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

Valeria Zurla*

July 3, 2026

**Abstract**

This paper studies how mothers value benefit generosity relative to job protection after childbirth. In Italy, mothers may choose between job-protected parental leave with low benefits and unemployment insurance with higher benefits but no job protection. Using reforms to benefit levels and duration, I show that greater generosity induces mothers to trade job protection for unemployment insurance, with persistent employment and earnings losses. These choices reveal substantial value for post-birth transfers. Interpreting them poses a methodological challenge: in parental leave, time at home with an infant is partly an intended welfare benefit rather than only a behavioral cost. The revealed valuation reflects resources during leave, caregiving time, and relief from childcare constraints, weighed against the perceived cost of giving up job protection. Heterogeneity across reforms, household income, and regional characteristics provides evidence on these components and on behavioral frictions that may arise when households make program choices shortly after childbirth. In an MVPF framework, both reforms are approximately welfare-neutral under aggregate valuations; conservative benchmarks and statistical-discrimination costs lower the welfare value of the duration reform. The results show that post-birth transfers have meaningful value, but that their welfare gains depend on whether benefit generosity is paired with job protection and with measures addressing the liquidity, childcare, and behavioral frictions shaping mothers' choices.

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

**JEL Codes:** J13, J18, I38, J22

*CSEF, University of Naples Federico II, Population Wellbeing Initiative - UT Austin. Email: valeria.zurla@unina.it. I am enormously grateful to John Friedman, Emily Oster and Neil Thakral for their continued guidance, advice and encouragement throughout this project. This study uses the anonymous data from the Italian Institute of Social Security (INPS). Data access was provided as part of the Visitinps Scholars Initiative. I thank the staff of Direzione Centrale Studi e Ricerche at INPS for their invaluable support with the data and the institutional setting. This project benefited from discussions with Anna Aizer, Marika Cabral, Nathan Hendren, Giulia Giupponi, Andreas Peichl, Marco Stenborg Petterson, Jesse Shapiro, Bryce Steinberg, Matt Weinzierl and seminar participants at Brown, INPS, AIEL, CSEF, Tor Vergata, Stockholm University, Tilburg University, University of Bergen, University of Gothenburg, Bank of Italy, UT Austin, Harvard Policy Impacts Conference, EALE, Brucchi Luchino Labor Economics Workshop, EWMES ASSA, NBER SI Public, IIPF. Financial support from the PSTC International Fellowship, the VisitINPS fellowship and the Policy Impact Early Career grant is gratefully acknowledged. The findings and conclusions expressed here are solely those of the author and do not represent the views of INPS.

# 1 Introduction

Parental leave policies combine three distinct forms of support: income replacement, time away from work, and protection of the employment relationship. Most policy reforms change several of these dimensions together, making it difficult to disentangle how families value each one. Yet this distinction is central to policy design. A mother may value a generous transfer because it smooths consumption, because it makes time with an infant affordable, or because childcare alternatives are unavailable. At the same time, providing benefits without preserving the employment relationship may generate persistent career costs. Understanding how mothers trade off these dimensions is necessary for evaluating both the generosity and the structure of parental-leave systems.

The question matters beyond program design. Parental-leave decisions are a primary determinant of mothers' long-run labor-market attachment, and the cumulative effect of these choices contributes to the persistent gender gaps in earnings and labor force participation that have proven resistant to other policy interventions (Kleven, Landais, and Søgaard, 2019; Kleven et al., 2024). Focusing on what mothers value about parental leave, and when choices may diverge from experienced welfare, is therefore central to designing policies that support both children and women's careers.

This paper studies a setting in which the trade-off across these dimensions is directly observable. After compulsory maternity leave, Italian mothers may use job-protected parental leave, which replaces a relatively small share of earnings, or resign and claim unemployment insurance, which provides higher benefits but requires them to give up job protection. Two national reforms increased unemployment-insurance generosity: one raised benefit levels immediately after the end of compulsory maternity leave, the other extended potential benefit duration. These reforms shifted the relative price of generosity and job protection without changing the statutory parental-leave program, creating quasi-experimental variation across two dimensions of program generosity. Using administrative data from the Italian Social Security Institute on the universe of mothers employed in the private sector before childbirth, I employ a difference-in-differences strategy comparing cohorts of mothers differentially exposed to each reform.

Mothers respond strongly. Greater unemployment-insurance generosity increases unemployment-benefit take-up and produces an approximately offsetting decline in parental-leave use. The resulting substitution is consequential: mothers induced to choose unemployment insurance lose attachment to their pre-birth employer and experience persistent employment and earnings losses after benefits expire. Over the four years following childbirth, marginal mothers forgo approx-

imately €29,000–€35,000 in cumulative earnings, costs that far exceed the value of the additional benefits received. The central empirical fact is therefore stark: some mothers are willing to sacrifice substantial future earnings and job protection for greater resources following childbirth.

What does this choice reveal about welfare? Parental leave differs from standard unemployment-insurance settings because time away from work after childbirth may itself be part of what the policy is designed to provide. Greater benefits can therefore generate welfare value through three channels: they raise resources during a period of high household needs, make caregiving time with an infant financially feasible, and relax constraints created by limited childcare availability. At the same time, observed choices may also reflect wedges between revealed valuation and experienced welfare (Mullainathan, Schwartzstein, and Congdon, 2012; Chetty, 2015). Childbirth is a pivotal moment in the life cycle when mothers may underweight future employment costs, pay limited attention to re-employment risk, or place disproportionate weight on immediately available transfers. The empirical analysis cannot separately quantify each component, but this distinction between welfare-relevant sources of value and behavioral wedges is central to interpreting the revealed marginal benefits.

To quantify what these choices reveal about welfare, I apply a revealed-preference framework developed for the welfare analysis of social insurance (Chetty, 2008; Hendren, 2017; Fadlon and Nielsen, 2019). The framework recovers mothers' marginal benefit of UI generosity from the behavioral responses to the two reforms. I estimate marginal benefits (MBs) of 0.43 for the replacement rate reform and 0.34 for the duration reform. Both are substantial, indicating that mothers place high revealed value on greater generosity during the post-childbirth period. The comparison between the two reforms is also informative because they change distinct margins of generosity: the replacement-rate reform delivers additional resources earlier in the leave spell, while the duration reform extends the period during which benefits make remaining at home financially feasible.

I use the two reforms, together with heterogeneity by household income, childcare availability, and local labor-market conditions, to interpret the mechanisms underlying these revealed valuations. Stronger responses among lower-income households and in regions with limited childcare availability point to liquidity needs and childcare constraints as important welfare-relevant sources of valuation. The comparison between the replacement-rate and duration reforms is informative about the relative role of immediate resources and later support for remaining at home. In contrast, the weak relationship between take-up responses and local re-employment risk, together with stronger responses to immediately available transfers, is consistent with limited attention and present-biased valuation. These patterns motivate reporting both aggregate revealed valua-

tions and conservative scenarios that illustrate how welfare conclusions change when particular constraints or behavioral frictions are less severe.

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 1.03 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, which more than offsets its smaller marginal benefit. Using both reforms is therefore important for comparing how welfare conclusions change when generosity is expanded through higher benefit levels rather than longer potential duration.

Under more conservative MB assumptions for the duration reform, the MVPF falls to 0.92–0.94, slightly below the welfare-neutral threshold. Because the policy margin studied here requires mothers to give up job protection, a full welfare assessment must also consider costs beyond the mothers making the program choice. Using the statistical-discrimination response documented by Carta et al. (2024) for the duration reform, in which firms shift young-women hires from permanent toward fixed-term contracts, I incorporate the fiscal component of this externality into the MVPF calculation. This lowers the MVPF to 0.90 under aggregate MB assumptions and to 0.79–0.81 under conservative MB assumptions. Taken together, the results do not imply that post-birth transfers lack value. Rather, they show that the welfare gains from expanding benefit generosity depend on whether policy also addresses the liquidity constraints, childcare barriers, and behavioral frictions that shape program choice. Expanding generosity through a margin that requires mothers to give up job protection can generate substantial private, fiscal, and broader labor-market costs.

This paper makes three contributions. First, it provides novel revealed-preference evidence on how mothers value benefit generosity relative to job protection, exploiting two reforms to distinguish responses to higher benefit levels from responses to longer benefit duration. Second, it shows that choosing greater generosity without job protection causes large and persistent employment and earnings losses, documenting the long-run consequences of separating income support from employment protection. Third, the paper shows how revealed-preference welfare analysis must be interpreted differently in the parental-leave setting. The estimated marginal benefit is not simply the value of consumption smoothing. It reflects mothers' valuation of post-birth support, including resources during leave, caregiving time with an infant, and relief from childcare

constraints, relative to the perceived cost of giving up job protection. This feature distinguishes parental leave from settings such as unemployment insurance, where induced nonemployment is typically interpreted primarily as a costly behavioral response to transfers. The timing of the decision also matters: program choices are made shortly after childbirth, when households face elevated financial needs, limited childcare options, and potentially high cognitive and informational burdens. These features make it especially important to distinguish welfare-relevant valuations from behavioral wedges. Combining these valuations with fiscal costs and the consequences of statistical discrimination provides new evidence on the welfare effects of expanding parental-leave generosity along different margins.

**Related Literature** This paper contributes to different strands of literature. First, 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 chooses 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 estimate 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. The approach also makes it possible to examine how welfare conclusions change under alternative interpretations of mothers' revealed valuations, including interpretations that allow for childcare constraints, liquidity needs, and behavioral frictions.

Second, the paper 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, childcare availability, and uncertainty about future labor-market prospects are particularly important. The distinction is sharpest relative to unemployment-insurance applications, where induced nonemployment is typically interpreted as moral hazard. It is closer in spirit to disability insurance, where reduced work may be welfare relevant because it accommodates a health limitation. In parental leave, the analogous source of value is different: reduced work may enable caregiving time with an infant and may reflect constraints created by limited childcare availability. Survivor-benefit applications also interpret transfers through household labor supply and income effects, but the insured state remains an adverse event rather than a policy-supported use of time. The parental-leave setting therefore changes the interpretation of revealed valuations: induced nonemployment may be part of the policy's welfare value, rather than only a behavioral response to transfers. 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).

Third, the paper relates to work on behavioral frictions in social insurance and public policy. The structure of the two reforms, one that increases benefits received immediately during the leave spell and one that extends benefits further into the future, provides variation in the timing of transfers. I use this variation, together with heterogeneity by household income and local labor-market conditions, to provide evidence on how liquidity needs, transfer timing, and perceived re-employment risk shape mothers' program choices after childbirth. In this respect, the paper connects to work on benefit timing and consumption smoothing in social insurance (Ganong and Noel, 2019; Gerard and Naritomi, 2021) and to work on the implications of present bias, inattention,

and misinformation for public policy (Mullainathan, Schwartzstein, and Congdon, 2012; Chetty, 2015; Costa-Ramón et al., 2024). Rather than quantifying a behavioral correction to revealed willingness to pay, the analysis provides evidence consistent with behavioral frictions and shows how the welfare interpretation changes when such frictions are more or less salient.

Finally, 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 a revealed-preference estimate of mothers' willingness to pay for parental-leave generosity, compares the welfare implications of expansions along the benefit-level and benefit-duration margins, and uses heterogeneity and reform timing to assess the mechanisms underlying 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

[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.

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 between parents. Each parent can claim up to six months of leave, subject to a household cap of ten months. 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 voluntarily resign between childbirth and the child's first birthday. This is a key exception to the standard UI system, under which workers who resign voluntarily are not ordinarily entitled to UI benefits. For eligible mothers, UI offers higher income replacement than PL and similar or longer potential duration, but it requires separation from the pre-birth employer and therefore entails losing job protection. Only mothers, not fathers, could benefit from this exception during the period studied.

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.[4] 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.

[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.

[3]Source: *INPS*.

[4]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.

The first reform, which I will refer to as the *replacement rate reform* (RR), took effect on January 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.[5] 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.

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

**UI as Alternative to PL** The empirical interpretation relies on mothers perceiving UI as a feasible alternative to PL, rather than as a conventional unemployment program unrelated to post-birth leave. Two concerns are natural. First, stigma or lack of information could attenuate take-up relative to a formal paid-leave expansion of equivalent generosity. Second, job-search requirements could make UI less comparable to paid leave by pushing recipients back into employment.

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 a commercial service explicitly marketing UI as an alternative to PL. Panel (b) illustrates peer-to-peer dissemination from a popular online forum, with mothers describing having used UI as PL and as 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 the estimates. This concern is unlikely to be quantitatively important 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. At the same time, UI is not literally parental leave. The magnitude of substitution observed here is specific to the Italian setting, where UI was visible and accessible as a post-birth option and where job-search requirements were weakly enforced. The results should therefore be interpreted as evidence on the trade-off between benefit generosity and job protection in a setting where that trade-off is explicit, rather than as the effect of a generic unemployment-insurance expansion or a formal paid-leave reform.

### 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 mothers whose first observed birth in the administrative data occurs between 2012 and 2016. The monthly panel follows approximately 640,000 mothers employed in the private sector before childbirth, from two years before the end of compulsory maternity leave to four years after. 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 means and standard deviations for the main sample. Mothers working in the private sector and giving birth to their first child in the sample period are on average 33.5 years old. Most are employed with a permanent contract (93 percent), 65 percent work full time, and 60 percent are employed in white-collar occupations. Average gross monthly earnings are €1,521.

**Linking Households** For a subset of mothers, I can link mothers to partners using information reported in parental-leave applications. This linked-household sample allows me to examine whether partners adjust labor supply in response to the reforms, although it is positively selected because reporting partner information is not mandatory. Because of this, the sample of mothers who can be linked to fathers is likely subject to positive selection. Table A1 compares linked and

unlinked mothers in the combined reform analysis sample used in the baseline analysis, defined as mothers who enter either the replacement-rate or duration reform sample.[6] Overall, 44.6 percent of mothers in the combined reform analysis sample can be linked to partners in the administrative data. Among mothers linked to partners, fathers' average pre-birth gross monthly earnings are approximately €2,105. Mothers in the linked 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), comparing cohorts of mothers who 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 twho 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. This design estimates the effect of access to a more generous UI regime during the post-maternity choice period, rather than the effect of realized UI take-up itself. 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

[6] As explained in detail in Section 3.2.

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).[7] $\lambda_i^m$ 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. $\mathbf{X_i'}$ 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 $\beta_k$ measure the differential impact of the reforms in the treatment group relative to the control group with respect to the month before the end of compulsory maternity leave.[8]

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 $\beta_k$ for $k < 0$. For the monthly UI 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 monthly outcomes[9]: (i) the probability of being on UI, (ii) the probability of being on parental leave (PL), and (iii) the probability of employment.

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

[8] 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.

[9] The three monthly outcomes are not an exhaustive partition of all post-birth states, so their estimated effects need not sum to zero. The residual category includes months in which mothers are not observed in parental leave, UI, or covered employment in the administrative records, including unpaid time at home or other nonemployment states.

#### 4.1.1 Replacement Rate Reform

Figure 3 Panel (a) reports the effects of the replacement-rate reform on mothers' monthly program status and employment after childbirth. The green triangle series reports the coefficients $\beta_k$ 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 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 the share of mothers on UI is accompanied by changes in both parental-leave use and employment. The pink diamond series shows the effect on the probability of being on PL. During the first six months after maternity leave, the increase in UI status is mirrored by a corresponding decrease in PL use, 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 UI, PL 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 $\beta_k$ 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 UI, PL, 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 be on UI by 1.06 percentage points five months after the end

of ML, with a corresponding 1.16 percentage point decrease in PL use. By month 9, the share on UI increases by 1.72 percentage points, representing an 11.6% increase relative to the control group mean of 14.8%. Parental-leave use 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 respond to extended duration in qualitatively similar ways as to increased replacement rates. Treatment mothers are more likely to be on UI immediately after maternity leave, with a corresponding decrease in PL use, 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 $\beta_k$ from model 1 on the share of mothers on UI, the share of mothers on PL, and the share of mothers working. The share on UI 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 use 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%. The duration reform displays a less pronounced substitution pattern than the replacement-rate reform. UI status rises, but PL use does not fall persistently and is slightly higher by month 9. This pattern is consistent with some mothers using the longer UI entitlement to extend time away from work after, or alongside, parental leave rather than replacing PL one-for-one. Complementarity between programs therefore appears more relevant for the duration reform.

