Child-Related Transfers, Fertility, and Maternal Labor Supply: Evidence from Reform-Induced Income Shocks

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Abstract

This paper studies how increases in child-related cash transfers affect fertility and maternal labor supply. Using population-wide administrative data and exploiting heterogeneous reform-induced changes in benefit generosity, we estimate the causal effects of discrete increases in non-labor income on fertility and maternal labor supply at the extensive and intensive margins. Our design compares outcomes across adjacent treatment-intensity groups defined using predetermined household economic conditions, allowing identification in the absence of untreated units. We find that higher transfers increase the probability of having an additional child only over a limited range of benefit increases, with effects concentrated in the middle of the income distribution. At the same time, increased benefit generosity reduces maternal labor supply in the medium run, even net of fertility responses, through both employment exits and reductions in working hours. We translate these effects into local elasticities and document substantial heterogeneity across income groups.

1 Introduction

In most advanced economies, fertility rates have fallen below replacement levels, while female labor force participation – particularly among mothers – remains constrained by caregiving responsibilities and institutional frictions (Doepke et al., 2023). Governments increasingly rely on child-related transfers to support families, yet the behavioral responses to such policies remain imperfectly understood.

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The simultaneity and complexity of fertility and female employment decisions complicate both empirical analysis and policy design. From a theoretical perspective, child-related transfers generate competing forces. By relaxing household budget constraints, they may increase fertility and facilitate labor market participation through improved access to childcare and other market substitutes for home production. At the same time, by raising non-labor income, they may reduce labor supply through income effects, especially along margins where labor supply is elastic (Olivetti and Petrongolo, 2017; Albanesi et al., 2023). Which of these channels dominates, and along which margins, depends on policy design, household characteristics, and local economic conditions, making the net effects of child-related transfers an empirical question.

In this paper, we study how increases in child-related cash transfers affect fertility and maternal labor supply. Using population-wide administrative data and a research design that exploits heterogeneous changes in benefit generosity, we estimate the causal effects of discrete increases in non-labor income on fertility decisions and on labor supply adjustments along both the extensive and intensive margins.

Our empirical setting is a major reform of child-related transfers in Italy: the introduction of the Assegno Unico Universale (AUU), a universal monthly cash benefit paid for each dependent child. The AUU replaces a fragmented system of tax deductions and employment-based family allowances with a single, permanent, and largely unconditional transfer. A key challenge in this context is that the reform does not generate untreated units, as all households with children are affected. We address this challenge by exploiting heterogeneity in treatment intensity, defined as the reform-induced change in child-related transfers. Specifically, we discretize this variation using predetermined household economic conditions measured by the Indicatore della Situazione Economica Equivalente (ISEE) – a comprehensive means index based on household income, assets, and family composition – prior to the reform, and compare outcomes across adjacent treatment-intensity groups. This approach allows us to identify local causal effects of discrete increases in benefit generosity under plausible parallel-trends assumptions.

Our analysis leverages rich administrative data from the Italian Social Security Institute (INPS) covering the universe of AUU applications and payments, linked to longitudinal labor market histories at the individual level. These data allow us to reconstruct fertility histories for mothers and to observe detailed labor supply outcomes at monthly frequency over time. The granularity of the data enable us to study dynamic responses, distinguish between extensive- and intensive-margin labor supply adjustments, and examine heterogeneity across mothers' demographic characteristics, labor market attachment, and local economic conditions.

We find that increases in child-related transfers lead to a statistically significant rise in the probability of having an additional child only for women in specific economic conditions, namely those located in the middle of the ISEE distribution. This pattern implies that the change in benefit generosity does not affect fertility choices among women who are financially constrained or among those who are sufficiently affluent.

We further show that higher benefit generosity reduces maternal labor supply in the medium run for some, but not all, adjacent treatment-intensity comparisons. The absence of a monotonic pattern across treatment-intensity groups suggests that labor supply responses to the AUU reform are non-linear. These effects operate through both employment exits and reductions in working hours among continuously employed mothers. Importantly, labor supply responses persist even when excluding mothers who have an additional child, indicating that they are not driven solely by fertility-related adjustments. Patterns of heterogeneity suggest that fertility responses arise primarily among mothers facing fewer biological and socio-economic constraints, while labor supply reductions are concentrated among mothers at earlier stages of their careers and with weaker attachment to the labor market.

We then translate these reduced-form effects into elasticities of maternal labor supply with respect to household disposable income and benefit generosity. These elasticities are local and policy-relevant, capturing behavioral responses to discrete increases in transfers rather than to wages. We find substantial heterogeneity across the ISEE distribution: elasticities are small at lower levels of benefit increases and rise sharply for comparisons among intermediate adjacent treatment-intensity groups. The magnitudes we obtain are broadly consistent with estimates in the literature on income effects in labor supply.

The paper contributes to three strands of literature. First, it adds to the work on the effects of child-related benefits on fertility and maternal labor supply by studying a permanent and predictable transfer that affects households over a long horizon, rather than short-lived or one-off payments. For example, González (2013) and González and Trommlerová (2023) study the introduction and subsequent cancellation of a lump-sum universal child benefit in Spain and find positive fertility responses and a reduction in maternal labor supply in the short run, followed by a decline in birth rates after the elimination of the benefit. Havnes and Mogstad (2011) examine an expansion of subsidized childcare in Norway and document little effect on maternal employment. Using administrative data from Austria, Kleven et al. (2024) show that family policies such as parental leave and childcare subsidies have limited effects on gender convergence in the labor market[1]. Our analysis complements this literature by focusing on a permanent cash transfer and by jointly studying fertility and labor supply responses within a unified framework.

Second, our study contributes to the literature on income effects and non-wage labor supply elasticities. For the United States, Hotz and Scholz (2001) document heterogeneous labor supply

[1]Other related empirical studies focusing on the effect of child-related transfers on fertility in a natural experiment setting are: Milligan (2005) for Canada, Cohen et al. (2013) for Israel, Riphahn and Wiynck (2017) for Germany, and Kim (2024) for South Korea.

responses to expansions of tax credits targeted to families with children, with positive effects for single mothers and negative effects for married mothers. More recently, Jensen and Blundell (2024) exploit an unexpected cap on child benefit payments in Denmark and estimate sizable labor supply elasticities with respect to non-labor income. Our setting allows us to estimate elasticities both with respect to household disposable income and to benefit generosity, while accounting for the fact that households may internalize the future stream of payments implied by the design of the transfer.

Third, from a methodological perspective, we build on recent econometric advances by Callaway and Sant'Anna (2021) and Callaway et al. (2024) and provide an application that illustrates how heterogeneous treatment intensity can be exploited to recover causal effects in settings without untreated units, a feature common to many large-scale policy reforms.

The rest of the paper is structured as follows. Section 2 clarifies the institutional context; Section 3 describes the data; Section 4 outlines empirical strategy, identification assumptions, and sample selection criteria; Sections 5, 6, and 7 present and discuss the main results on fertility and female labor supply, as well as robustness checks and additional results on alternative samples; Section 8 concludes.

2 Institutional Context

The *Assegno Unico Universale* (AUU) is a comprehensive reform of the Italian system of child-related support, introducing an unconditional and universal monthly cash benefit for all households with at least one dependent child. The reform extends eligibility beyond dependent employees to groups previously only partially covered by child-related measures—most notably the unemployed and the self-employed—and represents a major shift towards a unified system of family support.

The benefit is paid for each child starting from the seventh month of pregnancy up to age 18. Eligibility is extended up to age 21 if the child is still economically dependent on the household, although with reduced amounts.

The AUU amount depends on household economic resources as measured by the *Indicatore della Situazione Economica Equivalente* (ISEE), and on number of children. The ISEE is a synthetic indicator that combines information on income, assets, and an equivalence scale reflecting household size and composition. It is widely used in Italy to determine eligibility and benefit levels for means-tested programs.

To receive the benefit, either parent—or the child if aged 18 or above—must submit an online application reporting the household's ISEE and basic demographic information. Eligibility is then renewed automatically, conditional on updating the ISEE on a yearly basis. If the ISEE is not reported, or if it exceeds 40,000 euros, the benefit is paid at the minimum amount (50 euros per child per month).

The pre-reform system. Before the AUU, child-related support was provided through two main instruments: the *Assegni per il Nucleo Familiare* (ANF) and the *Detrazioni Fiscali per Figli a Carico* (DF). Importantly, these instruments differed substantially in coverage and in the income concept used to determine eligibility and amounts.

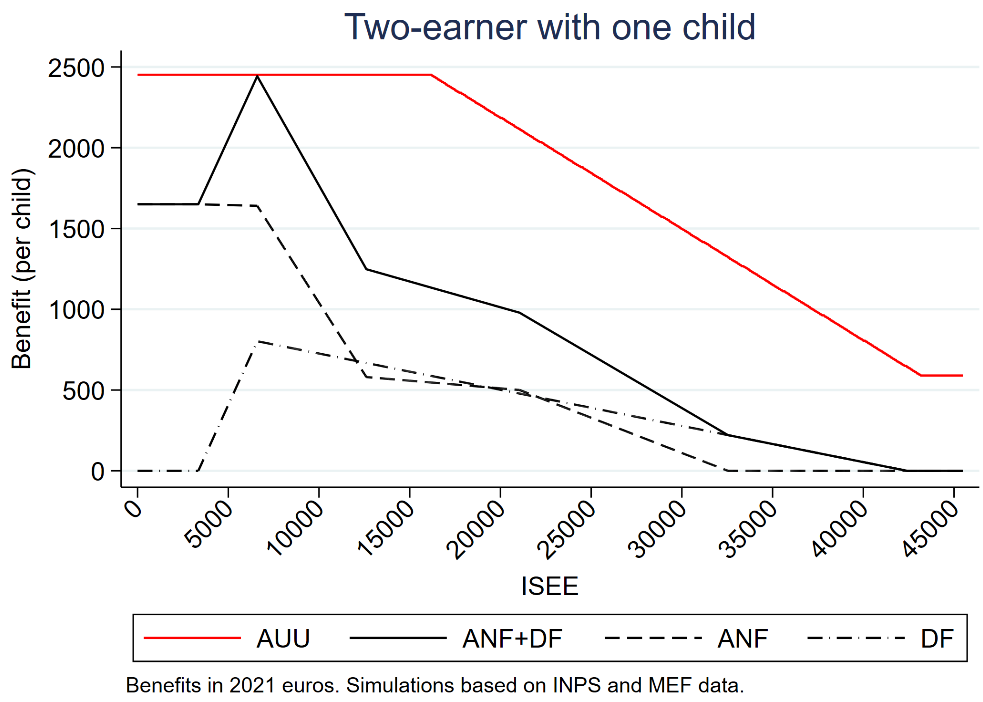
First, the ANF consisted of a monthly cash transfer targeted to dependent employees (and some categories of pensioners). Eligibility and benefit amounts depended on household income and family size, and the schedule was non-linear with discontinuities across income brackets and household types. Self-employed and unemployed individuals were generally excluded from ANF coverage, implying that pre-reform cash support for these groups was limited or absent.

Second, the DF were tax credits for dependent children based on taxable income. Amounts declined with income and varied with the number and age of children. Unlike the ANF, the DF applied more broadly but were tied to tax liability, hence providing limited support to households with zero or low taxable income.

The reform. The AUU replaced both the ANF and the DF with a single transfer scheme, with the stated goals of simplifying the tax-benefit system and extending child-related support to a larger share of households. The reform is not revenue neutral: once fully phased in, it entails annual spending of approximately 19.5 billion euros, representing roughly 7 billion euros in additional expenditure relative to the previous system(INPS, 2021).

Figure 1 illustrates annual child-related amounts under the pre-reform system (ANF and DF) and under the AUU as a function of ISEE for a representative household with two earners and one child. Similar figures for other household compositions are reported in Appendix B. The figure highlights the heterogeneous change in generosity induced by the reform across the ISEE distribution. The reform was announced in July 2021 and implemented in March 2022, when the first AUU payments were disbursed.

Figure 1: Pre and post reform yearly benefits as a function of ISEE – employees



Note: Annual pre-reform (ANF and DF) and post-reform (AUU) benefits as a function of ISEE.

3 Data

We use administrative data from the Italian Social Security Institute (INPS) covering the universe of applications submitted to receive the *Assegno Unico Universale* (AUU). The dataset spans the period 2021–2024, starting from the year in which the reform was announced and households filed the first applications, and includes all applications leading to benefit payments beginning in March 2022.

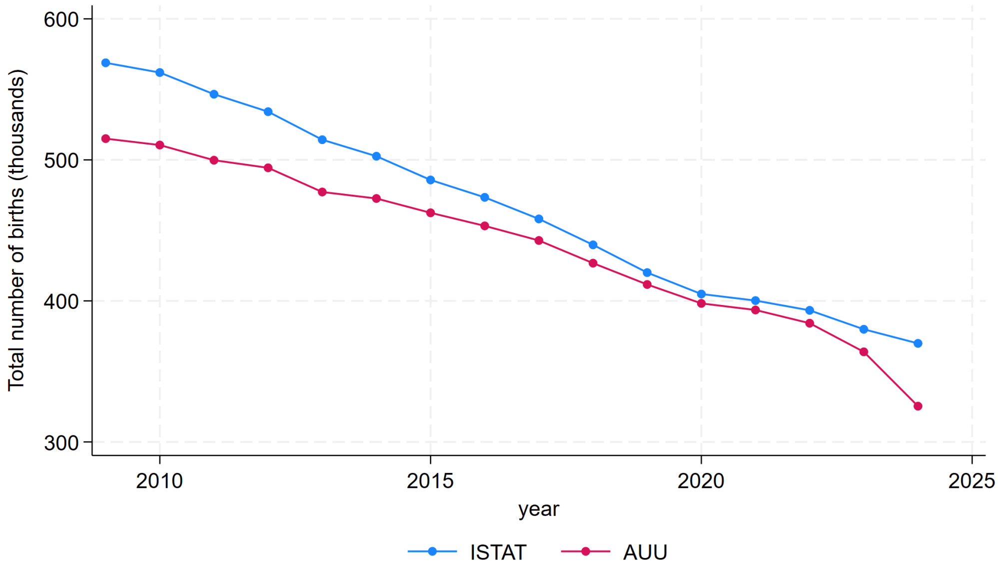
For each child for whom an application is filed, the data report the child's date of birth, an anonymized personal identifier (*codice fiscale*), municipality and province of residence, the monthly amount of AUU received, and the household's ISEE used for benefit determination. The records also include the parents' anonymized identifiers and dates of birth, which allow us to link children to their mothers and fathers.[2]

An important feature of these data is that they allow us to reconstruct mothers' fertility histories going backwards in time. Starting from the first observed child—or the oldest child younger than 21—we observe subsequent births as long as an AUU application is filed for each

[2]For the purpose of AUU determination, the relevant economic condition is assessed at the level of both parents, regardless of marital status or cohabitation. When the ISEE is not reported in the application, the benefit is automatically paid at the minimum amount corresponding to ISEE levels above 40,000 euros.

child. Figure 2 compares the annual number of births between 2010 and 2024 as recorded in the AUU data with the corresponding figures from the Italian National Statistical Institute (ISTAT). The two series closely overlap in the period 2020–2023, indicating near-universal take-up of the AUU among households with recently born children. The divergence observed for earlier years reflects lower take-up for older children, while the lower number of AUU-recorded births in 2024 is due to delays between childbirth and the filing of the first AUU application.

Figure 2: Number of births per year, ISTAT versus AUU



Using the anonymized personal identifiers, we link mothers in the AUU data to longitudinal labor market records maintained by INPS. These records provide detailed information on employment status, weeks worked, part-time versus full-time contracts, occupation, industry, sector, and contract type (temporary or open-ended), starting from the individual's entry into the labor market. The availability and quality of this information are particularly high for private-sector employees, motivating our focus on this group in the empirical analysis.

While the AUU data provide the ISEE level used for benefit determination, they do not include the underlying components used to compute ISEE, such as the breakdown of income sources or information on assets. Moreover, we do not observe complete information on taxable income that would be required to mechanically replicate pre-reform tax deductions for dependent children. These data limitations motivate the simulation-based approach used to construct pre-reform benefit amounts and to define treatment intensity, which is described in detail in Section 4.3 and Appendix B.

We further enrich the dataset by linking province-level characteristics to each child's place of residence. These include geographic location (North or South), the availability of public childcare services, and average wages. To proxy for local cost-of-living differences, we construct a measure of real wages by deflating average nominal wages at the province level using a local price index that combines housing prices and consumer prices (see Boeri et al. (2021)). Provinces are then classified as having high or low cost of living based on whether their real average wage lies above or below the median.

Our final dataset consists of a monthly panel of mothers observed from July 2020 to June 2024. We restrict the analysis to this four-year time window because AUU records for earlier years are less reliable due to lower take-up among households with older children, while data for the final months of 2024 are affected by reporting lags. For each mother-month observation, we construct indicators for employment status and labor supply, as well as a variable recording whether a birth occurs in that month. We also record the number of children a mother has at entry into the sample and detailed information on her pre-sample labor market attachment based on employment histories in the period 2012–2019.

4 Empirical Strategy

4.1 Timing of Events and Outcomes

The AUU reform was announced in July 2021 and implemented in March 2022, when the first monthly transfers were paid. We exploit this institutional timing to study the effects of the reform on maternal labor supply and fertility decisions, which operate on different time horizons.

Our dataset spans from July 2020 to June 2024 and allows us to observe mothers' fertility and labor market outcomes at a monthly frequency. Throughout the analysis, outcomes are aggregated at the quarterly level and event time is indexed in quarters relative to the relevant event time. We define two different event times depending on the outcome of interest.

