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

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Abstract

I study how mothers value different dimensions of parental leave generosity and what these valuations imply for parental leave design. I exploit a unique feature of the Italian social insurance system that allows mothers to choose between parental leave with job protection and relatively low benefits, and unemployment insurance with higher benefits but no job protection. Using difference-in-differences around two reforms that increased benefit levels and benefit duration, I show that mothers respond strongly to benefit generosity and frequently forgo job protection, leading to large and persistent earnings losses. I use these choices to estimate revealed-preference measures of mothers' willingness to pay for more generous benefits. I find substantial valuations, but show that interpreting them requires distinguishing between welfare-relevant motives, including liquidity needs, childcare constraints, and preferences for time spent with infants, and behavioral frictions such as inattention and present-biased responses to transfer timing. Embedding these estimates in an MVPF framework, I find that the welfare effects of expanding parental leave generosity depend critically on how mothers' choices are interpreted and on the extent to which underlying constraints and behavioral frictions are addressed.

Keywords: parental leave, job protection, labor supply, valuation of welfare transfers

JEL Codes: J13, J18, I38, J22

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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). This fact has spurred increasing interest in how government interventions can mitigate the adverse effects of childbirth on women's labor market outcomes. One of the most direct policy levers available to governments for shaping mothers' labor supply decisions around childbirth is parental leave. These programs are designed to achieve multiple objectives simultaneously: providing income support when parents temporarily leave work, facilitating time spent with newborn children, and maintaining labor-market attachment. Designing parental leave systems therefore requires balancing benefit generosity against the risk that extended absences weaken mothers' long-run labor-market outcomes. Despite the central role of parental leave in family policy, we know surprisingly little about how mothers value different features of these programs or how much welfare they derive from additional benefits.

Designing these programs is, however, difficult. Parental leave programs operate along multiple dimensions (replacement rate, duration and job protection) and it is hard to find policy changes that help isolate the effects of these different parental leave parameters. More fundamentally, we know relatively little about the value that different parental leave features provide to mothers. This is partly a data problem: few settings allow mothers to make explicit and observable choices between programs with different levels of generosity, limiting the scope for revealed-preference welfare analysis.

It is also a conceptual challenge. Unlike traditional social-insurance programs, parental leave benefits provide more than consumption smoothing. They may relax liquidity constraints, facilitate parental time investments, and allow mothers to remain at home with a newborn child during a period when childcare options are limited. At the same time, childbirth is a pivotal moment in the life cycle when decisions may be shaped by liquidity pressures, limited information, or behavioral biases rather than fully informed preferences. As a result, observed willingness to pay for more generous benefits may reflect a combination of welfare-relevant preferences and behavioral distortions. If mothers do not fully internalize the long-run consequences of their choices, interventions calibrated to observed behavioral responses may not deliver the expected welfare gains(Mullainathan, Schwartzstein, and Congdon, 2012; Chetty, 2015). These challenges have

constrained our ability to evaluate parental leave policies and to determine which dimensions of generosity generate the greatest welfare benefits. As a result, the central challenge is not simply measuring whether mothers respond to parental leave generosity, but understanding what those responses reveal about welfare.

This paper studies how mothers value different dimensions of parental leave generosity and what those valuations imply for the design of parental leave policies. To do so, I exploit a unique feature of the Italian social insurance system that allows mothers to make explicit choices between programs that differ in both benefit generosity and job protection. These observed choices provide a rare opportunity to conduct revealed-preference welfare analysis in the context of parental leave. After the end of compulsory maternity leave, mothers in Italy may either remain on parental leave (PL), which provides relatively low benefits and continued job protection, or voluntarily resign and claim unemployment insurance (UI), which provides substantially higher benefits but requires relinquishing job protection. This latter option is unique to the Italian system, which grants special UI eligibility to mothers who voluntarily resign within one year of childbirth, effectively creating a parental leave program without job protection. As a result, mothers face an unusually transparent trade-off between short-run financial support and long-run labor-market attachment.

This setting addresses both the empirical and conceptual challenges discussed above. First, it generates explicit and observable choices between programs with different characteristics, making it possible to infer how mothers value alternative parental leave arrangements. Second, it creates a transparent trade-off between short-run financial support and long-run labor-market attachment. By observing how mothers respond when this trade-off changes, I can recover the value they place on different dimensions of parental leave generosity.

I exploit two policy reforms that increased the generosity of UI benefits and mimic expansions of paid parental leave without job protection. The first reform (replacement-rate reform), introduced in 2013, increased the replacement rate from 60 percent to 75 percent, while the second (duration reform), introduced in 2015, extended potential benefit duration from 8 months to up to 24 months. By shifting benefit levels and benefit duration separately while holding job protection fixed, these reforms allow me to isolate the effects of distinct dimensions of parental leave generosity. Importantly, the reforms differ not only in the magnitude of the benefits they provide but also in their timing: the replacement-rate reform increases transfers immediately after childbirth, whereas the duration reform delivers gains later in the leave spell. This distinction proves useful for interpreting mothers' valuations and the welfare implications of their choices. Using administrative data on the universe of mothers working in the private sector before childbirth pro-

vided by the Italian Social Security Institute (INPS), I compare cohorts of mothers differentially exposed to the unemployment insurance reforms, before and after childbirth, within a difference-in-differences framework.

The first set of results documents how mothers respond to changes in benefit generosity. When benefits become more generous, mothers are more likely to choose programs with more generous benefits and to forgo job protection. Mothers respond strongly to both reforms. More generous benefits increase UI take-up and induce mothers to substitute away from job-protected parental leave. In doing so, mothers voluntarily forgo the option of returning to their previous employer in exchange for higher short-run benefits, delaying their return to work.

The willingness of mothers to give up job protection for more generous benefits comes at a substantial long-run cost. Both reforms lead to large and persistent declines in employment and earnings that extend well beyond the exhaustion of UI benefits, suggesting that many mothers are unwilling or unable to return to work once benefits expire. Decomposing earnings losses reveals that the primary mechanism is a decline in employment rather than changes in wages or hours conditional on work. The magnitudes are striking. Marginal mothers induced to take up UI by the reforms forgo approximately €29,000 in cumulative earnings over the four years following childbirth under the replacement-rate reform and approximately €35,000 under the duration reform. These losses substantially exceed the value of the additional benefits received. Thus, while more generous benefits provide greater financial support in the short run, they also induce choices that weaken long-run labor-market attachment and generate sizable fiscal externalities through lower employment and tax revenues.

Building on these findings, I use a revealed-preference approach (Chetty, 2008; Hendren, 2017; Fadlon and Nielsen, 2019) to estimate the value mothers place on additional parental leave benefits. The central insight is that mothers' willingness to sacrifice future earnings and job protection reveals the value they assign to more generous benefits following childbirth. I estimate substantial marginal benefits under both reforms: 0.42 for the replacement-rate reform and 0.35 for the duration reform. These estimates imply that mothers place considerable value on additional resources during the post-birth period.

Interpreting these valuations, however, is not straightforward. High revealed valuations may reflect welfare-relevant motives, including liquidity needs, childcare constraints, and preferences for time spent with infants. Alternatively, they may partly reflect behavioral frictions, such as limited attention to future labor-market consequences or present-biased responses to the timing of transfers. Distinguishing between these explanations is crucial because they imply very different

welfare conclusions and policy prescriptions. Consistent with an important role for structural constraints, mothers in low-income households and in regions with limited childcare availability respond more strongly to benefit expansions, suggesting that both short-run financial needs and the costs of returning to work shape policy choices after childbirth.

Interpreting the revealed-preference estimates requires assessing whether mothers' choices reflect fully informed preferences. I examine two patterns that are informative about the role of behavioral frictions. First, mothers' choices are largely insensitive to local labor-market conditions despite facing substantially different long-run costs of job loss across regions, a pattern consistent with limited attention to re-employment risk. Second, mothers respond more strongly to transfers delivered immediately after childbirth than to transfers received later in the leave spell, even after accounting for differences in the present value of benefits. This finding suggests an important role for transfer timing, liquidity needs, and potentially present-biased decision-making. Taken together, these results indicate that at least part of the observed willingness to pay may reflect behavioral responses rather than welfare-relevant preferences alone. Importantly, preferences for time spent at home with an infant are not treated as distortions. Parental leave policies are explicitly designed to facilitate parental time investments following childbirth, and to the extent that mothers value spending time with infants, this component represents a welfare benefit of the policy rather than a bias that should be removed from revealed-preference estimates. I therefore try to assess how sensitive the estimates are to environments in which behavioral frictions and structural constraints are plausibly less severe.

To do so, I focus on the duration reform, which is less susceptible to distortions arising from transfer timing, and estimate marginal benefits separately for mothers facing lower unemployment risk and greater childcare availability. The resulting estimates of 0.19–0.21 should be interpreted as conservative revealed valuations. They continue to incorporate the value mothers place on infant care and post-birth consumption smoothing while reducing the influence of inattention, present-biased responses to transfer timing, and binding childcare constraints.

Finally, I embed these estimates in a welfare analysis following Hendren and Sprung-Keyser (2020) to assess the fiscal and welfare implications of the two reforms. The marginal value of public funds (MVPF), defined as the ratio of mothers' willingness to pay per euro of transfer to the net fiscal cost of the policy, is 1.01 for the replacement rate reform and 1.05 for the duration reform under aggregate marginal benefit assumptions. Both reforms therefore hover at the welfare-neutral threshold: mothers' implicit willingness to pay approximately matches the net fiscal cost of providing the transfer. The slightly higher MVPF for the duration reform reflects its smaller baseline

fiscal externality (1 + FE = 1.29 vs 1.41), which more than offsets its smaller marginal benefit. This distinction between the two reforms, separately identifying the welfare costs of benefit-level and benefit-duration expansions, would be lost without exploiting both reforms.

Under more conservative MB assumptions for the duration reform, the MVPF falls to 0.92–0.94, slightly below the welfare-neutral threshold. A full welfare assessment of reforms that change the job-protection features of parental leave must also account for broader social costs, in particular the potential impacts on statistical discrimination against women of childbearing age. Incorporating the statistical discrimination externality documented by Carta et al. (2024) for the duration reform, firms shifting hiring of young women toward fixed-term contracts following higher maternal quit rates, substantially raises the effective fiscal cost of the duration reform, lowering its MVPF to 0.90 under aggregate MB assumptions and to 0.79–0.81 under conservative MB assumptions.Taken together, the results suggest that parental leave transfers generate meaningful value for mothers, but that the welfare gains from expanding benefit generosity depend critically on the extent to which underlying liquidity constraints, childcare barriers, and behavioral frictions are addressed. Realizing larger welfare gains requires complementary policies that address liquidity constraints and childcare scarcity directly.

Related Literature This paper contributes to different strands of literature. It contributes to the rich literature on the effects of parental leave policies on parents' labor market outcomes (Dahl et al., 2016; Olivetti and Petrongolo, 2017; Kleven, Landais, and Søgaard, 2019; Raute, 2019; Kleven et al., 2024; Bailey et al., 2025). While many studies evaluate how the duration of parental leave affects child penalties, the evidence is mixed, often suggesting that leave extensions have a neutral or slightly negative impact on long-run outcomes (Baker and Milligan, 2008; Dahl et al., 2016; Kleven et al., 2024). This paper advances this discussion by focusing on the composition of leave benefits. I show that when mothers are offered a choice between job-protected leave and higher-generosity unemployment insurance, a significant share opts for the latter, leading to permanent job displacement and exacerbated long-run earnings losses. The closest papers in this respect are Lalive et al. (2014) and Schönberg and Ludsteck (2014), who study the relative importance of job protection and benefit duration in Austria and Germany respectively. This paper complements their analysis by focusing more explicitly on benefit levels as a third dimension and by providing a welfare assessment of the trade-offs involved in mothers' decisions, rather than focusing solely on labor market effects. This paper also contributes to the nascent literature on the optimal design of parental leave systems. To my knowledge, only two prior papers have provided welfare

assessments of parental leave: Paradisi (2021) computes an MVPF for Danish parental leave using a calibrated willingness to pay, and Jørgensen and Søgaard (2024) estimate a structural household model of parents' joint willingness to pay for earmarked parental leave using Danish register data. This paper offers a complementary but distinct approach: rather than relying on calibration or structural assumptions, I exploit quasi-experimental reforms and observed program choices to directly identify the welfare value mothers place on benefit generosity relative to job protection. To my knowledge, this is the first revealed-preference estimate of mothers' willingness to pay for parental leave generosity. This approach also allows me to separately quantify the welfare-relevant component of mothers' valuations from the component driven by behavioral frictions, a distinction that is central to evaluating whether expanding parental leave benefits actually improves welfare.

The paper also contributes to the welfare analysis of social insurance programs (Baily, 1978; Chetty, 2006; Chetty and Finkelstein, 2013; Finkelstein and Hendren, 2020). A growing literature uses revealed-preference methods to estimate the welfare value of social insurance transfers in settings ranging from unemployment insurance (Landais and Spinnewijn, 2021) to survivor benefits (Coyne et al., 2024; Giupponi, 2024) and disability insurance (Haller and Staubli, 2023). This paper extends this approach to the parental leave setting, a context in which choices are made at a uniquely salient and pivotal moment in the life cycle, when liquidity needs, time constraints, and uncertainty about future labor market prospects are particularly acute. By focusing on the moment immediately after childbirth, the paper contributes to understanding how individuals make costly labor supply and program choices at pivotal life events, complementing recent work that studies maternal decision-making under limited attention and information constraints (Costa-Ramón et al., 2024).

A key contribution is to use the structure of two reforms, one that increases the value of benefits received immediately during the leave spell and one that extends benefits further into the future, to exploit variation in the timing of transfers and provide novel evidence on how present bias and liquidity constraints shape mothers' program choices after childbirth. In this respect the paper connects to a growing literature on behavioral frictions in social insurance (Ganong and Noel, 2019; Gerard and Naritomi, 2021) and on the implications of present bias, inattention and misinformation for public policy (Mullainathan, Schwartzstein, and Congdon, 2012; Chetty, 2015; Costa-Ramón et al., 2024) providing direct evidence that behavioral frictions may inflate revealed valuations in the parental leave context and quantifying the wedge between observed willingness to pay and welfare-relevant preferences.

The paper is also related to Carta et al. (2024), who study how firms respond to the duration

reform analyzed in this paper. Carta et al. (2024) document that firms exposed to the duration reform shifted hiring of young women toward fixed-term contracts in response to higher maternal quit rates, a statistical discrimination response to the supply-side behavioral change. This paper directly integrates their firm-side findings into the welfare analysis, using their estimated hiring response to quantify the discrimination externality and incorporate it into the MVPF calculation for the duration reform. While Carta et al. (2024) focus on firms' demand-side response, this paper analyzes mothers' supply-side decision-making and the welfare implications of their choices.

Taken together, the analysis contributes to these literatures along three dimensions: it provides the first revealed-preference estimate of mothers' willingness to pay for parental leave generosity, separately identifies the welfare costs of benefit-level and benefit-duration expansions, and quantifies the role of behavioral frictions in inflating revealed valuations.

The paper is structured as follows. Section 2 describes the institutional background. Section 3 describes the data and presents the empirical strategy. Results are presented in Section 4. Section 5 focuses on the welfare implications of the empirical findings. Section 6 discusses the mechanisms and drivers behind mothers' choices. Section 7 calculates the Marginal Value of Public Funds of the two policies. Section 8 concludes.

2 Institutional Background

2.1 Social Insurance for New Mothers in Italy

Social insurance for new working mothers in Italy comprises three main programs: compulsory maternity leave (ML), parental leave (PL), and unemployment insurance (UI).

By law, every mother must take a period of *compulsory maternity leave* (ML), lasting five months, with benefits equal to 80%[1] of the average daily wage in the last month before the start of the leave. This leave, which is job-protected, typically begins two months before the expected due date and ends three months after childbirth[2].

After the end of the compulsory ML, mothers, as illustrated in Figure 1, have three options: they can go back to work, they can take up *parental leave* (PL), or they can take up *unemployment insurance* (UI).

The *parental leave* program offers families a total of 10 months of leave, which can be shared

[1]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.

[2]The start of ML can be anticipated if the occupation is deemed risky for the pregnancy or if the pregnancy is at risk. In these cases, the compulsory ML lasts more than five months.

between parents. Each parent can claim up to six months of leave. If the father uses at least three months of PL, the household earns an additional month of leave, extending the total to 11 months. While PL is job-protected for its entire duration, monetary benefits are available only for six months out of the total 10 months. The monetary benefits are equal to 30% of the average daily wage before childbirth. Any additional leave beyond six months is unpaid. Despite the stated aim of the policy of encouraging fathers' take-up of PL, mothers account for around 80%[3] of PL beneficiaries and usually exhaust their paid leave within six months of completing compulsory ML.

The Italian system also provides special *unemployment insurance* (UI) eligibility to mothers who decide to voluntarily resign between childbirth and the child's first birthday. This is a key exception to the standard UI system as, under ordinary circumstances, workers who resign voluntarily are not entitled to UI benefits. In this context, UI offers higher income replacement rates compared to PL and similar or longer duration, making it a competitive option. Effectively, this provision transforms UI into an alternative to the standard PL program, offering income replacement after childbirth without job protection. 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). Note that only mothers, not fathers, can benefit from this exception[4]. While the policy aims to safeguard mothers during a period of vulnerability, it also creates incentives for mothers to leave the workforce, potentially distorting the return-to-work decision.

The Italian system therefore provides a unique environment in which, after childbirth, mothers can *choose* between programs of different generosity and characteristics. This choice takes place in the *choice period*, as represented in Figure 1, specifically between three and twelve months after childbirth.

The UI Reforms of 2013 and 2015 In recent years, two major reforms significantly reshaped Italy's unemployment insurance (UI) system[5]. Although neither reform explicitly focused on mothers, the resulting changes effectively serve as policy experiments that mimic expansions of paid parental leave without job protection.

The first reform, which I will refer to as the *replacement rate reform* (RR), took effect on January

[3]Source: *INPS*.

[4]In 2024, eligibility was extended to fathers; however, this extension falls outside the period examined in this analysis.

[5]Both reforms were part of broader structural labor market reforms, the 2013 reform under the Fornero package during the Monti government, and the 2015 reform as part of the Jobs Act under the Renzi government, rather than temporary cyclical measures. The robustness of the empirical findings to these different policy and economic contexts is discussed in Section 4.

1, 2013. This reform increased the UI replacement rate from 60% to 75%, while keeping eligibility requirements unchanged. For workers under 50, including new mothers, the maximum benefit duration remained fixed at eight months[6]. Figure A2 illustrates the impact of the reform on benefit level (Panel (a)) and benefit duration (Panel (b)) for women under 50 with a permanent contract around the introduction of the reform. The figure shows how the reform raised benefit levels due to the higher replacement rate while leaving benefit duration unchanged.

The second reform, which I will refer to as the *duration reform*, took effect on May 1, 2015. This reform modified the potential benefit duration while keeping the replacement rate basically unchanged. Under the new rules, benefit duration became directly tied to an individual's contribution history over the four years preceding their unemployment spell. Specifically, the potential duration was set at half the number of weeks worked within that period, with a maximum cap of two years.

For workers with stable and extended contribution records, this reform led to a significant increase in potential benefit duration. Panel (c) of Figure A2 confirms that the benefit level for women under 50 with permanent contracts remained unchanged, while Panel (d) highlights a substantial increase in benefit duration. On average, the reform extended the potential UI duration for this group by approximately 250 days, doubling it to around 500 days. Figure A3 further illustrates the distribution of potential benefit duration of UI benefits for mothers after the reform. On average, the new potential benefit duration was 15 months, with a median of 16 months, an almost 100% increase from previous levels. Unlike the replacement rate reform, which increased monetary benefits, the duration reform extended the length of benefits.