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

Overall, the results indicate that when UI benefits become more generous, either through higher benefit levels or longer potential duration, mothers shift toward unemployment insurance

despite the loss of job protection. The evidence points primarily to substitution between programs rather than complementarity with parental leave: in the months immediately after maternity leave, increases in the share on UI are accompanied by declines in PL use, and PL use does not rise systematically in response to the reforms.

At the same time, the effects do not sum mechanically to zero because mothers may remain out of work without claiming either program. This residual category is economically relevant: if the reforms move mothers from unpaid nonemployment into compensated UI, they increase household resources during months that otherwise would have involved no observed program income. Over time, the shift toward UI is accompanied by lower employment, indicating that the more generous UI option also extends time away from work beyond the initial program-substitution margin.

#### 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. Because eligibility is determined by the date at which compulsory maternity leave ends, the design compares adjacent cohorts around each reform cutoff and cannot include unrestricted calendar-year fixed effects, a feature shared with other studies using similar parental-leave reform designs (Schönberg and Ludsteck, 2014; Kleven et al., 2024). The identifying assumption is therefore that no other cohort-specific shock changed mothers' post-birth program choices discontinuously at the reform thresholds.

Several features of the setting mitigate this concern. First, both reforms were introduced as part of broader structural labor-market reforms with limited scope for strategic fertility timing. The replacement-rate reform was announced in June 2012 and entered into force in January 2013, while the duration reform was announced in March 2015 and entered into force in May 2015. In both cases, the notice period was shorter than the lead time required to time conception so that the end of compulsory maternity leave would fall after the reform cutoff. Consistent with this, monthly birth frequencies show no evidence of discontinuities or unusual bunching around the 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, the two reforms occurred under markedly different macroeconomic conditions. The 2013 replacement-rate reform was implemented during a severe recession, when Italian GDP contracted 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. Although this comparison alone cannot identify the policy effect, it is informative that the two reforms occurred under markedly different macroeconomic conditions yet produced similar increases in UI use. This pattern is consistent with the policy changes, rather than coincident macroeconomic shocks, being the primary driver of the results. Finally, the pre-determined controls included in the baseline specification, especially average earnings in the two years before the end of maternity leave, absorb level differences between cohorts.

For UI status, conventional pre-trends are not informative because mothers are employed before childbirth and therefore cannot take up UI through the post-maternity resignation channel before the choice period. 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 the UI outcome across these placebo birth cohorts. The placebo tests focus on UI status because pre-trends for earnings and employment are directly observable in the main event-study estimates.

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 the share on UI 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 status 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 ef-

fects. 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. As a diagnostic check, Appendix Table A2 examines subsequent births among mothers who return to formal employment, where later births are observable in administrative data. The estimates provide little evidence that differential fertility responses explain the persistence of the main effects: they are close to zero for the duration reform, and negative but very small for the replacement-rate reform. Because the analysis is limited to mothers who return to work, it cannot rule out fertility responses among mothers who remain outside the observed labor market.

### 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 $\beta_k$ normalized 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 the program-status results above: during the first six months after compulsory maternity leave, mothers shift from PL toward UI without 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 the probability of formal employment as the dependent variable.[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. Because the administrative records do not measure hours worked, I cannot decompose earnings losses into hours and wage-rate responses. I instead distinguish three observable margins: the probability of formal employment, days worked conditional on employment, and daily earnings conditional on employment. The results indicate that the persistent component of the earnings loss is driven primarily by reduced formal employment. For the replacement-rate reform, daily wage effects are negative in the short and medium run but fade by four years after the end of compulsory maternity leave, while employment remains significantly lower. For the duration reform, daily wage effects are negative at two years but are not statistically significant at one year or four years, and days worked conditional on employment are not significantly affected. Thus, wage-rate changes contribute to some medium-run earnings losses, but they do not account for the persistent earnings effects. The wage rate is defined as earnings per day worked conditional on employment. The table reports the estimates of the $\beta_k$ 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 on employment and therefore do not mechanically reflect the increase in non-employment among treated mothers. A negative effect is present in the short term but fades to statistical insignificance by $t = 48$. The dominant and durable channel through which the reform affects long-run earnings remains the extensive margin: the persistent

[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 $t$.

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 have significant effects on days worked conditional on employment. 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 show that more generous UI reduces mothers' post-birth employment and earnings after benefits expire. The evidence points primarily to reduced formal employment rather than to persistent changes in work intensity or wages among mothers who return to work: days worked conditional on employment are largely unaffected, and daily wage effects are temporary. The replacement-rate reform generates earnings losses that remain visible through the four-year horizon. For the duration reform, the effects are largest in the first two years after compulsory maternity leave and are smaller by the end of the four-year period.

#### 4.2.3 Who Responds to Each Reform? Characterizing Compliers

The results above show that some mothers are induced by greater UI generosity to choose UI, a margin that requires giving up job protection and separating from the pre-birth employer. 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 helps assess this concern. The two complier groups are similar along several observables: replacement-rate and duration compliers are nearly identical in age, have similar shares of blue-collar workers, and have comparable pre-birth wage rates. The most notable difference is in pre-birth full-time employment: 56.4% of replacement-rate compliers work full time, compared with 72.7% of duration-reform compliers. These patterns suggest that the replacement-rate reform reaches mothers with somewhat weaker pre-birth labor-market attachment, whereas the duration reform reaches mothers with stronger attachment to work. The complier characteristics are descriptive and do not rule out unobserved compositional differences, but they provide useful context for interpreting the reform-specific marginal benefits.

### 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 post-birth support.

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 1.5 months relative to the pre-reform baseline.[11] The per-complier earnings losses reported above should therefore be interpreted as primarily reflecting compliers' persistent nonemployment over the observed horizon rather than a mixture of compliers' and always-takers' responses. Section 5 returns to this distinction in the welfare analysis.

### 4.4 Household Responses

Because post-childbirth decisions are made within the household, the reforms may affect partners' employment as well as mothers' program choices. If partners systematically increase employment when mothers take up unemployment insurance, part of the household adjustment would occur through partners' earnings. This possibility is relevant for interpreting the marginal-benefit estimates, which could partly reflect intra-household income smoothing.

I examine partner responses among the 44.6 percent of mothers who can be linked to partners in the administrative data. This linked sample is positively selected because partners can be

[11] Specifically, the average potential benefit duration after the duration reform is 9.5 months (compared to the 8 months pre-reform). Given that always-takers represent approximately 19% of the eligible cohort under the duration reform, a 1.5 months 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.

identified only when the relevant information is observed in the administrative records. Table A1 compares linked and unlinked mothers.

The reforms generate modest partner responses (Table A3). The replacement-rate reform produces small temporary reductions in partners' employment of 0.3-0.5 percentage points, while the duration reform produces small increases of 0.5-0.9 percentage points concentrated in months 5-10. Partners' parental-leave take-up is largely unaffected by either reform, with small and inconsistent estimates relative to a baseline take-up rate below 1 percent.

The absence of a systematic offsetting employment response, together with the modest magnitude of the estimates relative to mothers' increase in UI use, suggests that household labor-supply adjustment is unlikely to be a primary driver of mothers' program choices or of the estimated marginal benefits. Because the linked sample is selected, these results should be interpreted as evidence against large partner responses in the linked sample rather than as definitive evidence for all households.

# 5 Welfare Implications

## 5.1 A Revealed-Preference Approach for Estimating the Value of Post-Birth Benefit Generosity

Building on the empirical evidence in Section 4, I now use mothers' program choices to recover their revealed valuation of more generous benefits after childbirth, along the lines of Chetty (2008); Hendren (2017); Fadlon and Nielsen (2019). The object is a within-state valuation: conditional on having a child and holding the post-childbirth state fixed, I compare choices under lower- and higher-benefit regimes. The marginal benefit therefore does not measure the value of motherhood, the value of access to parental leave, or the total value of income support after childbirth. It measures how much mothers value an increase in benefit generosity within the same post-childbirth state.

The revealed-preference logic is that mothers must take a costly action to access more generous benefits. In this setting, that action is giving up job protection. Operationally, the marginal benefit (MB) is identified from mothers' willingness to incur the private costs associated with losing job protection in order to receive more generous unemployment-insurance benefits. Intuitively, if mothers respond strongly to greater generosity despite the risk of persistent employment and earnings losses, their choices reveal that the additional post-birth support has substantial value.

I develop a simple model designed to capture the key trade-offs underlying mothers' program

choices. The model is static for transparency, but the same logic extends to a multi-period setting, which is especially relevant for the duration reform.

Let $p = 1$ denote taking up unemployment insurance and giving up job protection, and let $p = 0$ denote taking up job-protected parental leave. Let $b$ denote unemployment-insurance generosity. Let $\theta \sim F$ denote the mother's type, and let $\phi(\theta)$ be the net cost of giving up the job and losing job protection. This cost may reflect search frictions, loss of firm-specific human capital, uncertainty about re-employment, and other pecuniary or non-pecuniary costs of separating from the pre-birth employer.

Let $V(p; b)$ denote the value of program choice $p$, excluding the perceived cost of job separation. I follow a sufficient-statistics revealed-preference approach, in which willingness to pay for a marginal policy change is the dollar-equivalent compensation that makes the agent indifferent between the policy change and an equivalent cash transfer (Hendren and Sprung-Keyser, 2020). The framework does not restrict the welfare-relevant content of $V$ to consumption smoothing. In the parental-leave setting, $V$ bundles several welfare-relevant components: additional resources during leave, supported time at home with an infant, and relaxation of childcare constraints. The empirical object identified from mothers' choices is $V_b(1; b)$, the marginal revealed value of unemployment-insurance generosity in the post-childbirth state. Note that the framework does not separately identify these components. Section 6 uses the comparison across reforms and heterogeneity by household income, childcare availability, and local labor-market conditions to interpret which mechanisms contribute to the aggregate revealed valuation.

For analytical tractability, the derivation that follows specializes $V$ to the consumption-only case, $V(p; b) = u(c(p; b))$, where consumption is $c(p; b) = y(p) + B + \mathbf{1}\{p = 1\}b$. This specialization yields a closed-form expression for the marginal benefit that connects naturally to existing social-insurance applications (Chetty, 2008; Hendren, 2017; Fadlon and Nielsen, 2019). The interpretation of $V_b(1; b)$ as a revealed marginal valuation does not require the consumption-only specialization.

Here $B$ denotes baseline parental-leave benefits and $b$ denotes the additional unemployment-insurance benefit available when the mother chooses UI. Income $y(p)$ may differ across choices. Under parental leave and job protection, $y(0)$ reflects earnings associated with returning to the pre-birth job. Under unemployment insurance and no job protection, $y(1)$ reflects earnings after job separation.[12]

[12] In principle, the difference $y(0) - y(1)$ could be derived from a job-search model with search frictions, loss of firm-specific human capital, or scarring. I do not model these microfoundations explicitly. Instead, Section 4 directly estimates the reduced-form employment and earnings consequences of choosing unemployment insurance.

The mother's full utility is

$$U(p;b) = V(p;b) - \mathbf{1}\{p = 1\}\phi(\theta).$$

This formulation separates the value of the program option from the cost of accessing it through job separation. The value of additional resources, supported time at home with an infant, and relief from childcare constraints belongs in $V(1;b) - V(0;b)$: these are reasons why the UI option may be valuable after childbirth. By contrast, $\phi(\theta)$ captures the private cost of giving up job protection. The cost $\phi(\theta)$ does not include costs borne by firms from providing job protection, nor externalities operating through statistical discrimination. Those social costs are incorporated separately in the MVPF analysis in Section 7.

The mother chooses unemployment insurance if

$$U(1;b) \geq U(0;b),$$

or equivalently if

$$V(1;b) - V(0;b) \geq \phi(\theta).$$

The threshold type is therefore

$$\bar{\phi}(b) = V(1;b) - V(0;b).$$

Mothers with $\phi(\theta) \leq \bar{\phi}(b)$ choose unemployment insurance, while mothers with $\phi(\theta) > \bar{\phi}(b)$ remain attached to their pre-birth employer through parental leave. Let $\Phi(b)$ denote the job-protected participation rate:

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

An increase in unemployment-insurance generosity raises $\bar{\phi}(b)$, making unemployment insurance attractive to mothers with higher perceived costs of giving up job protection. As a result, the share remaining attached, $\Phi(b)$, falls.

Differentiating the threshold with respect to UI generosity gives:

$$\frac{d\bar{\phi}(b)}{db} = V_b(1;b).$$

The derivation can be illustrated in the consumption-only case, where $V_b(1;b) = u'(c(1;b))$.

More generally, $V_b(1;b)$ is the marginal revealed value of additional UI generosity in the post-childbirth state. The parental-leave participation rate responds to benefit generosity according to:

$$\frac{d\Phi(b)}{db} = -f(\bar{\phi}(b))\frac{d\bar{\phi}(b)}{db}$$

Combining the two equations gives:

$$V_b(1;b) = \frac{|d\Phi(b)/db|}{f(\bar{\phi}(b))}.$$

Thus, the employment-attachment response to benefit generosity reveals the marginal value mothers place on additional unemployment-insurance generosity.

The marginal benefit compares this revealed marginal value across the low- and high-generosity regimes:

$$MB = \frac{V_b(1;b^{low})}{V_b(1;b^{high})} - 1.$$

If $V$ is concave in $b$, $V_b(1;b)$ is decreasing in $b$: an additional euro of UI is more valuable when current post-birth support is less generous. The magnitude of $MB$ therefore measures how strongly mothers' marginal valuation of UI declines across the two regimes. Section 6 interprets this object by examining the roles of consumption smoothing, childcare constraints, the value of additional time at home with the infant, and behavioral frictions.

Appendix A shows that the marginal benefit can be written as

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

where

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

captures differences across regimes in the density-adjusted participation elasticity. The elasticity of job-protected parental-leave participation with respect to benefit generosity is defined as:

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

for $s \in \{low, high\}$.[13]

[13]Note that this is practically equivalent to following the approach of Hendren (2017), which recovers marginal benefits from behavioral responses to benefit generosity scaled by the relevant labor-supply semi-elasticity.

Behavioral responses to benefit changes reveal the value of transfers in the relevant state. The interpretation differs in the parental-leave setting. The marginal value $V_b(1;b)$ should not be read as the value of cash transfers alone. Choosing unemployment insurance after childbirth changes resources during leave, makes additional time at home financially feasible, and may relax childcare constraints, while also requiring mothers to give up job protection. The estimated marginal benefit therefore captures the revealed value of additional post-birth support relative to the perceived cost of job separation. Section 6 interprets this object by examining liquidity, childcare availability, preferences for time at home, and behavioral frictions.

**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 extent to which mothers undertake costly action, such as forgoing job protection and bearing significant long-run earnings losses, reveals the value they place on more generous post-birth support. In the parental-leave setting, this support may be valuable not only because it raises resources, but also because it makes time at home with an infant financially feasible.