For labor market outcomes, the relevant event is the announcement of the reform in July 2021. The announcement marks the point at which households become aware of the permanent introduction of the transfer and may adjust labor supply decisions in anticipation of future payments. We therefore define July 2021—and in particular the quarter from July 2021 and September 2021—as the cut off between pre and post treatment for labor market outcomes and study the dynamic response of employment and working arrangements in the quarters before and after the announcement.

For fertility outcomes, the relevant event is shifted forward to account for biological constraints. Given the time required for conception and pregnancy, we define September 2022—-approximately 15 months after the announcement—-as the cut-off for fertility outcomes (the corresponding quarter spans the period between July 2022 and September 2022). This timing captures the earliest period in which the reform could plausibly affect completed births rather than conceptions alone.

4.2 Treatment Intensity and Empirical Design

The AUU reform generates variation in treatment intensity in terms of the *change* in child-related transfers induced by the policy, rather than in the level of benefits received. All households with children experience a positive change in transfers following the reform, but the magnitude of this change varies with household economic conditions and family composition.

As a result, there are no untreated units in our setting. Instead, all mothers are exposed to the reform, but with heterogeneous treatment intensities corresponding to different reform-induced increases in child-related support.

Our empirical strategy exploits this heterogeneity by comparing outcomes across groups of mothers exposed to different treatment intensities. Following Callaway and Sant'Anna (2021) and the recent literature on difference-in-differences designs with multi-valued treatments (Callaway et al., 2024), we focus on comparisons between *adjacent* treatment-intensity groups.

Let $D \in \{d_1, d_2, \ldots, d_K\}$ denote discrete treatment intensities, where higher values of d correspond to larger reform-induced increases in child-related transfers. For two adjacent treatment intensities $l < h$, our parameter of interest is:

$$ATT(h \mid h) - ATT(l \mid l),$$

where

$$ATT(d \mid d) = \mathbb{E}\left[Y_t(d) - Y_t(0) \mid D = d\right]$$

denotes the average treatment effect of experiencing treatment intensity d for units that actually experienced d.

This estimand captures the causal effect of a *discrete and local* increase in treatment intensity by comparing mothers exposed to slightly different reform-induced changes in benefits. Details on the construction of treatment-intensity groups are provided in Section 4.3.

4.3 Construction of Treatment Intensity

Target population. The AUU reform extended eligibility to population groups that were partially or entirely excluded from pre-reform measures, including unemployed and self-employed individuals. However, while fertility outcomes can be observed for these groups, the administrative data available to us do not provide comparable longitudinal information on labor market participation and earnings for unemployed and self-employed individuals. Detailed and consistent labor market histories are observed primarily for private-sector employees in the INPS records. As a result, including unemployed and self-employed individuals would prevent a coherent analysis of the joint dynamics of fertility and labor market outcomes before and after the reform. For this reason, our empirical analysis focuses on the population of private-sector employees, for whom both fertility and labor market information can be consistently observed over time.

From income-based pre-reform rules to an ISEE-based schedule. While post-reform AUU amounts are directly observed in the administrative data, the pre-reform counterfactual level of child-related support must be reconstructed based on the institutional rules governing the ANF and DF. A key challenge is that pre-reform benefit amounts depend on labor income and/or taxable income, whereas post-reform AUU amounts are determined by household ISEE. In the administrative data, we observe the ISEE level used for benefit determination but not its underlying components, such as the breakdown of income sources and assets, nor complete information on taxable income.

To construct reform-induced changes in benefit generosity by ISEE, we proceed as follows. For each ISEE level and household composition (number of earners and number of children), we simulate pre-reform ANF and DF amounts using official schedules and available policy simulators. The mapping from ISEE to the income concept required by pre-reform rules is obtained by assuming that household economic resources are entirely driven by labor income. Under this assumption, a given ISEE level corresponds to an implied level of labor income, which can then be used to compute ANF and DF entitlements.

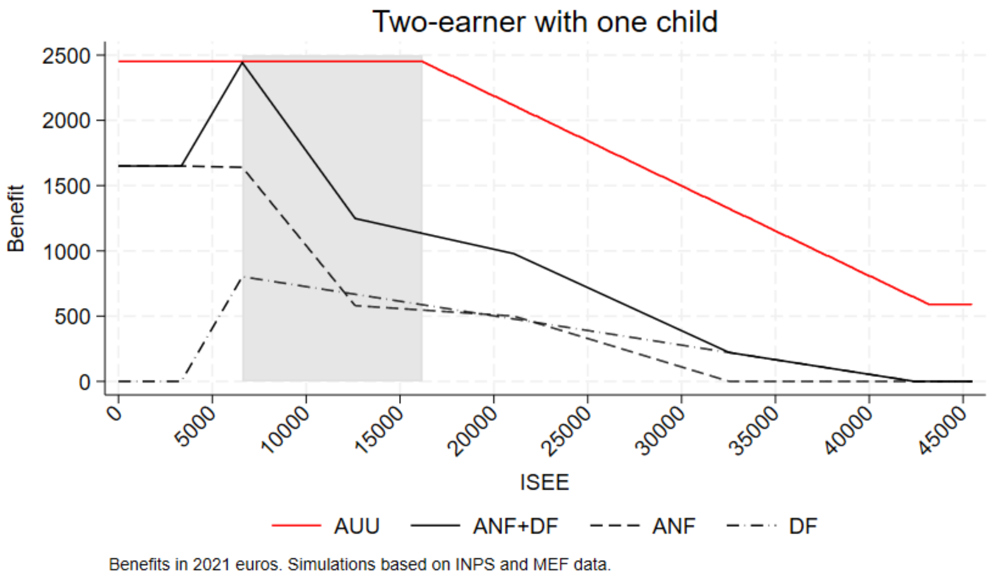
This approach constitutes an approximation. It is motivated by the fact that our empirical analysis focuses on the lower and middle part of the ISEE distribution and on households whose primary source of economic resources is labor income. Importantly, our identification relies on relative differences in reform-induced changes across adjacent ISEE groups rather than on the exact level of simulated pre-reform benefits. Appendix B provides a detailed description of the simulation procedure and underlying assumptions.

Choice of the ISEE measure. ISEE is updated annually, and households receive the minimum benefit if an updated ISEE is not reported. To avoid endogeneity concerns arising from post-reform changes in labor supply or fertility affecting contemporaneous ISEE, we assign treatment intensity using the ISEE reported in early 2023 (submitted by April 2023). This ISEE reflects household economic conditions in 2021, the year in which the reform was announced, and is therefore predetermined with respect to post-announcement behavioral responses.

A related concern is that ISEE is a dynamic measure that responds to changes in household composition and income. In principle, the labor supply responses we study could themselves affect ISEE and, consequently, the generosity of the benefit, which is decreasing in ISEE. We argue that this concern is mitigated by our focus on short-run labor supply responses, which are unlikely to generate sizable contemporaneous changes in the ISEE measure used for treatment assignment. As an additional robustness check, we replicate the analysis using ISEE reported in 2022. While this alternative measure has the drawback of reflecting households' economic conditions during the COVID-19 period, the results remain qualitatively similar, albeit smaller in magnitude. These findings are reported in Section 6.

Restriction of the analysis sample. Finally, we restrict the analysis to an interval of the ISEE distribution where the simulated reform-induced change in child-related transfers varies smoothly and monotonically with ISEE (see Figure 3).

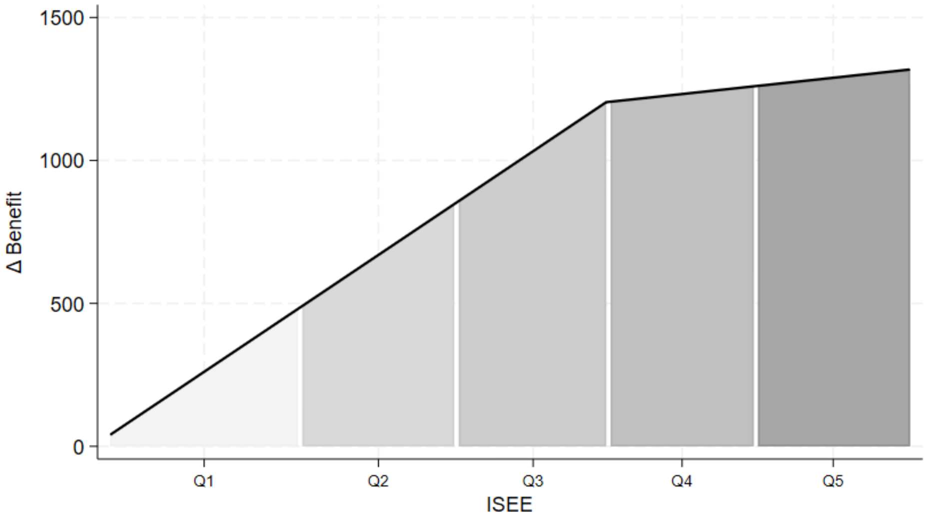
Figure 3: Pre and post reform yearly benefits: ISEE interval used for the analysis



Note: Annual pre-reform (ANF and DF) and post-reform (AUU) benefits as a function of ISEE. The shaded grey area indicates the ISEE interval used in the empirical analysis, which excludes households eligible for means-tested income support and focuses on the range where the reform-induced change in benefits varies smoothly with ISEE.

In particular, we focus on the ISEE interval 6,750-16,200 in order to exclude very low-ISEE households eligible for the minimum-income scheme (*Reddito di Cittadinanza*), for which changes in disposable income may reflect interactions between multiple welfare policies. Within the selected interval, we partition ISEE into quintiles and use these groups to define discrete treatment-intensity categories, as shown in Figure 4.

Figure 4: Increase in benefit generosity after AUU introduction by ISEE quintiles.



4.4 Identification Assumptions

In the absence of untreated units, identification relies on comparing outcome changes across groups exposed to different treatment intensities. Specifically, identification of the causal effect

$$\mathbb{E}\left[Y_t(h) - Y_t(l) \mid D = h\right]$$

requires that lower-intensity groups provide a valid counterfactual for higher-intensity groups in terms of the evolution of outcomes absent the difference in treatment intensity. This condition is referred to as the *Strong Parallel Trends* (SPT) assumption (Callaway et al., 2024).

Formally, SPT requires that the evolution of outcomes experienced by lower-intensity units under treatment intensity l reflects how higher-intensity units' outcomes would have evolved had they instead experienced intensity l. Importantly, this assumption cannot be directly tested, as it concerns counterfactual outcomes under alternative treatment intensities.

We do not require SPT to hold globally across the entire support of treatment intensities. Given our focus on comparisons between adjacent treatment-intensity groups, it is sufficient that SPT holds locally between neighboring groups. This local version of SPT is more plausible in settings where treatment intensity varies smoothly with observable characteristics.

The plausibility of this assumption is further enhanced by conditioning on a rich set of pre-treatment covariates, including mother's year of birth, year of birth of the first child, province of residence, and sector of employment. In addition, we assess pre-treatment dynamics using event-study estimates and conduct placebo exercises to detect potential violations of the assumption.

4.5 Estimation

We estimate dynamic treatment effects using the doubly robust difference-in-differences estimator developed by Sant'Anna and Zhao (2020) and extended to multiple periods by Callaway and Sant'Anna (2021). Estimation is implemented using the `csdid` command in Stata.

Our baseline specification relies on the default `dripw` estimator, which combines stabilized inverse probability weighting with outcome regression. Identification relies on a conditional parallel trends assumption, whereby potential outcomes would have evolved similarly across adjacent treatment-intensity groups conditional on a set of predetermined covariates. Accordingly, in the estimation we condition on mother's year of birth, the year of birth of the oldest child, the occupational sector in the last observed job, and province of residence.

For each adjacent pair of treatment-intensity groups, we implement an event-study design in which mothers in the higher-intensity group are treated as the treated units and mothers in the immediately lower-intensity group serve as the comparison group. This procedure yields group-time average treatment effects on the treated, $ATT(d \mid d, t)$.

Our main parameters of interest are differences in treatment effects across adjacent treatment-intensity groups,

$$ATT(h \mid h, t) - ATT(l \mid l, t),$$

which capture the effect of a discrete increase in treatment intensity under the identifying assumptions discussed above. Standard errors are clustered at the individual level to account for serial correlation in outcomes over time.

4.6 Sample

Our main analysis focuses on mothers who were of fertile age at the time of the reform announcement and for whom fertility and labor supply responses to the AUU are most relevant. Specifically, we select women born between 1975 and 2001, who were aged 20–45 in 2021, the year in which the reform was announced.

We restrict the sample to mothers who had exactly one child prior to the announcement of the reform and whose first child was born after 2010, so that the oldest child was at most 10 years old at the time of announcement. This restriction ensures that all women in the sample are plausibly at risk of having an additional child during the period of observation and allows us to study both the probability and the timing of second births.

Consistent with the empirical strategy described in Section 4, we focus on private-sector employees, for whom INPS labor market data are most complete and reliable. We further require mothers to have been employed at least once in the period 2012–2019, which provides information on pre-reform labor market attachment and allows us to observe labor supply adjustments following the announcement of the reform without confounding them with first-time labor market entry.

The resulting sample consists of approximately 245,000 mothers observed in a monthly panel from July 2020 to June 2024. The time dimension of the data is aggregated from monthly to quarterly level to reduce noise in the empirical analysis. Hence, each ISEE quintile contains roughly 49,000 mothers observed over 16 quarters.

Table 1 reports summary statistics by ISEE quintile. Employment rates increase monotonically with ISEE, ranging from 46% in the lowest quintile to 63% in the highest quintile. Conditional on employment, the share of women working part-time decreases from 67% in the bottom quintile to 54% in the top quintile. Labor market attachment, defined as continuous employment while in the sample, also increases with ISEE but remains below 40% even in the highest quintile.

Fertility outcomes are relatively homogeneous across ISEE groups. The probability of having a second child during the sample period is approximately 24% in all quintiles. Among mothers who have a second child, the average gap between the first and second birth is about 19 quarters (i.e. slightly less than five years). Demographic characteristics are also balanced across ISEE quintiles: the average age of mothers at announcement is around 35 years old, the average year of birth of the oldest child is approximately 2015, and about 38% of mothers experience a second birth between July 2020 and June 2024.

Table 1: Descriptive Statistics of Main Sample of Mothers

	Q1	Q2	Q3	Q4	Q5
working	0.46	0.50	0.55	0.60	0.63
days worked (conditional)	21	21	21	22	22
Part Time (conditional)	0.67	0.63	0.60	0.57	0.54
attached	0.20	0.23	0.27	0.31	0.35
2nd birth	0.24	0.24	0.24	0.24	0.23
gap between births (quarters)	19	19	19	19	19
age of mother	35	35	35	36	36
year of birth of oldest child	2015	2015	2015	2016	2016
had child in sample	0.38	0.39	0.38	0.38	0.37
Observations	2,336,158	2,336,325	2,336,398	2,336,487	2,336,318
Individuals	48,669	48,673	48,674	48,676	48,673

5 Results

This section presents the main results on the effects of the AUU reform on fertility and maternal labor supply. Our empirical strategy exploits heterogeneity in the reform-induced change in benefit generosity by comparing adjacent treatment-intensity groups, as described in Section 4.

Throughout this section, we focus on the main analysis sample of mothers of one child who were of fertile age at the time of the reform announcement and who were employed in the private sector prior to the reform. Results are presented separately for fertility and labor supply outcomes. For each outcome, we first report estimates from our baseline specifications in tabular form and then use event-study figures to illustrate the dynamic pattern of the effects and assess the plausibility of the identifying assumptions.

5.1 Fertility

We begin by analyzing the effects of the AUU reform on fertility outcomes, focusing on the probability of having a second child. Table 2 reports estimates of the effect of a discrete increase in benefit generosity across adjacent ISEE quintiles. To improve readability, we report event-study estimates aggregated over economically meaningful time windows. The pre-treatment period (*Pre*) spans from July 2020 to June 2022 (eight quarters). We exclude the transition quarter between July and September 2022. The *Short-run* period covers October 2022 to September 2023 (four quarters), while the *Medium-run* period extends from October 2023 to June 2024 (three quarters). Full dynamic event-studies profiles are reported in Appendix D.

The results indicate a positive and statistically significant effect on fertility only for the comparison between the third and the second ISEE quintile. For this group, the rise in benefit generosity induced by the reform leads to an increase of 0.5 percentage points in the probability of having a second child in the medium run, that is starting from one year (4 quarters) after September 2022. In contrast, we find no statistically significant effects for the comparison between the first and second quintiles, nor for higher-intensity comparisons. While point estimates for the upper part of the ISEE distribution are generally of the same sign, they are smaller in magnitude and imprecisely estimated.