An important feature of the institutional setting is that the duration reform occurs after the replacement-rate reform, implying that the two dimensions of generosity are not varied independently over time. As a result, the comparison between the two reforms should not be interpreted as a fully structural decomposition of the value of benefit levels versus duration. In particular, the valuation of additional duration is estimated conditional on the higher contemporaneous replacement rates introduced in 2013. If mothers' demand for additional insurance through longer duration is lower when current benefits are already relatively generous, this sequencing would tend to attenuate estimated responses to the duration reform relative to a setting with lower replacement rates.

[6]For workers aged 50 and older, the maximum benefit duration was also extended

UI as Alternative to PL There might be obvious concerns with interpreting unemployment insurance as an actual alternative to parental leave programs. If mothers perceived stigma from being on UI or were unaware that it could be claimed after childbirth, take-up would be attenuated relative to a hypothetical paid-leave extension of equivalent generosity. There are several reasons why this concern is likely limited in Italy. Post-maternity use of UI was widely understood and actively disseminated as a de facto extension of paid leave, through both commercial services and informal channels. Figure A1 collects representative evidence. Panel (a) shows evidence of a commercial service explicitly marketing UI to as PL. Panel (b) illustrates peer-to-peer dissemination from a popular online forum, with mothers describing as having used UI as PL and a bridge to preschool entry.

A related concern is whether job-search requirements attached to UI could push mothers back into employment earlier than a formal paid-leave benefit would, attenuating our estimates. This is not particularly relevant in the Italian setting. The only thing recipients are formally required to do is to sign a formal availability-for-work declaration and a job-search agreement signed with the local employment center. However, the link between unemployment benefits and job-search requirements in Italy has been weak. Although conditionality rules formally exist, they are largely not enforced in practice. Despite reform efforts, enforcement has remained uneven and limited, due to the lack of integrated IT systems linking employment services and benefit administration, the delayed definition of "suitable job offers", a key condition for enforcing conditionality, and weak enforcement capacity alongside the low number of vacancies mediated by public employment services (Xenogiani et al., 2019). While this institutional feature is specific to Italy, it provides a rare and valuable setting in which the trade-off between benefit generosity and job protection is made explicit and observable, enabling a revealed-preference welfare analysis that would not be possible in most other countries.

Taken together, these features make it a reasonable approximation to interpret unemployment insurance as a form of paid leave without job protection in this context.

2.2 The Availability of Childcare

In Italy, formal education is mandatory starting at the age of six, coinciding with the beginning of primary school. Prior to this, formal childcare is available through two main types of institutions: nurseries, which cater to children 0-2 year-old, and kindergartens, which serve children 3-5 year-old. Nurseries can be public or private and in both cases are managed at the municipal level, operating under regional regulations and overseen by the Ministry of Welfare (Carta and Rizzica, 2018).

While kindergartens are virtually free of charge for families and largely available (and therefore often considered as the first step of formal education in Italy), nurseries, either private or public, are expensive and their availability is limited. Public and private nurseries accommodate only 22.5% (Istituto Nazionale di Statistica (ISTAT), 2016) of 0–2 year-old children, with less than half of this share covered by public facilities. In terms of cost, the cost of public nurseries is shared between the municipality and users, and priority for slots is given based on family income. A family with three people and two working parents with median household income spend, on average, €300 per month. Private nurseries, on the other hand, cost on average €487 per month. There is large regional heterogeneity both in terms of provision (of both public and private nurseries) and in terms of cost.

3 Data and Empirical Strategy

3.1 Data

I use confidential administrative data from the Italian Social Security Institute (INPS) on the universe of private-sector employees. My primary data source consists of matched employer-employee records at the monthly level for the period 2009-2019. These records provide rich, detailed information for each worker-firm pairing, including: beginning and end date of the contract, reason 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); monthly earnings, days worked, and a unique firm and worker identifier. I further 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. The data also capture monthly events triggering benefit entitlements, such as maternity leave, parental leave, and sick leave. I identify maternity and parental leave events by supplementing the employer-employee records with information from maternity and parental leave applications registries. 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 and 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 a spouse identifier. This level of detail allows me to pinpoint the exact leave periods and the exact beginning and end of job protection.

I link these records to information on unemployment insurance claims from the *Sistema Percettori* database (*SIP*), which collects information on unemployment insurance spells. I observe the

unemployment insurance regime (e.g., before or after the *duration reform*), its start date, potential duration, actual duration, and total amount paid for every claim.

I construct a panel of mothers' working and benefit histories at a monthly frequency by integrating the administrative datasets described above. The final dataset is a balanced panel containing all mothers who gave birth to their first child 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. Focusing on first-time mothers provides several advantages. First, the labor 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.

Table 1 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, they are mainly employed with a permanent contract (94%), 63% employed are full-time, and 61% are employed in white-collar occupations. The average gross monthly earnings of these mothers are 1,536 EUR. Approximately 40% of them are employed in small firms with less than 15 employees.

Linking Households While the main focus of the analysis will be mothers, the parental leave problem is essentially a household-level one. It is therefore key to understand how the behavior of the household changes in response to these reform. In order to do this, parents need to be linked in the data. Unfortunately, at the moment, such linking is possible for a subset of households by using information from parental leave applications. Parents can be linked in one of two cases: if part of the parental leave is claimed by the father or if the application for parental leave submitted to the Social Security Institute includes information about both parents[7]. Because of this, the sample of mothers who can be linked to fathers is likely subject to positive selection. Table A1 compares

[7]Providing information about both parents is not mandatory. It is enough to provide information about the parent who takes up the leave.

mothers' characteristics before childbirth in the matched and unmatched sample. Mothers in the matched sample have higher earnings, are more likely to have a full time job and are more likely to be white collar.

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. 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 end 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.

My primary empirical strategy is a difference-in-differences approach similar to Kleven et al. (2024), that compares cohorts of mothers that were *fully eligible* for the more generous unemployment insurance regime, i.e. eligible for the more generous UI benefits for the entirety of their "choice period" (Month 3 and Month 12 in Figure 1), to cohorts of mothers that were *fully ineligible* for the more generous UI regime, i.e. not eligible for the more generous UI benefits at any point of their choice period, based on their end of compulsory ML date. The treatment and control groups correspond, respectively, to the fully treated and not treated cohorts in Figure A4. The reforms also create a group of *partially treated* mothers (*partially treated* in Figure A4): since mothers can take up unemployment insurance at any point within the choice period, some cohorts of mothers were only partially eligible for the more generous UI regime. These cohorts of mothers are excluded from the baseline empirical analysis. I estimate the following equation:

$$Y_{it} = \alpha + \sum_{k=-L}^{R} \delta_k D_{it}^k + \sum_{k=-L}^{R} \beta_k D_{it}^k \cdot T_i + \gamma T_i + \lambda_i^m + \mathbf{X_i}'\eta + \varepsilon_{it} \tag{1}$$

where t denotes event time (where $t = 0$ corresponds to the month of end of compulsory maternity leave), D_{it}^k is an event study indicator for each month relative to the month before the end of compulsory maternity leave and 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 (Figure A4)[8]. λ_i^m are month of end of maternity leave fixed effects to control for the seasonality of births and to ensure any seasonal difference in choices of programs, labor supply costs, or labor market conditions driven, for example, by holidays or childcare availability, is accounted for.

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

$\mathbf{X_i'}$ 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. 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 β_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.[9]

The causal interpretation of the results relies on the assumption that there is no confounding trend in outcomes by birth cohort. It is possible to evaluate whether the parallel trend assumption holds in the years leading up to the end of maternity leave by looking at the coefficients β_k for $k < 0$. When using UI take-up as outcome, pre-trends cannot be observed as mothers are, by definition, employed prior to giving birth. Therefore, I leverage a different set of over-identifying restrictions and show placebo results from specification 1 using a sample of mothers who gave birth in 2009 vs. 2010 and 2010 vs. 2011.

4 Results

4.1 Responses to Changes in Unemployment Insurance Benefits

This section presents the main results on how mothers respond to increased UI generosity along two dimensions: replacement rates and benefit duration. I estimate equation 1 using three primary outcomes: (i) the probability of taking up UI, (ii) the probability of taking up parental leave (PL), and (iii) the probability of employment.

4.1.1 Replacement Rate Reform

Figure 3 Panel (a) reports the estimates of the effects of the replacement rate reform on mothers' take-up of social programs and work behavior after childbirth. The green triangle series reports the coefficients β_k from specification 1 where the dependent variable is the share of mothers on UI. An increase in the replacement rate of UI increases the share of mothers on UI in the treatment group relative to the control group after the end of compulsory maternity leave (ML). The effect

[9] While using the preceding year of a reform as the control group is common in the literature (Schönberg and Ludsteck 2014; Kleven et al. 2024), 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.

emerges right after mandatory maternity leave ends and increases further around month 6, coinciding with the typical exhaustion of parental leave benefits for mothers who take PL immediately after maternity leave.

The increase in UI take-up reflects substitution from two margins: parental leave and employment. The pink diamond series shows the effect on PL take-up. During the first six months after maternity leave, the increase in UI take-up is mirrored by a corresponding decrease in PL take-up, with no significant change in employment. This suggests that in the immediate post-birth period, the reform primarily redistributes mothers between social programs: mothers induced to take UI by higher replacement rates would likely have taken PL in the absence of the reform.

The blue square series in Figure 3 shows the effect on employment. No significant differences in employment emerge during the first six months. However, employment begins to decline for treatment mothers relative to controls after month 6, and this decline persists well beyond the 8-month UI eligibility window. While part of the employment delay is mechanical, UI benefits last 8 months compared to 6 months for PL, the persistence of negative employment effects after UI exhaustion suggests that mothers who switch from PL to UI face difficulty returning to work or choose to remain out of the labor force longer.

Tables 2 reports baseline take-up and employment rates for the control cohorts at three horizons (immediately after the end of maternity leave, at month 5, and at month 9), allowing the magnitude of the estimated treatment effects to be assessed against the pre-reform program-choice landscape.

Table 2 helps understand the magnitudes of the effects by reporting the coefficients β_k from model 1 on the share of mothers on UI, the share of mothers on PL, and the share of mothers working, at three key time points: right after the end of ML ($k = 0$), five months later ($k = 5$) and nine months later ($k = 9$), at the end of the *choice period* (see Figure 1). The table also reports baseline take-up and employment rates for the control cohorts over months 0-9, allowing the magnitude of the estimated treatment effects to be assessed against the pre-reform program-choice landscape. Treatment mothers are more likely to take up UI by 1.06 p.p five months after the end of ML, with a corresponding 1.16 percentage point decrease in PL take-up. By month 9, UI take-up increases by 1.72 percentage points, representing an 11.6% increase relative to the control group mean of 14.8%. Parental leave take-up shows an initial decline of 1.16 percentage points at month 5 but converges back to the control group level by month 9. Employment falls by 0.86 percentage points by month 9, a 1.7% decrease from the baseline of 0.502.

4.1.2 Duration Reform

Figure 3 Panel (b) reports the results for the duration reform, which extended maximum UI duration from 8 months to 15 months on average. Mothers seem to respond to the increase in the duration of benefits in a similar way as to an increase in the level of benefits. Mothers respond to extended duration in qualitatively similar ways as to increased replacement rates. Treatment mothers are more likely to take up UI immediately after maternity leave, with a corresponding decrease in PL take-up, though the substitution between programs is less pronounced than for the replacement rate reform. The reform also delays return to work, with employment effects that persist and grow over time, consistent with longer UI benefit spells inducing more extended labor force exit.

Table 3 reports the coefficients β_k from model 1 on the share of mothers on UI, the share of mothers on PL, and the share of mothers working. UI take-up increases by 1.85 percentage points by month 9, representing a 9.9% increase relative to the control group mean of 18.7%. Parental leave take-up shows a small initial decline of 0.72 percentage points at month 5 but largely converges by month 9. Employment falls by 1.46 percentage points by month 9, a 2.9% decrease from the baseline of 50.3%.

Overall, the results indicate that when UI benefits become more generous, either in terms of *level* of benefits or in terms of *duration* of benefits, mothers choose to maximize short-term benefits, regardless of job protection. They do so by substituting the less generous PL program for the more generous UI program, which has immediate effects on their probability of returning to work.

Comparing the magnitude of effects across reforms, both induce similar increases in UI take-up in absolute terms (1.72pp vs 1.85pp), though these represent different percentage increases relative to baseline (11.6% vs 9.9%) due to higher baseline UI take-up in the duration reform cohorts. The employment effects are larger for the duration reform (1.46p.p. vs 0.86p.p.), consistent with longer benefit spells inducing more persistent labor force exit.

4.1.3 Robustness

As mentioned in Section 3.2, the key identification assumption for the difference-in-differences model in 1 is the absence of confounding trends in outcomes by birth cohort. Since the reforms define treatment and control groups based on adjacent birth cohorts, it is not possible to include calendar year fixed effects (a feature shared with other studies using similar designs such as Schönberg and Ludsteck (2014); Kleven et al. (2024)). Several features of the setting nevertheless mitigate

this concern.

First, both reforms were introduced as part of broader structural labor market reforms with relatively short notice, limiting scope for strategic fertility timing or anticipatory behavior. Examination of monthly birth frequencies shows no evidence of discontinuities or unusual bunching at the reform cutoff dates. Second, the empirical design compares mothers whose end of maternity leave falls within a narrow window around each reform cutoff, limiting the scope for broader macroeconomic trends to explain the results. Third, and most importantly, the two reforms occurred under markedly different macroeconomic conditions. The 2013 replacement rate reform was implemented during a severe recession, when Italian GDP contracted by approximately 1.8% and unemployment exceeded 12%. By contrast, the 2015 duration reform took place after GDP had returned to positive growth and unemployment had begun to stabilize. If the estimated effects were driven primarily by coincident economic shocks rather than by the policy changes themselves, we would expect the reforms to generate qualitatively different behavioral responses reflecting the different economic environments. Instead, both reforms produce similar increases in UI take-up (1.7–1.9 percentage points), suggesting that the policy changes rather than macroeconomic conditions drive the results. Finally, the pre-determined controls included in the baseline specification, in particular, average earnings in the two years before the end of maternity leave, directly absorb level differences between cohorts, further reducing the scope for confounding trends to bias the estimates. Results are also robust to directly controlling for regional unemployment rates at the time of labor market re-entry.

When using UI take-up as the outcome, pre-trends cannot be observed because mothers are, by definition, not on UI before birth. I therefore leverage a different set of overidentifying restrictions and conduct placebo tests by estimating 1 comparing mothers who ended their maternity leave in 2010 with those who completed their leave in 2009, as well as mothers who ended their maternity leave in 2011 with those who ended their leave in 2010. The results, reported in Figure A5, support the identification assumption by showing no significant trends in UI take-up across these placebo birth cohorts. For earnings and employment outcomes, the event study estimates in Figure 4 show no differential pre-trends between treatment and control cohorts in the months leading up to the end of maternity leave, further supporting the parallel trends assumption.

Additionally, the main results are robust to using broader and narrower time windows for the definitions of treatment and control groups, as shown in Figure A7. Results are also robust to adjusting for changes in macroeconomic conditions at the time of re-entry into the labor market by controlling for the local unemployment rate in the region of pre-birth employment before

childbirth.

Another potential concern is that the increase in UI take-up is not driven by active choices of mothers but rather by firings by firms.Figure A6 addresses this concern directly by estimating equation 1 using as dependent variable the probability of involuntary separations (layoffs, firm closures, and separations for just cause) which are not impacted by workers' decisions. The results show no significant increase in involuntary separations for treatment mothers relative to controls, providing strong evidence that the increase in UI take-up reflects active program choices by mothers rather than firm-driven separations. This interpretation is further supported by the Italian institutional setting: mothers who voluntarily resign around childbirth must follow a specific process involving an interview with the governmental office of labor, where they declare their voluntary resignation, followed by direct checks with employers.

A further concern is that mothers on UI may have additional children while receiving benefits, mechanically extending observed UI spells and contributing to the persistence of employment effects. Under Italian rules, UI is suspended during maternity leave for subsequent births and automatically resumes afterward, which could theoretically make closely spaced births more attractive under more generous UI regimes. To assess whether the reforms induce differential fertility responses, I examine subsequent birth rates in the sample of mothers who return to formal employment, where births are observable in administrative data. Point estimates are small and statistically insignificant for both reforms, providing no evidence of differential fertility responses. I note, however, that this analysis is limited to mothers who return to work and cannot speak to fertility behavior among mothers who remain out of the labor force, and therefore cannot rule out that differential fertility responses contribute to the observed persistence in program take-up and employment effects.

4.2 Effects on Labor Market Outcomes

The findings presented so far suggest that when the generosity of benefits increases, in terms of level or duration, many mothers choose to forgo job protection for higher benefits in the short run. What are the implications of making this choice in terms of labor market outcomes in the longer run? This is a key question to understand what type of trade-off mothers face when making decisions after childbirth, the effect of different parental leave policies on mothers' career trajectories, and the fiscal cost of the reforms for the government in terms of tax revenue.

I estimate specification 1 using monthly gross labor earnings as dependent variable, specified in levels in order to keep the zeros from non-participation. Figure 4 plots the coefficients β_k nor-

malized by the predicted earnings in the treatment group in the absence of children: $\frac{\beta_k}{E\left[\tilde{Y}_{ik}|k,T=1\right]}$, 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. 2024).

4.2.1 Replacement Rate Reform

Figure 4 Panel (a) reports the estimates of the effects of the replacement rate reform on earnings. The solid red line indicates the time of exhaustion of parental leave benefits, while "End of UI" indicates the time window of exhaustion of UI benefits if mothers resign during the choice period (between $t = 0$ and $t = 9$). The figure shows that the earnings of the treated cohorts are not significantly different from those of the control cohorts before the end of ML and up to six months after the end of ML. This is consistent with mothers in the treatment group moving from PL to UI, creating a *reshuffling* between social programs in the first six months from the end of compulsory ML but no significant differences in return to work patterns. After the first six months from the end of ML, the earnings of the treated cohorts significantly decrease relative to the control cohorts. The negative effect is persistent even after the exhaustion of UI benefits, though the earnings gap narrows somewhat as some mothers return to work. Despite this partial recovery, the gap remains, with a persistent negative effect observed up to four years after the end of compulsory maternity leave. Four years after the end of maternity leave, treated mothers' earnings are 2% lower than controls' mothers earnings relative to before the end of maternity leave.

What drives the negative and persistent effect on earnings? Figure 4 Panel (c) reports the results of the estimation of specification 1 using as dependent variable the probability of working.[10] The pattern of the impact of the reforms on participation closely mimics the results on earnings, suggesting that a decrease in labor force participation primarily drives the decrease in earnings.