The model captures costs and benefits accruing to mothers and the government budget, but it is partial equilibrium in nature and omits 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 $\kappa$, which captures differences across regimes in the density-adjusted participation elasticity. Following Fadlon and Nielsen (2019), in the empirical implementation I set $\kappa = 1$, which assumes that this density-adjusted elasticity is approximately stable across the low- and high-benefit regimes. Appendix A discusses this approximation.[14] Using this assumption, it is possible to write:

[14]The assumption that $\kappa = 1$ follows Fadlon and Nielsen (2019) "locally constant ratio" approach, which holds when the density $f(\cdot)$ is approximately constant in the region between the two benefit regimes and the elasticity structure

$$MB \simeq \frac{b^{high}}{b^{low}} \frac{\Phi^{low}}{\Phi^{high}} - 1 \tag{2}$$

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 change in the probability of remaining attached to the pre-birth employer, which identifies the behavioral response to benefit generosity; and (ii) the increase in the net present value of UI benefits generated by the reform. Together, these objects map observed choices into revealed-preference measures of mothers' willingness to trade long-run labor-market attachment for more generous post-birth support.

The benefit ratios $b^{high}/b^{low}$ reported in Table A4 are computed using the net present value of UI benefits under each regime. As a result, they need not coincide with simple ratios based on statutory replacement rates or maximum potential benefit durations. This distinction is especially important for the duration reform. An extension of potential benefit duration raises benefit value only when recipients remain on UI long enough to use the additional entitlement. A statutory increase in potential duration from 8 to 11.5 months[15] among UI recipients therefore does not translate one-for-one into benefit value, because the additional months matter only for recipients who remain on UI beyond the pre-reform 8-month limit. In practice, benefit values 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 benefit generosity generated by each reform. The compliers-only exercise below then examines how the marginal-benefit estimates change after accounting for mechanical gains accruing to mothers who would have taken up UI under either regime.

Table A4 reports the parameters entering the MB calculations for both reforms. For the replacement-rate reform, the NPV-based benefit ratio is $b^{high}/b^{low} = 1.40$, compared with the statutory replacement-rate ratio of 1.25. To estimate the participation response, I use the reduced-form estimates from

does not change substantially across regimes (see Appendix A for derivation and discussion).

[15]The 11.5 average reflects the shorter contribution histories of mothers who take UI post-childbirth relative to the broader population described in Section 2.

Section 4. The reform decreases the probability of remaining attached to the pre-birth employer 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 2 yields $MB^{RR} = 1.402 \times 1.017 - 1 = 0.43$.

The duration reform follows the same calculation procedure. The statutory ratio for potential benefit duration among UI recipients is $11.5/8 = 1.44$, while the corresponding NPV-based benefit ratio used in the marginal-benefit calculation is 1.32. The lower duration ratio reflects partial utilization of the additional entitlement and avoids treating potential duration as if it mechanically translated into realized transfers. Combining this benefit ratio with a participation ratio of 1.015 yields $MB^{Duration} = 0.34$. This estimate is lower than the MB for the replacement-rate reform, but the difference is modest. 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.

**Compliers-Only Marginal Benefits** A potential concern with the aggregate implementation is that the reduced-form effects entering the marginal-benefit formula combine two components. First, mothers induced to take up UI by the reform change their program choice on the extensive margin. Second, mothers who would have taken up UI even under the less generous regime may mechanically receive higher benefits or remain eligible for longer when generosity increases. To assess the importance of this second channel, I follow Haller, Staubli, and Zweimüller (2024) and separate the reduced-form effects into mechanical components accruing to always-takers and behavioral components associated with compliers, using the always-taker share $\pi_{AT}$ estimated in Section 4.2.3 and direct evidence on always-takers' UI spell length before and after the reforms.[16]

This adjustment yields complier-specific marginal benefit estimates of $MB^{C}_{RR} = 0.34$ for the replacement-rate reform and $MB^{C}_{Duration} = 0.30$ for the duration reform. These estimates are modestly lower than the aggregate estimates, consistent with some mechanical benefit gains accruing to always-takers. The relatively small gap between aggregate and complier-specific estimates indicates that the main conclusions are not driven by always-takers' mechanical gains. Instead, the estimates continue to reflect substantial revealed valuation among mothers whose program choices

[16]For the duration reform, always-takers extend their UI spells by approximately 1.5 months on average relative to the pre-reform baseline, well below the additional entitlement created by the reform.

are shifted by the reforms.

The complier-specific estimates are useful for interpretation because the welfare consequences of expanding benefit generosity depend importantly on mothers whose choices change in response to the policy. They also preserve the ranking of the aggregate estimates: the replacement-rate reform generates a somewhat higher marginal benefit than the duration reform, consistent with the importance of immediate resources and transfer timing.

These estimates are comparable in magnitude to marginal benefits estimated in other social-insurance settings, though the welfare object differs in the parental-leave context. Haller and Staubli (2023) estimate marginal benefits between 2.2 and 3.4 for disability insurance, while Coyne et al. (2024) and Giupponi (2024) estimate marginal benefits between 0.23 and 0.37 for survivor benefits. The estimates in this paper are therefore moderate relative to existing revealed-preference estimates. The comparison underscores that the estimates are economically meaningful, while the parental-leave setting requires interpreting them as valuations of resources and caregiving time relative to the perceived cost of giving up job protection, rather than as valuations of transfers alone.

# 6 Interpreting Mothers' Choices

The revealed-preference estimates imply substantial valuations of more generous unemployment-insurance benefits after childbirth: $MB = 0.43$ for the replacement-rate reform and $MB = 0.34$ for the duration reform. Interpreting these estimates requires understanding what mothers reveal when they trade job protection for greater generosity. In the parental-leave setting, the marginal benefit may reflect several welfare-relevant sources of value, including consumption smoothing, caregiving time with an infant, and relief from childcare constraints. It may also reflect behavioral frictions, such as limited attention to re-employment risk or present-biased valuation of immediate transfers. This section examines these mechanisms and uses them to motivate the welfare scenarios in the MVPF analysis.

## 6.1 What Does the Marginal Benefit Measure in the Parental Leave Setting?

The central challenge is not whether mothers value more generous benefits, but what their choices reveal. The revealed-preference estimates imply that many mothers are willing to sacrifice future earnings and labor-market attachment in exchange for greater generosity after childbirth. The

same observed choice, however, can reflect welfare-relevant sources of value as well as mechanisms that create a wedge between revealed valuation and experienced welfare.

The first set consists of sources of value. More generous benefits increase resources during a period of elevated household needs, make time at home with an infant financially feasible, and may relax constraints created by limited childcare availability. These channels are welfare relevant in the parental-leave setting. In particular, time away from work is not necessarily a behavioral cost: supported caregiving time may be an intended and valued output of parental-leave policy.

The second set consists of potential wedges. Mothers may underestimate the consequences of losing job protection, pay limited attention to re-employment risk, or place disproportionate weight on immediately available transfers. These frictions may be especially relevant in the post-childbirth period, when decisions are made under time pressure, changing household needs, and uncertainty about childcare and re-employment. These mechanisms can make observed willingness to pay differ from experienced welfare. The marginal benefit should therefore be interpreted as mothers' revealed valuation of additional post-birth support relative to the perceived cost of giving up job protection, not as the value of cash transfers alone.[17]

The empirical design does not separately identify the contribution of resources, caregiving time, childcare constraints, and behavioral frictions. It does, however, provide evidence on which mechanisms are likely to matter. The contrast between a reform that raised benefits immediately and one that extended benefit duration is informative about responses to immediate resources versus support received later in the leave spell. Heterogeneity by household income, childcare availability, and local labor-market conditions sheds light on the roles of liquidity constraints, childcare needs, and attention to re-employment risk. The following sections examine these mechanisms in turn.

### 6.2 Welfare-Relevant Sources of Valuation

I first examine mechanisms that raise the welfare-relevant value of post-birth support: liquidity needs, childcare constraints, and the value of supported time at home with an infant. These exercises should be interpreted as evidence on the patterns underlying revealed valuation rather than as a causal decomposition of the marginal benefit.

[17] The marginal-benefit estimates interpret mothers' revealed valuations holding other household behavior constant. If partners systematically increase their labor supply when mothers take up unemployment insurance, part of the measured valuation could reflect intra-household income smoothing rather than mothers' individual valuation. In the linked-household sample, however, the reforms generate only modest and inconsistent effects on partners' employment and little change in their parental-leave take-up. These results suggest that partner adjustment is unlikely to be a primary driver of the estimated marginal benefits.

**Liquidity Constraints** One explanation for the high valuation of benefits after childbirth is that some households cannot smooth consumption fully across this period. Benefits increase cash on hand when earnings fall and childbirth-related needs may be elevated. If liquidity is important, mothers in households with fewer resources should respond more strongly to increases in benefits, particularly when the additional transfers arrive immediately.

I proxy for households' ability to smooth consumption using their position in the household-income distribution and estimate the reforms' effects separately by income tercile.[18]

Figure 5 Panel (a) shows a pronounced income gradient for the replacement-rate reform. Mothers in the lowest income tercile increase UI use significantly more than mothers in the highest tercile when monthly benefits rise. This pattern is consistent with a high marginal value of cash on hand among lower-income households. For the duration reform, the gradient is flatter: UI use remains higher among lower-income mothers but the differences across terciles are not statistically significant.[19]

The contrast across reforms suggests that the timing of resources matters. The replacement-rate reform raises payments immediately after childbirth, whereas the duration reform provides additional resources later in the benefit spell. The stronger income gradient for the former is consistent with binding short-run liquidity constraints. It does not, by itself, distinguish liquidity from other reasons mothers may respond more strongly to immediate transfers, including present-biased valuation. I return to this distinction in Section 6.3.

The complier characteristics reported in Section 4.2.3 provide complementary evidence. Replacement-rate compliers are more likely to work part time before childbirth and have lower average monthly earnings, a profile consistent with weaker labor-market attachment and greater exposure to short-run liquidity needs. These characteristics are descriptive, but they align with the income-gradient evidence.

**Childcare Availability** Childcare availability provides a second welfare-relevant source of valuation. When affordable formal or informal childcare is scarce, returning to work may be costly or infeasible. Greater benefit generosity can then support home production of childcare and operate as a second-best response to an incomplete childcare market.

I examine whether the reforms' effects vary with regional availability of formal childcare for

[18] 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.

[19] Because household income is not randomly assigned, this gradient should be interpreted as only suggestive evidence of the role of liquidity constraints; it may also reflect correlated household characteristics, including specialization within the household and gender wage gaps.

children aged zero to two. Regions are divided according to whether childcare capacity is above or below the national median. Figure 6 shows that the increase in UI use is larger in low-childcare regions for both reforms. The pattern is especially relevant for the duration reform, which provides support later in the leave spell, when the availability of alternative care is likely to be important for returning to work.

These differences are consistent with childcare scarcity increasing the value of remaining at home. They suggest that, in the observed policy environment, limited access to suitable care is a genuine constraint faced by families. The comparison shows that the value of benefit generosity is likely to depend on the availability of complementary childcare policy.

**Preferences for Infant Care** Mothers may also value time with their infants independently of liquidity or childcare scarcity. Even mothers with sufficient resources and access to formal care may prefer to provide care themselves during the first months of a child's life. This value is part of the objective of parental-leave policy and belongs in the welfare calculation rather than being treated as a cost of induced nonemployment.

The empirical design does not separately identify the intrinsic value of infant-care time, because choosing unemployment insurance jointly changes household resources, time away from work, and attachment to the pre-birth employer. The contrast between the two reforms is nevertheless informative about whether preferences for time at home can explain the full response. The duration reform most directly expands the period during which benefits support remaining at home. If the value of infant-care time were the primary driver of mothers' choices, one would therefore expect mothers to value the duration expansion more strongly than the increase in immediately available benefits. Instead, the marginal benefit is 0.34 for the duration reform and 0.43 for the replacement-rate reform; the same ordering holds for the complier-specific estimates. This pattern suggests that preferences for time at home may contribute to the revealed valuation but cannot account for it fully. Immediate liquidity and the timing of transfers also appear to matter.[20]

This comparison does not quantify the value of infant-care time because the reforms differ along other dimensions and may affect different mothers. At the same time, the significant response to the duration reform, including in regions with greater childcare availability, indicates

[20] A related concern is that preferences for time at home may evolve after childbirth. All mothers complete the same five-month period of compulsory maternity leave before making the relevant choice between parental leave and unemployment insurance. Treatment and comparison groups are therefore exposed to the same initial period at home before their program choices diverge. This feature limits the extent to which the estimated reform effects can be attributed to differential preference changes during the initial bonding period, although it does not rule out subsequent state dependence in preferences.

that the value of remaining at home should not be dismissed. Taken together, the evidence points to a combination of infant-care preferences, liquidity needs, and transfer timing rather than to any single mechanism.

The characteristics of duration-reform compliers provide complementary context. These mothers are substantially more likely to work full time before childbirth and have correspondingly higher average monthly earnings. This profile is consistent with stronger labor-market attachment and comparatively more to lose from giving up job protection. Their willingness to do so in exchange for a longer period of benefit receipt indicates that extended post-birth support has meaningful value even among mothers with strong attachment to work.

Although the analysis cannot independently quantify the value of infant-care time, that value remains part of what mothers reveal when they choose more generous benefits. The induced reduction in employment can therefore have two distinct welfare consequences. Time at home during infancy may provide a direct caregiving benefit, while persistent nonemployment after benefits expire, resulting from the loss of job protection, generates future earnings losses and fiscal costs. The welfare analysis should therefore distinguish valued caregiving time from the longer-run employment costs of job separation rather than treating all time away from work as conventional unemployment-insurance moral hazard.

### 6.3 Evidence on 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 program choices may overstate welfare-relevant willingness to pay for unemployment-insurance generosity. Part of the estimated marginal benefit may then reflect biased beliefs or limited attention rather than preferences under full information.

A growing literature documents the importance of behavioral frictions in insurance and social-insurance choices (Ganong and Noel, 2019; Gerard and Naritomi, 2021). In this setting, such frictions can create a wedge between observed willingness to pay and mothers' experienced welfare from more generous benefits. This section examines two patterns that are informative about this possibility: limited attention to local re-employment risk and sensitivity to the timing of transfers. These analyses show whether mothers' choices vary with future labor-market costs and with when additional benefits are received. They also motivate the conservative benchmark valuations presented in 6.4.

**Local Re-employment Risk** If mothers fully incorporate local labor-market conditions, the cost of giving up job protection should be greater where finding another job is more difficult. Other things equal, one would therefore expect the take-up response to greater unemployment-insurance generosity to be smaller in regions with high unemployment.

Figure 7 shows instead that the take-up response is similar in regions above and below the median unemployment rate for both reforms. Figure 8 also shows that the subsequent employment consequences are substantially larger in high-unemployment regions. Mothers therefore exhibit similar program responses in environments where the realized cost of losing job protection differs markedly. This interpretation is close in spirit to Landais and Spinnewijn (2021): revealed valuations depend on perceived risks, while welfare consequences depend on realized risks. In the parental-leave setting, mothers' choices therefore reveal the value of additional post-birth support relative to the perceived cost of giving up job protection.

This disconnect is consistent with limited attention to re-employment risk or inaccurate beliefs about the consequences of job separation. It is not direct evidence of inattention: regional unemployment may be correlated with unobserved preferences, informal employment opportunities, childcare provision, or other determinants of program choice. The result nevertheless motivates examining valuations in low-unemployment regions, where the welfare consequences of any misperception of re-employment risk are smaller.

**Transfer Timing** The two reforms provide complementary evidence on the timing of benefits. The replacement-rate reform raises payments immediately, whereas the duration reform provides additional payments later in the spell. Aggregate marginal benefits are 0.43 for the replacement-rate reform and 0.34 for the duration reform. The same ordering appears in the complier-specific estimates, 0.34 and 0.30 respectively, indicating that it is not driven solely by the intensive-margin response of mothers who would have selected unemployment insurance absent the reforms. Because the reforms occur in different periods and may affect different complier populations, I do not interpret the cross-reform comparison as a structural decomposition of level and duration valuations. The comparison is instead suggestive evidence on the relative role of immediate resources and later supported time at home, interpreted alongside complier characteristics and macroeconomic conditions.

