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Agar Brugiavini

Child Penalties and Public Childcare Shortages under Fiscal Austerity

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Abstract

There is ample debate on the policy levers that might affect the child penalty in women's earnings. Studies on large public childcare expansions have yielded mixed results. Contexts with low supply remain overlooked, as do fiscal constraints which might hinder service provision. Research has also concentrated on average effects although access to formal childcare is unequally distributed across income groups and geographical areas. Few studies, in addition, have examined the interplay between public childcare and other policies like parental leave.

Focusing on Italy, we link child penalties to local fiscal rules and the supply of public childcare over a fifteen-year period (2001-2015). We rely on administrative data on private employees complemented by municipal budget records, and combine a regression discontinuity design with event studies. Leveraging spatial variation in fiscal constraints, we find that municipalities subjected to austerity face shortages driven by fewer daycare slots and (accepted) applications. Effects on the child penalty are null in country-wide analyses. In Italy's Centre, where daycare is more widespread and relative shortages are more pronounced, the child penalty increases by up to a quarter among women with higher earnings, likely as a result of shorter weeks on part-time arrangements. Use of parental leave also grows in response to shortages, albeit among women with lower earnings and without fuelling extra earnings losses.

Research and policies attending to income and geographical disparities might be best suited to understand and remedy gendered economic inequalities spurred by shortages and otherwise limited daycare.

Keywords: public childcare, child penalty, fiscal rules, austerity, parental leave
JEL codes: H31, J03, J13, J18

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Prospettive lavorative dopo la maternità e carenze nell'offerta pubblica di asili nido in regime di austerità fiscale

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Le politiche pubbliche a sostegno dell'occupazione femminile, specie dopo la nascita di un figlio, sono ampiamente dibattute. Alcuni studi sono stati capaci di isolare gli impatti medi di ampie espansioni dei servizi pubblici per l'infanzia. Scarseggiano invece le indagini in contesti dove gli asili tendono a mancare, per esempio, a causa di vincoli di bilancio. Quando incentrate su effetti medi, inoltre, le analisi possono nascondere importanti disparità a seconda del reddito o dell'area geografica. Infine, poche ricerche hanno analizzato le interazioni tra l'offerta pubblica di asili nido e altre politiche come il congedo parentale.

Questo lavoro evidenzia come le regole fiscali a livello locale possano influire sull'offerta pubblica di asili nido e, di conseguenza, sugli esiti occupazionali delle donne, esaminando il contesto italiano tra il 2001 e il 2015. Ci basiamo su dati amministrativi sui dipendenti privati e dati provenienti dai bilanci municipali, combinando due disegni econometrici adatti alla stima di effetti causali. Sfruttando la variazione geografica nell'applicabilità del Patto di Stabilità Interno, i nostri risultati mostrano che i comuni più soggetti ai vincoli di bilancio faticano a mantenere asili nido alla pari con gli altri comuni, specie in termini di posti disponibili e numero di domande. Questa discrepanza nei servizi non sembra influire sui redditi da lavoro o altri aspetti della partecipazione lavorativa delle donne quando analizzati nel loro complesso. Nelle regioni centrali, tuttavia, una carenza relativa nei servizi per l'infanzia è associata a un calo ulteriore dei redditi da lavoro negli anni successivi alla prima maternità. Questo impatto riguarda esclusivamente le donne con redditi relativamente alti e sembra essere trainato da settimane più corte con contratti part-time. Quando l'offerta di asili è più carente a causa del Patto, l'utilizzo del congedo parentale cresce, ma senza influenzare le traiettorie salariali.

I vincoli di bilancio comunali possono pertanto dare luogo a carenze nell'offerta di asili nido e influire così sulle disuguaglianze di genere nel mercato del lavoro, seppure con effetti concentrati sulle lavoratrici con redditi alti e in zone dove la copertura dei servizi è generalmente superiore alla media nazionale, quantomeno nel caso italiano.

Parole chiave: asili nido, *child penalty*, regole fiscali, austerità, congedo parentale
Codici JEL: H31, J03, J13, J18

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[3]La realizzazione del presente articolo è stata possibile grazie alle sponsorizzazioni e le erogazioni liberali a favore del programma VisitINPS Scholars. I risultati e le conclusioni espressi sono esclusivamente degli autori e non rappresentano le opinioni dell'INPS.

Following the birth of a child, women can lose up to two-thirds of their labour income compared to men and women who are child-free or childless (Kleven et al., 2025). Europe features one of the largest average child penalties compared to other regions of the world, with estimates ranging from 21-26% in Denmark and Sweden, 28-38% in Spain, around 50% in Austria and the Czech Republic, and exceeding 60% in Germany and Italy (Kleven, Landais, Posch, et al., 2019; De Quinto et al., 2021; Casarico & Lattanzio, 2023). These penalties are a key driver of persistent disparities in labour markets, and of gender and economic equality more broadly (e.g., Gangl & Ziefle, 2009; Angelov et al., 2016; Harkness, 2016; Adda et al., 2017; Kleven, Landais, & Søgaard, 2019; Gonalons-Pons & Marinescu, 2024). Despite a wealth of research on their magnitude and consequences, however, there is little consensus on whether and how (much) these penalties can be tempered, undone or exacerbated by state policies (cf., Olivetti & Petrongolo, 2017; Brady et al., 2020). Lately, research has suggested that even intense rafts of reform aimed at family policies might have a negligible impact on the child penalty (e.g., Kleven et al., 2024).

Here we revisit the question of policy effects on the child penalty by focusing on public childcare. Correlational evidence suggests that child penalties and other gender gaps in labour markets are smaller when public childcare coverage is more pervasive, across and within countries (Olivetti & Petrongolo, 2017; Rabaté & Rellstab, 2022; Casarico & Lattanzio, 2023). Findings from causal designs are a mixed bag, though, showing that child penalties are either unaffected (Rabaté & Rellstab, 2022; Kleven et al., 2024) or attenuated (Krapf et al., 2020; Andresen & Nix, 2022; Karademir et al., 2024) when public childcare supply is expanded. Contexts where major service expansions have not occurred remain outside of the literature's remit, and the external validity of studies in high-coverage settings might be limited (Nieuwenhuis et al., 2025). Besides, previous analyses have centred around average effects even though daycare access and its implications (for adults and children) are renowned to be unequal along the income distribution (e.g., Pavolini & Van Lancker, 2018; Van Huizen & Plantenga, 2018; Gonalons-Pons & Marinescu, 2024; for an exception, Krapf et al., 2020). Finally, research has seldom examined the interplay of different family policies, as in whether parental leave uptake might increase when formal childcare is lacking (or vice versa; cf., Lalive et al., 2014; Kleven et al., 2024).

Complementing prior research, we offer one of the first studies of childcare services under strain rather than expansion. Further, we highlight heterogeneous policy effects depending on women's relative earnings, including compensatory responses to the lack of public childcare via parental leave uptake. We focus on Italy, where child penalties stand out in comparative perspective (Casarico & Lattanzio, 2023; Kleven et al., 2025) and the socio-economic divide in access to

formal childcare services is also wide. More well-off families are around twenty percentage points more likely to rely on these services compared to their less affluent counterparts, despite eligibility rules often favouring the latter (Brilli et al., 2017; Pavolini & Van Lancker, 2018). Overall, public services cover a little more than one in ten children aged 0-2, although the availability of centre-based care is relatively more prominent in the richer regions of Italy's North and Centre (Del Boca et al., 2005; Carta & Rizzica, 2018; Casarico & Lattanzio, 2023). The combination of large child penalties, wide gaps in access to formal childcare, and limited and dispersed coverage distinguishes Italy from other contexts examined by previous research (cf., Rabaté & Rellstab, 2022; Kleven et al., 2024).

We link the literature on the child penalty and centre-based childcare to that on the social and economic impacts of fiscal rules (e.g., Grembi et al., 2016; Fetzer, 2019; Pavese & Rubolino, 2024; Neimanns & Bremer, 2024). Despite their influence on public childcare provision (Bianchi et al., 2023; Neimanns & Bremer, 2024), local fiscal policies have been relatively under-researched in studies on women's labour market opportunities. We consider fiscal rules introduced in Italy in the early 2000s that constrained public expenditures in municipalities with more than 5,000 inhabitants (e.g., Grembi et al., 2016; Pavese & Rubolino, 2024). Combining a regression discontinuity design with event studies, our analyses leverage this policy cut-off to ascertain if and how public childcare provisions respond to fiscal rules, and how women's labour market outcomes are affected, in turn, across the socio-economic divide. We answer these questions by relying on social security records for the universe of Italian private employees and rich municipal-level information on centre-based childcare.

1 Background

1.1. Why (and for whom) should centre-based childcare affect the child penalty?

The impact of formal childcare provisions on the child penalty is multifaceted. On the one hand, centre-based childcare might free up time for parents to spend in paid work. Availability is key[1], with studies showing that women's employment chances and work hours increase when nurseries are instituted or their opening hours are extended (e.g., Nollenberger & Rodríguez-Planas, 2015; Carta & Rizzica, 2018; Andresen & Havnes, 2019; Brewer et al., 2022; see Hegewisch & Gornick, 2011; Morrissey, 2017 for earlier surveys). Parents might also rely on daycare to combine paid and unpaid work without resorting to job changes that might prove costly after the birth of

[1]Here and in the remainder, we focus on the availability of centre-based care. For a review of studies concentrating on daycare costs and related subsidies see Morrissey (2017).

a child. Switching to a part-time position or moving to a new employer or occupation is often combined with less remunerative contractual arrangements, lower hourly wages, or flat wage-experience profiles – all factors known to contribute to the child penalty (e.g., Adda et al., 2017; Kleven, Landais, Posch, et al., 2019; Casarico & Lattanzio, 2023; Collischon et al., 2025).

Yet part of the literature has issued caution against straightforward conclusions on the correlation between women's labour market outcomes and public childcare (Steiber & Haas, 2012; Brady et al., 2020) and the latter's causal effect on the child penalty (Rabaté & Rellstab, 2022; Kleven et al., 2025). One reason is that the availability of formal childcare might contribute to people's decisions about having children – how many if at all, their timing or spacing – though findings are mixed in this respect (Bergsvik et al., 2021; Scherer et al., 2023). If better daycare means more births, nonetheless, earnings losses might be compounded, as women are confronted with additional economic penalties for higher-order births in contemporary labour markets (Cools et al., 2017; Markussen & Strøm, 2022).

Another caveat is that formal childcare availability does not equate to access. Other modes of childcare such as grandparental care, for example, might remain more convenient or desirable than formal childcare, whose impact should thus be evaluated against that of alternative care strategies[2] (e.g., Nollenberger & Rodríguez-Planas, 2015; Brewer et al., 2022). In addition, childcare provisions might be expanded more in settings where coverage is pervasive and maternal employment is already high, thus with modest impacts due to a ceiling effect (Hardoy & Schøne, 2015). Indirect evidence supports this claim, showing larger employment responses when public childcare availability is low before reforms take place (Bauernschuster & Schlotter, 2015; Brilli et al., 2016; Zoch, 2020), although recent studies on large expansions in low-coverage areas find null effects (Kleven et al., 2024). It could also be that norms prescribing to privatise childcare within families crystallise in contexts with historically scarce or uneven institutionalised provisions (Neimanns, 2021). In this case, the effects of service expansions might be muted unless prevailing norms also shift in the process (Zoch & Schober, 2018; Zoch, 2020).

On top of changes in family size, alternative modes of childcare, and broader contextual forces, the effects of public childcare might also differ along the socioeconomic divide. It is well-established that more affluent parents are more likely to access formal childcare (Pavolini & Van Lancker, 2018). At the same time, studies show that women with less formal education or lower incomes are more likely to be employed and earn more when public childcare is expanded.

[2] Alternative childcare provisions might also be zero-sum if formal childcare crowds out informal childcare. However, most of the evidence suggests that formal and informal childcare are complementary to each other (cf., Havnes & Mogstad, 2011; Nollenberger & Rodríguez-Planas, 2015; Gambaro et al., 2025; Gørtz et al., 2025).

This is true in Europe-wide comparative research focusing on local variation in public childcare supply (Del Boca et al., 2008; Scherer & Pavolini, 2023), as well as in a few analyses identifying reform effects in single countries (e.g., Nollenberger & Rodríguez-Planas, 2015; Geyer et al., 2015), including ongoing work looking at the child penalty (Krapf et al., 2020).

Overall, public childcare provisions hold promise to foster involvement and continuity in paid work after the transition to parenthood. Their efficacy, however, is likely contingent on social environments and socioeconomic resources, and the political choices that influence them, which we examine next for the Italian context.

1.2. Centre-based childcare in Italy

Italy's early childhood education and care is organised in a two-tier system comprising nurseries for children between the ages of six months and two years old, followed by kindergartens for children between the ages of three and five. Primary school starts at age six. Coverage in early kindergarten has been close to universal for decades now (Del Boca et al., 2005). Here and in the remainder, we will focus on nurseries.

Public nurseries (*asili nido*) were instituted in 1971. Coverage rates remained well below 10% of eligible children up until the 2010s (Del Boca et al., 2005; Carta & Rizzica, 2018). Despite some growth, the most recent estimate of public coverage stops at 14.3%[3], with private nurseries supplementing with another 15.7% (in 2022, Istituto Nazionale di Statistica [Istat], 2024). To note, private nurseries complement rather than substitute public services, as both are concentrated in the richer regions of Italy's North and Centre (Del Boca et al., 2005; Brilli et al., 2016; Istituto Nazionale di Statistica [Istat], 2024). In place of formal childcare, regular informal childcare provided by grandparents (grandmothers) is common in Italy and more prevalent there than in most European countries (Del Boca et al., 2005; Zanasi et al., 2023).

