reform. Increasing the duration of benefits generates parental leave savings of 25 EUR. Finally, the reform generates long-term losses in earnings of 647 EUR, which, assuming a tax rate of 27%, translates into tax revenue loss of 175 EUR. Taken together, the total net costs of changing the duration of benefits is equal to 1048 EUR.

**WTP** The first term on the right-hand side of equation 3 and 4 shows individuals' willingness to pay for parental leave benefits. I follow the optimal unemployment insurance literature and approximate individuals' WTP for 1 EUR of additional unemployment insurance by $1+\gamma\frac{\Delta c}{c}$ where $\gamma$ is a coefficient of risk aversion and $\Delta c$ is the impact of unemployment on consumption. As I do not observe the impact of parental leave benefits on consumption, I use estimates of $\gamma\frac{\Delta c}{c}$ reported in Schmieder and Von Wachter (2016). Conveniently Schmieder and Von Wachter (2016) and Hendren and Sprung-Keyser (2020) provide estimates of $\gamma\frac{\Delta c}{c}$ for both changes in benefit levels and changes in benefit duration. Assuming $\gamma = 2$, the estimates for $1+\gamma\frac{\Delta c}{c}$ are equal to 1.17 for benefit increases and to 1.30 for duration increases. I construct the WTP as the mechanical increase in benefits for inframarginal mothers (always-takers) multiplied by $1+\gamma\frac{\Delta c}{c}$. For an increase in replacement rate of 15 percentage points, considering that the average monthly wage for inframarginal mothers is equal to 1227 EUR, we have that: $WTP = \pi^{AT}\left[(1227 * 0.15) * 8 * 1.17\right] = 155\,EUR$, where $\pi^{AT}$ is the share of always-takers reported in Table 7[21].

Following the same reasoning, the WTP for the duration reform is equal to the increase in benefits due to the change in duration for always-takers times $1+\gamma\frac{\Delta c}{c}$. On average, the potential benefit duration increases by eight months after the reform. Considering that the monthly wage of always-takers is 1253 EUR, the WTP is equal to: $WTP = \pi^{AT}[4661 * 1.30] = 727\,EUR$.

These estimates imply a MVPF of 0.5 for the replacement rate reform and 0.7 for the duration reform. In both cases, the MVPF is lower than 1, implying that the net fiscal costs of both reforms are higher than the benefits. Overall, the MVPF of increasing parental leave benefits without offering hob protections for mothers is in line with estimates of the MVPF of unemployment insurance

[21] Appendix C describes how this share is estimated.

extensions (Hendren and Sprung-Keyser, 2020).

**MVPF when Optimality Fails** An extensive literature in behavioral economics suggests that individuals commonly make mistakes and do not make privately optimal decisions. Given the evidence from Section 5 that shows that mothers are willing to incur enormous losses in long-term earnings to get higher short-term benefits, it is not apparent that this assumption holds in this context. Mothers might not be able to perfectly forecast the effects of their leave decisions on labor market outcomes or may be myopic. If we do not assume that women are forward-looking and have rational expectations, we can no longer rely on the envelope theorem and assume that the welfare impact of the policy change for marginal recipients is zero. It is, therefore, necessary to take into account the private benefits of marginal individuals. This implies that the WTP is equal to $\pi^{AT} WTP_{inframarginal} + \pi^{C} WTP_{marginal}$, where $\pi^{C}$ is the share of compliers[22]. Considering the extreme case in which marginal mothers fail entirely to take into account the effects of the take-up of unprotected benefits on the future earnings losses, the total willingness to pay would be equal to:

$$\begin{aligned} WTP &= \pi^{AT} WTP_{inframarginal} + \pi^{C} WTP_{marginal} \\ &= \pi^{AT} WTP_{inframarginal} + \pi^{C} \hat{\gamma} \end{aligned}$$

where $\pi^{AT} WTP_{inframarginal}$ is equal to 155 EUR for the replacement rate reform and to 727 EUR for the duration reform and $\hat{\gamma}$ is the coefficient reported in Table 6. Table 9 reports the MVPF under the assumption that women are not privately optimizing. If women cannot fully take into account the private loss in future earnings derived by taking up unprotected benefits, we get a much lower MVPF, equal to -1.14 for the replacement rate reform and 0.06 for the duration reform.

### 7.3 Policy Implications

From the analysis above, I find that the MVPF for the replacement rate reform is equal to 0.5, while the MVPF for the duration reform is equal to 0.7. They are both lower than 1, reflecting that providing parental leave benefits is very costly per euro spent if it does not come with job protection.

Interestingly, the MVPF of the duration reform is higher than the MVPF of the replacement rate reform. This is likely to be because the duration reform has a higher insurance value for mothers and, in relative terms, has a lower impact on cumulative earnings (as shown in Section 5). Longer benefits, even without job protection, may allow mothers to search for better jobs while at the same time keeping some attachment to the labor force.

[22] See Table 7 and 8.

By providing an assessment of the value that women assign to parental leave benefits after birth, this analysis shows that the insurance value of cash benefits is extremely high for mothers and much higher than the value of job protection. At the same time, the incentive costs of providing unprotected benefits are high, as they result in considerable losses in terms of tax revenue for the government even beyond the exhaustion of the benefits.

The welfare analysis above has different implications. First, extending the duration or level of benefits while at the same time extending job protection is welfare improving for mothers. Second, extending the duration of benefits is preferable in terms of welfare than increasing the level of benefits. Third, leave policies that push for longer benefits should take into account the large costs in terms of earnings losses that they potentially generate. Fourth, the evidence shows that mothers do not internalize the career costs of their leave choices when making decisions and policy-makers should take this into account when designing leave policies. Finally, the results on childcare availability suggest that addressing the market failure behind the low availability of childcare, provided that costs are not too high, is likely to be welfare-improving by helping new mothers return to the labor force sooner.

# 8 Conclusions

In this paper, I provide novel insights on the effects of different policy instruments on the incentive-insurance trade-off that characterizes parental leave policies. Using Italian administrative data on the universe of working mothers and two helpful policy changes in the level and duration of benefits that mothers can receive after childbirth, I study the effects of providing increased benefits without offering job protection on women's decisions to return to work, labor market outcomes and welfare. Increasing the generosity of unprotected benefits generates significant behavioral responses as women are more likely to take up unemployment insurance which results in a significant increase in separations from their pre-birth employer. Taking advantage of the fact that I observe how women's choices (between protected benefits, unprotected benefits, and returning to work) change when the generosity of unprotected benefits increases, I relate the estimated behavioral responses to underlying changes in incentives. By revealed preferences, I can estimate the relative value of higher short-term benefits and job protection.

Mothers are willing to forgo an enormous amount of future labor earnings (the cost of losing job protection) in order to gain additional short-term benefits. Increasing the replacement rate of benefits yields an average net benefit increase of 5,040 EUR for mothers who take this up; the

cost of this is 31,000 EUR in foregone future earnings. Increasing the length of benefits, yields an extra 14,400 EUR per mother; the cost of this is 36,000 EUR in foregone future earnings. These estimates imply that the perceived insurance value of short-term benefits is exceptionally high for marginal mothers relative to the value of job protection. Moreover, the estimates suggest that women face incredibly high costs of work in the short term (combined with a lack of liquidity to support themselves without working). Exploiting the heterogeneity in effects across areas, I find that the difficulty of finding child care is a compelling explanation for this behavior.

In order to assess the normative implications of my findings, I develop a conceptual framework to evaluate the welfare effects of changes to parental leave policies in terms of empirically measurable sufficient statistics. I derive the MVPF of changing the level or duration of unprotected benefits and assess how it changes if we assume that mothers do not privately optimize and that the envelope theorem does not hold. I find that extending the duration of unprotected benefits is preferable in terms of welfare than increasing the level of benefits and that extending the duration or level of benefits while at the same time extending job protection is welfare improving for mothers.

## References


Abowd, John M, Francis Kramarz, and David N Margolis. 1999. "High wage workers and high wage firms." *Econometrica* 67 (2):251–333.

Bailey, Martha J, Tanya S Byker, Elena Patel, and Shanthi Ramnath. 2019. "The long-term effects of California?s 2004 paid family leave act on women?s careers: Evidence from US tax data." Tech. rep., National Bureau of Economic Research.

Baily, Martin Neil. 1978. "Some aspects of optimal unemployment insurance." *Journal of public Economics* 10 (3):379–402.

Baker, Michael and Kevin Milligan. 2008. "How does job-protected maternity leave affect mothers? employment?" *Journal of Labor Economics* 26 (4):655–691.

———. 2010. "Evidence from maternity leave expansions of the impact of maternal care on early child development." *Journal of human Resources* 45 (1):1–32.

Bertrand, Marianne, Claudia Goldin, and Lawrence F Katz. 2010. "Dynamics of the gender gap for young professionals in the financial and corporate sectors." *American economic journal: applied economics* 2 (3):228–55.

Borghans, Lex, Anne C Gielen, and Erzo FP Luttmer. 2014. "Social Support Substitution and the Earnings Rebound: Evidence from a Regression Discontinuity in Disability insurance reform." *American Economic Journal: Economic Policy* 6 (4):34–70.

Callaway, Brantly and Pedro HC Sant'Anna. 2020. "Difference-in-differences with multiple time periods." *Journal of Econometrics* .

Carta, Francesca and Lucia Rizzica. 2018. "Early kindergarten, maternal labor supply and children's outcomes: Evidence from Italy." *Journal of Public Economics* 158:79–102. URL https://www.sciencedirect.com/science/article/pii/S0047272717302141.

Casarico, Alessandra and Salvatore Lattanzio. 2021. "Behind the Child Penalty: Understanding What Contributes to the Labour Market Costs of Motherhood." .

Chetty, Raj. 2008. "Moral hazard versus liquidity and optimal unemployment insurance." *Journal of political Economy* 116 (2):173–234.

Chetty, Raj and Amy Finkelstein. 2013. "Social insurance: Connecting theory to data." In *Handbook of Public Economics*, vol. 5. Elsevier, 111–193.

CittadinanzaAttiva. 2015. "C'è un Nido? Rilevazione Annuale sui Costi e sui Numeri degli Asili Nido in italia." Tech. rep., Osservatorio Prezzi e Tariffe.

Dahl, Gordon B, Katrine V Løken, Magne Mogstad, and Kari Vea Salvanes. 2016. "What is the case for paid maternity leave?" *Review of Economics and Statistics* 98 (4):655–670.

Danzer, Natalia, Martin Halla, Nicole Schneeweis, and Martina Zweimüller. 2020. "Parental leave,(in) formal childcare and long-term child outcomes." *Journal of Human Resources* :0619–10257R1.

De Chaisemartin, Clément and Xavier d?Haultfoeuille. 2018. "Fuzzy differences-in-differences." *The Review of Economic Studies* 85 (2):999–1028.

De Chaisemartin, Clément and Xavier d'Haultfoeuille. 2020. "Two-way fixed effects estimators with heterogeneous treatment effects." *American Economic Review* 110 (9):2964–96.

Deshpande, Manasi. 2016. "The effect of disability payments on household earnings and income: Evidence from the SSI children's program." *Review of Economics and Statistics* 98 (4):638–654.

Finkelstein, Amy and Nathaniel Hendren. 2020. "Welfare Analysis Meets Causal Inference." *Journal of Economic Perspectives* 34 (4):146–67.

Frijters, Paul and Bas van der Klaauw. 2006. "Job search with nonparticipation." *The Economic Journal* 116 (508):45–83.

Ginja, Rita, Jenny Jans, and Arizo Karimi. 2020. "Parental leave benefits, household labor supply, and children?s long-run outcomes." *Journal of Labor Economics* 38 (1):261–320.

Ginja, Rita, Arizo Karimi, and Pengpeng Xiao. 2020. "Employer Responses to Family Leave Programs." *Working Paper* .

Giupponi, Giulia. 2019. "When Income Effects Are Large: Labor Supply Responses and the Value of Welfare Transfers." *Working Paper* .

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* .

Haller, Andreas, Stefan Staubli, and Josef Zweimüller. 2020. "Designing Disability Insurance Reforms: Tightening Eligibility Rules or Reducing Benefits." *NBER Working Paper* .

Hendren, Nathaniel. 2016. "The Policy Elasticity." *Tax Policy and the Economy* 30 (1):51–89.

Hendren, Nathaniel and Ben Sprung-Keyser. 2020. "A Unified Welfare Analysis of Government Policies." *The Quarterly Journal of Economics* 135 (3):1209–1318.

Inderbitzin, Lukas, Stefan Staubli, and Josef Zweimüller. 2016. "Extended Unemployment Benefits and Early Retirement: Program Complementarity and Program Substitution." *American Economic Journal: Economic Policy* 8 (1):253–88.

IstitutoDegliInnocenti. 2011. "Monitoraggio del piano di sviluppo dei servizi socio-educativi per la prima infanzia." Tech. rep., Rapporto al 31 Dicembre 2011.

Jäger, Simon, Benjamin Schoefer, and Josef Zweimüller. 2019. "Marginal jobs and job surplus: a test of the efficiency of separations." Tech. rep., National Bureau of Economic Research.

Jørgensen, Thomas Høgholm and Jakob Egholt Søgaard. 2021. "Welfare Reforms and the Division of Parental Leave." .

Kleven, Henrik, Camille Landais, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller. 2020. "Do Family Policies Reduce Gender Inequality? Evidence from 60 Years of Policy Experimentation." *NBER Working Paper* .

Kleven, Henrik, Camille Landais, and Jakob Egholt Søgaard. 2019. "Children and Gender Inequality: Evidence from Denmark." *American Economic Journal: Applied Economics* 11 (4):181–209.

Kline, Patrick and Christopher R Walters. 2016. "Evaluating Public Programs with Close Substitutes: The Case of Head Start." *The Quarterly Journal of Economics* 131 (4):1795–1848.

Kuka, Elira and Na'ama Shenhav. 2020. "Long-Run Effects of Incentivizing Work After Childbirth." Tech. rep., National Bureau of Economic Research.

Kuziemko, Ilyana, Jessica Pan, Jenny Shen, and Ebonya Washington. 2018. "The mommy effect: Do women anticipate the employment effects of motherhood?" Tech. rep., National Bureau of Economic Research.

Lalive, Rafael, Analía Schlosser, Andreas Steinhauer, and Josef Zweimüller. 2014. "Parental Leave and Mothers' careers: The Relative Importance of Job Protection and Cash Benefits." *Review of Economic Studies* 81 (1):219–265.