Table 4 decomposes the effects on earnings into the extensive margin response, intensive margin response and wage rate. Since the administrative data do not provide information on hours worked, I use days worked conditional on employment to measure the intensive margin of employment. The wage rate is defined as earnings per day worked conditional on employment. The table reports the estimates of the β_k coefficients from equation 1 at $k = 12$, $k = 24$, and $k = 48$. The table shows no effect on days worked one year and two years after the end of ML. Four years after the end of ML, there is a small negative effect on days worked in the treatment group relative to the control group. The wage effects reported in column (4) of Tables 4 are estimated conditional

[10] 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.

on employment and therefore do not mechanically reflect the increase in non-employment among treated mothers. A negative effect is persent in the short term but fades to statistical insignificance by t=48 for both reforms. The dominant and durable channel through which the reform affect long-run earnings remains the extensive margin: the persistent reduction in the probability of employment rather than a lasting depression of wages conditional on working

4.2.2 Duration Reform

Figure 4 Panel (b) reports the result for the duration reform. The pattern broadly mirrors that of the replacement rate reform. Earnings of treated and control mothers evolve in parallel during the first six months after the end of ML, before diverging sharply as mothers in the treatment group delay their return to work. The negative effect persists well beyond the exhaustion of UI benefits. However, four years after the end of maternity leave, the gap almost closes. As for the replacement rate reform, the effect on earnings is closely mimicked by a decrease in participation. Table 5 reports the results on the intensive margin and the wage rate. The duration reform does not seem to have any effect on the intensive margin, as measured by the days worked. In terms of the wage rate, while there is a negative effect $t = 24$, there are no significant differences four years after the end of ML.

Taken together, the results paint a consistent picture across both reforms. While taking up UI provides mothers with greater financial security in the short run, it comes at the cost of significant and persistent earnings losses driven almost entirely by reduced labor force participation. The bulk of these losses occur after the exhaustion of UI benefits, pointing to the loss of job protection, rather than the mechanical effect of being on leave longer, as the primary mechanism.

4.2.3 Who Responds to Each Reform? Characterizing Compliers

The results above show that marginal women are willing to give up the certainty of going back to their pre-birth employer for more generous benefits in the short term. In this section, I conduct a simple complier analysis. The goal is to trace some persistent observable attributes that characterize mothers who respond to the two reforms and distinguish them from mothers who do not. 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 by comparing the demographic characteristics of the cohort of mothers ineligible for the more generous UI regime (control group) to the characteristics of the cohort of mothers

eligible for the more generous UI regime (treatment group). The comparison is based on the intuition that 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), the demographic characteristics of always-takers should be the same in expectation before and after the policy change. This allows me to back out the demographic characteristics of compliers, as described in detail in Appendix Section B.

Results of the complier analysis are reported in Table 6. Compliers represent 1.7% of the population for the replacement rate reform and 1.9% for the duration reform.

Characteristics of Compliers Across both reforms, compliers occupy an intermediate position between always-takers and never-takers along most observable dimensions. They have higher pre-birth monthly wages than always-takers but lower wages than never-takers. They are younger than never-takers (approximately 34 years old vs 36 years) and similar in age to always-takers. Compliers are more likely to work in blue-collar occupations than never-takers but have similar or slightly higher rates than always-takers. This pattern is consistent with compliers representing mothers who are marginally constrained: not so constrained that they would take UI regardless of generosity (like always-takers), but sufficiently constrained that modest increases in benefit levels or duration shift their program choices.

Compliers Across Reforms A natural concern when comparing behavioral responses and welfare valuations across the two reforms is that they may induce different types of mothers to take up UI, confounding differences in preferences or constraints with compositional differences. Table 6 speaks to this concern. The complier populations are overall similar across the two reforms. Compliers induced by the replacement rate reform and those induced by the duration reform are nearly identical in age (33.87 years for both), have similar shares of blue-collar workers (53.0% vs 49.5%), and comparable pre-birth monthly wages. The most notable difference is in full-time employment: compliers for the duration reform are more likely to work full-time before birth. Together, these patterns suggest that the replacement rate reform may have been relatively more salient for mothers with lower pre-birth earnings and weaker labor market attachment, whereas the duration reform appears to affect mothers with stronger pre-birth attachment to the labor market. Section 5 develops a revealed-preference framework that maps these behavioral responses into estimates of mothers' willingness to pay.

4.3 Magnitude of Earnings Losses

The decision to take up UI rather than parental leave carries substantial long-run costs through foregone earnings. The replacement rate reform reduces average earnings by €494, while the duration reform reduces earnings by €662. Scaling these aggregate effects by the share of compliers yields per-individual losses of approximately €29,000 for the replacement rate reform and €35,000 for the duration reform, or roughly €7,000–9,000 per year. The magnitude of these losses validates that mothers' program choices reveal meaningful trade-offs: mothers are willing to incur substantial long-run earnings costs to access more generous short-run transfers.

These per-complier estimates assume that the reduced-form effects are driven primarily by compliers' extensive-margin switching from PL to UI. Always-takers (mothers who would take up UI under either regime) can in principle also contribute to the aggregate effect through intensive-margin responses, most notably by remaining on UI longer when benefits are more generous. In this setting, however, this contribution is quantitatively small under both reforms. For the replacement rate reform, the 8-month duration cap is binding in both regimes, mechanically limiting always-takers' scope to extend their UI spells. For the duration reform, the average always-taker extends her UI spell by at most approximately one months relative to the pre-reform baseline[11]. The per-complier earnings losses reported above should therefore be interpreted as primarily reflecting compliers' permanent labor force exit rather than a mixture of compliers' and always-takers' responses. Section 5 returns to this distinction in the welfare analysis.

5 Welfare Implications

5.1 A Revealed-Preference Approach for Estimating the Value of Parental Leave Transfers

Building on the empirical evidence in Section 4, I now use a revealed-preference approach to infer the within-state value of more generous benefits along the lines of (Chetty, 2008; Hendren, 2017; Fadlon and Nielsen, 2019). The key idea is to use observed behavioral responses to changes in benefit generosity to recover mothers' revealed valuation of additional transfers in the post-childbirth period. Because program choices jointly determine income, labor market attachment, and time

[11] Specifically, the average potential benefit duration after the duration reform is 9 months (compared to the 8 months pre-reform). Given that always-takers represent approximately 19% of the eligible cohort under the duration reform, a one-month extension among always-takers implies an aggregate contribution to the reduced-form effect on labor force participation of less than one percentage point over a four-year window.

spent at home with the child, this valuation should be interpreted as capturing both the liquidity value of transfers and the value mothers place on the non-pecuniary aspects of remaining out of work after childbirth.

I focus on mothers' willingness to pay for more generous benefits within the motherhood state, that is, conditional on having a child and holding the state of nature fixed. Exploiting variation in benefit generosity across regimes, I compare mothers' choices under low- and high-benefit regimes within the same state. Operationally, the marginal benefit (MB) is identified from mothers' willingness to incur the long-run earnings costs associated with losing job protection in order to access more generous benefits. Intuitively, the extent to which mothers are willing to undertake costly actions, such as forgoing job protection and bearing persistent earnings losses, in response to more generous benefits reveals the revealed value they place on additional short-run insurance after childbirth[12].

To do so, I develop a very simple model designed to capture the key trade-offs and intuitions underlying mothers' program choices[13].

Let $V = u(c) - 1\{p = 1\}\phi(\theta)$ represent the mother's utility, where $p = 1$ if a mother chooses to take up UI after birth and give up her job (forgoing job protection) and $p = 0$ if a mother chooses to take up PL and remain employed. Let $\phi(\theta)$ be the net cost of giving up the job and losing job protection, which depends on the mother's type $\theta \sim F(\theta)$. Note that $\phi(\theta)$ incorporates multiple components: the cost of searching for a new job, the loss of firm-specific human capital, and non-monetary aspects of the return-to-work decision, including childcare considerations and preferences for time spent at home with the child. As such, the framework captures both pecuniary and non-pecuniary determinants of mothers' choices in a reduced-form way. Note that $\phi(\theta)$ reflects the mother's private cost of losing job protection but does not include the cost to firms of providing job protection (e.g., holding the position open), nor potential externalities through statistical discrimination. These social costs are incorporated in the welfare analysis in Section 7.

The budget constraint is $c = y(p) + B + 1\{p = 1\}b$ where $y(p)$ is the mother's income, which depends on her choice. B are the baseline benefits when $p = 0$ (she takes PL). b are the additional UI benefit (relative to the baseline) when $p = 1$ (she takes UI).

Note that crucially income depends on the mother's choice. When $p = 0$, with PL and job

[12]Importantly, MB should be interpreted as a reduced-form revealed valuation that incorporates multiple factors shaping mothers' post-childbirth decisions, including liquidity needs, childcare constraints, preferences for time spent at home with an infant, and potentially behavioral distortions such as present bias or inattention to re-employment risk. Section 6.

[13]The model is kept simple but can be extended and generalized to multiple periods, which may better fit the analysis of the duration reform. However, the main mechanisms and ideas behind the results are unchanged.

protection, $y(0)$ corresponds to earnings from returning to the pre-birth job. With UI and no job protection, $y(1)$ reflects earnings from finding a new job or exiting the labor market[14].

Let $V(p)$ denote the indirect utility function. A mother will decide to take up UI and give up her job after childbirth if:

$$V(1) - V(0) \geq \phi(\theta)$$

So a mother decides to take up UI and give up her job when the value of additional consumption from UI benefits outweighs the earnings losses from losing job protection. This is equivalent to a threshold rule where a mother decides to leave her job if and only if $\phi(\theta) \leq \bar{\phi}$.

Taking the derivative of the decision rule with respect to b we have that $V^{'}(1) = u^{'}(c(1))$ where $c(1) = y(1) + B + b$ is the consumption when choosing UI. This allows us to write the marginal benefit as $MB = \frac{u^{'}(c(1)) - u^{'}(c(0))}{u^{'}(c(0))} = \frac{u^{'}(1) - u^{'}(0)}{u^{'}(0)}$, where I use the shorthand notation $u^{'}(c(1)) = u^{'}(1)$ for $p \in \{0, 1\}$. This measures how much mothers value an additional euro of UI benefits relative to the baseline state of taking PL. The numerator u captures the change in marginal utility of consumption when switching from PL to UI. The denominator normalizes this change by the marginal utility in the PL state, yielding a measure of willingness to pay in euro terms.

Calling $\Phi = F(\bar{\phi})$ the participation rate, Appendix A shows that the marginal benefit (MB) can be written as:

$$MB = \kappa \frac{\Phi^{high}}{\Phi^{low}} - 1 \tag{2}$$

where $\kappa = \frac{\frac{|\varepsilon(\Phi^{low}, b^{low})|}{f(\bar{\phi}^{low})}}{\frac{|\varepsilon(\Phi^{high}, b^{high})|}{f(\bar{\phi}^{high})}}$ captures the relative elasticities across benefit regimes, with $\varepsilon(\Phi^s, b^s) = \frac{b^s}{\Phi^s} \frac{\delta \bar{\phi}^s}{\delta b}$ for $s \in \{low, high\}$[15].

Economic interpretation This expression shows that the value of benefits is proportional to the labor supply response to increased benefit generosity after childbirth. Larger labor supply responses indicate higher valuation of additional benefits in the motherhood state. Intuitively, the

[14] In principle, one could model the earnings difference $y(0) - y(1)$ as arising endogenously from a job-search model in which mothers who forgo job protection face search frictions, lose firm-specific human capital, or experience scarring. I do not model these microfoundations explicitly. Instead, I directly estimate the reduced-form earnings difference in Section 4, finding cumulative per-complier losses of approximately €29,000 over the four years following childbirth for the replacement-rate reform and €35,000 for the duration reform.

[15] Note that this is practically equivalent to following the approach of Hendren (2017) and Giupponi (2024) and estimating the MB using the following formula: $MB = \frac{\frac{d\Phi}{db} b}{\epsilon}$, where ϵ is the semi-elasticity of labor force participation to labor earnings.

extent to which mothers undertake costly action, such as forgoing job protection and bearing significant long-run earnings losses, to increase their consumption in the high-benefit state provides a measure of the utility gain from more generous transfers.

The model capture costs and benefits accruing to mothers and the government budget, but are partial equilibrium in nature and omit externalities operating through firms. When mothers take up more UI, firms face higher expected turnover among women of childbearing age and respond by shifting hiring of young women toward temporary rather than permanent contracts. Carta et al. (2024) document this mechanism for the duration reform analyzed in this paper, interpreting it as statistical discrimination: firms that observe higher maternal quit rates offer lower-quality employment to young women who were not themselves covered by the reform. This externality is not internalized by the mother making the program choice and therefore does not enter the MB estimates derived above. Section 7 quantifies this channel and incorporates it into an extended MVPF calculation.

Empirical Implementation The formula requires calibrating κ, which captures the relative cost of participation across regimes through the joint behavior of $f(\cdot)$ and the elasticity $\mid \varepsilon(\Phi^s, b^s) \mid$. In the empirical implementation, I follow Fadlon, Nielsen et al. (2015) and make the simplifying assumption that $\kappa = 1$[16]. Using this assumption, it is possible to write:

$$MB \simeq \frac{b^{high}}{b^{low}} \frac{\Phi^{low}}{\Phi^{high}} - 1 \tag{3}$$

An attractive feature of this approach is that it evaluates choices within a fixed state of the world: motherhood. This is particularly important in the context of parental leave, where preferences may evolve following childbirth itself. Recent evidence suggests that individuals may mispredict the utility consequences of parenthood and family time (Kuziemko et al., 2018). By holding the state fixed and comparing choices made after childbirth, the revealed-preference framework recovers mothers' valuations conditional on the post-birth state, rather than relying on preferences formed before the arrival of the child.

To operationalize the framework, I combine two empirical objects: (i) the decrease in labor force participation induced by each reform, which identifies the behavioral response to benefit generosity; and (ii) the observed increase in UI benefits generated by the reform.. Together, these

[16]The assumption that $\kappa = 1$ follows Fadlon, Nielsen et al. (2015) "locally constant ratio" approach, which holds when the density $f()$ is approximately constant in the region between the two benefit regimes and the elasticity structure does not change substantially across regimes (see Appendix A. for derivation and discussion).

objects map observed choices into revealed-preference measures of mothers' willingness to trade long-run labor market attachment for more generous short-run insurance after childbirth.

Table A2 reports the parameters entering the MB calculations for both reforms. For the replacement rate reform, I estimate the change in the net present value of UI benefits induced by the reform using the strategy described in Section 4. For UI takers in the treated cohort, the reform increases expected UI benefits by Δb = €1,613 The baseline NPV of benefits for always-takers under the less generous regime (60% replacement rate) is b^{low} = €4,021, implying a benefit ratio of $b^{high}/b^{low} = 1.40$[17].

To estimate the participation response, I use the reduced-form estimates from 4. The reform decreases labor force participation by an average of $\Delta\Phi = 1.04$ percentage points over the four years following childbirth. With baseline participation $\Phi^{low} = 0.628$, this implies $\Phi^{high} = 0.617$ and a participation ratio of $\Phi^{low}/\Phi^{high} = 1.017$. Applying equation 3 yields $MB^{RR} = 1.4 \times 1.0173 - 1 = 0.42$.

The duration reform follows the same calculation procedure. As reported in Table A2, the duration reform generates a benefit ratio of 1.33 and a participation ratio of 1.015, yielding $MB^{Duration} = 0.35$, which is lower than the MB estimated for the replacement rate reform[18].

Compliers-Only Marginal Benefits A potential concern with this empirical implementation is that the reduced-form effects entering the formula combine compliers' extensive-margin response with always-takers' intensive-margin response to higher benefits. To isolate compliers' threshold revealed valuation, I follow Haller, Staubli, and Zweimüller (2024) and decompose the reduced-form effects into mechanical (always-takers) and behavioral (complier) components, using the always-takers share π_{AT} estimated in Section 4.2.3 and direct evidence on always-takers' UI spell length pre- and post-reform. Applying this decomposition yields compliers-only marginal benefit estimates of $MB^{C}_{RR} = 0.32$ for the replacement rate reform and $MB^{C}_{Duration} = 0.26$ for the duration reform. TThese estimates are modestly lower than the corresponding aggregate estimates, consistent with always-takers exhibiting some intensive-margin response to more generous benefits. The

[17] The benefit ratios b^{high}/b^{low} reported in Table A2 are computed using the net present value of UI benefits received under each regime. As a result, they need not coincide with simple ratios based on statutory replacement rates or maximum potential benefit durations. In practice, realized benefit amounts reflect not only policy parameters but also observed claiming behavior, benefit durations, and the timing of payments. The NPV-based ratios therefore provide a measure of the effective increase in benefits generated by each reform.

[18] As noted in Section 2, the duration reform occurs after the replacement rate reform, so mothers' valuation of extended duration is estimated conditional on the higher 75% replacement rate already in place. If demand for longer duration is lower when current benefit levels are already generous, the sequential implementation would attenuate the estimated response to the duration reform. This implies that the differential marginal benefit estimates likely represent a lower bound on the true difference in mothers' valuations of immediate versus delayed transfers.

relatively small gap between aggregate and complier-specific estimates suggests that aggregate marginal benefits provide a close approximation to the valuation of mothers at the participation margin.[19].

These estimates are useful because the welfare consequences of benefit expansions are ultimately determined by mothers whose choices change in response to the policy. While aggregate marginal benefits recover average revealed valuations among all affected mothers, the complier-specific estimates isolate the valuation of mothers whose participation decisions are altered by the reforms. The similarity between the two sets of estimates indicates that the main conclusions are not driven by mechanical responses among always-takers, but instead reflect substantial willingness among marginal mothers to trade future labor-market attachment for more generous support following childbirth.

It is useful to compare these values to those found for other social insurance schemes. In the context of disability insurance Haller and Staubli (2023) find a MB between 2.2 and 3.4 in line with the MB I estimate for the replacement rate reform. In the context of survivor benefits Coyne et al. (2024) and Giupponi (2024) find a MB between 0.23 and 0.37. Compared to the literature, the estimates for the population of mothers are moderate, suggesting that motherhood is a state with high revealed valuation of short-run insurance relative to job protection. This suggests first, that increasing the level of parental leave programs would deliver considerable welfare gains.

6 Interpreting Mothers' Choices

Our revealed preference estimates imply high valuations of more generous UI benefits after childbirth: $MB = 0.42$ for the replacement rate reform and$MB = 0.35$ for the duration reform. These aggregate estimates reflect a mixture of welfare-relevant valuations, like genuine liquidity needs, childcare scarcity, and preferences for time at home with an infant, and potentially distortion-driven components arising from behavioral frictions. Distinguishing these is crucial for policy design. If high marginal benefits primarily reflect optimization failures (present bias, inattention to job-loss risk), the welfare gains are likely to be overstated, and policy should focus on debiasing interventions or information provision. Conversely, if they primarily reflect welfare-relevant pref-

[19] Always-takers contribute meaningfully to the reduced-form change in benefits through the mechanical increase in their per-period benefit (RR) and the mechanical extension of their eligibility window (Duration), but their contribution to the reduced-form change in labor force participation is small, as documented above. The cleaner compliers-only estimates therefore primarily isolate the threshold mother's valuation from always-takers' mechanical benefit gains rather than any meaningful labor supply response, leaving compliers' threshold revealed valuation as the dominant remaining welfare object.

erences and binding constraints, benefit generosity delivers substantial welfare gains. This section examines the contribution of each component in turn

6.1 Interpreting Revealed Valuations in the Parental Leave Setting

The central challenge is not whether mothers value more generous benefits, but why they do. The revealed-preference estimates imply that many mothers are willing to sacrifice future earnings and labor-market attachment in exchange for more generous benefits after childbirth. The same observed choice, however, may be rationalized by very different underlying mechanisms. In standard social-insurance settings such as unemployment insurance or disability insurance, willingness to pay for benefits is typically interpreted as reflecting the value of consumption smoothing and insurance against income risk. In the parental-leave setting, benefits affect a broader set of outcomes. More generous benefits increase resources available during a period in which household expenditures are elevated and labor supply opportunities may be limited. They also allow mothers to remain out of work for longer following childbirth, generating additional time at home with an infant. Finally, they may relax constraints created by limited childcare availability. These channels are welfare-relevant. At the same time, observed willingness to pay may also reflect behavioral frictions. Mothers may underestimate the long-run consequences of losing job protection, underappreciate re-employment difficulties, or place disproportionate weight on immediately available transfers relative to future outcomes. In these cases, revealed-preference estimates may overstate welfare-relevant valuations. The two reforms studied in this paper provide useful leverage on these questions. Both reforms increase the attractiveness of remaining out of work following childbirth, but they differ substantially in the timing of benefits. Comparing responses across the two reforms therefore helps distinguish between explanations based on infant-care preferences alone and explanations that emphasize liquidity needs or behavioral responses to the timing of transfers.