Municipalities and users share the costs of public childcare. Nurseries charge a fee that varies depending on family income, attendance (part-time or full-time), and local policies (Del Boca et al., 2005; Carta & Rizzica, 2018). The cost of a full-time spot in the period being considered (2012) in the municipality of Rome, for example, ranged from around 35 euros a month for a family in the lowest income bracket, to around 223-304 euros a month for one in the highest income bracket (Carta & Rizzica, 2018). According to different metrics capturing net childcare costs, indeed, Italy ranks favourably in terms of affordability across high-income countries

[3]For a comparison, previous studies on the child penalty have analysed public service expansions reaching from a lower bound of around 33% of all toddlers in Austria (Kleven, Landais, Posch, et al., 2019) to more than 75% in Norway (Andresen & Nix, 2022).

(OECD Database, 2025[4]; Gonalons-Pons & Marinescu, 2024).

Tasked with managing and financing nurseries, municipalities are also the main decision-makers when it comes to the allocation of childcare slots, which are often outnumbered by applications (Del Boca et al., 2005; Istituto Nazionale di Statistica [Istat], 2024). Above all, municipalities aim at guaranteeing access to families with disabled children (Istituto Nazionale di Statistica [Istat], 2025). There is considerable heterogeneity in other allocation rules, with some municipalities privileging children from disadvantaged backgrounds to foster social inclusion and equitable child development, and others focusing on supporting parents who are employed full-time (Brilli et al., 2016, 2017). Most often, though, by prioritising single parents, couples employed part-time or where one parent is out of the labour force, or even directly via means testing, enrolment rules may favour families with lower incomes (Carta & Rizzica, 2018; Istituto Nazionale di Statistica [Istat], 2025).

In sum, Italy's public nurseries provide affordable but limited coverage. Geographical disparities are large, and municipalities have a key role in funding and arranging centre-based childcare. Despite the aforementioned shortcomings, and in a context where informal childcare remains the norm, public childcare services have been linked to better employment chances for women in Italy (Brilli et al., 2016; Carta & Rizzica, 2018; Puccioni & Vuri, 2025). Looking at labour incomes, studies have further suggested that child penalties are smaller where public childcare coverage is more extensive, although focusing on coarse regional averages (Casarico & Lattanzio, 2023). Hence, it can be expected that public childcare might ameliorate child penalties by helping women to retain paid work after the birth of a child. Who benefits the most is unclear, though. Contrary to studies in other contexts, research for Italy has found positive employment effects among families with higher incomes and women with higher earning potential (Carta & Rizzica, 2018; Puccioni & Vuri, 2025).

1.3. Considering local fiscal rules: The Domestic Stability Pact

Against this background, we complement previous research by foregrounding the role of local fiscal rules in constraining public childcare supply. Fiscal rules have been linked to service provisions by a burgeoning literature (Grembi et al., 2016; Pavese & Rubolino, 2024), including studies on nurseries (Venturini, 2020; Bianchi et al., 2023; Neimanns & Bremer, 2024). By impacting the provision of centre-based care, local fiscal policies might shape labour market opportunities

[4]See, e.g., https://www.oecd.org/en/data/indicators/net-childcare-costs.html (last accessed on 17 June 2025).

for parents, but research has yet to establish if, how, and for whom that might be the case.

Here we focus on the consequences of the Domestic Stability Pact (DSP or *Patto di Stabilità Interno*; e.g., Grembi et al., 2016; Pavese & Rubolino, 2024). The DSP was introduced in 1999 to meet fiscal targets set by the European Union. Its enforcement started in 2001. To hold Italian municipalities accountable, the DSP imposed annual constraints on the budget balance and local government expenditures. The constraints were enforced via several sanction schemes for non-compliance, and have by and large applied to municipalities above 5,000 inhabitants up until 2015, when population thresholds were abandoned[5].

Initially, the DSP had a narrow reach limited to the growth rate of the fiscal gap, to current but not capital expenditures, and with several expenditure items exempted. In the mid 2000s, though, the DSP rules became particularly binding (Venturini, 2020). As municipalities' ability to increase revenue through additional taxation is limited, research has linked compliance with the DSP to lower local expenditure in public services like schools (Pavese & Rubolino, 2024). Local fiscal rules might have been even more consequential for childcare services, as their funding is fully devolved to municipalities under Italian laws. Studies have found that compliance with the DSP, especially after its tightening in 2007, led to a sharp decrease in a bundle of investment expenditures, including though not limited to those for nurseries (Venturini, 2020). Although we know little beyond expenditure levels, it is thus plausible that the DSP might have impinged on the supply of public childcare. Fiscal austerity might have had a detrimental effect on the availability of nurseries (Neimanns & Bremer, 2024), mirroring the expansionary effect assigned by previous studies to fiscal autonomy (Bianchi et al., 2023).

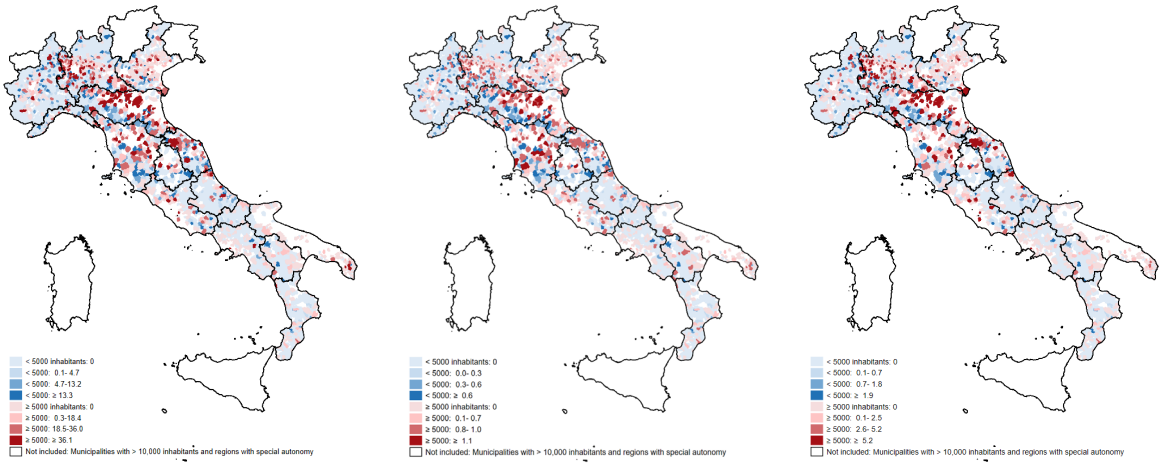
2 Data

2.1. Municipal budgets

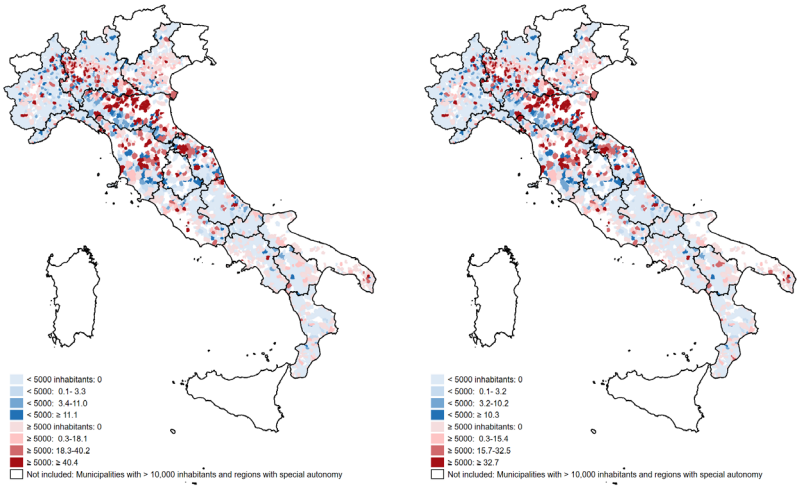
We examine public childcare supply by collating data from publicly available municipal budgets held by Italy's Ministry of the Interior. Municipal budgets are a valuable source of information on local government expenditures and the services they provide. We focus on a subset of service provisions (so-called *servizi a richiesta individuale*) for the period 2001-2015. We restrict our analyses to municipalities with up to 10,000 inhabitants to ensure a symmetrical selection at each

[5]The DSP threshold was changed to 3,000 inhabitants in 2005, then pushed to 5,000 again from 2006, and lowered to 1,000 inhabitants starting in 2012. The DSP was extended to all Italian municipalities in 2015 (for a more extensive review of policy changes, see Grembi et al., 2016; Venturini, 2020). Our analyses cover the period 2001-2015, before the final enlargement. Arguably, considering that the 2005 reform was temporary, and the 2012 reform occurred at the end of our observation period, it is unlikely that these changes altered public childcare provisions in the period we examine.

side of the DSP cut-off. Around two-thirds of Italian municipalities have less than 5,000 residents and are thus largely exempt from the DSP in the period we consider. In line with previous studies (Pavese & Rubolino, 2024), we exclude municipalities in regions with special autonomy (Valle d'Aosta, Friuli-Venezia Giulia, Trentino Alto Adige, Sardegna, Sicilia), which were allowed to set their own fiscal rules under the DSP. We further exclude 270 (0.34%) municipality-year observations due to implausible values[6].



(a) Total slots (b) Centres (c) Teachers



(e) Total applications (f) Accepted applications

Figure 1: Selected dimensions of public childcare supply (nurseries) averaged over the period 2001-2015. Areas in white are not included in our analyses (see main text for details). Different bands are derived using tertiles of each variable's distribution in our data. We assign a separate band to zeroes. Own elaboration of municipal budgets data from the Ministry of Interior.

[6]We exclude municipality-year observations in which more than three childcare centres were recorded or any of the variables mapped in Figure 1 took a value greater than the total number of children aged 0-2 in the corresponding municipality-year cell.

We can rely on 79,478 municipality-year observations for 5,574 municipalities in the period 2001-2015. Our analyses thus cover roughly 69% of Italy's 8,406 municipalities (2015). Municipal budgets comprise yearly information on *a*) the total number of childcare slots, *b*) the number of childcare centres, *c*) the number of teachers, *d*) the number of applications received, and *e*) the number of accepted applications Data comprises formal childcare provisions for children aged 0-2[7]. Services tracked in these data are either fully public or partially subsidised by public funds.

Figure 1 provides an overview of public childcare supply, averaged across the entire period available in our data. The maps reproduce several stylised facts about Italy's public childcare system surveyed in the previous sections. Across the board, service provision is concentrated in the northern regions and even more so in central regions such as Emilia-Romagna, Tuscany, and Marche. As per panel b), though, it is worth stressing that most municipalities lack any public childcare facility. No centre-based services are reported in around 89.6% of all municipality-year observations in our data. At the same time, Figure 1 reveals substantial heterogeneity at the local level, both within regions and across municipalities at each side of the policy cut-off instituted by the DSP. In the remainder, we will focus on this source of variation to identify the effects of a more limited supply of public childcare on women's labour market outcomes.

2.2. Social security records

We merge municipal-level data with administrative data from the National Institute for Social Security (*Istituto Nazionale della Previdenza Sociale*, INPS). INPS records provide us with rich yearly data on women's labour market outcomes and maternity leave spells. We focus on first-time mothers as identified via their first maternity leave request in the records. The data includes all women observed at least once in paid work in the (non-agricultural) private sector. We follow them for five years before childbirth and five years after. The first years after childbirth are not only those in which children might be enrolled in childcare, but also those in which the child penalty is the largest (e.g., Casarico & Lattanzio, 2023; Kleven et al., 2024).

The linkage between individual INPS records and municipal-level data is accomplished via the municipality registered in women's maternity leave requests. In Italy, the legal entitlement to maternity leave lasts five months. Maternity leave is compulsory for all employees with social security membership, and starts 4-8 weeks before the scheduled delivery date. Land register codes (*codici catastali*) allow us to match each woman to the municipality reported in their

[7]Services bridging nurseries and kindergarten when children are 24-36 months (*sezioni primavera*) are not covered by our data.

maternity leave request. Women are thus positioned below or above the DSP cut-off based on the municipality mentioned in their maternity leave records. In line with restrictions dictated by our focus on the DSP, we restrict our analyses to women requesting their first maternity leave in the period 2001-2015, living in municipalities with up to 10,000 inhabitants, and excluding those residing in regions with special autonomy. In doing so, we can examine a total of 3,293,445 records belonging to 388,634 women.

Throughout, we compare women with lower and higher relative earnings to assess variation in the child penalty and its relation with public childcare supply. We split our data based on the median value of women's gross pre-maternity labour incomes (including zeroes) which amounts to roughly €11,695 in 2015 prices. Compared to labour incomes in the general population, women in the low-earning group in our study are at the bottom of the earnings distribution, whereas the high-earning group includes women with middle to high earnings (e.g., Istituto Nazionale della Previdenza Sociale [INPS], 2023). Alternative splits, such as by tertiles of pre-maternity earnings, yield substantially similar conclusions (Appendix A3).

Other than along the income distribution, studies surveyed in the previous sections suggest that public childcare's impact might vary depending on local labour markets, norms, and the baseline levels of service provision prior to policy intervention (e.g., Hardoy & Schøne, 2015; Bauernschuster & Schlotter, 2015; Zoch & Schober, 2018; Nieuwenhuis et al., 2025). In all these respects, Italy's three macro-regions (North, Centre, South) differ widely (e.g., Del Boca et al., 2005; Casarico & Lattanzio, 2023). In addition, the fiscal constraints we are leveraging have been found to be more binding in the richer regions of the North and Centre compared to the South[8] (Grembi et al., 2016; Pavese & Rubolino, 2024). Baseline levels of public childcare coverage, regardless of fiscal rules, also lag behind in the South relative to the North and Centre (e.g., Brilli et al., 2016; Figure 1). Hence we will always combine our country-wide analyses with separate ones for each of Italy's three macro-regions.