Lawson, Nicholas. 2015. "Social Program Substitution and Optimal Policy." *Labour Economics* 37:13–27.

———. 2017. "Fiscal Externalities and Optimal Unemployment Insurance." *American Economic Journal: Economic Policy* 9 (4):281–312.

Lee, David S, Pauline Leung, Christopher J O'Leary, Zhuan Pei, and Simon Quach. 2019. "Are Sufficient Statistics Necessary? Nonparametric Measurement of Deadweight Loss from Unemployment Insurance." .

Leung, Pauline and Christopher O'Leary. 2019. "Unemployment Insurance and Means-Tested Program Interactions: Evidence from Administrative Data." *American Economic Journal: Economic Policy* .

Olivetti, Claudia and Barbara Petrongolo. 2017. "The economic consequences of family policies: lessons from a century of legislation in high-income countries." *Journal of Economic Perspectives* 31 (1):205–30.

Raute, Anna. 2019. "Can financial incentives reduce the baby gap? Evidence from a reform in maternity leave benefits." *Journal of Public Economics* 169:203–222.

Rossin-Slater, Maya. 2017. "Maternity and family leave policy." Tech. rep., National Bureau of Economic Research.

Rothstein, Jesse and Robert G Valletta. 2017. "Scraping by: Income and Program Participation After the Loss of Extended Unemployment Benefits." *Journal of Policy Analysis and Management* 36 (4):880–908.

Schmieder, Johannes F and Till Von Wachter. 2016. "The effects of unemployment insurance benefits: New evidence and interpretation." *Annual Review of Economics* 8:547–581.

Schmieder, Johannes F, Till Von Wachter, and Stefan Bender. 2012. "The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over 20 years." *The Quarterly Journal of Economics* 127 (2):701–752.

Schönberg, Uta and Johannes Ludsteck. 2014. "Expansions in maternity leave coverage and mothers? labor market outcomes after childbirth." *Journal of Labor Economics* 32 (3):469–505.

Staubli, Stefan. 2011. "The Impact of Stricter Criteria for Disability Insurance on Labor Force Participation." *Journal of Public Economics* 95 (9-10):1223–1235.

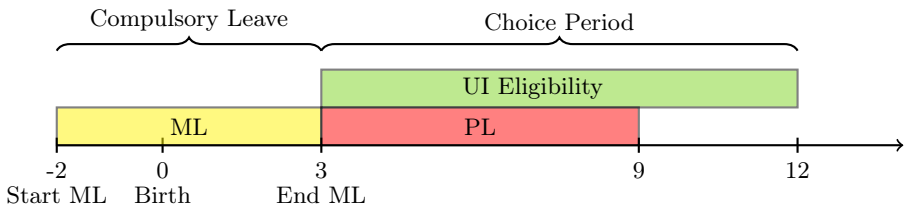
Sun, Liyang and Sarah Abraham. 2020. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics* .

Tô, Linh T. 2018. "The signaling role of parental leave." *Harvard University* .

Xiao, Pengpeng. 2021. "Wage and employment discrimination by gender in labor market equilibrium." .

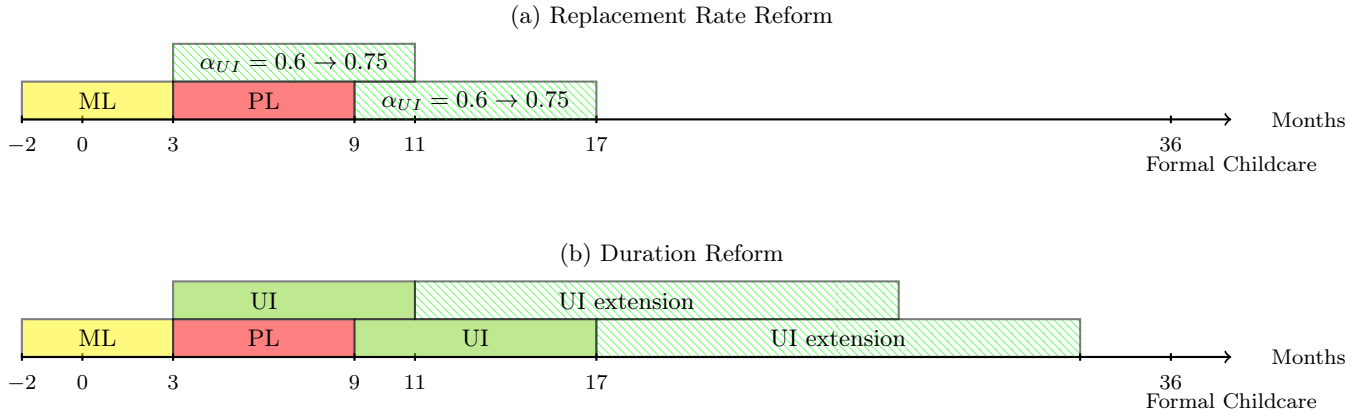
# Figures

Figure 1: TIMELINE




**Notes:** The figure shows the timeline of social programs available to mothers around childbirth. Denoting the time of childbirth with $t = 0$, mothers are compelled to use maternity leave between $t = -2$ and $t = 3$. Between $t = 3$ to $t = 12$ mothers can choose to return to work, to use parental leave, which lasts 6 months, or to take up unemployment insurance. Mothers can take up unemployment insurance at any time between $t = 3$ and $t = 12$. See Section 2 for details.

Figure 2: IMPACT OF REFORMS ON POLICY SPACE



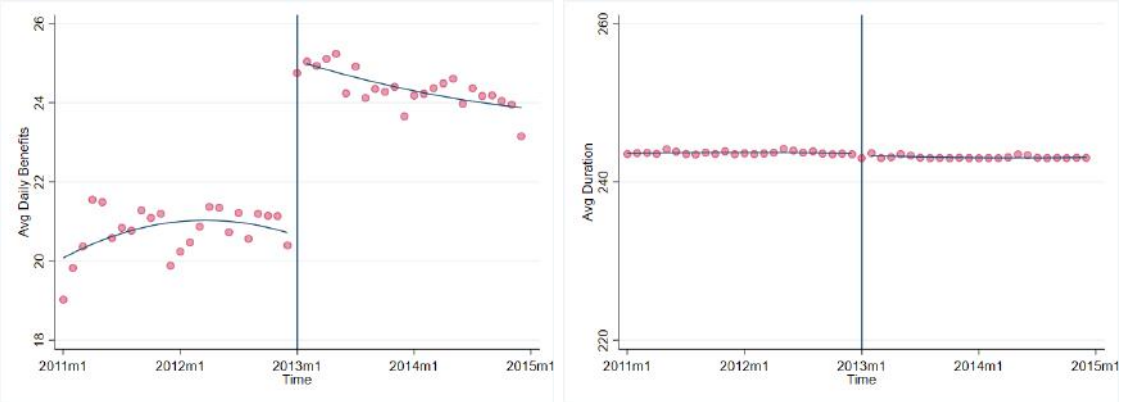

**Notes:** The graphs illustrate how the unemployment reforms impacted the policy space for women after childbirth. $t = 0$ corresponds to the time of childbirth. From $t = 3$ onwards women can choose to go back to work, take up PL or take up UI. The graph shows the impact of the reforms under the assumption that women take up PL at $t = 3$ and UI either at $t = 3$ or at $t = 9$, after the exhaustion of PL benefits. Panel A shows the effect of the replacement rate reform, which increased the replacement rate of UI from 60 to 75% while leaving the length of UI unchanged. Panel B shows the effect of the duration reform, which extended UI duration from 8 to a maximum of 24 months. See Section 2 for details.

Figure 3: Changes in Unemployment Insurance Generosity

Replacement Rate Reform

(a) Benefit Level

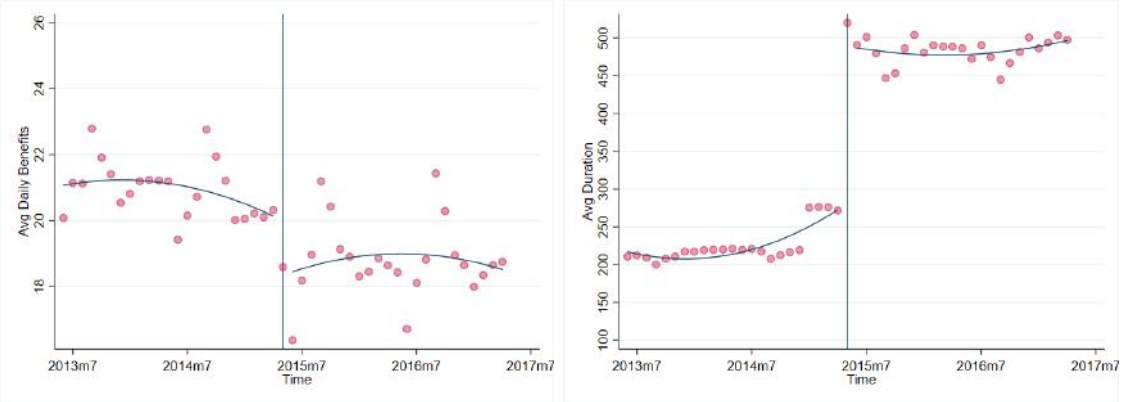
(b) Duration




Duration Reform

(c) Benefit Level

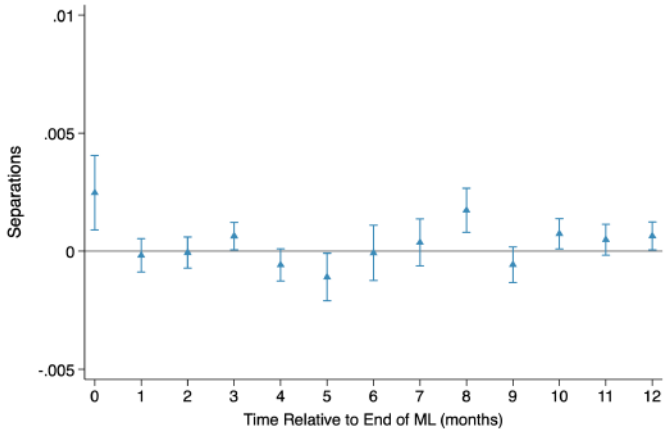
(d) Duration



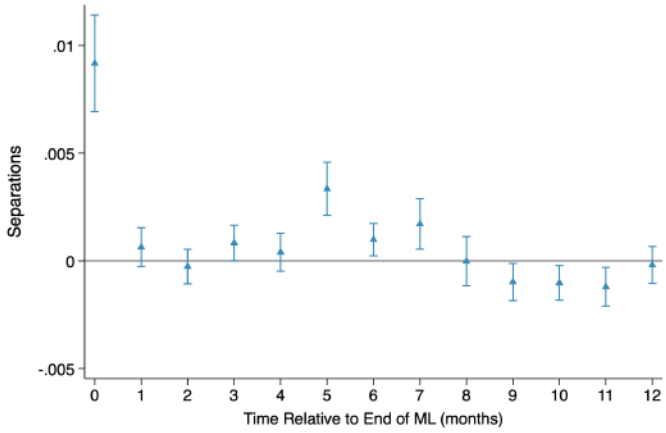

**Notes:** The graphs show the average daily unemployment insurance benefits and the average potential benefit duration (in days) by month-of-start of unemployment spell bin for women younger than 50 years old with a permanent contract. Panel A and B show the effects of the introduction of the replacement rate reform. Panel C and D show the effects of the introduction of the duration reform.

Figure 4: Effects of the Reforms on Separations from Pre-Birth Employer

(a) Replacement Rate Reform



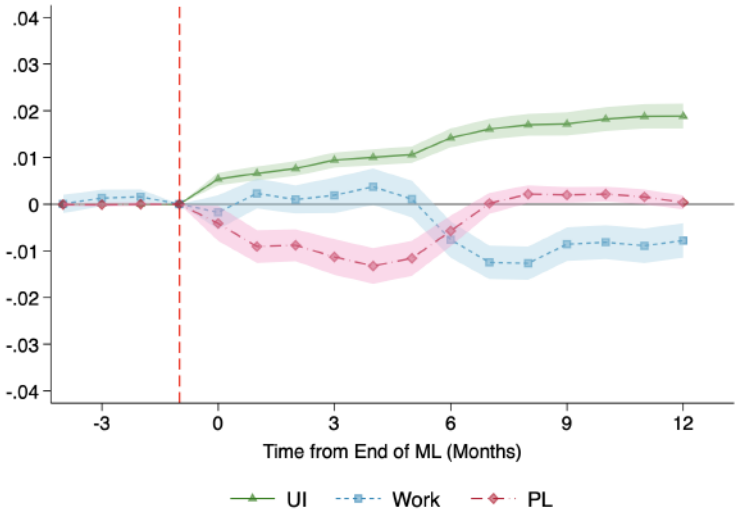

(b) Duration Reform



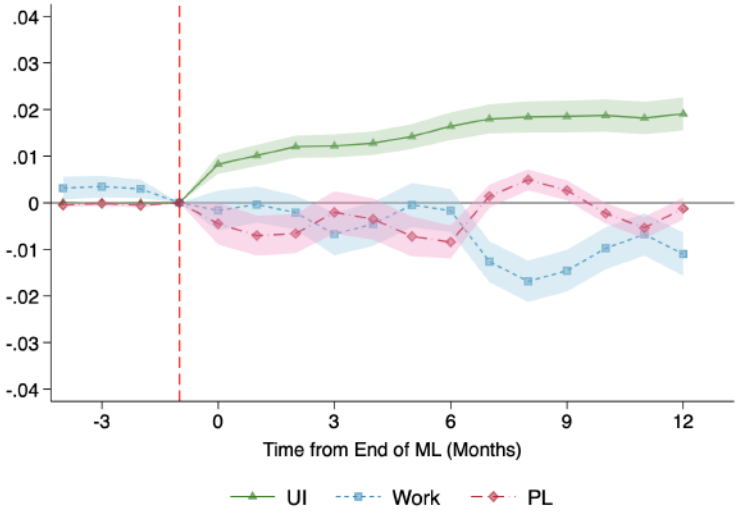

**Notes:** The figures report difference-in-differences estimates of the coefficients $\beta_k$ from specification 2 where the dependent variable is the probability that a mother voluntarily separates from the pre-birth employer at relative time $t$ (where $t = 0$ corresponds to the time of end of compulsory maternity leave). The figure reports the 95% confidence intervals. Standard errors are clustered at the individual level. Panel A reports results for the replacement rate reform while panel B reports results for the duration reform.