Table 2: AUU reform and fertility: event-study estimates aggregated over time windows

	Q2vsQ1	Q3vsQ2	Q4vsQ3	Q5vsQ4
Pre(avg.)	0.11	0.05	0.02	-0.05
	0.03	0.03	0.03	0.03
Short run (avg.)	0.01	0.08	0.11	0.02
	0.13	0.12	0.12	0.12
Medium run (avg.)	0.03	0.47	0.07	0.31
	0.18	0.18	0.18	0.18
Observations	1,525,452	1,525,530	1,525,566	1,525,489

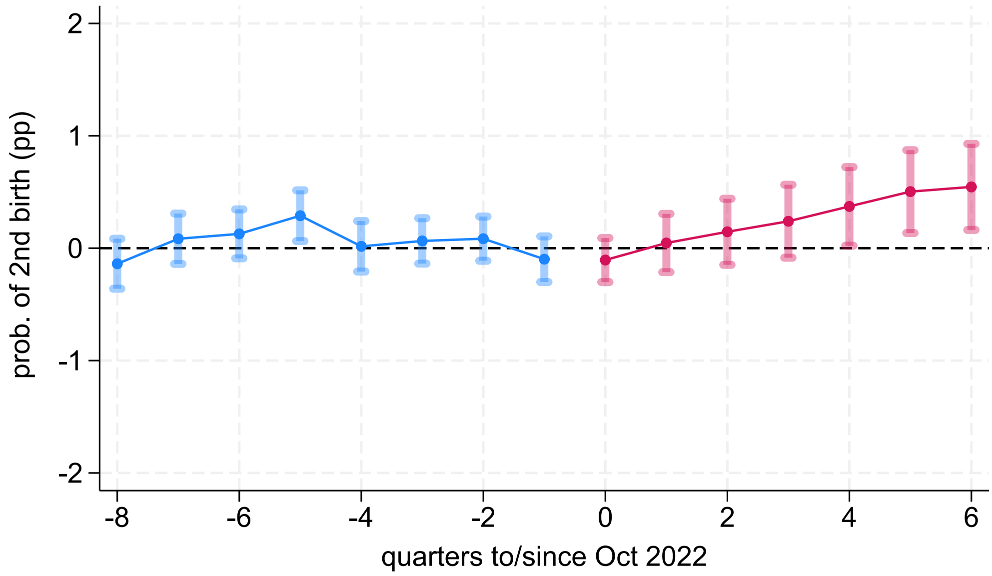
Notes: Entries report event-study estimates aggregated over time windows. Pre is $k \in [-8, -1]$, Short run is $k \in [0, 3]$, Medium run is $k \in [4, 6]$, where k denotes event time (quarter) relative to the omitted baseline period (the last pre-reform period). Each column compares adjacent ISEE quintiles. Standard errors are clustered at the individual level. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

This pattern suggests that fertility responses to the AUU reform are not linear in benefit generosity. Rather than increasing smoothly with the size of the transfer, fertility effects appear to be concentrated around a specific range of reform-induced benefit increases. One interpretation is that the reform affects fertility decisions primarily when the increase in transfers is sufficiently large to relax binding financial constraints, consistent with the presence of threshold effects.

Figure 5 illustrates the dynamic effects for the comparison between the third and second ISEE quintile. The event-study coefficients show no evidence of differential pre-trends prior to the reform, supporting the plausibility of the identifying assumptions. Following the reform, the probability of having a second child increases and remains persistently higher for mothers in the third quintile of the ISEE distribution relative to those in the second quintile.

As for the magnitude of the effect, a 0.5 percentage points estimate translates into an increase in the probability of a second birth of about 2% with respect to a sample average of 24% in this group of mothers.

Figure 5: Probability of 2nd Child: Q_3 vs Q_2



Note: Event-study estimates of the effect of the AUU reform on the probability of a second child for the comparison between the third and second ISEE quintiles.

Using the same identification strategy, we also look at the potential effect of the reform on the timing of births in terms of the gap in quarters between the first and the second births for those women who had a second child. Table 3 shows that, in general, the introduction of the AUU has not had any relevant effect on the time gap between the first and second births, except for a marginally significant reduction of the gap in the comparison between the second and first ISEE quintiles which suggests a tendency to anticipate the decision to have a second child in this subgroup of mothers.

Table 3: Gap between 1st and 2nd Births (Quarters)

	Q1 vs Q2	Q2 vs Q3	Q3 vs Q4	Q4 vs Q5
post	-0.429*	0.082	0.065	0.047
	(0.210)	(0.216)	(0.211)	(0.207)
Obs	36,230	36,465	36,117	35,406

Note: controls for mom's cohort of birth, province fe. Standard errors in parentheses. ${}^{*}p < 0.05$, ${}^{**}p < 0.01$, ${}^{***}p < 0.001$.

5.2 Labor Supply

5.2.1 Extensive Margin

We next turn to maternal labor supply responses to the AUU reform. Table 4 reports estimates of the effect of a discrete increase in benefit generosity on mothers' employment outcomes across adjacent ISEE quintiles. Event-study estimates are aggregated over economically meaningful time windows. The *Pre* announcement period spans from July 2020 to June 2021 (four quarters); the *Anticipation* period spans from October 2021 to March 2022, when first payments started (two quarters); the *Short* period covers the following four quarters, and the *Medium* run the last five quarters. The full dynamic event-studies profiles are in Appendix D.

Table 4: AUU reform and labor supply: event-study estimates aggregated over time windows

	Q2vsQ1	Q3vsQ2	Q4vsQ3	Q5vsQ4
Pre(avg.)	0.04	-0.01	-0.03	0.12
	0.06	0.06	0.06	0.06
Anticipation(avg.)	-0.06	0.08	-0.03	0.13
	0.17	0.17	0.16	0.15
Short run (avg.)	-0.43	-0.46	-0.34	-0.59
	0.23	0.22	0.22	0.20
Medium run (avg.)	-0.42	-1.15	-0.55	-1.02
	0.28	0.28	0.27	0.26
Observations	1,525,452	1,525,530	1,525,566	1,525,489

Notes: Entries report event-study estimates aggregated over time windows. Pre is $k \in [-, 4 - 1]$, Anticipation is $k \in [0, 1]$, Short run is $k \in [2, 6]$, Medium run is $k \in [7, 10]$, where k denotes event time (quarter) relative to the omitted baseline period (the last pre-reform period). Each column compares adjacent ISEE quintiles. Standard errors are clustered at the individual level.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Labor supply responses exhibit a highly heterogeneous pattern throughout the ISEE distribution. We find evidence of reduced labor supply in the medium run for some, but not all, adjacent treatment-intensity comparisons. In particular, the estimated effects are larger and statistically significant for the comparison between the third and second quintiles and, to a lesser extent, for the comparison between the fifth and fourth quintiles. In contrast, we do not detect significant effects for the comparison between the third and fourth quintiles.

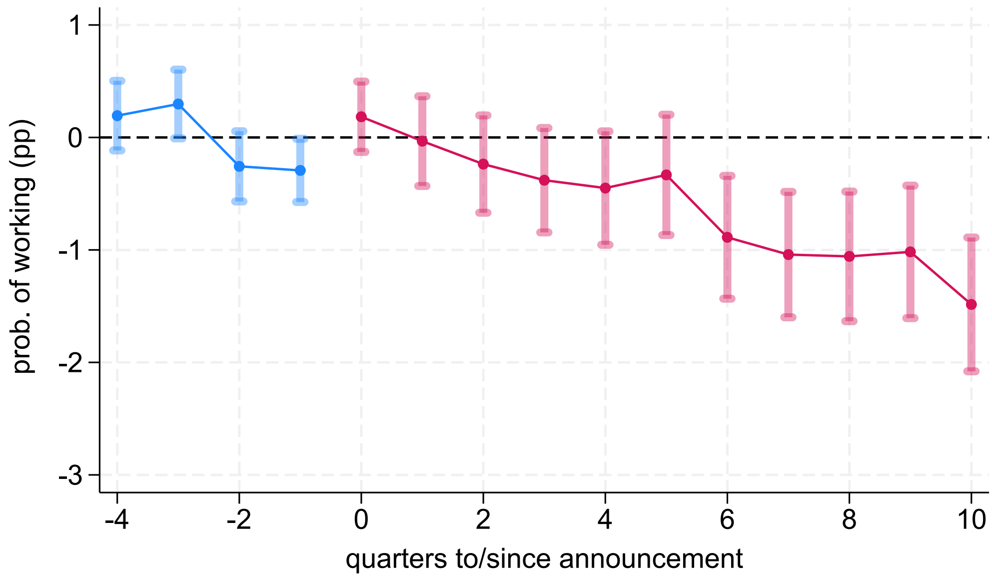
The absence of a monotonic pattern across treatment-intensity groups suggests that labor supply responses to the AUU reform are non-linear. This is consistent with standard labor supply models in which the response to non-labor income depends on households' position along the labor supply schedule, the presence of constraints, and the margin of adjustment that is most relevant. As a result, similar increases in benefit generosity may generate heterogeneous labor supply responses across the income distribution.

Figure 6 presents event-study estimates for the comparison between the third and second ISEE quintile. The estimates indicate no differential pre-trends prior to the reform and a post-reform decline in labor supply that persists over time.

In terms of magnitudes, the increase in benefit generosity leads to a reduction of 1.15 percentage points in the probability of working for the comparison between the third and second quintiles in the medium run. This corresponds to a decrease of about 2% relative to a baseline maternal employment rate of roughly 55%.

Together with the tabular results, these findings suggest that the AUU reform affected maternal labor supply for specific segments of the income distribution, rather than generating uniform responses across all households.

Figure 6: Labor supply effects: Q_3 vs Q_2



Note: Event-study estimates of the effect of the AUU reform on maternal labor supply for the comparison between the third and second ISEE quintiles.

Labor supply effects net of fertility adjustments. To assess the extent to which the observed labor supply responses are mediated by fertility adjustments, we exclude from the estimation mothers who had an additional child during the sample period[3]

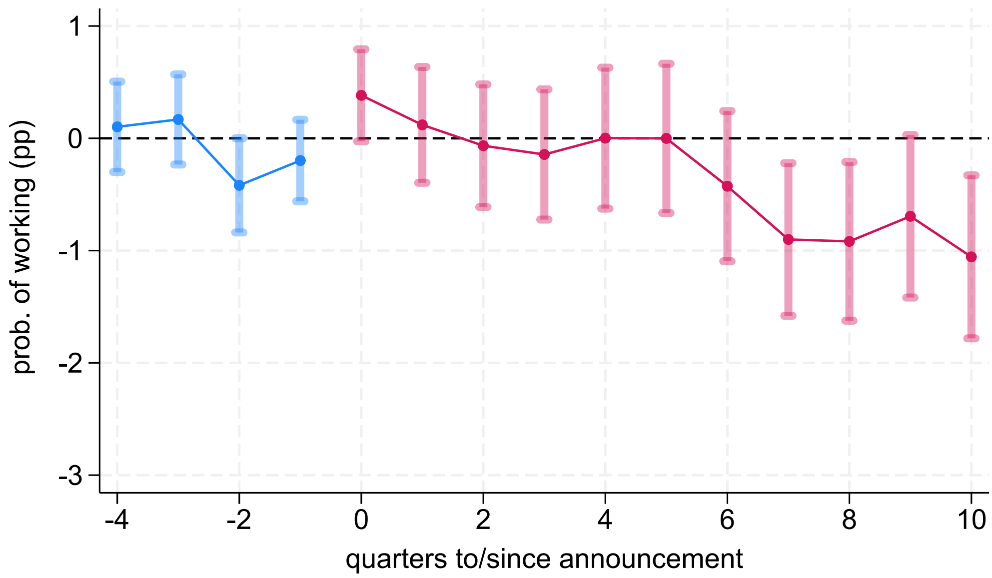
[3]By construction, this sample restriction shuts down the fertility channel, albeit at the cost of introducing endogenous selection, and should therefore be interpreted as a descriptive robustness check rather than as a separate causal analysis.

Given that fertility responses to the AUU reform are concentrated in the comparison between the third and second ISEE quintiles, we focus on this same comparison when assessing whether labor supply effects persist after excluding mothers who had an additional child during the sample period.[4]

Figure 7 reports the corresponding event-study estimates. We find that the negative labor supply effect following the announcement of the AUU reform persists in this more restricted sample for the same treatment-intensity comparisons and with similar timing as in the main analysis. However, the magnitude of the effect is smaller relative to that estimated in the full sample.

Taken together, these results suggest that the reduction in labor supply observed after the reform is only partly driven by fertility responses. While childbirth-related labor supply adjustments contribute to the overall effect, a non-negligible share of the decline in employment at the extensive margin appears to reflect direct labor supply responses to the increase in benefit generosity.

Figure 7: Labor supply effects excluding mothers with additional births: Q_3 vs Q_2



Note: Event-study estimates of the effect of the AUU reform on maternal labor supply for the comparison between the third and second ISEE quintiles. Estimates exclude mothers who had an additional child during the sample period.

5.2.2 Intensive margin

We examine labor supply responses along the intensive margin by studying changes in the probability of working part-time versus full-time among mothers who remain continuously

[4]Event-study estimates for all quintiles comparisons are reported in Table 22 in Appendix D

employed throughout the period 2020–2024. By restricting the sample to continuously employed mothers, this analysis isolates adjustments in working hours from employment entry and exit decisions.

Table 5 reports estimates of the effect of the AUU reform on the probability of working part-time, relative to full-time, across adjacent ISEE quintiles and aggregated over relevant event time windows. Consistent with the results on labor supply at the extensive margin, we find that the reform is associated with an increase in part-time work among lower-income mothers.

Table 5: AUU reform and working part-time: event-study estimates aggregated over time windows

	Q2vsQ1	Q3vsQ2	Q4vsQ3	Q5vsQ4
Pre(avg.)	0.07	0.04	0.05	-0.01
	0.07	0.07	0.06	0.06
Anticipation(avg.)	0.34	0.15	0.13	-0.03
	0.19	0.18	0.17	0.16
Short run (avg.)	0.65	0.60	0.27	0.32
	0.30	0.28	0.26	0.25
Medium run (avg.)	1.27	0.94	0.70	0.53
	0.40	0.36	0.34	0.33
Observations	410,946	477,899	551,844	622,352

Notes: Entries report event-study estimates aggregated over time windows. Pre is $k \in [-, 4-1]$, Anticipation is $k \in [0, 1]$, Short run is $k \in [2, 6]$, Medium run is $k \in [7, 10]$, where k denotes event time (quarter) relative to the omitted baseline period (the last pre-reform period). Each column compares adjacent ISEE quintiles. Standard errors are clustered at the individual level.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Specifically, the estimated effects are positive across all adjacent treatment-intensity comparisons, but they are statistically significant only for mothers in the lower part of the ISEE distribution—namely for the comparison between the second and first quintiles and between the third and second quintiles.

In terms of magnitude, the increase in benefit generosity induced by the reform leads to an increase in the probability of working part-time of approximately 1.27 percentage points for the comparison between the second and first quintiles in the medium run, corresponding to an increase of about 2% relative to a baseline part-time rate of roughly 60% among continuously employed mothers.

5.3 Elasticities

To summarize the magnitude of the estimated labor supply responses, we compute elasticities that relate the estimated treatment effects to the size of the reform-induced change in economic resources. Given the structure of the AUU reform and our empirical design, these elasticities should be interpreted as *reduced-form and local elasticities*, capturing behavioral responses to discrete increases in transfer generosity across adjacent ISEE groups.

Importantly, elasticities reported in this section are not directly estimated parameters. The numerator of each elasticity comes from the causal estimates of labor supply responses, while the denominator is entirely constructed using simulation-based measures of reform-induced changes in benefits and household income, based on predetermined ISEE levels. As a result, the magnitude of these elasticities depends on the maintained assumptions used to map ISEE into pre- and post-reform benefits and household income, which mirror those underlying the empirical identification strategy (see Section 4.3).

Definition. For each adjacent ISEE comparison q versus $q-1$, we compute two elasticities. First, an elasticity with respect to household income:

$$\varepsilon_{L,Y}^{(q,q-1)} = \frac{\%\Delta L_{q,q-1}}{\%\Delta Y_{q,q-1}}, \tag{1}$$

where $\%\Delta L_{q,q-1}$ denotes the percentage change in maternal labor supply implied by the estimated treatment effect, and $\%\Delta Y_{q,q-1}$ is the corresponding percentage change in household disposable income implied by the reform.

Second, we compute an elasticity with respect to benefit generosity:

$$\varepsilon_{L,G}^{(q,q-1)} = \frac{\%\Delta L_{q,q-1}}{\%\Delta G_{q,q-1}}, \tag{2}$$

where $\%\Delta G_{q,q-1}$ captures the differential reform-induced gain in child-related transfers, defined as the difference in gains between the two groups, $G_q - G_{q-1}$, expressed relative to average pre-reform benefits, $(B_q^{pre} + B_{q-1}^{pre})/2$.

Results. Table 6 reports elasticities computed separately for each adjacent ISEE comparison, as well as an aggregate elasticity pooling all comparisons. Both income and benefit elasticities display substantial heterogeneity across the income distribution. Elasticities are relatively small at the bottom of the ISEE range and increase sharply for higher-income comparisons.

In particular, the elasticity with respect to household income ranges from approximately 0.5 for the comparison between the second and first quintiles to values above one for intermediate comparisons, reaching much larger values for the top ISEE comparison. A similar pattern emerges for elasticities with respect to benefit gains, which range from about 0.05 in the lower part of the distribution to values above 0.3 at the top.

Aggregate elasticity. In addition to elasticities computed for each adjacent ISEE comparison, we report an aggregate elasticity intended to summarize the average labor supply response to the AUU reform.

The aggregate elasticity is constructed by combining the average estimated labor supply effect with an aggregate measure of the reform-induced change in economic resources. Specifically, the numerator is given by the mean estimated percentage change in labor supply across all adjacent comparisons. The denominator is constructed by computing the percentage change in average benefits (or household income) at the aggregate level, defined as the difference between average post-reform and pre-reform values divided by the average pre-reform level.

Importantly, this aggregate elasticity is not obtained as an average of local elasticities, nor as a ratio of comparison-specific treatment effects to comparison-specific changes in benefits. Instead, it measures the elasticity of the average labor supply response with respect to a change in average transfer generosity implied by the reform.

As a result, the aggregate elasticity can differ substantially from elasticities computed at specific points of the ISEE distribution and may be smaller than the minimum of local elasticities. This difference arises mechanically from the nonlinearity of the elasticity formula and from the fact that the aggregate denominator reflects average changes in benefits across the entire ISEE range, while local elasticities are evaluated at different points of the distribution. The aggregate elasticity should therefore be interpreted as a conservative summary measure of the average labor supply response to the reform.