The main outcome of interest is women's gross earnings. In line with recent research, we measure earnings in levels to keep the zeroes in (e.g., Kleven et al., 2024, 2025). In this way, our estimates include possible effects on the extensive margin of labour supply. We assume that women are not working for pay in the years in which they are not observed in our data.

[8]Municipalities with a high share of rigid spending can also opt out of the DSP. If fiscally rigid, a municipality has no room to cut spending except for employee salaries and debt repayment. Previous research has found a strong positive correlation between the pre-DSP share of rigid expenditures and non-compliance with the DSP (Pavese & Rubolino, 2024). Our conclusions are unaltered when excluding municipalities in the top quintile of rigid spending.

This assumption is plausible considering the low transition rates from the private to the public sector and to self-employment in the Italian context (Istituto Nazionale della Previdenza Sociale [INPS], 2023, 290-291). We drop records above the 99th percentile of the earnings distribution (around €41,090) to limit the influence of outliers. All values are in euros and indexed to 2015 consumer prices.

We then scale changes in annual earnings to the child penalty. We follow previous studies and first estimate women's counterfactual earnings, i.e., earnings absent the child penalty (e.g., Kleven, Landais, Posch, et al., 2019). We do so by extrapolating predicted values for women's earnings *after* maternity from a regression of women's earnings in the periods *before* maternity. We use the regression specification:

$$Y_{i,t} = \sum_{a} \beta_a \cdot \mathbb{1}[age = a] + \sum_{t} \gamma_t \cdot \mathbb{1}[year = t] + \epsilon_{i,t} \tag{1}$$

where earnings $Y_{i,t}$ are regressed on dummies for age and calendar year. Predicted values are taken to approximate women's earnings in each year-age cell absent the birth of a child. They are then used to compute the child penalty in each period k relative to maternity, i.e., the difference between observed and counterfactual earnings in that period divided by counterfactual earnings:

$$P_{i,k} = \frac{Y_{i,k} - \hat{Y}_{i,k}}{\hat{Y}_{i,k}} \tag{2}$$

We will then plug $P_{i,k}$ in place of earnings in our models to express our main findings in terms of the child penalty. To gauge its magnitude in our data, Figure 1A in the Appendix displays observed and counterfactual earnings trajectories around women's first maternity leave. Once averaged across all post-maternity periods, we estimate a child penalty of around 32% for women with lower relative earnings before childbirth. The penalty increases to around 40% for women with higher relative earnings. Earnings dynamics closely resemble those highlighted in previous studies, though estimates in our sample are smaller compared to the general population (Casarico & Lattanzio, 2023). Note that, due to our sample restrictions, our study excludes women living in large metropolitan areas where high-paying jobs are concentrated and in which child penalties might be steeper (e.g., Bütikofer et al., 2018).

Building on previous studies, we complement our analyses by examining a broad range of labour market outcomes which might underpin the child penalty. In this respect, research has pointed to continuity in paid work, working-time and contractual arrangements, wages, oc-

cupation, and employment changes (e.g., Gangl & Ziefle, 2009; Fernández-Kranz et al., 2013; Harkness, 2016; Adda et al., 2017; Kleven, Landais, Posch, et al., 2019; Mari & Cutuli, 2021; Collischon et al., 2025). Drawing on social security records, we track six related outcomes, namely: 1) days spent in paid work over a given calendar year, 2) having a part-time job, 3) holding a permanent contract, 4) weekly wages, 5) being employed in a white-collar occupation (including clerks, middle managers, and executives), and 6) firm changes. Whilst outcome 1) includes periods of non-employment (in the private sector), outcomes 2) to 6) are observed conditional on having had any employment in a given year.

Finally, we consider parental leave on the grounds that parents (mothers) might use more of it to compensate for the lack of public childcare. In Italy, mothers[9] can take additional parental leave after five months of compulsory maternity leave (Biasi & De Paola, 2025). The scheme's design was not altered in the period we consider and, despite reforms, regulations are fairly similar today (for details, see Istituto Nazionale della Previdenza Sociale [INPS], 2023). Between 2001-2015, parental leave benefits replaced 30% of parents' current earnings for a period of up to six months before a child's third birthday. Beyond these six months or between children's third and sixth birthday, parents could receive a means-tested benefit or take unpaid parental leave. Mothers[10] are more likely to use parental leave within children's first three years, and the proportion of those who do has remained rather stable at around 25% (Istituto Nazionale della Previdenza Sociale [INPS], 2023). By definition, we cannot observe parental leave usage before women's first maternity leave. Hence we created an indicator for any parental leave use in the periods we observe after women's first maternity leave. We thus rely on a collapsed version of Equation 3 by dropping the event-time dummies relative to the periods before childbirth. In other words, we track parental leave use around the policy cut-off exclusively in the periods after the birth of a child.

Table 1 reports some descriptive statistics for the pooled data in the period before childbirth. Grand averages in the last column are in line with previous studies covering longer periods with the same data (Casarico & Lattanzio, 2023). Compared to women with higher earnings and the grand average, women with lower relative earnings are younger, on average, before childbirth, and thus have their first child earlier. Before childbirth, women in the low-earning group earn an average of around €7,800 euro per year against €23,300 in the high-earning group. This

[9] One day of statutory paternity leave was first introduced in 2013. The entitlement has been progressively extended but remains limited to ten days.

[10] As of 2022, fathers' uptake of parental leave remains largely below 20% (Istituto Nazionale della Previdenza Sociale [INPS], 2023).

gap narrows when excluding periods of non-employment, as women with lower earnings spend more than a month less in paid work than their high-earning counterparts. Women in the less affluent group are also much more likely to be employed part-time (36% to 8%) and less likely to hold a permanent contract (76% to 95%). These differences are reflected in a stark wage differential in excess of €100 per week. Last, women with lower earnings before childbirth are less concentrated in white-collar occupations (43% to 70%) and change firms more frequently (32% to 9%). Whether limited public childcare amplifies or attenuates these disparities is an open question.

Table 1: Means and proportions for selected indicators averaged across the last five years before women's first maternity leave. Social Security data (Italy, 2001-2015).

	Low-earning group	High-earning group	Whole sample
Age	29	30.3	31.5
Annual earnings (1000s, incl. 0s)	7.8	23.3	12.4
Annual earnings (1000s, excl. 0s)	11.8	23.3	16.3
Days in paid work (annual)	260.9	293.4	270.5
Part-time employment	0.36	0.08	0.25
Permanent contract	0.76	0.95	0.84
Weekly wage	376.5	481.8	417.4
White collar	0.43	0.70	0.54
Firm change	0.32	0.09	0.23
n (incl. non-employment periods)	1,176,333	493,589	1,669,922

3 Empirical approach

We seek to identify the effects of changes to public childcare provisions deriving from fiscal rules. Without a policy change like that imposed by the DSP, spatial variation in public childcare might be influenced by the evolution of women's labour market opportunities (see, e.g., Kleven et al., 2024 for a discussion). For example, women in paid work, or who would be employed if they could, might push municipalities to expand daycare through advocacy, their voting choices, or in response to excess demand inducing a shortage in available childcare slots. Besides, areas with more centre-based childcare might be the ones with better local labour market conditions, more egalitarian norms or in which other forces might support women's participation in paid work regardless of public service provisions. Differently, we argue that (fiscal) policies whose application only depends on arbitrary population thresholds might provide an exogenous source of variation in public childcare supply.

We leverage this intuition in a regression discontinuity (RD) design (Cattaneo & Titiunik, 2022). A municipality's population size is our running variable with a cut-off point (c) placed at 5,000 inhabitants, a threshold above which fiscal rules become stricter due to the Domestic Stability Pact. In a conventional RD, we would be interested in the discrete change in each outcome around the policy cut-off, with outcome trajectories (slopes) free to vary on each side. We augment this setup via an event study to identify outcome changes at the cut-off *and* in each year relative to women's first maternity leave period. Formally, we rely on the following regression specification:

$$Y_{i,t} = \alpha + \beta_1 D_{m,t} + \beta_2 f(P_{m,t}) + \beta_3 (D_{m,t} \times f(P_{m,t})) + \sum_e \mathbb{1}[event = k] \cdot [\gamma_{1,k} + \gamma_{2,k} D_{m,t} + \gamma_{3,k} f(P_{m,t}) + \gamma_{4,k} (D_{m,t} \times f(P_{m,t}))] + \sum_a \zeta_a \cdot \mathbb{1}[age = a] + \sum_t \eta_t \cdot \mathbb{1}[year = t] + \epsilon_{i,t} \tag{3}$$

where $Y_{i,t}$ is any of the labour market outcomes presented in the previous sections. The coefficients β_1, β_2, and β_3 are associated with the traditional RD setup, whereby $D_{m,t}$ is a treatment dummy equal 1 if a given municipality m has 5,000 or more inhabitants at time t, and $f(P_{m,t})$ is a linear function of a municipality's population centred at the policy cut-off (c = 5,000). Event-time dummies are then introduced to track women's labour market outcomes from five years before to five year after their first maternity leave period, with the base year set to two years before leave-taking ($k = -2$). To combine our RD and event-study setups, we interact event-time indicators with the treatment dummy, the linear function of population size, and their interaction. The quantities of interest in our study are expressed by $\gamma_{2,k}$ and contrast average outcomes in municipalities above vs. below the policy cut-off in each year relative to women's first maternity leave period. In line with conventional event studies (Kleven et al., 2024, 2025), age and calendar-year fixed effects complete the specification to tap into lifecycle and period effects, respectively. Estimation is carried out within an optimal bandwidth around the policy cut-off (Calonico et al., 2017; Cattaneo et al., 2019). Standard errors are clustered at the individual level throughout.

Estimates from Equation 3 ($\gamma_{2,k}$) can approximate the causal effects of a more limited public childcare supply under a set of assumptions[11]. The first requires public childcare supply to vary

[11] Formally, $\gamma_{2,k}$ are reduced-form coefficients, meaning that we are regressing each outcome directly on the

at the cut-off instituted by local fiscal rules. In Table 2 we display estimates from RD analyses (Calonico et al., 2017) on the main features of public childcare supply we can track. We report estimates obtained from the pooled data combining social security and municipal records for the period 2001-2015. Table 2 provides strong evidence that public childcare supply declines in municipalities above the policy cut-off. The starkest differences concern the total number of childcare slots, the number of applications, and the number of accepted applications. While small in absolute value, these changes amount to up to a fourth of the respective averages. We also detect small discontinuities when it comes to the number of centres and teachers. In separate analyses across Italy's macro-regions (Table 1A in the Appendix), we find similar changes at the cut-off in northern regions. Whilst in a context with more widespread service provision, discontinuities at the DSP cut-off are largest in the Centre. There, for example, we estimate that municipalities under stricter fiscal rules have an average of four fewer daycare slots, accept two applications less, and employ one teacher less than towns exempt from fiscal austerity. In the South, finally, we find smaller discontinuities, some of them positive, vis-à-vis a much more scant supply of public childcare. In short, we find that the DSP constrained the supply of public childcare primarily by limiting childcare slots and the capacity to (attract and) accept applications. Especially in the North and Centre, municipalities under stricter fiscal rules might have been more ill-equipped to expand existing provisions to satisfy demand (Del Boca et al., 2005), resulting in more pronounced shortages than what could have happened in the absence of the DSP. In the South, where public childcare services are meagre, the DSP cut-off is less consequential. Further, differently signed estimates might indicate imperfect compliance with the DSP, in line with previous studies (see, e.g., Pavese & Rubolino, 2024).

Table 2: Regression discontinuity (RD) estimates for separate dimensions of public childcare provision. Social Security data merged with municipal budgets data (Italy, 2001-2015).

	Total slots	Centres	Teachers	Total applications	Accepted applications
Change at the policy cut-off	-2.06***	-0.06***	-0.35***	-1.67***	-1.98***
	(0.082)	(0.003)	(0.086)	(0.104)	(0.021)
Average	7.9	0.24	1.3	8.4	7

Note: p-values for robust inference (Calonico et al., 2014); *** $p < .001$.

We further assume that local fiscal rules affect women's labour market outcomes exclusively

DSP cut-off, which we take to be a valid instrument for the local supply of public childcare (see Table 2 and the remainder of this section).

via the changes in the supply of public childcare outlined in Table 2. Previous studies have found that public investments in schools decline in municipalities affected by the DSP (Pavese & Rubolino, 2024). For this reason, we limit our analyses of post-childbirth outcomes to the pre-school period. Other changes at the cut-off are also unlikely to inflate or confound our results. For one, remunerations for mayors and local council members also increase at the DSP cut-off. Studies have shown that elected officials above the DSP threshold have higher educational qualifications and manage public finances differently, for example by spending more on public service provisions (e.g., Gagliarducci & Nannicini, 2013). At worst, if nurseries are among such services, our estimates of the drop in public childcare supply and its labour market consequences might be conservative. As for other reforms in the period, municipalities with more than 5,000 inhabitants were required to rationalise their management of public services in 2010 and experienced a large transfer cut in 2012. However, the impact of these reforms on public childcare and the child penalty is likely moot, as nurseries were exempted from the first reform (Cremaschi et al., 2024), whilst the second mainly resulted in higher property taxes (Marattin et al., 2022).