Because both reforms increase unemployment-insurance generosity while preserving the same trade-off with job protection, their different timing provides information on the sources of mothers' valuations. The duration reform allows mothers to remain at home for longer, yet it does not

generate a larger valuation than the replacement-rate reform. This suggests that the value of time at home may contribute to mothers' valuations, but cannot account for the full difference in their responses to the two reforms. Generally, the greater response to immediately available benefits may reflect liquidity constraints, present-biased valuation, or a combination of both. This interpretation is consistent with evidence of present-biased or myopic behavior in the unemployment insurance setting (Ganong and Noel, 2019; Gerard and Naritomi, 2021). The comparison cannot determine whether liquidity needs or present-biased valuation drive the stronger response to the replacement-rate reform. It suggests, however, that additional supported time at home is unlikely to be the only force behind the cross-reform pattern.[21] Taken together, the local-labor-market and transfer-timing results suggest that perceived future costs may differ from realized costs for some mothers.

### 6.4 Conservative Benchmark Valuations

To construct conservative benchmark valuations, I restrict the analysis to the duration reform. Because its additional benefits accrue later in the leave spell, the valuation of this reform is less exposed to immediate liquidity needs and to potential present bias. Holding the reform fixed, I then re-estimate the marginal benefit among mothers in low-unemployment and high-childcare regions, where re-employment risk and childcare constraints are respectively less severe. These estimates do not purge all behavioral frictions or isolate preferences for infant-care time. Instead, they show how much revealed value remains when particular mechanisms are less prominent.[22]

The complier characteristics reported in Section 4.2.3 reinforce the relevance of these mechanisms. Replacement-rate compliers have lower monthly earnings and are less likely to work full time before childbirth, a profile consistent with weaker pre-birth labor-market attachment and greater exposure to short-run liquidity needs. Duration-reform compliers have higher monthly earnings and are more likely to work full time, consistent with stronger attachment to work. These profiles are descriptive and help interpret the benchmark exercises rather than identify the mechanisms themselves.

Table 7 reports the estimates. The aggregate marginal benefit for the duration reform is 0.34.

[21] The two reforms occur sequentially, and may affect different complier populations. These differences limit a structural interpretation of the cross-reform contrast and prevent the estimation of a present-bias parameter without additional assumptions.

[22] The use of observably lower-friction environments parallels Allcott, Lockwood, and Taubinsky (2019), who use heterogeneity in nutrition knowledge and self-control to construct behaviorally adjusted demand. In this setting, I follow a similar approach to provide conservative benchmarks in environments where particular mechanisms are plausibly less severe.

In low-unemployment regions, the corresponding estimate is 0.21. Because the realized consequences of misperceiving re-employment risk are smaller in these regions, this estimate provides a conservative benchmark for environments with lower labor-market risk.[23]

In high-childcare regions, the estimated marginal benefit is 0.19. Focusing on regions where formal care is more readily available reduces the role of childcare scarcity in mothers' program choices and provides a benchmark for the valuation observed when returning to work is less constrained by access to care. The positive and economically meaningful estimate indicates that mothers continue to value greater benefit generosity even when they have better childcare alternatives. This remaining valuation is consistent with consumption-smoothing needs and a direct value of time at home with an infant. Because childcare availability is measured at the regional level, I interpret the estimate as a conservative benchmark rather than as an exact decomposition of the aggregate marginal benefit.

The two estimates are similar and remain economically meaningful. They show that mothers continue to value greater generosity in environments with lower re-employment risk or greater childcare availability. The remaining valuation may reflect consumption smoothing, preferences for infant-care time, other constraints, and any behavioral components that persist within these subsamples. The analysis cannot separate these components further.

I use the aggregate and subsample estimates as distinct welfare scenarios in the MVPF calculations. The aggregate estimate measures revealed valuation in the observed policy environment. The low-unemployment and high-childcare estimates provide conservative benchmarks from environments in which specific risks or constraints are less severe. They should not be interpreted as formal upper and lower bounds or as an additive decomposition of the aggregate marginal benefit.

# 7 Fiscal Externalities and the Marginal Value of Public Funds

This section combines mothers' revealed valuations with the fiscal consequences of the reforms. Following Hendren and Sprung-Keyser (2020), I summarize the welfare impact using the marginal value of public funds (MVPF):

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

[23] Landais and Spinnewijn (2021) provide a related benchmark: accounting for the gap between perceived and realized job-loss risk reduces their estimated marginal rate of substitution for Swedish unemployment insurance by approximately one-third. Although the institutional setting and identification strategy differ, their result provides a useful benchmark for the economic magnitude of belief-related wedges in revealed-preference estimates.

where $1 + MB$ is mothers' willingness to pay per euro of additional transfer and $1 + FE$ is the net government cost per euro transferred. The analysis proceeds in three steps. I first estimate the direct fiscal externalities generated by changes in tax revenue and other social-insurance spending. I then calculate MVPFs using the aggregate and conservative benchmark valuations from Section 6. Finally, I extend the duration-reform calculation to incorporate the fiscal consequences of the statistical-discrimination response documented by Carta et al. (2024). The benchmark scenarios change the valuation used in the numerator, while the discrimination extension broadens the costs included in the denominator.

### 7.1 Fiscal Externalities

I estimate the net government cost of each reform using its reduced-form effects (following the same strategy highlighted in Section 3.2) on unemployment-insurance spending, parental-leave spending, and tax revenue over the four years following the end of compulsory leave. Program substitution matters for this calculation: when mothers move from parental leave to unemployment insurance, the increase in unemployment-insurance expenditure is partially offset by savings on parental-leave benefits. The accounting also captures movements from unpaid nonemployment into compensated UI: when mothers enter UI from months with no observed PL, UI, or employment, the corresponding UI payments are included in unemployment-insurance spending and in the net fiscal cost through the observed benefit amounts and benefit-duration measures.

For the replacement-rate reform, unemployment-insurance spending increases by €291.96. Tax revenue falls by €162.85, while parental-leave spending falls by €43.17. The total net government cost is therefore €411.64.

Dividing the total net cost by the additional unemployment-insurance transfer gives $1 + FE$. Thus, each euro of additional unemployment-insurance spending costs the government €1.41 once lost tax revenue and savings from program substitution are included.

For the duration reform, unemployment-insurance spending increases by €670.56, tax revenue falls by €218.46, and parental-leave spending falls by €20.00. The resulting net government cost is €869.02, which implies $1 + FE = 1.30$. Although the duration reform produces a larger total expenditure increase, its fiscal externality per euro transferred is smaller than that of the replacement-rate reform.

### 7.2 MVPF Under Aggregate and Benchmark Valuations

Panel (a) of Table 8 reports the baseline calculations using the aggregate marginal-benefit estimates. For the replacement-rate reform, $MB = 0.43$, so mothers' willingness to pay equals €1.43 per euro transferred. Dividing by the net fiscal cost of €1.41 yields an MVPF of 1.01. For the duration reform,$MB = 0.34$, implying willingness to pay of €1.34 per euro transferred and an MVPF of 1.03 given a net fiscal cost of €1.30.

Under the aggregate revealed valuations, both reforms therefore lie close to one: mothers' willingness to pay for greater generosity approximately equals the net fiscal cost of providing it. The duration reform has a slightly higher MVPF despite its lower aggregate marginal benefit because it generates a smaller fiscal externality per euro transferred.[24]

Panel (b) of Table 8 reports the duration-reform MVPF using the two conservative benchmark valuations from Section 6.4. The first three bars in Panel B of Figure 9 display the aggregate and benchmark MVPFs under direct fiscal costs. Using the marginal benefit of 0.21 estimated in low-unemployment regions gives willingness to pay of €1.21 per euro transferred and an MVPF of 0.94. Using the marginal benefit of 0.19 estimated in high-childcare regions gives willingness to pay of €1.19 and an MVPF of 0.92.

These calculations hold the fiscal cost of the duration reform fixed and vary the valuation placed on the transfer. They show that the duration reform moves modestly below one in environments where re-employment risk or childcare constraints are less severe. Importantly, mothers' willingness to pay remains close to the net fiscal cost under both conservative benchmarks, indicating that greater post-birth generosity retains substantial value even in these environments. Together, the scenarios provide a transparent assessment of how the MVPF varies as particular risks and constraints become less prominent.

### 7.3 Extension: Statistical Discrimination and Job Protection

The welfare calculations in Panels (a) and (b) incorporate costs accruing through mothers' employment and the government budget. Forgoing job protection may also affect other women if firms

[24]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).

respond to higher maternal separation rates by changing their hiring practices. Carta et al. (2024) show that firms exposed to the duration reform shift their hiring of young women from permanent toward fixed-term contracts. Because these workers are not necessarily mothers affected by the reform, this response represents a statistical-discrimination externality that is not internalized in mothers' program choices.

I quantify the fiscal component of this externality by combining the estimated shift toward fixed-term contracts with the earnings gap between fixed-term and permanent employment and the effective tax rate on foregone earnings. The resulting loss of tax revenue is approximately €0.21 per euro of additional unemployment-insurance transfer. This raises $FE$ for the duration reform from 0.29 to 0.50, or equivalently raises $1 + FE$ from 1.30 to 1.50. Full details are provided in Appendix C.

Panel (c) of Table 8 reports the construction of the extended MVPFs, and the final three bars in Panel B of Figure 9 show the results. With this additional fiscal cost, the duration reform's MVPF falls from 1.03 to 0.90 under the aggregate valuation. It falls to 0.81 using the low-unemployment benchmark and to 0.79 using the high-childcare benchmark. This extension is available only for the duration reform because Carta et al. (2024) estimate firms' responses to that reform. It should be interpreted as a quantified component of the broader discrimination externality, not as a complete measure of the welfare loss experienced by women whose employment opportunities deteriorate.

### 7.4 Interpretation and Policy Implications

Three conclusions emerge from the welfare calculations. First, under aggregate revealed valuations, both reforms are close to the MVPF benchmark of one. Mothers place substantial value on greater post-birth generosity, but the reforms also generate meaningful fiscal costs through lower employment and tax revenue. The central baseline result is therefore that recipient willingness to pay approximately matches net government cost.

Second, the duration reform's MVPF is moderately lower when the calculation uses valuations from low-unemployment or high-childcare environments. The movement from 1.03 to 0.92-0.94 shows that the welfare conclusion is sensitive to the mechanisms contributing to mothers' choices, but it does not eliminate the substantial value mothers place on post-birth support.

Third, incorporating the fiscal component of statistical discrimination lowers the duration-reform MVPF more substantially, to 0.79-0.90 depending on the valuation used. This result highlights a cost of reducing job protection that is not visible when the analysis considers only the mothers making the program choice. The calculation also remains conservative with respect to this

channel because it captures lost tax revenue but not the full welfare loss borne by other women.

Taken together, the results do not imply that parental-leave benefits lack value. They show that the design of generosity matters. Benefits that enable consumption smoothing, caregiving time, and relief from childcare constraints can generate meaningful welfare gains. But expanding generosity through a margin that requires mothers to surrender job protection creates persistent private, fiscal, and potentially broader labor-market costs. The findings therefore point toward policies that combine adequate income support with job protection and childcare access, complemented by information or defaults when mothers have difficulty assessing the long-run consequences of job separation.

Finally, the MVPF estimates focus primarily on mothers' revealed welfare and the government budget, supplemented by an extension capturing the fiscal component of statistical discrimination. A complete social-welfare analysis would also incorporate effects on child development, fertility, long-run family outcomes, and the welfare losses experienced by other women whose employment opportunities change. These outcomes lie beyond the scope of the present analysis and may create additional benefits or costs not captured by the reported MVPFs.

# 8 Conclusions

This paper studies how mothers value benefit generosity relative to job protection after childbirth. In the Italian setting, mothers can choose between job-protected parental leave with relatively low benefits and unemployment insurance with higher benefits but no job protection. Using two reforms that increased unemployment-insurance generosity along different margins, I show that mothers respond strongly to greater benefits by substituting from parental leave toward unemployment insurance. This substitution is consequential: mothers induced to give up job protection experience persistent employment and earnings losses after benefits expire.

These choices reveal substantial value for post-birth transfers. At the same time, their interpretation differs from standard unemployment-insurance settings. In parental leave, time at home with an infant may be part of the intended welfare benefit of the policy rather than only a behavioral response to transfers. The estimated marginal benefits therefore reflect a bundle of resources during leave, caregiving time, and relief from childcare constraints, relative to mothers' perceived cost of giving up job protection.

The evidence points to several mechanisms underlying these valuations. Responses are stronger among lower-income households and in regions with more limited childcare availability, con-

sistent with roles for liquidity needs and childcare constraints. The comparison between the replacement-rate and duration reforms shows that mothers value both immediate resources and support that makes remaining at home feasible for longer. At the same time, the weak relationship between take-up responses and local re-employment risk, together with the stronger response to immediately available transfers, is consistent with a role for limited attention and present-biased valuation.

The MVPF calculations summarize these interpretations. Under aggregate revealed valuations, both reforms are close to the welfare-neutral benchmark: mothers' willingness to pay for greater generosity approximately equals the net fiscal cost of providing it. Conservative benchmarks based on the duration reform in low-unemployment and high-childcare environments move the MVPF modestly below one, while the fiscal component of the statistical-discrimination externality lowers the duration-reform MVPF further. These scenarios show that post-birth transfers generate meaningful value, but that welfare gains depend on which mechanisms drive program choice and on whether broader labor-market costs are included.

The policy implication is not that parental-leave benefits lack value. The design of generosity matters. Under conservative welfare scenarios accounting for behavioral frictions and statistical discrimination, expanding UI as a substitute for parental leave yields MVPFs of 0.79–0.90. By contrast, the substantial revealed valuations that persist in low-friction environments ($MB = 0.21$ in low-unemployment regions, 0.19 in high-childcare regions) indicate that the welfare-relevant component of mothers' valuations is positive and economically meaningful. This suggests that expanding paid benefits within the job-protected PL program, preserving employer attachment while addressing the liquidity and childcare constraints that drive mothers' choices, would likely deliver welfare gains. Information or defaults that help mothers understand the long-run consequences of giving up job protection may provide an additional complement when choices are shaped by limited attention to re-employment risk.

Several dimensions remain outside the scope of the analysis. The MVPF calculations focus on mothers' revealed valuations and fiscal costs, supplemented by an extension capturing the fiscal component of statistical discrimination. A complete social-welfare analysis would also incorporate child development, fertility, household bargaining, and the welfare losses experienced by other women whose labor-market opportunities change. These outcomes may create additional benefits or costs beyond those quantified here.

## References


Abadie, Alberto. 2003. "Semiparametric instrumental variable estimation of treatment response models." *Journal of econometrics* 113 (2):231–263.

Abaluck, Jason and Jonathan Gruber. 2011. "Choice inconsistencies among the elderly: evidence from plan choice in the Medicare Part D program." *American Economic Review* 101 (4):1180–1210.

Allcott, Hunt, Benjamin B Lockwood, and Dmitry Taubinsky. 2019. "Regressive sin taxes, with an application to the optimal soda tax." *The Quarterly Journal of Economics* 134 (3):1557–1626.

Andrade, José, Héctor Pifarré i Arolas, and Libertad González. 2021. "The effect of childbirth on job separations." *CRES-UPF Working Paper* 202110-122 (122).

Bailey, Martha, Tanya Byker, Elena Patel, and Shanthi Ramnath. 2025. "The long-run effects of california's paid family leave act on women's careers and childbearing: New evidence from a regression discontinuity design and us tax data." *American Economic Journal: Economic Policy* 17 (1):401–431.

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

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

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

Blundell, Richard, Monica Costa Dias, Costas Meghir, and Jonathan Shaw. 2016. "Female labor supply, human capital, and welfare reform." *Econometrica* 84 (5):1705–1753.

Borghans, Lex, Anne C Gielen, and Erzo FP Luttmer. 2014. "Social support substitution and the earnings rebound: Evidence from a regression discontinuity in disability insurance reform." *American Economic Journal: Economic Policy* 6 (4):34–70.