6.2 The Role of Constraints

Liquidity Constraints One primary explanation for the high valuation of short-run benefits is that mothers are liquidity constrained and unable to smooth consumption perfectly across the period of childbirth. In this context, UI benefits provide a critical increase in cash-on-hand during a period in which short-run liquidity is particularly valuable. To test for the presence of these constraints, I proxy for a household's ability to smooth consumption using its position in the house-

hold income distribution, dividing mothers into three terciles based on household income.[20]

Figure 5 panel (a) shows the effect of the replacement rate reform on the take-up of UI by mothers in the first year after the end of ML, for each of the three terciles of the household income distribution. Mothers in the lowest income tercile are significantly more likely to take up UI in response to higher benefit levels than those in the top tercile. This steep gradient suggests that for low-income households, the liquidity effect is a primary determinant of their decisions.

Figure 5 panel (b) shows a flatter relationship for the duration reform. While low-income mothers remain more likely to take up UI, the differences across terciles are not statistically significant.

The divergence in behavioral responses across the two reforms provides a suggestive test for the importance of liquidity constraints. Mothers exhibit a significantly higher sensitivity to immediate transfers (the replacement rate reform), compared to the "deferred" liquidity provided by the duration reform. This suggests that, after childbirth, the demand for cash-on-hand is so high that mothers are willing to forgo the significant long-term value of job protection to secure immediate liquidity. Consistent with the theoretical framework, these results suggest that mothers prioritize short-term benefits, even when such a choice carries substantial future costs.

Childcare Availability Beyond liquidity, the behavioral response to these reforms likely reflects the significant cost of returning to work immediately following childbirth. If childcare is unavailable or expensive, the utility cost of employment increases, making extended leave more attractive even at the cost of job protection. I investigate this mechanism by examining the correlation between UI take-up and local childcare availability.

Figure 6 reports the effect of the reforms on UI take-up for mothers by availability of formal childcare for children aged 0-2 year-old. Availability of formal childcare is measured as the percentage of 0–2 year-old children that both public and private nurseries can accommodate, by region[21]. Regions with availability of childcare above the median are categorized as *High* childcare regions and regions with availability of childcare below the median are categorized as *Low* childcare regions.

Figure 6 panel (a) reports the results for the replacement rate reform. The figure shows that the effect of the replacement rate reform on UI take-up is significantly higher in regions with low childcare than in regions with high childcare. Figure 6 panel (b) reports the results for the duration reform, where the decreasing relationship between UI take-up and availability of childcare is also

[20] As explained in Section 3.1, due to the structure of the data, the sample of mothers which I can link to the corresponding fathers and for which I can calculate household income is likely to be positively selected.

[21] Italy has 20 regions, which correspond to the NUTS2 level.

pronounced. Because the duration reform extends the timeframe a mother can remain at home, it directly addresses the structural constraints created by a lack of nurseries. In regions where childcare is scarce, the implicit cost of returning to work is so high that the option to forgo job protection in exchange for extended benefits becomes particularly attractive. In regions with low provision, the UI system seems to function as a second-best home childcare subsidy.

Preferences for Infant Care Preferences for infant care deserve special attention because, unlike behavioral frictions, they are part of the policy objective itself. Parental leave policies are explicitly designed to allow parents to spend time away from work following childbirth. To the extent that mothers value spending time with infants, this component should be interpreted as a welfare benefit generated by the policy rather than a distortion that should be removed from revealed-preference estimates.

This distinction is important when interpreting the marginal-benefit estimates. In standard unemployment-insurance analyses, longer non-employment spells are often treated as the primary behavioral cost of benefit generosity. In the parental-leave setting, however, time away from work is partly the intended outcome of the policy. Mothers who choose more generous benefits are not simply purchasing additional consumption; they are purchasing a bundle consisting of higher short-run resources and additional time at home with a newborn child.

A potential concern is that leave itself may alter preferences, causing mothers to become increasingly attached to remaining out of work. While this possibility cannot be completely ruled out, several features of the setting suggest it is unlikely to be the dominant explanation for the results. First, all mothers experience the same five-month period of compulsory maternity leave before the relevant choice between parental leave and unemployment insurance arises. Second, if evolving preferences for infant care were the primary driver, one would expect substantially stronger responses to the duration reform, which extends time away from work by considerably more months than the replacement-rate reform. Instead, mothers respond more strongly to the reform that delivers benefits immediately. This pattern suggests that while preferences for infant care likely contribute to observed valuations, they cannot fully account for the estimated marginal benefits.

6.3 The Role of Behavioral Frictions

In a revealed-preference framework, the relevant object is mothers' perceived future cost of giving up job protection, rather than the true ex post cost. If mothers underperceive re-employment risk

or fail to internalize the long-run employment consequences of job separation, observed take-up behavior may overstate welfare-relevant willingness to pay for UI generosity. This implies that part of the estimated marginal benefit may reflect biased beliefs or limited attention rather than welfare-relevant valuations. A growing literature has documented the importance of behavioral frictions in insurance and social insurance choices (Ganong and Noel, 2019; Gerard and Naritomi, 2021). In this case, revealed-preference estimates may partially capture optimization failures rather than welfare-relevant preferences, creating a wedge between observed willingness to pay and mothers' true welfare gains from more generous benefits. This section explores whether the observed responses are affected by behavioral frictions by examining two patterns: inattention to labor market risk and present bias. To assess how sensitive the welfare conclusions are to these mechanisms, I estimate marginal benefits separately for subsamples of mothers less likely to face severe behavioral frictions or unusually high marginal utility associated with binding childcare. Specifically, I focus on mothers in low-unemployment regions, where re-employment risk is lower and easier to perceive, and mothers in regions with high childcare availability, where structural barriers to returning to work are less severe. These estimates should therefore be interpreted as conservative or lower-bound valuations relative to the aggregate population.

Inattention If mothers are fully rational and forward-looking, the decision to forgo job protection should depend on the true cost of job loss. Theoretically, the cost of losing job protection $\phi(\theta)$ should be significantly higher in depressed labor markets where the probability of re-employment is lower. One would expect that higher regional unemployment rates would lead to a lower take-up of UI, as mothers internalize the increased difficulty of finding a new job after benefits are exhausted.

Figure 7 shows the heterogeneity in the take-up of UI by levels of regional unemployment rate. The observations are divided in two groups: below and above the median regional unemployment rate. The figure shows that, for both reforms, the responses are strikingly similar across different labor market conditions (in fact that the take-up s slightly higher in high-unemployment regions). This near-zero sensitivity of take-up to local labor market conditions is difficult to reconcile with full information and rational optimization and suggests that mothers are not incorporating local re-employment risk into their program choices[22].

[22]This logic parallels Landais and Spinnewijn, 2021 who document a similar misperception channel in the context of Swedish unemployment insurance. Comparing elicited and realized job-loss probabilities, they find that workers' perceived risks correlate only weakly with realized outcomes, and correcting for this wedge reduces their estimated MRS by approximately one-third.

Figure 8 shows that the employment consequences of this choice are not uniform across regions. Mothers in high-unemployment regions suffer substantially larger labor force participation losses following the reforms than those in low-unemployment regions. The disconnect between identical take-up behavior and heterogeneous realized costs provides direct evidence of inattention: mothers in depressed labor markets are paying a substantially higher price for the same benefit, apparently without adjusting their choices accordingly. This suggests that the aggregate MB estimates are partially inflated by optimization failures and should be interpreted as upper bounds on welfare-relevant valuations.

Timing of Transfers and Present Bias The comparison between the two reforms is informative about the importance of benefit timing. Both reforms increase the attractiveness of remaining out of work following childbirth, but they differ substantially in when benefits are received. The replacement-rate reform increases payments immediately, whereas the duration reform delivers gains later in the leave spell. The revealed-preference estimates indicate systematically larger valuations for the replacement-rate reform than for the duration reform. Using aggregate estimates, I find ($MB_{RR} = 0.42$) compared to ($MB_{Duration} = 0.35$). The same pattern emerges for the complier-specific estimates ($MB^C RR = 0.32$ versus $MB^C Duration = 0.26$), indicating that the difference is not driven by always-takers' intensive-margin response. This pattern suggests that the timing of transfers matters. Preferences for infant care should increase the value of both reforms, since both expand the opportunity to remain at home following childbirth. By contrast, liquidity constraints and present-biased preferences predict stronger responses to the replacement-rate reform, whose gains are realized immediately, than to the duration reform, whose gains accrue later in the leave spell. The evidence does not provide a sharp test of present bias. Nevertheless, the stronger response to immediate transfers is difficult to reconcile with a framework in which mothers care only about total resources and time spent out of work. Taken together, the results suggest an important role for immediate liquidity and potentially present-biased valuation of transfers in shaping mothers' choices after childbirth.

6.4 Conservative Estimates of Welfare-Relevant Valuations

The discussion above suggests that the estimated marginal benefits reflect a combination of welfare-relevant motives and behavioral frictions. Welfare-relevant motives include consumption smoothing, preferences for time spent with infants, and the relaxation of childcare constraints. Behavioral frictions may include limited attention to labor-market risks and potentially present-biased

responses to the timing of transfers.

Note that, unlike in standard social insurance settings, preferences for time spent at home with an infant are not treated as distortions. Parental leave policies are explicitly designed to facilitate parental time investments following childbirth, and to the extent that mothers value spending time with infants, this component represents a welfare benefit of the policy rather than a bias that should be removed from revealed-preference estimates.

The objective of this section is to examine how sensitive the estimated marginal benefits are to environments in which behavioral frictions and structural constraints are plausibly less severe. The resulting estimates should be interpreted as conservative revealed valuations that continue to incorporate the value mothers place on infant care and post-birth consumption smoothing, while reducing the influence of factors such as inattention to labor-market risk, present-biased responses to transfer timing, and limited childcare availability.

To implement this approach, I estimate marginal benefits separately for mothers facing lower unemployment risk and for mothers living in areas with greater childcare availability. If behavioral frictions or structural constraints are important drivers of the baseline estimates, marginal benefits should decline in these environments. Conversely, persistence of substantial valuations would suggest that a large portion of mothers' willingness to pay reflects welfare-relevant preferences rather than distortions alone.

The evidence above suggests that aggregate marginal-benefit estimates reflect a combination of welfare-relevant motives and behavioral frictions. Welfare-relevant motives include liquidity needs, childcare constraints, and preferences for time spent with infants. Behavioral frictions include limited attention to labor-market risk and potentially present-biased responses to the timing of transfers. The objective of this section is not to recover a uniquely correct welfare parameter. Rather, it is to examine how sensitive the marginal-benefit estimates are to environments in which particular frictions or constraints are plausibly less severe. I therefore estimate marginal benefits for subsamples of mothers facing lower unemployment risk and greater childcare availability. These estimates should be interpreted as conservative lower-bound valuations rather than corrected welfare measures.

The evidence presented above suggests that aggregate valuations reflect three components: welfare-relevant liquidity needs, genuine preferences for time at home with an infant, both of which belong in the welfare-relevant MB, and distortion-driven components arising from present bias and inattention, which do not[23]. Distinguishing these is crucial for policy design: if high

[23]Importantly, preferences for time spent at home with an infant are not interpreted as distortions. The analysis is

marginal benefits primarily reflect optimization failures, the welfare gains are likely overstated and policy should focus on debiasing interventions. Conversely, if they primarily reflect welfare-relevant preferences and binding constraints, benefit generosity delivers substantial welfare gains.

To further examine the role of behavioral frictions and structural constraints, I estimate MB for different subsamples in which these forces are likely to be less severe[24]. To address present bias, I focus specifically on responses to the duration reform, which is likely to be considerably less susceptible to present-bias distortions than the replacement rate reform, since its gains accrue in future months of the spell rather than immediately. Within this reform, I estimate $MB_{Duration}$ for two subsamples: (1) where the gap between perceived and actual re-employment risk has the smallest welfare consequences; and (2) mothers in high-childcare regions, where structural barriers to returning to work are lower.

Table 7 reports $MB_{Duration}$ estimates across subsamples. Comparing the aggregate estimate ($MB_{Duration} = 0.35$) to the low-unemployment subsample ($MB = 0.21$) provides suggestive evidence on the role of inattention to labor market risk. In low-unemployment regions, the welfare consequences of any gap between perceived and realized re-employment risk are minimal, so program choices in this subsample are less distorted by inattention than in high-unemployment regions where the same misperception would carry larger consequences. Under this interpretation, inattention accounts for approximately 40% of the aggregate valuation, a magnitude in the same range as the wedge documented in other revealed-preference UI settings[25]. Comparing the aggregate estimate to the high-childcare subsample tries to isolate the component driven by structural childcare constraints. For this subsample the marginal benefit equals 0.19, suggesting that childcare scarcity accounts for approximately 46% of the aggregate valuation.

The complier characteristics support interpreting these subsample estimates as conservative measures of the welfare-relevant MB. The mothers driving MB_{RR} are younger, lower-earning, more likely to work part-time, and have shorter firm tenure: the profile of mothers for whom liquidity constraints are most binding and whose high valuation is most plausibly welfare-relevant.

intended to provide more conservative revealed valuations under conditions where behavioral frictions and structural constraints are plausibly less severe, while still incorporating the value mothers place on infant care and post-birth consumption smoothing.

[24] This approach parallels the strategy of Allcott, Lockwood, and Taubinsky (2019), who estimate welfare-relevant demand for sugar-sweetened beverages by predicting consumption among consumers with dietitian-level nutrition knowledge and perfect self-control. Just as they condition on observable proxies for internality bias (nutrition knowledge, self-control) to isolate the rational component of demand, I condition on observable proxies for behavioral frictions (regional unemployment rates, timing of benefit receipt, presence of childcare) to provide more conservative estimates of the welfare-relevant component of mothers' program valuations.

[25] Landais and Spinnewijn, 2021 find that correcting for risk misperception reduces their estimated MRS for Swedish UI by approximately one-third, a similar order of magnitude despite the different institutional context and identification strategy.

The mothers driving $MB_{Duration}$ are older, higher-earning, predominantly on permanent contracts, with longer firm tenure: mothers with the strongest labor market attachment and therefore the most to lose from giving up job protection. Their moderate revealed valuation is consistent with partial inflation by inattention to re-employment risk, as evidenced by the near-identical take-up responses across unemployment regions. The conservative subsample of low-unemployment duration-reform compliers provides a more conservative revealed valuation under conditions where behavioral frictions and structural constraints are plausibly less severe. The resulting estimate, $MB_{LowUnemployment} = 0.21$, suggests that mothers continue to place substantial value on post-birth consumption smoothing and time spent at home with an infant even in environments where distortions are likely less pronounced.

The progression

$$MB_{RR} = 0.42 \rightarrow MB_{Duration} = 0.35 \rightarrow MB_{Low\,Unemployment} = 0.21,\ MB_{High\,Childcare} = 0.19$$

summarizes the paper's central welfare finding. The gap between MB_{RR} and $MB_{Duration}$ reflects both rational liquidity preferences (immediate cash is most valuable when liquidity constraints bind) and present bias (mothers overweight transfers received today). The gaps between $MB_{Duration}$ and the subsample estimates bound the contribution of specific constraints and frictions. These complementary conservative estimates place the welfare-relevant valuation in the range of 0.19–0.21, considerably lower than the aggregate estimate of 0.35 but still meaningful, confirming that parental leave transfers retain welfare value even after correcting for behavioral frictions and structural barriers to employment[26]. Together, these estimates bracket the welfare-relevant MB: aggregate values represent an upper bound inflated by behavioral frictions and structural barriers, while the subsample estimates from low-friction populations provide a conservative lower bound in the range of 0.19–0.21.

Household Responses The marginal benefit estimates reflect mothers' revealed valuations holding other household behavior constant. If partners systematically increase labor supply when mothers take up UI, part of the measured MB would capture intra-household income smoothing rather than mothers' individual valuations. To assess this concern, I examine partner responses in the subset of mothers who can be linked to partners in administrative records (approximately

[26] I find that behavioral distortions and structural constraints together account for approximately 40–46% of the aggregate valuation, in the same order of magnitude as Allcott, Lockwood, and Taubinsky (2019), who find that internality bias accounts for 31–37% of sugar-sweetened beverage consumption in their contex.

44.6% of the sample, positively selected as seen in Table A1). In this selected sample, I find modest and inconsistent effects on partners' employment: small temporary reductions (0.3-0.5 percentage points) in the replacement rate reform and small increases (0.5-0.9 percentage points) in the duration reform, concentrated in months 5-10. Partners' take-up of parental leave is largely unaffected by either reform, with point estimates small and inconsistent relative to the very low baseline rate (< 1% of partners take PL). While the selected nature of the linked sample precludes definitive conclusions, the modest magnitude of partner responses relative to mothers' 1.9 percentage point increase in UI take-up suggests that household-level adjustments are unlikely to be the primary driver of the estimated marginal benefits.

Taken together, the evidence implies that aggregate MB estimates are best interpreted as upper bounds on welfare-relevant willingness to pay. Effective policy design should target the structural sources of high valuation, liquidity constraints and childcare scarcity, while mitigating the behavioral frictions that lead mothers to underweight long-run costs. Interventions such as information provision at the point of birth, defaults that favor job-protected leave, or expanded childcare subsidies may deliver welfare gains that are both larger and more durable than benefit expansions alone.

7 Fiscal Externalities and the Marginal Value of Public Funds

In this final section, I follow Hendren and Sprung-Keyser (2020) to consider the welfare impact of the two reforms analyzed. To quantify the effect of the reforms on welfare, I use the Marginal Value of Public Funds (MVPF), which is defined as:

$$MVPF = \frac{Benefits}{Net\ Government\ Costs} = \frac{1 + MB}{1 + FE}$$

where MB is mothers' marginal benefit estimated in Section 5 and FE is the fiscal externality measured below. The MVPF framework is particularly well suited to this setting because it allows the welfare implications of the two reforms to be compared on a common scale, and because it can accommodate multiple scenarios reflecting different assumptions about the welfare-relevant component of mothers' valuations and the full social cost of the reforms. I present three scenarios in Table 8, each progressively broadening the scope of the welfare analysis.

7.1 Baseline MVPF

To estimate net government cost (the denominator), I use reduced-form estimates (following the same strategy highlighted in Section 3.2) of the reforms on UI spending, tax revenues, and spending on other social programs (primarily PL), which quantify the cost or savings from program interactions (Inderbitzin, Staubli, and Zweimüller, 2016).

For the replacement rate reform, the estimates indicate that the reform leads to an increase in UI benefits of €291.96, while at the same time decreasing tax revenue in the four years after childbirth by €162.85. Importantly, some mothers after the reforms switch from PL to UI, and therefore the government saves around €43 in PL benefits. The estimates indicate an implied fiscal externality of $(162.85 - 43)/291.96 = 0.41$ per euro of transfer. For every euro spent on higher UI benefits, the government loses an additional 41 cents in tax revenue and PL savings ($1 + FE = 1.41$).

The duration reform follows a similar pattern but with larger magnitudes. UI spending increases by €671, PL spending falls by €20, and tax revenue falls by €218, yielding a net cost of €955 and a fiscal externality of 0.29 ($1 + FE = 1.29$).