One last assumption is that women (households) with different labour market prospects did not sort across municipalities depending on the 'bite' of the DSP and the availability of public childcare. If planning to have a child, for example, households with better information and higher incomes might more easily relocate to municipalities with less stringent fiscal rules and, thereby, better services. Likewise, we must assume that municipalities did not manipulate their official population numbers, as the resulting differences across municipalities above and below the policy cut-off might affect women's labour market outcomes regardless of the local supply of public childcare (Eggers et al., 2018). To buttress these assumptions, we take advantage of the fact that public childcare's effects can only manifest around the birth of a child and in its aftermath, yet labour market outcomes are observed before and after. The "no sorting" assumption would be violated if labour market outcomes differ around the policy cut-off long before women's first maternity leave. Within the event-study framework of Equation 3, we can test for outcome differences around the cut-off in each of the five years before women's first maternity leave[12]. If outcome differences are detected in each or most of these periods, sorting might be at play and invalidate (causal) inferences. By and large, our analyses provide no such indication. Hence, the estimates presented in the following sections are unlikely to reflect variation around the

[12]In practice, our event-study approach is akin to the difference-in-discontinuities design advocated for in studies using population thresholds and previously applied to the case of the DSP (Eggers et al., 2018; Pavese & Rubolino, 2024). While prior research differences out early periods in the implementation of the DSP, we can difference out early periods prior to childbirth.

policy cut-off which predate or are independent from its impact on public childcare supply. In addition, Appendix A8 provides sensitivity analyses using the 'donut' approach recommended by Eggers et al. (2018), whereby we exclude municipalities in the utmost proximity of the cut-off. Arguably, these municipalities are most likely to elude fiscal rules by manipulating their population numbers. These supplementary analyses yield no evidence that manipulation affects our main conclusions.

4 Main findings

4.1. Earnings and the child penalty

Figure 2 displays average differences in earnings around women's first maternity leave and across municipalities subjected to stricter fiscal rules and not. We find little evidence of differential earnings in response to the DSP and its impact on public childcare. Null effects hold similarly for women with lower (blue) and higher earnings (red) at baseline. Shifts are also small in size across most periods, as most interval estimates exclude gains or losses of €1,000 or more. Likewise, scaling estimates in Figure 2 to the implied change in the child penalty yields null results, as displayed in Figure 2A in the Appendix.

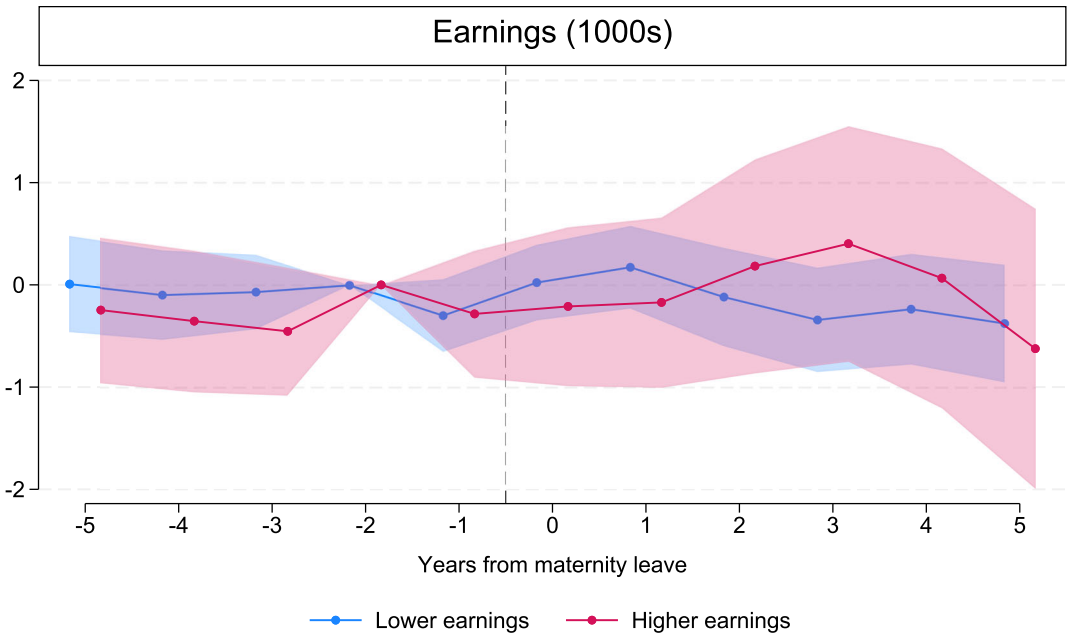


Figure 2: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

Yet average estimates may hide substantial heterogeneity across geographical areas. In Figure 3, we provide separate estimates for each of Italy's three macro-regions. In the North, on the right-hand side, we find some evidence of earnings changes across municipalities with diverging fiscal rules, though limited to women with lower earnings. Whilst largely similar in the first periods we observe, their earnings are found diverge at the policy cut-off in the year immediately after childbirth by about €510 ($p = .001$). Similar differences are detected in the year in which maternity leave was requested ($p = .012$) and three years after ($p = .025$). Whilst sparse, these changes might indicate that (some) women anticipate challenges in reconciling paid and unpaid work when public childcare is limited, and are pushed out of the labour force as a result. Caution is warranted, however, in that these point and interval estimates do not break significantly from the pre-trend, thus precluding conclusive interpretations in the event-study setting (e.g., Kleven, Landais, Posch, et al., 2019).

It is in Italy's Centre, though, that we find stark differences in the earnings trajectories of women at the DSP cut-off. More affluent women seem most affected, whereas differences are nil and stable for women with lower earnings. Relative to two years before childbirth, the high-earning group incurs in extra yearly losses between roughly €1,238 and €3,128 in the period spanning from the first to the fifth year after their maternity leave ($p = .037$, $p = .002$, $p < .001$, $p = .044$, and $p = .003$, respectively). Scaled to the implied child penalty, these losses translate to an upper-limit of around 10 extra points (Figure 3A in the Appendix). Differently, estimates for the South on the left-hand side of Figure 3 are null across the board (see also Figure 3A).

Taken together, findings suggest that fiscal rules are most consequential for women with higher labour incomes in Italy's central regions. As per Figure 1 and previous research (Del Boca et al., 2005; Brilli et al., 2016), these are the regions where public childcare is relatively more widespread. It follows that in those same regions, absent fiscal constraints to their expansion, existing services would be more likely to satisfy demand. Facing relative shortages, instead, women with higher relative earnings incur in larger child penalties, an increase of up to a quarter compared to the overall penalty (40%) estimated for this group in our data (Figure 1A).

4.2. What underpins child penalties?

Following up on our analyses of labour earnings, we examine here a broad range of labour supply and job-related outcomes which might accompany monetary losses. As the latter have been found to be concentrated in Central Italy, we focus on event studies for the same macro-region in Figure 4. Figures 8A, 9A, and 10A in the Appendix portray the corresponding estimates in country-

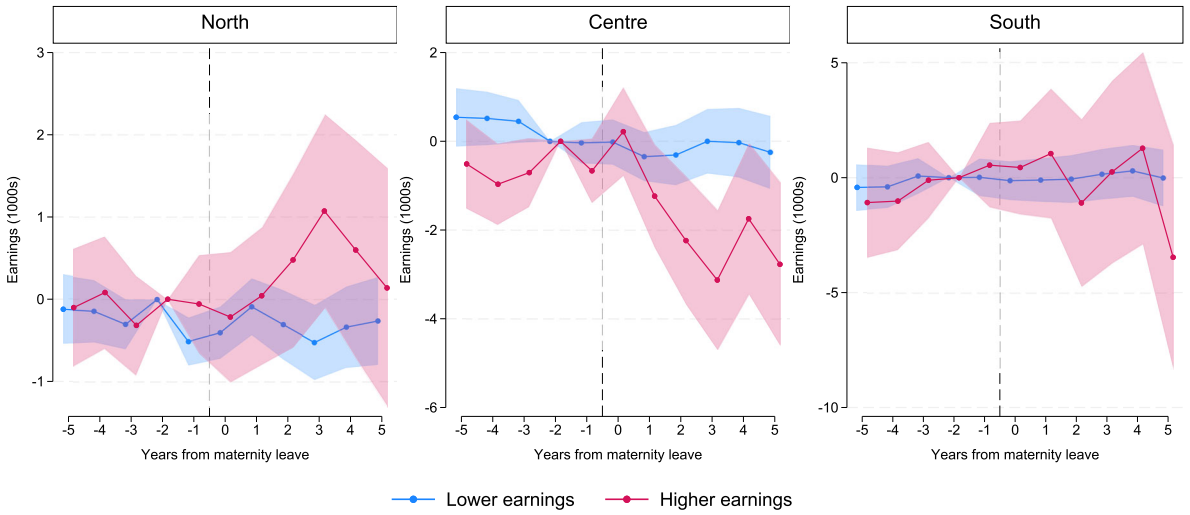


Figure 3: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings, and by geographical area. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

wide analyses and in Northern and Southern regions, respecitvely. In these areas, we find little evidence of outcome differentials developing at the policy cut-off in the aftermath of women's first maternity leave.

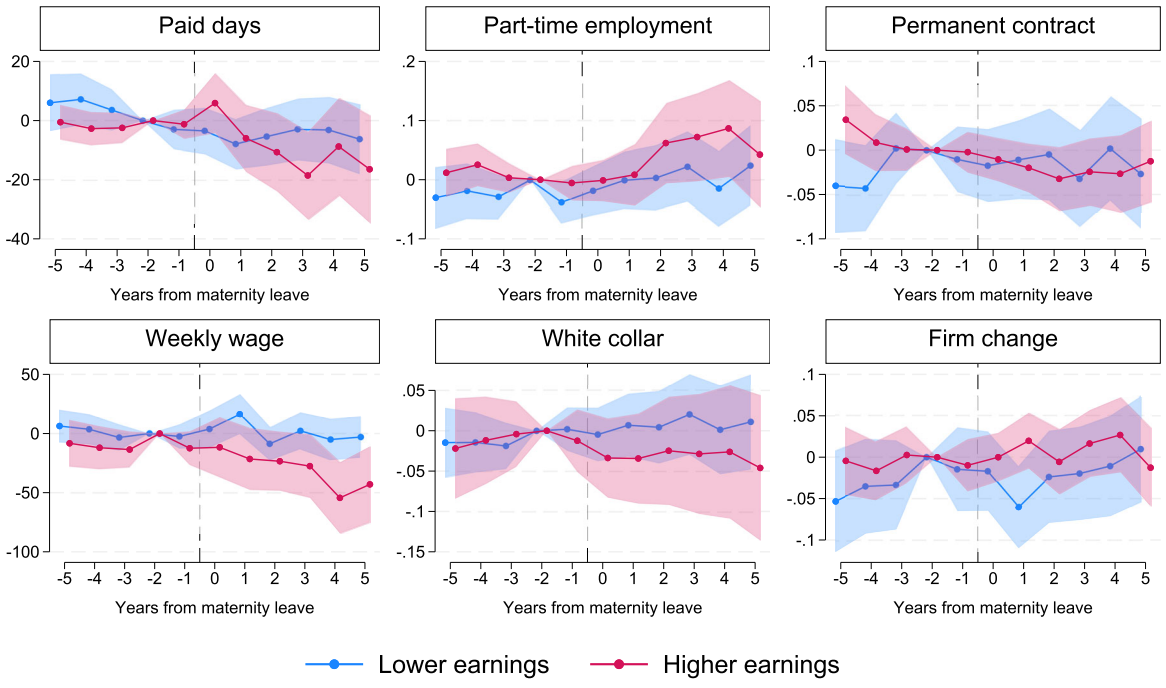
As for women in central regions, Figure 4 offers suggestive evidence of three main channels fuelling extra earnings losses at the DSP cut-off. Women with higher pre-maternity earnings (in red) record fewer days in paid work in the years following maternity leave. We detect more sizeable shifts two (although $p = .112$), three ($p = .014$), and five years ($p = .076$) after maternity leave. These reductions in time spent in paid work range from around 11 to 19 days, roughly two to four five-day working weeks in a given year.

Further, women with higher earnings above the policy cut-off are employed part-time more frequently after childbirth. The chances of holding a part-time post increase by around 6 to 9 percentage points two ($p = .074$), three ($p = .058$) and four years after maternity ($p = .037$). These changes are substantial considering that the part-time employment rate observed before childbirth in the high-earning group is 8% (Table 1).

The other outcome for which we detect significant changes at the policy cut-off is weekly wages, displayed in the lower panel of Figure 4. After the birth of a child, we observe extra wage losses between around €12 and €54 per week for women with higher earnings in municipalities under stricter fiscal rules. These differences are the most clear-cut across all outcomes, both

relative to women in the low-earning group and relative to the baseline two years before maternity leave – and especially three ($p = .044$), four ($p < .001$), and five years after ($p = .009$). Compared to average pre-maternity wages among women with higher relative earnings in Table 1, these estimates imply a decline in weekly wages of up to roughly 11%. Differently, evidence is inconclusive when it comes to permanent vs. temporary employment, white- vs. blue-collar occupation or the probability of changing firms.

All in all, the additional child penalty for women at the DSP cut-off seems driven by switches to part-time employment featuring shorter working weeks and lower weekly pay. Part-time employment might thus facilitate the combination of paid and unpaid work vis-à-vis public childcare shortages, though at the cost of lower earnings.



Note: Central Italy only.