Carta, Francesca, Alessandra Casarico, Marta De Philippis, and Salvatore Lattanzio. 2024. "Mom's out: employment after childbirth and firm-level responses." *Bank of Italy Temi di Discussione (Working Paper) No* 1458.

Carta, Francesca and Lucia Rizzica. 2018. "Early kindergarten, maternal labor supply and children's outcomes: Evidence from Italy." *Journal of Public Economics* 158:79–102.

Chetty, Raj. 2006. "A general formula for the optimal level of social insurance." *Journal of Public Economics* 90 (10-11):1879–1901.

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

———. 2015. "Behavioral economics and public policy: A pragmatic perspective." *American Economic Review* 105 (5):1–33.

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

CittadinanzaAttiva. 2015. Tech. rep., Osservatorio Prezzi e Tariffe.

Costa-Ramón, Ana, Ursina Schaede, Michaela Slotwinski, and Anne Ardila Brenoe. 2024. "(Not) thinking about the future: inattention and maternal labor supply." Working paper, CESifo.

Coyne, David, Itzik Fadlon, Shanthi P Ramnath, and Patricia K Tong. 2024. "Household labor supply and the value of social security survivors benefits." *American Economic Review* 114 (5):1248–1280.

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

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

Fadlon, Itzik and Torben Heien Nielsen. 2019. "Household labor supply and the gains from social insurance." *Journal of Public Economics* 171:18–28.

Fadlon, Itzik, Torben Heien Nielsen et al. 2015. *Household responses to severe health shocks and the design of social insurance*. National Bureau of Economic Research.

Finkelstein, Amy and Nathaniel Hendren. 2020. "Welfare analysis meets causal inference." *Journal of Economic Perspectives* 34 (4):146–67.

Ganong, Peter and Pascal Noel. 2019. "Consumer spending during unemployment: Positive and normative implications." *American Economic Review* 109 (7):2383–2424.

Gerard, François and Joana Naritomi. 2021. "Job displacement insurance and (the lack of) consumption-smoothing." *American Economic Review* 111 (3):899–942.

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

Ginja, Rita, Arizo Karimi, and Pengpeng Xiao. 2023. "Employer responses to family leave programs." *American Economic Journal: Applied Economics* 15 (1):107–135.

Giupponi, Giulia. 2024. "When income effects are large: labor supply responses and the value of welfare transfers." Working paper, Bocconi University.

Haller, Andreas and Stefan Staubli. 2023. "Measuring the Value of Disability Insurance from Take-Up Decisions." In *116th Annual Conference on Taxation*. NTA.

Haller, Andreas, Stefan Staubli, and Josef Zweimüller. 2024. "Designing Disability Insurance Reforms: Tightening Eligibility Rules or Reducing Benefits?" *Econometrica* 92 (1):79–110.

Handel, Benjamin R. 2013. "Adverse selection and inertia in health insurance markets: When nudging hurts." *American Economic Review* 103 (7):2643–82.

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

———. 2017. "Knowledge of future job loss and implications for unemployment insurance." *American Economic Review* 107 (7):1778–1823.

Hendren, Nathaniel and Ben Sprung-Keyser. 2020. "A unified welfare analysis of government policies." *The Quarterly Journal of Economics* 135 (3):1209–1318.

Immervoll, Herwig and Carlo Knotz. 2018. "How demanding are activation requirements for jobseekers?" Tech. rep., IZA discussion papers.

Inderbitzin, Lukas, Stefan Staubli, and Josef Zweimüller. 2016. "Extended unemployment benefits and early retirement: Program complementarity and program substitution." *American Economic Journal: Economic Policy* 8 (1):253–88.

ISTAT. 2016. "I differenziali retributivi nel settore privato." Tech. rep., Istituto Nazionale di Statistica.

Istituto Nazionale di Statistica (ISTAT). 2016. "Public Offer of Nurseries and Other Socio-Educational Services for the Early Childhood, 2014/2015 School Year."

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

Jørgensen, Thomas Høgholm and Jakob Egholt Søgaard. 2024. "The division of parental leave: Empirical evidence and policy design." *Journal of Public Economics* 238:105202.

Kleven, Henrik, Camille Landais, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller. 2024. "Do family policies reduce gender inequality? Evidence from 60 years of policy experimentation." *American Economic Journal: Economic Policy* 16 (2):110–149.

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

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

Lalive, Rafael, Analía Schlosser, Andreas Steinhauer, and Josef Zweimüller. 2014. "Parental leave and mothers' careers: The relative importance of job protection and cash benefits." *Review of Economic Studies* 81 (1):219–265.

Landais, Camille and Johannes Spinnewijn. 2021. "The value of unemployment insurance." *The Review of Economic Studies* 88 (6):3041–3085.

Ministero del Lavoro e delle Politiche Sociali. 2015. "La dinamica dei contratti di lavoro: Aprile 2015. Prime evidenze ricavate dal Sistema Informativo delle Comunicazioni Obbligatorie." Tech. rep., Ministero del Lavoro e delle Politiche Sociali.

Mullainathan, Sendhil, Joshua Schwartzstein, and William J Congdon. 2012. "A reduced-form approach to behavioral public finance." *Annu. Rev. Econ.* 4 (1):511–540.

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

Paradisi, Matteo. 2021. "Firms and policy incidence." Tech. rep., mimeo.

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

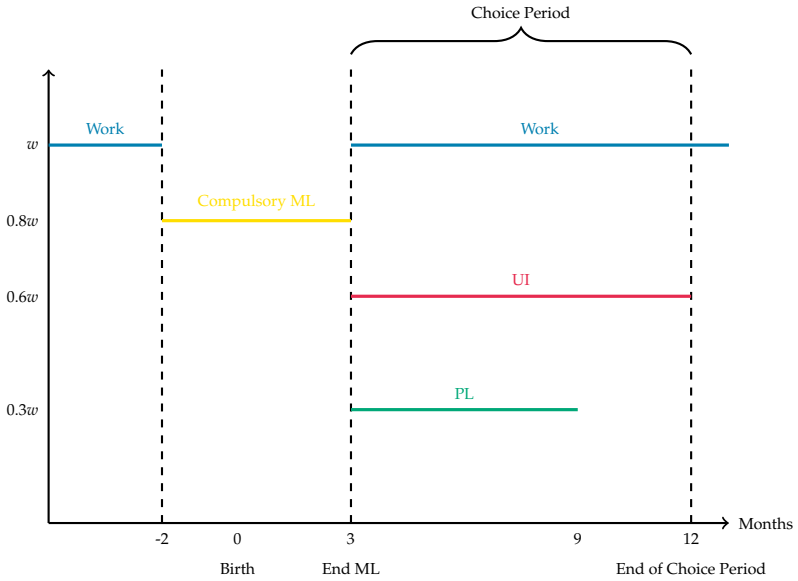
Schmieder, Johannes F and Till Von Wachter. 2016. "The effects of unemployment insurance benefits: New evidence and interpretation." *Annual Review of Economics* 8:547–581.

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

Xenogiani, Theodora, Anne Lauringson, Gianluca Scarano et al. 2019. *Strengthening Active Labour Market Policies in Italy*. OECD Publishing.

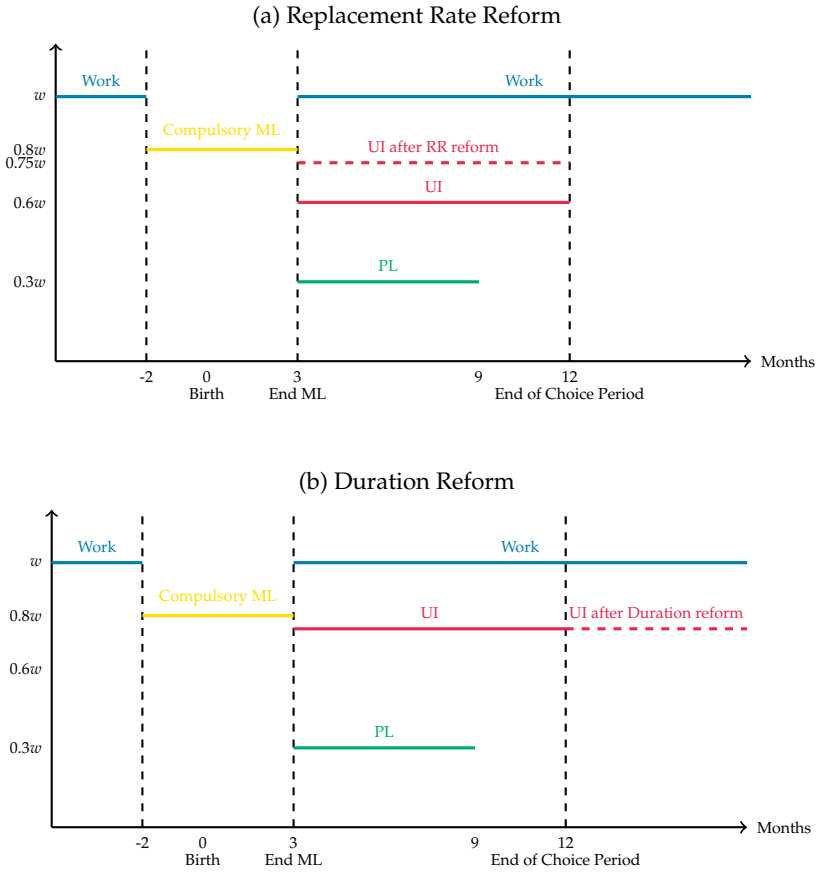
# Figures

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 $t = 3$ and $t = 12$ 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.

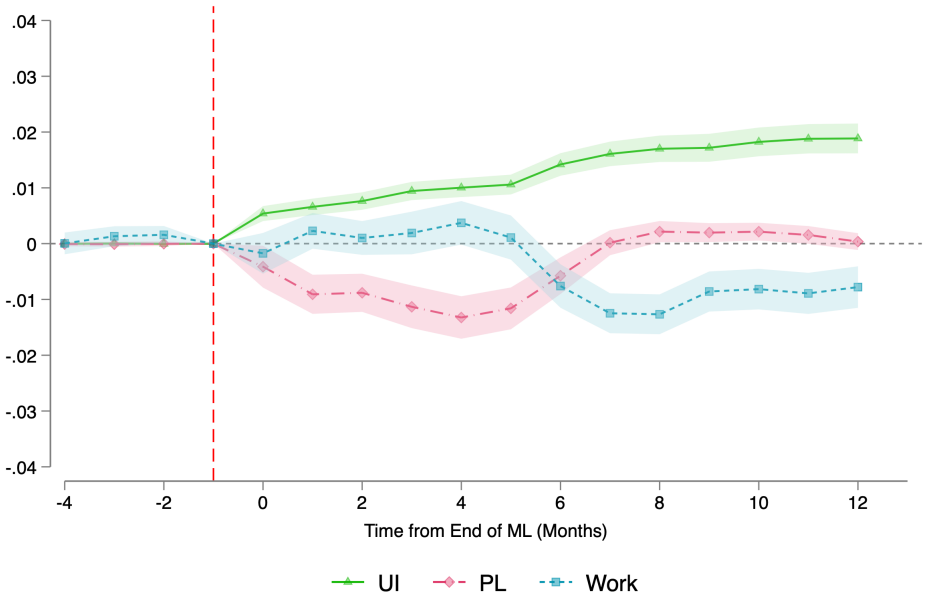
Figure 2: Effects of The Reforms on Options for Mothers



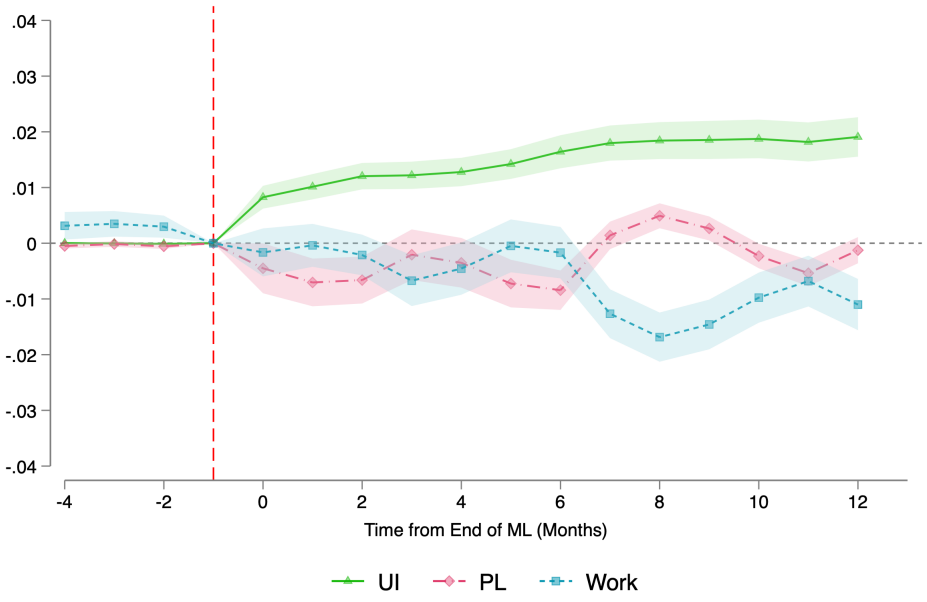

*Notes:* The graphs illustrate how the unemployment insurance reforms impacted the policy space for mothers after childbirth. $t = 0$ corresponds to the time of childbirth. From $t = 3$ onwards women can choose to go back to work, take up PL or take up UI. Panel (a) shows the effect of the replacement 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



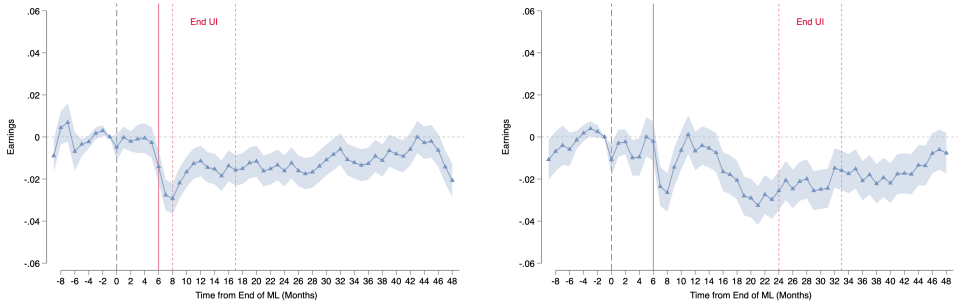

*Notes:* The figure reports difference-in-differences estimates of the coefficients $\beta_k$ from specification 1 estimated on three different dependent variables: the probability that a mother is on UI at relative time $t$ (green series), the probability that a mother is formally employed at relative 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. The three outcomes are not mutually exhaustive, see Section 4 for discussion of the residual category.