Table 6: Elasticities of Maternal Labor Supply

	Elasticity wrt household income	Elasticity wrt benefit gain
Q2 vs Q1	-0.421	-0.038
Q3 vs Q2	-1.451	-0.085
Q4 vs Q3	-1.343	-0.058
Q5 vs Q4	-9.227	-0.332
All	-0.420	-0.026

Notes: Elasticities are computed by combining estimated labor supply responses with simulation-based measures of reform-induced changes in household income and child-related benefits. Household income is inferred from ISEE under the assumption that ISEE is fully determined by labor income. Benefit gains are expressed relative to average pre-reform benefits. See Appendix E for details.

Comparison with the literature. The magnitude of the elasticities reported in Table 6 is broadly consistent with existing evidence on maternal labor supply responses to changes in child-related transfers and family income. In particular, Jensen and Blundell (2024), exploiting an unexpected cap on child benefit payments in Denmark, estimate female labor supply elasticities with respect to non-labor income ranging between 0.9 and 2.7 when accounting for both extensive and intensive margins. When focusing exclusively on participation responses, the implied extensive-margin elasticities lie between 0.65 and 1.94. These estimates are of comparable order of magnitude to those obtained in our setting for intermediate ISEE comparisons.

Earlier evidence summarized by Hotz and Scholz (2001) for the United States similarly points to income elasticities of maternal labor supply typically ranging between zero and 1, with larger absolute responses along the extensive margin and substantial heterogeneity by marital status. In particular, expansions of tax credits targeted to families with children are found to increase labor force participation among single mothers while reducing labor supply among married mothers, reflecting secondary-earner incentives and intra-household labor supply adjustments.

In our context, elasticities estimated for the upper end of the ISEE distribution, particularly for the comparison between the fourth and fifth quintiles, should be interpreted with caution. Household income is imputed from ISEE under simplifying assumptions, and measurement error in the constructed income measure may mechanically inflate elasticities when percentage changes in income are small. For this reason, we place more emphasis on elasticities estimated for comparisons between the second and third quintiles, where treatment effects are most precisely estimated and elasticities fall within the range documented in the existing literature.

Finally, following a long tradition in the analysis of disability insurance and social assistance programs (e.g. Bound and Burkhauser (1999)), we complement income elasticities with elasticities expressed with respect to benefit generosity. For both sets of reduced-form parameters, the elasticity is increasing in the level of household income.

5.4 Heterogeneity

We now examine how the fertility and labor supply responses to the AUU reform vary across maternal characteristics and local economic conditions. Rather than focusing on a single treatment-intensity comparison, we rely on the full set of heterogeneous estimates reported in Table Appendix D and use them to identify systematic patterns across adjacent ISEE quintiles.

Starting with fertility, we documented in section 5.1 that the positive effect of the reform is concentrated primarily in the comparison between the third and second ISEE quintiles, while estimates for other adjacent comparisons are generally smaller and imprecisely estimated. Across dimensions of heterogeneity, a consistent pattern emerges: positive fertility responses are driven by younger mothers, by mothers with open-ended contracts, and by those living in Northern provinces, in areas with higher real wages (implying lower costs of living) and greater availability of public childcare. In contrast, fertility effects are weak or absent among older mothers, those with temporary contracts, and in areas characterized by lower real wages or limited childcare provision.

Turning to labor supply at the extensive margin, the main estimates presented in Section 5.2 indicate that the negative employment response to the AUU reform is primarily concentrated in the comparison between the third and second ISEE quintiles, and, to a lesser extent, in the comparison between the fifth and fourth quintiles. Overall, while statistical significance varies across treatment-intensity comparisons, the sign of the estimates is broadly consistent with a reduction in employment following the reform.

Table 7 presents event-study estimates aggregated over time windows for labor supply across the most relevant dimensions of heterogeneity. Employment responses are stronger among younger mothers, particularly for the Q_3 versus Q_2 comparison. For higher-intensity comparisons, and in particular for Q_4 versus Q_3, the effect is weaker among younger mothers and marginally more pronounced among older mothers, although estimates remain imprecisely estimated.

Local labor market conditions play a central role in shaping these responses. The negative employment effect is consistently stronger in Northern provinces across all treatment-intensity comparisons, although the difference relative to other areas tends to decline as ISEE increases. Focusing on the Q_3 versus Q_2 comparison – where the main effect is most precisely estimated – the employment response is markedly stronger in provinces characterized by higher real wages and, therefore, lower costs of living. The negative effect on labor supply, similarly to the positive effect on fertility, is more pronounced in provinces with greater childcare availability. This pattern is consistent with the interpretation that the observed labor supply response is partly operating through the fertility channel.

Further insights emerge when distinguishing mothers by their degree of labor market attachment prior to the reform[5]. Among mothers with stronger attachment, employment responses are muted at lower treatment intensities and become positive for higher-intensity comparisons, reaching statistical significance for the Q_4 versus Q_3 comparison. By contrast, among mothers with weaker attachment, estimated effects are uniformly negative across treatment-intensity comparisons and statistically significant for the Q_3 versus Q_2 and Q_5 versus Q_4 comparisons, pointing to a greater propensity to exit employment in response to the reform.

Taken together, these findings suggest that the labor supply response to the AUU reform reflects a combination of income effects and heterogeneity in adjustment margins. Employment reductions are concentrated among mothers with weaker labor market attachment, while mothers with stronger attachment appear to adjust primarily through reductions in working in-

[5]We define mothers as attached to the labor market if they have been continuously employed since their entry in the labor market (which we proxy using the first time they were recorded in INPS data).

tensity. Moreover, favorable local labor market conditions – such as higher real wages, i.e. lower cost of living – amplify these responses by making reductions in labor supply more affordable, particularly at lower levels of treatment intensity.

Overall, the heterogeneity results point to a coherent interpretation of the main findings. Fertility responses to the AUU reform arise primarily among mothers who face fewer biological and institutional constraints, while labor supply reductions are driven by mothers at earlier stages of their careers and with weaker attachment to the labor market. Moreover, local economic conditions – such as cost of living and childcare availability – play a key role in shaping both the magnitude and the detectability of these responses across treatment intensities.

Table 7: AUU reform and labor supply – Heterogeneity

Panel A: Age

	Q2vsQ1		Q3vsQ2		Q4vsQ3		Q5vsQ4	
	young	old	young	old	young	old	young	old
Anticipation (avg.)	-0.05	-0.14	-0.14	0.29	0.01	-0.08	0.19	0.04
	0.26	0.23	0.25	0.22	0.25	0.21	0.24	0.19
Short run (avg.)	-0.57	-0.34	-1.15	0.18	0.02	-0.67	-0.80	-0.46
	0.35	0.31	0.35	0.30	0.34	0.28	0.34	0.26
Medium run (avg.)	-0.85	-0.00	-1.86	-0.56	-0.29	-0.78	-1.46	-0.73
	0.42	0.38	0.43	0.37	0.43	0.35	0.42	0.33
Observations	770,301	721,201	735,847	752,564	696,055	790,856	655,986	828,945

Panel B: Area

	Q2vsQ1		Q3vsQ2		Q4vsQ3		Q5vsQ4	
	North	South	North	South	North	South	North	South
Anticipation (avg.)	0.08	-0.32	-0.33	0.39	-0.22	0.14	0.13	0.02
	0.26	0.23	0.25	0.23	0.22	0.23	0.20	0.22
Short run (avg.)	-0.82	-0.33	-1.06	-0.08	-0.69	-0.07	-0.63	-0.58
	0.35	0.31	0.33	0.31	0.30	0.31	0.28	0.31
Medium run (avg.)	-0.85	-0.30	-1.82	-0.70	-0.99	-0.22	-1.08	-0.92
	0.43	0.37	0.41	0.38	0.38	0.38	0.35	0.38
Observations	678,294	828,908	731,029	775,354	782,615	722,638	826,788	676,952

Panel C: Real Wage

	Q2vsQ1		Q3vsQ2		Q4vsQ3		Q5vsQ4	
	Low RW	High RW	Low RW	High RW	Low RW	High RW	Low RW	High RW
Anticipation (avg.)	-0.16	-0.05	0.18	-0.18	0.16	-0.34	0.04	0.10
	0.22	0.26	0.22	0.26	0.22	0.24	0.21	0.21
Short run (avg.)	-0.01	-1.19	-0.43	-0.67	-0.22	-0.62	-0.50	-0.72
	0.30	0.37	0.30	0.35	0.29	0.33	0.29	0.30
Medium run (avg.)	-0.10	-1.11	-1.03	-1.48	-0.38	-0.93	-0.92	-1.09
	0.37	0.46	0.36	0.44	0.36	0.41	0.36	0.38
Observations	890,660	589,035	856,902	622,200	821,080	657,113	786,965	690,555

Panel D: Attached

	Q2vsQ1		Q3vsQ2		Q4vsQ3		Q5vsQ4	
	Yes	No	Yes	No	Yes	No	Yes	No
Anticipation (avg.)	0.09	0.02	0.44	0.07	0.09	0.02	0.67	-0.04
	0.33	0.20	0.28	0.20	0.24	0.20	0.20	0.20
Short run (avg.)	0.02	-0.11	0.91	-0.42	0.49	-0.27	0.62	-0.76
	0.42	0.27	0.37	0.27	0.31	0.27	0.28	0.27
Medium run (avg.)	0.13	0.09	0.86	-1.08	0.96	-0.58	0.57	-1.17
	0.51	0.33	0.46	0.33	0.40	0.34	0.35	0.34
Observations	325,155	1,200,297	382,995	1,142,535	444,354	1,081,212	506,028	1,019,461

6 Robustness

6.1 Alternative ISEE Range (Placebo)

As an additional validity check, we conduct a placebo analysis focusing on households located to the right of the ISEE interval used in the main analysis (see Figure 8. In this range of the ISEE distribution, the pre-reform and post-reform benefit schedules are approximately parallel, implying little to no variation in the reform-induced change in transfers across adjacent groups. We replicate our baseline empirical strategy by partitioning this high-ISEE region into quintiles and re-estimating the effects of the reform on maternal labor supply. Under the identifying assumptions of our design, we expect to find no systematic effects in this placebo sample.

Figure 8: Pre and post reform yearly benefits: ISEE interval used for the placebo analysis

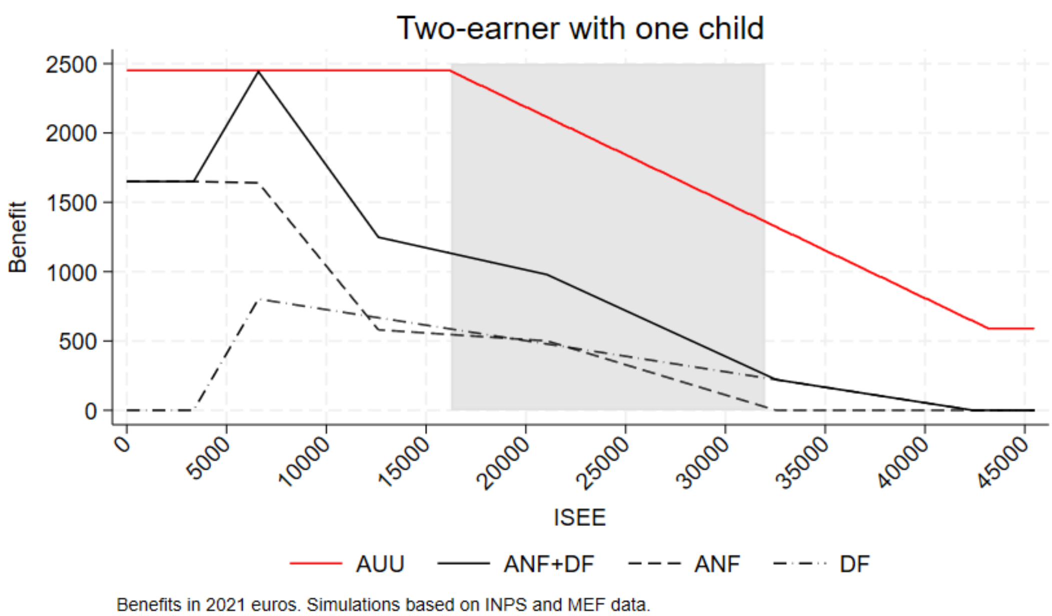


Table 8: AUU reform and labor supply: event-study estimates aggregated over time windows

	Q2vsQ1	Q3vsQ2	Q4vsQ3	Q5vsQ4
Pre(avg.)	-0.04	-0.02	-0.01	-0.07
	0.05	0.05	0.05	0.04
Anticipation(avg.)	-0.41	0.25	-0.07	-0.01
	0.13	0.13	0.12	0.11
Short run (avg.)	-0.45	0.20	-0.28	-0.07
	0.18	0.17	0.17	0.16
Medium run (avg.)	-0.62	0.06	-0.26	-0.23
	0.23	0.22	0.21	0.20
Observations	1,565,763	1,565,750	1,565,524	1,565,305

Notes: Entries report event-study estimates aggregated over time windows. Pre is $k \in [-, 4-1]$, Anticipation is $k \in [0, 1]$, Short run is $k \in [2, 6]$, Medium run is $k \in [7, 10]$, where k denotes event time (quarter) relative to the omitted baseline period (the last pre-reform period). Each column compares adjacent ISEE quintiles. Standard errors are clustered at the individual level. Full event-study profiles are reported in Appendix D. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Consistent with this expectation, we do not detect statistically significant labor supply effects for most adjacent quintile comparisons. The only exception is the first comparison immediately to the right of the main analysis interval, for which we estimate a small negative and statistically significant effect.

We interpret this isolated finding as reflecting measurement error in the construction of the reform-induced change in benefits rather than a genuine behavioral response. As discussed in Section 4, the mapping between pre-reform and post-reform benefits relies on simulations that translate ISEE values into pre-reform transfers under the assumption that household economic resources are entirely driven by labor income. This approximation is more likely to generate imprecision at the boundary of the analysis interval, where small differences in ISEE can correspond to non-negligible differences in simulated pre-reform benefits.

Importantly, the absence of significant effects for higher ISEE comparisons—where the simulated change in benefits is flat and the mapping is less sensitive—supports the interpretation that the main labor supply results are driven by genuine variation in reform-induced transfers rather than by spurious correlations.

6.2 Quartiles of ISEE distribution

As discussed in Section 4.3, our identification strategy is based on comparisons between mothers belonging to adjacent treatment-intensity categories. In the main analysis, we partition the

ISEE distribution into quintiles and use these groups to define five discrete treatment-intensity bins. This choice reflects a trade-off between bin width and variation in exposure to benefit generosity across adjacent categories. On the one hand, bins must be sufficiently narrow to ensure that mothers within each category are comparable in terms of unobserved characteristics; on the other hand, they must be sufficiently wide to generate meaningful differences in benefit generosity between adjacent groups.

Table 9: AUU reform and labor supply, quartile comparison: event-study estimates aggregated over time windows

	Q2vsQ1	Q3vsQ2	Q4vsQ3
Pre(avg.)	0.03	-0.02	0.05
	0.05	0.05	0.05
Anticipation(avg.)	-0.07	0.05	0.12
	0.15	0.15	0.14
Short run (avg.)	-0.55	-0.58	-0.52
	0.20	0.20	0.18
Medium run (avg.)	-0.94	-1.06	-1.00
	0.25	0.24	0.23
Observations	1,906,835	1,906,862	1,906,901

Notes: Entries report event-study estimates aggregated over time windows. Pre is $k \in [-, 4-1]$, Anticipation is $k \in [0, 1]$, Short run is $k \in [2, 6]$, Medium run is $k \in [7, 10]$, where k denotes event time (quarter) relative to the omitted baseline period (the last pre-reform period). Each column compares adjacent ISEE quartiles. Standard errors are clustered at the individual level. Full event-study profiles are reported in Appendix D.

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

To assess the sensitivity of our results to this discretization choice, we replicate the analysis for the employment outcome using ISEE quartiles instead of quintiles to define treatment-intensity categories. Table 9 shows that the main finding of a negative effect of benefit generosity on employment is robust to this alternative specification, which increases the width of the treatment-intensity categories. Consistent with the baseline results, the negative effect is larger and statistically significant in the medium run and concentrated in the middle of the ISEE distribution. In particular, the estimated effect for the comparison between the third and second quartiles (-1.06 percentage points) is very similar in magnitude to that obtained in the main analysis for the comparison between the third and second quintiles (-1.15 percentage points).

6.3 Alternative ISEE measure

As described in Section 4.3, mothers are assigned to treatment-intensity groups based on the ISEE submitted in April 2023, which reflects household economic conditions in 2021, when the AUU reform was announced. In this section, we assess the robustness of our findings to an alternative assignment criterion based on the ISEE submitted in April 2022, capturing households' economic conditions in 2020, one year prior to the announcement of the reform.

Table 10: AUU reform and labor supply: event-study estimates aggregated over time windows

	Q2vsQ1	Q3vsQ2	Q4vsQ3	Q5vsQ4
Pre(avg.)	-0.14	-0.05	-0.09	-0.10
	0.06	0.06	0.06	0.05
Anticipation(avg.)	-0.04	0.14	-0.00	0.13
	0.17	0.16	0.15	0.15
Short run (avg.)	-0.26	-0.17	-0.55	0.09
	0.23	0.22	0.21	0.20
Medium run (avg.)	-0.28	-0.64	-0.72	-0.19
	0.28	0.27	0.26	0.25
Observations	1,504,456	1,504,598	1,504,592	1,504,625

Notes: Entries report event-study estimates aggregated over time windows. Pre is $k \in [-, 4-1]$, Anticipation is $k \in [0, 1]$, Short run is $k \in [2, 6]$, Medium run is $k \in [7, 10]$, where k denotes event time (quarter) relative to the omitted baseline period (the last pre-reform period). Each column compares adjacent ISEE quintiles. Standard errors are clustered at the individual level. Full event-study profiles are reported in Appendix D.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 10 shows that the negative effect of exposure to higher benefit generosity on maternal labor supply in the medium run remains when the 2022 ISEE is used as the reference measure instead of the 2023 ISEE. The effect is particularly pronounced in the comparisons between the third and second quintiles and between the fourth and third quintiles. While the estimated magnitudes are smaller than those obtained in the main analysis, the qualitative pattern of the results is unchanged.