Figure 4: Differences in selected labour market outcomes across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

4.3. Compensatory responses via parental leave

Public childcare shortages could also be compensated for via parental leave uptake. Figure 5 reports estimates on women's differential use of parental leave at the policy cut-off in country-

wide analyses as well as across geographical areas.

On average, parental leave use increases around the policy cut-off for women with lower relative earnings. The largest uptick is observed one year after the birth of the first child (roughly 4 percentage points, $p = .002$). Estimates for the same group of women hover around 2 percentage points also in the year of childbirth ($p = .090$), as well as three ($p = .013$) and five years after ($p = .014$). Differently, estimates are by and large null and not statistically significant for those in the high-earning group, except for a relative decrease in parental leave use in the year after the first maternity leave (– 4 percentage points, $p = .001$). These are sizeable effects given that take-up generally hovers around an upper-limit of 25% (Istituto Nazionale di Statistica [Istat], 2024). Separate analyses by geographical areas reveal that the average positive estimates for women with lower earnings are stronger in Italy's North, whereas negative estimates for women with higher earnings are driven by Italy's Centre. Similar to our main analyses, we find little evidence of outcome changes around the policy cut-off in the South.

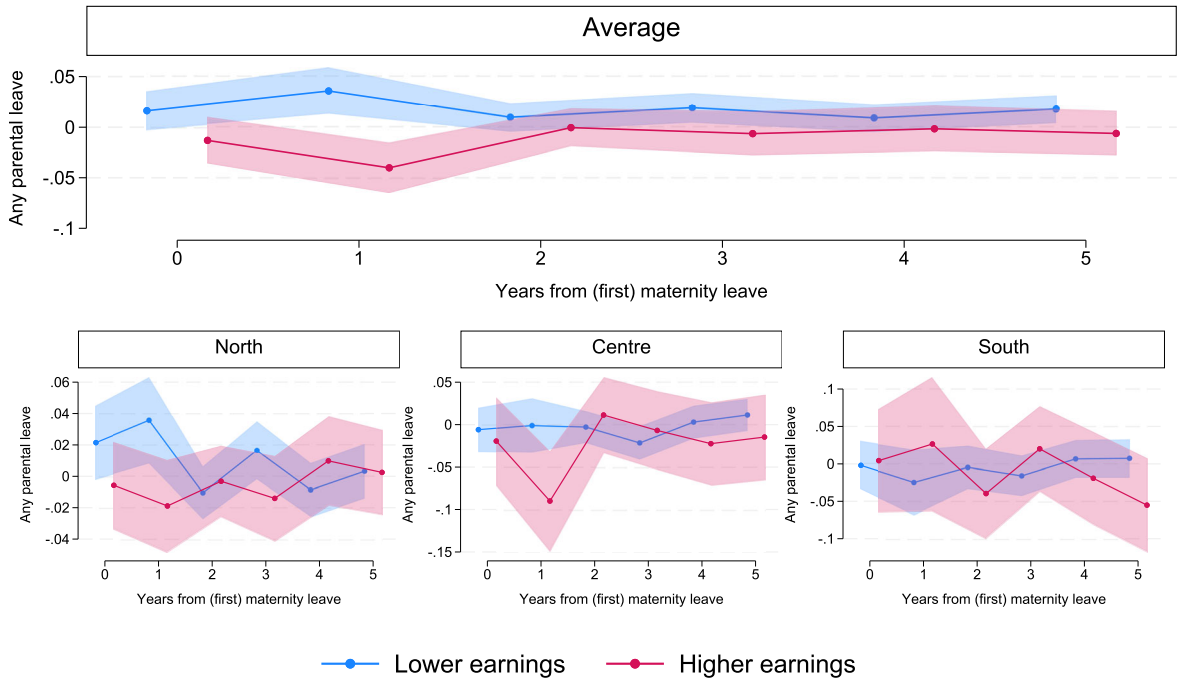


Figure 5: Differences in parental leave uptake across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from a simplified version of Equation 3 excluding event-time dummies for the periods before childbirth. Social Security data (Italy, 2001-2015).

Some women might thus take more parental leave in contexts with a more limited supply of

public childcare. Evidence in Figure 5 points to more clear-cut compensatory responses among women with lower earnings in the North, for whom earnings do not substantially differ around the transition to parenthood at each side of the policy cut-off (Figure 3). Hence, increased reliance on parental leave does not seem to contribute to extra child penalties in our setting.

5 Additional analyses

In the following paragraphs, we present a series of additional analyses meant to probe and extend our main findings.

Survey evidence on shortages. Our main conclusions on child penalties and related outcomes are predicated on the idea that mothers would have accessed daycare if not for shortages due to fiscal constraints. To buttress this interpretation, we complement our analyses of social security records with survey evidence on the reasons parents (mothers) offer for not accessing daycare despite wanting to. We rely on data from the Survey on Births and Mothers carried out by the Italian National Institute of Statistics (ISTAT) in 2012. These survey interviews were conducted on a nationally representative sample of women who had a child in 2009-2010 (see, e.g., Miaci et al., 2025). We exclude regions with special autonomy, yet we are unable to restrict our focus on smaller municipalities like in our main analyses. Other than across geographical areas, we distinguish respondents based on earning potential as proxied by their highest educational qualification, as income is unavailable in the survey.

Figure 11A in the Appendix reports the sample proportions of women indicating "insufficient slots" as one of the main reasons for non-enrolment. This motivation is relatively more prevalent in the Centre. The socioeconomic gradient is also starkest there, with women with a high-school diploma or more being around 10 percentage points more likely to select this reason compared to their counterparts with a lower-secondary diploma or less (23% vs. 13%). This chimes with the differences in public childcare supply we find at the DSP cut-off (Table 2). In line with our main findings, then, survey data supports the idea that relative shortages driven by the number of slots are consequential for women with higher earning potential in Italy's Centre[13].

Sick leave. If consequential, shortages might propel compensatory responses. We highlighted some of those with respect to parental leave access in the previous section. Yet parental leave

[13]Figure 12A in the Appendix offers a wider overview of the main hindrances to daycare access reported by new mothers in the sample. Costs are by far the main obstacle. Comparable shares of women with lower and higher educational credentials report costs being a barrier in the North (69% v. 64%). Women with lower-secondary education or less are far more likely to flag daycare costs in the Centre (67% vs. 40%) and South (60 vs. 36%). In line with evidence presented in Figure 1, the South also leads in the proportion of women, and especially those with at least a high-school diploma (31%), indicating that nurseries are either unavailable or too far.

rights are most extensive up to a child's third birthday and come with a rather meagre 30% replacement rate. Sick leave, on the other hand, has no time restrictions and is paid at a higher rate. When daycare is less available, parents might take more sick leave to cope with care demands or due to the latter's impact on health. We consider this alternative channel by relying on social security records available for the period 2006-2015 and examining sick leave as a share (%) of annual payslips.

Figure 13A in the Appendix displays our event studies for this outcome. Overall, we find little indication of a break compared to pre-trends across income groups and geographical areas. If anything, sick leave uptake trends upwards for high-earning women in the Centre, reaching a peak in the second year after childbirth (roughly 0.65 points, though $p = .083$). One caveat, though, is that sickness absences might also increase with the availability of daycare. For example, group childcare might expose children to more respiratory or viral illnesses which might be then passed on to other household members (see, e.g., Daysal et al., 2021). We cannot disentangle these effects from heightened sick leave use due to care reasons, as our data concerns access and not motives. With this in mind, we nonetheless find limited evidence that new mothers respond to childcare shortages by using more sick leave.

Other children. A more limited supply of public childcare may have consequences for family planning and formation. In turn, differences in the number or timing of children might affect labour market outcomes and influence, thereby, the policy effects we examined in the previous sections. In further analyses displayed in Figure 14A in the Appendix, we thus ask if the chances of claiming any maternity leave after the first differ at the policy cut-off. Recall that in our data we observe births whenever a spell of maternity leave is recorded. When relying on such a proxy, we find no evidence of changes in higher-order fertility at the policy cut-off. Interval estimates exclude substantial shifts in most cases. These estimates are by and large in line with the broader literature finding small or null effects of public childcare provisions on fertility in the Italian context (e.g., Del Boca, 2002; Scherer et al., 2023). In our setting, we can rule out that differences in the number or timing of children contribute to the observed changes in earnings or parental leave use at the policy cut-off.

6 Discussion

The main findings in this study speak to the link between local fiscal constraints, public childcare supply, and women's labour market chances around the birth of a child. Drawing on rich data from municipal budgets, we show that Italy's implementation of the Domestic Stability Pact

led to relative shortages in public childcare provisions. These shortages were primarily driven by the total number of daycare slots and (accepted) applications in municipalities subjected to fiscal austerity. At the same policy cut-off, the labour market outcomes of new mothers seem largely unaltered in country-wide analyses. Effects are heterogeneous, though, as we uncover extra earnings losses concentrated among women with higher relative earnings in Central Italy, where daycare coverage is otherwise more comprehensive and DSP-induced shortages were more pronounced. For this group, earnings losses are found to increase by up to a quarter compared to the average child penalty among women with higher labour incomes in our data. We suggest that shorter weeks on part-time contracts underpin these substantial monetary losses. In addition, we find some evidence that women access parental leave more frequently in municipalities with stricter fiscal rules and less formal childcare. Yet these compensatory responses are driven exclusively by women with lower earnings in Northern regions and do not appear to exacerbate earnings losses.

These findings contribute, first, to research on family policies such as public childcare. We contend that, alongside service expansions (e.g., Zoch & Schober, 2018; Rabaté & Rellstab, 2022; Kleven et al., 2024), limited public childcare supply also deserves attention. This is important at a time in which some countries face shortages and others have yet to establish widely available and/or affordable daycare. Evidence in this paper foregrounds how local fiscal policies, in particular, may influence centre-based childcare. We suggest that the broader institutional environment could be crucial when investigating the (in)effectiveness of family policies (cf., Thévenon, 2016; Olivetti & Petrongolo, 2017; Brady et al., 2020). With respect to formal childcare, future research could move beyond a single-policy focus to consider how service provision depends on fiscal constraints (Neimanns & Bremer, 2024; Nieuwenhuis et al., 2025).

With respect to fiscal policy itself, in addition, our findings expand on the growing literature on austerity. Previous studies have examined services other than formal childcare (e.g., schools) or bundles thereof, often with a focus on austerity-induced declines in public expenditure (Venturini, 2020; Pavese & Rubolino, 2024). We are able to show how fiscal rules can shape service provision across multiple dimensions (slots, applications, teachers, etc.), and that these impacts are reflected in the motives behind parents' inability to enrol children in daycare. Further, by linking the Domestic Stability Pact to child penalties in women's earnings, we add to the literature on the social and economic costs of austerity (e.g., Fetzer, 2019; Pavese & Rubolino, 2024; Villa, 2024; Sochas & Chanfreau, 2025). The gendered impacts of austerity policies remain an important avenue for research (Rubery, 2015). Our study shows that the linkage between

austerity, pubic services, and women's economic outcomes can be one of the many consequential foci for this research agenda (for others, see, e.g., Sochas & Chanfreau, 2025).

Most of all, though, our study might stimulate further inquiries on the heterogeneous effects of family policies. Like previous studies, we find null policy effects in country-wide analyses (Rabaté & Rellstab, 2022; Kleven et al., 2024). Yet average effects can conceal variation along socio-economic divides and across contexts, even within a single country (e.g., Zoch & Schober, 2018; Zoch, 2020). Specifically, our evidence of disproportionate effects on women with higher incomes is symmetrical to and extends previous studies on public childcare expansions and women's labour market participation in Italy (Carta & Rizzica, 2018; Puccioni & Vuri, 2025). It stands in contrast, though, with research on public childcare expansions in other settings, which finds labour market effects concentrated on women with lower incomes (e.g., Nollenberger & Rodríguez-Planas, 2015; Krapf et al., 2020). It could be that contexts like Italy are peculiar in that (public) childcare coverage remains low, and changes to it in either direction might first affect early adopters, i.e., more affluent parents. Opposite to 'ceiling' effects uncovered by previous research (Hardoy & Schøne, 2015), this interpretation would point to 'floor' effects. Alternatively, shortages might simply be qualitatively different from expansions. When large enough, expansions might have equalising effects by satisfying childcare demands and enhancing parental labour market opportunities across the socio-economic spectrum (Del Boca et al., 2008; Scherer & Pavolini, 2023). Shortages, on the other hand, might be more impactful for parents with higher earnings, who have the highest demand for formal childcare, face the largest opportunity costs when not accessing it, and might not be prioritised by enrolment criteria. In short, 'floor' effects and the distinctive consequences of shortages are promising directions for future studies. Investigations along these lines across multiple contexts, national and sub-national, could provide a fuller appreciation of the (dis)equalising effects of public childcare (Nieuwenhuis et al., 2025).

Finally, we provide some of the first evidence of changes in the use of parental leave vis-à-vis exogenous variation in public childcare provisions. Women who experience the largest child penalties are not the same who access parental leave more frequently at the policy cut-off in our setting. One explanation is that we might be picking up on a few additional days of leave that do not have large economic consequences. Besides, job protection rights such as the ones attached to the Italian parental leave scheme could be shielding women from further labour market penalties (e.g., Lalive et al., 2014; Mari & Cutuli, 2021; Biasi & De Paola, 2025). For those with higher earnings in the Centre, on the other hand, less frequent parental leave use

at the cut-off might indicate that this option is less available (due to the shorter time spent in paid work) or less palatable (due to the low replacement rates). We can speculate that, along the income distribution, the interplay between public childcare and parental leave schemes may vary depending on the availability and costs of the former, and the latter's duration, generosity, as well as job-protection and co-parenting rights. Depending on the mix, concomitant reforms to different family policies might cancel each other out or complement each other's effects on parental labour market outcomes and beyond. The often null or contradictory effects of family policies across the literature might be re-evaluated in light of such zero-sum or complementary dynamics (cf., Thévenon, 2016; Olivetti & Petrongolo, 2017; Brady et al., 2020; Kleven et al., 2024).