Panel (a) of Table 8 reports the baseline MVPF estimates using the aggregate marginal benefit estimates from Section 5 (MB = 0.42 for the replacement rate reform, 0.35 for the duration reform). The MVPF for the replacement rate reform is 1.01; for the duration reform, it is 1.05. Both reforms therefore are at or just above the welfare-neutral threshold under aggregate MB assumptions: mothers' implicit willingness to pay per euro of transfer approximately matches the net fiscal cost of providing the transfer.

To my knowledge, Paradisi (2021) is the only prior paper to compute an MVPF for parental leave. Studying the Danish setting, he reports an MVPF of 0.7 with firm-level spillovers included, falling to 0.11 when restricted to the workers directly affected. This paper differs in how the welfare numerator is constructed, which Paradisi (2021) does not estimate directly. The MVPFs reported here lie at the upper end of Paradisi's range and reflect what direct measurement of mothers' marginal benefit reveals: their willingness to forgo job protection in exchange for higher transfers implies a meaningful welfare value of parental leave generosity. These estimates are comparable to the range documented for adult social insurance programs (Hendren and Sprung-Keyser (2020) report MVPFs of 0.8 for retirement and 0.87 for disability insurance).

7.2 Conservative MVPF

The baseline MVPF estimates use aggregate MB values that, as documented in Section 6, reflect a mixture of welfare-relevant valuations and behavioral distortions. Panel (b) incorporates conservative MB estimates derived in Section 6.4, which focus on subsamples least affected by behavioral frictions or structural constraints. Because these subsamples are identified within the duration reform sample, the conservative MVPF estimates are computed for the duration reform only.

I present two conservative scenarios. The first uses $MB_{Low\,Unemployment} = 0.21$, estimated from mothers in low-unemployment regions responding to the duration reform. This serves as a conservative lower bound that nets out the contribution of inattention to re-employment risk. The second uses $MB_{High\,Childcare} = 0.19$, estimated from mothers in high-childcare regions responding to the duration reform. This corresponds to a counterfactual in which childcare scarcity no longer binds. Under the first conservative scenario, the MVPF for the duration reform is 0.94. Under the second scenario, it is 0.92. Restricting to subsamples where behavioral distortions and structural constraints are plausibly less severe therefore brings the duration reform's MVPF just below the welfare-neutral threshold. Even under these conservative assumptions, parental leave transfers retain meaningful welfare value: the welfare-relevant willingness to pay per euro of transfer (1.19–1.21) remains close to, though slightly below, the net fiscal cost (1.29).

7.3 Extended MVPF: Accounting for Job Protection Externalities

The welfare calculations in Panels (a) and (b) capture costs and benefits accruing to mothers and the government budget but omit externalities generated through effects on job protection. When mothers give up job protection to take UI, firms exposed to higher maternal quit rates respond by shifting hiring of young women toward fixed-term contracts rather than permanent contracts. Carta et al. (2024) document this mechanism for the duration reform analyzed in this paper, interpreting it as statistical discrimination: firms observing higher quit rates among mothers update their beliefs about the productivity of young women and offer lower-quality employment to workers who were not themselves covered by the reform. This externality is not internalized by mothers making the program choice and therefore does not enter the MB estimates in Section 5.

I quantify this externality by combining Carta et al. (2024) firm-level hiring response with ISTAT data on the earnings gap between fixed-term and permanent contracts, and applying an effective tax rate to the foregone earnings. The resulting estimate is approximately €0.21 per euro of

UI transfer for the duration reform[27].

Incorporating this externality raises the effective fiscal externality of the duration reform from 1.29 to 1.50, lowering the MVPF from 1.05 to 0.90 under aggregate MB assumptions, to 0.81 under the low-unemployment conservative MB, and to 0.79 under the high-childcare conservative MB. The discrimination externality is not estimated for the replacement rate reform because Carta et al. (2024) study only the duration reform's effects on firm hiring behavior.

7.4 Interpretation and Policy Implications

Table 8 summarizes all three scenarios. Four conclusions emerge from the table.

First, the welfare conclusion depends crucially on which marginal benefit estimate is used. Under the aggregate MB, both reforms are at or slightly above MVPF = 1, essentially at the welfare-neutral threshold (1.01 for the replacement rate reform, 1.05 for the duration reform). Under the conservative MB estimates obtained from subsamples where behavioral distortions and structural constraints are plausibly less severe (computed for the duration reform only, where these subsamples can be identified), the duration reform falls slightly below 1 (0.92–0.94). Accounting for the firm-side discrimination externality for the duration reform brings its MVPF further down to 0.79–0.90.

Second, the magnitude of MVPF estimates is sensitive to how mothers' valuations are interpreted. Under aggregate MB, the implicit recipient willingness to pay per euro of transfer (1.35–1.42) slightly exceeds the fiscal cost (1.29–1.41). Under conservative MB for the duration reform, willingness to pay falls to 1.19–1.21, below the fiscal cost of 1.29. The difference between aggregate and conservative MVPFs for the duration reform (1.05 vs 0.92–0.94) indicates that whether the duration reform clears the welfare threshold is sensitive to the contribution of behavioral distortions to the aggregate revealed valuation.

Third, the firm-side discrimination externality is an important welfare channel that materially lowers the welfare value of the duration reform. Incorporating the externality documented by Carta et al. 2024 lowers the duration reform's MVPF from 1.05 to 0.90 under aggregate MB, and to 0.79–0.81 under conservative MB. An analogous externality is not available for the replacement rate reform, since Carta et al. 2024 study only the duration reform's effects on firm hiring behavior. While comparable discrimination responses would likely also follow expansions of benefit levels, quantifying this remains outside the scope of the present analysis. The takeaway is that the broader

[27] Full details are provided in Appendix .

social costs of reduced job protection, when they can be measured, substantially reduce the welfare value of expanded parental leave generosity.

Fourth, the cross-reform structure of the analysis enriches both the welfare estimates and the externality accounting. The differences in benefit timing across the two reforms permit identification of present bias, which would not be separable from other sources of high valuation under a single reform. The discrimination externality is identified using variation specific to the duration reform; while comparable firm-side responses may well exist for the replacement rate reform, quantifying them requires evidence beyond the scope of the present paper. Together, these features provide more layered welfare evidence than a single-reform evaluation could deliver.

These results carry concrete policy implications. The headline takeaway is that expanding parental leave generosity is approximately welfare-neutral under aggregate revealed valuations, and modestly welfare-destroying for the duration reform once behavioral distortions are accounted for or once firm-side externalities are incorporated. The welfare returns accrue primarily to liquidity-constrained mothers and those facing high childcare costs, the structural sources of high post-birth marginal utility documented in Section 6. Realizing those gains more efficiently requires complementary policies that address these constraints directly. Expanded public childcare provision represents the most promising intervention. Expanded access would allow mothers to maintain labor force attachment while still receiving financial support during the post-birth period, addressing both the high marginal utility of post-birth liquidity and the structural barrier that drives mothers to forgo job protection. Information provision at the point of birth, which helps mothers make more informed decisions about job protection and re-employment risk, could address the inattention documented above. Defaults that favor job-protected parental leave over unemployment insurance could mitigate the effects of present bias while preserving choice. The MVPF calculations should therefore be read not as an indictment of parental leave policy but as a diagnostic of where the welfare returns are highest and where policy design has the most room for improvement.

The central insight is that benefit generosity alone is approximately welfare-neutral. Effective policy must simultaneously address the structural constraints (liquidity, childcare) that drive mothers' high valuations and the behavioral frictions (present bias, inattention) that lead them to underweight the long-run costs of forgoing job protection. The discrimination externality further suggests that policy should be evaluated not only in terms of its direct effects on mothers but also its indirect effects on the labor market opportunities of other young women. Finally, the MVPF estimates presented here focus on the direct fiscal and welfare effects for mothers. A complete social welfare analysis would incorporate effects on child development, fertility, and long-run family

outcomes, which are beyond the scope of this paper.

8 Conclusions

This paper provides new evidence on the effects of different parental leave parameters on mothers' choices after childbirth, their labor market trajectories, and the welfare implications of program design. Leveraging the unique institutional setting of Italy, where mothers can opt for higher unemployment insurance benefits in exchange for forgoing the job protection of standard parental leave, I provide revealed-preference estimates of the value mothers place on benefit generosity relative to career stability.

My empirical findings demonstrate that mothers are highly responsive to the composition and generosity of benefits. Following reforms that increased UI replacement rates and extended potential benefit duration, mothers significantly increased their take-up of UI, substituting away from protected parental leave. While this choice provides immediate financial security, it carries a substantial long-term cost. Marginal mothers induced to take up UI forgo approximately €29,000 in cumulative earnings over the four years following childbirth for the replacement rate reform, and €35,000 for the duration reform. This earnings loss is almost entirely an extensive margin phenomenon: the loss of job protection leads to persistent non-employment after benefits expire, rather than lower wages or fewer hours conditional on employment.

Using a revealed-preference framework, I find marginal benefits of 0.42 for the replacement rate reform and 0.35 for the duration reform, indicating that mothers place meaningful welfare value on additional parental leave transfers, particularly when transfers take the form of immediately higher benefit levels. These valuations are rooted in structural constraints: liquidity-constrained households and mothers in regions with scarce childcare are the most likely to forgo job protection for immediate cash. For these women, the UI system functions as a second-best home childcare subsidy and a source of liquidity when the marginal utility of consumption is high.

The analysis also reveals that aggregate revealed-preference valuations overstate the welfare-relevant component due to behavioral frictions. Mothers value immediate transfers from the replacement rate reform approximately 20% more than equivalent deferred transfers from the duration reform, a pattern consistent with present bias, and appear to underweight local labor market risks when making leave decisions, consistent with inattention to re-employment risk. Importantly, genuine preferences for time at home with an infant are welfare-relevant and are not treated as distortions. To bound the welfare-relevant component, I focus on the duration reform, which

is less susceptible to present-bias distortions, and estimate marginal benefits separately for subsamples of mothers where these frictions are likely to be less severe: those in low-unemployment regions and in high-childcare regions. The resulting marginal benefits of 0.19–0.21 should be read as conservative lower bounds: they are considerably lower than the aggregate estimates but still meaningful, confirming that parental leave transfers retain welfare value even under conservative assumptions. The gap between aggregate and conservative estimates is consistent with behavioral distortions and structural constraints accounting for approximately 40–46% of the aggregate valuation.

A welfare analysis using the MVPF framework yields baseline estimates of 1.01 for the replacement rate reform and 1.05 for the duration reform under aggregate marginal benefit assumptions. Both reforms therefore hover at the welfare-neutral threshold: mothers' implicit willingness to pay per euro of transfer approximately matches the net fiscal cost of providing the transfer. This near-neutrality, rather than a clear welfare loss or gain, is itself a substantive finding for the design of parental leave systems. Under more conservative MB assumptions for the duration reform, the MVPF falls to 0.92–0.94, slightly below the welfare-neutral threshold. A full welfare assessment must also account for the broader social costs of reduced job protection. Incorporating the statistical discrimination externality documented by Carta et al. (2024), firms shifting hiring of young women toward fixed-term contracts following higher maternal quit rates, substantially raises the effective fiscal cost of the duration reform, lowering its MVPF to 0.90 under aggregate MB assumptions and to 0.79–0.81 under conservative MB assumptions.

These results carry concrete policy implications. First, expanding parental leave generosity along margins that induce labor force exit is approximately welfare-neutral under aggregate revealed valuations and falls below welfare-neutrality once behavioral distortions or firm-side externalities are accounted for for the duration reform. Second, because present bias and inattention inflate revealed willingness to pay, simply raising benefit levels risks exacerbating long-run career costs for the mothers least equipped to bear them. Policies that address underlying frictions directly (information provision at the point of birth, defaults that favor job-protected leave, or expanded childcare subsidies) may deliver welfare gains that are both larger and more durable than benefit expansions alone.

The trade-off between benefit generosity and job protection studied here is a general feature of parental leave design, even where mothers do not face an explicit choice between programs as in Italy. The Italian setting makes the trade-off observable, allowing for direct revealed-preference estimation of the welfare value mothers place on benefit generosity relative to career stability.

The point estimates may not transfer directly to settings with different institutional features, but the broader insights, that mothers place meaningful welfare value on transfers, that liquidity and childcare constraints drive high valuations, that behavioral frictions inflate revealed willingness to pay, and that expanding generosity along margins that induce labor force exit delivers welfare value close to fiscal cost rather than substantially above it, are likely to apply to parental leave systems more broadly. Several dimensions lie beyond the scope of this analysis: the paper does not study effects on child development or fertility, and does not model the problem at the household level. Future work integrating household bargaining, child outcomes, and long-run fertility responses would complement the supply-side welfare analysis provided here. The findings nonetheless point to a clear design principle: in parental leave systems where mothers face a real trade-off between benefit generosity and job protection, benefit expansions alone deliver welfare returns close to neutrality; realizing larger welfare gains requires complementary policies that address the underlying liquidity and childcare constraints that drive high valuations.

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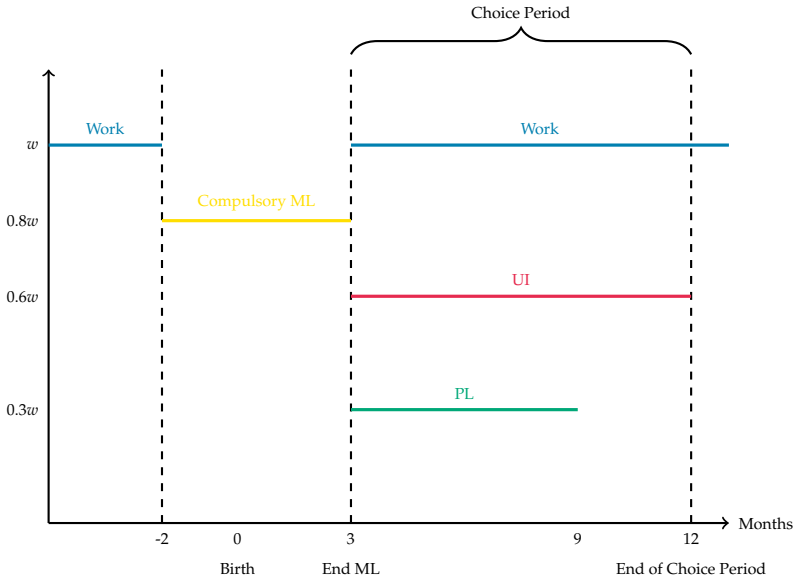
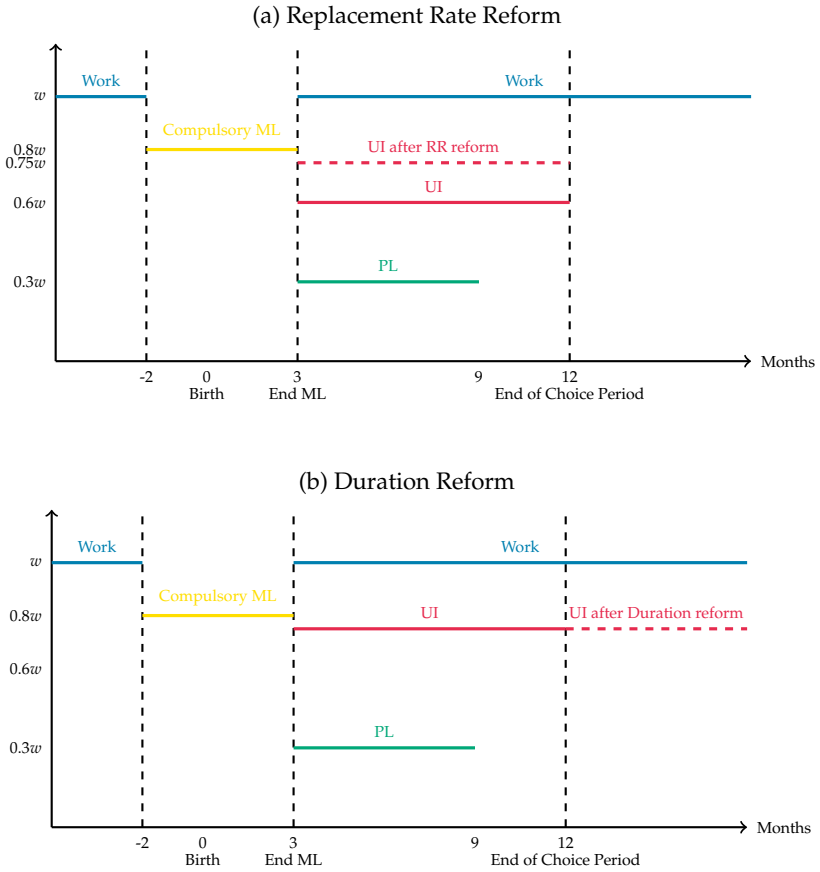
Figures

Figure 1: TIMELINE



Notes: The figure shows the timeline of programs and options 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 choose to resign and take up unemployment insurance at any time up to $t = 12$ ("choice period"). The y-axis indicates the benefit level as a share of the wage w for each option. See Section 2 for details.

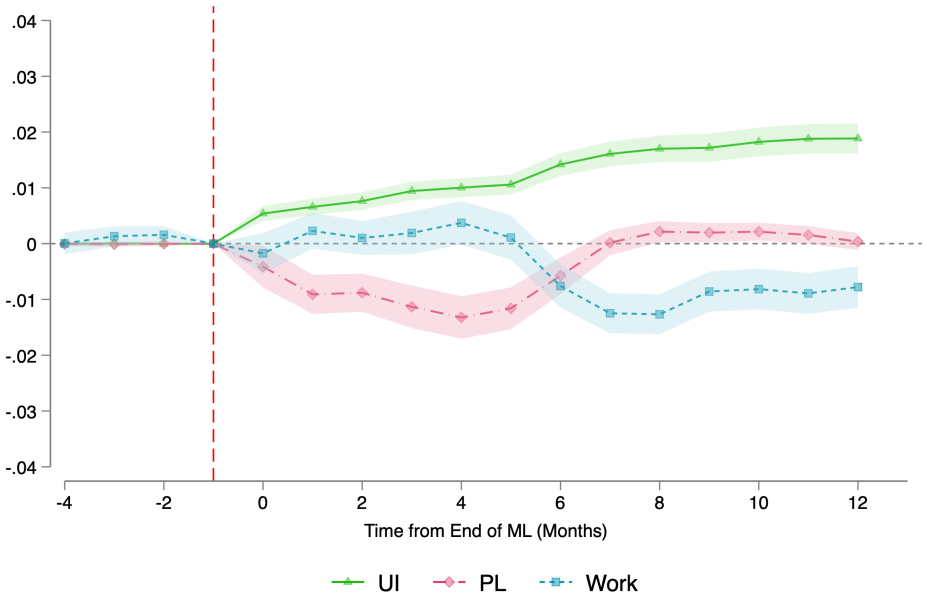
Figure 2: Effects of The Reforms on Options for Mothers



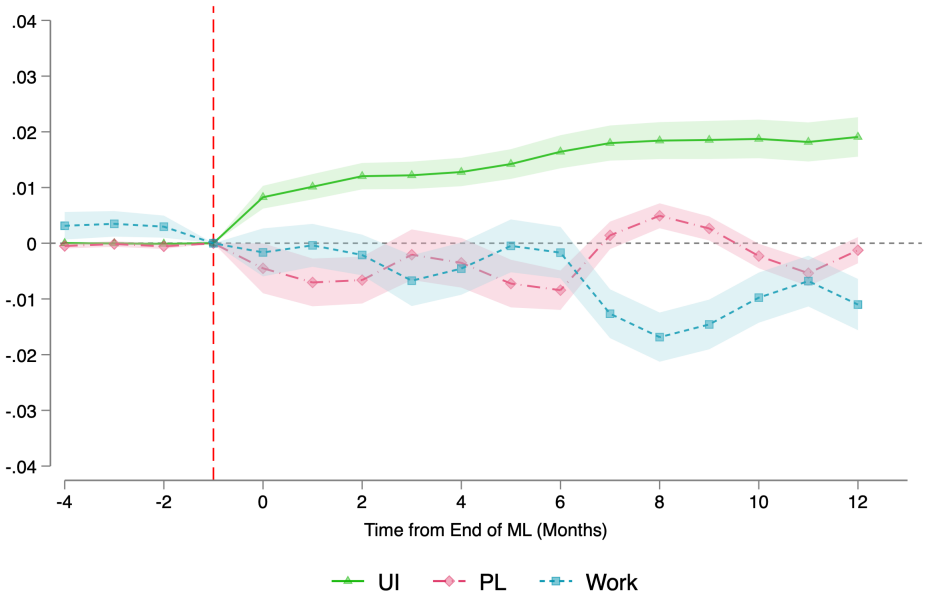
Notes: The graphs illustrate how the unemployment insurance reforms impacted the policy space for mothers after childbirth. $t = 0$ corresponds to the time of childbirth. From $t = 3$ onwards women can choose to go back to work, take up PL or take up UI. Panel (a) shows the effect of the replacement rate reform, which increased the replacement rate of UI from 60 to 75% 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. The y-axis indicates the benefit level as a share of the wage w for each option. See Section 2 for details.