Figure 5: Effects of the Reforms on Women's Choices

(a) Replacement Rate Reform




(b) Duration Reform

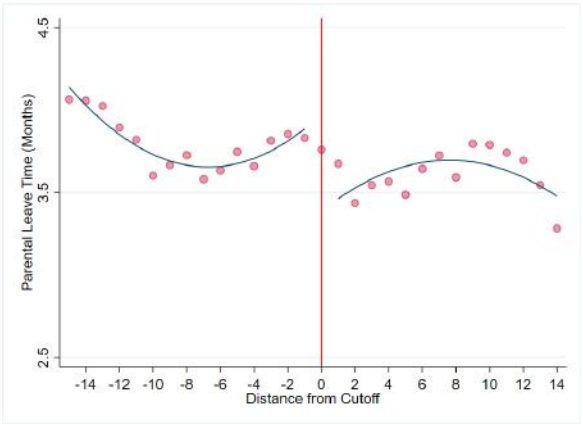



**Notes:** The figures report difference-in-differences estimates of the coefficients $\beta_k$ from specification 2 estimated on three different dependent variables: the probability that a mother is on UI at relative time $t$ (green series), the probability that a mother is working at relative time $t$ (blue series) and the probability that a mother is on parental leave at relative time $t$ (pink series). $t = 0$ corresponds to the time of end of compulsory maternity leave. Shaded areas correspond to 95% confidence intervals. Standard errors are clustered at the individual level. Panel A reports results for the replacement rate reform while panel B reports results for the duration reform.

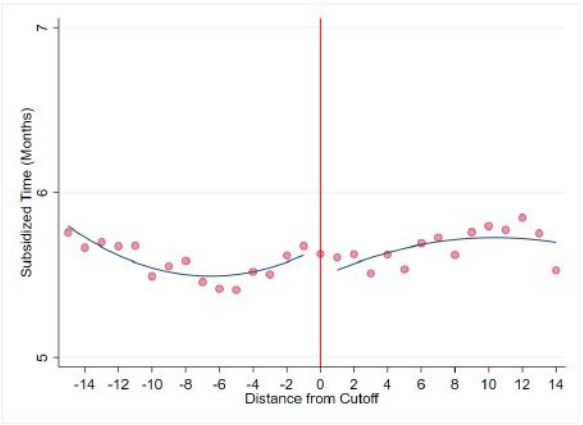
Figure 6: Effects of the Reforms on Insured Time

Replacement Rate Reform

(a) Non-Work Time Subsidized by Parental Leave

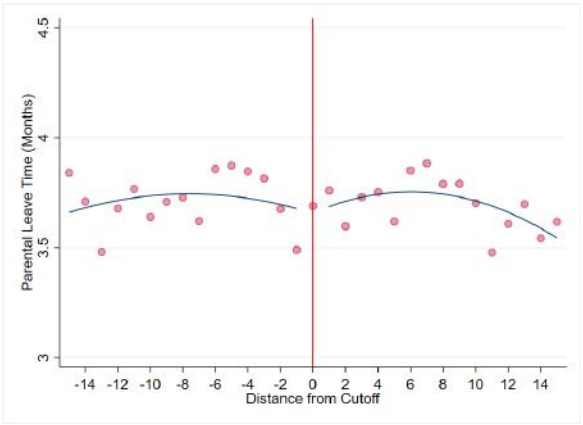



(b) Total Subsidized Time

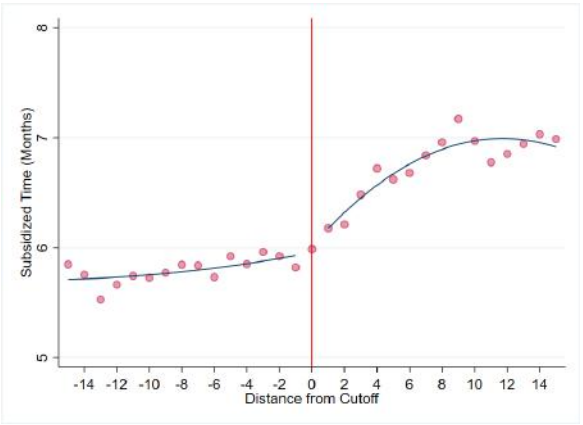



Duration Reform

(c) Non-Work Time Subsidized by Parental Leave




(d) Total Subsidized Time



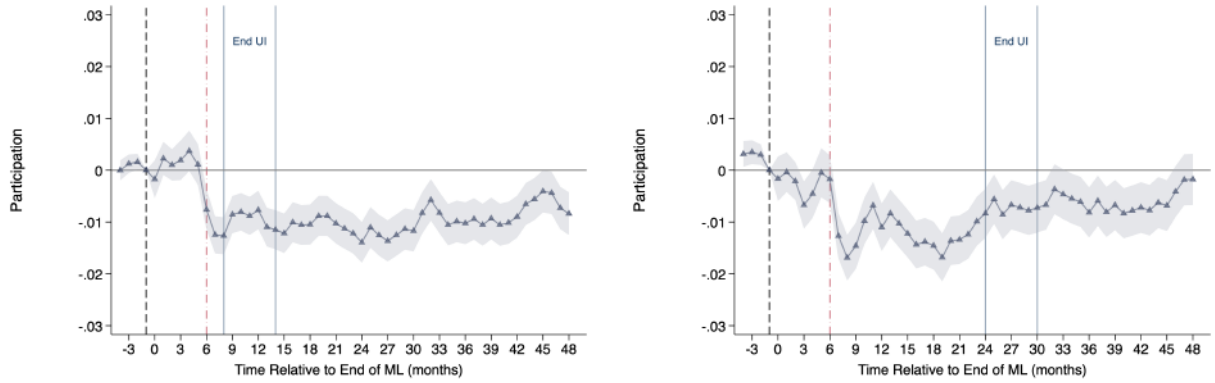

**Notes:** The graphs show the mean value of different outcome variables for the two reforms by month-of-end-of-compulsory-maternity-leave bin. Panel A and C refer to the time spent on parental leave during the first 9 months after the compulsory maternity leave. Panel B and D refer to the total *subsidized* time women spend out of the labor force (combining UI and PL benefits). The solid blue lines display quadratic spline estimates. The cutoffs refer to the cutoff date separating the *not treated* and the *partially treated* group as shown in Figure A4.

Figure 7: Effects of the Reforms on Labor Market Outcomes

**Participation**

(a) Replacement Rate Reform

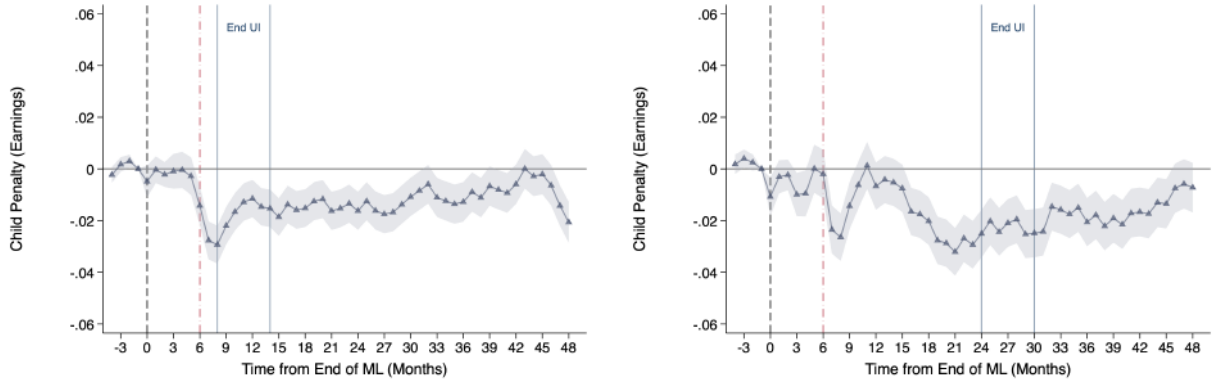
(b) Duration Reform




**Child Penalties in Earnings**

(c) Replacement Rate Reform

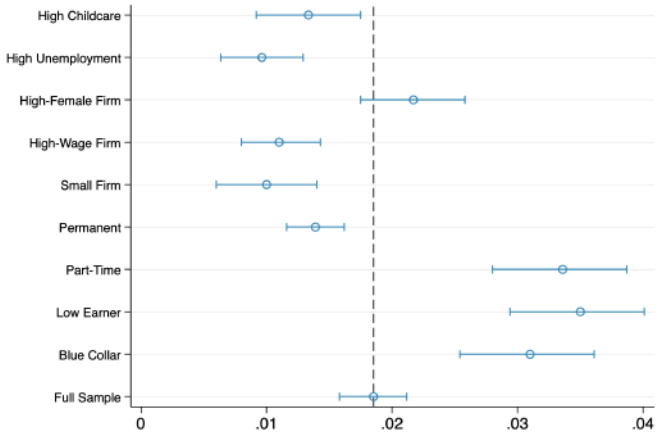
(d) Duration Reform



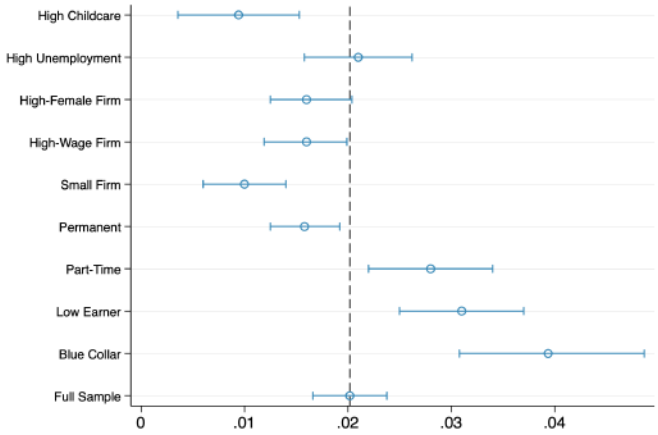

**Notes:** The figures show the estimated effects of increasing unemployment insurance generosity on the dynamics of female labor force participation and child penalties in gross labor earnings for both reforms. Panel A and B report the estimates of $\beta_k$ coefficients from specification 2 on the probability of working, defined as being matched with an employer, having positive earnings and not being on leave or on unemployment at time $t$, for the replacement rate reform and the duration reform respectively. Panel C and D report the estimates of $\beta_k$ coefficients from specification 2 on gross labor earnings in levels, normalized by the predicted earnings in the treatment group in the absence of children, $\frac{\hat{\beta_k}}{E\left[\hat{Y_{ik}}|k,T=1\right]}$. This allows to interpret the coefficients as the percentage-point change in the child penalty at time $t$ for women exposed to the more generous UI regime. Panel C reports result for the replacement rate reform and Panel D reports results for the duration reform. Shaded areas correspond to 95% confidence intervals. the red dot-dashed vertical line corresponds to the time of exhaustion of parental leave benefits. The "End UI" range characterizes the relative time periods that correspond to the exhaustion of UI benefits if mothers take up UI between $t = 0$ and $t = 6$.

Figure 8: Treatment Effect Heterogeneity: UI Take-Up

(a) Replacement Rate Reform



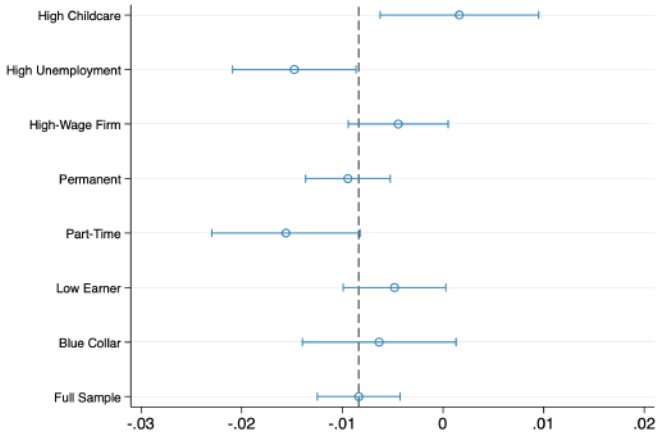

(b) Duration Reform



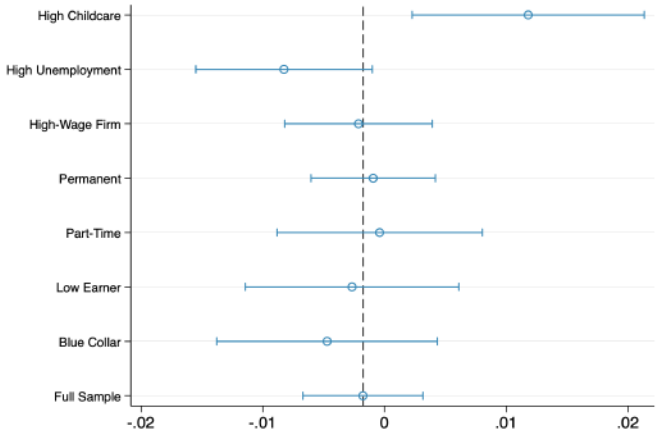

**Notes:** The figures show the heterogeneity in the treatment effect of UI reforms on UI take-up across different characteristics. I report the coefficient $\beta_k$ for $k = 9$ from specification 2 using as dependent variable the share of people enrolled in UI 9 months after the end of compulsory maternity leave for different samples. For binary characteristics (e.g., Blue Collar), the treatment effect for the group is reported. For continuous variables (earnings, childcare availability, unemployment rate), I report the treatment effect on the sample above the median (e.g., High Unemployment). The estimated treatment effect on the full sample is reported at the bottom and coincides with the vertical dashed line.