Together with its contributions, our study is also limited in a number of ways. For one, we can only identify local effects around a given policy cut-off. The policy we study is less consequential in Italy's South, where formal childcare is often unavailable (Figure 1 and Figure 12A). Hence, we can only speak to relative shortages in settings with a more widespread supply of public childcare, even if still low in comparative perspective. In addition, our data pertains to small municipalities and women who are or have been employed in the private sector. Our estimates could be conservative if child penalties are larger and more sensitive to the supply of public childcare in metropolitan areas. We also could not track the labour market outcomes of public-sector employees, some of which might even be childcare workers directly affected by the shortages we highlight, especially in Italy's Centre (Table 1A).

A second limitation regards possible adjustments to a limited supply of public childcare over and beyond those we could examine. On the supply side, municipalities might have reacted to fiscal constraints by outsourcing service provision to the private sector. With our data on public or co-financed services, we cannot pin down the role of private childcare services. Private facilities charge higher fees and tend to cluster together with public services in the Italian context. It follows that parents with higher incomes in Italy's North and Centre might more easily turn to privately-run daycare. Via this channel, women with higher earnings might have accessed formal childcare and averted larger child penalties. As for the demand side, we cannot observe the resources, childcare involvement, and labour market outcomes of partners or kin members. In couples, for example, partners might take additional parental leave depending on the scheme's replacement rates and partners' relative earnings (e.g., for Italy, Biasi et al., 2025). Grandparents might also spend (even) more time in informal childcare and mitigate the adverse effects of shortages – though for whom exactly may be influenced by multiple factors

including grandparental health, labour market participation, the quality of family relationships or geographical proximity. We leave these as directions for future research, re-iterating that grandparental childcare is more prevalent in Italy than elsewhere (Del Boca et al., 2005; Zanasi et al., 2023). Also from this perspective, we might be providing a conservative estimate of the labour market impacts of shortages in the supply of public childcare.

Conclusion

Our study suggests that family policies may have heterogeneous effects on the child penalty in women's earnings. We heed the call to consider public childcare in context (Nieuwenhuis et al., 2025) by examining its interaction with local fiscal policies as well as parental leave schemes. Findings point to relative shortages in the supply of public childcare in municipalities subjected to fiscal austerity. These shortages might have compounded the steep earnings drop experienced by women with higher incomes in Italy's otherwise 'daycare-rich' regions. Changes in parental leave use at the same policy cut-off seem unrelated to these economic losses.

In the period we consider, fiscal constraints on the supply of public childcare were already a salient issue in Italy. In 2014, the national parliament debated a law proposal (ddl 1260/2014) to exempt public expenditures for childcare provisions from the Domestic Stability Pact. The law did not pass, and fiscal rules under the DSP were extended to all Italian municipalities shortly after. Today, however, Italy and other European countries stand to gain from public investments under the Resilience and Recovery Plans. A portion of these funds has been destined to enhance the supply of formal childcare, though implementation in Italy has been riddled with delays and uncertainties. Research has offered multiple rationales for why available and affordable daycare can improve living standards and foster the life chances of adults and children (see, e.g., Morrissey, 2017; Van Huizen & Plantenga, 2018; Nieuwenhuis et al., 2025; Sandner et al., 2025). Part of a growing literature suggests that ameliorating child penalties might be among such logics (e.g., Krapf et al., 2020; Andresen & Nix, 2022; Karademir et al., 2024). We add that the impact of public childcare, especially when in short supply, might be unequal along the income distribution and geographical areas. Policies attending to these disparities – combining a relaxation of fiscal constraints, direct investments in public childcare, and well-designed parental leave – might be best suited to remedy the kind of gendered economic inequalities that can be spurred by shortages and otherwise limited daycare.

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Appendix

A1 Public childcare at the policy cut-off across macro-regions

Table 1A: Regression discontinuity (RD) estimates for separate dimensions of public childcare provision. Separate models by geographical area. Social Security data merged with municipal budgets data (Italy, 2001-2015).

	Total slots	Centres	Teachers	Total applications	Accepted applications
North					
Change at the policy cut-off	-2.02***	-0.06***	-0.28***	-1.60***	-1.89***
	(0.132)	(0.004)	(0.020)	(0.095)	(0.010)
Average	6.5	0.21	1.1	6.6	5.6
Centre					
Change at the policy cut-off	-3.93***	-0.10***	-1.19***	-1.22***	-2.03***
	(0.241)	(0.008)	(0.045)	(0.284)	(0.242)
Average	15.4	0.45	2.5	17.2	14
South					
Change at the policy cut-off	0.46***	-0.01***	0.06***	-0.07	-0.14***
	(0.034)	(0.001)	(0.004)	(0.032)	(0.018)
Average	1.4	0.05	0.1	1.2	1.1

Note: p-values for robust inference (Calonico et al., 2014); *** $p < .001$.

A2 Child penalties

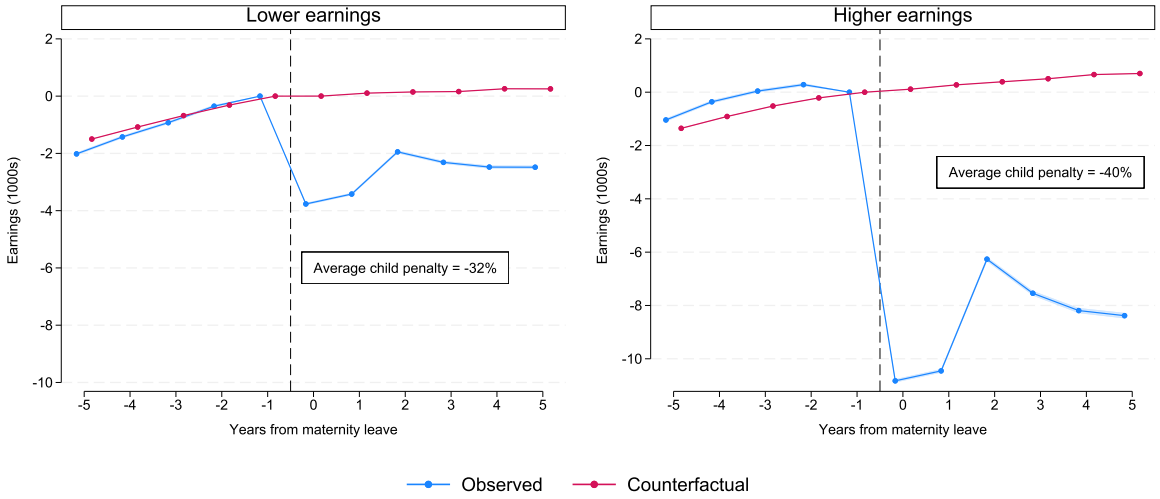


Figure 1A: Child penalties for lower- and higher-earning women. We assign to each woman her predicted (counterfactual) earnings in each age-year cell drawn from the predicted values $\hat{Y} = \sum_a \beta_a \cdot \mathbb{1}[age = a] + \sum_t \gamma_t \cdot \mathbb{1}[year = t]$ estimated using pre-maternity periods only. In other words, estimates in red refer to women's average predicted earnings based on lifecycle and period effects had it not been for the birth of a child. The child penalty is derived from the average difference between observed and counterfactual earnings in the years after the birth of a child.

Figure 2A: Differences in estimated child penalties (Equation 2) across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

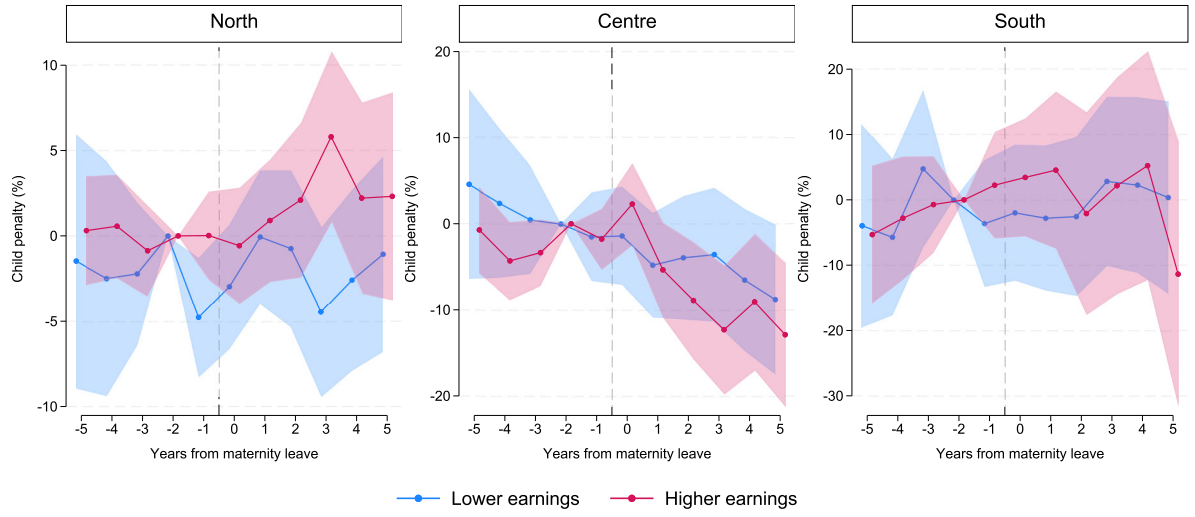


Figure 3A: Differences in estimated child penalties (Equation 2) across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings, and across geographical areas. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

A3 Different income groups

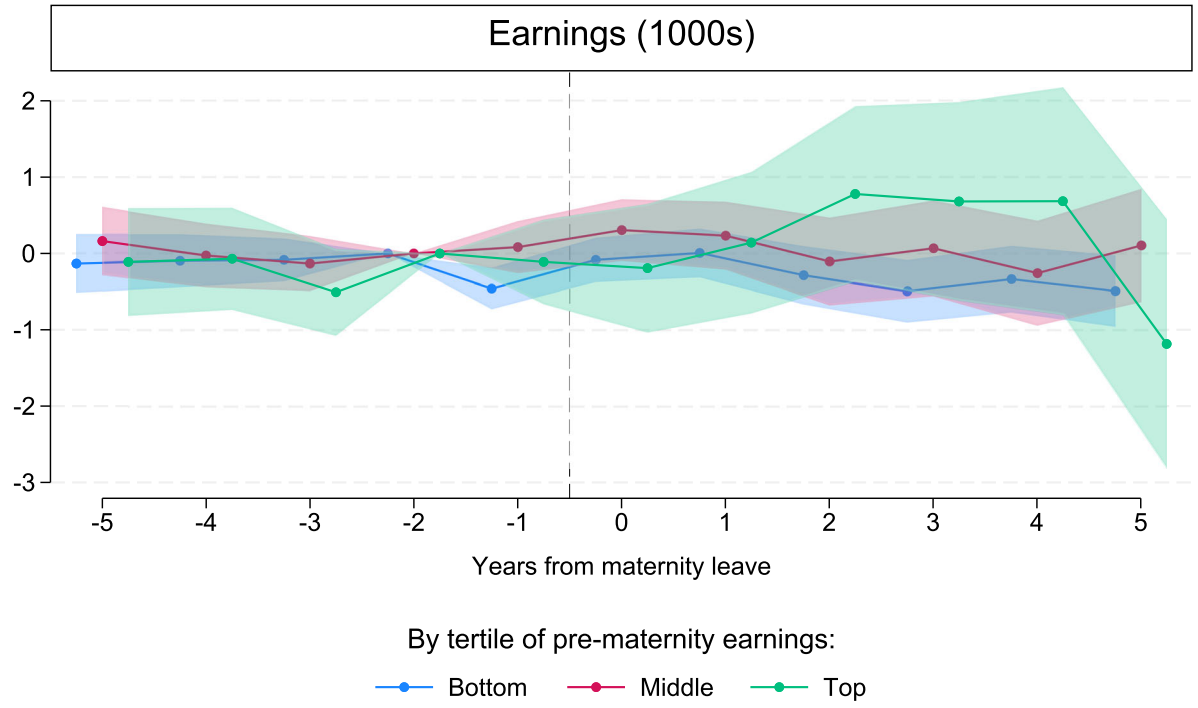


Figure 4A: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for women with low (blue), intermediate (red), and high earnings (green), split based on tertiles of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