Figure 3: Responses to Changes in UI Benefits

(a) Replacement Rate Reform



(b) Duration Reform

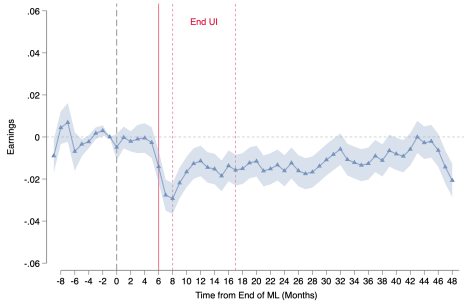


Notes: The figures report difference-in-differences estimates of the coefficients β_k from specification 1 estimated on three different dependent variables: the probability that a mother is on UI at relative time t (green series), the probability that a mother is working at relative time t (blue series) 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.

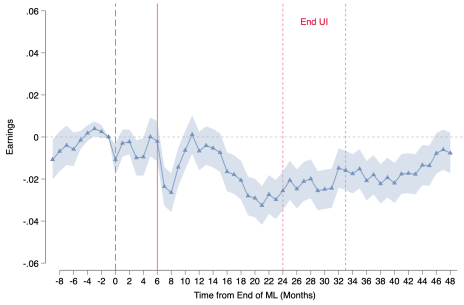
Figure 4: Effects of the Reforms on Earnings and Participation

Earnings

(a) Replacement Rate Reform

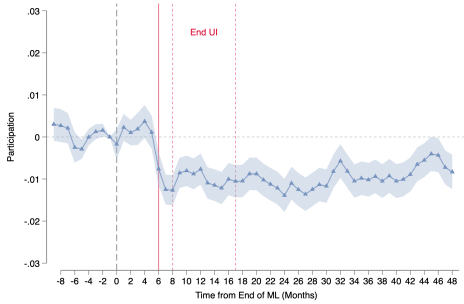


(b) Duration Reform

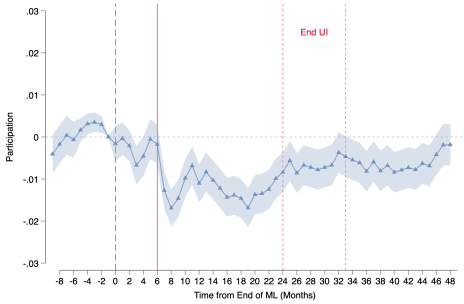


Employment

(c) Replacement Rate Reform

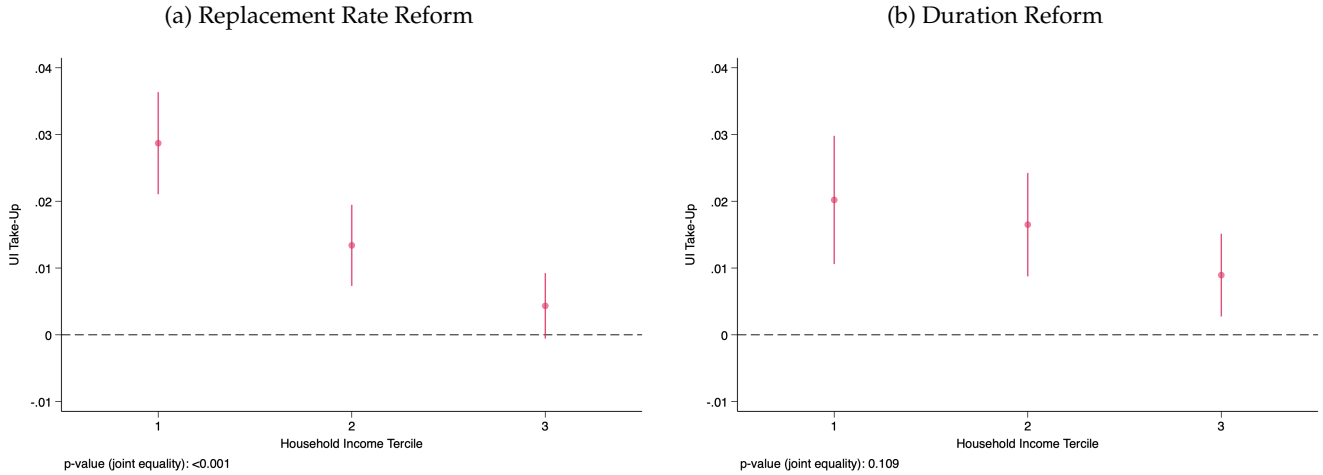


(d) Duration Reform



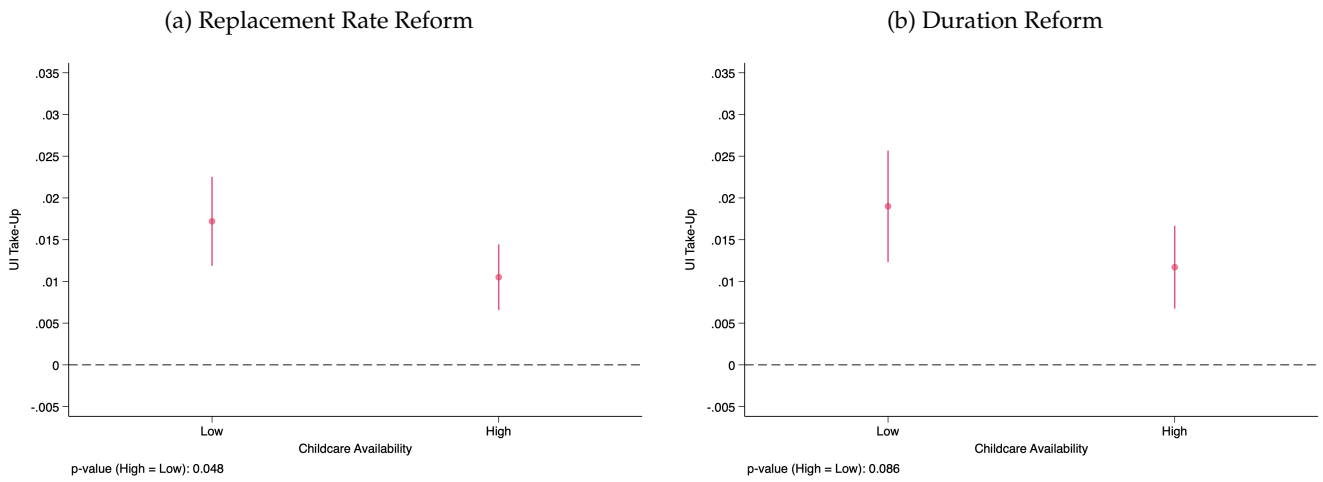
Notes: The figures report the estimates of β_k coefficients from specification 1 on gross labor earnings in levels, normalized in the following way: $\frac{\hat{\beta}_k}{E\left[\tilde{Y}_{ik}|k,T=1\right]}$, and on the probability of working (Panels (c) and (d)). The normalization allows to interpret the coefficients as the percentage change in the earnings at time t for mothers exposed to the more generous UI regime. Shaded areas correspond to 95% confidence intervals. The red solid vertical line corresponds to the time of exhaustion of parental leave benefits if taken at $t = 0$. The "End UI" range characterizes the relative time periods that correspond to the exhaustion of UI benefits if mothers take up UI at any point during the "choice period". Panel (a) reports the results for the replacement rate reform, Panel (b) for the duration reform.

Figure 5: The Role of Liquidity Constraints



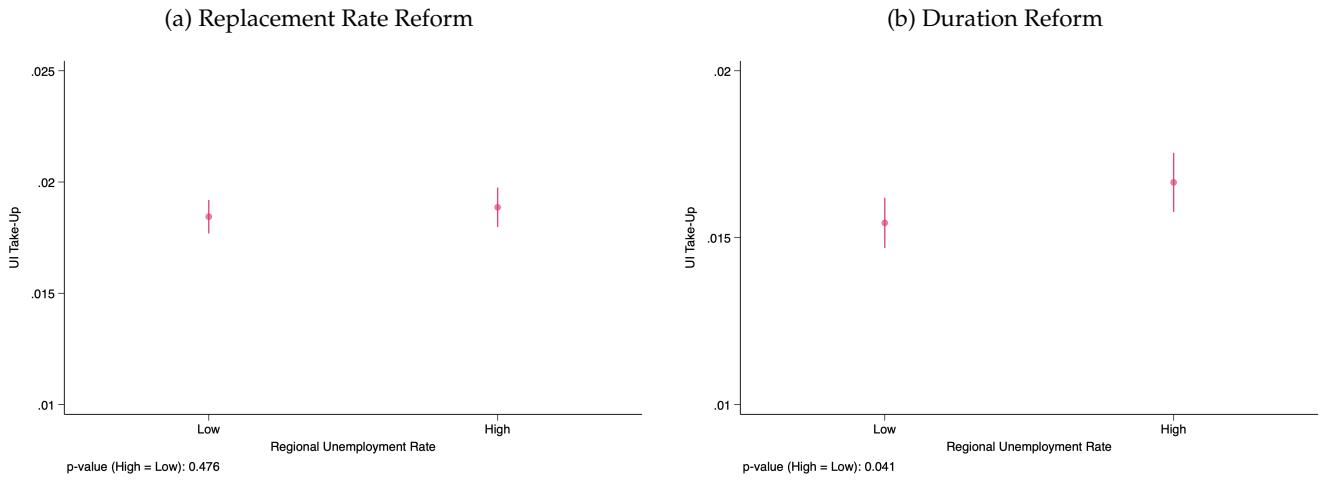
Notes: The figure shows the effect of increasing the generosity of unemployment insurance on the take-up of UI in the four years after the end of ML by tercile of household income. The graphs report the estimates of the compact version of specification 1, that collapses the relative time dummies into a single indicator for the post-end of ML period, on the probability of taking up UI after the end of compulsory maternity leave by tercile of household income. Panel (a) reports the results for the replacement rate reform, Panel (b) for the duration reform.

Figure 6: The Role of Childcare Availability



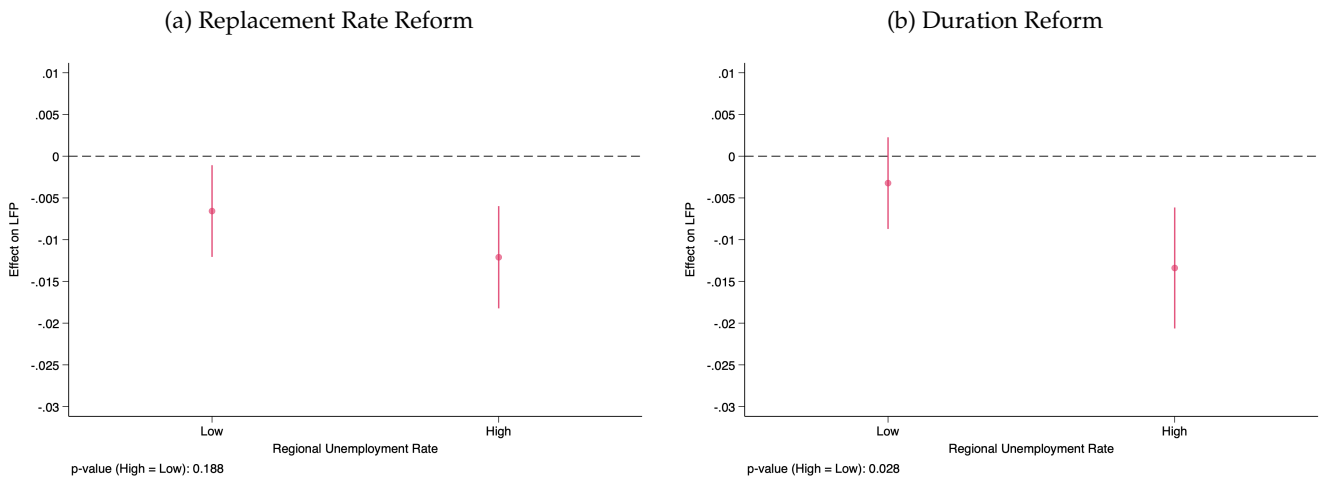
Notes: The figure shows the effect of increasing the generosity of unemployment insurance on the take-up of UI in the 4 years after the end of ML by availability of formal childcare for children aged 0-2 year-old. Availability of formal childcare is based on data from ISTAT and measured as the percentage of 0–2 year-old children that both public and private nurseries can accommodate, by region. Regions with availability of childcare above the median are categorized as *High* childcare regions and regions with availability of childcare below the median are categorized as *Low*. The graphs report the estimates of the compact version of specification 1 that collapses the relative time dummies into a single indicator for the post-end of ML period, on the probability of taking up UI. Panel (a) reports the results for the replacement rate reform, Panel (b) for the duration reform.

Figure 7: The Role of Regional Unemployment Rate: UI Take-Up



Notes: The figure shows the effect of increasing the generosity of unemployment insurance on the take-up of UI in the 4 years after the end of ML by levels of unemployment rate in the region of residence. Regions with levels of unemployment rate above the median are categorized as *High* and regions with levels of unemployment rate below the median are categorized as *Low*. The data on regional unemployment rate are taken from ISTAT. The graphs report the estimates of the compact version of specification 1, that collapses the relative time dummies into a single indicator for the post-end of ML period, on the probability of taking up UI. Panel (a) reports the results for the replacement rate reform, Panel (b) for the duration reform.

Figure 8: The Role of Regional Unemployment Rate: Labor Market Outcomes



Notes: The figure shows the effect of increasing the generosity of unemployment insurance on labor force participation 4 years after the end of ML by levels of unemployment rate in the region of residence. Regions with levels of unemployment rate above the median are categorized as *High* and regions with levels of unemployment rate below the median are categorized as *Low*. The data on regional unemployment rate are taken from ISTAT. The graphs report the estimates of the compact version of specification 1, that collapses the relative time dummies into a single indicator for the post-end of ML period. Panel (a) reports the results for the replacement rate reform, Panel (b) for the duration reform.

Tables

Table 1: Summary Statistics for the Full Sample of Mothers

	(1)
Age	33.51
	(5.012)
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	0.39
	(0.488)
N	641607

Notes: The table reports summary statistics for the full sample of mothers giving birth for the first time between 2012 and 2016. All variables are measured the month before the start of compulsory maternity leave. Monetary quantities are expressed in 2010 prices.

Table 2: Responses to Changes in UI Benefits: Replacement Rate Reform

Months from End of ML	(1) **UI**	(2) **PL**	(3) **Work**
0	0.0054***	-0.0041	-0.0017
	(0.0010)	(0.0028)	(0.0028)
5	0.0106***	-0.0116***	0.0011
	(0.0014)	(0.0028)	(0.0030)
9	0.0172***	0.0020	-0.0086***
	(0.0019)	(0.0013)	(0.0027)
Control mean (0-9 months)	0.148	0.782	0.502
N	2,061,605	2,061,605	2,061,605

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 β_k coefficients from specification 1 for $k = 0$, $k = 5$ and $k = 9$. The control group mean shows the average rate for the control cohort over months 0-9. Standard errors clustered at the individual level are reported in parentheses.

Table 3: Responses to Changes in UI Benefits: Duration Reform

Months from End of ML	(1) **UI**	(2) **PL**	(3) **Work**
0	0.0083***	-0.0045	-0.0016
	(0.0016)	(0.0033)	(0.0031)
5	0.0142***	-0.0072**	-0.0005
	(0.0020)	(0.0031)	(0.0036)
9	0.0185***	0.0027*	-0.0146***
	(0.0025)	(0.0016)	(0.0032)
Control mean (0-9 months)	0.187	0.750	0.503
N	1,509,700	1,509,700	1,509,700

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 β_k coefficients from specification 1 for $k = 0$, $k = 5$ and $k = 9$. The control group mean shows the average rate for the control cohort over months 0-9. Standard errors clustered at the individual level are reported in parentheses. N reports the number of person × month observations

Table 4: Effects on Labor Market Outcomes: Replacement Rate Reform

Months from End of ML	(1) Gross Labor Earnings	(2) Share Working	(3) Days Worked	(4) Daily Wage Rate
12	-9.932**	-0.0077***	0.0279	-1.415***
	(4.748)	(0.0028)	(0.0545)	(0.538)
24	-14.02***	-0.0139***	-0.0425	-0.931**
	(4.853)	(0.0029)	(0.0585)	(0.410)
48	-17.97***	-0.0084**	-0.0988*	-0.252
	(4.997)	(0.0029)	(0.0530)	(0.365)
N	125,469			

Notes: The table shows the estimated effects of increasing unemployment insurance generosity on labor market outcomes. Specifically, it reports the estimates of β_k coefficients from specification 1 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 5: Effects on Labor Market Outcomes: Duration Reform

Months from End of ML	(1) Gross Labor Earnings	(2) Share Working	(3) Days Worked	(4) Daily Wage Rate
12	-5.590	-0.011***	0.0907	-0.181
	(5.651)	(0.0033)	(0.0659)	(0.507)
24	-21.37***	-0.008***	-0.0331	-1.409***
	(5.678)	(0.0034)	(0.0722)	(0.432)
48	-6.417	-0.0018	0.0607	0.493
	(5.795)	(0.0034)	(0.0677)	(0.428)
N	91,253			

Notes: The table shows the estimated effects of increasing unemployment insurance generosity on labor market outcomes. Specifically, it reports the estimates of β_k coefficients from specification 1 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: Compliers Characteristics

	Replacement Rate			Duration		
	AT	C	NT	AT	C	NT
Variable	Mean/SE	Mean/SE	Mean/SE	Mean/SE	Mean/SE	Mean/SE
Share	0.148	0.0165	0.835	0.187	0.0182	0.795
Monthly Wage	1027.102 (0.255)	1181.519 (2.915)	1496.661 (0.055)	1015.107 (0.210)	1210.359 (2.769)	1516.745 (0.055)
Monthly Earnings	978.738 (0.445)	1143.115 (5.040)	1511.731 (0.088)	1015.337 (0.368)	1289.723 (4.910)	1564.200 (0.091)
Age	34.100 (0.002)	33.874 (0.020)	35.725 (0.000)	33.771 (0.002)	33.872 (0.019)	35.834 (0.000)
Blue Collar	0.440 (0.000)	0.530 (0.003)	0.293 (0.000)	0.452 (0.000)	0.495 (0.003)	0.280 (0.000)
Full Time	0.445 (0.000)	0.564 (0.003)	0.554 (0.000)	0.403 (0.000)	0.727 (0.003)	0.542 (0.000)

Notes: This table reports estimated characteristics of always-takers (AT), compliers (C), and never-takers (NT) for the replacement rate and duration reforms. Always-takers are mothers who would take UI under both the low- and high-generosity regimes; never-takers are mothers who would not take UI under either regime; compliers are mothers induced to take UI by the reform. Group shares and characteristics are estimated using the decomposition procedure described in Appendix B. Bootstrapped standard errors, reported in parentheses are obtained from 1,500 resamples; see Appendix B for details.