Figure 9: Heterogeneity in Labor Market Outcomes: Participation

(a) Replacement Rate Reform



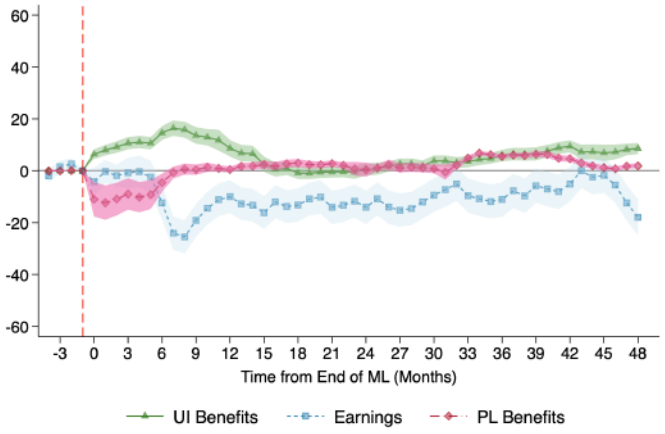

(b) Duration Reform



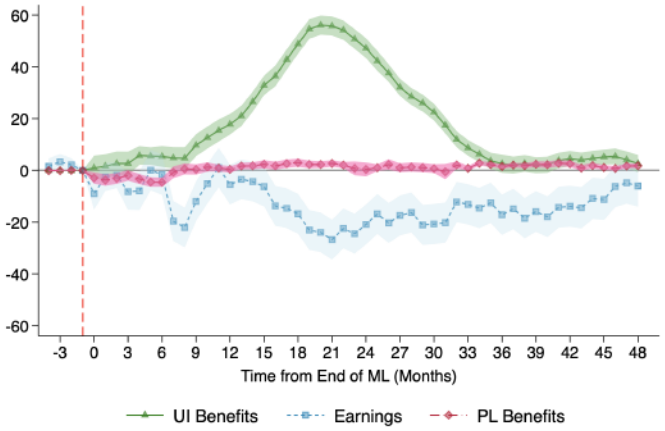

**Notes:** The figures show the heterogeneity in the effect of UI reforms on participation four years after compulsory maternity leave across different characteristics. I report the coefficient $\beta_k$ for $k = 48$ from specification 2 using as dependent variable the share of people working for different samples. For binary characteristics (e.g., Blue Collar), the treatment effect for the group is reported. For continuous variables (earnings, childcare availability, unemployment rate), I report the treatment effect on the sample above the median (e.g., High Unemployment). The estimated treatment effect on the full sample is reported at the bottom and coincides with the vertical dashed line.

Figure 10: Effects of the Reforms on Fiscal Costs

(a) Replacement Rate Reform




(b) Duration Reform




**Notes:** The figures show the fiscal effects of expanding unemployment insurance through a benefit increase (Panel A) and of expanding unemployment insurance through a duration increase (Panel B). They report the estimated $\beta_k$-coefficients from specification 2 using as dependent variable the individual-level monthly UI benefits (green series), the individual level monthly PL benefits (pink series) and the gross labor earnings, as a proxy for tax revenue (blue series). Standard errors are clustered at the individual level. Shaded areas correspond to 95% confidence intervals.

# Tables

Table 1: Pre-birth Characteristics: Replacement Rate Reform

| Variable | (1) Control Mean/SE | (2) Treatment Mean/SE | Difference (1)-(2) |
|---|---|---|---|
| Age | 33.425 | 33.432 | -0.007 |
| | (0.019) | (0.021) | |
| Tenure | 22.497 | 22.471 | 0.025 |
| | (0.022) | (0.024) | |
| Full Time | 0.668 | 0.659 | 0.008 |
| | (0.004) | (0.004) | |
| Permanent | 0.936 | 0.934 | 0.001 |
| | (0.001) | (0.001) | |
| White Collar | 0.602 | 0.600 | 0.002 |
| | (0.002) | (0.002) | |
| Blue Collar | 0.316 | 0.315 | 0.001 |
| | (0.002) | (0.002) | |
| Monthly Wage | 1419.824 | 1424.736 | -4.912 |
| | (2.421) | (2.598) | |
| Monthly Earnings | 1507.693 | 1510.473 | -2.780 |
| | (3.843) | (4.084) | |
| Days Worked | 24.589 | 24.556 | 0.033 |
| | (0.018) | (0.019) | |
| Small Firm | 0.390 | 0.393 | 0.003 |
| | (0.002) | (0.002) | |
| Observations | 67,437 | 59,857 | |

**Notes:** The table reports summary statistics of pre-birth characteristics and labour market performance of mothers who gave birth for the first time between January and May 2012 (Control) and January and May 2013 (Treatment). Column (3) reports differences in means between treatment and control group mothers. All variables are measured before the start of compulsory maternity leave (t = −5). Monetary quantities are expressed in 2010 prices. ***, **, and * indicate significant at 1,5 and 10% level.

Table 2: Pre-birth Characteristics Duration Reform

| Variable | (1) Control Mean/SE | (2) Treatment Mean/SE | Difference (1)-(2) |
|---|---|---|---|
| Age | 33.497 | 33.490 | -0.007 |
| | (0.023) | (0.024) | |
| Tenure | 22.613 | 22.552 | 0.059 |
| | (0.028) | (0.030) | |
| Full Time | 0.641 | 0.631 | 0.009* |
| | (0.003) | (0.003) | |
| Permanent | 0.913 | 0.910 | 0.003* |
| | (0.001) | (0.001) | |
| White Collar | 0.594 | 0.591 | 0.003 |
| | (0.002) | (0.002) | |
| Blue Collar | 0.308 | 0.308 | 0.000 |
| | (0.002) | (0.002) | |
| Monthly Wage | 1429.027 | 1425.064 | 3.962 |
| | (3.003) | (3.083) | |
| Monthly Earnings | 1468.003 | 1466.662 | 1.340 |
| | (4.740) | (4.822) | |
| Days Worked | 24.521 | 24.541 | -0.019 |
| | (0.022) | (0.022) | |
| Small Firm | 0.409 | 0.414 | -0.005 |
| | (0.002) | (0.002) | |
| Observations | 47,044 | 44,209 | |

**Notes:** The table reports summary statistics of pre-birth characteristics and labor market performance of mothers who gave birth for the first time between May and September 2014 (Control) and May and September 2015 (Treatment). Column (3) reports differences in means between treatment and control group mothers. All variables are measured before the start of compulsory maternity leave (t = −5). Monetary quantities are expressed in 2010 prices. ***, **, and * indicate significant at 1,5 and 10% level.

Table 3: UI Take-Up and Separations

| **Panel A** | *Replacement Rate Reform* | | |
|---|---|---|---|
| | Months since End of ML | | |
| | t=0 | t=6 | t=12 |
| UI take-up in t | 0.005*** | 0.003*** | -0.0001 |
| | (0.0007) | (0.0006) | (0.0004) |
| Separations in t | 0.0025*** | -0.00006 | 0.0006** |
| | (0.0008) | (0.0005) | (0.0003) |
| Observations | 127,294 | | |
| **Panel B** | *Duration Reform* | | |
| | Months since End of ML | | |
| | t=0 | t=5 | t=12 |
| UI take-up in t | 0.008*** | 0.003*** | -0.002** |
| | (0.001) | (0.0007) | (0.001) |
| Separations in t | 0.009*** | 0.003*** | -0.0001 |
| | (0.001) | (0.0006) | (0.0004) |
| Observations | 91,253 | | |

**Notes:** The table shows the estimated effects of increasing unemployment insurance generosity on the probability of taking up UI and separating from the pre-birth employer. Specifically, it reports the estimates of $\beta_k$ coefficients from specification 2 for $k = 0$, $k = 6$ and $k = 12$. Standard errors clustered at the individual level are reported in parentheses.

Table 4: Sources of Increase in UI Take-Up

| **Panel A** | *Replacement Rate Reform* | | |
|---|---|---|---|
| | Months since End of ML | | |
| | t=0 | t=6 | t=12 |
| Share in UI | 0.005*** | 0.014*** | 0.018*** |
| | (0.0007) | (0.001) | (0.0013) |
| Share in PL | -0.004*** | -0.006*** | 0.0003 |
| | (0.0019) | (0.0016) | (0.0007) |
| Share Work | -0.0017 | -0.0076** | -0.008*** |
| | (0.0018) | (0.0019) | (0.0019) |
| Observations | 127,294 | | |
| **Panel B** | *Duration Reform* | | |
| | Months since End of ML | | |
| | t=0 | t=6 | t=12 |
| Share in UI | 0.008*** | 0.016*** | 0.019*** |
| | (0.0010) | (0.0015) | (0.0018) |
| Share in PL | -0.005*** | -0.008*** | -0.0012 |
| | (0.0022) | (0.0018) | (0.0011) |
| Share Work | -0.0016 | -0.012*** | -0.011*** |
| | (0.0022) | (0.0022) | (0.0023) |
| Observations | 91,253 | | |

**Notes:** The table shows the estimated effects of increasing unemployment insurance generosity on the share of mothers on unemployment insurance, share of mothers on parental leave and share of mothers working, over time. Specifically, it reports the estimates of $\beta_k$ coefficients from specification 2 for $k = 0$, $k = 6$ and $k = 12$. Standard errors clustered at the individual level are reported in parentheses.

Table 5: Effects on Labor Market Outcomes

| **Panel A** | *Replacement Rate Reform* | | |
|---|---|---|---|
| | Months since End of ML | | |
| | t=12 | t=24 | t=48 |
| Share Working | -0.008*** | -0.014*** | -0.008** |
| | (0.0019) | (0.002) | (0.0021) |
| Share Working for Pre-Birth Employer | 0.0003 | -0.005* | -0.005* |
| | (0.0026) | (0.0028) | (0.0027) |
| Gross Labor Earnings | -7.98*** | -10.87** | -14.27*** |
| | (2.94) | (4.55) | (4.55) |
| Days Worked | 0.10*** | 0.18*** | 0.05 |
| | (0.038) | (0.035) | (0.031) |
| Daily Wage Rate | -1.48*** | -0.60 | -0.09 |
| | (0.51) | (0.41) | (0.344) |
| Observations | 125,469 | | |
| **Panel B** | *Duration Reform* | | |
| | Months since End of ML | | |
| | t=12 | t=24 | t=48 |
| Share Working | -0.011*** | -0.008*** | -0.0017 |
| | (0.0023) | (0.0024) | (0.0026) |
| Share Working for Pre-Birth Employer | -0.014*** | -0.016*** | -0.013*** |
| | (0.003) | (0.003) | (0.003) |
| Gross Labor Earnings | -8.35 | -23.78*** | -8.38 |
| | (5.31) | (5.35) | (5.44) |
| Days Worked | 0.11*** | -0.032 | -0.009 |
| | (0.044) | (0.041) | (0.041) |
| Daily Wage Rate | -0.165 | -1.68*** | -0.53 |
| | (0.482) | (0.411) | (0.401) |
| Observations | 91,312 | | |

**Notes:** The table shows the estimated effects of increasing unemployment insurance generosity on labor market outcomes. Specifically, it reports the estimates of $\beta_k$ coefficients from specification 2 for $k = 12$, $k = 24$ and $k = 48$. Standard errors clustered at the individual level are reported in parentheses. Monetary quantities are expressed in 2010 values. Days worked are defined as number of days worked conditional on employment. Daily wage rate is defined as earnings per day worked conditional on employment.

Table 6: Magnitudes of Earnings Losses

| | |
|---|---|
| **Replacement Rate Reform** | |
| Cumulative Earnings Losses $\hat{\beta}$ | -493.5*** |
| | (125.18) |
| UI Take-up $\hat{\delta}$ | 0.016*** |
| | (0.0012) |
| IV-scaled Coefficient $\hat{\gamma_{IV}} = \frac{\hat{\beta}}{\hat{\delta}}$ | -30,843 |
| $\varepsilon_b$ | -0.03 |
| Observations | 127,294 |
| **Duration Reform** | |
| Cumulative Earnings Losses $\hat{\beta}$ | -662*** |
| | (167.84) |
| UI Take-up $\hat{\delta}$ | 0.018*** |
| | (0.0017) |
| IV-scaled Coefficient $\hat{\gamma_{IV}} = \frac{\hat{\beta}}{\hat{\delta}}$ | -36,777 |
| $\varepsilon_B$ | -0.01 |
| Observations | 91,253 |

**Notes:** The table reports difference-in-difference estimates for the effect of the expansions in unemployment insurance on mothers' cumulative earnings through month 48 after the end of maternity leave ($\hat{\beta}$). It also reports the difference-in-difference estimates for the effect of the expansions in unemployment insurance on UI take-up ($\hat{\delta}$) and the IV-scaled coefficients for cumulative earnings $\hat{\gamma}_{IV} = \frac{\hat{\beta}}{\hat{\delta}}$, Standard errors clustered at the individual level are reported in parentheses. $\varepsilon_b$ is the elasticity of cumulative earnings with respect to benefit level $\varepsilon_b = \frac{dy}{db}\frac{b_0}{\bar{y}}$ where $b_0$ is the pre-reform replacement rate and $\bar{y}$ are the average pre-reform cumulative earnings up to $t = 48$. $\varepsilon_B$ is the elasticity of cumulative earnings with respect to benefit duration $\varepsilon_b = \frac{dy}{dB}\frac{B_o}{\bar{y}}$ where $B_0$ is the pre-reform potential benefit duration and $\bar{y}$ are the average pre-reform cumulative earnings up to $t = 48$.

Table 7: Complier Analysis: Replacement Rate Reform

| | Compliers C | Always-Takers AT | Never-Takers NT | Difference C-AT | Difference C-NT |
|---|---|---|---|---|---|
| Share | 0.016 | 0.093 | 0.891 | | |
| *Worker Characteristics* | | | | | |
| Age | 28.54 | 32.34 | 33.76 | -3.85*** | -5.28*** |
| | (0.625) | (0.059) | (0.021) | | |
| Permanent | 0.917 | 0.903 | 0.966 | 0.0132 | -0.049*** |
| | (0.03) | (0.004) | (0.001) | | |
| Full Time | 0.426 | 0.611 | 0.676 | -0.185*** | -0.249*** |
| | (0.054) | (0.006) | (0.002) | | |
| Blue Collar | 0.285 | 0.441 | 0.295 | -0.161*** | -0.011*** |
| | (0.05) | (0.006) | (0.002) | | |
| Pre-Birth Wage | 1026 | 1227 | 1503 | -200.9*** | -476.5*** |
| | (56.1) | (6.25) | (5.82) | | |
| *Firms Characteristics* | | | | | |
| Small (<15) | 0.80 | 0.64 | 0.36 | 0.15*** | 0.436*** |
| | (0.05) | (0.006) | (0.002) | | |
| High Quality Firm | 0.344 | 0.609 | 0.678 | -0.257*** | -0.324*** |
| | (0.059) | (0.006) | (0.0021) | | |
| High Share Female Firm | 0.658 | 0.596 | 0.479 | 0.062*** | 0.178*** |
| | (0.055) | (0.006) | (0.002) | | |
| *Environment Characteristics* | | | | | |
| High Childcare Availability | 0.271 | 0.239 | 0.335 | 0.03*** | -0.064*** |
| | (0.044) | (0.005) | (0.002) | | |
| High Unemployment Rate | 0.531 | 0.718 | 0.47 | -0.187*** | 0.061*** |
| | (0.059) | (0.006) | (0.0022) | | |

**Notes:** This table reports characteristics of compliers, never-takers, always-takers as well as the difference between the groups based on the methodology described in Section C. Compliers are those mothers who take up UI when under the more generous regime but would not have taken up UI under the less generous regime. For each of the variables and groups, the table reports means as well as standard errors (in parentheses) based on 5,000 bootstrap replications. All variables are measured before childbirth ($t = -5$). Monetary quantities are expressed in 2010 prices. High quality firm is an indicator equal to 1 if the worker is employed in a firm whose AKM firm effect is above the median. High unemployment rate is an indicator equal to 1 if the worker is employed in a region with unemployment rate above the pre-birth year median. High childcare availability is an indicator equal to 1 if the worker lives in a region where the availability of formal childcare (for 0-3 years old children) is above the median.