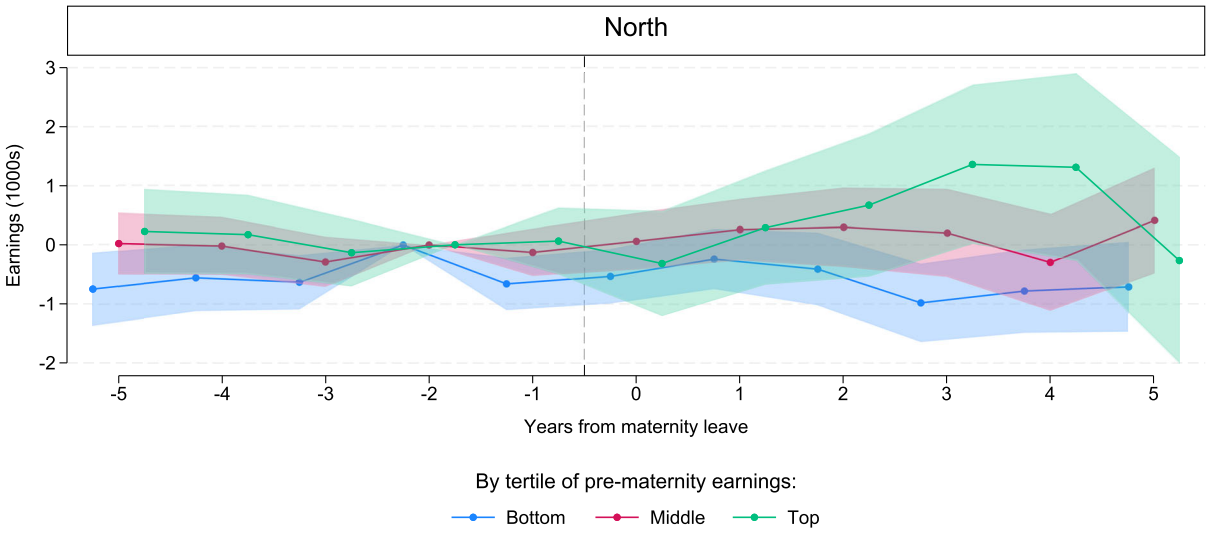


Figure 5A: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for women with low (blue), intermediate (red), and high earnings (green), split based on tertiles of pre-maternity earnings. Northern regions only. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

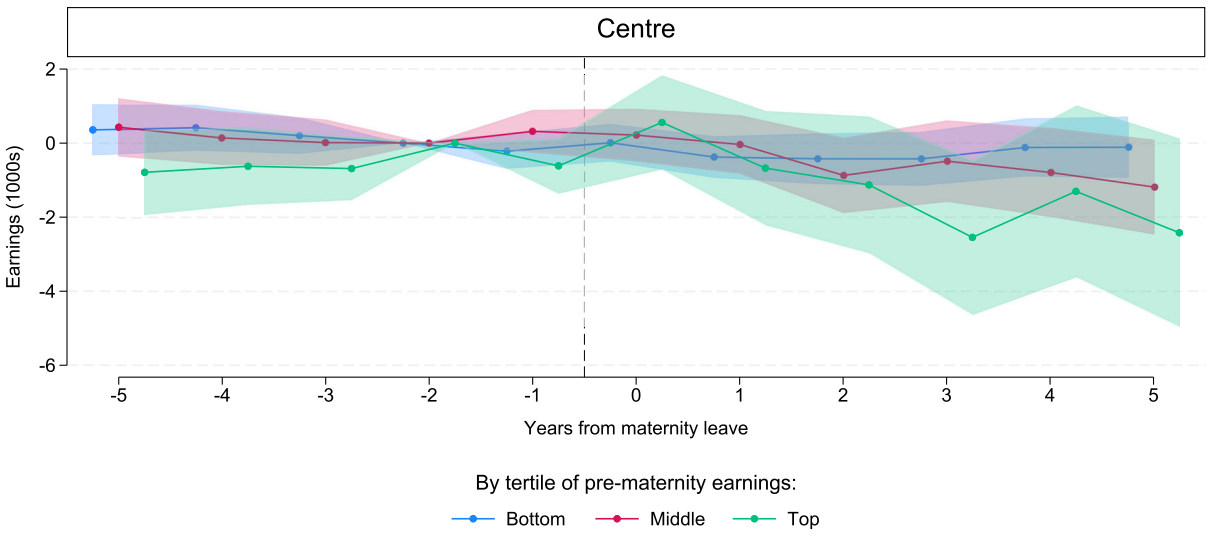


Figure 6A: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for women with low (blue), intermediate (red), and high earnings (green), split based on tertiles of pre-maternity earnings. Central regions only. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

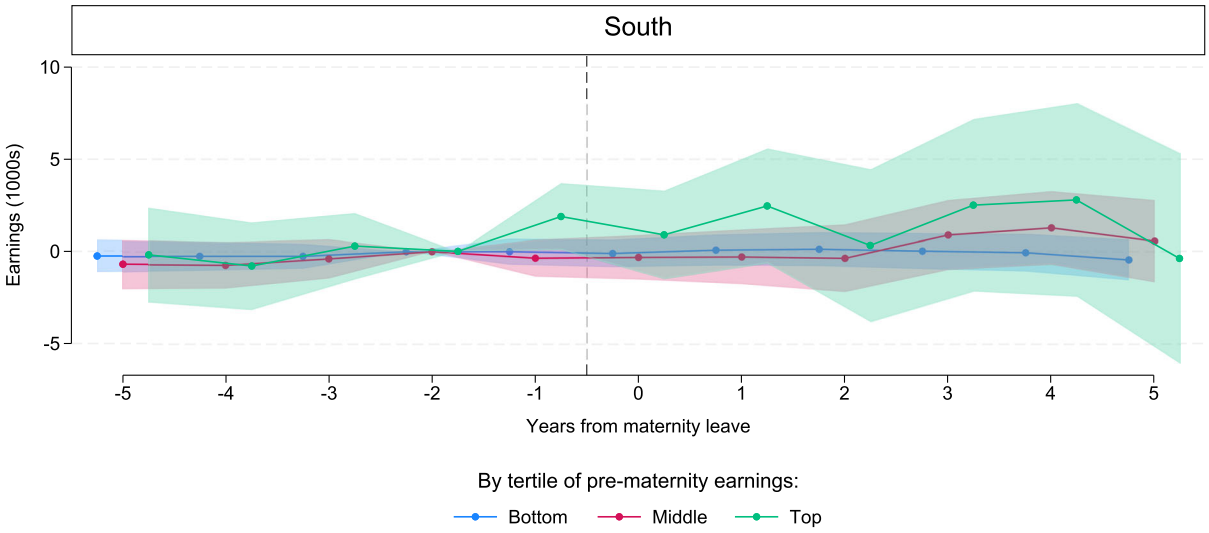


Figure 7A: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for women with low (blue), intermediate (red), and high earnings (green), split based on tertiles of pre-maternity earnings. Southern regions only. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

A4 Labour market outcomes

Note: Northern Italy only.

Figure 9A: Differences in selected labour market outcomes across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

Figure 8A: Differences in selected labour market outcomes across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

Note: Southern Italy only.

Figure 10A: Differences in selected labour market outcomes across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).

A5 Evidence from the Survey on Births and Mothers (2012)

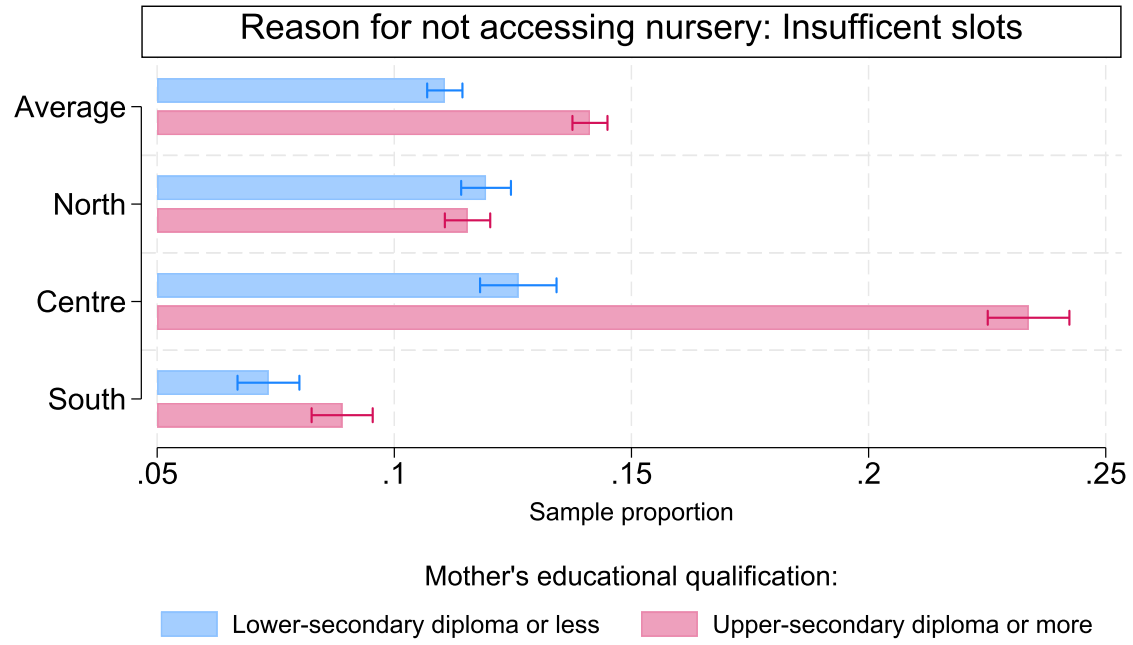


Figure 11A: Sample proportions of women reporting that "insufficient slots" were among the reasons why they did/could not access nurseries. Separate estimates by geographical area and highest level of education. 95% confidence intervals and survey weights are applied throughout ($n_{unweighted}$ = 1,720; $n_{weighted}$ = 61,260). Own elaboration of data from the Survey on Births and Mothers (*Indagine campionaria sulle nascite e le madri*, 2012).

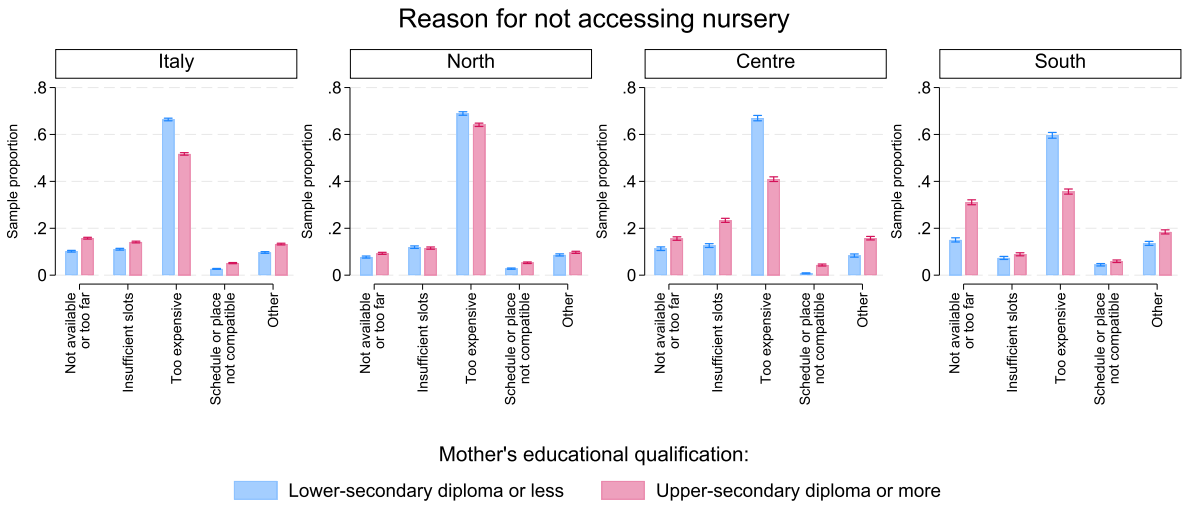


Figure 12A: Sample proportions of women reporting on the main reasons why they did/could not access nurseries. The category "Other" includes those who have withdrawn their child after enrolment (due to "frequent sickness", because the child "couldn't adapt" or parents were "dissatisfied"), those who did not enrol children for health-related reasons, because a partner or another family member disapproved, or another reason not listed in the survey. Separate estimates by geographical area and highest level of education. 95% confidence intervals and survey weights are applied throughout ($n_{unweighted}$ = 1,720; $n_{weighted}$ = 61,260). Own elaboration of data from the Survey on Births and Mothers (*Indagine campionaria sulle nascite e le madri*, 2012).

A6 Sick leave

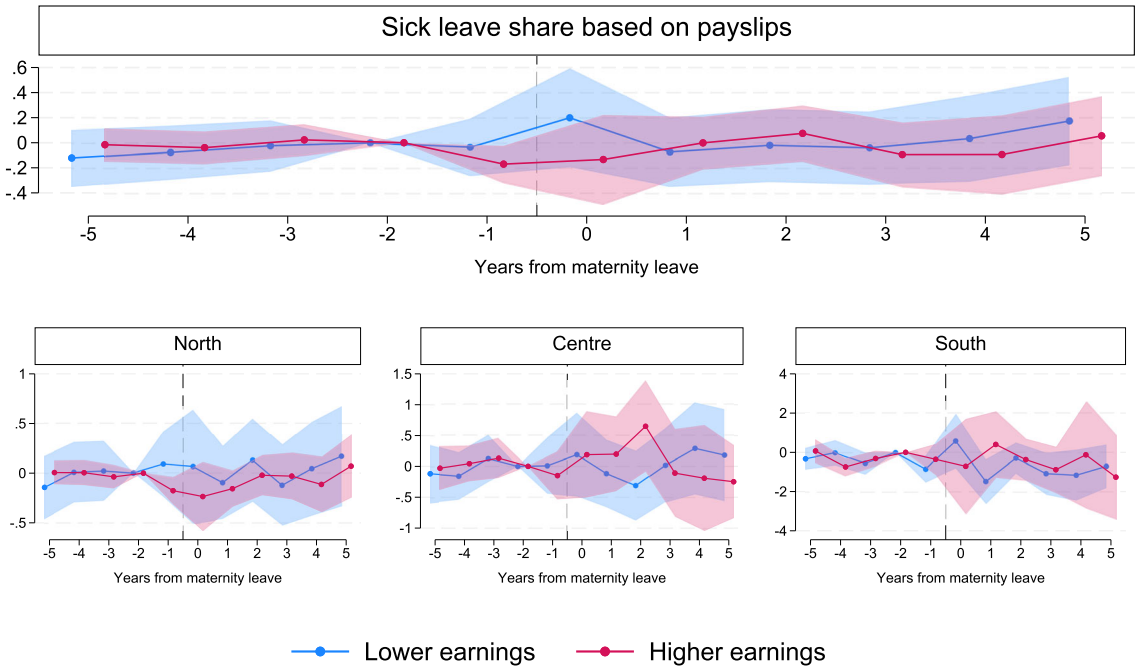


Figure 13A: Differences in sick leave uptake across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings, and for different geographical areas. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2006-2015).