Table 7: Decomposition of Marginal Benefit

renewcommand11.4

	Marginal Benefit	Interpretation
Replacement Rate Reform	0.42	Aggregate valuation: liquidity + present bias
Duration Reform	0.35	Aggregate valuation: baseline
Low Unemployment	0.21	Bounds inattention to labor market risk
High Childcare	0.19	Bounds childcare scarcity constraint

Notes: This table reports marginal benefit (MB) estimates calculated using equation [quation 3 from Section 5. The first two rows report aggregate MB estimates for the replacement rate and duration reforms using the full sample. The bottom two rows report MB estimates for the duration reform in subsamples designed to proxy for lower-friction environments: regions with below-median unemployment rates and regions with above-median childcare availability. Regional measures are constructed using ISTAT data. Unemployment rates are measured at the NUTS2 (regional) level; childcare availability is measured as the share of children aged 0-2 that public and private nurseries can accommodate in each region. These subsample estimates should be interpreted as descriptive decompositions rather than causal effects, since regional characteristics are not randomly assigned. The estimates are best interpreted as suggestive lower bounds on the welfare-relevant component of the aggregate MB.

Table 8: Construction of The MVPF

	Replacement Rate Reform	Duration Reform
Costs		
Δ UI Spending (cost)	291.96	670.56
Δ PL Spending (savings)	−43.17	−20.00
Δ Tax Revenue Loss (cost)	162.85	218.46
Total Net Costs	**411.64**	**869.02**
1+FE	**1.41**	**1.29**
Panel A: Baseline		
MB	0.42	0.35
1+MB	1.42	1.35
MVPF = (1+MB)/(1+FE)	**1.01**	**1.05**
Panel B: Conservative MB		
$MB_{\text{low unemployment}}$	n.a.	0.21
$1+MB_{\text{low unemployment}}$	n.a.	1.21
MVPF	n.a.	**0.94**
$MB_{\text{high childcare}}$	n.a.	0.19
$1+MB_{\text{high childcare}}$	n.a.	1.19
MVPF	n.a.	**0.92**
Panel C: Extended (+ Discrimination)		
Discrimination FE (per EUR transfer)	n.a.	0.21
1+FE (extended)	1.41	1.50
MVPF (aggregate MB)	**1.01**	**0.90**
MVPF ($MB_{\text{low unemployment}}$)	n.a.	**0.81**
MVPF ($MB_{\text{high childcare}}$)	n.a.	**0.79**

Notes: The table reports the marginal value of public funds (MVPF) for the two reforms under three scenarios. All monetary values are expressed in 2010 euros. The Costs block reports per-mother changes in government expenditure and revenue over the four years following childbirth, estimated from the reduced-form specifications in Section [sec:Empirical-Strategy]. PL Spending savings (negative) reduce net government costs; Tax Revenue Loss (positive) raises them. 1+FE denotes one plus the fiscal externality per euro of UI transfer. Panel A uses aggregate marginal benefit estimates from Section 5. Panel B uses conservative MB estimates from Section 6.4 for subsamples of mothers least affected by behavioral frictions or structural constraints. Panel C incorporates the statistical discrimination externality estimated in Appendix C. The discrimination externality is not estimated for the replacement rate reform because Carta et al. (2024) study only the duration reform's effects on firm hiring behavior.

Appendix

A Derivation of Marginal Benefit

This appendix provides a detailed derivation of the marginal benefit (MB) formula in Section 5.

A.1 Setup and Definitions

A mother chooses between two options after mandatory maternity leave ends:

1. Parental Leave (PL): Return to work with job protection, receive baseline benefits B
2. Unemployment Insurance (UI): Take UI benefits and forgo job protection, receive benefits $B + b$

Let $p \in \{0, 1\}$ denotes the mother's choice, where $p = 1$ indicates taking up UI after birth and giving up her job (forgoing job protection) and $p = 0$ indicates taking up PL and remaining employed.

The mother's utility is:

$$V = u(c) - 1\{p = 1\}\phi(\theta)$$

where:

- $u()$is strictly increasing and concave
- $c = y(p) + B + 1\{p = 1\}b$ is the budget constraint
- $y(p)$ is income, which depends on choice p, because losing job protection affects future earnings
- B is baseline benefits, received under both option
- b is the additional UI benefit (relative to baseline)
- $\phi(\theta)$ is the net cost associated with giving up the job and losing job protection (the cost of searching for a new job, savings from not paying for childcare, preferences for staying home, and other factors) which depends on the mother's type θ, with $\theta \sim F(\theta)$.

Decision rule Let $V(p)$ denote the indirect utility function. A mother decides to take up UI and give up her job if:

$$V(1) - V(0) \geq \phi(\theta)$$

This is equivalent to a threshold rule where a mother decides to leave her job if and only if $\phi(\theta) \leq \bar{\phi}(b)$.

Define $\Phi(b)$ as the share of mothers who take up PL (i.e. do **NOT** leave their job).

$$\Phi(b) = Pr[\phi(\theta) > \bar{\phi}(b)] = 1 - F(\bar{\phi}(b))$$

This measures the labor force participation rate in the sense of mothers who return to their pre-birth jobs rather than taking UI.

A.2 Derivation of Marginal Benefit

The marginal benefit measures the welfare gain to a mother from an additional euro of UI benefits, normalized by the marginal utility of consumption in the absence of the benefit increase.

Identification of marginal utility For the marginal mother (indifferent between UI and PL), the threshold condition is $\bar{\phi}(b) = V(1) - V(0)$. Taking the derivative of the decision rule with respect to b:

$$\frac{\delta \bar{\phi}}{\delta b} = V^{'}(1) = u^{'}(c(1))$$

since $\frac{\delta c(1)}{\delta b} = 1$ and $\frac{\delta c(0)}{\delta b} = 0$. The participation rate $\Phi(b)$ responds to the benefit changes according to:

$$\frac{d\Phi(b)}{db} = -f(\bar{\phi})\frac{\delta \bar{\phi}}{\delta b} = -f(\bar{\phi})u^{'}(c(1))$$

where $f(\cdot)$ is the density of the cost distribution. Rearranging yields

$$u^{'}(c(1)) = \frac{\mid \frac{d\Phi}{db} \mid}{f(\bar{\phi})}$$

The marginal utility of consumption in the UI state is identified by the participation response to benefits, scaled by the density of the cost distribution at the threshold.

Cross-regime comparison To recover the welfare-relevant MB, I exploit variation in benefit generosity across the two regimes induced by the reforms. Let b^{low} denote the pre-reform benefit level and b^{high} the post-reform benefit level, with $b^{high} > b^{low}$. The identification result applies in each regime:

$$u^{'}(c^{s}(1)) = \frac{|\frac{d\Phi^{s}}{db}|}{f(\bar{\phi}^{s})}, s \in \{low, high\}$$

The pre-reform regime provides the counterfactual marginal utility, the marginal utility a mother would have in the absence of the additional UI generosity introduced by the reform. The post-reform regime gives the marginal utility under the more generous policy. The relevant MB compares these two:

$$MB = \frac{u^{'}(c^{low}(1)) - u^{'}(c^{high}(1))}{u^{'}(c^{high}(1))} = \frac{u^{'}(c^{low}(1))}{u^{'}(c^{high}(1))} - 1$$

This expression compares the marginal utility of consumption in the counterfactual UI state absent the reform to the marginal utility of consumption in the UI state after the reform. For mothers responding to the reform on the UI margin, the relevant counterfactual is therefore not taking parental leave, but taking UI under the less generous pre-reform regime. This is the margin along which the reform changes resources. The sign of the expression follows directly from concavity of utility. Since the reform increases UI generosity, consumption in the UI state is higher under the post-reform regime than under the pre-reform regime. Because marginal utility decreases with consumption, it follows that marginal utility is higher in the low-benefit regime than in the high-benefit regime. Therefore, the marginal benefit is positive.

Expressing MB in terms of observable moments Define the elasticity of participation with respect to benefits as:

$$\varepsilon(\Phi^{s}, b^{s}) = \frac{b^{s}}{\Phi^{s}} \frac{d\Phi^{s}}{db}$$

so that $\frac{d\Phi^{s}}{db} = \frac{\varepsilon^{s}\Phi^{s}}{b^{s}}$. Since Φ^{s} denotes the share of mothers returning to work (not forgoing job protection), an increase in UI generosity reduces Φ^{s} , so $\varepsilon^{s} < 0$. Using the absolute value, the marginal utility of consumption in the UI state can be written as:

$$u^{'}(c^{s}(1)) = \frac{\mid \varepsilon^{s} \mid \Phi^{s}}{b^{s} f(\bar{\phi}^{s})}$$

The MB can therefore be written as:

$$MB = \frac{u^{'}(c^{low}(1))}{u^{'}(c^{high}(1))} - 1 = \frac{\frac{|\varepsilon^{low}|\Phi^{low}}{b^{low} f(\bar{\phi}^{low})}}{\frac{|\varepsilon^{high}|\Phi^{high}}{b^{high} f(\bar{\phi}^{high})}} - 1$$

This can be written as

$$MB = \frac{u^{'}(c^{low}(1))}{u^{'}(c^{high}(1))} - 1 = \kappa \frac{b^{high}}{b^{low}} \frac{\Phi^{low}}{\Phi^{high}} - 1 \tag{4}$$

where:

$$\kappa = \frac{\frac{|\varepsilon(\Phi^{low}, b^{low})|}{f(\bar{\phi}^{low})}}{\frac{|\varepsilon(\Phi^{high}, b^{high})|}{f(\bar{\phi}^{high})}}$$

captures the relative density-adjusted semi-elasticity across regimes, normalized by the benefit levels.

Empirical Implementation: Setting $\kappa = 1$ For the empirical implementation, in the spirit of Fadlon and Nielsen (2019) I set $\kappa = 1$. The assumption $\kappa = 1$ is equivalent to requiring that the density-weighted semi-elasticity $|\varepsilon^{s}|/f(\phi^{s})$ is approximately constant across the two regimes. Two conditions make this plausible in the present setting. First, the reforms are moderate in size relative to the support of ϕ: the implied threshold shift moves the participation rate Φ by roughly one percentage point on a baseline of 65%, suggesting that ϕ_{low} and ϕ_{high} lie close to one another in the support of F. Under local smoothness of f, this implies $f(\phi_{low}) \approx f(\phi_{high})$. Second, the elasticity of participation with respect to benefits is similar in magnitude across the two reforms (see Table A2), suggesting that $|\varepsilon^{low}| \approx |\varepsilon^{high}|$as well. As shown in Appendix A Fadlon and Nielsen (2019), these conditions correspond to a second-order approximation to the participation function and break down only when the two thresholds are far apart relative to the curvature of F.

Under this assumption, the MB simplifies to:

$$MB \simeq \frac{b^{high}}{b^{low}} \frac{\Phi^{low}}{\Phi^{high}} - 1$$

which is the formula used in Section 5.

B Complier Characteristics

Consider a binary variable Z in $\{0, 1\}$ that captures whether mothers are eligible for the more generous UI regime more generous UI regime. Z effectively shifts a component of the mother's outside option $b + Z\Delta b$, by either increasing the UI replacement rate or extending UI duration. Let $D \in 0, 1$ indicate whether a mother takes up UI, regardless of treatment status. Let $D_0$and D_1denote the potential values that$D$ takes under $Z = 0$ and $Z = 1$ respectively. We can characterize three groups of mothers by their potential outcomes: always-takers (AT) with potential outcomes $(D_0 = 1, D_1 = 1)$ and share π_{AT}; never-takers (NT) with potential outcomes $(D_0 = 0, D_1 = 0)$ and share π_{NT}; and compliers (C) with potential outcomes $(D_0 = 0, D_1 = 1)$ and share π_C.

In my setting, I compare eligible (c_1) and ineligible (c_0) cohorts based on their date of end of compulsory maternity leave, so that $Z = 1$ for cohort c_1 and $Z = 0$ for cohort c_0.

Estimating group shares first estimate the shares of always-takers, compliers, and never-takers by running the following regression:

$$Y_i = \alpha + \gamma T_i + \epsilon_i$$

where $T_i = 1$ 1 if mother i is eligible for the more generous UI regime and Y_i is an indicator equal to 1 if individual i took up UI at any point during the choice period, namely at any $t \in [0, 9]$ months from the end of compulsory maternity leave. The shares are then identified as $\hat{\pi}_C = \hat{\gamma}$, $\hat{\pi}_{AT} = \hat{\alpha}$, and $\hat{\pi}_{NT} = 1 - \hat{\alpha} - \hat{\gamma}$.

Estimating group characteristics I next estimate the expected value of a characteristic x for each group. Estimating the expected value for never-takers is straightforward. Under the standard monotonicity assumption from the instrumental variables literature, $D_1 - D_0 \geq 0$, all mothers in cohort c_1 who do not take up UI during the choice period are never-takers. Their characteristics can therefore be estimated by the sample mean:

$$\hat{E}_{NT}[x] = \frac{1}{N_{c_1}^{nt}} \sum_{i \in c_1} x_i \mathbf{1}(D_i = 0)$$

Estimating the expected value of a characteristic for always-takers and compliers is more challenging. The expected value of x for mothers in cohort c_1 who take up UI is a weighted average of the expected values for compliers and always-takers, with weights equal to their respective

population shares. The expected value of x for compliers is therefore:

$$\hat{E}_C[x] = E[x \mid D_0 = 0, D_1 = 1, c_1] = \frac{\pi_C + \pi_{AT}}{\pi_C} E[x \mid D_1 = 1, c_1] - \frac{\pi_{AT}}{\pi_C} E[x \mid D_0 = 1, c_1]$$

The first term on the right-hand side, $E[x \mid D_1 = 1, c_1]$, is the expected value of x for all mothers taking up UI after the reform, estimated by the sample mean:

$$\frac{1}{N^{at}_{c_1}} \sum_{i \in c_1} x_i \mathbf{1}(D_i = 1)$$

The second term, $E[x \mid D_0 = 1, c_1]$, is more difficult to estimate directly, since we cannot observe whether a mother who takes up UI after the reform would have done so in the absence of the reform. Under monotonicity, however, mothers who take up UI before the reform would also take up UI after the reform. Therefore, if trends in x are parallel across cohorts and Z is independent of both D and x, we have that:

$$E[x \mid D_0 = 1, c_1] = E[x \mid D_0 = 1, c_0]$$

which can be estimated by the sample mean among always-takers in the ineligible cohort:

$$\frac{1}{N^{at}_{c_0}} \sum_{i \in c_0} x_i \mathbf{1}(D_i = 1)$$

This approach follows Abadie (2003) and relies on the independence of Z from both D and x, which is guaranteed by the quasi-random assignment of reform eligibility based on the date of end of maternity leave. The resulting complier characteristics are reported in Table 6.

Standard Errors Standard errors for the estimated shares and complier characteristics are obtained via bootstrap. Because complier characteristics are nonlinear functions of estimated group shares and subsample means, analytical standard errors are not available in closed form. I therefore draw B = 1,500 bootstrap samples of size 500,000 with replacement from the analysis dataset. For each bootstrap sample, I re-estimate the group shares from the regression of UI take-up on the post and treatment indicators and their interaction, and compute the complier mean for each characteristic following the recovery formula above. The bootstrap standard errors reported in Table 6 are the standard deviations of the resulting empirical distributions across the 1,500 iterations.

C Quantifying the Statistical Discrimination Externality of Job Protection

This appendix details the calculation of the statistical discrimination externality reported in Section 7.3. The exercise combines firm-level estimates from Carta et al. (2024) with data on the Italian fixed-term–permanent earnings gap to quantify the fiscal externality generated when mothers' UI take-up induces firms to shift hiring of young women toward lower-quality contracts. All monetary values are expressed in 2010 prices using ISTAT consumer price indices.

C.1 Inputs

The calculation combines four empirical inputs:

1. **Firm-level hiring response**. Carta et al. (2024) estimate that firms exposed to the duration reform increased net hiring of young women by 6.1% relative to the pre-reform baseline (Table A.1, column 6).

2. **Earnings gap between fixed-term and permanent contracts.** I use the 21.5% hourly wage differential between permanent and fixed-term contracts documented by ISTAT (2016) using 2014 administrative data.

3. **Contract composition of new female hires.** Administrative data from the Italian Ministry of Labor (Ministero del Lavoro e delle Politiche Sociali, 2015) show that in April 2014, 69% of new female hires were on fixed-term contracts.

4. **Reference wage.** I use the ISTAT total hourly wage across all dependent workers in 2014 of €14.1 per hour (ISTAT, 2016). Assuming 1,800 working hours per year , a figure consistent with the mix of full-time and part-time arrangements observed in the affected population, the reference wage is €25,380 per year.

5. **Effective tax rate.** I apply an effective tax rate of XXX32% on foregone earnings, reflecting average Italian income tax plus employee social security contributions for workers in this earnings range.

C.2 Assumptions

One assumption is required to map these inputs into a per-euro externality.

Persistence of earnings penalty I assume the earnings penalty persists for two years following hire. This horizon reflects two offsetting considerations. On the one hand, the median duration of Italian fixed-term contracts is shorter than two years, suggesting some affected workers transition to permanent contracts within this window. On the other hand, scarring effects on earnings can persist beyond the initial contract through reduced firm-specific human capital accumulation and weaker labor market attachment. The two-year horizon is conservative relative to the four-year window over which UI transfers are accumulated in the denominator (see Section 5). The share of marginal hires on fixed-term contracts is not assumed but anchored directly to MLPS-SISCO data (Ministero del Lavoro e delle Politiche Sociali, 2015). I take the natural contract mix among new female hires (69% fixed-term) as the baseline contract composition of marginal hires induced by the reform. This is a lower bound on the share of marginal hires on fixed-term contracts: the statistical discrimination mechanism documented by Carta et al. (2024) implies that firms responding to higher anticipated maternal quit rates concentrate more heavily on the fixed-term margin than the baseline rate. Using the natural rate as the baseline therefore understates the discrimination response.