Table 8: Complier Analysis: Duration Reform

| | Compliers C | Always-Takers AT | Never-Takers NT | Difference C-AT | Difference C-NT |
|---|---|---|---|---|---|
| Share | 0.018 | 0.123 | 0.859 | | |
| *Worker Characteristics* | | | | | |
| Age | 31.941 | 31.541 | 33.963 | 0.401*** | -2.021*** |
| | (1.099) | (0.068) | (0.025) | | |
| Permanent | 0.981 | 0.914 | 0.966 | 0.085*** | 0.015*** |
| | (0.059) | (0.004) | (0.001) | | |
| Full-Time | 0.364 | 0.560 | 0.654 | -0.195*** | -0.290*** |
| | (0.11) | (0.006) | (0.002) | | |
| Blue Collar | 0.628 | 0.401 | 0.283 | 0.226*** | 0.344*** |
| | (0.114) | (0.007) | (0.002) | | |
| Pre-Birth Wage | 1425 | 1253 | 1696 | 171.64*** | -270.9*** |
| | (192.5) | (10.8) | (6.24) | | |
| *Firms Characteristics* | | | | | |
| Small (<15) | 0.46 | 0.68 | 0.36 | -0.216*** | 0.096** |
| | (0.11) | (0.006) | (0.002) | | |
| High Quality Firm | 0.499 | 0.552 | 0.671 | -0.052*** | -0.171*** |
| | (0.095) | (0.007) | (0.003) | | |
| High Share Female Firm | 0.306 | 0.631 | 0.479 | -0.325*** | -0.181*** |
| | (0.124) | (0.006) | (0.003) | | |
| *Environment Characteristics* | | | | | |
| High Childcare Availability | 0.188 | 0.260 | 0.281 | -0.072*** | -0.094*** |
| | (0.09) | (0.006) | (0.003) | | |
| High Unemployment Rate | 0.551 | 0.621 | 0.473 | -0.074*** | 0.078*** |
| | (0.09) | (0.006) | (0.003) | | |

**Notes:** This table reports characteristics of compliers, never-takers, always-takers as well as the difference between the groups based on the methodology described in Section C. Compliers are those mothers who take up UI when under the more generous regime but would not have taken up UI under the less generous regime. For each of the variables and groups, the table reports means as well as standard errors (in parentheses) based on 5,000 bootstrap replications. All variables are measured before childbirth ($t = -5$). Monetary quantities are expressed in 2010 prices. High quality firm is an indicator equal to 1 if the worker is employed in a firm whose AKM firm effect is above the median. High unemployment rate is an indicator equal to 1 if the worker is employed in a region with unemployment rate above the pre-birth year median. High childcare availability is an indicator equal to 1 if the worker lives in a region where the availability of formal childcare (for 0-3 years old children) is above the median.

Table 9: Construction of The MVPF

| | Replacement Rate Reform | Duration Reform |
|---|---|---|
| *Net Costs* | | |
| UI Benefits | 228 EUR | 898 EUR |
| PL Benefits | -67 EUR | -25 EUR |
| Tax Revenue | 134 EUR | 175 EUR |
| Total Net Costs | 295 EUR | 1048 EUR |
| | | |
| *Benefits* | | |
| $1+\gamma\frac{\Delta c}{c}$ | 1.17 | 1.30 |
| WTP | 155 EUR | 727 EUR |
| **MVPF** | 0.52 | 0.7 |
| | | |
| **MVPF no optimization** | -1.14 | 0.06 |

**Notes:** The table reports the different components used to calculate the MVPF for the replacement rate reform and for the duration reform in Section 7.

# Appendix

## A Data Construction

### A.1 Construction of the Dataset

I build my dataset using three primary sources of data: matched employer-employee records, maternity and parental leave registers, and unemployment insurance registers.

The unit of observation of the employer-employee matched data is the calendar month. This time aggregation poses an empirical challenge because maternity leave periods, parental leave periods, and UI benefits may be exhausted in the beginning, middle, or end of the calendar month. From the information available from the maternity and parental leave application registers, and the unemployment registers, I know the exact start and end date of each leave period. I assign each event to months in the following way:

- If I observe individuals working and on leave or unemployment insurance within the same month, I assign individuals to leave or unemployment insurance within that month if the start date of the leave period or unemployment insurance spell is before the 15th of the month
- If I observe individuals on two types of leave within the same month, I assign individuals the month to the type of leave that occupied the majority of the month

The definition of the month of the end of maternity leave is particularly important for my analysis which uses relative time from the end of maternity leave as the main time variation in the empirical strategy. Following the description above, I build the relative time variable in which $t = 0$ corresponds to the end of maternity leave in the following way:

- $t = 0$ corresponds to the last month in which I observe a maternity leave event in the matched employer-employee data if the day of the end of maternity leave reported in the maternity leave applications registered is before the 15th of the month
- $t = 0$ correspond to the month following the last month in which I observe a maternity leave event in the matched employer-employee data if the day of the end of maternity leave reported in the maternity leave applications register is after the 15th of the month

In the few cases in which I do not observe the exact date of the end of maternity leave, because I cannot match the employer-employee data with the maternity leave applications register, I assign

$t = 0$ in the following way. If I observe either parental leave events or unemployment insurance events following the end of maternity leave, I assign $t = 0$ based on information on the start date of these events. If women start working right after the end of maternity leave, I assign $t = 0$ as the last month in which I observe maternity leave. All the results are robust to the specification in which I assign $t = 0$ to the last month in which I observe maternity leave in the matched employer-employee datasets[23].

## A.2 Sample Selection

My primary analysis sample imposes two main sample restrictions. First, I restrict to first births as explained in Section 3.1. The results are robust to the inclusions of higher-parity births[24]. The second sample restriction I impose is that I exclude mothers whose temporary contract expires around the end of their maternity leave. In particular, I exclude mothers whose contract expires *before* the end of maternity leave reported in the register for applications. This is because this particular set of contract terminations is not an active choice of mothers, but it is mechanical and driven by the end of the contract. Therefore, these mothers are not entitled to job protection and face different incentives than other mothers. One potential concern is that part of the results shown in Figure 5 is mechanically driven by temporary contracts ending. I believe this is unlikely because there is no reason to believe that the reforms impacted the timing of ending and duration of temporary contracts, usually set to one or two years. Second, given the restriction explained above, temporary contracts represent a small portion of my sample. Finally, in Figure A11, I report a robustness check restricting to mothers who had a permanent contract before birth.

The final dataset is a balanced panel containing all mothers who gave birth to their first child (parity one) between 2012 and 2016. The panel spans from 2 years before the end of their compulsory maternity leave to 4 years after and comprises approximately 640,000 mothers employed in the private sector before giving birth. Note that with *balanced* panel I mean that I follow a mother from 2 years before birth to 4 year after birth by observing whether she is in one of this four categories:

1. Employed in the private sector
2. On any type of leave
3. On unemployment insurance

[23] Results available upon request.
[24] Results available upon request.

4. Not employed, on leave or unemployed. Not that in this case I cannot distinguish whether a mother exited the labor force or moved to the public sector or became self-employed.

Importantly, when I refer to non-participation, except for the periods of unemployment insurance, I specifically refer to non-participation to the private sector as I cannot exclude the possibility that the mother became self-employed or moved to public employment.

# B Alternative Empirical Strategy: Regression Discontinuity Design

In addition to the difference-in-differences design introduced in Section 3.2, I can use a regression discontinuity design using as cutoff the end of maternity leave date dividing fully untreated mothers to partially treated mothers (Figure A4). Following Deshpande 2016, I estimate the RD parametrically. The estimating equation is:

$$Y_i = \alpha + \beta \mathbb{1}[\tau_i \geq 0] + \sum_{k=1}^{K} \alpha_k \tau_i^k + \sum_{k=1}^{K} \alpha_k \tau_i^k \mathbb{1}[\tau_i \geq 0] + X_i^{'}\delta + \varepsilon_i \tag{8}$$

where $\tau_i$ is the end of maternity leave date of mother $i$ normalized so that $\tau = 0$ at the cutoff date (May 1, 2012 for the replacement rate reform and September 1, 2014 for the duration reform). $\tau_i^k$ is a polynomial of order $k$ in the mother's end of compulsory maternity leave date. $X_i$ is a matrix of covariates. The coefficient of interest is $\beta$, which gives the effect on $Y$ of ending the compulsory maternity leave right before or right after the cutoff.

The key assumption for identification in an RD design is that treatment is as good as randomly assigned in a neighborhood of the cutoff and that counterfactual outcomes are smooth at the cutoff. A potential threat to the RD design is strategic manipulation of the running variable. This is unlikely in this institutional setting for different reasons. First, the unemployment insurance reforms were unexpected and hard to anticipate. Second, the possibility of UI take up within the choice period, allowed mothers ending their maternity leave even before the actual introduction of the reform to be eligible for the more generous UI regime making threshold manipulation not necessary. Finally, the end of compulsory maternity leave date is overall hard to manipulate since it is strictly related to the exact date of birth. Unless mothers reacted to the unemployment insurance reforms by manipulating the timing of births, manipulation of the threshold is unlikely. Appendix Figure A6 shows the distribution of end of maternity leave dates around the cutoff dates (solid blue lines) and around the actual implementation of the reforms dates (dashed blue lines). On average, while there is expected seasonality across different months driven by seasonality in the number of births and an overall decrease in number of births over time, the sample of mothers ending their maternity leave around the introduction of the two reforms are balanced. I formally test for a discontinuity in the density function at the cutoff using the test proposed by McCrary (2008). As shown in Figure **??**, the McCrary test do not reject the null hypothesis of no discontinuity at the threshold. I also check for whether covariates are balanced around the cutoff. I perform a test for a number of pre-birth demographic and labor market characteristics and find that covariates are overall balanced.

# C Complier Characteristics

Consider a binary variable $Z \in \{0,1\}$ that captures whether mothers are eligible for the more generous UI regime. $Z$ effectively shifts a component of the mother's outside option $b + Z\Delta b$, by either increasing UI replacement rate or by increasing UI duration. Let $D \in \{0,1\}$ indicate if a mother takes up UI, whether she was treated or not. Let $D_0$ and $D_1$ denote the potential values that $D$ takes fro $Z = 0$ and $Z = 1$ respectively. We can characterize the three groups of mothers described above by their potential outcomes: always-takers (AT) with potential outcomes $(D_0 = 1, D_1 = 1)$ and share $\pi^{AT}$, never-takers (NT) with potential outcomes $(D_0 = 0, D_1 = 0)$ and share $\pi^{NT}$and compliers (C) with potential outcomes $(D_0 = 0, D_1 = 1)$ and share $\pi^{C}$.

In my setting, I compare eligible $(c_1)$ and ineligible $(c_0)$ cohorts, before and after childbirth $(t_0$ and $t_1)$ respectively. Therefore, $Z = 1$ for $c_1$ and 0 otherwise.

I first estimate the share of always-takers, compliers and never-takers. This is easily done by running the following simple regression: $Y_i = \alpha + \gamma T_i + \varepsilon_i$ , where $T_i = 1$ if $i$ is eligible for the more generous UI regime and 0 otherwise and $Y_i$ is an indicator equal to 1 if individual $i$ took up UI during the "choice period", namely at any $t \in [0, 9]$ from the end of compulsory maternity leave. We have that $\pi^C = \hat{\gamma}, \pi^{AT} = \hat{\alpha}, \pi^{NT} = 1 - \hat{\alpha} - \hat{\gamma}$.

Next, I estimate expected value of a characteristics $x$for the three groups. Estimating the expected value of a characteristic $x$ for *never-takers* is straightforward. All individuals in $c_1$ who do not take up UI in the "choice period" are never-takers if we assume that the standard monotonicity assumption in the instrumental variable literature, $(D_1 - D_0 \geq 0)$, holds. We can estimate the characteristic $x$ of a never-taker by the corresponding sample mean $\frac{1}{N_{c_1}^{nt}} \sum_{i \in c_1} x_i 1(D_i = 0)$.

Estimating the expected value of a characteristic for always-takers and compliers is more challenging. The expected value of a characteristic $x$ for mothers in cohort $c_1$ who do take up UI is a weighted average weighted average of the expected value of $x$ for compliers and always-takers, where the weights represent the share of compliers and always-takers. Therefore the expected value of $x$ for compliers is:

$$E^C[x] = E\left[x|D_0 = 0, D_1 = 1, c_1\right] = \frac{\pi^C + \pi^{AT}}{\pi^C} E\left[x|D_1 = 1, c_1\right] - \frac{\pi^{AT}}{\pi^C} E\left[x|D_0 = 1, c_1\right] \tag{9}$$

We can estimate the RHS of equation 9 empirically. $E\left[x|D_1 = 1, c_1\right]$ is the expected value of $x$ for all-takers, namely all mothers taking up UI after the reform. It is possible to estimate this using

the corresponding sample mean: $\left(\frac{1}{N_{c_1}^{all-takers}}\right)\sum_{i\in c_1} x_i 1(D_i = 1)$. Calculating $E\left[x|D_0 = 1, c_1\right]$ is more difficult, because we cannot know whether a worker who takes up UI after the reform would have taken up UI before the reform. Because of monotonicity we know that individuals who take up UI before the reform, also take up UI after the reform. Therefore, if trends in $x$ are parallel across cohorts and $Z$ is independent from $D$ and $x$, we have that $E\left[x|D_0 = 1, c_1\right] = E\left[x|D_0 = 1, c_0\right]$. This can be estimated by the corresponding sample mean $\left(\frac{1}{N_{c_0}^{always-takers}}\right)\sum_{i\in c_0} x_i 1(D_i = 1)$.