Figure 4: Effects of the Reforms on Earnings and Participation

Earnings

(a) Replacement Rate Reform

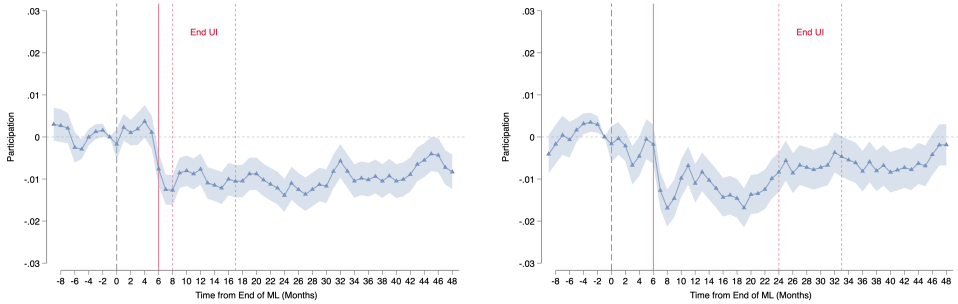
(b) Duration Reform




Employment

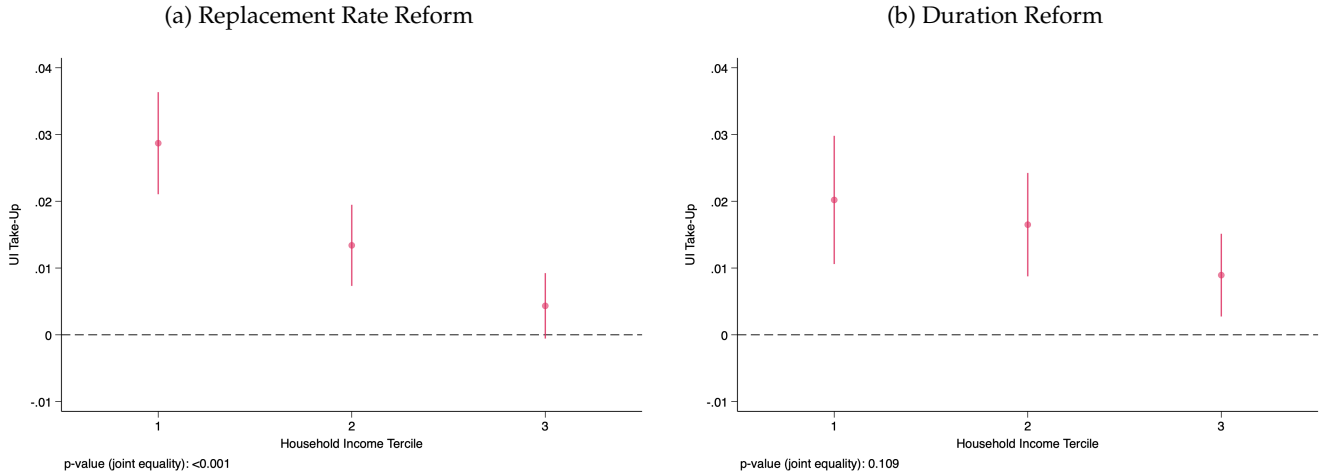
(c) Replacement Rate Reform

(d) Duration Reform



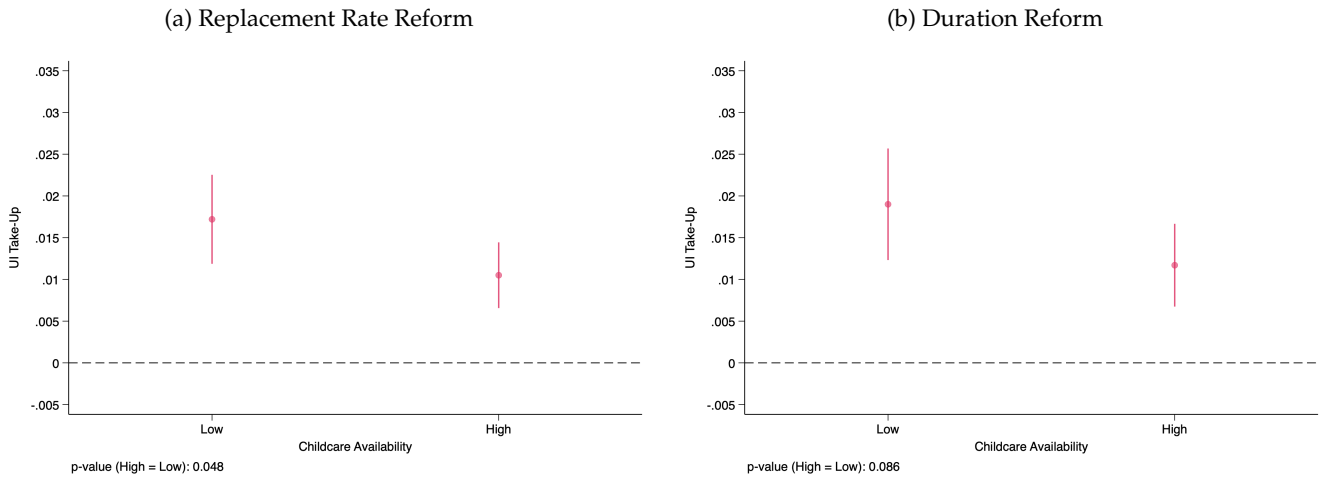

*Notes:* The figures report the estimates of $\beta_k$ coefficients from specification 1 on gross labor earnings in levels, normalized in the following way: $\frac{\hat{\beta}_k}{E\left[\tilde{Y}_{ik}|k,T=1\right]}$, and on the probability of formal employment (Panels (c) and (d)). The normalization allows the coefficients to be interpreted 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". Panels (a) and (c) report results for the replacement-rate reform; Panels (b) and (d) report results for the duration reform.

## Figure 5: The Role of Liquidity Constraints



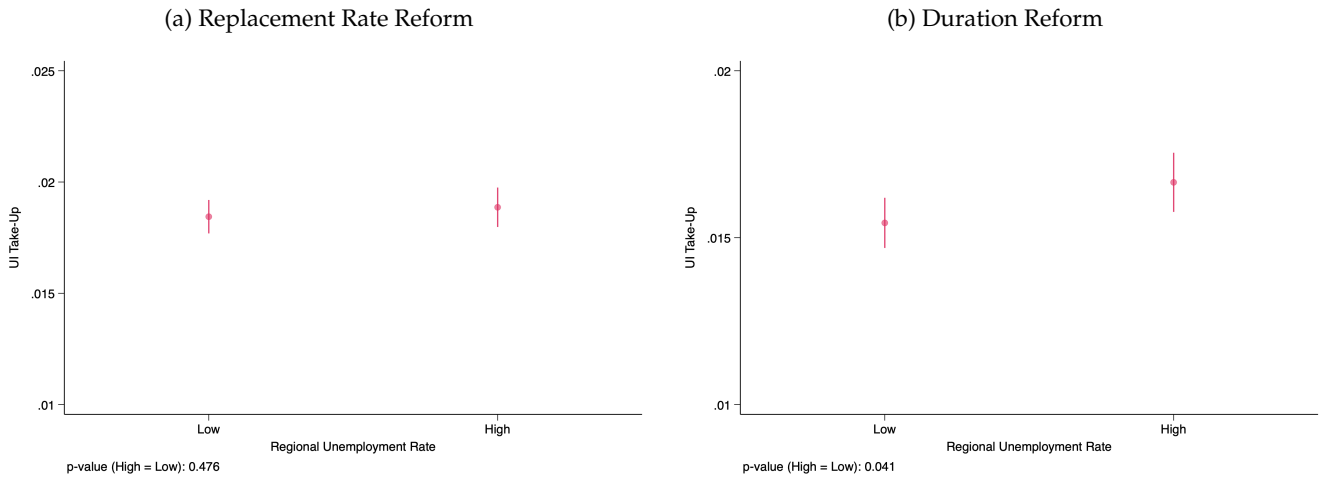

*Notes:* The figure shows the effect of increasing the generosity of unemployment insurance on the take-up of UI in the four years after the end of ML by tercile of household income. The graph reports the estimates of the compact version of specification 1, that collapses the relative time dummies into 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. P-values for joint tests of equality across terciles are reported in the figure.

## Figure 6: The Role of Childcare Availability



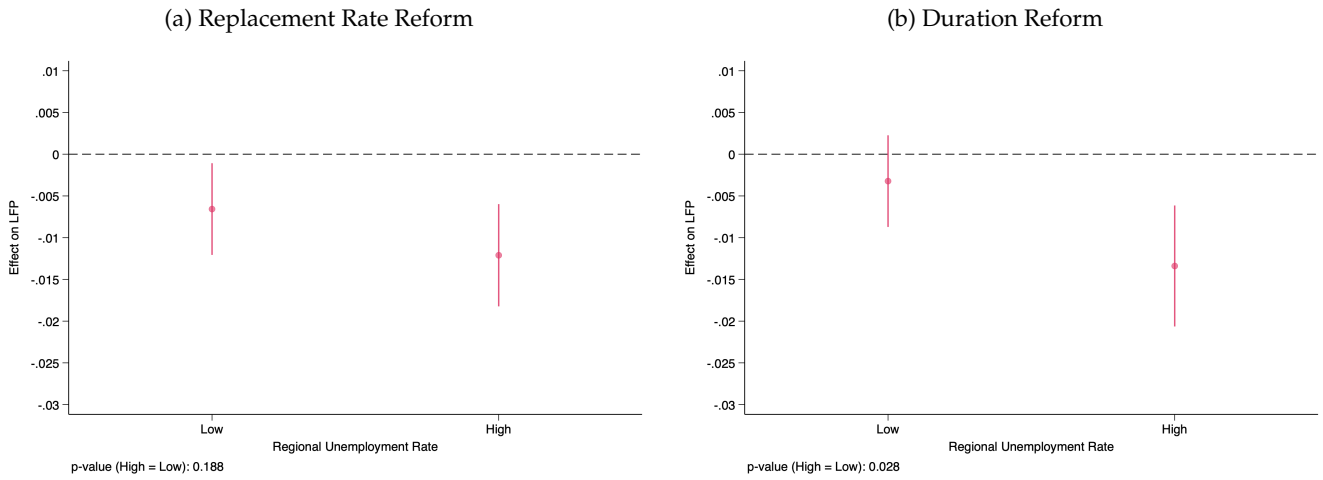

*Notes:* The figure shows the effect of increasing the generosity of unemployment insurance on the take-up of UI in the 4 years after the end of ML by availability of formal childcare for children aged 0-2. Availability of formal childcare is based on data from ISTAT and measured as the percentage of children aged 0–2 that public and private nurseries can accommodate 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. P-values for tests of equality across childcare-availability groups are reported in the figure.

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



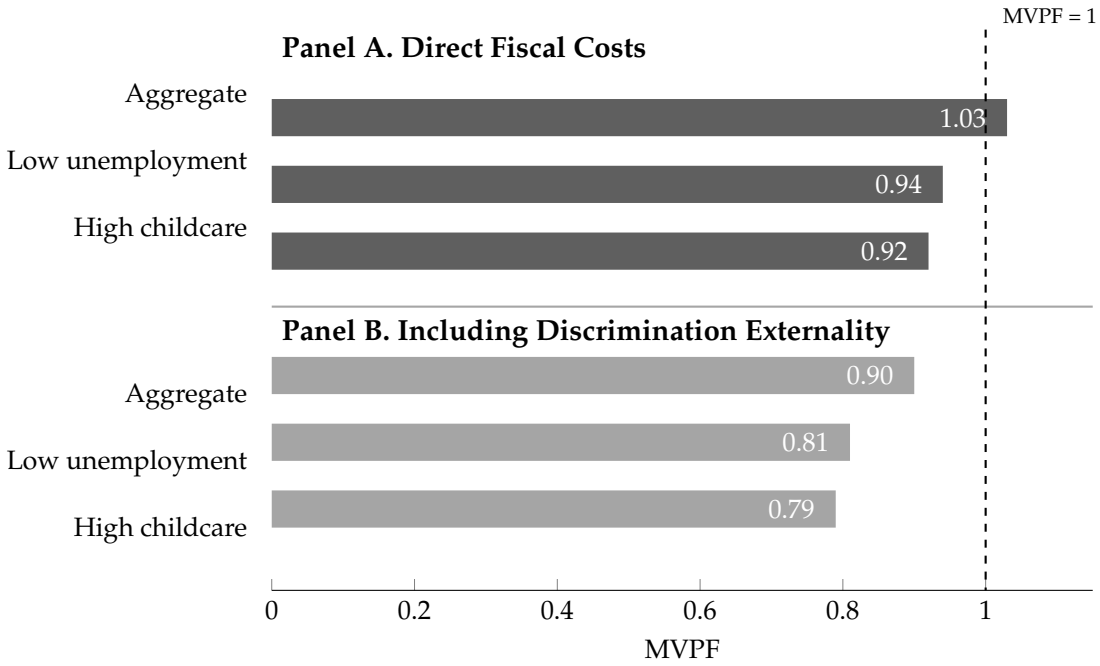

*Notes:* The figure shows the effect of increasing the generosity of unemployment insurance on the take-up of UI in the 4 years after the end of ML by levels of unemployment rate in the region of residence. Regions with levels of unemployment rate above the median are categorized as *High* and regions 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. P-values for tests of equality across unemployment-rate groups are reported in the figure.

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 formal employment in the four 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. P-values for tests of equality across unemployment-rate groups are reported in the figure.

Figure 9: Duration-Reform MVPF Under Alternative Valuation and Cost Scenarios




*Notes:* The figure reports MVPFs for the duration reform under aggregate and conservative benchmark valuations. Panel A uses direct fiscal costs only. Panel B adds the fiscal component of the statistical-discrimination externality. The dashed line marks $MVPF = 1$, where recipient willingness to pay equals net government cost. Table 8 reports the construction of each component.

# Tables

Table 1: Summary Statistics for the Full Sample of Mothers

| | Mean |
|---|---|
| Age | 33.51 |
| | (5.01) |
| Full-time job | 0.65 |
| | (0.48) |
| Permanent contract | 0.93 |
| | (0.26) |
| White collar | 0.60 |
| | (0.49) |
| Blue collar | 0.31 |
| | (0.46) |
| Monthly wage | 1,417.99 |
| | (2,970.30) |
| Monthly earnings | 1,521.19 |
| | (1,280.40) |
| N | 641,607 |

*Notes:* The table reports summary statistics for the full sample of first-time mothers employed in the private sector before childbirth between 2012 and 2016. Variables are measured in the last observed month before the start of compulsory maternity leave. Monetary quantities are expressed in 2010 euros. N indicates the number of mothers in the sample.

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 formally employed, over time. Specifically, it reports the estimates of $\beta_k$ 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. The three outcomes are not mutually exhaustive, see Section 4 for discussion of the residual category.

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 formally employed, over time. Specifically, it reports the estimates of $\beta_k$ 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. The three outcomes are not mutually exhaustive, see Section 4 for discussion of the residual category.

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

| *Months from End of ML* | (1) Gross Labor Earnings | (2) Share Employed | (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 $\beta_k$ 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 euros. 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. N reports the number of mothers in the analysis sample.

Table 5: Effects on Labor Market Outcomes: Duration Reform

| *Months from End of ML* | (1) Gross Labor Earnings | (2) Share Employed | (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 $\beta_k$ 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. N reports the number of mothers in the analysis sample.

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: Marginal Benefit Estimates and Benchmark Valuations

| | Marginal Benefit | Interpretation |
|---|---|---|
| Replacement Rate Reform | 0.43 | Aggregate valuation: liquidity + present bias |
| Duration Reform | 0.34 | Aggregate valuation: baseline |
| Low Unemployment | 0.21 | Lower re-employment risk environment |
| High Childcare | 0.19 | Greater childcare availability environment |

*Notes:* This table reports marginal benefit (MB) estimates calculated using equation 2 from Section 5. The first two rows report aggregate MB estimates for the replacement-rate and duration reform analysis samples. 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 conservative benchmark valuations rather than causal decompositions, since regional characteristics are not randomly assigned. They describe revealed valuations in environments where particular constraints or risks are plausibly less severe.

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** | **1.30** |
| **Panel (a): Baseline** | | |
| MB | 0.43 | 0.34 |
| 1+MB | 1.43 | 1.34 |
| **MVPF = (1+MB)/(1+FE)** | **1.01** | **1.03** |
| **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 €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 MVPFs 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 3.2. 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. Job-protected parental leave, denoted by $p = 0$. The mother remains attached to her pre-birth employer and receives baseline benefits $B$.

2. Unemployment insurance, denoted by $p = 1$. The mother gives up job protection and receives benefits $B + b$, where $b$ is the additional UI benefit relative to the baseline.