7 Additional Results

7.1 Older Mothers

To fully isolate labor supply responses to the AUU reform from fertility-related adjustments, we conduct a complementary analysis on a sample of older mothers who are no longer in their main fertility period.

As in the main analysis, we focus on private-sector employees who were employed at least once in the period 2012–2019. We select mothers born before 1975, who were aged 46 or older at the time of the reform announcement and who already had one or more children prior to the announcement. We do not impose restrictions on the age of the oldest child. To ensure that fertility decisions do not confound labor supply responses, we exclude the small number of mothers who, despite their age, had an additional child during the sample period.

This sample allows us to study labor supply effects of the AUU reform in a setting where fertility responses are unlikely by construction. Summary statistics for this group are reported in Table 13 in Appendix C.

Table 11 reports aggregated pre- and post-reform estimates of the effects of the AUU on the probability of working for this subsample of older mothers. Across all treatment-intensity comparisons, we do not detect statistically significant effects at the extensive margin. We also check for effects at the intensive margin – probability of working part-time for the continuously employed – and do not find any statistically significant changes[6]

[6]Full estimation results for extensive and intensive margins are reported in Appendix D.

Table 11: AUU reform and labor supply: event-study estimates aggregated over time windows

	Q2vsQ1	Q3vsQ2	Q4vsQ3	Q5vsQ4
Pre(avg.)	-0.12	0.02	-0.00	-0.07
	0.15	0.15	0.15	0.14
Anticipation(avg.)	0.57	0.25	-0.58	0.60
	0.46	0.45	0.44	0.42
Short run (avg.)	-0.02	0.38	-1.11	1.00
	0.63	0.60	0.57	0.55
Medium run (avg.)	-0.18	-0.26	-0.37	0.43
	0.76	0.73	0.71	0.69
Observations	151,949	151,936	151,913	151,917

Notes: Entries report event-study estimates aggregated over time windows. Pre is $k \in [-, 4-1]$, Anticipation is $k \in [0, 1]$, Short run is $k \in [2, 6]$, Medium run is $k \in [7, 10]$, where k denotes event time (quarter) relative to the omitted baseline period (the last pre-reform period). Each column compares adjacent ISEE quintiles. Standard errors are clustered at the individual level.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Overall, these results indicate that older mothers – who on average have older children, face lower childcare needs, and are less likely to adjust fertility – do not respond to the introduction of the AUU reform in terms of labor supply. This finding supports the interpretation that the labor supply responses documented in the main analysis are closely linked to stages of the life cycle in which fertility and child-related considerations are more salient.

7.2 First-Time Entrants

In this subsection, we study the impact of the AUU reform on labor market entry decisions using an alternative and complementary sample. We focus on mothers who entered the labor market for the first time between 2020 and 2024, a group that is deliberately excluded from our main analysis in order to avoid conflating labor supply responses with employment entry decisions.

We restrict attention to women who were already mothers of one or more children prior to the announcement of the reform. Given the limited size of this sample, we allow for a broader age range—mothers aged 16 to 45 at the time of the announcement—do not impose restrictions on the age of the oldest child, and include both private- and public-sector employees. Summary statistics for this sample are reported in Table 14 in Appendix C.

We define an indicator variable, *Entry*, which takes value one in the first period in which a mother appears in the labor market records and remains equal to one thereafter. Table 12 reports aggregated pre- and post-reform estimates of the effect of the AUU reform on the probability of labor market entry across adjacent ISEE quintiles.

Across all treatment-intensity comparisons, we do not detect statistically significant effects of the reform on labor market entry decisions. This result suggests that the expectation of receiving monthly child-related transfers over several future years does not materially affect the decision to enter the labor market for the first time. Taken together with the main findings, this evidence indicates that the labor supply responses to the AUU reform primarily operate through adjustments among mothers with prior labor market attachment, rather than through changes in labor market participation at entry.

Table 12: AUU reform and labor supply: event-study estimates aggregated over time windows

	Q2vsQ1	Q3vsQ2	Q4vsQ3	Q5vsQ4
Pre(avg.)	0.10	0.30	0.29	0.21
	0.08	0.09	0.09	0.09
Anticipation(avg.)	0.31	0.34	-0.19	0.53
	0.20	0.21	0.21	0.21
Short run (avg.)	0.41	0.42	0.16	0.54
	0.35	0.35	0.36	0.36
Medium run (avg.)	0.83	0.13	0.06	0.28
	0.43	0.44	0.43	0.43
Observations	326,290	324,671	323,413	323,225

Notes: Entries report event-study estimates aggregated over time windows. Pre is $k \in [-, 4-1]$, Anticipation is $k \in [0, 1]$, Short run is $k \in [2, 6]$, Medium run is $k \in [7, 10]$, where k denotes event time (quarter) relative to the omitted baseline period (the last pre-reform period). Each column compares adjacent ISEE quintiles. Standard errors are clustered at the individual level.

8 Conclusions

This paper examines the effects of increases in child-related cash transfers on fertility and maternal labor supply using population-wide administrative data and a design that exploits heterogeneous changes in benefit generosity. By comparing outcomes across adjacent treatment-intensity groups, we identify local causal effects in a setting without untreated units.

We find that higher transfers increase the probability of having an additional child only over a limited range of benefit increases, with effects concentrated in the middle of the income distribution. At the same time, increased benefit generosity reduces maternal labor supply in the medium run for specific groups. These labor supply responses operate through both employment exits and reductions in working hours and persist even when fertility-related adjustments are shut down. Translating these effects into elasticities reveals substantial heterogeneity across the income distribution, underscoring the importance of local responses and adjustment margins.

The findings have implications for the design and evaluation of family policies. Child-related cash transfers can stimulate fertility among financially constrained households, but they may also reduce maternal labor supply along margins that are sensitive to non-labor income. Policies aimed at addressing low fertility and low female employment should, therefore, account for heterogeneous responses and potential trade-offs across margins of adjustment, rather than relying on average effects alone.

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A Fertility Trends in Italy

Figures 9 and 10 below report the yearly number of birth from 2008 to 2023 in Italy and by Macro areas. The emerging trends are clearly negative (source: ISTAT).

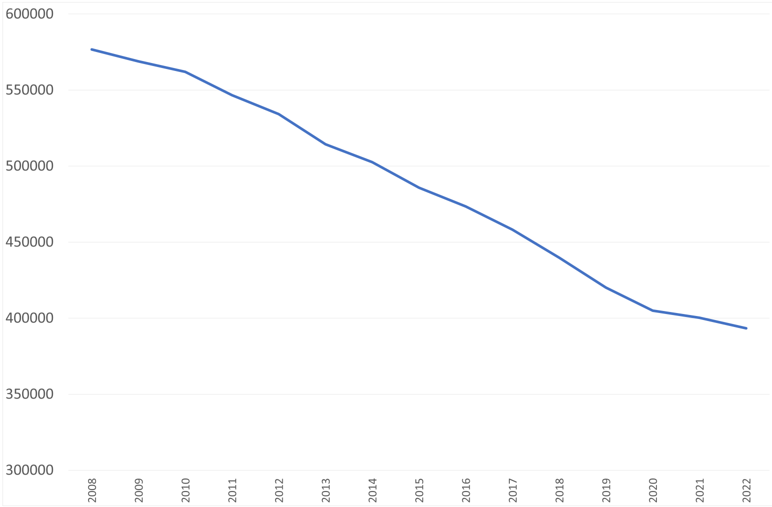


Figure 9: Annual number of births in Italy (2008-2022). Source: Istat.

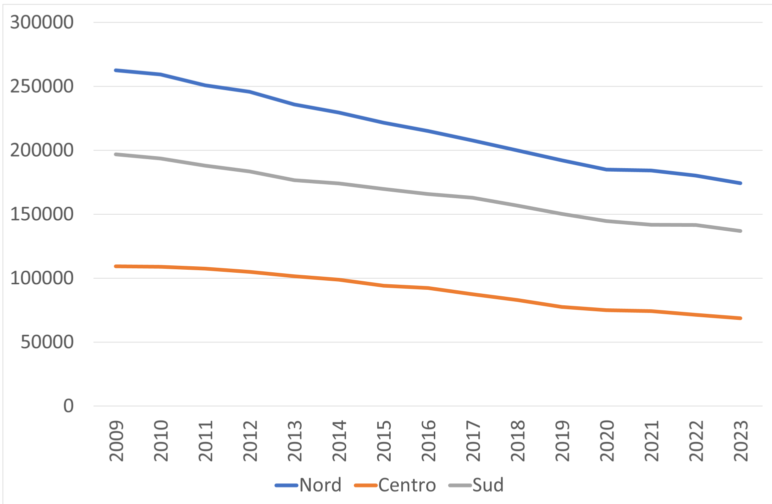


Figure 10: Annual number of births in Italy by macro areas (2008-2023). Source: Istat.

The introduction of AUU partly overlaps with the COVID-19 pandemic which has shown to affect fertility decisions, see Figure 11 (Rapporto ISTAT,2022). The effect of COVID-19 seems to be less marked for women aged 35-44. Younger women tend to postpone having a child and the effect is particularly negative for women aged above 45 where the role of medically assisted procreation is important and has been impeded by the COVID-19 restrictions. In addition, the drop in conceptions seems to be larger for foreign citizens (due to reduction in family re-unifications and/or less stable employments).

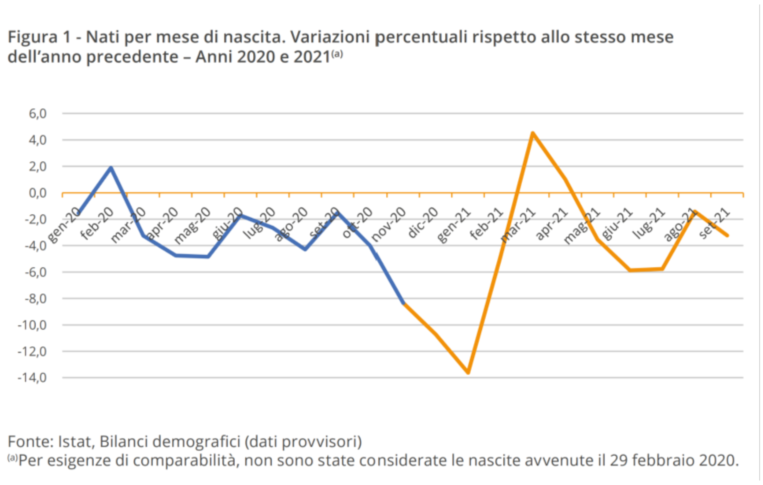


Figure 11: Source: "L'impatto della pandemia di COVID-19 su natalitá e condizioni delle nuove generazioni", from the second report by the Gruppo di esperti "Demografia e Covid-19"

B Construction of Pre-Reform Child-Related Benefits

This appendix describes the procedure used to construct household-level child-related benefits under the pre-reform system and to compute the reform-induced change in benefit generosity used in the empirical analysis.

Pre-Reform Child-Related Measures Under the pre-reform regime, child-related support in Italy was provided through two main instruments: the *Assegni per il Nucleo Familiare* (ANF) and tax deductions for dependent children (*Detrazioni Fiscali per Figli a Carico*, DF).

The ANF consisted of a monthly cash transfer targeted to dependent employees and some categories of pensioners. Eligibility and benefit amounts depended on household labor income, family size, and employment status, with non-linear schedules featuring discontinuities across income brackets and household compositions. Self-employed and unemployed individuals were generally excluded from ANF eligibility.

The DF were tax deductions or credits granted to households with dependent children, based on taxable income and varying with the number and age of children. While DF applied to a broader population than ANF, their value depended on tax liability and therefore provided limited support to households with low taxable income.

Data Limitations and Mapping Strategy In the administrative data used for the empirical analysis, we observe the household's ISEE level used for AUU determination but do not observe its underlying components, such as the breakdown of income sources and assets. Moreover, we do not observe complete information on taxable income that would be required to mechanically replicate pre-reform tax deductions.

To construct a reform-induced change in benefit generosity that is comparable across households, we adopt a simulation-based approach that maps pre-reform benefits into the ISEE-based post-reform framework.

For each ISEE level and household composition considered in the analysis (number of earners and number of children), we simulate pre-reform ANF and DF amounts using official benefit schedules and publicly available policy simulators, as employed at the time of the reform announcement.

The mapping from ISEE to the income concept required by pre-reform rules is obtained under the assumption that household economic resources are entirely driven by labor income. Under this assumption, a given ISEE level corresponds to an *implied* level of labor income, which is then used to compute ANF entitlements and tax deductions for dependent children.

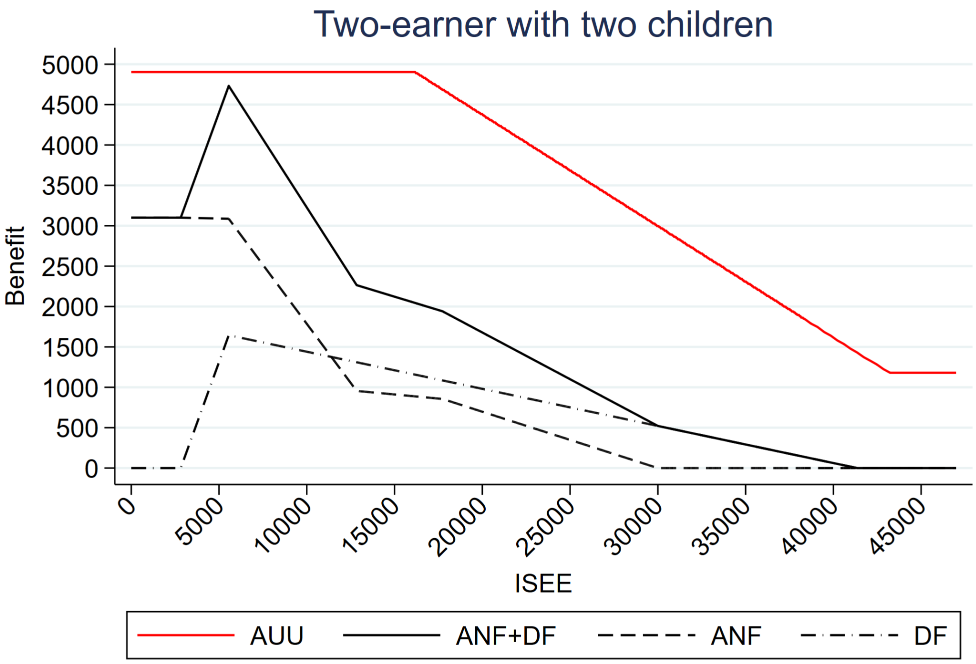
This approach allows us to construct a consistent measure of pre-reform child-related support as a function of ISEE and household composition. While this assumption abstracts from other sources of income and wealth, it is well suited to the context of our analysis, which focuses on private-sector employed households in the lower and middle part of the ISEE distribution, where labor income represents the primary source of economic resources.

Interpretation and Limitations The simulation procedure described above constitutes an approximation and is not intended to replicate the exact benefit amounts received by individual households prior to the reform. Instead, its purpose is to capture systematic differences in pre-reform child-related support across the ISEE distribution and to construct a coherent measure of the reform-induced change in benefit generosity.

Importantly, our empirical strategy relies on relative differences in reform-induced changes across adjacent ISEE groups rather than on the precise level of simulated pre-reform benefits. As a result, any measurement error arising from the mapping assumption is unlikely to materially affect our estimates of the causal effects of interest.