# D Welfare Effects Derivation

### Welfare Effect of Changes in Benefits

The government maximizes equation 4 subject to the budget constraint 5 and to the condition that the individual chooses her behavior optimally. Individual behavior is a function of PL benefits and durations so we can write: $\tau(b, B) = \frac{L(b,B)}{T-D(b,B)}b + \frac{E}{T-D(b,B)}$.

The government problem is $max_{b.B}W(b, B, \tau(b, B))$. The marginal effect of increasing $b$ on welfare is given by:

$$\frac{dW}{db} = \int_0^B S_t dt u^{'}(c_{l,t\leq B}) - \int_0^T [1 - S_t]\, dt v^{'}(c_e)\frac{d\tau}{db}$$

$$\frac{dW}{db} = Bu^{'}(b) - (T - D)v^{'}\frac{d\tau}{db}$$

where we use the envelope theorem. With some rearrangements we get:

$$\frac{dW}{db}\frac{1}{v^{'}(c_e)} = L \times \frac{u^{'}(c_{l,t\leq B}) - v^{'}(c_e)}{v^{'}(c_e)} - \left(\frac{dL}{db}b + \frac{dD}{db}\tau\right)$$

Normalizing by $L$, which is how much more it is transferred to women on leave, we get:

$$\frac{dW}{db}\frac{1}{Lv^{'}(c_e)} = \frac{u^{'}(c_{l,t\leq B}) - v^{'}(c_e)}{v^{'}(c_e)} - \left(\eta_{L,b} + \eta_{D,b}\frac{D}{L}\frac{\tau}{b}\right)$$

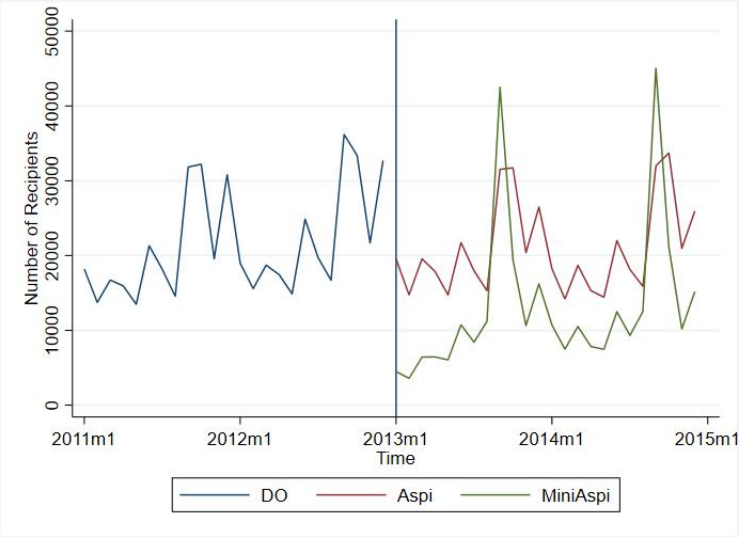
which corresponds to equation 6.

# E Additional Figures

Figure A1: Example of Use of Unemployment Insurance as Alternative to Parental Leave

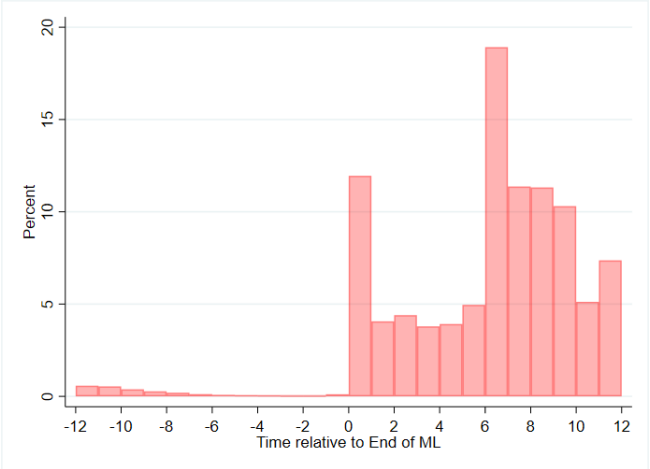

**Notes:** The figure shows an example of a website advertising the possibility of using unemployment insurance as an alternative to parental leave benefits.

Figure A2: Changes in UI Eligibility: Replacement Rate Reform



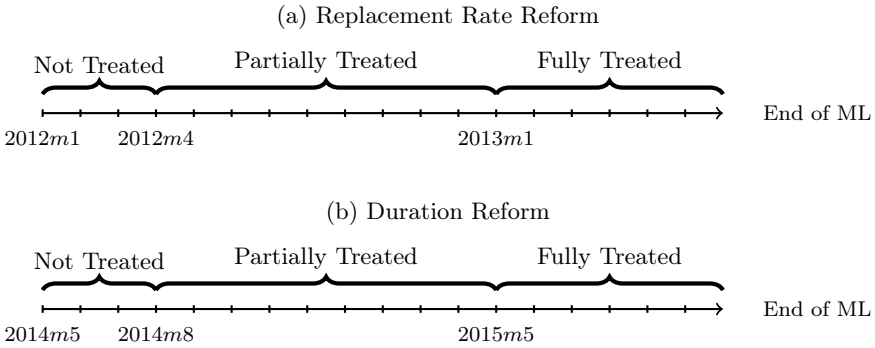

**Notes:** The figure reports the number of UI claims over time by women under the age of 50 before and after the replacement rate reform s distinguishing between *Aspi* and *MiniAspi*. Details on the characteristics of the subsidies are presented in Section 2.

Figure A3: Distribution of UI Take Up by Time Relative to End of ML



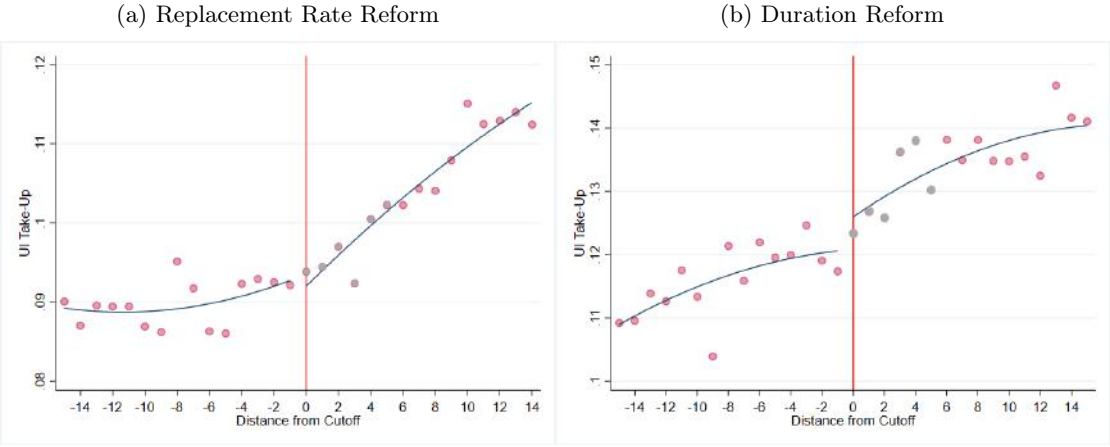

**Notes:** The figures reports the distribution of UI take-up by time relative to the end of compulsory maternity leave in 2012.

Figure A4: Eligibility for UI Regimes by End of Maternity Leave Date



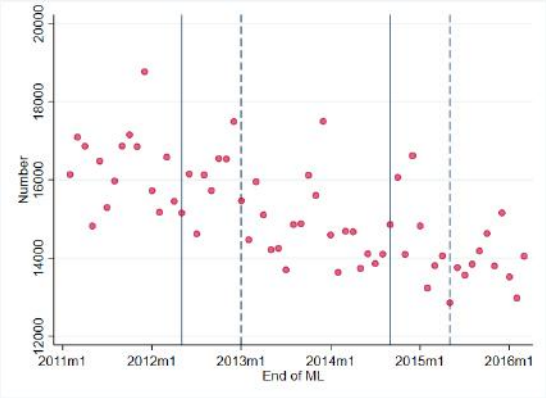

**Notes:** The figure illustrates for which dates the mothers were eligible for different unemployment insurance regimens, depending on the end of their maternity leave. Panel A shows the dates for the replacement rate reform. Panel B shows the dates for the duration reform.

Figure A5: Effect of the Reforms on UI Take-Up After Childbirth




**Notes:** The plots illustrate the share of mothers taking up unemployment insurance before their child's first birthday by month-of-end-of-ML bin for different UI regimes. The solid blue lines display quadratic spline estimates. The grey dots on the right of the cutoffs represent partially treated mothers (as shown in Figure A4). Panel A presents results for the replacement rate reform while Panel B presents results for the duration reform. The cutoffs refer to the cutoff date separating the *not treated* and the *partially treated* group as shown in Figure A4.

Figure A6: Frequency of End of Maternity Leave Dates

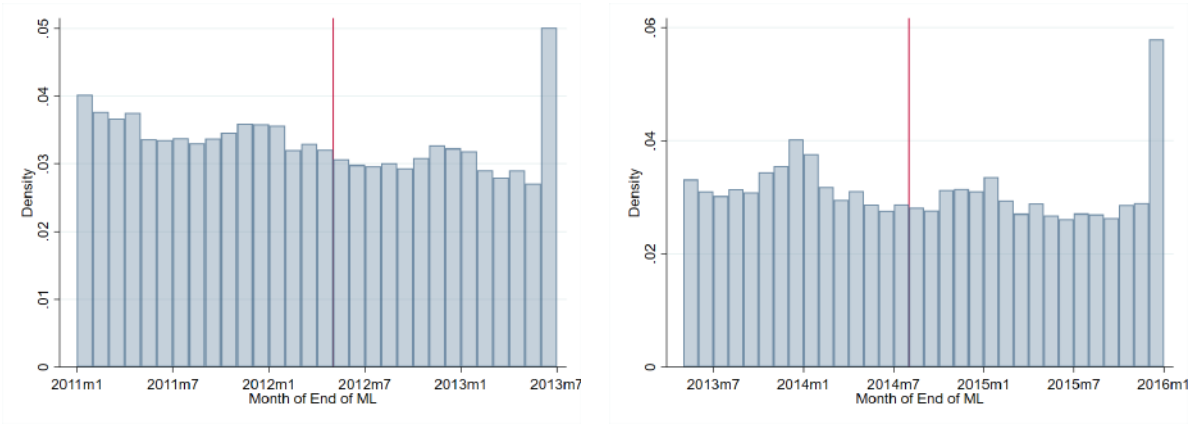



**Notes:** The figure reports the number of observations in each month-of-end-of-maternity-leave bin for the entire sample. The solid blue lines correspond to the cutoff dates using in the RD design specified in equation 8 (specifically to the cutoff date separating the *not treated* and the *partially treated* group in Figure A4). The dashed blue lines correspond to the actual time of introduction of the reforms (specifically to the cutoff date separating the *partially treated* and the *fully treated* group in Figure A4) for both reforms.

Figure A7: Density of End of Maternity Leave Dates and McCrary Test

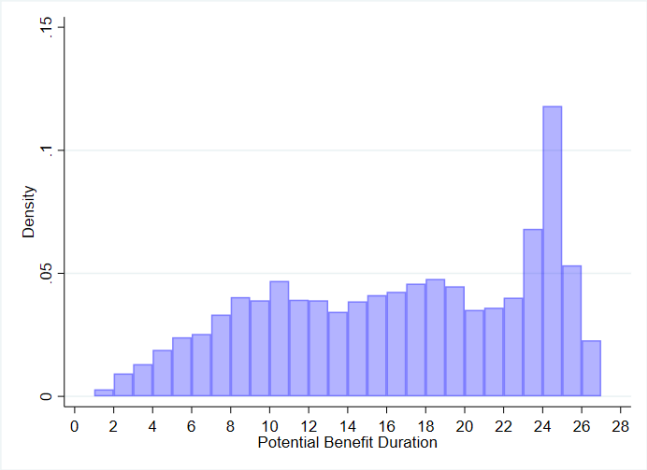
(a) Replacement Rate Reform

(b) Duration Reform




**Notes:** The graphs plot the density function of mothers by end-of-compulsory-maternity-leave date for the replacement rate reform (Panel A) and for the duration reform (Panel B). The test statistics (and associated p-value in parentheses) for the McCrary test of the discontinuity in the probability density function of the running variable at threshold is 0.639 (0.552) for the replacement rate reform and -0.564 (0.572) for the duration reform.

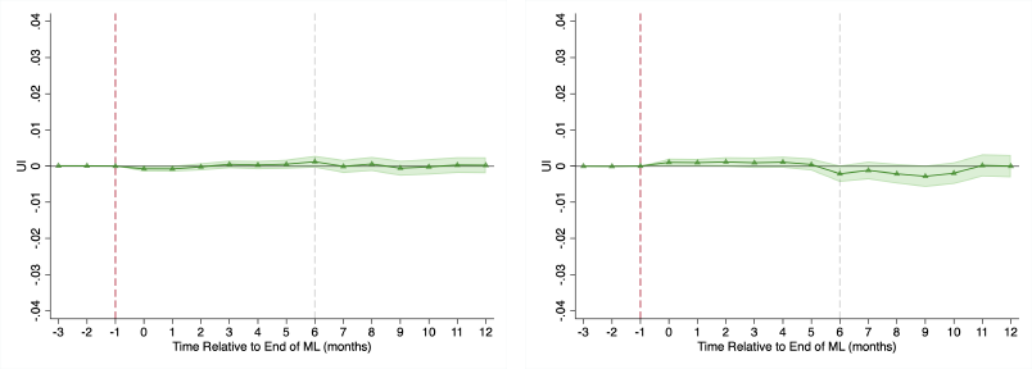
Figure A8: Distribution of UI Potential Benefit Duration After the Duration Reform




**Notes:** The figure shows the distribution of potential benefit duration for mothers taking up UI after the duration reform.