Let $V(p; b)$ denote the value of program choice $p$, excluding the cost of job separation. In the parental-leave setting, $V(p; b)$ should be interpreted broadly: it may include resources during leave, supported time at home with an infant, and relief from childcare constraints.

In the consumption-only case:

$$V(p; b) = u(c(p; b))$$

where $u(\cdot)$ is strictly increasing and concave and consumption is

$$c(p; b) = y(p) + B + \mathbf{1}\{p = 1\}b$$

Income $y(p)$ may depend on the program choice. Under job-protected parental leave, $y(0)$ reflects earnings associated with returning to the pre-birth job. Under unemployment insurance, $y(1)$reflects earnings after job separation. The reduced-form consequences of this choice for employment and earnings are estimated in Section 4.

Let $\theta \sim F$ denote the mother's type, and let $\phi(\theta)$ be the net cost of giving up the job and losing job protection. This cost may reflect search frictions, loss of firm-specific human capital, uncertainty about re-employment, and other pecuniary or non-pecuniary costs of separating from the pre-birth employer.

The full utility is

$$U(p;b) = V(p;b) - \mathbf{1}\{p = 1\}\phi(\theta)$$

This notation separates the value of the program option from the cost of accessing UI through job separation.

**Decision rule** A mother chooses UI if

$$U(1;b) \geq U(0;b)$$

or equivalently if

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

Define the threshold

$$\bar{\phi}(b) = V(1;b) - V(0;b)$$

Mothers with $\phi(\theta) \leq \bar{\phi}(b)$ choose UI, while mothers with $\phi(\theta) > \bar{\phi}(b)$ remain attached to their pre-birth employer through job-protected parental leave.

Let $\Phi(b)$ denote the job-protected parental-leave participation rate:

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

An increase in UI generosity raises $\bar{\phi}(b)$, making UI attractive to mothers with higher perceived costs of job separation.

## A.2 Identification of Marginal Value

For the marginal mother the threshold condition is:

$$\bar{\phi}(b) = V(1;b) - V(0;b)$$

Differentiating the threshold with respect to UI generosity gives: $\frac{d\bar{\phi}(b)}{db} = V_b(1;b)$,where $V_b(1;b)$ is the marginal value of UI generosity in the post-childbirth state. In the consumption-only case, where $V(1;b) = u(c(1;b))$ and $c(1;b) = y(1) + B + b$, this becomes:

$$V_b(1;b) = u'(c(1;b))$$

The parental-leave participation rate responds to benefit generosity according to:

$$\frac{d\Phi(b)}{db} = -f(\bar{\phi}(b))\frac{d\bar{\phi}(b)}{db} = -f(\bar{\phi}(b))V_b(1;b)$$

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

$$V_b(1;b) = \frac{|d\Phi(b)/db|}{f(\bar{\phi}(b))}$$

The decline in participation in response to UI generosity therefore identifies the marginal value of UI generosity, scaled by the density of mothers at the threshold.

**Cross-regime comparison** To recover the marginal benefit, I compare the marginal value of UI generosity across low- and high-benefit regimes. Let $b^{low}$ denote the low-benefit regime and $b^{high}$ the high-benefit regime, with $b^{high} > b^{low}$. The marginal benefit is:

$$MB = \frac{V_b(1;b^{low})}{V_b(1;b^{high})} - 1$$

In the consumption-only case, this becomes:

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

This expression compares the marginal value of UI generosity in the low-benefit regime to the marginal value in the high-benefit regime. For mothers responding to the reform on the UI margin, the relevant counterfactual is taking UI under the less generous regime. The reform changes resources on the UI margin, and the behavioral response reveals the value of that change.

**Expressing MB in terms of observable moments** The identification result applies in each regime:

$$V_b(1;b^s) = \frac{|d\Phi^s/db|}{f(\bar{\phi}^s)}, \qquad s \in \{low, high\}$$

Substituting this expression into the marginal-benefit formula gives:

$$MB = \frac{\frac{|d\Phi^{low}/db|}{f(\bar{\phi}^{low})}}{\frac{|d\Phi^{high}/db|}{f(\bar{\phi}^{high})}} - 1$$

Define the elasticity of participation with respect to UI generosity as:

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

Because$\Phi^s$ falls when UI generosity rises, this elasticity is negative. Therefore,

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

Substituting into the expression for $MB$ yields

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

Rearranging gives:

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

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 differences across regimes in the density-adjusted participation elasticity.

**Empirical Implementation: Setting $\kappa = 1$** For the empirical implementation, in the spirit of Fadlon and Nielsen (2019) I set $\kappa = 1$. This assumption requires the density-weighted semi-elasticity $\frac{|\varepsilon(\Phi^s, b^s)|}{f(\bar{\phi}^s)}$ to be approximately constant across the low- and high-benefit regimes.

Two features of the setting make this approximation plausible. First, the reforms are moderate in size relative to the support of $\phi$. The implied threshold shift moves the participation rate $\Phi$ by roughly one percentage point from a baseline of 65%, suggesting that $\bar{\phi}^{low}$ and $\bar{\phi}^{high}$ lie close to one another in the support of $F$. Under local smoothness of $f$, this implies$f(\bar{\phi}^{low}) \simeq f(\bar{\phi}^{high})$. Second, the elasticity of participation with respect to benefits is similar in magnitude across the two reforms (see Table A4), suggesting that $|\varepsilon^{low}| \simeq |\varepsilon^{high}|$as well. As shown in Appendix A of Fadlon and Nielsen (2019), these conditions correspond to a local approximation to the participation function and become less plausible 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. This expression is derived following the same sufficient-statistics approach in Hendren (2017). The behavioral response to UI generosity identifies the marginal value of additional benefits within the post-childbirth state. In the parental-leave setting, $V_b(1; b)$ should be interpreted as the revealed value of additional post-birth support, not as the value of cash transfers alone.

# B Complier Characteristics

This appendix describes how I estimate the characteristics of mothers induced to take up unemployment insurance by each reform. Consider a binary variable $Z$ in $\{0,1\}$ that captures whether a mother is eligible for the more generous UI regime, with $Z = 1$ for the eligible cohort and $Z = 0$ for the ineligible cohort. Let $D \in \{0,1\}$ indicate whether a mother takes up UI during the choice period, defined as the first nine months after the end of compulsory maternity leave. Let $D_0$ and $D_1$ denote potential UI take-up under the less generous and more generous regimes, respectively.

Under monotonicity, $D_1 \geq D_0$, mothers can be divided into three groups. Always-takers have $(D_0 = 1, D_1 = 1)$ and take up UI under both regimes. Never-takers have $(D_0 = 0, D_1 = 0)$ and do not take up UI under either regime. Compliers have $(D_0 = 0, D_1 = 1)$ and are induced to take up UI by the more generous regime. I do not observe both potential outcomes for the same mother, but the quasi-random timing of reform eligibility allows me to infer group shares and characteristics by comparing eligible and ineligible cohorts.

**Estimating group shares** For each reform, I estimate group shares using UI take-up during the choice period. In the two-cohort comparison, the always-taker share is the UI take-up rate in the ineligible cohort,$\hat{\pi}_{AT} = \hat{E}[D \mid Z = 0]$, and the complier share is the increase in UI take-up in the eligible cohort,$\hat{\pi}_C = \hat{E}[D \mid Z = 1] - \hat{E}[D \mid Z = 0]$. The never-taker share is the residual: $\hat{\pi}_{NT} = 1 - \hat{\pi}_{AT} - \hat{\pi}_C$.

Equivalently, these shares can be recovered from a regression of UI take-up on an indicator for eligibility:

$$D_i = \alpha + \gamma Z_i + \varepsilon_i,$$

where $\hat{\alpha}$ estimates $\pi_{AT}$, $\hat{\gamma}$ estimates $\pi_C$, and $1 - \hat{\alpha} - \hat{\gamma}$ estimates $\pi_{NT}$. In the implementation with the full difference-in-differences design, the corresponding group shares are recovered from the appropriate treatment effect on UI take-up.

**Estimating group characteristics** Let $x_i$be a pre-determined characteristic measured before childbirth. I estimate the expected value of $x_i$ separately for always-takers, compliers, and never-takers. Under monotonicity, mothers in the eligible cohort who do not take up UI are never-takers. Their characteristics are estimated by:

$$\hat{E}_{NT}[x] = \hat{E}[x \mid D = 0, Z = 1]$$

Always-takers are mothers who would take up UI even under the less generous regime. Their characteristics are estimated using UI takers in the ineligible cohort:

$$\hat{E}_{AT}[x] = \hat{E}[x \mid D = 1, Z = 0]$$

Mothers who take up UI in the eligible cohort are a mixture of always-takers and compliers:

$$\hat{E}[x \mid D = 1, Z = 1] = \frac{\pi_{AT}}{\pi_{AT} + \pi_C} E_{AT}[x] + \frac{\pi_C}{\pi_{AT} + \pi_C} E_C[x]$$

Solving for the complier mean gives the estimable formula:

$$\hat{E}_C[x] = \frac{\hat{\pi}_{AT} + \hat{\pi}_C}{\hat{\pi}_C} \hat{E}[x \mid D = 1, Z = 1] - \frac{\hat{\pi}_{AT}}{\hat{\pi}_C} \hat{E}[x \mid D = 1, Z = 0]$$

This expression subtracts the estimated contribution of always-takers from the characteristics of UI takers in the eligible cohort. The identifying assumption is that, absent the reform, always-takers in the eligible cohort would have the same expected characteristics as UI takers in the ineligible cohort. This follows from the quasi-random assignment of reform eligibility based on the date of end of compulsory maternity leave, together with the monotonicity assumption.

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.

**Interpretation** The complier characteristics are descriptive. They identify the observable characteristics of mothers induced to take up UI by each reform under the maintained monotonicity and quasi-random assignment assumptions. They provide context for interpreting the reform-specific responses and marginal-benefit estimates. In particular, differences between replacement-rate and duration-reform compliers help assess whether the two reforms induced similar groups of mothers to substitute from job-protected parental leave to UI.

# C Quantifying the Fiscal Component of the Statistical-Discrimination Externality

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 five 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.** 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 mar-

gin than the baseline rate. Using the natural rate as the baseline therefore understates the discrimination response.

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 33% 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).

## C.3 Calculation

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

$$Annual\ earnings\ loss = 0.215 \times e25,380 = e5,457$$

**Step 2: Annual fiscal loss** Applying the 33% tax wedge to the earnings loss:

$$Annual\ fiscal\ loss = 0.33 \times e5,457 = e1,801$$

**Step 3: Cumulative fiscal loss over the persistence horizon** Cumulating the annual fiscal loss over two years, using an annual factor of 0.95:

$$Cumulative\ fiscal\ loss = e1,801 \times (1 + 0.95) = €3,512$$

**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):

$$Externality\ per\ e\ UI = (0.69 \times 0.061 \times €3,512)/e671 = 0.21$$

## C.4 Sensitivity and Robustness

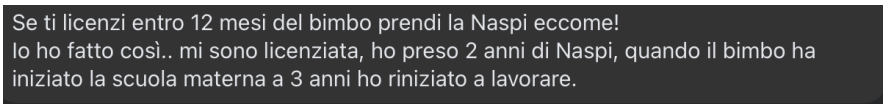
The estimate of €0.21 per euro of UI transfer is robust to alternative reasonable assumptions about the key inputs. Table C.1 reports the externality under alternative specifications. The baseline estimate of €0.21 lies in the lower-middle of this range and is conservative on multiple dimensions: it uses the natural rather than reform-induced contract mix; it applies the employee-side rather than total tax wedge; and it assumes the earnings penalty does not persist beyond two years. The calculation also captures only the direct earnings differential, omitting additional fiscal costs from higher unemployment-spell frequency among young women on temporary contracts and any welfare costs borne by the affected workers themselves. The true externality is therefore plausibly larger than €0.21.

# D Additional Figures

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

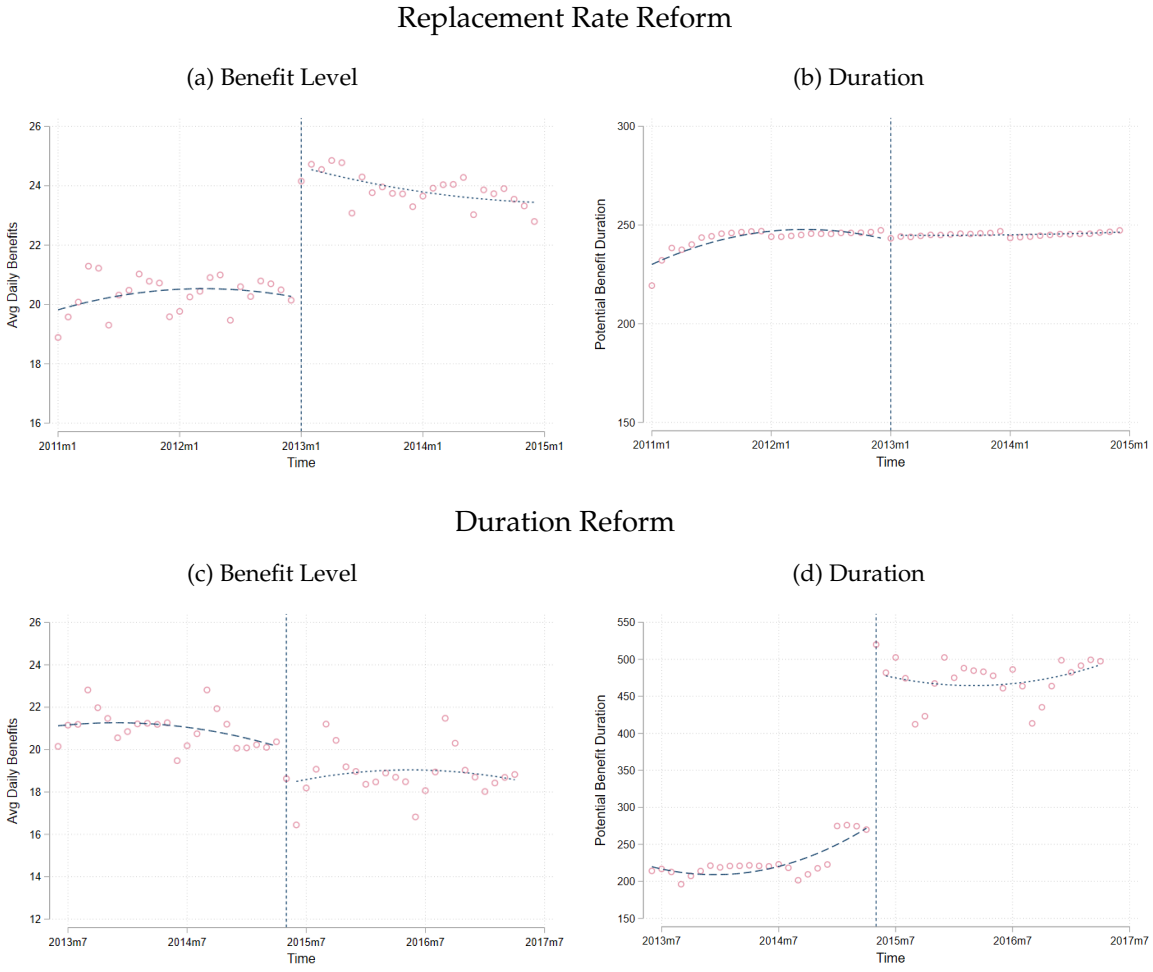
(a) Commercial service marketing resignations and UI as PL