A7 Other children: Any maternity leave after the first

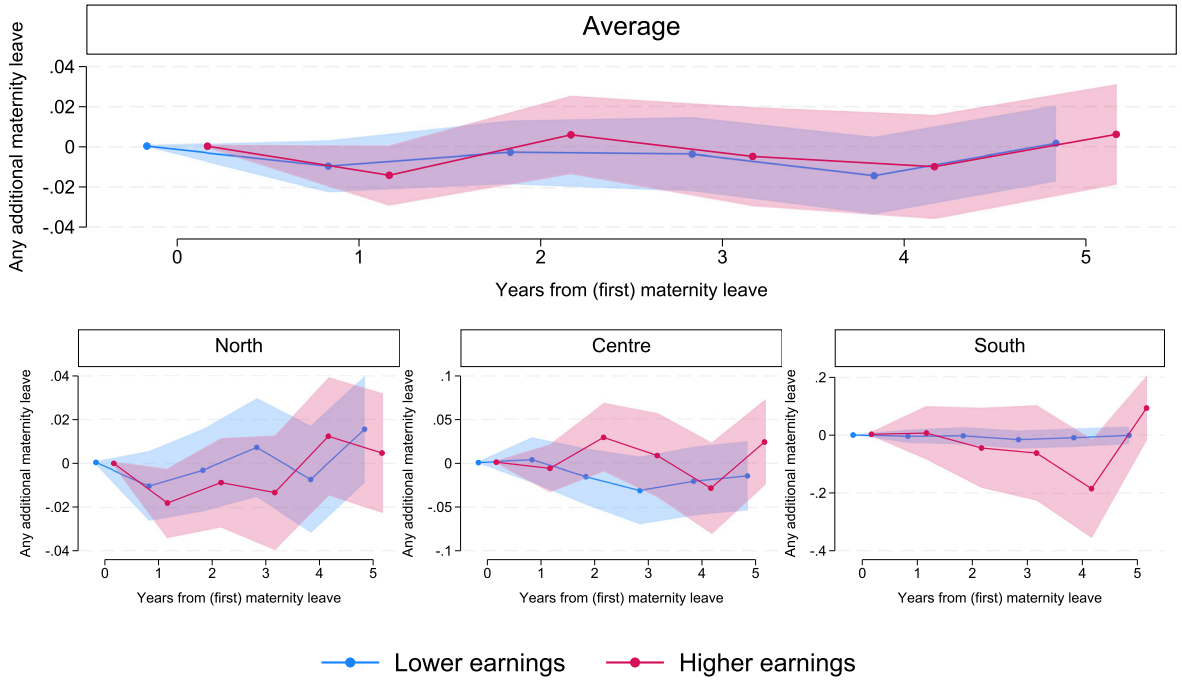
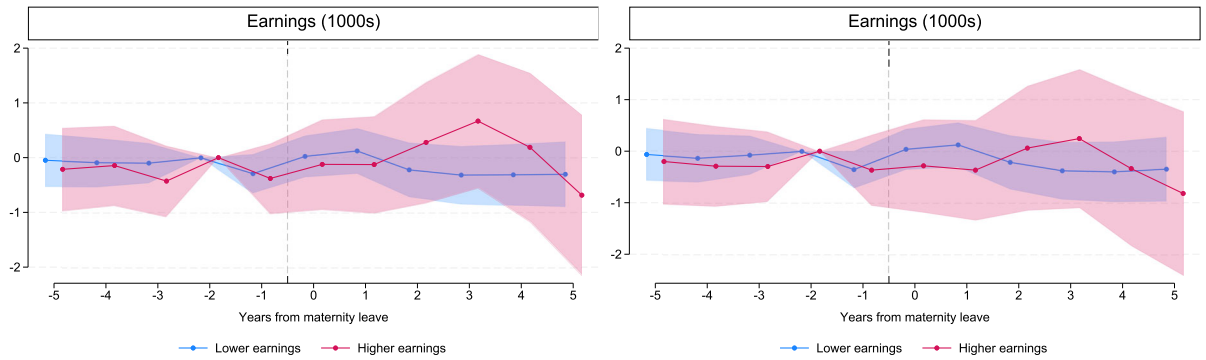


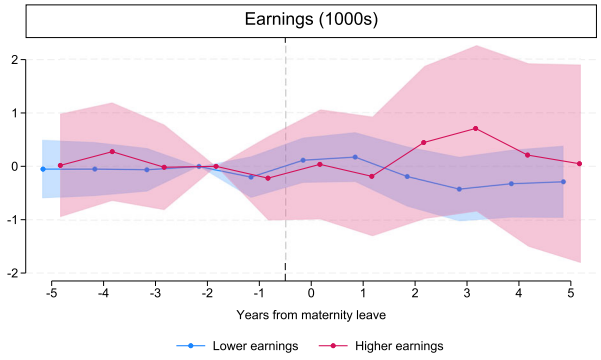
Figure 14A: Differences in maternity leave uptake across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from a simplified version of Equation 3 excluding event-time dummies for the periods before childbirth and the year in which women take their first maternity leave (0). Social Security data (Italy, 2001-2015).

A8 Sensitivity analyses: 'donut' approach



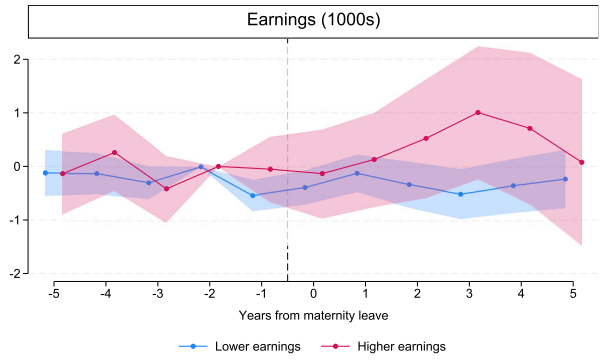
(a) Donut size = 10

(b) Donut size = 25

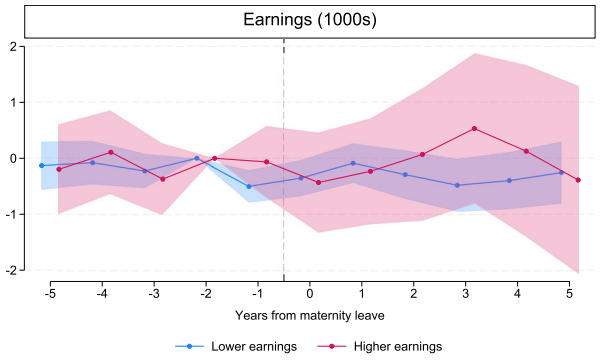


(c) Donut size = 50

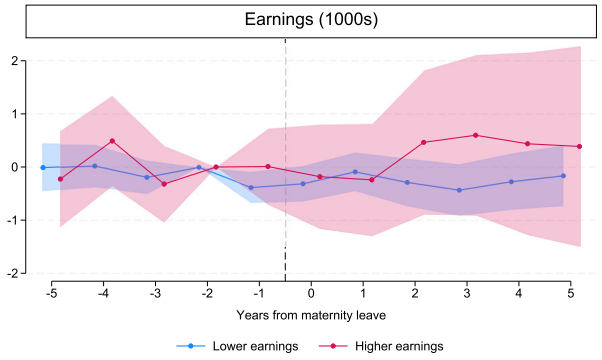
Figure 15A: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants). Municipalities with up to 10, 25, and 50 inhabitants above and below the cut-off are excluded from the analyses in panel a), b), and c), respectively. Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).



(a) Donut size = 10

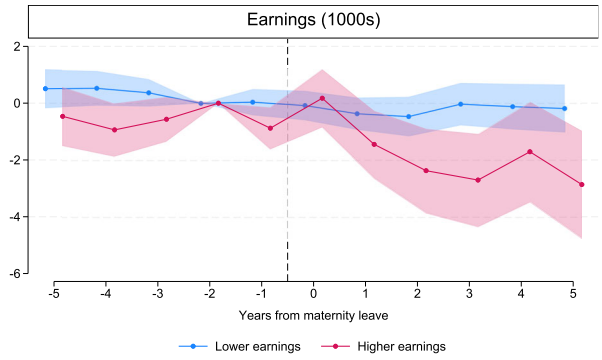


(b) Donut size = 25

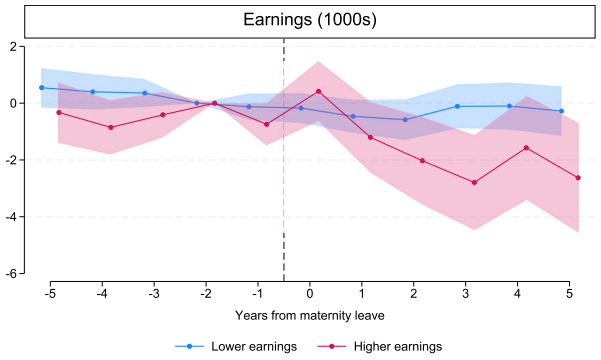


(c) Donut size = 50

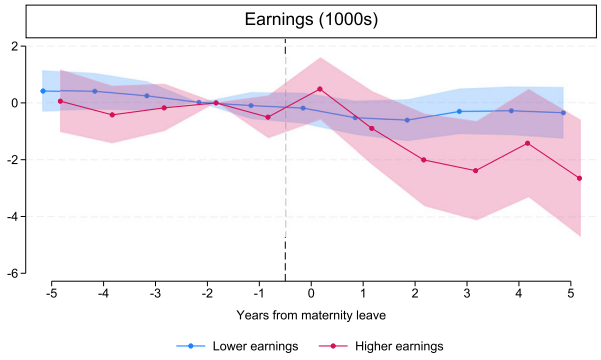
Figure 16A: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants) in Italy's northern regions. Municipalities with up to 10, 25, and 50 inhabitants above and below the cut-off are excluded from the analyses in panel a), b), and c), respectively. Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).



(a) Donut size = 10

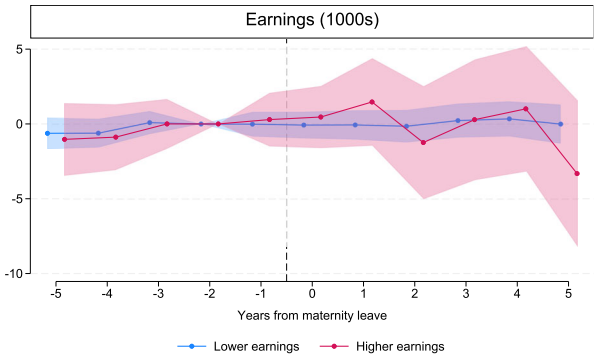


(b) Donut size = 25

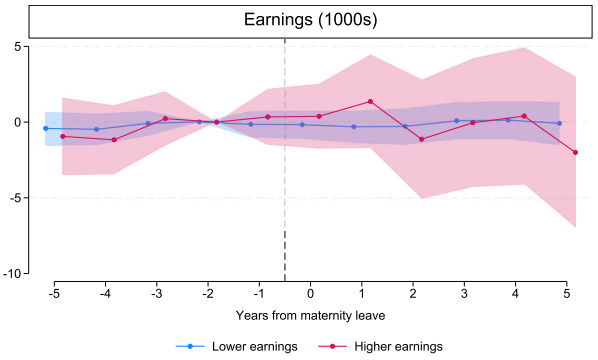


(c) Donut size = 50

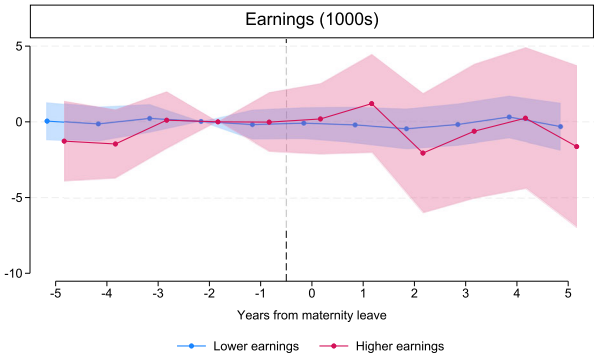
Figure 17A: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants) in Italy's central regions. Municipalities with up to 10, 25, and 50 inhabitants above and below the cut-off are excluded from the analyses in panel a), b), and c), respectively. Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).



(a) Donut size = 10



(b) Donut size = 25



(c) Donut size = 50

Figure 18A: Differences in women's earnings across municipalities subjected to fiscal austerity and not (above vs. below 5,000 inhabitants) in Italy's southern regions. Municipalities with up to 10, 25, and 50 inhabitants above and below the cut-off are excluded from the analyses in panel a), b), and c), respectively. Separate models for low- (blue) and high-earning women (red), split at the median of pre-maternity earnings. Point estimates and 95% confidence intervals are derived from Equation 3. Social Security data (Italy, 2001-2015).