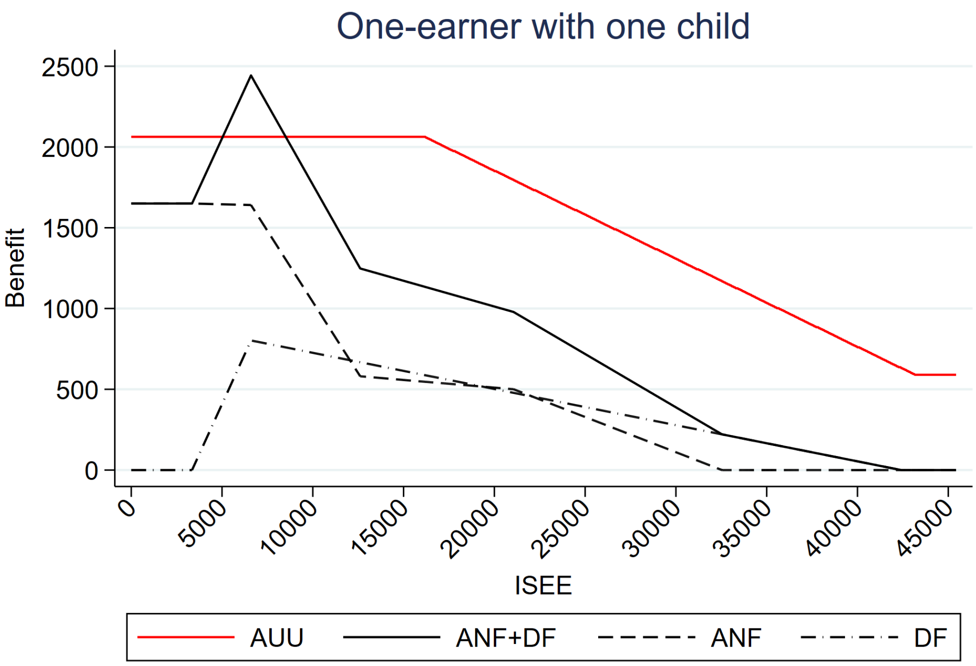
The resulting comparisons for different household compositions and labor market status are reported in Figures 12, 13, 14,

Figure 12: Pre and post reform yearly benefits as a function of ISEE – employees



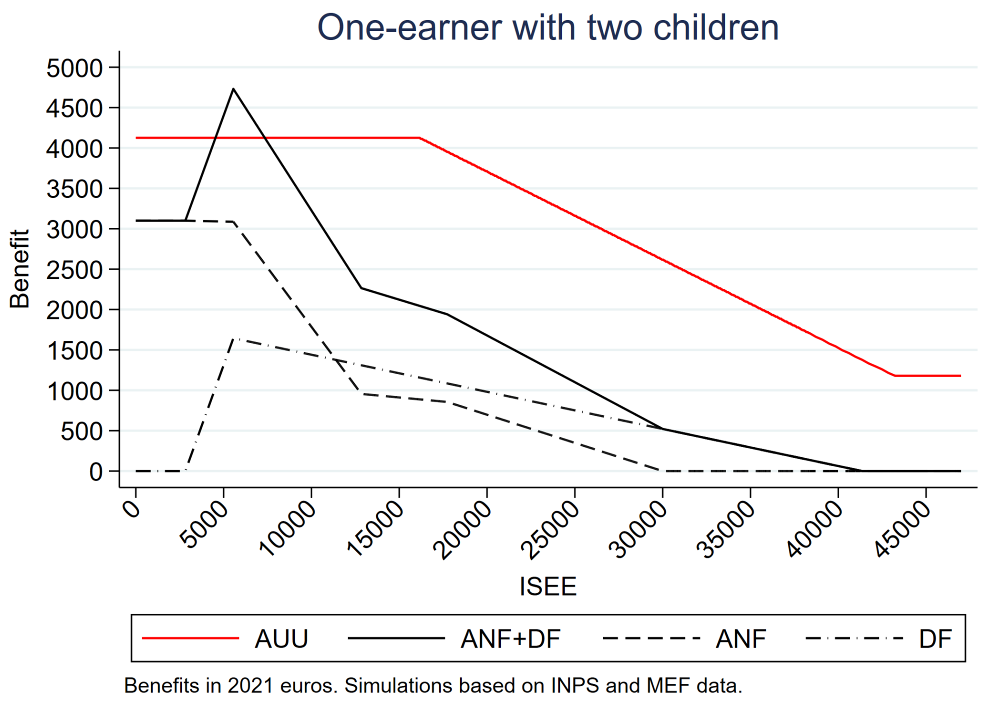
Benefits in 2021 euros. Simulations based on INPS and MEF data.

Figure 13: Pre and post reform yearly benefits as a function of ISEE – employees



Benefits in 2021 euros. Simulations based on INPS and MEF data.

Figure 14: Pre and post reform yearly benefits as a function of ISEE – employees



C Descriptive Statistics

Table 13: Descriptive Statistics of Sample of Older Mothers

	Q1	Q2	Q3	Q4	Q5
age of mother	48	48	48	48	48
working	0.56	0.58	0.62	0.65	0.65
days worked (conditional)	23	23	23	23	23
Part Time (conditional)	0.66	0.60	0.58	0.55	0.53
Observations	232,694	232,629	232,697	232,670	232,665
Individuals	4,847	4,846	4,847	4,847	4,847

Table 14: Descriptive Statistics of Sample of First Time Entrants

	Q1	Q2	Q3	Q4	Q5
age of mother	31	32	32	33	34
entrant	0.22	0.24	0.25	0.26	0.27
entry	0.14	0.15	0.16	0.17	0.18
Observations	514,243	514,328	514,351	514,392	514,416
Individuals	10,713	10,715	10,715	10,716	10,717

D Results: Regression Tables

D.0.1 Fertility

Table 15: Probability of 2nd Birth by ISEE Quintiles, Regressions

	Q2	Q3	Q4	Q5
$t_{1,2}$	0.345**	-0.138	0.024	-0.002
	(0.113)	(0.114)	(0.111)	(0.110)
$t_{2,3}$	0.480***	0.084	0.058	-0.287**
	(0.111)	(0.114)	(0.114)	(0.111)
$t_{3,4}$	0.286**	0.128	0.033	0.030
	(0.109)	(0.111)	(0.112)	(0.111)
$t_{4,5}$	-0.064	0.289*	-0.135	0.114
	(0.115)	(0.115)	(0.115)	(0.114)
$t_{5,6}$	0.078	0.017	-0.124	0.142
	(0.116)	(0.115)	(0.113)	(0.112)
$t_{6,7}$	-0.129	0.065	-0.001	-0.085
	(0.105)	(0.103)	(0.103)	(0.101)
$t_{7,8}$	-0.060	0.085	-0.031	-0.006
	(0.101)	(0.100)	(0.100)	(0.098)
$t_{8,9}$	-0.069	-0.098	0.303**	-0.302**
	(0.107)	(0.104)	(0.104)	(0.103)
$t_{9,10}$	-0.012	-0.105	0.116	-0.105
	(0.102)	(0.100)	(0.099)	(0.099)
$t_{9,11}$	0.019	0.046	0.032	-0.067
	(0.134)	(0.132)	(0.132)	(0.130)
$t_{9,12}$	0.013	0.146	0.120	0.107
	(0.151)	(0.150)	(0.150)	(0.150)
$t_{9,13}$	0.015	0.240	0.177	0.151
	(0.167)	(0.166)	(0.166)	(0.166)
$t_{9,14}$	0.026	0.372*	0.105	0.266
	(0.179)	(0.179)	(0.178)	(0.178)
$t_{9,15}$	0.003	0.504**	0.047	0.284
	(0.189)	(0.188)	(0.188)	(0.187)
$t_{9,16}$	0.059	0.545**	0.046	0.369
	(0.196)	(0.195)	(0.195)	(0.195)
N	1525452	1525530	1525566	1525489

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 16: Probability of 2nd Birth by ISEE Quintiles, by age of the mother

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.339	0.319*	-0.234	-0.086	-0.097	0.109	0.104	-0.086
	(0.179)	(0.147)	(0.183)	(0.147)	(0.183)	(0.143)	(0.187)	(0.140)
$t_{2,3}$	0.512**	0.433**	-0.099	0.219	0.139	-0.037	-0.556**	-0.121
	(0.180)	(0.138)	(0.188)	(0.144)	(0.193)	(0.143)	(0.193)	(0.138)
$t_{3,4}$	0.371*	0.198	0.074	0.164	-0.275	0.254	0.164	-0.098
	(0.178)	(0.134)	(0.187)	(0.137)	(0.190)	(0.138)	(0.194)	(0.138)
$t_{4,5}$	-0.075	-0.084	0.150	0.404**	-0.034	-0.254	-0.217	0.359**
	(0.190)	(0.138)	(0.195)	(0.140)	(0.201)	(0.137)	(0.205)	(0.136)
$t_{5,6}$	0.104	0.015	-0.127	0.133	-0.146	-0.143	0.225	0.064
	(0.189)	(0.142)	(0.193)	(0.141)	(0.195)	(0.137)	(0.202)	(0.133)
$t_{6,7}$	-0.177	-0.116	0.063	0.025	-0.122	0.075	0.013	-0.177
	(0.173)	(0.126)	(0.178)	(0.122)	(0.182)	(0.120)	(0.187)	(0.117)
$t_{7,8}$	-0.099	-0.048	0.197	-0.057	-0.292	0.161	0.111	-0.107
	(0.170)	(0.117)	(0.177)	(0.113)	(0.181)	(0.112)	(0.185)	(0.111)
$t_{8,9}$	-0.095	-0.092	-0.045	-0.170	0.380*	0.232*	-0.584**	-0.125
	(0.178)	(0.125)	(0.181)	(0.119)	(0.190)	(0.116)	(0.194)	(0.116)
$t_{9,10}$	-0.038	-0.006	-0.190	-0.067	0.075	0.123	-0.056	-0.160
	(0.172)	(0.118)	(0.175)	(0.115)	(0.179)	(0.113)	(0.186)	(0.111)
$t_{9,11}$	-0.078	0.088	0.066	-0.029	-0.086	0.096	-0.076	-0.095
	(0.225)	(0.152)	(0.233)	(0.150)	(0.240)	(0.147)	(0.247)	(0.145)
$t_{9,12}$	-0.033	0.021	0.131	0.087	0.133	0.067	0.211	0.001
	(0.257)	(0.169)	(0.266)	(0.167)	(0.275)	(0.164)	(0.287)	(0.163)
$t_{9,13}$	-0.009	-0.009	0.192	0.206	0.148	0.155	0.364	-0.044
	(0.285)	(0.185)	(0.295)	(0.183)	(0.305)	(0.181)	(0.319)	(0.180)
$t_{9,14}$	-0.056	0.054	0.508	0.165	-0.044	0.185	0.633	-0.043
	(0.306)	(0.199)	(0.318)	(0.197)	(0.329)	(0.194)	(0.343)	(0.192)
$t_{9,15}$	-0.071	0.012	0.800*	0.150	-0.161	0.166	0.619	-0.001
	(0.322)	(0.210)	(0.335)	(0.207)	(0.347)	(0.203)	(0.361)	(0.202)
$t_{9,16}$	0.070	-0.024	0.882*	0.151	-0.256	0.239	0.854*	-0.032
	(0.335)	(0.217)	(0.349)	(0.214)	(0.361)	(0.211)	(0.376)	(0.209)
N	770301	721201	735847	752564	696055	790856	655986	828945

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 17: Probability of 2nd Birth by ISEE Quintiles, by area

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.266	0.464**	0.028	-0.288	-0.022	0.032	-0.035	0.029
	(0.169)	(0.155)	(0.164)	(0.159)	(0.157)	(0.160)	(0.150)	(0.164)
$t_{2,3}$	0.620***	0.428**	0.151	-0.013	0.104	0.056	-0.320*	-0.281
	(0.162)	(0.154)	(0.164)	(0.161)	(0.159)	(0.165)	(0.151)	(0.166)
$t_{3,4}$	0.146	0.410**	0.097	0.134	-0.109	0.199	0.099	-0.015
	(0.167)	(0.146)	(0.162)	(0.155)	(0.155)	(0.163)	(0.149)	(0.169)
$t_{4,5}$	0.080	-0.113	0.104	0.430**	0.052	-0.221	0.376*	-0.237
	(0.167)	(0.160)	(0.161)	(0.166)	(0.156)	(0.172)	(0.155)	(0.170)
$t_{5,6}$	0.233	-0.020	-0.087	0.134	0.040	-0.289	0.218	0.058
	(0.169)	(0.160)	(0.164)	(0.163)	(0.156)	(0.165)	(0.153)	(0.166)
$t_{6,7}$	-0.097	-0.204	0.179	-0.033	0.132	-0.151	-0.155	-0.011
	(0.155)	(0.145)	(0.149)	(0.144)	(0.147)	(0.145)	(0.142)	(0.146)
$t_{7,8}$	0.089	-0.197	0.221	-0.014	-0.141	0.053	0.136	-0.156
	(0.149)	(0.139)	(0.146)	(0.139)	(0.141)	(0.143)	(0.136)	(0.145)
$t_{8,9}$	-0.056	-0.076	-0.033	-0.132	0.605***	0.002	-0.442**	-0.156
	(0.153)	(0.149)	(0.145)	(0.149)	(0.145)	(0.151)	(0.143)	(0.152)
$t_{9,10}$	0.183	-0.148	0.032	-0.198	0.180	0.011	-0.127	-0.093
	(0.146)	(0.144)	(0.143)	(0.142)	(0.139)	(0.143)	(0.136)	(0.145)
$t_{9,11}$	0.345	-0.201	0.214	-0.057	0.230	-0.201	-0.096	-0.065
	(0.191)	(0.188)	(0.188)	(0.189)	(0.184)	(0.191)	(0.179)	(0.193)
$t_{9,12}$	0.301	-0.131	0.522*	-0.088	0.225	0.028	0.204	-0.027
	(0.212)	(0.215)	(0.210)	(0.216)	(0.206)	(0.221)	(0.202)	(0.226)
$t_{9,13}$	0.149	0.008	0.836***	-0.122	0.140	0.255	0.373	-0.079
	(0.233)	(0.238)	(0.230)	(0.240)	(0.226)	(0.247)	(0.222)	(0.254)
$t_{9,14}$	0.276	-0.050	1.034***	0.007	0.020	0.252	0.529*	0.037
	(0.250)	(0.256)	(0.247)	(0.259)	(0.242)	(0.267)	(0.237)	(0.274)
$t_{9,15}$	0.219	-0.038	1.269***	0.069	-0.048	0.185	0.537*	0.121
	(0.264)	(0.269)	(0.261)	(0.273)	(0.256)	(0.280)	(0.249)	(0.288)
$t_{9,16}$	0.305	0.044	1.308***	0.155	0.002	0.155	0.598*	0.240
	(0.275)	(0.279)	(0.271)	(0.284)	(0.265)	(0.292)	(0.258)	(0.300)
N	678294	828908	731029	775354	782615	722638	826788	676952

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 18: Probability of 2nd Birth by ISEE Quintiles, by average real wage

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.275	0.549**	-0.194	-0.113	0.010	0.010	0.023	-0.037
	(0.148)	(0.183)	(0.149)	(0.182)	(0.149)	(0.174)	(0.151)	(0.168)
$t_{2,3}$	0.592***	0.452**	-0.120	0.303	0.081	0.043	-0.421**	-0.216
	(0.149)	(0.173)	(0.154)	(0.177)	(0.155)	(0.175)	(0.152)	(0.166)
$t_{3,4}$	0.390**	0.131	0.187	0.041	0.033	0.041	0.016	0.122
	(0.141)	(0.179)	(0.148)	(0.175)	(0.152)	(0.170)	(0.155)	(0.166)
$t_{4,5}$	-0.002	-0.114	0.349*	0.193	-0.214	0.035	-0.075	0.305
	(0.153)	(0.182)	(0.156)	(0.177)	(0.160)	(0.173)	(0.158)	(0.171)
$t_{5,6}$	-0.025	0.276	0.014	0.001	-0.070	-0.185	0.045	0.303
	(0.153)	(0.183)	(0.153)	(0.181)	(0.154)	(0.172)	(0.155)	(0.167)
$t_{6,7}$	-0.256	-0.028	-0.050	0.216	-0.045	0.048	-0.024	-0.148
	(0.139)	(0.167)	(0.135)	(0.165)	(0.136)	(0.162)	(0.137)	(0.156)
$t_{7,8}$	-0.289*	0.248	0.147	0.053	0.027	-0.146	-0.088	0.128
	(0.133)	(0.162)	(0.131)	(0.162)	(0.135)	(0.154)	(0.135)	(0.148)
$t_{8,9}$	-0.048	-0.104	-0.245	0.088	0.308*	0.352*	-0.307*	-0.326*
	(0.143)	(0.166)	(0.139)	(0.160)	(0.142)	(0.160)	(0.143)	(0.155)
$t_{9,10}$	-0.013	-0.021	-0.235	0.113	0.069	0.127	0.034	-0.263
	(0.137)	(0.160)	(0.133)	(0.157)	(0.134)	(0.154)	(0.136)	(0.147)
$t_{9,11}$	-0.011	0.067	-0.107	0.344	-0.057	0.094	0.061	-0.253
	(0.178)	(0.208)	(0.178)	(0.206)	(0.179)	(0.203)	(0.181)	(0.194)
$t_{9,12}$	0.076	-0.070	-0.075	0.610**	0.058	0.137	0.224	-0.068
	(0.203)	(0.233)	(0.203)	(0.231)	(0.205)	(0.228)	(0.210)	(0.221)
$t_{9,13}$	0.113	-0.110	-0.044	0.827**	0.216	0.098	0.272	0.005
	(0.225)	(0.256)	(0.225)	(0.253)	(0.229)	(0.250)	(0.235)	(0.242)
$t_{9,14}$	0.138	-0.155	0.032	1.118***	0.139	0.023	0.430	0.060
	(0.243)	(0.274)	(0.243)	(0.272)	(0.247)	(0.268)	(0.253)	(0.260)
$t_{9,15}$	0.153	-0.267	0.123	1.314***	0.048	-0.003	0.420	0.138
	(0.255)	(0.289)	(0.256)	(0.286)	(0.260)	(0.282)	(0.265)	(0.273)
$t_{9,16}$	0.288	-0.291	0.133	1.406***	-0.002	0.076	0.508	0.215
	(0.265)	(0.301)	(0.266)	(0.298)	(0.269)	(0.294)	(0.275)	(0.285)
N	890660	589035	856902	622200	821080	657113	786965	690555