C.3 Calculation

Step 1: Annual earnings loss Applying the 21.5% earnings gap to the reference wage:

$$\textit{Annual earnings loss} = 0.215 \times €25,380 = €5,457$$

Step 2: Annual fiscal loss Applying the 32% tax wedge to the earnings loss:

$$\textit{Annual fiscal loss} = 0.32 \times €5,457 = €1,746$$

Step 3: Cumulative fiscal loss over the persistence horizon Cumulating the annual fiscal loss over two years, using a monthly discount factor of 0.995 (annual factor ≈ 0.94):

$$\textit{Cumulative fiscal loss} = €1,746 \times (1 + 0.94) = €3,388$$

Step 4: Externality per euro of UI transfer I scale by the share of marginal hires on fixed-term contracts (69%) and the firm-level hiring response (6.1%), then normalize by the cumulative UI transfer per quitting mother (€671):

$$\textit{Externality per €}\ UI = €6710.69 \times 0.061 \times €3,388 = €671€142.6 \approx 0.21$$

C.4 Sensitivity and Robustness

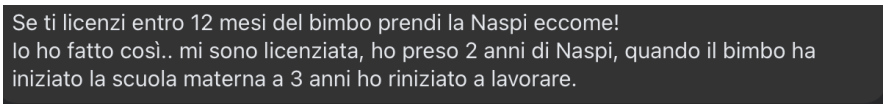
The estimate of €0.21 per euro of UI transfer is robust to alternative reasonable assumptions about the persistence horizon. Under a one-year persistence assumption, the externality is €0.11; under a four-year persistence assumption matching the UI accounting window, it is €0.39. The two-year baseline is conservative relative to the upper end of this range. As a further robustness check, using the regression-adjusted earnings gap of 16% reported elsewhere in ISTAT (2019), which controls for worker and job characteristics, the central estimate falls to €0.16 per euro of UI transfer. The baseline estimate of €0.21 is likely a lower bound on the true externality: the calculation ignores additional fiscal costs from higher unemployment-spell frequency among young women on fixed-term contracts, and assumes a persistence horizon shorter than the four-year UI accounting window.

D Additional Figures

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

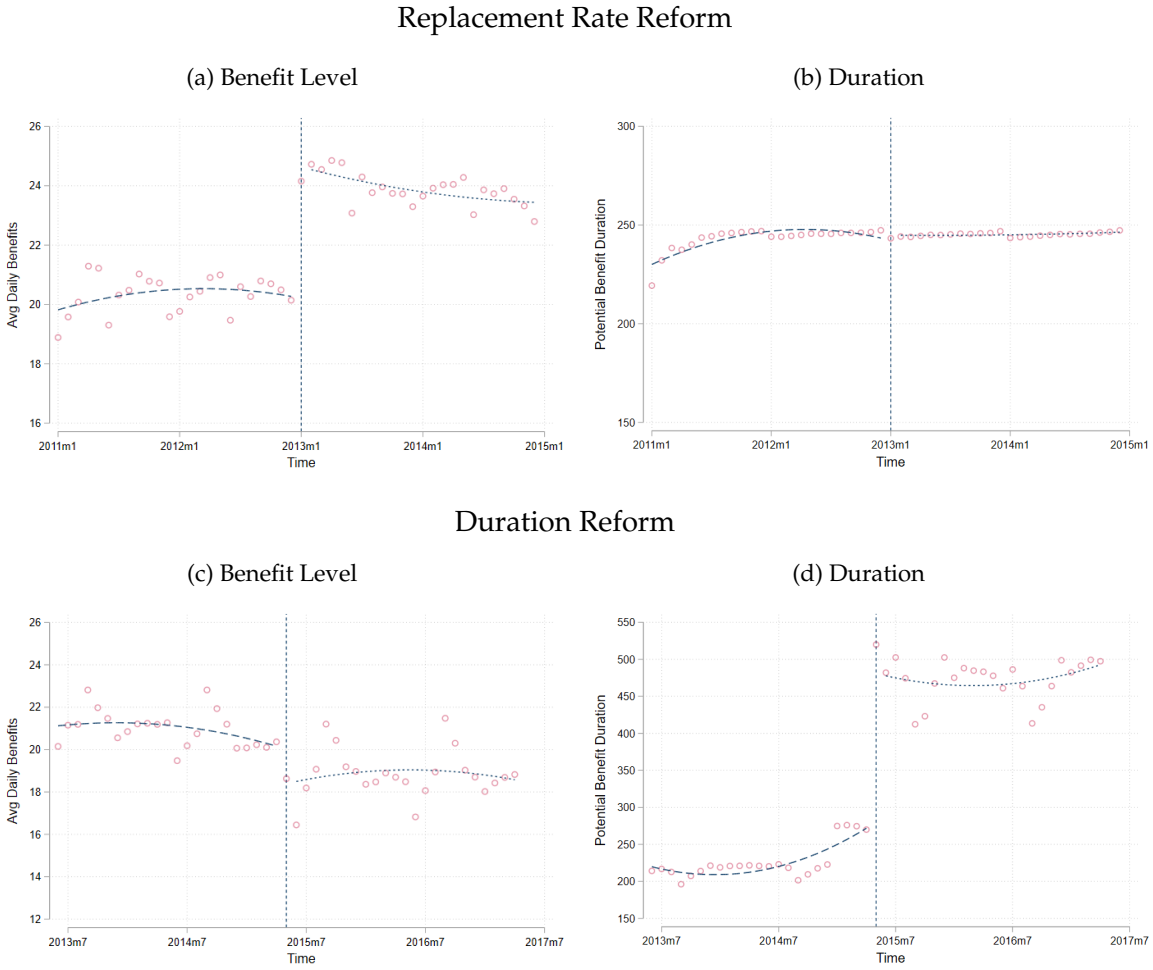
(a) Commercial service marketing resignations and UI as PL

(b) Example of UI used as PL



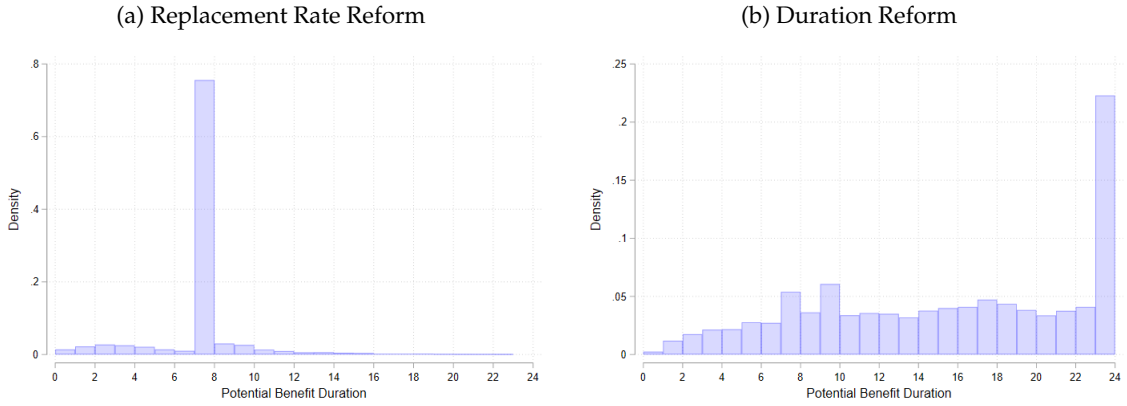
Notes: Panel (a) shows an example of a website advertising the possibility of using unemployment insurance as an alternative to parental leave benefits. Panel (b) shows a comment in a popular online forum for mothers and expectant mothers describing this use of UI. The translation is: "If you resign within the child's first 12 months you absolutely can get NASpI! That's what I did — I resigned, took two years of NASpI, and went back to work when my child started preschool at age three."

Figure A2: Changes in Unemployment Insurance Generosity



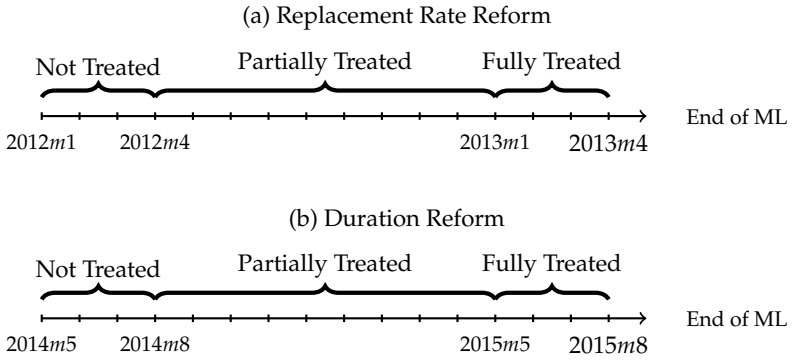
Notes: The graphs show the average daily unemployment insurance benefits and the average potential benefit duration (in days) by month-of-start of unemployment spell bin for women younger than 50 years old with a permanent contract. Panel (a) and (b) show the effects of the introduction of the replacement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

Figure A3: Distribution of UI Potential Benefit Duration After the Reforms



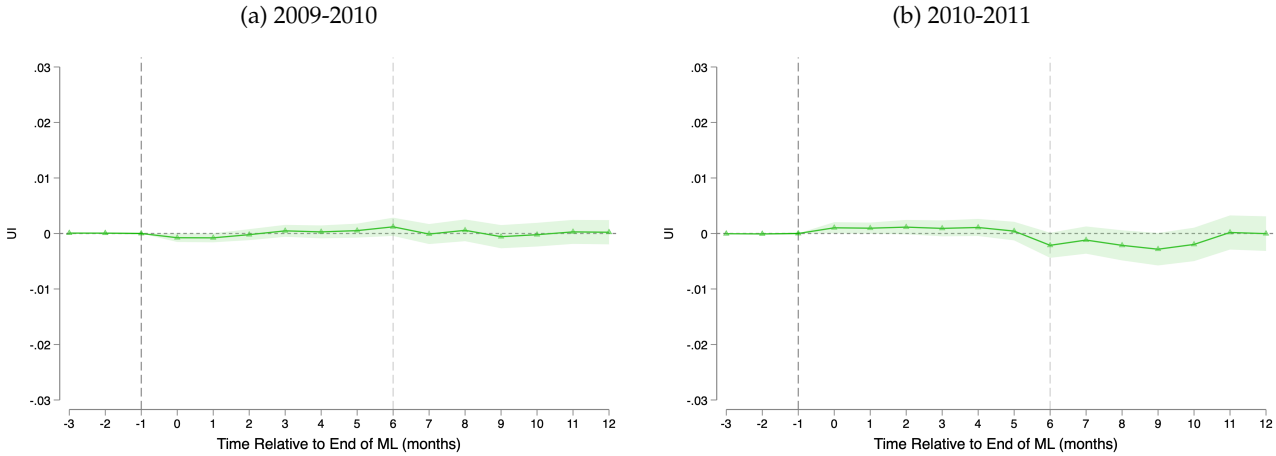
Notes: The figure shows the distribution of potential benefit duration of unemployment insurance after the replacement rate reform (Panel (a)) and after the duration reform (Panel (b)).

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



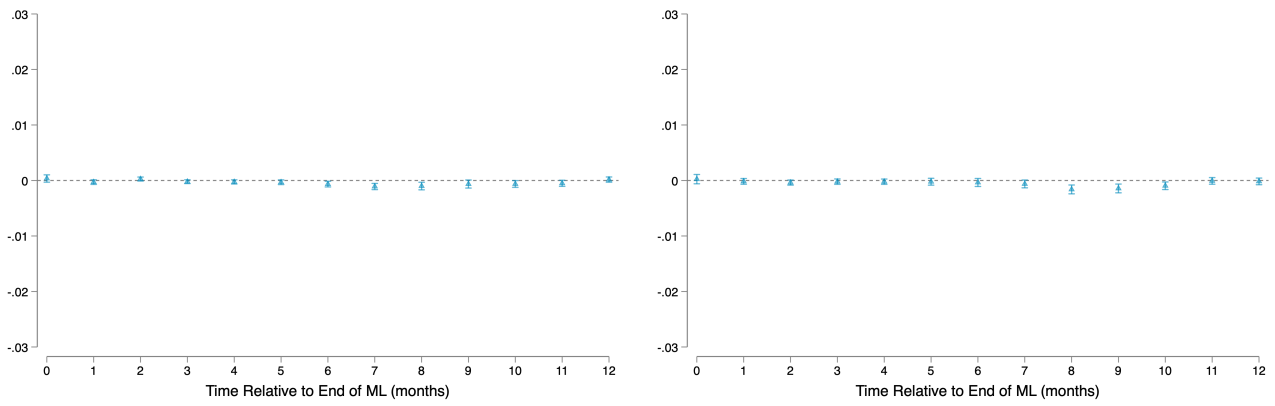
Notes: The figure illustrates for which dates the mothers were eligible for different unemployment insurance regimes, depending on the end of their maternity leave. Panel (a) shows the dates for the replacement rate reform. Panel (b) shows the dates for the duration reform.

Figure A5: Placebo Specifications



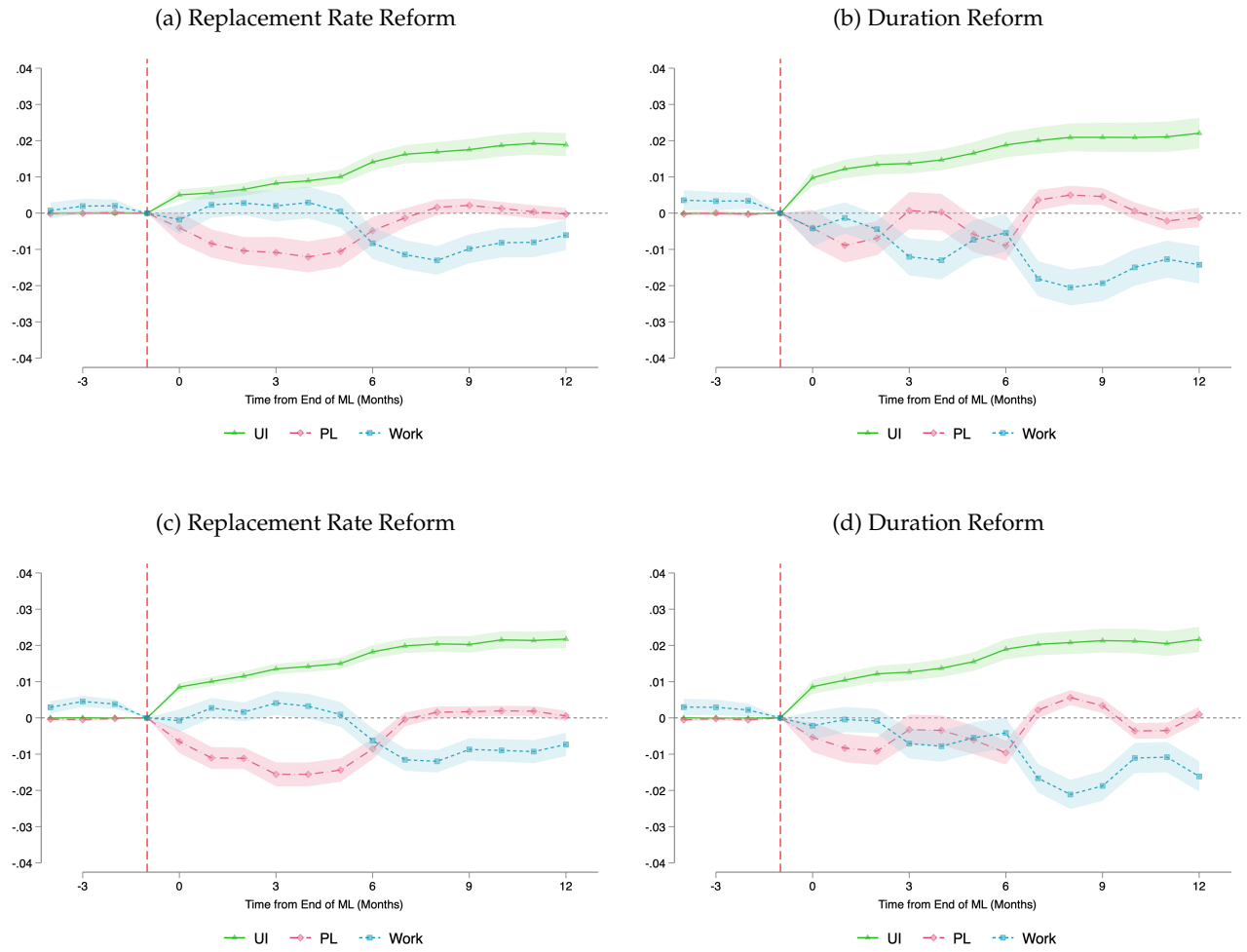
Notes: The figures report difference-in-differences estimates of the coefficients β_k from 1 where the dependent variable is the probability that a mother is on unemployment insurance at relative time t (where $t = 0$ corresponds to the time of end of compulsory maternity leave) for different placebo cohorts. The figure reports the 95% confidence intervals. Standard errors are clustered at the individual level. Panel (a) compares cohorts of mothers who gave birth in 2010 vs 2009. Panel (b) compares cohorts of mothers who gave birth in 2011 vs 2010.

Figure A6: Effect of the Reforms on Involuntary Separations



Notes: The figures report difference-in-differences estimates of the coefficients β_k from 1 where the dependent variable is the probability that a mother involuntary 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 A7: Responses to Changes in UI Benefits: Robustness to Different Definitions of Treatment and Control



Notes: The figures report difference-in-differences estimates of the coefficients β_k from specification 1, estimated on three 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 end of compulsory maternity leave. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the individual level. Panel (a) and (c) report results for the replacement rate reform, while panels (b) and (d) report results for the duration reform. Panels (a) and (b) use narrower definitions of treatment and control groups, while panels (c) and (d) use broader definitions. In panel (a), the treatment group includes mothers whose end of maternity leave falls between January 2013 and April 2013, and the control group includes mothers in the same months of 2012. In panel (b), the treatment group includes mothers whose end of maternity leave falls between May 2015 and August 2015, and the control group includes mothers in the same months of 2014. In panel (c), the treatment group includes mothers whose end of maternity leave falls between January 2013 and July 2013, and the control group includes mothers in the same months of 2012. In panel (d), the treatment group includes mothers whose end of maternity leave falls between May 2015 and November 2015 and the control group includes mothers in the same months of 2014.

E Additional Tables

Table A1: HOUSEHOLD SAMPLE: CHARACTERISTICS

Variable	(1) Not Matched Mean/SE	(2) Matched Mean/SE	T-test Difference (1)-(2)
Age	33.295	33.500	-0.206***
	(0.015)	(0.015)	
Permanent	0.894	0.976	-0.082***
	(0.001)	(0.000)	
Full Time	0.574	0.653	-0.079***
	(0.001)	(0.002)	
White Collar	0.583	0.638	-0.054***
	(0.001)	(0.002)	
Monthly Earnings	1441.232	1661.040	-219.808***
	(2.897)	(3.439)	
Small Firm (<15)	0.473	0.318	0.155***
	(0.001)	(0.002)	
N	354,957	286,650	

Notes: The table reports summary statistics for the sample of mothers in the data who cannot be linked to their partners (Column 1) and the sample of mothers who can be linked to their partners (Column 2), as explained in Section 3.1. Column 3 reports the t-test of the difference between the two groups. All variables are measured the month before the start of compulsory maternity leave. Monetary quantities are expressed in 2010 prices.

Table A2: Parameters for Marginal Benefits Calculations

	Replacement Rate Reform	Duration Reform
Behavioral Response		
Φ^{low}	0.628	0.610
$\Delta\Phi$	-0.0104	-0.0088
Φ^{low}/Φ^{high}	1.017	1.015
Benefit Generosity		
b^{low}	€4,021	€5,252
Δb	€1,613	€1,745
b^{high}/b^{low}	1.40	1.33
Marginal Benefit		
MB	**0.42**	**0.35**

Notes: The table reports the parameters used to calculate marginal benefit estimates using equation 3. Labor force participation rates measure the share of mothers who participate in the labor market, averaged over four years following childbirth. The estimates are from the reduced-form difference-in-differences analysis in Section 3.2. Net present values of benefits are calculated from administrative data on UI payments using a monthly discount rate of 0.95. The marginal benefit calculation assumes $\varkappa = 1$ and uses formula 3. See Appendix A for derivation and discussion of this assumption. All monetary values are in 2010 euros.