(b) Example of UI used as PL



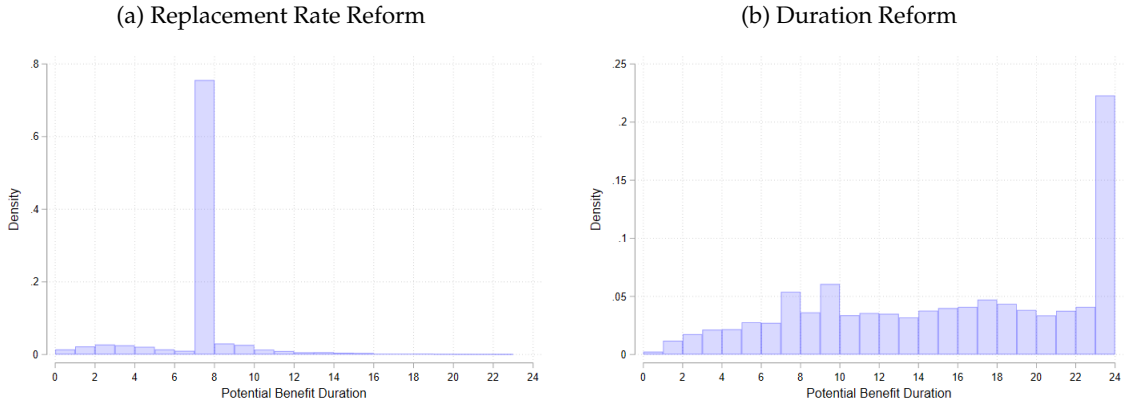

*Notes:* Panel (a) shows an example of a website advertising the possibility of using unemployment insurance as an alternative to parental leave benefits. Panel (b) shows a comment in a popular online forum for mothers and expectant mothers describing this use of UI. The translation is: "If you resign 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



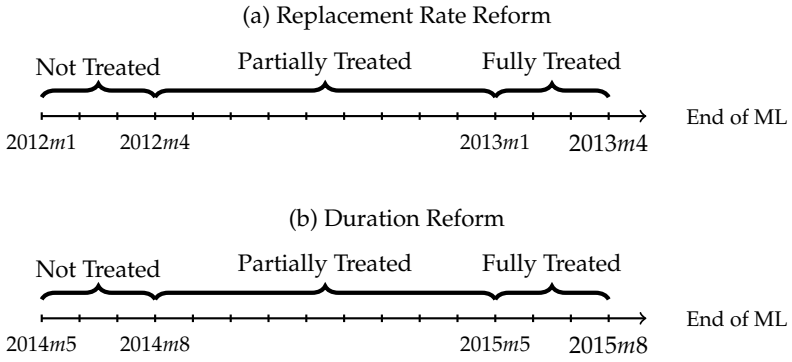

*Notes:* The graphs show the average daily unemployment insurance benefits and the average potential benefit duration (in days) by month-of-start of unemployment spell bin for women younger than 50 years old with a permanent contract. Panel (a) and (b) show the effects of the introduction of the replacement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



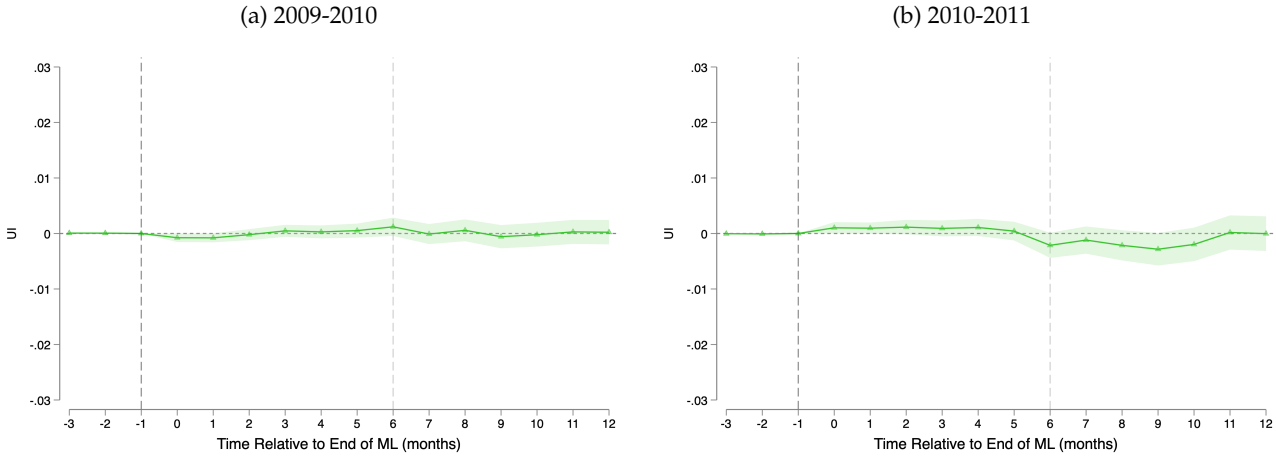

*Notes:* The figure shows the distribution of potential benefit duration of unemployment insurance after the replacement rate reform (Panel (a)) and after the duration reform (Panel (b)).

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



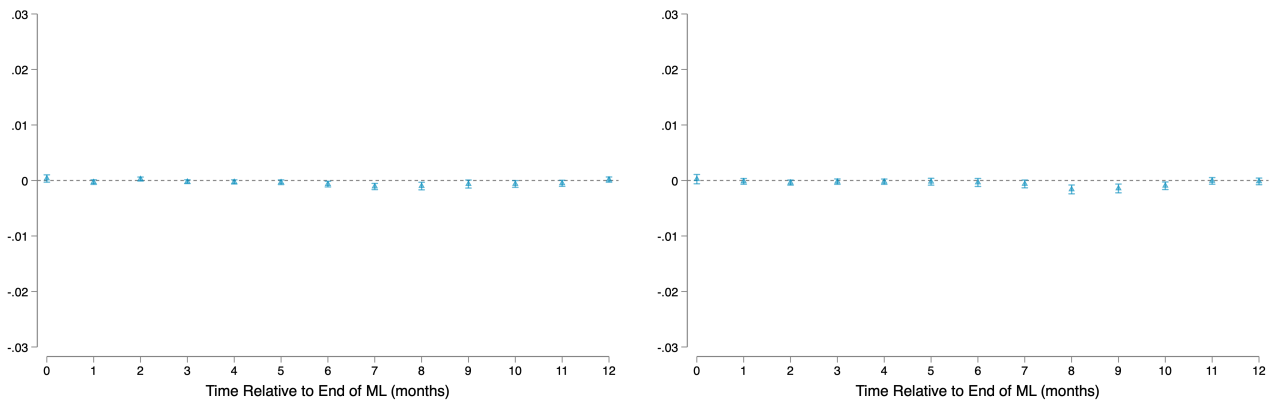

*Notes:* The figure illustrates for which dates the mothers were eligible for different unemployment insurance regimes, depending on the end of their maternity leave. Panel (a) shows the dates for the replacement rate reform. Panel (b) shows the dates for the duration reform.

## Figure A5: Placebo Specifications



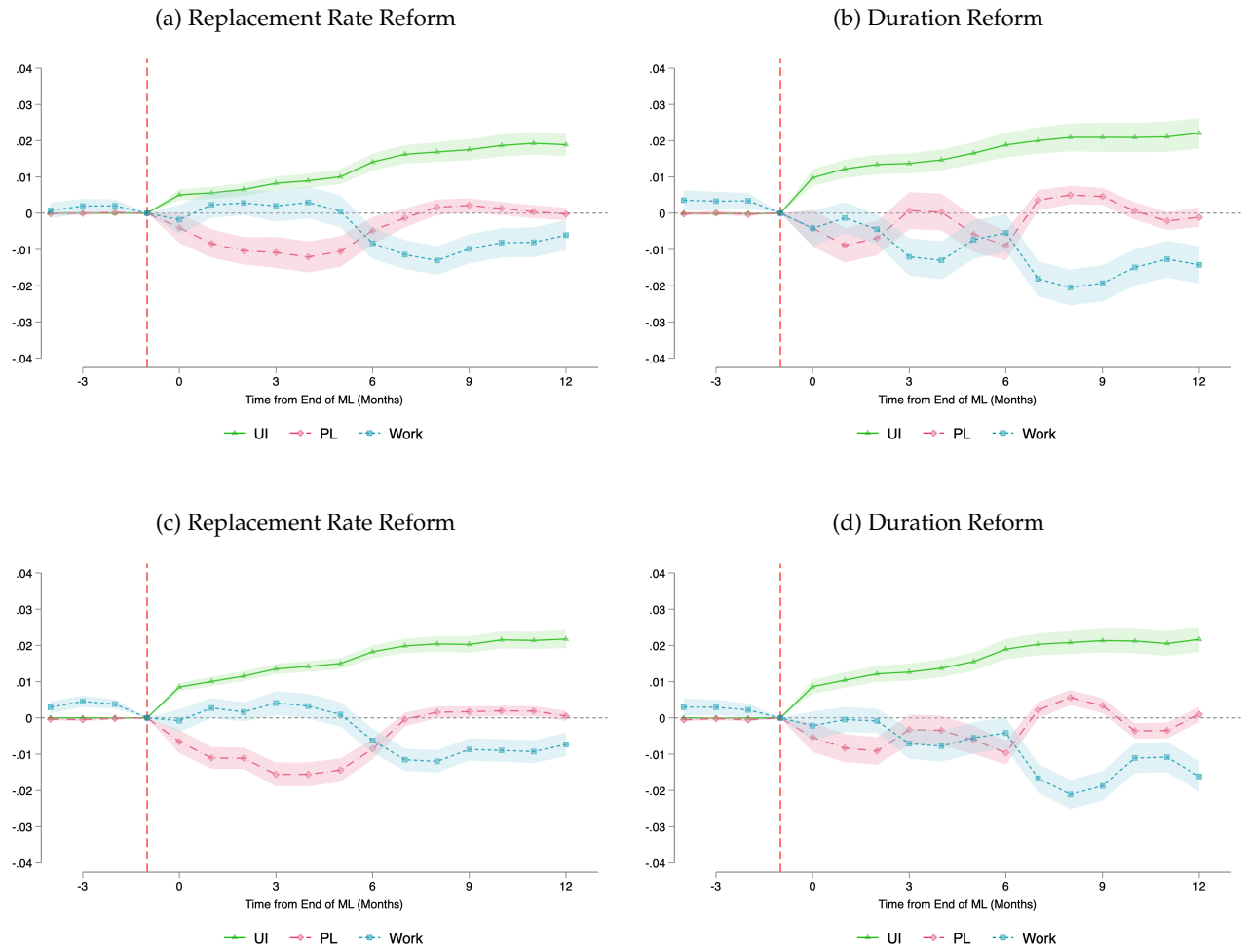

*Notes:* The figure reports difference-in-differences estimates of the coefficients $\beta_k$ from 1 where the dependent variable is the probability that a mother is on unemployment insurance at relative time $t$ (where $t = 0$ corresponds to the time of end of compulsory maternity leave) for different 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 figure reports difference-in-differences estimates of the coefficients $\beta_k$ 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 $\beta_k$ 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 formally employed 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) | |
| N | 120,064 | 96,658 | |

*Notes:* The table reports summary statistics for mothers in the combined reform analysis sample who cannot be linked to partners (Column 1) and mothers who can be linked to partners (Column 2). The combined reform analysis sample includes mothers in either the replacement-rate or duration reform sample. Column 3 reports the difference in means between the two groups, with significance from a t-test of equality of means. Variables are measured in the last observed month before the start of compulsory maternity leave. Monetary quantities are expressed in 2010 euros.

Table A2: Effects on Subsequent Births

| | Subsequent birth by | | |
|---|---|---|---|
| | 12 months | 24 months | 48 months |
| *Panel A: Replacement-rate reform* | | | |
| Reform effect | 0.003 | -0.026*** | -0.128*** |
| | (0.004) | (0.010) | (0.022) |
| N | | 9,154,640 | |
| *Panel B: Duration reform* | | | |
| Reform effect | -0.002 | 0.001 | -0.031 |
| | (0.005) | (0.015) | (0.034) |
| N | | 6,664,400 | |

*Notes:* The table reports difference-in-differences estimates of the effect of each reform on subsequent births among mothers who return to formal employment, where subsequent births are observable in the administrative data. The estimates come from the compact version of specification 1, that collapses the relative time dummies into a single indicator for the post-end of ML period. Outcomes are indicators for having an additional birth within 12, 24, or 48 months. Coefficients and standard errors are reported in percentage points. Robust standard errors are in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels. N reports the number of mother-month observations. Note that the compact specification uses all available observations per mother, not just those in a narrow event-study window.

Table A3: Effects on Partners' Employment and Parental-Leave Take-Up

| | Partner employed | | Partner on PL | |
|---|---|---|---|---|
| Months after maternity leave | RR reform | Duration reform | RR reform | Duration reform |
| 1 | -0.430*** | 0.051 | 0.034 | -0.027 |
| | (0.095) | (0.121) | (0.032) | (0.044) |
| 2 | -0.503*** | 0.261* | 0.034 | -0.076 |
| | (0.111) | (0.137) | (0.037) | (0.051) |
| 3 | -0.237** | 0.276* | 0.081** | 0.004 |
| | (0.116) | (0.142) | (0.040) | (0.053) |
| 5 | -0.031 | 0.467*** | 0.044 | 0.011 |
| | (0.124) | (0.154) | (0.041) | (0.049) |
| 6 | -0.017 | 0.863*** | -0.001 | -0.011 |
| | (0.129) | (0.161) | (0.043) | (0.050) |
| 9 | -0.011 | 0.613*** | 0.033 | 0.050 |
| | (0.139) | (0.173) | (0.039) | (0.047) |
| 12 | 0.247* | 0.352** | 0.060 | -0.009 |
| | (0.144) | (0.176) | (0.037) | (0.051) |
| Partner-month observations | 2,132,824 | 1,552,259 | 2,132,824 | 1,552,259 |

*Notes:* The table reports difference-in-differences estimates of the effect of each reform on partners' employment and parental-leave take-up in the linked-household sample. Estimates are selected event-study coefficients from specification 1. Coefficients and standard errors are reported in percentage points. Robust standard errors are in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

Table A4: Parameters for Marginal Benefits Calculations

| | Replacement Rate Reform | Duration Reform |
|---|---|---|
| *Behavioral Response* | | |
| $\Phi^{low}$ | 0.628 | 0.610 |
| $\Delta\Phi$ | $-0.0104$ | $-0.0088$ |
| $\Phi^{low}/\Phi^{high}$ | 1.017 | 1.015 |
| *Benefit Generosity* | | |
| $b^{low}$ | €4,599 | €6,006 |
| $\Delta b$ | €1,851 | €1,906 |
| $b^{high}/b^{low}$ | 1.40 | 1.32 |
| *Marginal Benefit* | | |
| MB | **0.43** | **0.34** |

*Notes:* The table reports the parameters used to calculate marginal benefit estimates using equation 2. Participation rates measure the share of mothers working, averaged over the 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 factor of 0.989. The marginal benefit calculation assumes $\kappa = 1$ and uses formula 2. See Appendix A for derivation and discussion of this assumption. All monetary values are in 2010 euros.

Table A5: Sensitivity of the Discrimination Externality

| Specification | Externality per € UI |
|---|---|
| **Baseline** | **€0.21** |
| 1-year persistence | €0.12 |
| 4-year persistence (matching UI accounting window) | €0.40 |
| Regression-adjusted earnings gap of 16% (ISTAT 2019) | €0.16 |
| Total tax wedge of 45% (OECD convention) | €0.30 |
| 100% fixed-term share among marginal hires | €0.32 |

*Notes:* The table reports the discrimination externality per euro of UI transfer under alternative assumptions. The baseline specification uses a 33% tax wedge and a two-year persistence horizon for the earnings penalty, the 21.5% fixed-term–permanent earnings gap from ISTAT (2016), the 69% baseline share of new female hires on fixed-term contracts, the 6.1% firm-level hiring response from Carta et al. 2024, and an annual discount factor of 0.95. Alternative specifications vary one input at a time relative to the baseline. The total tax wedge of 45% follows the OECD convention and includes employer-side social security contributions. All monetary values are in 2010 euros.