$*p < 0.05,^{**} p < 0.01,^{***} p < 0.001$

Table 19: Probability of 2nd Birth by ISEE Quintiles, by childcare availability

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.374*	0.308	-0.098	-0.183	-0.111	0.212	-0.020	0.019
	(0.148)	(0.176)	(0.151)	(0.172)	(0.150)	(0.166)	(0.149)	(0.164)
$t_{2,3}$	0.615***	0.289	0.068	0.130	0.130	-0.020	-0.397**	-0.138
	(0.146)	(0.171)	(0.153)	(0.171)	(0.156)	(0.167)	(0.153)	(0.160)
$t_{3,4}$	0.407**	0.104	0.192	0.054	0.029	0.056	-0.053	0.147
	(0.143)	(0.170)	(0.149)	(0.167)	(0.152)	(0.163)	(0.153)	(0.161)
$t_{4,5}$	-0.035	-0.081	0.235	0.394*	-0.281	0.067	0.112	0.117
	(0.153)	(0.173)	(0.155)	(0.171)	(0.155)	(0.172)	(0.154)	(0.169)
$t_{5,6}$	-0.163	0.469**	0.182	-0.215	-0.231	0.030	0.238	0.027
	(0.151)	(0.178)	(0.152)	(0.176)	(0.152)	(0.168)	(0.153)	(0.164)
$t_{6,7}$	-0.258	0.092	-0.104	0.319*	-0.089	0.112	0.050	-0.261
	(0.140)	(0.157)	(0.135)	(0.159)	(0.134)	(0.160)	(0.134)	(0.154)
$t_{7,8}$	-0.188	0.147	0.007	0.193	0.067	-0.165	-0.121	0.140
	(0.133)	(0.156)	(0.131)	(0.157)	(0.132)	(0.152)	(0.132)	(0.147)
$t_{8,9}$	-0.139	0.056	-0.141	-0.036	0.359*	0.228	-0.446**	-0.117
	(0.140)	(0.164)	(0.137)	(0.159)	(0.140)	(0.156)	(0.139)	(0.154)
$t_{9,10}$	-0.193	0.303*	-0.155	-0.019	0.052	0.203	-0.065	-0.158
	(0.138)	(0.150)	(0.133)	(0.151)	(0.133)	(0.149)	(0.134)	(0.146)
$t_{9,11}$	-0.267	0.519**	-0.043	0.200	0.029	0.046	-0.172	0.066
	(0.179)	(0.197)	(0.176)	(0.200)	(0.177)	(0.196)	(0.177)	(0.193)
$t_{9,12}$	-0.155	0.350	-0.036	0.461*	0.104	0.160	0.035	0.207
	(0.202)	(0.222)	(0.201)	(0.224)	(0.203)	(0.222)	(0.204)	(0.219)
$t_{9,13}$	-0.094	0.277	-0.037	0.714**	0.211	0.159	0.038	0.309
	(0.224)	(0.245)	(0.222)	(0.247)	(0.225)	(0.245)	(0.227)	(0.242)
$t_{9,14}$	-0.119	0.360	0.054	0.920***	0.195	0.020	0.149	0.437
	(0.240)	(0.264)	(0.239)	(0.266)	(0.242)	(0.263)	(0.245)	(0.259)
$t_{9,15}$	-0.139	0.353	0.088	1.191***	0.181	-0.079	0.156	0.460
	(0.253)	(0.278)	(0.252)	(0.281)	(0.255)	(0.277)	(0.257)	(0.272)
$t_{9,16}$	-0.000	0.260	0.052	1.382***	0.167	-0.070	0.243	0.557*
	(0.263)	(0.290)	(0.262)	(0.292)	(0.265)	(0.289)	(0.267)	(0.283)
N	931542	593722	904277	621098	876046	649370	853380	671952

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 20: Probability of 2nd Birth by ISEE Quintiles, by type of contract

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.469**	0.063	-0.213	-0.039	0.040	0.100	0.072	-0.329
	(0.148)	(0.192)	(0.146)	(0.198)	(0.139)	(0.205)	(0.136)	(0.207)
$t_{2,3}$	0.500***	0.458*	0.134	-0.106	0.182	-0.305	-0.497***	0.139
	(0.144)	(0.194)	(0.145)	(0.205)	(0.144)	(0.204)	(0.136)	(0.207)
$t_{3,4}$	0.483***	0.059	0.149	-0.033	0.105	-0.253	0.006	0.110
	(0.141)	(0.191)	(0.142)	(0.197)	(0.141)	(0.197)	(0.138)	(0.202)
$t_{4,5}$	-0.051	-0.105	0.394**	0.070	-0.049	-0.373	0.101	0.037
	(0.149)	(0.201)	(0.147)	(0.205)	(0.146)	(0.206)	(0.141)	(0.207)
$t_{5,6}$	0.282	-0.416*	0.004	0.209	-0.101	-0.303	0.217	-0.137
	(0.151)	(0.194)	(0.148)	(0.197)	(0.143)	(0.201)	(0.140)	(0.200)
$t_{6,7}$	-0.074	-0.308	0.151	-0.222	0.045	-0.019	-0.126	-0.150
	(0.137)	(0.180)	(0.133)	(0.176)	(0.131)	(0.177)	(0.127)	(0.179)
$t_{7,8}$	0.204	-0.378*	-0.061	0.194	-0.024	-0.126	-0.042	0.119
	(0.135)	(0.170)	(0.132)	(0.171)	(0.127)	(0.177)	(0.122)	(0.183)
$t_{8,9}$	0.152	-0.505**	-0.126	-0.049	0.315*	0.204	-0.318*	-0.306
	(0.139)	(0.180)	(0.134)	(0.176)	(0.132)	(0.184)	(0.128)	(0.188)
$t_{9,10}$	-0.058	0.078	-0.009	-0.359*	0.108	0.078	-0.163	-0.042
	(0.133)	(0.179)	(0.128)	(0.178)	(0.125)	(0.179)	(0.121)	(0.186)
$t_{9,11}$	0.027	0.031	0.208	-0.404	0.013	-0.068	-0.108	-0.084
	(0.173)	(0.235)	(0.168)	(0.237)	(0.165)	(0.238)	(0.160)	(0.244)
$t_{9,12}$	-0.115	0.307	0.346	-0.429	0.106	0.028	0.077	0.064
	(0.195)	(0.266)	(0.189)	(0.272)	(0.187)	(0.276)	(0.182)	(0.286)
$t_{9,13}$	-0.164	0.475	0.453*	-0.454	0.125	0.171	0.114	0.115
	(0.214)	(0.296)	(0.208)	(0.304)	(0.205)	(0.310)	(0.201)	(0.323)
$t_{9,14}$	-0.149	0.481	0.530*	-0.280	-0.043	0.308	0.277	0.259
	(0.230)	(0.319)	(0.223)	(0.329)	(0.219)	(0.339)	(0.214)	(0.355)
$t_{9,15}$	-0.074	0.381	0.567*	0.050	-0.147	0.382	0.296	0.282
	(0.241)	(0.336)	(0.235)	(0.348)	(0.230)	(0.361)	(0.223)	(0.378)
$t_{9,16}$	-0.075	0.569	0.611*	0.067	-0.194	0.481	0.419	0.354
	(0.250)	(0.351)	(0.243)	(0.363)	(0.238)	(0.377)	(0.232)	(0.395)
N	918652	502789	964968	465327	1010128	429204	1055497	393307

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

D.0.2 Labor Supply

Table 21: Probability of Working by ISEE Quintiles, Regressions

	Q2	Q3	Q4	Q5
$t_{1,2}$	0.429**	0.194	0.094	0.416**
	(0.162)	(0.158)	(0.154)	(0.148)
$t_{2,3}$	0.098	0.298	-0.068	0.318*
	(0.158)	(0.156)	(0.153)	(0.148)
$t_{3,4}$	-0.309	-0.257	-0.011	-0.377*
	(0.164)	(0.159)	(0.155)	(0.147)
$t_{4,5}$	-0.043	-0.293*	-0.120	0.127
	(0.148)	(0.143)	(0.140)	(0.133)
$t_{5,6}$	-0.095	0.184	-0.056	-0.009
	(0.165)	(0.160)	(0.152)	(0.143)
$t_{5,7}$	-0.019	-0.033	0.003	0.263
	(0.209)	(0.204)	(0.195)	(0.182)
$t_{5,8}$	-0.355	-0.237	-0.371	-0.297
	(0.228)	(0.221)	(0.210)	(0.198)
$t_{5,9}$	-0.493*	-0.380	-0.476*	-0.395
	(0.245)	(0.237)	(0.227)	(0.214)
$t_{5,10}$	-0.210	-0.450	-0.436	-0.419
	(0.265)	(0.257)	(0.247)	(0.234)
$t_{5,11}$	-0.454	-0.333	-0.247	-0.678**
	(0.279)	(0.273)	(0.263)	(0.250)
$t_{5,12}$	-0.620*	-0.888**	-0.145	-1.159***
	(0.285)	(0.279)	(0.268)	(0.255)
$t_{5,13}$	-0.480	-1.041***	-0.432	-1.062***
	(0.291)	(0.284)	(0.274)	(0.262)
$t_{5,14}$	-0.399	-1.057***	-0.473	-1.006***
	(0.300)	(0.294)	(0.283)	(0.271)
$t_{5,15}$	-0.325	-1.017***	-0.604*	-0.814**
	(0.307)	(0.301)	(0.290)	(0.277)
$t_{5,16}$	-0.465	-1.484***	-0.694*	-1.209***
	(0.310)	(0.303)	(0.292)	(0.280)
N	1525452	1525530	1525566	1525489

$^{*}p < 0.05,^{**} p < 0.01,^{***} p < 0.001$

Table 22: Probability of Working by ISEE Quintiles no additional child, Regressions

	Q2	Q3	Q4	Q5
$t_{1,2}$	0.443*	0.102	0.134	0.277
	(0.209)	(0.206)	(0.201)	(0.193)
$t_{2,3}$	0.233	0.168	-0.087	0.371
	(0.206)	(0.205)	(0.200)	(0.193)
$t_{3,4}$	-0.150	-0.419	0.083	-0.443*
	(0.218)	(0.214)	(0.208)	(0.197)
$t_{4,5}$	-0.064	-0.198	-0.300	0.164
	(0.190)	(0.185)	(0.181)	(0.172)
$t_{5,6}$	-0.322	0.383	-0.058	-0.124
	(0.215)	(0.209)	(0.197)	(0.185)
$t_{5,7}$	-0.118	0.120	-0.041	0.211
	(0.269)	(0.263)	(0.251)	(0.234)
$t_{5,8}$	-0.565*	-0.066	-0.398	-0.436
	(0.286)	(0.279)	(0.265)	(0.248)
$t_{5,9}$	-0.664*	-0.144	-0.579*	-0.524*
	(0.305)	(0.296)	(0.281)	(0.266)
$t_{5,10}$	-0.367	0.002	-0.580	-0.498
	(0.330)	(0.320)	(0.307)	(0.290)
$t_{5,11}$	-0.511	-0.000	-0.332	-0.562
	(0.348)	(0.339)	(0.325)	(0.308)
$t_{5,12}$	-0.663	-0.426	-0.283	-1.195***
	(0.350)	(0.341)	(0.327)	(0.310)
$t_{5,13}$	-0.378	-0.901**	-0.355	-1.165***
	(0.357)	(0.347)	(0.332)	(0.318)
$t_{5,14}$	-0.118	-0.918*	-0.446	-0.826*
	(0.370)	(0.360)	(0.346)	(0.330)
$t_{5,15}$	-0.147	-0.694	-0.513	-0.745*
	(0.378)	(0.369)	(0.355)	(0.339)
$t_{5,16}$	-0.246	-1.056**	-0.594	-1.246***
	(0.381)	(0.370)	(0.355)	(0.341)
N	941362	937745	943360	954835

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 23: Probability of Working Part-time by ISEE Quintiles, Regressions

	Q2	Q3	Q4	Q5
$t_{1,2}$	0.229	-0.220	0.195	-0.174
	(0.183)	(0.170)	(0.152)	(0.146)
$t_{2,3}$	-0.100	0.146	-0.001	-0.034
	(0.166)	(0.152)	(0.144)	(0.140)
$t_{3,4}$	-0.052	0.092	0.008	0.033
	(0.152)	(0.139)	(0.133)	(0.127)
$t_{4,5}$	0.184	0.149	0.011	0.141
	(0.167)	(0.154)	(0.145)	(0.137)
$t_{5,6}$	0.225	0.074	0.119	-0.026
	(0.175)	(0.162)	(0.154)	(0.142)
$t_{5,7}$	0.464	0.227	0.150	-0.033
	(0.242)	(0.225)	(0.213)	(0.195)
$t_{5,8}$	0.467	0.302	0.194	-0.015
	(0.278)	(0.258)	(0.244)	(0.226)
$t_{5,9}$	0.554	0.551	0.195	0.234
	(0.309)	(0.283)	(0.267)	(0.251)
$t_{5,10}$	0.576	0.599	0.304	0.367
	(0.334)	(0.306)	(0.293)	(0.276)
$t_{5,11}$	0.778*	0.880**	0.217	0.503
	(0.357)	(0.326)	(0.310)	(0.295)
$t_{5,12}$	0.890*	0.686*	0.432	0.529
	(0.379)	(0.345)	(0.326)	(0.308)
$t_{5,13}$	1.225**	0.627	0.667*	0.481
	(0.395)	(0.359)	(0.339)	(0.323)
$t_{5,14}$	1.059*	0.873*	0.724*	0.589
	(0.412)	(0.375)	(0.354)	(0.336)
$t_{5,15}$	1.350**	1.057**	0.709	0.438
	(0.427)	(0.389)	(0.367)	(0.349)
$t_{5,16}$	1.450***	1.190**	0.715	0.624
	(0.441)	(0.403)	(0.378)	(0.359)
N	410946	477899	551844	622352

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 24: Probability of Working by ISEE Quintiles, by age of the mother

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.394	0.427	0.181	0.230	0.265	-0.126	0.735**	0.269
	(0.242)	(0.221)	(0.244)	(0.210)	(0.245)	(0.199)	(0.243)	(0.188)
$t_{2,3}$	0.010	0.210	0.349	0.208	-0.055	-0.019	0.275	0.305
	(0.237)	(0.216)	(0.241)	(0.208)	(0.244)	(0.198)	(0.244)	(0.190)
$t_{3,4}$	-0.366	-0.256	0.107	-0.546**	-0.178	0.134	-0.416	-0.310
	(0.244)	(0.222)	(0.245)	(0.212)	(0.249)	(0.200)	(0.246)	(0.185)
$t_{4,5}$	0.076	-0.134	-0.393	-0.213	-0.132	-0.073	0.145	0.135
	(0.225)	(0.198)	(0.223)	(0.190)	(0.222)	(0.182)	(0.220)	(0.169)
$t_{5,6}$	-0.168	-0.095	0.106	0.280	-0.036	-0.109	-0.025	-0.027
	(0.247)	(0.224)	(0.245)	(0.213)	(0.240)	(0.198)	(0.235)	(0.182)
$t_{5,7}$	0.074	-0.185	-0.383	0.309	0.055	-0.060	0.413	0.110
	(0.312)	(0.285)	(0.311)	(0.275)	(0.308)	(0.257)	(0.299)	(0.232)
$t_{5,8}$	-0.415	-0.416	-0.645	0.265	-0.160	-0.625*	-0.431	-0.196
	(0.343)	(0.307)	(0.341)	(0.294)	(0.336)	(0.274)	(0.328)	(0.251)
$t_{5,9}$	-0.511	-0.546	-0.920*	0.199	-0.204	-0.785**	-0.638	-0.248
	(0.370)	(0.330)	(0.367)	(0.314)	(0.363)	(0.294)	(0.356)	(0.273)
$t_{5,10}$	-0.380	-0.103	-1.179**	0.219	-0.294	-0.592	-0.504	-0.392
	(0.400)	(0.358)	(0.399)	(0.341)	(0.395)	(0.321)	(0.388)	(0.298)
$t_{5,11}$	-0.769	-0.139	-1.158**	0.389	0.283	-0.726*	-0.979*	-0.519
	(0.419)	(0.379)	(0.423)	(0.363)	(0.420)	(0.341)	(0.414)	(0.319)
$t_{5,12}$	-0.777	-0.471	-1.856***	-0.147	0.492	-0.601	-1.454***	-0.969**
	(0.429)	(0.385)	(0.435)	(0.368)	(0.431)	(0.347)	(0.425)	(0.324)
$t_{5,13}$	-0.761	-0.164	-1.659***	-0.537	-0.078	-0.721*	-1.341**	-0.861**
	(0.439)	(0.393)	(0.444)	(0.375)	(0.440)	(0.354)	(0.436)	(0.333)
$t_{5,14}$	-0.749	-0.109	-1.874***	-0.323	-0.183	-0.743*	-1.333**	-0.783*
	(0.451)	(0.407)	(0.456)	(0.390)	(0.453)	(0.369)	(0.448)	(0.346)
$t_{5,15}$	-0.747	0.061	-1.761***	-0.382	-0.437	-0.776*	-1.320**	-0.479
	(0.460)	(0.418)	(0.465)	(0.400)	(0.463)	(0.377)	(0.458)	(0.354)
$t_{5,16}$	-1.137*	0.195	-2.132***	-0.991*	-0.448	-0.888*	-1.830***	-0.795*
	(0.466)	(0.421)	(0.470)	(0.403)	(0.468)	(0.380)	(0.464)	(0.357)
N	770301	721201	735847	752564	696055	790856	655986	828945

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 25: Probability of Working by ISEE Quintiles, by area