Figure A9: Placebo Specifications

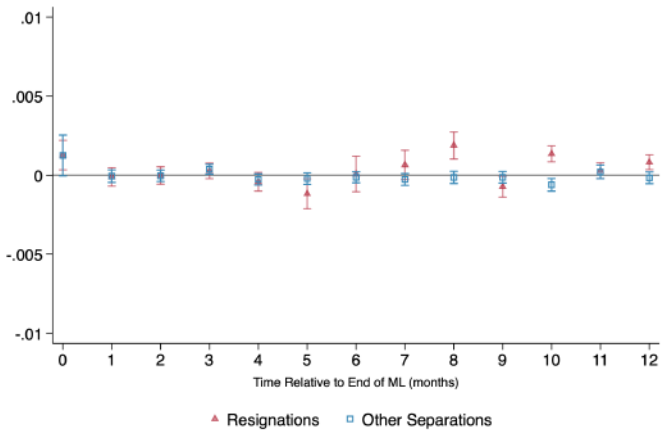
(a) 2009-2010 (b) 2010-2011



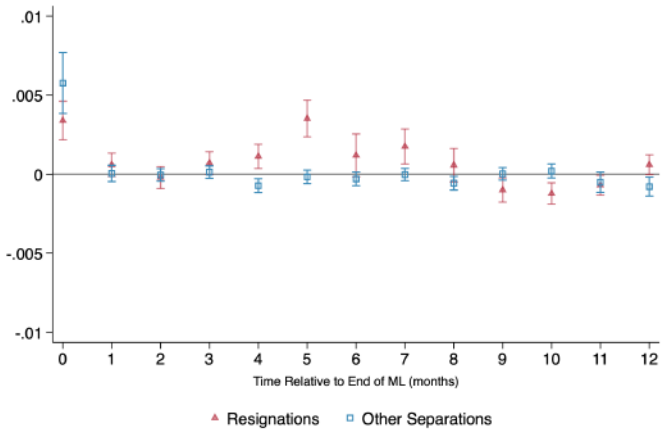

**Notes:** The figures report difference-in-differences estimates of the coefficients $\beta_k$ from 2 where the dependent variable is the probability that a mother is on unemployment insurance at relative time $t$ (where $t = 0$ corresponds to the time of end of compulsory maternity leave) for different placebo cohorts. The figure reports the 95% confidence intervals. Standard errors are clustered at the individual level.

Figure A10: Effects of the Reforms on Separations from Pre-Birth Employer: Decomposition

(a) Replacement Rate Reform



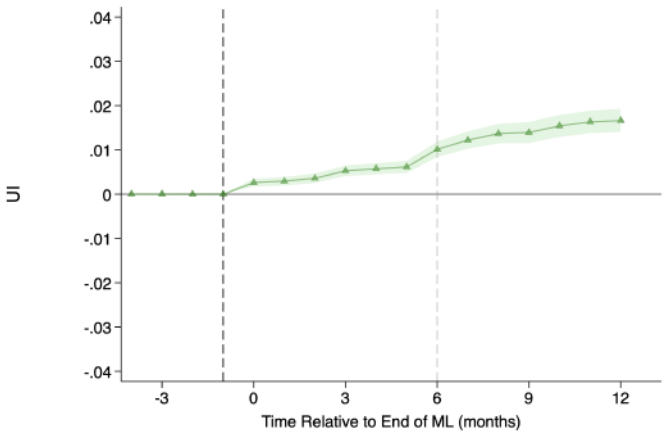

(b) Duration Reform



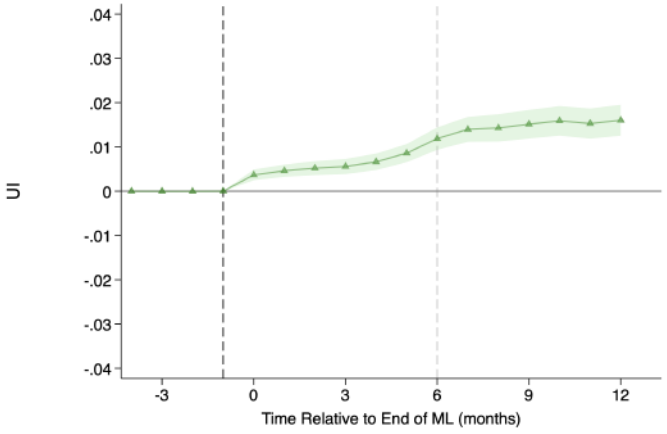

**Notes:** The figures report difference-in-differences estimates of the coefficients $\beta_k$ from 2 where the dependent variable is the probability that a mother resigns from the pre-birth employer (triangle) or the probability that a mother separates voluntarily through other types of separations (square) at relative time $t$ (where $t = 0$ corresponds to the time of end of compulsory maternity leave). The figure reports the 95% confidence intervals. Standard errors are clustered at the individual level. Panel A reports results for the replacement rate reform while panel B reports results for the duration reform.

Figure A11: Effects of the Reforms on UI Take-Up: Restricting to Permanent Workers

(a) Replacement Rate Reform

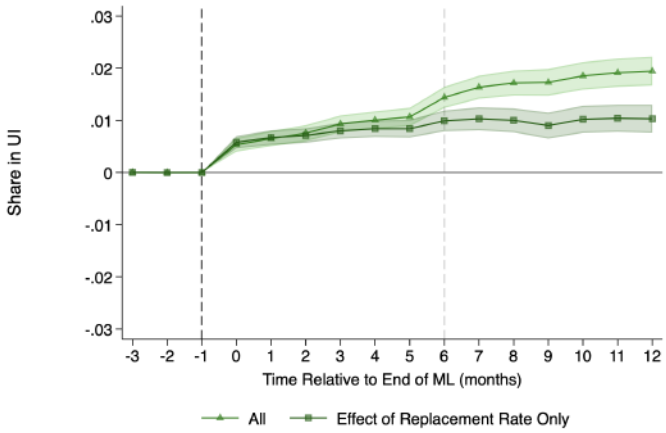



(b) Duration Reform




**Notes:** The figures report difference-in-differences estimates of the coefficients $\beta_k$ from 2 estimated on the probability that a mother is on UI at relative time $t$ restricting the sample to mothers with a permanent contract before childbirth. $t = 0$ corresponds to the time of end of compulsory maternity leave. Shaded areas correspond to 95% confidence intervals. Standard errors are clustered at the individual level. Panel A reports results for the replacement rate reform while panel B reports results for the duration reform.

Figure A13: Replacement Rate Reform Decomposition: Eligibility vs Replacement Rate

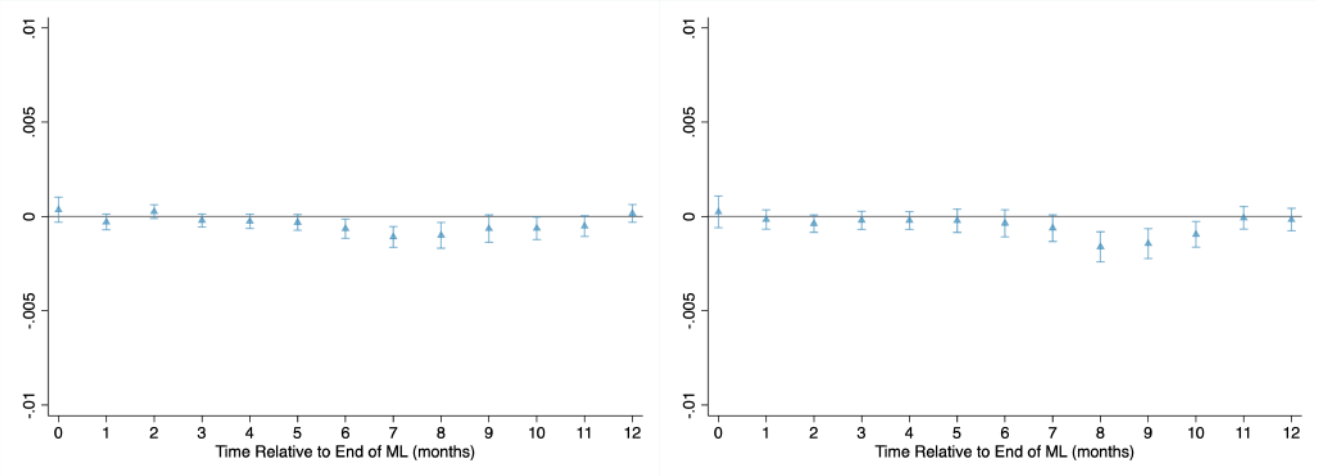



**Notes:** The graphs show the results of the estimation of specification 2 on take-up of unemployment insurance after the replacement rate reform. The light green series (triangle) reports the results using as dependent variable the overall unemployment insurance take-up while the dark green series (square) reports the results using as dependent variable unemployment insurance take-up excluding the take-up of *Miniaspi*. See Section 2 for details.

Figure A12: Robustness: Effect on Layoffs

(a) Replacement Rate Reform

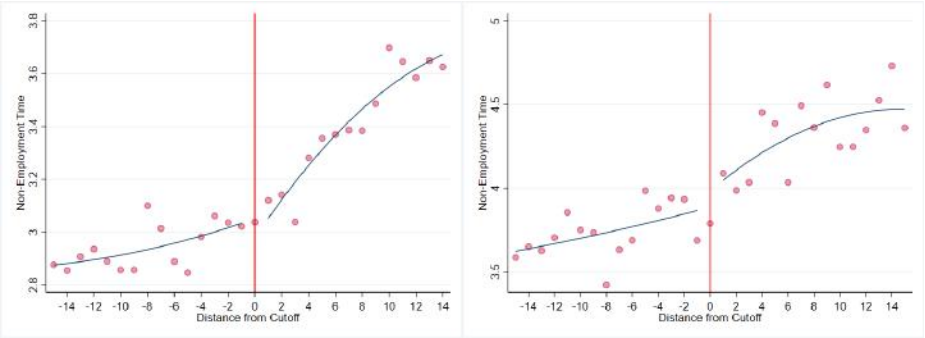
(b) Duration Reform




**Notes:** The figures report difference-in-differences estimates of the coefficients $\beta_k$ from 2 where the dependent variable is the probability that a mother resigns or separates from the pre-birth employer through a layoff, firm closure or due to just cause at relative time $t$ (where $t = 0$ corresponds to the time of end of compulsory maternity leave). The figure reports the 95% confidence intervals. Standard errors are clustered at the individual level. Panel A reports results for the replacement rate reform while panel B reports results for the duration reform.

Figure A14: Effect of the Reforms on Non-Participation Time

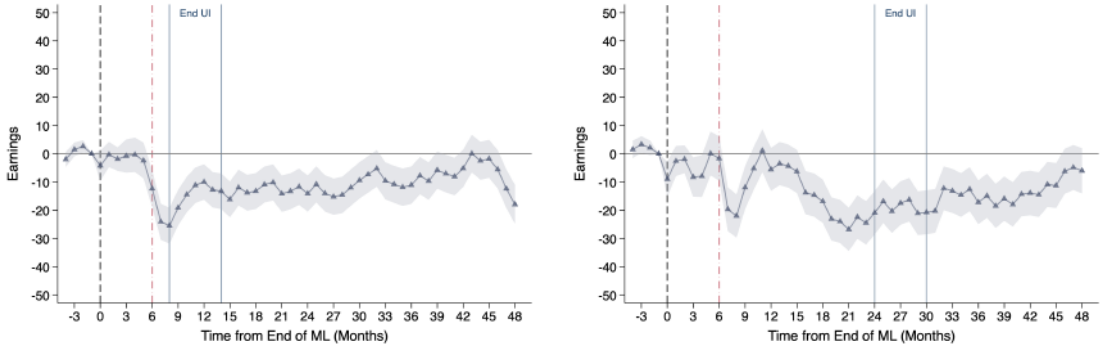
(a) Replacement Rate Reform (b) Duration Reform




**Notes:** The graphs show the mean value of the total time spent out of the labor force and not on parental leave after childbirth for the two reforms by month-of-end-of-compulsory-maternity-leave bin. Panel A refers to the replacement rate reform while Panel B refers to the duration reform. The solid blue lines display quadratic spline estimates. The cutoffs refer to the cutoff date separating the *not treated* and the *partially treated* group as shown in Figure A4.

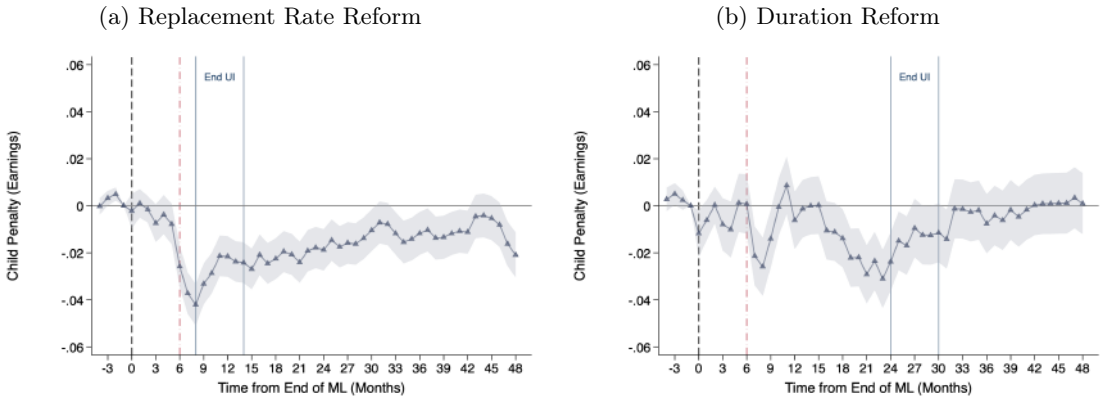
Figure A15: Effects of the Reforms on Earnings Levels

(a) Replacement Rate Reform (b) Duration Reform




**Notes:** The figures show the estimated effects of increasing unemployment insurance generosity on the dynamics of female earnings for both reforms. Each panel reports estimates of $\beta_k$ coefficients from specification 2, using as dependent variable gross labor earnings in levels (to include zeros from non-participation). Panel A reports the results for the replacement rate reform while Panel B reports the results for the duration reform. Shaded areas correspond to 95% confidence intervals.