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.589*	0.292	-0.027	0.298	0.262	-0.042	0.204	0.564*
	(0.254)	(0.211)	(0.236)	(0.214)	(0.218)	(0.219)	(0.202)	(0.220)
$t_{2,3}$	0.177	0.099	0.487*	0.161	-0.212	0.133	0.402	0.271
	(0.252)	(0.203)	(0.236)	(0.209)	(0.219)	(0.215)	(0.207)	(0.214)
$t_{3,4}$	-0.441	-0.292	-0.282	-0.282	-0.044	-0.060	-0.546**	-0.294
	(0.264)	(0.207)	(0.244)	(0.211)	(0.224)	(0.217)	(0.205)	(0.214)
$t_{4,5}$	-0.125	0.023	-0.302	-0.405*	-0.088	-0.181	0.028	0.224
	(0.231)	(0.194)	(0.213)	(0.195)	(0.199)	(0.197)	(0.181)	(0.197)
$t_{5,6}$	0.121	-0.367	-0.174	0.485*	-0.262	0.153	0.096	-0.209
	(0.254)	(0.218)	(0.239)	(0.216)	(0.215)	(0.216)	(0.192)	(0.215)
$t_{5,7}$	0.045	-0.264	-0.491	0.286	-0.172	0.123	0.159	0.252
	(0.320)	(0.278)	(0.302)	(0.279)	(0.275)	(0.280)	(0.245)	(0.274)
$t_{5,8}$	-0.534	-0.409	-0.923**	0.235	-0.756*	-0.023	-0.190	-0.503
	(0.348)	(0.304)	(0.325)	(0.304)	(0.296)	(0.302)	(0.267)	(0.298)
$t_{5,9}$	-0.997**	-0.305	-0.764*	-0.194	-0.980**	-0.033	-0.342	-0.526
	(0.375)	(0.327)	(0.349)	(0.326)	(0.320)	(0.325)	(0.291)	(0.321)
$t_{5,10}$	-0.738	-0.061	-1.203**	0.085	-0.562	-0.435	-0.625*	-0.296
	(0.407)	(0.352)	(0.379)	(0.354)	(0.349)	(0.354)	(0.319)	(0.351)
$t_{5,11}$	-0.875*	-0.335	-0.885*	-0.025	-0.524	-0.102	-0.820*	-0.571
	(0.430)	(0.370)	(0.404)	(0.375)	(0.372)	(0.376)	(0.341)	(0.373)
$t_{5,12}$	-0.962*	-0.518	-1.535***	-0.481	-0.604	0.219	-1.198***	-1.029**
	(0.439)	(0.378)	(0.412)	(0.383)	(0.379)	(0.384)	(0.349)	(0.381)
$t_{5,13}$	-0.826	-0.361	-1.538***	-0.797*	-1.018**	0.076	-0.980**	-1.050**
	(0.449)	(0.385)	(0.421)	(0.390)	(0.389)	(0.390)	(0.360)	(0.389)
$t_{5,14}$	-0.924*	-0.219	-1.724***	-0.591	-0.851*	-0.184	-1.045**	-0.939*
	(0.462)	(0.399)	(0.434)	(0.404)	(0.402)	(0.405)	(0.370)	(0.405)
$t_{5,15}$	-0.775	-0.282	-1.738***	-0.505	-1.059**	-0.318	-0.944*	-0.662
	(0.472)	(0.408)	(0.443)	(0.414)	(0.410)	(0.415)	(0.379)	(0.415)
$t_{5,16}$	-0.872	-0.333	-2.279***	-0.916*	-1.034*	-0.463	-1.334***	-1.034*
	(0.478)	(0.412)	(0.447)	(0.418)	(0.414)	(0.419)	(0.383)	(0.420)
N	678294	828908	731029	775354	782615	722638	826788	676952

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 26: Probability of Working by ISEE Quintiles, by average real wage

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.402	0.429	0.082	0.197	0.215	-0.134	0.401	0.335
	(0.211)	(0.259)	(0.210)	(0.245)	(0.212)	(0.228)	(0.210)	(0.212)
$t_{2,3}$	0.068	0.139	0.372	0.211	-0.265	0.111	0.479*	0.123
	(0.204)	(0.259)	(0.206)	(0.245)	(0.209)	(0.230)	(0.206)	(0.220)
$t_{3,4}$	-0.380	-0.188	-0.330	-0.140	0.108	0.017	-0.531*	-0.234
	(0.215)	(0.257)	(0.215)	(0.240)	(0.217)	(0.224)	(0.211)	(0.208)
$t_{4,5}$	-0.160	0.123	-0.280	-0.389	-0.212	0.071	0.227	0.060
	(0.191)	(0.242)	(0.189)	(0.226)	(0.186)	(0.215)	(0.182)	(0.200)
$t_{5,6}$	-0.133	-0.139	0.269	0.038	0.134	-0.366	-0.199	0.117
	(0.217)	(0.260)	(0.212)	(0.248)	(0.208)	(0.226)	(0.203)	(0.204)
$t_{5,7}$	-0.193	0.047	0.081	-0.401	0.177	-0.313	0.282	0.083
	(0.274)	(0.331)	(0.272)	(0.316)	(0.268)	(0.290)	(0.257)	(0.262)
$t_{5,8}$	-0.187	-0.695	-0.075	-0.604	-0.259	-0.549	-0.251	-0.415
	(0.297)	(0.367)	(0.293)	(0.347)	(0.287)	(0.318)	(0.278)	(0.289)
$t_{5,9}$	-0.051	-1.344***	-0.575	-0.210	-0.146	-0.936**	-0.380	-0.466
	(0.319)	(0.397)	(0.314)	(0.374)	(0.309)	(0.344)	(0.300)	(0.316)
$t_{5,10}$	0.360	-1.245**	-0.483	-0.616	-0.437	-0.640	-0.248	-0.720*
	(0.345)	(0.428)	(0.341)	(0.406)	(0.338)	(0.374)	(0.330)	(0.343)
$t_{5,11}$	0.029	-1.311**	-0.300	-0.608	-0.245	-0.529	-0.528	-0.878*
	(0.364)	(0.450)	(0.362)	(0.430)	(0.358)	(0.399)	(0.350)	(0.368)
$t_{5,12}$	-0.198	-1.336**	-0.719	-1.290**	0.009	-0.440	-1.074**	-1.144**
	(0.370)	(0.463)	(0.368)	(0.442)	(0.365)	(0.410)	(0.357)	(0.379)
$t_{5,13}$	-0.137	-1.164*	-0.924*	-1.333**	-0.209	-0.828*	-0.969**	-1.039**
	(0.377)	(0.476)	(0.375)	(0.452)	(0.371)	(0.419)	(0.365)	(0.390)
$t_{5,14}$	-0.060	-1.273**	-1.022**	-1.242**	-0.254	-0.897*	-0.823*	-1.158**
	(0.390)	(0.487)	(0.389)	(0.465)	(0.386)	(0.432)	(0.380)	(0.400)
$t_{5,15}$	-0.069	-0.933	-0.811*	-1.522**	-0.573	-0.931*	-0.625	-1.031*
	(0.399)	(0.497)	(0.398)	(0.474)	(0.394)	(0.442)	(0.388)	(0.411)
$t_{5,16}$	-0.152	-1.082*	-1.379***	-1.803***	-0.468	-1.078*	-1.251**	-1.126**
	(0.403)	(0.506)	(0.402)	(0.480)	(0.396)	(0.449)	(0.391)	(0.417)
N	890660	589035	856902	622200	821080	657113	786965	690555

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 27: Probability of Working by ISEE Quintiles, by childcare availability

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.416*	0.380	0.240	0.076	0.009	0.178	0.550**	0.222
	(0.207)	(0.260)	(0.207)	(0.245)	(0.206)	(0.230)	(0.201)	(0.217)
$t_{2,3}$	0.091	0.093	0.195	0.435	-0.072	-0.077	0.395*	0.213
	(0.199)	(0.260)	(0.200)	(0.250)	(0.202)	(0.235)	(0.199)	(0.222)
$t_{3,4}$	-0.412	-0.116	-0.230	-0.291	0.021	-0.035	-0.283	-0.480*
	(0.211)	(0.259)	(0.209)	(0.246)	(0.208)	(0.233)	(0.202)	(0.215)
$t_{4,5}$	0.023	-0.160	-0.299	-0.287	0.001	-0.285	-0.089	0.400*
	(0.189)	(0.238)	(0.189)	(0.220)	(0.187)	(0.211)	(0.180)	(0.196)
$t_{5,6}$	-0.195	0.002	0.565**	-0.424	-0.198	0.107	0.143	-0.218
	(0.211)	(0.263)	(0.207)	(0.250)	(0.202)	(0.229)	(0.196)	(0.207)
$t_{5,7}$	-0.216	0.196	0.375	-0.712*	-0.032	0.001	0.502*	-0.068
	(0.268)	(0.333)	(0.266)	(0.316)	(0.261)	(0.293)	(0.249)	(0.265)
$t_{5,8}$	-0.359	-0.427	0.160	-0.861*	-0.336	-0.444	-0.140	-0.502
	(0.290)	(0.366)	(0.287)	(0.346)	(0.279)	(0.321)	(0.268)	(0.293)
$t_{5,9}$	-0.328	-0.824*	-0.047	-0.904*	-0.528	-0.436	-0.282	-0.547
	(0.312)	(0.396)	(0.306)	(0.373)	(0.300)	(0.347)	(0.290)	(0.319)
$t_{5,10}$	-0.272	-0.239	-0.037	-1.141**	-0.426	-0.506	-0.164	-0.766*
	(0.338)	(0.427)	(0.334)	(0.403)	(0.327)	(0.377)	(0.316)	(0.348)
$t_{5,11}$	-0.450	-0.601	0.052	-0.990*	-0.277	-0.266	-0.452	-0.996**
	(0.356)	(0.451)	(0.354)	(0.429)	(0.348)	(0.401)	(0.338)	(0.371)
$t_{5,12}$	-0.663	-0.630	-0.576	-1.380**	-0.144	-0.180	-0.984**	-1.384***
	(0.362)	(0.460)	(0.360)	(0.440)	(0.354)	(0.411)	(0.344)	(0.381)
$t_{5,13}$	-0.366	-0.730	-0.794*	-1.440**	-0.577	-0.266	-0.828*	-1.365***
	(0.370)	(0.472)	(0.368)	(0.449)	(0.361)	(0.421)	(0.352)	(0.392)
$t_{5,14}$	-0.266	-0.706	-0.691	-1.657***	-0.756*	-0.133	-0.817*	-1.270**
	(0.383)	(0.484)	(0.381)	(0.463)	(0.375)	(0.433)	(0.366)	(0.402)
$t_{5,15}$	-0.269	-0.567	-0.533	-1.776***	-0.783*	-0.427	-0.761*	-0.892*
	(0.391)	(0.495)	(0.391)	(0.471)	(0.384)	(0.441)	(0.376)	(0.411)
$t_{5,16}$	-0.413	-0.663	-1.028**	-2.131***	-0.866*	-0.474	-1.079**	-1.362**
	(0.395)	(0.503)	(0.394)	(0.477)	(0.387)	(0.447)	(0.379)	(0.417)
N	931542	593722	904277	621098	876046	649370	853380	671952

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Table 28: Probability of Working by ISEE Quintiles, by labor mkt attachment

	Q2	Q2	Q3	Q3	Q4	Q4	Q5	Q5
$t_{1,2}$	0.345	0.579*	0.402*	-0.554*	-0.021	0.314	0.406*	0.357*
	(0.192)	(0.279)	(0.195)	(0.242)	(0.200)	(0.208)	(0.203)	(0.181)
$t_{2,3}$	0.059	0.127	0.170	0.528*	-0.142	-0.053	0.353	0.117
	(0.189)	(0.262)	(0.195)	(0.227)	(0.201)	(0.194)	(0.206)	(0.170)
$t_{3,4}$	-0.182	-0.504	-0.172	-0.195	0.110	-0.015	-0.372	-0.149
	(0.190)	(0.319)	(0.192)	(0.277)	(0.196)	(0.240)	(0.196)	(0.205)
$t_{4,5}$	0.050	-0.087	-0.175	-0.351	-0.175	0.237	0.179	0.226
	(0.173)	(0.276)	(0.175)	(0.238)	(0.178)	(0.207)	(0.179)	(0.178)
$t_{5,6}$	-0.052	-0.053	0.248	0.168	-0.009	-0.031	-0.197	0.471*
	(0.192)	(0.317)	(0.194)	(0.271)	(0.193)	(0.228)	(0.192)	(0.193)
$t_{5,7}$	0.088	0.229	-0.109	0.716*	0.055	0.215	0.108	0.864***
	(0.243)	(0.399)	(0.247)	(0.347)	(0.248)	(0.291)	(0.243)	(0.250)
$t_{5,8}$	-0.099	-0.153	-0.197	0.691	-0.345	0.267	-0.469	0.653*
	(0.266)	(0.413)	(0.269)	(0.363)	(0.269)	(0.303)	(0.265)	(0.263)
$t_{5,9}$	-0.285	0.150	-0.395	0.913*	-0.367	0.105	-0.541	0.653*
	(0.287)	(0.442)	(0.288)	(0.387)	(0.289)	(0.328)	(0.286)	(0.289)
$t_{5,10}$	0.088	0.318	-0.399	0.886*	-0.351	0.358	-0.560	0.762*
	(0.309)	(0.487)	(0.312)	(0.425)	(0.314)	(0.362)	(0.311)	(0.319)
$t_{5,11}$	-0.104	0.049	-0.275	1.129*	-0.241	0.822*	-0.842*	0.597
	(0.324)	(0.522)	(0.329)	(0.460)	(0.332)	(0.392)	(0.330)	(0.349)
$t_{5,12}$	-0.147	-0.277	-0.858*	0.925*	-0.043	0.886*	-1.380***	0.414
	(0.331)	(0.519)	(0.336)	(0.463)	(0.339)	(0.398)	(0.337)	(0.354)
$t_{5,13}$	0.066	-0.227	-1.045**	1.047*	-0.401	0.842*	-1.248***	0.519
	(0.338)	(0.533)	(0.343)	(0.473)	(0.345)	(0.408)	(0.345)	(0.366)
$t_{5,14}$	0.123	0.039	-0.979**	0.855	-0.589	1.189**	-1.097**	0.411
	(0.348)	(0.551)	(0.353)	(0.492)	(0.357)	(0.425)	(0.356)	(0.379)
$t_{5,15}$	0.183	0.249	-0.929*	0.950	-0.631	0.913*	-0.968**	0.795*
	(0.356)	(0.564)	(0.361)	(0.504)	(0.364)	(0.437)	(0.364)	(0.390)
$t_{5,16}$	0.003	0.459	-1.382***	0.598	-0.685	0.880*	-1.383***	0.547
	(0.361)	(0.562)	(0.365)	(0.502)	(0.368)	(0.437)	(0.368)	(0.391)
N	1200297	325155	1142535	382995	1081212	444354	1019461	506028

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

E Construction of Elasticities

This appendix describes the procedure used to construct the elasticities reported in Section 5.3.

Predetermined ISEE and treatment intensity Treatment intensity is defined using household ISEE measured in 2023, reflecting economic conditions prior to the AUU reform. This predetermined ISEE is used consistently to assign mothers to ISEE quintiles and to simulate pre- and post-reform child-related benefits.

Simulation of benefits and reform-induced gains For each ISEE quintile q, we compute pre-reform benefits B_q^{pre}, post-reform AUU benefits B_q^{post}, and the reform-induced gain

$$G_q = B_q^{post} - B_q^{pre}.$$

Table 29 reports average values by quintile.

Table 29: Simulated Benefits and Household Income by ISEE Quintile

	Benefit pre	Benefit gain	Benefit post	ISEE	Household income
Q1	2190	260	2450	7862	17612
Q2	1777	669	2446	9920	22222
Q3	1420	1031	2451	11741	26300
Q4	1220	1232	2452	13509	30259
Q5	1160	1290	2450	15303	34278

Notes: Benefit amounts are annual and expressed in euros. Pre-reform benefits include simulated ANF and tax deductions. Household income is inferred from ISEE under the assumption that ISEE is fully determined by labor income.

Elasticity formulas For each adjacent comparison q versus $q-1$, the percentage change in benefit generosity is defined as:

$$\%\Delta G_{q,q-1} = \frac{G_q - G_{q-1}}{(B_q^{pre} + B_{q-1}^{pre})/2},$$

while the percentage change in household income is computed from the implied labor income levels associated with the two ISEE values.

$$\%\Delta Y_{q,q-1} = \frac{G_q - G_{q-1}}{(Y_q^{pre} + Y_{q-1}^{pre})/2},$$

Elasticities are then defined as:

$$\varepsilon_{L,G}^{(q,q-1)} = \frac{\%\Delta L_{q,q-1}}{\%\Delta G_{q,q-1}}, \qquad \varepsilon_{L,Y}^{(q,q-1)} = \frac{\%\Delta L_{q,q-1}}{\%\Delta Y_{q,q-1}}.$$

Aggregate elasticity The aggregate elasticity reported in Section 5.3 is constructed differently from the local elasticities defined above.

Specifically, the numerator is given by the average estimated percentage change in labor supply across all adjacent ISEE comparisons. The denominator is computed at the aggregate level as the percentage change in average benefits (or household income), defined as the difference between average post-reform and pre-reform values divided by the average pre-reform level.

Formally, the aggregate elasticity with respect to benefit generosity is defined as:

$$\varepsilon_{L,G}^{agg} = \frac{\overline{\%\Delta L}}{(\bar{B}^{post} - \bar{B}^{pre})/\bar{B}^{pre}},$$

and analogously for household income:

$$\varepsilon_{L,Y}^{agg} = \frac{\overline{\%\Delta L}}{(\bar{Y}^{post} - \bar{Y}^{pre})/\bar{Y}^{pre}}.$$

This aggregate elasticity should be interpreted as the elasticity of the average labor supply response with respect to a system-wide change in average transfer generosity implied by the reform, and not as an average of local elasticities.

Interpretation Because denominators are constructed from simulations based on ISEE, elasticities depend on the maintained assumptions linking ISEE to benefit entitlements and labor income. These assumptions are the same as those underlying the empirical identification strategy and should be interpreted jointly with the main results. Elasticities are reported to facilitate comparison with the literature and to summarize magnitudes, but they should not be interpreted as structural labor supply parameters.