Figure A16: Effects of the Reforms on Child Penalties: Fertility

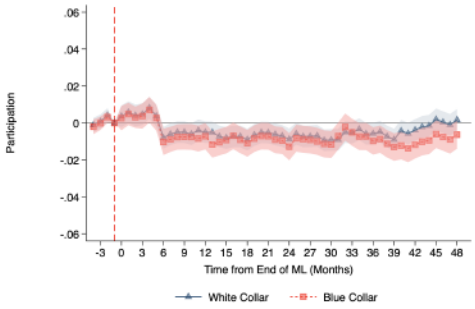



**Notes:** The figures show the estimated effects of increasing unemployment insurance generosity on the dynamics of female earnings for both reforms. Each panel reports estimates of $\beta_k$ coefficients from specification 2, normalized by the predicted earnings in the treatment group in the absence of children, $\frac{\hat{\beta_k}}{E\left[\hat{Y_{ik}}|k,T=1\right]}$, which correspond to the percentage-point change in the child penalty at time $t$ for women exposed to the more generous UI regime. The sample is restricted to mothers with only one child (completed fertility of one). Panel A reports the results for the replacement rate reform while Panel B reports the results for the duration reform. Shaded areas correspond to 95% confidence intervals.

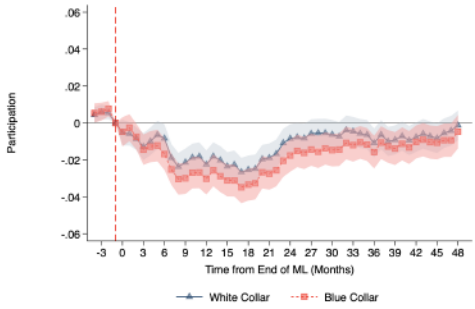
Figure A17: Effects of the Reforms on Participation: Heterogeneity

**Occupation**

(a) Replacement Rate Reform

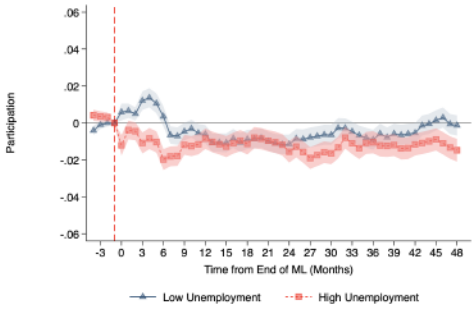



(b) Duration Reform

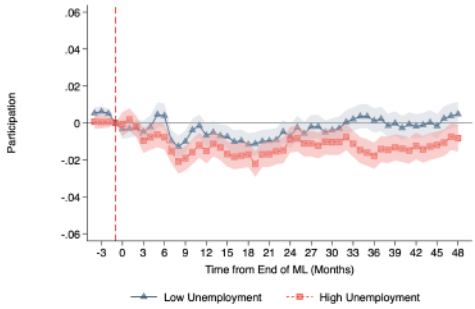



**Unemployment Rate**

(c) Replacement Rate Reform

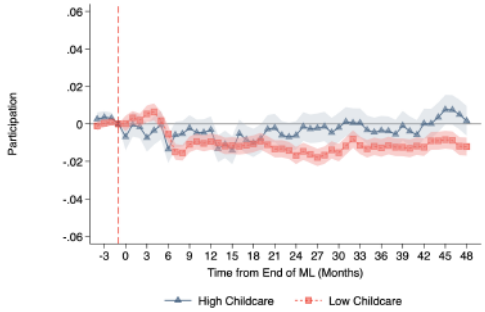



(d) Duration Reform

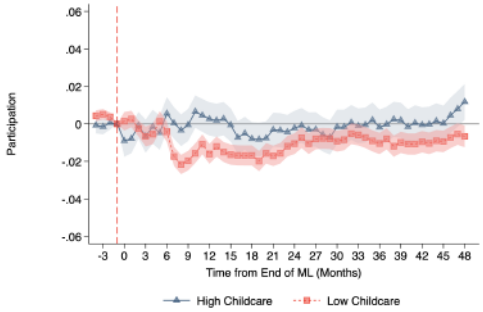



**Childcare Availability**

(e) Replacement Rate Reform




(f) Duration Reform



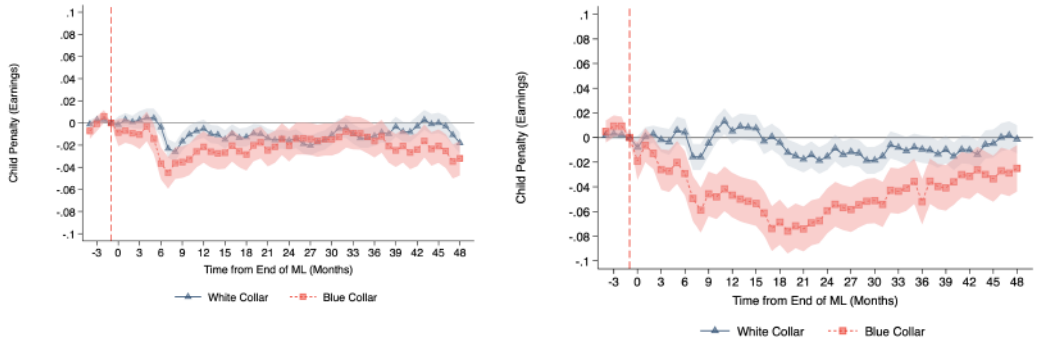

**Notes:** The figures show the estimated effects of increasing unemployment insurance generosity on the dynamics of female labor force participation for both reforms. Each panel reports estimates of $\beta_k$ coefficients from specification 2 on the probability of working (defined as being matched with an employer, having positive earnings and not being on leave or on unemployment at time $t$) for different subgroups of workers. Panel A and B report the results splitting the sample by occupation. Panel C and D report the results by the pre-birth unemployment rate in the region of residence. Panel E and F report the results by childcare availability. Shaded areas correspond to 95% confidence intervals.

Figure A18: Effects of the Reforms on Child Penalty In Earnings: Heterogeneity

## Occupation

(a) Replacement Rate Reform

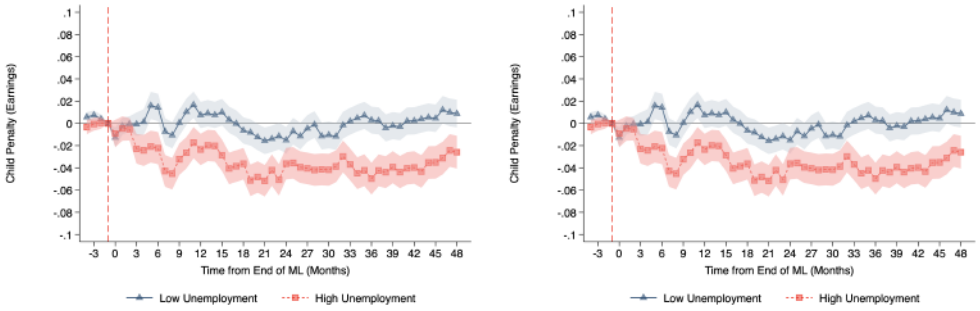
(b) Duration Reform




## Unemployment Rate

(c) Replacement Rate Reform

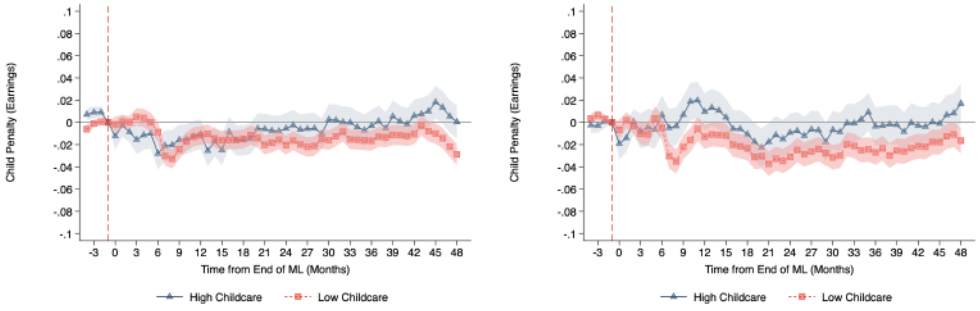
(d) Duration Reform




## Childcare Availability

(e) Replacement Rate Reform

(f) Duration Reform




**Notes:** The figures show the estimated effects of increasing unemployment insurance generosity on the dynamics of female gross labor earnings for both reforms. Each panel reports estimates of $\beta_k$ coefficients from specification 2 normalized by the predicted earnings in the treatment group in the absence of children, $\frac{\hat{\beta_k}}{E[\hat{Y_{ik}}|k,T=1]}$, which correspond to the percentage-point change in the child penalty at time $t$ for women exposed to the more generous UI regime. Panel A and B report the results splitting the sample by occupation. Panel C and D report the results by unemployment rate in the region of residence. Panel E and F report the results by childcare availability. Shaded areas correspond to 95% confidence intervals.

# F Additional Tables

Table A1: Illustration of Partially Treated Mothers: Replacement Rate Reform

| | Timing of Resignations | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Date of End of ML | $t=3$ | $t=4$ | $t=5$ | $t=6$ | $t=7$ | $t=8$ | $t=9$ | $t=10$ | $t=11$ | $t=12$ |
| up to 2012m3 | Old | Old | Old | Old | Old | Old | Old | Old | Old | Old |
| 2012m4 | Old | Old | Old | Old | Old | Old | Old | Old | Old | New |
| 2012m5 | Old | Old | Old | Old | Old | Old | Old | Old | New | New |
| 2012m6 | Old | Old | Old | Old | Old | Old | Old | New | New | New |
| 2012m7 | Old | Old | Old | Old | Old | Old | New | New | New | New |
| 2012m8 | Old | Old | Old | Old | Old | New | New | New | New | New |
| 2012m9 | Old | Old | Old | Old | New | New | New | New | New | New |
| 2012m10 | Old | Old | Old | New | New | New | New | New | New | New |
| 2012m11 | Old | Old | New | New | New | New | New | New | New | New |
| 2012m12 | Old | New | New | New | New | New | New | New | New | New |
| 2013m1 onwards | New | New | New | New | New | New | New | New | New | New |

**Notes:** The table shows how the eligibility for more generous unemployment insurance benefits changes with the end of maternity leave date and with the timing of resignations for the replacement rate reform. The new regime was effective from January 2013 onwards, implying that mothers ending their maternity leave from January 2013 were fully eligible for the reform, no matter the timing of their resignations. Mothers ending their maternity leave before April 2012 were fully ineligible for the reform. Mothers ending their maternity leave between April 2012 and December 2012 were partially eligible: they could access the more generous benefits only if they decided to resign at particular time periods. For example women ending their maternity leave in September 2012 would not be eligible for the more generous leave if they decided to resign right after the end of maternity leave but they would if they decided to resign from $t = 7$ to $t = 12$.

Table A2: Illustration of Partially Treated Mothers: Duration Reform

| | Timing of Resignations | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Date of End of ML | $t=3$ | $t=4$ | $t=5$ | $t=6$ | $t=7$ | $t=8$ | $t=9$ | $t=10$ | $t=11$ | $t=12$ |
| up to 2014m7 | Old | Old | Old | Old | Old | Old | Old | Old | Old | Old |
| 2014m8 | Old | Old | Old | Old | Old | Old | Old | Old | Old | New |
| 2014m9 | Old | Old | Old | Old | Old | Old | Old | Old | New | New |
| 2014m10 | Old | Old | Old | Old | Old | Old | Old | New | New | New |
| 2014m11 | Old | Old | Old | Old | Old | Old | New | New | New | New |
| 2014m12 | Old | Old | Old | Old | Old | New | New | New | New | New |
| 2015m1 | Old | Old | Old | Old | New | New | New | New | New | New |
| 2015m2 | Old | Old | Old | New | New | New | New | New | New | New |
| 2015m3 | Old | Old | New | New | New | New | New | New | New | New |
| 2015m4 | Old | New | New | New | New | New | New | New | New | New |
| 2015m5 onwards | New | New | New | New | New | New | New | New | New | New |

**Notes:** The table shows how the eligibility for more generous unemployment insurance benefits changes with the end of maternity leave date and with the timing of resignations for the duration. The new regime was effective from May 2015 onwards, implying that mothers ending their maternity leave from May 2015 were fully eligible for the reform, no matter the timing of their resignations. Mothers ending their maternity leave before August 2014 were fully ineligible for the reform. Mothers ending their maternity leave between August 2014 and April 2015 were partially eligible: they could access the more generous benefits only if they decided to resign at particular time periods. For example women ending their maternity leave in January 2015 would not be eligible for the more generous leave if they decided to resign right after the end of maternity leave but they would if they decided to resign from $t = 7$ to $t = 12$.

Table A3: Summary Statistics for the Full Sample of Mothers

| | Full Sample |
|---|---|
| Age | 33.51 |
| | (5.012) |
| Tenure | 22.49 |
| | (5.944) |
| Full Time | 0.65 |
| | (0.477) |
| Permanent | 0.93 |
| | (0.257) |
| White Collar | 0.60 |
| | (0.490) |
| Blue Collar | 0.31 |
| | (0.463) |
| Monthly Wage | 1417.99 |
| | (2970.3) |
| Monthly Earnings | 1521.19 |
| | (1280.4) |
| Small Firm ($< 15$) | 0.39 |
| | (0.488) |
| High Female Share Establishment | 0.50 |
| | (0.500) |
| High Quality Establishment | 0.65 |
| | (0.477) |
| Observations | 641607 |

**Notes:** The table reports summary statistics for the full balanced sample of mothers giving birth between 2012 and 2016. All variables are measured before the start of compulsory maternity leave ($t = -5$). Monetary quantities are expressed in 2010 prices. High quality establishment is an indicator equal to 1 if the worker is employed in a firm whose AKM firm effect is above the median.

Table A4: Magnitudes of Increase in Separations

| | |
|---|---|
| **Replacement Rate Reform** | |
| Cumulative Separations | 0.03** |
| | (0.016) |
| Elasticity | 0.4 |
| Observations | 127,294 |
| **Duration Reform** | |
| Cumulative Separations | 0.2*** |
| | (0.07) |
| Elasticity | 0.16 |
| Observations | 91,253 |

**Notes:** The table reports the pooled results of the effects of the reforms on cumulative separations up to 9 months from compulsory maternity leave. It also reports the elasticity of the level of benefits with respect to separations as well as the elasticity of the duration of benefits with respect to separations.